

# **Do Banks Overreact to Disaster Risk?\***

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## **Abstract**

We examine how banks respond to severe natural disasters when corporate borrowers are located in the neighborhood of the disaster area. We find robust evidence that banks charge significantly higher loan spreads for firms located in these areas following a disaster than for other firms. The observed effect is unlikely to be driven by regional spillovers, lender rent extraction, limited credit supply, or rational learning. We show that bank lenders also respond to severe disasters that occur far away if the borrowing firm is vulnerable to a similar type of disaster risk. Furthermore, banks' reaction is transitory, and is less pronounced when natural disasters nearby occur repeatedly. Overall, our empirical findings indicate that banks are subject to salience bias when assessing their clients' natural disaster risk.

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## 1. Introduction

As the number of natural disasters, and particularly weather-related disasters, increase significantly in the past several decades, climate change and its impact has become the center of debate among policy makers in recent years (Stroebe and Wurgler, 2021).<sup>1</sup> In addition to understanding how climate change affects the overall economy (Dell, Jones, and Olken, 2014; Hsiang et al., 2017), there is a growing interest in understanding its impact on the capital market, especially security and real estate prices. Some studies find that security prices accurately reflect long-run climate risks (e.g., Giglio, Maggiori, and Stroebe, 2015; Ouazad and Kahn, 2019; Murfin and Spiegel, 2020; Baldauf, Garlappi, and Yannelis, 2020).

However, using salient natural disasters, several recent papers also find that corporate managers and professional fund managers respond irrationally to climate risk in the short term. This is because salient natural events may affect individuals' risk perception, and accordingly their decision-making (Bordalo, Gennaioli, and Shleifer, 2012). For example, Dessaint and Matray (2017) find that after observing neighboring disasters, managers increase corporate cash holdings and express more concerns about disaster risk in their annual reports, and Alok, Kumar, and Wermers (2020) find that mutual fund managers within a major disaster region underweight disaster zone stocks to a much greater degree than distant managers.

In this paper, we seek to understand whether bank lenders—an important stakeholder of a firm—can correctly understand the implications of natural disaster risks. It is important to understand whether bank lenders can estimate the disaster impact on firms correctly.

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<sup>1</sup> For example, in the World Economic Forum (2020)'s Global Risk Report, extreme weather, climate action failure, and natural disasters are listed as the top three risks that are most likely to materialize.  
<https://www.weforum.org/reports/the-global-risks-report-2020>

Compared with corporate and fund managers, banks are more exposed to downside risks any may be more responsive to events weakening firm creditworthiness (e.g., natural disasters) (Gorton and Kahn, 2000). If bankers are subject to salience bias as corporate managers and mutual fund managers, it may lead to inefficient credit allocation and impose significant financing costs on firms. To answer this question, we follow Dessaint and Matray (2017) and study how banks respond to major disaster events when their borrowers are located in the neighborhood of the disaster area. We hypothesize that when banks have lending to firms close to the disaster area, they are more susceptible to salience bias and thus more likely to overestimate the risk of salient disasters. By focusing on borrowers that could have been affected by a disaster event but were not because of chance, we are able to assess whether bank lenders can gauge these borrowers' disaster risk correctly.

Using bank loan data from the DealScan during the period of 1987–2017, we find that about 13% of the corporate loans are issued to borrowers located in the neighborhood of a region hit by a natural disaster in the previous two years, and that the average loan spreads for these borrowers is 9.7 basis points higher than those paid by remote firms. The results are also similar if we conduct analysis based on an entropy-balanced sample. This is consistent with our hypothesis that a sudden shock to the perceived disaster risk leads lenders to increase the loan spreads for borrowers located in the neighborhood of the disaster area. It is important to note that the disasters hit a region by random, it does not materially affect the likelihood of its neighborhood region to be hit by the disaster in subsequent years (Dessaint and Matray, 2017).

To provide further evidence, we study the dynamic effects of the salient disaster events on neighboring firms' borrowing cost. Because a salient event loses importance as time passes (Hirshleifer, 2001), we expect to see the effect to diminish as time passes. Consistent with the

salient explanation, we find that the effect concentrates on two years after the disaster. We also find that there is no significant effect on loan spreads before the borrowing firm's neighborhood experiences a disaster, suggesting that the parallel trend assumption is not violated.

We consider a number of alternative interpretations for our findings. One of the most possible alternative stories is that the effect is driven by regional spillover. It is for this reason we have excluded borrowers that are major suppliers or customers of disaster-affected firms.<sup>2</sup> However, it is reasonable to argue that neighboring firms in our sample may have indirect exposure to natural disasters. We conduct multiple tests to further ensure that regional spillover is unlikely to drive our results. First, we find no evidence that these borrowers' fundamentals are adversely affected by the disasters relative to remote borrowers. This finding echos Dessaint and Matray (2017), who report that the direct exposure to natural disasters is relatively low for neighboring firms. Second, we further remove neighboring firms that are presumably more likely to be subject to the spillover effects, such as firms that have significant operations in disaster-affected counties or firms with high stock return co-movement with disaster-affected firms. Our results continue to hold.

The second alternative interpretation is that our results are driven by limited credit supply of banks. This is because, after a severe natural disaster, the lending capacity of banks located closer to the disaster areas might be adversely affected. However, we don't observe significant changes in bank's total commercial lending or deposits in disaster-neighboring counties after a major disaster. In addition, we show that our findings are robust to controlling for various bank characteristics as well as bank-year fixed effects, which presumably absorb

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<sup>2</sup> We identify suppliers and customers using information from the Compustat Segments data and remove loans issued to these supplier/customer firms within two years after the strike of a disaster.

time-variant bank level factors. The results continue to hold even after we remove loans issued by banks that have significant lending to regions directly affected by natural disasters.

The third alternative interpretation is that the effect is a result of lender rent extraction. Rent extraction motive suggests that, when borrowers overestimate the business risk, lenders extract some rent by charging higher spreads, even though bank lenders understand that a firm's objective business risk remains unchanged. We find that our results are not affected after controlling for corporate managers' expressed concern about natural disaster risk or climate change risk in their SEC filings and conference calls. Therefore, our findings are not a result of lender rent extraction.

Finally, we find that rational learning is unlikely to be the driving force behind our results. As mentioned earlier, the documented positive impact of salient disasters on loan spreads is transitory. We further find that the impact on loan spreads becomes smaller as similar disaster events occur repeatedly in the neighborhood. This is consistent with the psychology literature that the effect of stimuli on subjects decays with more experiences (e.g., Tobias, 2009).

To further alleviate the concern that some other economic channels are driving our results, we examine US firms located far away from the disaster area but have been hit by a similar disaster in the past. We find that bank lenders also respond to severe disasters that occur hundreds of miles or further away if the focal firm is vulnerable to similar disaster risk. These results lend further support to our claim that alternative explanations, such as regional spillovers or limited credit supply, are unlikely to be the main drivers of our primary findings.

We also conduct several robustness tests to ensure that our results are not sensitive to the way we construct the sample, the clustering strategy, or alternative model specifications.

Finally, we find that, in addition to loan spreads, bank lenders also adjust non-price terms for neighboring firms. Following a disaster, lenders impose more covenant restrictions and are more likely to require collateral when extending loans to firms located in the neighborhood of a disaster zone. Taken together, our empirical evidence suggests that bank lenders also experience a salience bias when assessing their clients' disaster risk.

Our paper contributes to the literature in a number of dimensions. First, our paper complements the literature that documents professionals' salience bias induced by natural disasters (e.g., Dessaint and Matray, 2017; Alok et al., 2020). We provide evidence that sophisticated lenders are also subject to salience bias. We show that bank lenders are likely to overweight more recent and vivid information when forming beliefs about borrowers' disaster risk. As a result, after severe natural disasters, lenders charge significantly higher rates on loans to firms located in the neighborhood of the disaster area.

Second, our paper adds to a growing literature that studies how experiences or behavioral biases affect lenders' decisions. For example, Koudijs and Voth (2016) show that lenders' bankruptcy experience may have adverse effects on their subsequent risk-taking behavior. Campbell, Loumioti, and Moerman (2019) show that the cognitive constraints of loan officers can negatively affect the quality of loans they grant. Garvalho, Gao, Ma (2021) find that the experience of housing market boom could bias loan officers' assessment of the risk of their borrowers. Our approach differs from prior studies in two important ways. First, Koudijs and Voth (2016) study financial experiences, and Garvalho, Gao, and Ma (2021) focus on economic experiences, but we study climate experiences. Second, we uncover a new bias that could affects lending decisions.<sup>3</sup>

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<sup>3</sup> Campbell, Loumioti, and Moerman (2019) document three types of behavioral biases in lending decisions, including limited attention, task-specific human capital, and common identity.

Third, our paper is related to the literature on how lenders respond to climate risk that mainly stems from hazardous chemicals and gas emissions (Chava, 2014; Ivanov, Kruttli, and Watugala, 2020), and climate regulatory risk (Seltzer, Starks, and Zhu, 2020). These papers find that lenders pay attention to climate (regulatory) risk when borrowers are directly subject to such risk. In addition to study a different type of climate risk, natural disaster risk, we focus on firms located in the neighborhood of the disaster area, and find that bank lenders overreact natural disaster risk in the short term.

The rest of the paper is organized as follows. Section 2 describes our data and sample construction. Section 3 discusses the empirical strategy. Section 4 presents our empirical results. Section 5 concludes.

## **2. Data and Empirical Methodology**

### **2.1. Data**

To construct our sample, we start with all corporate syndicated loans issued to U.S. firms that have financial information available from Compustat during the period of 1987–2017. Data on corporate loans are collected from the DealScan database maintained by the Loan Pricing Corporation (LPC). LPC provides data on loan facilities collected either from Securities and Exchange Commission (SEC) filings or from disclosures made by borrowers or lenders. It contains information about loan pricing and various loan terms at the origination, such as loan size, use of collateral, and covenants. We match the loan data with Compustat firms using the most updated DealScan–Compustat link file maintained by Michael Roberts and Wharton Research Data Services (Chava and Roberts, 2008). Loans with missing information on the key terms, such as the loan spread, lender identity, and loan amount, are

excluded from the sample. We also remove loans issued to financial firms (SIC 6000–6999) and regulated utilities (SIC 4900–4949).

We obtain major natural disasters from the Spatial Hazard Events and Losses Database for the United States (SHELDUS) at Arizona State University. For each event, SHELDUS provides information on the event dates, disaster types, property damages, fatalities, and affected-county locations of major climatic disasters in the US. To ensure that an event is salient enough, we focus on disasters with total estimated damages above 1 billion 2017 dollars and with duration less than 30 days (Barrot and Sauvagnat, 2016). This filtering procedure leaves us with 40 major disasters during 1987-2017. Table 1 reports summary statistics for these disasters. As we can see, these events are not clustered but rather dispersed over our sample period.

We follow the literature to determine whether a borrowing firm is affected by a disaster event based on its headquarters location (Chaney, Sraer, and Thesmar, 2012; Dessaint and Matray 2017). We obtain information on the historical locations of each firm’s headquarters during our sample period from the 10-X Header Database constructed by Bill McDonald.<sup>4</sup> For firms without electronic filings before 1994, we set the headquarters location equal to the earliest available location.

## **2.2. Empirical methodology**

We rely on both the occurrence of disasters and the geographical proximity of the borrower to the disaster area to identify situations in which bank’s perception of the borrowing firm’s natural disaster risk may increase significantly. In particular, we divide our borrowing

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<sup>4</sup> The data is available at <https://sraf.nd.edu/data/augmented-10-x-header-data/>. We thank Bill McDonald for making the data publicly available.



firms into three different groups based on the distance between a firm's headquarters location (i.e., at the county level) and the disaster area: disaster-area firms, disaster-neighboring firms, and remote firms. Disaster-area firms are defined as firms located in counties directly affected by a disaster event, as identified by SHELUDS. For each disaster event, we then identify the neighboring counties of each disaster affected county. Specifically, we collect information on county adjacency from the National Bureau of Economic Research and match each affected county with its five closest non-affected counties.<sup>5</sup> Firms located in these neighboring counties are defined as disaster-neighboring firms.<sup>6</sup> The remaining firms are classified as remote firms.

As argued by Dessaint and Matray (2017), both neighboring and remote firms in our sample are not directly affected by the natural disaster. However, from the perspective of bank lenders, the perceived risk of being struck is likely to be higher for firms in the neighborhood of the disaster area. Therefore, we assess the potential impact of salience bias by comparing bank loans granted to neighboring firms following the disaster with loans to remote firms. Though lenders' perception of risk in disaster affected area is likely to change after the event, loans to firms in disaster areas are removed from our main analysis. This is because these firms' fundamentals are likely to be affected by the disaster and it would be difficult for us to isolate the impact of salience bias on the cost of borrowing. Barro and Sauvagnat (2016) show that the impact of natural disasters on affected firms can last for more than one year (i.e., five quarters). Hence, if a loan is issued to a disaster-zone firm within two years following the disaster, it is removed from our analysis. In addition, Barrot and Sauvagnat (2016) find that the negative

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<sup>5</sup> Data on county distance is obtained from <https://www.nber.org/research/data/county-distance-database>. On average, a county is adjacent to other five counties. Our results are similar if we use four, six, or eight nearest non-affected counties to classify disaster-neighboring areas, or define neighboring counties as those within 60 miles from the disaster area. Over our sample period, 1,913 counties are directly hit by at least one salient disaster and 2,209 counties are neighboring to the disaster area. The average distance from a neighboring county to the disaster area is 465 miles and the average distance to the closest disaster-affected county is 29 miles.

<sup>6</sup> For short, we call these firms "neighboring firms" throughout the paper.

impact of natural disasters propagates through supply chain. To remove this potential spillover effect, for each disaster-affected firm, we identify its suppliers and customers using information from the Compustat Segments data and remove loans issued to these supplier/customer firms within two years after the strike of a disaster.<sup>7</sup>

To examine whether bank lenders respond differently to neighboring firms following a disaster, we estimate the following regression model:

$$\text{Log}(\text{Spread})_{f,i,b,t} = \alpha + \beta \text{Neighbor}_{c,t} + \gamma \text{Controls}_{f,i,t-1} + \delta_i + \mu_b + \vartheta_t + \varepsilon_{f,i,b,c,t}$$

The observation is at the loan-bank level because most syndicate loans have multiple lead arrangers.<sup>8</sup> This also allows us to capture the heterogeneity across different bank lenders.<sup>9</sup> In this specification,  $f$  denotes the loan facility,  $i$  denotes the borrowing firm,  $b$  denotes the bank,  $t$  denotes the loan initiation year, and  $c$  denotes the county in which the borrowing firm's headquarters is located. The dependent variable is  $\text{Log}(\text{Spread})_{f,i,b,t}$ , the natural logarithm of the all-in-drawn spread (in basis points) for loan facility  $f$  of firm  $i$  issued by bank  $b$  in year  $t$ . The explanatory variable of interest is  $\text{Neighbor}$ , an indicator variable that equals one if the borrowing firm's headquarters is located in the neighborhood of a county hit by a natural disaster over the past 24 months and zero otherwise.<sup>10</sup> As mentioned by Rhodes et al. (2004)

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<sup>7</sup> U.S. public firms are required to disclose customers that account for 10% or more of firms' total annual sales, from which we generate firm level supplier-customer pairs.

<sup>8</sup> We follow Sufi (2007) and identify lead banks within a loan syndication if the value of the variable "*LeadArrangerCredit*" in DealScan equals "Yes". Alternatively, following Bharath et al. (2011) and Ivashina (2009), we look at the information on lender roles and identify lead lenders as those marked as "*Lead arranger*", "*Arranger*", "*Agent*", "*Admin agent*", and "*Lead bank*". The results are all similar.

<sup>9</sup> We also conduct an analysis at the facility level. Results are reported in Table 9 column (4).

<sup>10</sup> We choose 24-month because prior literature suggests that salience bias arising from disastrous events usually disappears after two years (e.g., Dessaint and Matray, 2017; Alok et al., 2020). Our dynamic trend analysis also confirms this pattern.

and Murfin (2012), an average loan is negotiated two to three months in advance of the legal effective date of the loan. To account for this time lag, we lag the loan starting date in DealScan by three months.<sup>11</sup>

We include a number of firm characteristics that might affect loan spreads, including firm size (natural logarithm of total assets), cash holding, market-to-book ratio, financial leverage, ROA, asset tangibility, Z-score, and S&P credit rating (e.g., Graham et al., 2008; Lin et al., 2011; Jiang et al., 2018). All these firm characteristics are measured at the fiscal year prior to the loan initiation. To control for unobservable time-invariant heterogeneity across firms, we further include firm fixed effect ( $\delta_i$ ) in the regression. In addition, we control for various loan characteristics, including loan size, loan maturity, loan types (i.e., term loans, revolvers longer than one year, revolvers shorter than one year, and 364-day loans), loan purposes (i.e., general corporate purpose, refinancing, acquisition, backup line for commercial paper, and others), a dummy variable indicating whether the loan includes a contingent performance-based pricing clause, and an indicator for whether the facility is secured. As different banks may have different ability assessing a borrowing firm's risk, we also include bank fixed effect ( $\mu_b$ ) to control for time-invariant heterogeneity across banks. Finally, year fixed effects ( $\vartheta_t$ ) are included to control for time trend.

### 2.3. Summary statistics

After requiring non-missing values of main control variables, we obtain a sample of 28,963 loans during 1987-2017, issued by 1,205 lenders to 3,955 unique public firms. Table 2

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<sup>11</sup> The loan contract date typically takes place about three months prior to the contract effective date. Practitioner estimates suggest that the average syndicated transaction takes two months, between the date the borrower awards the lead bank a mandate (a contract to act as the lead arranger) and the date the loan is effective (Rhodes, 2004). In addition, it may take as long as a month between the time a bank approves a term sheet and receives a mandate.

reports the summary statistics for various firm and loan characteristics. Detailed definitions of all variables are provided in Appendix A. All dollar values are adjusted to 2017 using the Consumer Price Index data from the Bureau of Labor Statistics. To mitigate the influence of outliers, all continuous variables are winsorized at the 1st and 99th percentiles. The average loan spread in our sample is about 216 basis points, and 13% of loans are issued to borrowers located in the neighborhood of a county hit by a natural disaster over the past 24 months. An average borrowing firm has total assets of 5.6 billion, cash ratio of 0.09, market-to-book of 1.79, book leverage of 0.33, ROA of 0.13, PPE-to-assets ratio (*Tangibility*) of 0.31, and Z-score of 3.15. The average loan has size of around 597.9 million, and maturity of 52 months. 39% of these loans use performance pricing and 56% of them are secured loans.

### 3. Empirical Results

#### 3.1. Baseline results

Table 3 Panel A presents our baseline results. All t-statistics reported in the parenthesis are based on standard errors clustered at both county and year levels.<sup>12</sup> In column (1), we control for those above-mentioned firm characteristics. In column (2), we further control for various loan characteristics. In both specifications, we first find that the estimated coefficients for control variables are largely consistent with prior literature. For instance, larger firms, firms with higher market-to-book ratio, lower financial leverage, higher profitability (ROA), and higher credit rating are associated with lower cost of bank loans. More importantly, we find that the coefficient of *Neighbor* is positive and statistically significant at 1% level, suggesting

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<sup>12</sup> Our results are robust if we cluster the standard errors at county or firm level.

that within the two years after a natural disaster, banks charge higher loan spreads to firms located in the neighborhood of the disaster area than to other firms.

The effect on loan spreads is also economically meaningful. Estimates from column (2) indicate that if a firm's headquarters is located in the neighborhood of a region hit by a natural disaster in the prior two years, its loan spreads is 4.5% higher than those paid by other firms. Given the average loan spreads of 216.24 basis points, this corresponds to a 9.7 basis points higher interest rate paid by neighboring firms within 24 months after a disaster. The economic magnitude is comparable to that of Market-to-book (a one-standard-deviation reduction in Market-to-book increases the cost of bank loans by 11.7 bps), but smaller than that of ROA (19.1 bps) and Leverage (23.9 bps).<sup>13</sup>

In column (3), we replace year fixed effects with state-by-year fixed effects to control for time-varying state specific factors. This test allows us to compare loans to firms from the same state and year but with different distances to disaster zones. Our results remain the same, suggesting that our findings are not driven by unobserved time-variant state factors.<sup>14</sup>

Our results are consistent with the notion that bank lenders respond differently to neighboring firms after a disaster. To mitigate the concern that our results are driven by fundamental differences between neighboring firms after the disaster and remaining firms, we repeat our baseline analysis using an entropy-balanced sample. Recent studies have argued that the entropy-balancing technique has certain advantages over the propensity-score-matching method (e.g., Chapman, Miller, and White, 2019). This technique assigns weights to control

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<sup>13</sup> The magnitude of the effect is also comparable to effects documented by recent studies on bank loans. For example, Bharath, Sunder, and Sunder (2008), Hasan, Hoi, Wu, and Zhang (2014, 2017) document that a one-standard-deviation decrease in accounting quality, cash effective tax rate, and social capital in their respective samples is associated with an increase in bank loan spread by 6.7, 4.9, and 4.3 bps, respectively.

<sup>14</sup> The number of observations in this test becomes smaller as some singletons are dropped in the high-dimensional fixed effects analysis (Correia, 2015).

observations so that the distributional properties of the treatment group and the post-weighting control group are virtually identical.

Specifically, via a maximum-entropy reweighting scheme (see, e.g., Hainmueller and Xu (2013)), we perform entropy balancing on the first three moments (i.e., mean, variance, and skewness) of all firm covariates to ensure that the distributions of all included control variables are similar for neighboring firms and other firms. Table 3 Panel B presents the results, where coefficients on control variables are omitted for brevity. The results are similar to those in Panel A, suggesting that, following a natural disaster, bank lenders charge higher interest rates on loans to firms located in the neighborhood of a disaster region.

### **3.2. Dynamic trend**

To better establish causality, we next study the dynamic impact of a salient natural disaster on neighboring firms' borrowing costs. Such analysis can help ensure that the observed effect is not a result of prior trends or spurious correlation. In addition, it allows us to explore whether the effect is transitory, as suggested by Dessaint and Matray (2017) and Alok et al. (2020).

To implement the test, we generate five dummy variables: *Neighbor<sub>-2</sub>*, *Neighbor<sub>-1</sub>*, *Neighbor<sub>1</sub>*, *Neighbor<sub>2</sub>*, and *Neighbor<sub>2+</sub>*. Each dummy equals one if the borrowing firm's neighborhood area will experience or has experienced a significant natural disaster two years from now (*Neighbor<sub>-2</sub>*), one year from now (*Neighbor<sub>-1</sub>*), within the last one year (*Neighbor<sub>1</sub>*), within the year before last year (*Neighbor<sub>2</sub>*), and at least two years ago (*Neighbor<sub>2+</sub>*). The dummy variables *Neighbor<sub>-2</sub>* and *Neighbor<sub>-1</sub>* help us assess whether the loan spreads have changed preceding the disaster event date.

The results are reported in Table 4 column (1). All controls in Table 3 column (2) are included in the regression. Two important findings emerge. First, the coefficients of *Neighbor<sub>-2</sub>* and *Neighbor<sub>-1</sub>* are not statistically different from zero at any conventional significance level, suggesting that the increase in loan spreads does not take place leading up to the disaster event. Second, we find that, while the coefficients for *Neighbor<sub>1</sub>* and *Neighbor<sub>2</sub>* are both significantly positive, the impact on loan spreads declines over time, in terms of both economic magnitude and statistical significance. For example, the loan spread of disaster-neighboring firms increases by 5.3% in the first year following a major disaster, while the increase drops to 3.5% in the second year. This is consistent with a causal interpretation of our results, and also echo the findings of Dessaint and Matray (2017) and Alok et al. (2020) that the effect of salience bias decreases as time goes by. In column (2), we use seven time period dummies (i.e., *Neighbor<sub>-3</sub>* to *Neighbor<sub>3+</sub>*) and find similar results.

Taken together, we find no significant effect on a firm's loan spreads before the borrowing firm's neighborhood experiences the disaster, but significantly positive effect within the two years after the disaster.

### **3.3. Alternative explanations**

In this section, we conduct several tests to rule out potential alternative explanations that may confound our interpretation. Specially, we discuss the following possible interpretations: regional spillovers, lender rent extraction, limited credit supply, and lender rational learning.

#### **3.3.1. Regional spillovers**

As we mentioned earlier, the average distance from a neighboring county to disaster-affected counties is 465 miles, suggesting that the direct impact of disasters on adjacent

counties should be very minimal. However, it is still plausible that neighboring firms in our sample are adversely affected by the natural disasters due to regional spillovers, which in turn affects their cost of borrowing. We take several approaches to explore whether this alternative explanation drives our findings. First, we examine firm fundamentals that are related to its credit risk and see whether neighboring firms are significantly affected by these disasters. To do so, we construct several traditional measures of firm fundamental performance, including sales growth, ROA, operating cash flow (*OCF*), financial leverage, credit rating, and Z-score. We then regress these measures on *Neighbor* and the same set of firm characteristics used in Table 3, measured at the last fiscal year end.<sup>15</sup> Table 5 reports the results from firm-year level analysis. As shown, the coefficient on *Neighbor* is not statistically significant in any of these tests, suggesting that neighboring borrowers in our sample are not fundamentally different after their neighborhood experienced a disaster.

Regional spillovers may also lead to deteriorating local economic conditions that have caused the higher cost of borrowing for neighboring firms. The inclusion of state-by-year fixed effects in Table 3 column (3) should partly mitigate this concern. To further address this possibility, we augment the baseline model with time-varying local economic conditions as additional controls. Specifically, we include county per capita income and county unemployment rate in the regression, both measured in the year of loan initiation. The Data are collected from the Bureau of Economic Analysis. Table 6 column (1) reports the results. Our sample size is slightly reduced as information on county-level macroeconomic variables is only available after 1990.<sup>16</sup> We find that the estimated coefficient of *Neighbor* remains positive and

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<sup>15</sup> The results are the same if we remove the lagged dependent variable from the set of regressors. The results are also similar if we use quarterly data, i.e., quarterly sales growth.

<sup>16</sup> We find similar results when further controlling for county GDP, which is available from 2001 onwards.



significant and are comparable to those reported in our baseline results, implying that our findings are unlikely to be driven by changes in local economic conditions.

The third test we conduct to remove the potential impact of regional spillovers is based on neighboring firms' geographical operations. In our current analysis, we follow prior literature and define whether a firm is located in a disaster-affected area and its neighborhood area using the firm's headquarters location. However, a firm may have significant operations in nearby counties or states. To ensure that our findings are not contaminated by a firm's operations in disaster-affected areas, we gather information on each firm's establishments from the National Establishment Time-Series database (NETS). The NETS database provides annual employment and sales data for U.S. businesses and establishments from 1989 onwards. We match firms in our sample with NETS based on the name and address of each company's headquarters. Using establishment-level sales information, we identify all neighboring firms that have more than 10% of their total revenues generated from establishments in disaster-affected counties. We then remove loans issued to these firms within 24 months after a disaster and repeat our baseline analysis. The results are presented in column (2) of Table 6. As shown, we find similar results from this smaller sample.

Finally, to further rule out the spillover explanation, we use stock price co-movements to capture some unobserved economic links between neighboring firms and disaster-affected firms. In particular, for each disaster event, we calculate the sensitivity of each neighboring firm's stock return to that of the value-weighted portfolio of disaster-affected firms (i.e., beta), using data over a 60-month window before the event. If a neighboring firm's stock return co-moves significantly with that of firms in the disaster area, it suggests that some important economic links may exist between them. Hence, these firms are more likely to be subject to the

spillover effect. In column (3) of Table 6, we remove loans issued to borrowers if their co-movements with disaster-affected firms are in the top quartile of all neighboring firms. Our results from this subsample analysis continue to hold.<sup>17</sup> Collectively, all these results suggest that the increase in loan spreads for neighboring firms is not a result of changes in firm fundamentals or regional spillovers.

### **3.3.2. Rent extraction**

The second potential non-behavioral interpretation of our findings is rent extraction. Because corporate managers in neighboring firms may overestimate their disaster risk (e.g., Dessaint and Matray, 2017), banks could take advantage of this response by charging higher loan spreads, even though they understand that these borrowers' objective disaster-related risk remains unchanged.

To evaluate whether this alternative explanation drives our results, we explore whether our results hold after we control for corporate managers' expressed concern about natural disasters. To quantify managers' own assessment of disaster risk, we use two measures. The first measure is constructed following Dessaint and Matray (2017). Specifically, for each firm in each year, we perform a textual analysis of the firm's 10-Ks, 10-Qs, and 8-Ks filings. We search for expressions that contain natural hazard risk and count the mentions of these risk factors in these SEC filings (*Disaster mentions*).<sup>18</sup> Since most of the natural disasters in our sample are climate-related, the second measure we use is the climate change risk measure

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<sup>17</sup> Our results are similar if we remove loans to neighboring firms whose stock return co-movement with disaster-affected firms is higher than the sample median. Our results remain robust if we remove both loans to neighboring firms that have significant operations in disaster-affected counties and loans to neighboring firms that have high stock return co-movements.

<sup>18</sup> We generate the list of expressions for each natural hazard type following Dessaint and Matray (2017). For example, we use expressions such as "hurricane(s) risk(s)", "risk(s) of hurricane(s)", "hurricane(s) threat(s)", and "threat(s) of hurricane(s)" to measure a firm's mentioning of hurricane risk. If there are multiple natural hazards mentioned in one sentence, we count it as one mention.

constructed by Sautner et al. (2021). Using a machine learning keyword discovery algorithm, Sautner et al. (2021) construct firm-level climate change exposure measures using transcripts of earnings conference calls. Their climate change risk measure is calculated as the relative frequency of climate change bigrams mentioned in the same sentence as the words “risk” or “uncertainty” (*Climate change risk*).

The results are reported in Table 6 column 4. The sample size reduces significantly as Sautner et al.’s (2021) *Climate change risk* measure starts from 2001. We find that the coefficient on *Neighbor* remains positive and significant. Turning to variable *Disaster mentions* and *Climate change risk*, we find that the coefficients are both positive albeit insignificant.<sup>19</sup> Since firm fixed effects are included in our analysis, these results suggest that managers’ time-varying assessment of disaster risk or climate change risk does not affect their firms’ cost of bank loans. Taken together, these tests show that our results are unlikely to be driven by lender’s rent extraction motive.

### **3.3.3. Limited credit supply**

Another possible explanation for our findings is that natural disasters may affect the lending capacity of banks located closer to disaster areas. That is, reduced credit supply due to natural disasters could lead to an increase in the borrowing cost of neighboring firms. To investigate this possibility, we further control for bank characteristics that may affect a bank’s commercial lending. In particular, in Table 7 Panel A column (1), we include various bank holding company characteristics in the regression, including size, performance (ROA), and total deposits. In addition, we control for the lender’s loan portfolio mix, such as the fraction

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<sup>19</sup> In untabulated tests, we find that loans issued to firms with higher value of *climate change risk* are more likely to be secured.

of the bank's total loans that are commercial and industrial loans, real estate loans, personal loans, or agricultural loans.<sup>20</sup> As shown, our results continue to hold. Since most bank attributes are only observable at the holding company level, to better capture unobservable time-variant heterogeneity across banks, in column (2) we include bank-year fixed effects in the regression. We find that our results are largely unchanged.

Becker (2007) finds that a bank's commercial lending is often geographically segmented. If a bank has significant lending to regions directly affected by natural disasters, it may affect the bank's loan supply to neighboring firms. This means that a bank's credit supply can vary in different regions in the same year and that such effect cannot be captured by our bank-year fixed effects in column (2). To mitigate this concern, we identify banks that have significant lending to firms in disaster zone at the time of the disaster event, i.e., over 1% of all outstanding loans.<sup>21</sup> We then remove loans arranged by these banks within 24 months following the disaster. Since several mega-banks control the vast majority of syndicate loans in the U.S. (Ross, 2010), this approach leads to the exclusion of a large number of loan observations. Using this much smaller sample, our results remain robust, as presented in Table 7 Panel A column (3).

It is also plausible that banks reallocate post-disaster lending by prioritizing firms that are directly affected by disasters, resulting in more limited credit supply for non-affected firms. Such adverse effect on credit supply can be more severe for neighboring firms than for remote

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<sup>20</sup> Data on bank holding company characteristics are obtained from Compustat. We match it to the DealScan lenders using the link table created by Schwert (2018). Information on loan portfolio mix is obtained from bank holding company's Y-9C reports. We merge it with our main sample using the link table provided by the Federal Reserve Bank of New York. Since the data are only available for large U.S. banks, the sample size reduces in this test.

<sup>21</sup> To calculate the total loan outstanding, we use information on the allocated shares of each loan facility. When such information is not available in Dealscan, we divide the share equally among all participating banks. We find similar results when using 0.5% cutoff to define disaster-affected banks.

firms due to geographical segmentation (Becker, 2007). To address this concern, we next investigate whether banks experience any significant changes in their lending or deposits in disaster-neighboring counties after a major disaster. Specifically, for each county, we calculate the total amount of corporate loans issued to local firms (i.e., firms headquartered in this county) using loans information from Dealscan and calculate the total deposits in the county using bank deposits information from the Federal Insurance Deposit Corporation (FDIC).

Table 7 Panel B presents the results. In columns (1) and (2), we conduct county-month level analysis. The dependent variable in column (1) is the natural logarithm of a county's total amount of corporate loans issued by all banks in our sample in a month (*Log (County loan)*). In column (2), the dependent variable is the natural logarithm of total deposits in the county during the month (*Log (County deposits)*).<sup>22</sup> Our main variable is *Neighboring county*, a dummy variable indicating whether a county is within the neighborhood of a disaster occurred in the past 24 months. We also include county per capita income and county unemployment rate in the analysis. Finally, we control for county fixed effects to account for time-invariant county specific characteristics and year-month fixed effects to account for seasonality and overall time trend. Results in columns (1) and (2) show that the total bank lending and deposits in neighboring counties do not change significantly after the disaster. In columns (3) and (4), we conduct a similar analysis at the bank-county-month level, where both the lending and deposits are calculated for each bank in each county during the month. Similarly, we find that a bank lender's total amount of commercial loans or deposits in the neighboring counties is not significantly different following a disaster.

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<sup>22</sup> The number of observations is smaller than that in column (1) as deposits information from the FDIC is only available after 1994.

Taken together, our analysis in this section shows that bank lenders' credit supply to neighboring firms is not significantly affected by these major natural disasters and that our findings are unlikely to be driven by this supply-side factor.

#### **3.3.4. Rational learning of disaster risk**

Another alternative explanation for our results is that bank lenders may learn about the disaster risk in the neighboring area after the occurrence of disasters. For instance, lenders may underestimate the disaster risk before these major disaster events and update their belief afterwards. However, this explanation is not consistent with the temporary increase in loan spreads that we document. It is also hard to argue that bank lenders would expect that such increase in disaster risk is temporary (i.e., within 2 years) for these neighborhoods.

To further address the concern that our results are driven by rational learning, we next examine whether the documented effect is less prominent as neighboring natural disasters occur repeatedly. Prior literature shows that experience helps overcome the salience bias (e.g, Tobias, 2009; Gao et al., 2020). If it is salience bias that drives our results, then we expect the effect to be weaker when a county's neighborhood is hit by similar disaster events repeatedly.

To examine this conjecture, we construct a variable, *Neighbor occurrence*, calculated as the number of times a neighboring firm has been located in the neighborhood of a similar disaster. We categorize disasters in our study into two broad groups: climate-related disasters and earthquake.<sup>23</sup> On average, a neighboring county has been close to a disaster zone for three times. We then add *Neighbor occurrence* to our baseline regression. The results are presented in Table 7 Panel C. We find that the coefficient on *Neighbor* is 0.067, and the coefficient on *Neighbor occurrence* is -0.016, significant at the 10% level. Explained in an intuitive way,

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<sup>23</sup> Climate-related disasters include hurricane, wildfires, blizzard, tropical storm, ice storm, and flooding.

when the borrower's neighboring areas are hit by a disaster for the first time, the lender increases the interest rate by 6.7%. If it is the third time for the borrowing firm experiencing a disaster around its neighborhood, the overreaction of the lenders reduces to 3.5% ( $0.067 - 0.016 \times 2$ ). These results indicate that the documented positive impact of salient disasters on loan spreads becomes smaller as similar disaster events occur repeatedly in the neighborhood. This is not consistent with the alternative explanation that bank lenders learn about the true disaster risk in the neighboring area after the occurrence of disasters, as if this is the case, the positive effect should be the same or more pronounced after each disaster occurrence. Moreover, these results are also inconsistent with the spillover or credit supply explanation, in which the impact of disasters on neighboring firms' loan spreads should be similar after each occurrence.

Overall, our results in this section provide further support that lenders are likely to subject to salience bias after borrowing firms' neighborhood area is hit by a salient disaster, and that lenders adjust their loan contracts to these firms accordingly.

### **3.4. Additional analysis**

#### **3.4.1. Disaster vulnerable firms**

In this section, we further strengthen our analysis by examining an alternative group of borrowing firms—disaster-vulnerable firms. That is, firms not located in disaster-affected areas or their neighborhood areas, but have been hit by a similar disaster at least once in the past five years.<sup>24</sup> In our sample, we identify 2,471 loan facilities issued to 1,091 disaster-vulnerable firms. For these firms, the average distance to the disaster area is 904 miles and the average

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<sup>24</sup> Since we remove firms that have been hit by a natural disaster in the past 24 months from our analysis, this criteria means that these firms have experienced a similar disaster over the period of (-5, -3) years.

shortest distance to the disaster area is 471 miles, suggesting that these firms are unlikely to be directly affected by the disaster events. Therefore, if we find similar impact of major disasters on these firms' cost of bank debt, it would be consistent with our salience bias argument while inconsistent with other alternative explanations, such as regional spillovers and limited credit supply.

Table 8 columns (1) and (2) report the results. *Vulnerable* is a dummy variable that equals one if a major disaster has occurred in remote areas over the past 24 months (i.e., neither in the firm's county nor its neighboring counties) and that a similar disaster has hit the firm's county in the past five years. In column (1) we control for firm, bank and year fixed effects. In columns (2), we replace year fixed effects with bank-by-year fixed effects. We find that the coefficients on *Vulnerable* are both positive and significant at 1% level, suggesting that lenders charge higher loan spreads for these disaster-vulnerable firms after the disaster event. These results imply that lenders also respond to severe disasters that occur hundreds of miles away if the focal firm is vulnerable to similar disaster risk, which lends further support to our claim that regional spillover effects are unlikely to be the main drivers of our primary finding.

### **3.4.2. Evidence from earthquakes outside the US**

To further rule out alternative explanations, we next perform tests using earthquakes outside the US. In particular, we identify firms located in regions where earthquakes are frequently felt and then examine whether bank lenders charge higher loan spreads to these firms around the occurrence of extremely salient earthquakes outside the US. Since these earthquakes are far away from the US, regional spillovers or bank credit supply channels should be absent. Finding an increase in loan spreads would provide further credence to our behavioral argument.



We collect information on earthquake intensity from the “Did you feel it?” surveys. We compute the average earthquake intensity felt over the past 26 years (1990-2016) for each zip code and then merge the zip level earthquake intensity with firms in our sample using their headquarters locations. Firms whose average intensity is in the top quartile are considered to be more “vulnerable” to earthquake (i.e., firms in seismic hazard zone). Next, we obtain information on the largest earthquakes that have taken place outside the US from the Significant Earthquake Database. Appendix B presents the list of 11 most salient earthquakes outside the US over the period of 1987-2017.<sup>25</sup> We then construct *Vulnerable earthquake*, an indicator that equals one if the loan is granted to firms with higher earthquake risk within 24 months of a salient earthquake outside the US.

Table 8 columns (3) and (4) present the results. We find that the coefficient on *Vulnerable earthquake* is positive and significant at 10% level in both columns. These results indicate that, after a sudden and salient earthquake outside the US, banks charge higher interest rates to firms located in areas with higher earthquake intensity in the following 24 months. Collectively, results in Table 8 further suggest that our findings from the main setting are unlikely to be driven by these alternative explanations.

### **3.4.3. Neighboring vs. disaster-affected firms**

So far, we remove firms that are directly affected by disasters from our analysis. As a robustness check, we now keep these firms and examine how the costs of bank loans change for both neighboring firms and affected firms following a disaster. We create a dummy variable, *Affected*, which equals one if a loan is issued to a disaster-affected firm within 24 months

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<sup>25</sup> To make sure that the earthquakes are salient enough, we restrict our analysis to earthquakes with magnitude of 7 or more and total damage of at least \$1 billion. We further require that the value of “Death Description” and “Damage Description” in the Significant Earthquake Database is at the highest level (level 4) for these earthquakes.

following the natural disaster. To avoid confounding effects, we remove loans to firms that are neighboring to a disaster zone in a year and hit by a different disaster in another year.

The results are presented in Table 9 column (1). The coefficients on *Neighbor* and *Affected* are both positive and significant, suggesting that banks charge a higher interest rate to both disaster-affected firms and their neighboring firms after a disaster. However, as we discussed earlier, there can be various reasons behind the increase in loan spreads for disaster-affected firms, and therefore we prefer to exclude them from our main analysis.

#### **3.4.4. Adjust for seasonality and alternative clustering**

Natural disasters (e.g., hurricane), corporate operations, and credit market can all have seasonality (Murfin and Petersen, 2016). To account for the potential impact of seasonality, in Table 9 column (2), we include firm-quarter fixed effects and year-quarter fixed effects. The results are similar to our earlier findings. As another robustness check, we also repeat our baseline analysis by clustering standard errors at the county level. The results in column (3) suggest that our findings are robust to this alternative clustering strategy.

#### **3.4.5. Retaining one unique lead lender**

When constructing our main sample, we keep all lead banks of a syndicated loan to better account for bank-level heterogeneity. To ensure that our results are not sensitive to the way we construct the sample, we form another sample by retaining only one lead lender for each syndicated loan. Specifically, we use information on lending shares allocated to each participant bank and identify the bank with the largest allocation as the major lead bank.<sup>26</sup> We

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<sup>26</sup> If the information on share allocation is missing, we calculate the total lending amount of each lender throughout our sample period and consider the one with the most lending as the lead bank.

then repeat our baseline analysis at the loan facility level. The results presented in column (4) are comparable to those from our baseline regressions.

#### **3.4.6. Package level analysis**

Since a number of facilities can belong to the same loan package and be governed by the same contract (e.g., Bushman et al., 2021). As a final robustness test, we repeat our analysis using package level observations, where the facility amount weighted average loan spread is used as the dependent variable. The results in Table 9 column (5) show similar findings.

#### **3.4.7. Impact on other non-pricing terms**

In this section we examine whether, in addition to interest rates, banks also alter non-price terms for loans issued to neighboring firms. We conjecture that if bank lenders temporarily believe that these firms face higher disaster risk, then they may impose more covenant restrictions on these neighboring firms.

We conduct the analysis in Table 10. In column (1), the dependent variable is the natural logarithm of the number of covenants imposed in the loan contract (*Log (Covenants)*). We find that the estimated coefficient on *Neighbor* is positive and significant at 5% level, consistent with our conjecture. In column (2), we estimate a probit model examining the likelihood that a loan facility is secured with collaterals. We find that, following a disaster, lenders are more likely to require collateral when extending loans to firms located in the neighborhood of a disaster zone. In column (3), the dependent variable is a dummy variable indicating whether a loan facility has performance-contingent pricing provision (*Performance pricing*). We find that the estimated coefficient on *Neighbor* is positive albeit insignificant. We do not find evidence that banks adjust loan size (column 4) or loan maturity (column 5) for neighboring firms.

Taken together, we find that bank lenders do adjust some of the non-price terms for neighboring firms after a salient natural disaster.

#### **4. Conclusion**

In this paper, we provide empirical evidence showing that bank lenders are subject to salience bias when assessing their clients' natural disaster risk. We find that when a borrower is located in the neighborhood of a region recently hit by a natural disaster, bank lenders subsequently charge the firm higher loan spreads than they do for other firms. Such effect on loan spreads is absent before the borrowing firm's neighborhood experiences the disaster, but is significant within the two years after the disaster. This is consistent with the finding in the literature that salience bias is transitory.

Our analysis also shows that higher loan spreads are unlikely to be driven by regional spillovers, bank's rent extraction, limited credit supply of certain banks, or bank's rational learning. Furthermore, we find that lenders also respond to severe disasters that occur far away if the borrowing firm is vulnerable to similar disaster risk, further supporting our claim that these alternative channels are unlikely to be the main drivers of our primary findings. Finally, we show that, in addition to increasing loan spreads, bank lenders also impose more covenant restrictions and are more likely to require collateral on borrowing firms that are located in the neighborhood of a disaster area.

Overall, we find evidence that a sudden shock to the perceived disaster risk leads lenders to adjust both loan spreads and other non-price terms for borrowers located in the neighborhood of a disaster area. Our study suggests that such salience bias may impose significant costs on borrowing firms.

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## Appendix A Variable Definitions

This table provides the definition of variables used in this paper.

Variable	Definition
<i>Firm characteristics</i>	
Neighbor	A dummy variable that equals one if a loan is issued to a disaster-neighboring firm within 24 months following a natural disaster.
Log (Total assets)	The natural logarithm of total book assets (in millions).
Cash	Total cash holding or equivalents scaled by total assets.
Market-to-book	Market-to-book ratio.
Leverage	Long-term debt scaled by total assets.
ROA	EBITDA scaled by total asset.
Tangibility	Property, plant, and equipment over total asset.
Z-Score	Altman's (1986) modified Z-score = $(1.2 \times \text{working capital} + 1.4 \times \text{retained earnings} + 3.3 \times \text{ebit} + 0.999 \times \text{sales}) / \text{total assets} + 0.6 \times \text{equity market value} / \text{book value of total liability}$ .
Rating	Standard and Poor's corporate credit ratings, converted to an index from 0 to 23 as follows: AAA=23, AA+=22, AA=21.....DDD=3, DD=2 D=1, missing=0.
Sales growth	The annual growth rate of sales.
OCF	Operating cash flow scaled by total sales.
Disaster mentions	The number of mentions of disaster risk in a firm's SEC filings in a year.
Climate change risk	The relative frequency of climate change bigrams mentioned in the conference calls in a year, obtained from Sautner et al. (2021).
Neighbor occurrence	The number of occurrences a neighbor firm has been located in the neighborhood of the disaster area.
Vulnerable	A dummy variable that equals one if a major disaster has occurred in remote areas over the past 24 months (i.e., neither in the firm's county nor its neighboring counties) and that a similar disaster has hit the firm's county in the past five years.
Vulnerable earthquake	A dummy variable that equals one if a firm locates in regions with above-quartile earthquake intensity according to the "Did you feel it?" surveys, and if its loans are initiated within 24 months following a significant earthquake outside the US.
Affected	A dummy variable that equals one if a loan is initiated by a disaster-affected firm within 24 months following a natural disaster.
<i>Loan Characteristics</i>	
Log (Spread)	The natural logarithm of all-in-drawn interest rate paid over LIBOR.
Log (Covenants)	The natural logarithm of total number of covenants in a loan contract.
Log (Loan size)	The natural logarithm of dollar amount of credit granted in a loan facility in millions.
Log (Loan maturity)	The natural logarithm of months to maturity.
Secured	A dummy variable that equals one if the loan facility is secured by collateral and zero otherwise.
Performance pricing	A dummy variable that equals one if the loan facility uses performance pricing and zero otherwise.
Loan type	An indicator variable for loan types, including term loan, revolver greater than one year, revolver less than one year, and 364-day facility.
Loan purpose	An indicator variable for loan purposes, including corporate purposes, debt repayment, working capital and takeover.
<i>Bank Characteristics</i>	
Bank size	The natural logarithm of total book assets of a bank (in millions).
Bank ROA	The return on assets of a bank.

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Bank deposits	The natural logarithm of total deposits of a bank (in millions).
C&I loan	The amount of commercial and industrial loans over total loan outstanding.
Real estate loan	The amount of a bank's real estate loans over total loan outstanding.
Personal loan	The amount of a bank's personal loans over total loan outstanding.
Agriculture loan	The amount of a bank's agriculture loans over total loan outstanding.
Log (Bank county loan)	The natural logarithm of dollar amount of a bank's loan outstanding in a county (in millions)
Log (Bank county deposits)	The natural logarithm of dollar amount of a bank's deposits in a county (in millions).
<i>County Characteristics</i>	
County income	The per capita income of a county in a year divided by 10,000.
County unemployment	The unemployment rate of a county in a year.
Log (County loan)	The total dollar amount (in millions) of corporate loans issued to a county in a month.
Log (County deposits)	The total dollar amount (in millions) of deposits in a county in a month

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## Appendix B List of salient earthquakes outside the U.S.

This table lists the major earthquakes taking place outside the U.S. during 1987-2017, collected from the Significant Earthquake Database. To be included, an earthquake needs to have a magnitude of 7 or more, a total damage of at least \$1 billion, and the highest value of “Death Description” and “Damage Description” in the Significant Earthquake Database (level 4).

Country	Date	Magnitude	Fatality	Damages (in millions)
Iran	1990/6/20	7.3	40,000	7,200
Turkey	1999/8/17	7.6	17,118	20,000
Taiwan	1999/9/20	7.7	2,297	14,000
India	2001/2/26	7.7	20,005	2,623
Indonesia	2004/12/26	9.1	1,001	10,000
Pakistan	2005/10/8	7.6	76,213	6,680
China	2008/5/12	7.9	87,652	86,000
Indonesia	2009/9/30	7.5	1,117	2,200
Haiti	2010/1/12	7.0	316,000	8,000
Japan	2011/3/11	9.1	1,475	4,402
Nepal	2015/4/25	7.8	8,200	10,000

**Table 1 Disaster basics**

This table provides information on major natural disasters (with total damages above one billion in 2017 dollars) in the US during 1988-2017, including the event name, start date, end date, duration days, and the number of counties affected. The data is obtained from the Spatial Hazard Events and Losses Database for the United States (SHELDUS) at Arizona State University.

No.	Event Name	Start Date	End Date	Duration (Days)	No. of Counties affected
1	Hurricane 1989 Hugo	09/22/1989	09/22/1989	1	190
2	Earthquake 1989 - Loma Prieta	10/17/1989	10/17/1989	1	8
3	Hurricane 1991 Bob	08/18/1991	08/18/1991	1	85
4	Wildfires 1991 - Oakland Hills	10/20/1991	10/20/1991	1	1
5	Hurricane 1992 Andrew	08/27/1992	08/27/1992	1	78
6	Hurricane 1992 Iniki	09/11/1992	09/11/1992	1	1
7	Blizzard 1993 - Storm of the Century	03/13/1993	03/13/1993	1	940
8	Earthquake 1994 - Northridge	01/17/1994	01/17/1994	1	1
9	Tropical Storm Alberto 1994	07/03/1994	07/03/1994	1	89
10	Hurricane 1995 Opal	10/03/1995	10/03/1995	1	338
11	Blizzard/Flooding 1996	01/26/1996	01/26/1996	1	590
12	Hurricane 1996 Fran	09/06/1996	09/06/1996	1	207
13	Ice Storm 1998 - Northeast	01/07/1998	01/10/1998	4	42
14	Hurricane 1998 Bonnie	08/26/1998	08/27/1998	2	40
15	Hurricane 1998 Georges	09/21/1998	09/21/1998	1	126
16	Hurricane 1999 Floyd	09/16/1999	09/17/1999	2	300
17	Tropical Storm Allison 2001	06/11/2001	06/11/2001	1	201
18	Wildfires 2003 - Southern California	06/06/2003	06/06/2003	1	9
19	Hurricane 2003 Isabel	09/18/2003	09/18/2003	1	221
20	Hurricane 2004 Charley	08/11/2004	08/13/2004	3	82
21	Hurricane 2004 Frances	09/04/2004	09/05/2004	2	281
22	Hurricane 2004 Jeanne	09/04/2004	09/07/2004	4	158
23	Hurricane 2004 Ivan	09/13/2004	09/16/2004	4	426
24	Hurricane 2005 Dennis	07/09/2005	07/09/2005	1	267
25	Hurricane 2005 Katrina	08/29/2005	08/29/2005	1	315
26	Hurricane 2005 Rita	09/25/2005	09/25/2005	1	139
27	Hurricane 2005 Wilma	10/23/2005	10/23/2005	1	24
28	Flooding 2008 - Midwest	04/01/2008	04/24/2008	24	226
29	Hurricane 2008 Gustav	09/01/2008	09/01/2008	1	141
30	Hurricane 2008 Ike	09/11/2008	09/12/2008	2	511
31	Blizzard 2011 - Groundhog Day	02/01/2011	02/04/2011	4	241
32	Hurricane 2011 Irene	08/28/2011	08/28/2011	1	198
33	Tropical Storm Lee 2011	09/23/2011	09/23/2011	1	170
34	Hurricane 2012 Isaac	08/28/2012	08/28/2012	1	96
35	Hurricane 2012 Sandy	10/30/2012	10/30/2012	1	280
36	Flooding/Severe Weather 2013 - Illinois	04/18/2013	04/18/2013	1	43
37	Flooding 2013 - Colorado	09/12/2013	09/15/2013	4	8
38	Tornadoes/Flooding 2014 - Midwest/Southeast/Northeast	04/28/2014	04/28/2014	1	118
39	Flooding 2015 - East/SC	10/03/2015	10/03/2015	1	23
40	Hurricane 2016 Matthew	10/08/2016	10/08/2016	1	116

**Table 2 Descriptive statistics**

This table presents descriptive statistics of the main variables used in our analysis. Our sample consists of 28,963 loan facilities initiated during 1987–2017. All continuous variables are winsorized at the 1% and 99% levels. Detailed variable definitions are provided in Appendix A.

Variable	N	Mean	Std. Dev.	Q1	Median	Q3
Spread (basis point)	28,963	216.24	136.20	125.00	200.00	275.00
Neighbor	28,963	0.13	0.33	0.00	0.00	0.00
Total assets (\$million)	28,963	5,616.33	12,177.42	413.00	1,697.32	5,432.31
Cash	28,963	0.09	0.11	0.02	0.05	0.13
Market-to-book	28,963	1.79	0.97	1.17	1.49	2.05
Leverage	28,963	0.33	0.22	0.18	0.31	0.46
ROA	28,963	0.13	0.09	0.09	0.13	0.17
Tangibility	28,963	0.31	0.24	0.12	0.24	0.46
Z-score	28,963	3.15	2.80	1.51	2.59	4.02
Rating	28,963	7.17	6.80	0.00	9.00	13.00
Loan size (\$million)	28,963	597.92	1,009.54	83.57	275.94	700.64
Loan maturity (month)	28,963	51.94	22.64	36.00	60.00	60.00
Performance pricing	28,963	0.39	0.49	0.00	0.00	1.00
Secured	28,963	0.56	0.50	0.00	1.00	1.00

**Table 3 Salient disasters and cost of bank loans**

This table examines whether banks react to salient natural disaster events in their syndicate lending. The dependent variable is the natural log of basis points of all-in-drawn spread for each loan facility (*Log (Spread)*). The main variable of interest is *Neighbor*, which equals one if a loan is issued to a disaster-neighboring firm within 24 months following a natural disaster. Panel A presents the results from the baseline regressions. Panel B presents the regression results from the entropy-balanced sample. Detailed variable definitions are provided in Appendix A. Robust t-statistics, based on standard errors clustered at both county and year levels, are reported in parentheses. \*\*\*, \*\*, and \* indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Baseline results			
	(1)	(2)	(3)
	Log (Spread)		
Neighbor	0.059*** (2.841)	0.045*** (2.618)	0.052** (2.390)
Log (Total assets)	-0.144*** (-11.086)	-0.049*** (-4.115)	-0.075*** (-6.174)
Cash	0.133 (1.627)	-0.013 (-0.179)	0.035 (0.484)
Market-to-book	-0.085*** (-6.931)	-0.056*** (-5.272)	-0.065*** (-5.891)
Leverage	0.608*** (10.718)	0.503*** (10.297)	0.515*** (10.643)
ROA	-1.112*** (-11.106)	-0.980*** (-11.252)	-0.925*** (-10.557)
Tangibility	-0.022 (-0.263)	-0.007 (-0.093)	-0.043 (-0.608)
Z-score	0.005 (0.896)	0.002 (0.483)	0.001 (0.206)
Rating	-0.009*** (-4.927)	-0.007*** (-4.185)	-0.005*** (-3.153)
Log (Loan size)		-0.081*** (-14.782)	-0.080*** (-15.036)
Log (Loan maturity)		-0.029** (-2.244)	-0.026* (-1.951)
Performance pricing		-0.086*** (-7.601)	-0.079*** (-6.858)
Secured		-0.121*** (-3.279)	-0.118*** (-3.055)
Loan type and purpose FE	No	Yes	Yes
Firm FE	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes
Year FE	Yes	Yes	No
State-year FE	No	No	Yes
Observations	28,963	28,963	28,860
R-squared	0.746	0.798	0.825

Panel B: Entropy balancing

	(1)	(2)	(3)
	Log (Spread)		
Neighbor	0.042** (2.029)	0.040** (2.393)	0.060*** (2.690)
Firm characteristics	Yes	Yes	Yes
Loan characteristics	No	Yes	Yes
Firm FE	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes
Year FE	Yes	Yes	No
State-Year FE	No	No	Yes
Observations	28,963	28,963	28,860
R-squared	0.777	0.828	0.853

**Table 4 Dynamic trend analysis**

This table conducts dynamic trend analysis for changes in loan spreads around natural disasters. The dependent variable is the natural log of basis points of all-in-drawn spread for each loan facility (*Log (Spread)*). The variables of interest are time dummies, denoted as *Neighbor<sub>t</sub>*. All controls in Table 3 column (2) are included in the regressions, whose coefficients are not reported for brevity. Detailed variable definitions are provided in Appendix A. Robust t-statistics, based on standard errors clustered at both county and year levels, are reported in parentheses. \*\*\*, \*\*, and \* indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)
	Log (Spread)	
Neighbor <sub>-3</sub>		-0.003 (-0.089)
Neighbor <sub>-2</sub>	-0.001 (-0.035)	-0.000 (-0.011)
Neighbor <sub>-1</sub>	0.023 (1.133)	0.024 (1.156)
Neighbor <sub>1</sub>	0.055** (2.521)	0.058*** (2.625)
Neighbor <sub>2</sub>	0.034* (1.672)	0.037* (1.762)
Neighbor <sub>2+</sub>	0.023 (0.976)	
Neighbor <sub>3</sub>		0.024 (1.159)
Neighbor <sub>3+</sub>		0.022 (0.978)
Baseline controls	Yes	Yes
Firm FE	Yes	Yes
Bank FE	Yes	Yes
Year FE	Yes	Yes
Observations	28,963	28,963
R-squared	0.798	0.798



**Table 5 The impact of natural disasters on neighboring firms' fundamentals**

This table examines the impact of natural disasters on neighboring firms' financial fundamentals. The dependent variable in columns (1) to (6) is sales growth, return on asset (ROA), operating cash flow (*OCF*), financial leverage (*Leverage*), S&P credit rating (*Rating*), and financial distress (*Z-Score*), respectively. The explanatory variable of interest is *Neighbor*, which equals one if a firm's headquarters is located in the neighborhood of a county hit by a natural disaster over the past 24 months. All firm characteristics in Table 3 are included in the regressions, whose coefficients are not reported for brevity. Detailed variable definitions are provided in Appendix A. Robust t-statistics, based on standard errors clustered at both county and year levels, are reported in parentheses. \*\*\*, \*\*, and \* indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Sales growth	ROA	OCF	Leverage	Rating	Z-score
Neighbor	-0.003 (-0.291)	-0.002 (-1.017)	0.043 (0.688)	0.001 (0.389)	0.001 (0.009)	0.016 (0.353)
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,052	8,052	8,052	8,052	8,052	8,052
R-squared	0.479	0.749	0.348	0.892	0.934	0.899

**Table 6 Alternative explanations: Regional spillovers and rent extraction**

This table examines whether our results are driven by regional spillovers or bank rent extraction. The dependent variable is the natural log of basis points of all-in-drawn spread for each loan facility (*Log (Spread)*). The explanatory variable of interest is *Neighbor*, which equals one if a loan is issued to a disaster-neighboring firm within 24 months following a natural disaster. In column (1), we control for county-level macroeconomic variables. In column (2), we remove loans issued to neighboring firms that have more than 10% of their total revenues generated from establishments in disaster-affected counties. In column (3), we remove loans to neighboring firms with high stock return co-movement with disaster-affected firms. In column (4), we control for corporate managers' own assessment of disaster risk. All controls in Table 3 column (2) are included in the regressions, whose coefficients are not reported for brevity. Detailed variable definitions are provided in Appendix A. Robust t-statistics, based on standard errors clustered at both county and year levels, are reported in parentheses. \*\*\*, \*\*, and \* indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	Log (Spread)			
Neighbor	0.038** (2.300)	0.042** (2.232)	0.042** (2.274)	0.043** (1.968)
County income	-0.006 (-0.751)	-0.011 (-1.381)	-0.005 (-0.548)	
County unemployment rate	0.009 (1.409)	0.002 (0.212)	0.009 (1.410)	
Disaster mentions				0.006 (0.975)
Climate change risk				0.081 (0.892)
Baseline controls	Yes	Yes	Yes	Yes
Firm Fe	Yes	Yes	Yes	Yes
Bank Fe	Yes	Yes	Yes	Yes
Year Fe	Yes	Yes	Yes	Yes
Observations	27,510	23,145	26,501	16,611
R-squared	0.809	0.821	0.814	0.837

**Table 7 Alternative explanations: Limited credit supply and rational learning**

This table examines whether our results are driven by limited credit supply or lender rational learning. In Panel A, we control for bank heterogeneity. In column (1), we include several bank characteristics in the regression. In column (2), we include bank-by-year fixed effects. In column (3), we remove all loans that are issued within 24 months following a disaster and issued by banks that have significant lending to disaster areas at the time of the disaster event. In Panel B, we examine bank credit supply at the county level. In Panel C, we examine whether the effect gets weaker as neighboring natural disasters occur repeatedly. All controls in Table 3 column 2 are included in the regressions, whose coefficients are not reported for brevity. Detailed variable definitions are provided in Appendix A. Robust t-statistics, based on standard errors clustered at both county and year levels, are reported in parentheses. \*\*\*, \*\*, and \* indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Control for bank heterogeneity

	(1)	(2)	(3)
	Log (Spread)		
Neighbor	0.044** (2.442)	0.040** (2.280)	0.054* (1.840)
Bank size	-0.056 (-0.922)		
Bank ROA	-4.081*** (-2.977)		
Bank deposits	-0.067 (-1.053)		
C&I loan	0.164 (0.698)		
Real estate loan	0.523*** (3.218)		
Personal loan	0.623*** (2.730)		
Agriculture loan	0.863 (0.321)		
Baseline controls	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Bank FE	Yes	No	No
Year FE	Yes	No	No
Bank-year FE	No	Yes	Yes
Observations	18,312	27,866	13,049
R-squared	0.811	0.838	0.878

Panel B: Analysis on county credit supply

	(1) Log (County loan)	(2) Log (County deposits)	(3) Log (Bank county loan)	(4) Log (Bank county deposits)
Neighboring county	0.005 (0.090)	-0.004 (-0.292)	-0.038 (-1.000)	0.000 (0.002)
County income	0.023 (0.384)	0.079** (2.329)	-0.001 (-0.018)	0.012*** (2.672)
County unemployment	0.002 (0.063)	-0.010 (-0.930)	-0.068*** (-2.926)	-0.006 (-1.343)
County fixed effects	Yes	Yes	Yes	Yes
Year-month fixed	Yes	Yes	Yes	Yes
Bank fixed effects	-	-	Yes	Yes
Observations	8,733	7,946	14,339	13,670
R-squared	0.338	0.905	0.342	0.375

Panel C: Disaster occurrences

	Log (Spread)
Neighbor	0.067*** (3.007)
Neighbor occurrence	-0.016* (-1.789)
Baseline controls	Yes
Firm FE	Yes
Bank FE	Yes
Year FE	Yes
Observations	28,963
R-squared	0.801

**Table 8: Vulnerable firms**

This table examines whether lenders respond to remote disasters when the borrowing firm is vulnerable to similar disaster risk. In columns (1) and (2), we examine the impact of US disasters on the cost of bank loans for disaster-vulnerable firms. In columns (3) and (4), we examine the impact of earthquake outside the US on the cost of bank loans for US firms with higher earthquake risk. Detailed variable definitions are provided in Appendix A. Robust t-statistics, based on standard errors clustered at both county and year levels, are reported in parentheses. \*\*\*, \*\*, and \* indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	Log (Spread)			
Neighbor	0.051*** (3.037)	0.047*** (2.693)		
Vulnerable	0.037* (1.803)	0.038* (1.826)		
Vulnerable earthquake			0.042* (1.836)	0.044* (1.777)
Baseline controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Bank FE	Yes	No	Yes	No
Year FE	Yes	No	Yes	No
Bank-year FE	No	Yes	Yes	Yes
Observations	28,963	27,866	28,963	27,866
R-squared	0.802	0.838	0.802	0.838

**Table 9 Additional analysis**

This table reports the results of some additional tests. The explanatory variable is *Neighbor*, which equals one if a loan is initiated by a disaster-neighboring firm within 24 months following a natural disaster. In column (1), we include firms in the disaster zone in our sample and examine how their cost of bank loans changes following the natural disasters. *Affected* equals one if the borrower locates in the disaster zone and its loans are initiated within 24 months following a natural disaster. In column (2) we control for firm-quarter and year-quarter fixed effects. In column (3), we conduct a robust test by clustering standard errors at county level. In column (4), for each loan facility, we retain one unique lead lender and repeat our baseline regression using the facility level sample. In column (5), we aggregate our sample at loan package level using the facility amount weighted average values of loan characteristics and repeat our baseline regression. The dependent variable is the log of basis points of all-in-drawn spread for each loan facility. All controls from Table 3 column 2 are included in the regressions, whose coefficients are not reported for brevity. All continuous variables are winsorized at 1% and 99% level. Robust t-statistics, based on standard errors clustered at both county and year levels, are reported in parentheses. \*\*\*, \*\*, and \* indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
	Log (Spread)				
Neighbor	0.032** (2.284)	0.053** (2.356)	0.045** (2.353)	0.041** (2.384)	0.053*** (2.783)
Affected	0.035*** (3.007)				
Firm FE	Yes	No	Yes	Yes	
Bank FE	Yes	Yes	Yes	Yes	
Year FE	Yes	No	Yes	Yes	
Firm-quarter FE	No	Yes	No	No	
Year-quarter FE	No	Yes	No	No	
Observations	49,129	27,018	28,963	17,441	18,744
R-squared	0.776	0.871	0.798	0.796	0.776

**Table 10 Other loan contract terms**

This table examines whether banks overreact to salient events by tightening loan contract terms other than the loan spread. The explanatory variable is *Neighbor*, which equals one if a loan is initiated by a disaster-neighboring firm within 24 months following a natural disaster. The dependent variables from columns (1) to (5) is *Log (Covenants)*, *Secured*, *Perform pricing*, *Log (Loan size)*, and *Log (Loan maturity)*, respectively. All continuous variables are winsorized at 1% and 99% level. Detailed variable definitions are provided in Appendix A1. Robust t-statistics, based on standard errors clustered at both county and year levels, are reported in parentheses. \*\*\*, \*\*, and \* indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
	Log (Covenants)	Secured	Performance pricing	Log (Loan size)	Log (Loan maturity)
Neighbor	0.040** (1.991)	0.049*** (3.067)	0.024 (1.372)	-0.003 (-0.099)	-0.005 (-0.421)
Baseline controls	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
Bank fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	28,963	28,963	28,963	28,963	28,963
R-squared	0.709	0.641	0.493	0.777	0.733