

# Supplementary Appendix to “Financial Frictions and Employment during the Great Depression”

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## Abstract

This appendix presents supplemental materials for the paper “Financial Frictions and Employment During the Great Depression.” It includes a discussion of the definitions, sources, and methodologies used in constructing the variables of the paper, as well as supplemental results to the analysis presented in the paper.

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# A Data: Sources and Definitions

## A.1 Construction of main sample and sources of selection

The accounting data for 1928 and 1933 were collected from the 1929 and 1934 volumes of the *Moody's Manuals of Investments of Industrial Securities*, respectively. We recorded information for each firm in the manual, including, when available, the number of employees, basic financial data from the income statement and balance sheet, the firm's location, a description of its operations, and year of incorporation. In the description that follows, we refer to the year of the data instead of the manual year for simplicity.

Appendix Table A.1 presents detailed information on the construction of our main sample. We start by collecting information on all firms in each manual, a total of 5,111 firms in 1928 and 5,110 in 1933. About 100 of these firms in each year are foreign companies, and we exclude them from the sample. We then limit the sample to the 3,977 and 3,473 firms in each respective year that do not report the value of the firm's assets for a recent year. The majority of the observations lost in this step are due to firms not reporting balance sheet data. In some cases, however, the balance sheets correspond to a fiscal year that is too old to be relevant for our analysis. Specifically, we drop from the 1928 sample those firms that only present a financial statement for the fiscal year 1926 or earlier, and we eliminate from the 1933 sample cases in which the balance sheet corresponds to 1931 or earlier years. The sample is reduced even further to the 1,823 and 2,230 firms in 1928 and 1933 that also disclose a relevant number of employees.<sup>1</sup> We then match these firms across years. About 1,300 firms with non-missing assets and employment in 1928 appear in the 1933 data, regardless of whether they reported assets and employment in that year. Conversely, about 1,700 firms with non-missing assets and employment in 1933 appear in the 1928 data, without conditioning on the availability of additional information in that year. Finally, we restrict the matched sample to those firms for which we observe assets and employment in both years, and end up with a balanced sample of 1,026 firms for which we can construct the change in employment between 1928 and 1933. For this main sample of firms, we collect similar data for 1925 and 1927 from the 1926 and 1928 *Moody's Manuals*, respectively, to study differences in pretrends.

As Table A.1 reveals, there are two main sources of selection into the final sample of 1,026 firms, conditional on the firm being listed in *Moody's*. First, we do not always observe employment information. Specifically, only 46% of the firms with non-missing assets in 1928 also report the size of their workforce in that year. (Firms with non-missing information on

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<sup>1</sup>*Moody's* often reports the period to which the employment information corresponds to. Thus, we apply the same criteria used for the value of assets, and exclude firms for which we only observe employment levels prior to 1927 for the 1928 sample, and prior to 1931 for the 1933 sample.

assets were more likely to report employment in 1933, so this source of selection in less of a concern in that year.) Second, we cannot always match firms across the two years in the sample. Indeed, only 71% of the firms with non-missing employment and assets in 1928 can be found in the 1933 data. Next, we provide some insights into how these two sources of selection may affect the interpretation of our findings.

Appendix Table A.2 shows the difference in observable characteristics in 1928 across different subsamples. In column (1), we condition the initial sample solely on those firms for which we observe assets in 1928. In column (2) we further restrict the sample to those firms that also report employment in 1928. The comparison between these two columns therefore informs on the first source of selection. Firms that reported employment in 1928 tended to be about 1 year older, and to have somewhat lower leverage.

Column (3) of Table A.2 presents information for those firms that can be matched across 1928 and 1933, and that report assets in both years (regardless of whether they report employment in either year). Thus, comparing columns (1) and (3) provides some insights into selection due to matching. Importantly, we match firms across years based on their name. When our fuzzy matching algorithm produces multiple or imperfect matches, we utilize the information on the firms' location, industry, year of incorporation, and company history to assign the correct match. By construction, we therefore focus on firms that survived from 1928 to 1933 using the same company name. Some of the reasons why firms may drop out from our sample are therefore that they went bankrupt, changed their names, were acquired by other firms, or simply stopped being covered by *Moody's* over this period. These events are more likely to occur for firms suffering more from financial frictions, which suggests that these sources of selection into the sample would likely bias us against finding an effect of finance on employment. Indeed, a comparison of means suggests that we are more likely to match firms that are older, were more profitable, and had lower leverage prior to the crisis. Finally, column (4) presents information for the 1,026 firms in our main sample, which can be matched across 1928 and 1933, and report assets and employment in both years—the patterns of selection on observables are similar for this sample. Since our ultimate focus is on a sample of more established and financially healthier firms, the selection into the sample would likely bias us against finding an effect of financial frictions.

## A.2 Industry classifications

Given the nature of the *Moody's Manuals*, the firms in our sample are primarily in manufacturing and retail. The Great Depression did not affect all firms in these sectors equally. Thus, it is important to control for industry in our regressions. However, identifying

a firm’s industry in historical data is not trivial. We use two main methods to assign firms to industries in an objective manner. First, the *Moody’s Manuals* of 1930 and 1934 classified large companies (defined as those with assets above \$5 million and \$3 million, respectively) by industries. We assign these industry definitions to the firms in our sample in 1928 and 1933, respectively, and then match these definitions to the Fama-French 30 industry classification. For firms that are not covered by the *Moody’s Manuals* industry classifications, we assign a Fama-French code based on the full description of the firm’s activities reported in the Manuals. Reassuringly, we found no instances in which assigning an industry code based on the firm’s activities would have altered the classification for those firms listed in the *Moody’s Manuals* industry classifications.

Appendix Table A.3 presents the variation in the contraction in employment across industries for the 1,026 firms in our main sample. For all but three industries (utilities, communication, and real estate), we observe more than 10 firms in each sector, and therefore the aggregate changes in employment at the industry level are meaningful. The patterns across these sectors is reasonable. We observe the largest drops in employment for mining, construction, and electrical equipment, whereas large firms operating in healthcare, recreation services, and food products, for example, actually experienced an overall increase in employment of above 10%. When we aggregate the changes in employment across all firms in the sample, we find that the overall contraction in employment was about 12.9% from 1928 to 1933.

### A.3 Bond maturing data

We collect information on maturing bonds from various issues of the *Moody’s Manuals*. Beginning with the 1931 volume, the manuals began to disclose the bonds that would mature in future years (often about 5.5 years) in individual ‘chronological lists of maturing industrial bonds.’ Specifically, we use the 1931 manual to identify bonds maturing between June 1931 and June 1932, the 1932 manual for those maturing between July 1932 and June 1933, and the 1933 manual for those maturing between July 1933 and December 1934. We collect information from the manual published closest to the year in which the bonds mature to avoid ignoring any debt buybacks that may have reduced the value of bonds outstanding during the crisis. To obtain information on the value of corporate bonds maturing from 1930 to 1931, we utilize the 1930 *Moody’s Manual*. Since there are no specific lists of maturing bonds, we instead collect this information by inspecting the detailed descriptions of each bond outstanding for each firm in the sample. Similarly, we utilize the 1928 volume to obtain information on the bonds maturing in that year for our placebo analysis.

The individual lists of maturing bonds published since 1931 provide information on the bond name, amount maturing, and date in which the bond matures. We then match the bond names to the names of the companies in our sample. When the bond name does not match a company listed in the corresponding manual, we manually search for the parent company that has assumed the debt to correctly allocate bonds to firms. For each bond that matures from 1930 to 1934, we search the detailed descriptions of each bond outstanding to obtain information on the date in which the bond was issued.

## A.4 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 vast majority (about 94%) 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 U.S. 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 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.<sup>2</sup> 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 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 A.1 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

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<sup>2</sup>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.

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 A.1 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.

## A.5 Variable definitions

The definitions of the variables utilized in the empirical analysis are presented below. It should be kept in mind that the content of financial statements was not regulated or supervised until the Securities and Exchange Act of 1934. Prior to that date, only NYSE listed firms were required to provide annual financial statements, and even then the disclosure of information was not heavily regulated. Moreover, only a small fraction of the firms in our sample actually traded in that exchange. Thus, the financial statements from which our data were recorded are quite varied in their content and level of detail. Our data are therefore not as complete or as accurate as it would be with modern public companies.

*Employment*: Number of employees. When a range is provided, we use the average employment level.

*Book Leverage*: Interest-bearing debt/total assets. Interest-bearing debt includes various sources of long-term debt (principally composed of bonds and mortgages), and short-term debt, which was typically listed in the firms' balance sheets as notes, bills or loans payables, as reported in the firm's balance sheet. Our measure of leverage does not include trade credit, since this was not always an interest-bearing liability. To discuss the validity of our data, we construct an alternative leverage ratio that includes accounts payables in the numerator. Some company-year lump together notes and accounts payable; in these cases we treat the total amount as short-term debt.

*Return on Assets (ROA)*: Net income/total assets. Net income is measured after interest and tax, but before dividend payments. Earnings before interest and taxes is defined as operating revenues minus operating expenses.

*Firm Age*: The age of the firm in 1928, calculated as 1928 minus the year of incorporation reported in *Moody's*.

*Liquid Assets*: The value of cash and marketable securities reported in the 1929 *Moody's Manual*. We currently only have this information for the 1,026 firms in our main sample.

*Bonds Due, 1930-1934*: The value of corporate bonds due from January 1930 to December 1934, scaled by the firm's average level of assets in 1928 and 1933. In our baseline specification, this variable includes all bonds maturing during this period, regardless of when they were issued. For various robustness checks, we construct alternative versions of this variable that: (a) include only those bonds issued until December 1928, (b) include bonds due from January 1930 to December 1933, or (c) exclude bonds that had a maturity of less than five years at issuance.

*Bonds Due, 1928*: The value of corporate bonds due in 1928, scaled by the firm's average level of assets in 1928.

*Number of suspended national banks, 1929-1933*: The number of national banks in the firm's county that suspended at any point from 1929 to 1933, as a fraction of the number of national banks located in that county in 1928.

*Fraction of suspended national banks weighted by deposits, 1929-1933*: The total value of deposits in national banks in the firm's county that suspended at any point from 1929 to 1933, as a fraction of the value of deposits of national banks located in that county in 1928.



## B Additional Results and Robustness Checks

In this Section, we discuss additional robustness checks for the main empirical analysis provided in the paper to identify the effect of financial frictions on employment.

### B.1 Maturing long-term debt

Section 4.1 presents the main baseline results on the effect of maturing debt on employment changes. Next, we address a variety of potential concerns. The main results are presented in Appendix Table A.4. Although we only display the estimated coefficients for the main variable of interest, *BondsDue*, all our specifications include the same controls baseline regressions presented in Table 4.

A potential worry is that (good) firms may have anticipated the financial crisis, and altered their capital structure and employment levels accordingly. However, Panel A of Appendix Table A.4 shows that our findings are, if anything, larger when we exclude maturing bonds that were issued after December 1928. This cutoff is well before the earliest signs of the Great Depression. Therefore, our main treatment variable is unlikely to capture solely a differential endogenous response to the crisis.

Our baseline specification for the treatment variable includes bonds maturing in 1934. Once the recession was on its way, and public debt markets were frozen, firms may have anticipated the difficulties in refinancing maturing debts in the future. It is possible that firms acted in precautionary ways, reducing employment by 1933 in anticipation of their need to allocate cash flows to repay debts in 1934. In Panel B we instead shut down this channel by focusing only on bonds maturing between 1930 and 1933. While the magnitude of the estimated effects are largely unaffected by this change, we lose precision in some specifications.

Our baseline measure of *BondsDue* includes all bonds maturing in the 1930-34 period, regardless of when they were issued. However, firms are more likely to have bonds maturing in any given five-year period if they typically issue bonds of short maturities. If firms that use short-term financing differ from others in unobserved ways, our results may instead be due to those unobserved firm characteristics. However, in Panel C of Table A.4 we show that the estimated effects are larger (and always statistically significant) when we measure the *BondsDue* variable using only those bonds with a maturity of at least five years.

To further address concerns of unobserved firm characteristics, we perform a placebo experiment utilizing as a placebo treatment variable the value of the firm's debt maturing in 1928, as a fraction of the assets in that year. Panel D shows that having higher levels of debt

maturing well before the crisis started had no statistically significant effects on employment changes between 1928 and 1933. It is important to point out that these estimates are noisy, since the number of firms in the sample that had corporate bonds becoming due in one year is rather small. Still, the placebo exercise appeases some concerns that firm unobserved characteristics may bias our results. Specifically, it suggests that having a sizable fraction of debt relative to assets becoming due only has negative effects on firms' employment decisions when that debt matures during an economic downturn.

To address concerns that differences in capital structure may drive our results, in Panel E we replicate our main results by excluding firms with zero leverage. This constraint reduces our sample size by about a third, so perhaps it is not surprising that the estimated effects are not statistically significant in a few specifications. However, the magnitude of the coefficients are largely unchanged.

Much of the existing literature on the Great Depression has assumed that large firms did not experience financial constraints during the crisis because they had large cash holdings. In Panel F of Table A.4 we control for the firms' holdings of liquid assets in 1928. The estimated effects on maturing debt are marginally reduced in size, but they remain large and statistically significant. This suggests that the cash holdings that firms had entering the crisis were insufficient to cover for their financial commitments during this period and to simultaneously support their pre-crisis employment levels. Our findings therefore contradict one of the commonly upheld beliefs about large firms during the Depression.

Another potential concern is that our effects may be driven by local economic conditions. Our strategy partly addresses that concern by comparing firms with different levels of maturing debt to control for demand. However, if firms with maturing debt were more likely to be located in areas that suffered larger economic shocks, or be more sensitive to local economic conditions, our effects may be biased. In Panel G we control for the growth in retail sales between 1929 and 1933 in the county in which the firm operated, based on data from [Fishback, Horrace, and Kantor \(2005\)](#). In the absence of measures such as GDP at the level of counties or cities in the 1930s, historical work on the Great Depression has largely relied on the value of retail sales as the main metric of economic performance at this geographic level. The estimated effect of maturing bonds on employment changes are largely unchanged, suggesting that our main results are unlikely to be driven by differences in local economic conditions.

In the paper, we have utilize a continuous measure of maturing debt. Since a small fraction of the firms in our sample had bonds becoming due during the crisis, a potential concern is that our results are driven by a few outliers. Instead, here we evaluate discrete

effects. As with the robustness on maturing debt and bank failures, we do so by defining a dummy variable  $D_x$  that takes the value one if the dollar value of maturing debt exceeds  $x\%$  of their average level of assets between 1928 and 1933. We consider values of  $x$  equal to 0, 5, and 10. Table A.5 presents the results. The first row suggest that firms that had a positive value of debt due experienced a 4% to 8% greater drop in employment relative with no maturing bonds, though our data are too noisy to estimate these effects with precision. The estimates are somewhat more precise, and the contraction in employment is larger, for higher values of maturing debt. The decline in employment was in the range of 10% to 14%, or 13% to 20%, depending on whether the firms had to refinance more than 5% or 10% of their assets. It is important to keep in mind, however, that these estimates are based on a small number of firms—less than 10% of the firms in the sample had to refinance more than 5% of their assets. Nevertheless, the apparent positive gradient in the effects of maturing bonds on employment minimizes concerns related to the low leverage levels that characterized corporations during our sample period.

## B.2 Local banking conditions

Here, we analyze the direct effects of disruptions to the local banking systems on firm employment. Suspensions of national banks were fairly widespread—about 700 firms were located in counties in which at least one national bank suspended from 1929 to 1933, while the remaining 326 firms had their primary location in counties with no such failures.

Table A.6 presents a comparison in means and medians for firm characteristics for those firms that were located in counties that experienced at least one national bank failure from 1929 to 1933, with those of firms located in areas with no such failures. 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 that we utilize in our two main empirical strategies was uncorrelated with the health of the local national banks. This is relevant because it helps appease any concerns of reverse causality—that is, that the firm financial constraints imposed by having debt maturing during this period led to the failure of the local banks. We also do not find distinguishable differences in the employment level and profitability that firms had prior to the crisis by their location. But firms in counties with suspended national banks tended to be somewhat larger and had slightly higher leverage (mean leverage of 14% versus 11%) prior to the crisis. Interestingly, these differences suggest that an analysis of the impact of financial shocks during the Great Depression that is based solely on the intensity of bank failures, and that it does not control for differences in financial leverage across business units may suffer from omitted variable bias.

There are important reasons to expect the *direct* effect of bank failures to be small for the firms in our sample. The United States had a unit banking system that severely restricted the ability of commercial banks to branch (Calomiris, 2000). At the onset of the Depression, most commercial banks were small and undiversified. Perhaps due to these restrictions, large firms did not use much bank credit at that time; corporate bonds accounted for about two-thirds of the value of total debt outstanding for the average firm in the sample in 1928.

To estimate the 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:

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

where we replace the value of maturing debt by a discrete treatment,  $\text{BankFail}_i$ , 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.

Table A.7 presents the results. We find a negative relation between national bank suspensions and employment changes, but the estimated effects are not always statistically significant. The magnitude of those that are—in Columns (3) to (7)—imply that firms located in counties where national banks failed experienced a contraction in employment that was 7.6% to 9.6% larger than the declines exhibited by similar firms located in counties with no such suspensions. However, the estimated effects become smaller (3.0% to 3.8%) 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 when one focuses only on bank suspensions. The association between bank failures and employment changes is also sensitive to different ways of measuring the disruptions in local banking conditions, such as using the deposit-weighted measure of bank failures.

### B.3 Interaction of local banking conditions and firm financing needs

Tables 7 through 9 in the paper present the baseline effects and main robustness checks for the empirical strategy that identifies the effect of financial frictions from the interaction between maturing bonds and bank failures. In this section, we expand this analysis even further.

A potential remaining concern with our estimates is that firms with higher levels of debt maturing during the crisis were also more highly levered. Our regressions control for the level of leverage prior to the crisis. Moreover, our triple-difference strategy is less sensitive to this problem since the financial leverage of firms with maturing debt was similar independent of whether these firms operated in counties with or without bank failures (see Table 6). However, to address this concern even further, in Panel A of Table A.8 we show that the estimated effects of our interaction term are robust to excluding from the sample firms that had zero leverage in 1928.

Our baseline estimates do not take into account differences in the hoarding of liquid assets across firms. This is particularly important because the existing literature on the Great Depression has primarily argued that large firms were unconstrained and therefore did not suffer during the crisis. To address this possibility, we redefine the variable *BondsDue* as the value of maturing bonds from 1930 to 1934 net of the firm's holdings of cash and marketable securities in 1928, and then scale this measure of net maturing debt by the firm's assets. Panel B of Table A.8 presents the results. All interaction terms are negative, statistically different from zero, and economically sizable. Thus, the holdings of liquid assets were insufficient to satisfy the financial constraints imposed by maturing debt during the Depression, even among the largest businesses in the economy.

As we discussed earlier, a potential concern is that our effects may be driven by local economic conditions. Our strategy partly addresses that concern by comparing firms with different levels of maturing debt to control for demand. However, if firms with maturing debt were more likely to be located in areas that suffered larger economic shocks, or be more sensitive to local economic conditions, our effects may be biased. This concern is particularly salient in our estimation of the interaction between bank failures and maturing bonds, since areas with more bank distress may have suffered from larger economic shocks. Thus, in Panel C we again control for the growth in retail sales between 1929 and 1933 in the county in which the firm operated. The estimated effects of the interaction between maturing bonds and bank failures are robust to this change in the specifications, suggesting that our results are unlikely to be driven by differences in local economic conditions.

Finally, throughout the paper we measure the disruptions to local banking systems by focusing solely on the failures of national banks. We do so because these banks were arguably more likely to lend to the large firms in our sample, and because concerns about reverse causality would perhaps be more salient for other types financial institutions. We next show that ignoring the shocks to state banks in the areas in which the firms in our sample operated does not bias our results. Importantly, the number of state banks that failed was much larger, primarily because state banks were smaller than national banks. Thus, the majority of our firms operated in a county in which at least one state bank failed. To obtain useful geographical variation, we focus instead on the deposit-weighted measure of bank failures, and allow for separate effects of the failures of state and national banks, both interacted with the value of maturing bonds. Appendix Table A.9 presents the results. We do not find a differential effect those firms with high levels of maturing debts that were located in areas where the deposits in state banks that failed were large—the coefficients tend to be positive and are very noisy. More importantly, comparing the results to those of Panel E of Table 9, which presents the deposit-weighted measure of national banks only, reveals that the estimated effects of maturing bonds in areas with national bank failures are unchanged. Thus, local national banks do appear to have been the most salient important institutions for the large firms in our sample that may have needed to substitute public debt for private debt at the peak of the Depression.

## C Details on the model solution

We solve the model using value function iteration. We discretize the AR(1) productivity shock into a 25-point grid using the Tauchen-Hussey procedure. Our grid for maturing debt (in logs) has 201 equally-spaced points; with -5 and 0 as the end-points. Our grid for log employment has 1001 equally spaced points; the end-points are -10 and a value that is equal to two times the level of employment absent financing frictions. The solution is not sensitive to our choice of the end-points. The initial guess for the iterative procedure is the value function for a firm without financing frictions.

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## A. Appendix Tables

**Table A.1:** Selection into the main sample

Year of data	1928	1933
All firms in Moody's Manuals	5,111	5,110
exclude foreign firms	5,008	5,003
exclude firms with missing book assets	4,039	3,568
exclude firms with missing book assets (or old assets data)	3,977	3,473
exclude firms with missing employment	1,824	2,306
exclude firms with missing employment (or old employment data)	1,823	2,230
restrict sample to firms matched across years	1,301	1,740
restrict sample to firms matched across years with non-missing assets in both years	1,172	1,664
restrict sample to firms matched across years with non-missing assets and employment in both years	1,026	1,026

The data for 1928 were collected from the 1929 *Moody's Manual* and those for 1933 were obtained from the 1934 *Moody's Manual*. Each row of the table describes the (cumulative) filters applied in each stage. We begin with all firms that appear in each manual. In the second row we eliminate firms that report their main city in a foreign country. The third row drops firms that did not report a balance sheet, and for which we therefore do not observe the value of assets. We then limit the sample to those that reported a balance sheet close to our data year—1927 and 1928, or for 1932 and 1933, respectively. Similarly, we drop firms that do not report employment, or that report it for a much earlier year. We match firms across years based on their names.



**Table A.2:** Mean firm characteristics across sub-samples

	Assets in 1928 (1)	Assets and Employment in 1928 (2)	Assets in 1928 and 1933 (3)	Assets and Employment in 1928 and 1933 (4)
Book Assets, log, 1928	15.36 (0.02)	15.46 (0.03)	15.58 (0.03)	15.60 (0.04)
Number of Employees, log, 1928	6.58 (0.03)	6.58 (0.03)	6.78 (0.04)	6.77 (0.04)
Profitability, %, 1928	7.61 (0.15)	7.93 (0.21)	8.32 (0.18)	8.72 (0.27)
Leverage, %, 1928	14.62 (0.26)	13.86 (0.35)	13.21 (0.32)	12.80 (0.45)
Firm Age (years), 1928	20.65 (0.24)	21.56 (0.35)	21.76 (0.32)	22.43 (0.47)

For each variable, the first row presents the average across all firms in that sub-sample. Standard errors are reported in parenthesis. Column (1) restricts the sample to all firms with no missing assets in 1928; column (2) contains firms with no missing assets and employment information in 1928; column (3) presents information for firms that are matched across 1928 and 1933; and that report the value of assets in both years; column (4) is based on the baseline sample of 1,010 firms that match across both years, and that have non-missing employment and assets data in 1928 and 1933. Profitability is measured by the return on assets; leverage is the ratio of interest-bearing short-term and long-term debt to the value of assets; firm age is calculated as (1928 - the firm's year of incorporation).

**Table A.3:** Employment change across industries

Industry	Firms	Employment Change (%)
Apparel	67	-2.5
Automobiles and Trucks	49	-24.6
Aviation and Railroad Equipment	26	-25.9
Business Equipment	25	-19.1
Business Supplies and Shipping Containers	61	-17.7
Chemicals	29	-3.7
Coal Mining	21	-40.7
Communication	2	58.4
Construction and Construction Materials	80	-57.9
Consumer Goods	53	-17.2
Electrical Equipment	21	-47.6
Fabricated Products and Machinery	108	-22.4
Food and Beverages	73	18.6
Healthcare, Medical Equipment, Pharmaceuticals	11	28.3
Personal and Business Services	14	3.3
Petroleum and Natural Gas	38	-4.5
Precious Metals, Non-Metallic, and Industrial Metal Mining	26	-73.6
Printing and Publishing	20	9.2
Real Estate	2	-113.5
Recreation	13	23.0
Restaurants, Hotels, Motels	12	-13.2
Retail and Wholesale	86	15.8
Steel Works	51	-11.3
Textiles	83	2.9
Tobacco Products	16	-28.6
Transportation	36	0.2
Utilities	3	-28.0
Total	1026	-12.9

The table reports the aggregate contraction in employment from 1928 to 1933 across industries, based the sample of firms that can be matched across years, and that report assets and employment in both years.

**Table A.4:** Robustness: the effects of maturing debt

$\log E_{1933} - \log E_{1928}$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
<i>A. Only include bonds issued before January 1, 1929</i>										
BondsDue <sub>1930-34</sub>	-1.624*	-1.528**	-1.792***	-1.336**	-1.454**	-1.422**	-1.448**	-1.587*	-1.726*	-1.555*
	(0.816)	(0.635)	(0.593)	(0.603)	(0.624)	(0.580)	(0.689)	(0.915)	(0.932)	(0.868)
<i>B. Exclude bonds maturing in 1934</i>										
BondsDue <sub>1930-33</sub>	-1.366*	-1.120*	-1.248**	-0.870	-0.942	-1.012*	-1.063	-1.095	-1.136	-1.072
	(0.708)	(0.620)	(0.580)	(0.573)	(0.577)	(0.561)	(0.625)	(0.731)	(0.719)	(0.687)
<i>C. Exclude bonds with maturity less than 5 years when issued</i>										
BondsDue <sub>1930-34</sub>	-1.886**	-1.723***	-1.985***	-1.575***	-1.677***	-1.619***	-1.620**	-1.649*	-1.802**	-1.662**
	(0.725)	(0.584)	(0.529)	(0.561)	(0.576)	(0.553)	(0.632)	(0.828)	(0.813)	(0.773)
<i>D. Placebo (bonds maturing in 1928)</i>										
BondsDue <sub>1928</sub>	-0.489	2.219	3.577	5.438	4.509	5.606	1.680	1.404	2.154	3.054
	(4.949)	(5.117)	(6.033)	(5.886)	(5.915)	(5.531)	(5.797)	(4.547)	(3.655)	(3.469)
<i>E. Exclude firms with no leverage in 1928</i>										
BondsDue <sub>1930-34</sub>	-1.183**	-0.805	-1.055**	-0.934**	-0.966**	-0.986**	-0.931*	-1.021	-1.023	-0.974
	(0.562)	(0.476)	(0.412)	(0.403)	(0.416)	(0.421)	(0.492)	(0.656)	(0.730)	(0.713)
<i>F. Include liquid assets as control</i>										
BondsDue <sub>1930-34</sub>	-1.411**	-1.175**	-1.419***	-0.983**	-1.033**	-1.060**	-1.063**	-1.137*	-1.286**	-1.204*
	(0.558)	(0.495)	(0.444)	(0.430)	(0.447)	(0.432)	(0.494)	(0.626)	(0.624)	(0.595)
<i>G. Include growth in retail sales as control</i>										
BondsDue <sub>1930-34</sub>	-1.435**	-1.307**	-1.492***	-1.154**	-1.190**	-1.227**	-1.245**	-1.156*	-1.086*	-1.006*
	(0.615)	(0.563)	(0.496)	(0.476)	(0.491)	(0.473)	(0.536)	(0.615)	(0.592)	(0.564)
Observations (Panels A–D)	1026	1026	1026	1026	1026	1026	1026	840	804	801
Observations (Panel E)	712	712	712	712	712	712	712	591	565	563
Observations (Panel F)	698	698	697	697	697	697	697	576	550	548
Observations (Panel G)	958	958	958	958	958	958	958	784	750	747
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

The table presents a series of robustness checks to the baseline specification presented in Table 4, which evaluates the effect of bonds maturing from 1930 to 1934 on the change in employment between 1928 and 1933. As indicated in the bottom part of Table A.4, the controls included in each column are the same as in Table 4. In panel A, *BondsDue* only includes bonds that were issued prior to January 1st 1929; in panel B, bonds maturing in 1934 are excluded; in panel C, the variable is based only on bonds issued with a maturity of 5 or more years; panel D presents a placebo in which *BondsDue* is based on the value of bonds maturing in 1928 (as a fraction of 1928 assets); panel E excludes firms with zero leverage in 1928; Panel F controls for cash and marketable securities in 1928, as a fraction of the firm's assets in that year; Panel G controls for the growth in retail sales in the county from 1929 to 1933. Robust standard errors clustered at the industry level are presented in parentheses; \*\*\*, \*\* and \* indicate significance at the 1%, 5% and 10% level, respectively.

**Table A.5: Robustness: discrete measure of maturing bonds**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$\log E_{1933} - \log E_{1928}$										
BondsDue <sub>1930-34</sub> $\geq 0$	-0.085 (0.052)	-0.069 (0.048)	-0.078 (0.050)	-0.038 (0.049)	-0.036 (0.049)	-0.044 (0.047)	-0.051 (0.052)	-0.065 (0.063)	-0.061 (0.062)	-0.049 (0.061)
$R^2$	0.003	0.128	0.168	0.175	0.179	0.199	0.249	0.316	0.394	0.396
BondsDue <sub>1930-34</sub> $\geq 5\%$	-0.139** (0.061)	-0.124** (0.050)	-0.148*** (0.049)	-0.104** (0.047)	-0.113** (0.047)	-0.117** (0.044)	-0.118** (0.047)	-0.127** (0.061)	-0.135** (0.065)	-0.118* (0.061)
$R^2$	0.004	0.130	0.170	0.176	0.181	0.201	0.250	0.317	0.395	0.397
BondsDue <sub>1930-34</sub> $\geq 10\%$	-0.197* (0.112)	-0.157 (0.102)	-0.187* (0.092)	-0.139 (0.089)	-0.149 (0.090)	-0.161* (0.085)	-0.155 (0.095)	-0.143 (0.113)	-0.139 (0.120)	-0.134 (0.114)
$R^2$	0.005	0.129	0.170	0.176	0.181	0.201	0.250	0.317	0.394	0.396
Observations	1026	1026	1026	1026	1026	1026	1026	840	804	801
Leverage, 1928				Y	Y	Y	Y	Y	Y	Y
Employment, 1928					Y	Y	Y	Y	Y	Y
Book assets, 1928						Y	Y	Y	Y	Y
Profitability, 1928								Y	Y	Y
Profitability, 1933									Y	Y
Firm age										Y
Fixed effects	-	S	S, I	S, I	S, I	S, I	S, IxR	S, IxR	S, IxR	S, IxR

This table reports robustness checks to the baseline results presented in Table 4, which evaluates the effect of bonds maturing on the change in employment between 1928 and 1933, by using discrete definitions of maturing bonds. To facilitate comparisons, the controls included in each column are the same as in Table 4. *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 to 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 A.6:** Comparison on observables, by bank failures in the firms' local area

	No bank failed		Banks failed		Difference (p-value)	
	Mean	Median	Mean	Median	Mean	Median
	(1)	(2)	(3)	(4)	(5)	(6)
Employment, log, 1928	6.67	6.68	6.82	6.80	0.13	0.28
Profitability, 1928	0.08	0.07	0.09	0.08	0.38	0.12
Book Assets, log, 1928	15.48	15.36	15.67	15.60	0.02	0.04
Leverage,1928	0.11	0.05	0.14	0.09	0.01	0.00
Firm Age, log	2.89	3.00	2.83	2.94	0.20	0.25
Bonds Due, 1930-34	0.01	0.00	0.01	0.00	0.69	-
Number of firms	326		700			

Based on the sample of firms that match across years, and that have non-missing assets and employment in 1928 and 1933. Columns (1) and (2) present the mean and median values of the observable variables for those firms that operated in 1928 in counties where no national banks failed from 1929 to 1933. Columns (3) and (4) present the mean and median values of the observable variables for those firms that operated in 1928 in counties where at least one national bank failed from 1929 to 1933. 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 *BankFail* dummy, and are presented in column (6). Bonds due is the value of bonds maturing from 1930 to 1934, as a fraction of the firm's average value of assets in 1928 and 1933.

**Table A.7:** The effect of bank failures on employment

$\log E_{1933} - \log E_{1928}$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BankFail	-0.051 (0.047)	-0.056 (0.035)	-0.082** (0.038)	-0.079* (0.039)	-0.078* (0.039)	-0.076* (0.039)	-0.096** (0.035)	-0.103 (0.061)	-0.030 (0.053)	-0.038 (0.053)
Leverage <sub>1928</sub>				-0.394** (0.158)	-0.401** (0.156)	-0.450** (0.172)	-0.457** (0.187)	-0.239 (0.163)	-0.136 (0.151)	-0.176 (0.145)
$\log E_{1928}$					-0.032* (0.016)	-0.134*** (0.037)	-0.134*** (0.040)	-0.177*** (0.046)	-0.152*** (0.046)	-0.148*** (0.045)
$\log \text{Assets}_{1928}$						0.130*** (0.035)	0.126*** (0.039)	0.170*** (0.046)	0.128** (0.047)	0.125** (0.047)
Profitability <sub>1928</sub>								1.817*** (0.440)	1.135** (0.455)	1.030**> (0.417)
Profitability <sub>1933</sub>									2.711*** (0.429)	2.709***>* (0.431)
$\log \text{Age}$										-0.045 (0.031)
Observations	1026	1026	1026	1026	1026	1026	1026	840	804	801
$R^2$	0.002	0.129	0.169	0.177	0.181	0.200	0.250	0.318	0.394	0.396
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 A.8:** Additional Robustness: Maturing debt and bank failures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$\log E_{1933} - \log E_{1928}$										
<i>A. Exclude firms with zero or missing leverage in 1928</i>										
BankFail $\times$ BondsDue <sub>1930-34</sub>	-2.980*** (0.972)	-2.812*** (0.822)	-2.823*** (0.901)	-2.896*** (0.881)	-2.904*** (0.863)	-3.040*** (0.859)	-3.266*** (0.943)	-2.799** (1.243)	-3.487** (1.317)	-3.542*** (1.272)
Observations	712	712	712	712	712	712	712	591	565	563
<i>B. Include liquid assets as controls</i>										
BankFail $\times$ BondsDue <sub>1930-34</sub>	-2.923*** (0.929)	-2.779*** (0.786)	-2.696*** (0.827)	-2.822*** (0.793)	-2.835*** (0.776)	-2.916*** (0.726)	-3.099*** (0.829)	-2.783** (1.014)	-3.187*** (1.061)	-3.079*** (1.000)
Observations	1026	1026	1026	1026	1026	1026	1026	840	804	801
<i>C. Include growth in retail sales as controls</i>										
BankFail $\times$ BondsDue <sub>1930-34</sub>	-3.217*** (1.023)	-3.061*** (0.899)	-2.939*** (0.956)	-3.030*** (0.937)	-3.066*** (0.915)	-3.174*** (0.863)	-3.314*** (0.973)	-2.886** (1.099)	-3.050*** (1.070)	-2.915*** (1.040)
Observations	958	958	958	958	958	958	958	784	750	747
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 presents additional robustness checks the baseline specification presented in Table 7, which evaluates the effect of bonds maturing in areas that experienced bank failures on the change in employment between 1928 and 1933. As indicated in the bottom part of Table A.8, the controls included in each column are the same as in Table 7. Panel A excludes from the sample firms that had zero leverage in 1928; Panel B controls for cash and marketable securities in 1928, as a fraction of the firm's assets in that year; Panel C controls for the growth in retail sales in the county from 1929 to 1933. This net measure of maturing bonds is then scaled by the firm's average assets in 1928 and 1933. BankFail is an indicator variable that takes a value of one if at least one national bank suspended from 1929 to 1933 in the county in which the firm was located in 1928, and zero otherwise. Robust standard errors clustered at the industry level are presented in parentheses; \*\*\*, \*\* and \* indicate significance at the 1%, 5% and 10% level, respectively.

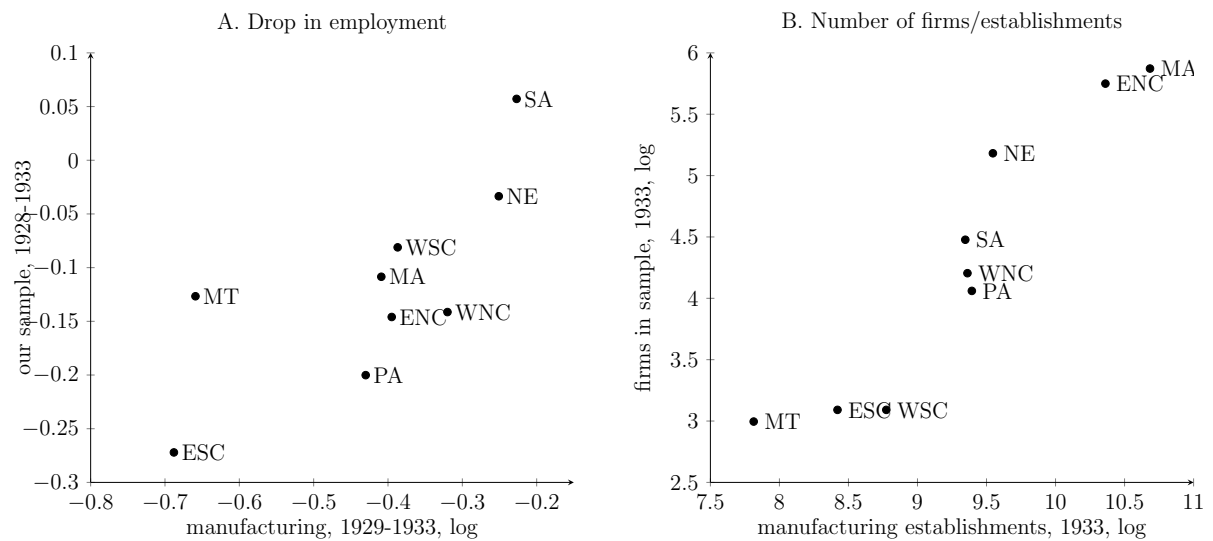
**Table A.9:** Robustness: state and national bank failures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BankFail (% deposits in suspended national banks)	-0.010 (0.063)	0.041 (0.063)	0.002 (0.080)	0.002 (0.078)	0.007 (0.073)	0.012 (0.071)	0.015 (0.077)	-0.085 (0.132)	-0.071 (0.107)	-0.073 (0.106)
BankFail (% deposits in suspended state banks)	0.085 (0.086)	0.170* (0.086)	0.109 (0.120)	0.109 (0.122)	0.112 (0.119)	0.128 (0.126)	0.077 (0.137)	0.065 (0.131)	0.064 (0.116)	0.052 (0.114)
BondsDue <sub>1930-34</sub>	-2.053** (0.870)	-1.471* (0.854)	-1.660* (0.821)	-1.358 (0.816)	-1.358 (0.836)	-1.466* (0.808)	-1.579* (0.875)	-1.713 (1.029)	-1.852* (1.060)	-1.722 (1.058)
BankFail (% deposits in suspended national banks) $\times$ BondsDue <sub>1930-34</sub>	-1.273 (0.837)	-1.594*** (0.570)	-1.498** (0.573)	-1.584** (0.583)	-1.573** (0.581)	-1.292** (0.576)	-1.332** (0.579)	-1.073* (0.592)	-1.338** (0.646)	-1.447** (0.623)
BankFail (% deposits in suspended state banks) $\times$ BondsDue <sub>1930-34</sub>	3.163 (1.923)	1.669 (1.992)	1.944 (2.011)	2.169 (2.029)	1.966 (2.006)	2.090 (1.903)	2.690 (2.000)	3.007 (2.338)	3.656 (2.442)	3.532 (2.484)
Observations	1026	1026	1026	1026	1026	1026	1026	840	804	801
$R^2$	0.012	0.139	0.177	0.182	0.187	0.206	0.254	0.323	0.403	0.404
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	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 presents additional robustness checks the specification presented in Panel E Table 9, which evaluates the differential effect on the change in employment between 1928 and 1933 of having bonds become due during the crisis by the degree of disruption to local banks, as measured by the value of deposits in national banks that suspended. As indicated in the bottom part of Table ??, the controls included in each column are the same as in Table 9. This table instead measures the disruptions of local banks by separately considering state banks and national banks that operated in the county in which the firm was located. For each type of bank, we calculate the value of deposits for those institutions that suspended from 1929 to 1933, as a fraction of the total value of deposits in 1928. Robust standard errors clustered at the industry level are presented in parentheses; \*\*\*, \*\* and \* indicate significance at the 1%, 5% and 10% level, respectively.

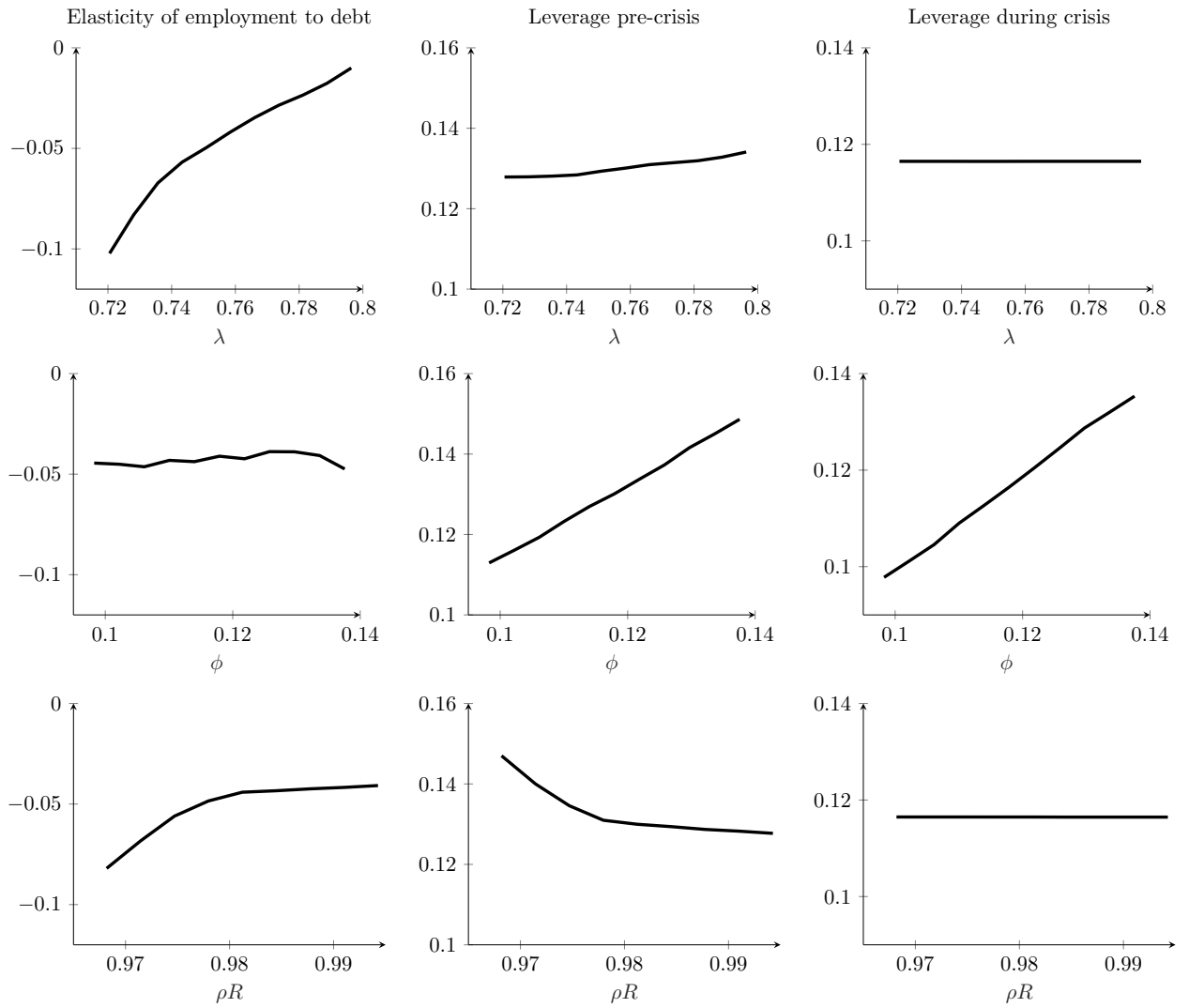


**Figure A.1:** Employment change across regions: comparing *Moody's* with the Census of Manufacturers



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 Manufacturers 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 \(2011\)](#).

**Figure A.2:** Model Comparative Statics



The figure displays comparative statics of the target moments with respect to the three parameters of the model that are estimated: the mismatch between hiring outlays and output  $\lambda$ ; the collateral constraint during the crisis  $\phi$ ; and the firm's discount factor  $\rho$  (multiplied by the interest rate, so values lower than one indicate that the firm's owners are impatient). The figure shows that the sensitivity of employment to debt primarily depends on  $\lambda$ ; the degree of leverage during the crisis is primarily determined by  $\phi$ ; while the firm's pre-crisis leverage is a function of all three parameters.