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ABSTRACT

Financial market imperfections can have significant impact on employment decisions of firms. We illustrate the economic importance of this channel by demonstrating that the responsiveness of employment decisions to firms' financial health is quantitatively similar to the much-studied responsiveness of investment decisions to cash-flows. We use a collage of three 'quasi-experiments' used previously in the investment-cash flow and finance-growth literatures to trace the effects of finance on employment. Our 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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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 relation between financial constraints and corporate investment has been studied extensively, comparatively little is known about the role that financial constraints and the availability of finance play in determining the level of unemployment and its propagation over time. 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.

Theoretically, the cost and availability of external finance should affect firm employment decisions for a number of reasons. First, when there is a mismatch between payments to labor and the ultimate generation of cash flow, firms will need to finance their labor activity throughout the production process (see for example Greenwald and Stiglitz (1988)). As such, when the ability to finance working capital deteriorates, firm employment should fall.[1]

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 activities. Finally, the availability of external finance may affect employment indirectly through its impact on firm level investment. Specifically, as in the investment-cash-flow sensitivity literature, in the presence of capital market frictions investment is limited by the availability of internal funds, and due to complementariness between labor and capital, employment is adjusted for the decline in 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.[2] Chief among these is the concern that variables measuring

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

[2]For two surveys of the literature on capital market imperfections and investment see Hubbard (1998) and Stein (2003).

firms' financial health – such as net worth, firm leverage, earnings, and sales – are also correlated 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, CDS rates, or the Federal Funds rate are also correlated with demand for firms' final product and hence influence its demand for labor. These alternatives suggest that employment should be negatively related to firm level financial constraints and to 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 investment-cash flow and finance-growth literatures that were developed to alleviate these identification concerns. Consistent with a role of financial constraints, we first show using Compustat data that firm level employment over last forty years is indeed positively related to cash flow in a large panel of publicly traded firms. Using the sorting/sample-splits approach, we also show that the sensitivity of employment to cash flow is higher for firms with higher financial leverage.[3] We next provide evidence from three 'quasi-experiments' that enables better identification of the effects of finance on employment.

First, we follow the approach in Almeida et al. (2010) 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 their peers that 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 debt coming to maturity which needs refinancing, part of their adjustment occurs through a reduction in their labor force.

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 company 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

[3] For a similar result, see Sharpe (1994).

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, states did indeed experience a statistically and economically significant drop in their unemployment rates. Similar results are documented by Beck et al, (2010). 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. 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 US 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.

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 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 the effect of financial constraints on capital formation, our empirical results suggest that financial constraints seem to affect both factors of production.

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 much smaller yet emerging literature on labor and financial constraints (see Pagano and Volpin (2008) and Pagano (2010)). We discuss related studies in these areas when we describe our results.

The rest of the paper is organized in the following manner. Section 1 displays the analysis using the reduced form regressions relating employment levels to cash flows employing Compustat data. Section 2 presents the evidence from the three 'quasi-experiments'. Section 3 concludes.

I. Evidence from Employment Cash-flow Sensitivities

A. Firm-level Data and Summary Statistics

We utilize several data sets in our paper. The firm-level data are from the Compustat Annual Industrial Files. We use these files to collect information on all non-financial firms during the years 1970–2009 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 51,609 firm-year observations. All dollar figures are adjusted for inflation using the Consumer Product Index.

Table 1 reports descriptive statistics on the characteristics of the firms in the sample. The mean

[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.

number of employees is 13,075, the median is 1,300. Since we drop observations with less than 500 employees, the number of employees ranges from 500 to 876,800. The mean annual percentage change in the number of employees, $\%\Delta employees$, is 6.0% (median=1.7%) and ranges from -70.8% to 239.9%. The mean percentage change in investment, $\%\Delta investment$, is 12.6%, while the level of investment (measured as investment scaled by beginning of period assets) or I/K is 0.082 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 and a dummy for whether the firm has a credit rating. Appendix A provides detailed information on the definitions of the variables used in the paper, their construction, and their data sources.

B. Employment Cash-flow Regressions

We now turn to study the sensitivity of employment decisions to cash flows. Similar to other studies in the literature (see e.g., Fazzari et al (1998), Rauh (2006)), we estimate different variants of the following regression:

$$\%\Delta employees_{it} = \alpha + \beta_p \times Profitability_{it} + \mathbf{X}_{it-1}\lambda + \mathbf{y}_t\theta + \mathbf{z}_i\psi + \epsilon_{it}, \tag{1}$$

where the dependent variable: $\%\Delta employees$, is the annual percentage change in the number of employees. $\mathbf{X}_{it-1}$ is a vector of firm specific control variables which 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, profitability, and the credit rating dummy. All regressions include year fixed effects, $\mathbf{y}_t$, to account for changing macroeconomic conditions. In addition, we account for unobserved industry- or firm-level time invariant heterogeneity by including either four-digit SIC fixed-effects or firm fixed-effects, denoted by the variable $\mathbf{z}$. All regressions are estimated with heteroscedasticity robust standard errors which are clustered by firm.

The main focus of the analysis in this section is on the sensitivity of employment changes to $Profitability_{it}$ or cash flows which we measure, following standard literature practice, as operating income divided by beginning of period assets. As argued by Fazzari et al. (1998) (henceforth FHP), a Neoclassical model of investment with perfect capital markets implies that the coefficient of cash flow – β_p in specification 1 – should be zero. In contrast, a positive and significant coefficient implies that some firms face financial constraints due to limited access to external financing and hence must rely on internal cash flows.

The FHP approach has been subject to criticism based on either the endogeneity of the main explanatory variables – that is, cash flows are capturing investment opportunities not captured fully by Q – or on theoretical grounds (see for example, Poterba (1988), Kaplan and Zingales (1997) and Stein (2003)). We attempt to address these concerns in additional empirical tests in the next section.

We report the results from estimating different variants of regression 1 in Table 2. Each column in the table displays the estimates from a separate regression. The first two columns include all non-financial firms while columns 3 and 4 report results for manufacturing firms only. We use the same set of control variables in the first four regressions as well as year fixed-effects and industry- or firm-fixed effects depending on the specification. As can be seen, column 1 of Table 2 documents a positive and statistically significant relation between the percentage change in number of employees and profitability. The coefficient on profitability, β_p, is 0.446 and is statistically significant at the one percent level, controlling for a battery of firm variables and industry and year fixed-effects. A positive β_p suggests that when financial constraints are binding, the ability of a firm to increase its labor force is constrained by the availability of internal funds. The magnitude of the β_p coefficient implies that a one standard deviation increase in profitability is associated with a 7.6% change in the number of employees. This represents approximately a third of the standard deviation of the unconditional percentage change in the number of employees. While this magnitude should be taken with caution – given the concerns about omitted variables pointed earlier and the potential endogeneity of profitability – we note that the specification controls for lagged values of market-to-book ratio, firm internal liquidity, size, leverage and asset maturity.

Turning to the other control variables, we find that the change in the number of employees is, as expected, positively related to the market-to-book ratio and to firms with longer-lived assets. Other measures of the firm's finances are consistent with the positive relation between profitability and the change in number of employees. We find that liquidity is positively and statistically significant in explaining the change in the number of employees (coefficient=0.283 and is statistically significant at the 1 percent level). Likewise, similar to the results in Calomiris, Orphanides, and Sharpe (1994), hanka (1998), Ofek (1993) and Sharpe (1994), we find that the coefficient of leverage is negative and significant.

While the first column of Table 2 includes industry fixed effects, we use firm-fixed effects in the second column of the table. As can be seen, the inclusion of firm fixed effects increase the point estimates of the coefficients on profitability. In particular, as the second column of Table 2 shows, β_p is now 0.721 (significant at the one percent level). Focusing on firm internal liquidity or leverage rather than firm profitability reveals a similar result: labor is sensitive to both liquidity and firm leverage. Controlling for firm fixed-effects, the coefficient on liquidity is 0.351. Similarly, leverage exhibits a negative relation with employment that is about nine times higher than in column 1. [5]

In columns 3 and 4 we restrict the analysis to only manufacturing firms. This results in a sample size of 27,967 compared to the 51,609 firm-year observations in our previous sample.[6] As the table shows, our results – especially in specifications with firm fixed effects – are similar to those in columns 1 and 2. As column 3 shows, the coefficient on profitability β_p is 0.597 as compared to 0.446 in column 1. Similarly, as column 4 shows, once we control for firm fixed-effects, β_p=0.807 as compared to 0.721 in column 2.

Taken together, these 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 (2011) that finds, among other variables, employment growth is related to mandatory pension contributions. Likewise, these findings are also similar in spirit to Campello, Giambona, Graham and Harvey (2010) who use survey evidence to show that credit lines eased the impact of the recent financial crisis on a battery of corporate decisions such as investment, R&D and employment. However, given the concerns about the endogeneity of profitability, liquidity, and leverage and the relation between these variables and the economic opportunities available to firms, we are cautious at this stage in arguing for a causal link between financial measures and employment.

[5]While adding firm fixed-effects control for unobserved heterogeneity at the firm-level it potentially amplifies the omitted variable concern. The endogeneity problem is potentially exacerbated when we include firm fixed-effects since the estimates are identified off of innovations in cash flows, cash holdings or debt that are more likely to be correlated with innovations in economic opportunities as would be the case in a model of employment without financial constraints. For example, Kashyap, Lamont and Stein (1994) use corporate cash holdings instead of cash flow in specifications that do not include firm-fixed effects to capture the effect of 'stale' variation in corporate liquidity on inventories. Our results hold whether or not we include firm fixed-effects.

[6]We define manufacturing firms as those operating in 4-digit SIC 2000-3999.

B.1 Does Capital Adjustment Drive Labor Sensitivities to Cash-flows?

One potential interpretation of our findings is that our results are driven solely by capital adjusting to financial constraints. According to this view, financial constraints do not affect labor directly since, unlike capital, labor does not require financing. Instead, as in the investment-cash-flow sensitivity 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 cash flows stems from the omission of investment from the regressions and not from an intrinsic need to finance labor; financial pressure causes firms to disinvest which mechanically leads to reduction in their labor force.[7]

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 sales. 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 are unaffected by financing needs. Still, in most production activities, and particularly those associated with manufacturing as opposed to services – labor is not paid only upon the sale of goods in the market, but rather needs to be financed throughout the production process.[8] Indeed, the larger sensitivities of employment to cash flows found in the analysis with manufacturing firms in Table 2 are consistent with the notion that labor in manufacturing industries is more likely to require financing as compared to labor in service industries.

Nevertheless, even the theoretical argument for labor representing solely a variable cost is not widely acceptable. Research in labor economics has suggested that labor is not a variable factor of production but rather a fixed, or at least a quasi-fixed, factor (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*

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

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

from investments by firms in hiring and training activities.[9]

We argue that labor has fixed-costs aspects that require financing to bridge upfront costs and revenues. These may give rise to the role that financial constraints play in the inability of firms to hoard highly trained employees even when the decline in demand for the firm's goods may be temporary

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 the employment-based regressions of specification 1. Results are reported in the last four columns of Table 2. If labor responds to cash-flows only through indirect complementarities between labor and capital, then controlling for concurrent measures of investment should fully absorb this effect and β_p in these regressions should be equal to zero.

As Table 2 demonstrates, controlling for the contemporaneous changes in investments ($\%\Delta investment$) as well as the concurrent level of scaled investment ($Investment/Assets_{t-1}$) barely affects the economic significance of our main findings. Both the percent change in investment and concurrent investment are positively and significantly correlated with employment change, suggesting that capital and labor indeed move together, probably due to the demand for production factors and capital-labor complementarities. In particular, the coefficients on the financial variables are hardly affected by the inclusion of investment-based measures. As column 1 shows, when we account for industry fixed-effects β_p declines to 0.336 (compared to 0.446 in column 1 of Table 2). Including firm fixed-effects results in a decrease of β_p from 0.721 to 0.600. Similar patterns are observed for the liquidity measure and for manufacturing firms (columns 7 and 8).[10]

To summarize, we find that labor is sensitive to cash flows even after accounting for the contemporaneous changes in investment. Our analysis therefore suggests that the potential effect of financial constraints on employment is unlikely to be driven entirely by an accompanying change in investment in response to these constraints.

[9]See Oi (1962) page 538.

[10]In unreported results we also add as an additional control the 4-digit SIC TFP growth. By doing so we are trying to control better for investment opportunities making sure our results are not likely to be driven by the omitted investment opportunities. Our results are unchanged.

C. Leverage Stratification and Employment and Investment Cash-flow Sensitivities

We now turn to test whether the sensitivity of 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 we document vary with the financial leverage of the firm. To do so we sort manufacturing firms into two groups based on their leverage – below and above the median. Sorting firms based on a-priori measures of financial constraints and estimating investment cash-flow sensitivities has been used in several previous studies of investment (e.g. Fazzari et al. (1988), Hoshi, Kashyap and Scharfstein (1991), Ramirez (1995) and Rauh (2006)).[11]

We re-estimate the employment regressions for each of the groups and report the results in Table 3. All the regressions are estimated with year and either industry of firm fixed-effects. As Table 3 shows, the sensitivity of the percentage changes in employment to cash-flows increases when moving from firms with leverage below the median to more levered firms. For example, β_p is 0.569 for high levered firms as compared to 0.391 for low levered firms in industry fixed-effects specifications, and is 0.897 compared to 0.633 when we include firm fixed-effects. Similar results are documented in manufacturing firms (columns 5-8).

These tests also suggest that measurement error concerns highlighted earlier are not likely driving our findings. In particular, one could have plausibly argued that our earlier regressions evaluating the labor and investment sensitivities suffered from measurement error in investment opportunities – as measured by Q – which in turn is captured partially by financial health variables. Consequently, the relation between financial health and employment was spurious and represented only a mechanical relation between investment opportunities and labor. By demonstrating that our results increase with leverage, we alleviate some of these endogeneity concerns. The relevant criticism for our findings therefore has to be that not only is there measurement error in investment opportunities – but also that this error has to be worse for highly leveraged firms. However, it is not a-priori clear why the measurement error in Q should be correlated with leverage as a stratifying variable.

[11] 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).

II. Evidence from Three Quasi-Experiments

Our findings in Section 1 are consistent with a role of financial constraints in firms' employment decisions but are also consistent with a Neoclassical model of investment and labor demand. We next turn to evidence from three 'quasi-experiments' used in the investment-cash flow and finance-growth literatures that allow us to more cleanly trace the effects of finance on employment.

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

We now attempt to alleviate endogeneity concerns about profitability and leverage by using the 'maturing-debt' approach pioneered by Almeida et al. (2009). The 'maturing-debt' empirical strategy 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 investment (as in Almeida et al. (2009)) or labor force (as in our paper) by more than their peers that do not face the need to refinance maturing long-term debt in the same year. If external capital is costly (e.g., Myers and Majluf (1984)) then firms which need to refinance large amounts of maturing long-term debt will, as a result, adjust their real activity and reduce employment.

The identification strategy relies 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, we use as our main independent variable measures of maturing long-term debt which take into account liabilities that were issued with a time-lag to the year of interest. For example, we compare employment outcomes of firms which in a particular year have a large amount of maturing debt (issued two, three or four years prior to the year we study) to those with a small amount of such debt maturing. Since this portion of the maturing debt was issued prior to the year of maturity, variation in its level is arguably exogenous to market conditions and investment opportunities that eventually arise in the year in which the debt becomes due.

Compustat reports the amount of long-term debt which is payable in more than one year through more than five years from the firm's fiscal year end. We collect data on the amount of future maturing debt. Specifically, we utilize Compustat variables *dd3*, *dd4*, and *dd5* which 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 maturing in the upcoming year of debt that was issued at least two, three, or four years prior to the base year. For example, at year t, *l2_dd3* measures the amount of long-term debt maturing at $t+1$ that was issued *prior* to year $t-2$. We scale the lagged variables by beginning of year assets.

Next, we construct a dummy variable that takes 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, 15, and 20 percent threshold levels. These variables capture whether a firm has a significant amount of long term debt maturing in the upcoming year which requires refinancing. By examining debt that was issued prior to the year of analysis, we alleviate concerns that the level of maturing debt co-moves with other market variables which have a direct impact on employment decisions. As control variables we use the same set of controls as in the previous section. Following Lamont, Kashyap, and Stein (1994), we also construct a dummy variable which takes on a value of one if a firm has a credit rating to measure the firm's access to the long-term bond market.[12]

Table 4 provides summary statistics for the maturing debt variables. 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 2.6, 2.4, and 2.3 percent of assets, respectively. We next define dummy variables that take the value of 1 if the maturing debt exceeds 5, 10, and 15 percent of the firm's total assets. As the table shows, 13.4 percent of firm-year observation have refinancing requirements that exceed 5 percent of total assets and that were issued at least 2 year prior to the year in which the debt comes due. Turning to higher levels of maturing debt, Table 4 shows that 4.9 percent of firm-year observations in the sample must refinance maturing long-term debt that was issued at least 2 years before the current year and that exceeds 10 percent of total assets. Similarly, 2.5 percent of the sample need to refinance maturing long-term debt that is higher than 15 percent of total assets.

Having defined the maturing debt variables, our baseline regression specification is:

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

where the dependent variable: $\%\Delta employees$ is the annual percentage change in the number of

[12] As is standard, we assume that firms with a missing observation in their credit rating are unrated.

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, or 10 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, profitability, and the credit rating dummy. 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 which are clustered by firm. Similar to the analysis in Table 2 we control for both the contemporaneous change in investment, $\%\Delta investment$, as well as the concurrent level of scaled investment ($Investment/Assets_{t-1}$) to control for the possibility that the effect on employment is completely driven by an accompanying change in investment and not through a direct link between the firm's financial position and its ability to retain labor or its need to fire employees.

Results are presented in Table 5. 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. The coefficient of -0.017 (statistically significant at the 1 percent level) implies that firms that have maturing debt that requires refinancing and that is worth at least 5% of the firm's total assets reduce the number of their employees by close to two percent. 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=-0.012, statistically significant at the 1 percent level).

Next, we further lag the maturing debt variable to ensure that financing decisions do 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 3 years prior to the base year. β_{LT} is -0.021 (significant at the 1 percent-level) when we include industry fixed-effects compared to -0.013 (significant at the 5 percent level) when we control for firm fixed-effects. Likewise, even when we lag debt issuance by 4 years (columns 5 and 6) we find that the effect of maturing debt on employment is negative and significant (-0.023 and -0.014 for industry- or firm-fixed effects, respectively.) We obtain similar results when we repeat the analysis using a dummy variable for maturing long-term debt that exceed ten percent of total assets (Table 6). As the table shows, the sensitivity of the change in the number of employees to maturing long-term debt β_{LT}, is now higher and equals -0.021 which is consistent with a larger refinancing need than the five percent-based dummy variable in Table 5. Furthermore, our main result still hold – and in some specifications the effect is indeed larger – when we lag the maturing debt variable by 3 or 4 years.

It is also important to note that while we focus our attention on maturing long-term debt as the key explanatory variable in our regressions, we still obtain the same magnitudes as before for both the profitability, liquidity and leverage variables. In some 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 each of the dependent variables.

As would be expected, we also find that the firm market-to-book ratio is positively related to employment growth. 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. 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.

We have also repeated the analysis of regression 2, using different threshold levels to define significant levels of long-term debt. In particular, rather than using 5% and 10% thresholds, we define dummy variables that take on the value of one if long-term debt maturing in the upcoming year is greater than 15 or 20 percent of assets. In unreported results we find that the negative relation between upcoming long-term debt and changes in firm level employment are robust to using different threshold levels when we control for 4-digit SIC fixed-effects. Further, as would be expected, 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, some of these effects become statistically insignificant when we add firm fixed-effects since there is not sufficient within-firm variation when we require very large maturing debt cutoffs for the dummy variables.

B. 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) which utilizes the introduction of state-level bank deregulation laws across the U.S. 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 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 banking deregulation laws reduced state-level unemployment.

Following these studies, we use cross-sectional and time-series variation in the introduction of bank deregulation laws – 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:

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

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, $\mathbf{z}_t$. 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 a state year-trend variable 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 comprises 1,152 state-year level observations.

Results of regression (3) are presented in Table 7. 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 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 7, 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. Similar results are presented in Beck et al (2010) who find that banking deregulation reduces income inequality and state-level unemployment, and by Pagano and Pica (2011) who show that across countries employment growth is associated with financial development.

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

While the results in Table 7 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.

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

We now provide more evidence on the link between finance and employment using a credit supply-shock experiment. We exploit a plausibly exogenous shock to bank loans 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 sheet 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 relatively 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 type of credit 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 due to asymmetric information in lending, U.S-based firms in the vicinity of Japanese-affiliated banks will find it difficult to switch banks and

[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.

escape the supply-side contraction in credit.

The data for this experiment come 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 data set 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 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 and cluster the standard errors at MSA level. The data spans the years 1990 to 1996.

As column 1 of Table 8 demonstrates, real estate lending by Japanese banks and affiliates does not explain MSA-level unemployment. In contrast, column 2 shows that, real estate lending by non-Japanese banks (defined as log of total real estate loans by non-Japanese affiliated banks located in a MSA) has a negative and statistically significant effect on unemployment during the same period

[16] The results reported in Table 8 include Hawaii and Massachusetts but are robust if we drop these states from our analysis.

suggesting that, in general, non-Japanese bank presence has a larger effect on 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 present 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 real estate index between 1993 and 1995 (about a 40 point change) led to about 16% decline in lending by Japanese affiliates around its mean level ($40 * .0055/1.43$).

We then assess how this translates into local unemployment by estimating a two-stage least-squares specification – instrumenting the Japanese affiliated lending by the Japanese real estate index – in column 4. As our results demonstrate, the IV estimates suggest that unemployment significantly increases in MSAs in which there was a contraction in Japanese affiliated banks following the real estate decline in Japan. These results are robust to the inclusion of state fixed-effect as well as time trends and state-trends in addition to additional controls. These effects are economically large as well. The 16% 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.

We next assess the robustness of our findings by conducting a placebo test. In particular, we estimate similar regressions as those in columns 3-5 instrumenting the non-Japanese affiliated bank lending by the Japanese real estate index. If the instrument is valid then 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 no correlation between movements of Japanese real estate index and real estate lending by the non-Japanese affiliated banks in the US. Moreover, the second stage IV regressions in columns 7 and 8 produce a statistically insignificant relationship between unemployment and non-Japanese affiliated bank lending. These tests therefore alleviate endogeneity concerns and concerns that the results found earlier are driven by spurious correlation.

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 if we use 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 which further corroborates our central thesis that credit affects employment.

III. 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 bank credit. This leads us to conclude that finance plays an important role in firm-level employment decisions. While most of our results are based on micro-level data or local 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).

Table 1:
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	13,075	1,300	3,100	9,800	36,400	500	876,800
%Δ employees	6.043%	-4.213%	1.688%	10.280%	24.683%	-70.782%	239.906%
%Δ investment	12.570%	-29.323%	-2.914%	29.640%	82.676%	-99.964%	1137.991%
Investment/Assets$_{t-1}$	0.082	0.033	0.060	0.102	0.079	0.000	0.913
Size$_{t-1}$	724.116	36.557	113.156	476.125	2,379.384	1.238	81,380.850
Asset Maturity$_{t-1}$	16.275	10.183	14.475	19.554	9.536	0.713	99.211
Q$_{t-1}$	1.366	0.938	1.139	1.549	0.710	0.274	9.576
Liquidity$_{t-1}$	0.096	0.017	0.048	0.122	0.125	0.000	0.974
Leverage$_{t-1}$	0.292	0.142	0.280	0.408	0.210	0.000	5.106
Profitability$_{t}$	0.149	0.093	0.140	0.200	0.106	-1.932	0.621
Credit rating dummy$_{t-1}$	0.430	0.000	0.000	1.000	0.495	0.000	1.000

Table 2:
Employment, Investment and Cash Flow

This table reports the results of regressions relating employment decision of firms to their cash flows for all non-financial firms and manufacturing firms in our sample. Manufacturing firms are defined to be those that are operating in 4-digit SIC 2000-3999. 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, the credit rating dummy and year fixed effects. We also include four-digit SIC fixed effects or firm fixed effects in these regressions. We control for contemporaneous change in investment as well as for the concurrent level of scaled investment in last four regressions. 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.

	All non-financial firms		Manufacturing firms only		All non-financial firms		Manufacturing firms only	
	%Δ employees	%Δ employees	%Δ employees	%Δ employees	%Δ employees	%Δ employees	%Δ employees	%Δ employees
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Q_{t-1}	0.043 a	0.039 a	0.012 b	0.011 c	0.030 a	0.027 a	0.007	0.009 a
	(0.004)	(0.005)	(0.005)	(0.006)	(0.004)	(0.005)	(0.005)	(0.006)
Liquidity_{t-1}	0.283 a	0.351 a	0.293 a	0.316 a	0.275 a	0.340 a	0.283 a	0.298 a
	(0.018)	(0.026)	(0.022)	(0.033)	(0.016)	(0.024)	(0.020)	(0.030)
Log size_{t-1}	-0.007 a	0.033 a	-0.008 a	0.039 a	-0.004 a	0.036 a	-0.005 a	0.043 a
	(0.001)	(0.004)	(0.001)	(0.005)	(0.001)	(0.004)	(0.001)	(0.005)
Leverage_{t-1}	-0.013 c	-0.096 a	0.007	-0.080 a	0.011	-0.060 a	0.013	-0.057 a
	(0.007)	(0.013)	(0.010)	(0.017)	(0.008)	(0.013)	(0.010)	(0.016)
Asset	0.002 a	0.005 a	0.001 a	0.006 a	0.001 a	0.003 a	0.0001	0.004 a
maturity_{t-1}	(0.0003)	(0.0004)	(0.0003)	(0.001)	(0.0002)	(0.0004)	(0.0004)	(0.001)
Profitability_t	0.446 a	0.721 a	0.597 a	0.807 a	0.336 a	0.600 a	0.477 a	0.701 a
	(0.027)	(0.035)	(0.031)	(0.040)	(0.025)	(0.035)	(0.029)	(0.038)
%Δ investment_t					0.023 a	0.020 a	0.027 a	0.026 a
					(0.002)	(0.002)	(0.003)	(0.003)
I/K_t					0.676 a	0.595 a	0.583 a	0.429 a
					(0.030)	(0.039)	(0.048)	(0.056)
Adjusted R^2	0.15	0.29	0.15	0.27	0.19	0.32	0.18	0.29
Fixed-Effects								
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
4-digit SIC	Yes	No	Yes	No	Yes	No	Yes	No
Firm	No	Yes	No	Yes	No	Yes	No	Yes
Observations	51,609	51,609	27,967	27,967	51,609	51,609	27,967	27,967

Table 3:
Employment and Investment Cash Flow Sensitivities: Stratified by Leverage

This table reports the results of regressions relating employment decision of firms to their cash flows estimated in samples stratified by leverage. The first four specifications are estimated using non-financial firms while last four specifications use only manufacturing firms. Manufacturing firms are defined to be those that are operating in 4-digit SIC 2000-3999. 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, the credit rating dummy and year fixed effects. We also include four-digit SIC fixed effects or firm fixed effects in these regressions. 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.

	All non-financial firms				Manufacturing firms only			
	Leverage		Leverage		Leverage		Leverage	
	low	*high*	*low*	*high*	*low*	*high*	*low*	*high*
	%Δ employees (1)	%Δ employees (2)	%Δ employees (3)	%Δ employees (4)	%Δ employees (5)	%Δ employees (6)	%Δ employees (7)	%Δ employees (8)
Q_{t-1}	0.045 a	0.055 a	0.034 a	0.064 a	0.015 a	0.021 b	0.003	0.038 a
	(0.005)	(0.007)	(0.006)	(0.010)	(0.005)	(0.010)	(0.007)	(0.013)
Liquidity_{t-1}	0.270 a	0.395 a	0.324 a	0.487 a	0.307 a	0.334 a	0.321 a	0.370 a
	(0.020)	(0.035)	(0.031)	(0.055)	(0.026)	(0.040)	(0.041)	(0.071)
Log size_{t-1}	-0.011 a	-0.006 a	0.028 a	0.040 a	-0.010 a	-0.008 a	0.033 a	0.053 a
	(0.001)	(0.001)	(0.007)	(0.006)	(0.002)	(0.002)	(0.009)	(0.008)
Leverage_{t-1}	0.123 a	-0.060 a	-0.068 a	-0.141 a	0.124 a	-0.060 a	-0.085 b	-0.093 a
	(0.022)	(0.013)	(0.032)	(0.020)	(0.027)	(0.017)	(0.038)	(0.026)
Asset maturity_{t-1}	0.001 c	0.002 a	0.005 a	0.005 a	0.001	0.002 a	0.006 a	0.006 a
	(0.0004)	(0.0002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Profitability_{t}	0.391 a	0.569 a	0.633 a	0.897 a	0.561 a	0.708 a	0.751 a	0.905 a
	(0.034)	(0.039)	(0.044)	(0.055)	(0.035)	(0.057)	(0.046)	(0.071)
Adjusted R^2	0.17	0.13	0.37	0.27	0.16	0.14	0.34	0.23
Fixed-Effects								
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
4-digit SIC	Yes	Yes	No	No	Yes	Yes	No	No
Firm	No	No	Yes	Yes	No	No	Yes	Yes
Observations	25,812	25,797	25,812	25,797	16,687	11,280	16,687	11,280

Table 4:
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	0.026	0.001	0.011	0.030	0.053	0.000	2.844
Long-term debt due issued 3 years ago	0.024	0.0007	0.009	0.026	0.052	0.000	1.888
Long-term debt due issued 4 years ago	0.023	0.0003	0.008	0.024	0.058	0.000	2.365
Panel B: Long-term debt due>5% of total assets (dummy variables)							
Long-term debt due issued 2 years ago	0.134	0.000	0.000	0.000	0.340	0.000	1.000
Long-term debt due issued 3 years ago	0.116	0.000	0.000	0.000	0.320	0.000	1.000
Long-term debt due issued 4 years ago	0.111	0.000	0.000	0.000	0.314	0.000	1.000
Panel C: Long-term debt due>10% of total assets (dummy variables)							
Long-term debt due issued 2 years ago	0.049	0.000	0.000	0.000	0.215	0.000	1.000
Long-term debt due issued 3 years ago	0.042	0.000	0.000	0.000	0.199	0.000	1.000
Long-term debt due issued 4 years ago	0.041	0.000	0.000	0.000	0.200	0.000	1.000
Panel D: Long-term debt due>15% of total assets (dummy variables)							
Long-term debt due issued 2 years ago	0.025	0.000	0.000	0.000	0.157	0.000	1.000
Long-term debt due issued 3 years ago	0.022	0.000	0.000	0.000	0.146	0.000	1.000
Long-term debt due issued 4 years ago	0.024	0.000	0.000	0.000	0.152	0.000	1.000

Table 5:
The Effect of Maturing Long-term Debt on Employment
(Maturing Debt at least 5% 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 $\%\Delta employees$. 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, the credit rating dummy 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.

	Long-term debt due>5% of total assets					
	%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	-0.017 a	-0.012 b				
	(0.004)	(0.005)				
Long-term debt due issued 3 years ago			-0.021 a	-0.013 b		
			(0.004)	(0.005)		
Long-term debt due issued 4 years ago					-0.023 a	-0.014 a
					(0.005)	(0.005)
%Δ investment$_t$	0.032 a	0.025 a	0.033 a	0.026 a	0.030 a	0.024 a
	(0.003)	(0.004)	(0.003)	(0.004)	(0.003)	(0.003)
I/K$_t$	0.632 a	0.572 a	0.629 a	0.583 a	0.611 a	0.581 a
	(0.048)	(0.067)	(0.049)	(0.068)	(0.050)	(0.070)
Q$_{t-1}$	0.013 b	0.003	0.009 a	0.002	0.009 b	0.003
	(0.004)	(0.006)	(0.004)	(0.006)	(0.004)	(0.006)
Liquidity$_{t-1}$	0.252 a	0.347 a	0.240 a	0.317 a	0.246 a	0.317 a
	(0.020)	(0.035)	(0.020)	(0.034)	(0.020)	(0.034)
Log size$_{t-1}$	0.005 a	0.058 a	0.006 a	0.059 a	0.006 a	0.054 a
	(0.002)	(0.006)	(0.002)	(0.006)	(0.002)	(0.006)
Leverage$_{t-1}$	0.014	-0.048 a	0.016	-0.049 a	0.017	-0.051 a
	(0.010)	(0.017)	(0.010)	(0.018)	(0.011)	(0.019)
Asset maturity$_{t-1}$	0.001 c	0.004 a	0.001 b	0.004 a	0.001 a	0.004 a
	(0.0003)	(0.001)	(0.0003)	(0.001)	(0.0003)	(0.001)
Credit rating dummy$_{t-1}$	-0.027 a	-0.035 a	-0.030 a	-0.036 a	-0.031 a	-0.043 a
	(0.005)	(0.009)	(0.005)	(0.009)	(0.005)	(0.009)
Profitability$_t$	0.382 a	0.684 a	0.414 a	0.702 a	0.433 a	0.711 a
	(0.031)	(0.044)	(0.031)	(0.044)	(0.030)	(0.046)
Adjusted R^2	0.17	0.29	0.17	0.29	0.17	0.30
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
Observations	25,374	25,374	24,940	24,940	23,779	23,779

Table 6:
The Effect of Maturing Long-term Debt on Employment (Maturing Debt at least 10% 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 $\%\Delta employees$. 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, the credit rating dummy 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.

	Long-term debt due>10% of total assets					
	%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	-0.021 a	-0.013 c				
	(0.006)	(0.007)				
Long-term debt due issued 3 years ago			-0.029 a	-0.013 c		
			(0.006)	(0.007)		
Long-term debt due issued 4 years ago					-0.027 a	-0.013 c
					(0.007)	(0.007)
%Δ investment$_t$	0.031 a	0.025 a	0.033 a	0.026 a	0.030 a	0.024 a
	(0.003)	(0.004)	(0.003)	(0.004)	(0.003)	(0.003)
I/K$_t$	0.633 a	0.574 a	0.630 a	0.585 a	0.614 a	0.583 a
	(0.048)	(0.067)	(0.049)	(0.068)	(0.050)	(0.070)
Q$_{t-1}$	0.013 b	0.003	0.010 b	0.002	0.009 b	0.003
	(0.004)	(0.006)	(0.004)	(0.006)	(0.004)	(0.006)
Liquidity$_{t-1}$	0.253 a	0.347 a	0.240 a	0.317 a	0.247 a	0.316 a
	(0.020)	(0.035)	(0.020)	(0.034)	(0.020)	(0.034)
Log size$_{t-1}$	0.005 a	0.058 a	0.005 a	0.059 a	0.006 a	0.054 a
	(0.002)	(0.006)	(0.002)	(0.006)	(0.002)	(0.006)
Leverage$_{t-1}$	0.012	-0.050 a	0.014	-0.051 a	0.015	-0.052 a
	(0.010)	(0.017)	(0.010)	(0.018)	(0.010)	(0.019)
Asset maturity$_{t-1}$	0.001 c	0.004 a	0.001 b	0.004 a	0.001 a	0.004 a
	(0.0003)	(0.001)	(0.0003)	(0.001)	(0.0003)	(0.001)
Credit rating	-0.027 a	-0.035 a	-0.030 a	-0.036 a	-0.032 a	-0.044 a
dummy$_{t-1}$	(0.005)	(0.009)	(0.005)	(0.009)	(0.005)	(0.009)
Profitability$_t$	0.382 a	0.684 a	0.414 a	0.702 a	0.433 a	0.711 a
	(0.031)	(0.044)	(0.031)	(0.044)	(0.030)	(0.046)
Adjusted R^2	0.17	0.29	0.17	0.29	0.17	0.30
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
Observations	25,374	25,374	24,940	24,940	23,779	23,779

Table 7:
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 8:

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			-5.194a	-4.290a			
	(0.123)			(1.855)	(1.332)			
Non-Japanese lending		-0.756b					54.97	-51.14
		(0.291)					(117.9)	(821.9)
Real Estate Index			0.006a			-0.0005		
			(0.002)			(0.001)		
GDP_{t-1}	-3.918a	-3.057b		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		10.39b	8.490a		-65.63	60.00
	(0.435)	(0.469)		(4.118)	(2.989)		(139.5)	(972.8)
Japanese $share_{t-1}$	-0.433	4.235		288.1c	238.6c		-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