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Financing Labor

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Financing Labor

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Financial market imperfections can have significant impact on employment decisions of firms. We illustrate the economic importance of this channel by showing that employment decisions are constrained by firms' financial health and liquidity. Our main analysis uses a collage of three 'quasi-experiments' to trace the effects of finance on employment. The results suggest that financial constraints and the availability of credit play an important role in firm-level employment decisions, as well as aggregate unemployment outcomes.

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1. Introduction

For more than eighty years – since the great depression of the 1920s – one of the key problems of macroeconomics has been the explanation of unemployment. More recently, following the recent financial crisis and economic recession, there has been an increasing interest in understanding the cyclical behavior of unemployment and in particular its relation to financial constraints and the availability of financing. While the empirical relation between financial constraints and corporate investment has been studied extensively (for a survey, see, e.g., Stein (2003)), comparatively little was known about the role that financial constraints and the availability of finance play in determining the level of unemployment and its propagation over time. Our NBER working paper (released in 2011)[1] and several subsequent studies that built on our work have filled this gap. These studies have substantially extended our understanding of the ways in which financial constraints affect firm-level employment (Chodorow-Reich (2014), Duygan-Bump, Levkov, and Montoriol-Garriga (2015), Falato and Liang (2016), Giroud and Mueller (2017), and Benmelech, Frydman, and Papanikolaou (2019)). Such understanding is crucial, as counter-cyclicality in the cost of external finance (e.g. Bernanke and Gertler (1995)) may create financial accelerator effects that amplify variation in employment levels over the business cycle.

One might naively argue that labor requires no external financing, as firms can fund labor costs from the revenue that workers produce. In particular, to the extent that hiring labor is profitable, it is also self-financing. Consequently, employment decisions should be immune to external financing. However, theoretically, the cost and availability of external finance should affect firm employment decisions for a number of reasons. First, if payments to labor are incurred before revenues are received, and the firm is unable to finance this mismatch with internal resources, it will need external finance (see, for example, Greenwald and Stiglitz (1988)). As such, when the ability to finance this labor-cash flow mismatch deteriorates, firm employment should fall.[2]

Second, frictions in capital markets will also affect firm employment decisions when labor is not solely a variable factor of production but rather has a fixed, or quasi-fixed cost component (see, for example, Oi (1962), Farmer (1985), Hamermesh (1989), and Hamermesh and Pfann (1996)). As first described in Oi (1962), such fixed costs include investments associated with hiring and training

[1] This paper is an updated and revised version of our NBER working paper, Benmelech, Bergman and Seru (2011).

[2] The argument that firms must finance labor payments before revenues are received is similar to that found in the literature on financial constraints and inventory investment: firms must finance inventory investment during the production process before revenues are received.

activities. Finally, the availability of external finance may affect employment indirectly through its impact on firm level investment. That is, in the presence of capital market frictions, investment is limited by the availability of internal funds, and employment is adjusted to the decline in capital due to complementarity between labor and capital.

Testing for a causal effect of financial constraints on firm employment decisions is complicated by identification concerns of endogeneity and measurement error similar to those found in the investment-to-cash flow literature.[3] The main concern among these is the concern that variables measuring firms' financial health – such as net worth, firm leverage, earnings, and sales – may also correlate with firms' demand for labor. Similarly, variables measuring availability of finance and fluctuations in the wedge between the cost of external and internal funds, such as credit spreads and CDS rates, may also be correlated with demand for firms' final product and hence influence its demand for labor. These alternative explanations suggest that employment could be negatively correlated with empirical measures of costly external finance even in a frictionless Neoclassical setting.

In this paper we analyze the relation between finance and labor using several empirical strategies previously employed in the financial constraints and finance-growth literatures that were developed to alleviate these identification concerns. We provide evidence from three 'quasi-experiments' in our main analysis that enables better identification of the effects of finance on employment.

First, we follow the approach in Almeida et al. (2012) by using a 'maturing-debt' empirical strategy which exploits heterogeneity in the maturity of long-term debt across firms. The empirical tests examine whether firms with long-term debt maturing in a particular year reduce their labor force by more than firms who do not face the need to refinance maturing long-term debt. We find a negative and statistically significant relation between maturing long-term debt and the change in the number of firm employees. That is, consistent with the presence of financial frictions, when firms have a large amount of maturing debt they often adjust by reducing their labor force. The economic magnitudes are significant: for example, we find that firms that have maturing debt that requires refinancing and that amounts to at least 5% of the firm's total assets reduce employment by approximately six percent relative to the unconditional mean.

We further hypothesize that the effect of maturing long-term debt will be particularly important among financially weak firms. Raising external finance will be more costly for these firms and they

[3]See, for example, Hubbard (1998), Roberts and Whited (2012), and Stein (2003).

may also lack a cash cushion that can be used to buffer episodes of illiquidity, as in the case of long-term debt rollovers. We find that the effect of maturing long-term debt on the change in the number of employees is indeed higher for firms that are more likely to be financially constrained, as proxied by firms with lower interest coverage ratios. Auxiliary tests using measures of credit availability in the economy provide additional support for these patterns.

In a second quasi-experiment we analyze the impact of bank deregulation on state-level unemployment rates. Our methodology follows Jayaratne and Strahan (1996) which utilizes the introduction of state-level bank deregulation laws across the United States. During the mid 1970s states began to deregulate local banks by removing restrictions on both intrastate and interstate bank branching. Deregulation allowed bank holding companies to consolidate their subsidiaries into branches and to open new branches within state lines. Furthermore, states passed laws allowing out-of-state banks to purchase banks within the state. If bank deregulation relaxes financial constraints and leads to more efficient capital allocation, we expect that following such deregulation, state level unemployment will drop. Consistent with such a finance-labor link, we find that post-deregulation of both intra and inter state branching laws, states did indeed experience a statistically and economically significant drop in their unemployment rates. Using a difference-in-difference specification we show that the introduction of intrastate bank deregulation laws is associated with a drop of between 0.45 and 0.86 percentage points in the state unemployment rate. These findings are similar to those in Beck et al, (2010) who exploit intrastate branching deregulation. Similarly, the introduction of inter-state bank deregulation laws, which enabled banks to open branches across state lines, decreases state unemployment rates by between 0.84 and 1.14 percentage points.

Finally, in the third quasi-experiment we analyze how a negative shock to bank loan supply adversely affects unemployment rates. We follow Peek and Rosengren (2000) by exploiting a loan supply shock transmitted by Japanese banks to markets in the United States. As a result of the dramatic decline in real estate prices in Japan during the 1990s and the concurrent negative shock to Japanese bank balance sheets, U.S. affiliates of Japanese banks contracted loan supply in U.S. markets. This shock was arguably exogenous to local U.S. market conditions and yet affected Japanese bank operations in the United States. Since Japanese bank penetration in real estate markets was quite substantial in many localities in the U.S., a withdrawal of loan supply stemming from losses arising due to market conditions in Japan would involve substantial disruptions to credit

availability. By focusing on U.S. lending markets with large Japanese bank market presence, we can thus analyze the effect of credit supply on local lending and unemployment.

As in Peek and Rosengren, we find that lending by Japanese affiliated banks did indeed decline in the U.S. concurrently with the large declines in real estate values in Japan in the early 1990s. Instrumenting for Japanese bank losses using real estate market movements in Japan, we find that Japanese-affiliated banks located in the U.S. contracted real-estate lending concurrently with losses stemming from operations in Japan. Using this result as a first stage in a two-stage least square specification, we find a significant link between finance and unemployment: unemployment increases by about one percentage point in MSAs where there was a contraction in Japanese affiliated bank lending following the real estate decline in Japan from 1993 to 1995. These are large effects. As a back of the envelope calculation, we use these estimates to calculate the effect on unemployment caused by the negative loan supply shock that resulted from the real-estate price drop in the United States during the Great Recession. Under the assumption that our estimates are externally valid, about 38 percent of the unemployment increase during the Great Recession could be explained by the contraction in loan supply.

To verify that shocks to Japanese real estate values do not vary with demand-side effects in the U.S., we also conduct a placebo test in which we instrument for non-Japanese affiliated bank lending using the Japanese real estate index. Consistent with a supply-side story affecting only Japanese-affiliated banks in the U.S., we find no evidence of a relation between innovations in Japanese real estate values and changes to lending by non-Japanese affiliated banks in the U.S in the first stage of the regression or between unemployment and instrumented non-Japanese affiliated bank lending in the second stage of the regression.

Taken together, our collage of findings are consistent with the view that finance is an important determinant of both firm-level employment decisions as well as aggregate-level unemployment rates. As financial constraints become binding, firms need to adjust both inputs of production – capital and labor. While much prior research has focused on thxe effect of financial constraints on capital formation, our empirical results suggest that financial constraints seem to affect labor as well.

Our paper is related to two strands of literature. First, it is connected to the vast literature examining the impact of credit market imperfections and investment behavior. It is also related to a growing literature on labor, employment, and financial constraints (for early contributions, see Pagano and Volpin, 2008; Pagano, 2010; Campello, Graham, and Harvey, 2010, and Benm-

elech, Bergman, and Seru, 2011). Additional examples include Michaels, Page and Whited (2019), Chodorow-Reich (2014), Duygan-Bump, Levkov, and Montoriol-Garriga (2015), Falato and Liang (2016), and Benmelech, Frydman, and Papanikolaou (2019). We discuss related studies in both of these areas when we describe our results.

The rest of the paper is organized in the following manner. Section 1 displays the analysis using maturing long-term debt, section 2 presents the evidence on the effect of banking deregulation on unemployment, and section 3 discusses the effect of Japan's real estate decline on unemployment in the U.S. in the early 1990s. Section 4 concludes.

2. The Effects of Maturing Long-term Debt on Employment

We follow the 'maturing-debt' approach first introduced by Almeida et al. (2012) by using an empirical strategy which exploits heterogeneity in maturity of long-term debt across firms. Our empirical tests examine whether firms with long-term debt maturing in a particular year reduce their labor force by more than those firms not facing the need to refinance maturing long-term debt. Since external capital is costly (e.g., Myers and Majluf, 1984), we hypothesize that firms which must refinance large amounts of maturing long-term debt will adjust their real activity and reduce employment.

The identification strategy hinges on the assumption that variation in the amount of long-term debt maturing in any given year is exogenous to corporate outcomes in that particular year. To lend credence to this assumption, the identification strategy relies on exploiting debt that was issued a number of years before the year of interest. For example, we examine employment of firms which in year t have a large amount of maturing debt issued at least two, three, or four years prior to t and compare it to firms that have a small amount of debt maturing in year t. Since this portion of the maturing debt was issued a good deal prior to the year of maturity, variation in its level is arguably exogenous to the market conditions and the investment opportunities that prevail when the debt eventually becomes due.

2.1 Data and Summary Statistics

Compustat reports the amount of long-term debt payable in more than one year through more than five years from firms' fiscal year end. We collect this data on the amount of future maturing debt. Specifically, we utilize Compustat variables *dd3*, *dd4*, and *dd5* that represent, respectively,

the amount of long-term debt maturing three, four, and five years after the annual reporting date. To measure the maturing debt structure of a firm in a particular year we construct lagged values of these debt maturity variables: *l2_dd3* is the two-year lag of *dd3*, *l3_dd4* is the three year lag of *dd4* and *l4_dd5* is the four year lag of *dd5*. By construction, these variables measure the amount of long-term debt issued at least two, three, or four years before the base year and maturing in the upcoming year. For example, at year t, *l2_dd3* measures the amount of long-term debt maturing at $t+1$ that was issued *before* year $t-2$. We scale the lagged variables by beginning of year assets.

Next, we construct dummy variables that take on the value of one for those firms for which long-term debt coming due in the upcoming year and issued at least t years ago is larger than 5 percent of total assets. We also define equivalent dummy variables using 10 and 15 percent threshold levels. These variables capture whether a firm has a significant amount of long term debt maturing in the upcoming year that requires refinancing. By examining debt that was issued before the year of analysis, we alleviate concerns that the level of maturing debt co-moves with other market variables or firm characteristics that have a direct impact on employment decisions.

Table 1 provides summary statistics for the maturing debt variables. As can be seen, the average amount of debt coming due in the upcoming year with an original maturity of greater than two, three, and four years equals on average 2.7, 2.5, and 2.5 percent of assets, respectively. We next define dummy variables that take the value of one if the maturing debt exceeds 5, 10, and 15 percent of the firm's total assets. As the table shows, 13.6 percent of firm-year observation have refinancing requirements that exceed 5 percent of total assets and that were issued at least two years before the year in which the debt comes due. Turning to higher levels of maturing debt, Table 1 shows that 5.3 percent of firm-year observations in the sample must refinance maturing long-term debt that was issued at least two years before the current year and that exceeds 10 percent of total assets. Similarly, in 2.7 percent of our sample, firms need to refinance maturing long-term debt that is higher than 15 percent of total assets.

We supplement our maturing debt variables with firm-level data from the Compustat Annual Industrial Files. We use these files to collect information on all non-financial firms during the years 1975–2019 with non-missing observations for the dependent and independent variables in the analysis. In addition to balance sheet and income statement information, Compustat also reports the number of workers employed by a firm. We define our main dependent variable as the annual percentage change in the number of employees at the firm level. To construct our sample,

we eliminate firms with less than 500 employees and, additionally, trim all variables by removing outliers at the 1st and 99th percentiles.[4] This results in a sample of 39,706 firm-year observations. All dollar figures are adjusted for inflation using the Consumer Product Index.

Table 2 reports descriptive statistics on the characteristics of the firms in the sample. The mean number of employees is 10,684, the median is 3,400. Since we drop observations with less than 500 employees, the number of employees ranges from 500 to 123,600. The mean annual percentage change in the number of employees, $\%\Delta employees$, is 5.239% (median=1.497%) and ranges from -72.7% to 233.1%. The mean percentage change in investment, $\%\Delta investment$, is 21.7%, while the level of investment (measured as investment scaled by beginning of period assets, or I/K) is 0.075, which is similar to the magnitudes found in studies of investment and financial constraints (see e.g., Rauh, 2006). The table also provides descriptive statistics on additional explanatory variables used in the analysis. We include the variables pertaining to firm size (in logs), Tobin's Q (proxied by market-to-book ratio), leverage, liquidity (measured as cash and marketable securities scaled by assets), asset maturity, profitability, a dummy for whether the firm has a credit rating, and interest coverage. Appendix A provides detailed information on the definitions of the variables used in the paper, their construction, and their data sources.

2.2 Baseline Results

In order to test the effects of maturing debt on employment we estimate the following baseline regression specification:

$$\%\Delta employees_{it} = \alpha + \beta_{LT} \times (Long\ term\ debt\ due)_{it}/Assets_{it-1} + \mathbf{X}_{it-1}\lambda + \mathbf{y}_t\theta + \mathbf{z}_i\psi + \epsilon_{it}, \quad (1)$$

where the dependent variable: $\%\Delta employees$ is the annual percentage change in the number of employees within a firm. $Long\ term\ debt\ due_{it}$ / $Assets_{it-1}$ is one of the variables described above that measures the ratio of long-term debt maturing in year $t+1$ and issued two, three, or four years prior to year t to the beginning of year book value of firm assets. $\mathbf{X}_{it-1}$ is a vector of firm specific control variables. These include lagged values of the firm market-to-book ratio, firm internal liquidity, $Liquidity_{it-1}$, the log of the book value of firm assets, firm leverage, asset maturity, and profitability. All regressions include year fixed effects, $\mathbf{y}_t$, and depending on the specification

[4] We use the 500 employee threshold to be consistent with the definition of small/large business in the U.S. Our results are not driven by this choice.

also include either four-digit SIC fixed effects or firm fixed-effects, denoted by the vector **z**. All regressions are estimated with heteroscedasticity robust standard errors that are clustered by firm.

We start by estimating the effects of long-term debt maturing in year $t+1$ and issued at least two, three, or four years prior to year t that is greater than 5 percent of the book value of firm assets on firm employment. The results are reported in Table 3. As column 1 demonstrates, we find a negative and statistically significant relation between the maturing long-term debt variable and the change in the number of firm employees after controlling for measures of Tobin's Q, liquidity, size, leverage, asset maturity, profitability and year and 4-digit SIC effects. The coefficient of -0.064 (statistically significant at the one percent level) implies that firms that have maturing debt that requires refinancing and that amounts to at least 5% of the firm's total assets reduce the number of their employees by close to six percent relative to the unconditional mean. That is, consistent with the presence of financial frictions, when firms have a large amount of debt coming to maturity which must be refinanced, part of their adjustment occurs through a reduction in labor force. As column 2 shows, this negative relation holds when we include firm-fixed effects as well (coefficient=-0.055, statistically significant at the five percent level).

Next, we further lag the maturing debt variable to ensure that the original debt issuance decisions did not coincide in time with employment decisions. As columns 3 and 4 demonstrate, the effect of maturing long-term debt is negative and statistically significant when we study the effect of debt issued at least three years before the base year. β_{LT} is -0.096 (significant at the one percent-level) when we include industry fixed-effects compared to -0.039 (significant at the five percent level) when we control for firm fixed-effects. Likewise, even when we lag debt issuance by four years (columns 5 and 6), focusing, therefore, on debt issued at least four years prior to the base year, we find that the effect of maturing debt on employment is negative and significant when we include industry fixed-effects (-0.079 and -0.012 for industry or firm fixed-effects, respectively).

It should be noted that while we focus our attention on maturing long-term debt as the key explanatory variable in our regressions, we obtain similar coefficients and magnitudes for the financial variables – leverage, liquidity and profitability – as in the investment and financial constraints literature. In this sense, we are 'over controlling' in these regressions, capturing separate effects of cash flow, cash holdings, and leverage, while studying the effect of debt that needs to be rolled-over on the change in the number of employees.

Examining the control variables, we find that the firm market-to-book ratio is positively related

to employment growth, as would be expected. Consistent with Kashyap, Lamont, and Stein (1994), we also find a positive relation between firm internal liquidity and the change in firm employment levels. In addition, we find that increased leverage predicts lower employment growth in the firm fixed effects regressions. This could be driven by the fact that firms in distress increase their leverage ratios, or alternatively, reflect firms' decision to reduce their labor force when faced with large future liabilities. We note, though, that the negative relation between the long-term debt maturity variables and the reduction in the labor force does not simply reflect a leverage effect, as the results hold even after controlling for leverage.

Taken together, the findings are consistent with the view that financial constraints are potentially an important determinant of firm-level employment decision. These results are related to Bakke and Whited (2012) that find, among other variables, a statistical relation between employment growth and mandatory pension contributions. Likewise, these findings are also similar in spirit to Campello, et al. (2011) who use survey evidence to show that credit lines served to ease the impact of the recent financial crisis on a battery of corporate decisions such as investment, R&D, and employment.

2.3 Does Capital Adjustment Drive Our Labor Findings?

One potential interpretation of the findings is that our results regarding employment decisions are driven solely by capital adjusting to financial constraints. Financial constraints, according to this view, do not affect labor directly since, unlike capital, labor does not require much financing. Instead, as in the financial constraints literature, investment is limited by the availability of internal funds, and labor, in turn, is adjusted for the decline in capital. That is, the sensitivity of labor to maturing debt stems from the omission of investment from the regressions and not from an intrinsic need to finance labor; Financial constraints cause firms to disinvest which leads to reductions in their labor force due to labor-capital complementarities.[5]

This alternative view hinges on the notion that while capital requires upfront investment to smooth the lumpiness associated with fixed costs, labor expenses are variable costs that are paid out of cash flow. An extreme variant of this story is the case in which labor is fully paid with the completion of a transaction – for example as in the case of waiters, bellhops, or realtors – and hence labor hoarding, hiring, and firing, will be unaffected by financing needs.

[5]Garmaise (2008) analyzes capital-labor decisions of financially constrained firms using small businesses data.

In most production activities, however, labor is not paid only upon the sale of goods in the market, but rather needs to be financed throughout the production process. This is particularly the case in manufacturing industries as opposed to services.[6] Further, the theoretical argument for labor representing solely a variable cost is not widely accepted. Research in labor economics has suggested that labor is not a variable factor of production but rather a fixed, or at least quasi-fixed, factor (see, e.g., Oi, 1962; Hamermesh, 1989; Hamermesh and Pfann, 1996). This argument has been suggested first by Oi (1962) who writes:

> *The cyclical behavior of labor markets reveals a number of puzzling features for which there are no truly satisfying explanations. [...] I believe that the major impediment to rational explanations for these phenomena lies in the classical treatment of labor as a purely variable factor. In this paper I propose a short-run theory of employment which rests on the premise that labor is a quasi-fixed factor. The fixed employment costs arise from investments by firms in hiring and training activities.*[7]

We argue that labor has fixed-costs aspects that require financing to bridge the difference between upfront costs and revenues. Put differently, if upfront labor-related costs are incurred prior to the realization of cash flow, the timing mismatch between cash outflows and inflows will generate a financing requirement.[8]

In order to test the alternative explanation that capital adjustments are fully responsible for the sensitivity of employment changes to financial constraints, we directly include *contemporaneous* changes in investment ($\%\Delta investment$) as well as the concurrent level of scaled investment ($Investment/Assets_{t-1}$) in our baseline regression specification (1). Results are reported in Table 4. If labor responds to changes in cash-flows indirectly, only through complementarities between labor and capital, then controlling for concurrent measures of investment should fully absorb this effect and β_{LT} in these regressions should be equal to zero.

Table 4 presents the results. First, as the table shows, both the change in investment and concurrent investment are positively and significantly correlated with employment change, suggesting that capital and labor indeed move together, most likely due to the demand for production factors

[6]The argument that labor must be financed is similar to that found in the literature on financial constraints and inventory investment: firms must finance inventory investment during the production process.

[7]See Oi (1962) page 538.

[8]One simple example of labor creating a financing requirement is the case of aviation engineers designing a new aircraft model. Since years may pass until the product is first sold on the market, engineer wages must be financed.

and capital-labor complementarities. Moreover, including the change in investment and concurrent investment raises the Adjusted R-Squared in the regressions from, for example, 0.12 in Column 1 of Table 3 to 0.33 in Column 1 of Table 4, suggesting that indeed the level and the change of investment are important determinants of employment at the firm level. Importantly, though, the results show that controlling for the contemporaneous change in investment ($\%\Delta investment$) as well as the concurrent level of scaled investment ($Investment/Assets_{t-1}$) barely affects the economic significance of our main findings. Our results continue to hold after taking into account capital adjustment, which suggests that labor-capital complementarities are not the driving force behind our findings.

One concern with the estimates reported in Table 4 is that the contemporaneous change in investment, $\%\Delta investment$, is endogenous and is determined simultaneously with $\%\Delta employees$. To further alleviate the concern that our results are driven entirely by a complementarity between capital and labor we stratify the sample by levels of capital intensity and show that our results continue to hold in firms with low capital intensity.

In particular, we calculate aggregate PP&E and total assets at the 4-digit SIC industry level for each year and define industry-year tangibility as total industry PP&E divided by total industry assets in a given year. Next, we assign firms into tangibility terciles based on their firm-level tangibility relative to the industry-based tangibility terciles.[9] Table 5 reports the results of the tangibility-stratified regressions and shows that the effect of maturing debt on employment holds also in low capital intensity firms.

To summarize, we find that labor is sensitive to maturing long-term debt even after accounting for contemporaneous changes in investment levels across different thresholds of maturing long-term debt. Moreover, maturing long-term debt affects employment in firms with both high and low capital intensity. Our analysis therefore suggests that the potential effect of financial constraints on employment is unlikely to be driven solely by an accompanying change in investment in response to these constraints.

[9]Note that since we define the tangibility terciles at the industry level, the number of firms across terciles is not equal since the distribution of firms across industries is not uniform.

2.4 Robustness: Using Different Thresholds for Maturing Debt

We next repeat our baseline analysis presented in Table 3 using the dummy variables described above that measures whether the value of long-term debt maturing in year $t+1$ and issued two, three, or four years prior to year t is greater than 5 ,10, or 15 percent of the book value of firm assets.

We estimate the following baseline regression specification:

$$\%\Delta employees_{it} = \alpha + \beta_{LT} \times (Long\ term\ debt\ due)_{it} + \mathbf{X}_{it-1}\lambda + \mathbf{y}_t\theta + \mathbf{z}_i\psi + \epsilon_{it}, \tag{2}$$

where the dependent variable: $\%\Delta employees$ is the annual percentage change in the number of employees within a firm. *Long term debt due*$_{it}$ is one of the dummy variables described above that measures whether the value of long-term debt maturing in year $t+1$ and issued two, three, or four years prior to year t is greater than 5, 10, or 15 percent of the book value of firm assets. $\mathbf{X}_{it-1}$ is a vector of firm specific control variables. These include lagged values of the firm market-to-book ratio, firm internal liquidity, $Liquidity_{it-1}$, the log of the book value of firm assets, firm leverage, asset maturity, and profitability. All regressions include year fixed effects, $\mathbf{y}_t$, and depending on the specification also include either four-digit SIC fixed effects or firm fixed-effects, denoted by the vector $\mathbf{z}$. All regressions are estimated with heteroscedasticity robust standard errors that are clustered by firm. Results are reported in Table 6.

As Table 6 demonstrates, we find that the negative relation between maturing long-term debt and changes in firm level employment is robust to the use of this different, thresholds-based definitions of maturing debt. Indeed, we find that the sensitivity of the change in the number of employees to maturing long-term debt, β_{LT}, is generally increasing with the threshold level. That is, the economic significance of the effect monotonically increases with the threshold level: as firms need to refinance a larger amount of debt, the reduction in employment levels is greater. However, the effects of debt issued at least four years prior to year t become statistically insignificant when we add firm fixed-effects since there is not sufficient within-firm variation when we combine large maturing debt cutoffs for debt issued long-time ago for the dummy variables.

2.5 Interest Coverage Stratification and the Effect of Maturing Debt on Employment

The rationale behind the maturing long-term debt empirical strategy is that episodes of illiquidity – and in particular long-term debt rollovers – will bring about reductions in employment due to the fact that external finance is costly. In this section, we further hypothesize that the effect of maturing long-term debt will be particularly important among financially weak firms. Raising external finance will be more costly for these firms and they may also lack a cash cushion that can be used to buffer episodes of illiquidity, as in the case of long-term debt rollovers. Thus, we analyze empirically whether the effect of maturing long-term debt on the change in the number of employees is higher for firms that are more likely to be financially constrained. In particular, we examine how the effects documented in the previous section vary by interest coverage levels.[10]

We calculate interest coverage, defined as EBITDA divided by the sum of interest expenses and debt in current liabilities, for each firm and year, associating lower coverage ratios with financially weaker firms. We then segment the entire sample into four groups based on three commonly used interest coverage threshold levels: 0.5, 1, and 2. Thus, the first group comprises all firms with interest coverage less than 0.5, the second group comprises those with interest coverage between 0.5 and 1, etc. We then re-estimate employment regressions similar to regression 1 for each of the groups, relating the percent change in employment to debt issued at least two years, three years, or four years prior to the base year scaled by beginning of the year assets. Table 7 reports the results.

As Panel A of Table 7 demonstrates, the sensitivity of the percentage change in employment to the maturing debt variable is indeed highest for firms with lower interest coverage ratios. For example, Column 1 of Panel A of Table 7 shows that β_{LT}, the coefficient on maturing debt for firms with coverage ratios below 0.5 and those between 0.5 and 1 are -0.167 and -0.215, respectively. In contrast, β_{LT} is -0.003 for firms with coverage rations of more than 2. As hypothesized, the results therefore show that the relation between maturing long-term debt and employment changes is indeed strongest in financially weak firms. Consistent with the notion that the cost of external finance is highest in these firms, financing labor will be more difficult during episodes of illiquidity.

Panels B and C of Table 7 repeat the analysis but consider longer lags of the maturing debt

[10]Sorting firms based on a-priori measures of financial constraints has been used in studies of investment such as Fazzari et al. (1988), Hoshi, Kashyap and Scharfstein (1991), Ramirez (1995) and Rauh (2006)) We note that these approaches have to be interpreted with usual caution and caveats as pointed in Kaplan and Zingales (1997). Some other studies discussing the role of financial constraints on investment decisions include Whited (1992), Kashyap, Lamont and Stein (1994), Calomiris and Hubbard (1995) and Gilchrist and Himmelberg (1995).

variable. As the table demonstrates, the results are quite similar, although when using debt issued at least four years ago some of the results are not statistically significant.

2.6 The Effect of Maturing Debt on Employment During Times of Tight Credit

We next turn to study the effect of maturing long-term debt on employment during times of tight credit. The main idea behind this approach is to study the effect of long-term debt that becomes due during times such as the Global Financial Crisis (GFC) and in general times in which credit supply is tight.[11]

We reestimate our baseline specification in regression 1 separately for the pre-GFC years (2004-2007) and GFC years (2008-2009) and report the results in Table 8. As the table demonstrates, the effect of maturing long term debt on employment – in particular, debt that was issued al least three or four years before the GFC – is larger during the years 2008 and 2009 as compared to the 2004-2007 period, while the effect of debt issued two years before year t is insignificant in both periods. For example, the coefficient on debt issued at least three years earlier is -0.028 during the 2004-2007 period, compared to -0.162 in 2008-2009. Likewise, during the 2004-2007 period, the coefficient on debt issued at least four years earlier is -0.072 compared to -0.133 in 2008-2009.

To further investigate the effect of maturing debt during times of tight credit on firm employment we use data from the Federal Reserve Senior Loan Officer Opinion Survey on Bank Lending Practices (SLOOSBLP). In this quarterly survey the Federal Reserve contacts up to eighty domestic bank and twenty-four U.S. branches and agencies of foreign banks and collects information on lending standards. We use the Net Percentage of Domestic Respondents reporting tightening of C&I Loans to large and medium firms as our measure of credit tightness. For each year in our sample we define $Tight\,Credit$ to be the simple average of the quarterly net percentage of loan officers reporting tightening, as compared to easing, of lending standards in that year. Higher values of the measure correspond to years in which credit standards tighten more. For example, in 2004, the net percentage of domestic responders reporting tightening of credit standards was -0.205, implying that most officers were easing rather than tightening credit standards. In contrast, in 2008 and 2009, the net percentage of domestic respondents reporting tightening of credit standard were 0.572 and 0.373, respectively.

We add $Tight\,Credit$ to the variables included in regression 1, as well as an interaction-term

[11] See Benmelech, Frydman and Papanikolaou (2019) for similar analysis during the Great Depression.

between $Tight\,Credit$ and the maturing debt variables. We hypothesize that the effect of maturing debt will be amplified during times of tight credit as firms will find it more costly to refinance their maturing obligations. Columns 7-9 of Table 8 report the results from estimating a specification that includes this interaction-term.[12] As Table 8 reports, the coefficients on the interaction-term between $Tight\,Credit$ and the maturing debt variables are all negative, and two of the three interaction coefficients (those using long-term debt issued three or four years prior to year t) are also statistically significant. For example, the results in Column 8 imply that a one standard deviation increase in $Tight\,Credit$ (0.229) is associated with an increase of the marginal effect of maturing debt on employment by 77.9 percent (from -0.062 to -0.110). These results confirm our hypothesis regarding the amplification of the maturing debt effect during crisis and lend credence to the maturing-debt-based identification strategy.

3. The Effect of Banking Deregulation on Unemployment

In the second 'quasi-experiment' we analyze the impact of bank deregulation on the level of state unemployment. Our methodology follows the seminal work of Jayaratne and Strahan (1996) that utilizes the introduction of state-level bank deregulation laws across the United States. Historically, U.S. banks faced legal restrictions on their ability to expand both within states and across state borders. The Douglas Amendment to the Bank Holding Company Act of 1956 barred, in effect, bank holding companies from expanding across state borders. In addition, most states had laws placing restrictions on the ability of bank holding companies to operate multiple branches in-state.

During the mid-1970s, states began to deregulate the banking industry by removing restrictions on both intrastate and interstate bank branching. States introduced laws that allowed bank holding companies to consolidate their subsidiaries into branches and to open new branches within state lines. Furthermore, states passed laws that allowed out-of-state banks to purchase banks within the state. Bank holding companies were thus enabled to expand across and within state lines. Prior studies have shown that state bank deregulation led to changes in the local banking industry, with associated increases in competition, improved bank efficiency, reductions in bank loan interest rates, and an increased likelihood of borrowing from banks (see e.g. Flannery, 1984; Jayaratne and Strahan, 1996, and Rice and Strahan, 2010). Further, bank deregulation has been shown to be related to real outcomes such as economic growth (Jayaratne and Strahan (1996)), income

[12] Note that that the variable $Tight\,Credit$ is fully absorbed by the year fixed effects.

distribution (Beck et al. (2010)), and economic volatility (Demyanyk et al. (2007)). In particular, while the main focus in Beck et al. (2010) is on the relation between finance and income inequality, they also show that intrastate branching deregulation reduced state-level unemployment.

Following these studies, we use cross-sectional and time-series variation in the introduction of *all* bank deregulation laws — i.e., both inter- and intra- state — to analyze the impact of positive shocks to banking markets on local unemployment levels. To do so, we collect information on state level unemployment from the Bureau of Labor Statistics for the period 1976-2009. Next, for each state, we obtain the year of inter- and intra-state banking deregulation. While banking deregulation occurred throughout the sample period, a large fraction of deregulation activity was concentrated in the mid to late 1980s. We use this information to define two dummy variables, *Intrastate Bank* and *Interstate Bank*. For any particular state, *Intrastate Bank*, takes on the value of one in all years following the introduction of the intra-state banking reform in that state. Similarly, *Interstate Bank* takes on the value of one in all years following the introduction of the inter-state banking reform. Our baseline regression specification is then as follows:

$$UE_{st} = \alpha + \beta \times Bank\,Deregulation_{st} + \mathbf{y}_t\theta + \mathbf{z}_s\psi + \epsilon_{st}, \tag{3}$$

where UE_{st} is the level of unemployment at state s at time t, $Bank\ Deregulation_{st}$ is one of the two bank deregulation dummy variables *Intrastate Bank* and *Interstate Bank* at state s at time t. We also include year fixed effects, $\mathbf{y}_t$ and state fixed-effects, $\mathbf{z}_s$. Year fixed effects control for nation-wide business cycle effects, while state fixed effects control for non time-varying determinants of state level unemployment such as regulatory predisposition or average tax rates. In some specifications we include state-trends rather than state fixed effects, while in others we include region-by-year fixed effects. Regions are defined as in Jayaratne and Strahan (1996) and split the United States into four groups: the Northeast, Midwest, West, and South. All regressions are estimated with heteroscedasticity robust standard errors which are clustered by state. Since the last state bank deregulation occurs in 1999 — by the state of Iowa — we run the regressions over the time period 1976-1999.[13] Our data comprise 1,152 state-year level observations.

Results of regression (3) are presented in Table 9. As can be seen, we find that banking deregulation is associated with reduced unemployment. Focusing first on intra-state deregulation (the first three columns of the table) we find that the introduction of intra-state deregulation

[13]Our results are robust to including additional years in the sample period to allow for a lag in the effect of banking deregulation.

reduces unemployment by between 0.45 and 0.86 percentage points. Since the average level of unemployment over the sample period is 6.16% percent, the economic magnitude of the effect is quite substantial. The last three columns of Table 9, analyze the effect of inter-state banking reform. Here too we find a consistent statistically significant negative relation between banking reform and unemployment. The effect also appears to be stronger than that of intra-state reform. Depending on the specification, passing inter-state banking reform laws which allow bank holding companies to expand across state lines reduces unemployment by between 0.84% and 1.14%, representing approximately a 15% decrease of the sample mean unemployment rate. These results are consistent with those presented in Beck et al. (2010) who find that banking deregulation reduces income inequality and with Pagano and Pica (2012) who show that across countries employment growth is associated with financial development.

While the results in Table 9 point to an important link between credit and unemployment they do not pin down the channel through which bank deregulation increase employment. However, coupled with prior evidence in the literature that points to an increase in bank loan allocation efficiency, reduction in interest rates, and diminishing economic volatility following bank deregulation, the results suggest that positive shocks to the financial intermediation environment within which businesses operate may have an important effect on firm employment outcomes.

4. The Effect of Japan's Real Estate Decline on Unemployment in the U.S.

The third 'quasi-experiment' provides more evidence on the link between finance and employment using a credit supply-shock experiment. We exploit a plausibly exogenous shock to bank loan supply in certain geographic areas in the U.S. and trace its impact on local unemployment rates. In particular, we study the contraction of loans made by Japanese affiliated banks in the U.S. during the early 1990s following the sharp economic downturn in Japan. As discussed in Peek and Rosengren (2000), this contraction in credit was due to negative shocks to the balance sheets of the Japanese parent banks of these affiliates as a result of the dramatic decline in real estate prices in Japan. While Japanese real estate shocks were arguably exogenous to investment opportunities of firms in the U.S., they led to a contraction in lending in U.S. regions in which Japanese affiliated banks were present.

At their peak in 1992, the penetration of Japanese banks in many real estate markets in the

U.S. was strikingly large.[14] This suggests that the contraction of such loans to firms in the vicinity of these banks could have a significant impact on the financial health of these firms – for instance by making refinancing of such loans difficult. In addition, reduction in real estate lending by Japanese-affiliated banks is also likely to be correlated with reduction in other types of credit provided by these banks.[15] The empirical strategy we follow mirrors Peek and Rosengren (2000) and seeks to trace out the impact of contraction of real estate loans by Japanese affiliated banks on unemployment in U.S. regions with substantial presence of these banks before the real estate collapse in Japan. The identification assumption relies on the notion that U.S-based firms in the vicinity of Japanese-affiliated banks will find it difficult to switch banks and escape the supply-side contraction in credit (e.g., due to asymmetric information and adverse selection effects in lending markets).

The data for this experiment are obtained from call reports provided by Chicago Federal Reserve Bank. In particular, we construct the market share (in terms of real estate loans) for Japanese owned banks in a given MSA. We follow Peek and Rosengren (2000) and first identify those entities that have a foreign owner (top holder) that is Japanese. We include those banks and branches where the entity has a U.S bank charter as well as branches of banks that do not have a U.S. charter. For each MSA, we create a panel dataset that includes all large domestically owned commercial banks located in the state that hold real estate loans in their portfolios, as well as Japanese bank branches and subsidiaries within the MSA. The domestically owned banks in these markets provide a comparison group for determining whether Japanese-owned banks presence has a differential effect on unemployment during the real estate crisis in Japan. Similar to Peek and Rosengren (2000) we restrict our analysis to MSAs where Japanese banks were present before the real estate peak in Japan in 1991.

The resulting dataset that we use is similar to the one reported in Peek and Rosengren (2000). Specifically, we find that MSAs in eight states have Japanese-bank-affiliate operations: California, Florida, Georgia, Illinois, New York, Oregon, Texas, and Washington. Two other states (Hawaii

[14]Peek and Rosengren (2000) note that, at their peak in 1992, U.S. subsidiaries and branches of Japanese banking organizations accounted for one-fifth of all commercial real estate loans held by domestically owned commercial banks plus foreign bank subsidiaries and branches in the United States. In many major urban markets, the Japanese penetration was far more substantial. Japanese branches and subsidiaries accounted at their peak for 44 percent of commercial real estate loans by large ($300 million or more in assets) U.S. commercial banks and foreign bank affiliates located in California, 35 percent in New York State, and 23 percent in Illinois.

[15]In our empirical analysis we confirm that this is indeed the case.

and Massachusetts) have Japanese bank presence for part of the sample period.[16]

We use Japanese affiliate real estate lending (log of total real estate loans by Japanese bank branches and subsidiaries located in a MSA) as an explanatory variable in explaining MSA unemployment levels. We obtain data on MSA level unemployment for the sample period from the Bureau of Labor Statistics. The other control variables include lagged log of state GDP, lagged log of labor force in the area, and lagged share of Japanese affiliate real estate lending relative to total real estate loans made by commercial banks in that MSA. We also include state fixed effects and a time trend to account for secular trends in unemployment. The data span the years 1990 to 1996 and all standard errors are clustered at the MSA level.

As Column 1 of Table 10 demonstrates, real estate lending by Japanese banks and affiliates does not explain MSA-level unemployment. In contrast, Column 2 shows that, there is a negative and statistically significant relation between real estate lending by non-Japanese banks (defined as log of total real estate loans by non-Japanese affiliated banks located in a MSA) and MSA unemployment.The results in Columns 1 & 2 provide average correlations across the sample period rather than the isolated effect of credit contraction by Japanese affiliated banks due to real estate decline in Japan. We now turn to the main empirical results in which we identify this effect.

We exploit time-series variation in the real-estate market in Japan using an annual Japanese real estate index as an instrument for the decline in U.S. lending by Japanese-affiliated banks. Column 3 presents the results obtained from the first stage of regressing lending on the Japanese real-estate index. Other controls in this regression are the same as those in Column 1. As can be seen from the table, there is a positive and statistically significant effect of the Japanese real estate index on real estate lending by the Japanese affiliated banks in the U.S. during the sample period. The effects are economically significant as well. In particular, the decline in the real estate index between 1993 and 1995 (about a 40 point change) led to about 24% decline in lending by Japanese affiliates.

Next, we assess how the decline in Japanese real estate prices transmitted into U.S. unemployment by estimating a two-stage least-squares specification — instrumenting the Japanese affiliated lending by the Japanese real estate index. The results are shown in Column 4. As can be seen, the IV estimates show that unemployment significantly increases in MSAs in which there was a

[16]The results reported in Table 10 include Hawaii and Massachusetts but are robust if we drop these states from our analysis.

contraction in Japanese affiliated banks following the real estate decline in Japan. These results are robust to the inclusion of state fixed-effects, as well as to time trends and state-trends (Column 5). The effects are economically large as well. The 24% contraction in lending by Japanese affiliated banks discussed above lead to a one percentage point increase in MSA-level unemployment. This is a reasonably large effect relative to mean unemployment rate of around 7.5% for these MSAs during the period of our analysis. These findings are consistent with those in Peek and Rosengren (2000) who show a drop in employment growth of construction workers in states with Japanese-affiliated lending after the real-estate collapse in Japan in early 1990s. However, our findings represent a broader decline in unemployment since we examine the impact of credit supply shock on unemployment rates across sectors within MSAs.

As a back of the envelope calculation, it is instructive to utilize these estimates to calculate the effect on unemployment caused by the negative loan supply shock that resulted from the real-estate price drop in the United States during the Great Recession. To this end, we use the Case-Shiller real estate index according to which average U.S. real-estate declined by approximately 30 percent during and in the aftermath of the recession. This drop is equivalent to a 60 point drop in the Japanese Index, which given our estimates, implies a 36% drop in real estate lending in the United States. Using the IV estimate from Column (5), this reduction in lending translates into a 1.9 percentage point increase in the MSA unemployment level. Since average unemployment in the US increased by around 5 percent points during the great recession, under the assumption that our estimates are externally valid, about 38 percent of the unemployment increase could be explained by the contraction in loan supply.

We next assess the robustness of our findings by conducting a placebo test. In particular, we estimate similar regressions to those in Columns 3-5, but instead instrument the non-Japanese affiliated bank lending by the Japanese real estate index. If the instrument is valid, changes in the real estate index in Japan should not be correlated with changes in the non-Japanese affiliated bank lending – and therefore should not correlate with changes in unemployment in the second stage. As can be observed in Column 6, the first stage reveals that there is indeed no correlation between movements of the Japanese real estate index and real estate lending by the non-Japanese affiliated banks in the U.S. Moreover, the second stage IV regressions in Columns 7 and 8 produce a statistically insignificant relationship between unemployment and non-Japanese affiliated bank lending – as instrumented by the Japanese real estate index. These tests therefore alleviate endogeneity

concerns and concerns that the results found earlier are driven by spurious correlations.

Finally, in unreported tests we confirm that the reduction in real estate lending by Japanese-affiliated banks is also correlated with reduction in other type of credit by these banks. All the results reported in this section are qualitatively similar when using total loans instead of real estate loans granted by Japanese and non-Japanese affiliated banks. Overall, we find a strong relationship between loan supply contraction and higher unemployment, further corroborating our central thesis that credit affects employment.

5. Conclusion

We analyze the effect of financial constraints, maturing debt, bank deregulation and bank balance-sheets shocks on firm employment and local-unemployment outcomes. By doing so we provide a collage of evidence showing that labor is sensitive to financial constraints and that unemployment is affected by the provision of credit. Finance appears to plays an important role in firm-level employment decisions. While most of our results are based on micro-level data or local measures of unemployment, our study has a broader message. Financial constraints and the availability of credit are important for employment and can potentially amplify variation in employment levels over the business cycle.

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Appendix A: Variable description and construction

For reference, the following is a list of the main variables used in the paper, their construction and their sources.

%Δ employees : the percentage change in number of employees from *t-1* to *t* [Compustat annual item *emp*]. (source: Compustat).

%Δ investment: the percentage change in investment from *t-1* to *t* [Compustat annual item *capx*]. (source: Compustat).

I/K: capital expenditure scaled by beginning of period's assets. [Compustat annual item $capx_t$ divided by at_{t-1}]. (source: Compustat).

Size: either the dollar book value or the natural logarithm of the book value of the assets [Compustat annual Item *at*] (Source: Compustat).

Market to Book: book value of assets [Compustat annual item *at*] plus the market value of equity [Compustat annual items *at+(csho*prcc_f)*] minus the book value of equity and deferred taxes [Compustat annual item *ceq+txdb*], all over (book value of assets*0.9 [Compustat annual item *at*]+market value of assets*0.10. (Source: Compustat).

Profitability: EBITDA [Compustat annual item *oibdp*] over beginning of period assets [Compustat annual item *at*] (Source: Compustat).

Leverage: total debt [Compustat annual items *dltt+dlc+dclo*] divided by total assets [Compustat annual item *at*]. (Source: Compustat).

Asset maturity: net property, plant, and equipment [Compustat annual item *ppegt*] divided by annual depreciation expenses [Compustat annual item *dp*]. (Source: Compustat).

Liquidity: cash plus marketable securities [Compustat annual item *cashplus*] divided by total assets [Compustat annual item *at*] (Source: Compustat).

Long-term debt due issued t years ago: a dummy that take the value of 1 if the amount of long-term debt maturing t+1 years after the annual reporting date [Compustat annual item *dd3*] lagged by t years divided by total assets [Compustat annual item *at*] is higher than 5%, 10%, 15% or 20%. (Source: Compustat).

Credit Rating Dummy: A dummy variable that takes the value of one and zero otherwise, if the firm has an S&P Long-Term Domestic Issuer Credit Rating. (Source: Standard and Poors).

Interest coverage: EBITDA [Compustat annual item *oibdp*] over the sum of interest expenses [Compustat annual item *xint* and debt in current liabilities. [Compustat annual item *dlc*] (Source: Compustat).

Tight Credit: The simple average of the quarterly net percentage of loan officers reporting tightening, as compared to easing, of lending standards in that year. (Source: Federal Reserve Senior Loan Officer Opinion Survey on Bank Lending Practices).

Table 1:
Maturing Long-term Debt Approach: Summary Statistics

This table provides descriptive statistics for the variables used in the analysis that uses the 'maturity-debt' approach. In Panel A we report the summary statistics of the amount of long-term debt coming due in the upcoming year as a percent of assets. In Panels B, C and D we report summary statistics of the dummy variable that takes a value of 1 if the maturing debt exceeds 5 percent, 10 percent and 15 percent of the firm's total assets, respectively. In each of the panels we take the long-term debt coming due in the upcoming year with an original maturity of greater than two, three, and four years. We report mean, median, 25th and 75th percentiles, standard deviation and the minimum and maximum values of these variables. Appendix A provides information on construction and definitions of these variables.

	Mean	25th Percentile	Median	75th Percentile	Standard Deviation	Min	Max
Panel A: Long-term debt due to total assets							
Long-term debt due issued 2 years ago	0.027	0.001	0.011	0.030	0.054	0.000	2.799
Long-term debt due issued 3 years ago	0.025	0.001	0.009	0.026	0.059	0.000	3.781
Long-term debt due issued 4 years ago	0.025	0.000	0.007	0.023	0.066	0.000	3.690
Panel B: Long-term debt due>5% of total assets (dummy variables)							
Long-term debt due issued 2 years ago	0.136	0.000	0.000	0.000	0.343	0.000	1.000
Long-term debt due issued 3 years ago	0.120	0.000	0.000	0.000	0.325	0.000	1.000
Long-term debt due issued 4 years ago	0.116	0.000	0.000	0.000	0.320	0.000	1.000
Panel C: Long-term debt due>10% of total assets (dummy variables)							
Long-term debt due issued 2 years ago	0.053	0.000	0.000	0.000	0.224	0.000	1.000
Long-term debt due issued 3 years ago	0.048	0.000	0.000	0.000	0.213	0.000	1.000
Long-term debt due issued 4 years ago	0.051	0.000	0.000	0.000	0.219	0.000	1.000
Panel D: Long-term debt due>15% of total assets (dummy variables)							
Long-term debt due issued 2 years ago	0.027	0.000	0.000	0.000	0.163	0.000	1.000
Long-term debt due issued 3 years ago	0.027	0.000	0.000	0.000	0.162	0.000	1.000
Long-term debt due issued 4 years ago	0.030	0.000	0.000	0.000	0.170	0.000	1.000

Table 2:
Descriptive Statistics: Main Variables

This table provides descriptive statistics for the variables used in the empirical analysis. We report mean, median, 25th and 75th percentiles, standard deviation and the minimum and maximum values of these variables. Appendix A provides information on construction and definitions of these variables.

	Mean	25th Percentile	Median	75th Percentile	Standard Deviation	Min	Max
Employees	10,684	1,373	3,400	10,637	18,354	500	123,600
%Δ employees	5.239%	-3.846%	1.497%	9.091%	22.351%	-72.727%	233.075%
%Δ investment	21.735%	-18.109%	7.072%	39.988%	75.319%	-100.0%	1400.021%
Investment/Assets$_{t-1}$	0.075	0.031	0.056	0.095	0.071	0.000	0.818
log(assets)$_t$	6.663	5.252	6.548	7.953	1.864	1.150	12.801
Asset Maturity$_{t-1}$	16.493	10.300	14.435	19.938	9.548	0.120	99.211
Q$_{t-1}$	1.355	0.956	1.158	1.543	0.644	0.151	9.576
Liquidity$_{t-1}$	0.095	0.016	0.048	0.127	0.121	0.000	0.979
Leverage$_{t-1}$	0.298	0.156	0.288	0.408	0.209	0.000	5.106
Profitability$_t$	0.148	0.094	0.139	0.196	0.102	-1.997	0.644
Interest coverage$_t$	7.675	1.129	2.305	4.874	26.882	-274.391	346.413

Table 3:
The Effect of Maturing Long-term Debt on Employment:

This table reports the results of regressions relating employment decision of firms to their maturing long-term debt for firms in our sample. The dependent variable used in the regressions is $\%\Delta employees$. All regressions include lagged values of the firm market-to-book ratio, firm internal liquidity, the log of the book value of firm assets, firm leverage, asset maturity, profitability and year fixed effects. The regressions also include four-digit SIC fixed effects or firm fixed effects. All regressions are estimated with heteroscedasticity robust standard errors which are clustered by firm and reported in parentheses. Variable definitions are provided in Appendix A. a, b and c denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	%Δ employees (1)	%Δ employees (2)	%Δ employees (3)	%Δ employees (4)	%Δ employees (5)	%Δ employees (6)
Long-term debt due issued 2 years ago	-0.064 a	-0.055 b				
	(0.023)	(0.022)				
Long-term debt due issued 3 years ago			-0.096 a	-0.0391 b		
			(0.020)	(0.020)		
Long-term debt due issued 4 years ago					-0.079 a	-0.012
					(0.020)	(0.021)
Q_{t-1}	0.039 a	0.032 a	0.031 a	0.025 b	0.023 a	0.017 a
	(0.005)	(0.006)	(0.005)	(0.006)	(0.004)	(0.006)
Liquidity_{t-1}	0.243 a	0.345 a	0.226 a	0.322 a	0.212 a	0.309 a
	(0.018)	(0.029)	(0.018)	(0.029)	(0.018)	(0.029)
Log size_{t-1}	-0.006	0.039 a	-0.005	0.044 a	-0.003 a	0.046 a
	(0.001)	(0.004)	(0.001)	(0.004)	(0.001)	(0.005)
Leverage_{t-1}	-0.009	-0.099 a	-0.009	-0.087 a	-0.009	-0.091 a
	(0.010)	(0.013)	(0.010)	(0.014)	(0.009)	(0.015)
$\text{Asset maturity}_{t-1}$	0.001 a	0.004 a	0.001 a	0.004 a	0.001 a	0.004 a
	(0.0002)	(0.001)	(0.0003)	(0.001)	(0.0002)	(0.0005)
Profitability_t	0.408 a	0.726 a	0.423 a	0.723 a	0.456 a	0.747 a
	(0.030)	(0.038)	(0.031)	(0.039)	(0.028)	(0.038)
Adjusted R^2	0.12	0.25	0.11	0.23	0.11	0.23
Fixed-Effects						
Year	Yes	Yes	Yes	Yes	Yes	Yes
4-digit SIC	Yes	No	Yes	No	Yes	No
Firm	No	Yes	No	Yes	No	Yes
Number of firms	4,421	4,421	4,055	4,055	3,695	3,695
Observations	39,706	39,706	35,636	35,636	31,921	31,921

Table 4:
Capital Adjustments and the Effect of Maturing Long-term Debt on Employment:

This table reports the results of regressions relating employment decision of firms to their maturing long-term debt for firms in our sample. The dependent variable used in the regressions is $\%\Delta employees$. All regressions include lagged values of the firm market-to-book ratio, firm internal liquidity, the log of the book value of firm assets, firm leverage, asset maturity, profitability and year fixed effects. We also control for contemporaneous investment change in the firm. The regressions also include four-digit SIC fixed effects or firm fixed effects. All regressions are estimated with heteroscedasticity robust standard errors which are clustered by firm and reported in parentheses. Variable definitions are provided in Appendix A. a, b and c denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	%Δ employees (1)	%Δ employees (2)	%Δ employees (3)	%Δ employees (4)	%Δ employees (5)	%Δ employees (6)
Long-term debt due issued 2 years ago	-0.060 a	-0.063 b				
	(0.017)	(0.019)				
Long-term debt due issued 3 years ago			-0.071 a	-0.043 a		
			(0.015)	(0.017)		
Long-term debt due issued 4 years ago					-0.053 a	-0.017
					(0.016)	(0.020)
%Δ investment$_t$	0.271 a	0.254 a	0.269 a	0.251 a	0.270 a	0.001
	(0.009)	(0.009)	(0.010)	(0.011)	(0.010)	(0.005)
I/K$_t$	0.419 a	0.418 a	0.399 a	0.431 a	0.341 a	0.366 a
	(0.028)	(0.032)	(0.029)	(0.035)	(0.030)	(0.037)
Q$_{t-1}$	0.012 a	0.007	0.010 a	0.006	0.005 a	0.001
	(0.003)	(0.005)	(0.004)	(0.005)	(0.003)	(0.005)
Liquidity$_{t-1}$	0.142 a	0.196 a	0.135 a	0.194 a	0.124 a	0.178 a
	(0.015)	(0.023)	(0.015)	(0.023)	(0.015)	(0.024)
Log size$_{t-1}$	-0.003 a	0.023 a	-0.002 a	0.027 a	-0.002 b	0.029 a
	(0.001)	(0.003)	(0.001)	(0.004)	(0.001)	(0.004)
Leverage$_{t-1}$	0.002	-0.051 a	0.002	-0.040 a	0.003	-0.040 a
	(0.007)	(0.012)	(0.006)	(0.012)	(0.007)	(0.012)
Asset maturity$_{t-1}$	-0.0003	0.002 a	-0.0002	0.002 a	0.0001	0.002 a
	(0.0002)	(0.0003)	(0.0002)	(0.0004)	(0.0002)	(0.0004)
Profitability$_t$	0.238 a	0.432 a	0.242 a	0.411 a	0.275 a	0.439 a
	(0.024)	(0.011)	(0.028)	(0.031)	(0.021)	(0.031)
Adjusted R^2	0.33	0.40	0.32	0.38	0.32	0.38
Fixed-Effects						
Year	Yes	Yes	Yes	Yes	Yes	Yes
4-digit SIC	Yes	No	Yes	No	Yes	No
Firm	No	Yes	No	Yes	No	Yes
Number of firms	4,168	4,168	3,824	3,824	3,499	3,499
Observations	37,561	37,561	33,731	33,731	30,239	30,239

Table 5:
The Effect of Maturing Long-term Debt on Employment Stratified by Tangibility

This table reports the results of regressions relating employment decision of firms to their maturing long-term debt for firms in our sample. The dependent variable used in the regressions is $\%\Delta employees$. All regressions include lagged values of the firm market-to-book ratio, firm internal liquidity, the log of the book value of firm assets, firm leverage, asset maturity, profitability and industry and year fixed effects. We stratify the regressions by industry-level tangibility terciles. All regressions are estimated with heteroscedasticity robust standard errors which are clustered by firm and reported in parentheses. Variable definitions are provided in Appendix A. a, b and c denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	Dependent variable: %Δ employees								
	First Tercile			Second Tercile			Third Tercile		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
LT debt due issued 2 years ago	-0.083 c (0.049)			-0.031 (0.042)			-0.074 b (0.030)		
LT debt due issued 3 years ago		-0.075 c (0.044)			-0.098 a (0.028)			-0.094 a (0.035)	
LT debt due issued 4 years ago			-0.069 b (0.035)			-0.078 b (0.032)			-0.080 b (0.039)
Adjusted R^2	0.15	0.15	0.14	0.12	0.12	0.12	0.11	0.10	0.10
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed-Effects									
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
4-digit SIC	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Firms	1,743	1,550	1,386	2,105	1,917	1,726	2,366	2,191	2,011
Observations	8,773	7,647	6,663	11,724	10,530	9,408	19,209	17,459	15,850

Table 6:
The Effect of Maturing Long-term Debt on Employment: Maturing Debt at least X% of Firm's Assets

This table reports the results of regressions relating employment decision of firms to their maturing long-term debt for firms in our sample. The dependent variable used in the regressions is %Δ*employees*. All regressions include lagged values of the firm market-to-book ratio, firm internal liquidity, the log of the book value of firm assets, firm leverage, asset maturity, profitability and year fixed effects. The regressions also include four-digit SIC fixed effects or firm fixed effects. All regressions are estimated with heteroscedasticity robust standard errors which are clustered by firm and reported in parentheses. Variable definitions are provided in Appendix A. a, b and c denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	%Δ employees (1)	%Δ employees (2)	%Δ employees (3)	%Δ employees (4)	%Δ employees (5)	%Δ employees (6)
Panel A: Long-term debt due>5% of total assets						
Long-term debt due	-0.003	-0.005				
issued 2 years ago	(0.003)	(0.004)				
Long-term debt due			-0.015 a	-0.010 b		
issued 3 years ago			(0.004)	(0.004)		
Long-term debt due					-0.013 a	-0.005
issued 4 years ago					(0.004)	(0.004)
Adjusted R^2	0.12	0.25	0.11	0.23	0.11	0.23
Panel B: Long-term debt due>10% of total assets						
Long-term debt due	-0.015 a	-0.014 b				
issued 2 years ago	(0.005)	(0.005)				
Long-term debt due			-0.023 a	-0.012 b		
issued 3 years ago			(0.005)	(0.005)		
Long-term debt due					-0.016 a	-0.004
issued 4 years ago					(0.006)	(0.006)
Adjusted R^2	0.12	0.25	0.11	0.23	0.11	0.23
Panel C: Long-term debt due>15% of total assets						
Long-term debt due	-0.022 a	-0.017 b				
issued 2 years ago	(0.007)	(0.007)				
Long-term debt due			-0.018 a	-0.0001		
issued 3 years ago			(0.007)	(0.008)		
Long-term debt due					-0.132 c	-0.001
issued 4 years ago					(0.007)	(0.009)
Adjusted R^2	0.12	0.25	0.11	0.23	0.11	0.23
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed-Effects: Year FE	Yes	Yes	Yes	Yes	Yes	Yes
4-digit SIC FE	Yes	No	Yes	No	Yes	No
Firm FE	No	Yes	No	Yes	No	Yes
Number of firms	4,421	4,421	4.055	4.055	3,695	3,695
Observations	39,709	39,709	35,636	35,636	31,921	31,921

Table 7:

The Effect of Maturing Long-term Debt on Employment Stratified by Interest Coverage

This table reports the results of regressions relating employment decision of firms to their maturing long-term debt for firms in our sample. The dependent variable used in the regressions is $\%\Delta employees$. All regressions include lagged values of the firm market-to-book ratio, firm internal liquidity, the log of the book value of firm assets, firm leverage, asset maturity, profitability and year fixed effects. We stratify the regressions by interest coverage ratios. The regressions also include four-digit SIC fixed effects or firm fixed effects. All regressions are estimated with heteroscedasticity robust standard errors which are clustered by firm and reported in parentheses. Variable definitions are provided in Appendix A. a, b and c denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	%Δ employees	%Δ employees	%Δ employees	%Δ employees
	(1)	(2)	(3)	(4)
Interest Coverage (IC)	IC<0.5	0.5≤IC<1	1≤IC <2	2≤IC
Long-term debt due	-0.167 a	-0.215 a	-0.153 a	-0.003
issued 2 years ago	(0.054)	(0.067)	(0.042)	(0.031)
Firm Controls	Yes	Yes	Yes	Yes
Year Fixed-Effects	Yes	Yes	Yes	Yes
Industry Fixed-Effect	Yes	Yes	Yes	Yes
Observations	4,202	4,082	8,567	22,855
Number of Firms	1,728	1,650	2,382	3,399
Adjusted R^2	0.16	0.15	0.17	0.14

	%Δ employees	%Δ employees	%Δ employees	%Δ employees
	(1)	(2)	(3)	(4)
Interest Coverage (IC)	IC<0.5	0.5≤IC<1	1≤IC <2	2≤IC
Long-term debt due	-0.162 a	-0.197 a	-0.136 a	-0.071 a
issued 3 years ago	(0.052)	(0.071)	(0.041)	(0.025)
Firm Controls	Yes	Yes	Yes	Yes
Year Fixed-Effects	Yes	Yes	Yes	Yes
Industry Fixed-Effect	Yes	Yes	Yes	Yes
Adjusted R^2	0.15	0.14	0.16	0.13
Number of Firms	1,516	1,498	2,178	3,126
Observations	3,635	3,676	7,747	20,578

	%Δ employees	%Δ employees	%Δ employees	%Δ employees
	(1)	(2)	(3)	(4)
Interest Coverage (IC)	IC<0.5	0.5≤IC<1	1≤IC <2	2≤IC
Long-term debt due	-0.080	-0.108	-0.101 c	-0.072 a
issued 4 years ago	(0.061)	(0.083)	(0.059)	(0.023)
Firm Controls	Yes	Yes	Yes	Yes
Year Fixed-Effects	Yes	Yes	Yes	Yes
Industry Fixed-Effect	Yes	Yes	Yes	Yes
Adjusted R^2	0.15	0.13	0.15	0.13
Number of Firms	1,351	1,372	1,985	2,867
Observations	3,176	3,289	6,991	18,465

Table 8:
The Effect of Maturing Long-term Debt on Employment: Crisis vs. Non-Crisis Years

This table reports the results of regressions relating employment decision of firms to their maturing long-term debt for firms in our sample. The dependent variable used in the regressions is $\%\Delta employees$. All regressions include lagged values of the firm market-to-book ratio, firm internal liquidity, the log of the book value of firm assets, firm leverage, asset maturity, profitability and year fixed effects. The regressions also include four-digit SIC fixed effects or firm fixed effects. Columns (7)-(9) also include interaction terms between *Tight Credit* and *LT debt due*. The regressions in Columns (1), (3) and (5) are estimated during the pre-crisis years (2004-2007). The regressions in Columns (2), (4) and (6) are estimated during the crisis years (2008-2009). The regressions in Columns (7)-(9) are estimated during the 2000-2019 period. All regressions are estimated with heteroscedasticity robust standard errors which are clustered by firm and reported in parentheses. Variable definitions are provided in Appendix A. a, b and c denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	Dependent variable: %Δ employees								
	(1) 2004-2007	(2) 2008-2009	(3) 2004-2007	(4) 2008-2009	(5) 2004-2007	(6) 2008-2009	(7) 2000-2019	(8) 2000-2019	(9) 2000-2019
LT debt due issued 2 years ago	-0.031	-0.029					-0.050 b		
	(0.061)	(0.109)					(0.024)		
LT debt due issued 3 years ago			-0.028	-0.162 b				-0.062 a	
			(0.042)	(0.076)				(0.019)	
LT debt due issued 4 years ago					-0.072 c	-0.133 b			-0.059 a
					(0.043)	(0.061)			(0.019)
Tight Credit× LT debt due issued 2 years ago							-0.121		
							(0.115)		
Tight Credit× LT debt due issued 3 years ago								-0.211 b	
								(0.085)	
Tight Credit× LT debt due issued 4 years ago									-0.153 c
									(0.083)
Adjusted R^2	0.15	0.17	0.14	0.15	0.13	0.13	0.15	0.14	0.13
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed-Effects									
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
4-digit SIC	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Firms	1,413	1,120	1,304	1,043	1,188	955	17,861	2,381	2,225
Observations	3,809	1,917	3,460	1,771	3,1808	1,597	2,539	16,540	15,159

Table 9:
Banking Deregulation and Unemployment

This table reports the results of regressions relating unemployment rates to the passing of state-level bank deregulation laws . The dependent variable is the state-level unemployment rates over the sample period 1976-1999. For each state, the two independent variables, *Intra-bank deregulation* and *Inter-bank deregulation* are dummy variables taking on the values of one in years following the passage of the state Intra- and Inter- banking deregulation laws. Region fixed effects are defined based on four U.S. geographic regions: Northeast, Midwest, West, and South. All regressions are estimated with heteroscedasticity robust standard errors which are clustered by state and are reported in parentheses. a, b and c denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	Unemployment	Unemployment	Unemployment	Unemployment	Unemployment	Unemployment
Intra-bank	-0.449 c	-0.856 b	-0.556 b			
deregulation	(0.236)	(0.359)	(0.227)			
Inter-bank				- 0.839 a	-1.081 a	-1.142 a
deregulation				(0.280)	(0.286)	(0.257)
Adjusted R^2	0.74	0.78	0.74	0.74	0.78	0.75
Fixed-Effects						
Year	Yes	Yes	Yes	Yes	Yes	Yes
State	Yes	Yes	Yes	Yes	Yes	Yes
State trends	No	Yes	No	No	Yes	No
Region*Year	No	No	Yes	No	No	Yes
Observations	1,152	1,152	1,152	1,152	1,152	1,152

Table 10:
Lending and Unemployment: The Effect of the Japanese Bank Crisis

This table reports the results of regressions relating unemployment rates in the U.S. to the lending by Japanese-affiliated banks during the real estate decline in Japan. The dependent variable is MSA-level unemployment rate over the sample period 1990-1996. For each MSA, we construct an independent variable, Japanese-affiliate lending which is the log of total real estate loans by Japanese bank branches and subsidiaries located in a MSA. Similarly, we construct the independent variable non-Japanese affiliate lending as the log of total real estate loans by non-Japanese commercial banks located in a MSA. These regressions all include time trends and state fixed effects. We also include state year trend fixed effects in two specifications reported in the table. All regressions are estimated with heteroscedasticity robust standard errors which are clustered by MSA and are reported in parentheses. a, b and c denote statistical significance at the 1%, 5%, and 10% levels, respectively. a, b and c denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	OLS		First Stage	IV		First Stage	IV	
	Unemployment		Japanese lending	Unemployment		Non-Japanese lending	Unemployment	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Japanese lending	0.070			-5.194 a	-4.290 a			
	(0.123)			(1.855)	(1.332)			
Non-Japanese lending		-0.756 b					54.97	-51.14
		(0.291)					(117.9)	(821.9)
Real Estate Index			0.006 a			-0.0005		
			(0.002)			(0.001)		
GDP_{t-1}	-3.918 a	-3.057 b		5.567	-0.755		-57.15	6.794
	(1.341)	(1.425)		(4.664)	(4.457)		(111.2)	(476.0)
Labor $force_{t-1}$	-0.676	0.368		10.39 b	8.490 a		-65.63	60.00
	(0.435)	(0.469)		(4.118)	(2.989)		(139.5)	(972.8)
Japanese $share_{t-1}$	-0.433	4.235		288.1 c	238.6 c		-55.69	59.72
	(9.414)	(8.481)		(163.4)	(126.7)		(160.0)	(913.2)
Adjusted R^2	0.297	0.317	0.693	.	.	0.813	.	.
Other Controls			Yes			Yes		
Time-trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed-Effects								
State	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State trend	No	No	No	No	Yes	No	No	Yes
Observations	684	684	684	684	684	684	684	684
Treated	68	68	68	68	68	68	68	68