Financial Frictions and Employment during the Great Depression*

Efraim Benmelech† Carola Frydman‡ Dimitris Papanikolaou§

Abstract

We provide new evidence that a disruption in credit supply played a quantitatively significant role in the unprecedented contraction of employment during the Great Depression. To analyze the role of financing frictions in firms' employment decisions, we use a novel, hand-collected dataset of large industrial firms. Our identification strategy exploits preexisting variation in the need to raise external funds at a time when public bond markets essentially froze. Local bank failures inhibited firms' ability to substitute public debt for private debt, which exacerbated financial constraints. We estimate a large and negative causal effect of financing frictions on firm employment. Interpreting the estimated elasticities through the lens of a simple structural model, we find that the lack of access to credit may have accounted for 9% to 30% of the aggregate decline in employment of large firms between 1928 and 1933.

JEL classification:

Keywords: Credit, Financial Constraints, Labor, Unemployment, Great Depression.

*We thank Andrew Ellul, Joseph Ferrie, Francois Gourio (discussant), Jose Liberti, Inessa Liskovich (discussant), Robert Margo, Joel Mokyr, Paige Ouimet (discussant), Marco Pagano, Giorgio Primiceri, Fabiano Schivardi (discussant), Amit Seru, Andrei Shleifer, Jeremy Stein, Toni Whited (discussant) and seminar participants at the CSEF-EIEF-SITE Conference on Finance and Labor, Federal Reserve Bank of New York, Hebrew University, NBER DAE meeting, NBER Summer Institute (Corporate Finance, Monetary Economics), Kellogg School of Management, Mitsui Finance Symposium on Labor and Finance, Northwestern's Economic History workshop, Princeton, 2016 Tsinghua Finance Workshop, University of Munich, Wharton, and Yale for very helpful comments. Eileen Driscoll, Jenna Fleischer, Sam Houskeeper, Ari Kaissar, Daniel Trubnick, and Yupeng Wang provided excellent research assistance.

†Kellogg School of Management and NBER, e-benmelech@kellogg.northwestern.edu

‡Kellogg School of Management and NBER, c-frydman@kellogg.northwestern.edu

§Kellogg School of Management and NBER, d-papanikolaou@kellogg.northwestern.edu

The Great Depression was the most severe and prolonged economic downturn of the modern industrialized world. From 1929 to 1933, real output in the United States contracted by 26%, and the unemployment rate increased from 3.2% to 25%, reaching its highest recorded level in American history (Margo, 1993). Despite the severity of the Depression and its undoubted influence on macroeconomic thinking, the causes of the rise in unemployment during the 1930s are still not well understood and remain important today, almost 90 years after the world entered its worst economic crisis. This paper provides new evidence that financial frictions were responsible for much of the decline in employment of large American firms during this period.

In a seminal paper, Bernanke (1983) argues that an increase in the real costs of intermediation during the Great Depression reduced the ability of some borrowers to obtain credit, which in turn contracted aggregate demand and exacerbated the downturn. Although this view has often been used to explain the protracted contraction in output, financial imperfections also offer a potential explanation for the staggering rise in unemployment during the Depression. When there is a lag between the payments to labor and the realization of revenues, firms need to finance their labor activity throughout the production process (see, e.g., Greenwald and Stiglitz, 1988). Moreover, unlike physical capital, labor cannot serve as collateral, which makes it harder to finance. Thus, any difficulties in obtaining external finance may have severe effects on firms' employment decisions. Yet the lack of firm-level data for the 1930s has posed an obstacle for understanding the effect of finance on employment during the Great Depression. In this paper we aim to fill this void.

Using newly collected data, we estimate the effects of financial frictions on the contraction in employment of large industrial firms during the Great Depression. Our identification strategy uses the preexisting variation in the value of long-term debt that became due during the crisis. We find that firms more burdened by maturing debts cut their employment levels more. These effects were particularly severe for firms located in areas where local banks were in distress and that could therefore not easily substitute public debt for bank financing. Our analysis suggests that financial frictions can explain between 9% and 30% of the overall drop in employment in our sample from 1928 to 1933.

The current understanding of unemployment during the 1930s is heavily based on either aggregate or establishment-level data (see Margo, 1993, for a review). Establishment-level data contain no financial information, however, and cannot therefore adequately measure the needs for external finance. Our analysis, by contrast, is based on a novel, hand-collected dataset from the *Moody's Manual of Investments*, which includes approximately one thousand of the largest industrial firms in the economy, a group of businesses that have received limited

attention in quantitative research on the Depression. Our data reveal that large enterprises actually suffered greatly during the crisis: the average firm in the sample experienced a 23% decline in employment from 1928, the year before the onset of the crisis, to 1933, when unemployment peaked. The profitability of large firms also collapsed over this period.[1] By using firm-level data we can link information on employment to the firms' operating characteristics and financing needs. Most important, we collect detailed information on the value and maturity structure of the firms' outstanding bonds, allowing us to measure the variation in the needs for external finance across firms. The fact that the *Moody's* manuals first began to report lists of maturing bonds for industrial firms in 1931 is strong historical indication that having debt mature during the recession was perceived to have a significant impact on firm health, and it motivates our identification strategy.

Similar to Almeida, Campello, Laranjeira, and Weisbenner (2011), we primarily identify the effect of financing frictions on firm employment changes by exploiting variation across firms in the maturity of corporate bonds, the primary source of debt financing of large firms at that time (Jacoby and Saulnier, 1947). The economic downturn led to a collapse of the public bond markets in the early 1930s (Hickman, 1960). Firms that happened to have bonds that matured during this time could not easily refinance them, and were therefore more likely to be constrained in allocating cash between servicing their debt and paying their workers. We find that a firm in the 90$^{\text{th}}$ percentile of the value of maturing debt (scaled by assets) contracted its employment between 1928 and 1933 by about 4–5% more than the median firm in the sample, which had no bonds maturing. Since our specifications control for leverage, among other observable characteristics, the estimated effects are not driven by differences in total indebtedness across firms. Moreover, the bonds that matured during the crisis were primarily issued well in advance of the onset of the Depression. Our findings are therefore unlikely to be influenced by changes in the firms' investment opportunities, and in their demand for external finance, in response to the negative aggregate economic shock.

Our analysis thus far exploits an aggregate shock to the supply of credit—the collapse of the public debt market. It is possible, however, that firms exposed to this shock could potentially obtain other types of credit during the crisis, such as bank debt. To obtain additional variation in credit supply shocks across firms, we also exploit spatial variation in bank distress by interacting the variation in the firms' maturing debt with the conditions

[1]These facts are consisted with the evidence reported in Graham, Hazarika, and Narasimhan (2011), who also study the outcomes of large industrial firms using data collected from the *Moody's Manuals*. (Graham et al., 2011) show that firms' pre-crisis leverage ratios were positively associated with the likelihood of becoming distressed during the Great Depression. Our study differs from Graham et al. (2011) in that we focus on a different outcome—employment—and use a different identification strategy, based on the preexisting variation in the need to refinance maturing debt.

of the local banking system where these firms operated. From 1929 to 1933, thousands of commercial banks experienced financial distress and suspended operations. These bank "failures" likely resulted in a contraction of credit supply for their borrowers. We assume that firms found it easier to borrow from banks in their area, and we measure a reduction in bank credit with an indicator for whether at least one national bank suspended operations in the county where each firm operated. We do not find strong evidence that disruptions in the local banking systems had a *direct* effect on the employment decisions for the firms in our sample, especially once we control for firm profitability.[2]

We find that firms with maturing debt that were located in areas where local banks failed contracted employment by more (about 11% to 17%) relative to those firms with similar levels of maturing debt that operated in areas with no bank disruptions. These estimated effects suggest that the impact of financial frictions on employment during the Great Depression was sizable, especially for those firms with maturing debt that could not easily substitute bond financing for bank loans.

Throughout our analysis, our main identification assumption is that firms with different levels of maturing debt in the early 1930s were differentially affected by shocks to credit supply but had similar exposure to other shocks that might affect the demand for credit. However, just like the collapse of the bond market, failures of local banks may reflect not only contractions in the supply of credit to local firms, but also other economic shocks that simultaneously affect bank health and firm outcomes. Hence, our strategy could be invalid if firms with different levels of maturing bonds were somehow differentially exposed to these local shocks. Additional tests, however, suggest that this is unlikely to be the case. First, we show that our findings are robust to restricting the sample to only those firms operating in the tradable sectors, which are less likely to be affected by local demand shocks. Our results are also unchanged when we include controls for the change in retail sales–a measure of economic activity–in the firms' area of operations during the crisis. Finally, we perform a placebo analysis and utilize alternative measures of maturing debt designed to ameliorate

[2]In contemporaneous work, Lee and Mezzanotti (2015) find a contraction in the city-industry employment levels of manufacturing establishments in response to local bank failures. Ziebarth (2013) finds that tight monetary policy, which contributed to the intensity of bank failures, led to lower employment at the county level but had no differential effects at the establishment level. These studies use establishment data obtained from the Census of Manufactures, and therefore lack direct information on firms' (or the establishments') financial health. By contrast, our data include a full set of firm financial variables and, most important, direct measures for the degree to which firms needed to refinance maturing debt. When examining the direct effect of bank failures in Appendix Table A.7, we find a negative but weak relation between national bank suspensions and employment changes. Importantly, the estimated effects are not statistically significant when we control for the firms' profitability. Our different findings on the direct effect of local bank failures may be driven by the possibility that the large industrial firms in our sample were less dependent on bank credit than the typical (much smaller) establishment in the economy. It is also possible that previous studies attribute to bank distress differences in profitability across establishments.

concerns that our results may be driven by unobserved firm characteristics correlated with the value of bonds becoming due and with employment outcomes.

Our difference-in-differences strategy provides an estimate of the elasticity of firm employment to a plausibly exogenous financing shock. We use this estimate to assess the importance of financial frictions for the aggregate contraction in employment. First, we calculate the counterfactual aggregate employment level in our sample under the assumption that the "treated" firms did not experience financial frictions. We find that employment would have been 0.9 to 1.4 percentage points higher in this case. This direct treatment effect accounts for between 9% and 15% of the overall drop in employment in our sample, but it is likely a conservative estimate because, among other reasons, it is based on firms that survived between 1928 and 1933. It is possible that firms that no longer appear in 1933 would have liked to access external funds to finance operations but could not do so because of the high cost of external finance during the Depression. To gauge the magnitude of the effect of financial frictions more broadly, we estimate a simple structural model that relates the cost of financial intermediation to aggregate employment outcomes. The estimated elasticity of employment to maturing debt allows us to calibrate the model parameter that captures the cost of external finance. We then use the model to compute the counterfactual level of employment for each firm in the sample if external finance was costless. We find that the aggregate level of employment would have been between 1.6 and 2.8 percentage points higher in the absence of financing frictions, which accounts for about 18% to 30% of the decline in employment in our sample.

In sum, we provide direct, firm-level evidence that a disruption in credit supply played a quantitatively significant role in the contraction in employment in the early 1930s. Our work thus contributes to the debate on the role that the financial system played in instigating the Great Depression.[3] Our evidence is consistent with Bernanke (1983), who argues that the difficulties banks experienced likely contributed to the severity and persistence of the recession by increasing the real cost of intermediation. Recent work has revisited this question empirically with the aim of providing causal evidence for the effects of bank failures on a variety of outcomes, including income growth (Calomiris and Mason, 2003), industrial output (Mladjan, 2016), business revenues (Ziebarth, 2013), and employment (Ziebarth, 2013; Lee

[3]Economists continue to debate on the relative importance of several (not mutually exclusive) forces, with some favoring aggregate-demand explanations (e.g., Temin, 1976) and others emphasizing the role of monetary forces (e.g., Friedman and Schwartz, 1963; Richardson and Troost, 2009). Alternative prominent explanations include, among others, the breakdown of international financial relations (Eichengreen, 1992), the contraction in consumer spending following the collapse in the stock market (Romer, 1993), and shocks to productivity (Cole and Ohanian, 2007).

and Mezzanotti, 2015).[4] These studies obtain identification primarily from variation in the health of banks across space, but they lack information on the firms' financial conditions. They cannot therefore measure directly the firms' need to access external finance, nor can they control for firm characteristics that may be correlated with the severity of local bank distress and with firm outcomes. By contrast, our data allow us to more convincingly isolate the effects of a contraction in the supply of credit by instead constructing a firm-level measure of the preexisting needs for external finance that is unlikely to be driven by changes in the firms' investment opportunities during the crisis. In this manner, our paper is closely related to the modern literature in corporate finance that studies the effect of financial constraints on firms' employment decisions.[5] We take a further step by combining the estimated elasticity of employment to maturing debt with a structural model, which allows us to quantify the effects of financial constraints on the aggregate contraction in employment in our sample.

Our work provides a set of novel stylized facts on the experiences of large firms during the Depression with important implications for macroeconomic interpretations of the crisis. The contraction in credit intermediation is considered to have been especially harmful for households and small firms; by contrast, large firms are typically thought to have been relatively unconstrained (Bernanke, 1983).[6] Under this view, the credit squeeze likely exacerbated the downturn by contracting aggregate demand—otherwise, the unconstrained large firms would have filled in any reductions in production experienced by the small constrained businesses, and the impact of the crisis on aggregate output would have been minimal. By contrast, we show that financial frictions had large, negative effects even among the largest firms in the economy. Our findings therefore suggest that a contraction in aggregate supply may also have played an important role in the severity and long duration of the Great Depression.

The Great Recession of 2008–2009 has renewed the interest of academics and policy makers in the Great Depression, yet the magnitudes of the economic shocks were very different.

[4] An alternative, but not mutually exclusive, channel by which disruptions in the banking sector may have affected economic activity is through a contraction in the money supply, as emphasized by Friedman and Schwartz (1963). Richardson and Troost (2009) provide convincing causal evidence for the importance of monetary policy by contrasting the level of commercial activity in areas of Mississippi exposed to different Federal Reserve policy regimes.

[5] Studies in this area include, among others, Almeida et al. (2011); Benmelech, Bergman, and Seru (2011); Chodorow-Reich (2014); Duygan-Bump, Levkov, and Montoriol-Garriga (2015); Michaels and Whited (2014); Pagano and Pica (2012).

[6] Bernanke's argument is based on the evidence of Lutz (1945), who finds that the cash balances of 45 large manufacturing firms remained relatively unchanged during the early 1930s, while those of small and medium firms exhibited a marked decline. Hunter (1982) validates this finding using aggregate data for all tax filers. These studies, however, consider neither the financing needs of large firms nor the heterogeneity of experiences among these firms. Our results suggest that financial frictions had important consequences, even after taking into account the firms' holdings of liquid assets.

Figure 1 contrasts the evolution of real GNP and unemployment rates for these two periods. Panel A shows that the economic contraction was an order of magnitude larger in the 1930s; output fell by 26% in the 1929–1933 period, whereas it contracted by only 3.3% from 2007 to 2009. As displayed in Panel B, the U.S. economy entered both crises with relatively low unemployment rates. During the Great Recession, the unemployment rate never surpassed 10%, and it almost regained its pre-crisis level after "only" eight years. By contrast, 25% of workers were out of a job at the peak of the Depression, and the unemployment rate remained above 10% for more than a decade. That the real effects of the financial crisis were much more severe in the 1930s is perhaps all the more surprising given that the financial sector doubled in importance (as a fraction of total output) from 1929 to 2007 (Philippon, 2015). Though it is certainly difficult to accurately contrast these two events, a simple comparison of our estimated elasticity of employment to maturing debt to a similar estimate calculated by Benmelech et al. (2011) for the 2008–2009 crisis suggests that the effect of financial frictions on unemployment may have been about two to five times larger in the Great Depression than in the Great Recession. In the 2000s, policy makers had the hindsight of history and labored to avoid past mistakes, expanding the money supply and arresting banking panics (see, e.g., Eichengreen, 2014). The contrast in the effects of financial frictions during the Great Depression and the Great Recession suggests that regulatory frameworks and policy decisions may have an important role in ameliorating the impact of financial shocks on the real economy.

The rest of the paper is organized as follows. Section 1 discusses the financial frictions we use as part of our identification strategy. Section 2 presents the data sources and the variables used in the analysis. Section 3 explores the effects of financial constraints on employment. Section 4 presents the analysis of the aggregate impact of our results. Section 5 concludes.

1 Identifying Financial Frictions in the 1930s

Our goal is to present convincing evidence that financial frictions had an important effect on firm employment levels during the Great Depression. In this section, we discuss the historical and economic underpinnings that provide a rationale for our empirical strategy.

1.1 Maturing Long-term Debt

Credible identification of the role of financial frictions requires a shock to the firms' access to external finance, and therefore to their cost of credit intermediation, that is unrelated to their investment opportunities. We follow Almeida et al. (2011), who exploit the variation

across firms in preexisting levels of long-term debt maturing during the 2008–2009 credit crisis. Since there is no information available on the maturity structure of bank loans for our sample period, we adapt their methodology and focus exclusively on corporate bonds. Thus, we measure the "financial shock" experienced by each firm using the value of bonds becoming due from 1930 to 1934 as a fraction of the firm's assets. Our empirical strategy relates this continuous "treatment" measure to the firms' change in employment between 1928 and 1933.

Our focus on corporate bonds is pertinent and helpful for identification. First, bonds were the primary source of debt financing for the large firms in our sample. Second, much like equity markets, public debt markets essentially shut down during the Depression. Figure 2 presents the total value of new bond offerings by industrial firms from 1920 to 1940. The issuance of bonds declined somewhat at the onset of the crisis, but it collapsed almost completely from 1931 to 1934, when the value of new offerings accounted for only 10% to 30% of its pre-crisis level in 1928. Firms that happened to have bonds maturing in this period struggled to refinance those debts and likely faced (exogenously) higher costs of intermediation.

The main identification assumption in our empirical strategy is therefore that the value of long-term bonds maturing from 1930 to 1934 was exogenous to any changes in the firms' investment opportunities that may have affected their employment decisions during the crisis. In other words, by comparing firms with different levels of maturing debt, after controlling for leverage and other characteristics, we hope to address differences in demand shocks across firms, which were in all likelihood uncorrelated with the timing of debt maturing. Since corporate bonds typically had long maturities, those debts becoming due during this period were primarily issued well before the stock market crash on October 29, 1929. Yet a potential concern is that firms with maturing long-term debt may have anticipated the recession, optimizing both their leverage and their employment levels accordingly before the crisis. If this were the case, our findings could be driven by unobserved differences in firm quality that may be correlated with the level of maturing bonds and changes in employment. But there is plenty of evidence to suggest that the Great Depression was largely unexpected. The earliest macroeconomic signs of impending troubles did not occur until the summer of 1929, when the Federal Reserve's index of industrial production began to decline (Atack and Passell, 1994, pp. 587-588). Moreover, credit spreads of corporate bonds remained largely unchanged until then (Calomiris, 1993, p. 69). Although some may have expected an economic slowdown or even a financial crisis, there is perhaps no greater consensus among economic historians of the Great Depression than the exact timing of the market crash, the collapse of credit and

bond markets, and the unprecedented severity of the protracted recession that ensued could not have been accurately anticipated.[7]

1.2 Spatial Variation in the Size of the Credit Supply Shock

Our strategy based on variation in maturing bonds helps us to address concerns of differences in economic shocks across firms, but it only allows us to identify credit supply shocks from a single aggregate shock–the freeze up of bond markets–that affects all firms at the same time. To obtain additional variation in the size of the credit shock across firms, we also utilize differences in bank health across space. In particular, as we discussed above, it was exceedingly difficult for firms to issue public debt during the crisis. Issuing new equity was also not an alternative source of external finance during this period. First, equity markets "dried up" following the stock market crash of 1929, even before the freeze-up of public debt markets (see, e.g., Benmelech and Bergman, 2016). Second, less than 20% of the firms in our sample were listed in the NYSE, suggesting that equity issuance was not their main source of new external finance. An alternative source of external finance would have been to obtain funds from a bank, even though bank loans were not the most common source of credit for the firms in our sample in good times.

Yet, local banks were not always able to supply credit. From 1929 to 1933, the American banking system experienced a major collapse; more than 40% of depository institutions suspended operations (see, among others, Alston, Grove, and Wheelock (1994), Wheelock (1995), and Richardson (2007)).[8] Much of the work on the Great Depression has used the variation in these bank failures to analyze their effects on real economic activity. Indeed, in modern economies, as well as in the past, firms typically establish long-lasting relation with financial intermediaries, perhaps to reduce frictions arising from asymmetries of information. When a financial intermediary fails, the bank's nonfinancial clients typically suffer (Khwaja and Mian, 2008; Schnabl, 2012; Chodorow-Reich, 2014; Frydman, Hilt, and Zhou, 2015). For our purposes, we can exploit geographic differences in bank failures to obtain additional spatial variation in the size of the credit supply shock.

In sum, we conjecture that financial frictions were particularly salient for those firms that had high levels of maturing debt *and* that were located in areas that suffered disruptions to

[7] See, among others, Atack and Passell (1994, p. 597), (Temin, 2000, pp. 304, 311), and Hughes and Cain (2011, pp. 468-469). Furthermore, Klug, Landon-Lane, and White (2005) use unique survey data on the forecasts of railroad shippers to show that American businesses were surprised by the depth and duration of the Great Depression.

[8] We follow the economic history literature and use the terms "suspensions" and "failures" interchangeably, although many banks that suspended operations did not ultimately fail. Richardson (2007) provides the definition of a bank suspension employed by the Federal Reserve.

their banking systems. Thus, we would expect these firms to contract employment by more than firms with similar levels of maturing bonds located in areas where the banking system was less impaired. Our analysis hinges on a few important assumptions. First, as we discuss in Section 1.1, we continue to assume that the level of maturing debt is uncorrelated with the firm's investment opportunities. In addition, we assume that firms likely found it easier to borrow from banks located in their area, perhaps due to asymmetric information problems (Agarwal and Hauswald (2010), Petersen and Rajan (2002)), and focus on bank failures in the county in which the firm operated to obtain additional variation in credit supply shocks. We also argue that there was at least some degree of substitutability between bank lending and public debt as sources of financing. Indeed, Rauh and Sufi (2010) and Becker and Ivashina (2014) suggest that, at least in recent decades, private and public debt have been partial substitutes.

To ensure that the variation in the size of the credit supply shock is relevant for the large industrial concerns in our sample, it is important to focus on those financial institutions that may have been likely to provide loans of an appropriate size. Unfortunately, there is no available information to identify conclusively which types of institutions were more likely to lend to large manufacturing businesses. But the two main types of commercial banks, state and national, operated under different regulatory constraints, and consequently differed substantially in their characteristics.[9] Most important, national banks were typically larger than state banks, and this pattern is evidenced in our data. For example, the average national bank in the counties in our sample, weighted by the number of banks in each area, had $43.9 million in deposits in 1928, whereas the average state bank in these counties had only $21.7 million. National banks were thus better positioned to lend to the firms in our sample, which were among the largest industrial companies in the economy, and likely had credit demands that could not be easily fulfilled by small financial institutions. We therefore base our analysis on national bank failures.

[9]State-chartered banks were primarily subject to state regulation and supervision, whereas the federally chartered national banks operated under uniform federal banking regulation. Whereas national banks provided detailed financial information to the Comptroller of the Currency, no similar information is consistently available for state-chartered banks. Though crude, the available evidence on the location and loan composition suggests that national banks were likely more salient for the firms in our sample. National banks were subject to greater lending restrictions, particularly on real estate loans. State banks were therefore more likely to service agricultural borrowers, and they were disproportionately located in agricultural states. By contrast, national banks were more likely to be situated in manufacturing areas. Moreover, White (1984) shows that state banks were more likely to hold commercial bonds, whereas national banks focused their portfolios on U.S. government bonds, which performed better during the crisis. Any declines in the price of the bonds issued by the firms in our sample may have disproportionately hurt state banks. Thus, evidence based on the failures of these institutions may also be subject to reverse causality concerns.

2 Data

We begin by describing the main features of our novel dataset.

2.1 Sources

We hand-collect the majority of the data from primary sources. In this section we briefly describe these sources and define the main variables in our analysis; we provide additional details in the Appendix. We construct a panel dataset containing firm-level information on accounting variables and employment for 1928 and 1933 for all American industrial firms listed in the 1929 and 1934 volumes of the *Moody's Manual of Investments* for Industrial Securities. We select these two specific years to contrast the change in employment from the peak in economic activity in 1928, before the outset of the crisis, to the trough of the Depression in 1933.[10] For each firm, we obtain 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 ROA). Each manual year contains about five thousand firms, but only a fraction of them (about 39% in 1928 and 53% in 1933) report employment figures. To match firms across the two years, we use information on the firm's name, year of incorporation, and, when necessary, description of activities. We restrict the analysis to a balanced panel of 1,026 firms that report non-missing information on employment and assets in both years. The selection of surviving firms with non-missing information will likely lead us to underestimate the effects of financial frictions (see the Appendix for details).

Our sample is composed primarily of firms operating in manufacturing and retail. The Great Depression, however, did not affect all industries equally. Our empirical specifications therefore control for industry effects. In order to use an industry definition that is meaningful but that nevertheless contains a sizable number of firms within each sector, we use the 30 industry classification of Fama and French (1997).

As with any novel dataset, the validity of the data in an important concern. In Appendix Section A.4, we show that the geographic and sectoral distribution of employment changes in our data replicate well-established patterns for this period from alternative sources. An additional issue is that our sample consists only of about one thousand firms, albeit some of the largest in the economy. To reassure readers of the external validity of our results, in the

[10] According to the NBER's Business Cycle Reference Dates, the peak of the cycle was in August 1929 and the trough was in March 1933. The unemployment rate reached its highest level in 1932 or in 1933, depending on whether persons with "work-relief" jobs are counted as employed or unemployed, respectively (Margo, 1993).

Appendix we show that the firms in our sample accounted for a substantial fraction of the employment in the American economy.

We identify credit frictions by exploiting the preexisting variation in the value of corporate bonds that became due during the crisis. Starting in 1931, the *Moody's* manuals provide a list of all bonds maturing in the period following the manual's publication. The prominent display of this information suggests that having debt maturing during the crisis was corporate hardship, and therefore valuable information for potential investors. From these lists, we obtain the bond name, amount due, and maturing date for all bonds that were due for each sample firm from mid-June 1931 through December 1934. To construct similar information from January 1930 to early June 1931, when the lists of maturing bonds were not provided, we use the detailed descriptions of all bonds outstanding for each firm from the 1930 manual. We also use these detailed descriptions from the manuals for each year to obtain the date of issuance for all bonds maturing in the 1930–34 period. Since the freeze-up of bond markets was particularly severe in 1934, we include the bonds that matured in this year in our baseline definition of the treatment variable. This treatment allows us to account for any precautionary reductions in employment that firms may have done in 1933, in anticipation of experiencing difficulties in funding their maturing debts in the following year. In robustness checks, we show that our results are largely unchanged when we exclude those bonds maturing in 1934 from the analysis.

Last, we obtain information on national bank suspensions from 1929 to 1933 from the Federal Deposit Insurance Corporation (FDIC) Data on Banks in the United States.[11] The FDIC data allow us to measure the bank suspensions 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, which typically identifies the main location in which the firm operated. We then match the firm's location to its corresponding county based on the city-county-state definitions from the 1930 Population Census. This procedure allows us to link the financial information of firms to the financial conditions of the local banking system.

2.2 Summary Statistics

Table 1 presents summary statistics for the main variables in our analysis. We focus on a sample of 1,026 firms with non-missing employment and balance sheet information in both 1928 and 1933; information on some measures, such as profitability and firm age, is missing for some firms. By construction, our data are based on firms that survived at least until 1933.

[11] These data were reported in the *Federal Reserve Bulletin* in 1937 and are available at ICPSR_00007.

To minimize the impact of outliers in our analysis, we winsorize all observations at the 2% and 98% level; using a 1%-99% threshold has no material impact on the analysis.

The *Moody's* manuals were designed for the use of investors in stocks and bonds, and therefore typically provided information for those firms that had listed securities—all "corporate enterprises of importance" (*Moody's Manual of Investments*, 1929, p. v). Our sample is therefore composed mostly of large, established firms. As shown in Table 1, the average firm was 18 years old in 1928, and about 75% of the firms in the sample were incorporated before 1923. Moreover, the median firm in 1928 employed approximately 850 workers, though the average firm had instead 1,840 employees. To address the sizable skewness of the data, we use the log number of employees in our analysis.

The existing consensus is that large firms suffered disproportionately less than smaller firms during the Depression (Bernanke, 1983). However, large firms did not emerge from the crisis unscathed. Table 1 shows that the average firm in our sample experienced a 0.23 log-point reduction in employment between 1928 and 1933. The contraction in employment was quite heterogeneous across firms; the standard deviation of employment changes is 0.60 log points. When we aggregate across firms, we find that the total reduction in employment in our sample was sizable, about -0.095 log points, suggesting that larger firms reduced employment by a proportionally smaller amount than smaller firms.

Another indication that large firms suffered during the Depression is the decline in profitability evidenced in our sample: the average ROA declined from 9% in 1928 to 1% in 1933. Given that the cross-sectional standard deviation of profitability was merely 7% in either 1928 or 1933, this suggests that the collapse in profitability was severe. In fact, 41% of the firms in our sample experienced negative profits in 1933, but fewer than 7% had losses before to the onset of the crisis. Since profitable firms may have been less financially constrained, we control for profitability.

The average (book) leverage ratio among the sample firms was 12.8% in 1928, although there was substantial heterogeneity (the standard deviation was 14.2%). To be sure, this level is small compared to the book leverage ratios exhibited by publicly traded American firms today. However, it is consistent with the evidence reported in Graham, Leary, and Roberts (2015) for our time period, which is also based on the *Moody's* manuals, and with aggregate evidence for corporations in relevant sectors filing tax returns.[12] Moreover, a sizable fraction of firms had no debt outstanding in 1928. In our empirical analysis, we perform several

[12]For example, the ratio of total debt—measured by the value of notes, accounts payable, bonded debt, and mortgages—to total assets for all corporations in mining, manufacturing, construction, trade, and services reporting non-negative net income (as most of our firms did) in 1928 was 19.9% (Statistics of Income for 1928, 1930: Table 19). This statistic is 15.5% for the firms in our sample in 1928.

robustness checks to address concerns related to the low leverage ratios. Last, it is important to note that public debt was salient for our sample firms: corporate bonds accounted for about 60% of the debt outstanding for the average firm in 1928.

Our identification relies on the shock imposed by long-term bonds that become due during the crisis. We construct this measure, which we refer to as *BondsDue*, by the dollar amount of bonds due from 1930 to 1934 as a fraction of the mean value of the firm's assets in 1928 and 1933. Table 1 presents summary statistics for this variable. Although most firms did not have bonds mature in such a short time span, this measure was positive for 148 firms and there was substantial variation in the amount that matured across firms. Conditional on having bonds that become due during the 1930-1934 period, the average firm had to refinance debt that was about 7% of its assets, and the cross-sectional standard deviation around this number was 6%. The level of the financial constraints imposed by debt becoming due was likely more severe for higher levels of that ratio. Thus, we use the *BondsDue* variable primarily as a continuous treatment.

The last three rows of Table 1 report summary statistics that describe the conditions of the banking system in the areas in which our firms operated. The failure of national banks from 1929 to 1933 was fairly widespread. Though there was considerable variation in the number of suspended banks across counties, these differences partly reflect variation in the number of national banks that existed in each area. Yet the average firm in the sample was located in a county where 22% of the national banks failed from 1929 to 1933. To address differences in bank size, we also calculate the total value of deposits of suspended national banks for the 1929-1933 period as a fraction of the value of deposits in the banks that operated in the county in 1928, which is essentially the deposit-weighted measure of the fraction of banks that suspended. The mean of this deposit-weighted measure is 16%, a bit lower than the unweighted measure, reflecting that smaller national banks were more likely to fail. But the dispersion in the deposit-weighted measure of bank failures is more than twice its average value, indicating that even some of the largest banks suspended in some areas. In our baseline specifications we simply compare firms located in counties in which at least one national bank suspended to those firms located in places in which no such institution failed, since this already signals an important disruption in the firms' local banking systems. However, our conclusions are robust to using instead a continuous treatment based on the number or the size of the national banks that suspended.

2.3 Employment and Firm Characteristics

A unique feature of our data is that we observe detailed information on employment and firm financial characteristics. We are thus able to present new facts on the correlation between firm employment changes and their financial leverage during the Great Depression. To do so, we estimate variants of the following regression:

$$\log(E_{i,1933}) - \log(E_{i,1928}) = \alpha + \beta\, Leverage_{i,1928} + \lambda\, \mathbf{X}_{i,1928} + \gamma\, \mathbf{k}_i + \psi\, \mathbf{s}_i + \epsilon_i, \qquad (1)$$

where the dependent variable is the log difference in the number of employees E in firm i between 1928 and 1933, *Leverage* is the firm's debt to assets ratio in 1928, and $\mathbf{X}_i$ includes controls such as the logarithm of employment in 1928, firm size (measured by the logarithm of total assets in 1928), profitability in 1928, and the logarithm of firm age. Since we are interested in isolating the correlation of these characteristics holding factors such as the firm's location constant, we include state s fixed effects. We include either industry k or industry-region fixed effects because the crisis did not affect all industries equally and because industry-specific shocks may have varied across areas. All regressions are estimated with heteroscedasticity-robust standard errors clustered by industry.

Table 2 presents the results. We find a negative correlation between the employment change between 1928 and 1933 and the level of the firm's leverage in 1928 that is statistically significant in most specifications. Focusing on specifications that control for industry or state fixed effects, the coefficients in Columns (2) through (5) imply that a firm in the 90$^{\text{th}}$ percentile of leverage in 1928 experienced a decline in employment from 1928 to 1933 of about 0.12 to 0.14 log points larger than the change in employment of a firm with median leverage. The magnitude of this association becomes a bit smaller when we control for the firm's profitability (in Column (7)), and for firm age (in Column (8)).

That high leverage levels likely had negative consequences during the Great Depression is further suggested by Graham et al. (2011), who show that the leverage ratio was positively associated with financial distress among publicly traded industrial firms. Our study documents a sizable effect of financial frictions on the employment of *surviving* firms. To the extent that financial frictions contributed to the failure of industrial enterprises, our study may underestimate the overall impact of frictions on the aggregate contraction in employment. More broadly, the documented relationship between leverage and employment changes suggests that entering a crisis with high levels of debt may constrain a firm's ability to grow or preserve its workforce during adverse economic conditions, and it is consistent with modern

evidence based on the large firms included in Compustat (see, e.g., Sharpe, 1994; Calomiris, Orphanides, and Sharpe, 1994; Benmelech et al., 2011).

Table 2 also reveals other interesting patterns in the data. Firms that entered the recession with a larger number of employees relative to similarly sized peers in the same industry and region had larger declines in employment levels. For example, firms in the 90th percentile of employment in 1928 reduced employment between 0.06 and 0.3 log points more than the median firm. By contrast, larger firms (as measured by total assets) did not reduce their employment as much as smaller firms. Firms in the 90th percentile of book assets in 1928 contracted employment between 0.24 and 0.32 log points *less* than the median-sized firm. Our data therefore corroborate the perception that large firms suffered less during the Great Depression, but outcomes still varied significantly even among some of the largest enterprises in the economy. The fact that the coefficients on log employment and log assets are similar in terms of magnitude (and have opposite signs) implies that these patterns can be summarized by the employment-to-assets ratio, and suggests that firms with excess labor (relative to their size) may have shed more employees during the crisis. The estimated effects in Columns (7) and (8) also show that firms that entered the recession with higher profitability reduced their labor force by relatively less, compared to otherwise similar firms. Last, in Column (8) we find no statistical differences in employment changes among young and old firms.

Although the results presented in Table 2 suggest that profitability and leverage potentially affected firm-level employment during the Great Depression, these variables are endogenous. These associations thus cannot be interpreted as evidence of a causal effect of financing on employment decisions. Next, we present an identification strategy to more credibly estimate the effect of financial frictions on firm employment.

3 The Effect of Financial Constraints on Employment

Here, we examine the effect of financial constraints on employment decisions.

3.1 Maturing Long-term Debt

We start by exploiting the variation in preexisting amounts of "maturing bonds" across the firms in the sample. Since these bonds were primarily issued before the crisis, their amounts becoming due from 1930 to 1934 are likely exogenous to market conditions and firms' investment opportunities during this period. We conjecture that firms with greater refinancing needs (due to higher levels of bonds maturing relative to their assets) would have experienced difficulties in borrowing to pay financial liabilities and wages, and would have

had to reduce their labor force by more than those firms not facing the need to refinance maturing long-term debt.

Comparison across Treatment and Control Groups

In our regression analysis, we consider a continuous treatment effect, under the assumption that those firms that had a higher value of bonds maturing during the crisis relative to their assets experienced a worse shock to financing frictions. But it is possible that firms with more bonds becoming due were different from other firms in ways that may confound our analysis. Thus, we start by presenting simple comparisons of observable characteristics for 'treated' firms—defined as those that had any positive level of bonds maturing from 1930 to 1934—and for "control" firms, which had no bonds becoming due in this period.

Panel A of Table 3 presents differences in means and medians for these two groups of firms. We find no statistically significant differences in employment levels or firm size before the crisis. However, treated firms were less profitable, had higher leverage, and were a bit older. We would expect firms with higher leverage to be also more likely to have bonds due in any given year. In Panel B we present similar comparisons but restrict the sample to those firms that had some debt outstanding in 1928. The two groups of firms are more balanced in this case. Although treated firms continue to have higher leverage ratios, the absolute differences with those firms with non-zero leverage in 1928 that had no bonds due from 1930 to 1934 are much smaller. We therefore include these characteristics in our regression analysis, and perform various robustness checks to address differences in initial indebtedness levels across firms.

Estimation Strategy

Similar to equation (1), our specification to estimate the effects of "maturing debt" on employment is as follows:

$$\log(E_{i,1933}) - \log(E_{i,1928}) = \alpha + \beta\, BondsDue_{i,1930-1934} + \lambda\, \mathbf{X}_{i,1928} + \gamma\, \mathbf{k}_i + \psi\, \mathbf{s}_i + \epsilon_i, \qquad (2)$$

where the continuous treatment variable $BondsDue$ is measured by the total value of corporate bonds that become due from 1930 to 1934, as a fraction of the firm's average level of assets between 1928 and 1933. (The results are quantitatively similar when we instead scale the value of maturing debt by the firm's book assets in 1928 or in 1933.) Given the documented differences in characteristics across treated and control firms, we include a rich set of controls

and fixed effects to address concerns about selection and omitted variables, similar to those used in (1) above.

Table 4 presents the results. Column (1) shows that the univariate relation between *BondsDue* and the change in the number of employees is negative and statistically significant. This effect is not driven by state-specific characteristics or shocks (Column (2)) or by differences across sectors (Column (3)). As shown in Table 3, highly levered firms were also more likely to have bonds maturing during the crisis. In Column (4), we control for the firms' leverage in 1928; the coefficient β then captures the effect of needing to refinance maturing bonds during the crisis on a firm's employment, relative to a similarly levered firm with no such debt maturing. The estimated effect of *BondsDue* does diminish slightly, but it remains sizable and statistically significant, suggesting that our results do not simply reflect that highly levered firms fared worse during the Depression.

It is also possible that some firms used debt to overexpand during the roaring 1920s. If this were the case, firms with higher levels of maturing debt would simultaneously have excess labor that perhaps could be more easily reduced. Yet in Column (5) we show that the effects of bonds maturing is robust to controlling for the firms' log employment level in 1928. The estimated effect of maturing debt on employment changes is also unaffected by controlling for firm size (in Column (6)) or by allowing industry shocks to vary by region (in Column (7)). It is also likely that more profitable firms may have needed less access to external finance and suffered less during the crisis. When we control for the firm's ROA in 1928 in Column (8) we indeed find that firms that were more profitable before the crisis experienced smaller reductions in employees, but the estimated effect of *BondsDue* on firm employment remains unaffected.In Column (9) we include a measure of profitability in 1933, to take into account that firms that performed better during the crisis may have suffered less from financial constraints. To be sure, ROA in 1933 is endogenous to financing frictions, and these results should be interpreted with caution since we are likely overcontrolling. Yet the estimated coefficient on maturing debt is robust in this specification as well. Last, Column (10) shows that these effects are not driven by differences in firm age.

The estimated coefficient on maturing debt β ranges between -1.2 and -1.5 across specifications. These effects imply that a one standard deviation increase in *BondsDue* is associated with a decline in the number of employees that is between 4.2% and 5.0%, representing about 18% to 21% of the average log change in employment between 1928 and 1933. A firm in the 90^{th} percentile of the distribution of maturing debt, which was faced with the need to refinance debt for about 3.6% of its assets, experienced a contraction in employment that

was 4.3% to 5.1% larger than the decline in the number of employees of the median firm, which had no bonds maturing from 1930 to 1934.

In Appendix Section B.1 we perform a number of robustness checks to address, among other concerns, the possibility that these results are driven by endogenous responses to the crisis, by local economic conditions, or by omitted characteristics correlated with the level and structure of the firms' leverage. An additional source of concern is that unobserved firm characteristics that become salient during the crisis may be responsible for our findings. In particular, the period before the Great Depression saw a rapid expansion of new industrial enterprises, perhaps aided by easy credit, a boom in innovation, and a bullish stock market. It is possible then that the most treated firms "overexpanded" more during the 1920s than other firms, and therefore experienced a more severe contraction. To study whether treated and control firms were on differential trends before the crisis, we collect information for the firms in our sample in earlier years from various *Moody's* manuals. Panel A of Table 5 presents the change in employment, profitability, firm size, and leverage for firms in the treated and control groups between 1927 and 1928. Reassuringly, we find no statistically significant differences for any of these variables between these two groups of firms (p-values shown in Columns (7) and (8)). To study pretrends over a longer time span, Panel B of Table 5 presents similar changes from 1925 to 1928. It is important to note that our ability to obtain information in 1925 is limited; many of the firms in our baseline sample did not appear in *Moody's* and, those that did were less likely to report employment. Treated and control firms differed only in their profitability, but these differences go in the opposite direction—firms with bonds maturing during the Depression saw larger increases in profits during the late 1920s than those firms in the control group.

In sum, our results show that firms that needed to refinance large amounts of debt relative to their size reduced their employment by more than 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). Next, we exploit the widespread suspension of banks across many areas of the country in the early 1930s as an additional source of variation on credit supply shocks across firms to further validate the importance of access to finance on firm employment during the Depression.

3.2 Exploiting Spatial Variation in Credit Supply Shocks

We next focus on the interaction between the firms' maturing debt and the geographic differences in bank failures, to obtain additional variation on the exposure to credit supply shocks across firms. (See Appendix Section B.2 for a detailed analysis of the direct effect

of bank failures on employment changes.) For the interaction analysis, we continue to rely on our main identification assumption that shocks to demand are the same for similar firms with different levels of maturing bonds. In addition, we now conjecture that firms that had high levels of bonds maturing when the public bond markets stopped functioning would have found it particularly difficult to obtain alternative sources of external capital to service (or roll over) those debts when they were located in areas that experienced bank failures.

We begin by examining whether observable firm characteristics varied systematically across firms with maturing debt by the conditions of their local banking systems. Importantly, Appendix Table A.6 shows that the level of maturing debt was uncorrelated with local bank failures, which partly alleviates concerns of reverse causality when including bank suspensions in our regressions. We further split the sample into four groups, depending on whether firms had any bonds maturing from 1930 to 1934 and whether they were located in counties where at least one national bank failed. Panel A of Table 6 presents summary statistics for the variables of interest for these four groups of firms. The most interesting comparisons are those between firms with maturing bonds located in areas with no bank failures (Columns (3) and (4)) relative to firms that also had maturing bonds but that happened to be exposed to bank failures (Columns (7) and (8)). Although a majority of firms (68%) were located in counties where banks failed, the fraction of firms with maturing debt was similar across areas with and without failures.The distribution of firms with maturing debt suggests that a reverse causality story in which a contraction in the balance sheet of the firms in our sample caused the collapse of local national banks is not very plausible. Moreover, these two groups of firms were similar on observables. Table 6 presents the p-values for the differences in means (in Column (9)) and medians (in Column (10)) for the "treated" firms ($BankFail = 1$ and $BondsDue > 0$) and the firms in the "control" group ($BankFail = 0$ and $BondsDue > 0$). We do not find any statistically significant differences. Importantly, the value of bonds maturing as a fraction of total assets were remarkably similar for these two groups of firms.

Panel B of Table 6 presents summary statistics for the changes in firm characteristics from 1927 and 1928. The last two columns show that the changes in employment, profitability, size, and leverage were essentially the same for those firms with maturing bonds regardless of location. Thus, differences in pre-crisis trends between treated and control firms are unlikely to drive our results.

To study the interaction between maturing bonds and local bank distress, we estimate:

$$\begin{aligned}\log(E_{i,1933}) - \log(E_{i,1928}) =& \alpha + \beta_1 \, BankFail_i + \beta_2 \, BondsDue_i + \\ &+ \beta_3 \, BankFail_i \times BondsDue_i + \lambda \, \mathbf{X}_{i,1928} + \gamma \, \mathbf{k}_i + \\ &+ \psi \, \mathbf{s}_i + \epsilon_i. \end{aligned} \tag{3}$$

where we now include $BankFail_i$, $BondsDue$, and their interaction, and focus on β_3 as the main coefficient of interest.

Table 7 presents the results. As expected, the estimated coefficient β_3 is negative and statistically significant across all specifications, and its magnitude ranges from -2.7 to -3.2. These estimates imply that a firm in the 90$^{\text{th}}$ percentile of maturing bonds outstanding experienced a 11% to 17% larger drop in employment when it was located in a county where at least one national bank failed, relative to a firm with similar characteristics and refinancing needs that was located in an area with no such bank failures. Thus, these estimates suggest that firms in these areas likely experienced a much larger shock to their supply of credit than the average firm. Similarly, when we compare the treatment group to firms with similar characteristics that were located in a region with bank failures, but had no bonds due during this period, we find that the treated firms experienced a 7–9% larger drop in employment. These findings further suggest that financial frictions had a sizable impact on the employment decisions of large firms, and suggest that the ability to substitute public debt for private debt may have helped firms to ease financial shocks, and conserve employment.

3.3 Robustness

In this section we perform a number of robustness checks to further validate our results. Thus far, we have utilized a continuous treatment on maturing debt. Since a small fraction of the firms in our sample had bonds becoming due, a potential concern is that our results are driven by a few outliers. Instead, we consider discrete effects. Specifically, 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, and interact each dummy with the indicator variable $BankFail_i$. Table 8 presents the results. The first row show that firms located in cities with national bank suspensions that had a positive value of debt due experienced a 14% to 21% greater drop in employment relative to firms also located in counties with bank failures but had no maturing bonds. The magnitude of this effect is sizable—about equal to the mean drop in employment in the sample. Further, the estimated effects are more pronounced for higher values of maturing

debt, with the contraction in employment being 23% to 32%, or 43% to 58%, depending on whether the firms had to refinance more than 5% or 10% of their assets. These estimates are based on a small number of firms—only 62 (31) firms located in cities with suspended national banks had to refinance more than 5% (10%) of their assets. With this caveat in mind, the positive gradient in the effects of maturing bonds on employment minimizes concerns related to the low leverage levels that characterized corporations during our sample period. In Appendix Table A.8 we also show that our baseline results are robust to excluding from the analysis those firms that had no leverage in 1928. Given the restrictions imposed by our sample size, for other robustness checks we focus on the continuous measure of *BondsDue*.

Table 9 presents additional results from a series of robustness checks. We include the same controls as in earlier tables, but in each panel we alter the definition of the treatment or the sample to address a different concern. To conserve space, we present only the estimated effects for the interaction term β_3.

We begin by studying the robustness of our results to alternative measures of maturing debt. Thus far, we have used the value of bonds becoming due from 1930 to 1934 as reported in *Moody's*, regardless of when these bonds were issued. Yet a small fraction of these securities were actually issued after the onset of the crisis, which may raise concerns that our estimated effects reflect an endogenous response of firms to the downturn. In Panel A we instead construct *BondsDue* using only those bonds that were issued before January 1, 1929. Reassuringly, our estimates are robust to this change. Our baseline definition of *BondsDue* includes those bonds that matured in 1934 to capture the possibility that firms acted in precautionary ways, reducing employment levels before bonds matured and conserving cash to repay their debts. In Panel B we instead measure *BondsDue* using only those bonds that became due from 1930 to 1933. Our estimated effects become somewhat larger, ranging from -2.9 to -3.5. This suggests that the effect of financing needs on employment was particularly severe from 1930 to 1933, when the banking system experienced the most strain. Our results are also robust to controlling for the firms' holdings of cash and marketable securities in 1928, which could have been used to pay down maturing liabilities (see Panel B of Appendix Table A.8). This finding refutes the current view that the financial turmoil mostly affected small firms, because large firms were unconstrained due to their abundant liquid assets (see, e.g., Lutz, 1945; Hunter, 1982; Calomiris, 1993).

Unobserved firm characteristics are an important threat to identification. To address this concern, we perform a placebo experiment by relating the changes in employment from 1928 to 1933 to the value of bonds that the firms in our sample had due in 1928, as a fraction of their assets in that year. Since these bonds matured well before there were any indications of

an impending crisis, we would expect them to be unrelated to the changes in employment during the Depression. Panel C of Table 9 indeed shows no such correlation. Although the estimates are noisy due to the small number of firms that had bonds maturing in 1928, they provide suggestive evidence that our main findings are unlikely to be solely the result of having maturing debt in any period. Another possibility is that our results are driven by unobserved firm characteristics that become salient during the crisis. Most importantly, firms that typically issued short-term bonds would have been more likely to have bonds mature from 1930 to 1934 (as well as in any other period). If these firms were also riskier, they may have also been more likely to suffer and lay off more workers during an economic downturn. To address this possibility, in Panel D we construct $BondsDue$ using only those bonds that were issued with a maturity of five or more years. The estimated effects are very similar to the baseline estimates presented in Table 7, ranging from -2.4 to -3.2 across specifications, and all but one are statistically significant at conventional levels. Further, in Panel C of Appendix Table A.8 provide evidence that our effects are not driven by differences in economic performance in the area in which the firms operate, measured by the growth in retail sales, obtained from Fishback, Horrace, and Kantor (2005).

Our baseline estimates use a discrete definition of bank failures. In Panel E of Table 9 we instead measure $BankFail$ using the amount of deposits in national banks that failed between 1929 and 1933 in the county in which the firm was located in 1928, scaled by the total amount of deposits in national banks in that county in 1928. The estimates of β_3 remain sizable and statistically significant across our specifications.

An important concern is that bank failures may be a partially endogenous regressor. In fact, a common criticism of the prior literature on the Great Depression, which primarily uses local bank distress to identify credit supply shocks, is that bank failures could instead be driven by disruptions in local demand. Our identification strategy should help somewhat to address this concern since it relies on the preexisting variation in maturing debt to control for exposure to demand shocks. But it is important to acknowledge that an additional threat to identification for the interaction effect between maturing bonds and bank suspensions is that firms with maturing debt may have more sensitive to local economic conditions. If that were the case, the estimated effects of maturing bonds and local bank failures could instead reflect local demand changes. To address this concern, in Panel F of Table 9 we show that our results are robust to including only firms that produced tradable goods. Specifically, we exclude from the analysis those firms that operated in the real estate, retail, construction, restaurant, personal and business services, recreation, transportation, and public utility sectors, whose demand may have been more sensitive to local conditions. Since firms producing tradables

were arguably more likely to have been affected primarily by aggregate demand, the findings in Panel F suggest that our main effects are unlikely to be driven by omitted characteristics correlated to local economic shocks.

4 Aggregate Impact of Financial Frictions

Our results indicate that having a substantial amount of bonds due in the period 1930–34 caused firms to cut employment sharply during the Depression. These estimated effects are substantially stronger when the firms were located in counties that experienced suspensions of national banks. Under the assumption that our identification strategy is valid, our analysis in the previous section provides an estimate of the elasticity of firm employment to maturing debt. However, evaluating the implications of this estimated elasticity for the aggregate change in employment is challenging. For instance, the treated firms account for a small fraction of the firms in our sample (approximately 11% to 15%, depending on the size of the credit shock). Hence even if the elasticity is well identified, the direct causal effect could perhaps account for only a small share of the overall contraction in employment in our sample.

We evaluate the aggregate impact of finance on employment in two main ways. In Section 4.1 we use the estimated elasticities to compute a counterfactual level of aggregate employment within the firms in our sample, assuming that the estimated treatment effect was equal to zero. Then, in Section 4.2 we use a structural model 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.

4.1 Evaluating the direct effect

To calculate the direct effect of frictions on aggregate employment, we compute the counterfactual level of employment in our sample under the scenario in which the treatment effect we estimate in equations (2) and (3) were uniformly equal to zero. Equation (2) yields an estimate that is based on the size of the average credit shock affecting all firms. Equation (3) represents a larger shock to credit supply, since these firms were also located in counties where the local banking sector was in distress. We therefore provide two sets of estimates corresponding to each of these shocks.

We first examine the definition of treated firms as those having maturing debt in the 1930-1934 period, which corresponds to equation (2). We compute the portion of the change in the number of employees E_i of firm i between 1928 and 1933 that can be attributed to the

term $\beta_1 BDUE_i$, as

$$\Delta \hat{E}^A_{i,1933} = \left[\exp\left(\hat{\beta}_1 BDUE_i + \hat{c}\, Z_{it}\right) - \exp\left(\hat{c}\, Z_{it}\right)\right] E_{i,1928}, \tag{4}$$

where $\hat{c}\, Z_{it}$ includes all the other variables in equation (2). All the estimated effects are computed using the specification in Column (10) of Table 4, which includes all controls and fixed effects. The sum is computed over the 801 firms that are included in this specification.

We repeat the same calculation using instead the estimates from equation (3), as

$$\Delta \hat{E}^B_{i,1933} = \left[\exp\left(\hat{\beta}_3\, BANKFAIL_i \times BDUE_i + \hat{c}\, Z_{it}\right) - \exp\left(\hat{c}\, Z_{it}\right)\right] E_{i,1928}. \tag{5}$$

and we use the point estimates corresponding to the specification in Column (10) of Table 7.

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

$$\hat{G}^r_E = \frac{\sum_f \Delta \hat{E}^r_{i,1933}}{\sum_f E_{i,1928}}. \tag{6}$$

Panel A of Table 10 summarizes our results. Our estimates of the aggregate magnitude of the direct treatment effect range from 0.8 to 1.4 percentage points 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 7 is equal to 9.4%. Using the estimates from equation (2), we find that the direct treatment effect is equal to 0.8 percentage points, or approximately 9% 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 counties with failed national banks – equation (3) – the magnitude of the direct treatment effect implies an aggregate drop of 1.4 percentage points in employment, about 15% of the overall drop.

Our identification strategy focuses on corporate bonds because we can observe when these bonds were due and because their long maturities allow us to argue that the preexisting variation in maturing debt was exogenous to the firms' investment opportunities. Our analysis therefore ignores other forms of debt that may have also matured during the crisis, and should therefore be taken as lower bound of the total effect.

4.2 Interpreting the estimates through a structural model

The analysis thus far aggregates the causal impact of financial frictions on employment within our sample, which is based exculsively on firms that survived from 1928 to 1933.

However, since firms with maturing debt that could not refinance during the Great Depression may have suspended operations, this analysis will lead to overly conservative estimates. Given that it is not possible to know for sure the true reason why firms (with or without maturing debt) may stop operating as the entity that they were in 1928, instead we rely on estimates from a calibrated structural model.

In this section, we present a simple structural model of firm employment with financial frictions. Our model accounts for the possibility that firms ended operations as a result of financial frictions. We calibrate the model to match the data along several dimensions and, most important, to deliver similar elasticities of employment to maturing debt in the simulated data as the estimates that we obtained in our empirical analysis. Matching this elasticity essentially identifies the parameter governing the severity of the financing friction in the model—i.e., the cost of external finance.

Model Setup

In the model, firms produce output $y_{i,t}$ with labor L using a decreasing-returns to scale technology,

$$y_{i,t} = e^{z_{i,t}} L_{i,t}^{\beta}. \tag{7}$$

Here, z is a firm-specific productivity shock that is realized at the beginning of period t and follows an AR(1) process,

$$z_{i,t} = \kappa\, z_{i,t-1} + \sigma_z \varepsilon_{i,t}, \tag{8}$$

where $\varepsilon_{i,t} \sim N(0,1)$ is an i.i.d. shock. We denote by $\sigma \equiv \sqrt{\sigma_z^2/(1-\kappa^2)}$ the steady-state dispersion in firm productivity z_i. In addition to labor, each firm is endowed with one unit of a fixed factor of production (land), which serves the role of collateral and enables firms to issue risk-free debt. Land has a collateral value ϕ_t that depends on the state of the world.

Importantly, there is a mismatch in the time at which labor is hired and the time at which output is produced. To keep the model simple, we assume that financing decisions and labor outlay costs occur at the *beginning* of the period, while a fraction $1-\lambda$ of the output is realized at the *end* of the period. Consequently, a firm that hires labor $L_{i,t}$ at the beginning of the period has financing needs, assuming the following expression is positive, equal to

$$w\, L_{i,t} + R\, D_{i,t} - \lambda\, e^{z_{i,t}} L_{i,t}^{\beta}, \tag{9}$$

where $D_{i,t}$ is the amount of debt maturing in period t. Firms can finance the potential shortfall in (9) either by issuing equity or by issuing debt up to the collateral value,

$$D_{i,t+1} \leq \phi_t. \tag{10}$$

There are two regimes: normal times and a financial crisis. In normal times, there are no restrictions to equity issues and firms can also issue risk-less one-period debt up to the value of the collateral value $\phi_t = 1$. To obtain meaningful levels of leverage, we assume that managers are impatient. In particular, firms discount the future at $\rho < 1/R$, where R is the interest rate (net of any tax benefits). During a financial crisis, firms cannot issue equity since equity markets tend to freeze and equity issuance come to a halt. In particular, the stock market crash in 1929 and the crisis that followed made equity issuance much less likely. While firms can issue debt, the collateral constraint is tightened, to $\phi_t = \underline{\phi} < 1$ consistent with the decline in prices of capital during the Great Depression. For example, according to Kindleberger (1973, pp. 144-45), "New lending stopped because of falling prices," which is consistent with a tightening of collateral constraints.

The transition probability of a crisis occurring, conditional on being in the non-crisis regime, is equal to p. The probability of exiting the crisis is equal to q.

The firm's optimization problem in the 'normal' (N) and 'crisis' (C) regimes can be written as follows. In normal times, the firm solves

$$V_N(D,z) = \max_{L,D'} \left\{ e^z L^\beta - w\,L - R\,D + D' + \rho\,E\left[p\,V_C(D',z') + (1-p)\,V_N(D',z',H) \middle| z \right] \right\}, \tag{11}$$

subject to

$$D' \leq 1. \tag{12}$$

By contrast, in the crisis state, the firm solves

$$V_C(D,z) = \max_{L,D'} \left\{ e^z L^\beta - w\,L - R\,D + D' + \rho\,E\left[(1-q)\,V_C(D',z') + q\,V_N(D',z') \middle| z \right] \right\}, \tag{13}$$

subject to

$$D' \leq \underline{\phi} \tag{14}$$

and

$$\lambda\,e^z L^\beta - w\,L - R\,D + D' \geq 0. \tag{15}$$

During a financial crisis, the firm faces both a tighter collateral constraint (14), as well as the constraint of no equity finance (15). Examining the firm's first order condition with

respect to labor L, we see that

$$\beta\, e^z L^{\beta-1} = w\, \frac{1+\gamma}{1+\lambda\,\gamma}, \tag{16}$$

where γ is the Lagrange multiplier on the constraint of no equity issuance during a crisis (15). Equation (16) reveals that the firm sets the marginal product of labor equal to its marginal cost. During a financial crisis, the marginal cost of labor may be higher than the wage w due to the financing friction (i.e., the presence of the no equity issuance constraint (15) and the collateral constraint (14)). Their effect is summarized by the lagrange multiplier γ on the issuance constraint (15). If the issuance constraint is not binding, as it is the case when $\gamma = 0$, then the firm makes the same employment decisions as an unconstrained firm.[13] Firms with low productivity z and/or high leverage D will be those likely to face a binding constraint ($\gamma > 0$).

The model also allows for the possibility of firm exit. Specifically, during a financial crisis, it is possible that some firms cannot satisfy both of the constraints (14)-(15) — these will be firms with low current productivity that enter the period with sufficiently high leverage that is close to the collateral constraint. In this case, the firm exits *permanently*, and equity holders obtain a continuation value V of zero.

Since firms solve a dynamic problem, the possibility of a financial crisis affects firm behavior in normal times. Specifically, in the normal regime, firms face no financial constraints, and therefore set the marginal product of labor equal to its marginal cost w. Firms choose a level of debt that trades off its benefits (recall $\rho < 1/R$) with its potential costs. These costs encapsulate the loss in firm value due to (potentially) distorted labor decisions in a financial crisis, along with the possibility of firm exit, and are encoded in the dependence of the firms' value function on debt D.

Fitting the model to the data

We next describe how we select the model's parameters. A subset of the model's parameters can be easily calibrated using observable features of the data. Panel A of Table 11 show the moments that we use an our calibration targets and the respective parameters. We chose $\beta = 2/3$ to match the average labor share. We chose a level of wages w such that, in normal

[13] Interestingly, the same is true if $\lambda = 1$, regardless of whether the no-equity issue constraint (15) is binding or not. This occurs due to the self-financing nature of the Cobb-Douglas technology. For financial constraints to affect labor decisions, there has to be a mismatch between when labor is hired and when output is realized. If all profits are realized at the same instant when labor is hired, firms will always be able to cover wages. One alternative approach would have been to specify the model so that labor is chosen one period in advance; however, a shortcoming of this approach would be that the firm would choose labor without knowing the realization of the productivity shock z, which would introduce additional effects due to uncertainty.

times, the average firm has a return on assets of 10%. We choose $\kappa = 0.8$ to generate a persistence in profitability over 5 years of approximately 0.32, which is consistent with the data. We choose a real (net) interest rate of 4.4%, which is consistent with the average real interest rate during the 1929-1934 period of 5% and a federal tax rate on corporate income of 12 % in 1928. We assume that the expected length of a crisis is two years ($p = 1/2$), and that financial crises happen, on average, once every 28 years ($q = 1/28$), which is in line with the evidence reported in Jordà, Schularick, and Taylor (2011). We calibrate the dispersion in firm productivity as $\sigma = 0.165$ in order to match the cross-sectional dispersion in profitability in 1928.

The remaining model parameters, ρ, λ, and $\underline{\phi}$ are estimated using the Simulated Method of Moments (SMM), using the identity matrix as the weighting matrix. As moments, we choose the average level of debt to assets in normal times and in the crisis period, as well as the sensitivity of employment growth to debt. To estimate this last parameter in the model, we first simulate a sample of 100,000 firms, which draw a level of productivity (z_0) and leverage (D_1) based on the joint distribution in the non-crisis state. Firms make labor decisions in the crisis state (L_1) given their productivity draw (z_1) and the level of maturing debt (D_1). We then closely follow equation (2) in the paper and estimate the elasticity of employment on debt due via a linear regression in the simulated data,

$$\log L_{f,1} - \log L_{f,0} = a + \beta\, D_{f,1} + c_0\, z_{f,1} + c_1\, z_{f,0} + c_2 \log L_{f,0} + \varepsilon_{f,1}. \tag{17}$$

The slope coefficient β is an additional moment that we use to calibrate the model. In our calibration, we target two values of β that correspond to our estimates using the two strategies described in the paper—that is, we values of β equal to -1.20 or -3.08—corresponding to the estimates from equations (2) and (3), respectively. To reduce simulation noise, in our estimation we normalize the estimates so that they correspond to a one-standard deviation increase in debt due (hence, our moment targets are -0.044 and -0.109 respectively). Panels B and C of Table 11 show the estimated parameters, along with standard errors.

We then use the model to examine how much employment in 1933 would have been in the absence of financial frictions. Specifically, we compute the aggregate drop in employment between normal times (state N) and the crisis (state C),

$$\log\left(\int_D \int_z L_C(D,z)\, p_N(D,z) dz\, dD\right) - \log\left(\int_D \int_z L_N(D,z)\, p_N(D,z) dz\, dD\right) \tag{18}$$

where $p_N(D,z)$ is the joint distribution of leverage and productivity in the normal regime, and L_N and L_C are the firm's optimal labor policy in the 'normal', and 'crisis' state,

respectively. There are two points worth noting. First, the above expression captures the drop in employment on *impact*; that is, the first term contains aggregate employment decisions that are functions of *pre-crisis* levels of leverage. Second, this calculation includes also firms that ceased operations in the crisis—that is, firms that choose to exit and effectively set $L = 0$.

Panel B of Table 10 summarizes our findings. Our calibration implies that eliminating the financing friction would result in a 1.5 to 3.1 percentage-point increase in the overall level of employment, depending on whether we target an elasticity of employment to debt equal to the estimates from equations (2) and (3), respectively. Here, note that these include employment losses due to exiting firms, hence they are not directly comparable to the 9.4% decline in employment among the firms in our sample (since these firms continue operations in 1933). When we decompose the aggregate drop in employment in the model into the intensive and the extensive margin, we find that the drop in employment among continuing firms is 0.8% and 1.8%, depending on the calibration, which is comparable to the estimates obtained by aggregating the direct effect in the data among continuing firms in Section 4.1. Our model therefore suggests that financing frictions may play a quantitatively significant role in the contraction in employment by contributing to firm exit.

Discussion and Caveats

The estimates in Panel B of Table 10 imply that financing frictions accounted for approximately up to one-third of the overall drop in employment. It is important to note that these estimates are based on fairly strong assumptions, as our simple model abstracts from many relevant features in the interest of simplicity. For example, in our framework (a) there is no capital; (b) there are no fixed costs of production; (c) our production function delivers a very tight link between current cash flows – which depend on $Z = \exp(z)$ – and the return to hiring employees – which is proportional to Z^{κ}; (d) there are no adjustment costs to labor; and (e) wages are constant.

Our intuition is that omitting 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 the lack of fixed costs (b) is rather conservative: if firms needed to also finance a fixed operating cost, many of them would exit, which would magnify the drop in employment in the model. Assumption (c) ameliorates the impact of the financing friction by introducing a strong correlation between cash flows from operations and hiring needs. In models where

shocks to investment opportunities are imperfectly correlated with the firms' operating cash flows (for instance, a model in the spirit of 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. Further, 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%.

The model emphasizes one channel through which financial frictions affect employment that is based on the timing mismatch between when output is realized and when workers are paid. However, alternative mechanisms are also possible. For instance, another possibility is that labor and capital and strong complements in production, and financial frictions primarily distort investments in physical capital. The resulting reduction in the capital stock can also lead to a fall in employment. Though this channel may have played an important role, we cannot assess its quantitative significance because we lack data on firms' capital expenditures.

5 Conclusion

This paper presents new evidence on the effects that financial frictions had on the high levels of unemployment experienced during the Great Depression. Firms that needed to refinance maturing bonds during the crisis contracted their workforce more than other similar businesses, particularly if their local banks were in distress and firms could not easily obtain alternative sources of external finance. Our aggregation exercises suggest that the aggregate level of employment in our sample would have been about 9% to 30% higher in the absence of financial frictions. Thus, disruptions to financial intermediation were likely an important contributor to the unparalleled severity and persistence of the economic contraction during the 1930s.

Our empirical design allows us to credibly identify the effects of financial constraints only for the firms in our sample. Although it is difficult to extrapolate our findings to other firms, it is important to note that our sample is composed of some of the largest industrial enterprises in the economy. We show that these large businesses were less dependent on bank financing than other corporations. Thus, the widespread failures of commercial banks in the early 1930s may have had a larger direct effect on other firms than what we find in our

sample. Large firms may have also suffered less from asymmetries of information than smaller firms. Thus, the increase in the cost of credit intermediation during the Great Depression may have been even larger for other firms in the economy. These two reasons suggest that our findings may therefore provide a conservative estimate of the role of financing frictions on employment among all American firms during the Great Depression.

Financial frictions have also been shown to have played a large role in the contraction in employment during the Great Recession (Chodorow-Reich, 2014). Both financial crises started with a collapse in asset prices—the stock market crash of 1929 and the market for securitized debt in 2008. But the disruption to financial markets was arguably more severe during the Great Depression, at least measured by the number of failed banks and the degree of freeze-up of public capital markets. Ultimately, the economic contraction was far deeper and persistent in the earlier crisis. Whether this difference is due to the size of the initial shock, the differences in regulatory frameworks, or the subsequent policy responses is open to debate and presents a fruitful avenue for future research.

References

Agarwal, Sumit and Robert Hauswald. "Distance and Private Information in Lending." *Review of Financial Studies*, 23(2010), 2757–2788.

Almeida, Heitor, Murillo Campello, Bruno Laranjeira, and Scott Weisbenner. "Corporate Debt Maturity and the Real Effects of the 2007 Credit Crisis." *Critical Finance Review*, 1(2011), 3–58.

Alston, Lee J., Wayne A. Grove, and David C. Wheelock. "Why Do Banks Fail? Evidence from the 1920s." *Explorations in Economic History*, 31(1994), 409 – 431.

Atack, J. and P. Passell. *A New Economic View of American History: From Colonial Times to 1940*. Norton (1994).

Becker, Bo and Victoria Ivashina. "Cyclicality of credit supply: Firm level evidence." *Journal of Monetary Economics*, 62(2014), 76–93.

Benmelech, Efraim and Nittai K. Bergman. "Financial Market Freezes." Working paper, Northwestern University (2016).

Benmelech, Efraim, Nittai K. Bergman, and Amit Seru. "Financing Labor." Working Paper 17144, National Bureau of Economic Research (2011).

Bernanke, Ben S. "Nonmonetary Effects of the Financial Crisis in the Propagation of the Great Depression." *The American Economic Review*, 73(1983), 257–276.

Calomiris, Charles W. "Financial factors in the Great Depression." *Journal of Economic Perspectives*, 7(1993), 61–85.

Calomiris, Charles W. and Joseph R. Mason. "Fundamentals, Panics, and Bank Distress During the Depression." *American Economic Review*, 93(2003), 1615–1647.

Calomiris, Charles W., Athanasios Orphanides, and Steven A. Sharpe. "Leverage as a State Variable for Employment, Inventory Accumulation, andFixed Investment." NBER Working Papers 4800, National Bureau of Economic Research, Inc (1994).

Chodorow-Reich, Gabriel. "The Employment Effects of Credit Market Disruptions: Firm-level Evidence from the 2008-09 Financial Crisis." *Quarterly Journal of Economics*, 129(2014), 1–59. Lead article.

Cole, Harold L. and Lee E. Ohanian. "A second look at the U.S. Great Depression from a neoclassical perspective." In "Great depressions of the twentieth century. Federal Reserve Bank of Minneapolis," 21–58.

Duygan-Bump, Burcu, Alexey Levkov, and Judit Montoriol-Garriga. "Financing constraints and unemployment: Evidence from the Great Recession." *Journal of Monetary Economics*, 75(2015), 89–105.

Eichengreen, B. *Hall of Mirrors: The Great Depression, the Great Recession, and the Uses-and Misuses-of History*. Oxford University Press (2014).

Eichengreen, Barry. "The Origins and Nature of the Great Slump Revisited." *Economic History Review*, XLV(1992), 213–239.

Fama, Eugene F. and Kenneth R. French. "Industry costs of equity." *Journal of Financial Economics*, 43(1997), 153 – 193.

Fishback, Price V., William C. Horrace, and Shawn Kantor. "Did New Deal Grant Programs Stimulate Local Economies? A Study of Federal Grants and Retail Sales During the Great Depression." *The Journal of Economic History*, 65(2005), 3671.

Friedman, M. and A.J. Schwartz. *A Monetary History of the United States, 1867-1960*. National Bureau of Economic Research Publications. Princeton University Press (1963).

Frydman, Carola, Eric Hilt, and Lily Y. Zhou. "Economic Effects of Runs on Early 'Shadow Banks': Trust Companies and the Impact of the Panic of 1907." *Journal of Political Economy*, 123(2015), 902–940.

Graham, John R., Sonali Hazarika, and Krishnamoorthy Narasimhan. "Financial Distress in the Great Depression." *Financial Management*, 40(2011), 821–844.

Graham, John R., Mark T. Leary, and Michael R. Roberts. "A century of capital structure: The leveraging of corporate America." *Journal of Financial Economics*, 118(2015), 658 – 683. NBER Symposium on New perspectives on corporate capital structures.

Greenwald, Bruce and Joseph E. Stiglitz. "Pareto Inefficiency of Market Economies: Search and Efficiency Wage Models." *The American Economic Review*, 78(1988), 351–355.

Hickman, W. Braddock. *Statistical Measures of Corporate Bond Financing Since 1900*. Number hick60-1 in NBER Books. National Bureau of Economic Research, Inc (1960).

Hughes, J.R.T. and L.P. Cain. *American Economic History*. Addison-Wesley series in economics. Pearson Addison-Wesley (2011).

Hunter, Helen M. "The Role of Business Liquidity During the Great Depression and Afterwards: Differences Between Large and Small Firms." *The Journal of Economic History*, 42(1982), 883–902.

Jacoby, Neil H. and Raymond J. Saulnier. *Business Finance and Banking*. NBER (1947).

Jordà, Òscar, Moritz Schularick, and Alan M Taylor. "Financial Crises, Credit Booms, and External Imbalances: 140 Years of Lessons." *IMF Economic Review*, 59(2011), 340–378.

Khwaja, Asim Ijaz and Atif Mian. "Tracing the Impact of Bank Liquidity Shocks: Evidence from an Emerging Market." *American Economic Review*, 98(2008), 1413–42.

Kindleberger, Charles P. *The World in Depression, 1929-1939*. Berkeley: University of California Press (1973).

Klug, Adam, John S. Landon-Lane, and Eugene N. White. "How could everyone have been so wrong? Forecasting the Great Depression with the railroads." *Explorations in Economic History*, 42(2005), 27–55.

Kogan, Leonid and Dimitris Papanikolaou. "Growth Opportunities, Technology Shocks, and Asset Prices." *Journal of Finance*, 69(2014).

Lee, James and Filippo Mezzanotti. "Bank Distress and Manufacturing: Evidence from the Great Depression." Working paper, Harvard University (2015).

Lutz, F.A. *Corporate cash balances, 1914-43: manufacturing and trade*. Studies in business financing. National Bureau of Economic Research (1945).

Margo, Robert A. "Employment and Unemployment in the 1930s." *Journal of Economic Perspectives*, 7(1993), 41–59.

Michaels, Beau Page, Ryan and Toni Whited. "Labor and Capital Dynamics under Financing Frictions." Working paper, University of Michigan (2014).

Mladjan, Mrdjan M. "Accelerating into the Abyss: Financial Dependence and the Great Depression." Working paper, EBS Business School (2016).

Pagano, Marco and Giovanni Pica. "Finance and Employment." *Economic Policy*, 27(2012), 5–55.

Petersen, Mitchell A. and Raghuram G. Rajan. "Does Distance Still Matter? The Information Revolution in Small Business Lending." *Journal of Finance*, 57(2002), 2533–2570.

Philippon, Thomas. "Has the US Finance Industry Become Less Efficient? On the Theory and Measurement of Financial Intermediation." *American Economic Review*, 105(2015), 1408–38.

Rauh, Joshua D. and Amir Sufi. "Capital Structure and Debt Structure." *Review of Financial Studies*, 23(2010), 4242–4280.

Richardson, Gary. "Categories and causes of bank distress during the great depression, 1929-1933: The illiquidity versus insolvency debate revisited." *Explorations in Economic History*, 44(2007), 588–607.

Richardson, Gary and William Troost. "Monetary Intervention Mitigated Banking Panics during the Great Depression: Quasi-Experimental Evidence from a Federal Reserve District Border, 1929–1933." *Journal of Political Economy*, 117(2009), 1031–1073.

Romer, Christina D. "The Nation in Depression." *Journal of Economic Perspectives*, 7(1993), 19–39.

Schnabl, Philipp. "The International Transmission of Bank Liquidity Shocks: Evidence from an Emerging Market." *The Journal of Finance*, 67(2012), 897–932.

Sharpe, Steven A. "Financial Market Imperfections, Firm Leverage, and the Cyclicality of Employment." *American Economic Review*, 84(1994), 1060–74.

Temin, Peter. *Did Monetary Forces Cause the Great Depression?* W. W. Norton (1976).

———. *The Cambridge Economic History of the United States*, chapter The Great Depression. Cambridge University Press (2000). 301–328.

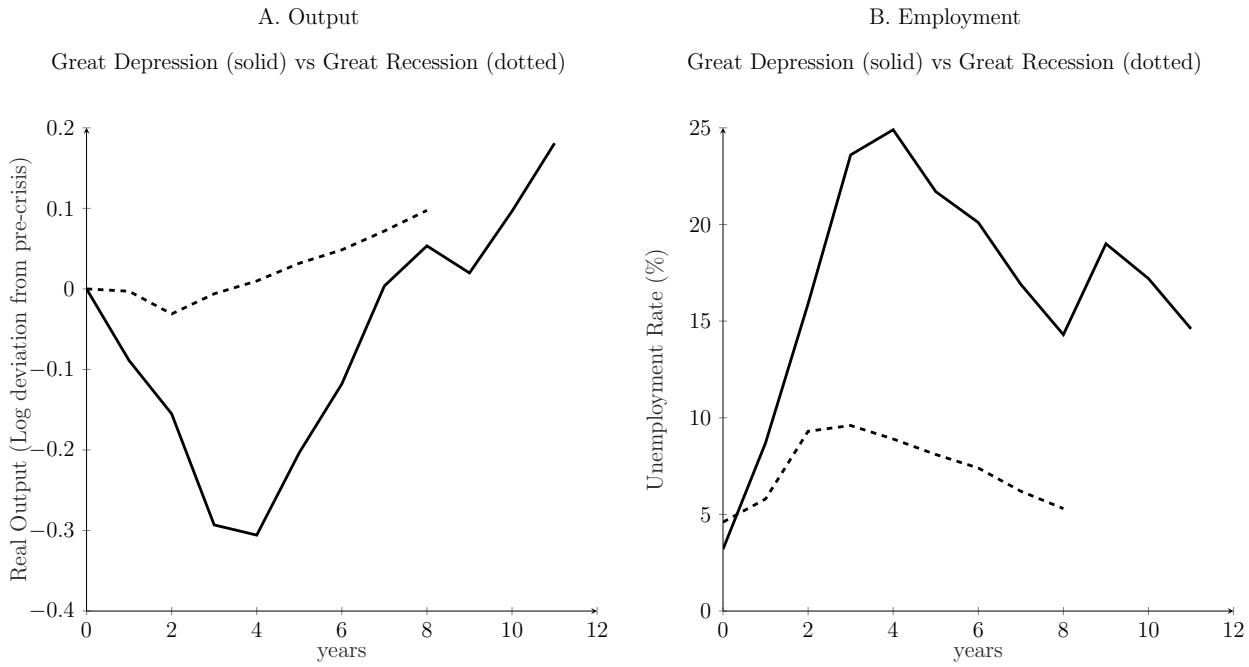
Wheelock, David C. "Regulation, market structure and the bank failures of the Great Depression." *Federal Reserve Bank of St Louis Review*, 77(1995).

White, Eugene Nelson. "A Reinterpretation of the Banking Crisis of 1930." *The Journal of Economic History*, 44(1984), 119–138.

Ziebarth, Nicolas L. "Identifying the Effects of Bank Failures from a Natural Experiment in Mississippi during the Great Depression." *American Economic Journal: Macroeconomics*, 5(2013), 81–101.

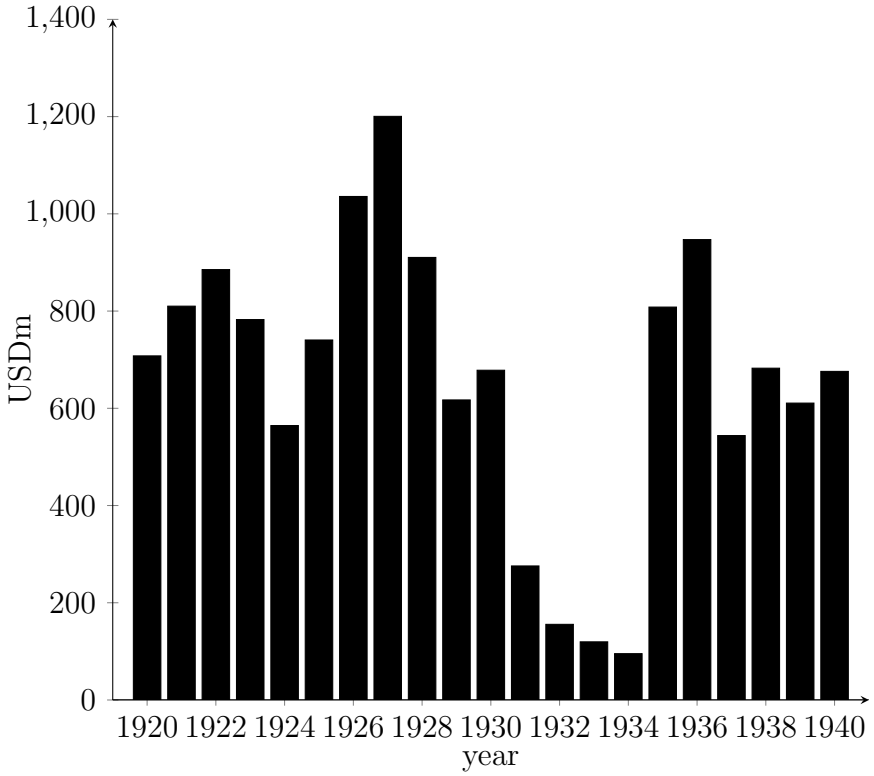
Tables and Figures

Figure 1: Comparison of the Great Depression and the Great Recession



Source: Output is real GDP in chained dollars, obtained from NIPA Table 1.1.6, row 1. The modern series for unemployment is from the Bureau of Labor Statistics. The historical unemployment data are obtained from Margo (1993) and are based on Lebergott's series, which counts persons on work relief as unemployed. To compare across both events, we define the pre-crisis period, or year zero, as 1929 for the Great Depression and 2007 for the Great Recession. In each figure, the dotted line presents data for the Great Recession, and the solid line presents data for the Great Depression.

Figure 2: Value of new offerings of industrial bonds



The figure plots the par value of new offerings of corporate bonds of all industrial firms from 1920 to 1940 (in millions of current dollars), as reported in Table 52 (Hickman, 1960).

Table 1: Summary statistics

	N	Mean	SD	p5	p10	p25	p50	p75	p90	p95
Employment, log, change (1928-1933)	1026	-0.23	0.60	-1.32	-0.97	-0.52	-0.18	0.12	0.45	0.67
Employment, log, 1928	1026	6.77	1.38	4.61	5.16	5.93	6.75	7.60	8.46	9.05
Employment, log, 1933	1026	6.53	1.50	4.09	4.68	5.56	6.55	7.44	8.37	9.12
Profitability, 1928	840	0.09	0.07	-0.01	0.01	0.04	0.07	0.12	0.19	0.26
Profitability, 1933	859	0.01	0.07	-0.13	-0.08	-0.03	0.01	0.05	0.09	0.12
Book assets, log, 1928	1026	15.61	1.24	13.82	14.12	14.71	15.52	16.41	17.35	18.06
Book assets, log, 1933	1026	15.40	1.32	13.46	13.79	14.44	15.25	16.14	17.24	18.02
Leverage, 1928 (%)	1026	12.75	14.25	0.00	0.00	0.00	7.24	21.84	35.59	41.49
Leverage, bonds only, 1928 (%)	1026	8.35	12.69	0.00	0.00	0.00	0.00	15.51	29.23	35.39
Leverage, 1933 (%)	1026	11.58	15.15	0.00	0.00	0.00	4.96	18.67	34.01	44.98
Leverage, bonds only, 1933 (%)	1026	8.40	13.65	0.00	0.00	0.00	0.00	14.16	29.29	40.26
Firm age, 1933	1023	22.09	14.78	5	6	9	19	31	42	51
Bonds due (1930-34), as fraction of total assets (%)	1026	1.10	3.50	0.00	0.00	0.00	0.00	0.00	3.59	9.23
Bonds due (1930-34), as fraction of total assets, conditional on bonds due > 0 (%)	154	7.33	6.01	0.19	0.67	2.30	5.81	10.73	17.43	21.00
Bonds due (1930-34), as fraction of bonds outstanding (%)	449	12.68	29.05	0.00	0.00	0.00	0.00	8.89	42.00	77.50
Number of suspended national banks (1929–1933)	1026	5.86	9.12	0	0	0	2	8	20	34
Fraction of suspended national banks (1929–1933)	1026	0.22	0.22	0	0	0	0.14	0.33	0.55	0.67
Fraction of suspended national banks, deposit-weighted (1929–1933)	1026	0.16	0.44	0	0	0	0.02	0.09	0.43	0.63

Employment is number of employees in either 1928 or 1933; 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 interest-bearing debt to the book value of assets in each year; bonds-only leverage considers only the value of long-term debt, typically listed as bonded and funded debt, and mortgages; firm age is the years since the firm's year of incorporation; bonds due is the total value of bonds that matured between January 1930 and December 1934, scaled by the average of book assets between 1928 and 1933. We also report the fraction of bonds due in 1930-34 as a fraction of the amount of bonds (funded debt) reported in firms' balance sheets as of 1928. 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 1929-1933 uses the total amount of deposits in national banks as of 1928 as the denominator. The sample is based on the 1,010 firms that matched across years, and that have non-missing information on employment and book assets in 1928 and 1933.

Table 2: Employment change and firm characteristics

$\log E_{1933} - \log E_{1928}$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Leverage_{1928}	-0.321*	-0.399**	-0.410**	-0.417**	-0.467**	-0.475**	-0.259	-0.296*
	(0.171)	(0.155)	(0.164)	(0.161)	(0.178)	(0.193)	(0.169)	(0.162)
$\log E_{1928}$				-0.033**	-0.138***	-0.140***	-0.184***	-0.181***
				(0.016)	(0.037)	(0.040)	(0.047)	(0.046)
$\log \text{Assets}_{1928}$					0.133***	0.130***	0.176***	0.173***
					(0.035)	(0.039)	(0.047)	(0.047)
$\text{Profitability}_{1928}$							1.865***	1.760***
							(0.464)	(0.418)
$\log Age$								-0.040
								(0.040)
Observations	1026	1026	1026	1026	1026	1026	840	837
R^2	0.006	0.135	0.175	0.179	0.199	0.248	0.315	0.315
Fixed effects	-	S	S, I	S, I	S, I	S, IxR	S, IxR	S, IxR

The table reports the coefficients from regressions relating of the change in log employment (measured by the number of employees reported in *Moody's*) between 1928 and 1933 on the firm's leverage ratio in 1928. Across the columns, controls include the log level of employment in 1928, log book assets in 1928, firm profitability in 1928 and 1933, and log firm age. Columns (2) to (8) include state fixed effects, Columns (3) to (5) include industry fixed effects, and Columns (6) to (8) include industry-region fixed effects, where regions are classified according the US Census definition (4 regions). Firms are classified into 30 industries following Fama and French (1997). Robust standard errors clustered at the industry level are presented in parentheses; ***,**, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 3: Comparison on observables for firms with and without maturing debt

	Bonds due = 0		Bonds due > 0		Difference (p-value)	
	Mean	Median	Mean	Median	Mean	Median
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. All firms						
Employment, log, 1928	6.77	6.80	6.76	6.57	0.59	0.09
Profitability, 1928	0.09	0.08	0.07	0.06	0.01	0.06
Book assets, log, 1928	115.59	15.51	15.72	15.57	0.52	0.96
Leverage,1928	0.11	0.05	0.22	0.22	0.01	0.01
Firm age, log	2.83	2.94	2.94	3.00	0.09	0.61
Firms	872		154			
Panel B. Firms with non-zero leverage in 1928						
Employment, log, 1928	6.84	6.86	6.73	6.55	0.21	0.09
Profitability, 1928	0.07	0.06	0.07	0.06	0.91	0.36
Book assets, log, 1928	15.68	15.61	15.72	15.53	0.81	0.27
Leverage,1928	0.17	0.13	0.23	0.22	0.00	0.00
Firm age, log	2.84	2.94	2.92	3.00	0.23	0.62
Number of firms	567		145			

Panel A is based on the baseline sample of firms that can be matched across years and that report employment and book assets in both 1928 and 1933. In Panel B, we further restrict the sample to firms with non-zero leverage in 1928. Columns (1) and (2) present the mean and median values of the observable variables for those firms that had no bonds mature from 1930 to 1934. Columns (3) and (4) present the mean and median values for the firms that had bonds mature over that period. Column (5) reports the p-value for the difference the means presented in Columns (1) and (3). The p-values for the difference in the medians reported in Columns (2) and (4) are obtained from a quantile regression on a treatment dummy for a positive amount of bonds due, and are presented in Column (6). Number of firms is based on those with information on assets.

Table 4: The effect of maturing debt on employment

$\log E_{1933} - \log E_{1928}$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$\text{BondsDue}_{1930-34}$	-1.427**	-1.244**	-1.479***	-1.140**	-1.190**	-1.218***	-1.199**	-1.195*	-1.290**	-1.206**
	(0.581)	(0.503)	(0.447)	(0.428)	(0.444)	(0.431)	(0.487)	(0.614)	(0.613)	(0.584)
Leverage_{1928}				-0.342**	-0.346**	-0.394**	-0.404**	-0.208	-0.090	-0.137
				(0.159)	(0.157)	(0.175)	(0.190)	(0.176)	(0.163)	(0.155)
$\log E_{1928}$					-0.034**	-0.140***	-0.143***	-0.188***	-0.161***	-0.156***
					(0.016)	(0.037)	(0.041)	(0.046)	(0.048)	(0.046)
$\log \text{Assets}_{1928}$						0.134***	0.132***	0.178***	0.135**	0.132**
						(0.035)	(0.039)	(0.046)	(0.049)	(0.049)
$\text{Profitability}_{1928}$								1.823***	1.163**	1.042**
								(0.456)	(0.470)	(0.430)
$\text{Profitability}_{1933}$									2.699***	2.703***
									(0.429)	(0.430)
$\log Age$										-0.051
										(0.031)
Observations	1026	1026	1026	1026	1026	1026	1026	840	804	801
R^2	0.007	0.132	0.173	0.178	0.183	0.203	0.252	0.318	0.397	0.398
Fixed effects	-	S	S, I	S, I	S, I	S, I	S, IxR	S, IxR	S, IxR	S, IxR

The table reports the coefficients regressions of the change in log employment (number of employees) between 1928 to 1933, on *BondsDue*, measured by the total dollar amount of bonds that became due from 1930 to 1934 scaled by the average of the firm's book assets between 1928 and 1933. As indicated, different specifications control for leverage in 1928, log employment in 1928 ($\log E_{1928}$), log book assets in 1928, firm profitability in 1928 and in 1933, and log firm age (measured in 1933). Columns (2) to (10) include state fixed effects, Columns (3) to (6) include industry fixed effects, and Columns (7) to (10) include industry-region fixed effects, where regions are classified according the US Census definition (4 regions). Firms are classified into 30 industries following Fama and French (1997). Robust standard errors clustered at the industry level are presented in parentheses; ***,**, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 5: Pre-crisis changes in observables, by the level of maturing debt

	Bonds due = 0			Bonds due > 0			Difference (p-value)	
	N	Mean	Median	N	Mean	Median	Mean	Median
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Change, 1927–1928								
Employment, log, change	545	0.063	0.000	106	0.076	0.000	0.98	1.00
Profitability, change	625	0.009	0.003	115	0.011	0.006	0.66	0.34
Book assets, log, change	773	0.053	0.025	142	0.064	0.025	0.39	0.71
Leverage, change	773	0.005	0.000	142	0.005	-0.004	0.93	0.39
B. Change, 1925–1928								
Employment, log, change	142	0.094	0.000	24	0.110	0.000	0.86	0.68
Profitability, change	419	-0.005	0.000	87	0.005	0.006	0.23	0.48
Book assets, log, change	565	0.098	0.056	105	0.137	0.085	0.41	0.77
Leverage, change	566	0.007	0.000	105	0.018	-0.003	0.23	0.72

Based on the sample of firms with non-missing observations for employment and book assets in both 1928 and 1933 that match across years. Panel A presents the change in observable characteristics between 1927 and 1928 for the set of these firms that also report information in 1927. Panel B presents the change in observable characteristics between 1925 and 1928 for the set of these firms that report similar information in 1925. For each variable, Columns (1) to (3) present the number of observations, mean, and median values for those firms that had no bonds mature from 1930 to 1934, and Columns (4) to (6) present the number of observations, mean, and median values for the firms that had bonds mature over that period. Column (7) reports the p-value for the difference the means presented in Columns (2) and (5). The p-values for the difference in the medians reported in Columns (3) and (6) are obtained from a quantile regression on a treatment dummy for a positive amount of bonds due, and are presented in Column (8).

Table 6: Comparison of observables, by maturing debt and bank failures

	No banks failed				Banks failed				Difference (p-value)	
	Bonds due = 0		Bonds due > 0		Bonds due = 0		Bonds due > 0			
	Mean	Median	Mean	Median	Mean	Median	Mean	Median	Mean	Median
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Firm characteristics in 1928										
Employment, log, 1928	6.67	6.68	6.55	6.50	6.81	6.80	6.87	6.62	0.23	0.63
Profitability, 1928	0.08	0.07	0.08	0.07	0.09	0.08	0.06	0.06	0.15	0.16
Book Assets, log, 1928	15.46	15.36	15.57	15.31	15.65	15.59	15.81	15.64	0.51	0.47
Leverage,1928	0.10	0.04	0.22	0.21	0.12	0.06	0.22	0.22	0.75	0.99
Firm Age, log	2.89	3.04	2.87	2.74	2.80	2.89	2.97	3.09	0.36	0.15
Bonds Due, 1930-34	-	-	0.08	0.06	-	-	0.07	0.06	0.83	0.73
Number of firms	282		44		590		110			
Panel B. Pre-crisis trends: change from 1927 to 1928										
Employment, log	0.063	0.000	0.042	0.000	0.061	0.000	0.087	0.000	0.22	1.00
Profitability	0.004	0.001	0.007	0.013	0.011	0.004	0.013	0.005	0.91	0.80
Book Assets, log	0.044	0.022	0.064	0.030	0.058	0.028	0.064	0.025	0.63	0.42
Leverage	0.001	0.000	-0.009	-0.007	0.006	0.000	0.011	-0.004	0.12	0.64
Number of firms	249		41		524		101			

Based on the sample of firms with non-missing observations for employment and book assets in both 1928 and 1933 that match across years. Panel A presents summary statistics in 1928. Panel B shows the change in observable characteristics between 1927 and 1928 for the set of these firms that also report information in 1927. Columns (1) and (2) are based on the firms located in areas where no national bank suspended from 1929 to 1933, and that had no bonds maturing from 1930 to 1934; Columns (3) and (4) are based on the firms located in areas where no national bank suspended from 1929 to 1933, and that had bonds maturing from 1930 to 1934; Columns (5) and (6) are based on the firms located in areas where at least one national bank suspended from 1929 to 1933, and that had no bonds maturing from 1930 to 1934; Columns (7) and (8) are based on the firms located in areas where at least national bank suspended from 1929 to 1933, and that had bonds maturing from 1930 to 1934. For each variable, Columns (1), (3), (5) and (7) report mean values in each respective sample, and Columns (2), (4), (6) and (8) report median values. Column (9) reports the p-value for the difference the means presented in Columns (3) and (7). The p-values for the difference in the medians reported in Columns (4) and (8) are obtained from a quantile regression on a treatment dummy for a positive amount of bonds due, and are presented in Column (10). Number of firms is based on those with information on assets.

Table 7: The effects of maturing debt and bank failures on employment

$\log E_{1933} - \log E_{1928}$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BankFail	-0.018	-0.024	-0.050	-0.046	-0.044	-0.041	-0.058	-0.069	0.007	-0.001
	(0.052)	(0.037)	(0.042)	(0.043)	(0.043)	(0.044)	(0.040)	(0.069)	(0.054)	(0.054)
$\text{BondsDue}_{1930-34}$	0.632	0.725	0.427	0.832	0.802	0.836	0.979	0.721	0.926	0.941
	(0.884)	(0.683)	(0.709)	(0.713)	(0.709)	(0.650)	(0.756)	(0.845)	(0.824)	(0.785)
$\text{BankFail} \times \text{BondsDue}_{1930-34}$	-2.998***	-2.874***	-2.745***	-2.844***	-2.870***	-2.957***	-3.083***	-2.673**	-3.159***	-3.080***
	(0.955)	(0.814)	(0.839)	(0.807)	(0.780)	(0.729)	(0.826)	(1.001)	(1.030)	(0.984)
Leverage_{1928}				-0.337**	-0.343**	-0.391**	-0.404**	-0.202	-0.098	-0.135
				(0.161)	(0.159)	(0.177)	(0.191)	(0.175)	(0.162)	(0.156)
$\log E_{1928}$					-0.033*	-0.137***	-0.139***	-0.182***	-0.155***	-0.152***
					(0.016)	(0.036)	(0.040)	(0.045)	(0.045)	(0.044)
$\log \text{Assets}_{1928}$						0.132***	0.129***	0.172***	0.130***	0.126**
						(0.034)	(0.037)	(0.045)	(0.045)	(0.046)
$\text{Profitability}_{1928}$								1.728***	1.038**	0.953**
								(0.435)	(0.447)	(0.409)
$\text{Profitability}_{1933}$									2.715***	2.714***
									(0.415)	(0.417)
$\log AGE$										-0.036
										(0.032)
Observations	1026	1026	1026	1026	1026	1026	1026	840	804	801
R^2	0.016	0.141	0.181	0.186	0.191	0.211	0.260	0.325	0.404	0.404
Fixed effects	-	S	S, I	S, I	S, I	S, I	S, IxR	S, IxR	S, IxR	S, IxR

This table reports the coefficients from regressions of the change in log employment (number of employees) from 1928 to 1933 on *BankFail*, *BondsDue*, and their interaction. *BankFail* is an indicator variable that takes the value of one if at least one national bank suspended between 1929 and 1933 in the county in which the firm was located, and zero otherwise. *BondsDue* is the total dollar amount of bonds that became due from 1930 to 1934, measured as a fraction of the firm's average of book assets between 1928 and 1933. As indicated in the table, different specifications control for leverage in 1928, log employment in 1928 ($\log E_{1928}$) , log book assets in 1928, firm profitability in 1928 and in 1933, and log firm age (measured in 1933). Columns (2) to (10) include state fixed effects, Columns (3) to (6) include industry fixed effects, and Columns (7) to (10) include industry-region fixed effects, where regions are classified according the US Census definition (4 regions). Firms are classified into 30 industries following Fama and French (1997). Robust standard errors clustered at the industry level are presented in parentheses; ***,**, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 8: Robustness: discrete treatment (maturing debt greater than x% of assets)

$\log E_{1933} - \log E_{1928}$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BankFail $\times \mathbb{1}(\text{BondsDue}_{1930-34} \geq 0)$	-0.207**	-0.185**	-0.166*	-0.173**	-0.165*	-0.156*	-0.188**	-0.137	-0.161	-0.162
	(0.091)	(0.077)	(0.081)	(0.084)	(0.083)	(0.085)	(0.088)	(0.115)	(0.112)	(0.109)
R^2	0.007	0.133	0.173	0.179	0.183	0.203	0.253	0.320	0.396	0.397
BankFail $\times \mathbb{1}(\text{BondsDue}_{1930-34} \geq 5\%)$	-0.270**	-0.263***	-0.233**	-0.247**	-0.253***	-0.268***	-0.283***	-0.253*	-0.321**	-0.315**
	(0.109)	(0.092)	(0.090)	(0.091)	(0.088)	(0.083)	(0.093)	(0.137)	(0.138)	(0.126)
R^2	0.009	0.136	0.176	0.182	0.186	0.206	0.255	0.322	0.400	0.400
BankFail $\times \mathbb{1}(\text{BondsDue}_{1930-34} \geq 10\%)$	-0.497***	-0.436***	-0.427**	-0.450**	-0.451**	-0.487***	-0.504***	-0.451**	-0.576**	-0.552**
	(0.154)	(0.154)	(0.182)	(0.175)	(0.173)	(0.156)	(0.179)	(0.196)	(0.215)	(0.210)
R^2	0.012	0.137	0.177	0.184	0.188	0.208	0.257	0.323	0.401	0.402
Observations	1026	1026	1026	1026	1026	1026	1026	840	804	801
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										Y
Fixed effects	-	S	S, I	S, I	S, I	S, I	S, IxR	S, IxR	S, IxR	S, IxR

This table reports robustness checks to the baseline results presented in Table 7, which evaluates the effect of bonds maturing in areas that experienced bank failures on the change in employment between 1928 and 1933, by using discrete treatment definitions of maturing bonds. To facilitate comparisons, the controls included in each column are the same as in Table 7. *BankFail* is an indicator variable that takes the value of one if at least one national bank suspended between 1929 and 1933 in the county in which the firm was located, and zero otherwise. *BondsDue* is an indicator variable that takes the value of 1 if the total dollar amount of bonds that became due from 1930 to 1934 (as a fraction of the firm's average book assets between 1928 and 1933) exceeds a threshold x%, where $x = 0, 5, 10]$. Separate regressions are estimated for each threshold. As indicated, different specifications control for leverage in 1928, log employment in 1928 ($\log E_{1928}$) , log book assets in 1928, firm profitability in 1928 and in 1933, and log firm age (measured in 1933). Columns (2) to (10) include state fixed effects, Columns (3) to (6) include industry fixed effects, and Columns (7) to (10) include industry-region fixed effects, where regions are classified according the US Census definition (4 regions). Firms are classified into 30 industries following Fama and French (1997). Robust standard errors clustered at the industry level are presented in parentheses; ***,**, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 9: Robustness: alternative measures of maturing debt and bank failures

$\log E_{1933} - \log E_{1928}$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. Only include bonds issued before January 1, 1929										
BankFail × $\text{BondsDue}_{1930-34}$	-2.974**	-2.811**	-2.327*	-2.532**	-2.472**	-2.750**	-2.892**	-2.719*	-3.008*	-3.023*
	(1.229)	(1.169)	(1.213)	(1.168)	(1.135)	(1.032)	(1.205)	(1.455)	(1.511)	(1.494)
B. Exclude bonds maturing in 1934										
BankFail × $\text{BondsDue}_{1930-33}$	-3.289***	-3.153***	-2.878***	-3.000***	-3.085***	-3.182***	-3.263***	-3.043**	-3.490***	-3.376***
	(1.148)	(1.040)	(1.035)	(0.993)	(0.957)	(0.909)	(0.988)	(1.114)	(1.203)	(1.165)
C. Placebo (bonds maturing in 1928)										
BankFail × BondsDue_{1928}	-5.065	-7.704	-8.780	-9.629	-10.083	-10.758	-5.345	5.764	-5.133	-4.183
	(7.451)	(6.079)	(7.623)	(7.970)	(7.903)	(8.060)	(8.919)	(13.633)	(9.586)	(10.111)
D. Exclude bonds with maturity less than 5 years when issued										
BankFail × $\text{BondsDue}_{1930-34}$	-3.174**	-2.901**	-2.355*	-2.585*	-2.519*	-2.778**	-2.888**	-2.579	-2.880	-2.921*
	(1.395)	(1.292)	(1.353)	(1.321)	(1.284)	(1.171)	(1.387)	(1.553)	(1.692)	(1.675)
E. Deposit-weighted measure of bank failures										
BankFail × $\text{BondsDue}_{1930-34}$	-1.250	-1.623**	-1.489**	-1.572**	-1.563**	-1.284*	-1.341*	-1.146*	-1.399*	-1.511**
	(0.942)	(0.646)	(0.644)	(0.651)	(0.657)	(0.675)	(0.676)	(0.650)	(0.758)	(0.730)
F. Firms in tradable sectors only										
BankFail × $\text{BondsDue}_{1930-34}$	-3.743***	-3.555***	-3.493***	-3.517***	-3.511***	-3.253***	-3.131***	-2.375*	-3.430**	-3.401**
	(1.180)	(0.969)	(0.900)	(0.912)	(0.900)	(0.935)	(1.044)	(1.222)	(1.366)	(1.314)
Observations (Panels A–E)	1026	1026	1026	1026	1026	1026	1026	840	804	801
Observations (Panel F)	780	780	780	780	780	780	780	639	611	609
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										Y
Fixed effects	-	S	S, I	S, I	S, I	S, I	S, IxR	S, IxR	S, IxR	S, IxR

This table reports several robustness checks to the baseline results presented in Table 7, which evaluates the effect of bonds maturing in areas that experienced bank failures on the change in employment between 1928 and 1933. To facilitate comparisons, the controls included in each column are the same as in Table 7. In panel A, the *BondsDue* includes only bonds issued before January 1, 1929; in panel B, bonds maturing in 1934 are excluded; panel C presents a placebo in which the *BondsDue* variable is based on the value of bonds maturing in 1928 (as a fraction of 1928 assets); panel D includes only bonds issued with a maturity of five or more years years; panel E measures the *BankFail* variable by value of deposits in national banks that suspended between 1929 and 1933 in the county in which the firm was located, scaled by the amount of deposits in all national banks in that area in 1928; panel F excludes firms in real estate, retail, construction, restaurants, personal and business services, recreation, transportation, and utilities. As indicated, different specifications control for leverage in 1928, log employment in 1928 ($\log E_{1928}$) , log book assets in 1928, firm profitability in 1928 and in 1933, and log firm age (measured in 1933). Columns (2) to (10) include state fixed effects, Columns (3) to (6) include industry fixed effects, and Columns (7) to (10) include industry-region fixed effects, where regions are classified according the US Census definition (4 regions). Firms are classified into 30 industries following Fama and French (1997). Robust standard errors clustered at the industry level are presented in parentheses; ***,** and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 10: Estimating aggregate effects of financing frictions

Overall drop in employment in firms included in Column (10) of Tables 4 or 7	-9.4%
A. Direct effect	Estimate
Using coefficient estimates from Table 4	
Aggregate effect of maturing bonds	-0.8%
Using coefficient estimates from Table 7	
Aggregate effect of maturing bonds	-1.4%
B. Indirect effect (model-implied)	Estimate
Using coefficient estimate from Table 4	
Overall drop in employment	-1.5%
Using coefficient estimate from Table 7	
Overall drop in employment	-3.1%

This table presents various aggregation exercises to determine the effect of financial frictions on the aggregate drop in employment among large firms during the Great Depression and the Great Recession, based on data for firms that report assets and employment in 1928 and 1933, and that can be matched across years. In Panel A, the effects are estimated from the $BondsDue$ treatment, as reported in Table 4, and from the $BondsDue$ X $BankFailed$ treatment, as reported in Table 7. Panel B presents estimates for all firms, as we as for exiting firms only. The values under "Estimate" calculate the effects from equation 6; the percent of total is calculated as the ratio of Column (1) to the aggregate contraction in employment of 9.4%.

Table 11: Model Calibration

Moment	D	M	Parameter		
A. Calibrated Parameters					
Labor share	2/3	2/3	Share of labor in production	β	2/3
Profitability, persistence 1928–1933	0.32	0.32	Persistence of firm productivity	κ	0.80
Real interest rate (%, net of tax benefits)	4.4	4.4	Interest rate	R	1.044
Profitability (in 1928), mean	0.10	0.10	Wage	w	1.24
Profitability (in 1928), dispersion	0.07	0.07	Dispersion in firm productivity	σ	0.165
Mean duration of financial crisis	2	2	Probability of exiting crisis	p	1/2
Mean years between financial crises	28	28	Probability of crisis occurring	q	1/28
B. Estimates from equation (2) (Bonds Due)					
Elasticity of labor to maturing debt (scaled)	-0.044	-0.045	Fraction of sales that finance labor	λ	0.758 (0.017)
Leverage (in 1928), mean	0.128	0.130	Firm discount rate	ρ	0.937 (0.007)
Leverage (in 1933), mean	0.116	0.117	Collateral constraint in crisis	$\underline{\phi}$	0.117 (0.008)
C. Estimates from equation (3) (Bonds Due X Bank Fail)					
Elasticity of labor to maturing debt (scaled)	-0.109	-0.110	Fraction of sales that finance labor	λ	0.714 (0.051)
Leverage (in 1928), mean	0.128	0.127	Firm discount rate	ρ	0.948 (0.077)
Leverage (in 1933), mean	0.116	0.114	Collateral constraint in crisis	$\underline{\phi}$	0.115 (0.004)

This table presents the parameters used to fit the model to the data. We use a mixture of calibration and structural estimation. In Panel A, we describe the calibrated parameters (on the right panel) and the moments in the data that we use to identify these parameters. In the bottom two panels B and C, we present results from a structural estimation, that are based on the point estimates of equations (2) and (3) respectively. The target set of moments (shown in the left panel) include the sensitivity of employment to maturing debt (scaled to correspond to a one-standard deviation change in debt due), and the level of leverage in 1928 and 1933. On the right panel we show the estimated parameters, along with standard errors in parenthesis. The parameters are estimated using Simulated Method of Moments, using the identity matrix to weight the moments.