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

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Abstract

Credit reallocation accelerates structural change 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 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 highlights substantial misallocation costs of policies which respond to crises by providing credit to declining industries such as the automaker bailout during the Great Recession.

Keywords: Structural change, reallocation, financial frictions

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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 the composition of the economy changes. Outside these established 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. I develop a model with technology-driven structural change and fixed costs of establishing new lending relationships to rationalize these results. These findings suggest that policies which seek to maintain financing for firms in declining sectors in the aftermath of a crisis can have the adverse effect of restricting credit flow into 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. To my knowledge, this is the first paper to document this fact. 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.

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.

The third step is to show that a formal 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, which I generate by combining long-run growth in manufacturing's relative productivity with CES preferences as in Ngai and Pissarides (2007).[1] The second is the requirement that firms need to obtain credit through a relationship with a bank and that there are fixed costs to establishing such a relationship.[2] The third feature is recessions which separate firm-bank matches.

[1]As I discuss in Section 5, my results all hold in a setting in which alternative mechanisms (such as income effects) are instead driving the decline.

[2]This is consistent with an extensive literature in economics and corporate finance related to relationship

In my model, it is the interaction between the long-run productivity-driven decline in the manufacturing share and fixed costs of credit reallocation that generates the stylized facts observed in the data. The secular trend in manufacturing's share of activity reduces the benefit of allocating credit to manufacturing firms over time. Rather than occurring smoothly, the presence of fixed costs will cause this reallocation to be concentrated in a few periods. At the heart of this mechanism is the idea that banks will choose between maintaining their existing relationships or paying fixed costs and establishing new ones. By destroying a portion of existing productive relationships, separations will reduce the value of inaction. Thus even holding constant the values of matching with new firms and the fixed costs of establishing relationships, countercyclical separations—a robust feature of the data—will lead to countercyclical reallocation.

These findings can inform policymakers about the potential consequences of interventions in credit markets during and after a recession, such as the US Treasury's Automotive Industry Refinancing Program (AIFP) from 2008-2014. As noted by Goolsbee and Krueger (2015), there were a variety of justifications for these programs—including worker-level job switching costs, deadweight losses from bankruptcy, and effects on supplier networks—that my model does not consider. What the model can do, however, is quantify the opportunity costs of such a policy that arise from preventing credit from flowing to new sectors: while a firm-bank match can be optimal at the onset of a crisis due to the presence of fixed costs, structural change means that re-establishing these destroyed relationships will not necessarily be efficient.

After calibrating the model to match the size and timing of structural change in the data, I simulate a policy which forcibly re-establishes all relationships destroyed during the Great Recession and maintains them for six years. My model suggests that the costs of preventing credit flows to more valuable sectors are significant. I find that the cumulative output losses 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 the true costs of these policies are significant even in cases where there is no credit risk.

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

lending. See for example Boot (2000), Elyasiani and Goldberg (2004), Elsas (2005), and Hachem (2011).

(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.[3] 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).

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

[3]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.

(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. (2017) 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, which abstracts from agriculture, 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 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.

This figure shows two accounting exercises to give a sense of the magnitudes of the changes that have occurred during recessions. 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.

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. It is only slightly below the dashed red line by 2018; the dashed black line, which shows value of this counterfactual series at the end of 2018, corresponds to the actual manufacturing employment share observed in 1990. 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 supply of available credit would thus be expected to lead to a decline in the manufacturing share of activity.

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.[4]

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

[4]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.

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. While the Lehman's failure and banking deregulation had opposite effects on the level of economic activity, the fact that the both have the same effect on its composition provides strong support for the existence of the credit reallocation channel over other purely cyclical mechanisms.

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.[5] The definition of "real investment" loans is based on Ivashina and

[5]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.

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.[6] 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 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.[7] 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[8]—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.[9] I focus on revolving lines of credit because the bankruptcy of a syndicate member

[6] 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.

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

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

[9] 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.

in this case results in a direct reduction in the quantity of credit available to the borrower.[10] My identification assumption is that, within each industry, firms with Lehman attachment would have performed the same as non-Lehman firms if Lehman had not declared bankruptcy.

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 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[11] 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

[10] 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.

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

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 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.

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.[12] 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

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

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 specification 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 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 and D.15 show that this reallocation was driven by the extensive loan margin. Finally, I show in Table D.16 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.[13] 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.[14] Allowing 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. (2017). 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

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

[14]While the existing literature on IBD has generally focused the US, Bertrand et al. (2007) find that banking regulation in France in the 1980s led to increases in job and asset reallocation for sectors which exhibit greater external financial dependence.

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:

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

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.[15] Because there is no variation in state treatment status after 1996, I follow Strahan (2003) and use data from 1976-1996.[16] 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

[15] 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. (2017). See Amel (1993) for more details regarding the nature and timing of these regulations.

[16] 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.

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 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. (2017), 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.[17]

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.[18] Taking into account the possibility of recessions, output in each sector can be written:

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

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 (α_t^M and α_t^N), productivity levels (θ_t^M and θ_t^N), and recessions (δ_t). The flow utility each period can be expressed:

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

[18] 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.

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

The economy has a finite horizon of N periods.[19] 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:

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

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

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

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.[20] 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.

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

[20] 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).

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 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.[21] 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.[22]

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.[23]

These results are illustrated in Figure 3. The dotted orange line corresponds to the optimal

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

[22] 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.

[23] 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.

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 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 observed in the data. 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.[24]

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.[25] 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 of which have

[24] 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.

[25] 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).

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.[26]

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 4. 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. 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.[27] 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.

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

[26]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.

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

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.

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7 Figures and Tables

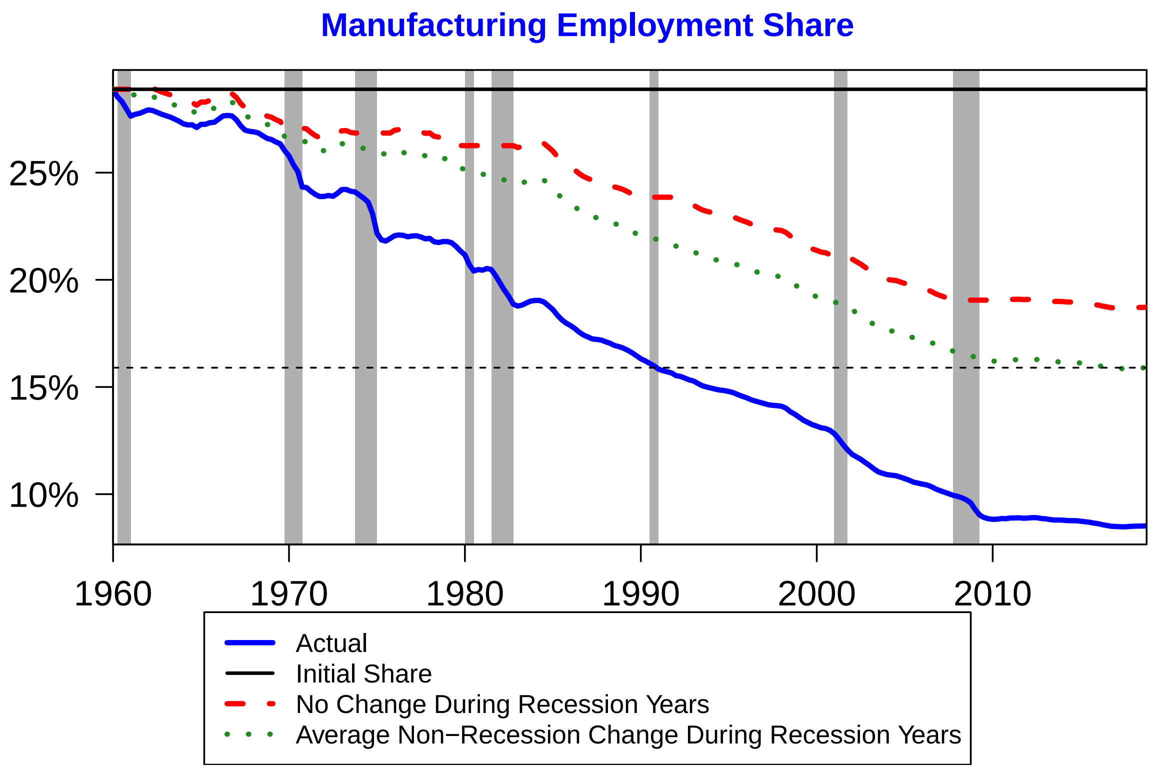

Figure 1: Change in US Manufacturing Employment Share, 1960-2018

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

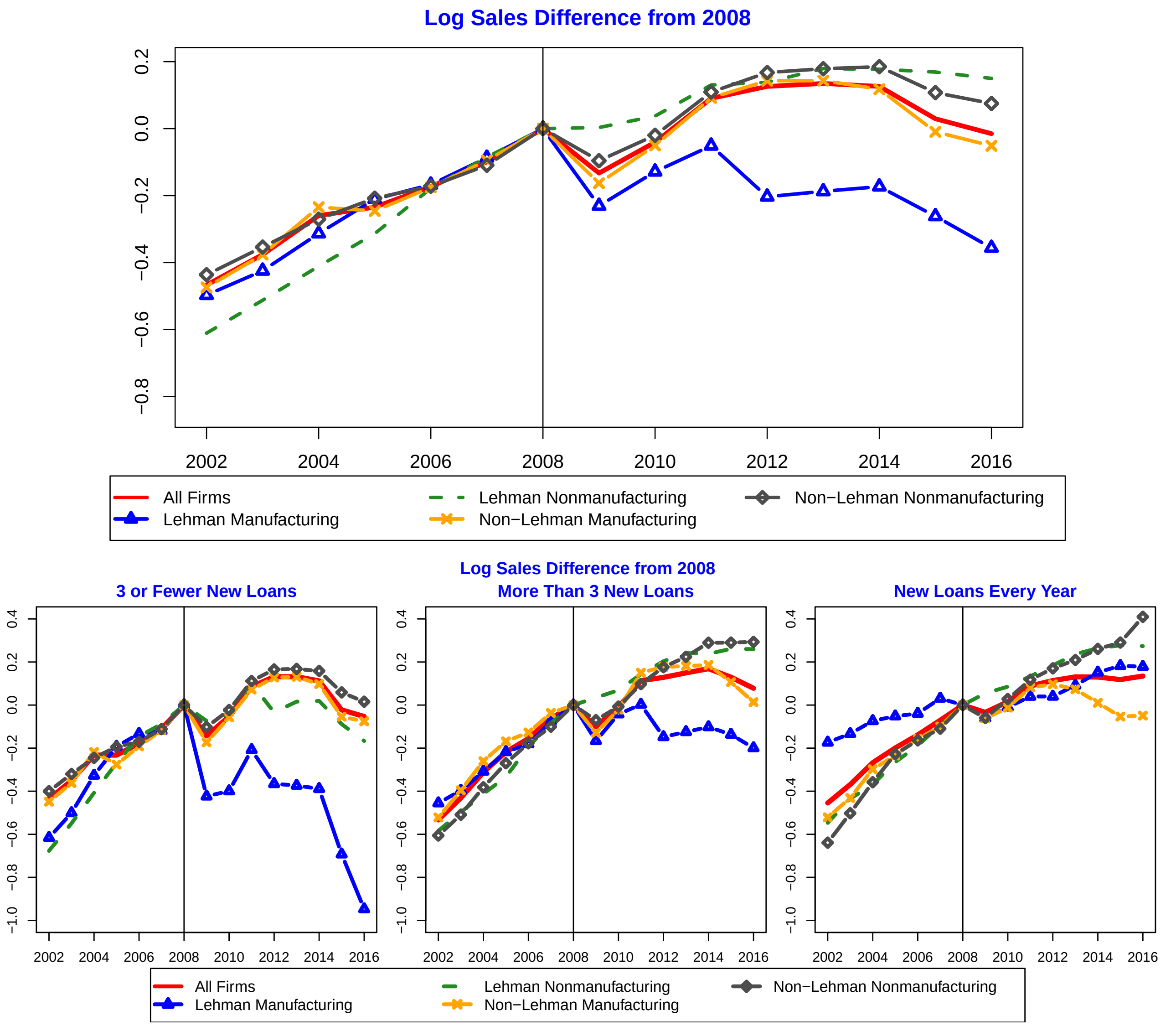

Figure 2: Aggregate Sales Growth Relative to 2008

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.

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

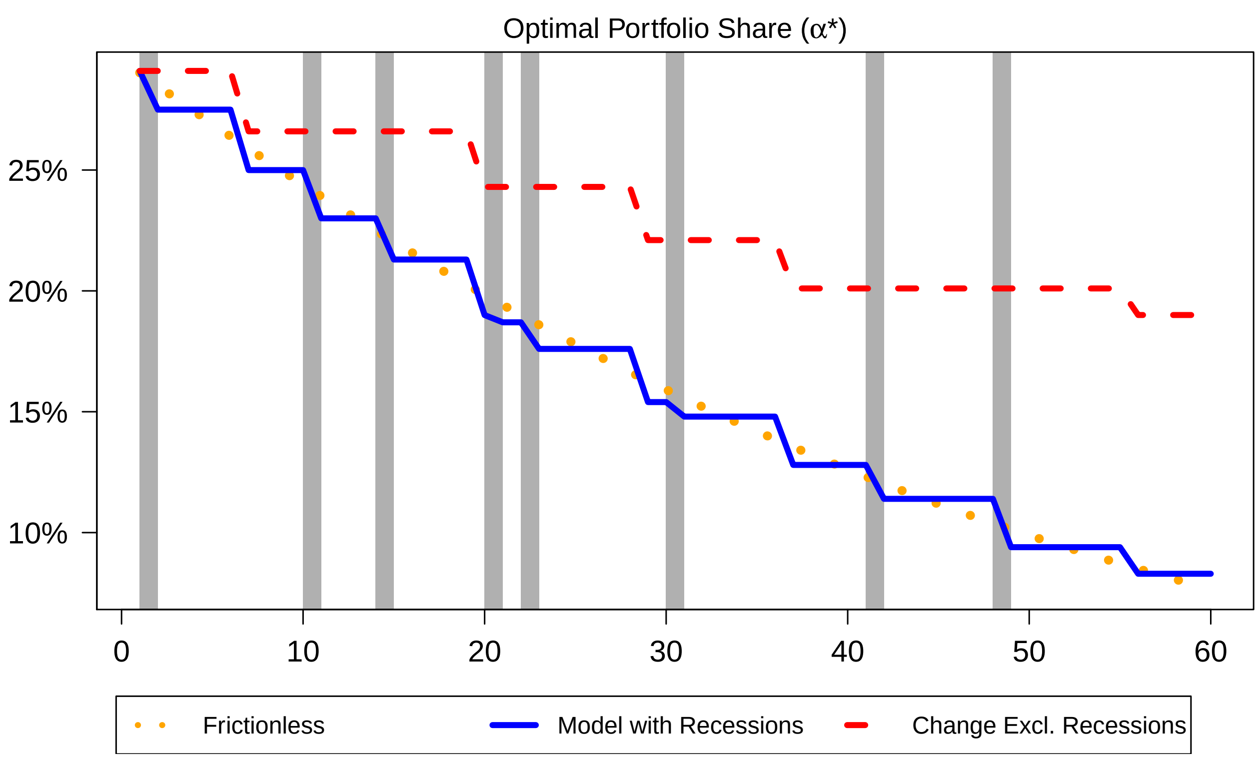

Figure 3: Model with Recessions

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.

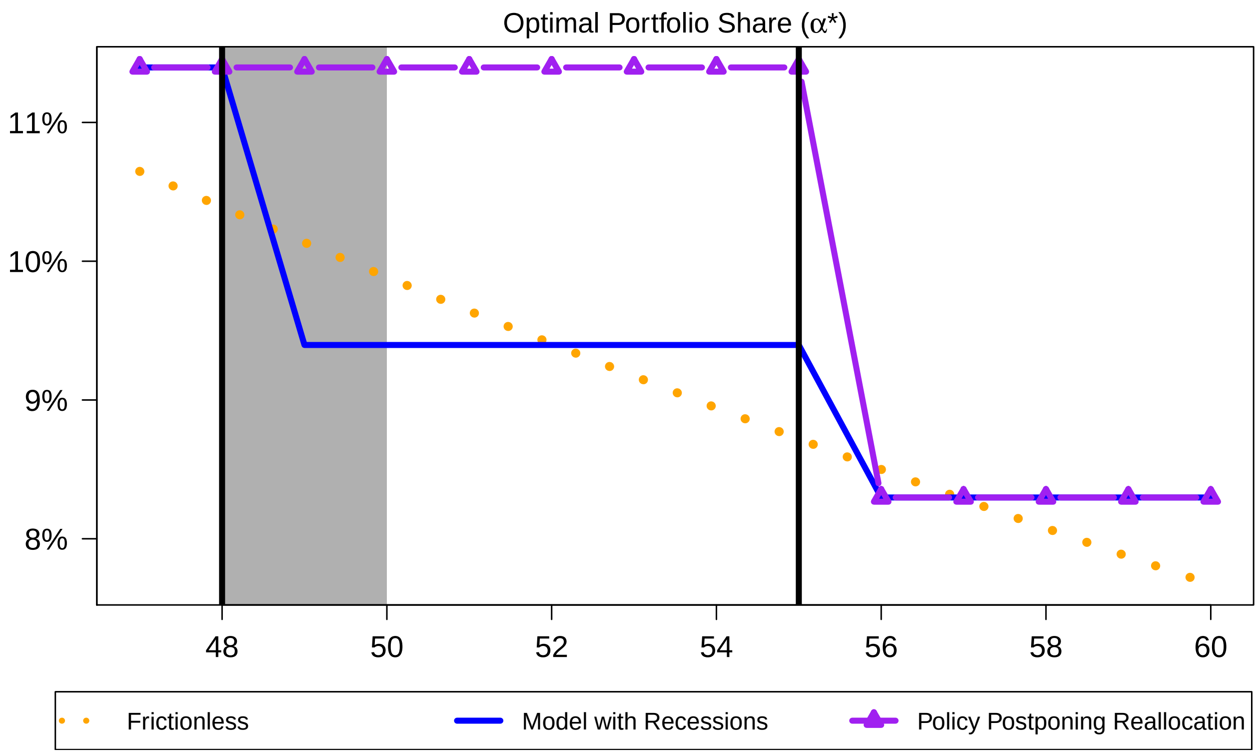

Figure 4: Effects of Policy Preventing Reallocation

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.