For Online Publication

Supplementary Materials for Why Does Structural Change Accelerate in Recessions? The Credit Reallocation Channel

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These supplementary materials contain additional details and results omitted from the main paper in the interest of space. Appendix A provides further evidence that structural change accelerates in recessions. Appendix B provides a more detailed description of the mechanism at the heart of the model along with several illustrations. Appendix C describes the various sources of data and their construction. Appendix D includes a series of extensions and robustness checks for the results based on the collapse of Lehman Brothers described in Section 3 of the paper. Appendix E does the same for the results based on interstate banking deregulation in Section 4. Finally, Appendix F includes additional figures and results from the model described in Section 5.

A Structural Change and Recessions

In this section I provide further evidence for the concentration of structural change in recessions and show that it is visible in other measures of economic activity rather than just employment shares. Figure A.1 repeats the exercise shown in Figure 1 of the main paper for the manufacturing share of nominal value added.

Further evidence for this phenomenon is summarized in Table A.1. The middle three columns show the shares coming from manufacturing for each of these series at the start of 1960, the end of 2018, and the percentage point change over this period. The "Recession Δ" column shows the total change that occurred in each series during years that had a recession. The rightmost column

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shows the share of the total change over this period that occurred during recessions. If the total change from 1960-2018 for each of these series were distributed uniformly across time, the "Ratio" column would show about 0.22 for all variables because that is the unconditional probability of a recession occurring over this period. Instead, this ratio is about one-half for employment and output, more than two-thirds for value added, and almost 0.9 for consumption.

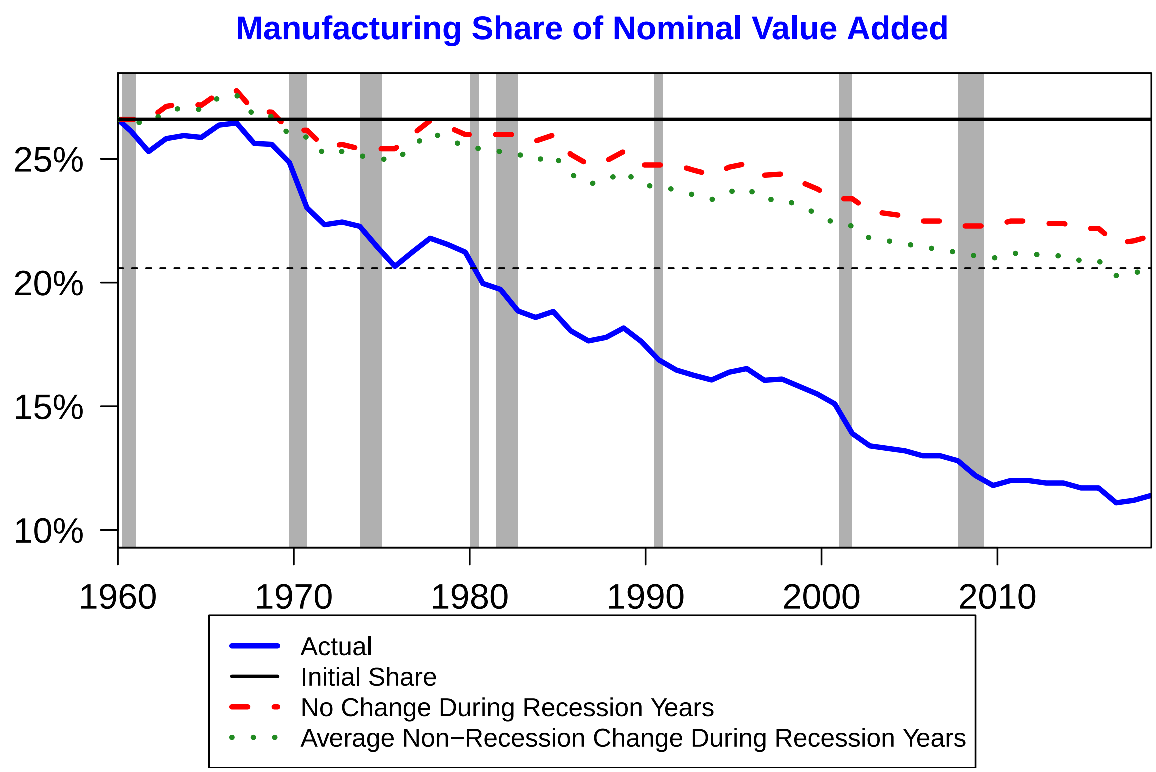


Figure A.1: Change in Manufacturing Share of US Nominal Value Added, 1960-2018

Note: The solid blue line shows the share of nominal GDP coming from the manufacturing sector from 1960-2018. Shaded areas indicate NBER-defined recessions. The dashed red line represents the cumulative change from the beginning of 1960 counting only years without recessions; during years that have at least one quarter classified as a recession this series will be flat, and in non-recession years it will track the blue line. The dotted green line is a counterfactual estimate that replaces the changes during recession years with the average change during non-recession years. Data come from the Bureau of Economic Analysis. Starting in 2005, the BEA reports data at a quarterly frequency; prior to that, I create a quarterly series by linearly interpolating the annual data.

Variable	1960	2018	Δ	Recession Δ	Ratio
Employment	28.9%	8.5%	-20.4pp	-10.2pp	**0.50**
Nominal value added	26.6%	11.4%	-15.2pp	-10.5pp	**0.69**
Nominal consumption	34.7%	23.8%	-10.9pp	-9.7pp	**0.89**
Nominal gross output	41.7%	19.2%	-22.5pp	-12.4pp	**0.55**

Table A.1: Measures of Manufacturing's Share of Economic Activity from 1960-2018

Note: This table provides a decomposition of the change in a variety of measure's of manufacturing's share of economic activity from 1960-2018. The leftmost column lists the measure of manufacturing's share of activity being referenced. The next two columns show the manufacturing share of that variable at the beginning of 1960 and at the end of 2018. The column labeled "Δ" is the total change in the share over this period and corresponds to the difference between the difference between the previous two columns. The "Recession Δ" column is the total change that occurred during years that included at least one quarter classified by the NBER as a recession. The rightmost column shows the share of the total change that has occurred during recession and is calculated as the ratio of the previous two columns. Employment comes from the Current Establishment Survey at the Bureau of Labor Statistics. Manufacturing consumption is calculated from the BEA's consumer expenditure data as expenditure on non-food goods.

B Model Mechanism

The mechanism at the core of my model is represented graphically in Figure B.1. Panel (a) shows a collection of manufacturing and service firms. Firms must obtain credit through a banking relationship—the initial formation of which incurs a fixed cost—in order to produce. Firms receiving credit through banking relationships are shown inside the green border representing the bank and are shaded in. Firms who do not have banking relationships (and are thus unable to produce) are represented by the dashed, empty squares outside of the bank. Over time, structural change increases the value of providing credit to nonmanufacturing firms. This is shown in panel (b). This mechanism does not rely on any one specific cause to drive this structural change; it requires only that it the share of productive resources being allocated to the manufacturing sector declines over time.[1] Fixed adjustment costs to forming new banking relationships mean that credit will not immediately shift to nonmanufacturing firms even though structural change has made them more valuable.

Panels (c) and (d) of Figure B.1 illustrate the destruction of a firm-bank relationship and its

[1] In Section 5 of the main paper I follow Ngai and Pissarides (2007) and model this decline as being driven by a combination of improving manufacturing productivity and CES preferences with an elasticity of substitution between manufacturing and nonmanufacturing goods less than unity. This assumption is not necessary and the decline could just as easily driven by other factors such as income effects.

consequences. One way for this destruction to occur is if a bank collapses. This is represented by the inward shift of the solid green line marking the firms in relationships and the dashed green border illustrating the firms who are forced to shut down because they are no longer receiving credit. If this destruction is not permanent, new bank credit will eventually be made available again, which is represented by the rightward expansion of the bank border to its original position in panel (d). Firm exit will also lead to separation of firm-bank matches. In this setting, it is only the destruction of the match that matters for credit reallocation.

Regardless of whether the openings are created by firm or bank failure, this expansion in credit creates opportunities for new banking relationships. Because structural change has led to a higher value for nonmanufacturing firms, they will be more likely to receive new credit. This change is illustrated in panel (e), which shows a greater share of economic activity devoted to nonmanfuacturing firms relative to the pre-crisis level. To test this mechanism, the ideal experiment—shown in panel (f)—would compare the outcomes of firms attached to a bank that exogenously failed to firms attached to a non-failing bank. This mechanism predicts that nonmanufacturing firms exposed to a failing bank will be more likely to obtain new credit in the aftermath of the crisis and will lead to a decline in the manufacturing share of activity. This prediction is tested in Section 3 of the main paper using the bankruptcy of Lehman Brothers.

This mechanism relies fundamentally on new credit, and this creation can take place during normal times too. Figure B.2 illustrates this by showing the effects of an expansion in available credit, which is shown in panel (a) as an outward shift in the boundary of the bank. Because structural change has improved the value of matches with nonmanufacturing firms, these firms will be disproportionately chosen to fill in the newly available openings. As a result, an exogenous increase in credit supply would be predicted to increase service employment while having no effect on manufacturing employment and thus lead to a reduction in the manufacturing employment share of treated firms. This prediction is tested in Section 4 of the main paper using US interstate banking deregulation in the 1980s.

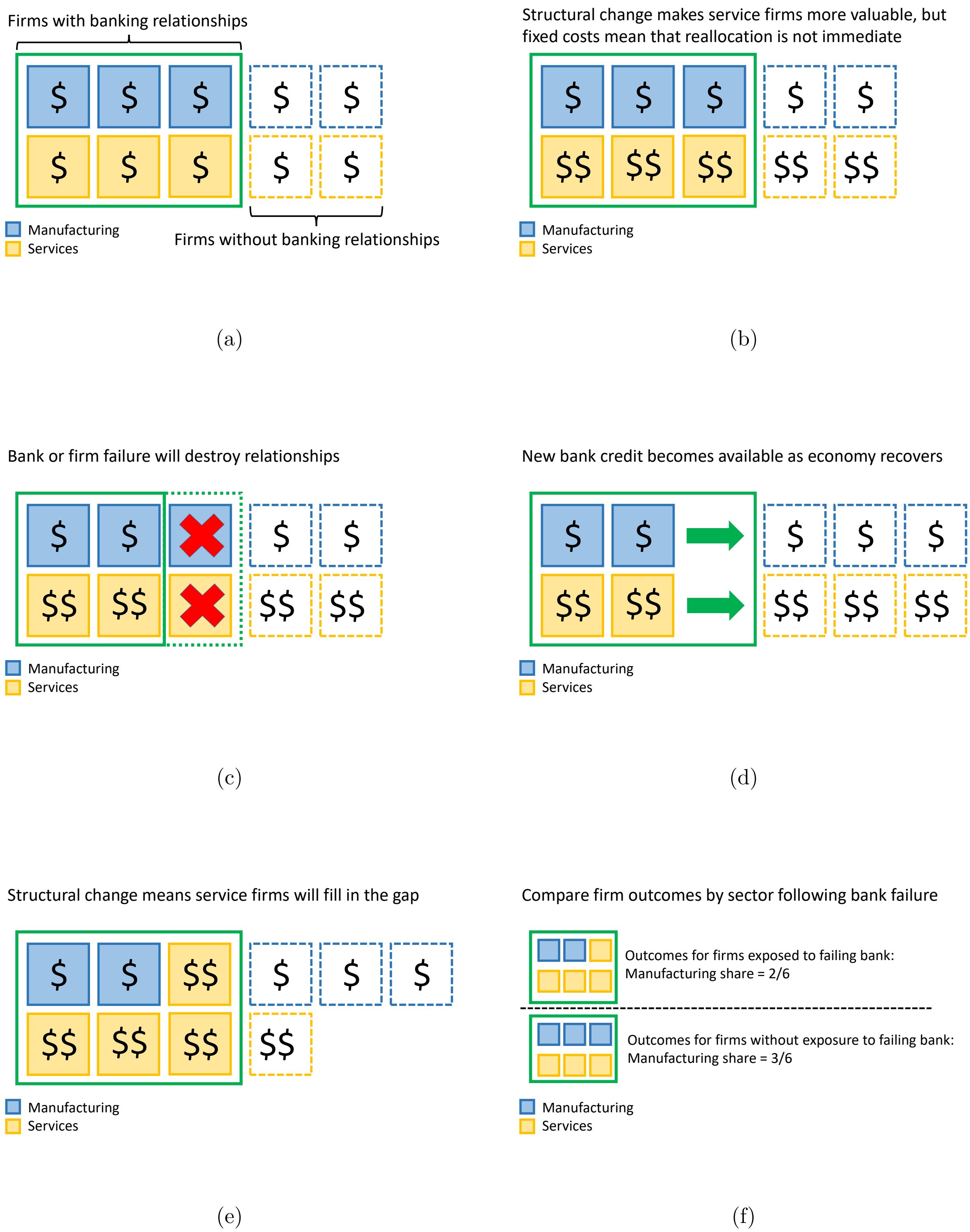
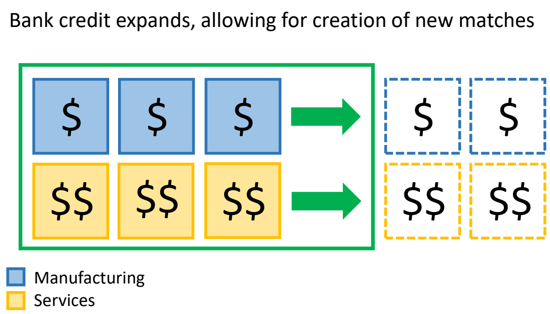
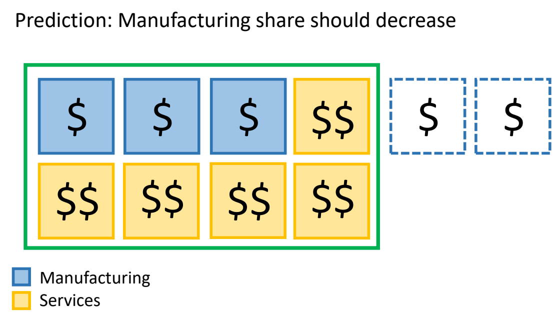


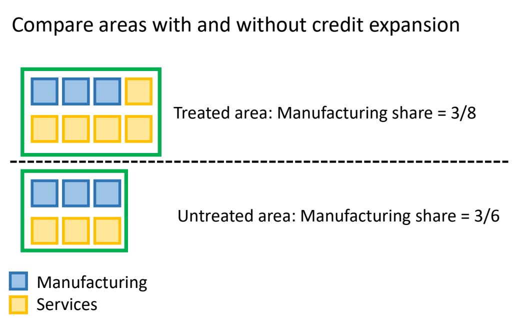
Figure B.1: Illustration of Bank Failure and Structural Change



(a)



(b)



(c)

Figure B.2: Illustration of Credit Expansion and Structural Change

C Appendix: Data

C.1 Example Syndicated Loan

Figure C.1: Example Syndicated Loan

Loan type: Revolving line of credit
Dates active: December 2006 through December 2011
Credit limit: \$1.1bn
Reported purpose: Working capital
"All-in-drawn" spread over London Interbank Offered Rate: 275bp

Figure C.1 shows an example of one of the credit facilities in my data. This particular loan was issued to Ford through a syndicate involving thirteen institutions. DealScan reports the type of loan (in this case, a revolving credit facility) as well as its size (\$1.1bn), active dates (December 2006 through December 2011), and reported purpose (working capital). The data also report the "all-in-drawn" spread, which is measured relative to the London Interbank Offered Rate (LIBOR) and represents the total cost, inclusive of bank fees, to drawing down the entire credit line.

C.2 DealScan Description and Sample Construction

The DealScan data are spread out across several files. First, I merge the "Company" file (which contains information about the firms which are borrowing) with the "Facility" file (which contains detailed information about the each loan) by using the company ID (this identifier is called *borrowercompanyid* in the "Facility" file and *companyid* in the "Company" file). The result is 372,980 observations after merging. This file is merged with the DealScan/Compustat crosswalk file developed in Chava and Roberts (2008). I drop observations for which there is no link between the Compustat identifier (*gvkey*) and the DealScan identifier (*borrowercompanyid*), which leaves 176,560 observations.

Next I merge in the pricing data. I focus on the "all-in-drawn spread", which combines the spread on the coupon with any recurring fees. These spreads are measured relative to the six-month London Interbank Offered Rate (LIBOR), with an adjustment based on historical spreads for loans with non-LIBOR reference rates. I keep loan observations even if they do not have pricing information.

I then merge the lenders file to incorporate information about each lender. Because there are multiple lenders associated with each facility, this increases the number of observations to 2,031,094. I drop loans if they are not made in the US, if they are not denominated in dollars, or if they have missing start/end dates, which drops the number of observations to 559,417.

From this sample I create variables representing the type of loan based on the classification of Ivashina and Scharfstein (2010). Loans are classified as "real investment" if they are for working capital or general corporate purposes, and "restructuring" otherwise. I drop firms in the finance (SIC codes 6000-6700), public administration (9100-9700), and utility (4900-5000) sectors. This leaves 465,423 observations.

I classify a facility as being involved with Lehman Brothers if any of the following are listed as the lender:

- Lehman Brothers Inc
- Lehman Brothers Holdings Inc
- Lehman Commercial Paper Inc
- Lehman Brothers Bank FSB
- Lehman Brothers Commercial Bank
- Lehman Commercial Paper Inc

- Lehman Bank Inc

This classifies Lehman involvement in a total of 2,015 facilities. I classify a facility as being exposed to Lehman's collapse if it satisfies the following properties:

- It was involved with Lehman (as classified above)
- It had a start date prior to 2008
- It had an end date in 2009 or later

I use a similar process to define attachment to three of Lehman's competitors: Goldman Sachs (4,875 facilities), Morgan Stanley (4,616 facilities), or JP Morgan (13,642 facilities).

Goldman Sachs includes any of the following lenders:

- Goldman Sachs & Co
- Goldman Sachs Credit Partners LP
- Goldman Sachs Bank USA
- Goldman Sachs Capital Partners
- Goldman Sachs Lending Partners LLC

Morgan Stanley includes any of the following lenders:

- Morgan Stanley
- Morgan Stanley MUFG Loan Partners LLC
- Morgan Stanley Senior Funding Inc
- Morgan Stanley Bank
- Morgan Stanley Bank NA
- Morgan Stanley Dean Witter & Co
- Morgan Stanley Group

- Morgan Stanley Dean Witter Prime Income Trust
- Morgan Stanley & Co International
- Morgan Stanley Bank AG
- Morgan Stanley Prime Income-Trust
- Morgan Stanley High-Yield Fund

JP Morgan includes any of the following lenders:

- JP Morgan
- JP Morgan Chase Bank NA
- JP Morgan & Co
- JP Morgan Chase
- JP Morgan Delaware
- JP Morgan Securities Inc

C.3 Banking Deregulation Data

C.3.1 Quarterly Census of Employment and Wages

The main outcomes in Section 4 of the main paper come from the Quarterly Census of Employment and Wages (QCEW).[2] These data are compiled from state-level unemployment insurance records and include about 95% of all jobs in the US. Examples of employees NOT in the data would be unincorporated self-employed workers, employees of national security agencies, and railroad employees (who have their own unemployment insurance program). Coverage includes all workers who worked during, or received pay for, the pay period including the 12th day of each month, including part-time workers as well as workers on vacation or paid leave.

The data include information on three primary outcomes: number of employees, number of establishments, and total compensation. Data are available by area, industry, ownership classification, and establishment size and are aggregated to the quarterly level by taking averages of

[2] The data can be found here: https://www.bls.gov/cew/overview.htm.

monthly values. Due to disclosure restrictions, however, not all cuts are available; for example, data by state and establishment size do not include any industry detail, and government sector employment is not available by industry. Because variation in banking regulations occur at the state level, I focus on data covering all non-government establishments broken down by industry and state.

I classify firms by industry based on NAICS post-1990, and SIC prior to that. Because differences in the SIC and NAICS classifications can lead to large jumps in levels when switching from one to the other, I first generate series that can be compared across time for each state. First, I download the NAICS data by industry and state, and keep only establishments with private employment. Next, I aggregate total employment as well as employment in the manufacturing sector for each state-quarter. I calculate the manufacturing employment share in each state-quarter by dividing manufacturing employment by total private employment. In the SIC data, I classify a firm as a manufacturing firm if its industry title is equal to “Manufacturing Division”, and in the NAICS data if its 4-digit NAICS code is 1013. To calculate manufacturing shares that are comparable across the entire sample period, I calculate the quarterly changes in the SIC data from 1975-1990 and retroactively apply these changes to the 1990 level in the NAICS data. I perform a similar exercise for employment and earnings using percent changes instead of level differences.

C.3.2 Interstate Banking Deregulation Dates

The dates used in the main analysis in Section 4 of the main paper are shown in Table C.1. Virtually all of the dates are taken from Strahan (2003) and Amel (1993) with a few exceptions. Hawaii did not pass IBD legislation prior to the passage of the Interstate Banking and Branching Efficiency Act of 1994, which allowed acquisition of out-of-state banks beginning at the end of September 1995. Because this went into effect at the end of the year and because Strahan (2003) classifies Hawaii as not being fully deregulated by 1996, I set 1996 as the deregulation date for Hawaii. Another exception is Maine, which passed legislation allowing reciprocal interstate banking in 1978. Because no state passed such legislation until New York in 1982, I set 1982 as the deregulation date for Maine. All results are virtually unchanged if I use the original dates from Strahan (2003).

State	Year	State	Year
Alabama	1987	Montana	1993
Alaska	1982	Nebraska	1990
Arizona	1986	Nevada	1985
Arkansas	1989	New Hampshire	1987
California	1987	New Jersey	1986
Colorado	1988	New Mexico	1989
Connecticut	1983	New York	1982
Delaware	1988*	North Carolina	1985
District of Columbia	1985	North Dakota	1988
Florida	1985	Ohio	1985
Georgia	1985	Oklahoma	1987
Hawaii	1996**	Oregon	1986
Idaho	1985	Pennsylvania	1986
Illinois	1986	Rhode Island	1984
Indiana	1986	South Carolina	1986
Iowa	1991	South Dakota	1988*
Kansas	1992	Tennessee	1985
Kentucky	1992	Texas	1987
Louisiana	1987	Utah	1984
Maine	1982***	Vermont	1988
Maryland	1985	Virginia	1985
Massachusetts	1983	Washington	1987
Michigan	1986	West Virginia	1988
Minnesota	1986	Wisconsin	1987
Mississippi	1988	Wyoming	1987
Missouri	1986		

Table C.1: Dates of Interstate Banking Deregulation

Note: This table shows the dates of interstate banking deregulation.
* Following the IBD literature, Delaware and South Dakota are excluded from the main analysis due to their role in the development of the credit card industry.
** Hawaii had not passed legislation allowing out-of-state banking by 1996, which was the first full year which the Interstate Banking and Branching Efficiency Act of 1994 was in effect.
*** Maine first passed legislation allowing interstate banking deregulation in 1978, but only allowed entry from banks based in states that had reciprocal arrangements. This first occurred when New York passed its IBD legislation in 1982, and so I set 1982 as the first effective date for Maine. The results are virtually unchanged if I use 1978 as the starting date for Maine instead.

D Robustness Checks and Additional Lehman Results

D.1 Comparison to Lehman's Peers

This section provides evidence that the firms attached to Lehman Brothers were, on the whole, indistinguishable from those who had similar relationships with other large banks who participated in syndicated loan markets. I choose Goldman Sachs, Morgan Stanley (MS), and JP Morgan (JPM) for this exercise. Goldman and MS in particular were US-based institutions with a very similar market position. JPM had a market share roughly six times larger than these three other institutions combined and is included for comparison because its clients are more likely to be representative of the general population of firms receiving syndicated loans.[3] Summary statistics for firms with attachment to one of these banks are found in Tables D.1, D.2, and D.3.

To show that the creditworthiness of firms with Lehman attachment did not differ systematically from those attached to Lehman's peers, I can leverage the frequent overlap of syndicate participants to compare the interest rates charged by different lenders to the same borrower. If Lehman were systematically worse than other banks at observing firms' underlying quality, this should lead to a difference across the spreads Lehman charged and the spreads charged by other banks. Consistent with my definition of Lehman attachment, I define a firm as being attached to one of Goldman, MS, or JPM if they had a revolving line of credit that opened prior to 2008 and was scheduled to extend into 2009 or beyond. Figure D.1 shows these splits.

The average rate across all loans paid by firms with Goldman attachment but without Lehman attachment, represented by the solid red line in the leftmost panel, was virtually identical to the average rates paid by firms that had both Lehman and Goldman attachment.[4] The sharp spike in loan rates for Lehman-attached firms in 2009 is consistent with the idea that these firms were forced to go out and try to obtain new credit at a time when it was particularly scarce, but the fact that the two lines quickly converge in 2010 and beyond suggests that, conditional on receiving a loan, there were not long-term differences in the creditworthiness across these groups.

[3] During the first half of 2008, league tables from Thomson Reuters showed that Lehman Brothers had the 9th-largest volume of proceeds from its role as a syndicate agent, totaling about $9.0bn over 18 new issues. These are quite similar to the corresponding numbers for Goldman ($9.6bn in fees, ranked 8th, 18 new issues) and MS ($5.5bn in fees, ranked 13th, 12 new issues). JPM was ranked first overall with proceeds of $158bn—more than 30% of the total volume—spread out across 297 new issues.

[4] While this overlap would clearly occur if a firm had a line of credit involving both Goldman and Lehman, I classify a firm as having both Lehman and Goldman attachment even if this exposure occurs through separate facilities.

Variable	Manufacturing Goldman	Manufacturing Non-Goldman	Nonmanufacturing Goldman	Nonmanufacturing Non-Goldman
Sales ($mil)	$10,077	$2,501	$7,652	$1,675
Assets ($mil)	$16,404	$2,806	$6,776	$1,875
Emp (thous)	27.4	7.6	43.9	8.8
# of firms	92	3,728	75	3,888
% with new loan	68.5	18.3	66.7	14.4

Table D.1: Summary stats from 2004 for firms with Goldman exposure

Note: These table describes summary statistics for firms with and without exposure to Goldman Sachs. As with my definition of Lehman exposure, I define a firm as being exposed to one of these banks if it had a revolving line of credit that started prior 2008, was scheduled to extend into 2009 or beyond, and was issued through a syndicate that included Goldman Sachs. Firm characteristics come from Compustat after merging the loan data through the matching process outlined in Chava and Roberts (2008).

Variable	Manufacturing JPM	Manufacturing Non-JPM	Nonmanufacturing JPM	Nonmanufacturing Non-JPM
Sales ($mil)	$7,051	$1,845	$5,282	$1,306
Assets ($mil)	$9,825	$1,911	$5,242	$ 1,561
Emp (thous)	22.7	5.2	30.9	6.3
# of firms	578	3,242	429	3,534
% with new loan	61.6	12.0	60.6	9.9

Table D.2: Summary stats from 2004 for firms with JP Morgan exposure

Note: These table describes summary statistics for firms with and without exposure to JP Morgan. As with my definition of Lehman exposure, I define a firm as being exposed to one of these banks if it had a revolving line of credit that started prior 2008, was scheduled to extend into 2009 or beyond, and was issued through a syndicate that included JP Morgan. Firm characteristics come from Compustat after merging the loan data through the matching process outlined in Chava and Roberts (2008).

Variable	Manufacturing MS	Manufacturing Non-MS	Nonmanufacturing MS	Nonmanufacturing Non-MS
Sales ($mil)	$16,474	$2,192	$9,025	$1,584
Assets ($mil)	$22,504	$2,467	$9,681	$1,758
Emp (thous)	47.1	6.7	54.7	8.1
# of firms	125	3,695	101	3,862
% with new loan	69.6	17.8	67.3	14.0

Table D.3: Summary stats from 2004 for firms with Morgan Stanley exposure

Note: These table describes summary statistics for firms with and without exposure to Morgan Stanley. As with my definition of Lehman exposure, I define a firm as being exposed to one of these banks if it had a revolving line of credit that started prior 2008, was scheduled to extend into 2009 or beyond, and was issued through a syndicate that included Morgan Stanley. Firm characteristics come from Compustat after merging the loan data through the matching process outlined in Chava and Roberts (2008).

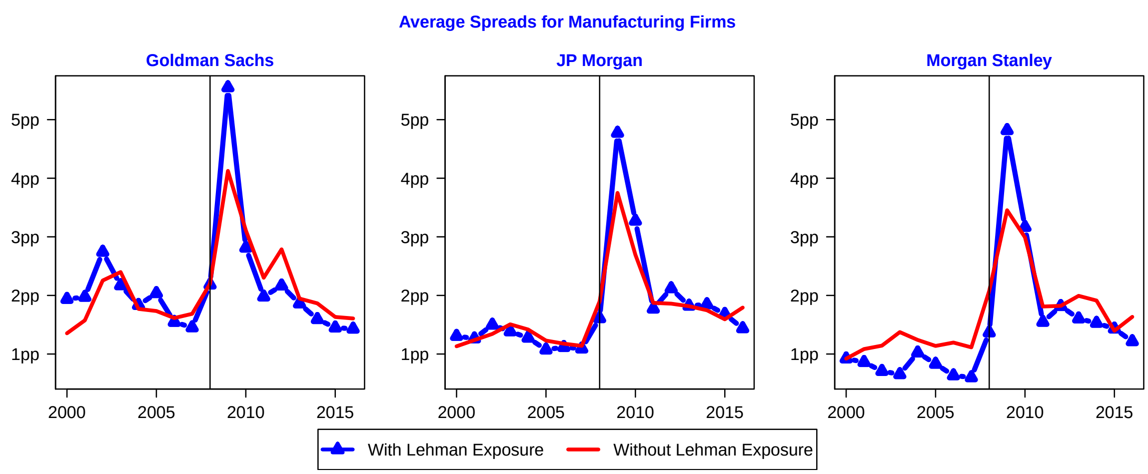


Figure D.1: Average interest rate splits by bank attachment

Note: This figure shows average interest rates paid by firms split by attachment to Goldman Sachs, JP Morgan, or Morgan Stanley. As with my definition of Lehman attachment, I classify a firm as being attached to these banks if a firm had a revolving line of credit that started prior to 2008 and was scheduled to extend into 2009 or beyond. The y-axis measures the average all-in-drawn spread for firms of each type in each year. The interest rate for each firm in each year is weighted by the size of the loan, while the average rates across firms in each group are calculated as a simple average. All calculations are conditional on a firm having a loan with a reported interest rate in each year. Each panel corresponds to the set of firms with attachment to the bank shown at the top. The blue triangle lines represent firms who had attachment to that bank in addition to exposure to Lehman Brothers, either through the same syndicate or through separate facilities. The red lines represent the average spread for firms that were exposed to that bank but had no exposure to Lehman.

Figure D.2 shows the behavior of sales aggregates for manufacturing firms split by attachment to different banks. This figure shows a much larger sales decline post-2009 for manufacturing firms who had attachment to Lehman Brothers than those with similar lines of credit at similar banks. This difference is not reflected in the pre-2009 series, with the sales growth of Lehman-attached firms almost exactly matching the total manufacturing series from 2002-2008. This suggests that even conditional on firms in the same sector who received the same types of loans from similar banks, manufacturing firms with Lehman attachment fared worse in the years following the Great Recession.

Interpreting these results is complicated by the fact that many firms, especially large ones, have multiple credit lines with multiple different banks. As a result, many of the firms counted in the Lehman line will also be counted in those of other banks. Thus to decompose these results even further, I can isolate the firms who had relationships with the other banks but not with Lehman Brothers. These results are shown in Figure D.3. The blue line with triangles shows the same Lehman aggregate series as in Figure D.2. The pink line plots aggregates for manufacturing firms that had open lines of credit with Lehman at the time of its collapse but not with any of Goldman Sachs, JP Morgan, or Morgan Stanley.[5] These firms showed even sharper declines than the total set of Lehman firms from 2009-2012, reaching a decline of up to 50% before settling into roughly the same trend as the total Lehman series by 2015. Thus restricting the sample of Lehman firms to those who did not have similar exposure to a selection of its close competitors leads to effects that are broadly similar as the baseline results, with more pronounced declines in the years immediately following the crisis.

Excluding firms that had loans involving Lehman Brothers significantly changes the aggregates for firms with attachment to Lehman's competitors, however.[6] Once these firms are excluded from these aggregates, the series for all three non-Lehman banks track very closely with the path of all manufacturing firms (shown as the solid line). These results suggest that Lehman attachment had a pronounced impact even relative to similar firms in the same industry with attachment to banks which were in most pre-crisis respects very similar to Lehman.

As a final comparison, Figure D.4 shows the same splits for Lehman's competitors as Figure D.2 but for nonmanufacturing firms. Unlike the manufacturing series, these series all trend very similarly both before and after the crisis. This provides direct evidence against the idea

[5] I only exclude firms who had open lines of credit that satisfied my definition of attachment; firms classified as "Only Lehman" may include firms that received other types of loans from Goldman Sachs, JP Morgan, or Morgan Stanley. These could include any type of loan before the crisis or nonrevolving loans during it.

[6] These series exclude firms with Lehman attachment still allow for overlap among Lehman's competitors. For example, the "Goldman ex-Lehman" series includes firms that had JP Morgan attachment.

that Lehman Brothers was systematically more likely to provide financing to firms that were ultimately more likely to fail.

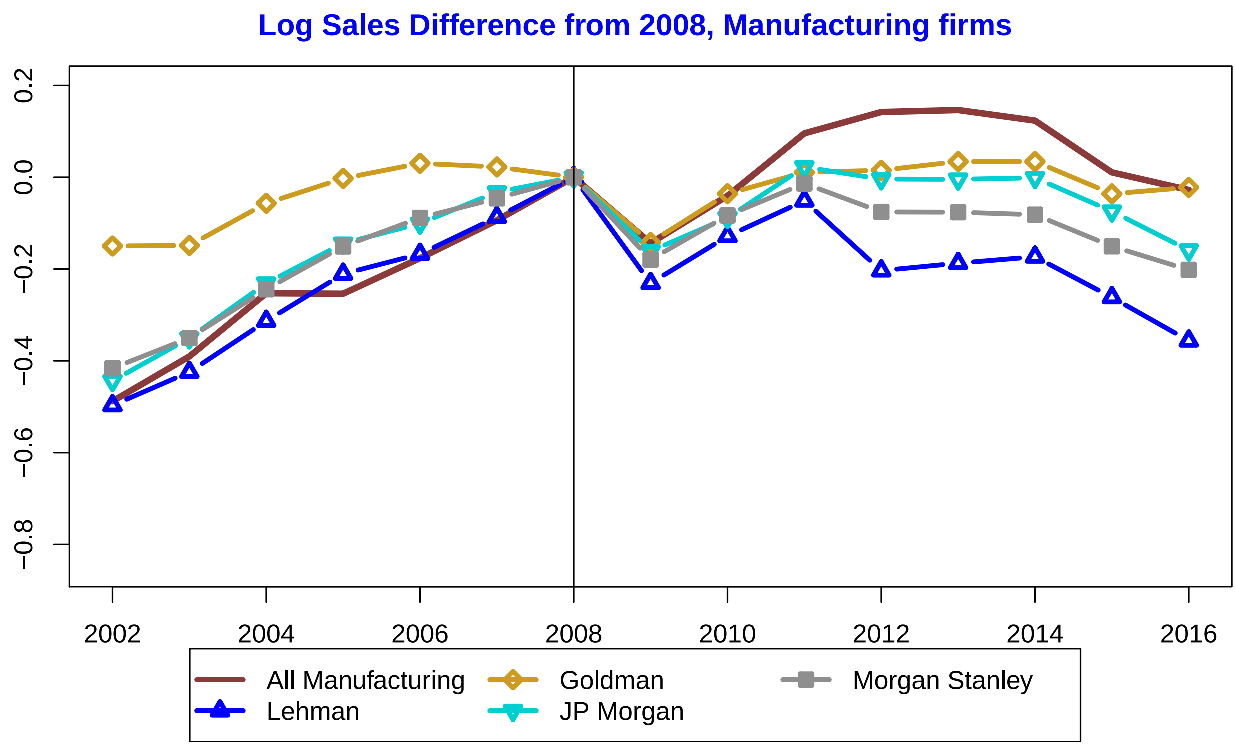


Figure D.2: Aggregate Sales and Employment Growth Relative to 2008

Note: This figure plots the log of total sales for manufacturing firms split by their bank attachment. A firm is classified as having attachment to Lehman, Goldman, JP Morgan, or Morgan Stanley if it had a revolving line of credit through a syndicate that included that bank which started prior to 2008 and was scheduled to extend into 2009 or later. Each line is calculated by taking the sum of all nominal sales for firms in that group, taking the log, and then subtracting the value for each year from the 2008 level for that group.

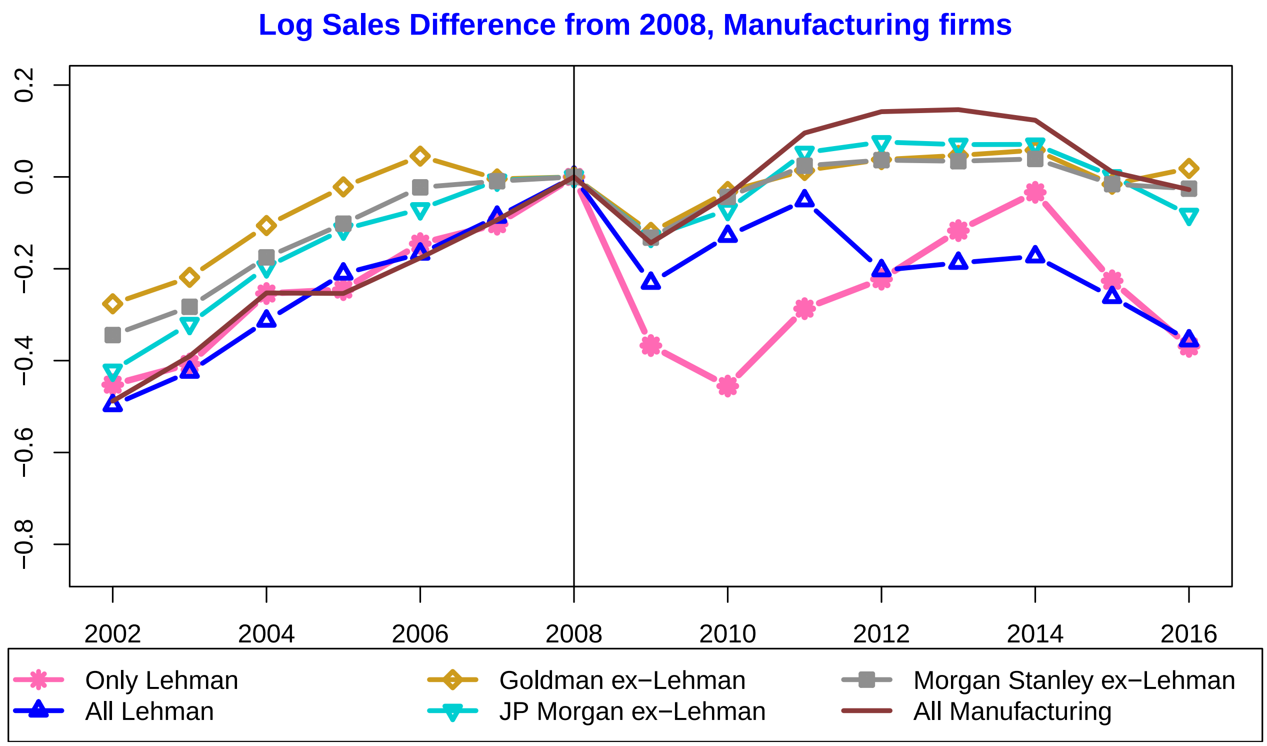


Figure D.3: Aggregate Sales and Employment Growth Relative to 2008

Note: This figure plots the log of total sales for manufacturing firms split by their bank attachment. Bank attachment is defined as having a revolving line of credit through a syndicate that included that bank which started prior to 2008 and was scheduled to extend into 2009 or later. The "ex-Lehman" series correspond to the set of firms that were exposed to that bank but not to Lehman. The "Only Lehman" series represents the set of firms who were exposed to Lehman but not to Goldman, JP Morgan, or Morgan Stanley. Each line is calculated by taking the sum of all nominal sales for firms in that group, taking the log, and then subtracting the value for each year from the 2008 level for that group.

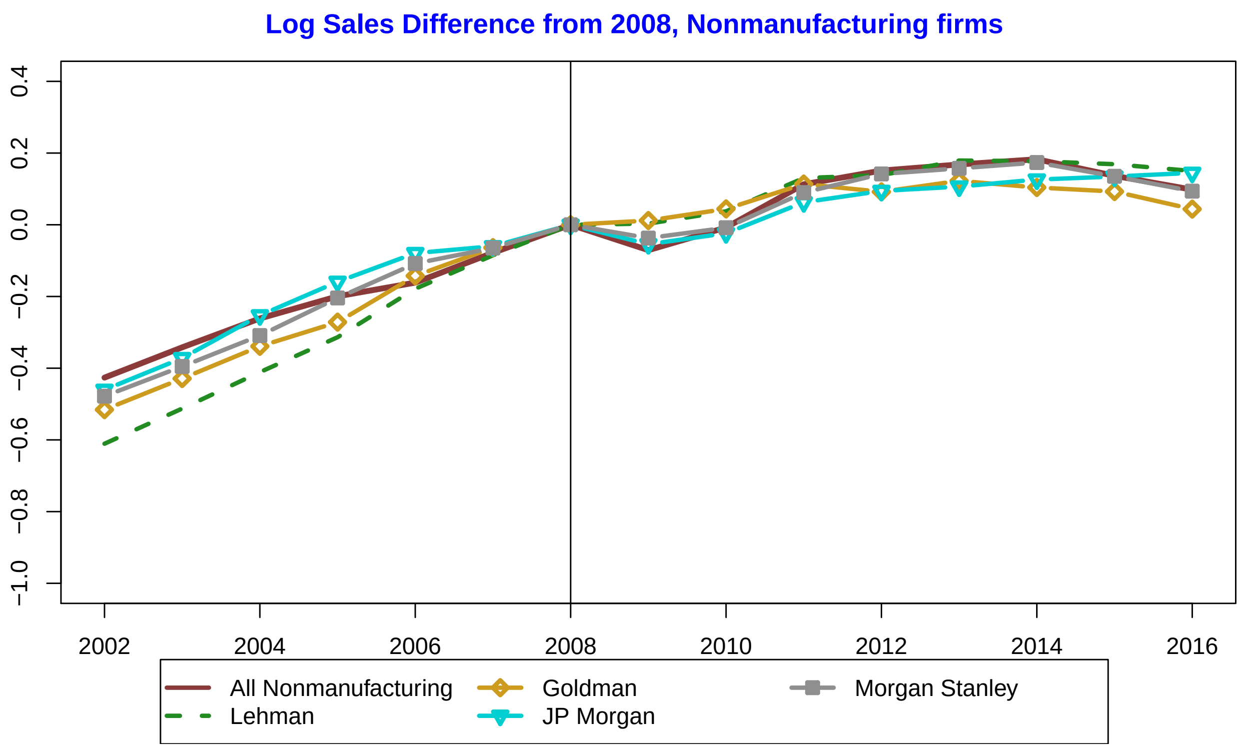


Figure D.4: Aggregate Sales and Employment Growth Relative to 2008

Note: This figure plots the log of total sales for nonmanufacturing firms split by their bank attachment. A firm is classified as having attachment to Lehman, Goldman, JP Morgan, or Morgan Stanley if it had a revolving line of credit through a syndicate that included that bank which started prior to 2008 and was scheduled to extend into 2009 or later. Each line is calculated by taking the sum of all nominal sales for firms in that group, taking the log, and then subtracting the value for each year from the 2008 level for that group.

D.2 Robustness Checks For Bank Exposure Results

D.2.1 Probability of Receiving Any New Loan Facility

In my baseline specification, I estimated the effects of Lehman attachment on the probability of receiving a real investment loan. Table D.4 below shows the estimation results using any loan facility. The coefficient estimates reflect the change in probability of receiving at least one new loan of any type in a given year caused by having one additional line of credit with Lehman brothers at the time of its collapse. The effect on nonmanufacturing firms, which was positive for real investment loans, becomes close to zero and insignificant. Despite this, the effects for manufacturing firms are negative and significant. This suggests that credit reallocation from manufacturing to nonmanufacturing firms was not restricted to a particular type of loan.

	(1)	(2)	(3)	(4)
$\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i$	-0.0149	-0.00990	-0.0108	-0.0171
	(0.0204)	(0.0171)	(0.0259)	(0.0207)
$\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{Mfg\}}$	-0.0396***	-0.0341***	-0.0439**	-0.0314*
	(0.0124)	(0.0122)	(0.0176)	(0.0163)
N	69940	44422	84061	37486

Driscoll-Kraay standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D.4: Probability of Receiving Any New Credit Facility

Note: This table shows the results of estimating Equation 1 in the main paper where the dependent variable is a dummy variable indicating whether a firm received at least one new credit facility of any type in a given year. $Lehman_i$ represents the total number of revolving credit facilities through a syndicate involving Lehman Brothers that were open prior to 2008 and scheduled to extend into 2009 or beyond. The first column represents my baseline specification, which includes data from 2000-2016 and includes only firms which were in Compustat by the start of this period. The second column restricts the sample of firms to only those who were matched to at least one loan in DealScan, regardless of when it occurred. The third column excludes the firm-level controls. The fourth column restricts the sample to only the set of firms who were observed in Compustat in at least one year in 2016 or later.

D.2.2 Alternate Measures of Lehman Exposure

In my baseline specification, I use the total number of revolving credit facilities a firm had that included Lehman as part of the syndicate which were open prior to 2008 and scheduled to extend into at least 2009. In this section, I show that my main results are robust to several alternative measures. The first is a dummy variable indicating whether a firm had at least one revolving line of credit with Lehman as classified previously. The second measure counts only the number of facilities in which Lehman was reported as having a role beyond "Participant". Finally, the third measure calculates the total volume of available credit through revolving facilities involving Lehman scaled by the average sales of each firm from 2006-2008. The results are shown for my baseline specification (corresponding to the first column of the other regression tables). The top of each column shows the outcome variable being referenced.

	(1) $\mathbb{1}^{RealInvest}$	(2) Sales	(3) Employment
$\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i$	0.171^{***}	0.0275^{**}	0.0249
	(0.0415)	(0.0118)	(0.0170)
$\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{Mfg\}}$	-0.0946^{***}	-0.0905^{***}	-0.0763^{***}
	(0.0233)	(0.0166)	(0.0181)
N	69940	69108	68555

Driscoll-Kraay standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D.5: Effects of Lehman Attachment Dummy

Note: This table shows the results of estimating Equation 1 in the main paper where $Lehman_i$ is measured as a dummy indicating whether a firm had at least one revolving credit line exposed to Lehman's bankruptcy. Each column represents a separate regression using my baseline specification (corresponding to the first column in the other regression tables). The dependent variables in each regression are labeled at the top of each column. $\mathbb{1}^{RealInvest}$ is a dummy variable indicating whether a firm received at least one new real investment loan; the other two columns show the results for log sales and log employment.

	(1) $\mathbb{1}^{RealInvest}$	(2) Sales	(3) Employment
$\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i$	0.109***	0.0103	0.00822
	(0.0302)	(0.00976)	(0.0115)
$\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{Mfg\}}$	-0.0248	-0.0801***	-0.0596***
	(0.0253)	(0.0166)	(0.0138)
N	69940	69108	68555

Driscoll-Kraay standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D.6: Effects of Attachment with Lehman Agent

	(1) $\mathbb{1}^{RealInvest}$	(2) Sales	(3) Employment
$\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i$	0.153**	0.0171	0.109***
	(0.0696)	(0.0120)	(0.0303)
$\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{Mfg\}}$	-0.151***	-0.0396***	-0.171***
	(0.0343)	(0.0149)	(0.0262)
N	60201	59637	59182

Driscoll-Kraay standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D.7: Effects of Lehman Attachment as Ratio of Sales

Note: These tables show the results of estimating Equation 1 in the main paper where $Lehman_i$ is measured as the number of revolving facilities exposed to Lehman's collapse in which Lehman had a role beyond "Participant" in Table D.6, and measured as the sum of all revolving credit facilities involving Lehman that started prior to 2008 and extended into 2009 or beyond divided by a firm's average sales from 2006-2008 in Table D.7. Each column represents a separate regression using my baseline specification (corresponding to the first column in the other regression tables). The dependent variables in each regression are labeled at the top of each column. $\mathbb{1}^{RealInvest}$ is a dummy variable indicating whether a firm received at least one new real investment loan; the other two columns show the results for log sales and log employment.

D.2.3 Controlling for Exposure to Lehman's Peers

The main analysis only directly considers exposure to Lehman Brothers. This section shows a set of robustness checks in which I control for firms' attachment to other banks. As with Lehman exposure, I define the measure of a firm's exposure to a bank to be the total number of revolving credit facilities it had with that bank starting prior to 2008 that were scheduled to extend into 2009 or beyond. Specifically, I modify my baseline regression to the following specification, where $i \in \{Lehman,\ Goldman,\ MS,\ JPM\}$:

$$
\begin{aligned}
Y_{i,t} = {} & \alpha_i + \sigma_t + \mathbb{1}_{\{Mfg\}} \times \theta_t + \gamma X_{i,t-1} + \\
& \sum_i \Big(\rho_i \times \mathbb{1}_{\{Year \geq 2009\}} \times Bank_i\Big) + \sum_i \Big(\Omega_i \times \mathbb{1}_{\{Year \geq 2009\}} \times Bank_i \times \mathbb{1}_{\{Mfg\}}\Big) + \epsilon_{i,t}
\end{aligned}
\tag{1}
$$

In the case of the loan for Ford shown in Figure C.1, for example, all four banks were involved in the syndicate. The substantial variation in overlap of bank participation across syndicates suggests that these estimates are capturing the true effect of Lehman exposure rather than simply some other characteristics common to other types of loans. Table D.8 compares these effects for sales and shows that, even after controlling for exposure to Goldman Sachs, Morgan Stanley, and JP Morgan, Lehman firms were adversely affected.

	Goldman	MS	JP Morgan	Lehman
$\mathbb{1}_{\{Year \geq 2009\}} \times Bank_i$	0.008	-0.013	-0.012	0.015**
	(0.006)	(0.014)	(0.012)	(0.006)
$\mathbb{1}_{\{Year \geq 2009\}} \times Bank_i \times \mathbb{1}_{\{Mfg\}}$	0.016	0.008	-0.014	-0.050***
	(0.016)	(0.0233)	(0.012)	(0.001)

Specification (1); Driscoll-Kraay standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D.8: Effects on Sales Including Exposure to Lehman's Peers

Note: This table shows the results of estimating Equation 1 where the dependent variable is log sales. Estimates come from the baseline specification, which corresponds to the first column of the other regressions using Lehman exposure. $Bank_i$ represents the total number of revolving credit facilities though syndicates that included each bank starting before 2008 and extending into 2009 or beyond. Each column corresponds to the coefficients ρ_i and Ω_i for the bank shown at the top of each column.

D.2.4 Comparisons to Pre-Crisis Lehman Loans

Table D.9 shows the estimated effects of Lehman attachment prior to the crisis. For this specification, I define a firm as being attached to Lehman if it had a revolving line of credit through a syndicate that included Lehman with a start date of 2000 or later and a scheduled end date of 2007 or earlier. These coefficients are several orders of magnitude smaller than the baseline estimates and statistically insignificant, suggesting that Lehman exposure outside of the financial crisis did not negatively affect firms' ability to obtain financing.

	(1)	(2)	(3)	(4)
$\mathbb{1}_{\{Year \geq 2009\}} \times LehmanPreCrisis_i$	0.00676	0.00352	0.00842	0.00621
	(0.00583)	(0.00540)	(0.00635)	(0.00598)
$\mathbb{1}_{\{Year \geq 2009\}} \times LehmanPreCrisis_i \times \mathbb{1}_{\{Mfg\}}$	-0.000611	-0.0000330	-0.00300	-0.00111
	(0.00427)	(0.00386)	(0.00365)	(0.00378)
Controls	Y	Y	N	Y
Loans>0	N	Y	N	N
2016 Survivors	N	N	N	Y
N	69940	44422	84061	37486

Driscoll-Kraay standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D.9: Effects of Pre-Crisis Lehman Exposure on Probability of Obtaining New Credit Facilities

Note: This table shows the results of estimating Equation 1 in the main paper where the dependent variable is a dummy variable indicating whether a firm received at least one new credit facility of any type in a given year. $LehmanPreCrisis_i$ represents the total number of revolving credit facilities through a syndicate involving Lehman Brothers that opened in 2000 or later and ended in 2007 or earlier. The first column represents my baseline specification, which includes data from 2000-2016 and includes only firms which were in Compustat by the start of this period. The second column restricts the sample of firms to only those who were matched to at least one loan in DealScan, regardless of when it occurred. The third column excludes the firm-level controls. The fourth column restricts the sample to only the set of firms who were observed in Compustat in at least one year in 2016 or later.

D.2.5 Controlling for Pre-Crisis Spreads

To test whether the estimated effects of Lehman attachment simply reflected the fact that Lehman was lending to riskier firms, I estimate the following regression:

$$\begin{aligned} Y_{i,t} = \alpha_i + \sigma_t + \mathbb{1}_{\{Mfg\}} \times \chi_t + \gamma X_{i,t-1} + \\ \rho \times \mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i + \\ \Omega \times \mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{Mfg\}} + \\ \xi \times \mathbb{1}_{\{Year \geq 2009\}} \times Spread_i^{2000-2007} + \\ \lambda \times \mathbb{1}_{\{Year \geq 2009\}} \times Spread_i^{2000-2007} \times \mathbb{1}_{\{Mfg\}} + \epsilon_{i,t} \end{aligned} \tag{2}$$

As shown in tables D.10 and D.11, Lehman attachment remains negative and significant throughout most specifications even after controlling for these measures, suggesting that Lehman was not simply lending to riskier firms.

	(1)	(2)	(3)	(4)
$\mathbb{1}_{\{Year \geq 2009\}} \times Spread_i^{2000-2007}$	-0.0199***		-0.0193***	-0.0135***
	(0.00433)		(0.00446)	(0.00515)
$\mathbb{1}_{\{Year \geq 2009\}} \times Spread_i^{2000-2007} \times \mathbb{1}_{\{Mfg\}}$	-0.00286		-0.00532	-0.0143
	(0.0141)		(0.0135)	(0.0184)
$\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i$	0.0656***		0.0672***	0.0686***
	(0.0221)		(0.0230)	(0.0259)
$\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{Mfg\}}$	-0.0491**		-0.0543***	-0.0555***
	(0.0201)		(0.0190)	(0.0151)
Controls	Y		N	Y
2016 Survivors	N		N	Y
N	34216		36278	20247

Driscoll-Kraay standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D.10: Effects on Probability of Obtaining New Loans Controlling for Spreads

Note: This table shows the results of estimating Equation 2 where the dependent variable is a dummy variable indicating whether a firm received a new real investment facility in a given year. $Lehman_i$ represents the total number of revolving credit facilities though syndicates that included Lehman Brothers starting before 2008 and extending into 2009 or beyond. $Spread_i^{2000-2007}$ represents the average interest rate "all-in-drawn" spread paid by firm i on loans with a start date between 2000 and 2007.

	(1)	(2)	(3)	(4)
$\mathbb{1}_{\{Year \geq 2009\}} \times Spread_i^{2000-2007}$	0.00554		0.0552***	0.00698
	(0.00855)		(0.0119)	(0.00709)
$\mathbb{1}_{\{Year \geq 2009\}} \times Spread_i^{2000-2007} \times \mathbb{1}_{\{Mfg\}}$	0.00274		-0.0495***	-0.00299
	(0.00768)		(0.0130)	(0.00825)
$\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i$	0.0127**		0.0653***	0.0114
	(0.00583)		(0.0128)	(0.00775)
$\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{Mfg\}}$	-0.0561***		-0.0381	-0.0738***
	(0.0133)		(0.0311)	(0.0116)
Controls	Y		N	Y
2016 Survivors	N		N	Y
N	34092		35768	20200

Driscoll-Kraay standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D.11: Effects of Lehman Exposure on Sales Controlling for Spreads

Note: This table shows the results of estimating Equation 2 where the dependent variable is log sales. $Lehman_i$ represents the total number of revolving credit facilities though syndicates that included Lehman Brothers starting before 2008 and extending into 2009 or beyond. $Spread_i^{2000-2007}$ represents the average interest rate "all-in-drawn" spread paid by firm i on loans with a start date between 2000 and 2007.

D.3 Aggregate Results

Section 3 of the main paper showed that manufacturing firms were disproportionately affected by Lehman's bankruptcy by using firm-level variation across time, sector, and bank exposure. This section supplements those results with additional exercises that provide support for the existence of the credit reallocation channel. First, in Section D.3.1, I show that credit reallocation from manufacturing to nonmanufacturing occurred even for firms without direct Lehman attachment and that it was driven by the extensive loan margin. Second, in Section D.3.2, I show that this reallocation occurred from slower- to faster-growing subsectors *within* both manufacturing and services.

D.3.1 Credit Reallocation Across Sectors

Even though the majority of firms in my sample did not have an open line of credit with Lehman at the time of its bankruptcy, they were still exposed to other types of widespread financial disruptions that were prevalent during the Great Recession. This turmoil in financial markets was visible in a wide range of metrics, including corporate bond spreads and growth in aggregate commercial and industrial (C&I) lending (see Figure D.5). This means that credit reallocation from manufacturing to nonmanufacturing firms should be visible as a more general phenomenon. To show this is the case, I begin by showing that all manufacturing firms were less likely to receive new loans and that this was driven by the extensive margin. I focus on all loans instead of real investment loans for this exercise to facilitate comparison with trends in aggregate data on financial markets, which do not have such detail available, although the results for real investment loans are very similar. My baseline regression specification is similar to the regressions in the previous section, but instead of measuring the effects of direct exposure to Lehman Brothers I analyze how outcomes changed post-2009 for all manufacturing firms:

$$Y_{i,t} = \alpha_i + \sigma_t + \gamma X_{i,t-1} + \beta \times \mathbb{1}_{\{Mfg\}} \times \mathbb{1}_{\{Year \geq 2009\}} + \epsilon_{i,t} \tag{3}$$

The coefficient of interest is β, which captures the differential effect on the probability of obtaining a loan for manufacturing firms relative to nonmanufacturing firms post-2009.[7] The baseline results are shown in Table D.12. The first column corresponds to my preferred specification and implies that a manufacturing firm was approximately 1.7pp less likely to receive a new loan post-2009 relative to a nonmanufacturing firm. Given that the unconditional probability of obtaining a loan in any given year in the early 2000s was approximately 10-15% across all firms,

[7] The dummies for manufacturing and post-2009 are absorbed by the firm and year fixed effects, respectively.

this represents a substantial effect. As was the case in the previous section, columns 2-4 represent alternative specifications that restrict the sample to firms which had at least one observed loan in DealScan (column 2), exclude the firm-level controls (column 3), or use only firms which showed up in Compustat throughout the entire sample (column 4). Table D.13 shows that these results are very similar if all Lehman-attached firms are excluded, suggesting that the aggregate patterns reflect broad-based credit market disruptions that extended beyond Lehman's immediate proximity, and Table D.14 shows that the results are similar if I use real investment loans instead of all loans.

The reduction in the probability of obtaining a loan had a significant effect on the total volume of credit each firm obtained. To show this, I modify the dependent variable in Equation 3 to be the log value of all facilities obtained in year t by firm i. The results are shown in Table D.15 and suggest that the reduction in loan volume for manufacturing firms relative to nonmanufacturing firms in the aftermath of the financial crisis was between 23-35%. The estimated magnitudes are approximately 2-3 times larger than the results implied by the simple loan probabilities in Table D.12, which is a result of the fact that many firms receive multiple loans per year.

Based on the loan probability results, at least some of this reduction in new loan volume comes through the extensive margin (fewer new loans). In principle, the intensive margin (a change in the size of loans issued) could also be responsible for the change in average loan volume. In practice this does not appear to be the case. Table D.16, which conditions on observations in which firms receive a loan, shows that the estimated volume of credit actually goes *up* for manufacturing firms relative to nonmanufacturing firms. The conditional effect is positive but small in the baseline specification (column 1) and the specification including only firms which survived until at least 2012 (column 4). The estimates which exclude firm controls (column 3) are close to zero and insignificant.[8] Table D.17 shows that the estimated effects on loan maturity are insignificant and quite small; the dependent variable is in levels, not logs, so the estimated effect is less than one month in all specifications.

[8] The specification in column 2, which restricts the sample to only firms which ever receive a loan, is excluded here because it is redundant when looking at only firm-year observations where firms receive a loan.

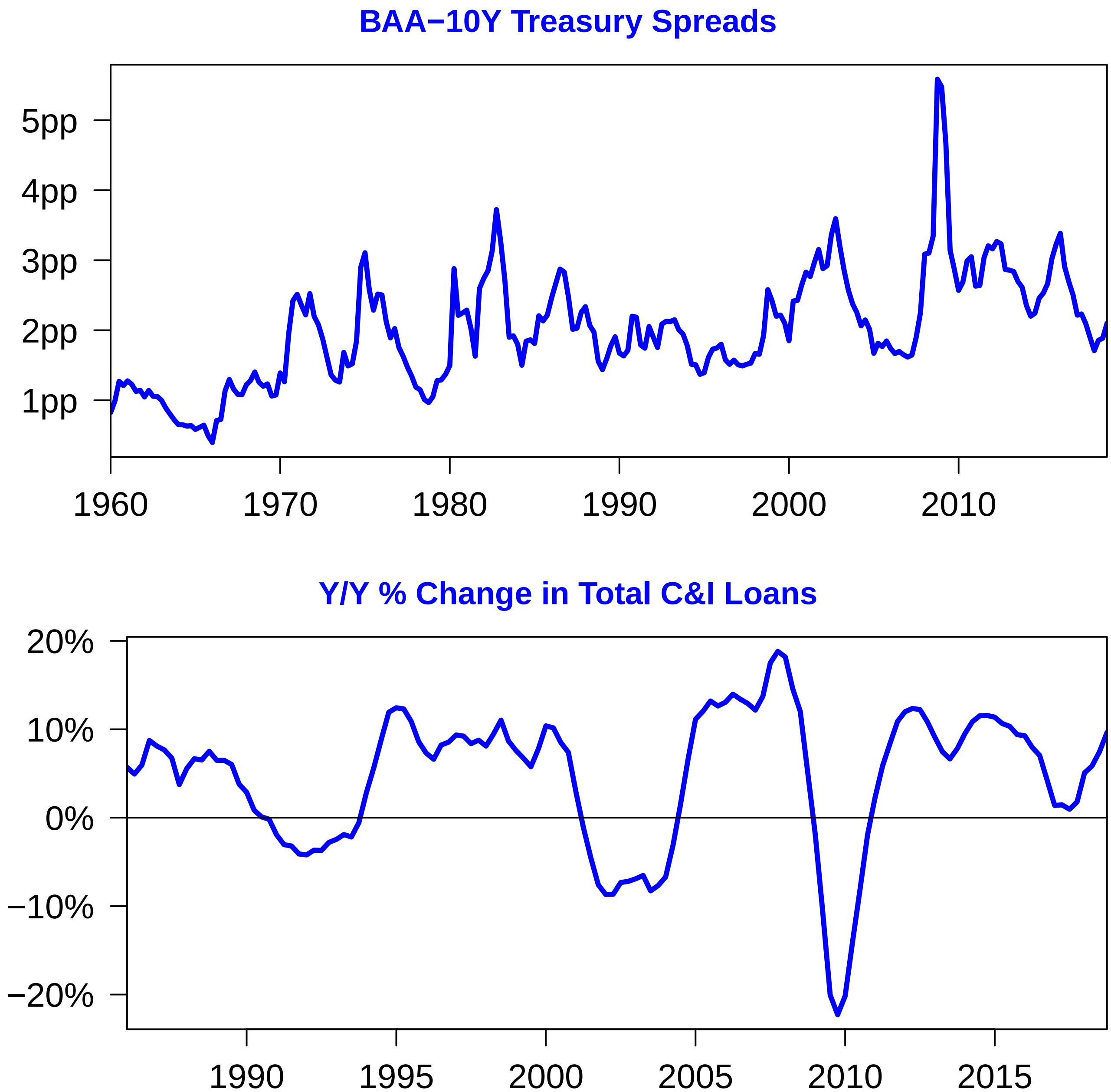


Figure D.5: Corporate Loan Spreads and C&I Loan Growth

Note: The top panel shows the spread of BAA rated bonds over 10-year US Treasury bonds. The bottom panel shows the year-over-year percentage change in the total volume of commercial and industrial loans on the balance sheets of commercial banks. Shaded areas indicate NBER-defined recessions.

	(1)	(2)	(3)	(4)
$\mathbb{1}_{\{Mfg\}} \times \mathbb{1}_{\{Year \geq 2009\}}$	-0.0165***	-0.0209**	-0.0209***	-0.0176**
	(0.00550)	(0.00837)	(0.00500)	(0.00700)
Controls	Y	Y	N	Y
Loans>0	N	Y	N	N
2016 Survivors	N	N	N	Y
N	69940	44422	84061	37486

Driscoll-Kraay standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D.12: Effects on Probability of Obtaining New Credit Facility

	(1)	(2)	(3)	(4)
$\mathbb{1}_{\{Mfg\}} \times \mathbb{1}_{\{Year \geq 2009\}}$	-0.0139***	-0.0173**	-0.0183***	-0.0150**
	(0.00519)	(0.00800)	(0.00465)	(0.00659)
Controls	Y	Y	N	Y
Loans>0	N	Y	N	N
2016 Survivors	N	N	N	Y
N	67903	42385	81776	36058

Driscoll-Kraay standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D.13: Effects Excluding Lehman-Attached Firms

Note: Tables D.12 and D.13 show the results of estimating Equation 3 where the dependent variable is a dummy variable indicating whether a firm received any new credit facility in a given year. Table D.12 includes all firms, while Table D.13 excludes observations from firms with Lehman exposure. The first column represents my baseline specification, which includes data from 2000-2016 and includes only firms which were in Compustat by the start of this period. The second column restricts the sample of firms to only those who were matched to at least one loan in DealScan, regardless of when it occurred. The third column excludes the firm-level controls. The fourth column restricts the sample to only the set of firms who were observed in Compustat in at least one year in 2016 or later.

	(1)	(2)	(3)	(4)
$\mathbb{1}_{\{Mfg\}} \times \mathbb{1}_{\{Year \geq 2009\}}$	-0.0109*	-0.0227***	-0.00892	-0.0127
	(0.00562)	(0.00755)	(0.00610)	(0.00782)
Controls	Y	Y	N	Y
Loans>0	N	Y	N	N
2016 Survivors	N	N	N	Y
N	69940	44422	84061	37486

Driscoll-Kraay standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D.14: Effects on Probability of Obtaining Real Investment Loans

Note: This table shows the results of estimating Equation 2 of the main paper where the dependent variable is a dummy variable indicating whether a firm received a real investment facility in a given year. The first column represents my baseline specification, which includes data from 2000-2016 and includes only firms which were in Compustat by the start of this period. The second column restricts the sample of firms to only those who were matched to at least one loan in DealScan, regardless of when it occurred. The third column excludes the firm-level controls. The fourth column restricts the sample to only the set of firms who were observed in Compustat in at least one year in 2016 or later.

	(1)	(2)	(3)	(4)
$\mathbb{1}_{\{Mfg\}} \times \mathbb{1}_{\{Year \geq 2009\}}$	-0.285**	-0.374**	-0.366***	-0.301**
	(0.115)	(0.176)	(0.105)	(0.151)
Controls	Y	Y	N	Y
Loans>0	N	Y	N	N
2016 Survivors	N	N	N	Y
N	69940	44422	84061	37486

Driscoll-Kraay standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D.15: Effects on Log Value of All New Loans

	(1)	(2)	(3)	(4)
$\mathbb{1}_{\{Mfg\}} \times \mathbb{1}_{\{Year \geq 2009\}}$	0.0580		-0.00325	0.0486
	(0.0559)		(0.0628)	(0.0615)
Controls	Y		N	Y
2016 Survivors	N		N	Y
N	13545		14220	8326

Driscoll-Kraay standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D.16: Effects on Log Value Conditional on Receiving a Loan

Note: Table D.15 shows the results of estimating Equation 3 where the dependent variable is the log of the total volume of new credit facilities obtained by a firm in a given year. Table D.16 shows the same results, but conditions on only observations where loans were received. The first column represents my baseline specification, which includes data from 2000-2016 and includes only firms which were in Compustat by the start of this period. The second column, which in other regressions restricts the sample of firms to only those who were matched to at least one loan in DealScan, is omitted here because conditioning on receiving a loan trivially restricts the sample to firms who had ever received a loan. The third column excludes the firm-level controls. The fourth column restricts the sample to only the set of firms who were observed in Compustat in at least one year in 2016 or later.

	(1)	(2)	(3)	(4)
$\mathbb{1}_{\{Mfg\}} \times \mathbb{1}_{\{Year \geq 2009\}}$	-0.304		0.0787	0.793
	(1.085)		(1.060)	(1.246)
Controls	Y		N	Y
2016 Survivors	N		N	Y
N	13541		14216	8325

Driscoll-Kraay standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D.17: Effects on Maturity Conditional on Receiving a Loan

Note: This table shows the results of estimating Equation 2 of the main paper where the dependent variable is the maturity of the loan (in months). The estimates include only firm-year observations in which at least one new loan was received. The first column represents my baseline specification, which includes data from 2000-2016 and includes only firms which were in Compustat by the start of this period. The second column, which in other regressions restricts the sample of firms to only those who were matched to at least one loan in DealScan, is omitted here because conditioning on receiving a loan trivially restricts the sample to firms who had ever received a loan. The third column excludes the firm-level controls. The fourth column restricts the sample to only the set of firms who were observed in Compustat in at least one year in 2016 or later.

D.3.2 Credit Reallocation Within Sectors

Next, I dig deeper to analyze heterogeneity in credit responses within sectors. This is an important check because the distinction between manufacturing and services in the context of structural change is often a proxy for "new vs. old" more generally. In a model of productivity-driven structural change, which I illustrate more formally in Section 5 of the main paper, real value added will grow faster in the sectors that are receiving inflows of productive resources. Thus to the extent that recessions are periods in which credit is reallocated to higher-value sectors, this distinction should be visible *within* both manufacturing and services.

I use average growth in real value added from the BEA from 2000-2008 as my measure of how fast each subsector was growing. Real value added from the manufacturing sector increased by just over 3% per year during this period. To give an idea of the extent of heterogeneity present across subsectors, growth averaged 17.7% annually for computer and electronics manufacturing, while real value added from apparel manufacturing declined by about 3.4% per year. Similar patterns emerge in the service sector, which grew at an average rate of 3.2% in the years leading up to the crisis. Growth was rapid in subsectors such as healthcare services (4.7% per year), professional and business services (4.1%), and information services (9.1%). Other types of services, such as retailers (1.9%), showed slower growth. If my main results are indicative of credit being reallocated from older to newer sectors, then firms in subsectors whose value added was increasing at a slower pace before the crisis should be less likely to obtain new loans post-2009 regardless of whether they are manufacturing or nonmanufacturing firms.

To test this explicitly I modify Equation 3 to include splits for several different subsectors. Motivated by the stylized facts described in the previous paragraph, I define "new" manufacturing firms to be those which produce computers and electronics, and "old" manufacturing firms refer to all others. I define "new" services to include business services (a category which includes software), healthcare services, and professional services such as engineering, accounting or management. "Old" services include all types of retail firms. To see how credit was affected for firms based on these classifications I estimate the following regression. The coefficients of interest will be the η^js, which capture the change in the annual probability of obtaining at least one new facility for a firm in group j (such as new manufacturing or old services) relative to the excluded group (in this case all other services not classified as new or old).

$$Y_{i,t} = \alpha_i + \sigma_t + \gamma X_{i,t-1} + \sum_j \eta^j \times \mathbb{1}_j \times \mathbb{1}_{\{Year \geq 2009\}} + \epsilon_{i,t} \tag{4}$$

The coefficient estimates shown in Table D.18 support the idea that credit was reallocated to higher-value sectors. Computer and electronics manufacturers are estimated to be about 4pp more likely to receive a new loan in the years following the financial crisis; other manufacturers, in contrast, were about 2pp less likely. Similarly, the estimates for old service firms are negative, generally insignificant, and noisy across specifications, while newer types of service firms were almost 3pp more likely to get at least one new loan annually.

	(1)	(2)	(3)	(4)
New manufacturing	0.0391***	0.0661***	0.0234**	0.0476***
	(0.0139)	(0.0232)	(0.0115)	(0.0169)
Other manufacturing	-0.0192***	-0.0202**	-0.0251***	-0.0260***
	(0.00658)	(0.00887)	(0.00648)	(0.00994)
Old services	-0.0139	-0.00175	-0.0173	-0.0314***
	(0.0114)	(0.00936)	(0.0131)	(0.00949)
New services	0.0259***	0.0430***	0.0189***	0.0210
	(0.00713)	(0.0116)	(0.00617)	(0.0132)
Controls	Y	Y	N	Y
Loans>0	N	Y	N	N
2016 Survivors	N	N	N	Y
N	69940	44422	84061	37486

Driscoll-Kraay standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D.18: Probability of Obtaining New Credit Facility

Note: This table shows the results of estimating Equation 4 where the dependent variable is a dummy variable indicating whether a firm received any new credit facility in a given year. "New manufacturing" includes computers and electronics (SIC code 36). "Other manufacturing" includes all other manufacturing categories. "Old services" includes all retailers (SIC codes 50-59). "New services" includes professional and business services (including software), healthcare services, and engineering/accounting/management services (SIC codes 73, 80, and 87). All estimates are relative to the excluded group of all services not classified as new or old. The first column represents my baseline specification, which includes data from 2000-2016 and includes only firms which were in Compustat by the start of this period. The second column, which in other regressions restricts the sample of firms to only those who were matched to at least one loan in DealScan, is omitted here because conditioning on receiving a loan trivially restricts the sample to firms who had ever received a loan. The third column excludes the firm-level controls. The fourth column restricts the sample to only the set of firms who were observed in Compustat in at least one year in 2016 or later.

E Robustness Checks and Additional IBD Results

This section outlines several robustness checks for my results analyzing the effects of interstate banking deregulation in Section 4 of the main paper. Section E.1 shows a pretrend exercise comparing outcomes for states which deregulated in 1985 (the most popular single year of deregulation) to those who deregulated at a later date. Section E.2 takes a more formal approach to analyzing pretrends using dynamic event study regressions to show that the manufacturing employment share did not predict deregulation, but fell significantly in response to it.

E.1 Comparing Pretrends

Figure E.1 shows the average change in the manufacturing employment share for states in two groups: those which deregulated in 1985, and those which deregulated later. I choose 1985 for this illustrative example because ten states deregulated that year, which was more than all previous years combined up to that point and the most common year of deregulation across the entire sample period. Figure E.1 shows that the manufacturing employment shares for all states were trending in a virtually identical manner prior to 1985. Following deregulation, however, the share began to fall more quickly for states which had deregulated relative to those which had not. These differences persisted through the mid-90s, at which point IBD was implemented nationwide.

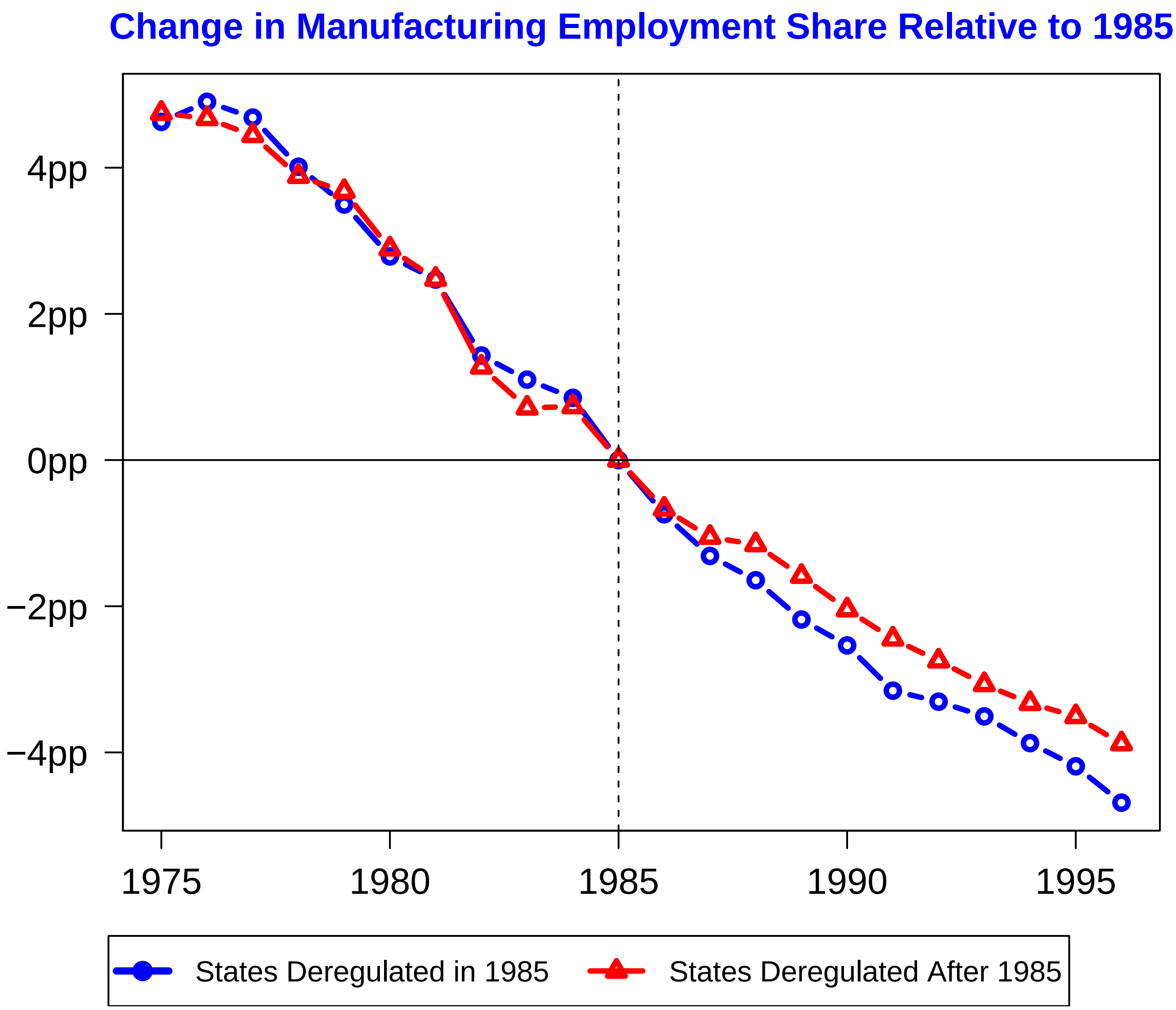


Figure E.1: Comparing Pretrends in Manufacturing Employment Share

Note: This figure compares the average change in the manufacturing employment share for the ten states that deregulated in 1985 (DC, FL, GA, ID, MD, NV, NC, OH, TN, and VA) to states that deregulated at a later date. States which deregulated prior to 1985 (AK, CT, KY, ME, MA, NY, RI, and UT) are not included. The series for each state is subtracted from its 1985 level, and simple averages are taken across states in each group.

E.2 Dynamic Estimates

This section supplements the difference-in-differences estimates in the paper by considering dynamic "event study" regressions. Rather than counting every year following the implementation of IBD in each state as being treated, the treatment variable in this specification instead takes a value of one only in the year IBD went into effect for each state. This exercise provides further evidence for the hypothesis that expansion of IBD led to persistent increases in nonmanufacturing employment without having any effect on manufacturing employment in two key ways. First, unlike the previous specification, which estimates the average effect across the entire post-IBD period, plotting the responses over a multi-year response horizon show that the effects on the manufacturing employment share are persistent rather than being driven by sharp changes immediately surrounding implementation. Second, this specification can be used to test for "pre-trends" by testing whether the implementation of IBD predicts growth in the years leading up to implementation.

I estimate the following regression and plot the coefficients β^h for $h \in \{-3, 7\}$ in Figure E.2:

$$\Delta^i_{t-1,t+h} = \alpha^i + \delta_t + \gamma^i \cdot t + \beta^h dereg^i_t + \epsilon^i_t \tag{5}$$

This figure shows no significant effect of deregulation in the years prior to implementation, with point estimates that are close to zero and statistically insignificant. The estimates become larger in magnitude and statistically significant by the fourth year following deregulation, with an estimated peak effect of about -0.4pp, and show an average effect of about -0.2pp during the estimated response horizon. This is consistent with the baseline difference-in-differences estimates from Section 4 of the main paper, which calculated the average effect across the entire post-deregulation period to be about -0.2pp.

In principle, a decline in the manufacturing employment share can be the result of either an increase in nonmanfuacturing employment or a decrease in manufacturing employment. The model has a clear prediction for how this change should occur, however: IBD, which is an expansionary credit shock, should increase nonmanufacturing employment without having any effect on manufacturing employment. As I show in Figure E.3, which estimates event study regressions for the *level* of employment in each sector, the data appear to match the model's prediction. While there is a small positive effect in the year of deregulation, the estimated effects of IBD on manufacturing employment are small and statistically insignificant throughout the rest of the response horizon. Nonmanufacturing employment, in contrast, increases steadily to a peak of close to 3% occurring four years after deregulation.

To summarize, regulatory changes allowing out-of-state banks to enter led to an acceleration in a state's manufacturing employment share that was driven entirely by an increase in nonmanufacturing employment. These results are consistent with the predictions of the credit reallocation channel because they show directly that creation of new credit disproportionately benefits newer industries.

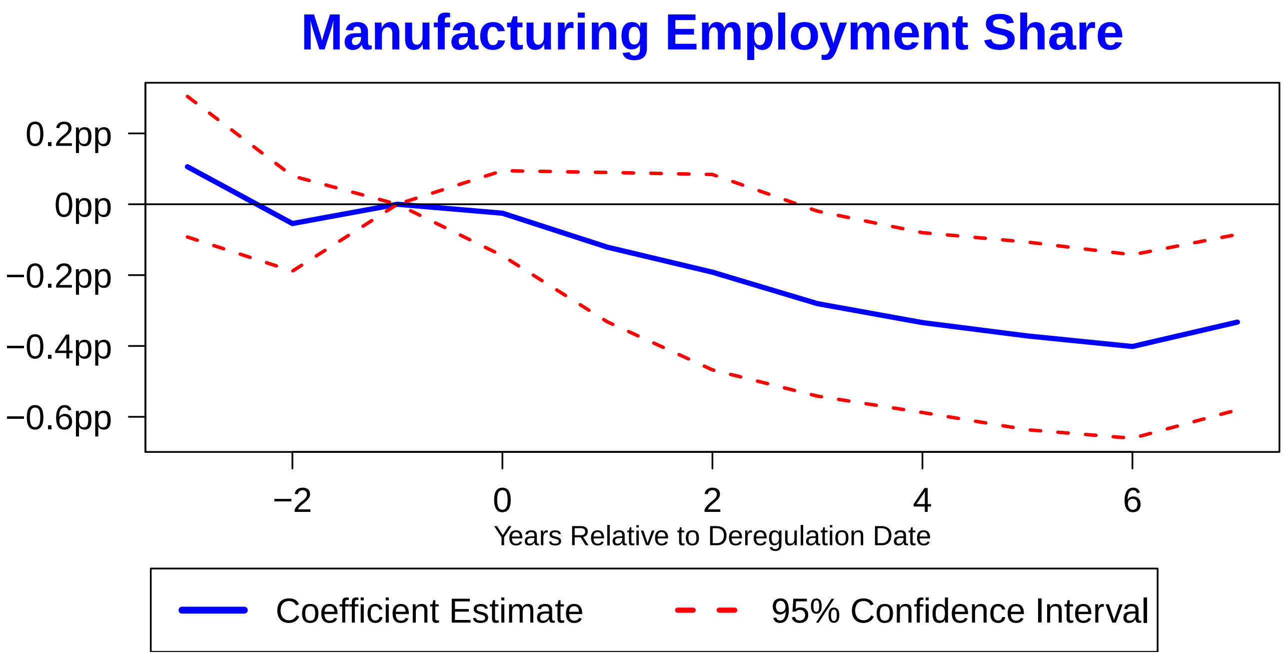


Figure E.2: Effect of IBD on Manufacturing Employment Share

Note: This figure shows the results of estimating Equation 5 for up to three years prior to deregulation and up to seven years after deregulation. The outcome variable is the difference in a state's manufacturing employment share in year h relative to the year immediately preceding deregulation. The independent variable is a dummy equal to one for the year in which deregulation went into effect and zero in all other years (including years following deregulation). Standard errors are clustered at the state level.

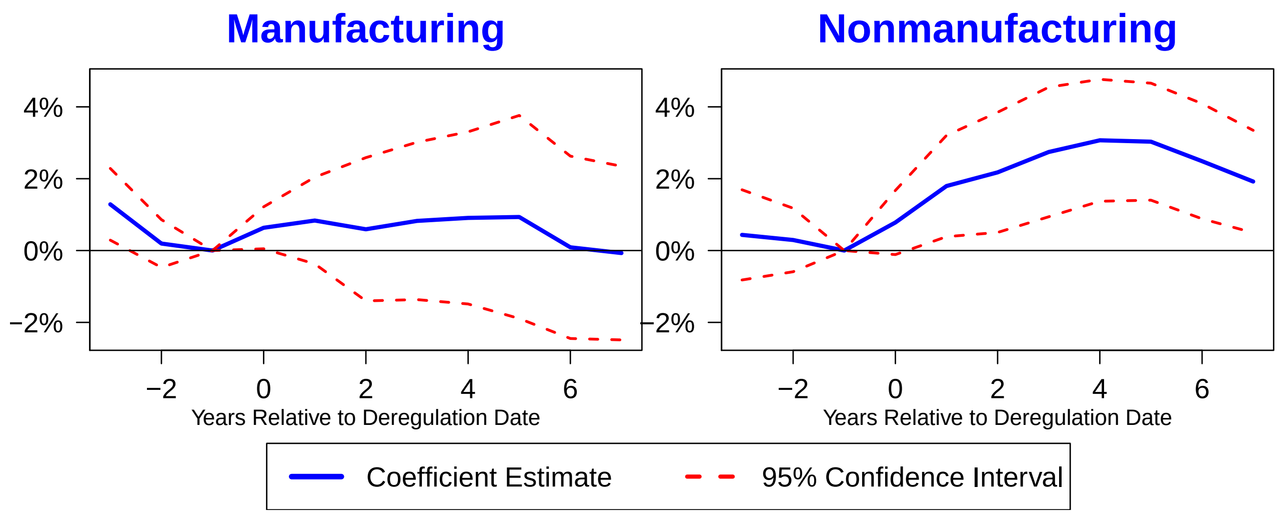


Figure E.3: Effect of IBD on Log Employment

Note: This figure shows the results of estimating Equation 5 for up to three years prior to deregulation and up to seven years after deregulation. The outcome variables in each figure are the log difference in a state's manufacturing or nonmanufacturing employment in year h relative to the year immediately preceding deregulation. The independent variable is a dummy equal to one for the year in which deregulation went into effect and zero in all other years (including years following deregulation). Standard errors are clustered at the state level.

F Additional Model Results

This section includes several additional details of the model omitted from the main paper in the interest of space. First, I show that the relative productivity of the manufacturing sector has increased over time in a manner consistent with my parameterization. This is calculated as the ratio of manufacturing productivity to total nonfarm productivity and is shown in Figure F.1 below. I use total productivity instead of nonmanufacturing productivity because the later is not available separately across the entire time period. My model assumes that the relative productivity of the manufacturing sector grew by a factor of just over 2.5, which is reasonably close to the actual value of 2.2.

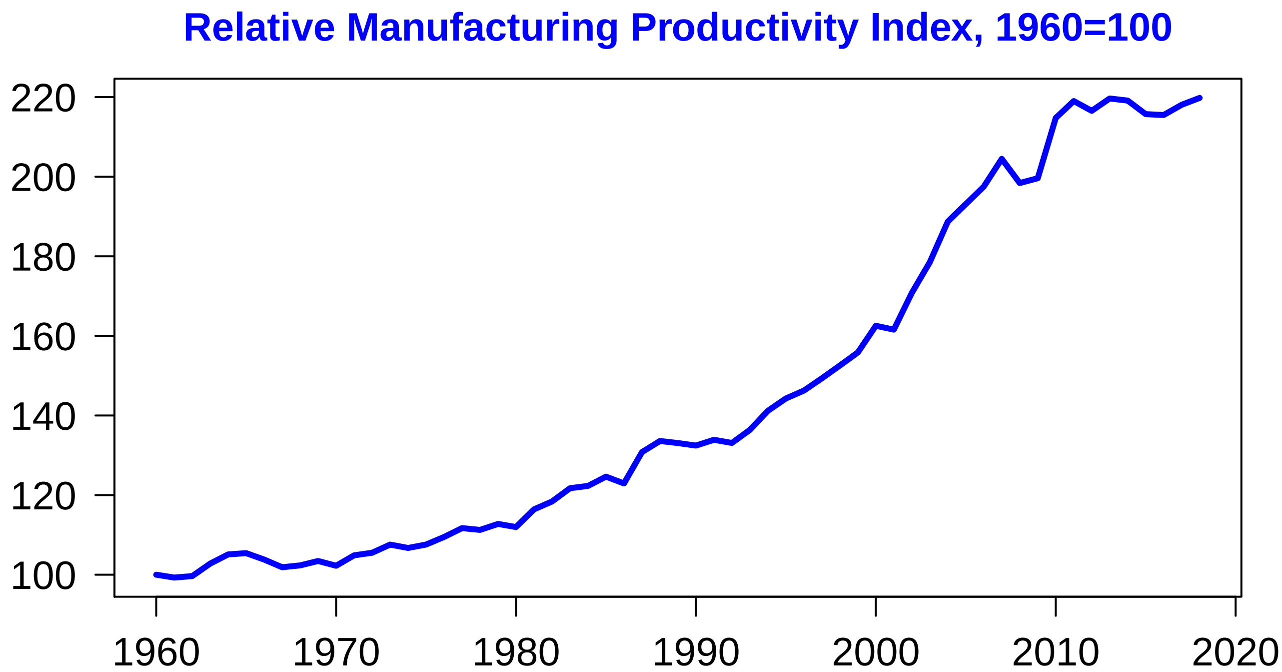


Figure F.1: Manufacturing Relative Productivity Growth

Note: This figure shows the ratio of manufacturing productivity to total nonfarm productivity for the US dating back to 1960. Data are indexed so that 1960 takes a value of 100 to show growth rates over time. Because this ratio does not have a clear interpretation on its own, I index it to take a value of 100 in 1960 to show its growth over time. Data from 1960-2011 come from the BLS International Labor Comparisons Program (ILC), which was discontinued in 2011. For later years, I calculate growth rates from BEA productivity data and apply these growth rates to the levels from the pre-2011 data.

Next, I simulate the model without recessions to give a sense of the role of fixed costs in determining the timing of structural change. The left panel of Figure F.2 shows the exogenous productivity trend for the manufacturing sector that I use in the simulation. The right panel shows the optimal credit share going to manufacturing α^* with and without adjustment frictions. The dotted orange line shows the optimal manufacturing credit share in the absence of fixed costs. This line is smooth because it adjusts continuously with growth in manufacturing productivity, which leads to a declining value of credit allocated to the manufacturing sector. In the presence of fixed costs, which are shown as the solid black line, adjustment becomes larger and less frequent. Because reallocation decisions are forward looking and the trends in manufacturing productivity are deterministic, when adjustment occurs it will overshoot the fully flexible benchmark in anticipation of remaining at that level for several periods.

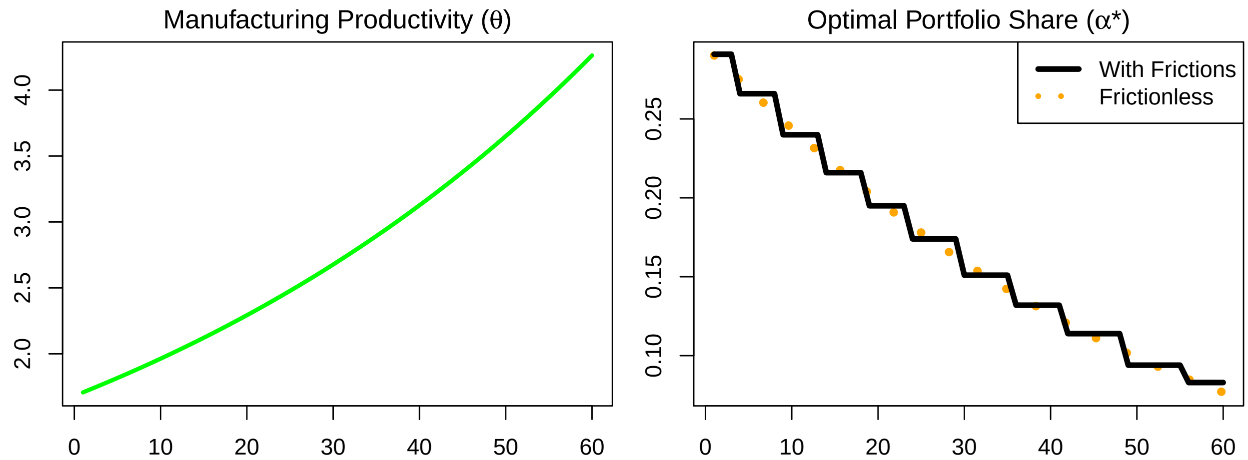


Figure F.2: Model without Recessions

Note: The left panel shows the deterministic productivity trend used in the model. The right panel shows the optimal share of credit allocated to the manufacturing sector with and without adjustment costs. The horizontal axis corresponds to time periods. The parameter values are shown in Table 5 of the main paper.

Figure F.3 illustrates how the distribution of firms receiving funding changes over time. The top row represents a hypothetical "old" economy in which credit is allocated evenly across sectors. The productivity distributions of the nonmanufacturing and manufacturing sectors are shown on the left (in red) and the right (in blue), respectively. The cutoffs z_M^* and z_N^* represent the cutoff productivity; above these thresholds, all firms in each sector will receive financing through their match with the bank. The bottom row illustrates a "new" economy in which the manufacturing sector has become more productive, which manifests as a rightward shift in the manufacturing productivity distribution. The cutoffs z_{t-1} for both types of firms correspond to the "worst" firm which received credit in the old economy and represent what the new cutoff will be if credit allocations are unchanged. The thresholds z_M^* and z_N^* correspond to the optimal choices in the frictionless setting. In the model with fixed costs, manufacturing firms in the gray area will receive credit while nonmanufacturing firms in the gray area will not. In the model without adjustment frictions, credit will instead be transferred away from the gray firms in the manufacturing sector and toward the gray firms in the nonmanufacturing sector.

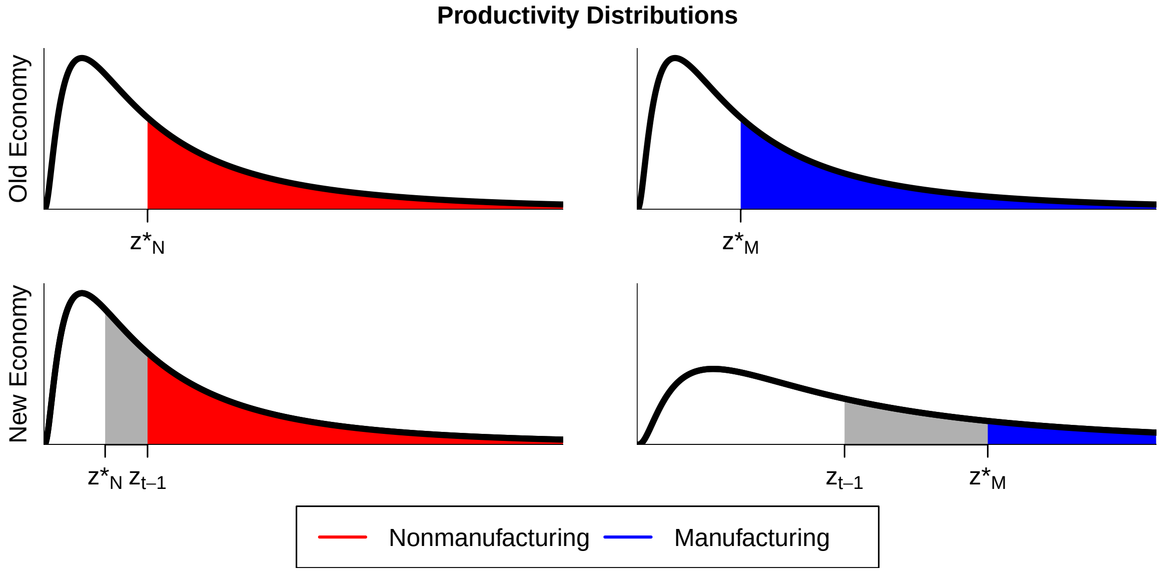


Figure F.3: Model Productivity Distributions

Note: The left panel shows an example of credit reallocation with and without fixed costs. z_{t-1} for each distribution corresponds to the cutoff firm if credit is not reallocated. z_N^* and z_M^* correspond to the optimal policies in the absence of fixed costs. The shaded gray areas represent the difference between the policies. In the model with fixed costs, the manufacturing firms in the gray area above z_{t-1} and below z_M^* will receive credit. In the version of the model without fixed costs, this credit will instead be reallocated toward the nonmanufacturing firms above z_N^* and below z_{t-1}.

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