WHEN CREDIT DRIES UP: JOB LOSSES IN THE GREAT RECESSION

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CEMFI Working Paper No. 1310

April 2015

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CEMFI Working Paper 1310
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Abstract

This paper studies whether the solvency problems of Spain's weakest Banks during the Great Recession had 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-fifth of losses due to exposed-firm exits.

JEL Codes: D92, G33, J23.
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 employment losses? Both questions have strongly resurfaced in the wake of the recent economic and financial crisis. In the US the Great Recession induced very large job losses, while in the European Union the experience was more varied but most countries did suffer significant employment declines, especially the peripheral ones. In fact, in both areas the employment rates in 2014 were still below those attained in 2008. Several potential culprits for the protracted recovery have been singled out in the US, such as lack of aggregate demand and higher uncertainty, while fiscal austerity and credit constraints have featured prominently in the European debate.[1]

In this paper we focus on the relationship between the reductions in credit supply and employment during the Great Recession. While the relationship between financial constraints and corporate investment has been extensively studied, comparatively little is known about the role that the availability of external finance plays in determining the employment level of firms. Yet, the literature on financial accelerators suggests that counter-cyclical fluctuations in the cost and availability of external finance may amplify or even generate fluctuations in output and employment.[2]

The theoretical literature has identified several potential transmission mechanisms through which shocks to the banking system might affect employment in the non-financial sector. 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. In addition, 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.[3]

[1] Mian and Sufi (2014) and Baker *et al.* (2013), respectively, for the US, and Giovannini *et al.* (2015) regarding credit constraints in Europe.

[2] See Bernanke and Gertler (1989, 1995), Bernanke *et al.* (1996), and Gertler and Kiyotaki (2010).

[3] See Wasmer and Weil (2004) and Petrosky-Nadeau and Wasmer (2013) on frictions, and Caggese

Our objective in this paper is to estimate the overall strength of these transmission mechanisms during the Great Recession with data from Spain. This country offers an ideal setting to explore the real effects from credit supply shocks, since it suffered a severe banking crisis with devastating effects on the economy. Between 2002 and 2007 employment grew at 4.2% per annum, whereas it fell by as much as 9% between 2007 and 2010. Concurrently, the flow of new credit by deposit institutions to non-financial firms increased in real terms by 23% from 2003 to 2007 and then fell by 38% to 2010. Moreover, the expansionary monetary policy of the European Central Bank (ECB) induced banks to take on substantially more risk (Jiménez *et al.*, 2014). The value of loans to firms in the real estate industry rose from 14.8% of GDP in 2002 to 43% in 2007, and there was a strong rise in mortgage loans during this period. This fueled a housing market bubble, with housing prices rising in real terms by 59% over that period and subsequently falling by 15% up to 2010.

All banks suffered from the collapse of the construction sector and the global economic downturn, but the main problems were concentrated in the savings banks (*cajas de ahorros*). Indeed, the Spanish government had to bail out 33 banks during the crisis, all but one of which were savings banks –and the only commercial bank was controlled by a savings bank. We show that the bailed-out or *weak banks* cut lending more than the rest (*healthy banks*). This stronger credit contraction started well before the first bailouts took place and its effects were still noticeable after Spain recapitalized its banks in 2012 with the help of a loan from the European Financial Stability Facility.

Our strategy is to exploit these large differences in lender health at the onset of the Great Recession to uncover whether the solvency problems of Spain's weakest banks caused a reduction in credit supply and, ultimately, in the employment levels of their client firms. To this aim we compare the evolution of employment at firms with pre-crisis loans from any of the weak banks to the change in employment at comparable firms with pre-crisis loans from healthy banks. Our sample period starts in 2006 and ends in 2010.

There are several reasons why we expect a relatively strong impact. Spanish firms are more reliant on bank credit than their counterparts in most advanced economies.

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and Cuñat (2009) on temporary jobs.

For example, in 2006 the stock of loans from credit institutions to non-financial corporations represented 86% of GDP compared to 62% in the European Union.[4] 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. The theory predicts that these firms are particularly vulnerable to a reduction in credit supply.

We are obviously not the first ones to estimate the impact of credit market disruptions, but a key contribution of our paper is that we have access to a unique data set that includes data from the confidential Central Credit Register of the Bank of Spain, which contains detailed information on virtually all existing and newly-granted loans to non-financial firms. These data allow us to reconstruct the complete banking relationships of almost 170,000 non-financial firms outside the real estate industry working with 239 banks. We also have information on loan demand through data on applications from non-current clients and on whether they are granted. This information is matched to the balance sheets of the banks and firms in our sample. Lastly, we have access to historical data on bank branch location and to the official register of firms –so that we can include firms that close down.

This exceptionally comprehensive matched firm-loan-bank data set allows us to address several challenging identification issues. First of all, we need to disentangle changes in credit supply from concurrent changes in credit demand. Between 2007 and 2009, the 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). Still, when we control for credit demand using the now standard procedure of Khwaja and Mian (2008) for firms that work with more than one bank, we find that weak banks curtailed credit vis-à-vis the 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

[4] Source: European Central Bank (2010), Annex Tables 4 and 14.

reduction in credit supply, which implies that the impact found at the firm-bank level was transmitted to the level of the firm. This result accords well with the theory of relationship lending (Sharpe, 1990), which indicates that stable relationships help to reduce the agency cost of lending, as banks acquire soft information on their clients. However, by the same token, they also make it more difficult for firms to switch to other lenders and more so in recessions (Gobbi and Sette, 2014). Lastly, we provide evidence that the reduction in credit supplied by weak banks was the underlying cause for the comparatively strong reduction in the employment levels of their client firms.

The second main challenge is to control for selection effects. A careful inspection of the data reveals that, on average, healthy banks worked with better firms than weak banks. This issue is crucial as a lack of exhaustive controls for these underlying differences would bias our estimates (e.g. Paravisini *et al.* 2015). Our ultimate goal is to replicate as closely as possible the conditions of a natural experiment in which firms are randomly assigned to weak and healthy banks. This strategy requires the ability to compare firms in many dimensions, in order to achieve homogeneity between treated and control firms. For this reason our benchmark difference-in-differences specification contains controls for trends in large sets of municipality, industry, and firm control variables. This specification allows us to adequately control for local demand effects and for non-random matching between banks and firms. The potential endogeneity of banking relationships is additionally addressed through an instrumental variable model that exploits a change in banking regulation in 1988.

The validity of our approach also depends on firms not being able to foresee the solvency problems of the weak banks when they formed their relationships. We provide indirect evidence on this point by analyzing the risk premia of weak and healthy banks in the run-up to the crisis.

Our baseline result is that weak-bank attachment caused employment losses of about 2.2 percentage points. This estimate is large, accounting for around 24% of the total fall in employment among exposed firms in our sample. But our analysis provides several other original results. First, we find that financially vulnerable firms, for example those with a patchy credit history, suffer job losses that are twice as large or more. Second, we find that the impact of weak-bank exposure is greatly reduced for single-bank firms. This finding implies that the current, widespread practice of controling for credit demand by focusing on multi-bank firms (Khwaja and Mian, 2008) could lead to an overestimation of the impact of credit supply shocks.

Lastly, we provide separate estimates for employment losses at surviving and closing firms that reveal an asymmetric impact. On the one hand, about one-half of aggregate job losses of exposed, surviving firms in our sample are explained by weak-bank attachment. On the other hand, such attachment accounts for around one-fifth of the aggregate employment reduction originated by closures of exposed firms in the sample. This suggests that credit constraints were relatively more binding on continuing than on closing firms, which is consistent with the large employment share of temporary contracts –around 21% in our sample ex-ante– permitting quick labor shedding (Bentolila *et al.*, 2012) to avoid closures, which would then depend more on other factors (captured by our controls).

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, Section 5 presents our empirical strategy, and Section 6 our key estimates. Section 7 presents some findings on the transmission mechanism and on exogeneity, and Section 8 some results on treatment heterogeneity. Section 9 shows estimates for the probability of firms closing down. Section 10 contains our conclusions. Two appendices provide information on weak banks and securitization, as well as details on the variables used.

2 Literature review

Apart from the theoretical literature on the channels linking the conditions of external finance and employment cited in the Introduction, in recent years there has been a surge of studies exploiting quasi-experimental techniques to estimate the real effects of credit supply shocks.[5] The two most closely related papers are by Greenstone *et al.*

[5] Some studies exploit the heterogeneous impact of large external shocks to the banking system (Chava and Purnanandam, 2011, Benmelech *et al.*, 2012, or Ongena *et al.*, 2013). Others exploit cross-sectional differences in the financial vulnerability of firms at the start of the Great Recession. Almeida *et al.* (2011), Benmelech *et al.* (2012), and Boeri *et al.* (2013) exploit differences in the debt maturity structure of firms. Lastly, Garicano and Steinwender (2014) compare the response of different types of investment at foreign-owned and nationally-based manufacturing firms in Spain.

(2014) and Chodorow-Reich (2014). Both exploit the cross-sectional variation 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. 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 all of the credit reduction is due to banks' credit supply decisions, the estimated effect is small, around 5% of the employment fall.

Chodorow-Reich (2014) does have access to loan-level data from the Dealscan syndicated loan database. He constructs a firm-specific credit supply shock that is equal to the weighted average of the reduction in lending that the firm's last pre-crisis syndicate imposes on other firms during the crisis. These data are matched to employment records from the LBD dataset for a sample of just over 2,000 firms. In line with Greenstone *et al.* (2014), he finds that SMEs that had 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, he finds no significant effects for the largest companies in his sample.

We also exploit differences in lender health to uncover the impact of credit supply shocks on firm-level employment, but there are important differences between these studies and ours. First, both studies impute a credit-supply shock to each firm in their sample, while we focus on the differential strength of the credit restrictions for firms attached to weak banks alone. We therefore cannot obtain an estimate of job losses in the control group, but this is more than compensated by our ability to obtain well identified and very precise estimates of differences attributable to credit constraints.

Indeed, as far as we know, ours is the first study in the field to use an official credit register. Thus we can construct the entire banking relationships of a sample of firms that is roughly one-hundred times bigger than the one in Chodorow-Reich, and the detailed information on borrowers and lenders allows us to refine our estimates in many directions. To start with, we implement what has become the standard practice to control for demand effects, which is quite different from the imputation methods in the above-mentioned studies. And we offer a detailed analysis of the interaction between banks' health and firms' financial vulnerability. Our analysis includes the typical indicators of financial vulnerability, such as firm size or the share of short-term debt, but also high-quality controls for a firm's creditworthiness. This analysis reveals that credit constraints, in our case through exposure to weak banks, can cause job losses that are twice as large for firms with a bad credit history. Moreover, we are the first to offer a decomposition of job losses along the intensive and extensive margins, and find larger overall effects in the former. This distinction can have important implications for 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 starts to recover.

Third, we have access to each firm's loans at the individual bank level, and so we can apply the Khwaja and Mian (2008) approach to isolate the credit supply shock for firms working with more than one bank, while perfectly controling for the demand side. This is important, because it allows us to uncover the credit transmission mechanism and, when comparing the local firm-bank level effect with the firm level one, to quantify the ability of firms to find new lenders to offset the credit shock.

Fourth, the loan records enable a differential analysis for firms that work with only one bank vis-à-vis those working with more than one, and we discover that the former suffer much lower job losses than the latter. Lastly, in line with the results of Paravisini *et al.* (2014), we find that it is key to compare firms within very narrowly-defined cells. As shown below, the introduction of an exhaustive set of trends by firm characteristics reduces our point estimates of the employment losses induced by weak-bank attachment by two-thirds. In the case of the US these selection problems may be less important, since the health of the largest banks depended largely on their exposure to mortgage-backed securities. While this does not obviously correlate with the characteristics of their corporate borrowers, it does not imply that the two are completely orthogonal either.

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 origin and the magnitude of this credit crunch and we show that it was strongest at weak banks.

3.1 The credit collapse

As mentioned in the Introduction, the main solvency problems were concentrated in the savings banks. These banks 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.[6]

Table 1 illustrates the differences in lender health between weak and healthy banks. It shows that in 2006 weak banks were on average larger and held less capital and liquid assets than the rest. Both the rate of return on assets and the share of non-performing loans are comparable for the two sets of banks, but this apparent similarity hides latent losses at weak banks, which surfaced later on, as witnessed by the vastly larger non-performing loan ratio of weak banks in 2012.[7] We conjecture that the main source of their troubles was their much larger share of loans to the real estate industry (REI), namely construction companies and real estate developers: 68% of all loans to non-financial firms vs. 37% for healthy banks. Among the banks that issued mortgage- or asset-backed securities, the ratio of securitized loans to assets was also larger for weak banks, but not significantly so, suggesting that this was not a key difference.

What matters most for our analysis is the markedly different evolution of credit flows at the two sets of banks. 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).

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

[7] Non-performing loan ratios 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.

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. Figure 2 portrays the latter by plotting acceptance rates for loan applications by potential clients (henceforth non-client firms).[8] During 2002-2004 they were higher for weak than for healthy banks and then became similar, in 2007-2008 both rates fell precipitously and at the end of the period they were lower for weak banks.

Lastly, Figure 3 depicts the average interest rates charged by the two sets of banks alongside the ECB policy rate. This evidence suggests that interest rates were scarcely used by weak banks to ration credit demand.[9] 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. We can therefore safely focus our analysis on the differential evolution of the volume of credit at the two sets of firms during the crisis.

3.2 The bank restructuring process

The solvency problems of savings banks were eventually dealt with through State bailouts that took two different forms. First, two small banks were nationalized, Caja Castilla-La Mancha in March 2009 (reprivatized in November 2009) and CajaSur in May 2010 (resold in July 2010). These operations entailed public support of 4.6 bn euro, equivalent to 0.44% of Spanish GDP at the time.

The Government subsequently favored the alternative route of fostering bank mergers (26 weak banks were involved) or the takeover of an ailing bank by a another bank (5 weak banks involved). 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). These operations started in March 2010 and by the end of that year the FROB had provided assistance or com-

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

[9] Stiglitz and Weiss (1981) show 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.

mitments in the amount of 11.6 bn euro, i.e. about 1.1% of Spanish GDP. All savings banks were forced to transform into commercial banks.

Thus a modest amount of funds, about 1.5% of GDP, had been committed to the restructuring process by the end of 2010. Further consolidation operations and the bulk of the nationalizations took place in 2011-2012 (see Appendix 1 and International Monetary Fund, 2012), but these operations did not restore the credit flow by weak banks, as is apparent in Figure 2. The client firms of weak banks could not quickly regain access to credit after the bailouts.

On the basis of this information, 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 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.

4 Data

In this section we provide a description of our data set, the sample selection procedure, and the construction of the treatment and control groups. Further details appear in Appendix 2.

4.1 Data set construction

Our data set combines six different sources that contain very rich information, allowing us to construct the history of firms' banking relationships. In particular, even though we focus 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 contains information on all bank loans to firms in the non-financial sector with a value above 6,000 euros (around 8,100 dollars). Given the low threshold, these data can be taken as a census. For each loan the CIR provides the identity of the parties involved. We also have information on the share of collateralized loans of each firm, its loan 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. We do not observe interest rates, but as noted above this is not a serious limitation.

We also have access to loan application data. 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". 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 balance sheets and income statements that Spanish corporations must submit yearly to the Spanish 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 (*código de identificación fiscal*). In this data set employment is measured as the average level over the year, weighing temporary employees by their weeks of work. The data also contain information on variables like the firm's age, size, and indebtedness. For most firms we only observe an abridged balance sheet with no breakdown of the liability structure, but for a subsample of about 8% of firms we have more detailed data on that structure, including trade credit, which we use in our robustness checks below. 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). It 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.[10] Lastly, we enlarge our information set with two databases on banks. The

[10]We do not observe mergers and acquisitions (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

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 has never been used for research purposes before.

4.2 The treatment variable and the sample

Our main analysis is at the level of the firm, since we are insterested in the real effects of the reduction of credit by weak banks. In Section 4 we will also descend to the level of the firm-bank relationship to understand the transmission mechanism behind our results. We define which firms are exposed to weak banks through the dummy variable WB_i, which takes the value 1 if firm i had any loans with a weak bank in 2006. 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 to this dating.

Furthermore, since the extent of credit restrictions is likely to depend on the intensity of the relationship, we also consider a continuous treatment variable, $WB\ Intensity_i$, defined as the ratio of loans from weak banks to the firm's asset value. This variable is the product of the ratio of debt with weak banks to total debt –weight of weak banks in debt– and the ratio of total debt to asset value –leverage.

Given the size of our data set we can adopt stringent rules for inclusion in the sample. To mitigate potential reverse causality –so that firms' troubles drive banks' problems– we exclude firms in the REI and in two-digit industries selling at least 20% of their value added to the REI in 2000 (see Appendix 2). The date is chosen to minimize potential endogeneity through credit decisions taken in the later part of the boom.[11] We also work with a balanced sample, only including firms with reliable information 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 missing in both registers. Lastly, since we are interested in

firms are above that threshold, we expect to have a much lower fraction. There may still be cases of a firm that is closed down with one ID and then opened with another one, but these cannot be identified.

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

bank credit, we exclude firms with no loans in 2006. This leaves us with a final sample of 169,295 firms.

In 2006 the firms in our sample represented 21% of firms, 32% of value added, and 48% of private sector employees in the industries included in our analysis. They are relatively small, the average number of employees is 25. Indeed, 98.6% of them are SMEs according to the European Commission definition (with less than 250 employees and with either turnover below 50 million euros or balance sheet total below 43 million euros).

Table 2 provides descriptive statistics for our treatment and control groups in 2006. Aggregate employment in our sample fell by 8.1%, which is very close to the fall in the aggregate for the industries we cover. About 61% of firms had no credit from weak banks, while for those that did the average share of credit represented by weak banks was equal to 62% and their ratio of weak-bank credit to assets was equal to 18%. Compared with the control group, firms in the treatment group are on average older and larger, and they have more temporary workers. On the other hand, they have a worse financial profile: they are less capitalized and profitable, they hold less liquidity, and they are more indebted to banks. They work with three banks on average and over 2002-2005 they defaulted more often on their bank loans. These differences, while not always large, are statistically significant. They may originate in different credit standards or different regional or sectoral configurations. In any event, they imply 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.

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 in 2006 firms could have anticipated the future solvency problems of weak banks, we analyze the risk premia charged to Spanish banks' securitization issues prior to the recession. We employ data on tranches of mortgage backed securities (MBS) and asset backed securities (ABS) 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 a 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.[12] 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 Estimation strategy

We start with a difference-in-differences (DD) equation. However, rather than estimating it in levels, we estimate in differences vis-à-vis the initial year:

$$\Delta_4 \log(1 + n_{ijk}) = \alpha + \beta WB_i + X_i\gamma + d_j\delta + d_k\lambda + u_{ijk} \qquad (1)$$

where n_{ijk} is employment at firm i in municipality j and industry k, Δ_4 a four-year difference taken in 2010, WB_i a dummy variable for treated firms, X_i a set of firm controls (discussed below), d_j a vector of municipality dummy variables (3,697), d_k a vector of industry dummy variables at the 2-digit level (80), and u_{ijk} a random shock. All regressors are measured in 2006. Estimating in differences implies that we allow for an aggregate trend and also trends by municipality, industry, and firm characteristics.

For firms that are present in 2006 but had closed down by 2010 we set n_{ijk} to zero in 2010 and use $\log(1+n)$, so that we can measure employment changes in both surviving and closing firms, although below we will also study the probability of closure.

[12] 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 perceived risk for funders is not statistically different.

As already indicated, parameter β captures a partial effect that can be identified as causal, namely the differential impact of credit constraints arising from attachment to weak banks, as opposed to healthy banks. It is the average treatment effect on the treated (ATT). It can only be unbiased under random assignment of firms to the treatment and control groups, conditional on observables. What are the main threats to identification? Demand effects constitute an important concern (Mian and Sufi, 2014). On the one hand, lending grew especially for the real state industry and it was therefore more concentrated in certain areas, where we might observe in the recession 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 approach of analyzing employment changes within regions or even provinces may be too coarse to credibly control for these effects. For this reason, we allow for differential trends by municipality (3,697) and industry (80).[13]

Another key threat to identification is the non-random assignment of firms to banks prior to the crisis. On the one hand, aggregate shocks may differentially affect the productivity of different firms. If during the crisis product demand, say, fell more for low-quality than for high-quality firms, then the DD estimate would capture these effects as well as credit supply shocks. This is argued in Paravisini *et al.* (2015) regarding the elasticity of exports to credit using matched credit-export data from Peru. On the other hand, as shown in Section 4, firms in the treatment group have worse financial statistics. Therefore, laxer loan approval criteria at weak banks may have caused a systematic bias in the risk profile of treated companies and affect their access to credit in the crisis. In Section 7 we will show that the reduction of credit by weak banks is present after perfectly controling for firm-specific credit demand, via a local bank-firm regression. Still, while this approach allow us to identify a credit supply shock and to confirm the inability of firms to fully offset it, it also limits our analyisis to multi-banks

[13]In Section 6 we undertake an alternative check by focusing on tradable goods, along the lines of Mian and Sufi (2014).

firms which, as we will also show below, are not representative of the full sample.

For these reasons we allow for different trends by firm groups defined by a set of 18 firm controls included in X_i, which can be classified into two types. The first type is directly related to productivity or performance, namely age and its square, size (log assets), rate of return on assets, and the share of temporary contracts. The second type is related to financial health and creditworthiness: bank debt ratio, shares of short-term (up to one year) and long-term bank debt (above 5 years), liquidity and own-funds ratios, number of past (2002-2005) loan applications to non-current banks and an indicator for whether all were accepted, dummy variables for having any past loan defaults, any current (2006) loan defaults, and any credit lines, number of banking relationships and its square, and share of loan amounts that is uncollateralized. Furthermore, we also present estimates from an instrumental variables model aimed at obtaining exogenous variation in weak-bank attachment and an estimation via exact matching, where we directly compare firms in very narrowly defined cells, with almost identical results.

After discussing the ATT in the next Section we deal with other issues, such as making sure the results are actually driven by changes in credit and estimating the effect of heterogeneity in financial vulnerability. We end with an analysis of the probability of firm exits.

6 Main empirical results

This section presents the main empirical results. We start with a baseline specification and then present a battery of alternative specifications so as to ascertain the robustness of our findings.

6.1 Baseline specification

Table 3 presents the estimation results for our baseline DD equation (1). We report robust standard errors corrected for multiclustering at municipality, industry, and main bank level. If no fixed-effect trends are included, employment at firms exposed to weak banks is found to fall by 7.7 pp more than at non-exposed firms (col. 1). Including municipality trends and then adding industry trends reduces the impact of weak-bank exposure to -7.2 pp and -6.7 pp, respectively (cols. 2-3). In the next column we add trends for the set of firm controls that are intended to capture differences in performance, as indicated above, and this further reduces the estimated effect to -5.9 pp. Lastly, adding the second set of firm controls, related to financial aspects, significantly brings down the effect to -2.2 pp (col. 5).[14] We adopt this specification as our baseline, bearing in mind that it is very conservative as to the impact of credit constraints.[15]

Is this estimate large or small? We should start by clarifying that these microeconomic estimates cannot be 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.[16] With the estimate from our DD baseline, exposure to weak banks accounts for 23.7% of the total fall in employment among exposed firms in our sample, which is fairly large.

The impact of the reduction in credit supply grew stronger over time and did not become significant until 2010. To illustrate this point we have estimated the baseline specification for alternative ex-post periods, obtaining the following effects (in pp): -0.1 (0.3) for 2007, -0.2 (0.5) for 2008, and -1.0 (0.7) for 2009.

We also test for differences in pre-crisis trends for treated and control firms by running a placebo equation with 2002 as the pre-crisis year and 2006 as the post-crisis year (note that the sample gets reduced). As required, this specification test delivers a null coefficient (col. 6).[17]

[14] If we instead use municipality times industry fixed effects the estimate is -2.0 pp (0.4), and with municipality times industry times main bank fixed effects it is -2.0 pp (1.0). We do not adopt either specification since observing at least two firms in each multivariate cell substantially reduces sample size.

[15] 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 -1.7 pp (0.3).

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

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

6.2 Alternative specifications

Table 4 reports the estimates for a set of alternative specifications that further address the main identification threats.

We start by refining our procedure to control for selection effects. Instead of introducing the trends of the baseline specification, we compare treated and non-treated firms within narrowly-defined municipality, industry, and firm control cells. To do this 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 6,949 strata with observations, 4,496 of which can be matched across treated and control firms. Using weighted least squares, the estimated employment effect of weak bank attachment is equal to -3.4 pp (col. 1).

An alternative way to control for selection is to use a panel with several observations for the post period and allow for firm-specific trends. In first differences this leads to the following panel fixed-effects model (Wooldridge, 2010):

$$\Delta \log(1 + n_{ijkt}) = \alpha_i' + WB_i d_t \beta' + X_i d_t \gamma' + d_j d_t \delta' + d_k d_t \lambda' + d_t \phi + v_{ijkt} \qquad (2)$$

where common variables are defined as in equation (1), α_i' is a set of firm fixed effects, d_t a vector of time dummies for t=2007,...,2010, and v_{ijkt} a random shock. The equivalent of β in equation (1) is the element of the coefficient vector β' corresponding to 2010 –whose value is relative to 2007– which is equal to -3.0 pp (0.9).[18] This point estimate is larger but not statistically different from our baseline result.

Next, Mian and Sufi (2014) argue that local demand effects should only affect output in non-traded goods sectors, while credit supply shocks should affect both traded and non-traded goods sectors. We therefore aim at filtering out local demand effects by restricting attention to traded sectors. Mian and Sufi (2014) use two classifications, one based on ad-hoc tradability criteria and another one based on geographical concentration. We prefer the latter, since more concentrated industries are likely to be more traded and hence less dependent on local demand conditions.[19] We follow these authors in computing the Herfindahl concentration index for 3-digit industries and 50

[18] The number of observations is 641,739 and the R^2 is 0.760.

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

provinces, and labeling as tradable the goods in the highest quartile. This sample selection yields a negative effect on employment of 4.9 pp (col. 2), which is larger than the baseline (though it is not statistically different), possibly because these industries are more credit dependent than less concentrated ones.

The next exercise considers an alternative definition of weak banks. We characterize a bank as being weak if its 2006 loan exposure to firms in the REI, measured as the share of loans to the REI over the total value of its outstanding loans, was within the upper quartile of the distribution. Table 4 shows that this alternative definition yields an estimated impact of weak-bank exposure of 2.1 pp, which is very similar to our baseline (col. 3).

Next, the reference year is 2006, the last full year of the expansion which ended around mid-2007. If we instead use 2007 as the base year, the estimated weak-bank effect is -1.7 pp (col. 4), which is slightly smaller than the baseline.

Lastly, we expect the effect on employment to increase with the degree of exposure to weak banks. To find out if this is so we reestimate equation (1) replacing the weak-bank dummy by a measure of the intensity of exposure, given by the ratio of loans from weak banks to the firm's asset value in 2006. As indicated in Table 2, the average value of this ratio for exposed firms is 18% (ranging from around zero to 98%). Our estimated linear effect is 10.4 pp which, evaluated at the average intensity of exposure, gives an overall effect of 1.9 pp, again quite close to the baseline. We also allowed for non-linear effects by including a cubic polynomial of this ratio as well, but the higher-order terms were not significant.

7 Transmission mechanism and exogenous exposure

In this section we further test the robustness of our baseline estimates, following three avenues: checking that credit is the actual channel of transmission of the exposure to weak banks, analyzing whether the effect also holds for non-bank credit, and addressing the potential endogeneity of the treatment.

7.1 Is credit the transmission mechanism?

A clean way to ascertain whether weak banks cut credit more than healthy banks is to perform a local estimation on credit for bank-firm pairs:

$$\Delta_4 \log(1 + Credit_{ib}) = \theta_i + \vartheta WB_b + FB_{ib}\kappa + \epsilon_{ib} \tag{3}$$

where $Credit_{ib}$ is total credit committed by bank b to firm i in 2010, both drawn and undrawn so as to minimize potential endogeneity, WB_b is a dummy variable for weak banks, FB_{ib} a set of firm-bank controls which includes the log of the length of the bank-firm relationship in years (since 1984) and an indicator that takes the value 1 if the firm has past defaults with the bank, and ϵ_{ib} a random shock. Including firm fixed effects implies restricting the sample to firms working with at least two banks and having the same banks in 2006 and 2010 but, as stated by Khwaja and Mian (2008), it has the important advantage of perfectly controling for credit demand. As shown in Table 5, using the sample of firms from the loan-level data set alone the estimate is equal to -8.0 pp and with the firms in our baseline sample that satisfy those conditions to -9.0 pp., which establishes the differential credit reduction by weak banks.

Going back to job losses, to check whether credit is the key channel underlying the weak-bank effect, we estimate the following instrumental variable (IV) model for the change in employment:

$$\begin{aligned} \Delta_4 \log(1 + n_{ijk}) &= \alpha'' + \beta'' \Delta_4 \log(1 + Credit_{ijk}) + X_i\gamma'' + d_j\delta'' \\ &\quad + d_k\lambda'' + \varepsilon_{ijk} \\ \Delta_4 \log(1 + Credit_{ijk}) &= \rho + \mu WB_i + X_i\eta + d_j\sigma + d_k\psi + \omega_{ijk} \end{aligned} \tag{4}$$

where common variables are defined as before, $Credit_{ijk}$ is total credit committed by all banks to the firm in 2010, and ε_{ijk} and ω_{ijk} are random shocks.

Coefficient μ captures the differential impact of weak banks on credit committed, while β'' captures the pass-through from credit to employment. Thus, $\mu\beta''$ is equivalent to β in the previous section. The exclusion restriction is that working with a weak bank alters employment growth only through credit changes, as opposed to other channels.

As is apparent in the first-stage panel of Table 6, the instrumental variable is negatively correlated with credit and the F statistic confirms the absence of a weak instruments problem (col. 1). In the second stage, credit is a significant determinant of employment changes, so that a one percentage point increase in credit raises employment by 0.44 points. This coefficient times the weak-bank effect on credit yields an employment reduction of 2.2 pp, which is identical to the baseline DD estimate.

It is worth estimating our IV model for firms satisfying the same conditions as in the local estimation above. The first stage coefficient is equal to -6.1 pp (col. 2), which reveals two interesting facts. First, it is lower than the local bank-firm estimate of -9.0 pp, which means that when weak banks reduced credit to their clients, these firms were not able to fully offset the shock by turning to other lenders, and this is the mechanism behind the real effects that we find. And, second, the -6.1 pp estimate is larger than the one obtained for the full sample (col. 1), which suggests that multi-bank firms experienced a stronger credit supply shock than single-bank firms. We return to this issue in Section 8.

So far we have focused on bank credit as the major funding source for Spanish firms. However, trade credit is an alternative source, and a firm's suppliers may have advantages over banks as credit providers, in terms of acquiring information, monitoring, and efficient liquidation (Petersen and Rajan, 1997). We cannot fully ascertain whether trade credit may have compensated for bank credit constraints, because we only have data on firms' liability structure for a subsample of 12,889 firms (less than 8% of the total), which tend to be larger than the rest. For example, in 2006 their median assets were equal to 9.3 million euros, vis-à-vis 0.6 million euros in our full sample. At the median, financial institutions and trade credit respectively represent 32% and 35% of their liabilities.

For this sample we find that the weak bank dummy is significant in the first stage, a larger pass-through from credit to employment, and an overall effect of weak-bank attachment on employment of -3.3 pp (col. 3). Though none of these differences is statistically significant vis-à-vis those in col. (1), it makes sense that weak-bank attachment has a smaller effect on total credit than on bank credit and that total credit has a larger effect than bank credit on employment. The overall effect is higher than the full sample estimate of 2.2 pp, but this is not due to the different measure of credit, since the reduced-form estimate for bank credit in this sample is also equal to

-3.3 pp (0.9). The disparity is therefore fully attributable to the different characteristics of the firms in the reduced sample. We can then conclude that, for those firms for which we observe it, trade credit did not alleviate firm's bank credit constraints. This result is consistent with work by Molina Pérez (2012), who finds no increase in trade credit taken by firms over the early crisis (2008-2010) with a sample of 9,602 Spanish firms, 85% of which are below 250 employees.

7.2 Exogenous variation in exposure to weak banks

Firms choosing to work with a weak bank may have been driven by particular motives such as its laxer credit standards. Thus, to further try to rule out selection effects we need an exogenous source of variation in attachment to these banks. For this purpose we can exploit a regulation-based instrumental variable. Up 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 removed (Real Decreto-ley 1582/1988). We therefore use as IV the local weak-bank density, i.e. the share of bank branches in December 1988 belonging to weak banks in the firm's municipality. This variable should capture exogenous variation in the probability of weak-bank attachment, since a firm is more likely to work with a bank that has traditionally operated in its area.

Since our instrumental variable varies by municipality, we cannot include trends by municipality as well. Instead we allow for different trends in coastal provinces –more precisely, provinces in the Mediterranean Coast and in the Balearic or Canary Islands– and the rest, because in these provinces the housing price and credit booms were the most pronounced. The exclusion restriction is that local weak-bank density only affects a firm's employment through its exposure to weak banks. This condition cannot be tested and therefore this exercise may not be fully conclusive. In any event, in Table 6 we see that high weak-bank density in 1988 significantly predicts weak-bank attachment 18 years later and that the associated employment effect amounts to -3.0 pp, which is in the ballpark of other foregoing estimates (col. 4).

8 Financial vulnerability

The literature on relationship lending and financial accelerators indicates 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 begin with a triple difference (DDD) model, again estimated in four-year differences, which apart from the trends in the firm control variables included in the baseline specification, adds interaction of five of them with the weak-bank dummy to try to capture the effects of financial vulnerability. The first two indicators measure whether a firm has defaulted on any loan or whether it has received a rejection of any loan application in the period between 2002 and 2006. The other three indicators refer to 2006. First, a standard way to measure financial vulnerability is through the value of a firm's short-term debt at the onset of the crisis (Almeida *et al.*, 2012). Accordingly, we create a dummy that takes the value 1 if the firm has a share of short-term debt in total bank debt above one-half, implying that it subsequently had to renew a sizable fraction of it. The final two indicators capture size –log(Total Assets)– and firms indebted to just one bank.[20,21]

The estimates appear in Table 7 (col. 1). As expected, having a bad credit record, given by past rejected loan applications and, especially, loan defaults, or high short-term debt entails more job losses during the crisis, whereas larger firms suffer less. These effects are stronger for firms exposed to weak banks. In particular, the weak bank effect is almost twice higher for firms with rejected applications and 50% higher for firms with past defaults. Firm size does not have an additional effect for firms working with weak banks, which is noteworthy given that the latter were created especially to support small firms.

Lastly, we examine whether the impact of credit constraints varies with the number of banking relations.[22] We distinguish between firms working with only one bank from

[20] 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.

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

[22] This differs from the IV analysis in col. (2) of Table 6, where we restricted attention to firms that

the rest. The empirical literature has not reached a robust conclusion on this issue, but evidence for the Great Recession does find less credit constraints for single-bank firms (Gobbi and Sette, 2014). For Spain we expect to obtain the same, since in our sample single-bank firms have better ratios of capitalization, liquidity, return on assets, bank debt, and credit record than multi-bank firms, regardless of whether they work with weak or healthy banks. These expectations are confirmed: job losses at single-bank firms are 3.3 pp lower than at multi-bank firms. Given the ratios above, this can be interpreted as the result of a flight to quality. Moreover, remarkably, the negative effect of working with a weak bank is more than offset for single-bank firms.

Indicators of firm creditworthiness are usually not available in standard data sets. Thus, it is worth exploring how the estimates vary when they are excluded from the estimation. Column (2) of Table 7 indicates that in our case they are hardly affected.

9 Probability of exit

Employment may not recover at the same speed if a large fraction of firms downsize than if they close down. 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 (1) for surviving firms alone. The estimated impact of weak-bank exposure is equal to 1.1 pp, which is (marginally) significantly lower than for the full sample.[23] The extensive margin is explored by estimating the effect of weak-bank attachment on firm exit probability. We start with a linear probability model for exit in 2010 with respect to 2006, using the same specification as in equation (1) but now for a binary dependent variable.

As seen in the first column of Table 8, weak-bank exposure leads to a marginal increase in the exit probability of weak-bank dependent firms of 0.1 pp vs. non-exposed firms (col. 1). We also try an alternative specification with our continuous treatment variable, the ratio of weak-bank credit to assets, in place of the dummy. The estimated effect is 7.0 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

kept their banking relationships throughout the whole period and we did not include other interactions.

[23] The standard error is 0.5 pp, the R^2 is 0.074 and the number of observations is 152,209.

ninth decile –with an exposure ratio of one-quarter– has a 1.7% higher probability of closing down, which amounts to 17% of the baseline exit rate of 10%.

The last column confirms that single-bank firms are much less affected by exposure to a weak bank. Their marginal probability of exit is 2.6 pp., i.e. 5.4 pp less than for multi-bank firms. Therefore, the ninth-decile firm working with a weak bank would only be 0.6% more likely to close down than the first-decile firm.

Using these estimates we can compute separately employment losses at surviving and closing firms. On the one hand, we compute employment reductions for survivors as we did for the baseline result, using the estimate quoted above. On the other hand, we calculate the number of firm closures from the estimated probability of exit in Table 8 and the employment drops so induced. Adding up both estimates the overall job loss at exposed firms is 28.1%, which somewhat higher than the 23.7% found for the baseline equation. About one half of this overall loss is accounted by each group of firms. However, the estimated effect on each one differs markedly. Weak-bank exposure accounts for a full 52.1% of job losses at surviving firms, whereas it explains 18.9% of jobs lost due to firms closing down. This suggests that credit constraints were relatively more important in inducing firms to downsize than in forcing them to close down.

10 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 small and medium-sized, for whom 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 2.2 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 at 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. 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 19% of losses originated by firm closures are. This suggests that credit constraints were relatively more important in inducing firms to downsize than to close down. Our paper is the first to offer this type of decomposition, which carries interesting implications for the speed of recovery after slumps.

We also contribute to the literature on the interaction between credit constraints and the number of banks that firms work with. Our results show that in the Spanish case firms that relied on a single bank were scarcely affected by that bank being weak.

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 total job losses suffered by firms attached to weak banks were 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		
23	Caja S. Fernando	Cajasol			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 securites 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 firm had any loan from a weak bank in 2006. The Weak Bank Intensity is the ratio between the total value of a firm's loans from weak banks and its book value in 2006.

Municipality. There are 3,697 municipalities, corresponding to the firm's headquarters. They need to have at least two firms in the sample.
Province. There are 50 provinces, see www.ine.es.

Industry. 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). Size (Total assets), Age (current year minus year of creation), Return on Assets (earnings before interest, taxes, depreciation and amortization/Assets), Temporary Employment (temporary employees divided by total number of employees.), 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), Own Funds (own Funds/total assets), Liquidity (liquid assets/total assets), Past Loan Applications (number in 2002-2005), All Past Applications Accepted (0-1 dummy), Past Defaults (1 if any non-performing loan in 2002-2005), Current Defaults (1 if any non-performing loan in 2006), Credit Line (1 if at least one), Banking Relationships (number of banks with outstanding loans), and Uncollateralized Loans (uncollateralized loans/total bank debt).
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: Age, Return on Assets, Temporary Employment, Bank Debt, Own Funds, Liquidity, Past Loan Applications, and Uncollateralized Loans. 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: All Past Applications Accepted, Past Defaults, Current Defaults, and Credit Line.

Acknowledgements

Bentolila is also affiliated with CEPR and CESifo, Jansen is also affiliated with Fedea and IZA. This paper is the sole responsibility of its authors. The views presented here do not necessarily reflect those of the Banco de España or the Eurosystem. We are very grateful to Joshua Angrist, Manuel Arellano, Stéphane Bonhomme, Gabriel Chodorow-Reich, Juan J. Dolado, Markus Demary, David Dorn, Pietro Garibaldi, Tullio Jappelli, Juan F. Jimeno, José Liberti, Pedro Mira, Rafael Repullo, Javier Suarez, John van Reenen, Ernesto Villanueva, Etienne Wasmer. We are also grateful for comments by seminar audiences at BBVA Research Department, Banco de España, CEMFI, European Central Bank, and IMT Lucca; at conferences at Banco de Portugal, CSEF (Naples), De Nederlandsche Bank, IZA/CEPR European Summer Symposium on Labor Economics, Kiel Institute for the World Economy, and Simposio de la Asociación Española de Economía; and at the following universities: Alcalá de Henares, Alicante, Autònoma de Barcelona, Autónoma de Madrid, Ca' Foscari Venezia, Complutense de Madrid, and Mainz. We also wish to thank Ana Esteban and José I. González-Pascual, from the Statistics Office of the Banco de España, for help with the data. All errors are our own. Jansen acknowledges funding from the Ministerio de Economía y Competitividad Grant ECO2012-37742.

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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)	13.74	2.11	16.40	0.97	-7.10
Own Funds/Total Assets	8.38	9.02	5.15	1.24	2.05
Liquidity/Total Assets	23.72	22.40	11.49	4.50	3.12
Return on Assets	1.04	1.73	0.89	0.28	0.50
Non-performing Loan Ratio	1.52	6.29	0.70	0.55	0.75
Non-performing Loan Ratio (2012)	8.55	12.75	21.99	5.99	-3.47
Loans to REI/Total Loans to NFF	36.76	22.32	67.87	8.07	-7.91
Securitized loans/Total Assets	14.86	10.48	18.51	6.25	-1.56

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 on 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.	Mean	St. Dev.	t test
Loans with WB/Assets	0.00	0.00	17.95	17.46	–
Share of loans with WB	0.00	0.00	61.58	35.81	–
Employment (employees)	19.75	134.40	33.39	478.98	-8.64
Temporary Employment	20.81	25.97	22.54	25.74	-13.35
Age (years)	12.08	9.52	12.39	9.33	-6.48
Size (million euros)	3.47	36.36	7.30	145.35	-8.07
Own Funds/Total Assets	34.29	23.91	27.21	20.21	62.98
Liquidity/Total Assets	12.73	15.22	8.96	12.26	53.37
Return on Assets	6.65	11.61	5.48	9.19	21.84
Bank Debt	31.60	27.03	43.75	25.44	-92.22
Banking Relationships (no.)	1.67	1.08	3.10	2.63	-160.00
Past Defaults	1.34	11.50	2.32	15.05	-15.11

Notes. Observations: 103,441 control firms and 65,854 treated firms. WB denotes weak banks. Variables are ratios in percentages unless otherwise indicated. The share of loans with weak banks is in 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. The employment effect of weak-bank attachment. Difference in Differences

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

	(1)	(2)	(3)	(4)	(5) Baseline	(6) Placebo
WB_i	-0.077***	-0.072***	-0.067***	-0.059***	-0.022***	0.004
	(0.011)	(0.008)	(0.010)	(0.010)	(0.007)	(0.008)
Municipality f.e.	no	yes	yes	yes	yes	yes
Industry f.e.	no	no	yes	yes	yes	yes
Firm controls	no	no	no	yes	yes	yes
R^2	0.001	0.032	0.050	0.063	0.074	0.156
No. obs.	169,295	169,295	169,295	169,295	169,295	126.997

Notes. OLS estimates for 2010, except in col. (6), where 2006 is used. Firm controls (see Appendix 2 for definitions): In col. (4): Size, Age, Age Squared, Return on Assets, and Temporary Employment. In col. (5): those in col. (4) plus Bank Debt, Short-Term Bank Debt, Long-Term Bank Debt, Own Funds, Liquidity, Past Loan Applications, All Past Applications Accepted, Past Defaults, Current Defaults, Credit Line, Banking Relationships, Banking Relationships Squared, and Uncollateralized Loans. "yes/no" indicates whether the corresponding set of variables is included. Robust standard errors corrected for multiclustering at the municipality, industry, and main bank level appear between parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 4. The employment effect of weak-bank attachment. Difference in Differences

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

	(1)	(2)	(3)	(4)	(5)
	Exact Matching	Tradable Goods	Loans to REI	2007 ex-ante	Intensity
WB_i	-0.034***	-0.049***	-0.021***	-0.017***	
	(0.010)	(0.020)	(0.008)	(0.007)	
$WB\ Intensity_i$					-0.104***
					(0.028)
Municipality f.e.	no	yes	yes	yes	yes
Industry f.e.	no	yes	yes	yes	yes
Firm controls	no	yes	yes	yes	yes
R^2	0.001	0.109	0.074	0.057	0.074
No. obs.	166,315	21,029	169,295	159,649	169,295

Notes. OLS estimates for the four-year difference in 2010, except in col. (4), where a three-year difference is used. In col. (1) a matching estimator is used (see text); the no. of strata is 6,949 and the no. of matched strata is 4,496. See text for a description of the sets of firms included in the sample in the other columns. Firm controls (see Appendix 2 for definitions): Size, Age, Age Squared, Return on Assets, and Temporary Employment. In col. (5): those in col. (4) plus Bank Debt, Short-Term Bank Debt, Long-Term Bank Debt, Own Funds, Liquidity, Past Loan Applications, All Past Applications Accepted, Past Defaults, Current Defaults, Credit Line, Banking Relationships, Banking Relationships Squared, and Uncollateralized Loans (all transformed into dummy variables in col. (1), see Appendix 2). "yes/no" indicates whether the corresponding set of variables is included. Robust standard errors corrected for multiclustering at the municipality, industry, and main bank level appear between parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 5. The credit effect of weak-bank attachment
Dependent variable: $\Delta_4 \log(1 + Credit_{ib})$

	(1)	(2)
WB_b	-0.080**	-0.090**
	(0.038)	(0.046)
Firm-bank controls	yes	yes
Firm f.e.	yes	yes
R^2	0.411	0.393
No. firms	93,562	47,847
No. obs.	264,236	141,695

Notes. OLS estimates for 2010. Firm-bank controls (see Appendix 2 for definitions): Log of length of the bank-firm relationship and an indicator for Past Defaults with the bank. "yes/no" indicates whether the corresponding set of variables is included. Observations are winsorized at both extremes. Robust standard errors corrected for multiclustering at the bank and firm level 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: $\Delta_4 \log(1 + n_{ij})$

Instrumented Variable	(1) Δ_4 log (1+Credit$_{ijk}$)	(2) Δ_4 log (1+Credit$_{ijk}$)	(3) Δ_4 log (1+Credit$_{ijk}$)^a	(4) WB_i
	0.447***	0.301***	0.849***	-0.061***
	(0.127)	(0.132)	(0.367)	(0.026)
First stage				
WB_i	-0.048***	-0.061***	-0.039***	
	(0.010)	(0.014)	(0.019)	
Local weak-bank density$_i$				0.496***
				(0.071)
Municipality f.e.	yes	yes	yes	no
Industry f.e.	yes	yes	yes	yes
Firm controls	yes	yes	yes	yes
Coastal province f.e.	–	–	–	yes
Overall effect ($\mu\beta''$)	-0.022	-0.018	-0.033	-0.030
F test / p value	23.1/0.00	17.9/0.00	4.09/0.05	13.3/0.00
No. obs.	169,295	47,847	12,889	169,295

Notes. [a] In column (3) Credit is total credit, i.e. bank and trade credit. Instrumental variables estimates for 2010. In column (2) the sample includes firms working with at least two banks, which were the same in 2006 and 2010. Firm controls (see Appendix 2 for definitions): Size, Age, Age Squared, Return on Assets, and Temporary Employment. In col. (5): those in col. (4) plus Bank Debt, Short-Term Bank Debt, Long-Term Bank Debt, Own Funds, Liquidity, Past Loan Applications, All Past Applications Accepted, Past Defaults, Current Defaults, Credit Line, Banking Relationships, Banking Relationships Squared, and Uncollateralized Loans. "yes/no" indicates whether the corresponding set of variables is included, "–" indicates that the corresponding set of variables is comprised in a wider set of fixed effects. Robust standard errors corrected for multiclustering at the municipality, industry, and main bank level appear between parentheses. Significance levels: * p<0.10, ** p<0.05, *** p<0.01. F test and p values for the inclusion of the IV in the first stage are reported (Staiger and Stock, 1997).

Table 7. The employment effect of weak-bank attachment. Triple Differences
Dependent variable: $\Delta_4 \log(1 + n_{ijk})$

	(1)	(2)
WB_i	-0.035***	-0.036***
	(0.007)	(0.007)
Rejected application$_i$	-0.065***	
	(0.008)	
$WB_i \times$ Rejected application$_i$	-0.027**	
	(0.013)	
Past Defaults$_i$	-0.212***	
	(0.031)	
$WB_i \times$ Past Defaults$_i$	-0.059**	
	(0.033)	
Short-term debt$_i$	-0.083***	-0.083***
	(0.013)	(0.013)
$WB_i \times$ Short-term debt$_i$	-0.016	-0.017
	(0.014)	(0.014)
log(Total Assets$_i$)	0.018***	0.019***
	(0.005)	(0.005)
$WB_i \times$log(Total Assets$_i$)	0.004	0.002
	(0.006)	(0.006)
Single bank$_i$	0.033***	0.033***
	(0.007)	(0.007)
$WB_i \times$ Single bank$_i$	0.037***	0.043***
	(0.016)	(0.016)
Municipality f.e.	yes	yes
Industry f.e.	yes	yes
Firm controls	yes	yes
R^2	0.071	0.071
No. obs.	169,295	169,295

Notes. OLS estimates for 2010. Firm controls (see Appendix 2 for definitions): Size, Age, Age Squared, Return on Assets, and Temporary Employment. In col. (5): those in col. (4) plus Bank Debt, Short-Term Bank Debt, Long-Term Bank Debt, Own Funds, Liquidity, Past Loan Applications, All Past Applications Accepted, Past Defaults, Current Defaults, Credit Line, Banking Relationships, Banking Relationships Squared, and Uncollateralized Loans. "yes/no" indicates whether the corresponding set of variables is included. Robust standard errors corrected for multiclustering at the municipality, industry, and main bank level appear between parentheses. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Table 8. Effect of weak-bank attachment on the probability of exit
Dependent variable: Probability of exit from 2006 to 2010_i

	(1)	(2)	(3)
WB_i	0.010^{***}		
	(0.004)		
$WB\ Intensity_i$		0.070^{***}	0.080^{***}
		(0.015)	(0.016)
$WB\ Intensity_i$			-0.054^{***}
$\times$ Single bank_i			(0.017)
Municipality f.e.	yes	yes	yes
Industry f.e.	yes	yes	yes
Firm controls	yes	yes	yes
R^2	0.080	0.081	0.078
No. obs.	169,295	169,295	169,295

Notes. OLS estimates for 2010. Firm controls (see Appendix 2 for definitions): Size, Age, Age Squared, Return on Assets, and Temporary Employment. In col. (5): those in col. (4) plus Bank Debt, Short-Term Bank Debt, Long-Term Bank Debt, Own Funds, Liquidity, Past Loan Applications, All Past Applications Accepted, Past Defaults, Current Defaults, Credit Line, Banking Relationships, Banking Relationships Squared, and Uncollateralized Loans. "yes/no" indicates whether the corresponding set of variables is included. Robust standard errors corrected for multiclustering at the municipality, industry, and main bank level appear between parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$..

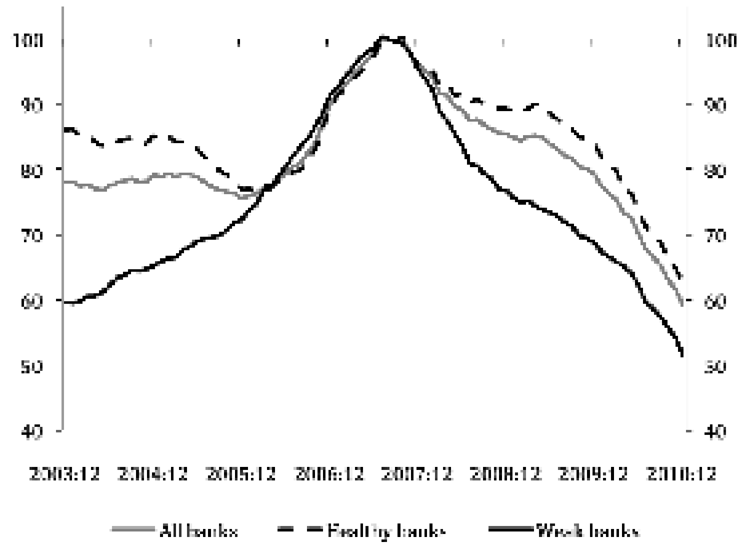

Figure 1: New credit to non-financial firms by bank type (12-month backward moving average, 2007:10=100)

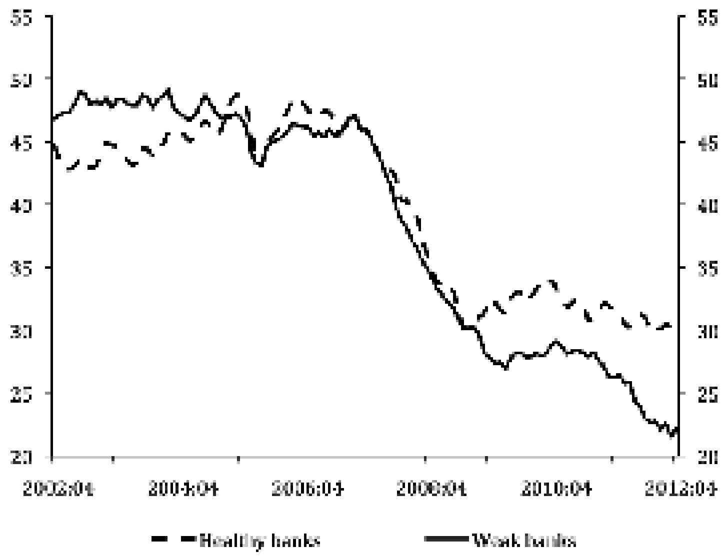

Figure 2: Acceptance rates of loan applications by non-current clients, by bank type (%)

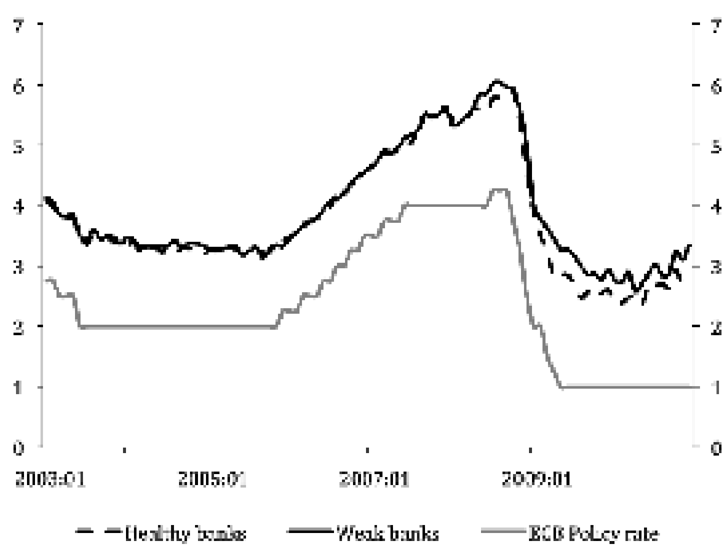

Figure 3: Average annual interest rate for new loans to non-financial firms by bank type and the policy rate (%)

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