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

Cooper Howes†

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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
JEL Codes: E32, E44, E51

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†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 decline of US manufacturing since 1960. The manufacturing employment share fell from 28.9% in January 1960 to 8.4% in December 2019. More than half of the decline during this period occurred during the 12.5% of quarters classified by the National Bureau of Economic Research as being in a recession, and shares of other activity measures such as value added or gross output show similar patterns. A range of statistical trend-cycle decompositions imply that just under half of the drop in the manufacturing share during recession is structural rather than cyclical, suggesting 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. I use 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 exposure to Lehman were roughly 16.6 percentage points more likely to obtain a new loan in each year from 2009-2016. This effect is economically significant and represents more than half of the average annual probability of obtaining a loan for these firms. Manufacturing firms were only 7.6 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; Lehman exposure reduced sales and employment by roughly 7% for manufacturers, but had no effect for nonmanufacturing firms.

Second, I show that the predictions of the credit reallocation channel generalize beyond recessions. The Abraham and Katz (1986) critique of Lilien (1982) famously showed that purely cyclical channels can generate the concentration of an industry's decline during recessions. A novel prediction distinguishing my paper from these critiques is that an expansion in credit supply should have the opposite effect on the *level* of economic activity as a credit contraction, but the same effect on its *composition*. I use evidence from interstate banking deregulation to test this prediction. Between 1978-1994, almost all states passed laws that expanded firm access to credit by easing restrictions for out-of-state banks. I follow the approach pioneered in Jayaratne and Strahan (1996) to show that this deregulation led to a 0.13 percentage point decline in a state's manufacturing employment share that was driven entirely by an increase in nonmanufacturing employment. This provides direct evidence that changes in credit supply have a secular, rather than purely cyclical, effect on the manufacturing share.

Finally, I 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, which I model as 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. In the model there is a fixed cost 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.

In the model, it is the interaction between the long-run decline in the manufacturing share and

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

the 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. Recessions reduce the opportunity cost of reallocation by destroying relationships and decreasing 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. Following the recession, it is the lowest-productivity manufacturers—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—who find themselves on the losing end of credit reallocation.

This fact has important consequences for policy interventions during 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 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 the 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.[2] 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 of Japanese bank branches in the United States to analyze how Japanese financial shocks in the 1990s were transmitted to the US. 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. Siemer (2019) documents a role for credit constraints in explaining employment outcomes for small and large firms in the US during the Great Recession. 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 heterogeneous 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

[2] 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.

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 dates 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 undesirable.

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 in the 1980s and 1990s. 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 the end of 2019, that share had fallen to 8.4%. This trend is shown as the solid blue line in Figure 1. Rather than falling uniformly, this share has tended to decline disproportionately during NBER 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 recession. If the quarter has a recession, I instead impose a change of zero. The total series has declined by 20.5pp between 1960 and 2019 (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 10.3pp decline occurred during recessions, corresponding to the gap between the blue and red lines. Thus purely from an accounting standpoint recessions played more of a role in the decline in the manufacturing employment share than non-recessions despite the fact that they occurred in just 12.5% of quarters from 1960 to 2019.

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. In Section 2.2, I use a variety of trend/cycle decomposition techniques to argue that roughly half of the decline during recessions has been secular rather than cyclical. In Section 2.3, I describe how credit reallocation channel can generate these patterns and outline several testable implications.

2.2 Secular and Cyclical Changes in the Manufacturing Share

Changes in the manufacturing employment share can be decomposed into secular and cyclical components. This section uses a variety of trend-cycle decompositions to quantify their relative importance in the data. Taking an average across these specifications, I find that just under half of the decline during recessions has been structural rather than cyclical. This suggests that purely cyclical mechanisms, such as the Abraham and Katz (1986) critique of Lilien (1982), cannot fully account for the behavior of the manufacturing share over the business cycle.

As a first approach I follow Chodorow-Reich and Wieland (2020) to create a "through-the-cycle" measure of the manufacturing employment share. They classify time periods into one of three categories. The first is recessions, which are defined according to the NBER. The second is recoveries, which they define as the time from the end of a recession until the level of private employment reaches its pre-recession value. The final category is expansions, which includes all other time periods. The average cyclical component of the manufacturing share should by definition be equal to zero across a recession and the subsequent recovery, so the change over this period can be attributed to secular factors. This approach is described in detail in Appendix C and suggests that that 46% of the decline in the manufacturing share during recessions is secular.

As a complement to this methodology, I employ three other econometric techniques developed to decompose trends and cycles in time series data and compare them in Table 1: the Hodrick-Prescott (HP) filter, the Hamilton (2018) filter, and the Baxter and King (1999) filter. For each of these series, I calculate the ratio of the total decline in the trend component to the total decline in the actual data during recessions. The HP filter is the most widely known and used of these approaches and suggests that 38.5% of the decline in the manufacturing share during recessions is permanent. Hamilton (2018) notes that the HP filter can lead to spurious dynamic correlations and proposes a simple alternative; using his approach gives a value of 25.0%. I also consider the bandpass filter developed in Baxter and King (1999), which Hodrick (2020) argues offers an improvement over the Hamilton filter for more complex data generating processes and which gives a much higher value of 80.3%. As a final check, I calculate the trend as a simple three-year centered moving average, which corresponds to the average cycle length in Chodorow-Reich and Wieland (2020). This approach gives a value of 50.3%.

While the range of these methodologies is somewhat wide, the mean (48.0%) and median (45.8%) both suggest that almost half of the decline in the manufacturing share during recessions can be attributed to secular factors. Explaining the cyclical behavior of the manufacturing share in the data thus requires a theory that can account for significant secular changes. In the next subsection, I argue that the credit reallocation channel can generate a concentrated burst of secular change in recessions and describe several of its testable predictions.

2.3 Framework and Mechanism

The previous subsections showed that manufacturing's decline has been concentrated during recessions and provided empirical evidence that almost half of this decline has been due to secular factors. Here I argue that a credit reallocation channel can explain these findings. 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. 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. Any exogenous increase in supply of available credit would also be expected to lead to a decline in the manufacturing share. The fact that positive and negative credit supply shocks have opposite effects on the *level* of economic activity but the same effect on its *composition* cannot be explained by purely cyclical mechanisms such as those in Abraham and Katz (1986), but they are both straightforward consequences of the credit reallocation channel.

This effect will be most pronounced during the initial periods in which the manufacturing sector is larger. Over time as manufacturing shrinks, a smaller number of manufacturing firms will lose access to credit during a recession, and as a result the magnitude of reallocation will be smaller. This is consistent with the patterns in Figure 1, in which the earliest recessions show the most pronounced declines. The fact that the manufacturing share fell as much as it did during the Great Recession despite starting from such a low base level reflects the exceptional magnitude of the financial and economic distress during that period.

This paper uses two natural experiments to test these predictions. In Section 3, I examine the effects of bank failure using the collapse of Lehman Brothers in 2008. Relative to nonmanufacturing firms, manufacturers exposed to Lehman were persistently less likely to be able to obtain new loans and experienced worse real outcomes in 2009 and beyond. 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.

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. 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. Syndicated lending has become steadily more popular since its inception in the 1980s because it diversifies the risk faced by any single bank and allows nonbank financial institutions to obtain exposure to corporate credit. This type of 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 D.1 in the Appendix.

Table 2 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, and 90% of loans were at least $8mn. 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. I follow Ivashina and Scharfstein (2010) and focus on 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. DealScan also includes information about the borrowers and 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 D.

3.2 Identification Strategy

Lehman Brothers declared bankruptcy on September 15, 2008 during one of the most tumultuous days in the history of modern financial markets. 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[3]—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. I focus on revolving lines of credit because the bankruptcy of a syndicate member in this case would result in a direct reduction in the quantity of credit available to the borrower, and thus make it more likely that the borrower would need to seek new sources of financing.

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

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

sales and employment in the post-recession period. Based on observable characteristics this does not appear to be the case. Table 3 shows summary statistics from 2004 for firms with and without an open line of revolving credit involving 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 than their non-Lehman counterparts. Spreads between Lehman non-Lehman firms were very similar across sectors, averaging 32bp for manufacturers and 40bp for nonmanufacturers. These observations are in line with market perceptions that clients of Lehman Brothers tended to be larger institutions[4] but do not suggest any differential selection across sectors. Appendix E documents more thoroughly the similarity between Lehman's clients and those of other large investment banks.

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. Figure 3 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 almost 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. The next section supplements these aggregate results with evidence from firm-level regressions.

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 main results. This

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

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 hit harder during the Great Recession. The variable $Lehman_i$ is a dummy variable equal to one if a firm had a revolving credit facility including Lehman that started prior to 2008 and was originally scheduled to end in 2009 or later. Appendix E shows similar results using alternative measures of firms' Lehman exposure including the total number or total volume of revolving facilities.

The coefficient ρ captures the average effect on Y of having a 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.[5] The identification assumption for Ω 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 first outcome of interest is a dummy variable indicating whether firm i obtained at least one new credit facility in year t. While DealScan plausibly captures all observations for which this variable equals one, determining when to record values of zero is more complicated because of firm exit. To mitigate this issue, these loan probability calculations use only firm-year observations recorded in Compustat for the denominator. I use data covering 2000-2016. I start in 2000 because 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 which had entered 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

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

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 loan are shown in the first column of the top rows of Table 4. The first row, which corresponds to ρ in Equation 1, shows that nonmanufacturing firms with an open line of credit through Lehman became about 16.6 percentage points more likely (relative to the average firm without Lehman attachment) to obtain new loans following Lehman's collapse. This effect represents more than half of the unconditional average annual probability of getting a loan for these firms.[6] 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 -9.0pp, leading to a much smaller total effect of 7.6pp. Put another way, credit shocks pushed firms in all sectors to seek out additional financing, but manufacturers were much less likely to obtain it.

The last three columns show a variety of alternative specifications that generate very similar coefficient estimates. The second column restricts the sample to the set of firms that ever received 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. 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 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 were less likely to receive new loans relative to their nonmanufacturing counterparts.

The middle and bottom sections of Table 4 show the effects for sales and employment. The baseline specification suggests that Lehman exposure led to a decline in both employment and sales of roughly 7% for manufacturing firms but had no statistically significant effect for nonmanufacturing firms. These results are generally similar across specifications; although the exclusion of firm-level controls (column 3) attenuates the estimated sales effects, the effects on employment approximately double. In Table 5 I report estimates Ω using a modified version of Equation 1 that also interacts the lagged firm-level controls $X_{i,t-1}$ with the Lehman exposure dummy and the post-2009 indicator. The effects are very similar to my baseline estimates, providing evidence that

[6]The annual average probability that a Lehman-attached firm received a loan with a reported purpose of "real investment" or "working capital" from 2000-2016 was 30.5%.

my findings are not simply picking up persistent differences in non-industry firm characteristics that may have differed across lenders.

In Appendix E I consider a wide range of additional extensions and robustness checks. I show that these results are robust to using a continuous rather than binary measure of Lehman exposure, using only loans in which Lehman had a role beyond participant, scaling the total amount of credit obtained through Lehman by a lagged measure of sales, replacing the binary new loan indicator with a continuous outcome measuring the time until each firm's first post-crisis loan, or excluding outliers within each sector. 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 Evidence Beyond Manufacturing

My empirical results in the previous section showed that the effects of credit contractions are more severe for manufacturing firms. But the mechanism in my paper is really about firms with limited long-run growth prospects, regardless of whether they fall under manufacturing or services. To provide some evidence that these effects are more general, this section shows that the effects of Lehman exposure were less severe for firms in more subsectors with better long-run prospects, regardless of whether they fell under manufacturing or services.

For this exercise, I replace the manufacturing indicator in Equation 1 with a dummy variable representing more valuable sectors across both manufacturing and nonmanufacturing. Classification of these "high-value" sectors is based on the broad categories with the fastest growth in real value added between 2000 and 2008. High-value services include information, professional and business services, engineering services, and healthcare services. High-value manufacturing includes machinery, electronics, petroleum, and chemical products. This classification applies to just under 40% of the firms with Lehman attachment.

The mechanism in my paper predicts that among all firms with Lehman exposure, this group should have an easier time getting new credit and experience better real outcomes than firms in less valuable sectors despite the fact that 43% are manufacturers. Table 6 shows that this prediction is borne out in the data. Firms in high-value sectors were almost 10pp per year more likely to get a new loan relative to firms outside of this group, and sales were 3.8% higher. The estimated effects are smaller for employment and not statistically significant, but the point estimates remain positive.

The fact that these results generalize beyond just manufacturing and services connects this paper to two other important findings from the literature. The first is the persistent effects of losing bank credit emphasized by Chodorow-Reich (2014) and Huber (2018). In my paper this persistence arises as a consequence of the fact that many of the firms which lose access to credit would not be viable if a new relationship had to be built from scratch in order to finance them. The second, shown in Chodorow-Reich and Falato (Forthcoming) and Acharya et al. (2020), is that struggling lenders are more likely to reduce loan commitments to borrowers that have violated their covenants. To the extent firms experiencing a secular decline are more likely to find themselves in breach of their covenants during downturns, they will be more likely to end up on the losing end of a lender's credit reallocation decision. Thus by reducing the cost of reallocation during downturns, loan covenants will further concentrate structural change during these periods.

4 Evidence from Interstate Banking Deregulation

This section supplements the results from Section 3 with evidence from US interstate banking regulation (IBD) to test another prediction of the credit reallocation channel. One of the key features of the mechanism described in Section 2.3 and developed more formally in Section 5 is that adjustment costs will create inertia in the stock of credit tied up in matches. Conditional on reallocating, however, credit will increasingly flow to sectors made more valuable via structural change. Newly created credit that has not yet entered a match is not subject to this inertia and so it should be more likely to flow to newer and more valuable sectors.

By allowing banks without prior relationships to enter a state and begin making loans, IBD should lead to an influx of new credit that disproportionately flows to nonmanufacturing firms. Consistent with this prediction, I find that IBD led to persistent gains in a state's nonmanufacturing employment while having no effect on its manufacturing employment. I estimate that IBD led to a 0.13 percentage point decline in a state's manufacturing share, which is approximately two-thirds of the acceleration 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.[7] 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 through IBD has had positive impacts on aggregate real economic activity in the US.[8] 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. (2020). They find that employment gains were concentrated in nontradable sectors—which consist primarily of services—and that tradable sectors showed virtually no employment effects. In Section 4.3 I compare my findings to theirs in more detail.

4.2 Effects of IBD

The main source of employment data used in this section comes from the BEA. These data provide total employment split by SIC industry code for each state from 1970-2000. Data on the timing of interstate banking deregulation come from Strahan (2003). The employment data are merged with the deregulation dates to create a balanced panel at the state-by-year level. A detailed description of the data can be found in Appendix D.

[7] Kroszner and Strahan (2014) provide a detailed summary of the literature analyzing US banking deregulation.

[8] While most work on IBD has focused on the US, Bertrand et al. (2007) analyze deregulation in France.

Most existing work analyzing the effects of IBD uses a standard difference-in-differences (DID) framework with state and year fixed effects. This approach estimates the following equation, in which the treatment indicator $dereg_t^i$ is a dummy variable equal to zero prior to state i implementing IBD legislation and one afterward:

$$y_t^i = \alpha^i + \delta_t + \beta dereg_t^i + \epsilon_t^i \tag{2}$$

Recent papers including De Chaisemartin and d'Haultfoeuille (2020), Sun and Abraham (2020), Callaway and Sant'Anna (2020), Athey and Imbens (2018), and Goodman-Bacon (2018) have pointed out some of the issues that arise when post-treatment outcomes for one unit are used as controls for other untreated units, which can cause the standard DID estimate β to fail to capture the true average treatment effect. To obtain estimates that are valid in the case of heterogeneity in treatment effects over time and across groups, I use the *did_multiplegt* Stata package, which implements the estimator from De Chaisemartin and d'Haultfoeuille (2020).

The results for the manufacturing employment share as well as the log levels of manufacturing and nonmanufacturing employment are shown in Table 7. I estimate that allowing out-of-state banks to enter leads to a decline of approximately 0.13 percentage points in a state's manufacturing employment share relative to a state that has not yet implemented IBD. To provide some context, a decline of 0.13pp represents about 1.7% 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 30% of the average annual decline over that period. The Great Recession serves as another useful 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 back-of-the-envelope calculation suggests that the estimated effects of IBD on the manufacturing employment were roughly two-thirds of the acceleration 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 2.3 makes a clear prediction on the *composition* of this change: expansion of credit should benefit predominantly nonmanufacturing firms without having any direct effect on manufacturing firms. The bottom two rows of Table 7 support this interpretation. IBD leads to a statistically significant increase of around 1.2% in a state's nonmanufacturing employment while the effect on manufacturing employment is much smaller and statistically insignificant.

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 Figure F.2 of the Appendix, I show dynamic event study estimates of these treatment effects. I find support for the parallel trend assumption between treated and untreated states prior to IBD and estimate dynamic effects that align closely with those shown in Table 7.

4.3 Comparison with Mian et al. (2020)

My finding that interstate banking deregulation primarily benefited service firms echoes the results of Mian et al. (2020). They show that nontradable employment (which includes mostly services) expands following IBD but tradable employment (which includes mostly manufacturing) does not. While my paper interprets IBD as an expansionary supply shock for firms in faster-growing sectors, however, theirs interprets it as an expansionary demand shock for firms in nontradable sectors. These channels are not mutually exclusive, so to understand their relative importance I estimate a specification that allows both tradability and exposure to long-run structural change to affect the response of an industry's employment share to deregulation. I find that the effects of both channels are statistically and economically significant, but that exposure to secular change plays a much larger quantitative role in explaining my main empirical results.

For this exercise I use BEA employment data to create a state-by-industry-by-year panel. The list of included industries can be found in Table F.1 in the Appendix. To measure an industry's exposure to long-run secular change, I use a "leave-one-out" approach. For each state j and industry i, I define $SC_{i,-j}$ as the change in the employment share for industry i across all states excluding j from 1970-2000 so that a negative value corresponds to an industry which has become smaller over time. To measure tradability, I follow the geographic approach used in Mian and Sufi (2014) and construct Herfindahl-Hirschman indices HHI_i based on an industry's geographic concentration across all states in 1975. I estimate the following regression, which directly allows for both tradability and long-run secular change to affect how the employment share of industry i in state j and year t is affected by interstate banking deregulation:

$$share_{i,j,t} = \alpha_{i,j} + \delta_t + \beta Dereg_{j,t} + \gamma^T \left(Dereg_{j,t} \times HHI_i\right) + \gamma^S \left(Dereg_{j,t} \times SC_{i,-j}\right) + \epsilon_{i,j,t} \quad (3)$$

The separate terms for HHI_i and $SC_{i,-j}$ do not change over time and are thus absorbed into the industry-by-state fixed effects. To standardize the interpretation of these interaction coefficients, γ^T and γ^S are scaled to reflect the additional effect of IBD for a one standard deviation increase in HHI_i or $SC_{i,-j}$, respectively.[9] The results are shown in Table 8.

The first column shows that across all industries, a one standard deviation increase in tradability leads to an additional decline of about 0.06pp in an industry's employment share after a state deregulates its banking sector. The second column shows that an industry exposed to an additional standard deviation of long-run decline experiences a much larger decrease of 0.39pp. In the third column, which shows results estimating both of these interaction terms in the same specification, the coefficient on the $SC_{i,-j}$ interaction term barely changes while the coefficient on tradability attenuates. The R^2 values are also much higher for the specifications that include exposure to secular change. Thus while both factors are statistically and economically significant in explaining which industries shrink following IBD, supply-side factors have much larger effects and explain more of the variation in employment shares.

In Appendix F.3 I show several alternative tests analyzing the importance of tradability and exposure to long-run secular change in determining an industry's response to IBD. I first show in Table F.2 that the least tradable manufacturing industries (such as wood, stone, clay, or glass products) do not experience any expansion in employment following deregulation. In contrast, employment in several of the most tradable nonmanufacturing industries is estimated to increase. I also use my baseline empirical specification (Equation 2) to estimate the response of state-level prices measured using both the series from Mian et al. (2020) and the more recent series developed in Hazell et al. (2020). While these results are not statistically significant, I find point estimates that are deflationary for nontradables and inflationary for tradables across both price measures.

While these results suggest that supply-side factors are quantitatively more important for determining which industries shrink following exposure to IBD, they do not preclude the existence of a household demand channel. A richer model that includes more realistic business cycle frictions in addition to the long-run secular change could simultaneously match both the cyclical facts presented in Mian et al. (2020) and Mian and Sufi (2014), and the longer-run secular facts documented here. Developing such a model is an important avenue for future research.

In summary, this section used 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. While tradability played a quantitatively important role, supply-side factors were

[9] For HHI_i, the standard deviation is 0.038. For $SC_{i,-j}$, it is 0.010.

the more important channel through which IBD affected the composition of 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 2.3 to construct a quantitative model that can 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. 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 will be the sum of output for each firm weighted by its measure within the economy:

$$Y_t^i = \int_j \left[\mathbb{1}_j^{Credit}\right] z_t^j dF_t^i(z_t^j). \tag{4}$$

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:

$$Y_t^i = \int_{z_t^{i^*}}^{\infty} \tilde{z} dF_t^i(\tilde{z}) \tag{5}$$

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. The total quantity of credit allocated to each sector can be written as one minus the CDF evaluated at the cutoff productivity level:

$$\sum_i \alpha_t^i = 1, \text{ where } \alpha_t^i = \left(1 - F_t^i(z_t^{i^*})\right). \tag{6}$$

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. Output in each sector will vary from one period to the next even if α_t remains constant 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 model implicitly assumes that the flows of real resources necessary for production, such as labor or capital, display the same cyclical properties as flows of credit. While labor reallocation has been shown to be countercyclical by Davis et al. (1998) and others, Eisfeldt and Rampini (2006) argue that reallocation of physical capital is actually procyclical. For the purposes of my model the key moment is the cyclicality of reallocation *across* sectors. To the extent that most equipment used to produce manufactured goods cannot easily be used by service-producing firms, this channel will not affect my model's conclusions.[10]

[10] A more thorough discussion can be found in Appendix G.2.

I define δ_t as the share of firms which become exogenously separated from their match with the bank. These separations occur uniformly across sectors and firm types. The destruction of firm-bank matches lowers output on impact and all destroyed matches remain unproductive until reallocation occurs. After incorporating recessions, which I model as being completely unexpected, output in each sector can be written:

$$Y_t^i = (1 - \delta_t) \int_{z_t^{i*}}^{\infty} \tilde{z} dF_t^i(\tilde{z}). \tag{7}$$

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):

$$Y_t = \left[\omega \left(Y_t^M\right)^{\frac{\epsilon-1}{\epsilon}} + (1-\omega)\left(Y_t^N\right)^{\frac{\epsilon-1}{\epsilon}}\right]^{\frac{\epsilon}{\epsilon-1}}. \tag{8}$$

The two key parameters for this utility specification are 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), productivity levels (θ_t^M and θ_t^N), and recessions (δ_t). The flow utility each period can be expressed:

$$u_t = \log(Y_t) - c \times \mathbb{1}_{\alpha_t \neq \alpha_{t-1}}. \tag{9}$$

The economy has a finite horizon of N periods. I normalize the productivity of the nonmanufacturing sector (θ^N) to 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 $t \in \{0, ..., N\}$:

$$V(\alpha_{t-1}, \theta_t, \delta_t) = \max\left\{V^{Adjust}, V^{NoAdjust}\right\}, \tag{10}$$

subject to equations 6 and 7, where the value of changing the credit share is:

$$V^{Adjust}(\alpha_{t-1}, \theta_t, \delta_t) = \max_{\alpha_t \in [0,1]} \left\{ \log \left(\left[\omega \left(Y_t^M(\alpha_t, \theta_t, \delta_t) \right)^{\frac{\epsilon-1}{\epsilon}} + (1-\omega) \left(Y_t^N(\alpha_t, \theta_t, \delta_t) \right)^{\frac{\epsilon-1}{\epsilon}} \right]^{\frac{\epsilon}{\epsilon-1}} \right) - c + \beta V(\alpha_t, \theta_{t+1}, \delta_{t+1}) \right\}, \tag{11}$$

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

$$V^{NoAdjust}(\alpha_{t-1}, \theta_t, \delta_t) = \left\{ \log \left(\left[\omega \left(Y_t^M(\alpha_{t-1}, \theta_t, \delta_t) \right)^{\frac{\epsilon-1}{\epsilon}} + (1-\omega) \left(Y_t^N(\alpha_{t-1}, \theta_t, \delta_t) \right)^{\frac{\epsilon-1}{\epsilon}} \right]^{\frac{\epsilon}{\epsilon-1}} \right) + \beta V(\alpha_{t-1}, \theta_{t+1}, \delta_{t+1}) \right\}. \tag{12}$$

The key tradeoff in the model arises because the relative productivity of the manufacturing sector θ_t is growing, which combined with an elasticity $\epsilon < 1$ implies that the marginal value of providing credit to manufacturing firms will decrease over time. In a decentralized equilibrium, this would manifest as a fall in the relative price of manufactured goods.[11]. Choosing to leave α_t unchanged allows the planner to temporarily avoid paying the fixed cost, but increases the value of reallocation in future periods when the value of manufacturing output will be even lower.

Figure 4 illustrates how the distribution of firms receiving funding changes over time using a simplified two-period example. 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 a world without adjustment frictions. 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.

[11] This phenomenon is known as "Baumol's Cost Disease"; see Baumol and Bowen (1965) and Nordhaus (2008).

5.3 Simulation

The parameter values are summarized in Table 9. 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 G.1 of the Appendix). This leads to a decline in the manufacturing share of credit and labor (which are equivalent in the model) 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. 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 per year, which almost exactly matches the value of 1.36pp observed in the data.

In the absence of fixed costs the composition of economy will adjust smoothly in response to increasing manufacturing productivity. This is shown in Figure G.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.[12]

These results are illustrated in Figure 5. 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 manufacturing share, which is very close to the 50.2% 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.

[12] An exogenous influx of new credit such as IBD will have the same effect on the *composition* of the economy as a recession but will lead to an increase in the *level* of total output.

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. The 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.[13]

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 been explored in work such as Hyman (2018) and Autor et al. (2014). The model also abstracts from frictions which can affect the costs of reallocating other productive inputs. Chodorow-Reich and Wieland (2020), for example, show that wage ridigidity and imperfect labor mobility exacerbate downturns for areas with higher exposure to reallocation. The contribution of my model is to highlight and quantify a 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 employment in the industry fell by 38% between 2000 and 2007 even as total nonfarm payrolls rose by almost 6% over this same time.

I consider the effects of such a policy implemented during the last recession observed in the

[13] Details can be found in Congressional Oversight Panel (2010), US Department of the Treasury (2015), and Office of the Special Inspector General for the Troubled Asset Relief Program (2014).

model (corresponding to the timing of the Great Recession). 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 6. 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. Nonetheless, the model is useful for highlighting inter-industry misallocation as an important cost credit intervention 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. 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 substantially 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 such as sales and employment.

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. Rather than occurring evenly, reallocation of productive resources across sectors is the product of a 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 can 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 rather than simply returning funding to the firms which had it prior to the recession.

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7 Figures and Tables

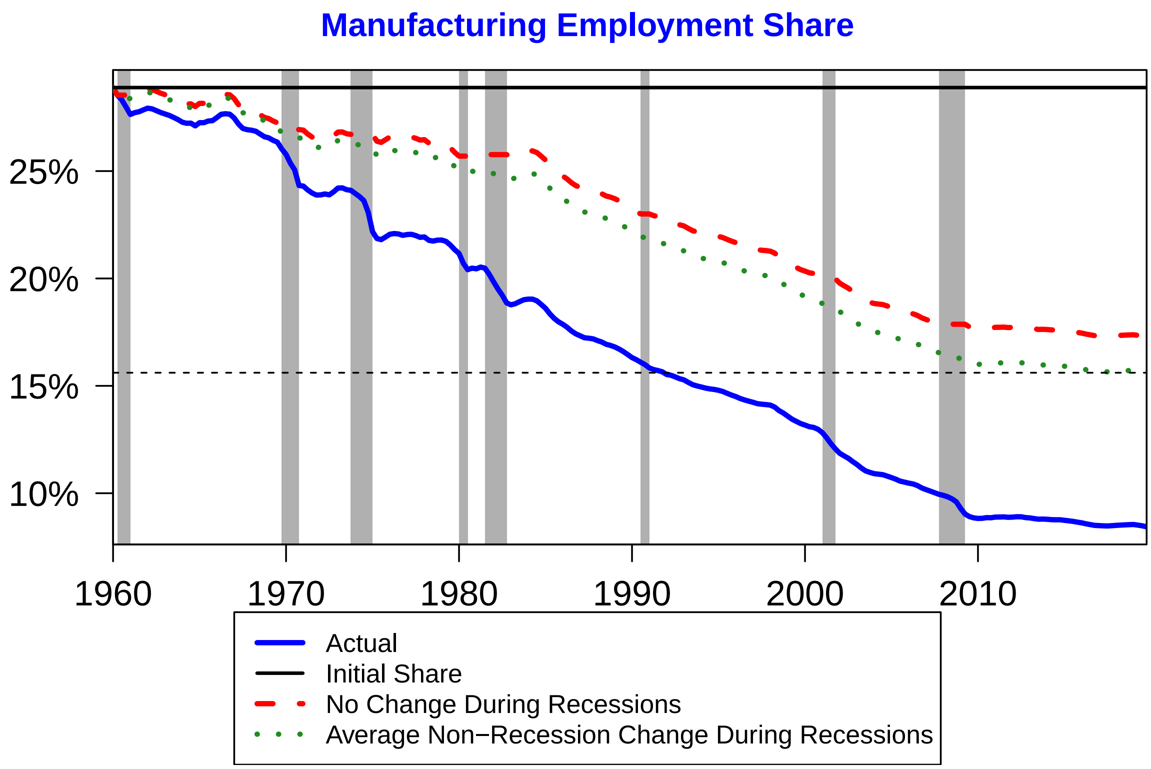


Figure 1: Change in US Manufacturing Employment Share, 1960-2018

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.

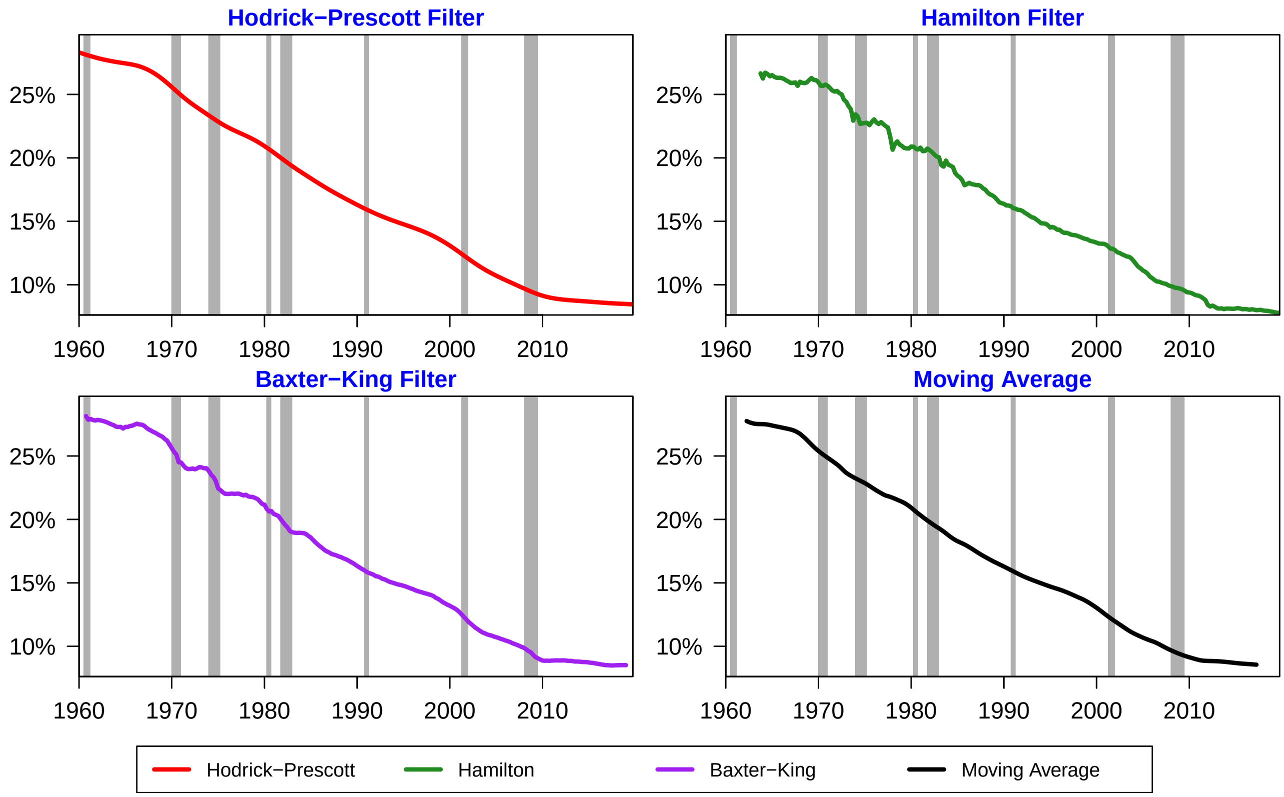


Figure 2: Trend Measures of Manufacturing Employment Share

Note: This figure shows several measures of the manufacturing employment share's trend component. The Hodrick-Prescott filter uses the default quarterly smoothing parameter of $\lambda = 1600$. Smoothing parameters for the other filters are based on the length of cycles as measured in Chodorow-Reich and Wieland (2020), which have a range of between 5 and 24 quarters with an average of 12. The Hamilton (2018) filter uses the default value of one year of lagged data for forecasting ($p = 4$) and a three-year forecast horizon to match the average cycle length in my data ($h = 12$). The Baxter and King (1999) filter uses a frequency range of 5 to 24 quarters to match the range of cycle lengths. The moving average series is calculated as a 12-quarter centered moving average to match the average cycle length.

Trend calculation methodology	Recession decline share
Chodorow-Reich and Wieland (2020)	45.8%
Hodrick-Prescott	38.5%
Hamilton (2018)	25.0%
Baxter and King (1999)	80.3%
Centered moving average	50.3%
Median	**45.8%**
Mean	**48.0%**

Table 1: Secular Portion of Manufacturing Share Decline During Recessions

Note: This table shows the average share of the manufacturing employment share decline that can be attributed to secular factors for various measures of the trend component. The numerator for each of these approaches is the average decline in the trend during recessions, and the denominator is the average quarterly change in the manufacturing share during recessions (0.29pp). The approach of Chodorow-Reich and Wieland (2020) calculates average changes across an entire cycle, which is starts in a recession and ends when the level of private employment surpasses its pre-recession level. The other filters are calculated as described in the notes to Figure 2.

Variable	Entire Sample	2000-2008	2009+
Number of loans	165,253	52,933	58,898
Revolving (%)	5.6%	2.5%	0.7%
Working capital/corporate purposes (%)	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 2: 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. "Working capital/corporate purposes (%)" is the share of loans whose reported purpose fell into one of these two categories.

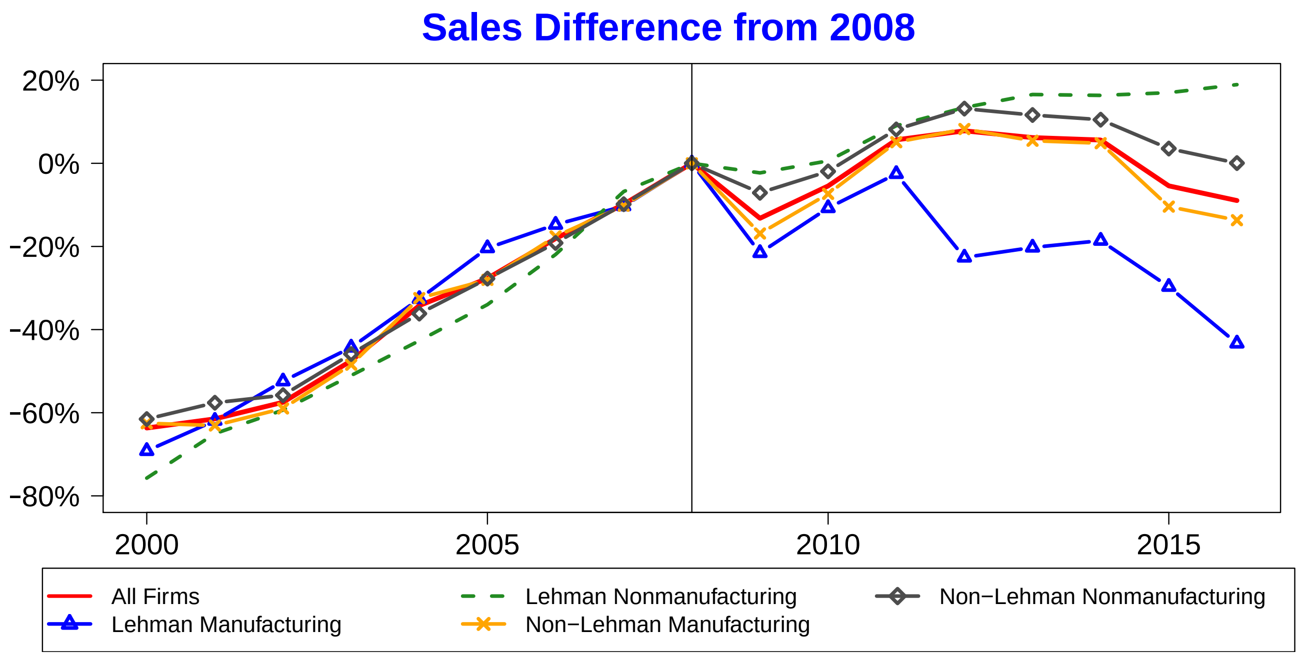


Figure 3: Aggregate Sales Growth Relative to 2008

Note: This figure shows aggregate sales splits based on a firm's industry and whether it had exposure to Lehman Brothers. 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. I exclude firms that entered Compustat after 2000 or exited Compustat prior to 2008. Each line is calculated by taking the sum of all nominal sales for firms in that group, taking logs, and then subtracting the value for each year from the 2008 level for that group.

Variable	Manufacturing Lehman	Manufacturing Non-Lehman	Nonmanufacturing Lehman	Nonmanufacturing Non-Lehman
Sales ($mil)				
Mean	$12,459	$2,335	$10,045	$1,318
Median	$2,839	$ 100	$1,771	$73
Std. Dev.	$25,934	$12,626	$29,520	$5,491
95th Percentile	$53,918	$8,760	$42,089	$5,637
Assets ($mil)				
Mean	$15,282	$2,790	$12,788	$1,703
Median	$3,997	$118	$2,504	$103
Std. Dev.	$38,213	$18,935	$26,132	$8,818
95th Percentile	$63,666	$9,877	$104,694	$6,623
Emp (thous)				
Mean	29.8	7.4	50.9	8.3
Median	9.4	0.5	8.8	0.5
Std. Dev.	54.3	26.6	169.7	34.4
95th Percentile	119.0	162	162.0	37.0
Avg spread (bp)				
Mean	171	203	180	220
Median	175	175	175	200
Std. Dev.	108	164	118	158
95th Percentile	325	500	350	450
Leverage				
Mean	0.42	0.31	0.39	0.36
Median	0.39	0.16	0.37	0.16
Std. Dev.	0.29	0.62	0.27	0.72
95th Percentile	0.98	1.06	0.93	1.17
Profitability				
Mean	0.28	0.27	0.29	0.31
Median	0.23	0.29	0.22	0.25
Std. Dev.	0.16	0.39	0.20	0.40
95th Percentile	0.65	0.84	0.69	1.03
Tobin's Q				
Mean	1.89	4.51	1.93	5.60
Median	1.53	1.91	1.56	1.87
Std. Dev.	0.95	10.42	1.27	13.64
95th Percentile	2.73	13.11	4.11	20.14
# of firms	93	3,789	97	3,802
% with new loan	72.2	17.9	65.0	14.4

Table 3: Summary Statistics from 2004 for Firms Split by Lehman Exposure

Note: I define a firm as being exposed to Lehman Brothers 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 Lehman. Firm characteristics come from Compustat after merging the loan data through the matching process outlined in Chava and Roberts (2008). Interest rate spread calculations include only firms that received loans. Leverage is defined as the ratio of total debt to total assets. Profitibility is defined as sales minus COGS divided by assets. I winsorize the top and bottom 1% of observations for leverage, profitability, and Tobin's Q . "% with new loan" is the percentage of firms who received any new loan in 2004.

	(1)	(2)	(3)	(4)
New Loan Probability (pp)				
$\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i$	16.57***	13.62***	17.37***	16.46***
	(4.087)	(3.404)	(4.712)	(4.250)
$\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{Mfg\}}$	-8.99***	-7.64***	-9.45***	-9.14**
	(2.61)	(2.55)	(2.56)	(3.29)
Sales (%)				
$\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i$	1.38	1.13	1.86	0.16
	(1.54)	(1.16)	(3.35)	(1.93)
$\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{Mfg\}}$	-6.61**	-5.36***	-1.17	-7.67***
	(2.29)	(1.76)	(5.01)	(2.22)
Employment (%)				
$\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i$	1.90	1.18	4.92	0.042
	(2.04)	(1.87)	(4.09)	(1.94)
$\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{Mfg\}}$	-7.11***	-6.99***	-11.37**	-7.33***
	(1.57)	(1.81)	(4.48)	(2.11)
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 4: 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 loan with a reported purpose of either "working capital" or "corporate purposes" in a given year. In the middle and bottom sections, the dependent variables are log sales and log employment, respectively. $Lehman_i$ is a dummy variable capturing whether a firm had at least one revolving credit facility through a syndicate involving Lehman Brothers that was open prior to 2008 and scheduled to extend into 2009 or beyond. 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.

	$\mathbb{1}^{NewLoan}$ (pp)	Sales (%)	Employment (%)
Lagged Sales	-11.97***	-5.42**	-4.03***
	(2.18)	(2.18)	(1.31)
Lagged Employment	-10.88***	-6.76**	-4.15***
	(2.43)	(2.43)	(1.16)
Lagged Assets	-10.67***	-6.33**	-5.37***
	(2.30)	(2.32)	(1.26)
Lagged Leverage	-8.95***	-7.14***	-6.99***
	(2.57)	(2.08)	(1.74)
Lagged Sales and Lagged Assets	-11.86***	-4.62**	-4.44***
	(2.15)	(1.97)	(1.42)
All Lagged Controls	-11.97***	-5.18***	-3.07*
	(2.17)	(1.66)	(1.66)

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

Table 5: Effects of Lehman Exposure with Control Interactions

Note: This table shows estimates of Ω from Equation 1 with additional interaction terms between the controls shown in each row and $\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i$. $\mathbb{1}^{NewLoan}$ is a dummy variable for whether a firm received at least one new loan with a reported purpose of "working capital" or "corporate purposes" in a given year.

	$\mathbb{1}^{NewLoan}$ (pp)	Sales (%)	Employment (%)
$\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{HighValue\}}$	9.92***	3.76**	1.90
	(2.57)	(1.78)	(1.89)

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

Table 6: Effects of Lehman Exposure for Alternative Industry Classification

Note: This table shows estimates of Ω from Equation 1 where the dummy for manufacturing replaced with a "high-value" sector indicator described in Section 3.4. $\mathbb{1}^{NewLoan}$ is a dummy variable for whether a firm received at least one new loan with a reported purpose of "working capital" or "corporate purposes" in a given year.

	Estimated effect of IBD
Manufacturing employment share (pp)	-0.13**
	(0.052)
Manufacturing employment (%)	0.65
	(0.48)
Nonmanufacturing employment (%)	1.22***
	(0.28)

Standard errors clustered at the state level in parentheses

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

Table 7: Effect of IBD on Employment

Note: This table estimates the effects of IBD using the approach of De Chaisemartin and d'Haultfoeuille (2020). In the first row, the dependent variable is the share of manufacturing employment to total employment measured in percentage points. In the second and third rows, the dependent variable is employment in the manufacturing and nonmanufacturing sectors in log points multiplied by 100, so that a value of 1 corresponds to a 1% increase. Standard errors are calculated based on 100 bootstrap draws clustered at the state level. DE and SD are not included. The regressions use data from the BEA at the state-year level from 1975-1996.

Effect on employment shares (pp)	(1)	(2)	(3)
$Dereg_{j,t}$	0.098***	-0.024**	0.003
	(0.017)	(0.011)	(0.016)
$Dereg_{j,t} \times HHI_i$	-0.059***		-0.016**
	(0.009)		(0.008)
$Dereg_{j,t} \times SC_{i,-j}$		0.393***	0.391***
		(0.012)	(0.012)
R^2	0.001	0.093	0.094
N	77616	77616	77616

Standard errors clustered at the state-by-industry level shown in parentheses

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

Table 8: Comparing Tradability and Exposure to Long-Run Change

Note: This table shows the results of estimating Equation 3. Employment data at the state-industry-year (i, j, t) level come from the BEA. Regressions include data from 1975-1996. $Dereg_{j,t}$ is a dummy variable equal to one after state j implements IBD and zero otherwise. HHI_i is the geographic concentration index for industry i calculated across all states in 1975. $SC_{i,-j}$ is the change in the employment share for industry i between 1970 and 2000 for the US excluding state j. Negative values of $SC_{i,-j}$ correspond to industries which have become smaller over time. Interaction coefficients are standardized so that the values for the interaction coefficients for $SC_{i,-j}$ and HHI_i represent the marginal effect of a one standard deviation increase (calculated across all states) of each variable. Standard errors are clustered at the state-by-industry level.

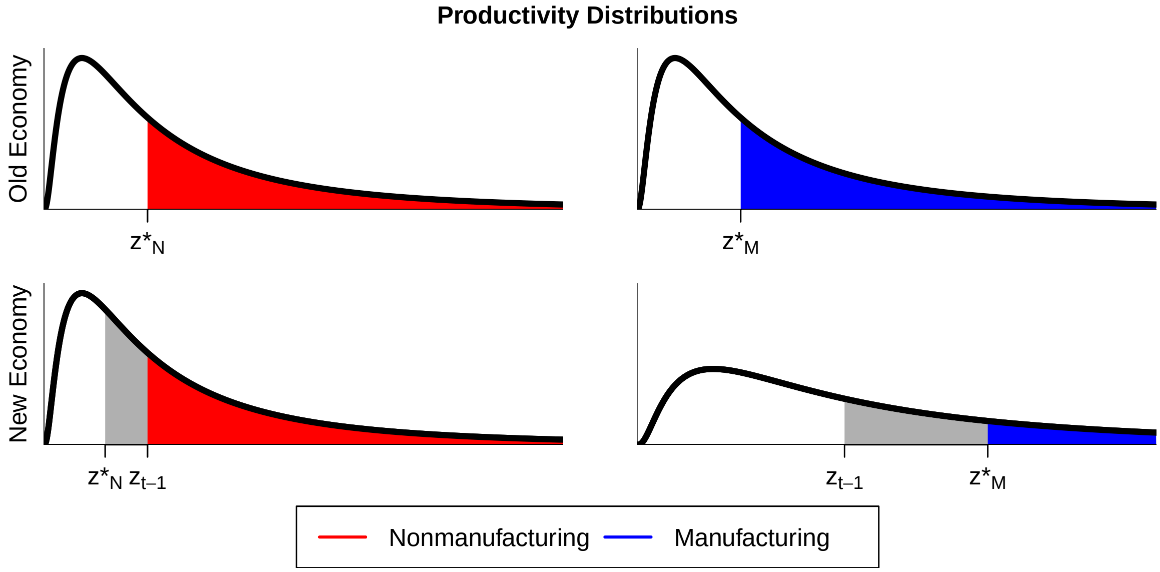


Figure 4: Model Productivity Distributions

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 9: Model Parameter Values

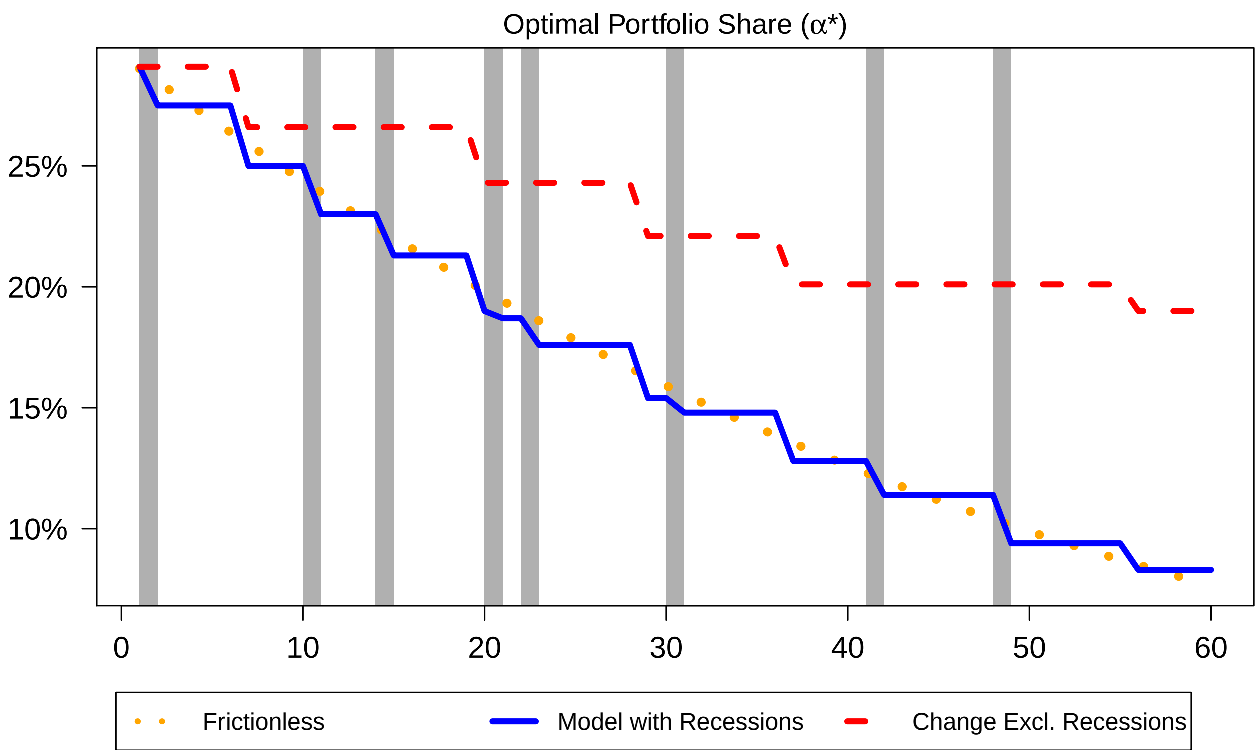


Figure 5: Model with Recessions

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 9.

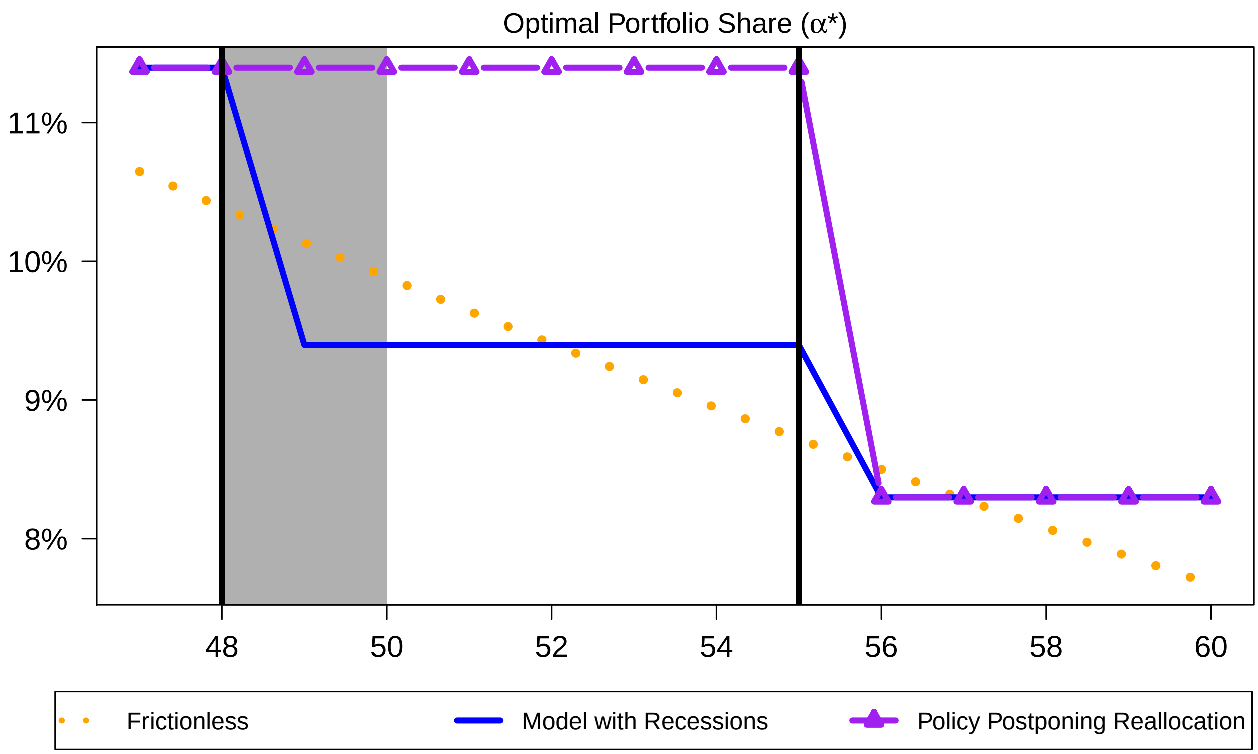


Figure 6: Effects of Policy Preventing Reallocation

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 9.

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 details of the trend-cycle decompositions used in Section 2.2. Appendix D describes the various sources of data and their construction. Appendix E includes a series of extensions and robustness checks for the results based on the collapse of Lehman Brothers described in Section 3 of the paper. Appendix F does the same for the results based on interstate banking deregulation in Section 4. Finally, Appendix G includes additional figures and results from the model described in Section 5.

A Structural Change and Recessions

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

Further evidence for this phenomenon is summarized in Table A.1. The middle three columns show the shares coming from manufacturing for each of these series at the start of 1960, the end of 2018, and the percentage point change over this period. The "Recession Δ" column shows the total change that occurred in each series during years that had a recession. These calculations are based on years that include at least one recession to allow direct comparison across activity measures since some are only available annually. The rightmost column shows the share of the total change over this period that occurred during these years. If the total change from 1960-2018 for each of these series were distributed uniformly across time, the "Ratio" column would show about 0.22 for all variables because that is the unconditional probability of a recession occurring over this period. Instead, this ratio is about one-half for employment and output, more than two-thirds for value added, and almost 0.9 for consumption.

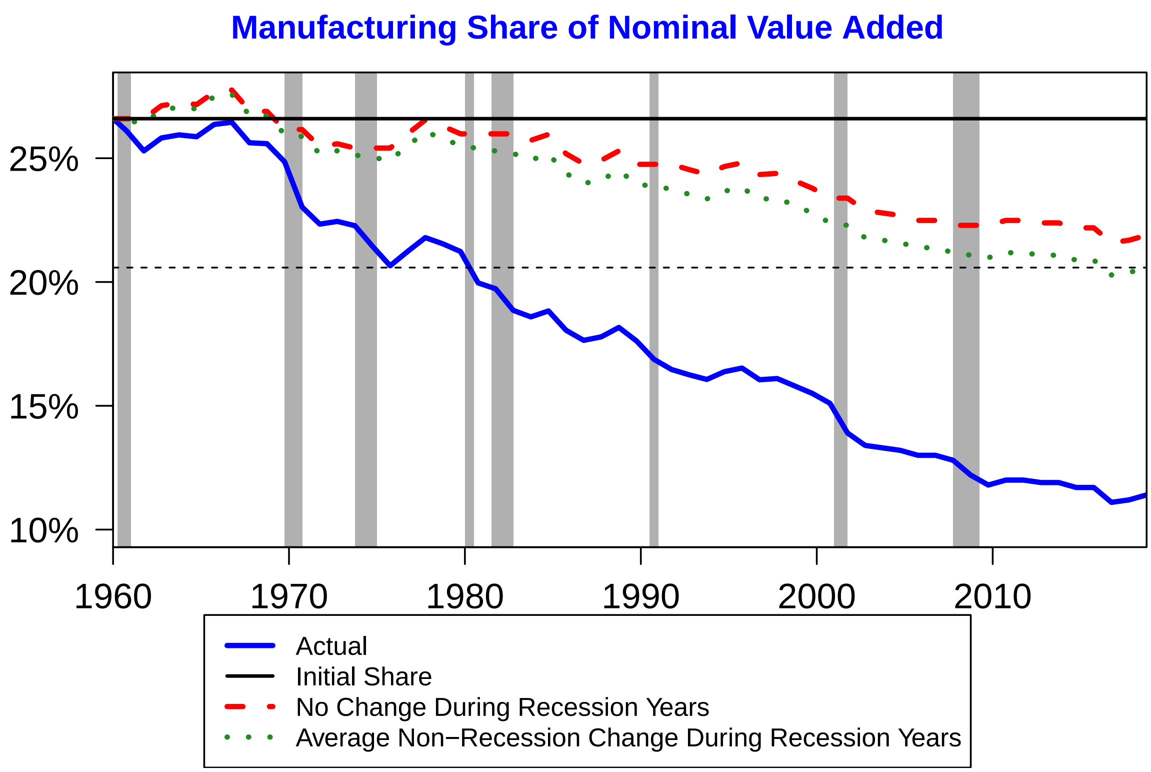


Figure A.1: Change in Manufacturing Share of US Nominal Value Added, 1960-2018

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

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

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

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

B Model Mechanism

The mechanism at the core of my model is represented graphically in Figure B.1. Panel (a) shows a collection of manufacturing and service firms. Firms must obtain credit through a banking relationship—the initial formation of which incurs a fixed cost—in order to produce. Firms receiving credit through banking relationships are shown inside the green border representing the bank and are shaded in. Firms who do not have banking relationships (and are thus unable to produce) are represented by the dashed, empty squares outside of the bank. Over time, structural change increases the value of providing credit to nonmanufacturing firms. This is shown in panel (b). This mechanism does not rely on any one specific cause to drive this structural change; it requires only that it the share of productive resources being allocated to the manufacturing sector declines over time.[14] Fixed adjustment costs to forming new banking relationships mean that credit will not immediately shift to nonmanufacturing firms even though structural change has made them more valuable.

Panels (c) and (d) of Figure B.1 illustrate the destruction of a firm-bank relationship and its consequences. One way for this destruction to occur is if a bank collapses. This is represented by the inward shift of the solid green line marking the firms in relationships and the dashed green border illustrating the firms who are forced to shut down because they are no longer receiving credit. If this destruction is not permanent, new bank credit will eventually be made available again, which is represented by the rightward expansion of the bank border to its original position in panel (d). Firm exit will also lead to separation of firm-bank matches. In this setting, it is only the destruction of the match that matters for credit reallocation.

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

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

Section 3 of the main paper using the bankruptcy of Lehman Brothers.

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

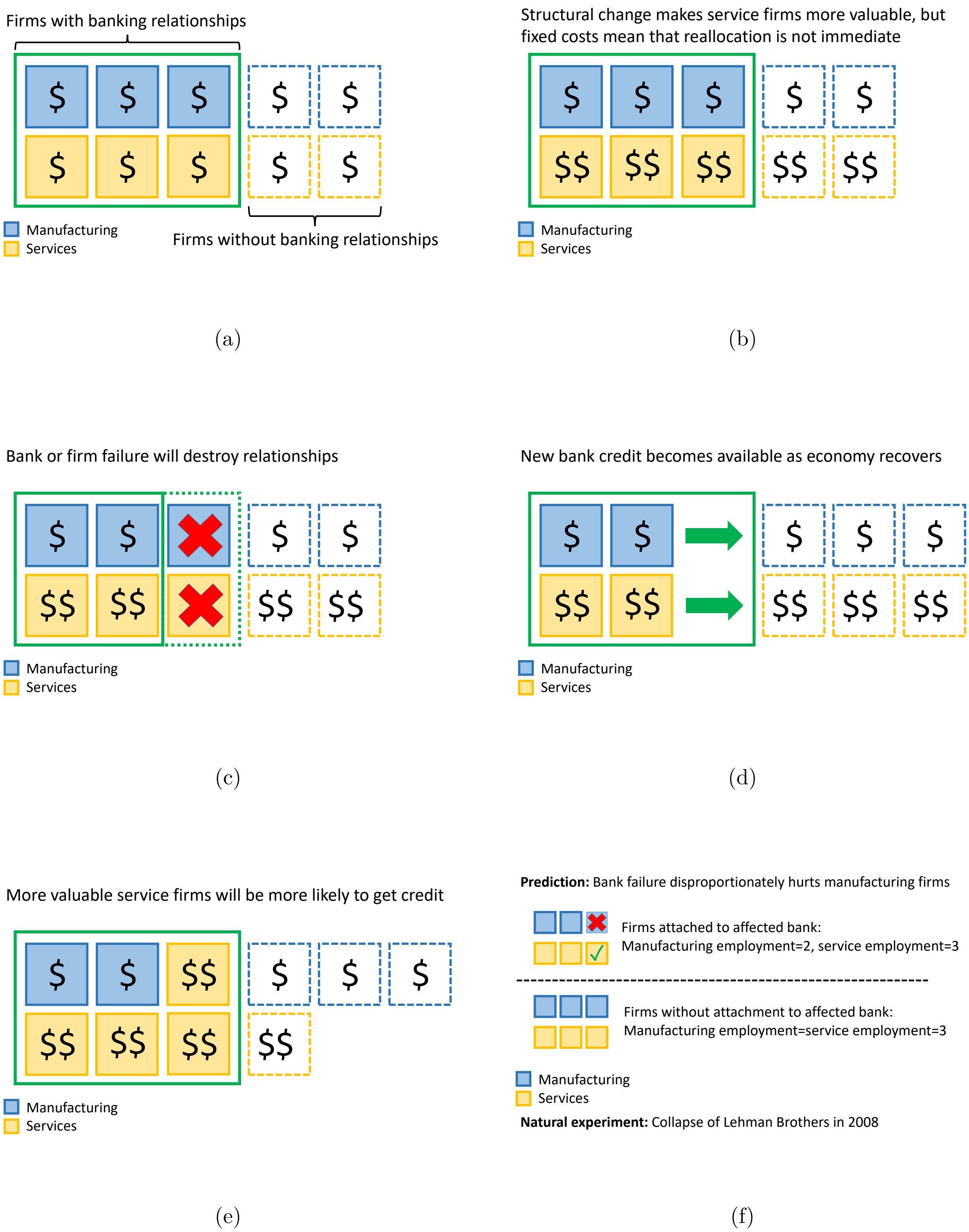
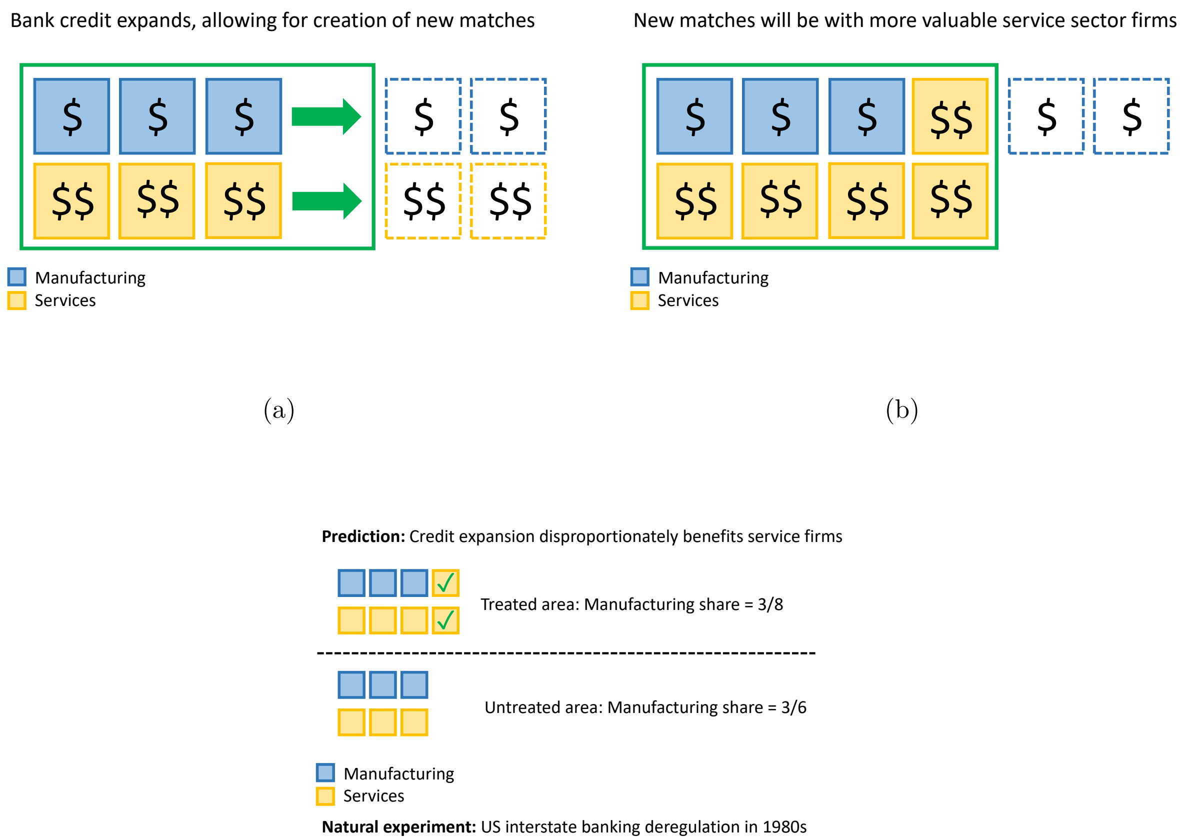


Figure B.1: Illustration of Bank Failure and Structural Change



(a) (b)

(c)

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

C Details of Trend-Cycle Decomposition

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

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

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

$$\Delta^S_R = \frac{\Delta^S - (1 - \theta_R)\Delta^S_E}{\theta_R} = \frac{0.13 - (1 - 0.404)\Delta^S_E}{0.404} \tag{13}$$

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

D Data Description

D.1 Example Syndicated Loan

Figure D.1: Example Syndicated Loan

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

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

D.2 DealScan Description and Sample Construction

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

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

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

From this sample I create variables representing the type of loan based on the classification of Ivashina and Scharfstein (2010). My sample includes loans with reported uses of either working capital or general corporate purposes. I drop firms in the finance (SIC codes 6000-6700), public administration (9100-9700), and utility (4900-5000) sectors. This leaves 465,423 observations.

I classify a facility as being involved with Lehman Brothers if any of the following are listed as the lender: Lehman Brothers Inc, Lehman Brothers Holdings Inc, Lehman Commercial Paper Inc, Lehman Brothers Bank FSB, Lehman Brothers Commercial Bank, Lehman Commercial Paper Inc, or Lehman Bank Inc.

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

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

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

Goldman Sachs includes any of the following lenders: Goldman Sachs & Co, Goldman Sachs Credit Partners LP, Goldman Sachs Bank USA, Goldman Sachs Capital Partners, or Goldman Sachs Lending Partners LLC.

Morgan Stanley includes any of the following lenders: Morgan Stanley, Morgan Stanley MUFG Loan Partners LLC, Morgan Stanley Senior Funding Inc, Morgan Stanley Bank, Morgan Stanley Bank NA, Morgan Stanley Dean Witter & Co, Morgan Stanley Group, Morgan Stanley Dean Witter Prime Income Trust, Morgan Stanley & Co International, Morgan Stanley Bank AG, Morgan Stanley Prime Income-Trust, or Morgan Stanley High-Yield Fund.

JP Morgan includes any of the following lenders: JP Morgan, JP Morgan Chase Bank NA, JP Morgan & Co, JP Morgan Chase, JP Morgan Delaware, or JP Morgan Securities Inc.

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

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

D.3 Banking Deregulation Data

D.3.1 Interstate Banking Deregulation Dates

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

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

Table D.1: Dates of Interstate Banking Deregulation

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

E Robustness Checks and Additional Lehman Results

E.1 Comparison to Lehman's Peers

This section provides evidence that the firms attached to Lehman Brothers were, on the whole, indistinguishable from those who had similar relationships with other large banks who participated in syndicated loan markets. I choose Goldman Sachs, Morgan Stanley (MS), and JP Morgan (JPM) for this exercise. Goldman and MS in particular were US-based institutions with a very similar market position. JPM had a market share roughly six times larger than these three other institutions combined and is included for comparison because its clients are more likely to be representative of the general population of firms receiving syndicated loans.[15] Summary statistics for firms with attachment to one of these banks are found in Tables E.1, E.2, and E.3.

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

The average rate across all loans paid by firms with attachment to one of Lehman's competitors but not Lehman, represented by the solid red lines, were very similar to the average rates paid by firms that had both.[16] 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.

[15]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.

[16]I classify a firm as having both Lehman and Goldman attachment even if this exposure occurs through separate facilities.

Variable	**Manufacturing** Goldman	Non-Goldman	**Nonmanufacturing** Goldman	Non-Goldman
Sales ($mil)	$9,459	$2,393	$8,247	$1,416
Assets ($mil)	$14,841	$2,781	$8,372	$1,861
Emp (thous)	26.0	7.4	46.5	8.7
# of firms	94	3,786	72	3,812
% with new loan	68.5	18.3	66.7	14.4

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

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

Variable	**Manufacturing** JPM	Non-JPM	**Nonmanufacturing** JPM	Non-JPM
Sales ($mil)	$6,835	$1,818	$5,435	$1,038
Assets ($mil)	$9,463	$1,957	$5,796	$ 1,490
Emp (thous)	22.1	5.2	31.0	6.2
# of firms	567	3,313	438	3,446
% with new loan	61.6	12.0	60.6	9.9

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

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

Variable	Manufacturing MS	Manufacturing Non-MS	Nonmanufacturing MS	Nonmanufacturing Non-MS
Sales ($mil)	$15,148	$2,147	$10,745	$1,289
Assets ($mil)	$19,999	$2,514	$13,082	$1,677
Emp (thous)	42.1	6.7	59.8	7.8
# of firms	122	3,758	103	3,781
% with new loan	69.6	17.8	67.3	14.0

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

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

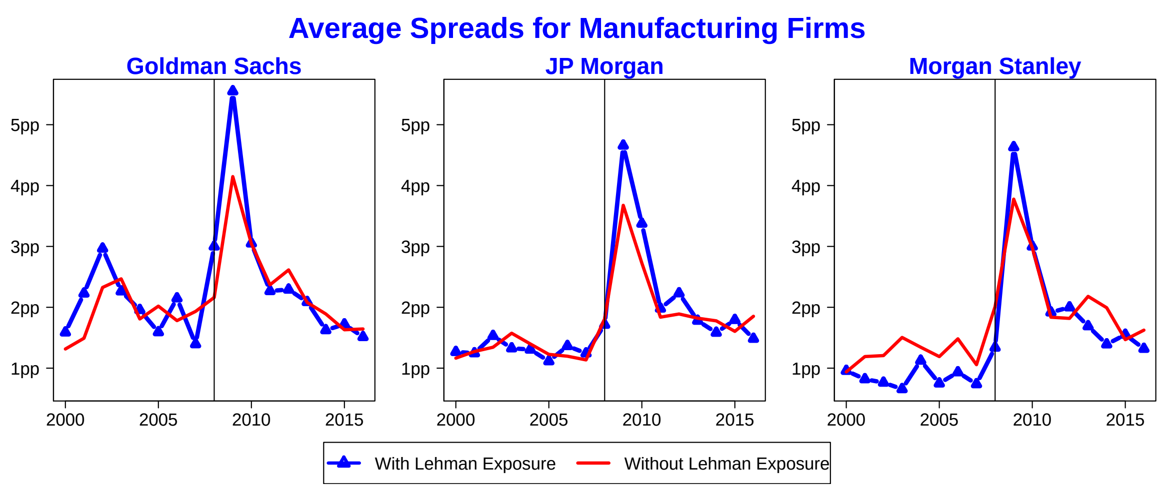


Figure E.1: Average interest rate splits by bank attachment

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

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

Interpreting these results is complicated by the fact that many firms, especially large ones, have multiple credit lines with multiple different banks. As a result, many of the firms counted in the Lehman line will also be counted in those of other banks. Thus to decompose these results even further, I can isolate the firms who had relationships with the other banks but not with Lehman Brothers. These results are shown in Figure E.3. The blue line with triangles shows the same Lehman aggregate series as in Figure E.2. The other lines show sales for manufacturing firms who had lines of credit through syndicates that included at least one of Lehman's competitors but not Lehman. These results provide further evidence that Lehman attachment had a pronounced impact even relative to firms in the same industry with attachment to other, similar banks.

As a final comparison, Figure E.4 shows the same splits for Lehman's competitors as Figure E.2 but for nonmanufacturing firms. Unlike the manufacturing series, these series all trend very similarly both before and after the crisis. This provides direct evidence against the idea that Lehman Brothers was systematically more likely to provide financing to firms that were ultimately more likely to fail.

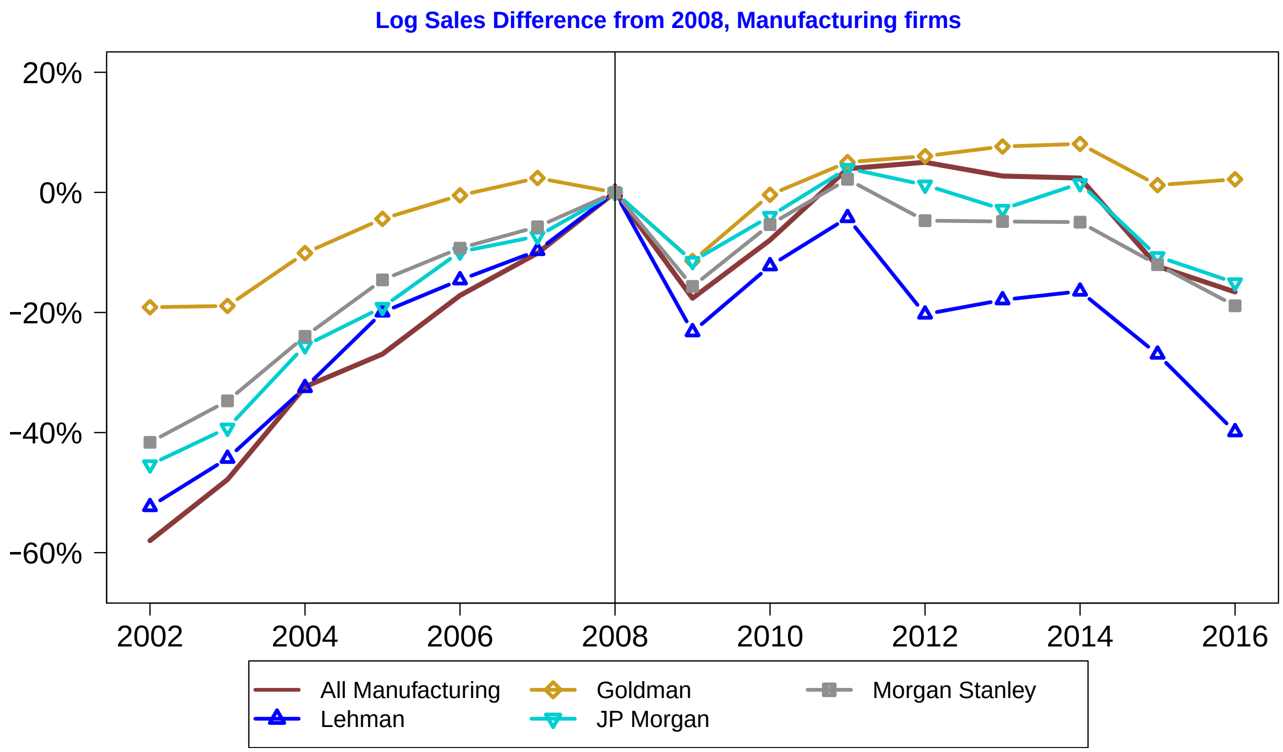


Figure E.2: Aggregate Sales Growth Relative to 2008

Note: This figure plots the log of total sales for manufacturing firms split by their bank attachment. A firm is classified as having attachment to Lehman, Goldman, JP Morgan, or Morgan Stanley if it had a revolving line of credit through a syndicate that included that bank which started prior to 2008 and was scheduled to extend into 2009 or later. I exclude firms that entered Compustat after 2000 or exited Compustat prior to 2008. Each line is calculated by taking the sum of all nominal sales for firms in that group, taking the log, and then subtracting the value for each year from the 2008 level for that group.

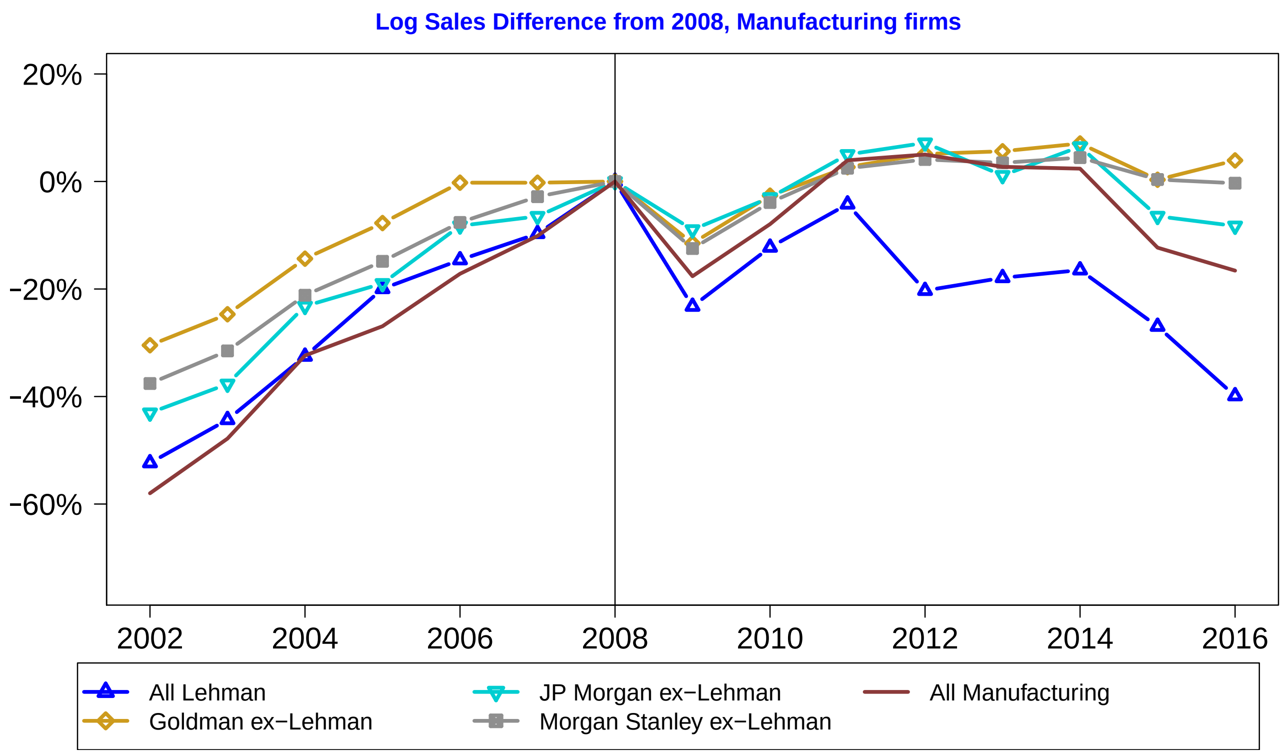


Figure E.3: Aggregate Sales and Employment Growth Relative to 2008

Note: This figure plots the log of total sales for manufacturing firms split by their bank attachment. Bank attachment is defined as having a revolving line of credit through a syndicate that included that bank which started prior to 2008 and was scheduled to extend into 2009 or later. I exclude firms that entered Compustat after 2000 or exited Compustat prior to 2008. The "ex-Lehman" series correspond to the set of firms that were exposed to that bank but not to Lehman. The "Only Lehman" series represents the set of firms who were exposed to Lehman but not to Goldman, JP Morgan, or Morgan Stanley. Each line is calculated by taking the sum of all nominal sales for firms in that group, taking the log, and then subtracting the value for each year from the 2008 level for that group.

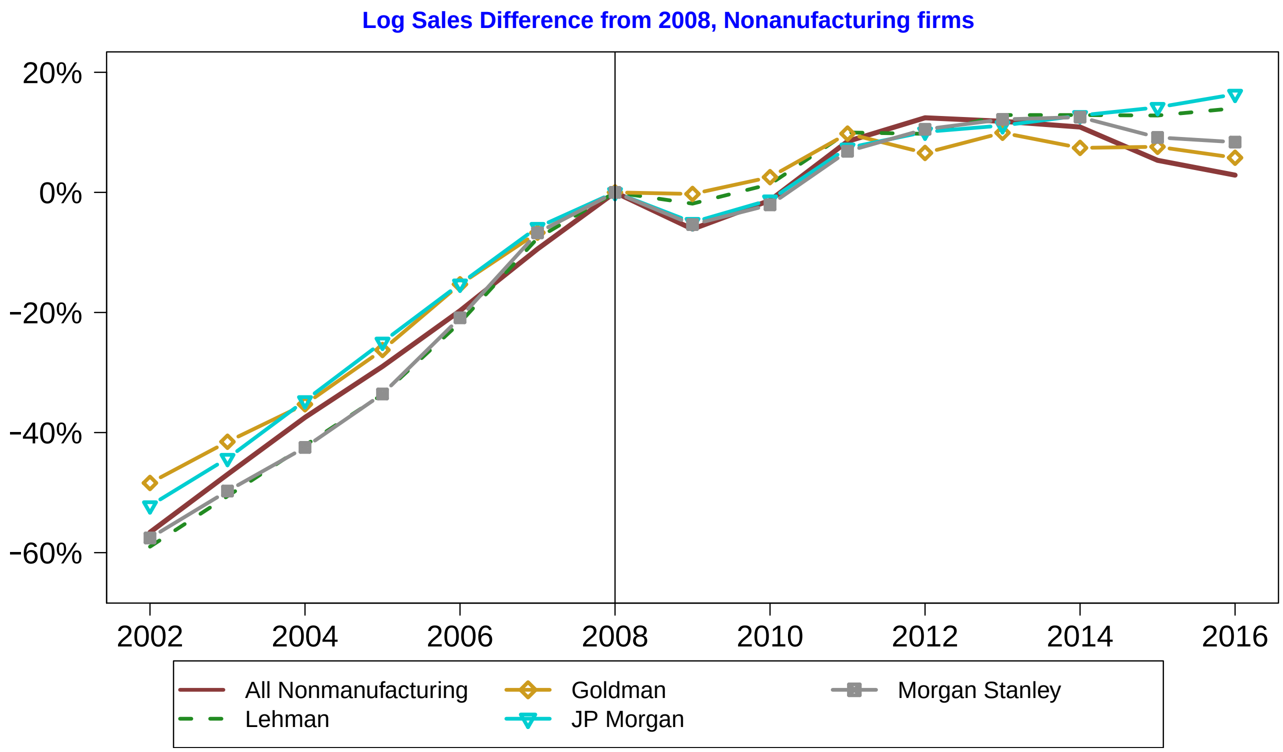


Figure E.4: Aggregate Sales and Employment Growth Relative to 2008

Note: This figure plots the log of total sales for nonmanufacturing firms split by their bank attachment. A firm is classified as having attachment to Lehman, Goldman, JP Morgan, or Morgan Stanley if it had a revolving line of credit through a syndicate that included that bank which started prior to 2008 and was scheduled to extend into 2009 or later. I exclude firms that entered Compustat after 2000 or exited Compustat prior to 2008. Each line is calculated by taking the sum of all nominal sales for firms in that group, taking the log, and then subtracting the value for each year from the 2008 level for that group.

E.2 Robustness Checks For Bank Exposure Results

E.2.1 Probability of Receiving Any New Loan Facility

In my baseline specification, I estimated the effects of Lehman attachment on the probability of receiving a loan for either working capital or corporate purposes. Table E.4 below shows the estimation results using any loan facility. The coefficient estimates reflect the change in probability of receiving at least one new loan of any type in a given year caused by exposure to Lehman brothers at the time of its collapse. The effect on nonmanufacturing firms, which was positive in my main specification, becomes much smaller and statistically insignificant. The effects for manufacturing firms remain negative and statistically significant, however, and the magnitudes are similar to my baseline results. This suggests that credit reallocation from manufacturing to nonmanufacturing firms was not restricted to a particular type of loan.

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

Driscoll-Kraay standard errors in parentheses

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

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

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

E.2.2 Alternate Measures of Lehman Exposure

In my baseline specification, I use a dummy variable representing whether a firm had an line of credit through Lehman open prior to 2008 and scheduled to extend into at least 2009 as my measure of Lehman exposure. In this section, I show that my main results are robust to several alternative measures. The first is a continuous measure representing the number of loans involving Lehman as classified previously. The second measure counts only the number of facilities in which Lehman was reported as having a role beyond "Participant". Finally, the third measure calculates the total volume of available credit through revolving facilities involving Lehman scaled by the average sales of each firm from 2006-2008; for this measure, coefficients capture the effect of a 1pp increase in this ratio. The results are shown for my baseline specification (corresponding to the first column of the other regression tables). The top of each column shows the outcome variable being referenced. These measures all show qualitatively similar results, suggesting that the differential effects of Lehman exposure for manufacturing firms are not driven by a specific measurement approach.

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

Driscoll-Kraay standard errors in parentheses

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

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

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

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

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

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

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

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

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

Note: These tables show the results of estimating Equation 1 in the main paper where $Lehman_i$ is measured as the number of revolving facilities exposed to Lehman's collapse in which Lehman had a role beyond "Participant" in Table E.6, and measured as the ratio of the total value of all revolving credit facilities involving Lehman that started prior to 2008 and extended into 2009 or beyond divided by the firm's average sales from 2006 through 2008 (in percent) in Table E.7. Coefficients in Table E.7 correspond to the effect of a 1pp increase in this ratio. The effects on probability of getting a new loan are measured in percentage points, while the effects on sales and employment are measured in percentages.

E.2.3 Controlling for Exposure to Lehman's Peers

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

$$\begin{aligned} Y_{i,t} = \alpha_i + \sigma_t + \mathbb{1}_{\{Mfg\}} \times \theta_t + \gamma X_{i,t-1} + \\ \sum_i \Big(\rho_i \times \mathbb{1}_{\{Year \geq 2009\}} \times Bank_i\Big) + \sum_i \Big(\Omega_i \times \mathbb{1}_{\{Year \geq 2009\}} \times Bank_i \times \mathbb{1}_{\{Mfg\}}\Big) + \epsilon_{i,t} \end{aligned} \tag{14}$$

In the case of the loan for Ford shown in Figure D.1, for example, all four banks were involved in the syndicate. Table E.8 compares these effects for sales and shows that, even after controlling for exposure to Goldman Sachs, Morgan Stanley, and JP Morgan, Lehman firms were adversely affected.

	Goldman	MS	JP Morgan	Lehman
$\mathbb{1}_{\{Year \geq 2009\}} \times Bank_i$	0.871	-2.080	-2.791	3.020**
	(0.818)	(1.665)	(1.956)	(1.279)
$\mathbb{1}_{\{Year \geq 2009\}} \times Bank_i \times \mathbb{1}_{\{Mfg\}}$	1.753	-0.106	-2.791	-4.698**
	(1.581)	(1.681)	(1.956)	(1.805)

Specification (1); Driscoll-Kraay standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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

Note: This table shows the results of estimating Equation 14 where the dependent variable is log sales. Estimates come from the baseline specification, which corresponds to the first column of the other regressions using Lehman exposure. $Bank_i$ is a dummy variable equal to one if a firm had at least one revolving line of credit though syndicates that included each bank starting before 2008 and extending into 2009 or beyond.

E.2.4 Comparisons to Pre-Crisis Lehman Loans

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

	(1)	(2)	(3)	(4)
$\mathbb{1}_{\{Year \geq 2009\}} \times LehmanPreCrisis_i$	0.421	0.0684	0.593	0.342
	(0.610)	(0.557)	(0.669)	(0.627)
$\mathbb{1}_{\{Year \geq 2009\}} \times LehmanPreCrisis_i \times \mathbb{1}_{\{Mfg\}}$	0.527	0.637	0.250	0.542
	(0.459)	(0.428)	(0.402)	(0.409)
Controls	Y	Y	N	Y
Loans>0	N	Y	N	N
2016 Survivors	N	N	N	Y
N	69940	44422	84061	37486

Driscoll-Kraay standard errors in parentheses

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

Table E.9: Effects of Pre-Crisis Lehman Exposure on Probability of Obtaining New Loan (pp)

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

E.2.5 Controlling for Pre-Crisis Spreads

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

$$\begin{aligned} Y_{i,t} = \alpha_i + \sigma_t + \mathbb{1}_{\{Mfg\}} \times \chi_t + \gamma X_{i,t-1} + \\ \rho \times \mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i + \\ \Omega \times \mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{Mfg\}} + \\ \xi \times \mathbb{1}_{\{Year \geq 2009\}} \times Spread_i^{2000-2007} + \\ \lambda \times \mathbb{1}_{\{Year \geq 2009\}} \times Spread_i^{2000-2007} \times \mathbb{1}_{\{Mfg\}} + \epsilon_{i,t} \end{aligned} \tag{15}$$

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

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

Driscoll-Kraay standard errors in parentheses

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

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

Note: This table shows the results of estimating Equation 15 where the dependent variable is a dummy variable indicating whether a firm received a new loan facility in a given year. $Lehman_i$ represents the total number of revolving credit facilities though syndicates that included Lehman Brothers starting before 2008 and extending into 2009 or beyond. $Spread_i^{2000-2007}$ represents the average interest rate "all-in-drawn" spread paid by firm i on loans with a start date between 2000 and 2007.

E.2.6 Time to Next Loan

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

$$\begin{aligned} TTL_{i,t} = {} & \alpha_i + \sigma_t + \mathbb{1}_{\{Mfg\}} \times \chi_t + \gamma X_{i,t-1} + \xi Z_{i,t-1} \\ & \rho \times \mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i + \\ & \Omega \times \mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{Mfg\}} + \epsilon_{i,t} \end{aligned} \tag{16}$$

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

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

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

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

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

Note: This table shows the results of estimating Equation 16. The dependent variable is the number of years until a firm receives a new loan with a reported purpose of either "working capital" or "corporate purposes". $Lehman_i$ is a dummy variable capturing whether a firm had at least one revolving credit facility through a syndicate involving Lehman Brothers that was open prior to 2008 and scheduled to extend into 2009 or beyond.

E.2.7 Robustness to Outliers

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

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

Driscoll-Kraay standard errors in parentheses

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

Table E.12: Estimates after Excluding Sales Outliers

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

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

Driscoll-Kraay standard errors in parentheses

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

Table E.13: Estimates after Excluding Asset Outliers

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

E.3 Aggregate Results

Section 3 of the main paper showed that manufacturing firms were disproportionately affected by Lehman's bankruptcy by using firm-level variation across time, sector, and bank exposure. This section supplements those results with additional exercises that provide support for the existence of the credit reallocation channel.

E.3.1 Credit Reallocation Across Sectors

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

$$Y_{i,t} = \alpha_i + \sigma_t + \gamma X_{i,t-1} + \beta \times \mathbb{1}_{\{Mfg\}} \times \mathbb{1}_{\{Year \geq 2009\}} + \epsilon_{i,t} \tag{17}$$

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.[17] The baseline results are shown in Table E.14. The first column corresponds to my preferred specification and implies that a manufacturing firm was approximately 1.1pp less likely to receive a new loan post-2009 relative to a nonmanufacturing firm. Given that the unconditional probability of obtaining a loan in any given year in the early 2000s was approximately 10-15% across all firms, this represents a substantial effect. Columns 2-4 represent alternative specifications that restrict the sample to firms which had at least one observed loan in DealScan (column 2), exclude the firm-level controls (column 3), or use only firms which showed up in Compustat throughout the entire sample (column 4).

The reduction in the probability of obtaining a loan had a significant effect on the total volume of credit each firm obtained. To show this, I modify the dependent variable in Equation 17 to be

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

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

Based on the loan probability results, at least some of this reduction in new loan volume comes through the extensive margin (fewer new loans). In principle, the intensive margin (a change in the size of loans issued) could also be responsible for the change in average loan volume. In practice this does not appear to be the case. Table E.16, which conditions on observations in which firms receive a loan, shows that the estimated volume of credit actually goes *up* for manufacturing firms relative to nonmanufacturing firms. Table E.17 shows that the estimated effects on loan maturity are insignificant and quite small; the dependent variable is in levels, not logs, so the estimated effect is less than one month in all specifications.

	(1)	(2)	(3)	(4)
$\mathbb{1}_{\{Mfg\}} \times \mathbb{1}_{\{Year \geq 2009\}}$	-1.072*	-2.329***	-0.945*	-1.175
	(0.552)	(0.755)	(0.572)	(0.815)
Controls	Y	Y	N	Y
Loans>0	N	Y	N	N
2016 Survivors	N	N	N	Y
N	69940	44422	84061	37486

Driscoll-Kraay standard errors in parentheses

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

Table E.14: Effects on Probability of Obtaining New Loans (pp)

Note: Table E.14 shows the results of estimating Equation 17 where the dependent variable is a dummy variable indicating whether a firm received a new credit facility with a reported purpose of "working capital" or "corporate purposes" in a given year in percentage points. The first column represents my baseline specification, which includes data from 2000-2016 and includes only firms which were in Compustat by the start of this period. The second column restricts the sample of firms to only those who were matched to at least one loan in DealScan, regardless of when it occurred. The third column excludes the firm-level controls. The fourth column restricts the sample to only the set of firms who were observed in Compustat in at least one year in 2016 or later.

	(1)	(2)	(3)	(4)
$\mathbb{1}_{\{Mfg\}} \times \mathbb{1}_{\{Year \geq 2009\}}$	-17.13	-20.29	-29.44**	-14.36
	(12.94)	(19.89)	(12.21)	(13.82)
Controls	Y	Y	N	Y
Loans>0	N	Y	N	N
2016 Survivors	N	N	N	Y
N	69940	44422	84061	37486

Driscoll-Kraay standard errors in parentheses

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

Table E.15: Effects on Total Value of All New Loans (%)

	(1)	(2)	(3)	(4)
$\mathbb{1}_{\{Mfg\}} \times \mathbb{1}_{\{Year \geq 2009\}}$	10.24**		5.521	11.69***
	(3.726)		(4.753)	(3.514)
Controls	Y		N	Y
2016 Survivors	N		N	Y
N	13545		14220	8326

Driscoll-Kraay standard errors in parentheses

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

Table E.16: Effects on Total Loan Value Conditional on Receiving a Loan (%)

Note: Table E.15 shows the results of estimating Equation 17 where the dependent variable is the log of the total volume of new credit facilities obtained by a firm in a given year. Table E.16 shows the same results, but conditions on only observations where loans were received. The first column represents my baseline specification, which includes data from 2000-2016 and includes only firms which were in Compustat by the start of this period. The second column, which in other regressions restricts the sample of firms to only those who were matched to at least one loan in DealScan, is omitted here because conditioning on receiving a loan trivially restricts the sample to firms who had ever received a loan. The third column excludes the firm-level controls. The fourth column restricts the sample to only the set of firms who were observed in Compustat in at least one year in 2016 or later.

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

Driscoll-Kraay standard errors in parentheses

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

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

Note: This table shows the results of estimating Equation 17 where the dependent variable is the maturity of the loan (in months). The estimates include only firm-year observations in which at least one new loan was received. The first column represents my baseline specification, which includes data from 2000-2016 and includes only firms which were in Compustat by the start of this period. The second column, which in other regressions restricts the sample of firms to only those who were matched to at least one loan in DealScan, is omitted here because conditioning on receiving a loan trivially restricts the sample to firms who had ever received a loan. The third column excludes the firm-level controls. The fourth column restricts the sample to only the set of firms who were observed in Compustat in at least one year in 2016 or later.

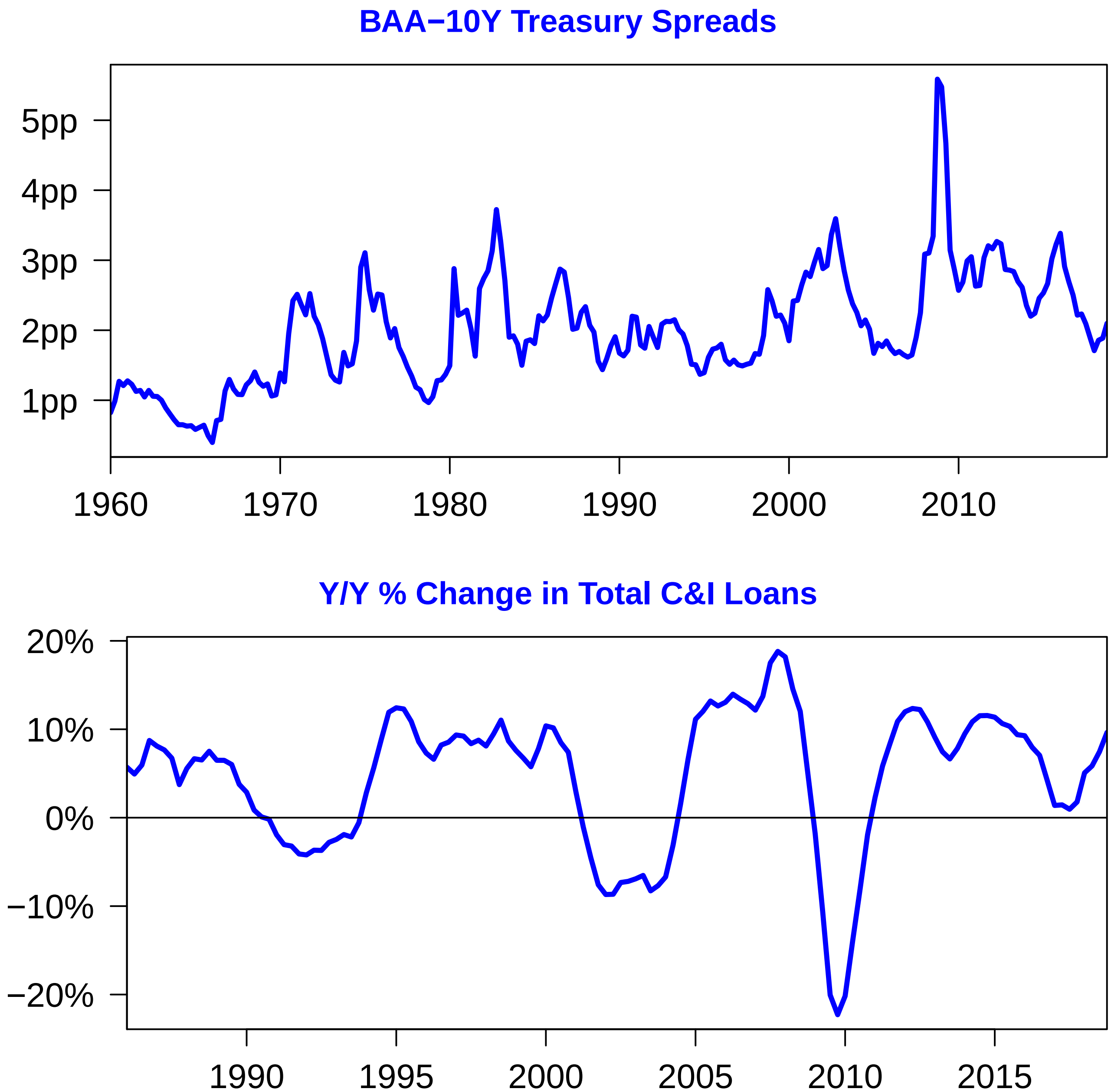


Figure E.5: Corporate Loan Spreads and C&I Loan Growth

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

F Robustness Checks and Additional IBD Results

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

F.1 Comparing Pretrends

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

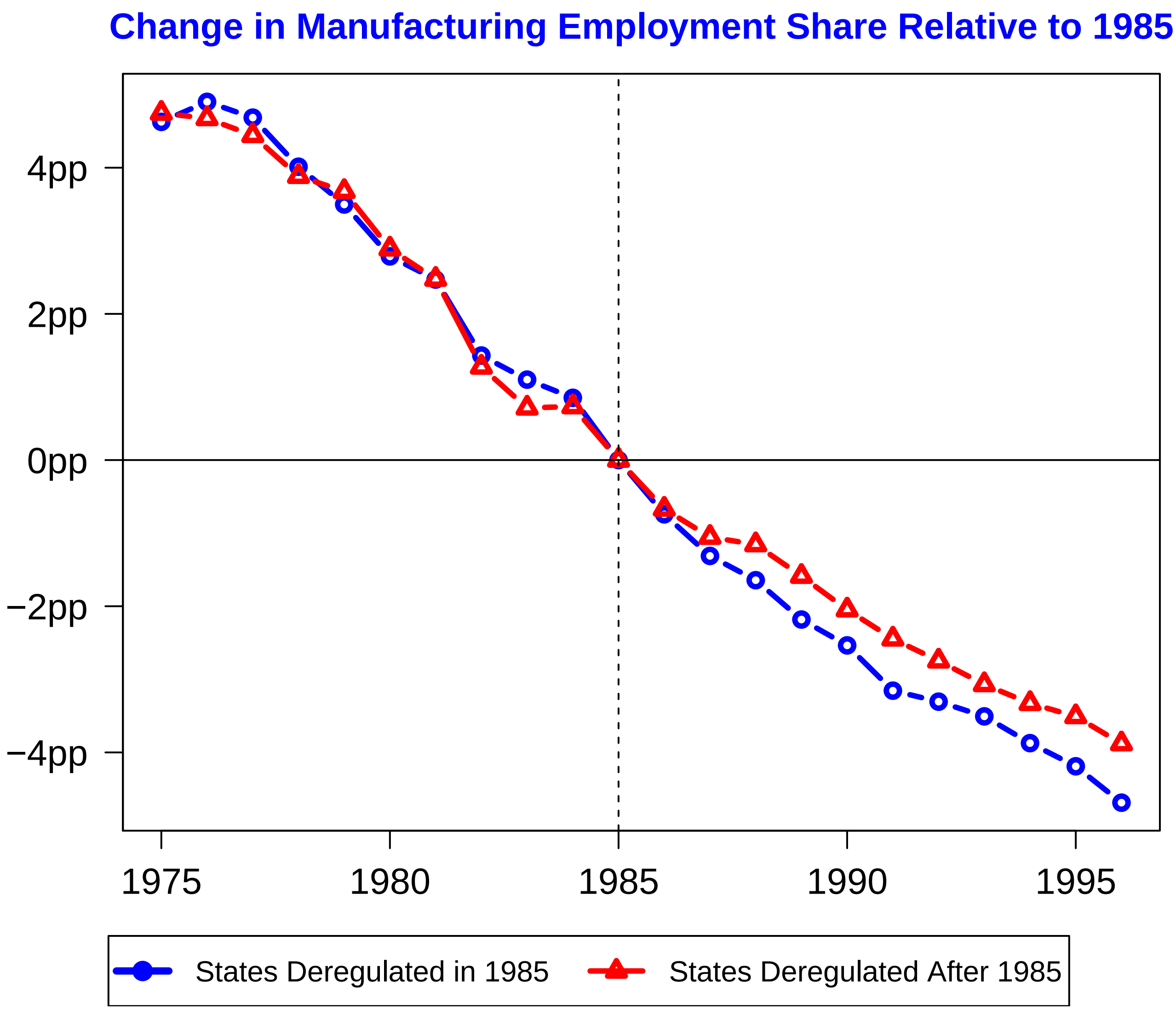


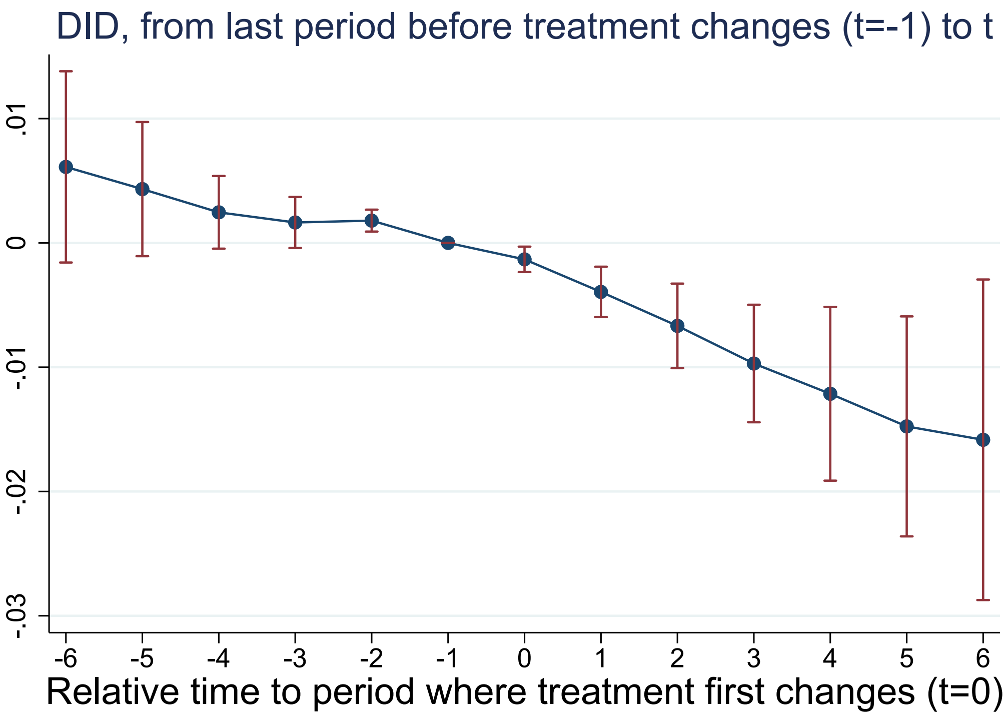
Figure F.1: Comparing Pretrends in Manufacturing Employment Share

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

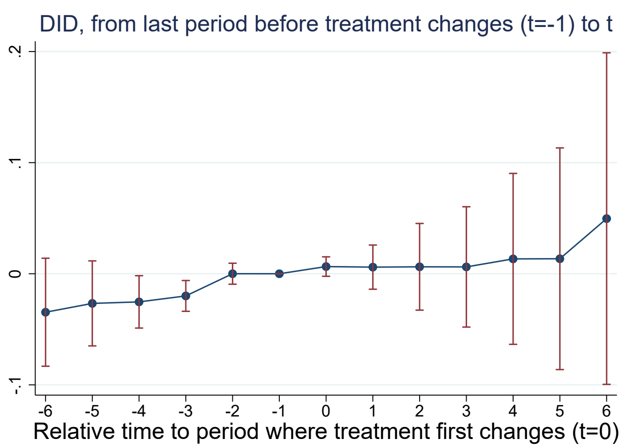
F.2 Dynamic Estimates

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

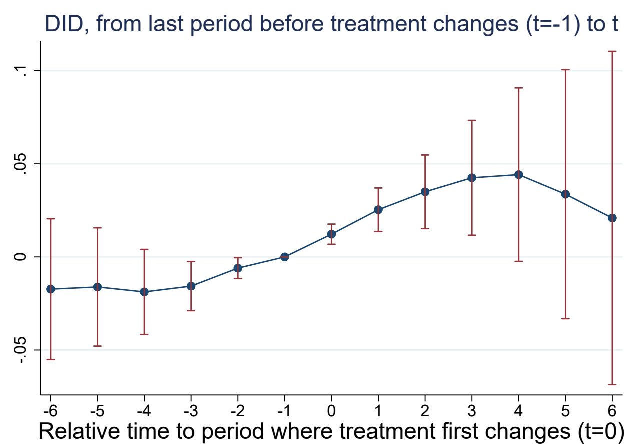
The results are shown in Figure F.2. The top panel shows the effects on the manufacturing employment share measured in percentage points. The coefficients are small and statistically insignificant throughout almost the entire pre-treatment period, but decline steadily following treatment and remain statistically significant up to six years later. The bottom panels show the effects on the log levels of manufacturing and nonmanufacturing employment. The estimates for manufacturing employment are statistically insignificant throughout most of the pre-treatment period and the entire post-treatment period. For nonmanufacturing employment, there is a statistically significant for the first few years after treatment, after which point the coefficient estimates remain positive but lose statistical significance. These findings are consistent with my estimates from Section 4 and provide further evidence that IBD led to a decline in the manufacturing employment share that was driven by an increase in nonmanufacturing employment.



(a) Manufacturing employment share



(b) Log manufacturing employment



(c) Log nonmanufacturing employment

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

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

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

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

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

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

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

Table F.2: Effects of IBD on Employment

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

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

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

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

G Additional Model Results

This section includes several additional details of the model omitted from the main paper in the interest of space. First, I show that the relative productivity of the manufacturing sector has increased over time in a manner consistent with my parameterization. I also reconcile my results to those in Eisfeldt and Rampini (2006).

G.1 Manufacturing Productivity Growth

This is calculated as the ratio of manufacturing productivity to total nonfarm productivity and is shown in Figure G.1 below. I use total productivity instead of nonmanufacturing productivity because the later is not available separately across the entire time period. My model assumes that the relative productivity of the manufacturing sector grew by a factor of just over 2.5, which is reasonably close to the actual value of 2.2.

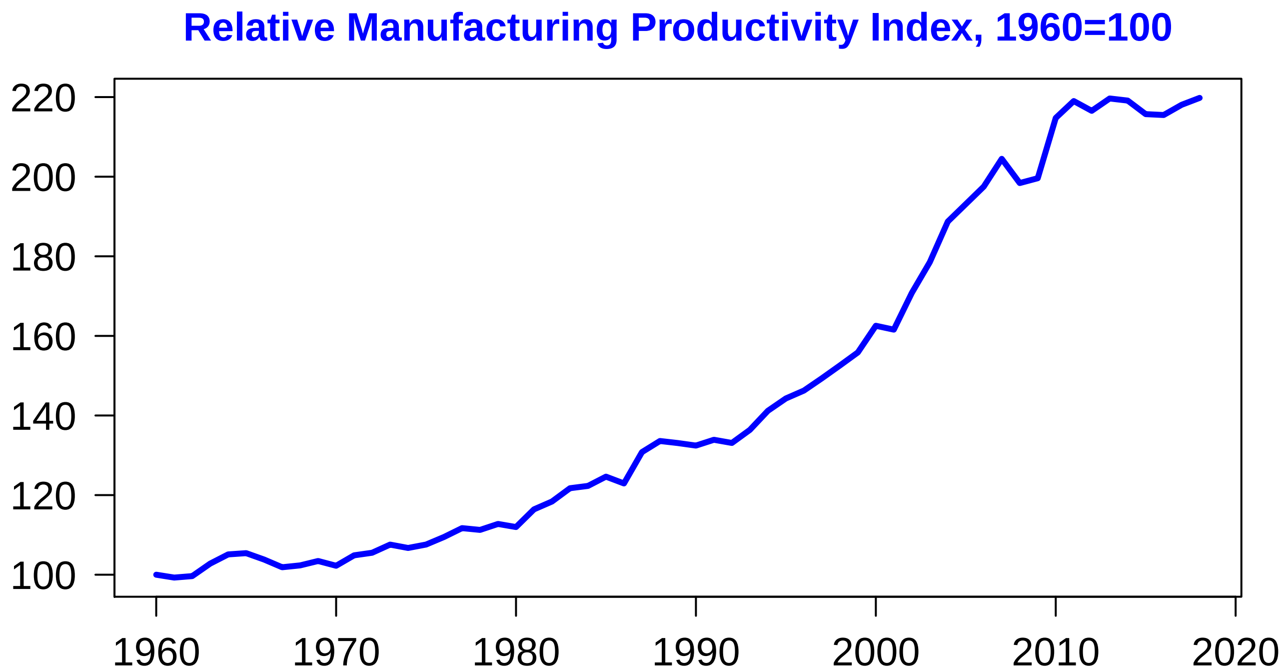


Figure G.1: Manufacturing Relative Productivity Growth

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

Next, I simulate the model without recessions to give a sense of the role of fixed costs in determining the timing of structural change. The left panel of Figure G.2 shows the exogenous productivity trend for the manufacturing sector that I use in the simulation. The right panel shows the optimal credit share going to manufacturing 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.

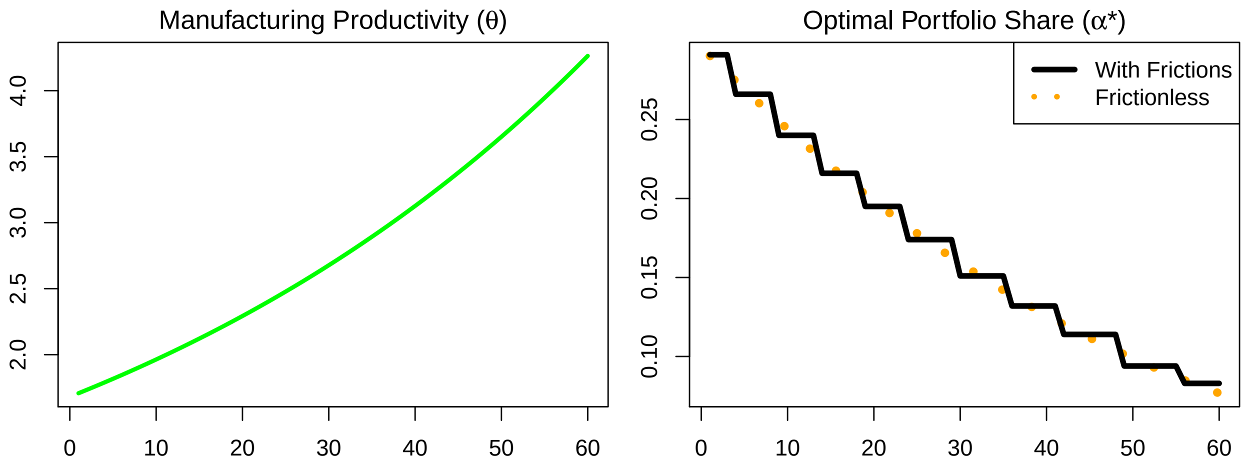


Figure G.2: Model without Recessions

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

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

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

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

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

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