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BROKEN RELATIONSHIPS: DE-RISKING BY CORRESPONDENT BANKS AND INTERNATIONAL TRADE

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JEL Classification: F14, F15, F36, G21, G28, L14

Keywords: N/A

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Broken Relationships: De-Risking by Correspondent Banks and International Trade*

Lea Borchert†, Ralph De Haas‡, Karolin Kirschenmann§, and Alison Schultz¶

July 31, 2024

Abstract

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Keywords: Correspondent banking; trade finance; de-risking, global banks; international trade; anti-money laundering

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1 Introduction

Global banking has changed substantially in the wake of the Great Recession, as new regulation, stricter supervision, and strengthened risk management prompted international banks to scale back foreign activities (Claessens 2017; De Haas and Van Horen 2013, 2017; Cerutti and Zhou 2018). A prime example of this retrenchment has been the sharp decline in correspondent banking. Driven by stricter anti-money laundering (AML) and counter-terrorism financing rules, many global banks have curtailed their correspondent banking services, especially in low-income countries (Rice, Peter, and Boar 2020).

The sharp contraction in correspondent banking over the past decade has sparked concerns among policymakers about its potential impact on international trade and economic growth in affected countries (CGD, 2015; World Bank, 2015; BIS, 2016; FSB, 2017; IMF, 2017). These concerns reflect the critical role correspondent banks play by providing the payment infrastructure that underpins much cross-border trade. Correspondent banks hold deposits from other lenders (respondents) and provide them with payment services, enabling exporters' and importers' banks to conduct trade-related transactions without direct bilateral account relationships. Moreover, correspondent banks offer trade finance solutions, such as letters of credit, which mitigate non-payment or non-shipment risks when enforcement is costly. By providing these services, correspondent banks reduce contractual frictions and enable trades that might otherwise not materialize due to financial constraints or a lack of trust between trading parties.

This paper examines the firm-level consequences of correspondent bank retrenchment, providing the first analysis of how this shock affects firms' exports, domestic revenues, employment, and survival probabilities. We focus on four emerging European countries—Bosnia & Herzegovina, Croatia, Hungary, and Turkey—which have traditionally relied heavily on correspondent banking services. This setting therefore provides a relevant and representative context to examine the effects of correspondent bank retrenchment on firm-level outcomes in emerging economies, where access to correspondent services is often a precondition for

engaging in international commerce.

To identify the impact of the withdrawal of correspondent banks on firm activity, we join three key pieces of information: time-varying data on individual respondent banks' lost correspondent relationships, information on the identity of the corporate customers of these banks, and data on exports and other relevant outcomes of these firms. Information on the loss of correspondent bank relationships comes from two proprietary surveys among respondent banks in our sample countries: the third wave of the Banking Environment and Performance Survey (BEPS III) and an online survey we conducted together with EBRD's Trade Facilitation Programme. We link these bank-level data to information about firms' main ('house') bank as reported by Bureau van Dijk's Orbis database. We then match this information with additional firm-level data from Orbis.

For estimation, we employ the imputation estimator of Borusyak, Jaravel, and Spiess (2024), which allows for heterogeneous treatment effects across different firms and dynamic effects around events.[1] Using this approach, we systematically compare the export performance of firms with a main bank that loses a correspondent banking relationship (treated firms) to similar firms with a main bank that does not lose any correspondent relationship up to the event year (control firms). We match treated with observationally similar control firms in terms of their pre-event export turnover, total assets, and total factor productivity, and keep all firms with common support.

Our results show that a decline in the supply of correspondent banking services negatively affects both the extensive and the intensive export margins. Exporting firms start to export less, or stop exporting altogether, when their main bank loses a correspondent banking relationship. At the extensive margin, the likelihood that a firm continues to export declines by 5.2 percentage points in the short term and by 19.8 percentage points in the medium term (four years after the shock). When correspondent banking relationships are terminated, some affected firms can compensate for the resulting export decline by boosting domestic sales.

1. Online Appendix E discusses the role of heterogeneous treatment effects in our setting.

However, many other firms experience a decrease in total revenues, have to lay off employees, or even go out of business entirely. These negative outcomes are especially pronounced among smaller and younger firms and indicate that, typically, firms cannot simply switch banks when their own bank can no longer provide correspondent services.

We proceed by assessing the relevance of these disruptions at the level of affected villages, towns, and cities ('localities'). Here we are interested in the local equilibrium effects of terminated correspondent relationships on the average exporting firm in a locality, regardless of whether a firm is a client of an affected bank or not. To do so, we first link bank-level data on correspondent bank withdrawals with comprehensive information about the geographical location of all bank branches, and then match this information with data on firm locations from Orbis. These combined data allow us to paint a detailed picture of the bank branches surrounding each firm and to identify, at the local level, the impact of the withdrawal of correspondent banks on business activity.

We find that the negative impacts of terminated correspondent relationships on firm-level trade reverberate through the local economy. When comparing localities where many bank branches lost a correspondent bank, to localities without such disruptions, local exporters on average exhibit significant export declines on both the extensive and intensive margins. Importantly, throughout our analysis, we include time-varying bank-level controls that capture general developments in local credit conditions, such as the size and capitalization of banks and their loan growth. This allows us to estimate the separate effect of terminated correspondent bank relationships over and above the role of general credit conditions.

Next, we present a within-industry spillover analysis (Berg, Reisinger, and Streitz 2021) and show that treated firms are *less* negatively affected in their probability to export, the greater the proportion of other treated firms in the industry. This likely reflects that, with more treated firms in an industry, trading partners have fewer possibilities to buy products from other suppliers elsewhere in the country. Moreover, control firms (exporters in localities without a decline in correspondent relationships) suffer from weak spillovers. Their export

probability declines slightly if the proportion of treated firms in the same industry is higher. This suggests that within an industry, suppliers from different regions complement each other rather than act as substitutes.

Our identification strategy relies on the parallel trends assumption: exporting firms in treatment and control groups would have evolved similarly absent the shock to the global correspondent banking network. We do not require random termination of correspondent relationships or identical pre-treatment characteristics across firms. We provide two pieces of evidence supporting the parallel trends assumption. First, before the decline in correspondent banking, there were no systematically different pre-trends, neither in the export performance of treated versus control firms (in the firm-level regressions) nor in treated versus control localities (in the locality-level regressions). This supports the idea that both groups would have developed similarly in the absence of the global shock to correspondent banking.

Second, while our design does not depend on treated and control firms being similar in levels, such similarity would add further credibility to the common-trends assumption. We therefore offer evidence that correspondent banks' withdrawal is orthogonal to a battery of firm, bank and locality characteristics. Throughout our analysis, we nevertheless control for such characteristics while accounting for time-varying country and industry trends through country-by-year and industry-by-year fixed effects. These fixed effects absorb unobserved heterogeneity that could bias our estimates.

We conclude the paper by extending our analysis to a broader sample of 17 emerging European markets. We use bilateral sectoral trade data from UN Comtrade and exploit the tightening of the U.S. regulator's enforcement of financial crime legislation in June 2014 as a negative shock to the supply of correspondent services (BIS, 2016). While these industry estimates are less cleanly identified than our firm-level ones, they support the external validity of the latter. A further advantage of the industry-level approach is the availability of data on imports. The sector-level results confirm our firm-level evidence: export and import growth decline significantly more in countries with a higher withdrawal of correspondent banks.

Our study contributes to two main strands of the literature. First, we provide new insights into the channels through which globally active banks mediate the impact of financial frictions on international trade (Kohn, Leibovici, and Szkup 2022). Prior work has shown that the physical presence of foreign banks supports trade between a host country and these banks' home countries (Portes and Rey 2005; Bronzini and D'Ignazio 2017; Claessens and Van Horen 2021; Brancati 2022). Caballero, Candelaria, and Hale (2018) show that syndicated loan connections between countries—that is, without foreign banks necessarily having a local presence on the ground—also boost bilateral exports. Lastly, Xu (2022) explores the long-term impact of the 1866 London banking crisis on global trade. Countries more exposed to British bank failures faced immediate export declines and persistent market share losses in export destinations for decades. This suggests temporary financial shocks can durably alter the geography of trade, likely due to the sunk costs of establishing trade relationships and the substitutability of goods across exporters.

Other papers focus on specific trade finance products. Niepmann and Schmidt-Eisenlohr (2017a, 2017b) and Ahn and Sarmiento (2019) analyze how bank-level financial shocks reduce the supply of letters of credit, negatively affecting firm exports.[2] Likewise, Demir and Javorcik (2020) and Crozet, Demir, and Javorcik (2022) show how a decline in letters of credit negatively affected international trade during the Covid-19 pandemic. Other work assesses the role of export credit insurance (Auboin and Engemann 2014; van der Veer 2015) and export guarantees (Felbermayr and Yalcin 2013; Heiland and Yalcin 2021; Custodio, Hansman, and Mendes 2024). Recently, Kabir et al. (2024) have used the temporary shutdown of the U.S. Export-Import Bank to document how losing access to government-backed export credit can reduce firms' global sales, exports, capital investment and employment.

While trade finance has been extensively researched, the specific role of correspondent banks in global trade remains unexplored. Our paper helps to address this gap in three

2. More generally, the role of banks in providing debt funding that facilitates trade has been well documented (Amiti and Weinstein 2011; Chor and Manova 2012; Michalski and Ors 2012; Manova 2013; Del Prete and Federico 2014; Paravisini et al. 2015; Demir, Michalski, and Ors 2017; Dogan and Hjortsø 2024).

ways. First, we quantify the firm-level impacts of disrupted correspondent banking relationships, focusing on their critical role in facilitating cross-border trade transactions. Second, we exploit a unique episode for identification: the retrenchment of correspondent banks in response to tightened financial crime regulations. Third, we innovate by hand-collecting detailed bank-level data on terminated correspondent relationships and on respondent banks' branch locations, enabling us to analyze both direct impacts and local spillover effects.

In doing so, we also contribute to the burgeoning literature on cross-border shock transmission through global banks. Previous studies have examined the transmission of financial crises (Peek and Rosengren 1997, 2000; Chava and Purnanandam 2011; Cetorelli and Goldberg 2011, 2012; Popov and Udell 2012; Schnabl 2012; De Haas and Van Horen 2012, 2013; Paravisini et al. 2015), shocks to risky assets (Popov and Van Horen 2015; Altavilla, Pagano, and Simonelli 2017; Acharya et al. 2018; De Marco 2019), tax reforms (Célérier, Kick, and Ongena 2020), micro- and macroprudential regulation (Aiyar et al. 2014; Tripathy 2020), monetary policy shocks (Bruno and Shin 2015) and, more recently, heightened trade uncertainty (Correa et al. 2023) and trade disruptions (Alfaro et al. 2024). We instead focus on the cross-border transmission of a sudden shock to regulatory compliance costs, which inadvertently disrupted the global correspondent banking network.

The remainder of this paper is organized as follows. Section 2 describes the institutional background, after which Section 3 introduces our data. Section 4 then sets out the empirical strategy, while Section 5 presents our results. Section 6 concludes.

2 Correspondent banking and international trade

This section discusses the role of correspondent banking in international trade (Section 2.1); the recent sharp decline in correspondent bank relationships (Section 2.2); and initial descriptive evidence on the impact of this decline on respondent banks (Section 2.3).

2.1 Correspondent banking: A primer

Correspondent banks form important nodes in the global financial system. These institutions, predominantly large multinational banks, serve as intermediaries by maintaining accounts for other banks, known as respondents. This arrangement facilitates international trade by enabling efficient cross-border payments between the local banks of exporters and importers. As correspondent banks handle the majority of payments underlying global trade, they form a cornerstone of the international payment system (Rice, Peter, and Boar 2020).

Beyond processing payments, correspondent banks bridge regulatory frameworks, currencies, and time zones. They do so by offering specialized services such as currency exchange, the issuance of letters of credit, and cash management solutions. Most international trade transactions take place on an open account basis, and prepayment is rare (Asmundson et al. 2011). The role of correspondent banks is therefore to help overcome the commitment problems and limited enforceability that can inhibit direct payment between trading partners. By maintaining ongoing relationships, these banks serve as credible intermediaries between local banks, so that payment and shipment take place as specified in the contract between the ultimate importer and exporter. This intermediary role is especially important when the risk of non-payment or non-shipment is high and enforcement is expensive (Schmidt-Eisenlohr 2013; Antras and Foley 2015)—as is often the case in developing economies.

Correspondent banks require significant expertise and resources to fulfill their intermediary role in international trade. They must comply with complex international regulations, including those related to anti-money laundering and anti-terrorism financing. This requires familiarity with foreign markets, diverse legal environments, and cross-border contractual enforcement. Due to the high fixed costs involved in these activities, the correspondent banking market is highly concentrated. For example, the five largest U.S. banks account for 92 percent of U.S. trade finance (Niepmann and Schmidt-Eisenlohr 2017b) while in Italy just ten banks provide virtually all trade finance (Del Prete and Federico 2014). This concentration makes the market susceptible to sudden shocks that can interrupt trade flows.

2.2 Financial crime and correspondent banking

Correspondent banks are vulnerable to financial crime. Criminals often use cross-border payments to disguise illicit funds by exploiting national differences in legislation, bank secrecy laws, and enforcement. Funds can, for example, be transferred between accounts in different countries and currencies, and (re-)exchanged for high-value items like real estate. Correspondent banks may also be implicated in criminal activities through their provision of trade finance, as trade transactions are a common method to validate illicit cross-border payments, such as through over- or multiple invoicing (FATF 2006).

Since the 1970s, governments have been developing and harmonizing legal frameworks to counteract financial crime in international payment systems. The Financial Action Task Force (FATF), the global watchdog on money laundering and terrorist financing, requires correspondent banks to reveal all parties involved in a cross-border transaction and perform due diligence on their customers. However, weak enforcement of these legal frameworks has undermined the fight against financial crime in practice (CGD 2015). Prosecution of offences only tightened after the global financial crisis, when increased regulatory scrutiny unearthed extensive evidence of financial crimes in the banking sector (Tomasic 2011).

The stricter enforcement of financial crime legislation is evident in surging fines. A prominent example is the record US$8.9 billion fine issued to French correspondent bank BNP Paribas in June 2014 for violating sanctions against Sudan, Cuba, and Iran. The extent of the penalty was unexpected, as BNP Paribas had set aside 'only' US$1.1 billion in provisions for litigation costs, and it greatly exceeded past fines, such as the US$1.9 billion fine issued to HSBC in December 2012 for money laundering. Crucially, in 2014, the U.S. Department of Justice made clear that any global transaction threatening the integrity of the U.S. financial system could be tried in a U.S. court. While high fines appear to have effectively prevented sanctions violations since the BNP Paribas trial, fines for violations of anti-money laundering regulations continue to rise. A recent example includes the three fines, totalling US$7.2 billion, that Goldman Sachs received in 2020 (Financial Crime News 2022).

2.3 The effects of de-risking by correspondent banks

The massive and unexpected 2014 fine for BNP Paribas accelerated the decline in global correspondent banking. Widely regarded as a harbinger of stricter regulatory enforcement in anti-money laundering and counter terrorist financing (AML/CTF), the fine led to a re-assessment of the cost of regulatory compliance in correspondent banking. First, the expected costs of non-compliance increased sharply due to the large penalties and the strict stance of the U.S. Department of Justice. Second, the due diligence costs to comply with (U.S.) financial crime legislation also increased, with banks significantly increasing spending on financial crime personnel (McKinsey 2017; Banking Exchange 2020) and highlighting inconsistencies in international regulation as another important cost factor (SWIFT 2016).

Higher compliance costs led banks to reconsider their correspondent banking strategies, as the industry shifted from low-risk/low-margin to high-risk/low-margin (BIS 2016). Many banks severely pruned their networks, ending relationships deemed no longer cost-effective or too risky (Financial Stability Board 2017; Rice, Peter, and Boar 2020). Figure 1 visualizes this global decline due to de-risking, showing the Gini coefficient of active correspondent banks per corridor between 2012 and 2022, using SWIFT data from the BIS. A corridor is a single-direction jurisdiction pair (for example, Croatia to the U.S. is a corridor and the U.S. to Croatia is another). Until 2014, the coefficient is stable, after which it increases steadily, reflecting growing concentration as more banks withdrew.

To verify if respondent banks agree that the sharp increase in regulatory compliance costs drove correspondent banks to withdraw or reduce services, we surveyed local respondent banks online in late 2019, covering the period 2009–2019. Of the 131 banks invited across 28 economies in Central and Eastern Europe, the former Soviet Union, and Northern Africa, 91 completed the questionnaire, a 69 percent response rate.[3] According to these respondent banks, the main reasons for the decline in correspondent banking were that it "does not gen-

3. These economies are Albania, Armenia, Belarus, Bosnia & Herzegovina, Bulgaria, Croatia, Cyprus, Egypt, Georgia, Greece, Jordan, Kazakhstan, Kosovo, Kyrgyzstan, Lebanon, Moldova, Mongolia, Montenegro, Morocco, North Macedonia, Romania, Serbia, Ukraine, Tajikistan, Tunisia, Turkey, Uzbekistan, and West Bank and Gaza.

erate sufficient business to justify the cost of additional customer due diligence" (37 percent) and that "foreign correspondent banks have terminated relationships as a consequence of the stricter enforcement of anti-money laundering and combating the financing of terrorism (AML/CFT)" (32 percent) (Figure 2). Only 3 percent considered "less demand from their customers" an important reason. These results corroborate that increased due diligence costs and concerns about AML/CFT regulations, rather than reduced demand, caused the decline in global correspondent bank relationships. This in turn acted as a negative shock to the availability of international payment and trade finance services for local respondent banks and their clients, many of which were suddenly cut off from their long-standing providers. The broad nature of the retrenchment, combined with the industry's concentration, made finding alternative providers difficult.[4]

Our survey also provides some first descriptive evidence on the local impact of reduced correspondent banking services. Figure 3 shows the proportion of local banks that had difficulties accessing, or were unable to access, cross-border payment transactions (black bars), trade finance (dark grey), and currency clearing (light grey) in 2013, 2015, 2017, and 2019. We observe a sharp uptick in the proportion experiencing difficulties. Respondent banks that maintained access saw costs increase by an average of 35 percent between 2017 and 2019. The contraction has also changed the industry's geographical distribution. While in 2013, 73 percent of correspondent banks were based in the U.S. and Germany, their combined market share declined to 60 percent in 2019. Correspondent banks from other countries have only partially filled this gap, leading to longer and costlier intermediation chains.

3 Data

We focus on four emerging European markets: Bosnia & Herzegovina, Croatia, Hungary, and Turkey. These countries have experienced declines in correspondent banking relationships

4. Data from the BIS (CPMI Correspondent Banking Chartpack) show that in Eastern Europe, the region we focus on, the number of active correspondent banks reduced by 34 percent between 2011 and 2022. This decline was 41 percent in Bosnia-Herzegovina, 28 percent in Croatia, 31.5 percent in Hungary, and 17 percent in Turkey.

comparable to regional trends, offering a representative sample (Online Appendix A). Other emerging European countries are omitted due to data limitations in firm-level exports or insufficient variation in terminated correspondent relationships. We extend our analysis to 17 emerging European countries using bilateral sector-level trade data in Section 5.6.

We estimate the impact of correspondent banks' retrenchment on firms' exports, total revenues, employment, and dissolution probability by merging multiple firm and bank-level datasets. Our empirical strategy links time-varying data on respondent banks' terminated correspondent relationships with their customer firms' exports and other real outcomes. Online Appendix B contains all variable definitions and data sources.

3.1 Measuring the withdrawal of correspondent banks

We combine information from two new surveys of respondent banks to retrieve unique and time-varying information about lost correspondent relationships. The first survey is BEPS III, which took place between October 2020 and June 2021. The BEPS III covers large and small banks, and aimed to survey banks that jointly represent at least 95 percent of all bank assets in a country. As part of BEPS III, senior consultants—each with considerable first-hand banking experience—conducted in-depth, face-to-face interviews with bank chief executive officers (CEOs) and heads of credit of 339 banks across 34 economies. CEOs answered questions about the number of correspondent banks their bank had access to at different points in time. Online Appendix C contains the BEPS III questions we use.

BEPS III provides us with information about (changes in) correspondent banking relationships for the 20 main respondent banks in our countries. We supplement this with similar information on four additional respondent banks in these countries, collected as part of an online survey we conducted together with EBRD's Trade Facilitation Programme.[5] This survey focused exclusively on correspondent banking. Online Appendix C contains the survey questions used.

5. This survey also covers some banks from the BEPS III survey. As BEPS III was conducted later and thus entails more recent information, we use the information obtained through BEPS III for these banks.

3.2 Firm exports and other firm characteristics

To estimate the impact of the rapid decline in correspondent banking services, we access firm-level data from Orbis. Orbis provides comprehensive information on balance sheets and income statements and, for the countries we focus on, also yearly data on export revenues. It also provides the address of firms and information on a firm's industry. We obtain the Orbis flat files of June 2022 and follow Kalemli-Özcan et al. (2024) to construct nationally representative samples.[6]

3.3 Bank characteristics and bank branch networks

In our main analysis, we match firms and banks by using Orbis information on each firm's main bank, which we refer to as its house bank. This establishes a direct link between individual firms and banks. We merge this information with Orbis BankFocus to obtain balance sheet and income statement data for each bank.

Another way to connect firms and banks is to match a firm's location with information on all nearby bank branches. Data on the geo-coordinates of branches was hand-collected as part of the BEPS III survey. In total, we have data on the geo-coordinates of 48,399 branches: a near complete picture of the branching landscape in 2020. We follow Beck et al. (2018) and, after ensuring that the names of localities (villages, towns, and cities) are spelled consistently in both data sets, match firms and branches by locality. For instance, we link all Orbis firms in the Croatian city of Dubrovnik to all bank branches in Dubrovnik. This assumes that a firm can access all branches in the locality where it is incorporated and that it may be negatively affected by the loss of correspondent relationships of such local banks.[7] We retain any locality in which we have at least one firm and at least one branch of a surveyed bank.

6. We adjust the export data for Hungarian firms to address a country-specific issue where firms either report positive exports or the variable is missing, but never report zero exports. For Hungarian firms that report positive exports in other years, we set any missing yearly export values to zero.

7. We posit that a firm's geographical location exogenously constrains its access to lenders (Berger et al. 2005). A substantial body of empirical research supports this spatial credit rationing hypothesis. For instance, Degryse and Ongena (2005) found that the median Belgian SME borrower was located 2.5 km from its lending bank branch, while U.S. studies by Petersen and Rajan (1994) and Agarwal and Hauswald (2010) reported median distances of 3.7 km and 4.2 km, respectively.

Linking firms to their main bank clearly distinguishes those directly affected by terminated correspondent relationships from those unaffected, without assuming access to nearby branches. A disadvantage is that Orbis only provides information on a firm's main bank for larger enterprises, thus skewing the sample towards firms that may be less affected by lost correspondent banking relationships. Locality-level matching focuses on local equilibrium effects, assuming competitive banking markets where firms' access to services can be constrained by local shocks to correspondent banking availability. We apply both matching approaches and show that the results are very similar.

We focus on pre-treatment exporters, as they are most likely impacted by reduced correspondent banking. The trade literature also shows that exporters are inherently different from other firms (Bernard et al. 2007). Our sample includes 65,698 unique exporters with 24 distinct main banks when matched via banks (Table 1, Panel A), and 121,097 unique firms across 743 localities when linked to local branches.

4 Identification and empirical strategy

4.1 Identification

We exploit the loss of banks' correspondent bank relationships as an exogenous shock to their corporate customers' access to payment and trade finance services. Our DiD framework then compares the outcomes of firms linked to a bank that lost at least one correspondent relationship (treated firms) to similar firms whose bank did not loose any such relationship up to the event year (control firms). Over the entire sample period, the banks of treated firms lost 1.27 correspondent relationships on average (Table 1, Panel A).

Our identification strategy assumes that, conditional on the comprehensive set of fixed effects and control variables, firms whose primary bank experienced the loss of one or more correspondent bank relationships were not subsequently exposed to unobserved shocks that were systematically correlated with being connected to that particular bank. Valid inference does not require correspondent relationships to terminate randomly across banks or localities,

nor does it necessitate identical pre-treatment characteristics for firms in treated and control groups. Our coefficients of interest will remain unbiased if exporters in both groups would have evolved similarly without the shock to the correspondent banking network. Although this assumption is inherently untestable, we provide two pieces of supporting evidence. First, in Section 5, we demonstrate that prior to the sudden decline in correspondent banking, there were no systematically different pre-trends in export performance between treated and control firms, suggesting that both groups would have developed comparably in the absence of the decline. Second, while our design doesn't rely on treated and control firms being similar in levels, such similarity would further bolster the credibility of the common trends assumption, which we substantiate in Section 5.

4.2 Matching

To retain as many observations as possible, while ensuring that treated and control firms are comparable in their propensities to export, we keep all firms on the common support in the year before treatment. We match on pre-event export turnover, total assets, and total factor productivity (TFP). TFP is calculated as the industry-adjusted residual of a two-factor Cobb-Douglas production function (Melitz 2003; Bernard et al. 2007).[8] Focusing on firms with common support also ensures that our results are not driven by outliers.

Table 1 provides summary statistics for the complete sample (Panel A) and the matched sample of 26,636 common-support firms (Panel B and Table 2). We also report the difference in averages by treatment status, scaled by the square root of the sum of the variances. This normalized difference provides a scale-free measure of the difference in distributions. As a rule of thumb, Imbens and Wooldridge (2009) suggest that normalized differences below 0.25 (in absolute values) indicate sufficient similarity in the variable distribution in the treatment and control groups. Panel A of Table 1 shows that these normalized differences are already well below the 0.25 threshold in the complete exporter sample.

8. For Turkish firms, we calculate TFP using a one-factor Cobb-Douglas production function, with total assets as the sole input, due to missing employee data for a fraction of Turkish firms in Orbis.

4.3 Specification and estimator

4.3.1 Empirical specification

To gauge the firm-level impact of broken correspondent bank relationships, we estimate:

$$Outcome_{it} = \sum_{k=-4,k\neq-1}^{k=+4} \beta_k \times D_k \times Lost\,relationship_{jt} + \beta_5 \times Firm\,controls_{it} + \beta_6 \times Bank\,controls_{jt} + \gamma_i + \delta_{ct} + \zeta_{st} + \epsilon_{ijcst} \quad (1)$$

where subscripts i, j, t, c, s stand for firm, bank, year, country, and sector (industry), respectively. Our $Outcome_{it}$ variables are *Export dummy*, *Exports*, *Domestic revenues*, *Total revenues*, *Employees*, and *Dissolved*. *Export dummy* captures the extensive export margin and is one if a firm exports in a given year; zero otherwise. *Exports* measures revenues from export activities in log euros. *Domestic revenues* measures revenues from domestic activities in log euros, while *Total revenues* captures total operating revenues in log euros. *Employees* is the log number of employees. Lastly, *Dissolved* is a dummy that is one if the firm exits Orbis in the subsequent year and is not acquired by another company; zero otherwise.

D_k are dummies that are one at time k with k indicating individual years before or after the event (i.e., a bank's loss of a correspondent bank relationship). $Lost\ relationship_{jt}$ is a dummy that equals one if bank j lost a correspondent relationship in year t or earlier. γ_i, δ_{ct}, and ζ_{st} are firm, country-by-year, and industry-by-year fixed effects, respectively. Firm fixed effects eliminate time-invariant heterogeneity across firms, specifically addressing potential pre-existing differences in attributes between treated and control firms. By including country-by-year (industry-by-year) fixed effects, we limit identifying variation to comparisons of firms within the same country (industry) in each time period. This absorbs all time-varying unobserved heterogeneity across countries (industries), such as variations in business (industrial) cycles, which might correlate with firm performance. Robust standard errors are clustered by bank.

We hypothesize that broken correspondent relationships negatively affect exports, leading to negative β_0 to β_4 coefficients for *Export dummy* and *Exports*. If firms partially offset reduced exports with increased domestic sales, the β coefficients will be positive for domestic revenues. Yet, the impact on overall revenues and employment will be negative unless firms fully compensate for lower exports with domestic sales. We also expect broken correspondent relationships to increase the likelihood of firm exit, resulting in positive β coefficients for *Dissolved*. The causal interpretation of our findings hinges on the parallel trends assumption, which would be supported by insignificant β coefficients in the pre-event years.

To mitigate any lingering concerns about omitted variable bias, we add a vector of time-varying $Firm\ controls_{it}$ and $Bank\ controls_{jt}$. At the firm level, we include log *Total assets* and *Total Factor Productivity*. $Bank\ controls_{jt}$ include *Loan growth* (a bank's annual percentage change in gross lending), *Equity/Total assets* (bank capitalization), *Loans/Customer deposits* (the extent to which bank loans are funded by wholesale funding rather than deposits), and *ROA* (return on assets). In our second sample, where firms are matched to bank branches in the same locality, we use branch-weighted averages by locality for these bank controls. This approach uses data from all banks with branches in a locality, regardless of the change, if any, in their correspondent relationships.

4.3.2 Choice of estimator

Standard Two-Way Fixed Effects (TWFE) regressions can bias estimates when treatment effects are heterogeneous due to implicit weighting (and potential negative weights) of firms' average treatment effects (ATEs) (de Chaisemartin and D'Haultfœuille 2020; Borusyak, Jaravel, and Spiess 2024). We therefore use the imputation estimator of Borusyak, Jaravel, and Spiess (2024), which allows for heterogeneous treatment effects across firms and dynamic effects around events.[9] This estimator fits unit and period fixed effects using only never treated and not-yet-treated observations, then imputes untreated potential outcomes for each treated observation. The treatment effect is the difference between a firm's potential

9. Online Appendix E shows that negative weights may indeed be relevant in our setting.

and actual outcome, with the estimator computing a weighted sum of these effects.

While we employ Borusyak, Jaravel, and Spiess (2024)'s approach for its efficiency, we replicate our results using de Chaisemartin and D'Haultfœuille (2024)'s estimator in Online Appendix F, as it performs better when errors correlate strongly (Harmon 2023).[10] This estimator also produces estimates suitable for visually assessing pre-trends, avoiding potentially misleading kinks or jumps at the time of treatment (Roth 2024). We systematically refer to our de Chaisemartin and D'Haultfœuille (2024) specifications when assessing pre-trends, noting that the two estimators report pre-trends for different periods. The de Chaisemartin and D'Haultfœuille (2024) estimator uses $t = -1$ as the baseline, while Borusyak, Jaravel, and Spiess (2024)'s estimator estimates pre-trends for all pre-event years.

5 Empirical results

This section examines the impact of broken correspondent relationships on firm outcomes, first by linking firms directly to their main bank (Section 5.1), and then by linking firms and banks at the locality level (Section 5.2). Section 5.3 explores treatment effect heterogeneity, while Section 5.4 estimates within-industry spillovers. The robustness of our findings is discussed in Section 5.5. Finally, Section 5.6 extends the analysis to a larger country sample using bilateral sectoral trade data.

5.1 Terminated correspondent relationships and firm-level outcomes

5.1.1 Likelihood to export and total export revenues

We first investigate the effect of terminated correspondent relationships on firms' likelihood to export and their export revenues. Figure 4 shows the dynamic DiD coefficient estimates, based on the Borusyak, Jaravel, and Spiess (2024) estimator and including *Firm controls*, *Bank controls*, country-by-year fixed effects, and industry-by-year fixed effects. The first

10. The estimator of de Chaisemartin and D'Haultfœuille (2024) follows an alternative method to account for non-parametric country and industry trends. When using that estimator, we will therefore refer to non-parametric country and industry trends rather than fixed effects.

two columns of Table 3 report the corresponding static regression results (i.e., the average estimated post-event coefficients).[11]

We find that when a firm's main bank loses one or more connections with correspondent banks, the chances of it continuing to export decrease significantly compared to similar firms whose main bank does not lose any such connections (Figure 4, left). The probability to export is 5.2 percentage points lower for treated compared with control firms right after the termination of one ore more correspondent relationships. This difference becomes more pronounced over time. After four years, treated firms have a 19.8 percentage point lower probability to export. These sizeable effects reflect that many firms in our sample are small and medium-sized enterprises, which may find it difficult to switch banks to replace lost correspondent relationships. The right-hand graph in Figure 4 shows similar results for export revenues. We again observe that the effects of the termination of correspondent relationships are felt immediately and increase over time.[12] Four years after the event, the export revenues of affected firms are 57 percent lower than those of similar control firms.[13]

As a robustness check, we re-run our analysis using the de Chaisemartin and D'Haultfœuille (2024) estimator. We report the results in the upper panel of Figure F1 in Online Appendix F and in the middle of Table 3.[14] The results are very similar. The insignificant pre-event effects suggest that treated and control firms would have developed along parallel paths in the absence of broken correspondent relationships. As mentioned in Section 4, an advantage of the de Chaisemartin and D'Haultfœuille (2024) estimator is that it provides us with valid visual heuristics to assess pre-trends when using long-differences for the pre-treatment and post-treatment coefficients (Roth 2024).

Overall, these results illustrate how a sudden termination of correspondent banking rela-

11. In Table F3 in Appendix F, we show that our coefficient estimates are quantitatively and qualitatively very similar when excluding firm-level covariates, bank-level covariates, or both.

12. If our estimated coefficients from the log-linear specifications were small enough, we could interpret them as percentage changes in the outcome. However, as shown in Table 3, the effect is, in fact, quite sizable. We therefore report equivalent coefficients from the exact linear transformation of the log-linear estimates.

13. The percentage change in outcome is calculated as follows: $exp(\beta) - 1 = exp(-0.845) - 1 = -0.570$.

14. We normalize D_{-1} to zero because the de Chaisemartin and D'Haultfœuille (2024) approach does not provide estimates for the pre-event year.

tionships negatively affects firms' export performance on the extensive and intensive margins.

5.1.2 Domestic and total revenues

Firms whose main bank lost access to global correspondent banks might turn to domestic markets to make up for their reduced ability to sell abroad. If they do so successfully, their total revenues and employment may be affected less negatively or perhaps not at all. We therefore now analyze how the termination of correspondent relationships affects firms' domestic and total revenues.

Figure 5 depicts our estimates for firms' domestic revenues (left) and total revenues (right). We find that firms are, on average, indeed successful in expanding their local sales in the four years after their main bank loses correspondent relationships. However, the right-side panel, and the average effects in column (4) of Table 3, show that affected firms nevertheless cannot completely offset their reduced exports with more local sales. After four years, total turnover is approximately 1.3 percent lower (on average and conditional on survival). Estimates using the de Chaisemartin and D'Haultfœuille (2024) approach again confirm our findings as well as the absence of differential pre-trends—see the middle panel of Figure F1 in Online Appendix F and column (4) in Table 3.

5.1.3 Employment and dissolution of firms

In line with the negative effect on firms' total revenues, the graph on the left of Figure 6 and the results in column (5) of Table 3, show a small negative effect on the number of firm employees. Firms whose main bank lost one or more correspondent relationships, and which managed to stay in business nevertheless, shrank their workforce by 1.8 percent within a year and 3.1 percent within four years, compared with similar unaffected firms.

The graph on the right of Figure 6 and the results in column (6) of Table 3, show that not all firms successfully cope with the loss of correspondent banking relationships (such as by refocusing on domestic sales). Firms whose main bank lost a correspondent bank relationship are significantly more likely to be dissolved within the subsequent year than similar control

firms. Compared to the average exit probability of not-yet and never-treated firms, the likelihood that an affected firm closes down altogether even becomes 2.5 times larger.

Our estimates using the de Chaisemartin and D'Haultfœuille (2024) approach are again similar (Online Appendix F, Figure F1, lower panel). While they do not show a precisely estimated negative impact on employment in the short run (this effect only becomes statistically significant at $t = 4$), they confirm the immediate impact on the probability of firms terminating their business altogether.

Our findings so far provide a comprehensive picture of how the fragmentation of global correspondent banking impacts firms. When a firm's main bank loses correspondent relationships, we observe declines in export revenue, total revenues, and employment levels, as well as an increased probability of firm closure. These impacts persist and intensify for several years after the shock, suggesting that firms struggle to switch lenders and regain access to necessary correspondent banking services. Before exploring which firm types are most vulnerable to these negative effects in Section 5.3, we examine locality-level matching as an alternative method for linking firms to banks.

5.2 Locality-level matching

So far, we have linked firms directly to their main bank. The advantage is that this allows us to pinpoint the specific firms affected by correspondent bank withdrawals. A disadvantage is that we lose sight of local equilibrium effects. For example, when a firm whose bank loses access to correspondent banking is no longer able to export, this may present an opportunity to other local exporters whose bank did not suffer any broken correspondent relationships.

In this section, we take a different approach, linking each firm to all bank branches in its locality of incorporation. This allows us to estimate the local equilibrium effect of terminated correspondent relationships on the average exporting firm in a locality, regardless of whether a firm is a client of an affected bank. We employ a DiD framework to compare firm-level outcomes in treatment localities (where at least one bank lost a correspondent relationship) with observationally similar firms in control localities, before and after the local shock to

correspondent banking. Figure 7 presents dynamic DiD results, while Table F2 in Online Appendix F shows the related static results. As before, we use the Borusyak, Jaravel, and Spiess (2024) estimator and include firm and bank covariates, as well as country-by-year and industry-by-year fixed effects in all specifications.[15]

The results using locality matching are similar to those using firm-bank linkages, with a local decline in the supply of correspondent services negatively affecting firms' extensive and intensive export margins. As these are local equilibrium effects, their magnitude is somewhat smaller than those based on firm-bank matching. For example, the average local effect of terminated relationships on firms' propensity to export is -14.9 percentage points after four years, compared to -19.8 percentage points when firms are directly linked to banks. However, these strong and persistent negative export impacts found at the local level suggest that broken correspondent relationships have consequences beyond directly affected firms.

We also find that surviving firms partially substitute foregone exports by expanding domestic sales, and conditional on surviving, local firms experience quite mild overall revenue losses on average. We observe no significant average local equilibrium effects on eiher employment or exit probability.[16]

5.3 Firm-level heterogeneity

We now analyze whether the impact of a decline in correspondent relationships varies meaningfully across different firm types. Small, young, and less productive firms may find it especially difficult to switch to other banks if their main lender can no longer offer the necessary services to facilitate international trade transactions. Such firms may also find it more challenging to reorient their sales towards the domestic market. Additionally, the characteristics of a firm's main lender may influence the impact of correspondent relationship losses. Banks with many correspondent relationships may mitigate the negative effect of losing a sin-

15. See Table F1 in Appendix F for related summary statistics.

16. We also estimate the locality-matched regressions using the de Chaisemartin and D'Haultfœuille (2024) estimator. The dynamic and static results, reported in Figure F2 and Table F2 of Appendix F, respectively, confirm the main results.

gle correspondent bank, while banks with just one or only a few correspondent relationships may be hit harder and consequently transmit more of this shock to their corporate clients.

To investigate heterogeneity along these dimensions, we run subsample regressions, reporting the dynamic DiD results in Figures 8–11 and static equivalents in Table F4, which also includes F-tests for significant differences across subsamples. First, we examine whether smaller firms are more sensitive to lost correspondent relationships than larger ones, defining small (large) firms as those with below (above) median total assets. The upper left graph of Figure 8 shows the effect of a decline in correspondent relationships on small firms' (red dots) and large firms' (blue triangles) probability to export (*Export dummy*). Although the export probability declines sharply for both subsamples, this decline is greater for small firms (as confirmed by F-tests in Table F4). The upper right graph of Figure 8 shows similar estimates for export revenues (*Log export revenues*), with a larger effect for small firms (although this difference at the intensive margin is not statistically significant).

The middle panel of Figure 8 reveals that the effects on total revenues are essentially zero for large firms, consistent with the findings for the entire sample. However, smaller firms experience significantly negative impacts on total revenues when hit by a loss in correspondent relationships. These small firms face a substantial decline not only in export revenues but also in *overall* revenues—a drop of about 10 percent compared to control firms. Consequently, as shown in the lower panel, small firms significantly reduce employment by up to 17 percent four years after the shock, while large firms' employment remains relatively unaffected. Although small firms experience a larger increase in the probability of being dissolved post-treatment, this impact is not statistically significantly different from that of larger firms.

In Figure 9, we examine the role of firm age. We define young (old) firms as those with a below (above) median age. While both young and old firms experience a significant decline in their probability to export and in their export revenues when their main bank's correspondent relationships deteriorate, this effect is more pronounced for young firms. The difference becomes starker over time and gets statistically significant at t=3. While young

firms partially mitigate export revenue losses by expanding their domestic sales more than older firms, they still experience a significant decline in total revenues post-treatment, unable to fully offset the sharp contraction in their export performance. Notably, we find no evidence that the likelihood of firm exit in response to negative shocks to their bank's correspondent network differs between young and old firms.

Next, we investigate whether firms with different average revenue product of capital (ARPC) are impacted differently by the decline in correspondent banking relationships. ARPC serves as a proxy for a firm's productivity of capital, holding other factors of production constant. We define ARPC as the ratio of a firm's total revenues to its fixed capital stock, and categorize firms as either below or above the median ARPC level. Figure 10 illustrates that the treatment effects on all firm outcomes are remarkably similar across firms with different ARPC levels. This indicates that the impact of disrupted correspondent banking relationships is indiscriminate and does not disproportionately affect firms with more or less productive capital utilization.

Finally, we investigate whether the number of correspondent relationships a firm's main bank has prior to losing access to some correspondent banks, affects the firm's vulnerability to this loss (Figure 11). We categorize banks into two groups: those with a below-median number of correspondent banks pre-treatment and those with an above-median number. Strikingly, firms whose main bank had relatively many initial correspondent banks do not experience significant effects for any of the outcome variables. In contrast, firms with banks that had below-median correspondent banking relationships experience significant and substantial effects across all variables of interest. Four years after treatment, their export probability is 51 percentage points lower than that of control firms; their export revenues have declined by approximately 20 percent; their total revenues have decreased by nearly 5 percent; their employment has reduced by about 15 percent; and their exit probability has increased by 0.40 percentage points. These findings clearly underscore the importance of a diversified network of correspondent banking relationships in mitigating the adverse effects

of correspondent bank withdrawals on firms. This is consistent with recent research showing that firms with concentrated relationships, whether with clients or suppliers, are more vulnerable to external shocks. For example, exporters relying on a small number of key clients have been found to be more exposed to demand fluctuations (Kramarz, Martin, and Mejean 2020), while firms dependent on specialized suppliers of differentiated inputs are at greater risk of disruption from idiosyncratic supplier issues (Barrot and Sauvagnat 2016).

5.4 Within-industry spillovers

In addition to within-locality spillovers (Section 5.2), we examine spillovers from treated to control firms and from treated to other treated firms within the same industry. Terminating correspondent relationships may benefit control firms in the same industry but in different locations, as their competitive position improves relative to treated firms that lost local access to correspondent banking services. Moreover, the impact on treated firms may depend on the proportion of affected firms within their industry. If a higher fraction of firms in an industry are treated, the negative impact on individual treated firms may be less severe, as trading partners have fewer unaffected suppliers to switch to.

To analyze the role of both types of spillovers, we follow Berg, Reisinger, and Streitz (2021) and use our locality-matched sample to estimate spillovers within industries.[17] More specifically, we estimate the following heterogeneous spillover model using OLS:

$$\begin{aligned} Outcome_{ijst} = \beta_0 + \beta_1 d_{ijst} + \beta_T \bar{d}_{st} d_{ijst} + \beta_C \bar{d}_{st}(1 - d_{ijst}) \\ + \beta_2 \times Firm\ controls_{it} + \beta_3 \times Bank\ controls_{jt} + \gamma_i + \delta_t + \epsilon_{ijst} \end{aligned} \tag{2}$$

where subscripts i, j, s and t stand for firm, bank, sector (industry), and year, respectively.

As dependent variables ($Outcome_{ijst}$), we focus on *Export dummy* and *Log export revenues*. d_{ijst} is our treatment indicator, which is one when at least one correspondent bank relationship is lost in the locality of firm i. $\bar{d}_{st}$ denotes the (time-varying) proportion of

17. We abstract from local spillovers as these were analyzed in Section 5.2, where we estimated local equilibrium effects that aggregate firms' individual treatment effects and locality-level spillovers.

treated firms in an industry (without firm i). *Firm controls*$_{it}$ include *Total assets* and *Total Factor Productivity*. *Bank controls*$_{jt}$ comprise *Local loan growth*, *Equity/Total assets*, *Loans/Customer deposits* and *ROA* as defined in Section 4.3. γ_i and δ_t are firm and year fixed effects, respectively. The coefficients of interest reflect the direct treatment effect (β_1); the spillover effect to treated firms (β_T); and the spillover effect to control firms (β_C).

We plot the outcomes *Export dummy* and *Log export revenues* as a function of treatment intensity—the proportion of treated firms in an industry—for treatment units, control units, and group averages. The regressions are estimated using OLS and Table 3 provides the equivalent static results without accounting for spillovers.[18]

Figure 12 (left) summarizes the spillover analysis for the probability to export (*Export dummy*). The direct treatment effect is the impact of a decline in correspondent relationships if no other firm in the same industry is treated. This effect, represented by the difference between treatment and control firms at a treatment fraction of zero, is -6.8 percentage points. The rising solid line indicates that the negative impact on treated firms diminishes as the proportion of other treated firms in the industry increases. This suggests that when more firms within an industry are affected, trading partners have fewer opportunities to source similar products from unaffected exporters in other locations.

The rising dotted line indicates that control firms—exporters in localities unaffected by the decline in correspondent relationships—benefit from positive spillovers. As the fraction of treated firms within an industry increases, the probability of control firms exporting rises. When treated suppliers face difficulties in exporting or cease exports altogether due to locally disrupted correspondent relationships, firms in unaffected localities may capture some of their business.

The positive spillovers among treated firms slightly exceed the spillovers to control firms, causing the difference between the two groups to narrow as the proportion of treated firms

18. Table 3 also reports static treatment effects using the Borusyak, Jaravel, and Spiess (2024) and de Chaisemartin and D'Haultfœuille (2024) estimators. All three approaches yield very similar results and confirm that cutting correspondent relationships reduces firms' exports at the intensive and extensive margins.

within an industry increases. Consequently, failing to account for spillover effects leads to a minor underestimation of the direct treatment effect. The dashed line in the left graph of Figure 12 shows the industry-level average probability of exporting as a function of the proportion of treated firms (normalized to zero). The increasing slope reflects the diminished negative impact on treated firms when they constitute a larger share of the industry.

The right graph of Figure 12 presents similar findings for export revenues (*Log export revenues*). When no other firm in the industry is treated, the direct treatment effect is a 23.7 percent decrease (equivalent to a log-linear coefficient of -0.274). The rising dotted line indicates positive spillovers to control firms as the proportion of treated firms increases. Similarly, the increasing solid line shows that treated firms experience a less severe negative impact when a larger fraction of their industry peers are also affected, consistent with our findings for the probability of exporting.

In summary, the observed spillover patterns suggest that treated firms face less severe consequences when a higher proportion of their industry peers are also impacted, as this constrains importers' ability to switch to alternative suppliers in unaffected localities. Conversely, untreated firms seem to benefit from positive spillovers as the share of treated firms in their industry grows, likely due to capturing some of the lost business from affected exporters.

5.5 Orthogonality test

While our methodology only requires treated and control firms to follow similar trends, rather than being comparable in levels, demonstrating their comparability would further support the common-trends assumption. We now provide evidence that correspondent banks' withdrawal decisions are not systematically associated with various traits of firms, banks, and localities.

In the first two columns of Table D1 in Online Appendix D, we use the cross-section of firms averaged over 2008–2020 to estimate the relationship between various firm, bank, and locality characteristics and whether a firm's main bank lost access to correspondent banking. These characteristics include firms' total assets, productivity, revenues, and employees; their main banks' total assets, equity ratios, profitability, loans over customer deposits, and loan

growth; the number of firms and bank branches; as well as local firm and bank concentration expressed as a Herfindahl-Hirschman Index (HHI). In column (2), we also include locality fixed effects while in column (3), we examine the fraction of treated firms in a locality based on the same (locality-averaged) firm, bank, and locality characteristics.

Table D1 demonstrates that a firm's main bank experiencing a correspondent withdrawal is largely independent of these firm, bank, and locality characteristics. Only three variables, namely banks' size, their capitalization, and the locality-level HHI, exhibit a correlation at the 5 percent significance level in at least one of the three specifications. To mitigate any potential bias stemming from these correlations, we incorporate firm and bank characteristics as control variables in all our regression analyses, as detailed in Section 4.3.

5.6 External validity: Bilateral sectoral trade data

We now expand our analysis to 17 Emerging European markets using bilateral sectoral trade data.[19] We exploit the tightening of the U.S. regulator's enforcement of financial crime legislation in June 2014 as a structural break that negatively shocked the supply of correspondent services. Although these industry-level estimates are less cleanly identified than our firm-level ones, they can support their external validity and allow us to examine whether firm-level export declines aggregate to the industry level. Additionally, the industry-level approach benefits from the availability of import data. Bank-intermediated trade finance products, such as letters of credit, can also be crucial when less developed countries import goods from advanced countries (Schmidt-Eisenlohr 2013; Antras and Foley 2015).

To examine the implications of correspondent bank withdrawal in this broader sample, we employ a DiD approach once again. We compare sectoral, bilateral trade growth in the 12 months before and after June 2014 between countries with high versus low levels of correspondent bank withdrawal. The key identifying assumption is that the withdrawal of correspondent banks is uncorrelated with concurrent shocks to the demand for correspondent

19. Bosnia and Herzegovina, Bulgaria, Croatia, Czech Republic, Estonia, Georgia, Hungary, Latvia, Lithuania, FYR Macedonia, Moldova, Montenegro, Poland, Romania, Serbia, Slovakia, and Slovenia.

banking services in specific countries. Appendix B provides definitions and sources for all variables, while Table F5 in Appendix F presents summary statistics.

We measure the supply shock to correspondent banking using survey data from the Financial Stability Board's Correspondent Banking Coordination Group (CBCG) (Financial Stability Board 2017). The CBCG surveyed 345 banks across 48 jurisdictions, covering most large correspondent banks and local banks. Our measure captures the percentage change in the number of active correspondent banks in a country from January 2011 to June 2016. Countries with high correspondent bank withdrawal experience an average 19 percent decline, while those with low withdrawal face an average decrease of only 7 percent.

We obtain monthly sectoral, bilateral trade data from the UN Comtrade database for 2012–2015 at the country-industry-counterparty level, in US dollars and using 2-digit HS industry codes. For each country-industry-counterparty triple, we aggregate observations into 12-month periods from July to June of the following year to match the time windows before and after the event. The dependent variables are first differences in log exports ($\Delta LogExports$) and log imports ($\Delta LogImports$) in the 12 months before and after June 2014. Aggregating trade flows into pre-event and post-event periods mitigates the risk of underestimating standard errors due to potential serial correlation in the monthly trade data (Bertrand, Duflo, and Mullainathan 2004). Defining the dependent variable in first differences follows previous literature, allowing for different pre-treatment time trends between treatment and control groups (Khandelwal, Schott, and Wei 2013; Claessens and Van Horen 2014; Demir, Michalski, and Ors 2017; Claessens and Van Horen 2021). The DiD model, focusing on changes in exports and imports during a narrow time window around the event date, is:

$$Outcome_{sijt} = \beta_1 \times Post_t \times High\ withdrawal_{it} + \beta_2 \times X_{sijt} + FE_i + \epsilon_{sijt} \tag{3}$$

where subscripts s, i, j, and t denote the sector, Emerging European country, trading partner country, and 12-month period before and after the event, respectively. $Outcome_{sijt}$ variables

are $\Delta LogExports_{sijt}$ and $\Delta LogImports_{sijt}$, which are growth rates in exports and imports from sector s in country i to/from country j in period t relative to period $t-1$. $Post_t$ is an indicator variable equal to zero in the July 2013–June 2014 period and one in the July 2014–June 2015 period. $High\ withdrawal_{it}$ equals one for countries with an above-median reduction in correspondent relationships; zero otherwise. X_{sijt} are country, trading partner country, sector, and time-specific control variables. FE_i are Emerging European country fixed effects that fully absorb any time-invariant factors affecting both treatment selection (i.e., correspondent bank retrenchment) and export or import growth. Robust standard errors are clustered at the trading-country level, following Claessens and Van Horen (2021).

Our variable of interest is the interaction term $Post \times High\ withdrawal$. β_1 captures the additional change in export or import growth for countries facing a high withdrawal of correspondent banks relative to those less affected. We expect β_1 to be negative.

In the baseline specifications, we include $\Delta LogExports_{st}$ *(World)* and $\Delta LogImports_{st}$ *(World)* to control for industry-level trade trends. These are the first differences of global industry exports and imports, respectively, at the 2-digit ISIC code (in natural logarithms). We also include $LogGDP\ Counterparty_{jt}$, the natural logarithm of the GDP of the trading partner country, and $LogDistance_{ijt}$, the natural logarithm of the distance between the Emerging European country i and the trading partner country j. Both serve as standard gravity variables.

In more saturated specifications, we include a battery of fixed effects to further mitigate concerns about omitted variables bias. Industry fixed effects control for time-invariant industry factors at the 2-digit ISIC level, while industry-time fixed effects also control for time-varying industry factors, such as industry-specific demand shocks. Partner-time fixed effects control for both time-invariant and time-varying factors related to the importing economy, such as demand shocks. Finally, trading-country pair fixed effects fully absorb overall export growth rates, geographical distance, and cultural and institutional proximity between exporter and importer countries.

We ensure that our results are not driven by the stronger dollar in the second half of 2014 on the back of expectations about interest-rate raises by the Federal Reserve. A stronger US$ could explain a reduction in imports of US$-denominated goods. As information on the proportion of US$-denominated trade is not available for our countries, we approximate the sector share of US$-denominated exports in total exports as the proportion of goods exported to the Americas (as trade with this region is predominantly dollar denominated). We then control for exposure to the dollar valuation shock by including the interaction term *Prop. US$ Exports x* $\Delta LogUS\$/EUR$, where *Prop. US$ Exports* is the proxy for the country-level proportion of US$-dollar exports in total exports and $\Delta LogUS\$/EUR$ is the first difference in the log US$/EUR exchange rate from July of year $t-1$ to June of year t.

Table 4 presents the DiD regressions, with first differences in log exports as the dependent variable in columns (1) to (3) and first differences in log imports in columns (4) to (6). Columns (1) and (4) show the baseline specification, while columns (2) and (5) include industry fixed effects and the interaction term *Prop. US$ Exports x* $\Delta LogUS\$/EUR$. Columns (3) and (6) incorporate industry-time, partner-time, and trading-country pair fixed effects.

We find that, across all specifications, export growth declines significantly more in countries with a high withdrawal of correspondent banks compared with countries where no or only few correspondent banks left. The economic magnitude of the effect is very similar across specifications. In the most saturated specification, column (3), the export growth rate is 8 percentage points lower in countries with a high withdrawal in correspondent banking than in countries with a low withdrawal. This effect is economically large given that the average pre-period export growth rate is 15 percent. We obtain similar results on the import side. The decline in import growth is significantly sharper for countries that experience a high withdrawal in correspondent banks. In the most saturated specification in column (6), import growth rates for high-withdrawal countries decrease an additional 24 percentage points relative to low-withdrawal countries.[20]

20. We ran robustness checks where Russia and Ukraine are omitted as trading partners to make sure Russia's war on Ukraine does not impact our results. We also ran specifications excluding trading partners

6 Conclusions

We have presented novel evidence on how a sudden decline in correspondent banking relationships, triggered by a sharp increase in regulatory compliance costs, hindered firms' ability to engage in international trade. Using detailed data on lost correspondent banking links of respondent banks in emerging Europe, we find strong negative effects on firm-level exports along both the extensive and intensive margins.

While some affected firms partially offset these foregone exports by expanding domestic sales, others experience a drop in overall revenues, reduce their workforce, or cease operations altogether. On average, a firm whose main bank loses one or more correspondent relationships has a 20 percentage point lower probability of exporting after four years. At the sector level, these firm-level impacts aggregate to an 8 percentage point lower export growth in countries with a high correspondent banking withdrawal. These effects are comparable to the 18 percent drop in global sales experienced by US firms affected by the temporary shutdown of EXIM bank (Kabir et al. 2024) and the 21 percent decline in long-term exports from countries exposed to the 19th century London banking crisis (Xu 2022).

Small and young firms are hit especially hard by the global retrenchment of correspondent banks, reflecting that such firms typically find it more difficult to switch to alternative banks when their main bank loses access to correspondent banking services. Similarly, firms whose banks had relatively few correspondent relationships to begin with suffer more, as the withdrawal of even a single correspondent bank can leave their main bank with insufficient cross-border payment and trade finance options.

Industry-level spillovers also shape the impact of correspondent banking shocks. Treated firms experience less severe export reductions when a larger share of their industry peers are similarly affected, likely because importers face greater difficulty switching to alternative

and industrial goods experiencing a particular large decline. In addition, we validate that our results are robust to measuring the country-level change in the availability of correspondent services as the percent change in the value of SWIFT transactions between 2012 and 2015 (taken from BIS 2016). Banks are assigned to the treatment (control) group if they experience an above (below) median reduction in SWIFT transactions. All these robustness results are very similar to the main ones and available upon request.

suppliers. Conversely, unaffected firms appear to benefit from positive spillovers as more of their industry rivals lose access to correspondent banking services, possibly capturing some of the forgone trade. These findings highlight how the effects of correspondent banking shocks can propagate across industries, influencing competitive dynamics and trade patterns.

The impact of broken correspondent relationships on emerging market exports may be long-lived, as rebuilding local knowledge and trade links takes time. Policies such as government-backed trade insurance schemes may help mitigate the fallout in the short term. In the longer term, efforts could focus on improving respondent banks' compliance with international financial-crime regulations and risk-management procedures, as well as exploring new financial technologies to facilitate safe and efficient cross-border payments. In particular, digitalizing customer screening by respondent banks may improve the quality and timeliness of information available to correspondent banks, making them more willing to re-engage.

More generally, our findings demonstrate how abrupt increases in regulatory scrutiny can unintentionally disrupt vital banking networks, resulting in significant and lasting negative impacts on firms' export opportunities and local economies. Policymakers must strike a balance between the benefits of tighter financial-crime regulations and the unintended economic costs associated with an unraveling correspondent banking network.

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Figures

Figure 1: Concentration in the global correspondent banking market

This figure shows the Gini coefficient of the number of active correspondent banks per corridor between 2012 and 2022 as a measure of concentration in the global correspondent banking market. The Gini coefficient is based on the three month moving average of active correspondents, using a constant number of corridors. A corridor is defined as a single-direction jurisdiction pair (for example, Croatia to the U.S. is a corridor and the U.S. to Croatia is another corridor). Source: SWIFT data from the Bank for International Settlements (BIS).

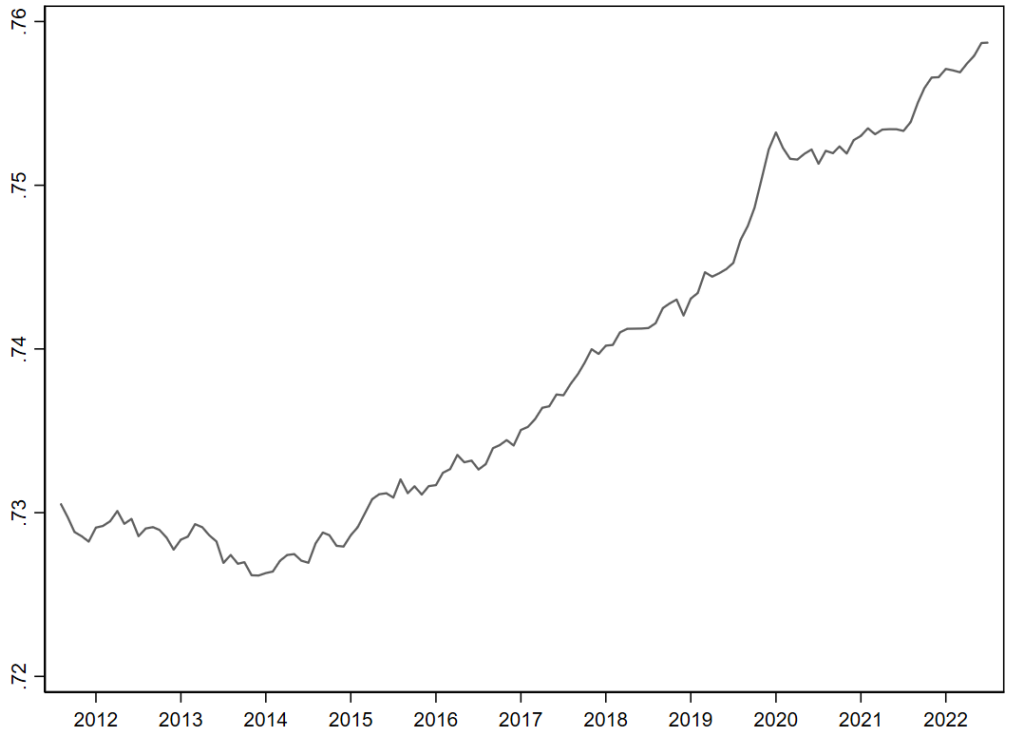


Figure 2: Reasons for the withdrawal of correspondent banks

This pie chart shows local respondent banks' answers to the question: "Out of all relevant causes for terminating correspondent bank relationships, which do you consider most important?". We asked this question as part of an online bank survey at the end of 2019. 91 banks across 28 countries answered the question.

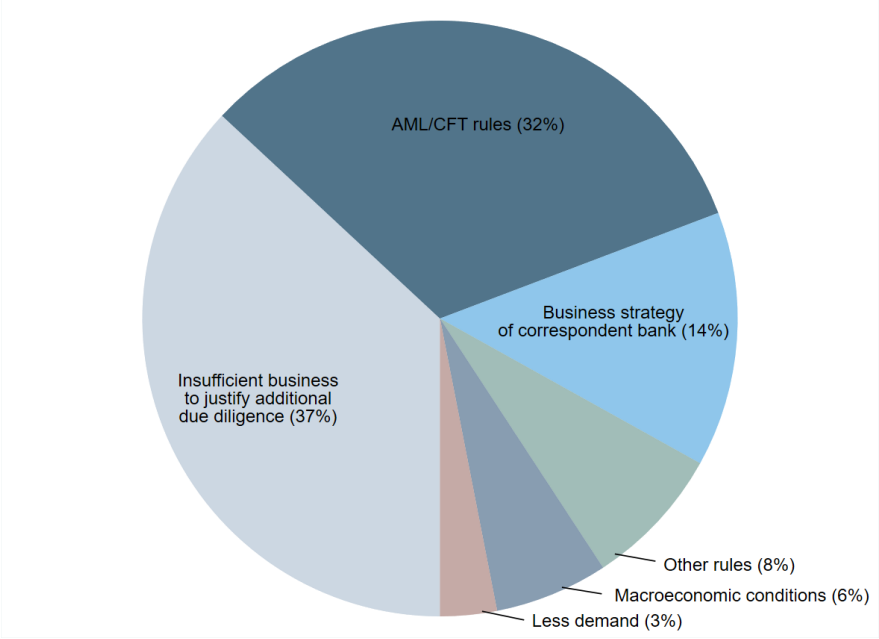


Figure 3: Restricted access to correspondent banking services

This figure shows the percentage of local respondent banks that indicated that a particular correspondent banking service was "difficult to access" or "not available at all" in a given year. Local banks responded to the question: "Please score the availability of the following types of correspondent banking services to your bank in 2013, 2015, 2017, and 2019". We asked this question as part of an online bank survey at the end of 2019. 91 banks across 28 countries answered the question.

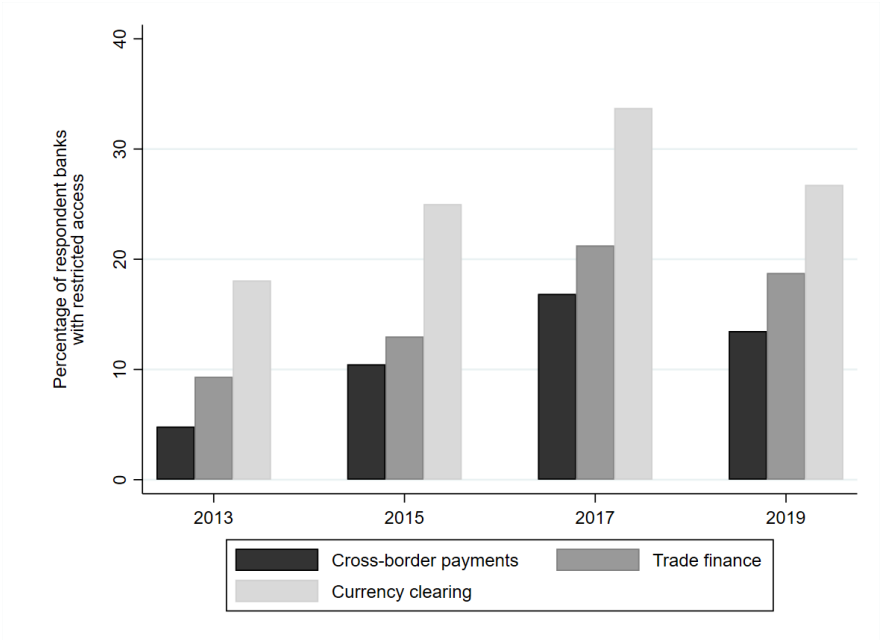


Figure 4: Terminated correspondent bank relationships and firm exports

This figure shows firms' *Export dummy* and *Exports* around the termination of one or more correspondent bank relationships. Treated (control) firms have a main bank that has (not) lost a correspondent bank relationship up to the event year. Information on firms' main bank is taken from Bureau van Dijk's Orbis 'Bankers' database. Reported coefficients are based on the Borusyak, Jaravel, and Spiess (2024) estimator. Regressions include firm controls (*Total assets* and *Total Factor Productivity*), bank controls (*Loan growth*, *Equity/Total assets*, *Loans/Customer deposits*, *ROA*), country-by-year fixed effects, and industry-by-year fixed effect. 95%-confidence intervals are based on standard errors clustered by bank. Variable definitions and sources are reported in Appendix B.

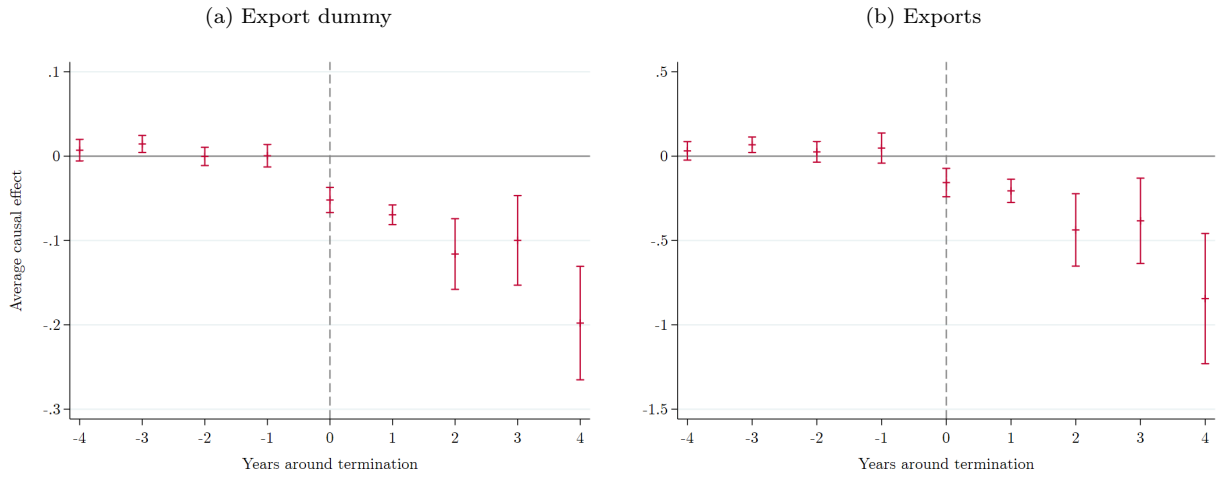


Figure 5: Terminated correspondent bank relationships and firm turnover

This figure shows firms' *Domestic revenues* and *Total revenues* around the termination of one or more correspondent bank relationships. Treated (control) firms have a main bank that has (not) lost a correspondent bank relationship up to the event year. Information on firms' main bank is taken from Bureau van Dijk's Orbis 'Bankers' database. Reported coefficients are based on the Borusyak, Jaravel, and Spiess (2024) estimator. Regressions include firm controls (*Total assets* and *Total Factor Productivity*), bank controls (*Loan growth*, *Equity/Total assets*, *Loans/Customer deposits*, *ROA*), country-by-year fixed effects, and industry-by-year fixed effect. 95%-confidence intervals are based on standard errors clustered by bank. Variable definitions and sources are reported in Appendix B.

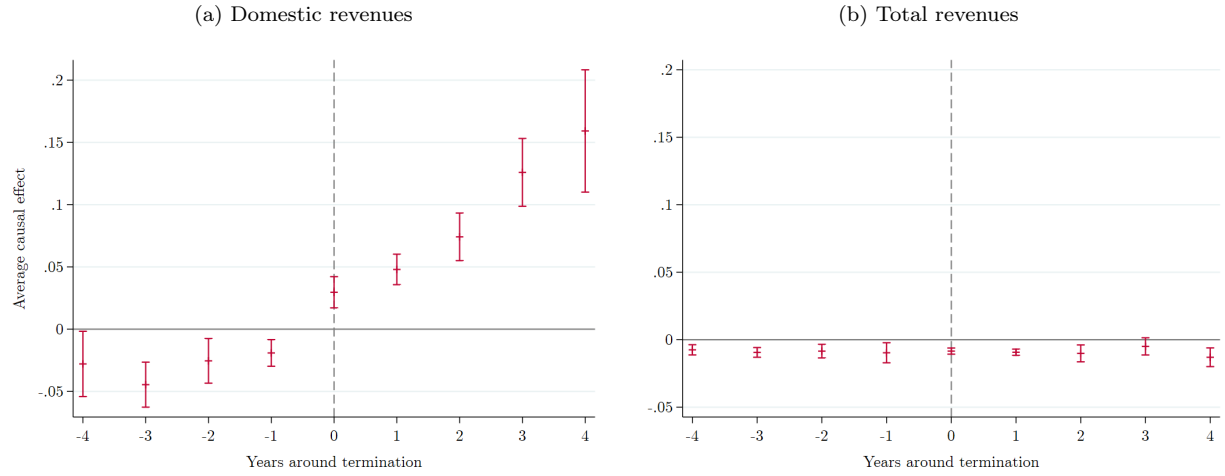


Figure 6: Terminated correspondent bank relationships, firm employment, and exit

This figure shows firms' *Employees* and probability to be *Dissolved* around the termination of one or more correspondent bank relationships. Treated (control) firms have a main bank that has (not) lost a correspondent bank relationship up to the event year. Information on firms' main bank is taken from Bureau van Dijk's Orbis 'Bankers' database. Reported coefficients are based on the Borusyak, Jaravel, and Spiess (2024) estimator. Regressions include firm controls (*Total assets* and *Total Factor Productivity*), bank controls (*Loan growth*, *Equity/Total assets*, *Loans/Customer deposits*, *ROA*), country-by-year fixed effects, and industry-by-year fixed effect. 95%-confidence intervals are based on standard errors clustered by bank. Variable definitions and sources are reported in Appendix B.

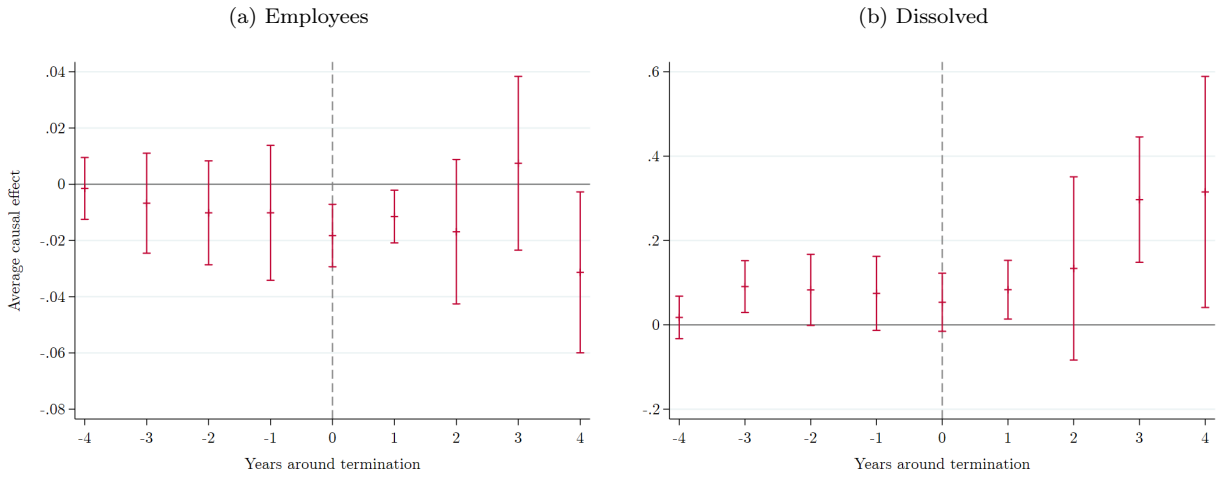
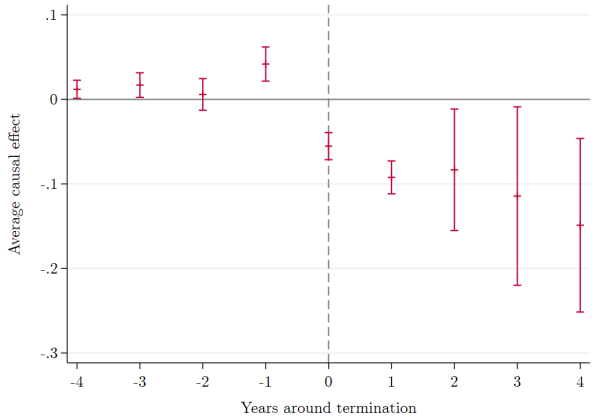


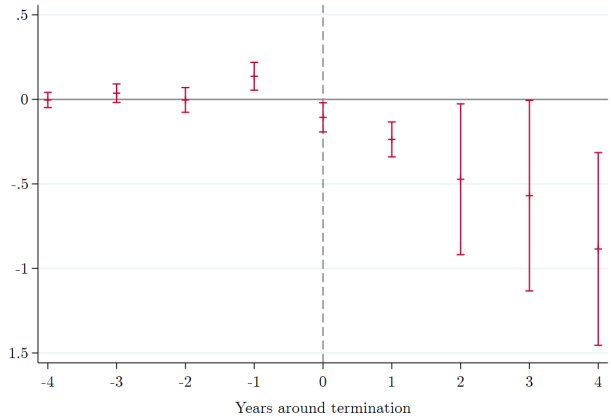
Figure 7: Terminated correspondent bank relationships: Locality-level treatment

This figure shows firm outcomes around the termination of one or more correspondent bank relationships in their locality, compared with control firms. Treated firms are located in a locality where at least one bank lost a correspondent bank relationship. Control firms are located in a locality where no bank lost a correspondent bank relationship up to the event year. Reported coefficients are based on the Borusyak, Jaravel, and Spiess (2024) estimator. The reported coefficients are from a regression including firm controls (*Total assets* and *Total Factor Productivity*), banks controls (*Local loan growth*, *Equity/Total assets*, *Loans/Customer deposits*, *ROA*), country-by-year fixed effects, and industry-by-year fixed effect. 95%-confidence intervals are based on standard errors clustered by locality. Variable definitions and sources are reported in Appendix B.

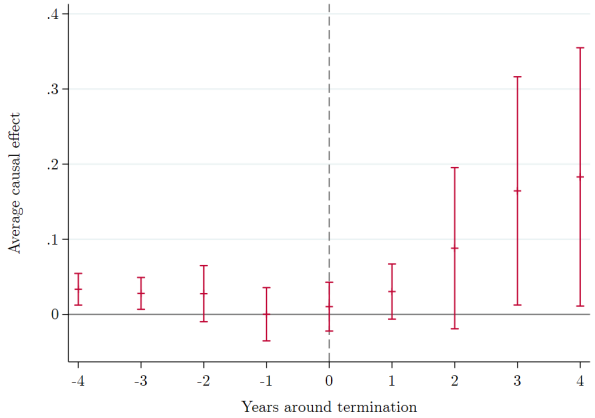
(a) Export dummy



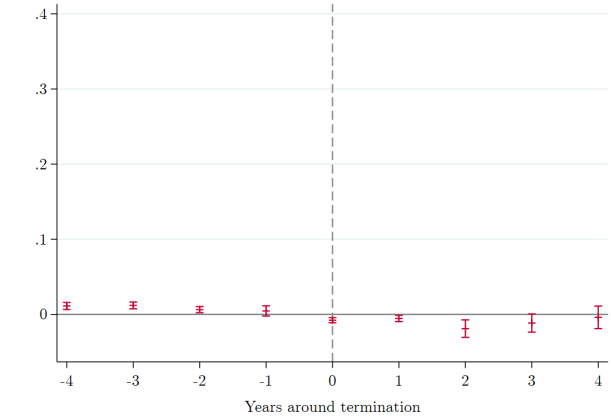
(b) Exports



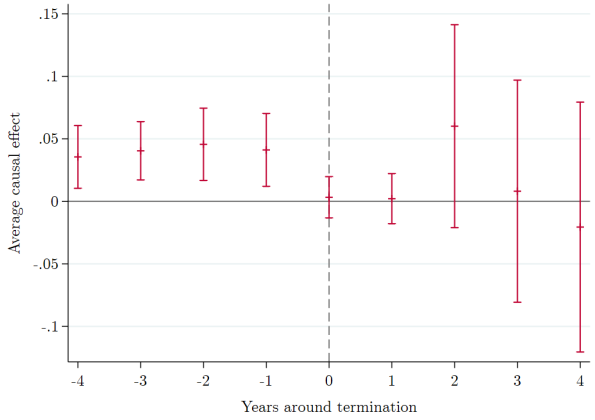
(c) Domestic revenues



(d) Total revenues



(e) Employees



(f) Dissolved

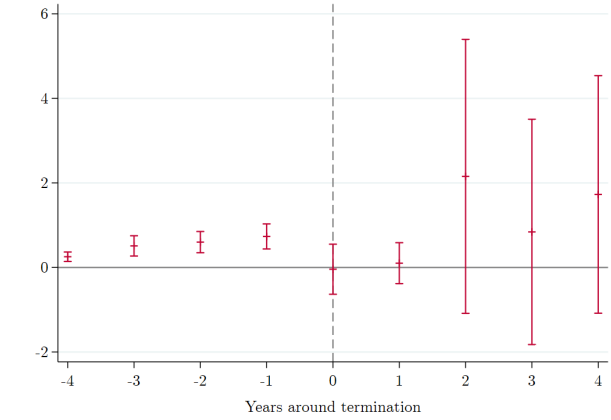


Figure 8: Heterogeneous effects for firms of different size

This figure shows firm outcomes around the termination of one or more correspondent bank relationships for the sub-sample of firms with an above-median amount of total assets (Large firms) and the sub-sample of firms with a below-median amount of total assets (Small firms). Treated (control) firms have a main bank that has (not) lost a correspondent bank relationship up to the event year. Information on firms' main bank is taken from Bureau van Dijk's Orbis 'Bankers' database. Reported coefficients are based on the Borusyak, Jaravel, and Spiess (2024) estimator. Regressions include firm controls (*Total assets* and *Total Factor Productivity*), bank controls (*Loan growth*, *Equity/Total assets*, *Loans/Customer deposits*, *ROA*), country-by-year fixed effects, and industry-by-year fixed effect. 95%-confidence intervals are based on standard errors clustered by bank. Variable definitions and sources are reported in Appendix B.

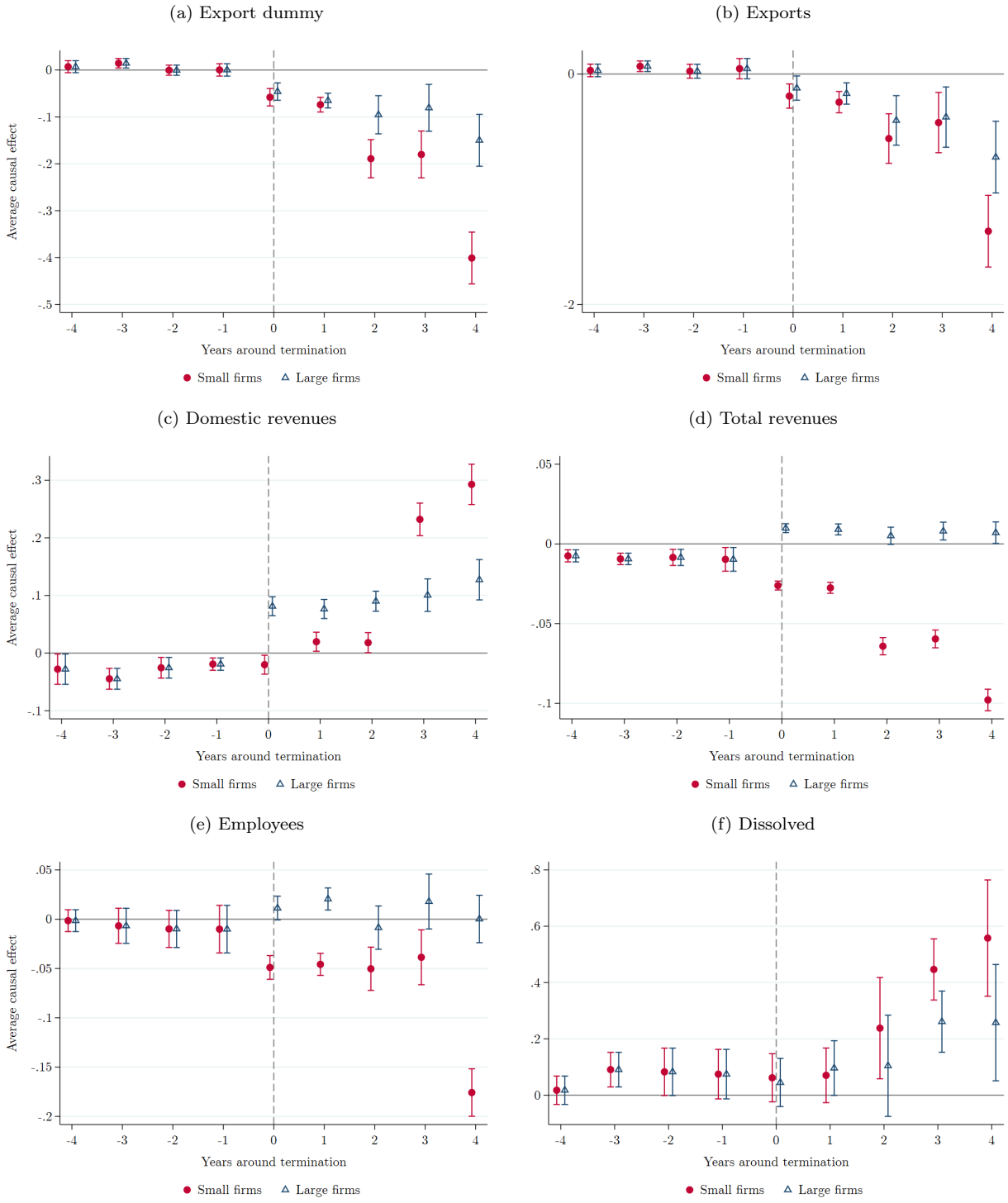


Figure 9: Heterogeneous effects for firms of different age

This figure shows firm outcomes around the termination of one or more correspondent bank relationships for the sub-sample of firms with an above-median age (Old firms) and the sub-sample of firms with a below-median age (Young firms). Treated (control) firms have a main bank that has (not) lost a correspondent bank relationship up to the event year. Information on firms' main bank is taken from Bureau van Dijk's Orbis 'Bankers' database. Reported coefficients are based on the Borusyak, Jaravel, and Spiess (2024) estimator. Regressions include firm controls (*Total assets* and *Total Factor Productivity*), bank controls (*Loan growth*, *Equity/Total assets*, *Loans/Customer deposits*, *ROA*), country-by-year fixed effects, and industry-by-year fixed effect. 95%-confidence intervals are based on standard errors clustered by bank. Variable definitions and sources are reported in Appendix B.

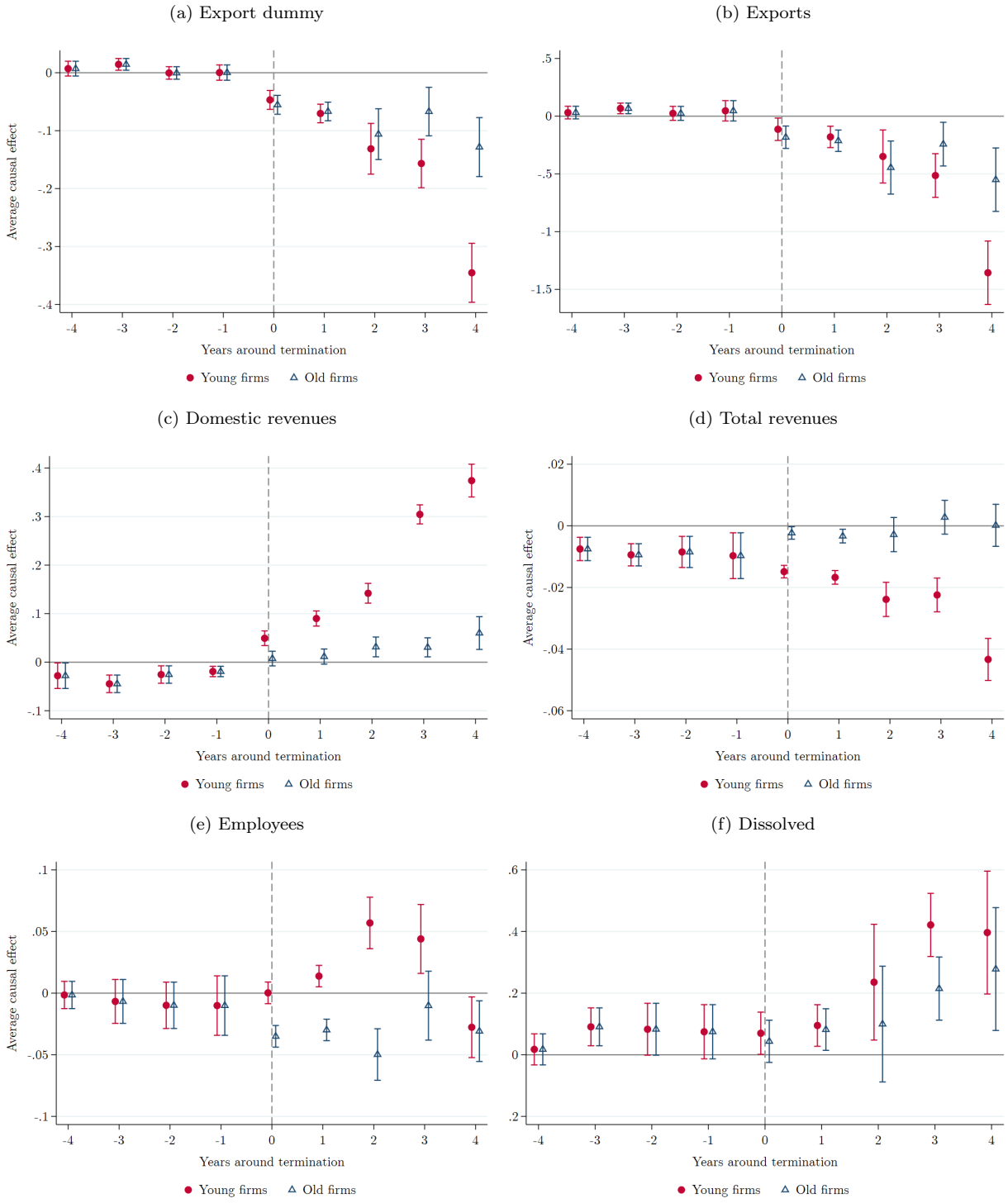


Figure 10: Heterogeneous effects for firms with different average revenue product of capital

This figure shows firm outcomes around the termination of one or more correspondent bank relationships for the sub-sample of firms with an above-median average revenue product of capital (ARPC), defined as total revenues divided by fixed assets, and the sub-sample of firms with a below-median ARPC. Treated (control) firms have a main bank that has (not) lost a correspondent bank relationship up to the event year. Information on firms' main bank is taken from Bureau van Dijk's Orbis 'Bankers' database. Reported coefficients are based on the Borusyak, Jaravel, and Spiess (2024) estimator. Regressions include firm controls (*Total assets* and *Total Factor Productivity*), bank controls (*Loan growth*, *Equity/Total assets*, *Loans/Customer deposits*, *ROA*), country-by-year fixed effects, and industry-by-year fixed effect. 95%-confidence intervals are based on standard errors clustered by bank. Variable definitions and sources are reported in Appendix B.

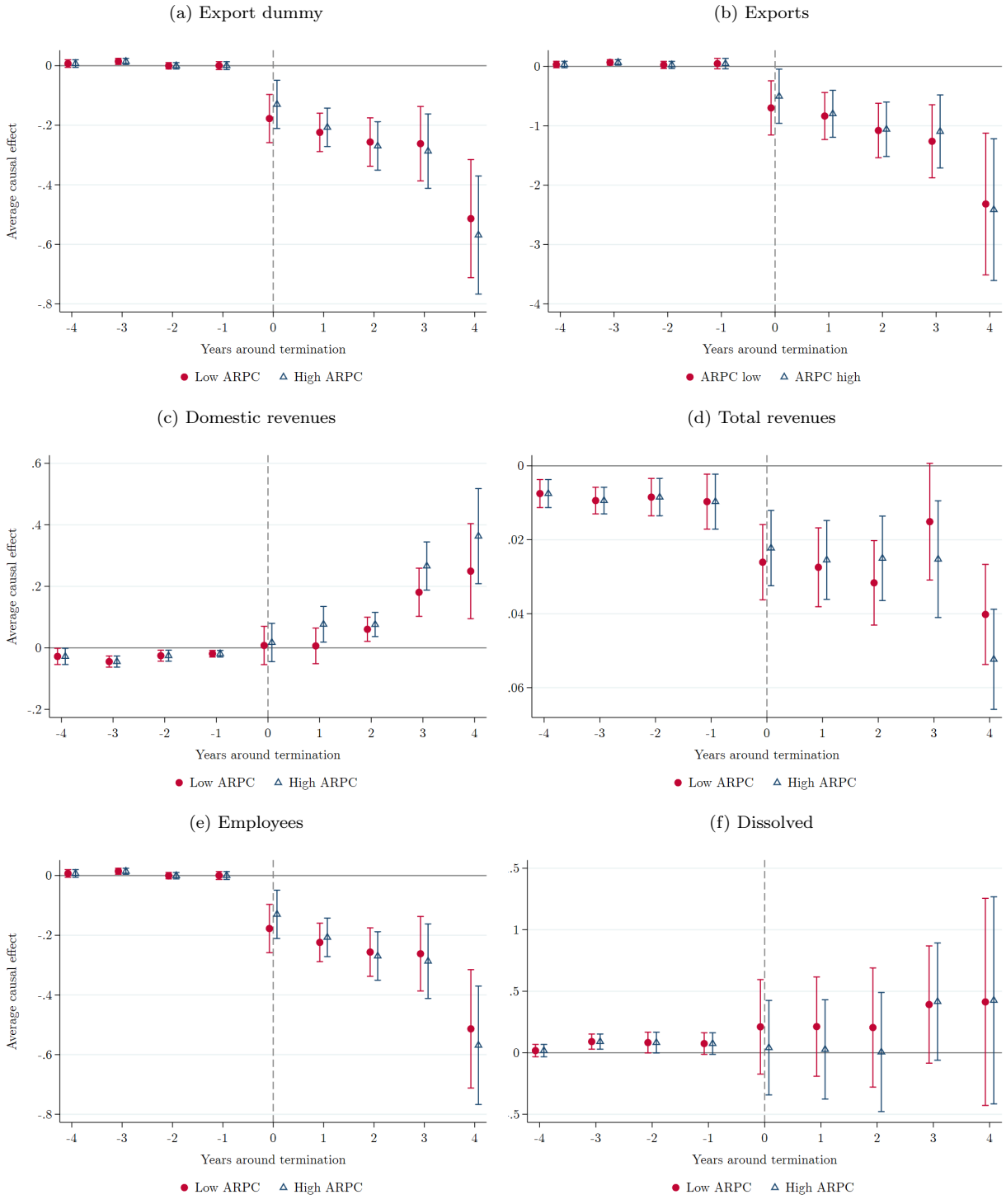


Figure 11: Heterogeneous effects for firms, by size correspondent network of main bank

This figure shows firm outcomes around the termination of one or more correspondent bank relationships for the sub-sample of firms whose main bank has an above-median number of correspondent relationships, and the sub-sample of firms with a below-median number of correspondent relationships (post withdrawal). Treated (control) firms have a main bank that has (not) lost a correspondent relationship up to the event year. Information on firms' main bank is taken from Bureau van Dijk's Orbis 'Bankers' database. Reported coefficients are based on the Borusyak, Jaravel, and Spiess (2024) estimator. Regressions include firm controls (*Total assets* and *Total Factor Productivity*), bank controls (*Loan growth*, *Equity/Total assets*, *Loans/Customer deposits*, *ROA*), country-by-year fixed effects, and industry-by-year fixed effect. 95%-confidence intervals are based on standard errors clustered by bank. Variable definitions and sources are reported in Appendix B.

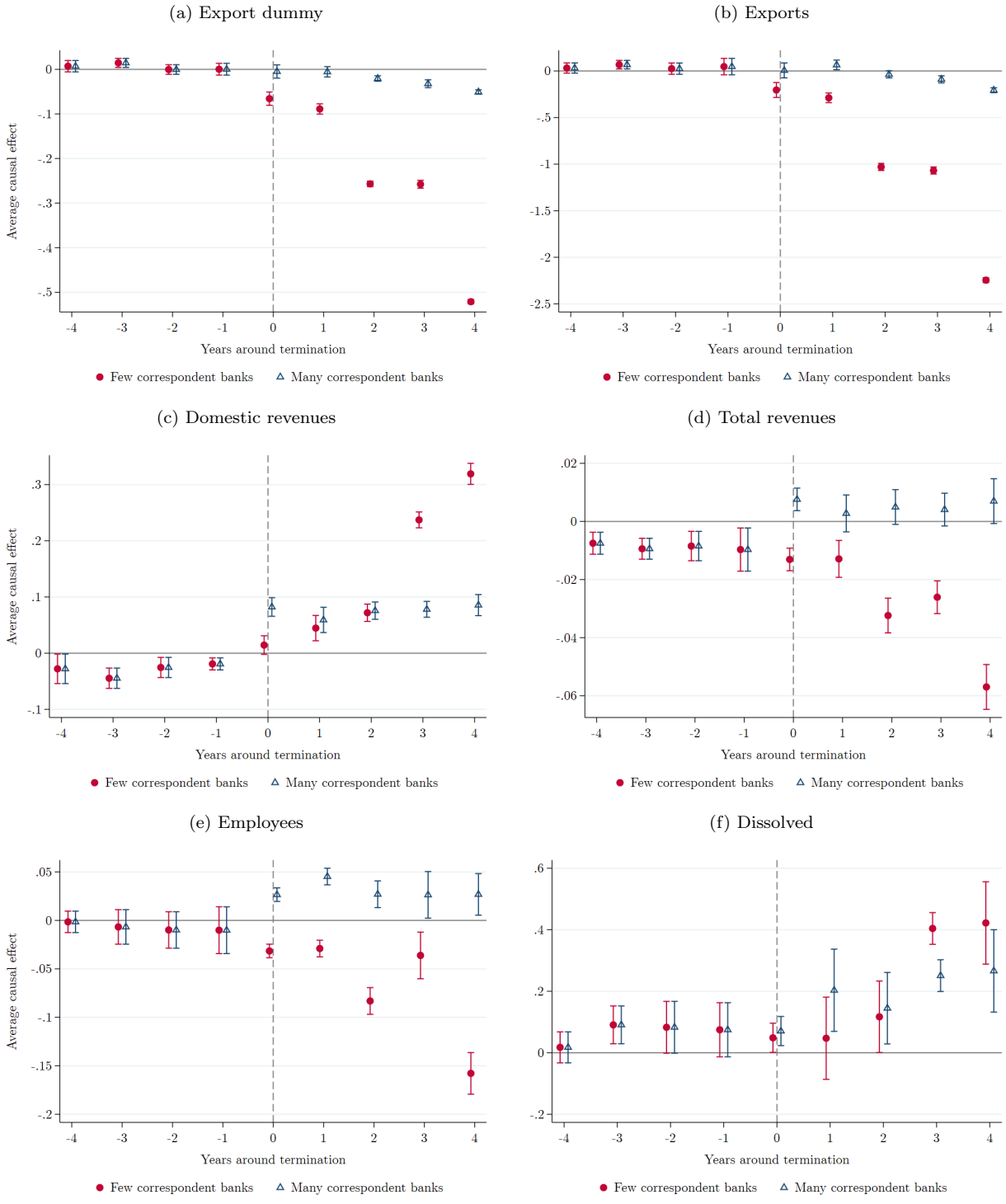
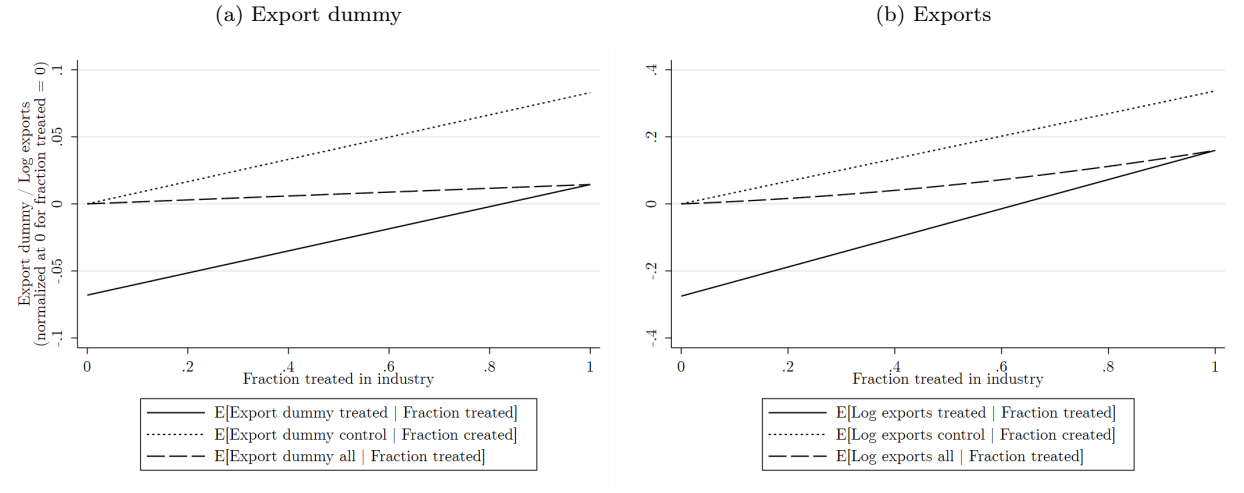


Figure 12: Industry spillovers

This figure illustrates the industry-level spillovers of the termination of correspondent banking relationships on treated and control firms. The figure plots for pre-treatment exporters *Export dummy* and *Exports* as a function of treatment intensity, i.e. the fraction of treated firms in an industry, using Equation 2. The underlying regressions are estimated using OLS. The solid line shows the spillover effects for the treated firms, while the dotted line shows the spillover effects for the control firms. The direct treatment effect is represented by the difference between treatment and control firms at a treatment fraction of zero. This indicates the impact of a decline in correspondent bank relationships if no other firms (in other localities) in the same industry would be treated. The dashed line represents the industry-level average probability to export (left Panel) and the industry-level average export turnover (right Panel) as a function of the fraction of treated firms. Variable definitions and sources are reported in Appendix B.



Tables

Table 1: Treatment-control balance in the full sample and the sample on common support

This table presents the pre-treatment characteristics of treated and control firms for the full sample of exporters (Panel A) and the sample restricted to firms on the common support in the year before treatment, which we employ in our analyses (Panel B). Treated (control) firms are those with (without) a main bank that has lost a correspondent bank relationship up to the event year. While the applied regression methods treat any firm before treatment as control firms, in this table, we summarize those control firms that have never lost a correspondent bank relationship. We report the t-statistics of the difference in normalized differences following Imbens and Wooldridge (2009). Variable definitions and sources are reported in Appendix B.

Panel A: Full sample

	Firm characteristics					**Bank characteristics**
	Exports	Total assets	TFP	N Employees	Age	Cut relationships
	(1)	(2)	(3)	(4)	(5)	(6)
Treated firms (N= 16,737)						
Mean	533	1,514	0.55	17	11	1.27
Median	0	123	0.76	3	9	1
SD	2,699	4,757	1.59	48	9	0.66
(Never-treated) control firms (N= 48,961)						
Mean	242	1,792	0.48	20	13	0
Median	0	171	0.64	7	12	0
SD	2,406	5,302	51	116	10	-
t(Difference)	11.83	-14.42	12.81	-12.09	-58.46	-
Normalized difference (Imbens-Wooldridge)	0.03	-0.04	0.03	-0.03	-0.17	-

Panel B: Sample on common support

	Firm characteristics					**Bank characteristics**
	Exports	Total assets	TFP	N Employees	Age	Cut relationships
	(1)	(2)	(3)	(4)	(5)	(6)
Treated firms (N= 7,131)						
Mean	1,207	3,338	0.26	36	12	1.15
Median	8	315	0.41	6	11	1
SD	4,824	11,675	1.12	98	9	0.51
(Never-treated) control firms (N= 19,505)						
Mean	1,001	4,584	0.283	42	14	0
Median	1.8	466	0.405	8	14	0
SD	4,417	15,461	1.08	113	11	-
t(Difference)	9.19	-17.02	-4.14	-11.07	-41.42	-
Normalized difference (Imbens-Wooldridge)	0.03	-0.06	-0.01	-0.04	-0.16	-

Table 2: Summary statistics

This table displays the firm and bank characteristics of the sample of exporters on the common support used in our analysis. Firms are matched to their main bank as specified in Bureau van Dijk's Orbis 'Bankers'. We only select treated and control firms that exported in the pre-event year and are on the common support in the pre-event year with respect to *Exports*, *Total Assets*, and *Total Factor Productivity*. Variable definitions and sources are reported in Appendix B.

	(1) Unit	(2) N	(3) Mean	(4) Median	(5) Min	(6) Max	(7) SD
Firm-variables (26,636 firms over a sample period of up to 12 years)							
Exports	1,000 Euros	266,285	1,131	3	0	37,313	4,732
Total assets	1,000 Euros	266,285	4,478	451	2	119,979	14,963
Total Factor Productivity		266,285	0.3	0.4	-10.9	9.1	1.1
Employees	N	248,399	42	8	1	820	112
Dissolved	%	266,285	0.18	0	0	100	4.25
Age	Years	253,206	14	13	0	198	11
Bank-variables (19 banks)							
Total assets	Mill. Euros	266,285	11,455	8,849	0	63,953	11,956
Equity/Total assets	%	266,285	13.5	13.4	8.9	18.2	2.3
Loans/Customer deposits	%	266,285	79.8	79.6	49.0	104.3	12.1
ROA	%	266,285	0.8	1.1	-6.9	2.4	1.3
Loan growth	%	266,285	13.47	0.00	-20.98	802.53	99.51

Table 3: Terminated correspondent bank relationships and firm-level outcomes: Static effects

This table shows static Borusyak, Jaravel, and Spiess (2024) estimates (top), static de Chaisemartin and D'Haultfœuille (2024) estimates (middle) and static TWFE OLS estimates (bottom) for firms' *Export dummy* and *Exports*, *Domestic revenues*, *Turnover*, *Employees*, and next year's *Dissolved* around the termination of one or more correspondent bank relationships. Treated (control) firms have a main bank that has (not) lost a correspondent bank relationship up to the event year. Information on firms' main bank is taken from Bureau van Dijk's Orbis 'Bankers' database. We only use treated and control firms that are on the common support in the pre-event year with respect to *Exports*, *Total assets* and *Total Factor Productivity*. Regressions include firm controls (*Total assets* and *Total Factor Productivity*), bank controls (*Loan growth*, *Equity/Total assets*, *Loans/Customer deposits*, *ROA*), country-by-year fixed effects, and industry-by-year fixed effect. Standard errors are clustered at the bank level and shown in parentheses. Note that the number of firm-years used to estimate the treatment effect by de Chaisemartin and D'Haultfœuille (2024) is smaller than the number of firm-years reported for the OLS estimator and for Borusyak, Jaravel, and Spiess (2024). As the de Chaisemartin and D'Haultfœuille (2024) estimator is based on valid first-differences between treated and control firms (see Section 4.3) it only includes the subset of firms that are treated and have at least one valid control or that are valid controls of a treated firm in the sample.

	Exports		Revenues		Employees	Dissolved
	Dummy	Amount	Domestic	Total		in %
	(1)	(2)	(3)	(4)	(5)	(6)
Borusyak, Jaravel, and Spiess (2024)	-0.111***	-0.415***	0.082***	-0.012***	-0.025***	0.422***
	(0.018)	(0.099)	(0.012)	(0.002)	(0.009)	(0.155)
Firm-years	267,451	267,451	267,169	267,451	267,451	267,451
de Chaisemartin and D'Haultfœuille (2024)	-0.028***	-0.085***	0.022*	-0.002***	-0.007	0.138
	(0.009)	(0.033)	(0.012)	(0.001)	(0.005)	(0.111)
Firm-years	25,248	25,248	25,186	25,248	23,747	25,248
Switchers	6,161	6,161	6,141	6,161	5,751	6,161
OLS	-0.050***	-0.158**	0.041***	-0.006	-0.009	0.332**
	(0.014)	(0.065)	(0.014)	(0.004)	(0.013)	(0.144)
Firm-years	257,386	257,386	257,103	257,386	239,907	257,386
Firm and bank controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE / Nonparametric industry trends	Yes	Yes	Yes	Yes	Yes	Yes
Country-year FE / Nonparametric country trends	Yes	Yes	Yes	Yes	Yes	Yes
Pre-event mean of treated in t=-1	1.00	3.26	5.63	6.21	2.35	0.00
Mean of not-yet and never treated	0.56	2.62	5.80	6.25	2.45	0.47

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Terminated correspondent bank relationships and sector-level trade

This table reports standard difference-in-differences regressions where the dependent variable is the first difference in log exports or log imports of industry s in country i in emerging Europe to or from country j in the rest of the world at time t. The dependent variable is winsorized at the 5th and 95th percentiles. Sector-level exports and imports are aggregated by pre-period observation [July 2013–June 2014] and post-period observation [July 2014–June 2015], and first differences are calculated relative to aggregate exports in the preceding 12 months, respectively. The withdrawal of correspondent banks is measured as the percent change in active correspondent banks (Financial Stability Board 2017). Countries are assigned to the treatment group (*High Withdrawal*) if the country-specific withdrawal of correspondent banks is higher than the median withdrawal in the sample. *Post* is a dummy variable that takes value 0 if t = [July 2013–June 2014] and value 1 if t = [July 2014–June 2015]. Regressions include country-level controls ($\Delta LogExports$ *(World)* or $\Delta LogImports$ *(World)*; *Log GDP Counterparty*, *Log Distance*, *Prop. USD Exports* x $\Delta LogUSD/EUR$). Standard errors are clustered at the country-partner level and shown in parentheses. Variable definitions and sources are reported in Appendix B.

	Δ Log Exports			Δ Log Imports		
	(1)	(2)	(3)	(4)	(5)	(6)
High Withdrawal × Post	-0.069**	-0.070**	-0.083**	-0.170***	-0.150***	-0.239***
	(0.031)	(0.032)	(0.032)	(0.026)	(0.027)	(0.027)
Post	-0.090***	-0.088***		-0.002	-0.080***	
	(0.018)	(0.038)		(0.018)	(0.038)	
Δ Log Exports (World)/						
Δ Log Imports (World)	1.275***	1.289***		1.156***	1.166***	
	(0.022)	(0.022)		(0.021)	(0.022)	
Log GDP Counterparty	0.018***	0.018***		0.009***	0.009***	
	(0.003)	(0.003)		(0.003)	(0.003)	
Log Distance	0.002	0.005		-0.020***	-0.019***	
	(0.005)	(0.005)		(0.005)	(0.005)	
Prop. USD Exports						
× Δ Log USD/EUR		0.187			-9.130**	
		(4.194)			(4.053)	
Observations	51,446	51,446	55,773	52,979	52,979	57,332
Industry FEs	No	Yes	No	No	Yes	No
Industry × Year FEs	No	No	Yes	No	No	Yes
Country FEs	Yes	Yes	No	Yes	Yes	No
Partner × Year FEs	No	No	Yes	No	No	Yes
Pair FEs	No	No	Yes	No	No	Yes
Pre-event mean	0.15	0.15	0.15	0.08	0.08	0.08

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Online Appendix for

Broken Relationships: De-Risking by Correspondent Banks and International Trade

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A Changes in correspondent bank relationships

Table A1: Changes in correspondent bank relationships, 2011–2022

This table shows the percentage change in correspondent bank relationships (measured as counterparties abroad) between 2011 and 2022 for all countries in emerging Europe and Central Asia for which data is available. Source: SWIFT data from the Bank for International Settlements (BIS).

Country	Change in correspondent bank relationships
Albania	-42.6%
Armenia	-20.8%
Azerbaijan	-33.1%
Belarus	-46.9%
Bosnia & Herzegovina	-41.0%
Bulgaria	-30.6%
Croatia	-28.0%
Czech Republic	-29.6%
Estonia	-40.1%
Georgia	12.0%
Hungary	-31.5%
Kazakhstan	-29.8%
Kyrgyzstan	-18.9%
Latvia	-51.4%
Lithuania	-43.0%
Macedonia	-53.4%
Moldova	-59.2%
Montenegro	-39.6%
Poland	-27.1%
Romania	-26.7%
Russia	-33.4%
Serbia	-39.0%
Slovakia	-39.2%
Slovenia	-36.6%
Tajikistan	-45.3%
Turkey	-17.0%
Turkmenistan	-35.1%
Ukraine	-54.1%
Uzbekistan	-5.4%
Average	**-34.0%**

B Variable definitions and sources

Table B1: Variable definitions and sources

Variable	Definition	Source
	Panel A: Firm level	
Export dummy	Dummy=1 if firm exports in a given year; 0 otherwise	Orbis
Exports	Export revenues in log 1,000 euros	Orbis
Total revenues	Total operating revenues in log 1,000 euros	Orbis
Dissolved	Dummy=1 if firm is dissolved (but not acquired) in the following year; 0 otherwise	Orbis
Domestic revenues	Domestic sales in log 1,000 euros	Orbis
Employees	Log number of employees	Orbis
Total assets	Total assets in log 1,000 euros	Orbis
Total Factor Productivity	Industry-adjusted residual of a two-factor Cobb-Douglas production function. The input factors are log number of employees and log total assets. Output is log total revenues	Own calculation based on Orbis
House bank	Main bank named by firm	Orbis Bankers
Locality	Village, town, or city where firm is incorporated	Orbis
Firm age	Firm age in years	Orbis
ARPC	Average revenue product of capital, measured as total revenues/fixed assets	Orbis
Industry	NACE Rev. 2 classification	Orbis
Number of banks	Number of bank relationships given in Orbis	Orbis Bankers
	Panel B: Bank level	
Lost relationship	Dummy=1 if at least one bank branch in locality has lost a correspondent relationship up to year t; 0 otherwise	BEPS III; EBRD TFP survey
Cut relationships	Number of terminated correspondent relationships in a locality up to year t (on bank branch level) divided by total number of branches in locality	BEPS III and EBRD TFP survey
Loan growth	Percentage annual change in gross lending	Orbis BankFocus
Equity/Total Assets	Bank equity divided by total bank assets	Orbis BankFocus
Loans/Customer Deposits	Net bank loans divided by a bank's customer deposits and short-term funding	Orbis BankFocus
ROA	Return on assets calculated as net income divided by total assets	Orbis BankFocus
Total assets	Total bank assets in million euros	Orbis BankFocus

Panel D: Country level

Post	Dummy=0 in the pre-period July 2013-June 2014 and 1 in the post-period July 2013–June 2014	
High Withdrawal	Dummy=1 if country faces above median reduction in number of correspondent banks during Jan. 2011–Jun. 2016, 0 otherwise	Financial Stability Board 2017
Log GDP Counterparty	Country-level log gross domestic product of trading counterparty (bn. USD)	Worldbank
Log Distance	Log distance to trading partner (km)	Kristian Skrede Gleditsch's website
Δ *log USD/EUR*	First differences in log USD-EUR exchange rate (exchange rates from end-June in 2013–2015)	European Central Bank
Prop. USD Exports	Proportion of exports from the ECA region to the Americas relative to all exports from the ECA region in the period July 2012–June 2013	UN Comtrade
Prop. USD Imports	Proportion of imports to the ECA region from the Americas relative to all exports to the ECA region in the period July 2012–June 2013	UN Comtrade

Panel E: Bilateral country industry level

Δ *Log Exports*	First differences of log exports in period July *t-1* to June *t* relative to period July *t-2* to June *t-1* (t=2014, 2015)	UN Comtrade
Δ *Log Imports*	First differences of log imports in period July *t-1* to June *t* relative to period July *t-2* to June *t-1* (t=2014, 2015)	UN Comtrade
Δ *Log Exports (World)*	First differences of log global exports in period July *t-1* to June *t* relative to period July *t-2* to June *t-1* (t=2014, 2015)	UN Comtrade
Δ *Log Imports (World)*	First differences of log global imports in period July *t-1* to June *t* relative to period July *t-2* to June *t-1* (t=2014, 2015)	UN Comtrade

C Survey questions

This Appendix reports the questions that respondent banks were asked in the third round of the EBRD Banking Environment and Performance Survey (BEPS) in 2021 and in the survey we conducted with partner banks of the EBRD Trade Facilitation Programme (TFP) in 2019.

EBRD BEPS III

- **H43**: Over the past decade, some major international correspondent banks have terminated relationships with respondent banks. Has any bank terminated its correspondent banking relationship with your bank since 2008?
  - Yes
  - No
  - Don't know
- **H44**: Please state the year of termination, the bank's name, and its country of origin. [Several mentions possible]
  - Year of termination
  - Bank name
  - Country

Survey with partner banks of the EBRD Trade Facilitation Programme (TFP)

- **Question 3**: Has any foreign correspondent bank terminated the relationship with your bank after 2008?
- **Question 4**: Which bank or which banks have terminated their correspondent banking relationship with your bank after 2008 and in which year was the relationship terminated?
- **Question 5**: Please score the availability of the following three different types of correspondent banking services to your bank in 2013, 2015, 2017, and the year 2019. [Respondents select between "Not available", "Difficult to access", "Easy to access", "Not relevant"]
  - Payment Transactions
  - Currency Clearing
  - Trade Finance
- **Question 6**: Please score the availability of correspondent banking services in different currencies to your bank in 2013, 2015, 2017, and the year 2019. [Respondents select between "Not available", "Difficult to access", "Easy to access", "Not relevant"]

  - US Dollar
  - Euro
  - Ruble

- **Question 10**: What do you consider the most likely reasons that foreign correspondent banks have decided to terminate or restrict their correspondent banking relationship with your bank/with other banks?

  - The correspondent banking relationship does not generate sufficient business to justify the cost of additional customer due diligence.
  - Foreign correspondent banks have reacted to the stricter enforcement of AML/CFT Anti-Money Laundering/Combating the Financing of Terrorism regulations.
  - Foreign correspondent banks have reacted to regulations unrelated to AML/CFT Anti-Money Laundering/Combating the Financing of Terrorism.
  - Foreign correspondent banks have reacted to changed macroeconomic conditions.
  - Foreign correspondent banks have terminated relationships with local banks because correspondent banks have changed their business strategy or have gone through structural changes (including mergers and industry consolidation).
  - Local respondent banks have less demand for correspondent banking services as compared to previous years.

- **Question 11**: Out of all relevant causes for terminating your/others' correspondent banking relationship, which do you consider most important?

  - The correspondent banking relationship does not generate sufficient business to justify the cost of additional customer due diligence.
  - Foreign correspondent banks have reacted to the stricter enforcement of AML/CFT regulations.
  - Foreign correspondent banks have reacted to regulations unrelated to AML/CFT.
  - Foreign correspondent banks have reacted to changed macroeconomic conditions.
  - Foreign correspondent banks have terminated relationships with local banks because correspondent banks have changed their business strategy or have gone through structural changes (including mergers and industry consolidation).
  - Local respondent banks have less demand for correspondent banking services as compared to previous years.

D Orthogonality

Table D1: Treatment status explained by firm, bank, and locality variables

Columns (1) and (2) of this table report OLS regressions of our treatment dummy (i.e. whether a firm's bank lost one or several correspondent bank relationships over the sample period) on various firm, bank, and locality characteristics. Variables are in levels and averaged over years. In column (3), firm and bank variables are averaged at the locality level. Standard errors are clustered at the country level and shown in parentheses.

	Treated dummy (firm-level)		Fraction of treated firms in city
	(1)	(2)	(3)
Firm characteristics (averaged over years)			
Total assets	-0.001	0.000	-0.006
	(0.001)	(0.001)	(0.002)
Productivity	-0.009	-0.005	-0.025
	(0.015)	(0.007)	(0.038)
Total revenues	0.009	0.002	0.007
	(0.008)	(0.003)	(0.007)
Employees	-0.003	-0.004	0.010
	(0.004)	(0.005)	(0.006)
Bank characteristics (averaged over years)			
Total assets	-0.186**	-0.226***	-0.199*
	(0.032)	(0.026)	(0.080)
Equity/Total assets	0.075*	0.093***	-0.040
	(0.026)	(0.008)	(0.129)
ROA	0.030	0.030	0.151
	(0.113)	(0.134)	(0.073)
Loans/Deposits	-0.251	-0.253*	-0.095
	(0.110)	(0.105)	(0.048)
Loan growth	-0.134*	-0.137	-0.061
	(0.055)	(0.073)	(0.119)
Locality characteristics (averaged over years)			
Number of firms	-0.017		-0.009
	(0.049)		(0.009)
Number of bank branches	0.001		0.025
	(0.020)		(0.012)
HHI firms	0.054		0.048
	(0.028)		(0.021)
HHI banks	-0.044		-0.093**
	(0.026)		(0.027)
Observations	20,511	25,561	515
City FEs	No	Yes	No
Adjusted R^2	0.44	0.54	0.55
p(Wald test)	0.007	0.000	0.048

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

E Heterogeneity of treatment effects

de Chaisemartin and D'Haultfœuille (2020) and Borusyak, Jaravel, and Spiess (2024) show that heterogeneous and dynamic treatment effects can bias the estimates of a conventional TWFE model. This Appendix first presents tests based on de Chaisemartin and D'Haultfœuille (2020) indicating that heterogeneous treatment effects may indeed be present in our setting. We then discuss how our estimator choice addresses this issue.

E.1 Risk of negative weights using TWFE in our setting

de Chaisemartin and D'Haultfœuille (2020) show that ATEs might be incorrectly estimated in linear regressions with period and group fixed effects. In particular, the estimated coefficient can have a different sign than all ATEs as the linear regression coefficient is (i) a weighted sum of ATEs in each group and period and (ii) weights of this sum may be negative.

Table E1 reports the sum of negative and positive weights in our baseline specifications that regress different firm-level outcomes on our treatment indicator, control variables, and firm and year fixed effects. We do this separately for our sample where we link firms to their main bank (Panel A) and the sample where treatment is defined at the locality level. By construction, the sum of weights is the same for different dependent variables when the exact same sample is used in the regression.

The presence of negative weights in both samples shows that treatment heterogeneity is an issue. For the sample where firms are linked to their main bank, this concern is minor, given the tiny share of negative weights of less than 1 percent. However, in the locality-level sample, negative weights sum up to more than 19%, implying that treatment effects of several treated groups and periods enter negatively in the linear estimator. The next subsection explains how we preempt any potential problems arising from these negative weights.

E.2 Choice of estimator

There are several estimators accounting for problems with heterogeneous effects and negative weights in event studies with staggered treatment, in particular those suggested by de Chaisemartin and D'Haultfœuille (2020), de Chaisemartin and D'Haultfœuille (2024), Borusyak, Jaravel, and Spiess (2024), Callaway and Sant'Anna (2021), and Sun and Abraham (2021). We use the imputation estimator by Borusyak, Jaravel, and Spiess (2024) as our main estimator as it accounts for both heterogeneous and dynamic treatment effects and has been shown to be the best unbiased estimator under spherical errors (Borusyak, Jaravel, and Spiess 2024; Harmon 2023). Harmon (2023) finds that approaches based on subgroup DiD, such as de Chaisemartin and D'Haultfœuille (2024), perform better than imputation approaches when errors are strongly correlated. We therefore replicate all our results using that estimator in Appendix F.

Table E1: Heterogeneity of treatment effects

This table shows the sum of positive and negative weights as well as the values for $\underline{\hat{\sigma}}$ and $\underline{\hat{\sigma}}$ of Corollary 1 in de Chaisemartin and D'Haultfœuille (2020). The numbers are based on TWFE regressions of our dependent variables *Export dummy*, *Exports*, *Turnover*, *Domestic turnover*, *Employees*, and *Dissolved* around the termination of one or more correspondent bank relationships. We report the weights for our two main samples for which treatment is defined at different levels. Panel (a) reports weights for the sample in which treated (control) firms have a main bank that has (not) lost a correspondent bank relationship up to the event year. Panel (b) reports weights for the sample in which treated firms are located in a locality in which at least one bank lost a correspondent relationship and control firms are located in a locality where no bank lost a correspondent relationship up to the event year. Information on firms' main bank comes from Bureau van Dijk's Orbis 'Bankers' database. Regressions include firm controls (*Total assets* and *Total Factor Productivity*), bank controls (*Loan growth*, *Equity/Total assets*, *Loans/Customer deposits*, *ROA*). All variables are defined in Appendix B. Note that weights will be the same for different dependent variables whenever the exact same sample is used in the regression.

Panel A: Treatment at firm-level

Dependent variable	Sum of positive weights	Sum of negative weights
Export dummy	1.0002	-0.0002
Exports	1.0002	-0.0002
Domestic revenues	1.0002	-0.0002
Total revenues	1.0002	-0.0002
Employees	1.0002	-0.0003
Dissolution	1.0002	-0.0002

Panel B: Treatment at city-level

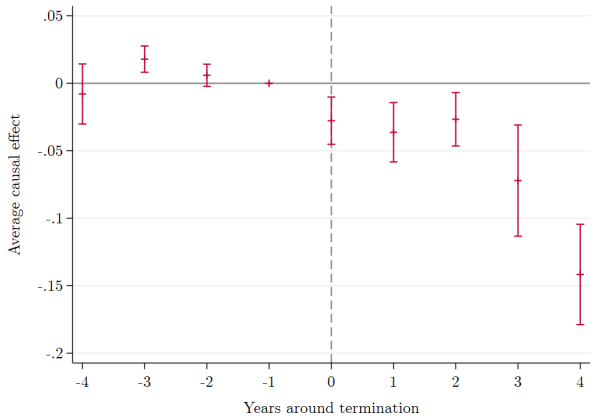
Dependent variable	Sum of positive weights	Sum of negative weights
Export dummy	1.1929	-0.1929
Exports	1.1929	-0.1929
Domestic revenues	1.1929	-0.1929
Total revenues	1.1929	-0.1929
Employees	1.1956	-0.1956
Dissolution	1.1929	-0.1929

F Robustness checks and miscellaneous

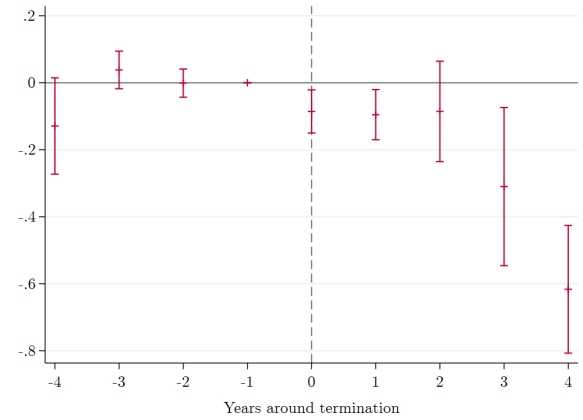
Figure F1: Terminated correspondent bank relationships and firm outcomes: de Chaisemartin and D'Haultfœuille (2024)

This figure shows different firm outcomes around the termination of one or more correspondent bank relationships. Treated (control) firms have a main bank that has (not) lost a correspondent bank relationship up to the event year. Information on firms' main bank is taken from Bureau van Dijk's Orbis 'Bankers' database. Reported coefficients are based on the de Chaisemartin and D'Haultfœuille (2024) estimator. Regressions include firm controls (*Total assets* and *Total Factor Productivity*), locality-average bank controls (*Loan growth*, *Equity/Total assets*, *Loans/Customer deposits*, *ROA*), and controlling for non-parametric country and industry trends. 95%-confidence intervals are based on standard errors clustered by bank. Variable definitions and sources are reported in Appendix B.

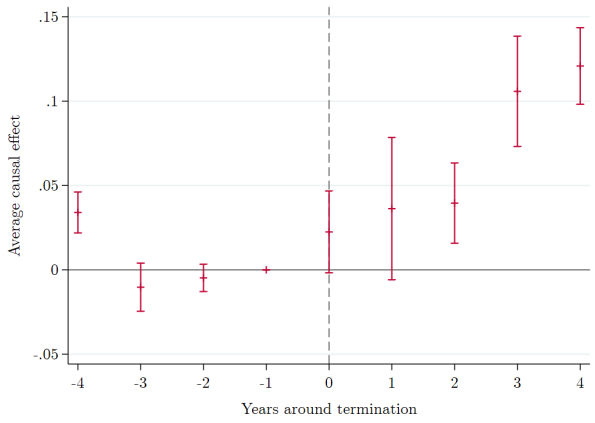
(a) Export dummy



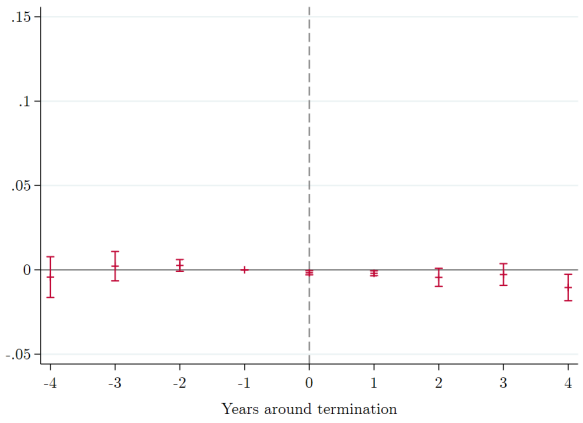
(b) Log export revenues



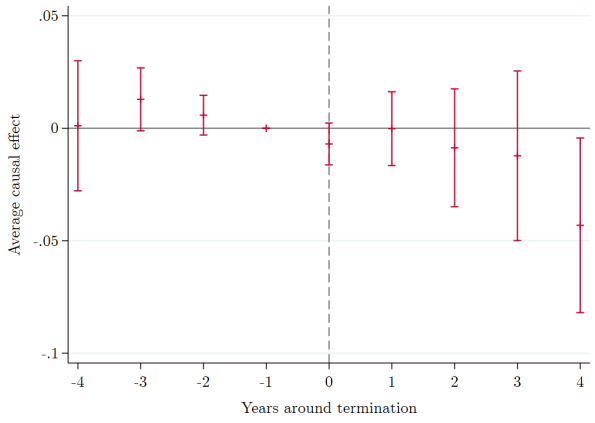
(c) Log domestic revenues



(d) Log total revenues



(e) Log number of employees



(f) Dissolved in subsequent year

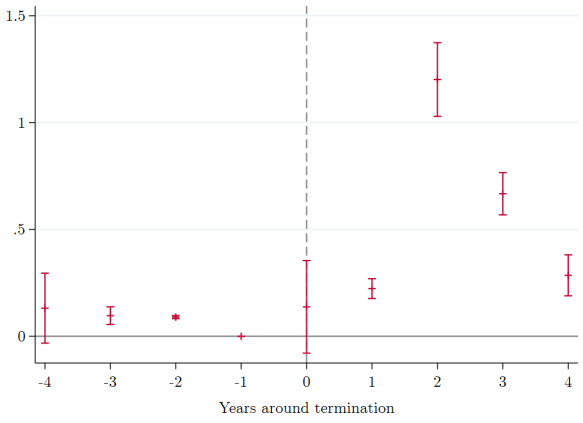


Figure F2: Terminated correspondent bank relationships and firm-level outcomes: Locality-level treatment, de Chaisemartin and D'Haultfœuille (2024)

This figure shows firm outcomes around the termination of one or more correspondent bank relationships in their locality, compared with control firms. Treated firms are located in a locality in which at least one bank lost a correspondent bank relationship. Control firms are located in a locality where no bank lost a correspondent bank relationship up to the event year. Reported coefficients are based on the de Chaisemartin and D'Haultfœuille (2024) estimator. The reported coefficients are from a regression including firm controls (*Total assets* and *Total Factor Productivity*), banks controls (*Local loan growth*, *Equity/Total assets*, *Loans/Customer deposits*, *ROA*), and controlling for non-parametric country and industry trends. 95%-confidence intervals are based on standard errors clustered by locality. Variable definitions and sources are reported in Appendix B.

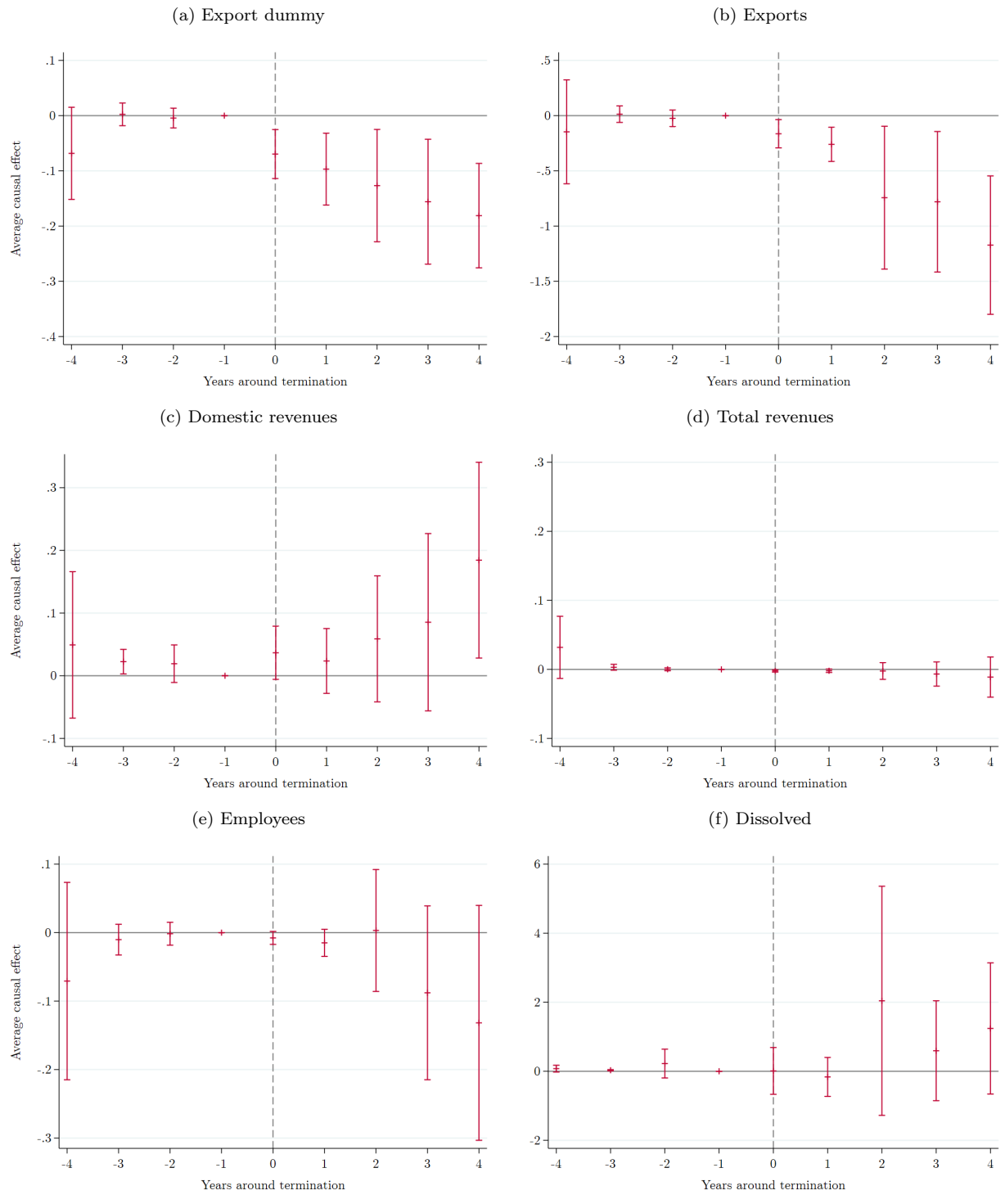


Table F1: Summary statistics locality-level treatment

This table displays the firm and bank characteristics of the sample of exporters on the common support used in our analysis based on the locality-level sample. We match each firm to all bank branches in its locality. We only select treated and control firms that exported in the pre-event year and are on the common support in the pre-event year with respect to *Exports*, *Total Assets*, and *Total Factor Productivity*. Variable definitions and sources are reported in Appendix B.

	(1) Unit	(2) N	(3) Mean	(4) Median	(5) Min	(6) Max	(7) SD
Firm-variables (39,522 firms over a sample period of up to 10 years)							
Exports	1,000 Euros	333,802	1,050	3.8	0.0	40,313	4,709
Total assets	1,000 Euros	333,802	3,702	308	1.4	113,600	13,370
Total Factor Productivity		333,802	0.5	0.6	-11.8	9.5	1.2
Employees	N	306,145	36.4	6.0	1.0	815.0	104.3
Dissolved	%	333,802	2.410	0	0	100	15.33
Age	Years	316,584	13.6	13.0	0.0	323.0	10.5
Bank-variables (averaged at the locality level, 582 localities)							
Total assets	Mill. Euros	333,802	5,917	5,029	0.0	23,188	3,611
Equity/Total assets	%	333,802	11.9	12.0	8.5	18.2	1.6
Loans/Customer deposits	%	333,802	77.5	76.9	55.8	110.6	8.1
ROA	%	333,802	0.3	0.5	-2.1	1.6	0.8
Local loan growth	%	333,802	2.06	0.474	-9.19	32.24	6.90

Table F2: Terminated correspondent bank relationships and firm-level outcomes: Static effects, locality-level treatment

This table shows static Borusyak, Jaravel, and Spiess (2024) estimates (top), static de Chaisemartin and D'Haultfœuille (2024) estimates (middle) and static TWFE OLS estimates (bottom) for firm outcomes around the termination of of one or more correspondent bank relationships in a firm's locality, compared to unaffected control firms. Treated firms are located in a locality in which at least one bank branch lost a correspondent relationship. Control firms are located in a locality where no bank lost a correspondent bank relationship up to the event year. We only use treated and control firms that are on the common support at the pre-event year with respect to *Exports*, *Total assets* and *Total Factor Productivity*. Regressions include firm controls (*Total assets* and *Total Factor Productivity*), locality-average bank controls (*Local loan growth*, *Equity/Total assets*, *Loans/Customer deposits*, *ROA*), country-by-year fixed effects, and industry-by-year fixed effect. Standard errors are clustered at the bank level and shown in parentheses. Note that the number of firm-years used to estimate the treatment effect by de Chaisemartin and D'Haultfœuille (2024) is smaller than the number of firm-years reported for the OLS estimator and for Borusyak, Jaravel, and Spiess (2024). As the de Chaisemartin and D'Haultfœuille (2024) estimator is based on valid first-differences between treated and control firms (see Section 4.3) it only includes the subset of firms that are treated and have at least one valid control or that are valid controls of a treated firm in the sample. Standard errors are clustered by locality and given in parenthesis. Variable definitions and sources are reported in Appendix B.

	Exports				Revenues		Employees	Dissolved
	Dummy		Amount		Domestic	Total		(in %)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Borusyak, Jaravel, and Spiess (2024)	-0.117***	-0.117***	-0.555***	-0.528***	0.100***	-0.007*	-0.009	-0.224
	(0.027)	(0.025)	(0.142)	(0.139)	(0.038)	(0.004)	(0.023)	(0.433)
Firm-years	356,743	356,743	356,743	356,743	356,266	356,743	327,387	356,743
de Chaisemartin and D'Haultfœuille (2024)	-0.074***	-0.070***	-0.182***	-0.164***	0.037*	-0.002***	-0.008	0.169
	(0.021)	(0.023)	(0.055)	(0.037)	(0.022)	(0.001)	(0.005)	(0.322)
Firm-years	138,361	51,181	138.361	51,181	51,045	51,181	46,966	51,181
Switchers	26,140	25,844	26,140	25,844	25,783	25,844	23,885	25,844
OLS	-0.066***	-0.067***	-0.214***	-0.216***	0.019	-0.012***	-0.019***	-0.210
	(0.005)	(0.005)	(0.029)	(0.028)	(0.012)	(0.003)	(0.007)	(0.164)
Firm-years	356,272	356,272	356,272	356,272	355,789	356,272	326,057	356,272
Firm and bank controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE / Nonparametric industry trends	No	Yes	No	Yes	Yes	Yes	Yes	Yes
Country-year FE / Nonparametric country trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-event mean of treated in t=-1	1.00	1.00	2.46	2.46	5.19	5.69	2.14	0.00
Mean of not-yet and never treated	0.57	0.57	2.46	2.46	5.19	5.69	2.14	2.85

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table F3: Terminated correspondent bank relationships and firm-level outcomes: Static effects without firm controls, bank controls, or neither

This table shows static Borusyak, Jaravel, and Spiess (2024) estimates for firms' *Export dummy* and *Exports*, *Domestic revenues*, *Turnover*, *Employees*, and next year's *Dissolved* around the termination of one or more correspondent bank relationships. In contrast to Table 3, the top panel does not include firm control variables, the mid panel does not include bank control variables, and the bottom panel does neither include firm nor bank variables. Treated (control) firms have a main bank that has (not) lost a correspondent bank relationship up to the event year. Information on firms' main bank is taken from Bureau van Dijk's Orbis 'Bankers' database. We only use treated and control firms that are on the common support in the pre-event year with respect to *Exports*, *Total assets* and *Total Factor Productivity*. Firm controls are *Total assets* and *Total Factor Productivity*, bank controls are *Loan growth*, *Equity/Total assets*, *Loans/Customer deposits*, *ROA*. All regressions include country-by-year fixed effects and industry-by-year fixed effect. Standard errors are clustered at the bank level and shown in parentheses. Variable definitions and sources are reported in Appendix B.

	Exports		Revenues		Employees	Dissolved
	Dummy	Amount	Domestic	Total		in %
	(1)	(2)	(3)	(4)	(5)	(6)
Without firm controls	-0.125***	-0.498***	-0.040*	-0.153***	-0.051***	0.429***
	(0.019)	(0.107)	(0.021)	(0.024)	(0.012)	(0.156)
Firm-years	268,273	268,273	267,208	267,490	249,835	268,273
Without bank controls	-0.108***	-0.408***	0.080***	-0.012***	-0.026***	0.408***
	(0.017)	(0.097)	(0.012)	(0.002)	(0.009)	(0.145)
Firm-years	267,451	267,451	267,269	267,451	249,503	267,451
Neither firm nor bank controls	-0.121***	-0.480***	-0.032	-0.141***	-0.048***	0.043***
	(0.018)	(0.104)	(0.020)	(0.022)	(0.012)	(0.146)
Firm-years	269,542	269,542	268,475	268,759	250,832	269,542
Industry-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Country-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Pre-event mean of treated in t=-1	1.00	3.26	5.63	6.21	2.35	0.00
Mean of not-yet and never treated	0.56	2.62	5.80	6.25	2.45	0.47

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table F4: Terminated correspondent bank relationship: Heterogeneous effects

This table shows Borusyak, Jaravel, and Spiess (2024)'s difference-in-differences estimates for firm outcomes around the termination of one or more correspondent bank relationships for different subsamples: 'Large firms' and 'Small firms' refer to the subsamples of firms with an above- and below-median amount of total assets, 'Old firms' and 'Young firms' refer to the subsamples of firms with an above- and below-median age, 'High ARPC firms' and 'Low ARPC firms' refer to the subsamples of firms with an above- and below-median average revenue product of capital (ARPC), and 'Many correspondent banks' and 'Few correspondent banks' refer to the subsamples of firms with a main bank with an above- and below-median number of correspondent banks, respectively. Treated (control) firms have a main bank that has (not) lost a correspondent bank relationship up to the event year. Information on firms' main bank is taken from Bureau van Dijk's Orbis 'Bankers' database. Regressions include firm controls (*Total assets* and *Total Factor Productivity*), bank controls (*Loan growth*, *Equity/Total assets*, *Loans/Customer deposits*, *ROA*), country-by-year fixed effects, and industry-by-year fixed effect. 95%-confidence intervals are based on standard errors clustered by bank. Variable definitions and sources are reported in Appendix B.

	Exports		Revenues		Employees	Dissolved
	Dummy	Amount	Domestic	Total		in %
	(1)	(2)	(3)	(4)	(5)	(6)
Large firms	-0.103***	-0.424***	0.102***	0.007***	0.002	0.118
	(0.018)	(0.103)	(0.012)	(0.002)	(0.009)	(0.078)
Small firms	- 0.122***	-0.401***	0.053	-0.040***	-0.069***	0.138*
	(0.017)	(0.100)	(0.013)	(0.002)	(0.010)	(0.080)
Difference	0.020**	-0.023	0.050***	0.047***	0.070***	-0.020
z(Difference)	2.17	-0.40	7.40	21.37	13.24	0.56
p-value	0.030	0.689	0.000	0.000	0.000	0.578
Old firms	-0.096***	-0.359***	0.028**	-0.004**	-0.041***	0.118*
	(0.017)	(0.095)	(0.012)	(0.002)	(0.008)	(0.068)
Young firms	-0.126***	-0.429***	0.146***	-0.023***	-0.001	0.136
	(0.018)	(0.099)	(0.012)	(0.002)	(0.011)	(0.088)
Difference	0.030***	0.070***	-0.118***	0.019***	-0.040***	-0.018
z(Difference)	5.73	2.70	-27.13	13.73	-6.42	-0.59
p-value	0.000	0.007	0.000	0.000	0.000	0.558
High ARPC firms	-0.319***	-1.301***	0.180***	-0.034***	-0.082***	0.145
	(0.061)	(0.355)	(0.042)	(0.006)	(0.030)	(0.274)
Low ARPC firms	-0.314***	-1.363***	0.127***	-0.032***	-0.117***	0.207
	(0.061)	(0.354)	(0.042)	(0.006)	(0.031)	(0.273)
Difference	-0.005*	0.062***	0.053***	-0.002**	0.036***	-0.062***
z(Difference)	-1.67	4.40	7.26	-1.96	4.33	-2.86
p-value	0.095	0.000	0.000	0.050	0.000	0.004
Many correspondent banks	-0.023***	0.052***	0.076***	0.005*	0.030***	0.187***
	(0.003)	(0.013)	(0.009)	(0.003)	(0.007)	(0.116)
Few correspondent banks	-0.159***	-0.612***	0.086***	-0.021***	-0.056***	0.092
	(0.026)	(0.150)	(0.018)	(0.002)	(0.013)	(0.048)
Difference	0.136***	0.560***	-0.010	0.027***	0.087***	0.095
z(Difference)	5.24	3.76	-0.50	7.02	5.76	0.76
p-value	0.000	0.000	0.615	0.000	0.000	0.450

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table F5: Sector-level analysis: Summary statistics

This table shows summary statistics of country characteristics and trade-related variables for the 17 emerging European markets included in the sector-level analysis. Variable definitions and sources are reported in Appendix B.

	(1) Unit	(2) N	(3) Mean	(4) Median	(5) Min	(6) Max	(7) SD
Country-variables (17 countries in emerging Europe)							
High Withdrawal	-	17	0.53	1	0	1	0.51
GDP Counterparty	billion USD	378	316.24	29.91	0.04	10,482.40	966.37
Distance to trading partner	km	2,385	5,341.85	4,628.55	29.08	18,398.04	4,029.71
USD-EUR exchange rate	-	3	1.26	1.31	1.12	1.37	0.13
USD exports	share	17	0.03	0.03	0.00	0.14	0.03
USD imports	share	17	0.03	0.03	0.01	0.14	0.05
Trade-variables (Between 17 countries in emerging Europe and their global counterparties)							
Exports	million USD	935,783	2.10	0.08	0.00	747.85	11.54
Imports	million USD	1,057,932	2.50	0.08	0.00	2020.41	17.49
Trade-variables (Global aggregates)							
Exports (World)	million USD	78	636.8	354.0	8.9	3465.22	776.1
Imports (World)	million USD	78	636.8	354.0	8.9	3465.22	776.1