When Credit Dries Up: Job Losses in the Great Recession

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Abstract

This paper studies whether the solvency problems of Spain's weakest banks during the Great Recession have caused real effects. Data from the official credit register of the Bank of Spain indicate that those banks curtailed lending well in advance of their bailout. We show the existence of a credit supply shock, controling for firm fixed effects, and assess its impact by comparing the change in employment between 2006 and 2010 at firms that were clients of weak banks to those at comparable non-client firms. 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 downsizing at attached surviving firms and one-third of losses due to exposed-firm exits.

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 give rise to substantial employment 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 employment 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 official credit register of the Bank of Spain to analyze the link between unprecedented drops in bank lending and employment in Spain. Our identification strategy exploits large cross-sectional differences in bank health at the onset of the crisis. Spain suffered the collapse of the construction sector when a housing bubble exploded, which affected the entire banking sector. Most of its banks were cut off from wholesale funding for a while, but the main problems were concentrated in savings banks (*Cajas de Ahorros*). We focus on the banks 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. This set of *weak* banks started to curtail lending relative to the other banks almost two years ahead of the bailout process. The objective of this paper is to explore how this credit supply shock affected employment at the firms that maintained a pre-crisis relationship with any of these weak banks during the period 2006-2010.

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, Annex Tables 4 and 14). 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 from credit supply shocks.

A number of recent studies have demonstrated the adverse impact of such shocks on investment, but much less is known about their impact on employment (see Section 2). Also, most of this work is 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. 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 representative 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 to determine whether the loan application was granted or not.

Our high-quality data allow us to perform more tests than related studies and to explore the existence of heterogeneous effects along many dimensions, but the main value of our detailed information on lenders and borrowers is that we are able 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 a clean identification of the credit supply shock we use the standard procedure of Khwaja and Mian (2008) to analyze credit growth at the firm-bank level. The results clearly show 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 show 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 firm-bank 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.

The second main challenge is to control for selection effects. An inspection of the data reveals that, on average, healthy banks worked with better firms than weak banks. This issue is crucial, since the absence of exhaustive controls for these underlying differences would bias our estimates (e.g. Paravisini *et al.* 2015). Moreover, selection may also occur on unobservables and in this case the introduction of firm controls need not be sufficient to avoid the bias. In the analysis of credit growth at the firm-bank level this problem is dealt with introducing firm fixed effects. Moreover, we also estimate a 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 for the impact of weak-bank attachment on credit growth is virtually identical to the one obtained in the within-firm specification. This clearly indicates 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 problem of selection on unobservables. Our baseline difference-in-differences specification regresses the growth rate of firm-level employment between 2006 and 2010 on a dummy variable for firms with a weak-bank loan-to-asset ratio above a certain threshold. This specification includes the same set of firm controls and industry-municipality fixed effects as our firm-level estimator for credit growth. Since we cannot perform the same counterfactual exercise as in the case of credit, we first follow a procedure developed in Oster (2015) that exploits the sensitivity of our coefficient of interest and the regression R^2 to the inclusion of observables to place an upper-bound on the impact of unobservables. 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 of these two cases do we find significant differences with our baseline. 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. Before this legal change savings banks could at most open twelve branches outside their home region. We calculate the share of bank branches at the municipal level in December 1988 that were owned by one of the weak banks and we use this as an instrument for firms' weak-bank attachment in 2006. The results of this instrumental variable (IV) setup have to interpreted with caution, as we explain 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 separate effects of weak-bank attachment on credit lines and loans with a maturity above one year, finding that weak banks strongly reduced access to credit lines relative to healthy banks while the opposite is true for long-term loans. 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 employment losses through adjustments along the internal and the 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 the job losses in downsizing firms than in exiting firms (54.2% vs. 33.8%, respectively). Lastly, we find that financially vulnerable firms, for example those with a patchy credit history, suffer much higher 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 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 two most closely related papers are by Greenstone *et al.* (2014) and Chodorow-Reich (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 is predictive of the reduction in county-level credit to small, standalone firms and their employment levels over 2008-2009. Still, 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.

[1] Below we review the related studies that exploit the variation in lender health at the onset of the global financial crisis of 2007-2009. Several other studies exploit the variation induced by large external shocks to the banking system (e.g. Chava and Purnanandam, 2011, Benmelech *et al.*, 2012). Alternatively, Almeida *et al.* (2012), Benmelech *et al.* (2012), and Boeri *et al.* (2013) exploit differences in the debt maturity structure of firms. Lastly, Garicano and Steinwender (2015) compare the response of different types of investment at foreign-owned and nationally-based manufacturing firms in Spain.

[2] Two other papers focus on the employment effects of the global financial crisis. Popov and Rocholl (2015) analyze the impact of German savings banks' exposure to the US subprime crisis on the labor demand of firms, while Fernandes and Ferreira (2015) explores the impact of financing constraints on the choice between permanent and fixed-term contracts. Neither of these studies has access to loan data. There are also some papers that include employment growth 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. Their objective is to test how the deterioration in the value of banks' portfolio of sovereign debt affects their lending behavior and how this feeds into the decisions of their client firms. 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. They do not observe credit flows, but the survey data allow them to recover the linkages of firms with banks.

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 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 these differences 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

[3] Cingano *et al.* (2013) study the effects of banks' exposure to the interbank market on the investment decisions of their clients using data from the Italian credit register, but they also present some 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 the main focus of their study is to analyze to what degree counter-cyclical capital buffers may help to smooth credit supply over the business cycle. These two studies and the ones mentioned in the preceding footnote, except Fernandes and Ferreira (2015), confirm the negative employment effects of impaired access to credit.

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 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.* (2014) 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. 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 assess the importance 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- and long-term funding, which indirectly informs us about the purpose of the loans. Next, we consider three alternative measures 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 (2015) and Fernandes and Ferreira (2015)

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

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.

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). The bailouts took two different forms. In a first stage, two small banks were nationalized: Caja Castilla-La Mancha in March 2009 (resold in November 2009) and CajaSur in May 2010 (resold in July 2010). These two operations entailed public support of 4.6 bn euro, equivalent to 0.44% of Spanish GDP at the time.

In the subsequent operations the Government fostered either bank mergers (involving 26 weak banks) or the takeover of ailing banks by other banks (involving 5 weak banks). The majority of these operations entailed State support, which was channeled through the Fund for the Orderly Restructuring of the Banking Sector (FROB) created in June 2009 (Banco de España, 2014). The mergers and takeovers started in March 2010 and by the end of that year the FROB had provided assistance or commitments in the amount of 11.6 bn euro, i.e. about 1.1% of Spanish GDP.

In the rest of our analysis a bank is classified as weak if it was nationalized, it participated in a merger with State funding support or it was insolvent and bought by another bank, with or without State support. Banks that received funds to absorb other banks with solvency problems are considered to be healthy rather than weak. Except for the two small nationalized banks, until the end of 2010 all weak banks were run by their incumbent managers rather than by government-appointed administrators. Moreover, due to the influence of the regional governments, all the mergers that took place in 2010 used a so-called Institutional Protection Scheme (or SIP). Under this contractual agreement all participating institutions 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 in the process all remaining savings banks were forced to convert into commercial banks as part of the agreement between Spain and the European Financial Stability Facility that provided financial assistance for the recapitalization of the banking sector. These latter operations fall outside the scope of our analysis, although they seem to have induced a further tightening of credit conditions at weak banks (see below).

Why did savings banks have to be bailed out? They were subject to the same prudential regulation and supervision by the Bank of Spain as commercial banks, but 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 the restructuring process and may have affected the credit allocation prior to the crisis.[5]

3.2 The differences in lender health

Table 1 illustrates the differences in lender health at the onset of the crisis. In 2006, weak banks were on average larger than healthy banks and they held less capital and liquid assets. By contrast, both the rate of return on assets and the share of non-performing loans are comparable across the two sets of banks, but this apparent similarity hides latent losses at weak banks, which surfaced in later years, as witnessed by the vastly larger ratio of non-performing loans of weak banks in 2012.[6] 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 ratio of securitized loans to assets is also larger for weak banks, but not significantly so, suggesting that this was not a key difference helping to explain the differential evolution of credit during the crisis at the

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

[6] Data are noisier before 2012, when the authorities carried out stringent stress tests on banks, supervised by the ECB, the European Commission, and the International Monetary Fund.

two bank groups.

The split between weak and healthy banks allows us to analyze the compound effect of the above-mentioned differences in lender health on credit supply during the crisis, 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, but we will also present results where we use the share of loans to the REI as a weak-bank indicator.

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– while the fall in the slump is also more pronounced at weak banks –46% vs. 35% from 2007 to 2010–. This differential evolution stems from changes at both the intensive and extensive margins. The latter is portrayed in Figure 2, which plots acceptance rates for loan applications by potential clients (henceforth non-client firms).[7] During the period 2002-2004 the acceptance rates were higher for weak than for healthy banks, they then became similar, and in 2007-2008 both rates fell precipitously, though at the end of the period they were lower at weak banks. This strong drop in acceptance rates is a reflection of the difficulties faced by Spanish firms trying to switch to a new lender during the crisis.

Lastly, Figure 3 depicts the average interest rates charged by the two sets of banks alongside the ECB policy rate. It suggests that interest rates were scarcely used by weak banks to ration credit demand during our sample period.[8] Indeed, the interest rates charged by both sets of banks closely follow the ECB policy rate and even after the freezing of wholesale markets in late 2008 the difference between them was always below 30 basis points until the end of our sample period in December 2010. We can therefore safely focus on the differential evolution of credit volumes at the two sets of

[7] See a description of our loan application data set in Section 4.

[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.

banks during the crisis.

Finally, inspection of the figures shows that the consolidation operations and nationalizations during the period 2011-2012 did not restore the credit flow to weak banks. The gaps between bank types regarding new credit flows and acceptance rates continue to grow during this period 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 firm-bank 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 firms in the non-financial sector above 6,000 euros (around 7,900 dollars at the end of 2006). Given the low threshold, these data can be taken as a census. The CIR provides the identity of the parties involved in a loan, the share collateralized loans by firm, its maturity structure, the identity of its main bank –namely 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 interest rates –though as noted above this is not a serious limitation– nor the purpose of the loan. We therefore have to rely on the information about the maturity of loans and the distinction between credit lines and loans to establish a potential link between bank lending and firms' hiring or their investment in capital.[9]

[9] As already 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 going backwards, so that 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 bank A and bank

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 entering firms.

We gather economic and financial information for more than 300,000 private, non-financial firms from the annual balance sheets and income statements that Spanish corporations must 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, in which 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 the vast majority of the firms we only observe an abridged balance sheet with no breakdown of the liability structure. Lastly, we observe the firm's industry and use a two-digit breakdown into 80 industries.

To disentangle job losses in surviving firms from those due to firms closing down, we use the Central Business Register (DIRCE), which allows us to make sure that firms that are in the sample in 2006 but 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 in research for the first time.

B, while keeping track of the weak identity of the banks if that is the case. Nevertheless, 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".

[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.

4.2 The treatment and control groups

In order to analyze the employment effects of the relatively strong 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 (henceforth weak-bank attachment).

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 weak-bank attachment, since the threshold is set at the lower end of the distribution.[12] We will nevertheless show the robustness of our findings by presenting estimates for the continuous weak-bank loan-to-asset ratio measure and for different cut-off levels for our weak bank dummy.

Given the size of our data set, we can adopt stringent sample selection rules. To avoid the problem of reverse causality –so that firms' troubles drive banks' problems rather than the other way around– we exclude firms in the REI or in two-digit industries selling at least 20% of their value added to the REI in 2000 (see Appendix 2). This early date is chosen to minimize potential endogeneity through credit decisions taken in the boom years.[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, firms are only classified as having closed down if they are missing in both registers. Moreover, since we are interested in bank credit, we exclude firms with no loans in 2006. This leaves us with

[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.

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

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 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 in our sample are very small. Indeed, 98.7% of them are SMEs according to the European Commission definition (with less than 250 employees and with 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 drop is very close to the aggregate reduction in employment for the industries we cover.

Table 2 provides 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 those 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 is equal to 22.8%. Compared with the control group, firms in the treatment group are on average younger and smaller, they have more temporary workers, and they are as likely to be exporters. In later sections we will use these variables to control for differences in firm level productivity.

The data also reflect the worse financial profile of firms in the treatment group: they are less profitable, hold less capital and liquidity, and are more indebted to banks, although the average maturity of their loans is higher. In addition, they work with more banks and over 2002-2006 they defaulted more often on their loans. They also applied for loans more often and, perhaps surprisingly, had a higher acceptance rate.

The differences in these real and financial characteristics are statistically significant. This implies that we must thoroughly control for firm-level characteristics in our empirical analysis, since weak banks were more likely to grant loans to less profitable and potentially more vulnerable firms than healthy banks. We will use the 17 variables listed below the employment level in the table as controls in our firm-level credit and employment estimation.

Before presenting our results we need to deal with the potential objection that our treatment is defined in terms of an outcome, bank 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 unforeseen. 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 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 problems of weak banks. In this part we first estimate a credit equation at the firm-bank level and then at the firm level. Once we establish that there is a credit supply shock, the second part deals with estimating the effects of the attachment to weak banks on employment.

[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.

5.1 Identification of the credit supply shock

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

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

where Δ_τ is a τ-year difference with respect to our reference 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 a weak bank, Z_{ib} is a vector of firm-bank controls that includes the length of their relationship and a control 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) have become the standard procedure to identify credit supply shocks. The firm fixed effects absorb any differences in observable and unobservable firm characteristics. As a result, they provide a perfect control for potentially confounding demand effects, 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 within-firm specification 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 (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 through the introduction of a rich set of firm controls and industry times municipality dummies to control for demand effects. Indeed, the result of a Hausman test implies that the treatment effect captures changes in the supply side.

Even if we confirm the presence of a credit shock at the firm-bank 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:

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

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

where WB_i is our treatment dummy, 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

Once we ascertain the existence of a credit supply shock at the firm level, we proceed to estimate its impact on employment. The descriptive statistics revealed substantial differences between firms in the treatment and control groups. To ensure that our estimates do not capture the effect of those differences rather than the effect of a credit supply shock, we adopt a difference-in-differences (DD) specification in growth rates that has the same structure as (2) (Wooldridge, 2010, secc. 10.6):

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

where n_{ij} is employment in firm i in industry-municipality cell j and u_{ij} is a random shock. Once again all regressors are measured in 2006. This estimate is an average treatment effect on the treated (ATT).

Estimating in differences implies that we allow for an aggregate trend and for differential trends by industry-municipality cells and firm characteristics. 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; nevertheless in an extension we will also study how weak-bank attachment affects the probability of a firm closure.

The above DD specification forms the basis of our analysis of the real effects of weak-bank attachment. But to check whether credit is the key channel underlying the weak-bank effect, we also estimate the following IV model for the change in employment:

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

in which WB_i acts as an instrument for access to credit and the first stage coincides with (2) As before, 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 the equivalent of parameter β in equation (3).

The exclusion restriction is that working with a weak bank alters employment growth only through the credit channel. Recall that, due to data availability reasons, we have assumed that all the effect of weak-bank attachment goes through the amount of credit granted. Though the difference between the average interest rate charged by weak and healthy banks is small, it is non-zero and higher for weak banks. The existence of an interest rate response, albeit a small one, would contradict the exclusion restriction for bank health affecting firms only through the quantity of credit received, and as a result our second stage coefficient would provide an upper-limit.

5.3 Threats to identification

Our baseline specification for job losses includes exhaustive controls for differences in observable firm characteristics and is more demanding than standard difference-in-differences specifications in levels. But an unbiased estimation of the causal impact of weak-bank attachment on employment has to rely on the unconfoundedness assumption, which requires that the assignment of firms to the treatment and control groups is completely random conditional on the controls for observables. Moreover, unlike the case of credit, we cannot perfectly control for confounding factors through the introduction of fixed effects. This raises several potential concerns.

First, as is usual in this type of analysis, there are demand effects at play that 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. The standard practice of analyzing employment changes within regions or provinces may be too coarse to credibly control for these 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.[16]

The other main threat to identification is the non-random assignment of firms to banks prior to the crisis. Aggregate shocks may differentially affect firms depending on their profitability, product quality or financial vulnerability. If during the crisis product demand, say, fell more for low-quality than for high-quality firms, and these firms are over-represented among the clients of weak banks –as is suggested by our descriptive statistics–, then the DD estimate 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 the problem is that selection may take place on both observables and 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 possibly need more resources to pay back outstanding loans and other liabilities. Given the importance of the selection effects we devote a separate section to this issue.

As a first step, we analyze the sensitivity of our results to the inclusion of observable controls so as to derive an upper bound for the possible bias arising from unobservables, following Altonji *et al.* (2008) and Oster (2015). But we also address the root of the problem by estimating three alternative specifications. The use of matching techniques helps us reduce the degree of heterogeneity between treated and control firms.[17] Next, we estimate a panel fixed effects model to rule out that the differential evolution of employment is driven by unobservable characteristics. Lastly, we also exploit a legal restriction on the location decision of weak banks that was suppressed in 1998 to generate exogenous variation in the propensity of firms' weak-bank attachment based on the pre-reform density of weak-bank branches at the municipal level. The remainder

[16] In Section 7 we undertake an alternative check by focusing on tradable goods.

[17] The use of a continuous treatment effect has the same effect, while the use of higher thresholds for our weak-bank dummy produces mixed effects. It raises the proportion of weak-bank clients in the control group, but it also raises the weak-bank loan-to-asset ratio for the treated firms and this tends to raise our estimate of the treatment effect.

of the paper presents results for specifications that allow for treatment heterogeneity and that explore several margins of adjustment. The latter includes an analysis of the impact of credit constraints on firm closure.

6 The differential evolution of credit

We start by validating our claim that the differential evolution of the volume of lending by the two sets of banks reflects a credit supply shock. In a first step we perform a local analysis at the firm-bank level. This exercise demonstrates that weak banks reduced credit more than healthy banks, not just in the aggregate but also at the level of individual firm-bank relationships. Next, we provide estimates at the firm level, which establish that affected firms were unable to fully offset the reduction in credit from weak banks with additional loans from healthy banks.

6.1 Local analysis

We begin by reporting the results for our baseline specification (1) and alternative specifications for the change in credit between 2006 and 2010 in Table 3. 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. Restricting the sample to firms with multiple banking relationships raises the estimate to 25.6 pp, which is virtually identical to the estimate for our baseline specification with firm fixed effects, 25.5 pp, estimated for the same sample of multi-bank firms. The similarity between these two estimates suggests that unobservables do not play a significant role in access to credit, despite the substantial differences in 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. The latter indicates that the weak bank dummy variable captures changes in credit supply. Finally, estimating the same three specifications on the entire sample of firms present in the CIR delivers similar but somewhat smaller estimates.[18]

[18] The corresponding estimates are, respectively, 20.4 pp, 23.8 pp and 22.1 pp. The difference between the pooled OLS estimations and the within-firm estimation is somewhat larger than for our baseline

These results show that weak bank exposure leads to considerable credit supply restrictions at the firm-bank level. The estimated reduction is in the range going from 20 to 25 pp, depending on the sample, the number of banking relationships, and the procedure used to control for selection. The measured effect is stronger in our baseline sample than in the entire sample of firms (25.5 vs. 22.1 pp) and also for multi- than for single-bank firms (25.6 vs. 23.2).

Before moving on to the firm-level analysis, it is interesting to check whether credit rationing by weak banks was stronger on short- or on long-term funding. For this purpose we interact WB_b with two indicators: one for firms that enjoyed a credit line in 2006 and another one for firms that had loans of maturity beyond one year with the bank. The results in Table 3 indicate that weak banks reduced credit to firms with credit lines by 7.8 pp and increased credit to firms with loans above one year by 9.4 pp relative to healthy banks. We will return to this issue when we discuss the channels of employment adjustment in Section 10.

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

Our results indicate that the standard indicators of bank health do not capture the full deterioration of the assets of the savings banks, since the weak bank dummy captures a differential effect beyond differences in bank health due to lower capital buffers, lower returns, fewer liquid assets, and more securitization. A logical candidate explanation relies on differences in banks' pre-crisis exposure to the REI. We measure this exposure by the 2006 share of each bank's loans to firms in the REI and we create a dummy variable that takes the value 1 for banks in the upper quartile of the distribution. The associated coefficient has the expected sign, but the treatment effect is approximately 10

sample because the CIR provides much fewer firm characteristics. As a result, the pooled OLS regressions only includes controls for past defaults and the number of banking relationships. Details are available upon request.

pp smaller and less significant than in our baseline despite the fact that this specification is estimated on the entire sample of firms in the CIR.

Our finding that a dummy based on bailout yields larger credit reductions than a dummy based on REI exposure may be due to several factors. First, weak banks may have suffered from a stigma effect, whereby some savings banks with a low exposure to the REI experienced the same funding problems as more exposed banks. Moreover, during the crisis financial markets may have changed their beliefs regarding the Government's willingness and ability to provide loans to savings banks. Third, latent losses elsewhere in the balance sheets of savings banks may have been sizeable and imperfectly correlated with 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, since such trends would lead to biased estimates. The evidence on pre-crisis trends appears in panel A of Figure 4, which 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.[19]

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 local analysis is replaced by our treatment dummy WB_i.

Table 4 shows that the estimated effect for the entire sample of firms is equal to -5.3 pp, while the corresponding estimate for multi-bank firms is -3.1 pp. These results indicate that treated firms managed to offset a substantial part of the reduction in credit supply by weak banks, because the estimates are much smaller than in the local analysis. Furthermore, while multi-bank firms suffered a stronger credit supply contraction at the

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

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 hit weak banks during the crisis.

In terms of actual magnitudes, 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 local and firm-level estimates may seem surprising, but it should be recalled that firms with a marginal attachment to weak banks are included in the control group. Weak banks may have predominantly severed their relationship with these firms, which would be consistent with our estimates for the firm-level effects being close to the estimated effects at the local level for continuing relationships (Table 3, col. 5). Moreover, it is 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 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.* (2015), who 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 in Italy using firm-level data. The comparison is not straightforward, since they define treatment based on the ratio between a bank's interbank market loans and its asset value. Surprisingly, and contrary to Jiménez *et al.* (2014) and to our case, they find similar coefficient estimates at the local 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 local 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 regarding the real effects 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.

7 Main results

This section presents our main empirical results, first for a baseline specification and then for a set of alternative specifications to ascertain its robustness.

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 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. Next, including firm-level controls for productivity differences –age, age squared, size, rate of return, and temporary employment share– reduces the treatment effect by 0.9 pp. Adding the remaining firm controls, related to their financial health in 2006, brings down the effect by a further 3.9 pp, to -2.8 pp. The latter result illustrates why it is so important to have access to credit register data: failure to control for differences in firms' creditworthiness may cause a substantial upward bias in the estimated effects of credit supply restrictions. Finally, including main bank fixed effects does not alter the results, 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 column (4) as our baseline, bearing in mind that it is conservative as to the impact of credit constraints.[20]

As happened before, our identification relies on the assumption of parallel pre-crisis trends for treated and control firms. To test the validity of this assumption, we ran a placebo test 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.[21] Further evidence for the absence of differential pre-crisis trends is provided in Figure 5. It depicts the estimated coefficients of WB_i and the size of the confidence intervals for the time period 2002-2010. Inspection of this figure shows that the treatment effect is significantly negative from 2008 onwards. Before that time, weak-bank attachment does not produce significant differences in the evolution of 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, as shown in Figure 4B, but these effects are estimated with less precision. The latter helps to explain why the treatment effect at the firm level does not 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– firms' expectations about access to credit changed dramatically and firms in the treatment group may have rationally anticipated a further tightening of the 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 periodic 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

[20] An alternative specification to capture zeros in the dependent variable is a Tobit model with municipality random effects, but their large number leads to non-convergence. A Tobit with province dummies yields an estimate of -1.9 pp (s.e. 0.4 pp).

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

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 not launched by the Bank of Spain until 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 represent the majority in our sample–, report that funding was obtained only in part or from credit institutions other than their usual ones, and 30% report that it was not possible for them to obtain bank credit. Of 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

The purpose of this section is to show that the differential evolution of employment is indeed driven by differential access to credit for firms in the treatment and control groups. To this aim we estimate the IV model described in Section 5. In the first stage weak-bank attachment is used as an instrument for credit growth and the second stage provides the pass-through of credit to employment. Table 6 presents the results.

As discussed in Section 5.3, the first stage of our IV coincides with our estimation of credit rationing at the firm level. For the entire sample of firms this delivered 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 to be 0.519. 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 obtained 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 case of the full sample, 0.797, yielding an overall impact of -2.5 pp. The coefficient estimates are highly significant and the F-statistics confirm the absence of a weak instrument problem.

In comparison, Cingano *et al.* (2015), 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 the one estimated in that paper.

7.3 Alternative specifications

In this section we perform a wide range of specification tests. Our aim is to show the robustness of the findings from our baseline DD specification. We start by considering the sensitivity of our results 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. In the next two columns 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.[22] 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. Thus, 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.[23] An alternative approach to check robustness is to redefine the dependent variable using a measure that allows us to account for both exit and entry, which was used 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.

Our next objective is to consider alternative procedures to control for local demand

[22] The estimated treatment effect when all firms with loans from weak banks are assigned to the treatment group is -1.9 pp.

[23] Using $\Delta_4 \log n_{ij}$ the effect is -2.0 pp, s.e. 0.5 pp.

effects. Mian and Sufi (2014) argue that local demand effects should only affect output in non-traded goods sectors, while 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 prefer the latter, since more concentrated industries are likely to be more traded and hence less dependent on local demand conditions.[24] We follow these authors in computing the Herfindahl concentration index for 3-digit industries and 50 provinces, and labeling as tradable those goods in the highest quartile. This sample selection yields an effect on employment of -5.8 pp, which 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, be more sensitive to changes in credit supply, 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 alternative definition the measured effect is equal to -3.0 pp, which is not statistically different from our baseline. 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, which suggests that the slowdown in 2007 altered the mix of employment at financially vulnerable and resilient firms in the treatment and control groups, though once again this estimate is not statistically different from the baseline. Lastly, we measure weak-bank attachment and all other variables in 2002. 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 it is significantly smaller than for the 2006 sample. Indeed, the measured effect is larger (-2.4 pp) when we measure firm characteristics in 2006 rather than 2002.

[24] As found by Mian and Sufi (2014) for the US and by Ramos and Moral-Benito (2013) for Spain.

8 Selection

Our baseline specification includes an exhaustive set of controls for observable firm characteristics, but this does not completely rule out the possibility of selection effects. Our list of firm controls may still be incomplete and our estimation strategy does not rule out selection on unobservables.

We can informally check the sensitivity of our estimates to the inclusion of observable controls, so as to derive bounds for the possible bias arising from unobservable variables, as in Oster (2015). 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 an estimated coefficient that is bias-adjusted, making the heuristic assumption that $R_{\max} = 1.3\widetilde{R}$, where $\widetilde{R}$ is the fully-controlled R^2 and $R_{\max}$ is the maximum 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 perform three further tests to corroborate our claim that the differential evolution of firm-level employment is not driven by selection. In a first extension we estimate a panel fixed effects model in order to rule out selection on unobservables. Next, we reestimate our DD specification using matching techniques to improve the precision of our estimates and to correct for a potentially insufficient overlap of the characteristics of firms between the treatment and control groups. Lastly, we use an instrument that exploits the 1988 liberalization of the location decisions of savings banks and that generates exogenous variation in the exposure to weak banks.

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 should serve to show 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 stressed the considerable 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. This yields 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 treatment effects with propensity score and exact matching are, respectively, -3.2 pp and -2.6 pp.[25] 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. 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 lie within the confidence intervals of our benchmark.

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). Our data allow us to calculate for each municipality the share of bank branches at the start of December 1998 –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 1998. It should be noted 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

[25] 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.

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 exclusion restriction is that local weak-bank density only affects a firm's employment through its attachment to weak banks, which cannot be tested. Moreover, we can only imperfectly control for local demand effects because our IV varies by municipality and so we cannot include trends at that level. Instead, we 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.[26] 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 except that they are significantly smaller in both assets and employment. Thus, while the balancing is imperfect, our heterogeneity analysis in the next section shows that firm size hardly affects job losses due to credit constraints, whereas financial ratios are found to matter.

The estimates in Table 10 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 have to be interpreted with caution 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 in credit market conditions. To find out if these features alter the real impact of credit constraints, we estimate a triple difference (DDD) model, again estimated in 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

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

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.[27,28]

The estimation results appear in Table 11. 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. Finally, 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 the remaining coefficients, larger firms suffer slightly lower losses and this marginal effect is the same for firms in the treatment and control groups. 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 in levels and in the interaction 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 –this is true 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 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

[27] 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.

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

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 overall effect

In our last set of exercises we wish to probe deeper into the margins of adjustment available to firms. Our analysis in the previous section served to quantify the additional job losses due to the credit-rationing by weak banks, but it is also relevant to know what types of jobs are most at risk when firms face credit constraints and to what degree firms have explored alternative margins of adjustment, like changes in wages, to alleviate the impact on employment. Lastly, in our final exercise 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 suggest that firms with a relatively large share of temporary workers in 2006 ended up shedding more labor than similar firms with a lower share. 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 the firm level.[29] The breakdown of employment by type of contract is observed for 91% of the surviving firms in our sample. The first column of Table 12 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 firms in the treatment group. 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 at this sample of treated firms. In other words, temporary jobs made up around one-quarter of pre-crisis employment but they accounted for 56% of employment adjustment in treated firms.

The over-representation of temporary workers among dismissed employees is also

[29] 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.

observed for aggregate job losses in Spain (Bentolila *et al.*, 2012) and is commonly attributed to the much lower termination 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, while the opposite is true for loans with maturity above one year. It therefore makes sense for 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. The question we would like to address 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, we have to conduct the analysis on the basis of the total wage bill of surviving firms.

Our estimate indicates that weak-bank attachment is associated with a 1.6 pp drop in the wage bill of the treated firms (Table 12, col. 2). 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.

10.3 Probability of firm exit

Employment may not recover at the same speed if a large fraction of firms close down than if they downsize. 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, a significantly lower figure than for the full sample. The extensive margin is explored 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 13, 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, which implies that *ceteris paribus*, compared to a firm with a ratio of weak-bank debt to assets at the first decile –which is roughly nil–, 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, which amounts to 14.5% of the baseline exit rate.

Are our estimates large or small? We should start by clarifying that these microeconomic estimates cannot be directly extrapolated to the aggregate economy. In general equilibrium there should be further 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.[30] 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.

Using our estimates we can compute separately employment losses at surviving and closing firms. On the one hand, we can use the estimate for survivors quoted above. On the other hand, we calculate the number of firm closures from the estimated probability of exit in Table 13 (col. 1) and the employment drops 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

[30] 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: $(\Delta_4 \widehat{n}_{ijt} \mid WB_i = 1) - (\Delta_4 \widehat{n}_{ijt} \mid WB_i = 0) = (1+n_{ijt-4})[\exp(\widehat{\alpha} + \widehat{\beta} + X_i\widehat{\gamma} + \widehat{\delta_j}) - \exp(\widehat{\alpha} + X_i\widehat{\gamma} + \widehat{\delta_j})]$.

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.

To get a sense of the size of the effects estimated in this paper, while keeping in mind the caveat above, we can try to relate the findings to the overall drop in employment in the Spanish economy. In 2006 our sample of firms accounted for 42% of employees in the industries we include in our analysis (which themselves represented 69% of all private sector employees). We can then assume that our sample is representative in such industries –but not of other sectors– and blow up job losses in our sample to the total of those industries and apply the corresponding share of losses due to credit constraints induced by weak-bank attachment, using our baseline estimate of 2.8 pp. The figure computed in this way amounts to 52,554 jobs lost due to credit constraints, which represent 4.9% of the total salaried jobs lost in the private sector from 2006 to 2010. The latter figure jumps to 8.2% using the separate estimates for surviving and closing down 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 during the Great Recession. We achieve identification by exploiting differences in lender health at 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 more challenging than is typical, since the bank solvency problems are linked to corporate loan portfolios.

We are not the first to study the link between external funding and employment outcomes, but 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 of 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. At the level of the average firm the additional job losses due to weak-bank attachment are around 2.8 percentage points. This estimate implies that around 24% of the total fall in employment among exposed firms in our sample is accounted for by weak bank exposure.

The extraordinary strength of the credit crunch in Spain is illustrated by the fact that we even find sizable effects 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 were borne by temporary employees, with little adjustment in wages. Separate estimates for employment losses at surviving and closing firms indicate that for the former 52% of job losses at exposed firms are explained by weak-bank attachment, while 34% of losses originated by firm closures are. Our paper is the first to offer this type of decomposition, which carries interesting implications for the speed of recovery after slumps.

We can also make a final statement regarding efficiency. Conditional on the validity of our quasi-experimental approach, the assignment of firms to weak banks, as opposed to healthy banks, is as good as random. Then, given our controls, had these firms not been attached to weak banks, they would have been granted more credit than they did. In this sense, while some part of job losses suffered by firms attached to weak banks was probably efficient, the estimated employment effects of the credit constraints we identify, once selection has been taken into account, were inefficient.

A Appendix 1. The Spanish banking system restructuring process and returns on securitizations

Table A1. Spanish savings banks' restructuring process

	December 2006	Dec. 2007	Dec. 2008	Dec.2009	December 2010	December 2011	June 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		
23	Caja S. Fernando	Cajasol					
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	G. Caja España-Duero	Banco CEISS
30	Caja Duero						
31	Caixa Manlleu				Unnim		Unnim Banc
32	Caixa Sabadell						
33	Caixa Terrassa						

Notes. The first column lists the 33 weak banks in 2006 that are the basis for our analysis. Shaded areas correspond to weak banks in 2010 and later. SIP refers to an Institutional Protection System, a contractual agreement between separate legal entities, depicted with boxes (see Section 3).

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.

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 (firm's 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
Variable	Mean	St. Dev.	Mean	St. Dev.	t test
ln(Total Assets)	13.7	2.1	16.4	1.0	7.1
Own Funds/Total Assets	8.4	9.0	5.2	1.2	-2.0
Liquidity/Total Assets	23.7	22.4	11.5	4.5	-3.1
Return on Assets	1.0	1.7	0.9	0.3	-0.5
Non-Performing Loan Ratio	1.5	6.3	0.7	0.6	-0.7
Non-Performing Loan Ratio (2012)	8.6	12.7	22.0	6.0	3.5
Loans to REI/Total Loans to NFF	36.8	22.3	67.9	8.1	7.9
Securitized Loans/Total Assets	14.9	10.5	18.5	6.3	1.6

Notes. There are 206 healthy and 33 weak banks. Non-performing Loan Ratio as a ratio of the value of loans. Securitized Loans/Total Assets for banks that securitize. NFF denotes non-financial firms. Except for the ln(Total Assets), variables are ratios in percentages. The last column shows the t ratio of the test for the difference of the means. 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
Variable	Mean	St. Dev.	P25	P50	P75	Mean	St. Dev.	P25	P50	P75	t test
Loans with WB/Assets	0.3	0.9	0.0	0.0	0.0	22.8	17.1	9.7	17.3	30.9	–
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	–
Employment (employees)	25.3	365.6	2.0	6.0	13.0	20.3	207.2	2.0	5.0	13.0	-2.7
Temporary Employment	20.4	25.4	0.0	11.1	33.3	22.7	26.0	0.0	14.5	36.6	16.0
Age (years)	12.6	9.8	6.0	11.0	17.0	11.7	8.7	6.0	10.0	16.0	-17.1
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
Exporter	13.0	33.7	0.0	0.0	0.0	13.1	33.7	0.0	0.0	0.0	0.3
Own Funds/Total Assets	34.4	23.8	14.2	30.3	51.1	24.9	18.5	10.0	20.8	35.8	-74.3
Liquidity/Total Assets	12.4	15.1	1.9	7.0	17.4	8.6	11.8	1.1	4.2	11.2	-47.3
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
Bank Debt	30.7	26.7	6.8	24.8	49.8	48.5	23.5	29.4	47.4	66.2	120.8
Short-Term Bank Debt (up to 1 year)	48.8	41.5	0.0	46.7	100.0	45.7	37.1	4.3	44.9	80.7	-13.2
Long-Term Bank Debt (above 5 years)	21.5	35.3	0.0	0.0	37.1	29.5	36.3	0.0	7.4	59.0	39.7
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
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
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
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
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
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
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

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 share of loans with weak banks is with respect to bank credit. The last column shows the t ratio on the test for the difference of the means. 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**	-0.142*
	(0.088)	(0.094)	(0.008)		(0.034)	(0.078)
I(Credit Line$_{ib}$)				0.093***		
				(0.018)		
I(Credit Line$_{ib}$) $\times WB_b$				-0.078**		
				(0.038)		
I(Maturity$_{ib}$ >1 year)				-0.042*		
				(0.021)		
I(Maturity$_{ib}$ >1 year) $\times WB_b$				0.094***		
				(0.033)		
Firm fixed effects	no	no	yes	no	yes	yes
Firm controls	yes	yes	–	–	–	–
Bank fixed effects	no	no	no	yes	yes	no
Bank controls	yes	yes	yes	no	yes	yes
Firm-bank controls	yes	yes	yes	yes	yes	yes
Ind.×Prov. f.e.	yes	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	0.408	0.394	0.446
No. obs.	304,089	236,691	236,691	236,691	126,863	595,079
No. firms	139,685	72,287	72,287	72,287	42,630	195,146

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 for the list and Appendix 2 for definitions. "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)
	All firms	Multi-bank
WB_i	-0.053***	-0.031***
	(0.015)	(0.011)
Firm controls	yes	yes
Industry×Municipality fixed effects	yes	yes
Multiple banking relationships	no	yes
Balance-sheet data	yes	yes
R^2	0.215	0.246
No. obs.	149,458	74,045

Notes. OLS estimates for 2010. Control variables: see Table 2 for the list and Appendix 2 for definitions. "yes/no/–" indicates whether the corresponding set of variables is either included, not included or redundant. 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)
				Baseline		Placebo
WB_i	-0.074***	-0.076***	-0.067***	-0.028***	-0.028**	0.006
	(0.013)	(0.010)	(0.009)	(0.006)	(0.013)	(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	0.163	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 for the list and Appendix 2 for definitions. "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 for the list and Appendix 2 for definitions. "yes/no/–" indicates whether the corresponding set of variables is either included, not included or redundant. Robust standard errors corrected for multi-clustering 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	0.182	0.177	0.177	0.130	0.188
No. obs.	149,458	149,458	149,458	133,122	129,246	16,199	149,458	145,322	71,703

Notes. OLS estimates for 2010. Control variables: see Table 2 for the list and Appendix 2 for definitions. "yes/no/–" indicates whether the corresponding set of variables is either included, not included or redundant. 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: $\Delta_4 \log(1+n_{ijt})$	
$WB_i \times 2008$	-0.012***
	(0.004)
$WB_i \times 2009$	-0.020***
	(0.004)
$WB_i \times 2010$	-0.027***
	(0.006)
Firm controls	yes
Firm fixed effects	yes
Industry×Municipality×Year fixed effects	yes
R^2	0.789
No. obs.	653,189

Notes. OLS estimates for 2007-2010. Control variables: see Table 2 for the list and Appendix 2 for definitions. "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:* $p<0.10$, ** $p<0.05$, *** $p<0.01$.

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***	-0.026***		
	(0.009)	(0.006)		
WB_i *Intensity*			-0.065***	-0.052***
			(0.024)	(0.021)
Firm controls	yes	yes	yes	yes
Municipality×Industry fixed effects	yes	yes	yes	yes
Overall effect	–	–	-0.015	-0.012
R^2	0.228	0.179	0.228	0.245
No. obs.	43,587	133,816	67,207	133,816

Notes. Weighted least squares estimates for 2010. Control variables: see Table 2 for the list and Appendix 2 for definitions. "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: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 10. 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 for the list and Appendix 2 for definitions. "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 11. 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)
$WB_i\times$ Rejected application$_i$	-0.029**	(0.012)
Past Defaults$_i$	-0.209***	(0.029)
$WB_i\times$ Past Defaults$_i$	-0.041**	(0.020)
Short-term debt$_i$	-0.089***	(0.013)
$WB_i\times$ Short-term debt$_i$	-0.036***	(0.011)
Bank debt$_i$	-0.096***	(0.017)
$WB_i\times$ Bank debt$_i$	-0.081***	(0.022)
Own funds ratio$_i$	0.061***	(0.026)
$WB_i\times$ Own funds ratio$_i$	0.134***	(0.027)
Liquidity ratio$_i$	0.118***	(0.022)
$WB_i\times$ Liquidity ratio$_i$	0.050	(0.061)
Single bank$_i$	0.012**	(0.005)
$WB_i\times$ Single bank$_i$	0.019	(0.015)
log(Total Assets$_i$)	0.009*	(0.005)
$WB_i\times$ log(Total Assets$_i$)	0.003	(0.005)
log(1+Age$_i$)	-0.054***	(0.006)
$WB_i\times$ log(1+Age$_i$)	0.027***	(0.008)
I(Exporter$_i$)	0.176***	(0.011)
$WB_i\times I$(Exporter$_i$)	0.062***	(0.020)
I(Temporary employees$_i$)	-0.112***	(0.010)
$WB_i\times I$(Temporary employees$_i$)	-0.027***	(0.014)
$WB_i\times I$(Services$_i$)	0.019	(0.017)
Industry×Municipality fixed effects		yes
Firm controls		yes
R^2		0.176
No. obs.		149,458

Notes. OLS estimates for 2007-2010. Control variables: see Table 2 for the list and Appendix 2 for definitions. "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.

Table 12. 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 for the list and Appendix 2 for definitions. “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.

Table 13. 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 for the list and Appendix 2 for definitions. "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: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

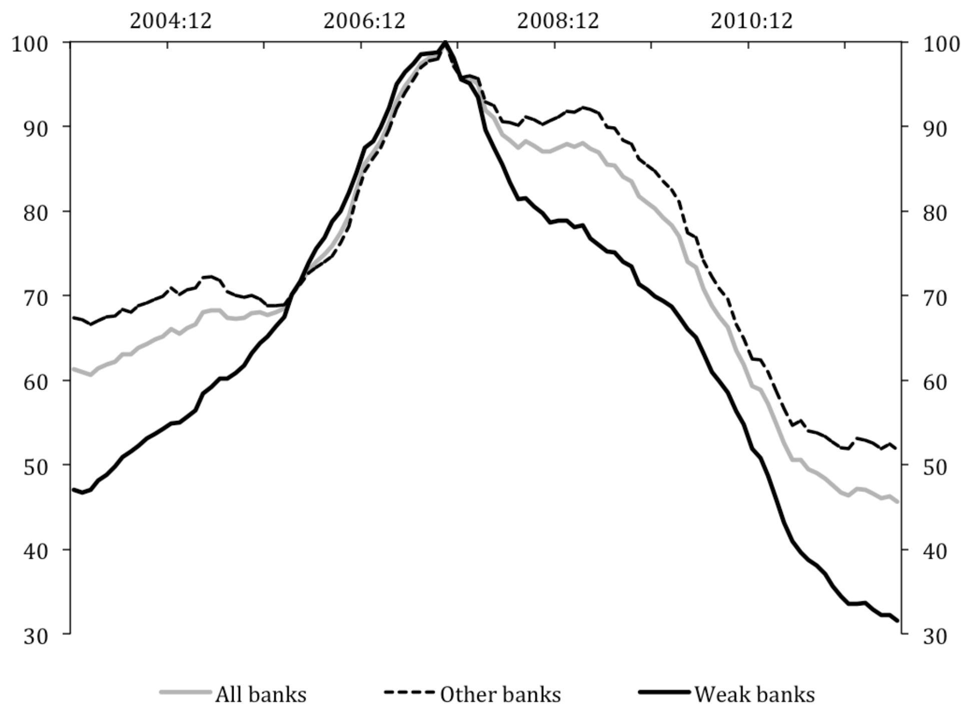

Figure 1: New credit to non-financial firms by bank type (12-month backward moving average, 2007:10=100)

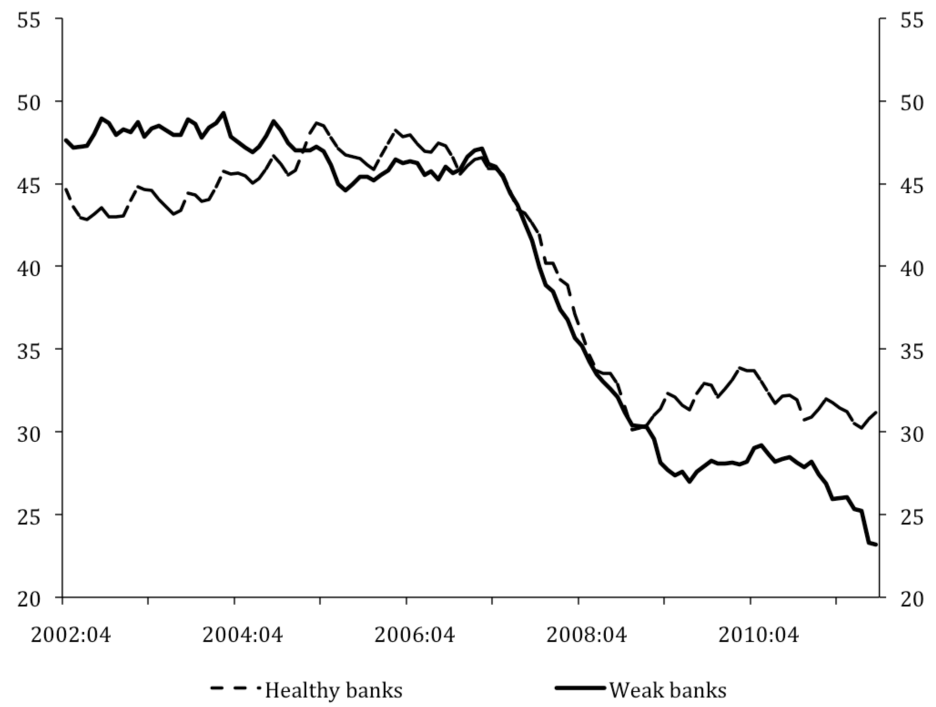

Figure 2: Acceptance rates of loan applications by non-current clients, by bank type, 2002:4-2012:6 (%)

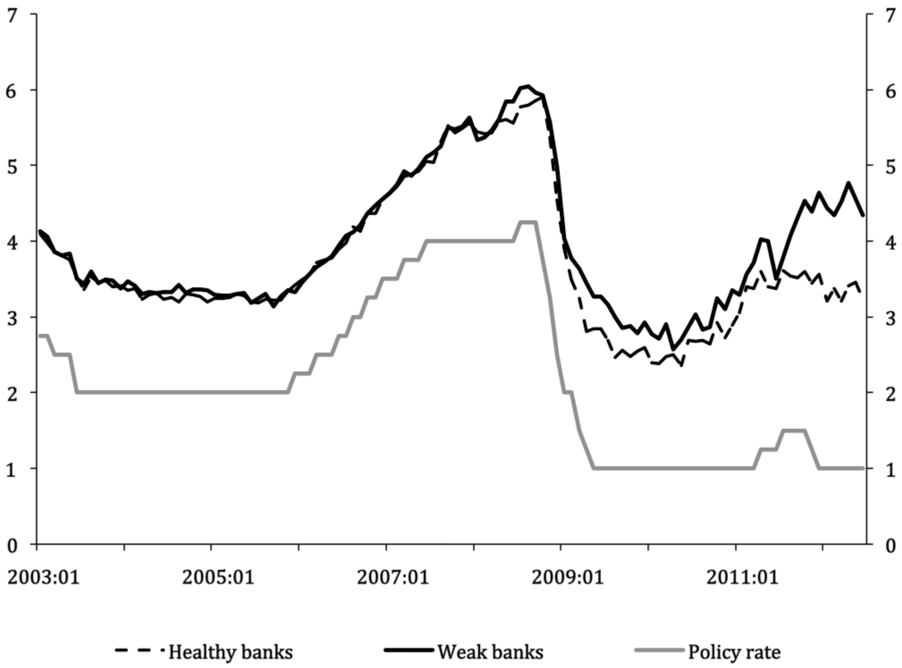

Figure 3: Average annual interest rae for new loans to non-financial firms by bank type and policy rate, 2003:1-2012:6 (%)

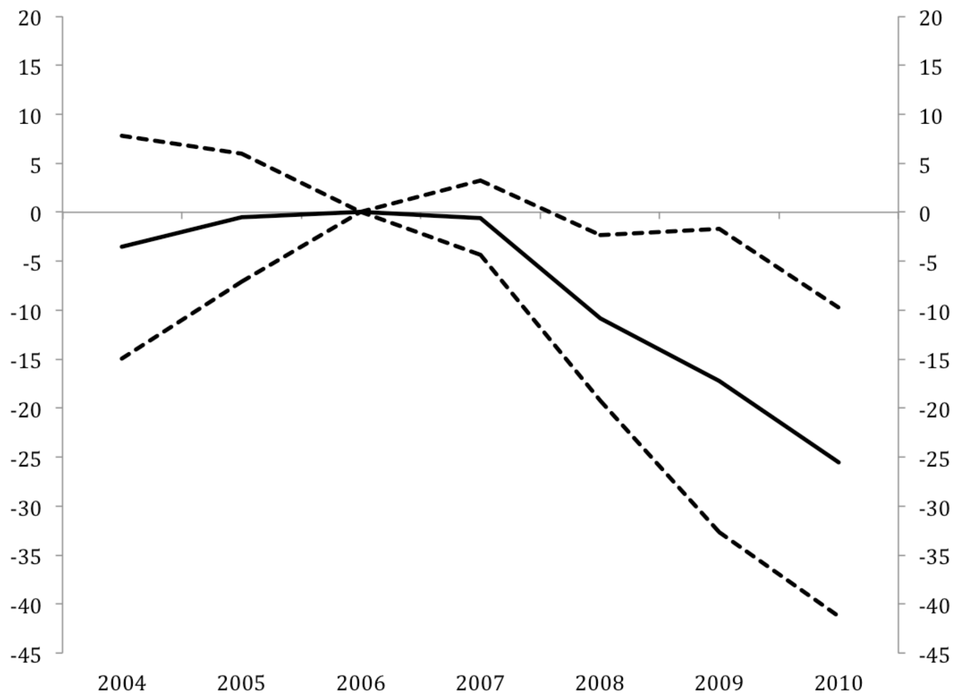

Figure 4a. Effect of weak-bank attachment on credit at the local level, 2002-2010 (pp)

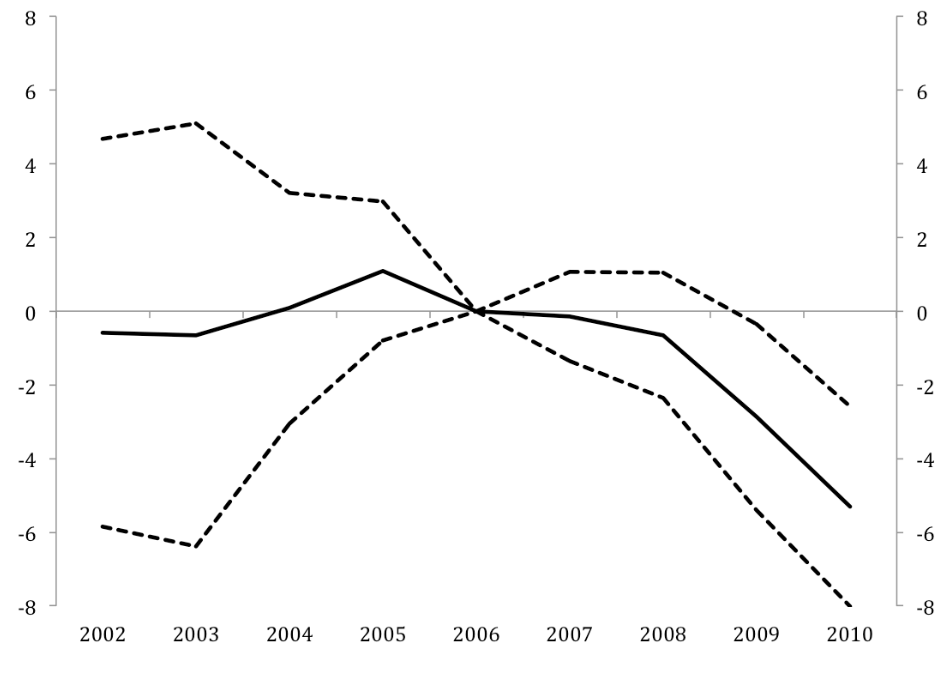

Figure 4b. Effect of weak-bank attachment at the firm level, 2002-2010 (pp)

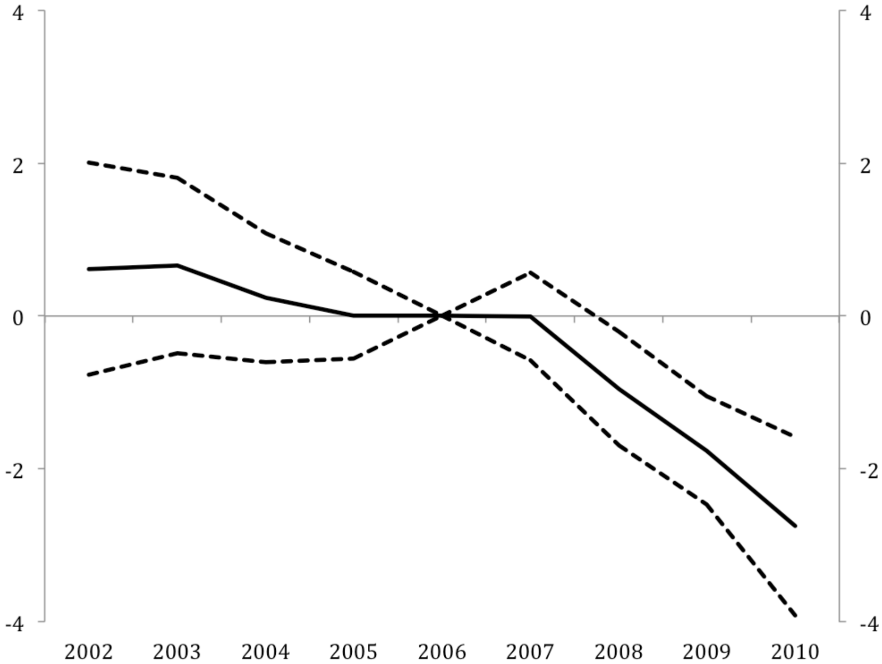

Figure 5. Employment effect of weak-bank attachment, 2002-2010 (pp)