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The impact of natural disasters on banks' impairment flow – Evidence from Germany

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Non-technical summary

Research question

The effects from an unexpected shock in the form of a natural disaster on the banking sector, e.g. as a consequence of climate change, are still debated. On the one hand, financial intermediaries can play a crucial role in mitigating the effects of a natural disaster on the local economy through the provision of additional funding. On the other hand, banks might be constrained in their lending capacities if they are themselves affected by the natural disaster, either directly or indirectly through their clients. In this paper, we explore the empirical relationship between the severe summer flood of 2013 and the performance of regionally operating savings and cooperative banks in Germany.

Contribution

Our paper adds to the existing literature by providing empirical evidence on additional channels through which regionally less diversified banks might be affected by a local natural disaster, including a sectoral dimension. We focus on the summer flood in Germany of 2013, because it was preceded by two extraordinary floods of similar spread in 2002 and 2006, from which we conclude a largely saturated insurance coverage as well as prudent provisioning policies by banks.

Results

Nevertheless, our results show that savings and cooperative banks located in regions affected by the flood in 2013 experienced a higher, but ephemeral, impairment flow compared to their unaffected peers in the years following the flood. Impairments were largely driven by corporate loans concentrated in specific sectors, such as agriculture and manufacturing, and to some extent by retail business, especially mortgage loans. From that, we conclude that expected loss provisioning proved to be insufficient to fully compensate for the actual losses incurred by banks. Therefore, banks and supervisors will have to render their risk management toolkit fit-for-purpose in order to better anticipate and steer losses from natural disasters, especially given that natural calamities in terms of frequency and magnitude of physical risk event due to climate change are expected to rise in the future.

Nichttechnische Zusammenfassung

Fragestellung

Die Auswirkungen einer unerwarteten Naturkatastrophe auf den Bankensektor, z. B. als Folge des Klimawandels, sind nach wie vor umstritten. Einerseits können Finanzintermediäre eine entscheidende Rolle bei der Abmilderung der Auswirkungen einer Naturkatastrophe auf die lokale Wirtschaft spielen, indem sie zusätzliche Finanzmittel bereitstellen. Andererseits können Banken in ihren Kreditvergabekapazitäten eingeschränkt sein, wenn sie selbst von der Naturkatastrophe betroffen sind, entweder direkt oder indirekt über ihre Kunden. In diesem Beitrag untersuchen wir empirisch die Auswirkungen des schweren Hochwassers im Sommer 2013 auf die regional tätigen Sparkassen und Genossenschaftsbanken in Deutschland.

Beitrag

Unsere Ergebnisse ergänzen die bestehende Literatur, indem wir empirische Belege zu weiteren Wirkungskanälen einschließlich einer sektoralen Dimension finden, über die regional weniger diversifizierte Banken von einer lokalen Naturkatastrophe betroffen sein könnten. Wir konzentrieren uns auf das Hochwasser aus dem Sommer 2013 in Deutschland, da diesem zwei außergewöhnliche Überschwemmungen mit ähnlichem Ausmaß in den Jahren 2002 und 2006 vorausgingen. Insofern unterstellen wir einen verbreiteten Versicherungsschutz sowie eine entsprechend umsichtige Rückstellungspolitik bei den Banken.

Ergebnisse

Dennoch zeigen unsere Ergebnisse, dass Sparkassen und Genossenschaftsbanken in von dem Hochwasser betroffenen Regionen in den Jahren nach dem Hochwasser einen vergleichsweise höheren, wenn auch vorübergehenden Wertberichtigungsbedarf hatten. Die Wertberichtigungen konzentrierten sich größtenteils auf Unternehmenskredite gegenüber einzelnen Sektoren, wie beispielsweise Landwirtschaft oder das verarbeitende Gewerbe, sowie in gewissem Maße auch auf das Privatkundengeschäft, hier insbesondere Wohnimmobilienkredite. Insofern schlussfolgern wir, dass sich die Rückstellungen auf Basis erwarteter Verluste als unzureichend erwiesen haben, um die tatsächlich entstandenen Verluste der Banken infolge des Hochwassers vollständig auszugleichen. Daher sollten Banken, aber auch Aufsichtsbehörden ihre Risikobewertung vorausschauend ausrichten, um Risiken aus Naturkatastrophen besser steuern zu können, zumal die Häufigkeit und das Ausmaß von Naturkatastrophen aufgrund des Klimawandels in Zukunft voraussichtlich zunehmen werden.

BUNDESBANK DISCUSSION PAPER NO 36/2022

The Impact of Natural Disasters on Banks' Impairment Flow – Evidence from Germany[1]

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Abstract

Climate change causes natural disasters to occur at higher frequency and increased severity. Using a unique dataset on German banks, this paper explores how regionally less diversified banks in Germany adjusted their loan loss provisioning following the severe summer flood of 2013, which affected widespread regions mostly in Eastern Germany. The analysis uses a difference-in-differences estimation with banks being allocated to the treatment and control group based on the region of their primary operational activities. This paper yields various results: German savings and cooperative banks located in the affected regions experienced a significantly higher, but ephemeral, impairment flow in the years following the flood. Impairments were mostly driven by corporate loans concentrated in specific sectors, such as agriculture and manufacturing, and to some extent by retail mortgage loans. While results suggest that the profitability of banks is impacted by additional factors, we do not find evidence that banks suffered from damages to their own property. The results are robust to various model specifications.

Keywords: Natural disaster, climate change, credit risk, profitability, difference-in-differences

JEL-Classification: C12, C21, C23, G21, Q54

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1 Introduction

In July 2021, Western Europe experienced heavy rainfalls causing severe flooding, in particular in areas around the Ahr/Erft in Germany and the Meuse in Belgium, resulting in more than two hundred deaths and damages to physical property and critical infrastructure. According to the World Weather Attribution initiative, the likelihood of such an event to occur under current climate conditions has increased considerably by a factor between 1.2 and 9 when compared to pre-industrial climate conditions (Kreienkamp et al., 2021). With continued global warming, such regionally highly disastrous events are expected to occur more frequently in the future. Even under the 1.5°C scenario, direct losses caused by river floods are estimated to increase by 160-240% compared to present levels (Dottori et al., 2018).

One of the increasing worries is that extreme weather occurrences caused by climate change, also referred to as acute physical risks, may harm the stability of the banking sectors, if not adequately addressed in banks' risk management frameworks even today (NGFS, 2019). This is also due to the fact that insurance coverage often tends to be incomplete and public support measures mostly address only part of the financial losses, leaving affected households and business owners turn to financial intermediaries for additional support (Garmaise and Moskowitz, 2009). However, even for banks the capacity for providing immediate funding might be limited if banks themselves are negatively affected by the natural disaster, either directly or indirectly through their clients (e.g. Noth and Schüwer, 2018; Brei et al., 2019; Schüwer et al., 2019; Bos et al., 2022).

This research paper adds to the still evolving literature that examines the channels through which physical risks affect bank performance. The analysis is based on the severe summer flood of 2013, which mostly concentrated around the Elbe basin, but also affected other regions in Eastern Germany, resulting in direct damages of up to EUR 8 billion in Germany alone (Thieken et al., 2016a). Unlike the Ahr/Erft flood from 2021, floods around the Elbe have been more frequent in the recent past, with the floods from 2002 and 2006 also being considered one-in-a-century events each of them resulting in considerable financial damage (Noth and Rehbein, 2019; BMU, 2021). Because of these extraordinary events sequencing in a relatively short period of twelve years rather than once a century, thereby disproving its nature of being a tail risk, the 2013 flood serves as a natural experiment in two ways. First, the insurance market in those regions is assumed to be more saturated given the higher frequency of events, i.e. firms and households are more likely to have obtained sufficient coverage following earlier events (demand side) or may have even lost their insurance coverage with difficulties to find adequate replacements (supply side). Second, banks in affected regions are assumed to have learnt from previous events also in terms of expected loan loss provisioning. For those reasons, the financial damages resulting from the 2013 flood cannot solely be attributed to one-off effects (Thieken et al., 2016b).

The analysis relies on a difference-in-differences estimation with regional banks being allocated to the treatment and control group based on the region of their primary operational activities and the treatment defined at county-level based on the issuance of disaster alerts. The results indicate that German savings and cooperative banks located in affected regions experienced a significantly higher, but ephemeral, impairment flow in the years following the flood. Impairments were mostly driven by corporate loans concentrated in specific sectors, such as agriculture and manufacturing, and to some extent by retail business, especially mortgage loans. Furthermore, the profitability of banks in affected regions has been impacted by additional factors. However, there is no evidence that banks suffered from damages to their own property. The effects are economically significant despite the financial support provided by the government to affected households or business owners. Furthermore, the results suggest that the extent of additional loan loss provisioning varies across economic sectors, which hints not only at sectoral, but also to firm-level vulnerabilities to physical risks.

Finally, while assuming an ex-post perspective when analysing past events as done in this case, the results also stress the importance of integrating physical risks in a more forward-looking manner into banks' risk management frameworks. This is evidenced by the fact that the areas under consideration were already affected by two one-in-a-century floods shortly before 2013 flood, namely in 2002 and 2006, thereby not only indicating the higher frequency at which such extraordinary events occur already today, but also that incurred losses exceeded the expected loss anticipated by banks. Therefore, banks (but also supervisors) will have to render their risk management toolkit fit-for-purpose in order to better anticipate and steer losses from physical risk events, especially given that natural calamities in terms of frequency and magnitude are expected to rise in the future (e.g. Caloia and Jansen, 2021).

The remainder of the paper is structured as follows. The next section reviews the related literature, while section 3 provides more background information on the 2013 flood in Germany. Section 4 and 5 describe the data and identification strategy in more detail. The interpretation of results along with a range of robustness checks are presented in section 6 and 7, respectively. Finally, section 8 covers the conclusion, also hinting at related future research possibilities.

2 Related literature

Previous studies often focused on the impact of natural disasters on the macroeconomy (e.g. Noy, 2009; Toya and Skidmore, 2007; Fomby et al., 2013; Panwar and Sen, 2019), while the literature that studies the implications of natural disasters for the banking sector is still burgeoning, with partially quite heterogeneous results depending on the particular research design. Bos et al. (2022) provide a framework to illustrate two of the key mechanisms through which banks may be affected by natural disasters. While banks might suffer from higher credit risk in the short term through the destruction of borrowers' assets limiting their repayment capacity, natural disasters might also stimulate loan demand to finance reconstruction. Garmaise and Moskowitz (2009) develop a model showing that banks might also restrict lending to disaster-prone regions, especially under prevailing insurance market imperfections.

A first strain of empirical literature deals with the effects of natural disasters on the stability of banks. Analysing different types of natural disasters across more than 160 countries, Klomp (2014) finds evidence for an increase in banks' probability to default following hydro-meteorological disasters, such as floods and storms, especially in financially constrained countries, which have more difficulties reverting to their original growth state due to financial frailty, lack of credit availability and insufficient insurance coverage. Specifically for the United States, Noth and Schüwer (2018) suggest that damages from weather-related natural disasters affect banks' stability as evidenced by higher probabilities of default, higher credit risk, and lower profitability in the short-term following a natural disaster. On the contrary, Blickle et al. (2022) find that weather-related natural disasters only seem to have limited impact on banks' stability in the US as banks manage to offset losses by increasing credit supply, while especially local banks tend to restrict mortgage lending to areas, which are more prone to flood risk. When analysing banks with differing geographical coverage of operations in the US, Walker et al. (2022) demonstrate that risks to capital are more precarious for smaller, regionally less diversified banks, which are located in high-flood risk areas. Focussing on smaller, less significant banks in Europe, Pagliari (2021) also suggests a lower profitability following an adverse event for those banks located in disaster-prone areas. For the East Asia Pacific region, Thuy Thi Nguyen et al. (2020) provide evidence that natural disasters are associated with higher bank credit and default risk, especially in developing countries.

Another stain of literature explicitly investigates banks' risk taking and lending activities towards firms exposed to natural disasters. Faiella and Natoli (2018) find that lending to non-financial firms located in areas of high flood risk is generally lower compared to firms in low-risk areas in Italy. When analysing loan applications and (dis)approvals in the aftermath of volcanic eruptions in Ecuador, Berg and Schrader (2012) provide evidence for credit rationing despite an increasing demand for credit which is, however, less pronounced in close bank-borrower relationships which seems to be an important factor for maintaining access to credit. Cortés and Strahan (2017) examine how US banks respond to local credit demand shocks from natural disasters. While larger banks do not seem to adjust their lending to non-affected regions, smaller banks seem to compensate an increasing supply of credit to affected regions by reducing their exposures to non-affected regions. Similar findings are reported by Ivanov et al. (2022) who focus on loan syndicate networks. On the contrary, Bos et al. (2022) show for the US that after natural disasters commercial banks reshuffle their investments and rather extend real estate financing by selling government bonds, while Duqi et al. (2021) find that especially banks in less competitive markets tend to extend their financing of mortgage loans following natural disasters. Focussing on securitization dynamics in the aftermath of natural disasters, Ouazad and Kahn (2021) postulate an increase in mortgage securitizations, which allow for the transfer of climate risk.

The paper closest to ours is Koetter et al. (2020) who focus on capacity of small and medium-sized banks in Germany to provide recovery lending in the aftermath of the flood in 2013. According to their findings, recovery lending to firms in affected regions is especially provided by banks headquartered in unaffected regions, also stressing the role of relationship lending. While their results do not suggest that the expansion in lending is associated with higher credit risk or rent skimming, our study provides empirical evidence on multiple credit risk channels affecting banks' profitability. Rehbein and Ongena (2020) who also study the effects of the 2013 flood in Germany find evidence for local spillovers from weakly capitalized banks affected by the flood to firms located in unaffected areas through a reduction in lending.

Some studies also focus more explicitly on the pricing of disaster risk. Garbarino and Guin (2021) find that lenders fail to update their valuation of collateral in areas of continued flooding in England, and hence, seem to underprice climate-related credit risk when studying mortgage and property transactions in the UK around a severe flood event in 2013-14. In contrast, Brown et al. (2021) provide evidence that banks indeed charge higher interest rates and tighten credit standards for borrowers increasingly relying and extending their credit lines following abnormal winter weather affecting part of the US during 2014-15. Likewise, findings by Nguyen et al. (2021) suggest that US banks seem to charge higher premia for longer-term mortgages exposed to sea level rise. Similar findings are also reported by Barth et al. (2019) who demonstrate that US banks seem to increase interest rates on loans, while at the same time also respond to natural disasters by higher interest rates on deposits to secure funding. According to Brei et al. (2019), deposit withdrawals following hurricane strikes in the Caribbean even led to a contraction in bank lending due to the inability to rollover short-term funding rather than loan defaults and other negative shocks bank capital.

Our paper adds to the existing literature by providing empirical evidence on the channels through which regionally less diversified banks are affected by a local natural disaster. We focus on the summer flood in Germany of 2013, which was preceded by two extraordinary floods of similar spread in 2002 and 2006, from which we conclude a largely saturated insurance coverage as well as prudent provisioning policies by banks. Yet, we find that German savings and cooperative banks located in regions affected by the flood in 2013 experienced a higher, but ephemeral, impairment flow compared to their unaffected peers in the years following the flood. Furthermore, our results suggest that the extent of additional loan loss provisioning varies across economic sectors, while the profitability of banks in affected regions seems to be impacted by additional factors in the years following the flood.

3 The summer flood of 2013

In June 2013, a large-scale flooding occurred in major parts of central Europe causing 25 deaths and billions of economic losses. Most of the main rivers had high water levels where severe flooding occurred particularly along the Danube and Elbe rivers. The two key drivers for this natural calamity were initial soil moisture and heavy rainfalls. The exceptional rainfall amounts that finally triggered the flood occurred at the end of May/ beginning of June 2013 (DKKV, 2015). Some embankments were unable to withstand the high-water levels at certain locations which resulted in dike breaches and inundation (Stein and Malitz, 2013). Figure 1 of the German Weather Service below depicts cumulated rainfalls recorded in Germany between May 30th and June 2nd, 2013, mostly concentrated in South and Eastern Germany.

The resulting summer flood of June 2013 was one of the most damaging in Germany over the last 60 years causing immense direct and indirect damages (Schröter et al., 2015). Direct damages are mostly referred to as immediate consequences, such as life losses or injuries as well as the destruction of physical assets, while indirect damages are commonly understood as secondary effects (DKKV, 2015). These include disruptions to economic activity or transport systems, but also migration or adverse psychological effects (Noy, 2009). The German Insurance Association (GDV) estimated that economic direct losses amounted to roughly EUR 6.6 billion with an additional EUR 1.8 billion of insured losses. While insurance coverage even more than doubled since 2002, still only one third of the German population was insured against flood risk in 2013 (GDV, 2013). Saxony, Saxony-Anhalt and Bavaria were the federal states in Germany which were most affected by the flood, each accounting for 20-37% of the overall losses. However, as pointed out by Thieken et al. (2016a), loss estimates are normally based on direct damages and are therefore likely to underestimate overall losses significantly.

To mitigate financial losses, the German government set up a financial support fund with a total volume of EUR 8 billion shortly after the flood. Additionally, an amount of around EUR 108 million in form of private donations was recorded (Thieken et al., 2016a).

Affected households or firms could have reimbursed up to 80% of their direct damages. Indirect damages and losses, such as business interruptions, were not part of the initial estimate and not covered by government aid. This was an important factor for firms as almost 90% of businesses claimed supply chain interruptions which led to sale losses and forgone income (DKKV, 2015). Based on a survey of households and firms conducted nine months after the disaster, Thieken et al. (2016a) additionally record for almost half of the respondents a delayed reimbursement process.

The anticipation of public support measures might have also incentivized households and firms not to increase their insurance coverage, known as charity hazard (Raschky et al., 2007; Andor et al., 2020). This might also explain why insurance penetration did not significantly increase even after the flood in 2013 (Thieken et al., 2016a).

4 Data and methodology

4.1 Bank-level data

The following analysis relies on a unique bank-level dataset, which is built on three pillars from the Bundesbank's statistical data warehouse, namely the balance sheet statistics, the borrowers' statistics, as well as and banks' profit and loss accounts. The balance sheet statistics records monthly information on all German banks' balance sheet items. The borrowers' statistics records quarterly information on outstanding domestic loans to corporations and households subdivided by type, duration, and sector. Banks' profit and loss accounts are available on a yearly frequency. The final dataset for our analyses aggregates yearly information on all German banks for the time period 2009-2015 in Germany. We exclude the period before 2009 to avoid interference with the financial crisis of 2007/08 as well as the years after 2015 based on results suggesting the shock of the natural disaster to be rather short-lived, thereby also minimizing the effect of confounding events. Bank-level data are merger treated, i.e. merged banks enter the dataset as a new bank, and winsorized at the 1st and 99th percentile to control for outliers. Furthermore, granular data on real GDP obtained from the Federal Statistical Office and unemployment rates from the Federal Employment Agency are included in the dataset as controls for the economic conditions at the county level.

Because our analysis is built on county-level disaster information, we limit the sample to German savings and cooperative banks only as they tend to be less diversified on a regional scope than private banks either de jure following the regional or de facto restricted to operating in their region (Koetter et al., 2020). Thus, by only including savings and cooperative banks, we avoid endogeneity in terms of the banks' location. It is important to note that regional savings and cooperative banks, i.e. excluding the central institutions of both sectors at federal (state) level, still account for around 23% of total bank assets in Germany in June 2013 (Deutsche Bundesbank, 2013). They can be characterized as banks engaging in close bank-borrower relationships with their clients at the regional level.

4.2 Identification strategy

We implement a standard difference-in-differences analysis to estimate the effect of the flood on the impairment flow. In this setting, the treatment itself is defined by the regional spread of the natural disaster, which is exogenous by construction. The allocation of banks into the treatment or control group is determined by whether or not the local administration of a county, where a particular bank is headquartered, issued an official disaster alert because of the flood. Generally, any county administration in Germany can issue such a disaster alert if the damages significantly impede the current living or become even life threatening. The conditions based on which a disaster alert can be issued are regulated by the civil protection law of the federal states. The alert is also a signal to the federal government that the county administration is not capable anymore to handle the situation on its own, and hence, a request for operational but also financial assistance.

In total, 55 counties had issued a disaster alert during the 2013 flood (BMI, 2013). Figure 2 depicts the allocation of counties into treatment and control group. Banks are assigned to the treatment group if they are located in counties that had issued a disaster alert during the flood; these are highlighted in red. The control group constitutes of banks whose counties are bordering on the affected counties, but where no disaster alert was issued. Banks located in these counties are assumed to be most comparable, e.g. in terms of size and business model, with the only difference being that these banks were not directly operating in regions affected by the natural disaster (Huang, 2008). The first adjacent counties, highlighted in yellow, are those that are directly bordering on the treated counties. However, one concern is that even first adjacent counties might have been partially flooded, just not severely enough that would have justified the issuance of a disaster alert. Therefore, we also identify the second adjacent counties, highlighted in green, which are bordering on the first adjacent ones. With these counties, we are more confident to rule out any smaller flooding and banks being affected by the flood, while these banks are still comparable to those in affected areas. In our baseline, we group banks located in the first adjacent or second adjacent counties to the control group.

Overall, we identify 642 German savings and cooperative banks located in the treated and control counties for the observation period from 2009-2015, of which 191 banks belong to the treatment group and 451 banks to the control group with 202 and 249 located in first and second adjacent counties, respectively. In summary, the model itself benefits from exogeneity in various dimensions: exogeneity in terms of banks' location, exogeneity in term of the treatment (natural disaster) and exogeneity in terms of selection of banks into treatment and control group.

4.3 Model specification

We estimate the impact of the 2013 summer flood on the banks' impairment flow using the following difference-in-differences specification as our baseline:

$$y_{it} = \beta_0 + \propto_i + \propto_t + \sum_{j=1}^{k} \beta_j X_{jit} + \tau_1 (Treated_i \times Post_t) + \varepsilon_{it}$$

where y_{it} is the dependent variable represented by the impairment flow of bank i in year t. $Treated_i$ is the dummy indicating a bank's location. It is equal to one if the bank is located in a county which had issued a disaster alert due to the flood, and zero otherwise. Since the flood occurred in the middle of 2013, $Post_t$ is an indicator variable equal to one if the year-end accounts in our dataset relate to years 2013 or later, and zero otherwise. The coefficient of interest capturing the treatment effect is τ_1 from the interaction term. The interaction indicator variable is equal to one for observations on banks in the treatment group for the post period. The sample period for our baseline analysis covers the years from 2009-2015, hence, we remain with four pre-treatment and three treatment years. We exclude the year 2016 for now because the effects of the flood are found to be short-lived. By restricting the sample period, we also minimize the effects of confounding events. Bank fixed effects $\propto_i$ are included to control for unobservable bank-specific time-invariant characteristics. Time fixed effects $\propto_t$ are included to control for unobservable, but time-varying characteristics that are constant across banks. We also include several observed bank-related and macroeconomic explanatory variables to account for additional heterogeneity. Standard errors are clustered at the county level following the identification of the treatment group at county level. The results are insensitive to clustering at different levels.

4.4 Descriptive statistics

Table 1 summarizes the definition of variables that enter the regressions. Our main analyses focus on the effect of the flood on banks' relative impairment flow, measured as the net valuation adjustments on domestic loans over total assets. We concentrate on the domestic part of the loan portfolio for which a sectoral breakdown is available in the Bundesbank borrowers' statistics. We expect a higher impairment flow for banks in affected counties following the flood. Furthermore, we also scrutinize other dependent variables, namely return on assets (before any release or allocation to German nGAAP reserves), net interest income, and changes in own property valuation, which could hint at other potential channels through which banks' performance might be affected, such as the granting of forbearance measures to affected clients or damages to own property and infrastructure. A set of standard independent variables controls for bank-specific characteristics, including bank size, capital adequacy, asset risk, liquidity, or business mix. Other bank-specific variables control for loan volume, non-performing loans, and cost-to-income ratio. Additional macroeconomic variables, namely the regional GDP growth and the change in the unemployment rate serve as controls for the economic conditions at county level.

Panel A of Table 2 shows the descriptive statistics for the variables used in the analyses for the entire sample period from 2009 to 2015 by treatment and control group. In total, the dataset consists of 3,921 yearly observations with 1,188 observations from 191 banks in the treatment group and 2,733 observations from the 451 banks in the control group, i.e. banks located in the first and second adjacent counties. The average impairment flow for the treated group is 0.05 and 0.07 for the control group.

Panel B of Table 2 additionally shows the descriptive statistics and normalized differences separately for the treated and control group for the years 2009-2012 to examine potential differences in trends between the two groups prior to the flood. If the normalized difference is larger than 0.25, differences are deemed significant (Imbens and Wooldridge, 2009; Schüwer et al., 2019; Koetter et al., 2020). In our case, the normalized difference is below 0.25 for the dependent variables and most of the independent variables we use. The significant difference concerning loan volumes and liquidity can be explained by the relatively larger share of loans compared to securities which is typical for small and medium-sized banks in Western Germany, mainly because of better local investment opportunities. However, our main results are insensitive to the exclusion of these variables from our model. We are confident to assume that the assumption of parallel trend holds.

5 Results

5.1 Impairment flow

We estimate our baseline equation in a standard difference-in-differences framework to analyse the effect from the flood on the impairment flow on banks in affected counties. The results are shown in Table 3. Column 1 represents the parsimonious specification only including the interaction term $Treated_i \times Post_t$ as well as bank and time fixed effects, but without the inclusion of any additional bank-level or macroeconomic control variables. In column 2 and 3, we add bank-specific control variables in two steps to account for the larger difference in means between the treatment and control group regarding loans and liquidity, which however does not affect our results. Column 4 depicts the full regression with the addition of macro-specific control variables. Our coefficient of interest for the interaction term $Treated_i \times Post_t$ is positive and statistically significant at the 1% level in all specifications. Accordingly, banks located in areas affected by the flood experienced a significantly higher impairment flow by around 0.03 percentage points compared to the banks in the control group. The magnitude of the effect is also economically significant considering the mean of the impairment is 0.06 for the treatment group in the pre-treatment period.

The findings are different from Koetter et al. (2020), who cannot find a significant effect of the flood on the credit risk of banks exposed to affected areas, however using the stock of impaired loans as a ratio of gross loans as dependent variable. They relate this to the lower information asymmetry between regional banks and their local borrower due to relationship lending and also refer to the public and private financial support measures provided. However, as indicated by our results, banks in affected regions still had to increase their loan loss provisioning following the flood. The results are even more striking as the 2013 flood was preceded by two floods of similar spread in 2002 and 2006, both also considered as one-in-a-century events. Therefore, assuming a largely saturated insurance coverage, both from demand and supply side, the results suggest that banks still had to absorb losses stemming from not (fully) insured exposures. Also, banks in affected regions can be assumed to have adjusted their expected loan loss provisioning to some extent to account for flood risk given the increasing frequency of tail risk events in affected regions. Yet, we find a significant effect on the impairment flow, which hints at ex-ante insufficient expected loan loss provisioning. With physical risk events becoming even more frequent and severe in the upcoming years as a consequence of climate change, our findings underscore the need for taking a forward-looking perspective when accounting for the impact of climate-induced physical risk events on banks' balance sheets.

In a subsequent set of analyses, we gauge the effect of the flood on the impairment flow by economic sectors separately for corporate and retail exposures. The results are summarized in Table 4. Column 1 shows again the baseline regression based on the aggregate impairment flow from Table 3 column 4. Referring to columns 2 and 5, the total effect (0.029) can be partitioned into the effect originating from corporate (0.022) and retail exposures (0.007), both of them being significant at the 5% level. These findings suggest that the total effect largely stems from corporate exposures, while retail exposures only contribute to a smaller extent. This is in line with the previous findings based on surveys done by Thieken et al. (2016a). Accordingly, public financial support for firms and households only covered up to 80% of direct damages. However, losses arising from supply chain interruptions were only classified as indirect damages. This might be one factor explaining the different effects for corporate and retail exposures.

When further decomposing corporate and retail exposures into sub-sectors, we find slightly significant, but small effects for firms operating in the agricultural (column 3) and manufacturing (column 4) sectors. For the agricultural sector, the timing of the flood in early summer was critical, presumably affecting crop fields the most. For the manufacturing sector, the floods were likely to impede production and reduce outputs because of damage to facilities, equipment, and raw materials. Regarding retail exposures, the effect distributes evenly across mortgage (column 6) and other retail loans (column 7).

Finally, we provide an alternative approach to examine the disaggregated yearly effect of the flood on impairments by interacting the $Treated_i$ indicator with the yearly time fixed effects instead of the $Post_t$ identifier. The year 2012, i.e. the year before the event, serves as the base year. Thereby, the existence of parallel trends is also further substantiated. Figure 3 plots the point estimates and 95% confidence intervals from the specification above. The figure shows that treatment and control group were not statistically different from each other in any year before the flood occurred. Only after the disaster event, the coefficients become statistically significant. The effect of the flood is strongest in the year in which it occurs with some persistency, but seems to vanish after three years following the flood as indicated by the decrease in economic and statistical significance. This concords with the literature finding a rather short-term effect of natural disasters on banks in developed countries (Garmaise and Moskowitz, 2009; Noth and Schüwer, 2018).

5.2 Additional channels

To test for additional channels through which banks' profit and loss accounts might be affected by the flood, we employ alternative specifications of our baseline equation using alternative dependent variables. The results are summarized in Table 5.

To this end, we start by regressing the return on assets on the interaction term and the set of control variables from our baseline regression (column 1). Leveraging on the use of supervisory data, we are able to define the numerator as net income before any release or allocation to German nGAAP reserves, which are not disclosed, but allowing banks to smooth profits or losses (Bornemann et al., 2012). The effects of which can be substantial (Dombret et al., 2017). The unique specification of the return on assets allows us to narrow down any effects to the actual operational profitability. In line with previous findings, the interaction term shows a negative coefficient (-0.059), which is also highly significant at the 1% level. As the coefficient is much higher in absolute terms when compared to our baseline specification in which the impairment flow serves as dependent variable, results seem to hint at other profit and loss items, apart from impairments, through which banks in our treatment group were affected by the flood.

Therefore, we also scrutinize net interest income over total assets as dependent variable (column 2). A negative coefficient for the interaction term would be consistent to our previous findings and could hint at overdue interest payments resulting from forbearance measures or defaulted exposures. However, we do not find any statistically significant relationship, similar to Koetter et al. (2020).

Finally, we investigate whether banks themselves were also directly affected by the flood, e.g. through damages to property, equipment and other infrastructure. For this, the year-on-year change of the stock of tangible assets over total assets serves as dependent variable (column 3). However, in this setup the coefficient of the interaction term remains statistically insignificant.

6 Robustness checks

We employ different sets of robustness checks. First, we check for the sensitivity of our baseline results against the inclusion of different sets of fixed effects. The results are summarized in Table 6. In column 1, no fixed effects are included. Therefore, $Treated_i$ and $Post_t$ are not absorbed and enter the regression. In column 2, we only include bank fixed effects to control for unobserved bank-specific, but time-invariant characteristics. Next, we include the interaction of bank type and time fixed effects in column 3, where bank type acts a binary identifier for savings and corporative banks, respectively. Furthermore, we address the potential concern that affected areas are mostly located in former East Germany, which is even after more than 30 years since the German reunification in 1990 economically still lagging behind, and that our model might therefore suffer from unobserved heterogeneity arising from this fact. To this end, we include in column 4 an interaction of time fixed effects with an east/west indicator identifying if a bank is located in a county, which belonged to former East Germany. The last column 5 represents the most restrictive specification including additionally an interaction term for bank type, east/west and time fixed effects. As expected, the economic magnitude reduces somewhat with each column as we control for more unobserved heterogeneity. However, the coefficient of interest remains statistically significant throughout all specifications.

A second set of robustness checks concerns the definition of the control group. As outlined above, the allocation of banks to the treatment group is based on whether the county, in which a bank is headquartered, had issued a disaster alert because of the flood. The control group is exogenously defined by only including banks, which are located in a county bordering on a treated county. We call these bordering counties the first adjacent counties. However, there is one concern that even first adjacent counties might have been partially flooded, just not severely enough that would have justified the issuance of a disaster alert. Therefore, we also identify the second adjacent counties, which are bordering on the first adjacent ones. Thereby, we limit the control group in our baseline regression to banks, which can still be assumed to be comparable to those in affected areas. To gauge the effect of the flood based on alternative control groups, we run our baseline regression only against first and second adjacent countries, respectively, as well as against all non-treated counties in Germany. The results are displayed in Table 7, with column 1 showing again the results of our baseline specification with banks in first or second adjacent countries being assigned the control group. Column 2 shows the results when limiting the control group to first adjacent counties only. We find a slightly smaller effect of the flood on the impairment flow both in terms of economic and statistical significance, which is consistent with our hypothesis that these counties might have been also affected by the flood, but to a lesser extent. In contrast, column 3 shows the results when limiting the control group to second adjacent counties only. As a result, the effect becomes larger and even more significant at slightly lower standard errors. This finding is plausible and consistent to the previous one and provides further support for extending the control group by banks located in second adjacent countries. It should also be noted that the adjusted R^2 is also slightly larger when including both first and second adjacent countries in the control group. In column 4, the control group includes all regional banks in Germany located in non-treated areas for the sake of completeness. As a result, the effect becomes even larger. However, when defining the control group more spaciously, it might become less relevant for a meaningful comparison against banks in affected areas.

Additional robustness checks are presented in Table 8. To address potential autocorrelation concerns in our multi-period set-up, we collapse our dataset by averaging the pre and post treatment observations by banks as suggested by Bertrand et al. (2004). Hence, the number of observations drops to 1,086 as shown in column 1. However, our coefficient of interest remains similar in magnitude and highly significant at the 1% level, while also the adjusted R^2 increases significantly. Therefore, we rule out that our results are driven by serial correlation.

Furthermore, we include a pre-disaster exposure indicator as suggested by Cortés and Strahan (2017), which is equal to one in the years before the flood. The coefficient is denoted as $Treated_i \times Pre_t$ in column 2 and, as expected, is far from being statistically significant, again reinforcing that our results are not driven by systematic differences relating to observable characteristics in the years before the flood.

Finally, we perform two other placebo tests. The first one is based on assuming a 'fake' flood in 2012, i.e. the year preceding the actual flood. We re-estimate our baseline specification, but only for the pre-flood period (2009-2012). The coefficient of interest should become insignificant in order for the placebo test to hold. As shown in column 3, the coefficients is indeed insignificant when assuming a pseudo flood in 2012. The second placebo test is based on a randomized allocation of banks into treatment and control group, which is shown in column 4. Similarly, we expect the coefficient to become insignificant which indicates that our results are indeed driven by exogenously given assignment into treatment and control groups based on the approach described above. We also repeat this randomization 1,000 times. In only 51 out of 1,000 runs, the effect of the flood is statistically significant at the 5% level for affected banks. Therefore, we are confident that there was no other confounding event, which yielded the statistical significances in our coefficient estimates other than the flood.

7 Conclusion

Empirical evidence on the effects of a local natural disaster on the banking sector is yet inconclusive and still evolving. Our paper adds to the current literature by providing empirical evidence on the channels through which regionally less diversified banks are affected by a local natural disaster. The analysis relies on a difference-in-differences estimation with banks being allocated to the treatment and control group based on the region of their primary operational activities and the treatment being defined at county-level based on the issuance of disaster alerts. Our results indicate that German savings and cooperative banks located in affected regions experienced a higher, but ephemeral, impairment flow compared to their unaffected peers in the years following the summer flood in 2013. Impairments were mostly driven by corporate loans concentrated in specific sectors, such as agriculture and manufacturing, and to some extent by retail business, especially mortgage loans. Furthermore, we find that the profitability of banks in affected regions seems to be impacted by additional factors in the years following the flood.

The results are even more striking as the 2013 flood was preceded by two floods of similar spread in 2002 and 2006, both also considered as one-in-a-century events. Therefore, assuming a largely saturated insurance coverage, both from demand and supply side, results suggest that banks still had to absorb losses stemming from non or not fully insured exposures. Also, banks in affected regions can be assumed to have adjusted their expected loan loss provisioning following the previous floods to account for flood risk. Yet, we find a significant effect on the impairment flow which hints at ex-ante insufficient expected loan loss provisioning. With physical risk events becoming even more frequent and severe in the upcoming years as a consequence of climate change, our findings underscore the need for taking a forward-looking perspective when accounting for the impact of climate-induced physical risk events on banks' balance sheets.

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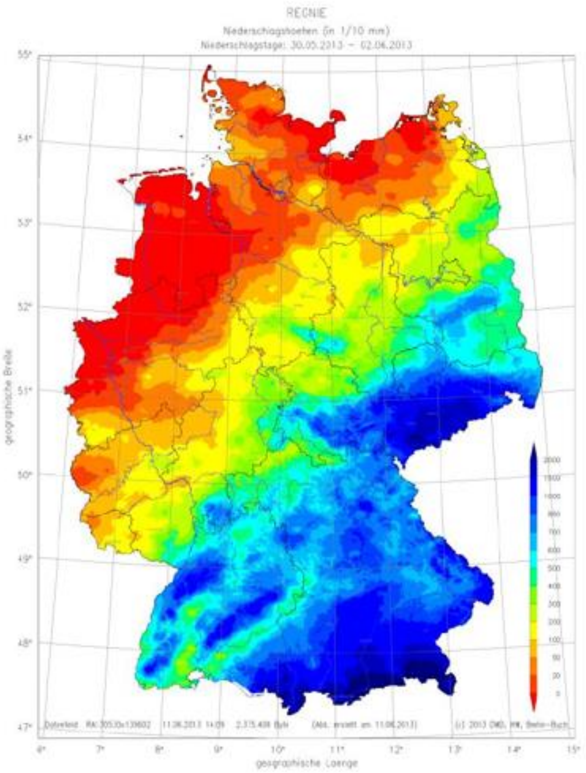
Tables and Figures

Figure 1: Cumulated rainfalls over the period May 30th - June 2nd, 2013



Retrieved from Stein and Malitz (2013).

Figure 2: Composition of Treatment and Control Group

This figure shows the composition of the treatment and control group. Counties are highlighted in red where a disaster alert was issued. The banks located in these counties are in the treatment group. The counties bordering on the treated counties are marked in yellow and called first adjacent counties. We additionally identify the second adjacent counties which are bordering on the first adjacent counties. In our baseline, banks located in either the first or second adjacent counties belong to the control group.

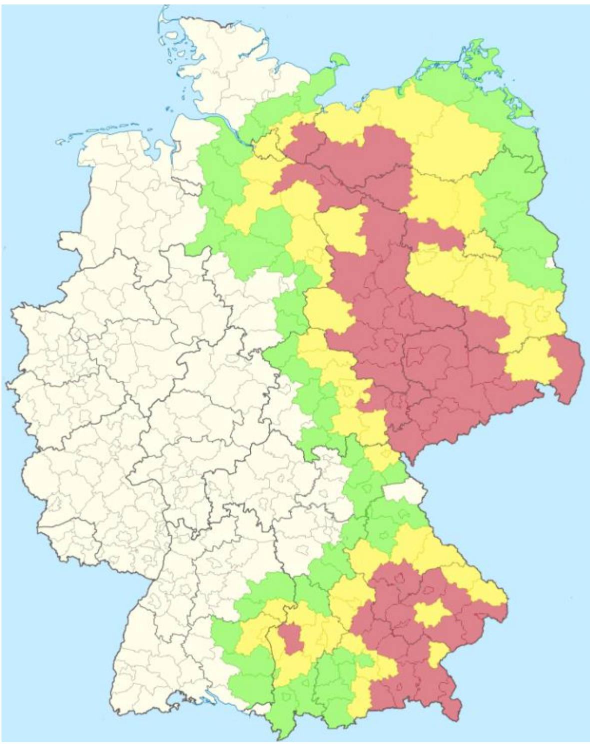


Table 1: Definition of variables

The table provides the definition of the variables used in our regressions, which are observed annually. All bank-related variables are sourced from the Bundesbank's statistical data warehouse, namely the balance sheet statistics, the borrowers' statistics, as well as banks' profit and loss accounts. Macroeconomic and financial data are sourced from official accounts.

Variable name	Unit	Variable definition
Dependent variables		
Impairment flow	%	Net valuation adjustments on domestic loans over total assets
RoA	%	Return on assets; calculated as net income before any release or allocation to German nGAAP reserves over total assets
Property	%	One-year change in the value of property investments over total assets
Net interest income	%	Net interest income over total assets
Independent variables		
Bank size	ln	Natural logarithm of total assets
Capital adequacy	%	Regulatory capital ratio; calculated as total capital over risk-weighted assets
Business mix	%	Herfindahl-Hirschman index over nine business sectors
Loans	%	Domestic loans to non-financial corporations over total assets
Liquidity	%	Loans-to-deposits ratio
CIR	%	Cost-to-income ratio
NPL ratio	%	Non-performing loans over total loans
Asset risk	%	Risk-weighted assets density; calculated as risk-weighted assets over total assets
GDP growth	%	County-level growth of real GDP obtained from the Federal Statistical Office
Unemployment	%	One-year change in the county-level unemployment rate obtained from the Federal Employment Agency
Treated	0/1	Indicator variable equal to one if the bank is located in a county which has issued a disaster alert during the Elbe flood; equal to zero if the bank is located in the first or second adjacent county bordering on the treated counties
Post	0/1	Indicator variable equal to one for the years 2013-2015 and zero for 2009-2012

Table 2: Descriptive statistics

This table shows the descriptive statistics for the variables used in the regressions. Panel A covers the whole sample period (2009-2015) by treatment and control group whereas Panel B only covers the pre-treatment period (2009-2012) for the assessment the parallel trends. For that purpose, Panel B also includes the normalized differences (ND) between the treatment and control group, where a normalized difference greater ±0.25 commonly hints at significant differences in the trends prior to the treatment. All bank-related variables are winsorized at the 1st and 99th percentiles.

Panel A: Descriptive statistics by treatment and control group

	Treatment group (191 banks)						Control group (451 banks)					
					Percentile						Percentile	
Variables	**N**	**Mean**	**SD**	**5th**	**50th**	**95th**	**N**	**Mean**	**SD**	**5th**	**50th**	**95th**
Impairment flow	1188	0.05	0.15	-0.15	0.02	0.33	2733	0.07	0.18	-0.16	0.03	0.39
RoA	1188	1.07	0.35	0.47	1.07	1.71	2733	1.00	0.36	0.44	0.98	1.62
Property	1188	0.01	0.08	-0.05	-0.01	0.15	2733	0.01	0.06	-0.05	-0.01	0.13
Net interest income	1188	2.21	0.39	1.55	2.24	2.82	2733	2.25	0.40	1.63	2.24	2.93
Bank size	1188	20.04	1.12	18.25	19.92	21.96	2733	19.94	1.30	17.65	19.97	22.08
Capital adequacy	1188	18.43	4.84	12.49	17.33	28.27	2733	17.92	4.67	12.10	16.94	27.41
Business mix	1188	21.84	5.33	15.99	20.49	31.28	2733	24.43	10.34	16.42	21.59	40.82
Loans	1188	51.16	14.48	26.66	52.58	72.24	2733	56.76	13.10	32.60	57.40	77.94
Liquidity	1188	1.11	0.38	0.51	1.12	1.75	2733	1.32	0.45	0.69	1.25	2.18
CIR	1188	65.16	8.19	51.98	64.96	78.78	2733	65.79	8.25	51.56	65.84	78.92
NPL ratio	1188	3.18	2.30	0.54	2.68	7.85	2733	3.33	2.51	0.50	2.81	8.05
Asset risk	1188	52.84	12.95	30.91	53.98	72.10	2733	55.72	11.01	36.96	56.26	73.10
GDP growth	1188	1.32	4.82	-5.80	1.39	7.69	2733	1.49	4.36	-6.28	1.72	7.54
Unemployment	1188	-0.29	0.56	-1.26	-0.21	0.54	2733	-0.18	0.50	-0.94	-0.17	0.63

Table 2 continued

Panel B: Descriptive statistics for the pre-treatment period (2009-2012)

	Treatment group		Control group		
Variables	**Mean**	**SD**	**Mean**	**SD**	**ND**
Impairment flow	0.06	0.16	0.09	0.2	0.117
RoA	1.12	0.37	1.02	0.39	0.186
Property	0.01	0.06	0	0.06	0.118
Net interest income	2.27	0.4	2.31	0.42	0.069
Bank size	19.99	1.12	19.88	1.3	0.064
Capital adequacy	17.94	4.9	17.37	4.65	0.084
Business mix	21.68	5.25	24.33	10.16	0.232
Loans	49.97	14.31	55.84	12.79	0.306
Liquidity	1.12	0.39	1.33	0.45	0.353
CIR	64.69	8.17	65.5	8.28	-0.07
NPL ratio	3.83	2.5	3.89	2.63	0.017
Asset risk	51.53	13.24	54.64	11.07	-0.18
GDP growth	1.16	6	1.21	5.2	0.006
Unemployment	-0.36	0.66	-0.22	0.6	0.157

Table 3: Effect of the flood on the impairment flow

This table shows the results from the regression of our baseline specification where the impairment flow serves as dependent variable for the sample period 2009-2015. Column 1 excludes any bank or macroeconomic control variables. In column 2 and 3, we add bank-specific control variables, with the difference that variables on loans and liquidity only enter into the regression in column 3. Column 4 depicts the full regression with the addition of macroeconomic control variables. The statistical significance of results is indicated by *, **, *** for the 10%, 5% and 1% level, respectively. Standard errors clustered on county level are in parentheses.

	(1) Impairment flow	(2) Impairment flow	(3) Impairment flow	(4) Impairment flow
Treated × Post	0.029***	0.030***	0.029***	0.029***
	(0.011)	(0.010)	(0.010)	(0.010)
Bank size		0.127*	0.136*	0.142*
		(0.076)	(0.081)	(0.082)
Capital adequacy		-0.004*	-0.003	-0.003
		(0.002)	(0.002)	(0.002)
Business mix		0.001	0.001	0.001
		(0.001)	(0.001)	(0.001)
NPL ratio		0.018***	0.018***	0.018***
		(0.003)	(0.003)	(0.003)
Asset risk		0.000	-0.001	-0.001
		(0.001)	(0.001)	(0.001)
CIR		-0.001	-0.001	-0.001
		(0.001)	(0.001)	(0.001)
Loans			0.002	0.002
			(0.001)	(0.001)
Liquidity			0.015	0.014
			(0.037)	(0.036)
GDP growth				0.001
				(0.001)
Unemployment				0.023**
				(0.010)
Observations	3,891	3,891	3,891	3,891
Adj. R^2	0.227	0.250	0.251	0.252
Bank FE	yes	yes	yes	yes
Time FE	yes	yes	yes	yes

Standard errors clustered on county level in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 4: Effect of the flood on the impairment flow by sectors

This table shows the results from the regression of our baseline specification where the impairment flow serves as the dependent variable for the sample period 2009-2015. Impairment flow is defined as the net valuation adjustments on domestic loans (in a given sector) over total assets. Column 1 shows again the initial full regression from Table 3, column 4. This total effect can be partitioned into the effect from corporates (column 2) and retailers (column 5) separately. Columns 3 and 4 show the effect of the impairment flow for firms operating in the agricultural and manufacturing sectors, respectively. Column 6 and 7 depict the effect from retail mortgage and other retail loans on the impairment flow. Bank and macroeconomic controls as well as bank and time fixed effects are included in all specifications. The statistical significance of results is indicated by *, **, *** for the 10%, 5% and 1% level, respectively. Standard errors clustered on county level are in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Corporate			Retail		
	Total	Total	Agriculture	Manufacturing	Total	Mortgage	Other
Treated × Post	0.029***	0.022**	0.002*	0.005*	0.007**	0.003*	0.003*
	(0.010)	(0.009)	(0.001)	(0.003)	(0.003)	(0.002)	(0.002)
Observations	3,891	3,891	3,891	3,891	3,891	3,891	3,891
Adj. R^2	0.252	0.214	0.030	0.067	0.183	0.259	0.097
Controls	yes	yes	yes	yes	yes	yes	yes
Bank FE	yes	yes	yes	yes	yes	yes	yes
Time FE	yes	yes	yes	yes	yes	yes	yes

Standard errors clustered on county level in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Figure 3: Yearly disaggregation and parallel trends

This figure plots the coefficient estimates and the 95% confidence interval for the impairment flow of affected banks relative to unaffected banks using the sample period 2009-2016, where 2012 represents the base year. We run the following difference-in-differences regression:

$$y_{it} = \beta_0 + \propto_i + \propto_t + \sum_{j=1}^{k} \beta_j X_{jit} + \sum_{n=2009}^{2016} \tau_n (Treated_i \times Year_n) + \varepsilon_{it}$$

where y represents the impairment flow of bank i in year t. The indicator $Treated_i$ is equal to one for banks affected by the flood, and zero otherwise. The indicator $Year_n$ are year dummies for 2009-2016. We include observed bank-related and macroeconomic explanatory variables as well as bank and year fixed effects. Standard errors are clustered at the county level.

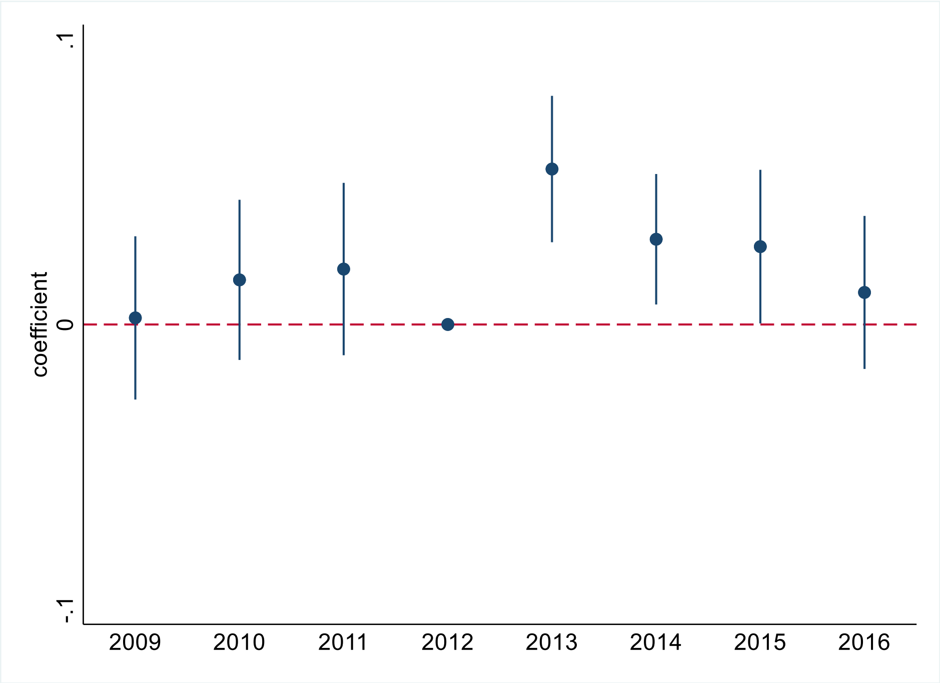


Table 5: Effect of the flood on other profit and loss items

This table shows the results from the regression of our baseline specification where alternative profit and loss items serve as dependent variable for the sample period 2009-2015. The dependent variable in column 1 is return on assets, in column 2 net interest income and in column 3 the change in the value of property investments. Bank and macroeconomic controls as well as bank and time fixed effects are included in all specifications. The statistical significance of results is indicated by *, **, *** for the 10%, 5% and 1% level, respectively. Standard errors clustered on county level are in parentheses.

	(1) RoA	(2) Net interest income	(3) Property
Treated × Post	-0.059***	-0.008	0.005
	(0.020)	(0.016)	(0.007)
Bank size	-0.493***	-0.707***	-0.127***
	(0.091)	(0.098)	(0.037)
Capital adequacy	-0.011***	0.001	-0.002**
	(0.003)	(0.003)	(0.001)
Loans	0.003	0.010***	-0.000
	(0.002)	(0.002)	(0.001)
Business mix	-0.003	-0.005**	0.000
	(0.002)	(0.002)	(0.001)
NPL ratio	-0.027***	0.009***	0.001
	(0.006)	(0.002)	(0.001)
Asset risk	-0.001	0.002	0.001
	(0.002)	(0.001)	(0.001)
CIR	-0.021***	-0.017***	0.000
	(0.002)	(0.001)	(0.000)
Liquidity	-0.086*	-0.100**	-0.013
	(0.049)	(0.042)	(0.011)
GDP growth	0.000	-0.000	-0.000
	(0.001)	(0.001)	(0.000)
Unemployment	-0.005	-0.002	0.000
	(0.012)	(0.008)	(0.003)
Observations	3,891	3,891	3,891
Adj. R^2	0.618	0.898	0.177
Bank FE	yes	yes	yes
Time FE	yes	yes	yes

Standard errors clustered on county level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6: Fixed effects robustness checks

This table shows the results from the regression of our baseline specification where the impairment flow serves as the dependent variable for the sample period 2009-2015. Column 1 shows the regression without the inclusion of any fixed effects. In column 2, only bank fixed effects are included, whereas in column 3 the interaction of bank type and time fixed effects is additionally incorporated. Column 4 instead includes an interaction of time fixed effects with an east/west indicator, which is equal to one if the bank is located in a county that belonged to former East Germany. The last column shows the regression with an interaction of bank type, east/west, and time fixed effects. Bank and macroeconomic controls are included in all specifications. The statistical significance of results is indicated by *, **, *** for the 10%, 5% and 1% level, respectively. Standard errors clustered on county level are in parentheses.

	(1) Impairment flow	(2) Impairment flow	(3) Impairment flow	(4) Impairment flow	(5) Impairment flow
Treated × Post	0.035***	0.032***	0.028***	0.023**	0.024**
	(0.010)	(0.010)	(0.010)	(0.009)	(0.010)
Post	-0.019**	-0.007			
	(0.008)	(0.013)			
Treated	-0.021**				
	(0.010)				
Observations	3,921	3,891	3,891	3,891	3,891
Adj. R^2	0.119	0.238	0.253	0.253	0.253
Controls	yes	yes	yes	yes	yes
Bank FE	no	yes	yes	yes	yes
Bank type × Time FE	no	no	yes	no	yes
East/west × Time FE	no	no	no	yes	yes
Bank type × East/west × Time FE	no	no	no	no	yes

Standard errors clustered on county level in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 7: Alternative definitions of the control group

This table shows the results from the regression of our baseline specification where the impairment flow serves as dependent variable for the sample period 2009-2015, but with different compositions of the control group. Column 1 shows again the initial full regression from Table 3, column 4, where the control group consists of counties directly bordering on the treated counties (first adjacent) and counties bordering on the first adjacent counties (second adjacent). In column 2, only the first adjacent counties serve as the control group, whereas in column 3 the control group only includes the second adjacent counties. In the last column, all non-treated counties in Germany (including first and second adjacent) serve as control group. Bank and macroeconomic controls as well as bank and time fixed effects are included in all specifications. The statistical significance of results is indicated by *, **, *** for the 10%, 5% and 1% level, respectively. Standard errors clustered on county level are in parentheses.

	(1) Adjacent 1+2 Impairment flow	(2) Adjacent 1 Impairment flow	(3) Adjacent 2 Impairment flow	(4) All non-treated counties in Germany Impairment flow
Treated × Post	0.029***	0.021*	0.034***	0.043***
	(0.010)	(0.011)	(0.012)	(0.009)
Observations	3,891	2,362	2,706	10,405
Adj. R^2	0.252	0.271	0.231	0.325
Controls	yes	yes	yes	yes
Bank FE	yes	yes	yes	yes
Time FE	yes	yes	yes	yes

Standard errors clustered on county level in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 8: Additional robustness checks

This table shows further robustness checks for different versions of our baseline specification where the impairment flow serves as the dependent variable. First, Bertrand et al. (2004) propose to collapse the multi-period data into pre and post treatment averages to overcome potential serial correlation issues. Following Cortés and Strahan (2017), the second specification reported in column 2 additionally includes a pre-disaster exposure indicator variable. The pre-disaster variable is equal to one during the four years before the flood. Both checks cover the sample period 2009-2015. Column 3 reports the coefficients of a placebo regression where a hypothetical disaster is assumed to take place in 2012, i.e. one year before the actual event. Therefore, the sample period only covers the years from 2009-2012. Column 4 reports the coefficients of another placebo test, where banks allocation to the treatment and control group is randomized. The randomization is repeated 1,000 times. In only 51 out of 1,000 runs, the effect of the flood is statistically significant at the 5% level for affected banks. This check again covers the sample period 2009-2015. Bank and macroeconomic controls as well as bank and time fixed effects are included in all specifications. The statistical significance of results is indicated by *, **, *** for the 10%, 5% and 1% level, respectively. Standard errors clustered on county level are in parentheses.

	(1) Bertrand et al. (2004) Impairment flow	(2) Cortés and Strahan (2017) Impairment flow	(3) Placebo event in 2012 Impairment flow	(4) Placebo control group randomization Impairment flow
Treated × Post	0.025***	0.034***	0.018	-0.014
	(0.010)	(0.011)	(0.013)	(0.011)
Treated × Pre		0.018		
		(0.013)		
Observations	1,086	3,891	2,259	3,891
Adj. R^2	0.414	0.252	0.286	0.251
Controls	yes	yes	yes	yes
Bank FE	yes	yes	yes	yes
Time FE	yes	yes	yes	yes

Standard errors clustered on county level in parentheses

*** p<0.01, ** p<0.05, * p<0.1