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Financial Crisis and Long-Run Labor Demand: Evidence from the Swedish Banking Crisis in the Early 90s*

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Abstract

The Swedish banking crisis in the early 90s counts as one of the five most severe financial crises in history. We examine how firms more exposed to this event adjusted employment in the long-run and the mechanisms involved. Our analysis draws on matched employer-employee data containing the financial statements for a large sample of firms. Our difference-in-differences estimates show that firms with a greater *pre-crisis* debt burden experienced more difficulties in accessing external capital during the crisis compared to firms with lower baseline debts. This is consistent with the most exposed firms becoming financially constrained. More exposed firms exhibit stronger downward employment adjustments than less exposed firms, and the reductions are mainly concentrated among low-skilled workers. Employment in more exposed firms started to recover four years after the crisis and had fully recuperated about a decade later. These firms also temporarily saw a larger drop in both productivity and investment. We do not find a significant effect on the wage bill, and the estimates are precise enough to rule out even moderate effect sizes.

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1. INTRODUCTION

To what extent do financial crises influence labor markets? The fact that financial crises typically precede large aggregate employment losses has been used to motivate government interventions to support distressed banks and to regulate credit markets. Yet, the effectiveness of such polices ultimately depends on whether this observation can be interpreted causally and what the underlying mechanisms might be. One of the main challenges is that the amount of loans in an economy is an equilibrium outcome of both supply and demand factors, making it difficult to separate supply shocks from demand ones. Declines in the local economic environment may, for instance, force firms to reduce employment, and at the same time, demand less credit. Moreover, financial crises often occur in tandem with other macroeconomic imbalances and usually affect firms and households simultaneously, making it hard to disentangle the basic mechanisms. Partly for these reasons, there is still an ongoing debate regarding the underlying sources of the large employment drops witnessed in the United States during the Great Recession in 2007/08. While several recent studies convincingly show that the housing market burst lead to a contraction in loan supply that forced non-financial firms to cut employment (e.g., Chodorow-Reich 2014, Giroud and Mueller 2017, Huber 2018)[1], others present credible evidence of either modest employment effects (Greenstone, Mas and Hoai-Luu 2015), or that the main channel behind the employment losses was reduced consumer demand (Mian and Sufi 2014; Gertler and Gilchrist 2018).

Economic theory suggests several reasons for why financial distress may be linked to (non-financial) firm labor demand (e.g., Brunner and Meltzer 1963; Bernanke 1983). From the supply

[1] Other studies examining the Great Recession include Amberg (2016), Bentolila, Jansen and Jimenez (2017), Chen, Cornille, Rycx and Tojerow (2018), Hanson and Stein (2017), Dörr, Raissi and Weber (2017), Duygan-Bump, Levkov and Garriga (2015), Sforza (2017), and Seimer (2018).

side, the basic idea is that banks that suffer from adverse shocks experience an increase in their marginal cost of funds, which lowers the profitability of lending. Profit maximizing banks that equate the marginal cost and marginal revenue of lending will then respond by contracting loan supply and raising interest rates, which will drain firm liquidity. From the demand side, asymmetric information in the credit market creates an external finance premium, resulting in a wedge between the cost of using external and internal funds (e.g. Bernanke and Gertler 1989). The premium is increasing in the debt-to-assets ratio because of greater agency costs, meaning that adverse shocks will raise the costs of external finance (Bernanke 1983). Regardless of its underlying sources, increased financial pressure will force firms to adjust employment through several mechanisms (e.g. Boeri et al. 2013). First, when facing recessions, some firms may choose to hoard more workers than necessary to reduce turnover costs. Since these firms must temporarily cover the retained workers' wages while waiting for the business cycle to turn, they need access to liquidity. However, during financial crisis, firms may have less chance of meeting the short-run liquidity needs and may therefore be forced to lay off workers. Second, if capital and labor are complements in production, then employment will fluctuate with the availability of external finance through the effect of access to external capital on investment (Bemelech et al. 2011). Third, firms under financial pressure may experience lower productivity, possibly due to reduced innovation (Huber 2018). Such productivity losses may generate persistently lower employment levels. Lastly, firms may find it optimal to reduce employment for precautionary reasons if they are uncertain about their future need of external capital and believe that the likelihood of attracting external funds is reduced (e.g. Bloom 2009). This channel may also affect firms that currently do not need to borrow.

Our study's context is the Swedish banking crisis in 1991-93, which is considered one of the five most severe financial crises in history (Reinhart and Rogoff 2008). Similar to the Great Recession, it was preceded by financial innovation and inadequate risk awareness, which contributed to extensive lending. This was later followed by falling real estate prices and bank credit losses. In 1992, bank problems intensified due to the European currency crisis, which forced the central bank to increase interest rates to defend the fixed exchange rate. The crisis threw Sweden into its deepest recession since the Great Depression (e.g. Englund 1999, Holmlund 2011). The recession, which implied a five-fold increase in the aggregate unemployment rate, terminated a period of overheated labor markets and marked the onset of persistently higher unemployment levels. The crisis's resolution involved large injections of public-sector capital into the banking system, the abandonment of the fixed exchange rate regime, significant fiscal easing, mainly as the result of automatic stabilizers, and finally also a blanket guarantee. We examine how this dramatic and largely unexplored macroeconomic event affected short and long-run firm behavior and the mechanisms producing these effects.

We are able to sidestep many of the methodological problems by combining rich employer-employee data with the Swedish crisis's sudden and unexpected nature. Our panel dataset comprises a large sample of Swedish firms that we observe for two decades starting in 1985. The dataset includes details on employment (flows), wages, assets, productivity, and investment. Information on financial statements allows us to measure financial constraints based on each firm's position in the *pre-crisis* debt-to-assets distribution. Our measure is motivated by an extensive literature in economics and corporate finance trying to estimate financial constraints (e.g. Farre-Mensa and Ljungqvist 2013; Kaplan and Zingales 1997). The rationale behind the measure is that

firms that depend more on loans to fund their operations should be affected more strongly by adverse cash-flow shocks, reduced access to capital, and depleted collateral.

Our empirical analysis is based on a differences-in-differences research design that traces out the long-run dynamic effects of interest and provides a transparent visual assessment of its key assumption: common trends. We examine the differential behavioral response of firms that *prior* to the crisis relied more heavily on loans compared to firms that relied less heavily on loans. We show how the coefficients on the interaction between year and the debt ratio quartile evolve before, during, and after the crisis, holding constant firm effects and local labor market shocks. Intuitively, we contrast changes in outcomes within firms operating in the same industry and geographic area who experience similar regional shocks, the only difference being that some firms before the crisis were more dependent on loans than others. This approach absorbs most potential confounders such as (time-invariant) credit availability, firm culture, and local consumer demand shocks. Because the crisis was largely unanticipated by the market, we should expect to find no appreciable changes in the outcomes before the crisis struck. With only a few exceptions, this is also what we find.

While the crisis originated in the financial system, it also led to a myriad of macroeconomic changes (e.g. falling house prices, reduced consumer demand, and subsequently that Sweden in 1992 abandoned the pegged exchange rate regime). We verify empirically that highly indebted firms came under more financial pressure during the crisis. While our main dataset does not contain direct information on interest payments, we use an auxiliary dataset covering some of the largest listed firms in Sweden to show that highly indebted firms experienced large increases in their interest rate expenses: about 30 percent more than less constrained firms during the crisis and

almost no difference after the crisis. We also show that these firms had significantly lower liquidity (about 23 percent), as approximated by the cash flow to sales ratio. In our main dataset, we use information on current assets to provide further evidence consistent with the most exposed firms becoming more financially constrained. Our results show that more exposed firms indeed experienced significantly larger reductions in liquid assets during the crisis compared to less exposed firms.

We then proceed to analyze the effect of the crisis on employment. We find that firms in the third and fourth quartiles experienced a significant and sharp reduction in employment when the crisis hit. The estimates suggest that these firms reduced employment by about 18 percent more than firms in the first quartile.[2] In line with the theoretical predictions, the effect size is increasing monotonically in financial pressure. Employment starts to recover after about four years and has almost fully recuperated after about a decade. The employment contraction is mainly due to an increase in worker separation rather than to a decrease in hiring. The employment losses are slightly larger for low skilled workers.

Given these results, we next attempt to disentangle some of the mechanisms through which the crisis may have influenced employment. Financially constrained firms may lower labor costs not only by reducing employment but also by cutting wages. However, our results show no evidence that the crisis significantly affected wages. In fact, our estimates are precise enough to rule out

[2] To gauge the size of the estimates, we note that firms connected to distressed banks during the Great Recession cut employment by about 5 percentage points more than firms linked to healthier banks (Bentolia et al. 2017, Chodorow-Reich 2014, Huber 2018).

even moderate effect sizes.[3] Another possibility is that firms suffering from reduced access to credit experience lower productivity, perhaps because of fewer innovations (see Huber 2018). Such productivity losses may explain the persistent employment reductions we observe and have been suggested as one potential mechanism behind labor market hysteresis.[4] Our results show that while value-added per worker fell in the more exposed firms, productivity recovered in just a few years. Finally, as already noted, limited access to external finance may affect firms' investment in capital (e.g. Fazzari, Hubbard and Petersen 2000). If capital and labor are complements in production, then employment may also be affected. While our estimates are imprecise, we find that the most exposed firms reduced their net investment by almost 50 percent in the midst of the crisis.[5]

Our results primarily add to a burgeoning literature that combines microdata with novel research designs to estimate the effects of financial constraints on employment (e.g., Bentolia et al. 2017, Chodorow-Reich 2014, Greenstone et al., 2015, Giroud and Muller 2017, Huber 2018, Mian and Sufi 2014).[6] But whereas previous studies mainly have focused on short-term effects, much less is

[3] This finding is consistent with survey evidence showing that Sweden has much stronger wage rigidity than countries like the United States, Canada and Switzerland, where employers may dismiss a worker who refuses to take a wage cut (Bennmarker and Agell 2003). Bennmarker and Agell (2003) present cross-sectional evidence from surveys sent to to Swedish firms that only 1.1 percent of all workers experienced wage cuts during the Swedish banking crisis.

[4] The other mechanisms include union bargaining favoring inside workers and skill depreciation among long-term unemployed.

[5] Although the estimates are imprecise, the results are in line with the theoretical and empirical findings in Kaplan and Zingales (1997).

[6] Early seminal work on this topic typically exploited either cross-country data (Rajan and Zingales 1988) or used small samples of firms (Nickell and Wadhwani 1991). Bentolia et al. (2017), Chodorow-Reich (2014), Huber (2018) and Greenstone et al. (2015) all significantly advanced the literature with respect to identification by exploiting cross-sectional differences in lender health at the onset of the Great Recession to study the link between credit supply shocks and employment. The research design builds on the work of Khwaja and MiaFfn (2008), who identify credit supply shocks from credit demand shocks by analyzing credit growth at the bank-firm level. Chodorow-Reich (2014) use access to loan-level data from the Dealscan syndicated loan database. He constructs a firm-specific credit supply shock that is equal to the weighted average of the reduction in lending that the firm's last pre-crisis syndicate imposes on other firms during the crisis. These data are matched to employment records for a sample of just over 2,000 firms. The results show that firms with pre-crisis relationships with less healthy banks faced stronger credit constraints after the fall of Lehman Brothers and reduced their employment more than clients of healthier banks, attributing between one-

known about financial crises' long-run consequences. This is not surprising since most studies focus on the Great Recession for which not enough time has elapsed to study labor market adjustments in the long-run.[7] Consequently, we still know little about the post-crisis labor market recovery phase.[8] Our results suggest that financial crises may indeed have long lasting effects on labor demand. Even less well understood is the role of different mechanisms through which financial crisis may influence labor demand. The fact that our data allow us to undertake a coherent analysis of a broad set of relevant outcomes both in the short and in the long run should allow for a more complete picture of the consequences of financial crises.[9] Lastly, our study is one of few

third and one-half of job losses to this factor. Huber (2018) uses a similar identification strategy in which he constructs an instrument for regional exposure to the lending cut based on a historic, postwar breakup of a major bank in Germany. The lending cut affected firms independently of their banking relationships, through lower aggregate demand and agglomeration spillovers in counties exposed to the lending cut. Output and employment remained persistently low even after bank lending had normalized. He also finds that innovation and productivity fell. Giroud and Mueller (2017) use employment data at the establishment level from the U.S. Census Bureau's Longitudinal Business Database combined with balance sheet and income statement data at the firm level to investigate the role of firms in the transmission of consumer demand shocks during the Great Recession. Their research design is based on estimating empirical specifications that control for firm and region-by-industry fixed effects. They find that highly levered firms experienced significantly larger employment losses in response to declining local consumer demand. Also, counties with more highly levered firms experienced considerably larger employment declines in response to local consumer demand shocks. The results suggest that firms' balance sheets also matter for aggregate employment. Bentolia et al. (2017) use bank-firm relations combined with firm fixed effects and show that close to one-quarter of the job losses in firms with relations to weak banks were due to credit supply shocks. Greenstone et al. (2014) do not have access to loan-level data or information about firms' banking relationships, but they construct a county-level credit supply shock from the product of the change in US banks small-business lending at the national level and their predetermined credit market share at the county level. They find that this measure predicts the reduction in county-level credit to small, individual firms and their employment levels over 2008-2009. The estimated effect, however, is small (around 5% of the employment reduction).

[7] For instance, Bentolia et al. (2017), Chodorow-Reich (2014), Giroud and Mueller (2017), and Greenstone et al. (2015) can observe employment up to two years after the onset of the Great Recession and Huber (2018) observes employment four years after the crisis.

[8] In their literature review, Gertler and Gilchrist (2018) note that recoveries from financial crises are often much longer than normal, mainly because tightness in credit markets tends to persist. The authors call for studies of the long-run post-crisis recovery phase.

[9] One may be concerned that evidence that only draws from a limited set of short-run outcomes may understate the total costs for society of financial crises and misdirect policy effort. Data limitations have forced most previous studies to focus on single outcomes such as employment (Chodorow-Reich 2014, Duygan-Bump et al. 2015, Greenstone et al. 2015), sales (Benmelech, Meisenzahl and Ramcharan 2016), and investment (Amiti and Weinstein 2018). Among the few exceptions are Giroud and Mueller (2016) and Huber (2018), who, besides employment, also present evidence on the effects of the Great Recession on firm productivity. However, the latter studies, lack individual-level employment data and are therefore not able to shed light on details such as worker separation/hiring, wages, and to distinguish between worker skills.

to investigate financial crises other than the Great Recession. This detail may improve our understanding of how well the results in the existing literature extend to other contexts.

The rest of this paper is organized as follows. The next section provides a background to the Swedish banking crisis. Section 3 describes our data and empirical strategy. The results are presented in Section 4 and Section 5 concludes.

2. THE SWEDISH BANKING CRISIS

The Swedish recession began in 1990/91 and lasted until 1993. Between 1991 and 1993, GDP fell by 5.1% while the unemployment rate rose from 2.4 in 1990 to 10 percent in 1993 (see Figure A.4).[10] This section describes the origins, outburst, and aftermath of the Swedish banking crisis. The crisis has been extensively documented, and our exposition draws heavily on a series of excellent reviews (e.g. Englund 1999, Holmlund 2001, Jonung, Kiander and Vartia 2009, and Wallander 1994).

Origins

The crisis's roots are usually traced back to the credit boom that followed the financial deregulation process in the mid-1980s. Instead of being forced to invest in government and housing bonds, lending guidelines were gradually abolished. Consequently, new opportunities for market share competition opened up for banks and other previously heavily regulated financial institutions. The

[10] To put these numbers into perspective, in the United States during the Great Recession, the corresponding fall in GDP was 4.3 percent, and unemployment increased from 4 percent to 10 percent (Hoffman and Lemieux 2014). Brunnermeier (2009) and Gertler and Gilchrist (2018) describe the key factors leading up to the Great Recession in the United States. While there certainly are differences, Bharadwaj et al. (2015) highlight several similarities between the Swedish crisis and the Great Recession in the US. In particular, in both countries, debts in the real estate sector played a key role.

liberalization of the financial system resulted in a rapid increase in credit supply that was further encouraged by the combination of expansive fiscal policy, a fixed exchange rate, and excess risk-taking (Englund, 1999). The credit expansion contributed to a rapid growth in residential investment and consumption, which pushed the level of spending beyond the level of income. As can be seen in Figure A.1, bank lending to firms increased faster than lending to households. While bank lending to households increased from 240 billion SEK to 400 (in current prices) between 1985 and 1989, lending to firms increased from 380 billion SEK to 680. Parallel to this development, credit flows shifted from the unregulated sector to the regulated, and banks gained market shares at the expense of financial institutions that had thrived in the regulated environment.

According to Jonung, Kiander and Vartia (2008), forecasts and analyses of the effects of the major changes in the financial system were mostly missing. Policymakers were, therefore, largely unaware of the forces they set in motion by abolishing the lending guidelines. However, it was not the lending boom per se that caused the banking crisis but rather a combination of several exogenous and unexpected shocks and severe policy mistakes (see Englund and Vihriälä 2009).

Outburst[11]

Prior to the end of 1989, there were few signs of a looming financial crisis, but strong indications of an overheated economy (Englund 1999). The unemployment rate hit a record low of 2,2 percent in 1989, and by the end of 1989, the stock market index was down by 8 percent from its peak in August 1989. Meanwhile, early signs indicated that the property market had peaked and that credit

[11] This description follows closely Englund (1999).

losses in the financial sector started to increase. However, losses were still small, and nothing suggested the breakout of a full-fledged financial crisis.

By the end of the 80s and early 90s, Sweden experienced a sharp interest rate increase, and the post-tax real interest rate went from -1 percent in 1989 to 5 percent in 1991. Several exogenous events mainly drove this change. First, the international interest rates increased following the reunification of Germany in 1989. Second, in 1990, the government announced that they would change its economic policy, and combatting inflation became their top priority. Third, a major tax reform was implemented in 1991 that reduced interest rate deduction for most taxpayers from 50 to 30 percent. Subsequently, the interest rate increased further due to the Central Bank's defense of the fixed exchange rate, and as shown in Figure A.3, the average bank lending rate increased to 17 percent in 1992.[12]

The fall of 1990 marked the real onset of the crisis when one of the major finance companies, Nyckeln, with considerable exposure to the real estate sector, could not roll over their securities. The entire market for securities collapsed within a few days, and, in the coming months, several finance companies went into bankruptcy. Since banks' and finance companies were closely linked, the crisis spread to the bank sector by the end of 1990. Increased real after-tax interest rates worsened the situation and contributed to declining asset values. As a result, the bankruptcy rate more than doubled between 1990 and 1992. Meanwhile, as shown in figure A.2, credit losses among the three largest Swedish banks increased dramatically, and went from virtually zero in 1989 to 50 billion SEK in 1992. The accumulated credit losses in the banking sector between 1990

[12] The likely cause of the sharp reduction in the average bank lending rate in 1993 was the change in exchange rate regime through Sweden's abandonment of the pegged exchange rate system by the end of 1992.

and 1993 amounted to 16.8 percent of total lending, and, as noted by Wallander (1994), lending to real estate accounted for about half of the credit losses, but 10-15 percent of all lending.

Policy resolution and aftermath

The way Sweden handled its 1990s banking crisis is widely recognized as a model of effective policy intervention[13]. The Swedish government's resolution involved significant monetary and fiscal easing, the abandonment of the fixed exchange rate regime, and a massive injection of public-sector capital into the banking system. Out of Sweden's six major banks, two were taken over by the government with the aim of being re-privatized, and their "toxic" assets were transferred to asset-management companies. The financial sector was thoroughly restructured and recovered from the crisis relatively quickly. After the crisis, the banking sector emerged as highly efficient, and Sweden experienced a fairly rapid economic recovery (Englund and Vihriälä 2009). However, the aggregate unemployment never fully reverted to its pre-crisis levels, and the recession became the starting point of a period of persistently high unemployment in Sweden.

3. DATA AND RESEARCH DESIGN

Our empirical analysis draws on administrative data covering the entire Swedish population aged 16 and above from 1985–2016. The data span various registers that are connected through anonymized identification codes at the personal and firm-level. The main register is the employment register (RAMS) containing information from the national tax authorities. The statutory income statements, filed by the employers, identify both the employee and the firm. We obtain wages and employment spells for all years. This data is then merged with information on a

[13] See e.g. *New York Times*, September 22, 2008, and *Time*, September 24, 2008.

wide array of worker background characteristics, including the highest completed level of education, gender, and age.

We use detailed financial information on Swedish firms comprehensive since 1996/1997 for firms whose size was above a certain cutoff before then.[14] In line with previous studies, we restrict our attention to private firms (both listed and unlisted) outside of the financial and insurance sectors. We have annual data from income statements and balance sheets collected by Statistics Sweden through a survey for each firm until 1996.[15]

We retrieve assets value, investment, and value-added. Value-added is the total value added at each stage of production and is equivalent to total revenues minus intermediate consumption of goods and services. Worker productivity is measured as the firm's valued-added divided by the number of workers. Investment is the total yearly amount spent on machinery and land, net of disinvestments, and depreciation. The firm-level data also include variables such as industry codes (4-digit) and geographic location.

Some firms may close during the observation period. To identify firm closures, we first select firms with a non-missing identifier at the end of year *t*, but whose identifier was no longer present in the following year *t+1*. Some firms may have exited for reasons other than closure (e.g., mergers, acquisitions, or firm restructuring). To account for this, we follow the convention in the

[14] All firms with at least 50 employees and all manufacturing firms with at least 20 employees are included in the data. For smaller firms, Statistics Sweden uses a random stratified sample, with larger sampling probabilities for larger firms.

[15] The registers contain a subset of variables from the income and balance sheet of the firms. For some firms, the financial year is not the same as the calendar year. In such cases, Statistics Sweden adjusts the financial statement data to match the calendar year.

literature (e.g., Hethey-Maier and Schmieder 2013) and define closures as those firms where no cluster of more than 50 percent of the workforce at the exiting firm in year *t* was found at the same firm in year *t*+1.[16]

Financial constraints are never directly observable in the data. For this reason, we approximate financial constraints using the ratio of total outstanding debt-to-assets (debt ratio). The debt ratio is a key component in the standard Kaplan-Zingales index of financial constraints (Kaplan and Zingales 1997)[17]. Another advantage of this measure is that a borrower's balance sheet is a fundamental factor shared by macroeconomic models of the interaction between financial and real sectors that have guided much of the empirical literature in the area (see Gertler and Gilchrist 2018). To reduce noise and to generate a strong predictor of ("permanent") financial constraints, we compute the average debt ratio for each firm in the pre-crisis period 1985–1989.[18]

Figure 1 shows the debt-to-asset ratio distribution for the 4,723 firms in our sample, and the median debt ratio is about 60 percent. It is important to note that while the administrative data on employment and wages are available for all firms every year, financial statements are only observed for firms that were above the employment threshold or happened to be sampled in the

[16] More specifically, if firm *i* exists in year *t* and disappears in year *t*+1 and firm *i'* does not exist in year *t* but exists in year *t*+1, then firms *i* and *i'* are assumed to be the same firm if more than 50 percent of all employees in firm *i* in year *t* are found in firm *i'* in year *t*+1, and make up more than 50 percent of the workforce in firm *i'* in year *t*+1. All firms with less than four employees that disappears are coded as true closures (as in, e.g., Hethey-Maier and Schmeider 2013).

[17] Farre-Mensa and Ljungqvist (2013) attempt to evaluate the reliability of different measures of financial constraints and conclude that the Kaplan-Zingales index best approximates financial constraints.

[18] During this period, data on financial statements were not collected directly from the Swedish Tax Agency's administrative records to which reporting is mandatory by law but through a survey administered by Statistics Sweden. In practice, this means that the firms in our sample started to operate no later than 1985. We drop firms with negative equity since most of these firms were about to file for bankruptcy.

financial statements survey. In other words, we need at least some firms in our sample to be re-sampled post 1989 to observe their post-crisis financial statements. While this is not an issue for the larger firms that are always sampled, it is more challenging for the smaller ones. This issue implies that the estimates pertaining to the financial statement data will be less precise compared to the employment and wage data. Two other complications lead us to interpret our estimates as lower bounds for the crisis's effect on these outcomes. First, there is a risk that firms precisely because of the crisis are forced to reduce employment and therefore fall below the employment cutoff used in the survey. Second, firm size decreases monotonically with the firms' debt burden. Thus, those firms that are most exposed to financial constraints also face the greatest probability of falling below the firm size cutoff used in the sample.

Descriptive statistics

Table 1 shows descriptive statistics for selected variables by quartile of the debt ratio distribution. We can see that firms with relatively large debts have on average fewer employees and a slightly larger share of college-educated workers. This is a standard result in the corporate finance literature, which argues that smaller firms are more likely to be financially constrained since they tend to be younger.[19] We also observe that labor costs per worker and value-added per worker are relatively similar across the quartiles of the debt ratio distribution.

Table 1 further shows the distribution of firms across industries. We can see that most of the firms in our sample are manufacturing firms (65.8 percent). The overrepresentation of manufacturing

[19] In fact, small firms are sometimes used as a proxy for financial constraints (see Farre-Mensa and Ljungqvist 2013). Figure A.5 shows the distribution of firm size for our sample of firms.

firms is due to the sampling frame that includes financial information for all firms with at least 50 employees except for manufacturing firms where the cutoff is 20 employees. From Table 1, we also learn that firms in the top two quartiles of the debt ratio distribution are less likely to be manufacturing firms and more likely to belong to the construction, retail, restaurant, and hotel sectors.

Empirical strategy

To examine the effects of the financial crisis on firm behavior, we exploit the panel structure of the data in a difference-in-differences setting. Our baseline OLS model is specified as follows

$$y_{icqt} = \sum_{\substack{m=85 \\ m\neq 89}}^{05} \beta_{1m} \times \mathbf{1_{m-t}} + \sum_{\substack{m=85 \\ m\neq 89}}^{05} \sum_{q=2}^{4} \beta_{qm}(\mathbf{1_{k-q}} \times \mathbf{1_{m-t}}) + \theta_i + \mu_{ct} + \varepsilon_{icqt}$$

where the outcome, y_{icqt}, is employment (or other measures of firm behavior) observed for firm *i*, located in county *c*, in pre-crisis debt quartile *q*, and year *t*. θ_i is firm-specific fixed effects, $\mathbf{1_{m-t}}$ is an indicator function set to unity for year *t* and $\mathbf{1_{k-q}}$ is an indicator function set to unity if the firm belonged to quartile *q*. The reference year is 1989, so differences in outcomes between firms are measured relative to differences observed in 1989. $\hat{\beta}_{1m}$ is the OLS estimate of the average employment of firms in the 1st quartile relative to the average employment of the corresponding firms in 1989. $\hat{\beta}_{km}$ provides the differences in average employment in year *m* between firms in quartile *k* and firms in quartile 1 relatively to the same difference in 1989. The model controls for

unobserved heterogeneity across firms that is constant over time as well as nationwide macroeconomic events.[20]

The model allows us to examine how increased financial pressure during the banking crisis affected firms. An important advantage of the model is that it allows for a transparent visual assessment of the key underlying assumption of common trends. We examine this by plotting the year-by-debt quartile coefficients for each group of firms before, during, and after the crisis.

The main threat to identification is that firms with unobserved characteristics that co-vary with their employment decisions may have taken up large loans before the crisis. Fortunately, the firm fixed effects account for this type of non-random assignment of firms to loans by absorbing time-invariant differences (both observed and unobserved) across firms. For example, the model accounts for differences in firm performance, firm culture, financial vulnerability, and creditworthiness, at least to the extent that these factors are relatively stable over time. However, factors that change over time may still confound our estimates. Fortunately, these factors will show up as differential pre-trends for firms in different positions in the baseline distribution of the debt ratio, allowing us to assess the likely severity of this problem.

Another important concern is that that demand effects may bias our estimates. As previously discussed, financial crises affect many aspects of the economy simultaneously, and it is debated whether the employment reduction during the Great Recession was due to the financial constraints of the firms or to demand shocks that were transmitted through the contraction of household wealth

[20] The main effect of debt quartile is absorbed by the firm fixed effects.

(Mian and Sufi 2014). It is possible that highly indebted households in locally overheated housing markets responded to the crisis by reducing consumer demand, and that this negative demand chock in turn may in turn reduce local labor demand.[21] [22]

There are several reasons for why we believe that our estimates are unconfounded by demand shocks. First, note that demand shocks only jeopardize the research design's validity if they *differentially* affect firms depending on their *pre-crisis* debt levels. While one certainly can imagine this situation, all regressions control for region (county)-by-year fixed effects, meaning that we effectively compare differences in the labor demand decisions of firms that are active in the same local labor market and experience the similar regional shocks. Moreover, asymmetric industry shocks could potentially invalidate our results. However, it is comforting that our main result is fairly robust to firms' debt-to-assets ranking within their industry (one-digit level). Finally, changes in demand will likely show up as changes in sales. However, we found no significant differential effect of the crisis on sales in firms with different baseline debt levels conditional on firm and county-by-year effects.[23]

4. RESULTS

This section presents the results from our empirical analysis of the effect of financial constraints on firm behavior following the Swedish banking crisis. We start by examining the impact on employment. We then investigate some of the mechanisms that may produce the results we

[21] Gertler and Gilchrist (2018) present evidence from panel data VAR-models showing that the household balance sheet channel is important for regional variation in employment, but that bank health also mattered for the overall employment contraction during the Great Recession.

[22] A related issue is that changes in the employment of one firm could influence the employment decisions in surrounding firms.

[23] This is not to say that sales did not drop during the crisis, only that there was no differential drop depending on the pre-crisis relative debts of firms.

document. In most specifications, we use the log transformation on the outcome of interest, but in cases when the dependent variable includes many zeros, we approximate the log using the inverse hyperbolic sine transformation. For clarity and transparency, we focus on a graphical presentation of the results. Table A.1 summarizes the key results by presenting the point estimates and corresponding standard errors for the crisis period. We follow firms until 2005 which is a few years after we can see that employment has fully recovered from the crisis. In all figures, the dots represent the coefficients on year interacted with the baseline debt-to-asset ratio quartile. All regressions control for shocks to the local labor markets by including region-by-year fixed effects. The standard errors are clustered at the firm level to account for serial correlation.

Effects on employment

Figure 2 shows the main estimation results. The dependent variable is the log number of workers. We find large and significant decreases in employment for firms in Q3 and Q4 of the pre-crisis debt ratio distribution. Relative to employment in 1989 among firms in Q1 of this distribution, firms in Q4 had about 18 percent fewer employees in 1993. We also see a significant drop in employment for firms in Q3 but no significant effect for firms in Q2. The monotonic increase in the effect size across the debt ratio distribution is reassuring as we would expect firms under more financial pressure to respond more strongly. Importantly, while the *F*-stats for the joint significance of the interaction coefficients are significant for firms in Q3 and Q4 during the crisis, they are not statistically significant before the crisis. This suggests that the assumption of common trends is likely to hold. We also see that employment in the most affected firms starts to recover about four years after the crisis and took about one decade to fully recover. This finding is important given that previous studies investigating the Great Recessions have only been able to study employment

for between two and four years. The results are robust to ranking firms by the debt ratio distribution within (1 digit) industry (see Figure A.6 in the Appendix). Moreover, results are similar when clustering both at the firm-level and the debt ratio quartile-level (results are available on request).

Next we ask how worker flows can explain the overall effect on employment. Figure 3 shows the effect on job separations while, Figure 4 presents the impact on hirings. The results show that job separations increased by up to 17-19 percent more for firms in Q3 and Q4 during the crisis, relatively to firms in Q1. While the estimates are imprecise, the point estimates' temporal pattern also suggests a decrease in hiring. Taken together, however, it seems as if firms adjusted employment to a greater extent by laying off workers rather than by hiring fewer workers.

The results presented so far are conditional on firm survival. While this is similar to previous studies that all examine the pre vs. post difference in employment over the Great Recession (e.g. Chodorow-Reich 2014; Giroud and Mueller 2017), the design implies that the composition of firms that exist after the crisis may be positively selected compared to the firms that existed before the crisis. In this case, our estimates may represent lower bounds of the true effect of the financial pressure on employment. To better understand the importance of dynamic selection, we investigate the impact of the crisis on firm closures by estimating regressions where the dependent variable is an indicator set to unity for firms that existed in a given year and zero if they had closed. Figure 5 confirms that firms in Q3 and Q4 were less likely to survive than those in Q1. For instance, our estimates indicate that 12 percentage points fewer firms in Q4 were still in operation in 1994

relative to firms in Q1 in 1989. This result is also consistent with time series evidence documenting an increase in firm bankruptcies during the crisis.[24]

Our findings suggest that dynamic selection could lead us to understate the total employment effect. To probe this issue further, we estimate regressions where we allow firms to remain in the sample by assigning 1 employee to those that have closed before taking logs. In Figure 6, we observe that allowing exiting firms to remain in the sample increases the absolute value of point estimates substantially. Still, because allowing closing firms to stay in the sample makes less sense when studying other outcomes of firm behavior (e.g. productivity), and because we want to contrast our results to the previous literature, we proceed by ignoring dynamic selection while noting that our estimates should likely be interpreted as a lower bound.

Next, we investigate if the effect of the crisis on employment varies by worker skills. Using US data, Mueller (2017) documents that in recessions, the pool of unemployed shifts toward high skilled workers and that the compositional shift is almost entirely driven by job separations. Mueller shows that introducing a constraint on cash flows in recessions produces more cyclical separations for high-ability workers and shows that a shift in unemployment toward high-ability workers is consistent with the data.[25] Figure 7 and 8 decompose the overall employment effects by worker skills.[26] The results reveal a slightly larger decrease in employment for low-skilled workers

[24] The reason why the survival rate is not always decreasing monotonically is the difficulty of identifying true firm closures using administrative data. In a few cases, firms may actually reappear after having been coded as closed (see e.g. Hethey-Maier and Schmieder 2013).
[25] In absence of financial constraints, firms are willing to pay workers above the value of their current marginal productivity if they are compensated by positive expected future returns. However, if firms face a constraint on their cash flow, workers and firms may separate even if it is in both parties' interest to continue the relationship.
[26] In the year 2000, there is a change in how educational attainment is coded. For this analysis, we therefore, only follow the firms until 1999.

(defined as those with at most two years of upper secondary education) than high-skilled workers (those with more than two years of upper secondary education). The difference is however small with an average decrease during the crisis (1991-1993) by about 10 percent for low skilled workers compared to 7.6 percent for high skilled workers (see Table A.1).

Mechanisms

In this section, we attempt to shed some light on the mechanisms through which the Swedish baking crisis affected firms' employment behavior. Before proceeding, it is useful to investigate the "first-stage" relationship between financial pressure and liquidity. Our idea is simple: If adverse shocks to bank health, higher agency costs, weakened cash flow, or depleted collateral increased financial pressure relatively more for highly indebted firms, we would expect to see that these firms experience larger increases in interest expenses and deplete their cash more quickly, relative to less indebted firms. Recall that Figures A.1 and A.2 showed that aggregate bank lending fell sharply and lending costs increased substantially at the national level. Here, we conduct a similar exercise at the firm level using our difference-in-differences estimator.

While our main dataset does not contain direct information on interest expenses, we collected these data from the Thompson Reuters database that covers some of the largest listed firms in Sweden. A drawback of the data is that the sample is small: only 79 firms. On the other hand, these large firms represented a substantial share of the aggregate employment in Sweden. To increase power, we estimate a slightly less flexible model than that described in equation (1), where we focus on the interaction between a dummy set to unity for firms that belonged to the top quartile in the pre-crisis debts-to-assets distribution and period dummies (crisis and post-crisis), and to zero

otherwise. The results shown in Table 2 are consistent with highly indebted firms experiencing larger increases in their interest rate expenses. The estimate in column (1) suggests that these firms had about 30 percent higher interest expenses than less constrained firms during the crisis. Given the small number of firms, the precision of the estimate is, however, somewhat poor (p-value: .140). We also see that these firms experienced larger reductions in cash-flow/sales during the crisis. The statistically significant estimate in column (2) suggests a drop in cash-flow/sales by about 23.1 percent relative to firms in the first three quartiles of the pre-crisis debt ratio distribution. Note that both in column (1) and column (2), the estimates for the post-crisis period are close to zero and far from being statistically significant, suggesting that the effect's timing is consistent with the timing of the crisis. Also note that these estimates potentially represent a lower bound of the actual impact since the large listed firms in our sample are more likely to have had access to credits from other sources (e.g. trade credits, cooperate bonds).

An alternative way to verify that the crisis increased financial pressure for indebted firms is to examine whether those firms that were the most financially constrained before the crisis also more quickly depleted their liquid assets. While our full sample of firms lacks data on cash flow, we approximate this by studying the change (i.e. the growth rate) in current assets (i.e. cash equivalents and other short term assets).[27] Finding a larger decrease in the growth rate of current assets among the most exposed firms compared to the less exposed firms would suggest that these firms were more likely to deplete their liquid assets in response to the crisis. Figure 9 shows that there is indeed a significant drop in the growth rate of current assets for firms in Q2-Q4 during the years 1991-1993 While the *F*-stats clearly reject the null hypothesis that the coefficients on the

[27] Note that current assets is a suboptimal proxy for cash reserves since it includes the stock of inventory, and we would expect the stock of inventory to increase as a result of reduced demand.

interaction between year and debt ratio quartile are zero for all quartiles, they cannot reject the null in the pre-crisis period.

Overall, we interpret these results as suggestive evidence that increased financial pressure induced by the financial crisis lead to higher interest rate expenses and reduced liquidity for the most exposed firms.

Firms may react to increased financial pressure not only by cutting employment but also by adjusting wages. If wages are flexible, a fall in wages may mitigate some of the employment losses. On the other hand, there is a wealth of evidence suggesting that wage rigidity is more prominent in Sweden than in other similar countries (e.g. Bennmarker and Agell 2003). Figure 10 presents evidence on the effect on wages.[28] The results suggest no significant impact of the crisis on wages. The estimates are also rather precise and allow us to rule out large to moderate effect sizes. This is consistent with a story in which Sweden's wage rigidities forced firms to reduce labor costs by laying off workers rather than by adjusting wages.

It is also possible that limited access to external finance affects firms' investment in capital (e.g. Fazzari, Hubbard and Petersen 2000). To the extent that capital and labor are complements in the production, employment will also vary with the level of investment. However, there is a debate in the macroeconomic literature regarding the link between access to external capital and investment where, e.g., Kaplan and Zingales (1997) argue that there is no such link. We investigate this by

[28] Wages are measured as the annual earnings from a specific firm. We drop workers' earning less than 50,000 SEK to focus on full time workers.

estimating model (1) using the inverse hyperbolic sine of the stock of machines and land as the dependent variable. The results from our analysis are presented in Figure 11. Although the estimates are fairly imprecise, likely due to the sampling scheme used by Statistics Sweden, we do find a significant short-run large negative effect of the crisis on investment in 1993. For instance, during the crisis investments fell by 44.5 percent more for firms in Q4 relative to firms in Q1.[29]

It is also conceivable that firms suffering from reduced access to credit experience lower productivity (see Huber 2018). One reason could be reduced innovation. Such productivity losses may explain the persistent employment reductions we observe and have been suggested as a potential mechanism for labor market hysteresis.[30] Our results in Figure 11 reveal that value added per worker fell in the more exposed firms, but there is no clear monotonic relation between pre-crisis debt level and effect size. The fact that productivity recovered within a few years after the crisis suggests that productivity losses alone cannot explain the persistently higher unemployment rate. However, note that there are clear signs of differential pre-crisis trends, suggesting that some caution is warranted when interpreting these results.

5. CONCLUDING REMARKS

The ability to design policies to effectively combat contemporary employment problems hinges on our understanding of how labor markets work. We know surprisingly little about what role of financial crises play for the behavior of firms. The banking crisis which hit the Swedish economy in the early 1990s has gained increased interest from policy commentators, as it provides an

[29] This number is fairly consistent with aggregate statistics that show a drop in investments during the crisis by about 35 percent.
[30] The other mechanisms include union bargaining favoring inside workers and skill depreciation among long-term unemployed.

insightful, yet previously unexplored, case-study with high relevance to improve our understanding of how financial constraints impact firm behavior. This paper uses the Swedish experience to shed some light on the mechanisms through which financial crises may affect firms' employment decisions and behavior in the long run. The analysis draws on rich matched employer-employee containing the financial statements of the firms.

The results from our analysis suggest that firms that are more heavily dependent on external capital to finance their operations experience stronger downward employment adjustments in the presence of reduced borrowing opportunities than firms that are less dependent on external sources of financing. The estimates suggest that these firms reduced employment by about 18 percent more than firms in the first quartile. Employment did not completely recover until about one decade after the crisis. This insight is important as the existing literature has mainly investigated the short-rum (2-4 years) employment effects of the Great Recession. The employment contraction is primarily explained by an increase in worker separations. We also find that the employment-loss is relatively larger for low-skilled workers. While we find no evidence that the crisis significantly affected wages, we find that both investment and value added per worker fell more in the most exposed firms, but that productivity quickly recovered.

Our results suggest that policies to ease financial constraints, stabilize financial markets, and increase liquidity in firms may be used as a tool to stimulate aggregate employment during financial crises. One example of such policy is the "Quickpay" reform implemented during the Great Recession, whereby the US government accelerated payments to a subset of small business

contractors, reducing the time taken between invoice receipt and payment to improve their cash flows and stimulate employment (Barott and Nanda 2018).

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Table 1 Summary statistics, by quartile of total-debt-to-total-assets ratio

	Q_1	Q_2	Q_3	Q_4	Total
Number of employees	341 (1,577)	219 (763)	176 (555)	127 (346)	216 (939)
Wages (labor costs)/worker	28,249 (107,805)	26,647 (79,701)	23,830 (36,432)	29,151 (133,027)	26,969 (96,179)
Value added/worker	60,209 (239,579)	55,078 (185,079)	44,072 (61,778)	55,061 (388,213)	53,606 (248,073)
% College educated	7.8 (9.3)	7.9 (11.2)	8.3 (11.8)	8.4 (11.6)	8.1 (11.0)
% Firms in:					
Manufacturing	80.2 (39.8)	69.2 (46.2)	61.5 (48.7)	52.3 (50.0)	65.8 (47.4)
Retailing, restaurants and hotels	9.49 (29.3)	14.1 (34.8)	15.4 (36.1)	16.3 (36.9)	13.8 (34.5)
Construction	3.3 (17.8)	7.2 (25.8)	11.1 (31.4)	14.5 (35.2)	9.0 (28.6)
Transportation and communication	2.3 (15.1)	3.3 (17.8)	3.1 (17.3)	6.7 (25.0)	3.8 (19.2)
Law, accounting, marketing etc.	1.2 (11.0)	2.7 (16.2)	4.4 (20.4)	5.2 (22.2)	3.4 (18.1)
Health care, hair and beauty, laundry, repair etc.	1.6 (12.5)	2.5 (15.7)	3.1 (17.4)	2.6 (15.8)	2.5 (15.5)
Other industries (electricity, heating, mining, agriculture etc.	1.9 (13.5)	1.0 (10.1)	1.5 (12.0)	2.5 (15.5)	1.7 (12.9)

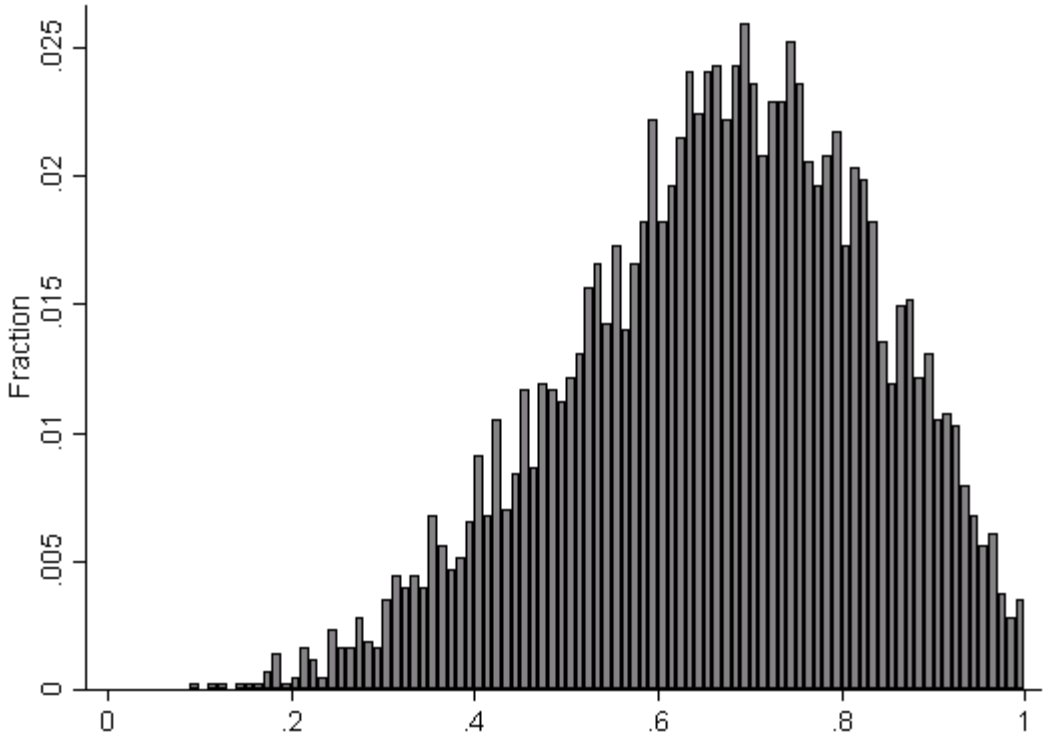
Notes: This table provides summary statistics for firms in our sample (N=4,723). Public sector firms and financial firms are also excluded. The sample is partitioned into quartiles based on each firm's position in the distribution of total debt to total assets between 1985-1989. Statistics for each group are displayed in the columns. All statistics are averages and standard deviation (in parentheses) over the years 1985–1989. All monetary variables are expressed in 1,000 USD and inflated to 2017 prices.

Table 2. Effects of the financial crisis on interest rate expenses and liquidity for the largest listed firms

Dependent variable:	(log) Interest expenses (1)	(log) Cash-flow/sales (2)
Coeff. on High debt firm×Crisis	.302	-.231
	(.206)	(.133)
Coeff. on High debt firm×Post crisis	-.074	-.060
	(.300)	(.141)
Firm FEs	Yes	Yes

Notes: The sample consists of firms listed on Sweden's largest stock exchange (the General Index) observed at least once in the period 1985-1989. The number of firms is 79, and the number of firm-year observations is 997. High debt firm is a dummy equal to 1 if the firm was in the top quartile of the pre-crisis debts-to-assets distribution and zero otherwise. Crisis is a dummy equal to 1 for the years 1990–1993 and zero otherwise. Post crisis is a dummy equal to 1 for the period 1994–2005. Standard errors are shown in parentheses.

Figure 1 Distribution of total-debt-to-total-assets ratio in 1985-1989



Notes: This figure shows the distribution of the total-debt-to-total assets-ratio in 1985-1989 for firms in our sample (N=4,723). Non-corporate businesses (i.e. sole proprietorships or partnerships) are excluded. Public sector firms and financial firms are also excluded.

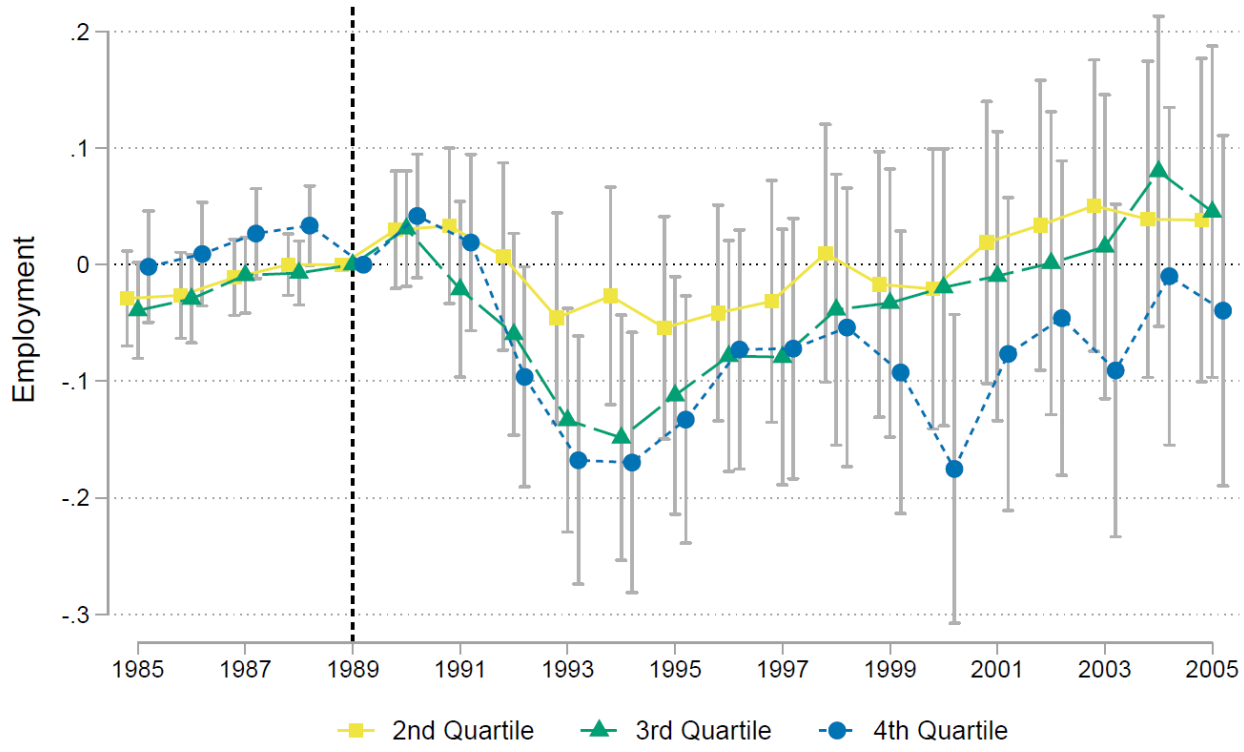


Figure 2: The effect of the crisis on employment

Notes: The dots represent the coefficients on year interacted quartile of debt ratio in a firm fixed effects regression where the dependent variable is the log number of employees. The regression controls for year-by-county fixed effects. The estimates should be interpreted in relation to employment in 1989 of firms in the first quartile of the debt ratio distribution where the debt ratio is measured as an average over the period 1985–1989.

p-values for joint significance in pre-crisis period 1985–11988; Q2: 0.344, Q3: 0.146, Q4: 0.052.
p-values for joint significance in crisis period 1991–1993; Q2: 0.243, Q3: 0.041 Q4: 0.001.

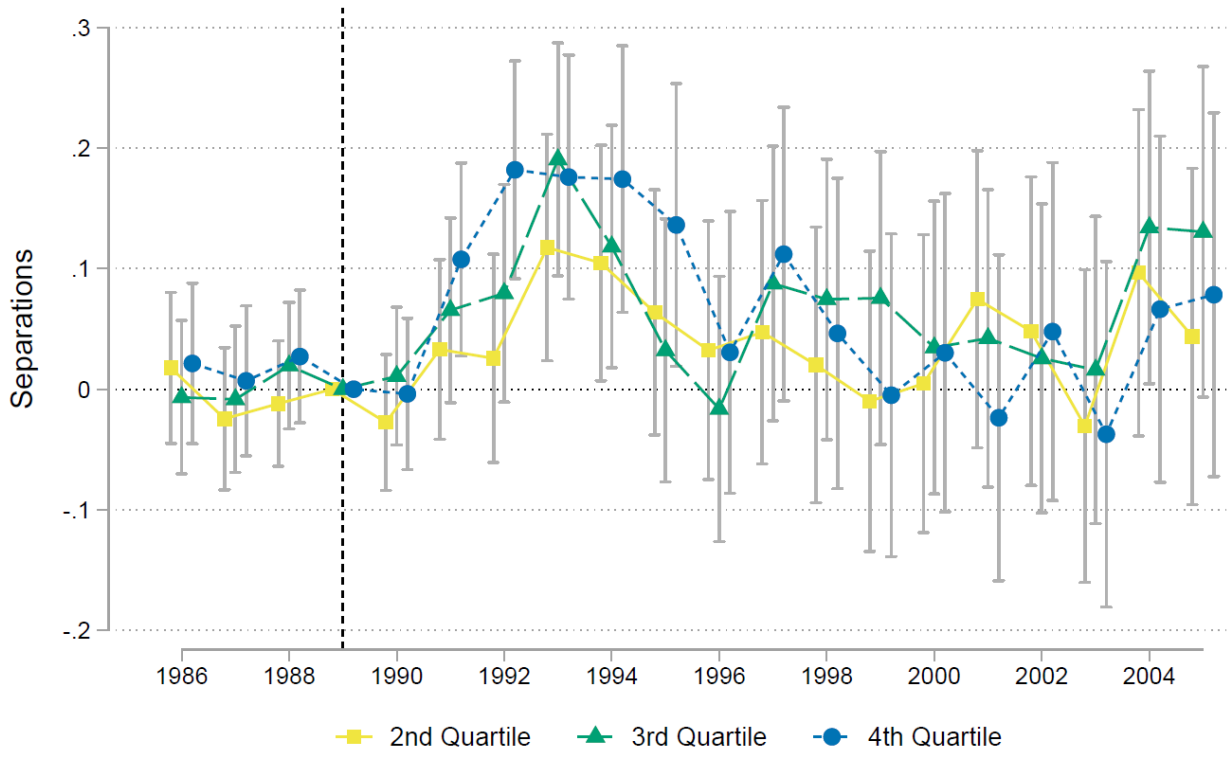


Figure 3: The effect of the crisis on separations

Notes: The dots represent the coefficients on year interacted with debt rate quartile in a firm fixed effects regression. The dependent variable is the (inverse hyperbolic sine of) number of worker separations in each firm. The regression controls for year-by-county fixed effects. The estimates should be interpreted in relation to separations in 1989 for firms in the first quartile of the debt ratio distribution where the debt ratio is measured as an average over the period 1985–1989.

p-values for joint significance in pre-crisis period 1985–1988; Q2: 0.413, Q3: 0.727, Q4: 0.724.
p-values for joint significance in crisis period 1991–1993; Q2: 0.073, Q3: 0.001 Q4: 0.001.

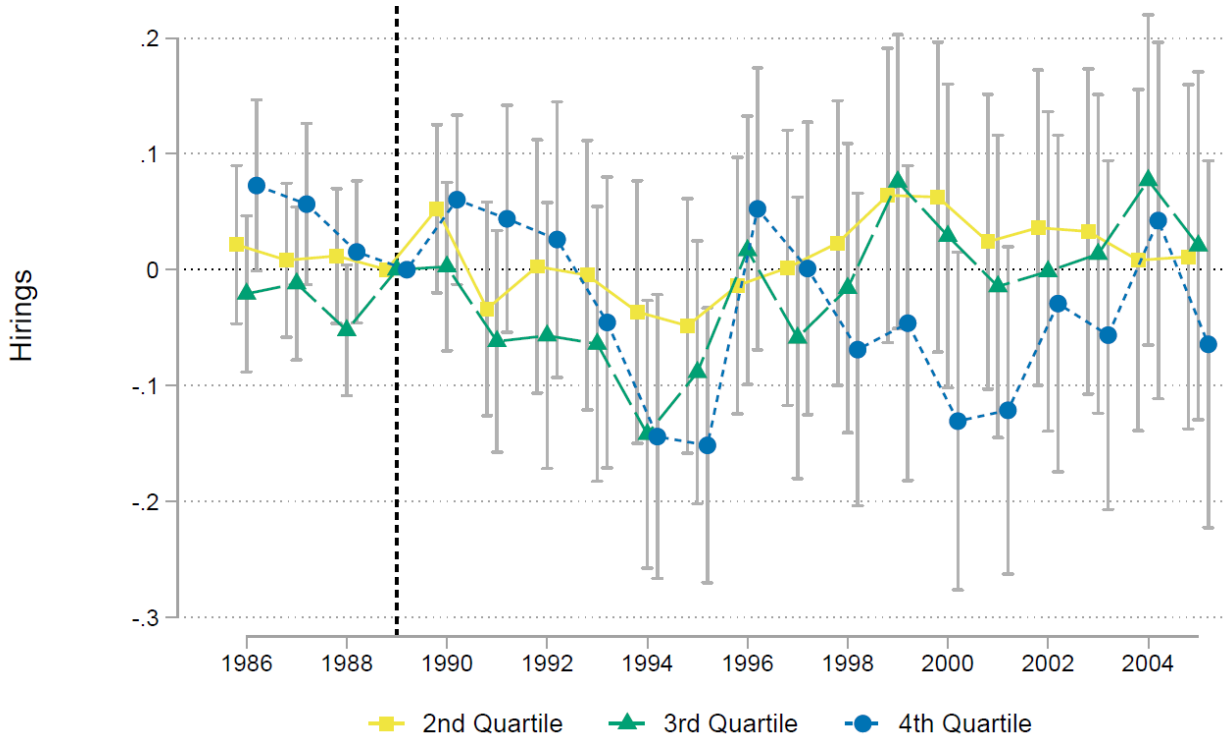


Figure 4: The effect of the crisis on hirings

Notes: The dots represent the coefficients on year interacted with debt rate quartile in a firm fixed effects regression. The dependent variable is the (inverse hyperbolic sine of) number of worker hirings in each firm. The regression controls for year-by-county fixed effects. The estimates should be interpreted in relation to hirings in 1989 for firms in the first quartile of the debt ratio distribution where the debt ratio is measured as an average over the period 1985–1989.

p-values for joint significance in pre-crisis period 1985–1988; Q2: 0.928, Q3: 0.265, Q4: 0.174.
p-values for joint significance in crisis period 1991–1993; Q2: 0.853, Q3: 0.585 Q4: 0.446.

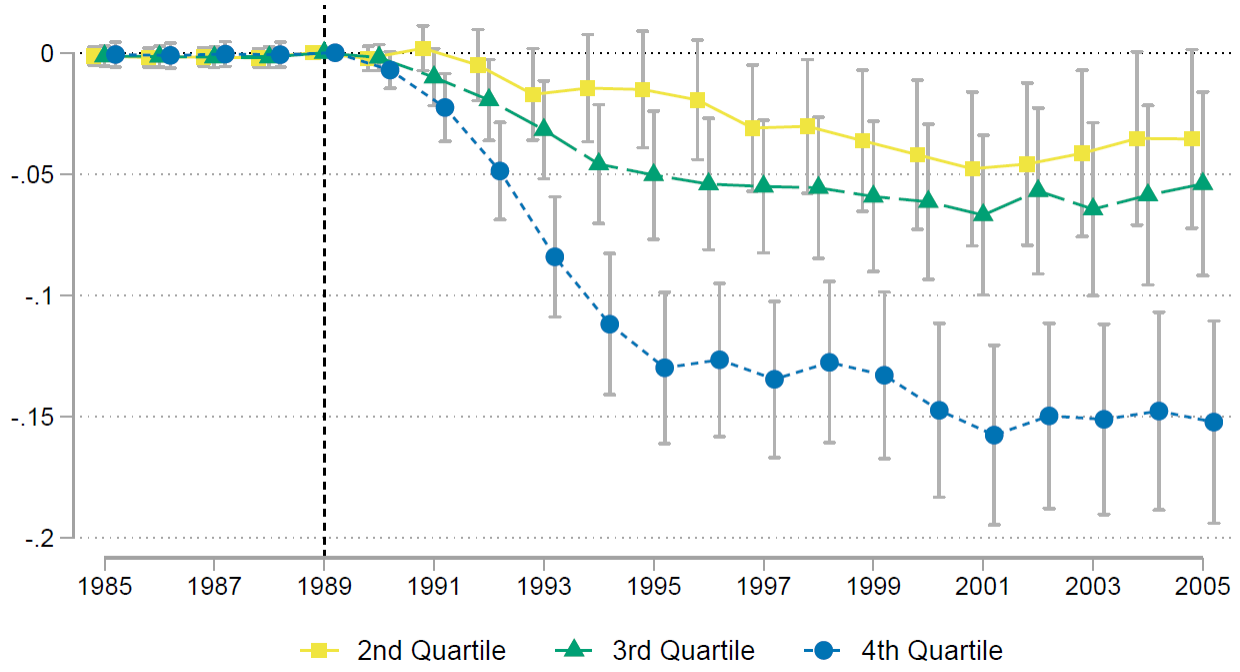


Figure 5: The effect of the crisis on firm survival

Notes: The dots represent the coefficients on year interacted with debt rate quartile in a firm fixed effects regression. The dependent variable is an indicator set to unity if the firm still exists in a given year and zero otherwise. The regression controls for year-by-county fixed effects.

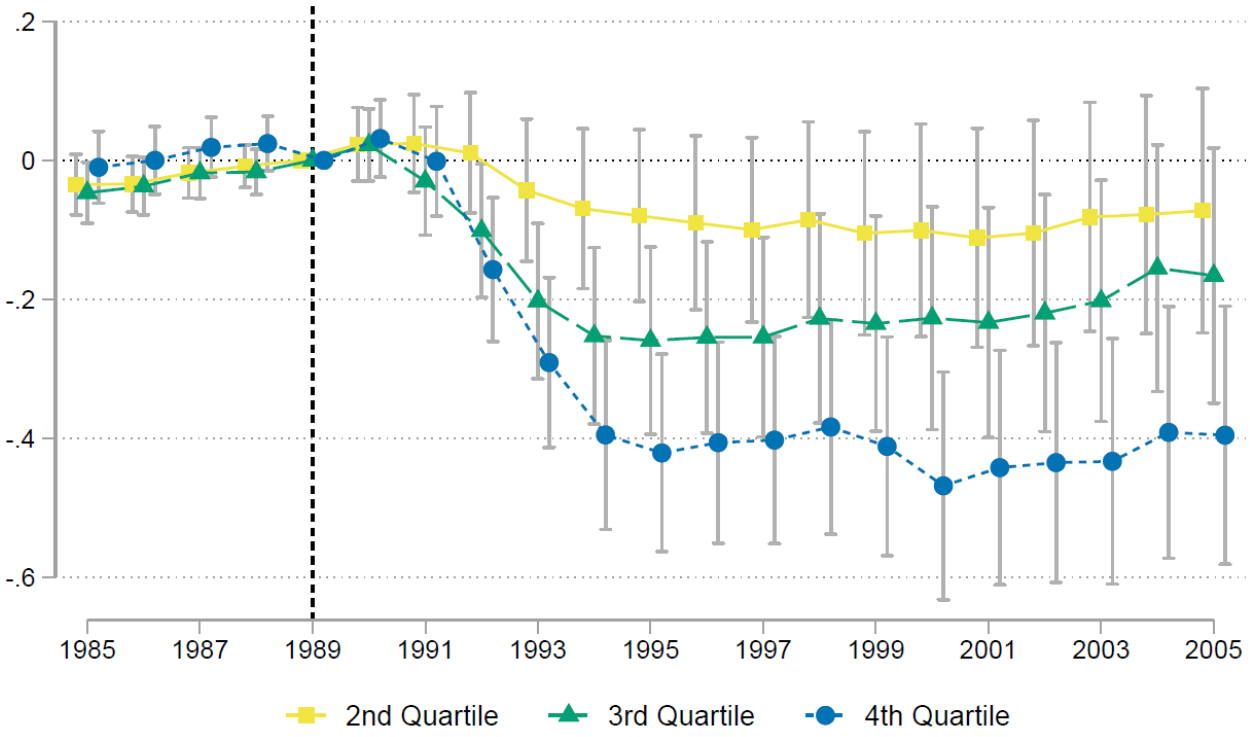


Figure 6: The potential role of dynamic selection for the effect of the crisis on employment

Notes: The dots represent the coefficients on year interacted with debt rate quartile in a firm fixed effects regression. The dependent variable is the log number of employees, and the regression controls for year-by-county fixed effects. The estimates should be interpreted in relation to employment in 1989 of firms in the first quartile of the debt ratio distribution where the debt ratio is measured as an average over the period 1985–1989.

p-values for joint significance in pre-crisis period 1985–1988; Q2: 0.344, Q3: 0.146, Q4: 0.052.
p-values for joint significance in crisis period 1991–1993; Q2: 0.242, Q3: 0.001 Q4: 0.001.

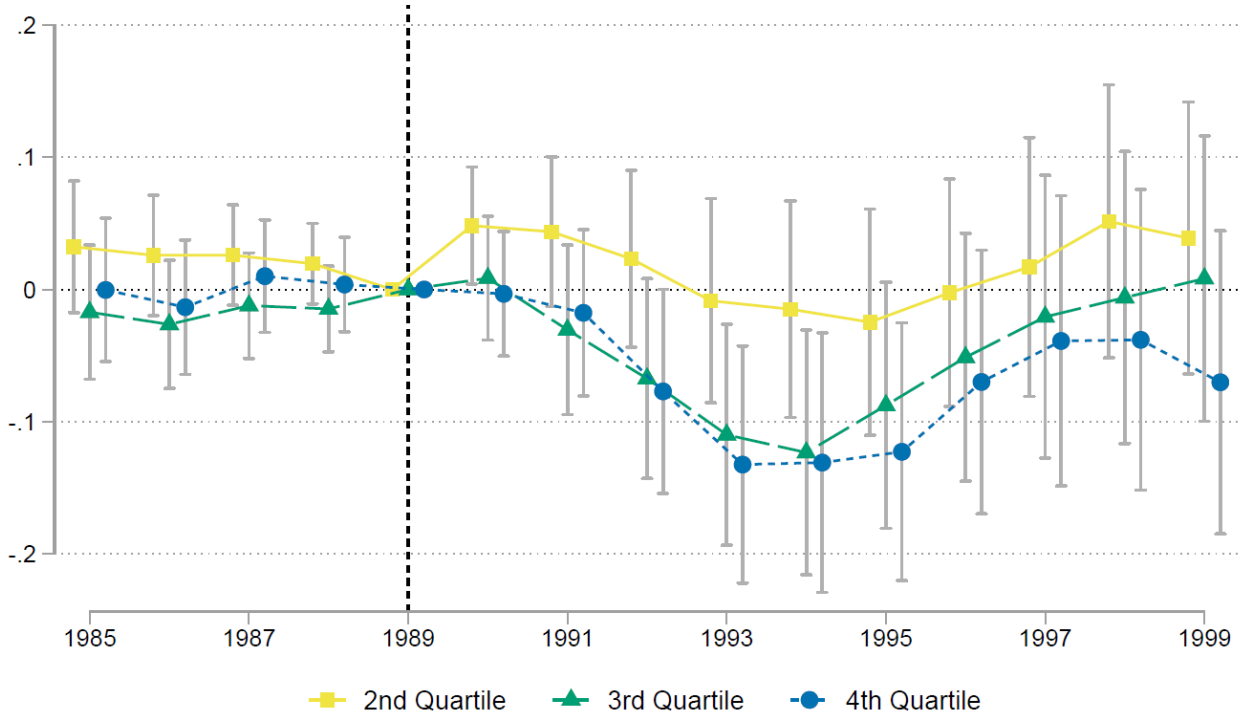


Figure 7: The effect of the crisis on high skilled employment

Notes: The dots represent the coefficients on year interacted with debt rate quartile in a firm fixed effects regressions. The dependent variable is the log number of high skilled workers defined as workers with at least three years of upper secondary education. The regression controls for year-by-county fixed effects. The estimates should be interpreted in relation to the year 1989 for firms in the first quartile of the debt ratio distribution where the debt ratio is measured as an average over the period 1985–1989.

p-values for joint significance in pre-crisis period 1985–1988; Q2: 0.672, Q3: 0.755, Q4: 0.595. *p*-values for joint significance in crisis period 1991–1993; Q2: 0.154, Q3: 0.051 Q4: 0.042.

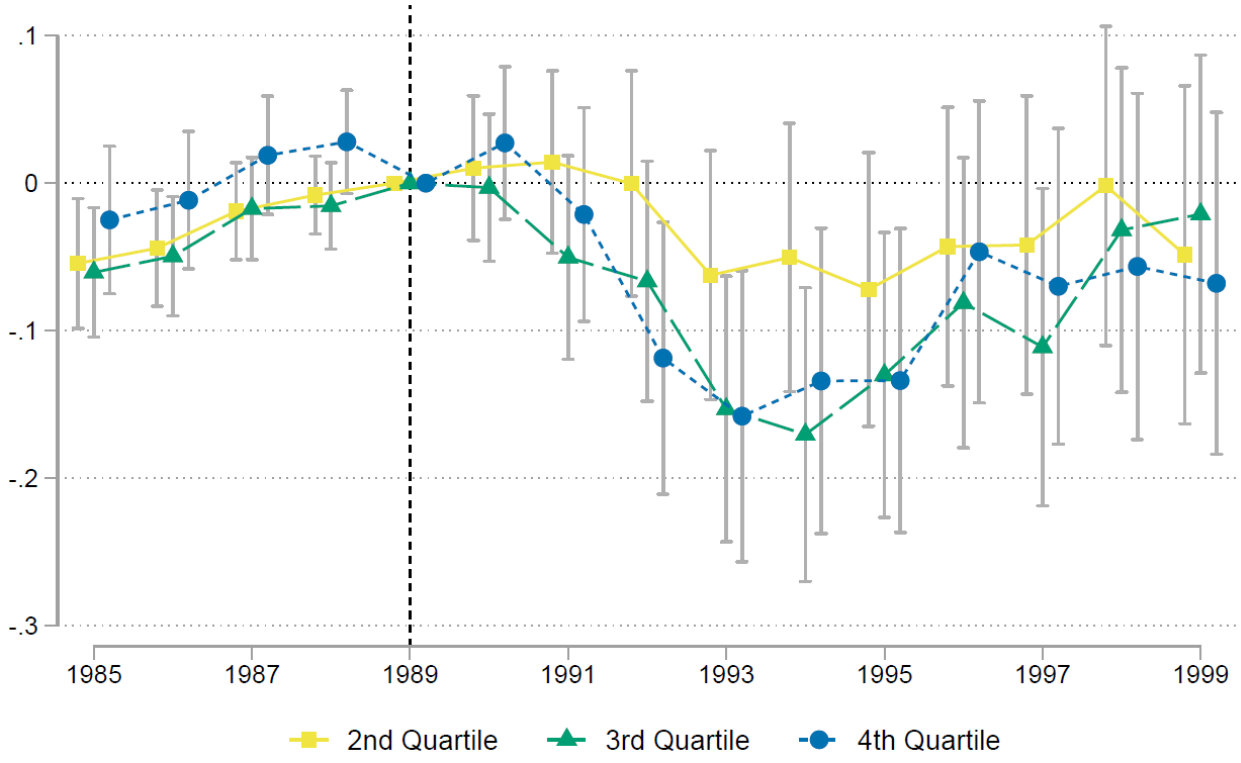


Figure 8: The effect of the crisis on low skilled employment

Notes: The dots represent the coefficients on year interacted with debt rate quartile in a firm fixed effects regressions where the dependent variable is the log number of low skilled workers defined as workers with at most two years of upper secondary education. The regression controls for year-by-county fixed effects. The estimates should be interpreted in relation to the year 1989 for firms in the first quartile of the debt ratio distribution where the debt ratio is measured as an average over the period 1985–1989.

p-values for joint significance in pre-crisis period 1985–1988; Q2: 0.081, Q3: 0.001, Q4: 0.001.
p-values for joint significance in crisis period 1991–1993; Q2: 0.236, Q3: 0.001 Q4: 0.001.

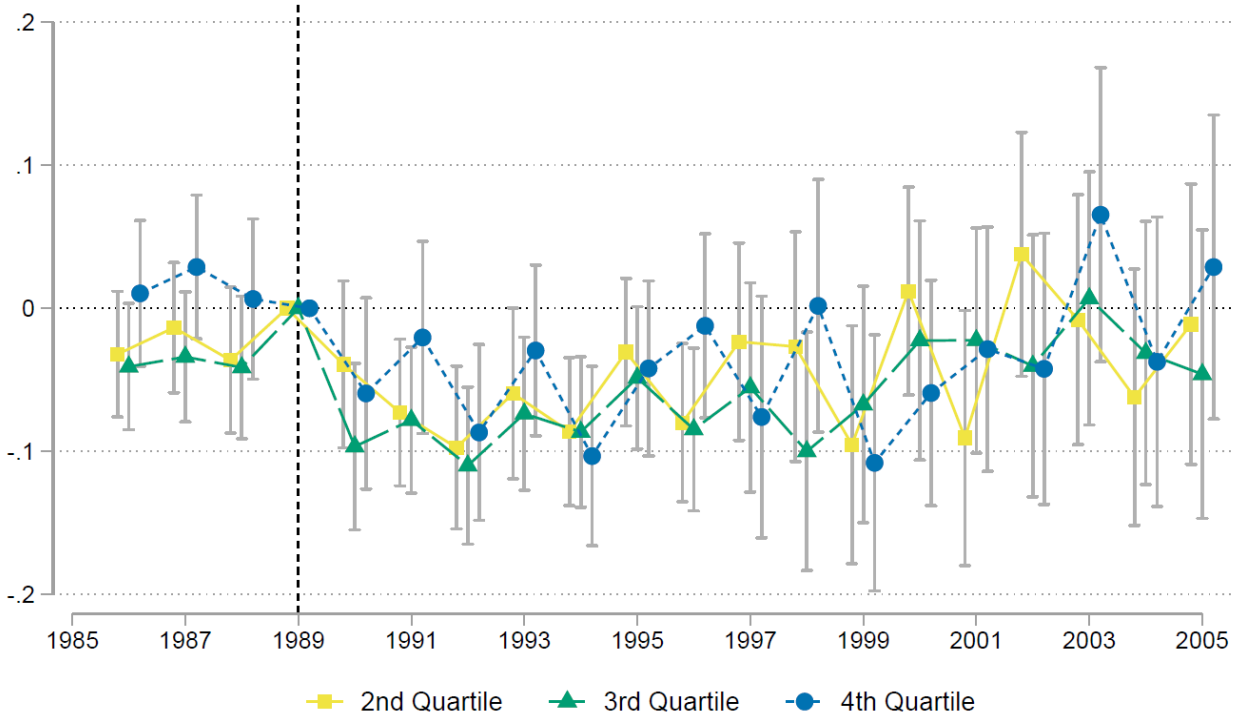


Figure 9: The effect of the crisis on liquidity

Notes: The dots represent the coefficients on year interacted with debt rate quartile in a firm fixed effects regression where the dependent variable is the log change in current assets between year t and year t-1. The regression controls for year-by-county fixed effects. The estimates should be interpreted in relation to the year 1989 for firms in the first quartile of the debt ratio distribution where the debt ratio is measured as an average over the period 1985–1989. The variable is winsorized at the top 5 percent.

p-values for joint significance in pre-crisis period 1985*p*1988; Q2: 0.422, Q3: 0.306, Q4: 0.663.
p-values for joint significance in crisis period 1991*p*1993; Q2: 0.002, Q3: 0.001 Q4: 0.048.

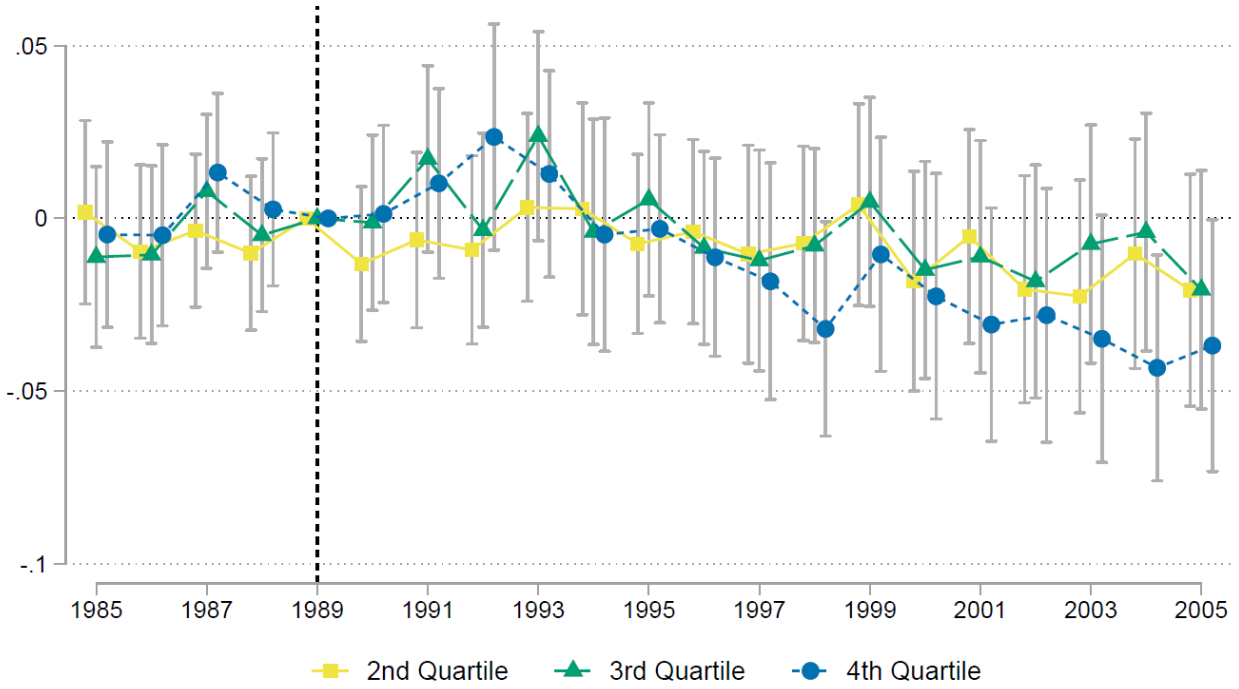


Figure 10: The effect of the crisis on the wage bill

Notes: The red line represents the coefficients on year interacted with debt rate quartile in a firm fixed effects regressions where the dependent variable is the log average earnings in each specific firm for workers earning at least 50,000 SEK on an annually (in 2017 prices). The regression controls for year-by-county fixed effects. The estimates should be interpreted in relation to the year 1989 for firms in the first quartile of the debt ratio distribution where the debt ratio is measured as an average over the period 1985–1989.

p-values for joint significance in pre-crisis period 1985–1988; Q2: 0.729, Q3: 0.379, Q4: 0.451.
p-values for joint significance in crisis period 1991–1993; Q2: 0.705, Q3: 0.068 Q4: 0.566.

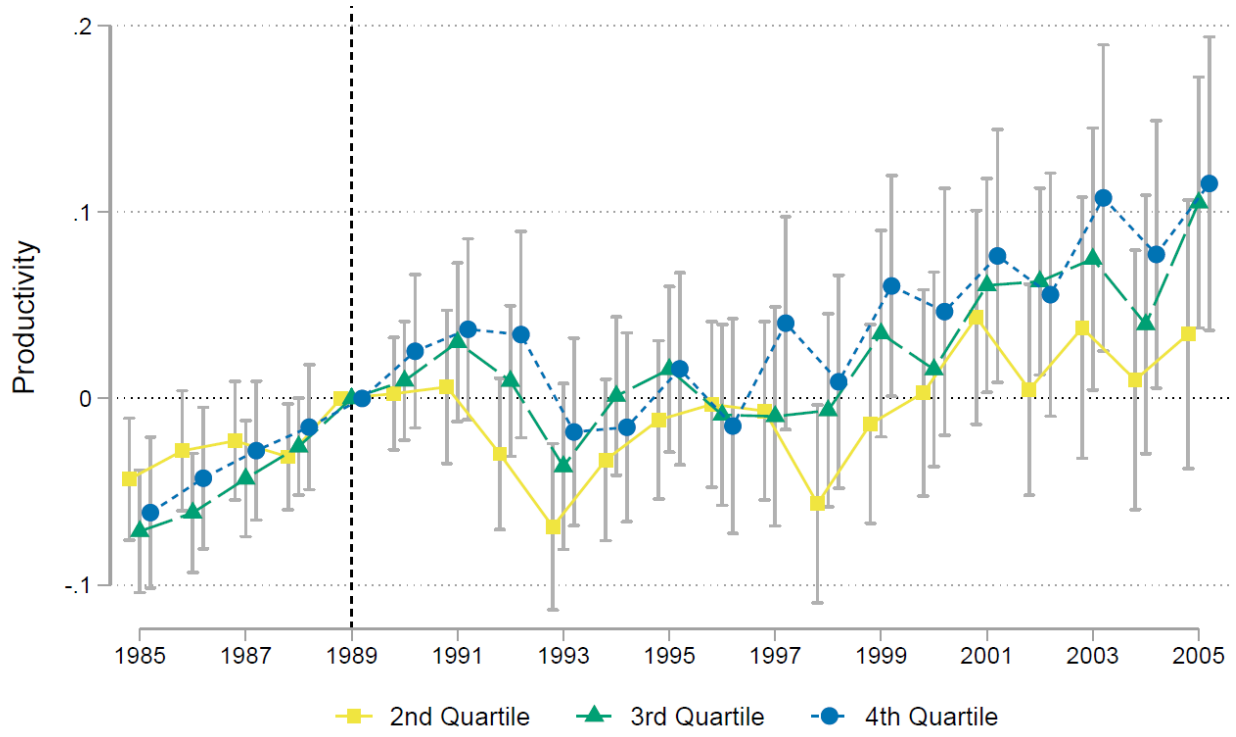


Figure 11: The effect of the crisis on productivity

Notes: The dots represent the coefficients on year interacted with debt rate quartile in a firm fixed effects regressions where the dependent variable is the log value added per worker. The regression controls for year-by-county fixed effects. The estimates should be interpreted in relation to the year 1989 for firms in the first quartile of the debt ratio distribution where the debt ratio is measured as an average over 1985–1989. The value-added variable is winsorized at the top 5 percent.

p-values for joint significance in pre-crisis period 1985–1988; Q2: 0.065, Q3: 0.000, Q4: 0.033.
p-values for joint significance in crisis period 1991–1993; Q2: 0.005, Q3: 0.042 Q4: 0.086.

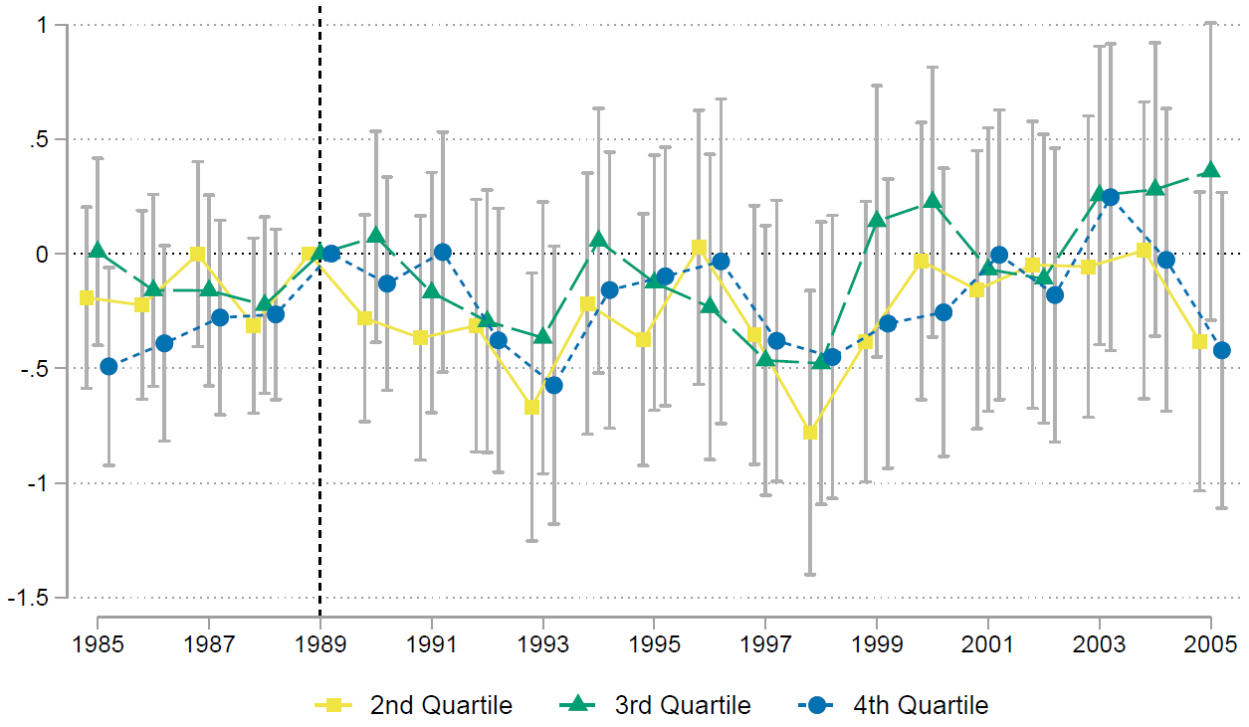


Figure 12: The effect of the crisis on investment

Notes: The dots represent the coefficients on year interacted with debt rate quartile in a firm fixed effects regressions where the dependent variable is the log difference in the stock of machines and buildings between year *t* and year *t*-1. The regression controls for year-by-county fixed effects. The estimates should be interpreted in relation to the year 1989 for firms in the first quartile of the debt ratio distribution where the debt ratio is measured as an average over 1985–1989. The investment variable is winsorized at the top 5 percent.

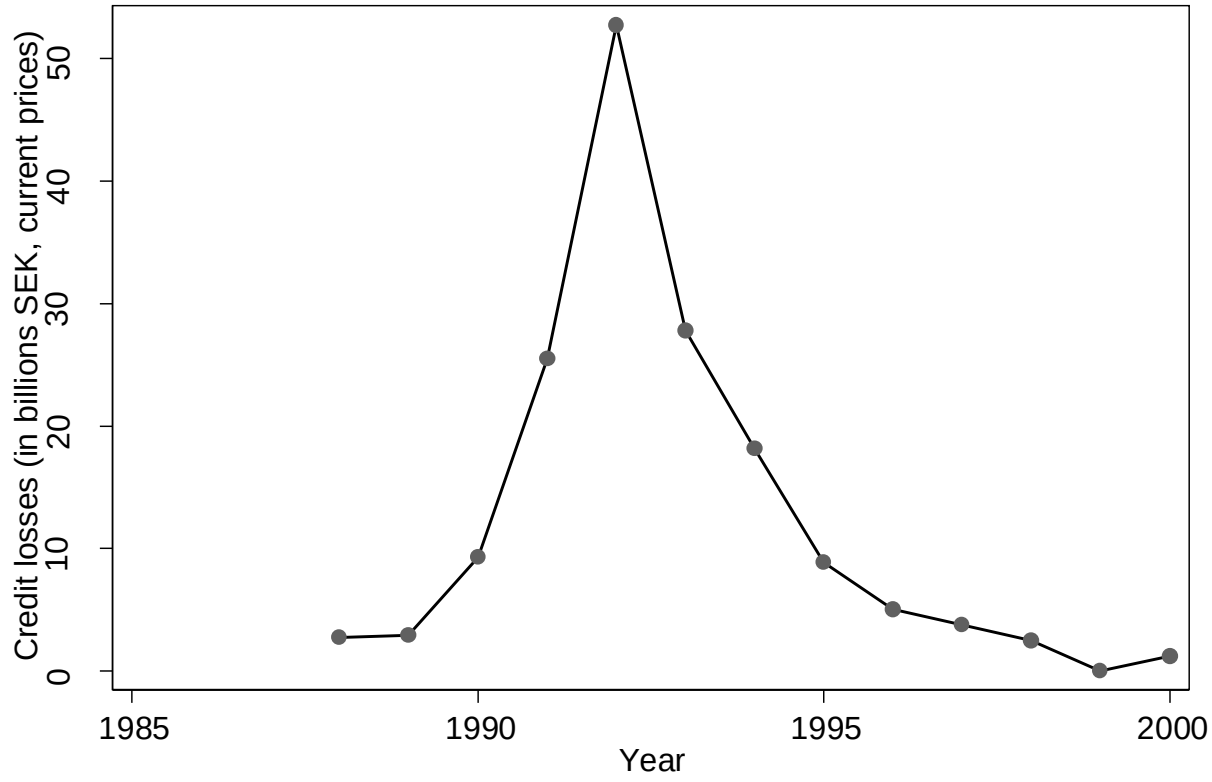
p-values for joint significance in pre-crisis period 1985–1988; Q2: 0.195, Q3: 0.240, Q4: 0.129.
p-values for joint significance in crisis period 1991–1993; Q2: 0.930, Q3: 0.967 Q4: 0.294.

Online Appendix

Figure A.1 Bank lending in Sweden 1985–2000

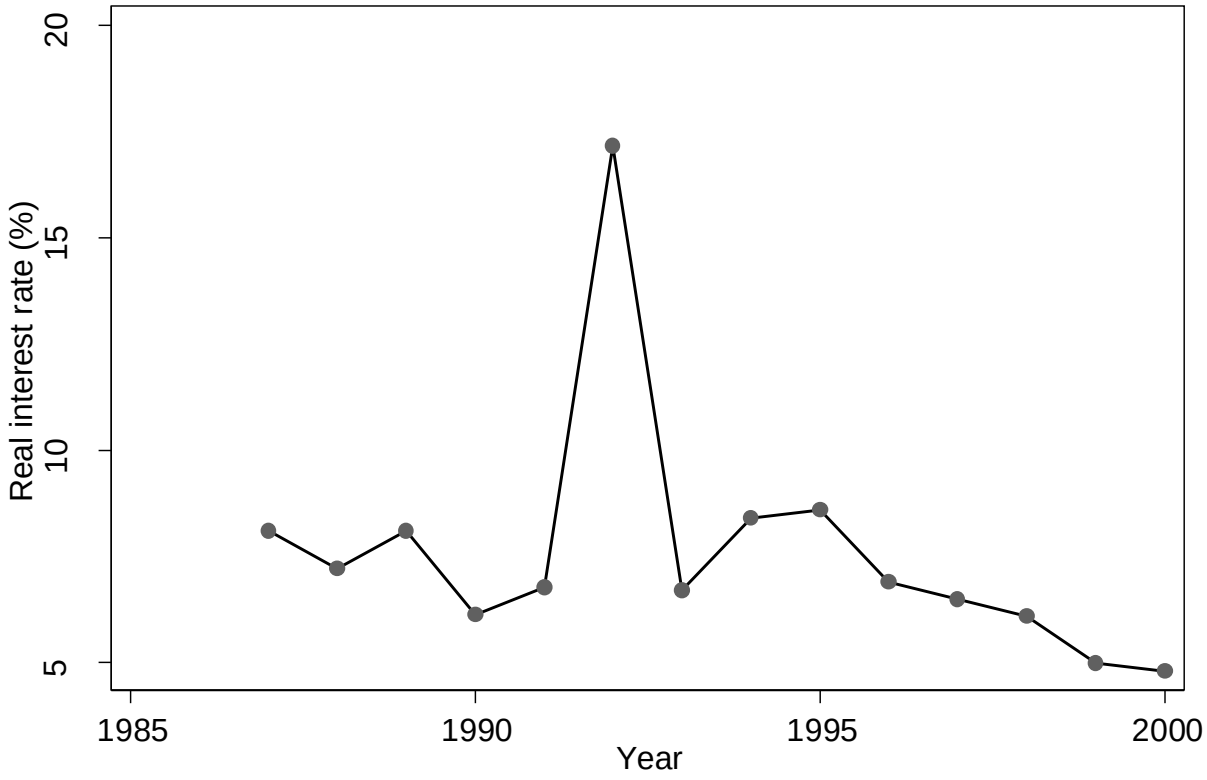
Notes: The figure shows the average bank lending in billions of SEK (current prices) to firms or households. Authors' calculations based on data from the Central Bank's *Statistisk Årsbok*.

Figure A.2 Credit losses in the three largest Swedish commercial banks 1987–2000



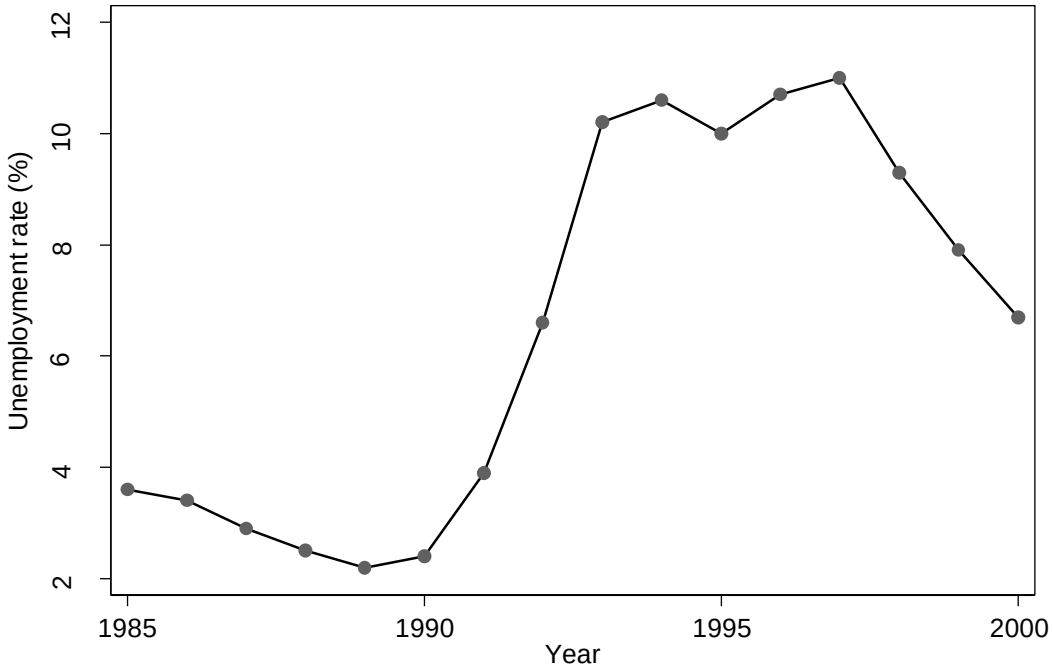
Notes: The figure shows credit losses in billions of SEK (current prices) is for Sweden's three largest commercial banks. Authors' calculations based on data from the volumes *Bankernas kreditförluster 1988–1995* and *Bankkrisen*.

Figure A.3 Average bank lending rate in Sweden 1987-2000



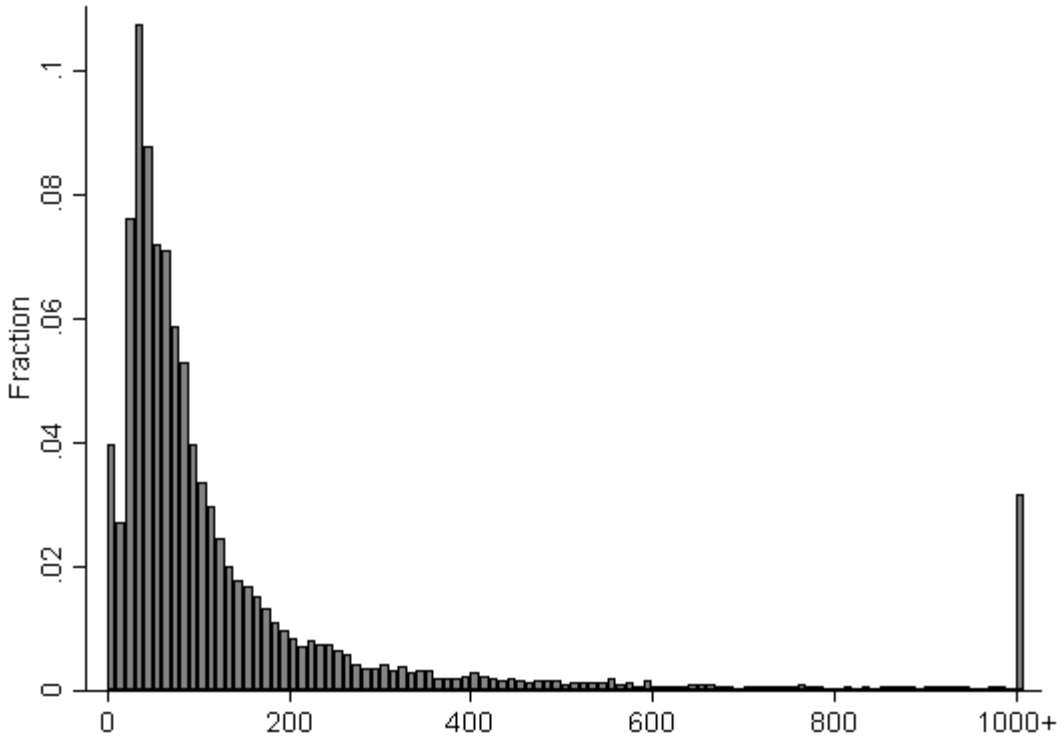
Notes: The figure shows the average bank lending real interest rate. Authors' calculations based on data from The Central Bank's *Statistisk Årsbok*.

Figure A.4 Aggregate unemployment in Sweden 1985-2000



Notes: The figure shows the evolution of the unemployment rate in Sweden among individuals aged 16–64 for the period 1985–2000. *Source*: Statistics Sweden.

Figure A.5: Firm size distribution



Notes: This figure shows the distribution of the average number of employees for firms in our sample over the period 1985–1989. Non-corporate businesses (i.e. sole proprietorships or partnerships) are excluded. Public sector firms and financial firms are also excluded.

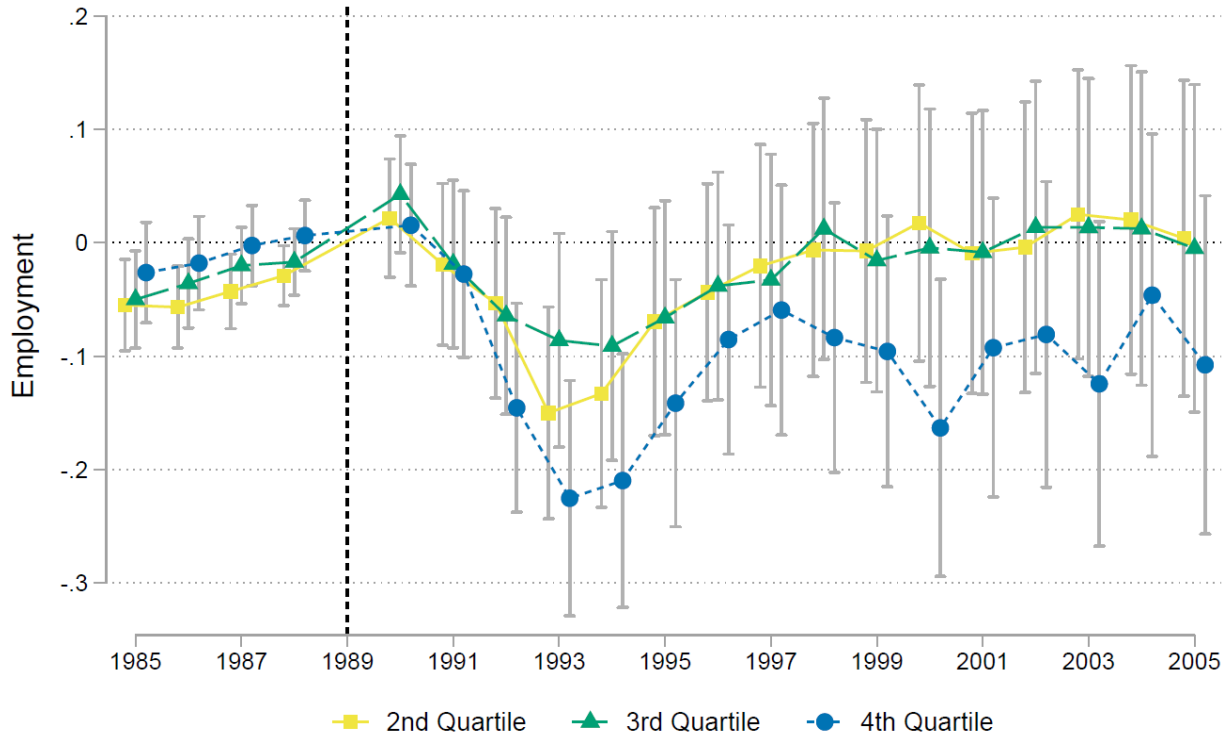


Figure A.6: Ranking firm debt within 1-digit industry

Notes: The dots represent the coefficients on year interacted with debt rate quartile in a firm fixed effects regression where the dependent variable is the log number of employees. The regression controls for year-by-county fixed effects. The estimates should be interpreted in relation to employment in 1989 of firms in the first quartile of the debt ratio distribution where the debt ratio is measured as an average over the period 1985–1989.

p-values for joint significance in pre-crisis period 1985–1988; Q2: 0.046, Q3: 0.136, Q4: 0.297.
p-values for joint significance in crisis period 1991–1993; Q2: 0.007, Q3: 0.027 Q4: 0.001.

Table A.1. Summary of the effects of the crisis

Dependent variable:	Employment (Fig. 2)	Separations (Fig. 3)	Hirings (Fig. 4)	Low skill empl. (Fig. 7)	High skill empl. (Fig. 8)
Panel A Employment outcomes					
Coeff. on Q2×Crisis	-.002	.059	-.012	-.016	.019
	(.035)	(.036)	(.045)	(.032)	(.030)
Coeff. on Q3×Crisis	-.071	.112	-.061	-.090	-.069
	(.037)	(.036)	(.046)	(.034)	(.033)
Coeff. on Q4×Crisis	-.082	.155	.008	-.100	-.076
	(.040)	(.037)	(.048)	(.038)	(.034)
Panel B Other outcomes	Liquidity (Fig. 9)	Wages (Fig. 10)	Productivity (Fig. 11)	Investments (Fig. 12)	
Coeff. on Q2×Crisis	-.077	.004	-.031	-.471	
	(.022)	(.012)	(.018)	(.257)	
Coeff. on Q3×Crisis	-.087	.013	.001	-.162	
	(.022)	(.013)	(.018)	(.259)	
Coeff. on Q4×Crisis	-.046	.016	.017	-.445	
	(.025)	(.013)	(.022)	(.254)	

Notes: The table summarizes the figures that provide a visual presentation of the main results by displaying the average of the coefficients on the interaction between pre-crisis debt quartile and year for the crisis period 1991-1993. The regression specification is identical to that in the figures. The estimates should be interpreted in relation to employment in 1989 of firms in the first quartile of the pre-crisis debt ratio distribution. Standard errors are shown in parentheses.