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Flooded Through the Back Door: The Role of Bank Capital in Local Shock Spillovers

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Abstract

This article demonstrates that low bank capital carries a negative externality because it amplifies local shock spillovers. We exploit a natural disaster that is transmitted to firms in nondisaster areas via their banks. Firms connected to a strongly disaster-exposed bank with lowest-quartile capitalization significantly reduce their total borrowing by 6.6% and tangible assets by 6.9% compared to similar firms connected to a well-capitalized bank. These findings translate to negative regional effects on GDP and unemployment. Additionally, following a disaster event, banks reduce their exposure to currently unaffected but generally disaster-prone areas.

I. Introduction

High levels of bank capital help to prevent bank failure and, as a result, can increase the stability of the financial system. This article demonstrates that, in addition, high levels of bank capital can also help to prevent real economic spillovers from one region to another. Using a natural disaster as a shock to the real economy, we show that the disaster spreads through low-capital banks to nondisaster-affected

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firms, causing a significant decline in such firms' real outcomes and translating into negative effects on aggregate regional GDP. The results indicate that the presence of excess bank capital can be beneficial, even if the stability of a given bank or the financial system as a whole is not threatened, because better-capitalized banks do not transfer shocks to out-of-region, nonshocked firms.

The article proceeds in two steps to demonstrate that the disaster spills over to out-of-region firms, which are exposed to the disaster exclusively through their banks. Exploiting the significant flooding of German regions in June 2013, we identify firms in disaster areas and use their bank connections to identify the disaster exposure of banks. Using a similar strategy, Koetter, Noth, and Rehbein (2020) show at the bank level that German banks increase their lending in the aftermath of flooding into the disaster region. They also demonstrate that firms in the disaster region benefit from this additional lending. This article identifies firms in *non-flooded* areas that are connected to disaster-exposed banks and compares them to firms that are located in the same region but do not have a connection to a disaster-exposed bank. This approach is designed to isolate the effect of a reduction in bank funding for firms, as banks reduce lending in nonflooded areas in order to provide loans to flood-affected firms.

On average, banks' lending shifts from nondisaster regions to disaster regions entail a reduction in borrowing of 2.4%, in employment of 2.4%, and in tangible assets of 5.1% for firms with a connection to a strongly exposed bank. Importantly, this negative lending shift is almost exclusively driven by low-capital banks. After splitting banks into capitalization quartiles, only firms connected to strongly exposed low-capital banks experience a significant decrease in borrowing of 6.6% and in tangible assets of 6.9%. Firms connected to disaster-exposed banks with larger capital buffers are unaffected by indirect disaster exposure. We show that the effects of this local shock amplification stemming from low levels of bank capital also affect aggregate regional GDP. Thus, even if an increase in bank capital causes lending reductions in normal times and is thus costly for firms (Gropp, Mosk, Ongena, and Wix (2019)), higher bank capital levels can prevent lending reductions after small but frequent shocks to the real economy. The occurrence of firm-level real effects also strongly suggests that better-capitalized banks do not jump in to replace reduced lending from less-capitalized banks. This may be because bank-firm relationships tend to be relatively stable and because obtaining loans from other banks might be associated with high switching costs and information asymmetries.

Additional results demonstrate that firms located in regions with higher ex ante disaster risk (but outside of the 2013 disaster regions) also suffer disproportionately large real effects. This finding is independent of the level of bank capital held by indirectly disaster-affected firms' banks. It suggests that banks shift lending away from disaster risk after a natural disaster, which then negatively affects firms in high-disaster-risk regions. We suggest that this phenomenon can be most easily explained by banks' need to rebalance their portfolios with regard to disaster risk, following an increase in lending to disaster regions. This finding further underscores the idea that banks' lending increase to natural disaster regions, which has been shown previously in the literature (Cortés and Strahan (2017)), implies lending reductions and, as a result, illustrates real effects through a variety of channels, two of which we uncover in this article.

Understanding the role of bank capital in *local* shock spillovers is crucial. While global financial crises are rare, sudden changes to a regional economy are frequent. In Germany, each year, approximately 12% of counties experience a decline in GDP, even when omitting the crisis years of 2007, 2008, and 2009. For approximately 1% of regions, this negative shock to GDP is comparable in size to the shock of the financial crisis to the overall German economy (−5.6% GDP in 2009).[1] This back-of-the-envelope calculation implies that every year, approximately 5 German counties experience a negative event that has similar economic effects as the financial crisis. As a result, understanding banks' lending reallocation patterns following local shocks is extremely important.

Consequently, our contribution highlights the importance of bank capital in preventing local shocks from spilling over into other regions. We contribute to the literature that investigates banks' reallocation patterns after local shocks in various settings, including natural disasters (Cortés and Strahan (2017), Koetter et al. (2020)), by demonstrating the key role that the capital level of the firm's bank plays in such local shock spillovers. It also contributes by demonstrating that even small, local shock spillovers can create spillovers with *real economic* implications. This latter point is extremely important. Much of the evidence of bank-lending-induced real effects stems from times of broad-based financial distress (Chodorow-Reich (2014), Huber (2018)). There are good reasons to believe that real effects are nonexistent for smaller shocks because firms can simply switch lenders, finance on capital markets, or exploit trade credit lines in normal times. However, our article demonstrates that this does not appear to be the case. Even when small local shocks transfer via banks to otherwise unaffected firms, we uncover firm-level effects of this lending reduction, despite the fact that the nonlocal environment is unchanged. Another important difference from other related literature is that the natural disaster is likely a credit *demand* shock. As the Covid-19 pandemic highlights, understanding credit demand shocks and how they create spillovers is very important. In the following paragraphs, we further expand on our contribution to the three main areas connected to our article: local shock spillovers, the role of bank capital, and real effects.

There is large literature investigating spillovers in finance. There is ample evidence that financial shocks cross international borders (Puri, Rocholl, and Steffen (2011), Popov and Udell (2012), and Schnabl (2012)). Another strand of literature investigates *local* shocks that create spillovers to other regions. One strand of this literature is concerned with spillovers in housing prices or mortgage rates after the financial crisis. For example, various papers demonstrate that foreclosures affect the values of other houses in the neighborhood (Harding, Rosenblatt, and Yao (2009), Campbell, Giglio, and Pathak (2011), Anenberg and Kung (2014), and Gupta (2019)). Additionally, Chakraborty, Goldstein, and MacKinlay (2018) demonstrate that housing price increases can crowd out lending to firms, thus creating cross-sectoral spillover effects, and Loutskina and Strahan (2015) show that integration in financial markets amplifies shocks from housing

[1] *Source:* Statistisches Bundesamt and own calculations. Specifically, 12% of year-on-year changes in GDP are negative at the county level, and 1% are a 5% decline or more. Frequently, this is not due to a declining trend but rather to unexpected regional developments.

price booms. This article focuses on spillovers that arise due to shifts in bank lending. Prominently, Gilje, Loutskina, and Strahan (2016) demonstrate that banks that receive a sudden shock of deposit inflows from unexpected discoveries of shale gas pass this windfall on to other regions through their branch network. Further, Gilje (2019) demonstrates that local banking markets are slower to obtain access to funding due to a similar channel. Garmaise and Natividad (2016) demonstrate that bank-firm relationships can generate positive information spillovers from one firm to another. We expand on these findings by demonstrating that banks also pass on negative shocks to their lending capability stemming from a demand shock. In this regard, our question is closely related to Cortés and Strahan (2017) and Koetter et al. (2020), who demonstrate that banks serve higher credit demand in natural disaster regions by retracting credit from more distant, unaffected regions. We contribute to this strand of the literature in three significant ways. First, we demonstrate that low levels of bank capital significantly amplify these spillover effects. Second, we demonstrate that these local events have real economic implications. Third, we show that retractions are largest from areas in which the bank might reasonably expect a repetition of the disaster in the future.

Our findings are related to the literature on bank capital as an amplifier of lending shocks. A great deal of focus has been placed on the effect of bank capital on banks' lending behavior, especially during financial crises (Jayaratne and Morgan (2000), Kishan and Opiela (2000), Gambacorta and Mistrulli (2004), and Meh and Moran (2010)).[2] Gan (2007) and Kapan and Minoiu (2018) take it a step further and look at firm-level effects. They show that higher lender capital ratios are associated with a better performance of borrowing firms following large banking crises. The latter point is where we aim to contribute. While it has been demonstrated that bank capital is important for the transmission of large-scale international banking crises to firms, we provide novel evidence of its importance in local shocks.

This differentiation has important policy implications because most macroprudential regulations have been designed only with large-scale international shocks in mind. While our results do not directly speak toward the optimal level set by bank capital regulation, we do show that regulators should take the ability of bank capital to absorb local shocks from propagating into different regions seriously. However, simply setting higher uniform bank capital requirements is unlikely to be successful, since unexpected local shocks need to be absorbed by capital in excess of the requirement. Instead, regulators should find a way to induce banks to hold more such equity (e.g., by removing the tax shield of debt). Alternatively, regulators could provide bank capital the ability to vary with local economic conditions, akin to a *local* counter-cyclical capital buffer.

The Covid-19 pandemic has highlighted the importance of credit demand shocks. There has been an unprecedented draw-down on existing credit lines that has been supported by large-scale central bank interventions (Li, Strahan, and Zhang (2020)). Additionally, considering the differences in public health policies

[2]This literature is also 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, Dell'Ariccia, Laeven, Ratnovski, and Tong (2016)), only a few studies examine the real effects of bank capital regulation (Jiménez Ongena, Peydró and Saurina (2017b), Gropp et al. (2019)).

surrounding the pandemic, these demand shocks are likely to vary by locality. However, there is little evidence of the effect of such (local) demand shocks, especially with regard to spillovers to firms. We contribute to this literature by investigating the real effects of a shock stemming from higher local credit *demand.* We show that higher demand can be a driver of negative local shock spillovers and that bank capital levels play an important role in preventing them.[3]

In addition to demonstrating the amplification effect of low bank capital levels, this article also contributes to the literature on the real effects of bank lending reductions on firms (Chodorow-Reich (2014), Huber (2018)).[4] Our first contribution to this literature is methodological. Most prior studies rely on banks' exposure to financial market frictions, such as exposure to the financial crisis. A major caveat here is that bank choice may not be completely orthogonal to banks' exposure to risky international financial markets. We argue that the credit supply shock arising from a natural disaster is better in this regard because it is unexpected, especially for firms that are not directly located within the disaster region. Our identification relies only on the assumption that bank customers are unaware of their banks' disaster exposure prior to the flood. Given that there is some evidence that even insurance markets often fail to correctly price disaster risk (Froot (2001)), it seems unlikely that bank customers correctly price their banks' disaster risk. Additionally, firms' bank choice must not be correlated with other factors that might be affected by flooding. We perform a number of additional checks to rule out these potential confounding factors without any change to the results.

[3]Interpreting a natural disaster in a developed country as a demand shock is strongly supported by the literature. Chavaz (2016) and Cortés and Strahan (2017) document for the United States that banks reallocate funds toward mortgage loans in disaster-affected areas while decreasing their lending to nonaffected areas, and Koetter et al. (2020) demonstrate this demand effect specifically for the disaster examined in this article. 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 to finance necessary rebuilding efforts. The unfulfilled loan demand in the aftermath of disasters in developing countries (Berg and Schrader (2012)) indicates that insurance and government aid 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. See Section II for details regarding the specific flood and the subsequent government aid payments.

[4]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. Chava and Purnanandam (2011) show that during the Russian crisis, firms that relied on bank financing suffered real consequences. Almeida, Campello, Laranjeira, and Weisbenner (2012) show that firms whose debt was maturing during the financial crisis cut their investment. Using firm-bank level data from Eastern Europe and Central Asia, Ongena, Peydro, and van Horen (2015) show that firms connected to internationally active banks suffer more during a financial shock. Using bank-firm data from Italy, Cingano, Manaresi, and Sette (2016) estimate that the collapse of the interbank market decreased firm-level investment by 20%. Popov and Rocholl (2018) show that firms connected to German savings banks with exposure to U.S. mortgage markets performed worse than otherwise similar firms. Berg (2018) provides evidence of negative real effects with rejected loan application data. Acharya, Eisert, Eufinger, and Hirsch (2018) provide evidence that the European sovereign debt crisis had real, firm-level effects. Gropp et al. (2019) show that higher capital requirements cause credit reductions and subsequent negative real effects in firms.

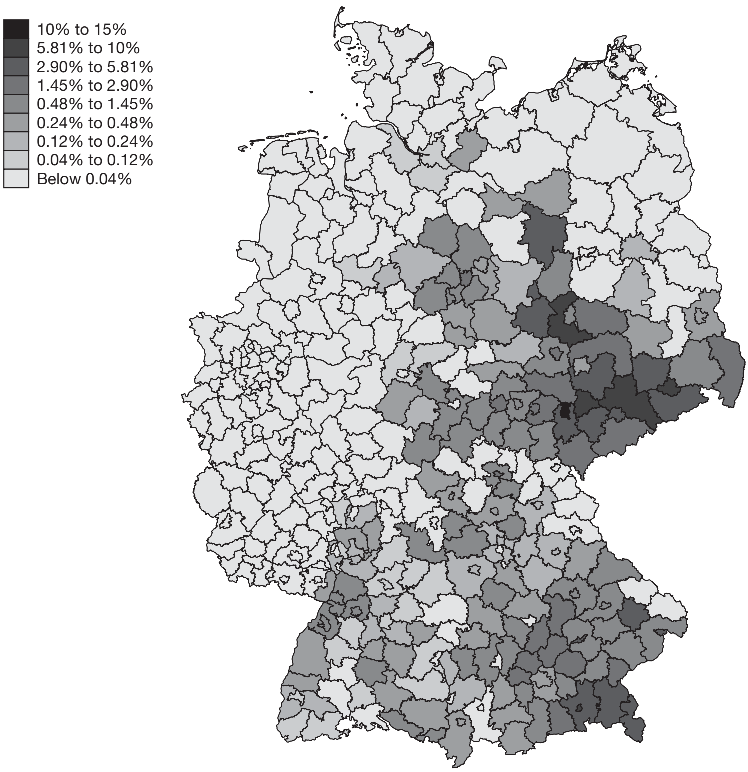
II. The 2013 Flood, Insurance, and Government Aid

Widespread flooding caused significant damage 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 rainfall from May 30 to June 2 (Schröter, Kunz, Elmer, Mühr, and Merz (2015)). Heavy flooding followed in many regions of Austria and in the following weeks in southeastern Germany and the Czech Republic, causing many levee breaches and widespread flooding. Germany was mostly flooded in the areas around the Danube and Elbe Rivers 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.[5]

FIGURE 1

Affected German Counties by Damage Categories

Figure 1 shows the distribution of the damage sustained from flooding in Germany from May 25 through June 15, 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 are provided by the German Association of Insurers.



[5]Some of this damage was permanent. For example, the ice hockey stadium in Halle (Saale) was flooded and has not been rebuilt to date.

The 2013 flood was the largest 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 flood of 2002, possibly because of flood protection measures instituted after the latter event (Thieken (2016)). While initial reports indicated that the 2013 flooding exceeded the 2002 event in terms of damage, final estimates report that the two events are similar in terms of the final economic damage: approximately 6–8 billion euros for the 2013 flood and 11 billion euros for the 2002 flood. Of the 6–8 billion euros in damages, only 2 billion euros were insured (GDV (2013)), despite the 2002 flooding. This finding is in line with the idea that flood insurance costs rise after a flood, as insurance companies adjust the rates after tail risks materialize. This idea is supported by the fact that insurance coverage remained 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 2 weeks after the damage is reported (GDV (2013)), in practice, insurers' resources are often insufficient to accommodate so many contemporaneous claims.[6] As a result, going to a bank for flood relief and rebuilding efforts can be faster than waiting for insurance payouts, especially when there is an option to draw down on existing credit lines.

Floods of this magnitude have several direct and indirect effects on firms in the flood areas, and many are difficult to estimate. Direct effects include damage to buildings and machines as well as sales revenue losses during floods and rebuilding/repair efforts. Indirect effects include health effects and interruptions of supply chains due to destroyed infrastructure. Thieken (2016) conducted a business survey following the 2013 flood and found that the most frequent problem for businesses was in fact the loss of sales revenue, while the most significant problem in terms of economic damage was destroyed buildings and equipment. Considering the average total assets in our data set of 14 million euros, losses to firms were significant: on average, the surveyed firms reported approximately 1 million euros in damages.

To recover these losses, uninsured firms were able to 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. Furthermore, only direct damages were reimbursed; indirect damages, such as losses from lost sales revenue, 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 to finance their rebuilding efforts. Systematic evidence that banks indeed provided additional lending to disaster areas for the benefit of firms following the 2013 flood

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

is provided by Koetter et al. (2020), who show that lending by banks and borrowing by firms increased in the disaster regions following the 2013 flood.

Flood prevention measures were taken after the 2002 flooding; however, there is no indication that the 2013 flood was anticipated. Even during the flood, there was uncertainty about the extent to which water levels would rise. However, the 2002 flood may have increased the efficiency and especially the speed with which aid relief was delivered following the 2013 flood (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 expanded significantly only 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 2002 flood (Noth and Rehbein (2019)).

Taken together, the facts about the 2013 flood indicate that it was a significant and unexpected event for firms that 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. Banks are also likely to have served this additional demand because repeated lending to already screened projects may have been perceived as more profitable and may have been accompanied by lower (internal) risk weights. As a result, we hypothesize that banks who lent to government-supported disaster areas reduced lending to other areas, resulting in potential negative real outcomes for firms located in nondisaster areas, especially when banks were constrained by low capital levels. We hypothesize that this is due to the fact that it is harder to expand balance sheets of capital-constrained banks in the short-run, without violating regulatory constraints or implicit internal capital ratio targets.

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 nonfinancial corporations in Germany amounted to approximately 800 billion euros over the flood period. Therefore, if roughly one-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. Our results are particularly striking in this light, as banks propagate not only large financial shocks but also small local shocks to "innocent" firm clients. This finding is important, as local shocks can have multiple causes and occur much more frequently than large-scale financial crises.

III. Data

German firm-level data stem from the Dafne and Amadeus databases, both provided by Bureau van Dijk. The former contains the name of the bank (or banks) with which each firm maintains a payment relationship (Popov and Rocholl (2018)).[7] Annual vintages of the Dafne database are used to construct a time-series

[7]The construction of the firm-bank level data largely follows Koetter et al. (2020), although they collapse the data to the bank level, while our data are on the firm level, which requires some additional cleaning. Firm-bank payment relationship data originate from scans of the firms' letterheads. We do not

of firm-bank relationships for more than a million firms between 2003 and 2014. We augment these firm-bank relationship data with firm-specific, annual financial accounts data from Amadeus.[8] The firm-level data are 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.[9]

To gauge the damage inflicted by the Elbe flood of 2013, we use a data set provided by the German Insurance Association (GDV). The data contain claims filed for insurance properties that were damaged during the flood between May 25 and June 15, 2013, as a proportion of total insurance contracts, aggregated by county ("Kreis"), into nine damage categories.[10] Lower categories indicate less damage relative to the asset values covered by insurance contracts.[11] The GDV collects this information from all 460 of its 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)). We merge these flood-level data with firms via their postal code.

The combination of the three data sets yields a firm-level data set 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. We conduct a number of cleaning steps with the merged data set. First, we drop firms and banks for which no valid postal code can be matched and all inactive firms.[12] We also require firms to have reported at least their total assets because otherwise, the reporting accuracy might be questionable. We also drop all observations before 2008 because the 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 set (Popov and Rocholl (2018)). Because firms often do not report for all years,[13] we require firms to be in the data set for at least 1 year before

observe credit relationships directly. We 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 approximately 1.4 million firms appeared in the database by 2014.

[8]Bureau van Dijk takes this information for German firms from the "Bundesanzeiger," where firms can report their balance sheet information. This reporting was more rigorously enforced starting in 2008.

[9]Because we lack any other relationship information other than the banks' names in the Dafne database, we manually inspect many matches to ensure that the firm-level data are combined with the correct financial information about the banks from Bankscope. We match approximately 99% of all firm-bank relationships.

[10]Thus, we do not observe the damage inflicted on individual banks or firms. Additionally, we do not have information on plants. As a result, we implicitly assume that the firm's location (i.e., the headquarters, is the same as its plant location). Considering that we examine mainly SMEs, which are usually single-plant firms, this assumption appears to be reasonable.

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

[12]Because we cannot observe the reason why firms are dropped from the data set or become inactive, we choose not to investigate this as an outcome variable.

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

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the flood of 2013 and 1 year after. Additionally, we require that the lags of the control variables be nonmissing and drop all observations for which this is not the case. Finally, we drop financial firms from the data set to ensure that our results are not driven by banks and other financial institutions. The resulting data set contains observations for approximately 115,000 firms for the period of 2009 to 2014.

IV. Identification

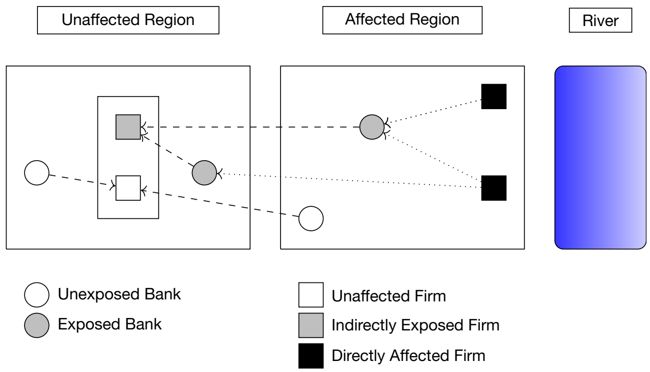
The goal of this article is to compare firms that are outside of the direct disaster area yet conduct business with a bank that has sufficient disaster exposure with firms outside of the disaster area that do not have a relationship with a disaster-exposed bank. The underlying idea is that disaster-exposed banks reduce lending to nondisaster firms, especially if they have little capital.

We illustrate graphically in Figure 2 how we identify such firms. We 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 value of 9, and firms in nonflooded counties receive a 1. Next, we identify the banks' exposure to the flood by averaging the category numbers of the banks' firm customers, weighted by the relative firm size (equation (2)). This process is illustrated in the figure by the dotted arrows. Next, we identify indirectly affected firms by identifying their banks' exposure to the flood and averaging if the firm has multiple banks. This is illustrated by the dashed arrows in the figure. This indirect disaster exposure measure serves as a continuous treatment indicator intended to compare indirectly affected and unaffected firms. We identify firms without such an indirect exposure (illustrated by the blank squares) and compare indirectly affected with nonindirectly affected firms. Because we use county × year fixed effects, this comparison is strictly within

FIGURE 2

Indirectly Disaster-Exposed Firms: Illustration

Figure 2 illustrates the identification of indirectly exposed firms. Firms are depicted as rectangles and banks as circles. Directly affected firms (solid black) are identified by their location in the affected region. Exposed banks (gray circle) are considered exposed due to their customers' location. As such, they can also be located outside of the affected region (Koetter et al. (2020)). Indirectly exposed firms are identified if the average exposure of their banks to the flood meets a certain threshold (gray rectangle). Region × time fixed effects imply a strictly within-region comparison between indirectly exposed firms and nonindirectly exposed firms (as illustrated by the rectangular framework in the unaffected region).



region. The estimated comparison is illustrated by the smaller black frame within the unaffected region. In essence, this illustrated comparison is the focus: firms located in unaffected regions with varying levels of indirect exposure to the flood because of their banks.[14]

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. We exploit this *indirect* effect as an exogenous funding shock to firms to investigate the real effects of small, local shocks to the real economy.

A. Directly and Indirectly Affected Firms

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

$$(1)\quad \text{DIRECTLY\_AFFECTED}_i = \begin{cases} 0 & \text{if CLAIM\_RATIO\_CATEGORY}_{r_j} = 1 \\ 1 & \text{if CLAIM\_RATIO\_CATEGORY}_{r_j} \geq 4 \end{cases}.$$

To understand the indirect effect of a bank-level lending shift on firms, we estimate bank exposure to the disaster. To do so, we follow the identification employed by Koetter et al. (2020), 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 banks' flood exposure is similar to the method used in Chavaz (2016) and Cortés and Strahan (2017), 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

[14]As an example, the data include 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, and 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 in areas exposed to the flood. It is unknown why the firm maintains a relationship with this rather distant savings bank, although an internet search suggests that its founder might have lived there. Nevertheless, concerning the 2013 flood, it is connected to the affected region only via its bank, not through any other discernible connection.

firm are based on the firm's 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.

$$\text{(2)}\quad \text{EXPOSURE}_i = \sum_{j \in N_i} \left(\frac{\text{ASSETS}_{j,N}}{\text{TOTAL\_ASSETS}_{N_i}} \times \text{CLAIM\_RATIO\_CATEGORY}_{r_j} \right),$$

where N_i are the firms j of bank i located in region r_j. CLAIM_RATIO_CATEGORY$_{r_j}$ is a value between 1 and 9 based on the firms' locations in the counties, as shown in Figure 1.[15] Because firm-bank connections vary slightly over time, we use pre-disaster exposure in 2012 for the analysis. Because any firm can report payment relationships with multiple banks (although the majority only report one), to construct the firms' exposure to the *indirect* effect of the flood, we then average the exposure of all of the firm's banks, which results in the variable INDIR_DIS_EXP$_j$ and is constructed as the average exposure$_i$ of all banks i working with firm j.[16] This yields a firm-specific *indirect* average exposure of the firm's banks to the flood. Higher levels indicate that the firms' banks have many (large) customers in the disaster area to which they extend credit.

B. Estimation

Using this classification of indirectly exposed firms, we estimate a difference-in-differences regression with continuous treatment. Equation (3) provides the estimation equation, where Y_{it} are the real outcome variables for 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. $C_{\text{kit}-1}$ are firm-specific lagged control variables, specifically cash, size (TOTAL_ASSETS), debt (CURRENT_LIABILITIES), and capital ratio (COMMON_EQUITY/TOTAL_ASSETS).[17]

$$\text{(3)}\quad \ln(Y_{jt}) = \beta(\text{POST}_t \times \text{INDIR\_DIS\_EXP}_j) + \alpha_j + \alpha_r \times \alpha_t + \sum_{k=1}^{K} \gamma_k C_{kjt-1} + \varepsilon_{jt}.$$

We initially choose three key dependent variables (Y_{jt}) to estimate the impact on firms' real performance.[18] First, we investigate the amount of total borrowing by the firm. A detailed analysis of lending patterns by banks with flood-affected customers was performed on the bank level by Koetter et al. (2020). Since they use a very similar approach to measure changes in bank lending for the same flood event and data, we choose to stay exclusively on the firm level to avoid unnecessary repetition. As a result, we start by investigating whether indirectly affected firms' total borrowing decreases. However, the data do not allow separating (specific)

[15] Note that because there is geographical variation in the banks' customers, the banks' exposure to the flood is bank specific instead of county specific.

[16] We use alternatives to averaging in Section V.C.

[17] The exact definition of the control variables can be found in Table OA1 of the Supplementary Material. All variables are winsorized at the 5% level.

[18] We additionally test other variables that are related to firm health. The results can be found in Table OA2 of the Supplementary Material.

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bank loans from other loans taken by the firm, and for many (small) firms, loans must not be reported separately from total liabilities. As a result, we use firms' total liabilities to investigate firms' overall borrowing since it certainly captures all loans taken by the firm.

Next, we investigate 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 is traditionally highly important from a policy perspective (Chodorow-Reich (2014), Popov and Rocholl (2018)). In addition to employment, firms can also reduce their capital input if they are faced with a funding reduction from banks. We specifically test tangible fixed assets as a proxy for firms' capital input.

Crucially, in these estimations, we are able to control for firm and county × year fixed effects because the indirect disaster exposure measure is firm-specific. This is particularly important for two reasons. First, it removes the possibility that governmental aid biases 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, county × time fixed effects control for regional demand and trade. Of course, firms may 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.[19]

This described identification requires that some firms exist outside the direct flood impact that still have exposure to banks affected by the flood via their firm customers. To confirm that this is indeed the case, we show the distribution of *indirectly* affected firms outside of directly affected regions in Figure 3. Graph A displays the mean of INDIR_DIS_EXP_j per region, while Graph 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 result is to be expected and a crucial reason why county × year fixed effects are important. Graph B) further demonstrates that there are at least some indirectly affected firms in most regions. This finding increases confidence in the idea 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 article's identification can be exploited in most regions.

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

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

FIGURE 3

Distribution of *Indirect* Exposure of Firms in Nondirectly Affected Areas

Figure 3 shows the distribution of the firm's average exposure of its banks to the disaster (INDIR_DIS_EXP) by German regions. Section IV.A describes how this measure of firms' indirect exposure to the disaster via its banks is derived. Graph A shows the mean exposure of all firms in the region. Graph B shows the maximum exposure of firms in the region. Labels are displayed in the upper left corner of each graph.

Graph A. Mean Exposure of Indirectly Affected Firms *Graph B. Maximum Exposure of Indirectly Affected Firms*

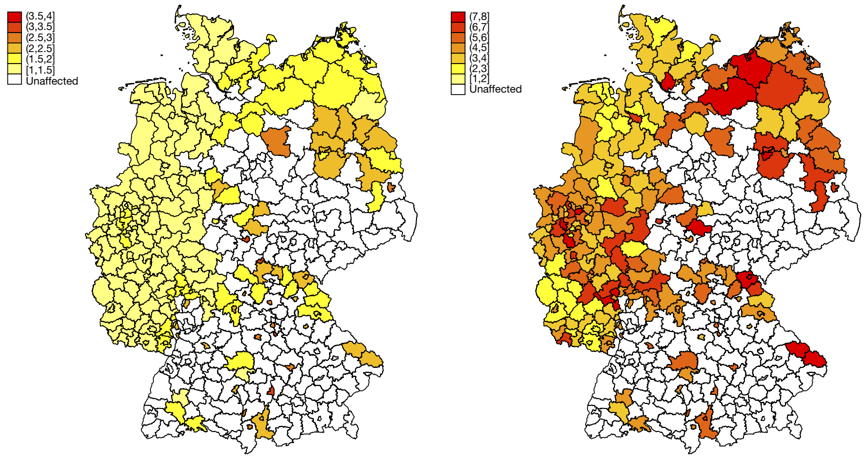

TABLE 1

Descriptive Statistics

Table 1 presents summary statistics for all variables used in the subsequent regressions. Detailed variable definitions can be found in Table OA1 of the Supplementary Material. All variables except for the DIRECTLY_AFFECTED dummy are reported only for nondirectly affected firms. DIRECTLY_AFFECTED is a dummy variable based on the firm's location with regard to the flood (cf. Figure 1), according to equation (1). Indirect disaster exposure measures the exposure of the firm to the flood via its banks, according to equation (2). Cash, total assets and current liabilities are reported in levels but included as logs in the regressions. All control variables are used as first lags in the regressions. Firms' banking characteristics are taken at pre-flood levels. All firm-level variables are taken from the Amadeus database.

	N	Mean	Median	Std. Dev.	Min	Max
Identification variables						
DIRECTLY_AFFECTED	639,799	0.32	0.00	0.47	0	1
INDIR_DIS_EXP	437,451	1.53	1.16	0.62	1.0	7.82
Dependent variables						
TOLI (mil. EUR)	437,451	8.23	0.63	251.67	0.0	45,984
EMPL	437,451	53.78	14.00	540.36	1.0	95,791
TFAS (mil. EUR)	437,451	3.21	0.12	72.12	0.0	19,953
Control variables						
L_CASH (mil. EUR)	437,451	0.94	0.08	16.52	0.0	3405
L_TOTAL_ASSETS (mil. EUR)	437,451	12.83	0.98	329.18	0.0	56,042
L_CURRENT_LIABILITIES (mil. EUR)	437,451	3.56	0.11	132.99	0.0	27,230
L_CAPITAL_RATIO	437,451	0.33	0.29	0.27	0.0	1
Interaction variables						
CAP_RATIO	437,451	0.16	0.17	0.04	0.1	0.8
PRE_2013_DIS_RISK	437,451	2.12	2.17	0.72	1.0	3.5
Main bank's variables						
TOAS_B (mil. EUR)	437,451	139.53	5.14	236.99	0.0	5,961
COOP	437,451	0.21	0.00	0.41	0.0	1.0
SAVING	437,451	0.50	0.00	0.50	0.0	1.0
IRB	437,451	0.28	0.00	0.45	0.0	1.0
Relationship variables						
SINGLE	437,451	0.52	1.00	0.50	0.0	1.0
REL_LEN	437,451	1.69	2.00	1.35	0.0	5.0

C. Importance of Bank Capital in Disaster Shock Transmission

While there is some evidence that low-capital banks are more likely to transmit financial shocks to firms (Gan (2007); Jiménez, Ongena, Peydró, and Saurina (2017a)), the role of bank capital in *local* shock spillovers has received little attention. However, the same mechanisms that cause a general reduction in lending during the 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 riskier by the market. This finding is derived from the fundamental idea of how much leverage economic agents can acquire before the market recognizes the increased riskiness of the remaining equity stake (Modigliani and Miller (1958)). 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. This argument is based on the idea that an expanding balance sheet will usually decrease capital ratios at least in the short term because funding through debt markets is usually faster and easier for banks than financing through equity.[20] Note that banks usually hold an internally determined safety buffer above the regulatory threshold.

A key part of this article is to contribute to the understanding of whether bank capital is important for the transmission and amplification of unexpected local shocks. We thus add triple-interaction effects to our difference-in-differences analysis and estimate equation (4) in the following way:

$$\begin{aligned}(4)\qquad \ln(\mathrm{Y}_{jt}) = {} & \beta_1(\mathrm{POST}_t \times \mathrm{INDIR\_DIS\_EXP}_j) \\ & + \beta_2(\mathrm{POST}_t \times \mathrm{INDIR\_DIS\_EXP}_j \times \mathrm{BANK\_CAPITAL}_j) \\ & + \beta_3(\mathrm{POST}_t \times \mathrm{BANK\_CAPITAL}_j) \\ & + \alpha_j + \alpha_r \times \alpha_t + \sum_{k=1}^{K} \gamma_k C_{kjt-1} + \varepsilon_{jt}.\end{aligned}$$

We specify $\mathrm{BANK\_CAPITAL}_j$ in two different ways. First, we create bank capitalization quartiles by splitting the sample into firms whose main bank had very low, low, high, and very high bank capital. Specifically, we average the capitalization of each firm's main bank in 2012 and 2013 and set the variable equal to 0 if the firm's main bank is in the highest quartile of the distribution, 1 if it is in the second-highest, 2 in the third-highest, and 3 if the main bank's capitalization is in the lowest quartile of the capitalization distribution. We then investigate β_2 to determine whether such firms suffer significantly more from the *indirect* shock than other firms. Second, we estimate a continuous interaction with the main bank's pre-flood

[20]While there is a vigorous debate on the cost of raising equity for financial institutions (Admati, DeMarzo, Hellwig, and Pfleiderer (2013), Baker and Wurgler (2015), and Gandhi, Lustig, and Plazzi (2020)), overall the literature seems to suggest that at least in the medium term capital requirements are binding and that there are significant frictions in the market of outside equity for banks.

regulatory capital ratio (CAP_RATIO), which allows us to investigate the effect of the main bank's regulatory capital ratio on different levels of the distribution.

Banks' capital regulation in Germany follows EU regulations 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 had 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 our sample is 8%, 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 or without violating EU regulations. 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 slightly 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, DeYoung, Flannery, Lee, and Öztekin (2008), Francis and Osborne (2012), and Lepetit, Saghi-Zedek, and Tarazi (2015)), which may be binding and prevent significant lending expansions. This finding implies that it is difficult to identify whether the regulatory minimums are relevant for lending outcomes because the binding effect of regulation may be different for each bank, depending on their internal capital buffers.

D. Loan Demand and Loan Supply

Natural disasters tend to be interpreted as loan demand shocks from the bank's perspective (Cortés (2014), Chavaz (2016), Cortés and Strahan (2017), and Koetter et al. (2020)). 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. Crucially, using very similar data and the same flood event, Koetter et al. (2020) present strong evidence in favor of this view by demonstrating that lending to disaster regions increased without affecting banks' profitability. They also show that the share of impaired loans does not increase, suggesting that supply effects are likely not at play.

There are a few potential explanations for why banks, especially those with little capital, might respond to additional lending demand. One might believe that financially constrained banks may choose to abstain from providing loans, especially to disaster areas, that may have uncertain prospects. While we cannot specifically test whether loans to disaster areas are more profitable than loans to other areas, Koetter et al. (2020) suggest that they are at least not less profitable than ordinary loans and do not default at higher rates. Furthermore, loans to disaster areas may in fact be less risky than intuition suggests. First, most loans to disaster-affected businesses would be to finance previously screened projects. The bank is now financing the same investment for a second time, and it has gathered information about loan performance throughout the previous financing process. Second, government aid payments may not only limit losses on previous loans but also serve as a good source of collateral because they are cash payment promises by the government. Combined, these reasons can make lending to disaster regions more (or at least similarly) attractive to lending in nondisaster areas. Independent of *why*

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banks choose to lend to disaster regions, there exists strong previous evidence that they do, and as a result, the possibility arises that this situation is costly for other connected firms.

However, it cannot be completely 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 article's identification does not hinge on the shock being a loan *demand* shock to banks. Because we do not examine banks directly, but rather the banks' firm customers in nonflooded areas, it is important that the bank is induced to reduce loans in unaffected areas. This finding 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 these losses. A *demand* shock would result in the flood-exposed bank having to raise additional funds 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 at the bank level, and none of the results in this article suggest another interpretation. Thus, we 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.

V. Results

A. Indirect Effect

Based on previous literature and the flood characteristics presented in Section II, we hypothesize that banks shift lending from directly unaffected areas into directly affected areas, especially if banks hold little capital. 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. To do this, banks have two options: raise funds on financial markets (increase liabilities) or shift existing lending away from other areas (e.g., by not renewing loans) increasing prices or increasing funding requirements (reducing assets).[21] If banks opt for the former option, firms in nonflooded areas should be unaffected. If banks opt for the latter, firms in nonflooded areas may become "flooded through the back-door" (i.e., unintentionally affected by a funding reduction from banks exposed to the disaster).

We thus examine whether the firm's banks' flood exposure affected the firm's loans as reported on the firm's balance sheet. We test this question by estimating equation (3) using OLS with standard errors clustered at the firm level. Columns 1–3

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

TABLE 2

Flooded Through the Back Door: Firm Outcomes After *Indirect* Disaster Exposure

Table 2 presents the results of the indirect effect of flooding on firms for three different outcomes: total liabilities (TOLI), employment (EMPL), and tangible fixed assets (TFAS). Firms are indirectly exposed if their banks have large flood exposure on average due to their firm-customer location with regard to the flood (see Section IV for details). All regressions are strictly within nonflooded regions. INDIR_DIS_EXP is a continuous variable measuring the exposure of the firm to the flood via its banks, according to equation (2). Regressions include 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.

	ln(TOLI)	ln(EMPL)	ln(TFAS)
	1	2	3
POST × INDIR_DIS_EXP	−0.008***	−0.008***	−0.017***
	(0.002)	(0.002)	(0.004)
ln(L_CASH)	−0.006***	0.002***	0.009***
	(0.001)	(0.000)	(0.001)
ln(L_TOTAL_ASSETS)	0.267***	0.115***	0.426***
	(0.005)	(0.003)	(0.007)
ln(L_CURRENT_LIABILITIES)	−0.000	0.000***	0.000
	(0.000)	(0.000)	(0.000)
L_CAPITAL_RATIO	−0.404***	0.036***	0.199***
	(0.010)	(0.006)	(0.015)
No. of obs.	437,451	437,451	437,451
No. of firms	103,380	103,380	103,380
Within R^2	0.062	0.019	0.042
Firm fixed effects	Yes	Yes	Yes
County × year fixed effects	Yes	Yes	Yes

of Table 2 report the results for firms located *outside* the flood radius (i.e., firms classified as not directly affected according to equation (1)).

Column 1 of Table 2 suggests that firms are borrowing less overall, as total liabilities decrease significantly with increasing indirect disaster exposure. Each increase in exposure by one,[22] implies a decrease in total borrowing of 0.8%. Compared to a completely unaffected firm, firms that are maximally exposed to the disaster through their bank reduce their borrowing by 5.5%. A firm with strong indirect exposure to the flood – defined as having a bank relationship to a bank whose firm customers are on average affected by the definition of equation (1) – will decrease borrowing by 2.4%.[23]

Importantly, these borrowing reductions appear to cause real effects on firms. The results indicate that there is a drop in employment of 0.8% and a decrease in tangible fixed assets of 1.7% per point increase in indirect disaster exposure for firms in nonflooded regions. This finding implies that the most exposed firms decrease employment by 5.5% and tangible fixed assets by 11.6% compared to firms without indirect disaster exposure. In other words, a strongly affected firm will reduce employment by 2.4% and fixed assets by 5.1% compared to a completely unaffected firm.

[22]Originally, the indirect disaster exposure measure stems from the 9 affected categories. It is thus theoretically limited at 1 and has a maximum of 9 (if all the banks' firms are located in the most heavily flooded regions).

[23]The maximum exposure is 7.82. Comparing a completely unexposed firm (1) to a maximally exposed firm (7.82) thus implies a decrease in total liabilities of $0.8\% \times 6.82 = 5.46\%$. Comparing a completely unexposed firm (1) to a firm one with strong indirect exposure (4) yields $0.8\% \times (4-1) = 2.4\%$.

In line with overall credit reductions implied by a reduction in total firm borrowing, significant real effects arise in both employment and tangible assets. This finding is interesting, as even smaller funding shocks, such as those from the Elbe flood to indirectly affected firms, appear to entail real effects. This new finding suggests that there is no need for widespread failure of banking systems for firms to suffer consequences from a reduction in credit, implying that firms cannot switch easily to other funding sources, even in normal times.

B. Amplification of Shock Transmission

The effect of banks' lending shift following natural disasters from unaffected to affected regions may depend 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 (Gan (2007), Jiménez et al. (2017a)).[24] Low capital ratios impede banks' ability to raise external funds for two reasons: first, low capital ratios imply a 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. 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 to raise funds by reducing other lending assets because borrowing additional funds would violate capital regulations. Importantly, banks do not need to be exactly at the threshold for this effect to take hold, as they may choose to hold a (fixed) buffer above the regulatory requirement for other liquidity-related reasons. Both of these explanations imply that low-capital banks have to cut back lending to out-of-region firms if they are faced with a local shock.

We test whether 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 (4). Table 3 shows the results of a regression using interactions with pre-flood (2012) capitalization quartiles. Firms connected to banks in the lowest quartile of the capital distribution appear to be the drivers of reductions in total firm borrowing. Firms with a connection to a bank that is in the lowest quartile of the bank capital distribution decrease overall borrowing by 2.2% per point of disaster exposure (equivalent to a 6.6% decrease for strongly affected firms), whereas disaster exposure does not change lending at any other capitalization quartile.

Significantly, this reduction affects firm input factors: employment outcomes are slightly worse for firms whose main bank holds little capital, although the effect is not statistically significant. However, tangible assets decrease by 2.3% per point of indirect disaster exposure if the bank is located in the lowest quartile of the capital

[24]There is a large debate on what exactly best constitutes banks' financial constraints. The aim of the article is not to contribute to that debate, so we focus on the simplest and most policy relevant measure: banks' regulatory capital ratios. We extensively discuss alternative explanations in Section II of the Supplementary Material.

TABLE 3

Amplifying the Shock: Main Bank's Capital Buffer

Table 3 presents interactions of the continuous difference-in-differences estimation from Table 2 interacted with the pre-flood regulatory capital of the firm's main bank. Columns 1–3 specify interactions with a capitalization quartile categorical variable, which places the firm's main bank in the lowest, low, high or highest quartile of the pre-flood capital distribution. Columns 4–6 specify continuous interactions with the main bank's pre-flood capital. Double interaction coefficients are included but not reported. INDIR_DIS_EXP is a continuous variable measuring the exposure of the firm to the flood via its banks, according to equation (2). Regressions include firm and county × year fixed effects and the control variables from Table 2 (unreported). 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.

	ln(TOLI)	ln(EMPL)	ln(TFAS)	ln(TOLI)	ln(EMPL)	ln(TFAS)
	1	2	3	4	5	6
POST × INDIR_DIS_EXP	0.003	−0.006*	−0.011	−0.071***	−0.028	−0.154***
	(0.005)	(0.003)	(0.009)	(0.027)	(0.021)	(0.047)
POST × INDIR_DIS_EXP × HIGHEST_CAP	−0.010	0.001	0.006			
	(0.007)	(0.005)	(0.013)			
POST × INDIR_DIS_EXP × LOW_CAP	−0.007	0.003	−0.016			
	(0.007)	(0.005)	(0.012)			
POST × INDIR_DIS_EXP × LOWEST_CAP	−0.022***	−0.002	−0.023*			
	(0.007)	(0.006)	(0.012)			
POST × INDIR_DIS_EXP × ln(CAP_RATIO)				0.023**	0.007	0.049***
				(0.010)	(0.007)	(0.017)
No. of obs.	437,451	437,451	437,451	437,451	437,451	437,451
No. of firms	103,380	103,380	103,380	103,380	103,380	103,380
Within R^2	0.062	0.019	0.042	0.062	0.019	0.042
Controls (lagged)	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
County × year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

distribution. Compared to a completely unaffected firm, firms with little capital that are strongly indirectly affected by the disaster thus experience a reduction in tangible fixed assets of approximately 6.9%.

To investigate whether there is a continuous nature of bank capital in shock absorption, columns 4–6 of Table 3 provide the results of a continuous interaction of the difference-in-differences term with the pre-flood main bank regulatory capital ratio. Analogous to Gropp et al. (2019), we use the log of bank capital in the main specification, although to illustrate our interpretation, we derive graphs from linear interactions. The results of the continuous interaction indicate that higher capital ratios in the firm's main bank imply larger lending and (tangible) fixed asset effects, balancing the negative effect of the simple difference-in-differences estimate.

We plot the relationship between bank capital and firm-level real effects of the indirect disaster shock in Figure 4. As higher regulatory capital ratios imply larger (differential) borrowing, employment and capital stock effects, the slope of all curves is increasing. For all dependent variables, capital ratios below approximately 20% are associated with a significant decrease in outcomes due to indirect disaster exposure. It does not appear to be the case that more capital increases lending, but it does seem that a certain level of bank capital is required to prevent negative spillovers.

The effects of this disaster spillover are sizable and occur mostly at low levels of bank capital, namely, up to 20%. For each dependent variable, only low-capital banks appear to transfer the shock from flooded to nonflooded areas. Lending reductions result in large real effects, mostly in terms of capital input, implying

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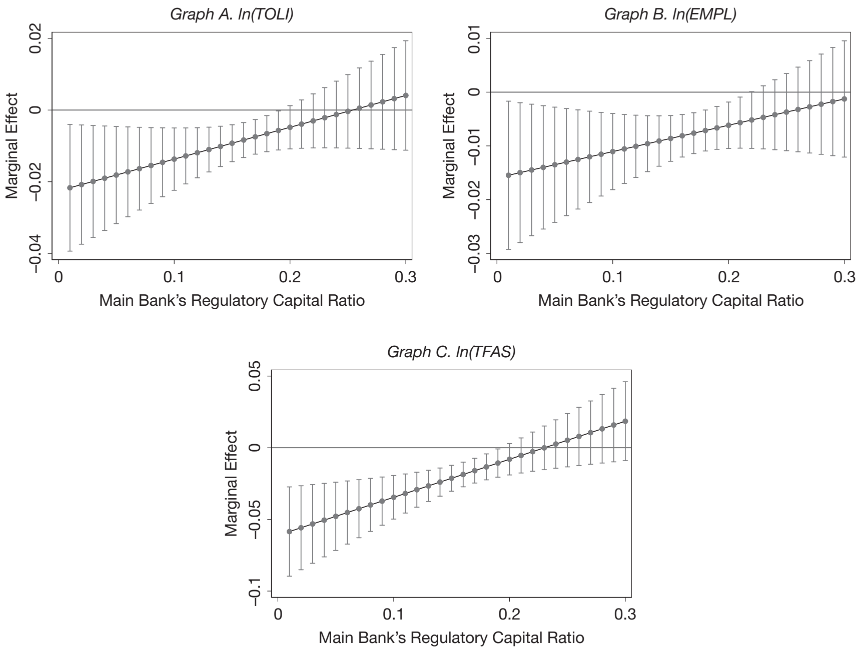
FIGURE 4

Marginal Effect of the Difference-in-Differences Coefficient at Different Values of Main Bank Capitalization: Real Effects

Figure 4 shows the marginal effects of the difference-in-differences estimation of being exposed to a bank funding shock resulting from flooding in other regions at different values of the capital ratio of the firm's main bank. The corresponding regression is provided in Table 3. 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.



that firms are unable to substitute by borrowing from other sources even in normal times. Overall, these results clearly indicate that banks' capital ratios are extremely important in determining whether local shocks are buffered or amplified by banks. Larger capital ratios are helpful in preventing banks from spreading shocks to other sectors of the economy that have no direct exposure to the shock themselves.

These results are difficult to interpret in light of capital regulation policy. While the results clearly demonstrate that more capital is useful to absorb local shocks instead of amplifying them, it is less clear that this more capital can simply be achieved by higher regulatory bank capital ratios. All banks in the sample are subject to largely the same (macroprudential) capital regulations, and a potential explanation for our effect is precisely that banks are worried about violating those regulations. As a result, the policy implications from our results are not straightforward. The simplest interpretation is that banks should be incentivized to hold more capital without forcing them to hold a certain amount of capital. One possibility would be to give tax advantages to holding more capital (or the reverse, removing the tax shield of debt). Another possibility might be to instate higher capital ratios but allow them to be flexible in times of local economic shocks, similar to the countercyclical capital buffer in Basel III but on a much more granular level.

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C. Shock Transmission and Regional Effects

The question naturally arises as to why firms are unable to substitute funding from other banks when their main bank's funding structure prohibits them from lending. This situation is true especially for firms with multiple bank relationships. If the firm has optionality over which bank to borrow from, we need to also ask which disaster exposure matters for the firm. Until this point, we averaged the disaster exposure of all the firm's banks under the assumption that this figure is most representative of the loan supply shock the firm experiences. However, if banks can choose which banks to access capital from,[25] this averaging may not be the right approach. In Table 4, we rerun our regression using only the main bank's exposure. If firms can utilize their bank relationships strategically, we should find smaller or even no effects when using only the main bank's exposure.

Columns 1–3 of Table 4 are very similar to the baseline results, suggesting that the main bank's exposure is likely the cause of the large lending reductions to firms. This finding is intuitive, as German SMEs are likely to rely on their main bank for a large majority of their funding. We then turn to single-relationship firms. If there is some option value in multiple bank relationships, we should find much stronger effects for single-relationship banks. Columns 4–6 provide limited evidence for this idea. The effects are somewhat stronger, although the results

TABLE 4

Which Bank's Exposure Matters? Variations in Disaster Exposure

Table 4 presents the results of continuous difference-in-differences estimations using the main bank's indirect disaster exposure interacted with a capitalization quartile categorical variable, which places the firm's main bank in the lowest, low, high or highest quartile of the pre-flood capital distribution. MAIN_BANK_EXP is a continuous variable measuring the exposure of the firm to the flood via its main bank. Columns 4–6 include only single-bank firms. The pre-flood capitalization is based on the main banks' regulatory capital ratio in 2012. Double interaction coefficients are included but not reported. Regressions include firm and county × year fixed effects and the control variables from Table 2 (unreported). 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.

	Main Bank's Exposure			Main Bank's Exposure: Single Bank		
	ln(TOLI)	ln(EMPL)	ln(TFAS)	ln(TOLI)	ln(EMPL)	ln(TFAS)
	1	2	3	4	5	6
POST × MAIN_BANK_EXP	0.002	−0.011***	0.008	0.013*	−0.007	−0.003
	(0.005)	(0.004)	(0.010)	(0.007)	(0.005)	(0.014)
POST × MAIN_BANK_EXP × HIGHEST_CAP	−0.007	0.009*	−0.010	−0.013	0.008	0.004
	(0.007)	(0.005)	(0.013)	(0.010)	(0.006)	(0.018)
POST × MAIN_BANK_EXP × LOW_CAP	−0.003	0.013***	−0.028**	−0.015	0.009	−0.039**
	(0.007)	(0.005)	(0.012)	(0.010)	(0.007)	(0.017)
POST × MAIN_BANK_EXP × LOWEST_CAP	−0.016**	0.008	−0.028**	−0.030***	0.008	−0.034*
	(0.007)	(0.006)	(0.012)	(0.010)	(0.009)	(0.018)
No. of obs.	437,451	437,451	437,451	233,322	233,322	233,322
No. of firms	103,380	103,380	103,380	56,827	56,827	56,827
Within R^2	0.062	0.019	0.042	0.059	0.018	0.044
Controls (lagged)	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
County × year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

[25]The evidence in Table OA9 of the Supplementary Material suggests that at least the mere presence of multiple banks does not increase post-disaster firm performance. In fact, the opposite is true: single-relationship banks perform slightly better than their counterparts.

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TABLE 5

Which Type of Capital Is Relevant? Variations in Capital Measurement

Table 5 presents interactions of the difference-in-differences regressions with tier 1 capital and the leverage ratio of the main bank. Interactions are with a capitalization quartile categorical variable, which places the firm's main bank is in the lowest, low, high or highest quartile of the respective pre-flood distribution of the tier 1 capital ratio or the leverage ratio. The categorical variable in Columns 1–3 is based on the main bank's tier 1 capital ratio. The categorical variable in Columns 4–6 is based on the main bank's leverage ratio. Double interaction coefficients are included but not reported. Regressions include firm and county × year fixed effects and the control variables from Table 2 (unreported). 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.

	Tier 1 Capital			Leverage Ratio		
	ln(TOLI)	ln(EMPL)	ln(TFAS)	ln(TOLI)	ln(EMPL)	ln(TFAS)
	1	2	3	4	5	6
POST × INDIR_DIS_EXP	−0.007	−0.007**	−0.010	−0.010**	−0.004	−0.028***
	(0.005)	(0.003)	(0.009)	(0.005)	(0.004)	(0.010)
POST × INDIR_DIS_EXP × T1_HIGH_CAP	−0.421	−0.202	−0.531			
	(0.501)	(0.500)	(0.943)			
POST × INDIR_DIS_EXP × T1_LOW_CAP	−0.001	−0.001	−0.005			
	(0.005)	(0.004)	(0.010)			
POST × INDIR_DIS_EXP × T1_LOWEST_CAP	−0.011*	0.004	−0.021*			
	(0.006)	(0.005)	(0.012)			
POST × INDIR_DIS_EXP × L_HIGH_CAP				0.004	−0.001	0.013
				(0.006)	(0.005)	(0.012)
POST × INDIR_DIS_EXP × L_LOW_CAP				0.003	−0.004	0.011
				(0.006)	(0.005)	(0.012)
POST × INDIR_DIS_EXP × L_LOWEST_CAP				0.012	0.006	−0.004
				(0.008)	(0.006)	(0.015)
No. of obs.	372,350	372,350	372,350	437,451	437,451	437,451
No. of firms	87,787	87,787	87,787	103,380	103,380	103,380
Within R^2	0.063	0.019	0.042	0.062	0.019	0.042
Controls (lagged)	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
County × year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

do not suggest that the coefficients are significantly different from each other.[26] We further investigate whether firms access funding from the best-capitalized banks in times of funding shortfalls but similarly find little evidence that the exposure of the best-capitalized bank matters for the firm (Table OA3 of the Supplementary Material).

Overall, these results suggest what is intuitive about borrowing for German SMEs: what matters is the funding availability from the firm's main bank. If the main bank is subject to a demand shock from a natural disaster region, firms have trouble accessing funds from other sources. In that regard, the disaster exposure of the main bank appears to be the relevant factor.

Another question concerns the nature of capital that enables banks to absorb demand shocks rather than pass them on to other firms. In the previous tables, we have focused exclusively on the overall regulatory capital. Table 5 additionally considers the tier 1 capital ratio and the leverage ratio as potential alternative channels. The results suggest that the overall regulatory capital used in the baseline regression is most important. Columns 1–3 suggest that the tier 1 capital ratio has similar effects as the overall regulatory capital ratio, but the effects are somewhat smaller and less significant. With regard to the leverage ratio in columns 4–6,

[26]Using a simple Z-test, $Z = \frac{\beta_1 - \beta_2}{\sqrt{(\mathrm{SE}\beta_1)^2 + (\mathrm{SE}\beta_2)^2}}$ suggests that even the large difference between columns 1 and 4 is not statistically significant.

TABLE 6

Entire Regions Suffer: Regional Amplification Effects of Low Bank Capital

Table 6 presents the results of county-level regressions indicating the effect of low bank capital on post-disaster regional performance. The regressions span the years 2006 to 2015. Only nondirectly affected (nonflooded) counties are considered. INDIR_DIS_EXP_R is a continuous variable indicating the regionally aggregated average indirect exposure of all firms in the county to the flood through their banks. AVG_CAP_R is a continuous variable and captures the mean level of bank capital held by the firm's banks in the county prior to the flood in 2012. UNEMPL is the regional unemployment rate in %. GDP is per capita regional GDP. INSOLV is the absolute number of insolvencies. PUB_DEBT is the public debt in the county. We control for county and year fixed effects. Clustered standard errors on the county level of the point estimates are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	UNEMPL (%)	ln(GDP)	INSOLV	ln(PUB_DEBT)
	1	2	3	4
POST × INDIR_DIS_EXP_R	2.939	−0.335**	0.000	0.525
	(2.989)	(0.153)	(0.001)	(1.251)
POST × AVG_CAP_R	−0.713	−0.165*	−0.000	0.803
	(1.898)	(0.095)	(0.001)	(0.809)
POST × INDIR_DIS_EXP_R × AVG_CAP_R	−1.075	0.121**	−0.000	−0.282
	(1.043)	(0.053)	(0.000)	(0.434)
No. of obs.	2,658	2,430	2,391	2,357
No. of counties	270	270	270	266
Within R^2	0.066	0.024	0.005	0.035
County fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes

no effect can be found. Since the leverage ratio is very rarely binding (or even close to binding) for most banks, this result is not surprising. Overall, the evidence suggests that existing regulatory capital regulation is likely the underlying factor for the amplification effect of low bank capital.

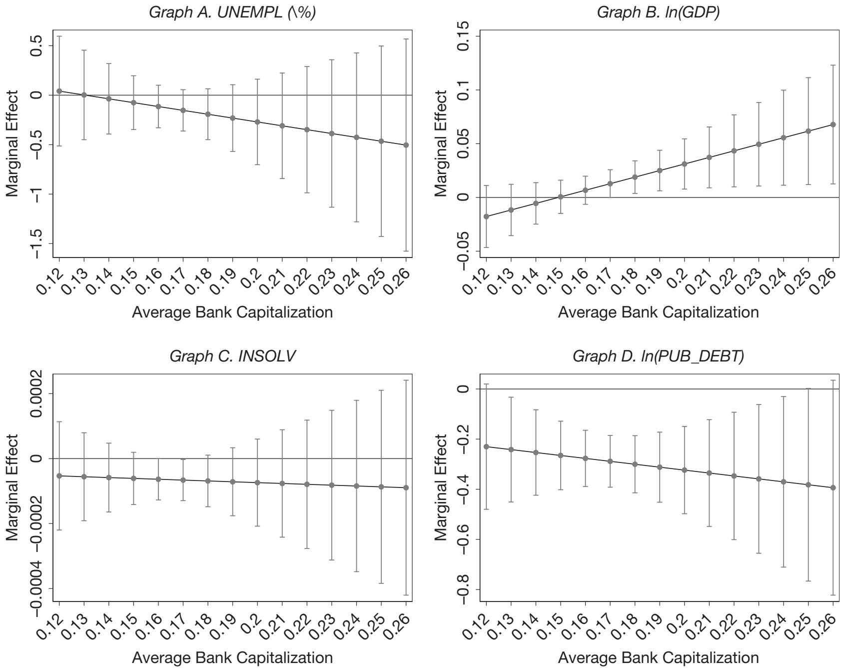
We shed further light on the importance of regional shock transfers by low-capital banks in regional regressions. Table 6 shows estimates for the effects of higher regional average bank capital on outcomes on the German county level. In this regression, we construct the county average of the indirect exposure of all firms in the county. We then estimate a post-disaster continuous difference-in-differences model and interact it with the average level of the firm's banks' capital. The results confirm the firm-level outcomes. Counties with a larger disaster exposure on average have lower post-flood per capita GDP levels, which is buffered if these counties' banks are better capitalized (column 2). Quantitatively, an increase in average indirect exposure by 1-standard-deviation (0.55, unreported) implies a lower per capita GDP of 6.5%, which is a large effect. However, an increase in 1-standard-deviation of average county bank capitalization (0.02, unreported) decreases this effect by 1.5%. We plot the marginal effect of a larger share of indirect flood exposure at different levels of average bank capitalization on the county level of these regional regressions in Figure 5. For high levels of bank capitalization, the previously negative effect on per capita GDP becomes insignificant and ultimately positive, confirming that the more bank capital exists in the system, the less the negative impacts spread to other regions. Unemployment, insolvencies, and public debt are statistically unaffected by the local spillover of the flood.

This finding demonstrates that the effects of higher bank capital levels buffering regional spillovers can prevent negative effects not only on individual firms but also even on the regional economy. Thus, because local shock spillovers can be mitigated by higher bank capital levels, our results imply a previously disregarded

FIGURE 5

Marginal Effect of Indirect Disaster Exposure by Different Levels of Average Bank Capital on the County Level

Figure 5 shows the marginal effect of regional indirect disaster exposure (average of all firms' indirect disaster exposure in the region) at different values of the regions' average bank capital (measured as the pre-flood capital ratio of firms' banks in the region). Average 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.



benefit (a positive externality) of higher levels of bank capital. This does not necessarily mean that higher regulatory capital ratios will be able to internalize this externality, because banks need to hold capital in excess of regulatory requirements to serve demand in emergency situations.[27] Since unexpected regional negative shocks may occur quite often, this externality may have significant macroeconomic effects, although this question requires further research.

The previous results demonstrate the importance of bank capital for local shock amplification. In addition, other factors may be highly relevant for shock transmission, which has received much less attention in the literature. One of these potential channels is the propensity of banks to rebalance their portfolio following a shock. When banks increase their exposure to a certain type of asset (in this case, by lending more to disaster regions) they also increase this asset-specific risk at the same time. In turn, demand for diversification may drive banks to reduce exposure to assets with similar risk structures. In the case of natural disasters, an increase in lending to disaster regions, which has often been demonstrated in the previous

[27]See Section V.B for a more detailed discussion.

literature, will likely increase banks' disaster exposure ex post. To maintain diversification, banks may decrease lending to other high-disaster risk areas as a response, thereby transferring the shock to high-disaster risk areas that were unaffected by the 2013 flood.

We test this hypothesis by adding an interaction with ex ante disaster risk to the difference-in-differences estimation. Ex ante disaster risk is measured at the county level by adding disaster damage from previous flood damage reports. Information on damages from previous events is provided to us similar to the 2013 damages by the German Association of Insurers. In total, it is based on 6 additional flood-like events. Only one of those events (the flood of 2002) is also classified as a major disaster. The other events are minor local flood-like events that produced flood-related insurance claims. Data are also provided in 9 damage categories for the 2013 disaster.

The results of the estimation are presented in Table 7 and demonstrate the expected results. Firms located in regions with higher ex ante disaster risk suffer increased reductions in total borrowing, employment, and tangible fixed assets. A 1-standard-deviation increase in pre-flood disaster risk is associated with a decrease in borrowing of 0.4%, a reduction in employment of 0.4% and a reduction in tangible fixed assets of 0.7%. These results are consistent with the idea that banks withdraw from other high-risk areas to rebalance their disaster risk. This unanticipated loan shifting to adjust risk portfolios has real effects on firms that are located in high-risk (though nonflooded in 2013) regions. To our knowledge, such real effects of portfolio reallocation are a novel finding, especially in the context of natural disasters. Interestingly, these results can also be interpreted as an information spillover in the style of Garmaise and Natividad (2016). As banks learn more about disaster risk by lending to disaster-affected firms, they also retract lending to other firms that are exposed to disaster risk.

TABLE 7

Disaster-Prone Areas Are Most Affected: Real Effects in High-Disaster-Risk Areas

Table 7 presents the interactions of the continuous difference-in-differences estimation from Table 2 interacted with the pre-disaster risk of the firm's county. Only nondirectly affected firms are included. PRE_2013_DIS_RISK is calculated as the average damages from previous flood-related disasters from 2002 to 2012. Double interaction coefficients and control variables are included but not reported. INDIR_DIS_EXP is a continuous variable measuring the exposure of the firm to the flood via its banks, according to equation (2). We control for firm and county × year fixed effects and the control variables from Table 2 (unreported). 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.

	ln(TOLI)	ln(EMPL)	ln(TFAS)
	1	2	3
POST × INDIR_DIS_EXP	0.008	0.009	0.001
	(0.008)	(0.006)	(0.014)
POST × INDIR_DIS_EXP × PRE_2013_DIS_RISK	−0.008**	−0.008***	−0.009
	(0.003)	(0.003)	(0.006)
No. of obs.	437,451	437,451	437,451
No. of firms	103,380	103,380	103,380
Within R^2	0.062	0.019	0.042
Controls (lagged)	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes
County × year fixed effects	Yes	Yes	Yes

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D. Robustness

Next, we test whether the baseline results hold up to several robustness tests. Table 8 presents robustness tests using tangible fixed assets as the dependent variable (column 3 of Table 3). Robustness tests for columns 1 and 2 can be found in Tables OA4 and OA5 of the Supplementary Material. First, we ensure that the results are not driven by the selection of years; thus, column 1 estimates a regression using the same length of pre- and post-periods (i.e., 2010–2014). The results are displayed in column 1 and are very similar to the original findings.

Next, we test whether the data satisfy the parallel trends assumption, which is crucial to the difference-in-differences analysis. Since our treatment is continuous, displaying parallel trends graphically is difficult. However, we can formally test pretreatment similarity in growth paths of the dependent variable using a placebo regression, which is provided in column 2 of Table 8. Here, the year 2011 is set as the flood year, while the years 2012–2014 are excluded. The results are not significant, indicating that the treatment variable 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, to the effects of the flood. Mainly, it is possible that firms choose banks in places where their suppliers or customers are located. If that were the case, our effect might be capturing direct flood exposure via channels other than lending. We provide two tests to account for this possibility.

TABLE 8

Robustness Tests for Low Bank Capital Dummy: Tangible Fixed Assets

Table 8 presents robustness tests for the results presented in column 3 of Table 3. Column 1 presents the results using equal base and post periods. Column 2 presents the results of a placebo test using the year 2010 as the placebo event year. Column 3 includes post-flood firm-bank distance control. Column 4 includes sector × year fixed effects. Column 5 provides estimates using only firm fixed effects. Column 6 is estimated with the main banks' capital winsorized at the 5% level. Interactions are specified with a capitalization quartile categorical variable, which places the firm's main bank in the lowest, low, high, or highest quartile of the pre-flood capital distribution. Double interaction coefficients, single interactions, and control variables are included but not reported. INDIR_DIS_EXP is a continuous variable measuring the exposure of the firm to the flood via its banks, according to equation (2). TFAS is the log of the firm's tangible fixed assets. We control for firm and county × year fixed effects and the control variables from Table 2 (unreported). 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 the other dependent variables in Table 3 can be found in the Supplementary Material.

	Equal Periods	Placebo	Distance	Sector × Time	Only Firm FE	Winsor. Cap.
	ln(TFAS)	ln(TFAS)	ln(TFAS)	ln(TFAS)	ln(TFAS)	ln(TFAS)
	1	2	3	4	5	6
POST × INDIR_DIS_EXP	−0.010	0.003	−0.012	−0.008	−0.015*	−0.011
	(0.009)	(0.011)	(0.010)	(0.009)	(0.008)	(0.009)
POST × INDIR_DIS_EXP × HIGHEST_CAP	0.004	0.004	0.002	0.006	0.015	0.006
	(0.012)	(0.016)	(0.013)	(0.013)	(0.012)	(0.013)
POST × INDIR_DIS_EXP × LOW_CAP	−0.015	−0.020	−0.017	−0.017	0.004	−0.016
	(0.012)	(0.016)	(0.013)	(0.012)	(0.011)	(0.012)
POST × INDIR_DIS_EXP × LOWEST_CAP	−0.022*	−0.019	−0.034***	−0.025**	−0.012	−0.023*
	(0.012)	(0.014)	(0.013)	(0.012)	(0.012)	(0.012)
POST × BANK_DISTANCE			−0.001			
			(0.002)			
No. of obs.	373,433	153,706	420,500	437,451	437,451	437,451
No. of firms	103,380	90,504	99,278	103,380	103,380	103,380
Within R^2	0.032	0.005	0.000	0.042		0.042
Controls (lagged)	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
County × year fixed effects	Yes	Yes	Yes	Yes	No	Yes

First, we include an interaction with the post dummy and the firm-bank distance. If the effect were driven by the distance between banks and firms, this coefficient should pick up the variation. Column 3 of Table 8 shows that this interaction is not statistically significant and does not eliminate the original result. Second, to mute concerns that "specialty" banks are driving the result, we additionally include sector $\times$ time fixed effects in column 4, again without a change in the result.

To investigate the extent to which the inclusion of a robust set of fixed effects is important for our findings, we provide the results of regressions with only firm fixed effects (column 5). Interestingly, the inclusion of fixed effects is not important for the overall finding that the flood's effects spread through the banking network to other firms, but it does appear to be important for the effects of bank capital levels. This suggests that controlling for local demand through region $\times$ time fixed effects may be very important to understand the effects of bank capital on the loan supply. Lastly, we check whether the results might be driven by a few banks that have extraordinarily high or low capital ratios. Column 6 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.

Lastly, there might be some concern that variables that are correlated with bank capital might be responsible for our results. This is an important concern since bank capital levels are not necessarily exogenous, even if the natural disaster shock is. While we cannot rule out endogeneity of capital entirely, we attempt to rule out possible alternative explanations in Table 9. Most importantly, the level of bank capital should not be correlated with the indirect disaster exposure measure. Column 1 confirms that the main bank's capital and the disaster shock are uncorrelated. Note also that even in this almost empty specification, approximately 87% of the variation is explained through only firm fixed effects.[28] This finding implies that in our baseline estimates, firm and region $\times$ time fixed effects account for a large part of the variables that would otherwise be correlated with the main bank's capital.

Despite the fact that firm fixed effects account for a large share of the variation in the main bank's capital, columns 2–5 of Table 9 investigate alternative explanations. Potentially, firm characteristics might determine the capital level of the firm's bank and in turn, may be responsible for real effects. We include various firm controls in column 2. None of the firm variables is statistically significant, which mutes the concern that other firm variables might be the underlying explanation for the effect of low bank capital levels. Nevertheless, we investigate whether firm capital or liquidity can serve as alternative explanations in Table OA6 of the Supplementary Material without success.

If firm-level variables cannot serve as alternative explanations for our main effect, perhaps other bank variables are the true driver of our observed effects. Bank total assets, bank liquidity, bank type and the banks usage of the internal ratings based approach (IRB) are thus included in column 3 of Table 9. The results demonstrate that indeed all factors are determinants of pre-flood capital, but when we run these factors in triple interactions against the main bank's capital in

[28]Including the pre-flood disaster measure that is used throughout the article, we show a time-varying disaster measure in this regression to demonstrate the correlation.

TABLE 9

What Determines the Capital of the Firm's Main Bank?

Table 9 presents the results using the main bank's capital as the dependent variable. Column 1 includes the main bank's disaster exposure. Column 2 includes firm-level determinants. Column 3 includes bank-level variables. Column 4 includes relationship variables. Column 5 includes firm × time fixed effects. All regressions include firm fixed effects. Clustered standard errors on the firm level of the point estimates are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable				
	Disaster	Firm-Level	Bank-Level	Relationship	Full FE
CAP_RATIO	1	2	3	4	5
INDIR_DIS_EXP (time var.)	−0.007 (0.069)	0.186 (0.114)	0.162 (0.192)	0.221 (0.192)	0.181 (0.194)
ln(TOTAL_ASSETS)		−0.040 (0.033)	−0.042 (0.033)	−0.034 (0.033)	−0.015 (0.033)
ln(CASH)		0.002 (0.006)	0.002 (0.006)	0.002 (0.006)	0.003 (0.005)
CAPITAL_RATIO		−0.061 (0.090)	−0.060 (0.091)	−0.047 (0.090)	−0.043 (0.087)
ROA (%)		−0.000 (0.001)	−0.000 (0.001)	−0.000 (0.001)	0.000 (0.001)
ln(TOAS_B)			0.080 (0.072)	0.071 (0.072)	0.086 (0.073)
COOP			0.634 (0.502)	0.562 (0.504)	0.555 (0.501)
SAVING			0.822* (0.424)	0.833** (0.424)	0.840** (0.423)
IRB			0.574** (0.270)	0.736*** (0.270)	0.712*** (0.271)
SINGLE				0.117 (0.098)	0.130 (0.095)
REL_LEN				0.379*** (0.100)	0.370*** (0.099)
No. of obs.	414,746	88,383	88,383	88,383	88,377
No. of firms	103,015	23,839	23,839	23,839	23,839
R^2	0.872	0.865	0.865	0.865	0.880
Within R^2	0.000	0.001	0.002	0.004	0.004
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
County × year fixed effects	No	No	No	No	Yes

Tables OA7 and OA8 of the Supplementary Material, the results demonstrate that these factors cannot serve as alternative explanations.

In column 4 of Table 9, we check whether bank-firm relationship variables are correlated with the main bank's capital. A single bank relationship does not necessarily result in higher capital, but a longer relationship between the bank and the firm does. We again test these two potential alternative explanations against our baseline finding in Table OA9 of the Supplementary Material and again find that they cannot serve as alternative explanations.[29]

VI. Conclusion

This article demonstrates the importance of bank capital to prevent real economic shock spillovers from one region to another. We demonstrate this local

[29]We provide a much more detailed analysis of these alternative explanations in Section II of the Supplementary Material.

shock amplification effect of low bank capital by examining a funding shock caused by banks' lending shifts following a natural disaster. As banks redirect lending from nondisaster to disaster areas, firms unaffected by the disaster, yet with a connection to a disaster-exposed bank, reduce their borrowing and, as a result, employment and tangible assets significantly.

This baseline effect is driven mainly 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 a significant reduction in borrowing, employment, and tangible assets. Firms connected to a strongly exposed low-capital bank experience a significant decrease in borrowing of 6.6% and in tangible assets of 6.9%. The estimates further show that banks in particular decrease lending to areas that were unaffected by this flood but are otherwise exposed to high flood risk. These results imply that even small regional shocks can be transmitted through the banking sector to otherwise nonshocked firms, especially if the level of bank capital is small. As small local shocks (which do not necessarily have to be natural disasters) are fairly common, a badly capitalized banking system may unnecessarily propagate shocks to other regions instead of absorbing them.

Our results highlight the importance of bank capital to prevent the propagation of smaller (real economic) shocks through the financial system and avoid lending reductions to firms, even if the health of the financial system or even that of a single bank is not threatened. For banks, this shock propagation might be efficient ex ante, but our results demonstrate that firms and the regional economy suffer real consequences if banks do not hold sufficient capital. This finding provides strong evidence that even on a limited regional scale, low bank capital may involve previously disregarded negative externalities. Policies aimed at increasing banks' capital, preferably through channels other than simply raising capital requirements, may provide benefits even for nonsystemically relevant banks and even if potential bank failure is not an issue.

Supplementary Material

Supplementary Material for this article is available at https://doi.org/10.1017/S0022109022000321.

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