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Flooded Through the Back Door: Firm-level Effects of Banks' Lending Shifts

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Editor

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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.

Keywords: natural disaster, real effects, shock transmission, bank capital

JEL Classification: E24, E44, G21, G29

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1 Introduction

This paper expands on the recent finding that banks shift their lending from non-disaster areas into disaster areas (Cortés 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 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 (Cortés 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., 2016). First, a natural disaster is random and exogenous, especially to firms outside the disaster area. Given that there is some 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

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

[2] And 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.

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., 2016).

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 (2014) and Cortés and Strahan (2017) document for the United States that banks reallocate funds towards mortgage loans in disaster-affected areas, while decreasing their funding in non-affected areas. Cortés and Strahan (2017) demonstrate that banks predominately reduce lending in non-core markets in order to serve the loan

[3]Local 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.

[4]See section 4.4 for a more detailed discussion of demand vs. supply in my setting.

demand arising in disaster affected areas, while Chavaz (2014) 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 aid[5] 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 (2014), Cortés 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 Cortés 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

[5]See section 2 for details regarding the specific flood and the subsequent government aid payments.

[6]A number of further studies use natural disasters as identification in the finance context. Lambert et al. (2015) 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) find a mitigating effect of payday lenders on foreclosures following natural disasters.

[7]Only 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.

et al. (2012) look at the effect of a natural disaster, 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'

[8]The 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 (2016) 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. (2016) provide evidence that the European sovereign debt crisis had real, firm-level effects. Gropp et al. (2016) show that higher capital requirements cause credit reductions and subsequent negative real effects in firms.

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 shocks cross international borders (Popov and Udell, 2010; 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 (2014) and Cortés 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. (2016) 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

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

[10] Often 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., 2016).

[11] For 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

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 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 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 1) and affected many major metropolitan areas, including

capital buffers also see: Chatelain et al. (2003)

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 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 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

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

[13]Usually 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.

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 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.

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, 2016).[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

[14]Total 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.

[15]The 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.

[16]Firm-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.

with firm-specific, annual financial accounts data from 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

[17]Bureau 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 from 2008.

[18]Because 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.

[19]Thus, 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.

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

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 time. As a result, firms in the data before 2008 may have self-selected into the data (Popov and Rocholl, 2016). Because firms are often not reporting for all years[22] 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.

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, 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 1). A firm in the most heavily flooded county is assigned a 9 and non-flooded counties receive a

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

[22]Despite 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.

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). 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×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 indirectly affected firms in unaffected regions.[23]

Such an *indirect* effect, as Cortés 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 around here –

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

[23]As 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.

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.

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

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 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 Cortés and Strahan (2017) and Chavaz (2014), 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

[24]For an overview of the categories, refer to Figure 1.

each firm are based on the firms' location in any of the nine damage categories reported, as shown in Figure 1. Equation 2 demonstrates how the bank-specific exposure measure is constructed.

$$\text{Exposure}_i = \sum_{j \in N_i} \left(\frac{\text{Assets}_{j,N}}{\text{Total Assets}_{N_i}} \times \text{Claim Ratio Category}_{r_j} \right) \tag{2}$$

Where N_i are the firms j of bank i located in region r_j. $ClaimRatioCategory_{r_j}$ is a value between 1-9 based on the firms' location in the counties as shown in Figure 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 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]

$$\text{IndirAffected}_j = \begin{cases} 0 & \text{if AvgExposure}_j < 2.5 \\ 1 & \text{if AvgExposure}_j \geq 4 \end{cases} \tag{3}$$

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

[26] I show in Figures 7 and 8 that the exact thresholds chosen do not matter much for the results.

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 4 provides the estimation equation, where Y_{it} 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]

$$\ln \mathrm{Y}_{jt} = \beta(\mathrm{IndirectAffected}_j \times \mathrm{Post}_t) + \alpha_j + \alpha_r \times \alpha_t \ + \sum_{k=1}^{K} \gamma_k C_{kjt-1} + \epsilon_{jt} \tag{4}$$

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, 2016). 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.

[27]The exact definition of the control variables can be found in Table OA1.

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

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. 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 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 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

[29] To 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.

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 1 around here –

– Table 2 around here –

Descriptive statistics for all the variables used in the analysis of the paper can be found in Table 1. Detailed variable definitions are provided in Table OA1 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. 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 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.

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 its origin. I thus add triple-interaction effects to my difference-in-difference analysis and estimate Equation 5 in the following way:

$$\begin{aligned} \ln \mathrm{Y}_{jt} = {} & \beta_1(\mathrm{IndirectAffected}_j \times \mathrm{Post}_t) + \beta_2(\mathrm{IndirectAffected}_j \times \mathrm{Post}_t \times \mathrm{cap}_j) \\ & + \beta_3(\mathrm{cap}_j \times \mathrm{Post}_t) + \alpha_j + \alpha_r \times \alpha_t + \sum_{k=1}^{K} \gamma_k C_{kjt-1} + \epsilon_{jt} \end{aligned} \tag{5}$$

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.

4.4 Loan supply vs. loan demand

Natural disasters tend to be interpreted as loan demand shocks from the banks' perspective (Cortés and Strahan, 2017; Koetter et al., 2016; Cortés, 2014; Chavaz, 2014). 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, 2014) 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.

5 Results

5.1 Indirect Effect

Based on previous literature and the flood characteristics presented in section 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 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 5.3.

– Figure 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 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 3).

Table 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 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 3 around here –

The results indicate that there is a statistically significant negative effect on investment by about 16 percentage points[30] 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 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

[30]While 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.

[31]I test additional dependent variables with less success. These results can be found in Table OA2 of the online appendix.

(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 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, 2016). 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.

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 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

[32] Banks 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.

[33] There 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 OA3 and Figure OA1.

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 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 5. Columns (1)-(3) in Table 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, investment[37] and fixed asset 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

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

[35] Because 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

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

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

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 5 around here –

To further investigate the transmission of shocks at different bank capital ratios, Figure 5 displays margin-plots for the continuous interactions presented in Table 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

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

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 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.

5.3 Robustness and mechanism

Robustness Next I test whether the results hold up to several robustness tests. Table 5 presents the robustness checks for Column (1) of Table 5. Robustness tests for Columns (2) and (3) can be found in the Appendix (Table OA4+OA5). 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 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 OA6 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×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 OA7, 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 OA8 of the online appendix. In all regressions, the results stay very similar to the original result.

– Table 5 around here –

– Figure 6 around here –

There may be concerns that the results are driven by choice of the affected threshold in Equation 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 5 at different thresholds and plot the resulting coefficients in Figure 7 and Figure 8. Figure 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 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

[39] In Figure 7 the upper bound remains fixed at 4, while in Figure 8 the lower bound remains fixed at 2.5.

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 7 around here –

– Figure 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 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 6 around here –

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 interactions[40] (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).

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

[40] I provide the marginsplots for the continuous interactions in Figure OA2 of the online appendix.

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

(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, 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 7 around here –

Table 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; 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, 2014) or because they tend to have larger capital or liquidity buffers they can exploit in crises.

– Table 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, 2014; Cortés and Strahan, 2017). German savings and cooperative banks are banks that are typically restricted to

[42] I provide the marginsplots for the interactions with these continuous relationship variables in Figure OA3 and Figure OA4 in the online appendix.

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 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 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 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

[43] Savings 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 seeked 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.

it could substitute with alternative financing options, such as cash reserves or its own capital. Table 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.

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 (Cortés and Strahan, 2017). As banks redirect lending from non-disaster to disaster areas, firms unaffected by the disaster, yet 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 regionals shocks can be transmitted through the banking sector to otherwise non-shocked firms, especially if the level of capital in banks

[44] Figure OA5 and Figure OA6 in the online appendix show the marginsplots for these continuous interactions.

(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, 2014), 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.

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Figures and Tables

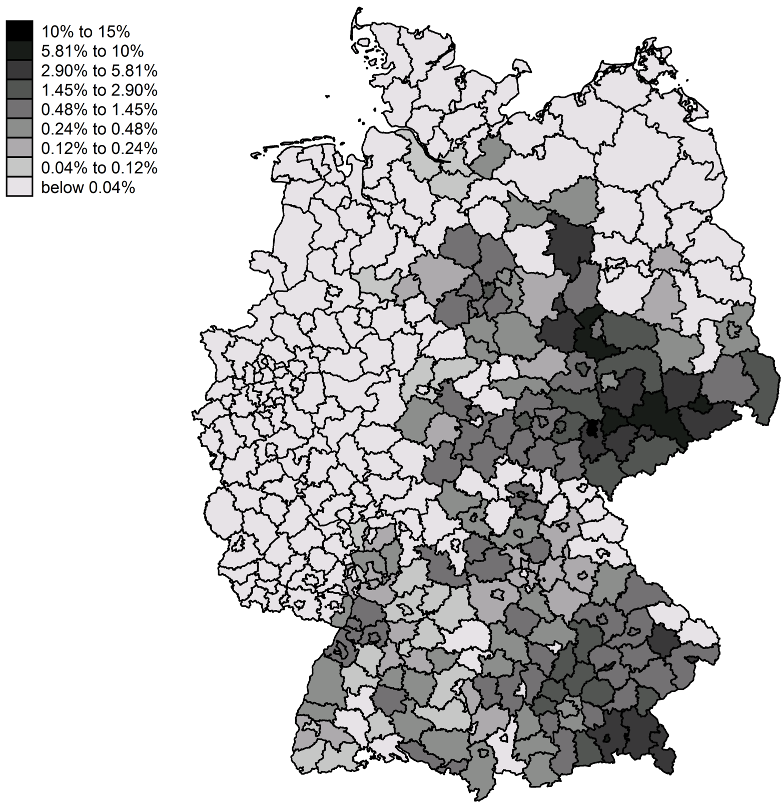


Figure 1: Affected German counties by damage categories

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.

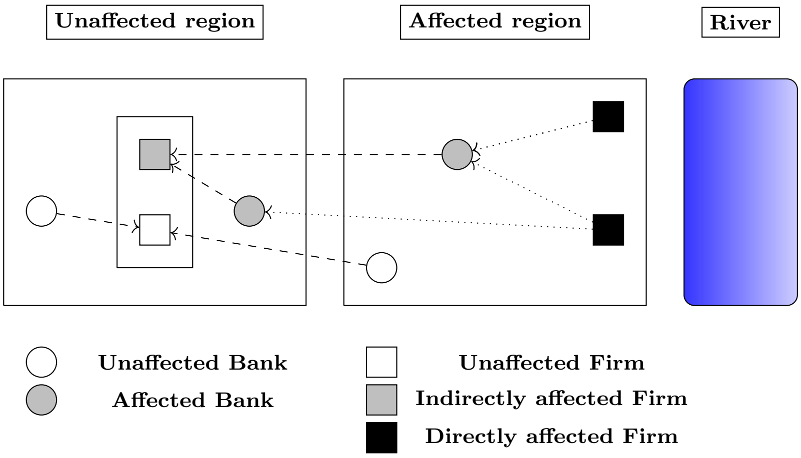
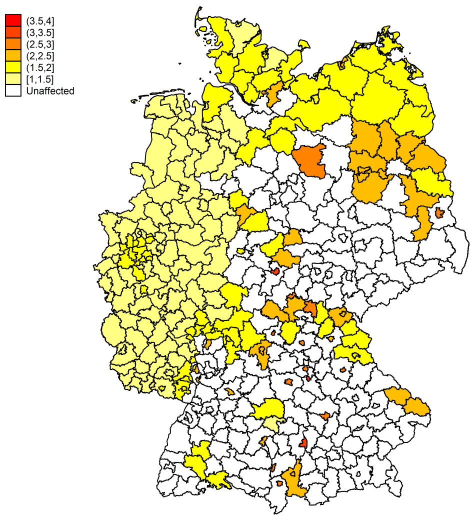
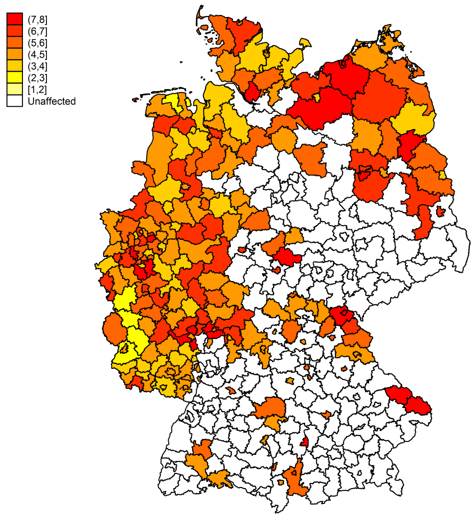


Figure 2: Indirectly affected firms: Illustration

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×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).



(a) Mean exposure of *indirectly* affected firms



(b) Maximum exposure of *indirectly* affected firms

Figure 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 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.

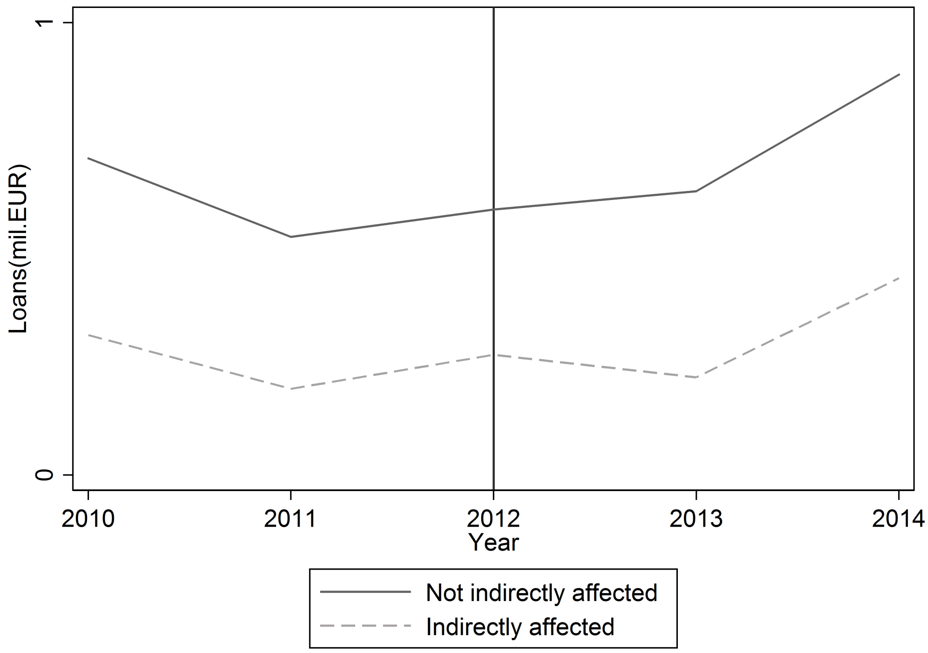


Figure 4: Development of loans by firms indirectly affected by the flood (full sample)

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 3. Values are displayed for the years 2009-2014.

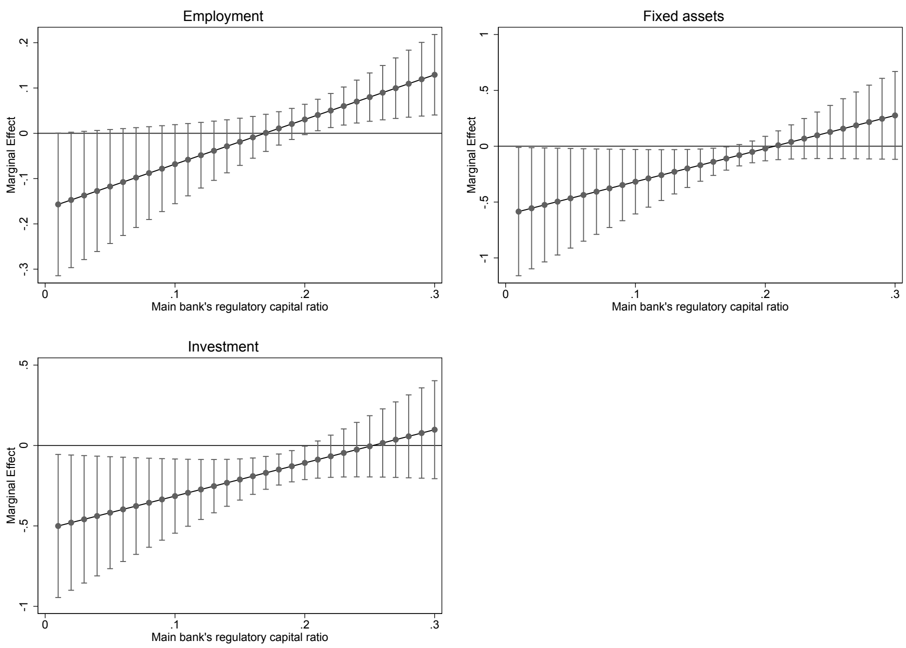


Figure 5: Marginal Effect of the interaction with the difference-in-difference coefficient at different values of main bank's capitalization: Real effects

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 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 4.

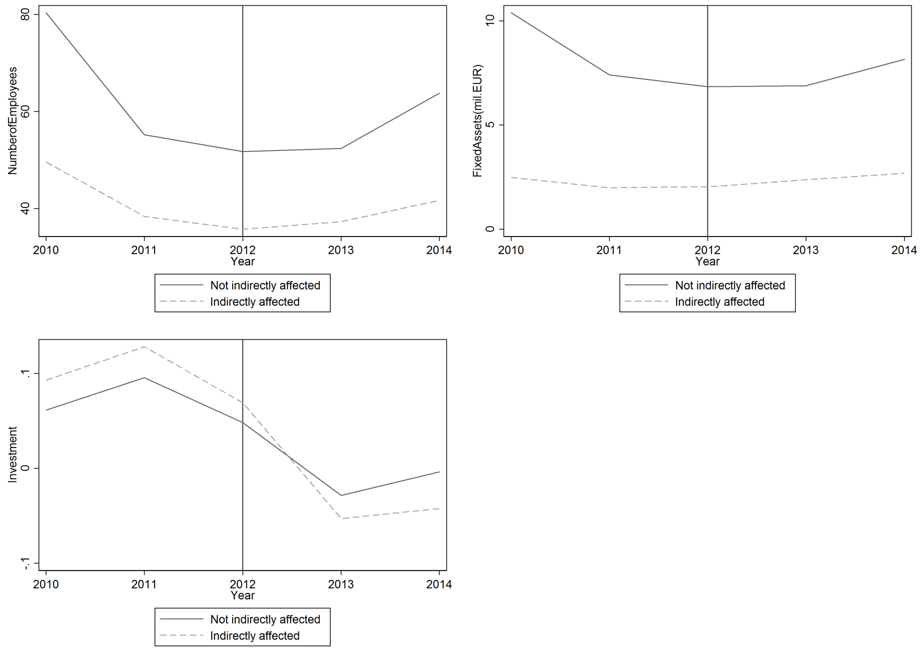


Figure 6: Parallel trends of dependent variables: Indirect effect

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.

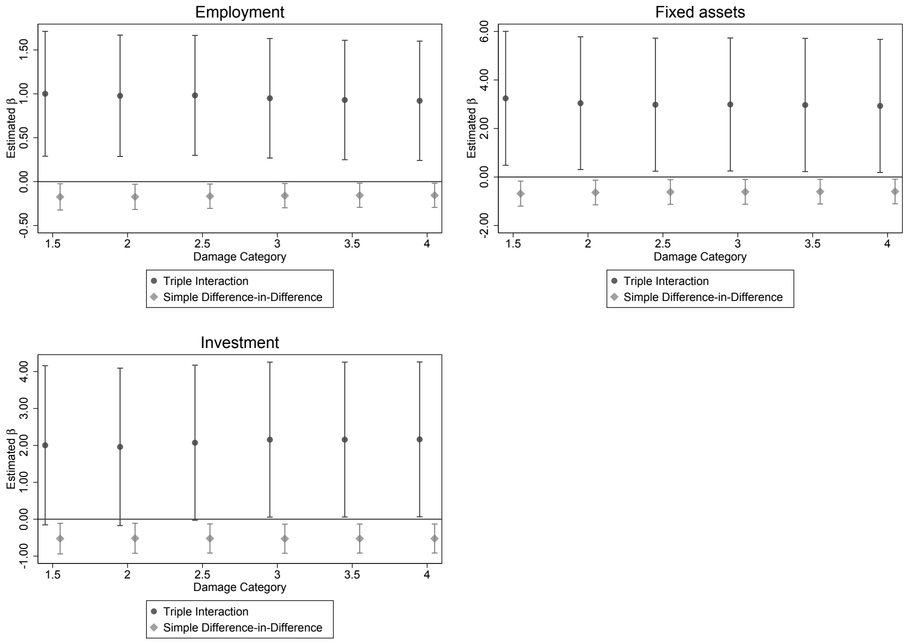


Figure 7: Varying the lower bound of the *indirectly* affected threshold

This figure displays the estimated β coefficients from Equation 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.

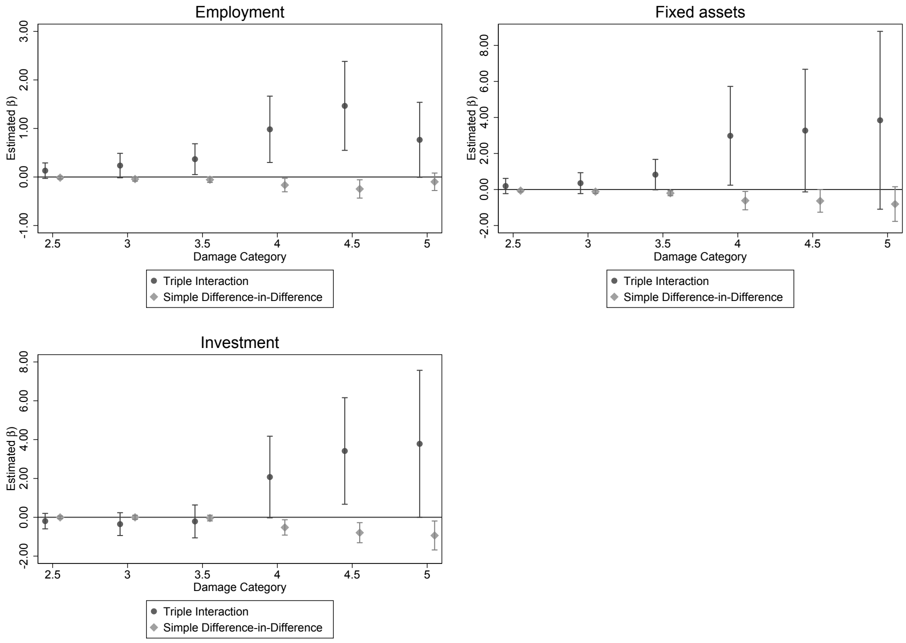


Figure 8: Varying the upper bound of the *indirectly* affected threshold

This figure displays the estimated β coefficients from Equation 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 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 1), according to Equation 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 and 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(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: 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 3). Columns (1)-(3) report statistics for all firms who are classified as unaffected (AvgExposure$_j$ < 2.5), Columns (4)-(6) for those that are omitted as buffer (2.5 < AvgExposure$_j$ < 4) and Columns (7)-(9) for those classified as indirectly affected (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(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 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	-0.162***	-0.106**	-0.002	0.012	0.028***
	(0.019)	(0.048)	(0.050)	(0.004)	(0.012)	(0.010)
L.Cash	0.001***	0.027***	0.005***	0.002***	0.031***	0.001
	(0.000)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
L.Total Assets	0.091***	-0.512***	0.388***	0.099***	-0.475***	0.411***
	(0.003)	(0.014)	(0.011)	(0.005)	(0.025)	(0.018)
L.Current Liabilities	0.000	-0.000	0.000	0.001***	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)
L.Capital	0.037***	0.244***	0.225***	0.034***	0.259***	0.269***
	(0.006)	(0.025)	(0.016)	(0.010)	(0.040)	(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 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 and 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 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***	-0.089	0.020	-0.167**	-0.521**	-0.615**
	(0.024)	(0.069)	(0.057)	(0.084)	(0.239)	(0.310)
Post×IndirAffected×lowcap	-0.111***	-0.134	-0.216**			
	(0.037)	(0.095)	(0.095)			
Post×IndirAffected×cap_pre				0.987**	2.065	2.968*
				(0.415)	(1.273)	(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 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 and 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 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)
	Employment	Employment	Employment	Employment	Employment	Employment	Employment
Post×IndirAffected	0.049**	0.062***	0.031	0.072***	0.067***	0.056**	0.051**
	(0.022)	(0.023)	(0.025)	(0.024)	(0.024)	(0.022)	(0.022)
Post×IndirAffected×lowcap	-0.090***	-0.111***	-0.006	-0.112***	-0.112***	-0.098***	-0.099***
	(0.035)	(0.038)	(0.038)	(0.037)	(0.037)	(0.037)	(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
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 (1) of Table 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 and 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 4 can be found in the Appendix.

Table 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	-0.244	-0.079	-1.152	-0.704	0.266
	(0.322)	(0.291)	(0.137)	(0.912)	(0.853)	(0.625)
Post×IndirAffected×lowcap	-0.736*	-0.005	0.118			
	(0.425)	(0.382)	(0.245)			
Post×IndirAffected×cap_pre				6.228	2.691	-1.575
				(4.814)	(4.755)	(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 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 and 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 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*	-0.064	-0.026	-0.003	-0.120	-0.016
	(0.021)	(0.061)	(0.056)	(0.039)	(0.101)	(0.104)
Post×IndirAffected×dist_pre	-0.023**	-0.055	-0.036			
	(0.011)	(0.038)	(0.029)			
Post×IndirAffected×bank_count_pre				0.005	-0.032	-0.057
				(0.020)	(0.061)	(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 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 and 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 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×IndirAffected	0.029*	-0.040	-0.070	-0.005	-0.258***	-0.151**	0.001	-0.164***	-0.070
	(0.017)	(0.063)	(0.069)	(0.029)	(0.068)	(0.070)	(0.020)	(0.049)	(0.046)
Post×IndirAffected×savings	-0.045	-0.240***	-0.048						
	(0.036)	(0.093)	(0.096)						
Post×IndirAffected×coop				0.023	0.241***	0.140			
				(0.034)	(0.090)	(0.090)			
Post×IndirAffected×comm							0.039	-0.071	-0.451
							(0.055)	(0.281)	(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 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. IndirAffected is a dummy variable constructed by measuring the exposure of the firm to the flood via its banks, according to Equation 2 and 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 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	-0.303***	-0.163*	-0.003	-0.141**	-0.114*
	(0.027)	(0.084)	(0.093)	(0.025)	(0.065)	(0.060)
Post×IndirAffected×adequacy_pre	-0.041	0.430***	0.220			
	(0.047)	(0.164)	(0.184)			
Post×IndirAffected×liq_pre				0.066	-0.131	0.110
				(0.069)	(0.337)	(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 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 and 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.

7 Appendix

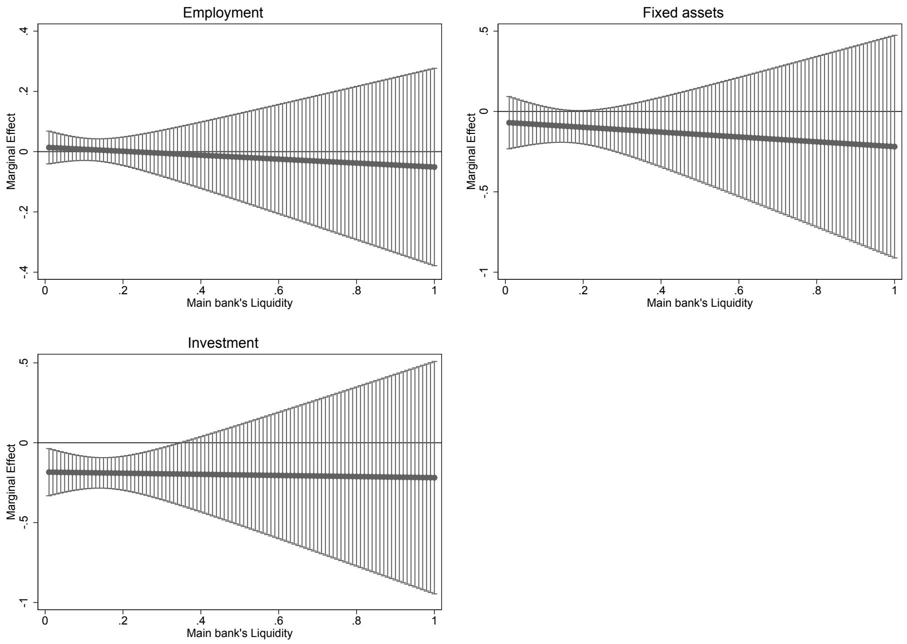


Figure OA1: Marginal Effect of the interaction with the difference-in-difference coefficient at different values of banks' Liquidity

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 OA3). 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.

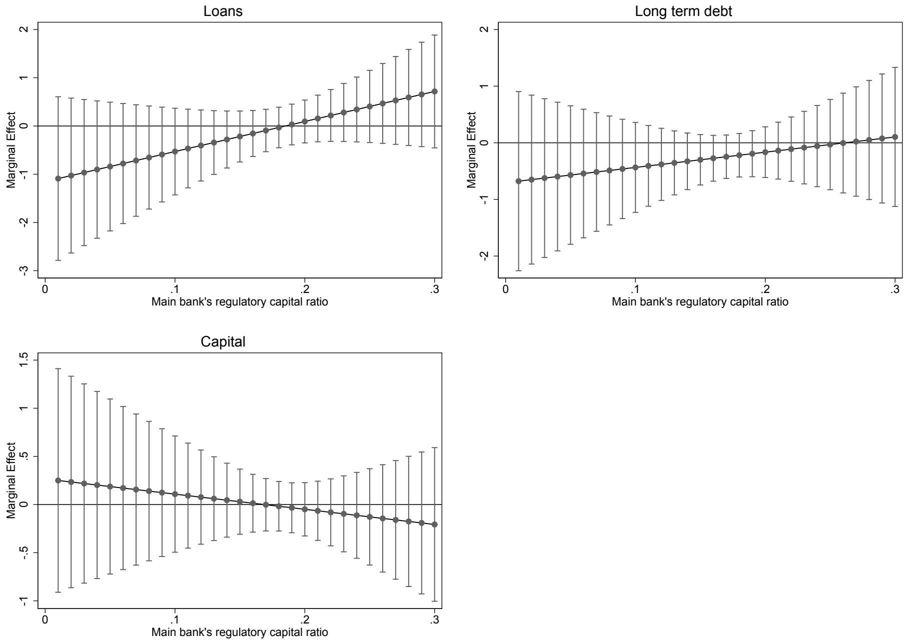


Figure OA2: Marginal Effect of the interaction with the difference-in-difference coefficient at different values of main bank's capitalization: Liabilities (channel)

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 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.

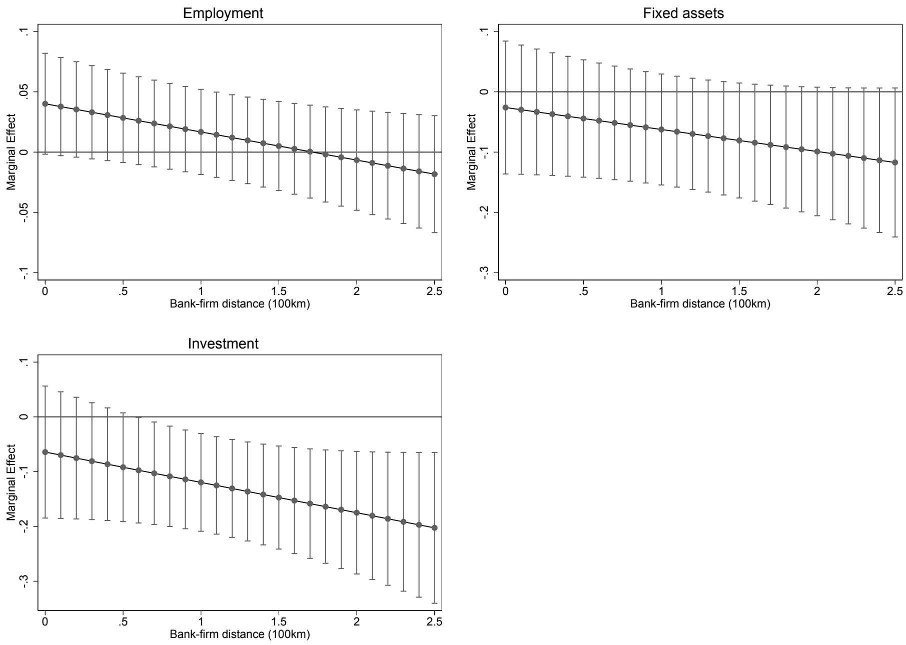


Figure OA3: Marginal Effect of the interaction with the difference-in-difference coefficient at different values of firm bank distance

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 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.

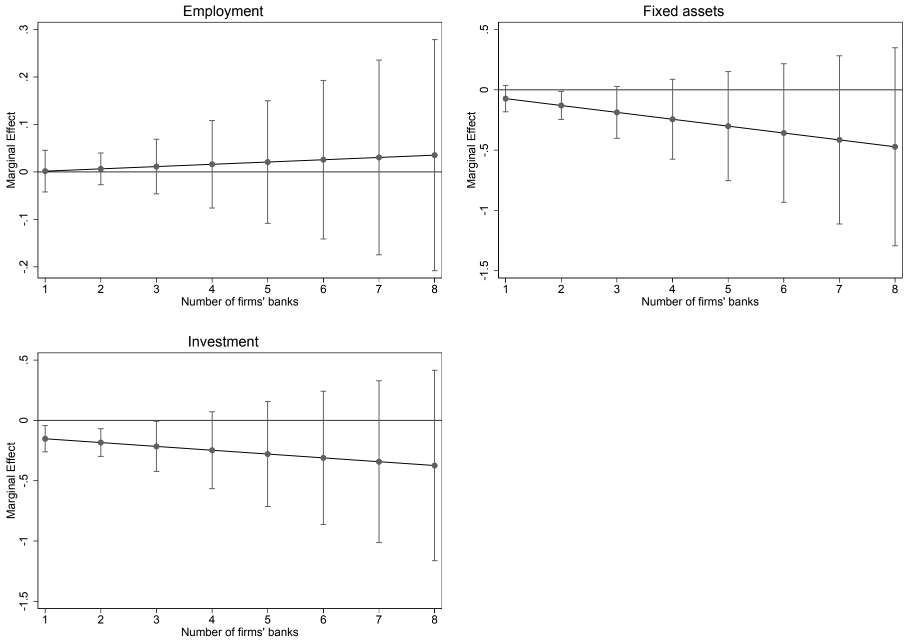


Figure OA4: Marginal Effect of the interaction with the difference-in-difference coefficient at different values of firms' bank number

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 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.

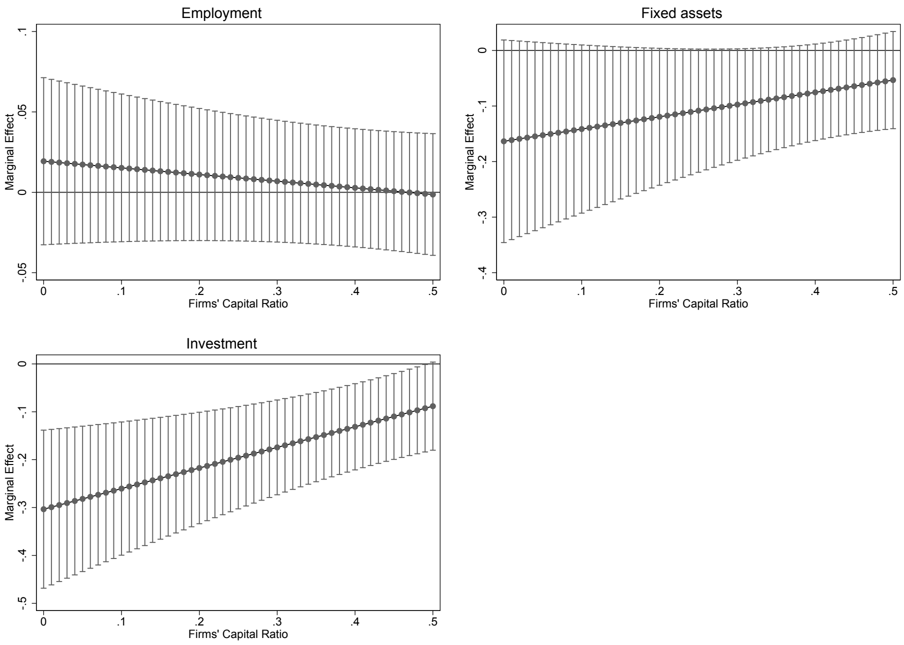


Figure OA5: Marginal Effect of the interaction with the difference-in-difference coefficient at different values of firms' capitalization

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 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.

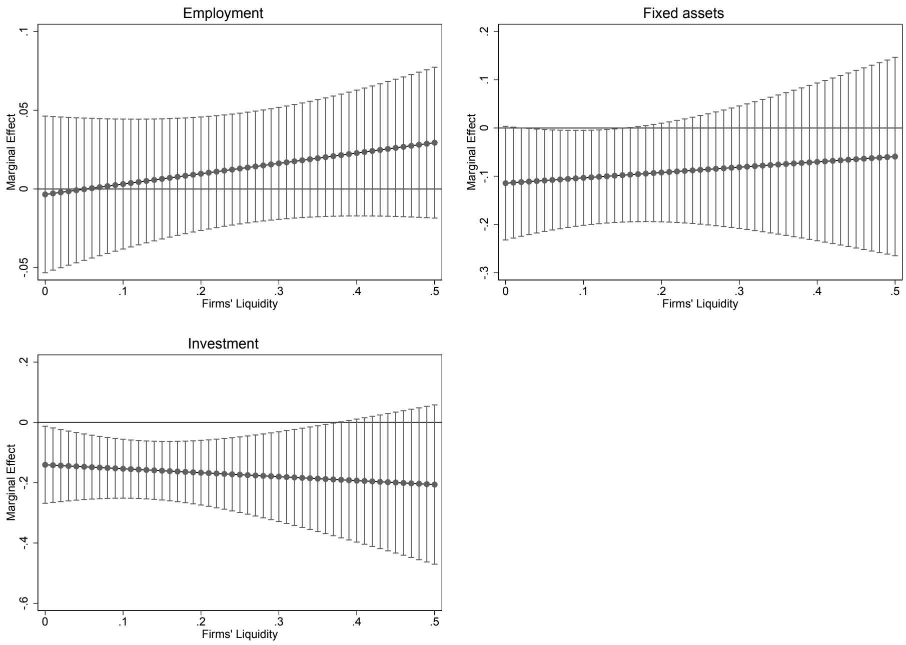


Figure OA6: Marginal Effect of the interaction with the difference-in-difference coefficient at different values of firms' Liquidity

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 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 OA1: 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 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 and 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 (lowcap)	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 OA2: Baseline regression for additional variables

	(1) TFAS	(2) Cash	(3) TOAS	(4) Prov	(5) NCAS	(6) Depr	(7) RoE	(8) RoA	(9) Capital Ratio
Post×IndirAffected	-0.025	0.116	0.008	0.087	0.015	0.082	6.786*	0.189	-0.008
	(0.063)	(0.095)	(0.022)	(0.061)	(0.059)	(0.090)	(3.755)	(1.636)	(0.008)
Post×IndirAffected×lowcap	-0.225**	0.022	0.023	0.135	0.132	-0.068	-4.288	-0.724	-0.000
	(0.099)	(0.131)	(0.032)	(0.091)	(0.085)	(0.225)	(16.386)	(3.836)	(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 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 and 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 OA3: 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	-0.215**	-0.021	0.014	-0.184**	-0.068
	(0.041)	(0.093)	(0.066)	(0.029)	(0.079)	(0.086)
Post×IndirAffected×lowliq	0.026	0.073	-0.125			
	(0.044)	(0.106)	(0.092)			
Post×IndirAffected×liq_pre				-0.065	-0.035	-0.151
				(0.189)	(0.430)	(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 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 and 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 OA4: 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	-0.123	0.130	-0.109	-0.086	-0.052	-0.054
	(0.077)	(0.076)	(0.110)	(0.068)	(0.070)	(0.064)	(0.058)
Post×IndirAffected×lowcap	-0.142	-0.088	-0.254*	-0.127	-0.132	-0.151	-0.171*
	(0.110)	(0.102)	(0.136)	(0.095)	(0.095)	(0.093)	(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 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 and 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 4 can be found in the Appendix.

Table OA5: Robustness tests for low bank capital dummy: Fixed assets

	Collapsed sample	Equal periods	Placebo	Distance	Sector×Time	Only Firm FE	No FE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Fixed Assets	Fixed Assets	Fixed Assets	Fixed Assets	Fixed Assets	Fixed Assets	Fixed Assets
Post×IndirAffected	0.034	-0.002	0.139*	0.044	0.024	0.056	0.036
	(0.060)	(0.055)	(0.081)	(0.060)	(0.057)	(0.056)	(0.054)
Post×IndirAffected×lowcap	-0.202**	-0.176*	-0.204*	-0.182*	-0.213**	-0.218**	-0.219**
	(0.098)	(0.092)	(0.113)	(0.097)	(0.095)	(0.095)	(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		
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 (3) of Table 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 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). 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 and 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 4 can be found in the Appendix.

Table OA6: 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			Low cap. dummy			Continuous Interaction		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Empl	Invest	FAssets	Empl	Invest	FAssets	Empl	Invest	FAssets	Empl	Invest	FAssets
Post×IndirAffected	0.065**	-0.122	0.012	-0.163*	-0.586**	-0.627**	0.031	-0.265**	-0.105	-0.266**	-1.011***	-0.634
	(0.026)	(0.080)	(0.062)	(0.089)	(0.247)	(0.317)	(0.043)	(0.117)	(0.106)	(0.124)	(0.325)	(0.400)
Post×Treated×IndirAffected	-0.111***	-0.149	-0.205**				-0.093*	-0.183	-0.119			
	(0.038)	(0.100)	(0.096)				(0.055)	(0.129)	(0.140)			
Post×IndirAffected×cap_pre				0.951**	2.201*	3.008*				1.365**	3.721**	2.606
				(0.430)	(1.299)	(1.711)				(0.647)	(1.634)	(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 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 and 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 OA7: 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***	-0.124*	0.018	-0.153*	-0.601**	-0.500
	(0.022)	(0.072)	(0.059)	(0.089)	(0.256)	(0.305)
Post×IndirAffected×lowcap	-0.116***	-0.148	-0.222**			
	(0.040)	(0.104)	(0.103)			
Post×IndirAffected×cap_pre				0.947**	2.347*	2.375
				(0.436)	(1.365)	(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 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 and 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 OA8: Robustness: Variation of fixed effects

	Firm & time FE			Firm FE			NO FE		
	(1) Employment	(2) Invest	(3) Fixed Assets	(4) Employment	(5) Invest	(6) Fixed Assets	(7) Employment	(8) Invest	(9) Fixed Assets
Post×IndirAffected	0.057**	-0.052	0.057	0.056**	-0.052	0.056	0.038	-0.052	0.083
	(0.022)	(0.064)	(0.056)	(0.022)	(0.064)	(0.056)	(0.037)	(0.057)	(0.077)
Post×lowcap	0.004**	0.008	0.010**	0.003*	0.008	0.010**	0.010***	0.006	0.007
	(0.002)	(0.005)	(0.005)	(0.002)	(0.005)	(0.005)	(0.003)	(0.005)	(0.006)
Post×IndirAffected×lowcap	-0.099***	-0.152	-0.219**	-0.098***	-0.151	-0.218**	-0.154***	-0.172**	-0.249**
	(0.037)	(0.093)	(0.095)	(0.037)	(0.093)	(0.095)	(0.059)	(0.087)	(0.115)
Post				0.027***	-0.060***	-0.028***	-0.009***	-0.091***	-0.104***
				(0.001)	(0.004)	(0.004)	(0.002)	(0.004)	(0.004)
IndirAffected							-0.074	0.042	-0.073
							(0.079)	(0.039)	(0.150)
lowcap							-0.017***	0.000	0.099***
							(0.007)	(0.003)	(0.009)
IndirAffected×lowcap							0.021	0.040	0.165
							(0.111)	(0.060)	(0.180)
Constant							-3.115***	0.200***	-4.269***
							(0.037)	(0.016)	(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	0.316	0.005	0.593
Within R^2	0.015	0.033	0.035	0.028	0.038	0.037			
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 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 and 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.

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