Credit Market Disruptions and Employment during the Great Depression: Evidence from Firm-level Data*

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Abstract

Financial market imperfections can have significant impact on employment decisions of firms. In this paper we show that the collapse of the bond market during the Great Depression and the suspension of many banks have led firms to curtail employment.Using a novel hand-collected dataset of U.S firms during the Great Depression we estimate that lack of availability of credit may have accounted for about 20% of the increase in unemployment between 1929 and 1933.

JEL classification:

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Introduction

Between 1929 and 1933 real output fell by 29 percent in the United States and millions lost their jobs. In 1929 only 3.2 percent – or 1.5 million persons – of the labor force were unemployed. By 1933, well over 10 million Americans were unemployed, and another 2.2 million had make-work jobs at low pay from state, local, and federal governments, resulting in a staggering unemployment rate of 25 percent.

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.[1] 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. In this paper we investigate the role of finance and credit in the propagation of unemployment in the U.S. between 1929 and 1933.

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.[2]

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. That is, 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.

[1] Notable exceptions are Chodorow-Reich (2014) and Duygan-Bump, Levkov, and Montoriol-Garriga (2015).

[2] 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.

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 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 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 firm level financial constraints and empirical measures of costly external finance even in a frictionless Neoclassical setting.

In this paper we analyze the relation between finance and labor using three empirical strategies that we developed to alleviate these identification concerns. We provide evidence 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 during the depression 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.

Second, we also exploit variation in the local availability of credit from banks. While the type of firms that rely on bank borrowing is potentially different from firms that financed their operation through the bond markets, there is still some degree of substitutability between bank lending and public bond markets. For example, Bo and Ivashina (2014) document evidence of substitution from bank loans to bonds during the 1990-2010 period. Our implicit assumption is that firms find it easier to borrow from banks in the same location, possibly to asymmetric information problems. Hence, we will construct a local measure of the (lack of) availability of bank credit by exploiting the number of bank failures that occurred in the same city where firm is located. In doing so, we restrict attention to the failure of *national*, as opposed to regional, banks. We do so for two reasons. First, national banks are arguably less sensitive to local economic conditions than regional banks. This partly alleviates our concern that the variation across cities in the number of failed banks simply reflects variation in local economic conditions which affect local firms via a demand channel.

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

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Finally, our preferred empirical specification exploits the interaction of these two treatment effects. Specifically, our main results compare firms that were located in cities with failed national banks that needed to refinance a portion of their debt during the crisis relative to firms also located in cities with failed banks but that did not need to refinance maturing bonds. This 'triple difference' specification helps alleviate some of the remaining concerns that would arise if we were to use only one of the above treatment definitions. Specifically, one might argue that variation in the total amount of bonds outstanding in 1931-33 could be correlated with unobservable firm-specific factors that affect employment. While, it is still possible that national banks were affected by local economic conditions; if these suspensions were simply driven by variation in local demand, we would not expect to find a differential effect across firms with different levels of pre-crisis maturing debt in 1931-33.

Taken together, our 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 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 on labor and financial constraints (see Michael, Page and Whited (2014), Pagano and Volpin (2008) and Pagano (2010)). Second, and perhaps more importantly we shed new light on an old question by showing that the lack

of financing was an important determinant of high unemployment rates during the Great Depression.

The rest of the paper is organized in the following manner. Section 1 discusses the identification strategy. section 2 presents the data and explains the construction of the variables used in the analysis. Section 3 explores the relation between employment and firm characteristics. Section 4 presents the analysis of the effect of 'maturing-debt' on firm employment. Section 5 focuses on the implications of local credit market conditions on employment. Section 6 concludes.

1 Identification strategy

Our goal is to identify an exogenous 'financial shock' to firms, that is, a plausibly exogenous shock in their ability to access external finance that is unrelated to their investment opportunities. To that end we exploit an important feature of the great depression in which public debt markets essentially shut down – making it difficult for firms to refinance their maturing bonds. This freezing of debt markets during the great depressions is well illustrated by Figure 1. As the figure shows, the total dollar volume of new bond issues by industrial firms fell by between 70% and 90% in the 1931-33 period relative to its 1928 level. In our empirical analysis we exploit two plausibly exogenous sources of cross-sectional variation in the degree of this market shutdown and demonstrate that the freezing of the market affected firms during the Great Depression.

In particular, we first use the 'maturing-debt' approach first introduced by Almeida, Campello, Laranjeira, and Weisbenner (2011), which exploits pre-existing heterogeneity in the maturity of long-term debt across firms. Specifically, the 'treatment' sample contains firms with pre-existing levels of long-term bonds that were maturing in the 1929-1933 period. The total amount of long-term bonds maturing during that period can be interpreted as a form of 'exogenous' short term debt. Since these maturing bonds were issued prior to the Great Depression, variation in its level is arguably exogenous to market conditions and investment opportunities during the great depression - when the debt eventually becomes due. More precisely, our 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, our identification strategy relies on exploiting debt that was issued a number of years before the year of interest.

Second, we also exploit variation in the local availability of credit from banks. While the type of firms that rely on bank borrowing is potentially different from firms that financed their

operation through the bond markets, there is still some degree of substitutability between bank lending and public bond markets. For example, Bo and Ivashina (2014) document evidence of substitution from bank loans to bonds during the 1990-2010 period. Our implicit assumption is that firms find it easier to borrow from banks in the same location, possibly to asymmetric information problems.

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Our preferred empirical specification exploits the interaction of these two treatment effects. Specifically, our main results compare firms that were located in cities with failed national banks that needed to refinance a portion of their debt during the crisis relative to firms also located in cities with failed banks but that did not need to refinance maturing bonds. This 'triple difference' specification helps alleviate some of the remaining concerns that would arise if we were to use only one of the above treatment definitions. Specifically, one might argue that variation in the total amount of bonds outstanding in 1931-33 could be correlated with unobservable firm-specific factors that affect employment. While, it is still possible that national banks were affected by local economic conditions; if these suspensions were simply driven by variation in local demand, we would not expect to find a differential effect across firms with different levels of pre-crisis maturing debt in 1931-33.

Our identification strategy hinges on the notion that the great depressions was not anticipated by firms with maturing long-term debt and hence firms were unlikely to refinance their maturing-bonds prior to the depression. The view that the Great Depression was an unexpected shock is supported by the data. For example, (Calomiris, 9943) documents that corporate bonds credit spreads were on the decline prior to the onset of the crisis in 1929.[4] Some scholars argue that the stock market crash in October 1929 did not come as a total surprise. For example, according to Atack and Passel (1994):

> *It was the stock market crash that brought the economic situation to the attention of most Americans, but this was not the first indication of impending trouble,*

[4]See (Calomiris, 9943) Figure 2 in page 69.

and nothing prepared anyone for what was to follow. Nor was the crash it self all that sudden. Signs of a recession appeared in the summer of 1929, when the Federal Reserve's index of industrial production turned down after growing 5 percent during the first half of the year. The great British economist John Maynard Keynes and others have attributed this slowdown to an abrupt change in Federal Reserve policy begun in January 1928.[5]

However, since the earliest signs of the impending crisis appeared only in summer 1929 - it did not leave firms much time to manage the maturity structure of their bonds. Furthermore, most economist tend to agree that the stock market crash and the severity of the Great Depressions that followed the crash were not anticipated or expected. As Atack and Passel (1994 write:

There is remarkably little unanimity among economists about the issues, explanations, or tests pf the theories concerning the Great Depression. Fr example, writing in 1976, MIT economist Peter Temin argued that monetary forces were not the cause of the depression, which he attributes to unanticipated and unexplained decline in consumption expenditure. John Maynard Keynes in the early 1930s attributed the crisis to the impact of changes in Federal Reserve monetary policy but later, in his influential book The General Theory of Interest, Money, and Employment in 1936, blamed the decline upon the loss of business confidence that undermined investment spending...Milton Friedman and his collaborator AnnaSchwartz, in A Monetary History of the United States, 1867-1960 also emphasize the role of Federal Reserve policy and the impact of specific monetary shocks to the financial system.[6]

While Temin seem to focus on the events of 1929 and 1930; Friedman and Schwartz put greater emphasis on the events from late 1930 onward. Whether the prolonged and severe depression resulted from loss of confidence, unanticipated decline in consumption of the Federal Reserve "doing too little too late" – it seems reasonable to assume that firms could not anticipate the market crash, the collapse of credit and bond markets and the severity of the depression.

[5]See Atack and Passel (1994) pp. 587-588.
[6]See Atack and Passel (1994) p. 592.

2 Data sources and variable definitions

This section describes the sources and methods used to collect the data. We construct a panel dataset of firm-level accounting information for 1928 and 1933 for all American industrial firms listed in the *Moody's Manuals of Investments*. We hand collect the data from the *Moody's Manuals of Investments* manuals of 1929 and 1934 – the information in each of the manuals represent accounting information for the previous year. We select these two specific years to contrast the change in employment from the peak in economics activity in 1928 to the trough of the 1929-1933 depression in 1933 when the unemployment rate was at its highest level.[7] For each firm, we hand collect information on the number of employees, firm size (measured by the book value of assets), leverage (defined as the ratio of short-term and long-term debt to the book value of assets), and profitability (measured by return on assets).[8] Each manual year contains about 5,000 firms, but only a fraction of them (39% in 1928 and 53% in 1933) actually reports employment figures.[9] We also use the firm's name, year of incorporation and, when necessary, description of activities, to match firms across years. We restrict the analysis to a balanced panel of 1,130 firms that report non-missing employment informtion in both years.

Given the nature of the Moody's Manuals, the firms in our sample are primarily in manufacturing and retail. The Great Depression did not affect all firms in these sectors equally. Thus, our empirical analysis controls for industry eeffects. In order to use industry classification that will be both meaningful and maintain a meaningful number of firms within each sector, we use the 30 industry classification of Fama and French.[10] We use two methods to objectively assign firms to industries. First, the *Moody's Manuals* of 1930 and 1934 classified large companies (defined as those with assets above $5 million and $3 million, respectively) by industries. We assign these industry definitions to the firms in our sample in 1928 and 1934, respectively, and then match these definitions to the Fama-French classification. For firms that are not covered by the *Moody's Manuals* industry classifications, we assign a Fama-French code based on the full description of the firm's activities reported in the manual. We exclude the 'Other' category, Utilities and Financials, since these firms are not covered by the manuals.Reassuringly, we found no instances in which assigning an

[7] Note that according to the NBER's Business Cycle Reference Dates, the peak of the cycle was in August 1929 and the trough in March 1933. We select the year 1928 as our reference point to compare employment rates before the outset of the crisis to their level during the trough of the cycle.

[8] We obtain information on the level of debt from the firms' balance sheets. These financial statements typically reported detailed data on long-term debt, usually identified as bonded or funded debt. To measure interest-bearing short-term liabilities, we collect information on notes, bills or loans payable.

[9] Larger and younger firms are more likely to disclose the number of employees.

[10] Our regression analysis excludes industries with less than five firms.

industry code based on the firm's activities would have altered the classification for those firms listed in the *Moody's Manuals* industry classifications.

Our main identification strategy utilizes pre-existing variation in the amount of long-term debt that became due during the crisis period. Starting in 193, the *Moody's Manuals* began to list the value of individual bonds maturing in future years that were issued by firms ilisted in the manual. From these lists of bond issues, we obtain the total value of bonds due for each of our sample firms from June 1931 to December 1933.[11]

Finally, we obtain information on national bank suspensions from the Federal Deposit Insurance Corporation (FDIC) Data on Banks in the United States, available from ICPSR. The FDIC data allow us to measure the suspension of national banks between 1929 and 1933 at the county level. To match our firm level data to the bank information, we collect the firm's primary address (city and state) from the *Moody's Manuals*. The address reported in the manuals identifies the main location in which the firm operates. We then match the firm's location to counties based on the definitions from the 1930 Population Census.[12] This procedure allows us to link the financial information of firms to the financial conditions of the banking system.

2.1 Summary statistics

Table 1 presents summary statistics of our variables of interests in our sample of 1,130 firms with non-missing observation for employment in both 1928 and 1933. To minimize the impact of data errors in our analysis, we winsorize all observations at the 2.5% and 97.5% level.[13] As the first row of the table demonstrates, the average firm experiences a 0.24 log points reduction in employment between 1928 and 1933, although there is substantial heterogeneity in outcomes across firms – the standard deviation of employment changes across firms is 0.58 log points. The total aggregate change in employment among the firms in our sample is -0.095 log points. This number is smaller than the average change in employment of -0.24, because it is essentially a value-weighted average – as we will see next, large firms reduced employment by a proportionally smaller amount than smaller firms.

[11] Specifically, we use the 1931 manual to identify bonds maturing between June 1931 and June 1932, the 1932 manual for those maturing between July 1932 and June 1933, and the 1933 manual for those maturing between July 1933 and December 1933. When the bond name does not match a company listed in the corresponding manual, we manually search for the parent company that has assumed the debt to correctly allocate bonds to firms.

[12] Our matching rate is very high; we are unable to identify the county for only less than four percent of the 9,530 firm-year observations with non-missing city information. In the few cases in which city spans multiple counties, we aggregate the bank data across the relevant counties.

[13] Using a winsorization threshold of 1% and 99% has no material impact on the analysis.

As Table 1 illustrates, average profitability – measured as Return on Assets (ROA) – dropped from 9% in 1928 to 1% in 1933. This substantial drop in profitability exceeds the cross-sectional standard deviation of profitability across firms in 1928 (7%). Put differently, the average drop in profitability between 1928 and 1933 was comparable to the cross-sectional differences across firms in either 1928 or 1933. Since firm profitability is a potentially important determinant of employment, we will control for firm's profitability in both 1928 and in 1933 in our empirical regression models.

The average book leverage in 1928 in our sample is 0.13, though there is substantial heterogeneity (cross-sectional standard deviation of 0.14). Further, there is a large number of firms in 1928 that report zero leverage. While we will include firms without debt in 1928 in our empirical analysis, we omit them from our analysis in a robustness check. Our results are unaffected whether we exclude zero-leverage firms from the analysis.

Table 1 also reports summary statistics for our main treatment variable, which is equal to the dollar amount of bonds due in 1931-33 as a fraction of the mean value of firm assets in 1928 and 1933. While the *Bonds due* variable equals zero for many firms in the sample – it is positive for 114 firms. Nevertheless, there is substantial variation in this ratio within the affected firms. Conditional on having a non-zero amount of bonds that become due during the years 1931-33, the average firm had to refinance debt that was equal to 5.2% of its assets, and the cross-sectional standard deviation around this number is 4%. Since fluctuations in this ratio are likely to be informative about the firm's financial condition, we will use the *Bonds due* variable as a continuous treatment. However, our results are also robust to using a discrete treatment – a dummy of whether the firm had any debt due during this period.

In the last two rows of Table 1 we report summary statistics for our second treatment variable, that is, the number of national banks with branches in the same state as the firm's location that were suspended. There are 324 firms located in cities in which no national bank was suspended, while 337 firms were located in counties/cities where 1-5 national banks were suspended, 238 firms were located in cities with 6-10 suspended national banks, and 115 were located in cities with more than 10 suspended national banks.

Since some of the variation in the number of suspended banks reflects differences in the number of banks in the region we also calculate the fraction of national banks in the region that were suspended and report summary statistics in the last row of Table 1. We calculate the fraction of national banks in the region that were suspended between the years 1929 and 1933 ,by dividing the number of suspended banks by the average number of national banks in the region. Our preferred empirical specification does not exploit the variation in the intensive margin, and simply compares firms in counties with suspended national

banks to counties without suspended national banks. Our motivation for doing so is that banks may choose to limit their lending to firms in anticipation of bank runs and hence the number of suspensions may not fully reflect the degree of credit curtailment in the region. Nevertheless, we perform a battery of robustness checks and show that our conclusions are robust to alternative definitions of a continuous treatment that is increasing in either the number and/or the size of the national banks that were suspended in the county.

2.2 Spatial variation in Employment during the Great Depression

Panel A of Figure 2, illustrates the spatial variation of employment drop in our sample by comparing the estimates of employment drop in our sample (on the vertical axis) to the employment drop in manufacturing firms reported by the Census of Manufacturers (horizontal axiss). Similarly, Panel B displays the (log) number of firms located in each region in our sample (vertical axis) and the Census of Manufacturing (horizontal axis). As Panel A shows, the regions that saw the largest employment decline in our sample were the East South Central states (Alabama, Kentucky, Tennessee and Mississippi); firms in these region collectively shed more than 25% of their work force. However, these states only accounted for a relatively small share of the firms in the U.S. As Panel B of Figure 2 demonstrates, the divisions with the largest number of firms were Middle Atlantic (New York, Philadelphia and New Jersey), and East North Central (Illinois, Ohio, Indiana, Michigan and Wisconsin). Collectively, firms in these regions shed approximately 10 and 15% of their workforce, respectively.

We compare our estimates of employment decline across U.S. regions to data from Rosenbloom and Sundstrom (1999) who use Census data on establishments covering 20 manufacturing industries. Panel A of Figure 2, plots our region-level estimates of employment decline versus the estimated drop in employment in manufacturing establishments located in the same region, using the figures reported in Rosenbloom and Sundstrom (1999). We note that the results are qualitatively similar; that is, the regions that suffered the most in terms of employment growth in our data are identical to those in Rosenbloom and Sundstrom (1999). The magnitudes are different however; a regression line has a slope coefficient of 0.46, implying that the overall employment drop in our sample is approximately one-half the overall employment drop in the subsample of manufacturing establishments in Rosenbloom and Sundstrom (1999). This difference is likely driven by two factors. First, typical firm in our data is likely larger than the average firm in the Census and hence may be more resilient to the business cycle. Second, the manufacturing sector is – in general – more cyclical than other industries that are included in our sample of firms.

3 Employment and Firm Characteristics

This section presents the results from the empirical analysis of the effects of the availability of credit, leverage and maturing debt on firm-level employment.

3.1 Employment and firm characteristics

We begin our empirical analysis by examining the correlation of employment growth at the firm level with a battery of firm characteristics. We do so by estimating different variants of the following regression:

$$\log(E_{i,1933}) - \log(E_{i,1928}) = \alpha + \beta_1 \, LEV_{i,1933} + \beta_2 \, ROA_{i,1933} + \lambda \, \mathbf{X}_{i,1928} + \gamma \, \mathbf{k}_i + \psi \, \mathbf{s}_i + \epsilon_{it}, \quad (1)$$

where the dependent variable is the log difference in the number of employees E between 1928 and 1933. We examine a vector of firm-specific variables $\mathbf{X}_i$ that includes the firm's debt to assets ratio (LEV) in 1933, the logarithm of employment in 1928, the logarithm of total assets in 1928, and the return on assets in 1928 and 1933. We are interested in isolating the correlation of these characteristics holding factors such as industry and the firm's location constant. Hence, we include either industry k, state s or industry times region fixed-effects, where we use the census definition of geographical regions in the U.S. Adding industry×regions fixed-effects enable us to control for heterogeneity in industry shocks across different regions. All regressions are estimated with heteroscedasticity-robust standard errors which are clustered by industry. The results are reported in Table 2.

As Table 2 shows, we find a negative and statistically significant correlation between employment change and the firm's leverage in 1933. The negative correlation between leverage and employment growth is robust to the specification we use. Furthermore, the results are similar if we replace the firm's leverage in 1933 with its leverage in 1928. The coefficients in columns (1) through (5) imply that a firm in the 90th percentile of leverage in 1933 experienced a decline that is 0.07 to 0.20 log points lower in employment growth from 1928 to 1933, relative to the a firm with a median leverage. These results are consistent with the idea that when burdened with debt, the ability of a firm to either grow or hoard its labor force during adverse conditions is constrained by its leverage. Our results are consistent with the findings in Sharpe (1994), Calomiris, Orphanides, and Sharpe (1994) and Benmelech, Bergman, and Seru (2011) who find a similar negative effect of leverage on employment using Compustat data.

We also find that firms with more employees relative to otherwise similar firms in the same industry and region experienced larger declines in their employment. For example,

Firms in the 90th percentile of employment in 1928 reduced employment by between 0.07 and 0.3 log points relative to the median firm – potentially suggesting that these firms employed more labor than they should. In contrast, larger firms based on their book value of assets in 1928 did not reduce their employment as much as smaller firms did. For example, firms in the 90th percentile of book assets in 1928 reduced their employment by between 0.21 and 0.28 log points *less* than the median firm in terms of assets. This fact is consistent with the idea that larger firms suffered less during the Great Depression

The last two columns of Table 2 show that the change in employment between 1933 and 1928 is strongly related to firm profitability. Firms that were highly profitable in 1928 reduced their employe force by relatively less, compared to otherwise similar firms. For example, firms in the 50th percentile of 1928 profitability reduced employment by 0.13 to 0.20 log points more relative to firms in the 90th percentile. As the last column of the table demonstrates, the effect of profitability also survives when we control for profitability in 1933.

While the results presented in Table 2 suggest that profitability and leverage potentially affect firm-level employment, these variables are endogenous and hence the results in the table do not imply a causal effect. We devise an empirical identification strategy and estimate the effect of financial constraints and credit market disruptions in the next section.

4 The Effects of Maturing Long-term Debt on Employment

We next perform sharper tests that exploit variation in pre-existing amounts of 'maturing bonds' as well as local financing conditions based on the firm's location. We conjecture that firms with greater refinancing needs will reduce their labor force by more than those firms not facing the need to refinance maturing long-term debt. Since external capital markets were essentially frozen during the Great Depression, firms could not borrow to pay their wage bill; hence, these adversely affected firms would have to adjust their real activity and reduce employment.

Our baseline regression specification of the 'maturing-debt' approach is as follows.

$$\log(E_{i,1933}) - \log(E_{i,1928}) = \alpha + \beta_1 \, BDUE_{i,1931-1933} + \beta_2 \, ROA_{i,1933} + \lambda \, \mathbf{X}_{i,1928} + \gamma \, \mathbf{k}_i + \psi \, \mathbf{s}_i + \epsilon_{it}, \quad (2)$$

where the dependent variable is the log difference in the number of employees E between 1928 and 1933. Our treatment variable $BDUE$ is equal to the total dollar amount of long-term bonds that were issued prior to 1928 and are due between 1931 and 1933, as a fraction of the

firm's average level of assets between 1928 and 1933.[14] We saturate our specification with a rich set of controls and fixed effects to assuage concerns about selection and omitted variables. Specifically, we include a vector of firm-specific controls $\mathbf{X}_{i,1928}$ which include lagged values of the the logarithm of employment in 1928, the logarithm of total assets in 1928, return on assets in 1928, and leverage in 1928. As in Regression 1 we also control for the the change in the return on assets between 1928 and 1933. In addition, we account for unobserved industry or regional time invariant heterogeneity by including either industry, state or industry×region fixed-effects. Adding industry×regions fixed-effects enable us to control for heterogeneity in industry shocks across different regions. All regressions are estimated with heteroscedasticity robust standard errors that are clustered by industry. The main coefficient of interest in this regression is β_1 which measures the sensitivity of employment to $BDUE$.

4.1 Comparison across treatment and control groups

Before we move to the regression results we compare firms across the treatment and control groups in terms of the change in employment between 1933 and 1928 as well as in their other explanatory variables. Table 4 compares firms with some bonds due in 1931-33 to firms that had some bonds outstanding, but none of which were maturing during that period. As the Table shows, firms with some bonds due had slightly smaller employment level in 1928 relative to firms without bonds due, though the difference is not statistically significant. In general, the two sets of firms are similar in terms of characteristics as of 1928, with the exception of leverage. Perhaps unsurprisingly, firms with more bonds due as a fraction of assets during that period also had more debt overall. We control for all these variables in our regressions analysis.

4.2 Regression Analysis

We report the results from estimating different variants of regression 2 in Table 5. As before, each column in the table displays the estimates from a separate regression. Column (1) does not include any of the control variables or the fixed-effects, while Column (2) includes state fixed-effects and Column (3) controls for both state and industry fixed-effects. Columns (4) to (9) add each of the remaining control variables in X sequentially, that is, the 1928 level of leverage (D/A), employment (E), book assets (A), profitability (ROA) and industry times region fixed effects.

[14] As a robustness check, we also examined scaling the fraction of debt outstanding by the firm's book assets in 1933. The results are quantitatively very similar.

As Column (1) of Table 5 demonstrates, we find a negative and statistically significant relation between $BDUE$ and the change in the number of firm employees. The negative effect of debt that becomes due during the Great Depression on employment is robust to the inclusion of state and industry fixed effects (Column 3), as well as when we control for the 1928 level of leverage (Column 4), log employment (Column 5), and log assets (Column 6). Column (7) includes state and industry×region fixed-effects to control for heterogeneity in industry shocks across different regions. We see that the negative correlation between $BDUE$ and employment is robust to the inclusion of the control variables and the state and industry×region fixed-effects. Last, in Columns (8) and (9) we include as additional controls the level of firm profitability (return on assets) in 1928 and 1933, respectively. Doing so reduces somewhat our sample, since not all firms reported income statements in 1928 and 1933. Even though firm profitability was a significant determinant of firm employment during that period, we still find a negative and statistically significant effect of $BDUE$ on log employment change.

The coefficient on $BDUE$, β_1 ranges between -1.2 and -1.9 and is statistically significant in most of the specifications reported in Table 5 – the exception is Columns (4) and (5), though the p values are only marginally higher than 10%. The magnitude of β_1 implies that a one standard deviation increase in $BDUE$ is associated with a decline in the number of employees that is between 2.6% and 3.9%, representing between 11% and 16% of the mean log change in the number of employees between 1928 and 1933. Comparing firms in the 90th percentile of $BDUE$ to the median firm (which has no bonds outstanding during that period), we see that the firms in the 90th percentile experienced a 1.4-2.1% larger drop in employment relative to the median firm.

In sum, our results show that firms that needed to refinance large amounts of previously issued debt, relative to their size, reduced their employment relative to their peers. This fact is similar to the effects found during the financial crisis of 2008-2009 by (Almeida et al., 2011; Benmelech et al., 2011). We next turn to exploit empirically a unique feature of the Great Depression that pertains to the collapse of the banking system and the suspension of many banks during the crisis.

5 The effects of disruptions in local credit conditions on firm-level employment

Next, we study the implications of disruptions in local credit conditions as an an additional source of variation in identifying the effects of financial constraints on firm-level employment.

As we have argued and as Figure 1 illustrates, public bond markets were essentially frozen during the Great Depression. However, firms could also secure lending from banks that were large enough to accommodate their financial needs.

Since it is likely that firms found it easier to borrow from banks in the same location, possibly to asymmetric information problems, we would expect to find a differential effect of maturing long-term debt – depending on the health of the local banking system – in particular, the number of banks that were suspended.

We compare the response of employment growth of firms located in cities that experienced a failure of a national bank relative to firms that were located in cities that experienced no bank failures. Specifically, we estimate a specification similar to equation (2) above:

$$\log(E_{i,1933}) - \log(E_{i,1928}) = \alpha + \beta_1 \, BANKFAIL_i + \beta_2 \, ROA_{i,1933} + \lambda \, \mathbf{X}_{i,1928} + \gamma \, \mathbf{k}_i + \psi \, \mathbf{s}_i + \epsilon_{it}. \tag{3}$$

As before, the dependent variable is the log difference in the number of employees between 1928 and 1933; $BANKFAIL_i$ is the treatment dummy; $ROA_{i,1933}$ is the firm's profitability (net income to assets) in 1933; and $\mathbf{X}_{i,1928}$ is a vector of firm specific control variables which include lagged values of the the logarithm of employment in 1928, the logarithm of total assets in 1928, return on assets in 1928, and leverage in 1928. As before, we account for unobserved industry or regional time invariant heterogeneity by including either industry k, state s or industry times region fixed-effects. All regressions are estimated with heteroscedasticity-robust standard errors which are clustered by industry. We are in particular interested in the coefficient $\beta_1$which captures the sensitivity of firm-level employment to local bank failures.

Table 6 compares firms in counties that had a branch of a national bank that was suspended to firms in counties where no national bank was suspended. As before, the two sets of firms are somewhat similar in terms of their 1928 number of employees or profitability. However, firms in cities with suspended national banks tended to be somewhat larger and also had slightly higher leverage (mean leverage of 13.5% versus 11.2%).

Table 7 presents the results of estimating (3). Examining columns (1) through (8), we find that there exists a negative and statistically significant cross-sectional relation between national bank suspensions and firm employment. Firms in cities that experienced bank failures experienced a 5.6% to 11.2% drop in employment relative to peer firms that were located in cities did not contain any branches of suspended national banks. This effect is statistically significant in Columns (3) to (8) in Table 7, that is, assuming we include state and industry fixed effects.

However, column (9) of Table 7 shows that, once we control for firm profitability in 1933, the point estimates are substantially smaller – the difference in employment growth

is now 3.9% – and not statistically significant from zero. That is, we cannot reject the hypothesis that both bank failures and the contraction in employment were the result of poor economic conditions which affected both firms and banks, or that bank distress affected firm employment through a demand channel as opposed to a reduction in the supply of credit.

5.1 The interaction between local credit conditions and firm financing needs

We now turn to exploit the variation in both firm-level maturing debt as well as the spatial variation in bank failures. We conjecture that firms that have their bonds maturing during the Great Depression will find it in particular difficult to refinance if the local banking system experienced disruptions. Since we exploit the interaction of both treatments: $BDUE$ and $BANKFAIL$ our main treatment focuses on firms that were both located in cities where at least one national bank failed ($BANKFAIL = 1$) and needed to refinance part of their debt during the crisis ($BDUE > 0$). As Table 8 shows, based on observables, the treated firms ($BANKFAIL = 1$ and $BDUE > 0$) are not statistically different from the control group ($BANKFAIL = 0$ and $BDUE > 0$).

Our regression model is essentially a 'triple-difference' specification, in which we compare employment between 1928 and 1933, across firms located in cities that experienced national bank suspensions versus not, interacted with whether firms had bonds maturing during the period. We estimate the following specification:

$$\begin{aligned}\log(E_{i,1933}) - \log(E_{i,1928}) =& \alpha + \beta_1 \, BANKFAIL_i + \beta_2 \, BDUE_i + \beta_3 \, BANKFAIL_i \times BDUE_i + \\ &+ \beta_4 \, ROA_{i,1933} + \lambda \, \mathbf{X}_{i,1928} + \gamma \, \mathbf{k}_i + \psi \, \mathbf{s}_i + \epsilon_{it}. \end{aligned} \quad (4)$$

The dependent variable is the log difference in the number of employees between 1928 and 1933; $BANKFAIL_i$ a dummy that takes the value one if the firm is located in a city with suspensions of national banks; $BDUE$ is the ratio of the dollar amount of bonds outstanding between 1931 and 1933 to the average value of assets between 1928 and 1933; $ROA_{i,1933}$ is the firm's profitability (net income to assets) in 1933; and $\mathbf{X}_{i,1928}$ is a vector of firm specific control variables which include lagged values of the the logarithm of employment in 1928, the logarithm of total assets in 1928, return on assets in 1928, and leverage in 1928. As before, we account for unobserved industry or regional time invariant heterogeneity by including either industry k, state s or industry times region fixed-effects. All regressions are estimated with heteroscedasticity-robust standard errors which are clustered by industry.

Our main focus is on the coefficient β_3 on the interaction term. Examining Table 9, we see that the estimated coefficients are negative, and statistically significant across specifications. The magnitude of the estimated coefficient β_3 ranges from -3.7 to -4.8 across specifications. To interpret these magnitudes, a firm that is located in a city with a suspended national bank, and that is in the 90th percentile in terms of maturing bonds outstanding (as a fraction of total assets) would experience a 4.1 to 5.3% greater drop in employment relative to an otherwise similar firm in the same city that had no bonds due during that period (the median firm).

5.2 Robustness Checks

In this section we perform a number of robustness checks to to our main specification 4. First, specification 4 interacts a continuous with a discrete treatment effect. As a robustness check, we also consider a specification with only discrete treatment effects. In particular, we define a dummy variable D_x that takes the value one if the dollar value of maturing debt exceeds x% of their average level of assets between 1928 and 1933. We consider values of x equal to 0, 5 and 10. Table 10 presents the results.

Examining the first row of Table 10, we see that, first, the economic magnitudes are substantial: the firms in cities with national bank suspensions that had debt due during the 1931-33 period experienced a 23-28% greater drop in employment relative to firms in the same city that had no bonds due in 1931-33. The magnitude of this effect is comparable to the mean drop in employment across all firms. Further, higher values of x lead to higher magnitudes, as we would expect: the firms that were located in cities with failed national banks and had to refinance more than 5% (or 10%) of their assets experienced a 30-40% (or 50%-55%) drop in employment relative to their peers. These magnitudes are quite substantial, but we should emphasize that we are focusing now on the extreme tail of the distribution of BDUE – the number of firms that had to refinance more than 5% or 10% of their assets is 40 and 20, respectively.

Second, we examine the robustness of our findings to a continuous, as opposed to a discrete definition of treatment for $BANKFAIL$. In particular, we might expect that the disruption in local market conditions would be more severe if more national banks failed, or if these national banks accounted for a larger share of deposits. We thus construct four continuous versions of $BANKFAIL$. In the first two specifications, the numerator of $BANKFAIL$ is the number of national banks that were suspended from 1929 to 1933 in the county. In the first specification we scale this number by the average number of banks in the county from 1928 to 1932; in the second specification we use the average number of national banks. In the

last two specifications, the numerator of $BANKFAIL$ is the total dollar amount of deposits in suspended national banks from 1929 to 1933 in the county. In the third specification, we scale this amount by the average level of deposits in all banks from 1928 to 1932 in that county. In the last specification, we use the average level of deposits in national banks from 1928 to 1932 in the county. Columns (1) through (4) of Table 11 report the results from re-estimating specification4 using each of these four definitions. To conserve space, we only report the results using the full set of controls. Examining this table we see that our main results – the coefficient β_3 remain statistically significant.

Third, our sample of firms contains firms that reported as having zero (or missing) leverage in 1928. It is possible that this sample of firms is quite special, and therefore should not be part of the control group. As a robustness check, we repeat our analysis by re-estimating equation (4), but now excluding all firms with zero or missing leverage in 1928. Table 12 reports the results. Excluding firms with zero leverage in 1928 results in a reduction of our sample from 1,010 to 698 firms, which has a moderate effect on the precision of our estimates. However, the point estimates of the coefficient β_3 on the interaction term is negative and statistically different from zero across all specifications. Our point estimates are somewhat smaller than before, with β_3 ranging from -3.2 to -4.1 (compared to -3.7 to -4.8). However, the difference is not statistically different from zero.

5.3 Aggregate Effects

Our results so far indicate that firms that had bonds due in 1931-33 that also happened to be located in cities that experienced suspensions of national banks sharply cut employment. However, these firms account for a only 7% of our sample. Hence, even if the effect is well-identified, it may be quantitatively small.

To quantify the contribution of the causal effect we uncover in equation (4) to the aggregate drop in employment within our sample of firms, we compute the portion of the change in the number of employees N_i of firm i between 1933 and 1928 that can be attributed to the causal effect – the interaction term $\beta_3\, BANKFAIL_i \times BDUE_i$, as

$$\Delta \hat{N}_{i,1933} = \left[\exp\left(\hat{\beta}_3\, BANKFAIL_i \times BDUE_i + \hat{c}\, Z_{it}\right) - \exp\left(\hat{c}\, Z_{it}\right)\right] N_{i,1928}, \qquad (5)$$

where the term $\hat{c}\, Z_{it}$ includes all the other variables in equation (4). All the coefficients, the main coefficient of interest $\hat{\beta}_3$ plus the coefficients on the remaining variables and fixed effects $\hat{c}$ are computed using the specification in column (9) of Table 9, which includes all

controls and fixed effects. The sum is computed over all the 786 firms that are included in this specification.

Then, we aggregate these estimates across all firms in the sample to obtain the component of aggregate employment growth that can be directly attributed to financial constraints as

$$\hat{G}_N = \frac{\sum_f \Delta \hat{N}_{i,1933}}{\sum_f N_{i,1928}}. \tag{6}$$

As a robustness check, we also compute (5)-(6) using the discrete treatment specification of Table 10, which replaces $BDUE$ in equation (4) with a dummy that takes the value one if the firm had any debt outstanding between 1931-33.

The total drop in employment among all the firms in our sample that are included in the specification of column (9) of Table 9 is equal to 9.0%. In terms of the aggregate causal effect 6, our coefficient estimates in column (9) of Table 9 imply an aggregate drop of 1.1% – or approximately 12% of the overall drop. If instead we use the discrete treatment specification of Table 10, our estimates imply an overall drop in employment of 1.8%, or approximately 19% of the total drop.

In addition, we perform the same calculation across different industries. In terms of the magnitude of the causal effect, the five most affected industries are: auto manufacturers (4.8-7.4% drop); manufacturers of aviation and railroad equipment (6.7-9.4% drop); manufacturers of steel products (2.3-3.1% drop); tobacco (3.1-3.7% drop); and business supplies (2-2.8% drop).

In terms of regions, our estimates imply that the Midwest and the Northeast were the most severely affected (drop of 1.6-2.3% and 1.0-1.8% respectively). In the western US the magnitude of the causal effects is smaller (0.4% drop), while in the South it was close to zero.

6 Conclusion

We analyze the effects of credit market disruptions, maturing debt, and suspensions of national banks during the Great Depression on firm-level employment from 1929 to 1933. By doing so we provide the first firm-level evidence that unemployment during the Great Depression was affected by the disruptions in the bond market and the difficulties of the banking system. This leads us to conclude that finance played an important role in the severity of the prolonged depression during the 1930s. The availability of credit can potentially amplify variation in employment levels over the business cycle as evident from the worse contracting of economic activity to date – namely, the Great Depression.

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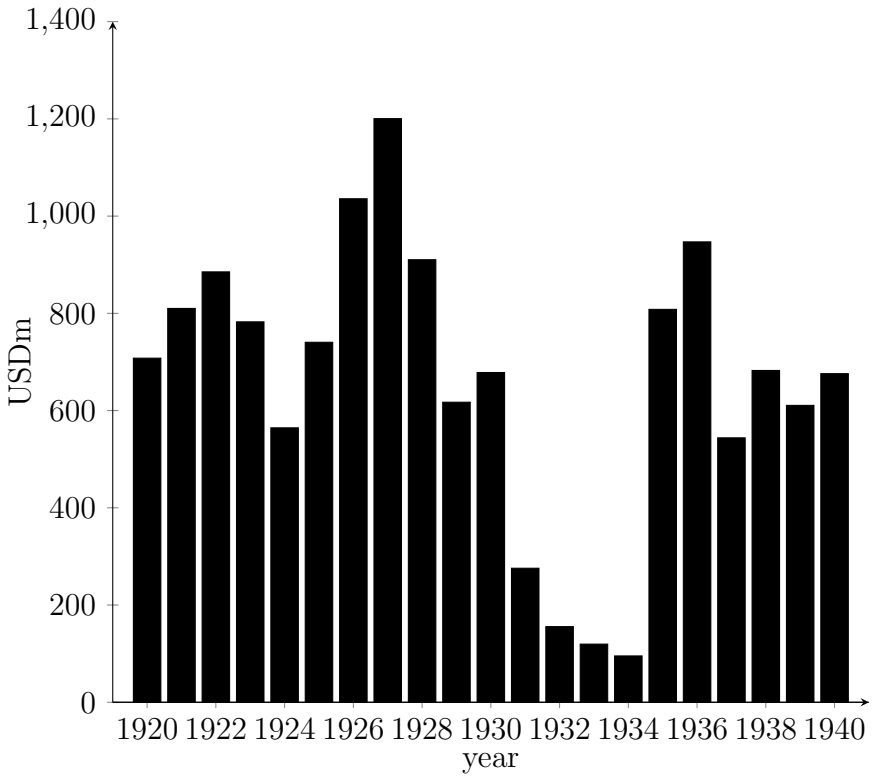
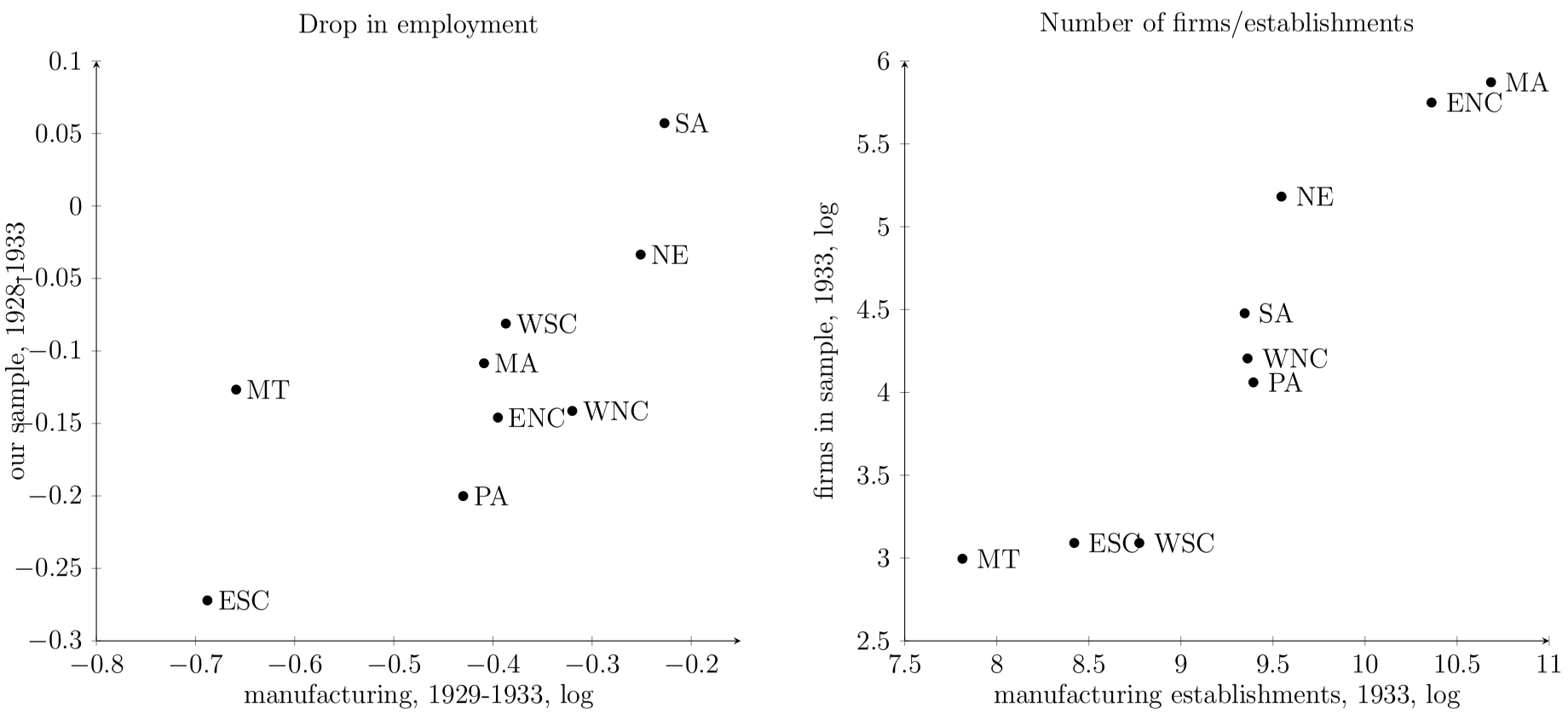
Tables and Figures

Figure 1: Par amount of new offerings, Industrials



Source, 'Statistical Measures of Corporate Bond Financing Since 1900', W. Braddock Hickman

Figure 2: Employment change across census regions: Our sample vs Census of Manufacturers



Employment drop in manufacturing is from Rosenbloom and Sundstrom (1999), Table 7, which covers 20 selected manufacturing industries. Number of manufacturing establishments are from Price Fishback (mfgest) @@@.

Table 1: Descriptive Statistics

	N	Mean	SD	p5	p10	p25	p50	p75	p90	p95
Employment, log, change (1928-1933)	1130	-0.24	0.58	-1.32	-0.97	-0.51	-0.18	0.11	0.44	0.64
Employment, log, 1928	1130	6.68	1.33	4.51	5.07	5.86	6.68	7.51	8.31	8.78
Employment, log, 1933	1130	6.44	1.46	4.09	4.62	5.52	6.48	7.31	8.18	8.82
Profitability: net income to book assets, 1928	851	0.09	0.07	-0.01	0.01	0.03	0.07	0.12	0.19	0.25
Profitability: net income to book assets, 1933	865	0.01	0.07	-0.12	-0.08	-0.03	0.01	0.05	0.09	0.12
Book Assets, log, 1928	1059	15.55	1.18	13.79	14.11	14.68	15.45	16.31	17.12	17.76
Book Assets, log, 1933	1040	15.34	1.26	13.46	13.78	14.43	15.24	16.06	17.01	17.74
Debt to Assets, 1928	1059	0.13	0.14	0.00	0.00	0.00	0.08	0.22	0.36	0.42
Debt to Assets, 1933	1040	0.12	0.15	0.00	0.00	0.00	0.05	0.19	0.34	0.45
Bonds due (1931-33) to book assets	1010	0.01	0.02	0.00	0.00	0.00	0.00	0.00	0.01	0.05
Number of suspended national banks (1929-33)	1013	5.82	9.11	0	0	0	2	8	20	34
Fraction of suspended national banks (1929-33)	1013	0.25	0.25	0.00	0.00	0.00	0.17	0.38	0.64	0.83

We only include observations with non-missing observations for employment in both 1928 and 1933. Amount of bonds due is reported as a fraction of the average level of assets between 1928 and 1933.

Table 2: Employment change and firm characteristics

$\Delta \log E_t$	(1)	(2)	(3)	(4)	(5)
D_t/A_t	-0.550***	-0.563***	-0.612***	-0.505***	-0.213**
	(0.138)	(0.131)	(0.128)	(0.142)	(0.095)
$\log E_{t-1}$		-0.044**	-0.140***	-0.185***	-0.158***
		(0.017)	(0.039)	(0.045)	(0.047)
$\log A_{t-1}$			0.124***	0.169***	0.127**
			(0.039)	(0.046)	(0.049)
ROA_{t-1}				1.694***	1.116**
				(0.435)	(0.442)
ROA_t					2.721***
					(0.423)
Observations	1039	1039	1009	823	787
R^2	0.232	0.239	0.259	0.328	0.402
Fixed Effects	S, IxR	S, IxR	S, IxR	S, IxR	S, IxR

Standard errors in parentheses: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table reports the coefficients from a regression of the change in log employment (number of employees) between 1928 to 1933 on the firm's leverage in 1933 D_t/A_t. We control for log employment in 1928 ($\log E_{t-1}$) , log book assets in 1928 ($\log A_{t-1}$), change in firm profitability between 1933 and 1928 (ΔROA_t), profitability in 1928 (ROA_{t-1}), and leverage in 1928 (D_{t-1}/A_{t-1}) . Depending on the specification, we include state (columns 2-7), industry (columns 3-5) or state-region (columns 6-8) fixed effects, where regions are classified according the US Census definition (4 regions) We classify firms into 30 industries following Fama and French 1997. All variables are winsorized at the 2.5% level.

Table 3: Employment change across industries

Industry	Firms	Employment Change (%)
Apparel	74	-2.7
Automobiles and Trucks	49	-21.1
Aviation and Railroad Equipment	29	-15.6
Business Equipment	26	-19.4
Business Supplies and Shipping Materials	69	-20.6
Chemicals	33	-1.8
Coal Mining	27	-38.2
Construction and Construction Materials	97	-53.3
Consumer Goods	57	-19.2
Electrical Equipment	22	-57.5
Fabricated Products and Machinery	114	-24.0
Food and Beverages	87	15.8
Healthcare, Medical Equipment, Pharmaceuticals	11	28.3
Personal and Business Services	16	-3.5
Petroleum and Natural Gas	36	-7.6
Mining	27	-75.4
Printing and Publishing	23	9.6
Recreation	13	13.6
Restaurants, Hotels, Motels	12	-13.2
Retail and Wholesale	89	12.7
Steel Works	60	-13.3
Textiles	93	6.9
Tobacco Products	17	-29.4
Transportation	41	5.8

Table reports the aggregate drop in employment across industries in our sample of firms.

Table 4: Comparison on observables between treatment and control 1: bonds due

Bonds due 1931-33	0		> 0		t test (p-value)	
	Mean	Median	Mean	Median	Mean	Median
Employment, log, 1928	6.693	6.685	6.681	6.551	0.93	0.33
Profitability, 1928	0.068	0.057	0.067	0.062	0.84	0.42
Book Assets, log, 1928	15.598	15.523	15.678	15.434	0.54	0.67
Debt to Assets,1928	0.177	0.146	0.243	0.227	0.00	0.00
Firms	698		108			

We only include observations with non-missing observations for employment and book assets in both 1928 and 1933, and firms with non-zero leverage in 1928.

Table 5: Treatment A: firm funding needs and fall in employment

$\Delta \log E_t$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
BDUE	-1.852*	-1.575*	-1.735**	-1.219	-1.287	-1.307*	-1.416*	-1.470*	-1.523*
	(0.984)	(0.824)	(0.818)	(0.800)	(0.812)	(0.748)	(0.840)	(0.856)	(0.863)
D_{t-1}/A_{t-1}				-0.411**	-0.421***	-0.475***	-0.469**	-0.285*	-0.173
				(0.153)	(0.149)	(0.166)	(0.183)	(0.164)	(0.163)
$\log E_{t-1}$					-0.037**	-0.143***	-0.144***	-0.186***	-0.159***
					(0.016)	(0.037)	(0.041)	(0.046)	(0.048)
$\log A_{t-1}$						0.135***	0.129***	0.173***	0.130**
						(0.036)	(0.040)	(0.047)	(0.049)
ROA_{t-1}								1.776***	1.081**
								(0.434)	(0.446)
ROA_t									2.790***
									(0.427)
Observations	1010	1010	1009	1008	1008	1008	1008	822	786
R^2	0.004	0.130	0.169	0.178	0.183	0.204	0.253	0.321	0.404
Fixed Effects	-	S	S, I	S, I	S, I	S, I	S, IxR	S, IxR	S, IxR

Standard errors in parentheses: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table reports the coefficients from a regression of the change in log employment (number of employees) between 1928 to 1933, on the total dollar amount of bonds that were issued @@@ and are due between 1931-1933, as a fraction of the average of book assets between 1928 and 1933 (BDUE). We control for log employment in 1928 ($\log E_{t-1}$) , log book assets in 1928 ($\log A_{t-1}$), firm profitability in 1933 (ΔROA_t), profitability in 1928 (ROA_{t-1}), and leverage in 1928 (D_{t-1}/A_{t-1}) . Depending on the specification, we include state (columns 2-7), industry (columns 3-5) or state-region (columns 6-8) fixed effects, where regions are classified according the US Census definition (4 regions) We classify firms into 30 industries following Fama and French 1997. All variables are winsorized at the 2.5% level.

Table 6: Comparison on observables between treatment and control 2: number of bank failures

BANKFAIL	0		> 0		t test (p-value)	
	Mean	Median	Mean	Median	Mean	Median
Employment, log, 1928	6.644	6.661	6.761	6.745	0.13	0.28
Profitability, 1928	0.083	0.066	0.089	0.076	0.38	0.12
Book Assets, log, 1928	15.452	15.354	15.623	15.571	0.02	0.04
Debt to Assets,1928	0.112	0.050	0.135	0.088	0.01	0.00
Firms	323		687			

We only include observations with non-missing observations for employment and book assets in both 1928 and 1933.

Table 7: Treatment B: Bank failures and fall in employment

$\Delta \log E_t$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
BANKFAIL	-0.056	-0.061	-0.089**	-0.086**	-0.086**	-0.085**	-0.104***	-0.112*	-0.039
	(0.048)	(0.036)	(0.038)	(0.040)	(0.040)	(0.040)	(0.037)	(0.059)	(0.052)
D_{t-1}/A_{t-1}				-0.441***	-0.452***	-0.506***	-0.503**	-0.299*	-0.199
				(0.157)	(0.153)	(0.168)	(0.184)	(0.161)	(0.162)
$\log E_{t-1}$					-0.036**	-0.142***	-0.141***	-0.183***	-0.158***
					(0.016)	(0.037)	(0.041)	(0.047)	(0.048)
$\log A_{t-1}$						0.135***	0.128***	0.170***	0.128**
						(0.036)	(0.040)	(0.048)	(0.049)
ROA_{t-1}								1.798***	1.098**
								(0.434)	(0.450)
ROA_t									2.790***
									(0.434)
Observations	1010	1010	1009	1008	1008	1008	1008	822	786
R^2	0.002	0.129	0.168	0.179	0.184	0.205	0.255	0.323	0.403
Fixed Effects	-	S	S, I	S, I	S, I	S, I	S, IxR	S, IxR	S, IxR

Standard errors in parentheses: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table reports the coefficients from a regression of the change in log employment (number of employees) between 1928 to 1933 on a dummy that takes the value one if any national banks were suspended in the city the firm is located during the 1929-1933 period. We control for log employment in 1928 ($\log E_{t-1}$) , log book assets in 1928 ($\log A_{t-1}$), change in firm profitability between 1933 and 1928 (ΔROA_t), profitability in 1928 (ROA_{t-1}), and leverage in 1928 (D_{t-1}/A_{t-1}). Depending on the specification, we include state (columns 2-7), industry (columns 3-5) or state-region (columns 6-8) fixed effects, where regions are classified according the US Census definition (4 regions) We classify firms into 30 industries following Fama and French 1997. All variables are winsorized at the 2.5% level.

Table 8: Comparison on observables between treatment and control 3: interaction

	$BANKFAIL = 0$				$BANKFAIL = 1$			
	BDUE=0		$BDUE > 0$		BDUE=0		$BDUE > 0$	
	Mean	p50	Mean	p50	Mean	p50	Mean	p50
Employment, log, 1928	6.699	6.691	6.693	6.586	6.813	6.860	6.675	6.533
	0.099	0.124	0.216	0.270	0.065	0.081	0.160	0.200
Profitability, 1928	0.061	0.049	0.073	0.059	0.071	0.061	0.064	0.062
	0.005	0.006	0.010	0.013	0.003	0.004	0.006	0.007
Book Assets, log, 1928	15.530	15.455	15.704	15.471	15.679	15.639	15.666	15.434
	0.084	0.106	0.231	0.290	0.059	0.073	0.146	0.183
Debt to Assets,1928	0.158	0.122	0.236	0.217	0.181	0.149	0.247	0.231
	0.010	0.012	0.024	0.030	0.007	0.009	0.012	0.015
Firms	178		34		414		74	

We only include observations with non-missing observations for employment and assets in both 1928 and 1933, and firms with non-zero leverage in 1928.

Table 9: Main results: firm funding needs × bank failures (discrete), and fall in employment

$\Delta \log E_t$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
BANKFAIL	-0.033	-0.038	-0.070*	-0.064	-0.062	-0.061	-0.078*	-0.084	-0.01
	(0.051)	(0.038)	(0.040)	(0.043)	(0.043)	(0.043)	(0.040)	(0.064)	(0.05
BDUE	0.591	0.828	0.440	1.088	1.164	1.162	1.190	1.171	1.40
	(1.258)	(1.001)	(1.138)	(1.065)	(1.044)	(0.905)	(1.025)	(1.129)	(1.12
N BANKFAIL X BDUE	-4.144***	-4.108***	-3.738***	-3.941***	-4.184***	-4.211***	-4.340***	-4.352***	-4.815
	(1.457)	(1.271)	(1.280)	(1.239)	(1.132)	(1.007)	(1.099)	(1.165)	(1.24
D_{t-1}/A_{t-1}				-0.415**	-0.425***	-0.480***	-0.476**	-0.282	-0.18
				(0.155)	(0.152)	(0.168)	(0.184)	(0.165)	(0.16
$\log E_{t-1}$					-0.039**	-0.144***	-0.145***	-0.187***	-0.162
					(0.016)	(0.038)	(0.041)	(0.047)	(0.04
$\log A_{t-1}$						0.135***	0.129***	0.171***	0.131
						(0.036)	(0.040)	(0.047)	(0.04
ROA_{t-1}								1.756***	1.057
								(0.429)	(0.44
ROA_t									2.777
									(0.42
Observations	1010	1010	1009	1008	1008	1008	1008	822	786
R^2	0.012	0.138	0.176	0.185	0.191	0.212	0.262	0.330	0.41
Fixed Effects	-	S	S, I	S, I	S, I	S, I	S, IxR	S, IxR	S, Ix

Standard errors in parentheses: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table reports the coefficients from a regression of the change in log employment (number of employees) between 1928 to 1933, on the total dollar amount of bonds that were issued @@@ and are due between 1931-1933, as a fraction of book assets in 1928 (BDUE) interacted with dummy that takes the value one if any national banks were suspended in the city the firm is located. We control for log employment in 1928 ($\log E_{t-1}$) , log book assets in 1928 ($\log A_{t-1}$), change in firm profitability between 1933 and 1928 (ΔROA_t), profitability in 1928 (ROA_{t-1}), and leverage in 1928 (D_{t-1}/A_{t-1}). Depending on the specification, we include state (columns 2-7), industry (columns 3-5) or state-region (columns 6-8) fixed effects, where regions are classified according the US Census definition (4 regions) We classify firms into 30 industries following Fama and French 1997. All variables are winsorized at the 2.5% level.

Table 10: Robustness 1: Discrete treatment: Firm funding needs (greater than x% of assets)

$\Delta \log E_t$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$NBFAIL \times D(BDUE \geq 0)$	-0.278***	-0.245**	-0.226**	-0.244**	-0.251**	-0.243**	-0.261***	-0.275***	-0.278*
	(0.099)	(0.093)	(0.099)	(0.096)	(0.093)	(0.089)	(0.090)	(0.091)	(0.108
R^2	0.009	0.135	0.174	0.183	0.188	0.209	0.259	0.327	0.407
$NBFAIL \times D(BDUE \geq 5\%)$	-0.376**	-0.366**	-0.295*	-0.327**	-0.352**	-0.374**	-0.423**	-0.389**	-0.398*
	(0.168)	(0.141)	(0.144)	(0.144)	(0.147)	(0.137)	(0.152)	(0.182)	(0.179
R^2	0.008	0.134	0.172	0.182	0.188	0.209	0.261	0.327	0.407
$NBFAIL \times D(BDUE \geq 10\%)$	-0.497*	-0.490**	-0.477**	-0.523**	-0.545**	-0.551***	-0.554**	-0.452**	-0.559*
	(0.250)	(0.219)	(0.220)	(0.213)	(0.199)	(0.176)	(0.204)	(0.187)	(0.250
R^2	0.011	0.136	0.175	0.185	0.190	0.211	0.261	0.328	0.409
Observations	1059	1059	1057	1055	1055	1055	1055	848	786
Controls									
Leverage, 1928				Y	Y	Y	Y	Y	Y
Employment, 1928					Y	Y	Y	Y	Y
Book Assets, 1928						Y	Y	Y	Y
Profitability, 1928								Y	Y
Profitability, 1933									Y
Fixed Effects	-	S	S, I	S, I	S, I	S, I	S, IxR	S, IxR	S, IxF

Standard errors in parentheses: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 11: Robustness 2: Continuous treatment for bank failures

$\Delta \log E_t$	(1)	(2)	(3)	(4)
BANKFAIL	0.342	0.052	-0.031	-0.051
	(0.381)	(0.130)	(0.316)	(0.152)
BDUE	0.407	1.109	-0.885	-0.854
	(1.050)	(1.034)	(1.005)	(1.026)
BANKFAIL X BDUE	-31.474**	-10.530***	-10.054**	-4.510**
	(12.655)	(1.984)	(4.503)	(1.735)
D_{t-1}/A_{t-1}	-0.167	-0.177	-0.165	-0.167
	(0.166)	(0.164)	(0.164)	(0.163)
$\log A_{t-1}$	0.129**	0.126**	0.127**	0.127**
	(0.049)	(0.048)	(0.050)	(0.050)
$\log E_{t-1}$	-0.160***	-0.158***	-0.158***	-0.158***
	(0.048)	(0.047)	(0.048)	(0.048)
ROA_t	2.812***	2.817***	2.808***	2.803***
	(0.422)	(0.423)	(0.435)	(0.434)
ROA_{t-1}	1.065**	1.037**	1.089**	1.097**
	(0.452)	(0.455)	(0.444)	(0.436)
Observations	787	787	787	787
R^2	0.407	0.411	0.406	0.407
Fixed Effects	S, IxR	S, IxR	S, IxR	S, IxR

Standard errors in parentheses: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table repeats the analysis in Column (9) of Table 9 using a continuous treatment of variation in local credit conditions, $BANKFAIL$, which is constructed as the following ratio. In columns (1) and (2), the numerator in $BANKFAIL$ is the number of national banks that were suspended from 1929-33. In column (1) we scale by the average number of banks in the region from 1928-32. In column (2) we scale by the average number of national banks in the region from 1928-32. In columns (3)-(4), the numerator of $BANKFAIL$ is the total dollar amount of deposits in suspended national banks from 1929 to 1933 in the county. In column (3), we scale this amount by the average level of deposits in all banks from 1928 to 1932 in that county. In column (3), we use the average level of deposits in national banks from 1928 to 1932 in the county. See notes to Table 5 for the definition of BDUE.

Table 12: Robustness 3: exclude firms with zero or missing leverage in 1928

$\Delta \log E_t$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
BANKFAIL	-0.039	-0.026	-0.019	-0.018	-0.017	-0.013	-0.016	-0.006	0.049
	(0.064)	(0.048)	(0.064)	(0.065)	(0.064)	(0.061)	(0.074)	(0.115)	(0.097)
BDUE	0.596	1.042	0.772	1.031	1.060	1.094	1.190	0.789	1.305
	(1.153)	(0.913)	(1.015)	(0.981)	(0.948)	(0.881)	(1.009)	(1.080)	(1.184)
BANKFAIL X BDUE	-3.533**	-3.413***	-3.194**	-3.328**	-3.481***	-3.570***	-3.719***	-3.514**	-4.094**
	(1.363)	(1.132)	(1.285)	(1.247)	(1.124)	(1.015)	(1.139)	(1.431)	(1.549)
D_{t-1}/A_{t-1}				-0.263	-0.285	-0.338*	-0.322	-0.152	-0.084
				(0.170)	(0.171)	(0.186)	(0.214)	(0.206)	(0.218)
$\log E_{t-1}$					-0.025	-0.132***	-0.119***	-0.149***	-0.134**
					(0.023)	(0.040)	(0.042)	(0.047)	(0.052)
$\log A_{t-1}$						0.137***	0.119***	0.147***	0.119**
						(0.038)	(0.039)	(0.047)	(0.050)
ROA_{t-1}								2.587***	1.581**
								(0.580)	(0.633)
ROA_t									2.533***
									(0.480)
Observations	698	698	697	697	697	697	697	576	550
R^2	0.014	0.177	0.222	0.225	0.227	0.248	0.302	0.369	0.447
Fixed Effects	-	S	S, I	S, I	S, I	S, I	S, IxR	S, IxR	S, IxR

Standard errors in parentheses: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table reports the coefficients from a regression of the change in log employment (number of employees) between 1928 to 1933, on the total dollar amount of bonds that were issued @@@ and are due between 1931-1933, as a fraction of book assets in 1928 (BDUE) interacted with dummy that takes the value one if any national banks were suspended in the city the firm is located. We control for log employment in 1928 ($\log E_{t-1}$) , log book assets in 1928 ($\log A_{t-1}$), change in firm profitability between 1933 and 1928 (ΔROA_t), profitability in 1928 (ROA_{t-1}), and leverage in 1928 (D_{t-1}/A_{t-1}). Depending on the specification, we include state (columns 2-7), industry (columns 3-5) or state-region (columns 6-8) fixed effects, where regions are classified according the US Census definition (4 regions) We classify firms into 30 industries following Fama and French 1997. All variables are winsorized at the 2.5% level.