When Credit Dries Up: Job Losses in the Great Recession

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Abstract

We study whether the solvency problems of Spain's weakest banks in the Great Recession have caused employment losses outside the financial sector. Our analysis focuses on the set of banks that were bailed out by the Spanish authorities. Data from the credit register of the Bank of Spain indicate that these banks curtailed lending well in advance of their bailout. We show the existence of a credit supply shock, controling for unobserved heterogeneity through firm fixed effects, and assess its impact on employment. To this aim, we compare the changes in employment between 2006 and 2010 at client firms of weak banks to those at comparable firms with no significant pre-crisis relationship to weak banks. Our estimates imply that around 24% of job losses at firms attached to weak banks in our sample are due to this exposure. This accounts for one-half of the employment losses at those that survived and one-third of employment losses at those that closed down.

Keywords: Job losses, Great Recession, credit constraints.
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1 Introduction

Do shocks to the banking system have real effects and, if so, do they cause job losses outside the financial sector? Both questions have strongly resurfaced in the wake of the economic and financial crisis that started in 2008. The renewed interest in the real effects of credit supply shocks is motivated by the exceptionally strong and persistent contraction of economic activity in the countries that suffered a banking crisis, like the US and several peripheral countries in Europe.

In this study we use data from the credit register of the Bank of Spain to analyze the link between the unprecedented drops in bank lending and employment in Spain. Our identification strategy exploits large differences in bank health observed at the onset of the crisis. The entire banking sector suffered the consequences of the collapse of the construction sector and the bursting of a housing bubble, and most banks were cut off from wholesale funding for a while, but the main problems were concentrated in savings banks (*cajas de ahorros*). We focus our attention on the ones that were bailed out by the Spanish government as part of a large-scale restructuring process that involved 32 savings banks and one commercial bank. We show that this set of *weak banks* started to curtail lending relative to the other banks almost two years ahead of the bailout process. Our main objective is to explore how this credit supply shock affected employment growth in 2006-2010 at the firms that maintained a pre-crisis relationship with any of these weak banks.

The Spanish economy provides an ideal setting to analyze this issue. Spanish firms are more reliant on bank credit than their counterparts in most advanced economies. In 2006, the stock of loans from credit institutions to non-financial corporations represented 86% of GDP compared to 62% in the European Union (European Central Bank, 2010). On the contrary, funding through financial markets is rarely used: on average only five large corporations per year issued publicly traded debt between 2002 and 2010, and the number of companies listed in the stock market is tiny. Finally, the vast majority of firms in Spain are small and medium-sized enterprises (SMEs), and many of them became highly leveraged prior to the recession.

We are obviously not the first to estimate the real effects of credit supply shocks.

A number of recent studies have demonstrated the adverse impact of such shocks on employment and investment (see Section 2). Most of this work is however based on either incomplete data on bank loans to the corporate sector –such as syndicated loans– or information about banking relationships rather than loans. One of the main contributions of our study is to use a comprehensive data set that includes loan level data. The credit register of the Bank of Spain provides exhaustive information about all bank loans to firms in the non-financial sector and these data are matched to balance sheet data for all banks and nearly 150,000 firms. We are thus able to trace all credit flows to a large sample of firms, for which we also have information on their credit history, such as loan defaults, and their applications for a first loan from other banks. Hence, we observe credit demand by firms that apparently need to establish a new banking relationship and the data from the credit register allow us determine whether the loan application was granted or not.

Our high-quality data let us perform more tests than related studies carry out and explore the presence of heterogeneous effects along many dimensions, but above all they allow us to resolve several key identification issues. The first challenge is the need to disentangle changes in credit supply from concurrent changes in credit demand. Between 2007 and 2009, the European Central Bank (ECB) Bank Lending Survey indices for Spain show a simultaneous increase of around 40% in the bank lending standards applied to non-financial firms and a similarly-sized drop in their loan demand (Banco de España, 2015).

To obtain clean identification of the credit supply shock we use the standard procedure of Khwaja and Mian (2008) to analyze credit growth at the bank-firm level. The results clearly demonstrate that weak banks curtailed credit vis-à-vis healthy banks that lent to the same firm. Thus, controling for firm fixed effects, we show that there was indeed a differential credit supply shock. Moreover, we also establish that the affected firms could not find new lenders to fully compensate this reduction in credit supply, which implies that the impact found at the bank-firm level was partially transmitted to the level of the firm. Finally, when we analyze employment growth we find that these firm-level credit shocks caused relatively large employment losses at firms with a pre-crisis relationship to weak banks. To capture these shocks, our baseline difference-in-differences specification includes a dummy variable for firms with a weak-bank loan-to-asset ratio above a minimum threshold.

The second main challenge is to control for selection effects. Before the crisis, healthy banks may have worked with better firms than weak banks, as indicated by differences in the observable characteristics between firms above and below the threshold for the weak bank loan-to-asset ratio. Failing to control for these differences would bias our estimates upwards if the firms above the threshold suffered a stronger contraction of demand or if they were more vulnerable to reductions in the availability of credit than the firms below the threshold. Moreover, selection may also occur on unobservables and in this case the introduction of firm controls need not suffice to avoid the bias. As already indicated, in the analysis of credit growth at the bank-firm level this problem is dealt with by introducing firm fixed effects. Moreover, we also estimate a firm-level specification in which the fixed effects are replaced by firm control variables. This difference-in-differences specification in growth rates includes exhaustive controls for differential trends in credit growth by firm characteristics and it includes a full set of fixed effects by industry-municipality pairs to control for local demand effects. The resulting estimate of the impact of weak-bank attachment on credit growth is virtually identical to the one obtained in the within-firm specification. This reveals that unobservables do not play a significant role as far as access to credit is concerned, but they could still generate different patterns of employment growth.

In our analysis of the impact of weak-bank attachment on firm-level employment we perform several tests to deal with the potential problem of selection on unobservables. First, we show, using the normalized difference test of Imbens and Wooldridge (2009), that selection on observables is not that important, because firms attached to weak banks only differ significantly from the rest in three of the 14 controls that we use: they have a lower capital-to-assets ratio, a higher debt ratio, and they work with more banks. In addition, we estimate a panel fixed-effects model with yearly observations for employment growth and we use matching techniques to directly compare firms within narrowly-defined cells. In neither case do we find any significant difference with our baseline. We nevertheless also apply the procedure in Oster (2017) to place an upper bound on the impact of unobservables on our coefficient of interest. Lastly, we exploit a legal change in the regulation of savings banks in December of 1988 to construct an instrument that generates exogenous variation in weak-bank attachment. The results of this instrumental variable model must be interpreted with caution, as explained below, but they confirm that weak-bank attachment exerted a significant negative effect on employment growth.

In the rest of the analysis we consider several extensions of our baseline setup to analyze the transmission mechanism of the credit shock and to explore the role of firms' financial vulnerability. We start by estimating a separate effect of weak-bank attachment on credit lines, finding that weak banks strongly reduced access to credit lines relative to healthy banks. A direct impact of working capital on employment therefore seems to prevail over potential indirect effects via reduced investment. Furthermore, we offer a novel decomposition of job losses through adjustments along the internal and external margins and we estimate the effect of weak-bank attachment on changes in the wage bill and in the share of employees with a temporary contract. To the best of our knowledge, we are the first to offer a comprehensive analysis of these margins of adjustment. Lastly, we interact the treatment variable with an extensive set of firm characteristics that capture different dimensions of firms' financial vulnerability.

Our baseline result is that weak-bank attachment caused employment losses of about 2.8 percentage points. This estimate is large, accounting for 24.4% of the total fall in employment among exposed firms in our sample. Surviving firms account for about one-half of the overall loss, while the remaining half corresponds to job losses in exiting firms. Nonetheless, weak-bank exposure explains a larger share of job losses in downsizing firms than in exiting firms (around one-half vs. one third, respectively), and we show that temporary employees bore the brunt of the employment adjustment. Lastly, we find that financially vulnerable firms suffered the largest job losses.

The rest of the paper is organized as follows. In Section 2 we review previous empirical work on the topic and in Section 3 we provide background information on the Spanish economy before and during the financial crisis. Section 4 describes our data and Section 5 presents our empirical strategy. In Section 6 we show our estimates of the effect of weak-bank attachment on credit growth and in Section 7 our baseline employment effect estimates. Selection effects are dealt with in Section 8 and Section 9 presents results on treatment heterogeneity. Various margins of adjustment are studied in Section 10. Section 11 contains our conclusions. Two appendices provide information on weak banks and securitization, as well as details on the variables used.

2 Literature review

In recent years there has been a surge of studies exploiting quasi-experimental techniques to estimate the real effects of credit supply shocks.[1] The closest ones are the seminal contribution of Chodorow-Reich (2014) and the article by Greenstone *et al.* (2014).[2] Both studies exploit cross-sectional differences in lender health at the onset of the recent crisis to study the link between credit supply shocks and employment. In Greenstone *et al.* (2014) this link is indirect, as they do not have access to loan-level data or information about firms' banking relationships. To circumvent this problem, 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. Using confidential data from the Bureau of Labor Statistics Longitudinal Database (LBD), they find that this measure predicts the reduction in county-level credit to small, standalone firms and their employment levels over 2008-2009. But even assuming that the entire reduction in lending is due to a drop in credit supply, the estimated effect is small, around 5% of the employment fall.

Chodorow-Reich (2014) does have 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

[1] Several studies exploit the variation induced by large external shocks to the banking system (Chava and Purnanandam, 2011, Benmelech *et al.*, 2012). Almeida *et al.* (2012), Benmelech *et al.* (2012), and Boeri *et al.* (2013) exploit differences in the debt maturity structure of firms. Garicano and Steinwender (2015) compare the response of different types of investment at foreign-owned and nationally-based manufacturing firms in Spain.

[2] Popov and Rocholl (2016) analyze the impact of German savings banks' exposure to the US subprime crisis on firms' labor demand. Fernandes and Ferreira (2017) explore the impact of financing constraints on the choice between permanent and fixed-term contracts. Berton *et al.* (2017) apply the Greenstone *et al.* (2014) approach to examine the impact of credit constraints in Italy on different types of workers. Other papers that study the employment effects of the financial crisis are Duygan-Bump *et al.* (2015), Hochfellner *et al.* (2015), and Siemer (2014). Some papers include employment among a broader set of real outcomes. Acharya *et al.* (2016) study the real effects of the sovereign debt crisis in Europe using syndicated loan data. Balduzzi *et al.* (2015) use a survey of Italian firms to analyze the real effects of fluctuations in banks' cost of funding during the 2007-2009 financial crisis and the ensuing sovereign debt crisis.

imposes on other firms during the crisis. These data are matched to employment records from the LBD data set for a sample of just over 2,000 firms. In line with Greenstone *et al.* (2014), he finds that SMEs with pre-crisis relationships with less healthy banks faced stronger credit constraints after the fall of Lehman Brothers and reduced their employment more compared to clients of healthier banks, attributing between one-third and one-half of job losses in SMEs to this factor. By contrast, there are no significant effects for the largest companies in the sample.

In this paper we also exploit differences in lender health to uncover the employment effects of credit supply shocks, but the access to credit register data represents a substantial improvement on the existing work in this field. First, we are able to reconstruct the entire banking history of firms and to trace back all credit flows and not only syndicated loans.[3] Second, the representative nature of our large sample of firms is important to gauge the overall effect of the credit shock on employment. Studies relying on data for relatively large firms may substantially underestimate the impact of credit shocks if larger firms are more able to find substitutes for bank credit than smaller firms. We do not find compelling evidence of such differences by firm size, but in other countries they may be important when large firms have access to well-developed markets for private debt. Third, access to detailed micro data allows us to perform a wider range of robustness checks and to explore the presence of heterogeneous effects along more dimensions than most existing studies.

Our analysis pays particular attention to the role of firms' financial vulnerability. Apart from standard indicators, such as firm size or age, our analysis also includes controls for firms' degree of bank dependence, the term structure of their bank debt, and their credit history. This analysis reveals that a bad credit history in the form of past defaults triples the negative effects associated with a pre-crisis relationship with a weak bank and these effects come on top of the almost 20 pp reduction in credit growth for all firms with a bad credit history. Similarly, for bank-dependent firms with

[3] Cingano *et al.* (2016) study the effects of banks' exposure to the interbank market on the investment decisions of their clients using data from the Italian credit register, also presenting results for employment growth. Jiménez *et al.* (2016) include an estimate of the impact of the size of banks' capital buffers on employment growth in Spain, but they focus on the impact of counter-cyclical capital buffers on credit supply over the business cycle. These two studies confirm the negative employment effects of impaired access to credit.

a ratio of bank debt to total debt above the median, job losses are five times bigger than the average treatment effect. These strong differences in the intensity of the effects confirm the finding in Paravisini *et al.* (2015) that it is key to compare firms within very narrowly-defined cells to avoid omitted variable bias.

The theoretical literature has identified several potential transmission mechanisms through which shocks to the banking system might affect employment in non-financial firms (see the review in Boeri *et al.*, 2013). First, mismatch between the timing of payments to workers and the generation of cash flow may force firms to finance salaries as part of their working capital. Second, turnover costs in the labor market transform labor into a quasi-fixed factor of production, creating a link between employment and external finance that is similar to the well-known link with investment. Third, financial frictions may alter the optimal mix of permanent and temporary jobs, as the latter are cheaper to destroy, and this may in turn have important implications for the cyclical volatility of employment. Lastly, the availability of external finance may indirectly alter the use of labor if capital and labor are complements in production.[4]

While we cannot identify the relevance of these mechanisms, we try to shed some light on them in several ways. On the one hand, we explore the relative importance of weak-bank attachment on short-term funding, which indirectly informs us about the purpose of the loans. Next, we consider three alternative margins of employment adjustment. First, we offer a decomposition of job losses along the internal and external margins, showing that weak-bank attachment leads to a significant increase in firms' exit probability. This finding helps to understand the persistence of the effects of credit shocks, since it is cheaper and quicker to create jobs at ongoing businesses than to rebuild firms once the economy recovers. And second, for the sample of surviving firms we estimate the effect of weak-bank attachment on the size of the wage bill and the share of temporary jobs. Popov and Rocholl (2016) and Fernandes and Ferreira (2017) offer comparable results on the importance of wage cuts and changes in the composition of employment, respectively, but we are the first to consider all three margins jointly.

[4]See Wasmer and Weil (2004) and Petrosky-Nadeau and Wasmer (2013) on frictions, and Caggese and Cuñat (2009) on temporary jobs.

3 The financial crisis in Spain

The Spanish economy experienced a severe credit crunch in the Great Recession. In this section we briefly document the magnitude of this credit crunch, but we start by defining the set of weak banks, so that we can compare the evolution of lending by weak and healthy banks.

3.1 The bank restructuring process

During our sample period, the Spanish Government intervened a total of 33 banks (see Table A1). First two small banks were nationalized and quickly resold. Later on the Government fostered either bank mergers (26 weak banks) or the takeover of ailing banks by other banks (5 banks). Most of these operations entailed State support, in the amount of 11.6 bn euro, i.e. about 1.1% of Spanish GDP (Banco de España, 2014).

We classify a bank as weak if it was nationalized, it participated in a merger with State funding support or it was insolvent and bought by another bank. Banks that absorbed other banks are considered to be healthy. During our sample period, all weak banks except the two small nationalized ones were run by their incumbent managers and all the institutions participating in mergers remained separate legal entities.

Further consolidation operations and the bulk of the nationalizations took place in 2011-2012 (see Appendix 1 and International Monetary Fund, 2012), and savings banks were forced to convert into commercial banks. These operations fall outside the scope of our analysis and the same is true for the recapitalization of the banking sector that took place in 2012 with financial assistance from the European Financial Stability Facility.

Finally, it is important to stress that savings banks were subject to the same regulation and supervision by the Bank of Spain as commercial banks, though they had a different ownership and governance structure. Not being listed in the stock market, they were less exposed to market discipline than commercial banks but also quite limited in their ability to raise capital in response to the crisis. Furthermore, they were *de facto* controled by regional governments, which led to delays in their restructuring and may have affected their credit allocation prior to the crisis.[5]

[5] See Cuñat and Garicano (2010), Fernández-Villaverde *et al.* (2013), and Santos (2014).

3.2 The differences in lender health

Table 1 illustrates the differences in lender health at the onset of the crisis. In the last two columns we report the t-statistic and the normalized difference test of Imbens and Wooldridge (2009), for which Imbens and Rubin (2015) suggest a heuristic threshold of 0.25.[6] Both tests yield the same results. In 2006, weak banks were on average larger than healthy banks and held less capital and liquid assets. By contrast, both their rate of return on assets and their share of non-performing loans were comparable, but this apparent similarity hides latent losses at weak banks, which surfaced later, as witnessed by their vastly larger ratio of non-performing loans in 2012.[7] The ratio of securitized loans to assets is also larger for weak banks, but not significantly so, suggesting that it was not key in explaining the differential evolution of credit during the crisis between the two sets of banks. Below we conjecture that the comparatively large share of loans to construction companies and real estate developers –henceforth real estate industry or REI– is a key source of the surge in loan non-performance and the comparatively strong contraction of lending by weak banks. Loans to the REI make up 68% of all loans of weak banks to non-financial firms compared to 37% for healthy banks.

The split between weak and healthy banks allows us to analyze the compound effect of these initial differences in lender health on credit supply, including latent losses not officially recognized until much later. The weak bank label should therefore be interpreted as a proxy for the relatively strong deterioration of the balance sheets and the lending capacity of the most vulnerable banks.

3.3 The credit collapse

Figure 1 depicts the real value of the annual flow of new credit to non-financial firms by month and bank type (average over the past 12 months). It reveals that the flow of new credit grew significantly more at weak than at healthy banks during the boom –60% vs. 12% from 2002 to 2007– and it also fell more in the slump –46% vs. 35% from 2007 to 2010. This evolution stems from both the intensive and extensive margins. The

[6] See a brief explanation of the use of this statistic in large samples in Section 4.2.

[7] In 2012 the authorities carried out stringent stress tests on banks, supervised by the ECB, the European Commission, and the International Monetary Fund.

latter is captured in Figure 2, which represents acceptance rates for loan applications by potential clients (non-client firms). Initially acceptance rates were higher for weak than for healthy banks, they then converged, subsequently both fell precipitously, and at the end of the period they were lower at weak banks. This evolution reflects the difficulties faced by Spanish firms trying to switch to a new lender during the crisis.

Figure 3 shows that the average interest rates charged by the two sets of banks closely follow the ECB policy rate. Until the end of our sample period the difference between them was always below 30 basis points. This suggests that interest rates were scarcely used by weak banks to ration credit demand during that period.[8] We can therefore safely focus on the differential evolution of credit volume at the two sets of banks during the crisis.

The same graphs also show that the consolidation operations and nationalizations during 2011-2012 did not restore the credit flow to weak banks. The gaps between bank types regarding new credit flows and acceptance rates continued to grow and weak banks also started to ration credit by charging substantially higher average interest rates than healthy banks.

4 Data

In this section we describe our data set, the sample selection procedure, and the construction of the treatment and control groups. For further details see Appendix 2.

4.1 Data sources

We construct a matched bank-firm data set with detailed information on all bank loans to non-financial firms. Even though our analysis focuses on the period 2006-2010, we collect data starting in 2000. The loan data is obtained from the Central Credit Register (CIR) of the Bank of Spain, which records all bank loans to non-financial firms above 6,000 euros (around 7,900 dollars at the end of 2006). Given the low threshold, these data can be taken as the census. The CIR provides the identity of the parties involved

[8] Stiglitz and Weiss (1981) explain why imperfect information leads to credit rationing rather than interest rate differences, and Petersen and Rajan (1994) show that US banking relationships operate more through quantities than through prices.

in a loan, the share of collateralized loans by firm, its maturity structure, the identity of its main bank –i.e. the one with the largest value of outstanding loans–, and indicators of its creditworthiness, such as the value of the firm's non-performing and potentially problematic loans. It does neither record the interest rate nor the purpose of the loan. We therefore have to rely on the distinction between credit lines and loans to indirectly establish a shortage of working capital or of funds for investment.[9]

Apart from the information on new and outstanding loans, we also have access to loan applications from non-clients.[10] By matching the records on loan applications with the CIR we infer whether the loan materialized. If not, either the bank denied it or else the firm obtained funding elsewhere (Jiménez *et al.*, 2012). Since the application data set only provides information on borrowing for firms with a credit history, we exclude firms with no prior loans.

We gather economic and financial information for more than 300,000 private, non-financial firms from the mandatory annual balance sheets and income statements that Spanish corporations submit to the Mercantile Registers. Our source is the Iberian Balance Sheet Analysis System (SABI) produced by INFORMA D&B in collaboration with Bureau Van Dijk and the Central Balance Sheet Data Office (CBSO) of the Bank of Spain. We match the data on loans, banks, and firms through firms' tax ID. Employment is measured as the annual average of employees, where temporary workers are weighted according to their weeks of work. SABI also provides information on variables like the firm's age, size, and indebtedness, though for most firms we only observe an abridged balance sheet with no liability breakdown. Lastly, we observe the firm's industry and use a two-digit breakdown into 78 industries.

To disentangle job losses in surviving firms from those due to firm closures, we use

[9] As indicated, there was significant merger and acquisitions (M&A) activity in the Spanish banking industry in the sample period. In most empirical specifications we only use loans with banks in 2006. In one specification however, in Section 6.1, we use firm-bank relationships as units of analysis throughout the period 2006-2010. To ensure that we follow credit relationships after a bank is acquired or merged into a new entity, we artificially reconstruct the banking relationships from 2010 backwards. So if, from 2006 to 2010, bank A acquires bank B and a firm had a loan from bank B, a single relationship will appear in 2006 with bank A, which will encompass both banks, while keeping track of the weak identity of either bank if that is the case. Still, bank merger activity was only relevant from 2010 onwards.

[10] Banks receive monthly information from the CIR on their borrowers' total indebtedness and defaults vis-à-vis all banks in Spain, but they can also get it on "any firm that seriously approaches the bank to obtain credit".

the Central Business Register (DIRCE), which allows us to ensure that firms that are in the sample in 2006 but which disappear from it in subsequent years have indeed closed down.[11] Lastly, we exploit two databases on banks. The first one, used for supervisory purposes, records their financial statements. It includes 239 banks, comprising commercial banks, savings banks, and credit cooperatives. The second one contains historical data on the location of bank branches at the municipal level, which is used for the first time for research purposes.

4.2 The treatment and control groups

To analyze the employment effects of the credit reductions by weak banks, we divide the sample of firms into two groups depending on the strength of their pre-crisis relationship to weak banks. We measure weak-bank attachment through the ratio of the total amount of loans from weak banks to the firm's asset value. It is the product of the firm's ratio of debt with weak banks to total debt –i.e. the weight of weak banks in debt– and the ratio of total debt to asset value –leverage– and it is measured in 2006. Our baseline treatment measure is a dummy variable denoted WB_i, which takes the value 1 if the weak-bank loan-to-asset ratio for firm i is above the first quartile of the distribution of firms with non-zero exposure.

The chosen threshold for our weak-bank dummy has the advantage of excluding firms with marginal attachment to weak banks from the treatment group, while still providing a conservative estimate of the impact of such attachment, since the threshold is set at the lower end of the distribution.[12] We will nevertheless show the robustness of our findings to the use of the continuous weak-bank loan-to-asset ratio measure and to different cut-off levels for the weak-bank dummy.

Given the size of our data set, we adopt stringent sample selection rules. To avoid the problem of reverse causality –so that firms' troubles drive banks' problems rather than the opposite– we exclude firms in the REI or in two-digit industries selling at

[11] We do not observe M&A. However, the CBSO sample of firms above 50 workers contains such information and in 2012 only 3% of all firm closures according to the DIRCE resulted from M&A. Since M&A usually take place among large firms and in our sample only 5% of firms are above that threshold, we expect to have a much lower fraction. A firm may be closed down with one ID and then opened with another one, but this type of transaction cannot be identified.

[12] The firm at the median exposure ratio in the overall distribution, which could be an alternative threshold for the treatment, has no loans from weak banks.

least 20% of their value added to the REI in 2000 (see Appendix 2). This date is chosen to minimize potential endogeneity through credit decisions taken in the boom.[13] Throughout the analysis we work with a balanced sample and we only include firms in our sample for which we have reliable observations on all variables from 2006 to 2010. In particular, we exclude firms that do not deposit their accounts after 2006 but still appear in the Central Business Register. Hence, only firms that are missing in both registers are classified as having closed down. Moreover, since we are interested in bank credit, we exclude firms with no loans in 2006. This leaves us with a final sample of 149,458 firms.

We choose 2006 as the base year because both GDP and real credit were growing very quickly, at 4.1% and 19% p.a., respectively, so that neither the recession nor the credit crunch were generally anticipated then. However, in one specification we set 2007 as the base year to check the robustness of our results to this dating.

In 2006 the firms in our sample represented 19% of firms, 28% of value added, and 42% of private sector employees in the industries included in our analysis. Most firms are very small, indeed, 98.7% of them are SMEs according to the European Commission definition (i.e. having less than 250 employees and either turnover below 50 million euros or a balance sheet total below 43 million euros). On average these firms reduced employment by 8.1% during the sample period. This figure is very close to the aggregate reduction in employment for the industries we cover.

Table 2 presents descriptive statistics for our treatment and control groups. About 69% of firms have either no credit from weak banks or a weak-bank loan ratio to assets below the first quartile –which is equal to 4.8%. For firms above this threshold, the average share of credit from weak banks is equal to 68.5% and their ratio of weak-bank credit to assets to 22.8%.

Compared with the control group, firms in the treatment group seem to be on average younger and smaller, they have more temporary workers, and they are as likely to be exporters. The data also reflect the worse financial profile of firms in the treatment group: they are less profitable, hold less capital and liquid assets, and they are more indebted to banks, although the average maturity of their loans is higher. In addition,

[13] The housing bubble is thought to have started around mid-2003 (Ayuso and Restoy, 2006).

they work with more banks and over 2002-2006 they defaulted more often on their loans. They also applied for loans more often and had a higher acceptance rate.

These differences are significant according to the t-ratio, but in very large samples this statistic has the problem that it increases with the number of observations. Imbens and Wooldridge (2009) propose a scale-free alternative measure, the difference in averages scaled by the square root of the sum of the variances. Indeed, as shown in the last two columns of Table 2, the results from these two tests diverge in our case. Using the normalized difference test and the heuristic threshold of 0.25 suggested by Imbens and Rubin (2015), the only significant difference is that firms exposed to weak banks have a lower degree of capitalization and a higher level of indebtedness, and they work with more banks than non-exposed firms. Their lower liquidity and higher number of past loan applications are close to being significant. This implies, first of all, that selection on observables is not a very important problem, once we take into account the large sample used. Second, this also means that we should control for those three firm-level characteristics –or at most five– in our empirical analysis. Nevertheless, we will use all 17 variables listed in the table below the employment level as controls in our firm-level credit and employment estimation to be sure that we are conditionally comparing very similar firms.

At this stage we need to deal with the potential objection that our treatment is defined in terms of an outcome, a bank‘s bailout, that is realized after the crisis broke out. Using an ex-post criterion does not invalidate our results, however, as long as the outcome was unexpected. To study whether firms could have anticipated in 2006 the future solvency problems of weak banks, we analyze the risk premia charged to Spanish banks' securitization issues prior to the recession. We use data on tranches of mortgage backed securities and asset backed securities in 2006, grouping the ratings into prime (AAA), investment grade (AA+ to BBB-), and speculative (BB+ to D). We have 303 observations (deal-tranches) from Dealogic, with floating rate, quarterly coupon frequency, and referenced to the 3-month Euribor, from 24 issuer parents.

Without any controls, weak banks actually paid 7 basis points less than healthy banks. To control for issue characteristics, we regress coupon differentials in basis points on a set of variables capturing the type of securitization, risk category, month of issue, years to maturity, collateral type, and guarantor type. Standard errors are clustered by issuer parent. The estimated coefficient associated with the weak-bank dummy is positive but non-significant: 2.8 basis points, with a p-value of 0.55 (see Table A2). Hence we cannot reject the hypothesis that financial markets failed to recognize the buildup of differential risk at weak banks in 2006.[14] It seems safe to assume that private firms, with a lower capacity to process available information than financial markets, could not possibly have predicted it either.

5 Empirical strategy

Our identification strategy proceeds in two steps. The first step consists of establishing the presence of a credit supply shock associated with the troubles of weak banks. In this part we first estimate a credit equation at the bank-firm level and then at the firm level. The second part consists of estimating the effects of weak-bank attachment on employment.

5.1 Identification of the credit supply shock

We start by estimating the following credit growth equation for bank-firm pairs:[15]

$$\Delta_\tau \log(1 + Credit_{ib}) = \theta_i + \pi WB_b + Z'_{ib}\kappa + S'_b\lambda + \epsilon_{ib} \qquad (1)$$

where Δ_τ is a τ-year difference with respect to the year 2006, $Credit_{ib}$ is total credit committed by bank b to firm i –both drawn and undrawn, so as to minimize potential endogeneity–, θ_i is a firm fixed effect, WB_b is a dummy variable that takes the value 1 if bank b is weak, Z_{ib} is a vector of bank-firm controls that includes the length of their relationship and a dummy variable for past defaults, S_b is a vector of bank controls, and ϵ_{ib} is a random shock. Our coefficient of interest is π.

Specifications like (1) are the standard procedure to identify credit supply shocks. The fixed effects absorb any differences in observable and unobservable firm characteristics. As a result, they perfectly control for potentially confounding demand effects,

[14] Financial markets operators may have been aware of the concentration of risks in savings banks, but they may have also anticipated an implicit bailout guarantee. Either way, the risk perceived by funders is not statistically different.

[15] Khwaja and Mian (2008) label this as the local analysis and the subsequent firm-level equation the aggregate analysis.

allowing us to test whether the same firm experiences a larger reduction in lending from weak banks than from healthy banks once we control for differences in Z_{ib} and S_b. This equation can however be estimated only for firms that work with more than one bank. Since many firms in our sample work with a single bank, we also estimate a between-firm variant of equation (1) in which the firm fixed effects are replaced by a vector X_i of firm controls. As originally explained by Khwaja and Mian (2008), this ordinary least squares (OLS) specification may yield biased estimates of π in the presence of both credit demand and supply shocks, but we show that this risk can be minimized by introducing a rich set of firm controls and industry times municipality dummies to control for demand effects.

Next, even if we confirm the presence of a credit shock at the bank-firm level, we still need to check whether the affected firms managed to offset the reduction in credit supply by weak banks with additional loans from other banks. For this purpose we estimate the following firm-level equation:

$$\Delta_\tau \log\left(1 + Credit_{ij}\right) = \rho + \mu WB_i + X_i'\eta + \delta_j + v_{ij} \tag{2}$$

which links credit growth to our treatment variable WB_i, where X_i contains the 17 variables listed in Table 2, and δ_j is a vector of industry (78) times municipality (2,749) dummies that control for local credit demand conditions. Here our coefficient of interest is μ, which will typically be smaller than π to the extent that firms managed to obtain credit from healthy banks when weak banks curtailed their supply.

5.2 The employment impact of credit constraints

We then proceed to estimate the impact of the credit supply shock on employment. To ensure that our estimates do not capture the effect of differences in observable characteristics of firms rather than the effect of credit supply, we adopt a difference-in-differences (DD) specification in growth rates that has the same structure as equation (2) (Wooldridge, 2010):

$$\Delta_\tau \log\left(1 + n_{ij}\right) = \alpha + \beta WB_i + X_i'\gamma + \delta_j + u_{ij} \tag{3}$$

where n_{ij} is employment in firm i in industry-municipality cell j and u_{ij} is a random shock. All regressors are again measured in 2006. This estimate is an average treatment effect on the treated (ATT). To measure the employment adjustment in both surviving and closing firms, we set n_{ij} to zero for firms that are present in 2006 but have closed down τ years later.

Estimating in differences implies that we are including an aggregate trend and differential trends by industry-municipality cells and by firm characteristics, which is substantially more demanding than the standard DD specification in levels. Unbiased estimation of the causal impact of weak-bank attachment on employment however relies on the unconfoundedness assumption, which requires the assignment of firms to the treatment and control groups to be completely random conditional on the controls for observables. Moreover, unlike for credit, this specification cannot perfectly control for confounding factors through the introduction of fixed effects.

The main threat to identification is the non-random assignment of firms to banks before the crisis. Aggregate shocks may differentially affect firms depending on their profitability, product quality or financial vulnerability. If, for example, the crisis hit more financially vulnerable firms harder and these firms were over-represented among the clients of weak banks, then the DD specification would tend to overestimate the real effects of the credit shock. Our firm controls, X_i, are meant to absorb potential differences in both firms' performance and their financial vulnerability and creditworthiness, but selection may take place on both observables and unobservables. This may not be an overriding problem, since only three of the 17 firm characteristics that we use are statistically different, but we still devote Section 8 to deal with this issue.

The other main concern is that demand effects may bias our estimation (Mian and Sufi, 2014). Before the crisis, lending grew especially in the real state industry and it was more concentrated in certain areas, where in the recession we might observe both a larger drop in demand by households and a higher density of (non-REI) firms exposed to weak banks. In these circumstances employment reductions would stem from lower consumption demand rather than from less credit. The fact that small firms tend to be financed by local banks (Petersen and Rajan, 2002; Guiso *et al.*, 2013) would additionally contribute to the presence of local demand effects. For this reason we allow for differential trends in the δ_j cells defined by the product of 2-digit industry and municipality dummy variables.

6 The differential evolution of credit

The first step is to validate our claim that the differential evolution of the volume of lending by the two sets of banks reflects a credit supply shock, which we carry out through both bank-firm and firm-level analyses.

6.1 Bank-firm analysis

Table 3 reports the results for our baseline equation (1) and alternative specifications for the change in credit between 2006 and 2010. Robust standard errors are corrected for multiclustering at the firm and bank level. The specification with firm controls and industry-municipality dummies yields an estimated differential reduction in credit of 23.2 percentage points (pp) for weak banks vis-à-vis healthy banks (col. 1). Restricting the sample to firms with multiple banking relationships reduces the estimate to -25.6 pp (col. 2), which is virtually identical to the estimate for our baseline specification with firm fixed effects, -25.5 pp, estimated with the same sample of multi-bank firms (col. 3). The similarity between these two estimates suggests that unobservables do not play a significant role in access to credit, despite the differences in three of the observable firm characteristics. Moreover, a Hausman test fails to reject the null hypothesis of orthogonality between the firm fixed effects and WB_b with a p-value of 0.372. These results confirm that our weak bank dummy variable captures changes in credit supply rather than in credit demand.

Was credit rationing by weak banks stronger on short-term funding? To answer this question we interact WB_b with an indicator for firms that enjoyed a credit line in 2006. The results in Table 3 indicate that weak banks reduced credit to firms with credit lines by 10.6 pp more than healthy banks (col. 4). A natural interpretation is that credit lines were the easiest loans to cut for banks in distress and, possibly, that the credit constraint affected more working capital than investment. In Appendix Table A3, we present the results when we also include dummies for maturities of 1-3 years, 3-5 years, above 5 years, and for unknown maturity (affecting 4% of the loans), with loans below one year being the reference category, and their interactions with the weak-bank dummy variable. The estimates do not indicate a differential effect of the credit supply shock for different loan maturities, as none of the coefficients of the interactions is statistically significant.

Next, we show the estimate for bank-firm relationships which were still alive in 2010 (col. 5). In this case, the difference in the reduction of credit supply is equal to 7.9 pp, indicating that adjustments at the internal margin –i.e. reductions in loan volume to existing lenders– account for a small share of the reduction in lending by weak banks, so that the external margin dominates –i.e. committed credit is reduced to zero or renewal of expired loans is denied.

The differential effect captured by the weak-bank dummy indicates that standard measures of bank health do not fully capture the deterioration of weak banks' assets. A natural explanation for this finding relies on differences in banks' pre-crisis exposure to the REI. We measure it by the 2006 share of each bank's loans to firms in the REI and create a dummy variable that takes the value 1 for banks in the upper quartile of the cross-sectional distribution. The associated coefficient is 7.5 pp smaller and less significant than in our baseline (col. 6). Hence, latent losses elsewhere in the balance sheets of weak banks may have been imperfectly correlated with their exposure to the REI.

Lastly, our identification procedure relies on the absence of different pre-crisis trends in access to credit for firms in the treatment and control groups, which would lead to biased estimates. To check this issue, panel A of Figure 4 shows the yearly coefficients, from 2004 to 2010, for our baseline specification with firm fixed effects. The coefficient of WB_b is not significantly different from zero between 2004 and 2007; indeed, except for 2004 the point estimates are equal to zero. The treatment effect becomes significant in 2008 and it grows over time from -10 pp in 2008 to over -25 pp in 2010. This shows that weak-bank exposure has no significant impact on access to credit prior to 2007 once we control for firm-fixed effects.[16] In sum, we have shown that weak banks reduced credit more than healthy banks, not just in the aggregate but also at the level of individual bank-firm relationships.

[16] In the case of single-bank firms we cannot include firm fixed effects, but introducing firm controls leads to similar results.

6.2 Firm-level analysis

We now study credit rationing at the firm level. The dependent variable is the log difference between the firm's total credit outstanding in 2006 and 2010, and the weak-bank indicator of the bank-firm analysis is replaced by our treatment dummy WB_i.

The estimated drop in credit supply at the firm level of 5.3 pp in col. (1) of Table 4 indicates that treated firms managed to offset around two-thirds of the fall in credit supply by weak banks. The corresponding estimate for multi-bank firms is -3.1 pp (cols. 2). Using the weak bank measure based on REI exposure yields a smaller effect on credit, although it is not statistically different from the baseline estimate (col. 3).[17] Furthermore, while multi-bank firms suffered a stronger credit supply contraction at the local level than the average client firm of weak banks, the reverse is true at the firm level. A pre-crisis banking relationship with more than one bank thus provided some insurance against the shocks that subsequently hit weak banks. The average change in credit is equal to -23.1% for unattached firms and to -31.3% for attached firms. Out of this 8.2 pp difference, 2.8 pp are due to the attachment to weak banks, which therefore explains 34% of the fall in credit for attached firms.

The large difference between the bank-firm and firm-level estimates may seem surprising, but weak banks may have predominantly severed their relationship with those firms with a marginal attachment to weak banks that are included in the control group. This would be consistent with our estimates for the firm-level effects being close to the estimated effects at the bank-firm level for continuing relationships (Table 3, col. 5). It is also worth noting that our results are at variance with those in Jiménez *et al.* (2014), who –using the same CIR data– find a positive credit shock for banks that securitized mortgages in the years of the Spanish boom (2004Q4 to 2007Q4), but then find no transmission of this positive shock at the firm level. At that time the credit market was booming and acceptance rates for loan applications were high for all banks, as shown in Section 3.3. On the contrary, the steep fall in acceptance rates during our sample

[17] If there was no offsetting, firms attached to weak banks should suffer a reduction that is proportional to their share of loans from weak banks in total loans, which is 68.5% (Table 2). Since non-attached firms are also exposed, with a share of 8.5%, the differential degree of exposure of attached firms is 60%. Thus, in the no-offsetting case their differential credit reduction would be 15.3 pp (25.5 pp times 60%).

period made it much harder for firms to offset credit rationing by weak banks through new loans from healthy banks.

Our results are qualitatively similar to those in Cingano *et al.* (2016), who use data on Italian firms to estimate the real effects of the bank lending channel exploiting the 2007 liquidity drought in interbank markets as a source of variation in banks' credit supply. The comparison is not straightforward, since they define treatment based on the ratio between a bank's interbank market loans and its asset value. Contrary to Jiménez *et al.* (2014) and to our case, they find similar coefficient estimates at the bank-firm and firm levels. Their estimate implies that a 1 pp increase in the interbank-to-asset ratio leads to a reduction in credit growth of 2.4 pp for a firm with a degree of exposure that is 1 standard deviation above the mean.

Gobbi and Sette (2014) also study credit growth at the firm level for Italian firms during the Great Recession, finding that the number of banking relationships has a negative impact on credit growth, with the effect being strongest when firms move from one to two banks. In contrast, in the bank-firm analysis we find that the negative effect of weak-bank attachment is larger for multi-bank firms than for single-bank firms, whereas in the firm-level analysis the opposite holds. The latter is the most relevant evidence regarding the effect of the credit crunch, and when estimated for all firms (Table 4, col. 1) the coefficient on a control variable for the number of banking relationships is equal to 0.022 (s.e. 0.006), which means that single-bank firms fared worse in terms of getting credit, and more so if attached to weak banks.[18]

7 Main results

This section presents the empirical results for our baseline specification for the employment effects of credit constraints and for a set of robustness checks.

7.1 Difference in differences

Table 5 presents the estimation results for our baseline DD equation (3). We report robust standard errors corrected for multiclustering at industry, municipality, and main bank level. In order to illustrate the sensitivity of our estimates to changes in the set

[18] The full estimation details are available upon request.

of control variables, we subsequently add more controls until we arrive at our baseline specification.

If we only include industry and municipality fixed effects, we obtain that employment in firms attached to weak banks falls by 7.4 pp relative to employment in unattached firms, while allowing for differential trends at the industry times municipality level leads to a treatment effect of -7.6 pp (cols. 1 and 2). Next, including the only three firm-level controls that are significant according to the normalized difference test –namely capitalization, bank debt, and the number of banks– reduces the treatment effect to 3.5 pp, while adding all remaining firm controls brings down the effect marginally to -2.8 pp (cols. 3 and 4), with half of the difference being due to the inclusion of the next two variables with the largest normalized difference, i.e. liquidity and number of past loan applications (not shown). Thus, while the estimate in col. (3) is significantly different from the one obtained without any controls (col. 2), further including the remaining 14 control variables does not yield a statistically different estimate. Finally, including main bank fixed effects does not alter the results (col. 5), which is further proof that our weak bank indicator captures the relevant dimensions that explain the reduced access to credit for treated firms. We therefore adopt the specification in col. (4) as our baseline.

Our identification relies on the assumption of parallel pre-crisis trends for treated and control firms. The validity of this assumption is tested by running a placebo regression with 2002 as the pre-crisis year and 2006 as the post-crisis year. As required, this specification test delivers a coefficient that is not significantly different from zero (col. 6).[19] Further evidence is provided in Figure 5. It depicts the estimated coefficients of WB_i and the confidence intervals for the period 2002-2010 and it shows that the treatment effect is significantly negative from 2008 onwards. Before that time, weak-bank attachment does not produce significant differences in employment in firms in the treatment and control groups. Hence, the timing of the real effects coincides with the timing of the credit constraints at the local level (Figure 4a). Credit rationing at the firm level also follows the same pattern (Figure 4b), but these effects are less precisely estimated, which helps to explain why the treatment effect at the firm level does not

[19] We also estimated the same model for every year from 2002 to 2005 getting the same result.

become significant until 2009.

The coincidence between the timing of the changes in credit supply and employment is reassuring, but not sufficient to establish a causal relationship. We need to demonstrate that the reductions in credit supply drive the differential evolution of employment at the firm level. Moreover, it would be incorrect to limit the analysis to a year-to-year comparison between the extent of credit rationing and the size of the employment adjustment. When the crisis erupted –and in particular after the fall of Lehman Brothers in September 2008– expectations about access to credit changed dramatically and firms in the treatment group may have rationally anticipated a further tightening of credit conditions in later years. To some extent, real effects may therefore be observed before actual credit rationing shows up in the data.

Indeed, in a survey of banks undertaken by the ECB, the net balance of banks expecting an increase in the supply of credit to non-financial firms and banks expecting a decrease went from roughly zero in 2007Q2 to -40% already in 2007Q4, remaining there for the subsequent four quarters (Martínez-Pagés, 2009). A similar survey of firms was launched by the Bank of Spain in March 2009 (Banco de España, 2009). When asked about their ability to obtain funding from banks over the preceding six months, 40% of firms up to 50 employees –which are the majority in our sample–, reported that funding was obtained only in part or from credit institutions other than their usual ones, and 30% reported that the could not obtain any bank credit. In these two groups of firms, 65% reported that the main reason for not obtaining the funding was a change in attitude of credit institutions.

Taking due account of the above observations, we now proceed with a formal test of the direct link between access to credit and changes in employment at the firm level using an IV setup that spans our entire sample period 2006-2010.

7.2 The credit channel

We now decompose the estimated effect of the credit supply shock into two parts: the impact of weak bank attachment on the amount of credit obtained by firms, i.e. a credit volume effect, and the pass-through of this measure of the credit shock to employment, using the following IV model:

$$\begin{aligned}\Delta_\tau \log(1+n_{ij}) &= \sigma + \phi\Delta_\tau \log(1+Credit_{ij}) + X_i'\xi + \delta_j + \varepsilon_{ij} \\ \Delta_\tau \log(1+Credit_{ij}) &= \rho + \mu WB_i + X_i'\eta + \delta_j + v_{ij}\end{aligned} \tag{4}$$

in which WB_i acts as an instrument for access to credit and the first stage coincides with equation (2). Therefore μ captures the differential impact of weak bank attachment on committed credit, while ϕ captures the pass-through from credit to employment. Thus, the product $\mu\phi$ is equivalent to parameter β in equation (3).

The exclusion restriction is that working with a weak bank alters employment growth only through credit. Although the difference between the average interest rate charged by weak and healthy banks is quite small, it is non-zero and higher for weak banks. The presence of an interest rate response, albeit small, would contradict the exclusion restriction and as a result our second stage coefficient should be interpreted as an upper-limit.

Table 6 reports the estimates. The first stage coincides with our equation for credit rationing at the firm level. For the entire sample of firms it delivers a differential drop in credit due to weak-bank attachment of 5.3 pp, while the elasticity of employment with respect to credit is estimated at 0.519 (col. 1), i.e. that the elasticity of employment to the volume of credit is about one-half. This yields a compound effect on employment of -2.8 pp, which coincides with the baseline of the previous section. Next, for multi-bank firms we obtain a smaller impact of WB_i on credit growth, -3.1 pp, but interestingly the pass-through is estimated to be larger than in the full sample, 0.797, yielding an overall impact of -2.5 pp (col. 2). The estimates are highly significant and the F-statistics confirm the absence of a weak instrument problem.

In comparison, Cingano *et al.* (2016), who explore the effects of the 2007 liquidity drought in interbank markets on Italian firms, find that a 10 pp reduction in credit growth reduces employment by 1.4 pp, whereas the equivalent figure in our case would be 5.2 pp, indicating a much higher elasticity than theirs.

7.3 Alternative specifications

In this section we perform a wide range of specification tests to check the robustness of our results. We start by considering their sensitivity to alternative definitions of the treatment variable.

So far we have used a discrete treatment measure and the threshold for assignment to the treatment group was set at the first quartile of the distribution of the weak-bank loan-to-asset ratio. In our first exercise, we replace the treatment dummy by the ratio itself, which allows the intensity of credit constraints to depend on the normalized size of firms' debt with weak banks in 2006. The corresponding coefficient, reported in the first column of Table 7, is -9.2 pp. Evaluated at the average ratio (22.8%), this delivers an overall effect of -2.1 pp. Then we report the estimates when the threshold for our discrete treatment measure is set, respectively, at the median and the third quartile of the distribution. As we raise the threshold, the estimated treatment effect becomes stronger, going from -3.0 pp to -3.3 pp (cols. 2 and 3). Neither of these estimates is statistically different from our baseline, but this exercise reveals that the magnitude of the impact increases with the degree of exposure, indicating that our choice of the first quartile as the threshold is quite conservative.

The aim of the next exercise is to separate employment adjustments along the intensive and the extensive margins. Restricting the sample to surviving firms, the estimated treatment effect drops to -1.4 pp, which is exactly half the size of our baseline estimate (col. 4). A further robustness check is to use a different definition of the dependent variable that also allows us to account for both exit and entry, originally proposed by Davis *et al.* (1996) to study establishment-level data, namely $(n_{ijt} - n_{ijt-1})/(0.5(n_{ijt} + n_{ijt-1}))$. The associated coefficient is -3.4 pp, which is larger but not statistically different from our baseline estimate (col. 5).

We now consider alternative procedures to control for local demand effects. Mian and Sufi (2014) argue that local demand effects should only affect output in non-traded goods sectors, whereas credit supply shocks should affect traded good sectors as well. We therefore aim at filtering out local demand effects by restricting attention to traded sectors. Mian and Sufi (2014) use two classifications, based on either ad-hoc tradability criteria or geographical concentration. We choose the latter, because more concentrated industries are likely to be more traded and hence less dependent on local demand conditions.[20] We follow these authors in computing the Herfindahl concentration index for 3-digit industries and 50 provinces, and label as tradable the goods in the highest quartile. This sample selection yields an effect on employment of -5.8 pp (col. 8). It is statistically different from our baseline estimate, presumably because these firms sell in a wider geographical area and may therefore rely more on bank credit or be more sensitive to changes in credit supply, or be more sensitive to the cycle, or a combination of these factors. For our purposes what matters is that these estimates are not the result of local demand shocks.

We next check the impact of the alternative definition of weak bank, already used in Section 6.1, where weak banks are defined as those in the upper quartile of the distribution of exposure to the REI. For this definition the measured impact is equal to -3.0 pp, which is again very similar to our baseline (col. 7). In the two final checks we alter the reference period. First, we redefine the pre-crisis year to 2007. This choice is motivated by the fact that aggregate employment in Spain kept growing until the third quarter of 2007. Surprisingly, the estimated weak-bank effect drops to -1.9 pp (col. 8). This result suggests that the slowdown in 2007 altered the mix of employment in financially vulnerable and resilient firms in the treatment and control groups, though once again this estimate is not differ statistically from the baseline. Lastly, we measure weak-bank attachment and all other variables in 2002 (col. 9). Hence, firms in this sample are at least five years old at the start of the crisis. The table shows that the treatment effect survives, though at -1.5 pp it is significantly smaller than for the 2006 sample, suggesting that older firms were less affected by the credit supply shock, which will be confirmed in Section 9.

8 Selection

Our baseline specification includes an exhaustive set of controls for observable firm characteristics, many of which moreover do not significantly differ across exposed and non-exposed firms. However, this does not completely rule out the possibility of selection effects. Our list of firm controls may still be incomplete and our estimation

[20] As found by Mian and Sufi (2014) for the US and by Ramos and Moral-Benito (2015) for Spain.

strategy does preclude selection on unobservables.

Selection on unobservables would not be a concern if the same unobservables that are relevant for credit demand fully captured the unobservable demand effects relevant for employment growth during the crisis. There are however no strong reasons to believe that this should hold. For example, firms facing a low product demand may demand less credit to the extent that they need to produce less; however they may also have higher demand for credit since they have lower cash flow and may need more resources to pay back outstanding liabilities. Given the importance of the selection effects we devote Section 8 to this issue.

We can informally check the sensitivity of our estimates to the inclusion of the observable controls, so as to derive bounds for the possible bias arising from unobservable variables, as in Oster (2017). If the value of the regression R^2 increases when the controls are included but the coefficient of interest does not vary much, then it is expected that the inclusion of unobservables would not alter it either. We compute a bias-adjusted estimated coefficient, following Oster (2017) in making the heuristic assumption that the maximum R^2 would be 30% higher than the R^2 that would be obtained if all potential determinants were included. The estimate of the effect of WB_i is equal to -1.1 pp, which places a lower bound on the effect of interest. More formally, we now perform three further tests to corroborate our claim that the differential evolution of firm-level employment is not driven by selection.

8.1 Panel estimates

Our DD model is based on a cross-section and cannot therefore include firm fixed effects. To rule out the differential evolution of employment being driven by unobservable characteristics, we estimate the following panel fixed effects model (Wooldridge, 2010):

$$\Delta \log(1 + n_{ijt}) = \alpha_i' + WB_i' d_t \beta' + X_i' d_t \gamma' + d_t \delta_j + d_t \psi + v_{ijt} \tag{5}$$

where α_i' is a set of firm fixed effects, d_t a vector of time dummies for t=2007,...,2010, and v_{ijt} a random shock. The rest of the variables are defined as before. This model includes industry times municipality times year fixed effects and both the treatment dummy and the vector of time-invariant firm characteristics are interacted with year dummies. The equivalent of β in equation (3) is the element of the coefficient vector β' corresponding to 2010 –whose value is relative to 2007.

As reported in Table 8, in this panel fixed effects specification the treatment effect amounts to -2.7, which is indistinguishable from the baseline. Interestingly, the treatment effect is statistically significant in 2008 and monotonically increasing in absolute value over time. These estimates indicate that unobservables do not play a significant role in the transmission of the credit supply shock once we filter out any trends at the industry-municipality level and across firms with different observable characteristics.

8.2 Matching estimates

We have already pointed out the presence of some degree of heterogeneity between treated and control firms. Matching techniques allow us to directly compare similar firms in both groups. This avoids problems derived from a possible lack of overlap between the characteristics of firms in the two groups and it improves the efficiency of our estimates. For the sake of completeness, we use both propensity score and exact matching for our discrete and continuous treatment variables, thus obtaining four different estimates.

The propensity score matching estimates are derived from first estimating a probit model for the probability that a firm borrows from a weak bank –which includes the same controls as the baseline regression– and then estimating our baseline model using the weights coming from the sample balanced on all the observables used for the propensity score. In exact matching we compare treated and non-treated firms within industry times municipality and firm control cells. For the latter we use the coarsened exact matching method (Iacus *et al.*, 2011) where all characteristics are entered as 0-1 dummy variables (see Appendix 2 for details). We end up with 6,556 strata with observations that can be matched across treated and control firms, out of a total of 13,520 strata, so that only 2,122 firms (5.1%) in the treatment group are left without a matching control firm, and the treatment effect is estimated using the method of weighted least squares.

Table 9 reports the results. For the discrete treatment measure WB_i, the estimated effects with propensity score and exact matching are, respectively, -3.2 pp and -1.6 pp (cols. 1 and 2).[21] With the continuous measure, the corresponding coefficients are -6.5 pp and -5.2 pp. Since average exposure among attached firms is 22.8%, the average treatment effects are, respectively, -1.5 pp and -1.2 pp (cols. 3 and 4). Thus, in both cases propensity score matching delivers larger effects and, in line with our previous results, the continuous measure implies somewhat lower effects than the discrete treatment dummy. Importantly, all four estimates are significantly different from zero and they lie within the confidence intervals of their respective baseline estimates.

8.3 Exogenous variation in weak-bank attachment

Ultimately, weak bank attachment is an endogenous choice. Private firms may not have been able to predict the solvency problems of weak banks during the crisis, but the most vulnerable firms may have ended up working with weak banks due to relatively lax credit standards or poor risk management at weak banks. Thus, to further try to rule out selection effects we need an exogenous source of variation in weak-bank attachment.

For this purpose we exploit a change in banking regulation. Until 1988 savings banks could not open more than 12 branches outside their region of origin, but at the end of December 1988 all location restrictions were lifted (Real Decreto-ley 1582/1988). We calculate for each municipality the share of bank branches at the start of December 1988 –i.e. right before the adoption of the reform– that belonged to any of the weak banks and we use this indicator for local weak-bank density as an instrument to explain the weak-bank attachment of firms in 2006. In other words, we are assuming that firms are more likely to work with weak banks when these banks have traditionally held a strong market position in the municipality of the firm as reflected by the local weak-bank density in 1988.

The exclusion restriction is that local weak-bank density only affects a firm's employment through its attachment to weak banks, which cannot be tested. Having neighboring firms that are more likely to have borrowed from a weak bank may affect a given

[21] Post-matching balance tables, for both exact and propensity score matching, show no trace of significant differences in the control variables across treated and control firms according to the normalized difference test of Imbens and Wooldridge (2009). The mean distance for each of the matching variables is smaller than $3.2e^{-14}$ and the $\mathcal{L}_1$ statistic, computed by the cem command in Stata and introduced in Iacus *et al.* (2012), which is a comprehensive measure of global imbalance based on the L_1 difference between the multidimensional histogram of all pre-treatment covariates in the treated group and the control group, has a value of zero in both cases. These results are available upon request.

firm, which would violate the exclusion restriction. Using geography-based variation therefore means that the estimates may include local general equilibrium effects from having a weak bank. We can only imperfectly control for local demand effects because our IV varies by municipality and so we cannot include trends at that level. However, we do include a dummy for Spain's coastal provinces among the regressors, to capture the comparatively strong growth in credit and housing prices in these provinces prior to the crisis.[22]

To be a valid instrument, local weak-bank density in 1988 would have to be as good as randomly assigned vis-à-vis the set of firms that existed in that area in 2006. In terms of our control variables, firms in municipalities with local weak-bank density above the median (0.25) are very similar to firms below the median in all respects. As can be seen in Table 10, none of the firm characteristics, other than the share of loans with weak banks, is significantly different across the two groups of firms according to the normalized difference test of Imbens and Wooldridge (2009).

It should be stressed that the overwhelming majority of weak bank branches in December 1988 belonged to savings banks that were founded in that particular region. In other words, our IV analysis uses weak bank attachment in the home region of the savings banks. This point is relevant because the available evidence suggests that the expansion of the savings banks beyond their region of origin was accompanied by a deterioration of their client pool (Illueca *et al.*, 2013).

The estimates in Table 11 show that high weak-bank density in 1988 significantly predicts weak-bank attachment 18 years later and that the associated employment effect amounts to -7.6 pp with the WB dummy variable and -7.3 pp with the continuous measure. Both estimates are larger than the foregoing ones, but they should not be taken at face value due to the above-mentioned caveats about the potential violation of the exclusion restriction.

9 Financial vulnerability

The literatures on relationship lending and financial accelerators indicate that smaller, less transparent, and financially weaker firms should be more vulnerable to changes

[22] They include provinces along the Mediterranean Coast and in the Balearic and Canary Islands.

in credit market conditions. To find out if these features alter the impact of credit constraints, we estimate a triple difference (DDD) model, again implemented with four-year differences. We interact the treatment dummy with an exhaustive set of firm controls that capture the firm's credit history (captured by past defaults and rejected loan applications), financial strength (own funds and liquidity), bank dependence (bank debt over total debt), short-term liabilities (the share of bank debt that expires within 12 months), credit lines, number of banks (single- or multi-bank), productivity (service sector and export status), and other variables like age and size, that are traditionally used to proxy for the strength of agency problems.[23,24]

The estimation results appear in Table 12. As expected, having a bad credit record, as evidenced by rejected loan applications and, especially, past loan defaults, entails higher job losses during the crisis. More traditional financial indicators also attract the expected signs. The contraction of employment is stronger for firms with more debt and a higher share of short-term debt, and lower capital and liquidity. All these effects are stronger for firms exposed to weak banks, except for the case of liquidity. Lastly, firms working with a single bank reduced employment less than firms with multiple banking relationships, but as in the case of liquidity there are no significant differences between firms in the treatment and control groups.

As to other results, larger firms suffer slightly lower losses and this marginal effect is the same for firms in both groups of firms. Older firms, on the contrary, suffer higher losses, but the effect is cut by half for firms attached to weak banks. The positive sign on export status, both alone and interacted with WB_i, may have a couple of explanations. Exporting firms may have suffered a smaller drop in demand than firms that produce for the Spanish market –even though international trade also experienced a sizeable but short-lived drop at the start of the global financial crisis. Indeed, while real internal demand fell by 3.1% from 2006 to 2010, real exports increased by 4.5%. Alternatively, export status may capture cross-sectional differences in productivity –even

[23] The dummy variable for any application rejected is the complement of the one for all applications accepted in the baseline. The dummy for short-term debt above 50% replaces the share of short-term debt that was present in the baseline. The dummy for firms indebted with only one bank replaces the variable for the number of banking relationships.

[24] To avoid having to weigh estimates by the variables' average values, regressors are in deviations from their means.

within narrowly defined sectors, the most productive firms are typically the ones that manage to sell their products in international markets, while the least productive firms only sell in the domestic market. As expected, firms with a higher share of temporary jobs suffer higher job losses and the effect is stronger for firms in the treatment group. Lastly, we do not observe any significant difference by broad sector.

10 Margins of adjustment and aggregate effects

We end by probing deeper into the margins of adjustment available to firms. Our analysis in the previous section quantified the additional job losses due to the credit-rationing by weak banks, but it is also important to know what types of jobs are most at risk when firms face credit constraints and whether firms have explored alternative margins of adjustment, like changes in wages, to alleviate the impact on employment. Lastly, we estimate the causal impact of weak-bank attachment on firm exit.

10.1 The contribution of temporary jobs to job losses

Our DDD estimates indicate that labor shedding was higher the larger the shares of temporary workers that firms had in 2006. To further gauge the importance of temporary employment in the transmission of the credit shock, we analyze how weak-bank attachment affected the share of temporary workers at surviving firms.[25] The breakdown of employment by type of contract is observed for 91% of these firms. The first column of Table 13 shows that for this sample the temporary employment share fell by -0.5 pp between 2006 and 2010, as a result of the stronger credit constraints faced by the treated firms. Since weak-bank attachment caused a 1.4 pp drop in total employment for this sample of firms (Table 7) and the initial share of temporary jobs among surviving firms was equal to 21%, this means that temporary employment fell by 3.7 pp, contributing 0.8 pp to the overall employment loss in this sample of treated firms. In other words, temporary jobs made up around one-quarter of pre-crisis employment but accounted for 56% of employment adjustment in treated firms.[26]

[25] Financial market frictions may also have distorted the ex-ante mix of contract types in favor of temporary jobs, as shown in Caggese and Cuñat (2008), but this issue is outside the scope of our analysis.

[26] Berton *et al.* (2016) find a similar effect for firms in the Italian region of Veneto.

The over-representation of temporary workers in job losses is also observed in the aggregate (Bentolila *et al.*, 2012) and is commonly attributed to the lower firing costs of these contracts. Nevertheless, in the recent crisis credit rationing may also have played a role. Weak banks curtailed the funding of working capital through credit lines more than healthy banks. It is therefore natural to expect firms in the treatment group to concentrate their downsizing among temporary workers.

10.2 Changes in the wage bill

Our next experiment analyzes how weak-bank attachment affected the evolution of the total wage bill. What we would like to estimate is to what extent firms in the treatment group managed to alleviate the stringency of the credit constraints through a reduction in wages. Since we do not observe individual wages and the information on the characteristics of workers in our data is minimal, however, we have to conduct the analysis on the basis of the total wage bill of surviving firms.

Our estimate, in the second column of Table 13, indicates that weak-bank attachment is associated with a 1.6 pp drop in the wage bill of the treated firms. Accordingly, we conclude that the average wage of the employees of treated firms fell by 0.2 pp compared to the corresponding wage of the employees of the firms in the control group. The differential drop in the average wage is significant but also very small, and it could be driven by composition effects rather than by an adjustment of nominal wages, since we cannot control for worker characteristics. At any rate, our results indicate that wage adjustments have not played a meaningful role in alleviating the impact of credit constraints faced by Spanish firms during the crisis, which is consistent with the well-documented high real wage rigidity in Spain (Font *et al.*, 2015).

10.3 Probability of firm exit

After a recession, employment may recover more slowly if a large fraction of firms have close down than if they have only downsized. For this reason we estimate the effect of credit constraints at the intensive and extensive margins. The former is given by reestimating our baseline DD equation (3) for surviving firms alone, which as already reported yields a coefficient of -1.4 pp (Table 7, col. 4), a significantly lower figure than for the full sample. The extensive margin is tackled by estimating the effect of weak-bank attachment on firms' exit probability. We start with a linear probability model for exit in 2010 with respect to 2006, using the same specification as in equation (3) but now for a binary dependent variable. As seen in the first column of Table 14, weak-bank exposure leads to a marginal increase in the exit probability of weak-bank dependent firms of 1.1 pp, which represents an increase of 10.8% with respect to the baseline exit rate of 10.2%. We also try an alternative specification including the continuous treatment variable in place of the dummy variable. The estimated effect is 5.9 pp. This means, *ceteris paribus*, that a firm located at the ninth decile –with a ratio of weak-bank debt to assets of one-quarter– has a 1.5% higher probability of closing down, compared to a firm with a ratio of weak-bank debt to assets at the first decile –which is roughly nil. This amounts to 14.5% of the baseline exit rate.

10.4 Aggregate effects

Are the foregoing estimates large or small? We should clarify that these microeconomic estimates cannot be directly extrapolated to the aggregate economy. In general equilibrium there are further potential effects (Chodorow-Reich, 2014). For example, a drop in aggregate demand generally reduces labor demand by both constrained and unconstrained firms, but product demand may be shifted from the former to the latter, thus inducing an increase in their labor demand. With this caveat in mind, we can estimate job losses due to weak-bank attachment for each individual firm and then add them up over all firms in the sample.[27] Using the estimate from our DD baseline estimate of 2.8 pp (Table 3, col. 4), exposure to weak banks accounts for 24.4% of the total fall in employment among exposed firms in our sample. Given the employment share of exposed firms, this represents 7% of total job losses in our full sample. If we use the lower bound resulting from the Oster (2017) conjecture, which yielded an estimate of 1.1 pp, then the figures would respectively be 9.8% and 2.8%.

Using our estimates we can compute separately employment losses at surviving

[27] From equation (1), $(1+n_{ijt})/(1+n_{ijt-4}) = \exp(\Delta_4 \log(1+n_{ijt}))$, where $t=$2010. The estimated employment growth rate is then equal to: $\Delta_4 \widehat{n}_{ijt} = (1+n_{ijt-4})[\exp(\widehat{\alpha} + \widehat{\beta} WB_i + X_i\widehat{\gamma} + \widehat{\delta}_j) - 1]$. Estimated job losses due to weak-bank attachment then equal: $(\Delta_4 \widehat{n}_{ijt} \mid WB_i = 1) - (\Delta_4 \widehat{n}_{ijt} \mid WB_i = 0) = (1+n_{ijt-4})[\exp(\widehat{\alpha} + \widehat{\beta} + X_i\widehat{\gamma} + \widehat{\delta}_j) - \exp(\widehat{\alpha} + X_i\widehat{\gamma} + \widehat{\delta}_j)]$.

and closing firms. On the one hand, we can use the previously cited estimate for the surviving firms. On the other hand, we calculate the number of firm closures from the estimated probability of exit in Table 14 (col. 1) and the employment reduction so induced. Adding up both estimates, the overall job loss at exposed firms is 33.8%, which is higher than the baseline estimate, so that credit constraints explain 12% of total job losses in the sample. Survivors account for 48% of this overall loss and the rest corresponds to exiting firms. However, the estimated effect on each one differs markedly. Weak-bank exposure accounts for a full 54.2% of job losses at surviving firms, whereas it explains 33.8% of jobs lost due to firms closing down. The latter suggests that shocks other than credit supply restrictions play a larger role in the exit decision of firms.

11 Conclusions

In this paper we have analyzed the link between the solvency problems of Spain's weakest banks and the severe drop in employment suffered by this country during the Great Recession. We achieve identification by exploiting differences in lender health at the start of the crisis, as evidenced by public bailouts of savings banks. We proceed by comparing employment changes from the expansion to the recession between firms that are exposed to weak banks and those that are not. Our exercise is challenging, since the bank solvency problems are linked to corporate loan portfolios.

While we are not the first to study the link between external funding and employment outcomes, we do provide the first exhaustive analysis of these links on the basis of loan data from an official credit register. For practical purposes this data set can be considered as the census of loans to non-financial firms of all sizes, but mainly to the small and medium-sized, for which credit restrictions are strongest according to standard theory. Our exceptionally large and high-quality matched bank-loan-firm data set allows us to control exhaustively for ex-ante characteristics of firms and for potential endogeneity, as well as to perform a wide range of robustness checks. It also allows us to obtain more precise estimates and to refine the analysis in more directions than any existing study in the field.

Our results show that the firms attached to weak banks indeed destroyed more jobs than very similar firms working with healthier banks. For the average firm, the additional job losses due to weak-bank attachment are around 2.8 percentage points. This estimate implies that weak-bank exposure accounts for around 24% of the total fall in employment among exposed firms in our sample.

The extraordinary strength of the credit crunch in Spain is illustrated by the finding of sizeable effects even for the largest firms in our sample, whereas the evidence for the US only points to employment losses at the smallest firms. Furthermore, our analysis uncovers striking differences in the intensity of credit restrictions depending on firms' creditworthiness and the structure of their banking relationships. We also show that the brunt of the job losses due to credit constrains was borne by temporary employees, with little adjustment in wages. Separate estimates for employment losses at surviving and closing firms indicate that for survivors 52% of job losses at exposed firms are explained by weak-bank attachment, while 34% of the losses originated by firm closures are. Our paper is the first to offer this type of decomposition, which carries relevant implications for the speed of recovery after slumps.

A Appendix A. Additional tables

Table A.1. Spanish savings banks' restructuring process (end of year)

2006	2007	2010	2011	2012
		Banco Base (SIP)	Liberbank (SIP)	Liberbank
		Cajastur	Cajastur	
1 Caja C.-La Mancha		Banco CLM	Banco CLM	
2 Caja Cantabria		Caja Cantabria		
3 Caja Extremadura		Caja Extremadura		
				Banco Sabadell
4 C. A. Mediterráneo		C. A. Mediterráneo	Banco CAM	
		BFA (SIP)		Bankia / BFA
5 Caja Madrid		Caja Madrid		
6 Caja Rioja		Caja Rioja		
7 Caixa Laietana		Caixa Laietana		
8 Caja I. Canarias		Caja I. Canarias		
9 Caja de Segovia		Caja de Segovia		
10 Caja de Ávila		Caja de Ávila		
11 Bancaja		Bancaja		
12 Banco de Valencia		Banco de Valencia		
				Banco de Valencia
		Mare Nostrum (SIP)		Mare Nostrum (SIP)
13 La General		La General		
14 Caja de Murcia		Caja de Murcia		
15 Caixa Penedès		Caixa Penedès		
16 Sa Nostra		Sa Nostra		
17 Caixa Catalunya		CatalunyaCaixa		Catalunya Banc
18 Caixa Manresa				
19 Caixa Tarragona				
		Banca Cívica (SIP)		Banca Cívica (SIP)
20 Caja de Burgos		Caja de Burgos		
21 Caja de Navarra		Caja de Navarra		
22 Caja Canarias		Caja Canarias		
	Cajasol	Cajasol		
23 Caja S. Fernando				
24 El Monte				
25 Caja Guadalajara				
26 Caixanova		Novacaixagalicia		NCG Banco
27 Caixagalicia				
			Grupo BBK	Grupo Kutxabank
28 Cajasur		Cajasur		
29 Caja España		Caja España-Duero		Banco CEISS
30 Caja Duero				
31 Caixa Manlleu		Unnim		Unnim Banc
32 Caixa Sabadell				
33 Caixa Terrassa				

Note. SIP: Institutional Protection System (see Section 3). Source: Banco de España (2014).

Table A2. Returns on securities issued by Spanish banks in 2006
Dependent Variable: Coupon differential (basis points)

	Coefficient	St. error
Weak Bank	2.84	4.74
Mortgage Backed Security	15.55	0.29
Years to Maturity	0.83	0.13
Investment Grade (AA+ to BBB-)	24.37***	2.35
Speculative Grade (BB+ to D)	131.01***	25.17
Collateralized Debt Obligation	0.32	17.61
Customer Loan	2.76	7.95
Corporate Loan	5.55	14.16
Residential Mortgage	-18.90**	8.82
No Guarantor	-5.65	6.96
Private Sector Bank Guarantor	13.33	13.43
State/Provincial Authority Guarantor	-4.41	10.56
Supranational Guarantor	4.65	5.43
R^2	0.44	
No. of observations	255	

Notes. OLS estimates of coupon differentials of all asset and mortgage backed securities issued by Spanish banks in 2006 with reference to the 3-month Euribor. Reference group: Asset Backed Security, Prime Risk (AAA), Auto Receivables as collateral, Central Government as guarantor. Data for 24 issuer parents drawn from Dealscan. Month of issue dummies are included. Standard errors are adjusted for 24 clusters in issuing bank.

Table A3. Credit rationing at the firm-bank level. Loan maturity

Dependent variable: $\Delta_4 \log\left(1 + Credit_{ijb}\right)$

I(Credit Line$_{ib}$)	0.057***	(0.016)
I(Credit Line$_{ib}$) $\times$ WB_i	-0.082**	(0.040)
I(Maturity$_{ib}$ 1-3 years)	-0.035*	(0.020)
I(Maturity$_{ib}$ 1-3 years) $\times$ WB_i	0.045	(0.046)
I(Maturity$_{ib}$ 3-5 years)	-0.115***	(0.022)
I(Maturity$_{ib}$ 3-5 years)$\times$ WB_i	0.034	(0.048)
I(Maturity$_{ib}$ 5 years)	0.012	(0.021)
I(Maturity$_{ib}$ 5 years)$\times$ WB_i	0.091*	(0.048)
I(Maturity$_{ib}$ indeterminate)	0.201	(0.147)
I(Maturity$_{ib}$ indeterminate)$\times$ WB_i	0.170	(0.247)
Firm fixed effects	yes	
Firm controls	–	
Bank fixed effects	yes	
Bank controls	yes	
Firm-bank controls	yes	
Industry $\times$Province fixed effects	–	
Several banks	yes	
Balance-sheet data	yes	
R^2	0.453	
No. obs.	236,691	
	72,287	

Notes. OLS estimates for 2010. Bank controls: log of total assets, leverage ratio, liquidity ratio, rate of return on assets and provisions normalized by net interest income. Firm-bank controls: length of firm-bank relationship in months and past defaults. Firm control variables: see Table 2. "yes/no/–" indicates whether the corresponding set of variables is either included, not included or redundant. Robust standard errors corrected for multiclustering at the firm and bank level appear between parentheses. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

B Appendix 2. Definitions of variables

Employment. Computed as the average level over the year, weighing temporary employees by their weeks of work.
Treatment. The weak-bank dummy (0-1) is equal to 1 if the ratio of the firm's loans from weak banks to total assets in 2006 is above the first quartile of the distribution. Weak Bank Intensity is the ratio itself.

Municipality (2,749). They correspond to firms' headquarters. They need to have at least two firms in the sample.
Industry (78). Excluded industries (share of output sold to Construction and Real Estate in 2000 shown between parentheses): Extraction of Non-metallic Minerals (35.2%), Wood and Cork (21.1%), Cement, Lime, and Plaster (46.4%), Clay (60.1%), Non-metallic Mineral Products n.e.c. (85.4%), Fabricated Metal Products except Machinery and Equipment (23.3%), Machinery and Electric Materials (19.2%), and Rental of Machinery and Household Goods (26.2%).
Industry dummies (firms' main activity in 2006): Standard 2-digit NACE rev. 1.1 classification, see www.ine.es/daco/daco42/clasificaciones/cnae09/estructura_en.pdf.

Control variables (stocks are book values in December). Temporary Employment (temporary employees/total number of employees), Age (current year–year of creation), Size (Total assets in million euros), Exporter (indicator for selling abroad), Own Funds (own funds/total assets), Liquidity (liquid assets/total assets), Return on Assets (earnings before interest, taxes, depreciation and amortization/Assets), Bank Debt (bank debt/total debt), Short-Term Bank Debt (debt up to one year/total bank debt), Long-Term Bank Debt (debt of five years or more/total bank debt), Non-Collateralized Bank Debt (non-collateralized loans/total bank debt), Credit Line (1 if at least one), Banking Relationships (number of banks with outstanding loans), Current Loan Defaults (1 if any non-performing loan in 2006), Past Loan Defaults (1 if any non-performing loan in 2002-2005), Past Loan Applications (number in 2002-2005), and All Loan Applications Accepted (0-1 dummy).
Cells for matching estimation. Province (1 if East coast of Spain and Balearic or Canary Islands). Industry (1 if Agriculture, Farming, Fisheries, and Extractive). Value of 1 if above the median: Temporary Employment, Age, Size, Own Funds, Liquidity, Return on Assets, Bank Debt, Non-Collateralized Bank Debt, and Past Loan Applications. Value of 1 if above 50%: Short-Term Bank Debt and Long-Term Bank Debt. Value of 1 if variable equal to 1: Banking Relationships. Already 0-1 dummies in baseline specification: Exporter, Current Loan Defaults, Past Loan Defaults, Credit Line, and All Past Applications Accepted.

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Table 1. Descriptive statistics of healthy and weak banks (2006)

	Healthy banks		Weak banks		Mean	Normalized
	Mean	St. dev.	Mean	St. dev.	t test	difference
ln(Assets)	13.7	2.1	16.4	1.0	7.1	1.14
Own Funds/Assets	8.4	9.0	5.2	1.2	-2.0	-0.35
Liquidity/Assets	23.7	22.4	11.5	4.5	-3.1	-0.54
Return on Assets	1.0	1.7	0.9	0.3	-0.5	-0.09
Non-Performing Loans	1.5	6.3	0.7	0.6	-0.7	-0.13
Non-Performing Loans (2012)	8.6	12.7	22.0	6.0	3.5	0.95
Loans to REI/Loans to NFF	36.8	22.3	67.9	8.1	7.9	1.31
Securitized Loans/Assets	14.9	10.5	18.5	6.3	1.6	0.30

Notes. There are 206 healthy and 33 weak banks. Non-performing Loans is a ratio on the value of all loans. Securitized Loans/Assets is computed only for banks that securitize. NFF denotes non-financial firms. Except for assets, variables are ratios in percentages. The last column shows the t ratio of the test for the difference of the means and the last column the normalized difference test of Imbens and Wooldridge (2009). See definitions in Appendix 2. Source: Own computations on bank balance sheet data from the Bank of Spain.

Table 2. Descriptive statistics of control and treated firms (2006)

	Control					Treated					Mean	Norm.
	Mean	St. Dev.	P25	P50	P75	Mean	St. Dev.	P25	P50	P75	t test	diff.
Loans with WB/Assets	0.3	0.9	0.0	0.0	0.0	22.8	17.1	9.7	17.3	30.9	427.3	1.31
Share of Loans with WB	8.5	24.1	0.0	0.0	0.0	68.5	30.3	40.9	73.3	100.0	403.9	1.55
Employment (employees)	25.3	365.6	2.0	6.0	13.0	20.3	207.2	2.0	5.0	13.0	-2.7	-0.01
Temporary Employment	20.4	25.4	0.0	11.1	33.3	22.7	26.0	0.0	14.5	36.6	16.0	-0.06
Age (years)	12.6	9.8	6.0	11.0	17.0	11.7	8.7	6.0	10.0	16.0	-17.1	-0.07
Size (million euros)	5.8	118.2	0.3	0.6	1.7	3.6	27.5	0.3	0.6	1.7	-4.0	-0.02
Exporter	13.0	33.7	0.0	0.0	0.0	13.1	33.7	0.0	0.0	0.0	0.3	0.00
Own Funds/Assets	34.4	23.8	14.2	30.3	51.1	24.9	18.5	10.0	20.8	35.8	-74.3	-0.31
Liquidity/Assets	12.4	15.1	1.9	7.0	17.4	8.6	11.8	1.1	4.2	11.2	-47.3	-0.20
Return on Assets	6.7	11.4	1.8	4.9	10.2	5.2	9.0	2.0	4.4	8.0	-23.8	-0.10
Bank Debt	30.7	26.7	6.8	24.8	49.8	48.5	23.5	29.4	47.4	66.2	120.8	0.50
Short-Term Bank Debt (< 1 yr)	48.8	41.5	0.0	46.7	100.0	45.7	37.1	4.3	44.9	80.7	-13.2	-0.05
Long-Term Bank Debt (> 5 yrs)	21.5	35.3	0.0	0.0	37.1	29.5	36.3	0.0	7.4	59.0	39.7	0.16
Non-Collateralized Bank Debt	81.9	33.4	82.7	100.0	100.0	73.7	35.6	47.4	100.0	100.0	-42.3	-0.17
Credit Line (has one)	69.0	46.3	0.0	100.0	100.0	72.2	44.8	0.0	100.0	100.0	12.5	0.05
Banking Relationships (no.)	1.9	1.5	1.0	1.0	2.0	3.0	2.7	1.0	2.0	4.0	103.7	0.37
Current Loan Defaults	0.3	5.6	0.0	0.0	0.0	0.6	7.4	0.0	0.0	0.0	6.8	0.03
Past Loan Defaults	1.4	11.9	0.0	0.0	0.0	2.4	15.2	0.0	0.0	0.0	12.7	0.05
Past Loan Applications	54.2	49.8	0.0	100.00	100.00	68.9	46.3	0.0	100.0	100.0	52.7	0.22
All Loan Applications Accepted	22.0	41.4	0.0	0.0	0.0	26.2	44.0	0.0	0.0	100.0	17.6	0.07

Notes. Observations: 149,458 firms; 106,128 control and 43,330 treated firms. WB denotes weak banks. Variables are ratios in percentages unless otherwise indicated. The twelfth column shows the t ratio on the test for the difference of the means and the last column the normalized di§erence test of Imbens and Wooldridge (2009). See definitions in Appendix 2.

Table 3. Credit rationing at the firm-bank level
Dependent variable: $\Delta_4 \log(1 + Credit_{ijb})$

	(1)	(2)	(3)	(4)	(5)	(6)
	All firms	Multi-bank	Fixed effects	Inter-actions	Positive credit	Real estate
WB_b	-0.232***	-0.256***	-0.255***		-0.079**	-0.180*
	(0.088)	(0.094)	(0.008)		(0.034)	(0.096)
I(Credit Line$_{ib}$)				0.074***		
				(0.015)		
I(Credit Line$_{ib}$)$\times WB_b$				-0.106**		
				(0.039)		
Firm fixed effects	no	no	yes	yes	yes	yes
Firm controls	yes	yes	–	–	–	–
Bank fixed effects	no	no	no	yes	no	no
Bank controls	yes	yes	yes	yes	yes	yes
Firm-bank controls	yes	yes	yes	yes	yes	yes
Ind.$\times$Prov. f.e.	yes	yes	–	–	–	–
Several banks	no	yes	yes	yes	yes	yes
Balance-sheet data	yes	yes	yes	yes	yes	yes
R^2	0.060	0.059	0.407	0.452	0.394	0.406
No. obs.	304,089	236,691	236,691	236,691	126,863	236,691
No. firms	139,685	72,287	72,287	72,287	42,630	72,287

Notes. OLS estimates for 2010. Bank controls: log of total assets, leverage ratio, liquidity ratio, rate of return on assets and provisions normalized by net interest income. Firm-bank controls: length of firm-bank relationship in months and past defaults. Firm control variables: see Table 2. "yes/no/–" indicates whether the corresponding set of variables is either included, not included or redundant. Robust standard errors corrected for multiclustering at the firm and bank level appear between parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 4. Credit rationing at the firm level
Dependent variable: $\Delta_4 \log(1 + Credit_{ij})$

	(1)	(2)	(3)
	All firms	Multi-bank	Real estate
WB_i	-0.053***	-0.031***	-0.039***
	(0.015)	(0.011)	(0.017)
Firm controls	yes	yes	yes
Industry×Municipality fixed effects	yes	yes	yes
Multiple banking relationships	no	yes	no
Balance-sheet data	yes	yes	yes
R^2	0.215	0.246	0.215
No. obs.	149,458	74,045	149,458

Notes. OLS estimates for 2010. Control variables: see Table 2. Robust standard errors corrected for multiclustering at the industry, municipality, and main bank levels appear between parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5. The employment effect of weak-bank attachment. Difference in Differences
Dependent variable: $\Delta_4 \log(1 + n_{ij})$

	(1)	(2)	(3)	(4)	(5)	(6)
			Sign. ctrls	Baseline		Placebo
WB_i	-0.074***	-0.076***	-0.035***	-0.028***	-0.028**	0.006
	(0.013)	(0.010)	(0.006)	(0.006)	(0.007)	(0.007)
Firm controls (1)	no	no	yes	yes	yes	yes
Firm controls (2)	no	no	no	yes	yes	yes
Municipality fixed effects	yes	–	–	–	–	–
Industry fixed effects	yes	–	–	–	–	–
Industry×Municipal. f.e.	no	yes	yes	yes	yes	yes
Main bank fixed effects	no	no	no	no	yes	no
R^2	0.046	0.150	0.155	0.177	0.179	0.203
No. obs.	149,458	149,458	149,458	149,458	149,458	112,933

Notes. OLS estimates for 2010, except for column (6), which is dated in 2006. Control variables: see Table 2. "yes/no/–" indicates whether the corresponding set of variables is either included, not included or redundant. In column (3) only performance-related firm control variables are included. Robust standard errors corrected for multiclustering at the industry, municipality, and main bank levels appear between parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 6. The employment effect of weak-bank attachment. Instrumental Variables

Dependent variable: $\Delta_4 \log(1 + n_{ij})$

	(1)	(2)
	All firms	Multi-bank firms
Instrumented variable	$\Delta_4 \log(1 + Credit_{ijk})$	
	0.519***	0.797***
	(0.179)	(0.294)
First stage		
WB_i	-0.053***	-0.031***
	(0.015)	(0.011)
Firm controls	yes	yes
Industry×Municipality fixed effects	yes	yes
Overall effect ($\mu\phi$)	-0.028	-0.025
F test / p value	13.1/0.00	7.65/0.00
No. obs.	149,458	74,045

Notes. Instrumental variable estimates for 2010. Control variables: see Table 2. Robust standard errors corrected for multiclustering at the industry, municipality, and main bank levels appear between parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 7. The employment effect of weak-bank attachment. Difference in Differences

Dependent variable: $\Delta_4 \log(1 + n_{ij})$

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	WB Intensity	Median	Third quartile	Survivors	Alternat. measure	Tradable goods	Loans to REI	2007 ex-ante	2002
WB_i	-0.092^{***}	-0.030^{***}	-0.033^{***}	-0.014^{***}	-0.034^{***}	-0.058^{***}	-0.030^{***}	-0.019^{***}	-0.015^{**}
	(0.020)	(0.008)	(0.008)	(0.004)	(0.004)	(0.023)	(0.008)	(0.006)	(0.006)
Firm controls	yes	yes	yes	yes	yes	yes	yes	yes	yes
Industry × Municipality f.e.	yes	yes	yes	yes	yes	yes	yes	yes	yes
R^2	0.177	0.177	0.177	0.181	0.183	0.200	0.177	0.130	0.188
No. obs.	149,458	149,458	149,458	133,122	149,458	16,199	149,458	145,322	71,703

Notes. OLS estimates for 2010. Control variables: see Table 2. Robust standard errors corrected for multiclustering at the industry, municipality, and main bank levels appear between parentheses. Significance level: $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 8. The employment effect of weak-bank attachment. Panel estimates

Dependent variable: $\Delta_4 \log(1 + n_{ijt})$

$2008 \times WB_i$	-0.012***
	(0.004)
$2009 \times WB_i$	-0.020***
	(0.004)
$2010 \times WB_i$	-0.027***
	(0.006)
Firm controls	yes
Firm fixed effects	yes
Industry×Municipality×Year fixed effects	yes
R^2	0.789
No. obs.	563,189

Notes. OLS estimates for 2007-2010. Control variables: see Table 2. Robust standard errors corrected for multiclustering at the industry, municipality, and main bank levels appear between parentheses. Significance level:* p<0.10, ** p<0.05, *** p<0.01.

Table 9. The employment effect of weak-bank attachment. Matching

Dependent variable: $\Delta_4 \log(1 + n_{ijt})$

	(1)	(2)	(3)	(4)
	Propensity score	Exact	Propensity score	Exact
WB_i	-0.032***	-0.016**		
	(0.009)	(0.009)		
WB_i *Intensity*			-0.065***	-0.052**
			(0.016)	(0.020)
Firm controls	yes	yes	yes	yes
Municipality×Industry fixed effects	yes	yes	yes	yes
Overall effect	–	–	-0.016	-0.012
R^2	0.228	0.245	0.228	0.245
No. obs.	55,712	133,816	55,712	133,816

Notes. Weighted least squares estimates for 2010. Control variables: see Table 2. Robust standard errors corrected for multiclustering at the industry, municipality, and main bank levels appear between parentheses. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Table 10. Descriptive statistics of firms below (control) and above (treated) the median of weak bank density ratio in 1988 (2006)

	Control					Treated					Normalized
	Mean	St. Dev.	P25	P50	P75	Mean	St. Dev.	P25	P50	P75	differences
Loans with WB/Assets	4.8	11.9	0.0	0.0	2.1	8.8	15.1	0.0	0.0	11.8	0.20
Share of Loans with WB	19.3	34.2	0.0	0.0	24.2	32.3	39.8	0.0	5.0	67.8	0.25
Employment (employees)	29.5	375.3	2.0	6.0	14.0	18.3	273.3	2.0	5.0	12.0	-0.02
Temporary Employment	19.9	25.3	0.0	10.0	33.3	22.2	25.9	0.0	14.0	35.1	0.06
Age (years)	12.6	9.9	6.0	11.0	17.0	12.0	9.1	6.0	11.0	16.0	-0.04
Size (million euros)	6.9	132.9	0.3	0.6	1.8	3.5	52.8	0.2	0.6	1.6	-0.02
Exporter	12.6	33.1	0.0	0.0	0.0	13.6	34.2	0.0	0.0	0.0	0.02
Own Funds/Assets	32.6	23.2	13.3	28.2	48.3	30.6	22.5	12.1	25.9	45.2	-0.06
Liquidity/Assets	11.7	14.8	1.6	6.1	16.1	11.0	13.8	1.6	5.9	15.1	-0.03
Return on Assets	6.4	11.3	1.7	4.7	9.9	6.0	10.3	1.9	4.7	9.1	-0.03
Bank Debt	34.3	27,2	10.0	30.4	54.6	37.4	26.8	14.0	34.6	57.6	0.08
Short-Term Bank Debt (< 1 yr)	48.4	40.9	0.0	46.7	97.3	47.4	39.8	0.0	45.5	90.6	-0.02
Long-Term Bank Debt (> 5 yrs)	23.0	35.7	0.0	0.0	43.4	24.6	35.7	0.0	0.0	48.6	0.03
Non-Collateralized Bank Debt	79.7	34.5	68.2	100.0	100.0	79.4	34.0	64.5	100.0	100.0	-0.01
Credit Line (has one)	71.0	45.4	0.0	100.0	100.0	68.8	46.3	0.0	100.0	100.0	-0.03
Banking Relationships (no.)	2.2	2.0	1.0	2.0	3.0	2.3	2.0	1.0	2.0	3.0	0.03
Current Loan Defaults	0.4	6.2	0.0	0.0	0.0	0.4	6.2	0.0	0.0	0.0	0.00
Past Loan Defaults	1.7	12.9	0.0	0.0	0.0	1.7	13.0	0.0	0.0	0.0	0.00
Past Loan Applications	57.2	49.5	0.0	100.00	100.00	59.8	49.0	0.0	100.0	100.0	0.04
All Loan Applications Accepted	22.7	41.9	0.0	0.0	0.0	23.7	42.5	0.0	0.0	100.0	0.02

Notes. Observations: 149,458 firms; 106,128 control and 43,330 treated firms. WB denotes weak banks. Variables are ratios in percentages unless otherwise indicated. The last column shows the normalized difference test of Imbens and Wooldridge (2009). See definitions in Appendix 2.

Table 11. The employment effect of weak-bank attachment. Exogenous variation

Dependent variable: $\Delta_4 \log(1 + n_{ijt})$

	(1)	(2)
Instrumented variable	WB_i	WB_i $Intensity$
	-0.076**	-0.320**
	(0.036)	(0.157)
First stage		
$Weak\ bank\ density_i$	0.445***	0.105***
	(0.084)	(0.025)
Firm controls	yes	yes
Municipality×Industry fixed effects	no	no
Industry fixed effects	yes	yes
Coast fixed effects	yes	yes
Overall effect	-0.076	-0.073
F test / p value	17.8/0.00	28.3/0.00
No. obs.	149,458	149,458

Notes. Instrumental variable estimates for 2010. Control variables: see Table 2. "yes/no" indicates whether the corresponding set of variables is either included, not included. Robust standard errors corrected for multiclustering at the industry, municipality, and main bank levels appear between parentheses. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Table 12. The employment effect of weak-bank attachment. Triple Differences

Dependent variable: $\Delta_4 \log(1 + n_{ijt})$

WB_i	-0.019***	(0.007)
Rejected application$_i$	-0.066***	(0.008)
Rejected application$_i \times WB_i$	-0.029**	(0.012)
Past Defaults$_i$	-0.209***	(0.029)
Past Defaults$_i \times WB_i$	-0.041**	(0.020)
Short-term debt$_i$	-0.089***	(0.013)
Short-term debt$_i \times WB_i$	-0.036***	(0.011)
Bank debt$_i$	-0.096***	(0.017)
Bank debt$_i \times WB_i$	-0.081***	(0.022)
Own funds ratio$_i$	0.061***	(0.026)
Own funds ratio$_i \times WB_i$	0.134***	(0.027)
Liquidity ratio$_i$	0.118***	(0.022)
Liquidity ratio$_i \times WB_i$	0.050	(0.061)
Single bank$_i$	0.012**	(0.005)
Single bank$_i \times WB_i$	0.019	(0.015)
log(Total Assets$_i$)	0.009*	(0.005)
log(Total Assets$_i$)$\times WB_i$	0.003	(0.005)
log(1+Age$_i$)	-0.054***	(0.006)
log(1+Age$_i$)$\times WB_i$	0.027***	(0.008)
I(Exporter$_i$)	0.176***	(0.011)
I(Exporter$_i$) $\times WB_i$	0.062***	(0.020)
I(Temporary employees$_i$)	-0.112***	(0.010)
I(Temporary employees$_i$) $\times WB_i$	-0.027***	(0.014)
I(Services$_i$) $\times WB_i$	0.019	(0.017)
Industry×Municipality fixed effects	yes	
Firm controls	yes	
R^2	0.177	
No. obs.	149,458	

Notes. OLS estimates for 2007-2010. Control variables: see Table 2. Robust standard errors corrected for multiclustering at the industry, municipality, and main bank levels appear between parentheses. Significance level: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 13. The employment effect of weak-bank attachment. Margins of adjustment

	(1)	(2)
Dependent variable	$\Delta_4\,(n_{temp,ijk}/n_{ijk})$	$\Delta_4 \log\,(Wage\ bill_{ijk})$
WB_i	-0.005***	-0.016***
	(0.002)	(0.006)
Firm controls	yes	yes
Municipality×Industry×Year fixed effects	yes	yes
R^2	0.174	0.205
No. obs.	122,725	87,451

Notes. OLS estimates for 2007-2010. Control variables: see Table 2. Robust standard errors corrected for multiclustering at the industry, municipality, and main bank levels appear between parentheses. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Table 14. Effect of weak-bank attachment on firm exit
Dependent variable: Probability of exit

	(1)	(2)
WB_i	0.011***	
	(0.004)	
WB_i *Intensity*		0.059***
		(0.014)
Firm controls	yes	yes
Municipality×Industry fixed effects	yes	yes
R^2	0.173	0.173
No. obs.	150,442	150,442

Notes. OLS estimates for 2010. Control variables: see Table 2. "yes/no" indicates whether the corresponding set of variables is either included or not. Robust standard errors corrected for multiclustering at the industry, municipality, and main bank levels appear between parentheses. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

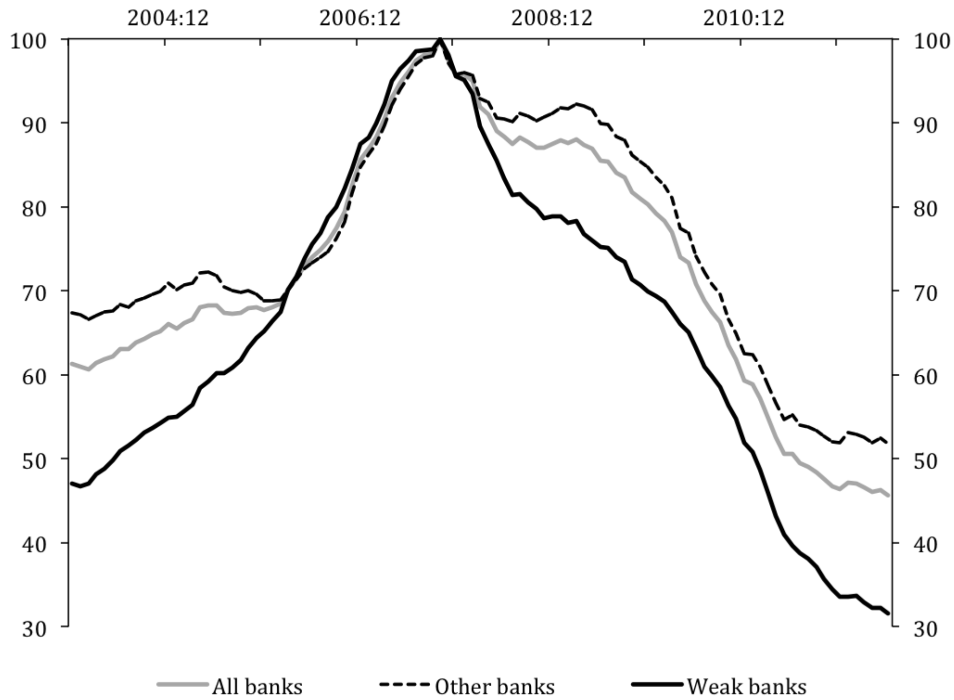

Figure 1: New credit to non-financial firms by bank type (12-month backward moving average, 2007:10=100)

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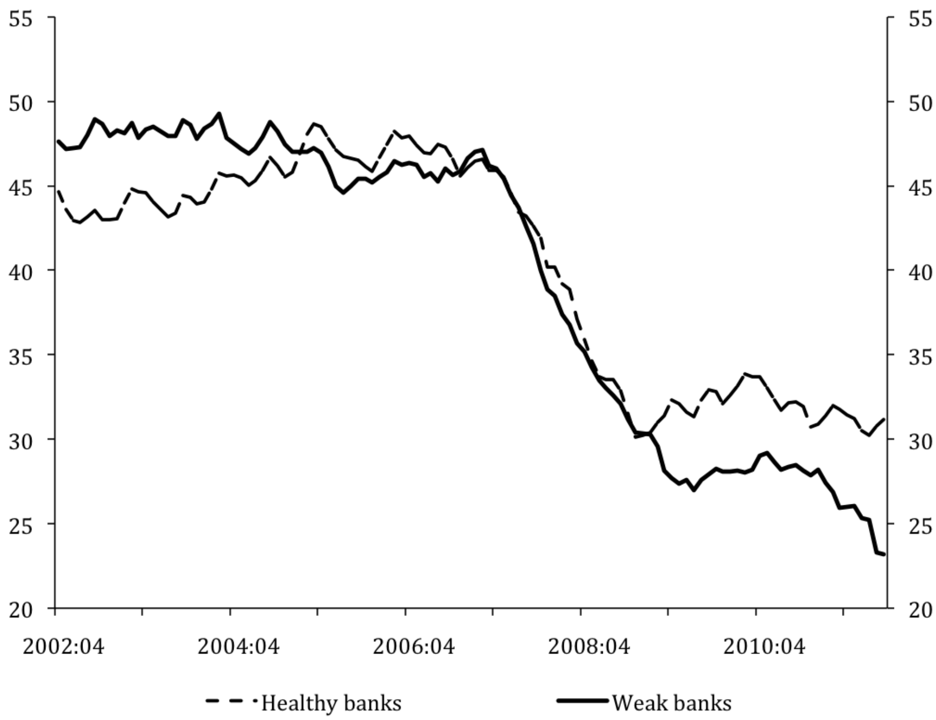

Figure 2: Acceptance rates of loan applications by non-current clients, by bank type, 2002:4-2012:6 (%)

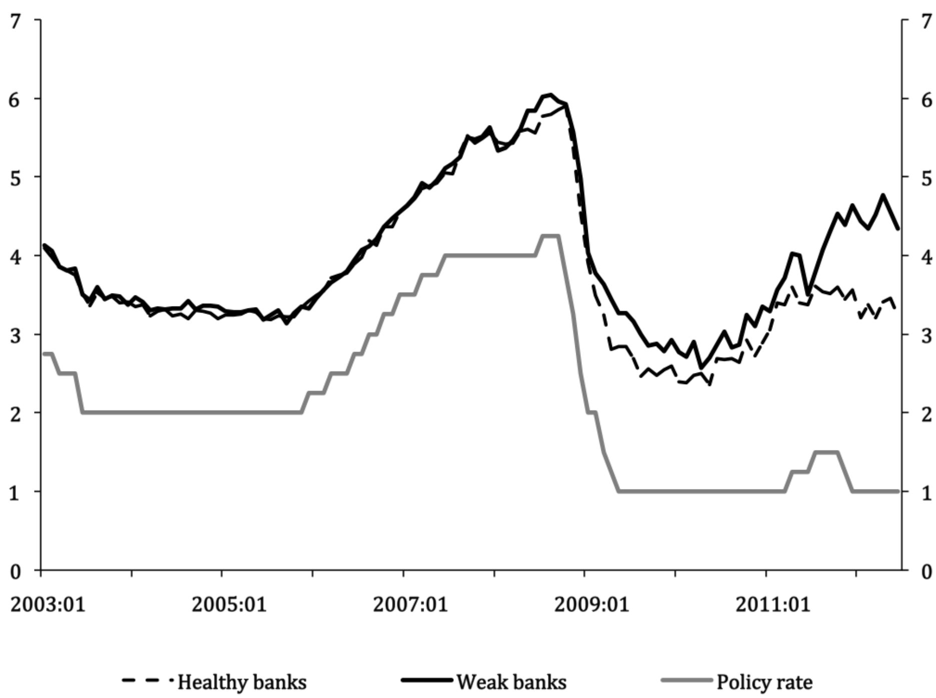

Figure 3: Average annual interest rate for new loans to non-financial firms by bank type and the policy rate, 2003:1-2012:6 (%)

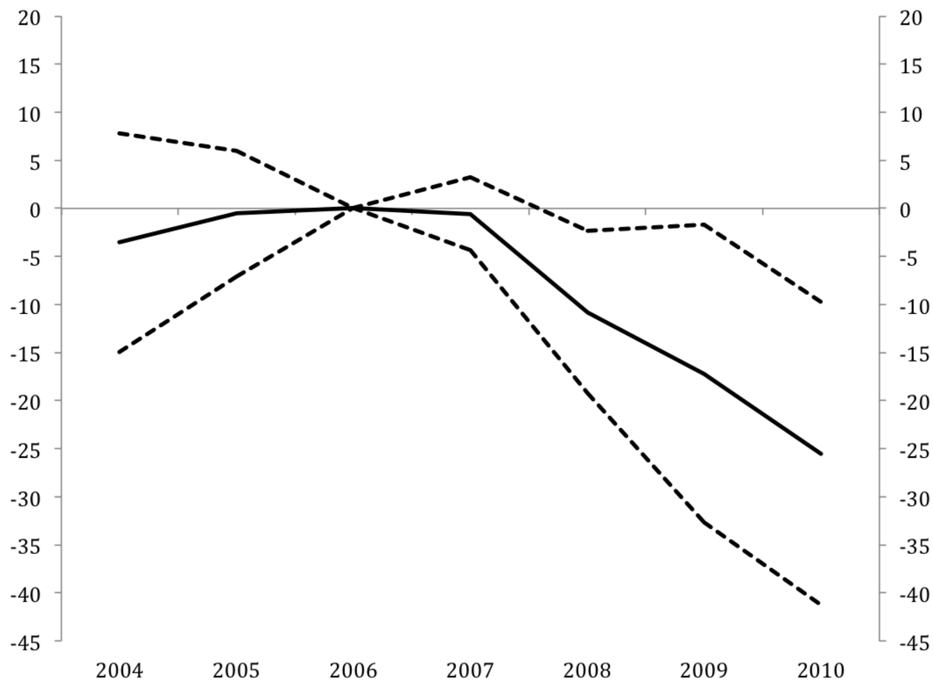

Figure 4a. Effect of weak-bank attachment on credit at the local level, 2002-2010 (pp)

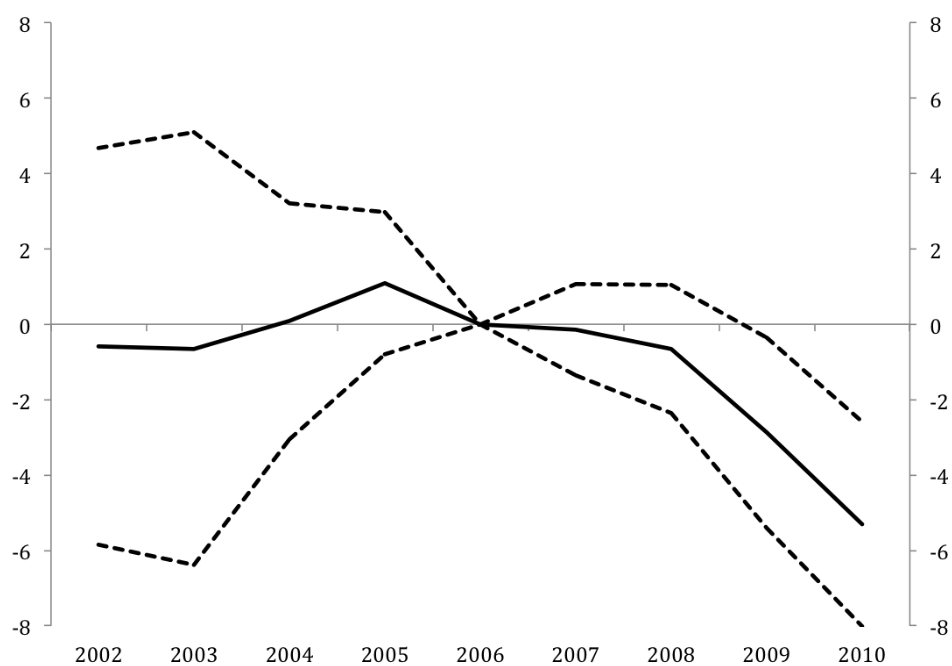

Figure 4b. Effect of weak-bank attachment at the firm level, 2002-2010 (pp)

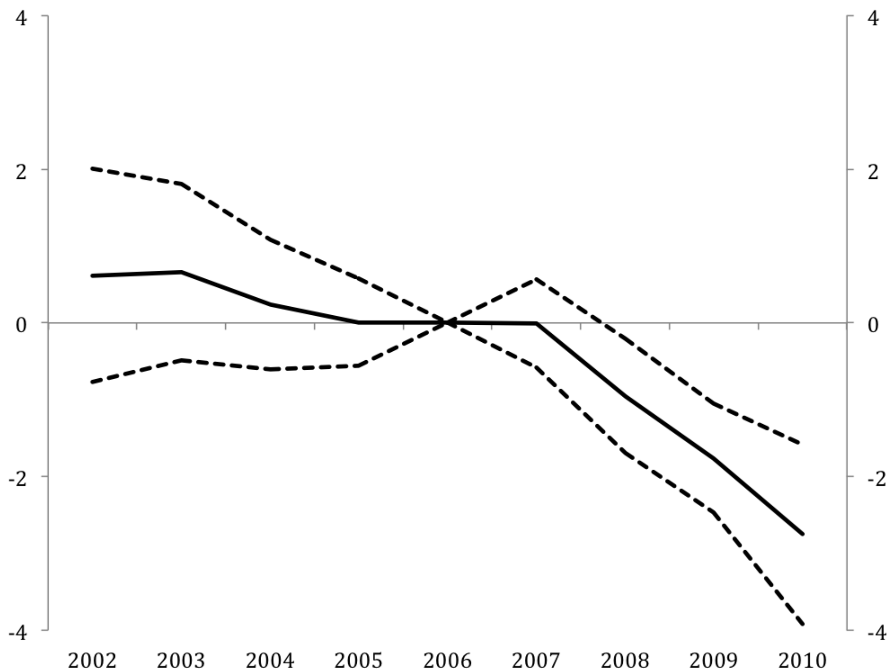

Figure 5. Employment effect of weak-bank attachment, 2002-2010 (pp)