

Courts, contracts, and international trade: judicial enforcement and global value chain participation

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Abstract

This article examines whether judicial enforcement shapes firms' participation in global value chains (GVCs). Exploiting Italy's 2013 court reorganization as a natural experiment, we combine firm-level survey data with administrative records and implement a spatial discontinuity instrumental variable design. We find that longer trials significantly reduce the probability of GVC participation: adding one month of court time lowers participation by 2.3 percentage points, roughly a 10 per cent decline relative to the average participation rate. The effect emerges across different positions in the value chain, is amplified for contractually complex products and sectors, and is consistent with external finance as a key mechanism.

Keywords: global value chains; judicial enforcement; regional development; product complexity.

JEL classifications: F10, F61, K41, R11.

1. Introduction

Institutions that guarantee the enforcement of contracts are a fundamental determinant of international trade (Greif 1992; Anderson and Marcouiller 2002; Levchenko 2007). When firms engage in cross-border transactions, exporters must be confident they will receive timely and adequate payment, while importers must be certain of receiving goods of the agreed quality and quantity. In the absence of effective judicial enforcement, these conditions may not hold, raising transaction costs and limiting the scope of international integration (Greif 1993; Berkowitz et al. 2006).

This issue is particularly relevant in the context of global value chains (GVCs), where production is fragmented across borders and firms rely heavily on repeated contractual exchanges for intermediate goods and services. In such networks, weak enforcement may disrupt supply relationships, discourage

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investment in relationship-specific assets, and ultimately hinder participation in complex cross-border production processes (Antràs and Chor 2022). Yet, GVC participation is a key channel for firm-level productivity gains (Crescenzi et al. 2014; Bernard et al. 2018), through access to larger markets, technology transfer, and learning opportunities, and a crucial driver of broader economic development (Blaum et al. 2018; Bernard et al. 2019; Comotti et al. 2020). Understanding whether, and to what extent, local disparities in judicial efficiency affect firms' integration into GVCs is therefore of the utmost importance.

While a large body of research has examined the role of institutional quality in shaping trade patterns across countries (Berkowitz et al. 2006; Levchenko 2007), the evidence at the subnational level remains more limited. Existing studies have focused on the impact of judicial enforcement on firms' behavior in areas such as access to finance, investment decisions, or labor adjustments (Boehm and Oberfield 2020; Chemin 2020; Pezone 2023), but only a handful of studies (see, e.g., Accetturo et al. 2017) have analyzed how local disparities in court efficiency affect firms' integration into GVCs.

This article fills this gap by providing causal evidence on whether, and to what extent, judicial enforcement at the subnational level shapes firms' ability to participate in global production networks, exploiting the case of Italian firms. To fix ideas, consider the following stylized example. Imagine two otherwise identical Italian firms, Firm A and Firm B, both producing customized semfinished components in the same sector, with similar productivity, size, and financial conditions. The only difference is that Firm A operates under the jurisdiction of an efficient court, while Firm B falls under a slow and inefficient one. Suppose a German multinational seeks reliable suppliers for its automotive value chain. For Firm A, efficient enforcement implies that any dispute over quality or delivery could be resolved swiftly, which strengthens its incentives to comply with contractual obligations. By contrast, working with Firm B entails the possibility of years-long disputes and higher costs. Faced with this asymmetry, the buyer is more likely to select Firm A as a supplier because the local enforcement environment makes cooperation less risky and more efficient. A similar logic applies when Italian firms participate in GVCs as buyers of inputs rather than suppliers. Suppose Firm C and Firm D are both importers of specialized components, with comparable size and productivity. Firm C is located in a jurisdiction with efficient courts, while Firm D operates in a district with severe judicial delays. A foreign supplier evaluating the two perceives that contracts with Firm C are less exposed to enforcement frictions: if Firm C defaults or delays payment, disputes can be resolved relatively quickly, reducing counterparty risk. By contrast, supplying to Firm D entails the possibility of protracted litigation and higher enforcement costs. As a result, the foreign partner is more likely to engage with Firm C.

Italy provides a particularly suitable context for studying how judicial enforcement shapes firms' participation in GVCs. First, its productive structure is characterized by small and medium-sized enterprises that are deeply embedded in international production networks, often specializing in customized intermediate inputs supplied to foreign lead firms (Brancati et al. 2017; Minetti et al. 2019). Second, despite a uniform national legal framework, the Italian judicial system displays substantial heterogeneity in court efficiency across districts, with large differences in the expected duration of civil proceedings (Bianco et al. 2007; Coviello et al. 2015). Such spatial variation in judicial enforcement translates into meaningful differences in the credibility and cost of contract enforcement faced by firms operating in otherwise similar economic environments, with potentially important implications for firms' ability to participate in global production networks. Moreover, a major court reorganization implemented in 2013, based on pre-determined criteria unrelated to local economic conditions or court performance, provides a source of plausibly exogenous variation in judicial efficiency that we exploit for identification.

To perform our empirical analysis, we combine several data sources. The primary source of firm-level information is the MET survey, a large and nationally representative dataset covering about 25,000 Italian companies in each wave. In this article, we rely on four waves of the survey, focusing on the period 2011–17. The survey provides detailed information on firms' international activities, including import and export status, the nature of traded goods (raw materials, intermediates, final goods, and services), and the presence of long-term relationships with foreign partners, which we use to construct measures of GVC participation. To enrich these data, we merge the survey with balance sheet

information from Moody's Orbis, which supplies standardized financial indicators and precise geographical identifiers at the municipality level. We further integrate data from the Italian Ministry of Justice, which reports annual statistics on the inflow, resolution, and stock of civil cases at the court-district level, from which we derive our main measure of judicial efficiency, trial length.¹

To estimate the causal effect of judicial efficiency on firms' participation in GVCs, we adopt an instrumental variable strategy that exploits the 2013 court reorganization as a source of exogenous variation in trial length. The reform closed and merged a number of courts according to predetermined criteria unrelated to judicial performance or local economic conditions, generating plausibly exogenous changes in the efficiency of judicial enforcement across districts.² We use the predicted variation in trial length induced by the reform as an instrument for actual trial duration. To further mitigate concerns of unobserved local heterogeneity, we complement this approach with a spatial discontinuity design that restricts the analysis to firms located along pre-reform judicial borders, thereby comparing firms exposed to different courts but facing otherwise similar economic environments.

Our results show that judicial inefficiency significantly hampers firms' ability to participate in GVCs. A 10 per cent increase in trial length (about 37 days at the sample mean) reduces the probability of GVC participation by about 2.9 percentage points, a magnitude that is both statistically robust and economically meaningful. In practical terms, adding a month of court time cuts GVC participation by about 2.3 percentage points, equivalent to roughly a 10 per cent decline relative to the sample mean participation rate (22.7 per cent). This effect is confirmed across a wide battery of robustness checks, including alternative definitions of GVC participation and different measures of judicial efficiency. The estimates remain stable when accounting for time-varying local conditions, firm heterogeneity, and nonlinear specifications, reinforcing the view that the relationship is not an artifact of model choice or sample composition. The analysis also reveals heterogeneities that help rationalize the baseline findings. Using the MET survey information on the nature of traded items, we first distinguish firms' roles in cross-border production stages. The adverse effect of longer trials emerges both for upstream-oriented and midstream-oriented participation, indicating that judicial delays hinder integration into cross-border production linkages across different positions in the chain. We then examine heterogeneous effects by contractual complexity, which links court efficiency to incomplete contracting in international production. The negative impact of longer trials is systematically stronger in environments where verification is harder, and contracts are more incomplete: in particular, it is larger for differentiated products (Rauch 1999) and in sectors with higher contract intensity (Nunn 2007). A consistent pattern also emerges when using the granularity of firms' product/industry classification as a within-data proxy for product specificity. Finally, we investigate external finance as a mechanism linking judicial efficiency to firms' GVC participation. Reform-driven variation in trial length significantly affects firms' equilibrium reliance on both bank credit and trade credit. Consistent with this channel, the negative effect of longer trials on GVC participation is concentrated among firms that actually use bank and trade credit, suggesting that timely enforcement supports participation in cross-border production by strengthening creditor rights and the credibility of repayment in both bank and supplier-buyer relationships.

This article makes three main contributions. First, we contribute to the literature on institutional quality and international trade, and to the growing work on the determinants of GVC participation, by providing causal micro-level evidence on how judicial enforcement shapes firms' integration into cross-border production networks. Classic contributions in international trade emphasize that contract enforcement

1. While the exact duration of trials is not directly observed for our study period, for 2016 the Italian Ministry of Justice released an official measure of actual trial length (Bartolomeo 2017). Using that release, Pezone (2023) validates the standard proxy by documenting a strong positive correlation with observed durations. He also shows that the proxy is tightly associated with backlog per judge, a widely used efficiency indicator in the literature (Ponticelli and Alencar 2016). These external checks give confidence that the proxy closely tracks true trial duration and court (in)efficiency.
2. Institutional details of the 2013 court reorganization, including its design, closure/merger criteria, and timeline, are provided in Section 2.

is a key determinant of comparative advantage and trade patterns, especially for complex, contract-intensive goods (Greif 1992; Anderson and Marcouiller 2002; Berkowitz et al. 2006; Levchenko 2007). At the same time, the GVC literature highlights that international production is organized through repeated contractual exchanges for intermediates and services, so that enforcement frictions can distort firms' ability to join and remain in fragmented production processes (Antràs and Chor 2013, 2022; Fernandes et al. 2022). We speak directly to this intersection by leveraging the detailed MET survey information on the nature of traded items to construct a precise firm-level measure that targets cross-border production linkages. This measurement choice allows us to move beyond standard import/export proxies and to focus on participation in fragmented production stages rather than on internationalization per se (Bernard et al. 2018). Methodologically, we identify the effect of enforcement within a single country and a uniform legal framework by exploiting Italy's 2013 court reorganization as a natural experiment and embedding it in a spatial discontinuity instrumental variable (IV) design along pre-reform court borders. This approach delivers plausibly exogenous variation in judicial efficiency and mitigates concerns that cross-sectional differences in court performance may reflect persistent local characteristics correlated with both enforcement and firms' international outcomes. In this sense, a closely related study is Accetturo et al. (2017), which uses cross-sectional Italian firm data to study participation in international production networks. Our article is complementary and advances this line of research by exploiting the reform-induced shock to enforcement and a panel structure, which allow us to strengthen identification through an IV strategy rooted in predetermined administrative criteria.

Second, we add to the literature on contractual complexity and incomplete contracting in international production by showing that the impact of judicial enforcement on GVC participation is heterogeneous and systematically stronger where verification is difficult, and contracts are more incomplete. In particular, our results connect subnational variation in court efficiency to the classic insight that enforcement frictions are most consequential when transactions involve customization and relationship-specific inputs, in line with the contract-enforcement and incomplete-contracting literature in international trade and GVCs (Berkowitz et al. 2006; Nunn 2007; Antràs and Chor 2013, 2022).

Third, we contribute to the literature on judicial enforcement, creditor rights, and firm outcomes by documenting external finance as a mechanism linking court efficiency to firms' GVC participation. A large body of work shows that court efficiency affects credit markets by shaping collateral values and expected recovery, with implications for borrowing costs, credit supply, and firm real outcomes (Jappelli et al. 2005; Zazzaro 2005; Visaria 2009; Rodano et al. 2016; Chemin 2020; Degryse et al. 2023; Pezone 2023). In parallel, international trade research emphasizes that financial frictions are central for firms' international activity and that improved access to finance affects both extensive and intensive margins of importing and exporting (Minetti and Zhu 2011; Manova 2013). We complement these findings by providing direct evidence that reform-driven improvements in judicial efficiency translate into greater reliance on external finance, and that the negative effect of longer trials on GVC participation is concentrated among firms that use bank and trade credit, consistent with enforcement shaping the feasibility of financing working-capital needs and sustaining supplier–buyer credit relationships in cross-border production networks (Murro and Peruzzi 2022).

The remainder of the article is organized as follows. Section 2 describes the Italian court system and the 2013 reform that we exploit for identification. Section 3 presents the data, the main variables, and our empirical strategy. Section 4 reports the baseline estimates, as well as a battery of robustness checks, and Section 5 explores heterogeneity by value chain characteristics. Section 6 investigates the mechanism that rationalizes the main results. Section 7 concludes.

2. The Italian court system and the 2013 reform

This section describes the institutional setting underlying our empirical analysis. We first outline the main features of the Italian civil court system that are relevant for contract enforcement in domestic and

cross-border commercial disputes, emphasizing the substantial heterogeneity in enforcement across judicial districts. We then present the 2013 court reorganization, which constitutes the core source of exogenous variation exploited in our identification strategy and will be formally incorporated into the empirical framework discussed in Section 3.3.

2.1 Cross-border commercial disputes in Italy

In the Italian legal system, jurisdiction in civil and commercial disputes is determined by law and typically corresponds to the district where the firm's establishment is located. As a consequence, when a contract is signed, the parties cannot unilaterally opt into a different domestic court. Under the rules of international private law, parties to a cross-border contract may instead agree to submit disputes to a specific court or to arbitration, and such a forum need not be located in the jurisdiction of either the buyer or the supplier. However, once a judgment or arbitral award is issued, its practical effectiveness depends on the domestic court's ability to execute promptly against the assets of the party found liable. This institutional feature is reinforced by Article 26 of the Code of Civil Procedure, which assigns execution to the court in the district where the debtor's assets are located. Accordingly, even when adjudication takes place in a foreign court or through arbitration, the speed and effectiveness of enforcement ultimately depend on the efficiency of the Italian court responsible for execution, which is determined by the location of the assets of the party against whom enforcement is sought.

2.2 Structure and performance of the Italian court system

The Italian civil court system has long been among the slowest in Europe and displays substantial heterogeneity across districts, with large differences in the efficiency and duration of judicial proceedings (Bianco et al. 2007; Coviello et al. 2015; Pezone 2023). According to the 2024 EU Justice Scoreboard, in 2022 the average time needed to resolve litigious civil and commercial cases in first instance was about 540 days, compared with a median of 239 days across EU Member States, and with most European countries completing such cases within a year (European Commission 2024). The World Bank's Doing Business 2020 report indicated that resolving a commercial dispute lasted an estimated 1,120 days in the representative Italian city, with substantial subnational variation (World Bank 2020). As illustrated in Fig. 1, which reports trial length in 2012 across Italian districts, some courts resolved cases in under one year, while others required more than twice as long, highlighting the pronounced territorial disparities in judicial efficiency and the marked delays in some areas. In our dataset, the most efficient district is the one of Rovereto (located in Trentino-Alto Adige, in the North-East of Italy), with an average trial length over the period 2007–16 of about 188 days, and a minimum of 167 in 2011. By contrast, in the district of Patti (situated in the north-eastern part of Sicily, in Southern Italy) the average trial length exceeded 1,098 days, with a peak of 1,399 days in 2011. Such protracted timelines and inefficiencies in judicial enforcement represent a structural barrier to economic activity, undermining firms' ability to operate and grow (Berkowitz et al. 2006; Iacopetta et al. 2024). In this context, understanding how variations in court performance shape firms' participation in global markets is of the utmost importance.

2.3 The 2013 judicial reform

The basic structure of the Italian judiciary dates back to the late nineteenth century, and despite some courts being suppressed in 1941 and minor revisions up until 1999, the organization remained largely stable until the recent reform of 2013. In 2012, against the backdrop of significant economic and political challenges related to the European sovereign debt crisis, Italy undertook a major reorganization of its judicial system. The reform was part of a broader package of measures introduced by the technocratic government that took office in late 2011 to curb public expenditure and restore the state's finances. Among its provisions, the legislative intervention included the closure and merger of several courts with the aim of concentrating administrative and financial resources in a smaller number of judicial offices.

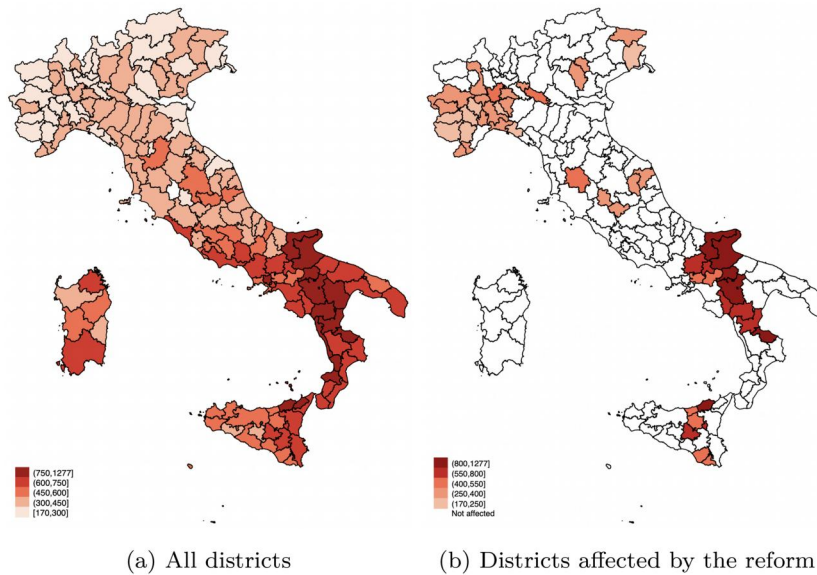


Figure 1. Trial length across Italian districts (2012).

Notes: The figure shows a choropleth map of trial length across (a) all Italian districts and (b) those affected by the 2013 reform, based on data as of 2012.

More specifically, the reform resulted in the closure of twenty-six courts, whose jurisdictions were merged into twenty-three remaining courts.³ The reorganization aimed primarily at reducing the number of courts, especially those not located in provincial capitals, to achieve cost savings and operational efficiency. Importantly, the selection criteria for court closures did not depend on court-specific productivity or other individual performance indicators; rather, they were based exclusively on ex-ante mechanical criteria: all courts not located in provincial capitals were to be suppressed, subject to the constraint of retaining at least three courts for each appeal district. Minor exceptions were introduced upon recommendation by the Direzione Nazionale Antimafia (the special prosecutor responsible for prosecuting mafia-related organizations), which advised against closing certain courts, thereby preventing the elimination of six Southern courts located in areas with a strong presence of organized crime. The reform became fully effective in September 2013, marking a significant restructuring of the Italian judicial landscape.

The exogenous nature of this reorganization, driven by predetermined criteria unrelated to the specific performance of individual courts, provides a natural experiment suitable for identifying the causal impact of variations in judicial efficiency on firms' behavior, including their participation in GVCs.

3. Data and empirical strategy

In this section, we describe the data sources (Section 3.1), the construction of the main variables (Section 3.2), and the empirical strategy adopted to identify the causal effect of judicial efficiency on firms' participation in GVCs (Section 3.3).

3. The complete list of pre- and post-reform districts is reported in [Table A.1](#).

3.1 Data sources

To perform our empirical investigation, we draw information from four main sources: (1) the MET survey on Italian firms; (2) the Moody's Orbis database; (3) official statistics from the Italian Ministry of Justice; (4) the Italian National Institute of Statistics (ISTAT).

The first and primary source of firm-level data is the MET survey, the largest private survey of firms ever conducted within a single European country.⁴ The survey currently consists of nine waves (2008, 2009, 2011, 2013, 2015, 2017, 2019, 2021, and 2023), each covering approximately 25,000 firms. It is explicitly designed to be representative of the Italian business sector across three dimensions: firm size, industry, and geographic region (North, Center, and South). Importantly, the survey covers micro-enterprises, which represent the overwhelming majority of Italian firms, as around 95 per cent of the population have fewer than ten employees (Di Giorgio and Murro 2021). To prevent the sample from being overly dominated by very small firms, the MET survey adopts a disproportionate sampling scheme that ensures representativeness across all strata defined by the intersection of firm size, region, and sector. Consistent with the national distribution of businesses, nearly half of the surveyed firms are located in the North, while the remainder are almost equally divided between the Center and the South. The survey provides rich information on firms' internationalization strategies, innovation activities, access to bank credit, corporate structure, and performance. This breadth makes it particularly well suited to studying participation in GVCs. For the purpose of our analysis, we restrict the sample to the period 2011–17, excluding the initial (2008 and 2009) and the more recent waves to ensure consistency with other data sources. Focusing on this time span also aligns the survey information with the 2013 judicial reform, which is central to our identification strategy.

The second source of data is Moody's Orbis, a comprehensive global database covering private and public companies, originally developed by Bureau van Dijk and now maintained by Moody's Analytics. We merge Orbis with the MET survey in order to enrich survey responses with standardized firm-level information. In particular, Orbis provides detailed balance sheet data and precise information on firms' location at the municipality level, which is crucial for linking firms to the corresponding judicial district.⁵

The third source is the Italian Ministry of Justice, from which we obtain annual data on incoming and resolved cases, as well as the full mapping of municipalities to judicial districts before and after the 2013 reform. This information allows us to measure judicial efficiency and to link firms to the relevant court of jurisdiction.

The fourth source is the ISTAT, which provides municipality-level variables used as controls in the empirical analysis. ISTAT also supplies the official registry of bordering municipalities as of 2011, which we exploit in our empirical design.

The construction of the working dataset proceeds in several steps. We first match the MET survey to municipality-level data in order to recover firms' location, which is necessary to assign each firm to its corresponding judicial district. Because municipality identifiers are not available for all surveyed firms, this step implies an initial reduction in the sample, yielding approximately 60,000 firm-year observations. We then merge the resulting dataset with Moody's Orbis to enrich the survey information with balance-sheet variables. This further reduces the sample to about 52,000 observations. We subsequently restrict the analysis to firms located in judicial districts affected by the 2013 reform and, in our preferred specification, to firms located along pre-reform judicial borders, which strengthens identification by

4. The MET survey has been used as a testing ground in a number of studies for other purposes, such as Balduzzi et al. (2024), who examine firms' expectations during the COVID-19 shock, Brancati (2025), analyzing the role of banks' support on firms' export activity, and Brancati et al. (2025), who study how supply chain linkages affect firms' innovation behavior.

5. Since a large share of firms in the MET survey are micro-enterprises, and many of these are not legally required to file full financial statements in Italy, the merging process leads to a substantial reduction in the number of usable observations. We return to this issue in Section 4.2, where we explicitly discuss the implications of this sample attrition when including balance sheet variables as controls in our econometric specification.

comparing firms facing different courts but otherwise similar local economic environments. These restrictions lead to a final estimation sample of roughly 4,500 observations.

Given the multiple data restrictions implied by the merging procedure and the quasi-experimental design, [Table A.2](#) documents the sample construction step by step (Panel A) and reports summary statistics for key firm characteristics and the sectoral composition across three benchmark samples: the full MET survey, the subsample for which municipality identifiers are available, and the final estimation sample affected by the court reform (Panel B). While the final sample is substantially smaller, its composition remains broadly similar in terms of firm size and sectoral distribution once municipality information is available. In contrast, the comparison with the full MET survey highlights that the successive data and design restrictions are associated with non-negligible compositional changes.⁶

Summary statistics for all variables employed in the analysis, computed on the final reference sample, are reported in [Table 1](#).

3.2 Measurement

3.2.1 GVC participation

Identifying firms that participate in GVCs is a challenging task. Empirical studies relying on micro-level data often resort to simple proxies, such as whether a firm engages in import or export activities (see, e.g., [Bernard et al. 2018](#); [Antràs and Chor 2022](#)). Yet, this criterion can be misleading in advanced economies, where cross-border trade may be driven by market expansion or cost-saving strategies rather than actual integration into global production networks. In this paper, following [Brancati et al. \(2017\)](#) and [Minetti et al. \(2025\)](#), we adopt a more comprehensive approach that better captures the complexity of the phenomenon and provides a more reliable proxy for GVC participation.

The MET survey collects detailed information on firms' international transactions, distinguishing whether traded goods concern raw materials, semifinished products, final goods, or services. Firms are asked whether they import and/or export and, conditional on a positive response, to specify the nature of the traded goods. This level of detail allows us to move beyond simple import/export indicators and to more precisely identify firms engaged in cross-border production stages. Based on this information, we construct our baseline measure of GVC participation to capture firms' engagement in cross-border production linkages. We classify as GVC participants, first, firms that both import raw materials or intermediate inputs and export semifinished products or components, or services, excluding exporters of final goods. This pattern reflects firms that are simultaneously connected to foreign suppliers and foreign customers within international production networks. Second, given that semifinished products or components are typically embedded in broader cross-border production processes, we also classify as GVC participants firms that export such goods even if they do not report importing inputs. Our dependent variable *GVC* is therefore a binary indicator equal to one for firms meeting either of these conditions and zero otherwise. In Section 4.2, we perform some robustness tests on this measure.

First, we adopt a more restrictive definition that excludes firms importing only raw materials. Under this definition, firms are classified as GVC participants if they import intermediate inputs and export goods or services, or if they export semifinished products or components. This specification restricts attention to firms more clearly engaged in cross-border production stages beyond the simple sourcing of raw materials. Second, we consider a definition that emphasizes the stability of cross-border linkages. Specifically, we classify as GVC participants only those firms that meet the baseline definition and, in addition, report having long-term and significant relationships with foreign companies in the MET survey.⁷

6. To assess whether our estimates are sensitive to these changes, in a robustness check we reweight the final estimation sample to match the covariate distribution of the full MET survey.

7. Although this measure is self-reported, it captures whether firms perceive their international linkages as stable and strategically relevant. By incorporating this information, we restrict attention to firms engaged in durable cross-border relationships rather than occasional international transactions.

Table 1. Summary statistics.

	All firms			GVC = 1			GVC = 0		
	Mean	Standard deviation	Observations	Mean	Observations	Mean	Observations	t-test	
Dependent variables									
GVC	0.227	0.419	4,548						
GVC: excluding importers of raw materials	0.219	0.413	4,548	0.964	1,031	0.000	3,517	-166.345	
GVC: long-term foreign partnerships	0.065	0.247	4,548	0.287	1,031	0.000	3,517	-20.367	
Independent variables									
Trial length	368.651	199.270	4,462	311.667	1,012	385.366	3,450	13.185	
Congestion rate	1.025	0.127	4,521	1.006	1,026	1.030	3,495	7.106	
Inefficiency rate	1.024	0.494	4,521	0.891	1,026	1.062	3,495	12.505	
Instrumental variable $\Delta \times 1(t > 2013)$	0.002	0.074	4,503	-0.001	1,023	0.003	3,480	1.372	
Control variables									
Age	25.500	17.022	4,536	28.862	1,031	24.512	3,505	-6.779	
Small	0.781	0.414	4,548	0.673	1,031	0.813	3,517	8.703	
Medium	0.172	0.377	4,548	0.259	1,031	0.146	3,517	-7.556	
Business group	0.173	0.378	4,548	0.273	1,031	0.144	3,517	-8.531	
Number of firms	84.308	15.459	4,548	84.372	1,031	84.289	3,517	-0.154	
Number of bank branches	0.560	0.253	4,548	0.592	1,031	0.551	3,517	-4.355	
Debt ratio	1.287	3.596	3,924	0.973	940	1.386	2,984	3.690	
Profitability (ROA)	0.011	0.097	3,988	0.022	947	0.007	3,041	-4.644	
Liquidity ratio	2.114	2.258	3,985	2.048	946	2.135	3,039	1.154	
Court suppressed	0.274	0.446	4,548	0.293	1,031	0.268	3,517	-1.565	

Notes: The table presents summary statistics and univariate tests for the main variables employed in the regressions. Definitions of all variables are provided in [Table A.3](#).

Figure 2 reports the spatial distribution of GVC participation across judicial districts, revealing substantial geographical heterogeneity. Participation is systematically higher in the Center and North of Italy and lower in the South, in line with well-established patterns of industrial specialization and international integration (see, e.g., Brancati et al. 2025). Sectoral variation is equally pronounced. Figure 3 illustrates the distribution of GVC participation across sectors in our sample. In the manufacturing sector, participation is particularly high in the manufacture of basic metals (24), where more than 60 per cent of firms are part of GVCs. Close to half of the firms are involved in GVCs in several other manufacturing industries, including the manufacture of basic pharmaceutical products and pharmaceutical preparations (21), the manufacture of rubber and plastic products (22), the manufacture of chemicals and chemical products (20), the manufacture of textiles (13), the manufacture of computer, electronic, and optical products (26), and the manufacture of electrical equipment (27). Among nonmanufacturing industries, the highest incidence of participation (around 30 per cent) is observed in wholesale trade, except for motor vehicles and motorcycles (46), financial service activities, except insurance and pension funding (64), real estate activities (68), and creative, arts and entertainment activities (90).

Additional descriptive evidence is provided in Table 1, which reports summary statistics for GVC participation under the alternative definitions considered in the analysis. Under the baseline measure, 22.7 per cent of firms in the reference sample are classified as GVC participants. This share declines slightly to 21.9 per cent when excluding firms that import only raw materials, indicating that a limited fraction of firms classified as participants under the baseline definition are driven by raw material sourcing. When we further restrict the definition to firms reporting long-term and significant relationships with foreign partners, the share decreases more markedly to 6.5 per cent, reflecting a narrower set of firms engaged

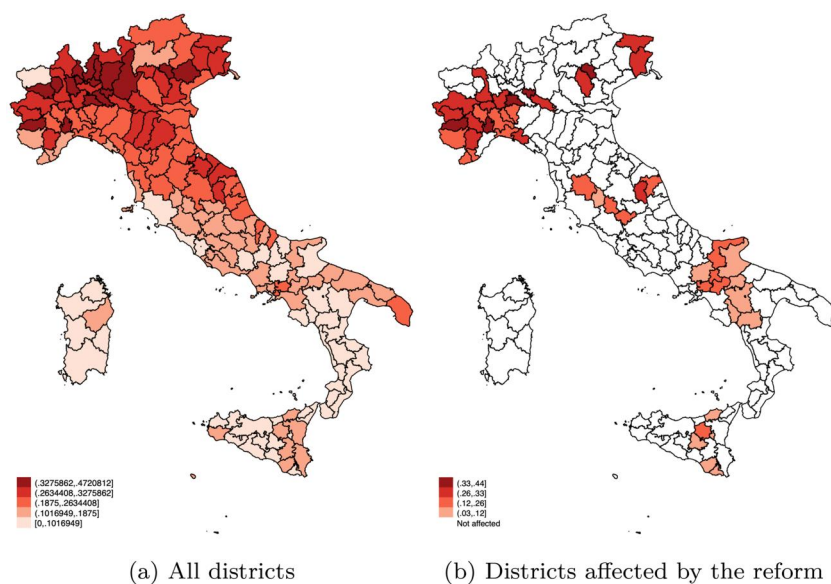


Figure 2. GVC participation across Italian districts (2011–17).

Notes: The figure shows a choropleth map of GVC participation across (a) all Italian districts and (b) those affected by the 2013 reform, based on average data for the period 2011–17.

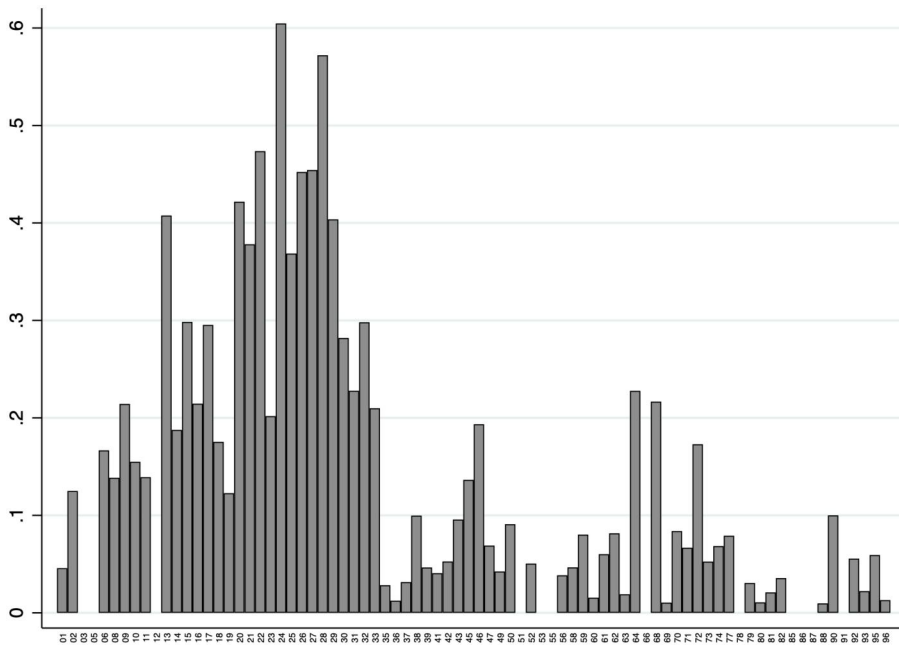


Figure 3. Participation in GVCs across NACE two-digit industry sectors (2011–17).

Notes: The figure shows the share of firms participating in GVCs in each two-digit NACE sector for the period 2011–17. The x-axis reports the two-digit NACE codes for the sectors: (1) Agriculture, forestry, and fishing (01–03); (2) Mining and quarrying (05–09); (3) Manufacturing (10–33); (4) Electricity, gas, steam, and air conditioning supply (35); (5) Water supply; sewerage, waste management, and remediation activities (36–39); (6) Construction (41–43); (7) Wholesale and retail trade; repair of motor vehicles and motorcycles (45–47); (8) Transportation and storage (49–53); (9) Accommodation and food service activities (55–56); (10) Information and communication (58–63); (11) Financial and insurance activities (64–66); (12) Real estate activities (68); (13) Professional, scientific, and technical activities (69–75); (14) Administrative and support service activities (77–82); (15) Public administration and defense; compulsory social security (84); (16) Education (85); (17) Human health and social work activities (86–88); (18) Arts, entertainment, and recreation (90–93); (19) Other service activities (94–96).

in durable and structured cross-border production linkages.⁸ Univariate tests further show that firms classified as GVC participants are systematically older, larger, more likely to belong to a business group, and display higher profitability relative to nonparticipants. Moreover, significant differences also emerge in terms of judicial efficiency: firms integrated in GVCs are on average located in districts with shorter trial length (311 days) compared to 385 days for nonparticipants.

3.2.2 Trial length

The exact duration of a trial in a court is generally not directly observed. Therefore, researchers often use a proxy based on readily available data on the number of pending, incoming, and resolved cases (Palumbo et al. 2013; Pezone 2023).⁹ This measure is defined as follows:

8. These magnitudes are in line with existing firm-level evidence on participation in international production networks, based on alternative data sources and measurement strategies. For example, estimates based on the EFIGE survey indicate that around 32 per cent of Italian firms participated in global value chains in 2010 (Minetti et al. 2025). Consistently, Accetturo et al. (2017), using firm-level data and an upstream-focused measure based on supplying customized inputs abroad, find participation rates of a comparable order of magnitude.
9. A potential concern about trial length is a speed-quality trade-off, where longer proceedings could reflect more diligent and thorough decisions. Two pieces of evidence mitigate this. First, Coviello et al. (2015), using judge-level data from the Court of Milan, find no correlation between a judge's decision speed and a standard proxy for decision quality, namely the share of cases appealed. Second, Leonardi and Rancan (2009) show, on 2006 data, no significant association between average trial length and the reversal rate, i.e., the share of first-instance decisions overturned on appeal.

$$Length_t = \frac{Pending_{t-1} + Pending_t}{Incoming_t + Resolved_t} \times 365 \quad (1)$$

For the years considered in our analysis, the trial length ranges from 111 to 2,435 days with an average of 397 and a median of 357 days. Figure 1 shows the geographic distribution of trial length across Italian districts (1) and within the affected courts (2) for the year 2012. Darker shades indicate longer trial lengths, implying worse enforcement. As shown in the figure, treated courts are relatively well distributed throughout the country, although there is a noticeable concentration in the Northwest and the South. While courts in the North generally process cases more quickly, there is considerable variation both within regions and, more importantly, between adjacent courts.¹⁰

To estimate the causal relationship between judicial efficiency (measured by trial length) and GVC participation, we first include trial length in lagged form to mitigate simultaneity concerns, and then adopt an IV strategy. This approach is particularly valuable in this context as it helps to address potential endogeneity issues, where unobserved factors could simultaneously influence both court efficiency and firms' internationalization decisions. To isolate the impact of judicial efficiency on GVC participation, we use the reorganization of court districts in Italy, as described in Section 2, as an exogenous shock to trial length. Specifically, we employ the predicted change in trial length of post-reform court districts as our instrument.

Consider two court districts: Rapid Court (R) and Delayed Court (D). Prior to the reform, Court R had an average trial duration of 50 days, whereas Court D required 100 days to resolve similar cases. After the judicial reorganization, Court R was closed and its judges reassigned to Court D. As a result, firms originally served by Court R are now under the jurisdiction of Court D. The expected trial duration in the merged district becomes $(50 + 100)/2 = 75$ days. Thus, firms initially under Court R experience a predicted increase of 25 days in trial length, whereas those originally under Court D benefit from an equivalent reduction. These predicted changes in trial length, expressed in logarithms, serve as instruments for the realized post-reform duration of proceedings.

More formally, let $x_{i,j,t}$ represent the value of variable x in year t for a pre-reform district i that became part of district j after the reform. The predicted trial length of the court in a post-reform district is calculated as follows:

$$\overline{Length}_{j,2012} = \frac{\sum_{i \in j} (Pending_{i,j,2011} + Pending_{i,j,2012})}{\sum_{i \in j} (Incoming_{i,j,2012} + Resolved_{i,j,2012})} \times 365 \quad (2)$$

For a firm belonging to the pre-reform district i , the predicted change in trial length is thus given by:

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

Naturally, the actual trial length of the new, larger court does not need to precisely match the predicted value. Larger courts may benefit from economies of scale, such as through increased specialization among judges, which can enhance overall efficiency. On the other hand, court reorganizations, particularly in the initial months following the reform, may introduce temporary delays due to logistical adjustments. Nonetheless, it is reasonable to expect that certain structural characteristics of the suppressed courts will persist in the newly merged districts. To illustrate this point, consider the case of the districts of Imperia and Sanremo, whose average pre-reform trial lengths were 337 and 382 days, respectively. As expected, the actual average duration for the merged district in the three years following the reform (2014–16) falls between these two values, at 372 days. The predicted value from Equation (2) is 365 days, remarkably close to the observed average.

10. In addition to trial length, in Section 4.2 we employ two alternative measures of judicial enforcement: the *Congestion rate*, defined as the ratio of pending to incoming cases, and the *Inefficiency rate*, defined as the ratio of pending to resolved cases. These measures provide complementary perspectives on judicial performance, capturing demand and supply-side sources of inefficiency respectively.

3.2.3 Control variables

To absorb observable determinants of firms' international engagement, our baseline specifications include a set of control variables. At the firm level, we control for age (*Age*, in log), which proxies experience and export know-how; size, using two dummy variables: *Small*, equal to one for firms with less than 50 employees, and *Medium*, equal to one for firms with 50–249 employees, with large firms as the reference category, to capture scale and fixed market-entry costs; and *Business group*, a dummy variable equal to one if the firm belongs to a corporate group, reflecting access to internal finance, shared services, and international networks.¹¹ At the local level, we include the number of active firms per 1,000 inhabitants in the municipality (*# Firms*) to proxy agglomeration and market thickness, and the number of bank branches per 1,000 inhabitants (*# Bank branches*) to capture local credit supply and financial intermediation. Variable definitions and sources are summarized in Table A.3. All specifications also include year and industry fixed effects to account for common macro shocks and sectoral differences in trade intensity and contracting needs. In tighter designs we further add border $\times$ year fixed effects.¹²

3.3 Empirical strategy

As discussed in Section 2, the design of the reform was such that neither institutional quality nor local economic conditions played a role in determining which courts were suppressed or merged. This exogeneity supports the use of the reform as a source of identification. However, it is difficult to entirely rule out the presence of confounding factors that may have disproportionately affected firms operating in districts that were initially more efficient. To address this concern, and following the law and finance literature (Ponticelli and Alencar 2016; Brown et al. 2017), we adopt a spatial discontinuity design that helps control for unobserved heterogeneity at the local level. This empirical strategy relies on the idea that firms located on opposite sides of a pre-reform district boundary are likely to share similar economic and institutional conditions, yet may have been differently exposed to the reform depending on their court assignment.

The IV model we estimate is specified as follows:

$$\text{Log}(\text{Length}_{f,i,j,t-1}) = \alpha \cdot \Delta_{ij} \times \mathbf{1}(t > 2013) + \delta X_{f,t} + \gamma_s + \eta_t + \varepsilon_{f,i,j,t} \quad (4)$$

where $\text{Log}(\text{Length})_{f,i,j,t-1}$ denotes the logarithm of trial length for firm f , which is under the jurisdiction of pre-reform district i and post-reform district j , in year $t - 1$, as defined in Equation (1); Δ_{ij} represents the predicted change in trial length due to the court district reorganization, where i indicates the pre-reform district that merged into the post-reform district j , as presented in Equation (3); $\mathbf{1}(t > 2013)$ is an indicator variable that equals 1 for the years after the 2013 reform and 0 otherwise; $X_{f,t}$ is a vector of firm-level control variables; γ_s captures industry fixed effects to control for sector-specific trends; η_t represents year fixed effects; $\varepsilon_{f,i,j,t}$ is the error term.

The predicted trial length from the first stage, $\widehat{\text{Log}(\text{Length})}_{f,i,j,t-1}$, is then used to estimate its effect on GVC participation:

$$\text{GVC}_{f,i,j,t} = \beta \cdot \widehat{\text{Log}(\text{Length})}_{f,i,j,t-1} + \zeta X_{f,t} + \gamma_s + \eta_t + \varepsilon_{f,i,j,t} \quad (5)$$

The coefficient $\hat{\beta}$ is the primary parameter of interest. Assuming the validity of the instrument, it provides a consistent estimate of the impact of trial length on firms' probability of participating in GVCs.

11. As noted above, we conduct a robustness check by augmenting the baseline with balance sheet variables from Orbis, confirming that the results are not driven by observable financial conditions, though at the cost of a reduced sample due to reporting requirements.

12. Untabulated regressions confirm that our findings are also robust to the inclusion of industry $\times$ year fixed effects.

In line with the spatial discontinuity design, Equations (4) and (5) are estimated on a restricted sample of firms located in municipalities close to the borders of affected pre-reform court districts. This sample restriction helps to isolate quasi-random exposure to the reform and provides a robust strategy to control for omitted variables that may vary locally over time. Firms on either side of a pre-reform district boundary are likely to face similar local conditions, but may have experienced different changes in court efficiency.

4. Results

4.1 Baseline results

Table 2 presents the baseline estimates of the relationship between judicial efficiency, proxied by trial length, and firms' participation in GVCs.¹³ Column (1) reports results using the full sample of firms, without restricting the analysis to those headquartered near a court border. While this broad sample is not the main focus of the article, it provides a useful benchmark for comparison. The coefficient on *Log(Length)* is negative (−0.0915) and highly significant, indicating that longer trial durations are strongly associated with a lower likelihood of GVC participation. Economically, this implies that a 10 per cent increase in trial length is associated with approximately a 0.9 percentage point reduction in the probability that a firm engages in GVCs.

While the estimate in column (1) provides an informative benchmark, it may be confounded by unobserved local characteristics, such as economic development, public sector quality, or social capital, that are likely correlated with both court efficiency and firms' internationalization decisions. To mitigate these concerns, column (2) restricts the sample to firms located in municipalities close to the borders of pre-reform judicial districts. This restriction reduces the sample size by approximately 40 per cent but allows for comparison between firms operating in geographically proximate areas that were exposed to different court assignments due to administrative boundaries. The coefficient on *Log(Length)* remains negative and statistically significant, suggesting that the baseline result is not driven solely by structural differences across districts. Column (3) further tightens the sample by focusing exclusively on bordering municipalities within court districts that were actually affected by the 2013 reform. This sample refinement increases the relevance of the reform-driven variation in trial length and aligns the estimation more closely with the identification strategy. The coefficient remains stable and significant, with an estimated value of −0.1062, providing additional support for a robust negative relationship between judicial delays and GVC participation.

Columns (4) and (5) implement the IV strategy to address potential endogeneity concerns. More specifically, in column (4) we use the 2SLS estimator on the sample of bordering municipalities in districts affected by the reform, including year and industry fixed effects. The instrument, reported at the bottom of the column and defined as the interaction between the predicted change in trial length and a post reform indicator ($\Delta_{ij} \times \mathbf{1}(t > 2013)$), is positively and significantly associated with actual trial length. The corresponding F-statistic ($F = 37.432$) exceeds the conventional threshold of 10, indicating that the instrument is sufficiently strong. The coefficient on *Log(Length)* is negative (−0.3006) and highly significant, implying that a 10 per cent increase in trial duration (equivalent to roughly thirty-seven additional days given the sample mean of about 370 days) reduces the likelihood of GVC participation by about 2.9 percentage points. Put differently, extending proceedings by about one week reduces the probability of GVC participation by nearly 0.6 percentage points, while an increase of one month lowers it by about 2.3 percentage points. Since the average participation rate in our sample is 22.7 per cent, these effects correspond to relative declines of 2.5 per cent and 10 per cent respectively, underscoring the economic significance of

13. To save space, the coefficients of the control variables are reported in Table A.4. Results are consistent with expectations: older firms and those belonging to a business group are more likely to participate in GVCs, while small firms are significantly less likely to do so.

Table 2. Baseline results.

Dependent variable	Full sample	Bordering cities	Bordering cities affected by the reform		
			Year + industry FE		Border × year FE
	OLS GVC (1)	OLS GVC (2)	OLS GVC (3)	2SLS GVC (4)	2SLS GVC (5)
Log(Length)	−0.0915*** (0.0048)	−0.0854*** (0.0068)	−0.1062*** (0.0159)	−0.3006** (0.1330)	−0.2730* (0.1604)
First stage: instrumental variable $\Delta \times 1(t > 2013)$				0.5684*** (0.0929)	0.4833*** (0.0850)
Controls	Y	Y	Y	Y	Y
Year fixed effects (FE)	Y	Y	Y	Y	N
Industry FE	Y	Y	Y	Y	Y
Border × year FE	N	N	N	N	Y
Observations	58,327	34,577	4,433	4,433	4,433
F instrument				37.43	32.33

Notes: This table reports the effects of trial length on firms' global value chain participation, estimated using ordinary least squares in columns (1)–(3) and 2SLS in columns (4) and (5). All regressions include control variables. Columns (1)–(4) include year and industry fixed effects. Column (5) includes industry and border × year fixed effects. Trial length is instrumented with the predicted change in trial length induced by the 2013 judicial reform × the post-reform dummy ($\Delta \times 1(t > 2013)$). The first stage estimate of the instrument is shown at the bottom of columns (4) and (5). The table also reports the Kleibergen–Paap Wald *F*-statistic from the first stage. Standard errors, reported in parentheses, are robust to heteroskedasticity. See [Table A.3](#) and Section 3.2 for details on the variables.

*, **, and *** are significant at 10 per cent, 5 per cent, and 1 per cent, respectively.

judicial inefficiency. In column (5), we introduce border × year fixed effects, thereby comparing only firms located on opposite sides of the same pre-reform judicial border. This strategy absorbs any time-varying shocks that are common to firms sharing the same local environment. The coefficient on *Log(Length)* remains negative and statistically significant (−0.2730), suggesting that a 10 per cent increase in trial duration leads to an approximate 2.6 percentage point decrease in the likelihood of GVC participation.

It is worth noting that these 2SLS estimates should be interpreted as a local average treatment effect. They capture the causal impact of trial length for firms whose exposure to judicial delays is shifted by the reform-induced variation in predicted court efficiency within our reference sample, rather than the average effect associated with trial length differences across the full national distribution.¹⁴

The exclusion restriction underlying our identification strategy is that the reform affects the outcome variable only through its impact on judicial efficiency. To provide supporting evidence for this assumption, [Table A.5](#) compares the actual reduced-form results (i.e., the effect of the instrument on GVC participation of firms located in bordering affected municipalities) with a placebo reduced form (i.e., the effect of the instrument on GVC participation in the full sample). Column (1) shows a negative relationship between the instrument and GVC participation for firms located in bordering municipalities affected by the reform. Column (2), in contrast, presents no statistically significant effect in the full sample, and the point

14. To provide additional descriptive evidence on timing and pre-trends, [Appendix 2](#) reports an event-study analysis around the 2013 reform.

estimates are considerably smaller in magnitude. This pattern reinforces the interpretation that the effect of the instrument operates through judicial efficiency, rather than through some alternative channel.

Furthermore, as explained in Section 3.1, the merging procedure and the restrictions implied by our identification strategy reduce the estimation sample and alter its composition relative to the full MET survey. To assess whether our baseline findings are sensitive to these compositional shifts, in [Table A.6](#) we implement an inverse-probability reweighting exercise. Specifically, we estimate each firm's probability of entering the final estimation sample as a function of predetermined characteristics (i.e., survey year, firm age, firm-size indicators, and sector dummies) and then reestimate the baseline ordinary least squares and 2SLS specifications weighting each observation by the inverse of this predicted probability. The reweighted estimates remain qualitatively unchanged and close in magnitude to the baseline results, indicating that our main conclusions are not driven by the over-representation of particular firm types in the final sample.

4.2 Robustness tests

4.2.1 Robustness on measurement

[Table 3](#) examines the robustness of our findings to alternative definitions of GVC participation and judicial enforcement. Column (1) replicates the baseline specification of [Table 2](#), column (4), and is included for ease of comparison. In column (2), we adopt a more restrictive definition of GVC participation that excludes firms importing only raw materials. Under this specification, firms are classified as GVC participants if they import intermediate inputs (excluding raw materials) and export goods or services, or if they export semifinished products or components. The coefficient on *Log(Length)* remains negative and statistically significant, with a magnitude comparable to the baseline, indicating that the main result is

Table 3. Robustness on measurement.

Dependent variable	Baseline results.	Alternative dependent variables		Alternative independent variables	
	GVC (1)	GVC: excluding importers of raw materials (2)	GVC: long-term foreign partnerships (3)	GVC (4)	GVC (5)
<i>Log(Length)</i>	-0.3006** (0.1330)	-0.2537* (0.1300)	-0.1720* (0.0880)		
<i>Log(Congestion rate)</i>				-0.2478** (0.1022)	
<i>Log(Inefficiency rate)</i>					-0.2668 (0.1681)
Controls	Y	Y	Y	Y	Y
Year fixed effects (FE)	Y	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y	Y
Observations	4,433	4,433	4,433	4,486	4,486
F instrument	37.43	37.43	37.43	102.6	50.39

Notes: This table reports a set of robustness checks on the measurement of GVC participation and judicial enforcement, estimated using a 2SLS model. Column (1) reports the baseline estimates of [Table 2](#), column (4). Columns (2) and (3) report results with two alternative measures of GVC participation, that is, *GVC: excluding importers of raw materials*, and *GVC: long-term foreign partnerships*. Columns (4) and (5) report results with two alternative measures of judicial enforcement, that is, *Log (Congestion rate)*, and *Log(Inefficiency rate)*. All regressions include control variables, and industry and year fixed effects. Each measure of judicial enforcement is instrumented using the predicted change in the measure induced by the 2013 judicial reform $\times$ the post reform dummy. The table also reports the Kleibergen–Paap Wald *F*-statistic from the first stage. Standard errors, reported in parentheses, are robust to heteroskedasticity. See [Table A.3](#) and Section 3.2 for details on the variables. *, **, and *** are significant at 10 per cent, 5 per cent, and 1 per cent, respectively.

not driven by firms engaged merely in the cross-border sourcing of raw materials. Column (3) considers a definition that emphasizes the stability of cross-border linkages. Specifically, we classify as GVC participants only firms that meet the baseline definition and, in addition, report having long-term and significant relationships with foreign companies in the MET survey. The estimated effect of trial length is again negative and statistically significant, suggesting that judicial inefficiency hampers not only firms engaged in fragmented production linkages but also those involved in more durable and structured international partnerships.

In columns (4) and (5), we turn to alternative measures of judicial enforcement. Column (4) uses the *Congestion rate*, defined as the ratio of pending to incoming trials, which captures the combined effect of demand pressures (litigiousness) and court caseload on judicial performance (Fabbri and Padula 2004; Comi et al. 2021). Column (5) uses the *Inefficiency rate*, defined as the ratio of pending to resolved cases in a given year, which reflects a supply-side notion of inefficiency and measures a court's inability to dispose of its existing backlog (Palumbo et al. 2013; Comi et al. 2021). For both measures, the instruments are constructed analogously to trial length, that is, as the predicted change in the respective indicator induced by the 2013 judicial reform, interacted with a post-reform dummy. The coefficient estimated with the congestion rate (column 4) is negative and statistically significant, consistent with our baseline findings. In contrast, the specification with the inefficiency rate (column 5) delivers a negative but statistically insignificant coefficient, indicating weaker evidence when relying on this alternative proxy.

4.2.2 Other robustness checks

Table 4 reports a set of additional robustness exercises that further corroborate our baseline findings. Column (1) estimates an IV Probit specification, which allows us to account for the binary nature of the dependent variable. The coefficient on *Log(Length)* remains negative and highly significant, indicating that the baseline results are not sensitive to the choice of a nonlinear model. Column (2) includes a dummy for firms located in judicial districts that were suppressed after the 2013 reform. This test addresses the concern that the reorganization of court districts may have exerted a direct impact on firms beyond its effect on trial length, for instance through the closure of local judicial facilities. The results show that the inclusion of the *Court suppressed* dummy does not alter the main coefficient of interest, which remains negative and statistically significant, in line with evidence in Pezone (2023) that court mergers did not systematically affect firms through channels other than judicial efficiency. In column (3), in addition to the baseline control variables, we further add balance sheet indicators: debt ratio, profitability, and liquidity. In this case, the sample size declines markedly, since many micro-enterprises in the MET survey are not legally required to file financial statements. Despite this reduction in observations, the coefficient on *Log(Length)* remains negative (−0.3676) and significant, confirming that the baseline effect is not explained by observable firm-level or financial characteristics. Column (4) restricts the analysis to the subsample of firms located outside provincial capitals. This check mitigates the concern that large cities, which often host provincial capitals, might follow distinct economic dynamics compared to smaller municipalities. The negative and significant coefficient confirms that our findings are not driven by large urban centers. Finally, column (5) aggregates the data at the municipal level, thereby reducing potential noise in firm-level outcomes and addressing concerns of intra-municipality correlation. The point estimate remains negative and statistically significant.

Overall, these additional robustness checks reinforce the validity of our baseline findings and indicate that the negative relationship between judicial inefficiency and GVC participation is not driven by specific model choices, sample restrictions, or alternative specifications.

5. Heterogeneous effects

GVC participation takes different forms, and the role of judicial enforcement is unlikely to be uniform across them. In this section, we use the granular information in the MET survey to study heterogeneity

Table 4. Other robustness checks.

Dependent variable	IV probit	Including <i>Court suppressed</i> dummy	Additional controls	Excluding provincial capitals	Municipality-level regression
	GVC (1)	GVC (2)	GVC (3)	GVC (4)	GVC (5)
Log(<i>Length</i>)	−1.4344*** (0.4145)	−0.2800** (0.1263)	−0.3676** (0.1574)	−0.4320** (0.1771)	−0.4117* (0.2486)
Court suppressed		−0.0195 (0.0146)			
Debt ratio			−0.0016 (0.0017)		
Profitability (ROA)			0.1628*** (0.0620)		
Liquidity ratio			−0.0003 (0.0027)		
Controls	Y	Y	Y	Y	Y
Year fixed effects (FE)	Y	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y	N
Observations	4,355	4,433	3,832	2,329	968
F instrument		43.30	29.92	26.24	9.698

Notes: This table reports a set of additional robustness checks. Column (1) reports IV Probit estimates of the main independent variable (*Log(Length)*) together with the first stage estimate of the instrument ($\Delta \times 1(t > 2013)$). Column (2), estimated using 2SLS, includes a dummy variable indicating whether the firm was located in a court suppressed after the 2013 judicial reform (*Court suppressed*). Column (3) reports 2SLS estimates with additional control variables from the balance sheet. Column (4) reports 2SLS estimates for the subsample of firms not located in provincial capitals. Column (5) reports 2SLS estimates at the municipality level. Trial length is instrumented using the predicted change in trial length induced by the 2013 judicial reform $\times$ the post reform dummy. The table also reports the Kleibergen–Paap Wald *F*-statistic from the first stage. Standard errors, reported in parentheses, are robust to heteroskedasticity. See [Table A.3](#) and Section 3.2 for details on the variables.

*, **, and *** are significant at 10 per cent, 5 per cent, and 1 per cent, respectively.

along two dimensions that are directly related to contracting frictions: firms' position in cross-border production stages (Section 5.1), and the contractual complexity of the products they trade (Section 5.2).

5.1 Value-chain position

Participation in GVCs involves different roles along the production process, and the relevance of contract enforcement may vary accordingly. A simple way to capture this heterogeneity is to distinguish firms that mainly operate at relatively upstream stages, typically supplying intermediate inputs and services (while either not importing inputs or importing only raw materials), from midstream firms that are more centrally embedded in cross-border production sequences where both sourcing and selling involve semi-finished goods. These different positions expose firms to different contractual frictions. Upstream-oriented firms may face disputes related to product conformity, delivery delays, and performance mainly in their relationships with customers. Midstream firms, instead, are exposed to enforcement issues on both sides of the chain, as they rely on cross-border contracts both to secure inputs and to place their output. In both cases, slower courts raise expected enforcement costs and weaken the incentives to initiate and sustain cross-border production linkages, but the magnitude of the effect may differ depending on where the firm is positioned.

To capture this heterogeneity using the MET survey information on the nature of traded items, we construct two mutually exclusive indicators, consistent with our baseline measure of GVC participation

(Antràs et al. 2012; Bernard et al. 2018). We define upstream-oriented firms as those that export semifinished products/components or services while not importing intermediate inputs (i.e., firms that either do not import at all or import only raw materials). We define midstream-oriented firms as those that both import and export semifinished products/components. The first group can be interpreted as firms that participate in cross-border production primarily as suppliers of intermediate outputs, whereas the second group captures firms that are simultaneously connected to foreign suppliers and foreign customers through intermediate goods trade, consistent with more intensive cross-border production fragmentation.¹⁵ Table 5, in columns (1)–(2), reports 2SLS estimates for these two measures. The estimated coefficient on $\text{Log}(\text{Length})$ is negative and statistically significant in both cases, indicating that judicial delays depress participation in GVCs regardless of whether firms operate in a more upstream-oriented mode or in a more midstream configuration. Column (3) provides an additional benchmark by considering a distinct form of internationalization, that is, exporting of final outputs, which we exclude from our baseline GVC definition. The coefficient remains negative and statistically significant, suggesting that court inefficiency raises contractual frictions more broadly and can hinder not only cross-border production linkages based on intermediate trade, but also final outputs exporting.¹⁶

5.2 Contractual complexity: product differentiation and contract intensity

When products and transactions are more contractually complex, output is often customized. Key quality attributes are costly to specify ex ante and hard to verify ex post, and performance frequently requires adaptation over the course of the relationship. As a result, contracts are more incomplete, and disputes over quality, delivery, and specifications become more likely and more relevant. In such environments, timely and credible judicial enforcement is a critical input into cross-border production linkages: faster courts reduce the expected cost of resolving disputes, protect relationship-specific investments, and limit hold-up risks, thereby sustaining international relationships that rely on repeated contracting (Berkowitz et al. 2006; Antràs and Chor 2022). By contrast, when goods are standardized and easily verifiable, a larger share of contractual frictions can be mitigated through private governance, such as reputation, repeated interaction, and extrajudicial arrangements, making firms less dependent on the quality of public enforcement. We exploit this idea in a set of heterogeneity tests that examine whether the impact of trial length is stronger precisely in environments where contractual incompleteness and verification problems are more severe.

Table 6 implements this exercise using three complementary proxies of contractual/product complexity, each capturing a different dimension of the underlying mechanism. First, we rely on the Rauch (1999) classification to distinguish differentiated goods, where quality is less observable, and disputes are more likely, from homogeneous or reference-priced goods, for which standardization and observable benchmarks make contracting easier.¹⁷ Accordingly, we expect judicial delays to be more detrimental for firms operating in differentiated-product segments. Second, we use the measure of sectoral contract

15. Importantly, these categories are not meant to replicate value-added decompositions of upstream and downstream participation used in macro input-output approaches (Koopman et al. 2014); rather, they provide firm-level proxies of different modes of engagement in cross-border production stages based on the direction and type of traded items.

16. The indicator *Exporters of final outputs* identifies firms exporting final consumption goods and may include firms that also export semifinished products or components. To ensure that the estimated effect can be related to final-good exporting, we also reestimate column (3) using a stricter definition that identifies firms exporting only final goods (i.e., excluding those that also export semi-finished products or components). The coefficient on $\text{Log}(\text{Length})$ remains negative and statistically significant (-0.150^*), confirming that court inefficiency hinders international activity also for firms whose exporting is confined to final outputs.

17. We mapped our sectoral classification (NACE Rev. 2) into a product-based taxonomy using official correspondence tables and a standard concordance chain. Specifically, we first linked NACE (four-digit classes) to the associated CPA product categories (which are structurally aligned with NACE at the class level), and then used Eurostat correspondence tables to translate CPA into Combined Nomenclature (CN) product codes for the relevant year. Finally, we converted CN into SITC (Rev. 4) codes and applied the Rauch (1999) classification, distinguishing differentiated, reference-priced, and organized-exchange goods, at the SITC level.

Table 5. Heterogeneity by value-chain position and export of final outputs.

Dependent variable	Value-chain position		Final outputs
	Upstream-oriented GVC participation. (1)	Midstream-oriented GVC participation (2)	Exporters of final outputs (3)
Log(Length)	−0.2866** (0.1201)	−0.1740* (0.0980)	−0.2863** (0.1180)
Controls	Y	Y	Y
Year fixed effects (FE)	Y	Y	Y
Industry FE	Y	Y	Y
Observations	4,433	4,433	4,433
F instrument	37.43	37.43	37.43

Notes: The table reports heterogeneity estimates of the effect of trial length on firms' international engagement by value-chain position and export of final outputs. The dependent variable in column (1), *Upstream-oriented GVC participation*, is a dummy equal to one for firms exporting semifinished products/components or services, and either not importing inputs or importing only raw materials. In column (2) *Midstream-oriented GVC participation* is a dummy equal to one for firms importing semifinished products/components and exporting semifinished products/components. In column (3) *Exporters of final outputs* is a dummy equal to one for firms exporting final consumption goods. All regressions are estimated by 2SLS on the sample of bordering municipalities affected by the reform and include the baseline controls, plus year and industry fixed effects. Trial length is instrumented with the predicted change in trial length induced by the 2013 judicial reform $\times$ the post-reform dummy ($\Delta \times 1(t > 2013)$). The table reports the Kleibergen–Paap Wald *F*-statistic from the first stage. Standard errors, reported in parentheses, are clustered at the industry level. See Table A.3 and Section 3.2 for details on the variables.

*, **, and *** are significant at 10 per cent, 5 per cent, and 1 per cent, respectively.

intensity developed by Nunn (2007), which captures the extent to which production in a sector relies on relationship-specific intermediate inputs and thus on incomplete contracts. If enforcement matters through contractual frictions, the adverse effect of longer trials should be amplified in high contract-intensity sectors. Third, we complement these external classifications with a within-data proxy based on the granularity of the firm's product classification: firms mapped into more detailed NACE categories (e.g., 5–6 digit) are interpreted as operating in more specific product niches, whereas more aggregated categories (3–4 digit) are more likely to capture relatively standardized production (Herrera and Minetti 2007; Minetti et al. 2015).¹⁸ For each proxy, we estimate our baseline IV specification separately across the relevant groups and compare the implied semi-elasticities with respect to *Log(Length)*.

Consistent with the contract-enforcement interpretation, the estimates indicate that judicial inefficiency is systematically more damaging in segments characterized by greater contractual complexity and product differentiation. In columns (1)–(2), using the Rauch (1999) classification, the coefficient is sizeable and statistically significant for differentiated products, while it is smaller and imprecisely estimated for non-differentiated goods. A similar pattern emerges in columns (3)–(4) when splitting industries by Nunn (2007)'s contract intensity: the estimate is stronger and significant in high-intensity sectors and weaker in low-intensity sectors. Finally, in columns (5) to (7), the NACE-granularity split shows a clear result: the coefficient is close to zero at the 3-digit level, remains modest at 4-digit, and becomes large and highly significant for firms mapped to 5–6 digit categories, consistent with enforcement being most valuable in more specific product niches.

18. Firms are recorded in the NACE taxonomy at the finest level at which their activity can be unambiguously identified, and this level of detail reflects the firm's degree of specialization: more specialized firms, operating in a narrowly defined line of business, are assigned a more granular code (more digits), whereas firms with a broader activity are classified at a more aggregated level (fewer digits). We classify each firm according to the number of digits of its sector code, grouping firms into three categories: 3 digit, 4 digit, and 5–6 digit.

Table 6. Heterogeneity by contractual complexity and product classification.

Dependent variable	Product differentiation (Rauch 1999)		Contract intensity (Nunn 2007)		NACE product granularity		
	Differentiated products GVC (1)	Non-differentiated products GVC (2)	High intensity GVC (3)	Low intensity GVC (4)	3-digit NACE GVC (5)	4-digit NACE GVC (6)	5-6 digit NACE GVC (7)
Log(Length)	-0.4854* (0.2801)	-0.2061 (0.2948)	-0.4998** (0.2264)	-0.4266 (0.3440)	-0.0996 (0.4664)	-0.1275 (0.1598)	-0.6738*** (0.2177)
Controls	Y	Y	Y	Y	Y	Y	Y
Year fixed effects (FE)	Y	Y	Y	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y	Y	Y	Y
Observations	1,256	424	1,131	1,094	760	1,731	1,934
F instrument	11.63	18.54	16.70	19.39	3.831	23.85	30.06

Notes: The table reports heterogeneity estimates of the effect of trial length on firms' GVC participation across alternative proxies for contractual complexity and product differentiation. Columns (1) and (2) split the sample by Rauch (1999) product classification (differentiated vs. non-differentiated). Columns (3) and (4) split the sample by Nunn (2007)'s sectoral measure of contract intensity (high vs. low). Columns (5)–(7) split firms by the granularity of their NACE product classification (3-digit, 4-digit, and 5-6 digit). All specifications include year and industry fixed effects; the inclusion of baseline controls follows the row "Controls." Trial length is instrumented with the predicted change in trial length induced by the 2013 judicial reform $\times$ the post-reform dummy ($\Delta \times I(t > 2013)$). The table reports the Kleibergen-Paap Wald F -statistic from the first stage. Standard errors, reported in parentheses, are robust to heteroskedasticity. See Table A.3 and Section 3.2 for details on the variables.

*, **, and *** are significant at 10 per cent, 5 per cent, and 1 per cent, respectively.

Taken together, these heterogeneity patterns reinforce the contract-enforcement interpretation of the baseline results: judicial inefficiency is most detrimental in settings characterized by greater contractual complexity, higher scope for disputes, and limited reliance on private governance.

6. Mechanism: external finance

A natural mechanism through which judicial efficiency can shape firms' participation in GVCs is external finance. Cross-border production linkages typically require sizeable upfront and working-capital resources (to finance inputs, production, and inventories in transit) and expose firms to contractual risk along the supply chain. A large literature shows that financing frictions are therefore central for firms' international activity: credit constraints reduce trade participation, while improved access to finance raises both the extensive and intensive margins of importing and exporting (Minetti and Zhu 2011; Manova 2013; Bartoli et al. 2014). In this environment, judicial enforcement matters because it affects the value of collateral and the expected recovery in case of default, lowering lenders' risk and easing access to bank finance (Jappelli et al. 2005; Zazzaro 2005; Degryse et al. 2023); it also supports trade-credit relationships by disciplining late payment and facilitating dispute resolution between buyers and suppliers (Murro and Peruzzi 2022).

We provide evidence on this channel in two steps. First, we show that trial length affects firms' observed reliance on external finance. Table 7 reports 2SLS estimates where the dependent variables are balance-sheet amounts of bank credit, accounts payable (trade credit received), and accounts receivable (trade credit extended). Trial length is instrumented with the predicted change induced by the 2013 court reorganization interacted with a post-reform indicator. The estimates are negative and statistically significant for all three outcomes: the coefficient on *Log(Length)* is -0.8697 for bank credit, -0.5649 for accounts payable, and -0.5088 for accounts receivable. This pattern is consistent with slower courts weakening the enforceability of credit claims and reducing the willingness of lenders and trade partners to extend financing. We stress that these balance-sheet quantities reflect equilibrium outcomes and therefore combine both supply and demand components; nonetheless, the fact that reform-driven variation in judicial efficiency shifts these measures in the expected direction provides suggestive support for the relevance of the finance channel.

Second, we test whether the impact of judicial (in)efficiency on GVC participation is stronger among firms that rely on external finance. Table 8 keeps the baseline 2SLS specification and splits the sample by firms' use of bank credit and trade credit. For bank finance, we distinguish firms with strictly positive

Table 7. Trial length and firms' external finance.

Dependent variable	Bank credit (1)	Accounts payable (2)	Accounts receivable (3)
<i>Log(Length)</i>	-0.8697^{***} (0.3163)	-0.5649^{**} (0.2352)	-0.5088^* (0.2760)
Year fixed effects (FE)	Y	Y	Y
Firm FE	Y	Y	Y
Observations	47,395	47,395	47,395
F instrument	834.7	834.7	834.7

Notes: This table examines how judicial efficiency relates to firms' reliance on external finance. The dependent variables are balance-sheet amounts: Bank credit (credit extended by banks), Accounts payable (trade credit received), and Accounts receivable (trade credit extended). All specifications are 2SLS with firm and year fixed effects. Trial length is instrumented with the predicted change in trial length induced by the 2013 judicial reform $\times$ the post reform dummy ($\Delta \times 1(t > 2013)$). The table also reports the Kleibergen–Paap Wald *F*-statistic from the first stage. Standard errors, reported in parentheses, are robust to heteroskedasticity. See Table A.3 and Section 3.2 for details on the variables.

*, **, and *** are significant at 10 per cent, 5 per cent, and 1 per cent, respectively.

Table 8. Mechanism: external finance.

Dependent variable	Bank credit		Trade credit			
	Bank credit > 0 GVC (1)	Bank credit = 0 GVC (2)	Accounts payable > 0 GVC (3)	Accounts payable = 0 GVC (4)	Accounts receivable > 0 GVC (5)	Accounts receivable = 0 GVC (6)
Log(Length)	-0.4062* (0.2231)	-0.2182 (0.1755)	-0.3804* (0.1955)	-0.2239 (0.2048)	-0.3926* (0.2029)	-0.1994 (0.1911)
Controls	Y	Y	Y	Y	Y	Y
Year fixed effects (FE)	Y	Y	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y	Y	Y
Observations	1,518	2,912	1,943	2,485	1,935	2,491
F instrument	20.20	16.20	24.61	11.61	24.15	12.05

Notes: This table examines whether external finance mediates the effect of judicial efficiency on firms' participation in global value chains (GVCs). Columns (1)–(2) split the sample by use of bank credit (positive bank credit on the balance sheet vs. zero). Columns (3)–(4) split by trade credit received (accounts payable > 0 vs. = 0). Columns (5)–(6) split by trade credit extended (accounts receivable > 0 vs. = 0). All regressions are estimated by 2SLS and include the baseline controls, plus industry and year fixed effects. Trial length is instrumented using the predicted change in trial length induced by the 2013 judicial reform × the post reform dummy. The table also reports the Kleibergen–Paap Wald F-statistic from the first stage. Standard errors, reported in parentheses, are robust to heteroskedasticity. See Table A.3 and Section 3.2 for details on the variables.

*, **, and *** are significant at 10 per cent, 5 per cent, and 1 per cent, respectively.

bank credit on the balance sheet from those reporting zero bank credit. For trade credit, we consider both accounts payable (credit received from suppliers) and accounts receivable (credit extended to customers), since participation in cross-border production networks can require liquidity on the input side and the ability (or necessity) to grant payment terms on the output side.

The results suggest the existence of a finance-related effect. Starting from bank credit, the estimated coefficients are negative in both groups. Among firms with positive bank credit, longer trials significantly reduce the probability of GVC participation, while among firms with zero bank credit, the estimate is still negative but smaller and imprecisely estimated. These patterns are consistent with judicial delays being particularly costly for firms that rely on bank-intermediated funding to sustain cross-border activities. The picture is similar when looking at trade credit. The impact of longer trials is large and significant for firms with positive accounts payable and for firms with positive accounts receivable, while it is statistically insignificant for firms without credit received from suppliers or extended to customers. These differences are consistent with the view that timely enforcement matters most when cross-border production relies on supplier–buyer trade credit and repeated contracting, in which delayed payment and contractual disputes are more likely. First-stage statistics remain comfortably above conventional thresholds across splits: Kleibergen–Paap F-statistics range from 11.61 to 24.61.

Overall, these findings are consistent with a mechanism whereby efficient courts, by improving creditor rights and strengthening contract enforcement, enhance the effectiveness of both bank and trade finance in supporting cross-border production links. When enforcement is slow, the buffer and coordination role of external finance diminishes, so the adverse effect of judicial delays on GVC participation is amplified, especially for firms that rely more on these instruments.

7. Conclusions

This article has examined the role of judicial enforcement in shaping firms' participation in GVCs using the 2013 reform of the Italian court system as a natural experiment. Leveraging novel micro data and a spatially granular identification design, we provide causal evidence that judicial inefficiency significantly reduces the likelihood that firms integrate into GVCs. Even delays of just a few weeks in civil proceedings translate into sizeable declines in participation, underscoring the economic importance of timely contract enforcement.

The analysis highlights important heterogeneities. The adverse effect of judicial delays is present across different roles in cross-border production, emerging both for upstream-oriented and midstream-oriented forms of GVC participation. The effect is also systematically stronger in settings characterized by greater contractual complexity and incomplete contracting, particularly for differentiated products and in sectors with higher contract intensity, and more generally for firms operating in more specific product niches. Finally, the evidence points to external finance as a key mechanism: reform-driven improvements in court efficiency increase firms' equilibrium reliance on both bank and trade credit, and the negative impact of longer trials on GVC participation is concentrated among firms that actually use these sources of financing, consistent with enforcement supporting access to working capital and the credibility of repayment in supplier–buyer relationships.

Taken together, our results demonstrate that judicial efficiency is not only a legal or administrative matter, but a key determinant of firms' competitiveness in the global economy. Subnational disparities in court performance create significant frictions that hinder firms' opportunities to integrate into global production networks, with consequences for regional development and aggregate growth. These results are especially salient in light of recent global shocks, which have heightened contractual risks and increased the value of reliable enforcement. Investments that reduce trial times enhance contractual reliability, sustain trade finance, and bolster the resilience of local supplier ecosystems.

From a policy perspective, improving the efficiency of civil justice, reducing delays, and ensuring more homogeneous enforcement across space can generate substantial economic benefits by lowering

transaction costs, facilitating access to finance, and enabling more firms to seize the gains from globalization. Ultimately, our findings highlight civil justice reform as a concrete lever to strengthen local competitiveness and resilience in an era of evolving GVCs.

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Appendix 1: additional tables and figures

Table A.1. Pre- and post-reform districts.

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

Notes: The table reports all the courts affected by the reform. The first column lists the forty-nine courts that existed before the reform; the second column shows the twenty-three courts that remain after the reform.

Table A.2. Sample representativeness.**Panel A: Sample construction and attrition**

Sample	Observations
(1) MET survey	96,854
(2) + Municipality data	60,123
(3) + Orbis balance-sheet data	52,133
(4) + Bordering cities	35,095
(5) + Affected courts	4,549

Panel B: Comparison of firm characteristics and sectoral composition across samples

	Sample (1)			Sample (2)			Reference sample (5)		
	Mean	Standard deviation	Observations	Mean	Standard deviation	Observations	Mean	Standard deviation	Observations
Firm characteristics									
Age	23.391	15.101	95,377	24.763	15.567	59,966	25.390	16.668	4,537
Micro	0.524	0.499	96,844	0.340	0.474	60,116	0.339	0.473	4,549
Small	0.321	0.467	96,844	0.430	0.495	60,116	0.442	0.497	4,549
Medium	0.124	0.330	96,844	0.184	0.387	60,116	0.172	0.378	4,549
Large	0.031	0.175	96,844	0.047	0.212	60,116	0.047	0.212	4,549
Sectoral composition									
Agriculture and extractive industries	0.009	0.097	96,854	0.015	0.121	60,123	0.018	0.131	4,549
Manufacturing	0.370	0.483	96,854	0.560	0.496	60,123	0.529	0.499	4,549
Utilities and construction	0.039	0.193	96,854	0.060	0.237	60,123	0.060	0.238	4,549
Trade, transport, and storage	0.073	0.260	96,854	0.111	0.315	60,123	0.125	0.330	4,549
Business and financial services	0.109	0.312	96,854	0.169	0.375	60,123	0.179	0.383	4,549
Public and personal services	0.025	0.155	96,854	0.038	0.191	60,123	0.042	0.202	4,549

Notes: The table reports the construction of the estimation sample and how firm characteristics and sectoral composition evolve across key sample restrictions. Panel A shows the number of observations at each step of the sample construction: (1) the full MET survey sample, (2) the subsample for which municipality identifiers are available, (3) the subsample additionally matched to Orbis balance-sheet data, (4) the subsample located in bordering municipalities, and (5) the final estimation sample of bordering municipalities affected by the court reform. Panel B reports mean, standard deviation, and the number of observations for selected firm characteristics and sectoral composition in Samples (1), (2), and (5). Firm-size and sector variables are dummy indicators. Sectors are aggregated from two-digit NACE codes into six broad macro-sectors: Agriculture and extractive industries (A–B), Manufacturing (C), Utilities and construction (D–F), Trade, transport, and storage (G–H), Business and financial services (I–N), and Public and personal services (L, O–S). The sectoral classification is obtained from Orbis balance-sheet data and is missing for firms without available balance-sheet information. As a consequence, the reported sectoral shares are computed over the full number of firms in each sample and sum to the share of firms with non-missing sector information (about 62.5 per cent in Sample (1) and about 95.3 per cent in Samples (2) and (5)), rather than to one.

Table A.3. Variable definitions.

Variable	Description and source
Dependent variables	
GVC	Dummy variable equal to 1 if the firm is engaged in cross-border production linkages, and 0 otherwise. Firms are classified as GVC participants if they both import raw materials or intermediate inputs and export semifinished products or components, or services, excluding exporters of final goods. In addition, we classify as GVC participants firms that export semifinished products or components, even if they do not report importing inputs. (MET survey)
GVC: excluding importers of raw materials	Dummy variable equal to 1 if the firm participates in a global value chain under a more restrictive definition, and 0 otherwise. Firms are classified as GVC participants if they both import intermediate inputs (excluding raw materials) and export goods or services, excluding pure exporters of final goods. In addition, we classify as GVC participants firms that export semifinished products or components, even if they do not report importing inputs. (MET survey)
GVC: long-term foreign partnerships	Dummy variable equal to 1 if the firm participates in a global value chain and reports having long-term relationships with foreign partners, and 0 otherwise. Firms are classified as GVC participants if they meet the baseline definition of cross-border production linkages and report stable or ongoing relationships with foreign companies. This definition captures a more structured form of GVC participation characterized by repeated and durable cross-border interactions. (MET survey)
Independent variables	
Trial length	Average duration of proceedings in the court district. (Italian Ministry of Justice)
Congestion rate	Ratio of pending to incoming cases in the court district. (Italian Ministry of Justice)
Inefficiency rate	Ratio of pending to resolved cases in the court district. (Italian Ministry of Justice)
Instrumental variables	
Length: $\Delta \times 1(t > 2013)$	Predicted change in trial length induced by the 2013 judicial reform, interacted with a post-reform dummy. (Italian Ministry of Justice)
Congestion rate: $\Delta \times 1(t > 2013)$	Predicted change in the congestion rate induced by the 2013 judicial reform, interacted with a post-reform dummy. (Italian Ministry of Justice)
Inefficiency rate: $\Delta \times 1(t > 2013)$	Predicted change in the inefficiency rate induced by the 2013 judicial reform, interacted with a post-reform dummy. (Italian Ministry of Justice)
Control variables	
Age	Number of years since firm inception. (MET survey)
Small	Dummy variable equal to one if the firm has less than fifty employees. (MET survey)
Medium	Dummy variable equal to one if the firm has more than fifty employees and less than 250 employees. (MET survey)
Business group	Dummy variable equal to 1 if the firm belongs to a business group, and 0 otherwise. (MET survey)
# Firms	Number of active firms in the municipality (per 1,000 inhabitants). (ISTAT)
# Bank branches	Number of bank branches in the municipality (per 1,000 inhabitants). (Bank of Italy)
Debt ratio	Total debt over total assets. (Orbis)
Profitability (ROA)	Net income over total assets. (Orbis)
Liquidity ratio	Current assets over current liabilities. (Orbis)
Court suppressed	Dummy variable equal to one if the firm is located in a court suppressed after the 2013 judicial reform, and zero otherwise. (MET survey and Italian Ministry of Justice)

Table A.4. Controls of baseline regressions.

Dependent variable	Full sample	Bordering cities	Bordering cities affected by the reform			
			Year + industry fixed effects		Border × year industry fixed effects	
			OLS GVC (3)	2SLS GVC (4)	2SLS GVC (5)	
Log(Length)	−0.0915*** (0.0048)	−0.0854*** (0.0068)	−0.1062*** (0.0159)	−0.3006** (0.1330)	−0.2730* (0.1604)	
Log(Age)	0.0292*** (0.0022)	0.0269*** (0.0029)	0.0179** (0.0085)	0.0106 (0.0100)	0.0106 (0.0104)	
Small firm	−0.0991*** (0.0085)	−0.0916*** (0.0107)	−0.0272 (0.0300)	−0.0093 (0.0327)	−0.0033 (0.0331)	
Medium firm	0.0087 (0.0089)	0.0068 (0.0113)	0.0486 (0.0317)	0.0556* (0.0328)	0.0662** (0.0330)	
Business group	0.0597*** (0.0048)	0.0635*** (0.0061)	0.1064*** (0.0184)	0.1010*** (0.0192)	0.1080*** (0.0192)	
Number of firms	0.0001 (0.0001)	0.0000 (0.0001)	−0.0005 (0.0005)	−0.0024* (0.0014)	−0.0025 (0.0019)	
Number of bank branches	−0.0060 (0.0077)	0.0243** (0.0106)	0.0314 (0.0285)	−0.0420 (0.0574)	−0.0146 (0.0595)	
First stage: instrumental variable $\Delta \times 1(t > 2013)$				0.5684*** (0.0929)	0.4833*** (0.0850)	
Year fixed effects (FE)	Y	Y	Y	Y	N	
Industry FE	Y	Y	Y	Y	Y	
Border × year FE	N	N	N	N	Y	
Observations	58,327	34,577	4,445	4,433	4,433	
F instrument				37.43	32.33	

Notes: This table reports coefficient estimates for the baseline regressions including the full set of controls. Ordinary least squares results are reported in columns (1)–(3) and 2SLS results in columns (4) and (5). Columns (1)–(4) include year and industry fixed effects. Column (5) includes industry and border × year fixed effects. Trial length is instrumented with the predicted change in trial length induced by the 2013 judicial reform × the post-reform dummy ($\Delta \times 1(t > 2013)$). The first-stage coefficient is shown at the bottom of columns (4) and (5), together with the Kleibergen–Paap Wald F -statistic. Standard errors, reported in parentheses, are robust to heteroskedasticity. See Table A.3 and Section 3.2 for details on the variables. *, **, and *** are significant at 10 per cent, 5 per cent, and 1 per cent, respectively.

Table A.5. Actual and placebo reduced form.

Dependent variable	Actual reduced form GVC (1)	Placebo reduced form GVC (2)
$\Delta \times 1(t > 2013)$	-0.1717** (0.0714)	-0.0572 (0.0473)
Controls	Y	Y
Year fixed effects (FE)	Y	Y
Industry FE	Y	Y
Observations	4,486	58,947

Notes: This table reports the actual and placebo reduced forms. Column (1) reports the reduced form estimated on the identification sample—firms located in bordering municipalities within districts affected by the 2013 court reorganization. Column (2) reports a placebo reduced form estimated on the full sample. Both regressions are ordinary least squares and include the baseline controls, plus industry and year fixed effects. The regressor is the predicted change in trial length induced by the 2013 reorganization interacted with a post-reform indicator ($\Delta \times 1(t > 2013)$). Standard errors, reported in parentheses, are robust to heteroskedasticity. See Table A.3 and Section 3.2 for details on the variables.

*, **, and *** are significant at 10 per cent, 5 per cent, and 1 per cent, respectively.

Table A.6. Baseline estimates with inverse-probability reweighting.

Dependent variable	Bordering cities affected by the reform		
	Year + industry fixed (FE)		Border $\times$ year FE
	OLS GVC (1)	2SLS GVC (2)	2SLS GVC (3)
Log(Length)	-0.1083*** (0.0161)	-0.3026** (0.1343)	-0.2617* (0.1565)
First stage: instrumental variable $\Delta \times 1(t > 2013)$		0.5655*** (0.0932)	0.4971*** (0.0857)
Controls	Y	Y	Y
Year fixed effects (FE)	Y	Y	N
Industry FE	Y	Y	Y
Border $\times$ year FE	N	N	Y
Observations	4,445	4,433	4,433
F instrument		36.81	33.67

Notes: This table reports the effects of trial length on firms' global value chain participation, estimated using OLS in column (1) and 2SLS in columns (2)–(3), on the sample of bordering municipalities affected by the reform. All regressions include control variables. Columns (1) and (2) include year and industry fixed effects. Column (3) includes industry and border $\times$ year fixed effects. Trial length is instrumented with the predicted change in trial length induced by the 2013 judicial reform $\Delta \times 1(t > 2013)$. The first-stage estimate is reported at the bottom of columns (2) and (3), and the table reports the Kleibergen-Paap Wald F-statistic. All specifications are estimated using inverse-probability weights constructed to reweight the final estimation sample to match the covariate distribution of the full MET survey sample (Sample (1) in Table A.2). Standard errors, reported in parentheses, are robust to heteroskedasticity. See Table A.3 and Section 3.2 for details on the variables.

*, **, and *** are significant at 10 per cent, 5 per cent, and 1 per cent, respectively.

Appendix 2: additional evidence on dynamics and aggregate implications of the 2013 reform

This appendix provides evidence on the timing of outcomes around the 2013 court reorganization and on aggregate internationalization patterns in areas more exposed to the reform. Throughout the paper,

causal estimates rely on the border discontinuity IV design; the analyses below are meant to inform interpretation by describing dynamic patterns (including pre-trends) and by offering suggestive evidence on potential aggregate implications.

We first study how firms' GVC participation evolves around 2013 by estimating a dynamic (event-study) specification based on treated-versus-untreated differences over time. Specifically, we interact a set of event-time indicators (years relative to 2013) with an indicator for being exposed to the reform (i.e., belonging to courts affected by the reorganization), and we estimate the resulting coefficients while controlling for the same baseline covariates and fixed effects used in the main analysis. To extend the horizon beyond 2017, we also incorporate the 2019 MET survey wave for this descriptive exercise. Figure B.1 plots the event-time coefficients (with year 2013 as the omitted category). The pre-reform coefficients are

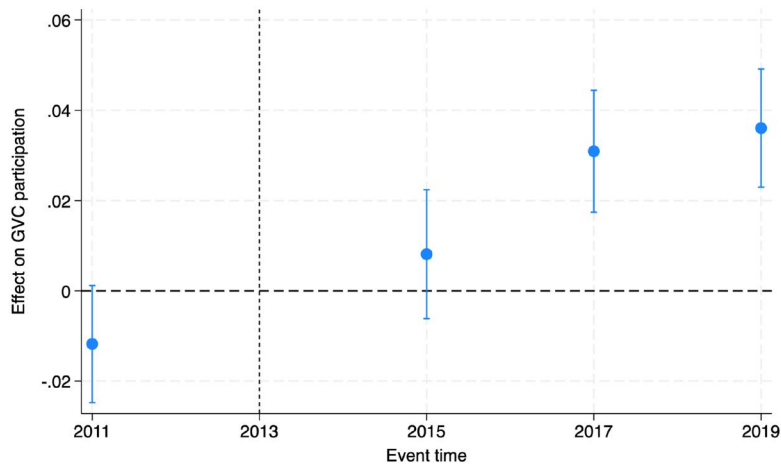


Figure B.1. Event-study around the 2013 court reform: post-reform effects on GVC participation.

Notes: The figure reports coefficients from a dynamic (event-study) specification that estimates year-by-year post-reform effects on firms' GVC participation using MET survey waves 2011, 2013, 2015, 2017, and 2019. Dots denote point estimates and vertical bars report 90 per cent confidence intervals. The dashed horizontal line indicates zero effect. The specification includes the baseline set of controls and fixed effects as in the main analysis.

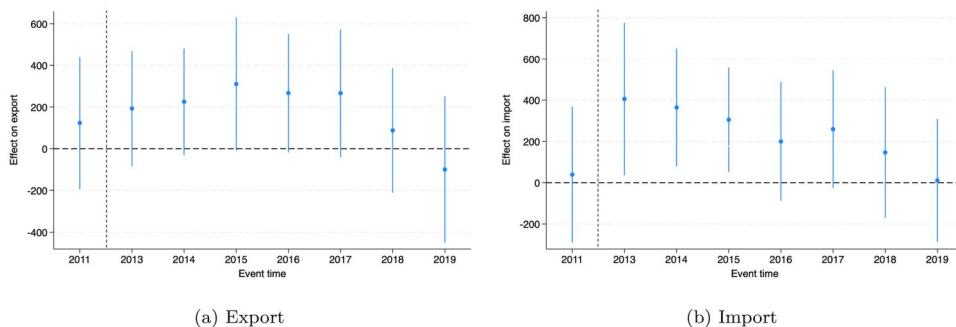


Figure B.2. Reform exposure and provincial trade dynamics

Notes: The figure reports coefficients from a dynamic (event-study) specification estimated on province-year data. Panel (a) uses total provincial exports (in Euros) as the dependent variable; panel (b) uses total provincial imports (in Euros). A province is classified as treated if it contains at least one court affected by the 2013 court reorganization. All regressions include province and year fixed effects and no additional controls. Dots denote point estimates and vertical bars report 90 per cent confidence intervals.

small and statistically indistinguishable from zero, while the estimates shift in the post-reform years and remain economically meaningful thereafter. Overall, the event-study profile is consistent with a change in outcomes occurring after the reform, rather than with diverging pre-existing trends.

We then provide complementary descriptive evidence at a more aggregate level by looking at international trade volumes. Using province-level data drawn from the ISTAT database, we replicate the same event-time approach with two outcomes: the value of total exports and the value of total imports. A province is classified as treated if it contains at least one court affected by the 2013 reorganization. The specification includes province and year fixed effects, and no additional controls. Figure B.2 reports the event-study coefficients for export (Panel A) and imports (Panel B), normalizing the omitted category to the reference year 2012. Overall, the point estimates suggest that treated provinces experienced, if anything, a relative increase in trade volumes after the reform. Export coefficients are consistently positive, although they are never statistically distinguishable from zero. Import coefficients are positive and statistically significant in the first post-reform years (2013–15) and remain positive thereafter, albeit estimated less precisely in the later years.

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