Why does structural change accelerate in recessions? The credit reallocation channel ☆

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Abstract

The decline of the U.S. manufacturing share since 1960 has occurred disproportionately during recessions. Using evidence from two natural experiments—the collapse of Lehman Brothers in 2008 and U.S. interstate banking deregulation in the 1980s—I find 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.

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JEL classification: E32, E44, E51

1. Introduction

One of the most prominent and well-established changes in the structure of U.S. 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 (Fig. 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 that 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)

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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 U.S. 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 that 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 showing the outsized role of recessions in accounting for the decline of U.S. 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 (NBER) 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 roughly 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 is 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 that 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 than the average firm without Lehman exposure to obtain a new loan in each year from 2009 to 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 and 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 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 for establishing such a relationship, which is consistent with an extensive literature in economics and corporate finance related to relationship lending, including Hachem (2011).[2] 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 the fixed costs of establishing new relationships that generates the patterns 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—that 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 U.S. Treasury's Automotive Industry Refinancing Program (AIFP) from 2008-2014, which provided credit to struggling U.S. 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 that 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 can be 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

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

of $12bn. The model is stylized and these costs must be weighed against potential benefits arising outside its scope. However, they suggest that the distortions of these policies could be significant, even in cases where there is no credit risk, and that policy makers 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 (1949, p.218), 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 and Rogerson (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 has led these papers to generally focus on reallocation occurring within a single sector.[3] A key contribution of this paper is to establish an important role for reallocation *across* sectors.

This paper also builds on work that leverages "natural experiments" in cross-sectional credit availability to identify the effects of these disruptions. Peek and Rosengren (1997) and Peek and Rosengren (2000) use geographic variation of Japanese bank branches in the United States to analyze how Japanese financial shocks in the 1990s were transmitted to the United States. 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) finds a role for credit constraints in explaining employment outcomes for small and large firms in the U.S. 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; these include Cingano, Manaresi and Sette (2016) (Italy), Bentolila, Jansen and Jiménez (2017) (Spain), Iyer, Peydró, da Rocha-Lopes and Schoar (2013) (Portugal), and Huber (2018) (Germany). Other examples of work in this vein include Schnabl (2012) and Paravisini, Rappoport, Schnabl and Wolfenzon (2014).

In terms of methodology, the paper in the credit shock literature that most most closely matches my own is Chodorow-Reich (2014), which also uses the Lehman Brothers bankruptcy as an exogenous credit shock, as proposed by Ivashina and Scharfstein (2010). The author's 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 collapse were disproportionately affected.

Analysis of the macroeconomic effects of U.S. interstate banking deregulation dates back to Jayaratne and Strahan (1996). Allowing entry by out-of-state banks has been

[3] These papers also tend to abstract from credit. The relation 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.

shown to boost credit availability for entrepreneurs (Black and Strahan, 2002), spur innovation (Amore, Schneider and Žaldokas, 2013; Chava, Oettl, Subramanian and Subramanian, 2013; Cornaggia, Mao, Tian and Wolfe, 2015), reduce the volatility of business cycles (Morgan, Rime and Strahan, 2004; Acharya, Imbs and Sturgess, 2011), and lead to increases in inter-firm credit reallocation (Herrera, Kolar and Minetti, 2014). More recent work by Bai, Carvalho and Phillips (2018) and Mian, Sufi and Verner (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, Rebelo and Xie (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, Rogerson and Valentinyi (2014), Boppart (2014), Comin, Lashkari and Mestieri (2021), and Alder, Boppart and Müller (2021). My work focuses on the decline of manufacturing activity in the U.S. over the past 60 years. I am aware of only one other paper that directly analyzes the relation between business cycles and structural change: Storesletten, Zhao and Zilibotti (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 U.S. manufacturing sector to establish a role for business cycles in explaining the timing of structural change.

Jaimovich and Siu (2020) 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 those reported in 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 could reflect similar underlying mechanisms. The core mechanism in my paper is also closely related to that in Foote (1998), which 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, Hoshi and Kashyap (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 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 U.S. 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 U.S. 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 policy makers. Finally, Section 6 concludes.

2. Background and motivation

2.1. The decline of U.S. manufacturing from 1960 to 2019

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 of U.S. 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 Fig. 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 that in the actual series if there is no recession. If the quarter has a recession, I instead impose a change of zero. The total series has declined by 20.5 percentage points (pp) 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 U.S. 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 the 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 roughly 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 46% of the decline in the manufacturing share during recessions is secular.

As a complement to this methodology, I employ two additional econometric techniques developed to decompose trends and cycles in time series data and compare them in Table 1: the Hodrick-Prescott (HP) 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. The band-pass filter developed in Baxter and King (1999), which Hodrick (2020) argues is well suited for complex data-generating processes, 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 (53.7%) and median (48.1%) both suggest that about half of the decline in the manufacturing share during recessions can be attributed to secular factors. In addition, even cyclical changes could eventually become structural through a process of hysteresis. 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 about half of this decline has been due to secular factors. Here I argue that a credit reallocation channel can explain these findings. This 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 the magnitude of reallocation will be smaller. This is consistent with the patterns in Fig. 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 U.S. 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 Securities and Exchange Commission (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 U.S. commercial and industrial lending, including around two-thirds with maturity greater than one year. A sample loan is shown in Appendix Fig. D.1.

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 U.S. 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 (bp). 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 U.S. investment bank, and its bankruptcy remains the largest in U.S. 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[4]—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 that received lines of credit from Lehman Brothers were systematically more likely to have unobserved qualities that caused lower 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 split by manufacturing and nonmanufacturing for firms with and without Lehman attachment. I also include summary statistics within each sector for the subset of non-Lehman firms that had an exposed line of credit through a syndicate that included at least one of JP Morgan, Goldman Sachs, or Morgan Stanley.

Lehman-attached firms 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 and non-Lehman firms were very similar across sectors, averaging 26bp for manufacturers and 43bp for nonmanufacturers. These observations are in line with market perceptions that clients of Lehman Brothers tended to be larger institutions,[5] but do not suggest any differential selection across sectors. This can also be seen in the columns showing summary statistics for clients of Lehman's competitors, which more closely resemble Lehman-attached firms. Appendix E shows 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. Fig. 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 at-

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

tachment saw large and persistent drops in aggregate sales and employment in the years following the Great Recession, reaching declines of more than 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 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 means 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 its 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.[6] 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

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

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 that 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 that had relationships with Lehman were much larger and obtained financing more frequently, so losing access to one source of credit would be likely to push them to seek out new ones. The equilibrium outcome for nonmanufacturers will depend on the relative strength of these two effects. In practice, the latter effect seems to dominate.

The baseline results for the probability of receiving a new loan are shown in the first column of the top rows of Table 4. The first row, which corresponds to ρ in Eq. 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.[7] The positive coefficient estimates are driven in part by the fact that the reported effects are relative to the average firm without Lehman attachment. These firms were generally smaller and obtained credit less frequently than the average firm with Lehman attachment, and thus a positive estimate of ρ does not necessarily imply that credit contractions lead to increases in aggregate lending. The second row, corresponding to Ω in Eq. 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 four 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. 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. Finally, the fifth column constrains the sample to only firms with a line of credit involving Lehman, JP Morgan, Goldman Sachs, or Morgan Stanley. This specification shows that it was variation *within* this set of large firms that drives my results, rather than differences between this group and the set of smaller firms that were less likely to be clients of these large banks.

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

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 of Ω using a modified version of Eq. 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 my findings are not simply picking up persistent differences in non-industry firm characteristics that could 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 that of a 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 subsectors with better long-run prospects, regardless of whether they fell under manufacturing or services.

For this exercise, I replace the manufacturing indicator in Eq. 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 that lose access to credit would not be viable if a new relationship had to be built from scratch to finance them. The second, shown in Chodorow-Reich and Falato (2021) and Acharya, Almeida, Ippolito and Orive (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 U.S. interstate banking deregulation (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 U.S. 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.[8] 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 United States.[9] Allowing entry by out-of-state banks has boosted credit availability for entrepreneurs (Black and Strahan, 2002), increased innovation (Amore, Schneider and Žaldokas, 2013; Chava, Oettl, Subramanian and Subramanian, 2013; Cornaggia, Mao, Tian and Wolfe, 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

[8] Kroszner and Strahan (2014) provide a summary of the literature analyzing U.S. banking deregulation.

[9] While most work on IBD has focused on the United States, Bertrand, Schoar and Thesmar (2007) analyze deregulation in France.

stable (Morgan, Rime and Strahan, 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, Kolar and Minetti (2014) show that IBD led to increases in empirical measures of inter-firm credit reallocation. Acharya, Imbs and Sturgess (2011) find that relaxing interstate banking restrictions led to a more diverse activity composition across sectors. Bai, Carvalho and Phillips (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, Sufi and Verner (2020). Its authors 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 is the U.S. Bureau of Economic Analysis (BEA). These data provide total employment split by SIC industry code for each state from 1970 to 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.

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 (2021), Callaway and Sant'Anna (2021), Athey and Imbens (2021), and Goodman-Bacon (2021), 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 U.S. 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 to 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 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. Interstate banking deregulation 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 Appendix Fig. F.2, 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, Sufi and Verner (2020)

My finding that interstate banking deregulation primarily benefited service firms echoes the results of Mian, Sufi and Verner (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 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 Appendix Table F.1. 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 to 2000 so that a negative value corresponds to an industry that 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:

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

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.[10] 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 larger and statistically significant 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 is very similar while the coefficient on HHI_i attenuates. The R^2 values are also much higher for the specifications that include exposure to secular change, suggesting that supply-side factors explain more of the variation in employment shares following IBD.

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 Appendix 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 (Eq. 2) to estimate the response of state-level prices measured using both the series from Mian, Sufi and Verner (2020) and the more recent series developed in Hazell, Herreño, Nakamura and Steinsson (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 long-run secular change could simultaneously match both the cyclical facts presented in Mian, Sufi and Verner (2020) and Mian and Sufi (2014), and the longer-run secular facts observed 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. 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.

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

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 constant elasticity of substitution (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 that policies preventing reallocation can have substantial opportunity costs.

5.1. Firms, banks, and production

The economy consists of two sectors: manufacturing (M) and nonmanufacturing (N). There is 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 firm's 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 cumulative distribution function 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 productivity.

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, Haltiwanger and Schuh (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.[11]

I define δ_t as the share of firms that 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

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

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 one 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 Eqs. 6 and 7, where the value of changing the credit share is:

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

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

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

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.[12]. 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.

Fig. 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

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

that 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.

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 to 2018 (see Fig. G.1 in 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 Fig. 1. I include eight recessions, corresponding to the number observed in the data since 1960, and set the share of separations to be 1%. Finally, I set $c = 0.0008$, which generates an average decline during recessions of 1.33pp per year that almost exactly matches the value of 1.36pp observed in the data. This calibration does not distinguish between fixed adjustment costs for credit and for other productive inputs such as labor, both of which could lead to a concentration of structural change in recessions. The model should thus be thought of as providing an upper bound for the magnitude of the effect coming specifically from credit reallocation.

In the absence of fixed costs, the composition of the economy will adjust smoothly in response to increasing manufacturing productivity. This is shown in Fig. G.2 in 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 that 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.[13]

These results are illustrated in Fig. 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 Fig. 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

[13] 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.

business cycle. 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-established 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 ended 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

Policy makers often find themselves tempted to intervene on behalf of entire industries. A recent example is the Automotive Industry Financing Program. The goal of this program was explicitly to stabilize the auto industry as a whole; in a speech in March 2009, Barack Obama said: "We cannot, and must not, and we will not let our auto industry simply vanish."[14] 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.[15]

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, Dorn, Hanson and Song (2014). The model also abstracts from frictions that can affect the costs of reallocating other productive inputs. Chodorow-Reich and Wieland (2020), for example, show that wage rigidity 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 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 six additional periods. I consider a policy that 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 Fig. 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.

Over the six years that the policy is in place, the cumulative output loss due to misallocation in the model is approximately 78% of the initial outlay. In the case of the AIFP,

[14] U.S. Office of the Press Secretary (2009).

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

this would represent \$63bn, more than six times the program's losses due to non-repayment. These results come from a simple model and are not intended to serve as precise quantitative counterfactuals. Instead, they are meant to highlight interindustry misallocation as a potentially important cost of credit intervention programs.

6. Conclusion

The role of manufacturing in the U.S. 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 demonstrate 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 U.S. interstate banking in the 1980s as a natural experiment. This period of deregulation led to increased 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 policy makers, 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 that had it prior to the recession.

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7. Figures and tables

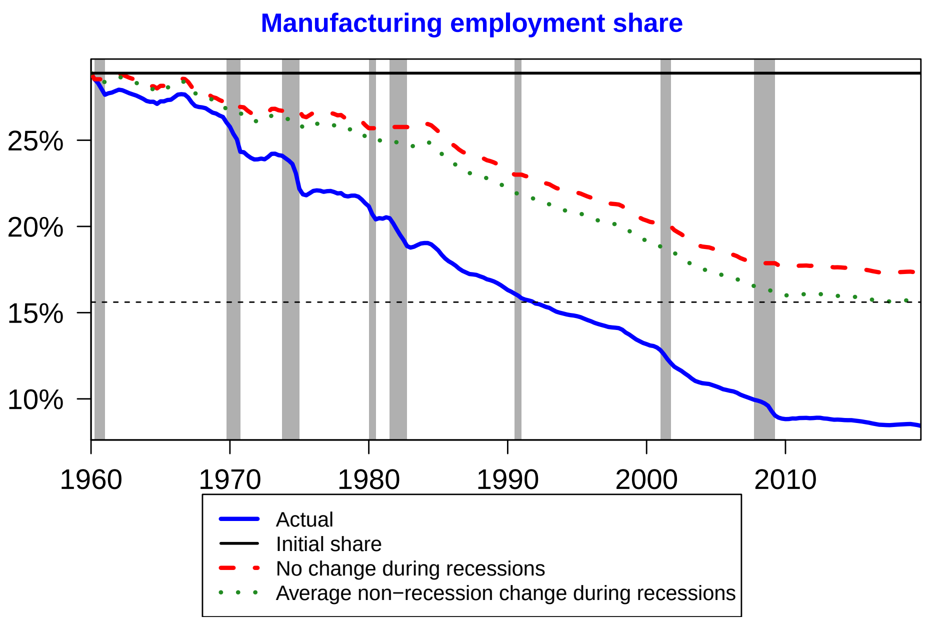

Fig. 1. Change in U.S. manufacturing employment share, 1960–2019.

The solid blue line shows the share of payroll employment from the Current Establishment Survey coming from the manufacturing sector from 1960 to 2019. Shaded areas indicate NBER-defined recessions. The dashed red line represents the cumulative change from the beginning of 1960 counting only non-recessions; during recessions this series will be flat, and in non-recessions it will track the blue line. The dotted green line is a counterfactual estimate that replaces the changes during recessions with the average change during non-recessions.

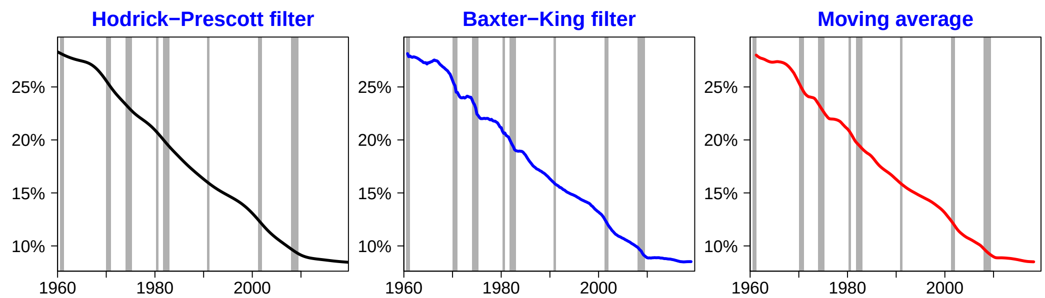

Fig. 2. Trend measures of manufacturing employment share.

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 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%
Baxter and King (1999)	80.3%
Centered moving average	50.3%
Median	48.1%
Mean	53.7%

Table 1. Secular portion of manufacturing share decline during recessions.

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 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 Fig. 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 U.S. loans.

This table shows a variety of summary statistics calculated from DealScan. All included loans are denominated in U.S. dollars and issued to U.S. 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.

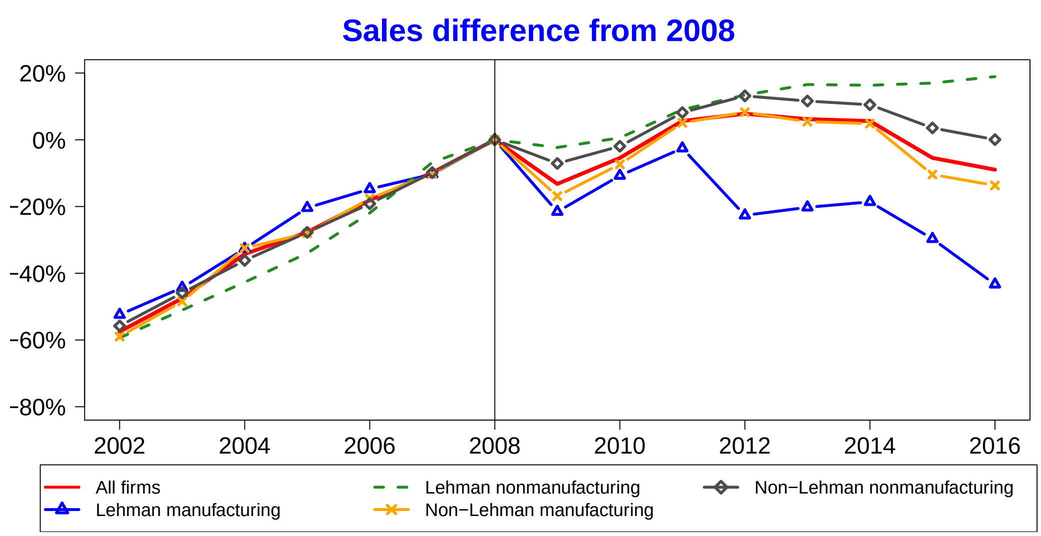

Fig. 3. Aggregate sales growth relative to 2008.

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	Manufacturing JPM/GS/MS	Nonmanufacturing Lehman	Nonmanufacturing Non-Lehman	Nonmanufacturing JPM/GS/MS
Sales ($mil)						
Mean	$12,459	$2,335	$5,294	$10,045	$1,318	$3,757
Median	$2,839	$100	$1,506	$1,771	$73	$1,257
Std. dev.	$25,934	$12,626	$13,591	$29,520	$5,491	$6,810
95th percentile	$53,918	$8,760	$20,245	$42,089	$5,637	$16,027
Assets ($mil)						
Mean	$15,282	$2,790	$7,619	$12,788	$1,703	$3,567
Median	$3,997	$118	$1,542	$2,504	$103	$1,278
Std. dev.	$38,213	$18,935	$40,174	$26,132	$8,818	$5,879
95th percentile	$63,666	$9,877	$24,750	$104,694	$6,623	$17,673
Emp (thous)						
Mean	29.8	7.4	19.0	50.9	8.3	23.7
Median	9.4	0.5	6.4	8.8	0.5	5.8
Std. dev.	54.3	26.6	37.5	169.7	34.4	58.2
95th percentile	119.0	162	76.9	162.0	37.0	96.7
Avg spread (bp)						
Mean	172	198	147	183	226	169
Median	150	175	100	175	200	150
Std. dev.	124	155	140	118	171	114
95th percentile	350	450	350	350	555	350
Leverage						
Mean	0.42	0.31	0.27	0.39	0.36	0.29
Median	0.39	0.16	0.24	0.37	0.16	0.26
Std. dev.	0.29	0.62	0.21	0.27	0.72	0.26
95th percentile	0.98	1.06	0.63	0.93	1.17	0.79
Profitability						
Mean	0.28	0.27	0.37	0.29	0.31	0.36
Median	0.23	0.29	0.32	0.22	0.25	0.30
Std. dev.	0.16	0.39	0.21	0.20	0.40	0.24
95th percentile	0.65	0.84	0.73	0.69	1.03	0.80
Tobin's Q						
Mean	1.89	4.51	2.02	1.93	5.60	2.01
Median	1.53	1.91	1.65	1.56	1.87	1.64
Std. dev.	0.95	10.42	1.19	1.27	13.64	1.09
95th percentile	2.73	13.11	4.22	4.11	20.14	4.07
# of firms	93	3,789	549	97	3,802	409
% with new loan	72.0	17.9	60.0	64.9	14.4	60.0

Table 3. Summary statistics from 2004 for firms split by Lehman exposure.

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. The Non-Lehman column include all firms not classified as having Lehman attachment. The JPM/GS/MS column includes firms that had a revolving line of credit that started prior to 2008, was scheduled to extend into 2009 or beyond, and was issued through a syndicate involving at least one of JP Morgan, Goldman Sachs, or Morgan Stanley, but not 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 that received any new loan in 2004.

	(1)	(2)	(3)	(4)	(5)
New loan probability (pp)					
$\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i$	16.57***	13.62***	17.37***	16.46***	11.51***
	(4.09)	(3.40)	(4.71)	(4.25)	(1.81)
$\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{Mfg\}}$	−8.99***	−7.64***	−9.45***	−9.14**	−10.11***
	(2.61)	(2.55)	(2.56)	(3.29)	(2.20)
Sales (%)					
$\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i$	1.38	1.13	1.86	0.16	3.69***
	(1.54)	(1.16)	(3.35)	(1.93)	(1.17)
$\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{Mfg\}}$	−6.61**	−5.36***	−1.17	−7.67***	−5.31***
	(2.29)	(1.76)	(5.01)	(2.22)	(1.70)
Employment (%)					
$\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i$	1.90	1.18	4.92	0.04	3.25**
	(2.04)	(1.87)	(4.09)	(1.94)	(1.46)
$\mathbb{1}_{\{Year \geq 2009\}} \times Lehman_i \times \mathbb{1}_{\{Mfg\}}$	−7.11***	−6.99***	−11.37**	7.33***	−7.58***
	(1.57)	(1.81)	(4.48)	(2.11)	(2.11)
Controls	Y	Y	N	Y	Y
Loans>0	N	Y	N	N	N
2016 survivors	N	N	N	Y	N
Only large bank clients	N	N	N	N	Y
N	69,940	44,422	84,061	37,486	14,353

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.

This table shows the results of estimating Eq. 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 to 2016 and includes only firms that were in Compustat by the start of this period. The second column restricts the sample of firms to only those that 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 that were observed in Compustat in at least one year in 2016 or later. The fifth column restricts the sample to firms that had at least one revolving line of credit that was open prior to 2008, was scheduled to extend into 2009 or beyond, and was issued from a syndicate that included at least one of Lehman, JP Morgan, Goldman Sachs, or Morgan Stanley.

	$\mathbb{1}^{NewLoan}$ (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

This table shows estimates of Ω from Eq. 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

This table shows estimates of Ω from Eq. 1, where the dummy for manufacturing has been 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.

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 to 1996.

Effect on employment shares (pp)	(1)	(2)	(3)
$Dereg_{j,t}$	0.098	-0.024	0.003
	(0.059)	(0.045)	(0.044)
$Dereg_{j,t} \times HHI_i$	-0.059		-0.016
	(0.038)		(0.014)
$Dereg_{j,t} \times SC_{i,-j}$		0.393^{***}	0.391^{***}
		(0.044)	(0.043)
R^2	0.007	0.296	0.297
N	77,616	77,616	77,616

Standard errors two-way clustered by state and industry 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 Eq. 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 U.S. excluding state j. Negative values of $SC_{i,-j}$ correspond to industries that 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. Within-R^2 values reported. Standard errors are two-way clustered by state and industry.

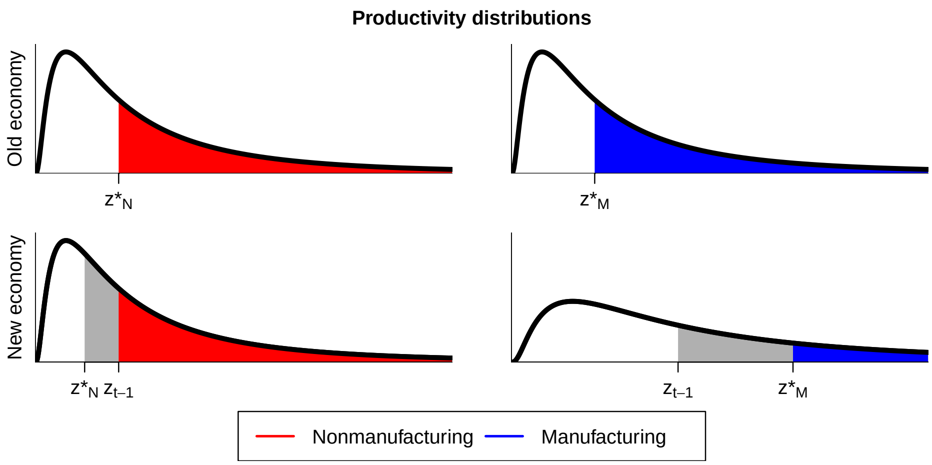

Fig. 4. Model productivity distributions.

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.

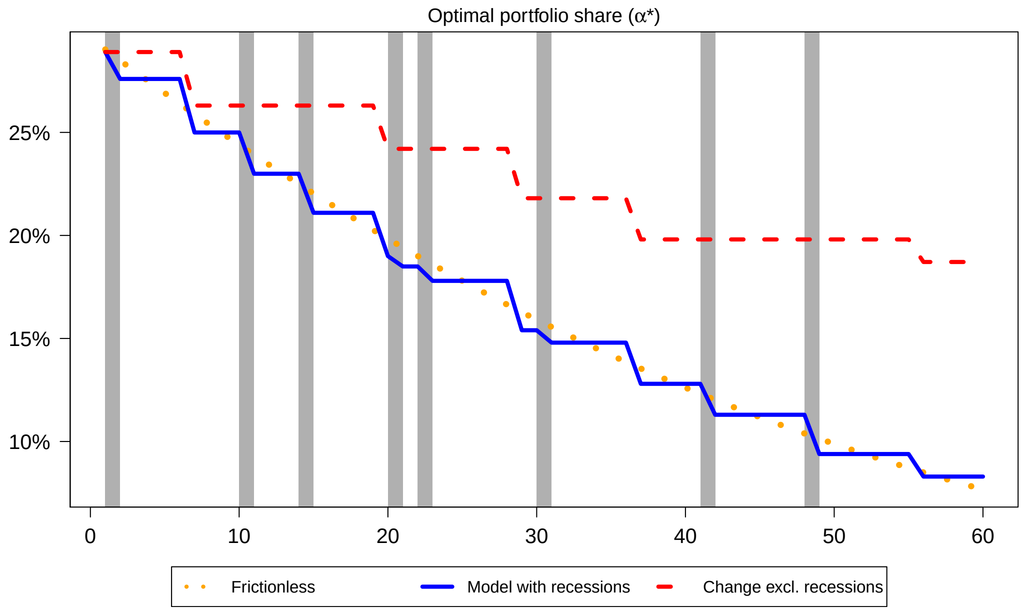

Fig. 5. Model with recessions.

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 Fig. 1). The parameter values are shown in Table 9.

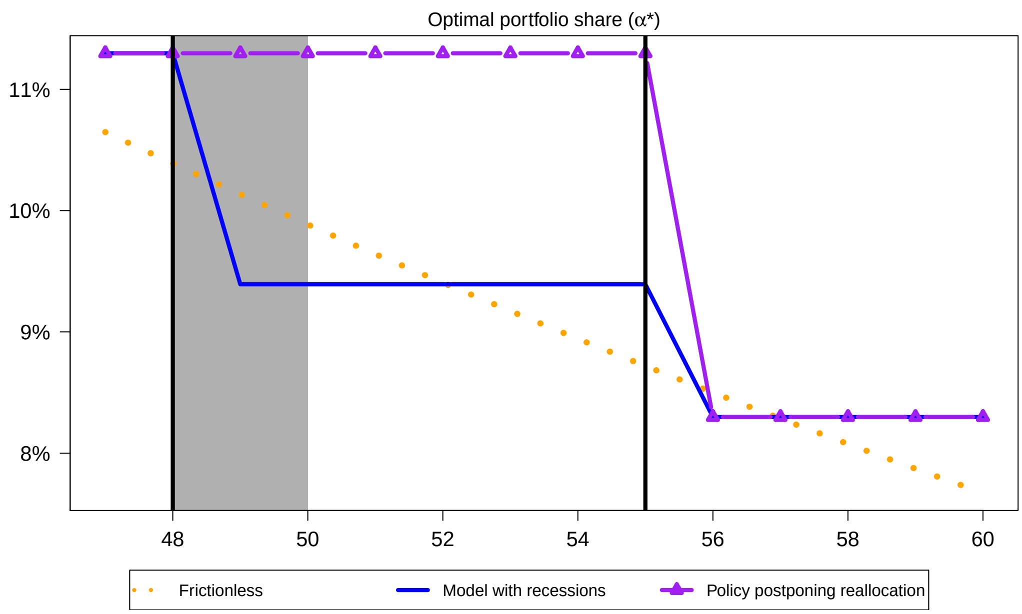

Fig. 6. Effects of policy preventing reallocation.

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 that 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.