Internet Appendix for "Market Competition and Shock Propagation: Implications for Margins and Distress"

Winston Wei Dou Shane A. Johnson Wei Wu

Abstract

This Internet Appendix contains anecdotes and additional empirical results of the main text. Section 1 provides anecdotes for both within-industry and cross-industry spillover effects, along with the impact of natural disasters. Section 2 describes empirical measures for distress risk and profit margins. It also illustrates the method we use to match Nielsen data to the CRSP/Compustat database. Section 3 presents additional results for the natural disaster setting, including robustness checks and examinations of alternative explanations. It also provides evidence for the cross-industry spillover effects. Sections 4 and 5 present evidence from the AJCA repatriation tax holiday and the Lehman crisis, respectively.

Keywords: Strategic market competition, Financial distress, Earnings-based borrowing constraints, Spillovers and treatment externalities, Inventory fire sales.

JEL Codes: G32, G33, L11.

Contents

1 Anecdotes for the Spillover Effects

In this section, we provide anecdotes for both the within-industry and cross-industry spillover effects. Additionally, we offer anecdotes regarding the impact of natural disasters and the associated spillover effects.

1.1 An Anecdote for the Within-Industry Spillover: Continental Airlines

In this section, we provide an anecdote of within-industry spillover effects. Specifically, we discuss the pricing behaviors of Continental Airlines and its competitors around Continental's two Chapter 11 reorganizations. The first reorganization period took place from September 1983 to June 1986, while the second one occurred from December 1990 to April 1993. During the first reorganization period, Continental Airlines sharply reduced its fares and initiated an aggressive fare war (see, e.g., Morrison and Winston, 1996; Busse, 2002). Figure OA.1 shows the coverage of the fare war by the Wall Street Journal. United Airlines, Continental's largest competitor, as well as other major competitors, engaged in the price war. Continental reduced its fares again during the second reorganization period, and United Airlines matched the price cuts.

Figure OA.2 depicts the price trends of the Continental Airlines and United Airlines during the two Chapter 11 reorganizations. Following previous studies (e.g., Borenstein and Rose, 1994, 1995), we construct prices using the Department of Transportation's Airline Origin and Destination Survey DB1B database. We also follow Borenstein and Rose (1995) to compare the ticket prices of each airline with the average price of all domestic tickets on routes within the same 100-mile distance block in each quarter. A normalized price of zero indicates that fares were equal to the distance-adjusted overall domestic average price.

Panel A of Figure OA.2 focuses on Continental's first Chapter 11 reorganization from September 1983 to June 1986.[1] We compute fare prices based on the routes operated by Continental airlines both before and after bankruptcy, ensuring that the price changes are not caused by changes to the flight routes offered by Continental around the bankruptcy. The solid red line traces the normalized prices for Continental Airlines. We find that Continental reduced its fair prices by more than 15% around the bankruptcy. The price reduction began in 1983 when Continental became deeply distressed. The price reduction is unlikely to be driven by a deterioration of service quality because previous studies (e.g., Phillips and Sertsios, 2013) have shown that airline service quality actually improves during Chapter 11 reorganizations. The blue dashed line traces the normalized prices for United Airlines, the largest competitor for Continental, on the same routes operated by Continental. We find that United Airlines engaged in a price war and sharply reduced its fare prices. The magnitude of the price cut from United Airlines matches the magnitude of the price cut from Continental, while the magnitude of the price cut from all competitors is about

[1] We plot the data from 1981 because of the data reliability issues associated with the DB1B database before 1980. Details about the data can be found on the NBER website: https://www.nber.org/research/data/department-transportation-db1adb1b.

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Continental Air Cuts Fares More In Some Markets --- Carrier's Response to United Expands Fight Eastward; Other Airlines Fear War

By Bryan Burrough
Staff Reporter of The Wall Street Journal
662 words
17 February 1984
The Wall Street Journal
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English

HOUSTON -- Continental Airlines cut fares in 79 Midwestern and Western markets, expanding and intensifying a price battle among major airlines.

Continental said its move is meant to beat cuts by United Airlines, which matched many of the carrier's fares in 60 Western markets earlier this week. But Continental extended yesterday's price cuts to markets farther east, including Detroit, Cleveland and Philadelphia, a move that widens the scope of what had been a regional price war. "They fought us on the western front, so now we're taking it to the eastern front," said a spokesman for Continental, a Texas Air Corp. unit.

Frontier Airlines immediately matched Continental's prices in 40 markets served through Denver, and some industry watchers worried that other major airlines would get pulled into the fray.

"We have stayed with them along, and we will continue to stay with them," said Daniel Hersh, vice president, market planning for Frontier. A spokesman for American Airlines said the company is studying the effects of Continental's cuts but hasn't decided whether to match the fares. "We really don't want to get pulled into this thing," he said.

Continental's lower fares alarmed some airline industry watchers, who fear a fullscale war could soon erupt. And even if that doesn't occur, further price-cutting at Continental's Denver and Houston "hubs" could mean big losses for several major airlines and could eliminate some fledgling airlines. "The probability is that others will match" Continental's fares, said Alfred Norling, an airline industry analyst with Kidder, Peabody & Co.

The new fares, most of which take effect Tuesday in 79, or less than 10%, of Continental's routes, carry no restrictions. Most last until April 30. Continental's announcement signaled that the carrier was prepared to carry out a fullscale war for market share. While it and other airlines had sought to minimize the possibility of another damaging fare war, Continental said it was willing to fight in all of its markets.

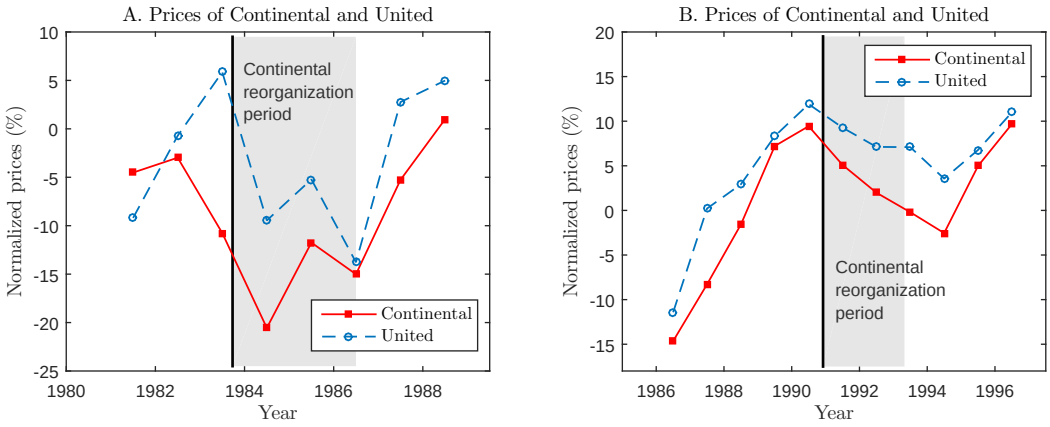
"If they want to play war games, we'll play war games," Continental's spokesman said.

Continental's reductions include flights between Chicago and eight Western cities and between Denver and five Midwestern cities. Some of the cuts are substantial. Fares on flights from Cleveland, for instance, to three cities on the West Coast will be cut to $195 from as much as $255.

Continental has been moving to become the first nationwide discount carrier since it filed for protection from its creditors under Chapter 11 of the federal Bankruptcy Code Sept. 24. Since then, the carrier has steadily increased its number of flights to take away passengers from such major carriers as United and Frontier.

In a statement, Continental struck a fiercely combative note apparently aimed at impressing consumers with willingness to risk low fares for their patronage.

Figure OA.1: The Wall Street Journal's coverage on the fare war launched by Continental Airlines during its Chapter 11 reorganization.



Note: This figure presents price trends of Continental Airlines and its largest competitor, United Airlines, around Continental's two Chapter 11 reorganizations. Panel A shows the price trend around Continental's Chapter 11 reorganization from September 1983 to June 1986. We compute fare prices based on the routes operated by Continental airlines both four quarters before and four quarters after the bankruptcy in September 1983. The solid red line traces the normalized prices for Continental Airlines. The blue dashed line traces the normalized prices for United Airlines (i.e., the largest competitor for Continental) on the same routes. The gray areas represent the Chapter 11 reorganization period for Continental Airlines, and the solid black vertical lines represent the onset of the reorganization period. Panel B shows the price trend around Continental's second Chapter 11 reorganization from December 1990 to April 1993. We compute fare prices based on the routes operated by Continental airlines both four quarters before and four quarters after the bankruptcy in December 1990. We construct prices using Department of Transportation's Airline Origin and Destination Survey DB1B database and exclude tickets with multiple ticketing carriers from the analysis. For each quarter, we compare the airlines' ticket prices to the average price for all domestic tickets on routes within the same 100-mile distance block. A value of zero for this normalized price reflects fares that are equal to the distance-adjusted overall domestic average price.

Figure OA.2: Pricing behaviors around the two Chapter 11 reorganizations of Continental Airlines.

two-thirds of the magnitude of the price cut from Continental. These results are consistent with the Wall Street Journal's coverage shown in Figure OA.1, and suggest that the distress of Continental Airlines led to intensified price competition in the airline industry.

Panel B of Figure OA.2 focuses on Continental's second Chapter 11 reorganization from December 1990 to April 1993. Similar to the previous reorganization, we find that Continental Airlines cut its fare prices aggressively. Its largest competitor, United Airline, responded to the price cut by lowering the fair prices, although the price cut did not trigger a full-blown price war (e.g., Borenstein and Rose, 1995; Morrison and Winston, 1996). Taken together, the findings from the Continental Airline examples nicely illustrate the within-industry spillover effects in our paper: distressed firms compete aggressively in the market and thus lower the profit margins of their industry competitors. The swift fluctuations in airline fares also align with the findings of Bils and Klenow (2004). They use price data from the U.S. Bureau of Labor Statistics (BLS) to demonstrate that airfares change frequently, with the mean duration between price changes being 0.7 months.

1.2 An Anecdote for the Cross-Industry Spillover: Sony Corporation

In this section, we provide an anecdote for cross-industry spillover effects. Specifically, we discuss the competition behaviors of Sony Corporation, which was a common market leader in both the personal computer (PC) and video game industries. The PC industry experienced a major setback in the fourth quarter of 2000, as a result of the dramatic turn in the US economy following the burst of the dotcom bubble. In response, PC vendors such as Dell, Compaq, and Gateway decided

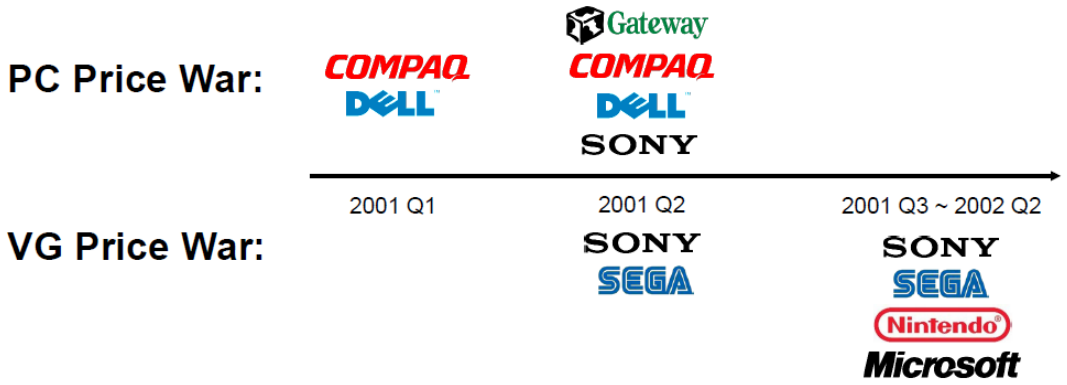


Figure OA.3: Competition spillover from the personal computer (PC) industry to the video game (VG) industry.

to aggressively slash product prices to compete for market share. Sony, as one of the major players in the PC industry, engaged in the price war and suffered losses due to sharp reductions in profit margins. Subsequently, the price war spilled over to the video game industry, in which Sony was also a top market leader. After 2001, Sony became very aggressive in pricing their console products in the video game industry, cutting the prices of PlayStation and PlayStation 2 by large margins globally. This move led to a brutal video game price war in which Sony's major competitors, including Sega, Nintendo, and Microsoft, also participated. Figure OA.3 illustrates the timeline and major participants of both the PC and video game price wars, while media coverage of these events is shown below.

Media coverage about the PC price war in 2000 Q4 and 2001 Q1

- "Analysts see PC price war on horizon." — Reuters, Dec 2000.
- Dell slashed prices. Competitors, namely Compaq, responded in March, 2001. — Dow Jones Newswires.

Media coverage about the PC price war in 2001 Q2

- "Gateway steps up attack in new PC price war." — The Wall Street Journal, May 2001.
- Gateway will sell PCs comparably outfitted to those from Dell, Compaq, HP, IBM, Sony, and Toshiba for one dollar less than the advertised price of its rivals. — The Wall Street Journal, May 2001.
- The slowing global economy takes its toll on Sony. The shares have fallen more than 30 percent from their high set two months ago. — Reuters, July 2001.
- Sony will have to make discounts to keep share. Competition for the PlayStation 2 will heat up in the autumn, with the appearance of rival high-tech consoles from Nintendo and Microsoft. — Reuters, July 2001.

Media coverage about the video game price war in 2001 Q2

- In March 2001, Sega cut the price of the Dreamcast game machine by more than 50%. — Dow Jones International News
- Sony Computer Entertainment to cut price of PlayStation 2 by 12% — Dow Jones International News, Jun 2001.

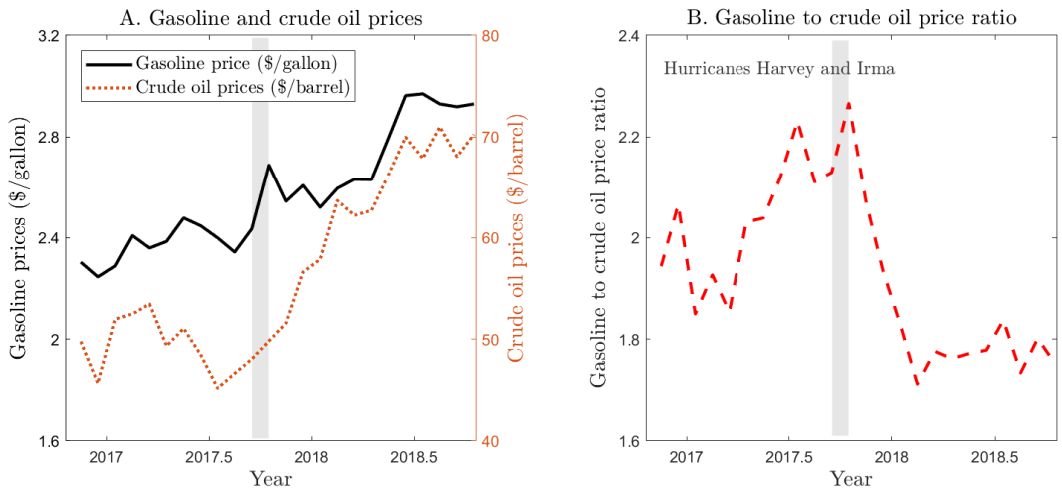
Media coverage about the video game price war from 2001 Q3 to 2002 Q2

- Sony UK slashes PlayStation 2 pricing. — Consumer Electronics, Oct 2001.
- "Sony cuts PS2 price in Japan 15%." — Consumer Electronics, Dec 2001.
- "Microsoft slashes Xbox price in Europe." — Reuters, Apr 2002.
- "Sony to cut PlayStation 2 price by $100." — The New York Times, May 2002.
- "Microsoft slashes Xbox price in reaction to PlayStation cost cut." — Dow Jones Business News, May 2002.
- "Nintendo to cut price of GameCube console by 20%." — Japan Computer Industry Scan, May 2002.

1.3 Anecdotes for the Impact of Natural Disasters

Impact of Hurricanes Harvey and Irma on Industry Profitability. Hurricanes Harvey and Irma inflicted substantial damage on the U.S. oil refinery industry. More than a dozen major oil refineries located in the Gulf Coast suffered significant losses due to these hurricanes. In response to the damages caused by these natural disasters, both gasoline and crude oil prices surged (see panel A of Figure OA.4). However, the percentage increase in gasoline prices was much lower than that of crude oil. Consequently, the profit margin of the oil refinery industry decreased significantly after the hurricanes (see panel B of Figure OA.4). This observation aligns with our hypotheses which predict intensified market competition in response to increased distress risk for firms. The swift fluctuations in gasoline and crude oil prices is consistent with the findings of Bils and Klenow (2004), who show that gasoline prices change frequently, with the mean duration between price changes being 0.6 months.

Within-Industry Spillover. Amphenol Corporation was a major market leader in the electronic connectors industry (SIC code 3678) in 2011. In late August and early September of that year, Hurricane Irene and Tropical Storm Lee hit the East Coast of the United States and caused more than $13.5 billion and $2.8 billion in total damage, respectively. Amphenol Corporation was hit by both Hurricane Irene and Tropical Storm Lee, incurring severe flood damage. On September 9th, 2011, the firm announced in an 8-K filing that it has incurred flood damage at its main manufacturing facility. The firm stated that "the entire region has been severely impacted by high



Note: Panel A of this figure displays the gasoline and crude oil prices during the time of Hurricanes Harvey and Irma. Both prices are sourced from the Federal Reserve Economic Data. Panel B of the figure illustrates the ratio between the gasoline price and the crude oil price. The gray areas in both panels denote the period during which Hurricanes Harvey and Irma occurred.

Figure OA.4: Profitability of the oil refinery industry during hurricanes Harvey and Irma.

rainfall and the back to back impacts of Hurricane Irene and Tropical Storm Lee, resulting in an unexpected and historic rise in water levels as well as pervasive flooding in the region." The company estimated that the flood related losses, in excess of related insurance coverage, were in the range of $20 to $35 million, which accounted for more than 0.5% of the firm's annual sales in 2011. After being hit by the hurricanes, the bond yield spread of Amphenol Corporation increased by 28 basis points (from 1.56% in 2010 Q4 to 1.84% in 2011 Q4), representing a 17.9% increase in the level of bond yield spread. The gross profit margin of the firm also reduced by 0.9 percentage points (from 35.5% in 2010 to 34.6% in 2011).

The adverse shock that affected Amphenol Corporation spilled over to its main industry competitor, TE Connectivity, which was the other major market leader in the electronic connectors industry. TE Connectivity was not directly affected by any natural disasters in 2011, but its gross profit margin reduced by 0.9 percentage points that year (from 35.8% in 2010 to 34.9% in 2011). TE Connectivity blamed intensified price competition for the reduction of its profit margin. In its 2011 10-K filing, the firm stated that "**price erosion** adversely affected organic sales by $192 million in 2011," which represents more than 1.3% of its annual sales. Because our bond yield spread data do not cover TE Connectivity, we examine the changes in TE Connectivity's failure probability based on the distress measure of Campbell, Hilscher and Szilagyi (2008), and we find that TE Connectivity's failure probability increased by 35% from 2010 to 2011.

Cross-Industry Spillover. CSX Corporation was a common market leader that operated in both the railroad industry (SIC code 4011) and the deep sea freight transportation industry (SIC code 4412) in 1999. In September of that year, Hurricane Floyd struck the East Coast of the United States and severely damaged CSX Corporation's business in the railroad sector. In the company's 1999 annual 10-K filing, it stated that "Things turned for the worse in September. Hurricane Floyd

wreaked havoc on the entire eastern half of the network. Extensive flooding, ranging from Miami to Boston, caused major track washouts, with the most extensive damage occurring on mainline routes in North Carolina and New Jersey. Operating plans for the upcoming fall traffic peak had to be revised as locomotives were stranded, and car types could not be moved efficiently to meet rising shipper requirements. The network went out of balance; key freight yards became choke points, and trains were backed up in high-volume corridors. Very costly actions had to be taken to deal with the storm and its aftermath. Needed power was leased from other railroads, manpower was shifted to troubled locations, and additional train crews were brought on to deal with the situation. Keeping the network running and successfully avoiding 'meltdown' was our highest priority." In the third quarter of 1999, CSX Corporation reported a loss of $113 million. The firm cited Hurricane Floyd as one of the major reasons for the loss. A Wall Street Journal article published on October 29th, 1999 stated that "Hurricane Floyd hit in mid-September, shutting down major rail lines, rerouting trains and backing up CSX's rail system." The article also cited John Snow, chairman and chief executive of CSX Corporation, who said that Hurricane Floyd "came at absolutely the worst time." The gross profit margin of CSX Corporation went down by 3.8 percentage points in 1999 (from 29.3% in 1998 to 25.5% in 1999).

The adverse shock caused by Hurricane Floyd that affected the railroad industry spilled over to the ocean freight transportation industry. A main competitor of CSX Corporation in the ocean freight transportation industry is Stolt-Nielsen, which was one of the largest operators in the tank container markets in 1999. Stolt-Nielsen was not directly affected by natural disasters in 1999, but the firm's gross profit margin went down by 2.4 percentage points that year (from 36.6% in 1998 to 34.2% in 1999). Stolt-Nielsen blamed intensified price competition for the reduction of profit margin. In its 20-F filing of 1999, the firm stated that "margins declined in 1999 due to **increased pricing pressure**." The distress level of Stolt-Nielsen also increased afterwards in the short run.

2 Data and Empirical Measures

2.1 Additional Data Description

In Section 3 of the main text, we describe the main datasets. Here, we provide detailed description for additional datasets.

Industry Classification. Our study focuses on strategic competition among firms whose products are close substitutes. We therefore use four-digit SIC codes to define industries, following the literature (e.g., Hou and Robinson, 2006; Gomes, Kogan and Yogo, 2009; Frésard, 2010; Giroud and Mueller, 2010, 2011; Bustamante and Donangelo, 2017). Like Bustamante and Donangelo (2017), we use four-digit SIC codes in Compustat instead of historical SIC codes from CRSP to define industries, because previous studies have concluded that Compustat-based SIC codes are, in general, more accurate (e.g., Guenther and Rosman, 1994; Kahle and Walkling, 1996; Bhojraj, Lee

and Oler, 2003). Earlier studies have also pointed out that the four-digit SIC codes in Compustat often end with a 0 or 9, which could represent a broader three-digit industry definition. To address this problem, we follow Bustamante and Donangelo (2017) and replace the SIC code of firms whose SIC code ends with a 0 or 9 with the SIC code of the main segment in the Compustat segment data. We further remove those firms whose four-digit SIC code still ends with a 0 or 9 after this adjustment.

Firms' Individual Consumer Data. We identify the geographic locations of firms' individual consumers using a detailed dataset from Baker, Baugh and Sammon (2020), which provides firms' sales to individual consumers at the city level from 2010 to 2015. We thank Scott Baker for generously allowing us to access this dataset. The individual consumer dataset is constructed based on a transaction-level database that covers debit and credit card spending across around two million American users to gain insights about the firms that they patronize, and it mainly covers firms in the consumer-facing industries (i.e., airlines, grocery stores, hotels, retailers, restaurants, utilities, and many online services).

Production Network Data. We identify firm-level supplier-customer links based on Compustat customer segment data and Factset Revere data following Barrot and Sauvagnat (2016) and Gofman, Segal and Wu (2020). We identify industry-level supplier-customer links based on the BEA Input-Output Accounts data following previous studies (e.g., Menzly and Ozbas, 2010; Acemoglu and Azar, 2020). We further supplement the industry-level production network connections based on the firm-level supplier-customer links constructed from Compustat customer segment data and Factset Revere data.

Other Data. We obtain analyst coverage from I/B/E/S, and institutional owernship from Thomson/Refinitiv 13-F data. Stock returns are from the Center for Research in Security Prices (CRSP).

2.2 Measures for Distress Risk

Distress Risk. We follow Campbell, Hilscher and Szilagyi (2008) in measuring distress risk ($Risk\_of\_Failure_{i,t}$), which captures the probability of firm bankruptcy or failure with a one-year forecasting horizon. Specifically, we define distress risk based on the third column in Table IV of Campbell, Hilscher and Szilagyi (2008) as follows:

$$\begin{aligned} Risk\_of\_Failure_{i,t} = &-9.164 - 20.264 NIMTAAVG_{i,t} + 1.416 TLMTA_{i,t} \\ &- 7.129 EXRETAVG_{i,t} + 1.411 SIGMA_{i,t} \\ &- 0.045 RSIZE_{i,t} - 2.132 CASHMTA_{i,t} \\ &+ 0.075 MB_{i,t} - 0.058 PRICE_{i,t}. \end{aligned}$$

Here, $NIMTAAVG$ is the moving average of the ratio between net income and market total assets. $TLMTA$ is the ratio between total liabilities and market value of total assets. $EXRETAVG$ is the moving average of stock returns in excess of the returns of the S&P 500 index. $SIGMA$ is the annualized standard deviation of daily returns over the past three months. $RSIZE$ is the relative size measured as the log ratio of a firm's market equity to that of the S&P 500 index. $CASHMTA$ is the ratio between cash and market value of total assets. MB is the ratio between market equity and book equity. $PRICE$ is the log of the stock price, truncated above at \$15. A higher level of $Risk\_of\_Failure_{i,t}$ implies a higher probability of bankruptcy or failure.

Distance to Default. We follow Bharath and Shumway (2008) in constructing the distance to default measure using the naive Merton default probability ($Dist\_to\_Default_{i,t}$). Specifically, we define the distance to default with a one-year forecasting horizon following equation 12 of Bharath and Shumway (2008):

$$Dist\_to\_Default_{i,t} = \frac{\ln\left((E_{i,t} + F_{i,t})/F_{i,t}\right) + (r_{i,t} - 0.5\sigma_{i,t}^2)}{\sigma_{i,t}}.$$

where E is the market value of the firm's equity and F is the face value of the firm's debt. Variable $r_{i,t}$ represents the firm's stock return over the year. Variable $\sigma_{i,t}$ represents the total volatility of the firm, which is approximated by:

$$\sigma_{i,t} = \frac{E_{i,t}}{E_{i,t} + F_{i,t}}\sigma_{i,t}^E + \frac{E_{i,t}}{E_{i,t} + F_{i,t}}\sigma_{i,t}^D,$$

where $\sigma_{i,t}^E$ is the annualized stock volatility computed based on daily stock returns over the year, and $\sigma_{i,t}^D$ is approximated by $\sigma_{i,t}^D = 0.05 + 0.25\sigma_{i,t}^E$. The distance to default measure negatively captures the distress risk. A lower level of $Dist\_to\_Default_{i,t}$ implies a higher probability of bankruptcy or failure.

2.3 Measures for Profit Margins.

Gross Profit Margins. Following the recent literature (e.g., Antras, Fort and Tintelnot, 2017; Anderson, Rebelo and Wong, 2023; Autor et al., 2020; De Loecker, Eeckhout and Unger, 2020), we use the wedge between sales and variable costs of production to measure gross profit margins in our main empirical analyses, and use cost of goods sold (COGS) from the financial statement of the firm as an empirical proxy for the variable cost of production. The item COGS bundles all expenses directly attributable to the production of the goods sold by the firm and includes materials and intermediate inputs, ordinary labor cost, energy, and so on. Specifically, gross profit margins are computed as the difference between sales and cost of goods sold divided by sales. The data of sales and cost of goods sold are from Compustat.

Net Profit Margins. For robustness, we also use the wedge between sales and total costs of operating the firm to measure net profit margins, as in Karabarbounis and Neiman (2018); Baqaee and Farhi (2019); Anderson, Rebelo and Wong (2023). We use selling, general and administrative expenses (SG&A) as an operating expenses from the financial statement of the firm to gauge fixed costs of operating the firm, interest expenses (XINT) to gauge fixed costs of working capital for running the firm (e.g., Bolton, Chen and Wang, 2011, 2014; Jermann and Quadrini, 2012), and capital depreciation (DP) to gauge additional variable costs of production (e.g., Greenwood, Hercowitz and Huffman, 1988). The total cost of operating the business is the sum of COGS, SG&A, DP, and XINT. The item SG&A includes selling expenses (salaries of sales personnel, advertising, rent), general operating expenses, and administration (executive salaries, general support related to the overall administration). Specifically, net profit margins are computed as the difference between sales and total costs of operating the firm (i.e., COGS + SG&A + DP + XINT) divided by sales. The data are from Compustat.

2.4 Match Nielsen Data to CRSP/Compustat

The Nielsen data contains prices and quantities of every unique product that had any sales in the 42,928 stores of more than 90 retail chains in the US market from January 2006 to December 2016. In total, the Nielsen data cover more than 3.5 million unique products identified by Universal Product Codes (UPCs); they represent 53%, 55%, 32%, 2%, and 1% of all sales in grocery stores, drug stores, mass merchandisers, convenience stores, and liquor stores, respectively (see, e.g., Argente, Lee and Moreira, 2018).

We use the product-firm links provided by GS1, the official source of UPC barcodes in the US, to match products in the Nielsen data to firms in CRSP/Compustat. We follow previous studies (see, e.g., Aguiar and Hurst, 2007; Broda and Weinstein, 2010; Hottman, Redding and Weinstein, 2016; Argente, Lee and Moreira, 2018; Kroft et al., 2022) to find the companies that own the products in the Nielsen data using the product-firm link table in the "GS1 US Data Hub | Company" data.[2] We match 95.3% of the products in the Nielsen data with firms in the GS1 data. Our matching rate is the same as those reported by Argente, Lee and Moreira (2018).

We further match the companies in the GS1 data to CRSP/Compustat based on their firm names. Specifically, we remove punctuations and clean special characters, and then transform the company names into upper case and standardize them. For example, "INDUSTRY" is standardized to "IND"; and "RESEARCH" is standardized to "RES"; and corporate form words (e.g., "LLC" and "CORP") are dropped. We then use the fuzzy name-matching algorithm (*matchit* command in Stata), which generates matching scores (Jaccard index) for all name pairs of companies in the GS1 data and firms in CRSP/Compustat.[3] We obtain a pool of potential matches based on two criteria:

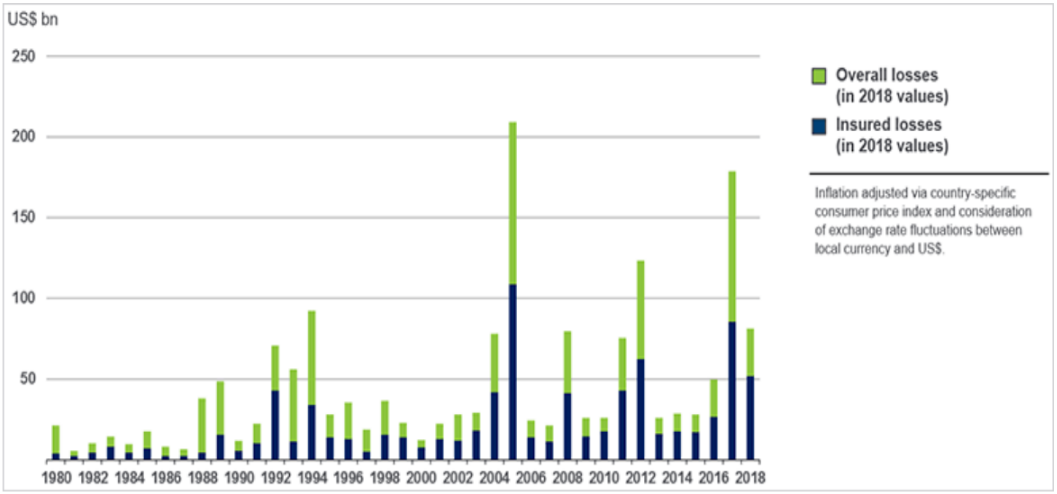
[2]The "GS1 US Data Hub | Company" data provide the company names, company addresses, and the UPC prefixes owned by the companies. More information about the "GS1 US Data Hub | Company" is available at https://www.gs1us.org/tools/gs1-us-data-hub/company.

[3]The Jaccard index measures the similarity between finite sample sets and is defined as the size of the intersection

(1) the matching score must be higher than 0.6; (2) the first three letters of the GS1 company must be the same as those of firms in CRSP/Compustat.[4] We then manually identify correct matches from all potential matching candidates.[5] Our merged sample covers the product prices of 653 firms from 174 three-digit SIC industries, and the sample period spans from 2006 to 2016.

3 Additional Evidence from the Natural Disaster Setting

3.1 Disaster Losses Are Only Partially Offset by Insurance



Source: © 2019 Munich Re, Geo Risks Research, NatCatSERVICE. As of March 2019.

Note: This figure depicts the total losses and insured losses from natural disasters in the US from 1980 to 2018. The data is sourced from the report titled "Facts + Statistics: US catastrophes" by the Insurance Information Institution, which can be accessed at www.iii.org/fact-statistic/facts-statistics-us-catastrophes.

Figure OA.5: Total losses and insured losses from natural disasters in the US from 1980 to 2018.

Although insurance coverage and public disaster assistance can help offset firms' losses in natural disasters, they only provide partial relief. Research by Froot (2001) indicates that disaster insurance premiums are much higher than the expected losses because of the high concentration of the catastrophe insurance market. In line with this finding, about half of the firms exposed to natural disasters do not have insurance policies (Henry et al., 2013), and around half of the natural disaster losses from 1980 to 2018 were not insured, as shown in Figure OA.5. Even for insured firms, coverage is not complete, which can disrupt investment activities (Garmaise and Moskowitz, 2009). Furthermore, delays in the settlement of insurance claims can lead to economic and financial distress for insured firms until eventual compensation (Aretz, Banerjee and Pryshchepa, 2019).

divided by the size of the union of the sample sets. The Jaccard index ranges between 0 and 1, with 1 reflecting perfect similarity.

[4]These two matching criteria are sufficiently conservative to ensure that exact matches are included in the pool of potential matches. For example, among all the exact matches in 2016, 97% of them satisfy the two matching criteria and are included in our pool of potential matches.

[5]We rely on firm names in the GS1 data and CRSP/Compustat to identify matches. In addition, we use location information in both datasets to facilitate the matching process.

Table OA.1: List of major natural disasters.

Disasters	Year	Affected States
Northridge Earthquake	1994	CA
Tropical Storm Alberto	1994	AL, FL, GA
Hurricane Opal	1995	AL, FL, GA, LA, MS, NC, SC
North American Blizzard of 1996	1996	CT, DE, IN, KY, MA, MD, NC, NJ, NY, PA, VA, WV
Hurricane Fran	1996	NC, SC, VA, WV
North American Ice Storm of 1998	1998	ME, NH, NY, VT
Hurricane Bonnie	1998	NC, VA
Tropical Storm Frances	1998	LA, TX
Hurricane Georges	1998	AL, FL, LA, MS
Hurricane Floyd	1999	CT, DC, DE, FL, MD, ME, NC, NH, NJ, NY, PA, SC, VA, VT
Tropical Storm Allison	2001	AL, FL, GA, LA, MS, PA, TX
Hurricane Isabel	2003	DE, MD, NC, NJ, NY, PA, RI, VA, VT, WV
Southern California Wildfires	2003	CA
Hurricane Charley	2004	FL, GA, NC, SC
Hurricane Frances	2004	AL, FL, GA, KY, MD, NC, NY, OH, PA, SC, VA, WV
Hurricane Ivan	2004	AL, FL, GA, KY, LA, MA, MD, MS, NC, NH, NJ, NY, PA, SC, TN, WV
Hurricane Jeanne	2004	DE, FL, GA, MD, NC, NJ, PA, SC, VA
Hurricane Dennis	2005	AL, FL, GA, MS, NC
Hurricane Katrina	2005	AL, AR, FL, GA, IN, KY, LA, MI, MS, OH, TN
Hurricane Rita	2005	AL, AR, FL, LA, MS, TX
Hurricane Wilma	2005	FL
Midwest Floods	2008	IA, IL, IN, MN, MO, NE, WI
Hurricane Gustav	2008	AR, LA, MS
Hurricane Ike	2008	AR, LA, MO, TN, TX
Groundhog Day Blizzard	2011	CT, IA, IL, IN, KS, MA, MO, NJ, NM, NY, OH, OK, PA, TX, WI
Hurricane Irene	2011	CT, MA, MD, NC, NJ, NY, VA, VT
Tropical Storm Lee	2011	AL, CT, GA, LA, MD, MS, NJ, NY, PA, TN, VA
Hurricane Isaac	2012	FL, LA, MS
Hurricane Sandy	2012	CT, DE, MA, MD, NC, NH, NJ, NY, OH, PA, RI, VA, WV
Illinois Flooding	2013	IL, IN, MO
Colorado Flooding	2013	CO
Louisiana Flooding	2016	LA
Hurricane Matthew	2016	FL, GA, NC, SC
Western California Wildfires	2017	CA
Hurricane Harvey	2017	TX
Hurricane Irma	2017	FL, PR
Hurricane Maria	2017	PR
Western California Wildfires	2018	CA
Hurricane Florence	2018	NC, SC
Hurricane Michael	2018	FL, GA, NC, SC, VA

Public disaster assistance also takes time to arrive, with the average duration of assistance lasting up to 6 years from the announcement date of a presidential disaster declaration according to the Federal Emergency Management Agency (FEMA) Disaster Declarations Database (e.g., Seetharam, 2018).

3.2 List of Major Natural Disasters

Table OA.1 lists the major natural disasters that occurred from 1994 to 2018. Following Barrot and Sauvagnat (2016), we define a major natural disaster as one that causes at least $1 billion in total estimated property damage and that lasts for less than 30 days. We source the data on property damage from the SHELDUS database. Figure OA.6 presents the frequency of major natural disasters for each county in the US mainland from 1994 to 2018.

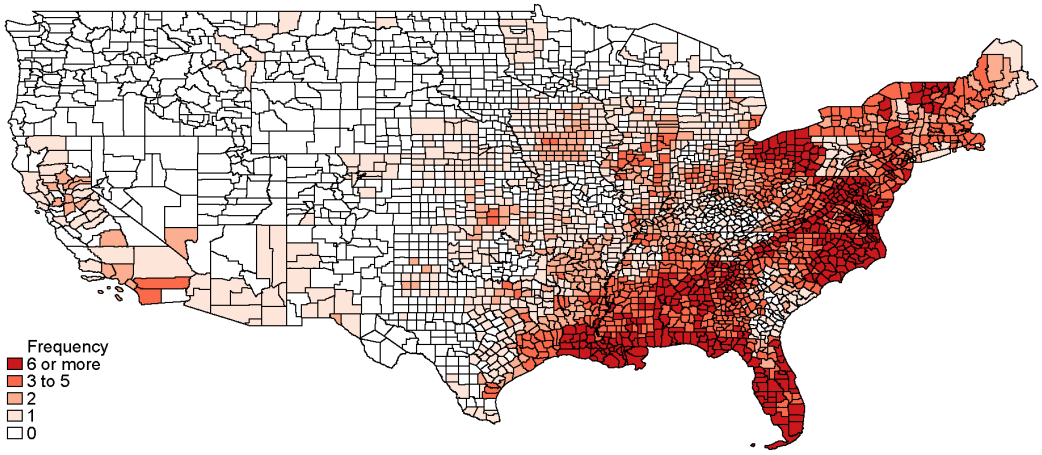


Figure OA.6: Frequency of major natural disasters by US county.

3.3 Robustness Checks for the Within-Industry Spillover Effects

We perform a comprehensive set of robustness checks. Our results are robust to alternative matching ratios between treated firms and non-treated competitors, including one-to-ten and one-to-three (see Table OA.2). The results also hold when defining industry competitors using the text-based network industry classifications (TNIC) developed by Hoberg and Phillips (2010, 2016) (see Table OA.3).

In addition, our findings are robust to an alternative measure to control for the strength of cross-industry spillover effects (see Table OA.4). Specifically, we construct variable $\ln(1+\mathcal{D}_{i,t})$ to capture the strength of cross-industry spillover effects. This variable is expressed as the natural logarithm of 1 plus the average amount of property damage (in millions of dollars) caused by major local natural disasters in year t across industries that are connected to firm i's industry through competition networks.

We address potential concerns about DID regressions with staggered treatment. While recent econometric advances suggest that standard DID regression estimates with staggered treatments may sometimes yield biased results, this is not universally the case (e.g., De Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021; Athey and Imbens, 2022). To ensure robustness, we follow the recommendation of Baker, Larcker and Wang (2022) to estimate the stacked regression estimators with "clean" controls (e.g., Gormley and Matsa, 2011; Cengiz et al., 2019; Deshpande and Li, 2019). It is worth pointing out that our regressions in the main text are stacked regressions already. Different from these regressions in the main text, in this robustness test, we further require that the control firms to be untreated within the entire treatment windows (i.e., two years before and after the natural disasters). In addition, we include the treated cohort $\times$ firm fixed effects (i.e., $\theta_{g,i}$) in the regressions, which spans the indicator variable for the treated firms (i.e., $Treat_{i,t}$). As shown in Table OA.5, the within-industry spillover effects remain robust after we addressing the potential concerns about DID regressions with staggered treatment.

Table OA.2: Alternative matching ratios between treated firms and non-treated competitors.

	Panel A: Matching 1 treated firm with up to 10 non-treated competitors					
	(1)	(2)	(3)	(4)	(5)	(6)
	$Risk\_of\_Failure_{i,t}$		$Dist\_to\_Default_{i,t}$		$Profit\_Margin_{i,t}$	
$Treat_{i,t} \times Post_{i,t}$	0.031^{***}	0.031^{***}	-0.086^{*}	-0.087^{*}	-0.005	-0.006
	[3.004]	[3.038]	[−1.715]	[−1.738]	[−0.957]	[−0.991]
$Treat_{i,t}$	-0.013	-0.013	0.090^{*}	0.091^{*}	0.001	0.001
	[−1.387]	[−1.400]	[1.903]	[1.914]	[0.119]	[0.135]
$Post_{i,t}$	0.050^{***}	0.049^{***}	-0.124^{***}	-0.120^{***}	-0.008^{**}	-0.007^{*}
	[7.623]	[7.539]	[−4.155]	[−4.056]	[−1.971]	[−1.831]
$\ln(1+n(\mathcal{C}_{i,t}))$		0.019^{**}		-0.050		-0.010^{**}
		[2.259]		[−1.331]		[−2.206]
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	207050	207050	164136	164136	205140	205140
R-squared	0.586	0.586	0.657	0.657	0.737	0.737
Test p-value: $\beta_1 + \beta_3 = 0$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	0.001	0.002
	Panel B: Matching 1 treated firm with up to 3 non-treated competitors					
	(1)	(2)	(3)	(4)	(5)	(6)
	$Risk\_of\_Failure_{i,t}$		$Dist\_to\_Default_{i,t}$		$Profit\_Margin_{i,t}$	
$Treat_{i,t} \times Post_{i,t}$	0.024^{**}	0.024^{**}	-0.087^{*}	-0.077^{*}	-0.000	-0.000
	[2.277]	[2.293]	[−1.662]	[−1.681]	[−0.060]	[−0.073]
$Treat_{i,t}$	-0.014	-0.014	0.093^{*}	0.093^{*}	0.001	0.001
	[−1.416]	[−1.419]	[1.718]	[1.726]	[0.315]	[0.318]
$Post_{i,t}$	0.055^{***}	0.054^{***}	-0.129^{***}	-0.123^{***}	-0.006^{***}	-0.006^{**}
	[7.283]	[7.202]	[−3.727]	[−3.581]	[−2.645]	[−2.544]
$\ln(1+n(\mathcal{C}_{i,t}))$		0.015^{**}		-0.070^{*}		-0.004
		[2.010]		[−1.913]		[−1.630]
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	100225	100225	82652	82652	99489	99489
R-squared	0.601	0.601	0.673	0.673	0.759	0.759
Test p-value: $\beta_1 + \beta_3 = 0$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	0.003	0.005

Note: This table examines the spillover effects of the major natural disasters with alternative matching ratios between treated firms and non-treated competitors. In panel A, we match each treated firm with up to 10 non-treated competitors in the same four-digit SIC industry based on firm asset size, tangibility, and age. In panel B, we match each treated firm with up to 3 non-treated competitors. The regression specification and the definition of the dependent and independent variables are explained in Table 2 of the main text. In the last row of the panels, we present the p-value for the null hypothesis that the total treatment effect for the treated firms is 0 (i.e., $\beta_1 + \beta_3 = 0$). The sample of this table spans from 1994 to 2018. Standard errors are clustered at the firm level. We include t-statistics in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

We confirm that the within-industry spillover effects are robust when we use net profit margin as an alternative measure of profitability (see Table OA.6). To address concerns that matched competitors may be geographically close to areas affected by natural disasters, we conduct two robustness tests. First, in panel A of Table OA.7, we restrict competitors for the DID estimation to those located outside states impacted by major local natural disasters. Second, in panel B of Table OA.7, we require that the matched competitors have headquarters and major establishments located more than 100 miles from any zip code negatively affected by major local natural disasters in the focal year for which they serve the match. Our results hold in both robustness tests.

Table OA.3: Matching industry competitors with text-based network industry classifications.

	(1)	(2)	(3)
	$Risk\_of\_Failure_{i,t}$	$Dist\_to\_Default_{i,t}$	$Profit\_Margin_{i,t}$
$Treat_{i,t} \times Post_{i,t}$	0.015	−0.025	−0.005
	[1.467]	[−0.541]	[−1.005]
$Treat_{i,t}$	−0.008	0.027	0.007*
	[−0.889]	[0.556]	[1.708]
$Post_{i,t}$	0.049***	−0.154***	−0.007**
	[7.542]	[−5.165]	[−2.051]
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	219992	176763	219133
R-squared	0.580	0.639	0.742
Test p-value: $\beta_1 + \beta_3 = 0$	$<10^{-3}$	$<10^{-3}$	0.002

Note: This table examines the within-industry spillover effects of the major natural disasters based on TNIC (Hoberg and Phillips, 2010, 2016). We perform a DID analysis. Specifically, we match each treated firm with up to 10 non-treated competitors in its TNIC industry based on firm asset size, tangibility, and age. We ensure that the matched competitors are neither suppliers nor customers of the treated firms. For each firm, we include four yearly observations (i.e., 2 years before and 2 years after major natural disaster) in the analysis. The regression specification is: $Y_{i,t} = \beta_1 Treat_{i,t} \times Post_{i,t} + \beta_2 Treat_{i,t} + \beta_3 Post_{i,t} + \theta_i + \delta_t + \varepsilon_{i,t}$. The dependent variables are the distress risk ($Risk\_of\_Failure_{i,t}$), distance to default ($Dist\_to\_Default_{i,t}$), and gross profit margin ($Profit\_Margin_{i,t}$). $Treat_{i,t}$ is an indicator variable that equals 1 if firm i is a treated firm. $Post_{i,t}$ is an indicator variable that equals 1 for observations after major natural disasters. The term θ_i represents firm fixed effects, and the term δ_t represents year fixed effects. In the last row of the panel, we present the p-value for the null hypothesis that the total treatment effect for the treated firms is 0 (i.e., $\beta_1 + \beta_3 = 0$). The sample of this table spans from 1994 to 2018. Standard errors are clustered at the firm level. We include t-statistics in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table OA.4: Alternative measure to control for cross-industry spillover effects.

	(1)	(2)	(3)	(4)	(5)	(6)
	$Risk\_of\_Failure_{i,t}$		$Dist\_to\_Default_{i,t}$		$Profit\_Margin_{i,t}$	
$Treat_{i,t} \times Post_{i,t}$	0.023**	0.029***	−0.087*	−0.102*	−0.001	0.000
	[2.275]	[2.780]	[−1.717]	[−1.923]	[−0.196]	[0.099]
$Treat_{i,t}$	−0.011	−0.013	0.096*	0.092*	−0.001	−0.001
	[−1.189]	[−1.337]	[1.940]	[1.770]	[−0.189]	[−0.163]
$Post_{i,t}$	0.055***	0.049***	−0.122***	−0.097***	−0.007**	−0.007**
	[8.223]	[7.180]	[−3.882]	[−3.055]	[−2.283]	[−2.291]
$\ln(1 + \mathcal{D}_{i,t})$		0.010***		−0.033**		−0.003***
		[3.349]		[−2.436]		[−2.634]
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	136181	124621	110581	101308	135037	124047
R-squared	0.597	0.611	0.667	0.676	0.745	0.748
Test p-value: $\beta_1 + \beta_3 = 0$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	0.004	0.015

Note: This table uses an alternative measure to control for cross-industry spillover effects. Different from Table 2 of the main text, we capture the strength of cross-industry spillover effects using $\ln(1 + \mathcal{D}_{i,t})$, which is the natural log of 1 plus the average amount of property damage (in millions of dollars) caused by major natural disasters in year t across industries that are connected to firm i's industry through competition networks. The regression specification is: $Y_{i,t} = \beta_1 Treat_{i,t} \times Post_{i,t} + \beta_2 Treat_{i,t} + \beta_3 Post_{i,t} + \beta_4 \ln(1 + \mathcal{D}_{i,t}) + \theta_i + \delta_t + \varepsilon_{i,t}$. The definition of the dependent and other independent variables are explained in Table 2 of the main text. The sample of this table spans from 1994 to 2018. Standard errors are clustered at the firm level. We include t-statistics in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table OA.5: Require control firms to be untreated throughout the event window in the staggered DID regressions.

	(1)	(2)	(3)	(4)	(5)	(6)
	$Risk\_of\_Failure_{i,t}$		$Dist\_to\_Default_{i,t}$		$Profit\_Margin_{i,t}$	
$Treat_{i,t} \times Post_{i,t}$	0.032***	0.040***	−0.124**	−0.129**	−0.006	−0.004
	[2.829]	[3.323]	[−2.217]	[−2.137]	[−1.035]	[−0.696]
$Post_{i,t}$	0.045***	0.043***	−0.112***	−0.121***	−0.011**	−0.011**
	[5.060]	[4.529]	[−3.050]	[−3.001]	[−2.407]	[−2.244]
$\ln(1+n(\mathcal{C}_{i,t}))$	0.013		−0.079**		−0.007**	
	[1.473]		[−2.116]		[−2.047]	
$\ln(1+\mathcal{D}_{i,t})$		0.011***		−0.038***		−0.005***
		[3.252]		[−2.687]		[−2.915]
Cohort × firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	122887	111736	97728	88865	121716	111151
R-squared	0.720	0.732	0.803	0.811	0.845	0.847
Test p-value: $\beta_1 + \beta_3 = 0$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	0.004	0.016

Note: In this table, we require control firms to be untreated throughout the event window (i.e., year $t-2$ to year $t+1$) in the staggered DID regressions. We include the treated cohort × firm fixed effects ($\theta_{g,i}$) in the regression, which spans the indicator variable for the treated firms (i.e., $Treat_{i,t}$). The regression specification is: $Y_{i,t} = \beta_1 Treat_{i,t} \times Post_{i,t} + \beta_3 Post_{i,t} + \beta_4 \ln(1+n(\mathcal{C}_{i,t})) + \theta_{g,i} + \delta_t + \varepsilon_{i,t}$ and $Y_{i,t} = \beta_1 Treat_{i,t} \times Post_{i,t} + \beta_3 Post_{i,t} + \beta_4 \ln(1+\mathcal{D}_{i,t}) + \theta_{g,i} + \delta_t + \varepsilon_{i,t}$. The definition of the dependent and other independent variables are explained in Table 2 of the main text and in Table OA.4. The sample of this table spans from 1994 to 2018. Standard errors are clustered at the firm level. We include t-statistics in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table OA.6: Net profit margin.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$NProfit\_Margin_{i,t}$		$NProfit\_Margin_{i,t}$		$NProfit\_Margin_{i,t}$		$NProfit\_Margin_{i,t}$	
Sample	Full sample		High entry barrier	Low entry barrier	Bad economic condition	Good economic condition	High financial constraint	Low financial constraint
$Treat_{i,t} \times Post_{i,t}$	−0.003	−0.003	−0.004	−0.005	0.001	−0.009	0.004	−0.000
	[−0.717]	[−0.736]	[−0.478]	[−1.148]	[0.082]	[−1.495]	[0.391]	[−0.068]
$Treat_{i,t}$	0.002	0.002	−0.000	0.006	0.004	0.003	−0.007	0.010*
	[0.498]	[0.505]	[−0.030]	[1.233]	[0.512]	[0.474]	[−0.765]	[1.810]
$Post_{i,t}$	−0.007***	−0.006**	−0.017***	0.005*	−0.025***	0.015***	−0.034***	0.007**
	[−2.119]	[−2.013]	[−3.052]	[1.770]	[−4.696]	[3.966]	[−3.709]	[2.111]
$\ln(1+n(\mathcal{C}_{i,t}))$		−0.006*	−0.024***	0.006	−0.012***	−0.003	−0.020***	−0.007*
		[−1.800]	[−3.910]	[1.623]	[−2.649]	[−1.049]	[−2.628]	[−1.811]
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	135468	135468	64714	70729	66698	64811	32927	61835
R-squared	0.778	0.778	0.750	0.832	0.808	0.796	0.771	0.820
Test p-value: $\beta_1 + \beta_3 = 0$	0.002	0.003	$<10^{-3}$	0.935	$<10^{-3}$	0.188	$<10^{-3}$	0.117

Note: This table examines the within-industry spillover effects in net profit margin following major natural disasters. The regression specification is: $NProfit\_Margin_{i,t} = \beta_1 Treat_{i,t} \times Post_{i,t} + \beta_2 Treat_{i,t} + \beta_3 Post_{i,t} + \beta_4 \ln(1+n(\mathcal{C}_{i,t})) + \theta_i + \delta_t + \varepsilon_{i,t}$. Net profit margins ($NProfit\_Margin_{i,t}$) are computed as the difference between sales and total costs of operating the firm (i.e., sales − cost of goods sold − selling, general and administrative expenses − depreciation − interest expenses) divided by sales. Columns (1) and (2) present results in the full sample. Columns (3) and (4) present results from DID analysis in industries with high entry barriers (top tertile) and low entry barriers (middle and bottom tertiles), respectively. The entry barrier of a four-digit SIC industry is measured by the sales-weighted average of fixed assets across firms in this industry. Columns (5) and (6) present results in industries with good economic conditions (top half) and bad economic conditions (bottom half) prior to the natural disasters, respectively. The economic condition of a four-digit SIC industry is measured by the change of the return on assets (ROA) in the industry from the previous year. Columns (7) and (8) present results in industries with high financial constraint (top tertile) and low financial constraint (middle and bottom tertiles) prior to the natural disasters. The financial constraint of a four-digit SIC industry is measured by the sales-weighted average of the delay investment score in the industry (Hoberg and Maksimovic, 2015). We sort industries into groups based on the industry-level entry barriers, economic conditions, and financial constraints 1 year prior to natural disaster shocks. The sample spans from 1994 to 2018 in Columns (1) to (6), while it spans from 1998 to 2016 in Columns (7) to (8) due to shorter sample period of the delay investment score. Standard errors are clustered at the firm level. We include t-statistics in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table OA.7: Require the matched competitors to be outside of the affected states or far from the disaster area.

Panel A: Matched non-treated firms outside of the affected states						
	(1)	(2)	(3)	(4)	(5)	(6)
	$Risk\_of\_Failure_{i,t}$		$Dist\_to\_Default_{i,t}$		$Profit\_Margin_{i,t}$	
$Treat_{i,t} \times Post_{i,t}$	0.024^{**}	0.024^{**}	-0.070	-0.071	-0.000	-0.000
	[2.323]	[2.344]	[−1.347]	[−1.371]	[−0.042]	[−0.060]
$Treat_{i,t}$	-0.010	-0.010	0.071	0.071	0.000	0.000
	[−0.981]	[−0.987]	[1.392]	[1.402]	[0.003]	[0.010]
$Post_{i,t}$	0.056^{***}	0.055^{***}	-0.134^{***}	-0.127^{***}	-0.008^{**}	-0.008^{**}
	[7.852]	[7.751]	[−4.009]	[−3.844]	[−2.261]	[−2.147]
$\ln(1+n(\mathcal{C}_{i,t}))$		0.018^{**}		-0.075^{**}		-0.007^{**}
		[2.308]		[−2.024]		[−2.142]
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	131591	131591	106101	106101	130519	130519
R-squared	0.598	0.598	0.670	0.670	0.742	0.742
Test p-value: $\beta_1 + \beta_3 = 0$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	0.005	0.008
Panel B: Matched non-treated firms far from the disaster area (i.e., $\geq$ 100 miles)						
	(1)	(2)	(3)	(4)	(5)	(6)
	$Risk\_of\_Failure_{i,t}$		$Dist\_to\_Default_{i,t}$		$Profit\_Margin_{i,t}$	
$Treat_{i,t} \times Post_{i,t}$	0.021	0.021	-0.089	-0.089	0.001	0.001
	[1.524]	[1.537]	[−1.346]	[−1.360]	[0.143]	[0.140]
$Treat_{i,t}$	-0.018	-0.018	0.113^{*}	0.114^{*}	0.000	0.000
	[−1.340]	[−1.360]	[1.681]	[1.703]	[0.009]	[0.014]
$Post_{i,t}$	0.068^{***}	0.067^{***}	-0.154^{***}	-0.145^{***}	-0.017^{*}	-0.017^{*}
	[5.934]	[5.810]	[−3.182]	[−3.015]	[−1.840]	[−1.829]
$\ln(1+n(\mathcal{C}_{i,t}))$		0.025^{**}		-0.103^{**}		-0.005
		[2.268]		[−2.297]		[−0.702]
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	104938	104938	84697	84697	104064	104064
R-squared	0.623	0.623	0.685	0.686	0.760	0.760
Test p-value: $\beta_1 + \beta_3 = 0$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	0.007	0.008

Note: This table tests the demand commonality channel. In panel A, we perform DID analysis by requiring the headquarters and the major establishments of the matched competitors to be outside of the states affected by major natural disasters in a given year. In panel B, we perform DID analysis by requiring the headquarters and the major establishments of the matched competitors to be more than 100 miles away from any zip code negatively affected by major natural disaster in a given year. The regression specification and the definition of the dependent and independent variables are explained in Table 2 of the main text. The merged sample of this table spans from 1994 to 2018. Standard errors are clustered at the firm level. We include t-statistics in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

3.4 Excluding Alternative Explanations for the Within-Industry Spillovers

The observed spillover effects of adverse shocks on competitors' profit margins, output prices, and distress risk within the same industry may not be driven solely by market competition mechanisms; they could also arise from other economic linkages that overlap with competitive relationships within the same industry. Here, we examine alternative spillover channels that could drive the observed spillover effects. These alternative channels include: (i) negative demand shocks from concurrent disasters, (ii) production network connections, (iii) shared creditors, and (iv) common blockholders. In the main text, we have already evaluated channels (i) and (ii). In this section, we examine channels (iii) and (iv) and control for all four alternative channels simultaneously.

Table OA.8: Testing the lender commonality channel.

	(1)	(2)	(3)	(4)	(5)	(6)
	$Risk\_of\_Failure_{i,t}$		$Dist\_to\_Default_{i,t}$		$Profit\_Margin_{i,t}$	
$Treat_{i,t} \times Post_{i,t}$	0.035**	0.035**	−0.171**	−0.172**	0.001	0.001
	[2.129]	[2.152]	[−2.291]	[−2.317]	[0.115]	[0.111]
$Treat_{i,t}$	−0.000	−0.000	0.067	0.067	−0.003	−0.003
	[−0.013]	[−0.010]	[0.982]	[0.987]	[−0.683]	[−0.691]
$Post_{i,t}$	0.063***	0.059***	−0.143***	−0.129***	−0.012*	−0.011*
	[5.547]	[5.312]	[−3.032]	[−2.794]	[−1.925]	[−1.809]
$Lender\_Exposure_{i,t-1}$	0.142**	0.139**	0.085	0.088	−0.002	−0.002
	[2.098]	[2.056]	[0.278]	[0.287]	[−0.087]	[−0.058]
$\ln(1+n(\mathcal{C}_{i,t}))$		0.048***		−0.152***		−0.014***
		[4.132]		[−2.988]		[−3.385]
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	50575	50575	46842	46842	50788	50788
R-squared	0.619	0.619	0.704	0.704	0.752	0.752
Test p-value: $\beta_1 + \beta_3 = 0$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	0.007	0.017

Note: This table tests the lender commonality channel. In the DID analysis, we require the matched competitors to share no common lenders with the treated firms. We also control for firms' exposure to natural disasters through lenders ($Lender\_Exposure_{i,t-1}$). To identify the borrower-lender relationship and construct $Lender\_Exposure_{i,t-1}$, we use the LPC DealScan database in two steps. First, we determine each lender l's exposure to natural disasters in year t. We calculate this by dividing the outstanding loans issued by lender l from $t-5$ to $t-1$ to firms that experienced natural disasters in year t by the total amount of outstanding loans issued by lender l from $t-5$ to $t-1$. We focus on loans issued in the preceding 5-year window, following the literature (e.g., Bharath et al., 2007). Second, for each firm i, we compute $Lender\_Exposure_{i,t-1}$ by averaging the lender-level exposure across all lenders of this firm. We weight the average based on the amount of outstanding loans borrowed from different lenders. The regression specification and the definition of the dependent and independent variables are explained in Table 2 of the main text. The merged sample of this table spans from 1994 to 2018. Standard errors are clustered at the firm level. We include t-statistics in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

We show that these alternative channels are unlikely to account for the observed within-industry spillover effects.

Common Creditors. We investigate the potential role of common creditors as an alternative explanation for our observed within-industry spillover effects. This alternative explanation posits that non-treated industry competitors may borrow from lenders with heavy exposure to treated firms experiencing major local natural disasters, who then curtail credit following the disasters. As a result, these unaffected competitors can suffer from suppressed profit margins. They also face heightened distress risk even though they are not directly affected by the major local natural disasters.

To test this possibility, we require the matched competitors to share no common lenders with the treated firms in the DID analysis. We also control for firms' exposure to natural disasters through lenders ($Lender\_Exposure_{i,t-1}$). We identify the borrower-lender relationship using the LPC DealScan database and construct $Lender\_Exposure_{i,t-1}$ in two steps. First, we identify each lender l's exposure to natural disasters in year t, which is the outstanding loans issued by lender l from $t-5$ to $t-1$ to firms that experience natural disasters in year t normalized by the total amount of outstanding loans issued by lender l from $t-5$ to $t-1$.[6] Second, for each firm i, we

[6]We focus on loans issued in the preceding 5-year window following the literature (e.g., Bharath et al., 2007). When there is more than one lender funding a loan, we focus on the lead lenders following previous studies (e.g., Schwert,

Table OA.9: Testing the institutional blockholder commonality channel.

	(1)	(2)	(3)	(4)	(5)	(6)
	$Risk\_of\_Failure_{i,t}$		$Dist\_to\_Default_{i,t}$		$Profit\_Margin_{i,t}$	
$Treat_{i,t} \times Post_{i,t}$	0.027**	0.027**	−0.102**	−0.103**	−0.001	−0.001
	[2.428]	[2.444]	[−1.963]	[−1.984]	[−0.258]	[−0.275]
$Treat_{i,t}$	−0.020**	−0.020**	0.132***	0.133***	0.001	0.001
	[−1.963]	[−1.968]	[2.579]	[2.591]	[0.313]	[0.319]
$Post_{i,t}$	0.055***	0.054***	−0.111***	−0.104***	−0.009**	−0.008**
	[7.458]	[7.403]	[−3.427]	[−3.265]	[−2.138]	[−2.054]
$\ln(1+n(\mathcal{C}_{i,t}))$		0.015*		−0.079**		−0.007*
		[1.764]		[−2.132]		[−1.751]
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	125488	125488	100840	100840	124472	124472
R-squared	0.593	0.593	0.663	0.663	0.755	0.755
Test p-value: $\beta_1 + \beta_3 = 0$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	0.005	0.007

Note: This table tests the institutional blockholder commonality channel. In the DID analysis, we require the matched competitors to share no common institutional blockholders with the treated firms. Institutional blockholders are defined as 13F institutions that hold 5% or more of the firm's market capitalization. The merged sample of this table spans from 1994 to 2018. Standard errors are clustered at the firm level. We include t-statistics in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

compute $Lender\_Exposure_{i,t-1}$ by averaging the lender-level exposure across all lenders of the firm. The average is weighted based on the amount of outstanding loans from different lenders. As shown in Table OA.8, our findings remain robust after controlling for $Lender\_Exposure_{i,t-1}$ and removing the matched competitors that share any common lender with the treated firms, suggesting that the common creditor channel is unlikely to be the primary driver of the observed within-industry spillover effects.[7]

Common Institutional Blockholders. We investigate the potential role of common blockholders as an alternative explanation for our observed within-industry spillover effects. This mechanism suggests that the blockholders of firms, such as mutual funds, may undergo fire sales when the firms experience major local natural disasters (e.g., Coval and Stafford, 2007). Consequently, if these blockholders hold a significant number of shares in the unaffected industry competitors of the treated firms, it may lead to a decline in the stock prices of these competitors and an increase in their distress risk.

To test this hypothesis, we use 13F institutional holdings data to identify matched competitors that share no common blockholders with the treated firms in our DID analysis. Following prior research (e.g., Hadlock and Schwartz-Ziv, 2019), we define a firm's blockholders as owners who hold 5% or more of the firm's market capitalization. Even after excluding matched firms with common blockholders, the within-industry spillover effects remain robust, suggesting that this

2018; Chodorow-Reich and Falato, 2021).

[7] Because DealScan data are mainly collected from commitment letters and credit agreements drawn from SEC filings, the database mainly covers medium to large-size loans. We limit our analysis in Table OA.8 to the firms covered by the DealScan data because we cannot accurately measure lender exposure for the firms outside of the DealScan universe.

channel is unlikely to be the primary driver of our findings. Further details are available in Table OA.9.

Controlling for All Alternative Channels Simultaneously. In Table OA.10, we examine the within-industry spillover effects by controlling for multiple alternative channels simultaneously. For each treated firm, we match it with up to five non-treated competitors in the same four-digit SIC industry. We construct a set of indicator variables to label the matched competitors that share common demand with the treated firms ($Disaster\_Demand_{i,t}$), that are connected to the treated firms through production networks ($Production\_Network_{i,t}$), that share common lenders with the treated firms ($Common\_Lender_{i,t}$), and that share common blockholders with the treated firms ($Common\_Blockholder_{i,t}$). We then add these dummies and their interactions with the $Post_{i,t}$ term to regression specification (4.3), together with controls for firms' exposure to natural disasters through lenders ($Lender\_Exposure_{i,t-1}$). As shown in Table OA.10, the within-industry spillover effects captured by the coefficient for $Post_{i,t}$ remain robust after controlling for all four alternative channels simultaneously.

3.5 Additional Results for the Within-Industry Spillover Effects

Within-Industry Spillovers in Covenant Strictness and Investment. In the main text, we document the within-industry spillover effects in firms' distress risk. In line with this finding, columns (1) to (4) in Panel A of Table OA.11 show that, if a firm is hit by a major local natural disaster shock, the covenant strictness of its unaffected competitors increases significantly.

We use Thomson Reuters LPC DealScan syndicated loan data to construct a firm-level covenant strictness measure at a quarterly frequency. The DealScan database contains comprehensive historical information on loan characteristics, such as borrower names, lender names, pricing, start dates, end dates, and loan purposes. The loan characteristics are compiled from filings of the US Securities and Exchange Commission (SEC) and other resources. The DealScan database covers between 50% and 75% of commercial loans in the US. We merge borrowers in DealScan to Compustat-CRSP based on the link table built by Chava and Roberts (2008). We merge lenders in DealScan to Compustat-CRSP based on the link table built by Schwert (2018).

We construct a firm-level covenant strictness measure at a quarterly frequency using a method similar to Murfin (2012). This strictness measure captures the probability that the firm might breach one of its covenant terms in the next period and is a quarterly ex-ante measure of technical default. Specifically, we focus on twelve covenant ratios on DealScan: (1) minimum fixed charge coverage ratio, (2) minimum cash interest coverage ratio, (3) minimum debt service coverage ratio, (4) minimum interest coverage ratio, (5) maximum debt to cash flow, (6) maximum senior debt to cash flow, (7) maximum leverage ratio, (8) minimum tangible net worth, (9) minimum net worth, (10) maximum debt to tangible net worth, (11) maximum debt to equity ratio, and (12) minimum current ratio. The first six covenant ratios are earnings-based while the last six are capital-based.

Table OA.10: Controlling for all alternative channels simultaneously.

	(1)	(2)	(3)	(4)	(5)	(6)
	$Risk\_of\_Failure_{i,t}$		$Dist\_to\_Default_{i,t}$		$Profit\_Margin_{i,t}$	
$Treat_{i,t} \times Post_{i,t}$	0.023*	0.023*	−0.039	−0.041	0.001	0.001
	[1.739]	[1.760]	[−0.610]	[−0.629]	[0.471]	[0.446]
$Treat_{i,t}$	−0.026*	−0.026*	0.081	0.082	−0.004	−0.003
	[−1.949]	[−1.958]	[1.218]	[1.233]	[−1.174]	[−1.163]
$Post_{i,t}$	0.053***	0.052***	−0.165***	−0.158***	−0.006**	−0.006**
	[4.898]	[4.811]	[−3.191]	[−3.075]	[−2.150]	[−2.039]
$Post_{i,t} \times Disaster\_Demand_{i,t}$	−0.010	−0.010	0.065	0.065	0.004	0.004
	[−0.744]	[−0.743]	[1.030]	[1.035]	[1.142]	[1.142]
$Post_{i,t} \times Production\_Network_{i,t}$	0.015	0.015	−0.011	−0.011	−0.003	−0.003
	[1.302]	[1.297]	[−0.199]	[−0.202]	[−1.402]	[−1.402]
$Post_{i,t} \times Common\_Lender_{i,t}$	0.005	0.005	0.039	0.040	0.007	0.007
	[0.360]	[0.351]	[0.520]	[0.528]	[1.559]	[1.563]
$Post_{i,t} \times Common\_Blockholder_{i,t}$	0.014	0.014	0.022	0.020	0.001	0.001
	[1.118]	[1.138]	[0.359]	[0.340]	[0.188]	[0.169]
$Disaster\_Demand_{i,t}$	−0.017	−0.017	0.001	0.001	−0.003	−0.003
	[−1.249]	[−1.256]	[0.015]	[0.019]	[−0.901]	[−0.895]
$Production\_Network_{i,t}$	−0.006	−0.005	−0.010	−0.010	0.002	0.002
	[−0.563]	[−0.537]	[−0.172]	[−0.189]	[0.858]	[0.829]
$Common\_Lender_{i,t}$	0.040***	0.040***	−0.244***	−0.243***	−0.013***	−0.013***
	[2.931]	[2.921]	[−3.275]	[−3.263]	[−3.767]	[−3.754]
$Common\_Blockholder_{i,t}$	−0.029***	−0.030***	0.082	0.083	0.002	0.002
	[−2.808]	[−2.830]	[1.487]	[1.514]	[0.837]	[0.859]
$Lender\_Exposure_{i,t-1}$	0.102**	0.103**	−0.231	−0.233	−0.011	−0.011
	[2.177]	[2.187]	[−0.988]	[−0.997]	[−1.251]	[−1.268]
$\ln(1 + n(\mathcal{C}_{i,t}))$		0.017**		−0.081**		−0.005***
		[2.294]		[−2.226]		[−3.156]
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	136465	136465	110883	110883	135322	135322
R-squared	0.598	0.598	0.667	0.667	0.793	0.793
Test p-value: $\beta_1 + \beta_3 = 0$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	0.002	0.003

Note: This table examines the within-industry spillover effects by controlling for all four alternative channels simultaneously. For each treated firm, we match it with up to five non-treated competitors in the same four-digit SIC industry, based on the values of three matching variables (i.e., firm asset size, tangibility, and age) prior to natural disaster shocks using the shortest distance method. $Disaster\_Demand_{i,t}$ is an indicator variable that equals one for the matched competitors that: i) are located within 100 miles from any zip code negatively affected by major natural disasters in the focal year for which they serve the match, or ii) have any business customers or individual consumers located in the areas affected by the natural disasters. We identify firms' business customers using Compustat customer segment data and Factset Revere data. We identify firms' retail customers based on the household-level financial transaction data constructed by Baker, Baugh and Sammon (2020). $Production\_Network_{i,t}$ is an indicator variable that equals one for the matched competitors that: i) are suppliers or customers of the treated firms, ii) share common suppliers or customers with the treated firms, or iii) have high vertical relatedness scores (Frésard, Hoberg and Phillips, 2020) with the treated firms (i.e., within top 10% of all firm pairs). $Common_Lender_{i,t}$ is an indicator variable that equals one for the matched competitors that share common lenders with the treated firms. $Common\_Blockholder_{i,t}$ is an indicator variable that equals one for the matched competitors that share common institutional blockholders with the treated firms. $Lender\_Exposure_{i,t-1}$ captures firms' exposure to natural disasters through lenders as explained in Table OA.8. The merged sample of this table spans from 1994 to 2018. Standard errors are clustered at the firm level. We include t-statistics in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table OA.11: Within-Industry Spillovers in Covenant Strictness and Investment

	Panel A: within-industry spillover effects					
	(1)	(2)	(3)	(4)	(5)	(6)
	$Strictness\_all_{i,t}$		$Strictness\_earnings_{i,t}$		$Investment_{i,t}$	
$Treat_{i,t} \times Post_{i,t}$	−0.004	−0.004	0.003	0.003	−0.032*	−0.032*
	[−0.723]	[−0.707]	[0.450]	[0.452]	[−1.786]	[−1.790]
$Treat_{i,t}$	0.008	0.008	0.001	0.001	0.044**	0.045**
	[1.427]	[1.417]	[0.097]	[0.095]	[2.237]	[2.247]
$Post_{i,t}$	0.017***	0.017***	0.013***	0.013***	−0.030***	−0.032***
	[4.117]	[4.025]	[3.164]	[3.125]	[−2.844]	[−2.942]
$\ln(1+n(\mathcal{C}_{i,t}))$		0.005		0.002		0.016
		[1.204]		[0.416]		[1.100]
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	42117	42117	31987	31987	133174	133174
R-squared	0.636	0.636	0.663	0.663	0.817	0.817
Test p-value: $\beta_1 + \beta_3 = 0$	0.005	0.006	0.001	0.001	$<10^{-3}$	$<10^{-3}$

	Panel B: Summary statistics of the firm-year panel							
	Obs. #	Mean	Median	SD	p10th	p25th	p75th	p90th
$Strictness\_all_{i,t}$	45053	0.483	0.472	0.269	0.113	0.274	0.690	0.865
$Strictness\_earnings_{i,t}$	33178	0.462	0.452	0.276	0.079	0.243	0.673	0.856
$Investment_{i,t}$	90884	0.959	0.326	1.329	0.080	0.157	1.013	3.846

Notes. This table examines the within-industry spillover effects of covenant strictness and investment following major local natural disasters. Panel A of this table reports the results from the DID analysis. $Strictness\_all_{i,t}$ is the covenant strictness computed from all types of covenants, representing the probability of a firm breaching one of its covenant terms in the next time period. $Strictness\_earnings_{i,t}$ is the covenant strictness computed from earnings-based covenants, representing the probability of a firm breaching one of its performance covenant terms in the next time period. $Strictness\_all_{i,t}$ and $Strictness\_earnings_{i,t}$ in year t refer to the strictness in the last quarter. $Investment_{i,t}$ is the sum of capital expenditures and R&D expenses scaled by the start of period net property, plant, and equipment. Panel B presents summary statistics for the firm-year panel. The samples span from 1994 to 2018. Standard errors are clustered at the firm level. We include t-statistics in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

If a firm has multiple loan packages outstanding, we compute the most restrictive covenant for each covenant ratio across all outstanding packages in any given quarter. If a covenant type is not present in any of the firm's active packages, we record a missing value for that covenant and compute strictness using the remaining covenants. We merge borrowers in DealScan with Compustat-CRSP using the link table developed by Chava and Roberts (2008). To compute the annualized financial ratios associated with these covenants, we use Compustat quarterly data and follow the definitions outlined in Demerjian and Owens (2016).

To construct our strictness measure, we follow the approach of Murfin (2012) by assuming that quarterly changes in the financial measures underlying the covenants follow a multivariate lognormal distribution. Specifically, for a given firm i, the log-growth of the financial ratios $\mathbf{r}$ between quarter t and quarter $t+1$ is:

$$\ln(\mathbf{r}_{i,t+1}) - \ln(\mathbf{r}_{i,t}) = \mu_i + \varepsilon_{i,t+1},$$

where $\mathbf{r}$ is a $N \times 1$ vector of financial ratios, μ_i is a $N \times 1$ vector of firm-specific constants, and $\varepsilon_{i,t+1} \sim N_N(0, \Sigma)$.

Suppose $\underline{\mathbf{r}}$ is an $N \times 1$ vector of active covenants. If the covenant for the n^{th} element of $\mathbf{r}$ is a

mininum allowable ratio, meaning that control rights are allocated to the lender if $r_n < \underline{r}_n$, (or, equivalently, if $\ln(r_n) < \ln(\underline{r}_n)$), then strictness is the probability that the firm will breach one of its covenant terms in the quarter $t+1$, which is defined as:

$$Strictness_{i,t} = 1 - \Phi\left(\widehat{\ln(\mathbf{r}_{i,t+1})} - \ln(\underline{\mathbf{r}})\right),$$

where Φ is the multivariate standard normal cumulative distribution function with mean $\mathbf{0}$ and variance Σ, and $\underline{\mathbf{r}}$ is an $N \times 1$ vector of active covenants. Similarly, for covenants with maximum allowable ratios, the strictness measure will be $1 - \Phi\left(\ln(\bar{\mathbf{r}}) - \widehat{\ln(\mathbf{r}_{i,t+1})}\right)$ instead. We compute two sets of strictness measures based on all twelve covenant ratios and the six earnings-based covenant ratios, respectively.

Consistent with prior research,[8] our findings indicate that an escalation in distress risk corresponds to a decline in corporate investment. This suggests that within-industry spillovers in distress risk lead to a decrease in investment for firms when their industry rivals face increased distress risk. Specifically, as depicted in columns (5) to (6) of Panel A in Table OA.11, firms that encounter major local natural disasters demonstrate a significant reduction in investment, leading their industry competitors to similarly cut investment levels despite not being directly affected by the natural disasters.

Changes of Inventory, Cash Holdings, and Market Shares. We examine the changes in firms' inventory, cash holdings, and market shares following the occurrences of natural disasters. Table OA.12 presents the findings. The total treatment effects are captured by $\beta_1 + \beta_3$ and the spillover effects are captured by β_3. We find that both affected firms and their industry competitors significantly reduce their inventory levels following the natural disasters. Together with the results documented previously regarding the reduction in profit margins, our findings support the hypothesis that adverse shocks intensify market competition, leading firms to sell their inventory holdings at discounted prices. Moreover, we find that the cash holdings of both affected firms and their industry competitors decrease following the natural disasters, consistent with the spillover results documented previously. On the other hand, we do not observe changes in market shares for either affected firms or their industry competitors, suggesting that the affected firms do not lose market share after the natural disasters. Our findings regarding changes in inventory align with Kim (2021), who shows that firms negatively impacted by the Lehman Brothers failure decrease inventory levels and increase their cash holdings. However, we observe reductions in cash holdings instead of an increase, likely because the market competition induced by natural disasters is associated with activities that require substantial cash expenditure. This cash expenditure seems to outweigh the cash accumulation resulting from inventory sales.

[8] Covenant violations, featuring the transfer of control rights, often lead to decrease in investment (e.g., Chava and Roberts, 2008). In addition, managers are likely to avoid covenant violations by cutting investment (e.g., Graham, Harvey and Rajgopal, 2005).

Table OA.12: Changes of Inventory, Cash Holdings, and Market Shares

	Panel A: within-industry spillover effects					
	(1)	(2)	(3)	(4)	(5)	(6)
	$Inventory_{i,t}$		$Cash_{i,t}$		$Market\ Share_{i,t}$	
$Treat_{i,t} \times Post_{i,t}$	−0.001	−0.001	0.001	0.001	0.000	0.000
	[−0.623]	[−0.617]	[0.508]	[0.517]	[0.825]	[0.608]
$Treat_{i,t}$	−0.001	−0.001	0.001	0.001	0.001**	0.001**
	[−0.584]	[−0.587]	[0.449]	[0.445]	[2.332]	[2.392]
$Post_{i,t}$	−0.002***	−0.002***	−0.007***	−0.007***	−0.000	0.000
	[−3.571]	[−3.661]	[−4.777]	[−4.862]	[−1.172]	[0.768]
$\ln(1+n(\mathcal{C}_{i,t}))$		0.001		0.001		−0.005***
		[0.675]		[0.713]		[−12.462]
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	129290	129290	132792	132792	124564	124564
R-squared	0.806	0.806	0.738	0.738	0.815	0.817
Test *p*-value: $\beta_1 + \beta_3 = 0$	0.001	$<10^{-3}$	0.004	0.004	0.968	0.239

	Panel B: Summary statistics of the firm-year panel							
	Obs. #	Mean	Median	SD	$p10^{th}$	$p25^{th}$	$p75^{th}$	$p90^{th}$
$Inventory_{i,t}$	88881	0.119	0.097	0.114	0	0.017	0.179	0.290
$Cash_{i,t}$	91306	0.238	0.100	0.570	0.009	0.029	0.288	0.596
$Market\ Share_{i,t}$	90973	0.111	0.013	0.222	0	0.001	0.092	0.362

Notes. This table examines changes of inventory, cash holdings and market shares following major local natural disasters. Panel A of this table reports the results from the DID analysis. $Inventory_{i,t}$ is the level of inventory scaled by the lagged sales. $Cash_{i,t}$ is the level of cash and cash equivalents scaled by the lagged asset size. $Market\ Share_{i,t}$ is sales of the firm scaled by the total sales of the industry. Panel B of this table presents summary statistics for the firm-year panel. The sample of this table spans from 1994 to 2018. Standard errors are clustered at the firm level. We include *t*-statistics in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Changes of Firm Values to Natural Disaster Shocks. We have documented the within-industry spillover of distress risk, which occur through rivals' profit margin competition. As emphasized by previous research, higher distress risk, characterized by an increased likelihood of covenant violation and debt default, heightens the risk of transferring control rights from shareholders to secured creditors, thereby damaging shareholders' value. Given this, we ask whether the within-industry spillover effects also manifest for firms' equity values. To address this, we examine the responses of firms' equity values to occurrences of major local natural disasters with a very short time window, similar in spirit to event studies. In Panel A of Table OA.13, we present the stock returns of treated firms around major local natural disasters. We find that the average equity value of the treated firms decreases by 2.01% in the month of major local natural disasters. Additionally, we observe a slight decline in firms' equity value in the month prior to natural disasters, which is statistically insignificant but likely due to the partial forecast of some natural disasters (e.g., hurricanes) one month ahead.

More importantly, in Panel B of Table OA.13, we report the stock returns of the matched non-treated competitors over the month prior to or during the major local natural disasters. Our findings indicate that the firm values of the matched non-treated competitors also drop significantly during the month in which their rivals are affected by major local natural disasters, underscoring the importance of within-industry spillover effects for firm values.

Table OA.13: Changes of Firm Values to Natural Disaster Shocks

Panel A: Treated firms				
	N	Mean	Z-score	95% CI
Return of the month prior to disasters (Ret_{-1})	8286	−0.29%	−1.47	[−0.67%, 0.10%]
Return of the month of disasters (Ret_0)	8284	−2.01%	−9.85	[−2.41%, −1.61%]
$Ret_0 + Ret_{-1}$	8273	−2.29%	−7.98	[−2.86%, −1.73%]
Panel B: Matched non-treated firms				
	N	Mean	Z-score	95% CI
Return of the month prior to disasters (Ret_{-1})	32389	−0.27%	−1.51	[−0.62%, 0.14%]
Return of the month of disasters (Ret_0)	32380	−1.94%	−14.53	[−2.20%, −1.68%]
$Ret_0 + Ret_{-1}$	32337	−2.32%	−12.37	[−2.69%, −1.96%]

Notes. This table shows the changes of firm values to natural disaster shocks for both the treated firms (panel A) and the matched non-treated firms (panel B). We report the mean stock returns in the month prior to disasters (Ret_{-1}), the mean stock returns in the month of disasters (Ret_0), and the sum of the two returns ($Ret_0 + Ret_{-1}$). We bootstrap the distribution of the returns in 1000 simulations. We report the Z-scores and the 2.5th and 97.5th estimated percentiles of the simulated distribution in brackets.

3.6 Cross-Industry Spillover Effects

We now examine the cross-industry spillover effects through the competition network that connects industries through common market leaders operating across different industries (i.e., product markets). The concept and empirical construction of this competition network were originally proposed by Chen et al. (2020). Common market leaders are firms that operate in multiple industries. According to the data, these firms are not necessarily the largest in the economy. For example, over 55% of common market leaders do not rank among the top 500 largest firms. While their levels of distress and financial constraints are slightly lower than the average firm in the economy, common market leaders exhibit a wide range of distress and financial constraint levels. Most of these firms operate in two or three industries, a pattern that remains consistent over time. This distribution suggests that diversification alone is unlikely to fully mitigate profit margin risks for common market leaders.

Given the within-industry spillover of distress risk, it is natural to ask whether the risk propagates to other industries when certain rivals are common market leaders that connect an industry with others. We would expect such an effect to be amplified when these market leaders are financially consolidated yet constrained. Specifically, in the event of an adverse idiosyncratic shock, a market leader tends to reduce its profit margins due to low collusion capacity or high incentives for liquidation sales of inventories to meet dire liquidity needs. In response, common market leaders of the same industry must defend their customer bases and prevent their rival from recovering by reducing their profit margins. Consequently, the cash flows of common market leaders decrease, making it more likely for them to default on debt or violate covenants. By the same token, if these common market leaders are financially consolidated yet constrained, they, in turn, would decrease their profit margins in the connected industries, resulting in lower profit margins and increased distress risk for their rivals in those industries.

In Section 4 of the main text, we provide some evidence for cross-industry spillover effects. In particular, Panel A of Table 2 of the main text shows that the coefficient for the cross-industry

spillover term (i.e., β_4 in equation 4.3 of the main text) is statistically significant, with the signs consistent with our predictions. In this section, using a two-stage regression analysis and various placebo tests, we further study cross-industry spillover effects by highlighting the role of the common market leaders in transmitting shocks across industries.

Baseline Regression Specifications for Cross-Industry Spillover Effects. We analyze cross-industry spillover effects in two stages. In the first stage, we estimate the spillover effects of profit margin or distress risk from standalone market leaders to common market leaders in the same industry. We use a panel dataset with each cross-section containing pairs of an industry and its common market leader. We estimate the following panel regression using observations over (j, c_j) and t:

$$Y_{c_j,t} = \alpha + \sum_{m=1}^{3} \theta_m ND\_mild_{j,t}^{(m)} + \sum_{m=1}^{3} \gamma_m ND\_severe_{j,t}^{(m)} + \varepsilon_{c_j,t}, \tag{3.1}$$

where $Y_{c_j,t}$ represents the distress risk or profit margin of the common market leader c_j that industry j shares with other industries, and the independent variables $ND\_mild_{j,t}^{(m)}$ and $ND\_severe_{j,t}^{(s)}$ are indicator variables that equal 1 if the m^{th} $(m = 1, 2, 3)$ largest firm (ranked by sales) in industry j in year t, after excluding common market leaders of industry j in year t, experiences mild and severe damage during natural disaster shocks, respectively.[9] We include both the $ND\_mild_{j,t}^{(m)}$ and $ND\_severe_{j,t}^{(m)}$ dummy variables to reflect the impact of major local natural disasters of varying magnitudes.

Our regression specification (3.1) estimates the impact of major local natural disaster shocks to the top three standalone market leaders in industry j on the distress risk or profit margin of common market leader c_j in year t. We compute the fitted value $\widehat{IdShock}_{j,t}$ as follows:

$$\widehat{IdShock}_{j,t} = \hat{\alpha} + \sum_{m=1}^{3} \hat{\theta}_m ND\_mild_{j,t}^{(m)} + \sum_{m=1}^{3} \hat{\gamma}_m ND\_severe_{j,t}^{(m)}, \tag{3.2}$$

where the fitted value $\widehat{IdShock}_{j,t}$ captures the estimated changes in the distress risk or profit margin of any common market leader c_j of industry j attributed to major local natural disaster shocks to the top three standalone market leaders in the same industry.

In the second stage, we estimate the cross-industry spillover effect based on the first-stage estimates. Specifically, for each industry i in year t, we identify every industry j that is connected to industry i through common market leaders. The collection of all such industries is denoted by $\mathcal{C}_{i,t}$. We then construct the changes in distress risk or profit margin of common market leaders in industry i attributed to major local natural disaster shocks to standalone market leaders in its peer

[9] We define $ND\_mild_{j,t}^{(m)}$ as 1 if the county in which the m^{th} largest firm is located experiences more than \$0.25 million but less than \$50 million in property losses. We define $ND\_severe_{j,t}^{(m)}$ as 1 if the county in which the m^{th} largest firm is located experiences more than \$50 million in property losses.

Table OA.14: Cross-Industry Spillover Effects in Distress Risk and Profit Margin

	Panel A: Construction of $\widehat{IdShock}_{-i,t}^{(c)}$ (first stage)		
	(1) $Risk\_of\_Failure_{c_j,t}$	(2) $Dist\_to\_Default_{c_j,t}$	(3) $Profit\_Margin_{c_j,t}$
$ND\_mild_{j,t}^{(1)}$	−0.024	0.258	−0.012*
	[−0.817]	[1.100]	[−1.694]
$ND\_severe_{j,t}^{(1)}$	0.156***	−1.277***	−0.032***
	[2.832]	[−3.189]	[−2.792]
$ND\_mild_{j,t}^{(2)}$	0.042	−0.135	−0.007
	[1.439]	[−0.636]	[−1.054]
$ND\_severe_{j,t}^{(2)}$	0.014	−0.200	−0.030***
	[0.259]	[−1.449]	[−2.749]
$ND\_mild_{j,t}^{(3)}$	0.032	0.040	0.004
	[1.114]	[0.193]	[0.651]
$ND\_severe_{j,t}^{(3)}$	0.121**	−0.927***	−0.030***
	[2.332]	[−2.706]	[−2.999]
Observations	7107	6882	7166
R-squared	0.003	0.004	0.006

	Panel B: Cross-industry spillover (second stage)					
	(1)	(2)	(3)	(4)	(5)	(6)
	$Risk\_of\_Failure_{i,t}^{(-c)}$		$Dist\_to\_Default_{i,t}^{(-c)}$		$Profit\_Margin_{i,t}^{(-c)}$	
$\widehat{IdShock}_{-i,t}^{(c)}$	0.556**	0.558**	0.393**	0.399**	0.528**	0.527**
	[2.151]	[2.151]	[1.966]	[1.990]	[2.257]	[2.265]
$\widehat{IdShock}_{-i,t}^{(c)}$		0.095**		0.005		0.824**
$\times$ $Frac\_Peers\_as\_Customers_{-i,i,t}$		[2.223]		[0.014]		[2.324]
$\widehat{IdShock}_{-i,t}^{(c)}$		−0.091**		−0.519		−0.875**
$\times$ $Frac\_Peers\_as\_Suppliers_{-i,i,t}$		[−2.201]		[−1.629]		[−2.465]
Controls for aggregate conditions	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5174	5170	5020	5016	5264	5260
R-squared	0.020	0.023	0.054	0.055	0.002	0.010
Permutation test p-value: $\beta_c = 0$	0.012	0.011	0.022	0.020	0.010	0.011

Notes. This table presents the outcomes of the two-stage estimation method for assessing cross-industry spillover effects. In Panel A, we estimate the first-stage specification as in equation (3.1). In Panel B, we utilize the fitted value of the first stage, denoted by $\widehat{IdShock}_{j,t}$ in equation (3.2), to generate the independent variable $\widehat{IdShock}_{-i,t}^{(c)}$ as their simple average across all industries connected to industry i via competition networks. We adopt the regression specification given by equation (3.4) to examine cross-industry spillovers. The variables $Frac\_Peers\_as\_Customers_{-i,i,t}$ and $Frac\_Peers\_as\_Suppliers_{-i,i,t}$ indicate the proportion of peer industries, connected to industry i through the competition network, that are also customer and supplier industries of industry i, respectively. Controls variables include various aggregate conditions, such as shocks to the AAA-10Y spread, the economic policy uncertainty index (Baker, Bloom and Davis, 2016), and the marcoeconomic uncertainty (Jurado, Ludvigson and Ng, 2015; Ludvigson, Ma and Ng, 2021). The permutation tests are one-sided tests on the null hypothesis $\beta_c = 0$ against the alternative $\beta_c > 0$. The sample period ranges from 1994 to 2018. We cluster standard errors at the industry level and present t-statistics in parentheses. Statistical significance is denoted by *, **, and ***, which correspond to the 10%, 5%, and 1% levels, respectively.

industries connected on the competition network as follows:

$$\widehat{IdShock}_{-i,t}^{(c)} \equiv \frac{1}{n(\mathcal{C}_{i,t})} \sum_{j \in \mathcal{C}_{i,t}} \widehat{IdShock}_{j,t}, \tag{3.3}$$

where variable $n(\mathcal{C}_{i,t})$ is the number of industries in set $\mathcal{C}_{i,t}$.

We then run the following panel regression using all industry-year observations on the

Table OA.15: Summary statistics for the cross-industry spillover analysis.

	Obs. #	Mean	Median	SD	p10th	p25th	p75th	p90th
$Risk\_of\_Failure_{c_j,t}$	7107	−7.826	−7.972	0.656	−8.539	−8.312	−7.484	−6.782
$Dist\_to\_Default_{c_j,t}$	6882	6.405	5.666	4.630	0.629	2.748	9.560	14.109
$Profit\_Margin_{c_j,t}$	7166	0.314	0.300	0.140	0.131	0.200	0.412	0.538
$ND\_mild^{(1)}_{j,t}$	8415	0.081	0	0.273	0	0	0	0
$ND\_severe^{(1)}_{j,t}$	8415	0.023	0	0.150	0	0	0	0
$ND\_mild^{(2)}_{j,t}$	8415	0.086	0	0.280	0	0	0	0
$ND\_severe^{(2)}_{j,t}$	8415	0.023	0	0.150	0	0	0	0
$ND\_mild^{(3)}_{j,t}$	8415	0.087	0	0.281	0	0	0	0
$ND\_severe^{(3)}_{j,t}$	8415	0.028	0	0.164	0	0	0	0
$Risk\_of\_Failure^{(-c)}_{i,t}$	5174	−7.719	−7.827	0.599	−8.404	−8.160	−7.384	−6.845
$Dist\_to\_Default^{(-c)}_{i,t}$	5020	5.966	5.484	3.635	1.480	3.240	8.225	11.462
$Profit\_Margin^{(-c)}_{i,t}$	5264	0.324	0.308	0.132	0.154	0.222	0.416	0.528
$\widehat{IdShock}^{(c)}_{-i,t}(Distress)$	5174	−7.826	−7.838	0.033	−7.838	−7.838	−7.830	−7.795
$\widehat{IdShock}^{(c)}_{-i,t}(DD)$	5020	6.407	6.453	0.249	6.318	6.453	6.453	6.515
$\widehat{IdShock}^{(c)}_{-i,t}(PM)$	5264	0.314	0.317	0.009	0.305	0.315	0.317	0.317
$Frac\_Peers\_as\_Customers_{-i,i,t}$	5260	0.025	0	0.043	0	0	0.042	0.083
$Frac\_Peers\_as\_Suppliers_{-i,i,t}$	5260	0.025	0	0.045	0	0	0.036	0.083

competition network:

$$Y^{(-c)}_{i,t} = \alpha_c + \beta_c \widehat{IdShock}^{(c)}_{-i,t} + Controls_{i,t} + \varepsilon_{i,t}, \tag{3.4}$$

where $Y^{(-c)}_{i,t}$ is the distress risk or profit margin of industry i sales-weighted across firms in industry i, excluding common market leaders in year t. Coefficient β_c, the coefficient of interest, captures how industry i's distress risk or profit margin responds to other industries' major local natural disaster shocks that propagate to industry i through common market leaders.

Baseline Results of Cross-Industry Spillover Effects. We present the cross-industry spillover analysis in Table OA.14 and the summary statistics in Table OA.15. Panel A of Table OA.14 presents the results from the first-stage regressions. We find that the common market leaders' distress risk (profit margin) is positively (negatively) associated with the major local natural disaster shocks to the top market leaders in the same industries, especially for severe natural disaster shocks. Panel B presents the second-stage estimates on the cross-industry spillover effects. The coefficient of $\widehat{IdShock}^{(c)}_{-i,t}$, denoted by β_c, is positive and statistically significant, indicating that the distress risk and profit margin of industry i are positively associated with idiosyncratic exogenous variations in the counterparts of industry i's neighbors on the competition network. The pass-through rate of cross-industry spillover effects in profit margin and distress risk is approximately 50%. Our results suggest that adverse idiosyncratic shocks in one industry can be transmitted to another through common leaders that operate in both industries. These findings are consistent with our predictions.

We further show that the cross-industry spillover results along the competition network links cannot be explained by production network externalities. Specifically, we control for the interaction between the predicted idiosyncratic shocks and the production network connectedness, measured by the fraction of peer industries connected to the focal industry i through the competition network that are also the customer industries and supplier industries of the focal industry i. As shown in Panel B of Table OA.14, the coefficient on the predicted idiosyncratic shocks remains positive and statistically significant.[10]

Concerns have been raised in some studies regarding the validity of traditional asymptotic standard error procedures for two-stage regressions. To ensure a reliable sampling distribution for the cross-industry spillover effect under the null hypothesis, we adopt a standard permutation test for regressions. Our results, presented in the last row of Table OA.14, show that the cross-industry spillover effects remain significant according to the permutation test.

Robustness of the Observed Cross-Industry Spillover Effects. A potential concern about the cross-industry spillover analysis is that we define industries at the four-digit SIC level, which makes it possible that the cross-industry spillover effects reflect the within-industry spillover effects in industries defined more broadly or reflect high product similarity across two four-digit SIC industries. To address this concern, we analyze cross-industry spillover effects beyond three-digit SIC industries. Results in Table OA.16 show that $\widehat{IdShock}^{(c)}_{-i,t}$ remains positive and statistically significant for industry spillover effects that extend outside the three-digit SIC industries.

To further alleviate the concern, we exclude competition network links between four-digit SIC industries with high TNIC pairwise similarity scores. Using TNIC pairwise similarity scores derived from Hoberg and Phillips (2010, 2016), we find that even after excluding such links, the coefficient of $\widehat{IdShock}^{(c)}_{-i,t}$ remains positive and statistically significant (Table OA.17). Specifically, we compute the TNIC pairwise similarity scores of a pair of four-digit SIC industries i and j by aggregating the firm-level pairwise similarity scores between firms in industry i and firms in industry j based on the multiplication results of the lagged sales of the two firms. The firm-level pairwise similarity scores come from Hoberg and Phillips (2010, 2016), and they are constructed based on text analysis of firms' product descriptions in their 10-K filings. We exclude the competition network links ranked in the top tertile in terms of the TNIC pairwise similarity scores each year. As shown in Table OA.17, the coefficient of $\widehat{IdShock}^{(c)}_{-i,t}$ remains positive and

[10] As shown by Columns (5) to (8) of Table OA.14, the coefficient for the interaction term between $\widehat{IdShock}^{(c)}_{-i,t}$ and $Frac\_Peers\_as\_Suppliers_{-i,i,t}$ is negative and statistically significant, suggesting that the cross-industry contagion spillover effect becomes weaker when the connected industries are also suppliers of the focal industry i. This result is not surprising because, in this situation, the connected industries' outputs are the inputs of the focal industry i. When the connected industries suffer from major local natural disasters, the resulting drop in their output prices pushes up the profit margin of the focal industry i. Although the coefficients of the interaction terms in Table OA.14 speak to the impact of the natural disaster shocks along the production network, our analysis differs from Barrot and Sauvagnat (2016) — our paper studies the spillover of the profit margin, whereas their paper focuses on the spillover of the sales growth rate.

Table OA.16: Cross-industry spillover effects outside of the three-digit SIC industries

	(1)	(2)	(3)	(4)	(5)	(6)
	$Risk\_of\_Failure_{i,t}^{(-c)}$		$Dist\_to\_Default_{i,t}^{(-c)}$		$Profit\_Margin_{i,t}^{(-c)}$	
$\widehat{IdShock}_{-i,t}^{(c)}$	0.582**	0.572**	0.475**	0.472**	0.484**	0.483**
	[2.172]	[2.117]	[2.261]	[2.238]	[1.982]	[1.982]
$\widehat{IdShock}_{-i,t}^{(c)} \times Frac\_Peers\_as\_Customers_{-i,i,t}$		0.067		−0.241		0.621
		[1.388]		[−0.575]		[1.603]
$\widehat{IdShock}_{-i,t}^{(c)} \times Frac\_Peers\_as\_Suppliers_{-i,i,t}$		−0.128***		−0.816**		−1.189***
		[−2.756]		[−2.504]		[−3.176]
Controls for aggregate conditions	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4907	4903	4774	4770	4989	4985
R-squared	0.017	0.021	0.051	0.056	0.002	0.013
Permutation test p-value: $\beta_c = 0$	0.011	0.013	0.015	0.014	0.034	0.033

Note: This table examines the cross-industry spillover effects outside of the three-digit SIC industries. The regression specification is: $Y_{i,t}^{(-c)} = \alpha_c + \beta_c \widehat{IdShock}_{-i,t}^{(c)} + Controls_{i,t} + \varepsilon_{i,t}$. We construct $\widehat{IdShock}_{-i,t}^{(c)}$ as the simple average of $\widehat{IdShock}_{j,t}$ averaged across all industries j that are connected to industry i through competition networks and are outside of the three-digit SIC industries of industry i, where $\widehat{IdShock}_{j,t}$ is the fitted value from the first-stage regression $Y_{c_j,t} = \alpha + \sum_{m=1}^{3} \theta_m ND\_mild_{j,t}^{(m)} + \sum_{m=1}^{3} \gamma_m ND\_severe_{j,t}^{(s)} + \varepsilon_{c_j,t}$. The permutation tests are one-sided tests on the null hypothesis $\beta_c = 0$ against the alternative $\beta_c > 0$. The sample spans the period from 1994 to 2018. Standard errors are clustered at the industry level. We include t-statistics in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table OA.17: Cross-industry spillover effects excluding links with high TNIC pairwise similarity scores

	(1)	(2)	(3)	(4)	(5)	(6)
	$Risk\_of\_Failure_{i,t}^{(-c)}$		$Dist\_to\_Default_{i,t}^{(-c)}$		$Profit\_Margin_{i,t}^{(-c)}$	
$\widehat{IdShock}_{-i,t}^{(c)}$	0.659**	0.644**	0.469*	0.473**	0.538**	0.531**
	[2.266]	[2.213]	[1.953]	[1.974]	[2.256]	[2.231]
$\widehat{IdShock}_{-i,t}^{(c)} \times Frac\_Peers\_as\_Customers_{-i,i,t}$		0.095*		0.044		0.570
		[1.882]		[0.098]		[1.624]
$\widehat{IdShock}_{-i,t}^{(c)} \times Frac\_Peers\_as\_Suppliers_{-i,i,t}$		−0.122***		−0.736**		−1.000***
		[−2.750]		[−2.102]		[−2.907]
Controls for aggregate conditions	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4098	4095	3984	3981	4188	4185
R-squared	0.020	0.024	0.053	0.056	0.002	0.011
Permutation test p-value: $\beta_c = 0$	0.012	0.013	0.030	0.029	0.016	0.017

Note: This table examines the cross-industry spillover effects excluding competition network links among four-digit SIC industries with high TNIC pairwise similarity scores. The regression specification is: $Y_{i,t}^{(-c)} = \alpha_c + \beta_c \widehat{IdShock}_{-i,t}^{(c)} + Controls_{i,t} + \varepsilon_{i,t}$. We construct $\widehat{IdShock}_{-i,t}^{(c)}$ as the simple average of $\widehat{IdShock}_{j,t}$ averaged across industries j that are connected to industry i through competition networks with relatively low TNIC pairwise similarity scores (i.e., middle and bottom tertiles), where $\widehat{IdShock}_{j,t}$ is the fitted value from the first-stage regression $Y_{c_j,t} = \alpha + \sum_{m=1}^{3} \theta_m ND\_mild_{j,t}^{(m)} + \sum_{m=1}^{3} \gamma_m ND\_severe_{j,t}^{(s)} + \varepsilon_{c_j,t}$. The permutation tests are one-sided tests on the null hypothesis $\beta_c = 0$ against the alternative $\beta_c > 0$. The sample spans the period from 1994 to 2018. Standard errors are clustered at the industry level. We include t-statistics in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

statistically significant even after excluding links with high TNIC pairwise similarity scores.

In addition, we show that the cross-industry spillover effects remain robust after excluding industries whose common market leaders are among the largest firms in the economy (i.e., top 50 firms ranked by sales). Specifically, we exclude an industry from our analysis if half or more than half of the links between this industry and other industries in the competition network are connected through the largest firms. As shown in Table OA.18, the coefficient of $\widehat{IdShock}_{-i,t}^{(c)}$

Table OA.18: Cross-industry spillover effects after excluding industries whose common market leaders are mainly superstar firms

	(1)	(2)	(3)	(4)	(5)	(6)
	$Risk\_of\_Failure_{i,t}^{(-c)}$		$Dist\_to\_Default_{i,t}^{(-c)}$		$Profit\_Margin_{i,t}^{(-c)}$	
$\widehat{IdShock}_{-i,t}^{(c)}$	0.520*	0.520*	0.352*	0.362*	0.481**	0.475**
	[1.955]	[1.948]	[1.676]	[1.717]	[1.993]	[1.973]
$\widehat{IdShock}_{-i,t}^{(c)} \times Frac\_Peers\_as\_Customers_{-i,i,t}$		0.096**		−0.073		0.655*
		[2.003]		[−0.180]		[1.697]
$\widehat{IdShock}_{-i,t}^{(c)} \times Frac\_Peers\_as\_Suppliers_{-i,i,t}$		−0.106**		−0.404		−0.850**
		[−2.379]		[−1.197]		[−2.340]
Controls for aggregate conditions	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4871	4869	4717	4715	4950	4948
R-squared	0.021	0.024	0.055	0.056	0.002	0.009
Permutation test p-value: $\beta_c = 0$	0.022	0.022	0.054	0.048	0.021	0.023

Note: This table reports the cross-industry spillover effects after excluding industries whose common market leaders are mainly superstar firms (i.e., top 50 firms ranked by sales). Specifically, we exclude an industry from our analysis if half or more than half of the links between this industry and other industries in the competition network are connected through superstar firms. The regression specification is: $Y_{i,t}^{(-c)} = \alpha_c + \beta_c \widehat{IdShock}_{-i,t}^{(c)} + Controls_{i,t} + \varepsilon_{i,t}$. We construct $\widehat{IdShock}_{-i,t}^{(c)}$ as the simple average of $\widehat{IdShock}_{j,t}$ averaged across all industries j that are connected to industry i through competition networks, where $\widehat{IdShock}_{j,t}$ is the fitted value from the first-stage regression $Y_{c_j,t} = \alpha + \sum_{m=1}^{3} \theta_m ND\_mild_{j,t}^{(m)} + \sum_{m=1}^{3} \gamma_m ND\_severe_{j,t}^{(s)} + \varepsilon_{c_j,t}$. The permutation tests are one-sided tests on the null hypothesis $\beta_c = 0$ against the alternative $\beta_c > 0$. The sample spans the period from 1994 to 2018. Standard errors are clustered at the industry level. We include t-statistics in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

remains positive and statistically significant after dropping these industries, suggesting that the cross-industry spillover effects are not simply driven by the largest firms.

Finally, we incorporate private firms in our analysis and investigate the cross-industry spillover effects through a competition network including both public and private entities. In the main text, we construct the competition network based on Compustat historical segment data, which only contain public firms. As a robustness test, here we redefine the competition networks by incorporating private firms. This alternative definition alleviates the concern that we may miss some important industry links in the competition network connected by private common market leaders. We gather sales information and the industry classification of private firms from the Capital IQ data. We show that the resulting competition network is very similar to the one constructed based on public firms only.

We obtain information about private firms from Capital IQ, which is one of the most comprehensive datasets covering private firms. Capital IQ provides the total sales of the private firms and the list of four-digit SIC industries in which firms operate ranked by the relative importance of these industries. The limitation of Capital IQ is that, unlike Compustat historical segment data, Capital IQ does not provide a breakdown of the industry-level sales within firms because the disclosure of private firms is in general less detailed. To overcome this limitation, we estimate the breakdown of the industry-level sales within firms using the weights computed based on public firms in the Compustat data. Specifically, for firms that operate in two industries, we assign 80% of sales to the primary industries and assign 20% of sales to the secondary industries. For firms that operate in three or more industries, we assign 68% of sales to the primary industries,

Table OA.19: Cross-industry spillover effects with both public and private firms

	(1)	(2)	(3)	(4)	(5)	(6)
	$Risk\_of\_Failure_{i,t}^{(-c)}$		$Dist\_to\_Default_{i,t}^{(-c)}$		$Profit\_Margin_{i,t}^{(-c)}$	
$\widehat{IdShock}_{-i,t}^{(c)}$	0.689**	0.703**	0.418**	0.435**	0.499**	0.594***
	[2.147]	[2.095]	[2.009]	[1.996]	[2.301]	[2.717]
$\widehat{IdShock}_{-i,t}^{(c)} \times Frac\_Peers\_as\_Customers_{-i,i,t}$		0.090**		0.058		0.714**
		[2.169]		[0.166]		[2.215]
$\widehat{IdShock}_{-i,t}^{(c)} \times Frac\_Peers\_as\_Suppliers_{-i,i,t}$		−0.088**		−0.436		−0.816***
		[−2.341]		[−1.510]		[−2.693]
Controls for aggregate conditions	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5431	5250	5277	5099	5532	5345
R-squared	0.020	0.023	0.054	0.054	0.002	0.010
Permutation test p-value: $\beta_c = 0$	0.016	0.014	0.030	0.037	0.016	0.003

Note: This table examines the cross-industry spillover effects through the competition network constructed using both public and private firms. The regression specification is: $Y_{i,t}^{(-c)} = \alpha_c + \beta_c \widehat{IdShock}_{-i,t}^{(c)} + Controls_{i,t} + \varepsilon_{i,t}$. We construct $\widehat{IdShock}_{-i,t}^{(c)}$ as the simple average of $\widehat{IdShock}_{j,t}$ averaged across all industries j that are connected to industry i through competition network constructed using both public and private firms, where $\widehat{IdShock}_{j,t}$ is the fitted value from the first-stage regression $Y_{c_j,t} = \alpha + \sum_{m=1}^{3} \theta_m ND\_mild_{j,t}^{(m)} + \sum_{m=1}^{3} \gamma_m ND\_severe_{j,t}^{(s)} + \varepsilon_{c_j,t}$. The permutation tests are one-sided tests on the null hypothesis $\beta_c = 0$ against the alternative $\beta_c > 0$. The sample spans the period from 1994 to 2018. Standard errors are clustered at the industry level. We include t-statistics in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

23% of sales to the secondary industries, and 9% of sales to the tertiary industries. Our findings remain robust if we assign sales to all industries in which the firms operate based on the weights estimated from public firms in the Compustat data.

Adding private firms only causes a minor change to the competition network. For example, more than 93% of the links in 1994 remain the same after we take private firms into consideration in forming the network. The distribution of node degree in 1994 also remains largely unchanged after adding private firms. We compare the competition networks with and without private firms in other snapshots and we find that the two sets of competition network are highly similar throughout our sample period.

We then add private firms into our analysis and examine the cross-industry spillover effects through the competition network constructed using both public and private firms. As shown in Table OA.19, the coefficient of $\widehat{IdShock}_{-i,t}^{(c)}$ remains positive and statistically significant, suggesting that the cross-industry spillover effects are robust to the addition of private firms.

Placebo Tests for the Unique Role of Common Market Leaders. In our hypothesis regarding spillovers from connected industries to a focal industry along competition network links, we propose that spillovers will only occur when common market leaders are financially consolidated yet financially constrained, are major players in both industries, and when strong within-industry spillover effects are evident in the focal industry. Conversely, we expect that cross-industry spillovers to the focal industry will be weak, or nonexistent, when common market leaders of the focal industry are not financially consolidated or not financially constrained, are not competing in both industries, or are operating in a non-tradable industry.

To test these ideas, we conduct placebo tests by adjusting the baseline cross-industry spillover regressions (3.3) and (3.4) to the following form:

$$Y_{i,t}^{(-c)} = \alpha_c + \beta_c \left[\frac{1}{n(\mathcal{O}_{i,t})} \sum_{j \in \mathcal{O}_{i,t}} \widehat{IdShock}_{j,t} \right] + Controls_{i,t-1} + \varepsilon_{i,t}, \tag{3.5}$$

where $\widehat{IdShock}_{j,t}$ is the same as in the baseline regression (3.2), but the set of connected industries $\mathcal{O}_{i,t}$ differs from the original set $\mathcal{C}_{i,t}$, reflecting various counterfactual scenarios that we test.

In the first placebo test, we examine the cross-industry spillover effects via the links without sufficient financial consolidation in the competition network. In the proposed economic mechanisms of our paper, cross-industry spillover effects rely critically on the financial consolidation of common market leaders. When a common market leader is not financially consolidated, the distress of one segment of this leader may not lead to changes in competition behaviors in its other segments, because the different segments do not share a unified balance sheet. Therefore, we expect cross-industry spillover effects to be stronger in industries with common leaders that are more financially consolidated. To test this prediction, we measure the level of financial consolidation exhibited by the common leaders of a four-digit SIC industry using the absolute value added by allocation in Rajan, Servaes and Zingales (2000) averaged across all common leaders in this industry. We sort industries into two groups based on the average financial consolidation level in year $t-1$. We define links without sufficient financial consolidation in year t as connected industries whose average financial consolidation level is below the median. Thus, the baseline cross-industry spillover regressions (3.3) and (3.4) are adjusted into (3.5) with $\mathcal{O}_{i,t}$ replacing $\mathcal{C}_{i,t}$ to focus on the industries whose average financial consolidation level is below the median in year $t-1$. Panel A of Table OA.20 tabulates the results. Consistent with our proposed economic mechanism, we find that cross-industry spillover effects captured by the coefficient of $\widehat{IdShock}_{-i,t}^{(c)}$ mostly concentrate in competition network links with high degree of financial consolidation by common market leaders, while they are almost absent in those links with low degree of financial consolidation by common market leaders. These findings are robust to controlling for production network connectedness.

In the second placebo test, we examine the cross-industry spillover effects via the financially non-constrained links in the competition network. The financial constraint of a four-digit SIC industry is measured by the sales-weighted average of the delay investment score in the industry (Hoberg and Maksimovic, 2015). We sort industries into tertiles based on industry-level financial constraints one year prior to natural disaster shocks. We define financially non-constrained links in year t as connected industries whose financial constraints measures are ranked in the bottom tertile in year $t-1$. Thus, the baseline cross-industry spillover regressions (3.3) and (3.4) are adjusted into (3.5) with $\mathcal{O}_{i,t}$ replacing $\mathcal{C}_{i,t}$ to focus on the industries whose financial constraints lie in the bottom tertile in year $t-1$. Panel B of Table OA.20 tabulates the results. Consistent with our proposed economic mechanism, we find that cross-industry spillover effects captured by the

Table OA.20: Placebo Tests for the Spillover Effects Across Industries

	Panel A: Common market leaders with low financial consolidation					
	(1)	(2)	(3)	(4)	(5)	(6)
	$Risk\_of\_Failure_{i,t}^{(-c)}$		$Dist\_to\_Default_{i,t}^{(-c)}$		$Profit\_Margin_{i,t}^{(-c)}$	
$\widehat{IdShock}_{-i,t}^{(c)}$	−0.075	−0.070	−0.186	−0.189	0.173	0.167
	[−0.199]	[−0.185]	[−0.665]	[−0.681]	[0.540]	[0.531]
Production network controls	No	Yes	No	Yes	No	Yes
Controls for aggregate conditions	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2459	2459	2372	2372	2496	2496
R-squared	0.019	0.022	0.054	0.060	0.002	0.012
Permutation test p-value: $\beta_c = 0$	0.584	0.576	0.751	0.760	0.306	0.311
	Panel B: Connected industries with low financial constraints					
	(1)	(2)	(3)	(4)	(5)	(6)
	$Risk\_of\_Failure_{i,t}^{(-c)}$		$Dist\_to\_Default_{i,t}^{(-c)}$		$Profit\_Margin_{i,t}^{(-c)}$	
$\widehat{IdShock}_{-i,t}^{(c)}$	−0.242	−0.244	0.007	0.029	0.237	0.240
	[−0.915]	[−0.912]	[0.032]	[0.126]	[0.914]	[0.929]
Production network controls	No	Yes	No	Yes	No	Yes
Controls for aggregate conditions	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3156	3154	3063	3061	3208	3206
R-squared	0.014	0.016	0.036	0.039	0.002	0.011
Permutation test p-value: $\beta_c = 0$	0.802	0.805	0.505	0.462	0.188	0.181
	Panel C: Inactive industry links in the industry competition network					
	(1)	(2)	(3)	(4)	(5)	(6)
	$Risk\_of\_Failure_{i,t}^{(-c)}$		$Dist\_to\_Default_{i,t}^{(-c)}$		$Profit\_Margin_{i,t}^{(-c)}$	
$\widehat{IdShock}_{-i,t}^{(c)}$	−0.019	−0.028	−0.095	−0.091	0.201	0.183
	[−0.054]	[−0.079]	[−0.398]	[−0.382]	[0.516]	[0.469]
Production network controls	No	Yes	No	Yes	No	Yes
Controls for aggregate conditions	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2977	2712	2896	2654	3031	2763
R-squared	0.021	0.021	0.056	0.057	0.004	0.008
Permutation test p-value: $\beta_c = 0$	0.524	0.538	0.668	0.660	0.290	0.309
	Panel D: Removing tradable industries from the focal industries					
	(1)	(2)	(3)	(4)	(5)	(6)
	$Risk\_of\_Failure_{i,t}^{(-c)}$		$Dist\_to\_Default_{i,t}^{(-c)}$		$Profit\_Margin_{i,t}^{(-c)}$	
$\widehat{IdShock}_{-i,t}^{(c)}$	0.351	0.360	0.174	0.186	0.252	0.261
	[1.040]	[1.049]	[0.674]	[0.713]	[0.829]	[0.852]
Production network controls	No	Yes	No	Yes	No	Yes
Controls for aggregate conditions	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2313	2309	2245	2241	2351	2347
R-squared	0.027	0.038	0.054	0.058	0.002	0.009
Permutation test p-value: $\beta_c = 0$	0.125	0.124	0.254	0.237	0.201	0.196

Notes. This table reports the results of four placebo tests examining cross-industry spillover effects. The permutation tests are one-sided tests on the null hypothesis $\beta_c = 0$ against the alternative $\beta_c > 0$. The sample in this table spans the period from 1994 to 2018. Standard errors are clustered at the industry level. We include t-statistics in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

coefficient of $\widehat{IdShock}^{(c)}_{-i,t}$ concentrate in competition network links with financially constrained common market leaders, while they are almost absent in those with non-constrained common market leaders.

In the third placebo test, we examine the cross-industry spillover effects via the inactive links in the competition network by counterfactually assuming these links continue to be active. To conduct such a test, we first need to identify the competition network links that become inactive and remain so thereafter. Specifically, we define inactive links as expired common market leaders that lose their positions between year $t-5$ and $t-3$. Thus, the baseline cross-industry spillover regressions (3.3) and (3.4) are adjusted into (3.5) with $\mathcal{O}_{i,t}$ replacing $\mathcal{C}_{i,t}$ to focus on expired common market leaders that lose their positions between year $t-5$ and $t-3$. Panel C of Table OA.20 reports the results. The coefficient of $\widehat{IdShock}^{(c)}_{-i,t}$ become statistically insignificant when we focus on the cross-industry spillover effects via the inactive links. The absence of the cross-industry spillover effects indicates that the cross-industry spillover effects are indeed transmitted through active links in the competition network, supporting our proposed economic mechanism.

In the fourth placebo test, we examine the cross-industry spillover effects when focal industries are non-tradable. The baseline cross-industry spillover regressions (3.3) and (3.4) are adjusted into (3.5) with $\mathcal{O}_{i,t}$ equals $\mathcal{C}_{i,t}$ if i is a non-tradable industry and equals an empty set otherwise. Panel D of Table OA.20 reports the results. The coefficient of $\widehat{IdShock}^{(c)}_{-i,t}$ becomes statistically insignificant when we focus on the cross-industry spillover effects when focal industries are non-tradable. This is intuitive because the concept of cross-industry spillover builds upon the idea of within-industry spillovers. The absence of the cross-industry spillover effects indicates that the cross-industry spillover effects are indeed transmitted through within-industry spillovers, providing strong support for our proposed economic mechanism.

4 Evidence from the AJCA Repatriation Tax Holiday

We exploit the setting of the AJCA repatriation tax holiday to investigate the impact of a reduction in financial distress on industry competitors and exploit the setting of the Lehman crisis to examine the impact of an increase in financial distress on industry competitors in Sections 4 and 5, respectively. Different from natural disasters, both the AJCA repatriation tax holiday and the Lehman crisis are one-time economy-wide shocks. Instead of using the DID approach, we estimate within-industry spillover effects using an econometric specification that takes advantage of exogenous variation in *industry treatment intensity* generated by the firm-level (quasi-)randomization of treatment. More precisely, industry treatment intensity captures the industry density of treatment firms that, by definition, have high exposures to the economy-wide shock. Similar econometric specifications and identification strategies that use group-level randomization of treatment to estimate peer effects have been widely adopted by recent studies (e.g., Miguel and Kremer, 2004; Berg, Reisinger and Streitz, 2021).

Table OA.21: Spillover effects in the setting of the AJCA repatriation tax holiday.

	(1)	(2)	(3)	(4)	(5)	(6)
	$\Delta Risk\_of\_Failure_i$		$\Delta Dist\_to\_Default_i$		$\Delta Profit\_Margin_i$	
$AJCA_i$	0.013	0.011	−0.167	−0.150	−0.015*	−0.014*
	[0.168]	[0.137]	[−0.348]	[−0.312]	[−1.733]	[−1.652]
$\overline{AJCA}_i$	−0.320**	−0.277*	2.059**	1.886**	0.042**	0.032*
	[−2.097]	[−1.771]	[2.217]	[1.968]	[2.442]	[1.781]
$Cross\_Industry\_Externality_i$		−0.053		0.246		0.013**
		[−1.001]		[0.842]		[2.248]
Observations	629	629	436	436	624	624
R-squared	0.008	0.010	0.018	0.020	0.010	0.018

Note: This table examines the spillover effects in the setting of the AJCA repatriation tax holiday by exploiting exogenous variation in *industry treatment intensity* generated by the firm-level (quasi-)randomization of treatment. The dependent variables are the change of distress level ($\Delta Risk\_of\_Failure_i$), the change of distance to default ($\Delta Dist\_to\_Default_i$), and the change of gross profit margin ($\Delta Profit\_Margin_i$) from the pre-AJCA period to the post-AJCA period. The distress level, distance to default, and profit margin in the pre-AJCA period are the average values from 2001 to 2003, while those in the post-AJCA period are the values of 2005. We include t-statistics in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

In this section, we study the spillover effect of a reduction in financial distress on competitors' competition behaviors and distress levels. Specifically, we examine the impact of the AJCA, which contains a provision to allow a temporary tax holiday for dividend repatriations of a 5.25% tax rate during a selected one-year window, rather than the existing 35% corporate tax rate. The AJCA passed the House on June 17th, the Senate on July 15th, and was signed into law on October 22nd, 2004. The passage of the AJCA reduces the distress levels of treated firms (i.e., those with a significant amount of pretax income from abroad), especially for those that were financially constrained prior to the AJCA (e.g., Faulkender and Petersen, 2012), because the reduction of the repatriation tax rate not only reduces firms' tax burden but also improves firms' internal capital market and better aligns the investment policy (e.g., Harford, Wang and Zhang, 2017).

Consistent with the prediction of the hypothesis on the within-industry spillover set forth in the main text, we find that, among firms that were financially constrained prior to the AJCA, a firm would compete less aggressively and become less distressed when its industry has a larger fraction of firms treated by the AJCA shock (i.e., when its industry has a higher industry treatment intensity).

Specifically, we run the following firm-level cross-sectional regression:

$$\Delta Y_i = \beta_1 \times AJCA_i + \beta_2 \times \overline{AJCA}_i + \beta_3 \times Cross\_Industry\_Externality_i + \varepsilon_i, \tag{4.1}$$

where ΔY_i represents the changes of firm i's distress or profit margin from the pre-AJCA period to the post-AJCA period, $AJCA_i$ is the treatment dummy that equals 1 if firm i has more than 33% pretax income from abroad during the period from 2001 to 2003 following the definition in Grieser and Liu (2019), $\overline{AJCA}_i$ is the industry treatment intensity of firm i's industry, defined as the fraction of treated firms in firm i's industry, and $Cross\_Industry\_Externality_i$ is an indicator variable that equals 1 if the average industry treatment intensity of all industries connected to firm i's industry through the competition network is higher than 20% in year t. $Cross\_Industry\_Externality_i$ is

Table OA.22: Heterogenous spillover effects in the AJCA tax holiday setting.

	(1)	(2)	(3)	(4)	(5)	(6)
	$\Delta Risk\_of\_Failure_i$		$\Delta Dist\_to\_Default_i$		$\Delta Profit\_Margin_i$	
$AJCA_i$	−0.111	−0.099	0.262	0.226	0.006	0.003
	[−0.976]	[−0.861]	[0.382]	[0.331]	[0.451]	[0.219]
$\overline{AJCA}_i \times AJCA_i$	−0.094	−0.087	1.335	1.271	0.004	0.002
	[−0.484]	[−0.449]	[1.086]	[1.040]	[0.188]	[0.085]
$\overline{AJCA}_i \times (1 - AJCA_i)$	−0.529**	−0.472*	2.855**	2.613*	0.079***	0.064**
	[−2.289]	[−1.954]	[2.043]	[1.787]	[3.262]	[2.526]
$Cross\_Industry\_Externality_i$		−0.041		0.211		0.011*
		[−0.769]		[0.713]		[1.901]
Observations	629	629	436	436	624	624
R-squared	0.011	0.012	0.020	0.021	0.017	0.023

Note: This table examines the heterogenous spillover effects in the AJCA tax holiday setting. We focus our analysis on the financially constrained firms (i.e., those with financial constraint ranked in the top quartile) prior to the passage of the AJCA. Financially constraint is measured as the average delay investment score of Hoberg and Maksimovic (2015) in the 5-year window prior to the the passage of the AJCA (i.e., 1999 to 2003). The regression specification is: $\Delta Y_i = \beta_1 AJCA_i + \beta_2 \overline{AJCA}_i \times AJCA_i + \beta_3 \overline{AJCA}_i \times (1 - AJCA_i) + \beta_4 Cross\_Industry\_Externality_i + \delta_t + \varepsilon_i$. The dependent variables are the change of distress risk ($\Delta Risk\_of\_Failure_i$), change of distance to default ($\Delta Dist\_to\_Default_i$), and change of gross profit margin ($\Delta Profit\_Margin_i$) from the pre-AJCA period to the post-AJCA period. The distress risk, distance to default, and profit margin in the pre-AJCA period are the average values from 2001 to 2003, while those in the post-AJCA period are the values of 2005. We follow Grieser and Liu (2019) to define $AJCA_i$ as an indicator variable that equals 1 if firm i has more than 33% pretax income from abroad during the period from 2001 to 2003. $\overline{AJCA}_i$ is the industry treatment intensity, which is the fraction of firms in firm i's industry with an $AJCA_i$ indicator that equals 1. $Cross\_Industry\_Externality_i$ captures the strength of cross-industry spillover effects through the competition network, and it is an indicator variable that equals one if the average industry treatment intensity for the industries connected to firm i's industry through competition networks is higher than 20%. We include t-statistics in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

a proxy for the strength of cross-industry spillover effects through the competition network. Because the passage of the AJCA altered corporate behaviors (e.g., investment) mostly for the financially constrained firms (e.g., Faulkender and Petersen, 2012; Grieser and Liu, 2019), we focus our analysis on the firms that were financially constrained prior to the passage of the AJCA. Specifically, we measure the extent to which a firm is financially constrained using the delay investment score proposed by Hoberg and Maksimovic (2015) averaged over the 5-year period prior to the the passage of the AJCA (i.e., 1999 to 2003) and focus our analysis on the firms ranked in the top quartile based on the financial constraint measure.

The industry treatment intensity of firm i's industry naturally affects the exposure of firm i to the AJCA shock, because a firm that competes in an industry with a higher fraction of treated firms is more exposed to the spillover effect of its treated competitors in the same industry. The effect of the AJCA on firm i's distress or profit margin is expected to depend on the industry treatment intensity $\overline{AJCA}_i$. Since the cross-industry spillover effect is captured in the $Cross\_Industry\_Externality_i$ term, the β_2 coefficient measures the AJCA treatment externalities across firms within an industry.

Table OA.21 presents the results from the regressions. The within-industry spillover effect, captured by the β_2 coefficient, is estimated to be positive and statistically significant for profit margin (see columns (5) and (6)). Thus, among firms that were financially constrained prior to the AJCA, a firm would compete less aggressively when its industry has a larger fraction of firms treated by the AJCA shock (i.e., when its industry has a higher industry treatment intensity).

Table OA.23: Spillover effects of bond yield spread in the AJCA tax holiday setting.

	(1)	(2)
	$\Delta Yield\_Spread_{i,t}(\%)$	
$AJCA_i$	0.038 [0.622]	0.040 [0.658]
$\overline{AJCA}_i$	−0.251** [−2.014]	−0.245* [−1.910]
$Cross\_Industry\_Externality_i$		−0.017 [−0.286]
Observations	316	316
R-squared	0.014	0.014

Note: This table examines the spillover effects of bond yield spread in the AJCA tax holiday setting. The regression specification is: $\Delta Y_{i,t} = \beta_1 AJCA_i + \beta_2 \overline{AJCA}_i + \beta_3 Cross\_Industry\_Externality_i + \varepsilon_i$. The dependent variable is the change of bond yield spread ($\Delta Yield\_Spread_i$) around the passage of the AJCA. Because the bond yield spread data cover a limited number of firms in the cross-section, we include all firms except those with the lowest financial constraint (bottom quintile) in our analysis. We include t-statistics in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Meanwhile, the coefficient β_2 is negative and statistically significant for distress level (see columns (1) and (2)), and it is positive and statistically significant for distance to default (see columns (3) and (4)). Thus, among firms that were financially constrained prior to the AJCA, a firm would become less distressed when its industry has a larger fraction of firms treated by the AJCA shock (i.e., when its industry has higher industry treatment intensity). Taken together, all these results demonstrate the existence of the within-industry spillover effects. In Table OA.22, we further examine the within-industry spillover effects by recognizing that the treated and non-treated firms may be subject to heterogeneous spillover effects from their industry competitors (e.g., Berg, Reisinger and Streitz, 2021). We find that the spillover effect is mainly from treated firms to non-treated competitors within an industry, not the other way around. Moreover, we consider the bond yield spreads as an additional market-based measure for distress level. As shown in Table OA.23, the within-industry spillover effects remain robust.

Table OA.21 also examines the cross-industry spillover effects. Coefficient β_3 is positive and statistically significant for profit margin (see column (6)). Thus, a firm tends to compete less aggressively when the connected industries of its own industry on the competition network have a higher average industry treatment intensity. Meanwhile, coefficient β_3 is negative for distress level (see column (2)), and it is positive for distance to default (see column (4)). Thus, a firm tends to be more distressed when the connected industries of its own industry on the competition network have a higher average industry treatment intensity. Taken together, these results support the prediction of the hypothesis on the cross-industry spillover effects set forth in the main text.

5 Evidence from the Lehman Crisis

In this section, we study the spillover effect of an increase in distress level on competitors' competition behaviors and distress levels. Specifically, we examine the impact of the Lehman crisis through exploiting the heterogeneous credit supply shocks across different firms induced

by Lehman's bankruptcy on September 15, 2008 (e.g., Ivashina and Scharfstein, 2010; Chodorow-Reich, 2014; Chodorow-Reich and Falato, 2021). We construct the proxy for exogenous variations in credit supply to borrowers closely following Chodorow-Reich (2014). To construct the variation in availability of credit for a firm, we first measure the healthiness of a bank using the quantity of loans made by the bank to all borrowers other than the firm relative to before the crisis; with the bank healthiness measure at hand, we then measure the loan supply to the firm using a weighted average over all members of the last precrisis loan syndicate. The idea behind the credit supply shock induced by Lehman's bankruptcy is rather intuitive: Owing to the sticky borrower-lender relationship and the fact that the origins of the Lehman crisis lay outside of the corporate loan sector, firms that had precrisis relationships with less healthy lenders suffered from an adverse credit supply shock — they had a lower likelihood of obtaining a loan following the Lehman bankruptcy and paid a higher interest rate if they did borrow.

To show the existence of the spillover effect of an increase in distress level on competitors' competition behaviors and distress levels, we run the following cross-sectional regression:

$$\Delta \ln(Price)_i = \beta_1 \times LEH_i + \beta_2 \times \overline{LEH}_i + \beta_3 \times Cross\_Industry\_Externality_i + \varepsilon_i, \tag{5.1}$$

where $\Delta \ln(Price)_i$ represents the changes of product prices of firm i after the Lehman crisis, LEH_i is an indicator variable that equals 1 if firm i experiences a below-median credit supply shock (i.e., the firm's credit supply reduces more than the median firm) during the Lehman crisis, $\overline{LEH}_i$ equals the fraction of firms in firm i's industry with $LEH_i = 1$, capturing the industry treatment intensity, and $Cross\_Industry\_Externality_i$ is an indicator variable that equals one if the average industry treatment intensity for the industries connected to firm i's industry through the competition network is higher than 20%, capturing the strength of cross-industry spillover effects through the competition network.[11]

We follow Chodorow-Reich (2014) to construct the measure of firm-specific credit supply shocks during the Lehman crisis. Specifically, we first define:

$$\Delta L_{-i,b} = \frac{\sum_{j \neq i} \alpha_{b,j,crisis} L_{b,j,crisis}}{0.5 \sum_{j \neq i} \alpha_{b,j,normal} L_{b,j,normal}}.$$

where $L_{b,j,t}$ is an indicator variable that equals to 1 if bank b lends to borrow j in period t, and $\alpha_{b,j,t}$ denotes bank b's share in each syndicated loan that is made to firm j in period t.[12] Because Dealscan only reports lender shares for about one-third of loans, we impute the missing lender shares using the same method of Chodorow-Reich (2014). The crisis period refers to the 9-month

[11] We find that the coefficient β_3 is insignificant in Table OA.24, which is likely due to two reasons: 1) Unlike the natural disaster setting, the Lehman setting is mainly a cross-sectional test, which limits its power in quantifying the cross-industry spillover effects; 2) Although Nielsen data provide detailed product prices at the UPC level, the data cover relatively a limited number of firms. The coverage limitation in the cross section of firms applies to the bond yield spread data as well.

[12] The syndicated loan data come from Thomson Reuters LPC DealScan and we focus on loans with either primary or secondary purpose listed as "working capital" or "corporate purpose".

Table OA.24: Spillover effects in the Lehman crisis setting.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$\Delta \ln(Price\_Geo)_i$		$\Delta \ln(Price\_EW)_i$		$\Delta \ln(Price\_VW)_i$		$\Delta Bond\_spread_i(\%)$	
LEH_i	0.039	0.039	0.059	0.059	0.062	0.061	−0.106	−0.099
	[0.780]	[0.770]	[1.238]	[1.233]	[1.358]	[1.329]	[−0.866]	[−0.801]
$\overline{LEH_i}$	−0.200**	−0.205**	−0.217**	−0.209**	−0.191**	−0.206**	0.526**	0.603**
	[−2.249]	[−2.206]	[−2.520]	[−2.371]	[−2.268]	[−2.372]	[1.982]	[2.122]
$Cross\_Industry\_Externality_i$		0.008		−0.012		0.028		−0.148
		[0.191]		[−0.333]		[0.749]		[−0.967]
Observations	384	384	384	384	384	384	419	419
R-squared	0.013	0.013	0.015	0.015	0.013	0.014	0.011	0.013

Note: This table examines the spillover effects in the Lehman crisis setting. The regression specification is: $\Delta Y_i = \beta_1 LEH_i + \beta_2 \overline{LEH_i} + \beta_3 Cross\_Industry\_Externality_i + \varepsilon_i$. The dependent variables in columns (1)–(6) are changes in firm product prices from 2007 to 2009. We use three different approaches to compute the price changes based on Nielsen data. Specifically, we first aggregate product prices across all products (i.e., unique UPCs) of firm i in product category c in year t (2007 or 2009) using three methods: geometric average ($Price\_Geo_{i,c,t}$, see Kim, 2021), equal-weighted average ($Price\_EW_{i,c,t}$), and sales-weighted average ($Price\_VW_{i,c,t}$). We then compute the price growth rate for each firm-product-category from 2007 to 2009 as the difference of the log prices: $\Delta \ln(Price)_{i,c} = \ln(Price_{i,c,2009}) - \ln(Price_{i,c,2007})$. Finally, we compute $\Delta \ln(Price)_i$ by aggregating the price growth rates across all product categories within firm i based on sales. The dependent variables in columns (7) and (8) are changes in the bond yield spread from 2007 to 2009. LEH_i is an indicator variable that equals 1 if firm i experiences a below-median credit supply shock during the Lehman crisis. The method we use to construct the measure of firm-specific credit supply shock is the same as that of Chodorow-Reich (2014). A lower level of credit supply shock implies that the lender health of the firm deteriorated more during the Lehman crisis. $\overline{LEH}_{i,t}$ is the industry treatment intensity which is the fraction of firms in firm i's industry with an LEH_i indicator that equals 1. $Cross\_Industry\_Externality_{i,t}$ captures the strength of cross-industry spillover effects through the competition network, and it is an indicator variable that equals one if the average industry treatment intensity for the industries connected to firm i's industry through competition networks is higher than 20% in year t. We include t-statistics in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

period from October 2008 to June 2009, and the pre-crisis normal period refers to the 18-month period containing October 2005 to June 2006 and October 2006 to June 2007. We multiply the denominator by 0.5 to account for the fact that the crisis period consists of 9 months while the pre-crisis normal period consists of 18 months. $\Delta L_{-i,b}$ captures the quantity of loans made by bank b to all borrowers other than firm i relative to the pre-crisis normal period.

Next, we aggregate $\Delta L_{-i,b}$ across all lenders that lend to firm i for the last syndicated loan that firm i borrowed before the Lehman crisis:

$$\Delta \widetilde{L}_i = \sum_{b \in s_i} \alpha_{b,i,last} \Delta L_{-i,b}.$$

where $\alpha_{b,i,last}$ is bank b's share in the last syndicated loan taken by firm i before the Lehman crisis and s_i denotes the set of banks that lend to firm i in that syndicated loan. $\Delta \widetilde{L}_i$ captures the credit supply shocks to firm i during the Lehman crisis. A lower level of $\Delta \widetilde{L}_i$ implies that the lender health of firm i deteriorated more during the Lehman crisis.

We use three different approaches to compute the price changes based on the Nielsen data. Specifically, we aggregate product prices across all products (i.e., unique UPCs) of firm i in product category c in year t (2007 or 2009) using three methods: geometric average ($Price\_Geo_{i,c,t}$, see Kim, 2021), equal-weighted average ($Price\_EW_{i,c,t}$), and sales-weighted average ($Price\_VW_{i,c,t}$). We then compute the price growth rate for each firm-product-category from 2007 to 2009 as the difference in log prices: $\Delta \ln(Price)_{i,c} = \ln(Price_{i,c,2009}) - \ln(Price_{i,c,2007})$. Finally, we compute $\Delta \ln(Price)_i$ by

aggregating the price growth rates across all product categories within firm i based on sales.

Table OA.24 presents the results. The outcome variables in columns (1)–(6) are the changes of firm product prices. Coefficient β_2 represents the within-industry spillover effects. It is negative and statistically significant, suggesting that firms compete more aggressively by reducing product prices when a larger fraction of firms in the industry experience adverse credit supply shocks during the Lehman crisis. This finding is robust to the three methods we use to aggregate product prices.

Finally, we examine the spillover effects in distress. Specifically, we replace the outcome variables in specification (5.1) with the bond yield spread. We focus on the bond yield spread instead of the accounting-based distress measure because it is a market-based measure and thus more suitable for the Lehman setting which is essentially an event study. We find that coefficient β_2 is positive and statistically significant for bond yield spread (see columns 7–8), suggesting that firms become more distressed when a larger fraction of firms in the industry experience adverse credit-supply shocks during the Lehman crisis. These findings are consistent with the predictions of our hypothesis and demonstrate the existence of the within-industry spillover effects.

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