Firm Trade Resilience and Credit Access *

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Abstract

This study examines how firms navigate trade disruptions and how credit access shapes their response. I obtain identification through anti-dumping investigations against Chinese exports, which target a particular product in a particular market. Using customs data matched with firm tax records, I find hit firms reduce exports of the targeted product to the initiating market, but redirect it toward other markets and keep their other products in the affected market, while retreating from products and markets not subject to the shock. Their export revenue increases, but profitability does not, due to rising costs of production, sales, and financing. Exploiting the staggered rollout of the VAT rebate loan program across Chinese cities, I find that hit firms with loan access maintain the affected export relationship, drop fewer unrelated export lines, and face no rise in the cost of debt. These findings suggest that credit access cushions adverse tariff shocks.

Keywords: Trade Barriers, Trade Diversion, Credit Access, Government Credit Policy
JEL Codes: F13, F14, G32, G38

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1. Introduction

Geopolitical tension is on the rise. Such tension often manifests into trade frictions such as the U.S.-China trade war. While many studies have looked into the effect of trade frictions or protective trade policies on U.S. importers (Crosignani et al., 2026; Ayyagari et al., 2024; Alfaro et al., 2025), little attention has been paid to foreign exporters. Understanding how exporter firms adjust to trade barriers can help us evaluate the effectiveness of protectionist policies and their effects on the balance of economic power across countries.

This paper investigates how Chinese firms respond when foreign trade barriers disrupt an existing market and how access to financing shapes the way firms reorganize their businesses. As China is the largest trade partner of the U.S., their responses have crucial implications for U.S. foreign and industrial policies. To answer this question, I combine detailed Chinese customs data with anti-dumping (AD) investigations against Chinese exports. An AD investigation targets a particular product in a particular foreign market, allowing me to trace how the affected firm reallocates sales across the rest of its product and destination portfolio.

I find that exporters reduce exports of the affected product to the market that initiates the trade barrier, but respond by redirecting the targeted product toward markets not subject to the barrier. These adjustments are costly, and resources within a firm are limited. This explains why a trade barrier leads firms to retreat from products and markets that are not directly hit by the disruption. Compared with similar never-hit firms, hit firms' export revenue increases, but their profitability does not change, due to rising costs of production, sales, and financing.

Importantly, I further examine how access to credit shapes this adjustment. I exploit the staggered rollout across Chinese cities of a government-facilitated policy innovation, the value-added tax (VAT) rebate loan program. Chinese exporters are entitled to substantial VAT rebates, but these payments can arrive with long delays, tightening corporate liquidity. The program allows exporters to use these unpaid rebates as collateral for short-term bank

loans, while lending decisions remain with commercial banks. The policy simply broadens the collateral space by making rebates pledgeable. Program adoption is plausibly exogenous to firms' adjustments to trade disruptions, as city-level characteristics do not predict the timing of adoption. I show that firms with loan access are more likely to maintain the affected export relationship, thereby preserving their comparative advantage. These results suggest that credit access cushions adverse tariff shocks by providing credit at lower cost.

I study this question in the context of AD investigations against Chinese firms for three reasons. First, China manufactures nearly the full range of traded products and ships them to nearly every destination, allowing me to observe all possible forms of adjustment. Second, China's setting offers detailed trade records, which are difficult to obtain in other contexts. Third, anti-dumping tariffs were the major obstacle facing Chinese exporters before the U.S.–China trade war. As trade in almost all countries encounters some level of disruption, the findings of this paper have important implications for firms in other countries.

I leverage several unique datasets to investigate the relationship among trade barriers, credit access, and exporters' adjustment. First, I use transaction-level customs data from the General Administration of Customs of China, covering the universe of Chinese exports from 2000 to 2015, with detailed information on exporters' names, products, and destinations. I aggregate the trade records to the firm–product (HS6)–destination–year level. These data allow me to capture both the extensive margin (i.e., whether a firm exports a product to a destination) and the intensive margin (i.e., export quantity, value, and unit price) of export decisions. Next, I link firms in the customs records to the Annual Tax Survey (ATS) database and obtain firm-level information, including financing, investment, and profitability. Finally, I gather and parse the timing and geography of the rollout, as well as the institutional details of the program, from government policy documents and local media reports.

I start by studying the direct impact of AD investigations on the targeted flow, namely the investigated product sold to the country that initiated the case. I compare this flow with the same firm's exports of the same product to its other destinations, before and

after the investigation. The AD investigation lowers the likelihood that the firm exports the targeted product to the initiating country by 2.6 percentage points. Conditional on exporting, quantity falls by 9% and the unit price rises by 10%, so export value does not change. Importantly, this effect is absent in the pre-event period and emerges immediately after the investigation.

My analysis includes firm–product–year fixed effects, which enable me to compare exports of the same product by the same firm to different countries at the same time. These fixed effects absorb time-varying firm conditions, the firm's production capability in a given product, and changes in the firm's overall strategy. I also include firm–country–product fixed effects, which allow me to track a firm's exports of a specific product to a particular country over time. These fixed effects help address concerns related to firmdestination country matching and firmproduct matching, as well as changes in global product demand dynamics following the AD investigations.

After the onset of an AD investigation, firms also respond to the trade disruption by making cross-product and cross-market adjustments. I compare the hit firm's other product–country flows with the same flows of never-hit firms, matched on export growth over the three years before the investigation. Firms hit by an investigation are 24 percentage points less likely to exit the investigated product from another destination than matched never-hit firms. Conditional on the flows they retain, hit firms increase export quantity by 18% and export value by 19% more than never-hit firms, at unchanged prices.

Besides increasing sales of the targeted product in other markets, firms may also attempt to increase sales of other products in the affected market. Indeed, hit firms are also 15 percentage points less likely to drop their other products from the initiating country, though they do not ship more of them there. These findings show that firms view the AD investigation as a barrier on one product in one market, and respond by selling that product in the other markets they already serve and keeping their other products in the initiating country.

The AD investigation is a disruptive event for the firm as a whole, not only for the

targeted product and market. Its impact spills over to flows not targeted by the investigation. Compared with never-hit firms, hit firms are 11 percentage points less likely to drop a product–destination line that shares neither the product nor the destination with the case. However, conditional on exporting, hit firms ship 8% less in quantity and 6% less in value. This decline fades four years after the shock. This pattern is consistent with the view that navigating an adverse shock in one product and one market crowds out management attention, sales effort, and working capital from other products and markets.

Do these adjustments pay off? To answer this question, I turn to the firm as a whole and compare the performance of hit firms with that of never-hit firms in the same industry, matched on size and export growth over the three years before the investigation. After the investigation, hit firms' export revenue rises by approximately a third and value added increases by 12%, but, surprisingly, profitability does not change. This can be explained by the accompanying higher costs of raw materials, labor, sales, and additional financing. Indeed, firms hit by an AD investigation experience an 8% increase in cost of goods sold, a 15% increase in selling expense, and a 7% increase in administrative expense.

Hit firms also borrow more to finance the costly adjustments. Their interest expense rises by 13%, current liabilities by 7%, and the cost of debt by 0.1 percentage points. The increase in the cost of debt is substantial, around 6% of the sample average of 1.8%. The absence of pre-trends reassures that hit firms were not already on a different growth path. Taken together, rerouting is an effective but costly response, and part of the cost is financed with more expensive short-term debt.

If borrowing becomes more expensive when firms adjust to disruptive forces, access to cheaper credit may change their response. I next ask whether access to credit changes this response. I use the staggered adoption of the VAT rebate loan program across Chinese cities and compare hit firms in cities that adopted the program before the investigation with hit firms in cities that never did. Credit can matter in three ways. It can keep firms in the affected market, it can let firms adjust on a larger scale, and it can keep firms from retreating

from the products and markets the case does not touch. I test each channel by adding a triple interaction between the hit indicator, the post-investigation indicator, and loan access to the corresponding specification. My analysis includes city–year fixed effects, which enable me to compare hit firms with never-hit firms in the same city and year.

Despite the rigorous fixed-effect structure, a remaining concern is that the decision to adopt the loan program may be influenced by city characteristics that simultaneously affect firms' adjustments to trade tensions. I take two steps to address this concern. First, I show that program adoption cannot be explained by local economic conditions, which makes it unlikely that firms anticipate the timing of adoption. None of the city characteristics, from exports and GDP to fiscal position and bank credit supply, predicts when a city adopts the program. Second, in the loan-related analyses, I drop cities that adopt the program after the AD investigation. This avoids contamination from a city's strategic response to the shock.

One underlying assumption behind the role of the loan program in firms' adjustments is that the program eases the financing costs of firms hit by the shock. Indeed, the cost of debt of hit firms without access rises by 0.2 percentage points after the investigation, and this increase is fully offset for hit firms with access.

How does access to the loan program facilitate firms' adjustments to trade tensions? If the program lowers financing costs, firms may make better adjustment decisions, which might in turn support higher profitability. I find that the decline in export likelihood is 7 percentage points smaller for hit firms with loan access than for hit firms without, which almost fully offsets the negative impact of the investigation. Importantly, this effect is absent in the pre-event period and emerges immediately after the investigation.

I do not find evidence that the loan program changes cross-product and cross-market adjustment. On the product–country flows not directly hit by the investigation, hit firms with access are 3.7 percentage points less likely to exit a line than hit firms without access. These results suggest that the positive impact of the loan program lies in retaining export lines (the extensive margin), rather than in increasing exports on the intensive margin, by

providing an additional credit option at lower cost.

In the last step of my analysis, I turn to firm-level performance and examine whether firms with and without access to the loan program perform differently in response to the trade disruption. I find no significant differences by credit access in export revenue, profitability, or costs. The lack of statistical power in the firm-level effects is likely due to the short four-year window imposed by data limitations. Staying in the affected market rather than exiting is undoubtedly a valuable option. When the trade disruption blows over, firms that stayed are better positioned to capture customer demand. Overall, the results suggest that financial access affects adjustments to trade disruption primarily through the extensive margin rather than the intensive margin. It helps firms secure more options rather than export substantially more.

This paper contributes to the growing literature on how firms adjust to geopolitical risk and trade disruptions, in the context of anti-dumping duties and trade-policy uncertainty (Bown and Crowley, 2007; Crowley et al., 2018; Felbermayr and Sandkamp, 2020; Bao et al., 2025; Crosignani et al., 2026). Existing studies show that anti-dumping duties deflect a large share of the targeted exports to third markets (Bown and Crowley, 2007), that they reduce exports and induce exit (Felbermayr and Sandkamp, 2020), and that trade-policy uncertainty deters entry into foreign markets (Crowley et al., 2018). Crosignani et al. (2026) show that firms hit by export controls struggle to form new trade relationships. The closest study is Bao et al. (2025), who use the same customs records and document that multiproduct firms shift exports across products and markets after an anti-dumping action. This strand of the trade literature explains within-firm reallocation with production capacity, competition, or strategic responses to policy.

Different from this literature, I go beyond export flows and examine firm performance, which brings a corporate-finance perspective to the study of trade disruptions. I show that cross-product and cross-market adjustments raise export revenue but not profit, because the firm pays for them with higher selling and administrative expenses and with more expensive

short-term debt. I also show that access to credit gives the firm the option to stay in the affected market. This option is likely valuable, because the cost of re-entering a market is high.

This paper also relates to the corporate-finance literature showing that financial constraints shape how firms allocate scarce resources across competing uses (Lamont, 1997; Almeida et al., 2004; Fresard, 2010; Campello et al., 2010; Giroud and Mueller, 2015, 2019; Barrot, 2016). Almeida et al. (2004) and Barrot (2016) study liquidity management and working-capital frictions, and Lamont (1997) and Giroud and Mueller (2015, 2019) study the allocation of capital across plants and divisions within the firm. In particular, Fresard (2010) shows that cash-rich firms gain market share from cash-poor rivals after import-tariff cuts. I extend this literature in two ways. First, instead of industry-level shocks, I exploit a less studied disruption that hits one product in one market, which makes reallocation across the firm's products and destinations directly observable. Second, I measure financial access by a firm's exposure to a plausibly exogenous credit program rather than by endogenous firm characteristics such as cash holdings. The finding that firms with financial access are better placed to stay where they are also adds to the view that financial access lets a firm gain from a disruption. Indeed, hit firms with access end with the same export revenue and profitability as hit firms without access, but keep the markets and product lines they already had.

More specifically, this paper relates to the literature on bank credit and exports (Paravisini et al., 2015, 2023; Xu, 2022). Paravisini et al. (2015) show that negative credit shocks reduce the intensive margin of exports, Paravisini et al. (2023) show that bank credit is destination-specific, and Xu (2022) shows that exposure to bank failures reduced export market shares for decades. These studies identify bank credit as a determinant of export performance through bank-side or crisis-wide contractions. My findings complement this literature by providing evidence on the role of credit on the extensive margin rather than the intensive margin. Credit does not change how much hit firms ship on the flows they keep, but it keeps them in their products and markets under uncertainty.

Finally, this paper relates to work on government credit, resource allocation, and government payment frictions (Bau and Matray, 2023; Kabir et al., 2024; Barrot and Nanda, 2020). Bau and Matray (2023) show that capital-account liberalization reduced misallocation. Kabir et al. (2024) show that the shutdown of the Export-Import Bank hurt high-marginal-product firms, and Barrot and Nanda (2020) show that faster government payments raise employment at constrained contractors. Different from traditional government intervention, the VAT rebate loan program is a government-led credit policy innovation in which banks keep full discretion over each lending decision. My findings show that this light-touch form of government-facilitated credit policy does not make hit firms export or earn more. It cushions them against giving up markets and product lines by keeping their cost of debt at its pre-shock level.

2. Institutional Background

2.1. Anti-Dumping Investigations against Chinese Exporters

Anti-dumping (AD) duties are tariffs that an importing country places on a foreign product it finds to be sold below fair value. A case starts when domestic producers file a petition against a product from a named country. The authority then investigates, issues a preliminary ruling, and issues a final ruling. The investigation typically takes six to twelve months. If the final ruling is affirmative, the duty applies to the named product from the named country, and to nothing else.

Two features of this process matter for my analysis. First, the shock is narrow. A case affects one product in one destination and leaves the firm's other products and other destinations untouched. This is what allows me to compare the targeted flow with the firm's own other flows and trace how the firm moves sales across them. Second, the outcome is uncertain while the case is pending. The firm learns the duty only at the final ruling, so during the investigation it has to decide whether to keep serving a market that may soon

close.

China has been the most frequent target of AD investigations in the world (Bown, 2016). Figure 1 plots the number of cases initiated against Chinese exporters in each year from 2006 to 2013, where a case is one initiating country and one HS6 product. There are 1,108 cases over these years. The count rises from 106 in 2006 to 228 in 2009, the year after the global financial crisis, and falls to below 100 a year from 2012.

Figure 1 plots the number of cases by year from 2006 to 2013. There are 1,108 country–product cases over these years. The count rises from 106 in 2006 to a peak of 228 in 2009 and falls to below 100 a year from 2012. Figure 2, Panels A and B, shows the distribution of these cases by targeted product and initiating country. Base metals account for 28% of cases, followed by textiles (17%), chemicals (11%), and machinery and electronics (11%). The United States, the European Union, and India each initiate 13% to 14% of cases, followed by Argentina, Brazil, and Turkey.

2.2. The VAT Rebate Loan Program

VAT rebates and rebate delays. Most countries levy a value-added tax (VAT) at each stage of production.[1] Firms pay VAT on their sales and receive credits for the VAT paid on their inputs. Exports are zero-rated, so an exporter is owed a refund of the input VAT on the goods it ships abroad. China introduced its export rebate in 1985 and has adjusted the rebate rate by product several times since 1994, using it to encourage or restrain the exports of particular goods. For most industrial goods, the rebate is between 10% and 13% of export value. Lu and Ma (2023) report an average rebate of about 12% of export value from 2007 to 2011. Because the rebate is a fixed share of export value, it is a substantial part of an exporter's cash flow.

Paying the rebate on time is difficult in practice, especially in developing countries (Pineda Pinto et al., 2024). In China, local governments financed 7.5% of export rebates

[1]The United States is the main exception (Bird and Gendron, 2007).

from 2005 to 2014 and had to fit the rebates into an annual quota within their approved budgets.[2] At the end of the fiscal year, a local government reports any shortfall, and the central government pays the remainder in the following year. Many firms therefore do not receive the full rebate they claim within the year. Lu and Ma (2023) document that more than 60% of Chinese exporters experience rebate delays. A delayed rebate is cash the firm has earned but cannot use. For an exporter that needs working capital, this cash is missing exactly when it is needed most. An AD investigation is one such moment. Sales in the targeted market fall while the firm has to pay for finding buyers and shipping goods elsewhere.

The program. The VAT rebate loan program facilitates an exporter's pledge of its unpaid rebate receivables as collateral for a short-term bank loan. The loan matures within one year, and its amount is capped at 70% to 90% of the outstanding receivable. In some cities, the local government subsidizes the interest, and annual rates of 4% to 5% are 200 to 300 basis points below the standard short-term lending rate. Figure A3 compares the standard rebate process (Panel A) with the process under the program (Panel B). The program does not change how much the firm is owed or when the government pays. It moves the cash forward to the time the firm needs it.

The amount of liquidity is meaningful. With a rebate of 10% to 13% of export value and a lending cap of about 80%, the program can lend a firm roughly 10% of its export sales.[3]

China first allowed rebate-backed loans in 2001, but banks rarely made them until local governments began to promote the program in the late 2000s. The biggest obstacle was information. Banks could not see the export and tax records held by the local bureaus, so they could not verify that a rebate claim was genuine and would not lend against it. Running the program therefore requires four parties, namely the firm, a commercial bank, the local tax bureau, and the local branch of the People's Bank of China (PBoC). After the firm applies, the customs and tax bureaus verify its rebate claim, the tax bureau designates

[2] From 2015, the central government pays the full rebate.

[3] This is comparable to the share of trade credit that U.S. firms typically obtain (Barrot and Nanda, 2020).

an escrow account, and the PBoC branch registers the loan. When the government pays the rebate, the money goes into the escrow account, and the bank takes principal and interest before releasing the rest to the firm.[4] The government verifies the collateral and coordinates the parties, but it does not directly provide credit to firms. Under China's Commercial Bank Law, the bank keeps full discretion over whether to make each loan. The program thus supports private credit rather than allocating public credit.

Why cities adopt. Cities adopted the program at different times. I hand-collect the adoption year and the stated motive of every prefecture-level city from official government documents, as described in Section 3.3. Of the 335 prefecture-level units, 182 adopted the program between 2008 and 2014, and 153 had not adopted it by 2014. Table A2 lists each adoption with its year and motive. Figure A6 maps the adoption year across cities. Adoption came in three waves, in 2008 to 2009, 2010 to 2011, and 2012 to 2014. Most of the early adopters are coastal export provinces, namely Tianjin, Jiangsu, Zhejiang, Fujian, and Guangdong, and most of the later adopters are inland.

Each adopting document states why the city introduces the program. Following the narrative approach of Romer and Romer (2010), I read each document and classify the stated motive into three categories, namely long-term development, a response to an export slowdown, and the relief of firms' credit constraints. A document can state more than one motive. Most documents cite long-term development. Fewer cite an export slowdown or firms' credit constraints. Figure A5 shows two examples. Shenzhen introduced an interest subsidy on rebate loans in 2009 to support exporters during the global financial crisis. Urumqi partnered with commercial banks in 2011 to lend up to 90% of rebate receivables at rates below the benchmark to inland exporters. Whether adoption reflects the economic conditions of a city is a question I test in Section 5.1.

[4] Figure A4 shows the seven steps from application to release.

3. Data and Sample

I combine four sources. Customs records give each firm's exports by product and destination. The Global Antidumping Database gives the AD cases initiated against Chinese exporters. Hand-collected city records give the adoption of the VAT rebate loan program. Firm tax returns give profits, costs, employment, and borrowing. This section describes each source and the samples I build from them. Appendix Appendix A defines every variable.

3.1. Customs Trade Data

Trade data come from China's General Administration of Customs, which records the universe of Chinese export transactions. For each transaction, the data report the exporting firm, the product at the HS8 level, the destination country, the year, the value in USD, the physical quantity, and the trade type, namely ordinary or processing trade. The data have been used by Lu et al. (2013) and Bao et al. (2025), among others. The raw files cover 2000 to 2016. I use 2007 to 2015, because the firm data begin in 2007 (Section 3.4) and the customs file for 2016 reports export value only, without quantity, so that unit prices cannot be computed for that year.

I clean the data in four steps, following Bao et al. (2025). First, I drop transactions with missing firm identifiers, product codes, or destination countries, and transactions with zero or missing export value. Second, I keep ordinary exports and drop processing trade, whose destinations and prices are largely set by the foreign partner that supplies the inputs and which may receive different treatment in AD investigations (Ludema et al., 2021). Third, I keep firms that can be linked to the tax survey described in Section 3.4, since the loan indicator requires the firm's city and the firm-level outcomes require its financial statements. The link uses a crosswalk between customs registration codes and tax survey identifiers, supplemented by matching on firm names. Appendix IA.3 provides a detailed description

of the linking and cleaning steps.[5] Fourth, I aggregate the transactions to the firm–HS6 product–destination–year level, summing value and quantity, with the member states of the European Union pooled as one destination.

The unit of observation is a flow, namely a firm–product–destination pair, followed over years. Of the linked firms, those with a tax survey return in the same year form the panel, which contains between 73,000 and 86,000 exporting firms in each year from 2009 to 2013. For each flow–year I observe whether the firm exports, the quantity, the value, and the unit price, defined as value divided by quantity. Quantity, value, and price are winsorized at the 0.1% and 99.9% levels.

3.2. Anti-Dumping Cases

Data on AD cases come from the World Bank Global Antidumping Database (Bown, 2016).[6] For each case, the database records the initiating country, the target country, the products covered, at levels ranging from HS4 to HS12, the initiation date, the decision date, and the outcome. I keep cases initiated against Chinese exporters and harmonize the product codes to the HS6 level of the trade data. A case is defined at the initiating country–HS6 product level, so that an investigation covering several products counts once for each product. When the same country–product pair is investigated more than once, I keep the first initiation. Because the trade data are annual and a case can be initiated at any point in the year, I follow Bao et al. (2025) and date a case initiated in the first six months of a year to that year, and a case initiated in the last six months to the following year. A case initiated late in the year is mainly investigated in the following year, since AD cases typically last six to twelve months. Cases initiated by the European Union count once for the Union.

I use the cases to build the samples for the two designs in Section 4. Appendix IA.3 details each step. Each sample covers three years before to four years after the investigation

[5] In each year from 2007 to 2015, the link covers between 37% and 58% of the firms that export in that year, and these firms account for between 61% and 79% of total export value. Coverage is highest in 2009 and falls in later years.

[6] The database is available at https://www.chadpbown.com/global-antidumping-database/.

year. I keep cohorts from 2009 to 2013, so that every cohort is observed for at least two years before and two years after the investigation. The direct-effect sample of Section 4.1 consists of firm–products that faced exactly one case and that the firm exported to the initiating country in at least one year of the sample period, restricted to active flows with positive exports in at least 40% of the window years observed in the data.

The trade diversion, spillover, and firm-level samples of Sections 4.2 to 4.4 compare hit firms with matched never-hit firms. A firm is hit if it exported the AD product to the initiating country in the initiation year of the case or later, for exactly one product–country pair. Its investigation year is the first such year, and firms with the same investigation year form a cohort. Never-hit firms are firms that never exported any AD product to its initiating country in the initiation year or later. The hit firm's flows other than the investigated one are matched to flows of never-hit firms in the same product, destination, and cohort, and hit firms are matched to never-hit firms in the same cohort and industry, as described in Sections 4.2 and 4.4.[7]

The firm-level sample contains 8,289 hit firms. In each year from 2009 to 2013, they make up between 1.6% and 2.4% of the exporting firms in the panel. Figure 2, Panels C and D, shows their distribution by the product and the initiating country of their case. The most common products are base metals (27%), machinery and electronics (21%), and textiles (12%). The most common initiating countries are the United States (43%), the European Union (22%), and India (16%). Compared with the cases in Panels A and B, the United States and the European Union account for a larger share of hit firms, and Argentina, Brazil, and Turkey for a smaller share. The reason is market size. More Chinese firms export to the United States and the European Union, so a case in either market hits more firms.

[7] Appendix Table IA.2 compares hit units with their matched controls on the matching variables and on other pre-investigation characteristics. On every matching variable, the standardized difference between hit units and their controls is below 0.06.

3.3. Loan Program Adoption

Section 2.2 describes the adoption of the VAT rebate loan program across cities. Here I describe how the adoption records are collected. I hand-collect the adoption year of each prefecture-level city through a systematic web search. For each city and each province, and for each year from 2005 to 2015, I search in Chinese for the name of the program together with the place name and the year, and screen the top 100 results. I give priority to official sources, namely provincial and municipal government websites and local branches of the People's Bank of China, over media coverage. Some adoptions are decisions of a single city, and others are provincial decisions that apply to every city in the province.[8] For each city, I record the earliest year in which the program became available to its exporters and the motive stated in the adopting document. Internet Appendix IA.3 describes how the records are assigned to cities and to firms.

I assign each firm to the city of its registered location in the tax survey. *Loan* equals one if the firm's city adopted the program before the investigation year and zero if the city never adopted it. In the direct-effect and stacked analyses, I exclude firms whose city adopted in the investigation year or after the investigation year.

To test whether adoption reflects local conditions (Section 5), I use city–year characteristics from the CSMAR China Economy Database, namely total exports, GDP, fiscal revenue and expenditure, real estate investment, and year-end outstanding bank loans. The city–year sample of Table A1 consists of the years up to and including a city's adoption year, and contains 1,319 city–years with all eight characteristics observed.

3.4. Firm Financial Data

Firm data come from the National Tax Survey, which is collected jointly by the State Administration of Taxation and the Ministry of Finance for tax monitoring and policy evaluation.

[8] Two annotators independently judge from the search results whether the program is in place in a given year, agree in more than 90% of cases, and resolve the rest by discussion.

The survey covers firms in thirty-one provinces and eighty-four industries. Relative to the Annual Survey of Industrial Firms, it covers small firms more completely while keeping a full representation of large firms, and it has been used by Giannetti et al. (2021) and Lu and Ma (2023). I use the survey years 2007 to 2015.

Each return reports three kinds of information. On taxation, it reports VAT payments and the export rebates the firm claimed and had not yet received at year end, which is what makes the loan program relevant for these firms. On financing, it reports cash flow, bank loans, interest expense, and current liabilities. On operations, it reports sales, exports, employment, profits, and the income statement, including operating profit, cost of goods sold, and selling and administrative expenses, with advertising, business entertainment, and research and development spending reported within them. Value added is reported through 2014 only, so the last years of the window are missing for that outcome in the later cohorts. The survey also records the firm's registered city, which I use to assign the loan indicator, and its industry code, which I use to describe the sample.

From these returns I construct the firm-level outcomes of Sections 4.4 and 5. Return on assets is operating profit over year-end total assets. The cost of debt is interest expense over year-end total liabilities. Level variables are winsorized at the 1% and 99% levels among positive values before taking logs, and ratios are winsorized at the 1% and 99% levels. Export revenue at the firm level comes from the customs data and is the firm's total export value across all products and destinations in a year, with firm–years without a customs record coded as zero.

3.5. Summary Statistics

Table 1 provides the summary statistics of the main variables in each estimation sample. Panel A is the direct-effect sample, which consists of firm–products that faced a single investigation, at the initiating country and at the firm's other destinations. In this sample, the average firm–product exports to a destination in 56% of the years in the window, and

85% of the flow–years belong to firms in cities that adopted the loan program before the investigation.

Panels B1 to B3 are the trade diversion and spillover samples, namely the hit firm's AD product sold to other destinations, its other products sold to the initiating country, and its other products sold to other destinations, each pooled with matched flows of never-hit firms. *Exit* is computed on flow–years in which the flow was active in the previous year, and between 62% and 67% of these flow–years end in exit, so a typical product–destination line of a Chinese exporter is short-lived. Between 74% and 76% of flow–years are firms with access to the loan program.

Panels C1 and C2 are the firm-level samples, in which each hit firm is matched to a never-hit firm in the same cohort and industry on its pre-investigation exports (C1) or on its pre-investigation revenue and assets (C2). The average firm in Panel C1 exports 2.4 million USD per year and the median firm 168,000 USD, so the distribution is skewed toward a few large exporters. In Panel C2, the average return on assets is 2.7% and the average cost of debt is 1.8%. Between 72% and 74% of firm–years are firms with access to the loan program.

Appendix Figures A1 and A2 describe the hit firms further. The most common industry among hit firms is wholesale trade, with 23% of firms, because trading companies export on behalf of manufacturers, followed by general machinery, metal products, computers and electronics, and rubber and plastics, each with about 6%. Among the export flows of hit firms other than the targeted one, the most common products are machinery and electronics (26%) and textiles (21%), and the most common destinations are the European Union (14%), Japan (7%), Hong Kong (6%), and the United States (4%). Hit firms are therefore multi-product, multi-market exporters.

4. How Firms Navigate Trade Disruptions

This section examines how a firm's exports adjust after one of its products is investigated in one of its markets. I proceed in four steps. I start with the targeted flow, namely the AD product sold to the initiating country, and ask how much of it the firm loses. I then follow the firm's remaining flows, first the AD product sold to other destinations, then the firm's other products sold to the initiating country, and finally the firm's other products sold to other destinations. I close with the firm as a whole and ask whether these adjustments pay off.

4.1. Direct Effects of Anti-Dumping Investigations

I start by examining whether an AD investigation reduces a firm's exports of the investigated product to the country that initiated the case. An investigation raises the expected cost of selling the product in that market, since duties follow if the case succeeds. The prediction for quantity is clear. The firm should ship less of the product to the initiating country than to its other markets. The prediction for price is not. A firm can raise its export price to narrow the dumping margin and lower the future duty, or cut it to hold on to demand, so the sign of the price response is an empirical question.

To test these predictions, I use the direct-effect sample of Section 3.2. I compare the firm's exports of the product to the initiating country with its exports of the same product to other destinations, before and after the investigation, by estimating

$$y_{ijdt} = \beta \, Treat_{ijd} \times Post_{ijt} + \alpha_{ijt} + \gamma_{ijd} + \varepsilon_{ijdt}, \tag{1}$$

where i indexes firms, j products, d destinations, and t years. Because each firm–product faces a single case, $Treat_{ijd}$ equals one if d is the country that initiated the case against product j of firm i, and $Post_{ijt}$ equals one from the investigation year of that case onward. The firm–product–year fixed effects α_{ijt} absorb anything that shifts the firm's exports of the

product in a given year across all destinations, including the main effect of $Post_{ijt}$. The firm–destination–product fixed effects γ_{ijd} absorb the level of each flow over the window, including the main effect of $Treat_{ijd}$. The coefficient of interest is β, which captures the differential change in the targeted flow relative to the firm's other flows of the same product after the investigation. Standard errors are double clustered by firm and destination country.

I examine four outcomes. On the extensive margin, *Have Export* is an indicator that equals one if the firm exports the product to the destination during the year. On the intensive margin, I use the logarithms of quantity, value, and unit price, where unit price is export value divided by quantity in USD, estimated on flow–years with positive exports.

Table 2 reports the results. Columns (1), (3), (5), and (7) include firm–product–year fixed effects together with firm–destination and destination–product fixed effects. Columns (2), (4), (6), and (8) replace the latter two with firm–destination–product fixed effects, so that identification comes from within-flow variation over time. The estimates are nearly identical across the two specifications, and I discuss the more restrictive ones.

Columns (1) and (2) show that the investigation lowers the probability that the firm exports the product to the initiating country by 2.6 percentage points, which is around 5% of the standard deviation of the dependent variable (= 0.026/0.496). As the dynamic estimates below show, this average reflects a drop in the first two years after the investigation rather than a permanent exit. Columns (3) and (4) show that, conditional on exporting, quantity falls by 0.092 log points, corresponding to 9% in percentage terms. Columns (7) and (8) show that the unit price rises by 0.097 log points, or 10%. Columns (5) and (6), in contrast, show no change in export value. The coefficient is −0.011 and not statistically different from zero. The quantity and price movements offset each other. The investigation therefore changes the composition of the targeted flow more than its size in dollars.

The price increase is consistent with the first of the two pricing strategies. An anti-dumping duty is set as the gap between the exporter's home price and its export price. A firm that raises its export price during the investigation narrows this gap and lowers the

duty it will face once the case is decided. Raising the price costs the firm some sales, which is what the lower quantity shows. Buyers accept part of the increase because the duty will apply to the product from every supplier named in the case, and because finding a new supplier within a year is costly. The firms that keep exporting to the initiating country therefore sell less, at a higher price, and earn about the same as before.[9] Because the unit price is measured at the HS6 level, the increase can also reflect a shift toward more expensive varieties within the same code, and the data do not distinguish between the two.

Figure 3 depicts the dynamic effects of the investigation on the targeted flow. For each outcome, I replace $Post_{ijt}$ in Equation (1) with event time indicators from three years before to four years after the investigation, with the year before the investigation as the benchmark, and plot the coefficients on $Treat_{ijd} \times 1[t = k]$. Reassuringly, the differential shifts in the targeted flow do not occur prior to the investigation, but emerge after it. The two margins respond differently. On the extensive margin, the effect is short-lived. The probability of exporting falls by 2.4 percentage points in the first and second year after the investigation and returns to the benchmark by the third year. On the intensive margin, the effect is lasting and grows over the window. Quantity falls from the investigation year onward, and by the fourth year it is 0.233 log points below the benchmark, corresponding to a 21% decline. The unit price rises more slowly. It is not statistically different from the benchmark in the first two years, becomes significant in the second year after the investigation, and reaches 0.223 log points by the fourth year, corresponding to a 25% increase, though this last estimate is significant only at the 10% level. Firms therefore do not abandon the initiating country. They stay, sell less, and charge more, and both adjustments deepen over the window.

[9] Blonigen and Park (2004) show that a firm under investigation can also cut its price to hold on to demand, so the average price response need not be positive. Bao et al. (2025) find a price effect close to zero for Chinese exporters and a 26% drop in quantity. In my sample the price increase dominates.

4.2. Trade Diversion

If the firm loses part of the targeted market, where does the product go? I next follow the hit firm's other flows and compare them with the flows of firms that were never investigated. The prediction for the AD product is clear. A firm that has lost sales in one market and can still produce the product should sell it in the markets it already serves, so the AD product should be retained in other destinations and shipped there in larger volumes. The prediction for the firm's other products in the initiating country is open. The duty applies to the investigated product only, so the firm has no direct reason to withdraw its other products. It may nonetheless do so if the case sours its relationship with buyers there or raises the risk of further cases.

To test these predictions, I use the trade diversion samples of Section 3.2, in which each flow of a hit firm is matched to a flow of a never-hit firm in the same product and destination, with the nearest match chosen on export quantity growth over the three years before the treated flow's investigation. I estimate

$$y_{ijdt} = \beta \, Treat_i \times Post_t + \gamma_{ijd,c} + \delta_{dt,c} + \varepsilon_{ijdt}, \tag{2}$$

where $Treat_i$ equals one if the firm faced an AD investigation, $Post_t$ equals one from the investigation year onward, and c indexes cohorts, defined by the investigation year. The firm–product–destination fixed effects $\gamma_{ijd,c}$ absorb the level of each flow, and the destination–year fixed effects $\delta_{dt,c}$ absorb demand conditions in each destination market in each year. Both are interacted with cohort indicators, so that a hit firm is compared only with control flows in the same event window. The coefficient of interest is β, which captures the differential change in the flows of hit firms relative to their matched controls after the investigation. Standard errors are double clustered by firm and destination country.

In Section 4.1 the extensive margin asked whether a flow exists. Here every flow in the sample was active in at least one of the three years before the investigation, so the question

becomes whether the firm keeps it. I therefore measure the extensive margin by exit. *Exit* equals one in a year if the firm exported the product to the destination in the previous year but does not in the current year, and zero if it exports in both years. It is defined only for years in which the flow was active the year before, since a flow that was already gone cannot exit again. The intensive margin uses the logarithms of quantity, value, and unit price, as in Section 4.1.

Table 3 reports the results. Panel A examines the AD product sold to destinations other than the initiating country. Column (1) shows that hit firms are 24.3 percentage points less likely to drop one of these flows than matched never-hit firms, which is around half of the standard deviation of the dependent variable (= 0.243/0.484). Columns (2) and (3) show that, on the flows they keep, quantity rises by 0.166 log points and value by 0.175 log points, corresponding to 18% and 19% in percentage terms. Column (4) shows no change in the unit price. Hit firms therefore sell more of the investigated product in their other markets at the prices they charged before. This pattern is consistent with the prediction that the product is rerouted rather than withdrawn.

Panel B examines the firm's other products sold to the initiating country. Column (1) shows that hit firms are 15.1 percentage points less likely to drop one of these flows, around 30% of the standard deviation of the dependent variable (= 0.151/0.485). Columns (2) and (3), in contrast, show no change in quantity or value, and column (4) shows that the unit price rises by 0.031 log points, or 3%, significant only at the 10% level. The investigation therefore does not push the firm out of the initiating country. The firm keeps its other products there but does not expand them, which is consistent with the duty attaching to the product rather than to the firm.

Figure 4 depicts the dynamic effects of the investigation on the hit firm's related flows. Panels A to C trace exit, quantity, and value for the AD product sold to other destinations, and Panel D traces exit for the firm's other products sold to the initiating country. For each outcome, I plot the coefficient on $Treat_i \times 1[t = k]$, where k indexes years around the

investigation and the year before the investigation is the benchmark. Prior to the investigation, hit firms are not less likely to drop these flows, nor do they ship more on them, than matched never-hit firms. The lack of a pre-trend is reassuring, suggesting that the rerouting is a response to the case rather than the continuation of a trend that was already under way. Following the investigation, hit firms retain the AD product in other destinations and ship more of it, and they retain their other products in the initiating country. Specifically, in the first year after the investigation, hit firms are 26 percentage points less likely to drop the AD product from another destination than matched never-hit firms, and they ship 0.13 (0.16) log points more in quantity (value), corresponding to 14% (17%) in percentage terms. In the investigation year, they are 22 percentage points less likely to drop one of their other products from the initiating country, a differential that narrows to 5 percentage points by the fourth year.

Taken together, the results in this subsection are consistent with the view that the firm treats the investigation as a barrier on one product in one market. It sells that product in the other markets it already serves and keeps selling its other products in the initiating country.

4.3. Spillover Effects

The flows in Section 4.2 share either the product or the destination with the case. I now turn to the flows that share neither, i.e., the hit firm's other products sold to other destinations. The investigation does not touch these flows, so they offer a test of whether the firm's response to the shock spills over to the rest of its export business. If a firm can draw on outside resources at will, rerouting the AD product should leave its unrelated flows unchanged. If instead management attention, sales effort, and working capital are limited, the effort spent on the affected product should crowd out the products and markets the case did not touch.

To test these predictions, I estimate Equation (2) on the unrelated flows of hit firms and their matched controls, built in the same way as the samples in Section 4.2.

Table 4 reports the results. Column (1) shows that hit firms are 11.3 percentage points

less likely to drop an unrelated flow than matched never-hit firms, around a quarter of the standard deviation of the dependent variable (= 0.113/0.469). Columns (2) and (3), in contrast, show that on the flows they keep, quantity falls by 0.081 log points and value by 0.064 log points, corresponding to 8% and 6% in percentage terms. Column (4) shows that the unit price rises by 0.017 log points, or 2%, significant only at the 10% level. Hit firms therefore keep their unrelated lines but ship less on each of them.

Figure 5 depicts the dynamic effects. For each outcome, I plot the coefficient on $Treat_i \times 1[t = k]$, with the year before the investigation as the benchmark. Prior to the investigation, hit firms are not more likely to drop these flows than matched never-hit firms. Following the investigation, hit firms keep their unrelated flows but ship less on them, and the decline fades by the end of the window. Specifically, in the investigation year hit firms are 14 percentage points less likely to drop an unrelated flow, a gap that narrows to 4 percentage points by the fourth year. In the second year after the investigation, they ship 0.13 (0.10) log points less in quantity (value), corresponding to 12% (10%) in percentage terms.

Taken together, the results are consistent with the view that a firm facing a trade barrier concentrates its limited resources on the affected product. Rerouting the AD product to other markets requires management attention, sales effort, and working capital. Spending these on the affected product leaves less for the products and markets the case did not touch. I do not observe inputs by product, so I cannot identify which resource is reallocated. Overall, the evidence suggests that the adjustment to the shock is not confined to the affected product but reaches the rest of the export portfolio, and that this cost is temporary, likely because the effort to reroute is concentrated in the first years after the case.

4.4. Firm-Level Responses

The flow-level results show that hit firms lose part of the targeted market and gain in others. I now move on to the firm as a whole and ask whether these adjustments pay off. If rerouting were costless, the gains in other markets should show up in export revenue and in profit alike.

If rerouting requires selling effort, new hires, and working capital, export revenue should rise while profitability does not, and the cost should appear in operating expenses and in borrowing. To test these predictions, I estimate the following regression on a balanced firm–year panel of hit firms and matched never-hit firms from three years before to four years after the investigation:

$$y_{it} = \beta \, Treat_i \times Post_t + \alpha_{i,c} + \tau_{t,c} + \varepsilon_{it}, \tag{3}$$

where i indexes firms, t years, and c cohorts. Each hit firm is matched to a never-hit firm in the same industry on the logarithm of exports and its growth over the three years before the hit firm's investigation for the export outcomes, and on the logarithm of revenue, its growth, and the logarithm of assets over the same years for the accounting outcomes. The firm fixed effects $\alpha_{i,c}$ and year fixed effects $\tau_{t,c}$ are each interacted with cohort indicators, so that a hit firm is compared only with its matched control in the same event window. The coefficient of interest is β, which captures the differential change in the outcome of hit firms relative to matched never-hit firms after the investigation. Standard errors are clustered by firm.

Table 5 reports the results. Panel A examines export revenue and profitability. Column (1) estimates export revenue in levels by Poisson pseudo-maximum likelihood, which keeps firm–years with zero exports in the sample, and column (2) uses the logarithm of exports on firm–years with positive exports. Both specifications show that hit firms expand their exports. Export revenue of hit firms rises by 0.293 log points in column (1) and 0.342 log points in column (2), corresponding to 34% and 41% in percentage terms relative to matched never-hit firms. Column (3) shows that value added rises by 0.118 log points, or 12%. Column (4), in contrast, shows no change in profitability. The coefficient on return on assets is 0.001, around 4% of the sample average of 0.027 (= 0.001/0.027), and not statistically different from zero. Hit firms therefore sell more abroad and produce more, but they are not more profitable. This pattern is consistent with the prediction that rerouting is costly.

Panel B examines where the cost falls. Every operating cost rises. Cost of goods sold increases by 0.080 log points and the number of employees by 0.060 log points, corresponding to 8% and 6% in percentage terms. These two increases are in line with the increase in output and indicate that hit firms add workers rather than only work existing capacity harder. Selling expense rises by 0.140 log points, or 15%, and administrative expense by 0.068 log points, or 7%. Within these categories, advertising rises by 19% and business entertainment by 6%, both significant at the 10% level, and research and development by 18%, significant at the 5% level. The advertising and R&D samples are small, with 7,088 and 3,523 firm–years, so I read these two estimates as suggestive. Selling and administrative expenses grow about twice as fast as cost of goods sold. This ordering is consistent with the prediction that reaching buyers in new markets costs more than producing for them.

Panel C examines the impacts of AD investigations on financing costs. Interest expense rises by 0.120 log points and current liabilities by 0.065 log points, corresponding to 13% and 7% in percentage terms. The cost of debt, defined as interest expense over total liabilities, rises by 0.1 percentage points, which is around 6% of the sample average of 1.8%. Hit firms therefore finance part of the adjustment with short-term debt, and they pay slightly more for it. This result motivates the analysis in Section 5. If borrowing becomes more expensive when firms adjust, access to cheaper credit may change how they adjust.

Figures 6 to 8 depict the dynamic effects of the investigation at the firm level. For each outcome, I plot the coefficient on $Treat_i \times 1[t = k]$, with the year before the investigation as the benchmark. Prior to the investigation, hit firms do not export more, produce more, spend more, or borrow more than matched never-hit firms. The lack of a pre-trend is reassuring, suggesting that hit firms were not already on a different growth path. Following the investigation, export revenue rises at once and stays higher, while costs and borrowing rise more gradually. Specifically, in the first year after the investigation, export revenue of hit firms is 0.27 log points higher than that of matched never-hit firms, corresponding to 31% in percentage terms. In the same year, cost of goods sold, selling expense, and administrative

expense are 0.08, 0.13, and 0.07 log points higher, or 8%, 13%, and 7%, and interest expense is 0.11 log points higher, or 12%. Value added is above the benchmark in every year after the investigation but is statistically significant only in the second year, at 0.19 log points, or 21%. The cost of debt is never statistically different from the benchmark in any single year. Appendix Figure A7 shows that advertising rises in the first three years after the investigation, while entertainment and R&D show no consistent pattern.

Taken together, the results in this section are consistent with the view that firms treat an anti-dumping investigation as a shock to one market rather than to the firm. Hit firms lose part of the targeted market, reroute the product, hold their other lines, and grow their export revenue by about a third. A key feature of these results is that they are inconsistent with hit firms simply being stronger exporters. If they were, export revenue and profitability should rise together. Instead, profitability does not change, because the gain is paid for with higher selling and administrative costs and with more expensive short-term debt. Overall, the evidence suggests that rerouting is an effective but costly response, likely because reaching buyers in new markets requires effort and financing that the firm must fund up front. This raises the question of whether firms with easier access to credit adjust differently, which I take up next.

5. Government Credit and Firm Responses

Section 4 shows that hit firms reroute the AD product, that rerouting is costly, and that they pay for part of the cost with short-term debt at a higher price. I now ask whether access to credit changes this response. To do so, I use the staggered adoption of the VAT rebate loan program across Chinese cities, described in Section 2.2, and compare hit firms in cities that adopted the program before the investigation with hit firms in cities that never did.

Credit can shape how firms navigate a trade disruption in three ways. First, it can keep firms from exiting the affected market. A firm with access to the program can carry the

affected flow while the case is pending, so it should be more likely to stay in the initiating country. Second, it can let firms adjust on a larger scale, by shipping more of the affected product to other markets or more of their other products to the affected market. Third, it can keep firms from retreating from products and markets that the case does not touch. The three channels are not mutually exclusive. Each has its own prediction on its own margin, and I test them in turn.

Throughout this section, *Loan* equals one if the firm's city adopted the program before the investigation and zero if the city never adopted it. Firms in cities that adopted in or after the investigation year gain access in the middle of the event window and are excluded. Each test adds the triple interaction $Treat \times Post \times Loan$ to the corresponding specification of Section 4. The coefficient on $Treat \times Post$ gives the response of hit firms without access, and the coefficient on the triple interaction, the coefficient of interest, gives how much the response of hit firms with access differs.

5.1. The Loan Program: Adoption and Financing after the Shock

Before comparing firms by loan access, I address two questions. The first is whether adoption of the program reflects local economic conditions. If cities adopted because their firms were struggling or because credit was scarce, then differences between firms with and without access could reflect the city rather than the program. The second is whether the program in fact eases financing for hit firms after the shock. If it does not, access to the program would have no reason to change how firms respond.

To address the first question, I regress adoption on city conditions, overall and by the stated motive classified in Section 2.2. I construct a city–year panel in which a city exits once it adopts, so that in each year adopting cities are compared with cities that have not yet adopted. The dependent variable is an indicator for whether the city adopts the program in that year. The regressors are eight city characteristics that cover the size, fiscal position, and credit supply of the city. All specifications include year and province fixed effects, which

restrict the comparison to cities in the same province at the same point in time.

Table A1 presents the results. In column (1), the dependent variable is adoption for any stated motive. In columns (2) to (4), it is adoption citing each of the three motives, namely long-term development, an export slowdown, and firms' credit constraints. None of the eight characteristics predicts adoption in column (1). City exports, GDP, fiscal revenue and expenditure, real estate investment, and bank lending all carry small and statistically insignificant coefficients. The same holds in columns (2) and (3). The one exception is the coefficient on outstanding bank loans in column (4). Cities with more bank credit are slightly more likely to adopt the program with firms' credit constraints as the stated motive, and the estimate is significant only at the 10% level. This pattern is reassuring rather than concerning. The loans are issued by commercial banks, so a city with ample bank credit is better placed to run the program. Adoption therefore does not proxy for a local credit shortage. Figure 9 plots the same relations as binscatters. By construction, the slope in each panel equals the corresponding coefficient in column (1), and the fitted lines are close to flat in every panel. Overall, adoption is not explained by the size, fiscal position, investment activity, or credit supply of a city.

To address the second question, I test whether hit firms with access to the program finance the adjustment on better terms than hit firms without access. Section 4.4 shows that hit firms borrow more after the investigation and pay a higher price for debt. If the program works as intended, the rise in the price of debt should be smaller for firms with access. To test this prediction, I estimate the following regression:

$$y_{it} = \beta_1 \, Treat_i \times Post_t + \beta_2 \, Treat_i \times Post_t \times Loan_i + \alpha_{i,c} + \tau_{mt,c} + \varepsilon_{it}, \tag{4}$$

on the firm–year panel of Section 4.4, restricted to firms in cities that adopted the program before the investigation or never adopted it. $Loan_i$ equals one if the city of firm i adopted the program before the investigation and zero if it never adopted the program. The city–year fixed effects $\tau_{mt,c}$, where m indexes cities, absorb any shock to a city in a given year,

including the main effect of $Post \times Loan$, so that a hit firm is compared with never-hit firms in the same city and year. The firm fixed effects absorb $Loan$ and $Treat \times Loan$. Both sets of fixed effects are interacted with cohort indicators. Standard errors are clustered by firm.

Table 6 reports the results. Column (3) shows that the cost of debt of hit firms without access rises by 0.2 percentage points after the investigation, around 12% of the sample average of 1.7% (= 0.002/0.017). The coefficient on the triple interaction is −0.002, so the increase is fully offset for hit firms with access. Columns (1) and (2) show that interest expense and current liabilities of hit firms without access rise by 0.175 and 0.044 log points, and that the triple interactions are negative, but none of these four estimates is statistically different from zero. The program therefore does not change how much hit firms borrow, but it keeps the price of that borrowing from rising. This is consistent with the pattern of the program which lends against rebates the firm is already owed, at rates below the benchmark lending rate, as described in Section 2.2.

5.2. Direct Effects by Loan Access

I begin with the first channel and ask whether access to the program keeps a hit firm in the targeted market. If a firm with access can carry the affected flow while the case is pending, it should be less likely to stop exporting the product to the initiating country than a firm without access.

To test this prediction, I add the triple interaction $Treat_{ijd} \times Post_{ijt} \times Loan_i$ to Equation (1), where $Loan_i$ equals one if the city of firm i adopted the program before the investigation and zero if it never adopted the program. The sample is that of Section 4.1, restricted to firms in cities that adopted the program before the investigation or never adopted it. The firm–product–year fixed effects absorb $Post \times Loan$ and any city–year shock, and the firm–destination–product fixed effects absorb $Treat \times Loan$. The comparison is therefore the same as in Section 4.1, between the targeted flow and the firm's other flows of the same product. The coefficient on $Treat \times Post$ captures this within-firm change for hit firms without ac-

cess, and the coefficient on the triple interaction captures how much the change differs for hit firms with access. Standard errors are double clustered by firm and destination country.

Table 7 reports the results. In column (1), the dependent variable is *Have Export*. Hit firms without access are 8.2 percentage points less likely to export the product to the initiating country after the investigation, around 14% of the sample average of 0.575. The coefficient on the triple interaction is 7.0 percentage points and statistically significant at the 1% level. The two coefficients nearly offset each other, so the net decline for firms with access is 1.2 percentage points. The average effect in Table 2 thus mainly comes from firms without access. In columns (2) to (4), the dependent variables are log quantity, log value, and log price. Firms without access raise the unit price by 0.141 log points, corresponding to 15% in percentage terms, and the triple interaction is negative but not statistically significant.[10] I do not find significant effects on quantity or value for either group.

The benefit of the program thus lies on the extensive margin, not the intensive margin. This is the margin that matters most. Once a firm leaves an export market, it is hard to return, since the buyers and distribution channels it built there have to be rebuilt. By keeping hit firms in the affected market while the case runs, the program preserves what would otherwise be lost, even though it does not change how much these firms ship there.

Figure 10 depicts the dynamic effects on *Have Export*, estimated separately for firms with and without access. Prior to the investigation, the estimates for firms without access are not statistically different from zero. For firms with access, the estimate two years before the investigation is positive and significant at the 5% level, so the pre-period for this group is not entirely flat. Following the investigation, the two groups diverge. Firms without access are 10 to 13 percentage points less likely to export the product to the initiating country in the investigation year and the year after, and 8 percentage points less likely in the second

[10] The coefficient on *Treat* × *Post* here is larger than the 0.097 in Table 2 because it applies only to firms without access. Firms with access have a price response of $0.141 - 0.077 = 0.064$ log points. Weighting the two groups by their shares in the sample, 14% without access and 86% with access, gives an average of 0.075 log points, close to the estimate in Table 2. The remaining gap reflects the smaller sample, which excludes firms in cities that adopted the program in or after the investigation year.

and fourth years after. Firms with access show no decline in any year. Their estimates after the investigation are small and positive, and those that are statistically significant, in the investigation year and the third and fourth years after, are between 3 and 4 percentage points.

Taken together, the results support the first channel. Firms with access stay in the affected market, and firms without access leave it. The program does not change the expected duty, so both groups face the same level of disruptions. What differs is whether the firm can keep serving an increasingly uncertain market while the case is pending, and that is what credit provides.

5.3. Trade Diversion by Loan Access

I next turn to the second channel and ask whether access to the program lets a hit firm adjust on a larger scale. The flows in this sample were already active before the investigation, so the question is not whether the firm finds new buyers but how much more it sells to the buyers it has. If working capital limits how much a firm can shift to these flows, firms with access should be less likely to drop them and should ship more on them than firms without access, both for the AD product in other markets and for their other products in the initiating country.

To test these predictions, I add the triple interaction $Treat_i \times Post_t \times Loan_i$ to Equation (2), together with city–year fixed effects. The city–year fixed effects absorb $Post \times Loan$ and any shock to a city in a given year, and the firm–product–destination fixed effects absorb $Treat \times Loan$. All fixed effects are interacted with cohort indicators. The sample is that of Section 4.2, restricted to firms in cities that adopted the program before the investigation or never adopted it. As before, the coefficient on $Treat \times Post$ examines the response of hit firms without access relative to their matched never-hit firms, and the coefficient on the triple interaction gives how much the response of hit firms with access differs.

Appendix Table A3 reports the results. Panel A examines the AD product sold to

destinations other than the initiating country. In column (1), hit firms without access are 17.4 percentage points less likely to drop one of these flows than matched never-hit firms. In columns (2) and (3), they ship 0.230 and 0.224 log points more in quantity and value on the flows they keep, corresponding to 26% and 25% in percentage terms. Hit firms without access therefore keep these flows and ship more on them, the same pattern as in Table 3. The triple interactions are negative in all three columns, but none is statistically different from zero. Firms with access do not reroute more than firms without access, and if anything they reroute slightly less. Panel B examines the firm's other products sold to the initiating country. In column (1), hit firms without access are 18.5 percentage points less likely to drop one of these flows, and the triple interaction is small and not statistically different from zero. I do not find significant effects on the intensive margin for either group.

These results do not support the second channel, under which credit lets a hit firm shift more to the flows it already has. If working capital were what limits how much of the AD product a firm sends to its other markets, or how much of its other products it sends to the initiating country, firms that can borrow against their rebates should shift more on both. I do not find that they do.

5.4. Spillover Effects by Loan Access

I turn to the third channel and ask whether access to the program keeps a hit firm from retreating from the products and markets the case does not touch. Section 4.3 shows that hit firms keep these unrelated flows but ship less on them. If working capital is one of the resources that rerouting draws away from the rest of the firm, firms with access should pull back less. To test this prediction, I estimate the specification of Section 5.3 on the unrelated flows of hit firms and their matched controls.

Table 8 reports the results. In column (1), hit firms without access are 7.2 percentage points less likely to drop an unrelated flow than matched never-hit firms, around 14% of the sample average of 0.514. The coefficient on the triple interaction is -3.7 percentage points

and significant at the 5% level, so hit firms with access are 10.9 percentage points less likely to drop an unrelated flow, about half again as much as firms without access. In columns (2) and (3), quantity and value on these flows fall by 0.110 and 0.095 log points for firms without access, corresponding to 10% and 9% in percentage terms. The triple interactions are positive but not statistically different from zero. I do not find a price effect for either group.

These results support the third channel for whether a unrelated line is kept (extensive margin), but not for how much is shipped on it (intensive margin). Access to the program helps hit firms keep their unrelated lines open, yet these firms cut shipments on the lines they keep by as much as firms without access. The two halves fit together. A firm that can borrow against its rebates does not have to drop a small line to free up cash for the affected product, so fewer lines are dropped. Shipping less on the lines it keeps is the same with or without access, which suggests that this part of the pull-back comes from resources that credit does not replace, such as management attention and sales effort.

5.5. Firm-Level Responses by Loan Access

The three channels add up at the level of the firm, so I finally ask whether firms with and without access end up in different places. If credit let firms adjust on a larger scale, the export revenue of firms with access should rise more. If credit only lets firms keep the affected market and their unrelated lines, the two groups should differ little in total exports and profitability, since both reroute the same amount and pay a similar cost to do so. To test these predictions, I estimate Equation (4) on the export revenue, profitability, and operating cost outcomes of Section 4.4.

Appendix Table A4 reports the results. Panel A examines export revenue, value added, and profitability. For hit firms without access, export revenue rises by 0.309 log points in the Poisson specification and 0.360 log points in logs, corresponding to 36% and 43% in percentage terms, and value added rises by 0.272 log points, or 31%. Return on assets

does not change. None of the triple interactions is statistically different from zero. Panel B examines operating costs. Cost of goods sold and selling expense rise by 0.104 and 0.114 log points for firms without access, as in Section 4.4, and none of the seven triple interactions is statistically different from zero.

As far as the data can tell, firms with access earn the same export revenue, pay the same operating costs, and end with the same profitability as firms without access. This is consistent with the flow-level results in Sections 5.2 to 5.4, where the program affects whether firms keep a flow but not how much they ship on it. Two facts explain why the firm-level totals do not differ. First, the two groups reroute the same amount, and rerouting is what drives the rise in total exports and in costs. Second, access keeps firms present in the affected market and on their unrelated lines, but does not raise what they sell there, so what is preserved shows up in where the firm sells rather than in how much it earns within the window.

Taken together, the results in this section are consistent with the view that government-facilitated credit changes what hit firms keep, not how much they sell. Firms with and without access reroute the same amount, ship the same on the flows they keep, and end with the same profitability. Firms with access, however, stay in the affected market, drop fewer of their unrelated lines, and borrow at no higher cost. The likely reason is that the program lends them cash against their pending rebates, so they do not have to give up a market or drop a line to pay for the costly adjustments.

The value of staying likely extends beyond the event window. A firm that keeps the flow alive can scale it back up once the case is resolved or the duty expires, while a firm that exited has to rebuild the relationship. I cannot test this directly, because the customs data end in 2015, so the window runs at most four years after the investigation.[11] But the pattern in Section 5.2 implies that firms with access are the ones still present when demand returns, while their competitors without access have left. The affected market may then become

[11]The customs data for 2016 report export value only, without quantity, so unit prices and quantities cannot be computed for that year. I therefore end the panel in 2015.

more profitable for the firms that stayed. In a world with more frequent trade disruptions, the ability to stay where a firm already is has value of its own.

6. Conclusion

This paper examines how Chinese exporters respond when a foreign trade barrier disrupts an existing market, and how access to financing shapes such responses. As trade tensions intensify and tariffs return as a policy tool, understanding how the firms on the receiving end of trade barriers survive the shock, redirect trade, and reorganize their product lines has become crucial for both corporate strategy and trade policy.

Utilizing transaction-level customs data matched to firm tax records, together with anti-dumping investigations against Chinese exports, I find that hit firms reduce exports of the targeted product to the initiating market but redirect it toward markets not subject to the barrier, and keep their other products in the affected market. These adjustments are costly, and resources within the firm are limited, so hit firms retreat from products and markets that the case does not touch. At the firm level, export revenue rises by about a third while profitability does not change, because the costs of production, sales, and financing rise with revenue. Importantly, access to credit shapes firms' adjustments. Exploiting the staggered rollout of the VAT rebate loan program across Chinese cities, I find that hit firms with loan access maintain the affected export relationship, while hit firms without access exit. Firms with access also drop fewer of their unrelated export lines and face no rise in the cost of debt after the shock. In contrast, I find no evidence that loan access changes cross-product and cross-market adjustment, and firms with and without access end with the same export revenue and profitability. Overall, the evidence is consistent with credit access working through the extensive margin, helping firms secure more options, rather than through the intensive margin, helping them export substantially more.

My findings have two implications. First, the consequences of trade barriers depend on

conditions in domestic financial markets. Whether a firm can redirect trade or remain in a contested market depends not only on the severity of the foreign barrier, but also on whether it has access to short-term liquidity. The incidence and effectiveness of a trade restriction are therefore shaped by financial conditions in the exporting country. Second, government financial support that broadens what firms can pledge, while leaving lending decisions with banks, can alter not only firms' survival during a trade disruption, but also the geography of their trade and the longer-run allocation of their product lines. In my setting, loan program does not make hit firms export or earn more, but it keeps them from giving up markets and product lines, which is a valuable option when the disruption blows over.

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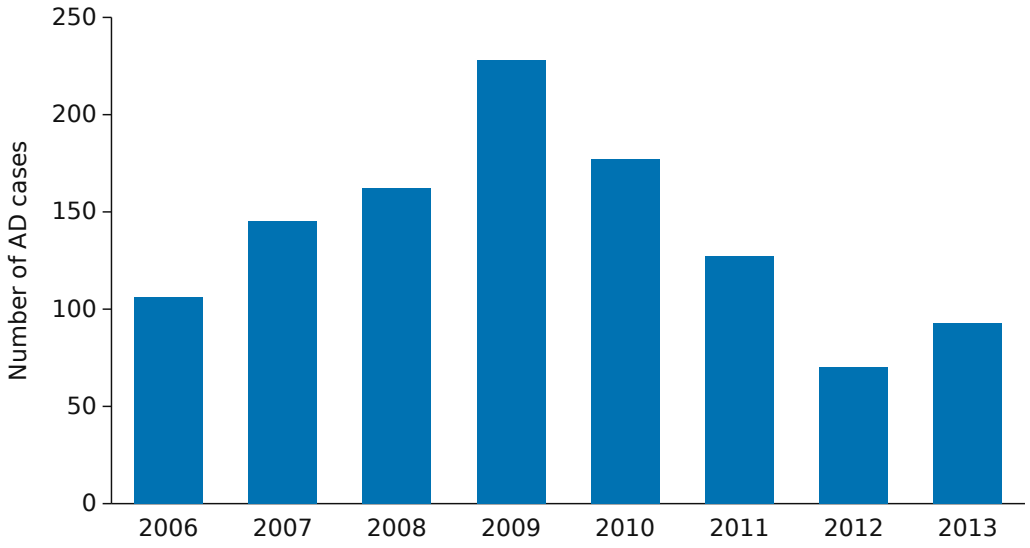
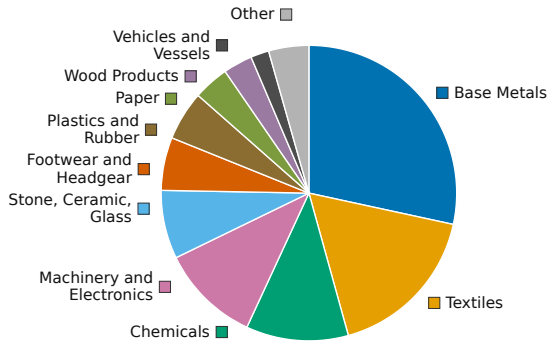
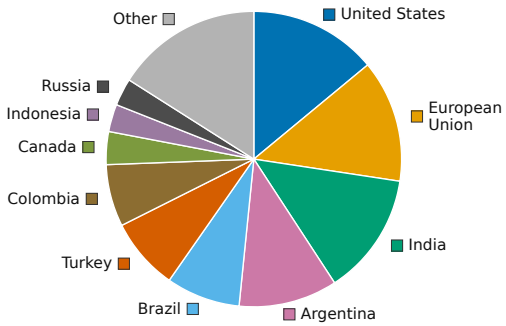


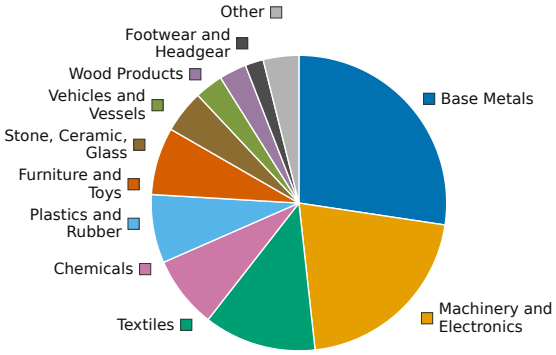
Figure 1: **Anti-dumping Cases against Chinese Exporters by Year.** This figure plots the annual number of anti-dumping (AD) cases initiated against Chinese exporters during 2006–2013, where a case is defined at the initiating country–product (HS6) level. Data are from the World Bank Global Antidumping Database.



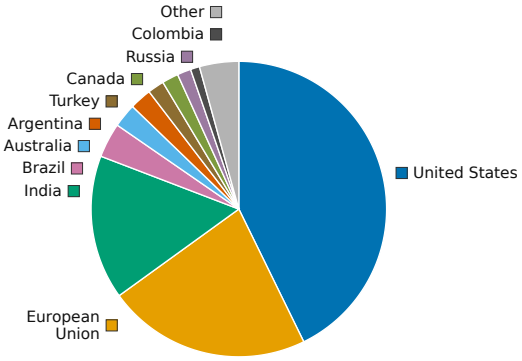
Panel A: Targeted Products, by AD Case



Panel B: Initiating Countries, by AD Case

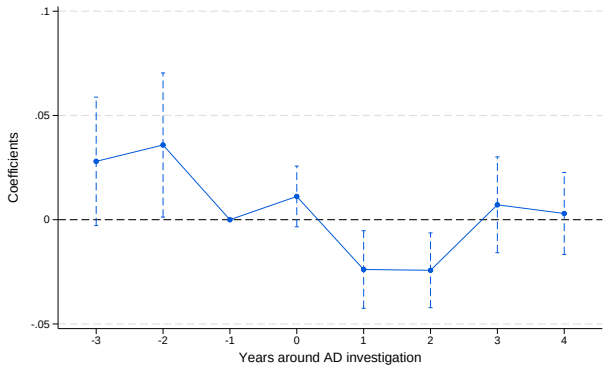


Panel C: Targeted Products, by Hit Firm

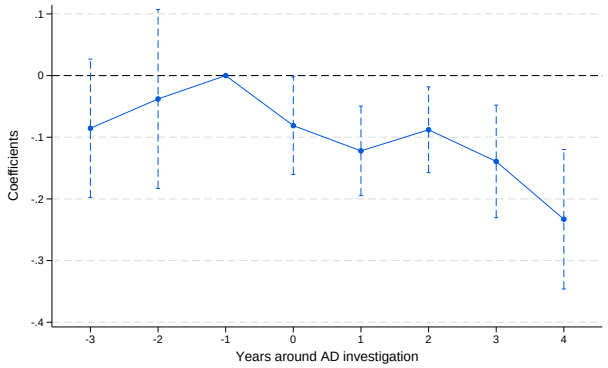


Panel D: Initiating Countries, by Hit Firm

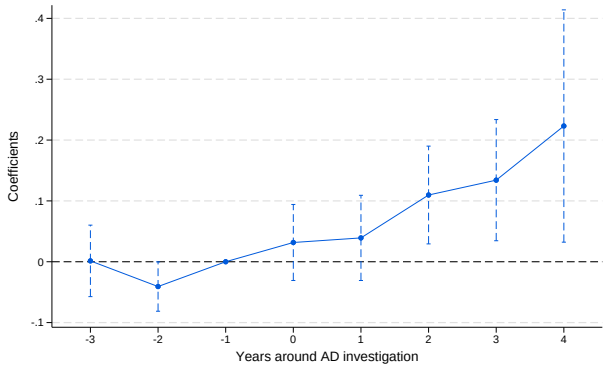
Figure 2: **Sample Distribution by Targeted Product and Initiating Country.** Panels A and B plot the distribution of AD cases initiated against Chinese exporters during 2006–2013 by targeted product and by initiating country. Panels C and D plot the distribution of hit firms in our sample by the product and the initiating country of the AD case they are subject to. Product is defined using Harmonized System (HS) Section codes; cases initiated by the European Union are pooled across member states. In each panel the top-10 categories are listed and the remaining categories are pooled into "Other."



Panel A: Have Export

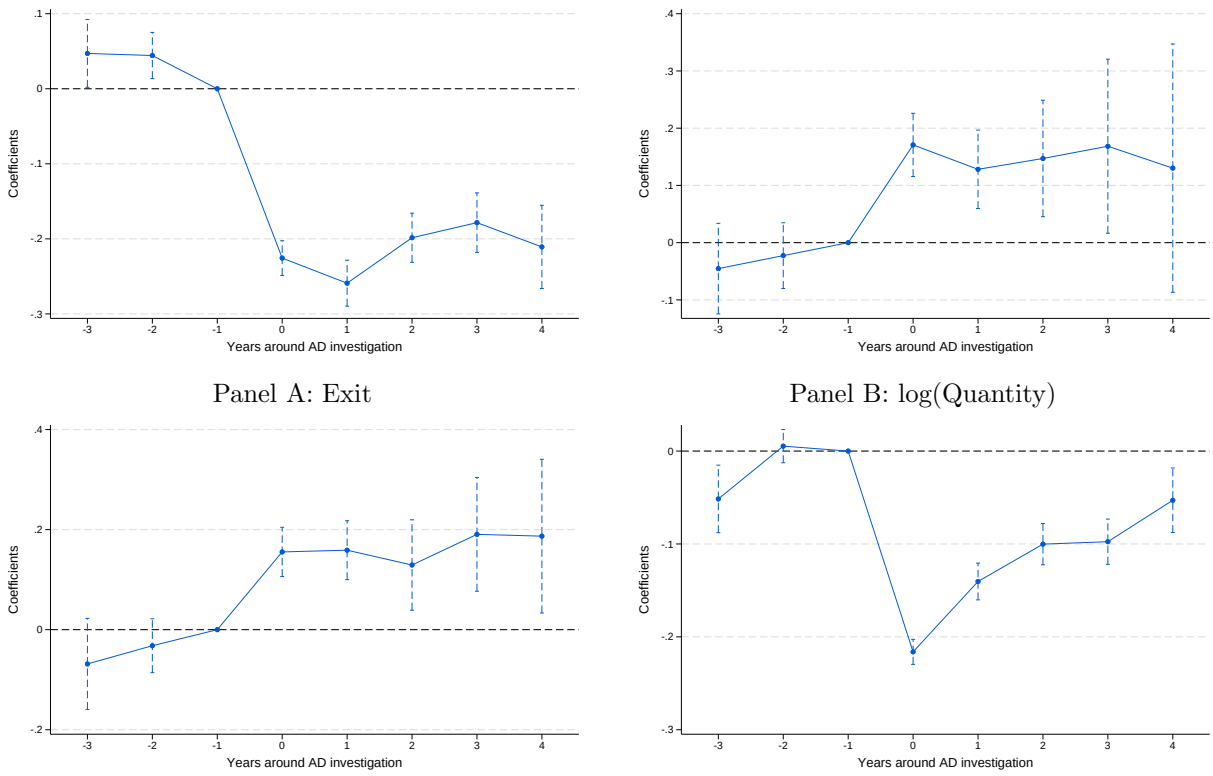


Panel B: log(Quantity)



Panel C: log(Price)

Figure 3: **Direct Effects of Anti-Dumping Investigation.** This figure plots the results from event studies around AD investigations, examining the differential change in a firm's exports of the AD product to the initiating country relative to its other destinations. The coefficients of interest are the interaction between *Treat*, the indicator for the destination being the initiating country, and event time indicators for three years before to four years after the investigation. Panel A reports results for Have Export, Panel B for log(Quantity), and Panel C for log(Price). All regressions control for firm–product–year and firm–destination–product fixed effects, as in Table 2. The blue dots represent the point estimates and the vertical lines represent 90 percent confidence intervals. The means of the outcome variables, Have Export, log(Quantity), and log(Price), are 0.560, 8.872, and 2.143, respectively.



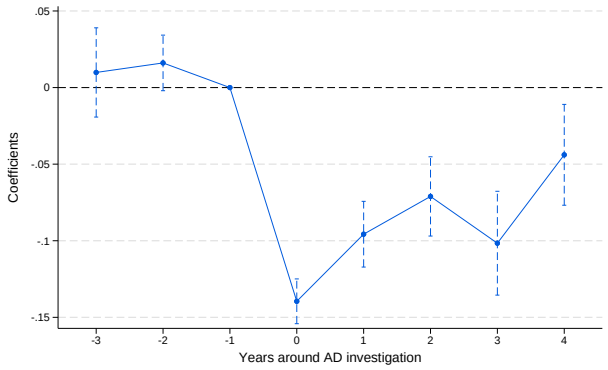
Panel A: Exit

Panel B: log(Quantity)

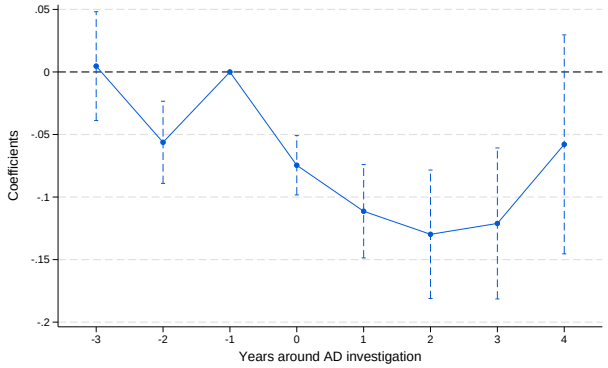
Panel C: log(Value)

Panel D: Exit, Non-AD Products

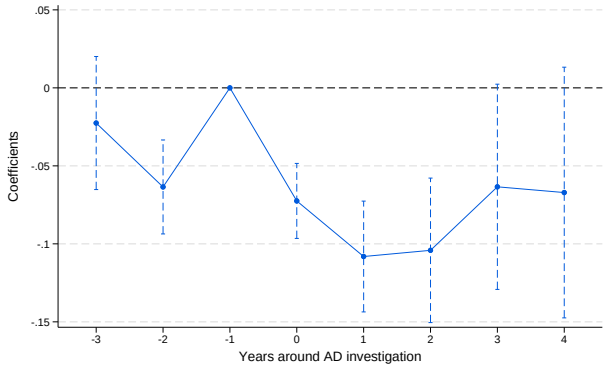
Figure 4: **Trade Diversion around Anti-Dumping Investigation.** This figure plots the dynamic effects of anti-dumping (AD) investigations on the exports of AD-hit firms. Panels A to C trace the AD product sold to non-AD destinations and Panel D the firm's non-AD products sold to the AD destination. The coefficients of interest are the interaction between *Treat* and event time indicators from three years before to four years after the investigation. Variables are defined in Table 3. All regressions control for firm–product–destination and destination–year fixed effects, each interacted with cohort indicators. The blue dots represent the point estimates and the vertical lines represent 90 percent confidence intervals based on standard errors double clustered by firm and destination country. The means of the outcome variables are 0.510, 9.144, 11.041, and 0.487 in Panels A to D, respectively.



Panel A: Exit

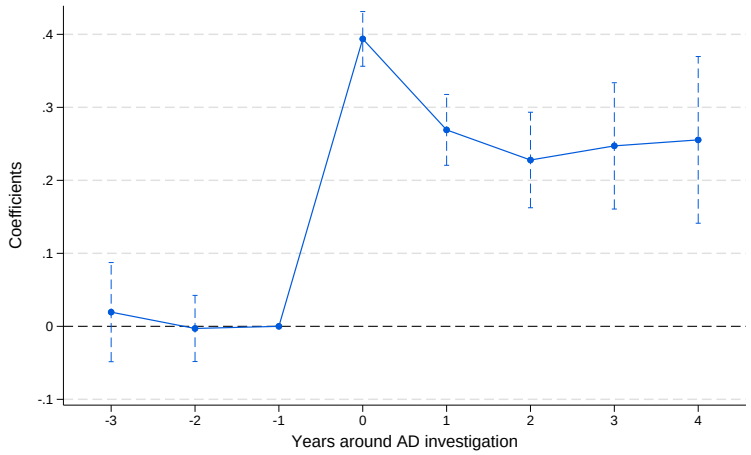


Panel B: log(Quantity)

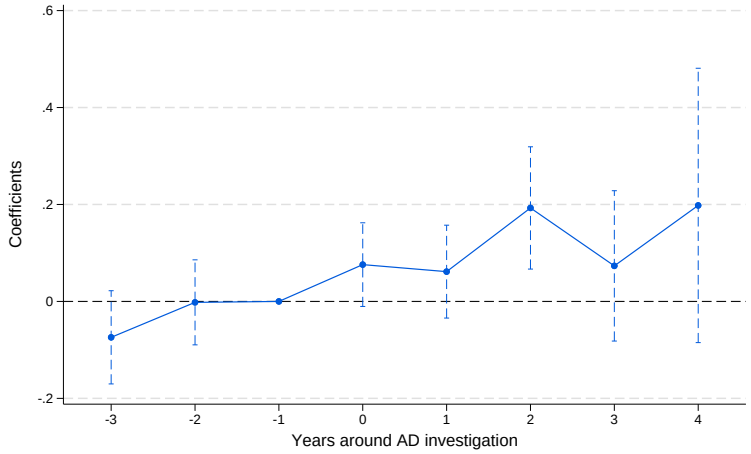


Panel C: log(Value)

Figure 5: **Spillover Effects of Anti-Dumping Investigation.** This figure plots the dynamic effects of anti-dumping (AD) investigations on exports of the firm's non-AD products to non-AD destinations. The coefficients of interest are the interaction between *Treat* and event time indicators. Variables are defined in Table 4. All regressions control for firm–product–destination and destination–year fixed effects, each interacted with cohort indicators. The blue dots represent the point estimates and the vertical lines represent 90 percent confidence intervals based on standard errors double clustered by firm and destination country. The means of the outcome variables, Exit, log(Quantity), and log(Value), are 0.516, 7.872, and 10.381, respectively.

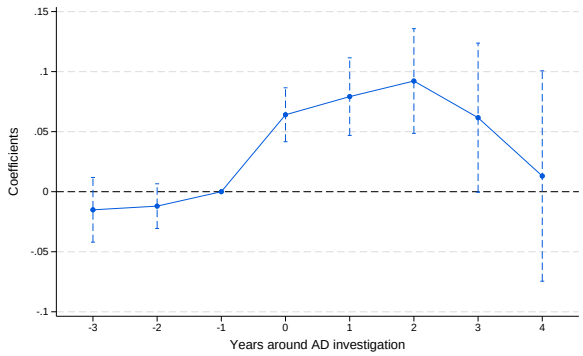


Panel A: Export Revenue

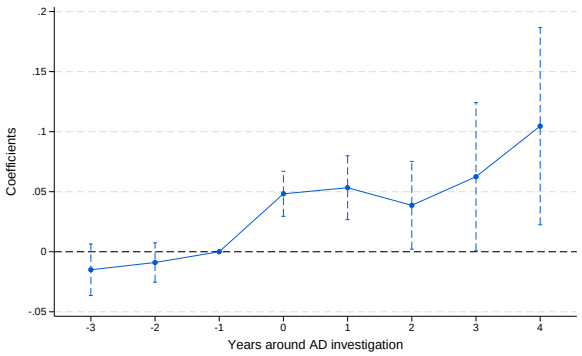


Panel B: log(Value Added)

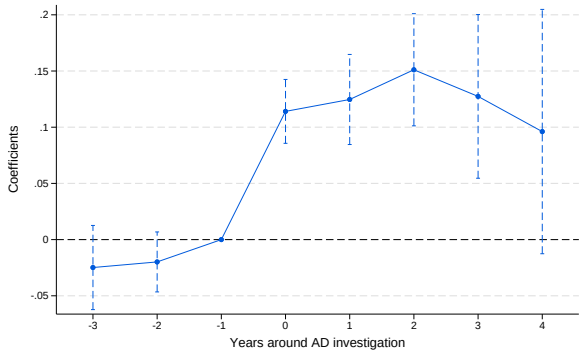
Figure 6: **Firm-Level Responses to Anti-Dumping: Export Revenue and Value Added** This figure plots the dynamic effects of anti-dumping (AD) investigations on the export revenue and value added of AD-hit firms. The coefficients of interest are the interaction between *Treat* and event time indicators from three years before to four years after the investigation. Panel A estimates export revenue in levels by Poisson pseudo-maximum likelihood and Panel B reports log(Value Added). Variables are defined in Table 5. All regressions control for firm and year fixed effects, each interacted with cohort indicators. The blue dots represent the point estimates and the vertical lines represent 90 percent confidence intervals based on standard errors clustered by firm.



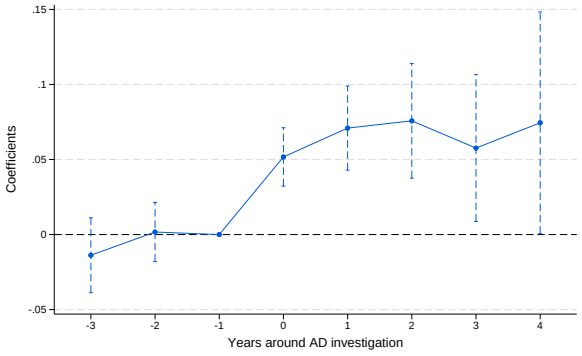
Panel A: log(COGS)



Panel B: log(Employees)

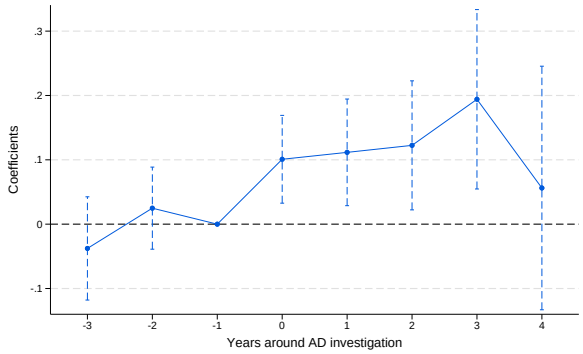


Panel C: log(Selling Expense)

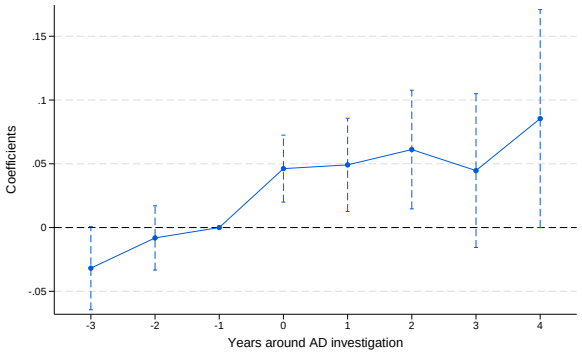


Panel D: log(Admin. Expense)

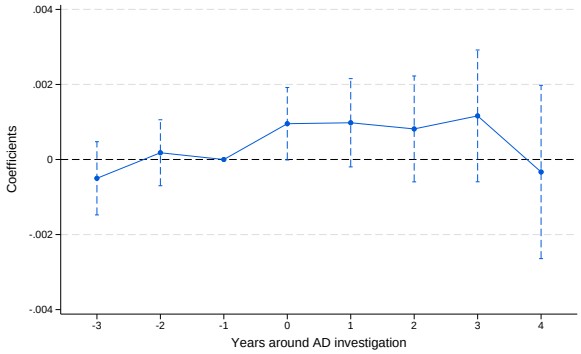
Figure 7: **Firm-Level Responses to Anti-Dumping: Operating Costs** This figure plots the dynamic effects of anti-dumping (AD) investigations on the operating costs of AD-hit firms. The coefficients of interest are the interaction between *Treat* and event time indicators from three years before to four years after the investigation. Panels A to D report the logarithm of COGS, number of employees, selling expense, and administrative expense. Variables are defined in Table 5. All regressions control for firm and year fixed effects, each interacted with cohort indicators. The blue dots represent the point estimates and the vertical lines represent 90 percent confidence intervals based on standard errors clustered by firm.



Panel A: log(Interest)



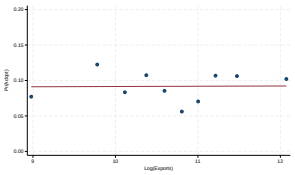
Panel B: log(Current Liabilities)



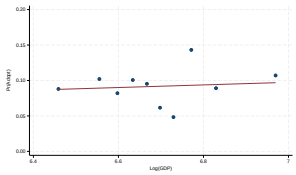
Panel C: Cost of Debt

Figure 8: **Firm-Level Responses to Anti-Dumping: Financing Costs** This figure plots the dynamic effects of anti-dumping (AD) investigations on the financing of AD-hit firms. The coefficients of interest are the interaction between *Treat* and event time indicators from three years before to four years after the investigation. Panels A to C report log(Interest), log(Current Liabilities), and Cost of Debt. Variables are defined in Table 5. All regressions control for firm and year fixed effects, each interacted with cohort indicators. The blue dots represent the point estimates and the vertical lines represent 90 percent confidence intervals based on standard errors clustered by firm.

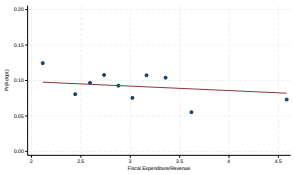
Figure 9: **Binscatter Plot of City Characteristics and Loan Program Adoption**
This figure plots the relation between city-level characteristics and adoption of the VAT rebate loan program at the city-year level. The x-axis in each panel indicates one of the eight city characteristics in Table A1. The y-axis indicates *1(Policy)*, an indicator that turns to one in the year a city adopts the program. The sample is the estimation sample of Table A1: city-years before and including the adoption year, and once a city adopts, it exits the sample. The dots represent the average adoption rate for each decile of the city characteristic, and the solid line represents the fitted regression line between the two variables. The regressions control for year and province fixed effects and for the other seven city characteristics. Both variables are re-centered at their sample means.



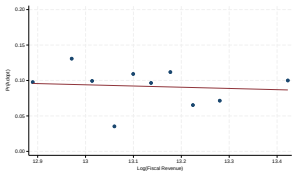
Panel A: Log(Exports)



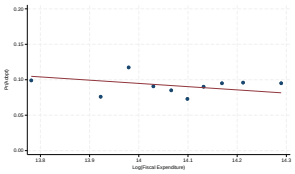
Panel B: Log(GDP)



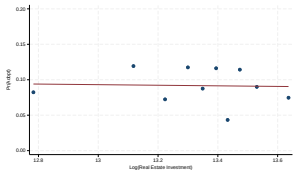
Panel C: Fiscal Expenditure/Revenue



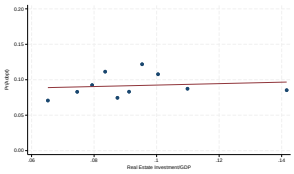
Panel D: Log(Fiscal Revenue)



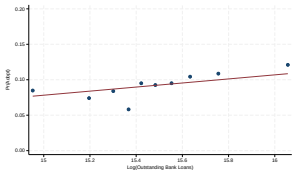
Panel E: Log(Fiscal Expenditure)



Panel F: Log(Real Estate Investment)



Panel G: Real Estate Investment/GDP



Panel H: Log(Outstanding Bank Loans)

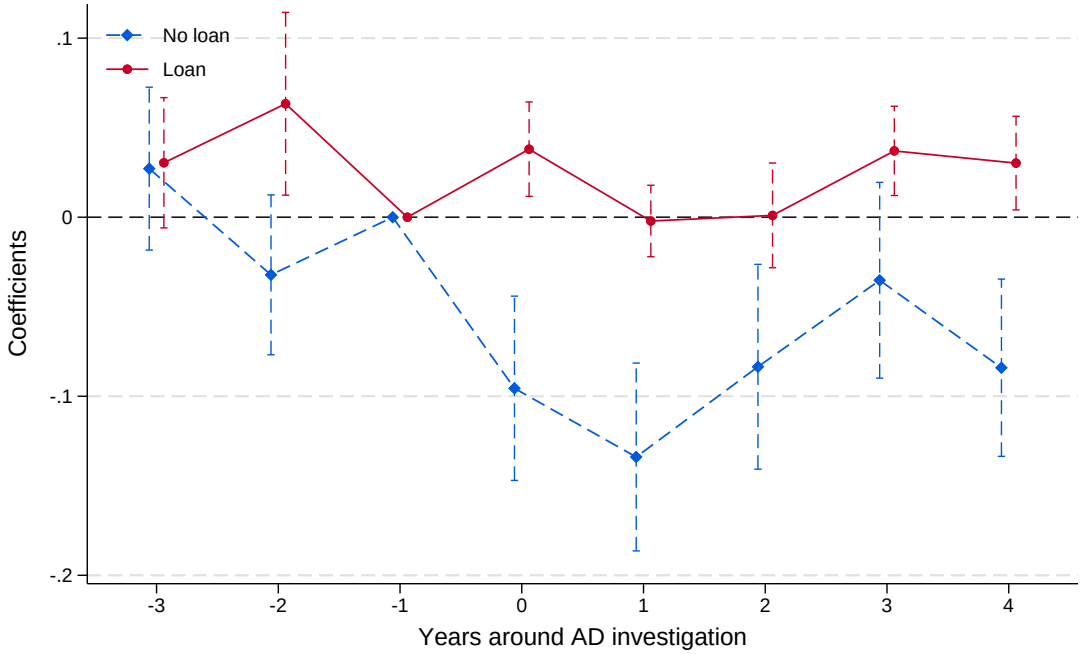


Figure 10: **Government Credit and Trade Diversion: Dynamic Effects by Loan Access.** This figure plots the results from event studies around AD investigations, examining the differential change in a firm's exports of the AD product to the initiating country relative to its other destinations, estimated separately for firms with and without access to the VAT rebate loan program. *Loan* is an indicator that equals one if the firm's city adopted the rebate-loan program before the investigation and zero if it never adopted the program; firms in cities that adopted the program in or after the investigation year are excluded. The coefficients of interest are the interaction between *Treat*, the indicator for the destination being the initiating country, and event time indicators for three years before to four years after the investigation, with the year before the investigation as the benchmark. The dependent variable is *Have Export*, an indicator that turns to one if a firm exports the product to a destination during a year. All regressions control for firm–product–year and firm–destination–product fixed effects, as in Table 2. The red dots (blue diamonds) represent the point estimates for firms with (without) loan access and the vertical lines represent 90 percent confidence intervals. The mean of Have Export is 0.575.

Table 1: **Descriptive Statistics**
This table reports summary statistics for the main variables in each estimation sample, including trade quantity measure, loan program adoption, and firm performance. Detailed variable definitions are provided in Appendix Appendix A.

Variable	N	Mean	Median	Std
Panel A: Direct Effect Sample				
Have Export	416,259	0.560	1	0.496
Log(Quantity)	233,293	8.872	9.220	3.196
Log(Value)	233,293	11.016	11.220	2.537
Loan	249,383	0.849	1	0.358
Panel B: Diversion Samples				
B1: AD Product to Other Destinations				
Exit	103,672	0.627	1	0.484
Log(Quantity)	104,086	8.801	9.192	3.170
Log(Value)	104,086	10.679	10.786	2.370
B2: Other Products to the AD Destination				
Exit	74,177	0.620	1	0.485
Log(Quantity)	74,069	7.774	8.071	3.470
Log(Value)	74,069	10.319	10.437	2.724
B3: Other Products to Other Destinations				
Exit	624,964	0.673	1	0.469
Log(Quantity)	622,704	7.426	7.732	3.341
Log(Value)	622,704	9.911	10.031	2.477
Panel C: Firm-Level Samples				
Log(Export Value)	71,466	13.864	14.107	2.000
ROA	40,803	0.027	0.015	0.101
Cost of Debt	32,031	0.018	0.009	0.022

Table 2: **Direct Effects of Anti-Dumping Investigation**

This table reports the effects of an AD investigation on a firm's exports of the AD product to the initiating country relative to its other destinations. The sample is a firm–product–destination–year panel from three years before to four years after the investigation. It covers firm–products that faced a single AD investigation and that the firm had exported to the initiating country. I keep active flows, i.e., firm–product–destination pairs with positive export volume for at least 40% of the window years. The "Extensive Margin" tests use this full sample. The "Intensive Margin" tests use only observations with nonzero export volume. The "Intensive Margin" tests use only observations with nonzero export volume. *Treat* is an indicator that equals one if the destination is the initiating country. *Post* is an indicator that equals one from the investigation year onward. *Have Export* is an indicator that turns to one if a firm exports the product to a destination during a year. *Quantity (Value)* refers to the export quantity (value in USD) of the product shipped by the firm to a destination in a year. *Price* is the average unit value in USD. The means of the outcome variables, Have Export, log(Quantity), log(Value), and log(Price), are 0.560, 8.872, 11.016, and 2.143, respectively. Standard errors are reported in parentheses and are double clustered by firm and destination country. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

	Extensive Margin		Intensive Margin					
Dep. Var.:	*Have Export*		*log(Quantity)*		*log(Value)*		*log(Price)*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat × *Post*	−0.026**	−0.026**	−0.087**	−0.092**	−0.006	−0.011	0.097**	0.097**
	(0.011)	(0.011)	(0.038)	(0.037)	(0.057)	(0.056)	(0.044)	(0.045)
Firm-Product-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Destination FE	Yes		Yes		Yes		Yes	
Destination-Product FE	Yes		Yes		Yes		Yes	
Firm-Destination-Product FE		Yes		Yes		Yes		Yes
Observations	372,693	372,693	204,299	204,268	204,299	204,268	204,299	204,268
Adj. R^2	0.288	0.311	0.756	0.776	0.689	0.715	0.842	0.855

Table 3: **Trade Diversion around Anti-Dumping Investigation**
This table examines the effects of anti-dumping (AD) investigations on the exports of AD-hit firms to other destinations and of other products. The sample is a firm–product–destination–year panel of AD-hit firms and matched never-hit firms from three years before to four years after the investigation. Panel A examines the AD product sold to non-AD destinations and Panel B the firmś non-AD products sold to the AD destination. *Treat* is an indicator that equals one if the firm faced an AD investigation. *Post* is an indicator that equals one from the investigation year onward. *Exit* is an indicator that turns to one if a firm exported a product to a destination in the previous year but not in the current year. *Quantity (Value)* refers to the export quantity (value in USD) of the product shipped by the firm to a destination in a year. *Price* is the average unit value in USD. The means of Exit, log(Quantity), log(Value), and log(Price) are 0.510, 9.144, 11.041, and 1.884 in Panel A and 0.487, 8.288, 10.855, and 2.570 in Panel B. All fixed effects are interacted with cohort indicators. Standard errors are reported in parentheses and are double clustered by firm and destination country. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: AD Product to Other Destinations

	Extensive	Intensive Margin		
Dep. Var.:	*Exit*	*log(Quantity)*	*log(Value)*	*log(Price)*
	(1)	(2)	(3)	(4)
Treat × *Post*	−0.243***	0.166***	0.175***	0.004
	(0.014)	(0.030)	(0.029)	(0.017)
Firm-Product-Destination FE	Yes	Yes	Yes	Yes
Destination-Year FE	Yes	Yes	Yes	Yes
Observations	78,182	77,786	77,786	77,786
Adj. R^2	0.097	0.792	0.695	0.873

Panel B: Non-AD Products to the AD Destination

	Extensive	Intensive Margin		
Dep. Var.:	*Exit*	*log(Quantity)*	*log(Value)*	*log(Price)*
	(1)	(2)	(3)	(4)
Treat × *Post*	−0.151***	−0.026	0.003	0.031*
	(0.009)	(0.036)	(0.028)	(0.017)
Firm-Product-Destination FE	Yes	Yes	Yes	Yes
Destination-Year FE	Yes	Yes	Yes	Yes
Observations	54,851	54,134	54,134	54,134
Adj. R^2	0.093	0.797	0.721	0.869

Table 4: **Spillover Effects of Anti-Dumping Investigation**
This table examines the effects of anti-dumping (AD) investigations on the exports of hit firms that are unrelated to the case: the firm's non-AD products sold to non-AD destinations. The sample is a firm–product–destination–year panel of AD-hit firms and matched never-hit firms from three years before to four years after the investigation. *Treat* is an indicator that equals one if the firm faced an AD investigation. *Post* is an indicator that equals one from the investigation year onward. *Exit* is an indicator that turns to one if a firm exported a product to a destination in the previous year but not in the current year. *Quantity (Value)* refers to the export quantity (value in USD) of the product shipped by the firm to a destination in a year. *Price* is the average unit value in USD. The means of Exit, log(Quantity), log(Value), and log(Price) are 0.516, 7.872, 10.381, and 2.499. All fixed effects are interacted with cohort indicators. Standard errors are reported in parentheses and are double clustered by firm and destination country. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

	Extensive	Intensive Margin		
Dep. Var.:	*Exit*	*log(Quantity)*	*log(Value)*	*log(Price)*
	(1)	(2)	(3)	(4)
Treat × *Post*	−0.113***	−0.081***	−0.064***	0.017*
	(0.011)	(0.019)	(0.020)	(0.010)
Firm-Product-Destination FE	Yes	Yes	Yes	Yes
Destination-Year FE	Yes	Yes	Yes	Yes
Observations	421,810	413,507	413,507	413,507
Adj. R^2	0.080	0.814	0.709	0.905

Table 5: **Firm-Level Responses to Anti-Dumping Investigation**
This table examines the effects of anti-dumping (AD) investigations on the export performance, profitability, costs, and financing of AD-hit firms. The sample is a balanced firm–year panel of AD-hit firms and matched never-hit firms in the same industry. *Treat* is an indicator that equals one if the firm faced an AD investigation. *Post* is an indicator that equals one from the investigation year onward. *Export Revenue* is the firm's total export value in USD; column (1) of Panel A estimates it in levels by Poisson pseudo-maximum likelihood. *Value Added* is the firm's annual value added. *ROA* is operating profit divided by total assets. *COGS* is the cost of goods sold. *Employees* is the number of employees. *Selling* and *Admin.* are the firm's total selling and administrative expenses. *Advertising* is advertising spending. *Entertain.* is business entertainment spending. *R&D* is spending on research and development. *Interest Exp.* is total interest paid. *Current Liab.* is debt due within one year. *Cost of Debt* is interest expense divided by year-end total liabilities. All fixed effects are interacted with cohort indicators, where a cohort is the investigation year. Standard errors are reported in parentheses and are clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: Export Revenue and Profitability

Dep. Var.:	*Export Revenue*	*log(Exports)*	*log(Value Added)*	*ROA*
	(1)	(2)	(3)	(4)
Treat × *Post*	0.293***	0.342***	0.118***	0.001
	(0.028)	(0.024)	(0.041)	(0.002)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	119,094	70,510	26,678	40,803
Adj. R^2	0.741	0.666	0.642	0.513

Panel B: Operating and Selling Costs

Dep. Var.: log(Y)	*COGS*	*Employees*	*Selling*	*Admin.*	*Advertising*	*Entertain.*	*R&D*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treat × *Post*	0.080***	0.060***	0.140***	0.068***	0.176*	0.054*	0.161**
	(0.017)	(0.014)	(0.021)	(0.015)	(0.090)	(0.032)	(0.078)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	40,773	40,990	36,534	40,525	7,088	27,312	3,523
Adj. R^2	0.911	0.912	0.882	0.926	0.607	0.746	0.758

Panel C: Financing Costs

Dep. Var.:	*log(Interest Exp.)*	*log(Current Liab.)*	*Cost of Debt*
	(1)	(2)	(3)
Treat × *Post*	0.120***	0.065***	0.001**
	(0.040)	(0.019)	(0.000)
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	21,024	40,047	31,785
Adj. R^2	0.782	0.914	0.489

Table 6: **Firm Financing around Anti-Dumping Investigations by Access to Government Credit**

This table examines whether access to the VAT-rebate loan program changes the financing responses of AD-hit firms to anti-dumping (AD) investigations. The sample is the firm–year panel of Table 5, restricted to firms in cities that adopted the program before the investigation or never adopted it. *Treat* is an indicator that equals one if the firm faced an AD investigation. *Post* is an indicator that equals one from the investigation year onward. *Loan* is an indicator that equals one if the firm's city adopted the rebate-loan program before the investigation and zero if it never adopted the program. *Interest Exp.* is total interest expenses. *Current Liab.* is debt due within one year. *Cost of Debt* is interest expense divided by year-end total liabilities. The means of the outcome variables, log(Interest), log(Current Liabilities), and Cost of Debt, are 7.189, 10.480, and 0.017, respectively. All fixed effects are interacted with cohort indicators, where a cohort is the investigation year. Standard errors are reported in parentheses and are clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Dep. Var.:	*log(Interest Exp.)*	*log(Current Liab.)*	*Cost of Debt*
	(1)	(2)	(3)
Treat × *Post*	0.175	0.044	0.002*
	(0.122)	(0.046)	(0.001)
Treat × *Post* × *Loan*	−0.164	−0.012	−0.002*
	(0.133)	(0.052)	(0.001)
Firm FE	Yes	Yes	Yes
City-Year FE	Yes	Yes	Yes
Observations	15,392	30,303	23,535
Adj. R^2	0.782	0.915	0.509

Table 7: **Direct Effects of AD Investigations by Access to Government Credit**
This table examines whether the VAT-rebate loan program buffers the effect of an anti-dumping (AD) investigation on a firm's exports of the AD product to the initiating country. The sample is a firm–product–destination–year panel of firm–products that faced a single AD investigation. *Treat* is an indicator that equals one for the initiating country. *Post* is an indicator that equals one from the investigation year onward. *Loan* is an indicator that equals one if the firm's city adopted the rebate-loan program before the investigation and zero if it never adopted the program. *Have Export* is an indicator that turns to one if a firm exports the product to a destination during a year. *Quantity (Value)* refers to the export quantity (value in USD) of the product shipped by the firm to a destination in a year. *Price* is the average unit value in USD. Standard errors are reported in parentheses and are double clustered by firm and destination country. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

	Extensive	Intensive Margin		
Dep. Var.:	*Have Export*	*log(Quantity)*	*log(Value)*	*log(Price)*
	(1)	(2)	(3)	(4)
Treat × *Post*	−0.082***	−0.140	−0.074	0.141**
	(0.018)	(0.120)	(0.101)	(0.056)
Treat × *Post* × *Loan*	0.070***	0.095	0.090	−0.077
	(0.018)	(0.133)	(0.134)	(0.053)
Firm-Product-Year FE	Yes	Yes	Yes	Yes
Firm-Destination-Product FE	Yes	Yes	Yes	Yes
Observations	224,916	126,437	126,437	126,437
Adj. R^2	0.288	0.766	0.720	0.837

Table 8: **Spillover Effects of Anti-Dumping by Access to Government Credit**
This table examines whether access to the VAT-rebate loan program changes the spillover of AD investigations to the exports of AD-hit firms that are unrelated to the case: the firm's non-AD products sold to non-AD destinations. The sample is the firm–product–destination–year panel of Table 4, restricted to firms in cities that adopted the program before the investigation or never adopted it. *Treat* is an indicator that equals one if the firm faced an AD investigation. *Post* is an indicator that equals one from the investigation year onward. *Loan* is an indicator that equals one if the firm's city adopted the rebate-loan program before the investigation and zero if it never adopted the program. *Exit* is an indicator that turns to one if a firm exported a product to a destination in the previous year but not in the current year. *Quantity (Value)* refers to the export quantity (value in USD) of the product shipped by the firm to a destination in a year. *Price* is the average unit value in USD. The means of Exit, log(Quantity), log(Value), and log(Price) are 0.514, 7.793, 10.368, and 2.567. All fixed effects are interacted with cohort indicators, where a cohort is the investigation year. Standard errors are reported in parentheses and are double clustered by firm and destination country. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

	Extensive	Intensive Margin		
Dep. Var.:	*Exit*	*log(Quantity)*	*log(Value)*	*log(Price)*
	(1)	(2)	(3)	(4)
Treat × *Post*	−0.072***	−0.110***	−0.095***	0.020
	(0.016)	(0.032)	(0.030)	(0.023)
Treat × *Post* × *Loan*	−0.037**	0.025	0.018	−0.017
	(0.018)	(0.038)	(0.038)	(0.025)
Firm-Product-Destination FE	Yes	Yes	Yes	Yes
Destination-Year FE	Yes	Yes	Yes	Yes
City-Year FE	Yes	Yes	Yes	Yes
Observations	333,963	326,494	326,494	326,494
Adj. R^2	0.119	0.811	0.713	0.902

Appendix A. Variable Definitions

This appendix defines the variables used in the tables and figures. Trade variables are measured at the firm–HS6 product–destination country–year level from Chinese customs records; firm variables are measured at the firm–year level from the National Tax Survey; city variables are measured at the city–year level.

A. Anti-Dumping Cases and Treatment

- *AD Case*: An anti-dumping (AD) investigation initiated against Chinese exporters, defined at the initiating country–HS6 product level, so that an investigation covering several products counts once for each product. When the same country–product pair is investigated more than once, only the first initiation is kept. A case is dated by its initiation year, with initiations in the second half of a year assigned to the following year. Cases initiated by the European Union are counted once for the Union and its member states are pooled as one destination. Data source: GAD.
- *AD Product*: The HS6 product named in the AD case. *AD Destination*, or the initiating country, is the country that initiated the case. Data source: GAD.
- *AD Product, Non-AD Destinations*: Export flows of the AD product by a hit firm to destinations other than the initiating country (Table 3, Panel A). Data source: GAD and CGAC.
- *Non-AD Products, AD Destination*: Export flows of a hit firm's products other than the AD product to the initiating country (Table 3, Panel B). Data source: GAD and CGAC.
- *Non-AD Products, Non-AD Destinations*: Export flows of a hit firm's products other than the AD product to destinations other than the initiating country (Table 4). Data source: GAD and CGAC.

B. Trade Flow Variables The continuous variables are winsorized at the 0.1% and 99.9% levels.

- *Have Export*: An indicator variable that equals one if a firm exports a certain HS6 product to a destination country during a year, and zero otherwise. The flow panel is balanced over the event window, so a flow–year without a customs record is coded as zero. Data source: CGAC.
- *Exit*: An indicator variable, defined for flow–years in which the firm exported the product to the destination in the previous year, that equals one if the firm does not export the product to the destination in the current year, and zero otherwise. Data source: CGAC.
- *Quantity*: The export quantity, in the product-specific physical unit recorded by customs, of a certain HS6 product shipped by a firm to a destination country during a year. Data source: CGAC.
- *Value*: The export value (in USD) of a certain HS6 product shipped by a firm to a destination country during a year. Data source: CGAC.
- *Price*: The average unit value (in USD) of a certain HS6 product shipped by a firm to a destination country during a year. Data source: CGAC.

C. Firm-Level Variables

Accounting variables are taken from the firm's annual tax survey return. Each continuous variable is winsorized at the 1% and 99% levels among positive values in the estimation sample before taking logs.

- *Export Revenue*: The firm's total export value (in USD) across all products and destinations during a year. A firm–year with no customs record is coded as zero. Positive values are winsorized at the 0.1% and 99.9% levels. Data source: CGAC.
- *Value Added*: The firm's value added during a year. Data source: NTS.
- *ROA*: Operating profit divided by year-end total assets. Data source: NTS.
- *COGS*: The cost of goods sold, i.e., the firm's cost of its main business operations during a year. Data source: NTS.
- *Employees*: The firm's number of employees during a year. Data source: NTS.
- *Selling Expense* (*Selling*): The firm's total selling expenses during a year. Data source: NTS.
- *Administrative Expense* (*Admin.*): The firm's total general and administrative expenses during a year. Data source: NTS.
- *Advertising*: The firm's advertising expenditure during a year, a component of selling expense. Data source: NTS.
- *Entertainment* (*Entertain.*): The firm's business entertainment expenditure during a year, a component of administrative expense. Data source: NTS.
- *R&D*: The firm's research and development expenditure during a year, a component of administrative expense. Data source: NTS.
- *Interest Expense* (*Interest Exp.*): The firm's total interest expenses during a year. Data source: NTS.
- *Current Liabilities* (*Current Liab.*): The firm's year-end current liabilities, i.e., debt due within one year. Data source: NTS.
- *Cost of Debt*: Interest expense during a year divided by year-end total liabilities. Data source: NTS.

D. VAT Rebate Loan Program

- *Loan*: An indicator variable that equals one if the firm's headquarter city adopted the VAT rebate loan program before the investigation year, and zero if the city never adopted the program. The firm's city is its registered location in the tax survey. Data source: hand-collected city–year adoption records from official government documents and NTS.

E. City-Level Variables

City characteristics are winsorized at the 1% and 99% levels.

- *1(Policy)*: An indicator variable that equals one in the year a city adopts the VAT rebate loan program, for any stated motive, and zero otherwise. Data source: hand-collected city–year adoption records from official government documents.
- *1(Long-term Econ. Dev.)*, *1(Export Slowdown)*, *1(Credit Constraint)*: Indicator variables that equal one in the year a city adopts the VAT rebate loan program citing, respectively, long-term economic development, an export slowdown, or firms' credit constraints as the stated motive, and zero otherwise. Motives are classified from the adopting document following the narrative approach of Romer and Romer (2010); the classification is reported in Table A2. Data source: hand-collected city–year adoption records from official government documents.
- *Log(Exports)*: The natural logarithm of the city's total export value during a year. Data source: CSMAR.
- *Log(GDP)*: The natural logarithm of the city's gross domestic product during a year. Data source: CSMAR.
- *Fiscal Expenditure/Revenue*: The city government's budgetary expenditure divided by its budgetary revenue during a year, capturing local fiscal pressure. Data source: CSMAR.
- *Log(Fiscal Revenue)*: The natural logarithm of the city government's budgetary revenue during a year. Data source: CSMAR.
- *Log(Fiscal Expenditure)*: The natural logarithm of the city government's budgetary expenditure during a year. Data source: CSMAR.
- *Log(Real Estate Investment)*: The natural logarithm of the city's real estate development investment during a year. Data source: CSMAR.
- *Real Estate Investment/GDP*: The city's real estate development investment divided by its GDP during a year. Data source: CSMAR.
- *Log(Outstanding Bank Loans)*: The natural logarithm of the year-end outstanding balance of loans issued by financial institutions located in the city, capturing local credit supply. Data source: CSMAR.

Appendix B. Figures and Tables

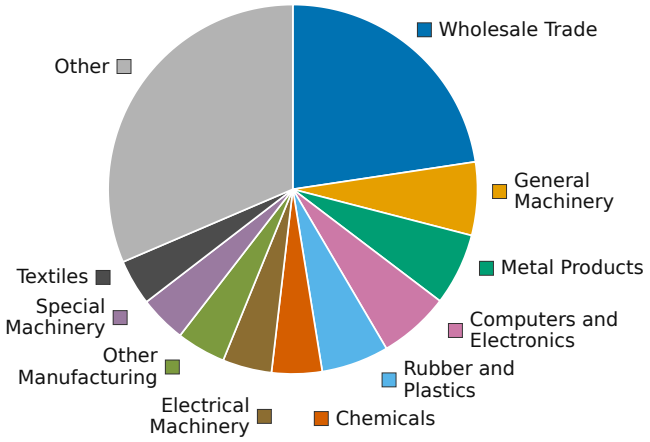
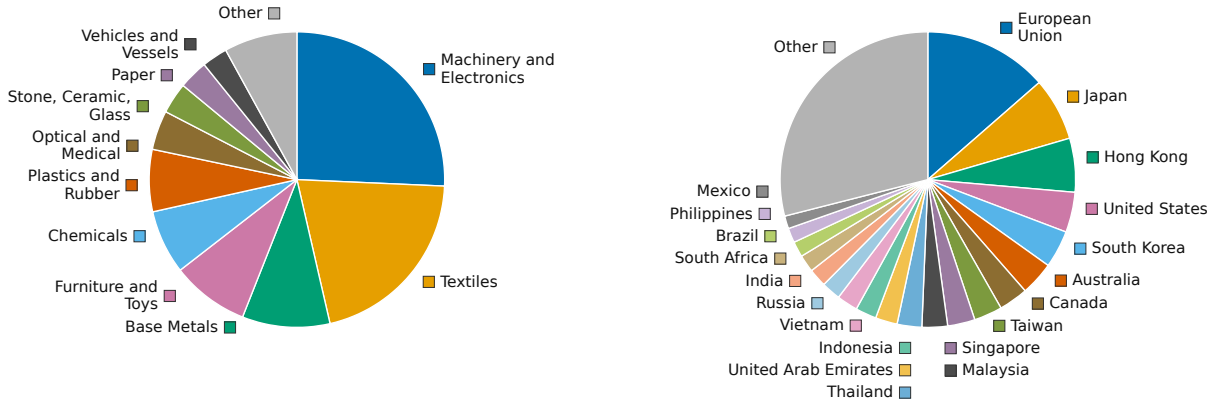


Figure A1: **Distribution of hit firms by industry.** This figure plots the distribution of hit firms in our sample across industries (CIC 2-digit code), where the top-10 represented industries are listed and the remaining industries are pooled into "Other."



Panel A: Other Products of Hit Firms

Panel B: Other Export Markets of Hit Firms

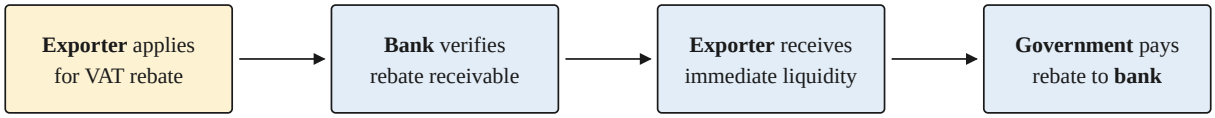
Figure A2: **Other export flows of hit firms by product and destination.** This figure plots the distribution of hit firms' export flows other than the targeted flow, which form the stacked diversion samples. The sample is at the firm–product–destination–cohort level. Panel A reports the share of flows in non-targeted products, where product is defined using Harmonized System (HS) Section codes. Panel B reports the share of flows to non-targeted destinations by country, with European Union member states pooled. In each panel the largest categories are listed and the remaining categories are pooled into "Other."

Panel A. *Without the Loan*



Average rebate ratio: 10–13% of export value

Panel B. *With the Loan*



Lending cap: 70–90% of expected rebate

Figure A3: **VAT rebate and loan program.** This figure illustrates the VAT rebate process for Chinese exporters under two scenarios. Panel A depicts the standard process without VAT rebate loan program, while Panel B shows the process with loan program.

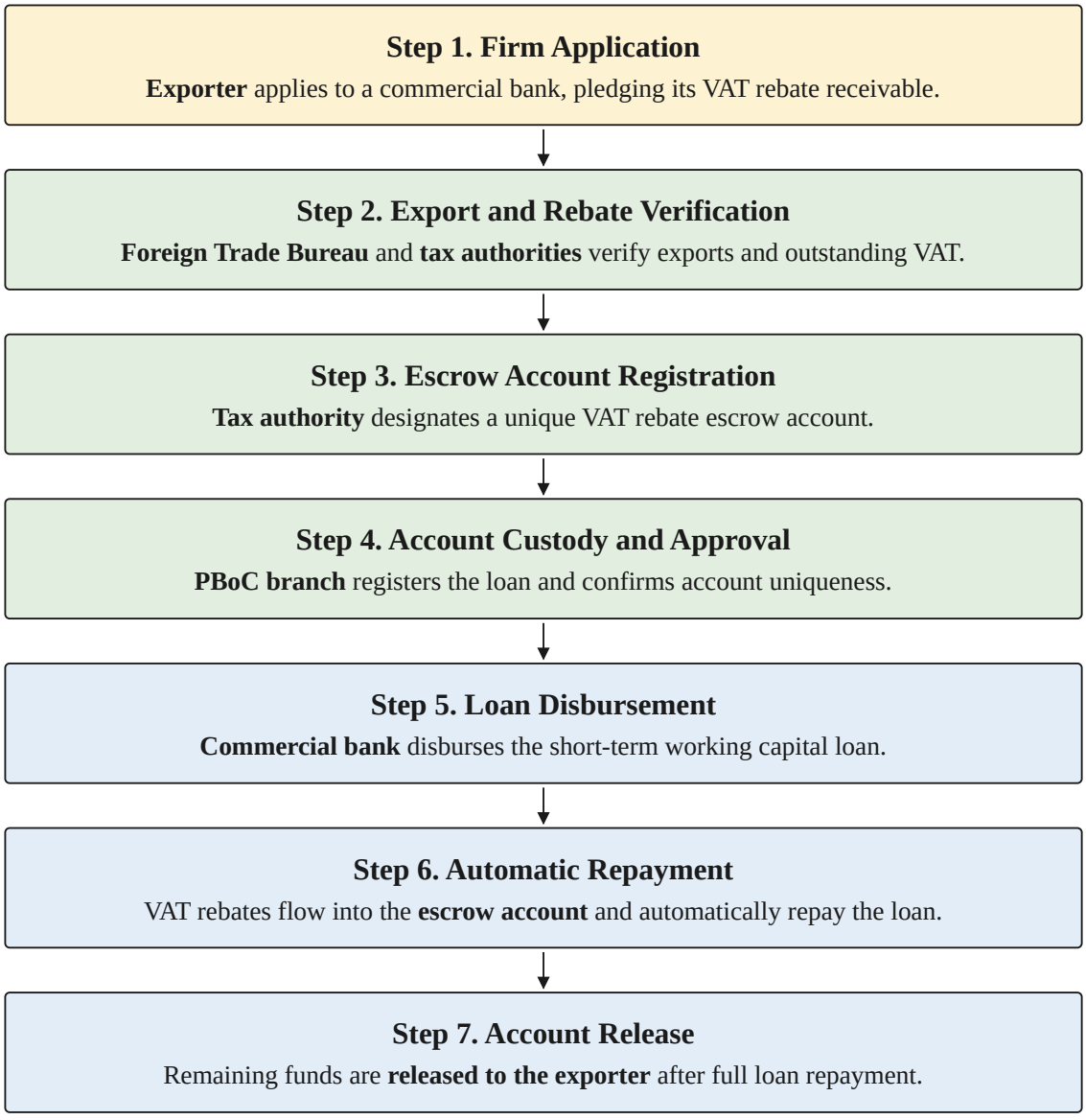


Figure A4: **VAT Rebate Receivable Loan Application and Disbursement Process.** This figure illustrates the seven-step process through which exporters obtain rebate-backed loans, from initial application to final account release. The process involves coordination among exporters, commercial banks, tax authorities, and the local branch of the People's Bank of China (PBoC). At its core is an escrow account in which VAT rebates, once received, are automatically applied to loan repayment.



退税质押缓解乌鲁木齐外贸企业资金紧缺压力

商务微新闻
中华人民共和国商务部新闻办 2011.07.11 16:40

由于央行连调存款准备金率，在资金面紧张的局面下，乌鲁木齐市进出口企业纷纷把目光转向了出口退税质押贷款。目前，乌鲁木齐市国税局已和乌鲁木齐市多家银行联合推出了出口退税质押贷款业务，银行由最初的1家扩至6家，包括建设银行、华夏银行、招商银行等。实行出口退税账户质押贷款后，企业申请质押贷款占其应得退税款的最高比例为90%，有效缓解了外贸企业流动资金紧缺的压力；出口退税账户质押贷款利率在同期商业银行基准贷款利率的基础上适当下浮，降低了企业的融资成本。

Export VAT Rebate Loans Alleviate Liquidity Constraints for Exporters in Urumqi (2011)

Following successive increases in the required reserve ratio by the People's Bank of China, liquidity conditions tightened. This prompted import and export firms in Urumqi to rely on VAT rebate loans for financing. In cooperation with several commercial banks, expanding from one to six, the local tax authority implemented a program allowing firms to borrow up to 90% of their VAT rebate receivables as collateral. This program eased working capital constraints for exporters, with loan rates set below the benchmark lending rate, thereby lowering financing costs.

Panel A: Policy Document from Shenzhen (2009)

二、鼓励商业银行开展出口退税账户托管贷款业务，对企业办理出口退税质押贷款给予贷款利息适当资助

（一）资助条件

在深圳市商业银行办理出口退税账户托管贷款业务并按时还清本息的在深圳市注册企业（以下简称"企业"）。

（二）资助标准

按企业实际已付贷款利息给予适当资助。

（三）申报材料

1、出口退税账户托管贷款利息资助申请表；

2、市国税局出具的"账户托管贷款企业出口退税申报证明"；

3、借款合同复印件；

4、办理贷款所产生的借据、归还借款的票据、利息单复印件。

（一）申报程序

1、在企业按时还清本息后60天内，贷款银行须向市贸工局计财处提交申报材料；

2、市贸工局计财处在收到资助申报材料30个工作日内核准资助金额，并转市财政局外金处复审、拨款。

II. Encourage Commercial Banks to Provide Export VAT Rebate Loans and Grant Interest Subsidies

(I) Eligibility

Firms registered in Shenzhen that have VAT rebate loans and repay both principal and interest on time from Shenzhen Commercial Bank.

(II) Subsidy Standard

Subsidies are based on the actual interest paid on the loan.

(III) Application Materials

1. Application form for the interest subsidy;
2. Export VAT Rebate Certificate issued by the Tax Bureau;
3. Copy of the loan contract;
4. Copies of loan receipts, repayment vouchers, and interest statements.

(IV) Application Procedures

1. Within 60 days of full repayment, the lending bank shall submit the application materials to the Bureau of Trade and Industry;
2. Within 30 working days of receipt, the Bureau determines the subsidy amount and forwards it to the Finance Bureau for review and disbursement.

Panel B: Media Coverage from Urumqi (2011)

Figure A5: **Examples of VAT Rebate Loan Program Announcements.** This figure presents two examples of local government announcements promoting VAT rebate loan programs. Panel A shows a policy document from Shenzhen (2009) outlining program requirements and procedures. Panel B shows media coverage of export VAT rebate loans in Urumqi (2011).

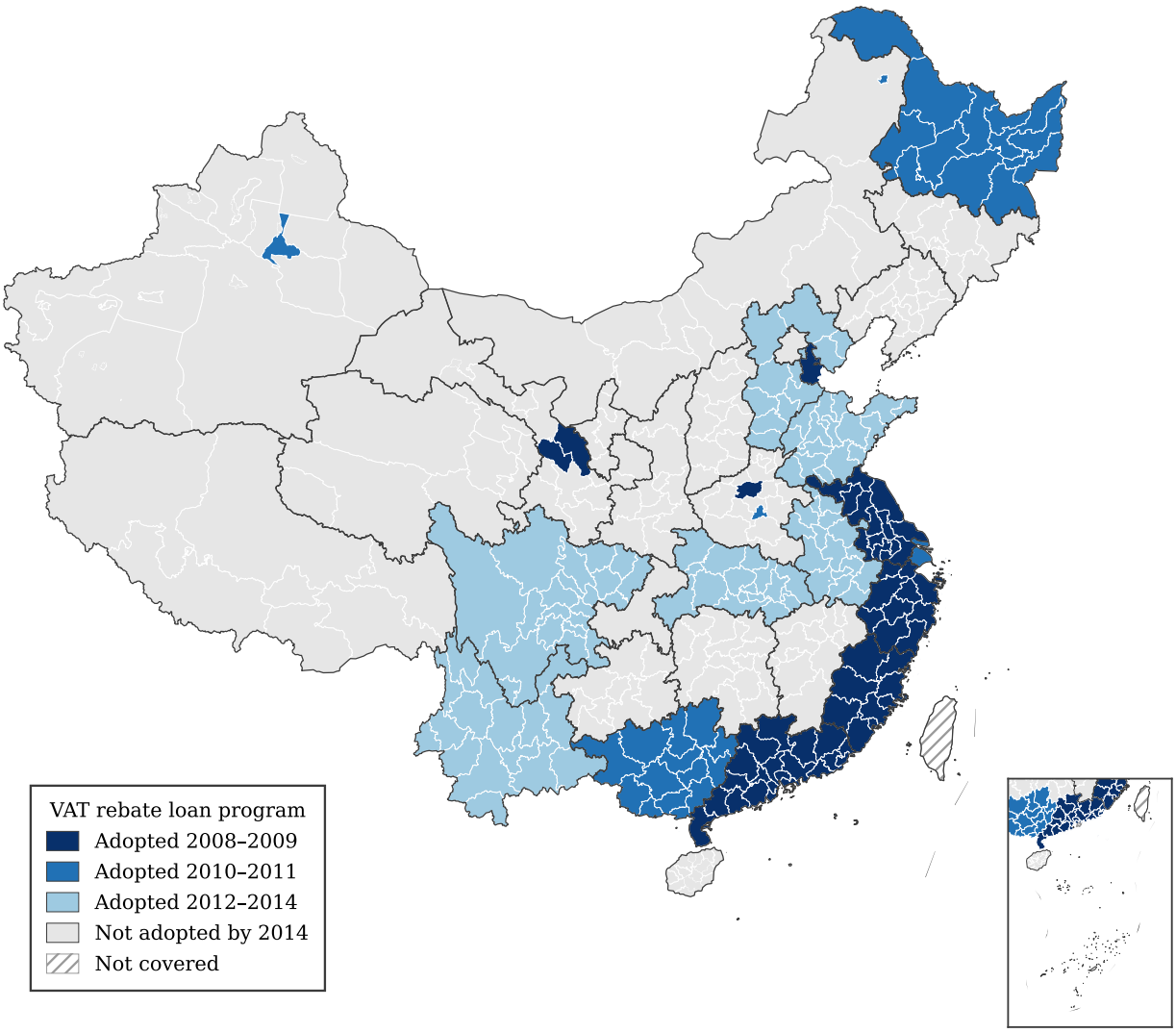
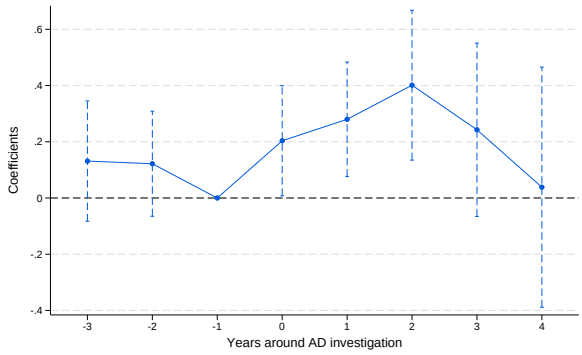
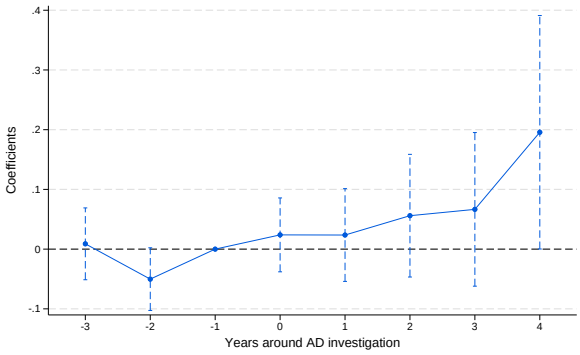


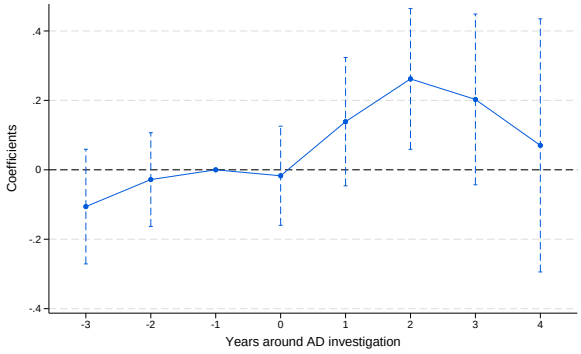
Figure A6: **Adoption of the VAT Rebate Loan Program across Chinese Cities.** This figure maps the staggered adoption of the VAT rebate loan program across Chinese prefecture-level cities. Cities are shaded by the year in which the program became available in the city, in three waves: 2008–2009, 2010–2011, and 2012–2014, with darker shading indicating earlier adoption. Cities in light gray had not adopted the program by 2014. Adoption years are those of Table A2; where a province adopted the program for all of its cities, every city in the province carries the provincial adoption year. City boundaries are from the DataV GeoAtlas of China.



Panel A: log(Advertising)



Panel B: log(Entertainment)



Panel C: log(R&D)

Figure A7: **Firm-Level Responses to Anti-Dumping: Advertising, Entertainment, and R&D** This figure plots the dynamic effects of anti-dumping (AD) investigations on the advertising, entertainment, and research and development spending of AD-hit firms. The coefficients of interest are the interaction between *Treat* and event time indicators from three years before to four years after the investigation. Panels A to C report the logarithm of Advertising, Entertainment, and R&D. Variables are defined in Table 5. All regressions control for firm and year fixed effects, each interacted with cohort indicators. The blue dots represent the point estimates and the vertical lines represent 90 percent confidence intervals based on standard errors clustered by firm.

Table A1: **City-Level Predictors of Loan Program Adoption**
This table examines whether observable city-level characteristics predict adoption of the VAT rebate loan program. The dependent variable is an indicator for whether a city adopts the program in a given year, categorized by stated motive: any adoption (column 1), long-term economic development (column 2), export slowdown (column 3), and firms' credit constraint (column 4). Once a city adopts, it exits the sample. Standard errors clustered at the province level are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Dep. Var.:	*1(Policy)*	*1(Long-term Econ. Dev.)*	*1(Export Slowdown)*	*1(Credit Constraint)*
	(1)	(2)	(3)	(4)
Log(Exports)	0.000	−0.001	−0.001	0.002
	(0.006)	(0.005)	(0.001)	(0.002)
Log(GDP)	0.018	0.034	−0.009	−0.028
	(0.035)	(0.025)	(0.008)	(0.021)
Fiscal Expenditure/Revenue	−0.006	0.003	−0.002	−0.001
	(0.009)	(0.007)	(0.002)	(0.003)
Log(Fiscal Revenue)	−0.017	0.010	−0.006	0.004
	(0.050)	(0.045)	(0.004)	(0.012)
Log(Fiscal Expenditure)	−0.046	−0.054	0.004	−0.014
	(0.073)	(0.070)	(0.004)	(0.017)
Log(Real Estate Investment)	−0.004	−0.003	0.001	−0.002
	(0.024)	(0.021)	(0.004)	(0.012)
Real Estate Investment/GDP	0.102	0.274	−0.020	−0.124
	(0.251)	(0.228)	(0.044)	(0.082)
Log(Outstanding Bank Loans)	0.029	−0.001	0.010	0.034*
	(0.026)	(0.017)	(0.007)	(0.017)
Year FE	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
Observations	1,319	1,319	1,319	1,319
Adj. R^2	0.308	0.359	0.476	0.386

Table A2: **Loan Program Adoption by City**

This table reports the timing and stated motives of VAT rebate loan program adoption across Chinese prefecture-level cities. A city is dated by the earliest official document that made the program available in the city; where a province adopted the program for all of its cities, every city in the province carries the provincial adoption year. We use a narrative approach following Romer and Romer (2010) to classify the stated motive into three categories: *Long-Term* (long-term development), *Export Slowdown* (response to export decline or external shocks), and *Credit Constraint* (alleviating exporter financing constraints). The Sichuan documents do not state a motive. Cities in provinces not listed had not adopted the program by 2014.

City	Year	*Long-Term*	*Export Slowdown*	*Credit Constraint*
Tianjin	2008	✓		
Fujian, all cities	2008	✓		
Jiangsu, all cities	2008	✓	✓	✓
Jiaxing, Zhejiang	2008	✓		✓
Zhejiang, other cities	2008	✓		
Baiyin, Gansu	2008	✓		
Guangdong, all cities	2009			✓
Zhengzhou, Henan	2009	✓		✓
Lanzhou, Gansu	2009		✓	✓
Shanghai	2010	✓		
Guangxi, all cities	2010	✓		
Heilongjiang, all cities	2011	✓		
Luohe, Henan	2011	✓		✓
Urumqi, Xinjiang	2011			✓
Hebei, all cities	2012	✓		
Hubei, all cities	2012	✓		
Sichuan, all cities	2012			
Qingdao, Shandong	2013	✓	✓	
Shandong, other cities	2013		✓	
Anhui, all cities	2014	✓		
Yunnan, all cities	2014			✓

Table A3: **Trade Diversion by Access to Government Credit**

This table examines whether access to the VAT-rebate loan program changes the trade diversion of AD-hit firms. The sample is the firm–product–destination–year panel of Table 3, restricted to firms in cities that adopted the program before the investigation or never adopted it. Panel A examines the AD product sold to non-AD destinations and Panel B the firm's non-AD products sold to the AD destination. *Treat* is an indicator that equals one if the firm faced an AD investigation. *Post* is an indicator that equals one from the investigation year onward. *Loan* is an indicator that equals one if the firm's city adopted the rebate-loan program before the investigation and zero if it never adopted the program. *Exit* is an indicator that turns to one if a firm exported a product to a destination in the previous year but not in the current year. *Quantity (Value)* refers to the export quantity (value in USD) of the product shipped by the firm to a destination in a year. *Price* is the average unit value in USD. All fixed effects are interacted with cohort indicators, where a cohort is the investigation year. Standard errors are reported in parentheses and are double clustered by firm and destination country. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: AD Product to Other Destinations

Dep. Var.:	Extensive	Intensive Margin		
	Exit	*log(Quantity)*	*log(Value)*	*log(Price)*
	(1)	(2)	(3)	(4)
Treat × *Post*	−0.174***	0.230**	0.224***	−0.025
	(0.038)	(0.096)	(0.081)	(0.050)
Treat × *Post* × *Loan*	−0.035	−0.076	−0.061	0.033
	(0.045)	(0.120)	(0.104)	(0.054)
Firm-Product-Destination FE	Yes	Yes	Yes	Yes
Destination-Year FE	Yes	Yes	Yes	Yes
City-Year FE	Yes	Yes	Yes	Yes
Observations	58,656	58,375	58,375	58,375
Adj. R^2	0.140	0.789	0.701	0.866

Panel B: Non-AD Products to the AD Destination

Dep. Var.:	Extensive	Intensive Margin		
	Exit	*log(Quantity)*	*log(Value)*	*log(Price)*
	(1)	(2)	(3)	(4)
Treat × *Post*	−0.185***	0.013	0.024	−0.011
	(0.035)	(0.069)	(0.066)	(0.035)
Treat × *Post* × *Loan*	0.033	−0.005	0.027	0.057
	(0.034)	(0.082)	(0.098)	(0.047)
Firm-Product-Destination FE	Yes	Yes	Yes	Yes
Destination-Year FE	Yes	Yes	Yes	Yes
City-Year FE	Yes	Yes	Yes	Yes
Observations	41,953	41,269	41,269	41,269
Adj. R^2	0.119	0.794	0.722	0.864

Table A4: **Firm-Level Responses to Anti-Dumping Investigation by Access to Government Credit**

This table examines whether access to the VAT-rebate loan program changes the effects of anti-dumping (AD) investigations on the export performance, profitability, and costs of AD-hit firms. The sample is the balanced firm–year panel of Table 5, restricted to firms in cities that adopted the program before the investigation or never adopted it. *Treat* is an indicator that equals one if the firm faced an AD investigation. *Post* is an indicator that equals one from the investigation year onward. *Loan* is an indicator that equals one if the firm's city adopted the rebate-loan program before the investigation and zero if it never adopted the program. *Export Revenue* is the firm's total export value in USD; column (1) of Panel A estimates it in levels by Poisson pseudo-maximum likelihood. All fixed effects are interacted with cohort indicators, where a cohort is the investigation year. Standard errors are reported in parentheses and are clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: Export Revenue and Profitability

Dep. Var.:	*Export Revenue*	*log(Exports)*	*log(Value Added)*	*ROA*
	(1)	(2)	(3)	(4)
Treat × *Post*	0.309***	0.360***	0.272**	−0.001
	(0.068)	(0.056)	(0.121)	(0.005)
Treat × *Post* × *Loan*	0.004	−0.030	−0.195	0.002
	(0.076)	(0.065)	(0.132)	(0.006)
Firm FE	Yes	Yes	Yes	Yes
City-Year FE	Yes	Yes	Yes	Yes
Observations	89,063	52,735	19,938	30,860
Adj. R^2	0.772	0.673	0.649	0.515

Panel B: Operating and Selling Costs

Dep. Var.: log(Y)	*COGS*	*Employees*	*Selling*	*Admin.*	*Advertising*	*Entertain.*	*R&D*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treat × *Post*	0.104**	0.028	0.114**	0.049	0.233	0.162**	−0.233
	(0.045)	(0.034)	(0.053)	(0.035)	(0.280)	(0.079)	(0.314)
Treat × *Post* × *Loan*	−0.059	0.059	0.030	0.012	0.101	−0.134	0.380
	(0.050)	(0.039)	(0.060)	(0.040)	(0.306)	(0.091)	(0.334)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	30,845	31,007	27,506	30,640	5,103	20,410	2,390
Adj. R^2	0.912	0.916	0.885	0.926	0.592	0.730	0.754

Internet Appendix

Yichen Wang

This document provides additional data descriptions and robustness tests.

IA.3. Sample Construction

This section details the construction of the estimation samples described in Section 3 of the paper.

IA.3.1. Customs Panel

I build the customs panel in the following steps:

1. I start with the universe of export transactions recorded by China's General Administration of Customs for 2007 to 2015 and keep exports only.
2. I link the customs registration code of each firm to its tax survey identifier through a crosswalk built from the matched records of the two agencies, which cover 685,312 tax survey firms over 2007 to 2015, and, for firms the crosswalk misses, by matching on firm names. Table IA.1 reports the coverage of the link by year.
3. I drop transactions with missing product codes or destination countries and transactions with zero or missing export value.
4. I keep ordinary trade and drop processing trade, using the trade-type field of the customs record.
5. I drop firms that report a product to a destination in more than one quantity unit, since their quantities cannot be added up.
6. I aggregate the remaining transactions to the firm–HS6 product–destination–year level, summing value and quantity, and define the unit price as value divided by quantity. Member states of the European Union are pooled as one destination.
7. I keep firm–years with a tax survey return in the same year, which provides the firm's city and financial statements. The resulting panel contains between 35,000 and 86,000 exporting firms per year, with the fewest in 2007 and 2015, when fewer of the linked firms have a tax survey return (Table IA.1, column 5).

Table IA.1: **Coverage of the Customs–Tax Survey Link by Year**
This table reports the coverage of the link between the customs transaction data and the tax survey for each year of the sample period. *Exporting Firms* is the number of firms with export transactions in the customs data. *Linked Firms* is the number of exporting firms that can be matched to a tax survey identifier. *Share of Firms* and *Share of Export Value* are the shares of exporting firms and of total export value that the linked firms account for. *Firms in the Panel* is the number of firms in the final estimation panel, namely linked firms with ordinary exports and a tax survey return in the same year.

Year	*Exporting Firms*	*Linked Firms*	*% of Firms*	*% of Export Value*	*Firms (Panel)*
	(1)	(2)	(3)	(4)	(5)
2007	193,567	105,860	55%	77%	34,970
2008	206,452	118,116	57%	78%	66,336
2009	216,221	125,685	58%	79%	72,527
2010	234,368	132,404	56%	79%	85,060
2011	254,617	135,607	53%	76%	86,286
2012	269,576	133,019	49%	73%	86,271
2013	281,866	129,657	46%	69%	77,539
2014	297,633	123,203	41%	68%	50,395
2015	312,601	114,270	37%	61%	34,501

IA.3.2. Direct-Effect Sample

The direct-effect sample (Tables 2 and 7) is built from the customs panel as follows:

1. I keep firm–products that faced exactly one AD case during the sample period, so that each firm–product has a single event date.
2. I keep firm–products that the firm exported to the initiating country in at least one year of the sample period, so that the firm was exposed to the case.
3. I keep active flows, namely firm–product–destination pairs with positive exports in at least 40% of the window years observed in the data, so that the sample consists of established trade relationships rather than one-off shipments.
4. I drop control destinations that faced their own AD case on the product, so that no control flow is under a case of its own.
5. Event time is measured from the initiation year of the case, and the panel runs from three years before to four years after it. Flow–years without a customs record within the window are coded as zero exports. With trade data from 2007 to 2015, the window is complete for the 2010 and 2011 cohorts. The 2009 cohort is observed from two years before the investigation, and the 2012 and 2013 cohorts until three and two years after it. Cases initiated between 2006 and 2013 are the ones that can produce a hit firm in these cohorts, since a firm is dated as hit no earlier than the initiation of its case. The period 2006 to 2013 therefore refers to the cases, and the trade and firm data run from 2007 to 2015.
6. *Treat* equals one for the flow to the initiating country and zero for the same firm–product's flows to other destinations. *Post* equals one from the initiation year onward.
7. In the analysis by loan access (Table 7), *Loan* equals one if the firm's city adopted the VAT rebate loan program before the initiation year and zero if it never adopted it. Firms in cities that adopted in or after the initiation year are excluded.

IA.3.3. Trade Diversion and Spillover Samples

The trade diversion and spillover samples (Tables 3, 4, 8, and A3) are built as follows:

1. A firm is hit if it exported the AD product to the initiating country in the initiation year of the case or later, for exactly one product–country pair. Firms hit on more than one pair are excluded. A firm that exported the AD product to the initiating country only before the initiation is not hit.
2. The investigation year of a hit firm is the first year at or after the initiation of the case in which it exports the AD product to the initiating country. Firms with the same investigation year form a cohort. I keep cohorts from 2009 to 2013.
3. Never-hit firms are firms that never exported any AD product to its initiating country in the initiation year of the case or later. They form the control pool.
4. I exclude the investigated flow itself and classify the hit firm's remaining flows into three groups, namely the AD product sold to other destinations, other products sold to the initiating country, and other products sold to other destinations. A flow enters if it had positive exports in at least one of the three years before the investigation.
5. Each treated flow is matched to one flow of a never-hit firm in the same product, destination, and cohort, with replacement, choosing the never-hit flow whose export quantity growth over the three years before the treated flow's investigation is closest. A control flow that is the closest match for several treated flows enters once for each. Control flows are assigned the investigation year of the treated flow they are matched to.
6. The panel is balanced from three years before to four years after the investigation year, with the same cutoffs at the start and end of the data as in Section IA.3.2. *Exit* is defined on flow–years in which the flow was active in the previous year. Quantity, value, and price are defined on flow–years with positive exports.
7. In the analyses by loan access, *Loan* equals one if the firm's city adopted the program before the investigation year. Firms in cities that adopted in the investigation year or in the following three years are excluded, and firms in cities that adopted later are coded as zero.

IA.3.4. Firm-Level Sample

The firm-level sample (Tables 5, 6, and A4) is built as follows:

1. Hit firms, investigation years, cohorts, and never-hit firms are defined as in Section IA.3.3.
2. Each hit firm is matched to one never-hit firm in the same cohort and industry, with replacement, where industry is the two-digit HS chapter of the firm's main export product over the three years before the investigation.
3. For the export outcomes, the match is on the logarithm of exports in the year before the investigation and its growth from three to two and from two to one years before, choosing the never-hit firm whose values are closest. For the accounting outcomes, the match is on the logarithm of revenue, its growth over the same years, and the logarithm of assets. A firm is matched only if all of its matching variables are observed.
4. The panel is balanced from three years before to four years after the investigation year. Export revenue is the firm's total export value from the customs data, with firm–years without a customs record coded as zero. Accounting outcomes come from the tax survey return of the same year.
5. *Loan* is defined as in Section IA.3.3.

IA.3.5. Loan Program Adoption Records

The adoption records are assigned to cities and firms as follows:

1. The adoption records cover the 335 prefecture-level units of China. For each unit, the record gives the year in which the VAT rebate loan program became available, from 2008 to 2014, or no year if the unit had not adopted the program by 2014. A provincial decision is assigned to every prefecture-level unit in the province.
2. Firms are assigned to units by the registered city in the tax survey. A small number of county-level units administered directly by a province, and two prefecture-level units, have no row in the records. Firms in these units, 0.5% of the firm panel, are coded as not having the program.
3. The stated motive of each adoption is classified from the adopting document into long-term development, an export slowdown, and firms' credit constraints, following the narrative approach of Romer and Romer (2010). A document can state more than one motive, and the documents of one province, Sichuan, state none. Table A2 reports the classification.
4. In the direct-effect sample, *Loan* equals one if the firm's unit adopted the program before the initiation year of the case and zero if it had not adopted by 2014; firms in units that adopted in or after the initiation year are excluded. In the stacked samples, *Loan* equals one if the unit adopted before the investigation year and zero if it adopted more than three years after the investigation year or not at all; firms in units that adopted in the investigation year or in the following three years are excluded.

IA.4. Covariate Balance in the Matched Samples

Table IA.2 compares hit units with their matched never-hit controls on pre-investigation characteristics, one row per matched flow or firm. Panels A to C are the trade diversion and spillover samples of Section IA.3.3, and Panels D and E are the firm-level samples of Section IA.3.4. For each sample, the table reports the variables used in the match and other pre-investigation characteristics that are not. On every matching variable, the standardized difference between hit units and their controls is below 0.06. The characteristics that a design does not match on are not balanced, as expected. Matching on exports leaves hit firms larger in revenue, assets, and employment than their controls, and matching on revenue and assets leaves hit firms with faster export growth in the year before the investigation.

Table IA.2: **Covariate Balance in the Matched Samples**
This table compares AD-hit units with their matched never-hit controls before the investigation. Panels A to C are the flow-level samples, in which each flow of a hit firm is matched to a never-hit flow of the same product, destination, and cohort on its pre-investigation quantity growth. Panels D and E are the firm-level samples, in which each hit firm is matched to a never-hit firm in the same industry and cohort on the variables listed under *Matching variables*. Growth rates are log differences between the indicated event years, where t is the investigation year. *Difference* is the treated mean minus the control mean. *Std. Diff.* is the difference divided by the pooled standard deviation. *t-stat.* is from a regression of the variable on the treatment indicator with standard errors clustered by firm.

Variable	*Treated*	*Control*	*Difference*	*Std. Diff.*	*t-stat.*
	(1)	(2)	(3)	(4)	(5)
Panel A: AD Product to Other Destinations 26,534 matched pairs					
Matching variables					
Quantity growth, $t-3$ to $t-2$	1.329	1.046	0.282	0.046	2.92
Quantity growth, $t-2$ to $t-1$	1.733	1.335	0.398	0.054	3.43
Other characteristics					
Log(1 + Quantity) at $t-1$	5.461	4.500	0.961	0.200	12.31
Log(1 + Value) at $t-1$	6.569	5.522	1.048	0.195	12.86
Have Export at $t-1$	0.619	0.548	0.071	0.145	9.81
Panel B: Non-AD Products to the AD Destination 19,038 matched pairs					
Matching variables					
Quantity growth, $t-3$ to $t-2$	1.197	1.089	0.109	0.020	1.00
Quantity growth, $t-2$ to $t-1$	1.499	1.422	0.077	0.012	0.50
Other characteristics					
Log(1 + Quantity) at $t-1$	4.585	4.364	0.221	0.049	2.33
Log(1 + Value) at $t-1$	6.098	5.777	0.321	0.061	2.75
Have Export at $t-1$	0.615	0.584	0.031	0.063	2.71
Panel C: Non-AD Products to Other Destinations 177,675 matched pairs					
Matching variables					
Quantity growth, $t-3$ to $t-2$	0.937	0.844	0.093	0.017	1.38
Quantity growth, $t-2$ to $t-1$	0.989	0.922	0.067	0.010	0.87
Other characteristics					
Log(1 + Quantity) at $t-1$	3.946	3.726	0.220	0.051	4.23
Log(1 + Value) at $t-1$	5.256	5.000	0.256	0.050	4.27
Have Export at $t-1$	0.544	0.522	0.022	0.044	3.69

Variable	*Treated*	*Control*	*Difference*	*Std. Diff.*	*t-stat.*
	(1)	(2)	(3)	(4)	(5)
Panel D: Firm-Level Sample, Matched on Exports 8,289 matched pairs					
Matching variables					
Log(1 + Exports) at $t-1$	12.147	12.156	−0.009	−0.002	−0.11
Export growth, $t-2$ to $t-1$	3.139	3.118	0.021	0.003	0.17
Export growth, $t-3$ to $t-2$	2.851	2.854	−0.003	0.000	−0.03
Other characteristics					
Log(Revenue) at $t-1$	10.678	10.390	0.287	0.158	9.17
Log(Assets) at $t-1$	10.453	10.190	0.263	0.134	7.75
Log(Employment) at $t-1$	4.483	4.288	0.195	0.119	6.84
ROA at $t-1$	0.019	0.011	0.008	0.066	3.86
Panel E: Firm-Level Sample, Matched on Revenue and Assets 3,532 matched pairs					
Matching variables					
Log(Revenue) at $t-1$	11.136	11.148	−0.013	−0.007	−0.28
Revenue growth, $t-2$ to $t-1$	0.083	0.067	0.016	0.032	1.32
Revenue growth, $t-3$ to $t-2$	0.165	0.157	0.007	0.012	0.49
Log(Assets) at $t-1$	11.029	11.028	0.001	0.001	0.03
Other characteristics					
Log(1 + Exports) at $t-1$	13.267	13.118	0.148	0.041	1.65
Export growth, $t-2$ to $t-1$	0.661	0.000	0.661	0.168	6.85
Export growth, $t-3$ to $t-2$	0.803	0.509	0.294	0.071	2.90
Log(Employment) at $t-1$	4.912	4.959	−0.047	−0.030	−1.19
ROA at $t-1$	0.031	0.027	0.003	0.032	1.29