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The Effects of Natural Catastrophes and
Merger Events on Financial Markets and
the Real Economy

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Chapter 1

Introduction

”Even the most severe critics of the financial sector agree that a good financial system is essential for a well-functioning economy”

-Luigi Zingales

2015 AFA Presidential Address

Understanding how banks react to unexpected events has become a very important economic and social question, especially since the financial crisis (Ivashina and Scharfstein, 2010; Puri et al., 2011). Whereas previous financial crises had largely stayed in the realm of finance, or very limited areas of the economy, the financial crisis of 2007-2008 demonstrated that unexpected financial shocks can have severe implications for the real economy in general, impacting the lives of a large cross-section of the population, for example through general reductions in employment (Chodorow-Reich, 2014; Popov and Rocholl, 2017). This new realization has led to an extensive literature on how banks react to unexpected events, especially if and how they transfer such shocks to firms and households. As a result, understanding exactly how shocks are transferred not only between banks (Popov and Udell, 2012; Schnabl, 2012), but also between banks and firms has become a crucial aspect of financial research (Peek and Rosengren, 2000; Gan, 2007; Ongena et al., 2015; Acharya et al., 2018; Gropp et al., 2018; Huber, 2018). It has returned into focus the idea that a functioning connection between banks and firms constitutes a crucial part of a well-functioning economy. This thesis aims to contribute to the understanding of how this bank-firm relationship functions and what pitfalls it might entail.

While there is much evidence on how shocks arising in financial markets affect firm-level performance in various aspects, much less is known about how unexpected changes in one firm's performance can affect other firms' performance via the banking network. Because banks most often intermediate more than one firm, a change in demand by one firm creates the possibility of spillovers to other firms, as banks may withdraw funding to serve the new demand. This firm-to-firm shock transfer via banks has been rarely investigated in the literature (Jiménez et al., 2017), perhaps because there is an ex-ante expectation that banks can easily buffer demand shocks by borrowing from somewhere else, essentially by expanding their balance sheets. However, this thesis demonstrates that low-capital banks can indeed act as amplifiers of local shocks, by reducing lending to 'unsuspecting' firms when demand rises elsewhere. There is also little knowledge about the effect of shocks that affect the firm-bank relationship and the resulting implication that it has for banks and firms alike (Degryse et al., 2011). As a result this thesis investigates the effect of changes in firm demand on the credit provision of banks, the resulting potential spillover to other firms and the potential disruptions of firm-bank relationships.

Another part of the contribution of this thesis is methodological. In order to expand the understanding of how banks react to unexpected shocks, this thesis exploits events that are - by all accounts - unexpected. A large part of this thesis exploits a natural disaster, specifically the Elbe flood of 2013 in Germany, as identification. Natural disasters are most often completely unexpected events; at the same time they are often quite significant both for banks and firms (Noth and Schüwer, 2017; Cortes and Strahan, 2017). Importantly, they also provide a shock that has significant within country variation, as opposed to, for example the financial crisis. This allows a clear separation of affected and unaffected regions within one country. This thesis aims to exploit this fact and investigates how banks and firms react to natural disasters. This allows for an investigation into the reaction of the financial system and the economy, in a very specific, yet random natural experiment. Learning how banks and firms react in the face of natural disasters may not only help to understand how to better protect against disasters, but may provide some valuable lessons about how financial institutions and the real economy react to events they did not anticipate.

This thesis aims to expand the understanding how the relationship between banks

and firms can contribute to a more stable financial system, resulting in a less volatile and more prosperous real economy, in four main chapters. Chapter 2 aims to demonstrate the importance of bank capital, not only for preventing bank failure and a collapse of the financial system (Berger et al., 1995; Meh and Moran, 2010; Admati and Hellwig, 2014; Dagher et al., 2016), but also for the absorption of local, real economic shocks.¹ Using the 2013 Elbe flood as an exogenous shock to the real economy, it demonstrates that negative effects of the flood can spill over to non-flooded regions (Cortes and Strahan, 2017), especially if banks do not hold enough capital. This problem arises, because banks cannot expand their balance sheets to serve additional credit demand arising in flooded regions, both because of regulatory capital regulation and perceived risk (Modigliani and Miller, 1958). As a result, low-capital banks that serve loan customers in the flooded regions reduce lending to non-flood regions, to the detriment of firms located there. Specifically, firms in non-flooded regions that are exposed to the disaster via their below-median-capital bank, are found to reduce employment by 11% and their fixed assets by 20% compared to firms in the same region without such a bank. The finding that firms are unable to substitute lost bank funding and reduce investment and employment, suggests that this is inefficient from the perspective of the overall economy, and thus encouraging banks to hold more capital can internalize these previously disregarded externalities.

Chapter 3 confirms the idea that banks intervene in the lending market after a natural disaster to provide loans to firms looking to replace their destroyed capital stock.² Using the same 2013 flood as identification, it shows that banks increase their lending to firms in disaster areas, even if the banks are located outside of the disaster area. Crucially, this lending increase is actually received by firms. The findings demonstrate that firms connected to such banks increase their borrowing compared to other firms. It further demonstrates that additional lending is not driven by more risk-taking or by banks being able to charge higher interest rates. Overall the results strongly suggest that banks play an active role in firms' recovery after a natural disaster and may provide disaster-struck firms with *recovery lending* in times of unexpected shocks.

¹Chapter 2 is based on an IWH Discussion Paper (Rehbein, 2018).

²This paper is based on a joined IWH Discussion Paper with Michael Koetter and Felix Noth (Koetter et al., 2016).

Chapter 4 is concerned with the simple, yet previously disregarded question of how firms perform following natural disasters in developed economies.³ Most studies previously focused on cross-country effects (Toya and Skidmore, 2007; Noy, 2009; McDermott et al., 2014), or developing countries (Becchetti and Castriota, 2011; Strobl, 2011; Berg and Schrader, 2012). Effects of disasters on the firm level in developed countries have not received much attention (Leiter et al., 2009). The chapter surprisingly demonstrates that disasters can be *positive* for firms in developed countries. Using a matched sample of flooded and non-flooded firms, flooded firms are shown to have higher turnover, more liquidity and are less risky after a natural disaster. This finding can be partially explained by firms learning from disasters. Additionally, differing effects between developing and developed countries can potentially be reconciled by the presence of disaster relief, which was a significant factor following the 2013 flooding in Germany.

Chapter 5 investigates (dis-)continuations of bank-relationships using bank mergers.⁴ There are strong indications that bank mergers can lead to inefficient destruction of bank-firm relationships, although the reasons for these drops are not clear (Degryse et al., 2011). The question thus remains why firm-bank relationships change after a merger and what - if anything - can be done to prevent such negative firm-level effects of bank mergers. This chapter provides evidence that banks and firms sever ties more frequently both if there is a lack of bank-competition and if there is a lack of firm collateral. In the presence of sufficient competition and collateral, firms are able to more easily switch their bank and do not have to drop their bank relationship without replacement. Additionally this chapter demonstrates novel evidence that *adding* a bank relationship is beneficial for firms after a merger has taken place. The combined results, in context with the existing literature suggest that banks and firms may be dropping their relationship because firms are subject to a reduction in bank lending after a merger (which Degryse et al. (2011) suggest is not due to being less efficient). As a result, firms search for alternative sources of funding and substitute their lending reductions by looking for other banks. In a competitive environment, or with sufficient collateral, firms are more likely to be able to switch to another bank or add another

³This paper is based on a joint paper with Felix Noth, which was published in *Finance Research Letters* (Noth and Rehbein, 2018).

⁴Chapter 5 is based on joined work with Santiago Carbo-Valverde.

bank relationship to compensate for the funding shortfall.

Overall the findings in this thesis contain three highly policy relevant findings. First, regional banks play an integral role in buffering firm-level shocks and thus serve a key role in a well-functioning economy. Second, sufficient bank capital is crucial in order to ensure that this buffering function does not come at the expense of other firms. Since setting higher regulatory capital ratios does not solve the issue, but may be enhancing it, other ways of encouraging banks to hold more capital need to be considered, for example removing the tax shield on debt. Third, bank competition is also an important factor for a functioning banking market. This is not only because of a competition in prices (or quantities), but also because it enables other funding alternatives for firms, in case they get hit with a sudden funding contraction from existing lenders.

Chapter 2

Flooded through the back door: Firm-level effects of banks' lending shifts

***Abstract:** I show that natural disasters transmit to firms in non-disaster areas via their banks. This spillover of non-financial shocks through the banking system is stronger for banks with less regulatory capital. Firms connected to a disaster-exposed bank with below median capital, reduce their employment by 11% and their fixed assets by 20% compared to firms in the same region without such a bank during the 2013 flooding in Germany. Relationship banking and higher firm capital also mitigate the effects of such negative cross-regional spillovers.**

2.1 Introduction

This paper expands on the recent finding that banks shift their lending from non-disaster areas into disaster areas (Cortes and Strahan, 2017) in order to identify negative real effects for firms. It contributes methodologically to existing papers demonstrating negative effects of credit supply shocks on firms (Chodorow-Reich, 2014; Cingano et al., 2016), by examining an exogenous shock that does not arise in financial markets and is thus more likely to be unexpected and orthogonal to banking characteristics. In addition, I provide evidence that the lending shift and its negative real consequences for firms are driven mainly by low-capital banks, demonstrating that higher bank capitalization is important for preventing real-economic spillovers from local shocks.

I proceed in two steps to isolate the effect of a bank funding shock for non-directly affected firms after a natural disaster. Using significant flooding of German regions

*A version of this chapter has been published in the IWH-Discussion Papers Series as Rehbein, Oliver (2018). Flooded through the back door: Firm-level effects of banks' lending shifts (No. 4/2018).

in June of 2013 as identification, I identify firms in disaster areas and use their bank connections to identify the disaster exposure of banks. I then identify firms in non-flooded areas which are connected to disaster exposed banks, and compare them to firms in the same region, but without a connection to a disaster exposed bank. This approach is designed to specifically isolate the effect of a reduction in bank funding for firms, as banks reduce lending in non-flooded areas to provide loans to flood-affected firms (Cortes and Strahan, 2017).

I show that banks' lending shifts from non-disaster regions into disaster regions entail a reduction in investment for firms outside the areas directly impacted by the flood by about 16 percentage points and a reduction in total fixed assets by about 11%. This effect only occurs if the firm has an indirect exposure to the disaster via its bank relationship. Importantly, low bank capital ratios exacerbate this effect, as firms connected to low-capital banks additionally experience a sharp drop in employment by 11% and a reduction in fixed assets by 22%.

Further results indicate that relationship banking (Boot, 2000) may play a small role in reducing the effects of the transferred shock on firms' real outcomes, as banks located closer to their firm customers will also reduce the impact of the effect in terms of employment on the firm level.¹ I find evidence that firms banking with local savings banks are exposed to higher risk, as they reduce investment more than firms banking with non-savings banks. The reverse holds for cooperative banks; firms connected to them reduce investment by lesser amounts than other bank types. Higher pre-flood firm capital ratios are important, as firms finance more of their investment with equity capital and have more collateral when applying for a loan (Jiménez et al., 2017). I do not find that firm or bank liquidity, measured as the share of cash of total assets, matters for shock transmission during a disaster.

Examining a local, real economic shock in the form of flooding in order to identify the real firm-level effects of a reduction in firms' bank funding availability expands in two ways on the current literature using global financial shocks as identification (Chodorow-Reich, 2014; Acharya et al., 2018). First, a natural disaster is random and exogenous, especially to firms outside the disaster area. Given that there is some

¹This finding is perfectly in line with Cortes and Strahan (2017) who demonstrate that banks mainly reduce lending in non-core markets.

evidence that even insurance markets often fail at correctly pricing disaster risk (Froot, 2001), it seems unlikely that bank-customers correctly price their banks' disaster risk. For this shock to be exogenous, the identification relies on the assumption that bank-customers are unaware of their banks' 2013 disaster-exposure prior to the flood.² Second, while local shocks may be smaller than global financial crises, they also occur more frequently. Because the regional economy is typically not as diversified, banks are likely to face regional demand (or supply) shocks quite frequently (Yeager, 2004).³

Distributing local shocks from one region to another might be ex-ante efficient from the banks' perspective, but it can have unintended negative real consequences if firms cannot substitute a sudden lending reduction. Especially if banks transmit even small local shocks to unsuspecting firms, banks might have an unintended destabilizing effect for the real economy. This paper's findings also demonstrate that higher bank capital can prevent such spillovers entirely. Firms, whose banks are exposed to the disaster, experience negative employment effects only if their bank has a relatively low regulatory capital ratio. This is intuitive, as banks with lower capital cannot expand or contract their balance sheet easily and thus will have to reduce other assets, which often implies cutting back lending to non-disaster affected firms (Jiménez et al., 2017). The findings highlight that bank capital is not only important to prevent bank failure, but also to prevent spillovers of real-economic shocks from one region to another. In these cases, more bank capital prevents lending reductions, thus running counter to the idea that more bank capital generally reduces lending and is thus costly for firms (Gropp et al., 2018).

This paper also contributes by investigating the real effects of a shock stemming from higher credit *demand* elsewhere, instead of credit supply frictions arising in financial markets.⁴ My results indicate that banks reduce lending in non-disaster areas primarily because they face higher loan demand in disaster areas. This view is strongly supported by the literature, as both Chavaz (2016) and Cortes and Strahan (2017) document for the United States that banks reallocate funds towards mortgage

²And that the firms' bank choice is not correlated with other factors that might be affected by flooding. I address some of these concerns in more detail in the robustness section.

³Local shocks do not have to be natural disasters. As long as events occur that influence loan demand (or supply) and are reasonably unexpected the results presented in this paper should be applicable.

⁴See section 2.4.4 for a more detailed discussion of demand vs. supply in my setting.

loans in disaster-affected areas, while decreasing their funding in non-affected areas. Cortes and Strahan (2017) demonstrate that banks predominately reduce lending in non-core markets in order to serve the loan demand arising in disaster affected areas, while Chavaz (2016) highlights the role of local banks diversifying through secondary markets to serve the additional demand. Similarly, Koetter et al. (2016) find that German banks increase their lending in the aftermath of flooding. The demand shock interpretation can be explained by the fact that bank lending is a good complement to insurance payouts and government aid for firms in the case of a natural disaster, in order to finance necessary rebuilding efforts. The unfulfilled loan demand in the aftermath of disasters in developing countries (Choudhary and Jain, 2017; Berg and Schrader, 2012) indicates that insurance and government aid⁵ may be crucial factors for banks to actually fulfill the increased loan demand in disaster regions; as such payments might serve as excellent down-payments or collateral for new loans. As a result, it is possible that banks' lending shifts at the expense of non-directly affected firms is due to an unintended consequence of significant government aid after the disaster.

I contribute to four major strands of literature. First, I add to the growing body of literature analyzing the effects of natural disasters in the context of banking.⁶ It builds on the results by Chavaz (2016), Cortes and Strahan (2017) and Koetter et al. (2016) who demonstrate on the bank level, that banks withdraw funding from non-disaster areas and channel them into disaster areas. I add to these papers by showing that the documented shift in lending away from non-disaster areas especially highlighted in Cortes and Strahan (2017) entails negative consequences on the *firm level*.⁷ Two studies have previously examined the effects of an *indirect* disaster shock on firms: Uchida et al. (2015) and Hosono et al. (2016) look at the effect of a natural disaster,

⁵See section 2.2 for details regarding the specific flood and the subsequent government aid payments.

⁶A number of further studies use natural disasters as identification in the finance context. Schüwer et al. (2018) find that banks increased their capital buffer after hurricane Katrina, and Noth and Schüwer (2017) find that bank stability decreases after being exposed to natural disasters. Gallagher and Hartley (2017) analyze the effects on household finance and find a reduction in total debt after natural disasters. Morse (2011) finds a mitigating effect of payday lenders on foreclosures following natural disasters.

⁷Only very few studies have evaluated the direct effects of natural disasters on firms in the context of banking and finance. Cortés (2014) examines employment after natural disasters and finds that the presence of relationship banks contributes to recovery from a natural disaster, especially for young and small firms.

namely the Great Tohoku Earthquake, on bankruptcy and investment of firms *outside* connected to banks *inside* the disaster area. While their approach and findings are similar to mine, I contribute to their findings in three significant ways. First, I exploit a bank-specific measure of disaster exposure and include county \times year fixed effects in my regression, ruling out other regional variation that might be at play, especially in the middle of a disaster. In fact, my results indicate that using only the direct location of the bank as identification is not precise enough to capture the effect of the bank funding shock in my setting. Second, I additionally focus on employment and the fixed asset stock of firms. Most importantly, I show that banks with low capital ratios are more likely to cause real effects in firms in non-affected regions, contributing to the understanding of how shocks can propagate through the banking system to otherwise unaffected firms, especially if banks are highly levered.

This paper is also closely related to the growing literature on the effect of credit frictions on the real economy. One prominent example is Chodorow-Reich (2014), who shows that firms connected to less healthy banks before the financial crisis perform significantly worse in terms of employment outcomes following the crisis.⁸ Most of these studies rely on banks' exposure to financial market frictions, such as the exposure to the financial crisis. One major caveat here is that bank choice may not be completely orthogonal to the banks' exposure to risky international financial markets. I argue that the credit supply shock arising from a natural disaster is significantly more exogenous, because it is unexpected, especially for firms that are not directly located within the disaster regions.

Another related strand of literature is concerned with the transmission of financial shocks across markets and geographical borders. There is ample evidence that financial

⁸The list of papers on the real effects of credit market frictions is long and growing. Peek and Rosengren (2000) show that Japanese credit market frictions had an effect on U.S. real activity. Gan (2007) shows reductions in investment and firm valuation for firms exposed via their banks to the land market collapse in Japan. Chava and Purnanandam (2011) show that during the Russian crisis, firms that relied on bank financing suffered real consequences. Almeida et al. (2012) show that firms whose debt was maturing during the financial crisis cut their investment. Using bank-firm data from Italy, Cingano et al. (2016) estimate that the collapse of the interbank market decreased firm-level investment by 20%. Popov and Rocholl (2017) show that firms connected to German savings banks with exposure to U.S. mortgage markets performed worse than otherwise similar firms. Using firm-bank level data from Eastern Europe and Central Asia, Ongena et al. (2015) show that firms connected to internationally active banks suffer more during a financial shock. Berg (2016) provides evidence of negative real effects with rejected loan application data. Acharya et al. (2018) provide evidence that the European sovereign debt crisis had real, firm-level effects. Gropp et al. (2018) show that higher capital requirements cause credit reductions and subsequent negative real effects in firms.

shocks cross international borders (Popov and Udell, 2012; Puri et al., 2011; Schnabl, 2012). There is also growing within-country evidence that shocks can propagate to other national regions via integrated financial systems. Chavaz (2016) and Cortes and Strahan (2017) demonstrate this shock transmission across county borders using natural disasters, while Ben-David et al. (2015) show that local deposit rates are influenced by loan growth in non-local markets and Gilje et al. (2016) demonstrate that cash windfalls from shale gas booms influence mortgage lending in connected, non-boom counties. Furthermore, Chakraborty et al. (2018) demonstrate that local shocks can also be transmitted to other market segments, by demonstrating that commercial loans are crowded out by booms in real estate markets.⁹ I add to this literature by demonstrating that such (regional) shock transmissions are likely to entail real effects on the firm level and are mostly driven by banks with little regulatory capital.

Lastly, this paper is related to the large, yet significant discussion about the importance of bank and firm capital, especially during a crisis.¹⁰ However, most of the literature focuses either on the bank level (Kashyap and Stein, 2000) and firm level effects independently (Bernanke et al., 1996).¹¹ Jiménez et al. (2017) are the first to jointly examine the effects of bank and firm-level capitalization on credit provision. They find that bank capital matters in crisis times, and firms' capital matters in both crisis and non-crisis times. I confirm their findings for small and medium sized firms (SMEs) in Germany and expand on their results by showing that variations in bank and firm capitalization have implications for firms' real outcomes. There are two further papers examining the importance of bank capital ratios for firms' real outcomes. Gan (2007) shows that higher lenders' capital ratio is associated with higher investment rates of the borrowing firm. Kapan and Minoiu (2016) show that banks with higher capital ratios were able to more effectively maintain lending supply following

⁹Note that all these findings imply imperfect capital markets, i.e. that banks are financially constrained.

¹⁰Often referred to in the literature as the *bank balance sheet channel* and the *firm balance sheet channel*. This literature is closely related to the literature on bank-capital regulation. While the literature on the bank-level (and systemic) effects of bank capital regulation is large (e.g., Admati (2016); Dagher et al. (2016)), only a few studies examine the real effects of bank capital regulation (Gropp et al., 2018).

¹¹For the importance of bank capital on loan supply also refer to: Kishan and Opiela (2000), Jayaratne and Morgan (2000), Gambacorta and Mistrulli (2004), Meh and Moran (2010). For the importance of firm capital buffers also see: Chatelain et al. (2003)

the financial crisis of 2008 and as a result, firms borrowing from low-capital banks performed significantly worse. My results add to these findings by demonstrating that bank capital matters to avert real economic effects also for smaller, more localized shocks.

2.2 The 2013 flood, insurance and government aid

Widespread flooding caused significant damages and loss of lives in Central Europe in June 2013 (Thieken, 2016). The flooding was caused by two main factors: pre-saturated soil levels combined with heavy rainfalls from May 30th to June 2nd (Schröter et al., 2015). Heavy flooding followed in many regions of Austria and in the following weeks in South-East Germany and the Czech Republic, causing many levee breaches and widespread flooding. Germany was mostly flooded in the areas around the Danube and Elbe river and their tributaries, which is why the event in Germany is often called “The Elbe Flood”. Despite its river-specific name, the 2013 flood event had a significant spatial distribution throughout Germany (see Figure 2.1) and affected many major metropolitan areas, including major damage to the cities of Dresden, Passau, Halle (Saale) and Magdeburg.¹²

The 2013 flood in Germany was the biggest flood in Germany in terms of water discharge in the river network since 1954. In terms of economic damage, it was slightly smaller than the flooding of 2002, possibly because of flood protection measures instituted afterwards (Thieken, 2016). While initial reports indicated that the 2013 flooding exceeded the 2002 event in terms of damages, final estimates report the two events are similar in terms of the final economic damage: around 6-8 billion Euros for the 2013 flood and 11 billion for the 2002 flood. Of the 6-8 billion in damages, only 2 billion was insured (GDV, 2013), despite the 2002 flooding. This is in line with the idea that flood insurance costs rise after the flood, as insurance companies adjust the rates after tail risks materialize. This is supported by the fact that insurance coverage is still low even after the 2013 flood (Thieken, 2016). In addition to low insurance coverage, the speed of insurance payments, especially during a large event can be slow. While the German Association of Insurers claims that payments can be

¹²Some of these damages were permanent. For example the ice hockey stadium in Halle (Saale) was flooded and has not been rebuilt to this date.

made as quickly as two weeks after the damage is reported (GDV, 2013), in practice insurers' resources are often insufficient to accommodate so many contemporaneous claims.¹³ As a result, going to a bank for flood relief and rebuilding efforts can be faster, especially when there is an option of drawing down on existing credit lines.

– Figure 2.1 around here –

Floods of this magnitude have several direct and indirect effects on firms in the flood area, with many difficult to estimate. Direct effects include damage to buildings and machines, but also turnover losses during the flood and during the rebuilding/repair effort. Indirect effects include health effects and interruptions of supply chains due to destroyed infrastructure. Thieken (2016) conducted a business survey following the flood, and found that the most frequent problem for businesses was in fact the loss of turnover, while the most significant in terms of economic damage was destroyed buildings and equipment. Considering the average total assets in my dataset of 7 million Euros, losses to firms were significant: on average surveyed firms reported around 1 million Euros in damages.

To recover the losses, uninsured firms could apply for flood relief from the German federal and state government. Even though the overall government fund was larger than the final damages, affected firms could claim a maximum of 80% of current asset value. For firms, rebuilding most often involves buying new equipment, which is more expensive than the current value of the previous equipment. Further, only direct damages were reimbursed; indirect damages, such as losses from lost turnover, interrupted supply chains or employee productivity reduction were not reimbursed (BMI, 2013b). For all these reasons, it is thus likely that firms had to complement government aid by borrowing from banks in order to finance rebuilding efforts.

Flood prevention measures were taken after the 2002 flooding, however there is no indication that the 2013 flood was anticipated. Even during the flood, there was uncertainty about the extent to which water levels would rise. However, the 2002 flood may have increased the efficiency and especially the speed, with which aid relief was

¹³Usually insurance claims that pass a certain amount will not be accepted on good faith, but the insurance company will send an expert to estimate the damage. Only after that assessment has taken place, the insurer will make a payment. Since such people are in limited supply, delays in the aftermath of disaster may be inevitable. There are no hard numbers on how long a "typical" insured person has to wait for insurance payments following a flood. Anecdotal evidence suggests that it is paid out within a few months, not a few weeks.

delivered following the 2013 flooding (BMI, 2013a). Both flood prevention measures and increased aid efficiency may have led to an overestimation of actual damages overall (Thieken, 2016), but there is no evidence that this effect was region or even firm specific. Live flood monitoring was also only expanded significantly after 2013, muting concerns that the 2002 flood caused the 2013 flood to be anticipated. Furthermore, there is no evidence that banks learned from the flood (Koetter et al., 2016).

Taken together, the facts about the 2013 flood indicate that it was a significant and unexpected event for firms, which likely required firms to increase borrowing from banks. The expected government aid payments are likely to have served as good collateral or down-payments for financing rebuilding efforts. As a result, I hypothesize that banks who lent to - government supported - disaster areas reduced lending in other areas, resulting in potential negative real outcomes for firms located in these areas. It is important to highlight that while the flood event was certainly significant, the resulting loan shifts should be small in financial system terms.¹⁴ The results are particularly striking in this light, as banks propagate not only large financial shocks, but also small local shocks to "innocent" firm clients.

2.3 Data

German firm-level data stems from the Dafne and Amadeus databases, both provided by Bureau van Dijk.¹⁵ The former contains the name of the bank (or banks) with which each firm maintains a payment relationship (Popov and Rocholl, 2017).¹⁶ Annual vintages of the Dafne database are used to construct a time-series of firm-bank relationships for more than a million firms between 2003 and 2014. I augment these firm-bank relationship data with firm-specific, annual financial accounts data from

¹⁴Total loans to non-financial corporations in Germany are roughly 800 billion Euros over the flood period. If roughly a third of the German financial system had to buffer the uninsured 4 billion in damages, this would still constitute just over 1% of total lending, hardly a large-scale shock in financial terms.

¹⁵The construction of the firm-bank level data largely follows Koetter et al. (2016), although they collapse the data to the bank level, while my data is on the firm level, which requires some additional cleaning.

¹⁶Firm-bank payment relationship data originate from scans of the firms' letterheads. I do not observe credit relationships directly. I also cannot identify branch-level information in the data. However, most banks in Germany are small, independent savings and cooperative banks with few or no branches. Additionally the identification strategy does not rely on the banks' (or branches) direct location. The coverage of the database has increased significantly over the years, such that some 22,000 firms were included in 2003, but about 1.4 million firms appear in the database by 2015.

Amadeus.¹⁷ The firm-level data is combined with bank-level data from Bankscope, another Bureau van Dijk database, using firm-bank relationships identified using a string-based match of bank names. Bankscope contains annual financial account information for the banks.¹⁸

To gauge the damage inflicted by the Elbe flood of 2013, I use a data set provided by the German Insurance Association (GDV). The data contain claims filed for insurance properties that were damaged during the flood between May 25 and June 15, 2013, as a proportion of total insurance contracts, aggregated by county (“Kreis”), into nine damage categories.¹⁹ Lower categories indicate less damage relative to the asset values covered by insurance contracts.²⁰ The GDV collects this information from all its 460 members, which include all major German insurance providers. The data also inform the risk calculation models of insurance companies and regional aggregates are reported regularly (GDV, 2013). I merge this flood level data with the firms via their postal code.

The combination of the three datasets yields a firm-level dataset with information on each of the firms’ banks, as well as the regional flood exposure of each firm based on the data from the German Insurance Association. I conduct a number of cleaning steps with the merged dataset. Initially, the dataset comprises about 1.6 million firms. After dropping firms and banks, for which no valid postal code can be matched and dropping all inactive firms, the number of firms left are roughly 870,000.²¹ I also require firms to have reported at least their total assets, because otherwise the reporting accuracy might be questionable. I also drop all observations before 2008, because reporting of balance sheet information was not well enforced prior to that

¹⁷Bureau van Dijk takes this information for German firms from the “Bundesanzeiger”, where firms can report their balance sheet information. This reporting became more rigorously enforced starting in 2008.

¹⁸Because I lack any other relationship information other than the banks’ names in the Dafne database, I manually inspect many matches to ensure that the firm-level data are combined with the correct financial information about the banks from Bankscope. I match around 99% of all firm-bank relationships.

¹⁹Thus, I do not observe the damage inflicted on individual banks or firms. Also I do not have information on plants. As a result, I implicitly assume that the firms’ location, i.e. the headquarter, is the same as its plant location. Considering that I examine SMEs which are usually single-plant firms, this assumption appears to be reasonable.

²⁰The precise definition of the categories is provided in Figure 2.1. Variation in percentage of activated insurance contracts per county ranges from Category 1 ($\leq 0.04\%$) to Category 9 (10%–15%).

²¹Because I cannot observe the reason that firms drop from the dataset, or become inactive, I choose not to investigate this as an outcome variable.

time. As a result, firms in the data before 2008 may have self-selected into the data (Popov and Rocholl, 2017). Because firms are often not reporting for all years²² I require firms to be in the dataset at least one year before the flood of 2013 and one year after. Additionally, I require that the lags of the control variables be non-missing, and drop all observations where this is not the case. Finally, I drop financial firms from the dataset, in order to ensure that my results are not driven by banks and other financial institutions. The resulting dataset contains observations for roughly 150,000 firms for the period 2009-2014.

2.4 Identification

The goal of this paper is to compare firms, which are outside of the direct disaster area, yet conduct business with a bank that has sufficient exposure to the disaster, to firms outside of the disaster area that do not have a relationship with a disaster-exposed bank. The underlying idea is that disaster-exposed banks reduce lending to non-disaster firms. I illustrate graphically in Figure 2.2, how I identify such firms and the control group. I first identify flood-affected and unaffected firms, based on their county, assigning them a value between 1 and 9 according to the insurance data (GDV, 2013) (Equation 2.1). A firm in the most heavily flooded county is assigned a 9 and non-flooded counties receive a 1. Next, I identify the banks' exposure to the flood by averaging these category numbers of the banks' firm customers, weighted by the relative firm size (Equation 2.2). This is illustrated in the figure by the dotted arrows. Next, I identify indirectly affected firms, by identifying their banks' exposure to flood and averaging if the firm has multiple banks. This is illustrated by the dashed arrows in the figure. Lastly, I identify firms without such an indirect exposure (illustrated by the blank squares) and compare indirectly affected with non-indirectly affected firms. Because I use county \times year fixed effects, this comparison is strictly within region. The estimated comparison is illustrated by the smaller black frame within the unaffected region. In essence this illustrated comparison is the focus: Indirectly affected vs. not

²²Despite mandatory reporting this still occurs quite often. It is not clear whether this is a failure of firms to report because of a lack of enforcement or whether this is due to the information acquisition process by Bureau van Dijk.

indirectly affected firms in unaffected regions.²³

Such an *indirect* effect, as Cortes and Strahan (2017) suggest, stems from banks that shift lending from outside the disaster region into the disaster region. I exploit this *indirect* effect as an exogenous funding shock to firms, in order to investigate the real effects of small, local shocks on the real economy.

– Figure 2.2 around here –

2.4.1 Directly and indirectly affected firms

In order to identify the *indirect* effect of the natural disaster via its banks, it is necessary to identify directly affected firms. This is necessary for two reasons: First, the intended comparison is strictly between indirectly and not indirectly affected firms, which requires that directly affected firms be excluded. Second, the banks' disaster exposure is based on its firms' direct disaster exposure. I define *directly* affected and unaffected firms, according to their location in the flood affected counties. Specifically, firms located in counties which are ranked as category 4 or larger are classified as affected, while those that are in the lowest category (1) are classified as unaffected.²⁴ Since I mainly investigate firms in *directly unaffected* counties the exact threshold choice of the *directly affected* firms only matters slightly.

$$\text{DirAffected}_i = \begin{cases} 0 & \text{if Claim Ratio Category}_{r_j} = 1 \\ 1 & \text{if Claim Ratio Category}_{r_j} \geq 4 \end{cases} \quad (2.1)$$

In order to understand the indirect effect of a bank-level lending shift on firms, I estimate bank exposure to the disaster. In order to do so, I follow the identification employed by Koetter et al. (2016), which creates a measure of the banks' flood exposure, by examining the exposure of its associated firms. Each bank is assigned an

²³As an example, the data includes Contra Sicherheitsrevision GmbH, which is a small firm (15 employees) specializing in security and risk assessment for (large) companies and individuals. Its customers include insurance companies and many firms transporting valuables across Europe (tobacco, jewelry, cash). It is located in northern Brandenburg, far away from flooded regions. However, it maintains a relationship with Sparkasse Celle, which is a savings bank located much closer to the flooded areas. This bank maintains sufficient customer relationships to flooded areas to be classified as affected. It is unknown, why the firm maintains a relationship with this rather distant savings bank, although an internet search suggests its founder might have lived there. Nevertheless, concerning the 2013 flood, it is only connected to the region via its bank, not through any other discernible connection.

²⁴For an overview of the categories, refer to Figure 2.1.

individual flood exposure value, based on the proximity of its firm customers to the flood. Banks with more customers located closer to disaster regions will likely reallocate more funds toward the affected regions because their customer base is located there. This way of calculating the banks' flood exposure is similar to the method used in Cortes and Strahan (2017) and Chavaz (2016), although they use exposure to mortgage credit instead of firm customers. Specifically, the exposure measure is constructed by calculating the weighted average of the damage categories of each bank's firms, where the weight is the relative size of the firm, compared to all other firms the bank reports a payment relationship with. The damage categories for each firm are based on the firms' location in any of the nine damage categories reported, as shown in Figure 2.1. Equation 2.2 demonstrates how the bank-specific exposure measure is constructed.

$$\text{Exposure}_i = \sum_{j \in N_i} \left(\frac{\text{Assets}_{j,N}}{\text{Total Assets}_{N_i}} \times \text{Claim Ratio Category}_{r_j} \right) \quad (2.2)$$

Where N_i are the firms j of bank i located in region r_j . $\text{ClaimRatioCategory}_{r_j}$ is a value between 1-9 based on the firms' location in the counties as shown in Figure 2.1.²⁵ Because firm-bank connections vary slightly over time, I use pre-disaster exposure in the year 2012 for the analysis. Because any firm can report payment relationships with multiple banks (although the majority only reports one), in order to construct the firms' exposure to the *indirect* effect of the flood, I then average the exposure of all of the firm's banks. Based on this firm-specific *indirect* exposure of the firm's average bank, I construct a dummy variable, categorizing firms as affected and unaffected from the indirect (funding) shock. Equation 2.3 demonstrates this classification. AvgExposure_j is the average exposure of all banks i working with firm j . I classify all firms as affected, if their average bank's exposure to the flood is larger or equal to four, and as unaffected if it is smaller than 2.5, with all other average exposures omitted as buffer categories.²⁶

²⁵Note that because there is geographical variation in the banks' customers, the banks' exposure to the flood is bank-specific as opposed to county specific.

²⁶I show in Figures 2.7 and 2.8 that the exact thresholds chosen do not matter much for the results.

$$\text{IndirAffected}_j = \begin{cases} 0 & \text{if AvgExposure}_j < 2.5 \\ 1 & \text{if AvgExposure}_j \geq 4 \end{cases} \quad (2.3)$$

2.4.2 Estimation

Using this classification of indirectly affected firms, I estimate a difference-in-difference regression, using the classification of firms' indirectly affected via their banks. Equation 2.4 provides the estimation equation, where Y_{jt} are real outcome variables of firm j . Post is a dummy for the period after the disaster, i.e. it is 0 for $t = 2009-2012$ and 1 for $t = 2013-2014$. α_j are firm fixed effects, while $\alpha_r \times \alpha_t$ are county-time fixed effects. C_{kit-1} are firm-specific lagged control variables, specifically: cash, size (total assets), debt (current liabilities), capital ratio (common equity/total assets).²⁷

$$\ln Y_{jt} = \beta(\text{IndirectAffected}_j \times \text{Post}_t) + \alpha_j + \alpha_r \times \alpha_t + \sum_{k=1}^K \gamma_k C_{kjt-1} + \epsilon_{jt} \quad (2.4)$$

I choose three key dependent variables – Y_{jt} – in order to estimate the impact on the firms' real performance. First, I investigate the number of employees of the firm (in logs). This is one of the central variables investigated by the literature on the effects of credit supply frictions on firm performance (Chodorow-Reich, 2014; Popov and Rocholl, 2017). It is a key measure of firm performance and important from a policy perspective. Second, I investigate firms' investment. Investment is proxied as the change in firms' tangible fixed assets.²⁸ Investment is arguably most sensitive to changes in financing availability, as most investments are financed by bank-loans, especially in a bank-based system like Germany. Lastly, I also investigate changes in the fixed asset stock of firms, in order to investigate the response of the firms' capital stock.

Crucially, in this estimation I am able to control for firm and county×year fixed effects, because the classification into affected and unaffected categories is not only region-, but indeed firm- specific. This is particularly important for two reasons.

²⁷The exact definition of the control variables can be found in Table A.I.

²⁸Only balance sheet information is available in the data, such that the change in tangible fixed assets is the best proxy available.

First it removes many concerns about governmental aid biasing the estimates. With county×year fixed effects, the only assumption needed is that government aid was orthogonal to firm specific characteristics, i.e. that no firm was given preferential treatment over another firm. According to the flood aid plan of the German government this is indeed true, because all firms were reimbursed as a fraction of their actual damages (BMI, 2013a). Additionally most of the demand and trade effect concerns about the estimates are removed by using these fixed effects. Firms may of course not only have been exposed to the disaster via their banks but also via decreased demand from their customers or decreased supply from their suppliers. However, these kinds of exposures should be similar for firms in any unaffected region and independent of their banks' flood exposure, through which the affected variable is constructed. This enables a clear identification of the *indirect* shock.²⁹

– Figure 2.3 around here –

This described identification requires some firms exist outside the direct flood impact which still have exposure to banks affected by the flood via their firm customers. To confirm that this is indeed the case, I show the distribution of *indirectly* affected firms outside of directly affected regions in Figure 2.3. Panel (a) displays the mean of AvgExposure_j per region, while Panel (b) displays the maximum values. Directly affected areas are displayed in white, independent of the indirect exposure. The figure demonstrates that firms' exposure to flood-affected banks is diversely distributed around Germany, although regions close to the flood tend to have more indirect flood-exposure. This is to be expected and a crucial reason why county×year fixed effects are important. Panel (b) further demonstrates that there are at least some *indirectly* affected firms in most regions. This increases confidence in the fact that the identification indeed captures firms' indirect flood-exposure via its banks, and not some unobserved other (regional) correlation and demonstrates that there are at least some firms for which this paper's identification can be exploited in most regions.

– Table 2.1 around here –

²⁹To the extent that firms' bank choice may not be orthogonal to the firms flood exposure, for example because a firm might choose a bank in a region where it has many suppliers / customers, I conduct several robustness tests, by controlling for the bank-firm distance and sector×time fixed effects.

– Table 2.2 around here –

Descriptive statistics for all the variables used in the analysis of the paper can be found in Table 2.1. Detailed variable definitions are provided in Table A.I in the online appendix. Additional descriptive evidence for the sample of firms in non-flooded areas prior to the flood, separated by (indirectly) unaffected, omitted and affected firms can be found in Table 2.2. It also provides a ttest to test for mean differences between the unaffected and the affected group, which suggests that there are significant pre-flood differences mainly for firm’s banking characteristics. One of the important structural differences is that indirectly affected firms are further away from their banks than the average firm-bank-distance. This is almost by definition, because indirectly affected firms within the flood area are excluded so only the more distant relationships are left in the sample. Concerns that firms with larger bank-distances might respond structurally different to the natural disaster shock are addressed in Section 2.5.3, where I show that the results hold when using a post-disaster distance control and when removing pre-flood differences through propensity score matching.

2.4.3 Importance of bank capital in disaster shock transmission

There is some evidence that low-capital banks are more likely to transmit financial shocks to firms (Gan, 2007; Jiménez et al., 2017). This effect might stem from two factors: first, banks with lower capital ratios might have more trouble refinancing loans on the interbank market, as they are perceived as more risky. Second, in the case of a loan demand shock, banks near the margin of mandatory capital requirements may not be able to raise liabilities to finance new loans without violating regulations. A key part of this paper is to contribute to the understanding of whether regulatory bank capital is important for the transmission of unexpected regional shocks that are non-financial, neither in scale nor in origin. I thus add triple-interaction effects to my difference-in-difference analysis and estimate Equation 2.5 in the following way:

$$\begin{aligned} \ln Y_{jt} = & \beta_1(\text{IndirectAffected}_j \times \text{Post}_t) + \beta_2(\text{IndirectAffected}_j \times \text{Post}_t \times \text{cap}_j) \\ & + \beta_3(\text{cap}_j \times \text{Post}_t) + \alpha_j + \alpha_r \times \alpha_t + \sum_{k=1}^K \gamma_k C_{kjt-1} + \epsilon_{jt} \end{aligned} \quad (2.5)$$

I specify cap_j in two different ways. First, I create a bank-capitalization dummy, by splitting the sample into firms whose main bank had little regulatory capital and firms connected to a high regulatory capital bank prior to the flood. Specifically, I average each firms' main banks' capitalization in 2012 and 2013 and set the dummy equal to 1 if the firms' main bank is below the median of the distribution. I then investigate β_2 in order to find out whether such firms suffer significantly more from the *indirect* shock. Second, I estimate a continuous interaction with the pre-flood main banks' regulatory capital ratio, which allows me to investigate the effect of the main banks' regulatory capital ratio on different levels of the distribution.

2.4.4 Loan supply vs. loan demand

Natural disasters tend to be interpreted as loan demand shocks from the banks' perspective (Cortes and Strahan, 2017; Koetter et al., 2016; Cortés, 2014; Chavaz, 2016). Most convincingly Berg and Schrader (2012) demonstrate this finding with loan application data from Ecuador. This finding is intuitive, as bank-customers in flooded areas try to secure funds for rebuilding, possibly substituted by government aid and insurance payments. However, it cannot be ruled out that banks connected to flood-affected firms may also be subject to a supply shock, as they may have to write off or incur losses on loans to affected areas. While this interpretation is inconsistent with previous results from the literature, it is nevertheless an important concern. Uniquely, this paper's identification does not hinge on the shock being a loan *demand* shock to banks. Because I do not examine banks directly, but rather the banks' firm customers in non-flooded areas, it is mainly of importance that the bank was induced to reduce loans in unaffected areas. This is consistent with both a demand and a supply shock interpretation.

The *supply* shock interpretation would imply that banks cut their lending elsewhere, because they have to write-off loans in the affected areas, and might thus be induced to sell other assets quickly to compensate for the losses. A *demand* shock would result in the flood-exposed bank having to raise additional funds in order to satisfy demand in the affected area. The bank can do this by either refinancing the newly demanded loans (Chavaz, 2016) or by cutting lending elsewhere. The demand shock interpretation is heavily supported by the literature on the bank level, and

none of the results in this paper suggest another interpretation. Thus, I choose to interpret the results as a negative funding shock stemming from an increase in demand, although the supply channel cannot be ruled out and it is plausible that both mechanisms are at work at the same time.

2.5 Results

2.5.1 Indirect Effect

Based on previous literature and the flood characteristics presented in section 2.2, I hypothesize that banks shift lending from directly unaffected areas into directly affected areas. Thus, firms in unaffected areas are *indirectly* exposed to the natural disaster via a financing shock from banks. First, I examine whether firms' banks' flood exposure matters to the firms' loans as reported on the firms' balance sheet. Figure 2.4 suggests that while all firms experienced an increase in loans following the disaster, loans to firms connected to a bank with disaster exposure increased slightly less, especially in 2013. This indicates that the exposure of banks to the disaster area matters to the availability of loans for firms after the flood and is consistent with the idea that banks with high natural disaster exposure indeed reallocate lending away from unaffected areas. I test and discuss this mechanism more explicitly in section 2.5.3.

– Figure 2.4 around here –

Does this shift of bank lending away from unaffected regions translate into firm-level real effects? I test this by estimating Equation 2.4 using OLS with standard errors clustered at the firm level. In this difference-in-difference estimation, firms are classified as affected only if their average bank is sufficiently exposed to the flood via its firms' clients (Equation 2.3). Table 2.3 reports the results from this indirect shock to firms. Columns (1)-(3) report the results for firms located *outside* the flood radius, i.e. firms classified as not directly affected according to Equation 2.1. Columns (4)-(6) report the effects for firms located *inside* the flood radius. I show the results for the latter group for two reasons: First, to test if the effect of being affected by a bank funding shock is different between the directly affected and unaffected regions;

second, in order to get some indication of whether firms in directly affected regions might actually benefit from banks shifting their funds toward the disaster area.

– Table 2.3 around here –

The results indicate that there is a statistically significant negative effect on investment by about 16 percentage points³⁰ and a drop in fixed assets by 11% for indirectly affected firms in non-flooded regions. However, no effects of a funding shock on firms in terms of employment can be identified, a fact that might be surprising given the well-documented recent literature on the real effects of credit supply shocks on employment (Chodorow-Reich, 2014). This may be attributed to the relatively (compared to international financial crises) small shock induced to banks by this particular flood event. The results indicate that while firms reduce investment and fixed assets if their banks reallocate lending away from them, these effects may not be sufficient to cause changes in employment.³¹

Columns (4)- (6) of Table 2.3 give some indication that firms inside of the flooded regions are indeed benefiting from additional investment and capital stock induced by higher credit provision. The coefficients of investment and fixed assets have the expected positive sign (as banks channel more funds into the affected areas) and the latter is highly statistically significant. The interpretation of these results is somewhat difficult as direct and indirect effects are not well separated for these firms. However, it provides some indication that there is indeed a transfer of funds from areas outside the disaster, to areas within the disaster radius.

I suggest two possible explanations for these results. First, employment decisions of firms may be more rigid than investment decisions and changes in the firms' fixed assets, especially in a country with a relatively rigid labor market like Germany. Thus, a reduction in investment due to a credit supply contraction may not manifest in employment effects until a couple of years after the disaster. Because there are only two years after the flood in the data, these effects may still be too small to detect. The second explanation is that the funding shock to firms is simply too small to entail any

³⁰While this effect seems large, it is only 1/6 of the standard deviation of the indirectly but not directly affected firms. This large variation in investment is also not driven strongly by a few extreme outliers. Even when winsorizing at the 5% level, the effect would still only be roughly 1/3 of a standard deviation.

³¹I test additional dependent variables with less success. These results can be found in Table A.II of the online appendix.

employment effects regardless of the time horizon. Firms invest less, but may be able to finance day-to-day business activity from trade credit or their own capital until the financing restrictions ease. This interpretation would suggest that not all financial shocks entail negative employment consequences as suggested by the recent literature (Chodorow-Reich, 2014; Popov and Rocholl, 2017). Instead, smaller funding shocks, such as those from the Elbe flood to indirectly affected firms, can be buffered by firms without any implications for employment, despite the fact that they in fact induce a reduction of investment and capital stock.

2.5.2 Transmission of shocks and bank capitalization

The effect of banks' lending shift following natural disasters from unaffected to affected regions may not be the same for all banks. In order to satisfy the demand for new loans in disaster regions, where firms are looking to finance rebuilding efforts, banks must themselves be able to finance these new loans. In order to do this, banks have two options: raise funds on financial markets (increase liabilities), or shift existing lending away from other areas, for example by not renewing loans, increasing prices or increasing funding requirements (reducing assets).³² If banks opt for the former option, firms in non-flooded areas should be unaffected. If banks opt for the latter, firms in non-flooded areas may become "flooded through the back-door" - i.e. unintentionally affected by a funding reduction from banks exposed to the disaster.

Banks' ability to finance new loans without reducing loans elsewhere crucially depends on their ability to raise funds externally. If banks are financially constrained, they may not be able to do so and must raise funds internally. Banks are typically constrained by low capital ratios to raise new funds (Jiménez et al., 2017; Gan, 2007).³³ Low capital ratios impede the banks' ability to raise external funds for two reasons: first, low capital ratios imply higher risk of lending to that bank (Modigliani and Miller, 1958). As a result banks with higher capital ratios should be able to refinance new loans more easily. I term this mechanism the *risk channel*. The second reason

³²Banks can also raise equity capital on financial markets, although this might be more difficult in the short term, especially for non-listed banks, which constitute the majority of the sample. This option would increase equity, which is inconsistent with the empirical results presented.

³³There is a large debate on what exactly best constitutes banks' financial constraint. The aim of the paper is not to contribute to that debate, so I focus on the most simple and policy relevant measure: banks' regulatory capital ratios. The online Appendix provides (non significant) results using banks' liquidity as an alternative indicator, see Table A.III and Figure A.I.

is mandatory regulatory capital requirements. If a bank cannot fall below a certain regulatory capital threshold, it cannot borrow more without raising new equity at the same time. Because raising equity is often difficult in the short term, sudden shocks (such as a natural disaster) may force banks into raising funds by reducing other lending assets, because borrowing additional funds would violate capital regulations. Importantly, banks do not need to be exactly at the threshold for this effect to take hold, as they may choose to hold a (fixed) buffer above the regulatory requirement for other liquidity related reasons. I term this the *regulatory channel*. Both of these channels imply that banks have to cut back lending at the expense of firms, resulting in negative real effects for firms. The two channels are difficult to disentangle, yet the results provide some indication that both channels are at work, albeit for different firm-level outcomes.

– Table 2.4 around here –

First, I test if banks with low capital ratios are more prone to transmit disaster shocks to firms in unaffected regions in two ways, according to the regression specified in Equation 2.5. Columns (1)-(3) in Table 2.4 show a regression using a low capitalization dummy, which is set equal to 1 if the firms' main bank is in the lower half of all banks in terms of its pre-flood regulatory capital ratio.³⁴ I find that negative employment outcomes are significantly larger for firms whose main bank holds little capital. These firms reduce employment by roughly 17.6% more than their high-bank-capital counterparts.³⁵ For this sample-split there is no evidence that investment is driven by low capital firms; in fact the significant coefficient from the direct difference-in-difference interaction disappears. However, the coefficients go in the expected directions.³⁶ Columns (4)-(6) provide the results of a continuous interaction of the difference-in-difference term with the pre-flood main bank regulatory capital ratio. The results of the continuous interaction indicate that higher capital ratios in the firms' main bank imply larger employment, investment³⁷ and fixed asset

³⁴I take the average of 2012 and 2013 as the pre-flood regulatory capital ratio, as the flood occurs in mid 2013.

³⁵Because the dummy is cut at the median, the double-interaction coefficient implies the effect for high-capitalized banks. As a result the difference between the two is: $0.066+0.111=0.177$

³⁶p-value of the difference-in-difference coefficient = 0.2; p-value of the triple interaction coefficient = 0.157.

³⁷Although the effect is not statistically different from zero: p-value = 0.105

stock effects, balancing the negative effect of the simple difference-in-difference estimate. With a negative baseline employment effect of 17%, an increase in the main banks' capital ratio by 1 percentage point reduces this effect by about 0.99%. This means that for banks at the margin of the EU tier 1 capital requirement of 6%, the reduction in associated firms' employment would be 11%.³⁸ For investment the slope of the increase in the capital ratio is steeper: the baseline effect is a reduction in investment by more than 50 percentage points, although each additional percentage point of capital decreases the effect by 2.1 percentage points. The steepest curve occurs with regard to the fixed asset stock of firms: a negative baseline effect of 62% with each additional point in capital reducing the effect by roughly 3%.

– Figure 2.5 around here –

To further investigate the transmission of shocks at different bank capital ratios, Figure 2.5 displays margin-plots for the continuous interactions presented in Table 2.4. As higher regulatory capital ratios imply larger (differential) investment and employment effects, the slope of these curves is increasing. Capital ratios above roughly 20% are found to have positive significant employment effects, while capital ratios below roughly 20% imply a significant reduction in investment and fixed asset stock. This is an interesting finding as it indicates that the negative investment effect is driven by banks at the lower bound of the regulatory capital ratio (*regulatory channel*), while the employment effects appear to be better explained by the *risk channel*. Because investment and employment appear to be closely related in a firm context, it is not quite clear why this dichotomy exists. Nevertheless my results imply that both channels are important for final firm outcomes.

Overall these results clearly indicate that banks' capital ratios matter for real economic performance of firms. Larger capital ratios are helpful in order to prevent banks from spreading shocks to other sectors of the economy who have no direct exposure to the shock themselves. In fact, negative employment effects are only realized if the firms' main bank is constrained by a low capital ratio. It is not clear if higher mandatory capital requirements are a good solution to this problem, as my results suggest that firms reduce investment and fixed assets most, if their bank is constrained by the

³⁸Since the average bank has a capital ratio of about 17%, the effect is roughly zero around the mean ($17 \times 0.99 - 16.7 = 0.13$).

mandatory capital requirement. Since my shock is not a macro-scale shock, even capital requirements tied to macroeconomic conditions would not remove these concerns. This implies that banks have to be given other incentives to increase capital, if the goal is to minimize the collateral damage to firms caused by frictions in the financial sector. It is important to recognize that the negative real effects implied by low bank capital ratios can be efficient from the banks' perspective. It is reasonable and perhaps intended that banks distribute local risk from one region to another. However, my results show that firms cannot, or at least do not hedge against this risk of banks shifting lending and thus, suffer real consequences as a result. Because this effect can be mitigated by higher bank capital ratios, it implies a previously disregarded firm level benefit – a positive externality – of higher bank capital. When making welfare calculations of the optimal bank capitalization – which this paper does not partake in – the results suggest that these positive externalities should be taken into account.

2.5.3 Robustness and mechanism

Robustness Next I test whether the results hold up to several robustness tests. Table 2.5 presents the robustness checks for Column (1) of Table 2.5. Robustness tests for Columns (2) and (3) can be found in the Appendix (Table A.IV+A.V). First, I address the challenge of autocorrelation in difference-in-difference estimation raised by Bertrand et al. (2004). In order to overcome the problem, I collapse the sample into the pre- and post-period and run a cross-sectional regression on the new sample. The results are displayed in Column (1), and are very similar to the original result; firms connected to low capital banks decrease employment by roughly 9%. Column (2) represents a regression using the same length of pre- and post periods (i.e., 2010-2014); here the results are almost exactly the same.

Next, I test whether the data satisfies the parallel trends assumption - which is crucial to difference-in-difference analysis - in two ways. First, I inspect the trends of the *indirectly* affected and unaffected firms in Figure 2.6 descriptively. Prior to the flood, trends for the means of all three dependent variables run parallel, although with varying level differences. In order to confirm that the triple interaction does not suffer from concerns regarding the parallel trends assumption, Column (4) provides a placebo regression. Here, the year 2011 is set as the flood year, with the years

2013-2014 being excluded. As can be seen, the results are not significant, indicating that the actual flood does not capture differing time trends. I mute concerns that pre-flood trends are driving my results by estimating my results on a 1:1 propensity score matched sample. These results are provided in Table A.VI of the online appendix and are very similar to the main regression, suggesting that pre-crisis differences are not driving my results.

Additionally, there is a concern that firms' bank choice is not orthogonal - even within region - to the flood, or more specifically the effects of the flood. Mainly, it is possible that firms choose banks where their supplier / customers are located. If that were the case, my effect might be capturing direct flood exposure via channels other than lending. I provide two tests to account for this possibility. First, I include an interaction with the post dummy and the firm-bank distance. If my effect is driven by the distance between banks and firms this coefficient should pick up the variation. Column (4) shows that indeed this interaction is statistically significant, however it does not eliminate the original result. Second, in order to mute concerns that "specialty" banks are driving the result, I additionally include sector \times time fixed effects, again without a change in the result. I exclusively examine SMEs as an additional robustness test, because SMEs are more likely to have a very local customer base. The results, which are provided in Table A.VII, are again very similar to the baseline, removing some of the concerns that my results are not driven by a reduction in bank lending.

The effects might be driven by over-fitting the data with fixed effects, thus the results of regressions with only firm fixed effects (Column(6)) and no fixed effects (Column(7)) are shown. I provide more variation in fixed effects in Table A.VIII of the online appendix. In all regressions, the results stay very similar to the original result.

– Table 2.5 around here –

– Figure 2.6 around here –

There may be concerns that the results are driven by choice of the affected threshold in Equation 2.3. In order to demonstrate that this is not the case, and that the effect is in fact robust to varying the threshold levels, I rerun the regression from

Equation 2.5 at different thresholds and plot the resulting coefficients in Figure 2.7 and Figure 2.8. Figure 2.7 plots the coefficient of β_1 and β_2 , while varying the lower bound of the indirectly affected group. As can be seen, the choice of the lower bound matters only slightly, as both β 's vary very little under different lower bound threshold choices. Figure 2.8 similarly plots β_1 and β_2 , while varying the upper bound of the indirectly affected group.³⁹ For all dependent variables, fixing the upper bound too close to the lower bound - i.e. the control group - will result in insignificant results. For employment the results are significant at 3.5, 4 and 4.5, while at 5 they become insignificant again, due to the lower number of observations. For investment, every choice above 4 yields (close to) significant results. Fixed assets are indeed only strictly significant at the 4 threshold. These results are in line with the expectations. The choice of the control group does not matter much, but choosing an affected group too close to the unaffected group will result in insignificant results, as indirectly affected and unaffected groups become indistinguishable from each other.

– Figure 2.7 around here –

– Figure 2.8 around here –

Real effects of firms connected to banks with little capital should be caused by a reduction in lending from banks. Because I do not have access to loan level data, I can only rely on the firms' liabilities to confirm this mechanism. This has several drawbacks. First, most firms only report non-detailed data on liabilities, which are difficult to disentangle. Additionally, firms might substitute a reduction in bank lending, by borrowing from other banks or relying on trade credit. Doing so might restore their liabilities, but might still be associated with switching costs which can lead to negative real outcomes. Table 2.6 displays the result of the baseline estimation for three dependent variables that might come closest to catching the effect of a credit reduction by banks: loans, long-term debt and capital. The first two are the best proxies of bank lending available in the data, while the latter indicates if firms' capital remains unaffected by the bank-lending mechanism.

– Table 2.6 around here –

³⁹In Figure 2.7 the upper bound remains fixed at 4, while in Figure 2.8 the lower bound remains fixed at 2.5.

The results indicate that firms indeed experience a reduction in both loans and long-term debt, if they are connected to a bank with a low capital ratio following the disaster. The effect on loans is significant at the 10% level, however only for the dummy interaction (Columns (1)-(3)), not for the continuous interactions⁴⁰ (Columns (4)-(6)). Nevertheless, this effect demonstrates that the negative real effects experienced by firms are indeed driven by a reduction in lending from low-capitalized banks. Capital increases for firms with low-capital banks, indicating that firms might be substituting liabilities with equity if their bank reduces lending. However these effects are non-significant. Overall the evidence that the assumed bank-lending shift appears on the firms' liability side is statistically weak, but the directions of the signs are consistent with the interpretation that banks reduce lending to unaffected firms following a natural disaster.

These results give some indication that the effects on employment, investment and fixed assets are likely driven by a reduction in lending from banks. The limited statistical significance of the results can be attributed to two factors. First, the liability side variables might simply be too noisy to pick up statistically significant effects. As loan data is not available, firms' liabilities side may not suffice. The second potential explanation involves switching costs: as banks reduce funding to firms, firms may be able to obtain funding from elsewhere, potentially other banks. This is reasonable especially during rather stable financial times in Germany in 2013 and 2014.⁴¹ Nevertheless switching banks may have led to significant costs for firms, which then have resulted in negative real consequences, in terms of employment and investment (Degryse et al., 2011).

2.5.4 Extensions

Relationship lending Additional banking characteristics may play a role for lending shifts following a natural disaster. Prior literature indicates that relationship banking (Boot, 2000) might play a twofold role following natural disasters. First, relationship banks may provide more lending to areas affected by the natural disaster (Cortés, 2014), because they have more proprietary information about borrowers,

⁴⁰I provide the marginsplots for the continuous interactions in Figure A.II of the online appendix.

⁴¹This may not have been easily possible during more general financial crises analyzed with regards to real effects in previous papers (Chodorow-Reich, 2014).

giving them a competitive advantage in times of crisis. As a result such banks may need to withdraw more funding from unaffected areas, simply because they lend more to disaster-affected areas. However, relationship banks may be less inclined to restrict credit to other firms, because they want to retain their lending relationship also in unaffected areas. They might thus shift less lending, or be more inclined to refinance their lending to disaster areas or fund it by raising new equity.

– Table 2.7 around here –

Table 2.7 provides two tests of differential effects for relationship banking indicators. First, I test whether firms, whose main bank is located closer in terms of geographical distance are more or less affected by the indirect shock from the flood. Columns (1)-(3) report the continuous interaction of the difference-in-difference estimator with the firm-bank distance in 100 kilometer intervals. The negative coefficient for the triple interaction term demonstrates that for firms whose banks are located further away, employment is reduced by about 2.3% more per 100 kilometers. However the other dependent variables appear not to be significantly affected, although they also show a negative coefficient. This result lends some credence to the hypothesis that relationship banks do not transfer shocks as much as arms-length lenders, or are at least able to do so without impacting borrowers in unaffected markets. Next, I test whether the number of banks for each firm matters, because firms with more relationships are more likely to be arms-length borrowers. I find that all variables are differentially unaffected. This provides some evidence that arms-length borrowing may not matter – neither negatively nor positively – for firms suffering from a random funding shock. ⁴²

Overall, the data provides only a weak indication that relationship banking may compensate slightly for the indirect shock, or stated differently, that relationship banks do not shift lending to the extent that arms-length lenders do, although the results are not consistent across the two indicators, or the three variables used. The result is somewhat surprising, given that relationship lenders might be especially inclined to provide lending to affected areas, because of their advantage in acquiring information about the future profitability of borrowers following the disaster (Koetter et al., 2016;

⁴²I provide the marginsplots for the interactions with these continuous relationship variables in Figure A.III and Figure A.IV in the online appendix.

Cortés, 2014). My findings suggest that for relationship banks, this does not occur at the cost of connected, yet not directly disaster affected firms. This may be explained by the fact that such banks are able to more credibly resell new loans on secondary markets (Chavaz, 2016) or because they tend to have larger capital or liquidity buffers they can exploit in crises.

– Table 2.8 around here –

Bank type Germany’s banking system is dominated by three major categories of banks: (government) savings banks, cooperative banks and commercial banks. The bank type may be important in explaining the extent of banks’ lending shifts. Government banks may be pressured into providing more loans to disaster-affected businesses, because it is politically beneficial for local and regional politicians (Carvalho, 2014). As a result, government banks might shift more lending from unaffected into affected regions. Government banks also constitute a major difference to the previous papers looking at bank lending in the aftermath of natural disasters in the United States (Chavaz, 2016; Cortes and Strahan, 2017). German savings and cooperative banks are banks that are typically restricted to a certain geographical area, although customers can also bank with more distant savings banks on occasion.⁴³ Nevertheless, they typically do not own distant branches, from which they are likely to shift lending to disaster areas. It is thus interesting whether these local German banks react differently to the disaster demand than commercial banks. I test this idea by interacting the difference-in-difference coefficient with a dummy for each of the three major bank types. The results are provided in Table 2.8. There is some evidence that government banks indeed cause a differentially larger reduction in real effects. The coefficients for all three dependent variables are negative, although only the effect on investment is statistically significant. This result supports the interpretation that government savings banks may have shifted more lending to disaster areas at the expense of other customers, an effect that may be caused by political pressures. Furthermore there is an indication that firms working with a cooperative bank experience a lower reduction

⁴³Savings banks are not allowed to actively acquire customers outside of its own region, but also do not have to reject them if they are actively sought out. Additionally bank-customers may stick with their regional savings banks, even if they change locations as savings banks cooperate nationwide for certain banking services such as cash withdrawals.

in investment (Column (6)) than other banks. This result is in line with an emerging literature demonstrating that cooperative banks can more easily smooth shocks (Ferri et al., 2014). It is also supportive of the idea that government banks may have been pressured by local politicians to shift more lending, as cooperative banks have a similar local business model, yet they are not controlled by local politicians.

– Table 2.9 around here –

Firms’ financial constraints The transition of financial shocks to the real economy might also depend on the financial constraint of individual firms. In fact, if firms do not face any financial constraints, a reduction in bank credit by their banks as a result of loan reallocation following natural disasters should not matter to the firm at all, as it could substitute with alternative financing options, such as cash reserves or its own capital. Table 2.9 demonstrates the results of a continuous interaction with both the firms’ pre-flood capital ratio (Columns (1)-(3)) and the firms’ pre-flood cash holdings (Columns (4)-(6)). The results in Column (2) shows that the negative investment effect of roughly 30 percentage points is reduced by 0.43 percentage points per additional percentage point in the firms’ capital ratio. However employment and fixed assets do not show any differential effects. The firms’ liquidity on the other hand does not appear to be driving the results.⁴⁴ This result underscores the importance of capital for the cross regional transmission of shocks. Larger capital buffers, rather than larger liquidity holdings play an important role on the bank and on the firm side, if real shocks are to be buffered successfully.

2.6 Conclusion

This paper investigates the effect of an exogenous bank funding shock on firms’ real outcomes in terms of employment, investment and the stock of fixed assets. I contribute to the growing literature on the real effects of financial disruptions (Chodorow-Reich, 2014; Ongena et al., 2015), by examining a funding shock caused by banks’ lending shifts following a natural disaster (Cortes and Strahan, 2017). As banks redirect lending from non-disaster to disaster areas, firms unaffected by the disaster, yet

⁴⁴Figure A.V and Figure A.VI in the online appendix show the marginsplots for these continuous interactions.

with a connection to a disaster exposed bank, reduce investment by about 16 percentage points and fixed assets by 11%. Firms connected to banks with low capital ratios, are most affected by such "flooding through the back door", as they experience an additional significant reduction in employment by 11%. These results imply that even small regional shocks can be transmitted through the banking sector to otherwise non-shocked firms, especially if the level of capital in banks (and firms) is small. As small regional shocks – which do not necessarily have to be natural disasters – are fairly common, a badly capitalized banking system may be propagating shocks across firms instead of absorbing them.

To identify these effects, I use a matched firm-bank level dataset during the flooding of German regions in 2013, one of the largest natural disasters in recent German history. First, I use the location of banks' firms in order to gauge the bank's exposure to the flood. Then I investigate the effects on firms in non-flooded areas, if they hold a relationship to banks with an exposure to the flood, and test if such firms perform differently with regard to employment, investment and fixed assets, especially when the exposed bank has little capital.

The results hold up to several robustness tests, including collapsing and varying the sample period. The parallel trends assumption also passes both visual inspection and a placebo test. I find some indications that these changes in real effects are in fact driven by a reduction in lending from banks. The identification of a reduction in lending might be difficult as firms substitute other forms of financing, in which case the observed real effects might be due to switching costs (Degryse et al., 2011) and not necessarily an absolute reduction in lending levels.

Additionally, I find some small support that relationship banks cause smaller reductions in firm-level employment of indirectly affected firms, despite the fact that the literature demonstrates they shift more loans into disaster-affected regions (Cortés, 2014; Koetter et al., 2016). This might be the case because relationship banks are able to more credibly resell their loans on secondary markets (Chavaz, 2016), or because they tend to have larger capital buffers. I find some support for the idea that bank type matters for the real effects of banks' lending shift, as government savings banks are more inclined, while cooperative banks are less inclined to pass disaster shocks to otherwise unaffected firms.

My results imply the importance of high bank capital ratios, not only to prevent bank failure and systematic collapses of the banking market, but additionally in order to prevent propagation of smaller (real economic) shocks through the financial system. For banks, this shock propagation might be efficient ex-ante, but my results demonstrate that firms suffer real consequences if the bank does not hold sufficient capital. This provides strong evidence that even on a limited regional scale, low bank capital may carry previously disregarded negative externalities, and policies aimed at increasing banks' capital may provide benefits even for non-systemically relevant banks.

Tables and Figures

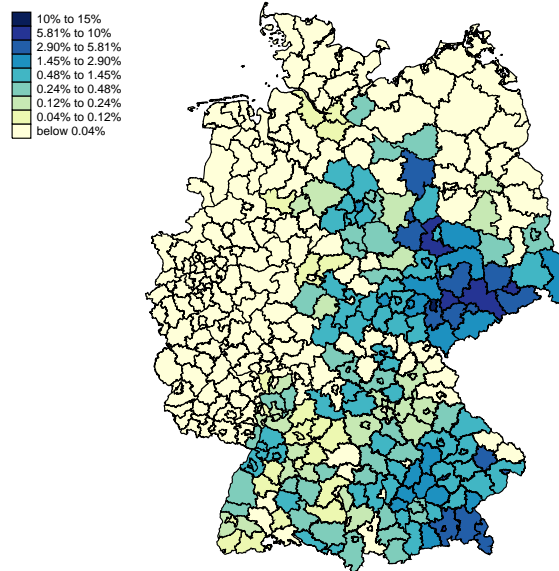


FIGURE 2.1: Affected German counties by damage categories

This Figure shows the distribution of the damage sustained from flooding in Germany from May 25th through June 15th 2013, by German counties (Kreise). Flooding damage is reported as the percentage of flood-insurance contracts activated during the period and is reported in 9 categories, from 0 to 15%. Data is provided by the German Association of Insurers.

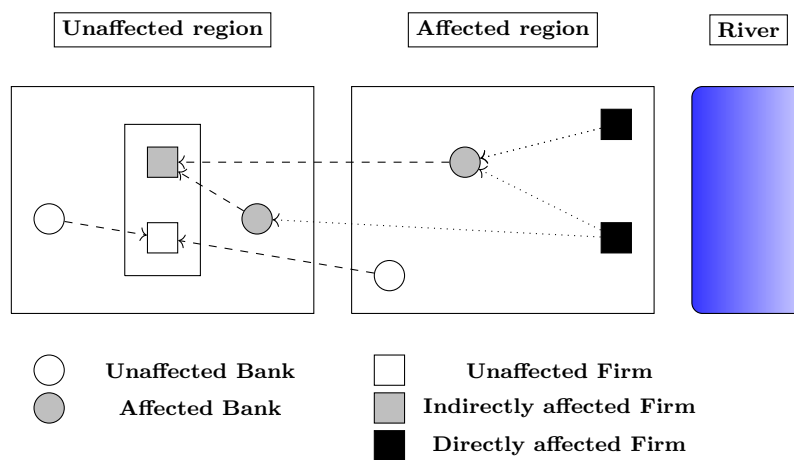


FIGURE 2.2: Indirectly affected firms: Illustration

This figure illustrates the identification of indirectly affected firms. Firms are depicted as rectangles, banks as circles. Directly affected firms (solid black) are identified by their location in the affected region. Affected banks (grey circle) are defined as affected by their customers location. As such they can also be located outside of the affected region (Koetter et al., 2016). Indirectly affected firms are identified, if their average bank is affected by the flood (grey rectangle). Region \times time fixed effects imply a strictly within region comparison between indirectly affected firms and not-indirectly affected firms (as illustrated by the rectangular framework in the unaffected region).

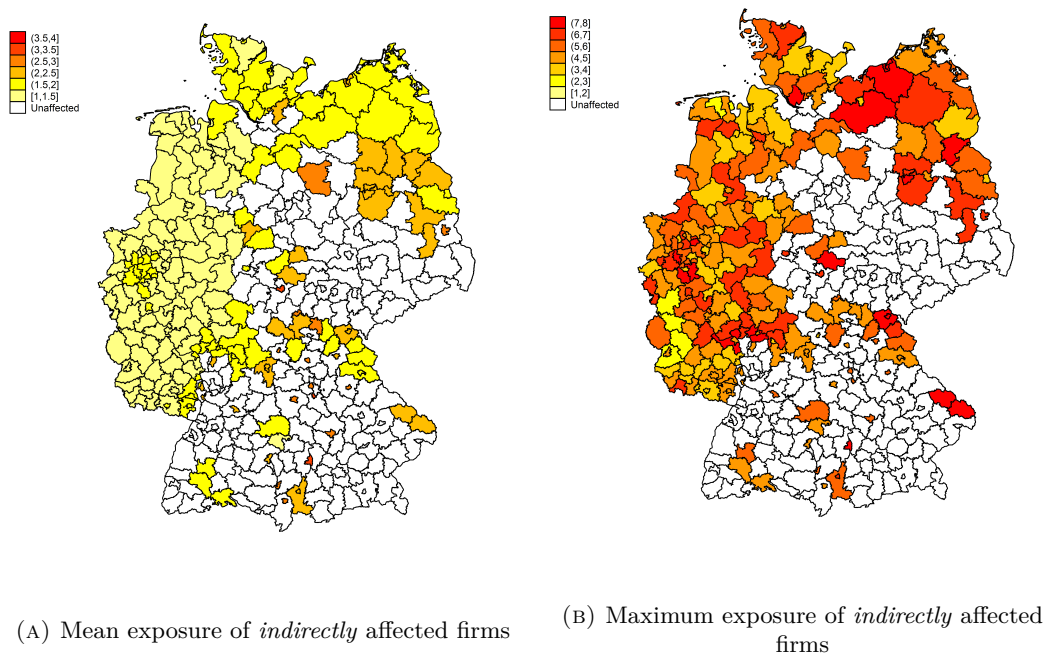


FIGURE 2.3: Distribution of *indirect* exposure of firms in non-directly affected areas

This figure shows the distribution of the firms' average exposure of its banks to the disaster (*AvgExposure*) by German regions. Section 2.4.1 describes how this measure of firms' indirect exposure to the disaster via its banks is derived. Panel (a) shows the mean exposure of all firms in the region. Panel (b) shows the maximum exposure of firms in the region. Labels are displayed in the upper left corner of each graph.

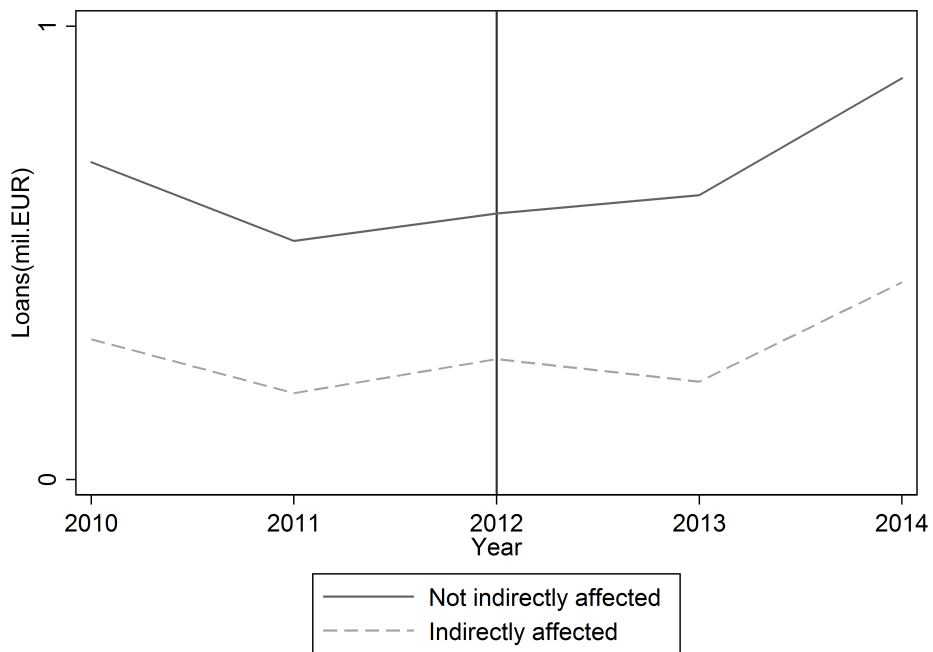


FIGURE 2.4: Development of loans by firms indirectly affected by the flood (full sample)

This figure shows the development of firm loans (liabilities) over time (in levels), differentiated by whether the firms are exposed to an indirect shock from the flood via their banks (dashed line) or not (solid line), according to the affected category described in Equation 2.3. Values are displayed for the years 2009-2014.

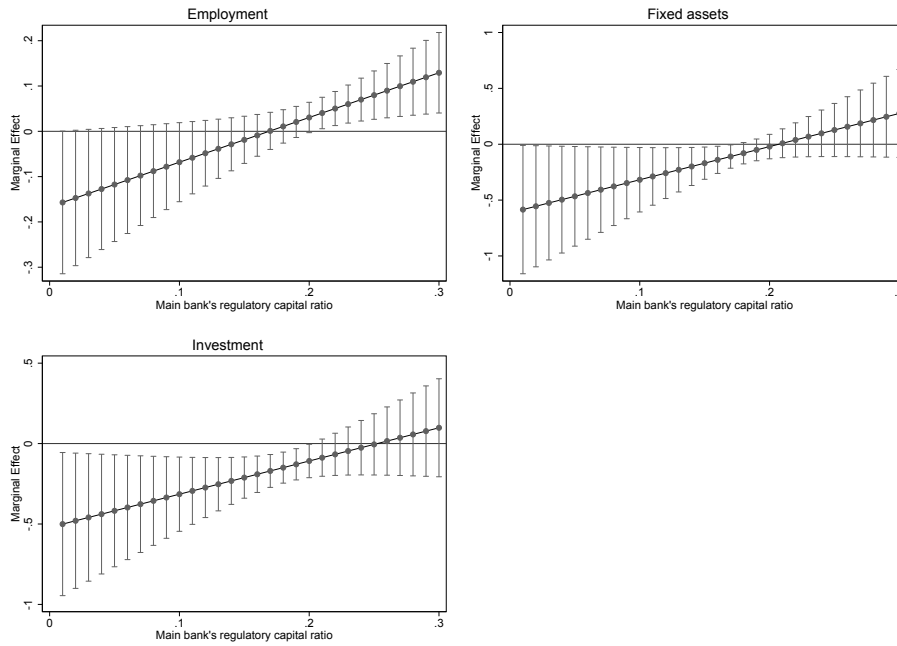


FIGURE 2.5: Marginal effect of the interaction with the difference-in-difference coefficient at different values of main bank's capitalization:
Real effects

This figure shows the marginal effects of the difference-in-difference estimation of being affected by a bank funding shock resulting from flooding in other regions at different values of the firms' main bank's capital ratio (according to the regression in Table 2.4). Capital ratios are indicated as shares on the x-axis. Each graph represents the marginal effects for a different dependent variable, as indicated by its title. Bars indicate 90% confidence intervals. The results of the regression are shown in Table 2.4.

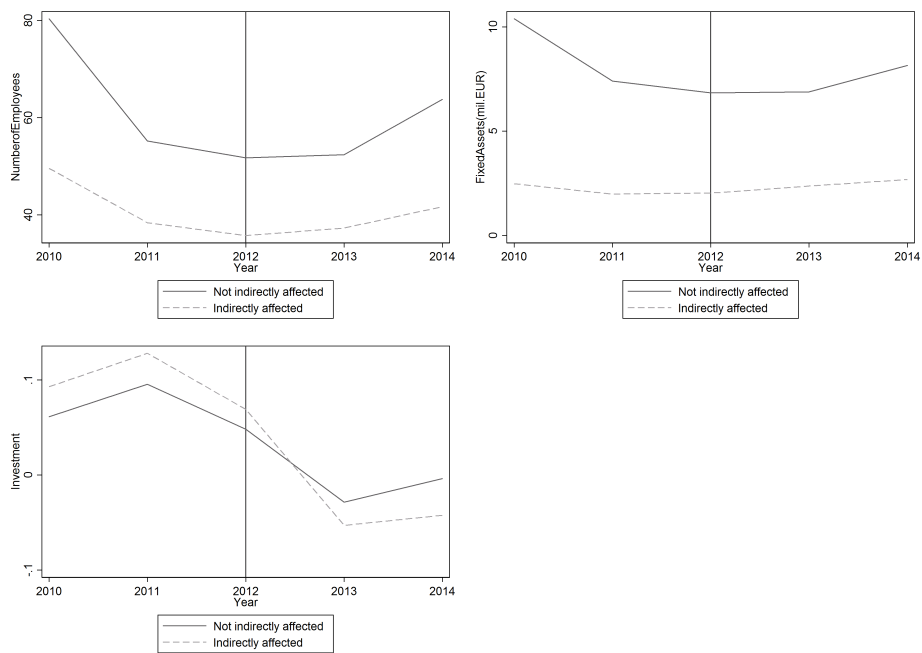


FIGURE 2.6: Parallel trends of dependent variables: Indirect effect

This figure shows the means of the key dependent variables over time (in levels), differentiated by whether the firms are exposed to an indirect shock from the flood via their banks (dashed line) or not (solid line). Only firms outside of directly affected regions are displayed. Values are displayed for the years 2009-2014, except for the investment variable where 2009 is excluded, because it is a growth variable.

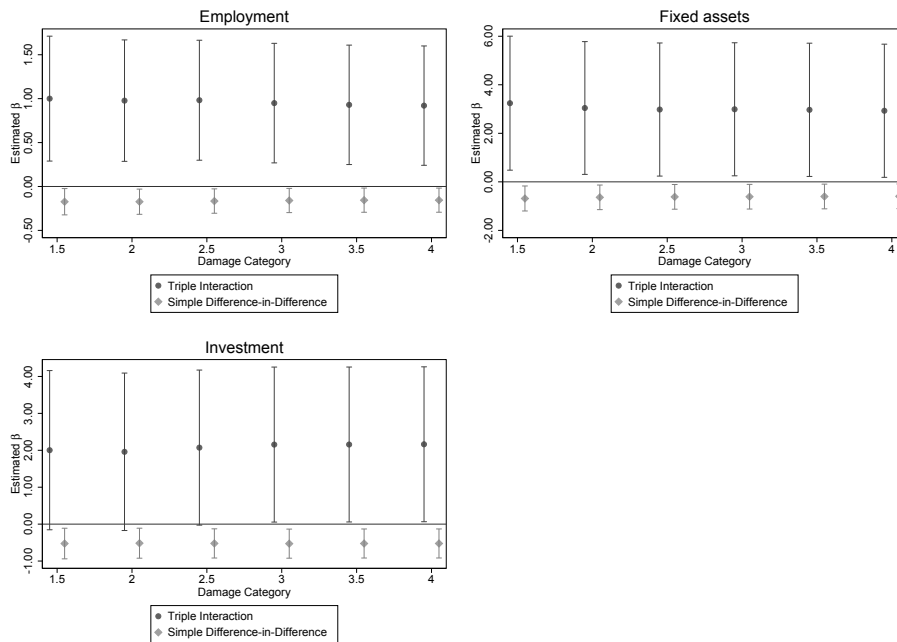


FIGURE 2.7: Varying the lower bound of the *indirectly* affected threshold

This figure displays the estimated β coefficients from Equation 2.5 using different thresholds of the indirectly affected variable (IndirAffected). Each graph indicates results for a different dependent variable as indicated by its title. The continuous triple interaction effect of the regression (β_2) is depicted by the dark circle, while the simple difference-in-difference effect is depicted by the light square (β_1). The threshold for indirectly affected banks is set to ≥ 4 , and the thresholds for unaffected banks varies according to the values displayed on the x-axis. If the unaffected threshold is set to < 1.5 , the number of unaffected banks is too low for reasonable estimates. Bars represent 90% confidence intervals.

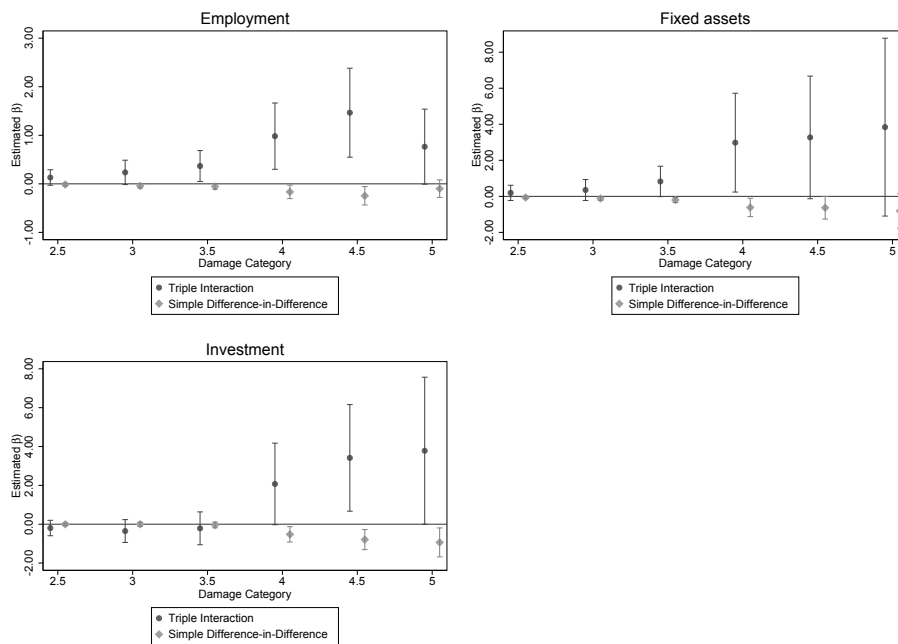


FIGURE 2.8: Varying the upper bound of the *indirectly* affected threshold

This figure displays the estimated β coefficients from Equation 2.5 using different thresholds of the indirectly affected variable (IndirAffected). Each graph indicates results for a different dependent variable as indicated by its title. The continuous triple interaction effect of the regression (β_2) is depicted by the dark circle, while the simple difference-in-difference effect is depicted by the light square (β_1). The threshold for unaffected banks is set to values lower than 2.5 and the upper thresholds varies according to the values displayed on the x-axis. If the affected threshold is >5 , the number of affected banks is too low for reasonable estimates.

TABLE 2.1: Descriptive Statistics

	N	Mean	SD	Min	Max
Identification					
DirAffected	785080	0.32	0.47	0.00	1.00
IndirAffected	763713	0.15	0.36	0.00	1.00
Dependent Variables					
Number of employees	986555	55.79	832.30	1.00	276418
Investment	986555	0.04	0.82	-19.47	20.81
Fixed assets (mil.EUR)	986555	7.10	221.66	0.00	45339
Control Variables					
Cash (mil.EUR)	986555	1.00	24.03	0.00	7089
Total assets (mil.EUR)	986555	13.04	356.67	0.00	85276
Capital ratio	986555	0.33	0.26	0.00	1.00
Current liabilities (mil.EUR)	986555	3.53	125.47	0.00	28261
Channel					
Loans (mil.EUR)	587914	0.62	13.09	0.00	3185
Long term debt(mil.EUR)	984851	2.82	76.19	0.00	30438
Capital (mil.EUR)	986555	5.22	145.28	0.00	37062
Firms' banking characteristics					
Main banks' regulatory capital ratio (cap_pre)	838865	0.17	0.04	0.08	0.78
Distance to main bank (100 km) (dist_pre)	946383	1.06	1.46	0.00	7.95
Number of banks per firm (bank_count_pre)	971025	1.68	0.88	1.00	7.00
Savings bank dummy (savings)	971025	0.41	0.49	0.00	1.00
Cooperative bank dummy (coop)	971025	0.20	0.40	0.00	1.00
Commercial bank dummy (comm)	971025	0.36	0.48	0.00	1.00

This table presents summary statistics for all variables used in the subsequent regressions. DirAffected is a dummy variable based on the firms' location with regard to the flood (c.f Figure 2.1), according to Equation 2.1. It is set equal to 1 if the firm is located in a county with a damage category of 4 or higher and set equal to 0 if it is located in a county with category 1. IndirAffected is a dummy variable constructed via measuring the exposure of the firm to the flood via its banks, according to Equation 2.2 and 2.3. It is set equal to 1 if the average exposure of the firm's banks is ≥ 4 and set equal to 0 if it is < 2.5 . Employment, fixed assets and invest are the main dependent variables. Employment and fixed assets are displayed in levels, but used as logs in the regressions. Invest is a proxy for investment, the change in $\ln(\text{total fixed assets})$ from $t-1$ to t . Cash, total assets and current liabilities are reported in levels, but included as logs in the regressions. Capital ratio is measured by common equity divided by total assets. All control variables are used in as first lags in the regressions. Banks regulatory capital ratio is each firm's main bank's regulatory capital ratio prior to the flood as a mean of 2012 and 2013 values. Distance to main bank is the distance between the center of the postal code of the firm, and the center of the banks postal code, scaled to 100 km intervals. Number of banks per firm refers to the number of bank relationships recorded for each firm. Firms' banking characteristics are taken as pre-flood levels. All firm-level variables are taken from the Amadeus database. All bank-level information stems from Bankscope.

TABLE 2.2: Pre-flood descriptive statistics for non-directly affected firms, by indirectly affected categories

	Indirectly unaffected			Omitted			Indirectly affected			ttest (δ)			
	N	N_firm	Mean	SD	N	N_firm	Mean	SD	N	N_firm	Mean	SD	ttest
Dependent Variables													
Number of employees	283170	122326	53.32	724.08	21314	9537	65.42	558.68	1096	499	40.26	167.49	0.60
Investment	283170	122326	0.07	0.82	21314	9537	0.07	0.88	1096	499	0.13	1.00	-2.40
Fixed assets (mil.EUR)	283170	122326	5.93	166.33	21314	9537	7.06	57.47	1096	499	2.32	13.23	0.72
Control Variables													
Cash (mil.EUR)	283170	122326	0.90	16.25	21314	9537	1.22	19.77	1096	499	0.47	4.36	0.87
Total assets(mil.EUR)	283170	122326	11.34	269.07	21314	9537	15.27	183.91	1096	499	4.58	26.70	0.83
Capital ratio	283170	122326	0.32	0.26	21314	9537	0.33	0.26	1096	499	0.31	0.27	1.45
Current liabilities (mil.EUR)	283170	122326	3.08	109.18	21314	9537	4.15	70.24	1096	499	1.17	8.35	0.58
Channel													
Loans(mil.EUR)	182782	92633	0.55	12.43	13867	7217	0.73	5.97	640	351	0.27	1.36	0.57
Long term debt(mil.EUR)	282581	122202	2.56	62.47	21278	9528	3.06	32.61	1091	498	1.30	4.38	0.67
Capital (mil.EUR)	283170	122326	4.44	111.86	21314	9537	5.70	56.60	1096	499	1.87	14.93	0.76
Firms' banking characteristics													
Main banks' regulatory capital ratio (cap_pre)	245589	105716	0.17	0.04	15602	6838	0.17	0.05	1025	463	0.17	0.04	-7.59
Distance to main bank (100 km) (dist_pre)	274735	117716	0.82	1.21	19640	8333	1.78	2.04	1061	477	1.49	1.60	-17.88
Number of banks per firm (bank_count_pre)	279854	120033	1.72	0.91	20251	8586	1.62	0.87	1078	486	1.30	0.61	15.20
Savings bank dummy (savings)	279854	120033	0.45	0.50	20251	8586	0.22	0.41	1078	486	0.53	0.50	-4.90
Cooperative bank dummy (coop)	279854	120033	0.18	0.39	20251	8586	0.22	0.41	1078	486	0.39	0.49	-17.29
Commercial bank dummy (comm)	279854	120033	0.34	0.47	20251	8586	0.49	0.50	1078	486	0.07	0.25	19.02

This table presents pre-flood summary statistics for firms in unaffected regions, separated by their categorization into indirectly unaffected, omitted and indirectly affected banks (Equation 2.3). Columns (1)-(3) report statistics for all firms who are classified as unaffected ($\text{AvgExposure}_j < 2.5$), Columns (4)-(6) for those that are omitted as buffer ($2.5 < \text{AvgExposure}_j < 4$) and Columns (7)-(9) for those classified as indirectly affected ($\text{AvgExposure}_j \geq 4$). Column (10) reports the results of a difference in means test, between unaffected and affected firms, t-statistics are reported. Employment, fixed assets and invest are the main dependent variables. Employment and fixed assets are displayed in levels, but used as logs in the regressions. Invest is a proxy for investment, the change in $\ln(\text{total fixed assets})$ from t-1 to t. Cash, total assets and current liabilities are reported in levels, but included as logs in the regressions. Capital ratio is measured by common equity divided by total assets. All control variables are used in as first lags in the regressions. Banks regulatory capital ratio is each firm's main bank's regulatory capital ratio prior to the flood as a mean of 2012 and 2013 values. Distance to main bank is the distance between the center of the postal code of the firm, and the center of the banks postal code, scaled to 100 km intervals. Number of banks per firm refers to the number of bank relationships recorded for each firm. Firms' banking characteristics are taken as pre-flood levels. All firm-level variables are taken from the Amadeus database. All bank-level information stems from Bankscope.

TABLE 2.3: Indirect effect of flooding on firms real outcomes

	Outside directly affected regions			Inside directly affected regions		
	(1) Employment	(2) Invest	(3) Fixed Assets	(4) Employment	(5) Invest	(6) Fixed Assets
Post × IndirAffected	-0.003 (0.019)	-0.162*** (0.048)	-0.106** (0.050)	-0.002 (0.004)	0.012 (0.012)	0.028*** (0.010)
L.Cash	0.001*** (0.000)	0.027*** (0.001)	0.005*** (0.001)	0.002*** (0.001)	0.031*** (0.002)	0.001 (0.002)
L.Total Assets	0.091*** (0.003)	-0.512*** (0.014)	0.388*** (0.011)	0.099*** (0.005)	-0.475*** (0.025)	0.411*** (0.018)
L.Current Liabilities	0.000 (0.000)	-0.000 (0.000)	0.000 (0.000)	0.001*** (0.000)	-0.000 (0.001)	0.000 (0.000)
L.Capital	0.037*** (0.006)	0.244*** (0.025)	0.225*** (0.016)	0.034*** (0.010)	0.259*** (0.040)	0.269*** (0.030)
N	496,858	496,858	496,858	152,090	152,090	152,090
Number of Firms	122,825	122,825	122,825	37,713	37,713	37,713
Treatment Group	499	499	499	28,155	28,155	28,155
Within R ²	0.015	0.032	0.035	0.020	0.032	0.041
Controls (lagged)	YES	YES	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	YES	YES	YES	YES
County × Year Fixed Effects	YES	YES	YES	YES	YES	YES

This table presents the results of the indirect effect of flooding on firms for three different outcomes: Employment, fixed assets and investment. Firms are indirectly affected, if their average bank has a large flood exposure, due to its firm-customer location with regard to the flood (see Section 2.4 for details). Effects are shown for firms outside the disaster area in Column (1)-(3) and for firms inside the disaster area (Columns (4)-(6)). IndirAffected is a dummy variable constructed by measuring the exposure of the firm to the flood via its banks, according to Equation 2.2 and 2.3. Employment is the log of the number of firms' employees. Turnover is the log of firms' fixed assets. Invest is the change in ln(total fixed assets) from t-1 to t. Control variables are cash, total assets, current liabilities and the capital ratio. All controls are included as first lags. Cash is the log of all cash and cash equivalent of firms and is a proxy for the firms' liquidity. Total assets is the log of the banks total assets and is proxy for firm size. Current liabilities is the log of the firms' current liabilities and is a proxy for the firms' short-term indebtedness. Capital ratio is common equity divided by total assets, and controls for the firms' relative equity position. I control for firm and county × year fixed effects. Clustered standard errors on the firm level of the point estimates are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE 2.4: Magnifying the shock: Main bank's capital buffer

	Low capitalization dummy			Continuous Interaction		
	(1) Employment	(2) Invest	(3) Fixed Assets	(4) Employment	(5) Invest	(6) Fixed Assets
Post × IndirAffected	0.066*** (0.024)	-0.089 (0.069)	0.020 (0.057)	-0.167** (0.084)	-0.521** (0.239)	-0.615** (0.310)
Post × IndirAffected × lowcap	-0.111*** (0.037)	-0.134 (0.095)	-0.216** (0.095)			
Post × IndirAffected × cap_pre				0.987** (0.415)	2.065 (1.273)	2.968* (1.668)
N	430,096	430,096	430,096	430,096	430,096	430,096
Number of Firms	105,594	105,594	105,594	105,594	105,594	105,594
Treatment Group	461	461	461	461	461	461
Triple Interaction Group	251	251	251			
Within R ²	0.015	0.033	0.035	0.015	0.033	0.035
Controls (lagged)	YES	YES	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	YES	YES	YES	YES
County × Year Fixed Effects	YES	YES	YES	YES	YES	YES

This table presents interactions of the standard difference-in-difference estimation from Table 2.3 with the capitalization of the firms' main bank. Only non-directly affected firms are included. Columns (1)-(3) specify the interactions with a low capitalization dummy (lowcap) which is set equal to 0 for all firms' banks above the median of the pre-flood capitalization distribution and set equal to 1 for the firms with banks below the median. Columns (4)-(6) represent the results of a continuous interaction with the pre-flood capitalization of the firms' main bank (cap_pre). The pre-flood capitalization is based on an average of the banks' regulatory capital ratio in the years 2012 and 2013. IndirAffected is a dummy variable constructed by measuring the exposure of the firm to the flood via its banks, according to Equation 2.2 and 2.3. Employment is the log of the number of firms' employees. Fixed Assets is the log of firms' fixed assets. Invest is the change in ln(total fixed assets) from t-1 to t. Unreported control variables are cash, total assets, current liabilities and the capital ratio. All controls are included as first lags. Cash is the log of all cash and cash equivalent of firms and is a proxy for the firms' liquidity. Total assets is the log of the banks total assets and is proxy for firm size. Current liabilities is the log of the firms' current liabilities and is a proxy for the firms' short-term indebtedness. Capital ratio is common equity divided by total assets, and controls for the firms' relative equity position. I control for firm and county × year fixed effects. Clustered standard errors on the firm level of the point estimates are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE 2.5: Robustness tests for low bank capital dummy: Employment

	Collapsed sample		Equal periods		Placebo		Distance		Sector×Year		Only Firm FE		No FE	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	Employment	Employment	Employment	Employment	Employment	Employment	Employment	Employment	Employment	Employment	Employment	Employment	Employment	Employment
Post × IndirAffected	0.049** (0.022)	0.062*** (0.023)	0.031 (0.025)	0.072*** (0.024)	0.067*** (0.024)	0.056** (0.022)	0.051** (0.022)							
Post × IndirAffected × lowcap	-0.090*** (0.035)	-0.111*** (0.038)	-0.006 (0.038)	-0.112*** (0.037)	-0.112*** (0.037)	-0.098*** (0.037)	-0.099*** (0.037)							
Post × dist_pre				-0.002** (0.001)										
N	211,188	380,060	245,809	420,630	430,096	430,096	430,096							
Number of Firms	105,594	105,594	105,594	103,102	105,594	105,594	105,594							
Treatment Group	461	461	461	451	461	461	461							
Triple Interaction Group	251	251	251	241	251	251	251							
Within R ²		0.012	0.007	0.000	0.014									
Controls (lagged)	YES	YES	YES	YES	YES	YES	YES							YES
Firm Fixed Effects	YES	YES	YES	YES	YES	YES	YES							NO
County×Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES							NO

This table presents robustness tests for the results presented in column (1) of Table 2.4. Column (1) presents the results of a regression on a collapsed data sample (Bertrand et al., 2004). Column (2) presents results for using equal base and post periods, here the year 2011 and 2012 are the base and 2013 and 2014 are the post period years. Column (3) presents the results of a placebo test using the year 2011 as the placebo event year (omitting the years 2013 and 2014). Column (4) includes a post-flood firm-bank distance control. Column (5) includes sector × year fixed effects. Column (6) and (7) provide estimates without county × year and firm fixed effects. lowcap is a dummy variable equal to 0 for all firms' banks above the median of the pre-flood capitalization distribution and equal to 1 for the firms with banks below the median. IndirAffected is a dummy variable constructed by measuring the exposure of the firm to the flood via its banks, according to Equation 2.2 and 2.3. Employment is the log of the number of firms' employees. Fixed Assets is the log of firms' fixed assets. Invest is the change in ln(total fixed assets) from t-1 to t. Unreported control variables are cash, total assets, current liabilities and the capital ratio. All controls are included as first lags. Cash is the log of all cash and cash equivalent of firms and is a proxy for the firms' liquidity. Total assets is the log of the banks total assets and is proxy for firm size. Current liabilities is the log of the firms' current liabilities and is a proxy for the firms' short-term indebtedness. Capital ratio is common equity divided by total assets, and controls for the firms' relative equity position. I control for firm and county × year fixed effects. Clustered standard errors on the firm level of the point estimates are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. Robustness tests for other specifications of Table 2.4 can be found in the Appendix.

TABLE 2.6: Channel of real effects: Indirect effect on firm borrowing

	Low capitalization dummy			Continuous Interaction		
	(1) Loans	(2) Long term debt	(3) Capital	(4) Loans	(5) Long term debt	(6) Capital
Post×IndirAffected	0.351 (0.322)	-0.244 (0.291)	-0.079 (0.137)	-1.152 (0.912)	-0.704 (0.853)	0.266 (0.625)
Post×IndirAffected×lowcap	-0.736* (0.425)	-0.005 (0.382)	0.118 (0.245)			
Post×IndirAffected×cap_pre				6.228 (4.814)	2.691 (4.755)	-1.575 (3.323)
N	254,467	429,264	430,096	254,467	429,264	430,096
Number of Firms	87,703	105,557	105,594	87,703	105,557	105,594
Treatment Group	373	461	461	373	461	461
Triple Interaction Group	201	251	251			
Within R ²	0.001	0.002	0.021	0.001	0.002	0.021
Controls (lagged)	YES	YES	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	YES	YES	YES	YES
County×Year Fixed Effects	YES	YES	YES	YES	YES	YES

This table presents the results of the indirect effects of flooding on firms liabilities to identify the bank lending channel. The specification is the same as in Table 2.3, with different dependent variables: loans, long-term debt and firms capital. Columns (1)-(3) specify the interactions with a low capitalization dummy (lowcap) which is set equal to 0 for all firms' banks above the median of the pre-flood capitalization distribution and set equal to 1 for the firms with banks below the median. Columns (4)-(6) represent the results of a continuous interaction with the pre-flood capitalization of the firms' main bank (cap_pre). The pre-flood capitalization is based on an average of the banks' regulatory capital ratio in the years 2012 and 2013. IndirAffected is a dummy variable constructed by measuring the exposure of the firm to the flood via its banks, according to Equation 2.2 and 2.3. Loans are a subcategory of current liabilities and include short-term borrowing. Long term debt is a non-current liability. Capital is equivalent to common equity. Unreported control variables are cash, total assets, current liabilities and the capital ratio. All controls are included as first lags. Cash is the log of all cash and cash equivalent of firms and is a proxy for the firms' liquidity. Total assets is the log of the banks total assets and is proxy for firm size. Current liabilities is the log of the firms' current liabilities and is a proxy for the firms' short-term indebtedness. Capital ratio is common equity divided by total assets, and controls for the firms' relative equity position. I control for firm and county×year fixed effects. Clustered standard errors on the firm level of the point estimates are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE 2.7: Relationship banking

	Firm-bank distance			Number of banks		
	(1) Employment	(2) Invest	(3) Fixed Assets	(4) Employment	(5) Invest	(6) Fixed Assets
Post×IndirAffected	0.040* (0.021)	-0.064 (0.061)	-0.026 (0.056)	-0.003 (0.039)	-0.120 (0.101)	-0.016 (0.104)
Post×IndirAffected×dist_pre	-0.023** (0.011)	-0.055 (0.038)	-0.036 (0.029)			
Post×IndirAffected×bank_count_pre				0.005 (0.020)	-0.032 (0.061)	-0.057 (0.064)
N	477,345	477,345	477,345	486,322	486,322	486,322
Number of Firms	116,462	116,462	116,462	118,743	118,743	118,743
Treatment Group	464	464	464	473	473	473
Within R ²	0.014	0.033	0.034	0.015	0.033	0.034
Controls (lagged)	YES	YES	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	YES	YES	YES	YES
County×Year Fixed Effects	YES	YES	YES	YES	YES	YES

This table presents interactions of the standard difference-in-difference estimation from Table 2.3 with relationship banking indicators. Columns (1)-(3) provide the results of a continuous interaction with the distance between the firm and its main bank (dist_pre). Distance is measured in 100 km intervals. Columns (4)-(6) provide the results of a continuous interaction with the number of banks each firm reports a relationship with (bank_count_pre). IndirAffected is a dummy variable constructed by measuring the exposure of the firm to the flood via its banks, according to Equation 2.2 and 2.3. Employment is the log of the number of firms' employees. Fixed Assets is the log of firms' fixed assets. Invest is the change in ln(total fixed assets) from t-1 to t. Unreported control variables are cash, total assets, current liabilities and the capital ratio. All controls are included as first lags. Cash is the log of all cash and cash equivalent of firms and is a proxy for the firms' liquidity. Total assets is the log of the banks total assets and is proxy for firm size. Current liabilities is the log of the firms' current liabilities and is a proxy for the firms' short-term indebtedness. Capital ratio is common equity divided by total assets, and controls for the firms' relative equity position. I control for firm and county×year fixed effects. Clustered standard errors on the firm level of the point estimates are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE 2.8: Bank type differentiation

	Savings banks			Cooperative banks			Commercial banks		
	(1) Employment	(2) Invest	(3) Fixed Assets	(4) Employment	(5) Invest	(6) Fixed Assets	(7) Employment	(8) Invest	(9) Fixed Assets
Post × Indir-Affected	0.029* (0.017)	-0.040 (0.063)	-0.070 (0.069)	-0.005 (0.029)	-0.258*** (0.068)	-0.151** (0.070)	0.001 (0.020)	-0.164*** (0.049)	-0.070 (0.046)
Post × Indir-Affected × savings	-0.045 (0.036)	-0.240*** (0.093)	-0.048 (0.096)						
Post × Indir-Affected × coop				0.023 (0.034)	0.241*** (0.090)	0.140 (0.090)			
Post × Indir-Affected × comm							0.039 (0.055)	-0.071 (0.281)	-0.451 (0.361)
N	486,322	486,322	486,322	486,322	486,322	486,322	486,322	486,322	486,322
Number of Firms	118,743	118,743	118,743	118,743	118,743	118,743	118,743	118,743	118,743
Treatment Group	499	499	499	499	499	499	499	499	499
Triple Interaction Group	246,000	246,000	246,000	189,000	189,000	189,000	29,000	29,000	29,000
Within R ²	0.014	0.033	0.034	0.014	0.033	0.034	0.015	0.033	0.034
Controls (lagged)	YES	YES	YES	YES	YES	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
County × Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES

This table presents interactions of the standard difference-in-difference estimation from Table 2.3 with three major German bank types: savings banks, cooperative banks and commercial banks. Savings is a dummy set equal to 1 if the firms' main bank is a savings bank and 0 if it is any other type of bank. Coop is a dummy set equal to 1 if the firms' main bank is a cooperative bank and 0 if it is any other type of bank. Comm is a dummy set equal to 1 if the firms' main bank is a commercial bank and 0 if it is any other type of bank. Indir-Affected is a dummy variable constructed by measuring the exposure of the firm to the flood via its banks, according to Equation 2.2 and 2.3. Employment is the log of the number of firms' employees. Fixed Assets is the log of firms' fixed assets. Invest is the change in ln(total fixed assets) from t-1 to t. Unreported control variables are cash, total assets, current liabilities and the capital ratio. All controls are included as first lags. Cash is the log of all cash and cash equivalent of firms and is a proxy for the firms' liquidity. Total assets is the log of the banks total assets and is proxy for firm size. Current liabilities is the log of the firms' current liabilities and is a proxy for the firms' short-term indebtedness. Capital ratio is common equity divided by total assets, and controls for the firms' relative equity position. I control for firm and county × year fixed effects. Clustered standard errors on the firm level of the point estimates are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE 2.9: Firms financial constraints: Firms' capitalization and liquidity

	Capital ratio			Liquidity		
	(1) Employment	(2) Invest	(3) Fixed Assets	(4) Employment	(5) Invest	(6) Fixed Assets
Post × IndirAffected	0.019 (0.027)	-0.303*** (0.084)	-0.163* (0.093)	-0.003 (0.025)	-0.141** (0.065)	-0.114* (0.060)
Post × IndirAffected × adequacy_pre	-0.041 (0.047)	0.430*** (0.164)	0.220 (0.184)			
Post × IndirAffected × liq_pre				0.066 (0.069)	-0.131 (0.337)	0.110 (0.253)
N	486,322	486,322	486,322	484,422	484,422	484,422
Number of Firms	118,743	118,743	118,743	118,470	118,470	118,470
Treatment Group	473	473	473	467	467	467
Within R ²	0.014	0.033	0.035	0.014	0.033	0.034
Controls (lagged)	YES	YES	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	YES	YES	YES	YES
County × Year Fixed Effects	YES	YES	YES	YES	YES	YES

This table presents interactions of the standard difference-in-difference estimation from Table 2.3 with firm financial constraint indicators. Columns (1)-(3) provide results for a continuous interaction with firm's pre-flood capital ratio (*adequacy_pre*). Columns (4)-(6) provide the results of a continuous interaction with the pre-flood liquidity of the firm in terms of its cash reserves (*cash_pre*). *IndirAffected* is a dummy variable constructed by measuring the exposure of the firm to the flood via its banks, according to Equation 2.2 and 2.3. *Employment* is the log of the number of firms' employees. *Fixed Assets* is the log of firms' fixed assets. *Invest* is the change in $\ln(\text{total fixed assets})$ from $t-1$ to t . Unreported control variables are cash, total assets, current liabilities and the capital ratio. All controls are included as first lags. *Cash* is the log of all cash and cash equivalent of firms and is a proxy for the firms' liquidity. *Total assets* is the log of the banks total assets and is proxy for firm size. *Current liabilities* is the log of the firms' current liabilities and is a proxy for the firms' short-term indebtedness. Capital ratio is common equity divided by total assets, and controls for the firms' relative equity position. I control for firm and county × year fixed effects. Clustered standard errors on the firm level of the point estimates are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Appendix A

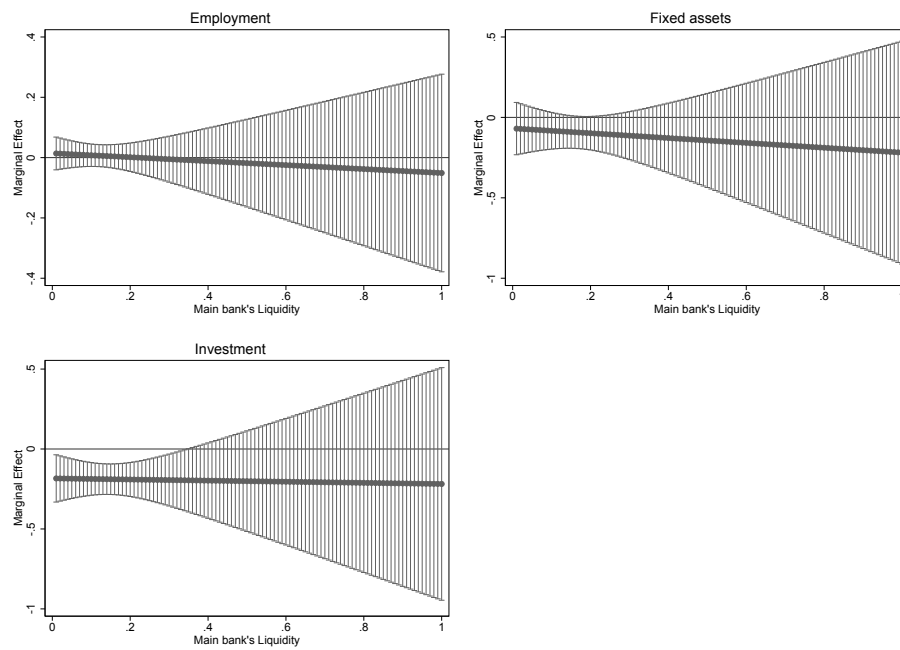


FIGURE A.I: Marginal effect of the interaction with the difference-in-difference coefficient at different values of banks' liquidity

This figure shows the marginal effects of the difference-in-difference estimation of being affected by a bank funding shock resulting from flooding in other regions at different values of the banks' liquidity (according to the regression in columns (4)-(6) of Table A.III). Bank liquidity is the share of cash on total assets, averaged over the years 2012 and 2013. Bank Liquidity is depicted on the x-axis. Each graph represents the marginal effects for a different dependent variable, as indicated by its title. Bars indicate 90% confidence intervals.

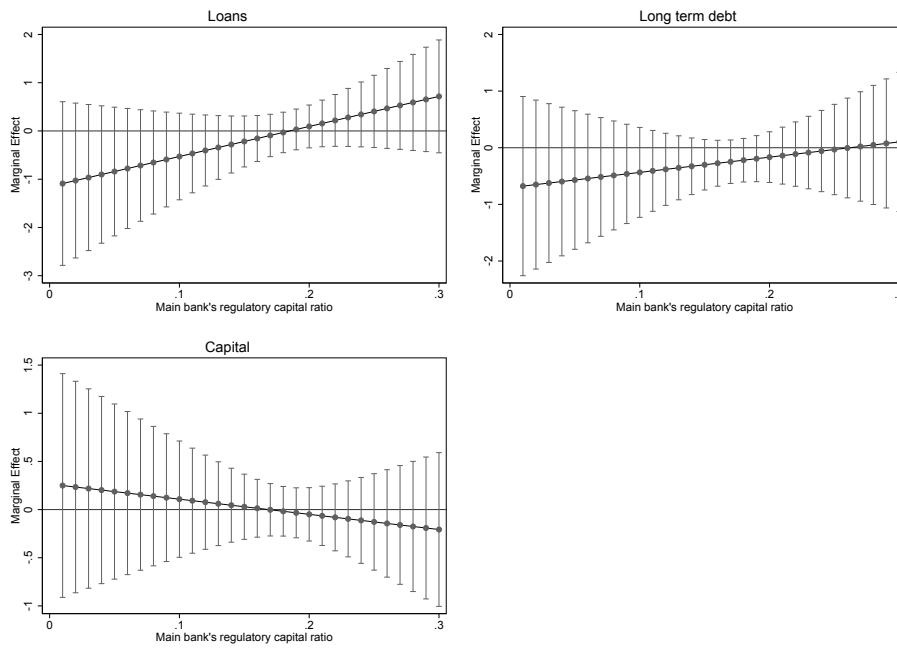


FIGURE A.II: Marginal effect of the interaction with the difference-in-difference coefficient at different values of main bank's capitalization: Liabilities (channel)

This figure shows the marginal effects of the difference-in-difference estimation of being affected by a bank funding shock resulting from flooding in other regions at different values of the firms' main bank's regulatory capital ratio (according to the regression in Table 2.4). Capital ratios are indicated as shares on the x-axis. Each graph represents the marginal effects for a different dependent variable, as indicated by its title. Bars indicate 90% confidence intervals.

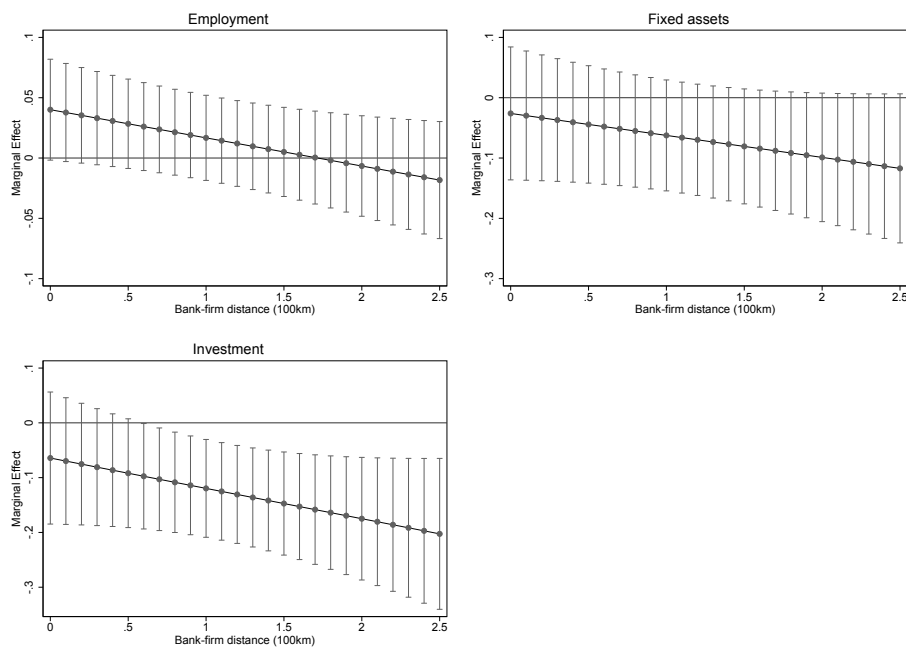


FIGURE A.III: Marginal effect of the interaction with the difference-in-difference coefficient at different values of firm bank distance

This figure shows the marginal effects of the difference-in-difference estimation of being affected by a bank funding shock resulting from flooding in other regions at different values of the firm-bank distance (according to the regressions in columns (1)-(3) of Table 2.7). Distance is indicated in 100 kilometer intervals on the x-axis. Each graph represents the marginal effects for a different dependent variable, as indicated by its title. Bars indicate 90% confidence intervals.

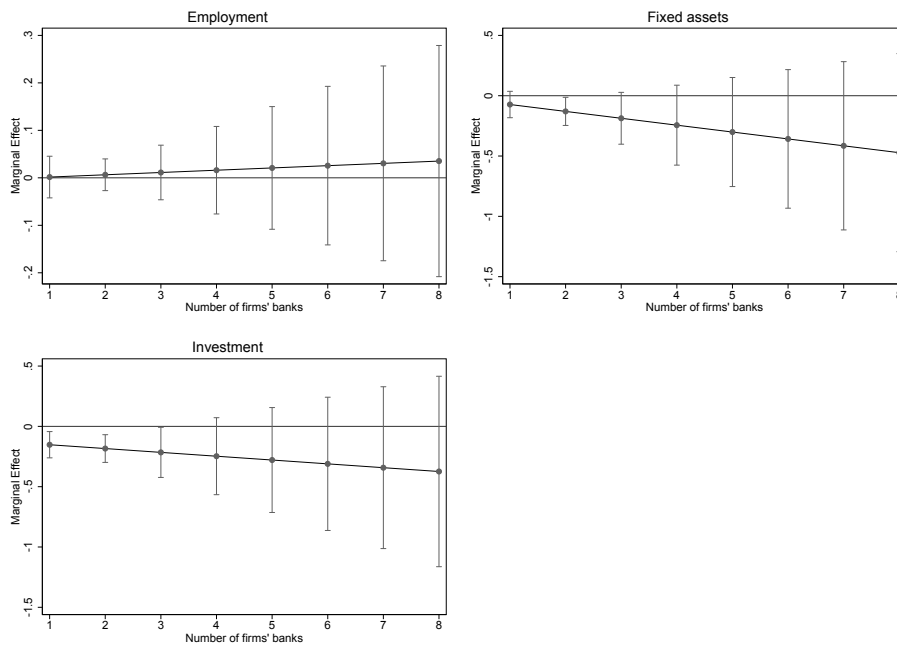


FIGURE A.IV: Marginal effect of the interaction with the difference-in-difference coefficient at different values of firms' bank number

This figure shows the marginal effects of the difference-in-difference estimation of being affected by a bank funding shock resulting from flooding in other regions at different values of the firms' bank number (according to the regression in columns (4)-(6) of Table 2.7). Bank number varies from 1-8 and is depicted on the x-axis. Each graph represents the marginal effects for a different dependent variable, as indicated by its title. Bars indicate 90% confidence intervals.

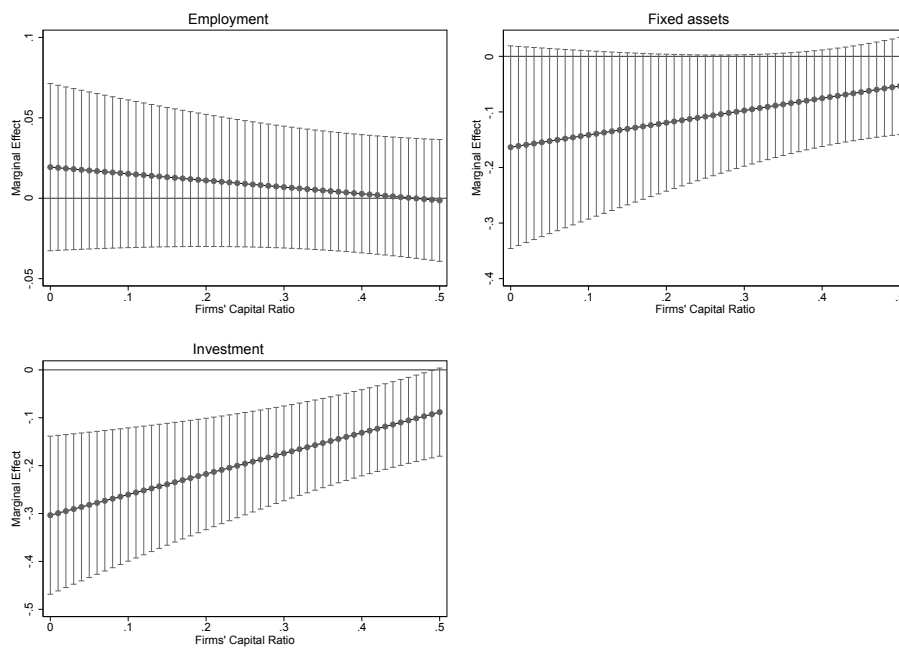


FIGURE A.V: Marginal effect of the interaction with the difference-in-difference coefficient at different values of firms' capitalization

This figure shows the marginal effects of the difference-in-difference estimation of being affected by a bank funding shock resulting from flooding in other regions at different values of the firms' capital (according to the regression in columns (1)-(3) of Table 2.9). Firm capital values are depicted as ratios on the x-axis. Each graph represents the marginal effects for a different dependent variable, as indicated by its title. Bars indicate 90% confidence intervals.

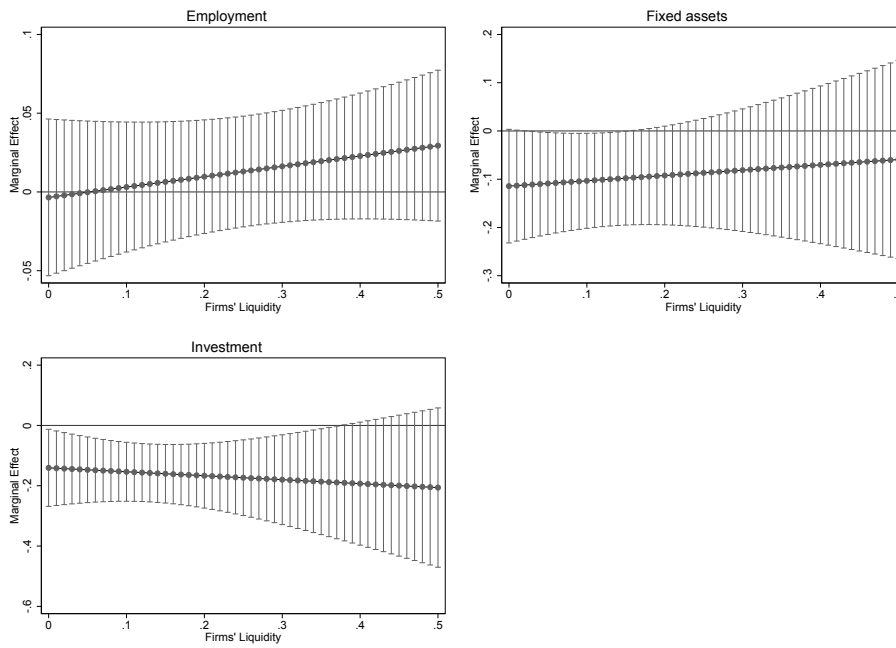


FIGURE A.VI: Marginal effect of the interaction with the difference-in-difference coefficient at different values of firms' liquidity

This figure shows the marginal effects of the difference-in-difference estimation of being affected by a bank funding shock resulting from flooding in other regions at different values of the firms' liquidity (according to the regression in columns (4)-(6) of Table 2.9). Firm liquidity is the share of cash and cash equivalent on total assets and is depicted on the x-axis. Each graph represents the marginal effects for a different dependent variable, as indicated by its title. Bars indicate 90% confidence intervals.

TABLE A.I: Variable definitions

Identification Variables:	
DirAffected	Dummy variable indicating whether the firm was located in a flooded region during the 2013 flooding. A value of 1 indicates that the firm is located in a county with a claim ratio category of 4 or larger. A value of 0 indicates its located in within an unaffected county (claim ratio category 1). For a description of the categories refer to Figure 2.1.
IndirAffected	Dummy variable indicating whether the firm is exposed to a funding shock from its banks, stemming from the flood. A value of 1 indicates that the firms average bank has an exposure to the disaster via its firms of 4 or larger. A value of 0 indicates the exposure is smaller than 2.5. See Equation 2.2 and 2.3 for details.
Post	Dummy variable set equal to 1 for the years 2013 and 2014 and set equal to 0 from 2009 to 2012.
Dependent Variables:	
Employment	Number of firms' employees. Used as natural logarithm in the regressions.
Investment / Invest	First difference of the natural logarithm of firm's total fixed assets from t-1 to t. Investment and Invest indicate the same variable, and is used interchangeably for cosmetic reasons.
Fixed Assets	Firms' fixed assets in millions of Euros. Used as natural logarithm in the regressions.
Control Variables:	
Cash	Cash and cash equivalent in millions of Euros.
Total Assets	Total assets in millions of Euros.
Capital Ratio	Shareholder funds (common equity) divided by total assets.
Current Liabilities	Current liabilities in millions of Euros.
Channel	
Loans	Current liabilities: loans in millions of Euros. Used as natural logarithm in the regressions.
Long term debt	Non current liabilities: long term debt in millions of Euros. Used as natural logarithm in the regressions.
Capital	Common equity in millions of Euros. Used as natural logarithm in the regressions.
Interaction Variables:	
Main bank's reg. capital ratio (cap_pre)	Regulatory capital ratio of the firms' main bank. Set to pre-flood levels as an average of 2012 and 2013.
Main bank's reg. capital ratio dummy (low-cap)	Dummy set equal to 1 if the main bank's regulatory capital ratio (cap_pre) is above the median and set to 0 if it is below the median.
Distance to main bank in km (dist_pre)	Distance between the middle of the firms postal code and the banks postal code in 100 kilometer intervals. Examined at 2012 (pre-flood) levels.
Number of banks per firm (bank_count_pre)	Number of banks the firm reports a relationship with. Examined at 2012 (pre-flood) levels.
Savings Bank dummy (savings)	Dummy variable set equal to 1 if the firm's main bank is a (government owned) savings bank.
Cooperative Bank dummy (coop)	Dummy variable set equal to 1 if the firm's main bank is a cooperative bank.
Commercial Bank dummy (comm)	Dummy variable set equal to 1 if the firm's main bank is a commercial bank.
Pre-flood firm capital ratio (adequacy_pre)	Firms capital ratio (capital/total assets) prior to the flood (in 2012).
Pre-flood firm liquidity (liq_pre)	Firms liquidity (cash/total assets) prior to the flood (in 2012).

This table presents definitions of all the variables used in the regression tables and figures used in the main text and the online appendix.

TABLE A.II: Baseline regression for additional variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post×IndirAffected	TFAS -0.025 (0.063)	Cash 0.116 (0.095)	TOAS 0.008 (0.022)	Prov 0.087 (0.061)	NCAS 0.015 (0.059)	Depr 0.082 (0.090)	RoE 6.786* (3.755)	RoA 0.189 (1.636)	Capital Ratio -0.008 (0.008)
Post×IndirAffected×lowcap	-0.225** (0.099)	0.022 (0.131)	0.023 (0.032)	0.135 (0.091)	0.132 (0.085)	-0.068 (0.225)	-4.288 (16.386)	-0.724 (3.836)	-0.000 (0.012)
N	430,096	428,569	430,096	429,380	392,835	96,763	92,955	96,696	430,096
Number of Firms	105,594	105,557	105,594	105,554	102,673	30,685	29,991	30,668	105,594
Treatment Group	461	461	461	461	446	115	112	115	461
Triple Interaction Group	251	251	251	251	240	60	58	60	251
Within R ²	0.026	0.003	0.082	0.007	0.015	0.049	0.010	0.030	0.091
Controls (lagged)	YES	YES	YES	YES	YES	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
County×Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES

This table presents interactions of the difference-in-difference estimation from Table 2.4 with the capitalization of the firms' main bank for several firm-level variables not used in the main body. TFAS is the log of total fixed assets. Cash is the log of cash and cash equivalent. TOAS is log of total assets. Prov is log of provisions. NCAS is log of firms net current assets. Depr is log of depreciation. RoE is return on equity. RoA is return on assets. Capital ratio is firm capital over total assets. Only non-directly affected firms are included in the regressions. All Columns include the interaction with a low capitalization dummy (lowcap) which is set equal to 0 for all firms' banks above the median of the pre-flood capitalization distribution and set equal to 1 for the firms with banks below the median. The pre-flood capitalization is based on an average of the banks' regulatory capital ratio in the years 2012 and 2013. IndirAffected is a dummy variable constructed by measuring the exposure of the firm to the flood via its banks, according to Equation 2.2 and 2.3. Employment is the log of the number of firms' employees. Fixed Assets is the log of firms' fixed assets. Invest is the change in ln(total fixed assets) from t-1 to t. Unreported control variables are cash, total assets, current liabilities and the capital ratio. All controls are included as first lags. Cash is the log of all cash and cash equivalent of firms and is a proxy for the firms' liquidity. Total assets is the log of the banks total assets and is proxy for firm size. Current liabilities is the log of the firms' current liabilities and is a proxy for the firms' short-term indebtedness. Capital ratio is common equity divided by total assets, and controls for the firms' relative equity position. I control for firm and county×year fixed effects. Clustered standard errors on the firm level of the point estimates are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE A.III: Magnifying the shock: Main bank's liquidity

	Low capitalization dummy			Continuous Interaction		
	(1) Employment	(2) Invest	(3) Fixed Assets	(4) Employment	(5) Invest	(6) Fixed Assets
Post×IndirAffected	-0.015 (0.041)	-0.215** (0.093)	-0.021 (0.066)	0.014 (0.029)	-0.184** (0.079)	-0.068 (0.086)
Post×IndirAffected×lowliq	0.026 (0.044)	0.073 (0.106)	-0.125 (0.092)			
Post×IndirAffected×liq_pre				-0.065 (0.189)	-0.035 (0.430)	-0.151 (0.419)
N	488,191	488,191	488,191	490,621	490,621	490,621
Number of Firms	119,671	119,671	119,671	120,256	120,256	120,256
Treatment Group	485	485	485	485	485	485
Triple Interaction Group	326	326	326			
Within R ²	0.014	0.033	0.034	0.000	0.000	0.000
Controls (lagged)	YES	YES	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	YES	YES	YES	YES
County×Year Fixed Effects	YES	YES	YES	YES	YES	YES

This table presents interactions of the standard difference-in-difference estimation from Table 2.3 with the liquidity of the firms' main bank. Only non-directly affected firms are included. Columns (1)-(3) specify the interactions with a low liquidity dummy (lowliq) which is set equal to 0 for all firms' banks above the median of the pre-flood liquidity distribution and set equal to 1 for the firms with banks below the median. Columns (4)-(6) represent the results of a continuous interaction with the pre-flood liquidity of the firms' main bank (liq_pre). The pre-flood liquidity is based on the average of the banks liquidity in the years 2012 and 2013. Liquidity is defined as the share of cash on total assets. IndirAffected is a dummy variable constructed by measuring the exposure of the firm to the flood via its banks, according to Equation 2.2 and 2.3. Employment is the log of the number of firms' employees. Fixed Assets is the log of firms' fixed assets. Invest is the change in ln(total fixed assets) from t-1 to t. Unreported control variables are cash, total assets, current liabilities and the capital ratio. All controls are included as first lags. Cash is the log of all cash and cash equivalent of firms and is a proxy for the firms' liquidity. Total assets is the log of the banks total assets and is proxy for firm size. Current liabilities is the log of the firms' current liabilities and is a proxy for the firms' short-term indebtedness. Capital ratio is common equity divided by total assets, and controls for the firms' relative equity position. I control for firm and county×year fixed effects. Clustered standard errors on the firm level of the point estimates are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE A. IV: Robustness tests for low bank capital dummy: Investment

	Collapsed sample	Equal periods	Placebo	Distance	Sector×Time	Only Firm FE	No FE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Investment	Investment	Investment	Investment	Investment	Investment	Investment
Post×IndirAffected	-0.095 (0.077)	-0.123 (0.076)	0.130 (0.110)	-0.109 (0.068)	-0.086 (0.070)	-0.052 (0.064)	-0.054 (0.058)
Post×IndirAffected×lowcap	-0.142 (0.110)	-0.088 (0.102)	-0.254* (0.136)	-0.127 (0.095)	-0.132 (0.095)	-0.151 (0.093)	-0.171* (0.087)
Post×dist_pre				-0.004 (0.003)			
N	211,188	380,060	245,809	420,630	430,096	430,096	430,096
Number of Firms	105,594	105,594	105,594	103,102	105,594	105,594	105,594
Treatment Group	461	461	461	451	461	461	461
Triple Interaction Group	251	251	251	241	251	251	251
Within R ²		0.031	0.059	0.000	0.033		
Controls (lagged)	YES	YES	YES	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	YES	YES	YES	YES	NO
County×Year Fixed Effects	YES	YES	YES	YES	YES	NO	NO

This table presents robustness tests for the results presented in column (2) of Table 2.4. The dependent variable for all Columns is investment. Column (1) presents the results of a regression on a collapsed data sample (Bertrand et al., 2004). Column (2) presents results for using equal base and post periods, here the year 2011 and 2012 are the base and 2013 and 2014 are the post period years. Column (3) omits the year 2013 from the regression, thus only 2014 is included in the post period. Column (4) presents the results of a placebo test using the year 2011 as the placebo event year (omitting the years 2013 and 2014). Investment is defined as the change in total fixed assets, from t-1 to t. lowcap is a dummy variable equal to 0 for all firms' banks above the median of the pre-flood capitalization distribution and equal to 1 for the firms with banks below the median. IndirAffected is a dummy variable constructed by measuring the exposure of the firm to the flood via its banks, according to Equation 2.2 and 2.3. Employment is the log of the number of firms' employees. Fixed Assets is the log of firms' fixed assets. Invest is the change in ln(total fixed assets) from t-1 to t. Unreported control variables are cash, total assets, current liabilities and the capital ratio. All controls are included as first lags. Cash is the log of all cash and cash equivalent of firms and is a proxy for the firms' liquidity. Total assets is the log of the banks total assets and is proxy for firm size. Current liabilities is the log of the firms' current liabilities and is a proxy for the firms' short-term indebtedness. Capital ratio is common equity divided by total assets, and controls for the firms' relative equity position. I control for firm and county×year fixed effects. Clustered standard errors on the firm level of the point estimates are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. Robustness tests for other specifications of Table 2.4 can be found in the Appendix.

TABLE A.V: Robustness tests for low bank capital dummy: Fixed assets

	Collapsed sample		Equal periods		Placebo		Distance		Sector×Time		Only Firm FE		No FE	
	(1) Fixed Assets	(2) Fixed Assets	(3) Fixed Assets	(4) Fixed Assets	(5) Fixed Assets	(6) Fixed Assets	(7) Fixed Assets	(8) Fixed Assets	(9) Fixed Assets	(10) Fixed Assets	(11) Fixed Assets	(12) Fixed Assets	(13) Fixed Assets	(14) Fixed Assets
Post × IndirAffected	0.034 (0.060)	-0.002 (0.055)	0.139* (0.081)	0.044 (0.060)	0.024 (0.057)	0.056 (0.056)	0.036 (0.054)							
Post × IndirAffected × lowcap	-0.202** (0.098)	-0.176* (0.092)	-0.204* (0.113)	-0.182* (0.097)	-0.213** (0.095)	-0.218** (0.095)	-0.219** (0.095)							
Post × dist_pre				-0.008*** (0.003)										
N	211,188	380,060	245,809	420,630	430,096	430,096	430,096							
Number of Firms	105,594	105,594	105,594	103,102	105,594	105,594	105,594							
Treatment Group	461	461	461	451	461	461	461							
Triple Interaction Group	251	251	251	241	251	251	251							
Within R ²		0.026	0.010	0.000	0.035	0.035	0.035							
Controls (lagged)	YES	YES	YES	YES	YES	YES	YES							YES
Firm Fixed Effects	YES	YES	YES	YES	YES	YES	YES							NO
County×Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES							NO

This table presents robustness tests for the results presented in column (3) of Table 2.4. The dependent variable for all Columns is fixed assets. Column (1) presents the results of a regression on a collapsed data sample (Bertrand et al., 2004). Column (2) presents results for using equal base and post periods, here the year 2011 and 2012 are the base and 2013 and 2014 are the post period years. Column (3) omits the year 2014 in the regression, thus only 2014 is included in the post period. Column (4) presents the results of a placebo test using the year 2011 as the placebo event year (omitting the years 2013 and 2014). lowcap is a dummy variable equal to 0 for all firms' banks above the median of the pre-flood capitalization distribution and equal to 1 for the firms with banks below the median. IndirAffected is a dummy variable constructed by measuring the exposure of the firm to the flood via its banks, according to Equation 2.2 and 2.3. Employment is the log of the number of firms' employees. Fixed Assets is the log of firms' fixed assets. Invest is the change in ln(total fixed assets) from t-1 to t. Unreported control variables are cash, total assets, current liabilities and the capital ratio. All controls are included as first lags. Cash is the log of all cash and cash equivalent of firms and is a proxy for the firms' liquidity. Total assets is the log of the banks total assets and is proxy for firm size. Current liabilities is the log of the firms' current liabilities and is a proxy for the firms' short-term indebtedness. Capital ratio is common equity divided by total assets, and controls for the firms' relative equity position. I control for firm and county×year fixed effects. Clustered standard errors on the firm level of the point estimates are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. Robustness tests for other specifications of Table 2.4 can be found in the Appendix.

TABLE A.VI: Robustness: Sample of firms using 1:1 propensity score matching

	Matching based on full sample						Matching only for indirectly affected only					
	Low cap. dummy		Continuous Interaction		Continuous Interaction		Low cap. dummy		Continuous Interaction		Continuous Interaction	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post×IndirAffected	Empl 0.065*** (0.026)	Invest -0.122 (0.080)	FAssets 0.012 (0.062)	Empl -0.163* (0.089)	Invest -0.586** (0.247)	FAssets -0.627** (0.317)	Empl 0.031 (0.043)	Invest -0.265** (0.117)	FAssets -0.105 (0.106)	Empl -0.266** (0.124)	Invest -1.011*** (0.325)	FAssets -0.634 (0.400)
Post×Treated×IndirAffected	-0.111*** (0.038)	-0.149 (0.100)	-0.205** (0.096)				-0.093* (0.055)	-0.183 (0.129)	-0.119 (0.140)			
Post×IndirAffected×cap_pre				0.951** (0.430)	2.201* (1.299)	3.008* (1.711)				1.365** (0.647)	3.721** (1.634)	2.606 (2.110)
N	85,162	85,162	85,162	85,162	85,162	85,162	3,638	3,638	3,638	3,638	3,638	3,638
Number of Firms	20,890	20,890	20,890	20,890	20,890	20,890	902	902	902	902	902	902
Treatment Group	451	451	451	451	451	451	451	451	451	451	451	451
Triple Interaction Group	246	246	246				246	246	246			
Within R ²	0.014	0.046	0.029	0.013	0.046	0.029	0.017	0.077	0.023	0.011	0.078	0.026
Controls (lagged)	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
County×Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

This table presents interactions of the difference-in-difference estimations from Table 2.4 using matched samples. For columns (1)-(6) the control group is based on a 1:1 propensity score matching using all firms in the sample. For columns (7)-(12) the control group is based on a 1:1 propensity score matching using only non-directly affected firms. Propensity score matching is done using the control variables values in the year prior to the flood (2012) as matching parameters. A caliper width of 0.01 is applied to the propensity score matching process. Only non-directly affected firms are included in all regressions. Columns (1)-(3) and (7)-(9) specify the interactions with a low capitalization dummy (lowcap) which is set equal to 0 for all firms' banks above the median of the pre-flood capitalization distribution and set equal to 1 for the firms with banks below the median. Columns (4)-(6) and (10)-(12) represent the results of a continuous interaction with the pre-flood capitalization of the firms' main bank (cap_pre). The pre-flood capitalization is based on an average of the banks' regulatory capital ratio in the years 2012 and 2013. IndirAffected is a dummy variable constructed by measuring the exposure of the firm to the flood via its banks, according to Equation 2.2 and 2.3. Employment is the log of the number of firms' employees. Fixed Assets is the log of firms' fixed assets. Invest is the change in ln(total fixed assets) from t-1 to t. Unreported control variables are cash, total assets, current liabilities and the capital ratio. All controls are included as first lags. Cash is the log of all cash and cash equivalent of firms and is a proxy for the firms' liquidity. Total assets is the log of the banks total assets and is proxy for firm size. Current liabilities is the log of the firms' current liabilities and is a proxy for the firms' short-term indebtedness. Capital ratio is common equity divided by total assets, and controls for the firms' relative equity position. I control for firm and county×year fixed effects. Clustered standard errors on the firm level of the point estimates are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE A.VII: Bank's capital: Relevance for small and medium-sized firms

	Low capitalization dummy			Continuous Interaction		
	(1) Employment	(2) Invest	(3) Fixed Assets	(4) Employment	(5) Invest	(6) Fixed Assets
Post×IndirAffected	0.067*** (0.022)	-0.124* (0.072)	0.018 (0.059)	-0.153* (0.089)	-0.601** (0.256)	-0.500 (0.305)
Post×IndirAffected×lowcap	-0.116*** (0.040)	-0.148 (0.104)	-0.222** (0.103)			
Post×IndirAffected×cap_pre				0.947** (0.436)	2.347* (1.365)	2.375 (1.659)
N	381,213	381,213	381,213	381,213	381,213	381,213
Number of Firms	94,437	94,437	94,437	94,437	94,437	94,437
Treatment Group	418	418	418	418	418	418
Triple Interaction Group	199	199	199			
Within R ²	0.014	0.032	0.033	0.013	0.032	0.033
Controls (lagged)	YES	YES	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	YES	YES	YES	YES
County×Year Fixed Effects	YES	YES	YES	YES	YES	YES

This table presents interactions of the difference-in-difference estimations from Table 2.4 using only small and medium sized enterprises. Small and medium sized companies are companies that are classified as such in the Amadeus database. It generally includes firms with fewer than 150 employees, less than 10 million Euro in operating revenue and less than 20 million Euro in total assets. Only non-directly affected firms are included. Columns (1)-(3) specify the interactions with a low capitalization dummy (lowcap) which is set equal to 0 for all firms' banks above the median of the pre-flood capitalization distribution and set equal to 1 for the firms with banks below the median. Columns (4)-(6) represent the results of a continuous interaction with the pre-flood capitalization of the firms' main bank (cap_pre). The pre-flood capitalization is based on an average of the banks' regulatory capital ratio in the years 2012 and 2013. IndirAffected is a dummy variable constructed by measuring the exposure of the firm to the flood via its banks, according to Equation 2.2 and 2.3. Employment is the log of the number of firms' employees. Fixed Assets is the log of firms' fixed assets. Invest is the change in ln(total fixed assets) from t-1 to t. Unreported control variables are cash, total assets, current liabilities and the capital ratio. All controls are included as first lags. Cash is the log of all cash and cash equivalent of firms and is a proxy for the firms' liquidity. Total assets is the log of the banks total assets and is proxy for firm size. Current liabilities is the log of the firms' current liabilities and is a proxy for the firms' short-term indebtedness. Capital ratio is common equity divided by total assets, and controls for the firms' relative equity position. I control for firm and county×year fixed effects. Clustered standard errors on the firm level of the point estimates are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE A. VIII: Robustness: Variation of fixed effects

	Firm & time FE			Firm FE			NO FE		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post×IndirAffected	Employment 0.057** (0.022)	Invest -0.052 (0.064)	Fixed Assets 0.057 (0.056)	Employment 0.056** (0.022)	Invest -0.052 (0.064)	Fixed Assets 0.056 (0.056)	Employment 0.038 (0.037)	Invest -0.052 (0.057)	Fixed Assets 0.083 (0.077)
Post×lowcap	0.004** (0.002)	0.008 (0.005)	0.010** (0.005)	0.003* (0.002)	0.008 (0.005)	0.010** (0.005)	0.010*** (0.003)	0.006 (0.005)	0.007 (0.006)
Post×IndirAffected×lowcap	-0.099*** (0.037)	-0.152 (0.093)	-0.219** (0.095)	-0.098*** (0.037)	-0.151 (0.093)	-0.218** (0.095)	-0.154*** (0.059)	-0.172** (0.087)	-0.249** (0.115)
Post				0.027*** (0.001)	-0.060*** (0.004)	-0.028*** (0.004)	-0.009*** (0.002)	-0.091*** (0.004)	-0.104*** (0.004)
IndirAffected							-0.074 (0.079)	0.042 (0.039)	-0.073 (0.150)
lowcap							-0.017*** (0.007)	0.000 (0.003)	0.099*** (0.009)
IndirAffected×lowcap							0.021 (0.111)	0.040 (0.060)	0.165 (0.180)
Constant							-3.115*** (0.037)	0.200*** (0.016)	-4.269*** (0.042)
N	430,096	430,096	430,096	430,096	430,096	430,096	430,096	430,096	430,096
Number of Firms	105,594	105,594	105,594	105,594	105,594	105,594	105,594	105,594	105,594
Treatment Group	461	461	461	461	461	461	461	461	461
Triple Interaction Group	251	251	251	251	251	251	251	251	251
Adjusted R ²	0.972	0.029	0.941	0.972	0.029	0.941	0.941	0.005	0.593
Within R ²	0.015	0.033	0.035	0.028	0.038	0.037	0.316		
Controls (lagged)	YES	YES	YES	YES	YES	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	YES	YES	YES	YES	NO	NO	NO
Time Fixed Effects	YES	YES	YES	NO	NO	NO	NO	NO	NO
County×Year Fixed Effects	NO	NO	NO	NO	NO	NO	NO	NO	NO

This table presents interactions of the difference-in-difference estimations from Table 2.4 using variations in the fixed effects. No regression includes region×time fixed effects. Only non-directly affected firms are included. All Columns specify the interaction if the difference-in-difference estimator with a low capitalization dummy (lowcap) which is set equal to 0 for all firms' banks above the median of the pre-flood capitalization distribution and set equal to 1 for the firms with banks below the median. The pre-flood capitalization is based on an average of the banks' regulatory capital ratio in the years 2012 and 2013. IndirAffected is a dummy variable constructed by measuring the exposure of the firm to the flood via its banks, according to Equation 2.2 and 2.3. Employment is the log of the number of firms' employees. Fixed Assets is the log of firms' fixed assets. Invest is the change in ln(total fixed assets) from t-1 to t. Unreported control variables are cash, total assets, current liabilities and the capital ratio. All controls are included as first lags. Cash is the log of all cash and cash equivalent of firms and is a proxy for the firms' liquidity. Total assets is the log of the banks total assets and is proxy for firm size. Current liabilities is the log of the firms' current liabilities and is a proxy for the firms' short-term indebtedness. Capital ratio is common equity divided by total assets, and controls for the firms' relative equity position. I control for firm and county×year fixed effects. Clustered standard errors on the firm level of the point estimates are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Chapter 3

Borrowers Under Water! Rare Disasters, Regional Banks, and Recovery Lending

Abstract: *We identify from the perspective of both banks and firms whether corporate recovery lending mitigates adverse economic effects after a natural catastrophe. We show that banks residing in counties that are unaffected by a natural disaster increase lending to firms inside affected regions by 3%. Firms domiciled in flooded counties, in turn, increase corporate borrowing if they are connected to banks in unaffected regions by 16%. We find no indication that recovery lending entails excessive risk-taking or rent-seeking. Our results suggests that regionally active banks are pivotal to mitigate disaster shocks for the real economy effectively.**

3.1 Introduction

Natural disasters have far-reaching socioeconomic implications. They can inflict casualties among the population (Cavallo et al., 2010); destroy the capital stock of entire regions (Odell and Weidenmier, 2004); depress investment, employment, and consumption (Vigdor, 2008; Strobl, 2011); and slow down long-term economic growth (Cavallo et al., 2013; McDermott et al., 2014).

We test if financial intermediaries mitigate the adverse economic effects of disaster shocks from the perspectives of both bank lending and firm borrowing.¹ First, we test

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¹A rich literature investigates banks' responses to financial shocks, which often imply a contraction of credit supply (e.g. Khwaja and Mian, 2008; Chodorow-Reich, 2014; Bolton et al., 2016), thereby contributing to recessions (Jermann and Quadrini, 2012). But rare disaster responses and subsequent credit market implications are much less well understood.

whether regional banks expand their corporate lending in response to the significant flooding of several German counties in late May and early June 2013, complementing evidence on banks' mortgage lending responses to natural disasters (see, e.g., Cortés, 2014; Cortes and Strahan, 2017; Chavaz, 2016). Next, we venture beyond banks' responses and consider the flip side of bank lending: firm-level borrowing. Based on a large firm-level sample of flooded firms, we track if any differential lending is routed towards small and medium-sized enterprises (SMEs) in regions struck by the disaster. Thereby, we provide an explicit test for corporate *recovery lending*.

Our identification strategy exploits the fact that banks are not only exposed to disaster shocks based on their own location, but also based on the location of their corporate customers. We therefore match loan customers' locations and bank identities to isolate the exogenous natural disaster shock faced by regional financial intermediaries emanating from their regional loan exposure. Our main test disregards banks in flooded regions (henceforth: **affected** counties) and compares only banks within unaffected regions that provided credit to corporate borrowers in affected counties (henceforth: **exposed** banks) with unexposed banks that primarily banked with local customers. Our insights on corporate *recovery lending* responses complement studies that identify mortgage loan responses to natural disasters (Cortés, 2014; Chavaz, 2016; Berrospide et al., 2016; Cortes and Strahan, 2017). In addition, we provide novel evidence about the existence and magnitude of *emergency borrowing* based on borrowing responses of firms that reside in affected counties, but are connected to banks domiciled in unaffected counties (henceforth: **treated** firms). Thereby, we complement studies on the direct effects of natural catastrophes on firm behavior (Noth and Rehbein, 2018).

We find that exposed banks in unaffected counties lend significantly more after 2013. This *recovery lending* effect amounts to an increase of 3% relative to unexposed banks. Against the backdrop of average loan growth of around 5% before the flood, this magnitude is economically meaningful. The differential lending increase is driven by unsecured, non-mortgage lending, consistent with the idea that banks plug the funding shortages of firms facing a sudden disaster shock. Regional banks' finance their response to the demand shock primarily by activating local savings. Wholesale funding, in contrast, exhibits no significant differential role to finance the lending

expansion of exposed lenders in unaffected regions. We find little evidence that *recovery lending* entails riskier banking. Neither proxies for solvency nor credit risk among exposed banks exhibit significantly different responses relative to unexposed banks. Likewise, we find no evidence that *recovery lenders* exploit flooded corporate borrowers as net interest income does not differ significantly.

Next, we trace the recipients of this differential lending expansion by exposed banks in unaffected counties. We compare corporate borrowing of treated firms in affected regions, i.e. those with ties to banks in non-flooded counties, to that of firms without such connections. Consistent with studies focusing on the direct effect of the Elbe flood on firm leverage by Noth and Rehbein (2018), we find that overall corporate borrowing declines after 2013 by 46%, commensurate with the contractionary nature of an adverse macro shock. The differential borrowing effect between treated and untreated firms is, however, significantly positive and amounts to 61%. Consequently, our results indicate that firms with existing ties to savings and cooperative banks in the region that were themselves not directly affected by the flood enjoyed *recovery lending*. This loan expansion overcompensated for adverse borrowing conditions, resulting in an overall increase of firm credit by 16%. Taken together, these results suggest that smaller, regionally active intermediaries constitute an important element in the financial system to mitigate tail risks that hit the real economy in general and are crucial to aid the recovery of disaster-struck SMEs in particular.

The Elbe flood was an economically significant shock, causing total damage of 6 billion Euros (BMI, 2013a) and igniting 180,000 insurance cases worth about 2 billion Euros (GDV, 2013). The flood resulted from a combination of heavy rainfalls, wet soil conditions, and a large hydraulic load in the river system during May 2013 (Schröter et al., 2015). The regions around the river Elbe and its tributaries were hit most harshly, yet other German regions were flooded, too. Figure 2.1 illustrates the geographical dispersion of damages inflicted by the flood in terms of the share of activated flood insurance contracts for nine categories after June 2013, which corresponds well to the spatial distribution of hydrological data (Thieken, 2016; Khazai et al., 2013).

We hypothesize that banks are important for any post-flood (re-)allocation of financial funds because only one-third of the flooding damages were insured in Germany (GDV, 2013).² Therefore, SMEs might have called on the banking sector catering to the region for additional credit to bridge potential financing gaps. Instead of relying on observed or expected physical damage, we exploit novel data on actual insurance claims to explain the role of small, regional banks as they respond to disaster-related damages. This metric gauges the economic magnitude of the natural disaster shock more directly compared to actual water levels during flooding (Gallagher and Hartley, 2017) or indicator variables if a disaster struck (Cortes and Strahan, 2017).

To analyze lending responses to disaster shocks, prior studies often identified disaster-struck financial institutions based on the location of the bank itself.³ The challenge is then to separate loan supply contractions from collapsing credit demand by non-financial firms in disaster-ridden regions, a difficult differentiation when both banks and firms are located in shocked regions themselves. Therefore, Cortes and Strahan (2017) identify disaster responses by exploiting the geographic exposure of U.S. banks to local mortgage markets with and without disaster shocks.

In this vein, our estimation of *recovery lending* effects relies on whether banks are exposed to disaster risk emanating from corporate borrowers in their portfolios that are, figuratively speaking, under water. Like Cortes and Strahan (2017), we refrain from defining banks as shocked if they are domiciled themselves in areas directly affected by the disaster. Instead, we gather data about banks' corporate customer locations. Banks are considered exposed to the disaster if and only if a critical share of their customers are located in counties directly affected by significant flooding damages as of category 5 in Figure 2.1. Cortes and Strahan (2017) then compare mortgage lending responses in non-shocked localities of multi-market banks with and without a connection to disaster-ridden markets.

We, in turn, provide evidence on how banks' exposures to a natural disaster through the spatial composition of corporate borrower locations influences post-shock

²Incomplete insurance coverage against disaster risk is consistent with evidence for the U.S. (Froot, 2001).

³See Sawada and Shimizutani (2008) for Japan, Klomp (2014) for a cross-country sample of banks, and Chavaz (2016) and Schüwer et al. (2018) for an identification based on U.S. branch and bank locations, respectively.

corporate lending, which is arguably more directly related to the recovery of the economy after a natural disaster than mortgage credit. Furthermore, we directly take the perspective of firms in affected counties and test explicitly for corporate *emergency borrowing* in terms of significant changes in firm leverage after the flood because of existing relationships to banks located in unaffected regions. Thereby, we demonstrate that additional lending actually arrives in the affected regions rather than being routed to investment opportunities commensurate with a “flight-to-safety” pattern.

Our approach gauges how disaster shocks affect corporate lending on a fairly granular level, because we observe bank-customer relationships and not only aggregate (mortgage) lending to geographical areas. We match approximately 1.1 million firms with around 2,000 banks operating in all German counties. To define bank-firm relationships, we (string) match bank and firm names from observable payment service relationships (Popov and Rocholl, 2017). Another advantage of our setting is the fact that many small savings and cooperative banks operate in Germany. These regionally active banks maintain close relationships with their corporate SME customers. Consequently, they are the first point of funding contact of constrained firms facing large information asymmetries that arise from the effects of a natural disaster (Cortés, 2014).

Regional banks are likely to play a pertinent role in terms of economic responses to and fallout from natural disasters for several reasons. Larrain (2006) established lower output volatility in countries with more credit due to banks’ superior abilities to pool and diversify shocks. Cortés (2014) shows that bank-dependent SMEs in the United States exhibit improved job retention and creation patterns if they are located in disaster-struck districts where truly local lenders operate, i.e., banks with more than two thirds of their deposit funding originating from within the district. She argues that the proximity of relationship banks helps to overcome increased opacity after natural disasters in SME lending. Generally, banks are better able than financial markets or insurances to cater to firms’ preferences for financial flexibility (Gorbenko and Strebulaev, 2010), for example with outright lending or additional credit commitments. Furthermore, the destruction of collateral implies an increase in information asymmetries after natural disaster shocks. Close relationship lenders with private information about local firms may be both more able and more willing

to provide (additional) credit under such circumstances (Degryse and Cayseele, 2000; Elsas, 2005).

3.2 Data: sources, combination, and culling

Firm-level data We obtain German firm-level data from the Dafne and Amadeus databases, both provided by Bureau van Dijk. The former contains the name of the bank (or banks) with which each firm maintains a payment relationship. We do not observe credit relationships directly. Based on annual vintages of the Dafne database, we construct a time-series of bank-firm relationships for more than a million firms between 2011 and 2015. This sample also includes the postal code of each firm, to which we match flood damage data obtained from the German Insurance Association (GDV, 2013), as depicted in Figure 2.1. We augment these bank-firm relationship data with firm-specific, annual financial accounts data from Amadeus. Because SMEs generally lack access to more complex financial markets, they mostly rely on banks for their financing needs. This renders the presence of SMEs in the data important in order to isolate any potential corporate *recovery lending*. The median firm in the sample has seven employees and assets of about 350,000 EUR, which is according to the definition of the European Commission (2003) a micro firm.

Bank-level data We collect data for small savings and cooperative banks, which are major players in regional markets and account for approximately one-third of aggregate total banking assets in Germany (German Council of Economic Advisors, 2014). They pursue regional relationship-based strategies, particularly aiming at SMEs. Therefore, they generate and possess more private information about their customers than do larger, nationally active banks (Behr et al., 2013), which reduces information asymmetries for assessing the borrowers' structural ability to repay their debts after a temporary, random disaster shock. These banks permit the clearest identification of lending responses to local disaster shocks, and we refer to them henceforth as local banks. Therefore, we do not sample commercial banks, central savings banks (Landesbanken), and the head organizations of the cooperative banking sector because they do not operate on confined regional markets, which obstructs the identification of

recovery lending. We combine the firm information with bank data from two Bureau van Dijk databases using bank-firm relationships that we obtain from a string-based match of bank names. The databases contain annual financial account information and provide the location of banks' headquarters. Thereby, we identify which banks were located in affected regions themselves.⁴ Because we lack any relationship information other than the banks' names in the Dafne database, we manually inspect many matches to ensure that the firm-level data are combined with the correct financial information about the banks from Bankscope and Orbis. We match around 99% of all bank-firm relationships. Most of the remaining 1% are (large) international banks that are mostly connected to large German firms, which we exclude.

Natural disaster data To gauge the damage inflicted by the Elbe flood of 2013, we use data on claims filed for insured properties that were damaged during the flood between May 25 and June 15, 2013, which we obtain from the German Insurance Association (GDV). The GDV aggregates the data on the economic value destroyed relative to what is covered by insurance contracts by county ("Kreis") for confidentiality reasons. We observe nine damage categories, which identify the percentage of insurance contracts for which a claim was filed by customers. Lower categories indicate less damage relative to the asset values covered by insurance contracts.⁵ In the baseline specification, we consider counties exhibiting average damage categories of 5 or higher as affected by the flood. The GDV collects these information from all of its 460 members, which include all major German insurance providers. The data also inform the risk calculation models of insurance companies and regional aggregates are reported regularly (GDV, 2013).

The regional aggregation of the damage data implies that we assume constant insurance coverage within each county. The identification strategy therefore exploits between-county variation in relative damage intensities. We combine the insurance claim rates data with the geographical location of both firms and banks. To isolate bank lending responses that are due to the disaster shock, our identification hinges on

⁴We use Bankscope until it was discontinued as of the accounting year of 2014 and the successor BvD product Orbis Bank Focus thereafter. For German banks the two data sources coincide almost perfectly. Descriptive statistics for the jointly available 2013 vintage of financial accounts is available upon request.

⁵Activation rates range from $\leq 0.04\%$ in Category 1 to 10%–15% in Category 9, see Figure 2.1 for a detailed definition.

banks that do business with SMEs that are located in affected counties, but not on the banks' own location.

Culling After combining valid firm, bank, and disaster data for the period 2011-2015, we take the following four steps to clean our data. First, we collapse the sample on the bank level and keep only relationships to savings and cooperative banks to ensure a correct mapping of regional damage data, which leaves us with 13,662 observations for 1,472 banks. Second, we sample only non-missing observations for the six bank variables that we use in our baseline regression: the natural logarithm of gross loans, equity over assets, the natural logarithm of total assets, cash over total assets, return on assets, and the share of securities relative to total assets. This process leaves us with a sample of 13,474 observations and 1,450 banks. Third, to consider a comparison of symmetric pre- and post-shock periods, we further restrict the sample to observations between 2011 and 2015 and exclude the year of the flood (2013) itself from our main analysis. This requirements leaves us with a sample of 5,307 observations for 1,373 banks. Last, we consider only banks that existed at least for one year before 2013 and at least for one year after 2013, thereby reducing the sample to 4,064 observations for 1,076 banks.

3.3 Specification and identification

3.3.1 Estimation of banks' responses

We compare the lending reported by banks and the total credit reported on firms' balance sheets in the two years prior to the flooding of German regions in 2013 with that of the two-year period after the flood. To test if lending differs significantly between exposed and unexposed banks in unaffected regions before and after 2013, we estimate a difference-in-difference regression:

$$\ln(\text{Loans})_{it} = \beta_1(\text{Exposed}_i \times \text{Post}_t) + \beta_2\text{Post}_t + \alpha_i + \alpha_t + \sum_{k=1}^K \gamma_k C_{kit-1} + \epsilon_{it}. \quad (3.1)$$

The dependent variable $\ln(\text{Loans})$ is the natural logarithm of gross loans for bank i in year t . Exposed is a dummy variable that identifies banks that lent primarily to firms located in affected counties. Subsection 3.3.2 details how we identify banks'

exposures. The Post dummy variable is equal to 0 for the years 2011 and 2012 and 1 for 2014 and 2015. We exclude the year of the flood (2013) itself because we want to unveil a more persistent *recovery lending* effect instead of instantaneous accounting adjustments due to the flood. The coefficient of interest is β_1 , which reveals the treatment effect of banks exposed, through borrowers, to the flood. To gauge unobservable bank traits, we specify bank fixed effects α_i which also absorb the single term Exposed. To capture business cycle effects, we specify year fixed effects α_t .⁶

– Table 3.1 around here –

All variables are defined in Table 3.1 and Table 3.2 shows descriptive statistics for the bank sample in the pre- and post-flood periods. The first panel of Table 3.2 shows that around 36% of German banks are exposed to the flood through their connections to borrowers headquartered in affected regions in 2013.

– Table 3.2 around here –

The second panel of Table 3.2 shows the five bank-specific control variables which enter Equation (3.1) in first lags. First, we use the natural logarithm of banks' total assets (Size) to differentiate small and large banks. Second, the ratio of banks' total equity to total assets (Capital Adequacy) controls for bank capitalization. In this way, we capture differences in the riskiness of banks and the distribution of abilities to buffer insolvency shocks in their credit portfolio. Third, Liquidity is the ratio of banks' cash holdings to total assets, which we use to gauge differences in banks' abilities to buffer short-term shocks. Fourth, we gauge the profitability of regional banks (RoA) with net return on annual average assets because more profitable banks should likewise be able to better absorb shocks (see, e.g. Gropp and Heider, 2010). Finally, we specify the share of securities relative to total assets to control for potentially different business models of banks (Securities). The average bank is small, with total assets of \$1.4 billion, and it exhibits an equity ratio of 8.5%. On average, banks in our sample have 1.8% cash, relative to their total assets. Profitability is low during our sample period, amounting to a mere 3 basis points on average. At the same time, security portfolios

⁶We also specify county-by-year fixed effects to control for unobservable business conditions per county in the online appendix.

account for only a quarter of banks' total assets, which corroborates the importance of lending business to these regional financial intermediaries.

The third panel of Table 3.2 contains additional dependent variables to differentiate any potential lending effects in response to disaster risk. We use Mortgage Loans and Customer Loans to investigate potential channels of recovery lending. Further, we employ Total Deposits and two sub-categories (deposits from customers (Customer Deposits) and banks (Bank Deposits)), and their wholesale funding (Wholesale Funding) to detect developments on the liability side. To test for systematic changes in loan quality, we regress loans, net of any write-offs, loan loss reserves, and impaired (or non-performing) loans (Impaired Loans) on flood exposure. We also show descriptive statistics for the performance variables that gauge banks' risk-return profiles. The z-score is the inverse distance to default (Laeven and Levine, 2009). We take the natural logarithm of the sum of the return on assets (RoA) and the capital ratio divided by the standard deviation of RoA. We also specify the ratio of net interest income over expenses (Net interest income) to gauge the relative importance of interest-bearing activities of the bank.

3.3.2 Identification through banks in unaffected regions

Our identification strategy isolates banks' corporate *recovery lending* responses by comparing intermediaries that were exposed to the disaster via the location of their firm customers to those that were not within unaffected counties only. The exclusion of banks that reside in affected regions themselves helps to avoid potentially confounding loan supply and credit demand responses, which lead to biased estimates (see, e.g. Berg and Schrader, 2012; Cortes and Strahan, 2017).

To identify which banks are exposed to this disaster-induced shock, we measure per bank how many borrowers are located in affected counties. For each bank, we take the weighted average of flood damage categories across all the firms that report a payment link with the bank. Each firm contributes to each banks' disaster exposure the damage category of the county where the firm is located. The demand shock exposure of bank i to the flood thus is the (size-weighted) flood damage to the bank's average firm-customer j , given the firms' county r , where N is the total number of

firms connected to bank i as of 2013:⁷

$$\text{Exposure}_i = \frac{\sum_{j=1}^N \left(\frac{\text{Assets}_j}{\text{Mean Assets}_N} \times \text{Claim Ratio Category}_r \right)}{N}. \quad (3.2)$$

We define banks as exposed (unexposed) if their exposure value is above (below) the median measured across all banks. Contrary to Cortés (2014) or Schüwer et al. (2018), we discard banks that are located in affected counties themselves.⁸ Figure 3.1 (a) illustrates how we isolate disaster shocks faced by exposed banks (depicted by solid circles) that are located in unaffected regions themselves, but are exposed to sufficiently many firms that reside in flooded (affected) regions (depicted by solid squares). Our identification hinges then on comparing these banks with unexposed banks in unaffected regions (depicted by transparent circles), which bank mostly with firms that have not been put under water by the flooding of the river Elbe in 2013.⁹

– Figure 3.1 around here –

We focus on banks that are not directly exposed in isolation because they offer the cleanest identification of changes in lending due to disaster shocks transmitted through banks' credit portfolios. To ensure that our exposure measure is valid, the local banks that we compare must not exhibit significantly different lending patterns before the flood of 2013, or differed in terms of other observable bank traits that might confound the effect of the disaster shock that we seek to identify. For the latter, we introduce bank and (region-) time fixed effects to ensure that level-differences in bank and region characteristics do not bias our results.

– Table 3.3 around here –

To make sure that exposed and unexposed banks followed similar trends in outcome before the disaster shock, Table 3.3 shows first and second moments of our

⁷This choice ensures to capture the actual relations in the year of the flood so as to obtain a precise measure of banks' flood exposures. In unreported results, we also show that Exposure barely varies between 2011 and 2013 and that our results are insensitive to changing the years of gauging weighted disaster exposures to 2011, 2012, or the average of both years.

⁸Recall that we consider counties with a damage category of 5 or larger as affected. We scrutinize this choice below. Within unaffected regions, our baseline sample has 390 exposed and 686 not exposed banks.

⁹An anecdotal example is Raiffeisenbank Ronshausen Marksuhl, a very small bank located in a county near the city of Kassel. It is located in a county of category 1, i.e. not affected directly by the flood. But most of its business is with firms residing one county closer to the Elbe. As a result, this bank is classified as affected due to the geographical composition of its corporate client portfolio.

variables measured in average changes in percent for the 2011-2012 period for both groups of banks. The rightmost column presents normalized differences (Imbens and Wooldridge, 2009; Lambert et al., 2017). A value for normalized differences larger than 0.25 would indicate that differences in the trends between both groups of banks are significant. The descriptive statistics illustrate that the variables fulfill the parallel trends assumption.

3.4 Banks' responses to the flood

3.4.1 Recovery lending: headline results

We estimate Equation (3.1) with OLS and clustered standard errors at the bank-level. Table 3.4 shows the main estimation results for our preferred sample of regional savings and cooperative banks residing in unaffected counties in the two years before and after the flooding of the river Elbe in 2013. The differential *recovery lending* effect is thus identified based on the within-county variation between exposed banks that catered credit to corporations in affected regions versus unexposed banks maintaining credit portfolios of local, non-flooded customers. The number of exposed banks in unaffected regions is 390 and compares to a total of 1,076 banks in these non-flooded regions, the market in the left panel of Figure 3.1.

The estimated coefficient β_1 is significant at the 5% level and positive in Column (1) of Table 3.4. Exposed savings and cooperative banks increased their lending after the Elbe flood relative to the unexposed savings and cooperative banks. The economic magnitude of this effect is very relevant: exposed savings and cooperative banks domiciled in unaffected regions increased their lending by roughly 3% ($(\exp(0.0288) - 1) \times 100 = 2.92$) compared with the group of unaffected banks in 2014 and 2015. This positive *recovery lending* effect corroborates the role played by local banks to cope with the fallout from disaster risk documented for developing economies in Berg and Schrader (2012) and the United States in Cortés (2014), Chavaz (2016), and Cortes and Strahan (2017).

This *recovery lending* effect contrasts, however, with the negative effects reported for more specialized (mortgage) lending in Japan (Sawada and Shimizutani, 2008) and the United States (Garmaise and Moskowitz, 2009) or micro-credit lending in developing economies (Berg and Schrader, 2012). A potential reason is that we consider overall corporate lending rather than specialized mortgage lending to private customers. Therefore, we specify mortgage lending as the dependent variable in Column (2). The interaction term between the post-flooding indicator and loans that are secured by real estate is not statistically significant. The result that local banks' mortgage lending does not respond significantly to the disaster shock may indicate that the value of corporate credit depends much more on (tacit) information that the bank possesses about the productivity of the firm and its management. In contrast, mortgage loan values depend more directly on tangible collateral that has been destroyed during the disaster. Our main findings are therefore consistent with the important role assigned to local lenders in Cortés (2014) and Chavaz (2016) to aid the recovery of SMEs.

Column (3) further isolates corporate *recovery lending* by showing that the baseline effect is driven by the customer loan. The interaction term is significantly positive and of a very similar magnitude compared to the baseline specification. Banks seem to have expanded credit in categories associated with non-land collateral requirements. In times when the quick provision of credit is important to firms – as after an event like a flood or another natural disaster – local lenders with private information about borrowers' structural ability to operate their business resort to the provision of (unsecured) lending.

The existence of *recovery lending* begs the question how local banks finance such additional lending. A first alternative is that local banks are best equipped to attract regional savers to fund shocked, yet structurally sound corporate borrowers in nearby markets. Retail savers may be reluctant to directly fund solvent, but opaque borrowers in neighboring affected regions. But they might be happy to entrust their local relationship lender to screen those borrowers on their behalf. Alternatively, observed lending expansions might actually result from intra-group treasury management. Local savings and cooperative banks are part of internal capital markets to the extent that head institutions – either central cooperative banks or so-called Landesbanken –

can and do channel funds between local entities (see Cremers et al., 2011; Puri et al., 2011).

To identify the sources of *recovery lending*, we therefore specify in Column (4) of Table 3.4 the logarithm of total deposits, separated into retail deposits in Column (5) and interbank deposits in Column (6). The significantly positive constituent term indicates that total deposits increased among all banks in those regions that are not directly affected by the flood. The positive interaction term further suggests that local banks that are indirectly affected via flooded corporate loan customers attract even more deposits compared to banks in unaffected regions that do not face a demand shock from borrowers that are under water.

Columns (5) and (6) shed further light on the channel how *recovery lending* is funded, via local savers alone or through internal capital markets of multi-market banks. For both types of deposits – retail and interbank – we find a positive differential effect between exposed and unexposed banks within unaffected regions. But together with the respective direct terms of the post-flood indicator, our results indicate a significant contraction of interbank deposits for all banks in unaffected regions as opposed to an increase in local retail deposits. This result is consistent with the finding reported by Cortes and Strahan (2017) that multi-market banks re-allocate funding to shocked localities, i.e. reduce interbank funding to unaffected regions. The significant increase in local retail funding documented in Column (6) corroborates the notion that local relationship lenders in unaffected markets succeed to activate local savings that can subsequently be routed to corporations in adjacent markets in addition to centrally managed internal capital market re-allocations. Below we will further scrutinize these first indications that treated banks do not only increase lending in general, but actually also route it to firms that were directly hit by the disaster.

In Column (7) we test if exposed banks in unaffected regions rely after the flood on capital markets as an alternative to satisfy the additional credit demand from their corporate loan customers in affected regions. The dependent variable is the logarithm of wholesale funding, defined as any securitized borrowing. Exposed banks in unaffected regions do not expand their reliance on capital market funding.

In sum, the evidence suggests that the existence of local banks alone might not

be the panacea to buffer regional macro shocks. Rather the existence of multi-market banking groups with strong local toeholds that re-allocate loan and funding portfolios as suggested by theoretical (Park and Pennacchi, 2009) and empirical evidence (Cremers et al., 2011) represent an important mechanism to facilitate *recovery lending*.

The effects of Size and securities shares are identical across lending specifications. Larger banks with lending-oriented balance sheets exhibit larger gross loan volumes. Capitalization, liquidity ratios, and profitability differences across banks in unaffected counties are not significant. In the two funding regressions depicted in Columns (4) and (5) it is also bank size, which primarily explains retail and wholesale funding volumes. There is some evidence that banks with relatively larger security shares relative to total assets also tend to rely more on wholesale funding. The remaining covariates do not have significant coefficients. We continue to include control variables but do not report their coefficients.

3.4.2 Robustness

Disaster threshold validity We realize that the definition of “disaster” exposure is fairly heuristic. Recall that we define banks as exposed if the asset-weighted average of the damage category associated with their observed customer portfolio is larger than the median. The control group comprises banks with a damage category average below that threshold. To ensure that the main results also hold for alternative definitions of exposed banks, we repeatedly estimate Equation (3.1) after redefining the disaster threshold to define the exposure status of banks starting at the 10th until the 90th percentile of the average damage exposure value distribution.

– Figure 3.2 around here –

Figure 3.2 (a) depicts the associated estimates of the interaction coefficient, with confidence bands pertaining to a certainty level of 10%. As of around the 35th percentile of the exposure distribution, these point estimates are positive for the largest part except a range of marginal significance between the 65th and the 75th percentile. The magnitude of this *recovery lending* effect tends to increase, which indicates that banks exposed to increasingly large credit demand shocks increase their supply of corporate credit to borrowers under water by more. We scrutinize this indication below

further by testing if obtained credit by firms in affected regions that are connected to banks in unaffected counties increases more compared to non-connected firms. The upshot is that the main effect is insensitive to the precise definition of disaster thresholds used to identify exposed and unexposed banks.

Placebo regressions To ensure that the flood of 2013 actually ignited *recovery lending* by banks, we test whether a pre-crisis lending trend existed by randomly assigning disaster-induced shocks to banks. Based on the unconditional probability of being shocked by the Elbe flood via corporate customer credit portfolios, we randomly allocate 1,000 times the status of disaster across the sample of banks and re-estimate Equation (3.1). Figure 3.2 (b) shows the resulting estimates of the interaction term's coefficient together with confidence bands at an α -level of 5%. If a confounding trend exists prior to the flood of 2013, it should reveal itself in terms of a significant difference-in-difference effect in these placebo regressions. Only in 63 out of 1,000 simulations a statistically significant *recovery lending* effect is estimated, mitigating concerns about confounding events.

Demand or supply Note that we cannot claim to have identified if the robust estimate of the positive difference-in-difference effect represents a post-disaster loan supply increase or a hike in *recovery lending* demand from disaster-struck customers. Isolating loan demand and supply would require credit registry data with multiple bank-relationships per customer to exploit within customer, between (flooded and non-flooded) bank lending variation and/or loan application and rejection data, both of which are unavailable. However, we argue that this limitation does not preclude the conclusion that local banks play a pivotal role in the economic recovery from natural disasters.

The main reason is that a *negative* post-disaster lending effect would clearly have deserved – if not required – more clarification on whether banks contracted supply, thereby possibly holding back recovery, or whether firms lowered demand because of bleaker economic prospects that were independent from banks' disaster responses. But it is hard to rationalize the *positive* lending response in our sample with a credit supply shock by banks in non-flooded regions rather than simply serving increased *recovery*

lending demand from disaster-struck corporate customers. Against the backdrop of this circumstantial pieces of evidence, it is rather difficult to reconcile our baseline finding with a credit supply side interpretation. Therefore, we have little reason not to side with the interpretation of Cortes and Strahan (2017) that local banks meet *recovery lending* needs, in particular of SMEs.

Further tests Before assessing the risk-return implications of *recovery lending* for banks in unaffected regions and subsequently the effects on observed credit usage by firms in affected regions, we expose the headline results to numerous robustness checks, for which we provide detailed results and discussions in Table B.I in the Online Appendix.

In a nutshell, we show that our results are robust against the exclusion of banks that are themselves located in affected regions and remain intact also when we identify exposed banks solely based on their location in affected regions. We further use a sample of matched banks across all regions based on the five observable bank traits, size, capitalization, liquidity, return on assets, and the security share of assets and show that our results are not driven by systematic differences pertaining to observable pre-crisis characteristics. We provide further details on the matching procedure in Table B.II and the Online Appendix. Next, we follow the example of Gormley and Matsa (2016) and re-estimate the main regression without time-varying (bank) control variables to show that our result do not hinge on a particular choice of covariates, which they do not. Our results also hold when we saturate the baseline regression with additional region \times time fixed effects. We also follow Cortes and Strahan (2017) and specify an indicator for potential parallel trend concerns, which again corroborates our results. Our results are also robust against including the crisis year of 2013 as an additional post disaster year and when we use a collapsed version of the sample to control for autocorrelation concerns as suggested by Bertrand et al. (2004). The main finding also obtains if we estimate a single cross-section of differences in covariates and a continuous indicator of disaster risk regressed on changes in lending in the spirit of Khwaja and Mian (2008).

3.4.3 Risk and return implications of *recovery lending*

The documented *recovery lending* effect raises the question, whether the expansion of lending entails some cost, for example in terms of taking more risk or by exploiting market power of disaster-captured SMEs. Therefore, we test in Table 3.5 if according indicators differed between exposed and unexposed banks.

– Table 3.5 about here –

Liquidity risk In Column (1) we test if *recovery lending* comes at the expense of reduced financial stability of local banks in terms of lower liquidity buffers. The significantly negative direct Post term in Table 3.5 confirms indeed that the asset share of cash and reserves held with the central bank declines among all local banks in unaffected regions. In addition, we find that exposed banks in unaffected regions reduce their liquidity buffer even further in the years 2014 and 2015 compared to their local peers without a large share of flooded corporate credit customers. This result indicates on the one hand that *recovery lending* implies a less resilient local banking system also in unaffected regions in terms of overall liquidity buffers held. The result also corroborates the importance of liquidity regulation beyond insuring against sudden deposit outflows. In addition to that function, liquidity buffers appear to be useful to fund the provision of additional lending to corporates subjected to regional macro shocks.

Credit risk To gauge changes in credit risk, we specify the ratio of impaired loans relative to gross loans as the dependent variable in Column (2).¹⁰ The effect of flood damage exposure on impaired loans is not statistically different from zero. A potential explanation for this result is that local banks are the “connoisseurs” of local SMEs and possess superior information about the general viability of their customers’ business models. Therefore, the practice to continue lending also after a disaster shock materialized reflects their ability to select those firms that pursue fundamentally sound business models. These firms may eventually return to their pre-flood growth paths once they have recovered from the massive disaster shock they have experienced.

¹⁰Note that these impaired loan data are only available for a smaller subsample.

Thus, banks' that appear to be financing firms' recovery from the flood do not seem to mechanistically have to incur systematically more credit risk.

Capital adequacy So far, the results suggest that additional lending in response to the additional demand from flooded customers is funded by an increase of retail deposit shares of total assets and reduction of liquidity shares. But it remains unclear whether total balance sheets expanded or contracted. A potential concern from a financial stability perspective could be that banks expanded their balance sheets with recovery loans without adjusting their equity positions. Therefore, we specify in Column (3) the ratio of total equity to total assets as the dependent variable. Strikingly, we do not only find that after the shock all banks increased their capital adequacy. In addition, exposed banks exhibit a differential built-up of equity relative to assets. Thus, banks *recovery lending* responses did not undermine the resilience of local banks.

Insolvency risk In Column (4) of Table 3.5, we test if the default risk of exposed banks in unaffected regions responded to the shock. We specify the Z-score (Laeven and Levine, 2009) as the dependent variable, where higher values imply more stable banks. Z-score of exposed and unexposed banks are not significantly different from another after the Elbe flood of 2013. This result confirms the preceding indication that exposed local banks manage to provide relatively more credit without having to increase the overall risk.

Rent extraction A potentially "dark side" to the so far benevolent interpretation of local banks' roles in providing *recovery lending* goes back to Rajan (1992). Locked-in borrowers may suffer from rent extraction by banks. In Column (5) of Table 3.5, we test if net interest margins charged by banks providing *recovery lending* are higher. The insignificant interaction term rejects the hypothesis of rent skimming by malicious local bankers. The increase in lending by affected banks does not seem to be driven by the exploitation of (temporary) market power of local lenders dealing with disaster-struck SMEs.

3.5 Firms' responses to lending adjustments

3.5.1 Specification

Results for banks' responses to the flooding of the river Elbe strongly suggest a corporate loan supply expansion by local banks induced by the demand shock experienced by borrowers put under water. But from observing banks' responses alone, we cannot yet firmly conclude that it is indeed *recovery lending* directed towards stressed firms. Any lending expansion may in fact be routed to non-stressed customers in unaffected regions. To answer the question who actually receives the additional lending, we analyze firm responses. We specify the following regression to explain observed corporate borrowing in affected regions:

$$\ln(\text{Credit})_{jt} = b_1(\text{Treated}_j \times \text{Post}_t) + b_2\text{Post}_t + a_j + a_t + \sum_{k=1}^K \lambda_k C_{kjt-1} + \epsilon_{jt}. \quad (3.3)$$

The dependent variable is the natural logarithm of firm j 's sum of total short term loans and long term debt in year t . Mimicking bank-level regressions, we compare the two years preceding the disaster 2011–2012 by setting the Post indicator equal to 0 with the two years after 2014–2015, which is when we code Post equal to 1. Treated indicates whether a firm is connected to an exposed bank in an unaffected region. In the baseline specification, firms with a relationship to at least one such local intermediary with a weighted average flood damage score based on their credit portfolio above the median are treated. We illustrate this approach in Figure 3.1 (b) and provide several alternative definitions of treatment and comparison samples below.

We estimate Equation (3.3) for a sample of firms with a headquarter in affected regions that have a connection to an exposed bank after 2013. This requirement avoids potential biases due to also including firms in the sample with which banks terminated credit relationships because of the flood of 2013. At the same time, firms in this sample may have selected themselves into banking relationships with intermediaries outside their home county because of the flood. We ensure below that our results also hold when we only include firms with identical banking relationships since 2011 until the end of our sample period.

The specified vector of firm-level control variables is lagged by one year and comprises the natural logarithm of total assets (L.Size^f), the Liquidity Ratio (the ratio of firms' cash and cash equivalents over short term liabilities), the Solvency Ratio (the ratio of the sum of net profits (after tax) and depreciations over total liabilities), and the Current Ratio (the ratio of firms' current assets over current liabilities). We only keep firms with non-missing observations for these traits and require that we have one observation per firm before and after 2013. This procedure leaves us with a sample of 5,657 observations for 1,677 firms. Descriptive statistics are depicted in the bottom panel of Table 3.2. The bottom panel of Table 3.3 corroborates the validity of our identification approach as all the observable firm traits do not exhibit statistically different developments prior to the flood of 2013.¹¹

3.5.2 Results on emergency borrowing by firms

All firm-level results provided in Table 3.6 refer to a sample of firms that are headquartered in affected counties. If the increase in bank loans documented above is indeed *recovery lending* directed towards stressed borrowers under water, we should observe a positive interaction effect on corporate borrowing for flooded firms that are connected to banks outside their flooded home-market, i.e. the treatment group of firms.

– Table 3.6 about here –

All banks To explore the effect on treated firms we start with a sample in the first column of Table 3.6 that comprises firms in affected counties with relationships to banks inside and outside affected regions. This sample has 40,796 observations for 11,965 firms. The difference-in-difference coefficient is significantly positive, which shows that corporate borrowers with a relationship to an exposed bank relative to firms with no such relationships expanded their borrowing in 2014 and 2015 compared to the pre-disaster period. In economic terms the estimated coefficient implies that treated firms increase their borrowing by roughly 61% ($(\exp(0.4768) - 1) \times 100 = 61.09$) compared to non-treated firms prior to 2013. Consistent with Noth and Rehbein

¹¹We do not winsorize the data, which results in large mean values for changes in firms' borrowings. Winsorizing all firm-level data at the 1st and 99th percentile does not affect estimated coefficients.

(2018), the direct effect of the flood on corporate debt is significantly negative and large, namely about -46%. That means that the total effect on treated firms coming from the flood is about 16%.

Out-of-region banking We imposed no restrictions on the firms sampled in Column (1) regarding the number and composition of their bank relationship(s). This approach implies that some firms might not only maintain a tie to an affected outside bank, but also a relationship to one or more banks that are domiciled in the affected areas themselves. In this setting it is difficult to identify if observed changes in credit are only driven by *recovery lending* from out-of-region banks that are exposed to the shock through their customer credit portfolios or reflect in part adjustments by banks that were flooded themselves. To mitigate this type of bias, we sample in Column (2) only firms that exclusively maintain ties to banks outside affected regions. The regression results confirm an increase in firms' credit if they were connected to exposed out-of-region banks, possibly because these banks possess and process better local expertise to assess the true fallout from the flood. Observed credit of treated firms increases after 2013 relative to firms with connections to an unexposed outside bank by around 123%. Non-treated firms inside affected regions exhibit, in turn, a significant borrowing drop of about 60% as indicated by the Post coefficient. The resulting total effect on treated firms of approximately 63% is economically large. As for the full sample, the negative direct term gauging the post-flood period indicates a reduction of corporates' debt levels after the flood in line with Noth and Rehbein (2018). Our results thus support their speculation that declining financial leverage of firms in disaster regions may be due to tighter financial constraints. The positive interaction term in this firm-level perspective corroborates the bank-level conclusions that local intermediaries seem to act as *recovery lenders* that are pivotal to bolster regional macro shocks.

Single bank relationships Whereas all firms in the sample of Column (2) are only connected to banks that are not themselves directly affected by the shock, some firms might still maintain multiple banking relationships to both exposed and unexposed out-of-region intermediaries. Therefore, we confine the sample in Column (3) to firms

with single bank relationships to avoid within-firm substitution that might bias our estimate of *recovery lending*. This specification further sharpens the test for *recovery lending* because those firms that rely on one exclusive provider of financial funds should respond most sensitively after a regional macro shock to such backstop lenders. In line with this notion, we estimate an interaction effect that is almost three times as large as in the headline results in Column (1).

SMEs Following up on this train of thought, we exclude in Column (4) also large firms because even if they report only one relationship to a bank, they might resort to capital markets to raise any additionally needed funding to buffer and recover from the shock. We limit the sample to SMEs as defined in the Amadeus database, which reduces the number of firms by 79 and the number of firm-year observations entering the estimation by 292. The resulting interaction term remains significantly positive.

Unaffected regions Column (5) presents a falsification exercise to challenge our previous conclusions. We test for a differential credit uptake also by firms that are connected to exposed banks, but reside themselves in unaffected regions. If *recovery lending* is indeed a response by banks in unaffected regions that are only indirectly exposed in response to credit demand hikes from shocked corporate borrowers in flooded regions, we should not see any differential borrowing by firms in unaffected regions that were high and dry. To test this hypothesis, we apply the same sample restrictions as in Column (4) to a sample of firms inside unaffected regions. The estimated difference-in-difference coefficient is insignificant indeed. Hence, the increase in credit by exposed banks seems not to be handed out as extra credit to firms in unaffected regions.

3.5.3 Robustness of firm-level results

Threshold validity affected regions As with the definition of the “disaster” exposure of banks before, the definition of affected and unaffected regions is fairly heuristic. Recall, that we defined regions as affected if their claim category is equal or larger than 5 (see Figure 2.1). To make sure that our firm results do not hinge on a specific

threshold choice, we provide regression results for the difference-in-difference coefficient from Column (3) of Table 3.6 for different thresholds between 1 to 8. We first estimate a regression in which counties with a claim category larger or equal to 1 are affected and then incrementally increase this threshold. The last estimate then defines counties as affected only with a claim category larger or equal to 8.

Figure B.I (a) in the Online Appendix depicts the associated estimates of the interaction coefficient, with 90% confidence bands. We find significant estimates for separating counties at category 4, 5, and 6. Thus, our results are robust against different stratification around the cut-off of 5. The insignificant estimates at the lower and upper bound are reasonable, too. At the lower bound, many unaffected counties enter the group of affected counties. For larger damage categories, too few counties are left for the affected group, which inflates the confidence intervals.

Placebo regressions Next, we provide a robustness test on the validity of the shock classification. We randomly allocate the status of treatment across all sampled firms 1,000 times and re-estimate Equation (3.3). Figure B.I (b) in the Online Appendix provides the set of estimates and 95% confidence bands of the difference-in-difference coefficient. Comparable to the results from the bank level, we find only in 45 out of 1,000 simulations an effect on firm borrowing that is statistically significant, which shows that also our firm level results are unlikely biased by confounding effects.

Further tests Table B.III provides further robustness tests of the firm-level results. Column (1) repeats the exercise from Column (4) of Table 3.6, but without control variables and shows that our results remain unchanged. With the same sample and similar to the robustness checks for the bank level, Column (2) checks for pre-existing trends. Again, we find our results unchanged. Note, that we also find no differences in changes in all variable prior to 2013 as shown in the bottom panel of Table 3.3. In Column (3) we restrict our sample to firms that pass a 1:1 matching on the control variables from the pre-2013 period. This reduces the sample to 260 firms with 883 observation. The main effect remains positive, significant, and also economically comparable to our baseline results in Table 3.6. In Column (4), we check for the less restricted sample from Column (2) of Table 3.6 whether our results are driven by firms

that have relationships to both, exposed and unexposed banks. We keep only firms with connections to banks that are either all exposed or all unexposed. Again, the main effects remain intact. In Column (5) of Table B.III, we show that our results also obtain when we restrict our sample to firms with unchanged bank-relationships through 2011-2014.

3.6 Conclusion

We investigate the role of local banking systems to spur economic recovery in the aftermath of natural disasters. Specifically, we isolate a corporate *recovery lending* channel by considering both the perspective of banks as the provider of financial funds as well as disaster-struck firms.

Our identification strategy exploits the flooding of the river Elbe of 2013 and its adjacent tributaries as an exogenous disaster shock. For a large sample of banks and small and medium enterprises (SME) in Germany between 2011 and 2015, we observe the inflicted economic damage at a granular regional level. By matching SMEs with their banks, we isolate financial intermediaries that are exposed to the disaster shock solely through their firm-customer portfolios, as opposed to being flooded themselves. Thereby, we are better able to separate the effects of natural disasters on loan demand and supply, respectively. By assessing lending responses of banks in unaffected counties as well as differential borrowing patterns of SME domiciled in flooded regions, we clearly demonstrate the existence and the importance of a corporate *recovery lending* channel to mitigate local macroeconomic shocks.

The first main outcome of our analysis is the identification of a statistically significant *recovery lending* effect. Local savings and cooperative banks that are located themselves in unaffected counties, but are at the same time exposed to disaster-ridden SMEs in flooded counties lend 3% more compared with unexposed local banks in the post-flood period, which compares to an average loan growth rate of 5% prior to 2013. Most of this credit expansion is funded by local retail deposits, which underpins the important role of regional banks to activate local depositors. This result is robust to matched sampling, placebo events, and falsification tests. This expansion in lending is neither associated with higher insolvency risk or higher loan impairment rates nor

with rent-skimming from (disaster-)captured SMEs. Instead, we show that shocked banks are better capitalized, albeit exhibiting reduced liquidity buffers.

The second main result emerges from firm-level analyses where we compare SME in flooded regions that have ties to outside *recovery lenders* in unaffected counties with those SMEs that do not have access to such backstop lenders. Firms treated with such regionally focused, yet out-of-region-banks exhibit indeed an overall positive effect on what we coin *emergency borrowing*. Firms without these ties, in turn, exhibit declining credit after the flood, corroborating earlier results on the direct effects of the flood on firms' leverage. This evidence therefore suggests that the lending hike by local banks in non-flooded counties is indeed used to cater to the additional credit demand by those firms directly hit by the shock.

Overall, these results suggest that local lenders fulfill an important function in developed financial systems, serving as providers of *recovery lending* to SMEs that have been struck by disaster.

Tables and Figures

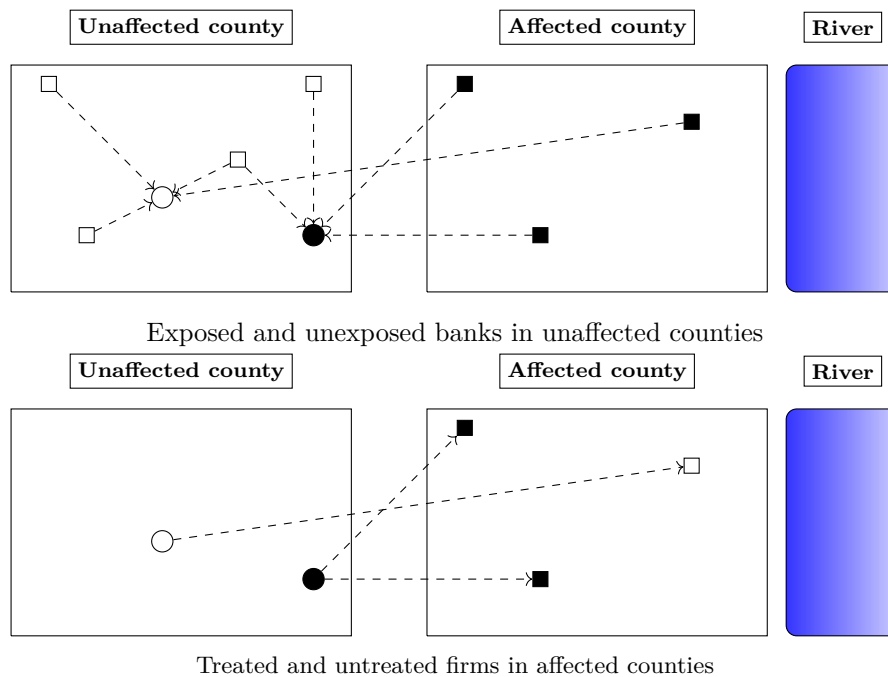
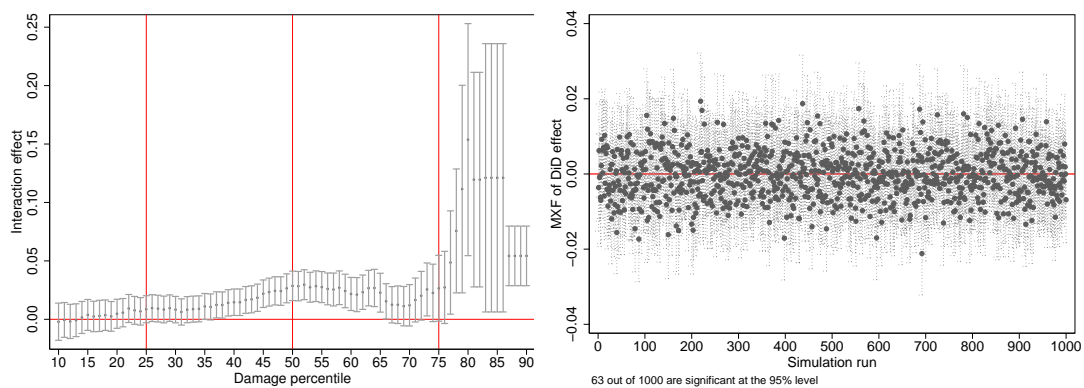


FIGURE 3.1: Identification of (un)affected regions, (un)exposed banks, and (un)treated firms

This figure shows banks (circles) and firms (rectangles) in affected and unaffected counties. In Subfigure (a), black rectangles indicate firms in affected counties whereas white rectangles indicate firms in unaffected counties. The dashed arrows connect firms to banks with which they maintain a relationship. The classification of exposed (black circles) and unexposed (white circles) banks depends on their exposure to firms in affected and unaffected regions, irrespective of their own location. Subfigure (b) shows the same exposed (black circle) and unexposed (white circles) banks outside the affected regions as before. The dashed arrows connect banks to firms with which they maintain a relationship. We categorize a firm in an affected county as treated if it is connected to an exposed bank in an unaffected county. Hence, black rectangles indicate treated firms in affected regions whereas white rectangles indicate untreated firms in affected counties.



(A) Variation of the exposed cut-offs

(B) Placebo tests for exposed

FIGURE 3.2: Robust bank exposure thresholds

Subfigure (a) shows the difference-in-difference point estimates (Interaction effect) and 90% confidence intervals (y-axis) from Equation (3.1) for varying definitions of the Exposed variable (x-axis, Damage percentile). We start with a separation of exposed and unexposed banks at the 10th percentile (i.e. Exposed is 0 for values below the 10th percentile and 1 for values equal or above the 10th percentile) and show the estimates until the 90th percentile cut-off. The baseline result is the estimate at the median. Subfigure (b) displays the interaction effect and the 95% confidence band for the baseline regression on the y-axis. Exposed is randomly distributed among all banks based on the actual average likelihood that a bank was affected in 2013. We repeat this randomization for 1,000 runs.

TABLE 3.1: Variable definitions

	Definition
Banks	
Exposure	The weighted average of flood damage categories across all the firms on the bank level.
Exposed	Dummy variable (median split of Exposure) indicating whether a bank is affected by the flood through its firms.
Post	Dummy variable that is one for the years 2014-2015 and zero for 2011-2012.
Gross Loans	Gross Loans in Billion USD. Includes Residential Mortgage Loans (Mortgage) + Other Mortgage Loans + Other Consumer/ Retail Loans + Corporate & Commercial Loans (Commercial) + Other Loans (Other) + Reserve against possible losses on impaired or non performing loans. The vast majority of banks only report the following three sub-categories:
Size	Total assets in Billion USD.
Capital Adequacy	% Share of equity on Total Assets. Includes common equity + Non-controlling interest + Securities revaluation reserves + Foreign exchange revaluation reserves+ Other revaluation reserves
Liquidity	Share of cash on total assets. Cash: Cash and non-interest-earning balances with central banks.
Securities	Share of Securities on Total Assets. Securities: Includes reverse repos and cash collateral + Trading securities + Derivatives + Available for sale securities + Held to maturity securities + At-equity investments + Other securities.
RoA	Net Income over Average Total Asset.
Mortgage Loans	Loans secured by a land charge (usually residential property).
Other Loans	All Loans and leases which do not fall into any other category. In practice: All loans not secured by residential property collateral.
Total deposits	Total deposits in Billion USD. Includes customer and bank deposits.
Bank Deposits	The sum of all deposits from other banks in Billion USD.
Customer Deposits	The sum of all customer deposits in Billion USD.
Wholesale Funding	Wholesale funding in Billion USD. Includes the sum of long term funding, trading liabilities and derivatives.
Impaired Loans	Reserve against possible losses on impaired or non performing loans as a share of Gross Loans.
Z-score	Distance to default measure. Z-score is defined as $\ln(\text{RoA} + (\text{Equity}/\text{Total Assets})/\text{sd}(\text{RoA}))$.
Net Interest Income	Net interest income or expense (net position) in million USD.
Firms	
Credit Treated	Total short term loans and long term debt in Million USD. Dummy variable that indicates whether a firm is connected to an exposed bank or not.
Post	Dummy variable that is one for the years 2014-2015 and zero for 2011-2012.
Size ^f	Total assets in Million USD.
Liquidity Ratio	The ratio of firms' cash and cash equivalents over short term liabilities.
Solvency Ratio	The ratio of the sum of net profits (after tax) and depreciations over total liabilities.
Current Ratio	The ratio of firms' current assets over current liabilities.

TABLE 3.2: Descriptive statistics

	Mean	SD	Percentile	
			1st	99th
Bank sample				
<i>Treatment and main left-hand side variable:</i>				
Exposure	1.841	1.055	1.000	4.357
Exposed	0.361	0.480	0.000	1.000
Gross Loans (bil\$)	0.897	1.935	0.021	7.526
<i>Controls:</i>				
Total Assets (billion\$)	1.401	2.765	0.040	11.062
Capital Adequacy	0.085	0.020	0.045	0.147
Liquidity	0.018	0.009	0.000	0.045
RoA	0.003	0.002	0.000	0.010
Securities	0.265	0.117	0.043	0.593
<i>Alternative left-hand side variable:</i>				
Mortgage Loans (billion\$)	0.407	0.886	0.000	3.542
Customer Loans (billion\$)	0.443	1.047	0.007	3.502
Total Deposits (billion\$)	1.232	2.359	0.037	9.655
Customer Deposits (billion\$)	1.026	1.957	0.030	8.445
Bank Deposits (billion\$)	2.051	4.589	0.020	18.690
Wholesale Funding (billion\$)	0.351	2.198	0.010	4.870
Impaired Loans	0.037	0.037	0.000	0.134
Z-score	4.714	1.293	2.565	8.330
Net Interest Income	0.023	0.004	0.013	0.034
Firm sample				
<i>Treatment and main left-hand side variable:</i>				
Treated	0.813	0.390	0.000	1.000
Credit (million\$)	2.405	13.367	0.000	32.918
<i>Controls:</i>				
Size ^f (million\$)	8.562	35.096	0.031	86.178
Liquidity Ratio	0.041	0.096	0.000	0.541
Solvency Ratio	0.305	0.328	-0.629	0.938
Current Ratio	0.048	0.100	0.001	0.595

This Table presents summary statistics for all the variables used in the analyses. The baseline bank-level sample comprises 4,064 observations for 1,076 banks. Due to missing information, we have only 4,058 observations for Mortgage Loans (1,076 banks), 2,459 observations for Impaired Loans (1,031 banks), and 3,810 observations for the Z-score (1,048 banks). Banks' balance sheet information come from Bankscope for the period 2011-2014 and from Orbis for 2015. The firm sample comprises 5,657 observations for 1,677 firms (see Column (2) of Table 3.6). Detailed definitions of the variables are provided in Table 3.1.

TABLE 3.3: Pre-2013 statistics for affected and unaffected banks

	Affected		Unaffected		ND
	Mean	SD	Mean	SD	
Pre-2013 average changes					
<i>Bank sample:</i>					
D.Gross Loans	0.056	0.073	0.051	0.069	0.05
D.Size	0.032	0.067	0.030	0.067	0.03
D.Capital Adequacy	0.093	0.102	0.064	0.083	0.22
D.Liquidity	-0.181	0.345	-0.202	0.428	0.04
D.RoA	0.095	0.701	0.048	0.585	0.05
D.Securities	0.077	0.203	0.086	0.205	-0.03
D.Mortgage Loans	0.054	0.272	0.044	0.241	0.03
D.Customer Loans	0.074	0.109	0.071	0.104	0.02
D.Total Deposits	0.030	0.068	0.029	0.069	0.01
D.Customer Deposits	0.034	0.065	0.032	0.066	0.02
D.Bank Deposits	0.039	0.297	0.034	0.214	0.02
D.Wholesale Funding	-0.099	0.315	-0.098	0.438	-0.00
D.Impaired Loans	-0.127	0.573	0.013	0.973	-0.12
D.Z-score	0.023	0.024	0.014	0.021	0.27
D.Net Interest Income	-0.027	0.101	-0.025	0.090	-0.01
<i>Firm sample:</i>					
D.Credit	186.320	4267.763	4.236	50.500	0.04
D.Size ^f	0.120	0.707	0.352	3.369	-0.07
D.Liquidity Ratio	0.525	5.714	0.697	4.789	-0.02
D.Solvency Ratio	4.361	118.803	1.843	18.175	0.02
D.Current Ratio	0.257	1.564	0.352	1.473	-0.04

This Table presents average changes (in percent) and standard deviations for the period 2011-2012 for the major variables that are used in the analyses for (un)exposed banks and (un)treated firms. Detailed definitions of the variables are provided in Table 3.1. Normalized differences (Imbens and Wooldridge, 2009) are shown in the last column. A value for normalized differences larger than 0.25 indicates that averages between both groups of banks are significant.

TABLE 3.4: Recovery lending by banks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	ln(Loans)	ln(Mortgage Loans)	ln(Customer Loans)	ln(Total Deposits)	ln(Bank Deposits)	ln(Customer Deposits)	ln(Wholesale Funding)
Post	0.0548*** (0.0077)	0.0576*** (0.0226)	0.0735*** (0.0139)	0.0193*** (0.0069)	-0.1324*** (0.0196)	0.0500*** (0.0069)	-0.6210*** (0.0562)
Exposed × Post	0.0288*** (0.0062)	0.0096 (0.0238)	0.0246*** (0.0115)	0.0142*** (0.0046)	0.0404*** (0.0170)	0.0113*** (0.0050)	-0.0447 (0.0559)
L.Size	0.6845*** (0.0518)	0.6903*** (0.1623)	0.6361*** (0.0629)	0.6856*** (0.0511)	1.1046*** (0.0986)	0.5806*** (0.0515)	0.9011*** (0.2452)
L.Capital Adequacy	-0.0192 (0.2603)	0.6259 (0.9141)	0.0764 (0.5096)	-0.5448*** (0.2365)	-1.7011*** (0.6669)	-0.2523 (0.2392)	-1.0088 (1.9194)
L.Liquidity	-0.5853* (0.3525)	0.3204 (1.0087)	0.6965 (0.7233)	-0.2780 (0.3320)	-1.3498 (0.8682)	0.1584 (0.3571)	-0.9916 (2.8294)
L.RoA	0.4885 (0.6707)	-1.4463 (3.9140)	-0.3669 (1.3157)	-0.3400 (0.7259)	-2.9917 (2.2003)	0.5659 (0.5931)	3.1125 (5.3878)
L.Securities	-0.3735*** (0.0503)	-0.5447*** (0.2275)	-0.4789*** (0.1001)	-0.0537 (0.0386)	0.1525 (0.1426)	-0.1230*** (0.0410)	1.1707*** (0.3849)
Within R2	0.67	0.13	0.52	0.61	0.20	0.64	0.30
Observations	4064	4058	4064	4064	4064	4064	4064
Banks	1076	1076	1076	1076	1076	1076	1076
Affected Banks	390	390	390	390	390	390	390
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean	5.90	4.87	5.24	6.30	4.37	6.11	1.56
SD	1.32	1.63	1.29	1.27	1.42	1.27	1.71

This Table shows regression results for Equation (3.1) with different dependent variables. Column (1) reports the results for gross loans. Column (2) reports the results for Mortgage Loans, which are secured by a land charge (usually residential property). Six observations for mortgage loans are missing. Our results for the other categories remain unchanged if we further reduce the sample to 4,058 observations. Column (3) reports the results for loans to cooperatives and households (excluding real estate); Customer Loans. The dependent variables in Columns (4), (5), and (6) are Total Deposits, Bank Deposits, and Customer Deposits. The dependent variable in Column (7) is Wholesale Funding. Exposed is a dummy variable indicating whether a bank is exposed to the flood through its firms, i.e., whether it is above or below the median exposure of its firm damages. Post is a dummy equal to 0 for the years 2011-2012 and 1 for 2014-2015. Included control variables are: Size as the natural logarithm of total assets; Capital Adequacy as the ratio of equity to total assets; Liquidity as the share of cash on total assets; RoA as return on assets; Securities as the share of total securities over total assets. All bank controls are lagged by one year (indicated by L.). We control for bank and year fixed effects. Clustered standard errors on the bank level of the point estimates are in parentheses. The last two rows show the mean and standard deviation of the right-hand side variable, respectively. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE 3.5: Bank liquidity and risk

	(1)	(2)	(3)	(4)	(5)
	Liquidity	Impaired Loans	Capital Adequacy	Z-score	Net Interest Income
Post	-0.0041*** (0.0006)	-0.0238*** (0.0053)	0.0078*** (0.0005)	0.2432*** (0.0494)	-0.0011*** (0.0002)
Exposed × Post	-0.0012*** (0.0005)	-0.0023 (0.0018)	0.0018*** (0.0004)	-0.0204 (0.0427)	0.0000 (0.0002)
Within R2	0.12	0.10	0.68	0.66	0.46
Observations	4064	2459	4064	3810	4064
Banks	1076	1031	1076	1048	1076
Affected Banks	390	377	390	376	390
Bank controls	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Mean	0.02	0.04	0.09	4.71	0.02
SD	0.01	0.04	0.02	1.29	0.00

This Table shows regression results for Equation (3.1) with different dependent variables. Column (1) reports the results to explain Liquidity. Column (2) presents the estimates with impaired loans as a share of gross loans as the dependent variable. Column (3) reports the results for Capital Adequacy. Column (4) presents the results for the Z-score, defined as $\ln(1 + RoA + (Equity/TotalAssets))/sd(RoA)$. For negative values of $RoA + (Equity/TotalAssets)/sd(RoA)$, we set the Z-score to zero. The Z-score is a distance to default measure, such that higher values indicate decreased riskiness (Laeven and Levine, 2009). In Column (5), we use the banks' net interest income (interest income - interest expense) over total assets as the dependent variable. Exposed is a dummy variable indicating whether a bank is affected by the flood through its firms, i.e., whether it is above or below the median exposure of its firm damages. Post is a dummy equal to 0 for the years 2011-2012 and 1 for 2014-2015. Included control variables are: Size as the natural logarithm of total assets; Capital Adequacy as the ratio of equity to total assets; Liquidity as the share of cash on total assets; RoA as return on assets; Securities as the share of total securities over total assets. All bank controls are lagged by one year (indicated by L.). Note that the results for Liquidity, Capital Adequacy, and the Z-score stay the same when we exclude (lagged) Liquidity, Capital Adequacy, and RoA as independent variables. We control for bank and year fixed effects. Clustered standard errors on the bank level of the point estimates are in parentheses. The last two rows show the mean and standard deviation of the right-hand side variable, respectively. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE 3.6: Credit of affected firms

	(1)	(2)	(3)	(4)	(5)
	All	No inside banks	One bank relationship	Only small and medium-sized	Firms in unaffected regions
	ln(Credit)				
Post	-0.6249*** (0.1995)	-0.9292*** (0.2487)	-1.3218*** (0.3003)	-1.1975*** (0.3219)	-0.2260*** (0.0414)
Treated × Post	0.4768** (0.2011)	0.8024*** (0.2629)	1.2955*** (0.3286)	1.0939*** (0.3507)	-0.0106 (0.0644)
L.Size ^f	0.8877*** (0.0797)	0.8474*** (0.1810)	0.9815*** (0.2179)	0.9555*** (0.2284)	0.9267*** (0.0497)
L.Liquidity Ratio	-0.3743 (0.8565)	-3.2074 (2.3697)	0.2934 (2.6362)	0.1297 (2.6060)	-2.8530*** (0.7059)
L.Solvency Ratio	-1.6665*** (0.1744)	-1.3491*** (0.4195)	-1.4896*** (0.5056)	-1.2835** (0.5134)	-1.7684*** (0.1082)
L.Current Ratio	0.7459 (0.6750)	3.2791 (2.1046)	0.4699 (2.4822)	0.3265 (2.4411)	3.0209*** (0.6327)
Within R2	0.02	0.02	0.04	0.03	0.02
Observations	40796	5657	2966	2674	70569
Firms	11965	1677	906	827	21506
Affected Firms	11490	1364	723	667	6184
Firm FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Mean	10.02	9.43	8.14	8.01	8.02
SD	5.77	5.98	6.10	5.97	5.86

This Table shows regression results for the firm level. The sample in the first four columns comprises firms in affected regions while the sample in the last column comprises firms in unaffected regions. In each column ln(Credit) as the natural logarithm of firm credit is the dependent variable. Treated is a dummy variable indicating, whether a firm has a relation to an exposed bank according to our baseline setup (median split of Exposure). In Column (1) we consider all firms as affected if they have any relation to an affected bank after 2013. In Column (2) we dismiss all firms with a connection to a bank inside an affected region. In Column (3) we further shrink the sample to firms with only single bank relationships. We further reduce the sample to small and medium-sized firms (according to the Amadeus definition) in Column (4). Column (5) has the same restrictions as Column (4) but for a sample of firms that reside in unaffected regions. Post is a dummy equal to 0 for the years 2011-2012 and 1 for 2014-2015. Included control variables are: Size^f as the natural logarithm of firms' total assets; Liquidity Ratio as the ratio of firms' cash and cash equivalents over short term liabilities; Solvency Ratio as the ratio of the sum of net profits (after tax) and depreciations over total liabilities; Current Ratio as the ratio of firms' current assets over current liabilities. All firm controls are lagged by one year (indicated by L.). We control for firm and year fixed effects. Clustered standard errors on the firm level of the point estimates are in parentheses. The last two rows show the mean and standard deviation of the right-hand side variable, respectively. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Appendix B

B.I Robustness for bank-level results

Sampling flooded banks. We test in columns (1) and (2) of Table B.I whether the identified *recovery lending* effect is driven by the exclusion of banks that are located in affected markets. Accordingly, we include the 304 local banks located in regions that were directly affected by the flood, leading to an increased sample size of 5,180 bank-year observations in the two pre- and post-flood years, respectively. The headline result is also confirmed for this enlarged sample, irrespective of whether we define an exposed bank as before as an above median share of customers residing in affected counties in Column (1) or solely based on location as shown in Column (2). We prefer the specification of exposed banks in unaffected regions only to better isolate the supply responses of banks to demand shocks more clearly.

Matching. We estimate the baseline regression in Column (3) of Table B.I on a sample of matched banks across all regions based on the five observable bank traits: size, capitalization, liquidity, return on assets, and the security share of assets. The matched sample helps to ensure that our results are not driven by systematic differences pertaining to observable pre-crisis characteristics. For example, larger banks might have a larger portfolio of out-of-region borrowers, which might render them more likely to be categorized as exposed. To mitigate spurious counterfactual concerns, we conduct propensity score matching based on size, capitalization, liquidity buffers, profitability, and capital-market affinity observed prior to the flood in 2012. We conduct this matching for the sample of regional banks in all counties, apply a 1:1 caliper match with a caliper width of 0.01 (Caliendo and Kopeinig, 2008), and identify propensity score matches for 377 exposed banks. Table B.II provides details for the matching procedure and shows the mean for the group of exposed and unexposed banks for the unmatched and matched sample for each variable, respectively. We further provide the bias between both groups and the reduction of the bias through the matching procedure in %, a t-test of the difference in means, and the variance ratio of exposed over unexposed banks which should equal 1 if there is perfect balance. The standardized % bias is the difference of the sample means between exposed

and unexposed banks as a percentage of the square root of the average of the sample variances in both groups. The bottom panel shows the overall statistics for both samples: the Pseudo- R^2 from the probit estimation and the corresponding p-values of the likelihood-ratio test of the joint insignificance of all the regressors (in predicting Exposed). Table B.II also shows the mean and median bias as summary indicators of the distribution of the absolute bias and Rubin's B and R, i.e. the standardized difference of the means and variances of the propensity score between both samples. The upshot of Table B.II is a reduction in the bias between the covariates of both groups. The variance ratio of exposed over unexposed that are close to 1 in the right-most column show that both groups of banks become even more similar in terms of covariates after the matching. The bottom panel corroborates this conclusion. The covariates from the matched sample have little power in predicting whether a bank is exposed or not.

The main estimation based on the matched sample yields a treatment effect that is significant at the 1%-level. The magnitude is somewhat smaller compared to the baseline estimate, but remains economically significant. We therefore conclude that neither the inclusion of banks in affected regions, nor unobservable confounding factors at the bank level appear to be driving the documented *recovery lending* by small relationship lenders to German SMEs after the Elbe flood.

Endogenous controls. Results from difference-in-difference estimation might be biased if the explanatory variables are affected by the treatment (Angrist and Pischke, 2009). Whereas we deliberately chose to rely on banks in unaffected counties paired with the spatial distribution of their corporate loan customer locations to identify exposure to the disaster shock, we cannot rule out that observable bank traits of local banks in unaffected counties are entirely orthogonal to local conditions of possibly neighboring regions. Therefore, we follow the example of Gormley and Matsa (2016) and specify in Column (4) of Table B.I only the post-flooding indicator and the interaction term. The explanatory power of this specification deteriorates considerably. The within- R^2 plummets to 47% compared to 67% in the main result. Yet our main effect of interest remains intact and statistically significant. Also the magnitude resembles the finding from our main regression very closely.

County-by-year fixed effects. Government aid and insurance payments may confound our *recovery lending* effect. Section B.III and Table B.V provide the sparse information available on the rather substantial federal transfer payments that were routed in the aftermath of the flood to the state and county level for quick and direct relief measures. Public disaster assistance after the 2013 Elbe flood was channeled through local governments in affected regions. In counties that were hit hard by the flood, federal financial assistance might have addressed most (up to 80%) of the needs of local municipalities, public utilities, and corporate customers of banks, such that they demanded less credit from the private sector. Detailed data on federal support schemes are unavailable. Therefore, we cannot test this explanation explicitly. But we can pursue a “brute force” approach and specify county-by-year fixed effects to mitigate concerns that fiscal or other transfers contaminate our results. Column (5) of Table B.I provides no evidence that government aid differed systematically within regions, in which case our fixed effects control for aid payments. *Recovery lending* and government aid thus seem to be complements rather than substitutes (see also Cortes and Strahan, 2017) .

Parallel trends. Although the univariate descriptive evidence strongly supports that exposed and unexposed banks did not exhibit systematic differences in observable traits, the validity to compare these banks in a multivariate context deserves scrutiny. To ensure that no differential trends already existed prior to the flooding event, we therefore follow Cortes and Strahan (2017) and specify an indicator for potential parallel trend concerns labeled *PT* and also interact it with the treatment indicator. Column (6) of Table B.I shows that our main effect remains intact. The control for *PT* is significantly different from zero though, possibly indicating differences in covariates of exposed and unexposed banks in the year 2012. We are cautious to assign too much weight to this latter finding though. For once, our annual analysis is much less suited to test for immediate pre-crisis trends – and their violation – compared to studies using higher-frequency data, such as e.g. monthly series in Cortes and Strahan (2017). Moreover, our results are not obviously prone to concerns about violated parallel trend assumptions given the insignificant normalized first differences for all covariates for both groups of banks between the periods 2011 and 2012, shown

in the top panel of Table 3 in the main body of the paper.

Exclusion of 2013. Next, we challenge our results with regard to a potential bias due to the exclusion of the year of the flood itself. The results in Column (7) of Table B.I include observations from the year 2013 as well and we specify the Post indicator equal to one in the years 2013, 2014, and 2015. The interaction effect remains statistically significant and positive. The magnitude of the estimated coefficient is also virtually identical compared to the baseline results without observations from 2013.

Autocorrelation concerns. Another concern in difference-in-difference regressions is the potential presence of auto-correlation in the dependent variable (Bertrand et al., 2004). The specification of year-by-county fixed effects above mitigates such concerns. In addition, we also remove the time-series component from the data by taking the means of both the dependent and explanatory variables for the pre- and post-disaster periods, i.e., 2011-2012 and 2014-2015. The results from this cross-sectional estimation appear in Column (8) of Table B.I. The interaction term is still positive and significant in this specification, confirming that our baseline results are not biased by auto-correlation.

Continuous exposure. Column (9) of Table B.I features a related test. We estimate a single cross-section of differences in covariates and a continuous indicator of disaster risk regressed on changes in lending. The dependent variable is the change in the natural log-levels of (average) pre- and post-flood lending, which is explained by the continuous damage indicator per bank resulting from the asset-weighted sum of damage indicators of its customers. The log-levels of control variables are specified in changes, too. Consequently, we neither have to control for unobserved bank nor time effects. We account for unobserved heterogeneity across regions with county fixed effects. The resulting coefficient is significantly positive, supporting our result that local banks that were more exposed to the natural disaster shock of the Elbe flood expanded their lending to SMEs. In economic terms that means that if we move a bank from the 5th percentile of damages (around 1.0) to the 95th percentile (6.0), lending increases by $5 \times 0.0209 = 0.105$, i.e., by around 11%, as of 2013 and relative to the pre-flood period.

TABLE B.I: Robustness of bank-level results

	(1) With affected regions	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
			Matching	Without controls	Regional × Time FE	Parallel trends	2013 in post period	Collapse	Continuous shock D, ln(Loans))
Post	0.0594*** (0.0069)	0.0613*** (0.0070)	0.0641*** (0.0100)	0.1829*** (0.0061)	0.0378*** (0.0080)	0.0526*** (0.0078)	0.0741*** (0.0073)	0.0534*** (0.0070)	
Exposed × Post	0.0257*** (0.0049)	0.0185*** (0.0049)	0.0185*** (0.0068)	0.0301*** (0.0093)	0.0498*** (0.0199)	0.0336*** (0.0071)	0.0227*** (0.0059)	0.0277*** (0.0061)	
Exposure									0.0209** (0.0089)
PT						0.0136*** (0.0040)			
Exposed × PT						0.0096** (0.0047)			
L.(D.)Size	0.6765*** (0.0468)	0.6767*** (0.0472)	0.6978*** (0.0638)	0.6750*** (0.0504)	0.6750*** (0.0504)	0.6855*** (0.0518)	0.5488*** (0.0331)	0.8295*** (0.0400)	0.9934*** (0.0272)
L.(D.)Capital Adequacy	-0.2763 (0.2297)	-0.2707 (0.2294)	0.0458 (0.3351)	0.3027 (0.2833)	0.3027 (0.2833)	0.0032 (0.2609)	-0.0158 (0.2237)	-0.0640 (0.2901)	0.9475*** (0.2710)
L.(D.)Liquidity	-0.7856** (0.3141)	-0.8169*** (0.3140)	0.1781 (0.4473)	-0.3547 (0.4338)	-0.3547 (0.4338)	-0.5746 (0.3532)	0.0777 (0.7348)	-0.5464 (0.4747)	0.1991 (0.2357)
L.(D.)RoA	0.7001 (0.5910)	0.7734 (0.5975)	0.8090 (0.6836)	1.1013 (0.7148)	1.1013 (0.7148)	0.5127 (0.6730)	0.4586 (0.5487)	1.0094 (1.2042)	-1.4397 (1.3552)
L.(D.)Securities	-0.4165*** (0.0451)	-0.4167*** (0.0455)	-0.3567*** (0.0608)	-0.3393*** (0.0549)	-0.3393*** (0.0549)	-0.3723*** (0.0502)	-0.2863*** (0.0415)	-0.5009*** (0.0661)	-0.5977*** (0.0531)
Within R2	0.68	0.68	0.67	0.47	0.77	0.67	0.58	0.80	0.80
Observations	5180	5180	2835	4156	4064	4064	5140	2052	1052
Banks	1372	1372	754	1076	1076	1076	1076	1076	1052
Affected Banks	686	673	377	390	390	390	390	390	
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	No	No	No	No	No	No	No	No	Yes
Region × Time FE	No	No	No	No	Yes	No	No	No	No
Mean	10.02	9.43	8.14	8.01	5.90	5.90	8.02	8.77	5.85
SD	5.77	5.98	6.10	5.97	1.32	1.32	5.86	5.80	1.30

This Table shows the robustness tests for the baseline regression. The dependent variable in the first eight columns is the natural logarithm of banks' gross loans (ln(Loans)). Column (1) and Column (2) report the effects if we include the affected regions in our sample (from Column (3) we return to the baseline sample of out-of-region firms only). In Column (1) Exposed follows our baseline median split of Exposure. In Column (2) we identify exposed banks not through their firm exposures but whether they reside in an affected region. In Column (3) we use a matched sample (for details see Table B.II). Column (4) reports the effects for our baseline setup without control variables. In Column (5) we further additionally use time-region fixed effects. In Column (6) we control for potential parallel trends according to Cortes and Strahan (2017). In Column (7) we include the year 2013 in the post period. Column (8) reports the effects for a collapsed data sample (Bertrand et al., 2004). Column (9) displays the results for a regression in which we use the changes of ln(Loans) between the pre and post period as the dependent variable (D,ln(Loans)) and use the continuous distribution of the damage as the main explanatory variable (here we also use the changes in the control variables instead). Exposed is a dummy variable indicating, whether a bank is exposed by the flood through its firms, i.e., whether it is above or below the median exposure of its firm damages. Post is a dummy equal to 0 for the years 2011-2012 and 1 for 2014-2015. Included control variables are: Size as the natural logarithm of total assets; Capital Adequacy as the ratio of equity to total assets; Liquidity as the share of cash on total assets; RoA as return on assets; Securities as the share of total securities over total assets. All bank controls in the first eight columns are lagged by one year (indicated by L.) or in first differences (indicated by D.) in the last column. The last two rows show the mean and standard deviation of the right-hand side variable, respectively. Clustered standard errors on the bank level of the point estimates are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE B.II: Matching performance on the bank level

Variable	Unmatched Matched	Mean		%Bias	%Reduction bias	t-test		V(T)/V(C)
		Treated	Control			t	p-Value	
Size	U	6.2179	6.3838	-12.7	-2.02	0.044	1.16	1.23
	M	6.2435	6.1973	3.5	72.1	0.49	0.624	
Capital Adequacy	U	0.07972	0.08332	-18.8	-2.95	0.003	0.96	1.08
	M	0.08018	0.08067	-2.6	86.3	-0.36	0.716	
Liquidity	U	0.01629	0.01651	-3.1	-0.48	0.633	0.80	0.99
	M	0.01623	0.01637	-1.9	38.7	-0.28	0.783	
RoA	U	0.00291	0.00269	8.4	1.37	0.172	1.65	1.49
	M	0.00286	0.00279	2.4	70.9	0.33	0.740	
Securities	U	0.29113	0.25742	29.3	4.62	0.000	1.00	0.84
	M	0.28477	0.29007	-4.6	84.3	-0.63	0.527	
Total	Pseudo R2	LR chi2	p-Value	Mean Bias	Median Bias	B	R	
Unmatched	0.028	38.77	0.000	14.4	12.7	39.9	1.02	
Matched	0.001	0.97	0.965	3.0	2.6	7.2	1.21	

This Table shows a comparison of the bank sample before and after matching. The dependent variable of the 1:1 (with a caliper of 0.01) propensity score matching is Exposed and we use Size, Capital Adequacy, Liquidity, RoA, and Securities as right-hand side variables. We match on observables in the years 2011 and 2012 and use the average of both years in the matching regression. The top panel reports for the unmatched and matched sample for each variable the mean for the exposed and unexposed group of banks, the bias between both and the reduction of the bias through matching in %, a t-test of the difference in means, and the variance ratio of exposed over unexposed which should equal 1 if there is perfect balance. Note, that the standardized % bias is the % difference of the sample means between exposed and unexposed banks as a percentage of the square root of the average of the sample variances in both groups. The bottom panel shows the overall statistics for both samples. The Pseudo R2 from the probit estimation and the corresponding p-values of the likelihood-ratio test of the joint insignificance of all the regressors (in predicting Exposed). Further, the mean and median bias as summary indicators of the distribution of the absolute bias and Rubin's B and R (the standardized difference of the means/variances of the propensity score between the exposed and unexposed sample).

B.II Robustness for firm-level results

This section provides additional robustness for the firm level regressions. Figure B.I shows the graphs that are discussed in the robustness section to the firm level results in the main body of the paper. In Subfigure B.I (a), we vary the threshold of affected counties. We consider a county as affected if the threshold is equal or higher than the respective threshold ranging from 1 to 8 in unitary increments. Our results hold for reasonable choices of the threshold around the values 4, 5, and 6. As for the bank-level robustness shown before, Subfigure B.I (b) shows simulations in which we redistribute firms' treatment status randomly. Only in 45 out of 1000 runs a significant coefficient at the 95%-level of confidence obtains, which supports the validity of our actual differentiation between treated and non-treated firms.

Table B.IV resembles the structure of the according bank-level matching shown in B.II and provides statistics for the matching procedure on the firm level. The matching renders the two groups, which were already very similar before matching, even more comparable. We are confident that our results do not hinge on matching since we use firm and time fixed effects and provide also evidence that pre-2013 development in all variable were not different between both groups of firms as shown in the bottom panel of Table 3 in the main body of the paper.

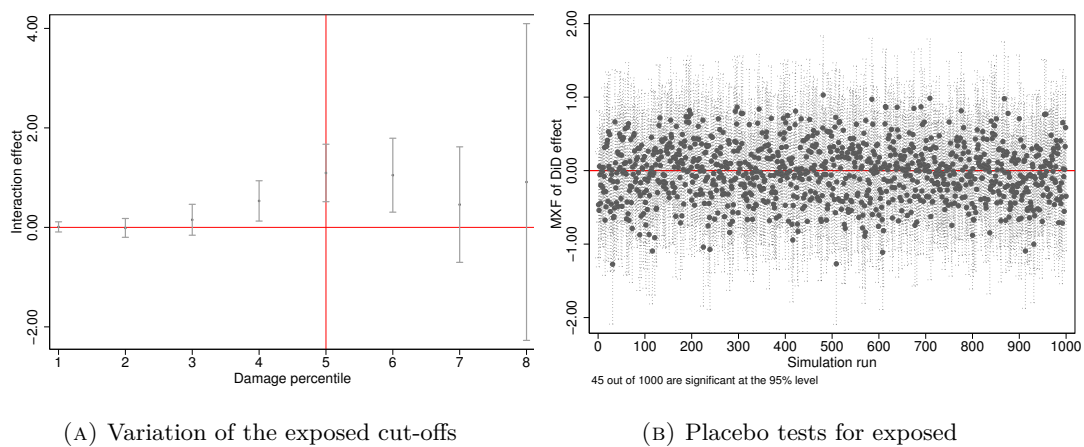


FIGURE B.I: Robustness of firm-level results

Subfigure (a) shows the difference-in-difference point estimates (Interaction effect) and 90% confidence intervals (y-axis) for regression in which we vary the threshold for whether a region is affected or not. We start with a threshold of 1 (here, all counties with a claim category larger than 1 are affected) and end by separating counties at a threshold of 8. Note that our baseline result is the estimate at a threshold at 5 (we use the sample from Column (3) of Table 6 from the paper). Subfigure (b) displays the interaction effect (and the 95% confidence band) for regressions on the y-axis. Here, Treated is randomly distributed among all firms based on the actual average likelihood that a firm was treated in 2013. We repeat this randomization for 1,000 runs.

TABLE B.III: Credit of affected firms

	(1)	(2)	(3)	(4)	(5)
		Only small and medium-sized		Sharp cut-off	Only small and medium-sized
	ln(Credit)				
Post	-1.1050*** (0.3280)	-1.0329*** (0.3339)	-0.9737*** (0.3421)	-1.0575*** (0.2631)	-1.1570*** (0.3652)
Treated×Post	1.1160*** (0.3562)	1.3093*** (0.4211)	1.1035** (0.4792)	0.8557*** (0.2791)	1.0550*** (0.3960)
Treated×PT		0.4220 (0.3611)			
L.Size ^f		0.9608*** (0.2293)	0.7774** (0.3370)	0.8450*** (0.1915)	0.9394*** (0.3025)
L.Liquidity Ratio		0.1012 (2.6071)	-1.7939 (3.0370)	-3.9308 (2.9409)	1.1946 (2.6520)
L.Solvency Ratio		-1.2818** (0.5133)	-0.4032 (0.7114)	-1.0792** (0.4320)	-1.4621** (0.6085)
L.Current Ratio		0.3136 (2.4399)	1.5879 (1.9846)	3.5871 (2.6685)	-0.2896 (2.5726)
Within R2	0.01	0.03	0.03	0.02	0.03
Observations	2674	2674	883	4526	2218
Firms	827	827	260	1374	673
Treated Firms	667	667	130	1114	546
Firm FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Mean	8.01	8.01	8.57	9.01	8.15
SD	5.97	5.97	6.08	5.77	5.94

This Table shows additional regression results on the firm level. All columns comprise firms in affected regions. In each column $\ln(\text{Credit})$ as the natural logarithm of firm credit is the dependent variable. Treated is a dummy variable indicating, whether a firm has a relation to an exposed bank according to our baseline setup (median split of Exposure). In each column we dismiss firms with a connection to a bank inside an affected region; we keep only single bank relationships, and small and medium-sized firms. In the first column we dismiss the firm controls. In Column (2) we check for parallel trend introducing a dummy variable PT that is one for 2012 (the pre-flood year). In Column (3) we use the matched sample according to Table B.IV. In Column (4) we use only firms with connections to banks that either are all exposed or not. In Column (5) we use only firms with a standing bank relationship between 2011 and 2014. Post is a dummy equal to 0 for the years 2011-2012 and 1 for 2014-2015. Included control variables are: Size^f as the natural logarithm of firms' total assets; Liquidity Ratio as the ratio of firms' cash and cash equivalents over short term liabilities; Solvency Ratio as the ratio of the sum of net profits (after tax) and depreciations over total liabilities; Current Ratio as the ratio of firms' current assets over current liabilities. All firm controls are lagged by one year (indicated by L.). We control for firm and year fixed effects. Clustered standard errors on the firm level of the point estimates are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE B.IV: Matching performance on the firm level

Variable	Unmatched Matched	Mean		%Bias	%Reduction bias	t-test		V(T)/V(C)
		Exposed	Unexposed			t	p-Value	
Size ^f	U	13.473	13.787	-21.1	-2.16	0.031	1.07	1.03
	M	14.035	13.755	18.8	10.9	1.53	0.126	
Liquidity Ratio	U	.04059	.05101	-10.0	-1.07	0.285	0.80	1.20
	M	.04727	.04856	-1.2	87.6	-0.09	0.925	
Solvency Ratio	U	.2651	.21605	13.7	1.40	0.161	1.07	1.01
	M	.19491	.22696	-8.9	34.7	-0.74	0.459	
Current Ratio	U	.04841	.05994	-10.2	-1.09	0.276	0.79	1.31
	M	.0587	.05738	1.2	88.5	0.09	0.931	
Total	Pseudo R2	LR chi2	p-Value	Mean Bias	Median Bias	B	R	
Unmatched	0.014	9.44	0.051	13.7	11.9	30.1	1.08	
Matched	0.010	3.55	0.470	7.5	5.1	23.3	1.07	

This Table shows a comparison of the bank sample before and after matching. The dependent variable of the 1:1 (with a caliper of 0.01) propensity score matching is Affected 2013 and we use Size, Capital Adequacy, Liquidity, RoA, and Securities as right-hand side variables. We match on observables in the years 2011 and 2012 and use the average of both years in the matching regression. The top panel reports for the unmatched and matched sample for each variable the mean for the control and treatment group, the bias between both and the reduction of the bias through matching in %, a t-test of the difference in means, and the variance ratio of treated over non-treated which should equal 1 if there is perfect balance. Note, that the standardized % bias is the % difference of the sample means between treated and non-treated banks as a percentage of the square root of the average of the sample variances in both groups. The bottom panel shows the overall statistics for both samples. The Pseudo R2 from the probit estimation and the corresponding p-values of the likelihood-ratio test of the joint insignificance of all the regressors (in predicting Affected 2013). Further, the mean and median bias as summary indicators of the distribution of the absolute bias and Rubin's B and R (the standardized difference of the means/variances of the propensity score between both samples).

B.III Flood aid in Germany

Private flood relief. About 32% of households are insured against flood damages in Germany and the GDV reports 2 billion Euros in insurance claims were paid after the 2013 flood (GDV, 2013). Insured damages were not subject to public flood aid. Private donations accounted for roughly 108 million Euros. It is not well documented how they were allocated, but most likely these funds were used in order to provide emergency relief in the days directly following or even during the flood.

Public flood relief. Public aid in Germany¹² went into effect very quickly after the flood of 2013. Flood aid was distributed in two major waves: immediate aid (roughly 900 million Euros) and reconstruction aid (5.5 billion Euros).¹³ The federal program for both types of aid was officially adopted by both houses on July 5th, with reconstruction aid coming into effect on July 19th. The vast majority of flood aid programs were funded 50% by the federal government and 50% by the German states; the federal government was financing the fund in full, though, to be repaid in half later. For all programs the principle applied that only current values were replaced, not the value of the new, non-depreciated assets. The distribution of the funds to the affected parties was administered by the states, with each one assigning different agencies with the task of reviewing applications and awarding funds (see Table B.V). Immediate aid was only funded by the German states and was available within days of the flood (the federal government provided matching funds after about one month). We provide detailed information about the final distribution of amounts paid in Table B.V.

Immediate aid was structured into six different tiers of different sizes. Numbers in parentheses are budget numbers. Of the immediate aid budgets only 212 million were used.¹⁴ Household aid (243 million Euros) was distributed to affected households in order to reduce the immediate effects of the flood, mainly to restore basic living conditions. Aid was distributed to businesses (418 million Euros) which could claim up to 50 % of any flood-related damage removal expenditures. A small labor market

¹²Information is based on (BMI, 2013a; BMI, 2013b)

¹³Note that the German government budgeted 8 billion Euros, the remaining amount was budgeted for federal infrastructure and other programs.

¹⁴Taken from the yearly report of the German finance ministry 2016, which was kindly provided to us by the German finance ministry.

program was set up to compensate employees for work outages (15 million Euros). Agricultural institutions were reimbursed for damages (up to 50 %). A liquidity and credit program by the German agricultural bank was constructed, offering cheaper credit and temporary repayment freezes. Communal infrastructure was reimbursed for damages (134 million Euros).

Reconstruction aid was split into similar tiers. Numbers in parentheses represent funds paid out.¹⁵ The major programs were reconstruction of households (427 million Euros), businesses (344 million Euros), agriculture (397 million Euros), local infrastructure (846 million) and regional infrastructure (339 million Euros). Business and household programs allowed claiming up to 80% of the damages caused; 100% in extreme cases only. Additional relief was channeled through the German development bank (KfW), which handed out 100 million Euros in loans at reduced rates. It also opened up credit applications for flooded businesses, treating them as “start-ups” with regard to credit rates and credit guarantees. Similar credit guarantee support was given by the Guarantee banks association, which partially or fully waived fees for affected parties.

¹⁵From the yearly report of the German finance ministry 2016.

TABLE B.V: Final administration and amounts paid out for each state to businesses

State	Administration done by	Amount as of 2015 (million Euros)	
		Immediate aid	Reconstruction aid
Baden-Württemberg	Ministry of Finance Baden-Württemberg		1.7
Bavaria	96 County administrations	32.4	108.1
Brandenburg	Investitionsbank des Landes Brandenburg	0.09	52.4
Hessen	County Administration		0.08
Lower Saxony	Nbank	0.78	0.3
Mecklenburg-Vorpommern	State Ministry of Economics, Employment and Health		0.15
Rhineland-Palatinate	State Ministry of Rhineland-Palatinate for Economic Affairs, Climate Protection Energy and Regional Planning		0.00
Saxony	Sächsische Aufbaubank	7.7	175.1
Saxony-Anhalt	Investitionsbank Sachsen-Anhalt	41.5	144.6
Schleswig-Holstein	Investitionsbank Schleswig-Holstein		1.6
Thuringia	Thüringer Aufbaubank	6.7	19.4

Note: Administrative offices were taken from (BMI, 2013a). Numbers stem from an official parliamentary request by the green party (Deutscher Bundestag, 2015). Numbers reported are predicted numbers of the total amount paid, based on flood aid applications received as of March 31, 2015. Numbers for Brandenburg are taken from an email request.

Chapter 4

Badly hurt? Natural disasters and direct firm effects

***Abstract:** We investigate firm outcomes after a major flood in Germany in 2013. We robustly find that firms located in the disaster regions have significantly higher turnover, lower leverage, and higher cash in the period after 2013. We provide evidence that the effects stem from firms that already experienced a similar major disaster in 2002. Overall, our results document a positive net effect on firm performance in the direct aftermath of a natural disaster.**

4.1 Introduction

The year 2017 has produced a number of tremendous natural disasters: Hurricanes Harvey, Irma and Maria, a triple-earthquake in Mexico, the deadliest wildfires in U.S. history, and one of the most destructive monsoon flooding in South Asia. Alongside the literature that investigates how protection against natural disaster can be enhanced and risks from such events can be shared (Temmerman et al., 2013; Jongman et al., 2014), existing studies in economics and finance have mainly focused on the effects of damages from natural disasters on the macro economy (Toya and Skidmore, 2007; Noy, 2009; Strobl, 2011; Cavallo et al., 2013; Fomby et al., 2013), banks' risk taking and lending (Klomp, 2014; Garmaise and Moskowitz, 2009; Berg and Schrader, 2012; Koetter et al., 2016; Chavaz, 2016; Cortes and Strahan, 2017; Schüwer et al., 2018), and insurance (Froot, 2001; Cummins et al., 2002; Niehaus, 2002; Jarzabkowski et al., 2015; Biener et al., 2017) and labor markets (Kirchberger, 2017). An important assumption in almost all studies is, that natural disasters are destructive for firms, so first order effects on firm performance should be negative. However, (Leiter et al.,

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2009) using European firm data demonstrate that there are positive effects on firms capital and labor input. We contribute to this literature by showing that a major flood in Germany in 2013, which caused damages of about 6-8 billion Euros (Koetter et al., 2016) had a significant positive effect on the performance of small and medium-sized enterprises (SMEs), which was not financed by increasing leverage or decreasing liquidity. We further document modest evidence that firms which already experienced a major disaster of the same type may have fared better after the disaster of 2013. There are several explanations for this positive (net) effect. While firms may cut back investment and lay off employees because working capital is destroyed and economic outlook is bad, governments and insurances may compensate the affected firms for some of the losses and thus counteract negative effects. Additionally, replacing old capital due to a disaster might lead to productivity improvements because it enables SMEs to modernize their capital stock.

4.2 Data and Methodology

We use two data sources for our analysis. First, we obtain firm level data from Bureau van Dijk's Amadeus database. After cleaning the data¹ we match affected and unaffected firms with propensity score matching.² This procedure leads to a final data set containing 217,742 observations for 48,524 firms between 2010 and 2015. Second, to measure the impact of the natural disaster we use data from the German Insurance Association. This is the same data as in Koetter et al. (2016) which provides regionally aggregated information about insurance claims for properties that were damaged by the flood of the Elbe river between May 25 and June 15, 2013. Figure 4.1 presents the

¹The full data set has roughly 8.9 million observations and 1.6 million firms. We drop all firms, for which we have no information about the location to which we can match to the flood data (reduces sample by 494,000 observations). We then drop all duplicate observations (27,000), all inactive firms (761,000; Table C.I provides a robustness including inactive firms), all observations with negative or missing total assets (2,564,000), and for which any balance sheet financial variables are negative (8,000). We also drop all very large firms (72,000), and then drop all years before 2010, so we have a balanced pre and post period (2,007,000). The last step is also done to ensure that the reported data is complete and accurate, as Amadeus for German firms becomes significantly more reliable after 2008, due to an increase in enforcement of existing reporting duties. We also exclude all firms that report two different locations during the period (50). Finally, we require all matching variables to be available in the pre-flood year (100,000) and the presence of outcome variables in the final data set (1,800,000). We also require each firm to have at least one observation in the pre and post period (196,000). Before matching, we are thus left with 800,000 observations for 185,210 firms.

²We provide details for the matching procedure in the Online Appendix and also provide statistics for the matching performance in Table C.II.

regional spread of the damage reported as the percentage of flood-insurance contracts activated during this period (left graph) and also shows the damages from another major flood from 2002 (right graph).

– Figure 4.1 about here –

We use the data to run our baseline analysis

$$Y_{irt} = \gamma_i + \gamma_t + \beta_1 (\text{Post } 2013_t \times \text{Affected } 2013_i) + \epsilon_{it}, \quad (4.1)$$

which is a difference-in-difference regression explaining firm i 's outcomes in year t (the firm resides in region r). We choose 4 dependent variables to test the effect of the flood on: firm performance (turnover), capital stock (total fixed assets), indebtedness (leverage) and liquidity (cash).³ The main explanatory variable is Affected 2013, which is a dummy separating affected firms from unaffected firms, based on their location in more or less heavily flooded regions. Post 2013 is a dummy equal to one in the years 2013–2015 (and zero before). Thereby, β_1 shows the differential effect on firm outcomes for firms residing in affected regions after the Elbe flooding relative to firms in unaffected regions prior to 2013. Figure 4.2 shows the separation of regions into affected and unaffected one (for both disaster periods). We further employ firm (γ_i) and year (γ_t) fixed effects to control for unobserved constant factors that may influence firm outcomes. In all our analyses, we use clustered standard errors on the firm level. We provide a detailed explanation of all variables in Table C.III and descriptive statistics in Tables C.IV and C.V in the Online Appendix.

– Figure 4.2 about here –

4.3 Results

Baseline results We provide our baseline results in Table 4.1. The first column investigates firms' turnover as a proxy for overall business activity (as in e.g., Acharya

³All four variables have been used in the economic literature as indicators of firm performance. Turnover is being used as an indicator for firm performance for example in Acharya et al. (2018) and Ongena et al. (2015). Investment proxies on the firm level have been investigated as firm outcomes for a long time (Gan, 2007). For example Alstadsæter et al., 2017 use the change in fixed assets as their investment proxy. Leverage is used as a dependent variable measuring the indebtedness of firms in many contexts (Garvey and Hanka, 1999; Degryse et al., 2011). Lastly, cash is a common measure of liquidity (Lins et al., 2010) and as such important in the aftermath of unexpected events (Huang, 2003), where one might expect a negative liquidity shock.

et al., 2018; Ongena et al., 2015). We find a positive and significant coefficient that indicates that firms affected by the Elbe flood of 2013 increase their turnover after 2013 by 1.4% compared to the group of unaffected firms relative to the period before 2013. Next we turn to firms' fixed assets as a proxy for relative post-flood investment (Alstadsæter et al., 2017).⁴ In Column (2) we find no significant effects due to the Elbe flood. Columns (3) shows that firms do not take out additional debt or reduce their liquidity holdings in response to the disaster; affected firms managed to reduce leverage by 0.2 percentage points and increase their cash holdings by 2.1% in the period after 2013. All our regressions deliver a good fit since we are able to explain between 77% and 96% of the variation of the dependent variables.⁵

Surprisingly, our results document mostly positive effects from the Elbe flood of 2013 on affected firms.⁶ We also provide descriptive evidence, that even in the year of the disaster, there is no indication of a negative effect on firms' outcomes (see the development over time in Figure C.I in the Online Appendix). We suggest two main explanations for our results. First, disasters provide an opportunity for firms to invest in newer (and potentially more productive) capital, which may not have been profitable before, due to opportunity cost of holding on to the old capital. This capital upgrading channel has been documented in the literature, particularly for highly developed countries (Crespo Cuaresma et al., 2008; Toya and Skidmore, 2007; Leiter et al., 2009). Our results indicate however, that quantitatively the capital stock remains unchanged. Nevertheless, more productive capital could be driving the positive effect in business activity. Insurance payments and government aid, the latter of which in Germany paid for 80% of current value destroyed, would provide at least some funds for capital upgrading / capital replacement. This may be a potential explanation of why we do not see firms decreasing their liquidity or increasing their debt to finance recovery. Similarly, banks might be inclined to fund re-investments, as they intend to keep their lender relationship or because they have collected private information during the pre-disaster period. The second potential explanation is a rise

⁴Note that the difference-in-difference effect demonstrates the change of the capital stock from the pre-disaster to the post-disaster period. As such it is a (relative) change in stock

⁵This comes mostly from the firm and time fixed effects. The difference-in-difference dummies explain around 1% to 3% only as shown in Table C.IX in the Online Appendix.

⁶In Table C.XIV we show that this translates to a decrease in the regional unemployment and debt rates.

in aggregate regional demand, which is strongly supported by the increase in turnover we find in our results. Cleanly disentangling these explanations and channels is difficult due to the lack of data. Nevertheless, the net effect appears to be positive instead of negative as frequently assumed in the scientific literature and conventional wisdom might dictate.

– Table 4.1 about here –

Our results are robust to several variations for which we provide results in the Online Appendix. There, Table C.V and Figure C.I provide evidence that the parallel trend assumption for both groups are valid. We additionally provide structural tests of this assumption using placebo regressions in Table C.VI and Table C.VII. Also, Table C.VIII shows that our results remain intact when we collapse the sample on the time dimension, taking care of potential auto correlation that may bias results (Bertrand et al., 2004). In Table C.IX we show that our results stay robust when we exclude any fixed effects, or use region fixed effects (Table C.X) in our regressions. The results are even stronger when using an unmatched sample of firms (Table C.XI) and also survive when we use the matching variables as controls (except for cash holdings, Table C.XII).

The flood of 2002 We make use of the information whether a firm was already affected by a major flood in 2002 (see the right graph of Figure 4.1 and 4.2).⁷ To analyze whether this affects firm outcomes after the 2013 flood, we augment Equation (4.1) by interacting all dummy variables with Affected 2002, which is a dummy variable equal to one if a firm existed in 2002 and resided in a region that was already hit by the flood of 2002. Our particular interest is on the coefficient for the interaction term $\text{Post 2013} \times \text{Affected 2013} \times \text{Affected 2002}$, which shows the differential effect for firms affected by both floods for the period starting in 2013 relative to firms only affected by the flood of 2013 and the period before 2013.

– Table 4.2 about here –

The columns of Table 4.2 show three things. First, our baseline results are much weaker for firms that did not experience the flood of 2002 as indicated by the double

⁷Table C.XIII in the Online Appendix show that our main results remain intact when we use only firms that already existed in 2002 in our baseline regression.

interaction term $\text{Post 2013} \times \text{Affected 2013}$. Second, for the sample of firms that were also affected by the Elbe flood of 2002, the Partial Effect 2013 of Table 4.2 shows much stronger (and significant) effects which shows that our main results are driven by firms affected by both floods. Third, however, the triple interaction effect $\text{Post 2013} \times \text{Affected 2013} \times \text{Affected 2002}$ indicates that the differences between both samples are not statistically significant. Thereby, we have only weak evidence for the fact that firms involved in a prior disaster adjusted their behavior for the next disaster. This implies that at firms either did not learn from the prior disaster or that they were content with the long-run outcome of their reactions and thus did not adjust their behavior.

4.4 Conclusion

It is a difficult empirical exercise to disentangle the different channels that affect firm outcomes after a natural disaster. We document the absence of a negative effect of natural disasters on firms and highlight that there is likely a multitude of factors, that may lead to a positive net effect for firms in the aftermath of huge disasters. Our results also indicate that learning effects play a minor role – if any – in the management of natural disasters on the firm level. On the positive side, our results indicate that the support after natural disasters seems to work – at least in the case of Germany – as affected firms fared comparably well.

Tables and Figures

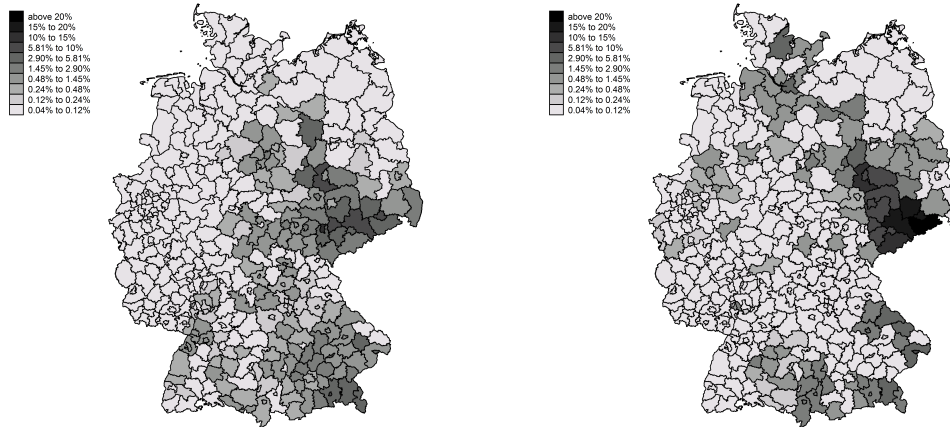


FIGURE 4.1: Distribution of flood damages in Germany 2013 and 2002

This figure shows the distribution of flood damages according to the German Association of Insurers (GDV). The damages are shown as the percentage of insurance contracts activated during the flooding period, according to the legend. The left-hand side displays the flood related damage distribution of the 2013 flood, while the right-hand side displays the damage distribution for the 2002 flood.

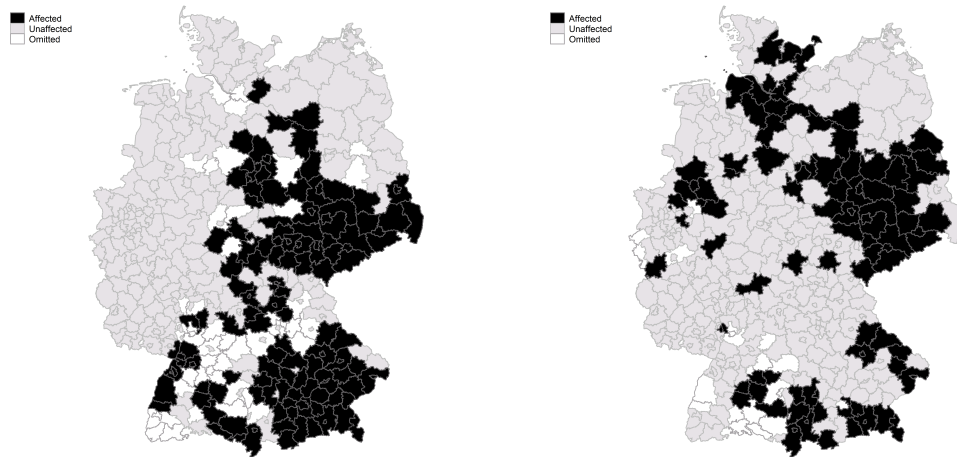


FIGURE 4.2: Distribution of flood damages in Germany 2013 and 2002

This figure shows the distribution of affected and unaffected regions. Regions which are designated as category 4 or higher in the insurance data (c.f. Figure 4.1) are classified as affected. Regions in category 1 are designated as unaffected and regions of categories 2 and 3 are omitted as a buffer category. The left-hand side displays the categorization for the 2013 flood and the right-hand side for the 2002 flood.

TABLE 4.1: Baseline regressions

	(1)	(2)	(3)	(4)
	log(turnover)	log(tangible fixed assets)	leverage ratio	log(cash)
Post 2013 × Affected 2013	0.014*** (0.003)	-0.004 (0.009)	-0.004*** (0.001)	0.021* (0.012)
N	217,742	217,742	217,742	217,742
Number of Firms	48,524	48,524	48,524	48,524
Treatment Group	24,262	24,262	24,262	24,262
Adjusted R ²	0.958	0.915	0.871	0.772
Firm Fixed Effects	YES	YES	YES	YES
Time Fixed Effects	YES	YES	YES	YES

This table presents the results of the direct effects of flooding on firms for several different outcomes: Turnover, tangible fixed assets, leverage and cash. Affected 2013 is a dummy variable based on the firms location with regard to the flood (c.f Figure 4.1). It is set equal to 1 if the firm is located in a county with a damage category of 4 or higher and set equal to 0 if it is in an unaffected county. Firms in counties with damage categories 2 and 3 are omitted from the analysis. The regression is based on a matched sample using 2012 values of the following variables: Cash/TA, Long term debt/TA, log(total assets), regional unemployment rate, regional GDP per capita, regional insolvency applications per capita, and regional public debt per capita. All regressions include firm and year fixed effects. Clustered standard errors on the firm level of the point estimates are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE 4.2: Firms affected by two floods within 12 years

	(1)	(2)	(3)	(4)
	log(turnover)	log(tangible fixed assets)	leverage ratio	log(cash)
Post 2013×Affected 2013	0.008* (0.004)	0.001 (0.012)	-0.003* (0.002)	0.006 (0.016)
Post 2013×Affected 2002	0.001 (0.006)	0.004 (0.018)	-0.000 (0.002)	-0.009 (0.025)
Post 2013×Affected 2013×Affected 2002	0.009 (0.008)	-0.004 (0.022)	-0.002 (0.003)	0.047 (0.030)
N	199,375	199,375	199,375	199,375
Number of Firms	44,470	44,470	44,470	44,470
Treatment Group2013	21,850	21,850	21,850	21,850
Treatment Group2002	14,613	14,613	14,613	14,613
Triple Interaction	11,105	11,105	11,105	11,105
Adjusted R ²	0.959	0.915	0.873	0.771
Partial Effect 2013	0.017	-0.003	-0.005	0.053
Partial Effect p-value	0.016	0.862	0.066	0.043
Firm Fixed Effects	YES	YES	YES	YES
Time Fixed Effects	YES	YES	YES	YES

This table presents the results of the direct effects of being flooded twice within 12 years for several firm-level outcomes: Turnover, tangible fixed assets, leverage and cash. Affected 2013 is a dummy variable based on the firms location with regard to the 2013 flood. Affected 2002 is a dummy variable based on the firms location with regard to the 2002 flood (c.f Figure 4.1). Both variables are set equal to 1 if the firm is located in a county with a damage category of 4 or higher for the respective flood and set equal to 0 if it is in an unaffected county. Firms in counties with damage categories 2 and 3 are omitted from the analysis. The regression is based on a matched sample using 2012 values of the following variables: Cash/TA, Long term debt/TA, log(total assets), regional unemployment rate, regional GDP per capita, regional insolvency applications per capita, and regional public debt per capita. All regressions include firm and year fixed effects. Clustered standard errors on the firm level of the point estimates are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Appendix C

TABLE C.I: Baseline regression with inactive firms

	(1)	(2)	(3)	(4)
	log(turnover)	log(total fixed assets)	leverage ratio	log(cash)
Post×Affected2013	0.013*** (0.003)	-0.001 (0.009)	-0.003** (0.001)	0.024** (0.012)
N	224,367	224,367	224,367	224,367
Number of Firms	50,362	50,362	50,362	50,362
Treatment Group	25,181	25,181	25,181	25,181
AdjustedR ²	0.958	0.911	0.871	0.769
Firm Fixed Effects	YES	YES	YES	YES
Time Fixed Effects	YES	YES	YES	YES

This table presents the results of the direct effects of flooding on firms for several different outcomes: Turnover, tangible fixed assets, leverage and cash. This table includes all firms in the dataset (as opposed to only active firms). Affected 2013 is a dummy variable based on the firms location with regard to the flood (c.f Figure 4.1). It is set equal to 1 if the firm is located in a county with a damage category of 4 or higher and set equal to 0 if it is in an unaffected county. Firms in counties with damage categories 2 and 3 are omitted from the analysis. The regression is based on a matched sample using 2012 values of the following variables: Cash/TA, Long term debt/TA, log(total assets), regional unemployment rate, regional GDP per capita, regional insolvency applications per capita, and regional public debt per capita. All regressions include firm and year fixed effects. Clustered standard errors on the firm level of the point estimates are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Matching procedure

All regressions are based on a matched sample of firms, in order to improve the compatibility of affected and unaffected firms. We employ propensity score matching that estimates the probability of being selected into the treatment group ($p(x)$), based on observable characteristics (x).

$$p(x) = \Pr(\text{Affected 2013} = 1 \mid X = x) \quad (\text{C.I.I})$$

X are firm level and regional variables: Cash (share of TA), long term debt (share of TA) and size; Unemployment (%), GDP per capita, insolvencies per capita and public debt per capita. Variable Definitions can be found in Table C.III. We use values of these variables before the flood in the year 2012, as to ensure that the matching parameters are not themselves affected by the flood. After estimating the propensity score, we find exactly one match (without replacement) for firms in the treatment group. We additionally employ a caliper band of 0.01, meaning that no firms propensity score match can be further away than 0.01. Using this method, we

identify a comparable control groups (in terms of their observables) for 24,262 firms. We then use only these firms and their controls in all further regressions (for which we use the full sample years). Table C.II displays the reduction in sample bias due to matching. We achieve a reduction in difference of observables for all matching variables over 80% with the exception of GDP per capita. Furthermore, matching reduces all previously statistically significant differences between control and treatment groups for the firm level variables. The difference in regional variables is however more difficult to overcome. Due to the limited number of flooded regions, the variation is not large enough to remove all significant differences in these variables between the treatment and control group. Note however, that as long as the Parallel trend assumptions holds, level differences between treatment and control group do matter for difference-in-difference estimation. We confirm that trends are parallel both visually (Figure C.I) and by analyzing the pre-flood differences over time (Table C.V). Both tests confirm that the parallel trends assumption holds in our setting.

TABLE C.II: Matching performance: Before and after sample comparison

Variable	Match	Mean		% Bias	% Bias reduction	t-statistic	p-value
		Treated	Control				
Cash (share of TA)	U	0.172	0.164	4.5		8.01	0.000
	M	0.172	0.171	0.5	89.4	0.52	0.605
Long term debt (share of TA)	U	0.312	0.317	-1.7		-2.97	0.003
	M	0.317	0.316	0.2	86.1	0.26	0.798
Size	U	13.799	13.767	2.1		3.79	0.000
	M	13.783	13.788	-0.3	84.6	-0.36	0.715
Unemployment (%)	U	5.7366	7.1353	-46.3		-84.32	0.000
	M	6.487	6.5699	-2.8	94.0	-2.95	0.003
GDP per capita	U	36,005	34,457	9.2		17.04	0.000
	M	31,463	32,539	-6.4	30.5	-8.63	0.000
Insolvencies per capita	U	0.0015	0.0020	-97.6		-174	0.000
	M	0.0018	0.0018	6.2	93.6	6.78	0.000
Public debt per capita	U	1,026	2,225	-103.6		-169.2	0.000
	M	1,320	1392	-6.3	93.9	-8.87	0.000

This table presents the outcome of the 1:1 propensity score matching between firms affected and unaffected by the flood used to create the sample for estimation. It displays the means of the matching variables for the sample of unmatched (U) and matched (M) firms, for the affected and unaffected groups. Furthermore it shows the reduction in bias and provides difference in means tests (ttest) for both samples.

Additional Figures and Tables

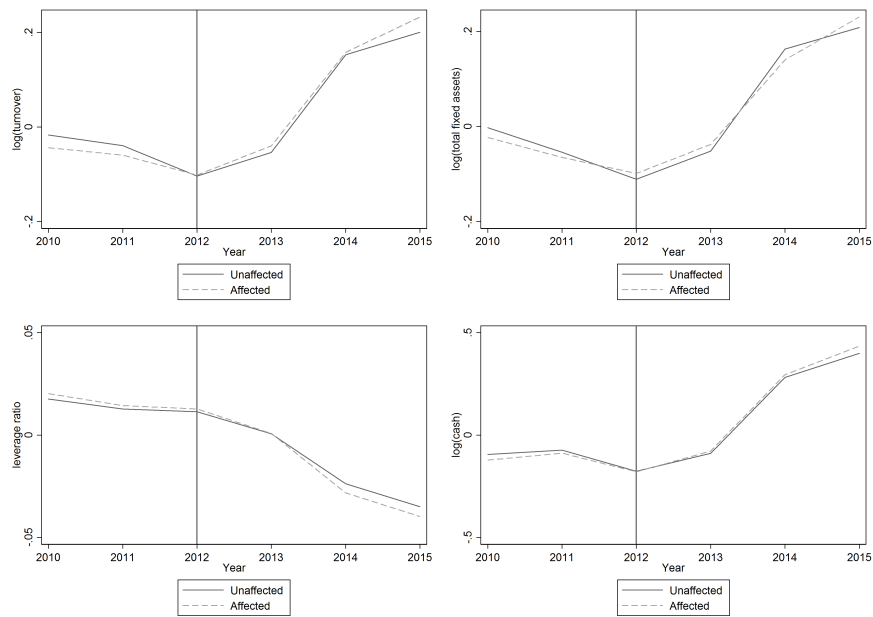


FIGURE C.I: Distribution of flood damages in Germany 2013 and 2002

This figure shows the trend of unaffected and affected firms for all four dependent variables over the sample period (2010-2015). All trends have been demeaned, i.e. the overall mean for any given year has been subtracted.

TABLE C.III: Variable definitions

Variable Name	Definition	Amadeus Code
Identification Variables		
Post 2013	Dummy variable set to 0 for 2010-2012 and set to 1 for 2013-2015.	
Affected 2013	Dummy variable set to 0 if no flood insurance claims were made during the 2013 flood event in the firms region and set to 1 if the firm is located in a county with flood damage category 4 or larger.	
Affected 2002	Dummy variable set to 0 if no flood insurance claims were made during the 2002 flood event in the firms region and set to 1 if the firm is located in a county with flood damage category 4 or larger.	
Dependent Variables		
Turnover (tsd EUR)	Operating Revenue (Turnover) of firms. Used in logs in the regression.	OPRE
Tangible fixed assets (tsd EUR)	Tangible Fixed Assets of firms. Used as logs in the regression.	TFAS
Leverage ratio	Total liabilities (non-current + current) divided by total assets.	(CULI+NCLI) / TOAS
Cash (tsd EUR)	Cash and cash equivalent of firms. Used as logs in the regression.	CASH
Matching Variables		
Size	log(total assets)	ln(TOAS)
Long term debt (share of TA)	Long term debt as the share of firms total assets.	LTDB / TOAS
Cash(share of TA)	Cash and cash equivalent as the share of total assets	CASH / TOAS
Insolvencies per capita	Regional insolvency applications / number of inhabitants in the region. Derived from official statistics.	
Unemployment (%)	Regional unemployment rate in %.	
Public debt per capita	Public debt of the local governments (counties and cities) / by the number of inhabitants in the region	
GDP per capita	Regional GDP / number of inhabitants in the region	

This table provides the definitions of the variables used in the regression and the matching procedure.

TABLE C.IV: Descriptives statistics

	N	Mean	SD	Min	Max
Affected Variables					
Affected 2013	217742	0.50	0.50	0.00	1.00
Affected 2002	199375	0.33	0.47	0.00	1.00
Dependent Variables					
Turnover (tsd EUR)	217742	5094	10754	58.18	69149
Tangible fixed assets (tsd EUR)	217742	1403	4917	0.00	36237
Leverage ratio	217742	0.65	0.27	0.07	1.00
Cash (tsd EUR)	217742	411.5	1053	0.07	7701
Matching Variables					
Size	217742	13.88	1.45	10.64	18.05
Long term debt (share of TA)	217742	0.31	0.30	0.00	0.99
Cash (share of TA)	217742	0.18	0.20	0.00	0.80
Insolvencies per capita	215041	1.73	0.57	0.73	3.60
Unemployment (%)	215041	6.50	3.00	2.20	13.90
Public debt per capita	216591	1332	889.00	45.06	6504
GDP per capita	193753	31998	13836	17431	96641

This table presents summary statistics for all variables used in the regressions and matching process. Affected 2013 is a dummy variable based on the firms location with regard to the 2013 flood. Affected 2002 is a dummy variable based on the firms location with regard to the 2002 flood. Both dummies are set equal to 1 if the firm is located in a county with a damage category of 4 or higher and set equal to 0 if it is in an unaffected county in the respective flood years. Dependent variables are displayed in levels in this table, but used in natural logs in the regressions. Matching variables are used for the pre-estimation, 1:1 propensity score matching. Matching is based on pre-flood (2012) characteristics.

TABLE C.V: Pre-2013 statistics for affected and unaffected firms

	Affected		Unaffected		ND
	Mean	SD	Mean	SD	
Dependent Variables					
Δ Revenue (tsd EUR)	0.184	5.625	0.211	6.863	-0.00
Δ Tangible fixed assets (tsd EUR)	27.17	2267	16.043	1281	0.00
Δ Leverage ratio	0.004	0.334	0.002	0.304	0.01
Δ Cash (tsd EUR)	9.333	303.8	8.260	123.1	0.00
Matching Variables					
Δ Size	0.005	0.023	0.005	0.023	0.01
Δ Long term debt (share of TA)	24.246	2830	9.687	1098	0.00
Δ Cash (share of TA)	7.309	125	6.879	98.77	0.00
Δ GDP per capita	0.032	0.036	0.030	0.032	0.05
Δ Unemployment (%)	-0.076	0.064	-0.049	0.064	-0.30
Δ Public debt per capita	0.008	0.082	0.020	0.110	-0.09
Δ Insolvencies per capita	-0.052	0.118	-0.048	0.131	-0.02

This table presents average changes (Δ , in percent) for the period 2010-2012 for the all variables used in our analyses for affected and unaffected banks. Detailed definitions of the variables are provided in Table C.IV. We provide normalized differences in the last column. A value for normalized differences larger than |0.25| indicates that averages between both groups of banks are significant.

TABLE C.VI: Placebo regression

	(1)	(2)	(3)	(4)
	log(turnover)	log(tangible fixed assets)	leverage ratio	log(cash)
Post 2013 × Affected 2013	0.005 (0.004)	0.011 (0.011)	-0.000 (0.001)	-0.016 (0.016)
N	109,623	109,623	109,623	109,623
Number of Firms	41,350	41,350	41,350	41,350
Treatment Group	24,262	24,262	24,262	24,262
Adjusted R ²	0.968	0.937	0.902	0.795
Firm Fixed Effects	YES	YES	YES	YES
Time Fixed Effects	YES	YES	YES	YES

This table presents the results of a placebo regression, using the year 2011 as the treatment year and excluding the years 2013-2015. Accordingly, post is a dummy set equal to 0 for the year 2010 and set equal to 1 for the years 2011 and 2012. Affected 2013 is a dummy variable based on the firms location with regard to the flood. It is set equal to 1 if the firm is located in a county with a damage category of 4 or higher and set equal to 0 if it is in an unaffected county. Firms in counties with damage categories 2 and 3 are omitted from the analysis. The regression is based on a matched sample using 2012 values of the following variables: Cash/TA, long term debt/TA, size, regional unemployment rate, regional GDP per capita, regional insolvency applications per capita, and regional public debt per capita. All regressions include firm fixed effects. Clustered standard errors on the firm level of the point estimates are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE C.VII: Second placebo regression

	(1)	(2)	(3)	(4)
	log(turnover)	log(total fixed assets)	leverage ratio	log(cash)
AffectedAfter	0.016*** (0.004)	-0.003 (0.011)	-0.003** (0.001)	0.017 (0.014)
AffectedBefore	0.005 (0.003)	0.004 (0.009)	0.001 (0.001)	-0.007 (0.014)
N	217,742	217,742	217,742	217,742
Number of Firms	48,524	48,524	48,524	48,524
Treatment Group	24,262	24,262	24,262	24,262
Adjusted R ²	0.958	0.915	0.871	0.772
Firm Fixed Effects	YES	YES	YES	YES
Time Fixed Effects	YES	YES	YES	YES

This table presents the results of a regression of being affected by the disaster before and being affected by the disaster after the disaster occurs. AffectedAfter is a dummy variable that is set equal to one if the firm is located in an affected area after 2013. AffectedBefore is a second dummy variable set equal to one if the firm is affected by the disaster, but in the year 2012. The regression is based on a matched sample using 2012 values of the following variables: Cash/TA, long term debt/TA, size, regional unemployment rate, regional GDP per capita, regional insolvency applications per capita, and regional public debt per capita. All regressions include firm fixed effects. Clustered standard errors on the firm level of the point estimates are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE C.VIII: Robustness: Collapsed sample

	(1)	(2)	(3)	(4)
	log(turnover)	log(tangible fixed assets)	leverage ratio	log(cash)
Post 2013	0.084*** (0.002)	0.104*** (0.007)	-0.025*** (0.001)	0.172*** (0.009)
Post 2013×Affected 2013	0.014*** (0.003)	-0.002 (0.009)	-0.004*** (0.001)	0.023* (0.012)
Constant	14.299*** (0.001)	11.481*** (0.002)	0.672*** (0.000)	10.822*** (0.003)
N	97,048	97,048	97,048	97,048
Number of Firms	48,524	48,524	48,524	48,524
Treatment Group	24,262	24,262	24,262	24,262
Adjusted R ²	0.055	0.010	0.045	0.019
Firm Fixed Effects	YES	YES	YES	YES
Time Fixed Effects	NO	NO	NO	NO

This table provides the results of OLS regressions of the baseline on a collapsed sample, in order to address concerns that the results are driven by autocorrelation. The sample is collapsed into the pre-flood and post-flood period for this regression. Affected 2013 is a dummy variable based on the firms location with regard to the flood. It is set equal to 1 if the firm is located in a county with a damage category of 4 or higher and set equal to 0 if it is in an unaffected county. Firms in counties with damage categories 2 and 3 are omitted from the analysis. The regression is based on a matched sample using 2012 values of the following variables: Cash/TA, long term debt/TA, size, regional unemployment rate, regional GDP per capita, regional insolvency applications per capita, and regional public debt per capita. All regressions include firm fixed effects. Clustered standard errors on the firm level of the point estimates are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE C.IX: Effect of flooding on firm performance: No fixed effects

	(1)	(2)	(3)	(4)
	log(turnover)	log(tangible fixed assets)	leverage ratio	log(cash)
Post 2013	0.085*** (0.002)	0.105*** (0.007)	-0.026*** (0.001)	0.191*** (0.008)
Affected 2013	-0.094*** (0.012)	0.027 (0.023)	-0.004* (0.002)	0.028 (0.020)
Post 2013×Affected 2013	0.015*** (0.003)	-0.003 (0.009)	-0.004*** (0.001)	0.022* (0.012)
Constant	14.350*** (0.009)	11.476*** (0.017)	0.674*** (0.002)	10.814*** (0.014)
N	217,742	217,742	217,742	217,742
Number of Firms	48,524	48,524	48,524	48,524
Treatment Group	24,262	24,262	24,262	24,262
R ²	0.004	0.001	0.003	0.003
Firm Fixed Effects	NO	NO	NO	NO
Time Fixed Effects	NO	NO	NO	NO

This table presents the results of a difference-in-difference estimation of the direct effects of the 2013 flooding on firms for several different outcomes, without including fixed effects. Affected 2013 is a dummy variable based on the firms location with regard to the flood (c.f Figure 4.1). It is set equal to 1 if the firm is located in a county with a damage category of 4 or higher and set equal to 0 if it is in an unaffected county. Firms in counties with damage categories 2 and 3 are omitted from the analysis. Post is a dummy set equal to 0 for the pre-flood years (2010-2012) and set equal to 1 for the post-flood years (2013-2015). The regression is based on a matched sample using 2012 values of the following variables: Cash/TA, long term debt/TA, size, regional unemployment rate, regional GDP per capita, regional insolvency applications per capita, and regional public debt per capita. Clustered standard errors on the firm level of the point estimates are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE C.X: Effect of flooding on firm performance: County fixed effects

	(1)	(2)	(3)	(4)
	log(turnover)	log(total fixed assets)	leverage ratio	log(cash)
Post × Affected2013	0.028*** (0.006)	0.010 (0.014)	-0.004*** (0.002)	0.038*** (0.014)
N	217,742	217,742	217,742	217,742
Number of Firms	48,524	48,524	48,524	48,524
Treatment Group	24,262	24,262	24,262	24,262
AdjustedR ²	0.045	0.046	0.026	0.025
County Fixed Effects	YES	YES	YES	YES
Time Fixed Effects	YES	YES	YES	YES

This table presents the results of a difference-in-difference estimation of the direct effects of the 2013 flooding on firms for several different outcomes, including county (German "Kreise") fixed effects. Affected 2013 is a dummy variable based on the firms location with regard to the flood (c.f Figure 4.1). It is set equal to 1 if the firm is located in a county with a damage category of 4 or higher and set equal to 0 if it is in an unaffected county. Firms in counties with damage categories 2 and 3 are omitted from the analysis. Post is a dummy set equal to 0 for the pre-flood years (2010-2012) and set equal to 1 for the post-flood years (2013-2015). The regression is based on a matched sample using 2012 values of the following variables: Cash/TA, long term debt/TA, size, regional unemployment rate, regional GDP per capita, regional insolvency applications per capita, and regional public debt per capita. Clustered standard errors on the firm level of the point estimates are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE C.XI: Effect of flooding on firm performance: Unmatched dataset

	(1)	(2)	(3)	(4)
	log(turnover)	log(tangible fixed assets)	leverage ratio	log(cash)
Post 2013 × Affected 2013	0.027*** (0.002)	0.032*** (0.006)	-0.005*** (0.001)	0.052*** (0.007)
N	662,744	662,744	662,744	662,744
Number of Firms	151,943	151,943	151,943	151,943
Treatment Group	54,609	54,609	54,609	54,609
AdjustedR ²	0.954	0.907	0.866	0.766
Firm Fixed Effects	YES	YES	YES	YES
Time Fixed Effects	YES	YES	YES	YES

This table presents the results of the baseline regression, for an unmatched sample of firms. Affected 2013 is a dummy variable based on the firms location with regard to the flood. It is set equal to 1 if the firm is located in a county with a damage category of 4 or higher and set equal to 0 if it is in an unaffected county. Firms in counties with damage categories 2 and 3 are omitted from the analysis. Post is a dummy set equal to 0 for the pre-flood years (2010-2012) and set equal to 1 for the post-flood years (2013-2015). All regressions include firm and year fixed effects. Clustered standard errors on the firm level of the point estimates are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE C.XII: Effect of flooding on firm performance: Matching variables as controls

	(1)	(2)	(3)	(4)
	log(turnover)	log(tangible fixed assets)	leverage ratio	log(cash)
Post 2013×Affected 2013	0.009*** (0.003)	-0.005 (0.009)	-0.002* (0.001)	0.007 (0.010)
Cash (share of TA)	0.048*** (0.011)	-0.855*** (0.033)	-0.148*** (0.004)	6.788*** (0.040)
Long term debt (share of TA)	-0.044*** (0.005)	0.124*** (0.014)	0.125*** (0.002)	-0.032** (0.015)
Size	0.323*** (0.007)	0.887*** (0.020)	0.058*** (0.002)	0.821*** (0.013)
GDP per capita	0.000** (0.000)	-0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Unemployment (%)	-0.003 (0.002)	-0.006 (0.007)	-0.000 (0.001)	0.005 (0.007)
Public debt per capita	-0.000*** (0.000)	-0.000 (0.000)	0.000** (0.000)	0.000 (0.000)
Insolvencies per capita	5.887 (4.392)	10.477 (12.164)	-1.435 (1.449)	6.425 (14.419)
N	190,958	190,958	190,958	190,958
Number of Firms	48,430	48,430	48,430	48,430
Treatment Group	24,262	24,262	24,262	24,262
AdjustedR ²	0.965	0.929	0.895	0.855
Firm Fixed Effects	YES	YES	YES	YES
Time Fixed Effects	YES	YES	YES	YES

This table presents the results of the direct effects of flooding on firms affected by the flood on several different outcomes. The regression includes the matching variables as control variables. Affected 2013 is a dummy variable based on the firms location with regard to the flood (c.f Figure 4.1). It is set equal to 1 if the firm is located in a county with a damage category of 4 or higher and set equal to 0 if it is in an unaffected county. Firms in counties with damage categories 2 and 3 are omitted from the analysis. The regression is based on a matched sample using 2012 values of the following variables: Cash/TA, long term debt/TA, ln(Total assets), regional unemployment rate, regional GDP per capita, regional insolvency applications per capita, and regional public debt per capita. All matching variables are used as control variables in this regression. All regressions include firm and year fixed effects. Clustered standard errors on the firm level of the point estimates are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE C.XIII: Baseline effect of flooding on firm performance: Non-missing 2002 flood firms

	(1)	(2)	(3)	(4)
	log(turnover)	log(tangible fixed assets)	leverage ratio	log(cash)
Post 2013×Affected 2013	0.013*** (0.003)	0.000 (0.010)	-0.004*** (0.001)	0.026** (0.012)
N	199,375	199,375	199,375	199,375
Number of Firms	44,470	44,470	44,470	44,470
Treatment Group	21,850	21,850	21,850	21,850
AdjustedR ²	0.959	0.915	0.873	0.771
Firm Fixed Effects	YES	YES	YES	YES
Time Fixed Effects	YES	YES	YES	YES

This table presents the results of the baseline regression, for the sample used in the double flood regression, i.e. excluding firms which are excluded due to the definition of the Affected 2002 variable. Affected 2013 is a dummy variable based on the firms location with regard to the flood. It is set equal to 1 if the firm is located in a county with a damage category of 4 or higher and set equal to 0 if it is in an unaffected county. Firms in counties with damage categories 2 and 3 are omitted from the analysis. Post is a dummy set equal to 0 for the pre-flood years (2010-2012) and set equal to 1 for the post-flood years (2013-2015). The regression is based on a matched sample using 2012 values of the following variables: Cash/TA, long term debt/TA, size, regional unemployment rate, regional GDP per capita, regional insolvency applications per capita, and regional public debt per capita. All regressions include firm and year fixed effects. Clustered standard errors on the firm level of the point estimates are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE C.XIV: Effect of the flood on the regional economy

	(1)	(2)	(3)	(4)
	Unemployment (%)	GDP per capita	Debt per capita	Insolvencies
Post × Affected	-0.332*** (0.070)	-106.885 (133.819)	-169.151*** (31.643)	0.025 (0.027)
N	2,000	1,678	2,005	2,000
Number of Counties	336	336	336	336
Treatment Group	126	126	126	126
AdjustedR ²	0.981	0.994	0.968	0.888
County Fixed Effects	YES	YES	YES	YES
Time Fixed Effects	YES	YES	YES	YES

This table presents the results of the direct effects of flooding on 4 indicators of the overall regional economy. Unemployment indicates the regional unemployment rate in %. GDP per capita is regional per capita GDP in Euro. Debt per capita is regional public debt per capita in Euro. Insolvencies in the number of businesses declaring insolvencies per 1000 inhabitants. Affected is a dummy variable set equal to 1 if the county is assigned a damage category of 4 or higher and set equal to 0 if it is assigned the lowest category (1). Counties with damage categories 2 and 3 are omitted from the analysis. All regressions include county and year fixed effects. Clustered standard errors on the county level of the point estimates are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Chapter 5

Nowhere else to go: The determinants of bank-firm relationship discontinuations after bank mergers

***Abstract:** The decision to change or terminate a bank-firm relationship has been demonstrated to be crucial for firm performance following bank mergers. We find both competition and the available firm collateral to be important factors in enabling firms to switching banks, instead of dropping their bank relationships. We also provide novel evidence that firms who are able to add a bank relationship following a merger exhibit significantly better post-merger performance. Our findings are consistent with the interpretation that bank-mergers cause a reduction in lending to most firms, leading them to search for alternative sources of finance.*

5.1 Introduction

Bank mergers constitute a significant event in the bank-firm relationship. As such, many studies have demonstrated that a bank merger can have negative effects on firms' access to credit (Berger et al., 1998) and as result on their real outcomes in terms of investment, returns and productivity (Di Patti and Gobbi, 2007; Degryse et al., 2011; Fraisse et al., 2018). These effects are especially relevant where consolidation is significantly increasing and where non-financial firms are particularly dependent on bank lending. Given the recent consolidation trends in most European banking sectors, the expectations on new merger waves and the reliability of many small and medium enterprises on bank financing, Europe appears to be an appropriate testing ground.¹

¹The Report on Bank Structures of the ECB shows that the number of credit institutions in the Eurozone declined from 8570 in 2008 to 6648 in 2016. Bank managers also seem to be very aware of this consolidation trend. A top official of a large European bank illustrated these concerns as follows: 'Europe needs more pan-European banks. JPMorgan is the biggest bank in the US with a market capitalization of almost 380 billion dollars, but the biggest European bank is Santander, with a market cap of 80 billion Euros (...) and banks are the only viable way to finance the continent's large population

Significantly, Degryse et al. (2011) show that negative firm-level effects of bank mergers mostly occur when the bank-firm relationship is dropped in the aftermath of a merger. However, they also show that this is not due to the fact that banks cut 'bad' firms after the merger. As a result, the question of why bank-firm relationships are terminated after a bank-merger remains open. We shed light on this question by demonstrating that firms are looking to replace lending reductions resulting from the merger, by attempting to replace or complement their current bank relationships. We present novel evidence that especially the latter firms benefit from the bank merger: Firms *adding* another bank relationship perform significantly better than their counterparts following a bank merger. We then demonstrate that this ability to find funding at other banks crucially depends on two factors: bank competition and firm collateral. Firms affected by a target bank merger are less likely to drop their bank relationship and more likely to switch to another bank if they are located in a less concentrated banking market. Similarly, firms with more available collateral are less likely to drop their bank relationship and more likely to switch to another bank. These results strongly suggest that increasing bank-market concentration comes at the expense of firms who have little collateral and are already in non-competitive environments, as such firms will struggle to find alternatives to their existing bank following a merger.

In order to arrive at our findings we use a bank-firm level dataset from Germany, which comprises almost 500,000 firms and their banks.² We merge this data with information on 526 German bank-mergers from 2005 to 2014. We then first estimate the effects of a bank-merger on firms' real outcomes using a difference-in-difference type approach. Additionally, we compute dummies on bank-firm relationship discontinuations from the dataset and then estimate logit regressions to derive the impact of the competitive environment and the firms' collateral on the probability to stay, switch, drop or add a bank relationship following a merger.

A large literature is concerned with the consequences of bank mergers, especially for lending to firms. DeYoung et al. (2009) summarizes various merger studies on lending and find mixed evidence for the net effect of banking market consolidation for

of small- and mid-sized businesses' (Financial Times: <https://www.ft.com/content/a4ca22b8-6188-11e8-90c2-9563a0613e56>).

²Similar data has been used in Popov and Rocholl (2017) and Koetter et al. (2016) among others.

price and availability of firm credit. While there are some studies that find an overall negative effect of credit availability for firms (Carow et al., 2006; Di Patti and Gobbi, 2007; Craig and Hardee, 2007), other studies show mixed results (Sapienza, 2002; Berger et al., 2007; Francis et al., 2008) and Marsch et al. (2007) finds no negative lending effects after bank mergers for Germany. The evidence for prices is also mixed, with mergers generally emerging to cause small decreases in prices except if the merger causes a significant shift in market share (Erel, 2011).

More recently, this literature has not only considered the effects on lending but also on firm outcomes. A robust theme is that mergers are more likely to disrupt the firm-bank relationship and as a result may be harmful to firms (Mercieca et al., 2009; Di Patti and Gobbi, 2007; Degryse et al., 2011). For example, Montoriol-Garriga (2008) provides evidence on the costs and benefits of bank mergers to small businesses using a sample of Spanish firms. The results show that mergers are harmful to small businesses because lending relationships are more likely to be disrupted following a merger. This study also identifies that small borrowers of target banks have a higher probability of having terminated a relationship with the consolidated bank and they will also find it harder to start new lending relationship with consolidated banks. Overall, her results suggest higher termination rate for existing borrowers is not compensated with a higher initiation rate of new lending relationships with small business after the merger. However continuing borrowers are shown to benefit from mergers in terms of reduced loan rates.

Importantly, real effects do not only emerge when a consolidation wave takes place. A single megamerger may also have a significant macroeconomic impact. Fraisse et al. (2018) study the effect of a merger between two large banks on credit market competition. They find that the megamerger has a negative effect on lending, in particular through termination of relationships. They find that, in the average market, bank credit decreases by 2.7 per cent. On the real side, firm exit increases by 4 per cent, whereas firms that do not exit and firms that start up experience no adverse real effect on investment and employment.

Using Belgian data, Degryse et al. (2011) demonstrate that adverse effects of bank mergers mainly materialize after firm-bank relationship 'drops', whereas firms who either stay with their bank, or manage to switch to another bank do not show many

negative effects in terms of credit and investment. Importantly, they demonstrate that these drops can not be explained by the fact that the merged bank is better at screening borrowers and thus efficiently drops borrowers from their portfolio, but rather that firms who should have been dropped, but were not, performed worse than the firms that were actually dropped.

Overall the literature indicates that the understanding of *why* firm-bank relationships are terminated following bank-mergers remains an unanswered question, on which we intend to shed light with our findings. In line with the literature, we propose in this paper that firms are subject to a general reduction in lending from a merged bank. Firms then search for alternative means of financing. Successful firms who are either able to switch to a different bank or add an additional bank, perform significantly better following the merger. We then demonstrate that this ability to switch or add a bank relationship crucially depends on both the firms available collateral and the bank-competition in the firms region, showing the importance of bank-alternatives for firms after one of their banks has merged.

5.2 Data

For our analysis we use a matched bank-firm level dataset for Germany, which attaches bank-level balance sheet data from Bankscope to firm level data from Dafne and Amadeus. All three databases are provided by Bureau van Dijk and contain balance sheet data of banks and firms, respectively. Matching of firm data to bank data occurs via (historical vintages) of the Dafne database. The same or similar datasets have recently been used in several studies (Popov and Rocholl, 2017; Koetter et al., 2016).³ While this dataset does not provide loan-level data, it identifies roughly 1.1 million firms (and 2000 banks) for Germany and the corresponding bank-firm relationships, which includes detailed information on small and medium sized enterprises and their banks, which are usually not included in loan-level databases.⁴ We merge this firm-bank level data to 526 bank mergers for German banks between 2005 and 2014, using official data provided to us by the German Bundesbank.

³The firm-bank level matched database relies on a string match between the bank name in the firm level data and the bank name in the bank level data. As a result, the match is not perfect although manual corrections lead to a 99% match of bank-firm relationships.

⁴As for example in DEALSCAN data for the US.

From this merged dataset we drop all financial firms, firms for which we do not observe any valid postcode, all inactive firms, all firms for which we do not observe total assets and all firms for which we have only one available year. We also apply some logic tests, and drop firms which fail them. For example, if firm equity exceeds firm assets. We also drop all firms whose banks were target of a merger more than once during our observation period, in order to remove potential concerns for overlapping merger effects and to make the effects of mergers comparable across firms. We also drop all observations for which we do not have data on our control variables. However, because the data coverage varies significantly over the firm-level variables, we choose not to restrict the sample along the lines of the dependent variables, thus keeping the sample for each regression as large as possible.⁵ The final sample consists of 463,740 firms and 2,116 banks.

TABLE 5.1 AROUND HERE

Descriptive statistics for the firms used in the analysis are displayed in Table 5.1. About 8% of our observations occur after a firms' bank has been subject to a merger, whereas some 36% of observations occur after the bank has been the buying party in a merger. Our discontinuation variables demonstrate that a change in bank relationships is relatively rare; only in 6% of cases is a switch or drop of a bank relationship is observed, whereas adds occur in about 10% of cases. The mean HHI - our measure of banking market concentration at the county level - is .56, although there is considerable variation in the data. The firm-level outcome and control variables demonstrate clearly that the firms in the sample are very small; the average amount of total bank loans amount to just 540,000 Euros at roughly 3% interest. Firms have 60 employees on average (median of 12), and are highly profitable on average with a return on equity of 34% (median 19%).

⁵There is strong indication that this lack of reporting of variables is non-random. Smaller firms generally have more missing variables. As a result this choice is also made to restrict selection bias in the sample.

5.3 Effects of bank mergers on firm outcomes

5.3.1 Overall effect on firm outcomes

First, we aim to compare firms whose banks have been subject to a bank merger to firms that did not. Because firms can have more than one bank relationship, we identify all firms, which had any of their banks merging during our sample period, and compare them to firms, which have not experienced a bank merger during during the sample period. To this end, we create a dummy variable which is 0 before the merger and 1 after a merger has taken place. In order to capture all post-merger effects this dummy remains at 1 for the rest of the periods in the sample. Di Patti and Gobbi (2007) and Degryse et al. (2011) estimate similar firm-level regressions of bank mergers, although their dummy is set to one for only a few periods after the merger. We chose our deviate from them for two reasons. First, we would like to capture all post-merger effects instead of only single-period effects. Second, having only one dummy makes the interpretation of our interaction models significantly easier.

As a result we formally estimate the following initial regression:

$$\ln Y_{jt} = \alpha_j + \alpha_r \times \alpha_t + \beta_1 \text{merger}_{jt} + \beta_2(\text{firmcontrols}) + \epsilon_{jt} \quad (5.1)$$

where Y_{jt} are the variables of interest for which we expect the bank merger to have significant effect: $\ln(\text{loans})$, interest rate, $\ln(\text{trade credit})$, collateral ($\ln(\text{tangible fixed assets})$), $\ln(\text{employees})$ and return on equity. We choose loans and the interest rate (proxied by total interest expense/loans) by firms in order to investigate whether the price and the volume of credit changes on the firm level after a merger. We then use trade credit to see if firms substitute a change in bank lending by adjusting their level of trade credit. We then investigate whether the merger additionally had any effect on firms' input factors: capital (tangible assets) and labor (employees) and finally whether it affected their return.

In addition to firm fixed effects we also control for region×time fixed effects in order to ensure that regional (demand) trends are not driving the frequencies of bank mergers. We include a number of lagged firm control variables: Cash, total assets,

current liabilities (all in logs) and the firms' capital ratio. All firm control variables are lagged by one period.

TABLE 5.2 AROUND HERE

Our results demonstrate that target bank mergers have a significant effect on firms real effects. However, as opposed to Degryse et al. (2011) and Di Patti and Gobbi (2007), our results point to a larger overall economic impact. Column (1) and (2) of Table 5.2 indicate that firms, whose banks were target of a merger experienced a decrease in lending by roughly 13% and an increase in the interest rate by 7 basis points. There is some evidence that firms substitute this decrease in funding by increasing trade credit financing, although the effect is not statistically significant (Column (3)). Interestingly, the decrease in bank funding does not lead to a decrease in capital inputs, as tangible fixed assets remain unchanged (Column(4)). However, labor inputs are negatively affected, as firms reduce employment by about 1.4% (Column (5)). Firms' returns appear to not be affected by the merger. For buying mergers, we find only positive effects on employment, however the effect is economically small with an increase in employment by 0.9%.

Overall these results are in line with the previous literature, although we show larger negative effects of bank mergers on credit and performance, independent of whether the firm-bank relationship is continued or discontinued as in Di Patti and Gobbi (2007) and also independent of whether the firm is dropped or not dropped as in Degryse et al. (2011). We are the first to document the effect on firms input factors.⁶ We curiously find that firms do not decrease assets, but rather decrease *employment*, despite the fact that lending is generally thought to affect capital inputs before employment. However, if firms believe the restricted access to credit is short-term only (which is supported by the findings in Di Patti and Gobbi (2007)), it might be easier to reduce the more flexible labor input.

5.3.2 Real effects by post-merger relationship status

Next, we test whether the findings by Degryse et al. (2011) that firms are more negatively affected by a bank merger if they drop their bank relationship in the aftermath

⁶Di Patti and Gobbi (2007) Only investigate credit and Degryse et al. (2011) investigate only asset (growth), bankruptcy and profitability.

of the merger hold for our sample. We do this by interacting our merger dummy with a categorical variable indicating the firm-bank relationship status after the merger. This categorical variable takes the value of 0 if the firm stays with the bank, 1 if the firm switches to another bank, 2 if it drops a bank relationship and 3 if the firm adds another bank to their portfolio. The variable is grouped over the the merger dummy, such that a change in any period after/before the merger is set to this value, independent of when it occurs. For example, if a firm-bank relationship drops after the merger, this variable will take value 0 before the merger and 2 after the merger. It thus captures the post-/pre- merger bank-firm relationship changes (or continuations in the case of stays). Interacting this variable with our merger dummy thus indicates whether a merging firm that dropped their bank relationship will perform better or worse than a firm who experienced a bank merger but stayed with their bank.

TABLE 5.3 AROUND HERE

Table 5.3 indicates that additional negative effects of target bank mergers arise if the bank-firm relationship is dropped after a bank was a target of a merger and such negative effects are mitigated if id the firm adds another bank to its portfolio. Column (1) shows that in addition to the negative baseline effect on lending of about 16 %, firms who drop their relationship at some point after the merger experience a decrease in bank loans by an additional 32%, while firms who are able to add another bank increase their bank loans by roughly 40%. Dropping firms also reduce employment significantly more than firms staying with their bank; the negative baseline effect of 1.1% decreases by a further 5.8% for dropping firms. Adding a bank relationship also compensates the negative employment effect, as such firms increase employment by 3.2% over the baseline. Interestingly, dropping firms do not perform worse, instead increasing their return on equity by more than 7%. This is in line with findings by (Degryse et al., 2011) that target droppers firms' profitability increases. Importantly, we show that firms who are able add an additional bank to their portfolio perform much better along most outcomes. In addition to more bank loans, they also receive 38% more trade credit (Column(3)), increase their tangible assets by roughly 10% (Column (4)) although their profitability remains unchanged.

For banks being the buying party in a merger, all firms exhibit a small decrease in bank loans by 6%. Adding an additional bank is highly effective in mitigating this negative effect, as it increases bank loans by roughly 35% (Column (7)). Firms who drop or add a bank after the merger receive more trade credit, which suggests that firms' loan demand is exceeding what is supplied to them by their banks (Column (9)). Again, input factors increase most for firms, which add a bank after the merger (Columns (10+11)).

The regressions suggest two main interpretations. First, target bank mergers affect firms more significantly than buyer mergers. Second, firms which drop their bank relationship after the merger perform significantly worse than firms who stay with or switch their bank. Both findings are similar to those in Degryse et al. (2011). Our regressions additionally demonstrate a novel effect; adding a bank relationship after the merger has very strong positive effects for firms, both in terms of lending and input factors. This is a strong indication that firms may be systematically supplied fewer loans than they actually demand after a bank merger. Firms adding another bank are able to compensate by borrowing from an additional bank.⁷ If it is in fact true that banks underserve their firm clients after a bank merger, there is much reason to suspect that bank-firm relationship termination is perhaps not driven by the banks decision to cut risky and not profitable firms,⁸ but by the firms decision to change lenders, because they demand more loans than they are able to get from their post-merger bank.

5.4 Decision to change relationships

Figure 5.1 illustrates descriptively that bank-firm relationships change more frequently after bank mergers.⁹ We display the relative frequency of stays, switches, drops and adds on the y-axis, differentiated by firms which are affected by a bank-merger (after the merger) and those firms that are not. The figure suggests that after a merger,

⁷Note that these findings are line with Di Patti and Gobbi, 2007 that firms with fewer lenders also experience a higher reduction in credit, presumably because they lack alternatives of obtaining additional credit.

⁸Degryse et al. (2011) show very nicely that dropped firms are actually *better* than non-dropped firms. This would be in line with the idea that profitable firms are looking for a new lender, because they are not served sufficient loans.

⁹Because we omitted firms with multiple changes these relative frequencies sum up to 1.

firm-bank relationships are almost twice as likely to be dropped and the chance to add another bank also increases. There is thus strong indication that in addition to firm outcomes suffering in case of a dropped relationship after a merger, the probability of a dropped bank relationship also increases. The goal of this section is to test whether this finding holds up to statistical tests, and why we find more drops after a merger.

FIGURE 5.1 AROUND HERE

5.4.1 Do firm-bank relationships change more frequently after mergers?

We estimate the decision to terminate the bank-firm relationship and whether this is influenced by the merger using separate logit regressions for each decision: Staying and not staying, switching and not switching, dropping and not dropping and adding and not adding. We chose separate logit models, because the decisions are quite distinct, and it is not clear that we are interested in the decision of dropping vs. staying more than the decision of dropping vs. switching. In fact, we demonstrate that a key factor in explaining post-merger relationship continuation decisions is mainly relevant in the switching vs. dropping comparison, namely competition. Again, because we estimate at the firm level, the decision refers to *any* firm-bank relationship. This way of identification allows us to investigate the decision to *add* a bank. We thus specify the following logit model:

$$\ln \left[\frac{p(\text{RelStatus}_{it} = 1)}{1 - p(\text{RelStatus}_{it} = 1)} \right] = \alpha_t + \beta_0 + \beta_1 \text{merger} + \beta_2(\text{firmcontrols}) + \epsilon_{jt} \quad (5.2)$$

where RelStatus_{it} refers to a staying, switching, dropping and adding dummy, which takes the value one if before/after the merger a firm-bank relationship stayed, switched, dropped or was added and 0 otherwise. As in Equation 5.1, β_1 is our coefficient of interest and merger can be a dummy for any of the firms' banks being subject to either the target or the buyer in a bank merger. We include the same controls as in Equation 5.1.¹⁰

¹⁰Note that high level fixed effects are impossible in logit models, because the models are less likely to converge and because the computational effort cannot be handled by the resources available to us.

TABLE 5.4 AROUND HERE

Table 5.4 displays the results of the regression of the merger dummy on the decision to stay, switch, drop or add a bank relationship for the firm. Marginal effects are displayed in the table. The results indicate that the probability to switch decreases by about 0.9 percentage points due to a target merger, which is a sizable effect given that the base probability of non-merger firms is roughly 4% (Figure 5.1). Firms are also significantly more likely to drop their bank relationship by 3% percentage points. The effect of being the buying party in a merger is somewhat different. Firms are slightly more likely to stay with their bank by 0.1 percentage points. They are however more likely to switch, drop and add a relationship after the merger. This indicates that after target mergers, firms either stay with their bank or have to drop the relationship completely, whereas buying mergers can more easily be compensated by switching or adding another bank.

5.4.2 Why do firm-bank relationships change after mergers?

Next we investigate the contributing factors to the decision to stay, switch, drop or add a bank, by interacting the merger dummy with two key aspects that play a role in the decision to stay, switch drop or add a bank relationship: competition and firm collateral. We measure competition by the bank-level Herfindahl Hirschmann Index (HHI) in the *firms* county and collateral by the log of firms tangible fixed assets. Again, we differentiate between firms whose banks were the target or the buying party in the merger. We then interact the merger variable with our continuous competition and collateral indicators.

The marginal effect of the target merger on the decision to stay, switch drop or stay at different levels of bank-competition is given in Figure 5.2, with the corresponding Table in the online appendix (without marginal effects, Table D.I). The figure shows, that there is an increase in the probability to keep the bank-firm relationship after a merger, but that effect does not vary much with the level of competition. Additionally, firms are less likely to switch banks after a merger, especially if concentration in the banking market is large. Firms are also much more likely to drop their relationship if bank concentration increases. The effect on adds is also increasing in concentration,

although only slightly. Overall the results appear to confirm, that firms decision to stay, switch and drop highly depends on the alternatives available to the firm. The more concentrated the banking market, the less likely firms are to switch banks and the more likely they are to drop their relationship. This is in line with the prior interpretation that firms may be underserved by their post-merger bank; if firms are in a more concentrated market and have fewer alternatives, they cannot switch to other banks and instead either stay or drop their relationship.

FIGURE 5.2 AROUND HERE

FIGURE 5.3 AROUND HERE

For buying mergers, we find quite different results. Firms are more likely to stay with their bank after the merger, but this effect is almost independent of the level of banking market concentration. Similar findings hold for switches. However there is a structural difference in the decision to drop; firms are more likely to drop after a buying merger in general, but are less likely to do so in more concentrated markets. This is the reverse of the target merger results. We hypothesize that buying mergers may be less intrusive than target mergers for firms, and as a result it might be more efficient for firms to stay with their bank than dropping the relationship outright and perhaps trying to find other forms of financing.

If firms experience a drop in loan supply following a bank merger that induces them to seek a switch of banks or financing more generally, firms that can offer more collateral should have an easier time to find another bank and as a result experience fewer drops. We test this by interacting the merger dummy with a dummy indicating the firms collateral. Because we are limited to balance sheet data for firms, we use the log of tangible fixed assets as a proxy for the firms assets that can be credibly pledged as collateral. We show the marginal effects of this interaction in Figure 5.4 and Figure 5.5 for target and buying mergers respectively.

FIGURE 5.4 AROUND HERE

FIGURE 5.5 AROUND HERE

Figure 5.4 shows that the decision to stay is negatively correlated with the level of available collateral. Additionally, firms are less likely to switch after their bank has

been subject to a target merger, and this probability increases significantly with the level of available collateral. While low-collateral firms are more likely to drop their relationship by 5 percentage points, high collateral firms are much less likely to drop their relationship after a firm's bank has been the target of a merger. Interestingly, adds are also less frequent with increasing collateral, indicating that available collateral is specifically important for the ability to switch from one bank to the other, rather than for obtaining post-merger funding in general.

For stays, switches and adds the effects of collateral on the post-buying merger banking decision is similar to the target merger decision (Figure 5.5).¹¹ Firms are slightly more likely to stay after a buying merger by 0.1 percentage points. But while having more collateral increases that probability, it is only a small effect. Switches are more likely, if the firm has more collateral. While the probability of switching increases after the merger by less than 1 percentage point for firms with the lowest level of collateral, it increases by 3 percentage points for firms showing the highest level of collateral. Interestingly, the probability to drop after a buying merger *increases* with the available collateral.

5.4.3 Robustness

Because the merger events may not be the same over time, our pre- and post- merger periods may be systematically different across the time dimension. This may be problematic for the estimation of the standard errors. Similar concerns are also raised by the difference-in-difference style setup of our regression (Bertrand et al., 2004). As a result, we test if our results hold when we remove the time dimension from the estimation. In order to do so, we collapse the sample to pre- and post merger periods, and re-estimate our regressions. We provide the table for this robustness check for our competition interaction in Table D.III and the figures for the marginal effects at different levels of HHI in Figures D.I and D.II. The results of these regressions is very similar to the results of the previous estimations for the target mergers. We proceed the same way with our interactions regarding firm collateral. The results are given in Table D.IV and Figures D.III and D.IV. All marginal effects graphs look almost

¹¹The corresponding output of the logit regression without marginal effects can be found in Table D.II

identical in the collapsed and non-collapsed sample, leading us to conclude that our results are robust with regard to collapsing the sample.

TABLE 5.4 AROUND HERE

Because the decision to stay, switch, drop or add banks following a bank merger are not really independent from each other, we also estimate our baseline regression using a multinomial logit model. Whereas this model lends itself to investigating the baseline decision after the merger, using in conjunction with our competition and collateral measure produces findings that may be difficult to interpret. As a result, we only estimate the basic interaction as a multinomial logit, to confirm that our baseline results hold. Table 5.5 confirms that this is indeed the case. Firms are less likely to switch to another bank following a bank merger, more likely to drop their bank relationship and marginally more likely to add another bank. We also confirm, that for buyer mergers all choices are more likely when compared with the base case.

5.5 Conclusion

Our paper confirms the previous literature by finding that bank-mergers can be harmful for firms. In line with Di Patti and Gobbi (2007) and Degryse et al. (2011), we demonstrate that firms suffer most extensively when their bank relationship drops following a bank merger. We add to this literature in two significant ways. First, we are first to show that firms who are able to *add* a bank relationship after a merger can benefit from the merger, as they perform much better than firms who stayed, switched or dropped their relationship. This finding is novel and somewhat unexpected, because bank mergers should be unrelated to the firms demand (to add another bank). We suggest that this finding can be explained by the fact that post-merger firms are subject to a lending reduction from the merging bank, and as a result adding another bank to compensate the funding shortfall is highly beneficial. These findings would be in line with the idea that bank mergers destroy bank-customer relationships (Allen et al., 2016) and as a result induce firms to seek alternative financing means. We thus provide a potential explanation for the puzzle demonstrated in Degryse et al. (2011) that bank-mergers lead to the dropping of high-quality borrowers, instead of low-quality borrowers.

We then demonstrate that the ability to find other such means of financing in the banking system crucially depends on the available competition and the available collateral. Firms in more competitive banking environments are more likely to switch and less likely to drop their bank relationship than other firms. Similar findings hold for collateral; firms with more collateral have an easier time switching to different banks. We thus provide evidence towards the fact that the environment in which bank-mergers take place are very important in order to evaluate their economic impact. While bank mergers in somewhat competitive markets may be less harmful, bank mergers in already highly concentrated markets will lead to an increase in firm-bank relationship drops with the resulting negative consequences for firms, suggesting that bank-competition may be important not only for pricing, but for (efficient) continuations of bank-firm relationships.

Tables and Figures

Figures and Tables

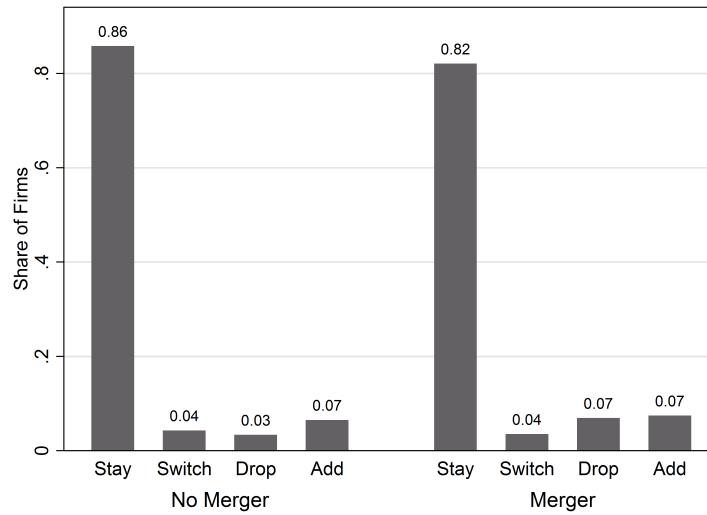


FIGURE 5.1: Bank relationship decisions after bank merger (target)

This Figure shows the relative share of relationship continuations (Stay), switches to another bank (Switch), dropping of a bank relationship (Drop) and adding an additional bank relationship (Add) at any point in time, by firms' banks participation in a merger (target). Share of firms sums to 1 for each respective group, as we exclude all firms, for which switches, drops and adds occur simultaneously.

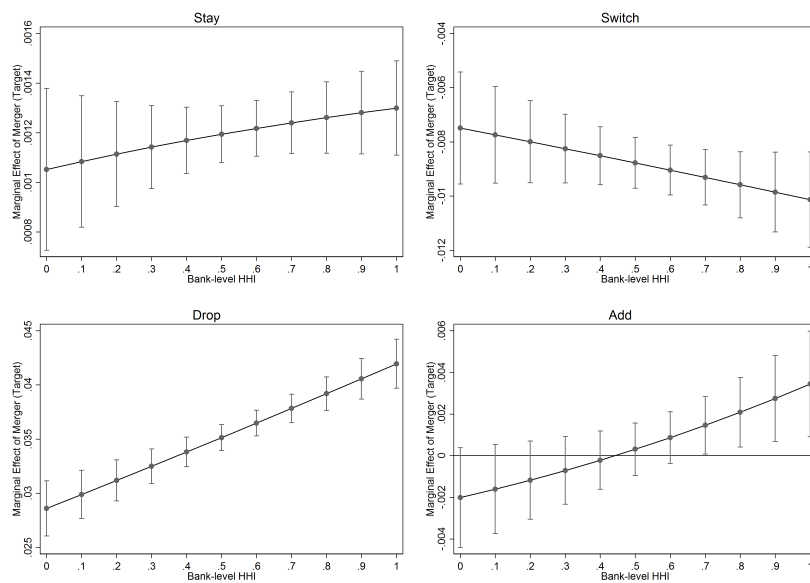


FIGURE 5.2: Marginal effect of merger (target) on relationship decision, by level of banking market concentration

This figure plots the marginal effect of a firms' bank being target of the merger on the firms decision to stay, switch, drop or add a bank at different levels of banking concentration in the firms county. The corresponding table without marginal effects is given in Table D.I. The error bars represent the 90% confidence intervals. HHI is the banking market Herfindahl Hirschman Index; the closer the index is to 1 the more concentrated is the banking market.

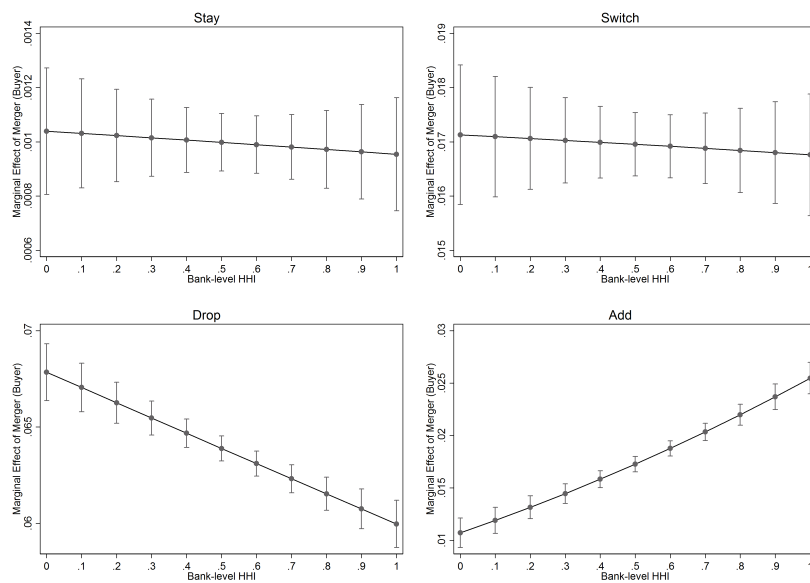


FIGURE 5.3: Marginal effect of merger (buyer) on relationship decision, by level of banking market concentration

This figure plots the marginal effect of a firms' bank being buyer in a bank merger on the firms decision to stay, switch, drop or add a bank, at different levels of banking concentration in the firms county. The corresponding table without marginal effects is given in Table D.I. The error bars represent the 90% confidence intervals. HHI is the banking market Herfindahl Hirschman Index; the closer the index is to 1 the more concentrated is the banking market.

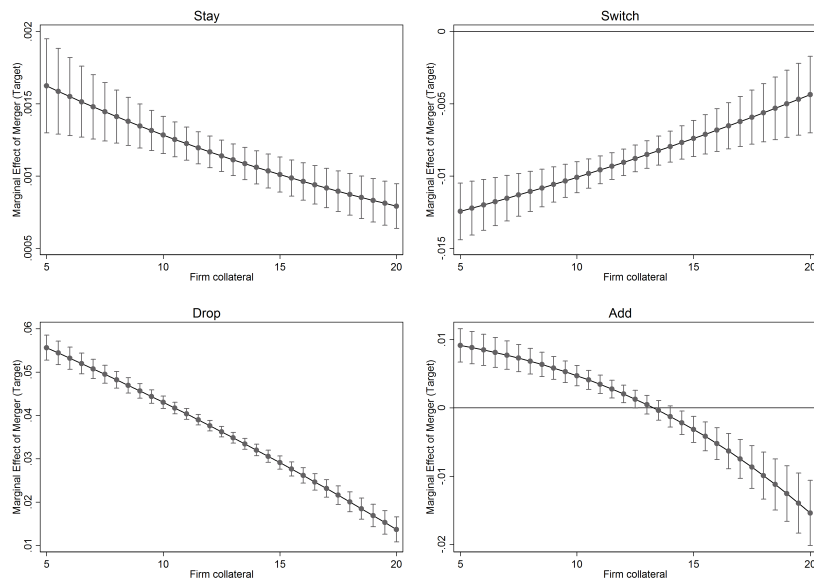


FIGURE 5.4: Marginal effect of merger (target) on relationship decision, by firms' collateral

This figure plots the marginal effect of a firms' bank being target in a bank merger on the firms decision to stay, switch, drop or add a bank, at different levels of the firms available collateral. Collateral is defined as the log of tangible fixed assets of the firm. The corresponding table without marginal effects is given in Table D.II. The error bars represent the 90% confidence intervals.

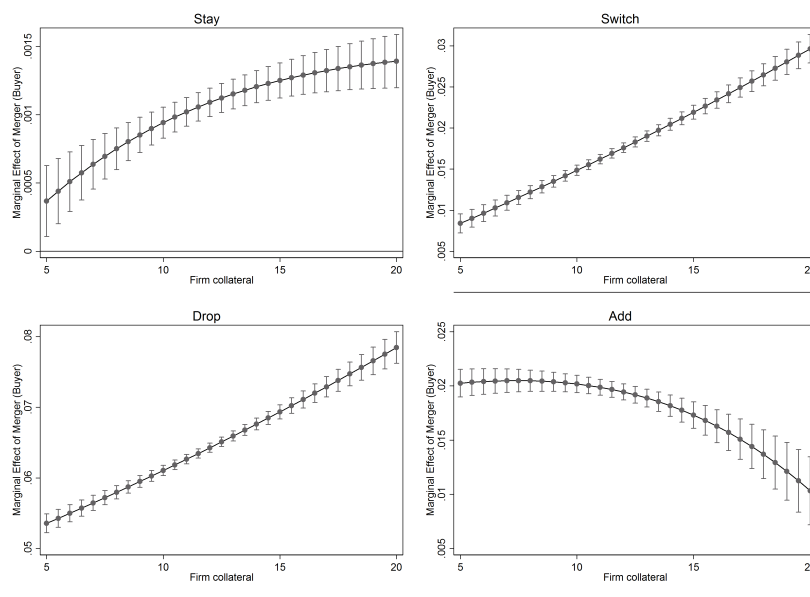


FIGURE 5.5: Marginal effect of merger (buyer) on relationship decision, by firms' collateral

This figure plots the marginal effect of a firms' bank being buyer in a bank merger on the firms decision to stay, switch, drop or add a bank, at different levels of the firms available collateral. Collateral is defined as the log of tangible fixed assets of the firm. The corresponding table without marginal effects is given in Table D.II. The error bars represent the 90% confidence intervals.

TABLE 5.1: Descriptive statistics

	N	Mean	SD	Min	Max
Merger Variables					
Merger Target	2024025	0.08	0.27	0.00	1.00
Merger Buyer	2024025	0.36	0.48	0.00	1.00
Relationship (dis-)continuation Variables					
Switch (Target)	2024025	0.06	0.23	0.00	1.00
Drop (Target)	2024025	0.06	0.24	0.00	1.00
Add (Target)	2024025	0.10	0.30	0.00	1.00
Switch (Buyer)	2024025	0.05	0.23	0.00	1.00
Drop (Buyer)	2024025	0.06	0.24	0.00	1.00
Add (Buyer)	2024025	0.09	0.29	0.00	1.00
Interaction Variables					
HHI	2024025	0.56	0.26	0.12	1.00
Collateral	2024025	10.97	3.40	0.00	23.77
Firm Outcome Variables					
Loans (mil.EUR)	1203880	0.54	15.63	0.00	5255
Interest Rate	354116	0.03	0.73	-0.10	395.45
Trade Credit (mil.EUR)	1203774	0.81	23.71	0.00	6119
Total Fixed Assets (mil.EUR)	2024025	2.94	65.70	0.00	21127
Number of Employees	1315985	60.06	984.95	1.00	276418
Return on Equity	337095	0.34	1.17	-10.00	10.00
Firm Control Variables					
L.Cash (mil.EUR)	2024025	0.85	27.30	0.00	15119
L.Total Assets (mil.EUR)	2024025	12.59	414.56	0.00	126562
L.Capital Ratio	2024025	0.34	0.28	0.00	1.00
L.Current Liabilities (mil.EUR)	2024025	3.10	122.42	0.00	30052

This table presents summary statistics for all variables of interest. Merger Target and Merger Buyer are dummy variables set equal to 1 after the firms' bank has been target or buyer in a merger, respectively, and 0 otherwise. Switch (Target/Buyer) is a dummy variable set equal to 1 if a post or pre-merger change of the bank relationship has taken place. Drop (Target/Buyer) is a dummy variable set equal to 1 if the bank relationship is dropped before or after the merger. Add (Target/Buyer) is a dummy set equal to 1 if another bank relationship is added after a merger. All dummy variables are 1 before or after the merger has taken place, never both. HHI is the bank-level Herfindahl Hirschmann Index (with the county as the regional unit), based on the concentration of bank assets. Collateral is the log of tangible fixed assets. Interest rate is calculated as interest income / total loans. Firm control variables are lagged by one period.

TABLE 5.2: Unconditional merger regressions

	Target						Buyer					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Merger Target	Loans -0.1307*** (0.042)	Interest rate 0.0077** (0.004)	Trade Cred. 0.050 (0.042)	Collateral 0.015 (0.010)	Employees -0.014*** (0.004)	RoE -0.016 (0.012)	Loans -0.032 (0.023)	Interest rate 0.002 (0.003)	Trade Cred. -0.003 (0.024)	Collateral 0.009 (0.006)	Employees 0.009*** (0.002)	RoE -0.012 (0.009)
Merger Buyer												
L. Cash	-0.036*** (0.003)	-0.001 (0.001)	-0.006** (0.003)	0.012*** (0.001)	0.003*** (0.000)	0.006*** (0.002)	-0.036*** (0.003)	-0.001 (0.001)	-0.006** (0.003)	0.012*** (0.001)	0.003*** (0.000)	0.006*** (0.002)
L. Total Assets	0.257*** (0.012)	0.010*** (0.003)	0.133*** (0.013)	0.452*** (0.006)	0.096*** (0.002)	-0.137*** (0.009)	0.258*** (0.012)	0.010*** (0.003)	0.133*** (0.013)	0.452*** (0.006)	0.096*** (0.002)	-0.137*** (0.009)
L. Capital Ratio	-0.681*** (0.034)	-0.041** (0.019)	-0.395*** (0.038)	0.101*** (0.013)	0.013*** (0.004)	-0.918*** (0.024)	-0.681*** (0.034)	-0.041** (0.019)	-0.395*** (0.038)	0.101*** (0.013)	0.013*** (0.004)	-0.918*** (0.024)
L. Current Liabilities	0.012*** (0.001)	-0.000 (0.000)	0.017*** (0.001)	0.001*** (0.000)	0.001*** (0.000)	-0.000 (0.000)	0.012*** (0.001)	-0.000 (0.000)	0.017*** (0.001)	0.001*** (0.000)	0.001*** (0.000)	-0.000 (0.000)
N	1,203,880	354,116	1,203,774	2,024,025	1,315,985	337,095	1,203,880	354,116	1,203,774	2,024,025	1,315,985	337,095
Number of Firms	378,352	98,061	378,876	463,740	387,139	91,927	378,352	98,061	378,876	463,740	387,139	91,927
Treatment Group	29,535	11,538	29,553	36,125	32,381	11,145	137,390	45,824	137,570	168,694	147,217	43,792
Adjusted R ²	0.696	0.866	0.727	0.927	0.970	0.567	0.696	0.866	0.727	0.927	0.970	0.567
Firm FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
County×time FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

This table displays the effect of bank-mergers on firm-level outcomes, as formally shown in Equation 5.1. Columns (1)-(6) represent the effects of target mergers, while columns (7)-(12) show the effects of the firms' banks being the buying party in a merger. Merger Target is a dummy variable set equal to 0 if the firms' banks have not been the target of a merger and set equal to 1 if any of the firms' banks have been the target of a merger. Merger Buyer is a dummy variable set equal to 0 if the firms' bank have not been the buying party in a merger and set equal to 1 if any of the firms' banks have been the buying party in a bank merger. Loans is the log of all firms' bank loans (borrowings). Interest rate is total interest expense divided by total loans. Trade credit is the log of firms' trade credit. Collateral is the log of firms' tangible fixed assets. Employees is the log of the firms employees. RoE is firms' return on equity. Firm and county×year fixed effects are included. Clustered standard errors on the firm level of the point estimates are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE 5.3: Real effects of bank mergers by relationship changes

	Target			Buyer								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Loans	Interest rate	Trade Credit	Collateral	Employees	RoE	Loans	Interest rate	Trade Credit	Collateral	Employees	RoE
Merger Target	-0.155*** (0.045)	0.008* (0.004)	0.027 (0.047)	0.009 (0.010)	-0.011** (0.005)	-0.033** (0.014)						
Merger Target×Switch	-0.092 (0.208)	-0.001 (0.009)	-0.127 (0.228)	0.015 (0.052)	-0.022 (0.026)	0.063 (0.043)						
Merger Target×Drop	-0.319* (0.164)	0.004 (0.006)	-0.149 (0.161)	-0.000 (0.042)	-0.058*** (0.019)	0.070** (0.035)						
Merger Target×Add	0.394** (0.162)	-0.002 (0.002)	0.382** (0.157)	0.107*** (0.036)	0.032* (0.018)	0.040 (0.032)						
Merger Buyer							-0.058** (0.024)	0.002 (0.004)	-0.031 (0.026)	-0.003 (0.006)	0.000 (0.002)	-0.014 (0.011)
Merger Buyer×Switch							-0.031 (0.107)	0.008 (0.006)	0.054 (0.110)	0.020 (0.029)	0.023** (0.012)	-0.020 (0.040)
Merger Buyer×Drop							-0.022 (0.118)	0.003 (0.003)	0.251** (0.117)	0.022 (0.029)	0.022* (0.013)	0.032 (0.028)
Merger Buyer×Add							0.358*** (0.089)	-0.000 (0.003)	0.254*** (0.091)	0.120*** (0.021)	0.070*** (0.008)	0.006 (0.024)
N	1,083,401	310,713	1,083,382	1,930,476	1,208,497	295,225	1,084,377	311,120	1,084,379	1,932,037	1,209,573	295,559
Number of Firms	287,223	74,563	287,719	414,629	311,650	69,166	287,488	74,704	287,983	414,929	311,941	69,287
Treatment Group	28,102	10,547	28,121	34,522	30,810	10,184	132,521	42,555	132,700	163,441	142,056	40,616
Adjusted R ²	0.704	0.245	0.733	0.926	0.969	0.516	0.703	0.245	0.732	0.926	0.969	0.517
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
County×time FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

This table shows the effects of bank mergers on firms, given that the firm switched, dropped or added a bank relationship after the merger. Switch, drop and add are values from 1, 2 and 3 from a categorical variable indicating the post-merger relationship status. Staying is the base category. Columns (1)-(6) represent the effects of target mergers, while columns (7)-(12) show the effects of the firms' banks being the buying party in a merger. Merger Target is a dummy variable set equal to 0 if the firms' banks have not been the target of a merger and set equal to 1 after any of the firms' banks have been the target of a merger. Buyer is a dummy set equal to 0 if the firms' bank have not been the buying party in a merger and set equal to 1 if any of the firms' banks have been the buying party in a bank merger. Loans is the log of all firms' bank loans (borrowing). Interest rate is total interest expense divided by total loans. Trade credit is the log of firms' trade credit taken. Collateral is the log of firms' tangible fixed assets. Employees is the log of the firms employees. RoE is firms' return on equity. Firm-level control variables are included, but not displayed. Firm and county×year fixed effects are included. Clustered standard errors on the firm level of the point estimates are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE 5.4: Decision to change bank relationship after merger: marginal effects

	Target				Buyer			
	(1) Stay	(2) Switch	(3) Drop	(4) Add	(5) Stay	(6) Switch	(7) Drop	(8) Add
Merger Target	0.002*** (0.000)	-0.009*** (0.001)	0.030*** (0.001)	0.001 (0.001)				
Merger Buyer					0.001*** (0.000)	0.016*** (0.000)	0.060*** (0.000)	0.018*** (0.000)
N	2,024,019	2,024,019	2,024,019	2,024,019	2,024,019	2,024,019	2,024,019	2,024,019
Number of Firms	463,736	463,736	463,736	463,736	463,736	463,736	463,736	463,736
Pseudo R ²	0.052	0.015	0.053	0.037	0.027	0.017	0.089	0.038
Firm Controls	YES	YES	YES	YES	YES	YES	YES	YES
Bank Controls	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES	YES	YES

This table presents logit regressions for the decision to stay, drop, switch or add a bank-relationship conditional on the firms' banks participation in a merger. Stay is set equal to 1 if the firm does not change its bank relationships at any point in time before or after the merger. Switch, drop and add are set equal to 1 if at any point in time after or before the merger, the firm decides to switch to another bank, drop a bank relationship or add an additional relationship. Merger Target is a dummy variable set equal to 0 if the firms' banks have not been the target of a merger and set equal to 1 after any of the firms' banks have been the target of a merger. Merger Buyer is a dummy set equal to 0 if the firms' bank have not been the buying party in a merger and set equal to 1 if any of the firms' banks have been the buying party in a bank merger. The reported coefficients are marginal effects of the independent variable on the probability of staying, switching, dropping or adding the lending relationship respectively. Standard errors (delta method) are displayed in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE 5.5: Robustness: Results of multinomial logit model

	(1)	(2)	(3)
	Switch vs. Stay	Drop vs. Stay	Add vs. Stay
Merger Target	-0.003*** (0.000)	0.005*** (0.000)	0.001** (0.000)
Merger Buyer	0.002*** (0.000)	0.011*** (0.000)	0.003*** (0.000)
N		2,024,025	
Number of Firms		463,736	
Stays		1,934,828	
Switches		25,537	
Drops		22,660	
Adds		40,994	
Pseudo R ²		0.035	

This table presents the marginal effect of a multinomial logit regression on the decision to switch, drop or add a bank relationship compared to the base category (stay). The dependent variable is a variable that takes the value of 1 if the firm stayed with their bank relationship in any particular year, 2 if it switched to another bank, 3 if it dropped a bank relationship and 4 if it added a bank relationship. Merger Target is a dummy variable set equal to 0 if the firms' banks have not been the target of a merger and set equal to 1 after any of the firms' banks have been the target of a merger. Merger Buyer is a dummy set equal to 0 if the firms' bank have not been the buying party in a merger and set equal to 1 if any of the firms' banks have been the buying party in a bank merger. The reported coefficients are marginal effects. Clustered standard errors are displayed in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Appendix D

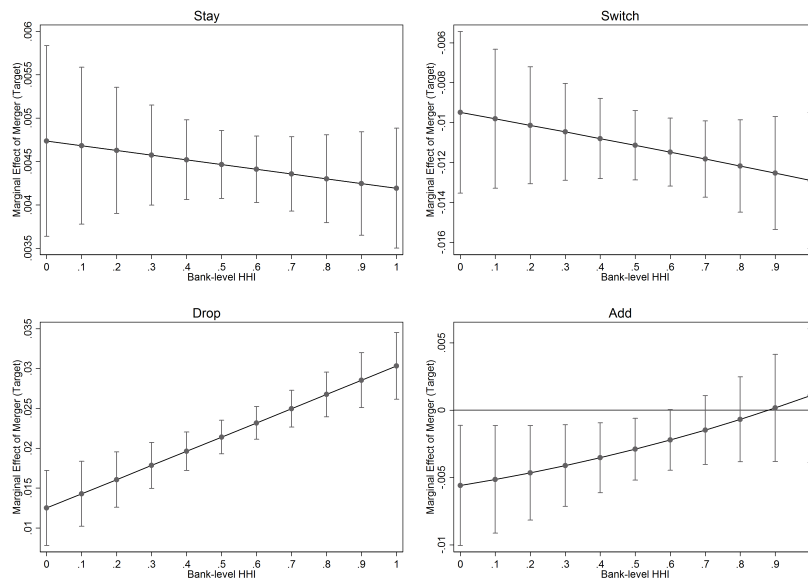


FIGURE D.I: Marginal effect of merger (target) on relationship decision, by level of banking market concentration: Collapsed sample

This figure plots the marginal effect of a firms' bank being target of the merger on the firms decision to stay, switch, drop or add a bank at different levels of banking concentration in the firms county, using a collapsed firm sample. The corresponding table without marginal effects is given in Table D.III. The error bars represent the 90% confidence intervals. HHI is the banking market Herfindahl Hirschman Index; the closer the index is to 1 the more concentrated is the banking market.

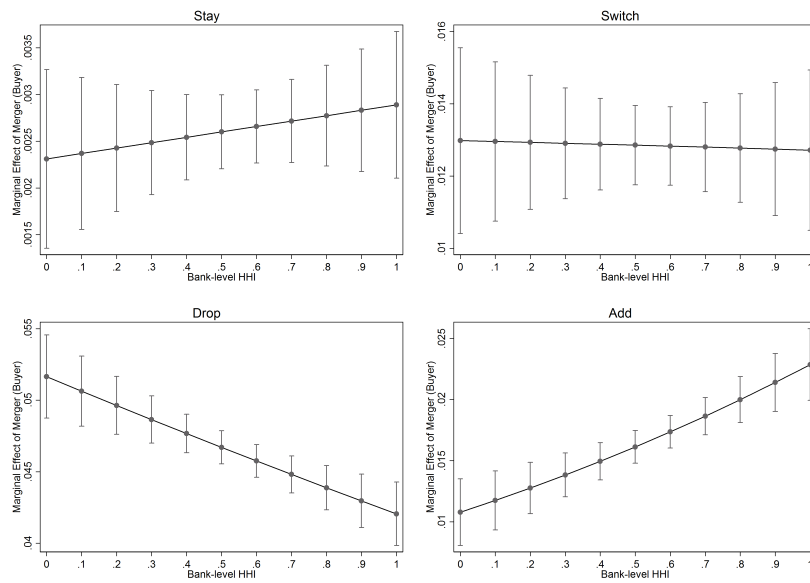


FIGURE D.II: Marginal effect of merger (buyer) on relationship decision, by level of banking market concentration: Collapsed sample

This figure plots the marginal effect of a firms' bank being buyer in a bank merger on the firms decision to stay, switch, drop or add a bank, at different levels of banking concentration in the firms county, using a collapsed firm sample. The corresponding table without marginal effects is given in Table D.III. The error bars represent the 90% confidence intervals. HHI is the banking market Herfindahl Hirschman Index; the closer the index is to 1 the more concentrated is the banking market.

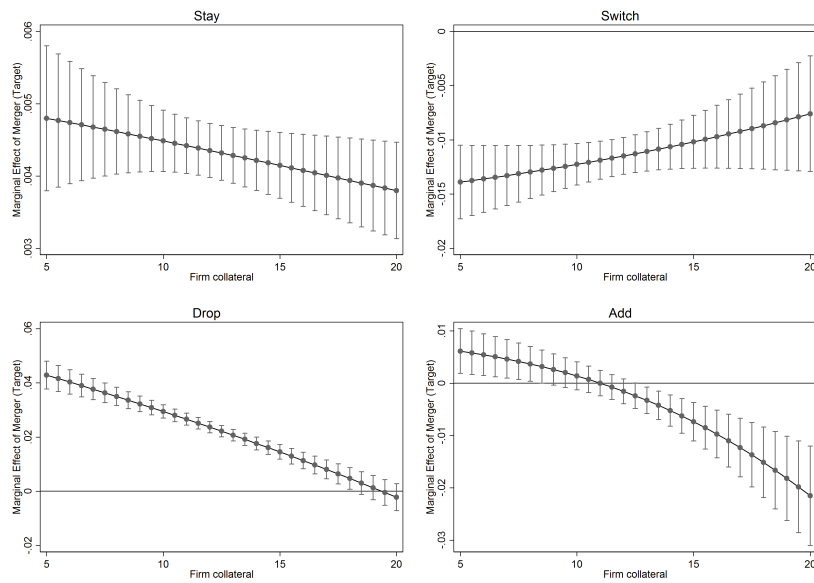


FIGURE D.III: Marginal effect of merger (target) on relationship decision, by firms' collateral: Collapsed sample

This figure plots the marginal effect of a firms' bank being target in a bank merger on the firms decision to stay, switch, drop or add a bank, at different levels of the firms available collateral, using a collapsed firm sample. Collateral is defined as the log of tangible fixed assets of the firm. The corresponding table without marginal effects is given in Table D.IV. The error bars represent the 90% confidence intervals.

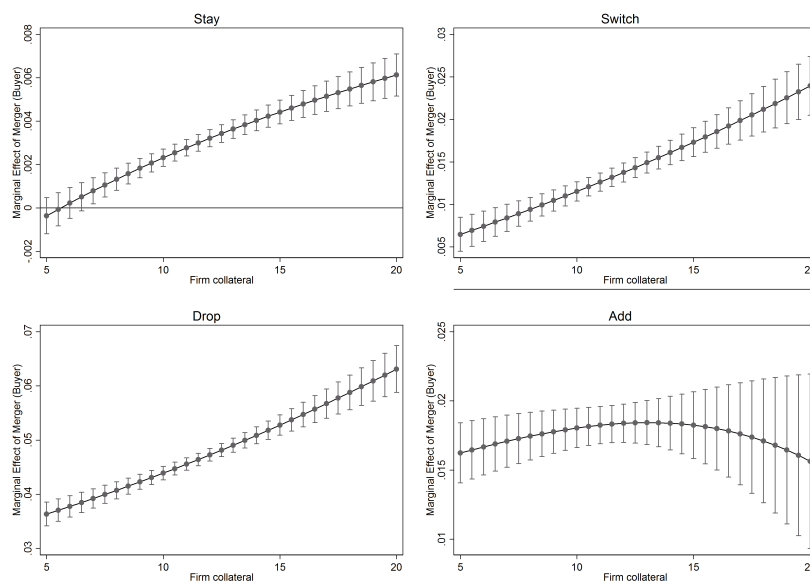


FIGURE D.IV: Marginal effect of merger (buyer) on relationship decision, by firms' collateral: Collapsed sample

This figure plots the marginal effect of a firms' bank being buyer in a bank merger on the firms decision to stay, switch, drop or add a bank, at different levels of the firms available collateral, using a collapsed firm sample. The corresponding table without marginal effects is given in Table D.IV. The error bars represent the 90% confidence intervals.

TABLE D.I: Decision to change bank relationship: Competitive environment

	Target				Buyer			
	(1) Stay	(2) Switch	(3) Drop	(4) Add	(5) Stay	(6) Switch	(7) Drop	(8) Add
HHI	0.015 (0.070)	0.090*** (0.012)	-0.042*** (0.012)	0.370*** (0.009)	-0.025 (0.069)	0.065*** (0.016)	0.062*** (0.018)	0.313*** (0.012)
Merger Target	0.957*** (0.284)	-0.153*** (0.027)	0.440*** (0.021)	-0.027 (0.020)				
Merger Target×HHI	0.522 (0.481)	-0.042 (0.043)	0.179*** (0.033)	0.062** (0.031)				
Merger Buyer					0.567*** (0.085)	0.337*** (0.015)	1.154*** (0.014)	0.152*** (0.012)
Merger Buyer×HHI					-0.077 (0.137)	-0.022 (0.024)	-0.126*** (0.023)	0.118*** (0.019)
N	2,024,019	2,024,019	2,024,019	2,024,019	2,024,019	2,024,019	2,024,019	2,024,019
Number of Firms	463,736	463,736	463,736	463,736	463,736	463,736	463,736	463,736
Affected Firms	36,125	36,125	36,125	36,125	168,693	168,693	168,693	168,693
Pseudo R ²	0.052	0.015	0.053	0.039	0.027	0.017	0.089	0.039
Firm Controls	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES	YES	YES

This table presents logit regressions for the decision to stay, drop, switch or add a bank-relationship conditional on the firms' banks participation in a merger and the banking concentration (HHI) in the firms county. HHI is the Herfindahl Hirschman Index of the banking market in the firms county. Stay is set equal to 1 if the firm does not change its bank relationships at any point in time before or after the merger. Switch, drop and add are set equal to 1 if at any point in time after or before the merger, the firm decides to switch to another bank, drop a bank relationship or add an additional relationship. Merger Target is a dummy variable set equal to 0 if the firms' banks have not been the target of a merger and set equal to 1 after any of the firms' banks have been the target of a merger. Merger Buyer is a dummy set equal to 0 if the firms' bank have not been the buying party in a merger and set equal to 1 if any of the firms' banks have been the buying party in a bank merger. Note that the coefficients are not marginal effects. Refer to Figure 5.2 and Figure 5.3 for the marginal effects of the merger at different levels of HHI. Standard errors are displayed in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE D.II: Decision to change bank relationship: Firm collateral

	Target				Buyer			
	(1) Stay	(2) Switch	(3) Drop	(4) Add	(5) Stay	(6) Switch	(7) Drop	(8) Add
Collateral	0.055*** (0.006)	0.007*** (0.001)	0.037*** (0.001)	0.073*** (0.001)	0.020*** (0.006)	-0.001 (0.001)	0.033*** (0.002)	0.080*** (0.001)
Merger Target	1.053*** (0.375)	-0.328*** (0.045)	1.110*** (0.034)	0.232*** (0.036)				
Merger Target×Collateral	0.016 (0.031)	0.012*** (0.004)	-0.046*** (0.003)	-0.017*** (0.003)				
Merger Buyer					-0.186* (0.102)	0.053** (0.023)	1.103*** (0.024)	0.445*** (0.021)
Merger Buyer×Collateral					0.066*** (0.009)	0.024*** (0.002)	-0.001 (0.002)	-0.018*** (0.002)
N	2,024,019	2,024,019	2,024,019	2,024,019	2,024,019	2,024,019	2,024,019	2,024,019
Number of Firms	463,736	463,736	463,736	463,736	463,736	463,736	463,736	463,736
Affected Firms	36,125	36,125	36,125	36,125	168,693	168,693	168,693	168,693
Pseudo R ²	0.054	0.015	0.054	0.041	0.029	0.018	0.090	0.041
Firm Controls	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES	YES	YES

This table presents logit regressions for the decision to stay, drop, switch or add a bank-relationship conditional on the firms' banks participation in a merger and the firms available collateral. Collateral is defined as the log of firms' tangible fixed assets. Stay is set equal to 1 if the firm does not change its bank relationships at any point in time before or after the merger. Switch, drop and add are set equal to 1 if at any point in time after or before the merger, the firm decides to switch to another bank, drop a bank relationship or add an additional relationship. Merger Target is a dummy variable set equal to 0 if the firms' banks have not been the target of a merger and set equal to 1 after any of the firms' banks have been the target of a merger. Merger Buyer is a dummy set equal to 0 if the firms' bank have not been the buying party in a merger and set equal to 1 if any of the firms' banks have been the buying party in a bank merger. Note that the coefficients are not marginal effects. Refer to Figure 5.4 and Figure 5.5 for the marginal effects of the merger at different levels of firms' collateral. Standard errors are displayed in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE D.III: Robustness: Collapsed sample for HHI interaction

	Target				Buyer			
	(1) Stay	(2) Switch	(3) Drop	(4) Add	(5) Stay	(6) Switch	(7) Drop	(8) Add
HHI	0.191** (0.081)	0.104*** (0.028)	-0.176*** (0.030)	0.389*** (0.023)	-0.037 (0.078)	0.072** (0.035)	-0.013 (0.043)	0.332*** (0.028)
Merger Target	1.243*** (0.308)	-0.234*** (0.066)	0.237*** (0.050)	-0.096** (0.048)				
Merger Target × HHI	0.188 (0.517)	-0.064 (0.107)	0.337*** (0.081)	0.109 (0.075)				
Merger Buyer					0.355*** (0.096)	0.295*** (0.034)	1.100*** (0.034)	0.184*** (0.028)
Merger Buyer × HHI					0.091 (0.157)	-0.021 (0.056)	-0.134** (0.056)	0.102** (0.044)
N	482,813	482,813	482,813	482,813	518,269	518,269	518,269	518,269
Affected Firms	36,125	36,125	36,125	36,125	168,693	168,693	168,693	168,693
Pseudo R ²	0.015	0.014	0.055	0.042	0.010	0.015	0.078	0.042
Firm Controls	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES	YES	YES

This table presents logit regressions for the decision to stay, drop, switch or add a bank-relationship conditional on the firms' banks participation in a merger and the banking concentration (HHI) in the firms county, using a collapsed sample of the data. HHI is the Herfindahl Hirschman Index of the banking market in the firms county. Stay is set equal to 1 if the firm does not change its bank relationships at any point in time before or after the merger. Switch, drop and add are set equal to 1 if at any point in time after or before the merger, the firm decides to switch to another bank, drop a bank relationship or add an additional relationship. Merger Target is a dummy variable set equal to 0 if the firms' banks have not been the target of a merger and set equal to 1 after any of the firms' banks have been the target of a merger. Merger Buyer is a dummy set equal to 0 if the firms' bank have not been the buying party in a merger and set equal to 1 if any of the firms' banks have been the buying party in a bank merger. Note that the coefficients are not marginal effects. Refer to Figure D.I and Figure D.II for the marginal effects of the merger at different levels of HHI. Standard errors are displayed in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE D.IV: Robustness: Collapsed sample for collateral interaction

	Target				Buyer			
	(1) Stay	(2) Switch	(3) Drop	(4) Add	(5) Stay	(6) Switch	(7) Drop	(8) Add
Collateral	0.026*** (0.006)	0.013*** (0.003)	0.044*** (0.003)	0.081*** (0.003)	-0.014** (0.007)	0.006** (0.003)	0.038*** (0.004)	0.086*** (0.003)
Merger Target	1.005** (0.396)	-0.442*** (0.103)	1.149*** (0.075)	0.227*** (0.080)				
Merger Target × Collateral	0.030 (0.033)	0.014* (0.008)	-0.059*** (0.006)	-0.021*** (0.006)				
Merger Buyer					-0.448*** (0.112)	0.055 (0.047)	0.996*** (0.052)	0.425*** (0.044)
Merger Buyer × Collateral					0.080*** (0.010)	0.020*** (0.004)	0.003 (0.004)	-0.015*** (0.004)
N	482,813	482,813	482,813	482,813	518,269	518,269	518,269	518,269
Number of Firms	463,736	463,736	463,736	463,736	463,736	463,736	463,736	463,736
Affected Firms	36,125	36,125	36,125	36,125	168,693	168,693	168,693	168,693
Pseudo R ²	0.016	0.014	0.057	0.045	0.011	0.015	0.079	0.045
Firm Controls	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES	YES	YES

This table presents logit regressions for the decision to stay, drop, switch or add a bank-relationship conditional on the firms' banks participation in a merger and the firms available collateral, using a collapsed data sample. Collateral is defined as the log of firms' tangible fixed assets. Stay is set equal to 1 if the firm does not change its bank relationships at any point in time before or after the merger. Switch, drop and add are set equal to 1 if at any point in time after or before the merger, the firm decides to switch to another bank, drop a bank relationship or add an additional relationship. Merger Target is a dummy variable set equal to 0 if the firms' banks have not been the target of a merger and set equal to 1 after any of the firms' banks have been the target of a merger. Merger Buyer is a dummy set equal to 0 if the firms' bank have not been the buying party in a merger and set equal to 1 if any of the firms' banks have been the buying party in a bank merger. Note that the coefficients are not marginal effects. Refer to Figure D.III and Figure D.IV for the marginal effects of the merger at different levels of firms' collateral. Standard errors are displayed in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Chapter 6

Conclusion

The interaction of banks and firms constitute a crucial part of a well-functioning economy. As a result, it is increasingly important to understand how this interaction comes about, is destroyed and how it affects the rest of the economy throughout the lending process. The aim of this thesis is to contribute to this understanding, by exploiting unexpected events, in particular natural disasters as a robust method for identification. Through this, it sheds light on the reaction of firms to unexpected events (Chapter 4), how banks react when their (borrowing) firms are affected by an unexpected event (Chapter 3), how other firms are potentially affected by this reaction through spillovers (Chapter 2), and finally how the probability to change the bank-firm relationship can be affected (Chapter 5).

To this extent Chapter 2 demonstrates the importance of bank capital in the transmission of local shocks from one region to another. It shows the presence of a firm-bank-firm shock transmission channel, by demonstrating that banks exposed to a natural disaster via their firm clients reduce lending to non-affected firms, particularly if the banks are not well capitalized. Firms in unaffected areas connected to banks with exposure to affected areas decrease employment by 11% and their fixed assets by 20%. Closer evaluation reveals that both perceived riskiness of banks as well as mandatory capital ratios may hinder banks to expand their balance sheets in times of increased loan demand. Further results indicate that relationship banking and higher levels of firm capital can also mitigate the risks of these shock spillovers. Overall, this chapters results highlight the importance of bank capital not only during times of financial crisis, but also in order to prevent local shock spillovers from one region to another, even if that shock drives higher loan demand, and does not contract supply (Cortes and Strahan, 2017).

Chapter 3 demonstrates that a shock arising from a natural disaster causes *recovery lending* by banks into disaster areas, even if the banks themselves are not located in the disaster area. The chapter first investigates the response of banks that have a firm customer portfolio in the affected areas, but are themselves located outside of the flooded area. Such banks increase credit by 3 %, relative to banks without such a flood exposure. In addition, corporate borrowing of firms with ties to banks in non-flooded counties, is 16% higher than for firms without such connections, despite the fact that there is an overall decline in credit. This demonstrates that lending by banks arrives at disaster-struck firms and helps them mitigate unfavorable credit conditions. Additionally, there is no evidence that this *recovery lending* increased riskiness of banks or changed interest rates for borrowers significantly. The results thus point to the fact that regionally active banks conduct lending to stressed firms, and help such firms to acquire financing during times of stress.¹

Chapter 4 investigates the direct effect of flooding on firm outcomes, in the context of a developed economy. Using the same flood data as in Chapter 2 and 3, this chapter demonstrates that the net effect of flooding on firms is unexpectedly *positive*. Compared to firms in non-flooded regions, firms in flooded regions have significantly higher turnover, lower leverage, and higher cash following the flood. There is some evidence that this effect is partially driven by firms learning from an earlier disaster which occurred in 2002. These positive effects on the firm level stand in contrast to the previous macroeconomic literature (McDermott et al., 2014). However, they can be reconciled in the context of *recovery lending* demonstrated in Chapter 3 and a presence of government aid and insurance, which is often present only in developed economies.

Finally Chapter 5 investigates bank mergers as a major disruption in the bank-firm relationship. It demonstrates that firms receive fewer loans and generally pay somewhat higher interest rates following bank mergers. This effect is particularly prominent for firms who drop their bank relationship following the merger (Degryse et al., 2011), but mitigated for firms who add an additional bank relationship. It also entails real effects for firm level factor inputs capital and employment. The chapter

¹Which in turn causes these banks to reduce lending to non-disaster areas, as pointed out in Chapter 2.

then demonstrates two key factors that are responsible for firm-bank relationship switches, drops or adds: Bank competition and firms available collateral. Both higher bank competition and higher firm collateral allow firms to more easily switch banks following a post-merger lending reduction. As a result, both factors are important in order for firms to avoid financing shortfalls and the resulting negative firm-level input effects. It thus highlights the importance of bank-competition not necessarily to reduce prices, but to allow firms to be able to more easily switch lenders in case they are faced with a sudden lending shortfall.

In summary, the thesis discusses important aspects of the bank firm relationship and its regulatory framework. It demonstrates the importance of the bank-firm relationship especially for buffering unexpected shocks. At the same time, this might come at the expense of non-crisis firms, especially in low bank-capital environments. It thus highlights the importance of incentivising higher bank capital ratios, because they can prevent regional shock spillovers. In addition to bank capital, the competitiveness of the banking sector is also highlighted. Both insights are of non-negligible significance given the continuing trend of banking market concentration and relatively (compared to other firms) low capitalization rates of banks.

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