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Physical climate risk, credit risk and lending activity

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Abstract

We study how physical climate risk shapes bank lending activity and credit quality by combining high-resolution Copernicus flood geospatial maps with loan-level AnaCredit data. We exploit four major European floods (2021–2024) in a spatial regression discontinuity design comparing firms located just inside versus just outside flood boundaries (within 300–500 meters). We find that immediately after floods there is an increase by about 3.5 to 5% in lending, driven by liquidity demand, followed by a contraction of similar magnitude in the subsequent quarter. Interest rates follow a similar pattern, while default rates rise persistently by around 0.7 percentage points. Exploiting multiple lending relationships and firm–time fixed effects, we show that demand factors dominate: banks with greater exposure to affected firms do not systematically tighten credit supply. Nonetheless, relationship banks extend roughly 10 percentage points more credit to affected firms while imposing tighter collateral requirements, consistent with risk-sharing rather than unconditional support. Sectoral composition and pre-existing firm risk are the primary axes of heterogeneity in the immediate response. The findings shed light on how physical climate shocks propagate through credit markets and inform financial stability analysis.

Keywords: Floods, Bank lending, Financial Stability, Spatial RDD

JEL Codes: Q54, G21, G32, C21, G28

Non-Technical Summary

Climate change is increasingly recognised as a source of financial risk for banks and the broader economy. Floods, the most economically consequential manifestation of this risk, strike suddenly, destroy productive capital, disrupt supply chains, impairing firm balance sheets with potentially non-linear consequences for default risk and credit supply. Despite growing policy interest, surprisingly little causal evidence exists on how large, localised climate events affect credit risk and bank lending in practice, and whether they can generate broader financial stability concerns. This paper fills that gap.

We study four major floods in Europe between 2021 and 2024: the catastrophic July 2021 floods in Germany, Belgium, and the Netherlands; flash floods in the Marche and Umbria regions of Italy in September 2022; the Emilia-Romagna floods of May 2023; and the Valencia flash floods of October 2024. These events were unexpected in timing, highly localised, and severe enough to cause several hundred fatalities and tens of billions of euros in economic damage.

Our empirical strategy relies on two unique datasets. AnaCredit, the ECB's granular loan-level credit register, which provides comprehensive data on bank–firm exposures, including loan amounts, interest rates, maturities, collateral, and default status. We complement this with high-resolution satellite flood maps from the Copernicus Emergency Management Service, which identify with metre-level precision which firms were located within inundated areas. To identify causal effects, we employ a spatial regression discontinuity design. We compare firms located just inside the flood boundary with firms just outside it. These firms are typically only a few hundred metres apart, and thus share similar local economic conditions and ex ante exposure to climate risk. In a such narrow area, whether a firm falls on the flooded or non-flooded side can be considered random. The validity of this approach is corroborated by standard statistical checks, including density continuity tests and balance in pre-event observable characteristics. This quasi-experimental setup enables us to isolate the effect of flood exposure from other factors that might also influence lending, such as differences in firm risk-taking, leverage, or sectoral composition. This design allows us to attribute differences in credit outcomes to flood exposure itself.

Main Findings: We document four main sets of results. First, lending volumes exhibit pronounced but temporary dynamics. Loans to affected firms rise by approximately 3.5 to 5% in the quarter of the flood, driven by liquidity demand as firms draw down credit lines and seek working capital to cover immediate disruptions. Lending then falls by a comparable magnitude in the following quarter, possibly as acute liquidity needs subside and the deteriorating investment outlook depresses credit demand. Over a two-quarter horizon, these effects largely offset, implying no sizeable net change in lending volumes. Second, interest rates move broadly in tandem with quantities. We find a modest upward shift in loan rates immediately after floods, consistent with higher perceived credit risk and increased borrowing demand, followed by a partial reversal in the subsequent quarter. Over two quarters together, neither the quantity nor the price effects are statistically significant in cumulative terms, suggesting that banks do not reprice credit risk in a lasting way, at least within the short horizon we explore. Third, credit risk rises persistently. Default rates on pre-existing loans to affected firms exceed those of comparable unaffected firms in the quarters following the floods. The estimated increase in the default ratio is approximately 0.7 percentage points. While modest in absolute terms, when annualised this corresponds to a near-doubling increase relative to an average annual baseline default rate of around 1.4%. Physical climate shocks can therefore materially impair loan quality even when aggregate credit supply remains broadly stable. Fourth, demand-side factors are the primary driver of lending dynamics, with limited evidence of supply-side constraints. This is corroborated by additional analyses focussing on firms with multiple bank relationships. We compare how different lenders adjust credit to the same borrower, controlling for the firm's own changing credit demand. This approach shows that banks with larger exposures to affected firms do not systematically cut back credit, raise interest rates, or tighten conditions more than less exposed banks. If anything, more exposed banks tend to marginally extend loan maturities, consistent with loan restructuring or "evergreening" than with a credit crunch.

Heterogeneity and Relationship Lending: Bank–firm relationships nevertheless play an important role. Main banks, defined as the lender with the largest pre-existing exposure to a given firm, provide more lending to affected firms immediately after floods but simultaneously tighten collateral requirements. Relationship lending thus cushions short-term funding stress without relaxing risk management standards. Meaningful heterogeneity also exists across firms. Sectoral composition is the dominant source of variation immediately after the event: wholesale and retail, construction, and services firms show significantly more credit than manufacturing firms, reflecting sector-specific liquidity needs, while also facing higher collateral requirements. These differences dissipate by the second quarter. Pre-existing firm risk emerges as a sharp dividing line: banks expand credit to lower-risk affected firms while curtailing it for higher-risk ones, consistent with active credit reallocation under uncertainty.

Policy Implications: Three policy messages emerge. First, physical climate risks materially affect credit quality through higher default risk, even when aggregate lending volumes remain broadly stable. Second, short-run lending dynamics are shaped largely by firm funding needs and relationship banking rather than by bank-level supply constraints, highlighting the importance of considering loan demand, collateral channels, and lender heterogeneity in climate risk assessments. Third, the impact of physical climate shocks is highly localised, granular, and state-contingent. In a setting where banks are well-capitalised and geographically diversified, shocks of the magnitude studied here are more likely to generate local than system-wide financial stability risks. This argues for supervisory frameworks that combine high-resolution geospatial exposure mapping with heterogeneous, non-linear climate scenarios, showing the value of merging loan-level credit registers with satellite-based flood data for financial stability policymaking.

1 Introduction

Physical climate risks have rapidly escalated from a distant threat to an immediate challenge to financial stability. The frequency and severity of extreme weather events have increased markedly in recent decades, with scientific consensus pointing to further intensification under ongoing climate change (Rodell and Li, 2023). Floods are among the most consequential manifestations of this risk: they strike suddenly, destroy productive capital and infrastructure, disrupt supply chains, and impair firm balance sheets, with potentially non-linear consequences for default risk and bank losses. The economic impact of floods is heterogeneous and can be persistent. European Central Bank (2025b) show that while high-income regions may recover through reconstruction-driven output gains, middle-income regions suffer lasting declines in industrial activity amplified by supply-chain spillovers, with insurance coverage and flood defences critically shaping recovery trajectories. Through these channels, extreme events may increase credit risk for banks and alter both the demand and supply of credit: firms curtail borrowing as cash flows weaken and investment contracts, while banks may restrict supply as they reappraise borrower risk or face balance-sheet constraints.

Several authorities are now incorporating physical climate risks into stress tests and prudential frameworks (Lehmann, 2020). Quantitative assessments underscore the relevance: Abbondanza et al. (2025) estimate that physical risk depletes CET1 by an additional 75 basis points on top of losses under the 2025 ECB adverse scenario, while Chen et al. (2025) find that flood exposure raises SME default probabilities by up to 24 basis points, with effects varying markedly across countries depending on flood management infrastructure and public disaster relief. Together, these findings highlight the importance of integrating physical risk into routine credit risk modelling and financial stability assessments.

Despite substantial policy interest, empirical evidence on how extreme weather events affect bank lending remains limited, in part due to persistent identification challenges. Firms choose where to locate, and location decisions may be correlated with unobservable characteristics, such as risk tolerance, business model fragility, and leverage, that independently affect credit outcomes. A higher risk tolerance, for instance, may be reflected both in greater exposure to physical risk and in a more fragile business model, so that simple comparisons of lending to affected and unaffected firms confound the causal effect of the shock with pre-existing heterogeneity. Establishing credible causal estimates, therefore, requires an identification strategy that directly confronts this selection problem. We do so by exploiting the precise geography of each flood: rather than comparing all exposed firms to all unexposed ones, we compare firms located just inside the flood narrow boundary with firms just outside it. These firms are typically only a few hundred metres apart, and therefore share virtually identical local economic conditions, ex ante exposure to climate risk, and pre-event lender characteristics. Within such a narrow band, whether a firm falls on the flooded or non-flooded side of the boundary is effectively determined by the precise spatial realisation of the event rather than by firm choices, making treatment assignment as good as random. To implement this spatial regression discontinuity (RDD) design, we match granular loan-level data from *AnaCredit*, the ECB's harmonised credit register covering broadly the population of bank–firm credit exposures in the euro area, with high-resolution flood maps produced by the *Copernicus Emergency Management Service* from satellite imagery. This combination allows us to identify, at the level of individual firm addresses, which borrowers were located within the flood perimeter of each event and to calculate each firm's precise distance to the boundary.

Motivation. Our objective is to assess the causal effects of extreme flood events on lending volumes, interest rates, collateral requirements, and default ratios on pre-existing loans, exploiting a unique combination of granular loan-level data and precise geospatial information to trace how these shocks propagate through bank–firm relationships. Our identification strategy relies on a spatial discontinuity at the flood narrow boundary.[1] We apply this design to four major European floods between 2021 and 2024: the catastrophic events in Germany, Belgium, and the Netherlands (July 2021); flash flooding in the Marche and Umbria regions of Italy (September 2022); the Emilia-Romagna floods (May 2023); and the Valencia flash floods (October 2024). Together, these events caused substantial loss of life and large total economic damage, providing meaningful variation in timing, geography, and economic structure.

Beyond aggregate causal effects, our detailed dataset provides a solid foundation for studying the transmission channels through which physical climate shocks propagate through credit markets. We can precisely characterise the heterogeneity of these impacts along firm, bank, and relationship dimensions. To disentangle loan demand effects from supply dynamics, we exploit the prevalence of firms with multiple lending relationships and saturate our regressions with firm–time fixed effects, following Khwaja and Mian (2008).[2]

Main findings. First, lending volumes exhibit pronounced but transitory dynamics. In the first post-flood quarter, credit to affected firms rises by 3.5 to 5%, as firms draw on existing credit lines and seek additional working capital to manage immediate disruptions. This short-run expansion unwinds in the following quarter, possibly as acute liquidity needs subside and the deteriorating investment outlook depresses credit demand. Over the two-quarter horizon, the two effects largely offset each other, leaving no significant cumulative change in lending volumes. Second, interest rates co-move with quantities, rising modestly in the first quarter (10 to 13 basis points), consistent with higher credit demand and higher perceived risk, and partially reversing thereafter, but the cumulative effect is likewise insignificant. The parallel movement

[1] This quasi-random assignment at the narrow boundary, supported by standard density continuity and covariate-balance tests, allows us to interpret differences in post-event credit outcomes as causal effects of flood exposure rather than artefacts of selection.

[2] By absorbing firm–time fixed effects, we net out time-varying credit demand and isolate the supply-side component of banks' responses. This allows a systematic investigation of how bank characteristics, such as portfolio exposure to affected areas, and relationship intensity shape the transmission of physical shocks through the credit channel.

of prices and quantities in both directions is consistent with loan demand, rather than supply, being the dominant margin of adjustment. Third, credit quality, by contrast, deteriorates persistently: default rates on pre-existing loans rise by approximately 0.5 percentage points after one quarter and by around 0.7 percentage points cumulatively over two quarters, representing a near-twofold increase relative to the pre-event baseline. Fourth, loan demand is the primary driver of the observed credit dynamics. Using within-firm, cross-bank variation from multiple lending relationships: banks with greater exposure to affected borrowers do not systematically restrict lending, raise interest rates, or tighten collateral requirements. More exposed banks to affected firms do marginally extend loan maturities, consistent with mild evergreening (Peek and Rosengren, 2005), though of limited quantitative significance. Main banks, defined as the lender with the largest bilateral exposure to a given firm, provide around 10 percentage points more credit to affected firms while tightening collateral requirements, indicative of risk sharing rather than unconditional support, consistent with the relationship banking literature (Sharpe, 1990; Bolton et al., 2016). Turning to firm-level heterogeneity, sectoral composition and pre-existing credit risk are the primary axes of variation. In the first quarter, wholesale and retail, construction, and services firms show significantly more credit than manufacturing firms, reflecting sector-specific liquidity needs, while also facing higher collateral requirements. Ex-ante firm risk is a sharp dividing line: banks expand lending to lower-risk affected firms while curtailing credit to higher-risk ones, with the net effect for high-risk firms being a decline of approximately 8 percentage points in the first quarter. These differences largely dissipate by the second quarter as the aggregate contraction applies uniformly.

Contribution to the literature. Our paper contributes to a growing body of research on the financial consequences of physical climate risk. Several studies document that flood-prone locations face higher borrowing costs (Bassetti et al., 2025; Carroll et al., 2025; Fontana et al., 2025) and that actual flood events affect firm fundamentals including leverage, output, and employment (Fatica et al., 2024b; Bossut and Kempa, 2025; European Central Bank, 2025b). Closer to our approach, Pérez Montes et al. (2025) study the same Valencia flood and find an increase in loan stocks immediately after the event and some deterioration in loan performance from late 2024; our analysis complements this by providing a fully identified causal design and a broader cross-event perspective. We advance the literature in three ways.

First, by combining metre-level geospatial precision with comprehensive loan-level data, we implement a spatial RDD design that directly addresses the selection problem that plagues much of the existing evidence. Second, by exploiting multiple lending relationships and absorbing firm–time fixed effects, we provide direct evidence on the demand-versus-supply decomposition that most prior studies can only conjecture about. Third, spanning four events across several countries and multiple sectors, our results carry external validity beyond any single disaster and speak to the general properties of the credit channel of physical risk. Our findings also shed new light on how relationship lending (Sharpe, 1990; Bolton et al., 2016; Jiménez et al., 2020) and loan evergreening (Peek and Rosengren, 2005) operate in the context of acute physical shocks.

Organisation of the paper. Section 2 reviews the related literature. Section 3 describes the data sources, the construction of our matched geospatial–credit dataset, and key measurement choices. Section 4 sets out the empirical strategy, including the spatial regression discontinuity design and the decomposition of demand and supply effects. Section 5 presents the main results on lending volumes, interest rates, credit risk, and transmission channels, as well as heterogeneity across firms and sectors. Section 6 concludes with implications for financial stability.

2 Literature review

Physical climate risk is increasingly studied as a driver of both borrower fundamentals and banks' credit supply, with evidence spanning pricing, quantities, and credit risk dynamics. The literature also highlights that the transmission of physical shocks can operate through direct exposure, collateral channels, spatial spillovers, and broader macroeconomic propagation, motivating empirical designs that exploit high-resolution geospatial information.

An expanding literature examines the link between banks' credit exposures and physical climate risks, highlighting potential interactions between environmental shocks and traditional credit risk parameters. Meucci and Rinaldi (2022) document that Italian banks exhibit limited aggregate exposure to physical climate risk; however, loan- and collateral-level exposures are highly correlated. This interdependence implies a potential positive association between the probability of default (PD) and the loss given default (LGD) if climate-related shocks materialise. They also show that approximately 20% of loans in AnaCredit are secured by physical collateral, underscoring the relevance of collateral channels in amplifying physical risk.

Faiella and Natoli (2018) find that the incidence of natural catastrophes is negatively correlated with lending to Italian firms, particularly SMEs. They attribute this outcome to the limited uptake of catastrophe risk insurance among smaller firms, consistent with their higher vulnerability to uninsured losses. Complementing this, Bassetti et al. (2025) analyse 419,040 firm-year observations from 2016 to 2019 and report a positive relationship between Italian firms' cost of debt and both direct flood risk exposure and spatial spillovers from neighbours' flood risk (within a 10-km radius). A one-standard-deviation increase in direct flood risk raises borrowing costs by 18.5 basis points, while neighbours' risk adds at least 10 basis points; these spillover effects are confined to small firms, attributable to their limited financial diversification and reliance on local credit intermediaries.

Fontana et al. (2025) provide evidence that banks increasingly charge a physical risk premium on mortgage lending, with the estimated premium displaying substantial heterogeneity across institutions and a discernible upward trend over recent years. Relatedly, Bellaver et al. (2025) study the Italian housing market using 550,000 mortgage-financed transactions from 2016 to

2024, combined with hedonic regressions and difference-in-differences estimation. They find that specific flood events do not reduce home prices in at-risk areas, but repeated flood exposure leads to discounts of up to 4% in frequently flooded regions; these effects are heterogeneous, with lower-income and younger buyers facing no discounts, resulting in socioeconomic sorting into riskier areas. Their analysis leverages Copernicus Emergency Management Service data to identify flood-affected properties, highlighting the role of historical memory in risk capitalisation. Also, D'Andrea et al. (2026) study flood impacts in Italy's Emilia-Romagna region and show that banks charge higher pre-disaster interest rates to firms subsequently more affected by flooding, consistent with the use of local information in pricing catastrophe risk and offsetting future impairments.

At the macro-regional level, Usman et al. (2025) study the medium-run effects of heatwaves, droughts, and floods across 1,160 EU regions, finding that heatwaves and droughts exert persistent negative effects on regional output while flood impacts vary with income levels, suggesting heterogeneous resilience. European Central Bank (2025b) corroborate the income-contingent nature of flood impacts, documenting reconstruction-driven output gains in high-income regions alongside persistent industrial declines in middle-income areas, amplified by supply-chain spillovers; insurance coverage and flood defences emerge as critical determinants of recovery, highlighting the financial stability stakes of the current climate adaptation financing gap. In this regard, Christophersen et al. (2023) document that only 25% of catastrophe losses in the EU are insured, with coverage rates below 5% in several Member States, and propose a layered approach—from private insurance and reinsurance to fiscal backstops and an EU-wide public scheme—to close this protection gap while managing moral hazard. At the firm and household level, Bossut and Kempa (2025) show that European manufacturing firms exposed to floods suffer sizeable but largely temporary losses in tangible assets, output, and profitability, with effects concentrated among financially distressed and underinsured firms facing tighter external finance constraints. Beyene (2025) provides complementary micro-level evidence from the 2013 German floods, finding that loan rates rose and credit originations declined among lower-income households, pointing to distributional consequences of physical shocks that aggregate analyses may obscure.

Micro-level analyses further illuminate how physical climate shocks propagate through credit markets. Fatica et al. (2024a) study securitised SME loans between 2008 and 2019 in three European countries. Using NUTS3-level flood data, they show that greater exposure to flood risk leads to higher loan interest rates, though actual flood events primarily raise risk salience rather than directly altering pricing. Their hazard-rate modelling indicates a short-term decline in PD within six months following a flood—likely owing to subsidies—followed by a significant medium-term increase after 12 to 24 months. Fatica et al. (2024b) extend this line of inquiry using Orbis firm-level data for manufacturing firms between 2007 and 2018. Constructing treatment and control groups based on ex-ante floodplain exposure, they find adverse effects on total assets, sales, employment, and productivity among firms in affected regions, with credit risk effects largely dissipating within one quarter. Similarly, Carroll et al. (2025) analyse 40,852 geolocated loans from AnaCredit (June 2022) matched to Irish flood risk maps, finding that borrowers in flood-risk areas (7% of the sample) face an interest rate premium of 7 to 13 basis points and a 3 to 7 percentage point higher probability of collateral requirements; mid-range climate change projections are partially priced in, indicating lenders' partial internalisation of future risks.

At a more localised scale, Pérez Montes et al. (2025) study the effects of the Valencia flash floods on bank lending to households and non-financial corporations. Shortly after the event, they observe a statistically significant increase in loan stocks in affected areas, accompanied by a moderate rise in non-performing and stage 2 loans from December 2024 onwards. These findings suggest that, while loan performance deteriorates modestly in the short run, the aggregate balance sheet impact remains contained, consistent with largely local—rather than systemic—financial stability implications. Barrutiabengoa et al. (2025) quantify the economic impact of the same Valencia floods using a two-stage framework: a non-linear damage function (based on EM-DAT data) estimates losses at 0.65% of Spanish GDP, while a dynamic spatial panel model projects a 1.4 percentage point employment decline in Valencia province (matching observed 1.5-1.6 points) and 0.2 points nationally, with non-linear effects for extreme events and mitigation via insurance payouts.

Extending to firm-level environmental performance, Gu et al. (2026) assess bank credit allocation relative to CO2 emissions using data from the US, EU, and Denmark. They find heterogeneous patterns: US lending hampers green transition by favouring high emitters, while EU and Danish lending facilitates it through reallocation to greener firms within industries, though this is largely a byproduct of green firms' growth rather than active bank stewardship. European Central Bank (2025a), drawing from the euro area bank lending survey, report that lower climate risks ease credit standards and terms for green and transitioning firms (net 20% and 13% easing, respectively), while tightening them for high emitters (net 35%); physical and transition risks both contribute, with green investments boosting loan demand.

Notwithstanding these contributions, the existing evidence base faces several limitations. Many studies rely on damage functions with restrictive assumptions, employ data of limited spatial and sectoral resolution, or focus on a single disaster type. Building on this literature, our study leverages high-resolution geospatial information from the *Copernicus Emergency Management Service* to capture the precise extent of physical damages associated with major floods. These data are matched with geocoded firm-level exposures from the AnaCredit database, thereby enabling a detailed assessment of how disaster impacts propagate through bank-firm relationships.

Our empirical design exploits the exogenous nature of natural disasters to construct a quasi-experimental framework, leveraging highly granular geospatial precision in disaster impact analysis. We rely on such data for all major physical disasters and match it with geocoded firm addresses from the AnaCredit database, allowing us to pinpoint with remarkable accuracy which firms were directly affected and which were not. Specifically, we apply a regression discontinuity design (RDD) focusing on firms situated immediately on either side of disaster boundaries; this dual approach mitigates potential endogeneity arising from unobserved firm characteristics and spatial heterogeneity. By integrating this geospatial data with high-frequency credit information, our analysis provides one of the most precise empirical assessments to date of how physical climate shocks affect corporate balance sheets and bank credit exposures. This level of granularity in disaster-firm linkages represents a significant methodological advance, offering new evidence on whether the transmission of physical risk is confined to local markets or propagates more broadly, thereby informing the ongoing policy debate on the systemic relevance of climate-related risks for financial stability.

3 Data description and empirical setup

3.1 Flood Events

Our empirical analysis focuses on four major flood events that occurred in Europe between 2021 and 2024. These events are selected because they represent large, exogenous physical climate shocks that were unexpected in timing, highly localised, and sufficiently severe to generate measurable economic and financial effects. Each episode is well documented (including in terms of data), triggered by extreme meteorological conditions, and resulted in substantial damage to productive capital, infrastructure and economic activity. As such, they provide a natural setting in which to study the transmission of physical climate risk to credit risk impacts at the firm and bank exposure level.

1. *Germany, Belgium, Netherlands (July 2021):* Catastrophic flooding triggered by extreme precipitation across western Germany, eastern Belgium, and parts of the Netherlands. The event caused over 200 fatalities and an estimated €40 billion in damages, making it one of Europe's costliest natural disasters. The floods particularly affected the German states of Rhineland-Palatinate and North Rhine-Westphalia, devastating infrastructure, residential areas, and industrial facilities.
2. *Marche and Umbria, Italy (September 2022):* An extreme "flash flood" caused by record-breaking rainfall over a short time span, leading to more than 10 fatalities and estimated damages exceeding €2 billion. The event disproportionately affected local SMEs and manufacturing clusters.
3. *Emilia-Romagna, Italy (May 2023):* Exceptionally persistent rainfall led to the overflow of 23 rivers and triggered more than 280 landslides. Estimated damages reached approximately €8.5 billion, with widespread disruption to one of Italy's most industrialised and

agriculturally intensive regions. The scale and sectoral breadth of this event allow us to study physical risk effects across multiple economic activities.

4. *Valencia, Spain (October 2024):* A catastrophic DANA (high-altitude isolated depression) event generated unprecedented flooding in the Valencia region, resulting in significant loss of life and severe damage to transport networks and critical infrastructure. Early estimates point to multi-billion euro economic losses, providing a recent and independent shock in a different institutional and geographical context.

Taken together, these events provide substantial variation in timing, geographical exposure and economic structure, while sharing a common origin in extreme weather phenomena. This combination allows us to empirically isolate physical climate risk as an exogenous shock and to examine how bank lending responds to sudden losses in collateral value and obligor creditworthiness, business continuity and regional economic capacity across different settings.

3.2 Data Sources

In this paper we combine several data sources, namely:

Lending and Credit Risk Data. We obtain data on lending outcomes and credit risk from *AnaCredit* (Analytical Credit Datasets), the ECB's harmonized database of individual bank loans across the euro area. AnaCredit collects granular, loan-level information on credit exposures, borrower characteristics, and loan terms for all credit institutions in participating countries. The database covers individual loans and credit lines above €25,000, reported on a borrower-by-borrower and instrument-by-instrument basis. For each loan, AnaCredit provides information on loan amounts, interest rates, maturity, collateral, credit risk indicators (including default status and loss provisions), and detailed borrower identifiers. Critically for our purposes, AnaCredit includes the registered address of each borrower, which we use to determine geographical exposure to flood events.

Geospatial Flood Mapping. To identify affected firms, we use the *Copernicus Emergency Management Service (CEMS)* mapping component. CEMS provides high-resolution geospatial information derived from satellite imagery (Sentinel-1 and Sentinel-2) to delineate the maximal extent of flooded areas during natural disasters. Unlike municipality-level administrative data, CEMS vector maps delineate the extent of inundated areas at multiple time points during and after the emergency, as well as assessments of damage to infrastructure and buildings where available.

Other Data Sources Supervisory data from the European Banking Authority's implementing technical standards (FINREP and COREP) are used to measure banks' balance sheet characteristics and credit risk exposures. Firm-level balance sheet information is obtained from the Orbis database. Ex-ante firm-level flood risk is captured using the ECB's Analytical Indicators on Physical Risk. This indicator is based on climate projections under Representative Concentration Pathway (RCP) 4.5, a moderate mitigation scenario that assumes the gradual implementation of policies to reduce greenhouse gas emissions. The indicator is expressed on a numerical scale from 0 to 5, where higher values indicate exposure to flood hazard maps associated with longer return periods and therefore greater flood risk.[3]

The variables used in the empirical analysis are formally defined in Table 33 in the Appendix, which provides precise variable descriptions, measurement units, and the regulatory or accounting standards underlying each indicator.

3.3 Sample Construction

We construct our final estimation sample through a multi-step spatial matching procedure:

1. *Event Delineation:* For each of the four events, we download all activation maps from the CEMS and overlay them using QGIS to obtain the maximum spatial extent of flooding. We extract the boundary of each flood event as a polygon, which serves as the reference perimeter for measuring firm distance.[4]

2. *Spatial Intersect:* We overlay the flood extent maps with EU municipality boundaries (NUTS 5/LAU level) to identify affected and adjacent administrative areas.

3. *Firm Identification:* We extract the universe of firms headquartered in these municipalities. Crucially, we do not restrict the sample to firms with active loans at the time of the event; this allows us to capture both the intensive margin (lending to existing borrowers) and the extensive margin (credit to new firms post-event).

4. *Geocoding:* We extract the registered addresses of all firm headquarters located in affected and adjacent municipalities from business registry data. Firm addresses are geocoded into precise GPS coordinates using the Google Maps Platform API to obtain precise latitude and longitude coordinates for each firm headquarters.

5. *Distance Calculation:* For each firm, we calculate the minimum (Euclidean) distance to the flood perimeter. Firms inside the perimeter are assigned negative distances, while those outside are assigned positive distances.

6. *Final RD Sample:* We restrict the final estimation sample to firms located within 2 kilometers of the flood boundary.

[3] We refer the interested reader to European Central Bank (2024) for more information on the indicator.

[4] Figure 4 illustrates this procedure. Because individual CEMS activation maps may reflect different temporal or spatial snapshots of the same event, we take the spatial union across all available maps to obtain the maximum flood extent, which serves as the definitive treatment boundary in our RDD design.

For this geographically defined sample of firms, we extract quarterly data on all bank-firm lending relationships from AnaCredit, covering four quarters before each event and two quarters after. Our data extraction date is May 2025; we therefore limit the post-event window to two quarters to maintain a balanced panel across all four events (the Valencia flood occurred in October 2024, providing only two complete post-event quarters by our extraction date).

3.4 Identification Caveats

Our empirical approach relies on several key measurement choices and assumptions that merit discussion. **Treatment assignment.** We define a firm as treated if its registered headquarters address falls within the flood boundary polygon. This approach does not capture the precise extent of damage suffered by each firm—for instance, a multi-story building may experience flooding only on ground floors, or a firm may be located in a flooded area but on elevated terrain. Being located within the flood perimeter identifies firms that were *exposed* to the event, but does not guarantee they were *directly hit* (e.g., firms located on higher floors). In any case, this measurement error likely attenuates our estimates toward zero, making our findings conservative estimates of the true effect of flood damage on credit outcomes.

Headquarters assumption. We identify firm location based on the registered headquarters address reported in AnaCredit. This assumption is standard in the literature (Fontana et al., 2025; Meucci and Rinaldi, 2022) and appropriate for the majority of firms in our sample, which are small and medium-sized enterprises typically operating from a single location. However, this approach may introduce measurement error for larger firms with multiple establishments or for firms whose operational headquarters differs from their legal address.

4 Empirical setup

As previously noted, the natural experimental setup we utilize—examining realized climate events—serves as a necessary but insufficient condition for achieving identification. This limitation arises from potential cross-firm heterogeneity in climate risk exposure, which is both unobservable and correlated with other factors influencing lending outcomes. To address this challenge, we adopt a spatial regression discontinuity (RD) design that compares lending outcomes between affected and unaffected firms located in close geographical proximity. By restricting the analysis to nearby firms, we ensure comparability in terms of climate risk exposure, allowing any remaining variation in treatment status to be treated as quasi-random. As shown below, the bandwidth selection yields comparisons among firms located a few hundred meters apart on average (approximately 300–500 meters).

In this exercise, the running variable is the distance from the perimeter of the affected area (positive if outside, negative if inside), and the threshold is set at zero. Given the exogenous nature of the climate event, the likelihood of firms near the border being affected is plausibly random. This approach fully exploits the granularity of our dataset, as we can precisely identify the geographical boundary and affected firms. The baseline specification is:

$$Y_{fbt} = \beta_0 + \beta_1 T_f + \beta_2 \mathrm{Dist}_f + \beta_3 (T_f \times \mathrm{Dist}_f) + \gamma \mathbf{X}_{fbt} + \epsilon_{bft} \tag{1}$$

where Y_{fbt} is the lending outcome for the relationship between bank b and firm f at time t; T_f is an indicator equal to one if firm f is located inside the affected perimeter, and zero otherwise.[5] We control for the role of distance through Dist_f, the distance of firm f from the flood perimeter, and allow the slope to differ between treated and non-treated groups via the interaction term $\beta_3(T_f \times \mathrm{Dist}_f)$. The vector $\mathbf{X}_{fbt}$ includes control variables such as residual loan maturity and time to the next interest rate reset date. The sample is restricted to observations in the two quarters immediately following the climate event. We employ MSE-optimal bandwidths selected via the `rdrobust` procedure with triangular kernel weights. Standard errors are clustered at the bank level to account for within-bank correlation.

We will be analysing a comprehensive set of lending outcomes. We estimate these regressions by considering as dependent variable lending volumes, lending rates, and lending conditions, and credit risk. We will also explore the role played by geographical concentration of banks' lending portfolios (i.e., share of lending extended to firms in the local economy affected by the climate event or to affected firms directly), as well as the role of bank-firm relationship specific factors, capturing the intensity of the lending relationship. In this setting, we extend the baseline specification to include the interaction term(s), as follows:

$$Y_{fbt} = \beta_0 + \beta_1 T_f + \beta_2 Dist_f + \beta_3 (T_f \times Dist_f) + \beta_4 H_{it} + \beta_5 (T_f \times H_{it}) + \gamma \mathbf{X}_{fbt} + \epsilon_{fbt} \tag{2}$$

where $i \in (b, f, bf)$. H_{it} represents bank, firm, or bank-firm time-varying characteristics.[6] The interaction $T_f \times H_{bt}$ identifies whether banks or firms with certain characteristics react differently to treated firms compared to non-treated firms at the boundary.

Finally, we dissect the transmission channel of physical climate risks, focusing on separating loan demand effects from supply dynamics. Using loan-level data and firms with multiple lending relationships, we apply the approach of Khwaja and Mian (2008) to account for demand conditions as follows:

$$Y_{fbt} = \alpha_{ft} + \beta_1 (T_f \times H_{it}) + \beta_2 H_{it} + \beta_3 T_f + \beta_4 Dist_f + \beta_5 (T_f \times Dist_f) + \gamma \mathbf{X}_{fbt} + \epsilon_{fbt} \tag{3}$$

[5] In principle, we should add e to the subscript, as bank-firm-time relationships are event-specific and may overlap across different events. However, given the non-overlapping geographical nature of the events we consider, this is not strictly necessary.

[6] In some specifications shown later, we simply use H_i (and $T_f \times H_{it}$) to study non time-varying characteristics (e.g., banks' ex-ante exposure to affected areas, to avoid endogeneity issues).

where α_{ft} are the firm-time fixed effects. In this saturated specification, β_1 is the coefficient of interest, as it identifies the supply shock while controlling for all observed and unobserved time-varying firm shocks (including credit demand). β_2 is the direct impact of the characteristic H_{it} on the outcome variable. $\beta_3, \beta_4, \beta_5$ are the RD local linear components. Note that while T_f and $Dist_f$ are included for conceptual completeness, they are subsumed by the firm-time fixed effects in the estimation.

4.1 Spatial Regression Discontinuity Design

Our identification strategy exploits a spatial discontinuity at the boundary of flooded areas delineated by CEMS. Firms located within the flooded area are classified as treated, while firms located immediately outside the flood boundary serve as the control group. The running variable is the distance to the flood boundary (in meters), with negative values indicating locations within the flooded area and positive values indicating locations outside. Treatment is assigned sharply at the boundary: firms with distance ≤ 0 are treated, while those with distance > 0 are untreated. This spatial RDD setting offers several advantages for causal inference. First, the assignment mechanism is determined by geography rather than individual choice or institutional decisions, reducing manipulation concerns. Second, treatment is dichotomously assigned at a precisely defined discontinuity, facilitating comparison of nearly identical locations that differ only in their exposure to the flood event. Third, the high resolution of CEMS data allows us to construct treatment variables at the building or firm address level, minimizing measurement error relative to coarser administrative boundaries. Figure 5 provides a representative example of this setup for the Valencia event. The dark blue areas indicate the maximum flood extent as delineated by CEMS, while the light blue perimeter marks the spatial bandwidth within which the local RDD comparison is conducted. Geolocated (randomised) firms are visible on both sides of the boundary, illustrating the quasi-experimental variation that our design exploits.

4.2 Validity of the Regression Discontinuity Design

To ensure the validity of our RDD approach, we conduct a series of diagnostic checks that test the key assumptions underlying causal inference at the discontinuity.

Sharp Treatment Assignment. Figure 1 in the Annex presents the treatment assignment mechanism at the flood boundary. The figure displays the probability of treatment (*Pr(Treated)*) as a function of distance to the flood boundary, clearly demonstrating a sharp discontinuity at zero distance. Firms located within the flooded area (negative distance) are treated with probability equal to 1, while firms located outside the boundary (positive distance) are treated with probability equal to 0. This sharp assignment provides strong support for the credibility of our RDD design, as it confirms that treatment is mechanically determined by geographical location rather than through any endogenous selection process.

Density Continuity and McCrary Test. A fundamental assumption of the RDD is that the distribution of the running variable should be continuous at the threshold in the absence of treatment. Discontinuities in the density of the running variable at the cutoff would suggest that units may have manipulated their position relative to the threshold, undermining causal identification. We test this assumption using the McCrary (2008) density test. Figure 2 in the Annex presents the results of the McCrary density test across a range of bandwidths (50m, 100m, 200m, 300m, 350m, 400m, 500m, 600m, and 1000m). The figure displays the empirical density of the running variable (distance to flood boundary) on either side of the threshold, along with smooth density estimates fitted separately to the treated and control samples.

Interpretation of McCrary Results. For bandwidths in the range of 300-500 meters, which align with our optimal bandwidth selection procedure, the McCrary test reveals no evidence of significant density manipulation at the boundary. The density of observations is approximately continuous at the discontinuity, and formal density tests do not reject the null hypothesis of equal densities on either side of the threshold. This finding suggests that the location of the flood boundary was not anticipated or manipulated by firms or banks prior to the event. At larger bandwidths (600m and 1000m), the density plots reveal an apparent imbalance, with greater density on the control (outside) side of the boundary. However, this imbalance is largely mechanical in nature and does not indicate treatment manipulation. Rather, it reflects the structural features of the geospatial data we use. The CEMS polygons delineating the flooded area are often not continuous or do not form a single large contiguous area. Instead, flooded areas are frequently characterized by multiple disconnected polygons. Within each polygon, treated firms tend to have a lower average distance to the boundary (being distributed throughout the smaller flooded area), while control firms are distributed across a much larger surrounding region outside each polygon. As the bandwidth expands, the control sample increasingly incorporates observations from distant unaffected areas, mechanically increasing the density asymmetry on the control side. This means that, in many cases, the CEMS polygons are not sufficiently wide to allow for substantially more observations within the treated area as bandwidth expands. Consequently, the treated sample size remains relatively stable across increasing bandwidths, while the control sample grows substantially. This sample-size asymmetry inflates the density discontinuity at the threshold as bandwidth increases, potentially introducing bias in the McCrary test statistic itself. This is a known issue in spatial RDD applications where the geographic feature of interest (the flood boundary) has an irregular shape and limited width, and does not reflect problematic manipulation of the assignment variable. Given these data characteristics, we rely on the density tests at smaller to moderate bandwidths (300-400m) where sample asymmetries are less severe. At these bandwidths, the density continuity assumption is satisfied, supporting the validity of our causal estimates.

Covariate Balance. A second key assumption is that observable covariates should be balanced across the treatment threshold, indicating that treatment assignment is orthogonal to pre-treatment characteristics. We assess this assumption by comparing the distributions of baseline covariates between the treated and control groups within the 300m bandwidth, which lays in the middle of the range usually selected by our optimal bandwidth selection procedure. Table 27 shows that the balance across treated and control groups is strong. The vast majority of covariates exhibit negligible differences between the two groups. Loan characteristics are well-balanced (lending amounts and log lending is slightly significant, but not at higher levels of significance), and share-stage indicators show standardized differences near zero. Firm characteristics are similarly balanced, with no meaningful differences in firm age, leverage (LTV), probability of default (PD). Relationship-level characteristics are balanced across the discontinuity, with virtually identical mean values for number of bank relationships and stock-level indicators. A small number of variables exhibit statistically significant differences at conventional levels. These include annualised interest rate (difference of 30bp), interest rate spread (20bp), and next interest rate reset date measured in months (0.34, roughly 10 days). However, the economic magnitude of these differences is minimal. Distance from flood boundary is also statistically significant, but this difference is mechanically driven due to the nature of our flood maps, as discussed before. The variables are hence overall balanced: the few instances of statistically significant differences involve either economically trivial magnitudes (interest rate differences of a few basis points, maturity differences of days) or mechanical differences reflecting the construction of the running variable itself. This pattern of balance is consistent with the validity of our spatial discontinuity design, supporting the assumption that treatment assignment near the flood boundary is exogenous with respect to pre-treatment firm, loan, and bank characteristics. We report the results for the balance at the 400-meter bandwidth in Table 28. Further, Figure 3 provides a graphical representation of covariate continuity at the flood boundary, following the approach of Skovron and Titiunik (2015). Across all pre-treatment covariates, which include lending amounts, loan stages, collateral, LTV, PD, interest rates, and firm characteristics, the fitted values on either side of the cutoff are statistically indistinguishable, corroborating the tabular balance results and supporting the validity of the RDD design.

No Sorting into the Discontinuity. A related assumption is that agents cannot perfectly sort into or out of treatment status based on the running variable. In our spatial setting, this would require that borrowers or banks anticipated the precise flood boundary and positioned themselves accordingly. Given that flood events are inherently unpredictable and the CEMS delineation was conducted *post-event*, this concern is minimal. Firms cannot have sorted into their locations based on anticipated flood exposure at this specific boundary. To provide empirical corroboration of the absence of endogenous sorting, we conduct a placebo regression testing whether treatment status predicts ex-ante flood risk exposure. Table 1 reports the results of regressing the ex-ante firm-level flood risk indicator on an indicator for treatment assignment. The coefficient on treatment is not significant for both specification of the treatment variable (discrete or binary). This result is informative in two ways. First, it confirms that the CEMS-mapped flooded area does not systematically align with pre-existing flood risk delineations –

if firms had sorted based on pre-flood risk exposure, treated firms would exhibit substantially different baseline risk profiles from control firms. Second, and more importantly for identification, the absence of a relationship between treatment assignment and ex-ante risk exposure suggests that the assignment at the boundary is effectively random with respect to firms' ex-ante flood vulnerability. If endogenous sorting had occurred, we would expect firms exposed to treatment to differ systematically in their baseline risk characteristics; instead, we observe no such difference. This supports the exogeneity of our treatment variable and validates the spatial discontinuity design.

Table 1: Placebo test for sorting around the flood boundary

	(1) Flood risk (0–5)	(2) Flood risk (high/low)
Treated	−0.020	−0.011
	(0.025)	(0.009)
Observations	13,189	13,189

Notes: This table reports placebo regressions testing for sorting around the flood boundary. The dependent variable is ex-ante flood risk at the firm headquarters location. Column (1) uses the original flood risk index (0–5), while column (2) uses a binary indicator for high versus low flood risk. *Treated* is an indicator equal to one for observations located on the flooded side of the boundary. All variables are measured prior to the flood event. The absence of statistically significant coefficients on *Treated* indicates no evidence of endogenous sorting with respect to baseline flood risk around the cutoff, supporting the validity of the regression discontinuity design. Robust standard errors are reported in parentheses. *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Summary. Our spatial RDD design satisfies the key assumptions required for causal inference. Treatment assignment is sharp and mechanically determined by geography. Density continuity holds at our preferred bandwidths, with observed imbalances at larger bandwidths explained by mechanical features of the geospatial data rather than treatment manipulation. While observable covariates show some differences across the discontinuity, these imbalances are modest and economically immaterial even where statistically significant, reflecting the large sample size rather than systematic differences in treatment assignment. Treatment status does not predict ex-ante flood risk exposure, further corroborating the exogeneity of treatment assignment. These diagnostic checks support the use of the regression discontinuity design to estimate the causal effect of flood exposure on lending outcomes and credit risk.

5 Results

Our paper studies how physical climate risk impacts multiple dimensions of lending activity and credit risk. To capture dynamic responses over time, we analyse separately at different quarters following the event, leveraging the quarterly frequency of our dataset, which records end-of-quarter values. We implement an approach inspired by local projections within the context of our RDD framework, assessing effects quarter by quarter rather than in a single regression. This strategy facilitates straightforward interpretation of the results and, consistent with insights from the local projection literature, delivers more robust estimates of dynamic effects.

We first analyse the impact of floods on lending volumes and interest rates. We then extend the analysis to additional loan terms, including residual maturity and collateral requirements, and assess the implications for asset quality with a particular focus on credit risk. Finally, we analyse potential transmission channels, exploiting the bank–firm relationship dimension of our dataset. We next investigate firm-level heterogeneity, as aggregate effects may conceal substantial variation in how different firms adjust to physical climate shocks. To this end, we augment the baseline spatial RDD specification with interaction terms between the treatment indicator and key firm attributes, namely size and sectoral composition. This framework allows us to assess whether vulnerabilities to extreme flood events are concentrated in specific segments of the corporate sector or are more widely distributed across firms. Taken together, these analyses yield a nuanced picture of how the four climate events under study shape lending behaviour along multiple dimensions.

5.1 Lending volumes

We begin by analysing the impact on lending volumes in the quarter during which the event occurs (first quarter). Bandwidths are endogenously determined in each specification using the MSE-optimal procedure. We consider alternative specifications with different controls, particularly including different fixed effects configurations. All specifications include the average residual maturity across outstanding loans as an additional control variable, which emerges as an important predictor of lending growth, reflecting its role in constraining both demand- and supply-side adjustments.

The results in Table 2 suggest that, on average, the event exerts a positive effect on lending volumes. The magnitude is economically meaningful, at approximately 3.5 to 5%, although statistical significance attenuates in specifications with area fixed effects. This attenuation likely reflects the absorption of area-specific trends in lending activity, which is expected given the substantial share of affected firms in local lending portfolios. The positive impact may reflect several factors, including a sharp increase in liquidity needs in the immediate aftermath of the event, when firms' operations are severely disrupted. In such circumstances, firms may increase borrowing almost mechanically by drawing more aggressively on unused credit lines, which are typically available to the vast majority of firms. This response may be further facilitated by public support measures such as loan moratoria programmes, which were adopted systematically across all events in our sample.

Table 2: Effect of Flood Exposure on Lending Volumes (Quarter 1)

	(1)	(2)	(3)
	Dependent variable: Δ Lending Amount		
Treated	0.036**	0.035*	0.050*
	(0.018)	(0.019)	(0.026)
Distance	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Treated × Distance	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Qrt.s from Event	1	1	1
Observations	7391	7360	6761
Bandwidth	[-484, 484]	[-482, 482]	[-439, 439]
Fixed Effects	No	Event	Area
Controls	Yes	Yes	Yes
Number of Firms	5242	5221	4783
Number of Banks	47	47	44
Number of FE Groups	0	4	196

Notes: This table reports regression discontinuity estimates of the effect of flood exposure on lending. The dependent variable is the change in log lending at the bank–firm level. The running variable is the distance to the flood boundary, normalised to zero at the cutoff. Treated is an indicator equal to one for observations located on the flooded side of the boundary. Controls include residual maturity. All specifications use local linear regressions with a triangular kernel and optimal bandwidth selection following Calonico et al. (2014). Bandwidths are reported in brackets. Fixed effects vary by specification and include time (quarter date), geographic area (postal code), or their interaction. Number of FE groups reports the number of distinct fixed-effect categories included in each specification. Standard errors are clustered at the bank level in all specifications. The unit of observation is a bank–firm–quarter.
*, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

The picture changes substantially when analysing the subsequent quarter (second quarter). Table 3 presents results for this lagged effect, maintaining the same specification choices and controls as in Table 2. Bandwidths are broadly comparable across quarters, supporting the commensurability of estimation samples. The lagged effect is negative and approximately equal in magnitude to the first-quarter effect, with statistical significance remaining strong across all specifications. This reversal could reflect the actual or anticipated detrimental impact of the climate event on the local business outlook, leading to a compression in investment activity. Alternatively, it may indicate a more cautious lending stance as banks reassess credit risk in the affected area. We explore both transmission channels in detail below.

Table 3: Effect of Flood Exposure on Lending Volumes (Quarter 2)

	(1)	(2)	(3)
	Dependent variable: Δ Lending Amount		
Treated	-0.050***	-0.035***	-0.052***
	(0.015)	(0.011)	(0.014)
Distance	-0.000	-0.000	-0.000***
	(0.000)	(0.000)	(0.000)
Treated × Distance	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)
Qrt.s from Event	2	2	2
Observations	6573	7333	6191
Bandwidth	[-424, 424]	[-488, 488]	[-400, 400]
Fixed Effects	No	Event	Area
Controls	Yes	Yes	Yes
Number of Firms	4658	5184	4377
Number of Banks	48	50	46
Number of FE Groups	0	4	179

Notes: This table reports regression discontinuity estimates of the effect of flood exposure on lending. The dependent variable is the change in log lending at the bank–firm level. The running variable is the distance to the flood boundary, normalised to zero at the cutoff. Treated is an indicator equal to one for observations located on the flooded side of the boundary. Controls include residual maturity. All specifications use local linear regressions with a triangular kernel and optimal bandwidth selection following Calonico et al. (2014). Bandwidths are reported in brackets. Fixed effects vary by specification and include time (quarter date), geographic area (postal code), or their interaction. Number of FE groups reports the number of distinct fixed-effect categories included in each specification. Standard errors are clustered at the bank level in all specifications. The unit of observation is a bank–firm–quarter.
*, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 4 summarises these findings by presenting cumulative effects over the two quarters following each climate event. Given the offsetting dynamics documented in Tables 2 and 3, the estimated cumulative effect is close to zero and consistently statistically insignificant across all specifications. Additional results, not reported for brevity, indicate that effects are absent beyond the second quarter, although this analysis is based on a subsample that excludes the Valencia flood event, for which our data cover only two post-event quarters.

Table 4: Effect of Flood Exposure on Lending Volumes (Quarters 1 and 2)

	(1)	(2)	(3)
	Dependent variable: Δ Lending Amount		
Treated	-0.005	0.001	0.004
	(0.013)	(0.011)	(0.015)
Distance	-0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)
Treated × Distance	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)
Qrt.s from Event	[1, 2]	[1, 2]	[1, 2]
Observations	15196	15104	15540
Bandwidth	[-510, 510]	[-503, 503]	[-532, 532]
Fixed Effects	No	Event	Area
Controls	Yes	Yes	Yes
Number of Firms	5607	5572	5730
Number of Banks	51	51	50
Number of FE Groups	0	8	277

Notes: This table reports regression discontinuity estimates of the effect of flood exposure on lending. The dependent variable is the change in log lending at the bank–firm level. The running variable is the distance to the flood boundary, normalised to zero at the cutoff. Treated is an indicator equal to one for observations located on the flooded side of the boundary. Controls include residual maturity. All specifications use local linear regressions with a triangular kernel and optimal bandwidth selection following Calonico et al. (2014). Bandwidths are reported in brackets. Fixed effects vary by specification and include time (quarter date), geographic area (postal code), or their interaction. Number of FE groups reports the number of distinct fixed-effect categories included in each specification. Standard errors are clustered at the bank level in all specifications. The unit of observation is a bank–firm–quarter.
*, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

The results are robust to alternative bandwidth choices. Table 29 reports lending volume estimates across a grid of asymmetric left and right bandwidths ranging from 200 to 600 metres, confirming that the positive first-quarter and negative second-quarter effects are stable in sign and significance across virtually all combinations.

5.2 Lending rates

We next study the impact of flood events on lending conditions through the lens of interest rates. Table 5 presents results for the quarterly change in the average interest rate on all outstanding exposures, measured at the individual bank–firm relationship level at the end of the first quarter. Regressions control for residual maturity, as before.

Table 5: Effect of Flood Exposure on Interest Rates (Quarter 1)

	(1)	(2)	(3)
	Dependent variable: Δ Interest Rate		
Treated	0.132	0.099	0.118
	(0.101)	(0.100)	(0.113)
Distance	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Treated × Distance	0.001**	0.001	0.000
	(0.000)	(0.001)	(0.001)
Qrt.s from Event	1	1	1
Observations	3980	3045	3073
Bandwidth	[-393, 393]	[-287, 287]	[-300, 300]
Fixed Effects	No	Event	Area
Controls	Yes	Yes	Yes
Number of Firms	2970	2272	2284
Number of Banks	33	32	29
Number of FE Groups	0	4	134

Notes: This table reports regression discontinuity estimates of the effect of flood exposure on lending. The dependent variable is the change in interest rates at the bank–firm level, expressed in percentages. The running variable is the distance to the flood boundary, normalised to zero at the cutoff. Treated is an indicator equal to one for observations located on the flooded side of the boundary. Controls include next interest rate reset date. All specifications use local linear regressions with a triangular kernel and optimal bandwidth selection following Calonico et al. (2014). Bandwidths are reported in brackets. Fixed effects vary by specification and include time (quarter date), geographic area (postal code), or their interaction. Number of FE groups reports the number of distinct fixed-effect categories included in each specification. Standard errors are clustered at the bank level in all specifications. The unit of observation is a bank–firm–quarter.
*, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

The results point to a modest increase in lending rates of approximately 10 to 13 basis points in the first quarter, though this effect is not statistically significant. The dynamics shift in the subsequent quarter, as shown in Table 6, which documents a statistically significant decline of approximately 14 basis points in the area fixed effects specification.

Table 6: Effect of Flood Exposure on Interest Rates (Quarter 2)

	(1)	(2)	(3)
	Dependent variable: Δ Interest Rate		
Treated	0.027	-0.061*	-0.142***
	(0.034)	(0.034)	(0.049)
Distance	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)
Treated × Distance	-0.000	-0.000	-0.001**
	(0.001)	(0.000)	(0.000)
Qrt.s from Event	2	2	2
Observations	2853	3564	3088
Bandwidth	[-271, 271]	[-345, 345]	[-302, 302]
Fixed Effects	No	Event	Area
Controls	Yes	Yes	Yes
Number of Firms	2112	2635	2277
Number of Banks	37	39	32
Number of FE Groups	0	4	134

Notes: This table reports regression discontinuity estimates of the effect of flood exposure on lending. The dependent variable is the change in interest rates at the bank–firm level, expressed in percentages. The running variable is the distance to the flood boundary, normalised to zero at the cutoff. Treated is an indicator equal to one for observations located on the flooded side of the boundary. Controls include next interest rate reset date. All specifications use local linear regressions with a triangular kernel and optimal bandwidth selection following Calonico et al. (2014). Bandwidths are reported in brackets. Fixed effects vary by specification and include time (quarter date), geographic area (postal code), or their interaction. Number of FE groups reports the number of distinct fixed-effect categories included in each specification. Standard errors are clustered at the bank level in all specifications. The unit of observation is a bank–firm–quarter.
*, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 7 presents the cumulative effects over both quarters. Consistent with the pattern observed for lending volumes, there is no economically or statistically significant cumulative effect on lending rates when both quarters are analysed together. Table 30 replicates the regressions across alternative bandwidths, confirming that the directional pattern is broadly robust, though precision naturally declines at narrower bandwidths.

Table 7: Effect of Flood Exposure on Interest Rates (Quarters 1 and 2)

	(1)	(2)	(3)
	Dependent variable: Δ Interest Rate		
Treated	0.079	0.015	-0.010
	(0.054)	(0.061)	(0.069)
Distance	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)
Treated × Distance	0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)
Qrt.s from Event	[1, 2]	[1, 2]	[1, 2]
Observations	6674	7345	9015
Bandwidth	[-322, 322]	[-357, 357]	[-457, 457]
Fixed Effects	No	Event	Area
Controls	Yes	Yes	Yes
Number of Firms	2696	2955	3570
Number of Banks	38	39	37
Number of FE Groups	0	8	230

Notes: This table reports regression discontinuity estimates of the effect of flood exposure on lending. The dependent variable is the change in interest rates at the bank–firm level, expressed in percentages. The running variable is the distance to the flood boundary, normalised to zero at the cutoff. Treated is an indicator equal to one for observations located on the flooded side of the boundary. Controls include next interest rate reset date. All specifications use local linear regressions with a triangular kernel and optimal bandwidth selection following Calonico et al. (2014). Bandwidths are reported in brackets. Fixed effects vary by specification and include time (quarter date), geographic area (postal code), or their interaction. Number of FE groups reports the number of distinct fixed-effect categories included in each specification. Standard errors are clustered at the bank level in all specifications. The unit of observation is a bank–firm–quarter.
*, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

The interest rate results are informative both in isolation and in conjunction with the lending volume findings, as they shed light on the transmission mechanism underlying the estimated effects. The systematic co-movement between quantities and prices across both quarters is consistent with loan demand being the predominant margin of adjustment. In the first quarter, concurrent increases in lending volumes and rates point to an outward shift in credit demand; in the second quarter, simultaneous declines in both are indicative of a demand contraction. While these patterns align with a demand-driven interpretation, supply-side responses—potentially reinforcing or partly offsetting the observed movements—cannot be excluded on the basis of reduced-form evidence alone. We address this identification challenge more rigorously in Section 5.4 by exploiting within-firm variation across multiple lending relationships.

5.3 Credit risk

Following the analysis of lending activity, we turn to the impact of flood events on credit risk. Tables 8 and 9 present estimates where the dependent variable is the default ratio at the bank–firm level, defined as the amount of newly defaulted loans in a given quarter relative to total loans outstanding at the beginning of that quarter. To capture the effect on loan quality precisely, we restrict attention to loans outstanding at the end of quarter 0 (the last quarter-end prior to the event) and trace their performance through the end of the first quarter (Table 8) and the second quarter (Table 9).

Table 8: Effect of Flood Exposure on Default Ratios (Quarter 1)

	(1)	(2)	(3)
		Dependent variable: Default Ratio	
Treated	0.568*	0.608*	0.494
	(0.334)	(0.347)	(0.302)
Distance	0.001*	0.002*	0.001*
	(0.001)	(0.001)	(0.001)
Treated × Distance	-0.001	-0.001	0.001
	(0.001)	(0.001)	(0.001)
Qrt.s from Event	1	1	1
Observations	7275	7069	8090
Bandwidth	[-398, 398]	[-382, 382]	[-456, 456]
Fixed Effects	No	Event	Area
Controls	No	No	No
Number of firms	4963	4811	5442
Number of banks	53	53	52

Notes: This table reports regression discontinuity estimates of the effect of flood exposure on credit risk. The dependent variable is the default ratio at the bank–firm level, computed as the amount of defaulted lending divided by total lending. The running variable is the distance to the flood boundary, normalised to zero at the cutoff. Treated is an indicator equal to one for observations located on the flooded side of the boundary. All specifications use local linear regressions with a triangular kernel and optimal bandwidth selection following Calonico et al. (2014). Bandwidths are reported in brackets. Fixed effects vary by specification and include time (quarter date) or geographic area (postal code). Standard errors are clustered at the bank level in all specifications. The unit of observation is a bank–firm–quarter.
*, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 9: Effect of Flood Exposure on Default Ratios (Quarters 1 and 2)

	(1)	(2)	(3)
	Dependent variable: Default Ratio		
Treated	0.680*	0.692*	0.639**
	(0.344)	(0.353)	(0.297)
Distance	0.002**	0.002**	0.002***
	(0.001)	(0.001)	(0.001)
Treated × Distance	-0.002*	-0.002*	-0.001
	(0.001)	(0.001)	(0.001)
Qrt.s from Event	[1, 2]	[1, 2]	[1, 2]
Observations	13577	13254	13916
Bandwidth	[-372, 372]	[-361, 361]	[-384, 384]
Fixed Effects	No	Event	Area
Controls	No	No	No
Number of firms	4880	4758	4994
Number of banks	59	59	59

Notes: This table reports regression discontinuity estimates of the effect of flood exposure on credit risk. The dependent variable is the default ratio at the bank–firm level, computed as the amount of defaulted lending divided by total lending. The running variable is the distance to the flood boundary, normalised to zero at the cutoff. Treated is an indicator equal to one for observations located on the flooded side of the boundary. All specifications use local linear regressions with a triangular kernel and optimal bandwidth selection following Calonico et al. (2014). Bandwidths are reported in brackets. Fixed effects vary by specification and include time (quarter date) or geographic area (postal code). Standard errors are clustered at the bank level in all specifications. The unit of observation is a bank–firm–quarter.
*, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

The estimates confirm that extreme climate events lead to a deterioration in the credit quality of firm loans. One quarter after the event, the default ratio rises by approximately half a percentage point, though this effect is only weakly significant. Over the two-quarter horizon, both the magnitude and statistical precision of the estimates increase moderately, with the default ratio rising by approximately 0.7 percentage points. While modest in absolute terms, this effect is economically meaningful: relative to an average annual default rate of approximately 1.4% among affected firms, it implies a near-twofold deterioration in the annualised default rate. These results are consistent with evidence that physical climate shocks can adversely affect borrower solvency and loan performance, primarily through disruptions to production, supply chains, and local economic activity (Klomp, 2014; Addoum et al., 2020; Noth and Schüwer, 2018). The gradual intensification of the effect over time further supports the view that the financial consequences of extreme weather events materialise with a lag, as firms' liquidity buffers and short-term adjustments provide only temporary mitigation.

Table 31 confirms the robustness of the default ratio estimates across alternative bandwidths. The cumulative two-quarter effect is consistently positive and statistically significant across most bandwidth combinations, with point estimates ranging from approximately 0.4 to 0.9 percentage points depending on the specification.

5.4 Transmission mechanism: loan demand versus loan supply

The joint analysis of lending volumes and rates suggests that demand-side factors play a significant role in shaping credit outcomes following extreme climate events. The opposite-signed effects on quantities and prices in both quarters are consistent with shifts in loan demand as the dominant force. However, this reduced-form evidence does not fully rule out supply-side factors operating alongside, or partially offsetting, demand effects.

We examine the demand-supply decomposition more rigorously by exploiting the panel structure of our loan-level dataset and the prevalence of multiple lending relationships, following Khwaja and Mian (2008). By comparing lending outcomes across different banks serving the same firm, we can absorb firm-specific demand factors through firm–time fixed effects. Within this framework, firm-level effects cannot be directly estimated as they are fully swept out by the fixed effects; attention therefore shifts to cross-bank heterogeneity, and specifically to bank characteristics that theory suggests should be relevant.

Bank Balance Sheet Constraints A first potential mechanism through which climate events could activate a bank lending channel operates via balance sheet strains on exposed intermediaries. Banks sustaining large losses from climate events may be compelled to reduce loan supply in order to deleverage, consistent with the mechanism described in Bernanke et al. (1991). This channel should be most relevant for banks with high portfolio concentration in affected areas or for banks exposed to particularly severe events. We therefore examine whether the bank-specific share of lending to affected borrowers—a proxy for the severity of the balance sheet shock—influences lending responses. Banks with elevated local exposure may face greater pressure to reduce credit to affected firms, thereby activating a supply-side lending channel.

Table 10: Transmission Channels: Lending Volumes

	Dependent variable: Δ Lending Amount					
	Quarter 1			**Quarter 2**		
	(1)	(2)	(3)	(4)	(5)	(6)
Share affected lending	0.091		0.072	0.125		0.117
	(0.103)		(0.102)	(0.174)		(0.174)
Share affected lending x Treated	-0.214		-0.188	-0.077		-0.129
	(0.229)		(0.253)	(0.403)		(0.406)
Main Bank		-0.083***	-0.085***		-0.031***	-0.031*
		(0.013)	(0.013)		(0.011)	(0.016)
Main Bank x Treated		0.096***	0.096***		-0.050	-0.076
		(0.018)	(0.022)		(0.052)	(0.072)
Observations	3980	4057	3995	1476	3881	1503
Bandwidth	[-603, 603]	[-605, 605]	[-604, 604]	[-337, 337]	[-579, 579]	[-341, 341]
Fixed Effects	Firm	Firm	Firm	Firm	Firm	Firm
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	1498	1532	1504	540	1458	551
Number of banks	21	41	21	13	41	13
Number of FE Groups	1498	1532	1504	540	1458	551

Notes: Controls include residual maturity. Bank-level clustered standard errors in parentheses.
*, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Column 1 of Table 10 incorporates firm–time fixed effects together with an interaction term between the treated firm indicator and the bank's share of affected lending. The coefficient on this interaction term is statistically insignificant, consistent with our earlier inference that demand-side factors are dominant. This null result persists in the second quarter (column 4) and across alternative dependent variables including interest rate changes (Table 11, columns 1 and 4) and changes in collateralisation (Table 12, columns 1 and 4).

Table 11: Transmission Channels: Interest Rates

	Dependent variable: Δ Interest Rate					
	Quarter 1			**Quarter 2**		
	(1)	(2)	(3)	(4)	(5)	(6)
Share affected lending	-0.240		-0.225	0.086		0.086
	(0.290)		(0.285)	(0.252)		(0.251)
Share affected lending x Treated	-0.189		-0.177	-0.057		-0.038
	(0.392)		(0.390)	(0.687)		(0.707)
Main Bank		0.072**	0.070**		-0.019	0.005
		(0.032)	(0.033)		(0.024)	(0.034)
Main Bank x Treated		-0.031	-0.035		0.033	0.028
		(0.045)	(0.048)		(0.040)	(0.098)
Observations	4170	4235	4168	1430	3904	1430
Bandwidth	[-640, 640]	[-642, 642]	[-640, 640]	[-327, 327]	[-582, 582]	[-328, 328]
Fixed Effects	Firm	Firm	Firm	Firm	Firm	Firm
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	1572	1601	1571	525	1466	525
Number of banks	21	41	21	13	41	13
Number of FE Groups	1572	1601	1571	525	1466	525

Notes: Controls include next interest rate reset date. Bank-level clustered standard errors in parentheses.
*, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 12: Transmission Channels: Collateral Requirements

	Dependent variable: Δ Collateral Amount					
	Quarter 1			**Quarter 2**		
	(1)	(2)	(3)	(4)	(5)	(6)
Share affected lending	0.107		0.089	0.324^{**}		0.319^{**}
	(0.131)		(0.127)	(0.140)		(0.138)
Share affected lending x Treated	0.010		0.077	-0.194		-0.206
	(0.216)		(0.202)	(0.372)		(0.329)
Main Bank		-0.045^{***}	-0.045^{***}		-0.019	-0.029
		(0.013)	(0.014)		(0.017)	(0.020)
Main Bank x Treated		0.089^{***}	0.091^{***}		-0.062	-0.060
		(0.026)	(0.026)		(0.046)	(0.068)
Observations	2889	2927	2889	1180	3166	1205
Bandwidth	[-545, 545]	[-547, 547]	[-545, 545]	[-335, 335]	[-605, 605]	[-342, 342]
Fixed Effects	Firm	Firm	Firm	Firm	Firm	Firm
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	1163	1179	1163	468	1268	480
Number of banks	20	30	20	12	30	12
Number of FE Groups	1163	1179	1163	468	1268	480

Notes: Controls include residual maturity. Bank-level clustered standard errors in parentheses.
*, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

A notable exception emerges in the analysis of loan maturity. Column 1 of Table 13 documents a significant positive coefficient on the interaction term, indicating that more exposed banks tend to extend loan maturities to affected firms in the first quarter. While this pattern is consistent with loan evergreening practices documented in Peek and Rosengren (2005), its quantitative relevance is limited.[7]

[7] In principle, maturity extensions could also reflect public credit support programmes such as moratoria. However, such programmes would apply uniformly to all affected firms and would therefore not generate correlation with bank-specific exposure measures, as we observe here.

Table 13: Transmission Channels: Residual Maturity

	Dependent variable: Residual Maturity					
	Quarter 1			**Quarter 2**		
	(1)	(2)	(3)	(4)	(5)	(6)
Share affected lending	-17.415*		-13.356	-19.515		-16.263
	(9.767)		(8.722)	(13.629)		(12.669)
Share affected lending x Treated	20.879**		24.851***	17.530		19.845*
	(8.176)		(7.580)	(10.214)		(9.502)
Main Bank		10.428***	10.118***		9.295***	11.631***
		(1.398)	(1.398)		(1.689)	(1.656)
Main Bank x Treated		-1.812	-0.773		-4.323*	-3.211
		(2.393)	(2.386)		(2.344)	(2.324)
Observations	4539	4635	4514	1499	4602	1499
Bandwidth	[-655, 655]	[-652, 652]	[-654, 654]	[-328, 328]	[-652, 652]	[-328, 328]
Fixed Effects	Firm	Firm	Firm	Firm	Firm	Firm
Controls	No	No	No	No	No	No
Number of firms	1700	1734	1690	546	1721	546
Number of banks	21	42	21	13	45	13
Number of FE Groups	1700	1734	1690	546	1721	546

Notes: Bank-level clustered standard errors in parentheses.
*, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Relationship Lending. A second supply-side channel operates through relationship intensity. A long-standing literature originating with Sharpe (1990) argues that close lending relationships create implicit contracts that may shape credit provision during adverse shocks. Our results indicate that relationship strength materially affects lending responses. Column 2 of Table 10 shows that firms' main bank provides substantially greater credit access in the first quarter, with an estimated effect of approximately 9.6 percentage points relative to non-main bank relationships. This heterogeneity is economically meaningful: the positive average effect on lending documented in Table 2 masks considerable cross-bank variation, with main banks providing markedly stronger support.[8]

However, this enhanced credit provision does not represent an unconditional relaxation of lending standards. Column 2 of Table 12 reveals that main banks simultaneously tighten collateralisation requirements for affected firms in the first quarter, indicating that relationship banks balance credit support with prudential risk management. Interaction terms involving the main bank indicator in other specifications—across different dependent variables and time periods—are generally insignificant.[9]

[8] The negative coefficient on the main bank dummy for unaffected firms may reflect balance-sheet reallocation by relationship lenders, who appear to reduce credit to unaffected firms in the same area, possibly driven by diversification motives.

[9] A negative and borderline-significant coefficient emerges for loan maturity in the second quarter (column 5

When both supply-side factors—the share of affected lending and the main bank indicator—are included jointly (columns 3 and 6 of Tables 10–12), the estimates remain virtually unchanged, confirming that the influence of these bank characteristics is stable and does not materially alter the baseline effects. Additional unreported results explore a broader set of bank characteristics—including capital ratios, liquidity ratios, non-performing loan ratios, bank size, and geographic specialisation (proxied by the share of local lending to both affected and unaffected firms)—none of which exhibit statistically significant effects on lending volumes, interest rates, loan maturities, or collateralisation, in either quarter.

Tables 18 to 21 further strengthen identification by augmenting the baseline specification with event-specific bank fixed effects. Since quarters 1 and 2 are estimated separately, these correspond to time-specific bank fixed effects, ensuring that all bank characteristics—observable or unobservable, time-invariant or time-varying—are fully absorbed. The results remain virtually unchanged, reinforcing the robustness of our conclusions.

Tables 22 to 25 replicate this exercise under an alternative specification that replaces firm fixed effects with a saturated set of country-by-industry-by-size-by-time interactions, exploiting the empirical equivalence to firm–time fixed effects established by Degryse et al. (2019). This approach has the practical advantage of not restricting the sample to firms with multiple banking relationships. The main results for lending volumes, interest rates, and the role of main banks are confirmed across all outcome variables. Although the significance of the affected-lending share shifts from the maturity to the collateralisation equation under this specification, this reallocation does not alter the overall interpretation.

Taken together, the evidence points to demand-side factors as the dominant driver of credit market responses to climate shocks. Bank balance-sheet constraints—whether measured by the share of affected lending or standard solvency indicators—do not exert a statistically significant effect on lending behaviour in either quarter. The average bank in our sample is well capitalised, with voluntary capital buffers approximately four percentage points above minimum regulatory requirements, and sufficiently geographically diversified to sustain only limited exposure to any individual event (the average share of affected lending is 0.2%).

Overall, supply-side constraints appear limited but not entirely absent. There is some indication that relatively more exposed banks extend loan maturities in the first quarter, a pattern potentially consistent with mild evergreening. Moreover, strong lending relationships demonstrably facilitated credit access for the most affected firms, albeit alongside a tightening of collateral requirements—a pattern coherent with the literature documenting relationship lending as both a stabilising and risk-mitigating mechanism during periods of borrower distress (Bolton et al., 2016; Jiménez et al., 2020).

of Table 13), but this result disappears once additional controls are introduced.

5.5 Firm heterogeneity

The aggregate effects documented above may conceal important variation in how different types of firms respond to physical climate shocks. To explore this dimension, we augment the baseline spatial RDD specification with interaction terms between the treatment indicator and two key firm characteristics: size (micro as baseline; SME; large) and sector (manufacturing as baseline; wholesale and retail; construction; services; other). These analyses shed light on whether vulnerability to extreme flood events is concentrated in particular segments of the corporate sector, or whether physical climate risk propagates relatively uniformly across firms.

Lending Volumes. Table 14 reports heterogeneous effects on lending volumes. Firm size does not generate significant heterogeneity in either quarter. By contrast, the sectoral dimension is informative in the first quarter: relative to manufacturing, wholesale and retail, construction, and services firms receive significantly more lending, with differential effects of approximately 11.2, 14.3, and 9.0 percentage points, respectively. These patterns are economically intuitive. Wholesale and retail firms likely face immediate disruptions to inventory and supply chains; construction firms may require bridge financing to cover stalled projects or damaged equipment; and services firms need liquidity to sustain fixed costs while revenues are temporarily impaired. The precision and magnitude of these estimates point to a rapid, sector-specific liquidity response by lenders in the immediate aftermath of the shock. By the second quarter, the picture shifts. The treated coefficient in the size specification turns negative, consistent with an aggregate reversal in lending following the initial expansion, though not precisely estimated on its own, and all sector interaction terms lose statistical significance. This convergence indicates that, while initial liquidity needs differ meaningfully by sector, the medium-term credit contraction operates uniformly across industries.

Table 14: Heterogeneous Effects on Lending Volumes: Firm Size and Sectoral Composition

	Dependent variable: Δ Lending Amount			
	Quarter 1		**Quarter 2**	
	(1)	(2)	(3)	(4)
Treated	0.021	-0.042	-0.120	-0.040
	(0.056)	(0.037)	(0.076)	(0.031)
SME × Treated	0.029		0.103	
	(0.061)		(0.072)	
Large × Treated	0.009		0.057	
	(0.059)		(0.083)	
Wholesale & Retail × Treated		0.112***		-0.044
		(0.038)		(0.037)
Construction × Treated		0.143**		-0.025
		(0.057)		(0.057)
Services × Treated		0.090**		0.015
		(0.042)		(0.040)
Other × Treated		0.103		-0.058
		(0.078)		(0.062)
Observations	7383	7371	6621	6549
Bandwidth	[-483, 483]	[-489, 489]	[-429, 429]	[-431, 431]
Fixed Effects	No	No	No	No
Controls	Yes	Yes	Yes	Yes
Number of firms	5236	5229	4693	4644
Number of banks	47	46	48	47

Notes: The baseline is micro firm or in manufacturing sector. Controls include residual maturity. Bank-level clustered standard errors in parentheses.
*, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Interest Rates. Table 15 presents heterogeneous effects on loan interest rates. In the first quarter, the treated baseline coefficient is negative across size specifications, consistent with a mild easing of borrowing costs for micro firms in the immediate aftermath of the flood, though this effect is not statistically significant. Against this baseline, SMEs face significantly higher rates, with the interaction term significant at the 1% level (27.9 basis points). Large firm rates also move upward relative to micro firms, but the interaction term is imprecisely estimated. Across sectors, wholesale and retail firms face a modest but significant rate increase of 13.2 basis points relative to manufacturing; construction firms face an increase of 18.9 basis points, significant at the 10% level. In the second quarter, significant heterogeneity emerges at the sectoral level. Construction firms benefit from a 26.7 basis point rate reduction relative to manufacturing, consistent with targeted forbearance or preferential lending terms for a sector perceived as particularly exposed to flood disruption. The residual “other” category exhibits a large and significant negative differential (31.3 basis points), reinforcing the interpretation that this group is compositionally distinct from the remaining sectors.

Table 15: Heterogeneous Effects on Interest Rates: Firm Size and Sectoral Composition

	Dependent variable: Δ Interest Rate			
	Quarter 1		**Quarter 2**	
	(1)	(2)	(3)	(4)
Treated	-0.117	0.053	-0.091	0.034
	(0.098)	(0.060)	(0.094)	(0.047)
SME × Treated	0.279***		0.172	
	(0.092)		(0.127)	
Large × Treated	0.175		0.098	
	(0.111)		(0.118)	
Wholesale & Retail × Treated		0.132**		-0.068
		(0.049)		(0.077)
Construction × Treated		0.189*		-0.267***
		(0.108)		(0.099)
Services × Treated		0.012		0.090
		(0.067)		(0.087)
Other × Treated		-0.126		-0.313***
		(0.104)		(0.082)
Observations	7169	7072	5009	4941
Bandwidth	[-410, 410]	[-406, 406]	[-272, 272]	[-270, 270]
Fixed Effects	No	No	No	No
Controls	Yes	Yes	Yes	Yes
Number of firms	4944	4878	3424	3375
Number of banks	46	45	49	48

Notes: The baseline is micro firm or in manufacturing sector. Controls include next interest rate reset date. Bank-level clustered standard errors in parentheses.
*, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Collateral Requirements. Table 16 reports heterogeneous effects on collateral requirements. In the first quarter, neither the treated baseline nor any size interaction achieves statistical significance, indicating no differential tightening of collateral conditions across firm sizes in the immediate aftermath of the flood. Sectorally, however, wholesale and retail (8.5 percentage points) and construction (8.0 percentage points) firms face significantly higher collateral requirements relative to manufacturing, consistent with banks pricing in elevated perceived risk in the sectors most directly disrupted by the shock. Services and the "other" category exhibit no significant differential response. In the second quarter, the pattern reverses: the treated coefficient turns negative and statistically significant (11.5 percentage points) in the size specification, indicating a broad easing of collateral conditions for micro firms as the immediate shock subsides. Against this backdrop, SMEs face a significant positive differential (12.5 percentage points), implying a relative tightening that partially offsets the aggregate easing for these borrowers. Large firms show no distinguishable response. Across sectors, all interaction terms are statistically insignificant in the second quarter, indicating that the initial sectoral heterogeneity in overall collateral requirements was transitory.

Table 16: Heterogeneous Effects on Collateral Requirements: Firm Size and Sectoral Composition

	Dependent variable: Δ Collateral Amount			
	Quarter 1		**Quarter 2**	
	(1)	(2)	(3)	(4)
Treated	0.028	0.026	-0.115**	-0.035
	(0.091)	(0.051)	(0.054)	(0.028)
SME × Treated	0.028		0.125**	
	(0.087)		(0.062)	
Large × Treated	0.013		0.089	
	(0.091)		(0.058)	
Wholesale & Retail × Treated		0.085**		0.003
		(0.034)		(0.042)
Construction × Treated		0.080***		0.006
		(0.023)		(0.065)
Services × Treated		-0.013		0.036
		(0.044)		(0.042)
Other × Treated		-0.006		-0.048
		(0.102)		(0.034)
Observations	4726	4518	6403	6186
Bandwidth	[-332, 332]	[-320, 320]	[-490, 490]	[-476, 476]
Fixed Effects	No	No	No	No
Controls	Yes	Yes	Yes	Yes
Number of firms	3559	3413	4771	4617
Number of banks	39	38	42	41

Notes: The baseline is micro firm or in manufacturing sector. Controls include residual maturity. Bank-level clustered standard errors in parentheses.
*, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Loan-to-Value Ratios. Table 17 analyses heterogeneity in LTV dynamics for loans backed by real estate collateral. The treated coefficient is positive and significant at the 5% level (5.3 percentage points) in the size specification, indicating that micro firms experience a meaningful increase in LTV following the flood—consistent with a decline in the market value of real estate collateral relative to outstanding loan balances. The interaction term for large firms is negative and significant (6.2 percentage points), implying a net tightening of LTV conditions for large relative to micro borrowers. This pattern suggests that banks adjust collateral valuations more conservatively for larger firms, possibly reflecting greater monitoring capacity or greater contractual flexibility in revising appraisals. SME interactions are statistically indistinguishable from zero. Across sectors and ex-ante risk categories, no significant heterogeneity emerges, indicating that the LTV response is driven primarily by firm size rather than industry affiliation or pre-existing credit risk.

Table 17: Heterogeneous Effects on Change in LTV (Real Estate Collateral): Firm Size, Sector, and Ex-Ante Risk

	(1)	(2)	(3)
		Dependent variable: Δ LTV	
Treated	0.053**	0.009	0.001
	(0.023)	(0.020)	(0.006)
SME x Treated	-0.036		
	(0.023)		
Large x Treated	-0.062**		
	(0.021)		
Wholesale & Retail x Treated		0.008	
		(0.022)	
Construction x Treated		-0.007	
		(0.028)	
Services x Treated		-0.011	
		(0.018)	
Other x Treated		-0.016	
		(0.020)	
High Risk x Treated			-0.005
			(0.011)
Observations	1734	1953	1952
Bandwidth	[-336, 336]	[-351, 351]	[-347, 347]
Fixed Effects	No	No	No
Controls	Yes	Yes	Yes
Number of firms	857	973	971
Number of banks	18	18	18

Notes: This table reports heterogeneous effects for loans backed by real estate collateral (residential, offices/commercial premises, and commercial real estate). The baseline is micro firm or in manufacturing sector. Controls include residual maturity. Bank-level clustered standard errors in parentheses.
*, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Lending Volumes of Ex-Ante High Risk Firms. Table 26 analyses whether ex-ante firm risk shapes the lending response to flood events. Column 3 shows that, in the first quarter, the treated baseline coefficient is positive and significant (3.6 percentage points), indicating a net increase in lending to low-risk treated firms. Against this baseline, the interaction term for high-risk firms is negative and strongly significant (11.9 percentage points), implying that banks sharply curtail credit to higher-risk borrowers even as they expand lending to their safer counterparts. The net first-quarter effect for high-risk firms is therefore negative, at approximately 8.3 percentage points. This pattern is consistent with active credit risk screening by banks in the immediate aftermath of the shock: rather than treating all affected borrowers symmetrically, lenders appear to reallocate credit toward lower-risk firms while withdrawing from those whose pre-existing risk profile makes recovery less certain. In the second quarter, the differential disappears, as the aggregate lending contraction documented in the baseline results applies uniformly across the risk distribution.

Summary. Taken together, these results reveal a nuanced but coherent pattern of heterogeneity in credit market responses to physical climate shocks. Sectoral composition is the dominant source of variation in the immediate aftermath of the event: wholesale and retail, construction, and services firms receive disproportionately larger lending increases in the first quarter, while wholesale and retail and construction firms simultaneously face higher collateral requirements, a combination consistent with banks extending emergency liquidity while tightening credit standards in the sectors most directly affected. These sectoral differences largely dissipate by the second quarter, as medium-term credit dynamics converge across industries. Firm size generates more limited but persistent heterogeneity: micro firms benefit from lower borrowing costs and eased collateral conditions over time, while SMEs face a relative tightening in collateral requirements in the second quarter. The LTV evidence further suggests that banks apply more conservative real estate collateral adjustments to larger borrowers. Ex-ante credit risk emerges as a sharp dividing line in the first quarter: banks expand credit to lower-risk affected firms while simultaneously contracting lending to higher-risk ones, consistent with active credit reallocation toward safer borrowers in the immediate aftermath of the shock. This risk-based differentiation is not sustained into the second quarter, when the aggregate credit contraction applies uniformly across the risk distribution. Overall, the findings indicate that physical climate risk propagates with broad uniformity across the corporate sector in the medium term, but that the immediate adjustment is shaped meaningfully by sector-specific liquidity needs, firm size, and pre-existing credit risk.

6 Conclusion

This paper provides causal evidence that major flood events materially affect the credit portfolios of European banks, but in a way that is considerably more nuanced than a simple narrative of disaster-driven credit contraction. By combining metre-level geospatial precision from the CEMS with loan-level data from AnaCredit, and exploiting a spatial regression discontinuity design across four European floods between 2021 and 2024, we are able to trace the full dynamic response of credit markets to physical climate shocks along multiple dimensions.

We find an initial expansion of lending to directly exposed firms, followed by a retrenchment one quarter later, and no significant net effect on lending volumes or loan rates over the two-quarter horizon. This pattern, together with the behaviour of interest rates, points to loan demand adjustments as the dominant driver of post-disaster lending dynamics, rather than binding supply constraints or an immediate tightening of bank credit standards. Flood-exposed firms initially draw on credit lines and seek working capital to manage acute liquidity needs; as the investment outlook subsequently deteriorates, credit demand contracts. This interpretation is directly confirmed by the demand–supply decomposition: banks with larger aggregate portfolio exposures to affected firms do not systematically restrict credit, raise rates, or tighten collateral requirements relative to less exposed intermediaries, ruling out a broad-based bank lending channel.

At the same time, we find clear evidence that physical climate shocks worsen credit quality. Default ratios on pre-existing loans increase persistently in the quarters following the floods, with magnitudes that are modest in absolute terms but substantial relative to baseline rates. This lagged deterioration is consistent with firms initially buffering the shock using liquidity and support measures, before weakening fundamentals translate into higher credit losses. The persistence and growing magnitude of the credit risk effect stands in contrast to the temporary and self-reversing lending effects, showing that physical climate shocks impose lasting costs on loan portfolios even when aggregate credit supply remains broadly stable.

Supply-side considerations are not entirely absent but are limited in scope and operate through specific channels rather than broadly. Relationship lending plays a central, yet subtle, role in the transmission of physical risk. Main banks, defined as the lender with the largest bilateral exposure to a given firm, provide stronger support to their clients in the immediate aftermath of the floods, cushioning liquidity stress and helping sustain credit flows when disruptions to production and cash flows are most acute. However, this support is coupled with tighter collateralisation, suggesting that main banks share risk with firms rather than absorbing it unconditionally.

The heterogeneity results add further information. In the immediate aftermath of the shock, sector and pre-existing firm risk are the primary axes along which the credit response differentiates. In the first quarter, wholesale and retail, construction, and services show significantly more credit than manufacturing firms, reflecting sector-specific liquidity needs, while also facing higher collateral requirements, consistent with banks extending emergency liquidity in the sectors most directly disrupted. Banks also conduct implicit credit screening in real time: lending expands toward lower-risk affected firms and contracts relative to higher-risk ones. These effects converge to a uniform pattern by the second quarter, as medium-term dynamics is driven by aggregate demand conditions rather than firm-specific characteristics.

Taken together, the results support the view that well-capitalised, geographically diversified European banks can intermediate sizeable local climate shocks without generating systemic credit contractions. The dominant channel through which flood exposure affects banks is the gradual and persistent deterioration of asset quality among exposed borrowers, not an immediate contraction in credit supply.

These findings have several implications for policy. First, climate stress tests that focus primarily on supply-side tightening miss a critical component of the adjustment: the short-run demand-driven lending expansion, which temporarily inflates exposure even as future credit losses are accruing. Stress scenarios should incorporate the full dynamic path of loan demand and credit quality, not just bank-level capital and liquidity constraints. Second, the results show the importance of collateral and relationship banking channels in the transmission of physical shocks. Frameworks that model only direct valuation of losses fail to capture how banks actively restructure credit conditions, extending maturities, tightening collateral requirements, and differentiating by borrower risk, in response to the shock. Third, the granular, localised nature of the effects argues for supervisory approaches that combine high-resolution geospatial exposure data with scenario designs that allow for non-linear, state-contingent physical risks across sectors and firm types.

Several important questions remain open. Our design identifies local effects at the flood boundary but does not capture spillovers to unaffected firms in the broader regional economy, which may be quantitatively important (Bassetti et al., 2025). Finally, the four events we study share a common character as localised, acute flood episodes; the extent to which these findings generalise to other types of physical risk, such as chronic heat stress or gradual sea-level rise, remains an open and important question for future research.

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A Appendix

A.1 Transmission Mechanism Tables: Firm-Bank-Event Fixed Effects

Table 18: Transmission Channels Robustness: Lending Volumes

	Dependent variable: Δ Lending Amount					
	Quarter 1			**Quarter 2**		
	(1)	(2)	(3)	(4)	(5)	(6)
Share affected lending x Treated	-0.162		-0.141	-0.094		-0.129
	(0.227)		(0.251)	(0.396)		(0.398)
Main Bank		-0.087***	-0.088***		-0.026**	-0.022
		(0.020)	(0.020)		(0.011)	(0.016)
Main Bank x Treated		0.094***	0.091***		-0.050	-0.060
		(0.018)	(0.021)		(0.052)	(0.075)
Observations	3966	4015	3976	1472	3862	1484
Bandwidth	[-600, 600]	[-602, 602]	[-602, 602]	[-335, 335]	[-579, 579]	[-339, 339]
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	1493	1515	1496	538	1451	543
Number of banks	21	28	21	13	30	13
Number of FE Groups	3966	4015	3976	1472	3862	1484

Notes: All specifications include firm-time-bank fixed effects. Controls include residual maturity. Bank-level clustered standard errors in parentheses.
*, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 19: Transmission Channels Robustness: Interest Rates

	Dependent variable: Δ Interest Rate					
	Quarter 1			**Quarter 2**		
	(1)	(2)	(3)	(4)	(5)	(6)
Share affected lending x Treated	-0.022		-0.025	0.294		0.311
	(0.345)		(0.343)	(0.754)		(0.764)
Main Bank		0.049	0.049		-0.017	-0.022
		(0.034)	(0.034)		(0.023)	(0.035)
Main Bank x Treated		-0.040	-0.039		0.031	0.038
		(0.050)	(0.050)		(0.040)	(0.095)
Observations	4172	4219	4172	1432	3887	1432
Bandwidth	[-642, 642]	[-642, 642]	[-641, 641]	[-329, 329]	[-583, 583]	[-330, 330]
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	1573	1595	1573	526	1460	526
Number of banks	21	29	21	13	30	13
Number of FE Groups	4172	4219	4172	1432	3887	1432

Notes: All specifications include firm-time-bank fixed effects. Controls include next interest rate reset. Bank-level clustered standard errors in parentheses.
*, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 20: Transmission Channels Robustness: Residual Maturity

	Dependent variable: Residual Maturity					
	Quarter 1			**Quarter 2**		
	(1)	(2)	(3)	(4)	(5)	(6)
Share affected lending x Treated	15.742*		19.579**	7.964		10.829
	(8.222)		(8.114)	(11.087)		(11.451)
Main Bank		10.366***	10.223***		9.263***	12.010***
		(1.135)	(1.182)		(1.573)	(1.487)
Main Bank x Treated		-1.594	-0.854		-3.681	-1.885
		(2.333)	(2.345)		(2.398)	(1.930)
Observations	4543	4627	4503	1544	4625	1544
Bandwidth	[-655, 655]	[-654, 654]	[-653, 653]	[-336, 336]	[-656, 656]	[-336, 336]
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No	No
Number of firms	1701	1729	1688	560	1729	560
Number of banks	21	31	21	13	32	13
Number of FE Groups	4543	4627	4503	1544	4625	1544

Notes: All specifications include firm-time-bank fixed effects. Bank-level clustered standard errors in parentheses.
*, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 21: Transmission Channels Robustness: Collateral Requirements

	Dependent variable: Δ Collateral Amount					
	Quarter 1			**Quarter 2**		
	(1)	(2)	(3)	(4)	(5)	(6)
Share affected lending x Treated	-0.030		0.028	-0.145		-0.167
	(0.180)		(0.178)	(0.383)		(0.339)
Main Bank		-0.047***	-0.047***		-0.018	-0.030
		(0.016)	(0.016)		(0.018)	(0.021)
Main Bank x Treated		0.081***	0.081**		-0.065	-0.049
		(0.028)	(0.029)		(0.050)	(0.072)
Observations	2889	2909	2895	1180	3152	1203
Bandwidth	[-545, 545]	[-546, 546]	[-546, 546]	[-335, 335]	[-606, 606]	[-341, 341]
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	1163	1170	1165	468	1261	479
Number of banks	20	22	20	12	25	12
Number of FE Groups	2889	2909	2895	1180	3152	1203

Notes: All specifications include firm-time-bank fixed effects. Controls include residual maturity. Bank-level clustered standard errors in parentheses.
*, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

A.2 Transmission Mechanism Tables: Country-Sector-Size-Event Fixed Effects

Table 22: Transmission Channels Robustness: Lending Volumes

	Dependent variable: Δ Lending Amount					
	Quarter 1			**Quarter 2**		
	(1)	(2)	(3)	(4)	(5)	(6)
Share affected lending	0.124		0.116	-0.044		-0.036
	(0.156)		(0.145)	(0.094)		(0.095)
Share affected lending x Treated	0.175		0.217**	0.061		0.002
	(0.109)		(0.103)	(0.085)		(0.092)
Main Bank		-0.074***	-0.076***		-0.026**	-0.024
		(0.017)	(0.017)		(0.013)	(0.017)
Main Bank x Treated		0.103***	0.113***		-0.031	-0.054
		(0.024)	(0.026)		(0.037)	(0.051)
Observations	6936	7347	6976	2900	7394	2651
Bandwidth	[-485, 485]	[-487, 487]	[-488, 488]	[-293, 293]	[-502, 502]	[-261, 261]
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	4843	5218	4868	2082	5228	1900
Number of banks	21	46	21	13	49	13
Number of FE Groups	15	20	15	10	20	10

Notes: All specifications include country-sector-size-event fixed effects (Degryse et al., 2019). Controls include residual maturity. Bank-level clustered standard errors in parentheses.
*, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 23: Transmission Channels Robustness: Interest Rates

	Dependent variable: Δ Interest Rate					
	Quarter 1			**Quarter 2**		
	(1)	(2)	(3)	(4)	(5)	(6)
Share affected lending	-0.487*		-0.489*	-0.196		-0.165
	(0.251)		(0.254)	(0.226)		(0.257)
Share affected lending x Treated	0.461		0.480	0.467		0.418
	(0.313)		(0.334)	(0.353)		(0.370)
Main Bank		-0.025	-0.027		-0.067**	-0.121***
		(0.034)	(0.035)		(0.027)	(0.032)
Main Bank x Treated		0.040	0.049		0.058	0.032
		(0.084)	(0.090)		(0.063)	(0.055)
Observations	3925	4014	3930	2454	6105	2449
Bandwidth	[-243, 243]	[-243, 243]	[-244, 244]	[-240, 240]	[-396, 396]	[-240, 240]
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	2754	2867	2757	1760	4336	1756
Number of banks	21	43	21	13	47	13
Number of FE Groups	15	20	15	10	20	10

Notes: All specifications include country-sector-size-event fixed effects (Degryse et al., 2019). Controls include next interest rate reset. Bank-level clustered standard errors in parentheses.
*, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 24: Transmission Channels Robustness: Residual Maturity

	Dependent variable: Residual Maturity					
	Quarter 1			**Quarter 2**		
	(1)	(2)	(3)	(4)	(5)	(6)
Share affected lending	-4.484		-3.489	14.051		9.246
	(13.165)		(11.279)	(15.979)		(12.580)
Share affected lending x Treated	-1.290		3.248	-23.223		-14.717
	(14.712)		(15.160)	(17.409)		(17.511)
Main Bank		13.466***	13.251***		12.720***	15.302***
		(1.039)	(1.049)		(1.314)	(1.719)
Main Bank x Treated		-0.097	0.607		-0.262	-5.215
		(2.971)	(3.350)		(2.611)	(4.020)
Observations	5730	6081	5766	1925	6371	1952
Bandwidth	[-361, 361]	[-362, 362]	[-364, 364]	[-187, 187]	[-386, 386]	[-191, 191]
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No	No
Number of firms	3973	4261	3996	1388	4464	1407
Number of banks	21	47	21	13	52	13
Number of FE Groups	15	20	15	10	20	10

Notes: All specifications include country-sector-size-event fixed effects (Degryse et al., 2019). Bank-level clustered standard errors in parentheses.
*, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 25: Transmission Channels Robustness: Collateral Requirements

	Dependent variable: Δ Collateral Amount					
	Quarter 1			**Quarter 2**		
	(1)	(2)	(3)	(4)	(5)	(6)
Share affected lending	0.197		0.192	0.005		0.012
	(0.158)		(0.151)	(0.112)		(0.110)
Share affected lending x Treated	0.476***		0.514***	-0.253		-0.270
	(0.156)		(0.149)	(0.187)		(0.174)
Main Bank		-0.045**	-0.046**		-0.007	-0.007
		(0.018)	(0.019)		(0.011)	(0.011)
Main Bank x Treated		0.076***	0.091***		-0.036	-0.034
		(0.021)	(0.024)		(0.030)	(0.042)
Observations	4805	4891	4794	2226	6027	2258
Bandwidth	[-360, 360]	[-349, 349]	[-359, 359]	[-240, 240]	[-462, 462]	[-244, 244]
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	3584	3690	3578	1676	4495	1703
Number of banks	20	38	20	12	40	12
Number of FE Groups	15	20	15	10	20	10

Notes: All specifications include country-sector-size-event fixed effects (Degryse et al., 2019). Controls include residual maturity. Bank-level clustered standard errors in parentheses.
*, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

A.3 Firm Heterogeneity: Role of Ex-Ante Firm Risk

Table 26: Heterogeneous Effects on Lending Volumes: Firm Size, Sector, and Ex-Ante Risk

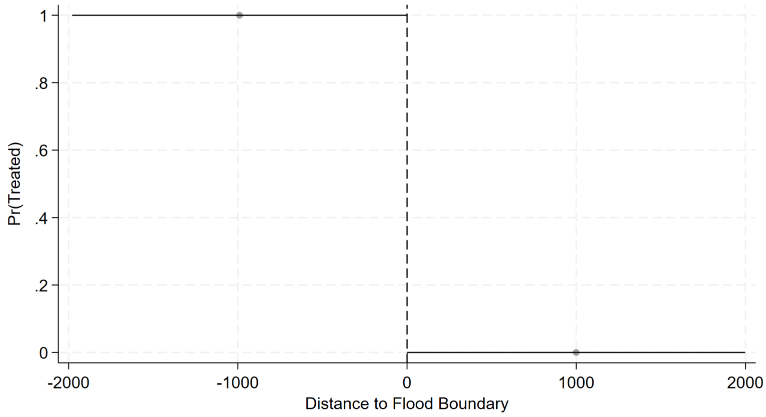
	Dependent variable: Δ Lending Amount					
	Quarter 1			**Quarter 2**		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.021	-0.042	0.036*	-0.120	-0.040	-0.038**
	(0.056)	(0.037)	(0.021)	(0.076)	(0.031)	(0.016)
SME × Treated	0.029		0.103			
	(0.061)		(0.072)			
Large × Treated	0.009		0.057			
	(0.059)		(0.083)			
Wholesale & Retail × Treated		0.112***			-0.044	
		(0.038)			(0.037)	
Construction × Treated		0.143**			-0.025	
		(0.057)			(0.057)	
Services × Treated		0.090**			0.015	
		(0.042)			(0.040)	
Other × Treated		0.103			-0.058	
		(0.078)			(0.062)	
High Risk × Treated			-0.119**			-0.028
			(0.050)			(0.053)
Observations	7383	7371	7383	6621	6549	6621
Bandwidth	[-483, 483]	[-489, 489]	[-483, 483]	[-429, 429]	[-431, 431]	[-430, 430]
Fixed Effects	No	No	No	No	No	No
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	5236	5229	5236	4693	4644	4693
Number of banks	47	46	47	48	47	48

Notes: The baseline is micro firm and/or in manufacturing sector. High Risk is an indicator for firms above the 95th percentile of ex-ante credit risk, computed by residualizing the pre-event interest rate spread from firm size, NUTS3 region, and sector. Controls include residual maturity. Bank-level clustered standard errors in parentheses.
*, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

A.4 Robustness and other tables

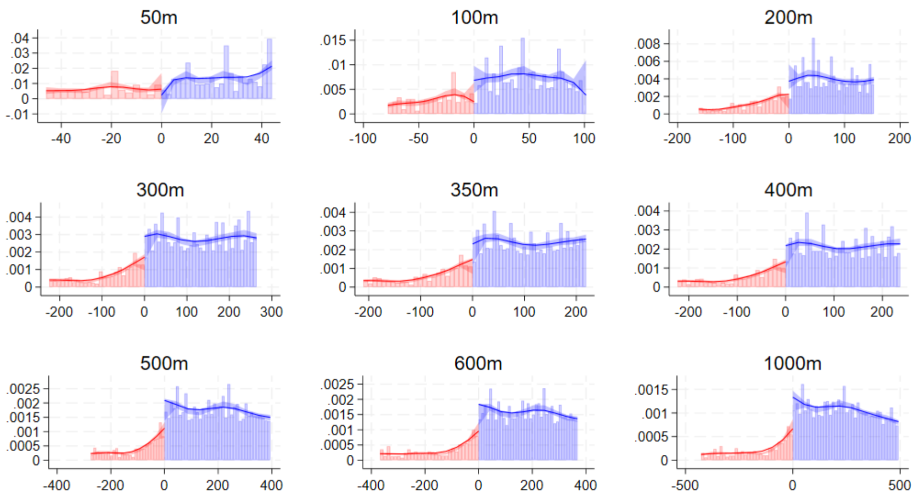
A.4.1 Basic RDD Checks

Figure 1: Treatment assignment around the flood boundary



Notes: Distance is normalised to zero at the flood boundary, with negative (positive) values corresponding to locations inside (outside) the flooded area. The probability of treatment exhibits a sharp jump at the cutoff, confirming the sharp nature of the regression discontinuity design.

Figure 2: McCrary Density Test at the Flood Boundary (Alternative Bandwidths)



Notes: McCrary (2008) density test assessing continuity of the running variable density at the flood boundary cutoff, estimated at alternative bandwidths from 50m to 1000m. The horizontal axis measures firm distance from the flood perimeter in metres (negative is flooded, positive is non-flooded). The vertical axis shows the estimated density of firms. Red and blue bars represent the empirical density histogram on each side of the cutoff. The solid line plots fitted values from a local linear regression of the density on distance, estimated separately on each side of the boundary, with the shaded area representing the 95% confidence interval. Across all bandwidth choices up to 600m, the density curves connect smoothly at the cutoff with overlapping confidence intervals, providing no significant visual evidence of systematic sorting of firms around the flood boundary. At 1000m, a mechanical asymmetry emerges between the two sides: as the bandwidth expands, the non-flooded sample grows disproportionately larger than the flooded one, which reflects the fact that the flood perimeter is a finite boundary on one side but open space on the other, causing the estimated density to diverge. This bandwidth is however never selected by the optimal data-driven procedure, precisely because this sample imbalance inflates the MSE on the non-flooded side.

A.4.2 Covariate Balance with Different Bandwidths

Table 27: Covariate Balance at the Flood Boundary (300m Bandwidth)

	All			Treated			Control			Difference	
Covariate	Mean	SD	No Firms	Mean	SD	No Firms	Mean	SD	No Firms	Diff.	SE
Ln(Lending Amount)	11.59	1.37	3,997	11.49	1.34	704	11.61	1.38	3,293	-0.12**	0.05
Share Stage 1 Loans	0.95	0.21	3,997	0.95	0.21	704	0.95	0.21	3,293	-0.00	0.01
Share Stage 2 Loans	0.01	0.07	3,997	0.00	0.06	704	0.01	0.08	3,293	-0.00	0.00
Number Loans	3.05	9.41	3,997	3.21	6.28	704	3.02	9.96	3,293	0.19	0.32
Provisions	2,929	7,112	3,997	2,632	6,740	704	2,993	7,189	3,293	-361	242
LTV	0.67	0.39	3,617	0.68	0.39	646	0.67	0.39	2,971	0.01	0.01
Annualised Interest Rate	0.04	0.03	3,997	0.04	0.03	704	0.04	0.03	3,293	-0.002**	0.001
Residual Maturity (months)	36.79	30.23	3,644	36.09	28.54	634	36.95	30.58	3,010	-0.86	1.10
Enterprise Size	3.44	0.83	3,744	3.43	0.86	666	3.44	0.82	3,078	-0.01	0.04
Number Employees	12.27	22.13	3,871	12.19	21.38	691	12.29	22.30	3,180	-0.10	0.93
Distance from Event Boundary	141.97	87.81	3,997	107.62	88.58	704	149.31	85.90	3,293	-41.69***	3.59
Flood Risk	0.13	0.34	3,997	0.11	0.32	704	0.13	0.34	3,293	-0.02	0.01
Number of Bank Relationships	1.48	0.95	3,997	1.49	1.00	704	1.47	0.94	3,293	0.02	0.04

Notes: This table presents covariate balance checks within a 300-meter window around the flood boundary. Reported statistics include group means and mean differences between treated and control units. All covariates are predetermined with respect to treatment assignment. The significant difference in distance from the boundary is mechanical due to the spatial geometry of flood polygons (treated firms located within small flooded areas are systematically closer to boundaries than control firms in the larger surrounding region). The modest pre-existing difference in interest rates is differenced out in our main specifications, which use changes in interest rates as the outcome variable. The lack of significant discontinuities in all other covariates supports the validity of the regression discontinuity design.

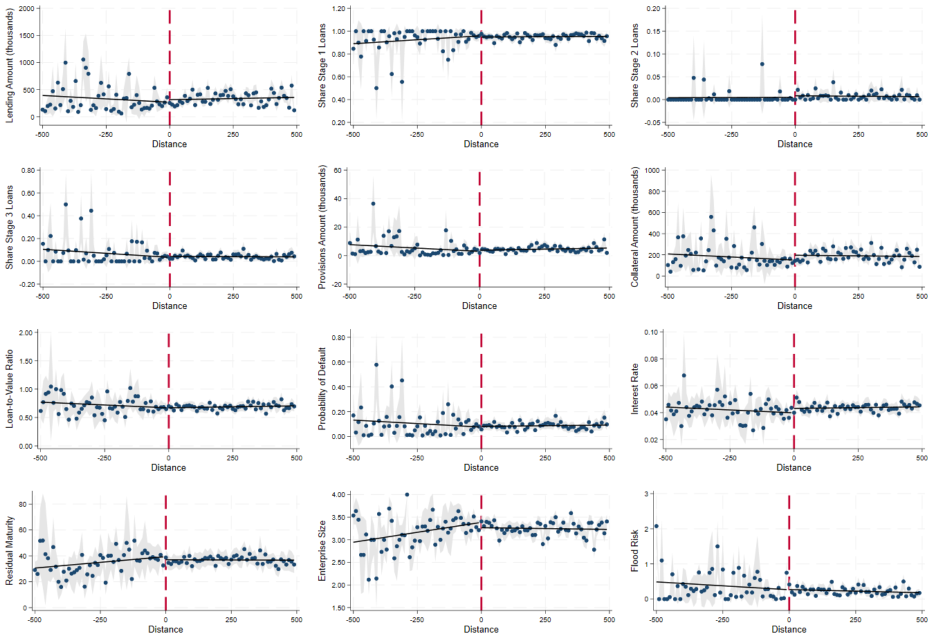
Table 28: Covariate Balance at the Flood Boundary (400m Bandwidth)

	All			Treated			Control			Difference	
Covariate	Mean	SD	No Firms	Mean	SD	No Firms	Mean	SD	No Firms	Diff.	SE
Ln(Lending Amount)	11.60	1.37	5,136	11.59	1.37	854	11.60	1.37	4,282	-0.01	0.04
Share Stage 1 Loans	0.95	0.21	5,136	0.94	0.23	854	0.95	0.21	4,282	-0.01*	0.01
Share Stage 2 Loans	0.01	0.08	5,136	0.00	0.06	854	0.01	0.08	4,282	-0.00	0.00
Number Loans	3.47	28.39	5,136	4.22	23.04	854	3.31	29.38	4,282	0.90	0.86
Provisions	2,921	7,122	5,136	3,198	7,565	854	2,863	7,026	4,282	334	216
LTV	0.67	0.39	4,651	0.68	0.40	779	0.67	0.39	3,872	0.00	0.01
Annualised Interest Rate	0.04	0.03	5,136	0.04	0.03	854	0.04	0.03	4,282	-0.002**	0.001
Residual Maturity (months)	36.84	30.41	4,702	34.20	28.09	775	37.39	30.85	3,927	-3.19***	0.98
Enterprise Size	3.44	0.83	4,816	3.40	0.88	812	3.44	0.82	4,004	-0.04	0.03
Number Employees	12.44	22.12	4,977	13.02	22.17	839	12.32	22.11	4,138	0.70	0.84
Distance from Event Boundary	187.82	116.53	5,136	150.63	123.58	854	195.24	113.64	4,282	-44.62***	4.32
Flood Risk	0.13	0.33	5,136	0.12	0.32	854	0.13	0.33	4,282	-0.01	0.01
Number of Bank Relationships	1.48	0.95	5,136	1.53	1.07	854	1.46	0.92	4,282	0.07*	0.04

Notes: This table presents covariate balance checks within a 400-meter window around the flood boundary. Reported statistics include group means and mean differences between treated and control units. All covariates are predetermined with respect to treatment assignment. The significant difference in distance from the boundary is mechanical due to the spatial geometry of flood polygons (treated firms located within small flooded areas are systematically closer to boundaries than control firms in the larger surrounding region). The modest pre-existing difference in interest rates is differenced out in our main specifications, which use changes in interest rates as the outcome variable. The lack of significant discontinuities in all other covariates supports the validity of the regression discontinuity design.

A.4.3 Continuity of Covariates

Figure 3: Continuity of covariates



Notes: The figure presents the test of continuity for covariates by Skovron and Titiunik (2015). The vertical axes presents the outcome variables: the lending amount; the share of loans in Stage 1, 2, 3; the provisions amount; the collateral amount; the loan-to-value ratio; the probability of default; the interest rate; the residual maturity; the enterprise size; the flood risk. The horizontal axis measures the distance from the flood boundary. The black line plots the fitted values of the dependent variable on a first-order polynomial in the distance from the flood. The fitted values are estimated separately on each side of the cutoff. The light grey area represents the 95 percent confidence interval. The covariates just above and below the cutoff are not statistically different across treated and untreated firms. This corroborates that firms do not manipulate the distance from the flood boundary. The lending, provisions, and collateral amounts are winsorized at the 5th and 95th percentiles.

A.4.4 Alternative Bandwidths on Main Results

Table 29: Delta Lending Amount - Bandwidth Robustness Across Alternative Bandwidths

	Qrt 1, No FE Left Bandwidth					**Qrt 2, No FE** Left Bandwidth				
Right Band	200	300	400	500	600	200	300	400	500	600
200	0.039	0.046	0.052**	0.052**	0.052**	-0.043*	-0.047***	-0.053***	-0.054***	-0.053***
300	0.028	0.036	0.042**	0.041**	0.041**	-0.043*	-0.047***	-0.053***	-0.054***	-0.053***
400	0.024	0.032	0.037**	0.037**	0.037**	-0.040*	-0.044***	-0.050***	-0.050***	-0.050***
500	0.023	0.031	0.036*	0.036**	0.036**	-0.037*	-0.041***	-0.047***	-0.047***	-0.047***
600	0.024	0.031	0.037*	0.037**	0.037**	-0.034	-0.037**	-0.043***	-0.044***	-0.043***
	Qrt 1, Event FE Left Bandwidth					**Qrt 2, Event FE** Left Bandwidth				
Right Band	200	300	400	500	600	200	300	400	500	600
200	0.038	0.047	0.052**	0.052**	0.052**	-0.032*	-0.036**	-0.040***	-0.039***	-0.038***
300	0.027	0.036	0.041**	0.041**	0.041**	-0.033*	-0.037***	-0.041***	-0.040***	-0.039***
400	0.023	0.031	0.036*	0.036**	0.036**	-0.029*	-0.034***	-0.038***	-0.037***	-0.036***
500	0.021	0.030	0.035*	0.035*	0.035*	-0.026	-0.031**	-0.035***	-0.034***	-0.033***
600	0.022	0.030	0.035*	0.035*	0.035*	-0.023	-0.027**	-0.032***	-0.030***	-0.029**
	Qrt 1, Area FE Left Bandwidth					**Qrt 2, Area FE** Left Bandwidth				
Right Band	200	300	400	500	600	200	300	400	500	600
200	0.060	0.059	0.062*	0.062*	0.062**	-0.054**	-0.054***	-0.057***	-0.053***	-0.051***
300	0.052	0.050	0.054*	0.055**	0.055**	-0.052**	-0.054***	-0.057***	-0.054***	-0.053***
400	0.048	0.047	0.050*	0.051**	0.052**	-0.046**	-0.049***	-0.052***	-0.050***	-0.049***
500	0.045	0.045	0.048*	0.049**	0.051**	-0.040*	-0.044***	-0.048***	-0.045***	-0.044***
600	0.044	0.044	0.047*	0.048**	0.050**	-0.037	-0.041**	-0.045***	-0.042***	-0.042***

Notes: Significance levels: * p<0.10, ** p<0.05, *** p<0.01

Table 30: Delta Interest Rate - Bandwidth Robustness Across Alternative Bandwidths

	Qrt 1, No FE Left Bandwidth					**Qrt 2, No FE** Left Bandwidth				
Right Band	200	300	400	500	600	200	300	400	500	600
200	0.152	0.153	0.150	0.150	0.145	0.062	0.047	0.074*	0.091**	0.099**
300	0.139	0.139	0.137	0.138	0.133	0.035	0.020	0.047*	0.064***	0.072***
400	0.134	0.134	0.132	0.132	0.128	0.030	0.015	0.042	0.059**	0.068***
500	0.139	0.139	0.137	0.137	0.133	0.034	0.019	0.046	0.063**	0.071***
600	0.135	0.135	0.133	0.133	0.129	0.038	0.023	0.050	0.066**	0.075***
	Qrt 1, Event FE Left Bandwidth					**Qrt 2, Event FE** Left Bandwidth				
Right Band	200	300	400	500	600	200	300	400	500	600
200	0.102	0.109	0.093	0.084	0.074	-0.056*	-0.058**	-0.048*	-0.043*	-0.040*
300	0.092	0.099	0.083	0.074	0.064	-0.063*	-0.065**	-0.055*	-0.050	-0.047
400	0.085	0.092	0.075	0.066	0.057	-0.064	-0.066*	-0.055	-0.051	-0.048
500	0.085	0.091	0.075	0.066	0.056	-0.061	-0.063*	-0.052	-0.048	-0.045
600	0.081	0.088	0.072	0.063	0.053	-0.056	-0.058*	-0.048	-0.043	-0.040
	Qrt 1, Area FE Left Bandwidth					**Qrt 2, Area FE** Left Bandwidth				
Right Band	200	300	400	500	600	200	300	400	500	600
200	0.122	0.132	0.118	0.114	0.107	-0.132**	-0.132***	-0.114***	-0.103***	-0.093***
300	0.108	0.118	0.102	0.098	0.091	-0.142**	-0.142***	-0.126***	-0.115**	-0.105**
400	0.098	0.108	0.091	0.087	0.081	-0.140**	-0.140***	-0.125**	-0.115**	-0.106**
500	0.097	0.106	0.089	0.084	0.078	-0.132**	-0.131***	-0.117**	-0.108**	-0.099**
600	0.090	0.099	0.082	0.077	0.070	-0.124**	-0.123***	-0.109**	-0.101**	-0.092**

Notes: Significance levels: * p<0.10, ** p<0.05, *** p<0.01

Table 31: Default Ratio - Bandwidth Robustness Across Alternative Bandwidths

	Qrt 1, No FE Left Bandwidth					**Qrts [1,2], No FE** Left Bandwidth				
Right Band	200	300	400	500	600	200	300	400	500	600
200	0.303	0.484*	0.589*	0.628*	0.625*	0.429	0.663**	0.758**	0.800**	0.802**
300	0.293	0.474*	0.579*	0.618*	0.614*	0.390	0.624**	0.719**	0.761**	0.763*
400	0.283	0.464*	0.569*	0.608*	0.605*	0.365	0.599*	0.694**	0.736*	0.738*
500	0.244	0.425*	0.530*	0.569*	0.565	0.313	0.547*	0.642*	0.684*	0.686*
600	0.201	0.382	0.486	0.526	0.522	0.264	0.498*	0.593*	0.635*	0.637*
	Qrt 1, Event FE Left Bandwidth					**Qrts [1,2], Event FE** Left Bandwidth				
Right Band	200	300	400	500	600	200	300	400	500	600
200	0.349	0.532*	0.649*	0.699*	0.702*	0.459*	0.698**	0.806**	0.858**	0.866**
300	0.331	0.511*	0.629*	0.678*	0.681*	0.404	0.641**	0.747**	0.798**	0.805**
400	0.325	0.504*	0.623*	0.673*	0.676*	0.381	0.617*	0.722*	0.774*	0.781*
500	0.290	0.468*	0.588*	0.639*	0.642*	0.333	0.567*	0.674*	0.725*	0.733*
600	0.245	0.423*	0.542*	0.594*	0.597*	0.283	0.517*	0.623*	0.675*	0.683*
	Qrt 1, Area FE Left Bandwidth					**Qrts [1,2], Area FE** Left Bandwidth				
Right Band	200	300	400	500	600	200	300	400	500	600
200	0.302	0.573	0.698*	0.711*	0.705*	0.490*	0.751**	0.847**	0.864**	0.857*
300	0.227	0.443	0.569*	0.586*	0.584	0.405	0.611**	0.707**	0.731**	0.732**
400	0.227	0.400	0.514*	0.533*	0.534	0.382	0.551**	0.637**	0.664**	0.671**
500	0.194	0.346	0.454	0.476	0.481	0.314	0.470*	0.551*	0.582*	0.593*
600	0.157	0.294	0.399	0.424	0.431	0.264	0.411*	0.490*	0.522*	0.536*

Notes: Significance levels: * p<0.10, ** p<0.05, *** p<0.01

A.4.5 Alternative Standard Errors Clusters on Main Result

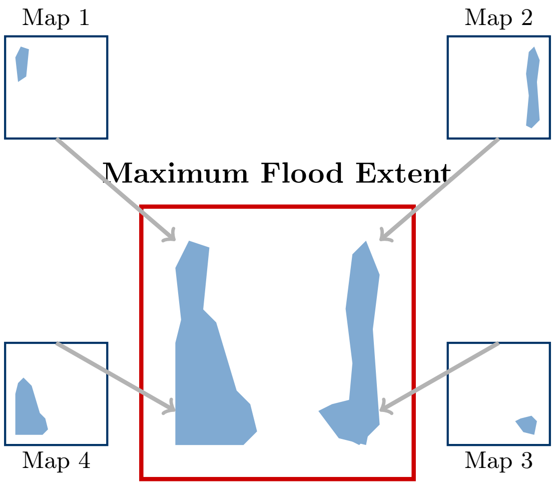
Table 32: Robustness: Clustering Specifications for Lending Volumes

	NUTS4		**Treated**		**NUTS4 × Treated**	
	Q1	Q2	Q1	Q2	Q1	Q2
	Dependent variable: Δ Lending Amount					
Treated	0.050***	-0.052*	0.050*	-0.052*	0.050***	-0.052**
	(0.019)	(0.029)	(0.004)	(0.005)	(0.019)	(0.024)
Distance	0.000	-0.000**	0.000*	-0.000*	0.000	-0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Treated × Distance	0.000	-0.000	0.000	-0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	6761	6191	6761	6191	6761	6191
Bandwidth	[-439, 439]	[-400, 400]	[-439, 439]	[-400, 400]	[-439, 439]	[-400, 400]
Fixed Effects	Area	Area	Area	Area	Area	Area
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of Firms	4783	4377	4783	4377	4783	4377
Number of Banks	44	46	44	46	44	46
Number of FE Groups	196	179	196	179	196	179

Notes: This table reports regression discontinuity estimates of the effect of flood exposure on lending using alternative clustering specifications. The dependent variable is the change in log lending at the bank–firm level. The running variable is the distance to the flood boundary, normalised to zero at the cutoff. Treated is an indicator equal to one for observations located on the flooded side of the boundary. Controls include residual maturity. All specifications use local linear regressions with a triangular kernel and optimal bandwidth selection following Calonico et al. (2014). Bandwidths are reported in brackets. Area fixed effects (postal code level) are included in all specifications. Standard errors are clustered at: NUTS4 level (columns 1-2), treatment status (columns 3-4, yields only 2 clusters), or NUTS4×Treated interaction (columns 5-6). The baseline specification with bank-level clustering is reported in Tables 2 and 3. The unit of observation is a bank–firm–quarter.
*, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

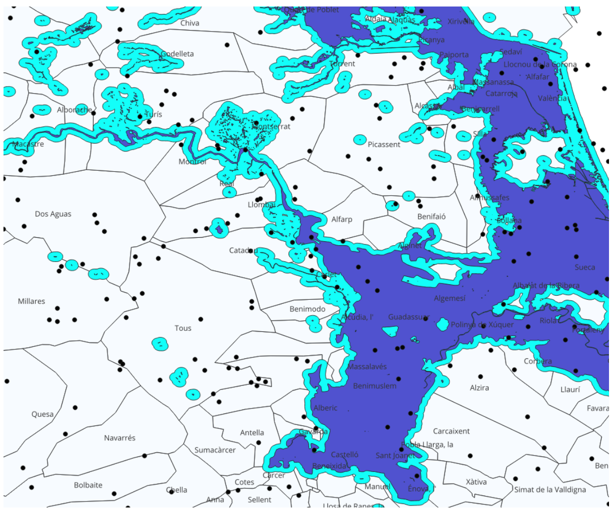
A.5 Sample Construction

Figure 4: Representation of Flood Map Overlay



Notes: This figure illustrates the methodological approach used to define the treatment area. Because individual satellite activation maps (Maps 1–4) may capture different temporal or spatial snapshots of the event, we perform a spatial union of all available data. The resulting "Maximum Flood Extent" (center) serves as the definitive boundary for the flood zone.)

Figure 5: Excerpt from Valencia Flood Mapping and RDD Sample Selection



Notes: This figure provides a representative snippet of the Valencia flood extent and firm sample we identify. The dark blue areas represent the maximum flood extent. The light blue shaded perimeter represents the spatial bandwidth surrounding the flood boundary, which defines the local sample for the RDD analysis. The black dots are randomised and indicate the precise geolocated positions of firms in the region.

A.6 Description of Main Variables

Table 33: Variable Definitions

Variable	Description
Lending Outcomes	
Lending Amount	Total outstanding nominal amount across all loans in the bank-firm relationship, in euros.
Number Loans	Total number of outstanding loans in the bank-firm relationship.
Interest Rate	Annualised agreed rate, expressed in percentage points, following Regulation (EU) No 1072/2013.
Interest Rate Spread	Margin over the reference rate used for interest rate calculation, in basis points.
Provisions	Total accumulated impairments and accumulated changes in fair value due to credit risk, in euros.
Credit Risk Indicators	
Share Stage 1 Loans	Share of outstanding loan amount classified as Stage 1 under IFRS 9 (performing loans with no significant increase in credit risk).
Share Stage 2 Loans	Share of outstanding loan amount classified as Stage 2 under IFRS 9 (performing loans with significant increase in credit risk).
Share Stage 3 Loans	Share of outstanding loan amount classified as Stage 3 under IFRS 9 (non-performing loans).
Default Ratio	Share of defaulted lending divided by total lending in the quarter (loans that defaulted in period t over total loans in period t).
Collateral and Loan Terms	
Collateral Amount	Total amount of collateral and credit protection securing the instrument, in euros.
LTV	Loan-to-value ratio, calculated as total exposure divided by total real estate collateral value.
Residual Maturity	Remaining time until loan maturity, in months.
Interest Rate Reset Date	Number of months until the next scheduled interest rate reset.
Firm Characteristics	
Enterprise Size	Categorical classification of firm size (micro, small, medium, large) according to Commission Recommendation 2003/361/EC.
Number Employees	Number of employees working for the firm, as defined in Article 5 of the Annex to Commission Recommendation 2003/361/EC.
Number of Bank Relationships	Total number of distinct banks with which the firm has at least one outstanding loan.
Treatment and Exposure Variables	
Distance from Event Boundary	Shortest distance between the firm's registered address and the flood boundary, in meters (negative if inside the flooded area, positive if outside).
Treated	Indicator variable equal to one if the firm is located within the flood boundary at the time of the event, zero otherwise.
Flood Risk	Ex-ante flood hazard exposure indicator based on the firm's location, using the RCP 4.5 climate scenario.
Relationship Variables	
Share Affected Lending	Share of the bank's total outstanding loan amount extended to firms located within flood boundaries.
Main Bank	Indicator variable equal to one if the bank is the firm's primary lender (largest outstanding loan amount), zero otherwise.

Notes: All variables are measured at the bank-firm-quarter level unless otherwise specified. Lending amounts and collateral are expressed in nominal euros. Stage classifications follow IFRS 9 accounting standards: Stage 1 comprises performing loans with no significant increase in credit risk since origination; Stage 2 comprises performing loans with significant increase in credit risk; Stage 3 comprises non-performing loans (credit-impaired). The treatment variable and distance measure are time-invariant for each firm-event combination. Bank-level variables are calculated using the bank's entire lending portfolio in the quarter before the flood.

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