Financial Frictions and Employment during the Great Depression*

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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 10% to 33% of the aggregate decline in employment of large firms between 1928 and 1933.

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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.

Financial frictions are one of several factors that may have played an important role in the Great Depression. In a seminal paper, Bernanke (1983) argues that an increase in the real costs of intermediation 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 to some extent. Thus, any difficulties in obtaining external finance may have severe effects on firms' employment decisions. Despite the potential appeal of this explanation, 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 10% and 33% of the overall drop in employment in our sample from 1928 to 1933.

Our 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. Perhaps surprisingly, our data reveal that large enterprises suffered greatly during the crisis: the average firm in the sample experienced a 24% 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, from an average return on assets (ROA) of 9% to merely 1%.[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.

To identify the effect of financial frictions on firm employment changes, we employ two main strategies. First, similar to Almeida, Campello, Laranjeira, and Weisbenner (2011), we exploit 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 from 1930 to 1934 (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^{th} percentile of the value of maturing debt (scaled by assets) contracted its employment between 1928 and 1933 by about 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 corporate bonds that matured during the crisis were primarily issued well in advance of the onset of the Depression. Our findings are unlikely to be influenced by changes in the firms' investment opportunities, and in their demand for external finance, in response to the negative economic shock. Our second identification strategy exploits spatial variation by interacting the variation in the firms' maturing debt with the conditions 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

[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.

for their borrowers. We assume that firms found it easier to borrow from banks in their area, and we measure the reduction in bank credit for each firm in the sample with an indicator for whether at least one national bank suspended operations in the county where the firm operated.[2] 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. Firms located in counties where national banks failed had larger declines in employment than similar firms located in areas with no such disruptions, but the differential effects become smaller and statistically insignificant when we control for firm profitability.[3]

However, the constraints on firms imposed by having bonds mature during the crisis should have been particularly salient for those firms that could not easily substitute bond financing for bank loans. Thus, our second empirical strategy analyzes the interaction between maturing debt and the disruptions in local bank systems. We find that firms in the 90^{th} percentile of the value of maturing debt contracted their employment levels by about 12% to 18% more when they were located in counties that experienced bank failures than 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.

Our estimation strategy, which is based on the substitution between private and public debt, also alleviates some identification concerns. For example, estimates of the role of financial frictions identified solely from variation in local bank failures, common in the existing literature on the Great Depression, may instead reflect shocks to local economic conditions that simultaneously affect bank health and firm investment opportunities. Our strategy instead effectively controls for the variation induced by these shocks by comparing firms that were located in areas with similar disruptions to the banking system, but that differed in their preexisting levels of maturing debt. In further analysis, we also show that our results are robust to different measures of maturing debt and bank failures and do not appear to be driven by preexisting trends in observable characteristics.

[2] As we discuss in Section 1.2, we focus on national banks, instead of state banks, since these institutions were arguably more likely to lend to large industrial concerns during our period of analysis.

[3] 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. 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.

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 10% and 17% of the overall drop in employment in our sample, but it is likely a conservative estimate for two main reasons.

First, our baseline estimates do not include whether firms may have also needed to refinance maturing *bank* debt, since our data contain detailed information only on maturing bonds. We incorporate these firms to our estimation of aggregate effects by applying our estimated elasticity to firm's level of outstanding bank debt. Depending on our assumptions regarding the fraction of a firm's bank debt in 1928 that matured during the crisis, we find that employment would have been 1.0 to 2.4 percentage points higher in the absence of frictions for both public and private debt. These estimates suggest that the disruption in credit supply may explain about one-third of the overall drop in employment in our sample.

Second, it is possible that firms with no maturing bonds 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 on *all* firms in the sample, 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 20% to 33% 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.[4] Our evidence is consistent with Bernanke (1983), who argues that

[4]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).

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 and Mezzanotti, 2015).[5] 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.[6] 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).[7] 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

[5] 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.

[6] 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, Page, and Whited (2014); Pagano and Pica (2012).

[7] 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.

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. Although there are certainly many parallels between these events, the magnitudes of the economic shocks were very different. Figure 1 contrasts the evolution of real GNP and unemployment rates for these two periods. Panel A shows that the magnitude of the economic contraction was an order of magnitude larger in the 1930s than in the recent crisis; 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, however, 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).

These differences in economic outcomes motivate a quantitative comparison of the importance of disruptions to financial intermediation across the two crises. The contrast of our estimated elasticity of employment to maturing debt to a similar estimate calculated by Benmelech et al. (2011) for the 2008–2009 crisis reveals that the effect of financial frictions on unemployment was about two to five times larger in the Great Depression than in the Great Recession.[8] Although this comparison should certainly be interpreted with caution, the smaller impact of financial disruptions during the recent crisis may provide some insights on the influence of the financial sector on the economy over time. 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 estimated effects of financial frictions on unemployment 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.

[8]Our work also complements Chodorow-Reich (2014), who shows that the withdrawal of bank lending accounted for between one-third and one-half of the drop in employment in small and medium-sized firms during the recent crisis. He finds no statistically significant effects among large firms, perhaps because these firms could issue bonds as an additional source of financing. By contrast, we find sizable effects of the total amount of maturing debt even among the largest firms in the economy, suggesting that the substitution between public and private debt is often impaired during financial crises.

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 The Long-Term Debt Approach

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 baseline 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, public debt markets essentially shut down during the Great Depression.[9] 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.[10] Firms that happened to have bonds maturing in this period would have experienced

[9]Importantly, issuing new equity was 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.

[10]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.

extreme difficulties in refinancing those debts and likely faced (exogenously) higher costs of intermediation.

A key assumption of 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. 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 economic 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 on any other issue about 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.[11]

1.2 Substitutability of Public and Private Debt

The American banking system experienced a major collapse during the Great Depression. From 1929 to 1933, more than 40% of depository institutions suspended operations (see, among others, Alston, Grove, and Wheelock (1994), Wheelock (1995), and Richardson (2007)).[12] Much of the work on the Great Depression has used the variation in these bank failures over time and space to analyze their effects on real economic activity. We add to this literature by studying the implications of disruptions in local credit markets on *firm-level* employment decisions.

[11] 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.

[12] 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.

Economic theory and modern empirical evidence support the view that bank failures may have exacerbated the contraction in economic activity during the Great Depression. In modern economies, as well as in the past, firms typically establish long-lasting relation with financial intermediaries. Banks may use these relation to obtain soft information about their borrowers, and thus reduce frictions arising from asymmetries of information. When a financial intermediary fails, the bank's nonfinancial clients typically struggle to obtain external funds from other institutions and experience negative outcomes, including decreases in borrowing levels and employment, higher probability of distress, and lower firm survival (Khwaja and Mian, 2008; Schnabl, 2012; Chodorow-Reich, 2014). To credibly establish the effects of this "lending channel," as proposed by Bernanke and Gertler (1995), these studies use within-firm variation in the exposure to bank shocks to control for the firms' investment opportunities that may affect their demand for credit.

Determining the causal effects of bank failures on firm outcomes during the Great Depression is particularly challenging since there are no data that identify specific firm-bank borrowing relations. Thus, we simply relate the suspension of banks in the county in which firms operate to their change in employment. The implicit assumption is 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)). If so, we would expect firms to be disproportionately affected by suspensions of banks in the counties in which they operated. Yet there are reasons to expect that this *direct* effect of bank failures may have been small for the firms in our sample. The United States had a unit banking system at that time, which severely restricted the ability of commercial banks to branch (Calomiris, 2000).[13] Thus, most commercial banks were small and undiversified. Perhaps due to these restrictions, large firms did not use much bank credit at that time—for example, corporate bonds accounted for about two-thirds of the value of total debt outstanding for the average firm in the sample in 1928.

It is also important to note that any evidence based solely on a measure of local bank failures not only may reflect a contraction in credit supply, but also may be the result of a reduction in credit demand. Moreover, local bank failures and firms' employment decisions may also be driven by negative (unobserved) shocks to local economic conditions. Because of these important considerations, an association between local bank failures and economic outcomes—which has been the primary identification strategy for financial frictions used thus

[13]In 1865, a ruling of the Comptroller of the Currency limited national banks to only one office, and most states imposed similar restrictions for state-chartered banks. The McFadden Act of 1927 granted limited branching rights to those national banks operating in states that conferred state banks the ability to branch. At the onset of the Great Depression, the United States was still characterized by an extremely large number of small, undiversified institutions.

far in the literature on the Great Depression—cannot conclusively establish the effect of the contraction in credit supply on economic activity. Instead, our main identification strategy relies on the substitutability between public and private debt.

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 their banking systems. Firms that had long-term bonds (exogenously) becoming due during the crisis likely needed to refinance those debts but could not use public bond markets to do so, since these markets had frozen. These firms may have been more likely to try to secure loans from local banks instead.[14] Under the assumption that firms were more likely to borrow from local banks, their ability to substitute maturing bonds with bank loans should have been more impaired when firms with bonds becoming due were located in areas that experienced bank failures.

To find evidence for such an indirect effect of bank distress, it is important to focus on those financial institutions that may have been likely to provide loans of an appropriate size for the large industrial concerns in our sample. 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.[15] 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.[16] National banks were thus better positioned to lend to the firms in our sample,

[14]It is possible that firms that typically relied on bank borrowing were different from those firms that financed their operations through the bond markets. Our identification strategy, however, depends solely on some degree of substitutability between bank lending and public debt as sources of financing. Rauh and Sufi (2010) and Becker and Ivashina (2014) suggest that, at least in recent decades, private and public debt have been partial substitutes.

[15]State-chartered banks were primarily subject to state regulation and supervision that varied substantially across states, whereas the federally chartered national banks operated under uniform federal banking regulation. Whereas national banks provided annual reports and other 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.

[16]The difference in size between the average national and state bank is similar if we focus instead on equal-weighted averages, about $7.6 million and $4.3 million, respectively.

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 exclusively on national bank failures.

To exploit the substitution between private and public debt markets as a source of identification, our main empirical strategy compares the changes in employment for the firms located in counties in which at least one national bank failed that needed to refinance some portion of their debt outstanding during the crisis, relative to other similar firms. By comparing firms with different levels of preexisting maturing debt that happen to be located in areas with similar disruptions to local banking systems, this strategy helps address concerns that local economic conditions drive the results.[17] In this manner, our paper improves on existing findings that are based solely on local banks failures. Moreover, our specification allows us to compare firms with similar levels of maturing debt that were located in areas with different disruptions to credit markets. Since unobserved firm characteristics correlated with maturing debt are unlikely to vary by the firms' location before the crisis, the strong effects of financial frictions on firm-level employment decisions that we document here are unlikely to be the result of omitted variable bias.

2 Data

We begin by describing the main features of our novel dataset. In Section 2.1 we describe the sources used to construct the data, and in Section 2.2 we present summary statistics. Section 2.3 compares our dataset to existing data on the Great Depression. Last, Section 2.4 presents new stylized facts that relate the characteristics of firms to their reductions in employment during the 1928-1933 period.

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

[17] Our empirical strategy therefore relies on the assumption that similar (unobserved) economic shocks affected areas with similar bank failures. As in our previous identification strategy, we continue to rely on the exogeneity of preexisting maturing bonds to changes in firms' investment opportunities due to the crisis.

Depression in 1933.[18] 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 (39% in 1928 and 53% in 1933) report employment figures.[19] 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 1,010 firms that report non-missing information on employment and assets in both years.

Our sample is composed primarily of firms operating in manufacturing and retail. The Great Depression, however, did not affect all industries equally. Romer (1990), for example, shows that the contraction in consumer consumption of durable goods was particularly large. 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).

Our main identification strategy uses preexisting variation in the value of corporate bonds that became due during the crisis. Starting in 1931, the *Moody's* manuals provided 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.[20] 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 in our sample from the 1930 manual. We then use similar detailed descriptions for the corresponding manuals to obtain the date of issuance for all bonds maturing in the 1930-1934 period.

Last, we obtain information on national bank suspensions from the Federal Deposit Insurance Corporation (FDIC) Data on Banks in the United States.[21] The FDIC data allow us to measure the suspension of national banks between 1929 and 1933 at the county level. To match our firm-level data to the bank information, we collect the firm's primary address

[18] 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).

[19] Conditional on observing assets in both years, we are more likely to have non-missing information on employment change for those firms that were more profitable or older in 1928.

[20] When the bond name does not match a company listed in the corresponding manual, we manually search for the parent company that has assumed the debt to allocate bonds to firms correctly.

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

(city and state) from the *Moody's* manuals. The address reported in the manuals 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 1,010 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. To minimize the impact of outliers in our analysis, we winsorize all observations at the 2% and 98% level.[22]

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 employed approximately 842 workers in 1928. The size employment distribution is actually fairly skewed; the average firm in the sample had instead 1,780 employees in that year. To address the 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 0.24 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 0.58 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 did suffer during the Depression is the decline in profitability evidenced in our sample: the average ROA declined from 9% in 1928 to 1% in 1933. To give a sense of the severity of this collapse, it is useful to note that the cross-sectional standard deviation of profitability was merely 7% in 1928 or in 1933. Further, approximately

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

41% of the firms in our sample experienced negative profits in 1933, but less than 7% had losses before to the onset of the crisis. Since profitable firms may have been less financially constrained, we control for profitability in some of our specifications.

We also find that the average (book) leverage ratio was 0.13 in 1928, although there was substantial heterogeneity across the firms in our sample (the standard deviation was 0.14 in that year). 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.[23] Moreover, a sizable fraction of firms had no debt outstanding in 1928. In our empirical analysis, we perform several 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 main identification strategy 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 this ratio within the affected firms. Conditional on having bonds that become due during the 1930-1934 period, the average firm had to refinance debt that was about 9% of its assets, and the cross-sectional standard deviation around this number was 11%. 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. However, our main results are robust to using a discrete treatment that identifies as treated firms those that had any bonds due during this period.

The last three rows of Table 1 report summary statistics that describe the conditions of the local banking systems in the areas in which our firms operated during the crisis. Specifically, we employ variation in the suspensions by national banks in the counties in which the firms were located. The failure of national banks from 1929 to 1933 was fairly widespread. Only

[23] 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. The limitations imposed by the unit banking system likely contributed to the low leverage levels of industrial corporations during the early decades of the twentieth century.

324 firms in our sample were located in counties in which no national bank suspended during this period. For the remaining firms, there was considerable variation in the intensity of the disruption to their local banking systems: 337 were located in counties where at most five national banks failed, 238 were located in counties with six to ten suspended national banks, and the remaining 115 operated in counties with more than ten such suspensions.

The variation in the number of suspended banks partly reflects differences in the number of national banks that existed in the region. Thus, we also calculate the fraction of suspended national banks in a county as a fraction of the number of national banks in that area in 1928. The average firm was located in a county where 22% of the national banks failed from 1929 to 1933, and the cross-sectional dispersion in this measure was sizable, also about 22%. The disruptions to local banking systems may have been affected not only by the failure rates of banks, but also by the relative size of those banks that failed. To take this possibility into account, we 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 Validity

As with any novel dataset, it is important to assess the validity of the data. In this section, we discuss two main issues. First, our results are based on a sample of about one thousand large firms. Thus, a valid concern is the broader representativeness of our evidence. The manuals obtained the employment numbers either from the firms' annual reports or directly from them. The quality of the employment data might therefore be questioned. We address each concern in turn.

Although our sample contains a few fairly small firms, the majority (about 95%) employed more than one hundred employees. The focus on large employers may raise concerns about the external validity of our estimates, as the typical firm in the economy was likely much

smaller. Owing to the lack of appropriate data, it is difficult to compare the size of our firms to other industrial companies operating in the United States at that time. The Census of Manufactures of 1929, for example, reported that the average establishment in manufacturing had approximately 49 employees. However, this estimate is likely much lower than the employment level of the average firm in the economy, since large firms are typically composed of multiple establishments. An alternative way to assess the relevance of the firms in our sample is to compare their total employment—a bit more than two million workers in 1928—to the relevant portion of the U.S. labor force. Given the variety of industries covered by our sample, there is no perfect comparison. We instead provide a range of estimates: our firms accounted for 20.8% of the total employment in manufacturing as reported in the 1929 Census of Manufactures (Vol. 1, chapter 2, Table 1), and for 6.9% of the total civilian private non-farm labor force in 1928 (Historical Statistics of the United States, Table Ba470–477).

Another indication of the relevance of our sample for the economy overall is that our relatively small number of firms accounted for 19.1% of the total assets of all corporations in the relevant sectors reporting this information in their income-tax returns—more than 250 thousand firms.[24] Combined with the statistics for aggregate employment presented above, this measure suggests that our firms represented a central part of the American economy.

Much of our analysis hinges on the importance of financial frictions. Our identification strategy relies on the existence of (long-term) corporate bonds that matured during the worst years of the crisis. Although the summary statistics in Table 1 show that these sources of debt financing were important for the firms in our sample, one concern might be that smaller firms in the economy may not have made use of these instruments. We find that bonds and mortgages accounted for about 8% of all assets of corporations with income-tax returns, exactly the same fraction that was evidenced for the average firm in our sample. Thus, access to public debt markets was arguably important for business enterprises more generally. However, total debt was a bit higher relative to assets for all corporations in the relevant sectors. This suggests that short-term debt (measured by notes payable) and/or trade credit (measured by accounts payable) were more important sources of financing for smaller firms in comparable industrial sectors.

Next, we use data sources other than the *Moody's* manuals to validate the accuracy of the employment data. Since the firms in our sample were among the largest in the economy, we do not necessarily expect them to suffer equally as others. But the severity of the recession

[24]To obtain a reasonable comparison for the firms in our sample, we focus on all corporations submitting a tax return in the following sectors: mining and quarrying, manufacturing, construction, trade, and services. About 89.2% of the firms with a filed return in these sectors reported balance sheet information, as reported in Table 19, p. 380, of the 1929 Statistics of Income.

varied greatly across regions and sectors. We would thus expect the geographic and industrial distribution of employment changes in our sample to be relatively similar to those experienced by other industrial firms.

Panel A of Figure 3 compares the geographic variation in the changes in employment across U.S. regions in our sample (displayed in the vertical axis) to the drop in employment in manufacturing establishments as reported in the Census of Manufactures (presented in the horizontal axis). The census data are based on information at the region level for 20 manufacturing industries constructed by Rosenbloom and Sundstrom (1999). The Figure shows a clear positive association across the two data sources, with the largest percentage declines in employment occurring in the East South Central states and the smallest ones taking place in the South Atlantic and the North East. Not surprisingly, the decline in employment was smaller among the firms in our sample: estimating a simple regression line in Panel A results in a slope coefficient of 0.46, which suggests that the contraction in employment in our sample was about half of the overall employment drop among the establishments in the Rosenbloom and Sundstrom (1999) data. The smaller decline in employment evidenced by the firms in our sample is not driven by differences in the geographic location of our large firms and the manufacturing establishments. Panel B of Figure 3 presents the (log) number of firms located in each region in our sample (in the vertical axis) and the (log) number of establishments in the Census of Manufactures (in the horizontal axis). Although there are fewer business units in our sample, the geographic distribution of firms and establishments is positively correlated. Still, our specifications control for state fixed effects to address differences in the magnitude of the economic shocks across the country.

What factors, then, may account for the differences in the magnitude of the shocks between these two data sources? First, the typical firm in our data is much larger than the average establishment in the census and may therefore have been more resilient to the financial crisis. Second, our sample includes industrial firms, whereas the Census data covers only establishments operating in manufacturing. The manufacturing sector is *in general* more procyclical than the other industries included in our sample. Moreover, the Great Depression did not affect all industries equally. To provide further validating evidence for our data, in Table A.3 we present the variation in employment decline across industries. Reassuringly, we find that firms in manufacturing—especially those producing durable goods—were among the hardest hit by the Great Depression. For example, firms in electrical equipment, mining and construction collectively shed more than 50% of their employees. By contrast, firms in services and agriculture were generally less affected. These estimates are consistent with the well-documented large decline in consumer demand for durable goods early in the crisis

(Romer, 1993). To ensure that our effects are not driven by cross-sectional differences across industries, we include industry effects in our specifications.

2.4 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 also control for 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 and statistically significant correlation between the employment change between 1928 and 1933 and the level of the firm's leverage in 1928. This correlation is robust across 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^{th} percentile of leverage in 1928 experienced a decline in employment from 1928 to 1933 of about 0.13 to 0.15 log points larger than the change in employment of a firm with median leverage. The magnitude of this association becomes a bit smaller at 0.09 to 0.10 log points, but remains significant when we also control for the firm's profitability (in Column (7)), and for firm age (in Column (8)). These results are consistent with the idea 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. The documented correlation between leverage and the contraction of employment during the Great Depression is also 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 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, especially when the firms' book value of assets is included as a control. For example, firms in the 90th percentile of employment in 1928 reduced employment between 0.07 and 0.3 log points more than the median firm, suggesting that perhaps these firms employed more labor than they should have before the onset of the crisis. By contrast, firms that were larger (as measured by total assets) in 1928 did not reduce their employment as much as smaller firms. For example, firms in the 90th percentile of book assets in 1928 contracted employment between 0.21 and 0.28 log points *less* than the median-sized firm. Our data therefore corroborate the perception that large firms did indeed suffer 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 very 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. For example, the median firm in the 1928 distribution of ROA reduced employment by approximately 0.20 log points more than those firms in the 90th percentile. 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 clearly endogenous. These associations thus cannot be interpreted as evidence of a causal effect of financing on employment decisions.[25] Next, we present an identification strategy to more credibly estimate the effect of financial frictions on firm employment.

[25]That high leverage levels likely had negative consequences during the crisis 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 during the Depression. 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, as suggested by Graham et al. (2011), our study may underestimate the overall impact of frictions on the aggregate contraction in employment.

3 The Effect of Financial Constraints on Employment

To assess the role of financial constraints on employment, we first apply the long-term debt approach in Section 3.1. In Section 3.2 we study the direct effects of local bank failures. We discuss our main results based on the substitutability of public and private debt in Section 3.3 and present a battery of robustness checks for this analysis in Section 3.4.

3.1 Maturing Long-term Debt

To identify the effect of financial constraints on employment decisions, 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 had to reduce their labor force by more than those firms not facing the need to refinance maturing long-term debt. The difficulties in rolling over corporate bonds were likely acute during the period of our study, since external capital markets were essentially frozen. Firms with a higher fraction of maturing debt likely experienced difficulties in borrowing to pay financial liabilities and wages, resulting in larger reductions in firm employment and, ultimately, in a contraction in aggregate economic activity.

Comparison across Treatment and Control Groups

In our main specification, 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. Before we proceed to the regression results, we present 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. Some of these differences are perhaps not surprising—firms with higher leverage overall should be also more likely to have bonds due in any given year. Therefore, 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. Given these differences, we include these characteristics in our regression analysis, and perform various robustness checks to address differences in initial indebtedness levels across firms.

Regression Analysis

Our main 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 dependent variable is the log difference in the number of employees E for firm i between 1928 and 1933. 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. Thus, the results presented thus far may simply reflect that highly levered firms fared worse during the Depression, regardless of their need to refinance. 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.

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. To address this concern, in Column (8) we control for the firm's ROA in 1928. As expected, firms that were more profitable before the Depression experienced smaller reductions in the number of employees. But importantly, the estimated effect of *BondsDue* on firm employment remains unaffected.[26] 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 -1.1 and -1.5 across specifications. These estimated effects imply that a one standard deviation increase in *BondsDue* is associated with a decline in the number of employees that is between 3.9% and 5.3%, representing about 16% to 22% 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 4% of its assets, experienced a contraction in employment that was 4.4% to 5.6% larger than the decline in the number of employees of the median firm, which had no bonds maturing from 1930 to 1934.

In the Supplementary Appendix (Section 2.1), 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 or by omitted characteristics correlated with the level and structure of the firms' leverage. But one remaining potential 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

[26] Interestingly, the effect of leverage becomes smaller and statistically insignificant when we control for ROA. This finding highlights the importance of properly controlling for a variety of firm or establishment characteristics, and cautions against interpreting associations between leverage ratios and firm outcomes as a causal effect of financing frictions on the economy.

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). Although we have made strides within the constraints of the available data to ensure that unobserved firm characteristics are not driving the results, our strategies thus far can address this concern only in part. Next, we exploit the collapse of the banking system and the widespread suspension of banks across many areas of the country in the early 1930s as an additional source of variation to further validate the importance of access to finance on firm employment.

3.2 Local Banking Conditions and Firm Employment

As we discussed in Section 1.2, our second empirical strategy is based on the substitution between private and public debt markets. Before we proceed with that analysis, we briefly study the direct effects of disruptions to the local banking systems on firm employment. Suspensions of national banks were fairly widespread—about 687 firms were located in counties in which at least one national bank suspended from 1929 to 1933, while the remaining 323 firms had their primary location in counties with no such failures. Appendix Table A.5 presents a simple comparison in means and medians for all variables of interest between these two groups of firms. Firms in counties with suspended national banks tended to be somewhat larger and had slightly higher leverage (mean leverage of 13.5% versus 11.2%) before the crisis. However, there were no distinguishable differences across most characteristics, including their employment level and profitability. Importantly, these two groups of firms did not differ in the value of bonds becoming due between 1930 and 1934, suggesting that the level of maturing debt was uncorrelated with the health of the local banking system.

To estimate the direct effects of disruptions of the local bank systems more formally, we compare the response of employment growth of firms located in counties that experienced the failure of at least one national bank relative to firms that were located in counties with no such bank failures. Specifically, we estimate a variant of equation (2), above:

$$\log(E_{i,1933}) - \log(E_{i,1928}) = \alpha + \beta\, BankFail_i + \lambda\, \mathbf{X}_{i,1928} + \gamma\, \mathbf{k}_i + \psi\, \mathbf{s}_i + \epsilon_i, \qquad (3)$$

where we replace the value of maturing debt by a discrete treatment, $BankFail_i$, a dummy variable that takes a value of one if at least one national bank suspended from 1929 to 1933 in the county in which firm i was located in 1928, and zero otherwise. We use the firm's location in 1928 to avoid contaminating the estimation by any endogenous firm reallocations due to geographic variation in industry shocks or in local bank conditions during the crisis. The main coefficient of interest, β, captures the sensitivity of firm-level employment to local bank failures.

Table 6 presents the results. We find a negative relation between national bank suspensions and employment changes, but the estimated effects are not always statistically different from zero. The magnitude of the statistically significant estimates in Columns (3) to (8) imply that firms located in counties where national banks failed experienced a contraction in employment that was 8.9% to 11.3% larger than the declines exhibited by similar firms located in counties with no such suspensions. However, the estimated effects become smaller (3.9% to 5.8%) and are no longer significant when we control for the firms' profitability in 1933 in Columns (9) and (10). This suggests that firms located in areas where banks failed were also more likely to be less profitable during the crisis, suggesting that local economic shocks that impaired both banks and firms may partly drive the association between credit disruptions and employment. The association between bank failures and employment changes is also sensitive to different ways of measuring the disruptions in local banking conditions. For example, the estimated effects are negative but quite imprecisely estimated when we use a continuous treatment of $BankFail$, defined as the amount of deposits in national banks that failed in the county between 1929 and 1933, scaled by the amount of deposits in these banks in 1928 (results not shown).

In sum, we find some evidence that failures of local banks had a direct effect on the employment decisions of the large firms in our sample, though the effects are not robust to controlling for profitability during the crisis or to alternative measures of bank shocks. Next, we study whether disruptions in local credit markets instead had an indirect effect, by impeding the substitution between public and private sources of debt financing.

3.3 Interaction of Local Credit Conditions and Firm Financing Needs

To better identify the effects of financial constraints on employment, we focus on the interaction between the variation in the firms' maturing debt and the geographic differences in bank failures. We conjecture that firms that had high levels of bonds maturing when the public bond markets stopped functioning would have found it particularly difficult to refinance those debts when they were located in areas that experienced bank failures. This strategy effectively compares the employment decisions of firms that had similar amounts of maturing bonds but varied in their ability to access bank credit simply because they happened to be located in areas where banks were in distress. It is unlikely that unobserved firm characteristics that were correlated with both the amount of debt maturing and changes in employment differed by firm location. Thus, this strategy partly helps address concerns that our findings may be driven by unobserved firm characteristics.

To describe the data, we 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 7 presents summary statistics for the variables of interest for these four groups of firms. Because our empirical strategy focuses on the interaction between maturing debt and bank failures, 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: 15.1% of firms located in counties where $BankFail = 1$ had maturing bonds ($BondsDue > 0$), whereas this ratio was 13.6% for firms operating in places with no failures ($BankFail = 0$). The similarity in 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 also similar on observables. Table 7 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 7 presents summary statistics for the changes in the firms' observable characteristics from 1927 and 1928 for all four groups of firms. 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.

Our regression model is essentially a "triple-difference" specification, in which we compare employment between 1928 and 1933, across firms located in counties that experienced national bank suspensions relative to those that did not, interacted with the firms' level of bonds maturing during the crisis. Specifically, we estimate:

$$\begin{aligned}\log(E_{i,1933}) - \log(E_{i,1928}) =& \alpha + \beta_1\, BankFail_i + \beta_2\, BondsDue_i + \\ &+ \beta_3\, BankFail_i \times BondsDue_i + \lambda\, \mathbf{X}_{i,1928} + \gamma\, \mathbf{k}_i + \\ &+ \psi\, \mathbf{s}_i + \epsilon_i. \end{aligned} \tag{4}$$

The main difference from our earlier specifications is that we now include $BankFail_i$, $BondsDue$, and their interaction. The main coefficient of interest is β_3, which allows us to compare the effects of having similar levels of maturing debt for firms facing different levels of disruption in their local banking systems. We expect this coefficient to be negative: firms that had larger amounts of preexisting bonds maturing should have had more difficulties in obtaining bank financing (and therefore been less likely to keep their workforce) when their local banks were under distress.

Table 8 presents the results. 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^{th} percentile of maturing bonds outstanding (as a fraction of total assets) experienced a 12% to 18% 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. 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.

In sum, we find that financial frictions had a sizable impact on the employment decisions of large firms during the Great Depression. Our results suggest that the ability to substitute public debt for private debt may have helped firms to ease financial shocks, and conserve employment.

3.4 Robustness

In this section we perform a number of robustness checks to further validate our results. Thus far, we have primarily exploited the continuous treatment given by the value of maturing debt relative to the firm's assets for identification. 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 split the treated firms with maturing bonds into three groups. 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 $BankFail_i$. As before, the excluded category are those firms with no maturing bonds.

Table 9 presents the estimated effects for these interactions. The coefficients presented in the first row imply that the economic magnitudes are substantial: firms located in cities with national bank suspensions that had a positive value of debt due during the crisis experienced a 21% to 25% greater drop in employment relative to firms also located in counties that experienced bank distress but had no bonds due from 1930 to 1934. The magnitude of this effect is comparable to the mean drop in employment across all firms in the sample. Further, the estimated effects are more pronounced for higher values of maturing debt. Firms that were located in counties with failed national banks experienced an additional contraction in employment of 24& to 34% (or 43% to 57%), depending on the specification, when they had to refinance more than 5% (or 10%) of their assets, relative to similar firms. These magnitudes are quite large, most likely because the estimates are based on a small number of firms on the end tail of the distribution of $BondsDue$. Indeed, only 62 firms located in cities with suspended national banks had to refinance more than 5% of their assets, and 31 firms had bonds maturing for more than 10% of their assets. With this caveat in mind, the positive gradient in the effects of maturing bonds on employment presented in Table 9 minimizes concerns related to the low leverage levels that characterized corporations during our sample period. In Appendix Table A.6 we also show that our baseline results are robust to excluding from the analysis those firms that had no leverage in 1928. Since leverage ratios were overall low (relative to modern firms), the typical value of maturing bonds was not exceedingly high. Yet our results suggest that even modest amounts of bonds becoming due represented a significant financial burden for large firms during the Great Depression, and that those burdens were particularly severe when the needs for debt refinancing were large (that is, more than 10% of assets). Given the restrictions imposed by our sample size, for other robustness checks we return to our baseline specification using the continuous treatment variable to measure $BondsDue$.

Table 10 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. Our baseline measure therefore captures any reductions in employment by the end of 1933 that may occur in anticipation of these cash outflows in 1934. 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 -3.6. This suggests that the effect from financing needs on employment was particularly severe from 1930 to 1933, when the banking system experienced the most strain. In Panel B of Appendix Table A.6, we show that our results are also robust to netting out the firms' available cash and marketable securities in 1928, which could have been used to pay down maturing liabilities, from the value of bonds becoming due. This finding refutes the current view that the financial turmoil mostly affected small firms, since 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 in a difference-in-difference design. Our empirical strategy based on the substitution between private and public debt partly appeases these concerns; next, we perform two additional exercises to address them further. First, 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 10 indeed shows no such correlation, although the estimates are noisy due to the small number of firms that had bonds maturing in 1928. These results provide at least suggestive evidence that our main findings are unlikely to be solely the result of having maturing debt in any period. Instead,

financial constraints appear to matter only during the crisis. Yet it is still possible that our results are driven by unobserved firm characteristics that become salient during the crisis. For example, 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 8, ranging from -2.5 to -3.2 across specifications, and all but one are statistically significant at conventional levels.

Finally, our baseline estimates use a discrete definition of bank failures. However, it is possible that the disruption in local banking conditions would be more severe when the national banks that failed accounted for a larger share of deposits. To address this concern, 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. Panel E of Table 10 shows that the estimates of β_3 remain sizable and statistically significant across our specifications.

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 these firms also happened to be 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 9% to 15% depending on the treatment definition). 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 during the Great Depression in several 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. In Section 4.2 we use the same estimated elasticity, but expand the definition of treatment to include other types of short-term leverage, even if they

are potentially endogenous. In Section 4.3 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. Finally, in Section 4.4 we compare these estimates to those obtained from the recent financial crisis using the empirical strategy in Benmelech et al. (2011).

4.1 Evaluating the direct effect

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

We first examine the definition of treated firms as those having maturing debt in 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

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

where $\hat{c} \, Z_{it}$ includes all the other variables in equation (2). All the estimates (the main coefficient of interest $\hat{\beta}_1$ plus the coefficients on the remaining variables and fixed effects $\hat{c}$) are computed using the specification in Column (10) of Table 4, which includes all controls and fixed effects. The sum is computed over the 785 firms that are included in this specification.

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

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

As before, we use the point estimates corresponding to the specification in Column (10) of Table 8, which includes all controls and fixed effects.

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

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

Panel A.i of Table 11 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 8 is equal to 8.6%. Using the estimates from equation (2), we find that the direct treatment effect is equal to 0.8 percentage points, or approximately 10% of the overall drop in employment. If we instead define the set of treated firms as those that had maturing debt and were located in counties with failed national banks – equation (4) – the magnitude of the direct treatment effect implies an aggregate drop of 1.4 percentage points in employment – or approximately 17% of the overall drop.

4.2 Extrapolating to firms with endogenous short-term leverage

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. Next, we assume that our estimated elasticity also applies to short-term interest-bearing debt that needed to be refinanced during the crisis.

We measure "short-term" sources of debt financing by the value of notes, bills, and loans payable in 1928 as a fraction of the firms' assets in that year. This item typically included bank loans and was often (but not always) listed as a short-term liability in firms' balance sheets. Since we do not have information on their exact maturity, we make different assumptions on the fraction of these loans that were due in the 1929–33 period. Our most conservative estimate assumes that 20% of them matured during the crisis, which is equal to the average fraction of bonds in the firms' balance sheets in 1928 that were due during the crisis. This is a conservative estimate because bank loans typically had shorter maturities than bonds (Jacoby and Saulnier, 1947). We therefore also consider the case in which 50% of the existing notes payable in 1928 matured during the crisis. Since firms could use some of their cash (or marketable securities) to finance these liabilities, in both cases we calculate net short-term debt due by subtracting the value of cash and marketable securities in 1928. We then recalculate our estimate of the aggregate contraction in employment due to financial constraints given by equation (7).

Panel A.i of Table 11 reports the results. If we use the estimates from equation (2), where treatment is defined solely by maturing debt, then the direct effect of financial frictions on employment is about 1 to 1.5 percentage points. Focusing on the estimates from our preferred specification that exploits the interaction of bank failures and firm financing needs (now defined by maturing bonds and short-term debt), the aggregate drop in employment due to

financing frictions increases to between 1.5 and 2.4 percentage points. These are substantial magnitudes, accounting for approximately 17% to 28% of the overall drop in employment in our sample.

The analysis in this section is predicated on the assumption that the causal estimates of equations (2) and (4) are also valid for maturing bank loans. In effect, we are assuming that public debt and bank loans were perfect substitutes. This is a big assumption, and these results should hence be interpreted with caution. If loans are easier to renegotiate than public debt, then the elasticity of employment to maturing *bank* debt would arguably be lower, and we would thus be overestimating the effect of financial frictions. For these reasons, we choose to exclude trade credit from these calculations. Trade credit was typically short-term, and firms would have had to refinance it during the crisis. But firms may more easily have extended the terms under which they would borrow from their suppliers than roll over their maturing bonds.

4.3 Interpreting the estimates through a structural model

The analysis in the previous sections abstracts from the possibility that firms without maturing debt could also have been adversely affected by the collapse of the financial sector during the Great Depression. In particular, firms may have reduced their labor demand if their current cashflows were insufficient to cover their hiring costs, even if they did not have any debt maturing during the financial crisis.

We present a a simple structural model of firm employment with financial frictions. Our model allows us to examine the impact of financial frictions on all firms in the sample, regardless of whether they had maturing debts. Specifically, firms in our model produce output using labor and are subject to a productivity shock. The two key assumptions in our model are, first, that labor needs to be paid before profits are realized and, second, that the firm faces convex costs of external finance. 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—the costs of external finance. To conserve space, we relegate the description of the model and details on its calibration to the Appendix.

We use the model to examine how much employment in 1933 would have been in the absence of financial frictions. Panel A.ii of Table 11 summarizes our findings. Our calibration implies that eliminating the financing friction would result in a 1.6 to 2.8 percentage-point increase in the overall level of employment, depending on whether we target an elasticity of

employment to debt equal to -1.16 or -3.1, corresponding to the estimates from equations (2) and (4), respectively. Thus, the estimates using our main specification imply that financing frictions accounted for approximately one-third of the overall drop in employment. These estimated effects are larger relative to those in Section 4.1 because the nature of the experiment is now different. Note that, the set of firms affected by the financing constraints does not consist simply of firms that need to refinance their maturing debt. Firms with no maturing debt are also affected, in particular those whose current current level of profitability is insufficient to achieve their desired level of employment and must therefore access external funds. These estimates are based on fairly strong assumptions, and several caveats are in order. In the Appendix, we argue that these assumptions are in fact conservative and would likely lead us to underestimate the importance of financial frictions.

4.4 Comparison to the Great Recession

The results so far indicate that financing frictions played an important role in the contraction of employment during the Great Depression. Our framework can also be used to provide a comparative analysis of the Great Recession. We start by estimating the elasticity of employment to maturing debt as defined in equation (2). We follow closely the empirical design proposed by Benmelech et al. (2011). Thus, we define the outcome variable as the change in log employment between 2008 and 2009 among the publicly traded firms that appear in the Compustat dataset. The treatment variable is the amount of long-term debt that was issued in 2004 and that matured in 2009 (Compustat variable *dd5* lagged four years) scaled by the book value of assets in 2008. To the extent possible, we include the same set of controls as the specification presented in Column (10) of Table 4. (We are forced to omit firm age because Compustat does not have information on the year of incorporation.) We do caution readers to be careful in the interpretation of our analysis, since our ability to construct a close comparison is limited.

Our estimate of the coefficient β in equation (2) during the Great Recession is -0.22 with a standard error of 0.08. A firm in the 90^{th} percentile of the distribution of maturing debt, which was faced with the need to refinance debt for about 3.9% of its assets, experienced a contraction in employment that was 0.9% larger than the decline in the number of employees of the median firm, which had no long-term debt maturing during that period. Relative to our estimates from the Great Depression presented in Table 4, we find that the elasticity of employment to maturing debt was about four to five times larger in the 1928–1933 crisis than in the Great Recession.

We next repeat the analysis in Section 4.1 to compute the aggregate direct effect in the 2008-09 crisis using equations (5) and (7). Panel B.i of Table 11 summarizes the results. The overall drop in employment between 2008 and 2009 among the 2,449 firms in the Compustat sample is 4.9%. The direct effect of maturing debt accounts for a drop of 0.4 percentage points in employment among the firms in the sample, or approximately 9% of the overall drop. Thus, direct effect accounts for approximately the same fraction of the overall contraction in employment in both crises (9% versus 10%), even though the size of the shock was two times larger in the Great Depression. One significant difference in the definition of treatment across the two crises is that the Compustat variable *dd5* includes the amount of *all* maturing long-term debt, whereas the baseline treatment for the Depression includes only maturing bonds. Hence, perhaps a closer point of comparison would be the analysis in Section 4.2, which evaluates the direct effect by extrapolating our estimates to other forms of short-term leverage. In this case, we find that the magnitude of the direct effect was two to six times larger in the 1930s.

Last, we use the structural model to compute the overall impact of financial frictions during the recent crisis. As we see in Panel B.ii of Table 11, our parameter estimates for the 2008–2009 crisis imply that financing frictions led to a 0.5 percentage-point reduction in overall employment, explaining about 10% of the total decline in the sample. By contrast, our model implies much larger effects during the Great Depression (1.6 percentage points and 19% of total, respectively).

5 Conclusion

This paper presents new evidence on the important effects that financial frictions had on the high levels of unemployment evidenced 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 provide alternative sources of external finance. Our aggregation exercises suggest that the aggregate level of employment in our sample would have been about 10% to 33% 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.

We also take a first step in producing a comparative analysis of the quantitative impact of financial frictions during the Great Depression and the Great Recession. Large firms with higher levels of maturing debts contracted employment more than their peers in both crises, but the magnitude of the adverse financing shock was about two to five times larger during the 1930s than it was in the 2008–2009 recession. 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.

Appendix: Model

Here, we present a simple structural model that allows us to quantify our empirical estimates for the importance of financial frictions.

Model Setup

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

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

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 liquidation value that is equal to $\bar{D}$.

Importantly, there is a mismatch in the time at which labor is hired and the time at which output is produced. That is, the labor that participates in production at time t in

equation (8) is decided at $t-1$. In deciding how many workers to hire, managers take into account their expectations about the firm's productivity at time t, which follows an AR(1) process,

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

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

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

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

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

As our baseline, we examine the case where the financing friction is purely transitory – it lasts for one period. Our focus is then on the actions of firms during the crisis $t = t'$, which inherit a stock of maturing debt D from the previous period and need to make a labor decision given a transitory shock to the costs of external finance ϕ. Our assumption that firms believe that the crisis is transitory, that is, $\phi = 0$ for $t > t'$, rules out any precautionary savings motive on the part of the firms. This is a conservative assumption that facilitates analytic tractability, which we relax as part of our robustness checks.

The firm's optimization problem can be written as,

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

where $\rho = 1/(1+r)$ is the firm's discount factor and $\hat{R} = 1 + (1-\tau)r$ is the cost of debt net of the tax shield. The firm's problem is subject to the constraint of no equity issuance,

$$D' = \left((1-\tau) \left(w\, L' - e^z L^\beta \right) + \hat{R}\, D \right)^+, \tag{12}$$

and the restriction that debt should be riskless, $D' \leq \bar{D}$.

Equation (12) implies that firms issue debt to finance any shortfalls. If after the labor decision there are surplus funds, these are paid out to equity owners. Since firms believe that there are no credit market distortions ($\phi = 0$) for $t > t'$, and assuming that $\bar{D}$ is sufficiently high, firms have no incentive to save. When solving the version of the model with permanent shocks, we relax this assumption and allow D to be negative.

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

$$w\left[\rho\,\hat{R}+\phi\left(\hat{R}\,D+(1-\tau)\left(wL'-e^{z}L'^{\beta}\right)\right)^{+}\right]=\beta\,\rho\,E[e^{z'}|z]\,L'^{\beta-1}. \tag{13}$$

The first order condition reveals that the presence of financial frictions increases the marginal cost of hiring workers. Since the costs of issuance are quadratic, the marginal cost of issuance is increasing with the amount of external finance needed. Having an analytic solution allows us to compute the elasticity of labor to the financing friction. Focusing on the region where the financing constraint is binding – firms want to issue new debt $D'>0$ – an implicit differentiation of equation (13) with respect to the financing friction ϕ yields

$$\frac{\partial \log L'}{\partial \phi}=-w\,\frac{\hat{R}D+(1-\tau)\left(w\,L'-e^{z}L'^{\beta}\right)}{\beta\rho\,(1-\beta)\,L'^{\beta-1}\,E[e^{z'}|z]+\phi\,(1-\tau)\,w^{2}\,L'}<0. \tag{14}$$

The elasticity that is presented in equation (14) is what we would ideally like to estimate: it summarizes the impact of the financial friction on the firm's overall employment. However, estimating this elasticity directly from the data is not feasible. Instead, our analysis allows us to obtain an estimate of the elasticity of labor with respect to maturing debt $F=\hat{R}\,D$. To obtain an expression for this elasticity, we implicitly differentiate equation (13) with respect to the face value of maturing debt F, which yields the following expression:

$$\frac{\partial \log L'}{\partial F}=-\frac{w\,\phi}{\beta\rho\,(1-\beta)\,L'^{\beta-1}\,E[e^{z'}|z]+\phi\,(1-\tau)\,w^{2}\,L'}<0. \tag{15}$$

This elasticity, which corresponds to our empirical estimates, is negative. More important, this elasticity is informative about the parameter governing the financial friction: the magnitude of equation (15) is monotonically increasing with ϕ. This fact is important, because it allows us to recover the severity of the financial friction ϕ in the model from the empirical estimates of equation (15).[27]

Model Simulation and Calibration

We use the model to examine how much employment in 1933 would have been in the absence of financial frictions ($\phi=0$). We first fix a set of parameters $\Theta=[\kappa,R,\beta,w,\phi]$. We simulate a panel of firms that make a hiring decision during the crisis. We simulate firms for

[27]The result that equation (15) is unambiguously increasing in ϕ may appear to be in contrast to the result in Kaplan and Zingales (1997) and Hennessy and Whited (2007), which show that the relation between the sensitivity of investment to cash flow and the parameters affecting the costs of external finance are ambiguous. However, the difference results from the fact that we are focusing on the elasticity with respect to maturing debt – as opposed to cash flows. In standard models, operating cash flows are correlated with firm's investment opportunities, which makes the sensitivity of investment to cashflows a non-monotone – and in many cases decreasing – function of the severity of financial constraints. By contrast, maturing debt is independent of the firm's investment opportunities.

two periods, $t = 0$ and $t = 1$. Since the model has no capital, we scale the dollar amount of bonds due and the number of employees in the data by book assets. We treat these as equivalent to D and L in the model. We use the empirical joint distribution of bonds due D and labor L to simulate N=50,000 firms. Hence, firms start with an exogenous level of D_0 and L_0.

We next use the observed cross-sectional dispersion in employment-to-assets in 1928 (L) to infer an initial value of idiosyncratic productivity $z_{i,-1}$. Specifically, we assume that labor was chosen before 1928 using equation (13) without any financial frictions, $\phi = 0$. After taking logs of equation (13), we obtain

$$(1-\beta)\log L_{i,0} = \log\frac{\beta}{wR} + \kappa z_{i,-1} + \frac{1}{2}\sigma_z^2. \tag{16}$$

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

$$z_{i,-1} = \frac{1-\beta}{\kappa}\left(\log L_{i,0} - \frac{1}{N}\sum_i \log L_{i,0}\right). \tag{17}$$

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

We next simulate the firm's labor choice during the crisis period taking the variables z_0, L_0, and D_0 as given. Given the level of ϕ, we obtain the firm's labor choice in the second period L_1 using equation (13). Given the firm's labor choice, we then estimate the equivalent of equation (2) in simulated data,

$$\log L_{i,1} - \log L_{i,0} = a + \beta D_{i,0} + \rho \log L_{i,0} + c\,\pi_{i,0} + \varepsilon_i, \tag{18}$$

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

Table M.1: Calibration targets

Moment	Data	Model		
		A	AxB	Great Recession
Elasticity of labor to maturing debt				
Treatment A	-1.164	-1.164	-	-
Treatment AxB	-3.072	-	-3.072	-
Great Recession	-0.22	-	-	-0.22
Median ROA, 1928	0.071	0.070	0.071	-
IQR ROA, 1928	0.085	0.087	0.086	-
Persistence in employment 1928–933	0.877	0.877	0.877	-
Median ROA, 2008	0.065	-	-	0.055
IQR ROA, 2008	0.065	-	-	0.075
Persistence in employment 2008–2009	0.945	-	-	0.945

The last step involves calibrating the remaining parameters of the model. We choose a real interest rate of 5%, which is equal to the average real interest rate during the 1929-1934 period. We set the corporate tax rate τ at 12 %, which is equal to the federal tax rate on corporate income in 1928. We choose $\bar{D} = 1$, which ensures that the constraint is almost never binding in our simulations. We choose the remaining parameters $[\kappa, \beta, w, \phi]$ to match the following four moments in the data: (a) the estimated elasticity of employment change to bonds due – the estimated coefficients β_1 and β_3 in equations (2)-(4) that includes the full set of controls (corresponding to Column (10) in Tables 4-8); (b) the correlation in log employment (scaled by assets) between 1928 and 1933; (c) the median level of profitability (scaled by assets) in 1928; and (d) the interquartile dispersion in profitability in 1928; last, we clear the labor market under the assumption $\phi = 0$. The last assumption ensures that aggregate employment would have remained constant between $t = 0$ and $t = 1$ in the absence of financial frictions ($\phi = 0$). This last condition determines the wage w so that it would clear the market in the absence of frictions. Effectively, we are assuming that wages remained constant in their pre-crisis, market clearing level. The estimated costs of external finance ϕ imply that firms which choose to issue debt in 1928–1933 incurred additional costs that were on average equal to 5.5% to 17% of their profits. The remaining parameters are largely in line with typical parametrizations in the literature. To conserve space, we refer the reader to Tables M.1 and M.2 show the calibration targets and parameters, respectively.

Table M.2: Parameters

Parameter	Symbol	Value		
		A	AxB	Great Recession
Persistence of Firm Shock	κ	0.864	0.867	0.945
Volatility of Firm Shock	σ	0.122	0.122	0.098
Decreasing returns to scale	β	0.740	0.740	0.740
Real interest rate	r	0.050	0.050	0.010
Wage	w	1.182	1.182	1.312
Financing friction	ϕ	0.621	2.675	0.081
Corporate tax rate	τ	0.120	0.120	0.350

Discussion and Caveats

The estimates in Panel A.ii of Table 11 are based on fairly strong assumptions, so several caveats are in order. In the interest of simplicity, our simple model abstracts from many relevant features. An incomplete list includes: (a) there is no capital; (b) there are no fixed costs of production; (c) our production function delivers a very tight link between current cash flows – which depends on $Z = \exp(z)$ – and the return to hiring employees – which is proportional to Z^{κ}; (d) there are no adjustment costs to labor; (e) wages in the model are constant; and (f) the financing friction is transitory and lasts for only one period.

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

the fall in employment in the model. However, this assumption is consistent with the data: between 1929 and 1933, wages actually *increased* in real terms by approximately 4%.

Last, the assumption that the financing shock is transitory (f) also leads to conservative estimates and is perhaps the most unrealistic. To evaluate the importance of this assumption, we numerically solve and calibrate an extended version of the model where we allow firms to borrow and save at a constant rate R; external borrowing is subject to a constant financing cost ϕ; firms can borrow up to the value of their collateral (equal to $\bar{D}$); if equity holders default, debt holders seize the collateral. In this version of the model, firms anticipate that the financing friction will exist permanently, which leads to more conservative hiring decisions. We perform the same calibration exercise using the extended model. Depending on the value of the elasticity of labor to maturing debt that we target, our estimates indicate that removing the financing friction would increase the overall level of employment by 2.1% to 3.5%, depending on the value of the estimated elasticity that we target.

Comparison to the 2008–09 crisis

When calibrating our model to the recent crisis, we choose a corporate tax rate of 35% and a real interest rate of 1%. The remaining parameters are calibrated using the same procedure as in Section 4.3 and are displayed in Table M.2.

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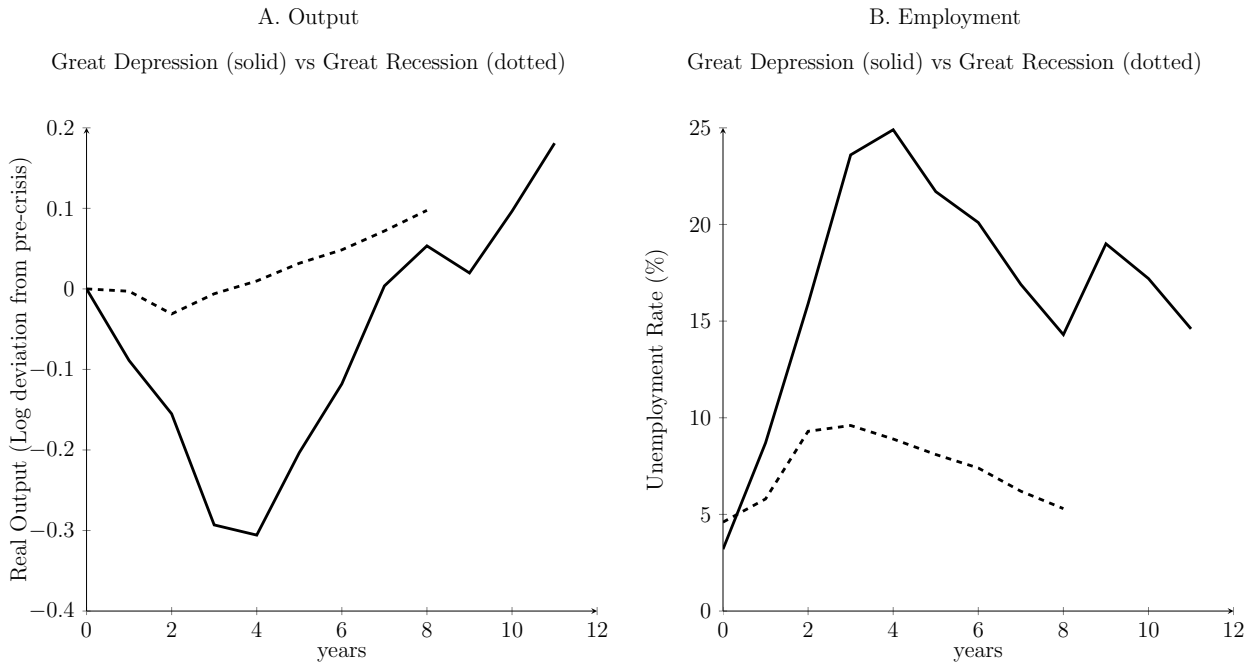
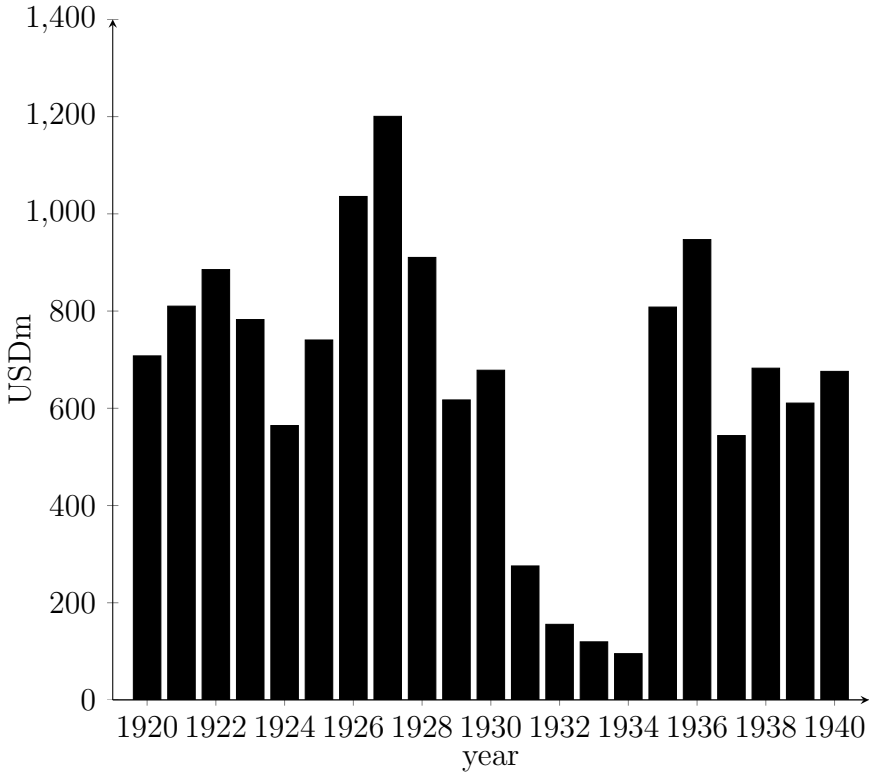
Tables and Figures

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



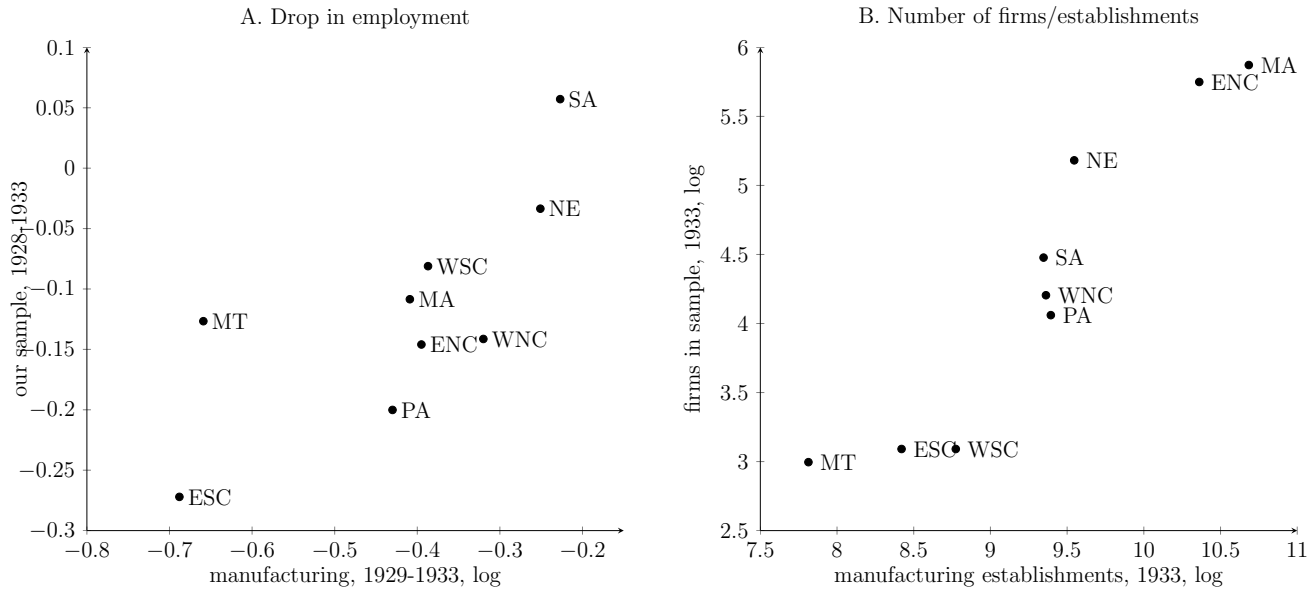
Source: Output is real GDP in chained dollars, obtained from NIPA Table 1.1.6, row 1. The modern series for unemployment is from the Bureau of Labor Statistics. The historical unemployment data are obtained from Margo (1993) and are based on Lebergott's series, which counts persons on work relief as 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).

Figure 3: Employment change across regions: comparing *Moody's* with the Census of Manufacturers



Panel A relates the aggregate change in employment from 1928 to 1933 in our sample of 1,010 firms (displayed on the y-axis) to the aggregate change in employment observed in data from the Census of Manufactures from 1929 to 1933 (displayed on the x-axis), across census regions. The contraction in employment in manufacturing is obtained from Table 7 of Rosenbloom and Sundstrom (1999), and it is based on all establishments for 20 selected manufacturing industries. Panel B compares the geographic distribution of the firms in our sample (displayed on the y-axis) with the location of manufacturing establishments in 1933 (displayed on the x-axis). The location in our data is based on the state for the primary address of the firm. Data on the number of manufacturing establishments in each region is constructed from the variable "mfgest" available in the dataset created by Fishback, Troesken, Kollmann, Haines, Rhode, and Thomasson (2011).

Table 1: Summary statistics

	N	Mean	SD	p5	p10	p25	p50	p75	p90	p95
Employment, log, change (1928-1933)	1010	-0.24	0.60	-1.32	-0.98	-0.52	-0.18	0.12	0.44	0.66
Employment, log, 1928	1010	6.72	1.33	4.58	5.14	5.91	6.74	7.60	8.39	8.88
Employment, log, 1933	1010	6.48	1.46	4.09	4.64	5.54	6.55	7.39	8.28	8.84
Profitability, 1928	824	0.09	0.07	-0.01	0.01	0.04	0.07	0.12	0.19	0.26
Profitability, 1933	843	0.01	0.07	-0.13	-0.08	-0.03	0.01	0.05	0.09	0.12
Book assets, log, 1928	1010	15.57	1.19	13.79	14.12	14.70	15.50	16.34	17.15	17.78
Book assets, log, 1933	1010	15.35	1.26	13.46	13.78	14.42	15.23	16.10	17.03	17.76
Leverage, 1928	1010	0.13	0.14	0.00	0.00	0.00	0.07	0.22	0.35	0.41
Leverage, bonds only, 1928	1010	0.08	0.13	0.00	0.00	0.00	0.00	0.16	0.29	0.35
Leverage, 1933	1010	0.12	0.15	0.00	0.00	0.00	0.05	0.19	0.34	0.45
Leverage, bonds only, 1933	1010	0.08	0.14	0.00	0.00	0.00	0.00	0.14	0.29	0.40
Firm age, 1933	1007	21.91	14.69	5	6	9	19	31	42	51
Bonds due (1930-34)	1010	0.01	0.04	0	0	0	0	0	0.04	0.09
Number of suspended national banks (1929–1933)	1010	5.83	9.12	0	0	0	2	8	20	34
Fraction of suspended national banks (1929–1933)	1010	0.22	0.22	0	0	0	0.14	0.33	0.55	0.67
Fraction of suspended national banks, deposit-weighted (1929–1933)	1010	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. 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}	-0.356**	-0.440***	-0.454***	-0.466***	-0.521***	-0.515**	-0.315*	-0.350**
	(0.170)	(0.151)	(0.162)	(0.158)	(0.174)	(0.192)	(0.167)	(0.161)
$\log E_{1928}$				-0.038**	-0.145***	-0.146***	-0.190***	-0.187***
				(0.016)	(0.037)	(0.041)	(0.048)	(0.046)
$\log \text{Assets}_{1928}$					0.137***	0.131***	0.177***	0.174***
					(0.037)	(0.040)	(0.048)	(0.048)
$\text{Profitability}_{1928}$							1.835***	1.729***
							(0.461)	(0.415)
$\log Age$								-0.041
								(0.041)
Observations	1010	1010	1010	1010	1010	1010	824	821
R^2	0.007	0.136	0.175	0.180	0.201	0.250	0.317	0.317
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	6.73	6.80	6.67	6.54	0.59	0.09
Profitability, 1928	0.09	0.08	0.07	0.06	0.01	0.05
Book assets, log, 1928	15.56	15.50	15.63	15.50	0.55	0.91
Leverage,1928	0.11	0.05	0.22	0.22	0.01	0.01
Firm age, log	2.83	2.94	2.93	3.00	0.09	0.63
Firms	862		148			
Panel B. Firms with non-zero leverage in 1928						
Employment, log, 1928	6.79	6.86	6.63	6.51	0.19	0.09
Profitability, 1928	0.07	0.06	0.07	0.06	0.93	0.34
Book assets, log, 1928	15.64	15.59	15.62	15.44	0.83	0.27
Leverage,1928	0.17	0.13	0.23	0.22	0.00	0.00
Firm age, log	2.83	2.94	2.91	2.89	0.22	0.62
Number of firms	559		139			

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)
$\text{BondsDue}_{1930-34}$	-1.418**	-1.235**	-1.496***	-1.114**	-1.156**	-1.193**	-1.175**	-1.175*	-1.256*	-1.164*
	(0.587)	(0.510)	(0.447)	(0.429)	(0.445)	(0.430)	(0.490)	(0.611)	(0.614)	(0.586)
Leverage_{1928}				-0.386**	-0.396**	-0.449**	-0.446**	-0.265	-0.154	-0.200
				(0.157)	(0.153)	(0.171)	(0.189)	(0.174)	(0.174)	(0.167)
$\log E_{1928}$					-0.039**	-0.146***	-0.149***	-0.194***	-0.166***	-0.162***
					(0.017)	(0.037)	(0.041)	(0.047)	(0.049)	(0.048)
$\log \text{Assets}_{1928}$						0.138***	0.133***	0.180***	0.136**	0.132**
						(0.037)	(0.041)	(0.048)	(0.051)	(0.051)
$\text{Profitability}_{1928}$								1.793***	1.140**	1.019**
								(0.452)	(0.463)	(0.425)
$\text{Profitability}_{1933}$									2.696***	2.700***
									(0.431)	(0.431)
$\log Age$										-0.052
										(0.031)
Observations	1010	1010	1010	1010	1010	1010	1010	824	788	785
R^2	0.007	0.131	0.171	0.178	0.184	0.205	0.254	0.320	0.400	0.402
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	536	0.061	0.000	102	0.069	0.000	0.70	1.00
Profitability, change	615	0.009	0.002	110	0.011	0.006	0.70	0.22
Book assets, log, change	763	0.053	0.025	136	0.059	0.025	0.68	0.97
Leverage, change	763	0.005	0.000	136	0.005	-0.007	0.97	0.15
B. Change, 1925–1928								
Employment, log, change	138	0.090	0.000	20	0.073	0.000	0.87	1.00
Profitability, change	409	-0.006	0.000	81	0.009	0.008	0.05	0.13
Book assets, log, change	555	0.095	0.056	99	0.105	0.068	0.74	0.69
Leverage, change	522	0.000	0.000	97	0.007	-0.011	0.63	0.54

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: The effect of bank failures on employment

$\log E_{1933} - \log E_{1928}$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BankFail	-0.055	-0.060	-0.091**	-0.088**	-0.088**	-0.086*	-0.107**	-0.116*	-0.044	-0.052
	(0.049)	(0.037)	(0.040)	(0.042)	(0.043)	(0.042)	(0.039)	(0.061)	(0.052)	(0.053)
Leverage_{1928}				-0.441***	-0.452***	-0.506***	-0.501**	-0.298*	-0.200	-0.239
				(0.158)	(0.155)	(0.170)	(0.187)	(0.163)	(0.164)	(0.160)
$\log E_{1928}$					-0.036**	-0.141***	-0.142***	-0.185***	-0.159***	-0.155***
					(0.016)	(0.037)	(0.041)	(0.047)	(0.048)	(0.047)
$\log \text{Assets}_{1928}$						0.135***	0.129***	0.173***	0.131**	0.127**
						(0.037)	(0.041)	(0.048)	(0.050)	(0.050)
$\text{Profitability}_{1928}$								1.794***	1.120**	1.014**
								(0.437)	(0.449)	(0.412)
$\text{Profitability}_{1933}$									2.710***	2.707***
									(0.434)	(0.434)
$\log Age$										-0.047
										(0.032)
Observations	1010	1010	1010	1010	1010	1010	1010	824	788	785
R^2	0.002	0.129	0.168	0.178	0.183	0.203	0.253	0.321	0.398	0.400
Fixed effects	-	S	S, I	S, I	S, I	S, I	S, IxR	S, IxR	S, IxR	S, IxR

The table reports the coefficients from regressions of the change in log employment (number of employees) from 1928 to 1933 on *BankFail*, 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. 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 7: 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	6.659	6.685	6.548	6.501	6.768	6.802	6.725	6.569	0.43	0.83
Profitability, 1928	0.084	0.066	0.083	0.071	0.093	0.080	0.065	0.062	0.15	0.22
Book Assets, log, 1928	15.434	15.354	15.568	15.307	15.619	15.579	15.652	15.550	0.71	0.47
Leverage,1928	0.095	0.035	0.216	0.210	0.120	0.059	0.218	0.218	0.95	0.97
Firm Age, log	2.888	3.045	2.867	2.740	2.790	2.890	2.955	3.091	0.46	0.11
Bonds Due, 1930-34	-	-	0.078	0.058	-	-	0.075	0.062	0.83	0.73
Number of firms	279		44		583		104			
Panel B. Pre-crisis trends: change from 1927 to 1928										
Employment, log	0.064	0.000	0.042	0.000	0.059	0.000	0.078	0.000	0.32	-
Profitability	0.004	0.001	0.007	0.013	0.011	0.004	0.013	0.006	0.62	0.42
Book Assets, log	0.043	0.020	0.064	0.030	0.058	0.028	0.057	0.024	0.79	0.78
Leverage	0.001	0.000	-0.009	-0.007	0.006	0.000	0.010	-0.008	0.12	0.87
Number of firms	246		41		517		96			

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 8: 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	-0.021	-0.028	-0.057	-0.053	-0.053	-0.051	-0.068	-0.081	-0.006	-0.015
	(0.054)	(0.039)	(0.044)	(0.046)	(0.046)	(0.046)	(0.044)	(0.070)	(0.054)	(0.054)
$\text{BondsDue}_{1930-34}$	0.642	0.749	0.434	0.897	0.873	0.898	1.034	0.741	0.956	0.972
	(0.889)	(0.699)	(0.711)	(0.714)	(0.710)	(0.643)	(0.749)	(0.842)	(0.815)	(0.780)
BankFail × $\text{BondsDue}_{1930-34}$	-3.001***	-2.895***	-2.783***	-2.897***	-2.921***	-3.011***	-3.126***	-2.673**	-3.158***	-3.072***
	(0.959)	(0.824)	(0.849)	(0.815)	(0.787)	(0.731)	(0.826)	(0.995)	(1.026)	(0.983)
Leverage_{1928}				-0.386**	-0.395**	-0.448**	-0.449**	-0.262	-0.163	-0.200
				(0.160)	(0.157)	(0.175)	(0.191)	(0.175)	(0.176)	(0.171)
$\log E_{1928}$					-0.038**	-0.144***	-0.147***	-0.189***	-0.162***	-0.159***
					(0.017)	(0.037)	(0.041)	(0.046)	(0.047)	(0.046)
$\log \text{Assets}_{1928}$						0.137***	0.132***	0.175***	0.134***	0.129**
						(0.036)	(0.039)	(0.047)	(0.048)	(0.048)
$\text{Profitability}_{1928}$								1.704***	1.023**	0.938**
								(0.433)	(0.442)	(0.404)
$\text{Profitability}_{1933}$									2.713***	2.712***
									(0.420)	(0.421)
$\log Age$										-0.037
										(0.033)
Observations	1010	1010	1010	1010	1010	1010	1010	824	788	785
R^2	0.016	0.140	0.180	0.187	0.193	0.213	0.263	0.328	0.407	0.408
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 9: 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 $\times \mathbb{1}(\text{BondsDue}_{1930-34} \geq 0)$	-0.249***	-0.218***	-0.208***	-0.217***	-0.212***	-0.203***	-0.243***	-0.195**	-0.220**	-0.222**
	(0.087)	(0.070)	(0.070)	(0.071)	(0.069)	(0.070)	(0.072)	(0.095)	(0.096)	(0.093)
R^2	0.011	0.135	0.174	0.182	0.187	0.207	0.258	0.325	0.403	0.404
BankFail $\times \mathbb{1}(\text{BondsDue}_{1930-34} \geq 5\%)$	-0.268**	-0.264***	-0.237**	-0.253***	-0.260***	-0.275***	-0.291***	-0.261*	-0.329**	-0.322**
	(0.110)	(0.093)	(0.090)	(0.091)	(0.088)	(0.083)	(0.091)	(0.133)	(0.134)	(0.122)
R^2	0.010	0.135	0.174	0.182	0.188	0.208	0.258	0.325	0.403	0.404
BankFail $\times \mathbb{1}(\text{BondsDue}_{1930-34} \geq 10\%)$	-0.496***	-0.436***	-0.432**	-0.457**	-0.458**	-0.496***	-0.508***	-0.449**	-0.574**	-0.549**
	(0.154)	(0.155)	(0.183)	(0.175)	(0.173)	(0.156)	(0.179)	(0.195)	(0.213)	(0.208)
R^2	0.012	0.136	0.176	0.185	0.190	0.212	0.261	0.327	0.406	0.408
Observations	1010	1010	1010	1010	1010	1010	1010	824	788	785
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 8, 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 8. *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 10: 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 × $\text{BondsDue}_{1930-34}$	-2.964**	-2.825**	-2.330*	-2.562**	-2.488**	-2.774**	-2.901**	-2.647*	-2.952*	-2.955*
	(1.230)	(1.176)	(1.220)	(1.174)	(1.134)	(1.036)	(1.204)	(1.427)	(1.498)	(1.476)
B. Exclude bonds maturing in 1934										
BankFail × $\text{BondsDue}_{1930-33}$	-3.289***	-3.180***	-2.908**	-3.048***	-3.142***	-3.238***	-3.303***	-3.042**	-3.489***	-3.369***
	(1.152)	(1.053)	(1.047)	(1.003)	(0.965)	(0.915)	(0.990)	(1.113)	(1.199)	(1.162)
C. Placebo (bonds maturing in 1928)										
BankFail × BondsDue_{1928}	-4.881	-6.940	-8.885	-9.831	-10.146	-10.813	-5.857	6.787	-4.053	-2.981
	(7.560)	(6.262)	(8.031)	(8.381)	(8.310)	(8.419)	(9.336)	(13.559)	(9.433)	(9.967)
D. Exclude bonds with maturity less than 5 years when issued										
BankFail × $\text{BondsDue}_{1930-34}$	-3.166**	-2.921**	-2.367*	-2.630*	-2.551*	-2.817**	-2.915**	-2.534	-2.847	-2.879*
	(1.397)	(1.300)	(1.361)	(1.328)	(1.285)	(1.173)	(1.383)	(1.525)	(1.678)	(1.657)
E. Continuous Treatment for bank failures										
BankFail × $\text{BondsDue}_{1930-34}$	-1.296	-1.689**	-1.546**	-1.642**	-1.620**	-1.348*	-1.430**	-1.261*	-1.528*	-1.641**
	(0.953)	(0.651)	(0.650)	(0.659)	(0.660)	(0.684)	(0.678)	(0.651)	(0.771)	(0.741)
Observations	1010	1010	1010	1010	1010	1010	1010	824	788	785
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 8, 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 8. In panel A, the treatment variable 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 treatment 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. 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 11: Estimating aggregate effects of financing frictions

A. Great Depression: Overall drop in employment in firms included in Column (10) of Tables 4 or 8	-8.6%	
i. Direct effect	Estimate	Percentage of Total
using coefficient estimates from Table 4		
BondsDue	-0.8%	10%
BondsDue, applied to endogenous short term leverage: 20 % bank loans minus cash and marketable securities	-1.0%	12%
BondsDue, applied to endogenous short term leverage: 50 % bank loans minus cash and marketable securities	-1.5%	17%
using coefficient estimates from Table 8		
BankFail X BondsDue	-1.4%	17%
BankFail X BondsDue, applied to endogenous short term leverage: 20 % bank loans minus cash and marketable securities	-1.5%	17%
BankFail X BondsDue, applied to endogenous short term leverage: 50 % bank loans minus cash and marketable securities	-2.4%	28%
ii. Indirect effect (model-implied)	Estimate	Percentage of Total
using coefficient estimate from		
treatment = BondsDue (Table 4)	-1.6%	19%
treatment = BankFail X BondsDue (Table 8)	-2.8%	33%
B. Comparison to the Great Recession: overall drop in employment in Compustat firms, 2008-09	-4.9%	
i. Direct effect	Estimate	Percent of Total
using coefficient estimate from effect of maturing debt		
treatment = BondsDue	-0.4%	9%
ii. Indirect effect (model-implied)	Estimate	Percentage of Total
using coefficient estimate from effect of maturing debt		
treatment = BondsDue	-0.5%	10%

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. Panel A is based on the baseline data for firms that report assets and employment in 1928 and 1933, and that can be matched across years. Panel B is based on all firms in the Compustat database with non-missing employment and assets in 2008 and 2009. In Panel A.i, the effects are estimated from the *BondsDue* treatment, as reported in Table 4, and from the *BondsDue* X *BankFailed* treatment, as reported in Table 8. The estimates including short-term debt are done by applying these effects to the value of notes and bills payables, net of cash and marketable securities, held by the firm in 1928. In Panel B.i, *BondsDue* are calculated as the amount of long-term debt that was issued in 2004 that matured in 2009 (Compustat variable dd5 lagged four years), scaled by the firm's book value of assets in 2008. Because Compustat does not provide information on the firm's year of incorporation, we do not include firm age in the regressions that estimate the elasticity of employment change from 2008 to 2009 to the value of long-term debt maturing in 2009. The values under "Estimate" calculate the effects from equation 7; the percent of total is calculated as the ratio of Column (1) to the aggregate contraction in employment of 8.6% in Panel A and of 4.9% in Panel B.