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Flooded through the Back Door: Firm-level Effects of Bank's Lending Shifts

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

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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. Low bank capital thus carries a negative externality because it amplifies regional shock spillovers. I show that bank liquidity, and firm capital and liquidity are less relevant to prevent shock transmission.

JEL classification: G21, G29, E44, E24
Keywords: natural disaster, real effects, shock transmission, bank capital

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1 Introduction

Post-crisis banking regulation has focused on inducing banks to hold more capital in order to prevent bank failure and make the banking system more stable. I demonstrate that bank capital is key in preventing real-economic spillovers from one region to another. Using a natural disaster as a shock to the real economy, I show that the disaster spreads through low-capital banks to non-disaster affected firms. The results indicate that there are positive externalities to higher bank capital, even if the stability of the financial system as a whole is not threatened, because better capitalized banks can more easily expand their balance sheets if they are faced with unexpected events.

The paper proceeds in two steps to demonstrate the effects 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).

Unconditionally, banks' lending shifts from non-disaster regions into disaster regions is small, although it entails a reduction in tangible assets by 12 % and total fixed assets by 9%. However, banks with low capital ratios significantly amplify the effect. Firms connected to low-capital banks experience a significant decrease in bank credit, reduce employment by 11% and tangible assets by 20%. Further results indicate that neither bank liquidity, nor firm capital or liquidity have the same amplification effect. I show that the effects of this regional shock amplification stemming from low levels of bank capital also affect regional GDP and unemployment rates. This result 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).

In addition to demonstrating the amplification effect of low bank capital levels, this paper also contributes to the literature on the real effects of financial shocks (Chodorow-Reich, 2014; Huber, 2018). As opposed to shocks stemming from financial markets, 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. The identification relies only on the assumption that bank-customers are unaware of their banks' 2013 disaster-exposure prior to the flood.[1] Second, while local (disaster) 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 unexpected regional demand (or supply) shocks quite frequently (Yeager, 2004). Such local shocks do not have to be natural disasters. As long as events occur that influence loan demand (or supply) and are reasonably unexpected, low bank capital levels might amplify their spillover effects.

This paper also contributes by investigating the real effects of a shock stemming from higher

[1] 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.

credit *demand* elsewhere, instead of credit supply frictions arising in financial markets.[2] My results indicate that banks reduce lending in non-disaster areas primarily because they face higher loan demand in disaster areas. This view is strongly supported by the literature, as both Chavaz (2016) and 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 demand arising in disaster affected areas, while Chavaz (2016) highlights the role of local banks diversifying through secondary markets to serve the additional demand. Similarly Koetter et al. (2016) find that German banks increase their lending in the aftermath of flooding. The demand shock interpretation can be explained by the fact that bank lending is a good complement to insurance payouts and government aid for firms in the case of a natural disaster, in order to finance necessary rebuilding efforts. The unfulfilled loan demand in the aftermath of disasters in developing countries (Choudhary and Jain, 2017; Berg and Schrader, 2012) indicates that insurance and government aid[3] 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.

This paper relates 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.[4] It builds on the results by Chavaz (2016), Cortés and Strahan (2017) and Koetter et al. (2016) who

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

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

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

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*.[5] Two studies have previously examined the effects of an *indirect* disaster shock on firms: Uchida et al. (2015) and Hosono et al. (2016) look at the effect of a natural disaster, 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 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.[6] Most of these studies rely on banks'

[5] 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.Basker and Miranda (2016) shows that especially small and unproductive firms declare bankruptcy after being hit by Hurricane Katrina, and tie part of the effect to a lack of financing availability for firms after the disaster.

[6] 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

exposure to financial market frictions, such as the exposure to the financial crisis. One major caveat here is that bank choice may not be completely orthogonal to the banks' exposure to risky international financial markets. I argue that the credit supply shock arising from a natural disaster is significantly more exogenous, because it is unexpected, especially for firms that are not directly located within the disaster regions.

Another related strand of literature is concerned with the transmission of financial shocks across markets and geographical borders. There is ample evidence that financial 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 (2016) 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. (2018) demonstrate that local shocks can also be transmitted to other market segments, by demonstrating that commercial loans are crowded out by booms in real estate markets.[7] 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.

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

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

Lastly, this paper is related to the large, yet significant discussion about the importance of bank and firm capital, especially during a crisis.[8] However, most of the literature focuses either on the bank level (Kashyap and Stein, 2000) and firm-level effects independently (Bernanke et al., 1996).[9] Jiménez et al. (2017) are the first to jointly examine the effects of bank and firm-level capitalization on credit provision. They find that bank capital matters in crisis times, and firms' capital matters in both crisis and non-crisis times. I expand on their results by showing that bank capital matter in the regional transmission of smaller real economic shocks. 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, and that it is especially important to prevent regional shock amplification.

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.,

[8]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).

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

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 major damage to the cities of Dresden, Passau, Halle (Saale) and Magdeburg.[10]

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.[11] As a result, going to a bank for flood relief and rebuilding efforts

[10]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.

[11]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

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 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

wait for insurance payments following a flood. Anecdotal evidence suggests that it is paid out within a few months, not a few weeks.

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. Total loans to non-financial corporations in Germany are roughly 800 billion Euros over the flood period. So 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. This papers' 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.[12] The former contains the name of the bank (or banks) with which each firm maintains a payment relationship (Popov and Rocholl, 2017).[13] Annual vintages of the Dafne database are used to construct a time-series of firm-bank relationships for more than a million firms between 2003 and 2014. I augment these firm-bank relationship data with firm-specific, annual financial accounts data from Amadeus.[14] 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.[15]

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 contains 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.[16] Lower categories indicate less damage relative to the asset values covered by

[12] 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.

[13] 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.

[14] 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.

[15] 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.

[16] 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.

insurance contracts.[17] The GDV collects this information from all its 460 members, which include all major German insurance providers. The data also inform the risk calculation models of insurance companies and regional aggregates are reported regularly (GDV, 2013). I merge this flood level data with the firms via their postal code.

The combination of the three datasets yields a firm-level dataset with information on each of the firms' banks, as well as the regional flood exposure of each firm based on the data from the German Insurance Association. I conduct a number of cleaning steps with the merged dataset. Initially, the dataset comprises about 1.6 million firms. After dropping firms and banks, for which no valid postal code can be matched and dropping all inactive firms, the number of firms left are roughly 870,000.[18] 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, 2017). Because firms are often not reporting for all years[19] 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.

[17]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%).

[18]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.

[19]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.

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, especially if they have little capital. 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 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.[20]

[20] 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.

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, I first identify *directly* affected firms. This is necessary for two reasons: First, the intended comparison is strictly between indirectly and not indirectly affected firms, which requires that directly affected firms be excluded. Second, the banks' disaster exposure is based on its firms' direct disaster exposure. I define *directly* affected and unaffected firms, according to their location in the flood affected counties. Specifically, firms located in counties which are ranked as category 4 or larger are classified as affected, while those that are in the lowest category (1) are classified as unaffected.[21] 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

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

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 (2016), although they use exposure to mortgage credit instead of firm customers. Specifically, the exposure measure is constructed by calculating the weighted average of the damage categories of each bank's firms, where the weight is the relative size of the firm, compared to all other firms the bank reports a payment relationship with. The damage categories for each firm are based on the firms' location in any of the nine damage categories reported, as shown in Figure 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.[22] 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

[22]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.

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.[23]

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

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).[24]

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

[23] I show in Figures 6 and 7 that the exact thresholds chosen do not matter much for the results.
[24] The exact definition of the control variables can be found in Table OA1.

I initially choose four key dependent variables[25] – Y_{jt} – in order to estimate the impact on the firms' real performance. First, I investigate the amount of loans taken by the firm. This variable is important, because presumably bank lending reduction occur via loan reductions. However the data does not allow separating (specific bank) loans from other loans taken by the firm. Next, I investigate the firms two main input factors: labor and capital. The second dependent variable is thus the number of employees of the firm (in logs). It is a key measure of firm performance and traditionally highly important from a policy perspective (Chodorow-Reich, 2014; Popov and Rocholl, 2017). In addition to employment, firms can also reduce their capital input if they are faced with a funding reduction from banks. I specifically test tangible fixed assets and fixed assets separately.

Crucially, in these estimations 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.[26]

[25] I additionally test other variables that are related to firm health. The results can be found in Table OA2.

[26] To the extent that firms' bank choice may not be orthogonal to the firms flood exposure, for example

– 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 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

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 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). However, there has not been much attention toward the fact how bank capital affects regional shock spillovers. However, the same mechanisms that cause a general reduction in lending during financial crisis might amplify regional spillovers. For smaller shocks, banks can reduce lending in certain regions if they lack sufficient capital. This spillover effect should be significantly affected by the banks ability to buffer even smaller shocks to its balance sheet with equity. Concretely, two factors may cause lower capital banks to amplify regional spillovers: first, banks with lower capital ratios might have more trouble refinancing loans on the interbank market, as they are perceived as more risky. I term this mechanism the *risk channel*. 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 capital regulations. I term this the *regulatory channel*. A key part of this paper is to contribute to the understanding of whether bank capital is important for the transmission and amplification of unexpected regional shocks and to get some idea about the channels through which it might work. I thus add triple-interaction effects to my difference-in-difference analysis and estimate Equation 5 in the following way:

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

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.

Banks' capital regulation in Germany follows EU regulation under Basel III. The total regulatory capital requirement was set to 8% in 2013, and Tier 1 capital had to be raised from 4.5 to 6% until 2019. In addition banks have to build a conservation buffer of 2.5%, increasing the total capital requirement in normal times to 10.5% by 2019. The minimum amount of regulatory capital held by banks in my sample is 8% (Table 1), which is exactly the minimum capital requirement for the years 2013-2015. At 8% regulatory capital, banks cannot extend new loans to firms without raising equity without violating EU regulation. However, the mean bank in the sample holds twice as much capital. Because many German banks are local savings and cooperative banks, they tend to hold a little bit more capital than large commercial banks. In addition, banks are likely to hold an internal capital target ratio that is in excess of the regulatory minimum (Berger et al., 2008; Francis and Osborne, 2012; Lepetit et al., 2015), which may be binding and prevent significant lending expansions. This implies that the *regulatory channel* is difficult to identify, because the binding effect of regulation may be different fro each bank, depending on their internal capital buffers.

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

The *supply* shock interpretation would imply that banks cut their lending elsewhere, because they have to write-off loans in the affected areas, and might thus be induced to sell other assets quickly to compensate for the losses. A *demand* shock would result in the flood-exposed bank having to raise additional funds in order to satisfy demand in the affected area. The bank can do this by either refinancing the newly demanded loans (Chavaz, 2016) or by cutting lending elsewhere. The demand shock interpretation is heavily supported by the literature on the bank level, and none of the results in this paper suggest another interpretation. Thus, I choose to interpret the results as a negative funding shock stemming from an increase in demand, although the supply channel cannot be ruled out and it is plausible that both mechanisms are at work at the same time.

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, especially when banks hold little capital. 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).[27] 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

[27]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.

banks exposed to the disaster.

I examine whether firms' banks' flood exposure matters to the firms' loans as reported on the firms' balance sheet. 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). Columns (1)-(4) report the results for firms located *outside* the flood radius, i.e. firms classified as not directly affected according to Equation 1. Columns (5)-(8) 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 –

First, I examine whether firms' banks' flood exposure matters to the firms' loans as reported on the firms' balance sheet. Being indirectly exposed to the flood does not appear to significantly affect firm borrowing. Although the coefficient in column (1) of Table 3 is negative, it is not statistically significant. Does this shift of bank lending away from unaffected regions translate into firm-level real effects? The results indicate that there is a drop in tangible fixed assets by 11% and fixed assets by 9% 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 flood event. The results indicate that while firms reduce tangible fixed assets if their banks reallocate lending away from them, these effects may not be sufficient to cause changes in employment.[28]

Columns (5)- (8) of Table 3 give some indication that firms *inside* of the flooded regions are indeed benefiting from a shift of bank lending. The coefficients of investment and fixed assets have the expected positive sign (as banks channel more funds into the affected areas) and the latter is 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.

It may be surprising that there are no statistically significant effects for borrowing and employment but significant effects for (tangible) fixed assets. However, the loans variable is very broadly defined in the firm data, and includes loans from non-banks. As a result the disaster may not induce enough variation for the effect to be significant. Also note that if firms switch lenders due to the disaster this might be costly, even if the absolute number of loans remains unchanged. Employment decisions of firms are likely to be more rigid than changes in the firms' fixed assets, especially in a country with a 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. A 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-

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

Reich, 2014; Popov and Rocholl, 2017). Instead, smaller funding shocks, such as those from the Elbe flood to indirectly affected firms, can be buffered by firms without any implications for employment, despite the fact that they in fact induce a reduction of the capital stock. Importantly, it indicates the Elbe flood itself spreads only little through the banking system into other areas, if the shock can be buffered by banks.

5.2 Transmission of shocks and bank capitalization

The effect of banks' lending shift following natural disasters from unaffected to affected regions may be dependent on the amount of bank capital available. 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).[29] 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 (*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 (*regulatory channel*). Importantly, banks do not need to be exactly at the threshold for this effect to take hold,

[29] 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 7 and Figure OA1.

as they may choose to hold a (fixed) buffer above the regulatory requirement for other liquidity related reasons. Both of these channels imply that low-capital banks have to cut back more lending to out of region firms, if they are face with a regional shock. 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 –

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)-(4) 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.[30] I find that indirectly affected firms whose banks holds below median capital, experiences a differential lending reduction of an extraordinary 70%. This effect is significant at the 10% level.[31] This result provides a strong indication that firms experience a reduction in bank lending as a result of regional lending redistributions from banks. Significantly, this reduction affects firms input factors: employment outcomes are significantly larger for firms whose main bank holds little capital. These firms reduce employment by roughly 17% more than their high-bank-capital counterparts. Since the mean firm has 57 employees, this implies that the mean firm releases 9-10 employees more than their high-bank capital counterpart. [32] A reduction of capital inputs – both in terms of tangible and overall fixed assets – are also exclusively caused by low-capital banks. Indirectly affected firms connected to low-capital banks reduce their (tangible) fixed assets by 20% more than their better capitalized counterparts.

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

[31]The effect is also not very robust. See table OA5 for details.

[32]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: 0.062+0.107=0.169.

Columns (5)-(8) 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 lending, employment, and (tangible) fixed asset effects, balancing the negative effect of the simple difference-in-difference estimate. [33] With a negative baseline employment effect of 16%, an increase in the main banks' capital ratio by 1 percentage point reduces this effect by about 0.91%. This means that for banks at the margin of the EU tier 1 capital requirement of 6%, the reduction in associated firms' employment would be 11%.[34] The effects on loans and (tangible) fixed assets are significantly steeper.

– Figure 4 around here –

To further investigate the transmission of shocks at different bank capital ratios, Figure 4 displays margin-plots for the continuous interactions presented in Table 4. As higher regulatory capital ratios imply larger (differential) borrowing, employment and capital stock effects, the slope of all 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 the firms' capital 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 ultimate firm outcomes.

[33] Only the employment interaction is statistically significant, although the other effects are close to being significant also(p-values: 0.163, 0.026, 0.104, 0.122).

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

Overall these results clearly indicate that banks' capital ratios are extremely important in determining whether regional shocks are buffered or amplified by banks. 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. All negative firm-level effects increase significantly if the firms' main bank is constrained by a low capital ratio. It is not clear if higher mandatory capital requirements are a good solution to this problem, as my results suggest that firms reduce (tangible) fixed assets most, if their bank is constrained by mandatory capital requirements. Since my shock is not a macro-scale shock, even capital requirements tied to macroeconomic conditions, such as the conservation buffer 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.

I further illustrate this with the help of a regional regression. Table 5 shows estimates for effects of higher regional average bank capital on outcomes on the German county level. In this regression, I split counties into indirectly affected and unaffected counties around the median affectedness of firms in that counties. Counties above the median in indirect disaster exposure are classified as affected. I then estimate a post-disaster difference in difference model and interact it with the average level of the firms' banks' capital. The results confirm the firm-level outcomes. Affected counties have higher, post disaster unemployment rates (column (1)) and lower GDP (column (2)). However higher average capital significantly buffers this effect. The more bank-capital there exists in the system, the less the negative impacts spread around to other regional economies. Insolvencies and public debt are unaffected by the regional spillover of the flood.

This demonstrates that the effects of higher bank capital buffering regional spillovers can not only prevent negative effects on individual firms but even to the regional economy. Thus, because regional shock spillovers can be mitigated by higher bank capital ratios, my results imply a previously disregarded benefit – a positive externality – of higher bank capital. Since unexpected regional negative shocks may occur quite often, this externality may have significant macroeconomic effects, although this area might require further research.

5.3 Robustness and mechanism

Robustness Next I test whether the results hold up to several robustness tests. Table 6 presents the robustness checks of the employment effects for Column (2) of Table 4. Robustness tests for Columns (1), (3) and (4) can be found in the Appendix (Table OA5, OA6 and OA7). 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 5 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.

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

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. Lastly, I check whether the results might be driven by a few banks that have extraordinary high or low capital ratios. Column (8) uses winsorized bank capital at the 5% level, to ensure that this is not the case. Indeed the results remain very similar, indicating that the results are not driven by extreme banks in the data.

– Table 6 around here –

– Figure 5 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 6 and Figure 7. Figure 6 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 all β's vary very little under different lower bound threshold choices. Figure 7 similarly plots β_1 and β_2, while varying the upper bound of the indirectly affected group.[35] For all dependent variables, fixing the upper bound too close to the lower bound - i.e. the control group - will result in insignificant results. 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 6 around here –

– Figure 7 around here –

5.4 Other potential shock amplifiers

The results of the previous section provide strong indication that low levels of bank capital amplify regional shock spillovers. I next investigate whether this amplification effect occurs

[35]In Figure 6 the upper bound remains fixed at 4, while in Figure 7 the lower bound remains fixed at 2.5.

only for bank capital, or whether there are other factors affecting spillovers. First, I test whether banks liquidity is relevant. I proxy bank liquidity as the share of liquid assets over total assets, and use the pre-flood value of the main bank as the indicator for bank liquidity. Table 7 indicates that banks' liquidity plays no role in the transmission of shocks. Both the triple interaction with a median-split dummy (columns (1)-(4)) and the continuous interactions (columns (5)-(8)) show no significant results.

– Table 7 around here –

The regional transmission of shocks to the real economy might also depend on the financial constraint of individual firms. In fact, if firms do not face any financial constraints, a reduction in bank credit by their banks as a result of loan reallocation following natural disasters should not matter to the firm at all, as it could substitute with alternative financing options, such as cash reserves or its own capital. Table 8 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)). There are few indications that firms' capital is important. Significant effects of higher firm capital can only be detected for (tangible) fixed assets (column (3)). Additionally, firm liquidity appears to matter for the borrwoing ability of firms (column (4)). [36] These results are somewhat surprising, given that firm capital has been found to matter for real effects in crisis times (Jiménez et al., 2017). However, regional shock transmission appears to be somewhat independent from firm factors and rather depend on the ability of banks to extend loans in emergency situations.

– Table 8 around here –

[36] Figure OA4 and Figure OA5 in the online appendix show the marginsplots for these continuous interactions.

6 Conclusion

This paper investigates the effect of an exogenous bank funding shock on firms' real outcomes in terms of borrowing, employment, and the capital stock. 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 their capital stock by 9-12%. This baseline effect is significantly amplified by low-levels of bank capital. 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 borrowing, which leads the mean firm in the sample to release 9-10 employees more than firms connected to above-median capital banks. 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 bank capital 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 borrowing, employment, and capital inputs, 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.

My results imply the importance of high bank capital ratios, not only to prevent bank failure and a systematic collapse 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 and the regionals economy suffer real consequences if banks do 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

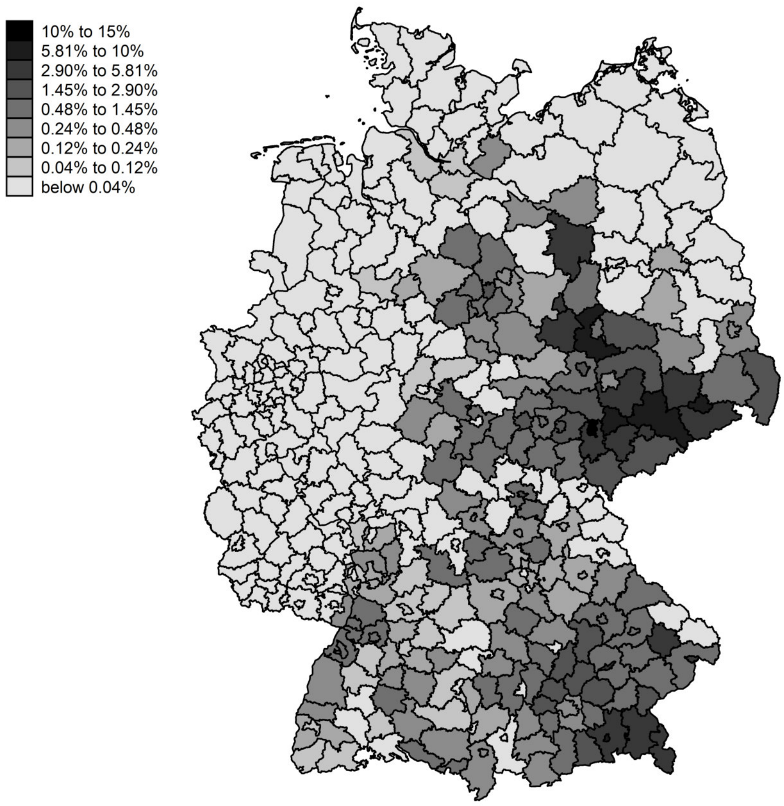


Figure 1: Affected German counties by damage categories

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.

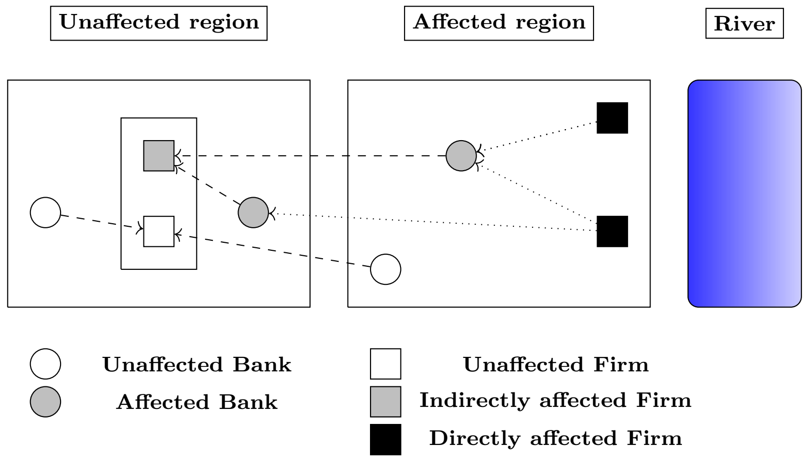
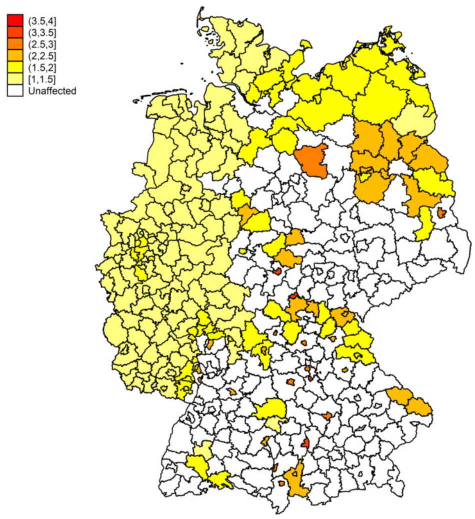
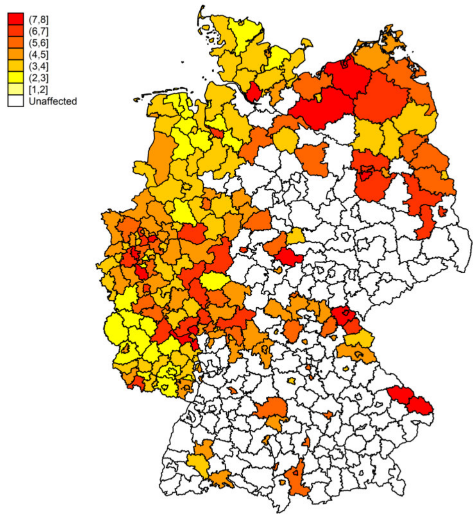


Figure 2: Indirectly affected firms: Illustration

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.

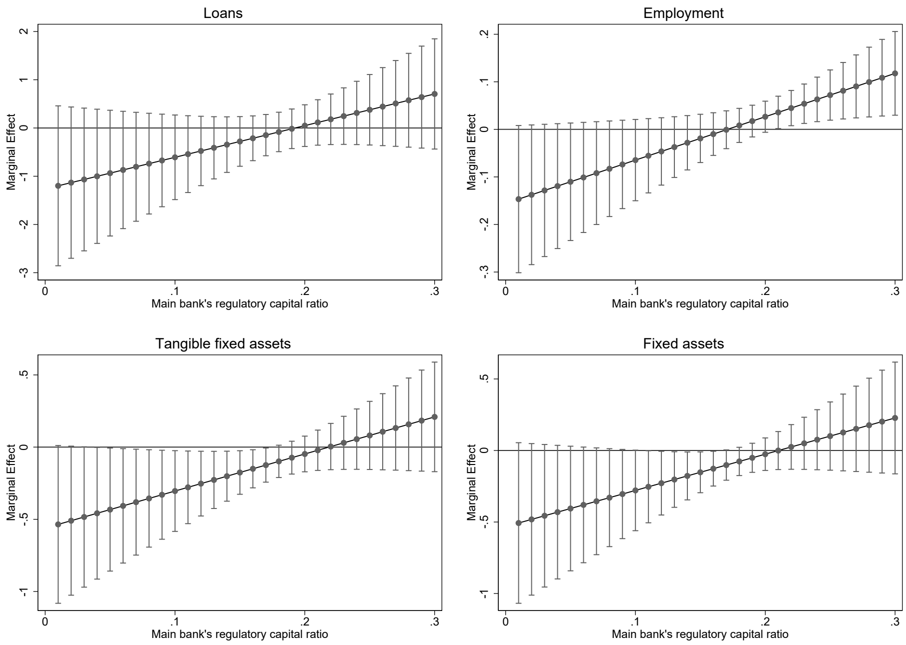


Figure 4: Marginal Effect of the interaction with the difference-in-difference coefficient at different values of main bank's capitalization: Real effects

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.

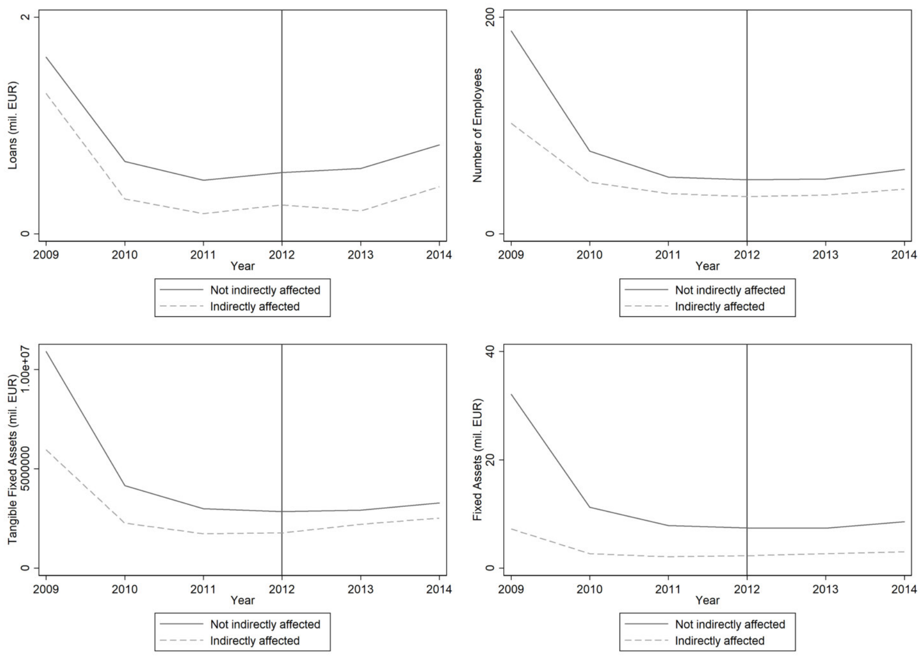


Figure 5: Parallel trends of dependent variables: Indirect effect

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.

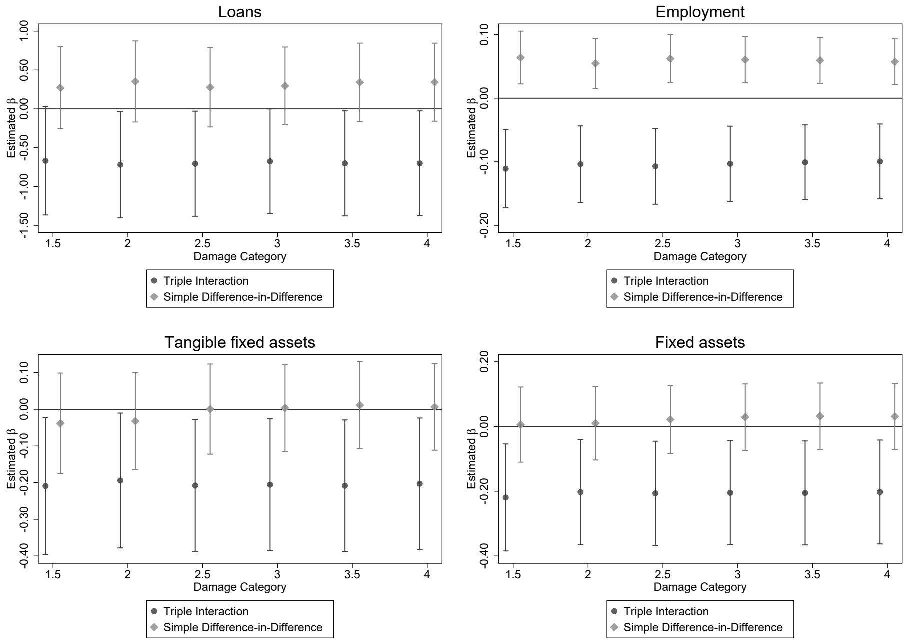


Figure 6: Varying the lower bound of the *indirectly* affected threshold

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.

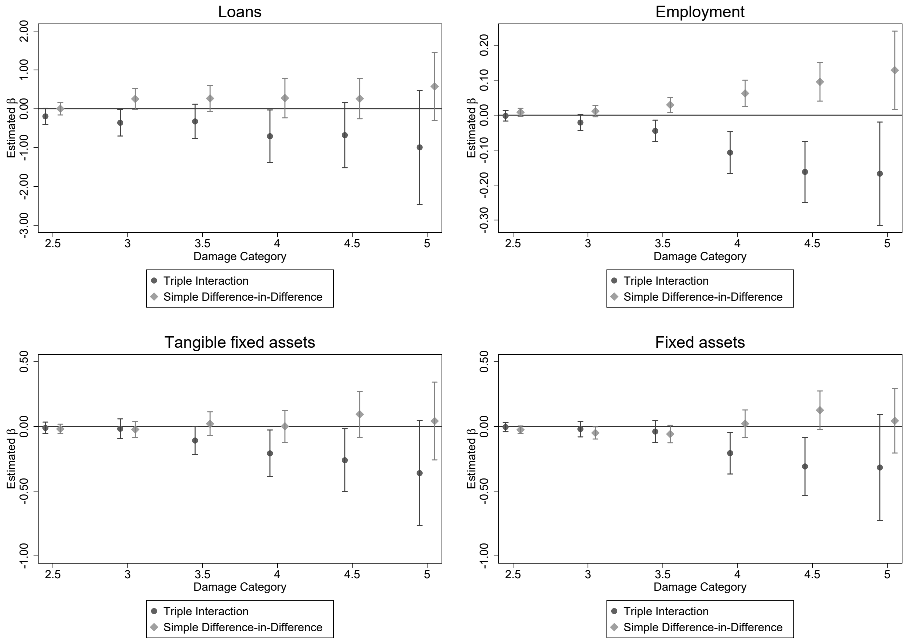


Figure 7: Varying the upper bound of the *indirectly* affected threshold

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	712365	0.31	0.46	0.00	1.00
IndirAffected	701773	0.16	0.37	0.00	1.00
Dependent Variables					
Loans (mil. EUR)	534667	0.64	13.50	0.00	3185.00
Number of Employees	895565	57.11	929.40	1.00	276418.00
Tangible Fixed Assets (mil. EUR)	895565	3.49	67.19	0.00	19953.00
Fixed Assets (mil. EUR)	895565	8.09	268.52	0.00	45448.00
Control Variables					
L.Cash(mil.EUR)	895565	1.02	24.95	0.00	7089.00
L.Total Assets (mil. EUR)	895565	14.09	409.40	0.00	85276.00
L.Capital Ratio	895565	0.34	0.27	0.00	1.00
L.Current Liabilities (mil. EUR)	895565	3.75	142.62	0.00	28261.00
Firms' banking characteristics					
Main banks' reg capital ratio (cap_pre)	895565	0.17	0.04	0.08	0.78
Distance to main bank (100 km) (dist_pre)	865453	0.87	1.33	0.00	7.92
Number of banks per firm (bank_count_pre)	891039	1.69	0.89	1.00	7.00
Savings Bank dummy (savings)	891039	0.46	0.50	0.00	1.00
Cooperative Bank dummy (coop)	891039	0.22	0.42	0.00	1.00
Commercial Bank dummy (comm)	891039	0.28	0.45	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. 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													
Loans (mil. EUR)	173877	82667	0.60	13.94	11340	5394	0.88	6.43	647	340	0.33	1.57	0.48
Number of Employees	268490	109071	57.47	899.54	17397	7108	71.02	607.41	1109	479	44.85	185.87	0.47
Tangible Fixed Assets (mil. EUR)	268490	109071	3.27	67.98	17397	7108	4.53	27.49	1109	479	2.08	12.18	0.59
Fixed Assets (mil. EUR)	268490	109071	7.41	245.92	17397	7108	8.50	66.04	1109	479	2.83	15.62	0.62
Control Variables													
L.Cash (mil. EUR)	268490	109071	0.98	18.69	17397	7108	1.42	22.46	1109	479	0.55	4.81	0.77
L.Total Assets (mil.EUR)	268490	109071	13.51	361.20	17397	7108	15.79	118.82	1109	479	5.32	29.78	0.75
L.Capital Ratio	268490	109071	0.32	0.26	17397	7108	0.33	0.26	1109	479	0.32	0.27	0.69
L.Current Liabilities (mil.EUR)	268490	109071	3.67	136.63	17397	7108	3.87	36.10	1109	479	1.34	9.19	0.57
Firms' banking characteristics													
Main banks' reg capital ratio (cap_pre)	268490	109071	0.17	0.04	17397	7108	0.17	0.05	1109	479	0.17	0.04	-7.19
Distance to main bank(100 km)(dist_pre)	262729	106440	0.68	1.10	16057	6322	1.12	1.45	1088	468	1.46	1.59	-23.34
Num of banks per firm (bank_count_pre)	267973	108726	1.71	0.91	16727	6578	1.72	0.91	1105	477	1.29	0.62	15.18
Savings Bank dummy (savings)	267973	108726	0.51	0.50	16727	6578	0.29	0.45	1105	477	0.54	0.50	-2.31
Cooperative Bank dummy (coop)	267973	108726	0.20	0.40	16727	6578	0.27	0.45	1105	477	0.39	0.49	-15.44
Commercial Bank dummy (comm)	267973	108726	0.28	0.45	16727	6578	0.34	0.47	1105	477	0.05	0.22	17.14

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)-(4) report statistics for all firms who are classified as unaffected ($\text{AvgExposure}_j < 2.5$), Columns (5)-(8) for those that are omitted as buffer ($2.5 < \text{AvgExposure}_j < 4$) and Columns (9)-(12) for those classified as indirectly affected ($\text{AvgExposure}_j \geq 4$). Column (11) reports the results of a difference in means test, between unaffected and affected firms, t-statistics are reported. 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) Loans	(2) Employment	(3) Tangible Assets	(4) Fixed Assets	(5) Loans	(6) Employment	(7) Tangible Assets	(8) Fixed Assets
Post×IndirAffected	-0.125	0.003	-0.117**	-0.094*	-0.065	-0.001	0.014	0.028**
	(0.215)	(0.020)	(0.059)	(0.052)	(0.069)	(0.004)	(0.016)	(0.013)
L.Cash	-0.031***	0.002***	0.012***	0.004***	-0.018	0.002***	0.005**	-0.001
	(0.007)	(0.000)	(0.002)	(0.001)	(0.013)	(0.001)	(0.002)	(0.002)
L.Total Assets	0.212***	0.091***	0.394***	0.402***	0.239***	0.097***	0.394***	0.401***
	(0.030)	(0.003)	(0.012)	(0.012)	(0.053)	(0.005)	(0.020)	(0.019)
L.Current Liabilities	0.011***	0.000***	0.001**	-0.000	0.016***	0.001***	0.001	0.001
	(0.002)	(0.000)	(0.000)	(0.000)	(0.004)	(0.000)	(0.001)	(0.000)
L.Capital Ratio	-0.607***	0.025***	0.128***	0.233***	-0.778***	0.017	0.194***	0.249***
	(0.084)	(0.007)	(0.025)	(0.019)	(0.156)	(0.011)	(0.041)	(0.035)
N	256,421	458,782	458,782	458,782	83,069	139,164	139,164	139,164
Number of Firms	74,046	108,954	108,954	108,954	27,915	33,067	33,067	33,067
Treatment Group	389	477	477	477	21,924	26,023	26,023	26,023
Within R^2	0.001	0.015	0.023	0.033	0.002	0.020	0.026	0.035
Controls (lagged)	YES	YES	YES	YES	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
County×Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES

This table presents the results of the indirect effect of flooding on firms for four different outcomes: Loans, employment, tangible assets and fixed assets. 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)-(4) and for firms inside the disaster area (Columns (5)-(8)). 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 is the log of firm borrowing. Employment is the log of the number of firms' employees. Tangible Assets is the log of firms' tangible fixed assets. Fixed Assets is the log of firms' fixed assets. 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: Amplifying the shock: Main bank's capital buffer

	Low capitalization dummy				Continuous Interaction			
	(1) Loans	(2) Employment	(3) Tangible Assets	(4) Fixed Assets	(5) Loans	(6) Employment	(7) Tangible Assets	(8) Fixed Assets
Post×IndirAffected	0.277	0.062***	0.000	0.022	-1.264	-0.156*	-0.561*	-0.532*
	(0.310)	(0.023)	(0.075)	(0.064)	(0.892)	(0.083)	(0.294)	(0.302)
Post×IndirAffected×lowcap	-0.707*	-0.107***	-0.208*	-0.207**				
	(0.411)	(0.036)	(0.110)	(0.098)				
Post×IndirAffected×cap_pre					6.567	0.912**	2.567	2.532
					(4.706)	(0.410)	(1.579)	(1.637)
N	272,779	458,783	458,783	458,783	272,779	458,783	458,783	458,783
Number of Firms	90,404	108,954	108,954	108,954	90,404	108,954	108,954	108,954
Treatment Group	389	477	477	477	389	477	477	477
Triple Interaction Group	211	261	261	261				
Within R^2	0.001	0.015	0.023	0.033	0.001	0.015	0.023	0.033
Controls (lagged)	YES	YES	YES	YES	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
County×Year Fixed Effects	YES	YES	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)-(4) 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 (5)-(8) 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 is the log of firm borrowing. Employment is the log of the number of firms' employees. Tangible Assets is the log of firms' tangible fixed assets. Fixed Assets is the log of firms' fixed assets. 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: Regional amplification effects of low bank capital after a shock

	(1) Unemployment (%)	(2) log(GDP)	(3) Insolvencies	(4) Public Debt
Post×Indirectly Affected	2.758**	-0.113*	-176.052	0.166
	(1.321)	(0.066)	(205.050)	(0.493)
Post×avg_cap_pre	-7.658	0.004	221.168	2.935
	(5.194)	(0.232)	(529.121)	(1.934)
Post×Indirectly Affected×avg_cap_pre	-16.953**	0.750**	962.819	-2.324
	(7.781)	(0.379)	(1041.041)	(2.808)
N	1,652	1,521	1,485	1,464
Number of Firms	169	169	169	166
Treatment Group	85	85	85	84
Within R^2	0.100	0.033	0.006	0.027
Firm Fixed Effects	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES

This table presents the results of county level regressions indicating the effect of low bank capital on post-disaster regional performance. Unemployment is the regional unemployment rate in %. log(GDP) is the natural logarithm of per capita regional GDP. Insolvencies are the absolute number insolvencies. Public Debt is the log of public debt in the county. Indirectly affected is a dummy variable set equal to 1 if the county's share of indirectly affected firms is larger than the median and 0 otherwise. Only non-directly affected (non-flooded) counties are considered. The continuous variable avg_cap_pre captures the mean level of bank capital held by firms' banks in the county, prior to the flood in 2012. Post is a dummy set equal to 1 after the disaster year (2013) and 0 otherwise. I control for County and time 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 6: Robustness tests for low bank capital dummy: Employment

	Collapsed sample	Equal periods	Placebo	Distance	Sector×Year	Only Firm FE	No FE	Winsorized capital
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Employment	Employment	Employment	Employment	Employment	Employment	Employment	Employment
Post×IndirAffected	0.045**	0.058**	0.032	0.069***	0.063***	0.051**	0.046**	0.062***
	(0.021)	(0.023)	(0.025)	(0.024)	(0.023)	(0.022)	(0.022)	(0.023)
Post×IndirAffected×lowcap	-0.086**	-0.105***	-0.011	-0.108***	-0.108***	-0.094***	-0.094***	-0.107***
	(0.034)	(0.037)	(0.038)	(0.037)	(0.036)	(0.036)	(0.036)	(0.036)
Post×dist_pre				-0.003***				
				(0.001)				
N	217,908	391,972	268,711	448,580	458,783	458,783	458,783	458,783
Number of Firms	108,954	108,954	108,954	106,329	108,954	108,954	108,954	108,954
Treatment Group	477	477	477	466	477	477	477	477
Within R^2		0.011	0.007	0.001	0.015	0.028		0.015
Controls (lagged)	YES	YES	YES	YES	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	YES	YES	YES	YES	NO	YES
County×Year FE	YES	YES	YES	YES	YES	NO	NO	YES

This table presents robustness tests for the results presented in column (2) 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. Column (8) is estimated with the main banks' capital winsorized at the 5% level. 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. 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 7: Other potential shock amplifiers: Main bank's liquidity

	Low capitalization dummy				Continuous Interaction			
	(1) Loans	(2) Employment	(3) Tangible Assets	(4) Fixed Assets	(5) Loans	(6) Employment	(7) Tangible Assets	(8) Fixed Assets
Post×IndirAffected	0.138	-0.014	-0.023	-0.024	-0.533	0.006	-0.195*	-0.182*
	(0.372)	(0.035)	(0.091)	(0.081)	(0.381)	(0.031)	(0.108)	(0.102)
Post×IndirAffected×lowliq	-0.443	0.026	-0.156	-0.118				
	(0.435)	(0.040)	(0.114)	(0.103)				
Post×IndirAffected×liq_pre					3.074	0.003	0.704	0.750
					(2.646)	(0.213)	(0.635)	(0.630)
N	272,862	458,930	458,930	458,930	272,863	458,934	458,934	458,934
Number of Firms	90,444	109,004	109,004	109,004	90,445	109,005	109,005	109,005
Treatment Group	389	477	477	477	389	477	477	477
Triple Interaction Group	227	286	286	286				
Within R^2	0.001	0.015	0.023	0.033	0.000	0.000	0.000	0.000
Controls (lagged)	YES	YES	YES	YES	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
County×Year Fixed Effects	YES	YES	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)-(4) 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 (5)-(8) 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. Loans is the log of firm borrowing. Employment is the log of the number of firms' employees. Tangible Assets is the log of firms' tangible fixed assets. Fixed Assets is the log of firms' fixed assets. 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: Other potential shock amplifiers: Firms' capitalization and liquidity

	Capital ratio				Liquidity			
	(1) Loans	(2) Employment	(3) Tangible Assets	(4) Fixed Assets	(5) Loans	(6) Employment	(7) Tangible Assets	(8) Fixed Assets
Post×IndirAffected	-0.336	0.029	-0.255***	-0.142	-0.458	0.004	-0.122	-0.106*
	(0.372)	(0.027)	(0.099)	(0.096)	(0.292)	(0.026)	(0.077)	(0.062)
Post×IndirAffected×adequacy_pre	0.512	-0.065	0.460**	0.180				
	(0.662)	(0.046)	(0.233)	(0.197)				
Post×IndirAffected×liq_pre					1.936***	0.024	0.219	0.172
					(0.742)	(0.067)	(0.385)	(0.298)
N	270,389	454,631	454,631	454,631	269,406	452,777	452,777	452,777
Number of Firms	89,275	107,568	107,568	107,568	89,025	107,325	107,325	107,325
Treatment Group	379	467	467	467	374	460	460	460
Within R^2	0.001	0.015	0.024	0.033	0.001	0.015	0.023	0.033
Controls (lagged)	YES	YES	YES	YES	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
County×Year Fixed Effects	YES	YES	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. Loans is the log of firm borrowing. Employment is the log of the number of firms' employees. Tangible Assets is the log of firms' tangible fixed assets. Fixed Assets is the log of firms' fixed assets. 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

7.1 Further Extensions

Relationship lending Additional banking characteristics may play a role for lending shifts following a natural disaster. Prior literature indicates that relationship banking (Boot, 2000) might play a twofold role following natural disasters. First, relationship banks may provide more lending to areas affected by the natural disaster (Cortés, 2014), because they have more proprietary information about borrowers, 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 OA4 around here –

Table OA4 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. [37]

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, 2016) or because they tend to have larger capital or liquidity buffers they can exploit in crises.

– Table OA3 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

[37] I provide the marginsplots for the interactions with these continuous relationship variables in Figure OA2 and Figure OA3 in the online appendix.

may be pressured into providing more loans to disaster-affected businesses, because it is politically beneficial for local and regional politicians (Carvalho, 2014). As a result, government banks might shift more lending from unaffected into affected regions. Government banks also constitute a major difference to the previous papers looking at bank lending in the aftermath of natural disasters in the United States (Chavaz, 2016; Cortés and Strahan, 2017). German savings and cooperative banks are banks that are typically restricted to a certain geographical area, although customers can also bank with more distant savings banks on occasion.[38] 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 OA3. 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.

[38] 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.

7.2 Figures and Tables

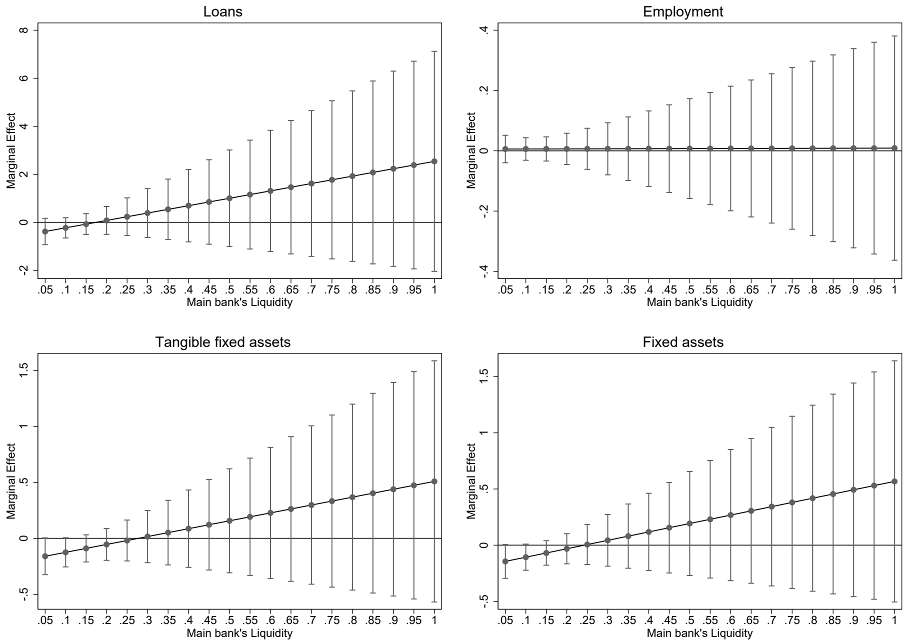


Figure OA1: Marginal Effect of the interaction with the difference-in-difference coefficient at different values of banks' Liquidity

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 (5)-(8) of Table 7). 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.

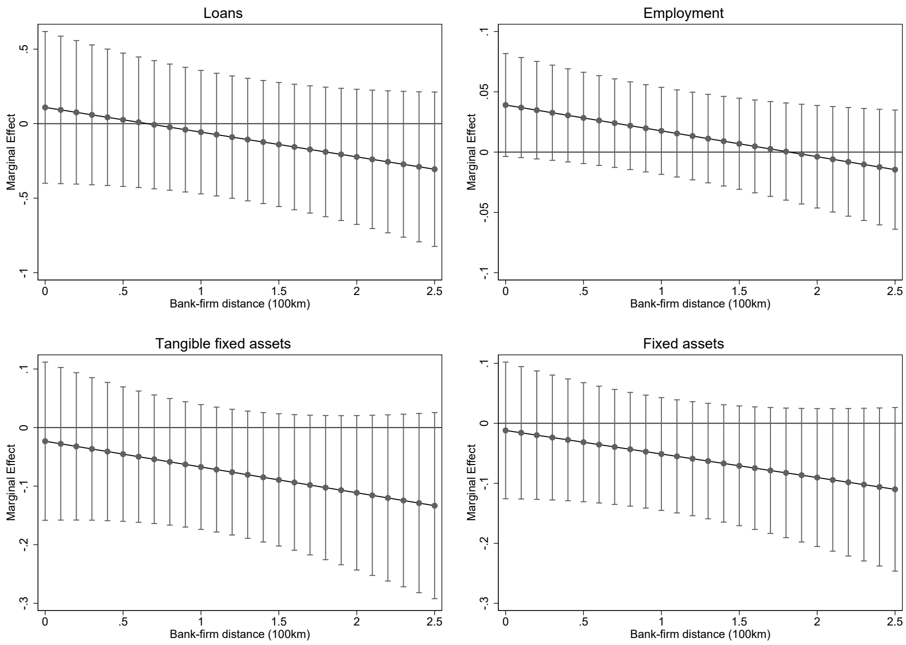


Figure OA2: Marginal Effect of the interaction with the difference-in-difference coefficient at different values of firm bank distance

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)-(4) of Table OA4). 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.

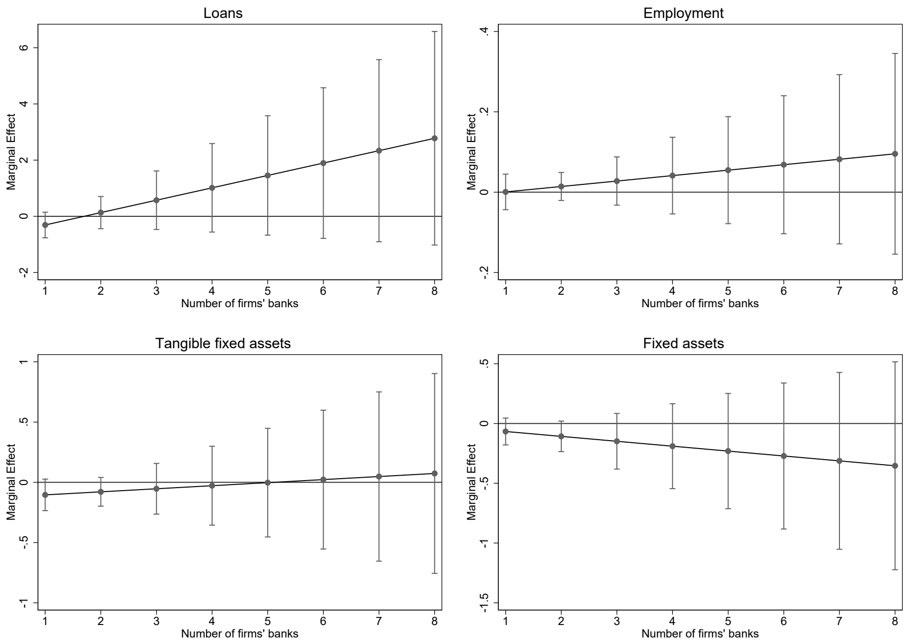


Figure OA3: Marginal Effect of the interaction with the difference-in-difference coefficient at different values of firms' bank number

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 (5)-(8) of Table OA4). 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.

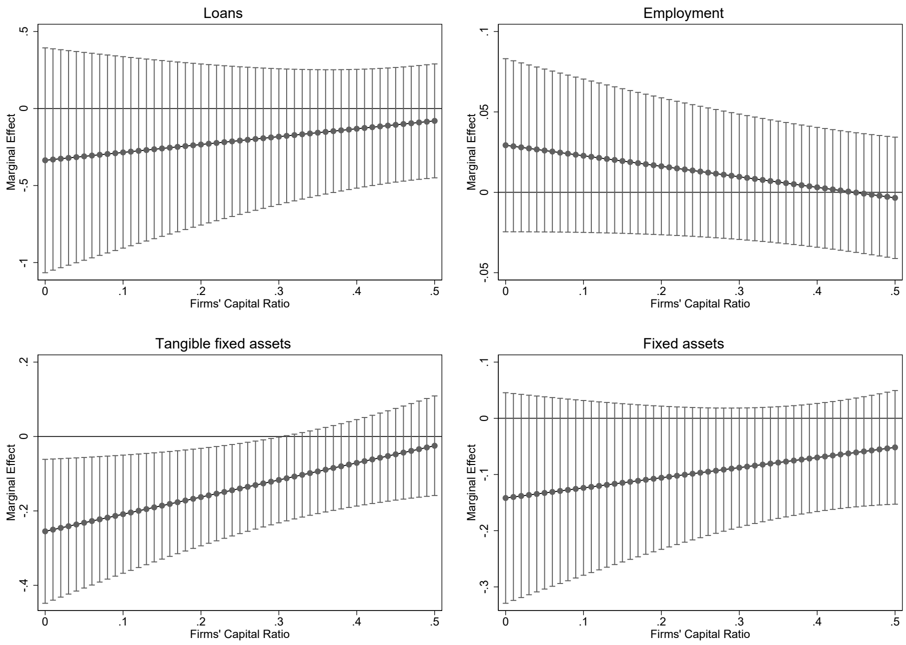


Figure OA4: Marginal Effect of the interaction with the difference-in-difference coefficient at different values of firms' capitalization

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)-(4) of Table 8). 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.

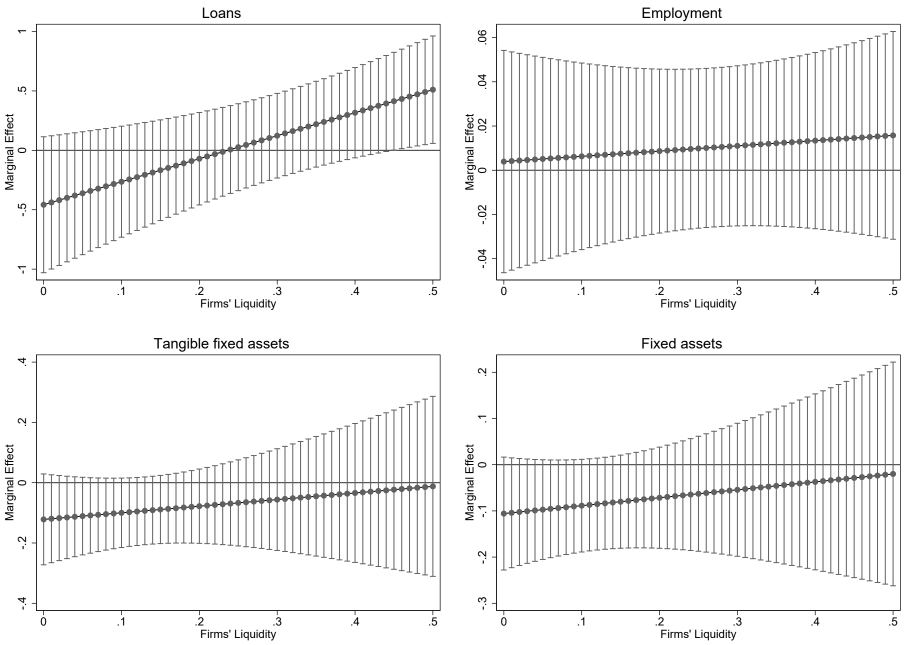


Figure OA5: Marginal Effect of the interaction with the difference-in-difference coefficient at different values of firms' Liquidity

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 (5)-(8) of Table 8). 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:	
Loans	Firm borrowing in millions of Euros. Used as natural logarithm in the regressions.
Employment	Number of firms' employees. Used as natural logarithm in the regressions.
Tangible Fixed Assets	Firms' tangible fixed assets in millions of Euros. Used as natural logarithm in the regressions.
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) Invest	(2) Cash	(3) TOAS	(4) Prov	(5) NCAS	(6) Depr	(7) RoE	(8) RoA	(9) Capital Ratio
Post×IndirAffected	-0.090	0.133	0.007	0.116*	0.003	0.081	6.535*	-0.047	-0.010
	(0.070)	(0.093)	(0.022)	(0.070)	(0.057)	(0.089)	(3.719)	(1.829)	(0.008)
Post×IndirAffected×lowcap	-0.134	0.015	0.025	0.145	0.132	-0.077	-4.181	-0.504	0.002
	(0.095)	(0.130)	(0.032)	(0.098)	(0.083)	(0.217)	(16.941)	(3.828)	(0.012)
N	432,509	457,005	458,783	457,804	418,903	105,905	102,785	107,344	458,783
Number of Firms	107,017	108,915	108,954	108,884	106,094	31,338	30,946	31,749	108,954
Treatment Group	470	477	477	476	462	121	119	123	477
Triple Interaction Group	257	261	261	260	250	64	63	66	261
Within R^2	0.032	0.003	0.087	0.008	0.016	0.051	0.009	0.026	0.103
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. Invest is the difference in tangible fixed assets between t-1 and t. 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. Unreported control variables are cash, total assets, current liabilities and the capital ratio. If the control variables are the dependent variables, they are excluded. 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: Bank Type Differentiation

	Savings banks				Cooperative banks				Commercial banks			
	(1) Loans	(2) Employment	(3) Tangible Assets	(4) Fixed Assets	(5) Loans	(6) Employment	(7) Tangible Assets	(8) Fixed Assets	(9) Loans	(10) Employment	(11) Tangible Assets	(12) Fixed Assets
Post×IndirAffected	-0.079	0.033*	-0.085	-0.049	-0.084	0.002	-0.118	-0.121	-0.183	0.001	-0.098*	-0.075
	(0.306)	(0.017)	(0.078)	(0.072)	(0.283)	(0.030)	(0.081)	(0.075)	(0.218)	(0.020)	(0.058)	(0.048)
Post×IndirAffected×savings	-0.149	-0.049	-0.032	-0.063								
	(0.407)	(0.036)	(0.111)	(0.100)								
Post×IndirAffected×coop					-0.216	0.012	0.044	0.091				
					(0.392)	(0.034)	(0.109)	(0.094)				
Post×IndirAffected×comm									0.637	0.082	-0.081	-0.187
									(1.173)	(0.052)	(0.392)	(0.477)
N	270,389	454,631	454,631	454,631	270,389	454,631	454,631	454,631	270,389	454,631	454,631	454,631
Number of Firms	89,275	107,568	107,568	107,568	89,275	107,568	107,568	107,568	89,275	107,568	107,568	107,568
Treatment Group	390	479	479	479	390	479	479	479	390	479	479	479
Triple Interaction Group	204	245	245	245	147	191	191	191	22	23	23	23
Within R^2	0.001	0.015	0.023	0.033	0.001	0.015	0.023	0.033	0.001	0.015	0.023	0.033
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 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. Loans is the log of firm borrowing. Employment is the log of the number of firms' employees. Tangible Assets is the log of firms' tangible fixed assets. Fixed Assets is the log of firms' fixed assets. 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: Relationship banking

	Firm-bank distance				Number of banks			
	(1) Loans	(2) Employment	(3) Tangible Assets	(4) Fixed Assets	(5) Loans	(6) Employment	(7) Tangible Assets	(8) Fixed Assets
Post×IndirAffected	0.109	0.039*	-0.023	-0.012	-0.750*	-0.013	-0.129	-0.026
	(0.260)	(0.022)	(0.069)	(0.058)	(0.435)	(0.039)	(0.117)	(0.106)
Post×IndirAffected×dist_pre	-0.166	-0.021*	-0.044	-0.039				
	(0.126)	(0.012)	(0.041)	(0.033)				
Post×IndirAffected×bank_count_pre					0.441	0.014	0.025	-0.041
					(0.288)	(0.020)	(0.065)	(0.067)
N	265,141	445,584	445,584	445,584	270,389	454,631	454,631	454,631
Number of Firms	87,419	105,319	105,319	105,319	89,275	107,568	107,568	107,568
Treatment Group	371	458	458	458	379	467	467	467
Within R^2	0.001	0.015	0.023	0.033	0.001	0.016	0.023	0.033
Controls (lagged)	YES	YES	YES	YES	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
County×Year Fixed Effects	YES	YES	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)-(4) 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 (5)-(8) 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. Loans is the log of firm borrowing. Employment is the log of the number of firms' employees. Tangible Assets is the log of firms' tangible fixed assets. Fixed Assets is the log of firms' fixed assets. 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 OA5: Robustness tests for low bank capital dummy: Lending

	Collapsed sample	Equal periods	Placebo	Distance	Sector×Time	Only Firm FE	No FE	Winsorized capital
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Loans	Loans	Loans	Loans	Loans	Loans	Loans	Loans
Post×IndirAffected	0.003	0.299	0.025	0.292	0.260	0.075	0.049	0.277
	(0.334)	(0.332)	(0.468)	(0.314)	(0.310)	(0.295)	(0.277)	(0.310)
Post×IndirAffected×lowcap	-0.405	-0.680	-0.094	-0.691*	-0.670	-0.523	-0.585	-0.707*
	(0.411)	(0.426)	(0.601)	(0.420)	(0.411)	(0.401)	(0.382)	(0.411)
Post×Distancetomainbank(km)				0.016				
				(0.016)				
N	155,215	224,664	173,981	266,831	272,779	272,779	272,779	272,779
Number of Firms	90,404	86,922	82,602	88,230	90,404	90,404	90,404	90,404
Treatment Group	389	373	339	379	389	389	389	389
Triple Interaction Group	211	200	189	202	211	211	211	
Within R^2		0.000	0.001	0.000	0.001			0.001
Controls (lagged)	YES	YES	YES	YES	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	YES	YES	YES	YES	NO	YES
County×Year Fixed Effects	YES	YES	YES	YES	YES	NO	NO	YES

This table presents robustness tests for the results presented in column (1) of Table 4. The dependent variable for all Columns is loans. 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. Column (8) is estimated with the main banks' capital winsorized at the 5% level. 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. 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 tests for low bank capital dummy: Tangible fixed assets

	Collapsed sample	Equal periods	Placebo	Distance	Sector×Time	Only Firm FE	No FE	Winsorized capital
	(1) Tangible Assets	(2) Tangible Assets	(3) Tangible Assets	(4) Tangible Assets	(5) Tangible Assets	(6) Tangible Assets	(7) Tangible Assets	(8) Tangible Assets
Post×IndirAffected	0.008	-0.019	0.152	0.035	0.006	0.029	0.009	0.000
	(0.077)	(0.076)	(0.095)	(0.079)	(0.075)	(0.070)	(0.067)	(0.075)
Post×IndirAffected×lowcap	-0.200*	-0.172	-0.183	-0.184	-0.206*	-0.207*	-0.209**	-0.208*
	(0.112)	(0.111)	(0.116)	(0.114)	(0.110)	(0.108)	(0.106)	(0.110)
Post×Distancetomainbank(km)				-0.008**				
				(0.004)				
N	217,908	391,972	268,711	448,580	458,783	458,783	458,783	458,783
Number of Firms	108,954	108,954	108,954	106,329	108,954	108,954	108,954	108,954
Treatment Group	477	477	477	466	477	477	477	477
Triple Interaction Group	261	261	261	251	261	261	261	261
Within R^2		0.017	0.008	0.000	0.023			0.023
Controls (lagged)	YES	YES	YES	YES	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	YES	YES	YES	YES	NO	YES
County×Year Fixed Effects	YES	YES	YES	YES	YES	NO	NO	YES

This table presents robustness tests for the results presented in column (3) of Table 4. The dependent variable for all Columns is tangible 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) 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. Column (8) is estimated with the main banks' capital winsorized at the 5% level. 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. 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 OA7: Robustness tests for low bank capital dummy: Fixed assets

	Collapsed sample	Equal periods	Placebo	Distance	Sector×Time	Only Firm FE	No FE	Winsorized capital
	(1) Fixed Assets	(2) Fixed Assets	(3) Fixed Assets	(4) Fixed Assets	(5) Fixed Assets	(6) Fixed Assets	(7) Fixed Assets	(8) Fixed Assets
Post×IndirAffected	0.041	0.004	0.166*	0.052	0.025	0.053	0.032	0.022
	(0.068)	(0.064)	(0.086)	(0.068)	(0.064)	(0.062)	(0.058)	(0.064)
Post×IndirAffected×lowcap=	-0.200*	-0.185*	-0.204*	-0.183*	-0.204**	-0.202**	-0.199**	-0.207**
	(0.102)	(0.097)	(0.123)	(0.102)	(0.098)	(0.098)	(0.096)	(0.098)
Post×Distancetomainbank(km)				-0.007**				
				(0.003)				
N	217,908	391,972	268,711	448,580	458,783	458,783	458,783	458,783
Number of Firms	108,954	108,954	108,954	106,329	108,954	108,954	108,954	108,954
Treatment Group	477	477	477	466	477	477	477	477
Triple Interaction Group	261	261	261	251	261	261	261	
Within R^2		0.023	0.011	0.000	0.033			0.033
Controls (lagged)	YES	YES	YES	YES	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	YES	YES	YES	YES	NO	YES
County×Year Fixed Effects	YES	YES	YES	YES	YES	NO	NO	YES

This table presents robustness tests for the results presented in column (4) 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) 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. Column (8) is estimated with the main banks' capital winsorized at the 5% level. 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. 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 OA8: Robustness: Variation of fixed effects

	Firm & time FE				Firm FE				NO FE			
	(1) Loans	(2) Employment	(3) Tangible Assets	(4) Fixed Assets	(5) Loans	(6) Employment	(7) Tangible Assets	(8) Fixed Assets	(9) Loans	(10) Employment	(11) Tangible Assets	(12) Fixed Assets
Post×IndirAffected	0.082	0.053**	0.031	0.054	0.075	0.051**	0.029	0.053	0.139	0.032	-0.042	0.027
	(0.294)	(0.022)	(0.070)	(0.062)	(0.295)	(0.022)	(0.070)	(0.062)	(0.318)	(0.042)	(0.089)	(0.077)
Post×lowcap	0.022	0.004**	0.001	0.007	0.022	0.003*	-0.000	0.006	0.048	0.021***	0.002	0.001
	(0.028)	(0.002)	(0.006)	(0.005)	(0.028)	(0.002)	(0.006)	(0.005)	(0.030)	(0.003)	(0.008)	(0.007)
Post×IndirAffected×lowcap	-0.522	-0.095***	-0.209*	-0.203**	-0.523	-0.094***	-0.207*	-0.202**	-0.782*	-0.163**	-0.248*	-0.172
	(0.401)	(0.036)	(0.108)	(0.098)	(0.401)	(0.036)	(0.108)	(0.098)	(0.462)	(0.064)	(0.143)	(0.114)
Post					-0.048**	0.029***	-0.021***	-0.027***	-0.336***	-0.029***	-0.070***	-0.107***
					(0.021)	(0.001)	(0.005)	(0.004)	(0.023)	(0.003)	(0.007)	(0.005)
IndirAffected									-0.290	-0.080	0.165	-0.034
									(0.384)	(0.084)	(0.141)	(0.147)
lowcap									0.033	-0.025***	0.191***	0.101***
									(0.035)	(0.007)	(0.014)	(0.010)
IndirAffected×lowcap									0.347	0.026	-0.123	0.182
									(0.532)	(0.116)	(0.201)	(0.177)
N	272,779	458,783	458,783	458,783	272,779	458,783	458,783	458,783	272,779	458,783	458,783	458,783
Number of Firms	90,404	108,954	108,954	108,954	90,404	108,954	108,954	108,954	90,404	108,954	108,954	108,954
Treatment Group	389	477	477	477	389	477	477	477	389	477	477	477
Triple Interaction Group	211	261	261	261	211	261	261	261	211	261	261	261
Adjusted R^2	0.730	0.970	0.929	0.937	0.730	0.970	0.929	0.937	0.215	0.325	0.385	0.563
Within R^2	0.001	0.016	0.024	0.033	0.001	0.028	0.025	0.035				
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	NO	NO	NO	NO
Time Fixed Effects	YES	YES	YES	YES	NO	NO	NO	NO	NO	NO	NO	NO
County×Year Fixed Effects	NO	NO	NO	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. Loans is the log of firm borrowing. Employment is the log of the number of firms' employees. Tangible Assets is the log of firms' tangible fixed assets. Fixed Assets is the log of firms' fixed assets. 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.