# Internet Appendix

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

Cooper Howes*

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 details of the trend-cycle decompositions used in Section 2.2. Appendix D describes the various sources of data and their construction. Appendix E 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 F does the same for the results based on interstate banking deregulation in Section 4. Finally, Appendix G 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. These calculations

*Federal Reserve Bank of Kansas City, 1 Memorial Drive, Kansas City MO 64108; cooper.howes@kc.frb.org

are based on years that include at least one recession to allow direct comparison across activity measures since some are only available annually. The rightmost column shows the share of the total change over this period that occurred during these years. 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.

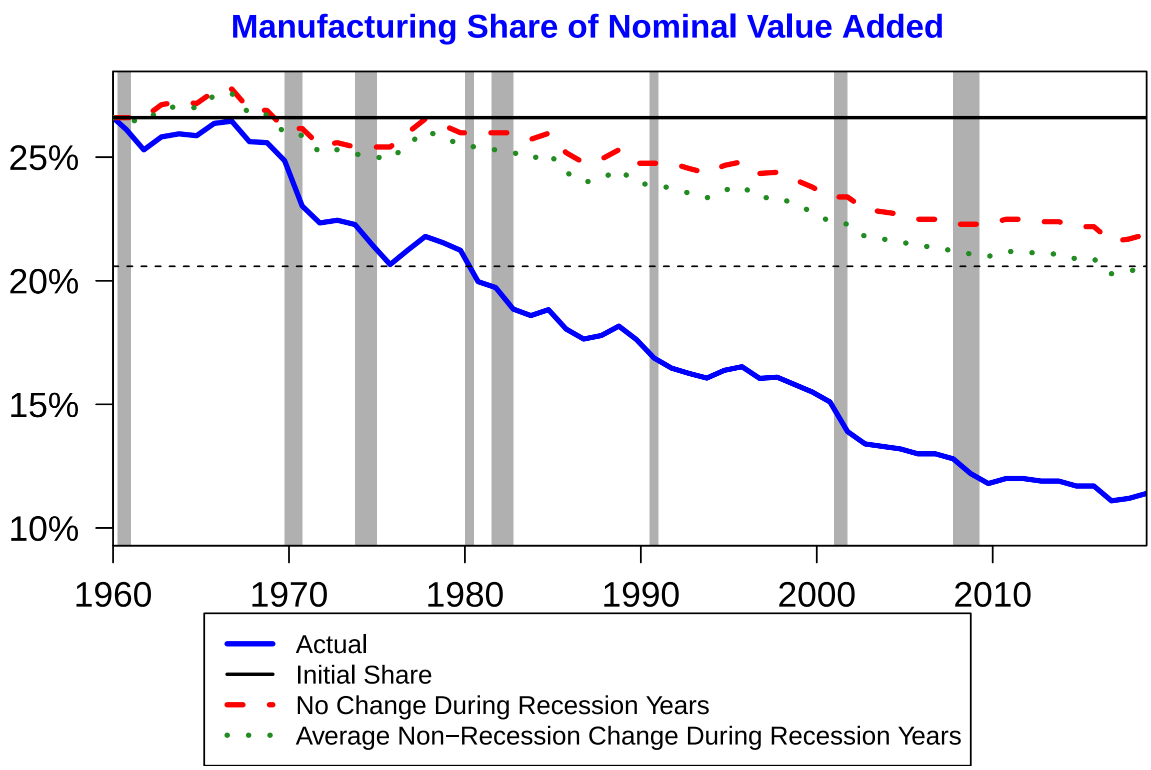



Figure A.1: Change in Manufacturing Share of US Nominal Value Added, 1960-2018

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** | $\Delta$ | **Recession** $\Delta$ | **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 "$\Delta$" 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 $\Delta$" 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 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

[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.

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.

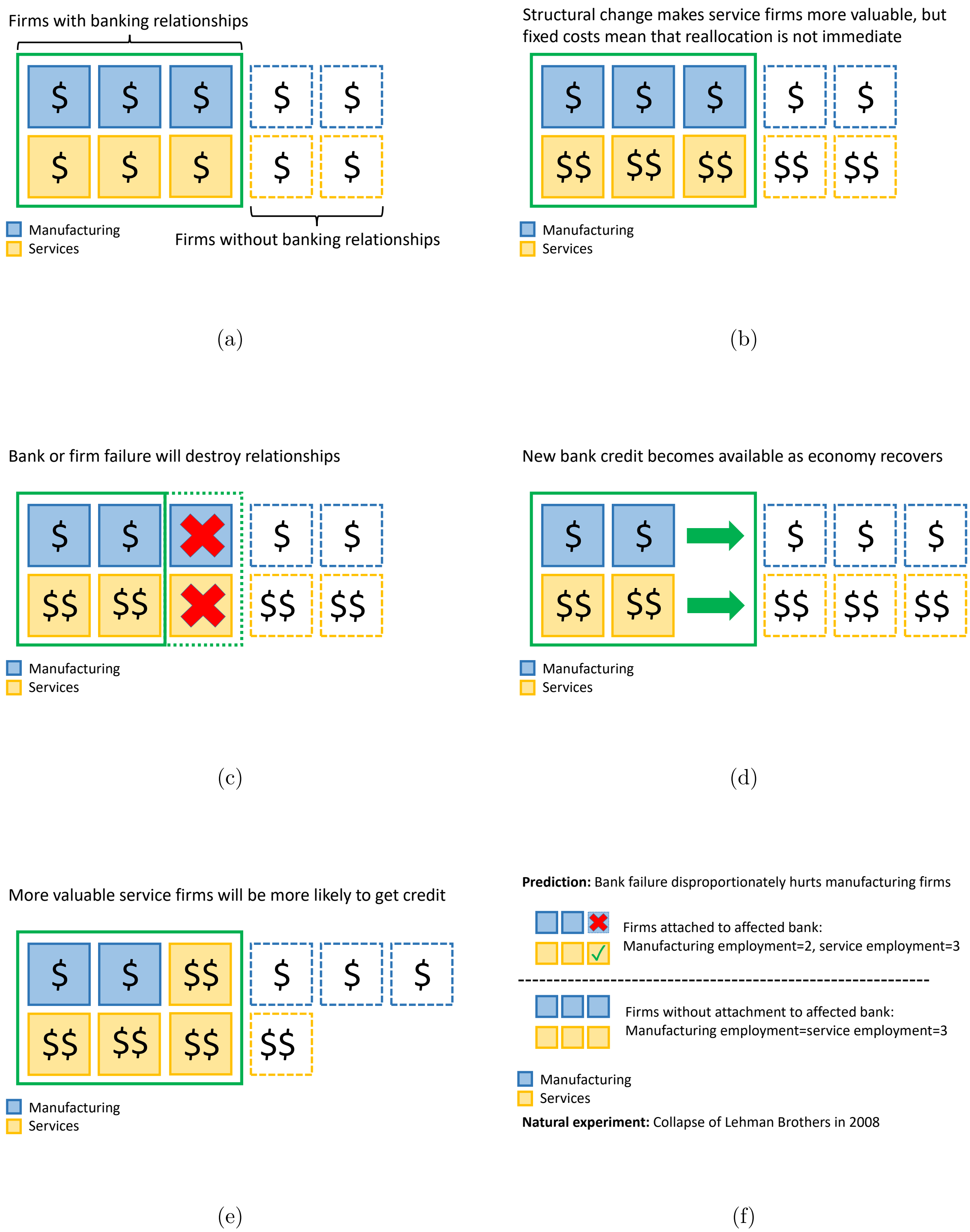
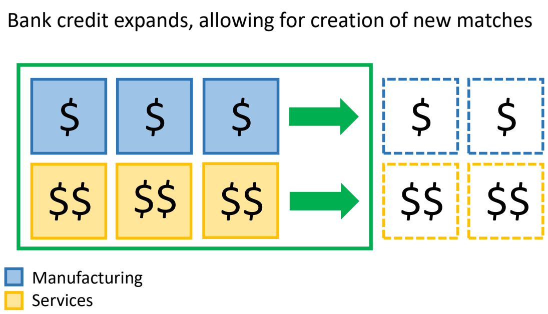
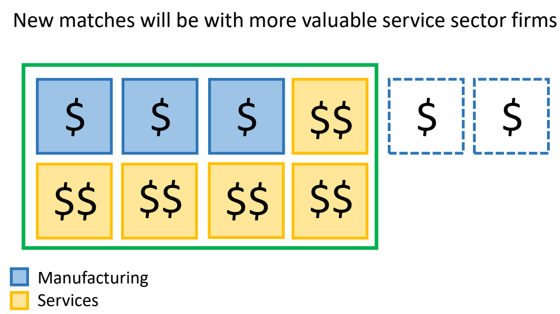


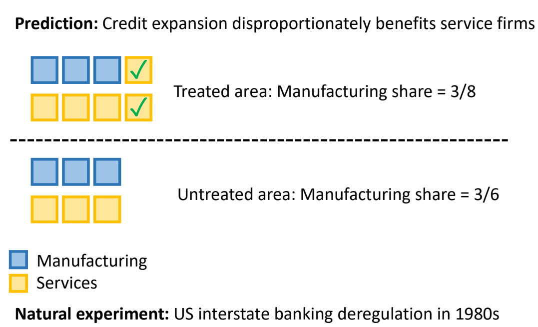

Figure B.1: Illustration of Bank Failure and Structural Change




(a)




(b)




(c)

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

# C Details of Trend-Cycle Decomposition

This section provides further detail about the approach of Chodorow-Reich and Wieland (2020) used to calculate secular changes in the manufacturing employment share during recessions in Section 2.2.

Define $\Delta BC$ to be the average quarterly change in the manufacturing employment share over the entire business cycle. This can be decomposed as the sum of secular and cyclical contributions: $\Delta BC = \Delta^S + \Delta^C$. The average cyclical component $\Delta^C$ will be equal to zero over the entire business cycle by definition, so the average change over this period will be equal to the secular component $\Delta^S$. From the first quarter of 1960 through the last quarter of 2019, $\Delta^{BC} = \Delta^S = 0.13$pp.

The object of primary interest is not $\Delta^S$, which represents changes over the entire business cycle, but instead the average quarterly change in the secular manufacturing share during recessions. This can be found by re-writing $\Delta^S$ as the average across recessions ($\Delta^S_R$) and expansions ($\Delta^S_E$) weighted by the time spent in each state ($\theta_R$): $\Delta^S = \theta_R \Delta^S_R + (1-\theta_R)\Delta^R_E$. Plugging in the observed value for $\Delta^S$ derived above, calculating $\theta_R = 0.404$ from the data, and re-arranging terms gives the following expression for $\Delta^S_R$:

$$\Delta^S_R = \frac{\Delta^S - (1-\theta_R)\Delta^S_E}{\theta_R} = \frac{0.13 - (1-0.404)\Delta^S_E}{0.404} \tag{1}$$

The magnitude of $\Delta^S_R$ will depend on the assumed properties of $\Delta^S_E$, which cannot be separately observed in the data. One approach is to assume that $\Delta^S_E$ takes the same value in recoveries that it does in expansions (during which the average decline is 0.06pp). Setting $\Delta^S_E = 0.06$ gives $\Delta^S_R = 0.23$, implying $\frac{0.23}{0.29} = 79\%$ of the decline in the manufacturing share during recessions is secular. This assumption is consistent with my model, which does not distinguish between recoveries and expansions, but does not allow for the possibility of any secular "catch-up" growth on the part of the manufacturing sector. A more conservative approach is to assume that $\Delta^S_R = \Delta^S_E = \Delta^S$, meaning the rate of secular decline is constant across both recessions and the subsequent recoveries. I consider this assumption to be more realistic because it takes into account that the process of reallocation may start during recessions but not be finalized until the recovery is underway. This approach gives $\Delta^S_R = \Delta^S = 0.13$, suggesting $\frac{0.13}{0.29} = 46\%$ of the decline in the manufacturing share during recessions is secular. To the extent that manufacturing's secular decline is more severe in recessions than recoveries, this will be a lower bound.

# D Data Description

## D.1 Example Syndicated Loan


Figure D.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 D.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.

## D.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). My sample includes loans with reported uses of either working capital or general corporate purposes. 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, or 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, or 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, or 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, or JP Morgan Securities Inc.

Finally, I define a firm as a manufacturer if it meets one of the two following criteria:

1. It has a primary or secondary two-digit SIC code between 20-39 according to DealScan
2. It does not have an industry classification in DealScan, but has a two-digit SIC code between 20-39 reported in Compustat

## D.3 Banking Deregulation Data

### D.3.1 Interstate Banking Deregulation Dates

The dates used in the main analysis in Section 4 of the main paper are shown in Table D.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 D.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.

# E Robustness Checks and Additional Lehman Results

## E.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.[2] Summary statistics for firms with attachment to one of these banks are found in Tables E.1, E.2, and E.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 E.1 shows these splits.

The average rate across all loans paid by firms with attachment to one of Lehman's competitors but not Lehman, represented by the solid red lines, were very similar to the average rates paid by firms that had both.[3] 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.

[2] 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.

[3] 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) | $9,459 | $2,393 | $8,247 | $1,416 |
| Assets ($mil) | $14,841 | $2,781 | $8,372 | $1,861 |
| Emp (thous) | 26.0 | 7.4 | 46.5 | 8.7 |
| # of firms | 94 | 3,786 | 72 | 3,812 |
| % with new loan | 68.5 | 18.3 | 66.7 | 14.4 |

Table E.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) | $6,835 | $1,818 | $5,435 | $1,038 |
| Assets ($mil) | $9,463 | $1,957 | $5,796 | $ 1,490 |
| Emp (thous) | 22.1 | 5.2 | 31.0 | 6.2 |
| # of firms | 567 | 3,313 | 438 | 3,446 |
| % with new loan | 61.6 | 12.0 | 60.6 | 9.9 |

Table E.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) | $15,148 | $2,147 | $10,745 | $1,289 |
| Assets ($mil) | $19,999 | $2,514 | $13,082 | $1,677 |
| Emp (thous) | 42.1 | 6.7 | 59.8 | 7.8 |
| # of firms | 122 | 3,758 | 103 | 3,781 |
| % with new loan | 69.6 | 17.8 | 67.3 | 14.0 |

Table E.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).

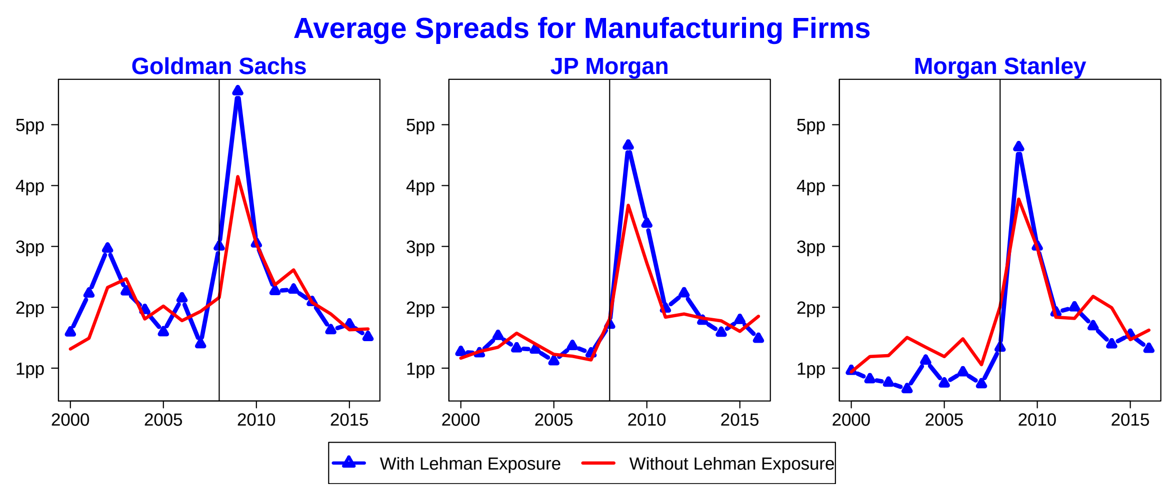



Figure E.1: Average interest rate splits by bank attachment

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 E.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 for 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 E.3. The blue line with triangles shows the same Lehman aggregate series as in Figure E.2. The other lines show sales for manufacturing firms who had lines of credit through syndicates that included at least one of Lehman's competitors but not Lehman. These results provide further evidence that Lehman attachment had a pronounced impact even relative to firms in the same industry with attachment to other, similar banks.

As a final comparison, Figure E.4 shows the same splits for Lehman's competitors as Figure E.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 that Lehman Brothers was systematically more likely to provide financing to firms that were ultimately more likely to fail.

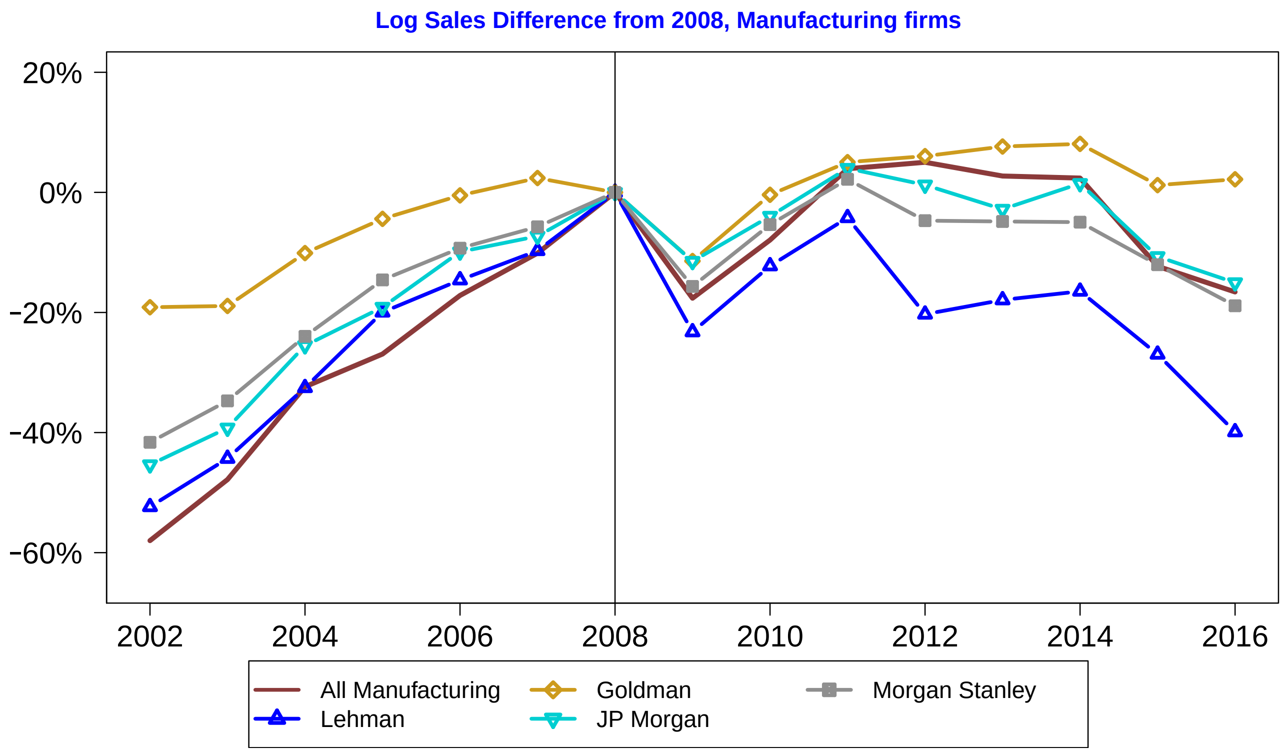



Figure E.2: Aggregate Sales Growth Relative to 2008

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. I exclude firms that entered Compustat after 2000 or exited Compustat prior to 2008. 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.

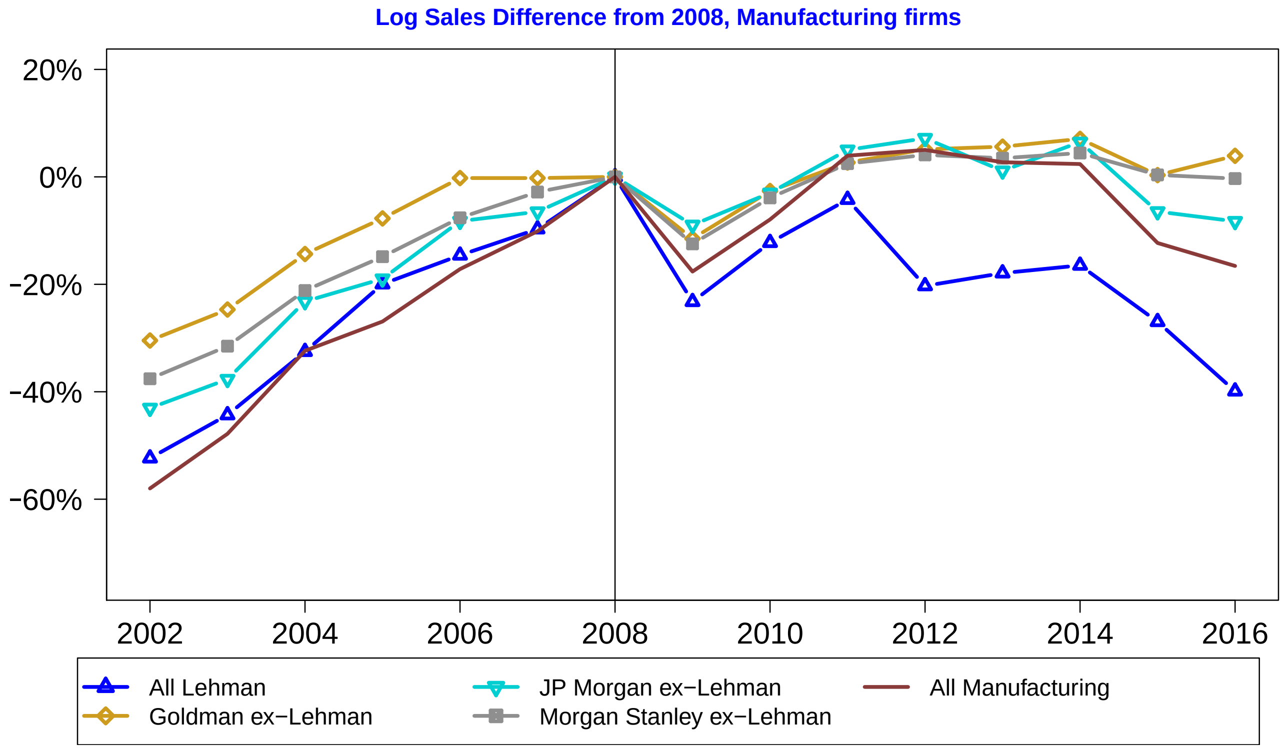



Figure E.3: Aggregate Sales and Employment Growth Relative to 2008

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. I exclude firms that entered Compustat after 2000 or exited Compustat prior to 2008. 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.

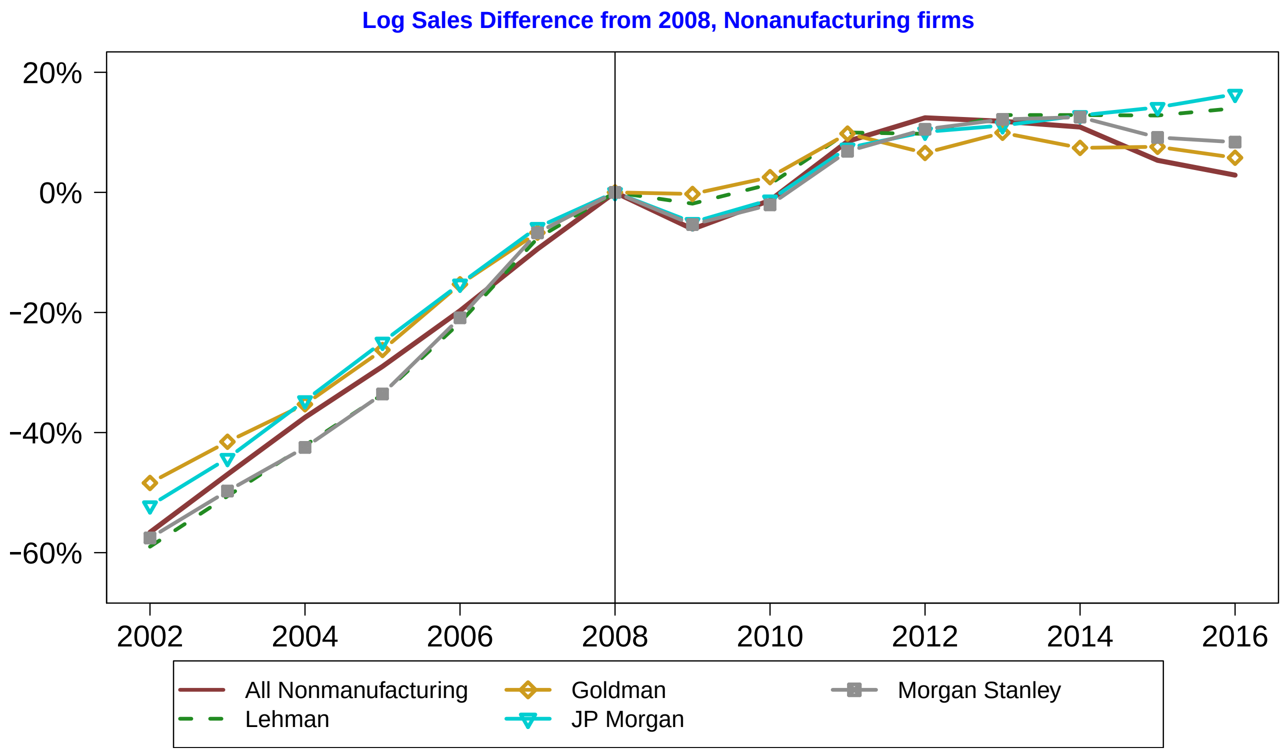



Figure E.4: Aggregate Sales and Employment Growth Relative to 2008

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. I exclude firms that entered Compustat after 2000 or exited Compustat prior to 2008. 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.

## E.2 Robustness Checks For Bank Exposure Results

### E.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 loan for either working capital or corporate purposes. Table E.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 exposure to Lehman brothers at the time of its collapse. The effect on nonmanufacturing firms, which was positive in my main specification, becomes much smaller and statistically insignificant. The effects for manufacturing firms remain negative and statistically significant, however, and the magnitudes are similar to my baseline results. This suggests that credit reallocation from manufacturing to nonmanufacturing firms was not restricted to a particular type of loan.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| $\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i$ | 1.594 | 3.004 | 2.161 | 1.513 |
| | (3.409) | (2.798) | (4.141) | (3.321) |
| $\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{Mfg\}}$ | -10.17$^{***}$ | -9.903$^{***}$ | -10.52$^{***}$ | -10.29$^{***}$ |
| | (2.123) | (2.284) | (1.749) | (2.931) |
| $N$ | 69940 | 44422 | 84061 | 37486 |

Driscoll-Kraay standard errors in parentheses

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table E.4: Probability of Receiving Any New Credit Facility (pp)

Note: This table shows the results of estimating Equation 1 in the main paper where the dependent variable is the probability (measured in percentage points) that a firm received at least one new credit facility of any type in a given year. $Lehman_i$ is a dummy variable equal to one if a firm had at least one revolving credit facility through a syndicate involving Lehman Brothers 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.

### E.2.2 Alternate Measures of Lehman Exposure

In my baseline specification, I use a dummy variable representing whether a firm had an line of credit through Lehman open prior to 2008 and scheduled to extend into at least 2009 as my measure of Lehman exposure. In this section, I show that my main results are robust to several alternative measures. The first is a continuous measure representing the number of loans involving 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; for this measure, coefficients capture the effect of a 1pp increase in this ratio. 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. These measures all show qualitatively similar results, suggesting that the differential effects of Lehman exposure for manufacturing firms are not driven by a specific measurement approach.

| | (1)<br>$\mathbb{1}^{NewLoan}$ (pp) | (2)<br>Sales (%) | (3)<br>Employment (%) |
|---|---|---|---|
| $\mathbb{1}_{\{Year\geq 2009\}} \times Lehman_i$ | 8.513*** | 0.211 | 1.310 |
| | (2.686) | (0.732) | (1.197) |
| $\mathbb{1}_{\{Year\geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{Mfg\}}$ | -5.449** | -5.427*** | -5.763*** |
| | (2.169) | (1.401) | (1.262) |
| $N$ | 69940 | 69108 | 68555 |

Driscoll-Kraay standard errors in parentheses

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table E.5: Effects of Lehman Attachment Measured by Total Number of Facilities

Note: This table shows the results of estimating Equation 1 in the main paper for new loans, sales, and employment where $Lehman_i$ measures the total number of revolving credit facilities a firm had that were exposed to Lehman Brothers. The effects on probability of getting a new loan are measured in percentage points, while the effects on sales and employment are measured in percentages.

| | (1) $\mathbb{1}^{NewLoan}$ (pp) | (2) Sales (%) | (3) Employment (%) |
|---|---|---|---|
| $\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i$ | 18.23*** | -0.0639 | 1.593 |
| | (4.200) | (1.694) | (2.138) |
| $\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{Mfg\}}$ | -5.078** | -4.396** | -7.395*** |
| | (2.377) | (1.932) | (1.542) |
| $N$ | 69940 | 69108 | 68555 |

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

Table E.6: Effects of Lehman Attachment Measured by Lehman Agent Status

| | (1) $\mathbb{1}^{NewLoan}$ (pp) | (2) Sales (%) | (3) Employment (%) |
|---|---|---|---|
| $\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i$ | 0.0611 | 0.0313 | 0.0244 |
| | (0.0628) | (0.0235) | (0.0324) |
| $\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{Mfg\}}$ | 0.0161 | -0.0829** | -0.0371** |
| | (0.0493) | (0.0379) | (0.0171) |
| $N$ | 60201 | 59637 | 59182 |

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

Table E.7: Effects of Lehman Attachment Measured by Sales Ratio

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 E.6, and measured as the ratio of the total value of all revolving credit facilities involving Lehman that started prior to 2008 and extended into 2009 or beyond divided by the firm's average sales from 2006 through 2008 (in percent) in Table E.7. Coefficients in Table E.7 correspond to the effect of a 1pp increase in this ratio. The effects on probability of getting a new loan are measured in percentage points, while the effects on sales and employment are measured in percentages.

### E.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 measure of a firm's exposure to a bank with a dummy variable equal to one if a firm had at least one revolving line of credit through a syndicated including 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 $i \in \{Lehman,\ Goldman,\ MS,\ JPM\}$:

$$\begin{aligned} Y_{i,t} = \alpha_i + \sigma_t + \mathbb{1}_{\{Mfg\}} \times \theta_t + \gamma X_{i,t-1} + \\ \sum_i \Big(\rho_i \times \mathbb{1}_{\{Year \geq 2009\}} \times Bank_i\Big) + \sum_i \Big(\Omega_i \times \mathbb{1}_{\{Year \geq 2009\}} \times Bank_i \times \mathbb{1}_{\{Mfg\}}\Big) + \epsilon_{i,t} \end{aligned} \tag{2}$$

In the case of the loan for Ford shown in Figure D.1, for example, all four banks were involved in the syndicate. Table E.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 |
|---|---|---|---|---|
| $\mathbb{1}_{\{Year \geq 2009\}} \times Bank_i$ | 0.871 | -2.080 | -2.791 | 3.020** |
| | (0.818) | (1.665) | (1.956) | (1.279) |
| $\mathbb{1}_{\{Year \geq 2009\}} \times Bank_i \times \mathbb{1}_{\{Mfg\}}$ | 1.753 | -0.106 | -2.791 | -4.698** |
| | (1.581) | (1.681) | (1.956) | (1.805) |

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

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

Table E.8: Effects on Sales Including Exposure to Lehman's Peers (%)

Note: This table shows the results of estimating Equation 2 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. $Bank_i$ is a dummy variable equal to one if a firm had at least one revolving line of credit though syndicates that included each bank starting before 2008 and extending into 2009 or beyond.

### E.2.4 Comparisons to Pre-Crisis Lehman Loans

Table E.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) |
|---|---|---|---|---|
| $\mathbb{1}_{\{Year \geq 2009\}} \times LehmanPreCrisis_i$ | 0.421 | 0.0684 | 0.593 | 0.342 |
| | (0.610) | (0.557) | (0.669) | (0.627) |
| $\mathbb{1}_{\{Year \geq 2009\}} \times LehmanPreCrisis_i \times \mathbb{1}_{\{Mfg\}}$ | 0.527 | 0.637 | 0.250 | 0.542 |
| | (0.459) | (0.428) | (0.402) | (0.409) |
| 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 E.9: Effects of Pre-Crisis Lehman Exposure on Probability of Obtaining New Loan (pp)

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 in a given year in percentage points. $LehmanPreCrisis_i$ 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.

### E.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:

$$\begin{aligned} Y_{i,t} = {} & \alpha_i + \sigma_t + \mathbb{1}_{\{Mfg\}} \times \chi_t + \gamma X_{i,t-1} + \\ & \rho \times \mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i + \\ & \Omega \times \mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{Mfg\}} + \\ & \xi \times \mathbb{1}_{\{Year \geq 2009\}} \times Spread_i^{2000-2007} + \\ & \lambda \times \mathbb{1}_{\{Year \geq 2009\}} \times Spread_i^{2000-2007} \times \mathbb{1}_{\{Mfg\}} + \epsilon_{i,t} \end{aligned} \tag{3}$$

As shown in Table E.10, Lehman attachment remains negative and significant for all of the outcomes of interest even after controlling for these measures, suggesting that my results cannot be explained purely by Lehman simply lending to riskier firms.

| | $\mathbb{1}^{NewLoan}$ (pp) | Sales (%) | Employment (%) |
|---|---|---|---|
| $\mathbb{1}_{\{Year \geq 2009\}} \times Spread_i^{2000-2007}$ | -2.168*** | 0.868 | 0.544 |
| | (0.546) | (0.828) | (0.565) |
| $\mathbb{1}_{\{Year \geq 2009\}} \times Spread_i^{2000-2007} \times \mathbb{1}_{\{Mfg\}}$ | -0.211 | 0.282 | -0.0190 |
| | (1.238) | (0.813) | (0.377) |
| $\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i$ | 13.29*** | 2.681*** | 2.625 |
| | (2.998) | (0.900) | (1.717) |
| $\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{Mfg\}}$ | -7.924*** | -5.391*** | -7.718*** |
| | (2.215) | (1.675) | (2.065) |
| $N$ | 34197 | 34073 | 33882 |

Driscoll-Kraay standard errors in parentheses

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

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

Note: This table shows the results of estimating Equation 3 where the dependent variable is a dummy variable indicating whether a firm received a new loan 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.

### E.2.6 Time to Next Loan

In my main analysis, I analyze how exposure to Lehman Brothers affected the probability that a firm would obtain a new loan each year. This section builds on that analysis by instead considering the effects of Lehman exposure on the time it took a firm to receive a new loan. Defining the number of years until firm $i$ receives its next loan as $TTL_{i,t}$, I estimate the following equation:

$$\begin{aligned} TTL_{i,t} = \alpha_i + \sigma_t + \mathbb{1}_{\{Mfg\}} \times \chi_t + \gamma X_{i,t-1} + \xi Z_{i,t-1} \\ \rho \times \mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i + \\ \Omega \times \mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{Mfg\}} + \epsilon_{i,t} \end{aligned} \tag{4}$$

This is the same as Equation 1 in the main paper but with one additional control $Z_{i,t-1}$ is a variable that captures the number of years (as of $t-1$) since a firm last received a loan. Controlling for this variable is crucial because the majority of loans in the data have a maturity of greater than a year; by not taking it into account, the results could be picking up variation in the timing of past loans rather than a firm's ability to obtain a new loan.

I estimate that Lehman exposure extended the time it took for manufacturing firms to find a new loan relative to nonmanufacturing firms by 0.71 years, or about eight and a half months. This number is both statistically and economically significant and represents about 14.5% of the average loan maturity over my sample (Table 2 of the main paper).

| | Time to Next Loan (years) |
|---|---|
| $\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i$ | 0.0263 |
| | (0.147) |
| $\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{Mfg\}}$ | $0.713^{***}$ |
| | (0.168) |

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

Table E.11: Effects of Lehman Exposure on Time Until Next Loan

Note: This table shows the results of estimating Equation 4. The dependent variable is the number of years until a firm receives a new loan with a reported purpose of either "working capital" or "corporate purposes". $Lehman_i$ is a dummy variable capturing whether a firm had at least one revolving credit facility through a syndicate involving Lehman Brothers that was open prior to 2008 and scheduled to extend into 2009 or beyond.

#### E.2.7 Robustness to Outliers

My main results are estimated from the firms in my sample that had exposure to Lehman Brothers when it collapsed. To show that my findings are robust to outliers among this group, this section estimates my main results for new loans, sales, and employment after dropping the three, five, or ten largest firms in terms of sales or assets from each sector. To minimize the impact of missing observations, I calculate firm ranks for sales and assets using the average of all reported values from 2002 through 2006.

| | (1) $\mathbb{1}^{NewLoan}$ (pp) | (2) Sales (%) | (3) Employment (%) |
|---|---|---|---|
| **Three sales outliers** | | | |
| $\mathbb{1}_{\{Year\geq 2009\}} \times Lehman_i$ | 15.64*** | 1.905 | 1.918 |
| | (3.669) | (1.340) | (2.103) |
| $\mathbb{1}_{\{Year\geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{Mfg\}}$ | -9.198*** | -5.761*** | -6.361*** |
| | (2.616) | (1.909) | (1.275) |
| **Five sales outliers** | | | |
| $\mathbb{1}_{\{Year\geq 2009\}} \times Lehman_i$ | 14.56*** | 1.992 | 3.243 |
| | (3.451) | (1.338) | (2.104) |
| $\mathbb{1}_{\{Year\geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{Mfg\}}$ | -8.159*** | -6.101*** | -6.761*** |
| | (2.791) | (1.773) | (1.001) |
| **Ten sales outliers** | | | |
| $\mathbb{1}_{\{Year\geq 2009\}} \times Lehman_i$ | 12.89*** | 2.108 | 4.266* |
| | (2.852) | (1.408) | (2.133) |
| $\mathbb{1}_{\{Year\geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{Mfg\}}$ | -7.633** | -5.020** | -5.715*** |
| | (3.028) | (2.013) | (0.797) |

Driscoll-Kraay standard errors in parentheses

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

Table E.12: Estimates after Excluding Sales Outliers

Note: This table shows the results of estimating Equation 1 of the main paper after dropping the largest three, five, or ten outliers from each sector in terms of sales among firms with Lehman exposure. $\mathbb{1}^{NewLoan}$ is a dummy variable for whether a firm received at least one new loan with a reported purpose of "working capital" or "corporate purposes" in a given year. Firm ranks within each sector are calculated based on the average across observations from 2002 through 2006.

| | (1) $\mathbb{1}^{NewLoan}$ (pp) | (2) Sales (%) | (3) Employment (%) |
|---|---|---|---|
| **Three asset outliers** | | | |
| $\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i$ | 15.11*** | 1.465 | 3.219 |
| | (3.664) | (1.494) | (2.067) |
| $\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{Mfg\}}$ | -9.194*** | -6.175*** | -7.626*** |
| | (2.573) | (2.033) | (1.407) |
| **Five asset outliers** | | | |
| $\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i$ | 14.55*** | 1.258 | 3.107 |
| | (3.593) | (1.457) | (2.108) |
| $\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{Mfg\}}$ | -8.888*** | -4.977** | -7.048*** |
| | (2.632) | (2.069) | (1.124) |
| **Ten asset outliers** | | | |
| $\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i$ | 13.83*** | 0.643 | 4.628** |
| | (3.240) | (1.272) | (2.035) |
| $\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{Mfg\}}$ | -8.978*** | -3.017* | -8.188*** |
| | (2.499) | (1.731) | (1.413) |

Driscoll-Kraay standard errors in parentheses

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

Table E.13: Estimates after Excluding Asset Outliers

Note: This table shows the results of estimating Equation 1 of the main paper after dropping the largest three, five, or ten outliers from each sector in terms of assets among firms with Lehman exposure. $\mathbb{1}^{NewLoan}$ is a dummy variable for whether a firm received at least one new loan with a reported purpose of "working capital" or "corporate purposes" in a given year. Firm ranks within each sector are calculated based on the average across observations from 2002 through 2006.

## E.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.

### E.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 E.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. 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:

$$Y_{i,t} = \alpha_i + \sigma_t + \gamma X_{i,t-1} + \beta \times \mathbb{1}_{\{Mfg\}} \times \mathbb{1}_{\{Year \geq 2009\}} + \epsilon_{i,t} \tag{5}$$

The coefficient of interest is $\beta$, which captures the differential effect on the probability of obtaining a loan for manufacturing firms relative to nonmanufacturing firms post-2009.[4] The baseline results are shown in Table E.14. The first column corresponds to my preferred specification and implies that a manufacturing firm was approximately 1.1pp 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, this represents a substantial effect. 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).

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 5 to be

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

the log value of all facilities obtained in year $t$ by firm $i$. The results are shown in Table E.15 and suggest that the reduction in loan volume for manufacturing firms relative to nonmanufacturing firms in the aftermath of the financial crisis was around 20%. The estimated magnitudes are larger than the results implied by the simple loan probabilities in Table E.14, which is a result of the fact that some 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 E.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. Table E.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.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| $\mathbb{1}_{\{Mfg\}} \times \mathbb{1}_{\{Year \geq 2009\}}$ | -1.072$^{*}$ | -2.329$^{***}$ | -0.945$^{*}$ | -1.175 |
| | (0.552) | (0.755) | (0.572) | (0.815) |
| 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 E.14: Effects on Probability of Obtaining New Loans (pp)

Note: Table E.14 shows the results of estimating Equation 5 where the dependent variable is a dummy variable indicating whether a firm received a new credit facility with a reported purpose of "working capital" or "corporate purposes" in a given year in percentage points. 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) |
|---|---|---|---|---|
| $\mathbb{1}_{\{Mfg\}} \times \mathbb{1}_{\{Year \geq 2009\}}$ | -17.13 | -20.29 | -29.44$^{**}$ | -14.36 |
| | (12.94) | (19.89) | (12.21) | (13.82) |
| 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 E.15: Effects on Total Value of All New Loans (%)

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| $\mathbb{1}_{\{Mfg\}} \times \mathbb{1}_{\{Year \geq 2009\}}$ | 10.24$^{**}$ | | 5.521 | 11.69$^{***}$ |
| | (3.726) | | (4.753) | (3.514) |
| 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 E.16: Effects on Total Loan Value Conditional on Receiving a Loan (%)

Note: Table E.15 shows the results of estimating Equation 5 where the dependent variable is the log of the total volume of new credit facilities obtained by a firm in a given year. Table E.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.

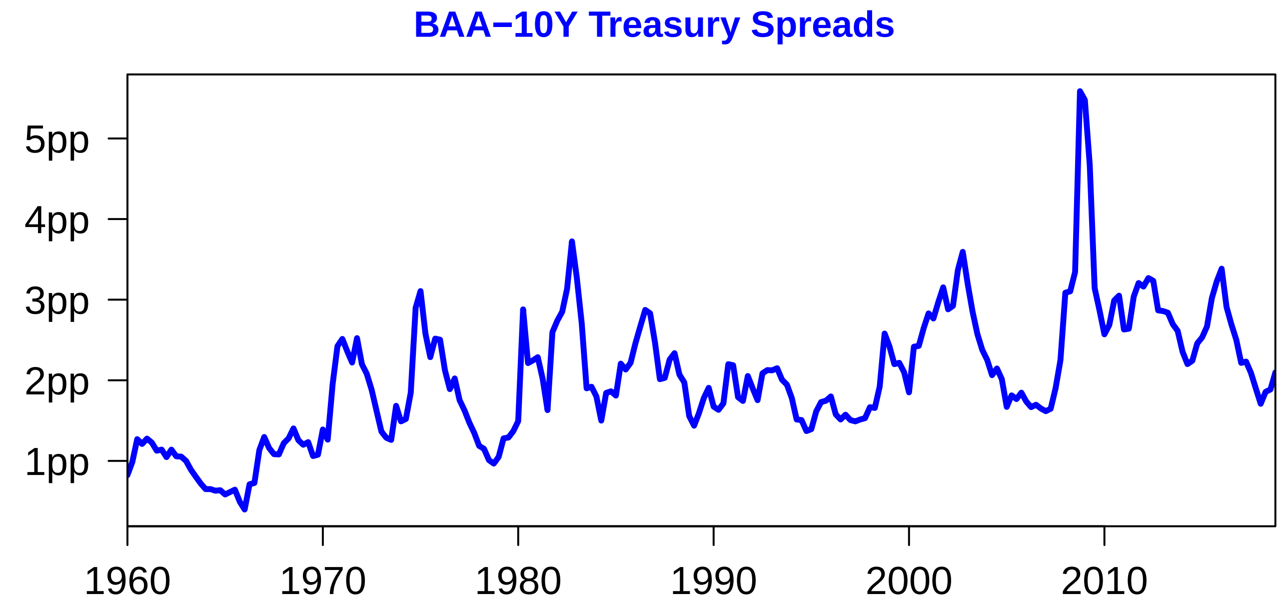
| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| $\mathbb{1}_{\{Mfg\}} \times \mathbb{1}_{\{Year \geq 2009\}}$ | -0.275 | | 0.0277 | 0.693 |
| | (0.705) | | (0.635) | (0.800) |
| Controls | Y | | N | Y |
| 2016 Survivors | N | | N | Y |
| $N$ | 13541 | | 14216 | 8325 |

Driscoll-Kraay standard errors in parentheses

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table E.17: Effects on Maturity (in Months) Conditional on Receiving a Loan (pp)

Note: This table shows the results of estimating Equation 5 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.



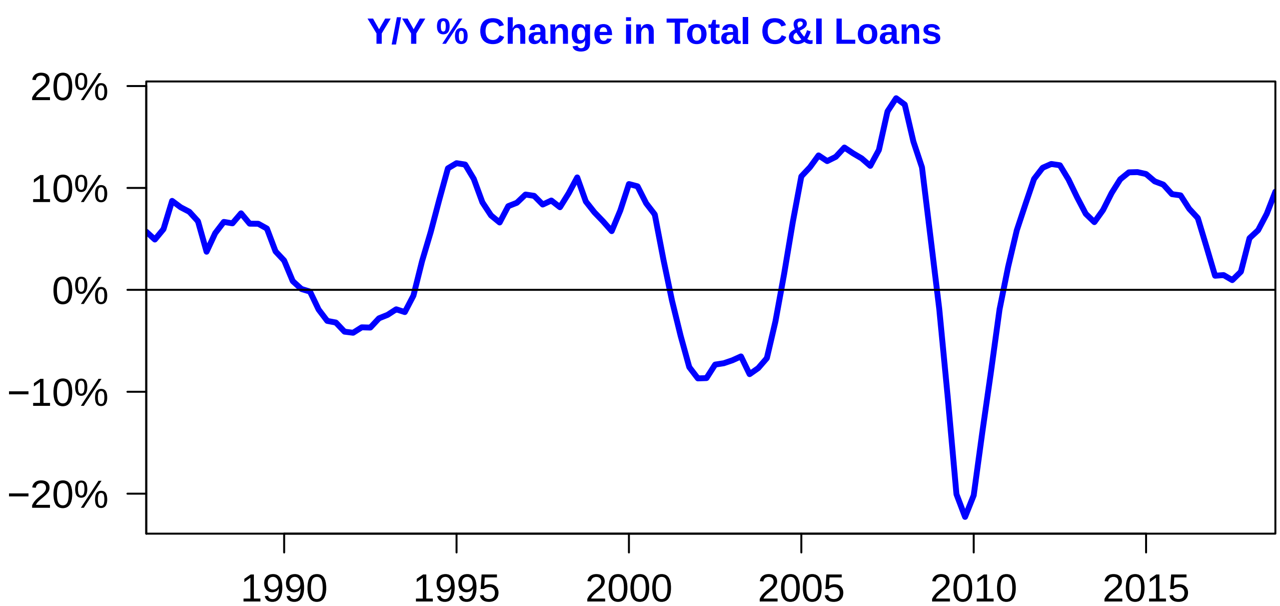




Figure E.5: Corporate Loan Spreads and C&I Loan Growth

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.

# F 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 F.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 F.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.

## F.1 Comparing Pretrends

Figure F.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 F.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.

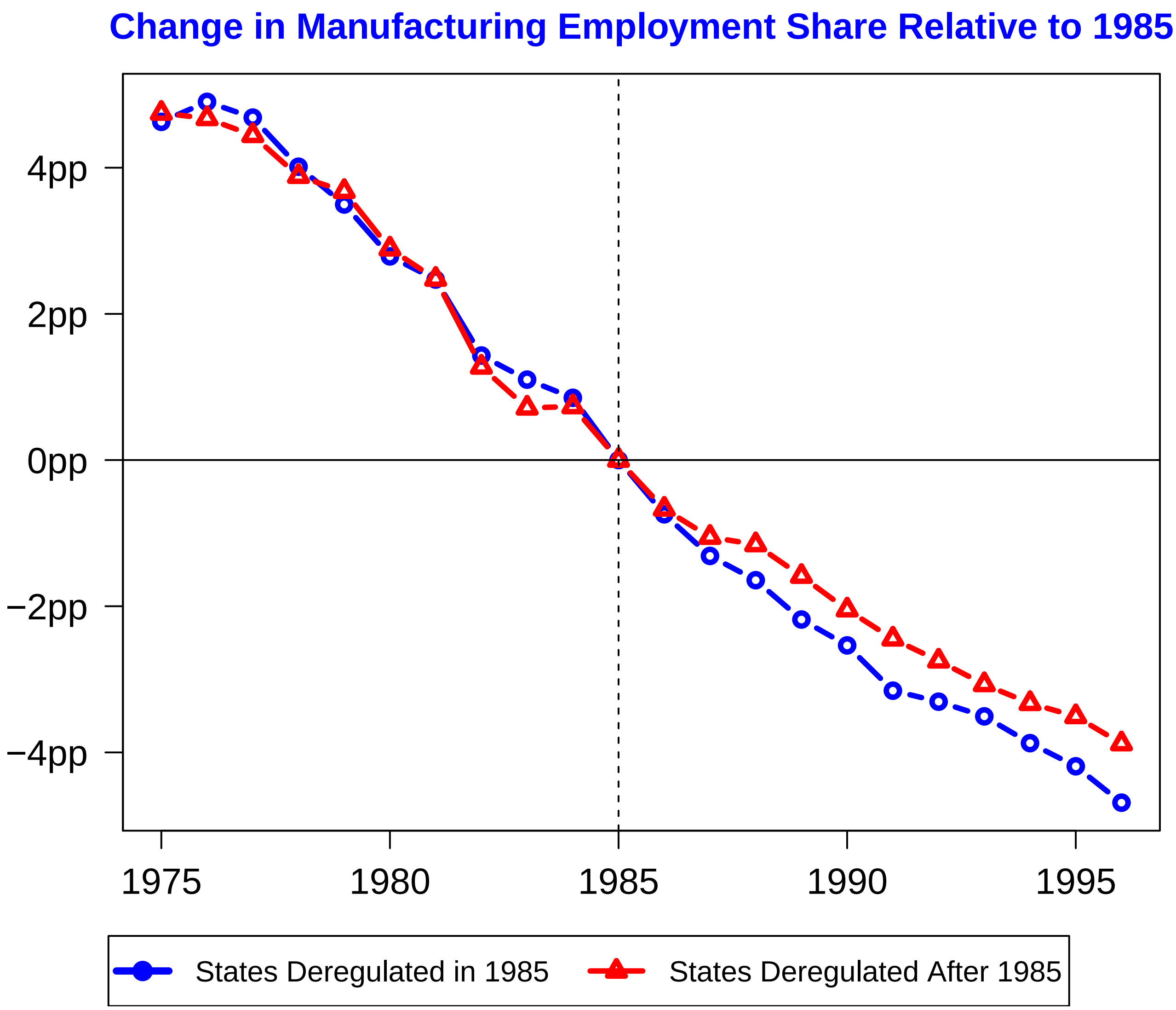



Figure F.1: Comparing Pretrends in Manufacturing Employment Share

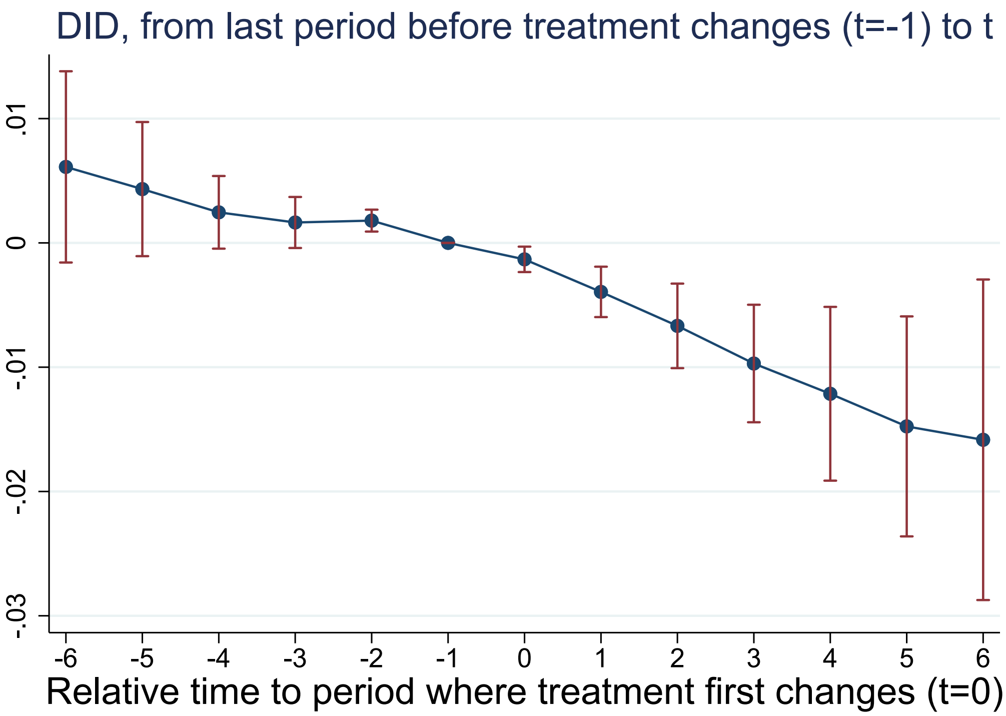
Note: This figure compares the average change in the manufacturing employment share for the ten states that deregulated in 1985 (DC, FL, GA, ID, MD, NV, NC, OH, TN, and VA) to states that deregulated at a later date. States which deregulated prior to 1985 (AK, CT, KY, ME, MA, NY, RI, and UT) are not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

## F.2 Dynamic Estimates

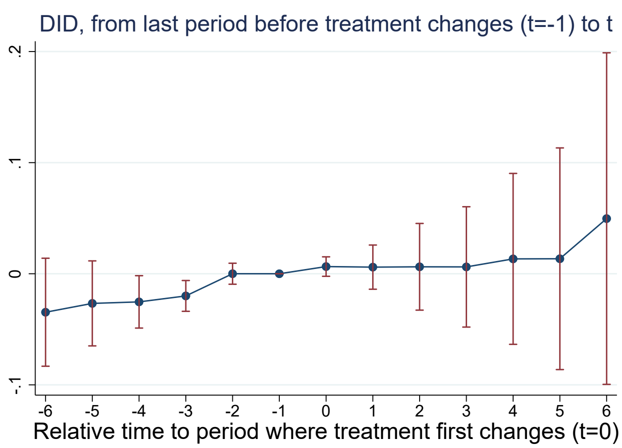
This section supplements the difference-in-differences estimates in the paper by considering dynamic "event study" regressions that directly test pretrend assumptions and provide insight into the timing of the treatment effects. This approach estimates the cumulative change $t$ years away from treatment relative to the year before treatment ($t = -1$). The value for $t = -3$, for example, corresponds to the estimated effect of treatment on the cumulative change in the outcome variable between 3 and 1 years prior to treatment, while the coefficient at $t = 4$ represents the cumulative treatment effect four years after treatment. These estimates are calculated using the *did_multiplegt* Stata package, which implements the approach of De Chaisemartin and d'Haultfoeuille (2020).

The results are shown in Figure F.2. The top panel shows the effects on the manufacturing employment share measured in percentage points. The coefficients are small and statistically insignificant throughout almost the entire pre-treatment period, but decline steadily following treatment and remain statistically significant up to six years later. The bottom panels show the effects on the log levels of manufacturing and nonmanufacturing employment. The estimates for manufacturing employment are statistically insignificant throughout most of the pre-treatment period and the entire post-treatment period. For nonmanufacturing employment, there is a statistically significant for the first few years after treatment, after which point the coefficient estimates remain positive but lose statistical significance.

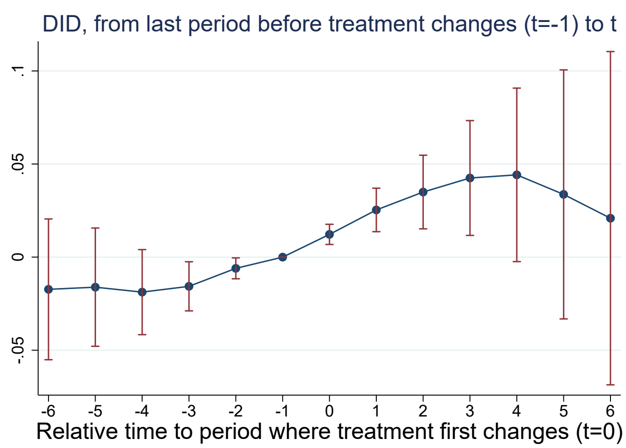
Figure F.3 shows the same estimates excluding the five states which deregulated their banking sectors after 1990. This group shows tighter pretrends in the years leading up to deregulation. The vast majority of states had passed IBD by 1990 and national deregulation legislation was being discussed at that time, so these states would have been the most likely to implement changes in the structure of their economy ahead of deregulation. This approach leads to very similar point estimates, suggesting that these outliers are not driving my baseline results. Overall, these findings are consistent with my estimates from Section 4 and provide further evidence that IBD led to a decline in the manufacturing employment share that was driven by an increase in nonmanufacturing employment.




(a) Manufacturing employment share



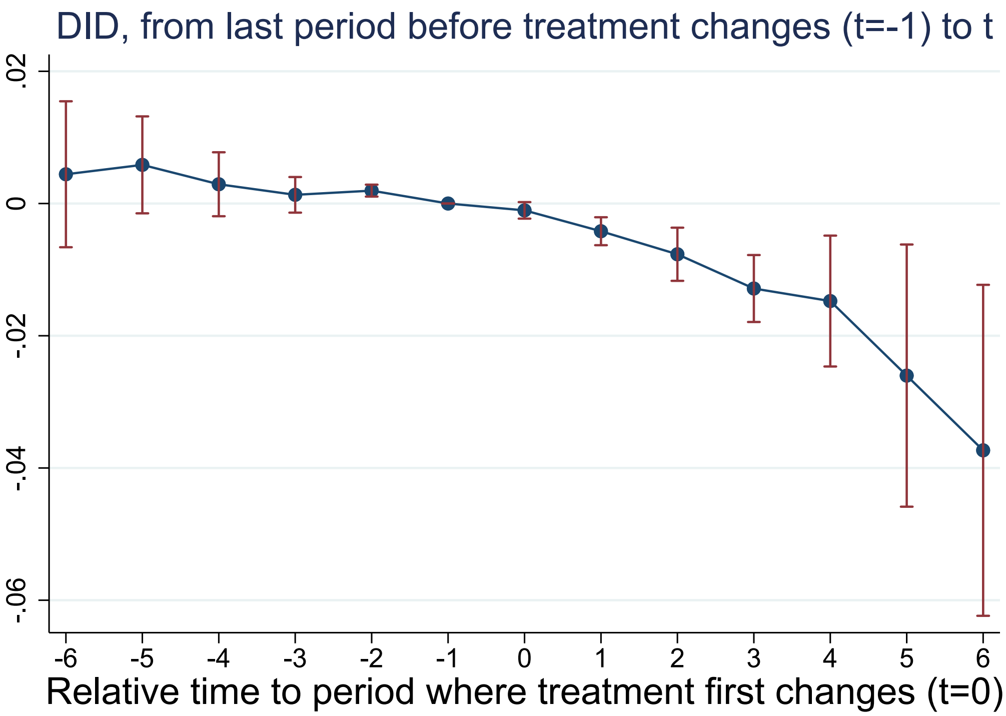

(b) Log manufacturing employment



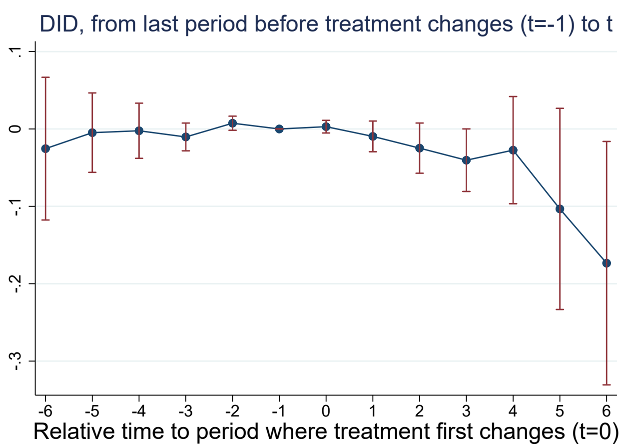

(c) Log nonmanufacturing employment

Figure F.2: Effect of IBD on Manufacturing and Nonmanufacturing Employment

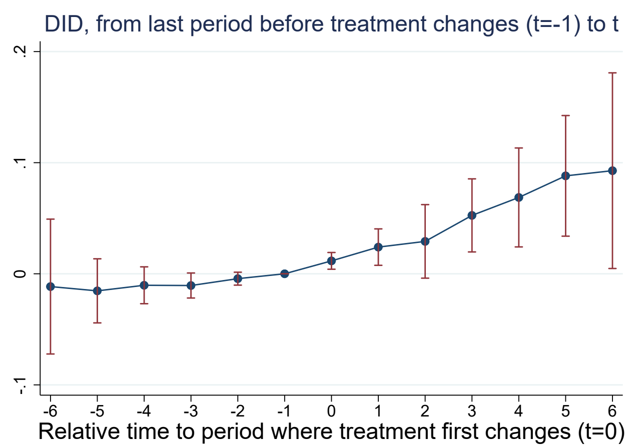
Note: This figure shows cumulative dynamic responses of the manufacturing employment share (top, pp) and log levels of manufacturing and nonmanufacturing employment (bottom, log points) to IBD relative to the year before treatment ($t = -1$) using the approach of De Chaisemartin and d'Haultfoeuille (2020). 95% confidence intervals are calculated from 100 boostrap draws and clustered at the state level.




(a) Manufacturing employment share




(b) Log manufacturing employment




(c) Log nonmanufacturing employment

Figure F.3: Effect of IBD For States Deregulating by 1990

Note: This figure shows cumulative dynamic responses of the manufacturing employment share (top, pp) and log levels of manufacturing and nonmanufacturing employment (bottom, log points) to IBD relative to the year before treatment ($t = -1$) using the approach of De Chaisemartin and d'Haultfoeuille (2020). States that deregulated after 1990 are excluded. 95% confidence intervals are calculated from 100 boostrap draws and clustered at the state level.

## F.3 Further Comparison to Mian et al. (2020)

| Manufacturing industries | Nonmanufacturing industries |
|---|---|
| Apparel and textile products | Holding and other investment offices |
| Chemicals | Insurance agents, brokers, and service |
| Electronics and electric equipment | Insurance carriers |
| Fabricated metal products | Local passenger transit |
| Food products | Pipelines (except natural gas) |
| Furniture and fixtures | Real estate |
| Industrial machinery and equipment | Security and commodity brokers |
| Instruments and related products | Air transportation |
| Leather products | Transportation services |
| Lumber and wood products | Amusement and recreation services |
| Miscellaneous manufacturing | Apparel and accessory stores |
| Motor vehicles | Automotive dealers |
| Ordnance | Automotive repair |
| Other transportation equipment | Building materials and garden stores |
| Paper products | Business services |
| Petroleum and coal products | Communications |
| Primary metals | Depository and nondepository institutions |
| Printing and publishing | Eating and drinking places |
| Rubber and plastic products | Educational services |
| Stone, clay, and glass products | Electric, gas, and sanitary services |
| Textile mill products | Engineering and management services |
| Tobacco products | Food stores |
| | General merchandise stores |
| | Health services |
| | Home furniture stores |
| | Hotels and lodging |
| | Legal services |
| | Membership organizations |
| | Miscellaneous repair services |
| | Miscellaneous retail |
| | Motion pictures |
| | Museums and zoos |
| | Other finance, insurance, and real estate |
| | Personal services |
| | Private households |
| | Railroad transportation |
| | Social services |
| | Trucking and warehousing |
| | Water transportation |
| | Wholesale trade |

Table F.1: Industry Classification for State-by-Industry Analysis

| | Employment effect of IBD (%) |
|---|---|
| **Nontradable manufacturing** | |
| Wood products | 0.34 |
| | (2.37) |
| Stone, clay, and glass | 0.96 |
| | (1.12) |
| **Tradable nonmanufacturing** | |
| Wholesale trade | 0.59 |
| | (0.57) |
| Communications | 0.39 |
| | (0.67) |
| Finance, insurance, and real estate | 1.73*** |
| | (0.67) |
| Business services | 1.61 |
| | (1.16) |
| Motion pictures | 2.47 |
| | (1.76) |

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

Table F.2: Effects of IBD on Employment

Note: This table shows the estimates of the effects of IBD on sectoral employment outcomes using the approach of De Chaisemartin and d'Haultfoeuille (2020). Industry selection is based on the manufacturing sectors classified as least tradable (and nonmanufacturing sectors classified as most tradable) from Mian and Sufi (2014). Standard errors are clustered at the state level and calculated using 100 bootstrap draws following for the DiD analysis.

| | $\pi^T$ | $\pi^N$ |
|---|---|---|
| Mian et al. (2020) prices | 0.33 | -1.09* |
| | (0.46) | (0.65) |
| Hazell et al. (2020) prices | 0.39 | -0.30 |
| | (0.53) | (0.61) |

Table F.3: Effects of IBD on Tradable and Nontradable Inflation

Note: This table shows the estimates of interstate banking deregulation on the prices of tradable and nontradable goods. Estimates are obtained from Equation 2 in Section 4 of the main paper using the approach of De Chaisemartin and d'Haultfoeuille (2020) where the dependent variable is replaced with measures of inflation. In MSV, tradable prices are calculated as the CPI for commodities, and nontradable prices are calculated as the CPI for services. For these results I follow MSV and exclude Alaska. Standard errors are clustered at the state level and calculated using 100 bootstrap draws.

# G 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. I also reconcile my results to those in Eisfeldt and Rampini (2006).

## G.1 Manufacturing Productivity Growth

This is calculated as the ratio of manufacturing productivity to total nonfarm productivity and is shown in Figure G.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.

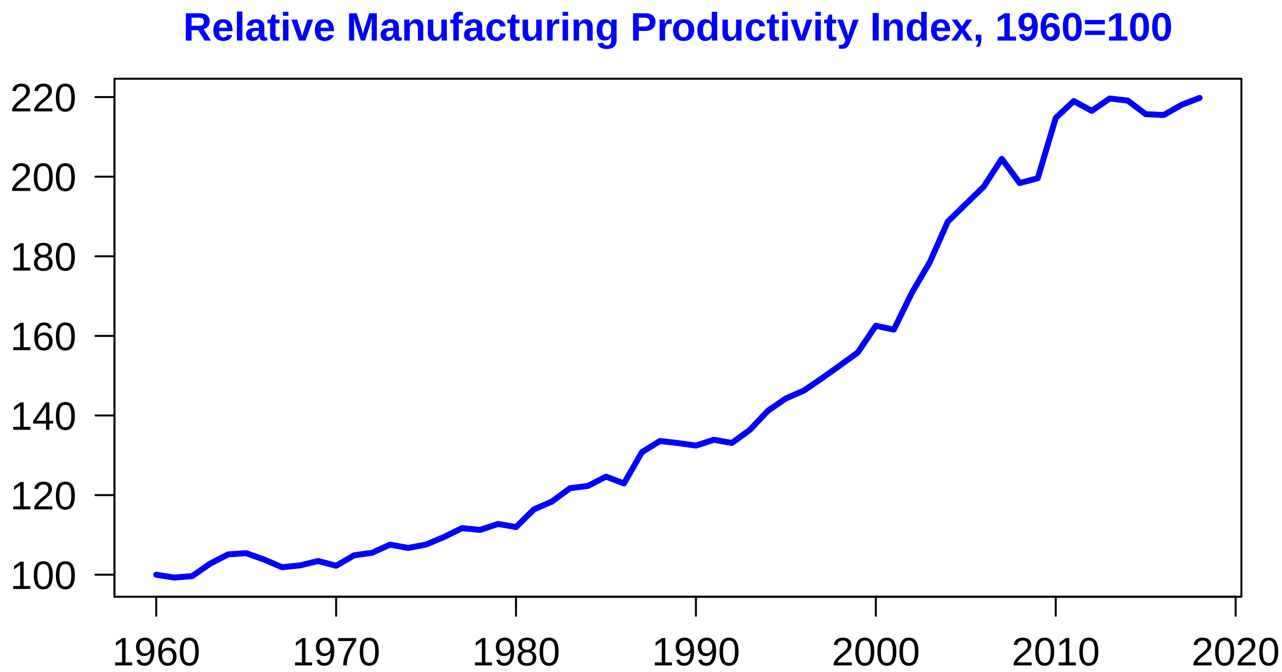



Figure G.1: Manufacturing Relative Productivity Growth

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 G.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 $\alpha^*$ 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.

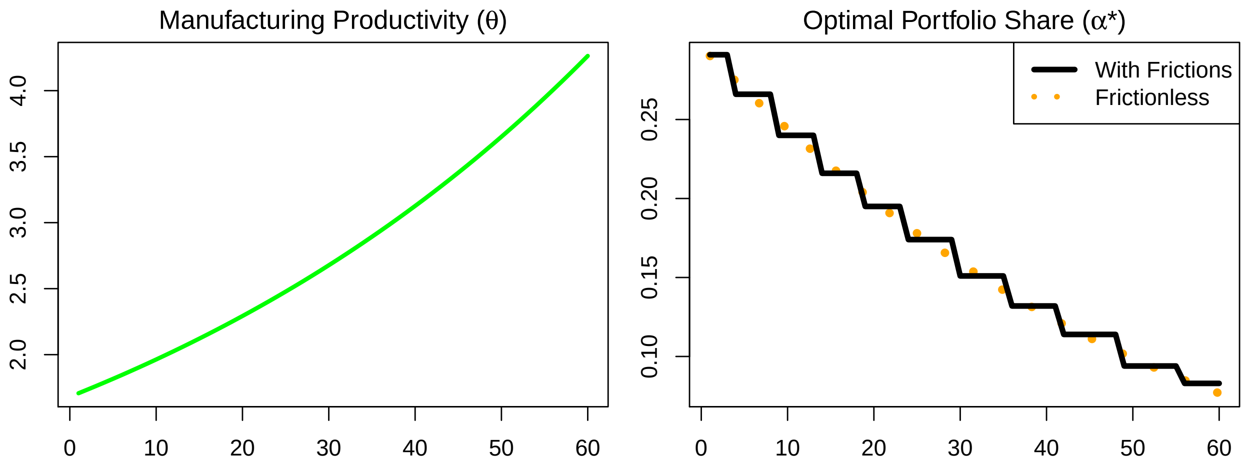



Figure G.2: Model without Recessions

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.

## G.2 Comparing Results with Eisfeldt and Rampini (2006)

The main conclusions of Eisfeldt and Rampini (2006) are that informational and contracting frictions associated with reallocating physical capital become worse during recessions. This section provides some discussion about why our results differ and argues that their findings are unlikely to have a meaningful quantitative impact on my main results.

The timing of adjustment is one potential factor that can cause these differences. Credit reallocation occurs in my model as credit is rebuilt following a recession. I assume for simplicity that this happens entirely within a recession, so that the world is back to normal the next period. In reality, this process is more gradual and can take several years. A more realistic model would allow for delays between when credit is lost and when it is reallocated. Furthermore, investment is well-known to exhibit gradual and hump-shaped responses to macroeconomic shocks (see Christiano et al. (2005)). This suggests a more complex version of the model could generate capital reallocation with cyclical properties closer to that of Eisfeldt and Rampini (2006).

Even taking the contrasts between our results at face value, however, the quantitative magnitudes that they find are small enough to allow their measure of reallocation to be procyclical without having a meaningful impact on my results. Because the stock of financial capital is fixed in my model, the most relevant measure of reallocation in their paper is the reallocation turnover rate. In the simplified setting in which there was no distinction between new and used capital, the reallocation turnover rate would be constant over the business cycle. Across specifications, they instead find that the ratio of reallocation in high-output states to low-output states is on the order of 1.1-1.2. The authors note that capital reallocation accounts for about 1.4% of assets on an annual basis (shown in Table 1 of their paper), which suggests that the cyclical variation in capital reallocation is small in magnitude relative to my main results.

In addition, what matters in my setting is the reallocation *across* sectors, and within-sector capital reallocation is not something I explicitly model. While I am not aware of any studies which analyze the flow of capital across industries, Golan et al. (2007) finds that roughly half of all job reallocation occurs across industries. Treating this as an upper bound, which I suspect is very conservative given the industry-specific purposes of many types of manufacturing equipment, would suggest that less than 1% of assets would be affected by this mechanism. Thus while this mechanism is interesting and shares many similarities with my main results, the size of the affected stock of assets and magnitude of cyclical fluctuations are unlikely to have a meaningful impact on my findings. Explicitly modeling how capital would affect these dynamics and thinking about the interactions with allocations of credit and labor both within and across industries is a very interesting question that I leave for further reasearch.

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