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DISCUSSION PAPER

// RALPH DE HAAS, KAROLIN KIRSCHENMANN,
AND ALISON SCHULTZ

Global Payment Disruptions and Firm-Level Exports

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Global Payment Disruptions and Firm Exports*

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

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Abstract

We exploit proprietary information on severed correspondent banking relationships—due to the stricter enforcement of financial crime regulation—to assess how payment disruptions impede cross-border trade. Using firm-level export data from emerging Europe, we show that when local respondent banks lose access to correspondent banking services, their corporate borrowers start to export less. This trade decline occurs on both the extensive and intensive margins and firms do not substitute foregone exports with higher domestic sales. As a result, total firm revenues and employment shrink. These findings highlight an often overlooked function of global banks: providing the payment infrastructure that enables firms in less-developed countries to export to richer parts of the world.

JEL codes: F14; F15; F36; G21; G28

Keywords: Correspondent banking; payment infrastructure; global banks; international trade; anti-money laundering

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†European Bank for Reconstruction and Development (EBRD), KU Leuven and CEPR, e-mail: dehaasr@ebrd.com

‡ZEW Leibniz Centre for European Economic Research Mannheim, e-mail: karolin.kirschenmann@zew.de

§University of Mannheim and Tax Justice Network, email: schultz@uni-mannheim.de

1 Introduction

Access to financial services is important for economic growth and development (Schumpeter 1911; Rajan and Zingales 1998; Levine 2005). In developing and emerging countries, globally active banks play a pivotal role in providing access to finance and facilitating financial development – by providing access to know-how and new technologies and by enhancing risk-sharing, competition and market discipline (among others, Claessens et al. 2001; Giannetti and Ongena 2009; Bruno and Hauswald 2014; see Claessens 2017 for a recent survey).[1] Most of the literature on global banks focuses on lending and deposit taking. In contrast, the role of global banks as correspondent banks facilitating cross-border transactions and trade flows has received considerably less attention in the academic literature. This is surprising, given that a restricted access to correspondent banking can have detrimental effects: Survey evidence suggests that the reduced availability of bank-intermediated trade finance products contributed significantly to the trade slump during the global financial crisis (Asmundson et al. 2011).

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 banking is essential for international trade for at least two reasons. First, correspondent banks enable cross-border trade-related payments from importers to exporters. Specifically, correspondent banks facilitate payments between the exporter's and the importer's local banks, which usually do not hold accounts with each other. Second, correspondent banks participate in bank-intermediated trade finance solutions, such as letters of credit, which facilitate trade when and where the probability of non-payment or non-shipment is high and enforcement is expensive.[2]

Despite the limited attention in the academic literature, access to correspondent bank-

[1] Several recent papers also highlight the risks of global banking and financial integration (e.g., Cetorelli and Goldberg 2011; Giannetti and Laeven 2012; De Haas and Van Horen 2012).

[2] Local banks also provide short-term loans against accounts receivable to finance the working capital needs arising from the time lag between production and sales revenues. Since such loans are usually not provided by correspondent banks, they are not the focus of our study.

ing services has become a major concern among policy makers in recent years: Across a wide range of countries, global banks have severely restricted the provision of correspondent banking services in reaction to ballooning compliance costs in the context of financial crime regulation and the low profitability of the correspondent banking business. The global reduction in correspondent banking has prompted policy makers to warn that losing access to correspondent banks may have serious consequences for international trade, growth, financial inclusion and the stability of the financial system (Rice et al. 2020; FSB 2017; BIS 2016; CGD 2015; World Bank 2015).

This paper is the first, to the best of our knowledge, to quantify the real effects of the global retrenchment of correspondent banks in a microeconometric analysis. Specifically, we combine novel time-varying respondent bank-level data on the loss of correspondent banking relationships with firm-level export data to investigate how the withdrawal of correspondent banks has affected firms' trade opportunities. We conjecture that the global withdrawal of correspondent banks negatively affects the extensive and intensive margins of exporting of affected firms: When a correspondent bank terminates the provision of payment services to a local respondent bank, finding alternatives for making international payments is associated with significant costs for the local respondent bank. Cash payments are usually not possible in international trade and the broad-based retrenchment of correspondent banks impedes the search for a new correspondent bank, in particular when the termination of the existing relationship sends a negative signal about financial crime concerns with the local respondent bank. The same reason impedes the search for alternate sources of trade finance. Local firms are affected by the withdrawal of correspondent banks by reduced access to or increased costs of payment and trade finance products. Building a new relationship with a provider of trade finance is further complicated by the guarantee-like features of trade finance products, which make information asymmetries about the customer's creditworthiness an additional hindrance.

We analyze the effect of the decrease in correspondent banking services between 2008

and 2020 on firms' probability to export, their export revenues and other real economic outcomes (total revenues, domestic revenues, employment) in four countries in the EBRD region[3] for which we can match firm-level export data to respondent bank-level information on correspondent bank withdrawal and bank branch locations: Bosnia-Herzegovina, Croatia, Hungary and Turkey. The region provides an ideal setting for our study because (i) correspondent banking is particularly important for developing and emerging countries (CGD 2015) and (ii) the extensive withdrawal of correspondent banks from the region is due to large-scale problems with financial crime in the form of money laundering (BIS 2016).

As a basis for our identification strategy we join three key pieces of information: time-varying respondent bank-level data on the loss of correspondent bank relationships; data on the geo-coordinates of respondent banks' branch networks; and data on exports (and other real outcomes) of individual firms that are surrounded by these branches. Information on the loss of correspondent bank relationships comes from two proprietary surveys among local banks in the EBRD region: 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 Program. We link our bank-level data to comprehensive information about the geographical location of bank branches and then match this information with firm-level data from Orbis. These combined data allow us to paint a detailed picture of the banks that surround each firm and to identify, at the local level, the impact of the withdrawal of correspondent banks on firms' exports and other real outcomes.

To do so, we employ the difference-in-differences estimators of intertemporal treatment effects introduced by De Chaisemartin and D'Haultfoeuille (2022). Their event-study approach for binary-and-staggered treatments allows for dynamic and heterogeneous treatment effects. In our differences-in-differences estimations, we aim to compare exports and other real economic outcomes of firms in cities in which at least one bank branch has lost a correspondent banking relationship (treated firms) to firms in a city which has not lost a correspondent

[3]See https://www.ebrd.com/where-we-are.html.

banking relationship up to the event year (control firms). To provide an accurate estimate of the impact of the decline in correspondent banking on firms' real economic outcomes, we first match treated to similar control firms and keep those on the common support for our estimation sample.

Our empirical approach rests on two key identifying assumptions. First, we assume that the withdrawal of correspondent banks is exogenous to individual firms' trade expectations and demand shocks. This assumption is plausible since the withdrawal of correspondent banks occurred on a wholesale basis from regions, countries, industries and banks (CGD 2015). Besides, our survey among local respondent banks confirms that less demand is only a negligible reason for withdrawal. While the withdrawal of correspondent banks may not be exogenous to the local respondent bank, the identification of our results rests on the withdrawal being random to the city in which the banks' branches are located and thus random to the firms in this city. Second, our empirical strategy relies on the location of banks and firms being independent of each other. Following Berger et al. (2005), we assume that the banking landscape near firms imposes an exogenous geographical limitation on the banks that firms have access to. Beck et al. (2018) show that this assumption is reasonable considering the lending techniques (relationship vs. transactional) of banks. Our setting resembles the Beck et al. (2018) setting as far as payment services, currency clearing and trade finance products are used by relationship banks to offer full-service bank relationships to their business clients.

Our results show that a decrease in correspondent banking services negatively affects both the extensive and the intensive margin of exports: Exporting firms are less likely to export and have a lower export turnover if a bank branch in their city has lost a correspondent banking relationship. Moreover, firms cannot make up for their decreased export turnover with increased domestic turnover and thus have to lay off workers with some time delay. A spillover analysis following Berg et al. (2021) 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 patterns of the spillovers are consistent with the withdrawal of correspondent banks inducing a price effect on treated firms. The negative effect of the increase in the costs to export is less severe for each individual treated firm the larger the fraction of treated firms in the same industry.

Our results also indicate that state-ownership of local banks significantly alleviates the negative effects on local firms' export activities from the decline in correspondent banking relationships. State-owned banks may have less pressure to pass on increased costs of providing correspondent banking services to their customers, i.e. the local firms. They may also be able to provide easier access to alternative trade insurance products such as government-guaranteed schemes.

Our study contributes to two strands of the literature. First, our paper adds to the literature on financial integration and trade that analyzes the role of global banks for international trade. Several papers show that the local presence of foreign banks supports trade between the local country and the foreign banks' home country (e.g., Portes and Rey 2005; Bronzini and D'Ignazio 2017; Claessens et al. 2017). Caballero et al. (2018) show that an increase in syndicated loan connections between countries also increases exports and Brancati (2022) finds that the acquisition of firms' local banks by international banks increases the likelihood of local firms to export to countries in which the international banks have a foreign branch. Kohn et al. (2022) provide a recent review on studies on the impact of financial frictions on international trade. A small number of papers studies the importance of bank-intermediated trade finance products for international trade. Demir and Javorcik (2020) use product-level data and study the effect of bank-intermediated trade finance products (letter of credits in particular) on international trade flows during the COVID crisis, while Crozet et al. (2022) compare the effect of bank-intermediated trade finance products on international trade during the COVID crisis and the 2007-08 financial crisis. Niepmann and Schmidt-Eisenlohr (2017) and Ahn and Sarmiento (2019) analyze how bank-level financial shocks reduce the supply of

letters of credit and, in turn, firm exports.[4] We extend this literature by explicitly studying the role of correspondent banks facilitating cross-border transactions for international trade in a cross-country sample of transition countries. Specifically, we quantify the real effects of a shock to the availability of correspondent banking services on firms' exports and other real outcomes in Emerging Europe.

Second, this paper contributes to the literature that studies the cross-border transmission of shocks through international banks and the effects on the real economy. Previous papers have exploited an array of shocks to identify the implications of cross-border contagion, including financial crises (e.g., Peek and Rosengren 1997; Peek and Rosengren 2000; Chava and Purnanandam 2011; 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 et al. 2015), shocks to risky sovereign bond holdings (e.g., Popov and Van Horen 2015; Altavilla et al. 2017; Balduzzi et al. 2018; Acharya et al. 2018; De Marco 2019) or tax reforms impacting the cost of equity (Célérier et al. 2017). We contribute to this literature by exploiting a supply shock to global banks' correspondent bank activities, which reduces local firms' access to payment transaction possibilities and trade finance arrangements or makes these more expensive for local firms. Our study therefore complements the previous literature, which focuses on bank lending to firms for investment or working capital purposes, with an analysis of the role of banks as providers of payment and trade finance services.

The remainder of this paper is organized as follows. Section 2 describes the institutional background and Section 3 introduces the data. Section 4 explains the empirical strategy, while Section 5 reports the results. Section 6 concludes.

[4]The role of (local) banks in providing working capital loans and thereby facilitating trade, in contrast, has been well documented (Amiti and Weinstein 2011; Chor and Manova 2012; Manova 2013; Del Prete and Federico 2014; Paravisini et al. 2015). Moreover, some papers study other forms of trade finance products such as export credit insurance (e.g., Auboin and Engemann 2014; van der Veer 2015) and export guarantees (e.g., Felbermayr and Yalcin 2013; Heiland and Yalcin 2021).

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. This, in turn, allows respondent banks—especially those in developing countries (CGD 2015)—to provide cross-border payment services to their own clients.

Correspondent banks can facilitate international trade in two main ways. First, they help channel trade-related cash flows across borders. More specifically, correspondent banks facilitate payments between the exporter's and importer's local banks (which typically do not hold accounts with each other). The bulk of payments underlying international trade therefore runs through correspondent banks (Rice et al. 2020).

Second, correspondent banks provide letters of credit and other trade finance solutions. Most international trade transactions take place on an open account basis and pre-payment is rare (Asmundson et al. 2011; Ahn 2014). Correspondent banks can then help to overcome the commitment problems and limited enforceability that often inhibit direct payment between trading partners. Because correspondent banks maintain relationships of an "on-going, repetitive nature" (Wolfsberg Group 2014) they operate as a credible intermediary between local banks. This in turn ensures that both 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) as is the case in many developing economies.

Due to the high fixed costs of establishing and maintaining correspondent bank relation-

ships, 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 implies that cross-border trade may be quite exposed to sudden shocks to this tight global banking network.

2.2 Anti-Money Laundering and correspondent banking

Correspondent banks are highly vulnerable to financial crime. Cross-border payments are often used to disguise illicit funds as differences in legislation, bank secrecy laws, and enforcement across jurisdictions complicate investigation and prosecution. Funds are transferred back and forth between accounts in different countries and currencies, and (re-)exchanged for high-value items such as real estate.[5] Furthermore, correspondent banks may become involved through the provision of trade finance for illegal fund transfers. Trade transactions are a common method to validate illicit cross-border payment transactions, such as through over- or multiple invoicing (FATF 2006).

Authorities worldwide have long recognized the magnitude and costs of cross-border financial crime and how it can compromise the reputation of financial institutions and countries alike (IMF 2001). Since the 1970s, governments have therefore developed and harmonized legal frameworks to counteract financial crime in international payment systems. The recommendations of the Financial Action Task Force (FATF), the global watchdog on money laundering and terrorist financing, require correspondent banks for example to reveal the identity of all parties involved in a cross-border transaction and to perform due diligence on their customers ("know-your-customer"). Yet, until the late 2000s, the weak enforcement of these legal frameworks undermined the fight against financial crime (CGD 2015).

The prosecution of financial crime offences only started to tighten in the aftermath of the Global Financial Crisis. Increased regulatory scrutiny unearthed extensive evidence of financial crimes in the banking sector (Tomasic 2011) and especially U.S. regulators stepped

[5]Money laundering is estimated to account for 2 to 5 percent of global GDP (Arnone and Borlini 2010).

up their enforcement efforts as a result. The stricter enforcement of financial crime legislation has been most evident in the issuance of surging fines (CGD 2015). Perhaps the most prominent example is the record US$8.9 bn. fine issued to French correspondent bank BNP Paribas in June 2014 for violating sanctions against Sudan, Cuba and Iran. The bank had removed information from wire transfers worth more than US$190 billion to obscure their destination. The extent of the penalty was unexpected (BNP Paribas itself had set aside 'only' US$1.1 billion in provisions for litigation costs) and greatly exceeded past fines (the highest fine in the context of financial crime had been the US$1.9 billion fine issued to HSBC in December 2012 for money laundering).

Crucially, the U.S. Department of Justice made clear in 2014 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 for sanctions violations seem to have been effective in preventing such cases in the aftermath of the BNP Paribas trial, fines for violations of AML regulations remain on the rise globally. A recent example includes the three fines, totalling US$7.2 billion, that Goldman Sachs received in 2020 (Financial Crime News 2022).

2.3 The effects of de-risking by correspondent banks

The massive and unexpected 2014 fine for BNP Paribas started a process of decline in the global correspondent banking industry. 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 sharp reassessment of the cost of regulatory compliance in correspondent banking. Compliance costs surged for two reasons. First, the expected costs of *non*-compliance increased massively in view of the large penalties and the strong signal that any correspondent bank providing access to the U.S. financial system could be held liable for financial crimes. Second, the costs of the due diligence needed to comply with U.S. financial crime legislation increased, too. Industry surveys show that banks had to step up their spending on financial crime personnel considerably (Dow Jones Risk & Compliance and ACAMS 2015; McKinsey 2017; Banking Exchange 2020). Banks

also highlighted the uncertainty about the appropriate degree of due diligence, as well as inconsistencies in international regulation, as important cost factors (BIS 2016; SWIFT 2016).

The sudden hike in AML/CTF compliance costs prompted global banks to reconsider their business strategy with regards to correspondent banking, a business that was seen as having shifted from being a low-risk/low-margin segment to a high-risk/low-margin one (BIS 2016). Many banks decided to severely prune their correspondent banking network by ending relationships that were no longer cost-effective or by exiting the business altogether (BIS 2016; FSB 2017; Rice et al. 2020). Correspondent banks started to withdraw especially from countries with a high risk of financial crime. As part of this "de-risking" strategy (CGD 2015), correspondent banks reduced their presence in risky regions in a wholesale rather than a case-by-case manner.

To verify whether 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 ran an on-line survey among a sample of local respondent banks in the end of 2019. The survey was organised in co-operation with EBRD's Trade Facilitation Program and questions covered the period 2009–2019. Out of the 131 invited banks, 91 banks across 28 economies in Central and Eastern Europe, the former Soviet Union, and Northern Africa completed the entire questionnaire, a response rate of 69 per cent.[6]

Figure 1 shows that according to respondent banks, the main reasons for the decline in correspondent banking services were that "correspondent banking does not generate sufficient business to justify the cost of additional customer due diligence" (37 per cent) 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" (32 per cent). Only 3 per cent of respondent banks considered "less demand from their customers" an important reason for the withdrawal of correspondent banks. These results corroborate that

[6]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.

increased due diligence costs and concerns about compliance with AML/CFT regulations, rather than a reduced demand for correspondent banking services, have been the main drivers of the decrease in global correspondent bank relationships and services.

[Insert Figure 1 here]

The decline in correspondent banking as of 2014 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-based nature of the retrenchment of correspondent banks, combined with the concentrated nature of this industry (Del Prete and Federico 2014; Niepmann and Schmidt-Eisenlohr 2017) also made it difficult to find alternative providers of these services.

Our survey of respondent banks provides some first descriptive evidence on the local impact of the reduced availability of correspondent banking services. Figure 2 shows the fraction of local respondent banks that had difficulties in accessing, or were even entirely unable to access, three main types of correspondent banking services in the years 2013, 2015, 2017, and 2019. We observe a sharp increase 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). Importantly, those respondent banks that continued to have access, experienced a sharp increase in the cost of these services. On average, banks estimate a cost increase of 35 per cent between 2017 and 2019.

[Insert Figure 2 here]

The de-risking and shrinking of global correspondent banking has also affected the geographical distribution of the industry. While in 2013, 73 per cent of all correspondent banks were based in the U.S. and Germany, these two countries held a combined market share of just 60 per cent in 2019. Correspondent banks from a variety of other countries, such as Austria, have filled this gap, though only partially. The replacement of U.S. correspondent

banks with those from other regions may be unfavorable to the extent that it leads to longer and hence costlier intermediation chains.

In sum, our survey provides suggestive evidence of how the decline in correspondent banking relationships has affected local respondent banks. In the remainder of this paper, we estimate more formally the impact that the sudden withdrawal of correspondent banks has had on local firms' exports, turnover and employment.

3 Data

Our empirical analysis focuses on four emerging European countries—Bosnia & Herzegovina, Croatia, Hungary, and Turkey. Each of these countries was until recently heavily reliant on correspondent banking services and hence provide a highly relevant and representative setting for our purposes. The withdrawal of correspondent banks from emerging Europe also reflected the type of concerns discussed in the previous section (BIS 2016).

For each of these 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 the following three pieces of data: (i) time-varying information for individual respondent banks about terminated correspondent banking relationships; (ii) data on the exact geo-coordinates of all branches of each of these respondent banks; and (iii) data on exports (and other real outcomes) of individual firms that are spatially close to these bank branches. We now discuss these data in turn.

3.1 Measuring the withdrawal of correspondent banks

We combine information from two new surveys among respondent banks to retrieve unique and time-varying information about lost correspondent bank relationships. The first source is the third wave of the Banking Environment and Performance Survey (BEPS III), which took place between October 2020 and June 2021. The BEPS III research design covers both 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 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. The BEPS III survey provides us with information about (changes in) correspondent banking relationships for 20 banks in our four sample countries. We supplement this with similar information on 3 additional banks in these countries, collected as part of an online survey that we conducted in 2019 together with EBRD's Trade Facilitation Program (TFP).[7] This survey focused exclusively on banks' correspondent banking relationships.

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 global database. Orbis provides comprehensive information on firms' balance sheets and income statements and, for some countries, yearly data on export revenues. Importantly for our study, Orbis also provides the exact location of each firm, allowing us to match firms to nearby bank branches, and information on a firm's industry, allowing us to match treated firm to control firms in the same industry. We obtain the data via the Orbis flatfiles of June 2022. To construct a nationally representative sample for our four countries, we follow the procedures set out in Kalemli-Ozcan et al. (2015).

3.3 Bank branch networks and bank characteristics

We carefully match our data on firm location 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 by downloading data from bank websites and subsequently double-checking them with the bank as well as with the SNL Financial database. Our data provide us with a near complete picture of the branching landscape in 2020. In total, we have data on

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

the geo-coordinates of 48,399 branches operated by 23 banks, representing 27 percent of all bank assets in our four sample countries. We also merge this information with BvD's Orbis BankFocus to get balance sheet and income statement data for each bank.

We then connect the firm and bank branch data following Beck et al. (2018). We make sure that the names of localities (cities and towns) 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 behind this procedure 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 the banks in this locality. In effect this approach focuses on local equilibrium effects and assumes that local credit markets are competitive in nature, so that firms' access to banking services depends on locality-wide financial shocks.

An alternative approach to match firms and banks is to use information on individual firms' main bank that is available in Orbis. 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 re-run our complete analysis with the firm-level matching and show that our results are qualitatively the same as those with the city-matched sample.

For our empirical analysis we focus on exporters, i.e. firms that export at least once during our observation period. These firms will be 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 firms based in 857 localities (towns and cities) across the four countries.[8]

Table 1 shows that the banks in our sample for which we have information on (changes

[8]In our regression analyses, we control for city-level bank characteristics. To construct these bank controls at the city level, we also include all banks for which we have the relevant variables, no matter if they are surveyed or not. In total, these are 140 banks with 10,123 bank branches.

in) their correspondent banking relationships, represent between 44 and 66 per cent of bank branches per city and bank assets per country in Bosnia-Herzegovina, Croatia, Hungary, while the coverage is lower in Turkey.

[Insert Table 1 here]

4 Empirical Strategy

In this section, we introduce our empirical strategy. We first outline the key identifying assumptions in Section 4.1. In Section 4.2, we explain the construction of our estimation sample by matching firms that are located in a city in which at least one bank branch has lost a correspondent banking relationship to firms in a city which has not lost a correspondent banking relationship up to the event year. In Section 4.3, we introduce our empirical specification and the variables. Definitions and sources of all variables are provided in Table A1 in the Appendix.

4.1 Identification

Our study exploits the loss of local respondent banks' correspondent bank relationships as an exogenous shock to local firms' access to correspondent banking services. To investigate the implications of the withdrawal of correspondent banks we use a differences-in-differences approach. We compare firms' likelihood to export, their export revenues, total revenues, local revenues and employees before and after the shock and between firms located in cities in which at least one bank branch has lost a correspondent banking relationship and firms in a city which has not lost a correspondent banking relationship up to the event year.

Our empirical approach rests on two key identifying assumptions. First, we assume that the withdrawal of correspondent banks is exogenous to individual firms' trade expectations and demand shocks. This assumption is plausible since the withdrawal of correspondent banks occurred on a wholesale basis from regions, countries, industries and banks (CGD 2015). With only 3 percent of respondents naming "less demand from their customers" as

a cause for the termination of a correspondent bank relationships, our survey among local respondent banks corroborates that less demand is only a negligible reason for withdrawal. Yet, the withdrawal of correspondent banks may not be exogenous to the local respondent bank. This does not threaten the identification of our results as long as the withdrawal is random to the city in which the banks' branches are located and thus random to the firms in this city.

Second, our empirical strategy relies on the location of banks and firms being independent of each other. Following Berger et al. (2005), we assume that the banking landscape near firms imposes an exogenous geographical limitation on the banks that firms have access to. Beck et al. (2018) show that this assumption is reasonable considering the lending techniques (relationship vs. transactional) of banks. Our setting resembles the Beck et al. (2018) setting as far as payment services, currency clearing and trade finance products are used by relationship banks to offer full-service bank relationships to their business clients.

4.2 Matching

In our differences-in-differences estimations, we aim to compare exports and other real economic outcomes of firms in cities in which at least one bank branch has lost a correspondent banking relationship (treated firms) to firms in a city which has not lost a correspondent banking relationship up to the event year (control firms). To provide an accurate estimate of the impact of the decline in correspondent banking on firms' real economic outcomes, we match treated and control firms and keep those on the common support for our estimation samples.[9]

To each treated firm, we match 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

[9]We also run all our analyses on the complete sample of firms. Results are qualitatively similar and available upon request.

two-factor Cobb-Douglas production function.[10] We match on total assets and productivity as the literature identifies these two variables as the most important firm-level determinants of the extensive and intensive margin of firms' exports (Bernard et al. 2007; Melitz 2003).[11] We only keep those treated firms in the sample for which we find an appropriate control firm and only keep firms with at least two observations, reducing our sample to 26,000 firms.

[Insert Table 2 here]

Table 2 provides summary statistics of the firm characteristics for the complete sample (Panel A) and the matched sample (Panel B). We also report the normalized difference by Imbens and Wooldridge (2009) to assess the magnitude of the differences in the averages of the firm and bank characteristics between the treated and the control group. The normalized difference is the difference in averages by treatment status, scaled by the square root of the sum of the variances, and thus provides a scale-free measure of the difference in distributions (Imbens and Wooldridge 2009). As a rule of thumb, Imbens and Wooldridge (2009) point out that normalized differences smaller than 0.25 (in absolute values) indicate sufficient similarity in the distributions of the respective variables in the treatment and control group.

Panel A of Table 2 shows that the normalized differences for the firm characteristics are already well below the 0.25-threshold in the complete exporter sample. However, matching still improves the similarity of the treatment and control groups with respect to observable firm characteristics as indicated by the lower normalized differences for the firm characteristics in Panel B compared to Panel A. In Table 2, we also report summary statistics for the drop in correspondent bank relationships in a city as the drop in correspondent bank relationships over the number of bank branches in a city (*Cut relationships (branch level) over branches in city*). This variable provides insights about the actual extent of terminated relationships

[10] To estimate the residual of the two-factor Cobb-Douglas production function, we need a firm's total assets and number of employees as input. As barely any Turkish firm has information on the number of employees, for Turkish firms, we apply a one-factor Cobb-Douglas function with total assets as the only input.

[11] In addition, there are important bank-level variables determining firms' exports, like access to credit(Berman and Héricourt 2010). We control for these bank-level variables (averaged on a city level) in our regression but do not include them in the matching to only match on actual firm-level variables.

rather than just indicating whether a city is treated or not. It shows that, on average, around 60 per cent of the branches in a treated city lose a correspondent bank relationship (median = 0.333). In contrast to the firm characteristics, we should find a large difference between the treated and the control groups in the *Cut relationships (branch level) over branches in city* variable as this variable corresponds to the treatment event. As expected, the normalized difference is much larger than 0.25.

We proceed with the matched sample in our regression analyses. The matched exporter sample consists of 26,000 firms and 20 banks with 8,361 branches in 706 cities. Summary statistics of all variables are provided in Table 3.

[Insert Table 3 here]

4.3 Empirical Specification

To gauge the impact of a decline in correspondent banking on local firms' exports and other real outcomes we employ the difference-in-differences estimators of intertemporal treatment effects introduced by De Chaisemartin and D'Haultfoeuille (2022). Their event-study approach for binary-and-staggered treatments allows for dynamic and heterogeneous treatment effects.

In a traditional difference-in-difference design based on two-way fixed effects, we would like to estimate the following regression:

$$Outcome_{i,t} = \sum_{k=-4,k\neq-1}^{k=+4} \beta_k \times D_k \times Lost\ Relationship_{j,t} + \beta_9 \times Firm\ Controls_{i,t} + \beta_{10} \times Bank\ Controls_{j,t} + \gamma_i + \delta_t + \epsilon_{i,t}$$

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

Our dependent $Outcome_{i,t}$ variables are *Export Dummy*, *Log Export Turnover*, *Log Turnover*, *Log Domestic Turnover* and *Log Number of Employees*. *Export Dummy* measures the extensive margin of exports and is one if a firm has export revenues in the given year, and zero if it

reports zero export revenues. *Log Export Turnover* measures the intensive margin of exports by the revenues from a firm's export activities in log euros, while *Log Turnover* captures the firm's total operating revenues in log euros and *Log Domestic Turnover* the revenues from its domestic economic activities in log euros. A firm's employment is measured by *Log Number of Employees*.

D_k are a set of dummies that are one at time k with k indicating the respective year before (for $-4 \leq k \leq -2$) or after ($0 \leq k \leq 4$) the event year. We normalize D_{-1} to 0. $Lost\,Relationship_{j,t}$ is a dummy that equals one if at least one bank branch in city j has lost a correspondent banking relationship up to year t. γ_i are firm fixed effects and δ_t are year fixed effects.

In such two-way fixed effects regressions, we would usually interpret β_k as the treatment effect of a lost relationship k years before or after the event year. However, De Chaisemartin and d'Haultfoeuille (2020) show that this approach can result in incorrect estimates due to the different implicit weighting of the average treatment effects (ATE) of firms experiencing their first treatment in different years. In particular, the two-way fixed effect approach does not satisfy the no-sign reversal property, which means that β_k could be positive, even though the ATE is negative for all firms of our sample. 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 average effect of having been treated from k=-4 until k=+4.

To circumvent these problems, we apply the estimator proposed by De Chaisemartin and D'Haultfoeuille (2022) which allows both for heterogenous treatment effects across different firms and for dynamic effects around the event. The estimator is a weighted average of difference-in-differences comparing the outcome evolution of switchers (i.e. firms that experienced a withdrawal at t-k) with the outcome evolution of not-yet switchers (i.e. firms that have not experienced a treatment up to t) between k=-4 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 city to have a negative effect on firm outcomes and therefore conjecture that β_0 to β_4 are negative. If firms are able to replace (some of) their export activity by increased local sales, then the negative effect of the decline in correspondent bank relationships may only be temporary and coefficients $\beta_k > 0$ are insignificant for the firms' turnover from all its economic activities and the number of employees. Like in other difference-in-difference designs, the causal interpretation of our findings rests on the parallel trends assumption. Insignificant coefficients on β_{-4}, β_{-3} and β_{-2}, i.e. the absence of an effect in the pre-event years, indicate that this assumption is reasonable.

To mitigate concerns about omitted variable bias, we add a vector of $Firm\,Controls_{i,t}$ and $BankControls_{j,t}$. As $FirmControls_{i,t}$ we include *Log Total Assets* to control for firm size and *Total Factor Productivity*, which is the industry-adjusted residual of a two-factor Cobb-Douglas production function. The input factors of this function are *Log Number of Employees* and *Log Total Assets* to account for labor and capital, and the output is *Log Turnover*.[12] The trade literature has identified firm size and productivity to be the most relevant factors to explain why firms export (e.g., Melitz 2003; Bernard et al. 2007). $Bank\,Controls_{j,t}$ comprise standard bank characteristics averaged at the city-level (from all banks with branches in a city and the respective balance sheet data available irrespective of whether we have information on the drop in their correspondent bank relationships or not) to ensure that our results are not driven by the structure of the local banking environment. *Equity/Total Assets* accounts for banks' capitalization. *Loans/Customer Deposits* indicates the extent to which banks' loans are funded by deposit (compared to wholesale) funding and *ROA* is the return on assets and measures banks' profitability.

Moreover, we account for for linear, as well as for non-parametric industry and country trends. The De Chaisemartin and D'Haultfoeuille (2022) estimator controls for linear trends by including fixed effects for the industry or country when residualizing the first-difference of

[12]Due to the lack of employment data for Turkey, we only use a one-factor Cobb-Douglas production function (i.e. only include total assets as an input) for Turkish firms.

the outcome. It accounts for nonparametric industry or country trends by using a weighted average of difference-in-differences comparing switchers and non-switchers from the same industry or country, respectively. When controlling for nonparametric trends, estimators are unbiased even if treated and control firms experience differential trends, provided all firms of the same industry or country experience parallel trends. We use robust standard errors and cluster them at the city-level.

For means of comparison, we also run two alternative difference-in-difference models. We estimate equation (1) by simple OLS and apply Borusyak et al. (2022)'s estimator, which uses an imputation approach allowing for arbitrary heterogeneity and dynamics of causal effects. While the OLS estimator could be subject to the abovementioned incorrect weighting problem and bias and is therefore our least preferred estimator, regressions based on Borusyak et al. (2022) allow us to set a plausible range for our effect sizes and serve as robustness checks.

5 Results

In this section, we present the results from our differences-in-differences estimations. We first investigate the effects of the supply shock to the availability of correspondent banking services on trade and other real outcomes (Section 5.1). We then estimate and discuss potential spillover effects (Section 5.2). We complete our analysis with analyzing the mediating effect of banks' state-ownership (Section 5.3).

5.1 Effects on trade and other real outcomes

We start our empirical analysis by investigating the effect of the decline in correspondent bank relationships on firms' likelihood to export and their export turnover.

Figure 3 graphically shows the results from the differences-in-differences regressions for firms' likelihood to export and their export turnover. The upper left-hand graph reports estimates and 95%-confidence intervals of the average causal effect of the decline in correspondent bank relationships on firms' probability to export (*Export Dummy*). The reported coefficients are from a regression following the de Chaisemartin and D'Haultfoeuille (2022)

approach, including *Firm Controls* and *Banks Controls* and controlling for linear country trends and nonparametric industry trends. The respective regression results are reported in Table 4, column (4). The results show that the likelihood to export is significantly lower for firms in cities in which at least one bank branch has lost a correspondent banking relationship (treated firms) than for firms in a city which has not lost a correspondent banking relationship up to the event year (control firms) after the termination of a correspondent bank relationship. This difference becomes even more pronounced over the years after the decline in correspondent bank relationships occurred. The insignificant and close to zero pre-event effects (except for t=-4) provide evidence for a parallel trend in the probability to export before the event in the treated and control groups.

[Insert Figure 3 here]

The lower left-hand graph in Figure 3 reports estimates of the effects of the decline in correspondent bank relationships on firms' probability to export based on our alternative estimators. Borusyak et al. (2022)'s imputation approach yields very similar results like the De Chaisemartin and D'Haultfoeuille (2022) estimator. The OLS estimator, however, reports insignificant effects until year 4, when it turns significantly negative. The inability of the OLS estimator to pick up the effect measured by the other two estimators indicates that we do, indeed, face a negative weighting problem described by De Chaisemartin and d'Haultfoeuille (2020).

Table 4 reports the corresponding regression results using the De Chaisemartin and D'Haultfoeuille (2022) methodology. Column (1) presents the difference-in-difference estimates without including controls or fixed effects. In column (2), we add *Firm Controls* and *Bank Controls*. In column (3), we control for non-parametric country trends and linear industry trends, while in column (4) we control for non-parametric industry trends and linear country trends, respectively.[13] Results are similar across all specifications showing a negative

[13]The methodology by De Chaisemartin and D'Haultfoeuille (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

and persistent effect of the decline in correspondent bank relationships in the four years after the event. Only in the case with non-parametric country trends, the estimates are less precisely measured but still show the same negative sign. Almost all of the effects in the years before the treatment are insignificant which is evidence for a parallel trend in the probability to export in the treated and control groups.

With regard to the size of the estimated effects, results in column (4) show that the probability to export is 3.8 percentage points lower for treated firms compared to control firms right after the drop in correspondent bank relationships (t=0). After four years (t=4), a firm that was treated in the event year compared to a firm that was not treated in the event year has a 35.2 percentage point lower probability to export. These effects are sizable. Yet, many of the firms in our sample are small and medium-sized businesses which are most likely suppliers to larger firms in other countries. The latter may find it relatively easy to replace their suppliers if these have to increase the costs of their products due to increased costs of correspondent banking services. At the same time, our survey among local respondent banks shows that some of these banks indeed lost access to certain correspondent banking services or currencies and therefore some local exporters may also have lost access to these services and currencies and therefore quit exporting.

[Insert Table 4 here]

The upper right-hand graph in Figure 3, in turn, depicts the results from the differences-in-differences regressions for firms' export turnover. It reports estimates and 95%-confidence intervals of the average causal effect of the decline in correspondent bank relationships on firms' export turnover (*Log Export Turnover*). The reported coefficients are, again, from a regression following De Chaisemartin and D'Haultfoeuille (2022) approach, including *Firm Controls* and *Bank Controls* and controlling for linear country trends and nonparametric industry trends. The respective regression results are reported in Table 4, column (8). The

effects and country×year fixed effects. The results are reported in Table A2 in the Appendix and yield similar conclusions as our De Chaisemartin and D'Haultfoeuille (2022) results.

results show that, after the event, export turnover is lower for firms in cities in which at least one bank branch has lost a correspondent banking relationship (treated firms) than for firms in a city which has not lost a correspondent banking relationship up to the event year (control firms). Like for the results for firms' probability to export, this difference is significant (starting in t=1) and becomes more pronounced over time. The regression results in Table 4 confirm this pattern across all specifications. Also with regard to firms' export turnover, we find evidence for a parallel trend before the event in the treated and control groups. Column (8) of Table 4 indicates that, for instance, one year after the event (t=1) the export turnover of firms in cities that lost a correspondent banking relationship in the event year is 18.2 per cent lower than of similar control firms.

The lower right-hand graph in Figure 3 reinforces this finding. Estimates based on Borusyak et al. (2022) are negative and significant from the event year on and of similar magnitude as our main estimator. The OLS estimator turns significantly negative only in year 4 which hints, again, towards the existence of negative weights when using a two-way fixed effects approach.

Overall, our results show that a decrease in correspondent banking services negatively affects both the extensive and the intensive margin of exports: Exporting firms are less likely to export and have a lower export turnover if a bank branch in their city has lost a correspondent banking relationship.

Firms with declining export opportunities might turn to domestic markets and increase their domestic sales making up for the loss in their export business. If this is the case, firms' other real outcomes like their total turnover, their domestic turnover and the number of their employees should not be negatively affected, at least not to the same extent. In the next step, we analyze how the decrease in correspondent banking affects firms' total turnover, their domestic turnover, and their number of employees to provide a broader picture of the real effects on local firms and economies.[14]

[14]The respective regression results are reported in Table 5 (for total and domestic turnover) and Table 6 (for the number of employees).

Figure 4 depicts the results from the De Chaisemartin and D'Haultfoeuille (2022) differences-in-differences regressions for firms' total turnover (*Log Turnover*) and firms' domestic turnover (*Log Domestic Turnover*), including *Firm Controls* and *Bank Controls* and controlling for linear country trends and nonparametric industry trends. The upper left-hand graph shows that, after the event, total turnover is lower for firms in cities in which at least one bank branch has lost a correspondent banking relationship (treated firms) than for firms in a city which has not lost a correspondent banking relationship up to the event year (control firms). This difference is significant starting in t=1 and becomes more pronounced over time. Consistent with the decrease in total turnover, in the upper right-hand graph we do not find evidence for a considerable increase in domestic turnover in the years after the event, indicating that firms cannot make up for the lost export turnover by selling more in domestic markets.

[Insert Figure 4 here]

In line with firms' reduced turnover due to the foregone exports, Figure 5 shows a negative treatment effect that becomes more pronounced over time for the number of firms' employees (*Log Number of Employees*) after a correspondent banking relationship has been lost. The respective regression results in column (4) of Table 6 indicate that firms that experienced a drop in correspondent banking in their city one year before has a 3.4 per cent smaller workforce compared to similar control firms that did not experience a drop in correspondent banking in their city. After four years (t=4), a firm that was treated in the event year compared to a firm that was not treated in the event year has a 12.5 per cent smaller workforce.

[Insert Figure 5 here]

In the lower graphs in Figure 4 and Figure 5, estimates based on Borusyak et al. (2022) confirm our findings from the main analysis. The OLS results, in contrast, hint, again, towards the existence of negative weights.

In sum, the results indicate that exporting firms lose export opportunities after the termination of a correspondent banking relationship, cannot make up for their decreased export turnover with increased domestic turnover and thus have to lay off workers with some time delay.

[Insert Table 5 here]

[Insert Table 6 here]

5.2 Spillover effects

This section analyzes potential heterogeneous spillover effects of the decrease in correspondent banking relationships from affected (treated) firms to unaffected (control) firms and within affected firms following Berg et al. (2021).

It is possible that the termination of a correspondent banking relationship induces spillovers on unaffected (control) firms of the same industry. The decline in correspondent banking is a shock on treated firms' price (i.e., exporting becomes more expensive for treated firms) and may thus improve unaffected firms' competitiveness. Furthermore, the decline in correspondent banking may be a shock on treated firms' capacity (i.e., some treated firms might reduce or stop their export activity), improving non-affected competitors' situation as well. Moreover, Berg et al. (2021) highlight that the effect of a shock on firm-level real outcome variables depends not only on a firm's own treatment status, but also on the fraction of treated firms in the same industry. If, for instance, the decline in correspondent banking induces treated firms to increase the prices for their exported goods to make up for the increase in prices of correspondent banking services, the negative effect on treated firms' probability to export and export turnover should be less severe the more firms from the same industry are treated. The reason is that with more treated firms the respective trading partners have fewer possibilities to buy their products more cheaply from control firms.

We focus on spillovers at the industry-level but do not investigate regional spillovers, e.g. a city-level demand shock. As we match firms to bank branches of the same city in

our main analysis, the loss of a correspondent bank relationship by any branch in a city leaves all firms in this city treated and our estimated treatment effects actually aggregate firms' individual treatment effects and city-level spillover effects. As suggested by Berg et al. (2021), we estimate the following heterogeneous spillover model using OLS:

$$\begin{aligned} Outcome_{i,s,t} = \beta_0 + \beta_1 d_{i,s,t} + \beta_T \bar{d}_{s,t} d_{i,s,t} + \beta_C \bar{d}_{s,t}(1 - d_{i,s,t}) \\ + \beta_2 \times Firm\,Controls_{i,t} + \beta_3 \times Bank\,Controls_{j,t} + \gamma_i + \delta_{c,t} + \epsilon_{i,t} \end{aligned}$$

where subscripts i, j, s, c and t stand for individual firm, city, sector (industry), country and year, respectively.

As dependent $Outcome_{i,s,t}$ variables, we use *Trade Dummy* and *Log Export Turnover* for the spillover analysis. $d_{i,s,t}$ denotes the treatment indicator which switches to one when there is a decline in correspondent bank relationships in the city in which the firm is located. $\bar{d}_{s,t}$ denotes the (time-varying) fraction of treated firms in an industry (without firm i). $FirmControls_{i,t}$ include *Log Total Assets* and *Total Factor Productivity* and $BankControls_{j,t}$ comprise *Equity/Total Assets*, *Loans/Customer Deposits* and *ROA* as defined in Section 4.3. γ_i are firm fixed effects and $\delta_{c,t}$ are country×year fixed effects. The heterogeneous spillover model provides us with three coefficients of interest: the direct treatment effect (β_1) as well as spillover effects to treated firms (β_T) and to control firms (β_C).

To assess potential spillover effects, we resort to a graphical analysis of the regression results. Following Berg et al. (2021), we plot the outcome variables *Export Dummy* and *Log Export Turnover* as a function of treatment intensity, i.e. the fraction of treated firms in an industry, for treatment units, control units, and group level averages. Note that the underlying regressions are estimated using OLS and do therefore not represent the event-study-like estimations capturing the treatment effects in several years around the event date as in our main analysis. Table 9 provides the respective results. The treatment effect in the OLS regressions is also significantly negative.

[Insert Table 7 here]

Figure 6 shows the results of the spillover analysis for the probability to export (*Export Dummy*). The direct treatment effect indicates the effect 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 -13.1 percentage points. Starting with potential spillover effects to control firms, i.e. exporting firms in cities that do not experience a decline in correspondent bank relationships, the dotted line shows that control firms' probability to export is independent of the fraction of treated firms in the same industry. This finding indicates that unaffected control firms do not (quickly) change their exporting status when the competitive environment changes. Turning to the treated firms, the increasing solid line shows that firms are less negatively affected in their probability to export the larger the fraction of other treated firms in the industry. One reason for this finding may be that the decline in correspondent banking is a shock on the treated firms' price (i.e., exporting becomes more expensive for treated firms) and thus with more treated firms in an industry the respective trading partners have fewer possibilities to buy their products more cheaply from control firms and fewer treated firms stop exporting. Overall, the difference between treatment and control firms diminishes with more firms in the same industry being treated as result of the heterogeneous spillovers. This means that not accounting for spillover effects leads to underestimating the direct treatment effect.

[Insert Figure 6 here]

The dashed line in Figure 6 presents the industry-level average probability to export depending on the fraction of treated firms. The slope is slightly declining up to a fraction of treated firms of 0.5 and slightly increasing afterwards, which is consistent with the spillovers to control (treated) firms dominating the aggregate effect when the fraction of treated firms is small (large).

Figure 7 shows the results of the spillover analysis for export turnover (*Log Export Turnover*). The direct treatment effect when the fraction of treated firms is zero is -12.7

per cent. The increasing dotted and solid lines show that there are positive spillovers both to control firms and within treated firms. For the control firms, this result indicates that they do benefit from the favorable change in their competitive environment by increasing their export revenues with more treated firms in the industry. For the treated firms this finding means 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. 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 as a result of these heterogeneous spillovers. This again means that not accounting for spillover effects leads to underestimating the direct treatment effect. The dashed line in Figure 7 presents the aggregate effect of the decline in correspondent bank relationships. The slope is slowly increasing up to a fraction of treated firms of 0.5 and more pronouncedly increasing for larger fractions of treated firms, which is again consistent with the spillovers to control (treated) firms dominating the aggregate effect when the fraction of treated firms is small (large).

[Insert Figure 7 here]

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.

5.3 The mediating effect of state-ownership of local banks

The negative effects of the drop in correspondent banking relationships on firms' extensive and intensive margins of exporting that we document in the previous analyses beg the question whether there are factors that may help to alleviate these negative effects on local firms. In this section, we study whether state-ownership of local banks is such a mediating factor. State-owned banks may be better able to buffer the negative effects of a decline in correspondent bank relationships, as they may have less pressure to pass on increased costs of providing correspondent banking services to their customers, i.e. the local firms. They may also be able to provide easier access to alternative trade insurance products such as government-guaranteed schemes.

We therefore split our sample and analyze cities with an above-average number of state-owned banks and cities with a below-average number of state-owned banks separately. We run the same De Chaisemartin and D'Haultfoeuille (2022) differences-in-differences estimations as in our main analysis in the two sub-samples respectively. We also present results using the Borusyak et al. (2022) approach and OLS regressions for comparison and robustness. Figure 8 and Tables 7 and 8 report the results. Figure 8 clearly shows that the negative effect of a decline in correspondent banking relationships on affected firms' probability to export and their export turnover is concentrated among firms in cities with comparably few state-owned banks (right-hand side graphs). In cities with an above-average number of state-owned banks (left-hand side graphs), in contrast, there is no significant treatment effect on the probability to export and only a very small negative treatment effect on the export turnover.

We conclude that state-ownership of local banks significantly alleviates the negative effects on local firms' export activities from the decline in correspondent banking relationships.

[Insert Figure 8 here]

6 Conclusion

Our analysis highlights that de-globalization in banking can have substantially negative real effects for firms in emerging markets. Our results show that a decrease in correspondent banking services negatively affects both the extensive and the intensive margin of exports: Exporting firms are less likely to export and have a lower export turnover if a bank branch in their city has lost a correspondent banking relationship compared to exporting firms in cities where bank branches have not lost correspondent bank relationships. Moreover, firms cannot make up for their decreased export turnover with increased domestic turnover and thus have to lay off workers with some time delay.

While our study only investigates the short- to mid-term implications of withdrawal, the effects on trade may be long-lived, even if correspondent banks decide to reenter certain countries (in part because local knowledge and relationships need to be re-established). While correspondent banks from East Asia and the Pacific Region are growing, they still mainly provide services in domestic currencies and thus cannot make up for reduction in international U.S. dollar and euro transactions.

However, our analysis also shows that the negative effects on trade can be offset by government involvement. We find that state-ownership of local banks significantly alleviates the negative effects on local firms' export activities from the decline in correspondent banking relationships. Further possibilities to partially offset the negative effects on trade are trade insurance products for exporters that shift risk to a counterparty (e.g. as part of a government-backed scheme). While adopting new insurance relationships might increase the cost of export initially, this would allow firms to keep their export business running. With regard to the payment services that global correspondent banks offer, new technologies may provide solutions in the future. Currently, however, Fintechs only play a very small role in the market for trade-related cross-border payments, perhaps due to the high cost of compliance with financial crime regulations. This stands in stark contrast to their large presence in the market for international retail payments by consumers. On the demand side, larger

firms in particular seem to prefer using the services of their local bank to which they have an established relationship, at least so far. This may change as firms become more digital overall and when reliable and trustworthy Fintech alternatives emerge. However, local banks themselves can act by bringing their compliance procedures up to the required international standards and by ensuring that their staff obtain professional certification, e.g. in customer due diligence, financial crime prevention, and money laundering risks in correspondent banking.

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Figures

Figure 1: Reasons for the withdrawal of correspondent banks

This figure shows local banks' responses to the question: "Out of all relevant causes for terminating your/others correspondent banking relationship, which do you consider most important?". The question was asked in an online survey that we conducted together with EBRD's Trade Facilitation Program at the end of 2019. 91 banks across 28 countries within the EBRD region responded to the question.

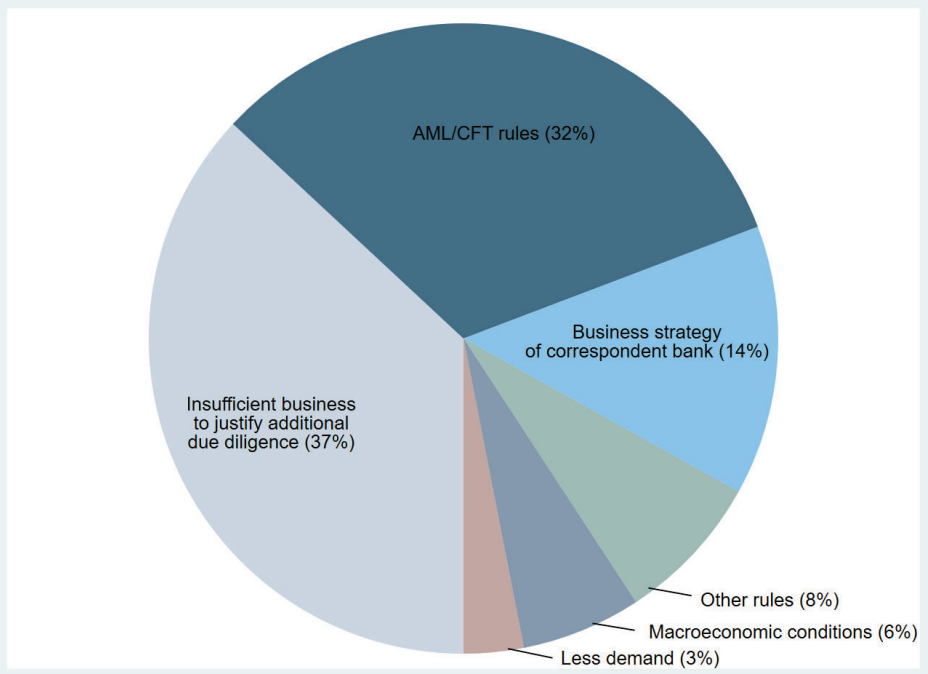


Figure 2: Local respondent banks with restricted access to different types of correspondent banking services

This figure shows the fraction of local banks who responded that a correspondent banking service has been "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 different types of correspondent banking services to your bank in 2013, 2015, 2017, and the year 2019". The question was asked in an online survey that we conducted together with EBRD's Trade Facilitation Program at the end of 2019. 91 banks across 28 countries within the EBRD region responded to the question.

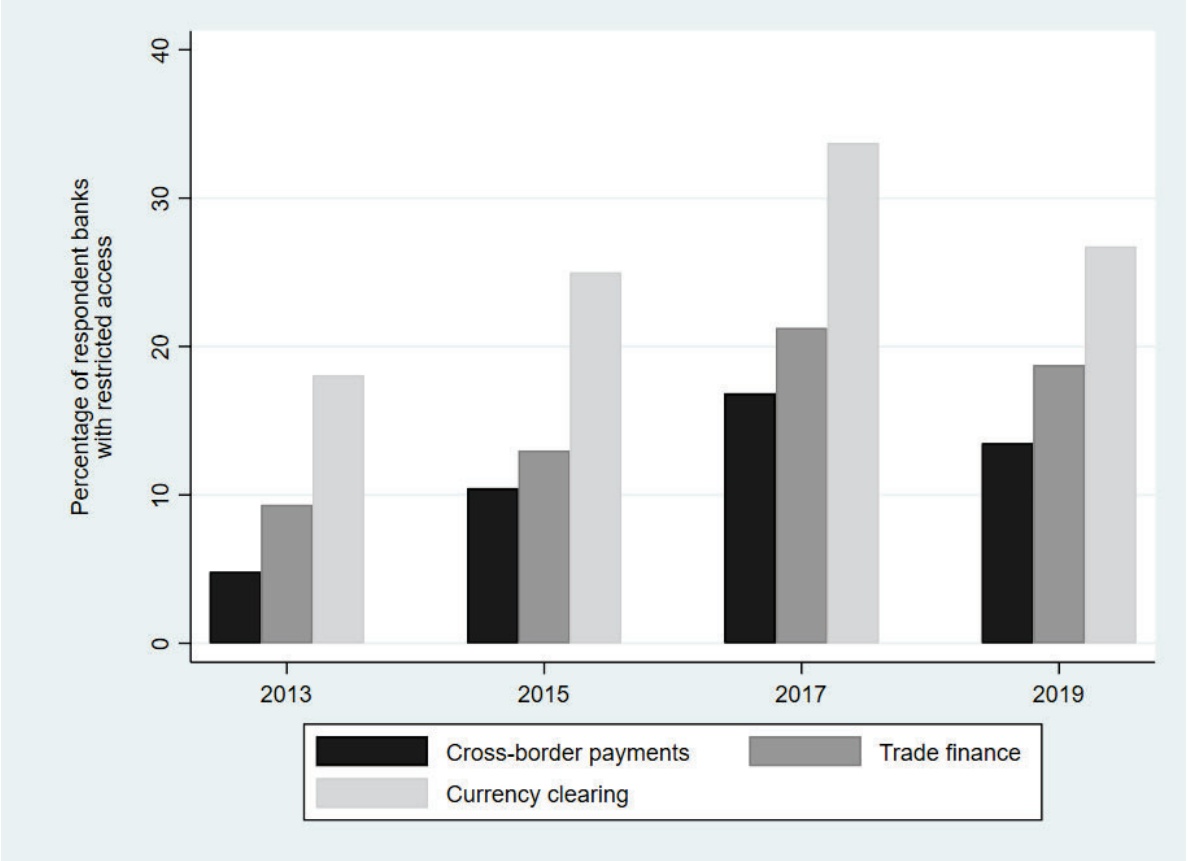


Figure 3: Firm exports after the withdrawal of a correspondent banking relationship

This figure shows firms' *Export Dummy* and firms' *Log Export Turnover* around the termination of a correspondent bank relationship in their city, compared to control firms. Treated firms are located in a city in which at least one bank branch has lost a correspondent banking relationship. Control firms are located in a city which has not lost a correspondent banking relationship up to the event-year. Reported coefficients are based on the differences-in-differences/event study approaches of de Chaisemartin and D'Haultfoeuille (2022) and Borusyak et al. (2022) as well as OLS. The reported coefficients are from a regression including firm controls (*Log Total Assets* and *Total Factor Productivity*), banks controls (*Equity/Total Assets*, *Loans/Customer Deposits*, *ROA*), and controlling for linear country trends and nonparametric industry trends. 95%-confidence intervals are based on standard errors clustered on the city level.

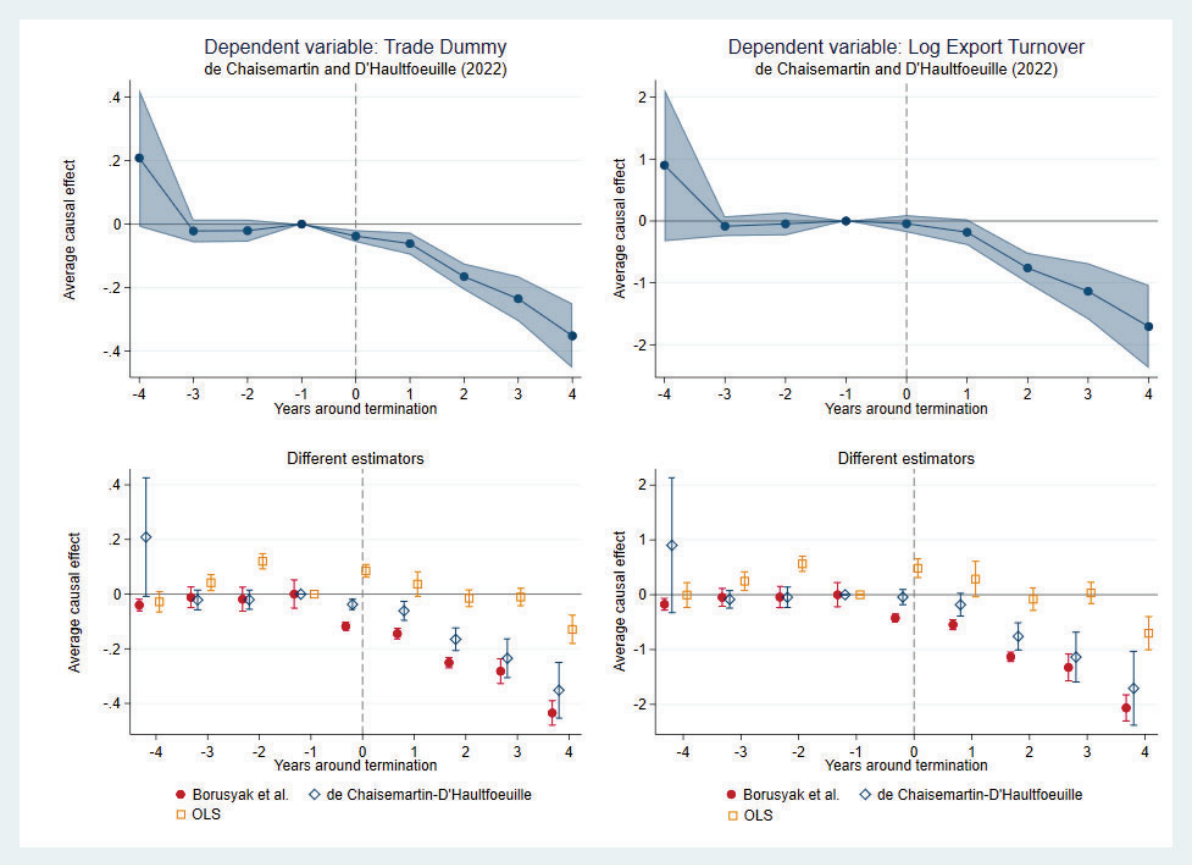


Figure 4: Firm turnover after the withdrawal of a correspondent banking relationship

This figure shows firms' *Log Turnover* and firms' *Log Domestic Turnover* around the termination of a correspondent bank relationship in their city, compared to control firms. Treated firms are located in a city in which at least one bank branch has lost a correspondent banking relationship. Control firms are located in a city which has not lost a correspondent banking relationship up to the event-year. Reported coefficients are based on the differences-in-differences/event study approaches of de Chaisemartin and D'Haultfoeuille (2022) and Borusyak et al. (2022) as well as OLS. The reported coefficients are from a regression including firm controls (*Log Total Assets* and *Total Factor Productivity*), banks controls (*Equity/Total Assets*, *Loans/Customer Deposits*, *ROA*), and controlling for linear country trends and nonparametric industry trends. 95%-confidence intervals are based on standard errors clustered on the city level.

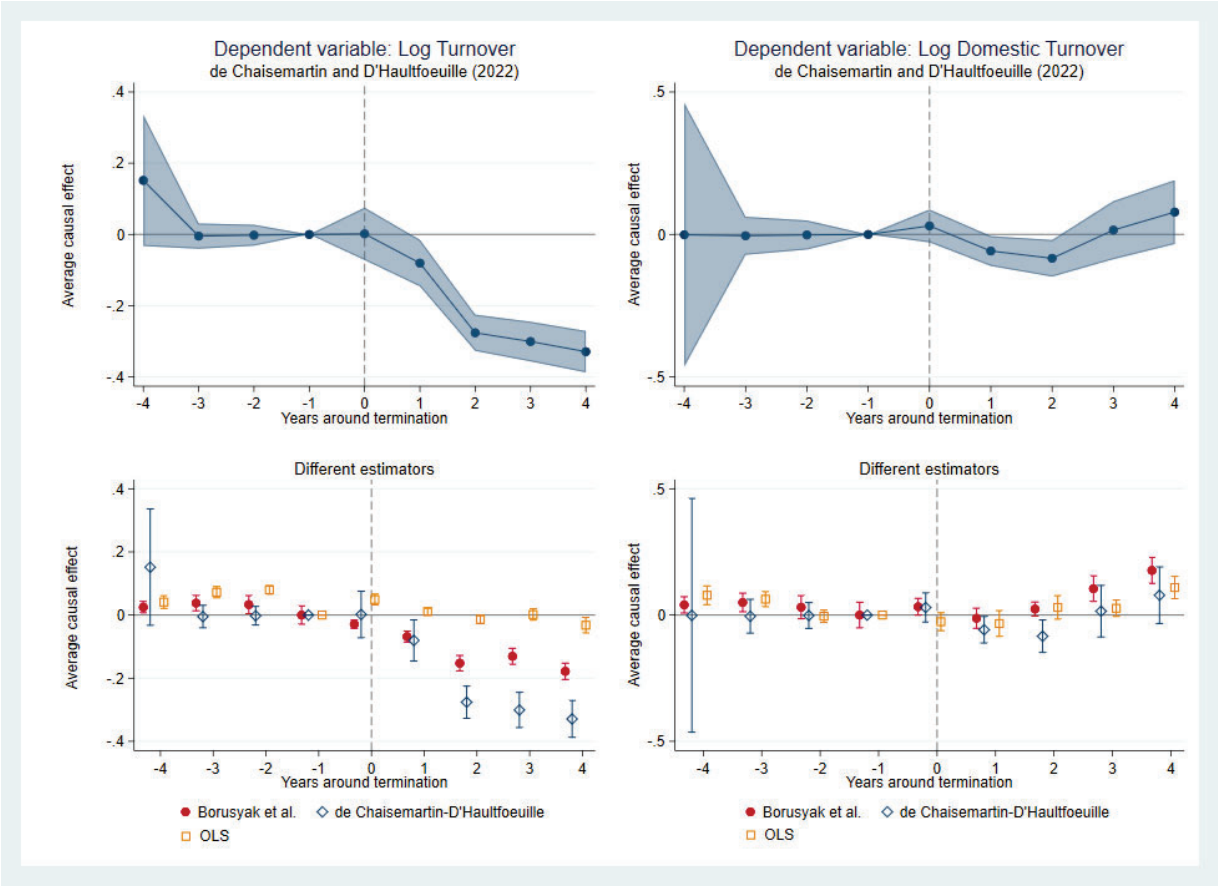


Figure 5: Firm employment after the withdrawal of a correspondent banking relationship

This figure shows firms' *Log Number of Employees* around the termination of a correspondent bank relationship in their city, compared to control firms. Treated firms are located in a city in which at least one bank branch has lost a correspondent banking relationship. Control firms are located in a city which has not lost a correspondent banking relationship up to the event-year. Reported coefficients are based on the differences-in-differences/event study approaches of de Chaisemartin and D'Haultfoeuille (2022) and Borusyak et al. (2022) as well as OLS. The reported coefficients are from a regression including firm controls (*Log Total Assets* and *Total Factor Productivity*), banks controls (*Equity/Total Assets*, *Loans/Customer Deposits*, *ROA*), and controlling for linear country trends and nonparametric industry trends. 95%-confidence intervals are based on standard errors clustered on the city level.

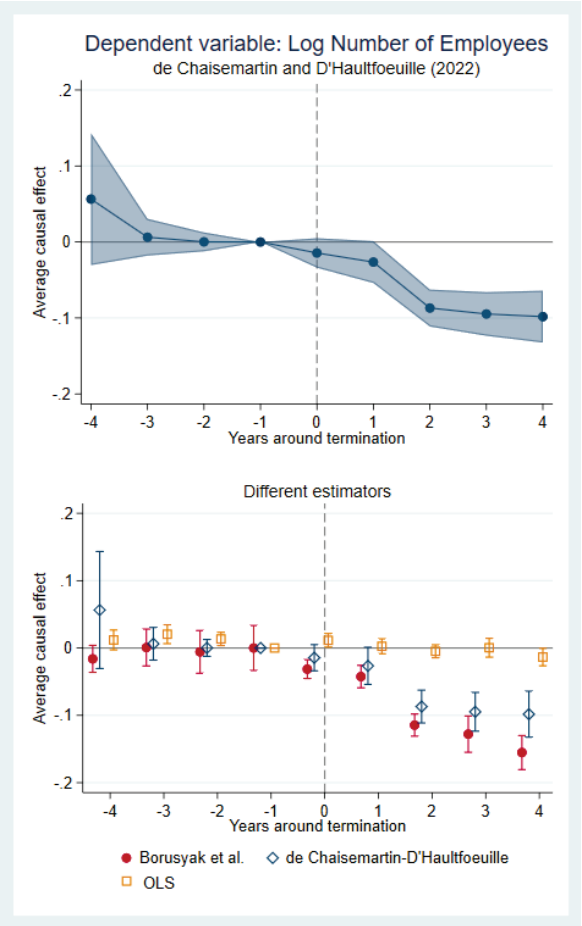


Figure 6: Industry spillovers after the termination of a correspondent banking relationship: Export Dummy

This figure illustrates the industry-level spillover effects of the termination of a correspondent banking relationship on treated and control firms. In particular, the figure plots previous exporters' *Export Dummy* as a function of the fraction of treated firms in each industry using equation (2).

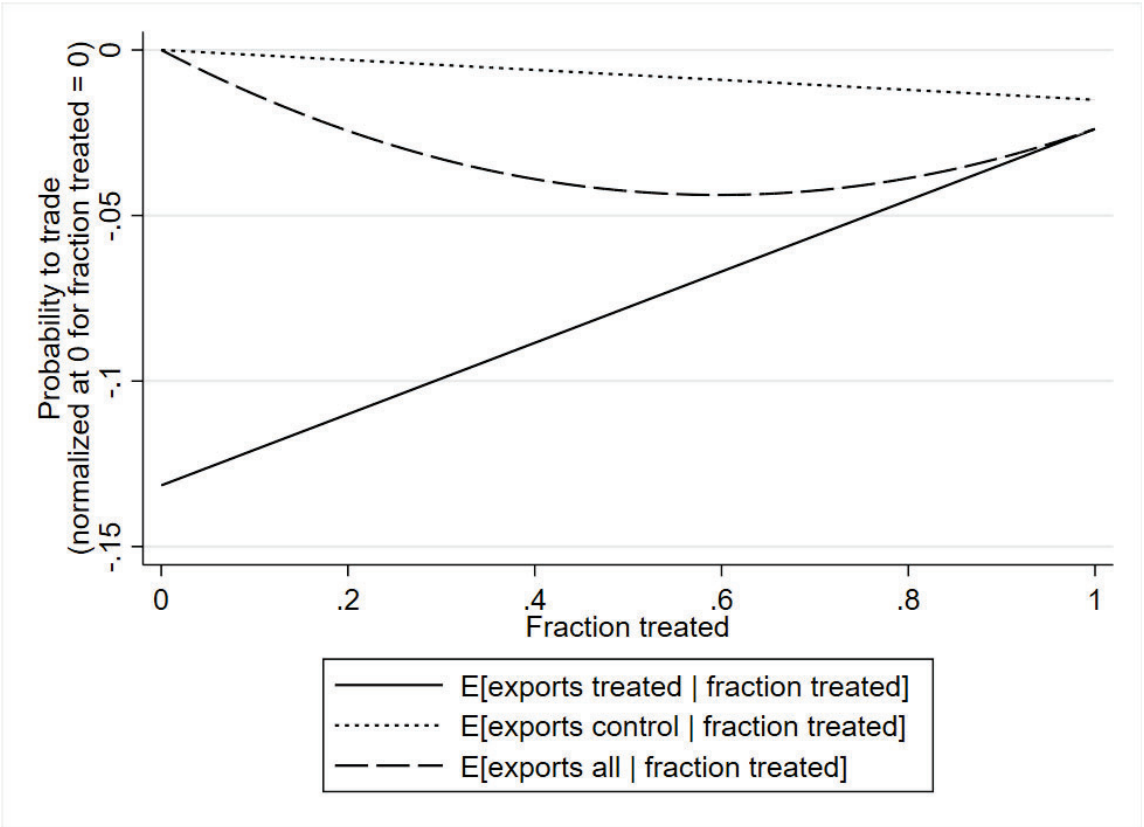


Figure 7: Industry spillovers after the termination of a correspondent banking relationship: Log Export Turnover

This figure illustrates the industry-level spillover effects of the termination of a correspondent banking relationship on treated and control firms. In particular, the figure plots previous exporters' *Log Export Turnover* as a function of the fraction of treated firms in each industry using equation (2).

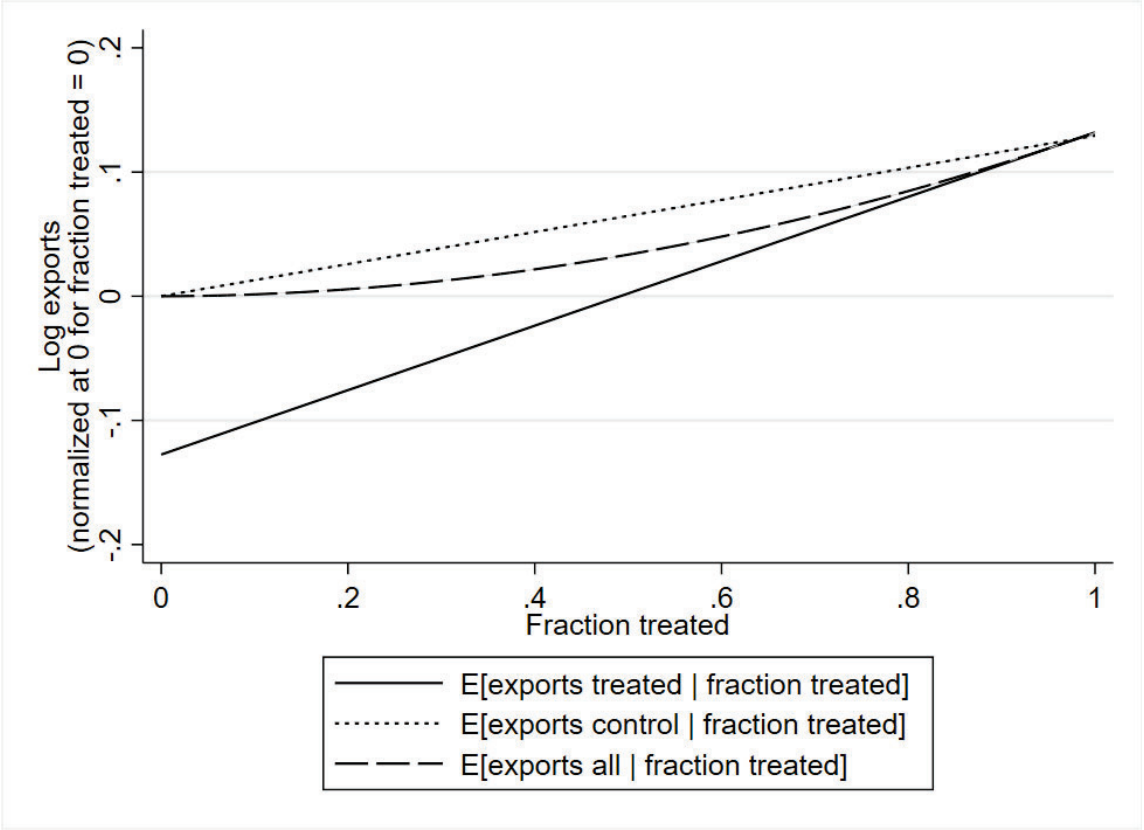
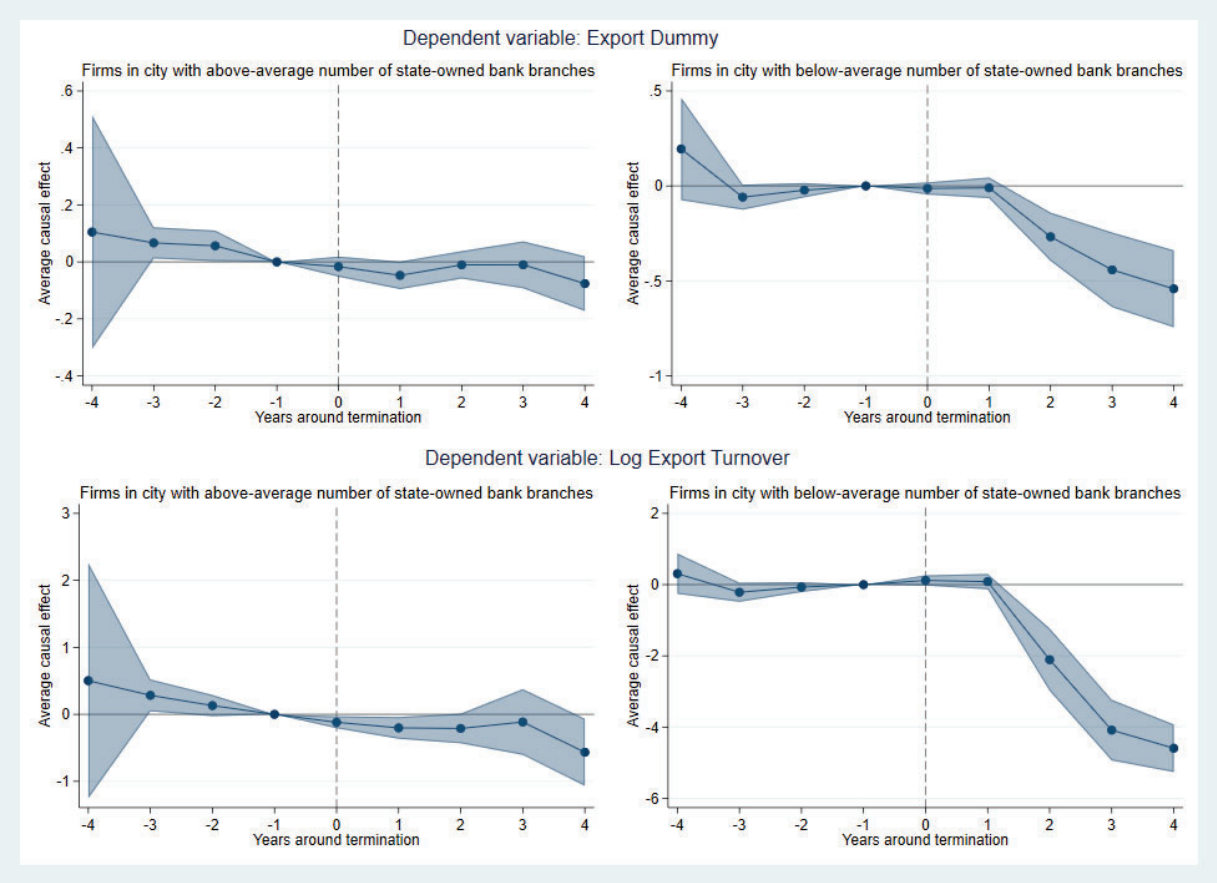


Figure 8: Effect in cities with an above and in cities with a below average number of state-owned bank branches

This figure shows firms' *Export Dummy* and *Log Export Turnover* around the termination of a correspondent bank relationship for the subsample of firms who have an above-average number of state-owned bank branches in their city and the subsample of firms who have a below-average number of state-owned bank branches in their city. Treated firms are located in a city in which at least one state-owned bank branch has lost a correspondent banking relationship. Control firms are located in a city in which no bank has lost a correspondent banking relationship up to the event year. Reported coefficients are based on the differences-in-differences/event study approach of de Chaisemartin and D'Haultfoeuille (2022). The reported coefficients are from a regression including firm controls (*Log Total Assets* and *Total Factor Productivity*), banks controls (*Equity/Total Assets*, *Loans/Customer Deposits*, *ROA*), and controlling for linear country trends and nonparametric industry trends. 95%-confidence intervals are based on standard errors clustered on the bank level.



Tables

Table 1: **Data Coverage**

This table shows the coverage of our data. It only includes firms that export at least once in our sample period.

	Number of cities (over 13 years)	Mean number of bank branches in city	% surveyed branches per city	Number of banks in country[15]	% number surveyed banks	% assets surveyed banks	Average number of firms in city
Bosnia-Herzegovina	101	6.5	65.4	23	34.8	65.7	390.8
Croatia	148	8.6	43.7	24	12.5	45.6	1,004.0
Hungary	417	5.3	55.0	21	28.6	57.0	1,467.1
Turkey	249	28.2	21.7	34	17.7	18.9	7.4

[15]= all banks with BEPS branch data

Table 2: Treatment and control group

This table shows relevant firm characteristics of treated and control firms of the complete and matched sample of exporters in the year before treatment. Treated firms are located in a city in which at least one bank branch has lost a correspondent banking relationship. Control firms are located in a city which has not lost a correspondent banking relationship throughout the sample period (complete sample) or which 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 *Export Turnover*, *Total Assets* and *Total Factor Productivity* (lowest Mahalanobis distance). We report the normalized difference following Imbens and Wooldridge (2009).

PANEL A: COMPLETE SAMPLE

	Firm characteristics					**Bank characteristics**
	Export Turnover	Total Assets	Total Factor Productivity	Number of Employees	Age	Cut relationships (branch level) over branches in city
Treated firms (N= 23,973)						
Mean	1,253	2,594	0.340	29.8	13.6	0.596
Median	82	612	0.327	8.0	12.0	0.333
SD	3,098	4,050	0.902	46.4	10.5	0.591
Control firms (N= 14,888)						
Mean	1,101	2,972	0.294	17.1	11.2	0
Median	72	1,025	0.333	5.0	9.0	0
SD	2,836	4,150	0.933	33.4	10.4	-
t(Difference)	4.85	-8.87	3.02	17.03	21.24	122.89
Normalized difference (Imbens/Woolridge)	0.036	-0.065	0.036	0.223	0.160	1.008

PANEL B: MATCHED SAMPLE

	Firm characteristics					**Bank characteristics**
	Export Turnover	Total Assets	Total Factor Productivity	Number of Employees	Age	Cut relationships (branch level) over branches in city
Treated firms (N= 21,588)						
Mean	2,302	5,325	0.308	46.0	14.2	0.604
Median	96	729	0.292	8.0	13.0	0.333
SD	8,320	16,114	0.888	118.3	10.5	0.601
Control firms (N= 21,588)[16]						
Mean	2,527	6,159	0.270	48.5	14.3	0
Median	147	1,237	0.235	12.0	13.0	0
SD	8,902	16,478	0.859	111	11.0	-
t(Difference)	-1.75	-3.55	-1.30	2.94	-1.01	78.22
Normalized difference (Imbens/Woolridge)	-0.018	-0.036	-0.016	0.031	-0.011	1.004

Table 3: 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 city. 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 *Export Turnover*, *Total Assets* and *Total Factor Productivity* (lowest Mahalanobis distance). Bank characteristics are the branch-weighted average per city.

	Unit	N	Mean	Median	Min	Max	SD
Firm-variables (26,000 firms over 13 years)							
Export Turnover	1,000 Euros	26,000	2,220	97	0	72,098	8,427
Total Assets	1,000 Euros	26,000	5,354	767	2.7	126,610	15,969
Number of Employees		21,881	43.9	8.0	1.0	867.0	113.3
Total Factor Productivity		26,000	0.3	0.3	-13.6	8.2	0.9
Age	Years	24,024	14.1	13.0	0.0	164.0	10.7
Bank-variables (averaged on city level, 706 cities)							
Total Assets	1,000 Euros	25,908	12,000,000	6,647,126	122,267	75,000,000	14,000,000
Equity/Total Assets	%	25,908	12.1	11.9	7.3	29.5	2.2
Loans/Customer Deposits		25,908	7,482	6,970	3,916	16,738	1,075
ROA	%	25,908	0.6	0.9	-4.2	2.1	0.8

Table 4: Firm exports after the termination of a correspondent banking relationship

This table shows difference-in-difference estimates on firms' *Export Dummy* and *Log Export Turnover* around the termination of a correspondent bank relationship in firms' city, compared to control firms based on the estimator introduced by de Chaisemartin and D'Haultfoeuille (2022). Treated firms are located in a city in which at least one bank branch has lost a correspondent banking relationship. Control firms are located in a city which has not 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 *Export Turnover*, *Total Assets* and *Total Factor Productivity* in the pre-event year. Firm controls include *Log Total Assets* and *Total Factor Productivity*, banks controls include *Equity/Total Assets*, *Loans/Customer Deposits*, and *ROA*. Standard errors are clustered on the city level and are shown in parenthesis.

	Dependent var.: Export Dummy				Dependent var.: Log Export Revenues			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Effect at t=0	-0.055***	-0.048***	-0.013	-0.038***	-0.053	-0.070*	-0.006	-0.043
	(0.007)	(0.007)	(0.009)	(0.011)	(0.061)	(0.042)	(0.056)	(0.068)
Effect at t=1	-0.092***	-0.068***	-0.013	-0.061***	-0.218**	-0.164**	0.047	-0.182*
	(0.010)	(0.013)	(0.013)	(0.021)	(0.099)	(0.077)	(0.075)	(0.100)
Effect at t=2	-0.182***	-0.154***	-0.030**	-0.165***	-0.833***	-0.680***	-0.311***	-0.760***
	(0.023)	(0.018)	(0.014)	(0.019)	(0.142)	(0.096)	(0.084)	(0.109)
Effect at t=3	-0.258***	-0.216***	-0.035	-0.235***	-1.239***	-0.978***	-0.048	-1.136***
	(0.036)	(0.030)	(0.026)	(0.032)	(0.248)	(0.188)	(0.124)	(0.230)
Effect at t=4	-0.380***	-0.336***	-0.046	-0.352***	-1.825***	-1.561***	-0.236	-1.706***
	(0.056)	(0.046)	(0.038)	(0.047)	(0.353)	(0.250)	(0.214)	(0.307)
Placebo at t=-2	-0.050***	-0.021	-0.014	-0.020	-0.119*	-0.028	-0.039	-0.046
	(0.017)	(0.017)	(0.012)	(0.017)	(0.064)	(0.076)	(0.146)	(0.071)
Placebo at t=-3	-0.036	-0.049**	-0.001	-0.021	-0.129	-0.134	0.103*	-0.084
	(0.022)	(0.024)	(0.015)	(0.017)	(0.101)	(0.082)	(0.055)	(0.086)
Placebo at t=-4	-0.120*	0.220	0.015	0.209*	-0.639**	0.937	-0.638***	0.902
	(0.063)	(0.138)	(0.087)	(0.124)	(0.300)	(0.616)	(0.243)	(0.619)
Effect at t=0 based on N firm-years	102,311	96,105	96,105	96,105	96,092	91,741	91,741	91,741
Effect at t=0 based on N switchers	22,059	21,289	21,289	21,289	19,161	18,900	18,900	18,900
Firm and bank controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Nonparametric industry trends	No	No	No	Yes	No	No	No	Yes
Nonparametric country trends	No	No	Yes	No	No	No	Yes	No
Linear industry trends	No	No	Yes	No	No	No	Yes	No
Linear country trends	No	No	No	Yes	No	No	No	Yes
Pre-event mean	1.00	1.00	1.00	1.00	4.73	4.73	4.73	4.73

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

Table 5: Firm turnover after the termination of a correspondent banking relationship

This table shows difference-in-difference estimates on firms' *Log Turnover* and *Log Domestic Turnover* around the termination of a correspondent bank relationship in firms' city, compared to control firms based on the estimator introduced by de Chaisemartin and D'Haultfoeuille (2022). Treated firms are located in a city in which at least one bank branch has lost a correspondent banking relationship. Control firms are located in a city which has not 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 *Export Turnover*, *Total Assets* and *Total Factor Productivity* in the pre-event year. Firm controls include *Log Total Assets* and *Total Factor Productivity*, banks controls include *Equity/Total Assets*, *Loans/Customer Deposits*, and *ROA*. Standard errors are clustered on the city level and are shown in parenthesis.

	Dependent var.: Log Turnover				Dependent var.: Log Domestic Turnover			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Effect at t=0	-0.017	0.002	-0.014	0.002	0.037	0.026*	-0.013	0.030
	(0.040)	(0.011)	(0.022)	(0.039)	(0.030)	(0.014)	(0.019)	(0.028)
Effect at t=1	-0.134***	-0.031**	-0.028	-0.081**	-0.055***	-0.028	-0.055	-0.058*
	(0.039)	(0.012)	(0.029)	(0.037)	(0.020)	(0.026)	(0.035)	(0.031)
Effect at t=2	-0.451***	-0.126***	-0.097**	-0.276***	-0.181***	-0.001	-0.081	-0.084***
	(0.036)	(0.015)	(0.041)	(0.021)	(0.031)	(0.023)	(0.090)	(0.031)
Effect at t=3	-0.518***	-0.101***	-0.116**	-0.301***	-0.109**	0.104**	-0.041	0.015
	(0.043)	(0.017)	(0.053)	(0.023)	(0.052)	(0.044)	(0.088)	(0.054)
Effect at t=4	-0.616***	-0.143***	-0.115	-0.329***	-0.084	0.169***	-0.005	0.079
	(0.047)	(0.019)	(0.075)	(0.022)	(0.052)	(0.052)	(0.146)	(0.052)
Placebo at t=-2	0.012	0.022*	-0.011	-0.002	0.022	0.006	-0.002	-0.002
	(0.031)	(0.011)	(0.010)	(0.014)	(0.032)	(0.023)	(0.029)	(0.028)
Placebo at t=-3	0.008	0.030***	-0.005	-0.005	0.035	0.046	0.003	-0.005
	(0.039)	(0.010)	(0.017)	(0.015)	(0.050)	(0.035)	(0.036)	(0.033)
Placebo at t=-4	0.118***	0.139	-0.080	0.152*	0.275***	-0.043	-0.017	-0.001
	(0.037)	(0.098)	(0.104)	(0.084)	(0.045)	(0.208)	(0.180)	(0.269)
Effect at t=0 based on N firm-years	100,757	96,105	96,105	96,105	95,750	91,406	91,406	91,406
Effect at t=0 based on N switchers	21,663	21,289	21,289	21,289	19,070	18,811	18,811	18,811
Firm and bank controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Nonparametric industry trends	No	No	No	Yes	No	No	No	Yes
Nonparametric country trends	No	No	Yes	No	No	No	Yes	No
Linear industry trends	No	No	Yes	No	No	No	Yes	No
Linear country trends	No	No	No	Yes	No	No	No	Yes
Pre-event mean	6.92	6.92	6.92	6.92	5.98	5.98	5.98	5.98

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

Table 6: Firm employment after the termination of a correspondent banking relationship

This table shows difference-in-difference estimates on firms' *Log Number of Employees* around the termination of a correspondent bank relationship in firms' city, compared to control firms based on the estimator introduced by de Chaisemartin and D'Haultfoeuille (2022). Treated firms are located in a city in which at least one bank branch has lost a correspondent banking relationship. Control firms are located in a city which has not 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 *Export Turnover*, *Total Assets* and *Total Factor Productivity* in the pre-event year. Firm controls include *Log Total Assets* and *Total Factor Productivity*, banks controls include *Equity/Total Assets*, *Loans/Customer Deposits*, and *ROA*. Standard errors are clustered on the city level and shown in parentheses.

	Dependent variable: Log Number of Employees			
	(1)	(2)	(3)	(4)
Effect at t=0	-0.020*	-0.018*	-0.010	-0.018
	(0.012)	(0.011)	(0.011)	(0.012)
Effect at t=1	-0.032*	-0.027	-0.012	-0.034*
	(0.017)	(0.017)	(0.016)	(0.019)
Effect at t=2	-0.105***	-0.085***	-0.007	-0.103***
	(0.016)	(0.010)	(0.037)	(0.016)
Effect at t=3	-0.117***	-0.090***	-0.028	-0.116***
	(0.020)	(0.014)	(0.046)	(0.020)
Effect at t=4	-0.127***	-0.092***	-0.074	-0.125***
	(0.025)	(0.018)	(0.049)	(0.024)
Placebo at t=-2	0.005	0.004	-0.014	0.002
	(0.006)	(0.007)	(0.013)	(0.006)
Placebo at t=-3	0.016**	0.016	-0.027	0.007
	(0.008)	(0.010)	(0.021)	(0.011)
Placebo at t=-4	0.033***	0.052	0.062	0.072
	(0.012)	(0.055)	(0.067)	(0.050)
Effect at t=0 based on N firm-years	84,980	84,418	84,418	84,418
Effect at t=0 based on N switchers	84,980	19,325	19,325	19,325
Firm and bank controls	No	Yes	Yes	Yes
Nonparametric industry trends	No	No	No	Yes
Nonparametric country trends	No	No	Yes	No
Linear industry trends	No	No	Yes	No
Linear country trends	No	No	No	Yes
Pre-event mean	2.49	2.49	2.49	2.49

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

Table 7: Firm exports after the termination of a correspondent banking relationship: OLS regression without dynamic effects

This table shows OLS difference-in-difference estimates on firms' *Export Dummy* and *Log Export Turnover* around the termination of a correspondent bank relationship for the subsample of firms who have a below-average number of state-owned bank branches in their city. Treated firms are located in a city in which at least one state-owned bank branch has lost a correspondent banking relationship. Control firms are located in a city 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 *Export Turnover*, *Total Assets* and *Total Factor Productivity* in the pre-event year. Firm controls include *Log Total Assets* and *Total Factor Productivity*, banks controls include *Equity/Total Assets*, *Loans/Customer Deposits*, and *ROA*. Standard errors are clustered on the city level and are shown in parenthesis.

	Dependent variable: Export Dummy				Dependent variable: Log Export Turnover			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Withdrawal × Post	-0.084***	-0.083***	-0.069***	-0.082***	-0.373***	-0.301***	-0.050	-0.306***
	(0.013)	(0.009)	(0.012)	(0.010)	(0.054)	(0.038)	(0.035)	(0.040)
Observations	271,013	207,699	207,699	207,696	234,016	180,033	180,033	180,029
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm and bank controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Country × Year Fixed Effects	No	No	Yes	No	No	No	Yes	No
Industry × Year Fixed Effects	No	No	No	Yes	No	No	No	Yes

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

Table 8: Firm exports after the termination of a correspondent banking relationship: Cities with an above-average number of state-owned bank branches

This table shows de Chaisemartin and D'Haultfoeuille (2022) difference-in-difference estimates on firms' *Export Dummy* and *Log Export Turnover* around the termination of a correspondent bank relationship for the subsample of firms who have an above-average number of state-owned bank branches in their city. Treated firms are located in a city in which at least one bank branch has lost a correspondent banking relationship. Control firms are located in a city 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 *Export Turnover*, *Total Assets* and *Total Factor Productivity* in the pre-event year. Firm controls include *Log Total Assets* and *Total Factor Productivity*, banks controls include *Equity/Total Assets*, *Loans/Customer Deposits*, and *ROA*. Standard errors are clustered on the city level and are shown in parenthesis.

	Dependent var.: Export Dummy				Dependent var.: Log Export Turnover			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Effect at t= 0	-0.012	-0.007	-0.007	-0.017	-0.106**	-0.049	-0.068	-0.117***
	(0.015)	(0.014)	(0.016)	(0.017)	(0.044)	(0.042)	(0.054)	(0.042)
Effect at t=1	-0.044***	-0.033	-0.013	-0.047*	-0.183**	-0.047	-0.036	-0.202***
	(0.017)	(0.020)	(0.017)	(0.025)	(0.075)	(0.072)	(0.104)	(0.077)
Effect at t=2	-0.025	0.027	0.004	-0.010	-0.174	-0.020	-0.219	-0.210**
	(0.021)	(0.021)	(0.028)	(0.025)	(0.116)	(0.106)	(0.133)	(0.082)
Effect at t=3	-0.031	0.046	0.028	-0.010	-0.016	0.224	0.086	-0.114
	(0.043)	(0.045)	(0.054)	(0.045)	(0.250)	(0.250)	(0.236)	(0.210)
Effect at t=4	-0.057*	0.056	0.006	-0.076*	-0.351	-0.007	-0.204	-0.566**
	(0.033)	(0.048)	(0.042)	(0.043)	(0.258)	(0.346)	(0.202)	(0.267)
Placebo at t=-2	-0.023	0.031	0.053**	0.056***	-0.012	0.084	0.117	0.131
	(0.026)	(0.021)	(0.025)	(0.022)	(0.098)	(0.069)	(0.117)	(0.089)
Placebo at t=-3	0.040***	0.009	0.066***	0.067**	0.267***	0.125	0.281**	0.284**
	(0.013)	(0.021)	(0.021)	(0.028)	(0.088)	(0.111)	(0.125)	(0.120)
Placebo at t=-4	-0.059	0.009	0.130	0.105	-0.111	0.395	0.690	0.503
	(0.074)	(0.200)	(0.233)	(0.195)	(0.318)	(0.820)	(0.935)	(0.989)
Effect at t=0 based on N firm-years	23,205	22,129	22,129	22,129	19,371	19,204	19,204	19,204
Effect at t=0 based on N switchers	10,453	9,957	9,957	9,957	7,896	7,849	7,849	7,849
Firm and bank controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Nonparametric industry trends	No	No	No	Yes	No	No	No	Yes
Nonparametric country trends	No	No	Yes	No	No	No	Yes	No
Linear industry trends	No	No	Yes	No	No	No	Yes	No
Linear country trends	No	No	No	Yes	No	No	No	Yes
Pre-event mean	1.00	1.00	1.00	1.00	5.35	5.35	5.35	5.35

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

Table 9: Firm exports after the termination of a correspondent banking relationship: Cities with a below-average number of state-owned bank branches

This table shows de Chaisemartin and D'Haultfoeuille (2022) difference-in-difference estimates on firms' *Export Dummy* and *Log Export Turnover* around the termination of a correspondent bank relationship for the subsample of firms who have a below-average number of state-owned bank branches in their city. Treated firms are located in a city in which at least one bank branch has lost a correspondent banking relationship. Control firms are located in a city 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 *Export Turnover*, *Total Assets* and *Total Factor Productivity* in the pre-event year. Firm controls include *Log Total Assets* and *Total Factor Productivity*, banks controls include *Equity/Total Assets*, *Loans/Customer Deposits*, and *ROA*. Standard errors are clustered on the city level and are shown in parenthesis.

	Dependent var.: Export Dummy				Dependent var.: Log Export Turnover			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Effect at t= 0	-0.032**	-0.032**	0.014	-0.013	0.122*	0.073	0.106**	0.118
	(0.016)	(0.013)	(0.014)	(0.013)	(0.070)	(0.063)	(0.052)	(0.073)
Effect at t=1	-0.044*	-0.025	0.020	-0.010	0.071	0.069	0.173**	0.085
	(0.023)	(0.022)	(0.023)	(0.021)	(0.106)	(0.099)	(0.074)	(0.075)
Effect at t=2	-0.290***	-0.244***	-0.065*	-0.268***	-2.209***	-1.856***	-0.264	-2.106***
	(0.056)	(0.048)	(0.038)	(0.045)	(0.505)	(0.361)	(0.274)	(0.506)
Effect at t=3	-0.476***	-0.415***	-0.042	-0.442***	-4.194***	-3.763***	-0.130	-4.084***
	(0.080)	(0.081)	(0.110)	(0.073)	(0.398)	(0.368)	(0.173)	(0.385)
Effect at t=4	-0.588***	-0.521***	0.027	-0.542***	-4.712***	-4.265***	-0.185	-4.593***
	(0.081)	(0.093)	(0.155)	(0.079)	(0.301)	(0.331)	(0.208)	(0.304)
Placebo at t=-2	-0.049***	-0.039**	-0.003	-0.022	-0.137*	-0.081	-0.046	-0.069
	(0.017)	(0.018)	(0.019)	(0.019)	(0.071)	(0.072)	(0.065)	(0.075)
Placebo at t=-3	-0.105***	-0.083**	0.008	-0.059*	-0.314*	-0.248	0.096	-0.214
	(0.035)	(0.036)	(0.022)	(0.033)	(0.179)	(0.183)	(0.085)	(0.139)
Placebo at t=-4	-0.028	0.249	0.081	0.195	-0.079	0.469*	-0.478	0.308
	(0.032)	(0.200)	(0.155)	(0.123)	(0.136)	(0.278)	(0.417)	(0.328)
Effect at t=0 based on N firm-years	42,930	40,350	40,350	40,350	41,801	39,506	39,506	39,506
Effect at t=0 based on N switchers	11,329	11,091	11,091	11,091	11,023	10,810	10,810	10,810
Firm and bank controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Nonparametric industry trends	No	No	No	Yes	No	No	No	Yes
Nonparametric country trends	No	No	Yes	No	No	No	Yes	No
Linear industry trends	No	No	Yes	No	No	No	Yes	No
Linear country trends	No	No	No	Yes	No	No	No	Yes
Pre-event mean	1.00	1.00	1.00	1.00	4.02	4.02	4.02	4.02

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

Appendix

Table A1: Variable definitions and sources

This table shows the definition of all variables used in our analysis as well as their sources.

Variable	Definition	Source
	Panel A: Firm variables	
Export Dummy	Dummy variable equal to one firm has export revenues in the given year	Orbis
Log Export Turnover	Revenues from a firm's export activities in log 1,000 euros	Orbis
Log Turnover	Total operating revenues in log 1,000 euros	
Log Domestic Turnover	Revenues from domestic economic activities in log 1,000 euros	Orbis
Log Number of Employees	Log number of employees	Orbis
Log 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 *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
City	City of headquarter	Orbis
	Panel B: Bank variables (averaged on city level weighted by number of branches)	
Lost Relationship	Dummy that equals one if at least one bank branch in city has lost a correspondent banking relationship up to year t.	BEPSIII and 2019 survey in co-operation with EBRD's Trade Facilitation Program
Cut relationships (branch level) over branches in city	Number of terminated correspondent bank relationships in a city up to year t (on bank branch level) divided by total number of bank branches in a city	BEPSIII and 2019 survey in co-operation with EBRD's Trade Facilitation Program
Branch network	Locations (cities) of all branches of a bank	BEPS III
Equity/Total Assets	Bank equity divided by total bank assets	Orbis BankFocus
Loans/Customer Deposits	Gross bank loans divided by a´bank's customer deposits	Orbis BankFocus
ROA	Return on assets caculated as net income divided by total assets	Orbis BankFocus
Total Assets	Total bank assets in 1,000 euros	Orbis BankFocus

Table A2: Firm exports after the termination of a correspondent banking relationship: OLS regressions with country×year and industry×year fixed effect

This table shows OLS difference-in-difference estimates on firms' *Export Dummy* and *Log Export Turnover* around the termination of a correspondent bank relationship. Treated firms are located in a city in which at least one state-owned bank branch has lost a correspondent banking relationship. Control firms are located in a city 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 *Export Turnover*, *Total Assets* and *Total Factor Productivity* in the pre-event year. Firm controls include *Log Total Assets* and *Total Factor Productivity*, banks controls include *Equity/Total Assets*, *Loans/Customer Deposits*, and *ROA*. Standard errors are clustered on the city level and are shown in parenthesis.

	(1) Dependent var.: Export Dummy	(2) Dependent var.: Log Export Turnover
Effect at t=0	-0.045***	-0.059**
	(0.010)	(0.028)
Effect at t=1	-0.071***	-0.053
	(0.013)	(0.036)
Effect at t=2	-0.106***	-0.054
	(0.017)	(0.041)
Effect at t=3	-0.106***	-0.050
	(0.021)	(0.059)
Effect at t=4	-0.120***	-0.032
	(0.023)	(0.039)
Effect at t=-2	0.010	-0.056*
	(0.010)	(0.030)
Effect at t=-3	0.008	-0.027
	(0.012)	(0.040)
Effect at t=-4	0.018	-0.028
	(0.011)	(0.040)
Observations	207,696	180,029
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

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

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ZEW – Leibniz-Zentrum für Europäische Wirtschaftsforschung GmbH Mannheim
ZEW – Leibniz Centre for European Economic Research

L 7,1 · 68161 Mannheim · Germany
Phone +49 621 1235-01
info@zew.de · zew.de