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

Cooper Howes

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

Cooper Howes[†]

First version: November 8, 2019
This version: November 2, 2020

Abstract

The decline of the US manufacturing share since 1960 has occurred disproportionately during recessions. Using evidence from two natural experiments—the collapse of Lehman Brothers in 2008 and US interstate banking deregulation in the 1980s—I document a role for credit reallocation in explaining this phenomenon by showing that losing access to credit disproportionately hurt manufacturing firms, and that the creation of new credit disproportionately benefited nonmanufacturing firms. These results arise endogenously from a model with technology-driven structural change and fixed costs of establishing new financial relationships. The model suggests an important role for long-run industry trajectories in properly accounting for the costs and benefits of policy interventions in credit markets.

Keywords: Structural change, reallocation, financial frictions

---

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

1 Introduction

One of the most prominent and well-documented changes in the structure of US economic activity over the past several decades has been a shrinking manufacturing sector and a corresponding increase in the size of the service sector. Less well-known is the fact that this reallocation has occurred predominantly during recessions (Figure 1). This paper argues that credit reallocation can account for this phenomenon. Due to the presence of fixed costs of establishing new financial relationships, many manufacturing firms which initially obtained financing during their industry's heyday will continue to receive credit even as technological progress changes the structure of the economy over time. Outside these relationships, however, manufacturing firms will increasingly be at a disadvantage relative to firms in an expanding service sector. Periods of increased destruction of firm-bank matches (recessions) will thus be followed by periods in which credit flows disproportionately to nonmanufacturing firms as new relationships are established (reallocation).

I provide empirical evidence for this mechanism using two natural experiments: the collapse of Lehman Brothers in 2008 and the staggered implementation of US interstate banking deregulation during the 1980s and early 1990s. While all firms with lines of credit through Lehman were exposed to a credit shock when it collapsed, manufacturers were persistently less likely to obtain new loans in the following years and suffered worse real outcomes. Similarly, the expansion of credit that followed the relaxation of interstate banking restrictions had no effect on manufacturing employment but led to persistent increases in nonmanufacturing employment. These findings suggest that policies which seek to maintain financing for firms in declining sectors in the aftermath of a crisis can restrict credit from flowing to newer, more valuable sectors.

I begin by documenting the outsized role of recessions in accounting for the declining importance of manufacturing in the US economy since 1960. The manufacturing employment share in the US has declined from 28.9% in 1960 to 8.5% at the end of 2018. Half of the decline during this period occurred during the 22% of years classified by the National Bureau of Economic Research as including a recession, and shares of other activity measures such as value added or gross output show similar patterns. This disproportionate decline in manufacturing during recessions suggests that business cycles and structural change are more tightly connected than commonly assumed.

The key contribution of this paper is to provide a mechanism that can account for this link: a credit reallocation channel. I do this in three steps. First, I follow Ivashina and Scharfstein (2010) and Chodorow-Reich (2014) and use the collapse of Lehman Brothers as an exogenous credit supply shock. My main analysis uses syndicated loan data from DealScan merged with firm characteristics from Compustat. This allows me to use variation across time, sectors, and

bank exposure to compare the long-term effects of having an open line of credit with Lehman at the time of its bankruptcy for firms in different sectors. All firms who lost access to credit when Lehman collapsed were more likely than the average firm without Lehman exposure to get new loans in subsequent years as they attempted to find new sources of financing. Nonmanufacturing firms with one additional revolving line of credit through Lehman were roughly 8.5 percentage points more likely to obtain a new loan in each year from 2009-2016. This effect is economically significant and represents more than one-third of the average annual probability of obtaining a loan for these firms. Manufacturing firms were only 3.1 percentage points more likely to get a loan during this time, however, suggesting that credit was reallocated out of this sector. This reallocation of credit had real effects; exposure to Lehman led to annual reductions in sales and employment of about six percent for manufacturers, but had no effect for nonmanufacturing firms.

Second, to show that this mechanism can be seen beyond the Great Recession, I follow the methodology developed in Jayaratne and Strahan (1996) to analyze the heterogeneous effects of US interstate banking deregulation across sectors. Over the course of 1978-1994, almost all states passed laws easing restrictions for out-of-state banks without previously established relationships, which led to an increase in available credit. This credit disproportionately benefited nonmanufacturing firms; deregulation led to an estimated 0.2 percentage point decline in a state's manufacturing employment share, effectively accelerating structural change by an amount equal to what was observed during the Great Recession. Despite having opposite effects on the *level* of activity as Lehman's collapse, banking deregulation had the same effect on its *composition*; these results are difficult to explain through purely cyclical mechanisms but both are clear predictions of the credit reallocation channel proposed in this paper. As a result, this experiment addresses directly the concern that my paper's main results are driven by a cyclically sensitive sector in the midst of a long-run decline.

The third step is to show that a model incorporating such a channel can account for both the long-run structural trends and cyclical properties of the manufacturing share. The model includes three key pieces. The first is an input share for manufacturing that declines over time. I follow an extensive structural change literature and consider a class of models in which this long-run decline is the result of exogenous growth in technological progress as in Ngai and Pissarides (2007). The second is the requirement that firms need to obtain credit through a relationship with a bank and the presence of fixed costs of establishing such a relationship, which is consistent with an extensive literature in economics and corporate finance related to relationship lending including Hachem (2011).[1] The third feature is the presence of recessions that separate firm-bank matches.

---

[1]Earlier examples include Boot (2000), Elyasiani and Goldberg (2004), and Elsas (2005).

In my model, it is the interaction between the long-run productivity-driven decline in the manufacturing share and fixed costs of establishing new relationships that generates the stylized facts observed in the data. The secular trend in manufacturing's share of activity reduces the benefit of providing credit to manufacturing firms over time. Rather than occurring smoothly, the presence of fixed costs will cause credit reallocation out of the manufacturing sector to be concentrated in a few periods.[2] Recessions will reduce the opportunity cost of reallocation by destroying relationships and lowering the value of inaction. While recessions in the model are periods of increased reallocation, the separations they cause are not efficient, and welfare in the model would be strictly higher in their absence.[3] Following the recession, it is the lowest-productivity manufacturing firms—which were only receiving credit prior to the recession because of the inertia resulting from switching costs, and which would eventually become obsolete due to structural change even if the recession had never occurred—that find themselves on the losing end of credit reallocation.

This fact has important consequences for how policymakers should respond to economic downturns. If recessions are the least costly times to reallocate, then policy interventions that respond to them by providing financing to firms in declining sectors can distort credit flows and reduce welfare. One example of an industry-specific intervention in credit markets during and after a recession is the US Treasury's Automotive Industry Refinancing Program (AIFP) from 2008-2014, which provided credit to struggling US automakers. As noted by Goolsbee and Krueger (2015), there were a variety of justifications for providing this credit to automakers rather than other firms—including worker-level job switching costs, deadweight losses from bankruptcy, and effects on supplier networks—that are beyond the scope of my model. What the model can do, however, is shed light on the following question: conditional on intervening in credit markets following a recession, what are the costs of lending to firms in declining sectors instead of growing ones?

After calibrating the model to match the size and timing of structural change in the data, I simulate a policy which re-establishes all relationships destroyed during the Great Recession and maintains them for six years. This policy provides credit to many manufacturing firms that

---

[2]This mechanism does not rely on the assumption of greater cyclical sensitivity of the manufacturing sector; in fact, manufacturing's decline is concentrated during recessions in the model even when the majority of matches destroyed during a recession are in the service sector.

[3]Policies *preventing* separations and maintaining financial relationships during recessions—even for the least productive manufacturing firms that receive credit—could improve welfare in this setting. I abstract from such bank bailout policies in this paper for two reasons. The first is that bank bailout policies have nothing to say about the allocation of credit created by the entry of new banks, which I show is a key part of the credit reallocation channel in Section 4. The second is that bailouts may not always be desirable for political or logistical reasons even if they are economically beneficial.

would otherwise not have been able to obtain it following the crisis. My model suggests that the costs of preventing credit flows to more valuable sectors are significant. The cumulative output losses due to misallocation over this six-year period are equal to approximately 78% of the initial credit outlay. In the case of the AIFP, this would amount to $63bn, far exceeding the program's realized losses due to non-repayment of $12bn. While these estimates must be weighed against the benefits arising outside my model, they suggest that the distortions of these policies are significant even in cases where there is no credit risk and that policymakers should take long-run industry trajectories into account when intervening in credit markets.

Related literature. The first strand of literature to which this paper contributes is the large body of work regarding the countercyclical reallocation of resources. The notion that reallocation of productive resources occurs disproportionately during economic downturns dates back to at least Schumpeter (1934), who referred to crises as "...[The] process by which economic life adapts itself to the new economic conditions." More recent examples include Davis and Haltiwanger (1992), Caballero and Hammour (1994), Caballero and Hammour (1996), Aghion and Saint-Paul (1998), Hall (2000), Caballero and Hammour (2005), Koenders et al. (2005), and Berger (2018). This line of research has provided formal analytical frameworks for thinking about the reallocation of resources over the business cycle, brought these theories to the data, and analyzed their causes and consequences. A desire for model parsimony and data constraints have led these papers to generally focus on reallocation occurring within a single sector.[4] A key contribution of this paper is to establish an important role for reallocation *across* sectors.

This paper also builds on work that leverages "natural experiments" in cross-sectional credit availability to identify the effects of these disruptions. Peek and Rosengren (1997) and Peek and Rosengren (2000) use geographic variation in the penetration of Japanese bank branches in the United States to show that financial shocks that originated in Japan in the 1990s were transmitted through US branches of Japanese banks. Financial crises in Japan are also used by Gan (2007), who looks at exposure to real estate markets for Japanese banks, and Amiti and Weinstein (2011), who analyze the behavior of Japanese exporters. Several papers use natural experiments in credit supply in the aftermath of the global financial crisis to analyze the effects on both real and financial outcomes in various European countries, including Cingano et al. (2016) (Italy), Bentolila et al. (2017) (Spain), Iyer et al. (2013) (Portugal), and Huber (2018) (Germany). Other examples of work in this vein include Schnabl (2012) and Paravisini et al. (2014).

---

[4]These papers also tend to abstract from credit. The relationship between business cycles and credit reallocation has been examined in work such as Barlevy (2003), Dell'Ariccia and Garibaldi (2005), Herrera et al. (2011), Herrera et al. (2014), Contessi et al. (2015), and Borio et al. (2016), but these papers do not consider the structural change implications.

In terms of methodology, the paper in the credit shock literature that most most closely matches my own is Chodorow-Reich (2014), who also uses the Lehman Brothers bankruptcy as an exogenous credit shock as proposed by Ivashina and Scharfstein (2010). His approach uses confidential Census microdata to demonstrate the heterogenous effects of changes in lender health across firms of different sizes, showing that small firms were disproportionately harmed when their lenders were exposed to credit shocks; my work, which uses data on publicly traded firms in Compustat, instead focuses on heterogeneity across sectors and finds that manufacturing firms directly exposed to credit shocks through syndicates involving Lehman at the time of its collapse were disproportionately affected.

Analysis of the macroeconomic effects of US interstate banking deregulation dates back to Jayaratne and Strahan (1996). Allowing entry by out-of-state banks has been shown to boost credit availability for entrepreneurs (Black and Strahan (2002)), spur innovation (Amore et al. (2013), Chava et al. (2013), and Cornaggia et al. (2015)), reduce the volatility of business cycles (Morgan et al. (2004), Acharya et al. (2011)), and lead to increases in inter-firm credit reallocation (Herrera et al. (2014)). More recent work by Bai et al. (2018) and Mian et al. (2020) has shown that these policies had the most benefit for young, productive firms and that they mostly affected the nonmanufacturing sector. My work differs from these papers by establishing a causal link between credit availability and the timing of long-run structural change.

Work analyzing the causes and consequences of structural change back to Kuznets (1957) and Baumol (1967) and includes more recent examples such as Kongsamut et al. (2001), Ngai and Pissarides (2007), Acemoglu and Guerrieri (2008), Buera and Kaboski (2009), Duarte and Restuccia (2010), Ray (2010), Alvarez-Cuadrado and Poschke (2011), Herrendorf et al. (2014), Boppart (2014), Comin et al. (2015), and Alder et al. (2019). My work focuses on the decline of manufacturing activity in the US over the past 60 years. I am aware of only one other paper that directly analyzes the relationship between business cycles and structural change: Storesletten et al. (2019) study how the industrialization of China's agricultural sector changed the properties of its business cycles over time. My paper focuses instead on the decline in the US manufacturing sector to establish a role for business cycles in explaining the timing of structural change.

Jaimovich and Siu (2019) also study the interaction between recessions and long-term trends, but in the context of job polarization (the reduction in the share of middle-skill jobs in the economy) rather than structural change. They find that job polarization accelerates during recessions and that this phenomenon can explain the "jobless recoveries" following recessions in recent decades. Their empirical findings are similar to my paper, in which the observed shift in activity from the manufacturing to nonmanufacturing sectors is concentrated during recessions

due to the countercyclical opportunity cost of reallocation. Similarly, Hershbein and Kahn (2018) find that skill-biased technological change accelerates during recessions. Work by Groshen and Potter (2003) and Bárány and Siegel (2018), who argue that long-run trends in job polarization are closely related to the secular decline in manufacturing, suggests that all of these results may reflect similar underlying mechanisms. The core mechanism in my paper is also closely related to that of Foote (1998), who analyzes the interaction of (S, s) bands with long-run trends in the context of manufacturing employment.

Finally, a closely related literature including Peek and Rosengren (2005) and Caballero et al. (2008) has analyzed the role for policy interventions preventing credit reallocation in creating "zombie" firms. These papers argue that banking regulations created perverse incentives for Japanese banks to pump credit into failing firms in the 1990s in order to avoid having to mark down assets on their balance sheets, resulting in inefficient flows of credit to weak firms. As a result, in their framework it is the same firms that shouldn't be receiving credit during normal times that benefit from increased credit access during downturns. In contrast, all loans in my my model are constrained efficient, and it is structural change rather than regulatory distortions that make re-establishing relationships destroyed during recessions costly.

The paper proceeds as follows. Section 2 discusses structural change in the US over the past several decades and provides a conceptual overview of the role for credit reallocation in explaining its timing over the business cycle. Section 3 uses firm-level loan data to provide empirical evidence of heterogeneity in responses to credit shocks across sectors. Section 4 shows that similar heterogeneity was observed following the wave of US interstate banking deregulation that occurred from 1978-1994. Section 5 describes the model, its ability to match the patterns observed in the data, and its implications for policymakers. Finally, Section 6 concludes.

2 Background and Motivation

2.1 The Decline of US Manufacturing from 1960-2018

Structural change is the phenomenon by which economies tend to transition from agriculture to manufacturing to services as they develop. I focus on the decline in the role of US manufacturing in this paper. In 1960, 28.9% of all nonfarm payroll employment was in the manufacturing sector. By 2018, that share had fallen to 8.5%. This trend is shown as the solid blue line in Figure 1. Rather than falling uniformly, this share has tended to decline disproportionately during years classified by the NBER as recessions, which are shown as the shaded gray areas.

The dashed red line plots the path that would have occurred if there were no change in the manufacturing share during recessions. To calculate this series, I start at the 1960 level. From this point, I apply the same change as the actual series if it occurs during a year that does not have a recession. If the quarter is part of a year with a recession, I instead impose a change of zero. The total series has declined by 20.4pp between 1960 and 2018 (represented by the gap between the black and blue lines). The contribution to this change from non-recession periods is 10.2pp and is represented by the difference between the red and black lines. The remaining 10.2pp decline occurred during recessions, corresponding to the gap between the blue and red lines. Thus purely from an accounting standpoint recessions account for as much of the decline in the manufacturing employment share as non-recessions despite the fact that recessions occurred in just 22% of years from 1960 and 2018.[5] As I show in Table A.1 of the appendix, similar patterns also show up in other measures of the role of manufacturing in the US economy including value added, consumption, or gross output. Regardless of how it is measured, manufacturing's decline has occurred disproportionately during recessions. The next section describes how a credit reallocation channel can generate this phenomenon and outlines several testable implications.

2.2   Framework and Mechanism

This subsection describes a channel through which credit reallocation can cause structural change to accelerate in recessions. This stylized illustration produces clear and testable predictions that will be taken to the data in Sections 3 and 4 and provides intuition for the model that will be developed in Section 5. A visual illustration of these descriptions can be found in Appendix B.

The first key assumption of the model is that firms must obtain credit through a banking relationship—the initial formation of which incurs a fixed cost—in order to produce. The second assumption is that long-run structural change will exogenously lower the value of allocating bank credit to manufacturing firms over time. Fixed costs of forming new banking relationships mean that, rather than occurring smoothly along with the fundamental forces driving structural change, the shift of credit from the manufacturing to nonmanufacturing sectors will be lumpy. The availability of new credit will have important consequences for the timing of structural change in this setting. Newly available credit—which, unlike the stock of existing credit, is not part of an existing match and has no opportunity cost of reallocation—will flow disproportionately to the service sector, because structural change has made this sector more valuable. Any exogenous increase in

---

[5]An alternate way to visualize this counterfactual change is to calculate the path for the manufacturing employment share that would have occurred if, instead of replacing changes during recession years as zero, I replace them with the average change during non-recession periods. This is shown in Figure 1 as the dotted green line.

supply of available credit would thus be expected to lead to a decline in the manufacturing share.

One way for new credit to become available is through the destruction of an existing match. In the case of bank failure, for example, all firms previously attached to that bank would be forced to re-enter the pool of firms seeking credit. This mechanism has a clear prediction for how these firms should fare: nonmanufacturing firms exposed to a failing bank will be more likely to obtain new credit in the aftermath of the crisis, leading to a decline in the manufacturing share of activity. Firm (rather thank bank) failure would also free up credit and thus be expected to have the same effect.[6]

This paper uses two natural experiments to test these predictions. In Section 3, I examine the effects of bank failure by using the collapse of Lehman Brothers in 2008. I find that manufacturing firms exposed to Lehman were persistently less likely to be able to obtain new loans and experienced worse real outcomes in 2009 and beyond relative to nonmanufacturing firms who had relationships with Lehman. Next, in Section 4, I analyze the effects of credit expansion by using variation in the timing of US interstate banking deregulation. I find that allowing out-of-state banks to enter significantly boosted a state's nonmanufacturing employment without having any effect on manufacturing employment, thus leading to a reduction in the manufacturing employment share.

The mechanism in my paper is distinct from that of past work such as Abraham and Katz (1986), who point out that a negative correlation between an industry's trend growth rate and its cyclical sensitivity will mechanically generate an increase in the dispersion of employment growth across industries during recessions. The cyclical explanation at the heart of their paper is ultimately symmetric; if the manufacturing share falls faster during downturns, it would be expected to fall more slowly (or rise) during booms. Instead, I find that the expansion of credit that followed interstate banking deregulation led to a substantial *acceleration* in the manfuacturing share's decline. The fact that both expansionary and contractionary credit supply shocks have the same effect on the manufacturing share cannot be explained by purely cyclical mechanisms but is a natural consequence of the credit reallocation channel proposed in this paper.

---

[6]The difference between firm and bank failure in this setting depends on how credit is rebuilt following bank exit. If bank failure is immediately followed by entry of new banks so that the total supply of credit is unchanged, the compositional effects of firm and bank failure are identical, as any newly available credit will still disproportionately flow to firms in newer sectors. While allowing for a delay in the creation of new credit can affect the magnitude and pace of reallocation, it does not change the sign of the predicted effect.

3 Evidence from Bank Failure

3.1 Data

The main source of data in this paper is Refinitiv's DealScan database of large bank loans. Information on these loans is gathered through a combination of SEC filings, media reports, and trade publications. Lenders generally have incentives to report these loans so they can be included in public rankings (so-called "league tables") that are often referenced for marketing purposes. The majority of loans in the data are syndicated, which means that the funding of the loan is provided by a group of banks and other financial institutions. These loans have become more common since their inception in the 1980s; in addition to increasing size of bank loans that firms could obtain by limiting the risk any single bank must hold, the syndicated loan market also allowed non-bank institutions (such as hedge funds, pension funds, and other more complex investment vehicles) to obtain exposure to corporate debt outside of bond markets. Syndicated lending represents close to half of all US commercial and industrial (C&I) lending, including around two-thirds with maturity greater than one year. An example loan is shown in Figure C.1 of the appendix.

Table 1 shows a range of summary statistics. While DealScan includes many loans for firms in other countries and in other currencies, I focus on US dollar-denominated loans starting in 2000. The average loan size is about \$250mn, with a median of \$75mn. 90% of loans were at least \$8mn, and this cutoff rose to \$14mn after the financial crisis. The "price" of the loans, which is measured as a spread over the London Interbank Offered Rate (LIBOR) inclusive of fees, averages around 200-300 basis points.[7] The definition of "real investment" loans is based on Ivashina and Scharfstein (2010) and includes loans reported for "working capital" or "corporate purposes"; in contrast to financing arrangements for purposes such as stock buybacks or leveraged buyouts, these loans are more likely to be used for financing day-to-day operations. In addition to these characteristics, DealScan also reports the borrower and all members of the lending syndicate for each loan.[8] They also include information on the terms of the loan such as its size, maturity, and purpose. To match the observed loans with detailed firm characteristics such as sales and

---

[7]This is referred to as the "all-in-drawn spread" and corresponds to the total spread that would be paid by the borrower if they were to draw down the entire facility. For the small minority of these loans with a base interest rate other than LIBOR, DealScan makes an adjustment based on the historical relationship between the alternative reference rate and LIBOR. This measure, while imperfect, is consistently available throughout the DealScan sample and as a result is widely used throughout the literature.

[8]While the data do not identify the exact liability distribution across syndicate members, they do provide some information about roles (such as "lead arranger" or "participant") that loosely correlate with the lender's stake.

employment, I use the matching procedure outlined in Chava and Roberts (2008). The process of creating my sample is described in detail in Appendix C.

3.2 Identification Strategy

Lehamn Brothers declared bankruptcy on September 15, 2008 during one of the most tumultuous days in the history of modern financial markets.[9] At that time, Lehman's $639 billion in total assets made it the fourth-largest US investment bank, and its bankruptcy remains the largest in US history. Despite showing signs of stress in the months leading up to its collapse—it was actively seeking buyers for its investment banking business at the time[10]—Lehman's failure was seen as a massive and unexpected shock to financial markets, as equities fell by almost 5% on September 15 and LIBOR rose more than 3 percentage points the following day. Ivashina and Scharfstein (2010) and Chodorow-Reich (2014) provide persuasive evidence that the root causes were found in Lehman's exposure to toxic real estate assets and that its corporate loan portfolio played no significant role. These factors, combined with Lehman's large and diverse set of customers, make for a useful laboratory in which to analyze the effects of credit supply shocks.

I define "Lehman attachment" throughout this paper to mean that a firm had a revolving line of credit that satisfied the following properties: 1) Lehman Brothers was one of the syndicate members; 2) the facility had a start date in 2007 or earlier; and 3) the facility had an end date of 2009 or later.[11] I focus on revolving lines of credit because the bankruptcy of a syndicate member in this case results in a direct reduction in the quantity of credit available to the borrower.[12]

This assumption would be violated if the manufacturing firms who received lines of credit from Lehman Brothers were systematically more likely to have unobserved qualities which caused lower sales and employment in the post-recession period. In this case, my specification would erroneously attribute the effects to Lehman attachment when the underlying cause was actually

---

[9]A complete timeline of the crisis can be found here: https://fraser.stlouisfed.org/timeline/financial-crisis

[10]See https://www.nytimes.com/2008/09/11/business/11lehman.html

[11]This approach is similar to the one used in Chodorow-Reich (2014), which is based on Ivashina and Scharfstein (2010). Whereas my analysis treats a firm as being exposed to a credit shock if it received a revolving loan through a syndicate involving Lehman, the approach used in these papers considers the effects of firm attachment to banks which themselves had a large degree of syndicate overlap with Lehman. Focusing on the set of firms which were directly exposed to credit shocks is important for my purposes because these firms were more likely to be forced to seek new financing immediately.

[12]If a firm had received a term loan in which Lehman was involved at the time it went bankrupt, the liability would change hands but it would not affect the amount of money that the firm had received. While this may have had other indirect effects on the firm's ability to obtain credit, particularly if they had cultivated a working relationship with Lehman, there would not be direct effects in terms of their access to credit.

unobserved firm quality. Based on observable characteristics this does not appear to be the case. Table 2 shows summary statistics from 2004 based on whether firms would end up having an open line of revolving credit with Lehman in 2008. Firms with Lehman attachment tended to be much larger in terms of sales, assets, and employment, but these gaps were similar across sectors. Similarly, Lehman's clients in all sectors received more loans and paid lower interest rates their non-Lehman counterparts. Spreads between Lehman non-Lehman firms were very similar across sectors, averaging 42 basis points for manufacturers and 45bp for nonmanufacturers. These observations are in line with market perceptions that clients of Lehman Brothers tended to be larger institutions[13] but do not suggest any differential selection across sectors. To supplement this evidence, I show in Appendix D that firms with Lehman attachment looked very similar to firms who were attached to one of Lehman's peer institutions over the same time period, and that Lehman's customers were not charged higher rates by its competitors.

Despite virtually no observable difference between firms with and without Lehman attachment in the years leading up to the crisis, the differences in outcomes for these two groups in 2009 and beyond are striking even in the raw data. The top panel of Figure 2 shows aggregates for sales in Compustat split by firms with and without Lehman attachment and by manufacturing/nonmanufacturing. Despite similar trends for all groups of firms in the years leading up to the recession, this figure shows that manufacturing firms with Lehman attachment saw large and persistent drops in aggregate sales and employment in the years following the Great Recession, reaching declines of up to 40% by 2016. Aggregates for both nonmanufacturing firms with Lehman attachment and manufacturing firms without Lehman attachment, on the other hand, experienced much faster recoveries. Fewer than 100 firms in each sector had direct exposure to Lehman Brothers, comprising a tiny fraction of the roughly eight thousand firms in the sample from 2000 through 2008. These firms tended to be much larger, however, and as a result their contributions to the aggregates were substantial; the set firms defined as having Lehman attachment during its collapse represented between 12-14% of all manufacturing sales and 10-15% of all nonmanufacturing sales from 2000-2008.

A key prediction of my mechanism is that the sales declines observed in the aggregate data should be driven by firms who are unable to obtain new credit. To test this, I split the sample by the number of loans that a firm received in the aftermath of the crisis. Conditional on receiving at least one new credit facility that started between 2009 and 2016, the average number of new facilities for a firm in my sample was about three. In the left two panels of the bottom row of Figure 2 I plot the results for firms above and below this cutoff. Manufacturing firms who

---

[13]See https://www.nytimes.com/2008/09/15/business/15lehman.html

obtained three or fewer new loans ultimately saw their aggregate sales fall below 2002 levels by 2015. Manufacturers who received more than three new loans still showed a decline in the aftermath of the crisis, ultimately falling by about 20% relative to 2008 levels by 2016, but fared much better than firms who received the fewest loans. Finally, manufacturing firms who received at least one new loan every year—plausibly representing the set of firms whose access to financing was impeded the least by the Great Recession and shown in the bottom rightmost panel—behaved very similarly to firms without Lehman attachment. This suggests that exposure to a credit shock mattered to the extent that firms were prevented from finding other sources of financing. While Lehman attachment had a negative impact on access to credit, it was not necessarily a fatal circumstance; the set of manufacturing firms who were ultimately able to obtain a steady supply of new credit returned to roughly the same trend as their peers in other industries.

While these aggregate plots provide suggestive evidence that manufacturing firms were more exposed to credit market shocks, they could in principle be driven by either the intensive (lower sales per firm) or extensive (fewer firms) margins. In practice both of these margins appear to be important. On one hand, of the manufacturing firms which had attachment to Lehman in 2008, only about 72% were still in Compustat in 2016; this suggests that firm exit was an important component of the decline in aggregate sales. In the next section, however, I use panel regressions in which firm-year observations are only included if a firm is present in Compustat to show that firms who continued to operate but at a smaller scale are also a crucial driver of the post-crisis decline in sales for manufacturing firms with Lehman attachment.

3.3 Regressions Based on Bank Attachment

To more rigorously test the hypothesis that manufacturing firms were disproportionately affected by Lehman exposure, I use a triple difference specification that compares firms across sectors (manufacturing/nonmanufacturing), time (pre/post-2009), and whether they had an open credit facility through Lehman at the time of its collapse. My baseline regression specification is:

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

The unit of observation in this setting is a firm-year. $Y_{i,t}$ is the outcome of interest; I consider the effects of Lehman exposure on new loans, sales, and employment in my baseline results. This regression includes firm (α_i) and sector-by-year (σ_t, χ_t) fixed effects as well as a vector of lagged firm controls ($X_{i,t-1}$) including the firm's leverage ratio (total debt divided by total assets)

as well as logs of sales, assets, and employment. The inclusion of sector-by-year fixed effects mean that my results cannot be explained purely by the fact that the manufacturing sector was disproportionately affected by the collapse in aggregate economic activity that occurred during the Great Recession. The variable $Lehman_i$ represents the total number of revolving credit facilities held by firm i involving Lehman Brothers that started prior to 2008 and were originally scheduled to end in 2009 or later. My identification assumption is that, in the absence of Lehman's bankruptcy, the difference between the performance of manufacturing and non-manufacturing firms would have been the same for firms with and without Lehman attachment.

The coefficient ρ captures the average effect on Y of having one additional revolving Lehman facility open at the time of their collapse in the years after the financial crisis compared to the years before. The inclusion of this variable means that my results are not mechanically driven by differences in the allocation of Lehman's loans across sectors relative to other lenders. Ω is the primary coefficient of interest and represents comparison across three dimensions: manufacturing/nonmanufacturing firms, firms with and without Lehman attachment, and before/after 2009.[14] I focus on the number of exposed facilities rather than the size of total exposure because my results suggest in Appendix D show that the extensive margin—that is, changes in the number of loans companies receive rather than the average loan size—is the crucial channel through which aggregate credit reallocation operates.

The baseline variable of interest is a dummy variable $Y_{i,t}$ indicating whether firm i obtained at least one new real investment credit facility in year t. While DealScan plausibly captures all observations for which $Y_{i,t} = 1$, determining when to record $Y_{i,t} = 0$ is a bit more complicated because there are many reasons why a firm might not have a new loan in a given year. Firms may have applied for loans at a large number of banks and had all of their applications rejected; they may not have applied for any loans because they held sufficient liquidity or because they were able to draw on a credit line obtained in a prior year; or they may have gone out of business. The last concern is of particular importance in my setting given that the Great Recession hit the manufacturing sector harder than many other sectors in the economy. If manufacturing firms were more likely to go out of business, then my specification could be picking up compositional effects even if the firm-level probabilities of obtaining a loan were unchanged.

Merging the loan-level data with Compustat helps alleviate this concern. By including only firm-year observations in which a firm receiving a loan is observed in Compustat, I limit the scope through which my results can be mechanically driven by firm entry and exit. My baseline speci-

---

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

fication includes firms in Compustat that never receive a loan, though it is robust to restricting the sample to the subset of firms which have at least one loan observation in DealScan. I use observations from 2000-2016; while the results are robust to making the start date later, the loan data are more sparsely populated prior to the late 1990s. I stop in 2016 because it is the last full year in which DealScan and Compustat observations can be matched using the procedure in Chava and Roberts (2008). Finally, I only include firms who are included in Compustat by 2000 to allow for more precise estimation of fixed effects.

Based on the aggregate evidence shown previously, the estimate of Ω would be expected to be negative, reflecting the fact that manufacturing firms had a relatively harder time obtaining funding after losing access to credit during the crisis. The predicted sign of ρ, which represents the average effect of an additional open line of credit with Lehman for nonmanufacturing firms, is ambiguous. On one hand, the relationship lending literature predicts that, all else equal, getting a loan from a new lender should be more difficult than getting a loan through an existing credit relationship. On the other hand, the firms who had relationships with Lehman were much larger and obtained financing more frequently, so losing access to one source of credit would be likely to push them to seek out new ones. The equilibrium outcome for nonmanufacturers will depend on the relative strength of these two effects. In practice, the latter effect seems to dominate.

The baseline results for the probability of receiving a new real investment loan are shown in the first column of the top rows of Table 3. The first row, which corresponds to ρ in Equation 1, shows that nonmanufacturing firms that had open lines of credit with Lehman became about 8.5pp more likely (relative to the average firm without Lehman attachment) to obtain new loans following Lehman's collapse. This is a substantial portion of the roughly 20% unconditional annual average probability of obtaining one of these loans for these types of firms. The positive coefficient estimate is consistent with the idea that these firms relied extensively on financing and sought to find new sources after Lehman's collapse. The second row, corresponding to Ω in Equation 1, shows that the additional effect for a manufacturing firm of having an open line of credit with Lehman was about -5.4pp, leading to a total effect (3.1pp) about one-third as large as the effect for nonmanufacturing firms with Lehman exposure. Put another way, credit shocks appear to have caused firms in all sectors to go out and look for more credit, but manufacturers were less likely to obtain it.

The last three columns show a variety of alternative specifications that generate very similar coefficient estimates, which speaks to the robustness of the main results. The second column restricts the sample to the set of firms that were ever observed receiving a loan. This is an important check because it ensures that my results aren't being driven by some unobserved

15

factors that prevent certain types of firms from accessing syndicated loan markets entirely. The third column shows that my results do not depend on my choice of controls by excluding all firm-level characteristics. Finally, the fourth column includes only firms who were observed in the sample until at least 2016. This specification addresses directly the concern that my aggregate results are driven purely by firm exit: even conditional on surviving throughout the entire sample, manufacturing firms with Lehman attachment are less likely—and nonmanufacturing firms with Lehman attachment more likely—to receive new loans.

The middle and bottom sections of Table 3 show the effects for sales and employment. The baseline specification suggests that, for each additional Lehman facility, both employment and sales fall by roughly 6% for manufacturing firms while remaining virtually unchanged for non-manufacturing firms. These results are generally unchanged across specifications; although the exclusion of firm-level controls (column 3) attenuates the estimated sales effects, the estimated effects on employment approximately double. In Appendix D, I show that these results are robust to using alternate measures of Lehman exposure including using a dummy variable instead of the number of facilities, using only loans in which Lehman had a role beyond participant, or scaling the total amount of credit obtained through Lehman by a lagged measure of sales. Across all of these measures and specifications, I find that manufacturing firms exposed to Lehman's collapse became less likely to obtain credit and had lower sales and employment than their nonmanufacturing counterparts.

3.4   Aggregate Evidence of Credit Reallocation

The previous section provided causal evidence that credit supply shocks disproportionately affected manufacturing firms, both in terms of obtaining credit and real outcomes. Because the majority of firms did not have open lines of credit with Lehman Brothers at the time of its collapse, however, I show in Appendix D that credit reallocation from manufacturing to nonmanufacturing firms that occurred in the aftermath of the Great Recession was also visible on a broader scale. In Tables D.12 and D.13 I show reallocation occurred at the sectoral level, even for firms without Lehman attachment. Consistent with predictions from the relationship lending literature, Tables D.14, D.15, D.16, and D.18 show that this reallocation was driven by the extensive loan margin and loan maturity did not change. Finally, I show in D.18 that reallocation occurred *within* both manufacturing and nonmanufacturing as credit flowed to firms in higher-tech sectors such as computers and software, which provides evidence that the mechanism at the core of this paper is not specific to the transition from manufacturing to services.

While these exercises suggest that the credit reallocation channel generalized beyond the firms directly connected to Lehman at the time of its collapse, they are all restricted to the Great Recession. The next section shows that the link between credit and structural change can also be seen during other time periods.

4   Evidence from Interstate Banking Deregulation

This section supplements the results from Section 3 by using evidence from US interstate banking regulation (IBD) to provide evidence for a credit reallocation channel. The key feature of the mechanism described in Section 2.2 and developed more formally in Section 5 is that structural change will lead newly available credit to increasingly flow to newer and more valuable sectors even as fixed costs of adjustment prevent rapid changes in flows of credit out of established relationships. By allowing banks without existing relationships to enter a state and begin making loans, newly issued credit following IBD should thus flow disproportionately to nonmanufacturing firms. Consistent with this prediction, I find that IBD lead to persistent gains in a state's nonmanufacturing employment while having no effect on its manufacturing employment. I estimate that IBD led to a 0.2 percentage point decline in a state's manufacturing share, which is approximately the same magnitude of acceleration that was observed in 2008-2009.

4.1   Background

Due to the presence of extensive state-level regulations banks in the US have historically operated on a local scale. Up until the 1970s, banks were not permitted to open branches or purchase other banks outside of the state in which they were headquartered. This began to change in 1978, when Maine passed a law allowing out-of-state bank holding companies (BHCs) to acquire its banks. Other states soon followed suit and by the time the Interstate Banking and Branching Efficiency Act of 1994 had passed, effectively eliminating these state restrictions nationwide, every state other than Hawaii had already passed individual laws allowing interstate banking.[15] Effectively this allowed banks (or BHCs) from one state to start making loans in new states in which they did not have any prior existing relationships.

Starting with Jayaratne and Strahan (1996), an extensive literature has shown this creation of newly available credit has had positive impacts on aggregate real economic activity.[16] Allowing

---

[15]Kroszner and Strahan (2014) provide a detailed summary of US banking deregulation and discuss the literature analyzing its causes and consequences.

[16]While the existing literature on IBD has generally focused the US, Bertrand et al. (2007) find that banking

entry by out-of-state banks has boosted credit availability for entrepreneurs (Black and Strahan (2002)), increased innovation (Amore et al. (2013), Chava et al. (2013), and Cornaggia et al. (2015)), increased asset and activity shares for large and geographically diverse banks (Strahan (2003)), and led to real growth that was both faster and more stable (Morgan et al. (2004)) compared to states that did not allow deregulation. The hypothesis of this paper is that these benefits should accrue disproportionately to firms in sectors whose shares of activity are increasing due to structural change.

There are several papers that provide suggestive evidence in support of this hypothesis. Herrera et al. (2014) show that IBD led to increases in empirical measures of inter-firm credit reallocation. Acharya et al. (2011) find that relaxing interstate banking restrictions led to a more diverse activity composition across sectors. Bai et al. (2018) show that IBD led to relative growth in employment and capital for more productive firms. While their analysis is restricted to manufacturing firms, they point out that the existence of banking relationships means that younger firms should be more likely to borrow from new banks entering a market, which aligns closely with the mechanism described in this paper. The only other paper I am aware of that directly considers the sectoral employment implications of IBD is Mian et al. (2020). They find that employment gains were concentrated in nontradable sectors and that tradable sectors showed virtually no employment effects. Their measures of tradable/nontradable industries generally map closely to manufacturing/services, so these results are consistent with my findings.

4.2 Results

The main source of data used in this section is the Quarterly Census of Employment and Wages (QCEW). These data include information on employment and wages in each state broken down by sector at a quarterly frequency and dating back to 1975. Because they are based on comprehensive unemployment insurance records, the QCEW data will include small and nonpublic firms that have limited access to capital markets and are thus most likely to benefit from expansion of local bank operations. Data on the timing of interstate banking deregulation come from Strahan (2003). Because these observations are only available at the annual level, I take the annual average of the QCEW data and merge these with the deregulation dates to create a balanced panel of 50 states plus Washington DC. A detailed description of the data can be found in Appendix C.

My baseline regression specification is a standard difference-in-differences approach and estimates the following equation:

---

regulation in France in the 1980s led to increases in job and asset reallocation for sectors which exhibit greater external financial dependence.

$$share_t^i = \alpha^i + \delta_t + \gamma share_{t-1}^i + \beta dereg_t^i + \epsilon_t^i \qquad (2)$$

The dependent variable is $share_t^i$, which represents the ratio of manufacturing employment to total private employment in state i and year t. Firm and year fixed effects are represented by α^i and δ_t, respectively. $dereg_t^i$ is an indicator variable set to zero for all years prior to the year each state passed legislation permitting interstate banking and one for the year of deregulation and all years after. The dates are taken directly from Strahan (2003) and are shown in Appendix C.[17] Because there is no variation in state treatment status after 1996, I follow Strahan (2003) and use data from 1976-1996.[18] The coefficient of interest will be β, which captures the average difference in the manufacturing employment share for states that have implemented deregulation relative to states that have not.

The estimates of Equation 2 are shown in the top row of Table 4. The baseline result shown in column 1 suggests that allowing out-of-state banks to enter leads to a roughly 0.25 percentage point decline in a state's manufacturing employment share relative to a state that has not yet implemented IBD. The other columns show that alternative specifications including additional controls, allowing for state-specific linear time trends in addition to year fixed effects, or extending the sample through 2018 to allow for more precise estimation of state fixed effects lead to estimates that are all in the neighborhood of 0.2pp. This effect represents about 2.6% of the 7.6pp decline in the manufacturing share for the US as a whole that occurred during the period of deregulation (1978-1996), or about half of the average annual decline. For comparison, the average annual decline in the manufacturing employment share was about 0.35pp per year from 2002-2007, but accelerated to 0.55pp in 2008-2009, resulting in a 0.2pp difference. This suggests that my estimates for the effects of IBD align almost exactly with the acceleration in the decline of the manufacturing share observed during the Great Recession.

While a state's manufacturing employment share will decline as long as nonmanufacturing employment grows more (or declines less) than manufacturing employment, the mechanism described in Section 2.2 makes a clear prediction on the *composition* of this change: expansion of

---

[17]The one exception is Maine, which first passed legislation in 1978 allowing entry of out-of-state BHCs but only from states which had reciprocal arrangements. Because no state did so until New York in 1982, I use 1982 as the deregulation date for Maine. This is also the first year considered in the analysis of Mian et al. (2020). See Amel (1993) for more details regarding the nature and timing of these regulations.

[18]I also follow standard practice in the literature by excluding Delaware and South Dakota given their unique usury laws that gave them an outsized role in the development of the credit card industry, leaving a panel of 48 states plus Washington DC.

credit should benefit nonmanufacturing firms without having any direct effect on manufacturing firms. This prediction can be directly tested by replacing the manufacturing employment share in Equation 2 with the log levels of each type of employment. The results of this exercise are shown in the bottom two rows of Table 4. I find that IBD leads to a statistically significant increase of around 1.5-2% in a state's nonmanufacturing employment. In contrast, the effect on manufacturing employment is very close to zero and statistically insignificant. These findings are consistent with those in Mian et al. (2020), who find that nontradable employment, which is predominantly in the service sector, benefits most from IBD.

Interpreting these results as causal relies on the assumption that deregulation was unrelated to current and expected economic conditions. The extensive literature using variation in IBD as a proxy for credit supply shocks has found this assumption to be a reasonable one. Kroszner and Strahan (2014) provide comprehensive evidence that the deregulation dates were not correlated with state-level business cycle conditions and that they were not passed in anticipation of improved future growth prospects. I find evidence that these results extend to the *composition* of an economy as well. In supplementary materials, I show that the manufacturing share pre-trends prior to deregulation are parallel before diverging once IBD is implemented (appendix Figure E.1). I also use event study regressions to show that deregulation does not appear to be correlated with employment shares or levels in the years leading up to its passage but has a substantial effect in subsequent years (appendix Figures E.2 and E.3).

In summary, this section uses variation in the timing of interstate banking deregulation to study how the composition of a state's economy changes in response to an expansion in credit supply. I find that the influx of new credit that accompanied a state's deregulation led to a decline in that state's manufacturing employment share driven entirely by an increase in nonmanufacturing employment. In the next section, I build a model that can explain why both the contraction of credit caused by the collapse of Lehman Brothers and the expansion of credit caused by IBD both had the same effect on the manufacturing share.

5   Model

Section 3 established that credit was reallocated from manufacturing firms to nonmanufacturing firms during and after the Great Recession, and that once credit was "lost" it didn't come back to that sector. Section 4 showed that the creation of new and unmatched credit following deregulation of a state's banking industry led to gains in nonmanufacturing employment but had no effect on manufacturing employment. In this section, I build on the intuition developed in Section 2.2

to construct a model that can more parsimoniously account for both of these findings.

Three key features of the model allow it to accomplish this goal. The first is CES preferences calibrated as in Ngai and Pissarides (2007), which lead to a decline in manufacturing's share of economic activity as its relative productivity increases. The second is fixed costs of credit reallocation, which lead to infrequent and lumpy adjustment on the part of banks. The third is the destruction of firm-bank matches that occurs during a recession, which reduces the opportunity cost of inaction and thus makes credit reallocation more likely. The model is able to match the empirical fact that half of the decline in manufacturing employment has occurred during recessions and suggests policies which prevent reallocation can have substantial opportunity costs.

5.1 Firms, Banks, and Production

The economy consists of two sectors: manufacturing (M) and nonmanufacturing (N). There are a continuum of firms in each sector indexed according to their productivity z_t, which is fully observable and distributed according to a cumulative distribution function $F_t^i(\cdot)$ that is allowed to vary across both sectors and time. Each firms' ranking within the distribution is invariant over time, meaning that the median firm in each sector will always be the same firm in each period even as its productivity changes over time along with the rest of the distribution. Firms must obtain credit through a match with a bank in order to produce. If firm j obtains credit at time t, it will produce a fixed quantity z_t^j; otherwise, it will produce zero. Total output in each sector Y_t^i can be aggregated by adding up the output of each firm weighted by its measure within the economy:

$$Y_t^i = \int_j \left[\mathbb{1}_j^{Credit} \right] z_t^j dF_t^i(z_t^j). \tag{3}$$

There is a fixed supply—normalized to one unit—of credit available that is provided through a bank. Because productivity is perfectly observable, credit will always be allocated "from the top down", meaning that no firm will be matched with a bank while a more productive firm in its sector remains unfunded. This implies a cutoff productivity z^{i*} for each sector so that total output in each sector will be:

$$Y_t^i = \int_{z_t^{i*}}^{\infty} \tilde{z} dF_t^i(\tilde{z}) \tag{4}$$

Credit reallocation is subject to a fixed cost c. If a bank chooses not to pay the fixed cost at time t, the measure of firms receiving credit in each sector remains unchanged. This fixed cost

can be thought of as an information asymmetry between firms and banks that forces banks to exert time and effort to learn about borrowers when establishing new lending relationships. This modeling choice is consistent with my empirical results shown in Appendix D, in which I find that credit reallocation is driven by a change in the probabilities of obtaining a loan (the extensive margin) rather than changes in the size of the loan conditional on obtaining it (the intensive margin). The total quantity of credit allocated to each sector can be written as one minus the CDF evaluated at the cutoff productivity level:

$$\sum_i \alpha_t^i = 1, \text{ where } \alpha_t^i = \left(1 - F_t^i(z_t^{i*})\right). \tag{5}$$

Here α_t^i can be equivalently thought of as each sector's credit share or, assuming each firm consists of a single employee, the labor share. Lowering (raising) the cutoff productivity level in one sector corresponds to shifting a larger (smaller) quantity of credit to that sector. Because the total amount of credit is fixed, this simplifies the problem to one of choosing the share of total credit going to the manufacturing sector, which I define for simplicity as α_t. Because the productivity distributions are changing over time, output in each sector can vary from one period to the next even if credit is not reallocated due to changes in θ_t^i.

Production in the model is subject to business cycle fluctuations, which I model as exogenous separations between firm and bank matches. This increase in separations could be thought of as coming from the collapse of a bank, as was the case for Lehman Brothers during the financial crisis, or from a firm going out of business. The latter is a more regular occurrence; while there are relatively few examples of large bank failures in the US, the countercyclicality of firm exit rates is a robust and well-known feature of the data.[19]

I define δ_t as the share of firms which become exogenously separated from their match with the bank. Consistent with my findings regarding the characteristics of firms with attachment to Lehman at the time of its collapse, these separations occur uniformly across sectors and firm types. Because they destroy contemporaneous relationships, they lower output in both sectors during the periods in which they occur. Once separated, all destroyed matches remain unproductive until reallocation occurs. For simplicity, I model these recessions as being completely unexpected.[20] Taking into account the possibility of recessions, output in each sector can be written:

---

[19]See for example Davis et al. (1998), Campbell (1998), and Lee and Mukoyama (2015).

[20]Extending the model to allow the bank to believe that there will be a non-zero probability of a recession leaves the results virtually unchanged. In fact, structural change will become even more concentrated during recessions if the dates of all future recessions are perfectly known, as the planner will delay making changes until recessions to minimize the opportunity cost of reallocation.

$$Y_t^i = (1 - \delta_t) \int_{z_t^{i*}}^{\infty} \tilde{z} dF_t^i(\tilde{z}). \tag{6}$$

5.2 Planner's Problem

Households in the economy consume a composite final good Y_t that is a CES aggregate of manufactured (Y_t^M) and nonmanufactured (Y_t^N) inputs as in Ngai and Pissarides (2007):

$$Y_t = \left[\omega \left(Y_t^M \right)^{\frac{\epsilon-1}{\epsilon}} + (1 - \omega) \left(Y_t^N \right)^{\frac{\epsilon-1}{\epsilon}} \right]^{\frac{\epsilon}{\epsilon-1}}. \tag{7}$$

The two key parameters for this utility specification are the the relative weights on each type of consumption ω and the elasticity of substitution ϵ. Choosing a value of $\epsilon < 1$ will lead to manufacturing's share of value added declining as the relative productivity of the manufacturing sector increases. I follow Ngai and Pissarides (2007) and consider the solution to a planner's problem. The planner will maximize total utility subject to the production function and credit limit. Reallocating credit, which is represented by changing the value of α^i from one period to the next, incurs a fixed cost of c. I assume that households have log utility over total consumption Y_t, which will be a function of the shares of credit allocated to each sector $(\alpha_t^M$ and $\alpha_t^N)$, productivity levels $(\theta_t^M$ and $\theta_t^N)$, and recessions (δ_t). The flow utility each period can be expressed:

$$u_t = \log(Y_t) - c \times \mathbb{1}_{\alpha_t \neq \alpha_{t-1}}. \tag{8}$$

The economy has a finite horizon of N periods.[21] For simplicity I treat nonmanufacturing productivity as fixed $(\theta^N = 1)$ and express the model purely in terms of the relative productivity of the manufacturing sector, which I call θ_t. The planner's value function $V(\cdot)$ can be written recursively for $t \in \{0, ..., N\}$:

$$V(\alpha_{t-1}, \theta_t, \delta_t) = \max \left\{ V^{Adjust}, V^{NoAdjust} \right\}, \tag{9}$$

subject to equations 5 and 6, where the value of changing the credit share is:

$$V^{Adjust}(\alpha_{t-1}, \theta_t, \delta_t) = \max_{\alpha_t \in [0,1]} \left\{ \log \left(\left[\omega \left(Y_t^M(\alpha_t, \theta_t, \delta_t) \right)^{\frac{\epsilon-1}{\epsilon}} + (1 - \omega) \left(Y_t^N(\alpha_t, \theta_t, \delta_t) \right)^{\frac{\epsilon-1}{\epsilon}} \right]^{\frac{\epsilon}{\epsilon-1}} \right) \right.$$
$$\left. - c + \beta V(\alpha_t, \theta_{t+1}, \delta_{t+1}) \right\}, \tag{10}$$

---

[21]This assumption is not necessary for any of the main results and is made only for clarity of exposition.

and the value of maintaining the credit share at its previous level is:

$$V^{NoAdjust}(\alpha_{t-1}, \theta_t, \delta_t) = \left\{ \log\left(\left[\omega \left(Y_t^M(\alpha_{t-1}, \theta_t, \delta_t) \right)^{\frac{\epsilon-1}{\epsilon}} + (1-\omega) \left(Y_t^N(\alpha_{t-1}, \theta_t, \delta_t) \right)^{\frac{\epsilon-1}{\epsilon}} \right]^{\frac{\epsilon}{\epsilon-1}} \right) \right. \tag{11}$$
$$\left. + \beta V(\alpha_{t-1}, \theta_{t+1}, \delta_{t+1}) \right\}.$$

The key tradeoff in the model arises because the relative productivity of the manufacturing sector θ_t is growing. As a result, the same choice of credit share in manufacturing α_t will result in more manufacturing output over time. As in Ngai and Pissarides (2007), setting the elasticity of the CES aggregator $\epsilon < 1$ implies that the marginal value of providing credit to manufacturing firms will decrease as its productivity rises.[22] Choosing not to reallocate in a given period means the planner does not have to pay the fixed cost, but the value of having credit attached to the manufacturing sector will only decline over time as it becomes more and more productive.

Figure 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.

5.3 Simulation

The parameter values are summarized in Table 5. The discount factor β is set at 0.95. The choices of ϵ and the range of values of θ will determine the scope and speed of structural change

---

[22]In a decentralized equilibrium, this would manifest as a fall in the price of manufactured goods. This phenomenon is known as "Baumol's Cost Disease" and dates back to Baumol and Bowen (1965) and Baumol (1967); a more recent discussion can be found in Nordhaus (2008).

in the model. I choose $\epsilon = \frac{1}{3}$ and increase θ from 1.7 at the beginning of the simulation to 4.3 at the end. This implies that the relative productivity of the manufacturing sector in the model grew by a factor of 2.53 over the course of the simulation, which is similar to the actual figure of 2.20 observed in the data from 1960-2018 (see Figure F.1 of the appendix). This leads to a decline in the manufacturing share of credit from 29.1% to 8.3% over the course of the 60-period simulation, which matches the long-run patterns of structural change in Figure 1.[23] The timing of structural change will depend on the the fixed adjustment cost c and the frequency of recessions. I include 8 recessions, corresponding to the number observed in the data since 1960, and set the share of separations to be 1%. Together with a value of $c = 0.0008$, this generates an average decline during recessions of 1.33pp, which almost exactly matches the value of 1.36pp observed in the data.[24]

In the absence of fixed costs the composition of economy will adjust smoothly in response to increasing manufacturing productivity. This is shown in Figure F.2 of the appendix and occurs regardless of whether the model includes recessions or not. The addition of fixed costs of establishing new relationships, however, makes recessions opportune times to reallocate credit. Following the onset of a recession, bank resources which were tied to now-separated firms will become idle and unproductive. If the bank does not reallocate credit, these resources will remain useless until the bank pays the fixed cost and changes its portfolio composition. If the bank chooses to reallocate its financial resources during the recession, it cannot offset the immediate drop in production, but it can ensure that the effects of the recession do not persist into future periods. This leads to a strongly procyclical value of inaction for the bank and is the key mechanism through which business cycles affect reallocation in the model.[25]

These results are illustrated in Figure 4. The dotted orange line corresponds to the optimal credit share in the absence of adjustment costs. The blue line represents the optimal credit allocation in the presence of adjustment costs. Recessions are shown as shaded gray areas. The red dashed line, as in Figure 1, represents the cumulative change in the manufacturing share outside of recessions. Recessions in the model account for 48.5% of the total change in the

---

[23]As noted previously, the credit share in the model will be equivalent to the labor share.

[24]While the model only considers one-period recessions, in reality recessions vary in length. To generate a comparable annualized statistic in the data I calculate the average change in the manufacturing employment share during quarters classified as recessions and multiply by 4.

[25]The exogenous influx of new credit, which I analyzed empirically using IBD in Section 4, will have the same effect on the *composition* of the economy as a recession in this model. Newly created and unmatched credit will be allocated to its most productive use; relative to the existing composition of firms receiving funding, the lack of existing relationships will mean this will consist of disproportionately nonmanufacturing firms. Because it expands the number of available matches, however, it will lead to an increase in the *level* of total output. Model illustrations comparing the effects of recessions and credit expansions are available upon request.

manufacturing share, which is very close to the 50.0% observed in the data.

This model, while simple, is able to match the concentration of reallocation during recessions even when the manufacturing sector does not display excess sensitivity to the business cycle.[26] The key inputs—a long-run decline in the role of manufacturing, fixed costs of establishing new financial relationships, and countercyclical separation rates—are all well-documented features of the data, and the results are consistent with my empirical findings in Sections 3 and 4. This model helps shed light on the question of whether the reallocation that occurs during a crisis is efficient or not. Because of the presence of fixed costs, two things can be simultaneously true of an existing bank-firm match: 1) it would be inefficient to sever the relationship, but 2) if the relationship were to be separated for some reason, it will not necessarily be optimal to re-establish it. The next section considers a more formal policy experiment to quantify this intuition.

5.4   Policy Implications

Policymakers often find themselves tempted to intervene on behalf of entire industries. A recent example is the Automotive Industry Financing Program (AIFP). The goal of this program was explicitly to stabilize the auto industry as a whole; in his President-Elect speech in November 2008, Barack Obama said: "We can't allow the auto industries to simply vanish. We've got to make sure that it is there and that the workers and suppliers and the businesses that rely on the auto industry stay in business." This policy ultimately led to \$80.7bn in financing provided to Chrysler and General Motors beginning in December 2008.[27]

This program concluded in December 2014 with the government recovering a total of \$70.5bn, a net loss of \$10.2bn that represented 12.7% of the original outlay.[28] In addition, as noted by Goolsbee and Krueger (2015), these programs saved jobs, stabilized supplier networks, avoided costly restructuring, protected the benefits of union workers, and avoided further roiling financial markets. My model is unable to speak to these potential benefits, the worker-level implications

---

[26]In the baseline calibration, recessions destroy the same share of matches across both sectors and thus do not mechanically change the composition of output relative to the pre-recession period. Because recessions induce firms to pay fixed reallocation costs regardless of which sectors are destroyed, my main results are similar if banking relationships with service firms are broken at much higher rates during recessions. Even in the extreme case in which *only* service relationships are severed, these periods still account for roughly 38% of the total decline in the manufacturing share.

[27]This included financing provided to Ally Financial, a financial services firm that started as a financial subsidiary of GM in 1919. Ally was spun off into a separate company in 2006 but maintained close financial ties with GM and was almost solely responsible for providing GM dealerships with the credit necessary to purchase their inventory. See Congressional Oversight Panel (2010) for more details.

[28]These numbers are summarized in US Department of the Treasury (2015) and covered in more detail in Office of the Special Inspector General for the Troubled Asset Relief Program (2014).

of which have been explored in work such as Hyman (2018) and Autor et al. (2014). What it can do, however, is highlight and quantify the substantial opportunity cost arising from these programs. If the government were willing to provide financing, it is not clear that the automotive industry was the most productive source for these funds given that the industry was in the midst of a long-run decline.[29]

I consider the effects of such a policy implemented during the last recession observed in the model (corresponding to the timing of the Great Depression). The model credit share immediately prior to this recession was 11.4%. During the recession, the level falls to 9.4%, at which point it remains for 6 additional periods. I consider a policy which fixes the credit share at its pre-recession level for these six periods (corresponding to the six years in which the AIFP facilities were active), after which point the policy expires. The effects are depicted in Figure 5. The solid vertical black lines represent the periods in which credit reallocation is prevented. The purple line represents the path of the credit share under this counterfactual restriction. As soon as the policy ends, the manufacturing share immediately jumps to the planner's allocation.

This policy would be trivially inefficient given that it deviates from the planner's solution.[30] Furthermore, there is no channel in the model through which a planner could ever benefit from such policy. Nonetheless, the model is useful for highlighting inter-industry misallocation as a novel cost of such policies and showing it is quantitatively substantial relative to the program's accounting losses. Over the six years that the policy is in place, the cumulative output loss due to misallocation is approximately 78% of the initial outlay.[31] In the case of the AIFP, this would represent $63bn, more than six times the program's losses due to non-repayment. Furthermore, the credit share immediately adjusts to its efficient level as soon as the policy expires, suggesting that policy-induced allocations will only last as long as the policies themselves. Ultimately, such credit policies can lead to temporary distortions without having any impact on long-run allocations.

---

[29]Even before the Great Recession, motor vehicle manufacturing employment had fallen by more than 38% between 2000 and 2007 while total nonfarm payrolls rose by almost 6% during this time.

[30]The model assumes that all credit flowing to destroyed matches during recessions will be available immediately for use the following period. This is a simplifying assumption that does not affect the model's ability to match my empirical results. It does, however, significantly change the scope for interventions in the model which either prevent credit from being destroyed in the first place or allow it to be rebuilt to its pre-recession level more quickly (such as bank bailouts or other government credit programs). Because credit has no value if is not deployed to a firm, in this setting these types of policy interventions could significantly improve welfare even if they allocate credit to the worst firms. I abstract from these *level* effects and instead focus on the *compositional* effects of misallocation for a given policy intervention.

[31]Put another way, these losses represent 34% of the output losses caused by the recession itself.

6   Conclusion

The role of manufacturing in the US economy has declined steadily during the past several decades. Rather than being evenly distributed across time, these changes have been disproportionately concentrated during recessions. This paper proposes a novel mechanism to explain these findings: a credit reallocation channel. To document the empirical relevance of this channel, I use the collapse of Lehman Brothers as a natural experiment to analyze heterogeneity in the effects of exposure to credit shocks across sectors. I find that credit was reallocated away from manufacturing firms with Lehman attachment in the aftermath of the Great Recession and that this reallocation led to worse real outcomes.

To show that this phenomenon generalizes outside of the Great Recession, I use the staggered deregulation of US interstate banking in the 1980s as a natural experiment. This period of deregulation led to the creation of newly available credit available for lending by financial institutions that, up to that point, had no existing relationships in a given state. Consistent with my model's predictions I find that deregulation led to persistent increases in a state's nonmanufacturing employment but no lasting effect on its manufacturing employment, leading to a sustained decline in a state's manufacturing employment share.

After establishing empirical evidence for the credit reallocation channel, I showed that my key empirical findings arise naturally from a model with technology-driven structural change and fixed costs of credit reallocation. In the model, resources move gradually from manufacturing to nonmanufacturing sectors over time. Rather than occurring evenly, fixed costs lead to few large adjustments even when productivity changes are smooth and gradual. By breaking existing relationships and thus reducing the value of inaction, recessions lower the opportunity cost of reallocation and allow the model to match the patterns observed in the data. These findings have significant implications for policymakers, who found themselves tempted to come to the aid of entire industries in the aftermath of the financial crisis. My results suggest that re-establishing matches destroyed during the crisis is not necessarily efficient, even if such allocations were efficient at the time, due to the presence of fixed costs. Any attempts to temporarily prevent credit from being reallocated out of the manufacturing sector in this setting will reduce welfare in the short run and ultimately lead to the same allocations in the long run. The effectiveness of policy interventions in credit markets following recessions could be improved substantially by taking into account long-run industry trajectories when allocating credit rather than simply returning it to the firms which had access prior to the recession.

References

Abraham, Katharine G and Lawrence F Katz, "Cyclical unemployment: sectoral shifts or aggregate disturbances?," *Journal of political Economy*, 1986, *94* (3, Part 1), 507–522.

Acemoglu, Daron and Veronica Guerrieri, "Capital deepening and nonbalanced economic growth," *Journal of political Economy*, 2008, *116* (3), 467–498.

Acharya, Viral V, Jean Imbs, and Jason Sturgess, "Finance and efficiency: do bank branching regulations matter?," *Review of Finance*, 2011, *15* (1), 135–172.

Aghion, Philippe and Gilles Saint-Paul, "Virtues of bad times interaction between productivity growth and economic fluctuations," *Macroeconomic Dynamics*, 1998, *2* (3), 322–344.

Alder, Simon, Timo Boppart, and Andreas Müller, "A theory of structural change that can fit the data," 2019.

Alvarez-Cuadrado, Francisco and Markus Poschke, "Structural change out of agriculture: Labor push versus labor pull," *American Economic Journal: Macroeconomics*, 2011, *3* (3), 127–58.

Amel, Dean F, *State laws affecting the geographic expansion of commercial banks*, Board of Governors of the Federal Reserve System, 1993.

Amiti, Mary and David E Weinstein, "Exports and financial shocks," *The Quarterly Journal of Economics*, 2011, *126* (4), 1841–1877.

Amore, Mario Daniele, Cédric Schneider, and Alminas Žaldokas, "Credit supply and corporate innovation," *Journal of Financial Economics*, 2013, *109* (3), 835–855.

Autor, David H, David Dorn, Gordon H Hanson, and Jae Song, "Trade adjustment: Worker-level evidence," *The Quarterly Journal of Economics*, 2014, *129* (4), 1799–1860.

Bai, John, Daniel Carvalho, and Gordon M Phillips, "The impact of bank credit on labor reallocation and aggregate industry productivity," *The Journal of Finance*, 2018, *73* (6), 2787–2836.

Bárány, Zsófia L and Christian Siegel, "Job polarization and structural change," *American Economic Journal: Macroeconomics*, 2018, *10* (1), 57–89.

Barlevy, Gadi, "Credit market frictions and the allocation of resources over the business cycle," *Journal of monetary Economics*, 2003, *50* (8), 1795–1818.

Baumol, William J, "Macroeconomics of unbalanced growth: the anatomy of urban crisis," *The American economic review*, 1967, *57* (3), 415–426.

___ **and William G Bowen**, "On the performing arts: the anatomy of their economic problems," *The American economic review*, 1965, *55* (1/2), 495–502.

Bentolila, Samuel, Marcel Jansen, and Gabriel Jiménez, "When credit dries up: Job losses in the great recession," *Journal of the European Economic Association*, 2017, *16* (3), 650–695.

Berger, David, "Countercyclical Restructuring and Jobless Recoveries," *Mimeo*, 2018.

Bertrand, Marianne, Antoinette Schoar, and David Thesmar, "Banking deregulation and industry structure: Evidence from the French banking reforms of 1985," *The Journal of Finance*, 2007, *62* (2), 597–628.

Black, Sandra E and Philip E Strahan, "Entrepreneurship and bank credit availability," *The Journal of Finance*, 2002, *57* (6), 2807–2833.

Boot, Arnoud WA, "Relationship Banking: What Do We Know?," *Journal of Financial Intermediation*, 2000, *9* (1), 7–25.

Boppart, Timo, "Structural change and the Kaldor facts in a growth model with relative price effects and non-Gorman preferences," *Econometrica*, 2014, *82* (6), 2167–2196.

Borio, Claudio EV, Enisse Kharroubi, Christian Upper, and Fabrizio Zampolli, "Labour reallocation and productivity dynamics: financial causes, real consequences," 2016.

Buera, Francisco J and Joseph P Kaboski, "Can traditional theories of structural change fit the data?," *Journal of the European Economic Association*, 2009, *7* (2-3), 469–477.

Caballero, Ricardo and Mohamad L Hammour, "The Cleansing Effect of Recessions," *American Economic Review*, 1994, *84* (5), 1350–68.

Caballero, Ricardo J and Mohamad L Hammour, "On the timing and efficiency of creative destruction," *The Quarterly Journal of Economics*, 1996, *111* (3), 805–852.

_ **and** _ , "The cost of recessions revisited: A reverse-liquidationist view," *The Review of Economic Studies*, 2005, *72* (2), 313–341.

_ **, Takeo Hoshi, and Anil K Kashyap**, "Zombie lending and depressed restructuring in Japan," *American Economic Review*, 2008, *98* (5), 1943–77.

Campbell, Jeffrey R, "Entry, exit, embodied technology, and business cycles," *Review of economic dynamics*, 1998, *1* (2), 371–408.

Chava, Sudheer, Alexander Oettl, Ajay Subramanian, and Krishnamurthy V Subramanian, "Banking deregulation and innovation," *Journal of Financial economics*, 2013, *109* (3), 759–774.

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

Chodorow-Reich, Gabriel, "The Employment Effects of Credit Market Disruptions: Firm-level Evidence from the 2008–9 Financial Crisis," *The Quarterly Journal of Economics*, 2014, *129* (1), 1–59.

Cingano, Federico, Francesco Manaresi, and Enrico Sette, "Does credit crunch investment down? New evidence on the real effects of the bank-lending channel," *The Review of Financial Studies*, 2016, *29* (10), 2737–2773.

Comin, Diego A, Danial Lashkari, and Martí Mestieri, "Structural change with long-run income and price effects," Technical Report, National Bureau of Economic Research 2015.

Congressional Oversight Panel, "March Oversight Report: The Unique Treatment of GMAC Under the TARP," March 2010.

Contessi, Silvio, Riccardo DiCecio, and Johanna Francis, "Aggregate shocks and the two sides of credit reallocation," *Monash Business School manuscript*, 2015.

Cornaggia, Jess, Yifei Mao, Xuan Tian, and Brian Wolfe, "Does banking competition affect innovation?," *Journal of financial economics*, 2015, *115* (1), 189–209.

Davis, Steven J and John Haltiwanger, "Gross job creation, gross job destruction, and employment reallocation," *The Quarterly Journal of Economics*, 1992, *107* (3), 819–863.

_ , **John C Haltiwanger, Scott Schuh et al.**, "Job creation and destruction," *MIT Press Books*, 1998, *1.*

Dell'Ariccia, Giovanni and Pietro Garibaldi, "Gross credit flows," *The Review of Economic Studies*, 2005, *72* (3), 665–685.

Duarte, Margarida and Diego Restuccia, "The role of the structural transformation in aggregate productivity," *The Quarterly Journal of Economics*, 2010, *125* (1), 129–173.

Elsas, Ralf, "Empirical determinants of relationship lending," *Journal of Financial Intermediation*, 2005, *14* (1), 32–57.

Elyasiani, Elyas and Lawrence G Goldberg, "Relationship lending: a survey of the literature," *Journal of Economics and Business*, 2004, *56* (4), 315–330.

Foote, Christopher L, "Trend employment growth and the bunching of job creation and destruction," *The Quarterly Journal of Economics*, 1998, *113* (3), 809–834.

Gan, Jie, "Collateral, debt capacity, and corporate investment: Evidence from a natural experiment," *Journal of Financial Economics*, 2007, *85* (3), 709–734.

Goolsbee, Austan D and Alan B Krueger, "A retrospective look at rescuing and restructuring General Motors and Chrysler," *Journal of Economic Perspectives*, 2015, *29* (2), 3–24.

Groshen, Erica L and Simon Potter, "Has structural change contributed to a jobless recovery?," *Current Issues in Economics and Finance*, 2003, *9* (8).

Hachem, Kinda, "Relationship lending and the transmission of monetary policy," *Journal of Monetary Economics*, 2011, *58* (6-8), 590–600.

Hall, Robert E, "Reorganization," in "Carnegie-Rochester Conference Series on Public Policy," Vol. 52 Elsevier 2000, pp. 1–22.

Herrendorf, Berthold, Richard Rogerson, and Ákos Valentinyi, "Growth and structural transformation," in "Handbook of economic growth," Vol. 2, Elsevier, 2014, pp. 855–941.

Herrera, Ana Maria, Marek Kolar, and Raoul Minetti, "Credit reallocation," *Journal of Monetary Economics*, 2011, *58* (6-8), 551–563.

_ , _ , **and** _ , "Credit reallocation and the macroeconomy," *Michigan State University manuscript*, 2014.

Hershbein, Brad and Lisa B Kahn, "Do recessions accelerate routine-biased technological change? Evidence from vacancy postings," *American Economic Review*, 2018, *108* (7), 1737–72.

Huber, Kilian, "Disentangling the effects of a banking crisis: evidence from German firms and counties," *American Economic Review*, 2018, *108* (3), 868–98.

Hyman, Benjamin, "Can displaced labor be retrained? Evidence from quasi-random assignment to trade adjustment assistance," *Mimeo*, 2018.

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

Iyer, Rajkamal, José-Luis Peydró, Samuel da Rocha-Lopes, and Antoinette Schoar, "Interbank liquidity crunch and the firm credit crunch: Evidence from the 2007–2009 crisis," *The Review of Financial Studies*, 2013, *27* (1), 347–372.

Jaimovich, Nir and Henry E Siu, "The trend is the cycle: Job polarization and jobless recoveries," *Review of Economics and Statistics*, 2019, *Forthcoming.*

Jayaratne, Jith and Philip E Strahan, "The finance-growth nexus: Evidence from bank branch deregulation," *The Quarterly Journal of Economics*, 1996, *111* (3), 639–670.

Koenders, Kathryn, Richard Rogerson et al., "Organizational dynamics over the business cycle: a view on jobless recoveries," *Review-Federal Reserve Bank of Saint Louis*, 2005, *87* (4), 555.

Kongsamut, Piyabha, Sergio Rebelo, and Danyang Xie, "Beyond balanced growth," *The Review of Economic Studies*, 2001, *68* (4), 869–882.

Kroszner, Randall S and Philip E Strahan, "Regulation and deregulation of the US banking industry: Causes, consequences, and implications for the future," in "Economic Regulation and Its Reform: What Have We Learned?," University of Chicago Press, 2014, pp. 485–543.

Kuznets, Simon, "Quantitative aspects of the economic growth of nations: II. industrial distribution of national product and labor force," *Economic development and cultural change*, 1957, *5* (S4), 1–111.

Lee, Yoonsoo and Toshihiko Mukoyama, "Entry and exit of manufacturing plants over the business cycle," *European Economic Review*, 2015, *77*, 20–27.

Mian, Atif, Amir Sufi, and Emil Verner, "How does credit supply expansion affect the real economy? the productive capacity and household demand channels," *The Journal of Finance*, 2020, *75* (2), 949–994.

Morgan, Donald P, Bertrand Rime, and Philip E Strahan, "Bank integration and state business cycles," *The Quarterly Journal of Economics*, 2004, *119* (4), 1555–1584.

Ngai, L Rachel and Christopher A Pissarides, "Structural change in a multisector model of growth," *American economic review*, 2007, *97* (1), 429–443.

Nordhaus, William D, "Baumol's diseases: a macroeconomic perspective," *The BE Journal of Macroeconomics*, 2008, *8* (1).

Office of the Special Inspector General for the Troubled Asset Relief Program, "Quarterly Report To Congress," April 2014.

Paravisini, Daniel, Veronica Rappoport, Philipp Schnabl, and Daniel Wolfenzon, "Dissecting the effect of credit supply on trade: Evidence from matched credit-export data," *The Review of Economic Studies*, 2014, *82* (1), 333–359.

Peek, Joe and Eric S Rosengren, "The international transmission of financial shocks: The case of Japan," *The American Economic Review*, 1997, pp. 495–505.

_ **and** _ , "Collateral damage: Effects of the Japanese bank crisis on real activity in the United States," *American Economic Review*, 2000, *90* (1), 30–45.

_ **and** _ , "Unnatural selection: Perverse incentives and the misallocation of credit in Japan," *American Economic Review*, 2005, *95* (4), 1144–1166.

Ray, Debraj, "Uneven growth: A framework for research in development economics," *Journal of Economic Perspectives*, 2010, *24* (3), 45–60.

Schnabl, Philipp, "The international transmission of bank liquidity shocks: Evidence from an emerging market," *The Journal of Finance*, 2012, *67* (3), 897–932.

Schumpeter, Joseph, *The Theory of Economic Development*, Harvard University Press, 1934.

Storesletten, Kjetil, Bo Zhao, and Fabrizio Zilibotti, "Business Cycle during Structural Change: Arthur Lewis' Theory from a Neoclassical Perspective.," *Mimeo*, 2019.

Strahan, Philip E, "The real effects of US banking deregulation," *Review-Federal Reserve Bank Of Saint Louis*, 2003, *85* (4), 111–128.

US Department of the Treasury, "Auto Industry Program Overview," January 2015.

7 Figures and Tables

Manufacturing Employment Share
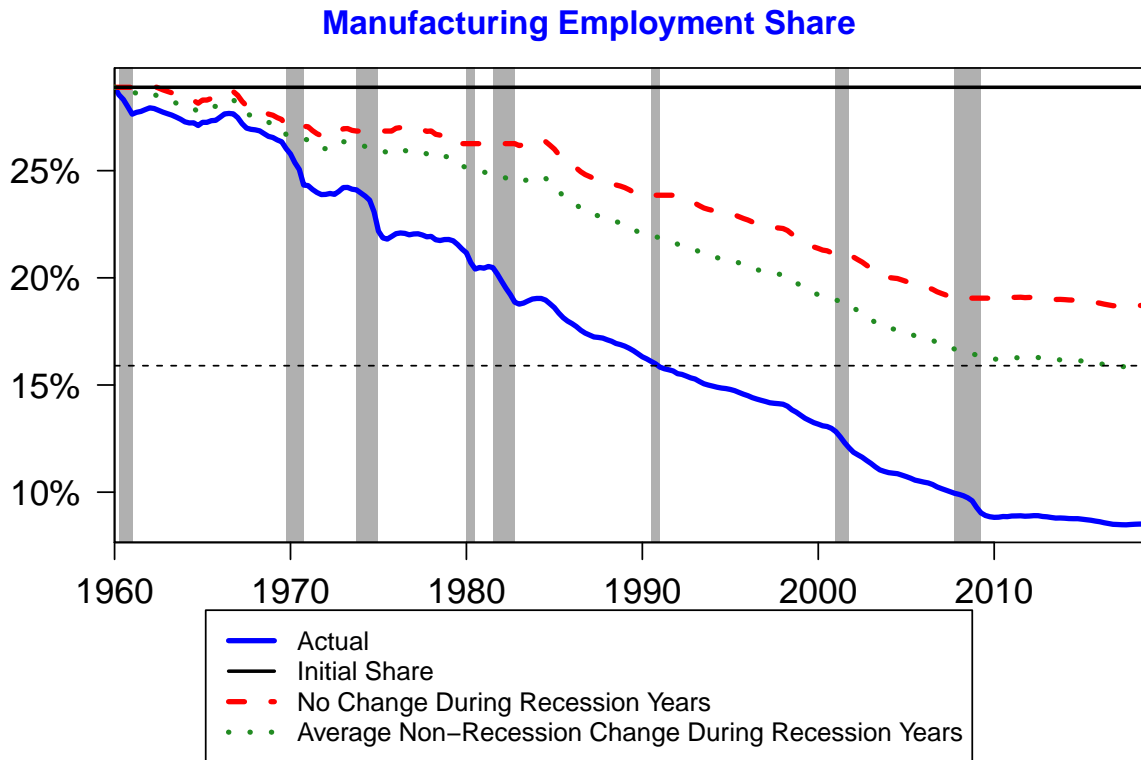
Figure 1: Change in US Manufacturing Employment Share, 1960-2018

Note: The solid blue line shows the share of payroll employment from the Current Establishment Survey 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 Labor Statistics.

Variable	Entire Sample	2000-2008	2009+
Number of loans	165,253	52,933	58,898
Revolving (%)	5.6%	2.5%	0.7%
"Real investment" (%)	54.3%	52.8%	64.3%
Average size ($mn)	$253	$238	$352
Median size ($mn)	$75	$75	$103
Average spread (bp)	264	242	323
Median spread (bp)	250	225	300
Median maturity (months)	60	48	60

Table 1: DealScan Summary Statistics for US Loans

Note: This table shows a variety of summary statistics calculated from DealScan. All included loans are denominated in US Dollars and issued to US companies. Statistics are split into three periods based on the reported start date of the loan: the entire sample (starting in 1987), 2000-2008, and 2009. "Revolving (%)" is the share of total loans classified as revolving lines of credit. "Real investment (%)" is the share of loans whose reported purpose was either "working capital" or "corporate purposes".

	Manufacturing		Nonmanufacturing	
Variable	Lehman	Non-Lehman	Lehman	Non-Lehman
Sales ($mil)	$13,541	$2,404	$9,025	$1,610
Assets ($mil)	$18,367	$2,750	$9,775	$1,783
Emp (thous)	35.2	7.4	50.3	8.5
Avg spread (bp)	160	202	180	225
# of firms	95	3,726	94	3,868
% with new loan	71.5	18.1	64.9	14.1

Table 2: Summary Statistics from 2004 for Firms Split by Lehman Exposure
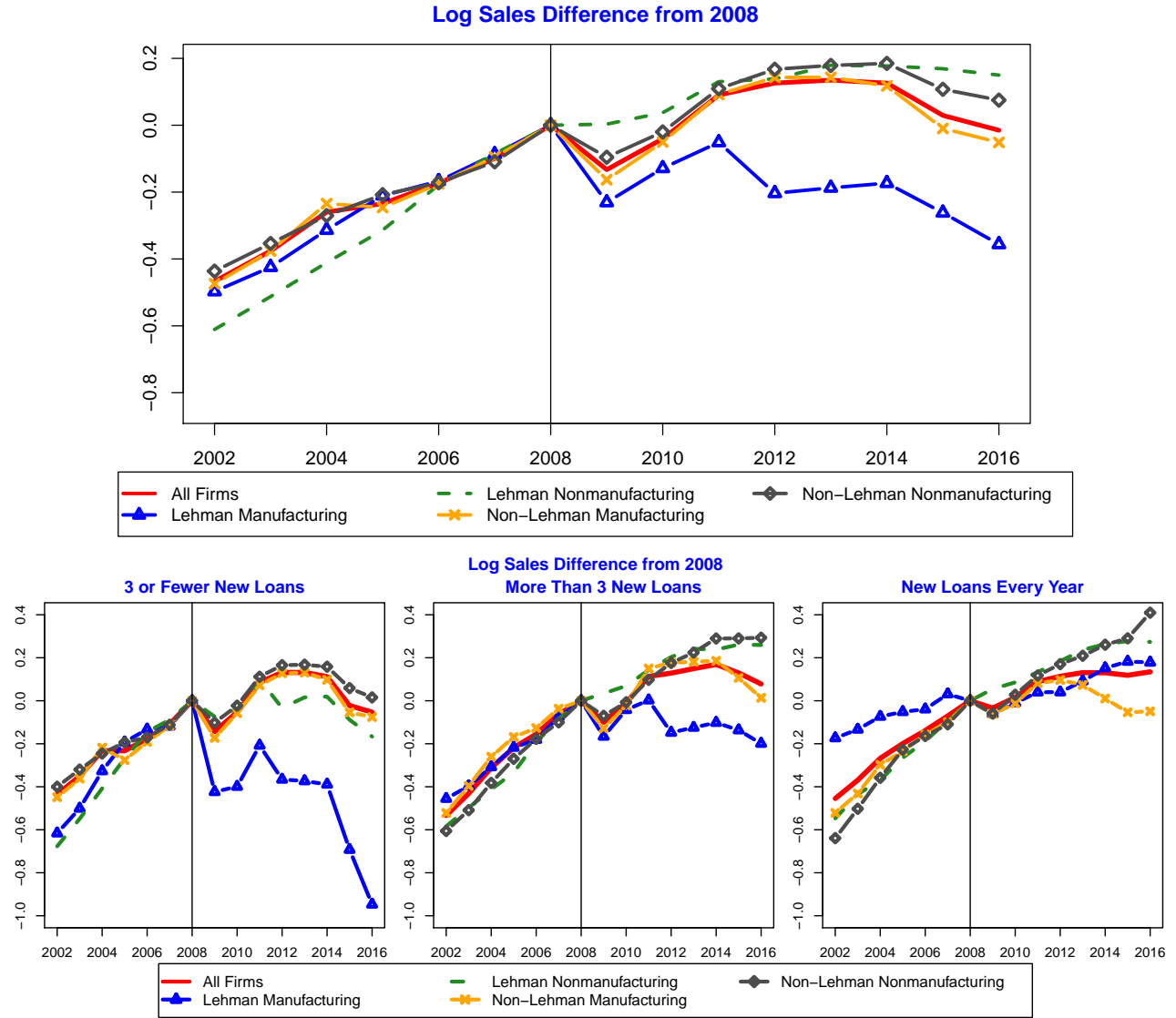
Figure 2: Aggregate Sales Growth Relative to 2008

Note: This figure shows aggregate sales splits based on a firm's industry, whether it had exposure to Lehman Brothers, and the number of loans that it went on to receive in 2009 and beyond. The top panel shows the results for all firms in my sample. The bottom left panel shows the sales outcomes split by firms who obtained three or fewer new loans from 2009-2016. The bottom middle panel shows splits by firms who received 3 or more new loans during the same period. The rightmost bottom panel, which is a subset of the firms in the middle panel, shows firms who received at least one new loan every year from 2009-2016. A firm is classified as having Lehman attachment if it had a revolving line of credit through a syndicate that included Lehman Brothers that 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.

	(1)	(2)	(3)	(4)
New Loan Probability				
$\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i$	0.0850***	0.0688***	0.0905***	0.0890***
	(0.0272)	(0.0243)	(0.0298)	(0.0300)
$\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{Mfg\}}$	-0.0541**	-0.0470**	-0.0611***	-0.0589***
	(0.0217)	(0.0213)	(0.0208)	(0.0170)
Sales				
$\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i$	0.00636	0.00438	0.0186	0.00438
	(0.00613)	(0.00542)	(0.0162)	(0.00758)
$\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{Mfg\}}$	-0.0635***	-0.0551***	-0.0129	-0.0786***
	(0.0123)	(0.0116)	(0.0366)	(0.0104)
Employment				
$\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i$	0.0145	0.0100	0.0437**	-0.00295
	(0.0106)	(0.0105)	(0.0210)	(0.0103)
$\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{Mfg\}}$	-0.0599***	-0.0590***	-0.109***	-0.0514***
	(0.0140)	(0.0155)	(0.0320)	(0.0163)
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 3: Effects of Lehman Exposure on New Loans, Sales, and Employment

Note: This table shows the results of estimating Equation 1 for new loans, sales, and employment. For the top section, the dependent variable is a dummy variable indicating whether a firm received at least one new "real investment" loan in a given year. In the middle and bottom sections, the dependent variables are log sales and log employment, respectively. $Lehman_i$ 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.

	(1)	(2)	(3)	(4)	(5)
Manufacturing employment share	-0.0025***	-0.0024***	-0.0021***	-0.0015**	-0.0020**
	(0.00065)	(0.00064)	(0.00065)	(0.00067)	(0.00067)
Log manufacturing employment	0.0013	0.0012	0.0057	0.0042	0.0062
	(0.0048)	(0.0029)	(0.0050)	(0.0038)	(0.0051)
Log nonmanufacturing employment	0.018***	0.016***	0.020***	0.018***	0.014***
	(0.0047)	(0.0050)	(0.0054)	(0.0051)	(0.0042)
Controls	N	Y	N	Y	N
State time trends	N	N	Y	Y	N
Data through 2018	N	N	N	N	Y
N	1,029	1,029	1,029	1,029	2,107

Standard errors clustered at the state level in parentheses

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

Table 4: Effect of IBD on Employment

Note: This table shows the results of estimating Equation 2. In the first row, the dependent variable is the share of manufacturing employment to total employment. In the second and third rows, the dependent variable is log employment in the manufacturing and nonmanufacturing sectors. Each set of coefficients comes from a separate regression. Standard errors are clustered at the state level. DE and SD are not included. The regressions use data from the QCEW and are at the state-year level from 1975-1996 for columns (1)-(4) and from 1975-2018 for column (5). In addition to one lag of state-level log employment, the specifications in columns (2) and (4) also include one year lags of state-level log wages as controls. Columns (3) and (4) include state-specific time trends in addition to year fixed effects.
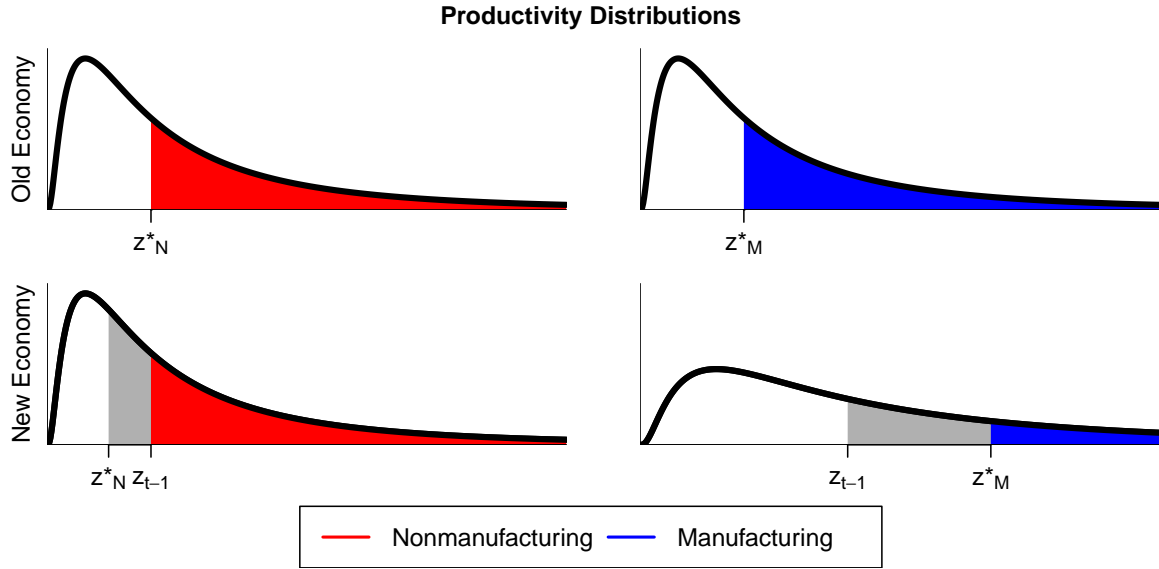
Productivity Distributions

Figure 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}.

Parameter	Value	Description
β	0.95	Discount factor
ω	0.5	Weight on manufactured good in utility function
ϵ	0.33	Elasticity of substitution in CES utility function
δ	0.01	Share of firm-bank matches destroyed during recessions
c	0.0008	Portfolio adjustment cost
θ	1.7 to 4.3	Range of values of manufacturing productivity

Table 5: Model Parameter Values
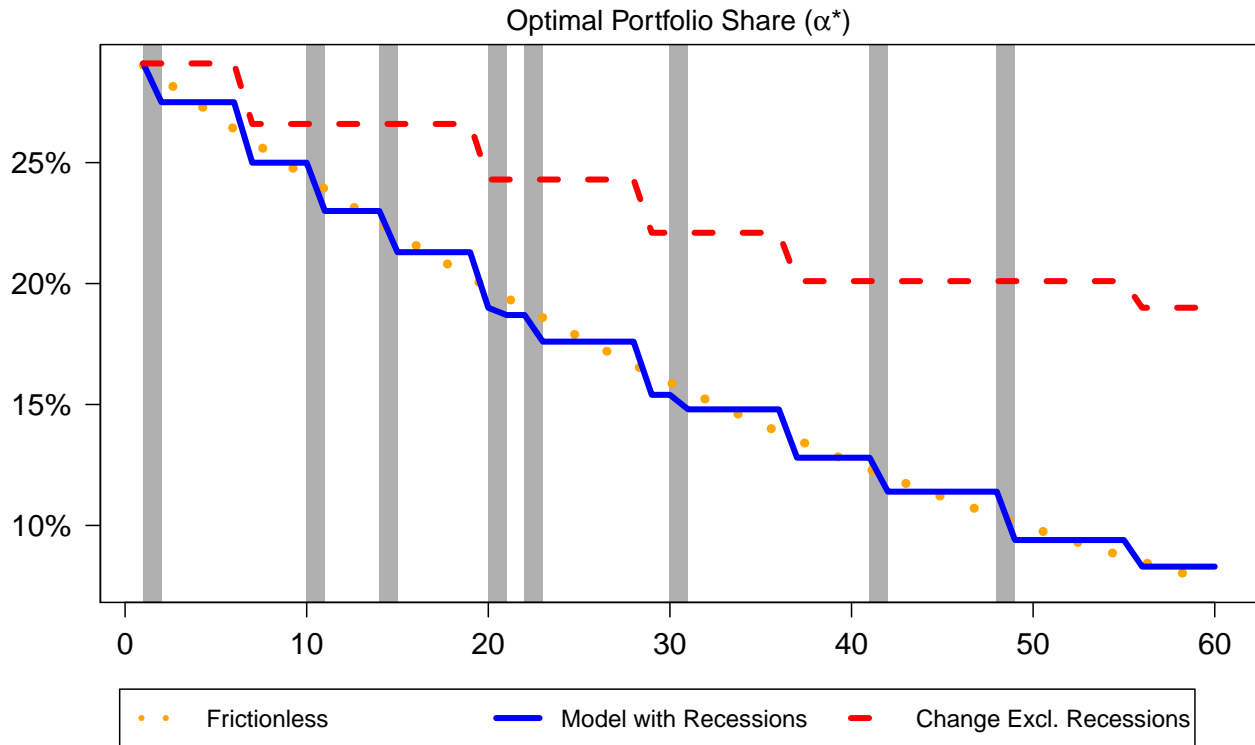
Figure 4: Model with Recessions

Note: The x-axis corresponds to time periods of the simulated model. The y-axis shows the share of credit allocated to manufacturing firms. The solid blue line represents the model simulation with adjustment costs and recessions (which are represented by the shaded gray areas). The dotted orange line represents the frictionless benchmark. The dashed red line represents the counterfactual change in the share after setting changes during recessions to zero (as in Figure 1). The parameter values are shown in Table 5.
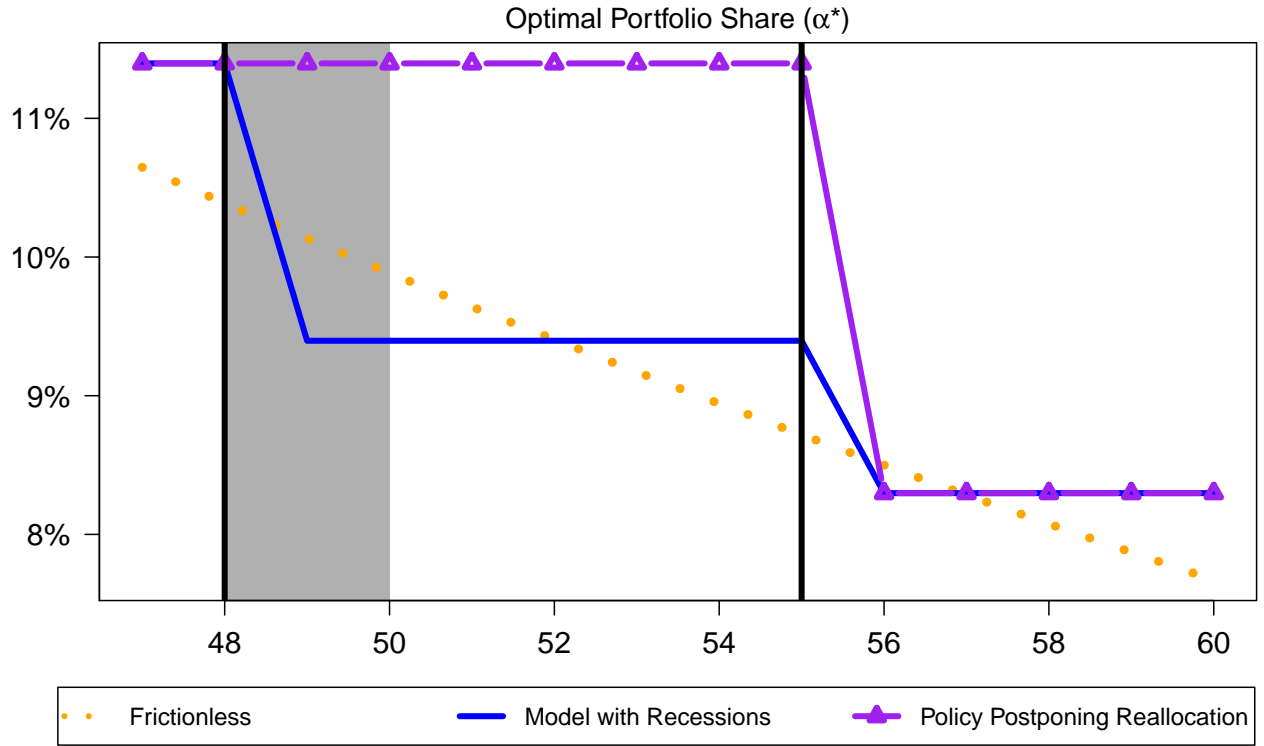
Figure 5: Effects of Policy Preventing Reallocation

Note: The x-axis corresponds to time periods of the simulated model. The y-axis shows the share of credit allocated to manufacturing firms. The solid blue line represents the model simulation in the presence of a recession which occurs at period 49 and is represented by the shaded gray area. The dotted orange line represents the frictionless benchmark. The vertical black lines correspond to the periods in which the economy is subject to the credit reallocation policy, which prevents credit from adjusting from its level prior to the recession. The purple line with triangles represents the path of credit under the policy. The parameter values are shown in Table 5.

Internet Appendix

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 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.
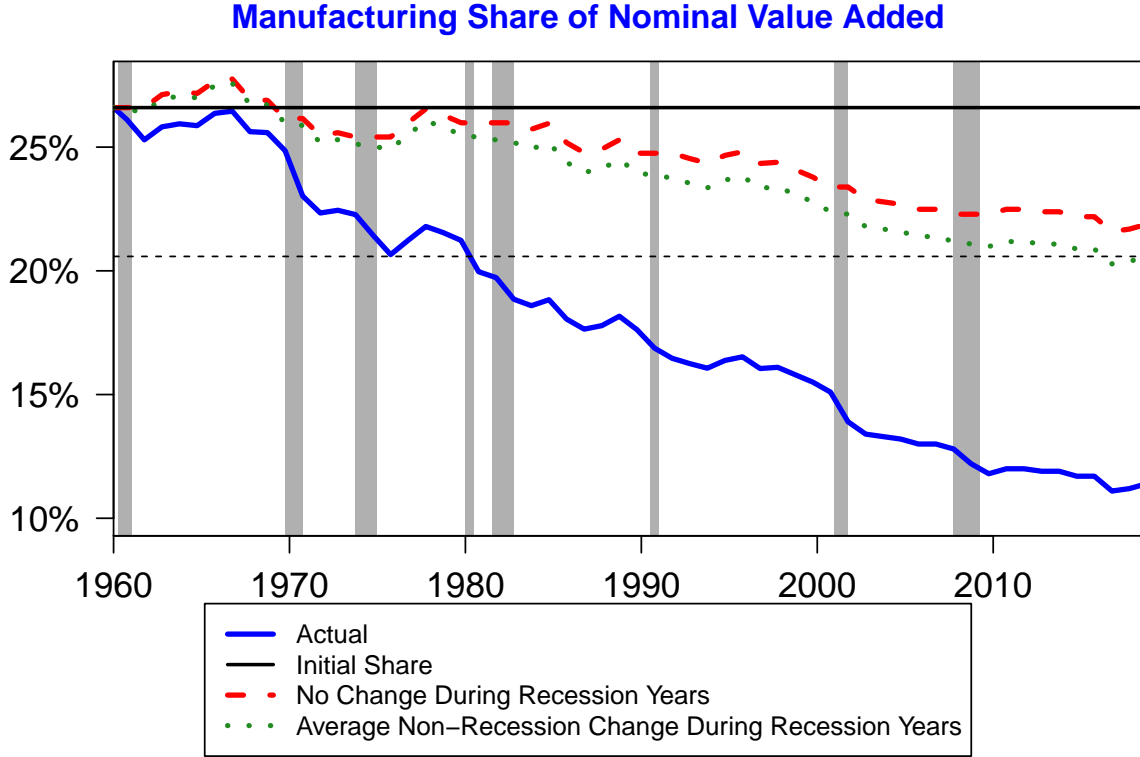
Manufacturing Share of Nominal Value Added

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.

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;

45

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.

it requires only that it the share of productive resources being allocated to the manufacturing sector declines over time.[32] 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

---

[32]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.

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.
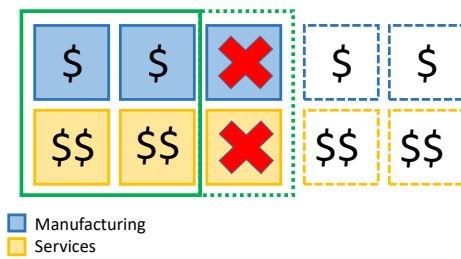
(a) Firms with banking relationships / Firms without banking relationships

Manufacturing
Services
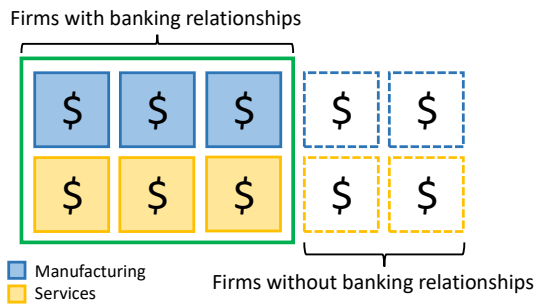
(b) Structural change makes service firms more valuable, but fixed costs mean that reallocation is not immediate

Manufacturing
Services
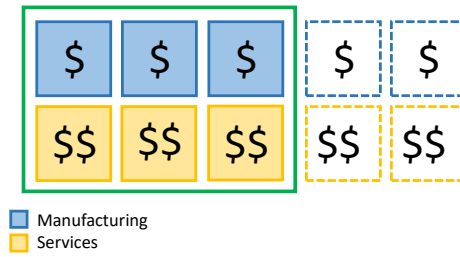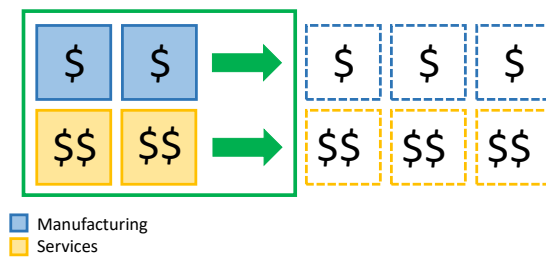
(c) Bank or firm failure will destroy relationships

Manufacturing
Services

(d) New bank credit becomes available as economy recovers

Manufacturing
Services

(e) More valuable service firms will be more likely to get credit

Manufacturing
Services
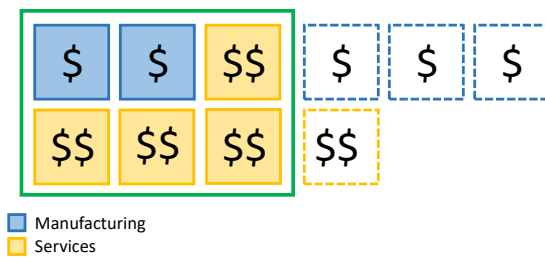
(f) **Prediction:** Bank failure disproportionately hurts manufacturing firms

Firms attached to affected bank:
Manufacturing employment=2, service employment=3

Firms without attachment to affected bank:
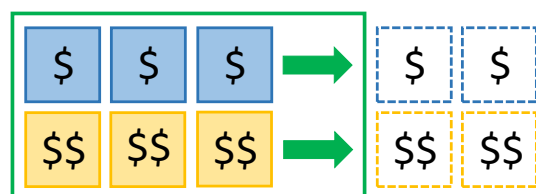Manufacturing employment=service employment=3
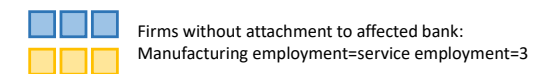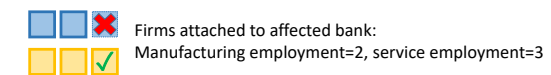
Manufacturing
Services

Natural experiment: Collapse of Lehman Brothers in 2008

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

48

Bank credit expands, allowing for creation of new matches

Manufacturing
Services

(a)

New matches will be with more valuable service sector firms

Manufacturing
Services

(b)

Prediction: Credit expansion disproportionately benefits service firms

Treated area: Manufacturing share = 3/8

Untreated area: Manufacturing share = 3/6

Manufacturing
Services

Natural experiment: US interstate banking deregulation in 1980s

(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).[33] 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

---

[33]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.[34] 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.[35] 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.

---

[34]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.

[35]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		Nonmanufacturing	
	Goldman	Non-Goldman	Goldman	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		Nonmanufacturing	
	JPM	Non-JPM	JPM	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).

	Manufacturing		Nonmanufacturing	
Variable	MS	Non-MS	MS	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).
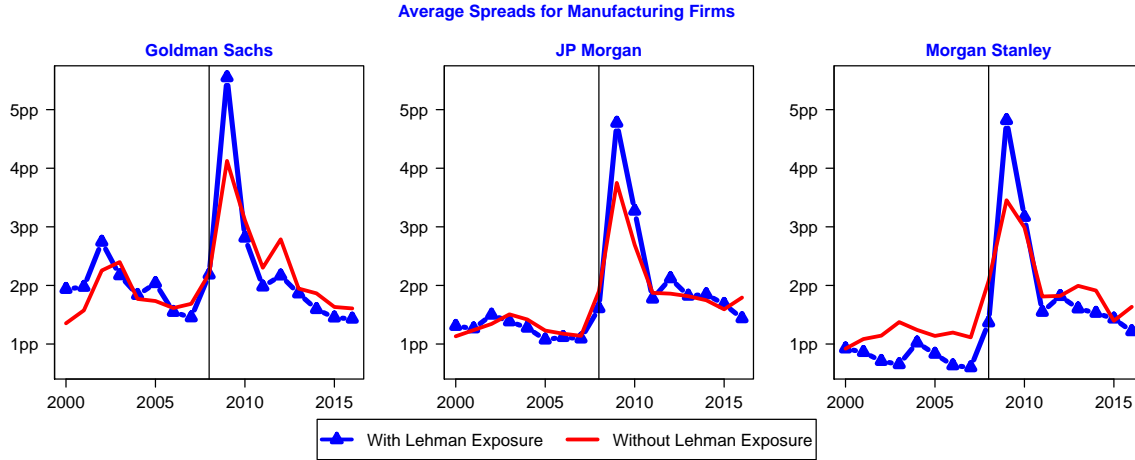

Figure D.1: Average interest rate splits by bank attachment

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.[36] 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.[37] 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

---

[36]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.

[37]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.

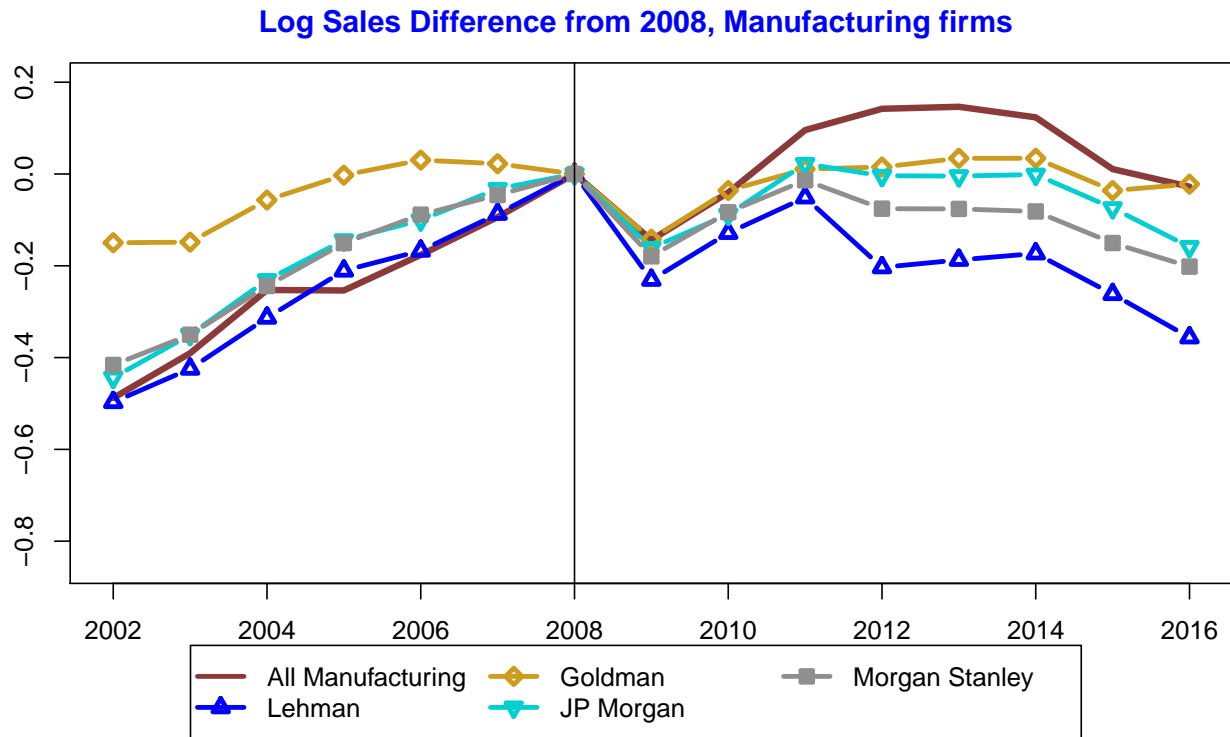**Log Sales Difference from 2008, Manufacturing firms**

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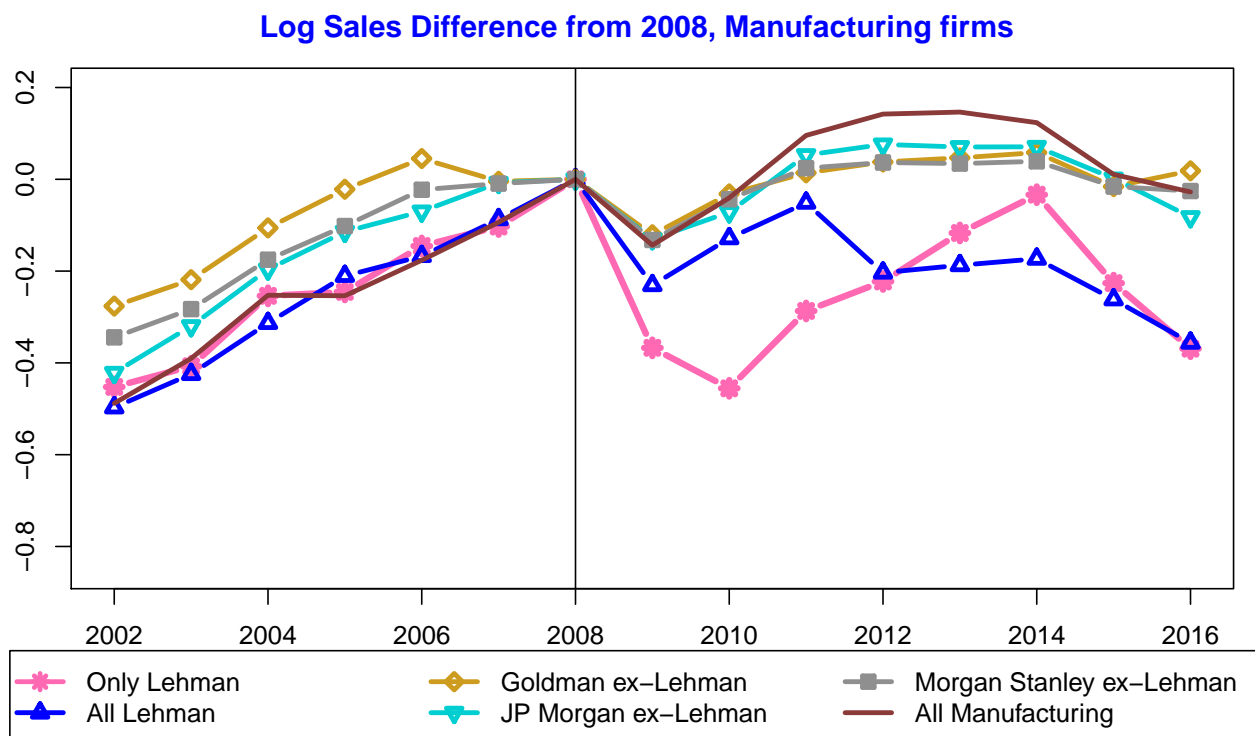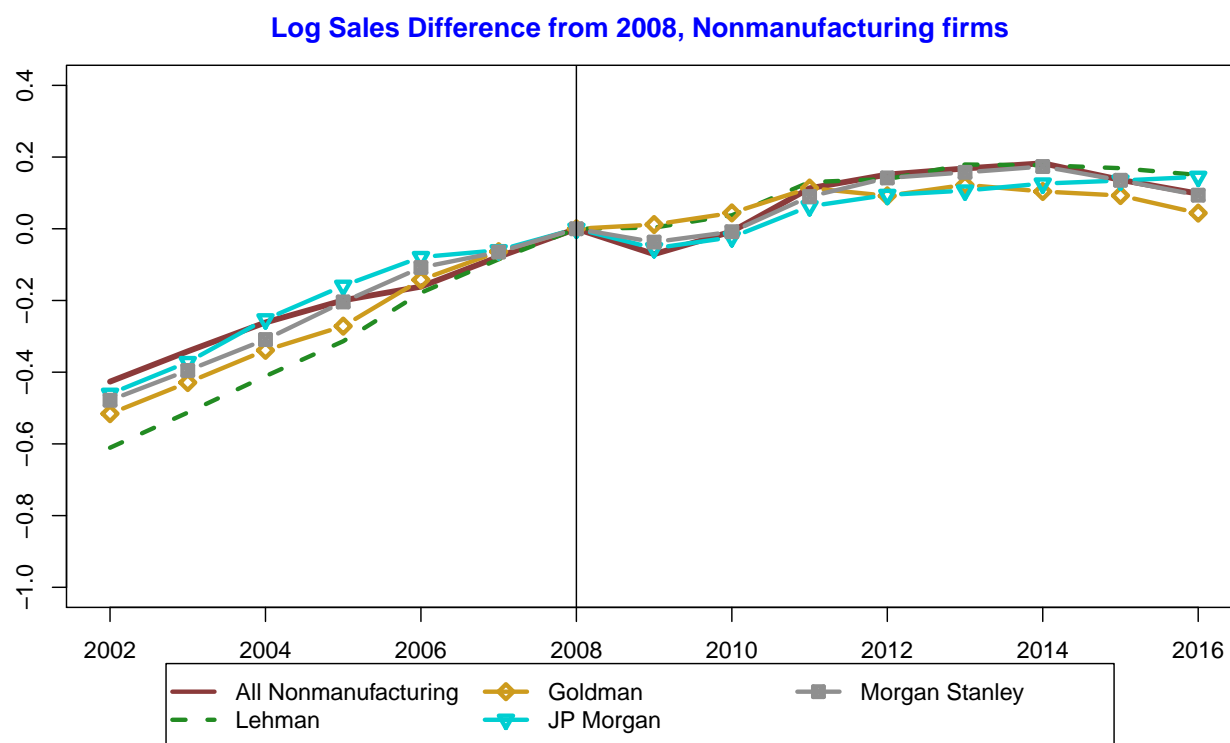
Figure D.4: Aggregate Sales and Employment Growth Relative to 2008

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 $i \in \{Lehman, \ Goldman, \ MS, \ JPM\}$:

$$
\begin{aligned}
Y_{i,t} = \alpha_i + \sigma_t + \mathbb{1}_{\{Mfg\}} \times \theta_t + \gamma X_{i,t-1} + \\
\sum_i \left(\rho_i \times \mathbb{1}_{\{Year \geq 2009\}} \times Bank_i \right) + \sum_i \left(\Omega_i \times \mathbb{1}_{\{Year \geq 2009\}} \times Bank_i \times \mathbb{1}_{\{Mfg\}} \right) + \epsilon_{i,t}
\end{aligned}
\tag{12}
$$

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 12 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:

$$
\begin{aligned}
Y_{i,t} = {}& \alpha_i + \sigma_t + \mathbb{1}_{\{Mfg\}} \times \chi_t + \gamma X_{i,t-1} + \\
& \rho \times \mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i + \\
& \Omega \times \mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{Mfg\}} + \\
& \xi \times \mathbb{1}_{\{Year \geq 2009\}} \times Spread_i^{2000-2007} + \\
& \lambda \times \mathbb{1}_{\{Year \geq 2009\}} \times Spread_i^{2000-2007} \times \mathbb{1}_{\{Mfg\}} + \epsilon_{i,t}
\end{aligned}
\tag{13}
$$

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 13 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 13 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:

$$Y_{i,t} = \alpha_i + \sigma_t + \gamma X_{i,t-1} + \beta \times \mathbb{1}_{\{Mfg\}} \times \mathbb{1}_{\{Year \geq 2009\}} + \epsilon_{i,t} \tag{14}$$

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.[38] 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,

---

[38]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 14 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.[39] 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.

---

[39]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.

BAA–10Y Treasury Spreads

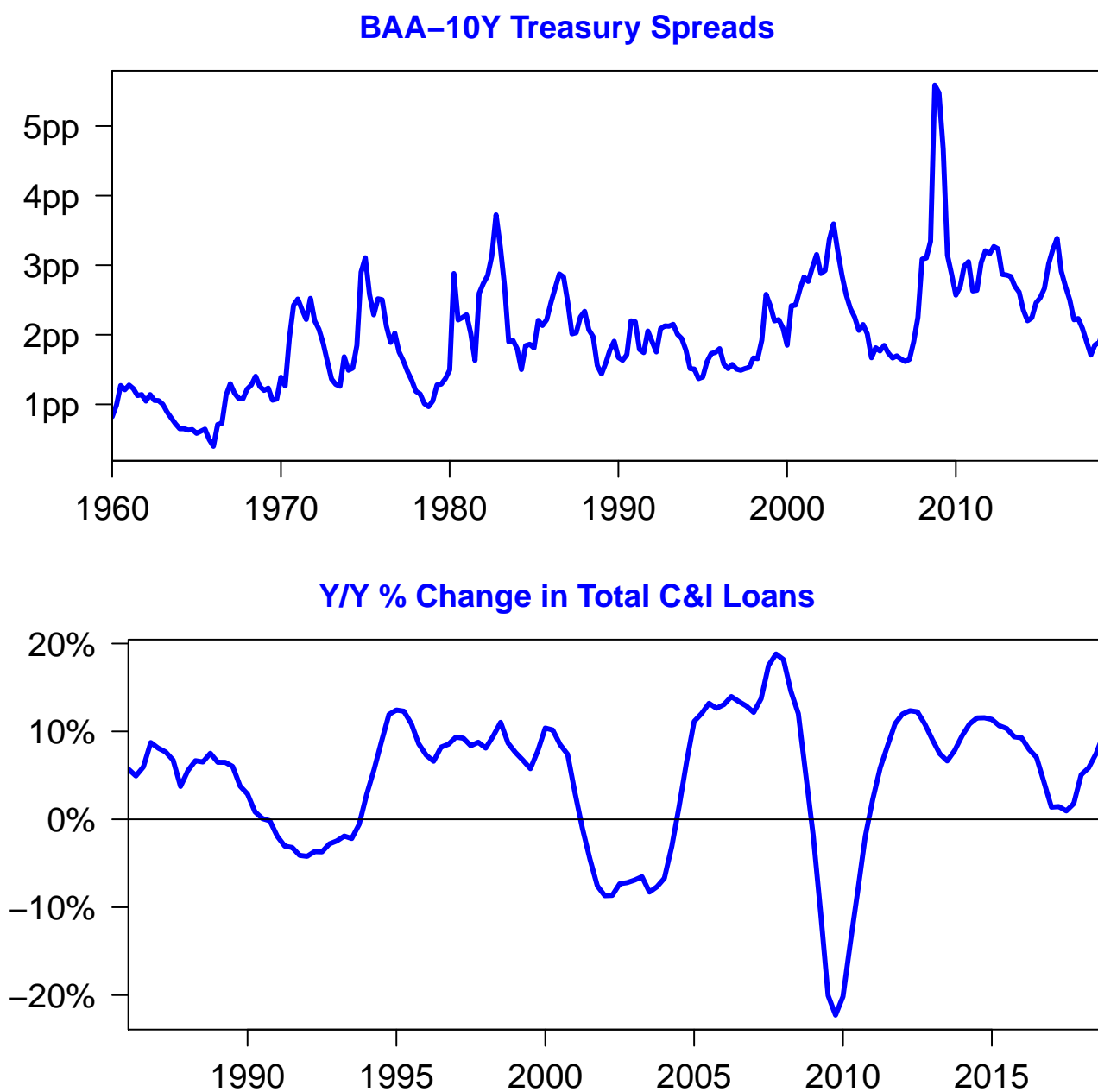

Y/Y % Change in Total C&I Loans

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 14 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 14 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 14 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).

$$Y_{i,t} = \alpha_i + \sigma_t + \gamma X_{i,t-1} + \sum_j \eta^j \times \mathbb{1}_j \times \mathbb{1}_{\{Year \geq 2009\}} + \epsilon_{i,t} \tag{15}$$

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 15 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.

Change in Manufacturing Employment Share Relative to 1985

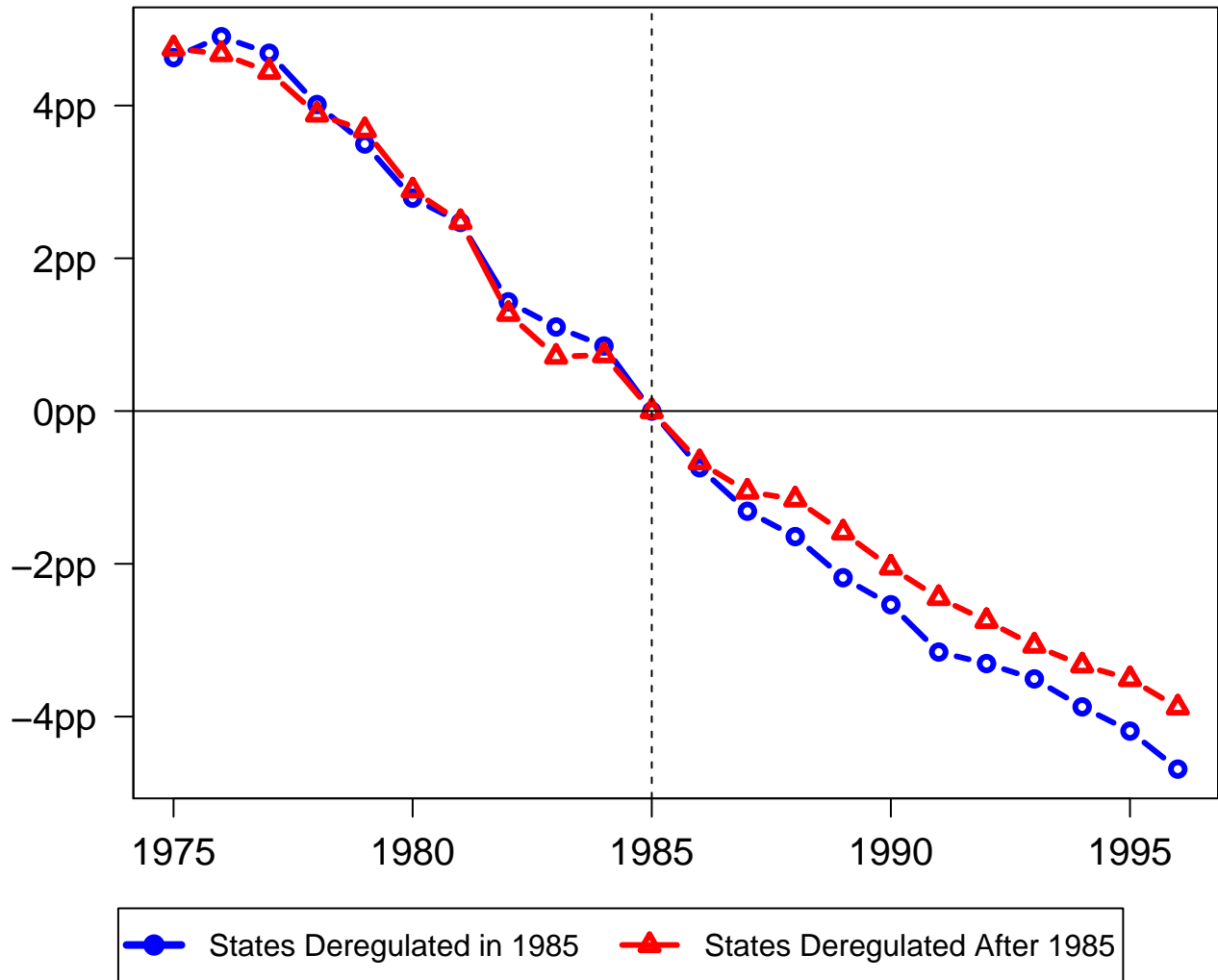Legend: States Deregulated in 1985 — States Deregulated After 1985

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:

$$\Delta_{t-1,t+h}^i = \alpha^i + \delta_t + \gamma^i \cdot t + \beta^h dereg_t^i + \epsilon_t^i \tag{16}$$

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.
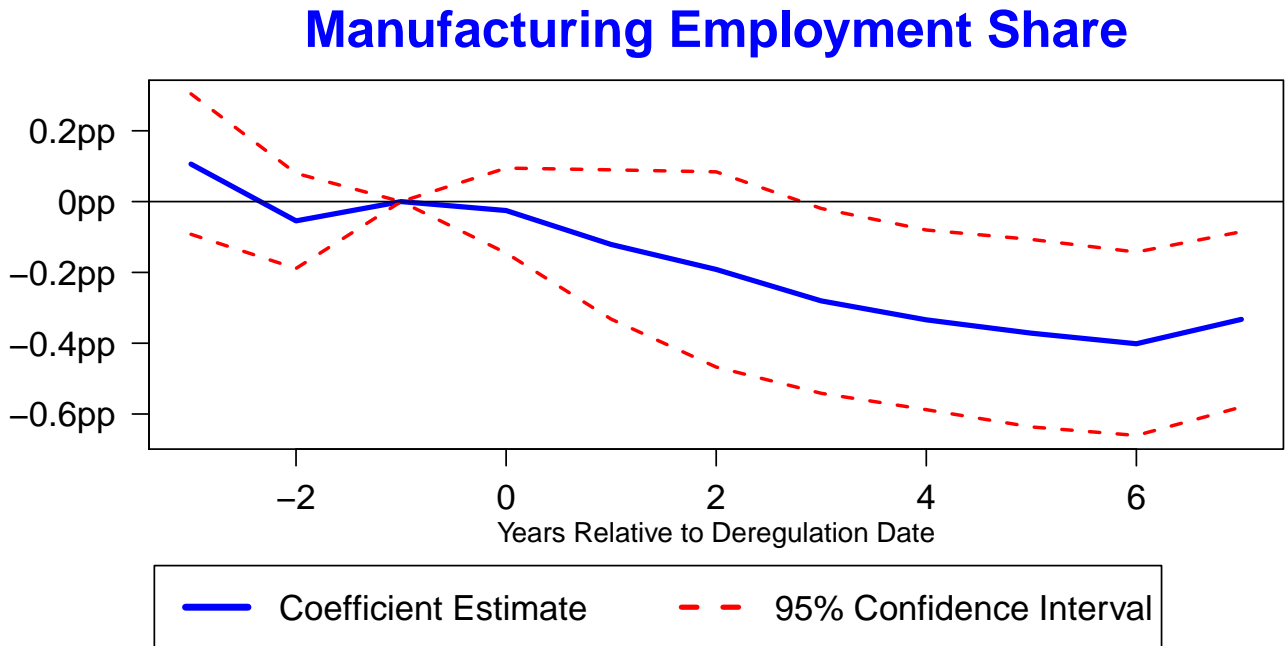
Manufacturing Employment Share

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

Note: This figure shows the results of estimating Equation 16 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.
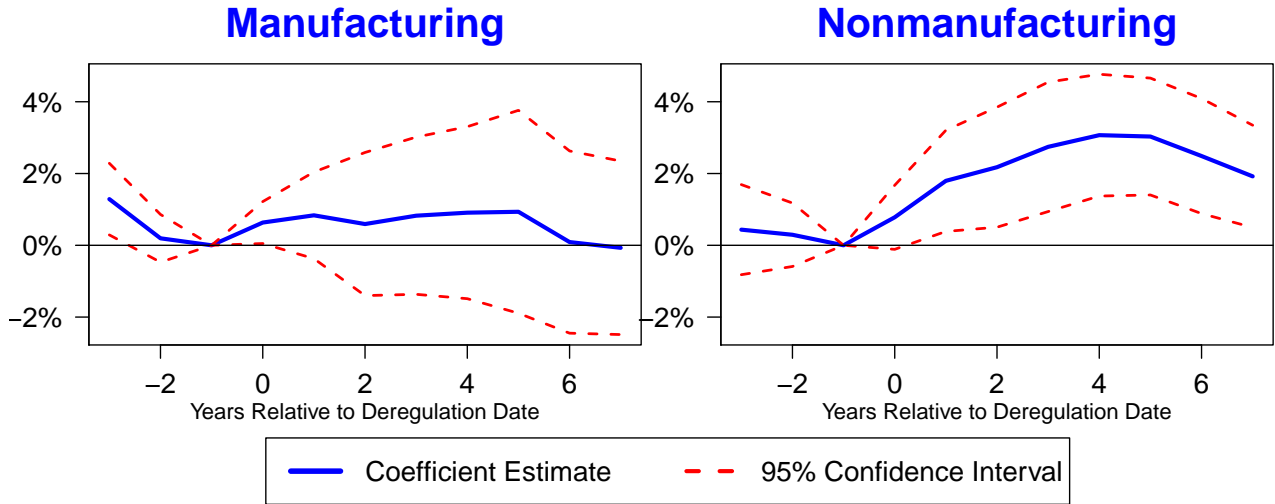
Figure E.3: Effect of IBD on Log Employment

Note: This figure shows the results of estimating Equation 16 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.
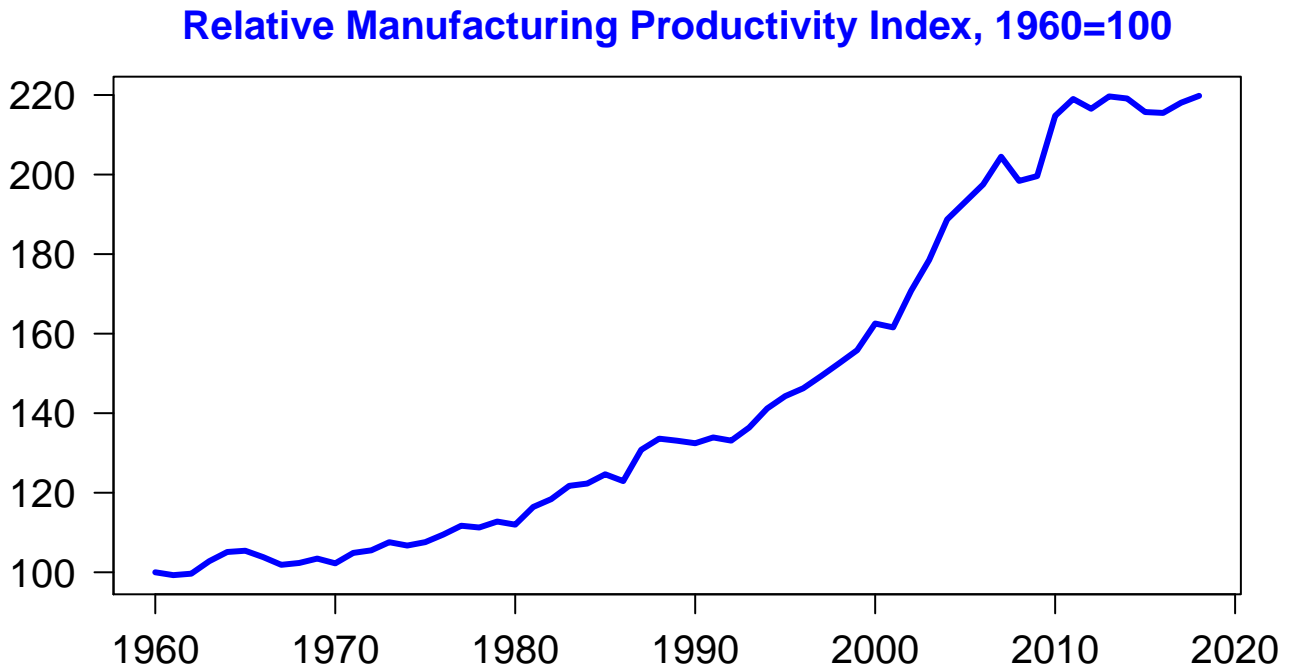
Figure F.1: Manufacturing Relative Productivity Growth

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.
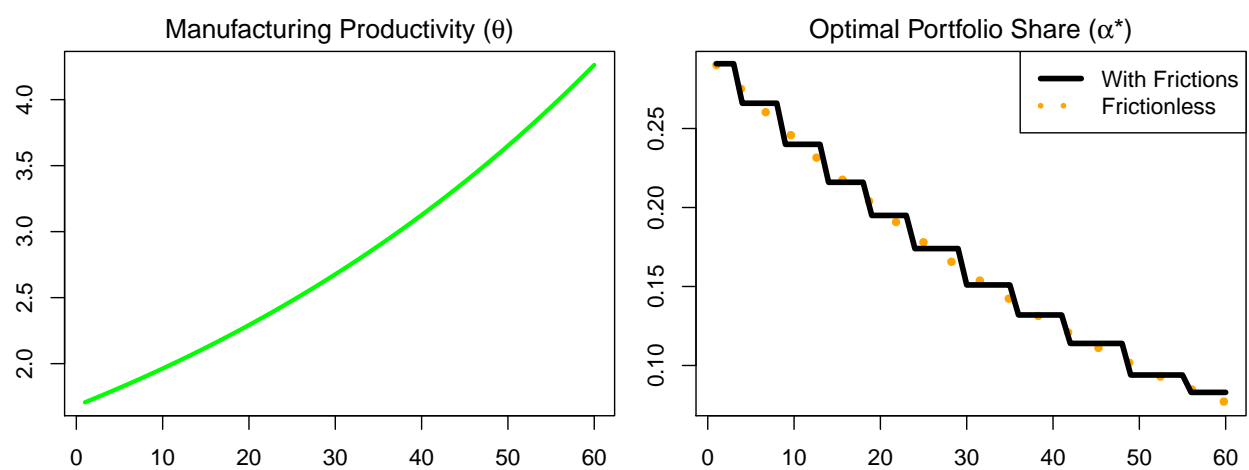
Figure F.2: Model without Recessions

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.