# From Trade to Terror: How Global Finance Impacts Local Economies

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Komm, wir legen unser Geld an
Den Strand und sehen weg
Und wenn wir wieder hinseh'n
Hat die See schon unser Geld versteckt.
*"Die Zeit", Lukas Meister*

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# Summary

Looking beyond the textbook understanding of banks as lenders and deposit-takers, this thesis delves into often overlooked functions of banks. The first chapter addresses the role global banks play in facilitating international trade. As "correspondent banks", they execute cross-border transfers and clear currencies for local respondent banks, providing the critical payment infrastructure that empowers local businesses to export their products worldwide.

However, due to the stricter enforcement of financial crime regulation, global banks have recently terminated numerous correspondent banking relationships, particularly in regions perceived as of high financial crime risk. This trend has been of concern to policy makers worldwide, who fear that it could hamper international trade and, in particular, deprive firms from middle and lower-income countries of the opportunity to reach the markets of richer parts of the world.

We are the first to document and quantify the impact of this disruption in international payments at the firm level to understand whether such concerns are warranted. Based on proprietary information on terminated correspondent banking relationships and firm-level export data from countries in emerging Europe, we compare the economic performance of firms whose local bank has lost a correspondent banking relationship to unaffected otherwise similar firms.

We find that firms experience lower export revenues or stop exporting entirely after their local respondent bank has lost access to correspondent banking services. Affected firms are only partly able to substitute lost export revenues by boosting domestic sales. As a consequence, they suffer a decrease in total revenues and lay off workers after some delay. These findings highlight the importance of firms' access to the global payment infrastructure for being able to export to customers around the world.

While the first chapter demonstrates the benefits of international financial flows for economies and individuals, the second chapter turns to their downsides. Specifically, it investigates how payments for illegal transplants from US kidney patients fund non-state violent conflicts around the world. International security agencies have expressed great concern over this matter, as non-state armed groups have been linked to organ trafficking. Of particular worry is groups' involvement in the "transplant tourism" industry, where individuals from high-income countries travel to lower-income destinations to obtain illegal organ transplants. While some descriptions of this phenomenon exist, we lack hard evidence for whether non-state armed groups are financing their attacks through transplant tourism.

The second chapter redresses this gap. I merge time-varying information on patients on the US waiting list for kidneys with geo-referenced data on local conflict events and hand-collected data on transplant infrastructure in countries known for transplant tourism. These data allow me to examine whether conflict patterns in localities with a transplant hospital show a response to an increase in US kidney demand, compared to localities that lack such infrastructure and where transplant tourism is unlikely to happen.

I find that higher US kidney demand increases violent conflicts in localities where transplanting is possible. Specifically, in localities with a transplant hospital, one additional standard deviation of kidney patients on the US waiting list increases the probability of conflict by 17% and the number of conflict events by 0.9%, compared to localities without transplant infrastructure. Notably, the impact of higher kidney demand is stronger for waiting list patients with higher income, who are more likely able to afford the exorbitant prices for an illegal kidney, and not significant for patients on dialysis who are unable to travel.

My findings also show that localized non-state armed groups are responsible for this increase in violence. Groups with a transplant hospital in their home region expand their attacks in response to a higher kidney demand, both in their home region and other regions, spreading violence over space. Finally, I observe that higher kidney demand is associated with an increase in suspicious payments from and to countries known for illegal organ trafficking, indicating that at least some of the financial transfers for this business pass the official banking system. Taken together, these results provide evidence that non-state armed groups finance their violent activities through illegal international organ trade.

The third chapter of this dissertation addresses another important function of banks that is often overlooked - lending between banks. Interbank markets are crucial in providing liquidity to both financial and real markets. However, to ensure that this liquidity is allocated effectively, interbank lenders must be able to distinguish between illiquid and insolvent peers. Whether banks are able to differentiate between the two is a subject of intense debate in the literature.

We weigh in on this question by introducing the relevance of portfolio quality between the lending and borrowing bank. We argue that banks can be effective monitors – but only for peers with a similar portfolio. To test our argument, we combine data from the German credit register with proprietary supervisory data on banks' loan portfolio quality. We then investigate whether interbank lenders adjust their lending in response to a decline in a peer's confidential portfolio quality.

Our findings reveal that, on average, lending banks do not adjust their interbank loans in response to a decline in a peer's portfolio quality, which suggests that they are not aware of such declines. However, when peers have similar loan portfolios in terms of industries and regions, lending banks significantly reduce their lending following a decline in the portfolio quality of the borrowing bank. Furthermore, banks with similar loan portfolios are more likely to extend credit to otherwise solvent banks that may have difficulty obtaining interbank loans due to the opacity of their loan portfolio. This indicates that banks with similar loan portfolios are better equipped to evaluate the solvency of their counterparts, possibly by using private information about their own loan portfolio.

Our results also suggest that banks are aware of this informational advantage. Even after controlling for established mechanisms of relationship lending and other well-known determinants of interbank lending, banks lend significantly more to similar banks. Taken together, these findings highlight the importance of considering portfolio similarity when assessing the effectiveness of peer monitoring in interbank markets.

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# Introduction

Standard economics textbooks introduce banks as intermediaries between lenders and deposit-takers. In this portrayal, the primary objective of a bank is to collect deposits from households and firms to provide loans for investments, such as buying a house or machinery (e.g., Mankiw 2020). While these functions undoubtedly are essential for the economy, they do not represent the only, nor necessarily the most common ways in which firms and households interact with banks. This thesis explores functions of banks beyond their traditional roles in lending and deposit-taking.

**Chapter I: Global payment disruption and firm exports**

The first chapter of this dissertation addresses banks' provision of payment services. It examines how disruptions in international payment systems impact local businesses.

Even though payment services are widely utilized by individuals and firms, they have not gained much interest in academic research or public debates. Two recent events have changed that. The first is the Wirecard scandal, which has launched a discussion about the importance of payment services to financial and economic stability, raising the question whether such services should fall under financial supervision (Langenbucher et al. 2020). The second event is the imposition of sanctions in response to the Russian aggression against Ukraine which aimed to isolate Russian firms from global markets. As part of these sanctions, the US Treasury banned all US financial institutions from processing payments for Sberbank or its subsidiaries, which required banks to terminate all correspondent banking relationships with Sberbank. The first chapter of this dissertation investigates what happens to local firms when such correspondent banking relationships become unavailable.

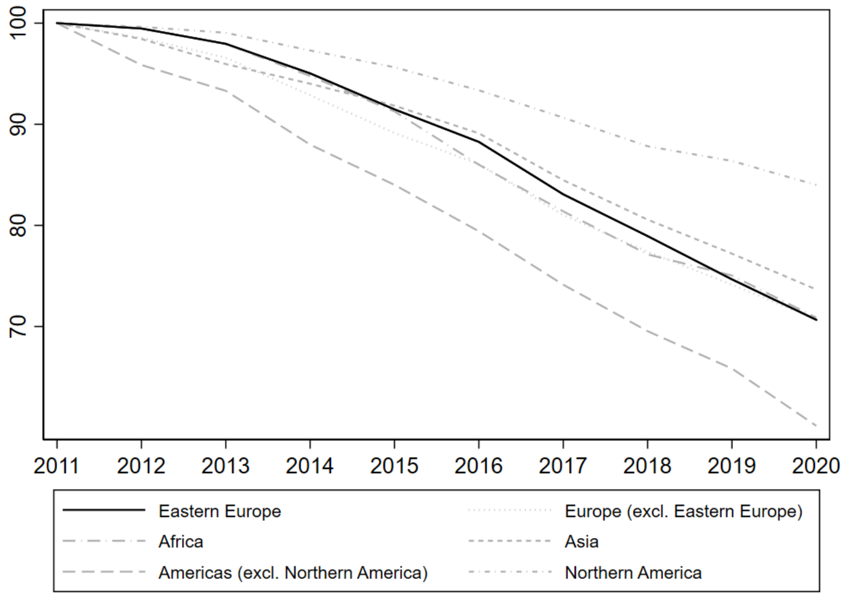
International trade relies on correspondent banking relationships, wherein a local bank, known as the respondent bank, maintains an account with a global correspondent bank. The global correspondent bank handles international payments or clears currencies on behalf of the local bank. In addition, global correspondent banks supply trade finance, i.e. insurance for exports happening on open account. As such, correspondent banks provide the critical payment infrastructure that enables trade, and in particular allows firms in poorer countries to export to the richer parts of the world.

Over the last decade, however, there has been a drastic decline in the number of correspondent banking relationships worldwide (see Figure 0.1). This decline owes to the surge in compliance costs, following the more stringent enforcement of anti-money laundering regulations starting from 2010 (Rice, Peter, and Boar 2020). The most prominent example of such compliance costs was the unprecedented US$8.9 billion fine imposed on the French correspondent bank BNP Paribas in June 2014 for violating US sanctions against Sudan, Cuba, and Iran as part of their correspondent banking business. Not only did this sentence exceed BNP Paribas' provisions for litigation costs by eight times, but it also established that any transaction that posed a threat to the integrity of the US financial system could be subject to a trial in a US court (Department of Justice 2014). Previously viewed as a low-risk/low-margin business, all of a sudden, correspondent banking was perceived by global banks as a high-risk/low-margin one (BIS 2016). In consequence, many banks severely pruned their correspondent banking networks, particularly in regions perceived as of high financial crime risk. This trend has been of concern to policy makers worldwide, who fear that it could hamper international trade and particularly deprive lower-income countries from trade opportunities (Rice, Peter, and Boar 2020; FSB 2017; BIS 2016; CGD 2015; World Bank 2015).

Our study is the first to document and quantify the impact of the global reduction in correspondent banks at the firm level. To do so, we combine confidential survey data on the loss of correspondent banking relationships by local banks, the geographical location of these banks' branches, and economic outcomes at the firm-level from Bureau van Dijk's Orbis database. Our sample covers firms in Bosnia and Herzegovina, Croatia, Hungary, and Turkey, countries that have traditionally relied heavily on correspondent banking services, making them relevant and representative for our study.

**Figure 0.1:** Decline in correspondent bank relationships

This figure shows the development of the number of correspondent banking relationships in different regions between 2011 and 2020, based on SWIFT data provided by the Bank for International Settlements (BIS). 2011 numbers are indexed to 100.



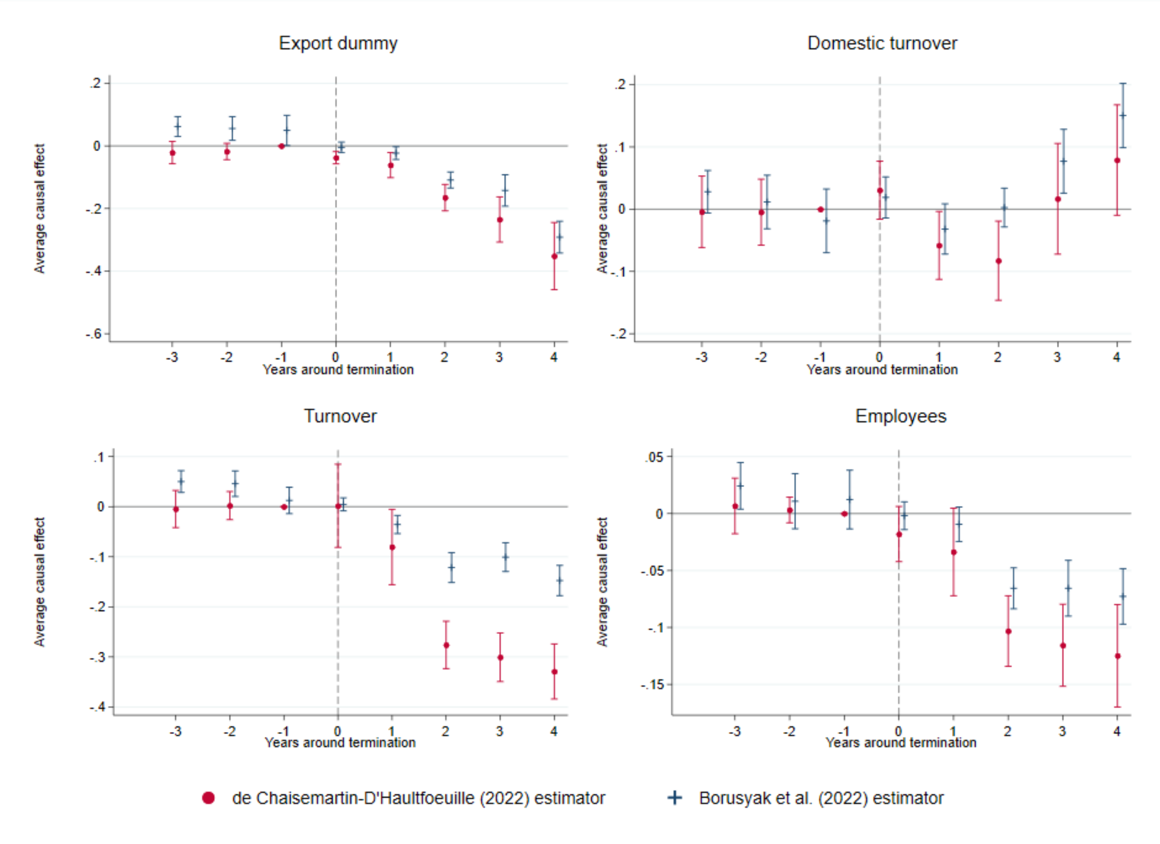

Based on these data, we analyze the effects of a terminated correspondent banking relationship on firms' exports, turnover, and employment. Specifically, we compare the changes in these economic outcomes for firms in a locality that has lost a correspondent banking relationship with similar firms in a locality that has not experienced any such loss up to the event year. We control for relevant firm and bank variables, industry-specific dynamic trends, and linear country trends.

The results from our analysis show that disruptions in payment services cause significant damage to local firms (see Figure 0.2). Firms in a locality where at least one correspondent banking relationship was terminated have a reduced probability of exporting and lower export revenues[1] in the years following the withdrawal, compared to unaffected firms. For instance, a firm's probability to export is 3.8 percentage points lower if a correspondent banking relationship was terminated in its locality in a given year, and 35.2 percentage points lower four years after the withdrawal has happened. As shown in the upper right hand part of Figure 0.2, affected firms can only partially

[1] See Figure 1.4 in Chapter I.

**Figure 0.2:** Terminated correspondent bank relationships and firm outcomes

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




compensate for the loss in export revenues by increasing local turnover, resulting in a significant decline in firms' total turnover (lower left hand part of Figure 0.2). Consequently, as shown in the lower right hand part of Figure 0.2, firms lay off workers with some delay.

To summarize, the initial chapter of this dissertation sheds light on the adverse impact that global payment disruptions can have on local economies. It thus highlights the critical role that international financial flows can play in fostering prosperity. The second chapter explores their downsides.

### Chapter II: Guns and kidneys – How transplant tourism finances global conflict

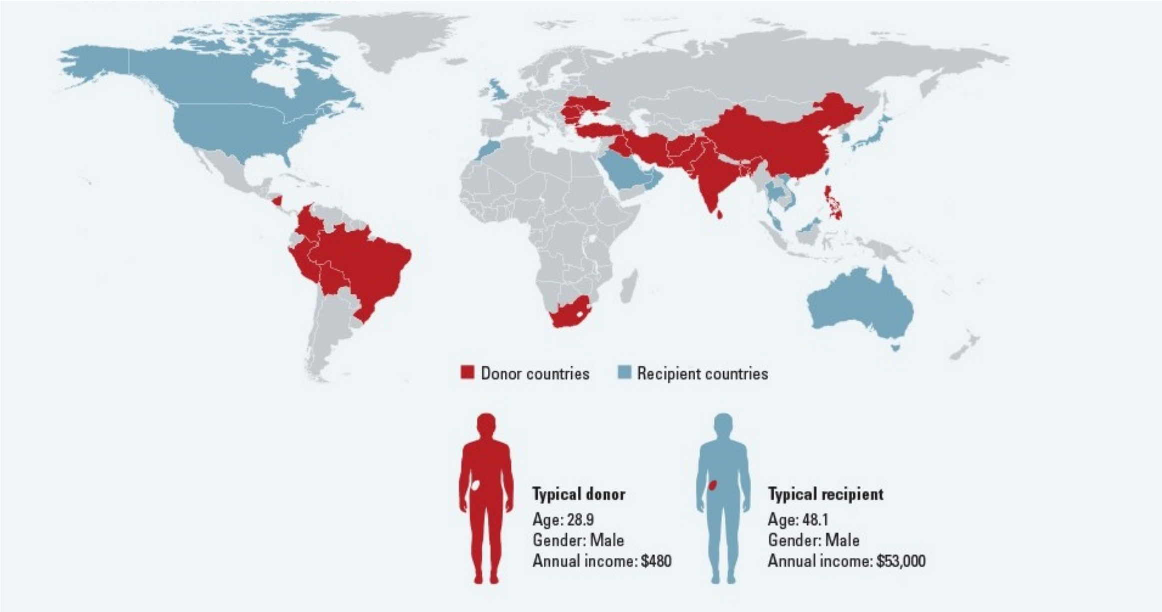
Banks not only enable legitimate global transactions but also process illicit payments that support criminal organizations and terrorist activities. To combat global non-state violence effectively, it is crucial to understand the sources and pathways of these illicit funds. However, tracing the funding mechanisms of non-state armed groups poses a significant challenge, as all involved parties have a vested interest in concealing the origins of illegal money. While existing research has identified funding sources from legal activities like donations (Limodio 2022) and mining business (Berman et al. 2017), discovering financing sources from illegal activities remains a challenge. The second chapter of this dissertation is one of the few that investigate an illegal source of funding by tracing whether non-state armed groups finance violent attacks through transplant tourism.

Transplant tourism is a lucrative business that involves individuals from high-income countries traveling to lower-income destinations to illegally obtain an organ in exchange for financial compensation. Figure 0.3 presents anecdotal evidence on transplant tourism agreements from newspaper articles and case studies. For example, a kidney patient from the US with a low likelihood of receiving a kidney via the official waiting list may choose to travel to a transplant hospital in India to receive a kidney "donated" by a local citizen. The cost of one illegal kidney can range between US$100,000 and US$200,000 for the recipient, while the donor may receive only US$500 to US$10,000. As a result, intermediaries are able to realize exorbitant profit margins of several hundred percent, even after accounting for surgery expenses and potential bribes.

While research on transplant tourism exists in medical anthropology, health ethics, and security studies (Scheper–Hughes 2000; Goyal et al. 2002; Cohen 2003; Scheper-Hughes 2003; Gill et al. 2008), we lack systematic evidence on whether armed groups use it as a means of financing their activities. My study fills this gap. Given the secrecy surrounding this issue, I approach the question by connecting visible dots. First, I measure kidney scarcity in the US by counting the number of patients on the waiting list for kidneys. This allows me to identify peaks in the waiting list that may

**Figure 0.3:** The global market for kidneys

This figure was compiled by Der Spiegel (31/2012) based on data from Coalition for Organ Failure Solutions, Organ Watch, and the European Society for Organ Transplantation. It visualizes anecdotal evidence from newspaper articles, security agency reports and case studies on global transplant tourism.



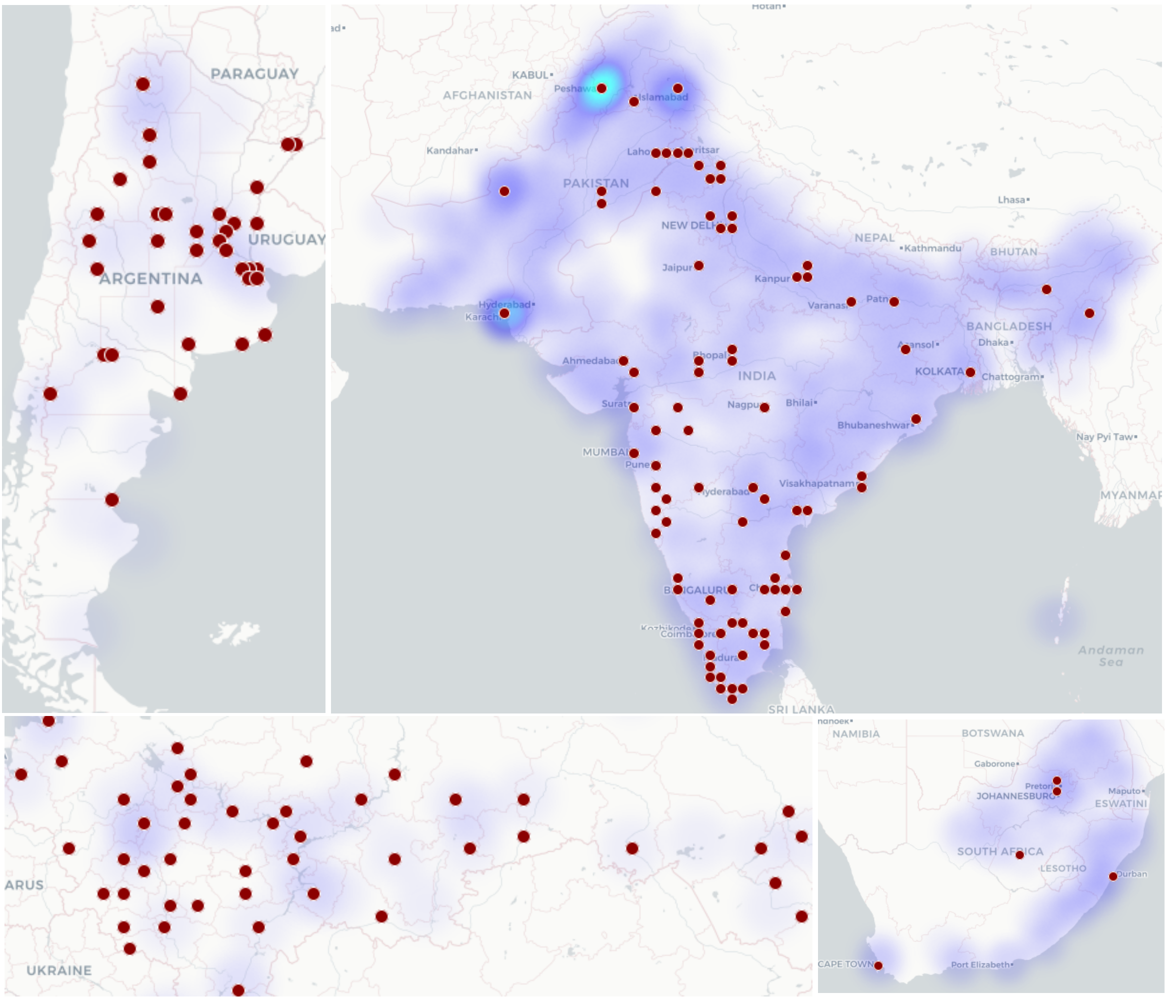

Source: Der Spiegel (31/2012)

indicate significant profit opportunities in the transplant tourism industry. Based on data from the Armed Conflict and Location & Event Data Project (ACLED), I then analyze incidents of non-state violent attacks in countries known as transplant tourism destinations. Within these countries, I compare conflict trajectories in localities where transplant tourism could occur to those in areas where such activities are unlikely to happen. I distinguish between these two types of locations using the fact that most reported transplant tourism cases happened in authorized transplant hospitals, alongside legal transplants. I therefore proxy the potential occurrence of transplant tourism in a locality by the (hand-collected) presence of an authorized transplant hospital. Figure 0.4 shows how non-state conflict and transplant hospitals are distributed over my sample countries.

My findings reveal that localities with transplant hospitals experience higher rates of violent attacks when US kidney demand is higher. Compared to areas without a transplant hospital, a one standard deviation increase in US kidney demand results in a 17% increase in the likelihood of non-state conflict and a 0.9% increase in the number

**Figure 0.4:** Spatial distribution of conflict events and transplant hospitals

This figure shows a heatmap of non-state violent conflicts from The Armed Conflict Location & Event Data Project (ACLED) that happened in my sample countries between January 2010 and March 2021. Deeper colors indicate a higher frequency of conflict. The map also shows hand-collected transplant hospitals as red dots.



of conflict events. I further analyze subsets of US kidney patients for which this association is particularly relevant, revealing that the effect sizes are more pronounced for those with labor income upon entering the waiting list. This finding aligns with the notion that individuals who can afford an illegal kidney through transplant tourism must have a certain level of financial stability. In addition, I observe that the effect is not present for patients on dialysis, which supports the hypothesis that transplant tourists must also be in decent health to undertake international travel.

After establishing the local relationship between transplant centers and violent attacks, I shift my focus to the armed groups responsible for these attacks. My sample primarily comprises small, localized non-state armed groups. These groups are well-suited to monopolize the transplant tourism industry in their respective regions, as they have strong local connections (Krause and Milliken 2009) that enable them to locate organ donors and collaborate with transplant surgeons and local authorities.

In a second step, I therefore use armed groups as the unit of analysis to investigate whether groups with access to transplant infrastructure are more likely to carry out attacks than groups without access to such infrastructure. Specifically, I compare the conflict trajectories of groups with a transplant hospital in their (hand-collected) home regions to those whose home region does not have transplant capacities. I find that groups with a transplant hospital at home are 13% more likely to carry out an attack in response to a one standard deviation increase in US kidney demand. These attacks occur both within the home regions of these groups and beyond, causing a spread of violence across space.

To explore whether kidney recipients are using the formal banking system to pay for organs, I use data from the FinCEN files, which contain leaked information on payments that have been flagged as suspicious to the US Financial Crimes Enforcement Network (FinCEN) by a global correspondent bank. I find that an increase in US kidney demand is associated with a higher number of suspicious payments to and from countries known for transplant tourism. Due to the limited and highly aggregated data, this association can only provide a first indicator that proceeds from transplant tourism indeed pass the official banking system.

Taken together, these findings provide evidence that non-state armed groups finance their violent activities through illegal organ trade. This is particularly concerning given the ongoing global shortage of legal organs and the associated growth potential of the transplant tourism industry.

**Chapter III: Banks of a feather – The informational advantage of being alike**

The third chapter of this thesis explores another function of banks that is often overlooked in standard textbooks: lending between banks. While interbank lending may on first glance appear merely as a specialized form of lending to firms, it differs from bank lending to other industries in both magnitude and function. First, interbank loans account for a large portion of all outstanding bank loans. In our sample from the German credit register, interbank loans represent 21% of German banks' total borrowing and 20% of their total lending. Although the overall value of these loans peaked at the end of 2008 with a total of €2.2 trillion and has decreased since, interbank lending is still of substantial volume.

Second, unlike loans to non-financial firms that are mainly used for real investments, interbank loans serve primarily as a tool for banks to manage their liquidity positions and balance uneven cash flows. The interbank market – when properly functioning – provides banks insurance against adverse liquidity shocks allowing them to keep lending money to the real economy without worrying about liquidity shortages (Bhattacharya et al. 1985). To fulfill this insurance function effectively, the interbank market must allocate funds to healthy banks in need while preventing insolvent banks from receiving funds. If it fails to allocate funds to healthy banks, for instance due to a lack of trust among lenders, a market freeze can occur. Conversely, if the interbank market fails to prevent insolvent banks from receiving funds, defaults on interbank loans can trigger widespread contagion effects (Flannery and Sorescu 1996; Freixas and Jorge 2008; Heider, Hoerova, and Holthausen 2015).

The question of whether interbank lenders can distinguish between illiquid and insolvent peers is a subject of intense debate. Some scholars argue that banks are adept at assessing the solvency of other banks (Rochet and Tirole 1996; Furfine 2001), while others believe that banks routinely fail to recognize (in)solvent counterparties (Freixas, Parigi, and Rochet 2000; Freixas and Jorge 2008). Our paper takes the position that banks can be effective monitors of peers, but only of those whose business model they understand well, as it is similar to their own. We hypothesize that having outstanding loans to the same industries and regions equips a bank with the relevant insights to effectively monitor a peer.

To examine this argument, we assess how banks react to a decline in the portfolio quality of a peer. Since banks' portfolio quality is private information which they might not reveal truthfully, we employ a confidential metric which is only observable to the supervisory authority: the probabilities of default that banks must disclose for each loan in their portfolio. These estimates reflect the banks' own evaluations of the likelihood of default for each loan. We aggregate this information weighted by portfolio composition to create a proxy for banks' loan portfolio quality which is not directly observable to peers. We confirm that this proxy is relevant for assessing a bank's solvency by demonstrating its ability to predict non-performing loans.

We find that, on average, interbank lenders do not reduce lending to a peer whose portfolio quality has declined, suggesting that banks are not able to monitor peers effectively. In line with our hypothesis, however, we do observe that banks reduce lending considerably once a *similar peer* experiences a decline in portfolio quality. We also demonstrate that similar banks are more likely to extend credit to otherwise solvent banks that may have difficulties obtaining interbank loans due to the opacity of their loan portfolio. These findings support our hypothesis that similar banks are better equipped to distinguish illiquid from insolvent peers, making them superior monitors.

We interpret our results as evidence that banks use private information about their own loan portfolio to evaluate the creditworthiness of a peer, giving them an informational advantage over interbank lenders who are not similar to the borrowing bank in need. Banks appear to be aware of this advantage, as we observe that they lend more to banks with similar portfolios. This preferential lending between similar banks is strong and persistent, even after accounting for established mechanisms of relationship lending and other known determinants of interbank lending.

Taken these findings together, the last chapter of this dissertation demonstrates that considering portfolio similarity in the analysis of interbank lending decisions can provide us with a more nuanced picture of banks' monitoring ability.

In conclusion, this thesis sheds light on important aspects of global banking, particularly with regards to payment and liquidity provisions. First, it highlights the critical role that the global payment infrastructure plays in facilitating international trade. Second, it reveals that non-state armed groups use transplant tourism to finance their violent activities. Finally, it shows how a similar loan portfolio can help to overcome information asymmetries in interbank markets.

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# Chapter I

# Global Payment Disruptions and Firm Exports

with Ralph de Haas and Karolin Kirschenmann

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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 only partially substitute these 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.

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

**JEL Codes:** F14; F15; F36; G21; G28

## 1.1 Introduction

The 2008-09 Global Financial Crisis halted a decades-long trend of financial globalization (Milesi-Ferretti and Tille 2011). New regulation, stricter supervision, and strengthened risk management have all prompted international banks to reduce or wind down foreign activities since then (Claessens 2017; De Haas and Van Horen 2017; Cerutti and Zhou 2018). As of yet, it remains unclear how this financial deglobalization is affecting real economic activity on the ground. We shed light on this issue by focusing on a specific episode of financial fragmentation: the sudden and stark decline in correspondent banking in response to the stricter enforcement of anti-money laundering regulation.

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

Against this background, it is potentially worrisome that global banks have radically pruned their correspondent bank relationships over the past decade (Figure 1.1). As we explain in Section 1.2, this retrenchment was caused by a sharp increase in compliance costs in the face of stricter enforcement of anti-money laundering regulation around 2014-15 (Rice, Peter, and Boar 2020). Policy makers are increasingly concerned that the broad-based withdrawal of correspondent banks is not only dampening international trade but also undermining the growth prospects of poorer economies (Rice, Peter, and Boar 2020; FSB 2017; BIS 2016; CGD 2015; World Bank 2015).

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

As a basis for our identification strategy, we join three key pieces of information: time-varying data on individual respondent banks' lost correspondent relationships; the geo-coordinates of these respondent banks' branches; and data on exports (and other real outcomes) of firms located near these branches. Information on the loss of correspondent bank relationships comes from two proprietary surveys among respondent banks in our four sample countries: the third wave of the Banking Environment and Performance Survey (BEPS III) and an on-line survey that we conducted at the end of 2019 together with EBRD's Trade Facilitation Program. We link these bank-level data to comprehensive information about the geographical location of their bank branches and then match this information with firm-level data from Bureau van Dijk's Orbis database. These combined data allow us to paint a detailed picture of the bank branches that surround each firm and to identify, at the local level, the impact of the withdrawal of correspondent banks on firm activity.

To do so, we employ the difference-in-differences estimator of intertemporal treatment effects by Chaisemartin and D'Haultfoeuille (2022). Their event-study approach for binary-and-staggered treatments allows for dynamic and heterogeneous treatment effects.[1] In our differences-in-differences estimations, we then compare the exports and other outcomes of firms in localities where at least one bank branch lost a correspondent banking relationship (treated firms) to firms in a locality where no bank branch lost a correspondent relationship up to the event year (control firms).

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

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

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

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

First, we show that before the sudden decline in correspondent banking, there were no systematically different pre-trends in the export performance of firms in treated versus control localities. Second, while our design does not depend on firms in treatment and control localities being similar in levels, such similarity would add further credibility to the common-trends assumption. We therefore offer evidence that correspondent banks' withdrawal is orthogonal to a battery of locality-level firm and bank characteristics. Throughout our analysis, we nevertheless control for these characteristics while also accounting for linear country and non-parametric industry trends. The inclusion of these controls absorbs many sources of unobserved heterogeneity that could otherwise bias our estimates.

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

These baseline results reflect local equilibrium effects of terminated correspondent relationships on the average exporting firm in a locality, regardless of whether a firm is a client of an affected bank or not. The fact that we find strong and persistent negative impacts indicates that, typically, firms cannot simply switch banks when their own bank can no longer provide correspondent banking services. We also show that all these results are robust to using a continuous treatment variable at the locality level rather than a binary one.

We then proceed by connecting individual firms to individual banks, using data on bank-firm relationships from the Orbis database. The advantage of this approach is that we now distinguish *within* localities between firms affected by the termination of correspondent relationships and those unaffected. Moreover, this approach lets us account for locality-level developments that may correlate with the decrease in correspondent bank relationships and could therefore confound our baseline estimates. We do so by including linear locality time trends. A downside is that we lose sight of possible equilibrium effects and that Orbis only provides information on a firm's main bank for larger enterprises, thus skewing the sample towards firms that may be less affected by lost correspondent banking relationships. We nevertheless find that the results using firm-bank linkages are qualitatively the same as those with the locality-matched sample in our main analysis.

Next, we present a spillover analysis in the vein of Berg, Reisinger, and Streitz (2021). We show that not accounting for heterogeneous spillover effects leads us to underestimate the direct treatment effect of a decline in correspondent banking on the probability to export and on export turnover. We find that treated firms are *less* negatively affected in their probability to export, the larger the fraction of other treated firms in the industry. One reason may be that with more treated firms in an industry, respective trading partners have fewer possibilities to buy their products from other suppliers elsewhere in the country. Moreover, we find that control firms, i.e. exporting firms in localities that do not experience a decline in correspondent bank relationships, suffer from weak spillover effects. Control firms' probability to export is slightly lower if the fraction of treated firms in the same industry is higher. This may reflect within-industry complementarities between suppliers across different parts of a country.

Lastly, we show that state-ownership of local banks tends to alleviate the negative effects on local firms' export activities from the decline in correspondent banking relationships. State-owned banks may be able to provide easier access to alternative trade insurance products such as government-guaranteed export schemes.

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

Other papers focus on the role of specific trade finance products for international trade. Niepmann and Schmidt-Eisenlohr (2017) and Ahn and Sarmiento (2019) both analyze how bank-level financial shocks reduce the supply of trade finance products (in particular, letters of credit) and, in turn, negatively affect firm exports.[2] In a similar vein, Demir and Javorcik (2020) and Crozet, Demir, and Javorcik (2022) show how a decline in bank-intermediated letter of credits negatively affected international trade flows during the COVID pandemic. Other work has assessed the role of different trade finance products such as export credit insurance (Auboin and Engemann 2014; Veer 2015) and export guarantees (Felbermayr and Yalcin 2013; Heiland and Yalcin 2021).

Our contribution here is to focus specifically on correspondent banking as a channel through which global banks improve the cross-border payment infrastructure and hence reduce financial frictions in global trade. For identification, we leverage the sudden and substantial increase in terminated correspondent banking relationships when global banks experienced a spike in the costs of financial crime regulation around 2014-15.

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

Using newly collected bank-level data on terminated correspondent bank relationships, we then trace the impact of these broken relationships across different localities within a number of European emerging markets. This allows us to quantify the real local effects of a shock to the availability of payment and trade finance services on exports and other firm-level outcomes.

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

The remainder of this chapter is organized as follows. Section 1.2 describes the institutional background, after which Section 1.3 introduces our data. Section 1.4 then sets out the empirical strategy, while Section 1.5 presents our results. Section 1.6 concludes.

## 1.2 Correspondent banking and global trade

This Section discusses the role of correspondent banking in international trade (Section 1.2.1); the recent unexpected decline in correspondent bank relationships (Section 1.2.2); and initial evidence on the impact of this decline on respondent banks (Section 1.2.3).

### 1.2.1 Correspondent banking: A primer

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

Second, correspondent banks provide trade finance products, such as letters of credit. Most international trade transactions take place on an open account basis and prepayment is rare (Asmundson et al. 2011; Ahn 2014). Correspondent banks then help overcome the commitment problems and limited enforceability that can inhibit direct payment between trading partners. Because correspondent banks maintain relationships of an on-going and repetitive nature, they are a credible intermediary between local banks and help to ensure that payment and shipment take place as specified in the contract between the ultimate importer and exporter. This is especially important when the risk of non-payment or non-shipment is high and enforcement is expensive (Schmidt-Eisenlohr 2013; Antras and Foley 2015) as is the case in many developing economies (CGD 2015).

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

### 1.2.2 Financial crime and correspondent banking

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

Since the 1970s, governments have been developing and harmonizing legal frameworks to counteract financial crime in international payment systems. For example, the recommendations of the Financial Action Task Force (FATF), the global watchdog on money laundering and terrorist financing, require correspondent banks to reveal the identity of all parties involved in a cross-border transaction and to perform due diligence on their customers. In practice, the weak enforcement of these legal frameworks has undermined the fight against financial crime (CGD 2015). The prosecution of offences only tightened in the aftermath of the Global Financial Crisis, when increased regulatory scrutiny unearthed extensive evidence of financial crimes in the banking sector (Tomasic 2011). Especially US regulators stepped up their enforcement as a result.

The stricter enforcement of financial crime legislation has been evident in the issuance of surging fines (CGD 2015). The most prominent example is the record US$8.9 billion fine issued to French correspondent bank BNP Paribas in June 2014 for violating sanctions against Sudan, Cuba and Iran. The 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 one had been the US$1.9 billion fine issued to HSBC in December 2012 for money laundering).

Crucially, in 2014 the US Department of Justice made clear that any global transaction threatening the integrity of the US financial system could be tried in front of a US court (Department of Justice 2014). While high fines appear to have been effective in preventing sanctions violations ever since the BNP Paribas trial, fines for violations of Anti-Money Laundering regulation remain on the rise. A recent example includes the three fines, totalling US$7.2 billion, that Goldman Sachs received in 2020 (Financial Crime News 2022).

### 1.2.3 The effects of de-risking by correspondent banks

The massive and unexpected 2014 fine for BNP Paribas accelerated a process of decline in global correspondent banking. The fine was widely regarded as a harbinger of stricter regulatory enforcement in the area of Anti-Money Laundering and Counter the Financing of Terrorism (AML/CFT). As such, it led to a sharp reassessment of the cost of regulatory compliance in correspondent banking. First of all, the expected costs of *non*-compliance increased sharply in view of the large penalties and the strict stance of the US Department of Justice. Second, the due diligence costs to comply with (US) financial crime legislation increased, too. Banks significantly increased spending on financial crime personnel (Dow Jones Risk & Compliance and ACAMS 2015; McKinsey 2017; Banking Exchange 2020) and highlighted inconsistencies in international regulation as another important cost factor (BIS 2016; SWIFT 2016).

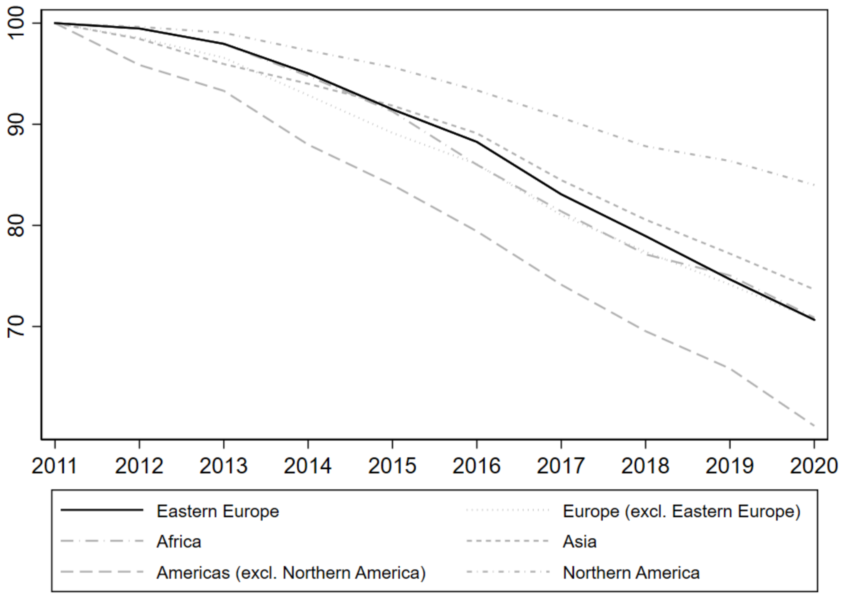
The sudden hike in compliance costs prompted banks to reconsider their business strategy with regards to correspondent banking, a business that was seen as shifting from a low-risk/low-margin to a high-risk/low-margin one (BIS 2016). Many banks severely pruned their correspondent banking networks by ending relationships that were no longer cost-effective or deemed too risky (BIS 2016; FSB 2017; Rice, Peter, and Boar 2020). As part of this "de-risking" strategy (CGD 2015), correspondent banks reduced their presence in risky regions in a wholesale rather than a country-by-country manner.

Figure 1.1 visualizes the global decline in correspondent banking. It shows the number of correspondent banks in different regions between 2011 and 2020 based on SWIFT data reported by the Bank for International Settlements. The bold line captures the decline in correspondent bank relationships in Eastern Europe, the region we focus on. Eastern Europe experienced a limited decline in correspondent bank relationships in 2012 and 2013, after while the withdrawal accelerated from 2014 onward. Overall, the region lost more than 25 percent of its correspondent bank relationships between 2011 and 2020.

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

**Figure 1.1:** Decline in correspondent bank relationships

This figure shows the development of the number of correspondent banking relationships in different regions between 2011 and 2020, based on SWIFT data provided by the Bank for International Settlements (BIS). 2011 numbers are indexed to 100.



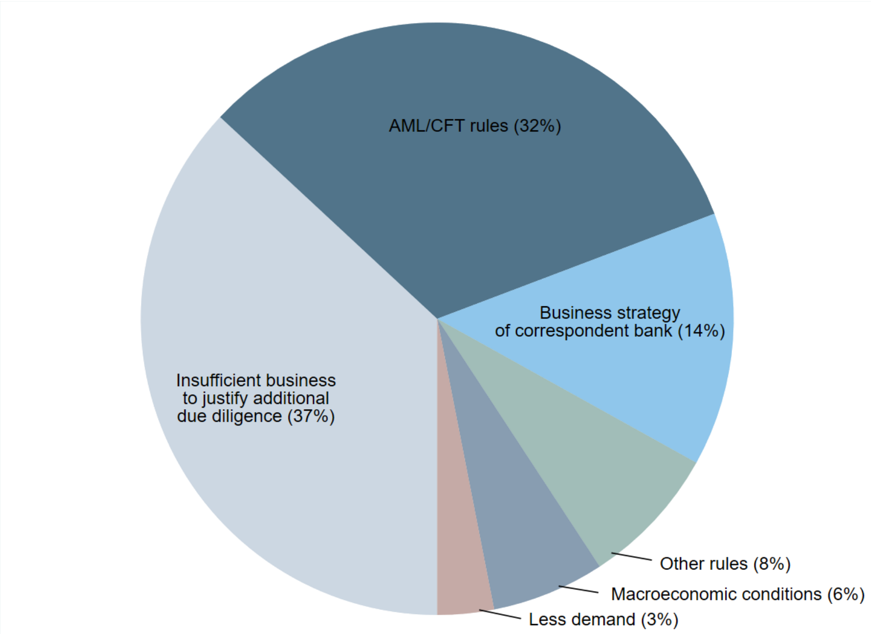

towards the end of 2019. Questions covered the period 2009–2019. Out of the 131 invited banks, 91 banks across 28 economies in Central and Eastern Europe, the former Soviet Union, and Northern Africa completed the entire questionnaire, a response rate of 69 per cent.[3]

Figure 1.2 shows that, according to respondent banks, the main reasons for the decline in correspondent banking were that it "does not generate sufficient business to justify the cost of additional customer due diligence" (37 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 increased due diligence costs and concerns about compliance with AML/CFT regulations, rather than a reduced demand, caused the decrease in

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

**Figure 1.2:** Reasons for the withdrawal of correspondent banks

This pie chart shows local respondent banks' answers to the question: "Out of all relevant causes for terminating correspondent bank relationships, which do you consider most important?". The question was asked in an on-line survey conducted together with EBRD's Trade Facilitation Program at the end of 2019. 91 banks across 28 countries answered the question.



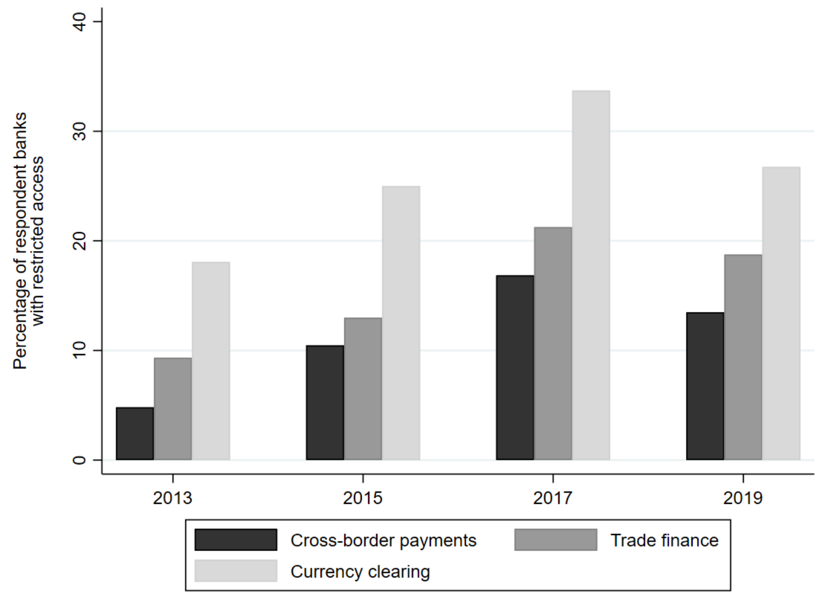

global correspondent bank relationships and services.

The decline in correspondent banking acted as a negative shock to the availability of international payment and trade finance services for local respondent banks and their clients, many of which were suddenly cut off from their long-standing providers of these services. The broad nature of the retrenchment of correspondent banks, combined with the concentrated nature of the industry (Del Prete and Federico 2014; Niepmann and Schmidt-Eisenlohr 2017) made it difficult to find alternative providers.

Our bank survey provides some first descriptive evidence on the local impact of the reduced availability of correspondent banking services. Figure 1.3 shows the fraction of local respondent banks that had difficulties in accessing, or were even entirely unable to access, three important types of correspondent banking services in the years 2013, 2015, 2017, and 2019. We observe a sharp uptick in the proportion of respondent banks that experienced difficulties in accessing cross-border payment transactions (black bars); trade finance (dark grey); and currency clearing (light grey). Importantly, respondent banks that continued to have access to these services, experienced a sharp increase in

**Figure 1.3:** Restricted access to correspondent banking services

This figure shows the fraction of local respondent banks that indicated that a particular correspondent banking service was "difficult to access" or "not available at all" in a given year. Local banks responded to the question: "Please score the availability of the following types of correspondent banking services to your bank in 2013, 2015, 2017, and 2019". The question was asked in an on-line survey together with EBRD's Trade Facilitation Program at the end of 2019. 91 banks across 28 countries answered the question.




their cost, of on average 35 per cent between 2017 and 2019 alone.

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

## 1.3 Data

Our empirical analysis focuses on four emerging European countries—Bosnia & Herzegovina, Croatia, Hungary, and Turkey. These countries were until recently heavily reliant on correspondent banking services and hence provide a 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). We were unable to include other countries from emerging Europe as they either do not report firm-level export data in Orbis or do not display sufficient variation in terminated correspondent banking relationships across localities.

We match several data sets at the firm level to estimate the impact of the decline in correspondent banking on firms' exports, turnover and employment. More specifically, our identification strategy relies on joining: (i) time-varying information for individual respondent banks about terminated correspondent banking relationships; (ii) data on the geo-coordinates of all branches of these respondent banks; and (iii) data on exports (and other real outcomes) of the firms that are geographically nearby these bank branches. We now discuss these data in turn. Appendix 1.A contains the definitions and sources of all variables.

### 1.3.1 Measuring the withdrawal of correspondent banks

We combine information from two new surveys of respondent banks to retrieve unique and time-varying information about lost correspondent bank relationships. The first source is 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. Appendix 1.B contains the BEPS III questions we use in this paper.

The BEPS III survey provides us with information about (changes in) correspondent banking relationships for 20 key respondent banks in our four sample countries. We supplement this with similar information on three additional respondent banks in these countries, collected as part of an on-line survey that we conducted in 2019 together with EBRD's Trade Facilitation Program.[4] This survey focused exclusively on banks' correspondent banking relationships. Appendix 1.B again contains the survey questions that we use.

### 1.3.2 Firm exports and other firm characteristics

To estimate the impact of the rapid decline in correspondent banking services at the grassroots level, we access firm-level data from Bureau van Dijk's Orbis database. Orbis provides comprehensive information on 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. We obtain the data via the Orbis flat files of June 2022 and ensure that our data cleaning is in line with Kalemli-Özcan et al. (2015) to construct a nationally representative sample for our four countries.

### 1.3.3 Bank branch networks and bank characteristics

We match our data on firms' exact geo-coordinates with information on all bank branches near these firms. This information was hand-collected as part of the BEPS III survey by either contacting banks or by downloading data from bank websites and subsequently double-checking them with the bank and the SNL Financial database. In total, we have data on the geo-coordinates of 48,399 branches: a near complete picture of the branching landscape in 2020. We merge this information with 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 (villages, towns, and cities) are spelled consistently in both data sets and then match firms and branches by locality. For instance, we link all Orbis firms in the Croatian city of Dubrovnik to all bank branches in

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

Dubrovnik. The (plausible) assumption is that a firm has access to all branches in the locality where it is incorporated and that it may be negatively affected by the loss of correspondent bank relationships of such local banks.[5] We thus focus on local equilibrium effects while assuming that local credit markets are competitive in nature, so that firms' access to banking services can be constrained by locality-level financial shocks. We include any locality in which we have at least one firm and at least one branch of a surveyed bank.

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

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

## 1.4 Empirical strategy

### 1.4.1 Identification

We exploit the loss of banks' correspondent bank relationships as an exogenous shock to firms' access to trade finance services at the local level. In a difference-in-differences framework, we compare, before and after this shock, firms' export performance, to-

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

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

tal revenues, and employment generation in treatment localities—where at least one bank lost a correspondent relationship—with observationally similar (matched) firms in control localities—where no such relationships were lost.

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

First, we show in Section 1.5.1 that before the sudden decline in correspondent banking, there were no systematically different pre-trends in the export performance of firms in treated versus control localities. This supports the idea that firms in both types of localities would have developed similarly in the absence of the global shock to correspondent banking.

Second, while our design does not depend on firms in treatment and control localities being similar in levels, such similarity would add further credibility to the common-trends assumption. We therefore offer evidence that correspondent banks' withdrawal (our treatment variable) was orthogonal to various locality traits. In the first two columns of Table 1.C1 of Appendix 1.C, we use a locality-year panel data set over the period 2012-2020[7] to estimate the relation between a broad set of time-varying locality characteristics and whether at least one local bank lost access to correspondent banking. These characteristics include local night-time light intensity (as a proxy for local economic development); the number of local firms; these firms' characteristics averaged at the locality level (total assets, total factor productivity, turnover, and number of employees); and local firm concentration expressed as a Herfindahl-Hirschman Index. We also include variables that characterize the local credit market: average total assets of the banks operating in the locality (weighted by each bank's number of local branches); their capitalization; loan-to-deposits ratio; and total loans outstanding. We also include banks' Herfindahl-Hirschman Index that gauges concentration in the local

[7]Unlike our main analyses, this analysis starts at 2012 because no comparable night-time light intensity data is available before 2012.

credit market. These local credit-market controls allow us to estimate the distinct effect of terminated correspondent bank relationships over and above general credit supply shocks at the locality level. Finally, we also include locality fixed effects in column 2. In columns 3 and 4, we present similar regressions while using a continuous outcome variable that measures the number of discontinued correspondent banking relationships in a year and locality, normalized by the number of branches in that locality. We then use Wald tests to check whether these locality characteristics jointly and significantly correlate with our treatment variables. The $p$-values at the bottom of Table 1.C1 show that we can never reject the null hypothesis of no systematic relation between, on the one hand, a large set of observable characteristics of local banks and businesses and, on the other hand, the locality-level decline in correspondent banking. That is, localities in which banks lost correspondent banking relationships and localities where banks did not, are similar across a broad array of covariates before the shock to global correspondent banking.

### 1.4.2 Matching

In our difference-in-differences estimations, we compare exports and other real-economic outcomes of firms in localities in which at least one bank branch lost a correspondent banking relationship (treated) to similar firms in localities where banks did not lose a correspondent banking relationship up to the event year (control). To provide unbiased estimates of the impact of the decline in correspondent banking, we match treated and control firms and keep those with common support in our sample.[8]

More specifically, we match each treated firm with one control firm from the same industry and country that also exports in the pre-event year. Using nearest neighbor matching, we select the control firm with the lowest Mahalanobis distance in terms of pre-event export turnover, total assets, and total factor productivity, calculated as the industry-adjusted residual of a two-factor Cobb-Douglas production function.[9] We match on total assets and productivity as the literature identifies these as the most important determinants of firm-level exports at the extensive and intensive margins

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

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

(Melitz 2003; Bernard et al. 2007).[10] We keep treated firms for which we find an appropriate control firm and for which we have at least two observations.

Table 1.1 provides summary statistics for the complete sample (Panel A) and the matched one (Panel B). We also report the difference in averages by treatment status, scaled by the square root of the sum of the variances. This normalized difference provides a scale-free measure of the difference in distributions. As a rule of thumb, Imbens and Wooldridge (2009) suggest that normalized differences below 0.25 (in absolute values) indicate sufficient similarity in the variable distribution in the treatment and control groups. Panel A of Table 1.1 shows that these normalized differences for the firm characteristics are already well below the 0.25 threshold in the complete exporter sample. Matching nevertheless further improves the similarity of the treatment and control groups with respect to observable firm characteristics, as indicated by the lower normalized differences in Panel B.

Table 1.1 also reports summary statistics for the decline in correspondent bank relationships in a locality, normalized by the number of bank branches in that locality (*Cut relationships (branch level) over branches in city*). This variable measures the extent of terminated relationships at the locality level. On average, around 60 per cent of the branches in a treated city lose a correspondent bank relationship.

We proceed with the matched sample in our regression analyses. The matched exporter sample consists of 23,751 firms across 706 cities. Table 1.2 provides summary statistics.

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

**Table 1.1:** Treatment-control balance in the full sample and the matched sample

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

**PANEL A: FULL SAMPLE**

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

**PANEL B: MATCHED SAMPLE**

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

**Table 1.2:** Summary statistics matched sample

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

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

### 1.4.3 Empirical specification

To gauge the impact of the sudden decline in correspondent banking on local firms' exports and other outcomes, we employ the difference-in-differences estimator of intertemporal treatment effects introduced by Chaisemartin and D'Haultfoeuille (2022). Their approach for binary-and-staggered treatments allows for dynamic and heterogeneous treatment effects. In a traditional design with two-way fixed effects (TWFE), we would estimate:

$$\begin{aligned} Outcome_{ijt} = &\sum_{k=-3,k\neq-1}^{k=+4} \beta_k \times D_k \times Lost\ Relationship_{jt} \\ &+ \beta_8 \times Firm\ controls_{ijt} + \beta_9 \times Bank\ controls_{jt} + \gamma_{ij} + \delta_t + \epsilon_{ijt} \end{aligned} \tag{1.1}$$

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

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

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

In this traditional dynamic TWFE regression, we would interpret $\beta_k$ as the treatment effect of a lost relationship $k$ years before or after the event year. However, 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 TWFE approach does not satisfy the no-sign reversal property, which means that $\beta_k$ could be positive, even though the ATE would be negative for all sample firms. Appendix 1.D shows that this is a relevant problem in our setting. Adding to this concern, Sun and Abraham (2021) show that, if treatment effects vary across firms and over time, $\beta_k$ may be biased for the average treatment effect from $k$=-3 until $k$=+4 (see also Baker, Larcker, and Wang 2022).

To avoid these issues, we apply the estimator of Chaisemartin and D'Haultfoeuille

(2022), which allows both for heterogeneous treatment effects across different firms and for dynamic effects around events. The estimator is a weighted average of difference-in-differences comparing the outcome evolution of switchers (firms that experienced a withdrawal at *t-k*) with the evolution of not-yet switchers (firms that have not experienced a treatment up to *t*) between *k*=-3 and *k*=4. We can then interpret our estimates for $\beta_k$ as the effect of having experienced a withdrawal for the first time *k* periods ago.

We expect the decline in correspondent bank relationships in a locality to have a negative effect on firm outcomes and therefore conjecture that $\beta_0$ to $\beta_4$ are negative. If firms can replace (some of) their export activity with increased local sales, then the coefficients $\beta_k > 0$ will be insignificant for firms' overall turnover and the number of employees. Like in any difference-in-differences design, the causal interpretation of our findings rests on the parallel trends assumption. Insignificant coefficients on $\beta_{-3}$ and $\beta_{-2}$, i.e. the absence of an effect in the pre-event years, indicate that this assumption is reasonable.[11]

To mitigate lingering concerns about omitted variable bias, even after our matching exercise on pre-treatment characteristics, we add a vector of time-varying $Firmcontrols_{ijt}$ and $Bank\ controls_{jt}$. At the firm level, we include log *Total assets* to control for firm size and *Total Factor Productivity*, the industry-adjusted residual of a two-factor Cobb-Douglas production function. The input factors are the log number of employees and log total assets to account for labor and capital, and the output is log total revenue.[12] The trade literature has identified firm size and productivity as key determinants of firm exports at the extensive margin (Melitz 2003; Bernard et al. 2007).

$Bankcontrols_{jt}$ comprise standard bank characteristics, calculated as branch-weighted averages by locality, to ensure that our results are not driven by the structure of the local banking environment. These variables are constructed using data on all banks with branches in a locality irrespective of whether we have information on the change in their correspondent bank relationships. *Local loan growth* is the percentage change in gross lending of the banks with branches in the locality. *Equity/Total assets* accounts for banks' capitalization. *Loans/Customer deposits* indicates the extent to which a bank's loans are funded by wholesale rather then deposit funding and *ROA* is the return on assets and measures banks' profitability.

Lastly, we account for linear country as well as non-parametric industry trends.

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

[12] Due to a lack of employment data for Turkey, we apply a one-factor Cobb-Douglas function with total assets as the only input for Turkish firms.

The Chaisemartin and D'Haultfoeuille (2022) estimator controls for linear country trends by including fixed effects for the country when residualizing the first-difference of the outcome. We account for non-parametric industry trends by using a weighted average of difference-in-differences comparing switchers and non-switchers from the same industry, respectively. When controlling for non-parametric trends, estimators are unbiased even if treated and control firms experience differential trends, provided all firms within the same industry experience parallel trends. For robustness, we also consistently present results based on Borusyak, Jaravel, and Spiess (2022)'s difference-in-differences estimator, which uses an imputation approach allowing for arbitrary heterogeneity and dynamics of causal effects.[13] This allows us to set a plausible range for our effect sizes.

## 1.5 Results

This section first investigates the impact of the sudden termination of correspondent banking services on firm-level exports and other real-economic outcomes (Section 1.5.1). Section 1.5.2 then subjects these baseline results to a battery of robustness tests. We estimate and discuss potential spillover effects in Section 1.5.3 and complete our analysis by studying the mediating effect of banks' state-ownership in Section 1.5.4.

### 1.5.1 Terminated correspondent banking relationships and firm-level outcomes

*Likelihood to export and total exports*

We start our empirical analysis by investigating the effect of the termination of correspondent bank relationships on firms' likelihood to export and on their export turnover. Figure 1.4 graphically shows the results from the dynamic difference-in-differences regressions for both these outcomes. The left-hand graph reports estimates and 95%-confidence intervals of the average effect of the decline in correspondent bank relationships on firms' probability to export (*Export dummy*). The reported coefficients (red dots) are from a regression following the Chaisemartin and D'Haultfoeuille (2022) approach, including *Firm controls* and *Bank controls* and controlling for linear country trends and non-parametric industry trends. The corresponding regression results are reported in Table 1.3, column (1).[14]

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

[14]The methodology by 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

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

The blue dashes in the left-hand graph of Figure 1.4 report estimates using Borusyak, Jaravel, and Spiess (2022)'s imputation approach. The estimator yields very similar results. Lastly, we note that the insignificant and close to zero pre-event effects of the Chaisemartin and D'Haultfoeuille (2022) estimator suggest that firms in both types of localities would have developed along parallel paths in case no correspondent banking relationships had been discontinued. For the estimator introduced by Borusyak, Jaravel, and Spiess (2022), these effects are significantly positive, albeit small. Note that, unlike the Chaisemartin and D'Haultfoeuille (2022) approach which does not provide estimates for a potential pre-trend in *t=-1*, Borusyak, Jaravel, and Spiess (2022) estimates coefficients for the pre-event year.

The right-hand graph in Figure 1.4 depicts the results from dynamic difference-in-differences regressions for firms' export turnover. The red dots again indicate coefficients from a regression following the Chaisemartin and D'Haultfoeuille (2022) approach, including *Firm controls* and *Bank controls* and controlling for linear country trends and non-parametric industry trends. The respective regression results are reported in Table 1.3, column (2).

We find that once one or several local correspondent banking relationships get terminated, local firms' total amount of exports starts to decline. The point estimates suggest that the full effects of the termination are not felt immediately. The impact instead materializes with some delay (and only becomes statistically significant at $t$=2) but becomes more pronounced over time. Two years after the event ($t$=2), the export turnover of firms in localities that lost correspondent banking relationships is 76 per cent lower than of similar control firms in unaffected localities. This stark average decline reflects both firms that stopped exporting altogether and firms that shrank their exports on the intensive margin.

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industry×year fixed effects and country×year fixed effects. The results are reported in Appendix 1.F and yield similar conclusions.

**Figure 1.4:** Terminated correspondent bank relationships and firm exports

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

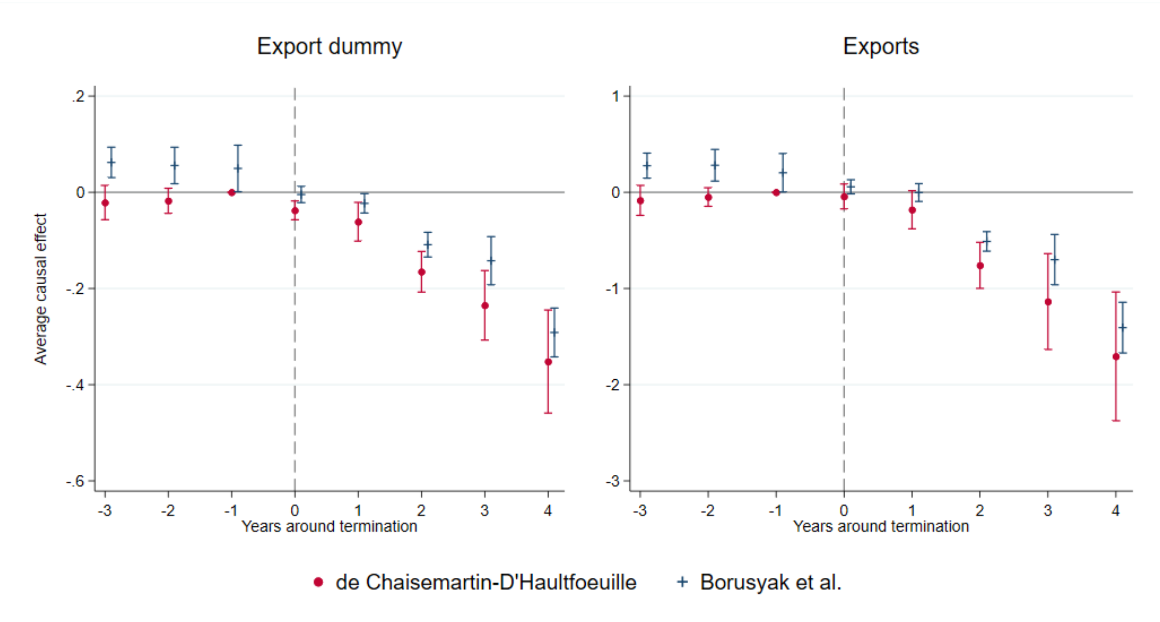


**Table 1.3:** Terminated correspondent bank relationships and firm-level outcomes

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

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

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

We again report estimates based on Borusyak, Jaravel, and Spiess (2022) as blue dashes. They are broadly in line with the patterns obtained using the Chaisemartin and D'Haultfoeuille (2022) estimator, but report some positive pre-trends. Overall, Figure 1.4 shows how a sudden termination of correspondent banking relationships negatively affects firms' export performance on the extensive and intensive margins.

*Domestic sales and total turnover*

Firms whose local bank has lost access to global correspondent banks, might turn to domestic markets to make up for their reduced ability to sell abroad. If they do so successfully, their total turnover and employment may be affected less negatively or perhaps not at all. We therefore also analyze how the termination of correspondent relationships affects firms' domestic and total turnover. This provides for a more complete picture of the firm-level impact of the fragmentation of the global correspondent banking network.

The red dots in Figure 1.5 depict the Chaisemartin and D'Haultfoeuille (2022) dynamic estimates for firms' domestic turnover (left) and total turnover (right). We again include firm and bank covariates and control for linear country trends and non-parametric industry trends.[15] The graph on the left of Figure 1.5 shows that immediately after the shock to local correspondent banking relationships, there is no strong increase in local firms' domestic sales—at least not in the first two years. In the medium-term, however, firms appear more successful in expanding their domestic turnover. While the Chaisemartin and D'Haultfoeuille (2022) estimates are noisy, the Borusyak, Jaravel, and Spiess (2022) estimates (again reported as blue dashes) show a similar but more precisely estimated pattern.[16] They confirm that, over time, firms successfully respond to increased export barriers by expanding their sales domestically.

Can affected firms offset their reduced exports one-for-one by higher local sales? The right-side panel of Figure 1.5 shows that this is not the case. Both estimators show clearly that total turnover (that is, foreign and domestic sales combined) declines more in localities where at least one bank branch loses access to correspondent banks, relative to firms in places where banks managed to maintain access to the global correspondent network.

*Employment*

In line with firms' reduced overall turnover, Figure 1.6 shows a negative average treatment effect on the number of firm employees (see also column (5) of Table 1.3). Firms that experienced the termination of one or more correspondent banking relationships

[15] We report the underlying regression results in columns (3) and (4) of Table 1.3.

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

**Figure 1.5:** Terminated correspondent bank relationships and firm turnover

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

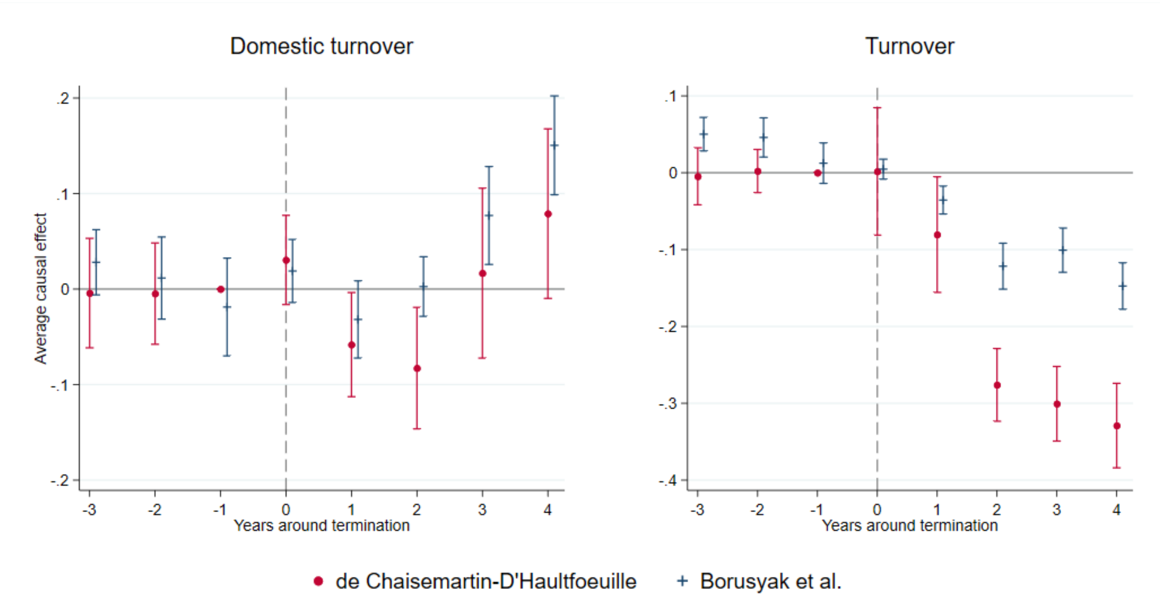
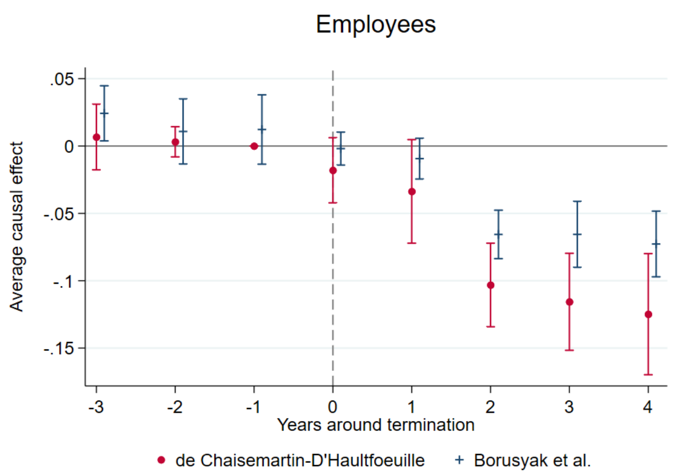


**Figure 1.6:** Terminated correspondent bank relationships and firm employment

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




in their locality shrink their work force by 3.4 per cent within a year, compared to similar unaffected firms. After four years ($t$=4), this differences has widened to 12.5 per cent.

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

### 1.5.2 Robustness

*An alternative strategy to link firms to banks*

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

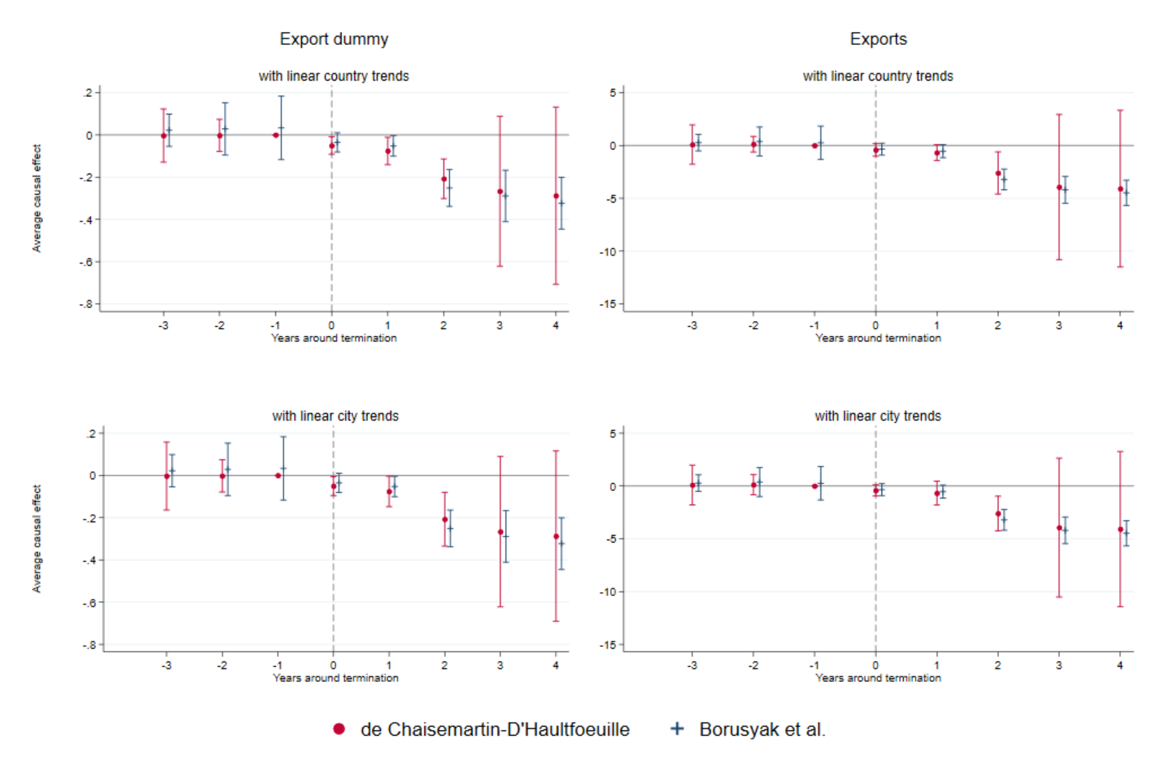
Another way to connect firms with banks is to use Orbis data on each firm's main bank. The advantage is that we now distinguish *within* localities between firms affected by the termination of correspondent relationships and those unaffected. Thus, we can account for locality-level developments that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

To investigate these issues, we re-run our main regressions using this Orbis-matched sample. Results for our export variables are presented in Figure 1.7 and in Table 1.E1 in Appendix 1.E. The left-hand graphs in Figure 1.7 depict the results from the differences-in-differences regressions for firms' export probability, while the right-hand graphs depict the results for firms' export turnover. The coefficients reported as red dots are, again, from a regression following the Chaisemartin and D'Haultfoeuille (2022) approach, including firm and bank covariates while controlling for linear country trends (upper graphs) or linear city trends to account for time-varying city characteristics (lower graphs), respectively, and non-parametric industry trends. Estimates based on Borusyak, Jaravel, and Spiess (2022) are reported as blue dashes and yield very similar point estimates. As before, these estimates tend to be more precise, especially at the start and the end of the sample window.

Overall, the results using firm-bank linkages are qualitatively the same as those with the locality-matched sample in our main analysis (Figure 1.4). A decrease in correspondent banking services negatively affects the extensive and the intensive margin of exports: Exporting firms are less likely to export and have a lower export turnover if their main bank has lost at least one correspondent banking relationship. Yet, as expected, since these are treatment effects for the specific group of firms whose own bank lost one or several correspondent banking relationships, the magnitude of the effects here is somewhat larger. For example, while the general local equilibrium effect of terminated banking relationships on firms' propensity to export is minus 3.8 percentage points (Figure 1.4), this effect is 5.1 percentage points when we directly link firms to banks. Moreover, given the smaller sample size of the Orbis-matched sample, confidence intervals of the Chaisemartin and D'Haultfoeuille (2022) estimator are

**Figure 1.7:** Terminated correspondent bank relationships and firm exports: bank-firm matching

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

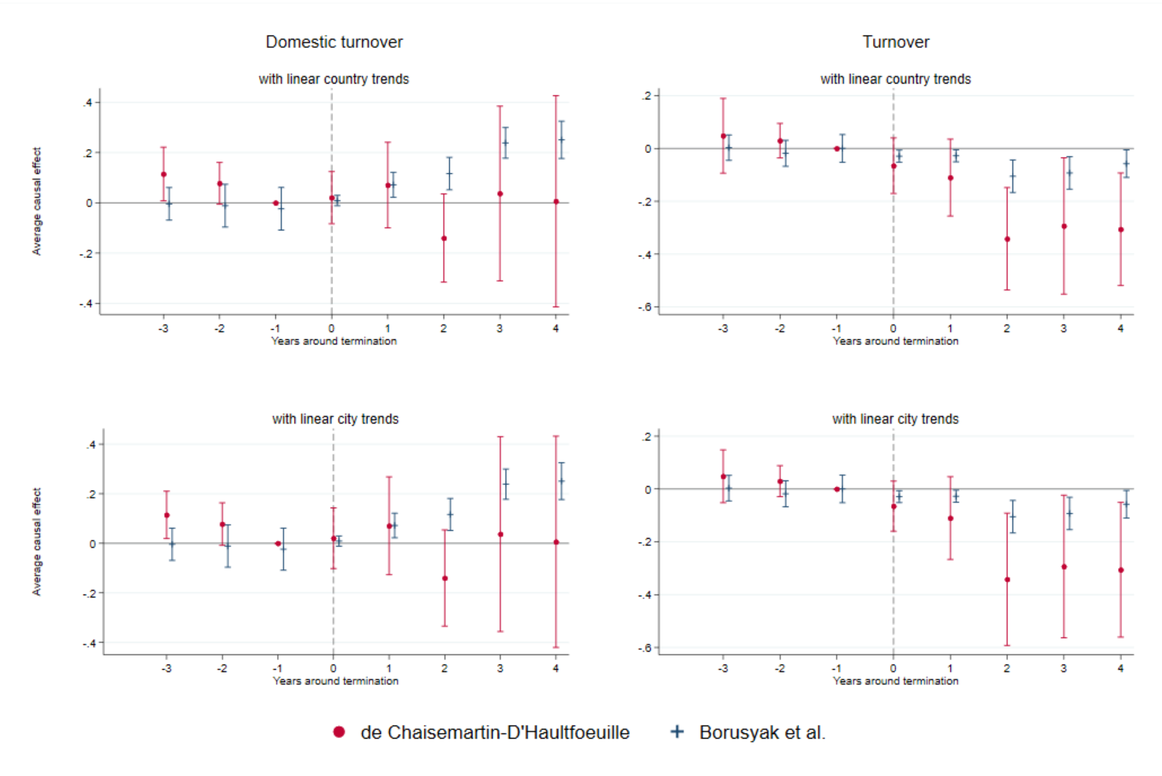



wider than in our main specification, especially in $t$ = *3* and $t$ = *4*. As causal effects are estimated based on first differences between treated and not-yet treated control firms at any $t$ by Chaisemartin and D'Haultfoeuille (2022), the fewer available treated-control pairs of the same industry and country after $t$ = *2* result in relatively large standard errors. For instance, the causal effects estimated for the *Export dummy* at $t$ = *3* and $t$ = *4* are based on 1,550 and 1,422 switchers and their controls, respectively, compared to 5,441 switchers for the estimate at $t$ = *0*.

Figure 1.8 and Figure 1.9 then show the results for domestic and total turnover as well as for the number of employees using the Orbis-matched sample. Again, the results using firm-bank relationships largely confirm our results from the locality-matched sample. Firms whose main bank loses at least one correspondent bank relationship

**Figure 1.8:** Terminated correspondent bank relationships and firm turnover: bank-firm matching

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




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

*A continuous treatment measure*

So far, we have used a binary treatment indicator: a dummy that equals one if at least one bank branch in a locality lost a correspondent relationship in year $t$ or earlier (*Lost relationship*). We now create a continuous treatment variable. *Cut relationships*

**Figure 1.9:** Terminated correspondent bank relationships and firm employment: bank-firm matching

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

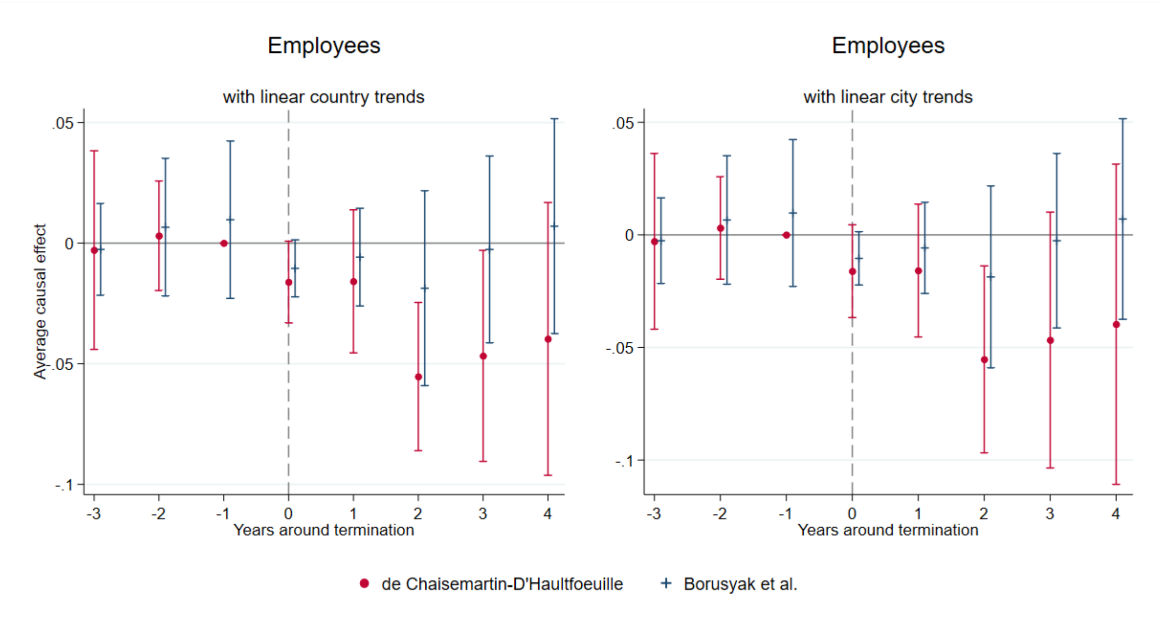
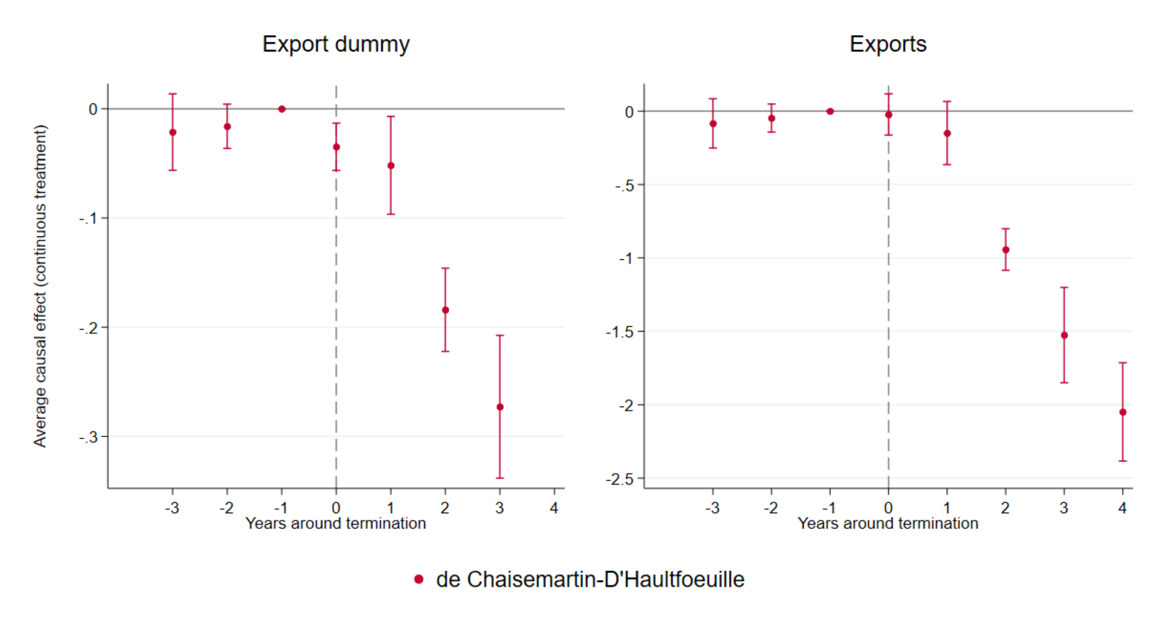


**Figure 1.10:** Terminated correspondent bank relationships and firm exports: continuous treatment

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




measures the number of terminated correspondent bank relationships in a locality in year $t$ or earlier, and normalizes this by the number of bank branches in the locality. This variable therefore gauges treatment intensity across localities.

Unlike the Borusyak, Jaravel, and Spiess (2022) estimator, the Chaisemartin and D'Haultfoeuille (2022) estimator can be used with continuous treatment measures. The challenge, however, is to have enough proper control firms in the sample. For instance, if a firm jumps from treatment=0.1 to treatment=0.2, the estimator needs control firms that stay at 0.1 during the years before and after the treatment. Naturally, this condition needs to be fulfilled for all possible treatment values, which is not given in our setting. As a solution, Chaisemartin and D'Haultfoeuille (2022) propose to consider small treatment changes as being essentially stable. We follow this approach and use any firm as a control whose change in the treatment level is 0.1 or less. We present results using this continuous treatment in Figure 1.10 to Figure 1.12 and in Table 1.E2 in Appendix 1.E.

Figure 1.10 confirms our earlier findings on firms' exports: a decrease in correspon-

**Figure 1.11:** Terminated correspondent bank relationships and firm turnover: continuous treatment

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

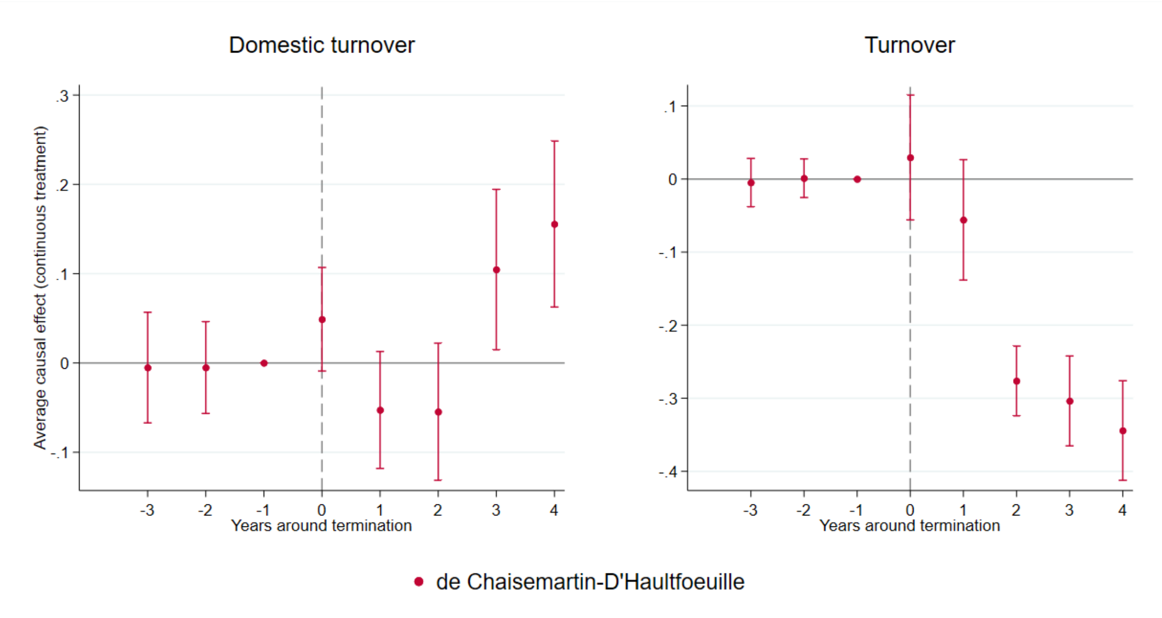
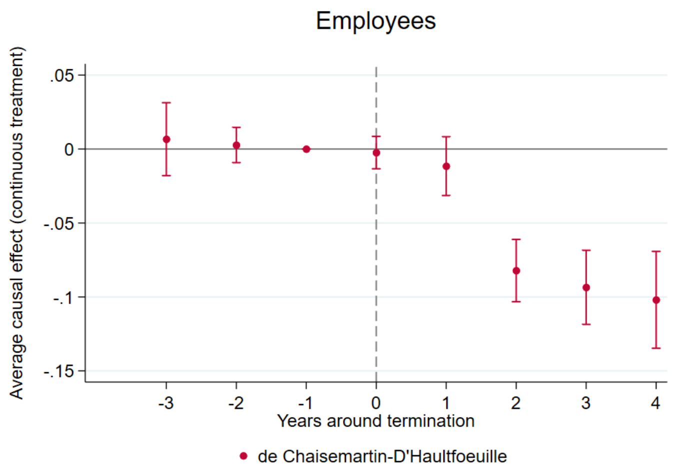


**Figure 1.12:** Terminated correspondent bank relationships and firm employment: continuous treatment

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



dent banking negatively affects the extensive and intensive export margins. Exporting firms are less likely to export, and have a lower export turnover, when more correspondent banking relationships are terminated in their locality compared to firms in localities where no or fewer such relationships disappear. The effects are economically meaningful, too: a one standard deviation increase in terminated correspondent relationships leads, for instance, to an immediate decline in the export probability of 2.1 percent and of 23 percent within four years. Figure 1.11 and Figure 1.12 confirm that firms in localities that experience a decline in correspondent bank relationships cannot fully compensate for this loss of access to foreign markets by expanding domestic sales and that affected firms therefore lay off part of their employees.

### 1.5.3 Spillover effects

The termination of correspondent relationships may generate spillovers to initially unaffected (control) firms within the same industry but located elsewhere. The competitive outlook of such firms may improve relative to treated firms that have lost local access to correspondent banking services. Moreover, Berg, Reisinger, and Streitz (2021) highlight that the effect of a shock on firm-level real outcomes depends not only on a firm's own treatment status, but also on the fraction of treated firms in the same industry. In our setting, the negative impact of broken correspondent relationships on treated firms may be less severe if more firms within the same industry are treated. This is because with more treated firms, the respective trading partners can less easily switch to other suppliers in the same industry but in unaffected localities.

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

$$\begin{aligned} Outcome_{ijst} = {} & \beta_0 + \beta_1 d_{ijst} + \beta_T \bar{d}_{st} d_{ijst} + \beta_C \bar{d}_{st}(1 - d_{ijst} \\ & + \beta_2 \times Firm\ controls_{ijt} + \beta_3 \times Bank\ controls_{jt} + \gamma_{ij} + \delta_t + \epsilon_{ijst} \end{aligned} \tag{1.2}$$

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

As dependent variables ($Outcome_{ijst}$) we use *Export dummy* and *Exports* for the spillover analysis. $d_{ijst}$ is our treatment indicator which switches to one when at least one correspondent bank relationship is lost in the locality of firm $i$). $\bar{d}_{st}$ denotes the (time-varying) fraction of treated firms in an industry (without firm $i$. $Firmcontrols_{ijt}$ include *Total assets* and *Total Factor Productivity*; and $Bank controls_{jt}$ comprise *Local loan growth*, *Equity/Total assets*, *Loans/Customer deposits* and *ROA* as defined in Section 1.4.3. $\gamma_{ij}$ are firm fixed effects and $\delta_t$ are year fixed effects. This heterogeneous spillover model provides us with three coefficients of interest: the direct treatment effect ($\beta_1$); the spillover effect to treated firms ($\beta_T$); and the spillover effect to control firms ($\beta_C$).

Following Berg, Reisinger, and Streitz (2021), we plot the outcome variables *Export dummy* and *Exports* as a function of treatment intensity—the fraction of treated firms in an industry—for treatment units, control units, and group averages. The underlying regressions are estimated using 'static' OLS, in contrast to the dynamic TWFE estimates that form the basis for the event-study plots in our main analysis. By way of comparison, Table 1.4 provides the related static OLS results without accounting for spillovers. The table also reports static treatment effects using the Chaisemartin and D'Haultfoeuille (2022) and Borusyak, Jaravel, and Spiess (2022) estimators. All three approaches yield very similar results and confirm that cutting correspondent bank relationships reduces firms' exports at the intensive and extensive margins.

**Table 1.4:** Terminated correspondent bank relationships and firm-level outcomes: Static effects

This table shows static TWFE estimates (top), static Chaisemartin and D'Haultfoeuille (2022) estimates (middle) and static Borusyak, Jaravel, and Spiess (2022) estimates (bottom) for firms' *Export dummy* (columns 1-3) and *Exports* (columns 4-6) around the termination of one or more correspondent bank relationships in a firm's locality and compared to unaffected control firms. Treated firms are located in a locality in which at least one bank branch lost a correspondent bank relationship. Control firms are located in a locality that did not lose a correspondent bank relationship up to the event year. We match each treated firm to one control firm in the same industry and country that has similar *Exports*, *Total assets* and *Total Factor Productivity* in the pre-event year. All specifications include firm and year FE or apply the difference-in-difference approach of Chaisemartin and D'Haultfoeuille (2022) or Borusyak, Jaravel, and Spiess (2022). All specifications include time-varying firm and bank controls (except column 1 and 4). Firm controls include *Total assets* and *Total Factor Productivity*; bank controls include *Local loan growth*, *Equity/Total assets*, *Loans/Customer deposits*, and *ROA*. Columns 3 and 6 also include country FE or linear country trends for dCdH (2022) respectively; industry x year FE or non-parametric industry trends for dCdH (2022), respectively. Standard errors are clustered at the locality level and shown in parentheses. Note that the number of firm-years used to estimate the treatment effect by Chaisemartin and D'Haultfoeuille (2022) is smaller than the number of firm-years reported for the OLS estimator and for Borusyak, Jaravel, and Spiess (2022). As the Chaisemartin and D'Haultfoeuille (2022) estimator is based on valid first-differences between treated and control firms (see Section 1.4.3) it only includes the subset of firms that are treated with a valid control or that are valid controls of a treated firm.

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

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

The left graph of Figure 1.13 shows the results of the spillover analysis for the probability to export (*Export dummy*). The direct treatment effect indicates the impact of a decline in correspondent bank relationships if no other firm in the same industry is treated. This effect, represented by the difference between treatment and control firms at a treatment fraction of zero, is -11.2 percentage points. The increasing solid line shows that treated firms are less negatively affected in their probability to export the larger the fraction of other treated firms in the industry. One reason may be that with more treated firms in an industry, respective trading partners have fewer possibilities to buy their products more cheaply from control firms and fewer treated firms stop exporting as a result.

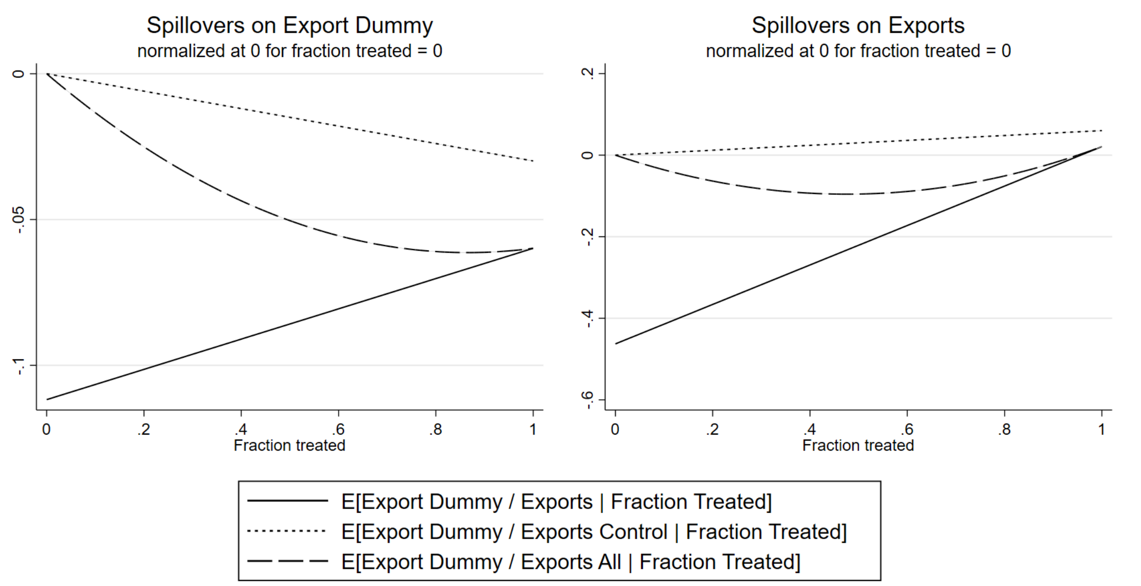
The decline in the dotted line shows that control firms, i.e. exporting firms in localities that do not experience a decline in correspondent bank relationships, suffer from some spillover effects. Control firms' probability to export decreases with the fraction of treated firms in the same industry.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

As the positive within-treated-firms spillovers are larger than the spillovers to control firms, the difference between treatment and control firms diminishes with more firms in the same industry being treated. This means that not accounting for spillover effects leads to underestimating the direct treatment effect. The dashed line in the left graph of Figure 1.13 presents the industry-level average probability to export depending on the fraction of treated firms (normalized at zero). The slope is declining up to a fraction of treated firms of 0.8 and is relatively flat afterwards, a result of the weakened negative effect treated firms experience when they represent a larger fraction of the industry.

The right graph of Figure 1.13 shows the results of the spillover analysis for export turnover (*Exports*). The direct treatment effect when the fraction of treated firms is zero is -46.3 per cent. The slightly increasing dotted line shows that there are some positive spillovers to control firms when the fraction of treated firms becomes larger. For the treated firms, the rising solid line illustrates that they are less negatively affected by the decline in correspondent bank relationships when the fraction of treated firms in the same industry increases, which is in line with our findings on spillover effects in the probability to export. The difference between treatment and control firms diminishes with more firms in the same industry being treated as a result of the larger positive within-treated-firms spillovers. This again means that not accounting

**Figure 1.13:** Industry spillovers

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



for spillover effects leads to underestimating the direct treatment effect.

### 1.5.4 The mediating effect of state-ownership of local banks

The negative impact of the drop in correspondent banking relationships on firms' extensive and intensive export margins begs the question whether there are factors that may help alleviate this negative impact. In this section, we assess whether state-ownership of local banks is such a mediating factor. State banks may be better able to buffer the negative effects of a decline in correspondent bank relationships, for example because they help firms to access alternative trade insurance products such as government-guaranteed schemes.

We split our sample and analyze localities with an above-median number of branches owned by state banks and localities with a below-median number of such branches. We then run the same Chaisemartin and D'Haultfoeuille (2022) estimations as before on both these sub-samples, while again providing results based on the Borusyak, Jaravel, and Spiess (2022) estimator by way of comparison. Figure 1.14 summarizes the results.[17]

Figure 1.14 shows that the negative effect of a decline in correspondent banking relationships on affected firms' probability to export and on their export turnover is concentrated among firms in localities with comparably few state banks (charts at the bottom). In localities with an above-median number of state banks (charts at the top), in contrast, there is no significant treatment effect on the probability to export and only a small negative treatment effect on the export turnover based on the Chaisemartin and D'Haultfoeuille (2022) estimator (red dots). Following the approach of Borusyak, Jaravel, and Spiess (2022) (blue dashes), firms in a locality with an above-median share of state-owned banks experience a slight increase in their probability to export and their export turnover, but the effect is marginal compared to the negative treatment effect on firms in localities with few state banks.

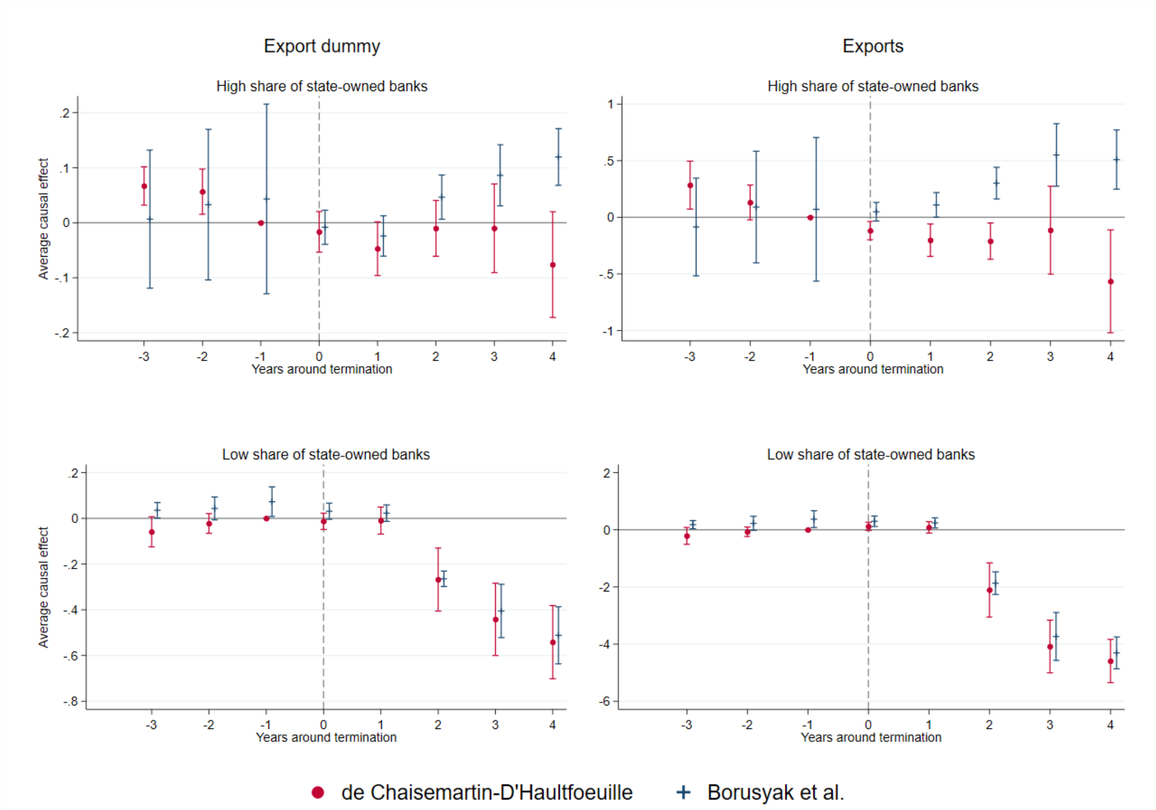
## 1.6 Conclusions

Our analysis highlights how financial fragmentation in response to the tighter enforcement of financial crime legislation, has had immediate and strong negative impacts on firms in emerging markets. At least in the short term, firms affected by these global payment disruptions have only been partially successful in replacing lost export opportunities with increased domestic sales. As a result, and with some delay, they have

[17] The regression results for firms' probability to export (*Export dummy* are reported in column (1) of Table 1.F1 in Appendix 1.F for cities with an above-median number of state bank branches and in column (3) for cities with a below-median number of state bank branches. Similarly, the regression results for firms' export turnover (*Exports* are reported in column (2) and (4) of Table 1.F1.

**Figure 1.14:** Terminated correspondent bank relationships, state banks, and firm exports

This figure shows firms' *Export dummy* and *Exports* around the termination of one or more correspondent bank relationships for the sub-sample of firms with an above-median number of state bank branches in their locality (upper graphs) and the sub-sample of firms with a below-median number of state bank branches in their locality (lower graphs). Treated (control) firms are located in a locality in which at least one (no) bank branch lost a correspondent bank relationship up to the event year. Reported coefficients are based on Chaisemartin and D'Haultfoeuille (2022) and Borusyak, Jaravel, and Spiess (2022). The reported coefficients are from a regression including firm controls (*Total assets* and *Total Factor Productivity*), banks controls (*Local loan growth*, *Equity/Total assets*, *Loans/Customer deposits*, *ROA*), and controlling for linear country trends and non-parametric industry trends. 95%-confidence intervals are based on standard errors clustered by locality.



had to lay off part of their work force. While we only investigate the short to medium-term implications of broken correspondent banking relationships, the effects on trade may be long-lived. Even if and when correspondent banks decide to (re-)enter certain countries, international trade may only gradually expand again as local knowledge and relationships take time to be re-established.

Our analysis also provides suggestive evidence that state-owned financial institutions can offset some of the negative trade effects associated with terminated correspondent banking relationships. Government-backed schemes, such as trade-insurance products for exporters, can alleviate the negative impacts of reduced private sector involvement in the management of trade-related payment risks. In the longer term, new private technologies may facilitate safe and speedy cross-border payments associated with trade transactions. Currently, however, FinTechs only play a limited role in the market for trade-related cross-border payments, again reflecting the high compliance costs of financial crime regulation. This stands in stark contrast to their increasingly prominent role in facilitating international retail payments.

Until reliable, cost-effective, and trustworthy FinTech alternatives enter the market, local respondent banks in emerging markets may need to bring their compliance procedures up to the required international standards and ensure that their staff obtain professional certification, such as in customer due diligence, financial crime prevention, and money laundering risks in correspondent banking.

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## Chapter I

# Appendix

## 1.A Variable definitions and sources

**Table 1.A1:** Variable definitions and sources

| **Variable** | **Definition** | **Source** |
|---|---|---|
| | **Panel A: Firm variables** | |
| *Export dummy* | Dummy variable equal to one if firm has export revenues in a given year | Orbis |
| *Exports* | Revenues from a firm's export activities in log 1,000 euros | Orbis |
| *Turnover* | Total operating revenues in log 1,000 euros | |
| *Domestic turnover* | Domestic sales in log 1,000 euros | Orbis |
| *Employees* | Log number of employees | Orbis |
| *Total assets* | Total assets in log 1,000 euros | Orbis |
| *Total Factor Productivity* | Industry-adjusted residual of a two-factor Cobb-Douglas production function. The input factors of this function are the log number of employees and log total assets to account for labor and capital, and the output is log turnover | Own calculation based on Orbis |
| *Firm age* | Firm age in years | Orbis |
| *Industry* | NACE Rev. 2 classification | Orbis |
| *Locality* | Village, town, or city of firm headquarter | Orbis |
| | **Panel B: Bank variables (branch-weighted averaged by locality)** | |
| *Lost relationship* | Dummy that equals one if at least one bank branch in locality has lost a correspondent banking relationship up to year t. | BEPS III and EBRD TFP survey |
| *Cut relationships* | Number of terminated correspondent bank relationships in a locality up to year t (on bank branch level) divided by total number of bank branches in a locality | BEPS III and EBRD TFP survey |
| *Branch network* | Locations (cities) of all branches of a bank | BEPS III |
| *Loan growth* | Percentage change in gross lending | Orbis BankFocus |
| *Equity/Total Assets* | Bank equity divided by total bank assets | Orbis BankFocus |
| *Loans/Customer Deposits* | Net bank loans divided by a bank's customer deposits and short-term funding | Orbis BankFocus |
| *ROA* | Return on assets calculated as net income divided by total assets | Orbis BankFocus |
| *Total assets* | Total bank assets in million euros | Orbis BankFocus |
| | **Panel C: Locality variables** | |
| *Nightlight* | Global VIIRS Nighttime Lights Derived from Monthly Averages, following Elvidge et al. 2021 | NASA/NOAA VIIRS |

## 1.B Survey questions

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

**EBRD Banking Environment and Performance Survey (BEPS) III**
This section relates to correspondent banks.

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

**Survey with partner banks of the EBRD Trade Facilitation Programme (TFP) in 2019**

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

  - Currency Clearing
  - Trade Finance

- **Question 6**: Please score the availability of correspondent banking services in different currencies to your bank in 2013, 2015, 2017, and the year 2019. [Respondents select between "Not available", "Difficult to access", "Easy to access", "Not relevant"]
  - US-Dollar
  - Euro
  - Ruble
- **Question 10**: What do you consider the most likely reasons that foreign correspondent banks have decided to terminate or restrict their correspondent banking relationship with your bank/with other banks?
  - The correspondent banking relationship does not generate sufficient business to justify the cost of additional customer due diligence.
  - Foreign correspondent banks have reacted to the stricter enforcement of AML/CFT Anti-Money Laundering/Combating the Financing of Terrorism regulations.
  - Foreign correspondent banks have reacted to regulations unrelated to AML/CFT Anti-Money Laundering/Combating the Financing of Terrorism.
  - Foreign correspondent banks have reacted to changed macroeconomic conditions.
  - Foreign correspondent banks have terminated relationships with local banks because correspondent banks have changed their business strategy or have gone through structural changes (including mergers and industry consolidation).
  - Local respondent banks have less demand for correspondent banking services as compared to previous years.
- **Question 11**: Out of all relevant causes for terminating your/others correspondent banking relationship, which do you consider most important?
  - The correspondent banking relationship does not generate sufficient business to justify the cost of additional customer due diligence.
  - Foreign correspondent banks have reacted to the stricter enforcement of AML/CFT regulations.

- Foreign correspondent banks have reacted to regulations unrelated to AML/CFT.
- Foreign correspondent banks have reacted to changed macroeconomic conditions.
- Foreign correspondent banks have terminated relationships with local banks because correspondent banks have changed their business strategy or have gone through structural changes (including mergers and industry consolidation).
- Local respondent banks have less demand for correspondent banking services as compared to previous years.

## 1.C Orthogonality tests

**Table 1.C1:** Treatment status explained by locality, firm, and bank variables

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

| | Correspondent bank withdrawal | | | |
|---|---|---|---|---|
| | Binary | | Continuous | |
| | (1) | (2) | (3) | (4) |
| **Locality characteristics** | | | | |
| Nightlight (t-1) | -0.545 | 0.446 | -1.007 | 1.425 |
| | (0.332) | (1.278) | (0.539) | (2.457) |
| **Firm characteristics (averaged at locality level)** | | | | |
| Number of firms (t-1) | 0.000 | 0.004 | -0.001** | 0.005 |
| | (0.000) | (0.004) | (0.000) | (0.004) |
| Total assets (t-1) | 0.545 | 0.268* | 0.534 | 0.291 |
| | (0.446) | (0.106) | (0.566) | (0.224) |
| Productivity (t-1) | 1.647 | -1.269 | 4.225 | 3.928 |
| | (2.020) | (1.959) | (3.432) | (3.029) |
| Turnover (t-1) | 0.314 | 0.710 | -0.202 | -1.191 |
| | (1.046) | (0.313) | (1.760) | (1.078) |
| Employees (t-1) | -0.005 | 0.001 | -0.009 | -0.001 |
| | (0.004) | (0.001) | (0.007) | (0.003) |
| HHI | -0.189* | 0.030 | -0.113 | 0.078 |
| | (0.073) | (0.026) | (0.090) | (0.047) |
| **Bank characteristics (averaged at locality level)** | | | | |
| Total assets (t-1) | 0.046 | 0.044 | 0.089 | 0.083 |
| | (0.070) | (0.033) | (0.116) | (0.051) |
| Equity/Total assets (t-1) | 0.227 | 1.313 | -4.813 | 3.091 |
| | (1.976) | (4.181) | (4.083) | (9.383) |
| Loans/Deposits (t-1) | -0.142 | 0.515* | 0.610 | 1.811** |
| | (0.771) | (0.216) | (1.529) | (0.468) |
| Gross loans (t-1) | -0.086 | -0.040 | -0.164 | -0.081 |
| | (0.108) | (0.030) | (0.185) | (0.046) |
| HHI | -0.344** | -3.489** | -0.130 | -4.519** |
| | (0.082) | (1.001) | (0.078) | (1.305) |
| F | 1.78 | 0.25 | 2.72 | 1.09 |
| Prob > F | 0.32 | 0.85 | 0.22 | 0.47 |
| Observations | 6,682 | 6,670 | 6,680 | 6,668 |
| $R^2$ | 0.28 | 0.79 | 0.25 | 0.82 |
| Locality FE | No | Yes | No | Yes |

Standard errors in parentheses

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

## 1.D Heterogeneity of treatment effects

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

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

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

Table 1.D1 below reports the estimated $\hat{\underline{\sigma}}$ and $\hat{\underline{\underline{\sigma}}}$ based on our baseline regressions that regress *Export dummy* and *Exports*, respectively, on our treatment indicator, control variables, and firm and year fixed effects. In the model with *Export dummy* as our dependent variable, $\hat{\underline{\sigma}} = 0.06$. This suggests that the ATT and the estimated beta may be of opposite sign if the standard deviation of the treatment effect across the treated groups and time periods was 0.06 or higher.

To assess if this is a reasonable value for treatment effect heterogeneity in our setting, we follow the thought experiment introduced by Chaisemartin and D'Haultfoeuille (2022): If treatment effects of the treated groups and time periods were drawn from a normal distribution around a mean of 0 and with a standard deviation of $\hat{\underline{\sigma}} = 0.06$, 95% of the treatment effects would be distributed within the interval $[-0.11, 0.11]$. Compared to our OLS beta estimate for the corresponding linear model of -0.08 (see Table 1.4, column (2)), this range does not seem unreasonably wide. A standard deviation of 0.06, consequently, is not implausibly high for the treatment effect across treated groups and time periods. For our regressions with *Export dummy* as the dependent variable, heterogeneous treatment effects could thus be a problem and betas estimated from a linear regression could have the opposite sign as the ATT.

The value of 0.10 for $\underline{\underline{\sigma}}$ indicates that obtaining a beta estimate of a different sign than the treatment effect in all treated groups and time periods is less of a concern in our setting. If, in contrast to our negative estimate, all treatment effects were positive and distributed uniformly with a standard deviation of 0.10[1], they would lie in the interval $[0, 0.33]$. This interval seems relatively wide, compared to our OLS estimate of -0.08 (Table 1.4, column (2)).

Following the same argument for the models with *Exports* as our dependent variable, $\underline{\hat{\sigma}} = 0.25$ does not seem unreasonably high either. In the associated normal distribution $N(0, 0.25^2)$, 95% of treatment effects would be in the interval $[-0.48, 0.48]$, which seems reasonable compared to our OLS estimate of -0.31 (see Table 1.4, column (5)). Again, the risk that beta has a different sign than the treatment effect in all the treated groups and time periods seems lower, but not unrealistic. In the associated uniform distribution, the values would be in the range $[0, 0.79]$. This range is relatively wide but still plausible, compared to our estimate of -0.31.

In line with our conclusion that treatment heterogeneity might be a concern in our setting, Table 1.D1 reports that the sum of negative weights is high in both models. This indicates that treatment effects of several treated groups and periods enter negatively in the linear estimator. To alleviate the potential problems arising from these negative weights, we account for possible heterogeneous treatment effects by applying the estimator suggested by Chaisemartin and D'Haultfoeuille (2022) and the estimator introduced by Borusyak, Jaravel, and Spiess (2022) throughout the paper.

**Table 1.D1:** Heterogeneity of treatment effects

This table shows the sum of positive and negative weights as well as the values for $\underline{\hat{\sigma}}$ and $\underline{\underline{\hat{\sigma}}}$ of Corollary 1 in Chaisemartin and D'Haultfoeuille (2020). The numbers are based on two-way fixed effects OLS regressions of our two main dependent variables *Export dummy* and *Exports* on our treatment variable *Lost relationship*, including *Total assets* and *Total Factor Productivity* as firm controls and *Local loan growth*, *Equity/Total assets*, *Loans/Customer deposits*, and *ROA* as bank controls.

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

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

## 1.E Robustness checks

**Table 1.E1:** Terminated correspondent bank relationships and firm-level outcomes: Bank-firm matching

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

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

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

**Table 1.E2:** Terminated correspondent bank relationships and firm-level outcomes: Continuous treatment

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

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

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

**Table 1.E3:** Firm exports after the termination of a correspondent banking relationship: OLS with country×year and industry×year fixed effect

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

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

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

## 1.F Terminated correspondent bank relationships, state banks, and firm exports

**Table 1.F1:** Terminated correspondent bank relationships, state banks, and firm exports

This table shows Chaisemartin and D'Haultfoeuille (2022) difference-in-difference estimates on firms' *Export dummy* and *Exports* around the termination of one or more correspondent bank relationships for the sub-sample of firms with an above-average number of state bank branches in their locality (columns (1) and (2)) and the sub-sample of firms with a below-average number of state bank branches in their locality (columns (3) and (4)). Treated (control) firms are located in a locality in which at least one (no) bank branch lost a correspondent bank relationship up to the event year. We match each treated firm to one control firm of the same industry and country that also exports and has similar *Exports*, *Total assets* and *Total Factor Productivity* in the pre-event year. Firm controls include *Total assets* and *Total Factor Productivity*, banks controls include *Local loan growth*, *Equity/Total assets*, *Loans/Customer deposits*, and *ROA*. Standard errors are clustered on the locality level and are shown in parenthesis.

| | State-owned banks Exports | | Non-state owned banks Exports | |
|---|---|---|---|---|
| | Dummy (1) | Amount (2) | Dummy (3) | Amount (4) |
| Effect at t=0 | -0.017 | -0.118 | -0.013 | 0.119 |
| | (0.019) | (0.041) | (0.018) | (0.073) |
| Effect at t=1 | -0.047 | -0.202*** | -0.010 | 0.085 |
| | (0.025) | (0.073) | (0.030) | 0.103 |
| Effect at t=2 | -0.010 | -0.210** | -0.268*** | -2.106*** |
| | (0.026) | (0.082) | (0.070) | (0.484) |
| Effect at t=3 | -0.010 | -0.113 | -0.442*** | -4.084*** |
| | (0.041) | (0.198) | (0.081) | (0.470) |
| Effect at t=4 | -0.076 | -0.566** | -0.542*** | -4.593*** |
| | (0.049) | (0.232) | (0.082) | (0.386) |
| Placebo at t=-2 | 0.056 | 0.131 | -0.022 | -0.069 |
| | (0.021) | (0.078) | (0.022) | (0.085) |
| Placebo at t=-3 | 0.067 | 0.284 | -0.059 | -0.214 |
| | (0.018) | (0.108) | (0.034) | (0.151) |
| $\beta_{t=0}$ based on N firm-years | 22,129 | 19,204 | 40,350 | 39,506 |
| $\beta_{t=0}$ based on N switchers | 9,957 | 7,849 | 11,091 | 10,810 |
| Firm and bank controls | Yes | Yes | Yes | Yes |
| NP industry trends | Yes | Yes | Yes | Yes |
| Linear country trends | Yes | Yes | Yes | Yes |
| Pre-event mean | 1.00 | 5.35 | 1.00 | 4.02 |

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

# Chapter II

# Guns and Kidneys: How Transplant Tourism Finances Global Conflict

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## Abstract

This paper investigates the impact of organ trafficking on local conflict using georeferenced data on conflict events and hand-collected data on local transplant infrastructure in five countries known for illegal transplanting. I exploit exogenous variation in kidney demand measured by the number of US waiting list patients, their payment capacity, and their physical condition. Higher kidney demand increases conflict in localities with a transplanting center. Specifically, a one-standard deviation increase in the US waiting list for kidneys leads to a 17% increase in the probability of conflict and a 0.9% increase in the number of conflict events compared to localities without transplant infrastructure. This effect is stronger for waiting list patients with income and absent for waiting list patients on dialysis. Consistent with the hypothesis that armed groups use organ trafficking to finance violent attacks, I find that non-state armed groups with transplanting capacities in their home region perform more attacks when kidney demand is higher. These attacks happen both in their home region and in other regions, spreading violence over space. My results further show that higher kidney demand is associated with an increase in suspicious payments from and to countries known for illegal organ trafficking. This corroborates the hypothesis that non-state armed groups finance their attacks by organ trade.



**Keywords:** conflict; fighting; medical tourism; organ; terrorist financing

**JEL Codes:** C23; D74; I10; K13

## 2.1 Introduction

"Transplant tourists" travel from high-income countries to lower-income destinations to illegally obtain an organ for financial compensation (Flaherty et al. 2021).[1] The vast margins in the black market for organs make transplant tourism a lucrative business: Kidney recipients report to pay between US$ 100,000 and US$ 200,000 while donors report to receive between US$ 500 and US$ 10,000 at most (Council of Europe 2019). International security agencies therefore worry that non-state armed groups could participate in organ trade and use its proceeds to finance violent attacks (see, e.g., the House Hearing on Counterterrorism and Intelligence in 2016).

However, due to the hidden nature of transplant tourism and the ensuing absence of data, we lack systematic evidence on the relationship between illegal transplanting and non-state violent activity (ECOSOC 2006; Organization for Security and Cooperation in Europe 2013). This paper is the first to document a causal relationship between global organ demand, armed groups' involvement in transplant tourism, and non-state violent attacks. To overcome the dearth of data, I proxy a group's potential involvement in transplant tourism by the local existence of an authorized transplant facility. Almost all reported cases of illegal transplanting happened alongside legal transplants, that is, in transplant centers or hospitals which perform transplants as their daily business and by doctors officially employed by these centers (Organization for Security and Cooperation in Europe 2013). To get involved in the business of transplant tourism, non-state armed groups therefore need to collaborate with existing transplant facilities. This collaboration is most likely in groups' home region, where they are well-connected to the local population, professionals, and administrative bodies (Krause and Milliken 2009), and where they also commit a large part of their attacks.[2]

[1] Delmonico (2009) lists few cases under which transplant tourism is legal after the Declaration of Istanbul. For living donation, this is the case (i) for recipients with a dual citizenship who wish to undergo transplantation from a family member in a country of citizenship that is not their residence, and (ii) for genetically related donors and recipients who wish to undergo transplantation in a country not of their residence. Deceased donation abroad can be legal under official organ sharing programs. As the vast majority of reported cases on transplant tourism do not fulfill these conditions, I focus on the illegal cases of transplant tourism.

[2] Based on data from Raleigh et al. (2010) used in this analysis, about 30% of conflict incidences of a group happen in their narrowly defined home region (cells of about 55km $\times$ 55 km) and adjacent regions.

Following Berman et al. (2017), I use georeferenced data on conflict events to compare the effect of increased kidney demand on local conflict in localities with a transplant center to the effect in localities without transplant infrastructure. I run my analyses on cells of 0.5° latitude × 0.5° longitude (about 55km × 55km at the equator), covering five countries notorious for transplant tourism and monthly observations between 2010 and 2021.[3] To rule out reverse causality concerns, i.e., the demand for transplants and the availability of organs caused by violent conflict, I proxy exogenous variation in kidney demand with the demand for kidneys *outside* of my sample countries, namely by the number of US waiting list patients, their payment capacity, and their physical condition. I establish causality by including cell fixed effects to account for locality-specific features and country-state×month fixed effects to account for time-varying developments within states in the countries of my sample. Hence, I estimate the within-transplant cell panel variation in non-state violence caused by exogenous changes in kidney demand, controlling for state-level conflict trajectories.

I find a positive and significant impact of higher kidney demand on conflict in localities in which transplanting is possible. More specifically, in 0.5° latitude × 0.5° longitude cells with a transplant center, a one-standard deviation increase in the US waiting list for kidneys is associated with a 17% increase in the probability of conflict and a 0.9% increase in the number of conflict events compared to localities without a transplant center. In line with my assumption that transplant tourists need to be rich enough to afford a kidney, this effect is more than double as high for an increase in the number of waiting list patients with labor income, where cells with a transplant center face a 45% increase in conflict probability and a 1.8% increase in the number of conflict events. In line with my assumption that transplant tourists need to be healthy enough for a multi-day travel, an increase in the number of waiting list patients on dialysis does neither affect the conflict probability nor the number of conflict events in transplant cells.[4]

[3]My sample countries are Argentina, India, Pakistan, Russia, and South Africa.

[4]Dialysis is the process of cleaning the blood from excess water, solutes, and toxins with the help of medical equipment, which patients with an acute kidney injury or an end-stage chronic kidney disease need to undergo. In North America, the treatment typically requires patients to visit a dialysis center for three times a week for 3 to 4 hours. While the urgency for receiving a kidney should be high for patients on dialysis, their condition hampers international travel, in particular to a lower-income country and a (supposedly) lower-quality hospital.

These findings are robust to alternative assumptions about the timing of transplants and conflicts, namely to measuring conflicts over the year following the change in kidney demand in a rolling window and to aggregating both kidney demand and conflicts on a yearly basis. The results are also present when using non-linear estimators. Moreover, I show that the positive impact of kidney demand in regions with a transplant center is no artefact of the different conflict trajectories in less and more densely populated regions. Placebo tests substituting the local transplant infrastructure with local nightlight intensity as a proxy for population density yield insignificant results.

Subsequently, I turn to the role of non-state armed groups and investigate whether armed groups with access to transplant infrastructure perform more attacks with increasing kidney demand. I determine a group's potential involvement in transplant tourism by the existence of an authorized transplant center in their hand-collected 0.5° latitude × 0.5° longitude cell of origin. Consistent with my hypothesis that armed groups use the proceeds from transplant tourism to finance attacks, groups with transplanting capacities perform more attacks when kidney demand increases, both in their home region and in other regions. In particular, a one standard deviation increase in the number of waiting list patients increases an armed groups' probability of conflict by 13% and its probability of performing an attack *outside* its home region by 16% if it has a transplant center in its home region. Again, the relationship is stronger for waiting list patients with labor income and absent for waiting list patients on dialysis. These results show that the involvement in transplant tourism allows armed groups to enhance their fighting capacities, both in their home region and abroad.

Finally, I focus on cross-border financial flows from organ recipients to non-state armed groups. To substantiate the hypothesis that proceeds from illegal transplant tourism pass the official banking system, at least in parts (c.f. Homeland Security Committee 2016), I use data on cross-border payments reported as suspicious to the Financial Crime Enforcement Network (FinCEN), which were leaked by the International Consortium of Investigative Journalists (ICIJ) in 2020. In a country-level analysis, I compare the effect of higher kidney demand on suspicious payments from and to countries known for transplant tourism to the effect in countries without ties to transplant tourism. The results show that, indeed, higher kidney demand is associated with an increase in suspicious payments from and to countries notorious for transplant tourism.

My findings contribute to two strands of literature: First, by demonstrating that armed groups use transplant tourism to finance their activities, I add to existing studies which suspect that criminal groups skim off huge profits from transplant agreements but lack systematic data to prove this claim (Fraser 2016; Shelley 2018). Thereby, my paper contributes to the literature on how terrorists and armed groups finance themselves and their attacks. Most existing papers in this stream of literature focus on legal sources of finance, such as donations (Limodio 2022), oil and gas business (Financial Action Task Force 2015), or mining activities (Berman et al. 2017). Illegal activities like robbery, smuggling, fraud, or kidnapping (Makarenko 2004), by contrast, are less understood - mainly because of the data scarcity on illegal activities (ECOSOC 2006; Organization for Security and Cooperation in Europe 2013).

Second, I add to the literature on transplant tourism and illegal organ trafficking. Scholars in medical anthropology, health ethics, and security studies have identified cases and discussed the dynamics of transplant tourism and illegal organ trafficking. They have analyzed the transnational space and power asymmetries in which organ transplants take place (Scheper–Hughes 2000; Scheper-Hughes 2003), have identified benefits and costs for donors (Goyal et al. 2002; Cohen 2003) and recipients (Gill et al. 2008) and have discussed the notion of informed consent (Scheper–Hughes 2000; Cohen 2003). My paper augments their case studies, observations, and (expert) interviews with a systematic, quantitative analysis based on a large dataset. Moreover, my study shows that, in addition to vulnerable donors and recipients, a third party, namely the local population, suffers from the consequences of illegal organ trafficking.

My paper is closely related to Berman et al. (2017) who demonstrate how minerals fuel conflict in Africa. I follow Berman et al. (2017)'s identification strategy, shedding light on another financing source for violent attacks.

The remainder of this paper is structured as follows. The following section gives an overview of the existing evidence on transplant tourism and introduces my conceptual framework. I then describe my data and variables in Section 2.3. Section 2.4 presents results on the impact of kidney demand on local conflict. Section 2.5 shows how conflict activity spreads over space by enhancing the financial capabilities of armed groups. In Section 2.6, I present the association between kidney demand, transplant infrastructure, and suspicious cross-border bank transfers. Section 2.7 concludes.

## 2.2 Existing evidence and conceptual framework

This chapter gives an overview of the literature on organ markets and transplant tourism. In addition to scientific studies, I include anecdotal evidence from institutional reports to build my hypotheses on any existing evidence which might help to understand transplant tourism as a source of terrorist financing. I then develop six hypotheses, which are tested in Section 2.4 to Section 2.6.

### 2.2.1 The market for organs

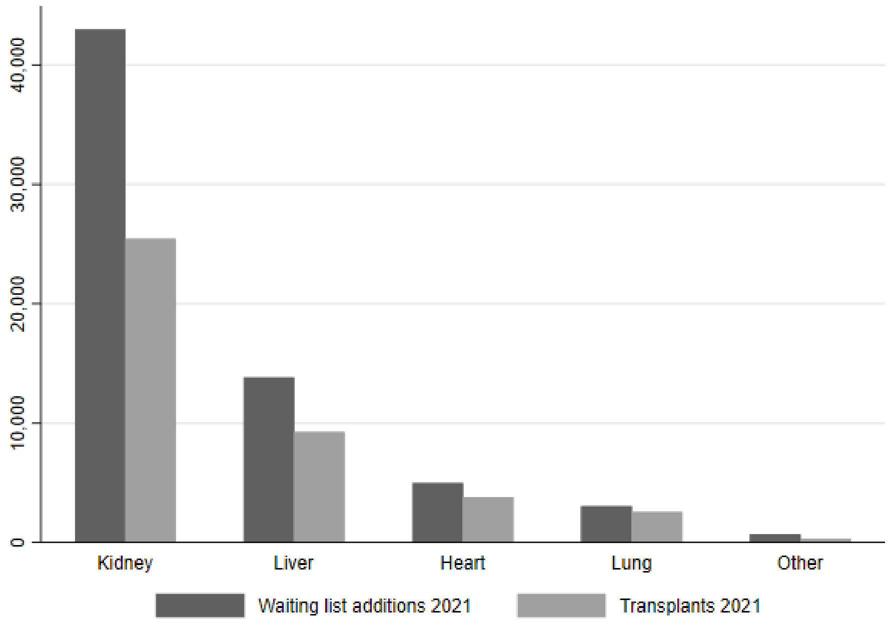
Like any market, organ markets have a demand and supply side. On the demand side, people whose organs are failing or working poorly wish to receive a substitute organ. A transplantation can lengthen patients' life and allow those with a chronic illness to live a normal lifespan. The demand for organs is thus driven by the desire for survival and, consequently, highly inelastic. On the supply side, deceased or living donors offer their organs to someone in need. While some organs, such as the heart, can only be transplanted from brain dead people, others, such as the kidney or parts of the liver, can be obtained from a living donor. Conditional on professional surgery and post-transplantation care, a living donor can live a normal, healthy life after donation, relying on her remaining kidney or a regrowing liver. Supplying a kidney is therefore a viable option for most healthy individuals.

Unlike for most other markets, the free exchange of organs between donors and recipients is forbidden in almost all countries of the world.[5] Instead, patients in need can put their names on waiting lists and receive an organ according to politically determined algorithms. These algorithms consider aspects of justice and medical utility. A patient's position on the waiting list therefore depends on the match between recipient and donor, on the waiting time, or on the urgency of the transplantation, among

[5]Iran is the only country which offers people a legal way to sell organs. However, the organ market in Iran is still strictly regulated: A government foundation registers buyers and sellers, matches them up and sets a fixed price of US$ 4,600 per organ (Bengali 2017).

**Figure 2.1:** US waiting lists additions and transplants performed 2021

This figure shows the number of patients added to the US waiting list for different organs in 2021 and the number of patients from the waiting list receiving an organ in 2021. Data is from the Organ Procurement and Transplantation Network.



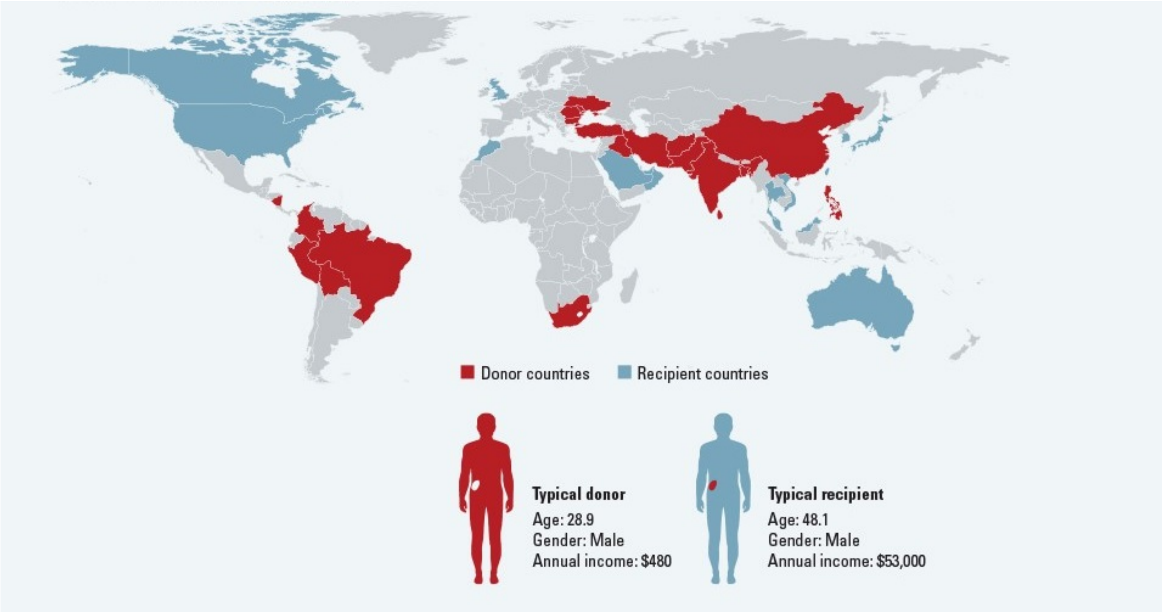

other things (Health Resources and Service Administration 2021; Organ Procurement and Transplantation Network 2022).[6]

A closer look into the global statistics on waiting lists and performed transplants reveals the core problem of existing organ markets: Demand highly exceeds supply (The Economist 2008; Health Resources and Service Administration 2021). For example, as shown in Figure 2.1, 43,617 patients joined the US waiting list for a kidney in 2021 (adding to the more than 100,000 patients already waiting in the end of 2020). In the same period, only 25,490 waiting list patients received a kidney. Consequently, more than half of all US patients die on the waiting list before having received an organ (Astier 2020). Worldwide, the WHO assumes that only one in ten patients in need receives a kidney by legal means (The Economist 2008).

[6]There is an extensive body of research investigating optimal allocation mechanisms for organs (especially kidneys), based on game-theoretical approaches. This research strand is, however, only loosely related to my research question. See, e.g., Roth, Sonmez, and Unver (2004), Roth, Sönmez, and Ünver (2005), Roth, Sönmez, and Utku Ünver (2005), Roth, Sönmez, and Ünver (2007), Ünver (2010), Ashlagi and Roth (2012), Kessler and Roth (2014), Giwa et al. (2017) and Roth (2018).

**Figure 2.2:** The global market for kidneys

This figure was compiled by Der Spiegel (31/2012) based on data from Coalition for Organ Failure Solutions, Organ Watch, and the European Society for Organ Transplantation. It visualizes anecdotal evidence from newspaper articles, security agency reports and case studies on global transplant tourism.




Source: Der Spiegel (31/2012)

### 2.2.2 Organ black markets and transplant tourism

As a consequence of the shortage in legal organs, illegal trade flourishes. Researchers expect that 5 to 10% of all transplants happen in black markets (Organization for Security and Cooperation in Europe 2013). The black market for organs is expected to be specifically vivid for living donations, given the willingness of people in need to make quick money by selling "spare" organs or organ parts. As 75% of the illegal trade is over kidneys (Hazell 2012), this paper focuses on transplant tourism for kidneys.

The black market for kidneys is global. Combining anecdotal information from newspaper articles, security agency reports and case studies, Figure 2.2 provides a stylized picture of the structure of transplant-tourism agreements: The mostly male donors are typically from low-income countries. They are, on average, younger than 30 and have an annual income of less than US$ 500. Recipients are also predominantly male. They come from high-income countries, are, on average, 48 years old and have an annual income of about US$ 53,000.

*Kidney donors in black markets*

Research on illegal organ donors shows that most of them consider compensated kidney donation as an opportunity to pay off debt. A minority also sells their kidneys to raise money for a dowry, to buy a house, or to start a business (Scheper–Hughes 2000; Goyal et al. 2002; Cohen 2003). However, Goyal et al. (2002) find that expected economic benefits did not materialize for a sample of 305 individuals who sold their kidney in Chennai, India, in the 1990s and 2000s: Some years after the donation, three quarters of participants were still (or, again) indebted. Average donor family income decreased by one third after the donation and the number of participants living in poverty increased. These negative economic consequences were mostly due to deteriorated employment opportunities caused by health problems in consequence of unprofessional surgeries or a lack of post-transplant care: About 86% of surveyed donors reported a deterioration in their health status after nephrectomy. As a result, 79% of participants would not recommend others to sell a kidney (Goyal et al. 2002).

In addition to voluntary donations, there are incidences of forced transplants, e.g., doctors who took out kidneys without the patient's knowledge during another surgery (Scheper–Hughes 2000), or criminals who killed for organs (Expansión 2014). Judging from newspaper articles and existing case studies, forced transplants are a minority of reported illegal organ trafficking cases (Organization for Security and Cooperation in Europe 2013). As my setting does not allow me to distinguish between voluntary and forced donation and both provide non-state armed groups an opportunity to finance violent attacks, I remain neutral about the question whether donation was forced out of circumstances or organs were taken without consent. Possible revenues from forced organ removal from prisoners after their execution, however, accrue to the government, rather than to non-state groups. I therefore remove China from my analysis, where this practice used to be most common (Allison et al. 2015).

*Kidney recipients in black markets*

Although most existing studies focus on the precarious situation of illegal kidney donors, kidney recipients might also suffer unfavorable consequences. Gill et al. (2008) investigate post-transplantation outcomes of 33 transplant tourists from the US and compare them with patients who underwent transplantation at the University of California, Los Angeles (UCLA). Most of the surveyed patients traveled to their region of ethnicity. The majority underwent living unrelated transplantation in China (44%), Iran (16%), and the Philippines (13%). Of Gill et al.'s sample, four patients needed urgent hospitalization, three of those lost their graft. Seventeen (52%) patients got infections, nine of them requiring hospitalization. One patient died from complications related to donor-contracted hepatitis B. Transplant tourist's one-year graft survival was 89%, compared to 98% for the matched UCLA cohort. The rate of acute rejection at one year was 30% in tourists and 12% in the matched cohort. This research implies that, while US based recipients should prefer to receive an organ through the official list, organ scarcity induces patients to search for an alternative abroad, notwithstanding the expected inferior conditions.

*The role of non-state groups*

Regardless the economic and health consequences for donors and recipients, illegal transplants are lucrative for other involved parties: The price paid by a kidney recipient is typically more than 20 times as high as the compensation received by the donor: The Council of Europe (2019) reports that recipients pay between US$ 100,000 and US$ 200,000 for a kidney, while donors receive between US$ 1,000 and US$ 10,000, at most, other sources report only US$ 500.[7] Deducting the costs of the surgery - costs for a legal, professional kidney transplant in India, for instance, range from US$ 8,500 to US$ 14,000 (Clinic Spots 2022) - results in profit margins of several 100%. Existing literature is ambiguous on who absorbs most of this profit: While newspaper articles have identified doctors and hospital as beneficiaries, middlemen or brokers are assumed to capture most of the profit (Council of Europe 2019).

In line with Fraser (2016) and Shelley's statement in the Homeland Security Committee (2016), I suspect that local, non-state armed groups act as a broker or collaborate with brokers by protecting their transplant tourism business.

[7]From the donors' perspective, this is still a large sum making paid donation a valid option, e.g., compared to an average yearly income of of about 700 US$ of the bottom 50% in India (Chancel et al. 2021). From the recipients perspective, this is still a decent price, given the average total costs of an official kidney transplant in the US also amount to over US$ 400,000 (Bentley and Ortner 2020), although costs for official transplants might be (partly) borne by patients' health insurance.

### 2.2.3 Hypotheses

I start from the assumption that non-state armed groups are financially constrained (Berman et al. 2017). Security experts suspect that, in addition to donations (Limodio 2022), oil and gas business (Financial Action Task Force 2015), and mining activities (Berman et al. 2017), armed groups use proceeds from illegal organ trade to finance attacks (Homeland Security Committee 2016). Accordingly, I expect that the more organs can be sold in a given time and the higher their price, the higher the probability of an attack and the higher the number of attacks.

The number of organs that can be sold in black markets and the price of these organs should depend on the mismatch between legal organ supply and organ demand in countries which organ recipients stem from. As I cannot observe the supply of organs in recipient countries, I assume it to be constant, on average. The potential to sell an organ via a transplant tourism agreement in a donor country should then be higher, (i) the more patients need an organ in a recipient country, (ii) the higher the payment capacities of these patients, and (ii) the better patients' ability to travel. I use US organ demand in my analyses as the US offers the best data on these three aspects of organ demand. The US is considered one of the most important – if not the most important – recipient country (United Nations Office on Drugs and Crime 2015). As the black market for organs predominantly trades kidneys (see Section 2.2.2), I focus on US kidney demand. I consequently test the following hypotheses:

*Hypothesis 1: The larger the number of patients on the US waiting list for kidneys, the higher the probability of an attack and the more attacks are performed in locations with transplant infrastructure.*

*Hypothesis 2: The relationship between conflict and kidney demand is stronger for waiting list patients with a higher income.*

*Hypothesis 3: The relationship between conflict and kidney demand is weaker for waiting list patients who are unable to travel.*

Non-state armed groups which establish a transplant tourism business or get critically involved in a transplant tourism scheme need to be well-connected locally: First, to be able to recruit donors, groups need to have the trust of the local population. Second, they need to build reliable relations with doctors in transplant hospitals who perform the surgeries. Third, a good connection to local authorities will help to avoid detection and prosecution of illegal transplant schemes. Most non-state armed groups do have these required local connections, with some important ties to administrative bodies and the local population, including professionals like doctors (Krause and Milliken 2009). They should therefore mainly participate in transplant tourism in their home region. In this context, it is worth mentioning that most non-state violent attacks in my sample are performed by relatively small, local groups, rather than by large, transnational groups like Al-Qaeda or Al-Nusra (see Appendix 2I). While these small groups will rarely have the possibility to involve in transplant tourism outside of their home region, they might still use the proceeds from transplant tourism to perform violent attacks all over the country, or even cross-border. I therefore test if the total number of a group's attacks increases with higher kidney demand, both in its home region and in all regions outside its home region:

*Hypothesis 4 : The larger the number of patients on the waiting list, the higher the probability and number of attacks by groups whose home region has a transplant infrastructure.*

*Hypothesis 5: The larger the number of patients on the waiting list, the higher the probability and number of attacks by groups whose home region has a transplant infrastructure performed* outside *their home region.*

Payments between broker and donor mainly occur cash on the spot and in local currency. Transfers between recipient and broker are, however, cross-border payments and require currency clearing. It is unclear how these payments are made. Security experts, e.g., in the Homeland Security Committee (2016), suspect that most of these payments are made via official bank transfers. Bain and Mari (2018) also assume that surgeons, anesthetists and nurses, laboratories or medical facilities, but also individual brokers receive payments for illegal transplants on their usual bank accounts. In the context of the financing of armed groups, one illegal transplant should induce several payments within a criminal network, both between different members of the network and between third parties, e.g., payments for weapons financed with the proceeds of transplant tourism. I therefore test a final hypothesis:

*Hypothesis 6: The larger the number of people on the waiting list, the more suspi-*

*cious payments are made to and from localities with transplant infrastructure.*

Before I test these hypotheses in Section 2.4 to Section 2.6, the following section provides details on data and variables.

## 2.3 Data and variables

I base my analyses on a sample of localities in five countries known for transplant tourism activities which also have relevant non-state violent activity, i.e., Argentina, India, Pakistan, Russia, and South Africa (Scheper–Hughes 2000; Goyal et al. 2002; Cohen 2003; Scheper-Hughes 2003; ECOSOC 2006; Organization for Security and Cooperation in Europe 2013; Council of Europe 2019). Following Berman et al. (2017), I define a locality as a subnational unit of 0.5° latitude $\times$ 0.5° longitude. The structure of my dataset is hence a full grid of the sample countries divided into subnational units of 55 $\times$ 55 kilometers size (at the equator) or a little larger (elsewhere). I prefer this level of aggregation over using administrative boundaries to avoid that my unit of observation is endogenous to conflict events (c.f. Berman et al. 2017). My level of analysis in the baseline analysis in Section 2.4 is cell-month. I use the months between January 2010 and March 2021, as conflict data is available in adequate detail for my sample from 2010 on only. In the following, I describe the data used and show descriptive statistics of my sample. A summary of all variable definitions and sources is provided in Appendix 2B.

### 2.3.1 Conflict events

The publicly available Armed Conflict Location and Event Data Project (ACLED) provides real-time data on locations, dates, actors, fatalities and types of all reported political violence and protest events across the world (Raleigh et al. 2010).[8] ACLED obtains events from various sources, including press accounts from regional and local news, humanitarian agencies, or research publications. The database serves my purpose well because it contains detailed information on conflict events, most importantly on the exact day and location of a conflict, but also on the type of events and on names and characteristics of all involved actors. Moreover, ACLED records political violence without a battle-related deaths threshold. This is important in my setting because

[8] Armed Conflict Location & Event Data Project (ACLED); acleddata.com

local, small groups usually do not kill that many people in one attack.

*Cell-level conflict events*

I assign each conflict event to a cell and a month using the information on latitude and longitude and the day associated with each event. I only include violent conflict events in which non-state groups participate. This embraces the event types "Battles" (except for battles in which the "Government regains territory"[9]), "Explosions/Remote violence" (except for "Air/drone strikes"[10]), and "Violence against civilians". I do not include events from the category "Protests", as they are defined as non-violent, nor of the category "Riots", which, though violent, are defined as mostly spontaneous actions by unorganized, unaffiliated members of society. "Strategic developments", pooling activities like "Agreements", "Arrests", or "Looting/property destruction" are also not included, as the financial necessities for these activities are not obvious. I construct two variables measuring different dimensions of conflict. First, I capture the extensive margin of conflict with a *Conflict dummy* indicating if at least one event has happened in a cell in a given month. Second, I measure the intensive margin of conflict by the number of *Conflict events* in a cell in a given month. As this number is skewed to the right, I use the logarithm of the number of conflict events plus 1 in the analyses.

Reported cases of transplant tourism suggest that organ recipients pay close to the operation date, either shortly before or shortly after the transplant (Organization for Security and Cooperation in Europe 2013). Based on Berman et al. (2017), I further assume that armed groups carry out attacks quickly after having enough money to do so. Accordingly, my main specification measures kidney demand and non-state violent attacks in the same month. However, my results are robust to measuring attacks for a rolling window of the 12 months following the month when kidney demand is measured (Appendix 2D) and to aggregating data on a yearly level (Appendix 2E).

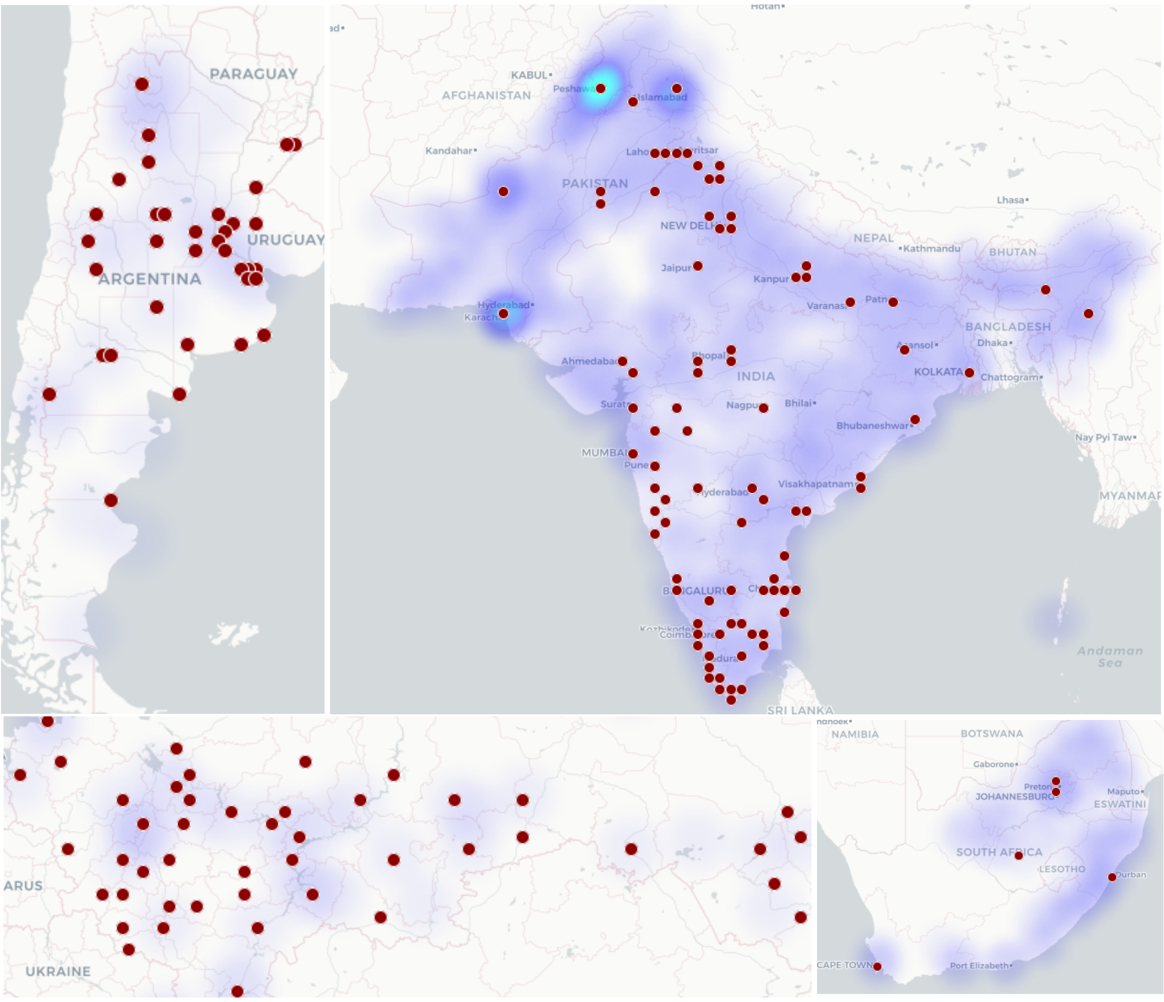
Figure 2.3 shows the spatial distribution of conflict events in my sample countries in a heat map. Figure 2.4 reports how the average probability of conflict (*Conflict*

[9]In the context of conflicts between the government and armed groups, events in which "Government regains territory" are mostly government operations to fight back armed groups. The timing of these operations is independent of the armed group and should therefore be unrelated to its financing.

[10]I assume that air/drone strikes are predominantly used by government forces. The non-state armed group targeted in these strikes might fight back, but has no power over the timing of the event.

**Figure 2.3:** Spatial distribution of conflict events and transplant centers

This figure shows a heatmap of non-state violent conflicts from The Armed Conflict Location & Event Data Project (ACLED) that happened in my sample countries between January 2010 and March 2021. Deeper colors indicate a higher frequency of conflict. The map also shows hand-collected transplant centers as red dots.



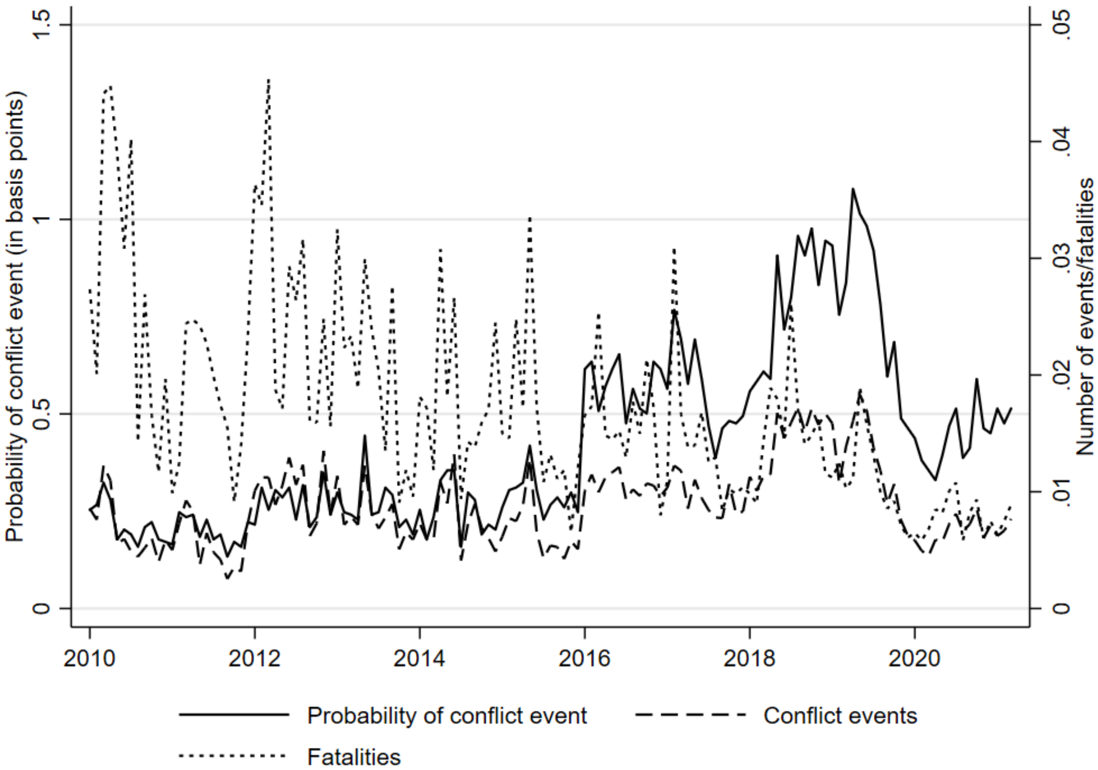

*dummy*), the number of conflict events (*Conflict events*, in levels in this graph) and the number of fatalities vary over time. The data exhibits considerable variation in both the local and the temporal dimension.

*Group-level conflict events*

In my second analysis, I investigate whether armed groups increase their overall number of attacks with higher kidney demand if their home region has a transplant infrastructure. To do so, I transform the dataset to an armed group-month level. Here, I define the *Conflict dummy* to be one if the group is involved in at least one event in a given month. I aggregate *Conflict events* on the group level and use the logarithm

**Figure 2.4:** Probability of conflict, conflict events and fatalities

This figure shows the average probability of a conflict event, the number of events and the number of fatalities in an 0.5° latitude × 0.5 longitude cell in my sample. Data is from The Armed Conflict Location & Event Data Project (ACLED).



of their total plus one.

I define a group's *Home region* as the cell in which (i) the group has its headquarters, or (ii) the group was founded, or (iii) the ethnic affiliation of the group is based, or (iv) the community mentioned in the group's name is based. I use Wikipedia and other online sources to determine these locations. I provide a list of all groups of the analysis and their manually determined home region in Appendix 2I.[11]

### 2.3.2 Transplant infrastructure

In almost all reported cases, illegal transplanting happened alongside legal transplants (Organization for Security and Cooperation in Europe 2013). I therefore proxy the local potential for transplant tourism by the existence of a legal transplant infrastructure in a given cell. I use official government lists of authorized transplant centers to determine their location. For some of the countries, these lists are publicly available via the health ministry's websites. For others countries, I contacted the health ministries or the agency responsible for transplantation via email. For some countries that I would have liked to include in my analysis, especially Libya, Lebanon, and Egypt, I was unable to obtain a list with official transplant centers as the relevant institution did not reply to my emails. Appendix 2A gives an overview of the data sources for authorized transplant centers in my sample.

Given the location obtained via a manual Google Maps search, I assign each transplant center to a 0.5° latitude × 0.5 longitude cell. I define the variable *Transplant center* to be one if at least one authorized transplant center is located in a cell and zero otherwise. I assume that transplant infrastructure is constant over my sample period as, in most countries, no information is available about when a transplant center first obtained or when it lost its authorization. Since it is unlikely that an armed group establishes an authorized transplant center with the sole aim to finance an increase of (already planned) attacks, reverse causality should not pose a problem here. If armed groups did, indeed, succeed in establishing new transplant centers as a source of finance, this would be captured by my analyses. My conclusions that non-state armed

[11] Berman et al. (2017) define a group's *Homeland* as their hand-collected ethnic origin, combined with geocoordinates of ethnic homelands from the Georeferencing of Ethnic Groups (GREG) dataset. For the cases where I can establish the headquarters or founding location of an armed group, I prefer this kind of information as, for being involved in the transplant tourism business, being present in a specific locality should matter more than a group's ethnic affiliation.

groups finance their attacks by transplant tourism agreements would remain valid in this case. There is no indication that any state would establish transplant centers preemptively in the expectation of increasing attacks or that groups would fight about the control of a transplant center. Figure 2.3 shows the distribution of transplant centers in my sample countries as red dots.

Naturally, using authorized transplant centers to proxy for the potential for illegal transplant activities ignores possible illegal transplant centers which have no local association with a legal center. However, this will affect my results only if illegal transplant centers are dis-proportionally placed in the absence of legal centers. In this case, my estimates would set a lower bound of the actual effect.

For my group-level analyses, I construct the variable *Transplant center at home region* if the group's 0.5° latitude × 0.5 longitude cell of origin has a transplant center.

For my country-level analyses on the relationship between suspicious payments and transplant infrastructure, I create a country-level variable identifying countries as possible candidates for transplant tourism. I define a country to be a *Trafficking country* if it has reported cases of organ trafficking, according to research articles, newspaper articles and reports. The following countries are defined as *Trafficking countries*: Azerbaijan (Bloomberg 2011; Rafizade and Mirzayeva 2020), Albania (Ambagtsheer and Weimar 2012), Belarus (Bloomberg 2011), Brazil (Newsweek 2009; Bloomberg 2011; Insightcrime 2012), Bosnia (Ambagtsheer and Weimar 2012), China (Fan 2014; Woan 2007), Colombia (Mendoza 2010a; Ambagtsheer and Weimar 2012), Costa Rica (Insightcrime 2013; Insightcrime 2019), Ecuador (Insightcrime 2012), Egypt (Ambagtsheer and Weimar 2012), Georgia (NBC News 2014), Haiti (CNN 2010), Israel (Ambagtsheer and Weimar 2012), India (Ambagtsheer and Weimar 2012), Kosovo (Der Spiegel 2012; Times of Israel 2018), Libya (Huffpost 2017), Mexico (Insightcrime 2012; Al Jazeera 2014; Insightcrime 2019), Moldova (Bloomberg 2011), Montenegro (Ambagtsheer and Weimar 2012), North Macedonia (Rafizade and Mirzayeva 2020), Pakistan (Fatima et al. 2018; CBS News 2023), Peru (Insightcrime 2012), Philippines (Mendoza 2010b; Ambagtsheer and Weimar 2012; CNA 2019), Russia (Khomyakova and Bagretsov 2021; Kochin and Kovalenko 2021), Serbia (Ambagtsheer and Weimar 2012), South Africa (Newsweek 2009; Ambagtsheer and Weimar 2012), Turkey (Ambagtsheer and Weimar 2012; Daily Sabah 2022), and the United States (Newsweek 2009).

### 2.3.3 Kidney demand

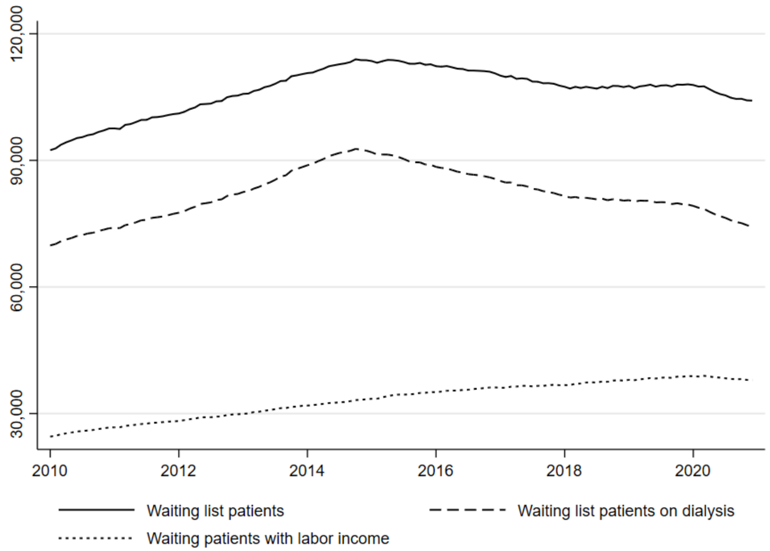
I use information on all waiting list registrations and transplants that have been listed or performed in the US since October 1, 1987 from the United Network of Organ Sharing (UNOS) Standard Transplant Analysis and Research File (National UNOS STAR file). This datafile includes detailed medical information on each patient registered on the waiting list. For my analysis, I use the exact day of joining and leaving the waiting list, the start and the end of a possible dialysis and the information if a patient has labor income when joining the list.

I first construct the variable *Waiting list patients*, counting the total number of patients on the US waiting list for a kidney in a given month. Second, to capture the payment capacity of people on the waiting list, I generate the variable *Waiting list patients with labor income* counting all people on the US waiting list for a kidney who had a labor income when joining the waiting list. Third, the variable *Waiting list patients on dialysis* proxies for patients' inability to travel. Patients with an acute kidney injury or an end-stage chronic kidney disease need to undergo dialysis, a process of cleaning the blood from excess water, solutes, and toxins with the help of medical equipment. In North America, the treatment typically requires patients to visit a dialysis center for three times a week for 3 to 4 hours. While the urgency for receiving a kidney should be high for patients on dialysis, their condition hampers international travel, in particular to a lower-income country and a (supposedly) lower-quality hospital. To calculate *Waiting list patients on dialysis*, I use the number of people on the US waiting list for a kidney who need dialysis in a given month. Figure 2.5 shows the number of waiting list patients, the subset of patients with labor income and the subset of patients under dialysis over my sample period.

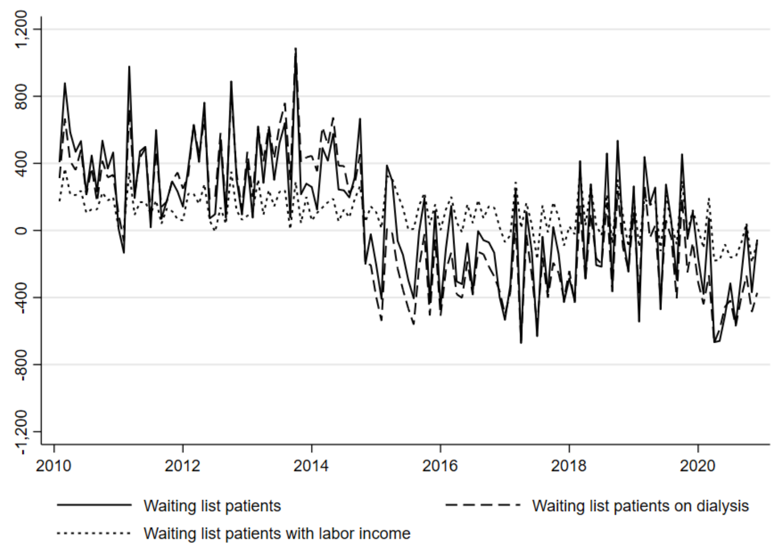
**Figure 2.5:** Demand for kidneys on the US waiting list

This figure shows the number of patients on the U.S waiting list for kidneys, the number of patients on the US waiting list for kidneys who had labor income when joining the waiting list, and the number of patients on the US waiting list for kidneys who are on dialysis. Panel (A) shows the absolute number, panel (B) presents monthly changes. Data is from The Armed Conflict Location & Event Data Project (ACLED).

Panel (A)



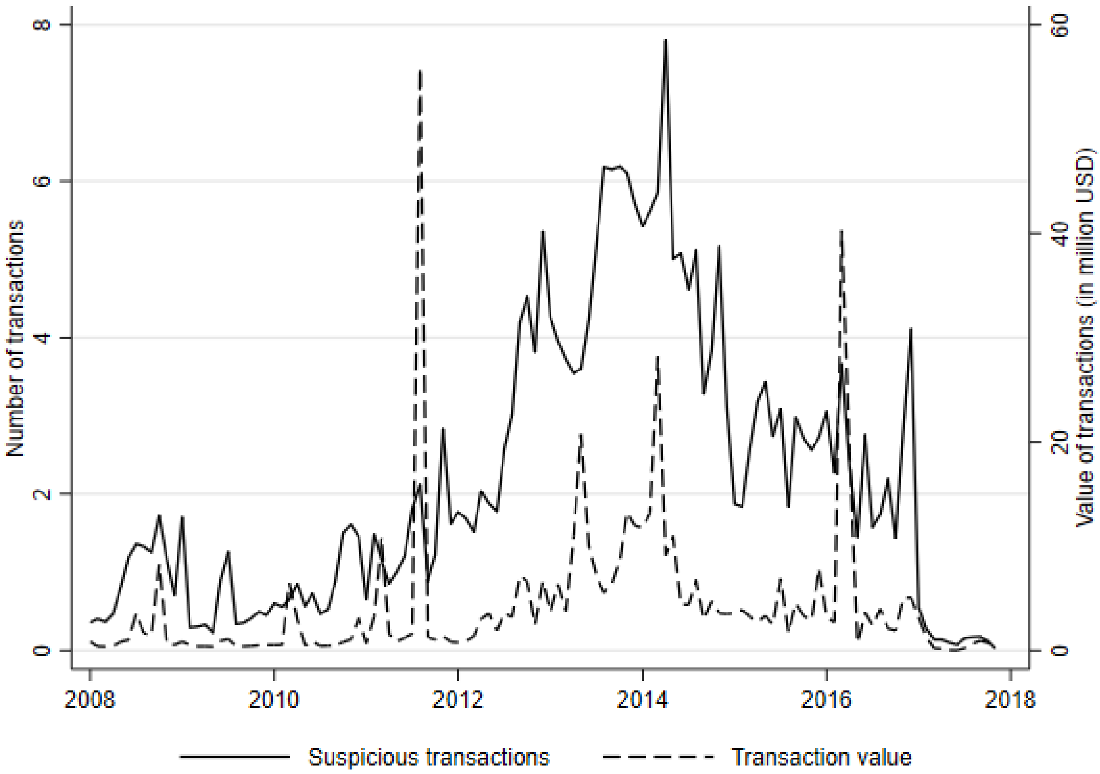

Panel (B)



As can be seen from Panel (A) in Figure 2.5, in the long run, the number of waiting list patients seems relatively stable. However, as Panel (B) shows, the number varies considerably on a monthly basis. As all my regressions include cell or group fixed-effects, what matters for my analysis is the change over time. Aggregating waiting list data on a yearly level eliminates some of the variation, which is why I use the monthly specification in my main analyses.[12] [13]

**Figure 2.6:** Suspicious payments

This figure shows the average number of suspicious payments from and to a country of my sample and the average transferred value. Payments are defined as suspicious if they have been reported to the US Financial Crime Enforcement Network (FinCEN) by a global correspondent bank. The (non-representative) sample of FinCEN data was leaked by the International Consortium of Investigative Journalists (ICIJ) in 2020.




[12]My results are robust to using yearly data and to measuring conflict events in the rolling window of 12 months after kidney demand is measured.

[13]One might wonder what happens to patients registered on the US waiting list after having obtained an organ via a transplant tourism agreement. Due to the illegality of the transaction, patients might not drop out of the waiting lists, or, if they do, under a pretext. Given the relatively small chance of receiving an organ via the list, most transplant tourists might simply stay registered until they die and are correctly classified as dead. Figure 2C.1 in Appendix 2C shows different reasons under which patients exit the list. Reasons that could subsume recipients leaving the list after a successful transplant tourism operation are highlighted in red.

### 2.3.4 Suspicious payments

To measure *Suspicious payments* from and to countries potentially involved in transplant tourism, I draw on available data from the *FinCEN files*. These files report international payments which global correspondent banks have flagged as suspicious with the US Financial Crime Enforcement Network (FinCEN). In fall 2020, the International Consortium on Investigative Journalists (ICIJ) leaked and published parts of this data.

I include all available countries in the analysis on suspicious payments. I aggregate payments on a country and month level for all available years, that is, from 2008 to 2018. I analyze both incoming and outgoing payments from countries as the business of transplant tourism may involve several partners, some of them receiving money within the country of the business, some of them receiving money outside of the transplanting country, e.g., as a compensation for arms delivery. As the number of suspicious payments is skewed to the right, I take the logarithm of the number of payments plus one. The average number of suspicious payments from and to a country from 2008 to 2018 is shown in Figure 2.6.

### 2.3.5 Descriptive statistics

Table 2.1 reports descriptive statistics for my sample. I use the data of Panel A, B and C in the locality-level analysis in Section 2.4. I use the data of Panel B, D and E in the armed group-level analysis in Section 2.5. I use the data of Panel F, G and B in the analysis on suspicious payments in Section 2.6.

Panel A of Table 2.1 shows the conflict activity in the 15,875 $0.5° \times 0.5°$ cells of my sample. The probability of having a conflict in a given month is 0.423%, with a standard deviation of 6.49. Per month, 0.0089 conflict events happen in an average cell. For the cells in which at least one conflict is reported, the number of conflict events is 2.3, on average. Panel D gives information on the conflict activity of each of the 708 non-state armed groups in my sample. The likelihood that a group is involved in a conflict in a given month is 1.59%, with an average monthly number of conflicts of 0.0303. Conditional on being involved in a conflict, the average group's number of conflict events is 1.90. Non-state armed groups' probability of being involved in a conflict outside their home cell is 1.18%. The average number of conflicts in other cells than their home cell is 0.0242, or 2.05, conditional on being involved in a conflict at all.

Panel B of Table 2.1 reports kidney demand over my sample period. On average, the US waiting list for kidneys contains 106,554 patients, with a standard deviation of 5,347. The number of waiting list patients who have a labor income when joining the waiting list is 33,409, on average, representing about one third of all patients[14]. The average number of waiting list patients on dialysis is 81,857. Note that a patient could join the waiting list without being on dialysis but could become part of the patients on dialysis later.

Panel C of Table 2.1 shows summary statistics on the transplant infrastructure in the 15,875 cells of my sample. The average number of transplant centers per cell is 0.0389 and ranges from zero to 31 in different cells. 1.33% of all cells in my analysis have a transplant center. Panel E gives information about access to transplant centers on the level of an armed group. On average, the 708 non-state armed groups from my sample have 2.7 transplant centers in their home region. This number, again, ranges from zero to 31. 36% of the groups have at least one transplant center in their home region. Panel G summarizes country-level data on transplant infrastructure for the 105 countries included in the country-level analysis on suspicious payments. 20 countries out of the 105 of my country-level analysis, or 19.8%, are defined as *Trafficking countries* (see Section 2.3.2).

Panel F of Table 2.1 reports the number of suspicious financial transactions reported to FinCEN by global correspondent banks.[15] On average, there are 1.59 suspicious payments reported from and to each of the 105 countries per month. The number of suspicious flags per country ranges from 0 to 174.

[14]Note that for some waiting list patients, there is no information whether they have a labor income when joining the waiting list or not. I exclude these patients from my analyses on patients with labor income. However, this means that a larger fraction of all patients than the one suggested in this table could have a labor income when joining the waiting list.

[15]As explained in Section 2.3.4, this sample period differs from my other sample period as the FinCEN data is only available between 2008 and 2018. As the data is anyways just a limited excerpt from all FinCEN reports, I use all available data.

**Table 2.1:** Descriptive Statistics

This table shows descriptive statistics for all variables used in the regression models in Section 2.4 to Section 2.6. Data in Panel A, B and C are used in the locality-level analysis in Section 2.4. Data in Panel B, D and E are used in the armed group level analysis in Section 2.5. Data in Panel F, G and B are used in the analysis on suspicious payments in Section 2.6.

| | N | Mean | SD | Median | Min | Max |
|---|---|---|---|---|---|---|
| **Panel A: Cell-month level** | | | | | | |
| **Conflict** in 15,875 cells over 135 months | | | | | | |
| Probability of conflict in % | 2,128,140 | .423 | 6.49 | 0 | 0 | 100 |
| Conflict events | 2,128,140 | .0089 | .257 | 0 | 0 | 61 |
| \| Events > 0 | 9,008 | 2.30 | 3.35 | 1 | 1 | 61 |
| **Panel B: Month level** | | | | | | |
| **Kidney demand** over 135 months | | | | | | |
| Waiting list patients | 2,128,140 | 106,554 | 5,347 | 107,526 | 92,409 | 113,951 |
| with labor income | 2,128,140 | 33,409 | 4,290 | 34,506 | 24,538 | 38,952 |
| on dialysis | 2,128,140 | 81,857 | 6,025 | 81,015 | 69,849 | 92,709 |
| **Panel C: Cell level** | | | | | | |
| **Transplant infrastructure** in 15,875 cells | | | | | | |
| N transplant centers | 2,128,140 | .0389 | .633 | 0 | 0 | 31 |
| At least one center in % | 2,128,140 | 1.33 | 11 | 0 | 0 | 100 |
| **Panel D: Group-month level** | | | | | | |
| **Conflict** of 708 groups over 135 months | | | | | | |
| Probability of conflict in % | 95,715 | 1.59 | 12.52 | 0 | 0 | 100 |
| Conflict events | 95,715 | .0303 | .3441 | 0 | 0 | 20 |
| \| Events > 0 | 1,526 | 1.90 | 1.97 | 1 | 1 | 20 |
| Prob. of conflict outside home region in % | 95,715 | 1.18 | 10.80 | 0 | 0 | 100 |
| Conflict events outside home region | 95,715 | .0242 | .3219 | 0 | 0 | 20 |
| \| Events outside home region > 0 | 1,129 | 2.05 | 2.15 | 1 | 1 | 20 |
| **Panel E: Group level** | | | | | | |
| **Transplant infrastructure at home region** of 708 groups | | | | | | |
| N transplant centers | 95,715 | 2.7 | 6.2 | 0 | 0 | 31 |
| At least one center in % | 95,715 | 36 | 48 | 0 | 0 | 100 |
| **Panel F: Country-month level** | | | | | | |
| **Financial transactions** from and to 105 countries over 170 months | | | | | | |
| Suspicious payments | 18,020 | 1.59 | 7.50 | 0 | 0 | 174 |
| **Panel G: Country level** | | | | | | |
| **Organ trafficking** in 105 countries | | | | | | |
| Trafficking country | 105 | 0.1981 | 0.3986 | 0 | 0 | 1 |

## 2.4 The impact of organ demand on local conflict

I now turn to the empirical analysis of how organ demand impacts local conflict. I first discuss my identification strategy and then report results of different specifications.

### 2.4.1 Methodological considerations

Establishing a causal relationship between global organ demand on local conflict involves several methodological challenges. The first and most important one is a concern about reverse causality: War zones are a major target for organ recruitment and create organ demand at the same time. Consequently, the more conflicts happen, the more organs are needed and the more organs can be acquired. This implies the same, positive correlation as my proposed hypothesis. To address this concern, I exploit variation in US organ demand, which is exogenous to local conflict in my sample countries.

The second concern refers to a potential spurious correlation between conflict and organ demand over time. As visible from Figure 2.4 and Figure 2.5, both the number of reported conflicts and the number of waiting list patients have increased in my sample over time, especially in the first years. A positive correlation between both variables could therefore be an artefact of their common trend. To solve this problem, I estimate my coefficients in a difference-in-difference manner: I compare the effect of a change in kidney demand on local conflict in those cells in which transplant tourism could take place, i.e., cells with transplant infrastructure, to the effect in cells in which this is not possible. In particular, I estimate the following regression for each locality $i$ in country $c$ and month $t$:

$$\begin{aligned} Conflict_{it} =& \beta_0 + \beta_1 Transplant\ center_i \times Kidney\ demand_t + \\ & FE_i + FE_{ct} + \epsilon_{it} \end{aligned} \tag{2.1}$$

$Conflict_{it}$ is one out of the two variables $Conflict\ dummy_{it}$ and $Conflict\ events_{it}$. $Transplant\ center_i$ is a binary variable assuming the value of 1 for cells with a transplant center and 0 for all other cells. $Kidney\ demand_t$ is the number of patients on the US waiting list for kidneys, the number of those patients who have joined the waiting list with labor income, or the number of waiting list patients on dialysis, respectively. $FE_i$ are cell fixed effects, $FE_{ct}$ are additional fixed effects which can vary at different levels, i.e. on the month, the country×month and the country-state×month level. Cell fixed effects absorb the base effect of transplant centers whose existence at a cell is fixed over time. Month, country×month, or country-state×month fixed effects absorb the base effect of kidney demand which is fixed across cells for each month. Therefore, Equation (2.1) does no include the variables $Transplant\ center_i$ and $Kidney\ demand_t$

separately, but only their interaction.

$\beta_1$ is the coefficient of interest. It can be interpreted as the difference between the impact of a one unit-increase in kidney demand on conflict in cells with a transplant center, compared to those without a transplant center.

I use a linear probability model to estimate the effect of kidney demand on the probability of conflict and a log-linear model to estimate the effect of kidney demand on the number of conflict events. I favor linear over nonlinear estimators, also for the binary outcome variable, as the linear estimators allow me to include several dimensions of fixed effects. I provide robustness checks using nonlinear estimators, namely conditional logit and Poisson pseudo-maximum-likelihood estimators in Appendix 2F.

As visible in Figure 2.3, both conflicts and transplant centers are locally clustered. I therefore apply a spatial HAC correction which allows for both cross-sectional spatial and location-specific serial correlation, building on Conley (1999) and Hsiang, Meng, and Cane (2011). Following Berman et al. (2017), I restrict spatial correlation to 500 km and assume serial correlation to only vanish in infinity (i.e., 100,000 months). Accordingly, I do not constrain the temporal decay for the Newey-West/Bartlett kernel which weights serial correlation across time periods.

One further concern with fixed effects models of (relatively) rare events data is that the elimination of no-event units from the sample may result in biased marginal effects (Cook, Hays, and Franzese 2020). Applying the penalized maximum likelihood fixed effects estimator proposed by Cook, Hays, and Franzese (2020) shows that correcting for this issue does not significantly alter my results (Appendix 2G). I do not use Cook, Hays, and Franzese (2020)'s estimator for my main specification as it does not allow for the extensive correction for spatial and serial clustering applied in my main analyses.

Existing evidence is unclear about the exact timing of events. Armed groups could wait with their attacks some months after receiving the money. Therefore, in addition to regressing conflict events on kidney demand of the same month, I run an alternative specification of events aggregated from month $t$, i.e., the month when kidney demand is measured, to month $t+11$, i.e., one year after kidney demand is measured (Appendix 2D). Moreover, I provide robustness checks using yearly data in Appendix 2E.

A final issue concerns the definition of different dimensions of kidney demand: Both the number of waiting list patients with labor income when joining the waiting list and those on dialysis are a subset of total waiting list patients. As such, they proxy for the total number of waiting list patients. Given a positive effect from kidney demand on conflict, any non-orthogonal subset of the number of total kidney demand should yield higher regression coefficients, by design. To address this issue and obtain comparable coefficients, I standardize the three waiting list variables in all my analyses.

### 2.4.2 Results

Table 2.2 reports the results for the linear probability model in which I regress the *Conflict dummy* on the independent variables. Coefficients are reported in basis points.

The regressions reveal a significant and sizable effect of increased kidney demand on violent conflict in cells with a transplant center. Compared to cells without a transplant center, a one standard-deviation increase in the number of patients on the waiting list increases the cell's probability of conflict by 93.13 basis points (column (1) of Table 2.2). Compared to a base probability of conflict of 5.50% in transplant cells, this is an increase of 17%. This effect is economically significant, considering that a one standard deviation increase in the waiting list for kidneys is equivalent to 5,347 new registrations on a list which has, on average, 106,554 patients. The effect is robust when including country×month fixed effects, i.e. controlling for country-specific time trends (column (2) of Table 2.2), or country-state×month fixed effects, i.e. controlling for country-state specific time trends (column (3) of Table 2.2).

In line with Hypothesis 2, the effect is stronger for waiting list patients who have joined the list with a labor income. A one standard deviation increase in the number of patients with income raises the probability of conflict by 2.48 percentage points, on average (column (4) of Table 2.2). This is an increase of 45%, compared to the base probability. Again, this effect is sizable considering that a one standard deviation increase in patients with income is equivalent to 4,290 new registrations to the average 33,409 patients with income. The effect is robust to the inclusion of country×month fixed effects (column (5) of Table 2.2), and country-state×month fixed effects (column (6) of Table 2.2).

In line with the idea that receiving an organ in a transplant tourism agreement requires the recipient to be healthy enough for traveling, coefficients for waiting list patients on dialysis are insignificant and small (columns (7) to (9) of Table 2.2).

Table 2.3 reports the results of regressing the log number of conflict events on the independent variables. The coefficients show that an increase in kidney demand does not only increase the extensive, but also the intensive margin of conflict. A one standard deviation increase in the waiting list for kidneys increases the number of conflict events in transplant cells by an average of 0.9%, as compared to non-transplant cells (column (1) of Table 2.3) The effect size is similar when including country×month or country-state×month fixed effects (columns (2) and (3) of Table 2.3). Like for the extensive margin, the effect is stronger for waiting list patients with income: On average, the number of conflict events in a cell with transplant infrastructure increases by 1.8% with a one standard-deviation increase in waiting list patients with income (column (4) of Table 2.3) and remains significantly positive when controlling for country or country-state specific time trends (columns (5) and (6) of

Table 2.3). Again, the effect is insignificant for waiting list patients on dialysis like hypothesized in Hypothesis 3 (columns (7) to (9) of Table 2.3).

Taken together, these results are in line with Hypothesis 1, 2, and 3: Conflicts increase with a rising kidney demand in cells with a transplant infrastructure, both in the extensive and the intensive margin. This effect is stronger for waiting list patients with income and absent for waiting list patients on dialysis.

**Table 2.2:** The impact of organ demand on conflict probability

This table reports coefficients of a linear probability model regressing a local binary conflict variable on the interaction between transplant infrastructure and kidney demand (see Equation (2.1)). Conley's (1999) standard errors, allowing for spatial correlation within a 500 km radius and for infinite serial correlation, are shown in parenthesis. The sample consists of monthly observations of 15,876 cells of 0.5° latitude × 0.5° longitude, covering 5 countries between 2010 and 2021. The dependent variable *Conflict dummy* is a binary variable indicating if a conflict took place in a cell in a given month. Independent variables are the binary variable *Transplant center*, indicating the existence of an authorized transplant center in an 0.5° latitude × 0.5° longitude cell, and the standardized number of (i) patients on the US waiting list for a kidney, (ii) patients on the US waiting list for a kidney who had labor income when joining the waiting list, and (iii) patients on the US waiting list for a kidney on dialysis. Models (1), (4), and (7) include cell and month fixed effects, models (2), (5), and (8) include cell and country×month fixed effects, models (3), (6), and (9) include cell and country-state×month fixed effects.

| | Dependent variable: Probability of conflict (in basis points) | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Transplant center | | | | | | | | | |
| × waiting list patients | 93.13*** | 75.41*** | 71.19*** | | | | | | |
| | (16.72) | (16.23) | (13.68) | | | | | | |
| × waiting list patients with income | | | | 247.65*** | 191.80*** | 178.85*** | | | |
| | | | | (38.25) | (36.23) | (28.82) | | | |
| × waiting list patients on dialysis | | | | | | | 2.22 | 6.88 | 7.06 |
| | | | | | | | (14.42) | (14.10) | (13.22) |
| Observations | 2,128,140 | 2,127,195 | 2,113,290 | 2,128,140 | 2,127,195 | 2,113,290 | 2,128,140 | 2,127,195 | 2,113,290 |
| Cell fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Month fixed effects | Yes | No | No | Yes | No | No | Yes | No | No |
| Country × month FEs | No | Yes | No | No | Yes | No | No | Yes | No |
| Country-state × month FEs | No | No | Yes | No | No | Yes | No | No | Yes |
| Base prob. transplant cells | 550.26 | 550.26 | 550.26 | 550.26 | 550.26 | 550.26 | 550.26 | 550.26 | 550.26 |
| R-squared | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.01 | 0.01 | 0.01 |

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

**Table 2.3:** The impact of organ demand on the number of conflict events

This table reports coefficients of a linear regression of the number of conflict events on the interaction between transplant infrastructure and kidney demand (see Equation (2.1)). Conley's (1999) standard errors, allowing for spatial correlation within a 500 km radius and for infinite serial correlation, are shown in parenthesis. The sample consists of monthly observations of 15,876 cells of 0.5° latitude × 0.5° longitude, covering 5 countries between 2010 and 2021. The dependent variable *Conflict events* is the log number of conflict events that took place in a cell in a given month. Independent variables are the binary variable *Transplant center*, indicating the existence of an authorized transplant center in an 0.5° latitude × 0.5° longitude cell, and the standardized number of (i) patients on the US waiting list for a kidney, (ii) patients on the US waiting list for a kidney who had labor income when joining the waiting list, and (iii) patients on the US waiting list for a kidney on dialysis. Models (1), (4), and (7) include cell and month fixed effects, models (2), (5), and (8) include cell and country×month fixed effects, models (3), (6), and (9) include cell and country-state×month fixed effects.

| | Dependent variable: Conflict events | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Transplant center | | | | | | | | | |
| × waiting list patients | 0.009*** | 0.008*** | 0.007*** | | | | | | |
| | (0.00) | (0.00) | (0.00) | | | | | | |
| × waiting list patients with income | | | | 0.018** | 0.014* | 0.012* | | | |
| | | | | (0.01) | (0.01) | (0.01) | | | |
| × waiting list patients on dialysis | | | | | | | 0.003 | 0.004 | 0.004 |
| | | | | | | | (0.00) | (0.00) | (0.00) |
| Observations | 2,128,140 | 2,127,195 | 2,113,290 | 2,128,140 | 2,127,195 | 2,113,290 | 2,128,140 | 2,127,195 | 2,127,195 |
| Cell fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Month fixed effects | Yes | No | No | Yes | No | No | Yes | No | No |
| Country × month FEs | No | Yes | No | No | Yes | No | No | Yes | No |
| Country-state × month FEs | No | No | Yes | No | No | Yes | No | No | Yes |
| Mean log events transplant cells | 0.07 | 0.07 | 0.07 | 0.07 | 0.07 | 0.07 | 0.07 | 0.07 | 0.07 |
| R-squared | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.01 | 0.01 | 0.01 |

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

### 2.4.3 Robustness and placebotests

As discussed in Section 2.4.1, these results are robust to a range of alternative specifications and methodological choices: Running the analysis on a yearly panel (Appendix 2E) or using a rolling window for *Conflict dummy* and *Conflict events*, i.e. aggregating conflicts over the year following the month when kidney demand is measured, yields qualitatively similar results (Appendix 2D). Results are also qualitatively similar when using nonlinear estimators, i.e. conditional logit and Poisson pseudo-maximum-likelihood estimators (Appendix 2F) or when applying the penalized maximum likelihood fixed effects estimator proposed by Cook, Hays, and Franzese (2020) which corrects for the probable bias resulting from my rare event data (Appendix 2G).

One remaining concern, however, is that general differences in the conflict trajectories of densely populated and sparsely populated regions could drive my results. For instance, the number of reported conflicts in well-populated regions might increase over the years due to better data quality, while sparsely populated region do not experience any conflicts over the entire sample period. At the same time, densely populated regions are more likely to have a transplant center. Combined with a relatively steady increase in kidney demand observed over some of the sample years, the different trajectories between highly populated and sparsely populated regions could therefore be spuriously correlated with kidney demand in cells with a transplant infrastructure, but not so in cells without a transplant infrastructure. This or a similar pattern could result in the effects reported in the previous section.

To alleviate this concern, I run a placebo test substituting the variable *Transplant center* with the variable *High nightlight*, proxying for population density. This variable is one for regions in which the Earth Observation Group's satellites report high nighttime light and zero for regions with low nighttime light in 2021.[16] I clean the data following Elvidge et al. (2021) and then define *High nightlight* such that the fraction of cells with *High nightlight* is similar to the fraction of cells with a *Transplant center*, resulting in a value of one for every cell with a nighttime light value above the 97th percentile. Appendix 2H reports the results. Cells with *High nightlight* do neither experience a higher conflict probability (Table 2H.1) nor a higher number of conflict events when the number of *Waiting list patients* or *Waiting list patients with income* increases. This indicates that, rather than merely taking up population density, a cell's transplant infrastructure is indeed responsible for the effect estimated in the previous section.

[16] I use nightlight from one year only as I aim at producing a variable that does not change over time, just like the variable *Transplant center*. I choose the year 2021 as I assume that data quality improves over time and my sample ends in 2021.

## 2.5 How transplant tourism increases fighting capabilities of armed groups

The findings of the previous section show that higher kidney demand induces local non-state violence in regions with a transplant center. In this section, I examine if increased financial capabilities of local armed groups are responsible for these attacks. The analysis is based on the assumption that non-state armed groups are most likely to involve in transplant tourism business in their home region, given that the business requires good connections to the local population, the doctors in transplant hospitals and local authorities. Non-state armed groups should then be able to extend their attacks in response to a higher kidney demand if and only if they have a transplant center in their home region.

### 2.5.1 Methodological considerations

As detailed in Section 2.3, I now focus on the attacks performed by a certain group, conditional on the existence of a transplant infrastructure in its home region. In particular, I run the following specification for armed groups $j$ in country $c$ and month $t$:

$$\begin{aligned} Conflict_{jt} = &\beta_0 + \beta_1 Transplant\ center\ at\ home\ region_j \times Kidney\ demand_t \\ &+ FE_j + FE_{tc} + \epsilon_{jt} \end{aligned} \tag{2.2}$$

$Conflict_{jt}$ captures the two dimensions of conflict: $Conflict\ dummy_{jt}$ is a dummy indicating if a group has performed an attack and $Conflict\ events_{jt}$ is the log number of attacks performed in a given month. $Transplant\ center\ at\ home\ region_j$ assumes the value of one if the group's home region has a transplant center and zero otherwise. $Kidney\ demand_t$ is the number of patients on the US waiting list for kidneys, the number of those patients who have joined the waiting list with labor income, or the number of waiting list patients on dialysis, respectively. $FE_j$ are group fixed effects, $FE_{ct}$ are additional fixed effects on the month or country×month level. I do not include country-state×month fixed effects in the group-level regressions as states absorb most of the group-level variation in transplant capacities[17]

$\beta_1$ is the coefficient of interest. It can be interpreted as the difference between the impact of a one unit-increase in kidney demand on attacks by groups with a transplant

[17] 17 out of the 46 states in my sample only have groups without transplant centers in their home region. Two states only have groups with transplant centers in their home region. 10 out of the remaining 27 states are the home region of only seven non-state armed groups or fewer.

center at home, compared to those without a transplant center. To account for within-group correlation and serial correlation, I cluster standard errors by group and month, using two-way clustering. Like in the previous section, I standardize the waiting list variables.

### 2.5.2 Results

Table 2.4 reports the results of regressing a group's *Conflict dummy* on the interaction between *Kidney demand* and *Transplant center at home region*. A one standard deviation increase in the number of waiting list patients increases the probability of conflict of a group with transplant infrastructure by 28.62 basis points, compared to a group without transplant infrastructure (column (1) of Table 2.4). In comparison to a transplant group's base probability of conflict of 2.18%, this is a jump of 13%. The result is robust to including country×month fixed effects (column (2) of Table 2.4).

As hypothesized, coefficients are larger for an increase in the number of waiting list patients who have joined the list with labor income: A one standard-deviation increase in the number of these patients is associated with an increase in conflict probability of 59.92 basis points, compared to groups without a transplant center at home (column (3) of Table 2.4). This 29% increase compared to the base probability remains when controlling for country-specific time trends (column (4) of Table 2.4). A higher number of waiting list patients on dialysis, again, has no disproportionate impact on violence of groups with and without transplant infrastructure at home.

Table 2.5 reports the results for the intensive margin of conflict, i.e., the coefficients of regressing a group's *Conflict events* on the interaction between *Transplant center at home region* and *Kidney demand*. A one standard deviation higher kidney demand is associated with 0.2% more conflict events of groups with a transplant center at home. However, the effect is only significant at the 10% level (column (1) of Table 2.5) and, though having the same size, gets insignificant when including country×month fixed effects (column (2) of Table 2.5). For those patients with labor income, a one standard-deviation increase in the number of waiting list patients increases the number of conflict by approximately 0.7% (columns (3) and (4) of Table 2.5). For waiting list patients on dialysis, coefficients are insignificant and small.

These results are in line with the idea that groups use revenues from transplant tourism to carry out attacks (Hypothesis 4), increasing the group's extensive and intensive margin of conflict. To investigate whether armed groups use the transplant infrastructure at home to finance attacks in other cells (Hypothesis 5), I consider a group's attacks *outside its home region* as the dependent variables in the following. Table 2.6 and Table 2.7 present the results from this analysis.[18]

[18] Conflicts outside the group's home region are a subset of all conflicts. Coefficients in Table 2.6

With a one standard deviation increase in patients on the waiting list, the base probability of a conflict outside a group's home region of 1.61% increases significantly by 25.79 basis points for groups with a transplant center at home (column (1) of Table 2.6). This is a 16% increase, compared to the base probability of 16.13%. The effect is larger for patients who joined the waiting list with income: A one standard-deviation increase in the number of these patients increases the base probability by 51.98 basis points (column (3) of Table 2.6), an increase of 32%. For waiting list patients on dialysis, the effect is insignificant and small. The specifications including country×month fixed effects (column (2), (4) and (6) of Table 2.6) are of similar size and significance.

Table 2.7 reports the results for the intensive margin. A one standard deviation increase in the number of waiting list patients leads to an increase in the number of outside attacks of approximately 0.2%, a one standard deviation increase of the number of waiting list patients with income by 0.6%, respectively. For waiting list patients on dialysis the effect does not significantly deviate from zero.

Overall, these results lend support to Hypothesis 5: An increase in kidney demand increases the probability of conflict and the number of conflict outside a group's home region more for those groups with a transplant center at home than for groups without such center in their home region. This indicates that armed groups, indeed, make use of transplant infrastructure at home to finance attacks, both at their home region and abroad.

and Table 2.7 should therefore, by design, be smaller than in Table 2.4 and Table 2.5, given that the hypothesized mechanism is at work. This is the case in my analyses.

**Table 2.4:** The impact of organ demand on a group's conflict probability

This table reports OLS coefficients of a linear probability model regressing a group's binary conflict variable on the interaction between transplant infrastructure and kidney demand (see Equation (2.2)). The sample consists of monthly observations of 708 non-state armed groups between 2010 and 2021. The dependent variable *Conflict dummy* is a binary variable indicating if the group was involved in a conflict in a given month. Independent variables are the binary variable *Transplant center at home region*, indicating the existence of an authorized transplant center in the group's home region, and the standardized number of (i) patients on the US waiting list for a kidney, (ii) patients on the US waiting list for a kidney who had labor income when joining the waiting list, and (iii) patients on the US waiting list for a kidney on dialysis. Models (1), (3), and (5) include group and month fixed effects, models (2), (4), and (6) include group and country×month fixed effects. Standard errors are two-way clustered by group and month and shown in parentheses.

| | Dependent variable: Group's probability of conflict (in basis points) | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Transplant center at home region | | | | | | |
| × waiting list patients | $28.62^{**}$ | $27.43^{**}$ | | | | |
| | (13.89) | (13.48) | | | | |
| × waiting list patients with income | | | $59.92^{**}$ | $64.20^{**}$ | | |
| | | | (29.72) | (29.86) | | |
| × waiting list patients on dialysis | | | | | 6.95 | 3.58 |
| | | | | | (13.62) | (12.69) |
| Observations | 95,580 | 95,580 | 95,580 | 95,580 | 95,580 | 95,580 |
| Group fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Month fixed effects | Yes | No | Yes | No | Yes | No |
| Country × month FEs | No | Yes | No | Yes | No | Yes |
| Base prob. transplant groups | 217.94 | 217.94 | 217.94 | 217.94 | 217.94 | 217.94 |
| R-squared | 0.12 | 0.13 | 0.12 | 0.13 | 0.12 | 0.13 |

$^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

**Table 2.5:** The impact of organ demand on a group's number of conflict events

This table reports OLS coefficients of regressing an armed group's number of attacks on the interaction between transplant infrastructure and kidney demand (see Equation (2.2)). The sample consists of monthly observations of 708 non-state armed groups between 2010 and 2021. The dependent variable is an armed group's log number of conflicts. Independent variables are the binary variable *Transplant center at home region*, indicating the existence of an authorized transplant center in the group's home region, and the standardized number of (i) patients on the US waiting list for a kidney, (ii) patients on the US waiting list for a kidney who had labor income when joining the waiting list, and (iii) patients on the US waiting list for a kidney on dialysis. Models (1), (3), and (5) include group and month fixed effects, models (2), (4), and (6) include group and country×month fixed effects. Standard errors are two-way clustered by group and month and shown in parentheses.

| | Dependent variable: Group's conflict events | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Transplant center at home region | | | | | | |
| × waiting list patients | 0.002* | 0.002 | | | | |
| | (0.00) | (0.00) | | | | |
| × waiting list patients with income | | | 0.007* | 0.007* | | |
| | | | (0.00) | (0.00) | | |
| × waiting list patients on dialysis | | | | | -0.000 | -0.000 |
| | | | | | (0.00) | (0.00) |
| Observations | 95,580 | 95,580 | 95,580 | 95,580 | 95,580 | 95,580 |
| Group fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Month fixed effects | Yes | No | Yes | No | Yes | No |
| Country × month FEs | No | Yes | No | Yes | No | Yes |
| Mean log events transplant groups | 0.02 | 0.02 | 0.02 | 0.02 | 0.02 | 0.02 |
| R-squared | 0.18 | 0.18 | 0.18 | 0.18 | 0.18 | 0.18 |

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

**Table 2.6:** The impact of organ demand on a group's conflict probability outside its home region

This table reports OLS coefficients of a linear probability model regressing a group's binary conflict variable on the interaction between transplant infrastructure and kidney demand (see Equation (2.2)). The sample consists of monthly observations of 708 non-state armed groups between 2010 and 2021. The dependent variable *Conflict dummy outside home region* is a binary variable indicating if the group was involved in a conflict outside its home region in a given month. Independent variables are the binary variable *Transplant center at home region*, indicating the existence of an authorized transplant center in the group's home region, and the standardized number of (i) patients on the US waiting list for a kidney, (ii) patients on the US waiting list for a kidney who had labor income when joining the waiting list, and (iii) patients on the US waiting list for a kidney on dialysis. Models (1), (3), and (5) include group and month fixed effects, models (2), (4), and (6) include group and country×month fixed effects. Standard errors are two-way clustered by group and month and shown in parentheses.

| | Dependent variable: Group's probability of conflict outside home region (in basis points) | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Transplant center at home region | | | | | | |
| × waiting list patients | 25.79** | 24.69** | | | | |
| | (12.78) | (12.29) | | | | |
| × waiting list patients with income | | | 51.98* | 55.86* | | |
| | | | (29.11) | (29.32) | | |
| × waiting list patients on dialysis | | | | | 6.49 | 3.45 |
| | | | | | (12.30) | (11.39) |
| Observations | 95,580 | 95,580 | 95,580 | 95,580 | 95,580 | 95,580 |
| Group fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Month fixed effects | Yes | No | Yes | No | Yes | No |
| Country × month FEs | No | Yes | No | Yes | No | Yes |
| Base prob. transplant groups | 161.32 | 161.32 | 161.32 | 161.32 | 161.32 | 161.32 |
| R-squared | 0.16 | 0.16 | 0.16 | 0.16 | 0.16 | 0.16 |

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

**Table 2.7:** The impact of organ demand on a group's number of conflict events outside its home region

This table reports OLS coefficients of regressing an armed group's number of attacks outside its home region on the interaction between transplant infrastructure and kidney demand (see Equation (2.2)). The sample consists of monthly observations of 708 non-state armed groups between 2010 and 2021. The dependent variable *Conflict events outside home region*is the log number of conflicts outside a group's home region. Independent variables are the binary variable *Transplant center at home region*, indicating the existence of an authorized transplant center in the group's home region, and the standardized number of (i) patients on the US waiting list for a kidney, (ii) patients on the US waiting list for a kidney who had labor income when joining the waiting list, and (iii) patients on the US waiting list for a kidney on dialysis. Models (1), (3), and (5) include group and month fixed effects, models (2), (4), and (6) include group and country×month fixed effects. Standard errors are two-way clustered by group and month and shown in parentheses.

| | Dependent variable: Conflict events outside home region | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Transplant center at home region | | | | | | |
| × waiting list patients | $0.002^{*}$ | $0.002^{*}$ | | | | |
| | (0.00) | (0.00) | | | | |
| × waiting list patients with income | | | 0.006 | $0.007^{*}$ | | |
| | | | (0.00) | (0.00) | | |
| × waiting list patients on dialysis | | | | | 0.000 | -0.000 |
| | | | | | (0.00) | (0.00) |
| Observations | 95,580 | 95,580 | 95,580 | 95,580 | 95,580 | 95,580 |
| Group fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Month fixed effects | Yes | No | Yes | No | Yes | No |
| Country × month FEs | No | Yes | No | Yes | No | Yes |
| Mean log events transplant groups | 0.02 | 0.02 | 0.02 | 0.02 | 0.02 | 0.02 |
| R-squared | 0.21 | 0.21 | 0.21 | 0.21 | 0.21 | 0.21 |

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

## 2.6 Organ demand, transplant infrastructure, and suspicious payments

In this section, I investigate the link between kidney demand, transplant infrastructure and suspicious payments. If at least some of the payments for transplant tourism are transferred via the official banking system and detected as suspicious by global banks, we should observe more suspicious international payments from and to regions known for transplant tourism.

### 2.6.1 Methodological considerations

Ideally, I would like to investigate suspicious payments on a granular local level. However, due to the lack of granular payment data, I use aggregated data on the country-month level. My results should consequently be interpreted with caution as those countries with transplant facilities might share other developments, which are spuriously related to US kidney demand. I include country and month fixed effects to adjust for unobserved country characteristics that are constant over time and for time-varying developments common to all countries. Specifically, for each country $c$ in month $t$, I estimate the following model:

$$\begin{aligned} Payments_{ct} = \beta_0 + \beta_1 Trafficking\ country_{ct} \times Kidney\ demand_t \\ + FE_c + FE_t + \epsilon_{ct} \end{aligned} \tag{2.3}$$

$Payments_{ct}$ is the log number of suspicious payments from and to country $c$ in a given month. I run the analysis for the sum of all payments, for all payments received by the country and for all payments sent from the country. $Trafficking\ country_c$ is a dummy assuming the value of one if the country is known for organ trafficking, and zero otherwise (see Section 2.3.4). $\beta_1$ is the coefficient of interest. It can be interpreted as the difference between the impact of a one unit-increase in kidney demand on suspicious payments from and to a country known for organ trafficking, compared to a country that has no organ trafficking record. To account for within-country correlation and serial correlation, I cluster standard errors by country and month, using two-way clustering.

### 2.6.2 Results

Table 2.8 reports the results of the analyses. The significantly positive coefficient of the interaction between $Trafficking\ country$ and $Kidney\ demand$ in column (1) of Table 2.8 indicates that the number of payments from and to countries with a record of organ trafficking increases more with an increase in US kidney demand, compared to the number of payments from and to countries without a trafficking record. In particular, a one standard deviation increase in the number of waiting list patients is associated with 13.6% more suspicious payments from and to transplant countries (column (1) of Table 2.8). This increase can be decomposed into a 10.1% increase in payments received from and an 8.3% increase in payments sent by trafficking countries. Note that suspicious payments of my sample are a small, non-representative subsample of all detected payments, as the ICIJ only published parts of the FinCEN data. Therefore, the mean number of payments from and to transplant countries reported in Table 2.8 should not be interpreted.

The reported correlations are in line with Hypothesis 6 that higher kidney demand induces more suspicious payments from and to transplant countries, highlighting the relevance of received payments. This is consistent with the notion that transplant tourism is, at least partly, processed via the official banking system. However, due to the high aggregation level and the inconsistent result for waiting list patients on dialysis, these associations should not be interpreted causally.

**Table 2.8:** Organ demand, transplant infrastructure, and suspicious payments

This table reports OLS coefficients of regressing suspicious payments on the interaction between transplant infrastructure and kidney demand (see Equation (2.3)). The sample consists of monthly observations of 105 countries between 2008 and 2018. The dependent variable *Suspicious payments* is the log number of payments that have been reported as suspicious to the FinCEN by a global correspondent bank from and to a country. Independent variables are the binary variable *Trafficking country*, indicating if a country is notorious for organ trafficking based on a list compiled by different sources (see Section 2.3.2) and the standardized number of patients on the US waiting list for a kidney. All models include country and month fixed effects. Standard errors are shown in parentheses.

| | Dependent variable: Suspicious payments | | |
|---|---|---|---|
| | All payments | Payments received | Payments sent |
| | (1) | (2) | (3) |
| Trafficking country × waiting list patients | 0.136*** | 0.101*** | 0.083*** |
| | (0.04) | (0.03) | (0.03) |
| Observations | 1,802 | 1,802 | 1,802 |
| Country fixed effects | Yes | Yes | Yes |
| Year fixed effects | Yes | Yes | Yes |
| Mean log payments trafficking countries | 0.47 | 0.31 | 0.29 |
| R-squared | 0.54 | 0.47 | 0.48 |

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

## 2.7 Conclusion

This paper provides systematic evidence on the impact of transplant tourism on non-state violent conflict. I use monthly panel data with a spatial resolution of 0.5° latitude × 0.5° longitude covering five countries from 2010 to 2021. Combining geo-referenced data on non-state conflict, hand-collected data on local transplant infrastructure, and data on exogenous kidney demand from the US waiting list for kidneys, I find a significant and sizable effect of higher kidney demand on the extensive and intensive margin of local conflict for localities with transplant infrastructure. Further, I show that groups with transplant infrastructure at their home region perform more violent attacks if kidney demand is higher.

My findings indicate that armed groups participate in the lucrative business of transplant tourism and use the proceeds from this business to finance violent attacks. This reinforces concerns of security agencies that the pressing organ scarcity provides new financing sources for violent groups and terrorists.

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# Chapter II

# Appendix

## 2.A Sources for authorized transplant centers

Table 2A.1 lists the sources for authorized transplant centers in the countries of my sample. I determined the exact coordinates for each center with the help of Google Maps.

**Table 2.A1:** Sources for authorized transplant centers

| Country | Source for authorized transplant centers |
|---|---|
| Argentina | https://www.argentina.gob.ar/salud/incucai/organismos-jurisdiccionales |
| India | https://www.mohanfoundation.org/transplant-centres/index.asp |
| Pakistan | https://applications.emro.who.int/emhj |
| Russia | https://www.transpl.ru |
| South Africa | Direct contact with ministry of health |

## 2.B Variable definitions

**Table 2.B1:** Definition and sources of all variables

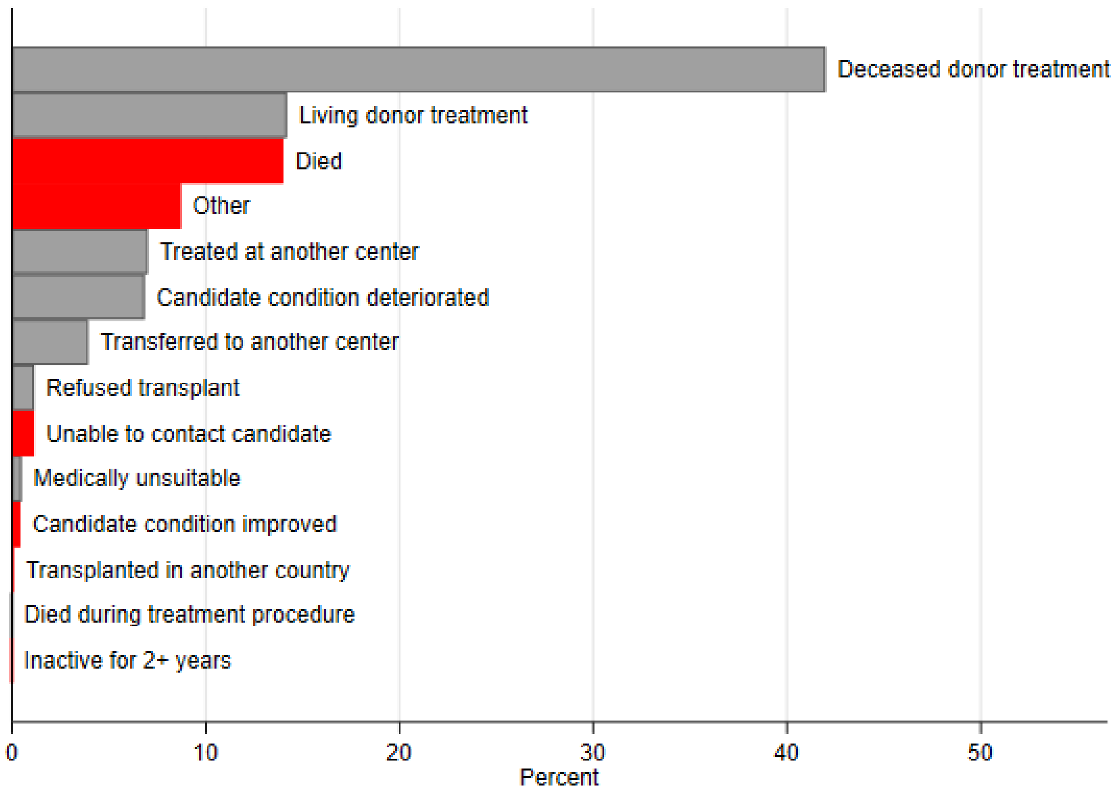
| Variable | Definition | Source |
|---|---|---|
| | **Panel A: Cell-month level** | |
| Conflict dummy | Binary variable indicating if at least one conflict event happened in a 0.5° latitude × 0.5° longitude cell in a month | The Armed Conflict Location & Event Data Project (ACLED) |
| Conflict events | Number of conflicts happening in a 0.5° latitude × 0.5° longitude cell in a month, for analyses: logarithm of events + 1 | ACLED |
| | **Panel B: Month level** | |
| Waiting list patients | Number of people on the waiting list in a given month | United Network of Organ Sharing Standard Transplant Analysis Research file (UNOS Star File) |
| Waiting list patients with labor income | Number of people on the waiting list in a given month who indicated that they have a labor income when entering the waiting list | UNOS Star File |
| Waiting list patients on dialysis | Number of people on the waiting list who are on dialysis in a given month | UNOS Star File |
| | **Panel C: Cell level** | |
| Transplant center | Binary variable indicating if there is at least one authorized transplant center in a 0.5° latitude × 0.5° longitude cell | Manual collection based on sources listed in Appendix 3A |
| | **Panel D: Group-month level** | |
| Conflict dummy | Binary variable indicating if a non-state armed group was involved in a conflict event in a given month | ACLED |
| Conflict events | Number of conflict events an armed group was involved in in a given month, for analyses: logarithm of events + 1 | ACLED |
| Conflict dummy outside home region | Binary variable indicating if a non-state armed group was involved in a conflict event outside its home region in a given month | ACLED |
| Conflict events outside home region | Number of conflict events outside a group's home region, for analyses: logarithm of events + 1 | ACLED |
| | **Panel E: Group level** | |
| Transplant center | Binary variable indicating if there is at least one transplant center in the 0.5° latitude × 0.5° longitude home region of an armed group | Manual collection based on sources given in Appendix 3A |
| Home region | 0.5° latitude × 0.5° longitude cell in which an armed group (i) has its headquarters, or (ii) was founded, or (iii) the ethnic affiliation of a group is based, or (iv) the community mentioned in a group's name is based. | Manual collection using Wikipedia and other online sources |
| | **Panel F: Country-month level** | |
| Suspicious payments | Number of payments from and to a country that have been flagged as 'suspicious' to the Financial Crime Enforcement Network (FinCen) by a global correspondent bank | International Consortium of Investigative Journalists (ICJA) (Leaked from FinCeN) |
| | **Panel G: Country level** | |
| Trafficking country | Binary variable indicating if country is listed as known for organ trafficking | Research articles, newspaper articles and reports cited in Section 2.3.2 |

## 2.C Transplant tourists on US waiting lists

What happens to patients registered on the US waiting list after having obtained an organ via a transplant tourism agreement? Due to the illegality of the transaction, patients might not drop out of the waiting lists, or, if they do, under a pretext. Given the relatively small chance of receiving an organ via the list, most transplant tourists might simply stay registered until they die and are correctly classified as dead. Figure 3C.1 shows different reasons under which patients exit the list. Stated reasons which could include successful transplant tourists are marked in red.

**Figure 2.C1:** Reasons for being removed from the US waiting list for kidneys

This figure shows the percentage of removals from the US waiting list kidneys for different reasons. Reasons that could subsume recipients leaving the list after a successful transplant tourism operation are marked in red. Data comes from the UNOS Star files.



## 2.D Conflict probability and events within a rolling window of 12 months

To account for the possibility that armed groups delay attacks for several months after the money inflow from a transplant, Table 2D.1 to Table 2D.6 show all my analyses with an alternative definition of the conflict variable: In this appendix, the $Conflict\ dummy_{it}$ is one if a conflict happened in month $t$ when kidney demand is measured, or in any of the following 11 months $t+1$ to $t+11$. $Conflict\ events_{it}$ are summed up from month $t$ to month $t+11$. All other variables are as defined in Section 2.3 and in Appendix 2B. The regression equations are specified in Section 2.4 and 2.5 of the paper.

**Table 2.D1:** The impact of organ demand on conflict probability over the next 12 months

This table reports coefficients of a linear probability model regressing a local binary conflict variable on the interaction between transplant infrastructure and kidney demand (see Equation (2.1)). Conley's (1999) standard errors, allowing for spatial correlation within a 500 km radius and for infinite serial correlation, are shown in parenthesis. The sample consists of monthly observations of 15,876 cells of 0.5° latitude × 0.5° longitude, covering 5 countries between 2010 and 2021. The dependent variable *Conflict dummy* is a binary variable indicating if a conflict took place between month t and month t+11. Independent variables are the binary variable *Transplant center*, indicating the existence of an authorized transplant center in an 0.5° latitude × 0.5° longitude cell, and the standardized number of (i) patients on the US waiting list for a kidney, (ii) patients on the US waiting list for a kidney who had labor income when joining the waiting list, and (iii) patients on the US waiting list for a kidney on dialysis. Models (1), (4), and (7) include cell and month fixed effects, models (2), (5), and (8) include cell and country×month fixed effects, models (3), (6), and (9) include cell and country-state×month fixed effects.

| | Dependent variable: Probability of conflict (in basis points) | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Transplant center | | | | | | | | | |
| × waiting list patients | 404.70*** | 262.08*** | 224.12*** | | | | | | |
| | (50.67) | (42.16) | (34.45) | | | | | | |
| × waiting list patients with income | | | | 984.90*** | 696.24*** | 608.39*** | | | |
| | | | | (92.45) | (80.45) | (65.51) | | | |
| × waiting list patients on dialysis | | | | | | | 53.40 | 12.00 | 4.42 |
| | | | | | | | (38.44) | (34.95) | (33.40) |
| Observations | 2,128,140 | 2,127,195 | 2,113,290 | 2,128,140 | 2,127,195 | 2,113,290 | 2,128,140 | 2,127,195 | 2,113,290 |
| Cell fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Month fixed effects | Yes | No | Yes | Yes | No | No | Yes | No | No |
| Country × month FEs | No | Yes | No | No | Yes | No | No | Yes | No |
| Country-state × month FEs | No | No | Yes | No | No | Yes | No | No | Yes |
| Base prob. transplant cells | 1754.85 | 1754.85 | 1754.85 | 1754.85 | 1754.85 | 1754.85 | 1754.85 | 1754.85 | 1754.85 |
| R-squared | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.01 | 0.01 | 0.01 |

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

**Table 2.D2:** The impact of organ demand on the number of conflict events over the next 12 months

This table reports coefficients of a linear regression of the number of conflict events on the interaction between transplant infrastructure and kidney demand (see Equation (2.1)). Conley's (1999) standard errors, allowing for spatial correlation within a 500 km radius and for infinite serial correlation, are shown in parenthesis. The sample consists of monthly observations of 15,876 cells of 0.5° latitude × 0.5° longitude, covering 5 countries between 2010 and 2021. The dependent variable *Conflict events* is the log number of conflict events that took place in a cell from month $t$ to month $t+11$. Independent variables are the binary variable *Transplant center*, indicating the existence of an authorized transplant center in an 0.5° latitude × 0.5° longitude cell, and the standardized number of (i) patients on the US waiting list for a kidney, (ii) patients on the US waiting list for a kidney who had labor income when joining the waiting list, and (iii) patients on the US waiting list for a kidney on dialysis. Models (1), (4), and (7) include cell and month fixed effects, models (2), (5), and (8) include cell and country×month fixed effects, models (3), (6), and (9) include cell and country-state×month fixed effects.

| | Dependent variable: Conflict events | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Transplant center | | | | | | | | | |
| × waiting list patients | 0.067*** | 0.050*** | 0.045*** | | | | | | |
| | (0.01) | (0.01) | (0.01) | | | | | | |
| × waiting list patients with income | | | | 0.127*** | 0.093*** | 0.084*** | | | |
| | | | | (0.02) | (0.02) | (0.01) | | | |
| × waiting list patients on dialysis | | | | | | | 0.027*** | 0.021*** | 0.020*** |
| | | | | | | | (0.01) | (0.01) | (0.01) |
| Observations | 2,128,129 | 2,127,184 | 2,113,279 | 2,128,129 | 2,127,184 | 2,113,279 | 2,128,129 | 2,127,184 | 2,113,279 |
| Cell fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Month fixed effects | Yes | No | No | Yes | No | No | Yes | No | No |
| Country × month FEs | No | Yes | No | No | Yes | No | No | Yes | No |
| Country-state × month FEs | No | No | Yes | No | No | Yes | No | No | Yes |
| Mean events12 transplant cells | 0.30 | 0.30 | 0.30 | 0.30 | 0.30 | 0.30 | 0.30 | 0.30 | 0.30 |
| R-squared | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.01 | 0.01 | 0.01 |

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

**Table 2.D3:** The impact of organ demand on a group's conflict probability over the next 12 months

This table reports OLS coefficients of a linear probability model regressing a group's binary conflict variable on the interaction between transplant infrastructure and kidney demand (see Equation (2.2)). The sample consists of monthly observations of 708 non-state armed groups between 2010 and 2021. The dependent variable *Conflict dummy* is a binary variable indicating if the group was involved in a conflict from month $t$ to month $t+11$. Independent variables are the binary variable *Transplant center at home region*, indicating the existence of an authorized transplant center in the group's home region, and the standardized number of (i) patients on the US waiting list for a kidney, (ii) patients on the US waiting list for a kidney who had labor income when joining the waiting list, and (iii) patients on the US waiting list for a kidney on dialysis. Models (1), (3), and (5) include group and month fixed effects, models (2), (4), and (6) include group and country×month fixed effects. Standard errors are two-way clustered by group and month and shown in parentheses.

| | Dependent variable: Group's probability of conflict (in basis points) | | | | | |
|---|---|---|---|---|---|---|
| Transplant center at home region | | | | | | |
| × waiting list patients | 111.48** | 119.86** | | | | |
| | (56.15) | (55.88) | | | | |
| × waiting list patients with income | | | 104.84 | 132.72* | | |
| | | | (74.25) | (69.30) | | |
| × waiting list patients on dialysis | | | | | 81.65 | 80.42 |
| | | | | | (61.68) | (61.25) |
| Observations | 95,580 | 95,580 | 95,580 | 95,580 | 95,580 | 95,580 |
| Group fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Month fixed effects | Yes | No | Yes | No | Yes | No |
| Country × month FEs | No | Yes | No | Yes | No | Yes |
| Base prob. transplant groups | 1394.24 | 1394.24 | 1394.24 | 1394.24 | 1394.24 | 1394.24 |
| R-squared | 0.19 | 0.21 | 0.19 | 0.21 | 0.19 | 0.21 |

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

**Table 2.D4:** The impact of organ demand on a group's number of conflict events over the next 12 months

This table reports OLS coefficients of regressing an armed group's number of attacks on the interaction between transplant infrastructure and kidney demand (see Equation (2.2)). The sample consists of monthly observations of 708 non-state armed groups between 2010 and 2021. The dependent variable is an armed group's log number of conflicts from month $t$ to month $t+11$. Independent variables are the binary variable *Transplant center at home region*, indicating the existence of an authorized transplant center in the group's home region, and the standardized number of (i) patients on the US waiting list for a kidney, (ii) patients on the US waiting list for a kidney who had labor income when joining the waiting list, and (iii) patients on the US waiting list for a kidney on dialysis. Models (1), (3), and (5) include group and month fixed effects, models (2), (4), and (6) include group and country×month fixed effects. Standard errors are two-way clustered by group and month and shown in parentheses.

| | Dependent variable: Group's conflict events | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Transplant center at home region | | | | | | |
| × waiting list patients | 0.018* | 0.019** | | | | |
| | (0.01) | (0.01) | | | | |
| × waiting list patients with income | | | 0.025* | 0.028** | | |
| | | | (0.01) | (0.01) | | |
| × waiting list patients on dialysis | | | | | 0.010 | 0.010 |
| | | | | | (0.01) | (0.01) |
| Observations | 95,569 | 95,569 | 95,569 | 95,569 | 95,569 | 95,569 |
| Group fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Month fixed effects | Yes | No | Yes | No | Yes | No |
| Country × month FEs | No | Yes | No | Yes | No | Yes |
| Mean log events transplant groups | 0.16 | 0.16 | 0.16 | 0.16 | 0.16 | 0.16 |
| R-squared | 0.34 | 0.36 | 0.34 | 0.36 | 0.34 | 0.35 |

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

**Table 2.D5:** The impact of organ demand on a group's conflict probability outside its home region over the next 12 months

This table reports OLS coefficients of a linear probability model regressing a group's binary conflict variable on the interaction between transplant infrastructure and kidney demand (see Equation (2.2)). The sample consists of monthly observations of 708 non-state armed groups between 2010 and 2021. The dependent variable *Conflict dummy* is a binary variable indicating if the group was involved in a conflict outside its home region from month *t* to month *t+11*. Independent variables are the binary variable *Transplant center at home region*, indicating the existence of an authorized transplant center in the group's home region, and the standardized number of (i) patients on the US waiting list for a kidney, (ii) patients on the US waiting list for a kidney who had labor income when joining the waiting list, and (iii) patients on the US waiting list for a kidney on dialysis. Models (1), (3), and (5) include group and month fixed effects, models (2), (4), and (6) include group and country×month fixed effects. Standard errors are two-way clustered by group and month and shown in parentheses.

| | Dependent variable: Group's probability of conflict outside home region (in basis points) | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Transplant center at home region | | | | | | |
| × waiting list patients | 55.874 | 61.652 | | | | |
| | (49.98) | (49.22) | | | | |
| × waiting list patients on dialysis | | | 45.365 | 43.387 | | |
| | | | (48.47) | (47.37) | | |
| × waiting list patients with income | | | | | 25.610 | 48.004 |
| | | | | | (71.69) | (68.99) |
| Observations | 95,580 | 95,580 | 95,580 | 95,580 | 95,580 | 95,580 |
| actor fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Month fixed effects | Yes | No | Yes | No | Yes | No |
| Country × month FEs | No | Yes | No | Yes | No | Yes |
| Base prob. transplant actors | 886.91 | 886.91 | 886.91 | 886.91 | 886.91 | 886.91 |
| R-squared | 0.22 | 0.23 | 0.22 | 0.23 | 0.22 | 0.23 |

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

**Table 2.D6:** The impact of organ demand on a group's number of conflict events outside its home region over the next 12 months

This table reports OLS coefficients of regressing an armed group's number of attacks outside its home region on the interaction between transplant infrastructure and kidney demand (see Equation (2.2)). The sample consists of monthly observations of 708 non-state armed groups between 2010 and 2021. The dependent variable *Conflict events outside home region* is the log number of conflicts outside a group's home region from month $t$ to month $t+11$. Independent variables are the binary variable *Transplant center at home region*, indicating the existence of an authorized transplant center in the group's home region, and the standardized number of (i) patients on the US waiting list for a kidney, (ii) patients on the US waiting list for a kidney who had labor income when joining the waiting list, and (iii) patients on the US waiting list for a kidney on dialysis. Models (1), (3), and (5) include group and month fixed effects, models (2), (4), and (6) include group and country×month fixed effects. Standard errors are two-way clustered by group and month and shown in parentheses.

| | Dependent variable: Conflict events outside home region | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Transplant center at home region | | | | | | |
| × waiting list patients | 0.014* | 0.015* | | | | |
| | (0.01) | (0.01) | | | | |
| × waiting list patients on dialysis | | | 0.008 | 0.008 | | |
| | | | (0.01) | (0.01) | | |
| × waiting list patients with income | | | | | 0.018 | 0.021 |
| | | | | | (0.01) | (0.01) |
| Observations | 95,569 | 95,569 | 95,569 | 95,569 | 95,569 | 95,569 |
| actor fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Month fixed effects | Yes | No | Yes | No | Yes | No |
| Country × month FEs | No | Yes | No | Yes | No | Yes |
| Mean events12 transplant actors | 0.11 | 0.11 | 0.11 | 0.11 | 0.11 | 0.11 |
| R-squared | 0.39 | 0.40 | 0.39 | 0.40 | 0.39 | 0.40 |

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

## 2.E Conflict and kidney demand, year-level analysis

To account for the possibility that payments are made some month before or after the transplant and that armed groups delay their attacks after the inflow from a transplant tourist operation, Table 2E.1 to Table 2E.6 show all my analyses on a cell-year level: The $Conflict\ dummy_{it}$ indicates if a conflict took place in cell $i$ in year $t$, $Conflict\ events_{it}$ are summed up over year $t$ for each cell. Conflict variables are regressed on the interaction of transplant infrastructure and beginning-of-the-year kidney demand. All other variables are as defined in Section 2.3 and in Appendix 2B. The regression equations are specified in Section 2.4 and Section 2.5.

**Table 2.E1:** The impact of organ demand on conflict probability (yearly panel)

This table reports coefficients of a linear probability model regressing a local binary conflict variable on the interaction between transplant infrastructure and kidney demand (see Equation (2.1)). Conley's (1999) standard errors, allowing for spatial correlation within a 500 km radius and for infinite serial correlation, are shown in parenthesis. The sample consists of yearly observations of 15,876 cells of 0.5° latitude × 0.5° longitude, covering 5 countries between 2010 and 2021. The dependent variable *Conflict dummy* is a binary variable indicating if a conflict took place in a given year. Independent variables are the binary variable *Transplant center*, indicating the existence of an authorized transplant center in an 0.5° latitude × 0.5° longitude cell, and the standardized number of (i) patients on the US waiting list for a kidney, (ii) patients on the US waiting list for a kidney who had labor income when joining the waiting list, and (iii) patients on the US waiting list for a kidney on dialysis in the beginning of the year. Models (1), (4), and (7) include cell and year fixed effects, models (2), (5), and (8) include cell and country × year fixed effects, models (3), (6), and (9) include cell and country-state × year fixed effects.

| | Dependent variable: Probability of conflict (in basis points) | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Transplant center | | | | | | | | | |
| × waiting list patients | 464.02*** | 307.27*** | 251.82*** | | | | | | |
| | (76.95) | (55.86) | (43.64) | | | | | | |
| × waiting list patients with income | | | | 934.12*** | 652.99*** | 562.79*** | | | |
| | | | | (102.18) | (83.31) | (64.75) | | | |
| × waiting list patients on dialysis | | | | | | | 157.98** | 92.47* | 62.81 |
| | | | | | | | (74.37) | (52.67) | (44.28) |
| Observations | 189,168 | 189,084 | 187,848 | 189,168 | 189,084 | 187,848 | 189,168 | 189,084 | 187,848 |
| Cell fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year fixed effects | Yes | No | No | Yes | No | No | Yes | No | No |
| Country × year FEs | No | Yes | No | No | Yes | No | No | Yes | No |
| Country-state × year FEs | No | No | Yes | No | No | Yes | No | No | Yes |
| Base prob. transplant cells | 1678.57 | 1678.57 | 1678.57 | 1678.57 | 1678.57 | 1678.57 | 1678.57 | 1678.57 | 1678.57 |
| R-squared | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.01 | 0.01 | 0.01 |

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

**Table 2.E2:** The impact of organ demand on the number of conflict events (yearly panel)

This table reports coefficients of a linear regression of the number of conflict events on the interaction between transplant infrastructure and kidney demand (see Equation (2.1)). Conley's (1999) standard errors, allowing for spatial correlation within a 500 km radius and for infinite serial correlation, are shown in parenthesis. The sample consists of yearly observations of 15,876 cells of 0.5° latitude × 0.5° longitude, covering 5 countries between 2010 and 2021. The dependent variable *Conflict events* is the log number of conflict events that took place in a cell in a given year. Independent variables are the binary variable *Transplant center*, indicating the existence of an authorized transplant center in an 0.5° latitude × 0.5° longitude cell, and the standardized number of (i) patients on the US waiting list for a kidney, (ii) patients on the US waiting list for a kidney who had labor income when joining the waiting list, and (iii) patients on the US waiting list for a kidney on dialysis in the beginning of the year. Models (1), (4), and (7) include cell and month fixed effects, models (2), (5), and (8) include cell and country×month fixed effects, models (3), (6), and (9) include cell and country-state×month fixed effects.

| | Dependent variable: Conflict events | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Transplant center | | | | | | | | | |
| × waiting list patients | 0.072*** | 0.054*** | 0.048*** | | | | | | |
| | (0.01) | (0.01) | (0.01) | | | | | | |
| × waiting list patients with income | | | | 0.117*** | 0.086*** | 0.076*** | | | |
| | | | | (0.02) | (0.02) | (0.02) | | | |
| × waiting list patients on dialysis | | | | | | | 0.040*** | 0.031*** | 0.029*** |
| | | | | | | | (0.01) | (0.01) | (0.01) |
| Observations | 189,168 | 189,084 | 187,848 | 189,168 | 189,084 | 187,848 | 189,168 | 189,084 | 187,848 |
| Cell fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year fixed effects | Yes | No | No | Yes | No | No | Yes | No | No |
| Country × year FEs | No | Yes | No | No | Yes | No | No | Yes | No |
| Country-state × year FEs | No | No | Yes | No | No | Yes | No | No | Yes |
| Mean events transplant cells | 0.29 | 0.29 | 0.29 | 0.29 | 0.29 | 0.29 | 0.29 | 0.29 | 0.29 |
| R-squared | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.01 | 0.01 | 0.01 |

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

**Table 2.E3:** The impact of organ demand on a group's conflict probability (yearly panel)

This table reports OLS coefficients of a linear probability model regressing a group's binary conflict variable on the interaction between transplant infrastructure and kidney demand (see Equation (2.2)). The sample consists of yearly observations of 708 non-state armed groups between 2010 and 2021. The dependent variable *Conflict dummy* is a binary variable indicating if the group was involved in a conflict in a given year. Independent variables are the binary variable *Transplant center at home region*, indicating the existence of an authorized transplant center in the group's home region, and the standardized number of (i) patients on the US waiting list for a kidney, (ii) patients on the US waiting list for a kidney who had labor income when joining the waiting list, and (iii) patients on the US waiting list for a kidney on dialysis in the beginning of the year. Models (1), (3), and (5) include group and year fixed effects, models (2), (4), and (6) include group and country × year fixed effects. Standard errors are two-way clustered by group and year and shown in parentheses.

| | Dependent variable: Group's probability of conflict (in basis points) | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Transplant center at home region | | | | | | |
| × waiting list patients | 132.84 | 138.20 | | | | |
| | (89.49) | (94.49) | | | | |
| × waiting list patients with income | | | 128.68* | 154.47** | | |
| | | | (61.81) | (55.67) | | |
| × waiting list patients on dialysis | | | | | 96.20 | 92.36 |
| | | | | | (112.54) | (117.36) |
| Observations | 8,496 | 8,496 | 8,496 | 8,496 | 8,496 | 8,496 |
| Group fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Year fixed effects | Yes | No | Yes | No | Yes | No |
| Country × year FEs | No | Yes | No | Yes | No | Yes |
| Base prob. transplant groups | 1388.89 | 1388.89 | 1388.89 | 1388.89 | 1388.89 | 1388.89 |
| R-squared | 0.20 | 0.22 | 0.20 | 0.22 | 0.20 | 0.22 |

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

**Table 2.E4:** The impact of organ demand on a group's number of conflict events (yearly panel)

This table reports OLS coefficients of regressing an armed group's number of attacks on the interaction between transplant infrastructure and kidney demand (see Equation (2.2)). The sample consists of yearly observations of 708 non-state armed groups between 2010 and 2021. The dependent variable is an armed group's log number of conflicts. Independent variables are the binary variable *Transplant center at home region*, indicating the existence of an authorized transplant center in the group's home region, and the standardized number of (i) patients on the US waiting list for a kidney, (ii) patients on the US waiting list for a kidney who had labor income when joining the waiting list, and (iii) patients on the US waiting list for a kidney on dialysis in the beginning of the year. Models (1), (3), and (5) include group and year fixed effects, models (2), (4), and (6) include group and country × year fixed effects. Standard errors are two-way clustered by group and year and shown in parentheses.

| | Dependent variable: Group's conflict events | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Transplant center at home region | | | | | | |
| × waiting list waiting list patients | 0.020 | 0.020 | | | | |
| | (0.01) | (0.01) | | | | |
| × waiting list patients with income | | | 0.025 | 0.028* | | |
| | | | (0.01) | (0.01) | | |
| × waiting list patients on dialysis | | | | | 0.013 | 0.013 |
| | | | | | (0.01) | (0.02) |
| Observations | 8,496 | 8,496 | 8,496 | 8,496 | 8,496 | 8,496 |
| Group fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Year fixed effects | Yes | No | Yes | No | Yes | No |
| Country × year FEs | No | Yes | No | Yes | No | Yes |
| Mean log events transplant groups | 0.15 | 0.15 | 0.15 | 0.15 | 0.15 | 0.15 |
| R-squared | 0.34 | 0.35 | 0.34 | 0.35 | 0.34 | 0.35 |

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

**Table 2.E5:** The impact of organ demand on a group's conflict probability outside its home region (yearly panel)

This table reports OLS coefficients of a linear probability model regressing a group's binary conflict variable on the interaction between transplant infrastructure and kidney demand (see Equation (2.2)). The sample consists of yearly observations of 708 non-state armed groups between 2010 and 2021. The dependent variable *Conflict dummy* is a binary variable indicating if the group was involved in a conflict outside its home region in a given year. Independent variables are the binary variable *Transplant center at home region*, indicating the existence of an authorized transplant center in the group's home region, and the standardized number of (i) patients on the US waiting list for a kidney, (ii) patients on the US waiting list for a kidney who had labor income when joining the waiting list, and (iii) patients on the US waiting list for a kidney on dialysis in the beginning of the year. Models (1), (3), and (5) include group and year fixed effects, models (2), (4), and (6) include group and country × year fixed effects. Standard errors are two-way clustered by group and year and shown in parentheses.

| | Dependent variable: Group's probability of conflict outside home region (in basis points) | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Transplant center at home region | | | | | | |
| × waiting list patients | 0.020 | 0.020 | | | | |
| | (0.01) | (0.01) | | | | |
| × waiting list patients with income | | | 0.025 | 0.028* | | |
| | | | (0.01) | (0.01) | | |
| × waiting list patients on dialysis | | | | | 0.013 | 0.013 |
| | | | | | (0.01) | (0.02) |
| Observations | 8,496 | 8,496 | 8,496 | 8,496 | 8,496 | 8,496 |
| Group fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Year fixed effects | Yes | No | Yes | No | Yes | No |
| Country × year FEs | No | Yes | No | Yes | No | Yes |
| Mean log events transplant groups | 0.15 | 0.15 | 0.15 | 0.15 | 0.15 | 0.15 |
| R-squared | 0.34 | 0.35 | 0.34 | 0.35 | 0.34 | 0.35 |

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

**Table 2.E6:** The impact of organ demand on a group's number of conflict events outside its home region (yearly panel)

This table reports OLS coefficients of regressing an armed group's number of attacks outside its home region on the interaction between transplant infrastructure and kidney demand (see Equation (2.2)). The sample consists of yearly observations of 708 non-state armed groups between 2010 and 2021. The dependent variable *Conflict events outside home region* is the log number of conflicts outside a group's home region. Independent variables are the binary variable *Transplant center at home region*, indicating the existence of an authorized transplant center in the group's home region, and the standardized number of (i) patients on the US waiting list for a kidney, (ii) patients on the US waiting list for a kidney who had labor income when joining the waiting list, and (iii) patients on the US waiting list for a kidney on dialysis. Models (1), (3), and (5) include group and year fixed effects, models (2), (4), and (6) include group and country × year fixed effects. Standard errors are two-way clustered by group and year and shown in parentheses.

| | Dependent variable: Conflict events outside home region | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Transplant center at home region | | | | | | |
| × waiting list patients | 0.016 | 0.016 | | | | |
| | (0.01) | (0.01) | | | | |
| × waiting list patients with income | | | 0.019 | 0.022 | | |
| | | | (0.01) | (0.01) | | |
| × waiting list patients on dialysis | | | | | 0.010 | 0.010 |
| | | | | | (0.01) | (0.01) |
| Observations | 8,496 | 8,496 | 8,496 | 8,496 | 8,496 | 8,496 |
| Group fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Year fixed effects | Yes | No | Yes | No | Yes | No |
| Country × year FEs | No | Yes | No | Yes | No | Yes |
| Mean log events transplant groups | 0.11 | 0.11 | 0.11 | 0.11 | 0.11 | 0.11 |
| R-squared | 0.38 | 0.38 | 0.38 | 0.39 | 0.38 | 0.38 |

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

## 2.F Conflict probability and kidney demand, nonlinear estimators

Table 2F.1 and Table 2F.2 report the results of regressing a local binary conflict variable on the interaction between transplant infrastructure and kidney demand using a conditional logit model (Table 2F.1) and a Poisson pseudo-maximum-likelihood model (Table 2F.2).

**Table 2.F1:** The impact of organ demand on conflict probability (Logit regression)

This table reports coefficients of a conditional logit model regressing a local binary conflict variable on the interaction between transplant infrastructure and kidney demand (see Equation (2.1)). The sample consists of monthly observations of 15,876 cells of 0.5° latitude × 0.5° longitude, covering 5 countries between 2010 and 2021. The dependent variable *Conflict dummy* is a binary variable indicating if a conflict took place in a cell in a given month. Independent variables are the binary variable *Transplant center*, indicating the existence of an authorized transplant center in an 0.5° latitude × 0.5° longitude cell, and the standardized number of (i) patients on the US waiting list for a kidney, (ii) patients on the US waiting list for a kidney who had labor income when joining the waiting list, and (iii) patients on the US waiting list for a kidney on dialysis. All models include cell and month fixed effects. Standard errors are reported in parenthesis.

| | Dependent variable: Conflict dummy | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Transplant center | | | |
| × waiting list patients | 0.167*** | | |
| | (0.05) | | |
| × waiting list patients with income | | 0.499*** | |
| | | (0.18) | |
| × waiting list patients on dialysis | | | -0.007 |
| | | | (0.06) |
| Observations | 140,670 | 140,670 | 140,670 |
| Cell fixed effects | Yes | Yes | Yes |
| Month fixed effects | Yes | Yes | Yes |

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

**Table 2.F2:** The impact of organ demand on conflict probability (Poisson pseudo-maximum-likelihood regression)

This table reports coefficients of a Poisson psuedo-maximum likelihood model regressing a local binary conflict variable on the interaction between transplant infrastructure and kidney demand (see Equation (2.1)). The sample consists of monthly observations of 15,876 cells of 0.5° latitude × 0.5° longitude, covering 5 countries between 2010 and 2021. The dependent variable *Conflict dummy* is a binary variable indicating if a conflict took place in a cell in a given month. Independent variables are the binary variable *Transplant center*, indicating the existence of an authorized transplant center in an 0.5° latitude × 0.5° longitude cell, and the standardized number of (i) patients on the US waiting list for a kidney, (ii) patients on the US waiting list for a kidney who had labor income when joining the waiting list, and (iii) patients on the US waiting list for a kidney on dialysis. All models include cell and month fixed effects. Standard errors are reported in parenthesis.

| | Dependent variable: Conflict dummy | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Transplant center | | | |
| × waiting list patients | 0.048 | | |
| | (0.06) | | |
| × waiting list patients with income | | 0.133 | |
| | | (0.14) | |
| × waiting list patients on dialysis | | | -0.005 |
| | | | (0.04) |
| Observations | 140,670 | 140,670 | 140,670 |
| Cell fixed effects | Yes | Yes | Yes |
| Month fixed effects | Yes | Yes | Yes |
| Base prob. transplant cells | 0.06 | 0.06 | 0.06 |

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

## 2.G Conflict probability and events with penalized maximum likelihood fixed-effects estimator

Cook, Hays, and Franzese (2020) raise the concern that marginal effects can be biased in fixed effects models of rare events data. To address this concern, I re-run my analyses using the penalized maximum likelihood fixed effects estimator suggested by Cook, Hays, and Franzese (2020) in this Appendix. I do not use Cook, Hays, and Franzese (2020)'s estimator for my main specification as it does not allow for the extensive correction for spatial and serial clustering applied in my main analyses. Due to computational limitation of the available server, I only present the results for India, the country with most conflict events and transplant centers. Estimations for the other countries are available upon request.

**Table 2.G1:** The impact of organ demand on conflict probability: Penalized maximum likelihood fixed effects estimator for India

This table reports coefficients of regressing a local binary conflict variable on the interaction between transplant infrastructure and kidney demand (see Equation (2.1)) using Cook, Hays, and Franzese (2020)'s penalized maximum likelihood fixed effects estimator. The sample consists of monthly observations of 1,175 cells of 0.5° latitude × 0.5° longitude in India between 2010 and 2021. The dependent variable *Conflict dummy* is a binary variable indicating if a conflict took place in a cell in a given month. Independent variables are the binary variable *Transplant center*, indicating the existence of an authorized transplant center in an 0.5° latitude × 0.5° longitude cell, and the standardized number of (i) patients on the US waiting list for a kidney, (ii) patients on the US waiting list for a kidney who had labor income when joining the waiting list, and (iii) patients on the US waiting list for a kidney on dialysis. All models include cell fixed effects, models (2), (4), and (6) include month fixed effects, in addition. Standard errors are reported in parenthesis.

| | Dependent variable: Conflict dummy | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Transplant center | | | |
| × waiting list patients | 0.405*** | | |
| | (0.049) | | |
| × waiting list patients with income | | 2.397*** | |
| | | (0.121) | |
| × waiting list patients on dialysis | | | -0.071* |
| | | | (0.043) |
| Observations | 158,625 | 158,625 | 158,625 |
| Cell fixed effects | Yes | Yes | Yes |

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

## 2.H Placebotests

This appendix presents placebo tests for the analyses of Section 2.4. The tests aim to rule out that the results presented in Table 2.2 and Table 2.3, i.e., that higher kidney demand causes more conflicts in localities with a transplant center compared to those without a transplant center, are the result of the distinct conflict trajectories of densely populated and sparsely populated regions. To rule out that a spurious correlation between this difference and kidney demand drives my results, I substitute the variable *Transplant Center* with the variable *High nightlight*, a proxy for population density. I run the following regression.

$$\begin{aligned} Conflict_{it} =& \beta_0 + \beta_1 High\ nightlight_i \times Kidney\ demand_t + \\ & FE_i + FE_{ct} + \epsilon_{it} \end{aligned} \tag{2.H1}$$

*High nightlight* assumes the value of one for any 0.5° latitude × 0.5° longitude cell with a nightlight higher than the 97th percentile and zero for all other cells. I chose the 97th percentile to obtain a similar fraction of cells with *High nightlight* like the fraction given by cells with a *Transplant center*. I take data on nightlights from the Earth Observation Group's satellites report and clean the data following Elvidge et al. (2021).

Table 2H.1 and 3H.2 report the results. Cells with *High nightlight* do neither experience a higher conflict probability (Table 2H.1) nor a higher number of conflict events when the number of *Waiting list patients*, *Waiting list patients with income*, or *Waiting list patients on dialysis* increases. It is therefore unlikely that the different conflict trajectories of densely and sparsely populated regions drive my results.

**Table 2.H1:** The impact of organ demand on conflict probability of densely populated regions (Placebotest)

This table reports coefficients of a linear probability model regressing a local binary conflict variable on the interaction between *High nightlight*, a proxy for densely populated regions, and kidney demand (see Equation (2H.1)). Conley's (1999) standard errors, allowing for spatial correlation within a 500 km radius and for infinite serial correlation, are shown in parenthesis. The sample consists of monthly observations of 15,876 cells of 0.5° latitude × 0.5° longitude, covering 5 countries between 2010 and 2021. The dependent variable *Conflict dummy* is a binary variable indicating if a conflict took place in a cell in a given month. Independent variables are the binary variable *High nighlight*, indicating if the nightlight in an 0.5° latitude × 0.5° longitude cell is stronger than the 97th percentile, and the standardized number of (i) patients on the US waiting list for a kidney, (ii) patients on the US waiting list for a kidney who had labor income when joining the waiting list, and (iii) patients on the US waiting list for a kidney on dialysis. Models (1), (4), and (7) include cell and month fixed effects, models (2), (5), and (8) include cell and country×month fixed effects, models (3), (6), and (9) include cell and country-state×month fixed effects.

| | Dependent variable: Probability of conflict (in basis points) | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| High nightlight | | | | | | | | | |
| × waiting list patients | -4.04 | -1.90 | -0.45 | | | | | | |
| | (3.78) | (3.85) | (4.47) | | | | | | |
| × waiting list patients with income | | | | -15.19*** | -6.80 | -2.43 | | | |
| | | | | (5.30) | (5.65) | (6.30) | | | |
| × waiting list patients on dialysis | | | | | | | 1.98 | 0.47 | -0.07 |
| | | | | | | | (3.05) | (2.80) | (3.18) |
| Observations | 2,128,140 | 2,127,195 | 2,113,290 | 2,128,140 | 2,127,195 | 2,113,290 | 2,128,140 | 2,127,195 | 2,113,290 |
| Cell fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Month fixed effects | Yes | No | No | Yes | No | No | Yes | No | No |
| Country × month FEs | No | Yes | No | No | Yes | No | No | Yes | No |
| Country-state × month FEs | No | No | Yes | No | No | Yes | No | No | Yes |
| Base prob. cells with high nightlight | 52.47 | 52.47 | 52.47 | 52.47 | 52.47 | 52.47 | 52.47 | 52.47 | 52.47 |
| R-squared | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |

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

**Table 2.H2:** The impact of organ demand on the number of conflict events in densely populated regions (Placebotest)

This table reports coefficients of a linear regression of the number of local conflict events on the interaction between *High nightlight*, a proxy for densely populated regions, and kidney demand (see Equation (2H.1)). Conley's (1999) standard errors, allowing for spatial correlation within a 500 km radius and for infinite serial correlation, are shown in parenthesis. The sample consists of monthly observations of 15,876 cells of 0.5° latitude × 0.5° longitude, covering 5 countries between 2010 and 2021. The dependent variable *Conflict events* is the log number of conflict events that took place in a cell in a given month. Independent variables are the binary variable *High nighlight*, indicating if the nightlight in an 0.5° latitude × 0.5° longitude cell is stronger than the 97th percentile, and the standardized number of (i) patients on the US waiting list for a kidney, (ii) patients on the US waiting list for a kidney who had labor income when joining the waiting list, and (iii) patients on the US waiting list for a kidney on dialysis. Models (1), (4), and (7) include cell and month fixed effects, models (2), (5), and (8) include cell and country×month fixed effects, models (3), (6), and (9) include cell and country-state×month fixed effects.

| | Dependent variable: Conflict events | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Hight nightlight | | | | | | | | | |
| × waiting list patients | 0.000 | 0.000 | 0.001 | | | | | | |
| | (0.00) | (0.00) | (0.00) | | | | | | |
| × waiting list patients with income | | | | -0.001** | -0.000 | 0.000 | | | |
| | | | | (0.00) | (0.00) | (0.00) | | | |
| × waiting list patients on dialysis | | | | | | | 0.001 | 0.001 | 0.001 |
| | | | | | | | (0.00) | (0.00) | (0.00) |
| Observations | 2,128,140 | 2,127,195 | 2,113,290 | 2,128,140 | 2,127,195 | 2,113,290 | 2,128,140 | 2,127,195 | 2,113,290 |
| Cell fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Month fixed effects | Yes | No | No | Yes | No | No | Yes | No | No |
| Country × month FEs | No | Yes | No | No | Yes | No | No | Yes | No |
| Country-state × month FEs | No | No | Yes | No | No | Yes | No | No | Yes |
| Mean events transplant cells | 0.01 | 0.01 | 0.01 | 0.01 | 0.01 | 0.01 | 0.01 | 0.01 | 0.01 |
| R-squared | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |

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

## 2.I Sample of non-state armed groups and their home region

Table 2I.1 list all non-state armed groups of my sample for which the home region could be determined. Group names are from the Armed Conflict Location & Event Data Project (ACLED). A group's home region is defined as the cell in which (i) the group has its headquarter, or (ii) the group was founded, or (iii) the ethnic affiliation of the group is based, or (iv) the community mentioned in the group's name is based. I use Wikipedia and other online sources to determine these locations.

**Table 2.I1:** Sample of non-state violent groups and their home region

This table reports my sample of non-state violent groups and their home region. Group names are from the Armed Conflict Location & Event Data Project (ACLED). A group's home region is defined as the cell in which (i) the group has its headquarter, or (ii) the group was founded, or (iii) the ethnic affiliation of the group is based, or (iv) the community mentioned in the group's name is based. I use Wikipedia and other online sources to determine these locations.

| **Actor** | **Home Region** | |
|---|---|---|
| | Latitude (rounded to half degree) | Longitude (rounded to half degree) |
| AAP: Aam Aadmi Party | 29 | 77 |
| Ababaki Communal Militia (Pakistan) | 30 | 67 |
| Ababeel Group | 33 | 76 |
| Abbas Nagar Communal Militia (Pakistan) | 32 | 73 |
| Abbottabad Communal Militia (Pakistan) | 34 | 73 |
| Abdul Ghafoor Communal Militia (Pakistan) | 25 | 67 |
| ABMSM: Abahlali Basemjondolo Shack Dwellers Movement | -34 | 19 |
| Abran Communal Militia (Pakistan) | 34 | 77 |
| ABVP: Akhil Bharatiya Vidyarthi Parishad | 19 | 73 |
| Adamzai Communal Militia (Pakistan) | 33 | 71 |
| Adezai Communal Militia (Pakistan) | 34 | 72 |
| Agang South Africa Party | -26 | 28 |
| Agwanpur Communal Militia (India) | 29 | 78 |
| Ahmedabad Communal Militia (India) | 23 | 73 |
| AIADMK: All India Anna Dravida Munnetra Kazhagam | 13 | 81 |
| Ajnala Communal Militia (India) | 33 | 74 |
| Akbarpura Communal Militia (Pakistan) | 32 | 75 |
| Akhnoor Communal Militia (India) | 33 | 75 |
| Akhorwal Tribal Militia (Pakistan) | 34 | 72 |
| Al-Badr | 35 | 73 |
| Aligarh Communal Militia (India) | 28 | 78 |
| All Jammu and Kashmir Muslim Conference | 35 | 74 |
| Alupur Communal Militia (India) | 28 | 77 |
| Aman Kot Communal Militia (Pakistan) | 34 | 72 |
| Aman Lashkar | 32 | 75 |
| Aman Nagar Communal Militia (India) | 24 | 70 |
| Amarkot Communal Militia (India) | 31 | 75 |
| Ambernath Communal Militia (India) | 19 | 73 |
| AMMK: Amma Makkal Munnetra Kazhagam | 13 | 81 |
| Anandapur Communal Militia (India) | 22 | 86 |
| Anantapur Communal Militia (India) | 15 | 78 |
| ANC: African National Congress | -29 | 26 |
| ANC-Motlanthe: African National Congress (Motlanthe Faction) | -29 | 26 |
| ANCYL: African National Congress Youth League | -29 | 26 |
| ANC-Zuma: African National Congress (Zuma Faction) | -26 | 28 |
| Angul Communal Militia (India) | 21 | 85 |
| ANLA: Achik National Liberation Army | 26 | 92 |
| Anoop Nagar Communal Militia (India) | 29 | 77 |
| ANP: Awami National Party | 34 | 73 |
| Antah Communal Militia (India) | 25 | 77 |
| Arain Communal Militia (Pakistan) | 31 | 76 |
| Areraj Communal Militia (India) | 27 | 85 |
| Arifwala Communal Militia (Pakistan) | 31 | 73 |
| Arnia Communal Militia (India) | 33 | 75 |
| ASS: Anjuman-e-Sipah-i-Sahaba | 34 | 73 |
| Atalgarh Communal Militia (Pakistan) | 31 | 77 |
| Athal Communal Militia (Pakistan) | 31 | 79 |
| Athwal Communal Militia (India) | 32 | 76 |
| Aurangzeb Butt Communal Militia (Pakistan) | 32 | 75 |
| Azadpur Mandi Communal Militia (India) | 32 | 77 |

| Actor | Home Region | |
|---|---|---|
| | Latitude (rounded to half degree) | Longitude (rounded to half degree) |
| Baba Goth Communal Militia (Pakistan) | 25 | 67 |
| Babanian Communal Militia (India) | 33 | 74 |
| Badaber Communal Militia (Pakistan) | 34 | 72 |
| Badbher Communal Militia (Pakistan) | 34 | 72 |
| Baddi Communal Militia (India) | 31 | 77 |
| Badli Communal Militia (India) | 16 | 75 |
| Badopal Communal Militia (India) | 30 | 74 |
| Bagrani Communal Militia (Pakistan) | 26 | 69 |
| Bagri Communal Militia (Pakistan) | 26 | 74 |
| Bahawalpur Communal Militia (Pakistan) | 30 | 72 |
| Bahmna Communal Militia (India) | 30 | 76 |
| Bajaur Communal Militia (Pakistan) | 35 | 72 |
| Bajaur Tribal Militia (Pakistan) | 35 | 72 |
| Bajrang Dal | 29 | 77 |
| Bakhshapur Communal Militia (Pakistan) | 29 | 70 |
| Bakshi Nagar Communal Militia (India) | 29 | 78 |
| Balaji Communal Militia (India) | 12 | 76 |
| Balasore Communal Militia (India) | 22 | 87 |
| Balluana Communal Militia (India) | 30 | 75 |
| Balraj Nagar Communal Militia (India) | 29 | 77 |
| Bambiha Communal Militia (India) | 30 | 75 |
| Bangarpet Communal Militia (India) | 13 | 78 |
| Bangial Communal Militia (Pakistan) | 33 | 74 |
| Bangulzai Communal Militia (Pakistan) | 29 | 68 |
| Bangwar Communal Militia (Pakistan) | 33 | 76 |
| Bannu Communal Militia (Pakistan) | 33 | 71 |
| Baradari Communal Militia (India) | 33 | 74 |
| Barara Communal Militia (India) | 30 | 77 |
| Barawal Communal Militia (Pakistan) | 25 | 73 |
| Barhalganj Communal Militia (India) | 27 | 84 |
| Baruajhar Communal Militia (India) | 27 | 92 |
| Bavla Communal Militia (India) | 28 | 75 |
| Begusarai Communal Militia (India) | 26 | 86 |
| Beharwal Communal Militia (India) | 32 | 75 |
| Bengaluru Communal Militia (India) | 13 | 78 |
| Besant Nagar Communal Militia (India) | 13 | 81 |
| Betma Communal Militia (India) | 23 | 76 |
| BGRD: Bhartiya Gau Raksha Dal | 29 | 77 |
| Bhadaur Communal Militia (India) | 31 | 76 |
| Bhag Communal Militia (Pakistan) | 29 | 68 |
| Bhagat Communal Militia (Pakistan) | 33 | 74 |
| Bhaggupur Uttar Communal Militia (India) | 32 | 75 |
| Bhagwantpura Communal Militia (India) | 26 | 75 |
| Bhakkar Communal Militia (Pakistan) | 32 | 71 |
| Bhakna Khurd Communal Militia (India) | 32 | 75 |
| Bhalwal Communal Militia (India) | 33 | 73 |
| Bhambayi Communal Militia (South Africa) | -30 | 31 |
| Bhan Communal Militia (Pakistan) | 27 | 68 |
| Bhana Mari Communal Militia (Pakistan) | 34 | 72 |
| Bhanada Communal Militia (India) | 23 | 69 |
| Bhangar Communal Militia (India) | 31 | 75 |
| Bharatpur Communal Militia (India) | 28 | 78 |
| Bhatti Communal Militia (Pakistan) | 28 | 68 |
| Bhayo Communal Militia (Pakistan) | 28 | 69 |
| Bhilgawan Communal Militia (India) | 27 | 78 |
| Bhurgari Communal Militia (Pakistan) | 25 | 69 |
| Bhut Ethnic Militia (Pakistan) | 31 | 78 |
| Bhutto Communal Militia (Pakistan) | 28 | 69 |
| Bibiwala Communal Militia (India) | 30 | 75 |
| Bichaula Communal Militia (India) | 28 | 79 |
| Bijarani Communal Militia (Pakistan) | 28 | 69 |

| | | |
|---|---|---|
| Bijarani Tribal Militia (Pakistan) | 28 | 69 |
| Bijnor Communal Militia (India) | 30 | 79 |
| Bikkavolu Communal Militia (India) | 17 | 82 |
| Bin Qasim Communal Militia (Pakistan) | 25 | 67 |
| Bindapur Communal Militia (India) | 29 | 77 |
| Binjhol Communal Militia (India) | 30 | 77 |
| Bishnah Communal Militia (India) | 33 | 75 |
| Bizana Communal Militia (South Africa) | -31 | 30 |
| BJD: Biju Janata Dal | 21 | 86 |
| BJP: Bharatiya Janata Party | 29 | 77 |
| BJYM: Bharatiya Janata Yuva Morcha | 29 | 77 |
| BLA: Baloch Liberation Army | 32 | 66 |
| Bori Kharak Communal Militia (Pakistan) | 33 | 71 |
| Borivali Communal Militia (India) | 19 | 73 |
| Brahmpura Communal Militia (India) | 25 | 75 |
| Brohi Communal Militia (Pakistan) | 26 | 70 |
| BSP: Bahujan Samaj Party | 29 | 77 |
| Bugti Communal Militia (Pakistan) | 29 | 69 |
| Bundi Communal Militia (India) | 26 | 76 |
| Buner Communal Militia (Pakistan) | 32 | 77 |
| Buriro Communal Militia (Pakistan) | 28 | 69 |
| Bushbuckridge Communal Militia (South Africa) | -25 | 31 |
| Central Kurram Communal Militia (Pakistan) | 33 | 71 |
| Chabba Communal Militia (India) | 32 | 75 |
| Chachar Communal Militia (Pakistan) | 28 | 69 |
| Chak 241-GB Communal Militia (Pakistan) | 31 | 73 |
| Chak Communal Militia (Pakistan) | 28 | 69 |
| Chak Hakim Communal Militia (India) | 33 | 75 |
| Chak Seven Hundred Fifty-seven Gugera Branch Communal Militia (Pakistan) | 31 | 74 |
| Chakdara Communal Militia (Pakistan) | 35 | 72 |
| Chakri Communal Militia (Pakistan) | 32 | 75 |
| Challar Communal Militia (Pakistan) | 25 | 70 |
| Chaman Communal Militia (Pakistan) | 31 | 67 |
| Chamiari Communal Militia (India) | 34 | 73 |
| Chamkani Communal Militia (Pakistan) | 34 | 72 |
| Chandigarh Communal Militia (India) | 31 | 77 |
| Chandio Communal Militia (Pakistan) | 25 | 67 |
| Chandpur Communal Militia (India) | 29 | 79 |
| Chapri Communal Militia (Pakistan) | 34 | 75 |
| Charsadda Communal Militia (Pakistan) | 34 | 72 |
| Charwazgai Communal Militia (Pakistan) | 34 | 71 |
| Chattar Communal Militia (Pakistan) | 33 | 75 |
| Cheeka Communal Militia (India) | 30 | 77 |
| Chennai Communal Militia (India) | 13 | 81 |
| Chhajla Communal Militia (India) | 30 | 76 |
| Chhawla Communal Militia (India) | 29 | 77 |
| Chhiniwal Kalan Communal Militia (India) | 31 | 76 |
| Chikkade Communal Militia (India) | 13 | 77 |
| Chota Lahore Communal Militia (Pakistan) | 34 | 73 |
| Chountra Communal Militia (Pakistan) | 34 | 73 |
| Curchorem Communal Militia (India) | 16 | 74 |
| DA: Democratic Alliance | -34 | 19 |
| Dabhola Communal Militia (India) | 33 | 74 |
| Dabri Communal Militia (India) | 30 | 80 |
| Daharki Communal Militia (Pakistan) | 28 | 70 |
| Dandeli Communal Militia (India) | 15 | 75 |
| Dargai Communal Militia (Pakistan) | 35 | 72 |
| Darrang Communal Militia (India) | 27 | 93 |
| Darya Gali Communal Militia (Pakistan) | 34 | 74 |
| Datewas Communal Militia (India) | 30 | 76 |
| Dedo Communal Militia (Pakistan) | 31 | 77 |
| Deh Nau Abad Communal Militia (Pakistan) | 31 | 75 |
| Dehri Communal Militia (India) | 29 | 77 |
| Dera Bugti Communal Militia (Pakistan) | 29 | 69 |
| Dera Ghazi Khan Communal Militia (Pakistan) | 30 | 71 |

| | | |
|---|---|---|
| Detho Communal Militia (Pakistan) | 28 | 69 |
| Devidaspura Communal Militia (India) | 32 | 73 |
| Dhari Communal Militia (India) | 30 | 80 |
| Dhobiana Basti Communal Militia (India) | 30 | 75 |
| Dhoke Mangtal Communal Militia (Pakistan) | 34 | 73 |
| Dhotian Communal Militia (India) | 32 | 75 |
| Dhulian Communal Militia (India) | 25 | 88 |
| Dina Ki Mandi Communal Militia (India) | 27 | 78 |
| Dir Communal Militia (Pakistan) | 35 | 72 |
| DMK: Dravida Munnetra Kazhagam | 13 | 81 |
| Dobandai Communal Militia (Pakistan) | 35 | 73 |
| Doboka Communal Militia (India) | 26 | 93 |
| Dogar Communal Militia (Pakistan) | 32 | 75 |
| Dohkih Communal Militia (Pakistan) | 28 | 77 |
| Doom Dooma Communal Militia (India) | 28 | 96 |
| Dubli Communal Militia (India) | 31 | 75 |
| Dudhai Communal Militia (India) | 24 | 70 |
| Dulehar Communal Militia (India) | 32 | 76 |
| Dungian Communal Militia (India) | 32 | 75 |
| DYFI: Democratic Youth Federation of India | 29 | 77 |
| EFF: Economic Freedom Fighters | -26 | 28 |
| English Bazar Communal Militia (India) | 25 | 88 |
| Faisalabad Communal Militia (Pakistan) | 32 | 73 |
| Faizalabad Communal Militia (Pakistan) | 33 | 73 |
| Faridkot Communal Militia (India) | 31 | 75 |
| Farman Communal Militia (Pakistan) | 32 | 77 |
| Farrukhabad Communal Militia (India) | 28 | 80 |
| Fateh Jang Communal Militia (Pakistan) | 34 | 73 |
| Fateh Khankhel Tribal Militia (Pakistan) | 33 | 71 |
| Fatehgarh Jattan Communal Militia (India) | 31 | 77 |
| Fatehpur Communal Militia (India) | 26 | 81 |
| Fatuwala Communal Militia (Pakistan) | 28 | 72 |
| Ferozewala Communal Militia (Pakistan) | 32 | 75 |
| Gabol Communal Militia (Pakistan) | 28 | 69 |
| Gadarpur Communal Militia (India) | 29 | 80 |
| Ga-Molepo Communal Militia (South Africa) | -24 | 30 |
| Gandi Khan Khel Communal Militia (Pakistan) | 33 | 71 |
| Garhi Sheru Communal Militia (India) | 31 | 76 |
| Garja Communal Militia (Pakistan) | 30 | 79 |
| Gawara Communal Militia (India) | 26 | 74 |
| Ghariala Communal Militia (India) | 30 | 72 |
| Gharo Communal Militia (Pakistan) | 28 | 67 |
| Gharota Communal Militia (India) | 32 | 76 |
| Ghatkopar Communal Militia (India) | 19 | 73 |
| Ghaziabad Communal Militia (India) | 29 | 78 |
| Ghazipur Communal Militia (India) | 26 | 84 |
| Ghotki Communal Militia (Pakistan) | 28 | 70 |
| Ghuman Communal Militia (India) | 31 | 76 |
| Ghuman Kalan Communal Militia (India) | 31 | 76 |
| Ghundi Communal Militia (Pakistan) | 33 | 72 |
| Gill Kalan Communal Militia (India) | 31 | 76 |
| GJM: Gorkha Janmukti Morcha | 27 | 89 |
| Gojra Communal Militia (Pakistan) | 31 | 73 |
| Gopang Ethnic Militia (Pakistan) | 28 | 69 |
| Goth Surab Khan Communal Militia (Pakistan) | 30 | 67 |
| Gotyibeni Communal Militia (South Africa) | -32 | 29 |
| Gujar Khan Communal Militia (Pakistan) | 34 | 74 |
| Gujjar Communal Militia (Pakistan) | 31 | 75 |
| Gujrani Communal Militia (Pakistan) | 29 | 76 |
| Gujranwala Communal Militia (Pakistan) | 32 | 74 |
| Gul Imam Communal Militia (Pakistan) | 33 | 71 |
| Guligram Communal Militia (Pakistan) | 35 | 73 |
| Gundala Communal Militia (India) | 15 | 78 |
| Gupchani Communal Militia (Pakistan) | 26 | 69 |
| Guwahati Communal Militia (India) | 26 | 92 |

| | | |
|---|---|---|
| Halepoto Communal Militia (Pakistan) | 25 | 69 |
| Hanjarwal Communal Militia (Pakistan) | 32 | 75 |
| Harban Communal Militia (Pakistan) | 36 | 74 |
| Hariharpara Communal Militia (India) | 24 | 89 |
| Hasil Faqir Bozdar Communal Militia (Pakistan) | 27 | 69 |
| Haud Communal Militia (India) | 28 | 76 |
| Helenvale Communal Militia (South Africa) | -34 | 26 |
| Hisar Communal Militia (India) | 29 | 76 |
| HM: Hizb-ul-Mujahideen | 35 | 74 |
| HNA: Hmar National Army | 23 | 93 |
| Hoskote Communal Militia (India) | 13 | 78 |
| Husri Communal Militia (Pakistan) | 15 | 75 |
| Hussain Basti Communal Militia (India) | 30 | 73 |
| Hussainpura Communal Militia (India) | 32 | 75 |
| Hyderabad Communal Militia (India) | 18 | 79 |
| HYV: Hindu Yuva Vahini | 27 | 84 |
| Idgah Maidan Communal Militia (India) | 17 | 75 |
| IFP: Inkatha Freedom Party | -30 | 31 |
| IJT: Islami Jamiat-e-Talaba | 32 | 75 |
| Imphal Communal Militia (India) | 25 | 94 |
| INC: Indian National Congress | 29 | 77 |
| IPFT: Indigenous Peoples Front of Tripura | 24 | 92 |
| IUML: Indian Union Muslim League | 13 | 81 |
| IYC: Indian Youth Congress | 29 | 77 |
| Jabbowal Communal Militia (India) | 32 | 76 |
| Jaffarabad Communal Militia (Pakistan) | 32 | 75 |
| Jagirani Communal Militia (Pakistan) | 28 | 68 |
| Jagti Communal Militia (India) | 33 | 75 |
| Jagtial Communal Militia (India) | 19 | 79 |
| Jaintia Communal Militia (India) | 26 | 93 |
| Jaipur Communal Militia (India) | 27 | 76 |
| Jakhrani Communal Militia (Pakistan) | 29 | 70 |
| Jakhrani Tribal Militia (Pakistan) | 29 | 70 |
| Jalalpur Communal Militia (India) | 27 | 83 |
| Jalbani Communal Militia (Pakistan) | 28 | 68 |
| Jammu Communal Militia (India) | 33 | 75 |
| Jampur Communal Militia (Pakistan) | 30 | 71 |
| Jamrud Communal Militia (Pakistan) | 34 | 72 |
| Jamshedpur Communal Militia (India) | 23 | 86 |
| Jandola Communal Militia (Pakistan) | 33 | 70 |
| Janwari Communal Militia (Pakistan) | 27 | 69 |
| Jaranwala Communal Militia (Pakistan) | 32 | 74 |
| Jat Communal Militia (Pakistan) | 17 | 76 |
| Jatli Communal Militia (Pakistan) | 33 | 73 |
| Jatoi Communal Militia (Pakistan) | 30 | 71 |
| Jawaki Ara Khel Communal Militia (Pakistan) | 34 | 72 |
| JD(S): Janata Dal (Secular) | 13 | 78 |
| JD(U): Janata Dal (United) | 29 | 77 |
| JeM: Jaish-e-Mohammad | 30 | 72 |
| Jewan Gondal Communal Militia (Pakistan) | 22 | 71 |
| Jewar Communal Militia (India) | 28 | 78 |
| Jhal Magsi Communal Militia (Pakistan) | 29 | 68 |
| Jhang Communal Militia (Pakistan) | 34 | 73 |
| Jhansi Communal Militia (Pakistan) | 26 | 79 |
| Jhark Communal Militia (Pakistan) | 32 | 72 |
| JI: Jamaat-e-Islami | 32 | 75 |
| JJMP: Jharkhand Jan Mukti Parishad | 24 | 86 |
| Jokhio Communal Militia (Pakistan) | 25 | 68 |
| JSMM: Jeay Sindh Muttahida Mahaz | 26 | 69 |
| JSQM: Jeay Sindh Qaumi Movement | 26 | 69 |
| JUD: Jamaat-ud-Dawa | 32 | 75 |
| JUI-F: Jamiat Ulema-e-Islam-Fazl | 32 | 71 |
| Kabirwala Communal Militia (Pakistan) | 31 | 72 |
| Kahna Nau Communal Militia (Pakistan) | 32 | 75 |
| Kahuta Communal Militia (Pakistan) | 34 | 74 |

| | | |
|---|---|---|
| Kaimganj Communal Militia (India) | 28 | 80 |
| Kakori Communal Militia (India) | 27 | 81 |
| Kalhoro Communal Militia (Pakistan) | 26 | 69 |
| Kali Dinga Communal Militia (India) | 33 | 74 |
| Kaliachak Communal Militia (India) | 25 | 88 |
| Kaliasot Communal Militia (India) | 23 | 78 |
| Kallar Communal Militia (Pakistan) | 10 | 77 |
| Kamali Banda Communal Militia (Pakistan) | 33 | 71 |
| Kamboke Communal Militia (India) | 32 | 75 |
| Kamoke Communal Militia (Pakistan) | 32 | 74 |
| Kandari Communal Militia (Pakistan) | 19 | 76 |
| Kandhkot Communal Militia (Pakistan) | 28 | 69 |
| Kanpur Dehat Communal Militia (India) | 27 | 80 |
| Kapoor Singh Wala Communal Militia (India) | 32 | 75 |
| Karachi Communal Militia (Pakistan) | 25 | 67 |
| Karmatanr Communal Militia (India) | 24 | 87 |
| Karur Communal Militia (India) | 11 | 78 |
| Katlang Communal Militia (Pakistan) | 35 | 72 |
| Katohar Communal Militia (Pakistan) | 32 | 76 |
| Katra Communal Militia (India) | 26 | 86 |
| KCP: Kangleipak Communist Party | 25 | 94 |
| Khadoli Communal Militia (India) | 20 | 73 |
| Khairpur Communal Militia (Pakistan) | 28 | 69 |
| Khan Garh Communal Militia (Pakistan) | 31 | 76 |
| Khanpur Communal Militia (India) | 26 | 86 |
| Khanpur Communal Militia (Pakistan) | 26 | 86 |
| Khanpur Mahar Communal Militia (Pakistan) | 28 | 70 |
| Kharal Communal Militia (Pakistan) | 26 | 73 |
| Kharan Communal Militia (Pakistan) | 25 | 77 |
| Khari Dhand Communal Militia (Pakistan) | 26 | 70 |
| Kharral Communal Militia (Pakistan) | 30 | 73 |
| Khaskheli Communal Militia (Pakistan) | 28 | 69 |
| Khatauli Communal Militia (India) | 30 | 78 |
| Khati Communal Militia (Pakistan) | 30 | 80 |
| Kheda Communal Militia (India) | 23 | 73 |
| Khiala Kalan Communal Militia (India) | 32 | 75 |
| Khokhar Communal Militia (India) | 27 | 75 |
| Khokhar Communal Militia (Pakistan) | 27 | 75 |
| Khosa Communal Militia (Pakistan) | 31 | 76 |
| Khoso Communal Militia (Pakistan) | 26 | 70 |
| Khoso Tribal Militia (Pakistan) | 26 | 70 |
| Khuleka Communal Militia (South Africa) | -29 | 32 |
| Khumari Communal Militia (Pakistan) | 22 | 80 |
| Khuzdar Communal Militia (Pakistan) | 28 | 67 |
| Khyber Communal Militia (Pakistan) | 37 | 75 |
| Killi Pathan Goth Communal Militia (Pakistan) | 26 | 69 |
| KNF: Kuki National Front | 25 | 94 |
| Kohat Communal Militia (Pakistan) | 34 | 72 |
| Koliwad Communal Militia (India) | 16 | 76 |
| Kolkata Communal Militia (India) | 23 | 89 |
| Korangi Communal Militia (Pakistan) | 25 | 67 |
| Kot Addu Communal Militia (Pakistan) | 31 | 71 |
| Kot Hassan Khan Communal Militia (Pakistan) | 32 | 72 |
| Kot Momin Communal Militia (Pakistan) | 32 | 73 |
| Kotla Doom Communal Militia (India) | 32 | 75 |
| Kotli Communal Militia (Pakistan) | 32 | 77 |
| Kotri Communal Militia (Pakistan) | 26 | 69 |
| Kotwali Communal Militia (India) | 30 | 79 |
| Kozhikode Communal Militia (India) | 11 | 76 |
| Krugersdorp Communal Militia (South Africa) | -26 | 28 |
| Kumbakonam Communal Militia (India) | 11 | 80 |
| Kurar Communal Militia (India) | 19 | 73 |
| Kurram Communal Militia (Pakistan) | 33 | 71 |
| KwaZulu-Natal Communal Militia (South Africa) | -29 | 31 |
| Laghari Communal Militia (Pakistan) | 32 | 72 |

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| Laheriasarai Communal Militia (India) | 26 | 86 |
| Lahian Communal Militia (India) | 32 | 75 |
| Lahore Communal Militia (Pakistan) | 32 | 75 |
| Lakher Communal Militia (Pakistan) | 28 | 76 |
| Lakki Marwat Communal Militia (Pakistan) | 33 | 71 |
| Lakshimpur Communal Militia (India) | 25 | 87 |
| Lalru Communal Militia (India) | 31 | 77 |
| Landhi Communal Militia (Pakistan) | 26 | 69 |
| Langah Communal Militia (Pakistan) | 32 | 75 |
| Larkana Communal Militia (Pakistan) | 28 | 68 |
| Lasbela Communal Militia (Pakistan) | 26 | 67 |
| Lashari Communal Militia (Pakistan) | 31 | 74 |
| Lasi Goth Communal Militia (Pakistan) | 25 | 67 |
| Lathi Communal Militia (Pakistan) | 27 | 72 |
| Lehian Communal Militia (India) | 32 | 75 |
| LeT: Lashkar-e-Taiba | 32 | 75 |
| Lingapura Communal Militia (India) | 13 | 78 |
| Lisana Communal Militia (India) | 28 | 77 |
| Lodra Communal Militia (Pakistan) | 21 | 83 |
| Loharka Kalan Communal Militia (India) | 32 | 75 |
| Los Monos Gang | -33 | -61 |
| Ludhiana Communal Militia (India) | 31 | 76 |
| Lyari Communal Militia (Pakistan) | 25 | 67 |
| Machhi Communal Militia (Pakistan) | 26 | 70 |
| Machhrauli Communal Militia (India) | 29 | 77 |
| Machi Communal Militia (Pakistan) | 25 | 94 |
| Magangangozi Communal Militia (South Africa) | -29 | 30 |
| Magsi Communal Militia (Pakistan) | 24 | 76 |
| Mahar Communal Militia (Pakistan) | 30 | 79 |
| Mahesar Communal Militia (Pakistan) | 20 | 83 |
| Maho Dheri Communal Militia (Pakistan) | 34 | 72 |
| Mahsud Communal Militia (Pakistan) | 33 | 70 |
| Mahua Khera Communal Militia (India) | 27 | 78 |
| Maidan Communal Militia (Pakistan) | 23 | 89 |
| Mainpuri Communal Militia (India) | 27 | 79 |
| Malgin Communal Militia (Pakistan) | 34 | 72 |
| Malik Din Khel Tribal Militia (Pakistan) | 34 | 71 |
| Malikpur Communal Militia (Pakistan) | 26 | 88 |
| Malir Communal Militia (Pakistan) | 25 | 67 |
| Malpur Communal Militia (Pakistan) | 25 | 74 |
| Maluwal Communal Militia (India) | 32 | 75 |
| Mambapur Communal Militia (India) | 18 | 78 |
| Mamelodi Communal Militia (South Africa) | -26 | 29 |
| Mananwala Communal Militia (Pakistan) | 32 | 74 |
| Manesar Communal Militia (India) | 29 | 77 |
| Manga Mandi Communal Militia (Pakistan) | 32 | 74 |
| Mangrio Communal Militia (Pakistan) | 25 | 67 |
| Mano Chak Communal Militia (Pakistan) | 33 | 74 |
| Manwal Communal Militia (India) | 33 | 75 |
| Mardan Communal Militia (Pakistan) | 34 | 72 |
| Maregaon Communal Militia (India) | 20 | 79 |
| Mari Kamboke Communal Militia (India) | 32 | 75 |
| Mari Tribal Militia (Pakistan) | 31 | 76 |
| Marri Tribal Militia (Pakistan) | 31 | 76 |
| Maryamzai Communal Militia (Pakistan) | 34 | 72 |
| Masaurhi Communal Militia (India) | 26 | 85 |
| Mastala Communal Militia (Pakistan) | 33 | 73 |
| Mathia Hata Communal Militia (India) | 27 | 84 |
| Mayo Gardens Communal Militia (Pakistan) | 32 | 75 |
| Mazari Communal Militia (Pakistan) | 30 | 78 |
| Mdantsane Communal Militia (South Africa) | -33 | 28 |
| Mehar Communal Militia (Pakistan) | 27 | 68 |
| Mehar Shah Communal Militia (Pakistan) | 32 | 71 |
| Mehatpur Communal Militia (India) | 31 | 76 |
| Mehma Sawai Communal Militia (India) | 31 | 75 |

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| Memon Communal Militia (Pakistan) | 25 | 67 |
| Memon Goth Communal Militia (Pakistan) | 25 | 68 |
| Mengal Communal Militia (Pakistan) | 30 | 68 |
| Mevasa Communal Militia (India) | 24 | 71 |
| Mhlwazini Communal Militia (South Africa) | -29 | 30 |
| Mianwali Communal Militia (Pakistan) | 33 | 72 |
| Mirza Nawaz Communal Militia (Pakistan) | 34 | 73 |
| Mitraon Communal Militia (India) | 29 | 77 |
| MNS: Maharashtra Navnirman Sena | 19 | 73 |
| Moga Communal Militia (India) | 31 | 75 |
| Mohan Garden Communal Militia (India) | 29 | 77 |
| Morbi Communal Militia (India) | 23 | 71 |
| MPN: Neuquen People's Movement | -39 | -70 |
| MQM: Muttahida Qaumi Movement | 25 | 67 |
| MQM-H: Mohajir Qaumi Movement-Haqiqi | 25 | 67 |
| MQM-L: Muttahida Qaumi Movement-London | 25 | 67 |
| MSF: Muslim Students Federation | 13 | 81 |
| Msinga Communal Militia (South Africa) | -29 | 31 |
| Mughal Communal Militia (Pakistan) | 32 | 75 |
| Muktsar Communal Militia (India) | 31 | 75 |
| Muneer Communal Militia (Pakistan) | 25 | 67 |
| Munnekolala Communal Militia (India) | 13 | 78 |
| Murhu Communal Militia (India) | 23 | 86 |
| Murree Communal Militia (Pakistan) | 34 | 74 |
| Nabha Communal Militia (India) | 31 | 76 |
| Nacho Communal Militia (India) | 29 | 94 |
| Nagpur Communal Militia (India) | 21 | 79 |
| Nahali Communal Militia (India) | 22 | 75 |
| Nai Abadi Communal Militia (Pakistan) | 34 | 73 |
| Naich Communal Militia (Pakistan) | 30 | 72 |
| Naik Muhammad Communal Militia (Pakistan) | 25 | 67 |
| Naik Ziarat Communal Militia (Pakistan) | 31 | 68 |
| Nainital Communal Militia (India) | 30 | 80 |
| Nainwal Communal Militia (India) | 29 | 77 |
| Nakur Communal Militia (India) | 30 | 78 |
| Nand Nagri Communal Militia (India) | 29 | 78 |
| Nangal Communal Militia (India) | 32 | 77 |
| Nankana Sahib Communal Militia (Pakistan) | 32 | 74 |
| Narayanpur Communal Militia (India) | 26 | 87 |
| Nasirpur Communal Militia (India) | 31 | 77 |
| Nathpura Communal Militia (India) | 23 | 75 |
| Nathuwala Communal Militia (Pakistan) | 31 | 75 |
| Naurang Communal Militia (Pakistan) | 31 | 75 |
| Nawada Communal Militia (India) | 25 | 86 |
| Nawan Killi Communal Militia (Pakistan) | 32 | 75 |
| Nayagarh Communal Militia (India) | 20 | 85 |
| Ndibela Communal Militia (South Africa) | -34 | 19 |
| NDPP: Nationalist Democratic Progressive Party | 26 | 94 |
| New Fatehgarh Communal Militia (India) | 21 | 86 |
| New Gurnam Nagar Communal Militia (India) | 28 | 77 |
| Nimbahera Communal Militia (India) | 25 | 75 |
| Nizamani Communal Militia (Pakistan) | 25 | 69 |
| Noida Communal Militia (India) | 29 | 78 |
| Noor Muhammad Communal Militia (Pakistan) | 26 | 70 |
| Noorpur Basti Communal Militia (Pakistan) | 27 | 83 |
| Nothia Communal Militia (Pakistan) | 34 | 72 |
| NSCN: National Socialist Council of Nagaland | 26 | 94 |
| NSCN-IM: National Socialist Council of Nagaland-Isak Muivah | 26 | 94 |
| NSCN-K: National Socialist Council of Nagaland-Khaplang | 26 | 94 |
| NSCN-KK: National Socialist Council of Nagaland-Khango Konyak | 26 | 94 |
| NSCN-K-NK: National Socialist Council of Nagaland-Khaplang-Nyemlang Konyak | 26 | 94 |
| NSCN-K-YA: National Socialist Council of Nagaland-Khaplang-Yung Aung | 26 | 94 |
| NSCN-R: National Socialist Council of Nagaland-Reformation | 26 | 94 |
| NSCN-U: National Socialist Council of Nagaland-Unification | 26 | 94 |
| NSUI: National Students Union of India | 29 | 77 |

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| Ntsimbini Communal Militia (South Africa) | -33 | 29 |
| NUM: National Union of Mineworkers | -26 | 28 |
| NUMSA: National Union of Metalworkers of South Africa | -26 | 28 |
| Nusrat Pur Communal Militia (Pakistan) | 34 | 73 |
| Nuzvid Communal Militia (India) | 17 | 81 |
| Oghi Communal Militia (Pakistan) | 35 | 73 |
| Okara Communal Militia (Pakistan) | 31 | 74 |
| Okhla Communal Militia (India) | 29 | 78 |
| Orakzai Communal Militia (Pakistan) | 34 | 71 |
| Orangi Communal Militia (Pakistan) | 25 | 67 |
| Othwal Communal Militia (Pakistan) | 33 | 73 |
| PAC: People's Aman Committee | 25 | 67 |
| PAGAD: People Against Gangsterism and Drugs | -34 | 19 |
| Pakhi Kalan Communal Militia (India) | 31 | 75 |
| Pakhtoon Communal Militia (Pakistan) | 19 | 73 |
| Pakpattan Communal Militia (Pakistan) | 31 | 74 |
| Palam Vihar Communal Militia (India) | 29 | 77 |
| Palamedu Communal Militia (India) | 10 | 78 |
| Palda Communal Militia (India) | 23 | 76 |
| Palh Communal Militia (Pakistan) | 28 | 76 |
| Pandra Communal Militia (India) | 24 | 86 |
| Panhwar Communal Militia (Pakistan) | 26 | 69 |
| Para Chamkani Tribal Militia (Pakistan) | 34 | 72 |
| Pari Bangla Communal Militia (Pakistan) | 26 | 74 |
| PASMA: Pan Africanist Student Movement of Azania | -34 | 19 |
| Pasrur Communal Militia (Pakistan) | 33 | 75 |
| Patakpur Communal Militia (India) | 28 | 77 |
| Pathan Communal Militia (Pakistan) | 33 | 76 |
| Patna Communal Militia (India) | 26 | 85 |
| Peerwala Communal Militia (Pakistan) | 31 | 78 |
| Peshawar Communal Militia (Pakistan) | 34 | 72 |
| Peshwar Communal Militia (Pakistan) | 34 | 72 |
| Petlurivaripalem Communal Militia (India) | 16 | 80 |
| Phagwara Communal Militia (India) | 31 | 76 |
| Phulgran Communal Militia (Pakistan) | 34 | 73 |
| Phulwari Communal Militia (India) | 26 | 85 |
| Pindi Bhattian Communal Militia (Pakistan) | 32 | 74 |
| Pipariya Communal Militia (India) | 25 | 86 |
| Pipli Communal Militia (India) | 31 | 79 |
| Pirmahal Communal Militia (Pakistan) | 31 | 73 |
| PLA: People's Liberation Army of Manipur | 25 | 94 |
| PML-F: Pakistan Muslim League-Functional | 25 | 67 |
| PML-N: Pakistan Muslim League-Nawaz | 32 | 75 |
| Port Blair Communal Militia (India) | 12 | 93 |
| Powat Communal Militia (India) | 31 | 77 |
| PPP: Pakistan Peoples Party | 34 | 73 |
| Pratapgarh Communal Militia (India) | 26 | 82 |
| PSF: People's Student Federation | 34 | 73 |
| PSF: Peoples Students Federation | 34 | 73 |
| PSF: Pukhtoon Students Federation | 34 | 72 |
| PSP: Pak Sarzameen Party | 25 | 67 |
| PTI: Pakistan Tehreek-i-Insaf | 34 | 73 |
| Pundar Communal Militia (India) | 31 | 78 |
| Pursapur Communal Militia (India) | 17 | 78 |
| Qambar Shahdadkot Communal Militia (Pakistan) | 28 | 68 |
| Qambrani Communal Militia (Pakistan) | 28 | 68 |
| Quetta Communal Militia (Pakistan) | 30 | 67 |
| QWP: Qaumi Watan Party | 34 | 72 |
| Radhanpur Communal Militia (India) | 24 | 72 |
| Raiganj Communal Militia (India) | 26 | 88 |
| Raipur Communal Militia (India) | 21 | 82 |
| Raisani Communal Militia (Pakistan) | 30 | 67 |
| Raiwind Communal Militia (Pakistan) | 31 | 74 |
| Rajar Communal Militia (Pakistan) | 19 | 73 |
| Rajeev Colony Communal Militia (India) | 18 | 80 |

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| Rajeev Nagar Communal Militia (India) | 29 | 78 |
| Rajjar Communal Militia (Pakistan) | 34 | 72 |
| Rajpar Communal Militia (Pakistan) | 23 | 70 |
| Rajpura Communal Militia (India) | 31 | 77 |
| Rakkathampatti Communal Militia (India) | 11 | 79 |
| Ram Nagar Communal Militia (India) | 27 | 76 |
| Rampur Communal Militia (India) | 29 | 79 |
| Rampuram Communal Militia (India) | 18 | 83 |
| Ranchi Communal Militia (India) | 24 | 86 |
| Ranchi Communal Militia (india) | 24 | 86 |
| Randfontein Communal Militia (South Africa) | -26 | 28 |
| Randhawa Communal Militia (Pakistan) | 30 | 77 |
| Ranewali Communal Militia (India) | 32 | 75 |
| Rangar Communal Militia (Pakistan) | 32 | 77 |
| Rangia Communal Militia (India) | 27 | 92 |
| Rani Bagh Communal Militia (India) | 29 | 77 |
| Rani Majra Communal Militia (India) | 31 | 77 |
| Rasulpur Communal Militia (India) | 23 | 88 |
| Rawalpindi Communal Militia (Pakistan) | 34 | 73 |
| Rawat Communal Militia (Pakistan) | 34 | 73 |
| Rehti Communal Militia (India) | 23 | 78 |
| Remuna Communal Militia (India) | 22 | 87 |
| RJD: Rashtriya Janata Dal | 29 | 77 |
| Rodala Communal Militia (Pakistan) | 26 | 73 |
| RSS: Rashtriya Swayamsevak Sangh | 21 | 79 |
| Rupawas Communal Militia (India) | 26 | 74 |
| Rureke Kalan Communal Militia (India) | 31 | 76 |
| Rustenburg Communal Militia (South Africa) | -26 | 27 |
| Sadar Communal Militia (Pakistan) | 26 | 83 |
| Saddar Communal Militia (Pakistan) | 25 | 67 |
| Sadiqabad Communal Militia (Pakistan) | 29 | 70 |
| Sadozai Communal Militia (Pakistan) | 27 | 66 |
| Saharanpur Communal Militia (India) | 30 | 78 |
| Sahiwal Communal Militia (Pakistan) | 31 | 73 |
| Salarpur Communal Militia (India) | 27 | 83 |
| Salempur Communal Militia (India) | 27 | 84 |
| Samundri Communal Militia (Pakistan) | 31 | 73 |
| SAMWU: South African Municipal Workers Union | -26 | 28 |
| Sangatpura Communal Militia (India) | 30 | 74 |
| Sangna Communal Militia (India) | 32 | 75 |
| Sango Romana Communal Militia (India) | 31 | 75 |
| Sanjrani Communal Militia (Pakistan) | 29 | 70 |
| Santipur Communal Militia (India) | 24 | 89 |
| Saraikela Communal Militia (India) | 23 | 86 |
| Sargani Communal Militia (Pakistan) | 31 | 71 |
| Sarthal Communal Militia (India) | 25 | 77 |
| Sasaram Communal Militia (India) | 25 | 84 |
| SASCO: South Africa Students Congress | -26 | 28 |
| Sasoli Communal Militia (Pakistan) | 32 | 76 |
| SATAWU: South African Transport and Allied Workers Union | -26 | 28 |
| Satghara Communal Militia (Pakistan) | 31 | 74 |
| Sawai Madhopur Communal Militia (India) | 26 | 77 |
| Sawaich Kamalu Communal Militia (India) | 30 | 75 |
| SDPI: Social Democratic Party of India | 29 | 77 |
| Sethar Communal Militia (Pakistan) | 27 | 68 |
| Shadbagh Communal Militia (Pakistan) | 25 | 67 |
| Shah Hassan Khel Communal Militia (Pakistan) | 34 | 72 |
| Shahdadpur Communal Militia (Pakistan) | 26 | 69 |
| Shahdara Communal Militia (Pakistan) | 29 | 78 |
| Shaheed Udam Singh Nagar Communal Militia (India) | 31 | 76 |
| Shaheeda Banda Communal Militia (Pakistan) | 34 | 71 |
| Shahjahanpur Communal Militia (India) | 28 | 80 |
| Shahpur Bela Communal Militia (India) | 26 | 82 |
| Shahpur Communal Militia (India) | 26 | 85 |
| Shahzad Communal Militia (Pakistan) | 32 | 75 |

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| Shalozan Communal Militia (Pakistan) | 34 | 70 |
| Shamli Communal Militia (India) | 30 | 78 |
| Shankarpura Communal Militia (India) | 27 | 78 |
| Shatabgarh Communal Militia (India) | 31 | 77 |
| Sheikhabad Communal Militia (Pakistan) | 34 | 72 |
| Sherani Communal Militia (Pakistan) | 31 | 70 |
| Sher-e-Bengal | 24 | 88 |
| Shikarpur Communal Militia (Pakistan) | 28 | 78 |
| Shopian Communal Militia (India) | 34 | 75 |
| Shyampur Communal Militia (India) | 30 | 78 |
| Sialkot Communal Militia (Pakistan) | 33 | 75 |
| Sihala Communal Militia (Pakistan) | 31 | 76 |
| Sikandarpur Communal Militia (India) | 34 | 73 |
| SIMI: Students Islamic Movement of India | 28 | 78 |
| Sincha Communal Militia (India) | 34 | 75 |
| Siyahlala Communal Militia (South Africa) | -34 | 19 |
| Siyal Communal Militia (Pakistan) | 32 | 77 |
| SKM: Sikkim Krantikari Morcha | 28 | 89 |
| SMP: Sipah-e-Muhammad Pakistan | 32 | 75 |
| Sohana Communal Militia (India) | 31 | 77 |
| Sohna Communal Militia (India) | 29 | 77 |
| Solangi Communal Militia (Pakistan) | 25 | 70 |
| Sonari Communal Militia (India) | 27 | 95 |
| Sorada Communal Militia (India) | 20 | 85 |
| Soraon Communal Militia (India) | 26 | 82 |
| SP: Samajwadi Party | 29 | 77 |
| Sperkai Tribal Militia (Pakistan) | 33 | 70 |
| Spinwam Communal Militia (Pakistan) | 33 | 71 |
| Srivaikuntam Communal Militia (India) | 9 | 78 |
| SSP: Sipah-e-Sahaba Pakistan | 34 | 73 |
| Sukkur Communal Militia (Pakistan) | 28 | 69 |
| Sultanpur Lodhi Communal Militia (India) | 31 | 75 |
| Suppi Communal Militia (India) | 27 | 86 |
| Surab Communal Militia (Pakistan) | 29 | 67 |
| Surajpur Communal Militia (India) | 24 | 83 |
| Swabi Communal Militia (Pakistan) | 34 | 72 |
| Swat Communal Militia (Pakistan) | 35 | 72 |
| Syed Bachal Shah Communal Militia (Pakistan) | 28 | 68 |
| Tablighi Jamaat | 30 | 78 |
| Tahir Shah Communal Militia (Pakistan) | 32 | 75 |
| Tajori Communal Militia (Pakistan) | 33 | 71 |
| Talaja Communal Militia (India) | 22 | 72 |
| Talpur Communal Militia (Pakistan) | 30 | 76 |
| Talwandi Sabo Communal Militia (India) | 30 | 75 |
| Tando Bago Communal Militia (Pakistan) | 25 | 69 |
| Tando Yousuf Communal Militia (Pakistan) | 26 | 69 |
| Tapriyan Communal Militia (India) | 25 | 77 |
| Tareen Communal Militia (Pakistan) | 30 | 72 |
| Tarn Taran Communal Militia (India) | 32 | 75 |
| Tarsikka Communal Militia (India) | 32 | 75 |
| TDP: Telugu Desam Party | 18 | 79 |
| Teghani Communal Militia (Pakistan) | 28 | 69 |
| Tehrik-e-Tuhafaz Pakistan | 34 | 73 |
| Thaheem Communal Militia (Pakistan) | 25 | 67 |
| Thakarwal Communal Militia (India) | 31 | 76 |
| Thanjavur Communal Militia (India) | 11 | 79 |
| Thari Mirwah Communal Militia (Pakistan) | 26 | 69 |
| Thikriwala Communal Militia (Pakistan) | 32 | 74 |
| Tiljala Communal Militia (India) | 23 | 89 |
| Tiwaripur Communal Militia (India) | 26 | 82 |
| TLP: Tehreek-e-Labbaik Pakistan | 32 | 75 |
| TMC: Trinamool Congress Party | 23 | 89 |
| TMCP: Trinamool Chhatra Parishad | 23 | 89 |
| Township Communal Militia (Pakistan) | 32 | 75 |
| TRS: Telangana Rashtra Samithi | 18 | 79 |

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| Tughlaqabad Communal Militia (India) | 29 | 78 |
| Tulsinagar Communal Militia (India) | 17 | 80 |
| Tushura Communal Militia (India) | 21 | 84 |
| Ubha Communal Militia (India) | 30 | 76 |
| Uch Sharif Communal Militia (Pakistan) | 29 | 71 |
| Udaka Communal Militia (India) | 28 | 78 |
| UDM: United Democratic Movement | -26 | 28 |
| Uggoke Communal Militia (Pakistan) | 31 | 76 |
| ULA/AA: United League of Arakan/Arakan Army | 25 | 98 |
| Umrani Communal Militia (Pakistan) | 29 | 68 |
| Urmar Communal Militia (Pakistan) | 32 | 76 |
| Urmar Payan Communal Militia (Pakistan) | 34 | 72 |
| Usta Muhammad Communal Militia (Pakistan) | 28 | 68 |
| Uttam Nagar Communal Militia (India) | 29 | 77 |
| Vadodara Communal Militia (India) | 23 | 73 |
| Vehari Communal Militia (Pakistan) | 30 | 73 |
| VHP: Vishwa Hindu Parishad | 29 | 77 |
| Vishnupur Communal Militia (India) | 23 | 88 |
| Wali Muhammad Communal Militia (Pakistan) | 30 | 72 |
| Wankaner Communal Militia (India) | 23 | 71 |
| Wapda Town Communal Militia (Pakistan) | 30 | 72 |
| Warah Communal Militia (Pakistan) | 32 | 71 |
| Wazirwala Communal Militia (Pakistan) | 33 | 72 |
| Welkom Communal Militia (South Africa) | -28 | 27 |
| Xolobeni Communal Militia (South Africa) | -31 | 30 |
| Yaqubi Communal Militia (Pakistan) | 34 | 73 |
| Yar Hussain Communal Militia (Pakistan) | 34 | 73 |
| Yazman Communal Militia (Pakistan) | 29 | 72 |
| YSRCP: Yuvajana, Sramika, Rythu Congress Party | 17 | 81 |
| Zhob Communal Militia (Pakistan) | 32 | 70 |
| Ziarat Communal Militia (Pakistan) | 31 | 68 |

# Chapter III

# Banks of a Feather: The Informational Advantage of Being Alike

with Peter Bednarek, Valeriya Dinger, and Natalja von Westernhagen

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## Abstract

Banks lend more to banks that are similar to them. Using data from the German credit register and proprietary supervisory data on the quality of banks' loan portfolio, we show that a similar portfolio of the lending and borrowing bank helps to overcome information asymmetries in interbank markets. While interbank lenders generally do not adjust their lending to information on the counterparty's portfolio quality, banks with an exposure to similar industries and regions strongly react to this private information. Lending between similar banks is particularly important for borrowers with an opaque loan portfolio, which do not obtain credit from dissimilar peers.


**Keywords:** Peer monitoring; interbank markets; asymmetric information; portfolio quality; portfolio similarity; systemic risk and contagion

**JEL Codes:** E50; G11; G21; G20; G21

## 3.1 Introduction

Can banks effectively monitor their peers? This question is of central importance, given the relevance of banks' monitoring ability for functioning interbank markets and, by implication, financial markets. With the tightening of monetary policies starting in the early 2020s and the associated regaining relevance of liquidity provision via interbank markets, understanding the mechanisms behind peer monitoring has become a pressing concern.[1] The degree to which banks can accurately assess the solvency of other banks under asymmetric information has important implications for central bank policy. If banks monitor effectively, central banks can reduce their involvement to a night-watchman role (Goodfriend and King 1988). If, in contrast, banks systematically fail to identify solvent counterparties, central banks should be more active (Freixas and Jorge 2008).

We argue that portfolio similarity between two banks is key to understanding their reciprocal monitoring ability. We hypothesize that banks use private information on their own loan portfolio to evaluate the quality of the loan portfolio of a peer. A lending bank will then be better informed about a borrowing bank, the more similar their exposure. Aware of this informational advantage, a bank should prefer lending to similar peers. The mitigation of information asymmetries through similar portfolios should be particularly relevant when information is scarce, that is, for opaque borrowers. Introducing portfolio similarity to the analysis of interbank lending and peer monitoring thus improves our understanding of (i) how lending banks obtain private information on peers, (ii) why lending banks differ in their ability to monitor peers (Pérignon, Thesmar, and Vuillemey 2018), and (iii) how information asymmetries can be overcome in the interbank market (Heider, Hoerova, and Holthausen 2015).

Our analysis is built on quarterly, bilateral bank-to-bank and bank-to-firm exposure of more than 2000 banks from the German credit register between 2009 and 2018. We introduce a novel measure for the private quality of a bank's loan portfolio based on the bank's confidential risk evaluation of every outstanding loan. We obtain this

[1]Even in our sample, which covers the period between 2009 and 2018 when central banks were actively providing liquidity through expansionary monetary policies, interbank exposure represents 21% of German banks' total borrowing and 20% of banks' total lending, respectively. Decisions about lending and borrowing in interbank markets therefore have always remained of high relevance for German banks.

information from proprietary supervisory data on the probability of default (PD), which banks need to report for each of their borrowers.[2] To capture the time-varying quality of the loan portfolio of a bank, we calculate its portfolio-weighted PD and deduct this value from one, i.e. from a hypothetical portfolio without any default risk. We confirm the relevance of our measure as a forward-looking assessment for portfolio quality by showing its predictive power for the bank's non-performing loans (NPL) ratio in the next quarter up to the next 2 years. We show that our measure is, indeed, confidential as most peers do not adjust their lending when the portfolio quality of a borrowing bank worsens. Instead, banks adjust their lending to inferior, backward-looking proxies for portfolio quality, like the NPL ratio. Though easily accessible and commonly used in the literature (Afonso, Kovner, and Schoar 2011; Craig, Fecht, and Tümer-Alkan 2015), the NPL ratio does not capture the default risk inherent in the *current* loan portfolio, but the one of the past.

We also include a new measure of portfolio opacity building on banks' disagreement about the PD of the same borrowing firm, i.e. the standard deviation of PDs assigned to the same firm by different banks. A bank's portfolio-weighted standard deviation of PDs captures the divergence of peers' evaluations of the bank's loan portfolio. It measures portfolio opacity as gauged by banks themselves and, therefore, more directly as compared to the disagreement of rating agencies or the volatility of credit default swap (CDS) spreads used in the literature (Braeuning and Fecht 2017; Morgan 2002).

To measure the similarity between the loan portfolio of the lending and the borrowing bank, we compute the cosine similarity between their real exposure to different industries and regions. Building on these measures, we estimate how the quality and opacity of a borrowing bank's loan portfolio affects lending between banks with different levels of similarity. To capture the extensive and intensive margin of interbank lending and account for the fact that entering a lending relationship is not random, we use a sample selection model similar to Heckman (1977) (c.f. Braeuning and Fecht 2017).

Our results draw a nuanced picture of banks' ability to monitor peers. We show that banks can be good monitors, albeit only of very similar peers. Interbank lenders grant credit less frequently and in smaller amounts when a borrowing bank's loan

[2] For detailed information, see Section 3.3.

portfolio deteriorates. However, lending banks only do so for borrowing banks with outstanding loans to similar industries and regions like themselves. Dissimilar bank pairs, in contrast, do not adjust their lending to a deterioration of the counterparty's loan portfolio. Instead, dissimilar peers react to the backward-looking NPL ratio, which only imperfectly proxies forward-looking credit risk.

In line with our theoretical argument, banks with a similar loan portfolio lend significantly more to each other, both at the extensive and the intensive margin. Economically, preferential lending between similar banks is of similar relevance as relationship lending, one of the most important determinants of interbank lending in the literature (Braeuning and Fecht 2017). Lending between similar banks proves to be particularly important for borrowers with an opaque loan portfolio. Our findings hold after controlling for relationship lending, established bank networks, characteristics of the lending and the borrowing bank, market conditions, lender, borrower and time fixed effects.

We ensure that our findings are driven by changes in interbank credit supply, rather than demand, by identifying changes in liquidity supply in an adapted version of Degryse, Karas, and Schoors (2019)'s methodology. The intuition behind our approach is that banks of the same class (i.e. private, cooperative, or public banks of similar size), which concentrate on the same industries and regions should have similar liquidity needs in a given quarter. The distinct liquidity provision towards different borrowing banks of the same type can thus be interpreted as a supply response to characteristics of the borrowing bank, like its portfolio quality or opacity.[3] Disentangling supply from demand effects offers additional insights on how interbank borrowers cope with restricted access to the interbank market: Borrowing banks with a deteriorated loan portfolio obtain less liquidity by similar peers, which are well-informed about their (bad) portfolio quality. To compensate the lack of lending by similar peers, they turn to less informed, dissimilar lenders, which grant them interbank loans. Borrowing banks with an opaque loan portfolio obtain less liquidity by dissimilar peers, which cannot assess their portfolio adequately. To compensate the lack of lending by dissimilar peers,

[3] Our procedure to identify liquidity supply provides us with borrower-level changes in liquidity supply. As such, the approach helps us to support the supply-based interpretation, but cannot substitute the bank-pair-level analysis as it does not allow us to include bank-pair characteristics like portfolio similarity.

they turn to better informed, similar lenders, which grant them interbank loans.

Finally, we explore how relevant the different determinants of interbank lending are. Following Lemmon, Roberts, and Zender (2008), we decompose the variance in interbank lending into the variance attributable to characteristics of the lending bank, characteristics of the borrowing bank, common characteristics of both banks, and market characteristics. In our specifications, common characteristics of the counterparties explain 98.0 percent of the variation in the extensive margin, and 18.9 percent of the variation in the intensive margin of interbank lending. In contrast, borrower, lender, or market characteristics only explain 0.8, 1.2 and 0.1 percent of the variation in the extensive, and 44.2, 35.6, and 9.1 percent of the variation in the intensive margin of interbank lending, respectively. This finding substantiates the importance of including common characteristics of the lending and borrowing bank, like portfolio similarity, in the analysis of interbank lending.

Our paper contributes to several strands of literature. First, we extend the literature on peer monitoring of banks in an environment characterized by asymmetric information. Goodfriend and King (1988) argue that peers are particularly capable of assessing the solvency of banks and Rochet and Tirole (1996) show that they have an incentive to apply this ability. Flannery and Sorescu (1996) and Furfine (2001) provide empirical support and conclude that banks can identify other banks' risk better than other institutions, given their similar business model. We take their analysis one step further by showing that, even among banks, the more similar a lender, the better its monitoring ability. This is in line with Pérignon, Thesmar, and Vuillemey (2018) who highlight the heterogeneity between informed and uninformed lenders in interbank markets. By identifying "informed lenders" as banks with a similar loan portfolio, we shed light on which lenders can gain access to the borrowing bank's private information and how they obtain this information.

A related strand of literature highlights the importance of repeated interactions to obtain information on a counterparty. Affinito (2012), Braeuning and Fecht (2017), Cocco, Gomes, and Martins (2009), Hatzopoulos et al. (2015), and Temizsoy, Iori, and Montes-Rojas (2015) show that banks form stable and persistent relationships in interbank markets. The authors rationalize this finding by bilateral information generation, which facilitates monitoring and screening. Our analysis reveals one kind of information banks want to obtain through such relationships – information on the quality of the counterparty's real exposure. In contrast to the previous literature, we show that, given a similar loan portfolio, no long-standing relationship is needed to receive this information.

Concerns about the effectiveness of peer monitoring are particularly high for opaque banks and during insecure periods, when market information is less reliable (Flannery and Sorescu 1996; Braeuning and Fecht 2017) show that less transparent institutions, which have more difficulties refinancing themselves on interbank markets, rely on long-standing relationships to secure access. We show that, in addition to long-standing relationships, portfolio similarity mitigates the problem of hampered interbank access of opaque banks.

Several papers investigate the importance of lender and borrower characteristics and market conditions for interbank lending decisions (Afonso, Kovner, and Schoar 2011; Angelini, Nobili, and Picillo 2011; Brossard and Saroyan 2016; Fecht, Nyborg, and Rocholl 2011; Furfine 2001). Controlling for established lender, borrower, and market characteristics, we incorporate portfolio similarity and thereby augment the analysis with common characteristics of the borrowing and lending bank. In network analysis terms, we extend the analysis of ego covariates (lender characteristics), alter covariates (borrower characteristics) and network covariates (market characteristics) by dyadic covariates (common characteristics of lender and borrower).

One consequence of portfolio similarity discussed in the literature are correlated liquidity shocks (Fecht, Nyborg, and Rocholl 2011): Banks with a similar loan portfolio should have fewer opportunities to lend to each other. While correlated liquidity shocks might play a role in our analysis, this role is not important enough to challenge the robust, positive relation between portfolio similarity and interbank lending in our data.

In contrast to existing research on interbank lender and borrower characteristics, we use granular data on banks' real exposure to industries and regions. This allows us to look behind aggregated bank-level ratios and explicitly incorporate banks' real credit exposure, which is indispensable to properly judging banks' asset quality. Drawing on proprietary, supervisory data on banks' self-assessed borrower-specific risk, we can analyze peers' reaction to confidential information of the bank.

Finally, our findings contribute to the literature on systemic risk and contagion in interbank markets (Allen and Gale 2000; Brusco and Castiglionesi 2007; Castiglionesi and Wagner 2013; Craig and Ma 2022; Cocco, Gomes, and Martins 2009; Ladley 2013). Regardless of their interbank connections, banks with a similar loan portfolio are exposed to the risk of indirect contagion, e.g. by fire sales or feedback effects with the real sector (Allen, Babus, and Carletti 2012; Diamond and Rajan 2011; Silva, Alexandre, and Tabak 2017). Banks with a similar portfolio should consequently avoid running the additional risk of direct contagion by interbank lending. We show that banks do not avoid this risk and, instead, expose themselves over-proportionally to similar counterparties. Elliott, Hazell, and Co-Pierre (2018) rationalize this socially sub-optimal pattern by arguing that banks deliberately create systemic risk to be able to realize gains in a favorable state and increase their probability of being saved in a non-favorable state. Their study highlights the trade-off between hedging risk by financial connections, on the one hand, while propagating shocks through exactly these connections, on the other. While we do not aim to rule out the presence of risk shifting, we show that lending banks and the social planner face at least one additional trade-off: The strong connection between similar counterparties alleviates information asymmetries and, hence, increases interbank markets' efficiency, however, at the costs of increased systemic risk. This tradeoff is similar to the conflict between focus and diversification in corporate lending analyzed by Acharya, Hasan, and Saunders (2006).

The remainder of this paper is structured as follows. The next section explores the theoretical links between peer monitoring, private information on the quality of a borrowing bank's loan portfolio, and portfolio similarity. Section 3.3 presents our data. In Section 3.4, we demonstrate that the average bank does not restrict interbank lending to peers with a lower-quality loan portfolio, but significantly to peers with a higher NPL ratio. Section 3.5 shows that banks with a similar portfolio, however, restrict lending to peers after a deterioration of their loan portfolio, while reacting significantly less to similar peers' NPL ratio. We endorse that our results are driven by supply effects in Section 3.6 and rule out that our results are driven by the correlated portfolio quality of similar peers in Section 3.7. In Section 3.8, we show that common characteristics, like portfolio similarity, are highly relevant for interbank lending decisions by disentangling the fraction of variation in interbank lending attributable to lender, borrower, bank-pair, and market characteristics. Section 3.9 concludes.

## 3.2 Peer monitoring, portfolio quality, and portfolio similarity

To fulfill their role as peer-monitors, interbank market participants must distinguish between illiquid and insolvent peers. According to Fecht, Nyborg, and Rocholl (2011), lending banks make this distinction based on information on (i) the peer's capital position, (ii) its liquidity position, (iii) its profitability, and (iv) its asset quality. Weighing the costs and benefits of obtaining information on these positions, a lending bank will determine the optimal level of information it generates on each item.

Information costs are different for these four positions: A lending bank can easily research a peer's capital, liquidity, and profitability, drawing on commercial data bases from providers, like Bloomberg, which all banks can access. All lenders should thus incorporate accurate information on the peer's capital, liquidity, and profitability to a similar degree.

Information on a peer's asset quality is, in contrast, private and thus more costly to obtain (Morgan 2002). We hypothesize that a lender proxies the quality of a peer's loan portfolio by the average quality of industries and regions of the peer's exposure.[4] Tracking the time-varying default risks of these industries and regions, however, requires costly information gathering. To facilitate information generation, a lending bank can draw on its own private information, i.e. on information the lender itself has generated when granting loans to different industries and regions. Costs of information generation are consequently lower for a peer with a similar portfolio. Ceteris paribus, a similar lender should thus obtain more information on the borrowing bank. Lending conditions between similar banks should therefore more accurately reflect a borrower's asset quality. Moreover, lenders should be aware of their informational advantage towards similar peers and prefer to lend to similar counterparties.

[4] We assume that the lender can observe the peer's exposure to different industries and regions, at least imperfectly. This assumption is in line with the literature on specialization and segmentation in bank lending, see, e.g. Acharya, Hasan, and Saunders (2006), Blickle, Parlatore, and Saunders (2021), and Paravisini, Rappoport, and Schnabl (2021).

We therefore test the following hypotheses:[5]

*Hypothesis 1: Lenders with a similar loan portfolio reduce lending when the borrower's portfolio quality deteriorates. Lenders with a dissimilar loan portfolio do not reduce lending when the borrower's portfolio quality deteriorates.*

*Hypothesis 2: Bank pairs with a similar loan portfolio lend more to each other in interbank markets.*

Generating information on the time-varying quality of a peer's credit exposure is more costly if the peer's portfolio is opaque, which increases the value of the lending bank's pre-existing private information. Therefore, the informational advantages of similar portfolios should be higher, the less transparent a borrower (Braeuning and Fecht 2017). Preferential lending between similar peers should, consequently, be more pronounced, the less transparent the borrowing bank's loan portfolio.
We therefore test the following hypotheses:

*Hypothesis 3: Banks with a less transparent loan portfolio receive less interbank loans.*

*Hypothesis 4: Banks with a less transparent loan portfolio receive more loans from peers with a similar loan portfolio.*

## 3.3 Data and variables

### 3.3.1 Data sources and sample construction

Our unit of analysis are quarter-bank-pairs. As interbank loans are decided on the level of the bank, rather than on the level of the bank holding company, our level of observation is a pair between two banks, rather than between two bank holding companies. We obtain bilateral bank-to-bank and bank-to-firm exposure from the German credit register for the years between 2009 and 2018. The credit register is administered by the Deutsche Bundesbank and contains information on German banks' credit exposure to firms, including to financial firms (i.e. other banks). Banks have to report any loan granted to a firm whose total outstanding loans to German financial institutions add up to at least €1.5 million. The reporting requirement also

[5] Our hypotheses focus on the effect of portfolio quality, portfolio opacity, and portfolio similarity on *the amount of bilateral interbank lending*, rather than on its price. While price effects are certainly important in our setting, our dataset does not entail interest rates and does therefore not allow for an analysis of price effects.

includes loans below €1.5 million if the borrower's total debt exceeds the threshold of €1.5 million. Due to this low reporting threshold, our sample covers the complete universe of interbank exposure and all relevant exposure to the real economy.[6]

The credit register provides additional information about each borrower of a bank's loan portfolio. Most importantly, it includes the borrower's probability of default (PD) as reported by the credit granting bank, and each borrower's industry and region. We use this information to construct our main explanatory variables (for details, see below). Information on the PD is only available from 2009 on, which therefore marks the start of our analysis. To control for relevant bank characteristics, we add information on the lending and borrowing bank balance sheet from supervisory data of the Deutsche Bundesbank.

Table 3.1 shows the banks and interbank relations used in our analysis. Our sample of 2,054 lending and 2,035 borrowing banks reflects the German banking system, which is dominated by a few, large private banks (with a market share of about 30%), many savings banks (market share about 30%) and cooperative banks (market share about 20%), as well as their head institutes, i.e, regional heads of the savings banks network ("Landesbanken") or head institutes of the cooperative financial services network.[7] [8] We create a balanced sample by extending the bank-pairs that enter a lending relationship at least once during our sample period over all quarters. This procedure results in 2,644,640 lender-borrower-quarter combinations.[9]

[6]For details, see https://www.bundesbank.de/resource/blob/882918/897f226302c2462141dc6c5ee21aa621/mL/2021-12-27-dkp-52-data.pdf (Section 3.2.2). Unfortunately, our data does not entail information about interest rates for interbank loans. We therefore focus on the existence of a bilateral lending relation and lending quantities as outcome variables, rather than on prices.

[7]For further details on the German banking sector, we refer to Braeuning and Fecht (2017)

[8]The small difference between the number of lending and borrowing banks is due to the fact that most banks appear both as a lender and a borrower in the interbank market, few banks of our sample have, however, only lent to, not borrowed from the interbank market. See also Footnote 6.

[9]We decide against the alternative of including any possible bank-pair combination to avoid to inflate our sample artificially by including bank-pairs that have never entered a bilateral lending relationship (and will, most likely, not do so in the future). We thereby capture all bank pairs that could realistically lend to each other. However, we ignore those bank pairs that could theoretically lend to each other, but will not do so in reality. This is in line with the empirical evidence of tiered interbank markets, i.e. the finding that most German banks do never lend to each other directly (Craig and Peter 2014).

**Table 3.1:** Banks and interbank credit relations

This table reports the type of banks that lend and borrow in the interbank market in our sample and the number of credit relations between these banks. *Lender-borrower relations* are all possible quarterly bank-to-bank combinations between banks which have entered a lending relationship at least once in our sample, *True Credit relations* are those bank-to-bank relationships which do actually have outstanding bilateral exposure in a given quarter.

| **Bank type** | **Lending banks** | **Borrowing banks** |
|---|---|---|
| Large private banks | 6 | 6 |
| Smaller private banks | 198 | 182 |
| Head institutes of cooperative & saving banks | 14 | 14 |
| Saving banks | 467 | 467 |
| Cooperative banks | 1,347 | 1,345 |
| Other/Not classified | 22 | 21 |
| Total | 2,054 | 2,035 |
| **Lender-borrower relations in 40 quarters** | 2,644,640 | |
| True credit relations | 701,533 | |
| - between banks of same network (saving or cooperative banks) | 102,044 | |
| - between banks of same holding company | 2,087 | |

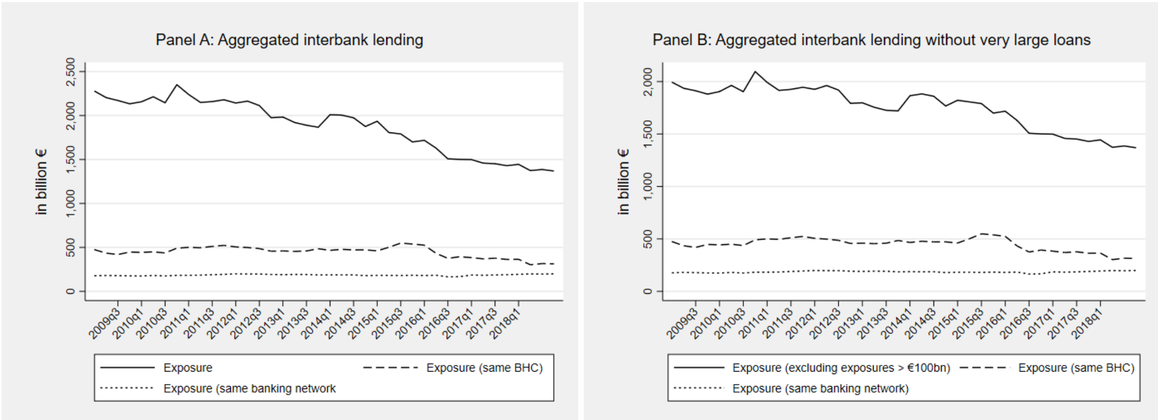
### 3.3.2 Dependent variables: Extensive and intensive margin of interbank lending

We identify an interbank credit relation between two banks by credit register entries of the lending bank indicating an outstanding exposure to the borrowing bank. As reported in Table 3.1, our sample includes 701,533 interbank credit relations, out of which 102,044 are between banks from the same banking network, e.g. between two savings banks or two cooperative banks, 2,087 credit relations are between banks from the same holding company.

Figure 3.1 shows the aggregated amount of quarterly interbank exposure between banks of our sample from 2009 to 2018. In accordance to previous studies (Allen, Covi, et al. 2020), the market has slightly shrunk over our sample period, in particular for very large loans. However, with an average quarterly credit exposure of about 1.4 trillion euros by the end of 2018, interbank exposure still represent 21% of German banks' total borrowing and 20% of banks' total lending, respectively. Decisions about lending and borrowing in interbank markets therefore remain of high relevance for German banks.

**Figure 3.1:** Interbank lending in Germany, 2009 to 2018

This figure shows the total amount of quarterly interbank lending between German banks. The solid line depicts total interbank exposure. The dotted line shows lending between banks of the same banking network. The dashed line shows lending between banks that belong to the same bank holding company.




A bank's decision to lend or borrow in the interbank market involves a decision about the extensive margin of credit, i.e. if to lend or borrow at all, and the intensive margin of credit, i.e. how much to lend or borrow. To address both dimensions, we construct two dependent variables: The binary variable $Credit\ relation_{i,j,t}$ captures the extensive margin of interbank lending. It assumes the value of one, if lending bank $i$ has an outstanding loan to borrowing bank $j$ at the end of quarter $t$, or if the borrowing bank $j$ has paid back the loan in quarter $t$. It is zero for all other lender-borrower combinations.[10]

To capture the intensive margin of interbank lending, we calculate the percentage change in on-balance bilateral exposure between lending bank $i$ and borrowing bank $j$ from quarter $t-1$ to quarter $t$ ($\Delta Exposure_{i,j,t}$). We interpret $\Delta Exposure_{i,j,t}$ as the granting of additional, respectively less liquidity by lender $i$ to a borrowing bank $j$ during quarter $t$. We calculate the (approximate) percentage change in bilateral exposure as:

$$\Delta Exposure_{i,j,t} = ln(Exposure_{i,j,t}) - ln(Exposure_{i,j,t-1}) \tag{3.1}$$

[10]Almost all banks appear both as a borrower and as a lender in the interbank market. For our sample, we therefore include each bank-pair twice, once with bank A as a lender and bank B as a borrower, once with bank B as a lender, bank A as a borrower. An exception are banks that have never lent or never borrowed in interbank markets in our sample period. We include those banks only in the role which they assume at least once during our sample period (i.e. only as a lender or only as a borrower).

Craig and Ma (2022) show that the majority of loans in the German interbank market are long-term. About 45% of loans maturities are even longer than a year and overnight loans make up for only 15% of total interbank lending. As a thorough evaluation of the counterparty's creditworthiness is most relevant for long-term exposure, the German data provides an excellent setting to study peer monitoring. Given the low share of overnight lending in the German market, our quarterly data captures the most important variation in interbank lending.

### 3.3.3 Explanatory variables

In the following section, we introduce our explanatory variables of interest measuring the private information on a bank's *Portfolio quality*, a bank's *Portfolio opacity* and the *Portfolio similarity* between two banks. Moreover, we introduce the control variables used in our analysis.

*Private information on quality of the bank's loan portfolio*

Judging a lending bank's ability to observe private information of a potential borrowing bank requires us (i) to identify information on a borrowing bank that is private, and (ii) to ensure this information is indeed relevant for the lending decision. In the following, we introduce our measure of *Portfolio quality.* We confirm its relevance for the interbank lending decision and its privacy in Section 3.4.

We measure the quality of a bank's loan portfolio by aggregating bank internal information about the credit risk of each of its borrowers. We obtain this information from quarterly regulatory filings, in which banks report the probability of default (PD) of each borrower to the regulator, which uses this information to quantify banks' credit risk, and, in turn, determine their capital requirement. The PD is a bank internal estimate of the likelihood that a counterparty will default on a loan or off-balance sheet financial contract within a year. Banks need to estimate the PD in accordance to data quality and methodological standards specified in the Capital Requirement Regulation (CRR, Article 180). Banks update their PD estimate quarterly for all counterparties, incorporating any new information obtained about borrowers' creditworthiness.[11]

Only banks using the Internal Rating-Based Approach need to report PDs. For

[11]For more details on the regulatory context of the PD, see the Capital Requirement Regulation (CRR), in particular Article 180.

banks using the Credit Risk Standardised Approach, PD reporting is not required.[12] To avoid a biased sample, we construct our measure of portfolio credit quality for all banks, including those following the Credit Risk Standardised Approach. To be able to do so, we obtain a borrower-specific PD, using the quarterly median PD reported for each borrower. For example, if firm A has outstanding credit to banks B and C, who use the Internal Rating-Based Approach, and to bank D who uses the Credit Risk Standardised Approach, we use the median of the PDs reported for firm A by banks B and C. This approach allows us to include PDs of all borrowers, except for those who only have exposure to banks following the Credit Risk Standardised Approach.

To construct a measure of *Portfolio quality*, we first calculate a bank's average portfolio PD as the exposure-weighted average of the PD of each borrower $k$, out of the bank's $K$ different borrowers at the end of quarter $t$. 'Borrower', in this context, refers to both counterparties with a loan on the bank's balance sheet and counterparties with an off-balance sheet financial contract, as both are relevant for a bank's portfolio quality. We then deduct the portfolio-weighted PD from the value of one. Thereby, we obtain a measure between zero - the quality of a hypothetical loan portfolio containing only borrowers with a PD of 1 - and one - the quality of a hypothetical loan portfolio containing only borrowers with a PD of 0:

$$Portfolio\,quality_t = 1 - \frac{1}{\sum_{k \in K} Exposure_{k,t}} \sum_{k \in K} Exposure_{k,t} \times PD_{k,t} \qquad (3.2)$$

In line with the regulatory intention, our measure of *Portfolio quality* is a forward-looking proxy for a bank's credit risk: Regressing banks' *Non-performing loans (NPL) ratio* on lagged values of *Portfolio quality* in Table 3.2 shows that *Portfolio quality* negatively and significantly predicts *NPL ratio*s of the next quarter up to the next 2 years, both in the cross-section of different banks (models in column (1)) and within each bank (models in column (2)). The variation in *Portfolio quality* explains between 16 and 17% of the cross-sectional variation of *NPL ratio*s in our sample (column (1)), and between 71% and 77% when including fixed effects (column (2)). A panel

[12] According to CRR, banks can decide if to use the Credit Risk Standardised Approach, for which the regulator assigns risk-weights based on asset class, or the Internal Rating-Based approach, for which the regulator estimates risk-weights based on bank-reported PDs for each borrower.

Granger causality test following Juodis, Karavias, and Sarafidis (2021) confirms that *Portfolio quality* precedes a bank's *NPL ratio* and that this negative relationship is highly significant for the next 5 to 50 quarters (Pooled Wald test statistics based on the Half Panel Jackknife procedure Dhaene and Jochmans (2015) $> 300$; Dumitrescu and Hurlin (2012)'s Z statistics $<$ -50).[13]

Much of banks' loan exposure is long term, in particular the exposure to the real economy. Consequently, both the series of *Portfolio quality* and *NPL ratio* are persistent to a certain extent. The presented analyses should thus be considered with caution. However, we take them as gentle evidence that *Portfolio quality* is indeed more forward-looking than the *NPL ratio* or that, at the very least, bank agents perceive it as such.

We will demonstrate that our measure of *Portfolio quality* is relevant for the lending decision and unobserved by the average counterparty when estimating the impact of *Portfolio quality* on interbank lending in Section 3.4.

The informative value and privacy of a supervisory measure to assess a counterparty is also supported by the literature: DeYoung et al. (1998) show that proprietary regulatory bank data contains useful private information about bank safety and soundness and that this information is unknown by other financial markets participants. This holds true even for banks that are extensively followed and analyzed by private investors and rating agencies. Similarly, Berger, Davies, and Flannery (2000) find that supervisors produce valuable information on bank conditions, which is complementary to information produced in the financial market.

*Portfolio opacity*

As we observe several PD assessments for borrowers, we build our measure of *Portfolio opacity* on peers' disagreement about a bank's *Portfolio quality*. For each borrower $k$ at quarter $t$, we determine the level of disagreement about its PD by the standard deviation of all PDs assigned to it in a quarter ($SD_{k,t}$). We then define a bank's *Portfolio opacity* as the quarterly, exposure-weighted average of these standard deviations:

[13] Coefficients from regressing the first differences of *NPL ratio* on *Portfolio quality* are insignificant and can be found in Table 3B.1 in Appendix 3B .

**Table 3.2:** Predicting non-performing loans ratios with portfolio quality

This table shows coefficients from OLS regressions of a bank's non-performing loans (NPL) ratios on its (lagged) *Portfolio quality*. Each cell shows the beta coefficient, standard error, $R^2$, and number of observations of regressing the *NPL ratio* at time t on *Portfolio quality* at time t, (t-1),(t-2), (t-3), (t-4), (t-5), (t-6), **or** (t-7), respectively. The sample consists of quarterly bank observations of 2054 banks between 2009 and 2018. Regressions in column (2) include bank fixed effects. Appendix 3A provides a detailed variable description.

| | Dependent variable: NPL ratio (t) | |
|---|---|---|
| | (1) | (2) |
| Portfolio quality (t) | -0.362*** | -0.098*** |
| | (0.00) | (0.00) |
| $R^2$ | 0.16 | 0.71 |
| $N$ | 53,200 | 53,174 |
| Portfolio quality (t-1) | -0.360*** | -0.084*** |
| | (0.00) | (0.00) |
| $R^2$ | 0.16 | 0.72 |
| $N$ | 51,003 | 50,945 |
| Portfolio quality (t-2) | -0.358*** | -0.068*** |
| | (0.00) | (0.00) |
| $R^2$ | 0.16 | 0.74 |
| $N$ | 48,871 | 48,848 |
| Portfolio quality (t-3) | -0.354*** | -0.050*** |
| | (0.00) | (0.00) |
| $R^2$ | 0.16 | 0.74 |
| $N$ | 46,863 | 46,829 |
| Portfolio quality (t-4) | -0.350*** | -0.030*** |
| | (0.00) | (0.00) |
| $R^2$ | 0.16 | 0.75 |
| $N$ | 44,908 | 44,882 |
| Portfolio quality (t-5) | -0.347*** | -0.018*** |
| | (0.00) | (0.00) |
| $R^2$ | 0.17 | 0.76 |
| $N$ | 42,988 | 42,926 |
| Portfolio quality (t-6) | -0.344*** | -0.007 |
| | (0.00) | (0.00) |
| $R^2$ | 0.17 | 0.77 |
| $N$ | 41,099 | 41,080 |
| Portfolio quality (t-7) | -0.341*** | -0.000 |
| | (0.00) | (0.00) |
| $R^2$ | 0.17 | 0.77 |
| $N$ | 39,284 | 39,245 |
| Bank Fixed Effects | No | Yes |

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

$$Portfolio\,Opacity_t = \frac{1}{\sum_{k \in K} Exposure_{k,t}} \sum_{k \in K} Exposure_{k,t} \times SD_{k,t} \tag{3.3}$$

*Portfolio opacity* captures asset opacity from the perspective of peers. For the decision on interbank credit, this should be more relevant than external measures that have been used in the literature, e.g. the disagreement of rating agencies or the volatility of credit default swap (CDS) spreads (Braeuning and Fecht 2017; Morgan 2002).

*Portfolio similarity*

With our measure of *Portfolio similarity* between a lending and borrowing bank, we aim at capturing how similar the firms are to which both banks have granted a loan or an off-balance sheet financial contract. As we assume that knowledge about a firm's situation requires knowledge about its industry and region, we consider a sectoral and a regional dimension of *Portfolio similarity.* We compute the cosine similarity between the loan portfolio of the lending and the borrowing bank based on banks' exposure towards different industries and regions.

To construct this cosine similarity measure, we first aggregate the on- and off-balance sheet exposure to different industries, respectively regions, for each bank in every quarter. For the sectoral exposure, we group loans to firms based on firms' principal activity. We classify the principal activity according to the first digit of *WZ 73*, the official industry classification scheme of the Federal statistical office of Germany.[14] This classification results in exposure to 10 distinct industries per bank. For robustness, we also include analyses based on the *WZ 73* two-digit classification code, resulting in 100 industries in Appendix 3C. To measure regional exposure, we group loans based on the first digit of the firms' zip code, resulting in exposure to a maximum of 9 distinct regions per bank.

For sectoral exposure, we construct the vectors $X_{i,t}$ and $X_{j,t}$ containing the exposure to each industry $p$ (out of $P = 10$ industries) of lending bank $i$, respectively borrowing bank $j$, at quarter $t$ in euros. Similarly, for regional exposure, we construct the vectors $Y_{i,t}$ and $Y_{j,t}$ containing the exposure to each region $q$ (out of $Q = 9$ regions)

[14]Unfortunately, we cannot use a more standard classification, like the NACE or SIC codes, as the credit register uses the *WZ 73* classification. More information on the industry classification can be found here: https://www.destatis.de/DE/Methoden/Klassifikationen/Gueter-Wirtschaftsklassifikationen/klassifikation-wz-2008.html

in euros. For each lender-borrower pair in quarter $t$, the cosine similarity between the two vectors is then defined as:

$$Portfolio\ Similarity\ (industries)_{i,j,t} = \frac{X_{i,t} \cdot X_{j,t}}{||X_{i,t}||||X_{j,t}||} = \frac{\sum_{p=1}^{P} x_{i,p,t}x_{j,p,t}}{\sum_{p=1}^{P} x_{i,p,t}^2 \sum_{p=1}^{P} x_{j,p,t}^2} \quad (3.4)$$

$$Portfolio\ Similarity\ (regions)_{i,j,t} = \frac{Y_{i,t} \cdot Y_{j,t}}{||Y_{i,t}||||Y_{j,t}||} = \frac{\sum_{q=1}^{Q} x_{i,q,t}x_{j,q,t}}{\sum_{q=1}^{Q} x_{i,q,t}^2 \sum_{q=1}^{Q} x_{j,q,t}^2} \quad (3.5)$$

The cosine of the angle between the two vectors $X_{i,t}$ and $X_{j,t}$, and $Y_{i,t}$ and $Y_{j,t}$, respectively, quantifies the extent to which the vectors point in the same direction. *Portfolio similarity* assumes a value of one if the two vectors are parallel, i.e. both banks possess exactly the same fraction of each industry or region. It assumes a value of zero for orthogonal vectors, that is, when the overlap between the industry or regional exposure of the two banks is zero. Since a bank cannot lend a negative amount, the measure ranges between zero and one for all other levels of similarity. As a scaled measure, it is independent of the vectors' length, respectively, of the total loan volume of a bank.

*Control variables*

Corresponding to our theoretical argument, we control for other indicators of bank solvency. Public information on a peer's capital position, liquidity position, and profitability should impact a lending decision, and could proxy loan portfolio risk. We therefore control for the borrowing bank's *Capital ratio* calculated as Equity/Risk-weighted-assets, its *Liquidity ratio* calculated as Liquid assets/Total assets, and its profitability measured by (risk-weighted) *Return on assets (ROA)*, calculated as net income divided by risk-weighted bank assets. To prevent that these values are affected by the availability of interbank loans in quarter $t$, we lag these control variables by one quarter.

For a bank pair with a high level of *Portfolio similarity*, the lending bank's solvency will resemble the borrower's solvency. We therefore also control for variables measuring the lender's solvency. In particular, we include the lender's *Portfolio quality*, *Portfolio opacity*, its *NPL ratio*, its *Liquidity ratio*, *Capital ratio*, and *ROA* in our analyses. However, the relatively high correlation between the *Portfolio quality* of similar peers poses another problem to our analysis: If a lending bank lends less in response to a deterioration of *its own portfolio*, we could misinterpret this as a response to the deterioration of the borrowing bank's similar portfolio. To make sure that the correlated *Portfolio quality* of similar bank pairs does not drive our results, we run additional analyses on a matched sample for which this correlation is the same for similar and non-similar pairs (see Section 3.8).

Long-standing lending relationships are an important determinant of interbank lending (Cocco, Gomes, and Martins 2009; Braeuning and Fecht 2017). To avoid confusing the impact of *Portfolio similarity* and relationship lending, we control for the frequency of previous interactions over a two-year window. Following Petersen and Rajan (1994) and Braeuning and Fecht (2017), we compute relationship lending as the logged sum of quarters $t'$ out of the last $T = 8$ quarters in which the lending bank $i$ has lent to the borrowing bank $j$.

$$Relationship\, lending_{i,j,t} = ln(1 + \sum_{t'=1}^{T} I(Credit\, relation_{i,j,t'} = 1)) \tag{3.6}$$

Analogously, we compute reverse relationship lending as the logged sum of quarters in which the borrowing bank $j$ has lent to the lending bank $i$.

$$Reverse\, relationship\, lending_{i,j,t} = ln(1 + \sum_{t'=1}^{T} I(Credit\, relation_{j,i,t'} = 1)) \tag{3.7}$$

A similar portfolio should go along with similar liquidity shocks (Fecht, Nyborg, and Rocholl 2011). As interbank lending requires one bank to have more, one to have less liquidity as compared to their desired level, similar banks should less often make a good lender-borrower match in the interbank market. We therefore control for the *Difference in liquidity surplus* between the lender and borrower. For each borrower-lender pair at the end of a quarter $t$, the variable is calculated as follows:

$$\begin{aligned} Difference\ in\ liquidity\ surplus_{i,j,t} &= Liquidity\ surplus_{i,t} - Liquidity\ surplus_{j,t} \\ &= Liquidity\ ratio_{i,t} - \overline{Liquidity\ ratio_i} \\ &- (Liquidity\ ratio_{j,t} - \overline{Liquidity\ ratio_j}) \end{aligned} \tag{3.8}$$

where $\overline{Liquidity\ ratio_i}$ is the lender's average liquidity ratio and $\overline{Liquidity\ ratio_j}$ is the borrower's average liquidity ratio.

Banks allocate liquidity within established banking networks, i.e. there is preferred lending between savings banks or cooperative banks (Fecht, Nyborg, and Rocholl 2011). As banks from the same network could also have similar credit exposure, we include a dummy variable indicating if lender and borrower are part of the same banking network, and if lender and borrower belong to the same bank holding company. Moreover, following the literature, we include the *Size* of the lending and borrowing bank as measured by ln(Total Assets) (Angelini, Nobili, and Picillo 2011; Ashcraft, McAndrews, and Skeie 2011; Fecht, Nyborg, and Rocholl 2011; Furfine 2001; Gabrieli 2011; Iori, Kapar, and Olmo 2015). To control for unobserved, stable bank-specific characteristics, we include lender and borrower fixed effects. To account for changing macroeconomic conditions which affect all banks (Angelini, Nobili, and Picillo 2011), we also include quarter-year fixed effects.

Our mechanism of interest is driven by the supply of interbank credit. To control for interbank credit demand, we capture a bank's need for liquidity by including its *Loans-to-assets* calculated by total loans over total assets as a control. As this control variable alone cannot rule out that demand effects, rather than supply effects could explain our findings, we perform additional analyses on the changes of interbank supply in Section 3.7).

Table 3.3 reports descriptive statistics for all relevant bank and interbank characteristics of our analysis.

**Table 3.3:** Bank and interbank characteristics

This table reports summary statistics of the bank and interbank characteristics of our sample. All variables are defined in Appendix 3A.

| | Observations | Unit | Mean | SD | p5 | Median | p95 |
|---|---|---|---|---|---|---|---|
| **Interbank Lending** | | | | | | | |
| Credit relation | 2,644,640 | Dummy | 0.27 | 0.44 | 0.00 | 0.00 | 1.00 |
| Δ Exposure | 2,623,392 | % | -0.41 | 36.08 | -4.01 | 0.00 | 2.59 |
| **Portfolio Similarity** | | | | | | | |
| Portfolio similarity (industries) | 2,644,640 | % | 91.92 | 14.27 | 65.13 | 97.28 | 99.80 |
| Portfolio similarity (industries, fine classification) | 2,644,640 | % | 74.51 | 21.54 | 29.49 | 79.86 | 98.48 |
| Portfolio similarity (regions) | 2,644,640 | % | 38.42 | 25.50 | 4.83 | 34.10 | 89.29 |
| **Bank characteristics** | | | | | | | |
| Interbank borrowing/total borrowing | 2,644,397 | % | 21.32 | 21.13 | 2.68 | 14.70 | 52.08 |
| Interbank lending/total lending[15] | 2,644,398 | % | 20.02 | 13.95 | 2.49 | 17.07 | 45.70 |
| Portfolio quality | 2,644,640 | % | 97.90 | 2.82 | 92.13 | 98.73 | 99.91 |
| Portfolio opacity | 2,644,640 | % | 1.81 | 1.68 | 0.31 | 1.30 | 5.00 |
| NPL ratio | 2,644,640 | % | 2.29 | 2.53 | 0.06 | 1.64 | 6.20 |
| Capital ratio | 2,639,307 | % | 23.58 | 31.92 | 11.52 | 18.54 | 33.92 |
| Liquidity ratio | 2,644,397 | % | 18.50 | 12.81 | 4.72 | 15.75 | 40.27 |
| ROA | 2,637,317 | % | 1.39 | 2.44 | -0.30 | 1.58 | 3.49 |
| Loans-to-assets | 2,644,357 | % | 52.85 | 19.20 | 13.45 | 56.02 | 79.73 |
| Size | 2,644,397 | log | 8.97 | 2.39 | 5.43 | 8.78 | 12.73 |
| **Relationship characteristics** | | | | | | | |
| Relationship lending | 2,644,640 | | 2.13 | 3.30 | 0.00 | 0.00 | 8.00 |
| Reverse relationship lending | 2,644,640 | | 2.11 | 3.29 | 0.00 | 0.00 | 8.00 |
| Δ Reverse exposure | 2,644,640 | % | -0.40 | 36.25 | -4.26 | 0.00 | 2.77 |
| Same network | 2,644,640 | Dummy | 0.12 | 0.32 | 0.00 | 0.00 | 1.00 |
| Same BHC | 2,644,640 | Dummy | 0.00 | 0.03 | 0.00 | 0.00 | 0.00 |
| Difference in liquidity surplus | 2,644,155 | ppt | 0.00 | 52.14 | -9.28 | 0.00 | 9.28 |

## 3.4 Interbank lenders' reaction to changes in the borrower's portfolio quality

This Section analyzes if banks incorporate forward-looking information on a peer's *Portfolio quality* in their decision to grant an interbank loan. We first discuss our empirical approach to estimate both the extensive and the intensive margin of interbank lending in a two-stage procedure. We then confirm the relevance and privacy of our *Portfolio quality* measure and find that, on average, banks with a lower *Portfolio quality* do not receive less funding, but banks with a higher *NPL ratio* do.

### 3.4.1 Methodological considerations

A lender's choice to supply liquidity to a bank in need involves two decisions: In a first step, the bank decides whether to lend at all (extensive margin). In a second step, it decides on the size of the loan (intensive margin).[16] Information on bilateral exposure, however, only exists for the subsample of bank pairs that have established a lending relation. To control for this non-random selection into our sample, we follow a two-step approach, as suggested by Heckman (1977) and used for the interbank market by Braeuning and Fecht (2017). We model the two steps by two equations, the selection equation and the outcome equation.

The selection equation defines the extensive margin of interbank lending. In the first stage of our regression, we estimate whether a bilateral loan ($Credit\,relation_{i,j,t}$) exists between lending bank $i$ and borrowing bank $j$ at quarter $t$ using the following Probit model:

$$\begin{aligned} P(Credit\ Relation_{i,j,t} = 1) = \Phi(&\beta_0 \\ &+ \beta_1 Portfolio\,quality_{j,t} + \beta_2 NPL\,ratio_{j,t} + \beta_3 Portfolio\,opacity_{j,t} \\ &+ \beta_4 Portfolio\,quality_{i,t} + \beta_5 NPL\,ratio_{i,t} + \beta_6 Portfolio\,opacity_{j,t} \\ &+ \beta_7 Credit\ Relation_{i,j,t-1} + Controls + FE_i + FE_j + FE_t + \epsilon_{i,j,t}) \end{aligned} \tag{3.9}$$

The outcome equation defines the intensive margin of interbank lending. It models the amount lent ($\Delta Exposure_{i,j,t}$) as a function of the covariates of interest. However, regressing $\Delta Exposure_{i,j,t}$ on our non-random sample would yield biased estimates. We

[16] Of course, these two decisions are interrelated, both temporally (i.e. they can be done simultaneously) and logically (i.e. the first decision can depend on the second). We separate between the two steps for analytical reasons. The second step involves more decisions such as the interest rate, the maturity of the loan or the requirement of collateral. However, this paper limits its attention to the size of the loan.

therefore include information on the non-existing pairs by controlling for the hazard of not entering into a lending relationship. This "non-selection hazard" is measured by the inverse Mills ratio (IMR), which we obtain from the first-stage Probit regression.

The IMR must contain some information that is not yet included in the second-stage estimation (exclusion restriction). Therefore, at least one variable should serve as an instrument: It should predict the matching between borrower and lender at the first stage, but be irrelevant for the change in exposure estimated at the second stage. We use $Credit\,relation_{i,j,t-1}$, i.e. the existence of a credit relation in $t-1$ as an instrument (Arellano and Bond 1991). As bilateral exposure often last longer than three months, this variable is highly predictive for the existence of a credit relation in t. However, a credit relation in $t-1$ bears no information about whether the bilateral exposure will increase or decrease over the next quarter. In the second stage, we estimate the following equation by OLS:

$$
\begin{aligned}
\Delta Exposure_{i,j,t} = {} & \beta_0 \\
& + \beta_1 Portfolio\,quality_{j,t} + \beta_2 NPL\,ratio_{j,t} + \beta_3 Portfolio\,opacity_{j,t} \\
& + \beta_4 Portfolio\,quality_{i,t} + \beta_5 NPL\,ratio_{i,t} + \beta_6 Portfolio\,opacity_{j,t} \\
& + \beta_7 IMR_{First\,Stage} + Controls + FE_i + FE_j + FE_t + \epsilon_{i,j,t}
\end{aligned}
\tag{3.10}
$$

### 3.4.2 Results

Table 3.4 reports the results of the Heckman sample selection model, estimating the effect of *Portfolio quality*, *Portfolio opacity*, and *NPL ratio* on the matching probability between two banks (Model (1)), on the changes in bilateral interbank exposure in the cross-section (Model (2)), on the changes in bilateral interbank exposure within a lending or borrowing bank (Model(3)) and on the changes in bilateral interbank exposure within a lending or borrowing bank, controlling for quarter-specific effects (Model (4)). To be able to compare coefficient sizes, we standardize all independent variables, except for binary variables.

**Table 3.4:** Interbank lending, portfolio quality, and portfolio opacity

This table shows the coefficients of a two-stage Heckman sample selection model. The sample consists of quarterly bank-pair observations of 2054 banks between 2009 and 2018. The dependent variables are the existence of a loan between lender $i$ and borrower $j$ at end-of-quarter $t$ (Model 1, Probit), and the percentage change of interbank exposure between lender $i$ and borrower $j$ over the period $t-1$ to $t$, respectively (Model 2 to 4, OLS). Model (3) includes lender and borrower fixed-effects, model (4) includes lender, borrower, and time fixed-effects. Coefficients are standardized, except for binary variables. Standard errors are clustered on the borrower and lender level. Control variables include the lagged values of ln(total assets), liquid assets/total assets, equity/risk-weighted assets, *ROA*, and (non interbank) *Loans-to-assets* of the borrowing and lending bank. Controls for the bank class, for being part of the same bank network, and of the same bank holding company are also included. All variables are defined in Appendix 3A.

| | Probit | OLS | | |
|---|---|---|---|---|
| | **Credit relation** | **Δ Exposure** | | |
| | (1) | (2) | (3) | (4) |
| **Borrower characteristics** | | | | |
| Portfolio quality | -0.046*** | -0.996 | -0.029 | -0.014 |
| | (0.00) | (0.75) | (0.48) | (0.60) |
| NPL ratio | -0.066*** | -2.360*** | -1.331*** | -1.536*** |
| | (0.00) | (0.50) | (0.39) | (0.49) |
| Portfolio opacity | -0.021*** | -1.106*** | -0.593** | -0.567* |
| | (0.00) | (0.32) | (0.24) | (0.30) |
| Capital ratio (t-1) | -0.021*** | -0.194 | -1.923*** | -0.324 |
| | (0.00) | (0.54) | (0.74) | (0.70) |
| Liquidity ratio (t-1) | -0.005** | -1.385** | 2.315 | 1.293 |
| | (0.00) | (0.65) | (2.49) | (2.52) |
| ROA (t-1) | 0.102*** | 3.861*** | 2.906*** | 2.395*** |
| | (0.00) | (0.66) | (0.79) | (0.83) |
| Loans-to-assets (t-1) | 0.050*** | 1.794*** | 1.925* | 2.837*** |
| | (0.00) | (0.56) | (1.07) | (0.93) |
| Size (t-1) | 0.208*** | 2.481*** | -0.660 | -0.940 |
| | (0.01) | (0.86) | (6.10) | (5.66) |
| **Lender characteristics** | | | | |
| Portfolio quality | 0.017*** | 0.892*** | 2.257*** | 2.154*** |
| | (0.00) | (0.27) | (0.52) | (0.55) |
| NPL ratio | -0.025*** | -0.488* | 1.421*** | 0.136 |
| | (0.00) | (0.26) | (0.39) | (0.40) |
| Portfolio opacity | 0.016*** | 0.553*** | 0.586*** | 0.766*** |
| | (0.00) | (0.17) | (0.16) | (0.17) |
| Capital ratio (t-1) | -0.094*** | -1.942*** | -2.447** | 0.110 |
| | (0.01) | (0.43) | (1.02) | (1.04) |
| Liquidity ratio (t-1) | 0.023*** | -1.158*** | -5.355** | -6.052** |
| | (0.00) | (0.31) | (2.48) | (2.52) |
| ROA (t-1) | 0.038*** | 1.661*** | 2.384*** | 1.357** |
| | (0.00) | (0.42) | (0.60) | (0.57) |
| Loans-to-assets (t-1) | -0.114*** | -3.041*** | 0.612 | 2.776** |
| | (0.00) | (0.34) | (1.18) | (1.16) |
| Size (t-1) | 0.028*** | -0.657 | -15.839*** | -6.973 |

**Table 3.4:** (continued) Interbank lending, portfolio quality, and portfolio opacity

| | Probit **Credit relation** | OLS **Δ Exposure** | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | (0.00) | (0.50) | (5.14) | (5.00) |
| **Relationship characteristics** | | | | |
| Relationship lending | 0.360*** | 3.528*** | 3.115*** | 3.260*** |
| | (0.00) | (0.57) | (0.56) | (0.56) |
| Reverse relationship lending | 0.077*** | 1.282*** | 1.661*** | 1.621*** |
| | (0.00) | (0.28) | (0.29) | (0.29) |
| Log reverse exposure | 0.019*** | 2.631*** | 2.588*** | 2.536*** |
| | (0.00) | (0.44) | (0.45) | (0.44) |
| Same BHC | 0.502*** | 13.735*** | 14.969*** | 14.873*** |
| | (0.06) | (2.16) | (2.31) | (2.31) |
| Same network | 0.391*** | 9.348*** | 7.796*** | 7.957*** |
| | (0.01) | (1.46) | (1.40) | (1.39) |
| Difference in liquidity surplus (t-1) | 0.000 | -0.529** | 11.229 | 9.988 |
| | (0.00) | (0.25) | (9.78) | (9.88) |
| **Heckman controls** | | | | |
| Credit relation (t-1) | 2.929*** | | | |
| | (0.01) | | | |
| IMR | | 60.919*** | 61.761*** | 61.817*** |
| | | (2.04) | (2.05) | (2.04) |
| Observations | 2,545,319 | 655,517 | 655,493 | 655,493 |
| Bank class controls | Yes | Yes | No | No |
| Lender & borrower FEs | No | No | Yes | Yes |
| Time FEs | No | No | No | Yes |
| (Pseudo) R-squared | 0.83 | 0.14 | 0.15 | 0.15 |

Standard errors (twoway clustered by lender and borrower) in parentheses

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

*Interbank lending and borrower's portfolio quality and opacity*

In contrast to the idea that interbank lending should be restricted for banks with a worse asset quality, coefficients on the borrowing bank's *Portfolio quality* are negative and mostly insignificant. At the extensive margin, banks with a better loan portfolio are even *less* likely to form an interbank relationship (Model (1)). Setting all other variables to the average and binary variables to zero, a one standard deviation decrease in *Portfolio quality* is associated with a 82 basis points lower probability of being an interbank borrower, compared to an unconditional probability of being an interbank borrower of 26.53 percent. At the intensive margin, there is no significant difference between banks with different levels of portfolio quality (Model (2)) and banks do not receive less interbank liquidity after a deterioration of their *Portfolio quality* (Model (3)), also not after controlling for quarter-year specific effects (Model (4)).

A higher *NPL ratio*, however, significantly decreases both a borrowing bank's probability of forming a lending relationship and the magnitude of a loan. A one-standard deviation increase in the borrowing bank's *NPL ratio* decreases its probability of receiving a loan by 118 basis points, holding all other variables at their mean and binary variables at zero (Model(1)). The amount of liquidity received by a bank with a one standard deviation higher *NPL ratio* is 236 basis points lower, compared to the cross section (Model (2)). Similarly, a borrower receives between 133 (Model (3)) and 154 basis points (Model(4)) less interbank liquidity with a one standard deviation increase in its *NPL ratio*. This is a relevant reduction, compared to the average quarterly change in bilateral interbank exposure of 1.46 percent (considering only banks with a lending relationship).

These findings are in line with the interpretation that the forward-looking *Portfolio quality* is unobserved by the average market participant. Therefore, banks resort to the backward-looking, though observable, information on peers' *NPL ratio*. Given the predictive power of a bank's *Portfolio quality* for *NPL ratios* in the following quarters reported in Section 3.3, the average lending bank thereby uses an inferior, though easily accessible proxy to assess the borrower's asset quality.

An opaque portfolio of the borrower has a significantly negative effect on interbank lending. Banks with a less transparent portfolio receive fewer and smaller loans. For banks with a one-standard deviation higher *Portfolio opacity*, the likelihood of receiving an interbank loan decreases by 38 basis points (Model (1)) and the amount of liquidity received decreases by 111 basis points (Model (2)). A bank's one-standard deviation increase of *Portfolio opacity* results in a reduction in interbank liquidity by 59 (Model (3)), or 57 basis points, when also controlled for quarter-specific effects (Model (4)), respectively.

Lenders' reluctance to grant loans to peers with a less transparent portfolio is in line with the expectation that opacity makes it harder to judge a counterparty's portfolio as it increases the risk of evaluating the peer's portfolio quality incorrectly.

*Interbank lending and lender's portfolio quality*

In line with existing research (Acharya and Merrouche 2013), lenders lend significantly less when their *own* asset quality worsens. For a lender with a one standard deviation lower *Portfolio quality*, the likelihood to start a new lending relationship is reduced by 29 basis points (Model (1)), the amount of liquidity provided is reduced by 89 basis points (Model (2)), compared to an unconditional probability to lend of 26.53 percent. In the cross-section, the economic magnitude of the effect is rather small. However, the effect of changes of one bank's *Portfolio quality* over time is large, compared to the average quarterly change in bilateral exposure of 1.46 percent: A one-standard deviation decrease in *Portfolio quality* reduces the amount provided in interbank markets by 226 (Model (3)), or 215 basis points, when controlled for quarter-specific effects (Model(4)), respectively.

While, on average, banks with a higher *NPL ratio* lend less in interbank markets, banks do not react negatively to an increase in their own *NPL ratio*: In the cross-section, banks with a one-standard deviation higher *NPL ratio* have a 44 basis points lower likelihood to lend in interbank markets (Model (1), compared to an unconditional probability to lend of 26.53 percent) and lend 49 basis points less (Model (2), compared to the average bilateral change in exposure of 1.46 percent). Within a potential lending bank, however, an increase in the *NPL ratio* shows no clear impact on the amount lent (Model (3) and Model (4)).

These results further support our interpretation that *Portfolio quality* is a relevant and private measure of asset quality. A lending bank, which can observe its own *Portfolio quality*, therefore responds to a change in this private measure and less to the inferior, but publicly available *NPL ratio*.

*Other variables and quality of the model*

Our results hold after controlling for established bank and relationship characteristics, for bank network affiliation, for belonging to the same bank holding company and for having (un)correlated liquidity shocks. The direction of included controls is in line with the existing literature: Larger banks borrow more in interbank markets and, even though appearing more often as a lender, lend less in interbank markets. Established relationship characteristics also show large effects in the expected direction.

Model (1) in Table 3.4 further shows that our first-stage instrument, the lagged existence of a credit relation, has a strong impact on the existence of a credit relation in quarter $t$ (t-statistic of 335), ruling out concerns about a weak instrument in our first-stage regression.

To sum up, the average lending bank does not react to a deterioration in the forward-looking *Portfolio quality*, even though it is predictive for future *NPL ratios*. Instead, lenders rely on current *NPL ratios*, an inferior measure capturing the "damage already done", not the one to expect in upcoming quarters. The stark reduction of lending after a deterioration of bank's own *Portfolio quality*, indicates that, in line with our analyses in Section 3.3, banks consider *Portfolio quality* a useful metric for its asset quality.

## 3.5 Interbank lenders' reaction to changes in the borrower's portfolio quality: The role of portfolio similarity

We now investigate which role *Portfolio similarity* plays in interbank lending. In particular, we evaluate (i) whether banks with a similar loan portfolio lend more or less to each other, (ii) whether banks with different levels of similarity react differently to a change in peers' asset quality as measured by *Portfolio quality* and the *NPL ratio*, and (iii) whether banks with different levels of similarity react differently to a change in the *Portfolio opacity* of peers. We first explain our specification and then report our results.

### 3.5.1 Methodological considerations

As in the previous section, we estimate bilateral matching probabilities and changes in the interbank exposure between bank pairs, controlling for sample selection issues with a Heckman sample selection model. In this section, however, we include *Portfolio similarity* between the lending and borrowing bank in our analysis. With the base effect of *Portfolio similarity*, we investigate whether banks with a similar loan portfolio lend more or less to each other. To test if similar banks react differently to the different measures of asset quality, we interact *Portfolio similarity* with *Portfolio quality* and *NPL ratio*. To identify a divergence in the reaction to *Portfolio opacity*, based on different similarity levels of the lending and borrowing bank, we also include the interaction between *Portfolio similarity* and *Portfolio opacity*. We do so for both the sectoral and regional dimension of *Portfolio similarity*. In particular, we estimate the following two equations. For simplicity, $Portfolio Similarity_{i,j,t}$ refers to both the sectoral and the regional similarity measure.

$$\begin{aligned}
P(Credit\ Relation_{i,j,t} = 1) = \Phi(&\beta_0 + \beta_1 Portfolio\ Similarity_{i,j,t} \\
&+ \beta_2 Portfolio\, quality_{j,t} + \beta_3 Portfolio\, quality_{j,t} \times Portfolio\ Similarity_{i,j,t} \\
&+ \beta_4 NPL\, ratio_{j,t} + \beta_5 NPL\, ratio_{j,t} \times Portfolio\ Similarity_{i,j,t} \\
&+ \beta_6 Portfolio\, opacity_{j,t} + \beta_7 Portfolio\, opacity_{j,t} \times Portfolio\ Similarity_{i,j,t} \\
&+ \beta_8 Portfolio\, quality_{i,t} + \beta_9 NPL\, ratio_{i,t} + \beta_{10} Portfolio\, opacity_{i,t} \\
&+ \beta_{11} Credit\ Relation_{i,j,t-1} + Controls + FE_i + FE_j + FE_t + \epsilon_{i,j,t})
\end{aligned} \tag{3.11}$$

$$
\begin{aligned}
\Delta Exposure_{i,j,t} &= \beta_0 + \beta_1 Portfolio\, Similarity_{i,j,t} \\
&+ \beta_2 Portfolio\, quality_{j,t} + \beta_3 Portfolio\, quality_{j,t} \times Portfolio\, Similarity_{i,j,t} \\
&+ \beta_4 NPL\, ratio_{j,t} + \beta_5 NPL\, ratio_{j,t} \times Portfolio\, Similarity_{i,j,t} \\
&+ \beta_6 Portfolio\, opacity_{j,t} + \beta_7 Portfolio\, opacity_{j,t} \times Portfolio\, Similarity_{i,j,t} \\
&+ \beta_8 Portfolio\, quality_{i,t} + \beta_9 NPL\, ratio_{i,t} + \beta_{10} Portfolio\, opacity_{i,t} \\
&+ \beta_{11} IMR_{First\, Stage} + Controls + FE_i + FE_j + FE_t + \epsilon_{i,j,t}
\end{aligned}
\tag{3.12}
$$

### 3.5.2 Results

Table 3.5 and 6 report the results of estimating Equation (3.11) (Model (1)) and Equation (3.12) (Models (2) to (4)), including bank fixed effects in Model (3) and bank and quarter-year fixed effects in Model (4). To be able to compare coefficient sizes, all independent variables, except for binary variables, are standardized.

*Interbank lending, borrower's portfolio quality, and portfolio similarity*

As in the previous section, the base effect of *Portfolio quality* on interbank lending is negative at the extensive, and mostly insignificant at the intensive margin. The significantly positive coefficients for the interaction between *Portfolio quality* and *Portfolio similarity*, however, show that the effect differs considerably for bank-pairs with different levels of *Portfolio similarity.* Table 3.6 reports marginal effects for the regressions of Table 3.5. "High" similarity refers to bank pairs with a 3 standard deviation higher similarity than the average, "low" similarity to bank pairs with a 3 standard deviation lower similarity than the average. We report marginal effects for these relatively extreme values of portfolio similarity to demonstrate how different a bank's most similar peers (i.e. the few banks with almost the same business model) react, compared to a bank's most dissimilar peers (i.e. the few banks specialized on completely different industries and regions).

**Table 3.5:** Interbank lending, portfolio similarity, and credit portfolio quality

This table shows the coefficients of a two-stage Heckman sample selection model on a matched sample. The sample consists of quarterly bank-pair observations of 2054 banks between 2009 and 2018. Bank pairs with a high-similarity in both regional and sectoral terms are matched to bank pairs of similar correlation between the lender's and the borrower's credit portfolio quality but with a low similarity both regional and sectoral terms. The dependent variables are the existence of a loan between lender $i$ and borrower $j$ at end-of-quarter $t$ (Model 1, Probit), and the percentage change of interbank exposure between lender $i$ and borrower $j$ over the period (t-1) to t, respectively (Model 2 to 4, OLS). Model (3) includes lender and borrower fixed-effects, model (4) includes lender, borrower, and time fixed-effects. Coefficients are standardized, except for binary variables. Standard errors are clustered on the borrower and lender level. All variables are defined in Appendix 3A.

| | Probit<br>**Credit relation** | OLS<br>**Δ Exposure** | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| **Common characteristics** | | | | |
| Portfolio similarity (industries) | 0.007** | 0.999* | 2.587*** | 2.109*** |
| | (0.00) | (0.51) | (0.68) | (0.68) |
| Portfolio similarity (regions) | 0.031*** | 0.843*** | 0.943*** | 1.142*** |
| | (0.00) | (0.23) | (0.20) | (0.21) |
| **Borrower characteristics** | | | | |
| Portfolio quality | -0.034*** | -0.773 | -0.012 | 0.036 |
| | (0.00) | (0.60) | (0.54) | (0.63) |
| Portfolio quality<br>× Portfolio similarity (industries) | 0.022*** | 0.921** | 0.735*** | 0.638** |
| | (0.00) | (0.43) | (0.27) | (0.27) |
| Portfolio quality<br>× Portfolio similarity (regions) | 0.017*** | 0.698*** | 0.496*** | 0.511*** |
| | (0.00) | (0.19) | (0.14) | (0.14) |
| NPL ratio | -0.063*** | -2.376*** | -1.257*** | -1.661*** |
| | (0.00) | (0.47) | (0.43) | (0.52) |
| NPL ratio<br>× Portfolio similarity (industries) | -0.000 | 0.071 | 0.114 | -0.158 |
| | (0.00) | (0.31) | (0.23) | (0.23) |
| NPL ratio<br>× Portfolio similarity (regions) | 0.016*** | 0.569*** | 0.401*** | 0.370*** |
| | (0.00) | (0.19) | (0.14) | (0.14) |
| Portfolio opacity | -0.021*** | -1.122*** | -0.672*** | -0.648** |
| | (0.00) | (0.31) | (0.25) | (0.31) |
| Portfolio opacity<br>× Portfolio similarity (industries) | 0.016*** | 0.594*** | 0.485*** | 0.462*** |
| | (0.00) | (0.17) | (0.12) | (0.13) |
| Portfolio opacity<br>× Portfolio similarity (regions) | 0.006*** | 0.210 | 0.235* | 0.293** |
| | (0.00) | (0.15) | (0.14) | (0.14) |
| Capital ratio (t-1) | -0.018*** | 0.114 | -1.450* | 0.022 |
| | (0.00) | (0.59) | (0.77) | (0.73) |
| Liquidity ratio (t-1) | -0.006** | -1.340** | 2.438 | 1.443 |
| | (0.00) | (0.65) | (2.49) | (2.53) |
| ROA (t-1) | 0.100*** | 3.762*** | 2.808*** | 2.385*** |
| | (0.00) | (0.66) | (0.76) | (0.81) |
| Loans-to-assets (t-1) | 0.053*** | 2.036*** | 2.322** | 3.015*** |
| | (0.00) | (0.58) | (1.08) | (0.91) |
| Size (t-1) | 0.210*** | 2.518*** | -0.468 | -1.101 |
| | (0.01) | (0.86) | (6.03) | (5.58) |

**Table 3.5:** (continued) Interbank lending, portfolio similarity, and portfolio quality

| | Probit<br>**Credit relation** | OLS<br>**Δ Exposure** | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| **Lender characteristics** | | | | |
| Portfolio quality | 0.016*** | 0.808*** | 2.224*** | 2.118*** |
| | (0.00) | (0.28) | (0.51) | (0.54) |
| NPL ratio | -0.022*** | -0.416* | 1.430*** | 0.239 |
| | (0.00) | (0.25) | (0.39) | (0.42) |
| Portfolio opacity | 0.015*** | 0.556*** | 0.613*** | 0.791*** |
| | (0.00) | (0.16) | (0.16) | (0.17) |
| Capital ratio (t-1) | -0.093*** | -1.845*** | -2.156** | 0.300 |
| | (0.01) | (0.41) | (1.04) | (1.05) |
| Liquidity ratio (t-1) | 0.022*** | -1.105*** | -5.495** | -6.189** |
| | (0.00) | (0.32) | (2.49) | (2.53) |
| ROA (t-1) | 0.039*** | 1.676*** | 2.335*** | 1.375** |
| | (0.00) | (0.41) | (0.60) | (0.56) |
| Loans-to-assets (t-1) | -0.113*** | -2.907*** | 0.978 | 2.961** |
| | (0.00) | (0.36) | (1.22) | (1.18) |
| Size (t-1) | 0.027*** | -0.672 | -14.539*** | -6.333 |
| | (0.00) | (0.50) | (5.08) | (5.02) |
| **Relationship characteristics** | | | | |
| Relationship lending | 0.359*** | 3.507*** | 3.074*** | 3.206*** |
| | (0.00) | (0.57) | (0.56) | (0.56) |
| Reverse relationship lending | 0.073*** | 1.129*** | 1.508*** | 1.440*** |
| | (0.00) | (0.27) | (0.29) | (0.28) |
| Δ Reverse exposure | 0.019*** | 2.626*** | 2.577*** | 2.526*** |
| | (0.00) | (0.44) | (0.44) | (0.44) |
| Same network | 0.388*** | 9.436*** | 7.702*** | 7.887*** |
| | (0.01) | (1.46) | (1.41) | (1.41) |
| Same BHC | 0.487*** | 13.595*** | 14.507*** | 14.321*** |
| | (0.06) | (2.15) | (2.33) | (2.33) |
| Difference in<br>liquidity surplus (t-1) | 0.000 | -0.525** | 11.797 | 10.585 |
| | (0.00) | (0.25) | (9.77) | (9.90) |
| **Heckman controls** | | | | |
| Credit relation (t-1) | 2.929*** | | | |
| | (0.01) | | | |
| IMR | | 60.981*** | 61.814*** | 61.870*** |
| | | (2.03) | (2.04) | (2.04) |
| Observations | 2,545,319 | 655,517 | 655,493 | 655,493 |
| Bank class controls | Yes | Yes | No | No |
| Lender & borrower FEs | No | No | Yes | Yes |
| Time FEs | No | No | No | Yes |
| R-squared | 0.83 | 0.14 | 0.15 | 0.15 |

Standard errors (twoway clustered by borrower and lender) in parentheses

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

**Table 3.6:** The impact of portfolio quality, NPL ratio, and portfolio opacity on interbank lending for different values of similarity (marginal effects)

This table reports marginal effects for the regression reported in Table 3.5. "Low similarity" refers to a similarity of 3 standard deviation below the variable mean, "high similarity" refers to a similarity of 3 standard deviations above the variable mean. All variables are defined in Appendix 3A.

| | Probit **Credit relation** | OLS **Δ Exposure** | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Portfolio quality (both similarities low) | -0.008*** | -5.629*** | -3.706*** | -3.410*** |
| | (0.00) | (1.65) | (0.87) | (1.00) |
| Portfolio quality (industry dissimilar, region similar) | -0.003*** | -1.441 | -0.728 | -0.346 |
| | (0.00) | (1.65) | (0.93) | (1.05) |
| Portfolio quality (industry similar, region dissimilar) | -0.001 | -0.105 | 0.704 | 0.418 |
| | (0.00) | (1.43) | (1.13) | (1.14) |
| Portfolio quality (both similarities high) | 0.005*** | 4.082*** | 3.682*** | 3.481*** |
| | (0.00) | (1.44) | (1.25) | (1.20) |
| NPL ratio (both similarities low) | -0.006*** | -4.295*** | -2.801*** | -2.296*** |
| | (0.00) | (1.16) | (0.69) | (0.79) |
| NPL ratio (industry dissimilar, locality similar) | -0.001 | -0.884 | -0.395 | -0.076 |
| | (0.00) | (1.37) | (0.87) | (0.85) |
| NPL ratio (industry similar, locality dissimilar) | -0.006*** | -3.867*** | -2.118* | -3.245*** |
| | (0.00) | (1.22) | (1.11) | (1.18) |
| NPL ratio (both similarities high) | -0.001 | -0.456 | 0.287 | -1.025 |
| | (0.00) | (0.95) | (0.97) | (0.95) |
| Portfolio opacity (both similarities low) | -0.004*** | -3.535*** | -2.832*** | -2.913*** |
| | (0.00) | (0.77) | (0.63) | (0.67) |
| Portfolio opacity (industry dissimilar, locality similar) | -0.003*** | -2.277*** | -1.419*** | -1.156** |
| | (0.00) | (0.63) | (0.50) | (0.55) |
| Portfolio opacity (industry similar, locality dissimilar) | 0.000 | 0.032 | 0.076 | -0.141 |
| | (0.00) | (0.81) | (0.69) | (0.77) |
| Portfolio opacity (both similarities high) | 0.002*** | 1.290 | 1.488** | 1.616*** |
| | (0.00) | (0.80) | (0.61) | (0.60) |
| Observations | 2,545,319 | 655,517 | 655,493 | 655,493 |
| Other variables included (see Table 3.5) | Yes | Yes | Yes | Yes |
| Lender & borrower FEs | No | No | Yes | Yes |
| Time FEs | No | No | No | Yes |

Standard errors (twoway clustered by lender and borrower) in parentheses

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

For the interpretation of marginal effects in Table 3.6, note that the variable *Credit relation* assumes either the value 0 or the value 1; a coefficient of 1 in the Probit model (Model (1)), therefore, means an increase in 100 percentage points. The variable $\Delta$ *Exposure*, in contrast, is reported in percentage points; a coefficient of 1 in the OLS model (Model (2) to Model (4)), therefore, means an increase in one percentage point.

Considering only bank pairs with a high level of similarity (Table 3.6, row "Portfolio quality (both similarities high)"), a one standard deviation increase in *Portfolio quality increases* the likelihood to enter a lending relationship by 50 basis points (Model (1)), compared to an unconditional probability of lending of 26.53 percent. Given a high level of similarity, a lending bank thus picks those peers with better asset quality. Similarly, for the intensive margin, bank pairs with a high level of similarity lend 408 basis points more to banks with a better asset quality (Model(2)). Moreover, for similar bank pairs, a deterioration of a bank's loan portfolio is associated with a significant reduction of interbank lending. In particular, a one standard deviation decrease in *Portfolio quality* leads to a 368 basis points decrease in interbank liquidity obtained (Model (3)), or a 348 basis points decrease in interbank liquidity, when controlling for quarter specific effects (Model (4)). These effects are strong, given the average change in bilateral exposure between similar banks of 10.95 percent.

For banks with very different portfolios (Table 3.6, row "Portfolio quality (both similarities low)"), in contrast, the likelihood of entering a lending relationship is 80 basis points lower for a one standard deviation increase in *Portfolio quality* (Model (1)). Similarly, in the intensive margin, the amount lent between dissimilar banks is 563 basis points lower for banks with an additional standard deviation of *Portfolio quality* (Model (2)), compared to an average change in exposure of -2.91 percent for banks with a very different portfolio. For dissimilar bank pairs, lending *increases* after a deterioration of the borrower's *Portfolio quality* by 370, respectively 342 basis points (Model (3) and Model (4)). We show in Section 3.6 that this effect is due to the higher liquidity demand from borrowers with a low *Portfolio quality*.

Like in the previous section, the average bank lends significantly less often and lower amounts to banks with a higher *NPL ratio*. The positive terms for the interaction between *NPL ratio* and *Portfolio similarity* in Table 3.5 and the marginal effects in Table 3.6 show that this effect vanishes for very similar bank pairs: For very similar bank pairs (Table 3.6, row "NPL ratio (both similarities high)", Model (1)), a higher *NPL ratio* is not associated with a significant decrease in the likelihood of entering a lending relationship, compared to a significant decrease of 60 basis points for very dissimilar banks pairs (Table 3.6, row "NPL ratio (both similarities low)", Model(1)). In the intensive margin, the effect is also insignificant for very similar bank pairs: Banks with a higher *NPL ratio* do neither receive less interbank lending from very similar lenders (Model (2)), nor do similar lenders decrease their loans after an increase of their *NPL ratio* (Model(3)) and (Model (4)).

These results support Hypothesis 1. In line with the notion that lending banks with a very similar portfolio can adequately access borrowers' private quality of the loan portfolio, they adjust their lending to the superior, forward-looking information on *Portfolio quality*. Therefore, similar banks need to rely less on the inferior backward looking *NPL ratio*.[17]

*Interbank lending, borrower's portfolio opacity, and portfolio similarity*

Like in Section 3.4, the effect of the borrower's *Portfolio opacity* is negative for a bank pair of average similarity, both at the extensive and the intensive margin. The significantly positive coefficients on the interaction effect between *Portfolio opacity* and the similarity measures reveal that this negative effect becomes weaker, the more similar the portfolio of the lending and borrowing bank: The marginal effects reported in Table 3.6 (row "Portfolio opacity (both similarities high)") show that bank pairs with a similarity level of 3 standard deviations above average are even 23 basis points *more* likely to form a lending relationship with one additional standard deviation of *Portfolio opacity* (Model (1)). They grant 129 basis points *more* loans to banks with a one standard deviation higher *Portfolio opacity* (Model (2)), compared to the average quarterly change in lending between similar banks of 10.95 percent. A bank that becomes less transparent by one standard deviation obtains 149 basis points (Model (3)), respectively 162 (Model (4)) basis points more loans by very similar banks.

These results support Hypotheses 3 and 4. While borrowers with an opaquer portfolio, on average, face difficulties to refinance themselves in interbank markets, interbank lenders "dare to" lend borrowers with an opaque portfolio if this portfolio is similar to their own.

*Interbank lending and portfolio similarity*

Table 3.5 shows that *Portfolio similarity* itself, both its sectoral and regional dimension, has a significantly positive effect at the extensive and intensive margin of lending. Bank pairs with a one standard deviation more similar loan portfolio with respect to industries are 2 basis points more likely to form a lending relationship; banks pairs with a one standard deviation more similar loan portfolio with respect to regions are 14 basis points more likely to form a lending relationship (Model (1)). Compared to the unconditional probability to lend of 26.53 percent, these effects on the extensive margin are rather small, but significant.

[17] So far, one might think that these findings are a pure artifact of the high correlation between similar banks *Portfolio quality*. We show in Section 3.7 that this is not the case.

In the intensive margin, banks with a one standard deviation more similar industry exposure increase their quarterly lending, on average, by 100 basis points, banks with an additional standard deviation of regional similarity by 84 basis points (Model (2)). Increasing *Portfolio similarity* between two banks by one standard deviation increases their granted lending by 259 basis points for sectoral similarity, and by 94 basis points for regional similarity (Model(3)). Controlling for quarter specific shocks, obtained interbank liquidity increases by 211 basis points after a one standard deviation increase of sectoral similarity and by 114 basis points after a one standard deviation increase in regional similarity (Model (4)).

These effect sizes are large, compared to the average quarterly change in interbank lending of 1.46 percent. Coefficients of both similarities for the intensive margin add up to an effect of similar size as relationship lending, the variable identified as the strongest predictor for interbank lending in the literature (Braeuning and Fecht 2017). In other words, a one standard deviation increase in *Portfolio similarity* in regional and sectoral terms increases interbank lending as much as having a one standard deviation longer relationship.

These results support Hypothesis 2. In line with the interpretation that banks with a similar portfolio are well aware about their informational advantage regarding the peer's *Portfolio quality*, they prefer lending to peers with a similar portfolio.

The results also demonstrate that, empirically, positive effects of *Portfolio similarity* dominate potential negative effects outlined in the introduction, i.e. reduced lending out of diversification concerns or reduced lending in the case of correlated liquidity shocks. However, while our results imply that informational advantages are important drivers of preferential lending between similar peers, we cannot rule our that the latter is also driven by risk shifting, i.e. by banks deliberately exposing themselves to banks with correlated risk to increase profits in the case of success and increase the probability of being rescued in case of failure.

*Other variables and quality of the model*

Our findings hold with the inclusion of control variables. Moreover, the coefficients of $Credit\,relation_{i,j,t-1}$ and $IMR_{firststage}$ in Table 3.5 are reassuring that our instrument is not too weak (t = 293).

Table 3.5, however, leaves two questions unanswered: First, why should dissimilar lenders, ceteris paribus, lend *more* to borrowers with a lower *Portfolio quality*? If the lending bank is, indeed, unable to observe the counterparty's *Portfolio quality*, it should not react to this information at all. In the next section, we will separate supply and demand effects to demonstrate that the negative coefficient of *Portfolio quality* for dissimilar banks is a result of a demand effect: Banks with a lower *Portfolio quality* have a greater demand for interbank liquidity, dissimilar lenders satisfy this demand.

A second concern is that the positive interaction effect of *Portfolio similarity* and borrower's *Portfolio quality* could be an artifact of the high correlation of the *Portfolio quality* of banks with a similar portfolio. Our analysis could then misinterpret a lending bank's reaction to a change *in its own Portfolio quality* as a reaction to the *Portfolio quality* of a similar borrower. We will show that this is not the case by running our analysis on a matched sample in Section 3.7.

## 3.6 Ruling out demand effects

We hypothesize that interbank lenders adjust their lending in response to changes in the *Portfolio quality* or *Portfolio opacity* of the borrowing bank. Our theoretical argument thus speaks to supply effects. Empirically, however, we can only observe equilibrium lending, that is, the exposure which the lending and borrowing bank have agreed on. To rule out demand-driven interpretations, this chapter investigates how interbank credit *supply* changes with different levels of the borrowing bank's *Portfolio quality*, *NPL ratio*, and *Portfolio opacity.* Our procedure to identify liquidity supply shocks provides us with borrower-level shocks. As such, the shocks help us to support the supply-based interpretation, but cannot substitute the bank-pair-level analysis from Section 3.5 and 6 as it does not allow us to include bank pair characteristics, like *Portfolio similarity.* In the following, we first explain our approach to disentangle supply effects from the observed equilibrium level of interbank lending and then present our results.

### 3.6.1 Methodological considerations

We identify liquidity supply shocks building on Degryse, Karas, and Schoors (2019). In particular, we borrow the idea that the average credit demand of firms of the same type in the same quarter is a proxy for a firm's credit demand and that supply effects can be estimated with the help of lending bank-time fixed effects.

We start with an adjusted definition of change in bilateral credit exposure $\Delta Exposure'_{i,j,t}$ , which, by limiting the range of values between -2 to 2, incorporates both the extensive and the intensive margin of lending (see Chodorow-Reich (2014); Davis and Haltiwanger (1992)).

$$\Delta Exposure'_{i,j,t} = \frac{Exposure_{i,j,t} - Exposure_{i,j,t-1}}{0.5(Exposure_{i,j,t} + Exposure_{i,j,t-1}} \tag{3.13}$$

To detect the change in interbank exposure attributable to changes in supply, we then regress $\Delta Exposure'_{i,j,t}$ on *lending bank-time fixed effects*, proxying liquidity supply, and *borrowing bank class-industry-region-time fixed effects*, proxying liquidity demand.

We obtain the latter fixed effects by classifying borrowing banks by their bank class, which includes information on the size of the bank (see Table 3.1), their industry focus, classified by the first digit of *WZ 73*, and their regional focus, classified by the first digit of the zip code. We proxy industry and regional focus by the industry/region to which the bank lends most in a given quarter. We exclude loans to the financial and the public sector, as those represent the highest share of loans for almost every bank.

$$\Delta Exposure'_{i,j,t} = FE_{j,t} + FE_{class_j,industry_j,region_j,t} + \epsilon_{i,j,t} \tag{3.14}$$

Assuming that a borrowing bank's liquidity demand is homogeneous across lending banks, the inclusion of $FE_{class,industry,region,t}$ deducts all changes in $\Delta Exposure'_{i,j,t}$ attributable to changes in demand of borrowing bank $j$.[18] $FE_{i,t}$ then accounts for time-specific changes in liquidity supply of lending bank $i$. In contrast to most other fixed effects regressions, we are interested in the effect sizes of $FE_{i,t}$, as they depict the actual changes in liquidity supply. In practice, we estimate fixed effects by including bank-time dummies for all but one bank. $FE_{i,t}$ is therefore fixed to zero for the omitted bank. To obtain comparable values for liquidity supply shocks for all banks, which we can later aggregate on the borrowing bank level, we deduct the time-specific mean from the estimate:

$$F\tilde{E}_{i,t} = F\hat{E}_{i,t} - F\bar{E}_t \tag{3.15}$$

We aggregate the liquidity supply shock experienced by borrowing bank $j$ from all its $I$ lenders at quarter $t$ to obtain:

$$\Delta Liquidity\ supply_{j,t} = \sum_{i \in I} F\tilde{E}_{i,t} \tag{3.16}$$

Note that, here, our intuition deviates from Degryse, Karas, and Schoors (2019). While Degryse, Karas, and Schoors (2019) aim to identify a credit supply shock which is exogenous to a borrower, we are interested in whether this credit supply shock depends on the solvency of different borrowers the bank has lent to in the interbank market. Therefore, Equation (3.16) aggregates shocks of lending banks on the level of the borrowing bank. The shock experienced by a borrowing bank consequently depends on the liquidity provision of its lenders. To assess if lenders' change in liquidity provision depends on the borrower's *Portfolio quality*, *NPL ratio* and *Portfolio opacity*, we estimate the following regression:

[18] As a borrowing bank should not care about which other bank provides them with liquidity, as long as they offer the same conditions, the assumption of homogeneous demand is reasonable in our setting.

$$\begin{aligned}\Delta\, Liquidity\ supply_{j,t} = \beta_0 + \beta_1 Portfolio\ quality_{j,t} + \beta_2 NPL\ ratio_{j,t} \\ + \beta_3 Portfolio\ opacity_{j,t} + Controls + FE_j + \epsilon_{j,t}\end{aligned} \tag{3.17}$$

### 3.6.2 Results

Table 3.7 reports the results of regressing changes in interbank supply on characteristics of the borrowing bank based on Equation (3.17). Like in previous tables, all explanatory variables are standardized for comparability. As the dependent variable is constructed in such a way to include both the extensive and the intensive margin of lending, we cannot interpret effect sizes in a meaningful way and will only interpret direction and significance of the coefficient.[19]

Disentangling supply effects reveals that, on average, liquidity supply is restricted when banks' *Portfolio quality* deteriorates and when their *NPL ratio* increases. Borrowers receive also less liquidity after their portfolio gets opaquer. These results show that, from a borrowing bank's perspective, a deteriorated loan portfolio actually reduces access to interbank market liquidity.

The results also reveal that the negative impact of *Portfolio quality* for lending between dissimilar banks reported in previous regressions (Table 3.4 and Table 3.5) is driven by demand effects: Banks with a lower or lowered *Portfolio quality* demand more interbank loans. However, they face difficulties receiving these loans, because they are shun by lenders with a similar portfolio. Consequently, they turn to dissimilar lenders, resulting in a negative association between *Portfolio quality* and interbank lending for dissimilar bank pairs.

The reverse is true for borrowers with an opaque portfolio: Opaque borrowers receive fewer and smaller loans as dissimilar lenders do not like to lend to them. To circumvent these constraints, opaque banks turn to their similar peers to obtain interbank liquidity.

[19] As we can only interpret $\Delta$ *Liquidity supply* at the level of a bank over time, we do not report the model without borrowing bank fixed effects.

**Table 3.7:** Interbank lending supply and borrower's solvency

This table shows the coefficients from an OLS regression of the change in liquidity supply on characteristics of the borrowing bank. The sample consists of 2054 banks between 2009 and 2018. Liquidity supply shocks are calculated following Degryse, Karas, and Schoors (2019), controlling for the extensive margin of lending. *Change in liquidity supply* is estimated following Equation (3.16). All other variables are defined in Appendix 3A.

| | Dependent variable: Change in liquidity supply |
|---|---|
| Portfolio quality | 0.034* |
| | (0.020) |
| NPL ratio | -0.318*** |
| | (0.019) |
| Portfolio opacity | -0.073*** |
| | (0.011) |
| Capital ratio (t-1) | 0.211*** |
| | (0.024) |
| Liquidity ratio (t-1) | 0.244*** |
| | (0.029) |
| ROA (t-1) | 0.024 |
| | (0.019) |
| Loans-to-assets (t-1) | -0.026 |
| | (0.039) |
| Size (t-1) | 1.267*** |
| | (0.145) |
| Observations | 115,968 |
| Borrower FEs | Yes |
| R-squared | 0.73 |

Standard errors in parentheses

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

## 3.7 Does the portfolio quality of lending banks drive our results?

Banks with a similar portfolio will also have a similar *Portfolio quality*.[20] A bank that reduces lending as a response to the deterioration of its own portfolio could thus appear to react on the deterioration of the portfolio of a similar peer. To rule out that the lender's reaction on its own *Portfolio quality* is driving our results, we rerun our analyses from Section 3.6 on a matched subsample of our data. In this subsample, we force the correlation between lender's and borrower's *Portfolio quality* to being independent of *Portfolio similarity*. In the following, we first describe our matching strategy and then report our results.

### 3.7.1 Matched sample

To force the correlation between lender's and borrower's *Portfolio quality* to being independent of *Portfolio similarity*, we create a subsample of our sample, in which the within-pair correlation of *Portfolio quality* is at a comparable level for similar and dissimilar banks pairs. If, in fact, banks only reacted to their own *Portfolio quality*, coefficients on the interaction between our similarity measures and *Portfolio quality* should be insignificant for this sample.

To create the matched sample, for each bank pair, we first determine the correlation between *Portfolio quality* of the borrower and lender over time. We then define bank pairs to be "similar", if their similarity measure is higher than the 75th percentile for both sectoral and regional similarity in the first quarter of 2009. We classify bank pairs as "dissimilar" if their similarity measure is lower than the 25th percentile for both sectoral and regional similarity in the first quarter of 2009. We then select our subsample by nearest-neighbour matching: To each "similar" bank pair, we assign those three "dissimilar" bank pairs which have the closest value for the correlation in *Portfolio quality*. We keep only the matched pairs in our sample and exclude banks for which we do not find an adequate match. As the sample consists only of very similar and very dissimilar bank pairs, we redefine similarity as a binary variable, which is 1 for "similar" and 0 for "dissimilar" banks. Appendix 3D reports details on our matched sample and on our matching success. We run all analyses described in Section 3.5 on the matched sample.

[20] In our sample, the correlation of *Portfolio quality* of two banks with an above-average level of similarity is 0.0499, while the correlation of *Portfolio quality* of two banks with a below-average level of similarity is only 0.0150.

### 3.7.2 Results

Table 3.8 presents the results of the Heckman sample selection model from Equation (3.11) (Model (1)) and Equation (3.12) (Models (2) to (4)) on our matched sample. Model (1) estimates the likelihood of forming a relationship based on our variables of interest, Model (2) estimates the additional loan granted between bank pairs, Model (3) includes lending and borrowing bank fixed effect in this estimation, and Model (4) adds quarter-year effects. To be able to compare coefficient sizes, all independent variables, except for binary variables, are standardized. Table 3.9 reports the marginal effects of the regression.

As our sample is very selective and only entails a non-random fraction of the variation in *Portfolio similarity* and *Portfolio quality*, our interpretation focuses on interaction effects and ignores base effects. Moreover, due to the non-randomness of our sample, we do not interpret coefficient sizes.

Table 3.8 and 9 show that the effect reported in the previous sections is also present in the matched sample: Even for the subset of bank pairs for which the similarity level does not imply anything for the correlation between the lender's and the borrower's *Portfolio quality*, the interaction term between the different measures of similarity and *Portfolio quality* is positive and mostly significant, so is the interaction term between the different measures of portfolio similarity and *NPL ratio*. Like in previous regressions, the interaction between the different measures of similarity and *Portfolio opacity* is also positive, though not always significantly. However, the non-significant coefficients in Model (3) and (4) are of comparable size to our coefficients in Table 3.5, indicating that the lower significance is mainly a consequence from the smaller sample size.

These results are reassuring regarding our previous interpretation: Similar banks avoid lending to low *Portfolio quality* borrowers; these borrowers turn to dissimilar banks. Dissimilar banks avoid lending to high NPL and opaque borrowers; these borrowers turn to similar banks.

**Table 3.8:** Interbank lending, portfolio similarity, and portfolio quality (matched sample)

This table shows the coefficients of a two-stage Heckman sample selection model. The sample consists of quarterly bank-pair observations of 2054 banks between 2009 and 2018. The dependent variables are the existence of a loan between lender $i$ and borrower $j$ at end-of-quarter $t$ (Model 1, Probit), and the percentage change of interbank exposure between lender $i$ and borrower $j$ over the period (t-1) to t, respectively (Model 2 to 4, OLS). Model (3) includes lender and borrower fixed-effects, model (4) includes lender, borrower, and time fixed-effects. Coefficients are standardized except for binary variables. Standard errors are clustered on the borrower and lender level. All variables are defined in Appendix 3A.

| | Probit **Credit relation** | OLS **Δ Exposure** | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| **Common characteristics** | | | | |
| Portfolio similarity (industries) | -0.005 | 0.037 | 1.048 | 0.539 |
| | (0.01) | (0.46) | (0.73) | (0.65) |
| Portfolio similarity (regions) | 0.050*** | 1.398*** | 1.490** | 1.390** |
| | (0.01) | (0.41) | (0.59) | (0.56) |
| **Borrower characteristics** | | | | |
| Portfolio quality | -0.054*** | -1.007 | 0.007 | -0.368 |
| | (0.01) | (0.72) | (0.88) | (0.95) |
| Portfolio quality × Portfolio similarity (industries) | 0.043*** | 1.257*** | 0.850* | 0.994** |
| | (0.01) | (0.36) | (0.49) | (0.44) |
| Portfolio quality × Portfolio similarity (regions) | 0.011 | 1.081** | 0.650 | 0.597 |
| | (0.01) | (0.46) | (0.55) | (0.53) |
| NPL ratio | -0.068*** | -2.195*** | -1.327* | -2.407*** |
| | (0.01) | (0.48) | (0.73) | (0.64) |
| NPL ratio × Portfolio similarity (industries) | 0.015** | 0.631* | 0.422 | 0.162 |
| | (0.01) | (0.34) | (0.43) | (0.40) |
| NPL ratio × Portfolio similarity (regions) | 0.026** | 1.074** | 0.780 | 0.515 |
| | (0.01) | (0.45) | (0.51) | (0.50) |
| Portfolio opacity | 0.006 | 0.147 | 0.198 | 0.180 |
| | (0.01) | (0.49) | (0.46) | (0.54) |
| Portfolio opacity × Portfolio similarity (industries) | 0.030*** | 0.708** | 0.540 | 0.601 |
| | (0.01) | (0.31) | (0.38) | (0.40) |
| Portfolio opacity × Portfolio similarity (regions) | 0.011 | 0.330 | 0.478 | 0.506 |
| | (0.01) | (0.59) | (0.69) | (0.68) |
| Capital ratio (t-1) | -0.036*** | -1.380*** | -1.819*** | -0.342 |
| | (0.01) | (0.48) | (0.69) | (0.75) |
| Liquidity ratio (t-1) | 0.009 | -1.235* | 3.722 | 2.093 |
| | (0.01) | (0.72) | (7.43) | (7.28) |
| ROA (t-1) | 0.054*** | 2.396*** | 3.154*** | 2.672*** |
| | (0.01) | (0.41) | (0.72) | (0.78) |
| Loans-to-assets (t-1) | 0.078*** | 2.563*** | 2.203 | 3.354* |
| | (0.01) | (0.58) | (1.48) | (1.72) |
| Size (t-1) | 0.220*** | 2.846*** | 5.041 | 7.460 |
| | (0.02) | (0.93) | (9.35) | (8.83) |

| | | | | |
|---|---|---|---|---|
| **Lender characteristics** | | | | |
| Portfolio quality | 0.017 | 1.241** | 1.335* | 1.421* |
| | (0.01) | (0.53) | (0.79) | (0.83) |
| NPL ratio | -0.012 | -0.073 | 1.467** | 0.111 |
| | (0.01) | (0.41) | (0.74) | (0.76) |
| Portfolio opacity | 0.003 | 0.382* | 0.433* | 0.717*** |
| | (0.01) | (0.22) | (0.23) | (0.26) |
| Capital ratio (t-1) | -0.086*** | -1.976*** | -3.795*** | -1.452 |
| | (0.01) | (0.48) | (1.00) | (1.07) |
| Liquidity ratio (t-1) | 0.001 | -1.635** | -7.023 | -7.007 |
| | (0.01) | (0.64) | (7.89) | (7.72) |
| ROA (t-1) | 0.021** | 0.863*** | 0.651 | -0.304 |
| | (0.01) | (0.33) | (0.54) | (0.56) |
| Loans-to-assets (t-1) | -0.112*** | -2.382*** | -0.270 | 2.070 |
| | (0.01) | (0.48) | (2.00) | (1.86) |
| Size (t-1) | 0.011 | -1.150* | -16.750** | -10.899* |
| | (0.02) | (0.67) | (6.60) | (6.35) |
| **Relationship characteristics** | | | | |
| Relationship lending | 0.420*** | 5.418*** | 3.558*** | 3.788*** |
| | (0.01) | (1.12) | (0.99) | (1.00) |
| Reverse relationship lending | 0.068*** | 1.152** | 2.396*** | 2.326*** |
| | (0.01) | (0.50) | (0.61) | (0.61) |
| $\Delta$ Reverse exposure | 0.025*** | 2.292*** | 2.224*** | 2.128*** |
| | (0.01) | (0.57) | (0.56) | (0.54) |
| Same network | 0.391*** | 6.468*** | 5.961** | 6.040** |
| | (0.03) | (1.65) | (2.81) | (2.81) |
| Same BHC | 0.548*** | 12.989** | 6.708 | 6.791 |
| | (0.18) | (5.18) | (4.27) | (4.19) |
| Difference in liquidity surplus (t-1) | 0.026*** | -0.053 | 2.116 | 1.776 |
| | (0.01) | (0.48) | (3.39) | (3.31) |
| **Heckman controls** | | | | |
| Credit relation (t-1) | 2.883*** | | | |
| | (0.03) | | | |
| IMR | | 61.691*** | 64.628*** | 64.614*** |
| | | (2.96) | (3.02) | (3.02) |
| Observations | 226,190 | 69,509 | 69,452 | 69,452 |
| Bank class controls | Yes | Yes | No | No |
| Lender & borrower FEs | No | No | Yes | Yes |
| Time FEs | No | No | No | Yes |
| R-squared | 0.84 | 0.12 | 0.14 | 0.14 |

Standard errors (twoway clustered by lender and borrower) in parentheses

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

**Table 3.9:** The effect of portfolio quality, NPL ratio, and portfolio opacity on interbank lending for different values of similarity (marginal effects, matched sample)

This table reports marginal effects for the regression on our matched sample reported in Table 3.8. "Low similarity" refers to a similarity of 3 standard deviation below the variable mean, "high similarity" refers to a similarity of 3 standard deviations above the variable mean. All variables are defined in Appendix 3A.

| | Probit **Credit relation** | OLS **Δ Exposure** | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Portfolio quality (both similarities low) | -0.011*** | -8.021*** | -4.492** | -5.141*** |
| | (0.00) | (1.34) | (1.84) | (1.59) |
| Portfolio quality (industry dissimilar, locality similar) | -0.009*** | -1.532 | -0.591 | -1.562 |
| | (0.00) | (1.97) | (2.37) | (2.35) |
| Portfolio quality (industry similar, locality dissimilar) | 0.002 | -0.481 | 0.606 | 0.825 |
| | (0.00) | (2.29) | (2.54) | (2.57) |
| Portfolio quality (both similarities high) | 0.006*** | 6.008*** | 4.506* | 4.404* |
| | (0.00) | (1.80) | (2.64) | (2.42) |
| NPL ratio (both similarities low) | -0.010*** | -7.308*** | -4.932*** | -4.436*** |
| | (0.00) | (1.26) | (1.33) | (1.29) |
| NPL ratio (industry dissimilar, locality similar) | -0.002** | -0.867 | -0.251 | -1.348 |
| | (0.00) | (2.04) | (2.38) | (2.27) |
| NPL ratio (industry similar, locality dissimilar) | -0.005** | -3.524 | -2.403 | -3.466 |
| | (0.00) | (2.16) | (2.57) | (2.41) |
| NPL ratio (both similarities high) | 0.003 | 2.918** | 2.279 | -0.378 |
| | (0.00) | (1.46) | (2.05) | (1.92) |
| Portfolio opacity (both similarities low) | -0.006*** | -2.968** | -2.855 | -3.139 |
| | (0.00) | (1.42) | (1.87) | (1.95) |
| Portfolio opacity (industry dissimilar, locality similar) | -0.003 | -0.987 | 0.012 | -0.105 |
| | (0.00) | (2.34) | (2.71) | (2.73) |
| Portfolio opacity (industry similar, locality dissimilar) | 0.003* | 1.281 | 0.384 | 0.466 |
| | (0.00) | (2.73) | (3.13) | (3.12) |
| Portfolio opacity (both similarities high) | 0.007*** | 3.262** | 3.251** | 3.499** |
| | (0.00) | (1.33) | (1.55) | (1.58) |
| Observations | 226,190 | 69,509 | 69,452 | 69,452 |
| Other variables included (see Table 3.8) | Yes | Yes | Yes | Yes |
| Lender & borrower FEs | No | No | Yes | Yes |
| Time FEs | No | No | No | Yes |

Standard errors (twoway clustered by lender and borrower) in parentheses

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

## 3.8 Decomposition of explanatory power

From our analyses, we conclude that *Portfolio similarity* is an important determinant for forming interbank lending relationships and for the size of interbank loans. In contrast to the existing literature, which focuses on characteristics of the lender, the borrower, their relationship or on market factors, we thereby draw the attention to *common characteristics* of the lending and borrowing bank. To put this novelty into perspective, we provide an estimate of the relative importance of the different factors determining lending patterns. Similar to Lemmon, Roberts, and Zender (2008), we decompose the variation in interbank lending attributable to lender characteristics, borrower characteristics, bank-pair (i.e. common and relationship) characteristics and market characteristics.

### 3.8.1 Methodological considerations

We use analysis of covariance (ANCOVA) to decompose the variation in lending attributable to different factors. We do so by estimating the following equations capturing the extensive and intensive margin of interbank lending:

$$\begin{aligned}
&Credit\,relation_{i,j,t} = \beta_0 + \\
&\quad \beta_1 Lender\ characteristics\ (varying)_{i,t} + \beta_2 Lender\ characteristics\ (fixed)_i + \\
&\quad \beta_3 Borrower\ characteristics\ (varying)_{j,t} + \beta_4 Borrower\ characteristics\ (fixed)_j + \\
&\quad \beta_5 Bank\ pair\ characteristics\ (varying)_{i,j,t} + \beta_6 Bank\ pair\ characteristics\ (fixed)_{i,j} + \\
&\quad \beta_7 Market\ characteristics_t + \epsilon_{i,j,t}
\end{aligned} \tag{3.18}$$

$$\begin{aligned}
&\Delta Exposure_{i,j,t} = \beta_0 + \\
&\quad \beta_1 Lender\ characteristics\ (varying)_{i,t} + \beta_2 Lender\ characteristics\ (fixed)_i + \\
&\quad \beta_3 Borrower\ characteristics\ (varying)_{j,t} + \beta_4 Borrower\ characteristics\ (fixed)_j + \\
&\quad \beta_5 Bank\ pair\ characteristics\ (varying)_{i,j,t} + \beta_6 Bank\ pair\ characteristics\ (fixed)_{i,j} + \\
&\quad \beta_7 Market\ characteristics_t + \epsilon_{i,j,t}
\end{aligned} \tag{3.19}$$

where:

- $Lender\ characteristics\ (varying)_{i,t}$ include the lending bank's *Portfolio quality*, *NPL ratio*, *Portfolio opacity*, lagged *Capital ratio*, lagged *Liquidity ratio*, lagged *ROA*, lagged *Loans-to-assets*, and lagged *Size*.

- $Lender\ characteristics\ (fixed)_i$ include the lending bank's *Bank class* and lender fixed effects.

- $Borrower\ characteristics\ (varying)_{j,t}$ include the borrowing bank's *Portfolio quality*, *NPL ratio*, *Portfolio opacity*, lagged *Capital ratio*, lagged *Liquidity ratio*, lagged *ROA*, lagged *Loans-to-assets*, and lagged *Size*.

- $Borrower\ characteristics\ (fixed)_j$ include the borrowing bank's *Bank class* and borrower fixed effects.

- $Bank\ pair\ characteristics\ (varying)_{i,j,t}$ include the variables *Portfolio similarity (industries)*, *Portfolio similarity (regions)*, *Relationship lending*, *Reverse relationship lending*, *Δ reverse exposure*, *Difference in liquidity surplus.*

- $Bank\ pair\ characteristics\ (fixed)_{i,j}$ include the dummies if banks are part of the same bank network and/or part of the same bank holding company.

- $Market\ characteristics_t$ are quarter-year fixed effects.

We obtain the fraction of the model sum of squares attributable to the each variable like follows: First, we divide the Type III partial sum of squares of this variable by the aggregate partial sum of squares for all variables to calculate the fraction of *total variance in lending* attributable to each variable.[21] Second, we scale this number by the fraction of overall variance explained by our model to obtain the variable's contribution to total *explained variance* by our model. We then aggregate the variables into varying and fixed lender, borrower, bank-pair and market fixed effects.[22]

### 3.8.2 Results

Table 3.10 presents the results of the variance decomposition for the extensive and intensive margin of interbank lending. The rows of the table, except for the last row, correspond to the fraction of Type III partial sum of squares for different model specifications. Intuitively, the table shows the fractions of the model sum of squares

[21] Following Lemmon, Roberts, and Zender (2008), we use Type III sum of squares as Type I sum of squares is sensitive to the variable's order (Scheffé 1959).

[22] For market characteristics, we do not distinguish between varying and fixed effects as they change over time per definition.

attributable to the different "characteristics", i.e. borrower, lender, market and bank-pair characteristics. The last row of Table 3.10 presents the adjusted R-square of each specification. For example, to explain the extensive margin of interbank lending, in the model without fixed effects, about 0.14% (=0.09%+0.05%) of the variation in interbank lending is attributable to lender characteristics (in network terms: ego covariates), about 0.25% are attributable to borrower characteristics (alter covariates), and about 99.60% are attributable to bank pair characteristics (dyadic covariates).

The results of the partitioning corroborate the relevance of common characteristics and relationship characteristics for interbank lending. When determining who enters an interbank lending relation at all (Panel (A)), more than 97% of the variation can be explained by bank pair characteristics. The explanatory power comes almost exclusively from varying bank-pair characteristics, like relationship lending or *Portfolio similarity*, and only marginally from fixed characteristics, like having the same bank holding company or being part of the same banking network. Market, lender, and borrower characteristics *together* are responsible for less that 3% of the variation. Out of this fraction, borrower characteristics are most important, explaining a between 0.25 and 1.18% of the total explained variation. From the characteristics of the lending and borrowing bank, fixed determinants, captured by bank fixed effects, are more relevant than varying determinants, like the bank's *Capital ratio* or other balance-sheet based variables.

For the variation in the size of interbank loans (Panel (B)), the characteristics of the lending and borrowing bank are more decisive: Between 21 and 44% of the variation in credit amounts can be traced back to lender characteristics, between 29 and 36% to borrower characteristics. Fixed bank characteristics captured by the included fixed effects are significantly more relevant than varying bank characteristics captured by the different balance sheet variables. At the intensive margin, market characteristics captured by the quarter-year fixed effects explain about 9% of the total explained variation. With fractions of explained variance ranging between 19 and 51%, bank pair characteristics also explain a considerable fraction of interbank loan sizes.

Interestingly, the explained variance for the extensive margin is considerably higher than for the intensive margin. Variables of our model, including the fixed effects, seem to be much better in explaining which bank-pair forms a credit relation than in explaining how much additional credit is granted.

These results are re-assuring, both for our analysis and for the focus of the recent literature: When trying to explain interbank lending patterns, relationship characteristics - the focus of recent studies - and common characteristics - the focus of our study - do, indeed, matter most.

**Table 3.10:** Variance decompositon of interbank lending

This table presents a variance decomposition for several different model specifications of the extensive and intensive margin of interbank lending, with adjusted R -squares at the bottom. We compute the Type III partial sum of squares for each effect in the model and then normalize each estimate by the sum across the effects, forcing each column to sum to one. For example, at the extensive margin (Panel A) with all fixed effects (last column), 0.09% of the explained sum of squares captured by the included covariates can be attributed to macroeconomic shocks. Firm FE are firm fixed effects. Time FE are quarter fixed effects (c.f. Lemmon, Roberts, and Zender (2008))

**Panel A**: Extensive margin

| | Model without FE | Borrower & lender FE | Borrower, lender & time FE |
|---|---|---|---|
| **Lender characteristics (Ego covariates)** | | | |
| Varying characteristics | 0.09% | 0.03% | 0.02% |
| Fixed characteristics | 0.05% | 0.77% | 0.77% |
| **Borrower characteristics (Alter covariates)** | | | |
| Varying characteristics | 0.17% | 0.05% | 0.06% |
| Fixed characteristics | 0.08% | 1.13% | 1.12% |
| **Bank-pair characteristics (Dyadic covariates)** | | | |
| Varying characteristics | 99.56% | 97.98% | 98.01% |
| Fixed characteristics | 0.04% | 0.02% | 0.02% |
| **Market characteristics (Network covariates)** | | | 0.09% |
| **Adj. R-squared** | 75.33% | 75.98% | 76.01% |

**Panel B**: Intensive margin

| | Model without FE | Borrower & lender FE | Borrower, lender & time FE |
|---|---|---|---|
| **Lender characteristics (Ego covariates)** | | | |
| Varying characteristics | 13.19% | 3.72% | 4.05% |
| Fixed characteristics | 7.77% | 39.62% | 40.23% |
| **Borrower characteristics (Alter covariates)** | | | |
| Varying characteristics | 14.89% | 1.16% | 1.16% |
| Fixed characteristics | 13.89% | 34.96% | 35.40% |
| **Bank-pair characteristics (Dyadic covariates)** | | | |
| Varying characteristics | 47.71% | 20.26% | 18.89% |
| Fixed characteristics | 2.54% | 0.29% | 0.27% |
| **Market characteristics (Network covariates)** | | | 9.11% |
| **Adj. R-squared** | 0.68% | 1.06% | 1.19% |

## 3.9 Conclusion

By allowing banks to manage, pool and redistribute funds, the interbank market allocates liquidity around the financial system and provides insurance against idiosyncratic liquidity shocks. It serves as an important transmission channel of monetary policy. Understanding the mechanisms within this market is thus of central importance for prudential regulation and adequate monetary policy.

This paper builds on research on banks' ability to monitor peers, adding a further puzzle piece to our understanding of the interbank market. It reconciles two seemingly opposing positions: On the one hand, we confirm that peer monitoring works: A large fraction of lending banks reacts to a deterioration of the counterparty's asset quality, even though this information is private. On the other hand, we confirm that peer-monitoring fails under asymmetric information: A just as large fraction of lending banks proves unable to react to private information on the deterioration of the counterparty's asset quality. These banks substitute private, forward-looking measures on the borrowing bank's asset quality by inferior, backward-looking, but publicly available measures.

Most importantly, we shed light on which banks have access to private information on the counterparty, and which do not. We show that the ability for effective peer-monitoring is restricted to similar bank pairs, that is, banks with a similar loan portfolio. This reveals a new channel of information generation in interbank markets: Banks use private information about their own portfolio to assess a peer in the interbank market. Given the superior information on peers with a similar loan portfolio, credit relations between similar banks are more frequent and involve larger sums.

Preferential lending between banks with a similar real exposure is paralleled by a lack of diversification and, consequently, induces risks to financial stability (Silva, Alexandre, and Tabak 2017; Silva, da Silva, and Tabak 2017). Our findings reveal trade-offs at both the micro and the macro level: From a lending bank's perspective, lending to a similar institution is associated with a better-informed risk-assessment. However, lending to a bank that is already exposed to similar industries and regions impedes portfolio diversification. From a market and societal perspective, lending between similar counterparties increases informational efficiency and monitoring in interbank markets. At the same time, the above-average direct interbank exposure between banks with a similar real exposure could multiply systemic risks and too-interconnected-to-fail concerns.

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## Chapter III

# Appendix

## 3.A Variable descriptions and sources

| Variable | Definition | Source |
|---|---|---|
| **Panel A: Bank-quarter level** | | |
| Portfolio quality | 1 - portfolio-weighted average of borrowers' probability of default, see Equation (3.2) | Bundesbank credit register |
| NPL ratio | Non-performing loans/Total loans outstanding | Bundesbank monthly balance sheet statistics |
| Portfolio opacity | Portfolio-weighted standard deviation of borrowers' probabilities of default, see Equation (3.3) | Bundesbank credit register |
| Capital ratio | Equity/Risk-weighted assets | Bundesbank monthly balance sheet statistics |
| Liquidity ratio | Liquid assets/Total assets | Bundesbank monthly balance sheet statistics |
| ROA | Return on risk-weighted assets | Bundesbank monthly balance sheet statistics |
| Loans-to-assets | Loans/Total assets | Bundesbank monthly balance sheet statistics |
| Size | Log total assets | Bundesbank monthly balance sheet statistics |
| **Panel B: Bank level** | | |
| Bank class | Dummy for each of the bank classes listed in Table 3.1 | Bundesbank monthly balance sheet statistics |
| **Panel C: Bank-pair-quarter level** | | |
| Credit relation | Binary variable that is one if there is outstanding credit between lending and borrowing bank at the end of the quarter | Bundesbank credit register |
| $\Delta$ Exposure | Percentage change in credit amount from lending to borrowing bank, see Equation (3.1) | Bundesbank credit register |
| $\Delta$ Reverse exposure | Percentage change in credit amount from borrowing to lending bank, see Equation (3.1) | Bundesbank credit register |
| Portfolio similarity (industries) | Cosine similarity between credit exposures of lending and borrowing bank to 10 different industries, see Equation (3.4) | Bundesbank credit register |
| Portfolio similarity (industries, fine) | Cosine similarity between credit exposures of lending and borrowing bank to 100 different industries, see Equation (3.4) | Bundesbank credit register |
| Portfolio similarity (regions) | Cosine similarity between credit exposures of lending and borrowing bank to 9 different regions, see Equation (3.5) | Bundesbank credit register |
| Relationship lending | Logged sum of quarters out of the last 8 quarters in which the lending bank has lent to the borrowing bank, see Equation (3.6) | Bundesbank credit register |
| Reverse relationship lending | Logged sum of quarters out of the last 8 quarters in which the borrowing bank has lent to the lending bank, see Equation (3.7) | Bundesbank credit register |
| Difference in liquidity surplus | Difference between lender's abnormal liquidity and borrower's abnormal liquidity, see Equation (3.8) | Bundesbank monthly balance sheet statistics |
| IMR | Inverse Mill's ratio calculated from the first-stage Probit regression | 1st-stage Probit regression |
| **Panel D: Bank-pair level** | | |
| Same BHC | Binary variable indicating if lending and borrowing bank are part of the same bank holding company | Bundesbank monthly balance sheet statistics |
| Same network | Binary variable indicating if lending and borrowing bank are part of the same bank network | Bundesbank monthly balance sheet statistics |

## 3.B Relationship between portfolio quality and NPL ratio

**Table 3.B1:** Regression of first differences of portfolio quality on NPL ratio, and NPL ratio on portfolio quality

This table shows coefficients from OLS regressions of a bank's *NPL ratio* on its *Portfolio quality* and vice versa, both in first differences. Standard errors are clustered on the bank level and shown in parenthesis. The sample consists of quarterly bank observations of 2054 banks between 2009 and 2018. Regressions in column (2) and (4) include bank fixed effects. Appendix 3A provides a detailed variable description.

| | Dependent variable: NPL ratio (first difference) | | Dependent variable: Portfolio quality (first difference) | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Portfolio quality (first difference) | 0.002 | 0.004 | | |
| | (0.00) | (0.00) | | |
| NPL ratio (first difference) | | | 0.006 | 0.009 |
| | | | (0.01) | (0.01) |
| Observations | 62,390 | 62,388 | 62,390 | 62,388 |
| Bank FEs | No | Yes | No | Yes |

Standard (clustered on bank level) errors in parentheses
$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

## 3.C Finer measure of portfolio similarity

This Appendix reports the analyses from Table 3.5, Table 3.6, Table 3.8 and Table 3.9 with the finer measure of sectoral portfolio similarity. Instead of using 10 different industries in calculating the similarity measure of Equation (3.4), we use 100 industries here.

**Table 3.C1:** Interbank lending, portfolio similarity (fine), and portfolio quality

This table shows the coefficients of a two-stage Heckman sample selection model on a matched sample. The sample consists of quarterly bank-pair observations of 2054 banks between 2009 and 2018. Bank pairs with a high-similarity in both regional and sectoral terms are matched to bank pairs of similar correlation between the lender's and the borrower's credit portfolio quality but with a low similarity both regional and sectoral terms. The dependent variables are the existence of a loan between lender $i$ and borrower $j$ at end-of-quarter $t$ (Model 1, Probit), and the percentage change of interbank exposure between lender $i$ and borrower $j$ over the period (t-1) to t, respectively (Model 2 to 4, OLS). Model (3) includes lender and borrower fixed-effects, model (4) includes lender, borrower, and time fixed-effects. Coefficients are standardized, except for binary variables. Standard errors are clustered on the borrower and lender level. All variables are defined in Appendix 3A.

| | Probit<br>**Credit relation** | OLS<br>**Δ Exposure** | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| **Common characteristics** | | | | |
| Portfolio similarity (industries, fine) | 0.019*** | 0.711* | 1.859*** | 1.291*** |
| | (0.00) | (0.41) | (0.42) | (0.41) |
| Portfolio similarity (regions) | 0.031*** | 0.864*** | 0.978*** | 1.178*** |
| | (0.00) | (0.23) | (0.20) | (0.21) |
| **Borrower characteristics** | | | | |
| Portfolio quality | -0.057*** | -1.326** | -0.338 | -0.283 |
| | (0.00) | (0.59) | (0.50) | (0.62) |
| Portfolio quality × Portfolio similarity (industries, fine) | -0.003 | 0.026 | 0.327 | 0.058 |
| | (0.00) | (0.47) | (0.26) | (0.25) |
| Portfolio quality × Portfolio similarity (regions) | 0.017*** | 0.713*** | 0.509*** | 0.525*** |
| | (0.00) | (0.20) | (0.14) | (0.14) |
| NPL ratio | -0.066*** | -2.490*** | -1.462*** | -1.761*** |
| | (0.00) | (0.44) | (0.42) | (0.50) |
| NPL ratio × Portfolio similarity (industries, fine) | 0.001 | -0.254 | 0.014 | -0.396 |
| | (0.00) | (0.44) | (0.25) | (0.26) |
| NPL ratio × Portfolio similarity (regions) | 0.016*** | 0.575*** | 0.415*** | 0.385*** |
| | (0.00) | (0.20) | (0.14) | (0.14) |
| Portfolio opacity | -0.023*** | -1.285*** | -0.695*** | -0.687** |
| | (0.00) | (0.31) | (0.24) | (0.30) |
| Portfolio opacity × Portfolio similarity (industries, fine) | 0.016*** | 0.622*** | 0.308** | 0.298** |
| | (0.00) | (0.15) | (0.13) | (0.13) |
| Portfolio opacity × Portfolio similarity (regions) | 0.005** | 0.231 | 0.235* | 0.298** |
| | (0.00) | (0.15) | (0.14) | (0.14) |
| Capital ratio (t-1) | -0.017*** | -0.094 | -1.625** | -0.147 |
| | (0.00) | (0.56) | (0.75) | (0.71) |
| Liquidity ratio (t-1) | -0.004 | -1.300** | 2.400 | 1.384 |
| | (0.00) | (0.65) | (2.50) | (2.53) |

**Table 3.C1:** (continued) Interbank lending, portfolio similarity (fine), and portfolio quality

| | Probit<br>**Credit relation** | OLS<br>**Δ Exposure** | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| ROA (t-1) | 0.102*** | 3.899*** | 2.889*** | 2.446*** |
| | (0.00) | (0.66) | (0.79) | (0.83) |
| Loans-to-assets (t-1) | 0.057*** | 2.121*** | 2.742** | 3.321*** |
| | (0.00) | (0.63) | (1.12) | (0.97) |
| Size (t-1) | 0.207*** | 2.509*** | -0.096 | -1.046 |
| | (0.01) | (0.85) | (6.02) | (5.57) |
| **Lender characteristics** | | | | |
| Portfolio quality | 0.016*** | 0.840*** | 2.169*** | 2.085*** |
| | (0.00) | (0.29) | (0.51) | (0.54) |
| NPL ratio | -0.023*** | -0.448* | 1.382*** | 0.181 |
| | (0.00) | (0.26) | (0.39) | (0.41) |
| Portfolio opacity | 0.016*** | 0.564*** | 0.601*** | 0.790*** |
| | (0.00) | (0.17) | (0.16) | (0.17) |
| Capital ratio (t-1) | -0.091*** | -1.857*** | -2.183** | 0.247 |
| | (0.01) | (0.42) | (1.03) | (1.04) |
| Liquidity ratio (t-1) | 0.022*** | -1.144*** | -5.416** | -6.114** |
| | (0.00) | (0.31) | (2.49) | (2.53) |
| ROA (t-1) | 0.039*** | 1.689*** | 2.330*** | 1.354** |
| | (0.00) | (0.41) | (0.60) | (0.57) |
| Loans-to-assets (t-1) | -0.111*** | -2.919*** | 1.087 | 3.034** |
| | (0.00) | (0.36) | (1.23) | (1.19) |
| Size (t-1) | 0.025*** | -0.707 | -14.586*** | -6.477 |
| | (0.00) | (0.51) | (5.05) | (4.99) |
| **Relationship characteristics** | | | | |
| Relationship lending | 0.358*** | 3.474*** | 3.067*** | 3.198*** |
| | (0.00) | (0.57) | (0.56) | (0.56) |
| Reverse relationship lending | 0.074*** | 1.163*** | 1.527*** | 1.456*** |
| | (0.00) | (0.27) | (0.29) | (0.28) |
| Δ Reverse exposure | 0.019*** | 2.628*** | 2.578*** | 2.528*** |
| | (0.00) | (0.44) | (0.44) | (0.44) |
| Same network | 0.392*** | 9.580*** | 7.598*** | 7.856*** |
| | (0.01) | (1.43) | (1.42) | (1.41) |
| Same BHC | 0.490*** | 13.493*** | 14.544*** | 14.339*** |
| | (0.06) | (2.15) | (2.30) | (2.30) |
| Difference in liquidity surplus (t-1) | 0.000 | -0.518** | 11.592 | 10.358 |
| | (0.00) | (0.25) | (9.75) | (9.88) |
| **Heckman controls** | | | | |
| Credit relation (t-1) | 2.930*** | | | |
| | (0.01) | | | |
| IMR | | 60.962*** | 61.812*** | 61.868*** |
| | | (2.04) | (2.05) | (2.04) |
| Observations | 2,545,319 | 655,517 | 655,493 | 655,493 |
| Bank class controls | Yes | Yes | Yes | Yes |
| Lender & borrower FEs | No | No | Yes | Yes |
| Time FEs | No | No | No | Yes |
| R-squared | 0.83 | 0.14 | 0.15 | 0.15 |

Standard errors (twoway clustered by lender and borrower) in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

**Table 3.C2:** The impact of portfolio quality, NPL ratio, and portfolio opacity on interbank lending for different values of similarity (marginal effects)

This table reports marginal effects for the regression reported in Table 3C.1. "Low similarity" refers to a similarity of 3 standard deviation below the variable mean, "high similarity" refers to a similarity of 3 standard deviations above the variable mean. All variables are defined in Appendix 3A.

| | Probit | OLS | | |
|---|---|---|---|---|
| | **Credit relation** | **Δ Exposure** | | |
| | (1) | (2) | (3) | (4) |
| Portfolio quality (both similarities low) | -0.005*** | -3.542** | -2.848*** | -2.031** |
| | (0.00) | (1.76) | (0.96) | (0.94) |
| Portfolio quality (industry dissimilar, locality similar) | 0.000 | 0.733 | 0.208 | 1.117 |
| | (0.00) | (1.71) | (1.05) | (1.03) |
| Portfolio quality (industry similar, locality dissimilar) | -0.006*** | -3.385** | -0.884 | -1.684 |
| | (0.00) | (1.64) | (1.03) | (1.15) |
| Portfolio quality (both similarities high) | -0.001 | 0.890 | 2.172** | 1.464 |
| | (0.00) | (1.36) | (1.05) | (1.09) |
| NPL ratio (both similarities low) | -0.006*** | -3.453** | -2.748*** | -1.727* |
| | (0.00) | (1.43) | (0.79) | (0.88) |
| NPL ratio (industry dissimilar, locality similar) | -0.001* | -0.004 | -0.257 | 0.583 |
| | (0.00) | (1.64) | (0.93) | (0.96) |
| NPL ratio (industry similar, locality dissimilar) | -0.006*** | -4.977*** | -2.667** | -4.105*** |
| | (0.00) | (1.69) | (1.10) | (1.22) |
| NPL ratio (both similarities high) | -0.001 | -1.528 | -0.176 | -1.796* |
| | (0.00) | (1.32) | (0.97) | (1.01) |
| Portfolio opacity (both similarities low) | -0.004*** | -3.845*** | -2.323*** | -2.476*** |
| | (0.00) | (0.80) | (0.70) | (0.71) |
| Portfolio opacity (industry dissimilar, locality similar) | -0.003*** | -2.459*** | -0.916* | -0.685 |
| | (0.00) | (0.59) | (0.53) | (0.57) |
| Portfolio opacity (industry similar, locality dissimilar) | 0.000 | -0.112 | -0.473 | -0.690 |
| | (0.00) | (0.69) | (0.64) | (0.73) |
| Portfolio opacity (both similarities high) | 0.002*** | 1.274* | 0.934 | 1.101* |
| | (0.00) | (0.72) | (0.61) | (0.58) |
| Observations | 2,545,319 | 655,517 | 655,493 | 655,493 |
| Other variables included (see Table 3C.1) | Yes | Yes | Yes | Yes |
| Lender & borrower FEs | No | No | Yes | Yes |
| Time FEs | No | No | No | Yes |

Standard errors (twoway clustered by lender and borrower) in parentheses

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

**Table 3.C3:** Interbank lending, portfolio similarity, and credit portfolio quality (matched sample)

This table shows the coefficients of a two-stage Heckman sample selection model on a matched sample. The sample consists of quarterly bank-pair observations of 2054 banks between 2009 and 2018. Bank pairs with a high-similarity in both regional and sectoral terms are matched to bank pairs of similar correlation between the lender's and the borrower's *Credit portfolio quality* but with a low similarity both regional and sectoral terms. The dependent variables are the existence of a loan between lender $i$ and borrower $j$ at end-of-quarter $t$ (Model 1, Probit), and the percentage change of interbank exposure between lender $i$ and borrower $j$ over the period (t-1) to t, respectively (Model 2 to 4, OLS). Model (3) includes lender and borrower fixed-effects, model (4) includes lender, borrower, and time fixed-effects. Coefficients are standardized, except for binary variables. Standard errors are clustered on the borrower and lender level. All variables are defined in Appendix 3A.

| | Probit<br>**Credit relation** | OLS<br>**Δ Exposure** | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| **Common characteristics** | | | | |
| Portfolio similarity (industries, fine) | 0.031*** | 1.092*** | 2.197*** | 1.495** |
| | (0.01) | (0.41) | (0.58) | (0.64) |
| Portfolio similarity (regions) | 0.045*** | 1.378*** | 1.518*** | 1.407** |
| | (0.01) | (0.40) | (0.58) | (0.55) |
| **Borrower characteristics** | | | | |
| Portfolio quality | -0.094*** | -2.156*** | -0.792 | -1.252 |
| | (0.01) | (0.72) | (0.80) | (0.87) |
| Portfolio quality<br>× Portfolio similarity (industries, fine) | -0.013 | -0.128 | -0.096 | -0.259 |
| | (0.01) | (0.47) | (0.62) | (0.60) |
| Portfolio quality<br>× Portfolio similarity (regions) | 0.030*** | 1.559*** | 0.900* | 0.915* |
| | (0.01) | (0.43) | (0.52) | (0.50) |
| NPL ratio | -0.067*** | -2.245*** | -1.469** | -2.248*** |
| | (0.01) | (0.49) | (0.66) | (0.60) |
| NPL ratio<br>× Portfolio similarity (industries, fine) | -0.002 | 0.114 | 0.096 | -0.268 |
| | (0.01) | (0.40) | (0.42) | (0.41) |
| NPL ratio<br>× Portfolio similarity (regions) | 0.033*** | 1.305*** | 0.820* | 0.579 |
| | (0.01) | (0.43) | (0.50) | (0.48) |
| Portfolio opacity | 0.001 | -0.041 | 0.152 | 0.134 |
| | (0.01) | (0.49) | (0.48) | (0.58) |
| Portfolio opacity<br>× Portfolio similarity (industries, fine) | 0.014** | 0.129 | -0.103 | -0.090 |
| | (0.01) | (0.35) | (0.40) | (0.42) |
| Portfolio opacity<br>× Portfolio similarity (regions) | 0.018** | 0.640 | 0.729 | 0.781 |
| | (0.01) | (0.52) | (0.64) | (0.64) |
| Capital ratio (t-1) | -0.032*** | -1.218** | -1.597** | -0.307 |
| | (0.01) | (0.49) | (0.66) | (0.74) |
| Liquidity ratio (t-1) | 0.010 | -1.148 | 3.840 | 2.352 |
| | (0.01) | (0.75) | (7.38) | (7.30) |
| ROA (t-1) | 0.055*** | 2.445*** | 3.159*** | 2.674*** |
| | (0.01) | (0.42) | (0.74) | (0.79) |
| Loans-to-assets (t-1) | 0.082*** | 2.789*** | 3.129** | 3.856** |
| | (0.01) | (0.62) | (1.51) | (1.79) |

**Table 3.C3:** (continued) Interbank lending, portfolio similarity, and credit portfolio quality (matched sample)

| | Probit **Credit relation** | OLS **Δ Exposure** | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Size (t-1) | 0.221*** | 2.824*** | 6.089 | 8.332 |
| | (0.02) | (0.94) | (9.23) | (8.78) |
| **Lender characteristics** | | | | |
| Portfolio quality | 0.013 | 1.122** | 1.176 | 1.274 |
| | (0.01) | (0.54) | (0.76) | (0.81) |
| NPL ratio | -0.017* | -0.208 | 1.287* | 0.073 |
| | (0.01) | (0.40) | (0.74) | (0.76) |
| Portfolio opacity | 0.005 | 0.430* | 0.466** | 0.747*** |
| | (0.01) | (0.22) | (0.23) | (0.26) |
| Capital ratio (t-1) | -0.081*** | -1.764*** | -3.540*** | -1.368 |
| | (0.01) | (0.49) | (1.01) | (1.06) |
| Liquidity ratio (t-1) | 0.000 | -1.608** | -6.970 | -7.081 |
| | (0.01) | (0.64) | (7.79) | (7.68) |
| ROA (t-1) | 0.020** | 0.823** | 0.545 | -0.339 |
| | (0.01) | (0.33) | (0.53) | (0.56) |
| Loans-to-assets (t-1) | -0.106*** | -2.186*** | 0.378 | 2.324 |
| | (0.01) | (0.48) | (2.01) | (1.83) |
| Size (t-1) | 0.007 | -1.279* | -14.500** | -9.332 |
| | (0.02) | (0.67) | (6.51) | (6.37) |
| **Relationship characteristics** | | | | |
| Relationship lending | 0.421*** | 5.442*** | 3.633*** | 3.832*** |
| | (0.01) | (1.12) | (0.99) | (1.00) |
| Reverse relationship lending | 0.069*** | 1.221** | 2.447*** | 2.371*** |
| | (0.01) | (0.50) | (0.62) | (0.61) |
| Δ Reverse exposure | 0.025*** | 2.293*** | 2.219*** | 2.127*** |
| | (0.01) | (0.57) | (0.56) | (0.54) |
| Same BHC | 0.564*** | 12.910** | 6.929 | 6.976* |
| | (0.18) | (5.09) | (4.24) | (4.18) |
| Same network | 0.391*** | 6.762*** | 5.743** | 5.925** |
| | (0.03) | (1.62) | (2.84) | (2.83) |
| Difference in liquidity surplus (t-1) | 0.028*** | -0.012 | 2.158 | 1.852 |
| | (0.01) | (0.48) | (3.35) | (3.29) |
| **Heckman controls** | | | | |
| Credit relation (t-1) | 2.885*** | | | |
| | (0.03) | | | |
| IMR | | 61.645*** | 64.609*** | 64.591*** |
| | | (2.95) | (3.01) | (3.02) |
| Observations | 226,190 | 69,509 | 69,452 | 69,452 |
| Bank class controls | Yes | Yes | Yes | Yes |
| Lender & borrower FEs | No | No | Yes | Yes |
| Time FEs | No | No | No | Yes |
| R-squared | 0.84 | 0.12 | 0.14 | 0.14 |

Standard errors (twoway clustered by lender and borrower) in parentheses

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

**Table 3.C4:** The effect of portfolio quality, NPL ratio, and portfolio opacity on interbank lending for different values of similarity (marginal effects, matched sample)

This table reports marginal effects for the regression on our matched sample reported in Table 3C.3. "Low similarity" refers to a similarity of 3 standard deviation below the variable mean, "high similarity" refers to a similarity of 3 standard deviations above the variable mean. All variables are defined in Appendix 3A.

| | Probit<br>**Credit relation** | OLS<br>**Δ Exposure** | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Portfolio quality (both similarities low) | -0.008*** | -6.451*** | -3.205 | -3.222 |
| | (0.00) | (1.81) | (2.52) | (2.23) |
| Portfolio quality (industry dissimilar, locality similar) | 0.002 | 2.905 | 2.197 | 2.270 |
| | (0.00) | (2.19) | (2.60) | (2.52) |
| Portfolio quality (industry similar, locality dissimilar) | -0.012*** | -7.217*** | -3.781 | -4.774* |
| | (0.00) | (2.26) | (2.35) | (2.51) |
| Portfolio quality (both similarities high) | -0.003 | 2.140 | 1.622 | 0.718 |
| | (0.00) | (1.91) | (2.79) | (2.67) |
| NPL ratio (both similarities low) | -0.008*** | -6.502*** | -4.217*** | -3.182** |
| | (0.00) | (1.49) | (1.50) | (1.45) |
| NPL ratio (industry dissimilar, locality similar) | 0.002 | 1.330 | 0.701 | 0.292 |
| | (0.00) | (1.95) | (2.47) | (2.38) |
| NPL ratio (industry similar, locality dissimilar) | -0.009*** | -5.820*** | -3.640* | -4.788** |
| | (0.00) | (2.08) | (2.18) | (2.14) |
| NPL ratio (both similarities high) | 0.002 | 2.013 | 1.279 | -1.313 |
| | (0.00) | (1.74) | (1.96) | (1.83) |
| Portfolio opacity (both similarities low) | -0.005*** | -2.347 | -1.726 | -1.938 |
| | (0.00) | (1.67) | (2.23) | (2.34) |
| Portfolio opacity (industry dissimilar, locality similar) | 0.001 | 1.493 | 2.649 | 2.748 |
| | (0.00) | (2.19) | (2.46) | (2.43) |
| Portfolio opacity (industry similar, locality dissimilar) | -0.000 | -1.575 | -2.344 | -2.480 |
| | (0.00) | (2.30) | (2.70) | (2.71) |
| | (0.00) | (1.76) | (1.96) | (1.99) |
| Portfolio opacity (both similarities high) | 0.006*** | 2.265 | 2.031 | 2.206 |
| | (0.00) | (1.47) | (1.79) | (1.86) |
| Observations | 226,190 | 69,509 | 69,452 | 69,452 |
| Other variables included (see Table 3C.3) C8) | Yes | Yes | Yes | Yes |
| Lender & borrower FEs | No | No | Yes | Yes |
| Time FEs | No | No | No | Yes |

Standard errors (twoway clustered by lender and borrower) in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

## 3.D Analyses on matched sample

This Appendix reports details on our matched sample and runs the analysis of Table 3.4 on our matched sample. The matching procedure is explained in Section 3.7.

**Table 3.D1:** Characteristics of similar and non-similar bank pairs in the matched sample

This table compares relevant covariates of similar and non-similar bank pairs in our matched sample. The sample consists of quarterly bank-pair observations of 2054 banks between 2009 and 2018. Bank pairs with a high-similarity in both regional and sectoral terms are matched to bank pairs of similar correlation between the lender's and the borrower's *Portfolio quality* but with a low similarity both regional and sectoral terms. All variables are defined in Appendix 3A. The normalized difference is calculated as suggested by Imbens and Wooldridge (2009).

| | Portfolio similarity (industries) | Portfolio similarity (regions) | Credit relation | $\Delta$ Exposure | Credit portfolio quality | NPL ratio | Portfolio opacity |
|---|---|---|---|---|---|---|---|
| **Similar bank pairs** | | | | | | | |
| (N=523,766) | | | | | | | |
| Mean | 97.86 | 63.25 | 0.31 | -0.06 | 98.61 | 2.33 | 2.34 |
| Median | 99.45 | 99.45 | 0 | 0 | 99.16 | 1.61 | 2.03 |
| SD | 6.20 | 6.20 | 0.461 | 43.47 | 1.78 | 3.13 | 2.05 |
| **Non-similar bank-pairs** | | | | | | | |
| (N=251,560)[1] | | | | | | | |
| Mean | 81.34 | 18.05 | 0.19 | -0.05 | 97.54 | 3.17 | 2.76 |
| Median | 88.15 | 11.86 | 0 | 0 | 99.04 | 2.25 | 2.49 |
| SD | 17.98 | 18.42 | 0.394 | 30.13 | 3.84 | 3.85 | 2.22 |
| Normalized difference (Imbens/Woolridge) | 0.869 | 2.326 | 0.198 | -0.000 | 0.253 | -0.169 | -0.139 |

**Table 3.D2:** Bank and interbank characteristics

This table reports summary statistics of the bank and interbank characteristics of our matched sample. All variables are defined in Appendix 3A.

| | Observations | Unit | Mean | SD | p5 | Median | p95 |
|---|---|---|---|---|---|---|---|
| **Interbank Lending** | | | | | | | |
| Credit relation | 234,944 | Dummy | 0.31 | 0.46 | 0.00 | 0.00 | 1.00 |
| Δ Exposure | 232,945 | % | -0.38 | 38.39 | -9.81 | 0.00 | 7.41 |
| **Portfolio Similarity** | | | | | | | |
| Portfolio similarity (industries) | 234,944 | % | 92.27 | 13.00 | 66.76 | 98.22 | 99.90 |
| Portfolio similarity (industries, fine classification) | 234,944 | % | 78.51 | 19.23 | 38.69 | 83.71 | 99.09 |
| Portfolio similarity (regions) | 234,944 | % | 48.77 | 31.41 | 3.49 | 50.41 | 97.21 |
| **Bank characteristics** | | | | | | | |
| Interbank borrowing/total borrowing | 234,927 | % | 5.24 | 7.43 | 0.00 | 2.76 | 16.39 |
| Interbank lending/total lending[2] | 234,927 | % | 5.02 | 7.10 | 0.00 | 2.25 | 20.64 |
| Portfolio quality | 234,944 | % | 98.07 | 2.77 | 91.49 | 98.92 | 99.92 |
| Portfolio opacity | 234,944 | % | 1.74 | 1.63 | 0.30 | 1.22 | 5.01 |
| NPL ratio | 234,944 | % | 2.05 | 2.12 | 0.08 | 1.51 | 6.20 |
| Capital ratio | 234,655 | % | 22.28 | 14.97 | 12.15 | 19.35 | 37.61 |
| Liquidity ratio | 234,927 | % | 19.23 | 14.04 | 4.39 | 16.21 | 41.94 |
| ROA | 234,433 | % | 1.71 | 1.23 | 0.15 | 1.62 | 3.69 |
| Loans-to-assets | 234,918 | % | 50.21 | 19.46 | 11.90 | 53.42 | 77.76 |
| Size | 234,927 | Log | 9.09 | 2.48 | 5.42 | 8.98 | 13.19 |
| **Relationship characteristics** | | | | | | | |
| Relationship lending | 234,944 | | 2.49 | 3.49 | 0.00 | 0.00 | 8.00 |
| Reverse relationship lending | 234,944 | | 2.47 | 3.48 | 0.00 | 0.00 | 8.00 |
| Δ Reverse exposure | 234,944 | % | -0.38 | 38.43 | -10.18 | 0.00 | 7.68 |
| Same BHC | 234,944 | Dummy | 0.00 | 0.05 | 0.00 | 0.00 | 0.00 |
| Same network | 234,944 | Dummy | 0.10 | 0.30 | 0.00 | 0.00 | 1.00 |
| Difference in liquidity surplus (t-1) | 234,910 | ppt | 0.00 | 6.15 | -9.60 | 0.00 | 9.60 |

**Table 3.D3:** Interbank lending, portfolio quality, and portfolio opacity (matched sample)

This table shows the coefficients of a two-stage Heckman sample selection model on a matched sample. The sample consists of quarterly bank-pair observations of 2054 banks between 2009 and 2018. Bank pairs with a high-similarity in both regional and sectoral terms are matched to bank pairs of similar correlation between the lender's and the borrower's *Portfolio quality* but with a low similarity both regional and sectoral terms. The dependent variables are the existence of a loan between lender $i$ and borrower $j$ at end-of-quarter $t$ (Model 1, Probit), and the percentage change of interbank exposure between lender $i$ and borrower $j$ over the period (t-1) to t, respectively (Model 2 to 4, OLS). Model (3) includes lender and borrower fixed-effects, model (4) includes lender, borrower, and time fixed-effects. Coefficients are standardized, except for binary variables. Standard errors are clustered on the borrower and lender level. All variables are defined in Appendix 3A.

| | Probit<br>**Credit relation** | OLS<br>**Δ Exposure** | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| **Borrower characteristics** | | | | |
| Portfolio quality | -0.072*** | -1.745** | -0.195 | -0.916 |
| | (0.01) | (0.86) | (0.83) | (0.87) |
| NPL ratio | -0.080*** | -2.447*** | -1.642** | -2.161*** |
| | (0.01) | (0.52) | (0.64) | (0.53) |
| Portfolio opacity | 0.000 | 0.125 | 0.315 | 0.269 |
| | (0.01) | (0.51) | (0.44) | (0.53) |
| Capital ratio (t-1) | -0.038*** | -1.442*** | -2.015*** | -0.452 |
| | (0.01) | (0.47) | (0.70) | (0.72) |
| Liquidity ratio (t-1) | 0.011 | -1.145 | 3.776 | 2.046 |
| | (0.01) | (0.71) | (7.44) | (7.32) |
| ROA (t-1) | 0.055*** | 2.388*** | 3.160*** | 2.540*** |
| | (0.01) | (0.42) | (0.69) | (0.77) |
| Loans-to-assets (t-1) | 0.072*** | 2.242*** | 1.846 | 3.224* |
| | (0.01) | (0.59) | (1.49) | (1.72) |
| Size (t-1) | 0.213*** | 2.599*** | 4.484 | 7.360 |
| | (0.02) | (0.91) | (9.52) | (9.03) |
| **Lender characteristics** | | | | |
| Portfolio quality | 0.022* | 1.284** | 1.339* | 1.454* |
| | (0.01) | (0.53) | (0.79) | (0.83) |
| NPL ratio | -0.019* | -0.264 | 1.425** | -0.048 |
| | (0.01) | (0.40) | (0.72) | (0.76) |
| Portfolio opacity | 0.006 | 0.441* | 0.460** | 0.721*** |
| | (0.01) | (0.23) | (0.23) | (0.26) |
| Capital ratio (t-1) | -0.088*** | -2.045*** | -4.041*** | -1.535 |
| | (0.01) | (0.47) | (1.00) | (1.08) |
| Liquidity ratio (t-1) | 0.001 | -1.688*** | -6.967 | -6.891 |
| | (0.01) | (0.65) | (7.86) | (7.71) |
| ROA (t-1) | 0.020** | 0.853*** | 0.734 | -0.300 |
| | (0.01) | (0.32) | (0.54) | (0.56) |
| Loans-to-assets (t-1) | -0.114*** | -2.435*** | -0.562 | 1.948 |
| | (0.01) | (0.48) | (2.02) | (1.86) |
| Size (t-1) | 0.008 | -1.313** | -17.029** | -10.598 |
| | (0.02) | (0.65) | (6.76) | (6.45) |
| **Relationship characteristics** | | | | |
| Relationship lending | 0.421*** | 5.388*** | 3.575*** | 3.844*** |
| | (0.01) | (1.12) | (1.00) | (1.01) |
| Reverse relationship lending | 0.072*** | 1.410*** | 2.509*** | 2.441*** |
| | (0.01) | (0.50) | (0.61) | (0.61) |

**Table 3.D3:** (continued) Interbank lending, portfolio quality, and portfolio opacity(matched sample)

| | Probit<br>**Credit relation** | OLS<br>**Δ Exposure** | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Log Reverse exposure | 0.025*** | 2.299*** | 2.229*** | 2.133*** |
| | (0.01) | (0.57) | (0.56) | (0.54) |
| Same BHC | 0.569*** | 12.476** | 7.135* | 7.120* |
| | (0.18) | (5.03) | (4.21) | (4.17) |
| Same network | 0.412*** | 6.484*** | 6.297** | 6.357** |
| | (0.03) | (1.68) | (2.76) | (2.77) |
| Difference in liquidity surplue (t-1) | 0.028*** | -0.009 | 2.113 | 1.723 |
| | (0.01) | (0.49) | (3.39) | (3.31) |
| **Heckman controls** | | | | |
| Credit relation (t-1) | 2.887*** | | | |
| | (0.03) | | | |
| IMR | | 61.572*** | 64.529*** | 64.532*** |
| | | (2.95) | (3.01) | (3.02) |
| Observations | 226,190 | 69,509 | 69,452 | 69,452 |
| Bank class controls | Yes | Yes | No | No |
| Lender & borrower FEs | No | No | Yes | Yes |
| Time FEs | No | No | No | Yes |
| (Pseudo) R-squared | 0.84 | 0.12 | 0.14 | 0.14 |

Standard errors (twoway clustered by lending and borrowing bank) in parentheses

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

# Curriculum Vitae

# Alison Schultz

## Education

| | |
|---|---|
| 2017 - 2022 | Doctoral Studies in Finance |
| | Graduate School of Social and Economic Sciences |
| | University of Mannheim, Germany |
| 2014 - 2017 | Master of Arts in Global Political Economy |
| | University of Kassel, Germany |
| | Grade: 1.0 (A) |
| 2012 - 2014 | Bachelor of Science in Economics, Minor: Political Science |
| | University of Mannheim, Germany |
| | Grade: 1.5 (A) |

## Professional Experience

| | |
|---|---|
| Since 2022 | Research Fellow |
| | Tax Justice Network, London, United Kingdom |
| Since 2022 | Contractor for Data Analytics and Research |
| | Bürgerbewegung Finanzwende e.V. & |
| | Finanzwende Recherche gGmbH, Berlin, Germany |
| 2018 - 2022 | Research Assistant |
| | Chair of Corporate Finance, University of Mannheim, Germany |
| 2020 | Research Assistant |
| | Deutsche Bundesbank, Frankfurt, Germany |
| 2016 | Research Assistant |
| | International Center for Development and Decent Work, Kassel, Germany |

## Teaching Experience

| | |
|---|---|
| 2019 - 2022 | Stata in Finance, Mannheim Master of Management |
| 2019 - 2021 | Corporate Finance I: Capital Structure, Cost of Capital and Valuation<br>Teaching Assistant, Mannheim Master of Management |
| 2019 - 2022 | Corporate Finance II: Mergers, Acquisitions and Divestitures<br>Teaching Assistant, Mannheim Master of Management |
| 2019 - 2022 | Supervision of Master's, Bachelor's and Seminar Theses<br>Mannheim Master of Management, B.Sc. Business Administration,<br>B.Sc. Business Education |
| 2019 - 2022 | Academic Writing, B.Sc. Business Education |
| 2016 | International Economics for Non-Economists, M.A. Global Political Economy |
| 2015 | Governance of the World Markets: Institutions, Instruments and Experiences<br>Teaching Assistant, M.A. Global Political Economy |

## Awards and Scholarships

| | |
|---|---|
| 2022 | Young Economist Award, European Economic Association |
| 2019 - 2022 | Doctoral Scholarship, Stiftung Geld und Währung |
| 2017 - 2018 | Graduate School Scholarship,<br>Graduate School for Economic and Social Sciences University of Mannheim |

## Voluntary Work

| | |
|---|---|
| Since 2020 | Coordinator of Campaign for the Transparent and<br>Sustainable Investment of Public Employees' Pensions<br>SustainVBL, Mannheim, Germany |
| 2018 - 2022 | Coordinator of Political Education Project<br>GLOBE - Globales Lernen an Berufsschulen, Berlin, Germany |
| 2015 - 2017 | Teacher German for Refugees<br>Student Council University of Kassel, Germany |