



CITP
CENTRE FOR
INCLUSIVE
TRADE POLICY

The Uneven Reach of IP Court Rulings: Spillovers through Technology and Product Networks

Richard Kneller, Joseph Mai, and Min Zhu

October 2026

Centre for Inclusive Trade Policy
Working Paper No.045



Economic
and Social
Research Council

Centre for Inclusive Trade Policy
<https://citp.ac.uk/> info@citp.ac.uk

Established in 2022, the Centre for Inclusive Trade Policy (CITP) is the first research centre dedicated to trade policy to be funded by the Economic and Social Research Council. As a centre of excellence for innovative research on trade policy and its inclusiveness, we aim to equip the UK with the capability to formulate and implement a trade policy tailored to the needs of the whole of the UK, while recognising the importance of the multilateral trading system and the UK's role within it. The CITP is funded by the Economic and Social Research Council [grant number ES/W002434/1]

This Working Paper is issued under the auspices of the Centre's research programme. Any opinions expressed here are those of the author(s) and not those of the Centre for Inclusive Trade Policy. Research disseminated by CITP may include views on policy, but the Centre itself takes no institutional policy positions.

These Working Papers often represent preliminary or incomplete work, circulated to encourage discussion and comment. Citation and use of such a paper should take account of its provisional character. The author(s) asserts their moral right to be identified as the author of this publication. For online use, we ask readers to link to the webpage for the resource on the Centre's website.

Abstract

A court ruling resolves a dispute between two firms, but its economic effects can reach firms that were never in court - and reach them differently depending on how they are connected to the litigants. Combining Chinese litigation records with production, patent, and customs data for 2000–2009, we construct each non-litigant's annual exposure from its pre-determined proximity to litigants in technology and product space. Greater technology-space exposure reduces total sales, with the decline concentrated in domestic sales; product-market exposure instead raises total sales through exports. Both market-specific responses emerge at judgment and attenuate by the following year. Export gains are larger in foreign product markets where market shares are more concentrated among Chinese exporters and occur without corresponding innovation or quality upgrading. Although a ruling directly binds only the parties to the dispute, its economic incidence is distributed through pre-existing firm networks.

©: Richard Kneller, Joseph Mai, and Min Zhu

Suggested citation

R, Kneller; J, Mai; M, Zhu (2026) The Uneven Reach of IP Court Rulings: Spillovers through Technology and Product Networks, Centre for Inclusive Trade Policy, Working Paper 045

Non-Technical Summary

Court rulings resolve disputes between the parties to a case. In intellectual property (IP) disputes, a ruling may also affect firms outside the case by changing control over contested rights and the litigants' competitive positions. This paper examines whether these wider effects occur and, if so, how the affected firms respond.

To answer this question, we combine Chinese court records with data on firms' sales, patents, and international trade from 2000 to 2009. For each firm, we measure two connections to the litigants using patent portfolios and export records from the beginning of the sample period. Technological similarity reflects overlap in the fields covered by their patents, while product similarity reflects overlap in the goods they export. These connections need not coincide. For example, a network equipment maker and a smartphone maker may patent in the same wireless-communication fields while selling different products. We then examine how firms' sales change when a court decides a case involving a related firm.

We find that firms whose patent portfolios resemble those of the litigants experience a fall in total sales. The decline is concentrated in domestic sales, with no detectable change in exports. Firms selling similar products instead see their total sales rise through increased exports, with no detectable change in domestic sales. A ruling can therefore have different consequences for firms outside the dispute, depending on how they are connected to the litigants.

Three checks help distinguish the effects of the rulings from changes that similar firms might have experienced anyway. The domestic sales decline and export increase appear in the year of the ruling, with little evidence of either beforehand, and become smaller the following year. We also repeat the analysis as if other, randomly selected firms had been involved in the court cases. This comparison suggests that the sales changes depend on connections to the firms that actually went to court. Estimates using differences in how often judge panels rule for plaintiffs also show domestic sales losses among technologically related firms and export gains among firms selling similar products.

To understand these different responses, we examine changes in firms' imports, research spending and export destinations. For technologically connected firms, lower domestic sales are accompanied by reduced imports of production inputs and equipment, with no detectable increase in research and development. This pattern suggests a broader contraction in business activity. By contrast, firms selling similar products expand their exports and change where they sell. Gains are larger in foreign product markets where sales are more concentrated among Chinese exporters, and these destinations account for a larger share of firms' exports. We find no accompanying improvement in measured product quality. Together with the short duration of the gains, this is consistent with firms making short-run adjustments to where they sell.

A court ruling can have economic consequences beyond the firms involved in the dispute, including in foreign markets where it has no direct legal force. Our findings show that these consequences differ according to firms' technological and product connections to the litigants. Looking only at the firms in court can miss these effects, while industry averages may obscure the different responses of connected firms. Similar effects may arise from other court rulings or regulatory decisions that change firms' rights or market positions.

The Uneven Reach of IP Court Rulings: Spillovers through Technology and Product Networks^{*}

Richard Kneller^{a,*}, Joseph Mai^b, Min Zhu^c

^a*School of Economics, University of Nottingham, GEP, CITP, CESIf*

^b*College of Business, Shanghai University of Finance and Economics*

^c*School of Economics, University of Nottingham, GEP, CITP*

Abstract

A court ruling resolves a dispute between two firms, but its economic effects can reach firms that were never in court - and reach them differently depending on how they are connected to the litigants. Combining Chinese litigation records with production, patent, and customs data for 2000–2009, we construct each non-litigant’s annual exposure from its pre-determined proximity to litigants in technology and product space. Greater technology-space exposure reduces total sales, with the decline concentrated in domestic sales; product-market exposure instead raises total sales through exports. Both market-specific responses emerge at judgment and attenuate by the following year. Export gains are larger in foreign product markets where market shares are more concentrated among Chinese exporters and occur without corresponding innovation or quality upgrading. Although a ruling directly binds only the parties to the dispute, its economic incidence is distributed through pre-existing firm networks.

JEL codes: O34, F14, K41, L25

Keywords: IP litigation, Spillovers, Technology proximity, Product similarity

^{*}Richard Kneller acknowledges the financial support from the Economic and Social Research Council Centre for Inclusive Trade Policy (CITP) [grant number ES/W002434/1]. Min Zhu acknowledges the financial support from the ESRC-funded Centre for Inclusive Trade Policy (CITP). We would like to thank Arnaud Costinot, Andrés Rodríguez-Clare, and all seminar participants at the University of Nottingham and CITP for their invaluable comments. All errors are our own.

^{*}Corresponding author. *Email:* richard.kneller@nottingham.ac.uk, *Address:* School of Economics, Sir Clive Granger Building, NG7 2RD, Nottingham, United Kingdom

Email addresses: josephm@mail.shufe.edu.cn (Joseph Mai), min.zhu@nottingham.ac.uk (Min Zhu)

1. Introduction

Intellectual property (IP) court rulings determine the scope and enforceability of contested rights, including an opposing party’s continued use of disputed IP, and in doing so can shift the litigants’ competitive positions.¹ Those positions matter beyond the firms directly involved in the dispute: they also change the competitive conditions facing firms that never appear in court. Existing research on IP enforcement shows that it shapes innovation incentives, competition, and firm performance (Galasso and Schankerman, 2015; Mezzanotti, 2021; Acikalin et al., 2026), but we know much less about the wider economic incidence of individual rulings. Which non-litigants are affected, how a ruling reaches them, and whether the consequences appear in domestic or export markets remain open questions.

A ruling’s reach is uneven. Which firms are affected, and how strongly, depends on their links to the litigants before the ruling. Technology-space proximity captures overlap in firms’ patent portfolios, identifying firms active in related technological fields even when they do not sell similar products. Product-market proximity instead captures overlap in firms’ export product baskets (Bloom et al., 2013).² A ruling does not directly bind either type of non-litigant. For technologically proximate firms, the resolution of disputed IP rights can alter the conditions under which firms compete in related technological fields (Farrell and Shapiro, 2008; Shapiro, 2000). For product-proximate firms, the resulting change in the litigants’ competitive positions can reshape competition among sellers of similar goods (Lee et al., 2025). Technology-space proximity and product-market proximity need not identify the same firms. Consistent with this distinction, the two measures are almost uncorrelated in our data, suggesting that they capture distinct dimensions of firm relatedness.

That unevenness may extend to where the effects surface. One plausible expectation is that effects transmitted through technology-space links are most visible in the domestic market, since a ruling has direct legal force only within the adjudicating jurisdiction. This is not a foregone conclusion, however, since technology proximity could also shape firms’ investment or input decisions in ways that carry over into export production. Whether product-market links affect firms’ export activity is similarly unclear ex ante. If they do, that need not mean the ruling carries legal force abroad. A ruling that changes a litigant’s competitive position could instead lead product-proximate non-litigants to reallocate exports

¹Patent rights are effective only if firms expect them to be enforced when needed; in practice, much enforcement occurs outside formal litigation (Weatherall and Webster, 2014). Litigation costs also affect firms’ willingness to enforce and the private value of patent rights (Lanjouw and Lerner, 2000).

²For example, a network-equipment producer and a smartphone producer may hold patents in the same wireless-communications classes while selling different products. Conversely, two smartphone exporters may sell similar products while holding patents in different technological fields.

across destinations, so an export response would reflect exporters adjusting where they sell rather than a direct effect of the ruling in the destination market itself.

To examine these questions, we link IP cases, including litigant identities and case outcomes, to firm-level production, patent, and customs data in China. We then build firm-year measures of each non-litigant’s exposure to court rulings based on its pre-determined proximity to litigants in technology and product space, following Bloom et al. (2013, 2016).³ Exposure therefore varies across firms according to their proximity to litigants and over time as different cases are decided. Because these links are constructed at the firm-pair level from separate patent and customs records, we can distinguish technology-space from product-market spillovers without relying on broad industry classifications.

China provides a useful setting for identifying these general mechanisms for two main reasons. First, following WTO accession, the government strengthened IP enforcement through revisions to patent law, an expansion of judicial capacity, and the development of specialized IP tribunals within the existing court system (Maskus et al., 2005; Ang et al., 2014). These reforms coincided with a sharp rise in litigation as firms moved away from imitation and incremental innovation and began asserting ownership of technologies, creating variation in the timing, technological content, and market scope of IP disputes. Second, China’s IP courts operate under a civil law framework, so judicial decisions carry no formal precedential weight for subsequent cases adjudicated by different courts. Non-litigating firms therefore acquire no direct legal rights or obligations from cases to which they are not a party, allowing us to distinguish economic responses to case-specific adjudication from direct legal effects through formal precedent. Much of the empirical evidence on IP litigation comes from the U.S. and other common law settings, where precedent can provide an additional channel through which rulings affect third parties. China therefore offers a complementary setting: the spillovers we document arise without formal precedent and may extend to other civil law jurisdictions where rulings bind only the parties.

The central finding is that the effects of adjudication depend on how firms are connected to the litigants. The same court ruling generates very different responses when transmitted through technology-space links rather than product-market links. Technology-space exposure lowers non-litigants’ total sales, whereas product-market exposure raises them. Splitting sales by market sharpens this contrast: technology-space exposure lowers domestic sales, with no significant effect on exports, while product-market exposure instead raises export sales, with no corresponding domestic effect. Court rulings therefore have economic effects beyond the

³Technology proximity is measured from patent portfolios and product proximity from export baskets in the baseline period, so that exposure is pre-determined and cannot be shaped by the ruling itself.

litigants, but those effects do not diffuse uniformly: pre-existing firm networks shape both which non-litigants are affected and whether the response appears in domestic or export markets.

These patterns are statistically significant for exposure to litigation winners, but not for losers. Among positively exposed firm-years, moving from the 25th to the 75th percentile of technology-space exposure implies an 8.6 percent decline in domestic sales, while the corresponding increase in product-market exposure implies a 1.2 percent rise in exports.

To address the concern that firms close in product or technology space may comove even without litigation, we use a permutation exercise. The observed statistics lie in the tails of the placebo distribution generated by randomly reassigning litigant status across firms: the positive product-market effect and the negative technology-space effect are both unlikely to arise under random reassignment. To alleviate the remaining concern that litigation outcomes may themselves be endogenous, we instrument plaintiff victory at the case level using judge-panel leniency and predicted win probabilities from pre-determined case characteristics. Both IV estimates reproduce the baseline sign pattern. Moreover, the asymmetry remains robust to alternative similarity measures, timing conventions, case definitions, court levels, policy support, local litigation intensity, and other firm-level determinants of sales. Dynamic estimates show little divergence before judgment. The market-specific responses emerge when the court rules and attenuate by the following year. This timing suggests that the effects reflect short-run adjustments triggered by adjudication rather than persistent differences between connected firms.

The two spillovers affect different markets. Technology-space exposure is associated with falling domestic sales and lower imports, without corresponding increases in innovation or input use, consistent with contraction rather than capability upgrading. Product-market exposure instead raises exports, with larger gains in destination-product markets where market shares are more concentrated among Chinese exporters. Because this result is identified from differences in a firm's sales across destinations within the same firm-year, aggregate export expansion is accompanied by a shift in destination composition rather than a uniform rise across markets. Export gains also do not vary systematically with destination income or IP protection, weakening demand-pull and foreign-enforcement explanations. The responses are therefore best understood as short-run, market-specific adjustments rather than a common process of capability accumulation.

The paper contributes to three main strands of the literature. First, it builds on the literature on IP litigation and patent enforcement. Much of this work studies firms drawn directly into patent disputes, including how litigation affects patenting incentives and the private costs of legal conflict (Lerner, 1995; Bessen and Meurer, 2012, 2008). Lanjouw and

Schankerman (2001) show that litigation risk is systematically related to patent characteristics and rivalry in technology space, while Lanjouw and Schankerman (2004) document that smaller firms are less able to enforce their rights. Other work uses broader shifts in patent enforcement to study innovation responses to stronger or weaker protection (Acikalin et al., 2026; Mezzanotti, 2021).⁴ Recent work further shows that adjudication can vary with litigant nationality and geopolitical conditions. Mai and Stoyanov (2019) and Choudhury et al. (2025) document home-court advantages in IP disputes, while Bo and Liu (2026) show that trade conflict can reduce foreign plaintiffs’ success in commercial litigation.

A smaller body of research examines how patent disputes affect firms other than the patent owner or focal plaintiff. Galasso and Schankerman (2015) show that invalidating a patent raises follow-on innovation by other firms, Awate and Makhija (2022) show that litigation can transmit proprietary knowledge to accused firms and raise their subsequent innovation, and Cohen et al. (2019) find that litigation by non-practicing entities depresses innovation at targeted firms. Closest to our setting, Mai et al. (2024) use the same Chinese litigation data to study innovation effects for litigants and industry-level spillovers to non-litigants. Relatedly, Lee et al. (2025) use China’s National Intellectual Property Strategy as a foreign regulatory shock to U.S. firms and show that heightened inter-firm patent litigation risk reduces patenting and narrows innovation, especially among firms exposed through product-market rivals. Together, these studies show that patent disputes and enforcement shocks can affect firms beyond the focal patent owner, but the measured responses are mainly innovation or patenting rather than sales and trade.

Although a ruling directly binds only the parties to the dispute, its economic incidence is distributed through pre-existing firm networks. We extend this literature in three ways: we shift the unit of analysis from litigants to non-litigants, broaden the outcomes to sales and international trade, and map the timing and outcomes of adjudication into non-litigant exposure through firm-specific networks rather than broad industry groupings. Empirically, we construct firm-year exposure by interacting the timing and outcomes of court rulings with non-litigants’ pre-determined similarity to litigants and then aggregating these links across litigants. Similarity is measured from patent portfolios and export product baskets drawn from separate administrative systems. This design allows us to distinguish technology-space from product-market spillovers and to identify the markets in which they appear. Evaluations

⁴There is a growing literature on law enforcement spillovers in other settings. Rincke and Traxler (2011) study households’ decisions to purchase TV licenses when enforcement occurs in their local neighborhood, while Alm et al. (2009) examine tax compliance in a lab setting. Related work studies behavioral responses to environmental, tax, financial, and anti-corruption enforcement (Evans et al., 2018; Choi et al., 2024; Galletta, 2017; Lopez-Luzuriaga and Scartascini, 2019).

centered on litigants or broad industry averages may therefore miss effects on connected non-litigants.

Second, it draws on evidence showing how firms reallocate exports in response to changing competitive conditions. A central theme in this literature is that firms adjust unevenly under competitive pressure rather than responding uniformly across all activities. [Bernard et al. \(2006\)](#), [Greenaway et al. \(2008\)](#), and [Bloom et al. \(2016\)](#) show that stronger competition induces uneven firm responses, including reallocation and exit, while [Mayer et al. \(2014\)](#) show that tougher competition across destinations reshapes firms’ export product mix. [Almunia et al. \(2021\)](#) further show that firms redirect sales toward export markets when domestic demand weakens, and [Fontagné and Orefice \(2018\)](#) show that multi-destination exporters respond to technical barriers in one market by redirecting exports toward unaffected destinations. Unlike these settings, the shock we study is a court ruling that changes litigants’ relative competitive positions and can spill over to firms connected to them before judgment. Following [Bloom et al. \(2013\)](#), we distinguish proximity in technology space from rivalry in product markets because the two need not connect the same firms or affect the same margins of firm performance. We contribute to this literature by identifying case-level IP adjudication as a new source of short-run export adjustment among non-litigants connected to litigants through pre-existing product-market links. Unlike the upgrading-based response emphasized by [Amiti and Khandelwal \(2013\)](#), this adjustment occurs without corresponding evidence of innovation or quality upgrading.

Third, it broadens the literature on TRIPS-era IP protection and institutional change in developing economies. Cross-country evidence on TRIPS adoption finds that stronger IP protection encouraged technology transfer and exports but had limited effects on domestic innovation ([Branstetter et al., 2006](#); [Chen and Puttitanun, 2005](#); [Ivus, 2010](#); [Cockburn et al., 2016](#); [Maskus, 2015](#)).⁵ Within-country studies of China’s reforms point to stronger firm-level responses. [Ang et al. \(2014\)](#) show that stronger provincial IP enforcement improves firms’ access to external finance and raises R&D, patenting, and new-product sales. Using SOE privatizations, [Fang et al. \(2017\)](#) find that stronger local IPR protection raises innovation, especially among private-sector firms, which respond more strongly than SOEs. [Tong et al. \(2026\)](#) further show that stronger protection can shift the direction of innovation, raising patenting among state-owned enterprises while tilting it toward more familiar, less exploratory technologies. Across these studies, the focus is on how the strength of IP institutions shapes innovation and technology transfer. We complement this literature by shifting

⁵A theoretical literature has developed alongside its empirical counterpart, using North–South models of international trade based on either product life cycles or quality ladders to study the effects of greater IP protection by Southern imitator countries; see, for example, [Maskus and Penubarti \(1995\)](#).

attention from changes in the average strength of IP institutions to the heterogeneous effects of case-level adjudication on firms outside the courtroom.

The remainder of this paper is organized as follows. Section 2 describes the data and empirical specification. Section 3 presents the main empirical results. Section 4 addresses identification concerns through permutation tests, instrumental variables, timing evidence, and additional checks. Section 5 presents evidence on firms’ adjustment channels. Section 6 concludes.

2. Data and Empirical Framework

This section describes the data and the empirical framework. We first introduce the data sources and explain how we match them at the firm level. We then present the empirical specification and explain how we construct firm-level exposure to IP court rulings.

2.1. Data

Our analysis combines four complementary data sources: (1) IP litigation records from the Legal Information Center of Peking University; (2) firm performance information from the Annual Survey of Industrial Firms (ASIF); (3) firm-level trade transaction records from the Chinese Customs Trade Statistics (CCTS); and (4) patent application and grant data from the China National Intellectual Property Administration (CNIPA). Together, these data allow us to observe who litigates, when courts rule, who wins, how firms are linked in product and technology space, and how non-litigants respond in their sales.

IP litigation records. Litigation data are obtained from the Legal Information Center of Peking University, a repository of published judicial proceedings across Chinese courts. We restrict the analysis to IP litigation between corporate entities adjudicated from 2000 to 2009, covering disputes involving patents, trademarks, copyrights, domain names, trade secrets, and other IP infringements.

We impose two restrictions when constructing the final sample. We retain only cases in which both the plaintiff and defendant are corporate entities, and omit cases without a conclusive judicial decision, including those adjourned indefinitely or withdrawn following an out-of-court settlement. The resulting dataset comprises 4,632 resolved IP cases. Figure A1 plots the annual number of cases, which rises markedly over the sample period, consistent with the broader expansion of IP enforcement in China during the 2000s.

For each case, we extract from the court documents the litigating firms’ identities and registered addresses, the adjudicating court, the type of IP involved, the number of IP rights at issue, monetary claims, and the court’s ruling on each of the plaintiff’s claims. Following Mai and Stoyanov (2019), we employ two alternative measures of litigation outcomes: (i)

a plaintiff victory is defined by success in at least one claim, and (ii) a stricter definition classifies the plaintiff as victorious only if at least half of its claims are upheld. The two measures are highly consistent (correlation = 0.95). These records therefore provide the timing, parties, and resolution of the judicial shocks that enter our spillover measures.

Firm production and financial information. Firm-level production and financial data come from the Annual Survey of Industrial Firms (ASIF), which covers Chinese manufacturing firms with annual sales above RMB 5 million from 1998 to 2009. These data provide our main firm-level performance outcomes, including total sales and domestic sales, measured as total sales net of exports. We also draw information on firms' location, sector, ownership, and R&D expenditure from ASIF.⁶

Firm trade transactions. Firm-level trade data come from the Chinese Customs Trade Statistics (CCTS), which provide a census of import and export transactions from 2000 to 2009. They report the value and quantity of all shipments at the firm-HS8-country level.⁷ The data also identify the customs regime under which each transaction was conducted, distinguishing between ordinary trade and processing trade.⁸ These data provide both our export outcomes and the HS8 export-basket vectors used to construct product-market similarity between firms. Our baseline analysis retains ordinary trade and excludes processing trade transactions because processing exporters operate under distinct contractual arrangements and face different production and export incentives (Feenstra and Hanson, 2005).

Patent data. Firm-level patent records come from the China National Intellectual Property Administration (CNIPA). The data cover the universe of patent applications filed in China and report applicant and inventor names, addresses, filing and grant dates, and patent type (invention, utility model, or design). Each application also carries International Patent Classification (IPC) codes. We use applications filed from 1995 to 2000 to construct firms' baseline technology portfolios and measure technology-space similarity.

⁶To ensure data quality and consistency, we apply the methodology outlined by Wang and Yu (2013) to eliminate the following observations: (a) firms operating in non-manufacturing industries; (b) observations with negative values for output, sales, exports, capital, total assets, total fixed assets, wages, or intermediate inputs; (c) observations with total assets less than total fixed assets or total liquid assets, or with total sales less than exports; and (d) observations with fewer than five employees.

⁷Export values are reported on a free-on-board (FOB) basis and import values on a cost, insurance, and freight (CIF) basis; both are denominated in US dollars. Quantities are reported in units including kilograms, pieces, meters, liters, and carats.

⁸Under ordinary trade, firms purchase inputs from domestic or foreign markets and retain control over production and sales decisions. Under processing trade, a substantial share of inputs is sourced from abroad and the resulting output must be exported.

Sample construction. We construct the final firm-level panel in three steps. First, we merge ASIF and CCTS to combine firm characteristics with detailed trade activity from 2000 to 2009. Second, because CNIPA records lack firm identifiers, we link patent applicants to the merged ASIF–CCTS data using a multi-stage procedure that standardizes applicant names and addresses and manually verifies ambiguous matches. This match allows us to recover firms’ technology portfolios. Third, we match the IP court records to the firm panel using litigant names and registered locations, identifying the parties to each case together with filing years, judgment years, and case outcomes. To make sample eligibility pre-determined, we restrict the panel to firms observed in the baseline year, 2000. Because our objective is to estimate spillovers to firms that are not party to the dispute, we further exclude all firms appearing as plaintiffs or defendants at any point during the sample period. This yields an unbalanced panel of 14,510 non-litigating firms and 100,840 firm-year observations from 2000 to 2009.

2.2. Empirical Specification

Using this panel, we estimate the following specification to examine how IP court judgments affect the sales of non-litigating firms:

$$\text{Outcome}_{ft} = \alpha_1 \text{SPILLTECH}_{ft} + \alpha_2 \text{SPILLHS8}_{ft} + \delta_f + \delta_s + \delta_{ct} + \varepsilon_{ft}, \quad (1)$$

where Outcome_{ft} denotes the logarithm of total sales, domestic sales, or export sales of firm f in year t . The specification absorbs firm (δ_f), two-digit-sector (δ_s), and city-year (δ_{ct}) fixed effects. Firm fixed effects remove time-invariant differences across firms, while sector fixed effects account for changes in firms’ reported sector affiliation over time. City-year fixed effects are particularly important because IP enforcement and economic conditions vary substantially across Chinese cities and over time (Ang et al., 2014). Standard errors are clustered at the firm level to allow for serial correlation within firms.

The main regressors of interest, SPILLTECH_{ft} and SPILLHS8_{ft} , measure firm-year exposure to resolved IP cases based on firms’ pre-determined positions in technology and product space. We measure technology proximity ($\text{TECH}_{ff'}$) among firms with patent portfolios, using patent share vectors across 605 four-digit IPC classes constructed from applications filed during 1995–2000. Similarly, we measure product-market proximity ($\text{HS8}_{ff'}$) using each firm’s HS8 export-share vector in the baseline year, 2000.⁹ The two proximity matrices come

⁹We construct product-market proximity at the HS8 level because it is the finest level of product classification available in the Chinese Customs transaction data. This tariff-line level of granularity allows us to capture highly disaggregated overlap in firms’ export product mixes that is closely related to product substitutability in international markets.

from independent administrative classification systems: four-digit IPC patent classes and HS8 customs products. Because both matrices are pre-determined, firms' network positions cannot be mechanically shaped by contemporaneous litigation outcomes, firm performance shocks, or strategic patenting. Proximity is measured as the cosine similarity between firm pairs, equivalently the uncentered correlation of their normalized share vectors, following Jaffe (1986) and Bloom et al. (2013).¹⁰ Figure A2 plots the two proximity measures across firm pairs. Their correlation is only 0.028, showing that firms close in product space need not also be close in technology space. This weak relationship supports treating the two networks as distinct sources of exposure.

These fixed network positions are then interacted with case timing. The variable $D_{f't}^{\text{litigation}}$ is an indicator equal to one if firm f' was involved as either plaintiff or defendant in an IP case that received a court judgment in year t , and zero otherwise. It captures the arrival of a resolved case regardless of whether the plaintiff wins or loses. As an indicator, it treats a litigant with several cases resolved in the same year as a single exposure event, so the measure reflects the arrival of resolved litigation for a connected firm rather than the number of individual rulings. The spillover exposure of each non-litigating firm f at time t is then constructed by aggregating the resulting pair-level links across firms f' :

$$\text{SPILLTECH}_{ft} = \sum_{f'} \text{TECH}_{ff'} \cdot D_{f't}^{\text{litigation}} \quad \text{SPILLHS8}_{ft} = \sum_{f'} \text{HS8}_{ff'} \cdot D_{f't}^{\text{litigation}}.$$

Exposure is therefore larger when firms more closely related to f in the relevant network receive judgments in year t . The baseline construction uses the court's judgment year as the event date; we test robustness using the filing year. In Section 3.1, we further distinguish winner- and loser-based exposure. These replace $D_{f't}^{\text{litigation}}$ with outcome-specific indicators: $D_{f't}^{\text{win}} = 1$ if litigant f' was on the prevailing side of at least one case resolved in year t , and $D_{f't}^{\text{lose}} = 1$ if it was on the losing side of at least one. Because a litigant may prevail in one case and lose another in the same year, the two indicators are not mutually exclusive, and winner- and loser-based exposure need not sum to total exposure.

This two-part exposure construction is central to interpreting α_1 and α_2 . Following Bloom et al. (2013), the two measures capture different links among firms: technology proximity captures closeness in knowledge space, whereas HS8 overlap captures competition among sellers of similar goods. A court ruling resolves contested IP rights and may thereby alter the litigants' relative competitive positions. Under China's civil law system, however, it creates no binding precedent and grants no direct legal rights or obligations to non-litigants.

¹⁰Full construction details are provided in Sections Appendix A and Appendix B.

Any effect on non-litigants must therefore be indirect, operating as changes in the litigants' competitive positions propagate through the pre-determined networks. Technology-space exposure may reflect changes in domestic competitive and appropriability conditions among firms using related knowledge, whereas product-market exposure may reflect export-market adjustment among sellers of similar goods. We leave the signs and market-specific effects of both channels to the data.

Identification comes from the interaction of firms' pre-determined proximity to litigants and variation over time in which litigants receive court rulings. As different litigants receive judgments, a non-litigant's exposure changes while its positions in the two networks remain fixed. With firm and city-year fixed effects, α_1 and α_2 are identified from these within-firm changes in exposure and comparisons among firms in the same city-year, where local legal and economic conditions, including aggregate litigation intensity, are held constant.

2.3. Summary statistics

Table 1 reports descriptive statistics for the main estimation sample. The panel contains 14,510 non-litigating firms and 100,840 firm-year observations from 2000 to 2009. Firms are sizable on average, with mean log total sales of 10.94, and exhibit substantial heterogeneity in both domestic and export activity. Trade activity varies substantially across firms. For both exports and imports, the table reports sales per product and per partner country, together with the number of products and countries, capturing intensive and extensive trade margins.

The two baseline exposure measures are highly skewed. Because rulings are discrete and weighted by firms' pre-determined proximity to litigants, both measures remain at or near zero through the 75th percentile and rise only for firm-years with stronger links to litigants receiving judgments. The identifying variation therefore comes disproportionately from the upper tail of the exposure distribution: firms whose close product-market or technology-space peers receive court rulings in a given year. The table also reports a coarser HS4-based measure (SPILLHS4_{ft}) and the winner- and loser-based exposure measures used in the main results.

Although exposure is zero in most firm-years, 54.1 percent of firms are exposed through at least one network during the sample period. Firm-years with positive technology-space exposure account for 13.2 percent of domestic sales, while those with positive product-market exposure account for 38.2 percent of exports. Thus, the exposure measures are sparse in frequency but cover an economically meaningful share of firm activity.

2.4. Benchmark Evidence on Litigants

Before turning to spillover effects on non-litigants, we establish whether litigation outcomes are associated with meaningful differences in the performance of litigating firms them-

Table 1: Summary Statistics

Variable	N	Mean	Std. Dev.	P25	P75
<i>Firm Outcomes (Log)</i>					
Total Sales	100,840	10.942	1.460	9.851	11.867
Domestic Sales	82,181	10.072	2.238	8.795	11.522
Exports	80,544	13.134	2.330	11.893	14.716
Exports per Product	80,544	11.690	2.066	10.567	13.058
Exports per Country	80,544	11.638	1.913	10.636	12.894
# Exported HS8 Products	80,544	1.445	1.058	0.693	2.197
# Exported Destinations	80,544	1.496	1.109	0.693	2.398
Import Sales	48,136	11.130	3.441	8.534	13.733
Import per Product	48,136	9.186	2.652	7.279	11.082
Import per Country	48,136	10.211	2.883	8.127	12.384
# Imported HS8 Products	48,136	1.944	1.496	0.693	3.091
# Import Source Countries	48,136	0.919	0.895	0.000	1.609
<i>Spillover Exposure (total)</i>					
SPILLTECH _{ft}	100,840	0.031	0.236	0.000	0.000
SPILLHS8 _{ft}	100,840	0.041	0.174	0.000	0.000
SPILLHS4 _{ft}	100,840	0.150	0.382	0.000	0.033
<i>Spillover Exposure (by litigation outcome)</i>					
SPILLTECH _{ft} ^{win}	100,840	0.026	0.212	0.000	0.000
SPILLHS8 _{ft} ^{win}	100,840	0.033	0.156	0.000	0.000
SPILLTECH _{ft} ^{lose}	100,840	0.005	0.055	0.000	0.000
SPILLHS8 _{ft} ^{lose}	100,840	0.010	0.078	0.000	0.000

Notes: This table reports summary statistics for the main estimation sample covering the period 2000–2009. The sample excludes all firms that appeared as plaintiffs or defendants in IP litigation during this period. Total Sales and Domestic Sales are sourced from ASIF and reported in 1,000 RMB; export and import sales measures are sourced from CCTS and reported in US dollars. The levels of the ASIF and CCTS sales variables are therefore not directly comparable. The domestic, export, and import samples are restricted to firms with positive sales in the corresponding market.

selves. Because both litigation participation and case outcomes may be endogenous to firm performance, we treat these estimates as descriptive rather than causal.

Appendix Table A2 reports associations between litigants' own sales and indicators for winning or losing a case resolved in year t . The broader litigant sample includes firms matched to the firm data at any point during 2000–2009, without requiring them to be observed in the baseline year. The winning-case indicator is positive for total, domestic, and export sales, with each estimate significant at the 10 percent level. The losing-case indicator is positive at the 10 percent level for total sales, while its domestic and export sales coefficients are statistically insignificant. They serve as a benchmark for the spillover analysis that follows.

3. Main Results

This section presents the effects of IP court rulings on non-litigating firms. We begin with total sales and then separate domestic and export responses.

3.1. *Asymmetric Spillover Effects*

Table 2 presents the baseline estimates. Column (1) shows that technology-space and product-market exposure have opposite effects on total sales: firms technologically close to litigants contract when a case is resolved, while firms close in product space expand. Both effects are statistically significant, and we quantify them by market below.

Columns (2) and (3) show where these total sales effects arise. Technology-space exposure is negatively associated with domestic sales, while its export coefficient is statistically insignificant. Product-market exposure shows the reverse pattern: its domestic sales coefficient is insignificant, while its export coefficient is positive and significant. The negative technology-space effect is therefore concentrated in domestic sales, whereas the positive product-market effect is concentrated in exports.

Among exposed firms, moving from the 25th to the 75th percentile of positive technology-space exposure is associated with an 8.6 percent decline in domestic sales, holding product-market exposure fixed. The corresponding change in positive product-market exposure is associated with a 1.2 percent rise in exports, holding technology-space exposure fixed.

Columns (4) to (6) distinguish winner- and loser-based exposure measures. Among firms linked to winners through technology space, total and domestic sales decline, while the export coefficient is statistically insignificant. Product-market exposure to winners is instead associated with higher total and export sales, with no significant domestic effect. The loser-based coefficients are statistically insignificant across all three outcomes, although these are largely explained by large standard errors indicating that these results have lower power compared to those of exposure to case winners.

Taken together, Table 2 shows that the effects of adjudication outside the courtroom cannot be summarized by a single undifferentiated spillover: both the sign and the market location of the response depend on the relationship linking a non-litigant to the litigants.

3.2. *Export Margins and Ownership*

We next characterize the product-market export response along intensive and extensive margins and examine whether the baseline effects vary with the ownership of litigants or non-litigants.

The literature on multi-product and multi-destination exporters shows how trade liberalization, destination-market competition, and market-specific trade barriers reshape the scale,

Table 2: Spillover Effects of IP Court Rulings on Non-Litigants

Dep.Var. (log)	Total exposure			Winner- and loser-based exposure		
	Total Sales	Domestic Sales	Exports	Total Sales	Domestic Sales	Exports
	(1)	(2)	(3)	(4)	(5)	(6)
SPILLTECH _{ft}	-0.069*** (0.018)	-0.105*** (0.024)	-0.050 (0.039)			
SPILLHS8 _{ft}	0.031** (0.013)	0.023 (0.025)	0.094*** (0.034)			
SPILLTECH _{ft} ^{win}				-0.072*** (0.018)	-0.107*** (0.025)	-0.062 (0.040)
SPILLHS8 _{ft} ^{win}				0.028** (0.014)	0.026 (0.026)	0.100*** (0.037)
SPILLTECH _{ft} ^{lose}				-0.023 (0.044)	-0.061 (0.062)	0.095 (0.111)
SPILLHS8 _{ft} ^{lose}				0.030 (0.024)	-0.017 (0.050)	0.031 (0.060)
<i>N</i>	97,607	79,881	77,564	97,607	79,881	77,564
Adj. <i>R</i> ²	0.859	0.798	0.671	0.859	0.798	0.671
Firm FE	✓	✓	✓	✓	✓	✓
City-year FE	✓	✓	✓	✓	✓	✓
Sector FE	✓	✓	✓	✓	✓	✓

Notes: Columns (1)–(3) use total technology-space (SPILLTECH_{ft}) and product-space (SPILLHS8_{ft}) exposure measures constructed from time-invariant patent class and HS8 export share similarities; columns (4)–(6) use the corresponding winner- and loser-based measures. All specifications exclude firms that ever litigate and include firm, city-year, and 2-digit sector fixed effects. Robust standard errors clustered at the firm level are reported in parentheses. Asterisks denote significance: **p* < 0.1, ***p* < 0.05, ****p* < 0.01.

scope, and allocation of exports (Eckel and Neary, 2010; Bernard et al., 2011; Mayer et al., 2014; Fontagné and Orefice, 2018). Appendix Table A3 examines a different source of adjustment: an IP court ruling transmitted through pre-determined product-market proximity. Product-similar non-litigants increase average exports per product and destination while also exporting more products and serving more destinations. Roughly two-thirds of the aggregate export response comes from higher export intensity and one-third from broader product and destination scope. The aggregate export response therefore reflects expansion on both the intensive and extensive margins, rather than a pure reshuffling of a fixed volume across a wider portfolio. Technology-space exposure produces no comparable expansion, reinforcing that the export adjustment is specific to product-market connections.

Ownership is economically relevant because domestic and foreign firms can receive different treatment in IP and commercial disputes (Mai and Stoyanov, 2019; Choudhury et al., 2025; Bo and Liu, 2026). If the export response were confined to foreign litigants or foreign-

owned non-litigants, it could reflect nationality-specific enforcement or multinational networks rather than a broader product-market response. Appendix Table A4 reports positive product-market export coefficients for exposure to both domestic and foreign litigants. Appendix Table A5 likewise reports similar point estimates for domestic and foreign non-litigants, although the domestic-firm estimate is less precise. Technology-space exposure remains associated with lower domestic sales across the ownership splits. Overall, the ownership splits produce the same qualitative contrast across groups, although some estimates are imprecise.

4. Identification and Robustness

The baseline results show a sharp asymmetry between technology-space and product-market spillovers; the same pattern is statistically significant for winner-based exposure but not for loser-based exposure. Whether this can be read as the effect of actual court rulings depends on addressing several alternatives. Firms close in technology or product space may comove for reasons unrelated to litigation; case outcomes may themselves reflect case strength or market conditions; exposed firms may already be on diverging paths before judgment; and the estimates may depend on how exposure and the estimation sample are defined. We address these concerns using permutation tests, instrumental-variable strategies, dynamic estimates, and a range of robustness checks.

4.1. Permutation

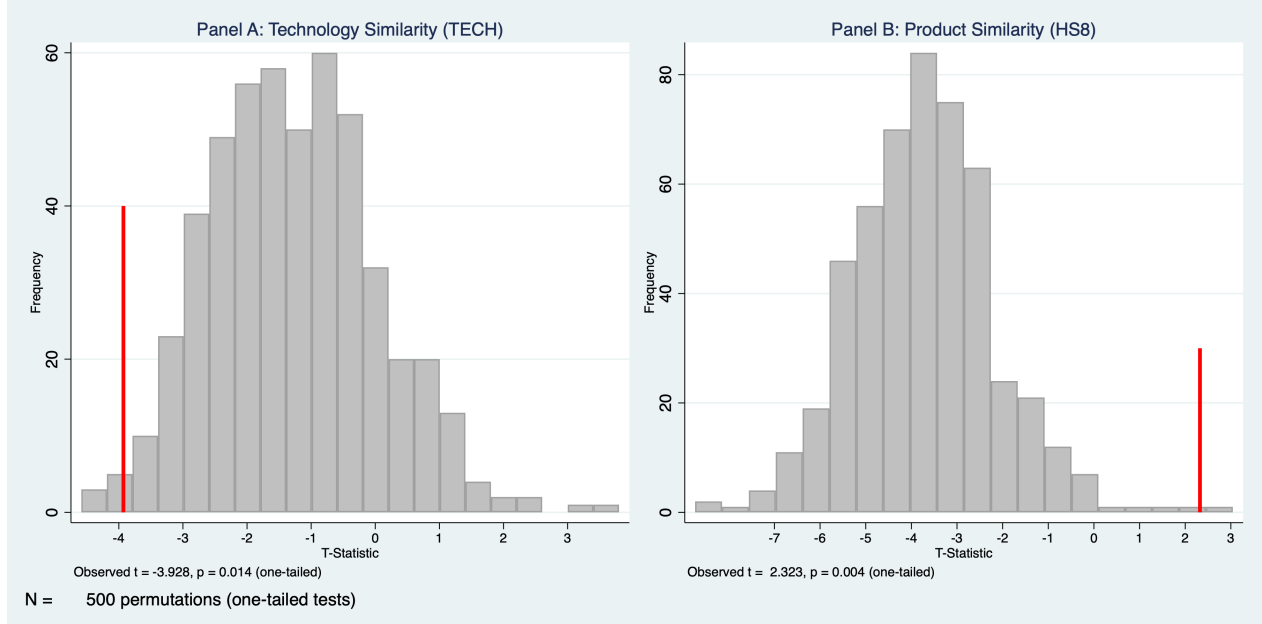
A first threat is that firms linked in technology or product space move together for reasons unrelated to litigation, such as shared demand, technology, or trade shocks, so that the baseline estimates reflect generic relatedness rather than the rulings themselves. To isolate this, we conduct a permutation exercise, following Fisher (1922), Young (2019), and Mezzanotti (2021).¹¹

We implement the test by randomly assigning non-litigant firms to the role of litigants while preserving the number of litigants in each year.¹² For each reassignment, we reconstruct the spillover measures and re-estimate the baseline specification. We repeat this procedure 500 times. If the baseline estimates are specific to firms connected to the actual litigants, the observed t-statistics should fall in the tails of the resulting placebo distributions. Following Mezzanotti (2021), we use the tail corresponding to the sign of each baseline estimate.

¹¹This non-parametric approach also allows inference without relying on asymptotic approximations or parametric assumptions about the error distribution, which is useful given potential concerns about clustering and serial correlation in our setting.

¹²This preserves the distribution of litigation over time, allowing the test to vary which firms are assigned litigation rather than aggregate litigation intensity.

Figure 1: Permutation Test: Distribution of t-statistics



Notes: These figures present results from permutation tests with 500 random reassignments of litigation status. The outcome is log total sales. Histograms show the distribution of t-statistics from permuted data. Red vertical lines indicate observed t -statistics from actual data. Panel A: Technology similarity (SPILLTECH_{ft}) spillovers. Panel B: Product similarity (SPILLHS8_{ft}) spillovers. p-values shown below each panel represent the proportion of permuted statistics at least as extreme as the observed statistic.

Figure 1 presents the results. Under random reassignment, the placebo t -statistics for both channels cluster below zero; this reflects the mechanics of the exposure design rather than a genuine effect of similarity, so the relevant question is where the observed statistic falls within each placebo distribution. For technology-space exposure, the observed t -statistic lies in the left tail, more negative than almost all placebo draws. For product-market exposure, the observed statistic is positive and lies in the right tail, opposite the bulk of the placebo distribution. In both cases the observed effect is unlikely to arise under random reassignment, indicating that it depends on which firms actually litigate rather than on the similarity network applied to arbitrary firms.

We repeat the permutation test separately for domestic and export sales. The results in Figure A3 are consistent with the market-specific patterns documented in Section 3.1. For domestic sales, the technology channel remains strongly negative and shows a clear ruling-specific component beyond the placebo, while the product-market coefficient remains statistically insignificant in the baseline estimates. For export sales, the pattern reverses and the product-market t -statistic lies deep in the right tail of the placebo distribution, while the

technology channel is indistinguishable from the placebo.

One limitation of this permutation test is that it preserves annual litigation intensity but not the characteristics of the firms that litigate. Actual litigants are generally larger than the non-litigating firms from which placebo litigants are drawn. They also tend to have broader export baskets, which can create product-market links to a larger number of firms. Uniformly drawn placebo litigants may therefore differ systematically from actual litigants in ways that affect the distribution of exposure even in the absence of a ruling. This is an instance of the nonrandom exposure problem in exposure designs (Borusyak and Hull, 2023): the distribution of exposure reflects which firms litigate, not only whether a ruling occurs.

We address differences in observable characteristics between actual and placebo litigants using a matched permutation exercise that reassigns litigant status within propensity score strata (Rosenbaum, 1984). Using firms’ pre-determined characteristics measured in 2000, we estimate each firm’s propensity to litigate as a function of log sales, employment, total assets, exporter status, and two-digit sector, and divide the similarity universe into propensity score deciles. For each year and decile, we draw exactly as many placebo litigants as there are actual litigants in that cell. We then reconstruct both exposure measures and re-estimate equation (1), repeating the procedure 500 times. The placebo firms thus resemble actual litigants in observable characteristics while their identities continue to be randomly assigned within each year–decile cell.¹³

Appendix Table A6 reports the results. For technology-space exposure, the observed statistics for total and domestic sales remain in the left tails of the matched placebo distributions, while the export statistic is indistinguishable from its placebo distribution. For product-market exposure, matching shifts the export placebo distribution above zero; the observed statistic remains in its upper tail, whereas the total and domestic sales statistics do not. The matched exercise reinforces the technology-space results; the product-market export response is the more fragile of the two, surviving this stricter comparison only at a one-tailed p -value of 0.094. Because the propensity model matches litigants on size and scope, which are correlated with a firm’s centrality in similarity space but do not capture it directly, some nonrandom exposure could remain if litigation reflects centrality beyond these characteristics.

4.2. Endogeneity and IV

A second threat is that case outcomes are endogenous: plaintiffs may be more likely to win in faster-growing technological fields or product markets, causing winner-based exposure to

¹³As an implementation check, applying the same exposure-construction procedure to the actual litigants reproduces the original SPILLTECH _{ft} and SPILLHS8 _{ft} measures exactly.

capture those underlying conditions rather than the consequences of the adjudicated outcome. We address this concern with two case-level instruments for plaintiff victory: judge-panel leniency and a predicted win probability constructed from pre-determined case characteristics. We aggregate both instruments to the firm-year level using the same pre-determined similarity weights as in the baseline exposure measures.

Our first instrument exploits variation in the plaintiff win propensity of the assigned three-judge panel. Following Galasso and Schankerman (2015), we measure each judge’s tendency to rule for plaintiffs using all of the judge’s other cases and combine the three leave-one-out measures according to the panel’s majority vote rule.¹⁴ Because litigants do not select the judges hearing their case, assignment to a more plaintiff-friendly panel provides plausibly exogenous variation in case outcomes. For non-litigants, the exclusion restriction requires panel composition to matter only through the adjudicated outcome. At the firm-year level, this means that the similarity-weighted combination of panel assignments across cases linked to firm f must be orthogonal to its contemporaneous sales shocks. This condition is plausible because the similarity weights are fixed before litigation and non-litigants choose neither the cases to which they are linked nor the panels assigned to those cases.

Formally, for each judge we compute a leave-one-out plaintiff win propensity and combine the three propensities associated with the assigned panel according to the majority vote rule:

$$\hat{p}_c^{\text{panel}} = f_{1c}f_{2c}f_{3c} + f_{1c}f_{2c}(1 - f_{3c}) + f_{1c}(1 - f_{2c})f_{3c} + (1 - f_{1c})f_{2c}f_{3c}, \quad (2)$$

where f_{kc} denotes judge k ’s leave-one-out plaintiff win rate for case c , calculated from all other cases involving that judge. Treating the three judge-specific propensities as independent, equation (2) gives the probability that at least two panel members rule in favor of the plaintiff. We then use this panel propensity as the principal predictor in the following case-level probit:

$$\Pr(\text{Win}_{ct} = 1) = \Phi(\gamma\hat{p}_c^{\text{panel}} + \Psi_1\text{CaseType}_c + \Psi_2\text{Jurisdiction}_c + \delta_{\text{city}} + \delta_t), \quad (3)$$

where $\text{Win}_{ct} = 1$ if the plaintiff wins case c in year t , and $\Phi(\cdot)$ is the standard normal cumulative distribution function. Controls include case-type categories (Patent, Trademark, Copyright, or Other) and court jurisdiction level (Primary, Intermediate, Higher). City and year fixed effects absorb systematic differences across local judicial environments and over

¹⁴Throughout our sample period, IP cases were adjudicated by three-judge collegial panels (mandated by the 2002 Provisions of the Supreme People’s Court, *Zui Gao Ren Min Fa Yuan [2002] No. 25*). Judges were assigned based on availability and subject-matter expertise rather than case characteristics; majority vote rules limited any single judge’s influence; and post-2007 computerized assignment in pilot jurisdictions further reduced scope for strategic panel formation.

Table 3: IV Estimates of IP Court Rulings on Non-Litigants' Total Sales

Dep.Var. (log)	Total Sales	
	Judge-panel leniency IV	Predicted win prob. IV
	(1)	(2)
SPILLTECH $_{ft}^{win}$	-0.077*** (0.020)	-0.071*** (0.020)
SPILLHS8 $_{ft}^{win}$	0.036** (0.018)	0.050*** (0.017)
N	97,607	97,607
Weak-ID (KP F)	6,900	8,455
Firm, City-year, Sector FE	✓	✓

Notes: Column (1) instruments winner-based exposure using case-level plaintiff win probabilities from the probit in equation (3), whose principal predictor is judge-panel leniency constructed from the leave-one-out propensities in equation (2). Column (2) uses fitted probabilities from an LPM based on pre-determined case and litigant characteristics, with city, year, sector, and judge fixed effects. In both columns, the fitted probability is assigned to the plaintiff and one minus this probability to the defendant. These probabilities are then averaged across cases within each litigant-year and weighted by pre-determined technology- or product-space similarity to construct firm-year instruments. The corresponding case-level prediction regressions are reported in Table A1. Weak-ID (KP F) reports the Kleibergen–Paap rk Wald F statistic for weak identification. All specifications exclude firms that ever litigate and include firm, city-year, and 2-digit sector fixed effects. Robust standard errors clustered at the firm level are reported in parentheses. Asterisks denote significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

time. Standard errors are clustered at the case level.

The fitted plaintiff win probabilities from equation (3) are first converted into litigant-year predicted win probabilities. We assign defendants one minus the predicted plaintiff win probability and average across multiple cases involving the same litigant in a given year. We then weight each litigant-year probability by its pre-determined HS8 or technology-space similarity to firm f and sum across linked litigants. This similarity-weighting procedure is a distinct part of our design: it maps panel-propensity-based case predictions into continuous firm-year excluded instruments.

We then construct a second, complementary instrument from a case-level linear probability model (LPM) of plaintiff victory, following [Lanjouw and Schankerman \(2001\)](#) and [Galasso](#)

and Schankerman (2010). The model uses pre-determined case and litigant characteristics, including court jurisdiction, dispute type, the plaintiff’s initial size, and foreign status, and absorbs city, year, sector, and judge fixed effects. We map the fitted probabilities to firm-year instruments as in the judge-panel design, assigning defendants one minus the predicted plaintiff win probability, averaging across cases within each litigant-year, and applying the same similarity weights.¹⁵ Because this strategy uses observable case and litigant characteristics rather than quasi-random panel assignment, we treat it as a complementary prediction-based check rather than our primary source of identification.

Table 3 reports the 2SLS estimates. Column (1) uses the judge-panel leniency instrument, while column (2) uses the predicted win probability instrument.¹⁶ Both instruments reproduce the sign pattern of the winner-based OLS estimates, with broadly similar or somewhat larger magnitudes: $\text{SPILLTECH}_{ft}^{\text{win}}$ remains negative and significant, while $\text{SPILLHS8}_{ft}^{\text{win}}$ remains positive and significant. The judge-panel estimates are particularly informative because they use variation in plaintiff victory associated with assigned panel leniency. Their similarity to the winner-based OLS estimates suggests that selection into winning is unlikely to account for the baseline pattern. The prediction-based estimates provide a complementary check using pre-determined case and litigant characteristics. Both instruments are mapped into firm-year exposure using the same pre-period similarity weights. Appendix Table A7 shows that both instruments also reproduce the market-specific pattern: technology-space exposure lowers domestic sales, whereas product-market exposure raises exports, with the corresponding cross-market coefficients statistically insignificant.

4.3. *Timing of Effects*

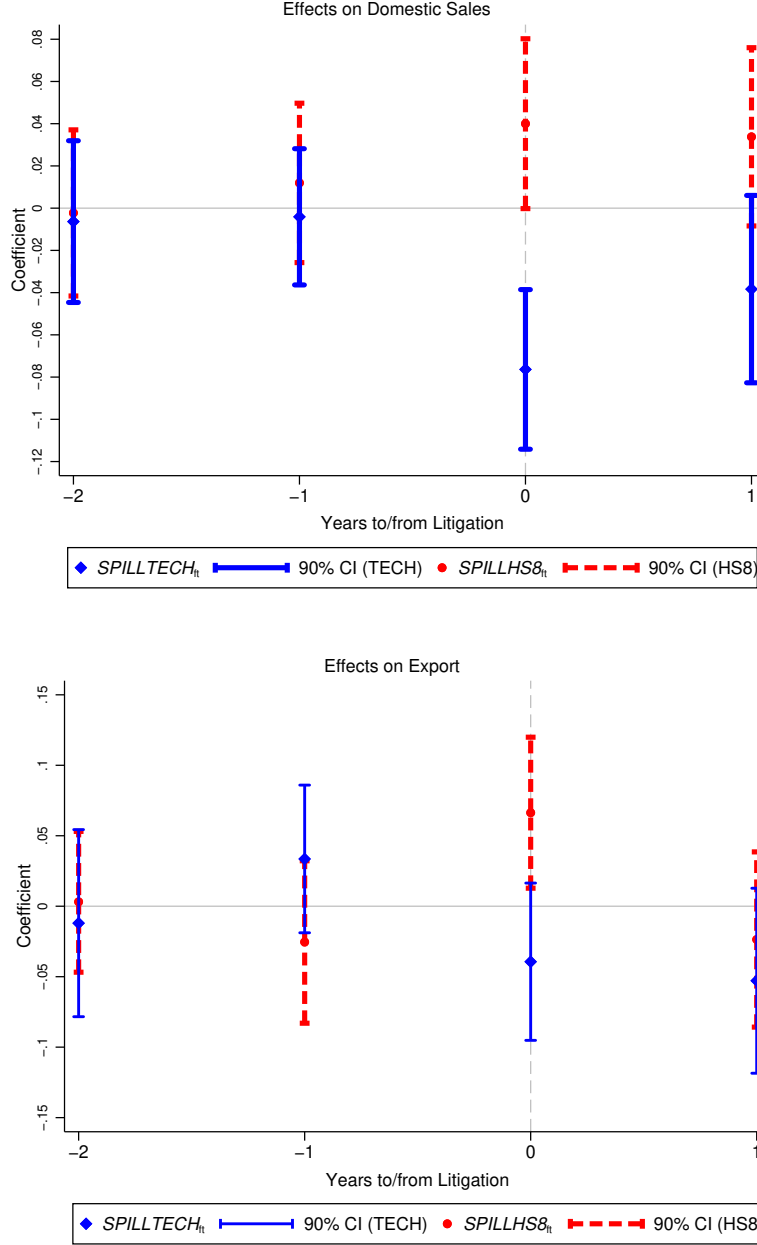
A third threat concerns timing. If the main estimates reflect differential pre-trends rather than the effect of court rulings, exposed firms should begin to diverge before judgment. We therefore estimate dynamic versions of the domestic and export sales specifications from two years before to one year after the court judgment. Figure 2 reports the corresponding coefficients for total spillover exposure, with 90 percent confidence intervals.

The coefficients corresponding to the baseline market-specific effects show little divergence before judgment. At the judgment date, technology-space exposure becomes sharply negative for domestic sales, while product-market exposure becomes positive for exports. Both coefficients move back toward zero by the following year. This timing is more consistent with a response to the ruling than with slow-moving differential trends or gradual changes in firm capabilities. Appendix Figure A4 reports the corresponding estimates for total sales, which

¹⁵Details are provided in Appendix D.

¹⁶The corresponding case-level prediction regressions are reported in Table A1.

Figure 2: Dynamic Spillover Effects of IP Court Rulings on Non-Litigating Firms



Notes: The figure plots dynamic coefficients for total spillover exposure to resolved IP cases. The upper panel uses log domestic sales as the outcome and the lower panel uses log export sales. The horizontal axis reports years relative to the court judgment year ($t = 0$). Error bars show 90 percent confidence intervals. Estimates come from dynamic specifications with firm, city-year, and 2-digit sector fixed effects, with standard errors clustered at the firm level. The sample excludes firms that ever litigate.

are broadly consistent with the baseline sign pattern.

4.4. Additional Robustness Checks

A final set of checks is reported in Appendix Table A8. The table examines the sensitivity of the baseline asymmetry to three sets of concerns: the construction of exposure, contemporaneous policy support and local litigation intensity, and alternative event timing and case definitions. These checks assess whether the baseline coefficients depend on particular measurement choices or capture factors correlated with litigation activity.

Columns (1) to (3) address measurement and potential confounding. Column (1) probes the functional form of the similarity metric: replacing cosine similarity with Mahalanobis-based similarity, which allows related categories to covary rather than treating them as orthogonal, leaves the asymmetric pattern intact. Column (2) guards against the spillovers proxying for contemporaneous policy support: adding firm-level controls for preferential tax treatment (index_tax_{ft}), government subsidies ($\text{index_subsidy}_{ft}$), and interest-rate support ($\text{index_interest}_{ft}$)¹⁷ does not alter the sign pattern of the main coefficients. Column (3) addresses local litigation intensity: adding the number of IP cases in the firm’s city-sector-year again leaves the technology-space coefficient negative and the product-market coefficient positive.

A related set of concerns involves how cases are timed and classified. Column (4) re-dates exposure by the filing year rather than the judgment year, yielding the same qualitative conclusion. Column (5) reconstructs exposure using only first-instance cases and leaves the main pattern unchanged. Column (6) reconstructs winner-based exposure using an alternative definition of plaintiff success that requires winning at least half of the claims; the coefficients retain the signs of the baseline winner-based estimates.

Column (7) turns to the concern that product-market proximity may be too sensitive to the very fine HS8 product definition. Reconstructing product-market exposure at the HS4 level still yields the same pattern: the technology-space coefficient remains negative, while the coarser product-market coefficient remains positive and significant.

Taken together, the estimates are stable across alternative measurement, controls, timing, case definitions, and product aggregation.

¹⁷Following Aghion et al. (2015), $\text{index_subsidy}_{ft}$ and index_tax_{ft} are dummy variables equal to one if a firm receives a subsidy or tax break in year t . $\text{index_interest}_{ft}$ equals one if the reported borrowing rate is below the 2-digit sector-year mean, and zero otherwise.

5. Evidence on Adjustment Channels

The main results point to different forms of adjustment across the two networks. Technology-space exposure reduces domestic sales, while product-market exposure raises exports. Both patterns could in principle reflect capability accumulation — non-litigants investing in innovation or upgrading product quality in response to a shift in the competitive landscape — rather than a market-specific reallocation. We test this possibility directly. To understand this contrast, we examine whether the domestic decline is accompanied by changes in imports or innovation, and whether the export gain reflects destination reallocation, foreign demand, or product upgrading.

5.1. *The Technology-Space Channel*

Technology-space exposure lowers domestic sales at judgment and leaves exports unchanged, pointing to a short-run domestic contraction. Does this decline extend to other dimensions of firm activity, or is it accompanied by greater innovation? A broader contraction should reduce imports, whereas an innovation response should raise R&D.

Imports provide evidence of contraction. As seen in Table 4, technology-space exposure reduces total imports by 0.258 log points and combined imports of intermediate and capital goods by 0.253 log points. When the two end uses are considered separately, both intermediate input and capital goods imports decline, whereas consumption goods imports show no detectable response. Imported intermediate inputs enter production directly and have been shown to raise productivity and expand product scope (Amiti and Konings, 2007; Goldberg et al., 2010; Halpern et al., 2015), while capital goods imports are closely tied to equipment investment and productive capacity (Caselli and Wilson, 2004; Huang et al., 2026). The import response therefore appears concentrated in goods associated with production and investment. Product-market exposure, in contrast, has no detectable effect on total imports or any end-use category.

R&D does not rise alongside the fall in imports, despite evidence that IP enforcement can alter firms' incentives to innovate (Galasso and Schankerman, 2015; Mezzanotti, 2021; Mai et al., 2024). Table A9 shows that neither total nor winner-based technology-space exposure affects the probability that a firm reports R&D or the amount invested.¹⁸ Together, the decline in intermediate and capital goods imports and the absence of an R&D response support a contractionary interpretation of the technology-space effect rather than an adjustment driven by greater innovative investment. The evidence does not, however,

¹⁸The only significant estimates are positive coefficients on product-market exposure to losing litigants, which do not parallel the winner-based results for sales and exports.

Table 4: Spillover Effects of IP Court Rulings on Non-litigants' Imports

Dep.Var. (log)	Intermediate +				
	Total (1)	capital (2)	Intermediate (3)	Capital (4)	Consumption (5)
SPILLTECH _{ft}	-0.258*** (0.065)	-0.253*** (0.065)	-0.179*** (0.065)	-0.276*** (0.091)	-0.058 (0.092)
SPILLHS8 _{ft}	-0.031 (0.054)	0.007 (0.054)	0.012 (0.053)	0.047 (0.090)	-0.112 (0.087)
<i>N</i>	45,509	42,914	41,165	20,749	19,514
Adj. <i>R</i> ²	0.776	0.774	0.784	0.653	0.655
Firm, City-year, Sector FE	✓	✓	✓	✓	✓

Notes: Column (1) uses log total imports. Columns (2)–(5) classify HS6 products by end use using the Broad Economic Categories classification. Column (2) combines intermediate and capital goods. Each dependent variable is defined for firm–year observations with positive imports in the relevant category. The key regressors measure firm f 's exposure to resolved IP cases through technology-space proximity (SPILLTECH_{ft}) and product-market proximity (SPILLHS8_{ft}). All specifications exclude firms that ever litigate and include firm, city-year, and 2-digit sector fixed effects. Robust standard errors clustered at the firm level are reported in parentheses. Asterisks denote significance levels: $*p < 0.1$, $**p < 0.05$, $***p < 0.01$.

distinguish whether the contraction reflects changes in access to IP, commercial terms, or other dimensions of domestic competition.

5.2. The Product-Market Channel

Product-market exposure raises non-litigants' exports, but leaves open whether this response is spread evenly across destinations or reflects a shift in where firms sell. Existing literature shows that exporters reallocate sales across markets following large devaluations, destination-specific demand shocks, and technical barriers, including by redirecting exports toward less affected or unrestricted destinations (Kohn et al., 2020; Oglend et al., 2024; Fontagné and Orefice, 2018). More generally, models and evidence on imperfect competition show that market structure shapes firms' pricing and output decisions, especially when market shares are concentrated (Atkeson and Burstein, 2008; Bresnahan and Reiss, 1991). We therefore test whether the product-market response varies with exporter concentration across destination-product markets.

To do so, we move to the firm–destination–year level and interact spillover exposure with the lagged Herfindahl–Hirschman Index (HHI) for the firm's leading HS8 product in each destination.¹⁹ The regression includes firm–destination, destination–year, and firm–year fixed

¹⁹For each HS8 product–destination–year cell, we calculate $HHI_{pdt} = \sum_j (X_{jpd}/\sum_k X_{kpdt})^2$, where X_{jpd} is ordinary-trade export value for Chinese exporter j in that cell. For years with no exports to a destination,

Table 5: Export Spillovers by Destination-Product Market Concentration

Dep.Var.	Exports _{fdt}	
	(1)	(2)
	Total exposure $\times$ HHI	Winner-loser $\times$ HHI
HHI _{pdt-1} ^z	0.010 (0.011)	0.010 (0.011)
SPILLTECH _{ft} $\times$ HHI _{pdt-1} ^z	0.003 (0.031)	
SPILLHS8 _{ft} $\times$ HHI _{pdt-1} ^z	0.056** (0.028)	
SPILLTECH _{ft} ^{win} $\times$ HHI _{pdt-1} ^z		-0.032 (0.031)
SPILLHS8 _{ft} ^{win} $\times$ HHI _{pdt-1} ^z		0.089*** (0.028)
SPILLTECH _{ft} ^{lose} $\times$ HHI _{pdt-1} ^z		0.288 (0.182)
SPILLHS8 _{ft} ^{lose} $\times$ HHI _{pdt-1} ^z		-0.099* (0.058)
<i>N</i>	1,086,658	1,086,658
Pseudo <i>R</i> ²	0.922	0.922
Firm-destination FE	✓	✓
Firm-year FE	✓	✓
Destination-year FE	✓	✓

Notes: This table reports PPML estimates for firm-destination-year exports. Both columns interact spillover exposure with lagged destination-product concentration, measured by the Herfindahl-Hirschman Index, HHI_{pdt-1}^z, computed among Chinese ordinary-trade exporters within the same HS8 $\times$ destination market in $t - 1$ and standardized within year. The lagged HHI is based on the firm's leading HS8 export product in that destination in the previous year; for years with no exports, the product anchor is filled within the firm-destination panel using years with positive exports. Column (1) uses total spillover exposure, while column (2) uses the corresponding winner- and loser-based measures. All specifications absorb firm-destination, firm-year, and destination-year fixed effects. The exposure main effects are absorbed by firm-year fixed effects, so the interactions are identified from within-firm-year variation in export allocation across destinations. Robust standard errors clustered at the firm level are reported in parentheses. Asterisks denote significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

effects. Firm-destination effects remove persistent bilateral differences, while destination-year effects absorb time-varying shocks common to all firms selling in the same destination and year. More importantly, firm-year effects account for the firm's aggregate export response

the firm's leading-product anchor is filled within the firm-destination panel using years with positive exports. We lag the resulting HHI by one year and standardize it within year.

in each year, so the interaction is estimated from differences in its export allocation across destinations.

Table 5 reports the results. In column (1), the interaction between total product-market exposure and lagged destination-product concentration is positive and statistically significant: for a highly exposed firm, a one-standard-deviation increase in a destination’s concentration is associated with roughly 5.8 percent higher exports to that destination than to its other destinations.²⁰ The corresponding technology-space interaction is close to zero. Column (2) separates winner- and loser-based exposure: the winner-based product-market interaction is positive and statistically significant, while neither technology-space interaction is significant.

These estimates sharpen the aggregate export result. The increase in exports associated with product-market exposure is not distributed evenly across a firm’s destinations: conditional on the firm’s total exports in that year, its export mix shifts toward more concentrated destination-product markets. Aggregate export expansion is therefore accompanied by destination reallocation rather than a uniform rise across markets.²¹

An alternative to reallocation is that export gains are concentrated in destinations with stronger demand or stronger IP protection. A ruling could raise foreign demand for related products if firms share a collective reputation (Bai et al., 2022; Bachmann et al., 2023), while stronger destination patent protection is itself associated with larger exports of protected goods (Ivus, 2010; Maskus, 2015). Table A10 tests whether the product-market response varies with destination GDP per capita or the Ginarte–Park index, a cross-country measure of the statutory strength of patent protection.²² Neither interaction is statistically significant. The estimates therefore provide no clear evidence that the product-market response varies systematically with destination income or IP protection.

A further possibility is that exposed firms adjust the quality of the products they sell. Trade policy and competitive shocks can induce firms to change product quality (Amiti and Khandelwal, 2013; Caselli et al., 2023). This explanation implies changes in export unit values or measured quality, not only in the destinations to which firms sell. Table A11

²⁰The one unit of product-market exposure used in this illustration is close to the 99th percentile of the baseline firm-year distribution.

²¹Results are similar after controlling for lagged destination-product market size, measured as log total Chinese ordinary-trade exports in the same HS8×destination cell, and its interactions with exposure. The HHI interaction is 0.055 ($p = 0.058$) for total product-market exposure and 0.089 ($p = 0.003$) for winner-based exposure; the corresponding product-market exposure interactions with market size are statistically insignificant.

²²The Ginarte–Park index ranges from zero to five and summarizes five features of national patent systems: coverage, membership in international patent agreements, duration, provisions governing loss of protection, and enforcement mechanisms. Higher values indicate stronger statutory patent protection (Ginarte and Park, 1997; Park, 2008).

looks at export unit values and demand-inferred quality.²³ Neither exposure coefficient is statistically distinguishable from zero for either outcome. The export response is thus not accompanied by higher unit values or higher measured quality, so it does not appear to run through product upgrading.

The export response is also not a case of venting out, in which firms redirect sales abroad when domestic demand falls (Almunia et al., 2021): product-market exposure does not lower domestic sales (Table 2), so there is no domestic slump to redirect.

Overall, the evidence points to different forms of adjustment across the two networks. Technology-space exposure is accompanied by lower domestic sales and lower imports of intermediate and capital goods, but not by greater R&D. Product-market exposure raises exports, with larger gains in more concentrated destination-product markets. The response does not vary systematically with destination income or IP protection and is not accompanied by higher export unit values or measured quality. We therefore interpret the first pattern as a contraction in domestic activity and the second as export expansion accompanied by destination reallocation. These tests narrow the possible explanations, although they do not identify a unique structural mechanism.

6. Conclusion

This paper asks whether IP court rulings affect firms that did not participate in the disputes. We combine litigation records with production, patent, and customs data from China and construct firm-year exposure from non-litigants' pre-determined similarity to litigants. Technology-space exposure reduces total sales, with the decline concentrated in domestic sales and no significant export response. Product-market exposure raises total sales through higher exports, with no corresponding domestic effect.

Additional results help characterize these responses. Technology-space effects appear at judgment and are accompanied by lower imports of intermediate and capital goods, but not by greater innovation. Product-market export gains are larger in concentrated destination-product markets and are not accompanied by quality upgrading. The evidence is therefore more consistent with short-run, market-specific adjustment than with gradual capability accumulation. Our estimates describe how the effects of rulings vary among connected non-

²³Following Khandelwal et al. (2013), we infer export quality from a CES demand system estimated at the firm-HS6-destination-year level. We estimate $\log q_{fpdt} + \eta_p \log uv_{fpdt} = \delta_p + \delta_{dt} + \varepsilon_{fpdt}$, where δ_p and δ_{dt} are HS6 and destination-year fixed effects, and recover log quality as $\log \hat{\lambda}_{fpdt} = \hat{\varepsilon}_{fpdt}/(\eta_p - 1)$. Here q is export quantity, uv is the unit value, and $\eta_p > 1$ is the absolute sector-level trade elasticity assigned to product p . Intuitively, a variety that sells more than its price alone would predict is inferred to be of higher quality. We assign η_p from the sector-level trade elasticities of Imbs and Méjean (2017), giving each HS6 product its sector's estimate and using the Chinese value where available and the U.S. value otherwise.

litigants; they do not provide a complete welfare accounting or establish a direct transfer of sales between firms.

The paper identifies a less-studied margin of adjudication: short-run, network-dependent adjustments among firms outside the courtroom. Case-level rulings affect non-litigants, and those effects depend on bilateral technological and product-market relationships that broad industry classifications do not capture. These effects can also appear in export markets even though the ruling carries no legal force abroad, as firms adjust the destination composition of their exports. The underlying issue is not specific to Chinese IP litigation: other court rulings and regulatory actions that change firms' relative positions may also affect connected firms that are not directly subject to the decision. Evaluations of adjudication should therefore consider firms outside the courtroom and the specific economic relationships linking them to the parties.

References

- ACIKALIN, U. U., T. CASKURLU, G. HOBERG, AND G. M. PHILLIPS (2026): “Intellectual Property Protection Lost and Competition: An Examination Using Large Language Models,” *Journal of Financial Economics*, 182, 104306.
- AGHION, P., J. CAI, M. DEWATRIPONT, L. DU, A. HARRISON, AND P. LEGROS (2015): “Industrial Policy and Competition,” *American Economic Journal: Macroeconomics*, 7, 1–32.
- ALM, J., B. R. JACKSON, AND M. MCKEE (2009): “Getting the Word Out: Enforcement Information Dissemination and Compliance Behavior,” *Journal of Public Economics*, 93, 392–402.
- ALMUNIA, M., P. ANTRÀS, D. LOPEZ-RODRIGUEZ, AND E. MORALES (2021): “Venting Out: Exports during a Domestic Slump,” *American Economic Review*, 111, 3611–3662.
- AMITI, M. AND A. K. KHANDELWAL (2013): “Import Competition and Quality Upgrading,” *The Review of Economics and Statistics*, 95, 476–490.
- AMITI, M. AND J. KONINGS (2007): “Trade Liberalization, Intermediate Inputs, and Productivity: Evidence from Indonesia,” *American Economic Review*, 97, 1611–1638.
- ANG, J. S., Y. CHENG, AND C. WU (2014): “Does Enforcement of Intellectual Property Rights Matter in China? Evidence from Financing and Investment Choices in the High-Tech Industry,” *The Review of Economics and Statistics*, 96, 332–348.
- ATKESON, A. AND A. BURSTEIN (2008): “Pricing-to-Market, Trade Costs, and International Relative Prices,” *American Economic Review*, 98, 1998–2031.
- AWATE, K. S. AND M. MAKHIJA (2022): “A Trojan Horse Inside the Gates? Knowledge Spillovers during Patent Litigation,” *Academy of Management Journal*, 65, 1747–1769.
- BACHMANN, R., G. EHRLICH, Y. FAN, D. RUZIC, AND B. LEARD (2023): “Firms and Collective Reputation: A Study of the Volkswagen Emissions Scandal,” *Journal of the European Economic Association*, 21, 484–525.
- BAI, J., L. GAZZE, AND Y. WANG (2022): “Collective Reputation in Trade: Evidence from the Chinese Dairy Industry,” *The Review of Economics and Statistics*, 104, 1121–1137.
- BERNARD, A. B., J. B. JENSEN, AND P. K. SCHOTT (2006): “Survival of the Best Fit: Exposure to Low-Wage Countries and the (Uneven) Growth of US Manufacturing Plants,” *Journal of International Economics*, 68, 219–237.
- BERNARD, A. B., S. J. REDDING, AND P. K. SCHOTT (2011): “Multiproduct Firms and Trade Liberalization,” *The Quarterly Journal of Economics*, 126, 1271–1318.
- BESSEN, J. AND M. J. MEURER (2008): *Patent Failure: How Judges, Bureaucrats, and Lawyers Put Innovators at Risk*, Princeton University Press.

- (2012): “The Private Costs of Patent Litigation,” *Journal of Law, Economics & Policy*, 9, 59.
- BLOOM, N., M. DRACA, AND J. VAN REENEN (2016): “Trade Induced Technical Change? The Impact of Chinese Imports on Innovation, IT and Productivity,” *The Review of Economic Studies*, 83, 87–117.
- BLOOM, N., M. SCHANKERMAN, AND J. VAN REENEN (2013): “Identifying Technology Spillovers and Product Market Rivalry,” *Econometrica*, 81, 1347–1393.
- BO, S. AND W. LIU (2026): “From Trade Protectionism to Legal Protectionism: How the Trump Trade War Harms US Firms in China,” *Journal of Development Economics*, 103829.
- BORUSYAK, K. AND P. HULL (2023): “Nonrandom Exposure to Exogenous Shocks,” *Econometrica*, 91, 2155–2185.
- BRANSTETTER, L. G., R. FISMAN, AND C. F. FOLEY (2006): “Do Stronger Intellectual Property Rights Increase International Technology Transfer? Empirical Evidence from US Firm-Level Panel Data,” *The Quarterly Journal of Economics*, 121, 321–349.
- BRESNAHAN, T. F. AND P. C. REISS (1991): “Entry and Competition in Concentrated Markets,” *Journal of Political Economy*, 99, 977–1009.
- CASELLI, F. AND D. J. WILSON (2004): “Importing Technology,” *Journal of Monetary Economics*, 51, 1–32.
- CASELLI, M., J. HUANG, C. TOMASI, AND M. ZHU (2023): “Anti-Dumping and Product Quality,” Discussion Papers 2023-08, University of Nottingham, GEP.
- CHEN, Y. AND T. PUTTITANUN (2005): “Intellectual Property Rights and Innovation in Developing Countries,” *Journal of Development Economics*, 78, 474–493.
- CHOI, H. M., J. M. KARPOFF, X. LOU, AND G. S. MARTIN (2024): “Enforcement Waves and Spillovers,” *Management Science*, 70, 834–859.
- CHOUDHURY, A., S. JANDHYALA, AND A. NANDKUMAR (2025): “Economic Nationalism and the Home Court Advantage,” *Strategic Management Journal*, 46, 242–272.
- COCKBURN, I. M., J. O. LANJOUW, AND M. SCHANKERMAN (2016): “Patents and the Global Diffusion of New Drugs,” *American Economic Review*, 106, 136–164.
- COHEN, L., U. G. GURUN, AND S. D. KOMINERS (2019): “Patent Trolls: Evidence from Targeted Firms,” *Management Science*, 65, 5461–5486.
- ECKEL, C. AND J. P. NEARY (2010): “Multi-Product Firms and Flexible Manufacturing in the Global Economy,” *The Review of Economic Studies*, 77, 188–217.

- EVANS, M. F., S. M. GILPATRIC, AND J. P. SHIMSHACK (2018): “Enforcement Spillovers: Lessons from Strategic Interactions in Regulation and Product Markets,” *The Journal of Law and Economics*, 61, 739–769.
- FANG, L. H., J. LERNER, AND C. WU (2017): “Intellectual Property Rights Protection, Ownership, and Innovation: Evidence from China,” *The Review of Financial Studies*, 30, 2446–2477.
- FARRELL, J. AND C. SHAPIRO (2008): “How Strong Are Weak Patents?” *American Economic Review*, 98, 1347–1369.
- FEENSTRA, R. C. AND G. H. HANSON (2005): “Ownership and Control in Outsourcing to China: Estimating the Property-Rights Theory of the Firm,” *The Quarterly Journal of Economics*, 120, 729–761.
- FISHER, R. A. (1922): “On the Mathematical Foundations of Theoretical Statistics,” *Philosophical Transactions of the Royal Society of London. Series A, Containing Papers of a Mathematical or Physical Character*, 222, 309–368.
- FONTAGNÉ, L. AND G. OREFICE (2018): “Let’s Try Next Door: Technical Barriers to Trade and Multi-Destination Firms,” *European Economic Review*, 101, 643–663.
- GALASSO, A. AND M. SCHANKERMAN (2010): “Patent Thickets, Courts, and the Market for Innovation,” *The RAND Journal of Economics*, 41, 472–503.
- (2015): “Patents and Cumulative Innovation: Causal Evidence from the Courts,” *The Quarterly Journal of Economics*, 130, 317–369.
- GALLETTA, S. (2017): “Law Enforcement, Municipal Budgets and Spillover Effects: Evidence from a Quasi-Experiment in Italy,” *Journal of Urban Economics*, 101, 90–105.
- GINARTE, J. C. AND W. G. PARK (1997): “Determinants of Patent Rights: A Cross-National Study,” *Research Policy*, 26, 283–301.
- GOLDBERG, P. K., A. K. KHANDELWAL, N. PAVCNİK, AND P. TOPALOVA (2010): “Imported Intermediate Inputs and Domestic Product Growth: Evidence from India,” *The Quarterly Journal of Economics*, 125, 1727–1767.
- GREENAWAY, D., J. GULLSTRAND, AND R. KNELLER (2008): “Surviving Globalisation,” *Journal of International Economics*, 74, 264–277.
- HALPERN, L., M. KOREN, AND A. SZEIDL (2015): “Imported Inputs and Productivity,” *American Economic Review*, 105, 3660–3703.
- HUANG, J., Y. WANG, AND M. ZHU (2026): “Gains from Targeting? Government Subsidies and Firm Performance in China,” *Journal of Corporate Finance*, 99, 103010.
- IMBS, J. AND I. MÉJEAN (2017): “Trade Elasticities,” *Review of International Economics*, 25, 383–402.

- IVUS, O. (2010): “Do Stronger Patent Rights Raise High-Tech Exports to the Developing World?” *Journal of International Economics*, 81, 38–47.
- JAFFE, A. B. (1986): “Technological Opportunity and Spillovers of R&D: Evidence from Firms’ Patents, Profits, and Market Value,” *American Economic Review*, 76, 984–1001.
- KHANDELWAL, A. K., P. K. SCHOTT, AND S.-J. WEI (2013): “Trade Liberalization and Embedded Institutional Reform: Evidence from Chinese Exporters,” *American Economic Review*, 103, 2169–2195.
- KOHN, D., F. LEIBOVICI, AND M. SZKUP (2020): “Financial Frictions and Export Dynamics in Large Devaluations,” *Journal of International Economics*, 122, 103257.
- LANJOUW, J. O. AND J. LERNER (2000): “The Enforcement of Intellectual Property Rights: A Survey of the Empirical Literature,” in *The Economics and Econometrics of Innovation*, Springer, 201–224.
- LANJOUW, J. O. AND M. SCHANKERMAN (2001): “Characteristics of Patent Litigation: A Window on Competition,” *The RAND Journal of Economics*, 129–151.
- (2004): “Protecting Intellectual Property Rights: Are Small Firms Handicapped?” *The Journal of Law and Economics*, 47, 45–74.
- LEE, J., S. OH, AND P. SUH (2025): “Inter-Firm Patent Litigation and Innovation Competition,” *Available at SSRN 3298557*, last revised October 21, 2025.
- LERNER, J. (1995): “Patenting in the Shadow of Competitors,” *The Journal of Law and Economics*, 38, 463–495.
- LOPEZ-LUZURIAGA, A. AND C. SCARTASCINI (2019): “Compliance Spillovers Across Taxes: The Role of Penalties and Detection,” *Journal of Economic Behavior & Organization*, 164, 518–534.
- MAI, J., Y. SHI, AND L. WANG (2024): “Judicial IPR Enforcement and Its Impact on Firms’ Innovation,” *Working paper, Shanghai University of Finance and Economics*.
- MAI, J. AND A. STOYANOV (2019): “Anti-Foreign Bias in the Court: Welfare Explanation and Evidence from Canadian Intellectual Property Litigations,” *Journal of International Economics*, 117, 21–36.
- MASKUS, K. E. (2015): “Intellectual Property in a Globalizing World: Issues for Economic Research,” *Asia-Pacific Journal of Accounting & Economics*, 22, 231–250.
- MASKUS, K. E., S. M. DOUGHERTY, AND A. MERTHA (2005): “Intellectual Property Rights and Economic Development in China,” *Intellectual Property and Development: Lessons from Recent Economic Research*, 295, 322–23.
- MASKUS, K. E. AND M. PENUBARTI (1995): “How Trade-Related Are Intellectual Property Rights?” *Journal of International Economics*, 39, 227–248.

- MAYER, T., M. J. MELITZ, AND G. I. OTTAVIANO (2014): “Market Size, Competition, and the Product Mix of Exporters,” *American Economic Review*, 104, 495–536.
- MEZZANOTTI, F. (2021): “Roadblock to Innovation: The Role of Patent Litigation in Corporate R&D,” *Management Science*, 67, 924–1022.
- OGLEND, A., F. ASCHE, AND H.-M. STRAUME (2024): “Crisis Averted: Cross-Market Reallocation during the Great Trade Collapse,” *The World Economy*, 47, 2855–2870.
- PARK, W. G. (2008): “International Patent Protection: 1960–2005,” *Research Policy*, 37, 761–766.
- RINCKE, J. AND C. TRAXLER (2011): “Enforcement Spillovers,” *The Review of Economics and Statistics*, 93, 1224–1234.
- ROSENBAUM, P. R. (1984): “Conditional Permutation Tests and the Propensity Score in Observational Studies,” *Journal of the American Statistical Association*, 79, 565–574.
- SHAPIRO, C. (2000): “Navigating the Patent Thicket: Cross Licenses, Patent Pools, and Standard Setting,” *Innovation Policy and the Economy*, 1, 119–150.
- TONG, T. W., W. HE, L. CHEN, Z.-L. HE, AND J. LU (2026): “Patent Regime Shift and Firm Innovation Strategy: Evidence from the Second Amendment to China’s Patent Law,” *Strategic Management Journal*, 1–36.
- WANG, Z. AND Z. YU (2013): “Trading Partners, Traded Products and Firm Performances of China’s Exporter Importers: Does Processing Trade Make a Difference?” *The World Economy*, 165–193.
- WEATHERALL, K. AND E. WEBSTER (2014): “Patent Enforcement: A Review of the Literature,” *Journal of Economic Surveys*, 28, 312–343.
- YOUNG, A. (2019): “Channelling Fisher: Randomization Tests and the Statistical Insignificance of Seemingly Significant Experimental Results,” *Quarterly Journal of Economics*, 134, 557–598.

Appendix

Appendix A. Calculating Product Market Proximity

Our main measure of product market closeness uses the merged ASIF-CCTS data for the year 2000. For each firm we calculate exports at the 8-digit Harmonized System (HS8) categories, of which there are 5,282 in total. On average, each manufacturing firm exports in 55 different HS8 tariff line products in this year. We use the share of a firm’s export sales within each HS8 tariff line as our measure of activity by product market, defining the vector $\mathbf{S}_i = (S_{i1}, S_{i2}, \dots, S_{iK})$, where $K = 5,282$ is the common set of HS8 tariff lines and S_{ik} is the share of export sales of firm i in HS8 tariff line k . Products not exported by the firm enter the vector with a value of zero.

To ensure that the similarity metric is comparable across firms with different scales of export activity, we normalize each firm’s export share vector by its Euclidean norm. Specifically, for each firm i , the Euclidean norm is defined as:

$$\|\mathbf{S}_i\| = \sqrt{\sum_{k=1}^K S_{ik}^2} \quad ,$$

We then normalize each element of firm i ’s export vector by dividing by this Euclidean norm, yielding a normalized export vector:

$$\mathbf{S}_i^{norm} = \left(\frac{S_{i1}}{\|\mathbf{S}_i\|}, \frac{S_{i2}}{\|\mathbf{S}_i\|}, \dots, \frac{S_{iK}}{\|\mathbf{S}_i\|} \right) \quad .$$

This normalization scales each firm’s export vector to have a length of 1. The product market closeness measure for any two different firms i and j , $HS8_{ij}$, is then calculated as the inner product of their normalized export vectors, following the methodology set out in [Bloom et al. \(2013\)](#) and [Jaffe \(1986\)](#):

$$HS8_{ij} = \mathbf{S}_i^{norm} \mathbf{S}_j^{norm} \quad . \tag{4}$$

This measure reflects the cosine of the angle between the two firms’ export share vectors in the HS8 product space.

There are 14,760 firms in the merged ASIF-CCTS data for the year 2000, and so we construct a firm-to-firm similarity matrix of size 14760×14760 . Each firm is paired with every other firm, resulting in a square matrix, where each cell $HS8_{ij}$ represents the product market proximity between firm i and firm j . This exhaustive pairing ensures that for each firm, we consider its proximity to the 14,760 firms in the dataset, including itself. In our analysis, we focus on $i \neq j$ to exclude self-similarity.

By anchoring the similarity matrix at the initial year, we mitigate potential endogeneity concerns related to dynamic changes in firms' export behaviors. Firms may alter their product mixes over time due to strategic decisions, technological advancements, or shifts in consumer demand. By using the fixed product market proximity from the base year, we ensure that our measure reflects the inherent similarities between firms at the starting point of our analysis.

Appendix B. Calculating Technological Proximity

Our measure of technological proximity is constructed in an analogous manner. We utilize patent data provided by the China National Intellectual Property Administration (CNIPA), which classifies all patents into 605 distinct 4-digit technology classes. Among the 14,760 firms in the merged ASIF-CCTS data for the year 2000, 1,605 firms have patent records in any year between 1995 and 2000. For each of these patenting firms, we calculate the average share of patents in each technology class over the period from 1995 to 2000. This results in a vector of technological activity for each patenting firm, denoted by $\mathbf{T}_i = (T_{i1}, T_{i2}, \dots, T_{i605})$, where T_{ik} is the share of patents of firm i in technology class k . Each element of this vector captures the proportion of a firm's patenting activity in a specific technology class.

To ensure comparability across firms with different scales of patenting activity, we normalize each firm's technology vector by its Euclidean norm. Specifically, for each patenting firm i , the Euclidean norm is defined as:

$$\|\mathbf{T}_i\| = \sqrt{\sum_{k=1}^K T_{ik}^2} \quad ,$$

where $K = 605$ is the common set of technology classes; classes in which firm i holds no patents enter the vector with a value of zero. We then normalize each element of firm i 's technology vector by dividing by this Euclidean norm, yielding a normalized technology vector:

$$\mathbf{T}_i^{norm} = \left(\frac{T_{i1}}{\|\mathbf{T}_i\|}, \frac{T_{i2}}{\|\mathbf{T}_i\|}, \dots, \frac{T_{iK}}{\|\mathbf{T}_i\|} \right) \quad .$$

This normalization ensures that the similarity measure depends on the distribution of patents across technology classes rather than the total number of patents.

Following the methodology of [Bloom et al. \(2013\)](#) and [Jaffe \(1986\)](#), the technological proximity between any two patenting firms i and j , denoted $TECH_{ij}$, is calculated as the inner product of their normalized technology vectors:

$$TECH_{ij} = \mathbf{T}_i^{norm} \mathbf{T}_j^{norm} \quad (5)$$

This measure reflects the cosine of the angle between the two firms' technology share vectors in the technological space.

Given that there are 1,605 patenting firms, we construct a technological proximity matrix of size $1,605 \times 1,605$. Each firm is paired with every other patenting firm, resulting in a square matrix where each cell $TECH_{ij}$ represents the technological proximity between firm i and firm j . As with the product market proximity measure, we fix this technological similarity matrix at the initial period (1995–2000) to ensure that our measure is not influenced by subsequent changes in firms' patenting behaviors over time.

Appendix C. Mahalanobis Extensions

The Jaffe correlation measures similarity within the same product or technology class but assumes independence across classes. To address this limitation, we adopt the Mahalanobis extension, following [Bloom et al. \(2013\)](#), which incorporates correlations between classes to capture indirect relationships between firms patenting in related fields.

Specifically, to calculate Mahalanobis technological similarity, we first construct a normalized technology vector $\mathbf{T}_i^{\text{norm}}$ for each firm i , where:

$$\mathbf{T}_i^{\text{norm}} = \left(\frac{T_{i1}}{\|\mathbf{T}_i\|}, \frac{T_{i2}}{\|\mathbf{T}_i\|}, \dots, \frac{T_{iK}}{\|\mathbf{T}_i\|} \right),$$

with $\|\mathbf{T}_i\|$ being the Euclidean norm of the firm's technology vector: $\|\mathbf{T}_i\| = \sqrt{\sum_{k=1}^K T_{ik}^2}$. Here, T_{ik} represents the share of firm i 's patents in technology class k , and K is the total number of technology classes.

Next, we construct the cross-class correlation matrix Ω , which captures the interdependencies between technology classes. We first form the raw cross-class matrix from firms' un-normalized patent shares and then normalize each entry by the norms of the two corresponding class columns:

$$\Omega_{kl} = \frac{\sum_{i=1}^N T_{ik} T_{il}}{\sqrt{\sum_{i=1}^N T_{ik}^2} \sqrt{\sum_{i=1}^N T_{il}^2}}.$$

Here, Ω is a $K \times K$ matrix, where K is the total number of technology classes and T_{ik} is the un-normalized share of firm i 's patents in class k . By construction the diagonal entries equal one, and each off-diagonal element Ω_{kl} is the uncentered correlation between the patent-share profiles of classes k and l across firms. Intuitively, Ω reflects the broader structure of technological relationships: a high Ω_{kl} value indicates that classes k and l tend to be used by the same firms and are therefore strongly related, for example, semiconductors and microprocessors, whereas a low Ω_{kl} value suggests that the two classes are weakly related or

rarely appear together.

The Mahalanobis technological similarity between firms i and j , denoted as $\text{TECH}_{ij}^{\text{Mahalanobis}}$, is then computed as:

$$\text{TECH}_{ij}^{\text{Mahalanobis}} = \mathbf{T}_i^{\text{norm}} \Omega \mathbf{T}_j^{\text{norm}\prime}.$$

This measure captures both direct overlaps in patent portfolios and the broader relationships between the technology classes in which the firms operate. For instance, if firms i and j do not patent in exactly the same classes, their similarity can still be high if the classes they specialize in are strongly correlated in Ω .

By incorporating Ω , the Mahalanobis extension provides a richer and more nuanced measure of technological proximity, going beyond the traditional Jaffe correlation to account for the complex interdependencies in the technological landscape.

An analogous measure is also constructed to capture proximity between firms in the product market. This measure uses firms' export compositions across product categories, normalized in a manner analogous to the technological similarity calculation. The Mahalanobis extension is applied to account for interdependencies between product categories, ensuring that relationships between related but distinct product groups are incorporated. This allows the product market proximity to reflect both direct overlap in export portfolios and indirect connections through correlated product categories.

Appendix D. Construction of the Prediction-Based Instrument

To construct the prediction-based instrument, we estimate a case-level model of plaintiff victory using only pre-determined and observable case and plaintiff characteristics. Formally, we estimate:

$$\begin{aligned} \text{Win}_{ct} = & \Gamma_1 \log \text{size}_f^{\text{initial}} + \Gamma_2 \text{Jurisdiction}_c + \Gamma_3 \text{ForeignPlaintiff}_c \\ & + \Gamma_4 \text{CaseType}_c + \delta_{\text{city}} + \delta_{\text{sector}} + \delta_{\text{year}} + \delta_{\text{judge}} + u_c, \end{aligned} \quad (6)$$

where Win_{ct} is a dummy equal to one if the plaintiff wins case c in year t . The set of covariates includes the plaintiff’s initial firm size (measured by log total assets), the jurisdiction level of the adjudicating court (Primary, Intermediate, or Higher), a dummy for whether the plaintiff is a foreign firm, and the type of IP dispute (Patent, Trademark, Copyright, or Other). City, sector, year, and judge fixed effects absorb unobserved heterogeneity across local judicial environments, industries, time, and judges. The city fixed effects refer to the prefectural-level city where the alleged infringement occurred, typically corresponding to the defendant’s location, thereby capturing variation in local judicial environments. The sector fixed effects are based on the industry classification of the plaintiff firm, controlling for differences in litigation patterns across sectors. Standard errors are heteroskedasticity robust.

Table A1 reports two complementary case-level regressions that predict plaintiff victory. Column (1) presents our prediction-based specification in Eq. (6), which relates litigation outcomes to pre-determined case and plaintiff characteristics. The results indicate that plaintiffs with larger initial firm size and cases involving copyright disputes are significantly more likely to win, while cases adjudicated in higher-level courts are associated with a lower probability of plaintiff victory. The coefficient on foreign plaintiffs is negative but statistically insignificant. Column (2) reports the judge-panel leave-one-out measure, $\hat{p}_c^{\text{panel}}$, which is strongly predictive of plaintiff victory, supporting the relevance of the judge-based instrument.

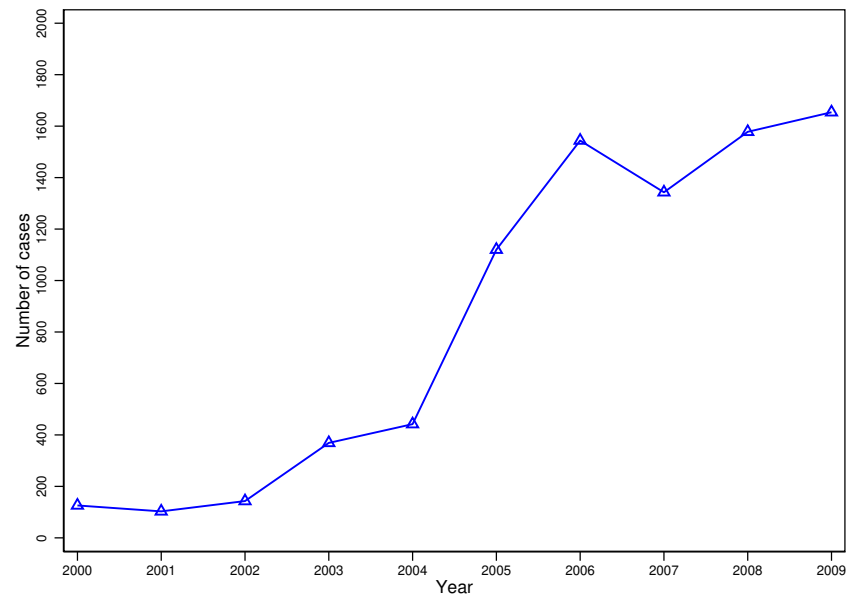
Table A1: Predicting Plaintiff Victory: Observable Characteristics vs. Judge-Panel Leniency

	(1)	(2)
	Characteristic-based prediction	Judge-based prediction
Dependent variable:	Win_{ct}	
$\log \text{size}_f^{\text{initial}}$	0.024*** (0.004)	
$\hat{p}_c^{\text{panel}}$		1.758*** (0.184)
Intermediate Court	-0.196 (0.136)	0.029 (0.120)
High Court	-0.277* (0.150)	-0.224* (0.129)
Foreign plaintiff	-0.017 (0.014)	
Casetype : Patent	0.022 (0.052)	0.050 (0.214)
Casetype : Trademark	0.077 (0.050)	0.360* (0.214)
Casetype : Copyright	0.174*** (0.055)	0.578** (0.237)
Observations	4,434	4,101
City fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Sector fixed effects	Yes	No
Judge fixed effects	Yes	No
Adj. R^2 / Pseudo- R^2	0.171	0.0927

Notes: The dependent variable equals one if the plaintiff wins at least one claim in case c . Column (1) reports a linear probability model using pre-determined case and plaintiff characteristics and absorbs city, year, sector, and judge fixed effects. Column (2) reports a case-level probit relating plaintiff victory to judge-panel leniency, $\hat{p}_c^{\text{panel}}$, controlling for court level, dispute type, city, and year. $\log \text{size}_f^{\text{initial}}$ is the log of the plaintiff's total assets measured in the initial observation period. The omitted category for court jurisdiction level is *Primary Court*. The omitted category for dispute type is *Other IPR*. Column (1) reports heteroskedasticity-robust standard errors; column (2) clusters standard errors by case. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

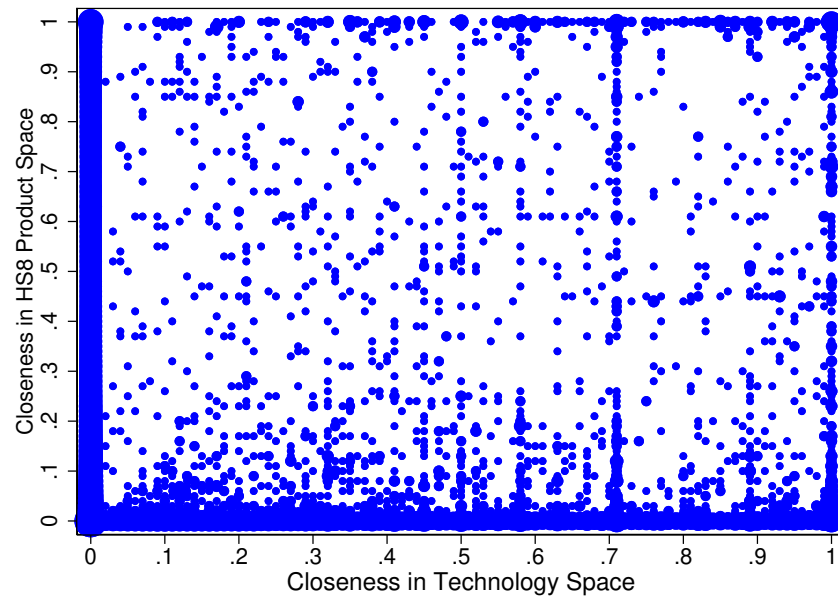
Appendix E. Additional figures and tables

Figure A1: Number of IP Litigation Cases in Chinese Courts Over Time



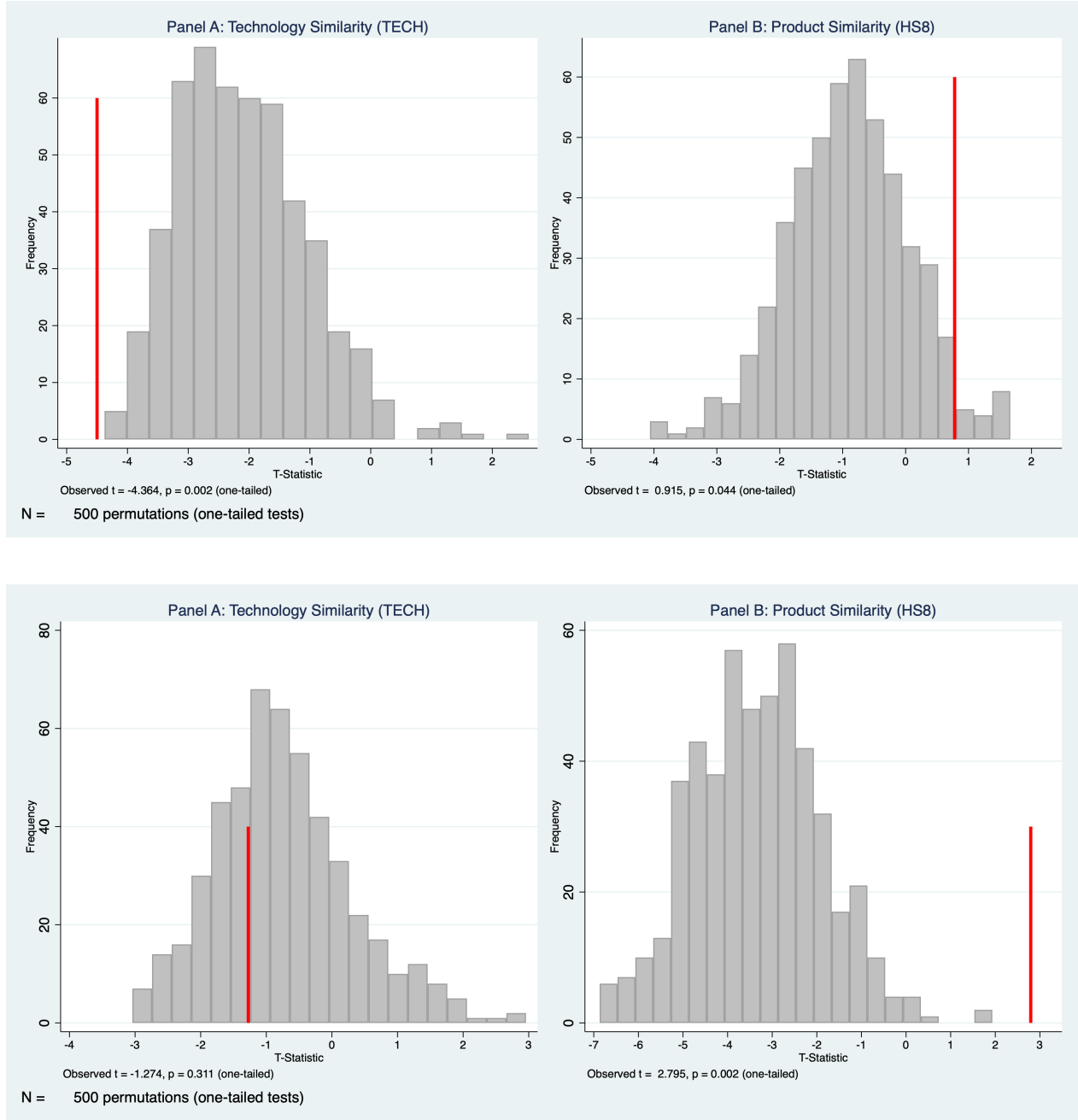
Notes: This figure plots the annual number of IP litigation cases in our court-case database. The counts include corporate IP disputes with a recorded court judgment and cover patents, trademarks, copyrights, domain names, trade secrets, and other IP cases. The figure shows a marked increase in case volume over the sample period.

Figure A2: Product-Space vs. Technology-Space Similarity Across Firm Pairs



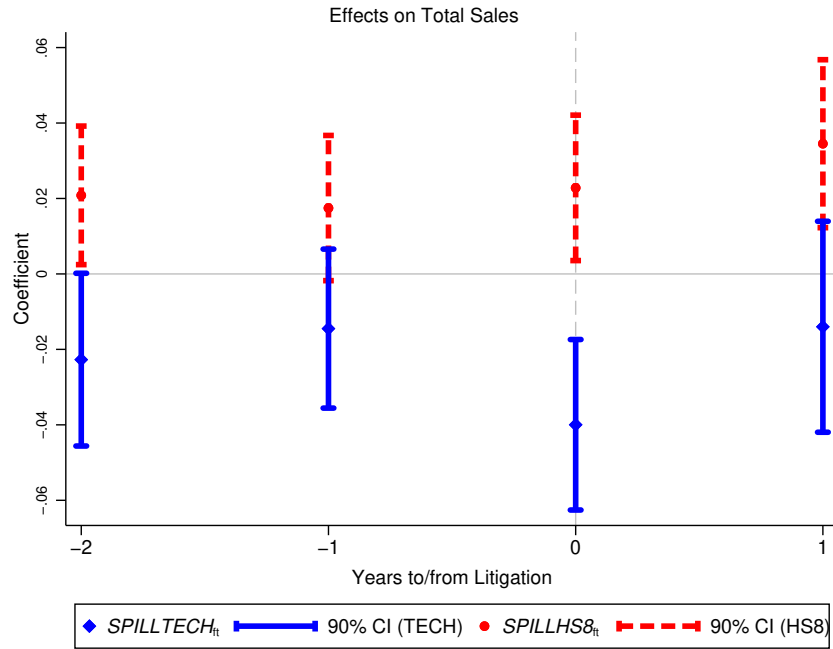
Notes: This figure plots pairwise HS8 product-space similarity against patent-based technology similarity for all firm pairs. The two measures exhibit no meaningful association (correlation = 0.028), confirming that product-market proximity and technology proximity capture distinct dimensions of firm relatedness in our empirical design.

Figure A3: Permutation Test: distribution of T-statistic for domestic sales and exports



Notes: These figures present results from permutation tests with 500 random reassignments of litigation status. The upper pair of panels uses log domestic sales as the outcome, and the lower pair uses log exports. Within each pair, Panel A reports technology-space exposure ($SPILLTECH_{ft}$), and Panel B reports product-market exposure ($SPILLHS8_{ft}$). Histograms show the distribution of t-statistics from the permuted data, and red vertical lines indicate the observed t-statistics. P-values shown below each panel represent the proportion of permuted statistics as extreme as or more extreme than the observed statistic in the relevant tail.

Figure A4: Dynamic Spillover Effects on Total Sales



Notes: The figure plots dynamic coefficients for total spillover exposure to resolved IP cases, using log total sales as the outcome. The horizontal axis reports years relative to the court judgment year ($t = 0$). Error bars show 90 percent confidence intervals. Estimates come from a dynamic specification with firm, city-year, and 2-digit sector fixed effects, with standard errors clustered at the firm level. The sample excludes firms that ever litigate.

Table A2: Case Outcomes and Litigants' Own Sales: Descriptive Evidence

Dep.Var. (log)	Total sales (1)	Domestic sales (2)	Exports (3)
Win _{ft}	0.035* (0.018)	0.040* (0.022)	0.118* (0.066)
Lose _{ft}	0.042* (0.023)	0.045 (0.033)	-0.064 (0.109)
<i>N</i>	14,015	13,513	4,631
Firms	1,978	1,944	843
Firm, City-year, Sector FE	✓	✓	✓

Notes: This table reports descriptive estimates of own litigation on litigants' outcomes using the broader litigant sample, which is not restricted to firms already present in 2000. The sample includes only firms that ever litigate. Win_{ft} and Lose_{ft} identify clean winning and clean losing litigant-years and are entered jointly. Mixed or ambiguous dispute years remain in the omitted category together with non-dispute years. All regressions include firm, city-year, and 2-digit sector fixed effects. Robust standard errors clustered at the firm level are reported in parentheses. Asterisks denote significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Spillover Effects of IP Court Rulings on Export Margins

Dep.Var. (log)	Exports/# HS8 (1)	Exports/# Destination (2)	# HS8 (3)	# Destinations (4)
SPILLTECH _{ft}	-0.036 (0.037)	-0.053* (0.030)	-0.014 (0.019)	0.004 (0.018)
SPILLHS8 _{ft}	0.064** (0.030)	0.067** (0.028)	0.030** (0.015)	0.028* (0.015)
<i>N</i>	77,564	77,564	77,564	77,564
Adj. R^2	0.667	0.649	0.708	0.760
Firm, City-year, Sector FE	✓	✓	✓	✓

Notes: The regressors are the total technology-space (SPILLTECH_{ft}) and product-market (SPILLHS8_{ft}) exposure measures used in the baseline specification. All specifications exclude firms that ever litigate and include firm, city-year, and 2-digit sector fixed effects. Robust standard errors clustered at the firm level are shown in parentheses. Asterisks denote significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Spillover Effects of IP Court Rulings by Litigant Ownership

Dep.Var. (log)	Domestic sales	Exports
	(1)	(2)
$\text{SPILLTECH}_{ft}^{\text{domestic}}$	-0.090*** (0.026)	0.008 (0.047)
$\text{SPILLHS8}_{ft}^{\text{domestic}}$	0.028 (0.036)	0.117** (0.050)
$\text{SPILLTECH}_{ft}^{\text{foreign}}$	-0.116*** (0.031)	-0.095* (0.050)
$\text{SPILLHS8}_{ft}^{\text{foreign}}$	0.018 (0.033)	0.076* (0.044)
N	79,881	77,564
Adj. R^2	0.798	0.671
Firm, City-year, Sector FE	✓	✓

Notes: This table separates spillover exposure by litigant ownership. $\text{SPILLTECH}_{ft}^{\text{domestic}}$ and $\text{SPILLHS8}_{ft}^{\text{domestic}}$ measure exposure to domestically owned litigants; $\text{SPILLTECH}_{ft}^{\text{foreign}}$ and $\text{SPILLHS8}_{ft}^{\text{foreign}}$ measure exposure to foreign-owned litigants. Domestically owned firms include SOEs and private domestic firms; foreign-owned firms include firms from Hong Kong, Macao, and Taiwan and other foreign-invested firms. All specifications exclude firms that ever litigate and include firm, city-year, and 2-digit sector fixed effects. Robust standard errors clustered at the firm level are reported in parentheses. Asterisks denote significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Spillover Effects of IP Court Rulings by Non-Litigant Ownership

Dep.Var. (log)	Domestic sales	Exports	Domestic sales	Exports
	Domestic firms		Foreign firms	
	(1)	(2)	(3)	(4)
SPILLTECH _{ft}	-0.186*** (0.047)	-0.033 (0.098)	-0.056** (0.026)	-0.064 (0.043)
SPILLHS8 _{ft}	0.025 (0.044)	0.096 (0.066)	0.027 (0.031)	0.092** (0.040)
<i>N</i>	25,944	20,640	53,308	56,284
Adj. <i>R</i> ²	0.829	0.636	0.773	0.677
Firm, City-year, Sector FE	✓	✓	✓	✓

Notes: This table splits the sample by the non-litigating firm's ownership in its first observed year. Domestically owned firms comprise SOEs and private domestic firms; foreign-owned firms comprise firms from Hong Kong, Macao, and Taiwan and other foreign-invested firms. All specifications exclude firms that ever litigate and include firm, city-year, and 2-digit sector fixed effects. Robust standard errors clustered at the firm level are reported in parentheses. Asterisks denote significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Matched Permutation Test

Dep.Var. (log)	Channel	Observed <i>t</i>	Placebo distribution		One-tailed <i>p</i>
			Mean	S.D.	
Total sales	SPILLTECH _{ft}	-3.666	-1.337	1.073	0.012
	SPILLHS8 _{ft}	2.147	0.817	1.509	0.182
Domestic sales	SPILLTECH _{ft}	-4.364	-2.487	0.926	0.008
	SPILLHS8 _{ft}	0.915	0.031	1.085	0.210
Exports	SPILLTECH _{ft}	-1.274	-1.081	0.903	0.434
	SPILLHS8 _{ft}	2.795	1.005	1.398	0.094

Notes: This table reports results from 500 placebo draws in which counterfactual litigants are matched to actual litigants on pre-determined 2000 characteristics. Firms in the similarity universe are sorted into propensity-to-litigate deciles estimated from log sales, employment, total assets, exporter status, and two-digit sector. Within each year, the number of placebo litigants drawn from each decile equals the number of actual litigants in the same year-decile cell. Both exposure measures are reconstructed for every draw, and equation (1) is re-estimated with firm, city-year, and two-digit sector fixed effects and standard errors clustered at the firm level. Observed t-statistics use the same samples and specifications. The one-tailed *p*-value is the share of placebo draws with a t-statistic at least as extreme as the observed statistic in the direction of the observed estimate.

Table A7: Market-Specific Spillover Effects: IV Estimates

Dep.Var. (log)	Domestic Sales		Exports	
	Judge-panel leniency IV	Predicted win prob. IV	Judge-panel leniency IV	Predicted win prob. IV
	(1)	(2)	(3)	(4)
SPILLTECH _{ft} ^{win}	-0.112*** (0.027)	-0.111*** (0.027)	-0.053 (0.044)	-0.060 (0.044)
SPILLHS8 _{ft} ^{win}	0.037 (0.031)	0.038 (0.030)	0.087** (0.044)	0.131*** (0.042)
Observations	79,881	79,881	77,564	77,564
Weak-ID (KP F)	6,791	7,949	4,992	6,199
Firm, City-year, Sector FE	Yes	Yes	Yes	Yes

Notes: Columns (1) and (3) instrument winner-based exposure using case-level plaintiff win probabilities from the probit in equation (3), whose principal predictor is judge-panel leniency constructed from the leave-one-out propensities in equation (2). Columns (2) and (4) use fitted probabilities from an LPM based on pre-determined case and litigant characteristics, with city, year, sector, and judge fixed effects. In all columns, the fitted probability is assigned to the plaintiff and one minus this probability to the defendant. These probabilities are then averaged across cases within each litigant-year and weighted by pre-determined technology- or product-space similarity to construct firm-year instruments. Columns (1) and (2) use log domestic sales as the outcome; columns (3) and (4) use log exports. The corresponding case-level prediction regressions are reported in Table A1. Weak-ID (KP F) reports the Kleibergen–Paap rk Wald F statistic for weak identification. All specifications exclude firms that ever litigate and include firm, city-year, and 2-digit sector fixed effects. Robust standard errors clustered at the firm level are reported in parentheses. Asterisks denote significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Spillover Effects of IP Litigation on Firms' Total Sales: Additional Robustness

Dep.Var. (log)	Total Sales						
	(1) Mahalanobis similarity	(2) Policy controls	(3) Aggregate litigation	(4) Alt. init. yr.	(5) First instance	(6) Alt. def. win.	(7) HS4
$SPILLTECH_{ft}^{Mah}$	-0.064*** (0.016)						
$SPILLHS8_{ft}^{Mah}$	0.025** (0.012)						
$SPILLTECH_{ft}$		-0.063*** (0.018)	-0.069*** (0.018)	-0.055*** (0.014)	-0.076*** (0.021)		-0.070*** (0.018)
$SPILLHS8_{ft}$		0.027** (0.013)	0.029** (0.013)	0.027** (0.014)	0.033** (0.015)		
$SPILLTECH_{ft}^{win,alt}$						-0.061*** (0.017)	
$SPILLHS8_{ft}^{win,alt}$						0.031** (0.013)	
$SPILLHS4_{ft}$							0.021*** (0.007)
index_tax		0.172*** (0.006)					
index_subsidy		0.088*** (0.006)					
index_interest		-0.056*** (0.007)					
litigation_count			0.007*** (0.002)				
N	97,607	88,870	97,607	97,607	91,668	97,607	97,607
Adj. R^2	0.859	0.883	0.859	0.859	0.859	0.859	0.859
Firm, City-year, Sector FE	✓	✓	✓	✓	✓	✓	✓

Notes: Column (1) reconstructs the exposure measures using Mahalanobis-based similarity in technology and product. Column (2) augments the baseline regression with policy controls: a tax index, a subsidy index, and an interest-rate support index. Column (3) adds the number of IP cases in the firm's city-sector-year. Column (4) dates exposure to the filing year rather than the judgment year. Column (5) reconstructs exposure using only first-instance cases. Column (6) reconstructs winner-based exposure using an alternative definition of plaintiff victory that requires the plaintiff to win at least half of all claims. Column (7) replaces HS8 similarity with HS4 similarity when constructing the product-market spillover measure. All specifications exclude firms that ever litigate and include firm, city-year, and 2-digit sector fixed effects. Robust standard errors clustered at the firm level are reported in parentheses. Asterisks denote significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Spillover Effects of IP Court Rulings on Non-Litigants' R&D

Dep.Var.	$\mathbf{1}\{RD_{ft} > 0\}$ (1)	$\log RD_{ft}$ (2)	$\mathbf{1}\{RD_{ft} > 0\}$ (3)	$\log RD_{ft}$ (4)
SPILLTECH _{ft}	0.001 (0.012)	0.032 (0.073)		
SPILLHS8 _{ft}	0.002 (0.010)	0.032 (0.066)		
SPILLTECH _{ft} ^{win}			-0.001 (0.013)	0.046 (0.078)
SPILLHS8 _{ft} ^{win}			-0.007 (0.012)	-0.025 (0.074)
SPILLTECH _{ft} ^{lose}			0.026 (0.044)	-0.026 (0.257)
SPILLHS8 _{ft} ^{lose}			0.034* (0.018)	0.247** (0.120)
<i>N</i>	35,458	35,442	35,458	35,442
Adj. <i>R</i> ²	0.575	0.665	0.575	0.665
Firm, City-year, Sector FE	✓	✓	✓	✓

Notes: Columns (1) and (3) use an indicator for positive R&D expenditure, estimated as linear probability models; columns (2) and (4) use $\log(1 + RD_{ft})$. The outcomes are defined for firm-year observations in which R&D expenditure is reported. Columns (1)–(2) use total technology-space and product-market exposure; columns (3)–(4) jointly include the corresponding winner- and loser-based exposure measures. All specifications exclude firms that ever litigate and include firm, city-year, and 2-digit sector fixed effects. Robust standard errors clustered at the firm level are reported in parentheses. Asterisks denote significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: Spillover Effects of IP Court Rulings by Destination IP Protection and Income

Dep.Var.	Exports _{fdt}	
	(1)	(2)
	Ginarte-Park	Log GDP per capita (PPP)
SPILLTECH _{ft} × GP _{dt}	0.018 (0.051)	
SPILLHS8 _{ft} × GP _{dt}	0.044 (0.028)	
SPILLTECH _{ft} × log GDPpc _{dt} ^{PPP}		0.002 (0.039)
SPILLHS8 _{ft} × log GDPpc _{dt} ^{PPP}		0.042 (0.028)
<i>N</i>	1,419,348	1,345,888
Pseudo <i>R</i> ²	0.916	0.918
Firm-destination FE	✓	✓
Firm-year FE	✓	✓
Destination-year FE	✓	✓

Notes: The dependent variable is firm-destination-year exports, and both specifications are estimated by PPML. Column (1) interacts spillover exposure with the Ginarte-Park index of destination-level IP protection, denoted GP_{dt}. Column (2) interacts spillover exposure with destination log GDP per capita in PPP terms, denoted log GDPpc_{dt}^{PPP}. All specifications absorb firm-destination, firm-year, and destination-year fixed effects, so identification comes from within-firm-year variation in export allocation across destinations. Because the main spillover measures are absorbed by firm-year fixed effects and the destination characteristics are absorbed by destination-year fixed effects, only the interaction terms are reported. Robust standard errors clustered at the firm level are reported in parentheses. Asterisks denote significance: **p* < 0.1, ***p* < 0.05, ****p* < 0.01.

Table A11: Spillover Effects of IP Court Rulings on Export Unit Values and Quality

Dep.Var. (log)	Export unit value	Export quality
	(1)	(2)
SPILLTECH _{ft}	-0.006 (0.014)	-0.006 (0.012)
SPILLHS8 _{ft}	0.020 (0.012)	0.007 (0.012)
<i>N</i>	1,423,473	1,423,473
Adj. <i>R</i> ²	0.720	0.320
Firm FE	✓	✓
Destination-year FE	✓	✓
HS4-year FE	✓	✓

Notes: This table reports estimates at the firm–HS6–destination–year level. Column (1) uses log export unit value, calculated as export value divided by quantity. Column (2) uses demand-inferred log export quality. Following [Khandelwal et al. \(2013\)](#), quality is recovered from residual demand conditional on unit value, using the country-sector elasticity estimates of [Imbs and Méjean \(2017\)](#) mapped to HS6 products. Both columns use total technology-space and product-market exposure and include firm, destination-year, and HS4-year fixed effects. The sample includes observations with positive export value and quantity and excludes firms that ever litigate. Robust standard errors clustered at the firm level are reported in parentheses. Asterisks denote significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.