INTERNET APPENDIX FOR "COMPETITION NETWORK: DISTRESS SPILLOVERS AND PREDICTABLE INDUSTRY RETURNS"

Winston Wei Dou Shane Johnson Wei Wu

July 19, 2023

Abstract

This note contains anecdotes and additional empirical results of the main text Dou, Johnson and Wu (2022). 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, covenant strictness, network centrality, and credit supply shocks during the Lehman crisis. It also illustrates the method we use to match Nielsen data to the CRSP/Compustat database, as well as the approach we use to construct industry-level supplier-customer links. Section 3 presents additional results for the natural disaster setting, including robustness checks, additional heterogeneity tests, and examinations of alternative explanations. Sections 4 through 6 present evidence from the AJCA repatriation tax holiday, the Lehman crisis, and enforcement against financial fraud, respectively. Lastly, Section 7 provides additional results for industry return predictability. *JEL Codes*: G32, G33, L11, L14.

Keywords: Competition, Cross-industry momentum, Financial distress, Covenants, Spillover and treatment externality, Predatory pricing.

Contents

1 Anecdotes for the 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 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 product market and thus lower the profit margins of their industry peers.

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

[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.

THE WALL STREET JOURNAL.
U.S. EDITION

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
J
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.

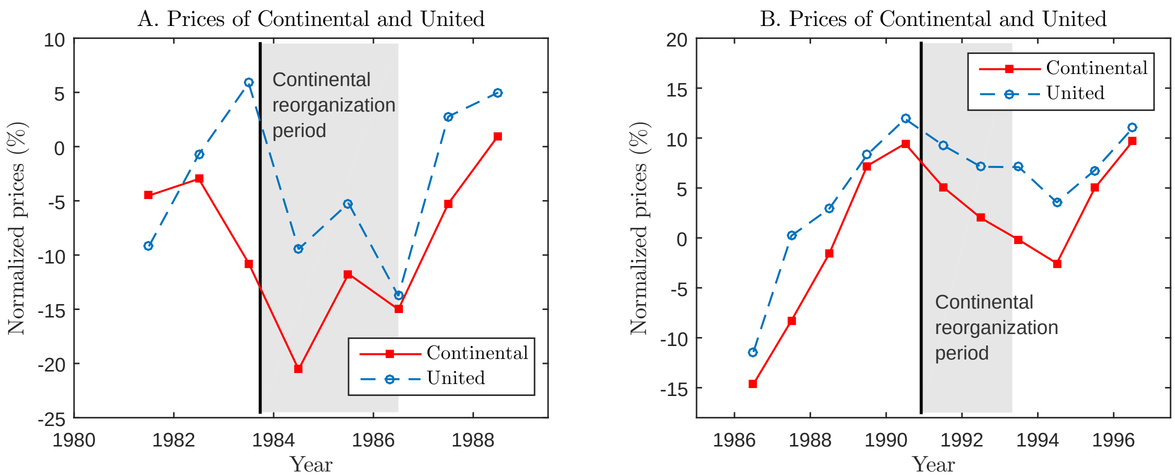
"If they want to play war games, we'll play war games," Continental's spokesman said.

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.

turn in the US economy following the burst of the dotcom bubble. In response, PC vendors such as Dell, Compaq, and Gateway decided 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 but 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.

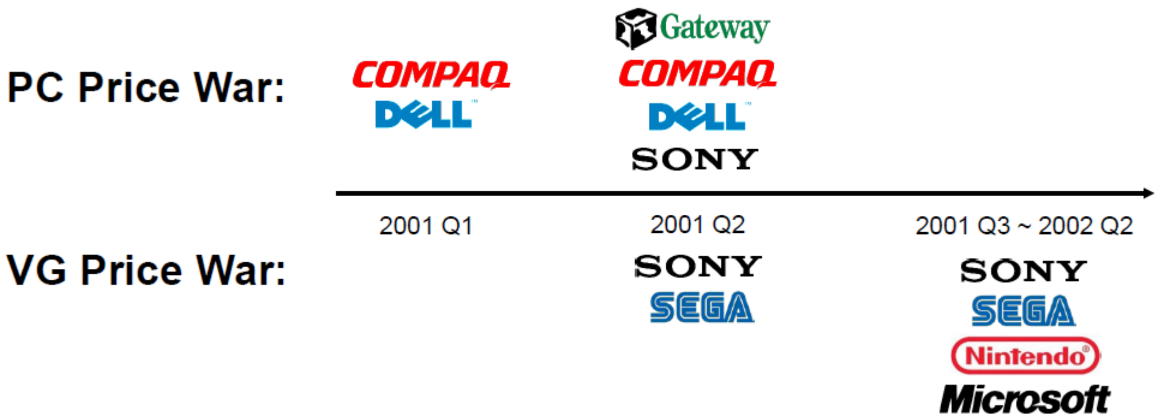


Figure OA.3: Competition spillover from the personal computer (PC) industry to the video game (VG) industry.

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

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 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 Measures for Distress Risk

Distress Risk. We follow Campbell, Hilscher and Szilagyi (2008) in measuring distress risk ($Distress_{i,t}$), which captures the probability of firm bankruptcy or failure. Specifically, we define distress risk based on the third column in Table IV of Campbell, Hilscher and Szilagyi (2008) as follows:

$$Distress_{i,t} = -9.164 - 20.264NIMTAAVG_{i,t} + 1.416TLMTA_{i,t} - 7.129EXRETAVG_{i,t} + 1.411SIGMA_{i,t} - 0.045RSIZE_{i,t} - 2.132CASHMTA_{i,t} + 0.075MB_{i,t} - 0.058PRICE_{i,t}. \tag{2.1}$$

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 $Distress_{i,t}$ 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 ($DD_{i,t}$). Specifically, we define the distance to default with a 1-year forecasting horizon following equation 12 of Bharath and Shumway (2008):

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

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 $DD_{i,t}$ implies a higher probability of bankruptcy or failure.

2.2 Measures for Covenant Strictness

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.

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 $1 - \Phi\left(\ln(\overline{\mathbf{r}}) - \widehat{\ln(\mathbf{r}_{i,t+1})}\right)$ instead. We compute two sets of strictness measures based on all twelve covenant ratios and the six earnings-based covenant ratios, respectively.

2.3 Measures for Network Centrality

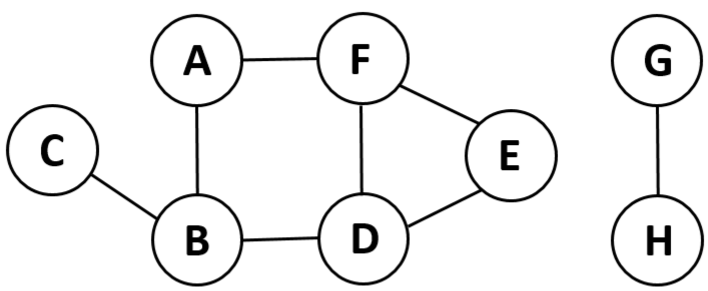


Figure OA.4: An example network.

We explain the mathematical definition of the four network centrality measures (degree, closeness, betweenness, and eigenvector centrality) in this section. We use an example network taken from El-Khatib, Fogel and Jandik (2015) to help with the illustration (see Figure OA.4).

Degree Centrality. Degree centrality is the number of direct links a node has with other nodes in the network. The more links the node has, the more central this node is in the network. The mathematical definition for degree centrality is:

$$Degree_i = \sum_{j \neq i} x_{i,j}, \tag{2.2}$$

where $x_{i,j}$ is an indicator variable that equals 1 if node i and node j are connected. For the network shown in Figure OA.4, the degree centrality for nodes A to H is 2, 3, 1, 3, 2, 3, 1, and 1, respectively.

Table OA.1: Competition network centrality measures.

Panel A: Correlation among centrality measures				
	Degree	*Closeness*	*Betweenness*	*Eigenvector*
Degree	1			
Closeness	0.59^{***}	1		
Betweenness	0.80^{***}	0.42^{***}	1	
Eigenvector	0.66^{***}	0.27^{***}	0.58^{***}	1
Panel B: Variance explained by the principal components				
	*PC*1	*PC*2	*PC*3	*PC*4
Variance explained (%)	67.28	18.72	10.05	3.95

Note: Panel A shows the correlation among the four centrality measures (degree, closeness, betweenness, and eigenvector) computed from the competition networks. The sample period of the data is from 1977 to 2018. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. We perform principal component analysis based on the time series of the four centrality measures. Panel B shows the percentage of variance explained by the four individual principal components.

Closeness Centrality. Closeness centrality is the inverse of the sum of the (shortest) weighted distances between a node and all other nodes in a given network. It indicates how easily a node can be affected by other disturbances to other nodes in the network. The mathematical definition for closeness centrality is:

$$Closeness_i = \frac{n-1}{\sum_{j \neq i} d_{i,j}} \times \frac{n}{N}, \tag{2.3}$$

where $d_{i,j}$ is the shortest distance between nodes i and j. Variable n is the size of the component to which node i belongs, and variable N is the size of the entire network. In the network example shown in Figure OA.4, there are two components in the network: one with a size of six nodes (nodes A to F) and the other with a size of two nodes (nodes G and H). The closeness centrality for nodes A to H is 0.469, 0.536, 0.341, 0.536, 0.417, 0.469, 0.250, and 0.250, respectively.

Betweenness Centrality. Betweenness centrality gauges how often a node lies on the shortest path between any other two nodes of the network. Hence, it indicates how much control a node could have on the spillover effect on the network, because a node located between two other nodes can either dampen or amplify the spillover effect between those two nodes through the network links. The mathematical definition for betweenness centrality is:

$$Betweenness_k = \sum_{i<j \neq k \in N} \frac{g_{i,j,(k)}/g_{i,j}}{(n-1)(n-2)/2}, \tag{2.4}$$

where $g_{i,j}$ is 1 for any geodesic connecting nodes i and j, and $g_{i,j,(k)}$ is 1 if the geodesic between nodes i and j also passes through node k. Variable n is the size of the component to which node i belongs, and variable N is the size of the entire network. For the network shown in Figure OA.4, the betweenness centrality for nodes A to H is 0.1, 0.45, 0, 0.3, 0, 0.15, 0, and 0, respectively.

Eigenvector Centrality. Eigenvector centrality is a measure of the importance of a node in the network. It takes into account the extent to which a node is connected with other highly connected nodes. Eigenvector centrality is solved by satisfying the following equation:

$$\lambda E'E = E'\mathbf{A}E, \tag{2.5}$$

where E is an eigenvector of connection matrix $\mathbf{A}$, and λ is its corresponding eigenvector. The eigenvector centrality for node i is thus the elements of eigenvector E^* associated with $\mathbf{A}$'s principal eigenvalue λ^*. For the network shown in Figure OA.4, the eigenvector centrality for nodes A to H is 0.358, 0.408, 0.161, 0.516, 0.401, 0.502, 0, and 0,

Table OA.2: Relation between competition network centrality and industry characteristics

Panel A: Summary statistics of the industry characteristics	Mean	Median	SD	p10	p25	p75	p90
$Centrality_{i,t}$	2.971	2.350	2.077	1.156	1.654	3.702	5.669
$Production\_Centrality_{i,t}$	1.731	1.543	0.958	0.889	1.416	1.893	2.344
$LnSales_{i,t}$	7.747	7.691	1.940	5.293	6.412	9.074	10.448
$LnBEME_{i,t}$	−0.531	−0.548	0.700	−1.336	−0.966	−0.105	0.298
$GP_{i,t}$	0.346	0.323	0.213	0.097	0.185	0.473	0.622
$HHI_{i,t}$	0.068	0.049	0.061	0.018	0.030	0.082	0.148

Panel B: Industry characteristics across portfolios sorted on competition network centrality

	Mean					Median				
$Centrality_{i,t}$ quintiles	Q1	Q2	Q3	Q4	Q5	Q1	Q2	Q3	Q4	Q5
$Centrality_{i,t}$	1.070	1.935	2.379	3.293	6.247	1.157	1.903	2.241	3.250	5.655
$Production\_Centrality_{i,t}$	1.680	1.734	1.766	1.702	1.777	1.534	1.624	1.523	1.524	1.543
$LnSales_{i,t}$	7.633	7.624	7.782	7.850	7.877	7.520	7.591	7.759	7.735	7.834
$LnBEME_{i,t}$	−0.601	−0.537	−0.513	−0.473	−0.527	−0.623	−0.543	−0.520	−0.496	−0.537
$GP_{i,t}$	0.402	0.314	0.308	0.354	0.352	0.378	0.284	0.277	0.335	0.326
$HHI_{i,t}$	0.066	0.064	0.068	0.066	0.073	0.048	0.044	0.054	0.049	0.051

Panel C: Relation between competition network centrality and industry characteristics

	(1)	(2)	(3)	(4)	(5) $Competition\_Centrality_{i,t}$	(6)	(7)	(8)	(9)	(10)
$Production\_Centrality_{i,t}$	0.046	0.036	0.050*	0.024	0.052*	0.027	0.053*	0.028	0.040	0.006
	[1.566]	[1.021]	[1.689]	[0.683]	[1.743]	[0.777]	[1.759]	[0.781]	[0.665]	[0.088]
$LnSales_{i,t}$			−0.013	0.040	−0.008	0.042	−0.008	0.042	0.135	0.171
			[−0.373]	[0.929]	[−0.206]	[0.939]	[−0.223]	[0.946]	[1.315]	[1.525]
$LnBEME_{i,t}$					0.047*	0.024	0.045*	0.016	−0.001	−0.028
					[1.792]	[0.871]	[1.790]	[0.592]	[−0.013]	[−0.539]
$GP_{i,t}$							−0.009	−0.028	−0.163*	−0.174*
							[−0.250]	[−0.760]	[−1.809]	[−1.936]
$HHI_{i,t}$									0.091	0.096
									[0.961]	[1.009]
Year FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	9195	9195	9186	9186	8840	8840	8840	8840	3327	3327
R-squared	0.002	0.020	0.002	0.021	0.005	0.022	0.005	0.023	0.036	0.066

Note: This table shows the relation between competition network centrality and industry characteristics. $Competition\_Centrality_{i,t}$ is the competition network centrality measure, which is the PC1 of the four centrality measures of the competition networks (i.e., degree centrality, closeness centrality, betweenness centrality, and eigenvector centrality). $Production\_Centrality_{i,t}$ is the production network centrality, which is the PC1 of four centrality measures of the production networks. $LnSales_{i,t}$ is the natural log of industry revenue. $LnBEME_{i,t}$ is the natural log of industry book-to-market ratio, which is the ratio between the book equity and the market equity of an industry. $GP_{i,t}$ is industry gross profitability, which is the gross profits (revenue minus cost of goods sold) scaled by assets, following the definition of Novy-Marx (2013). Industry-level revenue, cost of goods sold, book assets, book equity, and market equity are the sum of the corresponding firm-level measures for firms in the same industry. $HHI_{i,t}$ is the HHI of the top 50 firms. The concentration ratio data come from the US Census, which covers manufacturing industries. Panel A tabulates summary statistics of the industry characteristics. P10, p25, p75, and p90 are the 10^{th}, 25^{th}, 75^{th}, and 90^{th} percentiles. Panel B tabulates the mean and median values of the industry characteristics across industry quintile portfolios sorted on competition network centrality. The sorting is performed at yearly frequency. Panel C performs panel regressions which regress industry-level competition network centrality on various industry characteristics. The dependent variable and all the independent variables are standardized to have means of 0 and standard deviations of 1. Because common leaders and conglomerates operate in more than one industry, we exclude them from computing industry characteristics. We exclude from the analysis financial and utility industries and very small industries that contain fewer than three firms. The sample period of the data is from 1977 to 2018. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

respectively.

Construction of Competition Network Centrality Measures. We consider four centrality measures for all industries connected in the competition network: *closeness*, *degree*, *betweenness*, and *eigenvector*. These measures are widely used in the literature (e.g., Sabidussi, 1966; Bonacich, 1972; Freeman, 1977; El-Khatib, Fogel and Jandik, 2015). We construct all four measures and find that they are highly correlated with each other, as shown in Table OA.1. Given that each measure only captures some, but not all, aspects of the centrality of nodes on the competition network, we use the first principal component of the four measures as our centrality measure in the paper. The eigen-decomposition of the covariance matrix of the four measures of centrality exhibits a dominant highest eigenvalue and fast decay for the rest of the eigenvalues. Panel B of Table OA.1 shows that the first principal component (PC1) is the dominant common factor that drives much of the covariances of the four centrality measures on the competition network. As shown in

Table OA.2, competition network centrality appears to be largely unrelated to other industry characteristics, including production network centrality, industry size, industry-level book-to-market ratio, industry-level gross profitability, and Herfindahl-Hirschman index (HHI).

2.4 Measures for Credit Supply Shocks During the Lehman Crisis

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

$$\Delta L_{-i,b} = \frac{\sum_{j \neq i} \alpha_{b,j,crisis} L_{b,j,crisis}}{0.5 \sum_{j \neq i} \alpha_{b,j,normal} L_{b,j,normal}}. \tag{2.6}$$

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.[2] 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 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:

$$\Delta\widetilde{L}_i = \sum_{b \in s_i} \alpha_{b,i,last} \Delta L_{-i,b}. \tag{2.7}$$

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.

2.5 Match Nielsen Data to CRSP/Compustat

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.[3] 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

[2]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".

[3]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.

the GS1 data and firms in CRSP/Compustat.[4] We obtain a pool of potential matches based on two criteria: (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.[5] We then manually identify correct matches from all potential matching candidates.[6] 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.

2.6 Construct Industry-Level Supplier-Customer Links

We identify industry-level supplier-customer links based on a series of Benchmark Input-Output Surveys of the Bureau of Economic Analysis (BEA) following previous studies (e.g., Menzly and Ozbas, 2010; Acemoglu and Azar, 2020). BEA Surveys report the amount of inter-industry flows of goods and services in the Use Table, and they are published roughly once every 5 years. We draw on 9 different surveys (2012, 2007, 2002, 1997, 1992, 1987, 1982, 1977, and 1972) on a rolling basis to measure supplier-customer relations.

The BEA surveys are based on Input-Output (IO) industry classifications. We define an IO industry i as the customer industry of the IO industry j if industry i uses more than 1% of outputs of industry j. We apply the 1% cutoff to make sure the production network links we identify represent economically important supplier-customer relations. The results of our paper are robust to alternative choices of the cutoff value.

For the surveys before 1992, BEA provides link tables between the IO industries and the four-digit SIC industries, where one SIC industry is mapped to one IO industry. Therefore, we can map a given pair of SIC industries to a pair of IO industries and then identify the supplier-customer relation. For the surveys after 1997, BEA provides link tables between the IO industries and the North American Industry Classification System (NAICS) industries. We further map the NAICS industry to SIC industry based on the link table provided by the U.S. Census. While one NAICS industry is mapped to only one IO industry, one SIC industry can potentially be mapped to multiple NAICS industries. Thus, it is possible for one SIC industry to be mapped to multiple IO industries via NAICS after 1997. To maintain consistent mapping between SIC and IO industries throughout the sample period, when one SIC industry is mapped to multiple IO industries after 1997, we map this SIC industry to the most appropriate IO industry using both the Compustat Segment data and the industry name descriptions. Specifically, we identify the NAICS industry that is most frequently co-reported by firms for a given SIC industry based on the Compustat Segment data, which provide both SIC and NAICS industry classifications for firms' segments. We then manually check the industry descriptions to ensure the quality of the matching between SIC and NAICS industries. Once we map a SIC industry to only one NAICS industry, we can further map the SIC industry to one IO industry and subsequently identify the supplier-customer relation for each pair of SIC industries.

Finally, we enhance the industry-level production network connections by incorporating firm-level supplier-customer links derived from both Compustat customer segment data and Factset Revere data.

[4] The Jaccard index measures the similarity between finite sample sets and is defined as the size of the intersection divided by the size of the union of the sample sets. The Jaccard index ranges between 0 and 1, with zero reflecting perfect similarity.

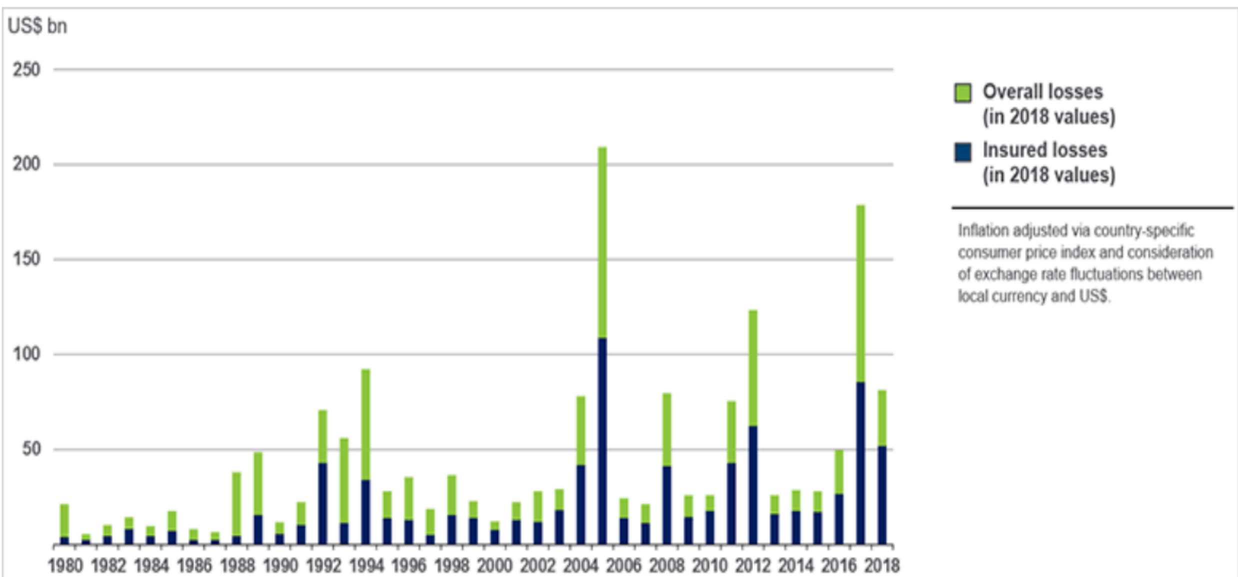
[5] These two matching criteria are sufficiently conservative to ensure that exact matches are included in the pool of potential matches. For example, among all the exact matches in 2016, 97% of them satisfy the two matching criteria and are included in our pool of potential matches.

[6] 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.

3 Additional Evidence from the Natural Disaster Setting

3.1 Disaster Losses Are Only Partially Offset by Insurance

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). 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).



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.

3.2 List of Major Natural Disasters

Table OA.3 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.

Table OA.3: 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

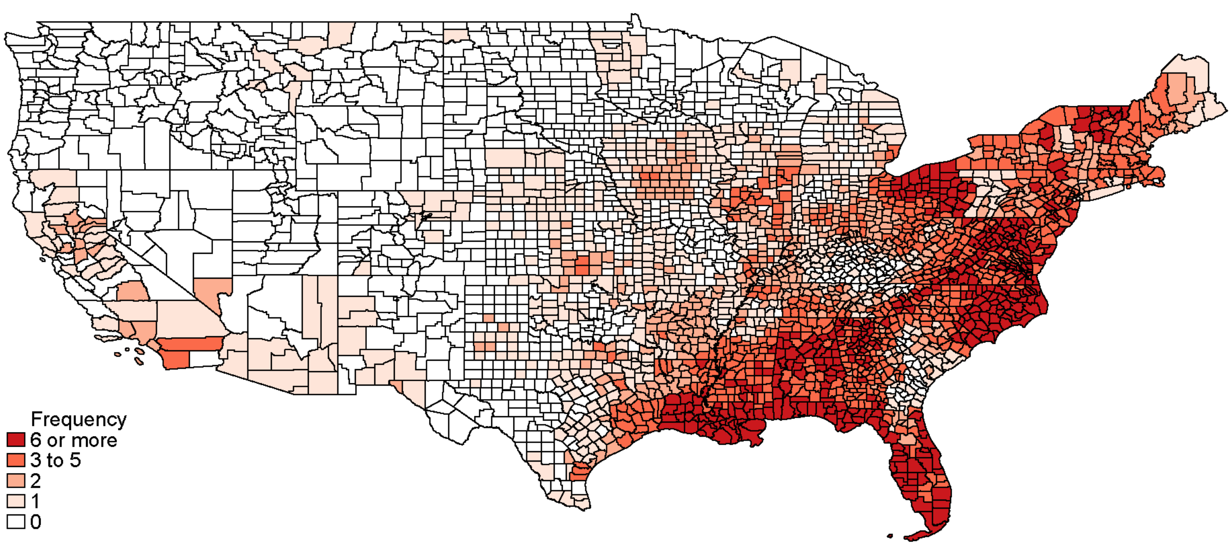


Figure OA.6: Frequency of major natural disasters by US county.

3.3 Robustness Checks for the Within-Industry Spillover Effects

To ensure the robustness of the within-industry spillover effects, we perform a comprehensive set of robustness checks.

First, we demonstrate in Table OA.4 that our results are robust to alternative matching ratios between treated firms and non-treated peer firms (i.e., one-to-ten and one-to-three).

Second, we show in Table OA.5 that our findings remain unchanged when we use text-based network industry classifications (TNIC) developed by Hoberg and Phillips (2010, 2016) to choose peer firms.

Third, we show in Table OA.6 that our findings are robust to an alternative measure to control for the strength of cross-industry spillover effects through the competition network. 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.

Fourth, we address potential concerns about DID regressions with staggered treatment. Recent advances in econometric theory suggest that standard DID regression estimates with staggered treatment timing can provide biased estimates when the treatment effects are heterogeneous (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 address this concern, 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 × 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.7, the within-industry spillover effects remain robust after we addressing the potential concerns about DID regressions with staggered treatment.

Fifth, we confirm in Table OA.8 that the within-industry spillover effects are robust when we use net profit margin as an alternative measure of profitability.

Sixth, we conduct two robustness tests in Table OA.9 to address concerns that matched peer firms may be geographically close to areas affected by natural disasters. First, in panel A of Table OA.9, we restrict the sample to peer firms located outside of any states affected by major local natural disasters. Second, in panel B of Table OA.9, we require that the matched peer firms be geographically distant from the natural disaster areas in the DID analysis, with headquarters and major establishments located more than 100 miles from any zip code negatively affected by major local natural disasters in a given year. In both robustness tests, we find that our results of the within-industry spillover effects remain robust.

Finally, because our analysis focuses on major local natural disasters in the US, we verify that our findings are driven by industries whose profits primarily come from the domestic market. Specifically, we exclude industries with the highest fraction of foreign profits (i.e., top quintile) from the DID analysis. As shown in Table OA.10, the within-industry spillover effects remain robust. In fact, the economic magnitudes of the spillover effects become larger compared to those in Table III of the main text. These results further validate the identification and estimation strategy we employ in this study.

Table OA.4: Alternative matching ratios between treated firms and non-treated peer firms.

	Panel A: Matching 1 treated firm with up to 10 non-treated peer firms							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$Distress_{i,t}$		$DD_{i,t}$		$PM_{i,t}$		$Markup_{i,t}$	
$Treat_{i,t} \times Post_{i,t}$	0.031***	0.031***	−0.086*	−0.087*	−0.005	−0.006	−0.005	−0.006
	[3.004]	[3.038]	[−1.715]	[−1.738]	[−0.957]	[−0.991]	[−0.925]	[−0.965]
$Treat_{i,t}$	−0.013	−0.013	0.090*	0.091*	0.001	0.001	−0.001	−0.001
	[−1.387]	[−1.400]	[1.903]	[1.914]	[0.119]	[0.135]	[−0.287]	[−0.270]
$Post_{i,t}$	0.050***	0.049***	−0.124***	−0.120***	−0.008**	−0.007*	−0.012***	−0.011**
	[7.623]	[7.539]	[−4.155]	[−4.056]	[−1.971]	[−1.831]	[−2.713]	[−2.553]
$\ln(1+n(\mathcal{C}_{i,t}))$		0.019**		−0.050		−0.010**		−0.012***
		[2.259]		[−1.331]		[−2.206]		[−2.634]
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	207050	207050	164136	164136	205140	205140	204972	204972
R-squared	0.586	0.586	0.657	0.657	0.737	0.737	0.760	0.760
Test p-value: $\beta_1 + \beta_3 = 0$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	0.001	0.002	$<10^{-3}$	$<10^{-3}$

	Panel B: Matching 1 treated firm with up to 3 non-treated peer firms							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$Distress_{i,t}$		$DD_{i,t}$		$PM_{i,t}$		$Markup_{i,t}$	
$Treat_{i,t} \times Post_{i,t}$	0.024**	0.024**	−0.087*	−0.077*	−0.000	−0.000	−0.000	−0.000
	[2.277]	[2.293]	[−1.662]	[−1.681]	[−0.060]	[−0.073]	[−0.080]	[−0.093]
$Treat_{i,t}$	−0.014	−0.014	0.093*	0.093*	0.001	0.001	0.001	0.001
	[−1.416]	[−1.419]	[1.718]	[1.726]	[0.315]	[0.318]	[0.163]	[0.166]
$Post_{i,t}$	0.055***	0.054***	−0.129***	−0.123***	−0.006***	−0.006**	−0.010***	−0.010***
	[7.283]	[7.202]	[−3.727]	[−3.581]	[−2.645]	[−2.544]	[−2.978]	[−2.879]
$\ln(1+n(\mathcal{C}_{i,t}))$		0.015**		−0.070*		−0.004		−0.005
		[2.010]		[−1.913]		[−1.630]		[−1.581]
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	100225	100225	82652	82652	99489	99489	99409	99409
R-squared	0.601	0.601	0.673	0.673	0.759	0.759	0.786	0.786
Test p-value: $\beta_1 + \beta_3 = 0$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	0.003	0.005	$<10^{-3}$	0.001

Note: This table examines the spillover effects of the major natural disasters with alternative matching ratios between treated firms and non-treated peer firms. In panel A, we match each treated firm with up to 10 non-treated peer firms 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 peer firms. The regression specification and the definition of the dependent and independent variables are explained in Table III 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.

Table OA.5: Matching industry peers with text-based network industry classifications.

	(1)	(2)	(3)	(4)
	$Distress_{i,t}$	$DD_{i,t}$	$PM_{i,t}$	$Markup_{i,t}$
$Treat_{i,t} \times Post_{i,t}$	0.015	−0.025	−0.005	−0.006
	[1.467]	[−0.541]	[−1.005]	[−1.231]
$Treat_{i,t}$	−0.008	0.027	0.007*	0.011**
	[−0.889]	[0.556]	[1.708]	[2.166]
$Post_{i,t}$	0.049***	−0.154***	−0.007**	−0.010**
	[7.542]	[−5.165]	[−2.051]	[−2.536]
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	219992	176763	219133	218988
R-squared	0.580	0.639	0.742	0.766
Test p-value: $\beta_1 + \beta_3 = 0$	$<10^{-3}$	$<10^{-3}$	0.002	$<10^{-3}$

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 peer firms in its TNIC industry based on firm asset size, tangibility, and age. We ensure that the matched peer firms 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 ($Distress_{i,t}$), distance to default ($DD_{i,t}$), gross profit margin ($PM_{i,t}$), and markup ($Markup_{i,t}$). $Treat_{i,t}$ 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.6: Alternative measure to control for cross-industry spillover effects.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$Distress_{i,t}$		$DD_{i,t}$		$PM_{i,t}$		$Markup_{i,t}$	
$Treat_{i,t} \times Post_{i,t}$	0.023**	0.029***	−0.087*	−0.102*	−0.001	0.000	−0.001	0.000
	[2.275]	[2.780]	[−1.717]	[−1.923]	[−0.196]	[0.099]	[−0.267]	[0.011]
$Treat_{i,t}$	−0.011	−0.013	0.096*	0.092*	−0.001	−0.001	−0.001	−0.000
	[−1.189]	[−1.337]	[1.940]	[1.770]	[−0.189]	[−0.163]	[−0.151]	[−0.024]
$Post_{i,t}$	0.055***	0.049***	−0.122***	−0.097***	−0.007**	−0.007**	−0.010***	−0.010***
	[8.223]	[7.180]	[−3.882]	[−3.055]	[−2.283]	[−2.291]	[−2.649]	[−2.583]
$\ln(1 + \mathcal{D}_{i,t})$		0.010***		−0.033**		−0.003***		−0.005***
		[3.349]		[−2.436]		[−2.634]		[−2.918]
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	136181	124621	110581	101308	135037	124047	134924	123949
R-squared	0.597	0.611	0.667	0.676	0.745	0.748	0.773	0.777
Test p-value: $\beta_1 + \beta_3 = 0$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	0.004	0.015	$<10^{-3}$	0.004

Note: This table uses an alternative measure to control for cross-industry spillover effects. Different from Table III 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 III 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.7: Require control firms to be untreated throughout the event window in the staggered DID regressions.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$Distress_{i,t}$		$DD_{i,t}$		$PM_{i,t}$		$Markup_{i,t}$	
$Treat_{i,t} \times Post_{i,t}$	0.032***	0.040***	−0.124**	−0.129**	−0.006	−0.004	−0.005	−0.005
	[2.829]	[3.323]	[−2.217]	[−2.137]	[−1.035]	[−0.696]	[−0.856]	[−0.724]
$Post_{i,t}$	0.045***	0.043***	−0.112***	−0.121***	−0.011**	−0.011**	−0.015***	−0.015***
	[5.060]	[4.529]	[−3.050]	[−3.001]	[−2.407]	[−2.244]	[−3.139]	[−2.835]
$\ln(1 + n(\mathcal{C}_{i,t}))$	0.013		−0.079**		−0.007**		−0.010**	
	[1.473]		[−2.116]		[−2.047]		[−2.336]	
$\ln(1 + \mathcal{D}_{i,t})$		0.011***		−0.038***		−0.005***		−0.006***
		[3.252]		[−2.687]		[−2.915]		[−3.502]
Cohort × firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	122887	111736	97728	88865	121716	111151	121627	111082
R-squared	0.720	0.732	0.803	0.811	0.845	0.847	0.870	0.873
Test p-value: $\beta_1 + \beta_3 = 0$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	0.004	0.016	0.001	0.004

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 III of the main text and in Table OA.6. 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.8: Net profit margin.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$NPM_{i,t}$		$NPM_{i,t}$		$NPM_{i,t}$		$NPM_{i,t}$	
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: $NPM_{i,t} = \beta_1 Treat_{i,t} \times Post_{i,t} + \beta_2 Treat_{i,t} + \beta_3 Post_{i,t} + \beta_4 \ln(1 + n(\mathcal{C}_{i,t})) + \theta_i + \delta_t + \varepsilon_{i,t}$. Net profit margins ($NPM_{i,t}$) 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.9: Require the matched peers 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)	(7)	(8)
	$Distress_{i,t}$		$DD_{i,t}$		$PM_{i,t}$		$Markup_{i,t}$	
$Treat_{i,t} \times Post_{i,t}$	0.024**	0.024**	−0.070	−0.071	−0.000	−0.000	−0.001	−0.001
	[2.323]	[2.344]	[−1.347]	[−1.371]	[−0.042]	[−0.060]	[−0.212]	[−0.231]
$Treat_{i,t}$	−0.010	−0.010	0.071	0.071	0.000	0.000	0.000	0.000
	[−0.981]	[−0.987]	[1.392]	[1.402]	[0.003]	[0.010]	[0.054]	[0.061]
$Post_{i,t}$	0.056***	0.055***	−0.134***	−0.127***	−0.008**	−0.008**	−0.011**	−0.010**
	[7.852]	[7.751]	[−4.009]	[−3.844]	[−2.261]	[−2.147]	[−2.569]	[−2.447]
$\ln(1 + n(\mathcal{C}_{i,t}))$		0.018**		−0.075**		−0.007**		−0.008**
		[2.308]		[−2.024]		[−2.142]		[−2.126]
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	131591	131591	106101	106101	130519	130519	130404	130404
R-squared	0.598	0.598	0.670	0.670	0.742	0.742	0.770	0.770
Test p-value: $\beta_1 + \beta_3 = 0$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	0.005	0.008	0.001	0.002

Panel B: Matched non-treated firms far from the disaster area (i.e., $\geq$ 100 miles)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$Distress_{i,t}$		$DD_{i,t}$		$PM_{i,t}$		$Markup_{i,t}$	
$Treat_{i,t} \times Post_{i,t}$	0.021	0.021	−0.089	−0.089	0.001	0.001	0.009	0.009
	[1.524]	[1.537]	[−1.346]	[−1.360]	[0.143]	[0.140]	[0.928]	[0.923]
$Treat_{i,t}$	−0.018	−0.018	0.113*	0.114*	0.000	0.000	−0.004	−0.004
	[−1.340]	[−1.360]	[1.681]	[1.703]	[0.009]	[0.014]	[−0.465]	[−0.454]
$Post_{i,t}$	0.068***	0.067***	−0.154***	−0.145***	−0.017*	−0.017*	−0.028***	−0.027***
	[5.934]	[5.810]	[−3.182]	[−3.015]	[−1.840]	[−1.829]	[−2.862]	[−2.830]
$\ln(1 + n(\mathcal{C}_{i,t}))$		0.025**		−0.103**		−0.005		−0.010
		[2.268]		[−2.297]		[−0.702]		[−1.288]
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	104938	104938	84697	84697	104064	104064	103967	103967
R-squared	0.623	0.623	0.685	0.686	0.760	0.760	0.779	0.779
Test p-value: $\beta_1 + \beta_3 = 0$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	0.007	0.008	$<10^{-3}$	0.001

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 peer firms 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 peer firms 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 III 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.

Table OA.10: Excluding industries with high fraction of profits from foreign countries.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$Distress_{i,t}$		$DD_{i,t}$		$PM_{i,t}$		$Markup_{i,t}$	
$Treat_{i,t} \times Post_{i,t}$	0.014	0.014	−0.044	−0.045	0.003	0.003	0.002	0.002
	[1.251]	[1.277]	[−0.787]	[−0.804]	[0.656]	[0.633]	[0.313]	[0.288]
$Treat_{i,t}$	−0.015	−0.015	0.122**	0.122**	0.001	0.001	0.001	0.001
	[−1.361]	[−1.372]	[2.118]	[2.127]	[0.173]	[0.188]	[0.237]	[0.250]
$Post_{i,t}$	0.066***	0.064***	−0.156***	−0.151***	−0.011***	−0.010***	−0.015***	−0.015***
	[8.439]	[8.348]	[−4.274]	[−4.198]	[−2.934]	[−2.822]	[−3.259]	[−3.130]
$\ln(1 + n(\mathcal{C}_{i,t}))$		0.020**		−0.056		−0.007**		−0.010**
		[2.484]		[−1.412]		[−2.192]		[−2.341]
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	106518	106518	87622	87622	105457	105457	105367	105367
R-squared	0.611	0.611	0.682	0.682	0.768	0.768	0.797	0.797
Test p-value: $\beta_1 + \beta_3 = 0$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	0.007	0.011	$<10^{-3}$	$<10^{-3}$

Note: This table examines the within-industry spillover effects of the major natural disasters by excluding industries with high fraction of profits from foreign countries. The fraction of foreign profits of an industry is the ratio between the industry-level foreign pre-taxable income and the industry-level total pre-tax income. The industry-level foreign (total) pre-taxable income is the sum of the firm-level foreign (total) pre-taxable income across firms in the industry, of which the data come from Compustat. We sort industries into quintiles based on the fraction of foreign profits each year. We control for the entry costs of the industry in the sorting to make sure the quintile assignment is orthogonal to entry costs. We exclude the industries in the top foreign profits quintile in the DID tests. The regression specification and the definition of the dependent and independent variables are explained in Table III 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 Additional Heterogeneity Tests for the Within-Industry Spillover Effects

In the main text, we conduct several heterogeneity tests across industries with different levels of entry barriers, different financial conditions, different amounts of inventory. We also illustrate the differences between tradable and non-tradable industries in the main text. In this session, we perform two additional heterogeneity tests.

First, we expect the within-industry spillover effects to be stronger in industries whose market leaders are more likely to tacitly collude with each other. To test this prediction, we proxy the prevalence of tacit collusion by the levels profitability comovement, which is the average pairwise correlation of the net profitability for top four firms ranked by sales in a given four-digit SIC industry. The pairwise correlation between two firms is calculated as the correlation coefficient of their net profitability in the previous ten years. We then sort industries into two groups based on the industry-level profitability comovement 1 year prior to major local natural disaster shocks and then examine the within-industry spillover effects in the industries with high profitability comovement (above median) and low profitability comovement (below median) using DID analysis. Table OA.11 tabulates the results. Consistent with our prediction, we find that the within-industry spillover effects captured by coefficient β_3 mostly concentrate in industries with high profitability comovement, while they are much weaker in industries with low profitability comovement.

Second, as illustrated in Section 2.1 of the main text, we expect the within-industry spillover effects to be stronger in industries with worse economic conditions prior to major local natural disasters. This is because firms in these industries have more incentives to compete after the arrival of negative shocks. To test this prediction, we measure the economic condition of a four-digit SIC industry using the change of the return on assets (ROA) in the industry from the previous year. We then sort industries into two groups based on the industry-level economic conditions 1 year prior to the natural disaster shocks and then examine the within-industry spillover effects in the industries with good economic conditions (top half) and bad economic conditions (bottom half) using DID analysis. Table OA.12 tabulates the results. Consistent with our prediction, we find that the within-industry spillover effects captured by coefficient β_3 mostly concentrate in industries with bad economic conditions, while they are almost absent in industries with good economic conditions. The total treatment effects are significant in all industries when we examine the distress levels of treated firms (see the last row of columns 1 to 4 in panel A), but they are only significant in industries with bad economic conditions when we examine the profit margins of the treated firms (see the last row of columns 5 to 8 in panel A). These findings are consistent with the prediction of our hypothesis, and they suggest that distressed treated firms engage in price competition only in industries with bad economic conditions, which leads to distress propagation to their industry peers.

Table OA.11: Heterogeneity across industries with different levels of profitability comovement.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$Distress_{i,t}$		$DD_{i,t}$		$PM_{i,t}$		$Markup_{i,t}$	
Profitability comovement	High	Low	High	Low	High	Low	High	Low
$Treat_{i,t} \times Post_{i,t}$	0.017	0.032**	−0.085	−0.087	−0.006	0.004	−0.007	0.004
	[1.158]	[2.225]	[−1.249]	[−1.190]	[−0.877]	[0.755]	[−0.875]	[0.681]
$Treat_{i,t}$	−0.009	−0.017	0.065	0.171**	−0.004	0.002	−0.001	0.001
	[−0.612]	[−1.144]	[0.880]	[2.089]	[−0.726]	[0.399]	[−0.100]	[0.083]
$Post_{i,t}$	0.066***	0.043***	−0.181***	−0.034	−0.011**	−0.003	−0.017**	−0.004
	[6.366]	[4.460]	[−3.694]	[−0.744]	[−2.116]	[−0.965]	[−2.443]	[−1.089]
$\ln(1+n(\mathcal{C}_{i,t}))$	0.032***	0.004	−0.067	−0.067	−0.016***	0.002	−0.022***	0.002
	[2.915]	[0.415]	[−1.353]	[−1.383]	[−3.738]	[0.481]	[−3.877]	[0.555]
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	63129	63382	52734	49305	63231	62122	63180	62062
R-squared	0.626	0.633	0.704	0.696	0.706	0.803	0.769	0.814
Test p-value: $\beta_1 + \beta_3 = 0$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	0.043	$<10^{-3}$	0.867	$<10^{-3}$	0.976

Note: This table examines the within-industry spillover effects following major natural disasters across industries with different levels of profitability comovement. The regression specification is: $Y_{i,t} = \beta_1 Treat_{i,t} \times Post_{i,t} + \beta_2 Treat_{i,t} + \beta_3 Post_{i,t} + \beta_4 \ln(1+n(\mathcal{C}_{i,t})) + \theta_i + \delta_t + \varepsilon_{i,t}$. We present results from DID analysis in industries with high profitability comovement (above median) and low profitability comovement (below median). The profitability comovement of a four-digit SIC industry is measured as the average pairwise correlation of the net profitability for top four firms ranked by sales in this industry. The pairwise correlation between two firms is calculated as the correlation coefficient of their net profitability in the previous ten years. The sample 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.12: Heterogeneity across industry economic conditions.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$Distress_{i,t}$		$DD_{i,t}$		$PM_{i,t}$		$Markup_{i,t}$	
Industry economic conditions	Bad	Good	Bad	Good	Bad	Good	Bad	Good
$Treat_{i,t} \times Post_{i,t}$	0.030**	0.025*	−0.070	−0.146**	−0.002	−0.002	−0.008	0.002
	[2.118]	[1.767]	[−1.013]	[−2.142]	[−0.340]	[−0.279]	[−0.985]	[0.357]
$Treat_{i,t}$	−0.031**	−0.013	0.129*	0.165**	0.001	0.001	0.001	0.001
	[−2.097]	[−0.902]	[1.685]	[2.200]	[0.112]	[0.123]	[0.106]	[0.071]
$Post_{i,t}$	0.080***	0.024**	−0.209***	0.004	−0.017***	0.005	−0.019***	0.002
	[7.675]	[2.376]	[−4.374]	[0.087]	[−3.192]	[1.564]	[−2.866]	[0.494]
$\ln(1+n(\mathcal{C}_{i,t}))$	0.033***	0.009	−0.120***	−0.092**	−0.014***	−0.002	−0.018***	−0.004
	[3.608]	[0.908]	[−2.635]	[−2.034]	[−3.543]	[−0.584]	[−3.784]	[−0.904]
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	67500	64754	54606	52132	66381	64702	66304	64664
R-squared	0.634	0.619	0.695	0.698	0.768	0.773	0.789	0.805
Test p-value: $\beta_1 + \beta_3 = 0$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	0.012	$<10^{-3}$	0.340	$<10^{-3}$	0.365

Note: This table examines the within-industry spillover effects following major natural disasters across industries with different economic conditions prior to the natural disasters. It presents the results in industries with good economic conditions (top half) and bad economic conditions (bottom half) prior to the natural disasters. 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. We sort industries into two groups based on the industry-level economic conditions 1 year prior to the natural disaster shocks. The sample 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.

3.5 Testing Alternative Explanations for the Within-Industry Spillover Effects

In this section, we examine the disaster-induced demand shock, the production network spillover effect, the common creditor channel, and the common blockholder channel as potential channels that could drive the observed spillover effects. We show that these alternative channels are unlikely to account for the observed within-industry spillover effects. Our analysis underscores the robustness of our findings and supports the view that the within-industry spillovers we document are primarily driven by the strategic product market competition mechanisms proposed in Section 2.1 of the main text.

Disaster-Induced Demand Shocks. The first alternative explanation that we test is the disaster-induced demand shock channel. This explanation posits that natural disasters lead to negative demand shocks that directly harm the treated firms, their matched unaffected industry peers, or both, resulting in lower profit margins and higher distress risk. However, we present two sets of evidence indicating that this explanation is unlikely to be the primary driver of the observed within-industry spillover effects.

The first set of evidence pertains to the geographic distribution of the spillover effects. Panel B of Table VII in the main text shows that the spillover effects are concentrated in tradable industries, whose firms typically have geographically dispersed customer bases, including customers located internationally. As a result, local natural disasters that hit the headquarters or major establishments of treated firms are unlikely to cause significant damage to their demand. Therefore, the disaster-induced demand shock channel is unlikely to be the main force behind the observed within-industry spillover effects.

The second set of evidence pertains to the possibility that the matched unaffected industry peers may also suffer negative demand shocks due to their customers being primarily located in natural disaster areas. We control for this possibility by imposing additional requirements on the matched industry peers, including the absence of customers negatively affected by natural disasters, both business customers and individual consumers. We identify firms' business customers and their geographic locations using Compustat customer segment data and Factset Revere data, and individual retail consumers and their geographic locations using a detailed dataset from Baker, Baugh and Sammon (2020).[7] Our results, presented in Table OA.13, show that the within-industry spillover effects persist even after imposing these additional requirements, indicating that the disaster-induced demand shock channel is unlikely to account for the observed spillover effects.

Production Network Spillover Effects. The second alternative explanation that we test is the production network externality channel. This explanation posits that the within-industry spillover effects arise from spillovers along supply chains. However, we present two sets of evidence indicating that this explanation is unlikely to be the primary driver of the observed within-industry spillover effects.

The first set of evidence pertains to direct supplier-customer relations. In the baseline DID test presented in Table III of the main text, we have already imposed the requirement that the matched peer firms cannot be suppliers or customers of the treated firms. The strong within-industry spillover effects observed in Table III of the main text suggest that supplier-customer relations are unlikely to be the cause of these effects. Furthermore, we also demonstrate that the within-industry spillover effects remain robust after excluding the potential customer-supplier relations, not just the existing customer-supplier relations in the DID test. We assess the likelihood that two firms may enter into a bilateral supplier-customer relation using their vertical relatedness scores (see, Frésard, Hoberg and Phillips, 2020). We define two firms as likely to engage in a bilateral supplier-customer relation if their vertical relatedness score ranks in the top 10% among the scores of all firm pairs.

[7] The full dataset contains more than two million users from 2010 to 2015. We make the assumption that firms with sales to individual consumers in a city in 2010 (2015) have sales to individual consumers in this city before 2010 (after 2015).

The second set of evidence pertains to indirect supplier-customer relations. To further strengthen our results, we require in Table OA.14 that the matched peer firms have no common customers or suppliers with the treated firms. This additional requirement rules out the alternative explanation that within-industry spillover effects arise from common customers or suppliers of both the treated firms and their industry peers.[8]

Common Creditors. The third alternative explanation that we test is the common creditor channel. This alternative explanation posits that non-treated industry peers may borrow from lenders that have heavy exposure to treated firms that experience major local natural disasters, and thus these unaffected peers can suffer from suppressed profit margin and heightened distress risk even though they are not negatively affected directly by the major local natural disasters.

To test this possibility, we require the matched peer firms 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 find out 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 $t-1$.[9] Second, for each firm i, we 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 borrowed from different lenders. As shown in Table OA.15, our findings remain robust after controlling for $Lender\_Exposure_{i,t-1}$ and removing the matched peer firms 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.[10]

Common Blockholders. We investigate the potential role of the common blockholder channel 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 peers of the treated firms, it may lead to a decline in the stock prices of these peers and an increase in their distress risk.

To test this hypothesis, we use 13F institutional holdings data to identify matched peer firms 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. Our analysis indicates that even after controlling for the common blockholder channel, the within-industry spillover effects remain robust, suggesting that this channel is unlikely to be the primary driver of our findings. Further details are available in Table OA.16.

Controlling for All Alternative Channels Simultaneously. In Table OA.17, 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 peer firms in the same four-digit SIC industry. We construct a set of indicator variables to label the

[8] In this explanation, natural disaster shocks increase the distress of the treated firms' customers, which in turn increases the distress risk of other suppliers to these customer firms. Similarly, natural disaster shocks can also increase the distress of the treated firms' suppliers, which in turn increases the distress risk of other customers of these supplier firms. If the firms shocked by natural disasters and their peer firms share common customers or suppliers, it is possible that the observed within-industry spillover effects are driven by production network externality rather than the product market competition mechanism.

[9] 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, 2018; Chodorow-Reich and Falato, 2021).

[10] 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 (e.g., Carey, Post and Sharpe, 1998). We limit our analysis in Table OA.15 to the firms covered by the DealScan data because we cannot accurately measure lender exposure for the firms outside of the DealScan universe.

matched peer firms that experience disaster-induced demand shocks ($Disaster\_Demand_{i,t}$), that are connected to the treated firms through the 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 (3.3) in the main text. In addition, we control for firms' exposure to natural disasters through lenders ($Lender\_Exposure_{i,t-1}$). As shown in Table OA.17, the within-industry spillover effects captured by the coefficient for $Post_{i,t}$ remain robust after controlling for all four alternative channels simultaneously.

Table OA.13: Testing the disaster-induced demand shock channel.

Panel A: Matched non-treated firms outside of the affected states + without affected business and retailed customers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$Distress_{i,t}$		$DD_{i,t}$		$PM_{i,t}$		$Markup_{i,t}$	
$Treat_{i,t} \times Post_{i,t}$	0.019*	0.019*	−0.036	−0.037	−0.003	−0.003	−0.004	−0.004
	[1.754]	[1.770]	[−0.665]	[−0.677]	[−0.491]	[−0.506]	[−0.654]	[−0.669]
$Treat_{i,t}$	−0.009	−0.009	0.066	0.066	0.002	0.002	0.002	0.002
	[−0.879]	[−0.889]	[1.250]	[1.258]	[0.365]	[0.376]	[0.362]	[0.372]
$Post_{i,t}$	0.057***	0.056***	−0.151***	−0.147***	−0.008*	−0.007*	−0.011**	−0.010**
	[7.635]	[7.537]	[−4.325]	[−4.227]	[−1.911]	[−1.821]	[−2.414]	[−2.326]
$\ln(1+n(\mathcal{C}_{i,t}))$		0.016*		−0.047		−0.007*		−0.008*
		[1.901]		[−1.210]		[−1.833]		[−1.804]
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	124123	124123	99874	99874	123011	123011	122889	122889
R-squared	0.602	0.602	0.670	0.670	0.746	0.746	0.773	0.773
Test p-value: $\beta_1 + \beta_3 = 0$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	0.006	0.009	0.001	0.001

Panel B: Matched non-treated firms far from the disaster area (i.e., $\geq$ 100 miles) + without affected business and retailed customers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$Distress_{i,t}$		$DD_{i,t}$		$PM_{i,t}$		$Markup_{i,t}$	
$Treat_{i,t} \times Post_{i,t}$	0.016	0.016	−0.064	−0.065	0.001	0.001	0.004	0.004
	[1.167]	[1.192]	[−0.895]	[−0.911]	[0.217]	[0.202]	[0.343]	[0.332]
$Treat_{i,t}$	−0.019	−0.019	0.119*	0.120*	0.001	0.001	0.004	0.004
	[−1.356]	[−1.389]	[1.664]	[1.685]	[0.163]	[0.187]	[0.407]	[0.426]
$Post_{i,t}$	0.071***	0.069***	−0.164***	−0.158***	−0.012**	−0.012**	−0.027***	−0.026***
	[6.019]	[5.889]	[−3.006]	[−2.907]	[−2.166]	[−2.126]	[−2.594]	[−2.588]
$\ln(1+n(\mathcal{C}_{i,t}))$		0.029**		−0.075		−0.008*		−0.012
		[2.407]		[−1.565]		[−1.662]		[−1.324]
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	98049	98049	78781	78781	97199	97199	97097	97097
R-squared	0.636	0.636	0.692	0.692	0.778	0.778	0.783	0.783
Test p-value: $\beta_1 + \beta_3 = 0$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	0.005	0.007	0.001	0.002

Note: This table tests the disaster-induced demand shock channel. In panel A, we perform DID analysis by requiring the headquarters and the major establishments of the matched peer firms 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 peer firms to be more than 100 miles away from any zip code negatively affected by major natural disaster in a given year. In both panels, we further require the matched peer firms to have no customers negatively affected by natural disaster. 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). 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.14: Testing the production network externality channel.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$Distress_{i,t}$		$DD_{i,t}$		$PM_{i,t}$		$Markup_{i,t}$	
$Treat_{i,t} \times Post_{i,t}$	0.030***	0.030***	−0.111**	−0.112**	−0.002	−0.002	−0.001	−0.001
	[2.649]	[2.661]	[−1.987]	[−2.004]	[−0.259]	[−0.269]	[−0.114]	[−0.127]
$Treat_{i,t}$	−0.022**	−0.022**	0.146***	0.146***	0.002	0.002	0.000	0.000
	[−2.006]	[−2.010]	[2.590]	[2.600]	[0.304]	[0.308]	[0.014]	[0.020]
$Post_{i,t}$	0.053***	0.051***	−0.123***	−0.116***	−0.010**	−0.009*	−0.014***	−0.013**
	[6.741]	[6.667]	[−3.327]	[−3.187]	[−2.019]	[−1.942]	[−2.599]	[−2.496]
$\ln(1+n(\mathcal{C}_{i,t}))$		0.018**		−0.085**		−0.008		−0.011**
		[1.980]		[−1.967]		[−1.598]		[−2.125]
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	112647	112647	88542	88542	111387	111387	111270	111270
R-squared	0.595	0.595	0.668	0.669	0.740	0.740	0.763	0.763
Test p-value: $\beta_1 + \beta_3 = 0$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	0.009	0.013	0.001	0.002

Note: This table tests the production network externality channel. As in Table III of the main text, we ensure that the matched peer firms are neither suppliers nor customers of the treated firms. We also require that the matched peer firms do not share any common customers or any common suppliers with the treated firms. Different from Table III of the main text, we further remove matched peer firms related to the treated firms vertically in the DID analysis. We define two firms as vertically connected if their vertical relatedness scores are within top 10% of all firm pairs (see, Frésard, Hoberg and Phillips, 2020). The regression specification and the definition of the dependent and independent variables are explained in Table III 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.

Table OA.15: Testing the lender commonality channel.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$Distress_{i,t}$		$DD_{i,t}$		$PM_{i,t}$		$Markup_{i,t}$	
$Treat_{i,t} \times Post_{i,t}$	0.035**	0.035**	−0.171**	−0.172**	0.001	0.001	−0.001	−0.001
	[2.129]	[2.152]	[−2.291]	[−2.317]	[0.115]	[0.111]	[−0.114]	[−0.120]
$Treat_{i,t}$	−0.000	−0.000	0.067	0.067	−0.003	−0.003	−0.004	−0.004
	[−0.013]	[−0.010]	[0.982]	[0.987]	[−0.683]	[−0.691]	[−0.695]	[−0.703]
$Post_{i,t}$	0.063***	0.059***	−0.143***	−0.129***	−0.012*	−0.011*	−0.015**	−0.014**
	[5.547]	[5.312]	[−3.032]	[−2.794]	[−1.925]	[−1.809]	[−2.257]	[−2.129]
$Lender\_Exposure_{i,t-1}$	0.142**	0.139**	0.085	0.088	−0.002	−0.002	0.002	0.003
	[2.098]	[2.056]	[0.278]	[0.287]	[−0.087]	[−0.058]	[0.065]	[0.100]
$\ln(1+n(\mathcal{C}_{i,t}))$		0.048***		−0.152***		−0.014***		−0.018***
		[4.132]		[−2.988]		[−3.385]		[−3.322]
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	50575	50575	46842	46842	50788	50788	50772	50772
R-squared	0.619	0.619	0.704	0.704	0.752	0.752	0.839	0.840
Test p-value: $\beta_1 + \beta_3 = 0$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	0.007	0.017	0.001	0.004

Note: This table tests the lender commonality channel. In the DID analysis, we require the matched peer firms 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 III 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.

Table OA.16: Testing the institutional blockholder commonality channel.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$Distress_{i,t}$		$DD_{i,t}$		$PM_{i,t}$		$Markup_{i,t}$	
$Treat_{i,t} \times Post_{i,t}$	0.027**	0.027**	−0.102**	−0.103**	−0.001	−0.001	−0.001	−0.001
	[2.428]	[2.444]	[−1.963]	[−1.984]	[−0.258]	[−0.275]	[−0.166]	[−0.188]
$Treat_{i,t}$	−0.020**	−0.020**	0.132***	0.133***	0.001	0.001	0.001	0.001
	[−1.963]	[−1.968]	[2.579]	[2.591]	[0.313]	[0.319]	[0.164]	[0.171]
$Post_{i,t}$	0.055***	0.054***	−0.111***	−0.104***	−0.009**	−0.008**	−0.012***	−0.011***
	[7.458]	[7.403]	[−3.427]	[−3.265]	[−2.138]	[−2.054]	[−2.696]	[−2.575]
$\ln(1+n(\mathcal{C}_{i,t}))$		0.015*		−0.079**		−0.007*		−0.010**
		[1.764]		[−2.132]		[−1.751]		[−2.238]
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	125488	125488	100840	100840	124472	124472	124344	124344
R-squared	0.593	0.593	0.663	0.663	0.755	0.755	0.773	0.773
Test p-value: $\beta_1 + \beta_3 = 0$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	0.005	0.007	0.001	0.003

Note: This table tests the institutional blockholder commonality channel. In the DID analysis, we require the matched peer firms 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.

Table OA.17: Controlling for all alternative channels simultaneously.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$Distress_{i,t}$		$DD_{i,t}$		$PM_{i,t}$		$Markup_{i,t}$	
$Treat_{i,t} \times Post_{i,t}$	0.023*	0.023*	−0.039	−0.041	0.001	0.001	0.003	0.003
	[1.739]	[1.760]	[−0.610]	[−0.629]	[0.471]	[0.446]	[0.558]	[0.536]
$Treat_{i,t}$	−0.026*	−0.026*	0.081	0.082	−0.004	−0.003	−0.005	−0.005
	[−1.949]	[−1.958]	[1.218]	[1.233]	[−1.174]	[−1.163]	[−0.918]	[−0.909]
$Post_{i,t}$	0.053***	0.052***	−0.165***	−0.158***	−0.006**	−0.006**	−0.011**	−0.011**
	[4.898]	[4.811]	[−3.191]	[−3.075]	[−2.150]	[−2.039]	[−2.234]	[−2.143]
$Post_{i,t} \times Disaster\_Demand_{i,t}$	−0.010	−0.010	0.065	0.065	0.004	0.004	0.007	0.007
	[−0.744]	[−0.743]	[1.030]	[1.035]	[1.142]	[1.142]	[1.283]	[1.282]
$Post_{i,t} \times Production\_Network_{i,t}$	0.015	0.015	−0.011	−0.011	−0.003	−0.003	−0.005	−0.005
	[1.302]	[1.297]	[−0.199]	[−0.202]	[−1.402]	[−1.402]	[−1.137]	[−1.137]
$Post_{i,t} \times Common\_Lender_{i,t}$	0.005	0.005	0.039	0.040	0.007	0.007	0.011	0.011
	[0.360]	[0.351]	[0.520]	[0.528]	[1.559]	[1.563]	[1.510]	[1.513]
$Post_{i,t} \times Common\_Blockholder_{i,t}$	0.014	0.014	0.022	0.020	0.001	0.001	0.000	0.000
	[1.118]	[1.138]	[0.359]	[0.340]	[0.188]	[0.169]	[0.063]	[0.047]
$Disaster\_Demand_{i,t}$	−0.017	−0.017	0.001	0.001	−0.003	−0.003	−0.006	−0.005
	[−1.249]	[−1.256]	[0.015]	[0.019]	[−0.901]	[−0.895]	[−0.912]	[−0.907]
$Production\_Network_{i,t}$	−0.006	−0.005	−0.010	−0.010	0.002	0.002	0.002	0.002
	[−0.563]	[−0.537]	[−0.172]	[−0.189]	[0.858]	[0.829]	[0.539]	[0.513]
$Common\_Lender_{i,t}$	0.040***	0.040***	−0.244***	−0.243***	−0.013***	−0.013***	−0.022***	−0.022***
	[2.931]	[2.921]	[−3.275]	[−3.263]	[−3.767]	[−3.754]	[−3.783]	[−3.772]
$Common\_Blockholder_{i,t}$	−0.029***	−0.030***	0.082	0.083	0.002	0.002	0.007	0.007
	[−2.808]	[−2.830]	[1.487]	[1.514]	[0.837]	[0.859]	[1.298]	[1.316]
$Lender\_Exposure_{i,t-1}$	0.102**	0.103**	−0.231	−0.233	−0.011	−0.011	−0.017	−0.017
	[2.177]	[2.187]	[−0.988]	[−0.997]	[−1.251]	[−1.268]	[−1.151]	[−1.166]
$\ln(1 + n(\mathcal{C}_{i,t}))$		0.017**		−0.081**		−0.005***		−0.008***
		[2.294]		[−2.226]		[−3.156]		[−2.827]
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	136465	136465	110883	110883	135322	135322	135209	135209
R-squared	0.598	0.598	0.667	0.667	0.793	0.793	0.805	0.805
Test p-value: $\beta_1 + \beta_3 = 0$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	0.002	0.003	0.001	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 peer firms 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 peer firms that: i) are located within 100 miles from any zip code negatively affected by major natural disasters in a given year, 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 peer firms 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 peer firms that share common lenders with the treated firms. $Common\_Blockholder_{i,t}$ is an indicator variable that equals one for the matched peer firms 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.15. 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.

3.6 Additional Results for the Cross-Industry Spillover Effects

Summary Statistics for the Cross-Industry Spillover Analysis. Table OA.18 reports the summary statistics for the variables in Table IX of the main text.

Table OA.18: Summary statistics for the cross-industry spillover analysis.

	Obs. #	Mean	Median	SD	$p10^{th}$	$p25^{th}$	$p75^{th}$	$p90^{th}$
$Distress_{c_j,t}$	7107	−7.826	−7.972	0.656	−8.539	−8.312	−7.484	−6.782
$DD_{c_j,t}$	6882	6.405	5.666	4.630	0.629	2.748	9.560	14.109
$PM_{c_j,t}$	7166	0.314	0.300	0.140	0.131	0.200	0.412	0.538
$Markup_{c_j,t}$	7166	0.400	0.356	0.220	0.141	0.223	0.530	0.773
$ND\_mild^{(1)}_{j,t}$	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
$Distress^{(-c)}_{i,t}$	5174	−7.719	−7.827	0.599	−8.404	−8.160	−7.384	−6.845
$DD^{(-c)}_{i,t}$	5020	5.966	5.484	3.635	1.480	3.240	8.225	11.462
$PM^{(-c)}_{i,t}$	5264	0.324	0.308	0.132	0.154	0.222	0.416	0.528
$Markup^{(-c)}_{i,t}$	5264	0.427	0.379	0.222	0.171	0.257	0.557	0.794
$\widehat{IdShock}^{(c)}_{-i,t}(Distress)$	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
$\widehat{IdShock}^{(c)}_{-i,t}(Markup)$	5264	0.400	0.405	0.014	0.385	0.401	0.405	0.405
$Frac\_Peers\_as\_Customers_{-i,i,t}$	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

Cross-Industry Spillover Effects Outside of Three-Digit SIC Industries. One potential concern about the cross-industry spillover analysis is that we define industries at the four-digit SIC level and thus it is possible that the cross-industry spillover effects may reflect the within-industry spillover effects in industries defined more broadly. To address this concern, we analyze cross-industry spillover effects beyond three-digit SIC industries. Results in Table OA.19 show that $\widehat{IdShock}^{(c)}_{-i,t}$ remains positive and statistically significant for industry spillover effects outside the three-digit SIC industries.

Cross-Industry Spillover Effects Excluding Links with High TNIC Pairwise Similarity Scores. Next, we address a related potential concern that the cross-industry spillover effects may reflect high product similarity of two four-digit SIC industries. To further alleviate this concern, we conduct the cross-industry spillover analysis by competition network links between four-digit SIC industries with high TNIC pairwise similarity scores. 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.20, the coefficient of $\widehat{IdShock}^{(c)}_{-i,t}$ remains positive and statistically significant even after excluding links with high TNIC pairwise similarity scores.

Cross-Industry Spillover Effects Excluding The Largest Firms. In addition, we show that the cross-industry spillover effects remain robust after excluding industries whose common market leaders are mainly the largest firms (i.e., top

Table OA.19: Cross-industry spillover effects outside of the three-digit SIC industries

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$Distress_{i,t}^{(-c)}$		$DD_{i,t}^{(-c)}$		$PM_{i,t}^{(-c)}$		$Markup_{i,t}^{(-c)}$	
$\widehat{IdShock}_{-i,t}^{(c)}$	0.582**	0.572**	0.475**	0.472**	0.484**	0.483**	0.437*	0.438*
	[2.172]	[2.117]	[2.261]	[2.238]	[1.982]	[1.982]	[1.698]	[1.712]
$\widehat{IdShock}_{-i,t}^{(c)} \times Frac\_Peers\_as\_Customers_{-i,i,t}$		0.067		−0.241		0.621		0.956*
		[1.388]		[−0.575]		[1.603]		[1.786]
$\widehat{IdShock}_{-i,t}^{(c)} \times Frac\_Peers\_as\_Suppliers_{-i,i,t}$		−0.128***		−0.816**		−1.189***		−1.480***
		[−2.756]		[−2.504]		[−3.176]		[−2.984]
Controls for aggregate conditions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4907	4903	4774	4770	4989	4985	4989	4985
R-squared	0.017	0.021	0.051	0.056	0.002	0.013	0.001	0.012
Permutation test p-value: $\beta_c = 0$	0.011	0.013	0.015	0.014	0.034	0.033	0.059	0.058

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.

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. As shown in Table OA.21, the coefficient of $\widehat{IdShock}_{-i,t}^{(c)}$ remains positive and statistically significant after dropping these industries, suggesting that the cross-industry spillover effects are not simply driven by the largest firms.

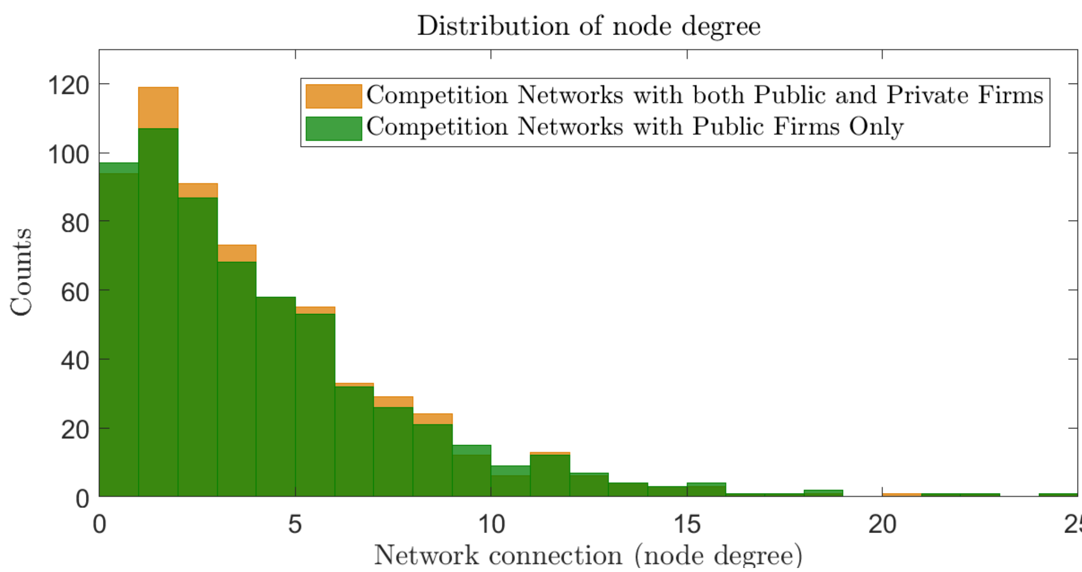


Figure OA.7: Node degree of the competition networks with and without private firms at the four-digit SIC industry level in 1994.

Cross-Industry Spillover Effects with Both Public and Private Firms. 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.

Table OA.20: Cross-industry spillover effects excluding links with high TNIC pairwise similarity scores

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$Distress_{i,t}^{(-c)}$		$DD_{i,t}^{(-c)}$		$PM_{i,t}^{(-c)}$		$Markup_{i,t}^{(-c)}$	
$\widehat{IdShock}_{-i,t}^{(c)}$	0.659**	0.644**	0.469*	0.473**	0.538**	0.531**	0.541**	0.535**
	[2.266]	[2.213]	[1.953]	[1.974]	[2.256]	[2.231]	[2.264]	[2.247]
$\widehat{IdShock}_{-i,t}^{(c)} \times Frac\_Peers\_as\_Customers_{-i,i,t}$		0.095*		0.044		0.570		0.923*
		[1.882]		[0.098]		[1.624]		[1.951]
$\widehat{IdShock}_{-i,t}^{(c)} \times Frac\_Peers\_as\_Suppliers_{-i,i,t}$		−0.122***		−0.736**		−1.000***		−1.292***
		[−2.750]		[−2.102]		[−2.907]		[−2.830]
Controls for aggregate conditions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4098	4095	3984	3981	4188	4185	4188	4185
R-squared	0.020	0.024	0.053	0.056	0.002	0.011	0.002	0.011
Permutation test p-value: $\beta_c = 0$	0.012	0.013	0.030	0.029	0.016	0.017	0.016	0.016

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.

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, 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.

Table OA.22 tabulates the connected four-digit SIC pairs of the competition networks with and without private firms in 1994. Adding private firms only causes a minor change to the competition network. More than 93% of the links remain the same after we take private firms into consideration in forming the network. Figure OA.7 shows the distribution of node degree of the competition networks with and without private firms in 1994. Again, we find that the distribution 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.23, 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.

Table OA.21: Cross-industry spillover effects after excluding industries whose common market leaders are mainly superstar firms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$Distress_{i,t}^{(-c)}$		$DD_{i,t}^{(-c)}$		$PM_{i,t}^{(-c)}$		$Markup_{i,t}^{(-c)}$	
$\widehat{IdShock}_{-i,t}^{(c)}$	0.520*	0.520*	0.352*	0.362*	0.481**	0.475**	0.494*	0.486*
	[1.955]	[1.948]	[1.676]	[1.717]	[1.993]	[1.973]	[1.960]	[1.939]
$\widehat{IdShock}_{-i,t}^{(c)} \times Frac\_Peers\_as\_Customers_{-i,i,t}$		0.096**		−0.073		0.655*		1.006*
		[2.003]		[−0.180]		[1.697]		[1.931]
$\widehat{IdShock}_{-i,t}^{(c)} \times Frac\_Peers\_as\_Suppliers_{-i,i,t}$		−0.106**		−0.404		−0.850**		−1.048**
		[−2.379]		[−1.197]		[−2.340]		[−2.182]
Controls for aggregate conditions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4871	4869	4717	4715	4950	4948	4950	4948
R-squared	0.021	0.024	0.055	0.056	0.002	0.009	0.002	0.009
Permutation test p-value: $\beta_c = 0$	0.022	0.022	0.054	0.048	0.021	0.023	0.028	0.027

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.

Table OA.22: Connected four-digit SIC pairs of the competition networks with and without private firms.

		Competition network with public firms only		
		0	1	Total
Competition network with both public and private firms	0	547,410	78	547,488
	1	77	1,063	1,140
	Total	547,487	1,141	548,628

Table OA.23: Cross-industry spillover effects with both public and private firms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$Distress_{i,t}^{(-c)}$		$DD_{i,t}^{(-c)}$		$PM_{i,t}^{(-c)}$		$Markup_{i,t}^{(-c)}$	
$\widehat{IdShock}_{-i,t}^{(c)}$	0.689**	0.703**	0.418**	0.435**	0.499**	0.594***	0.509**	0.604***
	[2.147]	[2.095]	[2.009]	[1.996]	[2.301]	[2.717]	[2.249]	[2.658]
$\widehat{IdShock}_{-i,t}^{(c)} \times Frac\_Peers\_as\_Customers_{-i,i,t}$		0.090**		0.058		0.714**		1.082**
		[2.169]		[0.166]		[2.215]		[2.478]
$\widehat{IdShock}_{-i,t}^{(c)} \times Frac\_Peers\_as\_Suppliers_{-i,i,t}$		−0.088**		−0.436		−0.816***		−1.008**
		[−2.341]		[−1.510]		[−2.693]		[−2.514]
Controls for aggregate conditions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5431	5250	5277	5099	5532	5345	5532	5345
R-squared	0.020	0.023	0.054	0.054	0.002	0.010	0.002	0.010
Permutation test p-value: $\beta_c = 0$	0.016	0.014	0.030	0.037	0.016	0.003	0.016	0.005

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.

4 Evidence from the AJCA Repatriation Tax Holiday

Table OA.24: Spillover effects in the setting of the AJCA repatriation tax holiday.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$\Delta Distress_i$		ΔDD_i		ΔPM_i		$\Delta Markup_i$	
$AJCA_i$	0.013	0.011	-0.167	-0.150	-0.015^{*}	-0.014^{*}	-0.026^{*}	-0.025
	[0.168]	[0.137]	[-0.348]	[-0.312]	[-1.733]	[-1.652]	[-1.653]	[-1.556]
$\overline{AJCA}_i$	-0.320^{**}	-0.277^{*}	2.059^{**}	1.886^{**}	0.042^{**}	0.032^{*}	0.079^{**}	0.056^{*}
	[-2.097]	[-1.771]	[2.217]	[1.968]	[2.442]	[1.781]	[2.526]	[1.706]
$Cross\_Industry\_Externality_i$		-0.053		0.246		0.013^{**}		0.029^{***}
		[-1.001]		[0.842]		[2.248]		[2.752]
Observations	629	629	436	436	624	624	622	622
R-squared	0.008	0.010	0.018	0.020	0.010	0.018	0.010	0.023

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 ($\Delta Distress_i$), the change of distance to default (ΔDD_i), the change of gross profit margin (ΔPM_i), and the change of markup ($\Delta Markup_i$) from the pre-AJCA period to the post-AJCA period. The distress level, distance to default, profit margin, and markup 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.

We exploit the setting of the AJCA repatriation tax holiday to investigate the impact of a reduction in financial distress (i.e., a positive distress shock) on industry peers and exploit the setting of the Lehman crisis to examine the impact of an increase in financial distress (i.e., a negative distress shock) on industry peers 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)

In this section, we study the spillover effect of a reduction in financial distress (i.e., a positive distress shock) on peer firms' product market 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 **Hypothesis 1** on the within-industry spillover set forth in Section 2 of the main text, we find that, among firms that were financially constrained prior to the AJCA, a firm would compete less aggressively in the product market 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:

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

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

Table OA.25: Heterogenous spillover effects in the AJCA tax holiday setting.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$\Delta Distress_i$		ΔDD_i		ΔPM_i		$\Delta Markup_i$	
$AJCA_i$	−0.111	−0.099	0.262	0.226	0.006	0.003	0.014	0.007
	[−0.976]	[−0.861]	[0.382]	[0.331]	[0.451]	[0.219]	[0.606]	[0.312]
$\overline{AJCA}_i \times AJCA_i$	−0.094	−0.087	1.335	1.271	0.004	0.002	0.005	−0.001
	[−0.484]	[−0.449]	[1.086]	[1.040]	[0.188]	[0.085]	[0.118]	[−0.015]
$\overline{AJCA}_i \times (1 - AJCA_i)$	−0.529**	−0.472*	2.855**	2.613*	0.079***	0.064**	0.150***	0.116**
	[−2.289]	[−1.954]	[2.043]	[1.787]	[3.262]	[2.526]	[3.385]	[2.459]
$Cross\_Industry\_Externality_i$		−0.041		0.211		0.011*		0.025**
		[−0.769]		[0.713]		[1.901]		[2.375]
Observations	629	629	436	436	624	624	622	622
R-squared	0.011	0.012	0.020	0.021	0.017	0.023	0.019	0.028

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 ($\Delta Distress_i$), change of distance to default (ΔDD_i), change of gross profit margin (ΔPM_i), and change of markup ($\Delta Markup_i$) from the pre-AJCA period to the post-AJCA period. The distress risk, distance to default, profit margin, and markup 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.

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 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 peers 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 externatilies across firms within an industry.

Table OA.24 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 both profit margin (see columns (5) and (6)) and markup (see columns (7) and (8)). Thus, among firms that were financially constrained prior to the AJCA, a firm would compete less aggressively in the product market 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). 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 verify the prediction of **Hypothesis 1** set forth in Section 2 of the main text, demonstrating the existence of the within-industry spillover effects. In Table OA.25, we further examine the within-industry spillover effects by recognizing that the

Table OA.26: Spillover effects of bond yield spread and CDS spread in the AJCA tax holiday setting.

	(1)	(2)	(3)	(4)
	$\Delta Bond\_yield\_spread_{i,t}(\%)$		$\Delta CDS\_spread_{i,t}(\%)$	
$AJCA_i$	0.038	0.040	0.126**	0.135***
	[0.622]	[0.658]	[2.522]	[2.668]
$\overline{AJCA}_i$	−0.251**	−0.245*	−0.218**	−0.190*
	[−2.014]	[−1.910]	[−1.970]	[−1.740]
$Cross\_Industry\_Externality_i$		−0.017		−0.082*
		[−0.286]		[−1.746]
Observations	316	316	372	372
R-squared	0.014	0.014	0.019	0.026

Note: This table examines the spillover effects of bond yield spread and CDS 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 variables are the change of bond yield spread ($\Delta Bond\_yield\_spread_i$) and change of CDS spread (ΔCDS_spread_i) around the passage of the AJCA. Because the bond yield spread data and the CDS 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.

treated and non-treated firms may be subject to heterogeneous spillover effects from their industry peers (e.g., Berg, Reisinger and Streitz, 2021). We find that the spillover effect is mainly from treated firms to non-treated peer firms within an industry, not the other way around. Moreover, we consider two additional market-based measures for distress level — bond yield spreads and CDS spreads. As shown in Table OA.26, the within-industry spillover effects remain robust for the market-based distress measures.

Table OA.24 also examines the cross-industry spillover effects. Coefficient β_3 is positive and statistically significant for profit margin (see column (6)) and markup (see column (8)). Thus, a firm tends to compete less aggressively in the product market 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 **Hypothesis 2** on the cross-industry spillover effects set forth in Section 2 of the main text.

5 Evidence from the Lehman Crisis

In this section, we study the spillover effect of an increase in distress level (i.e., a negative distress shock) on peer firms' product market 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 (i.e., a negative distress shock) on peer firms' product market 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,[11] $\overline{LEH}_i$ 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.[12]

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.27 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 in the product market 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.

Next, we examine the spillover effects in distress. Specifically, we replace the outcome variables in specification (5.1) with the bond yield spread and CDS spread.[13] We find that coefficient β_2 is positive and statistically significant

[11] We measure firm-specific credit supply shocks following Chodorow-Reich (2014), with the detailed construction methods explained in Section 2.4.

[12] We find that the coefficient β_3 is insignificant in Table OA.27, 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 and the CDS spread data as well.

[13] We focus on the bond yield spread and CDS spread instead of the accounting-based distress measure because the spread measures are market-based and thus more suitable for the Lehman setting which is essentially an event study.

Table OA.27: Spillover effects in the Lehman crisis setting.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	$\Delta\ln(Price\_Geo)_i$		$\Delta\ln(Price\_EW)_i$		$\Delta\ln(Price\_VW)_i$		$\Delta Bond\_spread_i(\%)$		$\Delta CDS\_spread_i(\%)$	
LEH_i	0.039	0.039	0.059	0.059	0.062	0.061	−0.106	−0.099	−0.040	−0.039
	[0.780]	[0.770]	[1.238]	[1.233]	[1.358]	[1.329]	[−0.866]	[−0.801]	[−0.533]	[−0.522]
$\overline{LEH}_i$	−0.200**	−0.205**	−0.217**	−0.209**	−0.191**	−0.206**	0.526**	0.603**	0.368**	0.401**
	[−2.249]	[−2.206]	[−2.520]	[−2.371]	[−2.268]	[−2.372]	[1.982]	[2.122]	[2.076]	[2.074]
$Cross\_Industry\_Externality_i$		0.008		−0.012		0.028		−0.148		−0.041
		[0.191]		[−0.333]		[0.749]		[−0.967]		[−0.478]
Observations	384	384	384	384	384	384	419	419	453	453
R-squared	0.013	0.013	0.015	0.015	0.013	0.014	0.011	0.013	0.011	0.011

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, while the dependent variables in columns (9) and (10) are changes in the CDS 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), and it is explained in Section 2.4. 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.

for both spreads (see columns 7–10), 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.

6 Evidence from Enforcement Against Financial Fraud

We follow Karpoff et al. (2017) and examine firms that have been prosecuted by the SEC and DOJ for Section 13(b) violations. Because violating firms face legal punishment and penalties imposed by the market, their distress risk increases significantly (e.g., Graham, Li and Qiu, 2008; Karpoff, Lee and Martin, 2008), which provides us a nice setting to examine the reaction of their industry peers.[14]

We assemble financial fraud data following Karpoff et al. (2017). First, we collect all enforcement actions brought by the SEC and the US Department of Justice (DOJ) for violations of Section 13(b) of the Securities Exchange Act of 1934. We then match violating firms to the Compustat-CRSP based on firm names. For each financial fraud case, we hand-collect the date of the first public announcement revealing to investors that a future enforcement action is possible (i.e., trigger date) by examining firms' 8-K filings downloaded from the EDGAR system and other news releases covered by the Factiva database. Our merged sample spans the period from 1976 to 2018 and it covers 838 unique violating firms that operate in non-financial industries.

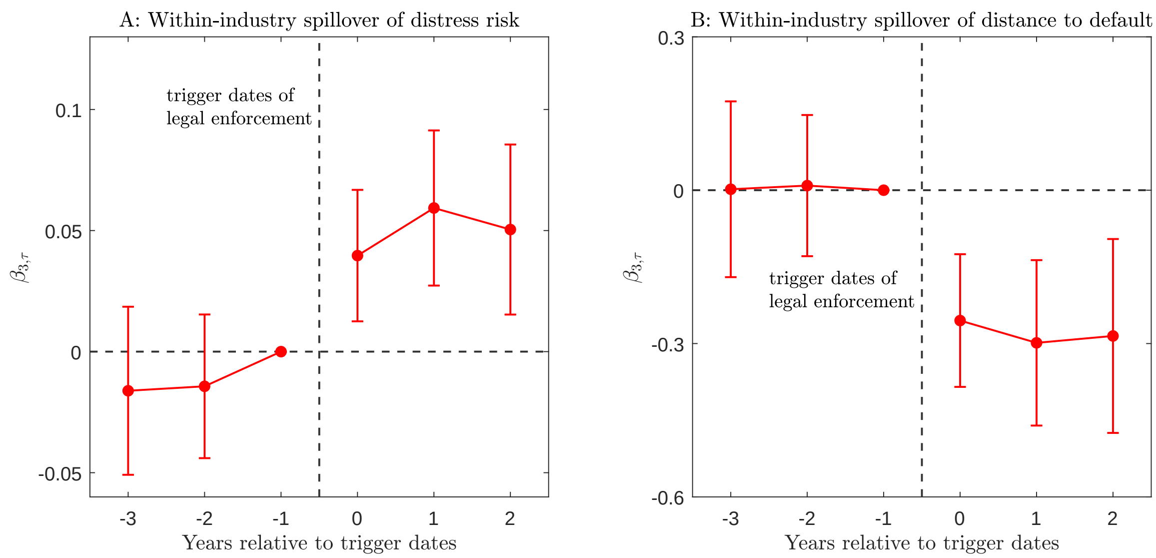
Similar to the setting of natural disasters, we use DID analysis to study the spillover effects from distressed firms to their industry peers. For each violating firm, we match it with up to ten non-violating peer firms in the same four-digit SIC industry based on the firm's asset size, tangibility, and age. We ensure that the matched peer firms are neither suppliers nor customers of the violating firms. We included four yearly observations for each firm in the analysis (i.e., two years before and two years after the year of fraud revelation). In contrast to natural disasters, financial fraud does not occur exogenously. It has been shown that financial fraud tends to peak towards the end of a boom and is then revealed in the ensuing bust (e.g., Povel, Singh and Winton, 2007). To control for business cyclicality, we add past average ROA and stock returns as additional control variables in the DID regressions. Our regression specification is:

$$\begin{aligned} Y_{i,t} =& \beta_1 Treat_{i,t} \times Post_{i,t} + \beta_2 Treat_{i,t} + \beta_3 Post_{i,t} + \beta_4 \ln(1 + n(\mathcal{C}_{i,t})) \\ &+ \beta_5 ROA_{i,t-3:t-1} + \beta_6 StockRet_{i,t-3:t-1} + \theta_i + \delta_t + \varepsilon_{i,t}, \end{aligned} \tag{6.1}$$

where $Treat_{i,t}$ is an indicator variable that equals 1 if firm i commits financial fraud. $Post_{i,t}$ is an indicator variable that equals 1 for observations after the trigger date, which is the date of the first public announcement revealing to investors that future enforcement action is possible. $\ln(1 + n(\mathcal{C}_{i,t}))$ captures the strength of cross-industry spillover through the competition network. $ROA_{i,t-3:t-1}$ is the average ROA of firm i from year $t-3$ to year $t-1$. $StockRet_{i,t-3:t-1}$ is the average stock returns of firm i from year $t-3$ to year $t-1$. The term θ_i represents firm fixed effects, and the term δ_t represents year fixed effects.

Table OA.28 presents the findings from the DID analysis. Consistent with the natural disaster setting, we find that coefficient β_3 is significantly positive for distress risk and significantly negative for distance to default, suggesting that industry peers of the violating firms become more distressed. Coefficient β_3 is significantly negative for gross profitability and markup, suggesting that industry peers of the violating firms engage in more aggressive product market competition after the revelation of fraud. In Figures OA.8 and OA.9, we examine the dynamics of the spillover effects. We find that the spillover effect emerges only after the revelation of fraud, and there is no significant change in distress risk or distance to default prior to the trigger dates. As with natural disasters, we observe that within-industry spillover effects tend to concentrate in tradable industries, as opposed to non-tradable ones (see Table OA.29). Finally, we should point out that the fraud setting has a caveat because there are, on average, fewer than 20 violating firms per year in our sample. The sparsity of the treated firms prevents us from studying the cross-industry spillover effects. Consistent with this caveat, the coefficient for the cross-industry spillover term (i.e., β_4) is statistically insignificant, as shown in Tables OA.28 and OA.29.

[14] We limit our analysis to fraud cases in which firms receive at least $0.25 million in monetary fines from the US government to ensure that the violating firms face sizable legal penalties. Our findings are robust to other cutoffs.



Note: This figure shows the within-industry spillover effects of distress risk around legal enforcement actions against financial fraud. For each violating firm, we match it with up to 10 non-violating peer firms in the same four-digit SIC industry based on firm asset size, tangibility, and age. We ensure that the matched peer firms are neither suppliers nor customers of the treated firms. The analysis includes six yearly observations for each firm, including 3 years before and 3 years after the trigger date, which is the date of the first public announcement revealing to investors that future enforcement action is possible. To estimate the dynamics of the spillover effect, we consider the yearly regression specification as follows: $Y_{i,t} = \sum_{\tau=-3}^{2} \beta_{1,\tau} \times Treat_{i,t} \times Fraud_{i,t-\tau} + \beta_2 \times Treat_{i,t} + \sum_{\tau=-3}^{2} \beta_{3,\tau} \times Fraud_{i,t-\tau} + \beta_4 \ln(1 + n(\mathcal{C}_{i,t})) + \beta_5 ROA_{i,t-3:t-1} + \beta_6 StockRet_{i,t-3:t-1} + \theta_i + \delta_t + \varepsilon_{i,t}$. The dependent variable ($Y_{i,t}$) is the distress risk ($Distress_{i,t}$) and the distance to default ($DD_{i,t}$) in panels A and B, respectively. $Treat_{i,t}$ is an indicator variable that equals 1 if firm i commits financial fraud. $Fraud_{i,t-\tau}$ is an indicator variable that equals 1 if the trigger date of the legal enforcement action against firm i (when firm i is a treated firm) or the treated firm to which firm i is matched (when firm i is a matched non-treated firm) takes place in year $t - \tau$. $\ln(1 + n(\mathcal{C}_{i,t}))$ captures the strength of cross-industry spillover effect, and it is the natural log of 1 plus the number of industries connected to firm i's industry through competition networks and containing violating firms in year t. $ROA_{i,t-3:t-1}$ is the average ROA of firm i from year $t - 3$ to year $t - 1$. $StockRet_{i,t-3:t-1}$ is the average stock returns of firm i from year $t - 3$ to year $t - 1$. The term θ_i represents firm fixed effects, and the term δ_t represents year fixed effects. When running the regression, we impose $\beta_{1,-1} = \beta_{3,-1} = 0$ to avoid collinearity in categorical regressions, and by doing this, we set the years immediately preceding the years of the trigger date as the benchmark. The sample of this figure spans from 1976 to 2018. We exclude firms in financial industries from the analysis. The figure plots estimated coefficients $\beta_{3,\tau}$ with $\tau = -3, -2, \cdots, 2$, as well as their 90% confidence intervals with standard errors clustered at the firm level. The vertical dashed lines represent the trigger dates of the legal enforcement actions against financial fraud.

Figure OA.8: Within-industry spillover effects of distress risk in the financial fraud setting.

Table OA.28: Evidence from legal enforcement actions against financial frauds.

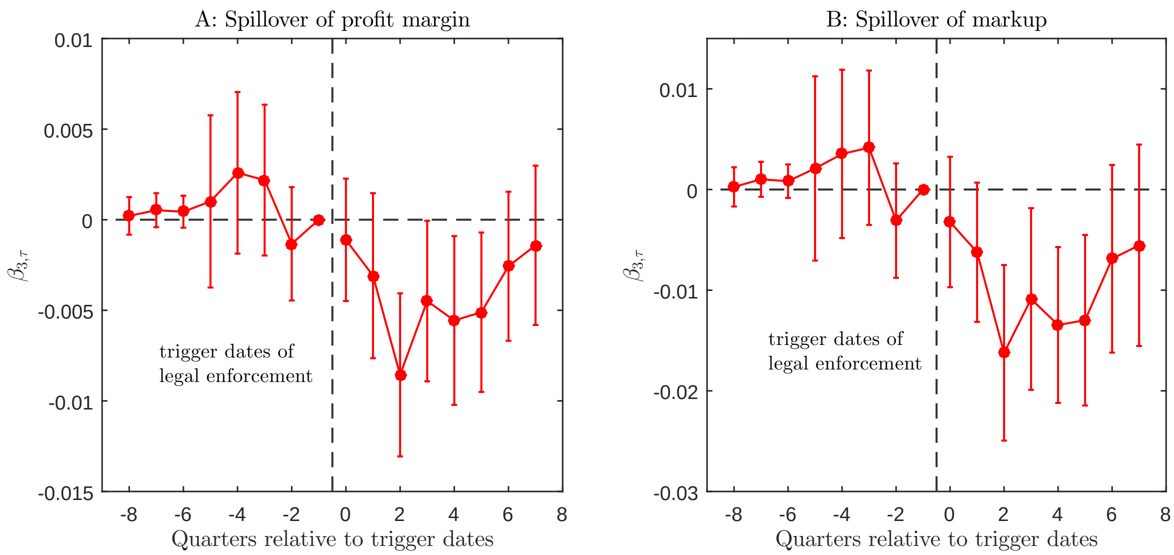
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$Distress_{i,t}$		$DD_{i,t}$		$PM_{i,t}$		$Markup_{i,t}$	
$Treat_{i,t} \times Post_{i,t}$	0.303***	0.303***	−1.057***	−1.058***	−0.008	−0.008	−0.020	−0.020
	[5.123]	[5.124]	[−3.725]	[−3.730]	[−1.202]	[−1.204]	[−1.554]	[−1.558]
$Treat_{i,t}$	−0.025	−0.025	−0.260	−0.262	0.003	0.003	0.012	0.012
	[−0.454]	[−0.455]	[−0.699]	[−0.704]	[0.383]	[0.382]	[0.636]	[0.632]
$Post_{i,t}$	0.057***	0.058***	−0.283***	−0.256***	−0.008***	−0.008***	−0.015***	−0.014**
	[3.682]	[3.677]	[−3.341]	[−3.037]	[−2.631]	[−2.512]	[−2.488]	[−2.280]
$\ln(1+n(\mathcal{C}_{i,t}))$		−0.005		−0.142		−0.001		−0.005
		[−0.205]		[−1.210]		[−0.324]		[−0.615]
$ROA_{i,t-3:t-1}$	0.113**	0.114**	0.578**	0.582**	−0.011	−0.011	−0.032	−0.032
	[1.995]	[1.995]	[1.975]	[1.989]	[−0.501]	[−0.500]	[−0.786]	[−0.784]
$StockRet_{i,t-3:t-1}$	−0.095***	−0.096***	0.462***	0.459***	0.006	0.006	0.011	0.011
	[−2.617]	[−2.621]	[2.902]	[2.894]	[0.954]	[0.947]	[0.884]	[0.872]
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9693	9693	8009	8009	9817	9817	9813	9813
R-squared	0.693	0.693	0.775	0.775	0.878	0.878	0.892	0.892
Test p-value: $\beta_1 + \beta_3 = 0$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	$<10^{-3}$	0.016	0.018	0.004	0.006

Note: This table presents the results of a DID analysis examining the response of distress risk and gross profit margin to legal enforcement actions against financial fraud of peer firms. For each violating firm, we match it with up to 10 non-violating peer firms in the same four-digit SIC industry based on firm asset size, tangibility, and age. We use a relatively high matching ratio to reduce noise because there are, on average, fewer than 20 violating firms per year in our sample. We ensure that the matched peer firms are neither suppliers nor customers of the violating firms. For each firm, we include four yearly observations in the analysis. Specifically, for each firm, we include two years before and two years after the trigger date, which is the date of the first public announcement revealing to investors that a future enforcement action is possible. The regression specification is: $Y_{i,t} = \beta_1 Treat_{i,t} \times Post_{i,t} + \beta_2 Treat_{i,t} + \beta_3 Post_{i,t} + \beta_4 \ln(1+n(\mathcal{C}_{i,t})) + \beta_5 ROA_{i,t-3:t-1} + \beta_6 StockRet_{i,t-3:t-1} + \theta_i + \delta_t + \varepsilon_{i,t}$. The dependent variables in columns (1) to (4) are the distress risk ($Distress_{i,t}$), distance to default ($DD_{i,t}$), gross profit margin ($PM_{i,t}$), and markup ($Markup_{i,t}$), respectively. $Treat_{i,t}$ is an indicator variable that equals 1 if firm i commits financial fraud. $Post_{i,t}$ is an indicator variable that equals 1 for observations after the trigger dates. $\ln(1+n(\mathcal{C}_{i,t}))$ captures the strength of cross-industry spillover effects, and it is the natural log of 1 plus the number of industries connected to firm i's industry through competition networks and containing violating firms in year t. $ROA_{i,t-3:t-1}$ is the average ROA of firm i from year $t-3$ to year $t-1$. $StockRet_{i,t-3:t-1}$ is the average stock returns of firm i from year $t-3$ to year $t-1$. The term θ_i represents firm fixed effects, and the term δ_t represents year fixed effects. In the last row of the table, we present the p-value for the null hypothesis that the total treatment effect for the treated firms is zero (i.e., $\beta_1 + \beta_3 = 0$). The sample of this table spans from 1976 to 2018. We exclude firms in financial industries from the analysis. 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.29: Heterogeneity between tradable and non-tradable industries.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$Distress_{i,t}$		$DD_{i,t}$		$PM_{i,t}$		$Markup_{i,t}$	
Industry types	Tradable	Non-tradable	Tradable	Non-tradable	Tradable	Non-tradable	Tradable	Non-tradable
$Treat_{i,t} \times Post_{i,t}$	0.209***	0.477***	−0.785**	−2.664***	−0.007	−0.012	−0.019	−0.014
	[3.002]	[2.719]	[−2.286]	[−3.394]	[−0.743]	[−1.553]	[−1.071]	[−1.367]
$Treat_{i,t}$	0.029	−0.300***	−0.405	2.072**	−0.004	0.012	−0.001	0.014
	[0.449]	[−2.749]	[−0.880]	[2.024]	[−0.426]	[0.910]	[−0.057]	[0.790]
$Post_{i,t}$	0.035*	0.023	−0.211**	−0.162	−0.009**	−0.007	−0.016*	−0.011
	[1.717]	[0.298]	[−1.979]	[−0.364]	[−2.082]	[−1.552]	[−1.863]	[−1.487]
$\ln(1+n(\mathcal{C}_{i,t}))$	−0.005	−0.127	−0.116	0.023	−0.000	0.013*	−0.003	0.016
	[−0.189]	[−0.662]	[−0.720]	[0.032]	[−0.075]	[1.768]	[−0.263]	[1.322]
$ROA_{i,t-3:t-1}$	0.042	0.631	0.889	1.626	0.003	−0.015	−0.011	−0.019
	[0.454]	[1.178]	[1.529]	[0.867]	[0.137]	[−0.348]	[−0.308]	[−0.272]
$StockRet_{i,t-3:t-1}$	−0.107**	−0.379**	0.545***	1.004	0.005	0.007	0.012	0.005
	[−2.257]	[−2.032]	[2.644]	[1.443]	[0.520]	[0.791]	[0.636]	[0.300]
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6163	621	4919	510	6238	622	6235	622
R-squared	0.681	0.744	0.773	0.837	0.871	0.964	0.888	0.963
Test p-value: $\beta_1 + \beta_3 = 0$	$<10^{-3}$	0.008	0.003	0.001	0.085	0.017	0.037	0.022

Note: This table examines the within-industry spillover effects following legal enforcement actions against financial fraud in tradable industries and non-tradable industries. The regression specification is: $Y_{i,t} = \beta_1 Treat_{i,t} \times Post_{i,t} + \beta_2 Treat_{i,t} + \beta_3 Post_{i,t} + \beta_4 \ln(1+n(\mathcal{C}_{i,t})) + \beta_5 ROA_{i,t-3:t-1} + \beta_6 StockRet_{i,t-3:t-1} + \theta_i + \delta_t + \varepsilon_{i,t}$. Columns (1), (3), (5), and (7) present the results in the tradable industries. Columns (2), (4), (6), and (8) present the results in the non-tradable industries. We define tradable and non-tradable industries following previous studies (e.g., Mian and Sufi, 2014; Bernstein et al., 2019; Giroud and Mueller, 2019). Specifically, an industry is defined as tradable if its imports plus exports exceed $10,000 per worker or $500M in total. Tradable industries are essentially manufacturing industries. Non-tradable industries are defined as the retail sector and restaurants. The sample of this table spans from 1976 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.



Note: This figure plots the within-industry spillover effects of profit margin around legal enforcement actions against financial fraud. For each violating firm, we match it with up to 10 non-violating peer firms in the same four-digit SIC industry based on firm asset size, tangibility, and age. We ensure that the matched peer firms are neither suppliers nor customers of the treated firms. For each firm, we include 16 quarterly observations in the analysis. Specifically, for each firm, we include eight quarters before and eight quarters after the trigger date, which is the date of the first public announcement revealing to investors that a future enforcement action is possible. To estimate the dynamics of the spillover effect, we consider the following quarterly regression specification: $Y_{i,t} = \sum_{\tau=-8}^{7} \beta_{1,\tau} \times Treat_{i,t} \times Fraud_{i,t-\tau} + \beta_2 \times Treat_{i,t} + \sum_{\tau=-8}^{7} \beta_{3,\tau} \times Fraud_{i,t-\tau} + \beta_4 \ln(1 + n(\mathcal{C}_{i,t})) + \beta_5 ROA_{i,t-12:t-1} + \beta_6 StockRet_{i,t-12:t-1} + \theta_i + \delta_t + \varepsilon_{i,t}$. The dependent variable ($Y_{i,t}$) is the gross profit margin ($PM_{i,t}$) and markup ($Markup_{i,t}$) in panels A and B, respectively. $Treat_{i,t}$ is an indicator variable that equals 1 if firm i is a firm that commits financial fraud. $Fraud_{i,t-\tau}$ is an indicator variable that equals 1 if the trigger date of the legal enforcement actions against firm i (when firm i is a treated firm) or the treated firm to which firm i is matched (when firm i is a matched non-treated firm) takes place in quarter $t - \tau$. $\ln(1 + n(\mathcal{C}_{i,t}))$ captures the strength of cross-industry spillover effect, and it is the natural log of 1 plus the number of industries connected to firm i's industry through competition networks and containing violating firms in year t. $ROA_{i,t-12:t-1}$ is the average ROA of firm i from quarter $t-12$ to quarter $t-1$. $StockRet_{i,t-12:t-1}$ is the average stock returns of firm i from quarter $t-12$ to quarter $t-1$. The term θ_i represents firm fixed effects, and the term δ_t represents quarter fixed effects. When running the regression, we impose $\beta_{1,-1} = \beta_{3,-1} = 0$ to avoid collinearity in categorical regressions, and by doing this, we set the quarters immediately preceding the quarters of the trigger date as the benchmark. The sample of this figure spans from 1976 to 2018. We exclude firms in the financial industries from the analysis. We plot estimated coefficients $\beta_{3,\tau}$ with $\tau = -8, -7, \cdots, 7$, as well as their 90% confidence intervals with standard errors clustered at the firm level. The vertical dashed lines represent the trigger dates of the legal enforcement actions against financial frauds.

Figure OA.9: Within-industry spillover effects of profit margin in the financial fraud setting.

7 Additional Evidence for Industry Return Predictability

7.1 Value-Weighted Industry Portfolio Returns

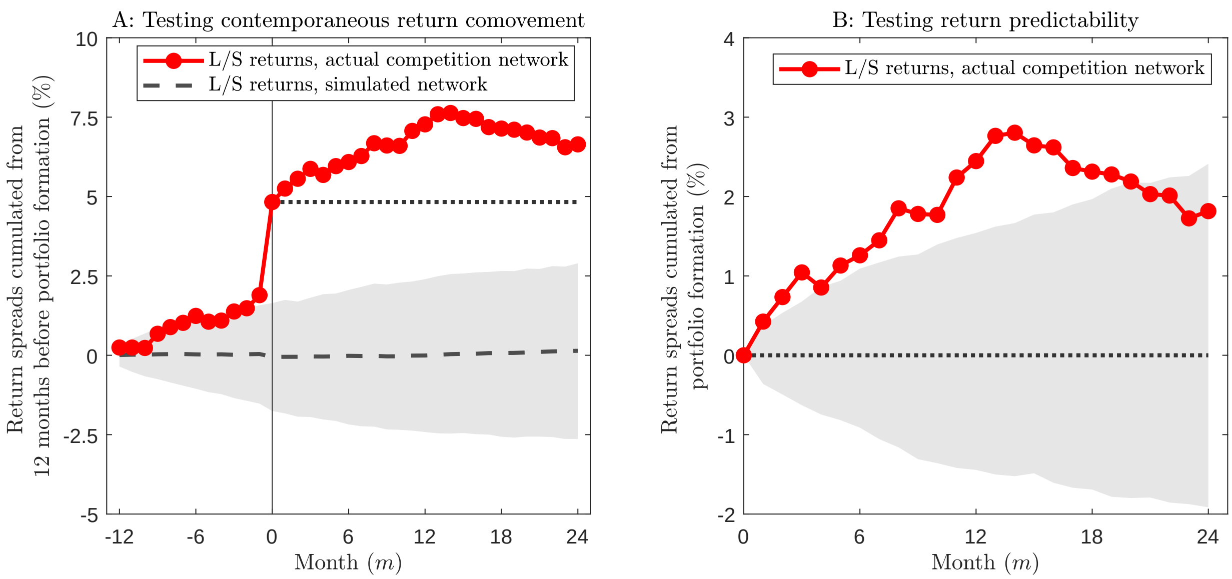
Table OA.30 shows the average excess returns of the industry portfolios sorted on the lagged returns of peer industries. Contrary to Table XII in the main text, we compute the returns of an industry quintile portfolio using value-weighted returns instead of equal-weighted returns across industries within this portfolio. Similar to what we find in the main text, industries with higher lagged peer industry returns are associated with higher excess returns.

Figure OA.10 plots the event-time cumulative returns of the long-short portfolios sorted based on the average returns of peer industries connected through the competition network. The long-short portfolio is a zero-cost portfolio that holds the industries with highest returns of peer industries (top quintile) and sells the industries with lowest returns of peer industries (bottom quintile). Contrary to Figure VIII in the main text, the returns of each industry quintile portfolio are value-weighted returns instead of equal-weighted returns across industries within this portfolio based on industries' 1-month lagged market capitalization. Similar to what we find in the main text, stock prices of the focal industries move contemporaneously in the same direction as their connected peer industries during the sorting period. Furthermore, the focal industries' stock prices continue to drift in the same direction as the initial price response.

Table OA.30: Return spreads for value-weighted industry portfolio returns.

Q1 (low)	Q2	Q3	Q4	Q5 (high)	Q5 − Q1
Panel A: Annualized returns with one-month holding period (%)					
6.40**	6.23**	10.92***	9.34***	10.84***	4.44*
[1.98]	[2.15]	[3.69]	[3.29]	[3.42]	[1.89]
Panel B: Annualized returns with three-month holding period (%)					
7.42**	7.49***	10.30***	9.35***	10.73***	3.31**
[2.49]	[2.71]	[3.83]	[3.40]	[3.55]	[2.00]
Panel C: Annualized returns with six-month holding period (%)					
8.18***	9.06***	9.87***	9.43***	10.13***	1.95*
[2.88]	[3.29]	[3.72]	[3.49]	[3.46]	[1.79]

Note: This table shows the annualized excess industry returns for calendar-time portfolios formed based on lagged returns of peer industries. We form industry portfolios and compute industry-level returns following the same methods as those explained in Table XII in the main text. Contrary to Table XII in the main text, the returns of each industry quintile portfolio are value-weighted returns instead of equal-weighted returns across industries within this portfolio based on industries' 1-month lagged market capitalization. Newey-West standard errors are estimated with one lag, three lags, and six lags in Panels A, B, and C, respectively. The sample period of the data is from Jan 1977 to June 2018. We include t-statistics in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.



Note: This figure plots the event-time cumulative return spreads of the long-short portfolios sorted based on the average returns of peer industries connected through the competition network. The long-short portfolio is a zero-investment portfolio that longs the industries with the highest returns of connected industries (top quintile) and shorts the industries with the lowest returns of connected industries (bottom quintile). Contrary to Figure VIII in the main text, the returns of each industry quintile portfolio are value-weighted returns instead of equal-weighted returns across industries within this portfolio based on industries' 1-month lagged market capitalization. To assess the statistical significance of the contemporaneous stock price response and the industry return predictability, we simulate 1,000 pseudo panels of competition networks by randomly shuffling the nodes (i.e., four-digit SIC industries) while keeping the original network links fixed. Specifically, each simulation involves a one-time reshuffling of the nodes, which remains consistent over time. We then compute the cumulative return spreads of the long-short portfolios sorted based on the simulated predicting signal, namely the returns of peer industries connected in the simulated competition network. The dashed black line represents the average cumulative return spreads of the long-short portfolios across the 1,000 simulated competition networks. The gray area corresponds to the 95% confidence interval ([2.5%, 97.5%]) of the sampling distribution for the cumulative excess returns of the long-short portfolios formed based on the simulated predicting signal. The black dotted line represents a horizontal line that shares the same y-intercept as the solid red line. In panel B, we focus on the predictability aspect by considering the accumulated return spreads of the long-short portfolios starting from month $m = 0$, which are represented by the solid red line. The gray area corresponds to the 95% confidence interval ([2.5%, 97.5%]) of the sampling distribution for the cumulative excess returns of the long-short portfolios starting from month $m = 0$ formed based on the simulated predicting signal. The black dotted line represents a horizontal line that intersects the y-axis at zero.

Figure OA.10: Event-time cumulative returns of the long-short portfolios, value-weighted approach.

7.2 Heterogeneity Across the Age of the Industry Links

We examine the heterogeneity of industry return predictability based on the age of network links. Our hypothesis posits that it takes investors a longer time to fully comprehend the economic connections between focal industries and their peers connected on the competition network when the network links are more recent. Consequently, we expect a slower reaction of focal industries' stock prices to news regarding their connected industries when the network links were formed within a shorter timeframe. Supporting our hypothesis, Table OA.31 provides empirical evidence on this issue. The results reveal that when the network links were formed within the past two years, it takes approximately six months for the stock prices of focal industries to respond to news concerning their connected industries. In contrast, when the network links were established more than two years ago, the price reaction of focal industries is significantly faster.

Table OA.31: Heterogeneity across the age of the industry links in the competition network.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$Ret_{i,t}$		$Ret_{i,t+1}$		$Ret_{i,t+1\to t+3}$		$Ret_{i,t+1\to t+6}$	
link age	young	old	young	old	young	old	young	old
$PeerRet_{i,t}$	0.010	0.059***	0.002	0.018***	0.017	0.032***	0.032**	0.021*
	[1.225]	[8.171]	[0.327]	[3.173]	[1.516]	[3.675]	[2.004]	[1.680]
Constant	0.010***	0.010***	0.010***	0.010***	0.029***	0.029***	0.059***	0.056***
	[3.484]	[3.615]	[3.406]	[3.325]	[3.703]	[3.655]	[4.340]	[4.020]
Average obs./month	145	277	146	278	145	276	144	273
Average R-squared	0.017	0.011	0.009	0.005	0.011	0.004	0.011	0.005

Note: This table examines the heterogeneity of industry return predictability across the network link age using Fama-MacBeth regressions. The dependent variables are industry returns in month t ($Ret_{i,t}$), industry returns in month $t+1$ ($Ret_{i,t+1}$), industry returns from month $t+1$ to month $t+3$ ($Ret_{i,t+1\to t+3}$), and industry returns from month $t+1$ to month $t+6$ ($Ret_{i,t+1\to t+6}$). The main independent variable is the average returns of peer industries in month t ($PeerRet_{i,t}$). In Columns (1), (3), (5), and (7), the peer industries are those connected with the focal industries through the competition network through new links (with link age of one or two years). In Columns (2), (4), (6), and (8), the peer industries are those connected with the focal industries through the competition network through old links (with link age of three years and above). Because common leaders and conglomerates operate in more than one industry, we exclude them in computing industry returns. Industry returns are value-weighted from stock returns of the stand-alone firms in the industries based on firms' 1-month lagged market capitalization. We exclude from the analysis financial and utility industries. Newey-West standard errors are estimated with one lag from Columns (1) to (4), three lags from Columns (5) to (6), and six lags from Columns (7) to (8), respectively. The sample period of the data is from Jan 1977 to June 2018. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

7.3 Competition Networks at the Three-Digit SIC Industry Level

In this section, we conduct a robustness test to redefine the competition network by requiring that the focal industries and peer industries do not share the same three-digit SIC codes. This alternative definition alleviates the concern that the industry return predictability may in fact reflect the within-industry spillover effects under broader industry definition. Results in Tables OA.32, OA.33, and OA.34 demonstrate the robustness of industry return predictability through the competition network when considering only connections between different three-digit SIC industries.

Table OA.32: Return spreads based on competition network links connecting different three-digit SIC industries.

Q1 (low)	Q2	Q3	Q4	Q5 (high)	Q5 − Q1
		Panel A: Annualized returns with one-month holding period (%)			
7.99**	7.67**	8.70***	11.40***	11.32***	3.33***
[2.44]	[2.40]	[2.84]	[3.68]	[3.53]	[2.60]
		Panel B: Annualized returns with three-month holding period (%)			
8.29**	8.38***	9.19***	9.59***	11.30***	3.02***
[2.51]	[2.64]	[2.99]	[3.10]	[3.47]	[4.02]
		Panel C: Annualized returns with six-month holding period (%)			
8.64***	8.64***	9.13***	9.89***	10.53***	1.89***
[2.68]	[2.74]	[3.03]	[3.25]	[3.35]	[3.33]

Note: This table shows the annualized excess industry returns for calendar-time portfolios formed based on lagged returns of peer industries. The peer industries are those connected with the focal industries through the competition network and do not share the same three-digit SIC codes as the focal industries. The sample period of the data is from Jan 1977 to June 2018. We include t-statistics in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table OA.33: Risk-adjusted return spreads based on competition network links connecting different three-digit SIC industries.

CAPM model	Fama-French three-factor model	Carhart four-factor model	Pástor-Stambaugh liquidity-factor model	Stambaugh-Yuan mispricing-factor model	Fama-French five-factor model	Hou-Xue-Zhang q-factor model
			Panel A: Annualized alphas with one-month holding period (%)			
3.74***	3.62***	3.43**	3.63***	3.56**	3.91***	4.76***
[2.87]	[2.76]	[2.51]	[2.67]	[2.50]	[2.87]	[3.37]
			Panel B: Annualized alphas with three-month holding period (%)			
3.37***	3.45***	2.64***	3.50***	1.86**	3.03***	2.90***
[4.28]	[4.12]	[3.12]	[4.10]	[2.03]	[3.27]	[2.95]
			Panel C: Annualized alphas with six-month holding period (%)			
2.12***	2.15***	1.30**	2.13***	1.17**	2.07***	1.88***
[3.84]	[3.65]	[2.21]	[3.70]	[2.01]	[3.07]	[2.75]

Note: This table shows the annualized alphas for calendar-time portfolios formed based on lagged returns of peer industries. The peer industries are those connected with the focal industries through the competition network and do not share the same three-digit SIC codes as the focal industries. The sample period of the data is from Jan 1977 to June 2018. We include t-statistics in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table OA.34: Fama-MacBeth regressions based on competition network links connecting different three-digit SIC industries.

	Panel A: Baseline regressions							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$Ret_{i,t}$		$Ret_{i,t+1}$		$Ret_{i,t+1\rightarrow t+3}$		$Ret_{i,t+1\rightarrow t+6}$	
$PeerRet_{i,t}$	0.057***	0.057***	0.014***	0.015***	0.033***	0.031***	0.044***	0.039***
	[9.418]	[9.414]	[2.686]	[2.860]	[4.012]	[3.912]	[3.652]	[3.689]
$PeerRet_{i,t-11\rightarrow t-1} \times \frac{1}{11}$		0.056***		0.052***		0.118***		0.195**
		[3.542]		[3.200]		[2.947]		[2.493]
$Ret_{i,t}$				−0.022***		−0.006		0.017
				[−3.503]		[−0.549]		[1.051]
$Ret_{i,t-11\rightarrow t-1} \times \frac{1}{11}$		0.064***		0.107***		0.276***		0.407***
		[2.728]		[4.750]		[5.226]		[3.979]
Constant	0.012***	0.010***	0.011***	0.009***	0.034***	0.028***	0.069***	0.059***
	[4.992]	[4.385]	[4.471]	[3.869]	[4.983]	[4.292]	[5.465]	[4.793]
Average obs./month	304	285	306	285	303	283	299	280
Average R-squared	0.008	0.033	0.004	0.045	0.004	0.043	0.005	0.042

	Panel B: Controlling for the returns of customer industries							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$Ret_{i,t}$		$Ret_{i,t+1}$		$Ret_{i,t+1\rightarrow t+3}$		$Ret_{i,t+1\rightarrow t+6}$	
$PeerRet_{i,t}$	0.046***	0.044***	0.016***	0.017***	0.035***	0.033***	0.044***	0.040***
	[6.678]	[6.253]	[2.696]	[2.906]	[3.865]	[3.710]	[3.244]	[3.350]
$PeerRet_{i,t-11\rightarrow t-1} \times \frac{1}{11}$		0.055***		0.052***		0.102**		0.167*
		[3.036]		[2.791]		[2.203]		[1.870]
$Ret_{i,t}$				−0.024***		−0.004		0.006
				[−3.343]		[−0.322]		[0.364]
$Ret_{i,t-11\rightarrow t-1} \times \frac{1}{11}$		0.044*		0.100***		0.255***		0.361***
		[1.855]		[4.344]		[4.535]		[3.367]
$CustomerRet_{i,t}$	0.095***	0.099***	0.026***	0.025***	0.070***	0.057***	0.104***	0.094***
	[10.420]	[10.889]	[2.771]	[2.840]	[4.029]	[3.555]	[3.736]	[3.649]
$CustomerRet_{i,t-11\rightarrow t-1} \times \frac{1}{11}$	0.100***	0.076***	0.103***	0.087***	0.225***	0.167**	0.356**	0.230*
	[3.899]	[3.087]	[3.509]	[3.092]	[3.132]	[2.496]	[2.585]	[1.716]
Constant	0.010***	0.009***	0.010***	0.009***	0.031***	0.028***	0.063***	0.056***
	[4.698]	[4.279]	[4.141]	[3.738]	[4.638]	[4.254]	[5.015]	[4.495]
Average obs./month	240	225	241	225	239	223	235	221
Average R-squared	0.025	0.053	0.018	0.065	0.019	0.064	0.020	0.064

Note: This table reports the slope coefficients and test statistics from Fama-MacBeth regressions. The dependent variables are industry returns in month t ($Ret_{i,t}$), industry returns in month $t+1$ ($Ret_{i,t+1}$), industry returns from month $t+1$ to month $t+3$ ($Ret_{i,t+1\rightarrow t+3}$), and industry returns from month $t+1$ to month $t+6$ ($Ret_{i,t+1\rightarrow t+6}$). The main independent variable is the average returns of peer industries in month t ($PeerRet_{i,t}$), which are the industries connected with the focal industries through the competition network and do not share the same three-digit SIC codes as the focal industries. In Panel A, we control for the average returns of peer industries from month $t-11$ to month $t-1$ ($PeerRet_{i,t-11\rightarrow t-1}$), as well as the historical returns of the focal industries ($Ret_{i,t}$ and $Ret_{i,t-11\rightarrow t-1}$). In Panel B, we add the returns of the industries that are customers of the focal industries ($CustomerRet_{i,t}$ and $CustomerRet_{i,t-11\rightarrow t-1}$) to the list of control variables. Because common leaders and conglomerates operate in more than one industry, we exclude them in computing industry returns. Industry returns are value-weighted from stock returns of the stand-alone firms in the industries based on firms' 1-month lagged market capitalization. We exclude from the analysis financial and utility industries. Newey-West standard errors are estimated with one lag from Columns (1) to (4), three lags from Columns (5) to (6), and six lags from Columns (7) to (8), respectively. The sample period of the data is from Jan 1977 to June 2018. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

7.4 Competition Networks Excluding Links with High TNIC Pairwise Similarity Scores

In this section, we conduct a robustness test to redefine the competition networks by excluding competition network links between four-digit SIC industries with high TNIC pairwise similarity scores. This alternative definition further alleviates the concern that industry return predictability may reflect high product similarity between two four-digit SIC industries. In Tables OA.35, OA.36, and OA.37, we show that industry return predictability through the competition network remains robust even after excluding competition network links among four-digit SIC industries with high TNIC pairwise similarity scores. 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.

Table OA.35: Return spreads after excluding network links with high TNIC pairwise similarity scores.

Q1 (low)	Q2	Q3	Q4	Q5 (high)	Q5 − Q1
Panel A: Annualized returns with one-month holding period (%)					
8.11**	7.23**	9.33***	10.67***	11.07***	2.96**
[2.48]	[2.31]	[2.96]	[3.47]	[3.39]	[2.37]
Panel B: Annualized returns with three-month holding period (%)					
8.15**	8.11***	9.43***	9.40***	11.10***	2.96***
[2.44]	[2.62]	[2.99]	[3.04]	[3.41]	[3.78]
Panel C: Annualized returns with six-month holding period (%)					
8.74***	8.14***	9.80***	9.27***	10.50***	1.76***
[2.67]	[2.63]	[3.17]	[3.03]	[3.30]	[3.28]

Note: This table shows the annualized excess industry returns for calendar-time portfolios formed based on lagged returns of peer industries. We exclude competition network links among four-digit SIC industries with high TNIC pairwise similarity scores. We include t-statistics in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table OA.36: Risk-adjusted return spreads after excluding network links with high TNIC pairwise similarity scores.

CAPM model	Fama-French three-factor model	Carhart four-factor model	Pástor-Stambaugh liquidity-factor model	Stambaugh-Yuan mispricing-factor model	Fama-French five-factor model	Hou-Xue-Zhang *q*-factor model
Panel A: Annualized alphas with one-month holding period (%)						
3.16**	2.93**	2.88**	2.89**	3.20**	3.27**	4.16***
[2.54]	[2.33]	[2.20]	[2.23]	[2.26]	[2.51]	[3.12]
Panel B: Annualized alphas with three-month holding period (%)						
3.29***	3.40***	2.83***	3.34***	1.91*	2.87***	2.83***
[3.98]	[3.89]	[3.08]	[3.71]	[1.86]	[3.27]	[2.96]
Panel C: Annualized alphas with six-month holding period (%)						
2.00***	2.02***	1.59***	1.92***	1.51**	1.87***	1.90***
[3.74]	[3.57]	[2.71]	[3.44]	[2.55]	[3.17]	[3.31]

Note: This table shows the annualized alphas for calendar-time portfolios formed based on lagged returns of peer industries. We exclude competition network links among four-digit SIC industries with high TNIC pairwise similarity scores. We include t-statistics in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table OA.37: Fama-MacBeth regressions after excluding network links with high TNIC pairwise similarity scores.

	Panel A: Baseline regressions							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$Ret_{i,t}$		$Ret_{i,t+1}$		$Ret_{i,t+1\to t+3}$		$Ret_{i,t+1\to t+6}$	
$PeerRet_{i,t}$	0.039***	0.037***	0.014***	0.015***	0.035***	0.034***	0.047***	0.045***
	[6.808]	[6.433]	[2.720]	[2.894]	[4.286]	[4.122]	[3.960]	[3.828]
$PeerRet_{i,t-11\to t-1} \times \frac{1}{11}$		0.060***		0.043***		0.093**		0.142**
		[3.974]		[2.787]		[2.486]		[1.982]
$Ret_{i,t}$				−0.025***		−0.010		0.015
				[−3.851]		[−0.937]		[0.924]
$Ret_{i,t-11\to t-1} \times \frac{1}{11}$		0.055**		0.103***		0.273***		0.399***
		[2.250]		[4.466]		[5.098]		[3.900]
Constant	0.012***	0.010***	0.011***	0.009***	0.034***	0.029***	0.068***	0.060***
	[4.842]	[4.139]	[4.382]	[3.859]	[4.887]	[4.302]	[5.355]	[4.812]
Average obs./month	285	264	286	264	284	262	280	259
Average R-squared	0.007	0.032	0.004	0.046	0.004	0.044	0.005	0.044
	Panel B: Controlling for the returns of customer industries							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$Ret_{i,t}$		$Ret_{i,t+1}$		$Ret_{i,t+1\to t+3}$		$Ret_{i,t+1\to t+6}$	
$PeerRet_{i,t}$	0.031***	0.028***	0.015***	0.015***	0.033***	0.030***	0.039***	0.038***
	[4.885]	[4.271]	[2.683]	[2.707]	[3.772]	[3.373]	[2.929]	[3.005]
$PeerRet_{i,t-11\to t-1} \times \frac{1}{11}$		0.050***		0.035*		0.056		0.076
		[2.866]		[1.959]		[1.314]		[0.991]
$Ret_{i,t}$				−0.027***		−0.009		0.003
				[−3.730]		[−0.779]		[0.171]
$Ret_{i,t-11\to t-1} \times \frac{1}{11}$		0.031		0.096***		0.251***		0.353***
		[1.274]		[4.126]		[4.511]		[3.319]
$CustomerRet_{i,t}$	0.093***	0.096***	0.029***	0.028***	0.073***	0.062***	0.101***	0.093***
	[9.976]	[10.143]	[2.941]	[2.943]	[4.064]	[3.657]	[3.554]	[3.467]
$CustomerRet_{i,t-11\to t-1} \times \frac{1}{11}$	0.094***	0.073***	0.094***	0.084***	0.208***	0.160**	0.344**	0.229
	[3.531]	[2.871]	[3.160]	[2.838]	[2.838]	[2.296]	[2.455]	[1.640]
Constant	0.010***	0.009***	0.010***	0.009***	0.031***	0.028***	0.062***	0.057***
	[4.565]	[4.077]	[4.098]	[3.801]	[4.537]	[4.293]	[4.981]	[4.606]
Average obs./month	228	213	230	213	227	211	224	209
Average R-squared	0.024	0.053	0.018	0.066	0.019	0.065	0.021	0.066

Note: This table reports the slope coefficients and test statistics from Fama-MacBeth regressions. The dependent variables are industry returns in month t ($Ret_{i,t}$), industry returns in month $t+1$ ($Ret_{i,t+1}$), industry returns from month $t+1$ to month $t+3$ ($Ret_{i,t+1\to t+3}$), and industry returns from month $t+1$ to month $t+6$ ($Ret_{i,t+1\to t+6}$). The main independent variable is the average returns of peer industries in month t ($PeerRet_{i,t}$). We exclude competition network links among four-digit SIC industries with high TNIC pairwise similarity scores. In Panel A, we control for the average returns of peer industries from month $t-11$ to month $t-1$ ($PeerRet_{i,t-11\to t-1}$), as well as the historical returns of the focal industries ($Ret_{i,t}$ and $Ret_{i,t-11\to t-1}$). In Panel B, we add the returns of the industries that are customers of the focal industries ($CustomerRet_{i,t}$ and $CustomerRet_{i,t-11\to t-1}$) to the list of control variables. Because common leaders and conglomerates operate in more than one industry, we exclude them in computing industry returns. Industry returns are value-weighted from stock returns of the stand-alone firms in the industries based on firms' 1-month lagged market capitalization. We exclude from the analysis financial and utility industries. Newey-West standard errors are estimated with one lag from Columns (1) to (4), three lags from Columns (5) to (6), and six lags from Columns (7) to (8), respectively. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

7.5 Competition Networks Excluding Links Connected by the Largest Firms

In this section, we conduct a robustness test to redefine the competition networks by excluding network links connected by the largest firms in the economy. This alternative definition alleviates the concern that the industry return predictability may be primarily driven by a few large firms that are less vulnerable to distress shocks. In Table OA.38, we compute the returns of the peer industries by excluding network links connected by common market leaders that are also largest firms in the economy. We show that the industry return predictability through competition network remains robust after we exclude network links connected by common market leaders that are the top 50, 100, and 200 firms ranked by sales in the economy. These findings indicate that the return predictability through the competition network is not solely driven by a small number of large firms in the economy.

Table OA.38: Return spreads after excluding network links connected by the largest firms.

Q1 (low)	Q2	Q3	Q4	Q5 (high)	Q5 − Q1
Panel A: Keep all network links connected by common market leaders (%)					
7.12**	7.19**	9.26***	11.77***	11.27***	4.15***
[2.18]	[2.26]	[2.99]	[3.79]	[3.51]	[3.29]
Panel B: Exclude network links connected by common market leaders in the top 50 firms (%)					
7.18**	7.60**	9.40***	11.28***	11.08***	3.90***
[2.21]	[2.41]	[3.01]	[3.64]	[3.43]	[3.14]
Panel C: Exclude network links connected by common market leaders in top 100 firms (%)					
7.05**	7.52**	9.92***	10.75***	11.35***	4.30***
[2.17]	[2.37]	[3.21]	[3.44]	[3.52]	[3.37]
Panel D: Exclude network links connected by common market leaders in top 200 firms (%)					
7.50**	7.31**	10.20***	10.51***	10.96***	3.45***
[2.32]	[2.29]	[3.28]	[3.35]	[3.35]	[2.63]
Panel E: Exclude network links connected by common market leaders in top 500 firms (%)					
7.21**	7.55**	9.29***	11.36***	10.37***	3.16**
[2.18]	[2.41]	[2.93]	[3.62]	[3.15]	[2.44]

Note: This table shows the average excess industry returns for the industry quintile portfolios sorted on the average 1-month lagged returns of peer industries connected through the competition network. The method that we use to construct portfolio returns is explained in Table XII in the main text. In panel A, we keep all network links connected by common market leaders. This panel is the same as panel A of Table XII in the main text. In panels B to E, we compute the returns of the peer industries by excluding network links connected by common market leaders that are the top 50, 100, 200, and 500 firms ranked by sales in the economy, respectively. The sample period of the data is from July 1977 to June 2018. We include t-statistics in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

7.6 Competition Networks with Both Public and Private Firms

In this section, we incorporate private firms into the construction of the competition network to capture potential industry links missed by focusing solely on public firms. Tables OA.39, OA.40, and OA.41 demonstrate that the industry return predictability through the competition network remains robust even after accounting for private firms.

Table OA.39: Return spreads based on the lagged returns of peer industries connected through competition network constructed using both public and private firms.

Q1 (low)	Q2	Q3	Q4	Q5 (high)	Q5 − Q1
		Panel A: Annualized returns with one-month holding period (%)			
7.05**	7.24**	9.43***	11.56***	11.38***	4.33***
[2.13]	[2.32]	[3.06]	[3.67]	[3.56]	[3.45]
		Panel B: Annualized returns with three-month holding period (%)			
7.72**	8.48***	9.01***	9.61***	11.49***	3.78***
[2.32]	[2.70]	[2.91]	[3.08]	[3.53]	[4.90]
		Panel C: Annualized returns with six-month holding period (%)			
8.48***	8.29***	9.34***	9.78***	10.64***	2.16***
[2.60]	[2.65]	[3.06]	[3.21]	[3.35]	[4.02]

Note: This table shows the annualized excess industry returns for calendar-time portfolios, formed based on lagged returns of the peer industries. The peer industries are the industries connected with the focal industries through the competition network constructed using both public and private firms. The sample period of the data is from Jan 1977 to June 2018. We include t-statistics in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table OA.40: Risk-adjusted return spreads based on the lagged returns of peer industries connected through competition network constructed using both public and private firms.

CAPM model	Fama-French three-factor model	Carhart four-factor model	Pástor-Stambaugh liquidity-factor model	Stambaugh-Yuan mispricing-factor model	Fama-French five-factor model	Hou-Xue-Zhang *q*-factor model
			Panel A: Annualized alphas with one-month holding period (%)			
4.75***	4.72***	4.45***	4.64***	4.37***	4.95***	5.08***
[3.68]	[3.58]	[3.30]	[3.39]	[3.01]	[3.69]	[3.62]
			Panel B: Annualized alphas with three-month holding period (%)			
4.15***	4.23***	3.38***	4.13***	2.67***	3.74***	3.37***
[5.16]	[5.06]	[4.15]	[4.74]	[2.92]	[4.24]	[3.66]
			Panel C: Annualized alphas with six-month holding period (%)			
2.38***	2.40***	1.62***	2.29***	1.64***	2.37***	2.16***
[4.48]	[4.38]	[3.06]	[4.32]	[2.87]	[4.06]	[3.87]

Note: This table shows the annualized alphas for calendar-time portfolios formed based on lagged returns of peer industries. The peer industries are the industries connected with the focal industries through the competition network constructed using both public and private firms. The sample period of the data is from Jan 1977 to June 2018. We include t-statistics in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table OA.41: Fama-MacBeth regressions for the competition network constructed using both public and private firms.

	Panel A: Baseline regressions							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$Ret_{i,t}$		$Ret_{i,t+1}$		$Ret_{i,t+1\to t+3}$		$Ret_{i,t+1\to t+6}$	
$PeerRet_{i,t}$	0.069^{***}	0.067^{***}	0.017^{***}	0.020^{***}	0.039^{***}	0.037^{***}	0.049^{***}	0.043^{***}
	[11.238]	[10.807]	[3.212]	[3.771]	[4.547]	[4.346]	[3.822]	[3.630]
$PeerRet_{i,t-11\to t-1} \times \frac{1}{11}$		0.058^{***}		0.051^{***}		0.123^{***}		0.206^{***}
		[3.511]		[3.079]		[3.082]		[2.592]
$Ret_{i,t}$				-0.023^{***}		-0.003		0.023
				[−3.741]		[−0.305]		[1.446]
$Ret_{i,t-11\to t-1} \times \frac{1}{11}$		0.066^{***}		0.110^{***}		0.278^{***}		0.412^{***}
		[2.819]		[4.813]		[5.110]		[3.919]
Constant	0.012^{***}	0.010^{***}	0.011^{***}	0.009^{***}	0.034^{***}	0.027^{***}	0.068^{***}	0.057^{***}
	[5.060]	[4.297]	[4.383]	[3.703]	[4.871]	[4.077]	[5.398]	[4.621]
Average obs./month	317	297	319	297	316	295	311	291
Average R-squared	0.009	0.034	0.005	0.045	0.004	0.043	0.005	0.043
	Panel B: Controlling for the returns of customer industries							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$Ret_{i,t}$		$Ret_{i,t+1}$		$Ret_{i,t+1\to t+3}$		$Ret_{i,t+1\to t+6}$	
$PeerRet_{i,t}$	0.059^{***}	0.057^{***}	0.018^{***}	0.021^{***}	0.042^{***}	0.038^{***}	0.050^{***}	0.047^{***}
	[8.508]	[7.960]	[2.952]	[3.461]	[4.425]	[4.038]	[3.518]	[3.400]
$PeerRet_{i,t-11\to t-1} \times \frac{1}{11}$		0.056^{***}		0.044^{**}		0.094^{**}		0.160^{*}
		[2.934]		[2.314]		[2.050]		[1.859]
$Ret_{i,t}$				-0.024^{***}		0.000		0.013
				[−3.447]		[0.005]		[0.816]
$Ret_{i,t-11\to t-1} \times \frac{1}{11}$		0.043^{*}		0.099^{***}		0.248^{***}		0.357^{***}
		[1.801]		[4.271]		[4.284]		[3.265]
$CustomerRet_{i,t}$	0.091^{***}	0.094^{***}	0.029^{***}	0.031^{***}	0.069^{***}	0.059^{***}	0.102^{***}	0.098^{***}
	[9.985]	[10.337]	[3.224]	[3.535]	[4.119]	[3.772]	[3.799]	[3.904]
$CustomerRet_{i,t-11\to t-1} \times \frac{1}{11}$	0.096^{***}	0.075^{**}	0.099^{***}	0.085^{**}	0.222^{***}	0.163^{**}	0.355^{***}	0.226^{*}
	[3.748]	[3.115]	[3.408]	[3.032]	[3.128]	[2.489]	[2.623]	[1.743]
Constant	0.010^{***}	0.009^{***}	0.010^{***}	0.009^{***}	0.031^{***}	0.027^{***}	0.062^{***}	0.056^{***}
	[4.821]	[4.278]	[4.136]	[3.747]	[4.538]	[4.148]	[4.982]	[4.451]
Average obs./month	248	233	249	233	247	231	243	228
Average R-squared	0.025	0.053	0.017	0.065	0.018	0.064	0.020	0.064

Note: This table reports the slope coefficients and test statistics from Fama-MacBeth regressions. The dependent variables are industry returns in month t ($Ret_{i,t}$), industry returns in month $t+1$ ($Ret_{i,t+1}$), industry returns from month $t+1$ to month $t+3$ ($Ret_{i,t+1\to t+3}$), and industry returns from month $t+1$ to month $t+6$ ($Ret_{i,t+1\to t+6}$). The main independent variable is the average returns of peer industries connected through the competition network in month t ($PeerRet_{i,t}$). The competition network is constructed using both public and private firms. In Panel A, we control for the average returns of peer industries from month $t-11$ to month $t-1$ ($PeerRet_{i,t-11\to t-1}$), and the historical returns of the focal industries ($Ret_{i,t}$ and $Ret_{i,t-11\to t-1}$). In Panel B, we add the returns of the industries that are customers of the focal industries ($CustomerRet_{i,t}$ and $CustomerRet_{i,t-11\to t-1}$) to the list of control variables. Because common leaders and conglomerates operate in more than one industry, we exclude them in computing industry returns. Industry returns are value-weighted from stock returns of the stand-alone firms in the industries based on firms' 1-month lagged market capitalization. We exclude from the analysis financial and utility industries. Newey-West standard errors are estimated with one lag from Columns (1) to (4), three lags from Columns (5) to (6), and six lags from Columns (7) to (8), respectively. The sample period of the data is from Jan 1977 to June 2018. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

References

Acemoglu, Daron, and Pablo D Azar. 2020. "Endogenous production networks." *Econometrica*, 88(1): 33–82.

Aguiar, Mark, and Erik Hurst. 2007. "Life-cycle prices and production." *American Economic Review*, 97(5): 1533–1559.

Aretz, Kevin, Shantanu Banerjee, and Oksana Pryshchepa. 2019. "In the path of the storm: Does distress risk cause industrial firms to risk-shift?" *Review of Finance*, 23(6): 1115–1154.

Argente, David, Munseob Lee, and Sara Moreira. 2018. "Innovation and product reallocation in the great recession." *Journal of Monetary Economics*, 93: 1–20.

Athey, Susan, and Guido W Imbens. 2022. "Design-based analysis in difference-in-differences settings with staggered adoption." *Journal of Econometrics*, 226(1): 62–79.

Baker, Andrew C, David F Larcker, and Charles CY Wang. 2022. "How much should we trust staggered difference-in-differences estimates?" *Journal of Financial Economics*, 144(2): 370–395.

Baker, Scott R, Brian Baugh, and Marco C Sammon. 2020. "Measuring customer churn and interconnectedness." NBER working paper.

Barrot, Jean-Noël, and Julien Sauvagnat. 2016. "Input specificity and the propagation of idiosyncratic shocks in production networks." *Quarterly Journal of Economics*, 131(3): 1543–1592.

Berg, Tobias, Markus Reisinger, and Daniel Streitz. 2021. "Handling spillover effects in empirical research." *Journal of Financial Economics*, 142(3): 1109–1127.

Bernstein, Shai, Emanuele Colonnelli, Xavier Giroud, and Benjamin Iverson. 2019. "Bankruptcy spillovers." *Journal of Financial Economics*, 133(3): 608–633.

Bharath, Sreedhar, Sandeep Dahiya, Anthony Saunders, and Anand Srinivasan. 2007. "So what do I get? The bank's view of lending relationships." *Journal of Financial Economics*, 85(2): 368–419.

Bharath, Sreedhar T, and Tyler Shumway. 2008. "Forecasting default with the Merton distance to default model." *Review of Financial Studies*, 21(3): 1339–1369.

Bonacich, Phillip. 1972. "Factoring and weighting approaches to status scores and clique identification." *Journal of Mathematical Sociology*, 2(1): 113–120.

Borenstein, Severin, and Nancy L Rose. 1994. "Competition and price dispersion in the US airline industry." *Journal of Political Economy*, 102(4): 653–683.

Borenstein, Severin, and Nancy L Rose. 1995. "Bankruptcy and pricing behavior in US airline markets." *American Economic Review P&P*, 85(2): 397–402.

Broda, Christian, and David E. Weinstein. 2010. "Product creation and destruction: Evidence and price implications." *American Economic Review*, 100(3): 691–723.

Busse, Meghan. 2002. "Firm financial condition and airline price wars." *RAND Journal of Economics*, 33(2): 298–318.

Callaway, Brantly, and Pedro HC Sant'Anna. 2021. "Difference-in-differences with multiple time periods." *Journal of Econometrics*, 225(2): 200–230.

Campbell, John, Jens Hilscher, and Jan Szilagyi. 2008. "In search of distress risk." *Journal of Finance*, 63(6): 2899–2939.

Carey, Mark, Mitch Post, and Steven A Sharpe. 1998. "Does corporate lending by banks and finance companies differ? Evidence on specialization in private debt contracting." *Journal of Finance*, 53(3): 845–878.

Cengiz, Doruk, Arindrajit Dube, Attila Lindner, and Ben Zipperer. 2019. "The effect of minimum wages on low-wage jobs." *Quarterly Journal of Economics*, 134(3): 1405–1454.

Chava, Sudheer, and Michael R Roberts. 2008. "How does financing impact investment? The role of debt covenants." *Journal of Finance*, 63(5): 2085–2121.

Chodorow-Reich, Gabriel. 2014. "The employment effects of credit market disruptions: Firm-level evidence from the 2008–9 financial crisis." *Quarterly Journal of Economics*, 129(1): 1–59.

Chodorow-Reich, Gabriel, and Antonio Falato. 2021. "The loan covenant channel: How bank health transmits to the real economy." *Journal of Finance*, 77(1): 85–128.

Coval, Joshua, and Erik Stafford. 2007. "Asset fire sales (and purchases) in equity markets." *Journal of Financial Economics*, 86(2): 479–512.

De Chaisemartin, Clément, and Xavier d'Haultfoeuille. 2020. "Two-way fixed effects estimators with heterogeneous treatment effects." *American Economic Review*, 110(9): 2964–2996.

Demerjian, Peter R, and Edward L Owens. 2016. "Measuring the probability of financial covenant violation in private debt contracts." *Journal of Accounting and Economics*, 61(2-3): 433–447.

Deshpande, Manasi, and Yue Li. 2019. "Who is screened out? Application costs and the targeting of disability programs." *American Economic Journal: Economic Policy*, 11(4): 213–248.

Dou, Winston Wei, Shane Johnson, and Wei Wu. 2022. "Competition network: distress spillovers and predictable industry returns." The Wharton School at University of Pennsylvania Working Papers.

El-Khatib, Rwan, Kathy Fogel, and Tomas Jandik. 2015. "CEO network centrality and merger performance." *Journal of Financial Economics*, 116(2): 349–382.

Faulkender, Michael, and Mitchell Petersen. 2012. "Investment and capital constraints: Repatriations under the American Jobs Creation Act." *Review of Financial Studies*, 25(11): 3351–3388.

Freeman, Linton C. 1977. "A set of measures of centrality based on betweenness." *Sociometry*, 40(1): 35–41.

Frésard, Laurent, Gerard Hoberg, and Gordon M Phillips. 2020. "Innovation activities and integration through vertical acquisitions." *Review of Financial Studies*, 33(7): 2937–2976.

Froot, Kenneth A. 2001. "The market for catastrophe risk: A clinical examination." *Journal of Financial Economics*, 60(2-3): 529–571.

Garmaise, Mark J, and Tobias J Moskowitz. 2009. "Catastrophic risk and credit markets." *Journal of Finance*, 64(2): 657–707.

Giroud, Xavier, and Holger M Mueller. 2019. "Firms' internal networks and local economic shocks." *American Economic Review*, 109(10): 3617–49.

Goodman-Bacon, Andrew. 2021. "Difference-in-differences with variation in treatment timing." *Journal of Econometrics*, 225(2): 254–277.

Gormley, Todd A, and David A Matsa. 2011. "Growing out of trouble? Corporate responses to liability risk." *Review of Financial Studies*, 24(8): 2781–2821.

Graham, John R., Si Li, and Jiaping Qiu. 2008. "Corporate misreporting and bank loan contracting." *Journal of Financial Economics*, 89(1): 44–61.

Grieser, William, and Zack Liu. 2019. "Corporate investment and innovation in the presence of competitor constraints." *Review of Financial Studies*, 32(11): 4271–4303.

Hadlock, Charles, and Miriam Schwartz-Ziv. 2019. "Blockholder heterogeneity, multiple blocks, and the dance between blockholders." *Review of Financial Studies*, 32(11): 4196–4227.

Harford, Jarrad, Cong Wang, and Kuo Zhang. 2017. "Foreign cash: Taxes, internal capital markets, and agency problems." *Review of Financial Studies*, 30(5): 1490–1538.

Henry, David K, Sandra Cooke-Hull, Jacqueline Savukinas, Fenwick Yu, Nicholas Elo, and B Vac Arnum. 2013. "Economic impact of Hurriane Sandy: potential economic activity lost and gained in New Jersey and New York." US Department of Commerce Report.

Hoberg, Gerard, and Gordon Phillips. 2010. "Product market synergies and competition in mergers and acquisitions: A text-based analysis." *Review of Financial Studies*, 23(10): 3773–3811.

Hoberg, Gerard, and Gordon Phillips. 2016. "Text-based network industries and endogenous product differentiation." *Journal of Political Economy*, 124(5): 1423–1465.

Hoberg, Gerard, and Vojislav Maksimovic. 2015. "Redefining financial constraints: A text-based analysis." *Review of Financial Studies*, 28(5): 1312–1352.

Hottman, Colin J., Stephen J. Redding, and David E. Weinstein. 2016. "Quantifying the sources of firm heterogeneity." *Quarterly Journal of Economics*, 131(3): 1291–1364.

Ivashina, Victoria, and David Scharfstein. 2010. "Bank lending during the financial crisis of 2008." *Journal of Financial economics*, 97(3): 319–338.

Karpoff, Jonathan M, Allison Koester, D Scott Lee, and Gerald S Martin. 2017. "Proxies and databases in financial misconduct research." *The Accounting Review*, 92(6).

Karpoff, Jonathan M, D Scott Lee, and Gerald S Martin. 2008. "The cost to firms of cooking the books." *Journal of Financial and Quantitative Analysis*, 43(3): 581–611.

Kim, Ryan. 2021. "The effect of the credit crunch on output price dynamics: The corporate inventory and liquidity management channel." *Quarterly Journal of Economics*, 136(1): 563–619.

Kroft, Kory, Jean-William P Laliberté, René Leal-Vizcaíno, and Matthew J Notowidigdo. 2022. "Salience and taxation with imperfect competition." *Review of Economic Studies*, forthcoming.

Menzly, Lior, and Oguzhan Ozbas. 2010. "Market segmentation and cross-predictability of returns." *Journal of Finance*, 65(4): 1555–1580.

Mian, Atif, and Amir Sufi. 2014. "What explains the 2007–2009 drop in employment?" *Econometrica*, 82(6): 2197–2223.

Miguel, Edward, and Michael Kremer. 2004. "Worms: Identifying impacts on education and health in the presence of treatment externalities." *Econometrica*, 72(1): 159–217.

Morrison, Steven A, and Clifford Winston. 1996. "Causes and consequences of airline fare wars." *Brookings Papers on Economic Activity, Microeconomics*, 1996: 85–131.

Murfin, Justin. 2012. "The supply-side determinants of loan contract strictness." *Journal of Finance*, 67(5): 1565–1601.

Novy-Marx, Robert. 2013. "The other side of value: the gross profitability premium." *Journal of Financial Economics*, 108(1): 1–28.

Phillips, Gordon, and Giorgo Sertsios. 2013. "How do firm financial conditions affect product quality and pricing?" *Management Science*, 59(8): 1764–1782.

Povel, Paul, Rajdeep Singh, and Andrew Winton. 2007. "Booms, busts, and fraud." *Review of Financial Studies*, 20(4): 1219–1254.

Sabidussi, G. 1966. "The centrality index of a graph." *Psychometrika*, 31: 581–603.

Schwert, Michael. 2018. "Bank capital and lending relationships." *Journal of Finance*, 73(2): 787–830.

Seetharam, Ishuwar. 2018. "The indirect effects of hurricanes: Evidence from firm internal networks." Working Paper.

Sun, Liyang, and Sarah Abraham. 2021. "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects." *Journal of Econometrics*, 225(2): 175–199.