Financial Frictions and Employment during the Great Depression*

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Abstract

We explore the role of financing frictions in firms' employment decisions during the Great Depression using a novel, hand-collected dataset of large industrial firms. We show that, contrary to the consensus view, large firms did not emerge from the Depression unscathed. Average profitability among the firms in our sample dropped from approximately 9% in 1928 to 1% in 1933, while the average drop in employment was 24%. Using a 'triple difference' identification strategy that exploits the interaction of two treatment effects – the need to refinance maturing long-term debt and local bank suspensions – we estimate a large negative causal effect of financing frictions on employment. When interpreting the estimated elasticities using a simple structural model, our analysis suggests that the lack of availability of credit may have accounted for between 10% and 30% of the fall in employment between 1929 and 1933. We conclude that financial frictions played a significant role in the unprecedented loss of jobs during the Great Depression.

JEL classification:

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Introduction

The Great Depression was the longest, deepest, and most widespread recession of the 20th century. In the United States, real output fell by 29 percent between 1929 and 1933, while 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. The issues related to the Depression's high unemployment remain important today – more than ninety years after the world entered its worse economic crisis.

For example, economist and former Fed chairman Ben Bernanke writes:

> To understand the Great Depression is the Holy Grail of macroeconomics. Not only did the Depression give birth to macroeconomics as a distinct field of study, but also—to an extent that is not fully appreciated—the experience of the 1930's continues to influence macroeconomists' beliefs, policy recommendations and research agendas. (Bernanke, 2000, p. 5.)

However, the reasons behind the staggering rise in unemployment during the Great Depression are still poorly understood. One view among economists – one that has been championed by Ben Bernanke – is that financial factors played an important role in the crisis (Bernanke, 1983). Indeed, financial imperfections offer an attractive explanation for the rise in unemployment: when there is a mismatch between payments to labor and the realization of output, firms will need to finance their labor activity throughout the production process (see, e.g., Greenwald and Stiglitz, 1988). Further, unlike capital, labor cannot serve as collateral; hence, in some respects labor is harder to finance. However, while the role of finance in the Great Depression might have been an important one, the lack of widely available firm-level data for this period poses an obstacle for deep empirical investigation of the effect of finance on employment during the Great Depression. In this paper we aim to fill this void.

We construct a novel dataset of firm-level employment and financial variables for a relatively large sample of industrial firms during the 1928-33 period and contribute to this debate along two dimensions. First and foremost, we provide a causal estimate of the importance of financial frictions for firm employment decisions. Our preferred specification isolates the impact of financial frictions by focusing on firms that needed to refinance their long-term debt during the crisis years and that could not have substituted from public debt to bank financing since they were located in cities in which the local banks were in distress.[1]

[1] Becker and Ivashina (2014) document evidence of substitution between bank loans and bonds during the 1990-2010 period.

Depending on the specification, our analysis of the estimated elasticities concludes that financial frictions accounted for between 10% to 30% of the overall drop in employment among the large firms in our sample.

Second, our data reveals new stylized facts about the behavior of employment in large public firms during the Great Depression and their operating performance. There are approximately 1,000 industrial firms in our sample with publicly listed securities that report their employment in 1928 and 1933. Since these are firms that had access to financial markets, they tend to be substantially larger than the average firm in the economy.[2] Contrary to the existing view that large firms emerged relatively unscathed from the Great Depression, we find that the average (median) drop in employment in our sample is 24% (18%), while the average level of profitability, as measured by return on assets, fell from 9% to 1%. In terms of correlations, we find that the firms that shed the most workers were the ones that had relatively more employees relative to their book assets, were younger, less profitable and had higher leverage.

Our identification strategy exploits the interaction of two treatment effects. First, we follow Almeida, Campello, Laranjeira, and Weisbenner (2011) and exploit heterogeneity in the maturity of long-term debt across firms. Specifically, the treatment group consists of firms that had bonds maturing during the 1930-34 period – right at the time of the collapse of the public bond market (Hickman, 1960). The control group is comprised of firms that did not need to refinance their maturing debt but were otherwise similar in terms of observables, including their financial leverage. We find that firms that had a greater need to refinance their debt – as a fraction of their assets – cut employment more than firms with no refinancing needs.

We conduct two robustness checks to alleviate concerns that the treatment group differs from the control group along unobservable characteristics. First, restricting the definition of maturing debt to only include bonds that were issued before the crisis yields similar and even somewhat stronger magnitudes. Second, a placebo test that uses the amount of bonds maturing *before the crisis* instead of bonds maturing *during the crisis* has no effect on firm's employment.

The second treatment exploits variation in the local supply of credit, measured by bank suspensions during this period. Our implicit assumption is that firms find it easier to borrow from banks in the same location, possibly to asymmetric information problems (Agarwal and Hauswald (2010), Petersen and Rajan (2002)). Hence, we construct a local measure of the (lack of) availability of bank credit by exploiting the number of bank failures that occurred

[2]The average firm in our sample employs approximately 2,000 employees; by comparison, the average establishment in the Census of Manufacturers of 1929 employees approximately 50 workers.

in the same city where firm is located.[3] We restrict attention to the suspension of *national*, as opposed to *state* banks. National banks were somewhat less sensitive to local economic conditions than state banks were, which partially alleviates concerns that the number of failed banks simply reflects local economic conditions. We find that firms located in cities with failed banks experienced a larger drop in employment relative to the firms located in cities that experienced no bank failures. However, we find that when we include controls for firm profitability the bank failures effect becomes smaller and statistically insignificant.

Our preferred empirical specification exploits the interaction of these two effects. That is, we exploit the fact that bank lending and public bond markets are to some degree substitutes. Firms that needed to refinance their maturing debt would find it much harder if they were also located in a city that experienced bank suspensions.[4] This 'triple difference' specification helps alleviate some of the remaining concerns that would arise if we were to use only one of the above empirical strategies. For instance, it is still possible that national banks were adversely 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 1930-34.

Our results suggest that the impact of financial frictions on employment during the Great Depression was quantitatively large. Specifically, when we compare firms in the 90th percentile in terms of the ratio of maturing debt to assets to the median firm – which has no bonds maturing during that period – we find that the firms in the 90th percentile experienced a 4.4-5.6% larger drop in employment relative to the median firm. The differences between these two sets of firms is much greater when we focus on cities with suspended national banks. Firms located in cities with a suspended national bank, and that were in the 90th percentile of the maturing debt measure experienced a 8.8 to 11.1% greater drop in employment relative to an otherwise similar firm in the same city that had no bonds due during the crisis.

Our analysis provides an estimate of the elasticity of firm employment to a plausibly exogenous financing shock. Interpreting what the magnitude of this estimated elasticity implies for the aggregate drop in employment during this period requires additional assumptions. We proceed in two ways. First, we use the estimated elasticities to compute a counterfactual

[3] In this respect, our study is related to contemporaneous work by Lee and Mezzanotti (2015), who study the behavior of employment in manufacturing establishments at the city-industry level in response to local bank failures. Our paper differs from theirs in that we use firm-level, instead of industry-level, data which enables us to measure financing needs at the firm level and control for individual firm characteristics – such as firm profitability.

[4] It is possible that firms that rely on bank borrowing are potentially different from firms that financed their operation through the bond markets. However, our treatment groups consists of firms that had outstanding public debt. Such firms should have had no difficulty borrowing from (national) banks. Rauh and Sufi (2010) and Becker and Ivashina (2014) provide evidence of substitution between bank loans and bonds in recent years.

level of aggregate employment within the firms in our sample under the assumption that the estimated treatment effect is equal to zero. We find that employment would have been 0.9% to 1.8% higher if no firms had debt due during the crisis, which accounts for between 10% and 20% of the overall drop in employment in our sample. We refer to this estimate as the direct treatment effect, which we view as a conservative estimate given the narrow definition of the treatment. After all, firms without any maturing debt that needed to access external funds were also likely to be adversely affected by the collapse of the financial sector in the Great Depression.

In order to infer the impact of financial frictions on the firms that had no debt due during the crisis, we estimate a structural model. We use the estimated elasticity of employment to maturing debt to calculate the parameter driving the cost of external finance in the model. We calibrate the model so that it matches the historical data along several dimensions and delivers similar elasticities of employment to maturing debt in simulated data. Using our calibrated model, we compute counterfactual levels of aggregate employment by setting the estimated costs of external finance to zero. Depending on the assumptions, our estimates imply that aggregate employment would have been 2.0% to 2.9% higher within our sample of firms in the absence of external financing frictions, which accounts for between 22% and 33% of the overall drop in employment.

Our work contributes to the long discussion on the role of the financial sector during the Great Depression. Friedman and Schwartz (1963) argue that banks likely contributed to the overall decline in economic activity due to a reduction in the money supply. Building on this view, Bernanke (1983) further proposes that the overall distress in the financial sector led to disruptions in the supply of credit, especially to households and small firms. Bernanke (1983) emphasizes that small firms suffered much more than large firms during the Great Depression, since large firms entered the crisis with sufficient liquid reserves to finance operations (Lutz, 1945).[5] Bernanke (1983) concludes that the financial crisis led to a decline in aggregate demand: "unless it is believed that the outputs of large and of small businesses are not potentially substitutes, the aggregate supply effect must be regarded as not of great quantitative importance". Our finding that large corporations not only experienced drops

[5]Based on a sample of 45 large manufacturing firms, Lutz (1945) documents that their cash balances in the early 1930s did not differ much from their pre depression level. This pattern is in contrast to the cash balances of small and medium firms which show a marked decline. However, in addition to cash, Lutz (1945) documents that large corporations also held marketable securities; unlike cash, their holdings of securities declined markedly in the 1929-37 period. Lutz (1945) writes that "large manufacturing corporations preferred to draw on their marketable securities instead of cash balances to finance part of their payments". In his view, the decision to hold cash and sell securities was driven by the fall of interest rates. Another possible reason is that corporations that needed cash to finance their cost of operations in the face of declining profits may have decide to liquidate their less liquid assets first.

in employment, but also a substantial fraction of this drop can be attributed to financing frictions casts doubt on this view.

In addition, our work is also related to the growing literature studying the impact of financial constraints on firm-level employment.[6] In terms of focus, our paper is closest to Almeida et al. (2011), Benmelech et al. (2011), and Chodorow-Reich (2014) who study the role of financial frictions in increasing unemployment during the recent 2008-09 financial crisis. We apply some of the same methodology in these papers to a different setting. Comparisons between the 2008-09 economic recession and the Great Depression are common, but the latter was far worse. At its peak, unemployment during the Great Depression was at 25% versus 10% in the 2008-09 crisis, while the cumulative fall in output was over 29% vs 3.3%. The accumulating evidence for both the Great Depression and the Great Recession illustrate the importance of the financial system and credit markets for the level of employment. Our paper is also related to the literature that examines the importance of financial frictions through the lens of structural models (see, e.g., Hennessy and Whited, 2007).

The rest of the paper is organized as follows. 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 evaluates the implications of our estimates for the aggregate drop in employment in our sample. Section 7 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 Depression 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 1931-34 period relative to its 1928 level. In our

[6] An incomplete list includes Almeida et al. (2011); Benmelech, Bergman, and Seru (2011); Chodorow-Reich (2014); Duygan-Bump, Levkov, and Montoriol-Garriga (2015); Michaels, Page, and Whited (2014); Pagano and Pica (2012).

empirical analysis we exploit two plausibly exogenous sources of cross-sectional variation in the degree that this freezing of the bond market affected firms during the Great Depression.[7]

In particular, we use the 'maturing-debt' approach first introduced by Almeida et al. (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 1930-1934 period.[8]

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 crisis - 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, we also show that our results are robust to restricting attention to bonds that were issued prior to January 1929, which is well before the onset of the Great Depression.

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, Becker 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 due to asymmetric information problems (Petersen and Rajan, 2002).

Hence, we 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 state, banks. We do so for two reasons. First, being more diversified, national banks are arguably less sensitive to local economic conditions than statel 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. Second, and more importantly, national banks tended to be larger than state banks, hence they are more likely potential

[7] In addition to the bond markets, the equity markets also experienced a substantial collapse as new equity issuance essentially 'dried up' (see, for example, Benmelech and Bergman, 2016). However, this freezing of the equity market is less relevant for our analysis, since less than 20% of the firms in our sample were publicly listed in the NYSE.

[8] Our main outcome variable is the change in employment between 1928 and 1933. Our treatment variable includes the bonds that also matured in 1934 since firms could act in a precautionary way and reduce employment in 1933 in anticipation of having difficulty funding their debt due next year. However, in a series of robustness checks, we show that including 1934 is not the main driver of our results.

lenders for the large firms win our sample. We define treated firms as those located in a city in which at least one national bank failed in the 1929-1933 period. We also construct continuous treatments in which we exploit variation in the fraction of failed national banks.

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 to 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 1930-34 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 1930-34.

Our identification strategy hinges on the notion that the Great Depression was not anticipated by firms with maturing long-term debt and hence firms were unlikely to strategically refinance their maturing-bonds prior to the recession. The view that the Great Depression was an unexpected shock is supported by the data. For example, Calomiris (1994) documents that corporate bonds credit spreads were on the decline prior to the onset of the crisis in 1929.[9] Some scholars argue that the stock market crash in October 1929 did not come as a total surprise. For example, according to Atack and Passell (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, 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 (Atack and Passell, 1994, pp. 587-588.)

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 Passell (1994) write:

[9] See Calomiris (1994) Figure 2 in page 69.

> There is remarkably little unanimity among economists about the issues, explanations, or tests of the theories concerning the Great Depression. For 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 Anna Schwartz, 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 (Atack and Passell, 1994, pp. 592.)

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, or the Federal Reserve "doing too little too late" – it seems reasonable to assume that firms could not accurately anticipate the timing of the market crash, the collapse of credit and bond markets, and the severity of the depression that ensued.

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 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 economic activity in 1928 to the trough of the 1929-1933 depression in 1933, when the unemployment rate was at its highest level.[10] For each firm, we 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).[11] Each manual

[10] 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.

[11] 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.

year contains about 5,000 firms, but only a fraction of them (39% in 1928 and 53% in 1933) actually reports employment figures.[12] 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 information in both years.

Given the nature of the *Moody's Manuals of Investments*, 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 effects. In order to use industry classification that will be both meaningful and maintain a sizable number of firms within each sector, we use the 30 industry classification of Fama and French (1997).[13] 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 1933, respectively, and then match these definitions to the Fama-French 30 industry 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 Fama-French 'Other', 'Utilities' and 'Financials' categories, since they are not covered by the manuals. Reassuringly, we found no instances in which assigning an 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 1931, the *Moody's Manuals* began to list detailed information on individual bonds maturing in future years that were issued by firms listed 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 through December 1934.[14] We supplement the information on maturing bonds for the year 1930 and for the first five months of 1931 using detailed descriptions of all bonds outstanding for each firm from the 1930 *Moody's Manuals of Investments*.

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

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

[13] In our empirical analysis, we exclude industries with less than five firms.

[14] 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.

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.[15] 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 interest 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% and 98% level.[16]

Our sample consists of firms that had access to the public bond or equity markets, hence it is composed of mostly large firms. Specifically, the median firm employed approximately 800 employees in 1928. The size distribution is fairly skewed, with the average firm having approximately 1,968 employees. Nevertheless, even though there is considerable dispersion, approximately 95% of the firms in our sample employed more than 100 employees. As a point of comparison, the average establishment in the Census of Manufacturers in 1929 had approximately 49 employees. Along these lines, most of the firms in our sample are fairly old. The average age of the firms in our sample is approximately 18 years as of 1928, and 75% of the firms were incorporated prior to 1923.

The consensus view on the Great Depression is that large firms suffered disproportionately less than smaller firms from the crisis (Bernanke, 1983). However, this does not imply that large firms emerged unscathed. As Table 1 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.

In addition, 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

[15] 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 a city spans multiple counties, we aggregate the bank data across the relevant counties.

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

drop in profitability between 1928 and 1933 was comparable to the cross-sectional differences across firms in either 1928 or 1933. Further, approximately 41% of the firms in our sample experienced operating losses in 1933, compared to less than 7% in 1928. 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). This number may seem small compared to modern firms, however it is consistent with the evidence reported in Graham, Leary, and Roberts (2015) for our time period. 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 or not 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 1930-34 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 140 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 1930-34, the average firm had to refinance debt that was equal to 9.0% of its assets, and the cross-sectional standard deviation around this number is 11%. 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 main 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 three 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 city 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. In addition, we also compute the fraction of deposits that were in suspended national banks during the same period. Again, we see considerable dispersion in the effect of the treatment on our firms. The average firm was located in a region were 22% of the national banks were suspended in

the 1929-33 region, with a cross-sectional dispersion of 22%. Smaller banks were more likely to fail, hence when weighting by deposits, the corresponding mean and dispersion estimates are 16% and 44%, respectively.

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, the typical firm in our data is much larger than the average firm in the Census and hence may be more resilient to the financial crisis. Second, our sample includes industrial firms, whereas the Rosenbloom and Sundstrom (1999) focuses on manufacturing; the manufacturing sector is *in general* more procyclical than the other industries that are included in our sample.

Table A.1 presents the employment decline across industries. We see that manufacturing industries – and especially those producing durable goods – are among the most affected by the Great Depression. For instance, firms in Electrical Equipment, Mining and Construction collectively shed more than 50% of their employees. By contrast, firms in services and agriculture were considerably less affected. These estimates are consistent with the documented large decline in consumer durables during the early part of the crisis (Romer, 1993). To ensure that our effects are not driven by cross-sectional differences across industries, we include industry effects in all our specifications.

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 between employment growth at the firm level and 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 et al. (2011) who find a similar negative association of leverage with employment using Compustat data.

Table 2 also illustrates 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 labor 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 affected firm-level employment during the Great Depression, 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 Maturing Long-term Debt and Employment

We next conduct sharper empirical 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.

$$\log(E_{i,1933}) - \log(E_{i,1928}) = \alpha + \beta_1 \, BDUE_{i,1930-1934} + \beta_2 \, ROA_{i,1933} + \lambda \, \mathbf{X}_{i,1928} + \gamma \, \mathbf{k}_i + \psi \, \mathbf{s}_i + \epsilon_{it}, \quad (2)$$

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 are due between 1930 and 1934, as a fraction of the firm's average level of assets between 1928 and 1933.[17] 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 return on assets between in 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 effects interacted with region 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 3 compares firms with some bonds due in 1930-34 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

[17] As a robustness check, we also examined scaling the fraction of debt outstanding by the firm's book assets in either 1928 or 1933. The results are quantitatively very similar.

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 4. 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 (10) 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), firm age, and industry times region fixed effects.

As Column (1) of Table 4 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 find 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. 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 their 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. Last, controlling for firm age in Column (10) has no material impact on our results.

The estimated coefficient on $BDUE$, β_1, ranges between -1.1 and -1.5 and is statistically significant in all of the specifications reported in Table 4. 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 3.9% and 5.3%, representing between 16% to 22% 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 find that the firms in the 90th percentile experienced a 4.4-5.6% larger drop in employment relative to the median firm.

We perform a number of robustness checks to our analysis. First, in calculating $BDUE$ we exclude bonds that were issued after December 1928. This cutoff is well before the earliest signs of the Great Depression and ensures that our treatment variable does not capture an endogenous response to the crisis. As Table 5 shows, we obtain economically similar results when we do so. In fact, β_1 is now even higher and ranges between -1.3 and -1.7. Second, we consider a placebo experiment where we replace the amount of debt maturing in 1930-34 with the amount of debt maturing in 1928. This robustness check ensures that our treatment variable is not picking up firms that typically issue short-term debt. However, as Table 6 demonstrates, this is unlikely to be the case. The coefficient on $BDUE$ in the placebo experiment is now positive but not statistically different from zero. Last, we perform a number of additional robustness checks; to conserve space, some of these results are reported in the Online Appendix. In our treatment definition we include bonds maturing in 1934, since we expect that firms could reduce employment in anticipation of these cash outflows. Table A.2 shows that our results are not driven by the inclusion of the bonds maturing in 1934 in our treatment definition. Next, 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. Table A.3 shows that we obtain similar results when we exclude firms with zero leverage in 1928 from the sample. Finally, while we include leverage in all of our regressions, given that the treated firms have higher leverage compared to non-treated firms (Table 3) we also include non-parametric leverage controls and present the results in Table A.4. As the table shows, the results remain unchanged.

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) and 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 Local Credit Conditions and 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 (Agarwal and Hauswald (2010), Petersen and Rajan (2002)), 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 a dummy variable that takes the value of 1 if at least one national bank was suspended in the city in which the firm is located during the 1929-1933 period, and zero otherwise; $ROA_{i,1933}$ 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 β_1 which captures the sensitivity of firm-level employment to local bank failures.

Table 7 compares firms in cities that had a branch of a national bank that was suspended to firms in cities 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%). Importantly, there is no difference in the average amount of bonds due between firms located in cities with suspended national banks relative to firms located in cities with no national bank suspensions.

Table 8 presents the results of estimating regression (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 5.5% to 11.3% drop in employment relative to peer firms that were located in cities with no suspended national banks. This effect is statistically significant

in Columns (3) through (8) in Table 8, that is, assuming we include state and industry fixed effects. However, Columns (9) and (10) of Table 8 show that, once we control for firm profitability in 1933, the point estimates are substantially smaller – the difference in employment growth is now 3.9-5.8% – and not statistically significant from zero. Further, these results are sensitive to the definition of the treatment variable. If instead we use a continuous treatment, where we now define $BANKFAIL$ as the amount of deposits in national banks that failed between 1929 and 1933 in the city the firm is located, scaled by the amount of deposits in national banks in 1928. In this case, the point estimates of β_1 are still negative but quite imprecisely estimated. Table A.5 shows that none of the estimated coefficients β_1 are statistically different from zero.

Our results in this section illustrate that we cannot distinguish between the hypothesis that financial frictions affected firm employment and two alternative hypothesis. First, that both bank failures and the contraction in employment were the result of poor economic conditions which affected both firms and banks, or second, that bank distress affected firm employment through a demand channel as opposed to a reduction in the supply of credit. To help distinguish among these hypotheses, we explore an empirical specification that interacts both treatment definitions.

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 9 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:

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

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 1930 and 1934 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 includes 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 10, we see that the estimated coefficients are negative, and statistically significant across all specifications. The magnitude of the estimated coefficient β_3 ranges from -2.4 to -3.0 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 8.8 to 11.1% 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 our tripple-difference specification (4). First, specification (4) interacts the continuous measure of bonds due 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 11 presents the results. Examining the first row of Table 11, we see that, first, the economic magnitudes are substantial: the firms in cities with national bank suspensions that had debt due during the 1930-34 period experienced a 21-25% greater drop in employment relative to firms in the same city that had no bonds due in 1930-34. 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 24-34% (or 43%-57%) 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 that were located in cities with suspended national banks are 62 and 31, 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 a continuous version of $BANKFAIL$, defined as the amount of deposits in national banks that failed between 1929 and 1933 in the city in which the firm is located, scaled by the amount of deposits in national banks in 1928. As Table 12 demonstrates, the coefficient β_3 remains statistically significant across our specification.

We perform a further set of robustness checks; to conserve space, we relegate these results to the online appendix. Table A.6 shows that repeating the placebo experiment in which we replace bonds maturing in 1930-34 with bonds maturing before the crisis (1928) results in statistically insignificant estimates of β_3 across specifications. Table A.7 shows that our main results are robust to excluding bonds that were issued prior to January 1929 from the definition of treatment. Table A.8 shows that our results are not materially affected by the inclusion of bonds maturing in 1934 from the construction of the treatment variable. Last, Table A.9 shows that our results are robust to dropping firms with no leverage in 1928 from the sample.

6 Aggregate Impact of Financial Frictions

Our results so far indicate that firms that had a substantial amount of bonds that were due in the 1930-34 period, also sharply cut employment. Further, these estimated effects are substantially stronger if these firms also happened to be located in cities that experienced suspensions of national banks. Under the assumption that our identification strategy is valid, our analysis in the previous section delivers a causally estimated elasticity of firm employment to short-term debt. However, evaluating the magnitude of this estimated elasticity is challenging – especially its implications for the aggregate change in employment within our sample. For instance, the treated firms account for a small fraction of the firms in

our sample (approximately 9%-15% depending on the treatment definition). Hence even if the elasticity is well-identified, the direct causal effect may only account for a small share of the overall drop in employment in our sample.

We attempt to evaluate the aggregate impact of finance on employment during the Great Depression in two ways. First, we use the estimated elasticities to compute a counterfactual level of aggregate employment within the firms in our sample assuming the estimated treatment effect was equal to zero. We report the results in Section 6.1; we find that employment would have been 0.9% to 1.8% higher than if no firms had debt due during the crisis, which accounts for between 10% and 20% of the overall drop in employment among our sample of firms (9%). We refer to this estimate as the direct treatment effect.

Second, in Section 6.2 we explore the implications of our estimates for the role of financial frictions during the Great Depression. That is, we try to identify the impact of financial frictions on firms that needed to access the external markets for reasons that were not limited to refinancing their maturing debt. This calculation is significantly more challenging; to make progress we need to interpret the magnitude of the estimated elasticity through the lens of a structural model. We use a simple model to compute a counterfactual aggregate employment level in the absence of any external financing frictions. Our estimates imply that aggregate employment would have been 2.0% to 3.9% higher in the absence of external financing frictions. However, since these estimates rely on fairly strong assumptions, some caveats are in order.

6.1 Evaluating the direct effect

Here, we compute the counterfactual level of employment within our sample under the scenario in which the treatment effect we estimate in equations (2) and (4) were uniformly equal to zero. We provide a range of estimates depending on our different definitions of the treatment.

We first examine the definition of treated firms as those having maturing debt in 1930-34, which corresponds to equation (2). We compute the portion of the change in the number of employees E_i of firm i between 1933 and 1928 that can be attributed to the term $\beta_1 \, BDUE_i$, as

$$\Delta \hat{E}^{A}_{i,1933} = \left[\exp\left(\hat{\beta}_1 BDUE_i + \hat{c} \, Z_{it} \right) - \exp\left(\hat{c} \, Z_{it} \right) \right] \, E_{i,1928}, \tag{5}$$

where $\hat{c} \, Z_{it}$ includes all the other variables in equation (2). All the estimates (the main coefficient of interest $\hat{\beta}_1$ plus the coefficients on the remaining variables and fixed effects $\hat{c}$) are computed using the specification in column (10) of Table 4, which includes all controls

and fixed effects. The sum is computed over all the 767 firms that are included in this specification.

We repeat the same calculation with our second definition of treatment – using the estimates from equation (4) – as

$$\Delta \hat{E}^{B}_{i,1933} = \left[\exp\left(\hat{\beta}_3\, BANKFAIL_i \times BDUE_i + \hat{c}\, Z_{it}\right) - \exp\left(\hat{c}\, Z_{it}\right)\right] E_{i,1928}, \tag{6}$$

As before, we use the point estimates corresponding to the specification in column (10) of Table 10, which includes all controls and fixed effects.

Next, 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

$$\hat{G}^{r}_{E} = \frac{\sum_f \Delta \hat{E}^{r}_{i,1933}}{\sum_f E_{i,1928}}. \tag{7}$$

As a robustness check, we also compute the quantities (5)-(7) above using the discrete treatment specification of Table 11, which replaces $BDUE$ in equation (4) with a dummy that takes the value one if the firm had any maturing debt between 1930-34, and zero otherwise.

Our estimates of the aggregate magnitude of the direct treatment effect range from 0.9% to 1.8% depending on the definition of treatment. To evaluate these magnitudes, note that the total drop in employment among all the firms in our sample that are included in the specification of column (10) of Tables 4 and 10 is equal to 9.0%. Using the estimates from equation (2), we find that the direct treatment effect is equal to 0.9%, or approximately 10% of the overall drop in employment. If we instead define the set of treated firms as those that had maturing debt and were located in cities with failed national banks – equation (4) – the magnitude of the direct treatment effect implies an aggregate drop of 1.4% in employment – or approximately 16% of the overall drop. Last, if instead we use the discrete treatment specification of Table 11, our estimates imply an overall drop in employment of 1.8%, or approximately 19% of the total contraction.

We also compute these direct treatment effects aggregated across industries and regions. To conserve space, we focus on the estimates from equation (4). We find that the five most affected industries are: auto manufacturers (3.6-4.1% drop); manufacturers of aviation and railroad equipment (4.5-7.2% drop); manufacturers of steel products (2.2-2.5% drop); chemicals (1.5-2.4% drop); and business supplies (3.5-4.6% drop). In terms of regions, our estimates imply that the Midwest and the Northeast were the most severely affected (drop of

2.1-2.4% and 1.2-1.9% respectively). In the western and southern U.S. the magnitude of the direct treatment effects is smaller – 0.3-0.4% and 0.3-0.6% drop in employment, respectively.

6.2 Interpreting the estimates through a structural model

The analysis in the previous section attempts to evaluate the aggregate impact of the treated effect on employment. However, since we are focusing on causally estimating the impact of the financing friction, it may be that our definition of the treatment is somewhat narrow, and hence those estimates are rather conservative. It is likely that firms without maturing debt were also dependent on access to external funds and were also likely to be adversely affected by the collapse of the financial sector in the Great Depression.

To evaluate the total effect of financial frictions on employment during the Great Depression we interpret the estimated elasticities found earlier through the lens of a simple structural model. The model allows us to run similar regressions as in our empirical analysis on data we simulate from the model. We calibrate the model so that it matches the data along several dimensions, and most importantly, such that it delivers similar elasticities of employment to maturing debt in simulated data as our empirical estimates. Matching this elasticity essentially identifies the parameter governing the severity of the financing friction in the model. We then compute counterfactual levels of employment by setting this parameter to zero.

Model Setup

We present a simple model of firm employment with financial frictions. In the model, firms produce output $y_{i,t}$ with labor L using a decreasing-returns to scale technology,

$$y_{i,t} = e^{z_{i,t}} L_{i,t}^{\beta}. \tag{8}$$

In addition to labor, each firms is endowed with one unit of a fixed factor of production (land). Land – in our model – serves the role of collateral and enables firms to issue risk-free debt. We assume that land has a liquidation value that is equal to $\bar{D}$.

Importantly, there is a mismatch in the time at which labor is hired, and the time at which output is produced. That is, the labor that participates in production at time t in (8) is decided at $t-1$. In deciding how many workers to hire, managers take into account their expectations about the firm's productivity at time t, which follows an AR(1) process,

$$z_{i,t} = \kappa\, z_{i,t-1} + \sigma_z \varepsilon_{i,t} \tag{9}$$

where $\varepsilon_{i,t} \sim N(0,1)$ is i.i.d.

For simplicity we assume that firms can only issue one-period bonds. Next, we model the financing friction with quadratic costs of debt issuance: if the firm wants to issue bonds with present value D_t, then it must also incur a cost equal to

$$c(D_t) = \frac{\phi_t}{2} D_t^2 \, \mathbf{1}_{\mathbf{D_t > 0}} \tag{10}$$

where ϕ_t parameterizes the costs of external finance. Since our focus is *not* in the endogenous dynamics of leverage – we will take the distribution of leverage across firms as given – we will assume that there are no taxes or costs of financial distress. Firms are endowed with one unit of land that serves as collateral for their bonds. The firm owners can choose to walk away from the firm; in that case, bond holders seize the land. We restrict attention to risk-less debt, which imposes the constraint $D_t \leq \bar{D}$ always.

We first examine the case where the financing friction is purely transitory – it lasts for one period. Denoting the crisis period by $t = t'$, we assume that the financing cost is uniformly zero in the non-crisis period, $\phi_t = 0$ for all $t \neq t'$ in which case firms can costlessly issue debt or equity. Our focus is then on the actions of firms during the crisis $t = t'$, which case firms cannot issue equity, and there is a cost of issuing debt, $\phi_{t'} = \phi$. That is, we examine the actions of firms that inherit a stock of maturing debt D from the previous period, and need make a labor decision given a transitory shock to the costs of external finance ϕ. Our assumption that firms believe that the crisis is transitory, $\phi = 0$ for $t > t'$ thus rule out any precautionary savings motive on the part of the firms, and is therefore conservative.

The firm's optimization problem can be written as,

$$V(D, L, z) = \max_{L', D'} \Big\{ e^z L^\beta - w\, L' - R\, D + D' - c(D') + R^{-1} E\Big[V(D', L', z') | z \Big] \Big\}, \tag{11}$$

subject to the constraints

$$D' = \left(w\, L' + R\, D - e^z L^\beta \right)^+ \tag{12}$$

and

$$D' \leq \bar{D}. \tag{13}$$

That is, we assume that firms issue debt to finance any shortfalls. If after the labor decision there are surplus funds, these are paid out to equity owners. Since firms believe that there are no credit market distortions ($\phi = 0$) for $t > t'$, and assuming that $\bar{D}$ is sufficiently high, firms have no incentive to save. We later relax this assumption and allow D to be negative.

Given our assumptions, the firm's optimal labor choice L' satisfies the first order condition,

$$w\Big[1+\phi\left(R\,D+wL'-e^{z}L'^{\beta}\right)^{+}\Big]=\beta\,R^{-1}\,e^{\kappa z+\frac{1}{2}\sigma_z^2}L'^{\beta-1}. \tag{14}$$

The first order condition reveals that the presence of financial frictions increase the marginal cost of hiring workers. Since the costs of issuance are quadratic, the marginal cost of issuance is increasing with the amount of external finance needed. Having an analytic solution allows us to compute the elasticity of labor to the financing friction. Focusing on the region where the financing constraint is binding – firms want to issue new debt $D'>0$, an implicit differentiation of (14) with respect to the financing friction ϕ yields

$$\frac{\partial\log L'}{\partial\phi}=-\frac{RD+w\,L'-e^{z}L'^{\beta}}{(1-\beta)(1+\phi(RD+w\,L'-e^{z}L'^{\beta})+\phi\,w\,L'}<0. \tag{15}$$

The elasticity (15) is what we would ideally like to estimate: it summarizes the impact of the financial friction on the firm's overall employment. However, estimating this elasticity directly from the data is not feasible. Instead, our analysis allows us to obtain an estimate of the elasticity of labor with respect to maturing debt. To obtain an expression for this elasticity, we implicitly differentiate (14) with respect to the face value of maturing debt $F=R\,D$, which yields the following expression

$$\frac{\partial\log L'}{\partial F}=-\frac{\phi}{(1-\beta)\left[1+\phi\left(F+w\,L'-e^{z}L'^{\beta}\right)\right]+(1-\beta)\,w\,\phi\,L'}<0. \tag{16}$$

We see that this elasticity, which corresponds to our empirical estimates, is negative. More importantly, this elasticity is informative about the parameter governing the financial friction: the magnitude of (16) is monotonically increasing with ϕ. This fact is important, because it allows us to recover the severity of the financial friction ϕ in the model from the empirical estimates of (16). The result that (16) is unambiguously increasing in ϕ may appear to be in contrast to the result in Kaplan and Zingales (1997) and Hennessy and Whited (2007), which show that the relation between the sensitivity of investment to cashflow and the parameters affecting the costs of external finance are ambiguous. However, the difference results from the fact that we are focusing on the elasticity with respect to maturing debt – as opposed to cashflows. In standard models, operating cashflows are correlated with firm's investment opportunities, which makes the sensitivity of investment to cashflows a non-monotone – and in many case, decreasing – function of the severity of financial constraints. By contrast, maturing debt is independent of the firm's investment opportunities.

Model Simulation and Calibration

We use the model to perform the following experiment. We first fix a set of parameters $\Theta = [\kappa, R, \beta, w, \phi]$. We simulate a panel of firms that make a hiring decision during the crisis. We simulate firms for two periods, $t = 0$ and $t = 1$. Since the model has no capital, we scale the dollar amount of bonds due and the number of employees in the data by book assets. We treat these as equivalent to D and L in the model. We use the empirical joint distribution of bonds due D and labor L to simulate N=50,000 firms. Hence, firms start with an exogenous level of D_0 and L_0.

We

to infer an initial value of idiosyncratic productivity $z_{i,-1}$. Specifically, we assume that labor was chosen prior to 1928 using (14) without any financial frictions, $\phi = 0$. After taking logs of (14), we obtain

$$(1-\beta)\log L_{i,0} = \log\frac{\beta}{wR} + \kappa z_{i,-1} + \frac{1}{2}\sigma_z^2. \tag{17}$$

Since $\frac{1}{N}\sum_f z_{f,-1} = 0$, we can obtain an estimate of implied productivity z under the null of the model using

$$z_{i,-1} = \frac{1-\beta}{\kappa}\left(\log L_{i,0} - \frac{1}{N}\sum_i \log L_{i,0}\right). \tag{18}$$

We use the empirical standard deviation of $z_{f,-1}$ to set $\sigma = \sigma(z_{f,-1})\sqrt{1-\kappa^2}$. We then obtain z_0 and z_1 by simulating according to (9).

We next simulate the firm's labor choice during the crisis period taking the variables z_0, L_0 and D_0 as given. Given the level of ϕ, we obtain the firm's labor choice in the second period L_1 using (14). Given the firm's labor choice, we then estimate the equivalent of equation (2) in simulated data,

$$\log L_{i,1} - \log L_{i,0} = a + \beta D_{i,0} + \rho \log L_{i,0} + c\,\pi_{i,0} + \varepsilon_i \tag{19}$$

where π_0 are accounting profits in period 0, i.e. $\pi_0 = y_{i,0} - wL_{i,0}$.

The last step involves calibrating the model. We choose a real interest rate of 5%, which is equal to the average real interest rate during the 1929-34 period. We choose $\bar{D} = 1$ which ensures that the constraint is almost never binding in our simulations. We choose the remaining parameters $[\kappa, \beta, w, \phi]$ to match the following four moments in the data: a) the estimated elasticity of employment change to bonds due – the estimated coefficients β_1 and β_3 in equations (2)-(4) that includes the full set of controls (corresponding to Column (10) in Tables 4-10); b) the correlation in log employment (scaled by assets) between 1928 and 1933; c) the median level of profitability (scaled by assets) in 1928; d) the interquartile dispersion

in profitability in 1928; last, we clear the labor market under the assumption $\phi = 0$. The last assumption ensures that aggregate employment would have remained constant between $t = 0$ and $t = 1$ in the absence of financial frictions ($\phi = 0$). This last condition determines the wage w so that it would clear the market in the absence of frictions. Effectively, we are assuming that wages remained constant in their pre-crisis, market clearing level.

Table 13 shows these calibration targets. Table 14 shows the model parameters. In general, the parameters are largely in line with typical parametrization in the literature: the firm productivity shock is fairly persistent ($\kappa = 0.86 - 0.88$) and volatile ($\sigma = 0.12$), and there are decreasing returns to scale ($\beta = 0.74$). The estimated costs of external finance ϕ imply that, firms that choose to issue debt in $t = 1$, incurred on average additional costs that were on average equal to 5.5% to 17% of their profits in $t = 1$, depending on whether we target the estimated coefficients β_1 and β_3 in equations (2) and (4), respectively.

Results, Discussion and Caveats

Our calibrated model implies that eliminating the financing friction would result in a 2.0-2.9% increase in the overall level of employment. Specifically, we compare the overall level of employment under the calibrated parameters to the counterfactual level under the assumption that $\phi = 0$. When we calibrate the model to match the estimated elasticity in equation (2), we obtain an overall effect equal to 2.0%. If instead we focus on matching the estimated elasticity of labor to maturing debt using the specification in (4), the estimated effect is somewhat higher at 2.9%, or approximately 30% of the overall drop.

These estimated effects are larger relative to those in Section 6.1 because the nature of the experiment is now different. Now, the set of firms that are affected by the financing constraints is not simply firms that need to refinance their maturing debt. Firms with no maturing debt are also affected, in particular those whose current current level of profitability is insufficient to achieve their desired level of employment and must therefore access external funds.

We emphasize that these estimates are based on fairly strong assumptions, hence many caveats are in order. In the interest of simplicity, our simple model abstracts from many relevant features. An incomplete list includes: a) there is no capital; b) there are no fixed costs of production; c) our production function delivers a very tight link between current cashflows – which depends on $Z = \exp(z)$ – and the return to hiring employees – which is proportional to Z^{κ}; d) there are no adjustment costs to labor; e) wages in the model are constant; and f) the financing friction is transitory and lasts for only one period.

Our intuition is that omitting some of these features likely leads us to obtain conservative estimates. Specifically, allowing for investment in capital (a) may have an ambiguous impact depending on the elasticity of substitution between capital and labor, but it could also lead to larger magnitudes if investment in capital also needs to be financed externally. The assumption of lack of fixed costs (b) is rather conservative; if firms needed to also finance a fixed operating cost, then many of them would exit, which would magnify the drop in employment in the model. Assumption (c) ameliorates the impact of the financing friction, since it introduces a fairly strong correlation between cashflows from operations and hiring needs. In alternative models in which firms received shocks to their investment opportunities that were uncorrelated with their operating cashflows (e.g. Kogan and Papanikolaou, 2014), the impact of financial frictions would likely be greater. Assumption (d) implies that hiring in the model is fairly sensitive to both productivity and the financing cost. If adjusting labor is costly, the model would need both a larger financing cost and a less persistent productivity process to match the elasticity of labor to debt due and the persistence of employment; both changes would likely lead to larger magnitudes. Our assumption of constant wages (e) may appear especially strong, since it precludes general equilibrium effects that could dampen the fall in employment in the model. However, this assumption is consistent with the data: between 1929 and 1933, wages actually *increased* in real terms by approximately 4%.

Last, the assumption that the financing shock is transitory (f) also leads to conservative estimates and is perhaps the most unrealistic. To evaluate the importance of this assumption, we numerically solve and calibrate an extended version of the model where we allow firms to borrow and save at a constant rate R; external borrowing is subject to a constant financing cost ϕ; firms can borrow up to the value of their collateral (equal to $\bar{D}$); if equity holders default, debt holders seize the collateral. In this version of the model, firms anticipate that the financing friction will exist permanently, which leads to more conservative hiring decisions. We perform the same calibration exercise using the extended model. Depending on the value of the elasticity of labor to maturing debt that we target, our (preliminary) estimates indicate that removing the financing friction would increase the overall level of employment by 2.8% to 3.9%, again depending on the value of the estimated elasticity that we target.

7 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 recession 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

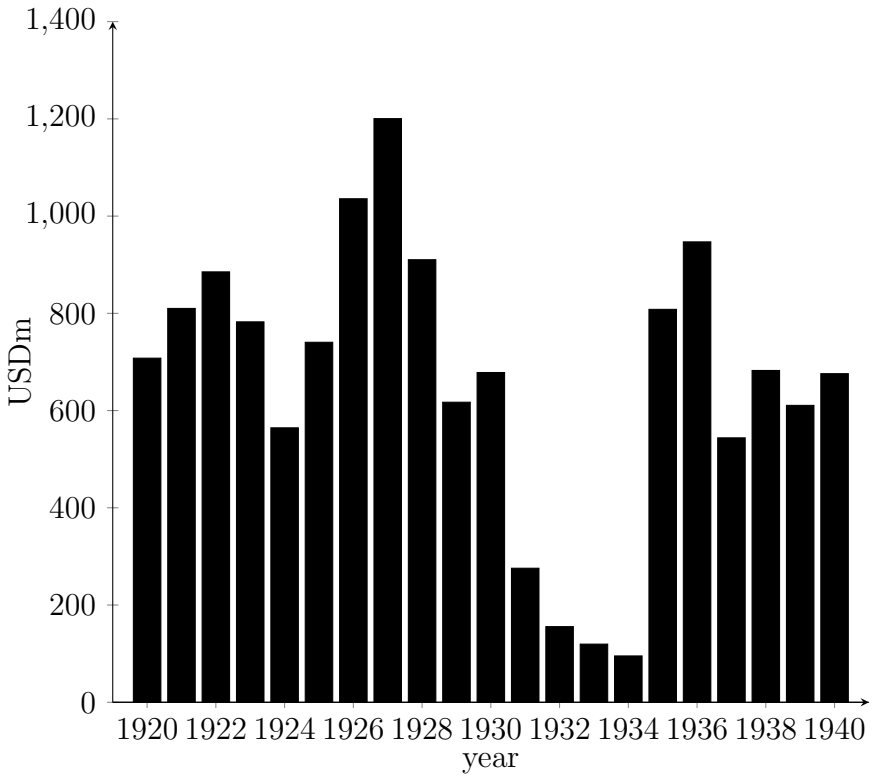
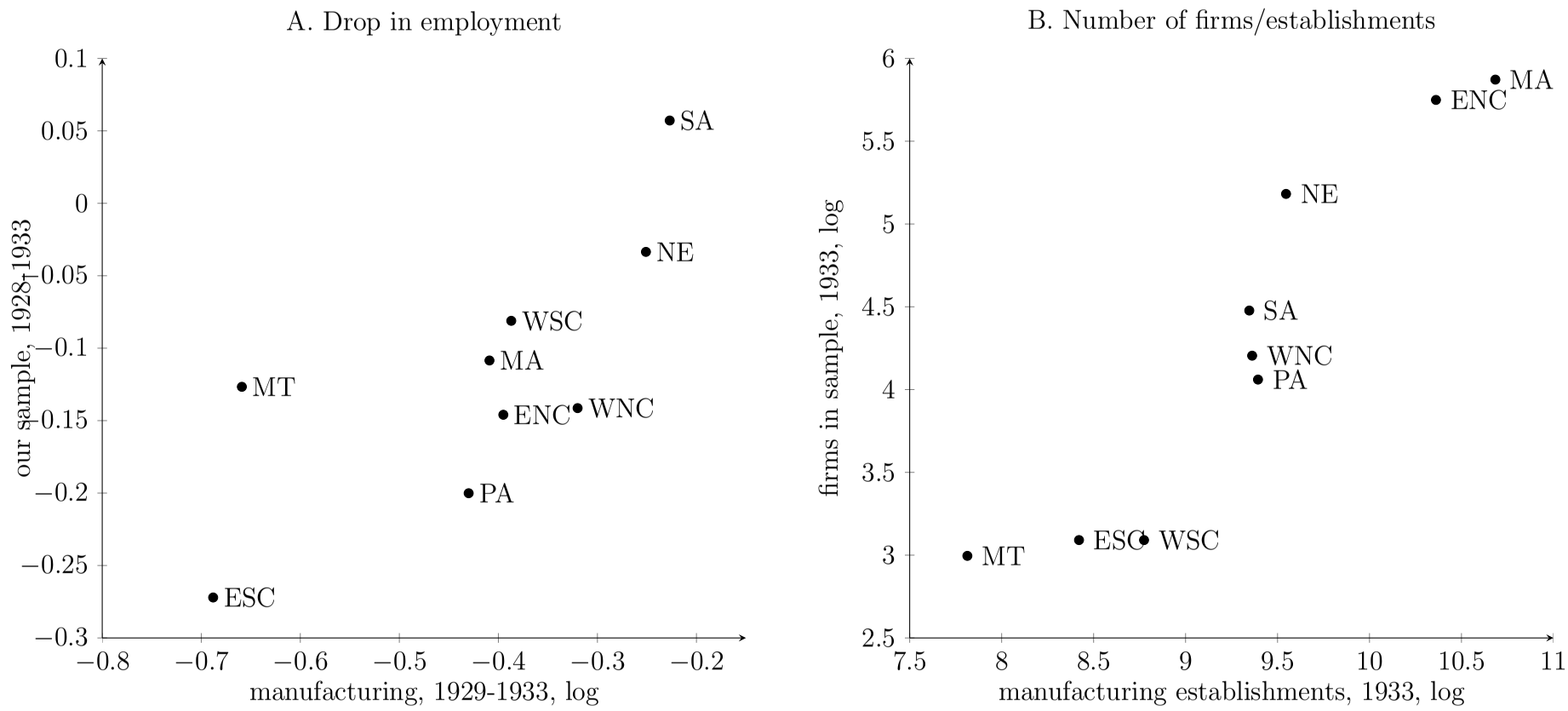


Figure plots the par amount of new offerings of corporate bonds in the 1920-40 period of Industrial firms. Source, "Statistical Measures of Corporate Bond Financing Since 1900, Table 52, (Hickman, 1960).

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



The employment drop in manufacturing is based on data from the Census of Manufacturers and is reported in Rosenbloom and Sundstrom (1999), Table 7, which covers 20 selected manufacturing industries. The number of manufacturing establishments are from Price Fishback's data library (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, 1928	851	0.09	0.07	-0.01	0.01	0.03	0.07	0.12	0.19	0.25
Profitability, 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
Leverage, 1928	1059	0.13	0.14	0.00	0.00	0.00	0.08	0.22	0.36	0.42
Leverage, 1933	1040	0.12	0.15	0.00	0.00	0.00	0.05	0.19	0.34	0.45
Firm Age, log, 1928	1102	2.47	1.10	0.00	0.69	1.79	2.77	3.33	3.66	3.85
Bonds due (1930-34)	1010	0.01	0.04	0.00	0.00	0.00	0.00	0.00	0.04	0.09
Number of suspended national banks (1929–33)	1010	5.83	9.12	0	0	0	2	8	20	34
Fraction of suspended national banks (1929–33)	1010	0.22	0.22	0.00	0.00	0.00	0.14	0.33	0.55	0.67
Fraction of suspended national banks, deposit-weighted (1929–33)	1010	0.16	0.44	0.00	0.00	0.00	0.02	0.09	0.43	0.63

Employment is number of employees; profitability is the ratio of net income to the book value of assets in each year; leverage is the ratio of the book value of debt to the book value of assets in each year; firm age is the years since the firm's year of incorporation; bonds due is the total amount that is due between January 1930 and December 1934, scaled by the average of book assets between 1928 and 1933. The data on suspended national banks comes from ICPSR. The fraction of suspended national banks between 1929 and 1933 in each city uses the number of national banks in 1928 as the denominator; similarly, the fraction of deposits in national banks that failed in 1929-33 uses the total amount of deposits in national banks as of 1928 as the denominator. We only include observations with non-missing observations for employment in both 1928 and 1933.

Table 2: Employment change and firm characteristics

$\Delta \log E_t$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
D_t/A_t	-0.555***	-0.540***	-0.550***	-0.562***	-0.611***	-0.613***	-0.510***	-0.224**	-0.245*
	(0.137)	(0.122)	(0.133)	(0.127)	(0.122)	(0.129)	(0.145)	(0.095)	(0.124)
$\log E_{t-1}$				-0.041**	-0.141***	-0.141***	-0.187***	-0.161***	-0.155***
				(0.016)	(0.036)	(0.039)	(0.045)	(0.047)	(0.045)
$\log A_{t-1}$					0.133***	0.126***	0.172***	0.131**	0.133**
					(0.036)	(0.039)	(0.047)	(0.049)	(0.048)
ROA_{t-1}							1.693***	1.139**	1.007**
							(0.438)	(0.441)	(0.402)
ROA_t								2.627***	2.593***
								(0.422)	(0.433)
$\log AGE$									-0.045*
									(0.025)
Observations	1040	1040	1039	1039	1009	1009	823	787	768
R^2	0.021	0.139	0.177	0.184	0.210	0.259	0.327	0.400	0.394
Fixed Effects	-	S	S, I	S, I	S, I	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 ($\log A_{t-1}$), firm profitability in 1933 (ROA_t), and profitability in 1928 (ROA_{t-1}). Depending on the specification, we include state (columns 2-9), industry (columns 3-5) or industry-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).

Table 3: Comparison on observables between treatment and control A: bonds due

Bonds due 1930-34	$= 0$ (Control)		> 0 (Treatment)		Difference (p-value)	
	Mean	Median	Mean	Median	Mean	Median
Employment, log, 1928	6.701	6.742	6.646	6.526	0.65	0.09
Profitability, 1928	0.068	0.057	0.068	0.062	0.91	0.38
Book Assets, log, 192	15.604	15.525	15.642	15.456	0.74	0.73
Debt to Assets,1928	0.176	0.141	0.234	0.220	0.00	0.00
Firm Age, log, 1928	2.458	2.773	2.588	2.705	0.15	0.37
Firms	673		140			

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. The treatment dummy takes the value of 1 if the firm had any bonds outstanding with amounts due between 1930 and 1934. The p-values on the difference in medians between the treatment and the control sample are obtained from a quantile regression on a treatment dummy.

Table 4: Treatment A: firm funding needs and fall in employment

$\Delta \log E_t$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BDUE	-1.415**	-1.234**	-1.487***	-1.111**	-1.151**	-1.188**	-1.171**	-1.149*	-1.230*	-1.291**
	(0.582)	(0.505)	(0.448)	(0.429)	(0.445)	(0.428)	(0.487)	(0.601)	(0.610)	(0.605)
D_{t-1}/A_{t-1}				-0.380**	-0.389**	-0.443**	-0.441**	-0.268	-0.154	-0.200
				(0.154)	(0.151)	(0.168)	(0.186)	(0.171)	(0.171)	(0.163)
$\log E_{t-1}$					-0.037**	-0.144***	-0.146***	-0.190***	-0.163***	-0.158***
					(0.016)	(0.037)	(0.041)	(0.046)	(0.047)	(0.045)
$\log A_{t-1}$						0.138***	0.132***	0.177***	0.135**	0.137***
						(0.036)	(0.040)	(0.047)	(0.049)	(0.047)
ROA_{t-1}								1.740***	1.075**	0.927**
								(0.434)	(0.440)	(0.401)
ROA_t									2.702***	2.666***
									(0.430)	(0.436)
$\log AGE$										-0.043
										(0.026)
Observations	1010	1010	1009	1008	1008	1008	1008	822	786	767
R^2	0.007	0.132	0.172	0.179	0.185	0.206	0.254	0.322	0.403	0.399
Fixed Effects	-	S	S, I	S, I	S, I	S, I	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 total dollar amount of bonds that are due between 1930-1934, 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 (ROA_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 2-10), industry (columns 3-6) or industry-region (columns 7-10) fixed effects, where regions are classified according the US Census definition (4 regions) We classify firms into 30 industries following Fama and French (1997).

Table 5: Robustness: firm funding needs and fall in employment – bonds issued prior to 1929

$\Delta \log E_t$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BDUE	-1.600*	-1.514**	-1.811***	-1.309**	-1.423**	-1.394**	-1.433**	-1.546	-1.671*	-1.738*
	(0.817)	(0.636)	(0.595)	(0.606)	(0.627)	(0.578)	(0.694)	(0.908)	(0.941)	(0.930)
D_{t-1}/A_{t-1}				-0.381**	-0.387**	-0.444**	-0.441**	-0.263	-0.150	-0.194
				(0.160)	(0.156)	(0.174)	(0.190)	(0.177)	(0.177)	(0.169)
$\log E_{t-1}$					-0.038**	-0.144***	-0.146***	-0.191***	-0.163***	-0.159***
					(0.016)	(0.037)	(0.041)	(0.046)	(0.047)	(0.045)
$\log A_{t-1}$						0.136***	0.131***	0.177***	0.134**	0.136***
						(0.036)	(0.040)	(0.047)	(0.049)	(0.047)
ROA_{t-1}								1.745***	1.084**	0.943**
								(0.433)	(0.442)	(0.404)
ROA_t									2.701***	2.667***
									(0.429)	(0.435)
$\log AGE$										-0.041
										(0.025)
Observations	1010	1010	1009	1008	1008	1008	1008	822	786	767
R^2	0.006	0.131	0.172	0.179	0.185	0.205	0.254	0.322	0.403	0.399
Fixed Effects	-	S	S, I	S, I	S, I	S, I	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 total dollar amount of bonds that are due between 1930-1934 and were issued prior to 1929, 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 (ROA_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 2-10), industry (columns 3-6) or industry-region (columns 7-10) fixed effects, where regions are classified according the US Census definition (4 regions) We classify firms into 30 industries following Fama and French (1997).

Table 6: Placebo: firm funding needs and fall in employment – bonds maturing in 1928

$\Delta \log E_t$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BDUE	-0.928	1.710	2.891	4.650	3.871	4.919	1.326	1.053	2.149	2.321
	(4.346)	(4.600)	(5.426)	(5.293)	(5.295)	(4.945)	(5.148)	(4.174)	(3.283)	(3.029)
D_{t-1}/A_{t-1}				-0.455***	-0.465***	-0.523***	-0.512**	-0.317*	-0.209	-0.261
				(0.159)	(0.155)	(0.173)	(0.188)	(0.166)	(0.166)	(0.161)
$\log E_{t-1}$					-0.036**	-0.142***	-0.143***	-0.187***	-0.160***	-0.155***
					(0.016)	(0.037)	(0.040)	(0.046)	(0.047)	(0.045)
$\log A_{t-1}$						0.138***	0.130***	0.175***	0.132**	0.134***
						(0.036)	(0.040)	(0.047)	(0.049)	(0.048)
ROA_{t-1}								1.785***	1.115**	0.961**
								(0.437)	(0.449)	(0.411)
ROA_t									2.721***	2.688***
									(0.434)	(0.441)
$\log AGE$										-0.046*
										(0.026)
Observations	1010	1010	1009	1008	1008	1008	1008	822	786	767
R^2	0.000	0.127	0.166	0.176	0.182	0.202	0.251	0.319	0.399	0.395
Fixed Effects	-	S	S, I	S, I	S, I	S, I	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 total dollar amount of bonds that are due in 1928, 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 (ROA_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 2-10), industry (columns 3-6) or industry-region (columns 7-10) fixed effects, where regions are classified according the US Census definition (4 regions) We classify firms into 30 industries following Fama and French (1997).

Table 7: Comparison on observables between treatment and control B: number of bank failures

BANKFAIL	0		1		Difference(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
Firm Age, log, 1928	2.520	2.833	2.426	2.708	0.20	0.25
Bonds Due	0.011	0.000	0.011	0.000	0.69	-
Firms	323		687			

We only include observations with non-missing observations for employment and book assets in both 1928 and 1933. The treatment dummy $BANKFAIL_i$, takes the value of 1 if at least one national bank was suspended in the city in which the firm is located during the 1929-1933 period, and zero otherwise. The p-values on the difference in medians between the treatment and the control sample are obtained from a quantile regression on a treatment dummy.

Table 8: Treatment B: Bank failures and fall in employment

$\Delta \log E_t$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BANKFAIL	-0.055	-0.060	-0.089**	-0.086**	-0.086**	-0.085*	-0.105**	-0.113*	-0.039	-0.058
	(0.049)	(0.037)	(0.039)	(0.041)	(0.042)	(0.041)	(0.038)	(0.060)	(0.053)	(0.052)
D_{t-1}/A_{t-1}				-0.440**	-0.451***	-0.506***	-0.502**	-0.298*	-0.200	-0.247
				(0.159)	(0.155)	(0.170)	(0.187)	(0.163)	(0.165)	(0.159)
$\log E_{t-1}$					-0.037**	-0.141***	-0.142***	-0.185***	-0.160***	-0.154***
					(0.016)	(0.037)	(0.041)	(0.047)	(0.048)	(0.046)
$\log A_{t-1}$						0.135***	0.128***	0.172***	0.130**	0.132**
						(0.037)	(0.041)	(0.049)	(0.050)	(0.049)
ROA_{t-1}								1.800***	1.126**	0.983**
								(0.436)	(0.448)	(0.406)
ROA_t									2.706***	2.664***
									(0.435)	(0.441)
$\log AGE$										-0.044*
										(0.025)
Observations	1010	1010	1009	1009	1009	1009	1009	823	787	768
R^2	0.002	0.129	0.168	0.178	0.183	0.203	0.254	0.322	0.399	0.394
Fixed Effects	-	S	S, I	S, I	S, I	S, I	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 $BANKFAIL_i$, a dummy variable that takes the value of 1 if at least one national bank was suspended in the city in which the firm is located during the 1929-1933 period, and zero otherwise. 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 (ROA_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 2-10), industry (columns 3-6) or industry-region (columns 7-10) fixed effects, where regions are classified according the US Census definition (4 regions) We classify firms into 30 industries following Fama and French (1997).

Table 9: Comparison on observables between treatment and control: interaction $A \times B$

	$BANKFAIL = 0$				$BANKFAIL = 1$				Difference	
	BDUE=0 (a)		$BDUE > 0$ (b)		BDUE=0 (c)		$BDUE > 0$ (d)		(d)-(b)	
	Mean	p50	Mean	p50	Mean	p50	Mean	p50	Mean	p50
Employment, log, 1928	6.740	6.774	6.528	6.501	6.817	6.908	6.697	6.533	0.47	0.95
Profitability, 1928	0.059	0.049	0.081	0.071	0.072	0.061	0.063	0.062	0.15	0.20
Book Assets, log, 1928	15.556	15.463	15.572	15.307	15.680	15.647	15.672	15.505	0.67	0.52
Debt to Assets,1928	0.157	0.121	0.227	0.217	0.180	0.142	0.237	0.224	0.66	0.81
Firm Age, log, 1928	2.503	2.890	2.438	2.398	2.406	2.708	2.655	2.917	0.20	0.11
Bonds Due	-	-	0.080	0.061	-	-	0.078	0.065	0.82	0.89
Firms	170		42		390		98			

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. The first treatment dummy (BDUE) takes the value of 1 if the firm had any bonds outstanding with amounts due between 1930 and 1934. The second treatment dummy takes the value of 1 if at least one national bank was suspended in the city in which the firm is located during the 1929-1933 period, and zero otherwise (BANKFAIL). The p-values on the difference in medians between the treatment and the control sample are obtained from a quantile regression on a treatment dummy.

Table 10: Main results: firm funding needs × bank failures, and fall in employment

$\Delta \log E_t$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BANKFAIL	-0.021	-0.028	-0.055	-0.051	-0.050	-0.048	-0.066	-0.078	-0.001	-0.021
	(0.054)	(0.039)	(0.043)	(0.045)	(0.045)	(0.045)	(0.043)	(0.069)	(0.054)	(0.053)
BDUE	0.642	0.749	0.459	0.920	0.900	0.922	1.034	0.765	0.987	0.788
	(0.889)	(0.699)	(0.718)	(0.719)	(0.713)	(0.646)	(0.753)	(0.851)	(0.824)	(0.876)
BANKFAIL × BDUE	-3.001***	-2.895***	-2.814***	-2.927***	-2.958***	-3.043***	-3.130***	-2.702**	-3.199***	-3.018***
	(0.959)	(0.824)	(0.855)	(0.820)	(0.791)	(0.737)	(0.827)	(1.009)	(1.040)	(1.061)
D_{t-1}/A_{t-1}				-0.385**	-0.395**	-0.448**	-0.450**	-0.262	-0.163	-0.202
				(0.161)	(0.158)	(0.175)	(0.191)	(0.175)	(0.176)	(0.168)
$\log E_{t-1}$					-0.039**	-0.144***	-0.147***	-0.189***	-0.163***	-0.158***
					(0.016)	(0.037)	(0.041)	(0.046)	(0.047)	(0.045)
$\log A_{t-1}$						0.137***	0.131***	0.174***	0.132**	0.135***
						(0.036)	(0.040)	(0.047)	(0.048)	(0.047)
ROA_{t-1}								1.710***	1.029**	0.905**
								(0.432)	(0.441)	(0.396)
ROA_t									2.710***	2.669***
									(0.422)	(0.425)
log(AGE										-0.039
										(0.026)
Observations	1010	1010	1009	1009	1009	1009	1009	823	787	768
R^2	0.016	0.140	0.180	0.187	0.193	0.214	0.263	0.329	0.409	0.404
Fixed Effects	-	S	S, I	S, I	S, I	S, I	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 total dollar amount of bonds that are due between 1930-1934, as a fraction of the average of book assets between 1928 and 1933 (BDUE), interacted with dummy that takes the value 1 if at least one national bank was suspended in the city in which the firm is located, and zero otherwise (BANKFAIL). 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 (ROA_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 2-10), industry (columns 3-6) or industry-region (columns 7-10) fixed effects, where regions are classified according the US Census definition (4 regions) We classify firms into 30 industries following Fama and French (1997).

Table 11: Robustness: Discrete treatment – Firm funding needs (greater than x% of assets)

$\Delta \log E_t$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$BANKFAIL \times D(BDUE \geq 0)$	-0.249***	-0.218***	-0.212***	-0.222***	-0.217***	-0.208***	-0.245***	-0.201**	-0.229**	-0.224**
	(0.087)	(0.070)	(0.071)	(0.071)	(0.070)	(0.072)	(0.074)	(0.097)	(0.100)	(0.104)
R^2	0.011	0.135	0.174	0.182	0.187	0.208	0.259	0.326	0.404	0.400
$BANKFAIL \times D(BDUE \geq 5\%)$	-0.268**	-0.264***	-0.243**	-0.260***	-0.268***	-0.281***	-0.294***	-0.271*	-0.343**	-0.317**
	(0.110)	(0.093)	(0.091)	(0.093)	(0.089)	(0.084)	(0.093)	(0.139)	(0.143)	(0.135)
R^2	0.010	0.135	0.175	0.182	0.188	0.209	0.259	0.326	0.405	0.400
$BANKFAIL \times D(BDUE \geq 10\%)$	-0.496***	-0.436***	-0.433**	-0.459**	-0.460**	-0.497***	-0.504***	-0.449**	-0.574**	-0.533**
	(0.154)	(0.155)	(0.183)	(0.175)	(0.173)	(0.156)	(0.179)	(0.195)	(0.213)	(0.213)
R^2	0.012	0.136	0.176	0.185	0.190	0.212	0.261	0.327	0.406	0.401
Observations	1010	1010	1009	1009	1009	1009	1009	823	787	768
Controls										
Leverage, 1928				Y	Y	Y	Y	Y	Y	Y
Employment, 1928					Y	Y	Y	Y	Y	Y
Book Assets, 1928						Y	Y	Y	Y	Y
Profitability, 1928								Y	Y	Y
Profitability, 1933									Y	Y
Firm Age, 1928										Y
Fixed Effects	-	S	S, I	S, I	S, I	S, I	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 a treatment dummy that takes the value of 1 if the total dollar amount of bonds that are due between 1930-1934, as a fraction of the average of book assets between 1928 and 1933 exceeds a threshold $x\%$, where $x = 0, 5, 10]$. We interact the bonds-due dummy with $BANKFAIL_i$, a dummy variable that takes the value of 1 if at least one national bank was suspended in the city in which the firm is located during the 1929-1933 period, and zero otherwise. 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 (ROA_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 2-10), industry (columns 3-6) or industry-region (columns 7-10) fixed effects, where regions are classified according the US Census definition (4 regions) We classify firms into 30 industries following Fama and French (1997).

Table 12: Robustness: firm funding needs × bank failures (continuous treatment, deposits weighted)

$\Delta \log E_t$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BANKFAIL	-0.001	0.049	0.002	0.002	0.005	0.012	0.019	-0.078	-0.063	-0.065
	(0.063)	(0.065)	(0.080)	(0.077)	(0.073)	(0.070)	(0.074)	(0.127)	(0.101)	(0.093)
BDUE	-1.184	-0.934	-1.199**	-0.802	-0.846	-0.934*	-0.897	-0.909	-0.914	-0.944
	(0.695)	(0.579)	(0.517)	(0.508)	(0.530)	(0.510)	(0.568)	(0.685)	(0.744)	(0.754)
BANKFAIL × BDUE	-1.296	-1.689**	-1.553**	-1.648**	-1.627**	-1.354*	-1.431**	-1.269*	-1.535*	-1.676**
	(0.953)	(0.651)	(0.647)	(0.657)	(0.657)	(0.682)	(0.677)	(0.649)	(0.769)	(0.767)
D_{t-1}/A_{t-1}				-0.382**	-0.391**	-0.442**	-0.442**	-0.257	-0.147	-0.192
				(0.155)	(0.152)	(0.169)	(0.186)	(0.175)	(0.174)	(0.167)
$\log E_{t-1}$					-0.038**	-0.142***	-0.144***	-0.187***	-0.160***	-0.154***
					(0.016)	(0.037)	(0.041)	(0.046)	(0.047)	(0.045)
$\log A_{t-1}$						0.134***	0.129***	0.173***	0.130**	0.132***
						(0.037)	(0.041)	(0.048)	(0.049)	(0.048)
ROA_{t-1}								1.792***	1.115**	0.963**
								(0.422)	(0.431)	(0.388)
ROA_t									2.711***	2.675***
									(0.437)	(0.445)
$\log AGE$										-0.043
										(0.026)
Observations	1010	1010	1009	1009	1009	1009	1009	823	787	768
R^2	0.008	0.134	0.174	0.181	0.186	0.206	0.255	0.324	0.405	0.401
Fixed Effects	-	S	S, I	S, I	S, I	S, I	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 total dollar amount of bonds that are due between 1930-1934, as a fraction of the average of book assets between 1928 and 1933 (BDUE), interacted with a continuous variable that equals the amount of deposits in national banks that failed between 1929 and 1933 in the city in which the firm is located, scaled by the amount of deposits in national banks in 1928 (BANKFAIL). 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 (ROA_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 2-10), industry (columns 3-6) or industry-region (columns 7-10) fixed effects, where regions are classified according the US Census definition (4 regions) We classify firms into 30 industries following Fama and French (1997).

Table 13: Calibration targets

Moment	Data	Model A	Model B
Elasticity of labor to maturing debt	-1.291 / -3.018	-1.291	-3.018
Median ROA, 1928	0.071	0.070	0.071
IQR ROA, 1928	0.085	0.087	0.086
Persistence in employment	0.877	0.877	0.877

Table 14: Parameters

Parameter	Symbol	Model A	Model B
Persistence of Firm Shock	κ	0.861	0.877
Volatility of Firm Shock	σ	0.122	0.122
Decreasing returns to scale	β	0.740	0.740
Real interest rate	R	1.050	1.050
Wage	w	1.183	1.180
Financing friction	ϕ	0.881	2.912

A. Additional Results and Robustness Checks

Table A.1: 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	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
Precious Metals, Non-Metallic, and Industrial Metal 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 A.2: Robustness: firm funding needs and fall in employment – exclude bonds due in 1934

$\Delta \log E_t$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BDUE	-1.358*	-1.116*	-1.259**	-0.840	-0.903	-0.982*	-1.036*	-1.052	-1.087	-1.156
	(0.712)	(0.622)	(0.583)	(0.578)	(0.581)	(0.563)	(0.629)	(0.724)	(0.719)	(0.716)
D_{t-1}/A_{t-1}				-0.407**	-0.415**	-0.468**	-0.460**	-0.280	-0.168	-0.215
				(0.159)	(0.155)	(0.172)	(0.190)	(0.171)	(0.171)	(0.165)
$\log E_{t-1}$					-0.037**	-0.144***	-0.145***	-0.189***	-0.163***	-0.157***
					(0.016)	(0.037)	(0.040)	(0.046)	(0.047)	(0.045)
$\log A_{t-1}$						0.138***	0.132***	0.177***	0.134**	0.137***
						(0.036)	(0.040)	(0.047)	(0.049)	(0.047)
ROA_{t-1}								1.752***	1.083**	0.932**
								(0.443)	(0.449)	(0.408)
ROA_t									2.704***	2.669***
									(0.430)	(0.437)
$\log AGE$										-0.044
										(0.026)
Observations	1010	1010	1009	1008	1008	1008	1008	822	786	767
R^2	0.005	0.130	0.169	0.177	0.183	0.204	0.253	0.321	0.401	0.397
Fixed Effects	-	S	S, I	S, I	S, I	S, I	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 total dollar amount of bonds that are due between 1930-1933, 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 (ROA_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 2-10), industry (columns 3-6) or industry-region (columns 7-10) fixed effects, where regions are classified according the US Census definition (4 regions) We classify firms into 30 industries following Fama and French (1997).

Table A.3: Robustness: firm funding needs and fall in employment – exclude firms with zero or missing leverage in 1928

$\Delta \log E_t$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BDUE	-1.600*	-1.514**	-1.811***	-1.309**	-1.423**	-1.394**	-1.433**	-1.546	-1.671*	-1.738*
	(0.817)	(0.636)	(0.595)	(0.606)	(0.627)	(0.578)	(0.694)	(0.908)	(0.941)	(0.930)
D_{t-1}/A_{t-1}				-0.381**	-0.387**	-0.444**	-0.441**	-0.263	-0.150	-0.194
				(0.160)	(0.156)	(0.174)	(0.190)	(0.177)	(0.177)	(0.169)
$\log E_{t-1}$					-0.038**	-0.144***	-0.146***	-0.191***	-0.163***	-0.159***
					(0.016)	(0.037)	(0.041)	(0.046)	(0.047)	(0.045)
$\log A_{t-1}$						0.136***	0.131***	0.177***	0.134**	0.136***
						(0.036)	(0.040)	(0.047)	(0.049)	(0.047)
ROA_{t-1}								1.745***	1.084**	0.943**
								(0.433)	(0.442)	(0.404)
ROA_t									2.701***	2.667***
									(0.429)	(0.435)
$\log AGE$										-0.041
										(0.025)
Observations	1010	1010	1009	1008	1008	1008	1008	822	786	767
R^2	0.006	0.131	0.172	0.179	0.185	0.205	0.254	0.322	0.403	0.399
Fixed Effects	-	S	S, I	S, I	S, I	S, I	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 total dollar amount of bonds that are due between 1930-1934 and were issued prior to 1929, 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 (ROA_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 2-10), industry (columns 3-6) or industry-region (columns 7-10) fixed effects, where regions are classified according the US Census definition (4 regions) We classify firms into 30 industries following Fama and French (1997).

Table A.4: Robustness: Firm funding needs and fall in employment – non-linear controls for leverage

$\Delta \log E_t$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BDUE	-1.415**	-1.234**	-1.487***	-1.048**	-1.096**	-1.137**	-1.111**	-1.121*	-1.232*	-1.285*
	(0.582)	(0.505)	(0.448)	(0.433)	(0.450)	(0.441)	(0.506)	(0.626)	(0.649)	(0.646)
LEV_L				-0.097**	-0.088*	-0.094**	-0.096*	-0.047	-0.062	-0.050
				(0.043)	(0.043)	(0.042)	(0.048)	(0.046)	(0.049)	(0.050)
LEV_M				-0.136**	-0.129**	-0.136**	-0.143**	-0.080	-0.048	-0.059
				(0.053)	(0.053)	(0.052)	(0.062)	(0.050)	(0.058)	(0.060)
LEV_H				-0.165**	-0.163**	-0.185***	-0.179**	-0.104	-0.075	-0.085
				(0.063)	(0.062)	(0.066)	(0.073)	(0.068)	(0.064)	(0.063)
$\log E_{t-1}$					-0.035**	-0.140***	-0.141***	-0.187***	-0.159***	-0.155***
					(0.017)	(0.037)	(0.041)	(0.046)	(0.048)	(0.046)
$\log A_{t-1}$						0.136***	0.129***	0.174***	0.131**	0.133***
						(0.036)	(0.039)	(0.047)	(0.049)	(0.047)
ROA_{t-1}								1.705***	1.029**	0.896**
								(0.423)	(0.446)	(0.419)
ROA_t									2.722***	2.684***
									(0.426)	(0.431)
$\log AGE$										-0.041
										(0.026)
Observations	1010	1010	1009	1009	1009	1009	1009	823	787	768
R^2	0.007	0.132	0.172	0.182	0.187	0.208	0.256	0.322	0.403	0.398
Fixed Effects	-	S	S, I	S, I	S, I	S, I	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 total dollar amount of bonds that are due between 1930-1934 and were issued prior to 1929, 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 (ROA_t), profitability in 1928 (ROA_{t-1}). In addition, we include non-linear controls for leverage: a dummy variable for firms in different leverage tercile, plus a separate dummy for firms with zero leverage. Depending on the specification, we include state (columns 2-10), industry (columns 3-6) or industry-region (columns 7-10) fixed effects, where regions are classified according the US Census definition (4 regions) We classify firms into 30 industries following Fama and French (1997).

Table A.5: Robustness: Bank failures and fall in employment (continuous treatment, deposits weighted)

$\Delta \log E_t$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BANKFAIL	-0.017	0.031	-0.016	-0.018	-0.015	-0.005	0.001	-0.093	-0.082	-0.088
	(0.068)	(0.070)	(0.086)	(0.083)	(0.079)	(0.076)	(0.082)	(0.141)	(0.119)	(0.116)
D_{t-1}/A_{t-1}				-0.444***	-0.456***	-0.510***	-0.506**	-0.304*	-0.197	-0.245
				(0.159)	(0.156)	(0.171)	(0.189)	(0.169)	(0.168)	(0.164)
$\log E_{t-1}$					-0.037**	-0.142***	-0.142***	-0.185***	-0.159***	-0.153***
					(0.016)	(0.037)	(0.041)	(0.047)	(0.048)	(0.046)
$\log A_{t-1}$						0.135***	0.128***	0.173***	0.130**	0.132**
						(0.037)	(0.041)	(0.048)	(0.050)	(0.048)
ROA_{t-1}								1.840***	1.165**	1.010**
								(0.434)	(0.440)	(0.396)
ROA_t									2.713***	2.670***
									(0.437)	(0.445)
$\log AGE$										-0.044*
										(0.025)
Observations	1010	1010	1009	1009	1009	1009	1009	823	787	768
R^2	0.000	0.127	0.166	0.175	0.181	0.201	0.250	0.320	0.400	0.395
Fixed Effects	-	S	S, I	S, I	S, I	S, I	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 a continuous variable that equals the amount of deposits in national banks that failed between 1929 and 1933 in the city in which the firm is located, scaled by the amount of deposits in national banks in 1928 (BANKFAIL). 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 (ROA_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 2-10), industry (columns 3-6) or industry-region (columns 7-10) fixed effects, where regions are classified according the US Census definition (4 regions) We classify firms into 30 industries following Fama and French (1997).

Table A.6: Placebo: firm funding needs × bank failures – bonds due in 1928

$\Delta \log E_t$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BANKFAIL	-0.053	-0.057	-0.083*	-0.079*	-0.079*	-0.078*	-0.102**	-0.118*	-0.036	-0.056
	(0.052)	(0.039)	(0.041)	(0.044)	(0.044)	(0.044)	(0.041)	(0.062)	(0.055)	(0.054)
BDUE	1.224	5.229	8.358	10.739	10.106	11.587	4.620	-3.868	5.011	2.659
	(4.598)	(4.564)	(6.012)	(6.671)	(6.536)	(7.103)	(8.342)	(10.807)	(7.398)	(8.453)
BANKFAIL × BDUE	-3.294	-5.426	-7.850	-8.750	-8.984	-9.633	-4.845	6.260	-4.121	-0.851
	(6.569)	(5.648)	(7.264)	(7.688)	(7.594)	(7.747)	(8.780)	(12.847)	(8.642)	(9.388)
D_{t-1}/A_{t-1}				-0.451***	-0.461***	-0.517***	-0.505**	-0.298*	-0.202	-0.250
				(0.159)	(0.155)	(0.172)	(0.187)	(0.164)	(0.166)	(0.160)
$\log E_{t-1}$					-0.037**	-0.142***	-0.142***	-0.185***	-0.160***	-0.154***
					(0.016)	(0.037)	(0.041)	(0.048)	(0.048)	(0.046)
$\log A_{t-1}$						0.136***	0.128***	0.172***	0.130**	0.132**
						(0.037)	(0.041)	(0.049)	(0.050)	(0.049)
ROA_{t-1}								1.805***	1.128**	0.986**
								(0.431)	(0.445)	(0.405)
ROA_t									2.715***	2.669***
									(0.437)	(0.442)
$\log AGE$										-0.044*
										(0.025)
Observations	1010	1010	1009	1009	1009	1009	1009	823	787	768
R^2	0.002	0.129	0.169	0.179	0.184	0.204	0.254	0.322	0.399	0.395
Fixed Effects	-	S	S, I	S, I	S, I	S, I	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 total dollar amount of bonds that are due in 1928, as a fraction of the average of book assets between 1928 and 1933 (BDUE), interacted with $BANKFAIL_i$, a dummy variable that takes the value of 1 if at least one national bank was suspended in the city in which the firm is located during the 1929-1933 period, and zero otherwise. 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 (ROA_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 2-10), industry (columns 3-6) or industry-region (columns 7-10) fixed effects, where regions are classified according the US Census definition (4 regions) We classify firms into 30 industries following Fama and French (1997).

Table A.7: Robustness: firm funding needs × bank failures – exclude bonds issued prior to 1929

$\Delta \log E_t$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BANKFAIL	-0.030	-0.038	-0.066	-0.061	-0.062	-0.058	-0.078*	-0.086	-0.013	-0.033
	(0.053)	(0.039)	(0.042)	(0.044)	(0.044)	(0.045)	(0.041)	(0.064)	(0.051)	(0.051)
BDUE	0.598	0.575	-0.038	0.647	0.479	0.719	0.802	0.572	0.593	0.438
	(1.376)	(1.121)	(1.106)	(1.112)	(1.122)	(1.014)	(1.184)	(1.358)	(1.471)	(1.665)
BANKFAIL × BDUE	-2.964**	-2.825**	-2.351*	-2.581**	-2.510**	-2.792**	-2.871**	-2.644*	-2.949*	-2.852*
	(1.230)	(1.176)	(1.224)	(1.176)	(1.136)	(1.037)	(1.200)	(1.426)	(1.497)	(1.628)
D_{t-1}/A_{t-1}				-0.388**	-0.394**	-0.451**	-0.451**	-0.257	-0.155	-0.196
				(0.163)	(0.159)	(0.177)	(0.192)	(0.176)	(0.177)	(0.170)
$\log E_{t-1}$					-0.039**	-0.144***	-0.146***	-0.190***	-0.164***	-0.159***
					(0.017)	(0.037)	(0.041)	(0.046)	(0.047)	(0.045)
$\log A_{t-1}$						0.136***	0.131***	0.175***	0.133**	0.135***
						(0.036)	(0.040)	(0.047)	(0.049)	(0.047)
ROA_{t-1}								1.738***	1.072**	0.945**
								(0.425)	(0.442)	(0.401)
ROA_t									2.691***	2.650***
									(0.426)	(0.429)
$\log AGE$										-0.039
										(0.024)
Observations	1010	1010	1009	1009	1009	1009	1009	823	787	768
R^2	0.011	0.137	0.176	0.183	0.189	0.210	0.260	0.327	0.405	0.401
Fixed Effects	-	S	S, I	S, I	S, I	S, I	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 total dollar amount of bonds that are due between 1930-1934 and were issued prior to 1929, as a fraction of the average of book assets between 1928 and 1933 (BDUE), interacted with $BANKFAIL_i$, a dummy variable that takes the value of 1 if at least one national bank was suspended in the city in which the firm is located during the 1929-1933 period, and zero otherwise. 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 (ROA_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 2-10), industry (columns 3-6) or industry-region (columns 7-10) fixed effects, where regions are classified according the US Census definition (4 regions) We classify firms into 30 industries following Fama and French (1997).

Table A.8: Robustness: firm funding needs × bank failures – exclude bonds maturing in 1934

$\Delta \log E_t$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BANKFAIL	-0.025	-0.032	-0.062	-0.056	-0.055	-0.054	-0.072*	-0.080	-0.003	-0.023
	(0.053)	(0.039)	(0.042)	(0.044)	(0.044)	(0.044)	(0.042)	(0.068)	(0.055)	(0.053)
BDUE	0.766	0.930	0.640	1.152	1.153	1.135	1.140	0.987	1.242	0.996
	(0.988)	(0.786)	(0.836)	(0.819)	(0.806)	(0.712)	(0.801)	(0.891)	(0.882)	(0.929)
BANKFAIL × BDUE	-3.289***	-3.180***	-2.938***	-3.077***	-3.179***	-3.270***	-3.305***	-3.066**	-3.523***	-3.280**
	(1.152)	(1.053)	(1.055)	(1.011)	(0.971)	(0.922)	(0.990)	(1.126)	(1.209)	(1.236)
D_{t-1}/A_{t-1}				-0.410**	-0.419**	-0.471**	-0.467**	-0.274	-0.177	-0.218
				(0.164)	(0.161)	(0.177)	(0.194)	(0.174)	(0.176)	(0.169)
$\log E_{t-1}$					-0.039**	-0.145***	-0.146***	-0.190***	-0.164***	-0.159***
					(0.016)	(0.037)	(0.041)	(0.046)	(0.047)	(0.045)
$\log A_{t-1}$						0.137***	0.130***	0.174***	0.133**	0.135***
						(0.036)	(0.039)	(0.047)	(0.048)	(0.047)
ROA_{t-1}								1.723***	1.038**	0.911**
								(0.446)	(0.455)	(0.406)
ROA_t									2.712***	2.671***
									(0.421)	(0.426)
log(AGE										-0.040
										(0.027)
Observations	1010	1010	1009	1009	1009	1009	1009	823	787	768
R^2	0.013	0.138	0.177	0.185	0.191	0.212	0.262	0.329	0.408	0.403
Fixed Effects	-	S	S, I	S, I	S, I	S, I	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 total dollar amount of bonds that are due between 1930-1933, as a fraction of the average of book assets between 1928 and 1933 (BDUE), interacted with $BANKFAIL_i$, a dummy variable that takes the value of 1 if at least one national bank was suspended in the city in which the firm is located during the 1929-1933 period, and zero otherwise. 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 (ROA_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 2-10), industry (columns 3-6) or industry-region (columns 7-10) fixed effects, where regions are classified according the US Census definition (4 regions) We classify firms into 30 industries following Fama and French (1997).

Table A.9: Robustness: firm funding needs × bank failures – exclude firms with zero or missing leverage in 1928

$\Delta \log E_t$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BANKFAIL	-0.034	-0.024	-0.009	-0.008	-0.009	-0.001	-0.007	0.009	0.060	0.031
	(0.067)	(0.052)	(0.070)	(0.070)	(0.070)	(0.067)	(0.078)	(0.118)	(0.096)	(0.094)
BDUE	0.833	1.093	0.739	1.083	0.938	1.266	1.407	1.146	1.180	0.785
	(1.363)	(1.093)	(1.040)	(1.052)	(1.090)	(1.020)	(1.166)	(1.205)	(1.393)	(1.694)
BANKFAIL × BDUE	-2.943**	-2.931**	-2.803**	-2.972**	-2.914**	-3.252**	-3.297**	-3.136*	-3.532**	-3.358*
	(1.263)	(1.193)	(1.316)	(1.283)	(1.266)	(1.189)	(1.347)	(1.585)	(1.674)	(1.819)
D_{t-1}/A_{t-1}				-0.247	-0.265	-0.325	-0.320	-0.165	-0.084	-0.108
				(0.179)	(0.178)	(0.196)	(0.219)	(0.221)	(0.230)	(0.217)
$\log E_{t-1}$					-0.025	-0.135***	-0.123***	-0.151***	-0.136**	-0.136**
					(0.024)	(0.039)	(0.042)	(0.047)	(0.052)	(0.051)
$\log A_{t-1}$						0.143***	0.126***	0.152***	0.123**	0.126**
						(0.038)	(0.039)	(0.047)	(0.050)	(0.049)
ROA_{t-1}								2.210***	1.309**	1.276**
								(0.534)	(0.587)	(0.551)
ROA_t									2.505***	2.492***
									(0.467)	(0.464)
$\log AGE$										-0.016
										(0.021)
Observations	698	698	697	697	697	697	697	576	550	538
R^2	0.014	0.176	0.222	0.225	0.227	0.249	0.302	0.363	0.442	0.439
Fixed Effects	-	S	S, I	S, I	S, I	S, I	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 total dollar amount of bonds that are due between 1930-1934, as a fraction of the average of book assets between 1928 and 1933 (BDUE), interacted with $BANKFAIL_i$, a dummy variable that takes the value of 1 if at least one national bank was suspended in the city in which the firm is located during the 1929-1933 period, and zero otherwise. We exclude firms that have no leverage in 1928. 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 (ROA_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 2-10), industry (columns 3-6) or industry-region (columns 7-10) fixed effects, where regions are classified according the US Census definition (4 regions) We classify firms into 30 industries following Fama and French (1997).