European Bank
for Reconstruction and Development

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

JEL classification numbers: F14; F15; F36; G21; G28

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We thank Çağatay Bircan, Neeltje van Horen, Laurent Weill (discussant) and seminar participants at the Mannheim Banking Workshop, University of Mainz, University of Mannheim and ZEW for valuable comments and suggestions. We are very grateful to Rudolf Putz, Kamola Makhmudova, and Yana Tamulyavchute at EBRD for their enthusiastic support of the respondent bank survey. The authors acknowledge support by the state of Baden-Württemberg through bwHPC and the German Research Foundation (DFG) through grant INST 35/1597-1 FUGG. Support by the German Research Foundation (DFG) through CRC TR 224 (Project C03) is gratefully acknowledged.

Working Paper No 285 **Prepared in December 2023**

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December 8, 2023

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1 Introduction

The 2008–09 global financial crisis ended a decades-long trend of financial globalization (Milesi-Ferretti and Tille 2011). Since then, new regulation, stricter supervision, and strengthened risk management have all prompted international banks to reduce or wind down foreign activities (Claessens 2017; De Haas and Van Horen 2017; Cerutti and Zhou 2018). As yet, it remains unclear how this financial deglobalization has begun to affect real economic activity on the ground. We shed light on this by focusing on a specific episode of financial fragmentation: the sudden and stark decline in correspondent banking in response to the stricter enforcement of financial crime regulation.

Besides deposit-taking and lending, global banks play an important role as correspondent banks. Correspondent banking refers to arrangements where one bank (the correspondent) holds deposits from another bank (the respondent) while providing international payments and other services. Correspondent banks facilitate cross-border trade in two main ways. First, they enable trade-related payments between the exporter's and the importer's local banks (which usually do not hold accounts with each other). Second, they provide trade finance solutions, such as letters of credit, that facilitate trade where and when the probability of non-payment or non-shipment is high and enforcement is expensive. By fulfilling these critical roles, correspondent banks provide the financial infrastructure that allows firms in less-developed countries to export to richer parts of the world.

Considering this, it is worrisome that global banks have radically pruned their correspondent bank relationships over the past decade. As we explain in Section 2, this retrenchment took place against the backdrop of a sharp increase in compliance costs due to stricter enforcement of financial crime regulation around 2014–15 (Rice, Peter, and Boar 2020). Policymakers have become concerned that the broad-based withdrawal of correspondent banks is not only dampening international trade but also undermining the growth prospects of poorer economies (Rice, Peter, and Boar 2020; FSB 2017; BIS 2016; CGD 2015; World Bank 2015).

The aim of this paper is to document and quantify the firm-level impact of this global

retrenchment of correspondent banks. To the best of our knowledge, this is the first paper to analyze the effect of this shock on firms' probability to export, their export revenues, and other real economic outcomes (total revenues, domestic revenues, employment) between 2011 and 2020. We focus on four emerging European countries—Bosnia & Herzegovina, Croatia, Hungary, and Turkey. These countries have traditionally relied heavily on correspondent banking services and hence provide a relevant and representative setting for our purposes. As in other parts of the world, the recent withdrawal of correspondent banks from emerging Europe mainly reflects the stricter enforcement of financial crime regulation in these banks' home countries (BIS 2016).

Our identification strategy rests on joining three key pieces of information: time-varying data on individual respondent banks' lost correspondent relationships; the geo-coordinates of these respondent banks' branches; and data on exports (and other real outcomes) of firms located near these branches. 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 that we conducted at the end of 2019 together with EBRD's Trade Facilitation Programme (TFP). We link these bank-level data to comprehensive information about the geographical location of their bank branches and then match this information with firm-level data from Bureau van Dijk's Orbis database. These combined data allow us to paint a detailed picture of the bank branches that surround each firm and to identify, at the local level, the impact of the withdrawal of correspondent banks on firm activity.

To do so, we use the difference-in-differences estimator of intertemporal treatment effects by Chaisemartin and D'Haultfœuille (2022). Their event study approach for binary and staggered treatments allows for dynamic and heterogeneous treatment effects.[1] In our differences-in-differences estimations, we systematically compare the exports and other firm outcomes in localities where at least one bank branch lost a correspondent banking rela-

1. Appendix E discusses why it is important to account for heterogeneous treatment effects in our setting.

tionship (treated firms) to firms in localities where no bank branch lost a correspondent relationship up to the event year (control firms).

Our results show that a decline in the supply of correspondent banking services negatively affects both the extensive and the intensive margin of exports. Exporting firms become less likely to continue to export and have a lower export turnover when one or several bank branches in their locality lose a correspondent banking relationship. We next show that firms affected by terminated correspondent banking relationships manage to only partially offset the resulting drop in exports by increasing their domestic sales. As a consequence, total turnover declines and firms have to lay off workers, albeit with some delay.

These baseline results reflect 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. The fact that we find strong and persistent negative impacts indicates that, typically, firms cannot simply switch banks when their own bank can no longer provide correspondent banking services. We also show that all these results are robust to using a continuous treatment variable at the locality level rather than a binary one.

We then proceed by connecting individual firms to individual banks, using data on bank-firm relationships from the Orbis database. The advantage of this approach is that we now distinguish *within* localities between firms directly affected by the termination of correspondent relationships and those that are not. Moreover, this approach lets us account for locality-level developments that may correlate with the decrease in correspondent bank relationships and could confound our baseline estimates. A downside is that we lose sight of possible equilibrium effects and 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. We nevertheless find that the results using firm-bank linkages are qualitatively the same as those with the locality-matched sample.

Importantly, throughout our analysis, we include time-varying locality-level controls that capture more general developments in local credit markets, in particular the average capi-

talization of local banks and total loans outstanding. These controls allow us to estimate the separate effect of terminated correspondent bank relationships over and above the role of general credit conditions at the locality level. To accurately estimate the impact of the decline in correspondent banking on firms, we also first match treated with observationally similar control firms and keep all firms with common support.

Our identification strategy does *not* require that the termination of correspondent banking relationships occurred randomly across localities, nor does it require that firms in treated and control localities have the same pre-treatment characteristics. Our estimates will be unbiased as long as exporting firms in treated and control localities would have evolved in the same way in the absence of the shock to the global correspondent banking network. We provide two main pieces of supporting evidence in this regard.

First, we show that, before the sudden decline in correspondent banking, there were no systematically different pre-trends in the export performance of firms in treated versus control localities. Second, while our design does not depend on firms in treatment and control localities 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 locality-level firm and bank characteristics. Throughout our analysis, we nevertheless control for these characteristics while also accounting for linear country and non-parametric industry trends. The inclusion of these controls absorbs many sources of unobserved heterogeneity that could otherwise bias our estimates.

Next, we present a spillover analysis in the vein of Berg, Reisinger, and Streitz (2021). We show that not accounting for heterogeneous spillovers leads us to underestimate the direct effect of a decline in correspondent banking on exports. We find that treated firms are *less* negatively affected in their probability to export, the greater the proportion of other treated firms in the industry. This may reflect 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. Control firms' probability to export is slightly lower if the proportion of treated firms in the same industry is higher. This likely reflects within-industry complementarities between suppliers across different parts of a country.

Last, we broaden our analysis to a larger 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 structural break that triggered a negative shock to the supply of correspondent banking services. While these industry-level 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. Bank-intermediated trade finance products, such as letters of credit, are, if anything, even more important when less developed countries import goods from advanced countries (Schmidt-Eisenlohr 2013; Antras and Foley 2015; Niepmann and Schmidt-Eisenlohr 2017). The sectoral-level results confirm our firm-level evidence: export and import growth decline significantly more in countries with a higher withdrawal of correspondent banks compared with countries where no or only few correspondent banks left.

Our study contributes to two strands of the literature. First, we provide new insights into the channels through which globally active banks can mediate the impact of financial frictions on international trade (Kohn, Leibovici, and Szkup 2022). Portes and Rey (2005), Bronzini and D'Ignazio (2017), and Claessens and Van Horen (2021) show that the physical presence of foreign banks supports trade between a host country and foreign banks' home countries. Moreover, Brancati (2022) finds that the acquisition of a firm's local bank by an international bank increases the likelihood that the firm starts to export to other countries in which the international bank also operates a branch. Caballero, Candelaria, and Hale (2018) show that an increase in syndicated loan connections between countries—that is, without foreign banks necessarily having a local presence on the ground—also boosts bilateral exports.

Other papers focus on the role of specific trade finance products for international trade. Niepmann and Schmidt-Eisenlohr (2017) and Ahn and Sarmiento (2019) analyze how bank-

level financial shocks reduce the supply of trade finance products (in particular, letters of credit) and, in turn, negatively affect firm exports.[2] In a similar vein, Demir and Javorcik (2020) and Crozet, Demir, and Javorcik (2022) show how a decline in bank-intermediated letters of credit negatively affected international trade flows during the Covid-19 pandemic. Other work has assessed the role of different trade finance products such as export credit insurance (Auboin and Engemann 2014; van der Veer 2015) and export guarantees (Felbermayr and Yalcin 2013; Heiland and Yalcin 2021).

Our contribution to this strand of the literature is to focus specifically on the role of correspondent banks in global trade. For identification, we leverage the surge in terminated correspondent banking relationships when financial crime regulation tightened around 2014–15. An important innovation is that we collect bank-level data on terminated correspondent relationships and on the exact location of respondent banks' branches. This allows us to quantify the real-economic effects of a local shock to the availability of correspondent services on exports and other firm-level outcomes. Our results highlight the importance of functioning correspondent banking services for international trade.

Second, we contribute to the literature documenting the cross-border transmission of various types of shocks through global banks, such as financial crises (Peek and Rosengren 1997; Peek and Rosengren 2000; Chava and Purnanandam 2011; Cetorelli and Goldberg 2011; Cetorelli and Goldberg 2012; Chor and Manova 2012; Popov and Udell 2012; Schnabl 2012; De Haas and Van Horen 2012; De Haas and Van Horen 2013; Paravisini et al. 2015, Ongena, Peydró, and Van Horen 2015), shocks to risky sovereign bond holdings (Popov and Van Horen 2015; Altavilla, Pagano, and Simonelli 2017; Balduzzi, Brancati, and Schiantarelli 2018; Acharya et al. 2018; De Marco 2019), tax reforms (Célérier, Kick, and Ongena 2020), micro- and macroprudential regulation (Aiyar et al. 2014; Tripathy 2020), and monetary policy shocks (Bruno and Shin 2015). We instead focus on the cross-border transmission of a

2. More generally, the role of local banks in providing working capital loans and thereby facilitating trade has been well documented (Amiti and Weinstein 2011; Chor and Manova 2012; Manova 2013; Del Prete and Federico 2014; Paravisini et al. 2015).

sudden shock to the costs of regulatory compliance, which had the unintended consequence of disrupting the global network of correspondent bank relationships.

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 global trade

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

2.1 Correspondent banking: A primer

Correspondent banking is an arrangement in which one bank (the correspondent) holds deposits of other banks (the respondents) and provides these respondent banks with payment and other financial services. In doing so, correspondent banks facilitate international trade in two main ways. First, they help channel trade-related cash flows across borders by enabling payments between exporters' and importers' local banks (which typically do not hold accounts with each other). The correspondent banking network forms the backbone of the international payment system (ECB 2020) and thus the bulk of payments underlying international trade runs through correspondent banks (Rice, Peter, and Boar 2020).

Second, correspondent banks provide trade finance products, such as letters of credit. Most international trade transactions take place on an open account basis and prepayment is rare (Asmundson et al. 2011; Ahn 2014). Correspondent banks then help overcome the commitment problems and limited enforceability that can inhibit direct payment between trading partners. Because correspondent banks maintain relationships of an ongoing and repetitive nature, they are a credible intermediary between local banks. They help ensure that payment and shipment take place as specified in the contract between the ultimate importer and exporter. This 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) such as in many developing economies (CGD 2015).

Due to the high fixed costs of establishing and maintaining correspondent bank relationships, trade finance is a very concentrated business. For example, the five largest U.S. banks account for 92 percent of all U.S. trade finance claims (Niepmann and Schmidt-Eisenlohr 2017). Likewise, in the whole of Italy, just ten banks provide trade finance (Del Prete and Federico 2014). The concentrated nature of correspondent banking may expose cross-border trade to sudden shocks to this tight international banking network.

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 be transferred back and forth between accounts in different countries and currencies, and (re-)exchanged for high-value items such as real estate. Correspondent banks may also be implicated in criminal activities through the provision of trade finance. 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. For example, the recommendations of the Financial Action Task Force (FATF), the global watchdog on money laundering and terrorist financing, require correspondent banks to reveal the identity of all parties involved in a cross-border transaction and to perform due diligence on their customers. However, in practice, the weak enforcement of these legal frameworks has undermined the fight against financial crime (CGD 2015). The prosecution of offences only tightened in the aftermath of the global financial crisis, when increased regulatory scrutiny unearthed extensive evidence of financial crimes in the banking sector (Tomasic 2011). U.S. regulators, in particular, stepped up their enforcement as a result.

The stricter enforcement of financial crime legislation has been evident in the issuance

of surging fines (CGD 2015). The most 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 (BNP Paribas had set aside 'only' US$1.1 billion in provisions for litigation costs) and greatly exceeded past fines (the highest had been 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 front of a U.S. court (Department of Justice 2014). While high fines appear to have been effective in preventing sanctions violations since the BNP Paribas trial, fines for violations of anti-money laundering regulation remain on the rise. A recent example includes the three fines, totalling US$7.2 billion, 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 a process of decline in global correspondent banking. The fine was widely regarded as a harbinger of stricter regulatory enforcement in the area of anti-money laundering and counter terrorist financing (AML/CTF). As such, it led to a reassessment of the cost of regulatory compliance in correspondent banking. First of all, the expected costs of *non*-compliance increased sharply in view of 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. Banks significantly increased spending on financial crime personnel (Dow Jones Risk & Compliance and ACAMS 2015; McKinsey 2017; Banking Exchange 2020) and highlighted inconsistencies in international regulation as another important cost factor (BIS 2016; SWIFT 2016).

The sudden hike in compliance costs prompted banks to reconsider their business strategies with regard to correspondent banking, a business that was seen as shifting from low-risk/low-margin to high-risk/low-margin (BIS 2016). Many banks severely pruned their correspondent banking networks by ending relationships that were no longer cost-effective or

were deemed too risky (BIS 2016; FSB 2017; Rice, Peter, and Boar 2020).

[Insert Figure 1 here]

Figure 1 visualizes the global decline in correspondent banking due to de-risking. We show the development of the Gini coefficient of the number of active correspondent banks per corridor between 2012 and 2022, using SWIFT data from the Bank for International Settlements (BIS). 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). Until 2014, the Gini coefficient is quite stable: the concentration of correspondent banks within corridors hardly changes. After 2014, however, there is a steady increase in the Gini coefficient. This reflects how, within corridors, the correspondent banking business became more and more concentrated as an increasing number of correspondent banks withdrew from the market.

To verify if respondent banks share the view that it was the sharp increase in regulatory compliance costs that induced correspondent banks to withdraw or reduce their services, we conducted a survey online among a sample of local respondent banks towards the end of 2019, covering the period 2009–2019. Of the 131 banks invited, 91 across 28 economies in Central and Eastern Europe, the former Soviet Union, and Northern Africa completed the entire questionnaire, a response rate of 69 percent.[3]

Figure 2 shows that, according to respondent banks, the main reasons for the decline in correspondent banking were that it "does not generate 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/CGT)" (32 percent). Only 3 percent of respondent banks considered "less demand from their customers" an important reason for the withdrawal of correspondent banks. These results corroborate that increased

3. These 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.

due diligence costs and concerns about compliance with AML/CFT regulations, rather than a reduced demand, caused the decrease in global correspondent bank relationships and services.

[Insert Figure 2 here]

The decline in correspondent banking 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 of these services. The broad nature of the retrenchment of correspondent banks, combined with the concentrated nature of the industry, made it difficult to find alternative providers.

Our bank survey provides the first descriptive evidence on the local impact of the reduced availability of correspondent banking services. Figure 3 shows the proportion of local banks that had difficulties in accessing, or were entirely unable to access, three important types of correspondent banking services in the years 2013, 2015, 2017, and 2019. We observe a sharp uptick in the proportion of respondent banks that experienced difficulties in accessing cross-border payment transactions (black bars); trade finance (dark grey); and currency clearing (light grey). Respondent banks that continued to have access to these services, experienced a sharp increase in their cost, on average, of 35 percent between 2017 and 2019 alone.

[Insert Figure 3 here]

The contraction of global correspondent banking has also changed the geographical distribution of the industry. While in 2013, 73 percent of all correspondent banks were based in the U.S. and Germany, the combined market share of these countries had declined to 60 percent in 2019. Correspondent banks from other countries have only partially filled this gap, and this substitution has led to longer and costlier intermediation chains.

3 Data

Our empirical analysis focuses on four emerging European countries—Bosnia & Herzegovina, Croatia, Hungary, and Turkey. These countries have seen similar declines in correspondent

banking relationships as the other countries in emerging Europe and Central Asia and hence provide a relevant and representative setting for our purposes (see Appendix A). We were unable to include other countries from emerging Europe as they either do not report firm-level export data in Orbis or do not display sufficient variation in terminated correspondent banking relationships within countries.

We match several data sets at the firm level to estimate the impact of the decline in correspondent banking on firms' exports, turnover, and employment. More specifically, our identification strategy relies on joining: (i) time-varying information for individual respondent banks about terminated correspondent banking relationships; (ii) data on the geo-coordinates of all branches of these respondent banks; and (iii) data on exports (and other real outcomes) of the firms that are geographically near these bank branches. We now discuss these data in turn. Appendix B contains the definitions and sources of all variables.

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 bank relationships. The first source is BEPS III, which took place between October 2020 and June 2021. The BEPS III research design covers large and small banks, and the aim was to survey banks that jointly represent at least 95 percent of all bank assets in a country. As part of BEPS III, senior financial 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. Bank CEOs answered questions about the number of correspondent banks their bank had access to at different points in time. Appendix C contains the BEPS III questions we use in this paper.

The BEPS III survey provides us with information about (changes in) correspondent banking relationships for 20 key respondent banks in our four sample countries. We supplement this with similar information on three additional respondent banks in these countries,

collected as part of an online survey we conducted in 2019 together with EBRD's TFP.[4] This survey focused exclusively on banks' correspondent banking relationships. Appendix C again contains the survey questions we use.

3.2 Firm exports and other firm characteristics

To estimate the impact of the rapid decline in correspondent banking services at the grassroots level, we access firm-level data from Bureau van Dijk's Orbis database. Orbis provides comprehensive information on balance sheets and income statements and, for some countries, yearly data on export revenues. Importantly, Orbis also provides the exact location of firms, allowing us to match firms to nearby bank branches, and information on a firm's industry. We obtain the Orbis flat files of June 2022 and ensure our data cleaning follows Kalemli-Özcan et al. (2023) to construct a nationally representative sample for our countries.

3.3 Bank branch networks and bank characteristics

We match our data on firms' exact geo-coordinates with information on all bank branches near these firms. This information was hand-collected as part of the BEPS III survey by either contacting banks or downloading data from bank websites and subsequently double-checking them with the bank and the SNL Financial database. In total, we have data on the geo-coordinates of 48,399 branches: a near complete picture of the branching landscape in 2020. We merge this information with Bureau van Dijk's Orbis BankFocus to obtain balance sheet and income statement data for each bank.

We then connect the firm and bank branch data following Beck et al. (2018). We ensure that the names of localities (villages, towns, and cities) are spelled consistently in both data sets and then 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. The (plausible) assumption is that a firm has access to all branches in the locality where it is incorporated and that it may be negatively affected by the loss of correspondent bank relationships of such local

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

banks.[5] We thus focus on local equilibrium effects while assuming that local credit markets are competitive in nature, so firms' access to banking services can be constrained by locality-level financial shocks. We include any locality in which we have at least one firm and at least one branch of a surveyed bank.

An alternative approach to match firms and banks is to use Orbis information on individual firms' main bank. This establishes a direct link between firms and banks, but comes at the cost of a somewhat smaller and more selective sample because the home bank information in Orbis is mostly available for larger firms. We rerun our complete analysis with this firm-level matching and show that our results are qualitatively the same.

For our empirical analysis we focus on exporters, defined as firms that export at least once during our observation period. These firms are likely most directly affected by a decline in correspondent banking. In addition, the trade literature shows that exporters are inherently different from other firms, so that studying a mixed sample of exporters and non-exporters would likely diffuse results. Overall, our sample of exporters comprises 224,346 unique firms based in 857 localities (villages, towns, and cities) across the four countries.[6]

4 Empirical strategy

4.1 Identification

We exploit the loss of banks' correspondent bank relationships as an exogenous shock to firms' access to trade finance services at the local level. In a difference-in-differences framework, we compare, before and after this shock, firms' export performance, total revenues, and employment generation in treatment localities—where at least one bank lost a correspondent relationship—with observationally similar (matched) firms in control localities—where no

5. That is, we assume that the banking landscape near firms imposes an exogenous geographical limitation on the lenders firms have access to (Berger et al. 2005). An extensive empirical literature provides evidence for such spatial credit rationing. For example, the median Belgian SME borrower in Degryse and Ongena (2005) was located 2.5 km from the lending bank branch. In the U.S. data of Petersen and Rajan (1994) and Agarwal and Hauswald (2010), the corresponding median distances were 3.7 km and 4.2 km, respectively.

6. In our regression analysis, we control for locality-level credit market characteristics. To construct these, we use information on *all* banks for which we have the relevant data, regardless of whether they were surveyed.

such relationships were lost.

Our framework does *not* require that the termination of correspondent relationships occurred randomly across localities, nor does it require that firms in treated and control localities have the same pre-treatment characteristics. Our coefficient of interest will be unbiased as long as exporters in treated and control localities would have evolved in the same way in the absence of the shock to the correspondent banking network. While this assumption is by its very nature untestable, we provide two pieces of supporting evidence.

First, we show in Section 5.1 that, before the sudden decline in correspondent banking, there were no systematically different pre-trends in the export performance of firms in treated versus control localities. This supports the idea that firms in both types of localities would have developed similarly in the absence of the global shock to correspondent banking.

Second, while our design does not depend on firms in treatment and control localities being similar in levels, such similarity would add further credibility to the common trends assumption. We therefore show that correspondent banks' withdrawal (our treatment) was orthogonal to various locality traits. In the first two columns of Table D1 of Appendix D, we use a locality-year panel data set over the period 2012–2020 to estimate the relationship between a broad set of time-varying locality characteristics and whether at least one local bank lost access to correspondent banking. These characteristics include local night time light intensity (a proxy for local economic development); the number of local firms; these firms' characteristics averaged by locality (total assets, total factor productivity, turnover, and employees); and local firm concentration expressed as a Herfindahl-Hirschman Index. We also include variables that characterize the local credit market: average total assets of the banks operating in the locality (weighted by each bank's number of local branches); their capitalization; loan-to-deposits ratio; and total loans outstanding. We also include a Herfindahl-Hirschman Index that gauges local credit concentration. These local credit market controls allow us to estimate the distinct effect of terminated correspondent bank relationships over and above general credit supply shocks at the locality level. Finally, we also include

locality fixed effects in column 2. In columns 3 and 4, we present similar regressions while using a continuous outcome variable that measures the number of discontinued correspondent banking relationships in a year and locality, normalized by the number of branches in that locality.

We use Wald tests to check whether these locality characteristics jointly and significantly correlate with our treatment variables. The p-values at the bottom of Table D1 show that we can never reject the null hypothesis of no systematic relation between, on the one hand, a large set of observable characteristics of local banks and businesses and, on the other, the locality-level decline in correspondent banking. That is, localities in which banks lost correspondent banking relationships and localities where banks did not are similar across a broad array of covariates before the shock to global correspondent banking.

4.2 Matching

In our difference-in-differences estimations, we compare exports and other real economic outcomes of firms in localities in which at least one bank branch lost a correspondent banking relationship (treated) to similar firms in localities where banks did not lose a correspondent banking relationship up to the event year (control). To provide unbiased estimates of the impact of the decline in correspondent banking, we match treated and control firms and retain those with common support in our sample.[7]

We match each treated firm with one control firm from the same industry and country that also exports in the pre-event year. Using nearest-neighbor matching, we select the control firm with the lowest Mahalanobis distance in terms of pre-event export turnover, total assets, and total factor productivity, calculated as the industry-adjusted residual of a two-factor Cobb-Douglas production function.[8] We match on total assets and productivity as the literature identifies these as the most important determinants of firm-level exports at the

7. We also run all our analyses on the complete firm sample. Results are qualitatively very similar and available on request.

8. The number of employees is only available for few Turkish firms in the Bureau van Dijk Orbis database. For Turkish firms we therefore calculate total factor productivity as the industry-adjusted residual of a production function based on firm total assets only.

extensive and intensive margins (Melitz 2003; Bernard et al. 2007).[9] We keep treated firms for which we find an appropriate control firm and for which we have at least two observations.

[Insert Table 1 here]

Table 1 provides summary statistics for the complete sample (Panel A) and the matched sample (Panel B). 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. Matching nevertheless further improves the similarity of the treatment and control groups with regard to observable firm characteristics, as indicated by the lower normalized differences in Panel B.

Table 1 also reports summary statistics for the decline in correspondent bank relationships in a locality, normalized by the number of bank branches in that locality (*Cut relationships (branch level) over branches in city*). This variable reveals the extent of terminated relationships at the locality level. On average, around 60 percent of the branches in a treated city lose a correspondent bank relationship.

We proceed with the matched sample in our regression analyses. The matched exporter sample consists of 23,751 firms across 706 cities. Table 2 provides summary statistics.

[Insert Table 2 here]

4.3 Empirical specification

To gauge the impact of the sudden decline in correspondent banking on local firms' exports and other outcomes, we employ the difference-in-differences estimator of intertemporal treat-

9. In addition, there are important financial variables determining firms' exports, such as access to credit (Berman and Héricourt 2010; Claessens and Van Horen 2021). We control for these bank-level variables (averaged at the locality level) in our regressions but do not include them in the matching so we only match on non-financial firm traits.

ment effects introduced by Chaisemartin and D'Haultfœuille (2022). Their approach for binary and staggered treatments allows for dynamic and heterogeneous treatment effects. In a traditional design with two-way fixed effects (TWFE), we would estimate:

$$Outcome_{ijt} = \sum_{k=-3,k\neq-1}^{k=+4} \beta_k \times D_k \times Lost\,relationship_{jt} + \beta_8 \times Firm\,controls_{ijt} + \beta_9 \times Bank\,controls_{jt} + \gamma_{ij} + \delta_t + \epsilon_{ijt} \quad (1)$$

where subscripts i, j and t stand for firm, locality, and year, respectively.

Our $Outcome_{ijt}$ variables are *Export dummy*, *Exports*, *Turnover*, *Domestic turnover*, and *Employees*. *Export dummy* measures the extensive export margin and is one if a firm exports in a given year; zero otherwise. *Exports* measures the revenues from export activities in log euros. *Turnover* captures operating revenues in log euros while *Domestic turnover* measures domestic sales in log euros. *Employees* is the log number of employees.

D_k are dummies that are 1 at time k with k indicating the year before (for $-3 \leq k \leq -2$) or after ($0 \leq k \leq 4$) the event year. We normalize D_{-1} to 0. $Lost\,relationship_{jt}$ is a dummy that equals one if at least one bank branch in city j has lost a correspondent banking relationship in year t or earlier. γ_{ij} and δ_t are firm and year fixed effects, respectively. Standard errors are robust and clustered at the locality level.

In this traditional dynamic TWFE regression, we would interpret β_k as the treatment effect of a lost relationship k years before or after the event year. However, Chaisemartin and D'Haultfœuille (2020) show that this approach can result in incorrect estimates due to the different implicit weighting of the average treatment effects (ATEs) of firms experiencing their first treatment in different years. Appendix E shows that this is a relevant problem in our setting. Adding to this concern, Sun and Abraham (2021) show that, if treatment effects vary across firms and over time, β_k may be biased for the ATE from k=-3 until k=+4 (see also Baker, Larcker, and Wang 2022).

To avoid these issues, we apply the estimator of Chaisemartin and D'Haultfœuille (2022), which allows both for heterogeneous treatment effects across different firms and for dynamic effects around events. The estimator is a weighted average of difference-in-differences comparing the outcome evolution of switchers (firms that experienced a withdrawal at t-k) with the evolution of not-yet switchers (firms untreated up to t) between k=-3 and k=4. We can then interpret our estimates for β_k as the effect of having experienced a withdrawal for the first time k periods ago.

We expect the decline in correspondent bank relationships in a locality to have a negative effect on firm outcomes and therefore conjecture that β_0 to β_4 are negative. If firms can replace (some of) their export activity with increased local sales, then the coefficients $\beta_k > 0$ will be insignificant for firms' overall turnover and the number of employees. Like in any difference-in-differences design, the causal interpretation of our findings rests on the parallel trends assumption. Insignificant coefficients on β_{-3} and β_{-2}, i.e. the absence of an effect in the pre-event years, indicate that this assumption is reasonable.[10]

To mitigate lingering concerns about omitted variable bias, even after matching on pre-treatment characteristics, we add a vector of time-varying $Firm\ controls_{ijt}$ and $Bank\ controls_{jt}$. At the firm level, we include log *Total assets* to control for firm size and *Total Factor Productivity*, the industry-adjusted residual of a two-factor Cobb-Douglas production function. $Bank\ controls_{jt}$ are branch-weighted averages by locality, to ensure our results are not driven by the structure of the local banking environment. These variables are constructed using data on all banks with branches in a locality, irrespective of whether or not we have data on the change in their correspondent bank relationships. *Local loan growth* is the percentage change in gross lending of the banks in the locality. *Equity/Total assets* accounts for banks' capitalization. *Loans/Customer deposits* indicates the extent to which a bank's

10. We only estimate pre-trends starting from t = *-3* as the Chaisemartin and D'Haultfœuille (2022) estimator is based on first differencing between treated and not-yet treated control firms at any t. As our sample starts in 2011 and many of our firms switch into treatment in 2014, few treated-control pairs of the same industry and country are available for $t <$ *-3*. For instance, the estimator for t = *-4* is based on only 202 switchers and their controls.

loans are funded by wholesale rather then deposit funding and *ROA* is the return on assets.

Last, the Chaisemartin and D'Haultfœuille (2022) estimator controls for linear country trends by including fixed effects for the country when residualizing the first difference of the outcome. We also account for non-parametric industry trends via a weighted average of difference-in-differences comparing switchers and non-switchers in the same industry. For robustness, we present results based on Borusyak, Jaravel, and Spiess (2022)'s difference-in-differences estimator, which uses an imputation approach allowing for arbitrary heterogeneity and dynamics of causal effects.[11] This sets a plausible range for the treatment effects.

5 Results

This section first investigates the impact of the sudden termination of correspondent banking services on firm-level exports and other real economic outcomes (Section 5.1). Section 5.2 then subjects these baseline results to a battery of robustness tests. We estimate and discuss potential spillover effects in Section 5.3 and complete our analysis by studying the effect of the withdrawal of correspondent banks in a large sample of emerging European countries using bilateral sectoral trade data in Section 5.4.

5.1 Terminated correspondent banking relationships and firm-level outcomes

5.1.1 Likelihood to export and total exports

We start our empirical analysis by investigating the effect of the termination of correspondent bank relationships on firms' likelihood to export and on their export turnover. Figure 4 shows the results from the dynamic difference-in-differences regressions for both outcomes. The left-hand graph reports estimates and 95%-confidence intervals of the average effect of the decline in correspondent bank relationships on firms' probability to export (*Export dummy*). The reported coefficients (red dots) are from a regression following the Chaisemartin and

11. Unlike the Chaisemartin and D'Haultfœuille (2022) estimator and Equation (1), the approach introduced by Borusyak, Jaravel, and Spiess (2022) also provides a test for potential pre-trends at *t=-1*. We therefore include coefficient estimates for the pre-treatment year in all specifications based on Borusyak, Jaravel, and Spiess (2022).

D'Haultfœuille (2022) approach, including *Firm controls* and *Bank controls*, and controlling for linear country trends and non-parametric industry trends. The corresponding regression results are reported in Table 3, column (1).[12]

The results show that, after the termination of one or more local correspondent bank relationships, the likelihood to export declines significantly for firms in affected localities compared with similar firms in localities where (as yet) no correspondent banking relationships were lost. The probability to export is 3.8 percentage points lower for treated firms compared with control firms right after the termination of one ore more correspondent bank relationships (t=0). This difference becomes more pronounced over time. After four years (t=4), treated firms even have a 35.2 percentage point lower probability to export. These effects are sizable and reflect that many firms in our sample are small and medium-sized enterprises. Such smaller firms often find it difficult to replace lost trade relationships when trade networks get distorted due to terminated correspondent bank relationships.

The blue dashes in the left-hand graph of Figure 4 indicate estimates using Borusyak, Jaravel, and Spiess (2022)'s imputation approach. The estimator yields very similar results. Last, we note that the insignificant and close to zero pre-event effects of the Chaisemartin and D'Haultfœuille (2022) estimator suggest that firms in both types of localities would have developed along parallel paths in case no correspondent banking relationships had been discontinued. For the estimator introduced by Borusyak, Jaravel, and Spiess (2022), these effects are significantly positive, albeit small. Note that, unlike the Chaisemartin and D'Haultfœuille (2022) approach, which does not provide estimates for a potential pre-trend in *t=-1*, Borusyak, Jaravel, and Spiess (2022) estimates coefficients for the pre-event year.

[Insert Figure 4 and Table 3 here]

The right-hand graph in Figure 4 depicts the results from dynamic difference-in-differences

12. The methodology by Chaisemartin and D'Haultfœuille (2022) does not allow for more than one set of non-parametric trends. We therefore repeat this analysis using OLS to include both industry×year fixed effects and country×year fixed effects. The results are reported in Table F3 in Appendix F and yield similar conclusions.

regressions for export turnover. The red dots again indicate coefficients from a regression following the Chaisemartin and D'Haultfœuille (2022) approach. The respective regression results are reported in Table 3, column (2). If our estimated coefficients of the log-linear specifications were small enough, we could interpret them as percentage changes in the outcome following the withdrawal of a correspondent banking relationship. However, as shown in Table 3, the coefficients are, in fact, quite sizable. We therefore forego this approximation throughout the paper. Instead, we always report and interpret equivalent coefficients resulting from a linear transformation of the log-linear estimates.

We find that, once one or several local correspondent banking relationships are terminated, local firms' total amount of exports begins to decline. The point estimates indicate that the full effects of the termination are not felt immediately. The impact instead materializes with some delay (and only becomes statistically significant at t=2) but becomes more pronounced over time. Two years after the event (t=2), the export turnover of firms in localities that lost correspondent banking relationships is 53.2 percent lower than that of similar control firms in unaffected localities.[13] This stark average decline reflects firms that stopped exporting altogether and firms that shrank their exports on the intensive margin.

We again report estimates based on Borusyak, Jaravel, and Spiess (2022) as blue dashes. They are broadly in line with the patterns obtained using the Chaisemartin and D'Haultfœuille (2022) estimator, but report some positive pre-trends. Overall, Figure 4 illustrates how a sudden termination of correspondent banking relationships negatively affects firms' export performance on the extensive and intensive margins.

5.1.2 Domestic sales and total turnover

Firms whose local bank has 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 turnover and employment may be affected less negatively or perhaps not at all. We therefore also analyze how the termination of correspondent relationships affects firms'

13. The percentage change in outcome is calculated as follows: $exp(\beta) - 1 = exp(-0.76) - 1 = -0.532$.

domestic and total turnover. This provides for a more complete picture of the firm-level impact of the fragmentation of the global correspondent banking network.

The red dots in Figure 5 depict the Chaisemartin and D'Haultfœuille (2022) dynamic estimates for firms' domestic turnover (left) and total turnover (right).[14] The graph on the left of Figure 5 shows that, immediately after the shock to local correspondent banking relationships, there is no strong increase in local firms' domestic sales—at least not in the first two years. In the medium term, however, firms appear more successful in expanding their domestic turnover. While the Chaisemartin and D'Haultfœuille (2022) estimates are noisy, the Borusyak, Jaravel, and Spiess (2022) estimates (again reported as blue dashes) present a similar but more precisely estimated pattern.[15] They confirm that, over time, firms successfully respond to increased export barriers by expanding their sales domestically.

Can affected firms offset their reduced exports one for one by higher local sales? The right-side panel of Figure 5 shows that this is not the case. Both estimators illustrate clearly that total turnover (that is, foreign and domestic sales combined) declines more in localities where at least one bank branch loses access to correspondent banks, relative to firms in places where banks managed to maintain access to the global correspondent network.

[Insert Figure 5 here]

5.1.3 Employment

In line with firms' reduced overall turnover, Figure 6 shows a negative ATE on the number of firm employees (see also column (5) of Table 3). Firms that experienced the termination of one or more correspondent banking relationships in their locality shrink their workforce by 3.3 percent within a year, compared with similar unaffected firms. After four years (t=4), this difference has widened to 11.8 percent.

[Insert Figure 6 here]

14. We report the underlying regression results in columns (3) and (4) of Table 3.

15. Simulations in Borusyak, Jaravel, and Spiess (2022) illustrate the additional statistical power of the imputation estimator relative to other dynamic TWFE estimators.

In summation, our results indicate that firms lose export opportunities when correspondent banking relationships are terminated in their locality; they cannot fully compensate for this loss of access to foreign markets by expanding domestic sales; and affected firms therefore lay off part of their employees.

5.2 Robustness

5.2.1 An alternative strategy to link firms to banks

Our baseline approach is to link each firm to all bank branches in the locality in which it is incorporated. 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 or not. If firms of affected banks can easily switch to other (unaffected) local banks, then this would attenuate our estimates. The fact we find strong and persistent negative impacts therefore indicates that small firms cannot simply switch banks when their own bank can no longer provide correspondent banking services.

Another way to connect firms with banks is to use Orbis data on each firm's main bank. The advantage is that we now distinguish *within* localities between firms affected by the termination of correspondent relationships and those unaffected. Thus, we can account for locality-level developments that may correlate with the decrease in correspondent bank relationships and hence confound our estimates. A disadvantage is that we lose sight of possible equilibrium effects along the lines described above. Moreover, 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.

To investigate these issues, we re-run our main regressions using this Orbis-matched sample. Results for our export variables are presented in Figure 7 and Table F1 in Appendix F. The left-hand graphs in Figure 7 depict the results from the differences-in-differences regressions for firms' export probability, while the right-hand graphs depict the results for firms' export turnover. The coefficients reported as red dots are, again, from a regression using the Chaisemartin and D'Haultfœuille (2022) estimator, including firm and bank covariates

while controlling for linear country trends (upper graphs) or linear city trends to account for time-varying city characteristics (lower graphs), respectively, and non-parametric industry trends. Estimates based on Borusyak, Jaravel, and Spiess (2022) are reported as blue dashes and yield very similar point estimates. As before, these estimates tend to be more precise, especially at the start and the end of the sample window.

Overall, the results using firm–bank linkages are qualitatively the same as those with the locality-matched sample in our main analysis (Figure 4). A decrease in correspondent banking services negatively affects the extensive and the intensive margin of exports. Exporting firms are less likely to export and have a lower export turnover if their main bank has lost at least one correspondent banking relationship. Since these are treatment effects for the specific group of firms whose own bank lost one or several correspondent banking relationships, as expected, the magnitude of the effects is somewhat larger. For example, while the general local equilibrium effect of terminated banking relationships on firms' propensity to export is -3.8 percentage points (Figure 4), this effect is 5.1 percentage points when we directly link firms to banks. Moreover, given the smaller sample size of the Orbis-matched sample, confidence intervals of the Chaisemartin and D'Haultfœuille (2022) estimator are wider than in our main specification, especially in $t = 3$ and $t = 4$. As causal effects are estimated based on first differences between treated and not-yet-treated control firms at any t by the Chaisemartin and D'Haultfœuille (2022) estimator, the fewer available treated-control pairs of the same industry and country after $t = 2$ result in relatively large standard errors. For instance, the effects estimated for the *Export dummy* at $t = 3$ and $t = 4$ are based on 1,550 and 1,422 switchers and their controls, respectively, compared with 5,441 switchers for the estimate at $t = 0$.

[Insert Figure 7 here]

Figure 8 and Figure 9 then show the results for domestic and total turnover, as well as for the number of employees using the Orbis-matched sample. Again, the results using firm-bank relationships largely confirm our results from the locality-matched sample. Firms

whose main bank loses at least one correspondent bank relationship are only able to expand their domestic sales in the medium-term. However, these additional domestic sales cannot make up for the loss in export turnover, so total turnover remains significantly lower. The results for the number of firm employees show a negative effect due to the shock to local correspondent bank relationships, as in the locality-matched sample, but are imprecisely estimated using the Orbis-matched sample.

[Insert Figures 8 and 9 here]

5.2.2 A continuous treatment measure

So far, we have used a binary treatment indicator: a dummy that equals one if at least one bank branch in a locality lost a correspondent relationship in year t or earlier (*Lost relationship*). We now create a continuous treatment variable. *Cut relationships* measures the number of terminated correspondent bank relationships in a locality in year t or earlier, and normalizes this by the number of bank branches in the locality. This variable therefore gauges treatment intensity across localities.

Unlike the Borusyak, Jaravel, and Spiess (2022) estimator, the Chaisemartin and D'Haultfœuille (2022) estimator can be used with continuous treatment measures. The challenge, however, is to have enough proper control firms in the sample. For instance, if a firm jumps from treatment=0.1 to treatment=0.2, the estimator needs control firms that stay at 0.1 during the years before and after the treatment. Naturally, this condition needs to be fulfilled for all possible treatment values, which is not the case in our setting. As a solution, Chaisemartin and D'Haultfœuille (2022) propose to consider small treatment changes as being essentially stable. We follow this approach and use any firm as a control whose change in the treatment level is 0.1 or less. We present results using this continuous treatment in Figure 10 to Figure 12 and in Table F2 in Appendix F.

[Insert Figures 10-11-12 here]

Figure 10 confirms our earlier findings on exports: a decrease in correspondent banking negatively affects the extensive and intensive export margins. Exporting firms become less likely to export, and export less when more correspondent banking relationships are terminated in their locality compared with firms in localities where no or fewer such relationships disappear. The effects are also economically meaningful: a one standard deviation increase in terminated correspondent relationships leads to an immediate decline in the export probability of 2.1 percentage points, and of 23 percentage points within four years. Figure 11 and Figure 12 confirm that firms in localities that experience a decline in correspondent bank relationships cannot fully compensate for this loss of access to foreign markets by expanding domestic sales. Affected firms therefore lay off part of their workforce.

5.3 Spillover effects

The termination of correspondent relationships may generate spillovers to initially unaffected (control) firms within the same industry, but located elsewhere. The competitive outlook of such firms may improve relative to treated firms that have lost local access to correspondent banking services. Moreover, the effect of a shock on firm-level real outcomes depends not only on a firm's own treatment status, but also on the fraction of treated firms in the same industry. In our setting, the negative impact of broken correspondent relationships on treated firms may be less severe if more firms within the same industry are treated. With more treated firms, the respective trading partners can switch less easily to other suppliers in the same industry but in unaffected localities.

This section follows Berg, Reisinger, and Streitz (2021) to analyze heterogeneous spillover effects from firms affected by the local termination of correspondent banking relationships. We focus on spillovers within industries but do not investigate spatial spillovers. As we match firms to bank branches within the same locality, the loss of correspondent relationships in that locality may affect all local firms. As previously discussed, this means we effectively estimate local equilibrium effects that already aggregate firms' individual treatment effects and locality-level spillovers. To estimate spillovers within industries, we estimate the following

heterogeneous spillover model using OLS:

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

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

As dependent variables ($Outcome_{ijst}$) we use *Export dummy* and *Exports* for the spillover analysis. d_{ijst} is our treatment indicator, which switches to 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 treated firms in an industry (without firm i). *Firm controls*$_{ijt}$ include *Total assets* and *Total Factor Productivity*; and *Bank controls*$_{jt}$ comprise *Local loan growth*, *Equity/Total assets*, *Loans/Customer deposits* and *ROA* as defined in Section 4.3. γ_{ij} are firm fixed effects and δ_t are year fixed effects. This heterogeneous spillover model provides us with three coefficients of interest: 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 outcome variables *Export dummy* and *Exports* as a function of treatment intensity—the proportion of treated firms in an industry—for treatment units, control units, and group averages. The underlying regressions are estimated using 'static' OLS, in contrast to the dynamic TWFE estimates that form the basis for the event-study plots in our main analysis. By way of comparison, Table 4 provides the related static OLS results without accounting for spillovers. The table also reports static treatment effects using the Chaisemartin and D'Haultfœuille (2022), and Borusyak, Jaravel, and Spiess (2022) estimators. All three approaches yield very similar results and confirm that cutting correspondent bank relationships reduces firms' exports at the intensive and extensive margins.

[Insert Table 4 here]

The left graph of Figure 13 presents the results of the spillover analysis for the probability to export (*Export dummy*). The direct treatment effect indicates the impact of a decline in

correspondent bank 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 -11.2 percentage points. The increasing solid line shows that treated firms are less negatively affected in their probability to export the larger the proportion of other treated firms in the industry. One reason may be that, with more treated firms in an industry, respective trading partners have fewer possibilities to buy their products more cheaply from control firms and fewer treated firms stop exporting as a result.

The decline in the dotted line shows that control firms, i.e. exporting firms in localities that do not experience a decline in correspondent bank relationships, suffer from some spillover effects. Control firms' probability to export decreases with the fraction of treated firms in the same industry. This may reflect within-industry complementarities between suppliers across different parts of a country. When treated suppliers find it more difficult to export due to locally disrupted correspondent relationships, some foreign buyers may decide to source all of their products from a different country, thus also reducing their demand for products in unaffected localities in the original country.

As the positive within-treated-firms spillovers are larger than the spillovers to control firms, the difference between treatment and control firms diminishes with more firms in the same industry being treated. This means that not accounting for spillover effects leads to underestimating the direct treatment effect. The dashed line in the left graph of Figure 13 presents the industry-level average probability to export depending on the proportion of treated firms (normalized at zero). The slope is declining up to the proportion of treated firms of 0.8 and is relatively flat afterwards, a result of the weakened negative effect treated firms experience when they represent a larger segment of the industry.

[Insert Figure 13 here]

The right graph of Figure 13 shows the results for export turnover (*Exports*). The direct treatment effect when the fraction of treated firms is zero is -37.1 percent (the percentage change equivalent with a log-linear coefficient of -0.463). The slightly increasing dotted line

shows there are some positive spillovers to control firms when the proportion of treated firms becomes larger. For the treated firms, the rising solid line illustrates that they are less negatively affected by the decline in correspondent bank relationships when the fraction of treated firms in the same industry increases, which is in line with our findings on spillover effects in the probability to export. The difference between treatment and control firms diminishes with more firms in the same industry being treated as a result of the larger positive within-treated-firms spillovers. This again means that not accounting for spillover effects leads to underestimating the direct treatment effect.

The dashed line in the right graph of Figure 13 presents the aggregate effect of the decline in correspondent bank relationships. The slope is decreasing up to a fraction of treated firms of 0.75 and increasing for larger fractions of treated firms, which is consistent with the spillovers to control (treated) firms dominating the aggregate effect when the fraction of treated firms is small (large).

Summarizing, the spillover analysis shows that not accounting for heterogeneous spillover effects underestimates the direct treatment effect of a decline in correspondent banking on the probability to export and the export turnover. The pattern of the spillovers are consistent with the withdrawal of correspondent banks inducing a price effect on treated firms so that the negative effect of the increase in the costs to export are less severe for each individual treated firm the larger the fraction of treated firms in the same industry. The estimated aggregate effects highlight that researchers may find very different treatment effects when not accounting for heterogeneous spillovers depending on the fraction of treated firms in their sample.

5.4 External validity: Bilateral sectoral trade data

We now broaden our analysis to a larger sample of 17 Emerging European markets.[16] We use bilateral sectoral trade data and exploit the tightening of the U.S. regulator's enforcement of

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

financial crime legislation in June 2014 as a structural break that triggered a negative shock to the supply of correspondent banking services. While the industry-level 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. Bank-intermediated trade finance products, such as letters of credit, are, if anything, even more important when less developed countries import goods from advanced countries (Schmidt-Eisenlohr 2013; Antras and Foley 2015; Niepmann and Schmidt-Eisenlohr 2017).

To investigate the implications of the withdrawal of correspondent banks in this broader sample, we use a differences-in-differences approach, comparing sectoral, bilateral trade growth in the 12 months before and after the event (the fining of BNP Paribas in June 2014) and between countries with a high withdrawal of correspondent banks versus those with a low withdrawal. The key identifying assumption is that the withdrawal of correspondent banks is uncorrelated with simultaneous shocks to the demand for correspondent banking services in specific countries. Panels D and E of Appendix B contain the definitions and sources of all variables and Table F4 in Appendix F the respective summary statistics.

Our measure for the supply shock to correspondent banking uses survey data collected by the Financial Stability Board's Correspondent Banking Coordination Group (CBCG) and published in FSB (2017). The CBCG interviewed 345 banks in 48 jurisdictions about the withdrawal of correspondent banks. The survey comprises answers from the majority of large correspondent banks as well as local banks from countries experiencing a strong withdrawal. We measure the withdrawal of correspondent banks as the percent change in the number of active correspondent banks in a country from January 2011 to June 2016.[17] Countries with a high withdrawal of correspondent banks experience, on average, a 19-percent decline in correspondent banks, while countries with a low withdrawal face, on average, only a 7-

17. This measure thus captures the withdrawal of correspondent banks over a somewhat longer horizon. This means that we do not directly quantify to which extent the BNP Paribas penalty in June 2014 triggered an accelerated withdrawal of correspondent banks. However, the measure should adequately mirror the withdrawal of correspondent banks since 2014 as Figure 1 indicates that the number of active correspondent banks was stable until 2013 and only started to decrease afterwards.

percent decrease in correspondent banks with some countries not being affected at all by the withdrawal of correspondent banks.

We obtain monthly sectoral, bilateral trade data from the UN Comtrade database for 2012–2015. Trade flows are at the country-industry-counterparty level, in US dollars and at the 2-digit HS industry code. For each country-industry-counterparty triple, observations are aggregated to 12-month periods from July to June of the following year, to match the 12-month time windows before and after the event. As dependent variables, we then use first differences in log exports ($\Delta LogExports$) and log imports ($\Delta LogImports$) in the 12 months before and after the BNP Paribas penalty. The aggregation of trade flows into one pre-event and post-event period, respectively, alleviates the risk of underestimating standard errors, given the potential serial correlation in the monthly trade data (Bertrand, Duflo, and Mullainathan 2004). The definition of the dependent variable in terms of first differences follows the previous literature to allow for different pre-treatment time trends between treatment and control group (Khandelwal, Schott, and Wei 2013; Claessens and Van Horen 2014; Demir, Michalski, and Ors 2017; Claessens and Van Horen 2021). This differences-in-differences model is then:

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

where subscripts s, i and j denote the sector, the Emerging European country, and the trading partner country, respectively, and subscript $t \in$ *[2013:07-2014:06; 2014:07-2015:06]* the 12-month period before and after the event. Our $Outcome_{sijt}$ variables are $\Delta LogExports_{sijt}$, which are growth rates in exports from sector s in country i to country j in period t relative to period $t-1$, and $\Delta LogImports_{sijt}$, which are growth rates in imports of sector s in country i from country j in period t relative to period $t-1$. $Post_t$ is an indicator variable that is 0 in the July 2013 to June 2014 period and 1 in the July 2014 to June 2015 period; $Highwithdrawal_{it}$ is the treatment dummy that is 1 for countries with an above median reduction in the number of correspondent banks and 0 otherwise; X_{sijt} are country, trading partner country, sector

and time-specific control variables. FE_i are Emerging European country-fixed effects that control for time-invariant country factors which may affect both the selection into treatment (i.e. the withdrawal of global banks) and export/import growth. Standard errors are robust and clustered at the trading-country level following Claessens and Van Horen (2021).

Our variable of interest is the interaction term *Post x Highwithdrawal*. β_1 captures the additional change in export/import growth rates for countries facing a high withdrawal of correspondent banks relative to those with a low withdrawal. Given that countries with a high withdrawal of correspondent banks should face a stronger negative supply shock to payment services and trade finance, we expect a significantly negative estimate of β_1.

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. Annual *GDP* data is obtained from the World Bank and data on pairwise distances (*Log Distance*) between countries from Kristian Skrede Gleditsch's website.

In more saturated specifications, we include a battery of fixed effects to mitigate any concerns about omitted variables bias. In particular, industry fixed effects control for time-invariant industry factors at the 2-digit ISIC level. When including industry-time fixed effects, all time-varying industry factors, such as industry-specific demand shocks, are controlled for as well. Partner-time fixed effects control for time-invariant as well as time-varying factors related to the importing economy, such as demand shocks. Lastly, trading-countries pair fixed effects control for the overall rates in export growth, the geographical distance and the cultural and institutional proximity between exporter and importer country.

We ensure that our results are not driven by the rise in the US$ in the second half of 2014 on the back of expectations about interest-rate raises by the Federal Reserve. A stronger US$

could especially explain a reduction in imports of US$-denominated goods. As information on the proportion of US$-denominated trade for the countries in our sample is not publicly available, we approximate the sector-specific share of US$-denominated exports in total exports as the proportion of goods that are exported to the Americas. This choice is driven by the fact that trade with this region is predominantly denominated in US$ (SWIFT, 2015). We then control for the exposure to the US$ valuation shock by including the interaction term *Prop. US$ Exports x* $\Delta LogUS\$/EUR$ in our regression, 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. Data on exchange rates comes from the European Central Bank.

Table 5 reports the baseline DiD regressions, where first differences in log exports serve as the dependent variable. Column (1) presents our baseline specification while the remaining columns include industry fixed effects (column 2); industry fixed effects and the interaction term *Prop. US$ Exports x* $\Delta LogUS\$/EUR$ (column 3); industry and partner-time fixed effects (column 4); industry , partner-time, and trading-countries pair fixed effects (column 5); or industry-time, partner-time, and trading-countries pair fixed effects (column 6).

[Insert Table 5 here]

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 (6), the export growth rate is, on average, 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 rates is 15 percent.

We obtain similar results for the import regressions (Table 6). Again, the reduction in import growth rates is significantly higher 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.[18]

[Insert Table 6 here]

6 Conclusions

We have demonstrated how broken correspondent banking relationships, in response to the stricter enforcement of financial crime legislation, have had immediate and strong negative impacts on exporting firms in emerging markets. Affected firms have only been partially successful in replacing lost export opportunities with increased domestic sales. As a result, and with some delay, they have had to lay off part of their workforce.

The impact of broken correspondent banking relationships on exports from emerging markets may be long-lived as it takes time for local knowledge and relationships to be re-established. In the short term, government-backed schemes, such as trade-insurance products for exporters, can be helpful in alleviating the negative impacts of reduced private sector involvement in the management of trade risks.

In the medium term, efforts will need to focus on reconnecting (former) exporters in emerging markets to the global trade system. To regain access to correspondent banking services, many respondent banks will need to better align their compliance procedures with international standards. They will also need to ensure their staff obtain all relevant professional certifications, such as in customer due diligence, financial crime prevention, and money laundering risks in correspondent banking.

In the longer term, new private technologies may help facilitate safe and speedy cross-border payments associated with trade transactions. Currently, however, fintechs only play a

18. We run robustness checks where Russia and Ukraine are omitted as trading partners from the regressions to reduce concerns that the repercussions of Russia's war on Ukraine may impact our results. We also run robustness checks in which we exclude trading partners and industrial goods experiencing a particular large decline. Again, results are qualitatively and quantitatively unchanged. In addition, we validate that our results are robust to using a different treatment variable. As an alternative, we measure the country-level change in the availability of correspondent banking services as the percent change in the value of SWIFT transactions between 2012 and 2015 from BIS (2016). Banks are assigned to the treatment (control) group if they experience an above (below) median reduction in the value of SWIFT transactions. All results from these robustness checks are very similar to the main results and available upon request.

limited role in the market for trade-related cross-border payments, again reflecting the high compliance costs of financial crime regulation.

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Figures

Figure 1: Gini coefficient on the number of active correspondents per corridor

This figure shows the GINI coefficient on the number of active correspondents per corridor between 2012 and 2022, based on the three month moving average of active correspondents and 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). Source: SWIFT data from the Bank for International Settlements (BIS).

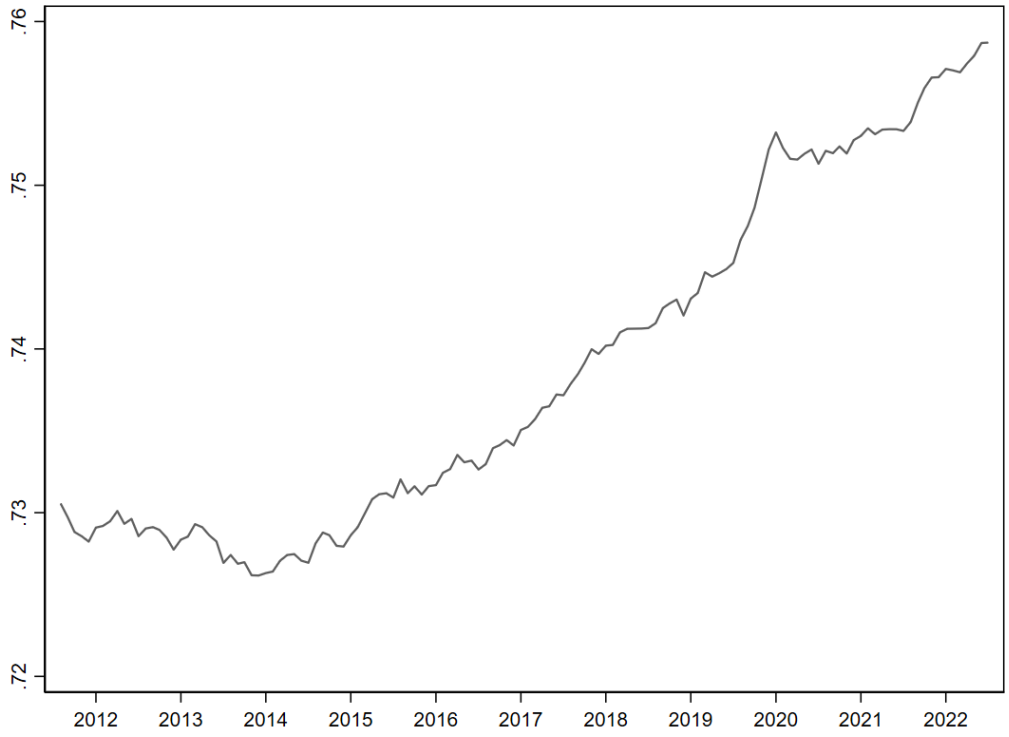


Figure 2: Reasons for the withdrawal of correspondent banks

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?". The question was asked in an online survey conducted together with EBRD's Trade Facilitation Program at the end of 2019. 91 banks across 28 countries answered the question.

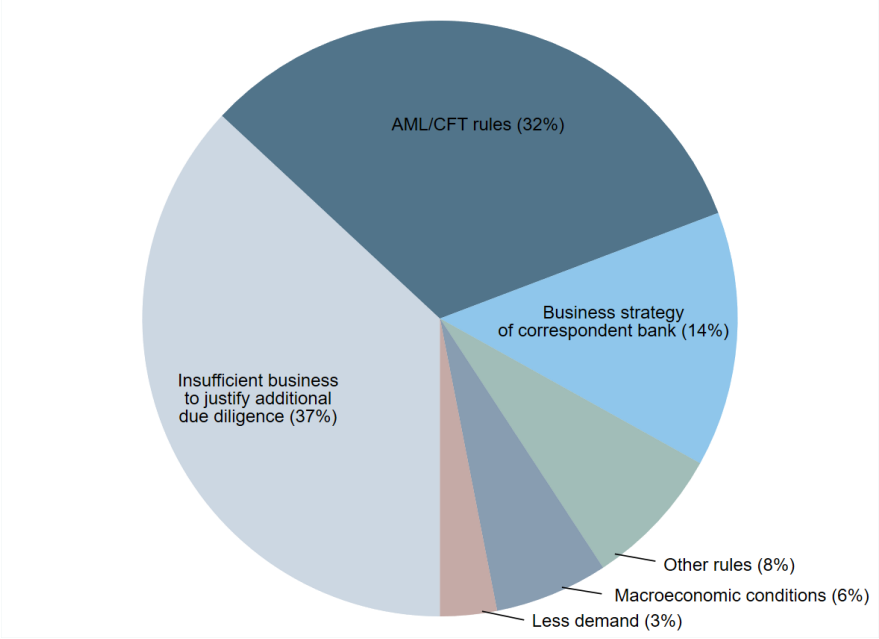


Figure 3: Restricted access to correspondent banking services

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". The question was asked in an online survey together with EBRD's Trade Facilitation Program at the end of 2019. 91 banks across 28 countries answered the question.

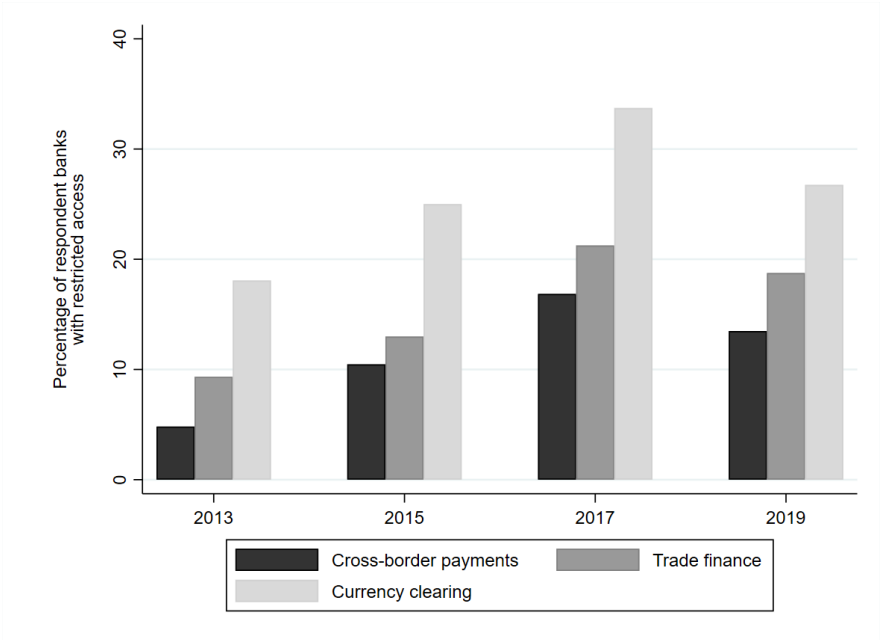


Figure 4: Terminated correspondent bank relationships and firm exports

This figure shows firms' *Export dummy* and *Exports* around the termination of one or more correspondent bank relationships in their locality, compared to control firms. Treated firms are located in a locality in which at least one bank branch lost a correspondent bank relationship. Control firms are located in a locality which has not lost a correspondent bank relationship up to the event-year. Reported coefficients are based on Chaisemartin and D'Haultfœuille 2022 and Borusyak, Jaravel, and Spiess 2022. Regressions include firm controls (*Total assets* and *Total Factor Productivity*), locality-average bank controls (*Local loan growth*, *Equity/Total assets*, *Loans/Customer deposits*, *ROA*), linear country trends and non-parametric industry trends. 95%-confidence intervals are based on standard errors clustered by locality.

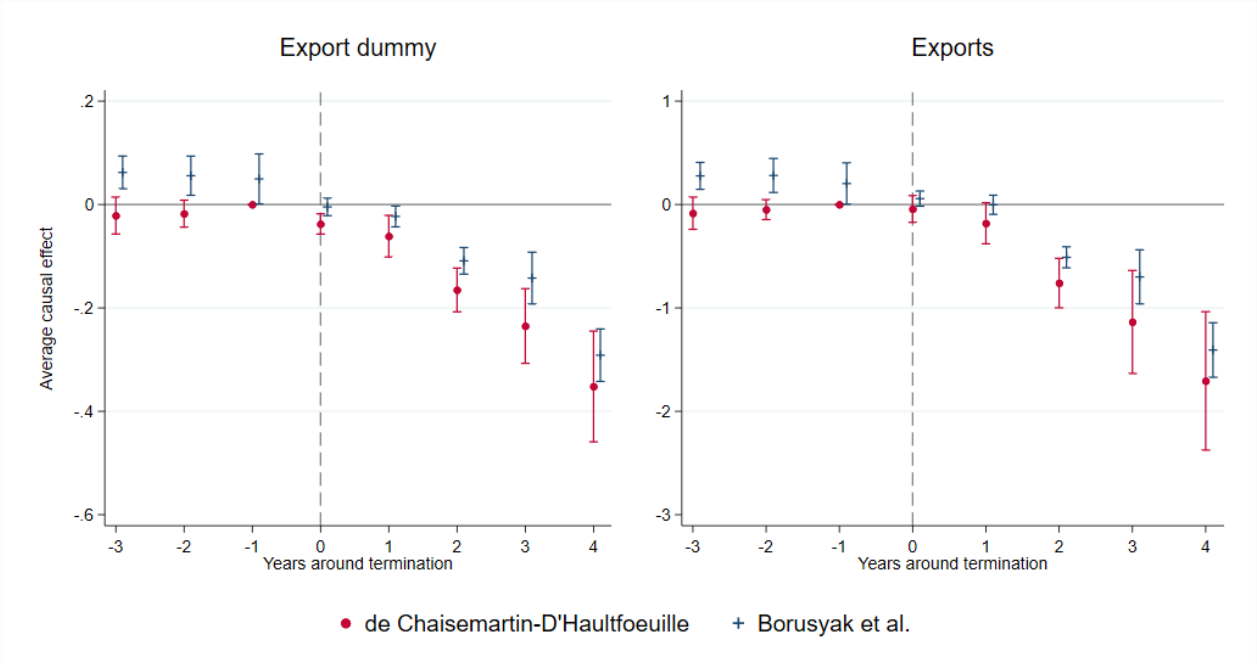


Figure 5: Terminated correspondent bank relationships and firm turnover

This figure shows firms' *Domestic turnover* and *Turnover* around the termination of one or more correspondent bank relationships in their locality, compared to control firms. Treated firms are located in a locality in which at least one bank branch has lost a correspondent bank relationship. Control firms are located in a locality that has not lost a correspondent bank relationship up to the event year. Reported coefficients are based on Chaisemartin and D'Haultfœuille 2022 and Borusyak, Jaravel, and Spiess 2022. 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 linear country trends and non-parametric industry trends. 95%-confidence intervals are based on standard errors clustered by locality.

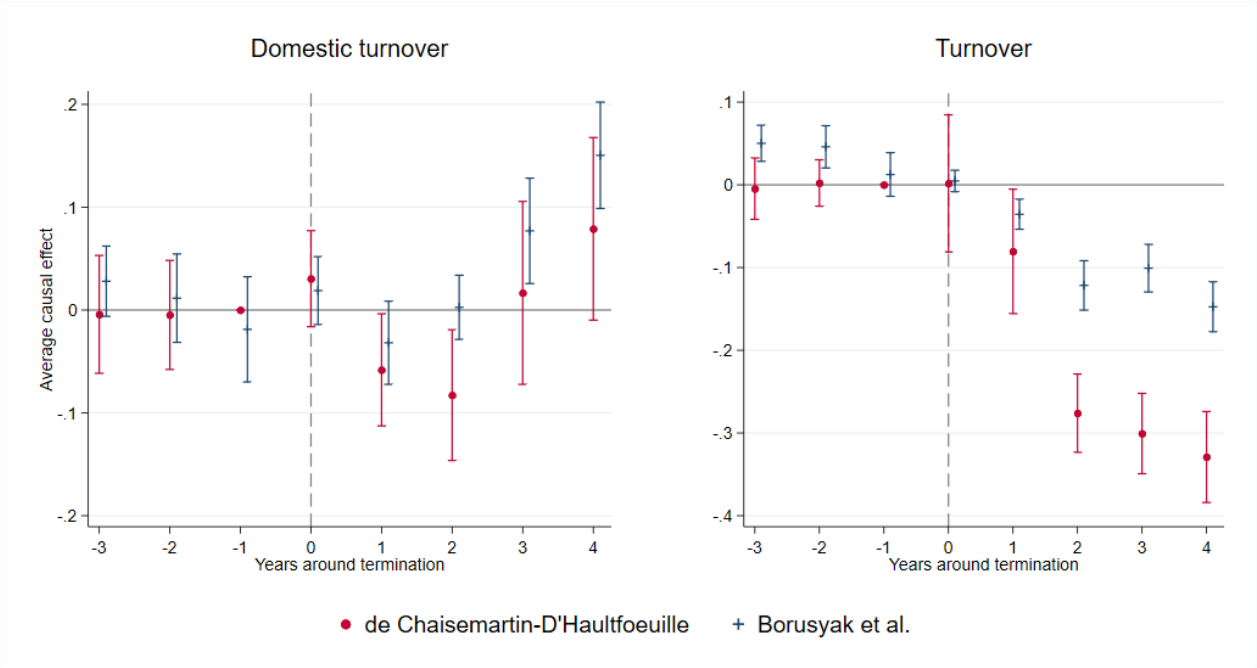


Figure 6: Terminated correspondent bank relationships and firm employment

This figure shows firms' *Employees* 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 branch lost a correspondent bank relationship. Control firms are located in a locality that has not lost a correspondent bank relationship up to the event year. Reported coefficients are based on Chaisemartin and D'Haultfœuille 2022 and Borusyak, Jaravel, and Spiess 2022. 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 linear country trends and non-parametric industry trends. 95%-confidence intervals are based on standard errors clustered by locality.

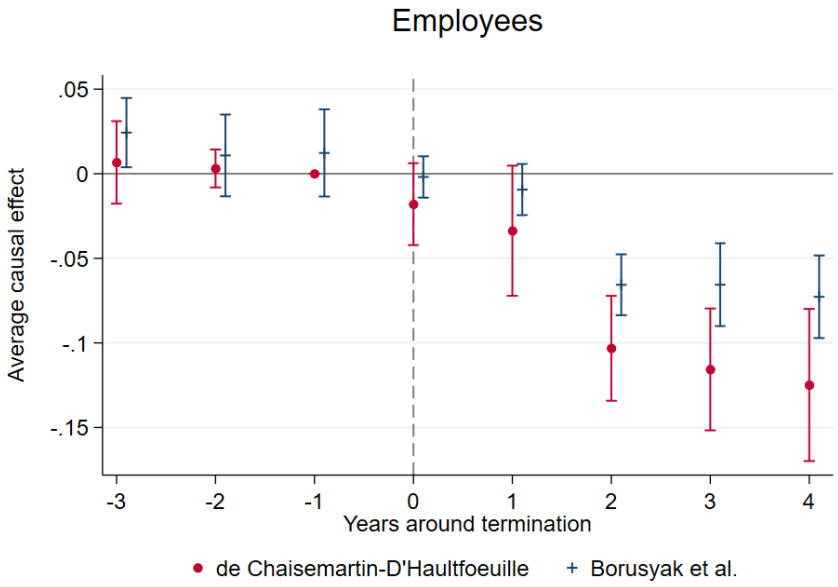


Figure 7: Terminated correspondent bank relationships and exports: Bank-firm matching

This figure shows firms' *Export dummy* and *Exports* around the termination of one or more correspondent bank relationships. Treated (control) firms have a main lender that has (not) lost a correspondent bank relationship up to the event year. Information on firms' main lenders is taken from Bureau Van Dijk's Orbis database. Reported coefficients are based on Chaisemartin and D'Haultfœuille 2022 and Borusyak, Jaravel, and Spiess 2022. Regressions include firm controls (*Total assets* and *Total Factor Productivity*), locality-average bank controls (*Local loan growth*, *Equity/Total assets*, *Loans/Customer deposits*, *ROA*), linear country trends, and non-parametric industry trends (upper graphs) or linear locality trends and non-parametric industry trends, respectively (lower graphs). 95%-confidence intervals are based on standard errors clustered by bank.

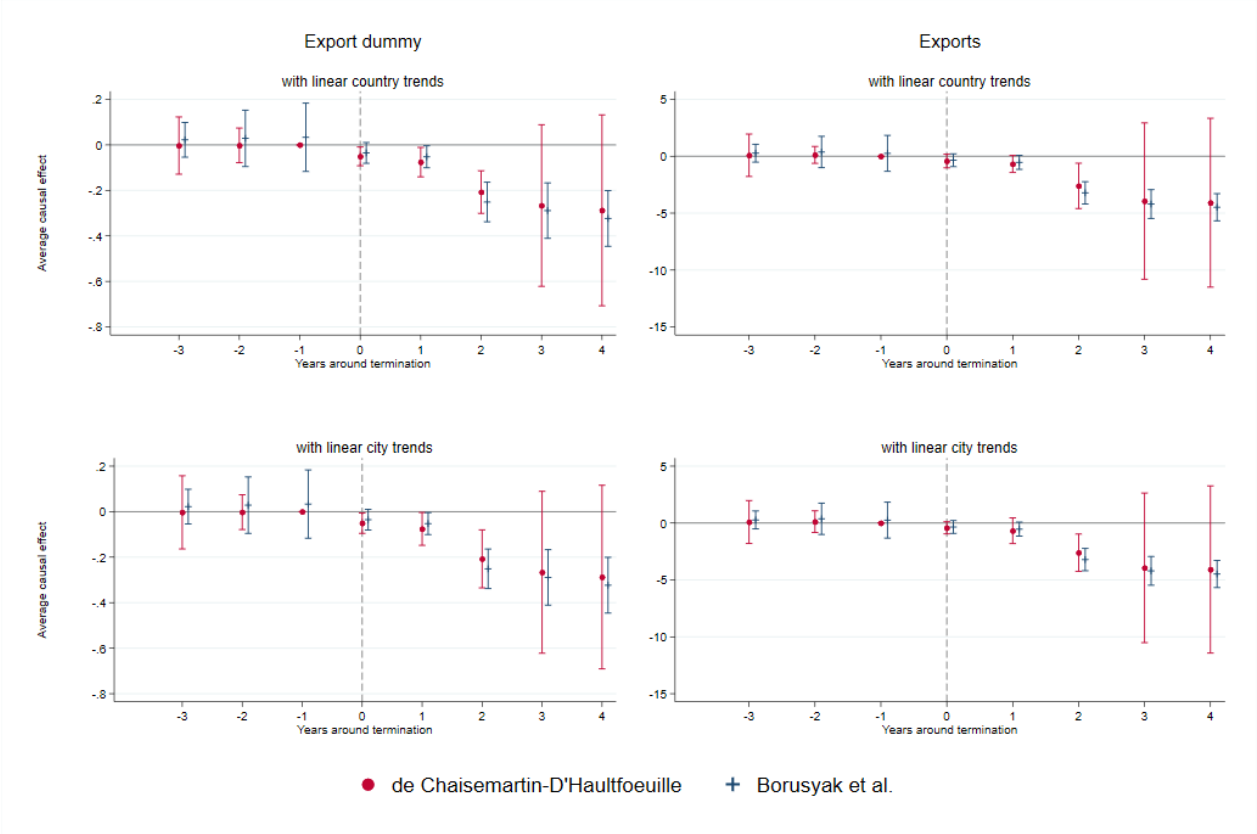


Figure 8: Terminated correspondent bank relationships and turnover: Bank-firm matching

This figure shows firms' *Domestic turnover* and *Turnover* around the termination of one or more correspondent bank relationships. Treated (control) firms have a main lender that has (not) lost a correspondent bank relationship up to the event year. Information on firms' main lenders is taken from Bureau Van Dijk's Orbis database. Reported coefficients are based on Chaisemartin and D'Haultfœuille 2022 and Borusyak, Jaravel, and Spiess 2022. Regressions include firm controls (*Total assets* and *Total Factor Productivity*), locality-average bank controls (*Local loan growth*, *Equity/Total assets*, *Loans/Customer deposits*, *ROA*), linear country trends, and non-parametric industry trends (upper graphs) or linear locality trends and non-parametric industry trends, respectively (lower graphs). 95%-confidence intervals are based on standard errors clustered by bank.

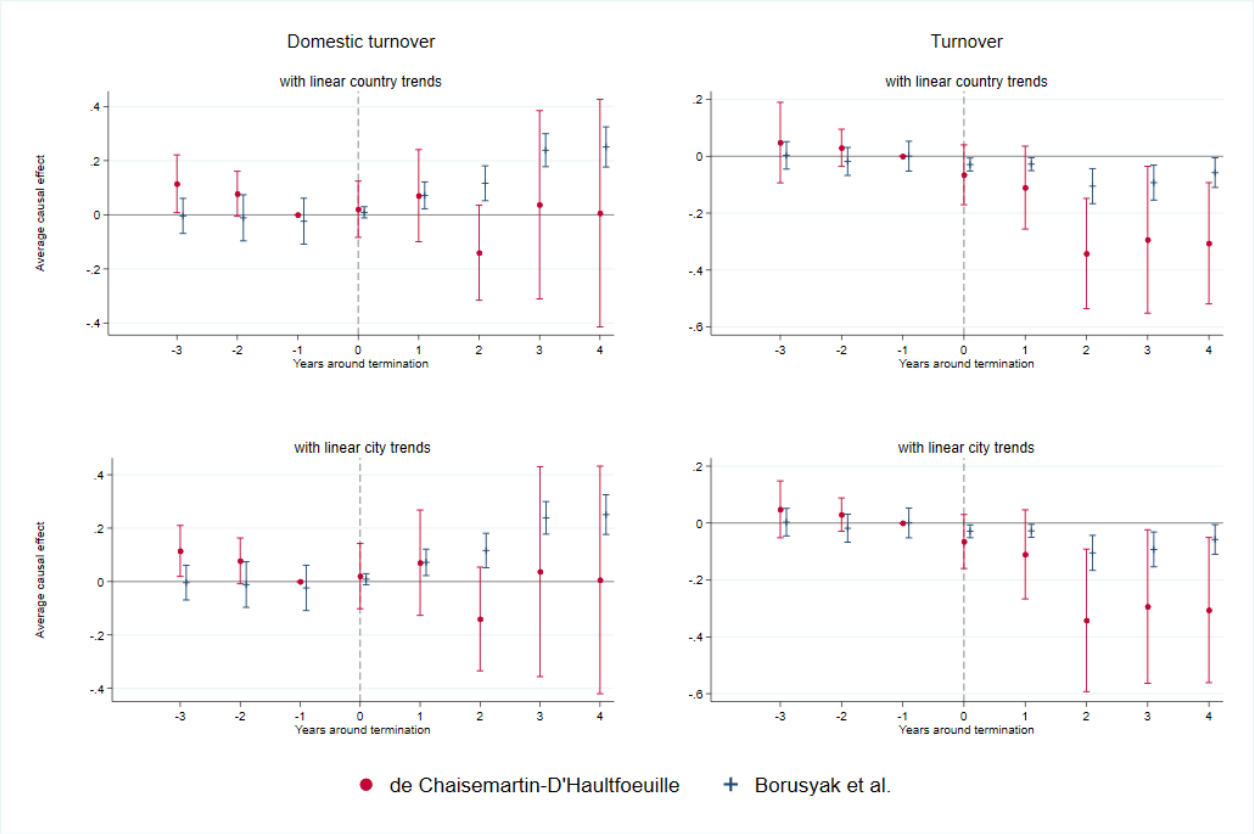


Figure 9: Terminated correspondent bank relationships and employees: Bank-firm matching

This figure shows firms' *Employees* around the termination of one or more correspondent bank relationships. Treated (control) firms have a main lender that has (not) lost a correspondent bank relationship up to the event year. Information on firms' main lenders is taken from Bureau Van Dijk's Orbis database. Reported coefficients are based on Chaisemartin and D'Haultfœuille 2022 and Borusyak, Jaravel, and Spiess 2022. Regressions include firm controls (*Total assets* and *Total Factor Productivity*), locality-average bank controls (*Local loan growth*, *Equity/Total assets*, *Loans/Customer deposits*, *ROA*), linear country trends, and non-parametric industry trends (upper graphs) or linear locality trends and non-parametric industry trends, respectively (lower graphs). 95%-confidence intervals are based on standard errors clustered by bank.

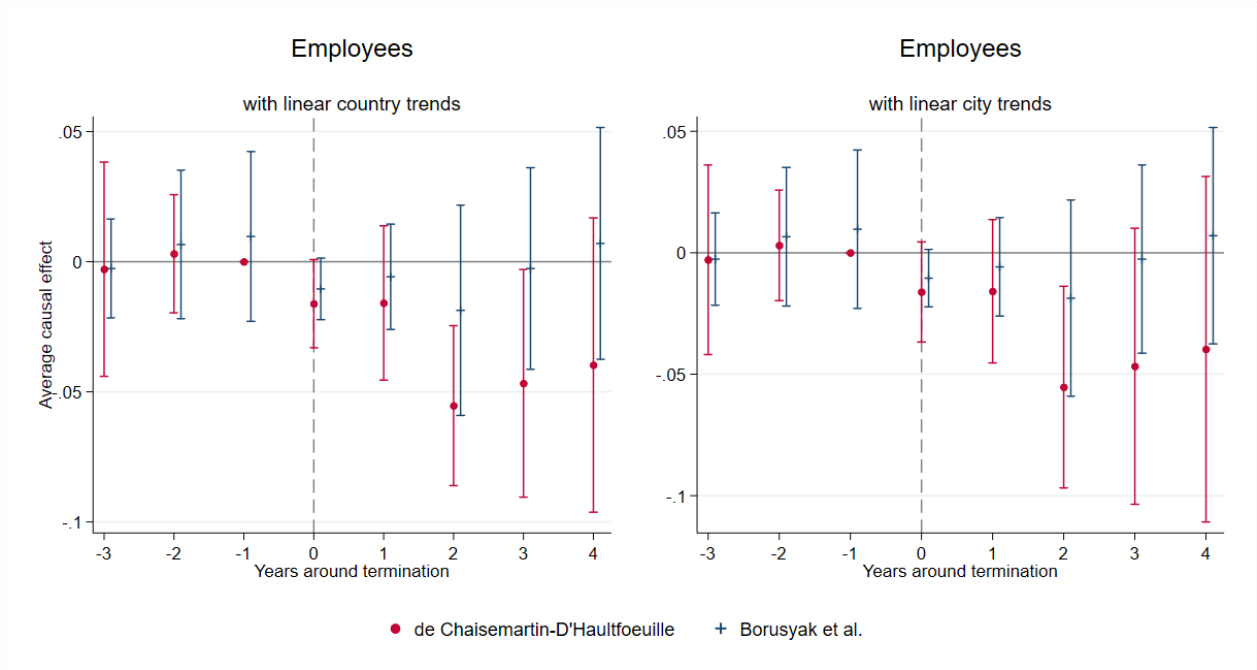


Figure 10: Terminated correspondent bank relationships and exports: Continuous treatment

This figure shows firms' *Export dummy* and *Exports* around the termination of one or more correspondent bank relationships in their locality, compared to control firms. The continuous treatment variable is the number of terminated correspondent bank relationships, divided by the number of bank branches in a locality. Reported coefficients are based on Chaisemartin and D'Haultfœuille 2022. Regressions include firm controls (*Total assets* and *Total Factor Productivity*), locality-average bank controls (*Local loan growth*, *Equity/Total assets*, *Loans/Customer deposits*, *ROA*), linear country trends, and non-parametric industry trends. 95%-confidence intervals are based on standard errors clustered at the locality level.

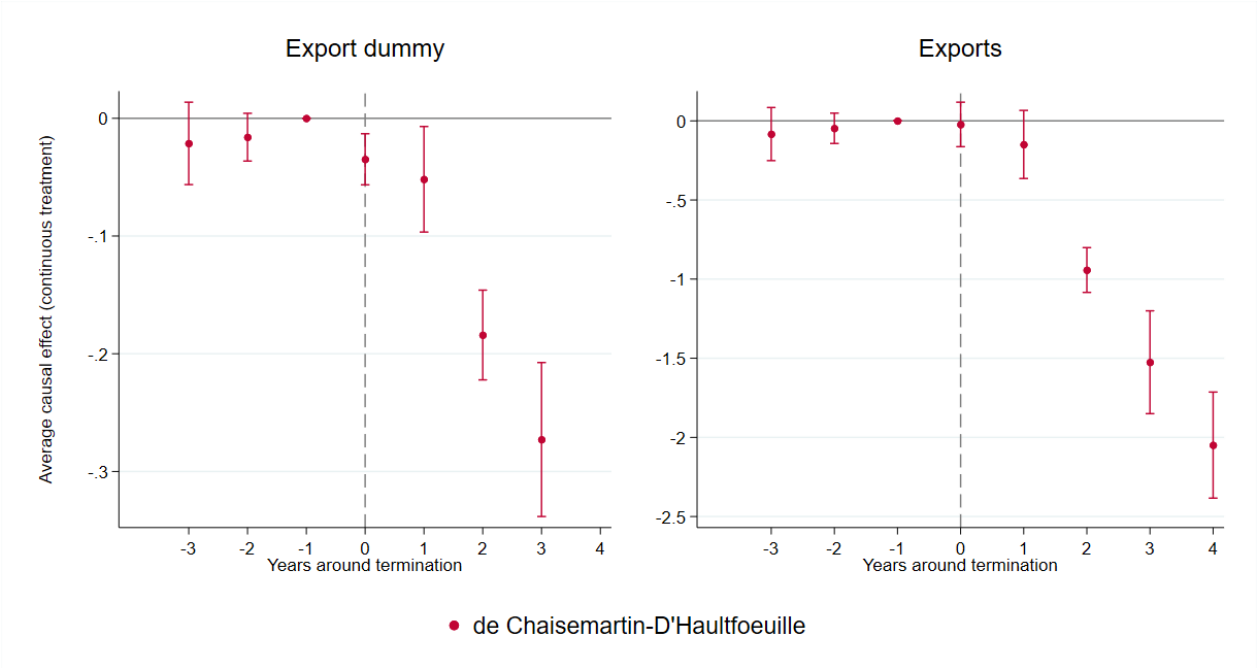


Figure 11: Terminated correspondent bank relationships and turnover: Continuous treatment

This figure shows firms' *Domestic turnover* and *Turnover* around the termination of one or more correspondent bank relationships in their locality, compared with control firms. The continuous treatment variable is the number of terminated correspondent bank relationships, divided by the number of bank branches in a locality. Reported coefficients are based on Chaisemartin and D'Haultfœuille 2022. Regressions include firm controls (*Total assets* and *Total Factor Productivity*), locality-average bank controls (*Local loan growth*, *Equity/Total assets*, *Loans/Customer deposits*, *ROA*), linear country trends, and non-parametric industry trends. 95%-confidence intervals are based on standard errors clustered at the locality level.

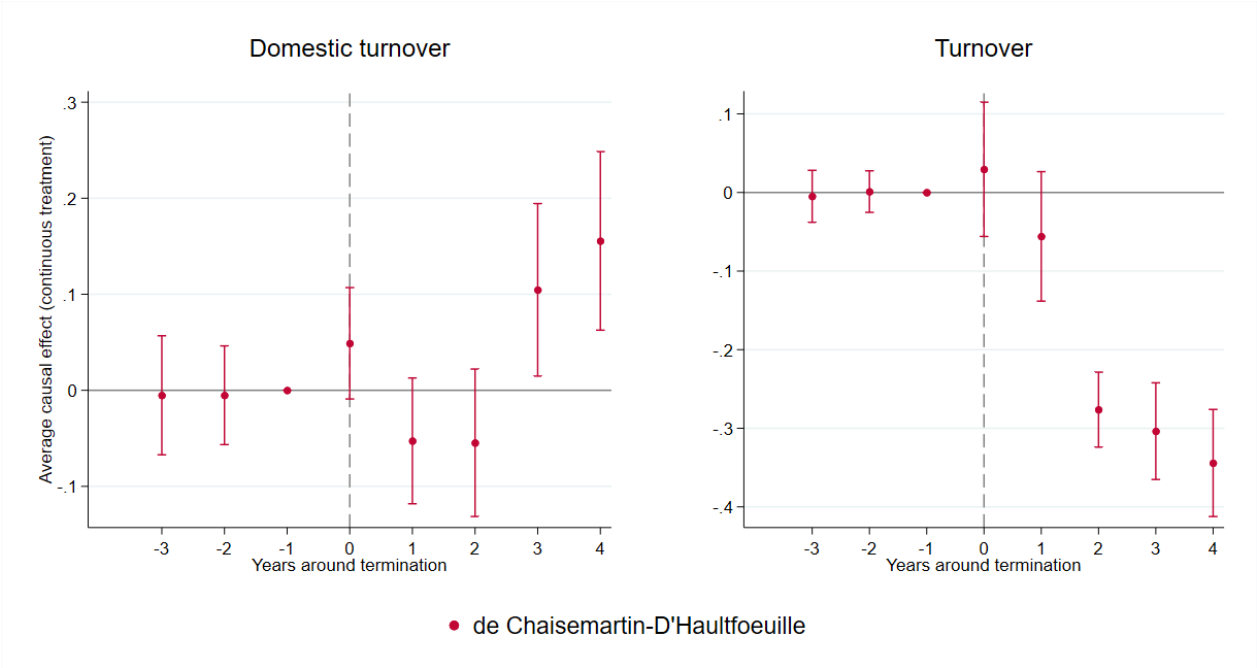


Figure 12: Terminated correspondent bank relationships and employees: Continuous treatment

This figure shows firms' *Employees* around the termination of one or more correspondent bank relationships in their locality, compared with control firms. The continuous treatment variable is the number of terminated correspondent bank relationships, divided by the number of bank branches in a locality. Reported coefficients are based on Chaisemartin and D'Haultfœuille 2022. Regressions include firm controls (*Total assets* and *Total Factor Productivity*), locality-average bank controls (*Local loan growth*, *Equity/Total assets*, *Loans/Customer deposits*, *ROA*), linear country trends, and non-parametric industry trends. 95%-confidence intervals are based on standard errors clustered at the locality level.

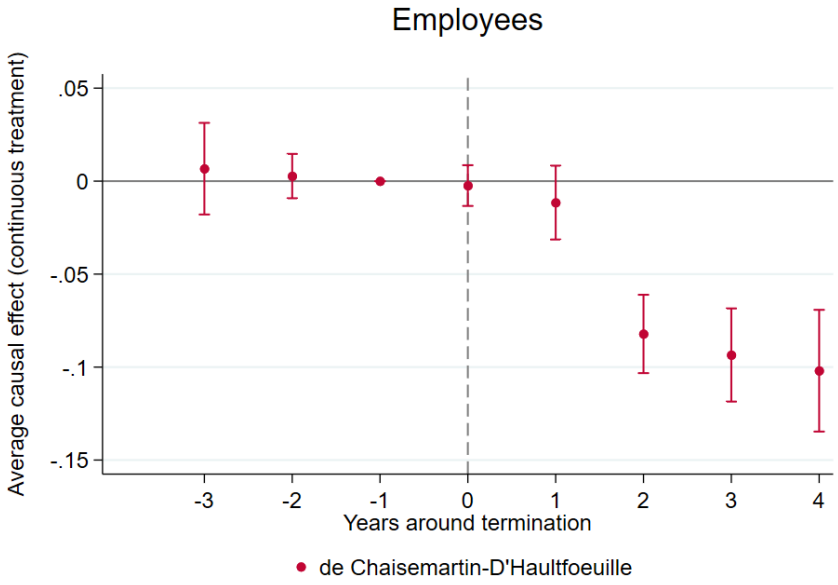
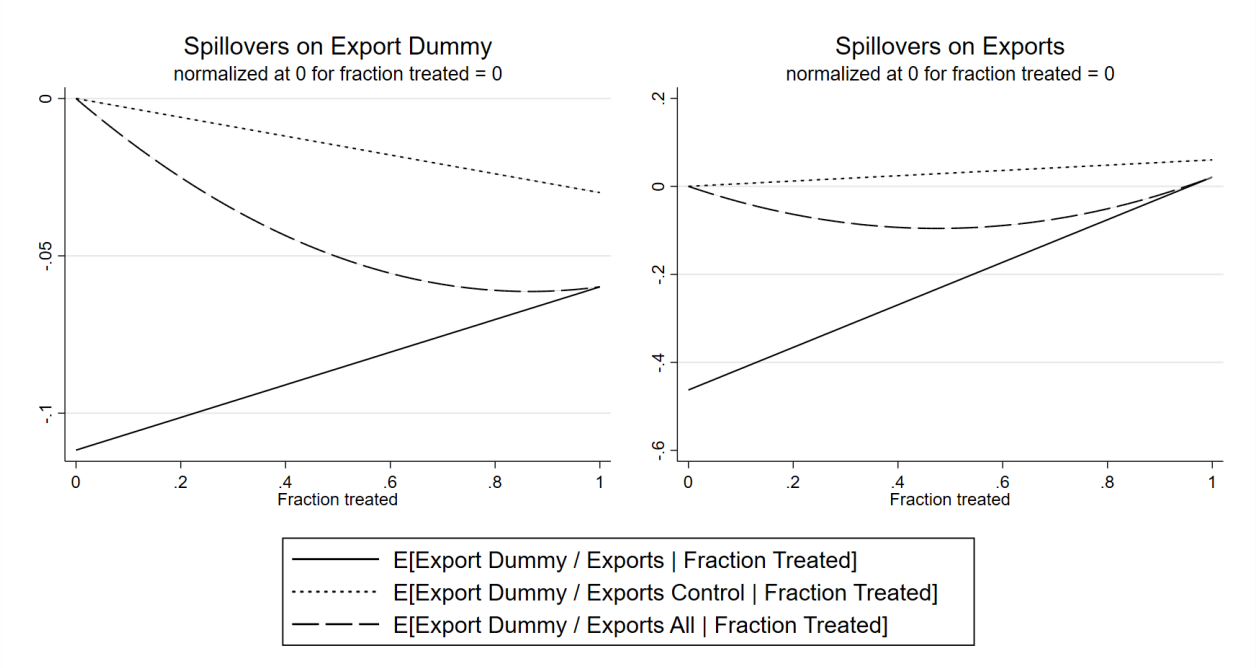


Figure 13: Industry spillovers

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* (left panel) and *Exports* (right panel) 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) depending on the fraction of treated firms.



Tables

Table 1: Treatment-control balance in the full and matched samples

This table shows characteristics of treated and control firms in the full and matched samples of exporters in the year before treatment. Treated firms are located in a locality in which at least one bank branch lost a correspondent banking relationship. Control firms are located in a locality that did not lose a correspondent banking relationship throughout the sample period (complete sample) or that has not lost a correspondent banking relationship up to the event year (matched sample), respectively. To each treated firm, we match one control firm from the same industry and country that also exports in the pre-event year and that is similar in terms of *Exports*, *Total assets* and *Total Factor Productivity* (lowest Mahalanobis distance). For each covariate, we report the normalized difference following Imbens and Wooldridge 2009. A firm can appear several times in this table because a treated firm can be a matched control firm before it gets treated, and it can serve as a control for different treated firms in different years. Therefore, the numbers in this table do not add up to the total number of firms in our sample, which is 23,751.

PANEL A: FULL SAMPLE

	Firm characteristics					**Bank characteristics**
	Exports	Total assets	TFP	N Employees	Age	Cut relationships (branch level) over branches in locality
	(1)	(2)	(3)	(4)	(5)	(6)
Treated firms (N= 21,965)						
Mean	1,345	2,633	0.287	30.2	13.6	0.561
Median	87	633	0.274	8.0	12.0	0.262
SD	3,304	4,021	0.856	45.4	10.4	0.589
Control firms (N= 13,149)						
Mean	1,300	3,266	0.125	15.5	12.0	0
Median	80	1,209	0.133	4.0	10.0	0
SD	3,211	4,294	0.803	30.2	10.6	-
t(Difference)	1.26	-13.91	16.99	18.29	-12.96	109.31
Normalized difference (Imbens-Wooldridge)	0.010	-0.108	0.138	0.269	0.102	-

PANEL B: MATCHED SAMPLE

	Firm characteristics					**Bank characteristics**
	Exports	Total assets	TFP	N Employees	Age	Cut relationships (branch level) over branches in locality
	(1)	(2)	(3)	(4)	(5)	(6)
Treated firms (N= 19,906)						
Mean	2,480	5,565	0.283	47.7	14.2	0.565
Median	103	777	0.272	8.5	13.0	0.262
SD	9,357	16,615	0.860	120.6	10.4	0.593
Control firms (N= 19,905)						
Mean	2,753	6,571	0.244	51.3	14.3	0
Median	168	1,419	0.213	12.0	13.0	0
SD	9,510	17,157	0.830	115	10.9	-
t(Difference)	-1.91	-3.95	2.99	-1.67	-1.03	70.72
Normalized difference (Imbens-Wooldridge)	-0.021	-0.042	0.032	-0.022	-0.011	-

Table 2: Summary statistics matched sample

This table shows firm and bank characteristics of the matched sample of exporters in the year before treatment. The bank-firm connection is established by firms and bank branches in the same locality. To each treated firm, we match one control firm from the same industry and country that also exports in the pre-event year, and is similar in terms of *Exports*, *Total assets* and *Total Factor Productivity* (lowest Mahalanobis distance). Bank characteristics are the branch-weighted average per locality.

	(1) Unit	(2) N	(3) Mean	(4) Median	(5) Min	(6) Max	(7) SD
Firm-variables (23,751 firms over a sample period of 10 years)							
Exports	1,000 Euros	23,751	2,430	106	0	75,046	8,976
Total assets	1,000 Euros	23,751	5,667	830	2.6	128,870	16,530
Total Factor Productivity		23,751	0.3	0.3	-13.6	8.2	0.8
Employees	N	19,694	45.7	8.0	1.0	864.0	116.0
Age	Years	22,848	14.2	13.0	0.0	164.0	10.6
Bank-variables (averaged at the locality level, 706 localities)							
Total assets	Mill. Euros	23,708	13,204	6,647	125	71,826	14,083
Local loan growth	%	23,751	6.2	8.3	-15.7	40.0	7.6
Equity/Total assets	%	23,708	11.8	11.9	8.2	29.5	1.7
Loans/Customer deposits	%	23,708	73.2	69.0	34.8	167.4	9.8
ROA	%	23,708	0.7	0.9	-4.2	2.1	0.8

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

This table shows dynamic difference-in-differences estimates for firms' *Export dummy*, *Exports*, *Turnover Domestic turnover*, and *Employees* around the termination of one or more correspondent bank relationships in a firm's locality, and compared with unaffected control firms, using the Chaisemartin and D'Haultfœuille 2022 estimator. Treated firms are based in a locality where at least one bank branch lost a correspondent bank relationship. Control firms are based in a locality that did not lose a correspondent bank relationship up to the event year. We match each treated firm to one control firm in the same industry and country that has similar *Exports*, *Total assets* and *Total Factor Productivity* in the pre-event year. Firm controls include *Total assets* and *Total Factor Productivity*; bank controls include *Local loan growth*, *Equity/Total assets*, *Loans/Customer deposits*, and *ROA*. Standard errors are clustered at the locality level and shown in parentheses.

	Exports		Turnover		Employees
	Dummy	Amount	All	Domestic	
	(1)	(2)	(3)	(4)	(5)
Effect at t=0	-0.038***	-0.043	0.002	0.030	-0.018
	(0.010)	(0.066)	(0.042)	(0.024)	(0.012)
Effect at t=1	-0.061***	-0.182	-0.081**	-0.058**	-0.034*
	(0.021)	(0.101)	(0.038)	(0.029)	(0.020)
Effect at t=2	-0.165***	-0.760***	-0.276***	-0.083**	-0.103***
	(0.022)	(0.122)	(0.024)	(0.032)	(0.016)
Effect at t=3	-0.235***	-1.136***	-0.301***	0.017	-0.116***
	(0.037)	(0.254)	(0.025)	(0.045)	(0.018)
Effect at t=4	-0.352***	-1.706***	-0.329***	0.079	-0.125***
	(0.055)	(0.342)	(0.028)	(0.045)	(0.023)
Placebo at t=-2	-0.018	-0.049	0.002	-0.005	0.003
	(0.013)	(0.049)	(0.014)	(0.027)	(0.006)
Placebo at t=-3	-0.021	-0.084	-0.005	-0.004	0.007
	(0.018)	(0.079)	(0.019)	(0.029)	(0.012)
$\beta_{t=0}$ based on N firm-years	96,105	91,741	96,105	91,405	84,418
$\beta_{t=0}$ based on N switchers	21,289	18,900	21,289	18,810	19,325
Firm and bank controls	Yes	Yes	Yes	Yes	Yes
NP industry trends	Yes	Yes	Yes	Yes	Yes
Linear country trends	Yes	Yes	Yes	Yes	Yes
Pre-event mean	1.00	4.73	6.92	5.98	2.49

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

Table 4: Terminated correspondent bank relationships and firm-level outcomes: Static effects

This table shows static TWFE estimates (top), static Chaisemartin and D'Haultfœuille 2022 estimates (middle) and static Borusyak, Jaravel, and Spiess 2022 estimates (bottom) for firms' *Export dummy* (columns 1–3) and *Exports* (columns 4–6) around the termination of one or more correspondent bank relationships in a firm's locality and compared with unaffected control firms. Treated firms are based in a locality where at least one bank branch lost a correspondent bank relationship. Control firms are based in a locality that did not lose a correspondent bank relationship up to the event year. We match each treated firm to one control firm in the same industry and country that has similar *Exports*, *Total assets* and *Total Factor Productivity* in the pre-event year. All specifications include firm and year fixed effects or apply the difference-in-differences approach of Chaisemartin and D'Haultfœuille 2022 or Borusyak, Jaravel, and Spiess 2022. All specifications include time-varying firm and bank controls (except column 1 and 4). Firm controls include *Total assets* and *Total Factor Productivity*; bank controls include *Local loan growth*, *Equity/Total assets*, *Loans/Customer deposits*, and *ROA*. Columns 3 and 6 also include country FE or linear country trends for dCdH (2022) respectively; industry x year FE or non-parametric industry trends for dCdH (2022), respectively. Standard errors are clustered at the locality level and shown in parentheses. Note that the number of firm-years used to estimate the treatment effect by Chaisemartin and D'Haultfœuille 2022 is smaller than the number of firm-years reported for the OLS estimator and for Borusyak, Jaravel, and Spiess 2022. As the Chaisemartin and D'Haultfœuille 2022 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 with a valid control or that are valid controls of a treated firm.

	Exports					
	Dummy			Amount		
	(1)	(2)	(3)	(4)	(5)	(6)
OLS estimator	-0.069***	-0.065***	-0.066***	-0.206***	-0.157**	-0.165***
	(0.012)	(0.015)	(0.016)	(0.065)	(0.062)	(0.064)
Firm-years	218,357	183,084	183,084	184,541	156,371	156,369
dCdH (2022)	-0.055***	-0.047***	-0.038***	-0.053	-0.065	-0.042
	(0.007)	(0.008)	(0.011)	(0.061)	(0.049)	(0.072)
Firm-years	119,907	112,728	112,728	110,729	105,815	105,815
Switchers	22,122	21,327	21,327	19,199	18,938	18,938
Borusyak, Jaravel, and Spiess 2022	-0.155***	-0.097***	-0.101***	-0.481***	-0.319***	-0.348***
	(0.015)	(0.017)	(0.016)	(0.075)	(0.068)	(0.065)
Firm-years	218,547	206,966	206,962	187,006	181,074	181,070
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Firm and bank controls	No	Yes	Yes	No	Yes	Yes
Country FEs / Linear country trends	No	No	Yes	No	No	Yes
Industry × Year FEs / NP industry trends	No	No	Yes	No	No	Yes

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

Table 5: Terminated correspondent bank relationships and sector-level outcomes: Exports

This table reports the results for standard difference-in-difference regressions of export growth. The dependent variable is the first difference in log exports of industry s in country i in emerging Europe to country j in the rest of the world at time t. Sector-level exports are aggregated into the 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 dependent variable is winsorized at the 5 percent level. The withdrawal of correspondent banks is measured as the percent change in active correspondent banks (FSB 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)*; *Log GDP Counterparty*, *Log Distance*, *Prop. USD Exports*, $\Delta LogUSD/EUR$). Standard errors are clustered at the country-partner level and shown in parentheses.

	Δ Log Exports					
	(1)	(2)	(3)	(4)	(5)	(6)
High Withdrawal × Post	-0.069**	-0.069**	-0.070**	-0.078***	-0.061**	-0.083***
	(0.031)	(0.031)	(0.032)	(0.030)	(0.029)	(0.032)
Post	-0.090***	-0.089***	-0.088**			
	(0.018)	(0.018)	(0.038)			
Δ Log Exports (World)	1.275***	1.289***	1.289***	1.277***	1.281***	
	(0.022)	(0.022)	(0.022)	(0.022)	(0.021)	
Log GDP Counterparty	0.018***	0.018***	0.018***			
	(0.003)	(0.003)	(0.003)			
Log Distance	0.002	0.005	0.005	0.025*		
	(0.005)	(0.005)	(0.005)	(0.013)		
Prop. USD Exports × Δ Log USD/EUR			0.187			
			(4.194)			
Observations	51,446	51,446	51,446	52,980	55,773	55,773
Industry FEs	No	Yes	Yes	Yes	Yes	No
Industry × Year FEs	No	No	No	No	No	Yes
Country FEs	Yes	Yes	Yes	Yes	No	No
Partner × Year FEs	No	No	No	Yes	Yes	Yes
Pair FEs	No	No	No	No	Yes	Yes
Pre-event mean	0.15	0.15	0.15	0.15	0.15	0.15

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

Table 6: Terminated correspondent bank relationships and sector-level outcomes: Imports

This table reports the results for standard difference-in-difference regressions of import growth. The dependent variable is the first difference in log imports of industry s in country i in emerging Europe from country j in the rest of the world at time t. Sector-level imports are aggregated into the pre-period observation [July 2013-June 2014] and post-period observation [July 2014-June 2015], and first differences are calculated relative to aggregate imports in the preceding 12 months, respectively. The dependent variable is winsorized at the 5 percent level. The withdrawal of correspondent banks is measured as the percent change in active correspondent banks (FSB 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 ($\Delta LogImports\ (World)$; *Log GDP Counterparty*, *Log Distance*, *Prop. USD Exports*, $\Delta LogUSD/EUR$). Standard errors are clustered at the country-partner level and shown in parentheses.

	Δ Log Imports					
	(1)	(2)	(3)	(4)	(5)	(6)
High Withdrawal × Post	-0.170***	-0.169***	-0.150***	-0.170***	-0.177***	-0.239***
	(0.026)	(0.026)	(0.027)	(0.025)	(0.024)	(0.027)
Post	-0.002	-0.002	-0.080**			
	(0.018)	(0.018)	(0.038)			
Δ Log Imports (World)	1.156***	1.166***	1.166***	1.171***	1.156***	
	(0.021)	(0.022)	(0.022)	(0.021)	(0.022)	
Log GDP Counterparty	0.009***	0.009***	0.009***			
	(0.003)	(0.003)	(0.003)			
Log Distance	-0.020***	-0.020***	-0.019***	0.008		
	(0.005)	(0.005)	(0.005)	(0.013)		
Prop. USD Exports × Δ Log USD/EUR			-9.130**			
			(4.053)			
Observations	52,979	52,979	52,979	54,399	57,332	57,332
Industry-FEs	No	Yes	Yes	Yes	Yes	No
Industry-time-FEs	No	No	No	No	No	Yes
Country-FEs	Yes	Yes	Yes	Yes	No	No
Partner-time-FEs	No	No	No	Yes	Yes	Yes
Pair-FEs	No	No	No	No	Yes	Yes
Pre-event mean	0.08	0.08	0.08	0.08	0.08	0.08

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

Appendix

A Changes in correspondent bank relationships

Table A1: Changes in correspondent bank relationships 2011-2022

This table shows the percent changes in correspondent bank relationships (measured as counterparties abroad) in the period 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%
Republic of Serbia	-39.0%
Romania	-26.7%
Russia	-33.4%
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 variable equal to one if firm has export revenues in a given year	Orbis
Exports	Revenues from a firm's export activities in log 1,000 euros	Orbis
Turnover	Total operating revenues in log 1,000 euros	
Domestic turnover	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 of this function are the log number of employees and log total assets to account for labor and capital, and the output is log turnover	Own calculation based on Orbis
Firm age	Firm age in years	Orbis
Industry	NACE Rev. 2 classification	Orbis
Locality	Village, town, or city of firm headquarters	Orbis
	Panel B: Bank level (branch-weighted averaged by locality)	
Lost relationship	Dummy that equals one if at least one bank branch in locality has lost a correspondent banking relationship up to year t.	BEPS III and EBRD TFP survey
Cut relationships	Number of terminated correspondent bank relationships in a locality up to year t (on bank branch level) divided by total number of bank branches in a locality	BEPS III and EBRD TFP survey
Branch network	Locations (cities) of all branches of a bank	BEPS III
Loan growth	Percentage 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 C: Locality level

Nightlight	Global VIIRS Nighttime Lights Derived from Monthly Averages, following Elvidge et al. 2021	NASA/NOAA VIIRS

Panel D: Country level

Post	Dummy variable taking value 0 in the pre-period [July 2013-June 2014] and 1 in the post-period [July 2013-June 2014]	
High Withdrawal	Dummy variable taking value 1 if country faces above median reduction in number of correspondent banks over the period Jan. 2011-Jun. 2016, and 0 otherwise	FSB 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 exports 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 which 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

This section relates to correspondent banks.

- **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) in 2019

- **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 tests

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

This table reports OLS specifications that regress our locality-level treatment variables on various locality, firm, and bank characteristics. Variables are in levels and averaged at the locality level. Standard errors are clustered at the country level and shown in parentheses.

	Correspondent bank withdrawal			
	Binary		Continuous	
	(1)	(2)	(3)	(4)
Locality characteristics				
Nightlight (t-1)	-0.568	0.565	-1.035	1.866
	(0.322)	(1.438)	(0.549)	(2.815)
Firm characteristics (averaged at locality level)				
Number of firms (t-1)	0.000	0.004	-0.001**	0.005
	(0.000)	(0.004)	(0.000)	(0.004)
Total assets (t-1)	0.546	0.269*	0.536	0.293
	(0.446)	(0.105)	(0.565)	(0.220)
Productivity (t-1)	1.681	-1.253	4.288	3.985
	(2.042)	(1.933)	(3.439)	(3.101)
Turnover (t-1)	0.313	0.709	-0.205	-1.197
	(1.045)	(0.311)	(1.757)	(1.066)
Employees (t-1)	-0.005	0.001	-0.009	-0.001
	(0.004)	(0.001)	(0.007)	(0.003)
HHI	-0.189*	0.030	-0.113	0.078
	(0.073)	(0.026)	(0.090)	(0.047)
Bank characteristics (averaged at locality level)				
Total assets (t-1)	0.046	0.044	0.089	0.083
	(0.070)	(0.033)	(0.116)	(0.051)
Equity/Total assets (t-1)	0.226	1.314	-4.816	3.095
	(1.974)	(4.182)	(4.080)	(9.380)
Loans/Deposits (t-1)	-0.141	0.515*	0.611	1.813**
	(0.771)	(0.216)	(1.529)	(0.466)
Gross loans (t-1)	-0.086	-0.040	-0.164	-0.081
	(0.108)	(0.030)	(0.185)	(0.046)
HHI	-0.344**	-3.488**	-0.130	-4.517**
	(0.082)	(1.002)	(0.078)	(1.309)
F	2.28	0.23	2.87	1.00
Prob > F	0.258	0.871	0.205	0.500
Observations	6,682	6,670	6,680	6,668
R^2	0.28	0.79	0.25	0.82
Locality FE	No	Yes	No	Yes

Standard errors in parentheses

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

E Heterogeneity of treatment effects

We use the estimator by Chaisemartin and D'Haultfœuille 2022 to account for the fact that heterogeneous and dynamic treatment effects might bias the estimates of a conventional TWFE model. This appendix presents tests indicating that heterogeneous treatment effects may indeed be a problem in our setting.

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

We use Corollary 1 of Chaisemartin and D'Haultfœuille 2020 to test whether, in our setting, treatment heterogeneity across firms and years gives rise to such concerns. Corollary 1 (i) defines $\underline{\sigma}$ as the minimal value of the standard deviation of the treatment effect across the treated groups and time periods under which beta and the average treatment effect on the treated (ATT) could be of opposite signs. Corollary 1 (ii) defines $\underline{\underline{\sigma}}$ as the minimal value of the standard deviation of the treatment effect across the treated groups and time periods under which beta could be of a different sign than the treatment effect in all the treated groups and time periods.

Table E1 below reports the estimated $\hat{\underline{\sigma}}$ and $\hat{\underline{\underline{\sigma}}}$ based on our baseline regressions that regress *Export dummy* and *Exports*, respectively, on our treatment indicator, control variables, and firm and year fixed effects. In the model with *Export dummy* as our dependent variable, $\hat{\underline{\sigma}} = 0.06$. This suggests that the ATT and the estimated beta may be of opposite sign if the standard deviation of the treatment effect across the treated groups and time periods was 0.06 or higher.

To assess if this is a reasonable value for treatment effect heterogeneity in our setting, we follow the thought experiment introduced by Chaisemartin and D'Haultfœuille 2022: if treatment effects of the treated groups and time periods were drawn from a normal distribution around a mean of 0 and with a standard deviation of $\hat{\underline{\sigma}} = 0.06$, 95 percent of the treatment effects would be distributed within the interval $[-0.11, 0.11]$. Compared to our OLS beta estimate for the corresponding linear model of -0.08 (see Table 4, column (2)), this range does not seem unreasonably wide. A standard deviation of 0.06, consequently, is not implausibly high for the treatment effect across treated groups and time periods. For our regressions with *Export dummy* as the dependent variable, heterogeneous treatment effects could thus be a problem, and betas estimated from a linear regression could have the opposite sign as the ATT.

The value of 0.10 for $\underline{\underline{\sigma}}$ indicates that obtaining a beta estimate of a different sign than the treatment effect in all treated groups and time periods is less of a concern in our setting. If, in contrast to our negative estimate, all treatment effects were positive and distributed

uniformly with a standard deviation of 0.10[19], they would lie in the interval $[0, 0.33]$. This interval seems relatively wide, compared to our OLS estimate of -0.08 (Table 4, column (2)).

Following the same argument for the models with *Exports* as our dependent variable, $\underline{\hat{\sigma}} = 0.25$ does not seem unreasonably high either. In the associated normal distribution $N(0, 0.25^2)$, 95 percent of treatment effects would be in the interval $[-0.48, 0.48]$, which seems reasonable compared to our OLS estimate of -0.31 (see Table 4, column (5)). Again, the risk that beta has a different sign than the treatment effect in all the treated groups and time periods seems lower, but not unrealistic. In the associated uniform distribution, the values would be in the range $[0, 0.79]$. This range is relatively wide but still plausible, compared to our estimate of -0.31.

In line with our conclusion that treatment heterogeneity might be a concern in our setting, Table E1 reports that the sum of negative weights is high in both models. This indicates that treatment effects of several treated groups and periods enter negatively in the linear estimator. To alleviate the potential problems arising from these negative weights, we account for possible heterogeneous treatment effects by applying the estimator suggested by Chaisemartin and D'Haultfœuille 2022 and the estimator introduced by Borusyak, Jaravel, and Spiess 2022 throughout the paper.

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 Chaisemartin and D'Haultfœuille 2020. The numbers are based on TWFE regressions of our main dependent variables *Export dummy* and *Exports* on our treatment variable *Lost relationship*, including *Total assets* and *Total Factor Productivity* as firm controls, and *Local loan growth*, *Equity/Total assets*, *Loans/Customer deposits*, and *ROA* as bank controls.

Dependent variable	$\underline{\hat{\sigma}}$	$\underline{\hat{\sigma}}$	Sum of positive weights	Sum of negative weights
Export dummy	0.06	0.10	1.30	-0.30
Exports	0.25	0.23	1.25	-0.25

19. As Corollary 1 (ii) assumes that all treatment effects have the same sign, they cannot be normally distributed. We therefore assume a uniform distribution for this thought experiment.

F Robustness checks and miscellaneous

Table F1: Terminated correspondent bank relationships and firm-level outcomes: Bank-firm matching

This table shows difference-in-differences estimates for firms' *Export dummy* and *Exports* around the termination of one or more correspondent bank relationships in a firm's locality and compared to unaffected control firms, using the Chaisemartin and D'Haultfœuille 2022 estimator. Treated (control) firms have a main lender that has (not) lost a correspondent bank relationship up to the event year. Information on firms' main lenders is taken from Bureau Van Dijk's Orbis database. Reported coefficients are based on Chaisemartin and D'Haultfœuille 2022. Regressions include firm controls (*Total assets* and *Total Factor Productivity*), locality-average bank controls (*Local loan growth*, *Equity/Total assets*, *Loans/Customer deposits*, *ROA*), linear country trends and non-parametric industry trends (upper graphs) or linear locality trends and non-parametric industry trends, respectively (lower graphs). 95%-confidence intervals are based on standard errors clustered by bank.

	Exports				Turnover				Employees	
	Dummy		Amount		All		Domestic			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Effect at t=0	-0.051**	-0.051**	-0.422	-0.422	-0.065	-0.065	0.020	0.020	-0.016	-0.016
	(0.021)	(0.023)	(0.306)	(0.272)	(0.054)	(0.048)	(0.053)	(0.062)	(0.009)	(0.011)
Effect at t=1	-0.076**	-0.076**	-0.679	-0.679	-0.110	-0.110	0.071	0.071	-0.016	-0.016
	(0.033)	(0.037)	(0.382)	(0.576)	(0.074)	(0.080)	(0.087)	(0.101)	(0.015)	(0.015)
Effect at t=2	-0.207***	-0.207***	-2.604	-2.604***	-0.342***	-0.342**	-0.140	-0.140	-0.055***	-0.055**
	(0.048)	(0.065)	(1.016)	(0.835)	(0.099)	(0.128)	(0.090)	(0.099)	(0.016)	(0.021)
Effect at t=3	-0.266	-0.266	-3.938	-3.938	-0.294**	-0.294**	0.037	0.037	-0.047**	-0.047
	(0.181)	(0.181)	(3.504)	(3.353)	(0.132)	(0.138)	(0.177)	(0.201)	(0.022)	(0.029)
Effect at t=4	-0.288	-0.288	-4.083	-4.083	-0.306***	-0.306**	0.006	0.006	-0.040	-0.040
	(0.214)	(0.206)	(3.786)	(3.749)	(0.109)	(0.130)	(0.214)	(0.217)	(0.029)	(0.036)
Placebo at t=-2	-0.002	-0.002	0.118	0.118	0.030	0.030	0.077	0.077	0.003	0.003
	(0.039)	(0.039)	(0.378)	(0.486)	(0.033)	(0.030)	(0.042)	(0.044)	(0.012)	(0.012)
Placebo at t=-3	-0.003	-0.003	0.087	0.087	0.048	0.048	0.115**	0.115**	-0.003	-0.003
	(0.064)	(0.082)	(0.950)	(0.962)	(0.072)	(0.051)	(0.054)	(0.049)	(0.021)	(0.020)
$\beta_{t=0}$ based on:										
N firm-years	42,379	42,379	39,340	39,340	42,379	42,379	39,221	39,221	42,065	42,065
N switchers	6,094	6,094	5,466	5,466	6,094	6,094	5,441	5,441	6,070	6,070
Firm & bank controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
NP industry trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Linear country trends	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Linear city trends	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Pre-event mean	1.00	1.00	11.62	11.62	13.87	13.87	12.74	12.74 2.65	2.65	

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

Table F2: Terminated correspondent bank relationships and firm-level outcomes: Continuous treatment

This table shows dynamic difference-in-differences estimates for firms' *Export dummy*, *Exports*, *Turnover Domestic turnover* and *Employees* around the termination of one or more correspondent bank relationships in a firm's locality and compared with unaffected control firms, using the Chaisemartin and D'Haultfœuille 2022 estimator and a continuous treatment variable. Treated firms are based in a locality where at least one bank branch lost a correspondent bank relationship. Control firms are based in a locality that did not lose a correspondent bank relationship up to the event year. The treatment level is calculated as the number of lost correspondent banking relationships up to year t divided by the number of bank branches in a locality. We match each treated firm to one control firm in the same industry and country that has similar *Exports*, *Total assets* and *Total Factor Productivity* in the pre-event year. Firm controls include *Total assets* and *Total Factor Productivity*; bank controls include *Local loan growth*, *Equity/Total assets*, *Loans/Customer deposits*, and *ROA*. Standard errors are clustered at the locality level and shown in parentheses.

	Exports		Turnover		Employees
	Dummy	Amount	All	Domestic	
	(1)	(2)	(3)	(4)	(5)
Effect at t=0	-0.035***	-0.023	0.029	0.049	-0.002
	(0.011)	(0.072)	(0.044)	(0.030)	(0.006)
Effect at t=1	-0.052**	-0.149	-0.056	-0.053	-0.012
	(0.023)	(0.110)	-(0.042)	(0.033)	(0.010)
Effect at t=2	-0.184***	-0.943***	-0.276***	-0.055	-0.082***
	(0.019)	(0.072)	(0.024)	(0.039)	(0.011)
Effect at t=3	-0.273***	-1.525***	-0.304***	0.105**	-0.094***
	(0.033)	(0.166)	(0.031)	(0.046)	(0.013)
Effect at t=4	-0.387***	-2.049***	-0.344***	0.156***	-0.102***
	(0.039)	(0.171)	(0.035)	(0.047)	(0.017)
Placebo at t=-2	-0.016	-0.047	0.001	-0.005	0.003
	(0.010)	(0.049)	(0.013)	(0.026)	(0.006)
Placebo at t=-3	-0.021	-0.084	-0.005	-0.005	0.007
	(0.018)	(0.086)	(0.017)	(0.032)	(0.013)
$\beta_{t=0}$ based on N firm-years	96,105	91,741	96,105	91,405	84,418
$\beta_{t=0}$ based on N switchers	17,807	15,818	17,807	15,739	15,850
Firm and bank controls	Yes	Yes	Yes	Yes	Yes
NP industry trends	Yes	Yes	Yes	Yes	Yes
Linear country trends	Yes	Yes	Yes	Yes	Yes
Pre-event mean	1.00	4.73	6.92	5.98	2.49

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

Table F3: Terminated correspondent bank relationships and firm exports: OLS with country×year and industry×year fixed effects

This table shows OLS difference-in-differences estimates on firms' *Export dummy* and *Log exports* around the termination of a correspondent bank relationship. Treated firms are based in a locality where at least one state-owned bank branch has lost a correspondent banking relationship. Control firms are based in a locality in which no bank has lost a correspondent banking relationship up to the event year. We match each treated firm to one control firm of the same industry and country that also exports and has similar *Exports*, *Total assets* and *Total Factor Productivity* in the pre-event year. Firm controls include *Total assets* and *Total Factor Productivity*, banks controls include *Local loan growth*, *Equity/Total assets*, *Loans/Customer deposits*, and *ROA*. Standard errors are clustered on the locality level and are shown in parentheses.

	Exports	
	Dummy	Amount
	(1)	(2)
Effect at t=0	-0.070***	-0.065**
	(0.015)	(0.027)
Effect at t=0	-0.077***	-0.062
	(0.014)	(0.038)
Effect at t=0	-0.121***	-0.067
	(0.019)	(0.043)
Effect at t=0	-0.111***	-0.062
	(0.023)	(0.062)
Effect at t=0	-0.129***	-0.033
	(0.025)	(0.038)
Placebo at t=-2	-0.015	-0.049
	(0.016)	(0.032)
Placebo at t=-3	0.005	0.004
	(0.014)	(0.042)
Observations	183,083	156,369
Firm Fixed Effects	Yes	Yes
Year Fixed Effect	Yes	Yes
Firm and bank controls	Yes	Yes
Country × Year Fixed Effects	Yes	Yes
Industry × Year Fixed Effects	Yes	Yes

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

Table F4: 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.

	(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	log billion USD	378	24.17	24.12	17.43	29.98	2.41
Distance to trading partner	log km	2,385	8.20	8.44	3.37	9.82	1.01
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	0.00	0.14	0.03
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
Δ Log Exports (World)	log million USD	52	-0.02	-0.01	-0.32	0.10	0.08
Δ Log Imports (World)	log million USD	52	-0.02	-0.01	-0.32	0.10	0.08