# When Credit Dries Up: Job Losses in the Great Recession*

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**Abstract**

The Great Recession has renewed interest in the real effects of credit supply shocks. In this paper we use a unique dataset to estimate job losses from credit constraints in Spain by exploiting differences in bank's health at the onset of the crisis. Due to solvency problems, many banks in Spain have been bailed out during the Great Recession. We show that these banks reduced credit supply more than the other banks and, to analyze the implications for employment, we compare employment changes from 2006 to 2010 at two groups of firms: those that obtained a significant share of their funding from weak banks and those receiving it from healthier banks. Our most conservative estimates imply that, once selection biases are controled for, firms attached to weak banks suffered an additional fall in employment between 3 and 6 percentage points, which represents a 15% to 33% higher reduction.

KEYWORDS: Job losses, Great Recession, credit constraints.

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# 1 Introduction

The current financial crisis has severely weakened the lending capacity of Spanish banks. In this paper we use a unique dataset to estimate the impact of this credit supply shock on employment at the firm level.

The Spanish experience during the Great Recession offers an ideal setting to explore the impact of a banking crisis on the real economy. Bank lending to non-financial firms contracted significantly during the crisis and, concurrently, the Spanish economy suffered an unprecedented fall in employment. In addition, there are interesting similarities between the events in Spain and in countries like the US or Ireland. All three of them experienced a boom-bust cycle in the housing market followed by a profound financial crisis that forced governments to finance bail-out schemes and to nationalize banks. Hence, the Spanish example may serve to draw lessons that are applicable elsewhere.

Last but not least, we have built a dataset of extraordinary quality to study the issue at hand. We have access to the Central Credit Register (CIR), a proprietary dataset of the Bank of Spain with detailed information on all the bank loans above 6,000 euros to non-financial firms granted since 1984. Using these data we are able to construct the complete banking relations of a representative sample of over 217,000 firms working with almost 230 banks. In fact, besides the data on committed loans, we also have data on loan applications from non-current customers of a bank and whether these applications turn into actual loans, and so we also have information about loan demand, and all this information is linked to balance sheet data of the firms and all the banks that operate in Spain. The result is, to the best of our knowledge, the most comprehensive matched firm-loan-bank dataset that has ever been assembled to estimate the real effects of shocks to the banking system.

The theoretical explanations for the link between credit supply shocks and real variables like investment or employment are mostly based on agency problems. The asymmetric information possessed by banks and firms drives a wedge between a firm's cost of internal and external funds and this may limit a profitable firm's ability to raise external funds or to substitute between alternative sources of funding, and more so during recessions (Chava and Purnanandam, 2011). A firm that suffers an unanticipated reduction in the supply of credit from its banks may therefore have no other choice than to reduce its scale of operations or even close down.

Any study that aims at estimating the response of employment to this kind of shocks needs to address various intricate identification problems. The main challenge is to disentangle the changes on the supply and the demand side of the credit market. A financial crisis may force banks to reduce credit supply, but at the same time it may induce firms to reduce their demand for credit as they have access to less profitable investment opportunities. In addition, the poor performance of firms may reinforce the economic difficulties of banks, leading to further reductions in credit supply, and on top of that there is a risk of selection effects. The average quality or creditworthiness of client companies may differ across banks and this may have important implications for the transmission of shocks.

The solution adopted in this paper is to exploit the pronounced differences in the health of Spanish banks at the onset of the crisis. The global financial crisis and the bursting of the Spanish housing bubble led to a weakening of the balance sheets of all banks, but the impact was far from homogenous. In recent years, the Spanish Government has bailed out many of the weakest ones, all but one being *Cajas de Ahorros* or Savings Banks (SBs), while the other banks managed to survive without financial assistance from the State. We show that the first group of banks reduced credit more than the other and, to identify the real effects of this different evolution of credit, we compare employment changes at two sets of firms: a treatment group of firms that obtained a significant share of their loans from the eventually bailed-out banks, henceforth *weak banks*, and a control group of firms that borrowed primarily from the banks that survived without capital injections by the State.

We define our treatment variable as the ratio between the joint value of a firm's loans from weak banks and the book value of the firm in 2006, and we assign firms with a sufficient degree of exposure to weak banks to the treatment group. Our identification strategy relies critically on the existence of frictions that do not permit firms to readily switch to a different bank, *i.e.* the firms in the treatment group must not have been able to get credit from healthier banks during the crisis. Second, it is equally important that firms could not predict the solvency problems of weak banks at the time when they formed their banking relationships. To address this issue, we collect data on the securitization by Spanish banks in 2006 and show that there were no significant differences in the risk premia paid by the two groups of banks. In other words, as late as 2006 financial markets still failed to recognize the differential buildup of risks in weak and healthy banks. This evidence supports our claim that firms could not predict the future solvency problems of weak banks.

Even so, we find small but significant differences in the characteristics of the firms in the treatment and control groups. The firms in the treatment group are younger and smaller than those in the control group and they generally present worse financial ratios. This is an observable reflection of differences in the risk management of banks. Prior to the crisis, weak banks accepted a larger proportion of loan applications and their total credit volume grew faster than at healthier banks. Furthermore, at the onset of the crisis weak banks accounted for a disproportionately large share of the loans to construction companies and real estate developers, henceforth referred to as the *real estate industry*.

The fact that weak banks seem to have granted loans to worse firms leads us to include firm controls in all our empirical specifications. Furthermore, in order to exclude the risk of reverse causality –with economic difficulties of client companies driving the intervention of banks– we exclude from our sample those firms belonging to the real estate industry and those selling a significant proportion of their output to this industry. Apart from this strategy to identify exogenous variation in firms' choice of banks, we perform a wide range of robustness checks and we experiment with different estimation techniques. Our aim is to replicate as close as possible the conditions of a natural experiment in which some firms are randomly assigned to weak banks and others to healthy banks. In this vein, we exploit an important change in the regulation of savings banks in 1988, whereby the location decisions of savings banks, which had been strongly constrained was fully liberalized. We use weak bank location prior to the regulatory change to instrument the exposure of firms to weak banks in 2006.

A final concern is whether the trouble faced by weak banks should be simply seen as the result of an aggressive policy of gaining market shares by weak bank management without due consideration for risk during the housing boom. We show empirically that this is not the full story, using banks' exposure to the real estate industry in 2000 –well before the housing bubble started– as an instrumental variable for weak bank attachment.

We briefly anticipate our empirical approaches and results. We start the analysis with a standard difference-in-differences approach, in which we compare the evolution of the employment levels of the two groups of firms using observations for the years 2006 and 2010. We include employment adjustments at both surviving and exiting firms. Initially, we fix all firm and bank level controls at their 2006 value, but we also consider earlier dates before the explosion of housing prices. In addition, we also use an alternative definition of weak banks based on the banks' pre-crisis exposure to the real estate industry. In this way we are able to quantify the spillover effects from the excessive lending to the real estate industry by some banks and the subsequent bursting of the housing bubble on the other sectors of the economy.

Moreover, banks need not treat all their clients alike and the degree of financial vulnerability of firms may vary substantially both across and within the two groups of banks. To capture the resulting implications for employment, we also estimate several specifications in which we interact the treatment dummy with firm level characteristics such as its size, its share of short-term debt, and whether they worked with a single bank, had defaulted on a loan or were subject to credit constraints in the form of rejected loan applications in the pre-crisis period.

In a second approach we exploit the large size of our sample using exact matching techniques to compare the evolution of employment at similar firms in the treatment and control groups. Firms are grouped into more than 4,800 cells with data and in roughly 74% of them we are able to match a firm from the treatment and control groups.

In the foregoing approaches it is assumed that the differential evolution of employment at the two groups of firms is driven by credit constraints. In our final approach we explicitly test this assumption using instrumental variable models with firm-level fixed effects. In particular, we link the yearly changes in employment at the firm level between 2007 and 2010 to three alternative indicators of credit constraints: the annual change in the total value of committed loans of a firm, a dummy variable for firms that had a loan application rejected, and the proportion of a firm's loan applications that are accepted. All three variables capture endogenous outcomes and so we instrument them with the product of our bank treatment dummy and a year dummy.

Regardless of the empirical approach followed, we find the same qualitative result. Firms with a relatively large exposure to weak banks at the onset of the crisis destroyed a larger percentage of their jobs in the period between 2006 and 2010 than other firms. Once we control for selection effects, our most conservative estimates indicate that firms in the treatment group destroyed an additional 3.2 to 6.2 percentage points of employment compared to the firms in the control group, i.e. a 18% to 35% larger job destruction than at non-attached firms. There is one exception: firms with loans from only one weak bank have, *ceteris paribus*, not suffered larger job losses than those attached to one healthy bank. These firms obtained more credit than comparable firms working with several banks, possibly as a result of the "evergreening" of their loans.

The rest of the paper is organized as follows. In Section 2 we review previous work and in Section 3 we provide some institutional and aggregate background on the Spanish economy before and during the financial crisis. Section 4 describes our data, Section 5 presents our empirical strategy, and Section 6 shows our estimation results. Section 7 contains our conclusions. Appendix 1 provides some details on the variables used.

# 2 Literature review

The literature on measuring credit constraints and their effects on company outcomes is large. We do not aim at summarizing it here. Pagano and Pica (2012) provide references and a theoretical model which explores various channels. One theoretical result that is relevant for our exercise is that financial development induces firms to rely on borrowing, as opposed to hoarding cash. Then, if banks undergo a crisis preventing them from providing liquidity, output and employment will be more affected in economies in which firms rely more on banks' services. Using a country-industry dataset for the period 1970-2003, they do find some evidence that in banking crises negative shocks hurt employment growth more in industries that are more financially dependent within financially developed nations.

Turning to quasi-experimental techniques, we are obviously not the first to estimate the real effects of shocks to credit supply. Broadly speaking, we can divide the recent studies in this field in three groups depending on their identification strategy. A first strand of papers exploits cross-sectional differences in the financial vulnerability of firms at the onset of the Great Recession. Almeida *et al.* (2011), Benmelech *et al.* (2011), and Boeri *et al.* (2013) exploit cross-sectional differences in the debt maturity structure of firms. The share of long-term debt that was maturing right after the fall of Lehman Brothers in September 2008 was determined several years in advance, and this leads to fairly exogenous differences in firms' refinancing needs. Similarly, Garicano and Steinwender (2013) try to elicit the impact of credit constraints in Spain by comparing the evolution of investment and employment at nationally-based manufacturing firms with foreign-owned ones, which have better access to credit. This study is based on a survey of around 3,000 firms with limited financial data.

A second strand of papers exploits the effects of large external shocks to the banking sector. Chava and Purnanandam (2011) analyze the real effects of the financial crisis that followed the announcement in 1998 by the Russian Government of its intention to default on its sovereign debt obligations. Many US banks suffered significant capital losses during this crisis and the authors estimate the effects of this external shock to the banking sector by comparing the performance –in terms of stock market valuation– of bank-dependent firms to that of firms that had access to the debt market, which continued to operate normally.[1]

The third route, which is the one adopted here, is to exploit cross-sectional differences in the health of banks. Greenstone and Mas (2012) construct a county-level credit supply shock from the interaction of the change in US banks' small-business lending at the

[1] In a similar exercise, Benmelech *et al.* (2011) use the real estate crisis in Japan in the early 1990s and its effect on the US banking sector to identify the causal effect of credit shocks on firms.

national level and the predetermined credit market share of these banks at county level. They find that this measure is highly predictive of the considerable reduction in county-level credit to small standalone firms and to their employment levels in the period from 2008 to 2010. Similarly, Chodorow-Reich (2013) uses data from the Dealscan syndicated loan database and measures the relative health of a firm's lenders using the reduction in lending to other borrowers during the crisis by the firm's pre-crisis syndicate. In line with Greenstone and Mas (2012), he finds that relatively smaller firms 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 pre-crisis clients of healthier banks. By contrast, for larger companies he finds no significant effects.

Compared to the above studies, our paper offers a number of significant contributions. First, unlike Greenstone and Mas (2012) or Chodorow-Reich (2013), we are able to reconstruct the complete banking relationships of all the firms in our sample. Chodorow-Reich (2013) observes the identify of the lenders that participate in the syndicated loans to the roughly 2,000 firms in his sample, but due to data restrictions he needs to impute the shares of the committed loans that correspond to each of the lenders and the syndicated loans only comprise a part of the new loans of each firm. Greenstone and Mas (2012) face even stronger data restrictions since they do not have any information to link individual lenders and borrowers. Second, both papers identify supply shocks using observed reductions in lending, either at the national level or to other borrowers who receive loans from the same banks. Here, by contrast, we exploit cross-sectional differences in the solvency of banks as evidenced by the fact that a subset of them had to rely on State aid and we explore the role of the real estate industry in generating these solvency problems. We also have access to a much larger set of controls, including information which is completely absent in other papers about loan demand and the creditworthiness of firms, which allows us to perform tests that are unfeasible with the available data for the US. Moreover, except for Greenstone and Mas (2012), data limitations have forced researchers to focus on large firms, which in principle should be less subject to credit constraints. For example, the average employment level in Chodorow-Reich (2013) is 2,978 employees and the median is 620. In our 2006 sample the corresponding figures are 23 and 6 employees, so that we are more likely to find effects of credit constraints if they exist.

# 3 The financial crisis in Spain

In this section we provide background information on the performance of the Spanish economy –weak banks in particular– and the evolution and distribution of credit to firms.

## 3.1 The demise of savings banks

The Spanish economy experienced a very long expansion, from 1996 to 2007, in which GDP grew at an average annual rate of 3.7% and employment at 4.1%. Then the Great Recession hit: GDP fell by 1.1% per year on average from 2007 to 2010 and employment had collapsed by 10% by the end of 2010. Unemployment soared from 8.6% at the end of 2007 to 20.3% three years later.

The effects of the international financial crisis cannot be isolated from events in the domestic credit market. During the boom, the expansionary monetary policy followed by the European Central Bank (ECB) induced Spanish banks to take risks (this is the so-called risk-taking channel of monetary policy). They reduced their credit standards and fueled the buildup of a bubble in the domestic housing market with easy and cheap loans to construction companies and real estate developers, as well as homeowners (Jiménez *et al.*, 2013). The stock of loans to the real estate industry grew from 10% of GDP in 1992 to 43% in 2007. As a result of the income and credit booms, nominal housing prices trebled from the 1996:4 to 2007:4, while by 2010:4 they had fallen by almost 11%.

The Spanish banking regulator, the Bank of Spain, forced commercial banks to keep almost all securitized assets on their balance sheets and implemented the so-called dynamic provisioning in 2000 –so that they had to provision for unrealized loan losses. This helped banks at the beginning of the recession (Jiménez *et al.*, 2012b). However, in Spain banks had mostly funded their lending from external sources and they were therefore more acutely hit by the 2008 freezing of wholesale Eurozone markets, having to heavily resort to loans from the ECB. Eventually, the poor performance of real estate industry after the bursting of the bubble ended up threatening the solvency of many banks.

Yet the buildup of excessive risks was not uniform across all banks, with the main risks being concentrated in the savings banks. Solvency problems at SBs were initially dealt with by fostering mergers and takeovers, but as their situation deteriorated several had to be bailed out by the Government. The bailout process entailed either a merger of banks or a takeover of an ailing bank by another bank –with or without loans from the public sector– or a bank's nationalization and subsequent selling through an auction to another private bank. During our period of analysis only two, very small SBs were nationalized and quickly auctioned off. Thus the weak banks that we focus on in this paper had not yet been bailed out during our sample period. Figure 1 illustrates this process, which eventually brought down the number of SBs from 47 in 2006 to 11 in 2011.[2]

Not by chance, all weak banks but one were SBs. Unlike regular commercial banks, SBs were not quoted in the stock market and their governance was peculiar. While their boards were formally appointed by political parties, labor unions, depositors, workers, and other social agents in varying shares, SBs were *de facto* controled by regional governments, which influenced their lending decisions (see Cuñat and Garicano, 2010).

In 2006 weak banks accounted for 32% of outstanding credit to the non-financial sector and for 39% of outstanding loans to the real estate industry. As shown in Panel A of Table 1, while loans to the real estate industry represented on average 33.5% of loans to the non-financial sector at healthy banks, they comprised 64% at weak banks. This gap showed up in the difference between the rates of non-performing loans at weak and healthy banks, which was non-existent by the end of 2006 but rose steadily thereafter, reaching 1.7 percentage points by the end of 2010. These developments led to considerable differences in the vulnerability of banks at the onset of the crisis, that we exploit to identify the effect of financing constraints on employment.

[2] See the website "The restructuring of the banking sector in Spain" at bde.es/bde/en and in particular the document there entitled "Summary table on restructuring of the Spanish banking sector".

## 3.2 The credit collapse

In Spain firms rely more on bank credit than in other countries. For example, in 2006 loans from credit institutions to non-financial corporations represented 86% of GDP in Spain, vis-à-vis 62% in the EU, not to mention the 35% of the United Kingdom.[3] For this reason, Spain is a very interesting country to study credit constraints arising from bank credit supply shocks, since alternative sources of funding are hard to come by.

Moreover, credit grew enormously during the boom. Figure 2 depicts flow of real new credit to non-financial firms provided by deposit institutions in Spain over 2003:12-2010:12, measured as a moving average over the past 12 months (using the CPI as deflator). From the initial period to the peak in 2007:10 this flow increased by 40%, whereas it subsequently fell by a similar fraction until the end of 2010. It is apparent that credit granted by weak banks grew more than at healthy banks during the boom (53% v. 33%) and it fell more during the slump (48% v. 37%). In terms of individual loans, summarized in Panel B of Table 1, the median reduction in new credit to non-financial firms in 2010 compared to 2006 was equal to 48% for weak banks and 42% for the remainder, while the respective averages were 46% and 5%.

This evolution could be the result of changes in the extensive margin –credit to firms that are new to banks– and/or the intensive margin –new credit to current clients. Acceptance rates for loan applications by firms to banks with whom they have no relationship at the time of the application indicate the first margin was important. As shown in Figure 3, over 2002-2004 acceptance rates were 3.1 pp higher for weak than for healthy banks. This pattern is reversed during the crisis: both rates fall precipitously during 2007-2008, but since then acceptance rates are 4 pp lower for weak banks in 2009-2010. More tellingly, Figure 4 represents acceptance rates for loans to firms that applied simultaneously to weak and healthy banks (i.e. it applied at least to one healthy and one weak bank). Controling in this simple way for loan demand reveals that over 2002-2004 acceptance rates at weak banks were 6.5 pp higher than at healthy banks and 6.3 pp lower over 2009-2010.

What about interest rates? Figure 5 shows there was an increase in the average annual

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

interest rate on loans of all maturities to non-financial Spanish companies from 3.3% in 2005:12 to 5.9% in 2008:9, which closely follows movements in the ECB's policy rate (one-week rate for main refinancing operations). However, the average loan interest rate falls thereafter to 2.4% in 2010:5, when it slowly steps up again. Thus, while there is some tightening, it is clearly reversed upon Lehman Brothers' failure. The graph also shows essentially no difference between the rates charged by weak and healthy banks up to the end of 2008, whereas weak banks start charging a slightly higher rate thereafter (34 basis points). This evolution justifies our empirical approach, which focuses on the volume of credit rather than on interest rates.

### 3.3 Bank heterogeneity

Our identification strategy relies on the existence of frictions that prevent firms that borrowed from weak banks before the crisis to readily switch to healthier banks during the recession. We will provide evidence on this point below. We end this section with a related issue, namely whether it was feasible for firms to anticipate during the credit boom the differences in the risk of insolvency of banks that came to the surface after the outbreak of the crisis. Anticipation would be expected if markets realized the implications of the differential exposure to the real estate industry across the two groups of banks, but such anticipation effects would invalidate our identification procedure.

To explore this issue, we collect data on the risk premia charged to Spanish banks' securitization issues prior to the recession (from Dealogic). To be more precise, we employ data on the risk premia of tranches of mortgage backed securities (MBS) and asset backed securities (ABS) in 2006. We group these ratings into three standard categories: prime (AAA), investment grade (AA+ to BBB-), and speculative (BB+ to D). In total we have 303 observations (deal-tranches), all of them floating-rate, with quarterly coupon frequency, and referenced to the 3-month Euribor.[4] Our final sample contains securities by 24 issuer parents.

We regress coupon differentials, in basis points, of weak and healthy banks on variables

[4] 43 observations that are referenced to Libor and 2 observations referenced to the 1-month Euribor are removed from the sample.

capturing the type of securitization (MBS or ABS), risk category, month of issue, years to maturity, collateral type, and guarantor type. We are interested in the coefficient on a dummy that takes on the value 1 for weak banks. Standard errors are clustered by issuer parent. Our results, reported in Table A1, show a coefficient associated with the weak bank dummy that is positive but not significant (2.8 basis points, with a $p$-value of 0.55). Hence, we cannot reject the null hypothesis that financial markets failed to recognize the buildup of differential risk at weak banks as late as 2006. It therefore seems safe to assume that private firms, with less capacity to process the available information than financial markets have, could not possibly have predicted them either.

# 4 Data

In this section we describe the unique features of our data and how they allow us to estimate the causal effect of shocks to bank credit supply on employment at the firm level.

As mentioned in the Introduction, a negative macroeconomic shock may affect both firms –reducing loan demand– and banks –cutting loan supply. Consequently, an observed decrease in a firm's employment after the shock may be due to either factor or both. To disentangle supply from demand, it is essential to have data on firm and bank characteristics and to have exogenous measures of firms' vulnerability to shortages in bank credit. Our database contains all of this information. It combines five different sources on the following areas: firm-level economic and financial data, firm creation and destruction, bank balance sheet information, individual loans granted by banks to firms, and individual loan applications to a bank by new customer firms.

Economic and financial information for more than 300,000 firms is obtained from public balance sheets and income statements that Spanish corporations must submit yearly to the Spanish Mercantile Registers. We collect these data from the Iberian Balance sheet Analysis System (SABI) elaborated by INFORMA D&B in collaboration with Bureau Van Dijk and the Central Balance Sheet Data Office of the Bank of Spain. This dataset contains information on employment, which is measured as the average number of employees over the year, broken down by type of contract, i.e. permanent and temporary. In the case of Spain it is vital to control for cross-sectional differences in the share of temporary contracts. These contracts can be terminated at much lower costs than permanent ones and so, other things equal, we expect larger employment adjustments at firms with a larger temporary share. Moreover, firms that expect to face financing constraints in the future have an incentive to maintain a buffer stock of temporary contracts (Caggese and Cuñat, 2008).

We do not exploit the whole sample. To avoid the presence of reverse causality –so that the troubles of firms drive the solvency of banks– we exclude firms belonging to the Construction and Real Estate sectors. We also leave out firms in industries that are closely linked to them, defined as 3-digit industries that sold at least one-fifth of their output to those sectors in 2000 according to the input-output tables.[5] The output share threshold is conservatively chosen and the date is pushed back to 2000, so as to minimize potential endogeneity driven by the excesses of the end of the boom. This leaves us with a sample of 217,025 firms.

SABI also contains detailed information on firms' income statements and balance sheets. We complement this information with data from the Central Business Register on firm entry and exit, which is especially relevant for employment, since it allows us to disentangle employment adjustments at the intensive and extensive margins.

We match these datasets with loan and bank information. As mentioned in the Introduction, our loan information is obtained from the Central Credit Register (CIR) of the Bank of Spain. The CIR contains proprietary information on virtually all business loans granted by all banks operating in Spain, given that all loans above 6,000 euros (around 8,100 dollars) are reported. Since we consider only loans to non-financial firms, this dataset can be taken as a census. The CIR allows us to have complete information about all banking relationships of the firms in our sample. Hence, we observe whether

[5] Apart from Construction and Real Estate, the excluded industries are: Extraction of Non-metallic Minerals, Wood and Cork, Cement, Lime, and Plaster, Clay, Non-metallic Mineral Products n.e.c., Fabricated Metal Products except Machinery and Equipment, Machinery and Electric Materials, Rental of Machinery and Household Goods. See Appendix 1.

a firm works with one or more weak banks, and for each firm we can compute the ratio between the value of loans from the weak banks and its total asset value, which is our key treatment variable. We also know the number of bank relationships, collateralized loans, and credit lines, as well as a measure of maturity for all loans –so that we can break them down between short-term (up to one year), long-term (above 5 years), and medium-term (in between). We can therefore control for firms' refinancing needs at the onset of the crisis. Since we are interested in the effects of bank credit constraints, we restrict our sample to firms with at least one loan.

Firms' creditworthiness is typically unobservable, but in our sample we have information from the CIR on non-performing loans and, especially, on potentially problematic loans. A firm's credit history should affect its ability to obtain loans, both before and during the crisis, and we therefore use it as a proxy of expected future credit constraints.

From the CIR we can identify all the banks that firms are borrowing from and also the one with the larger share, i.e. the main bank. Moreover, we enlarge our information set by using a data base on bank financial statements, which is used by the Bank of Spain for regulatory and supervisory purposes.

Lastly, we also have information about loan applications by potential borrowers of a bank. All banks routinely receive monthly-updated information from the CIR on their own current borrowers' total current credit exposures and loan defaults –vis-à-vis all banks in Spain. Banks can also costlessly obtain this information on a potential borrower, defined as "any firm that seriously approaches the bank to obtain credit". By matching the loan application dataset with the CIR we can also observe for each application whether the loan is granted or not. In case a bank requests information but does not grant the loan, either the loan was denied or the firm obtained funding somewhere else (see Jiménez *et al.*, 2012a, for details). Since only in the case of firms with a credit history does the loan applications dataset provide information on whether they borrowed from their own bank(s), we do not consider loan applications from newly entering firms. Accepted loan applications are an indicator for the ease with which firms have access to alternative sources of financing, while rejected loan applications are a useful indicator of the credit constraints that they face.

Our final sample, then, matches information on firms' and banks' balance-sheet and income statements, credit history and loan application datasets at firm level. We only work with non-financial, non-public, corporations and limited liability companies. Our final sample consists of 217,025 firms working with 226 banks, including commercial banks, savings banks, and credit cooperatives. The sample represents 27% of firms, 37% of value added, and 61% of private sector employees in the industries included in our analysis in Spain in 2006.

# 5 Empirical strategy

In this Section we start by presenting our empirical strategy, which follows three different approaches, and then go on to discuss how we deal with threats to identification. But before we do so we need to define our treatment variable.

## 5.1 The treatment variable and the sample

As explained in the Introduction, our aim is to measure the employment losses from the differential effect of the financial crisis on the lending capacity of banks due to differences in the health of these banks at the onset of the crisis. We do so by comparing the evolution of employment in firms with high and low shares of loans from banks that received financial support from the State. Our definition of weak banks only includes those that obtained funding to remain alive, excluding those that received funding to acquire other banks. As already indicated, during our period of analysis only two, very small SBs, were nationalized and quickly auctioned off, so that the vast majority of bailouts ocurred after 2010, i.e. outside our sample period. The data clearly reveal that these banks have reduced lending more than the rest and we want to test whether this leads to corresponding differences in the employment levels of the client firms of both groups of banks. We make our approach operational by focusing on the pre-crisis share of a firm's debt with weak banks, and we normalize this value by its total asset value. The resulting ratio thus reflects both the overall leverage ratio and the relative importance of weak banks in a firm's total debt.[6]

About one-third of firms had no credit from weak banks. The histogram of those which did is shown in Figure 6. In our benchmark we define the treatment group as given by firms above the third decile of the cross-sectional distribution of firms with strictly positive exposure to weak banks in 2006 and the 74th percentile of the overall distribution of firms. This threshold corresponds to a ratio of loans from weak banks to asset value of 6.3%. This may seem a low figure, but it corresponds to an average share of weak banks in total bank credit for treated firms of 51.4% so that, in this sense, treated firms do predominantly work with weak banks. Additionally, treated firms have an average ratio of credit with weak banks to assets of 25.1% and an average share of weak banks in total credit of 71%. The choice of the third decile is arbitrary, but we will also report estimation results for different values of the threshold and show that the results hold qualitatively for any other decile.

Furthermore, one objection to our approach could be that the treatment is defined in terms of an outcome, namely bank bailout, that is realized several years after the outbreak of the crisis. This use of an ex-post criterion does not invalidate our results, but in one of the robustness exercises we also experiment with an alternative definition of weak bank that relies on the *pre-crisis* exposure of banks to firms in the real estate industry. Details are given in Section 6.

In 2006, the firms in our sample had the following average features: 23 employees, 22% of whom were temporary, 11.8 years of age, 4.5 million euros in assets, an own funds ratio of 31%, a liquidity ratio of 11%, a rate of return on assets of 6%, a bank debt ratio of 37% –of which 47% was Short-Term and 25% Long-Term–, 79% of loans was uncollateralized and they had loans from 2.2 banks. 68% of firms had a credit line, 2% defaulted on loans over 2002-2005 and 0.5% in 2006, 59% made bank loan applications during 2002-2006, of which 23.2% were accepted. Firm that close down represent 4.4% of observations, and

[6] We focus on drawn credit, but in order to test the robustness of our results we include undrawn credit in one of our empirical specifications and in our instrumental variables approach explained below. In addition, we also consider an alternative in which the treatment variable is defined in terms of the absolute value of loans from weak banks, while the leverage ratio is included in the set of firm controls. The details are provided in Section 6.

excluding them only raises average employment from 23 to 24 employees. On the other hand, they represent one-quarter of aggregate job losses from 2006 to 2010.

Table A2 provides descriptive statistics for our benchmark choice of the treatment and control groups. It reveals significant differences in the mean characteristics of the firms in both samples. Compared with the control group, firms in the treatment group are on average younger and smaller in terms of both employment and assets, and they generally present worse financial characteristics: they are less capitalized, have less liquidity, and they are less profitable and more indebted with banks, though their loans are on average of higher maturity and are more likely to be collateralized. Between 2002 and 2005 they had made substantially more loan applications to banks of which they were not customers and had more applications accepted; they also have a worse credit history. These firms also worked with banks that were smaller, less capitalized, less profitable, with less liquidity, with more mortgages as a share of loans, and with a larger ratio of non-performing loans. The statistical significance of differences in the means of most firm-level characteristics is no surprise. Due to the large size of our samples, the standard deviations are tiny and so we are able to identify small significant differences. Nonetheless, this feature clearly suggests that we should control for firm-level characteristics in our empirical exercises, since weak banks granted loans to less profitable and potentially more vulnerable firms than the other banks.

## 5.2 Three takes on credit constraints

In our first set of empirical specifications, we follow the standard difference-in-differences (DD) approach, and estimate the following equation:

$$\log(1 + n_{it}) = \alpha + \delta WB_i + \gamma Post\ WB_i + \beta Post + \eta d_s + \theta Post\ d_s + X_i'\phi + u_{it} \qquad (1)$$

where $n_{it}$ is employment at firm $i$ in year $t$, where $t$ =2006 and 2010, $WB_i$ is a dummy variable taking on the value 1 if the firm had an ex-ante share of funding with a weak bank above the third decile of the distribution of exposure across firms with positive exposure in our sample in 2006, $Post$ is a dummy variable capturing the Great Recession (represented by 2010), $d_S$ jointly denotes sets of 50 province and nine industry dummy variables, $X_i$ is a set of control variables, and $u_{it}$ denotes random shocks.

Our sample is an unbalanced panel: though most firms are present in both periods, some firms are only observed in 2006 and others only in 2010 (see Appendix 1). We keep all observations so as to increase efficiency. For firms that are observed in 2006 but not in 2010 *because* they closed down we set $n_{it}$ to zero –and therefore use $\log(1 + n_{it})$ as the dependent variable–, so that we are measuring employment changes both at surviving firms and due to firm closures.

Our key hypothesis is that firms that worked more with weak banks suffer more stringent credit constraints during the crisis and this translates into larger employment losses. We do not aim at estimating all potential effects of credit constraints on employment, but rather the differential impact of those credit constraints stemming from being attached to a weak bank –as opposed to other banks– in the recession, which is measured by $\gamma$ in equation (1).

Our goal is to isolate the impact of credit constraints on observationally identical firms choosing ex-ante to borrow from an ex-post insolvent bank vis-à-vis a solvent one. Under these conditions, selection effects which may bias our estimates would be absent. The group controls ($d_S$) and other characteristics ($X_i$) included in the specifications are intended to achieve such ex-ante homogeneity across firms. Including these control variables allows us to estimate the average treatment effect on the treated (ATT) by matching firms in the treatment group to similar firms in the control group. Nevertheless, to check the robustness of our results, we also estimate the DD equation (1) using the coarsened exact matching method, whereby the effect is estimated within multivariate cells determined by discretization of the control variables.

Lastly, we wish to gauge the impact of weak-bank attachment arising from lower access to credit as opposed to other potential channels. To ensure that this is what we are measuring, we estimate the following instrumental variable (IV) model for the proportional change in employment:

$$
\begin{aligned}
\Delta \log(1+n_{it}) &= \alpha' + \delta' \Delta \log(1+Credit_{it}) + \beta' Post_t + \eta' d_s + \sigma' d_i + u'_{it} \\
\Delta \log(1+Credit_{it}) &= \pi + \mu Post_t\, WB_i + \omega Post_t + \rho d_s + \psi d_i + v_{it} \qquad (2)
\end{aligned}
$$

where $Credit_{it}$ is total credit committed by banks, both drawn and undrawn, to firm $i$ in year $t$ and $Post_t$ is a vector of year dummies for $t$ =2007,..., 2010, and $d_i$ is a firm fixed effect. The IV model is estimated as a panel, as opposed to the DD model. Coefficient $\mu$ in the first-stage regression captures the differential impact on credit committed by weak banks during the crisis. Our identification assumption is therefore that working with a weak bank will affect employment changes only through changes in credit, and not through any other channel. Both equations are estimated in first differences, as opposed to the levels used in DD specifications, because –in keeping with the literature– we are better able to explain changes in credit than credit levels.

Using weak-bank attachment as an instrument for credit is not the end of the story, since the choice of banks is still a firm's decision. To avoid selection effects of this type, we need some exogenous variation in the assignment to weak banks. For this purpose, we exploit an important change in the regulation of savings banks in 1988, whereby the location decisions of savings banks, which had been strongly constrained, was fully liberalized. We use weak bank location prior to the regulatory change to instrument the exposure of firms to weak banks in 2006. In a second instrumental variable model we aim at obtaining predetermined historical ties to real estate firms to check whether credit restrictions faced by firms indebted to weak banks do not simply result from poor managerial decisions at those banks of going for into real estate loans just before the crisis. For this purpose we use banks' exposure to the real estate industry in 2000 –well before the beginning of the bubble– to instrument weak bank attachment.

## 5.3 Threats to identification

The main challenges for identification are the non-random assignment of firms to banks prior to the crisis and the possibility of avoiding treatment by getting credit from healthy banks. The relevance of the first threat is highlighted by the different characteristics of firms working with weak and healthy banks before the crisis, as discussed at the beginning of this Section. The challenge is therefore that the set of client firms of healthy and weak banks differ along various dimensions that are important for credit provision. The reason is that the laxer criteria for approving loan applications by weak banks may have caused a systematic bias in the risk profiles of the companies in our control and treatment groups or in general may have been a motive for firms to self-select into weak banks. On the contrary, potential differences in bank default risk do not seem to play a role: financial markets did not recognize this difference in the runup to the crisis, as the results provided in Section 3 show. Indeed, we have estimated a panel regression of acceptance rates controlling for firm effects interacted with time fixed effects and bank controls and clustering at bank level, from 2002:02 to 2010:12, which confirms the fall: the rate for weak banks is above that for healthy banks up to 2004:12, by 4.6 pp (s.e. 1.6 pp), and below during 2008:12-2010:12, by 7.6 pp (s.e. 1.2 pp).

The exceptionally rich contents of our dataset helps us avoid many threats to identification. An obvious problem would arise if there were different pre-existing trends in employment at attached and non-attached firms. Since our data go back in time four years before the outbreak of the recession, we test below for the presence of such trends over that period. Secondly, biased estimates could arise from a different geographical or sectoral concentration of the activities of borrowers or the over-representation of banks in regions. To reduce this kind of bias, we control in all our regressions for province and industry dummies. Moreover, the recession might have affected different areas or industries differently; for example coastal areas have been strongly affected by the crisis because of a large concentration of construction activities. The inclusion of interactions between province and industry dummies and the *Post* variable absorb these differential changes.

We also employ a long list of control variables capturing firm characteristics ex-ante (2006 unless otherwise indicated) that could lead to differential employment outcomes, like the firm's age, size (in terms of assets), temporary employment ratio, and return on assets. A second set of variables is directly linked to its financial health, such as a firm's bank debt ratio and its short-term and long-term bank debt ratios. A third set captures directly the firm's financial vulnerability and several of them serve as direct proxies for expected credit constraints: number of past loan applications and whether all were accepted, past loan defaults, current loan defaults, whether the firm has a credit line, its liquidity and own funds ratios, the number of banking relationships, and the ratio of uncollateralized loans. Lastly, a full set of dummies (226) captures synthetically the characteristics of the main bank that a firm works with.

This rich set of controls affords much better identification than is typically available in the literature. There are 50 provinces in Spain, which provides us with more accurate control of the firm's location than in other papers that have used regions or states (in the US) instead. Also, most of the firm characteristics we introduce are unavailable in standard datasets, like those extracted from firms' balance sheets and income statements. However, what makes our exercise exceptional is the use of firms' banking relationships, in terms of the banks it works with and measures of the banks' assessment of the firm's creditworthiness via its past performance regarding repayment obligations, its decisions to apply for loans, and its success in such applications. The unavailability of this information has forced previous researchers to measure firms' access to credit either from responses to the questions about whether the firm had been denied loan applications in the past (e.g. Caggese and Cuñat, 2008) or from actual credit balances. Moreover, whereas typical sample sizes in the literature are around 2,000-3,000, our data on more than 217,000 firms allows us both to attain a very high precision for our estimates and to apply matching methods along many firm characteristics, so that very similar firms working and not working with weak banks are being compared. Our results indicate that we are indeed able to successfully control for any biases through our control variables.

On the other hand, as already indicated, we guard against self-selection of firms to banks through the estimation of instrumental variable models in which treatment is predicted using bank characteristics in the past, specifically almost twenty years before (1988), and to check whether our findings simply come from poor management decisions during the boom, we estimate another IV model using banks' exposure to real estate well before the bubble started (2000). Self-selection through unobservables is of course still a possibility, and to address it we rely on randomness of the assignment of firms to the control and treatment groups conditional on observables.

Our approach would not be valid however if firms could easily find funding from healthy banks. In the relationship banking framework (see Sharpe, 1990, and the survey by Freixas, 2005) banks have information on a firm's profitability and solvency which is obtained through a long-standing relationship. Therefore, firm-bank relationships should be very persistent. In this case, changing banks is very costly for firms, since it takes time for other banks to acquire such knowledge. As a result, when the Great Recession arrived, obtaining loans from new banks became very difficult and firms were therefore largely limited to the funding provided by banks with long-established relationships with them. As previously mentioned, acceptance rates for loan applications from non-current customers did sharply fall starting in early 2007. Moreover, as we will show with our sample through the first stage of our IV model in equation (2), committed credit fell significantly more for firms attached to weak banks.

# 6 Estimation results

We now present our estimates of the impact of the credit constraints, arising from being attached to a weak bank prior to the crisis, on job losses during the Great Recession. We start with difference in differences estimates, then go on to matching estimates, and end with instrumental variables results.

## 6.1 Difference in differences estimates

We start by presenting the results of the estimation of equation (1). Table 2 presents the results for the standard DD specification. We report robust standard errors that are corrected for clustering at firm and main bank level. Most of our estimates are significant at the 1% level. For our sample period, 2006 to 2010, the raw mean employment differential fall in the crisis at firms working with weak banks vis-à-vis other firms is equal to 8.5 percentage points (pp hereafter), while the *Post* dummy indicates a reduction of 18.7 pp (col. 1). This weak bank effect remains unaltered when only province and industry dummies are included, and it falls to 7.4 pp when firm characteristics are added, with a decline for the reference group of 21.4 pp, implying an additional decline at firms attached to weak banks of 34.5% (col. 2). It minimally falls to 7.3 pp when main bank dummies are included (not shown).[7] Lastly, allowing for differential employment trends by province and industry –through an interaction with the *Post* variable– significantly reduces the effect of weak-bank attachment, to 6.2 pp (col. 3), which we consider our baseline estimate, in particular regarding the set of controls included. The sum of the coefficient on *Post* and on the coefficients on all its interactions is equal to 17.7 pp, so that weak-bank attachment accounts for an extra 35.3% of job losses.

Table A3 shows the coefficients on the firm control variables, which are determinants of employment levels, obtained for this baseline specification; virtually all variables are very highly significant and show the expected signs.

Our results would not be valid if there were differential pre-existing trends between firms attached and non-attached to weak banks. For this reason we run a placebo equation (1) where the initial date is 2002 and the final date is 2006. The coefficient on the interaction $Post \times WB_i$ should not be significant if trends do not differ. As indicated by col. 1 of Table 3, that coefficient is equal to 0.1 pp and it is non-significant. We further check the robustness of our baseline estimate in many ways. We first explore the timing of the impact of the credit constraint on firms by shortening the ex-post period by one year at a time. Table 3 shows that the effect was actually positive in 2007 and it does not become negative and significant until 2009, when it is equal to 3.1 pp, then doubling to 6.2 pp in 2010. Secondly, to avoid possible anticipation effects, we progressively restrict the analysis to firms with long-run banking relationships that were established years before the outbreak of the crisis. In Table 4 we report the effect of shifting back the year at which the firm control variables are measured (cols. 1 and 2). This restriction moderates the effect to 5.9 pp when 2002 is used and to 6.1 pp when 2005 is the reference year.

[7] Since the estimated coefficient did not change when main bank dummies were replaced by either main bank characteristics or main, secondary, and tertiary bank dummies, we only retain the main bank dummies as controls.

Thirdly, we shift the dating of the treatment variable back in time. In this exercise the assignment of firms to the treatment group is based on their weak-bank exposure in either 2000 or 2002 (accordingly, we include main bank dummies for both the pre-crisis year that is used to define the treatment group and for the year 2006). This approach involves a tradeoff: it potentially reinforces the exogeneity assumption but it also brings us farther away from the conditions faced by firms just prior to the crisis –which are likely to be more relevant to outcomes during it. As Table 4 shows, the corresponding estimated effects of attachment to weak banks for benchmark years 2000 and 2002 are respectively 3.5 pp and 4.9 pp, smaller than the baseline but still sizeable (cols. 3 and 4).

Next, we also experiment with different definitions of the treatment variable. We first probe the bank side of real estate industry loan defaults, by redefining a weak bank as one having a real estate industry loan share above the third decile of the distribution across banks in 2006. This variation leads to an estimated employment effect of 6.2 pp (col. 5), which is identical to that obtained for the baseline. In the last two tests, we include undrawn loans in our calculation of the treatment variable and alternatively use the absolute value of the loans rather than its ratio to the total asset value of the firm. The corresponding coefficient estimates for the full sample of firms are, respectively, 5.7 pp and 5.4 pp (cols. 6 and 7).

Lastly, we estimate the effect only for surviving firms, i.e. leaving out those that close down. The estimate is equal to 1.3 pp, which is significantly lower than for the full sample, thus providing evidence on the relative importance of job destruction due to firm closures.

### 6.1.1 Triple difference estimates

It is interesting to study the impact of credit constraint on employment for different groups of firms. To this effect we implement a triple difference (DDD) strategy by interacting the product of the *Post* dummy and the weak bank dummy with several variables. We begin with the nine industry dummies. As shown in Table 5, for five out of nine industries we find significant effects in five industries, with estimates ranging from 8.4 pp in Manufacturing to 4.3 pp in Hotels and Catering. These results are reasonable, since credit is a necessary input for the former industry, e.g. in durable goods, but not so much for the latter.

A second set of DDD estimates tries to capture whether the employment cost of attachment to weak banks depends on a firm's financial situation at the start of the recession, in particular its financial vulnerability. We show the estimates when different measures of vulnerability are introduced separately and then all together, commenting only the latter results (col. 6). We start by interacting the product of $Post \times WB_i$ with a dummy that takes on the value of 1 if the firm had a loan application rejected during the period 2002-2006 (including also this dummy variable and its interaction with the $Post$ dummy as controls). Table 6 reveals that these firms have an extra 6 pp employment loss in the recession but no additional loss if they worked with weak banks. Secondly, firms that defaulted on a loan over the same period experience an extraordinary 22.7 pp additional job losses, though again working with a weak bank does not have additional effects. Next, firms with a share of short-term bank debt in total debt above one-half in 2006 experience 7.4 pp of additional job losses, and another 7.1 pp if they worked with weak banks. Further, small firms (namely those with assets below 10 million euros) suffer an extra 12 pp job losses, but only if they were attached to weak banks.

These findings are in accordance with standard theoretical predictions that smaller, less transparent, and financially weaker firms should suffer credit constraints more strongly. A final result is however surprising. We expected firms with a single banking relationship (loan) to be the most affected by the constraints. However, the opposite is found in Table 6: they suffer 3 pp lower job losses, and another 2.9 pp less if attached to weak banks. This finding is explored in the next subsection.

#### 6.1.2 The degree of exposure and single-bank firms

We have arbitrarily chosen the third decile as the threshold capturing a significant enough degree of exposure to weak banks. To check the sensitivity of our results to this choice, we subsequently reestimate equation (1) for firms with any loans with weak banks and for exposure levels above each decile of the distribution of firms in that set. As shown in Figure 7, the effect is present at all deciles and there is relatively little variation in the estimates for any exposure to the sixth decile, ranging from 5.2 to 6.3 pp. The magnitude however falls for higher deciles, to 2.4 pp above the ninth. In view of the lower effect on single-bank firms found in DDD estimates, we suspect that composition effects may be at work. Indeed, as Figure 8 reveals, the share of single-bank firms grows from 29% for any exposure to a whopping 50% above the ninth decile. For this reason, we estimate equation (1) separately for single- and multiple-bank firms. As depicted in Figure 7, now the estimates are very stable and very different: job losses are around 8.8 pp higher for multiple-bank firms but 2.8 pp *lower* for single-bank firms.

Why would firms concentrate their loans with a single bank? It may be cheaper, if there are fixed costs per loan with each bank (e.g. in posting the collateral). There may also be an advantage as the main bank acquires more information on the firm and it has a stronger stake in its economic success. Suggestive evidence on this hypothesis is given by Frazzoni *et al.* (2012), who study a set of Italian firms over 2004-2009 and find that the strength of a firm's relationship with its main bank –measured by the ratio of loans from such bank to the firm's asset value– has a positive impact on its propensity to innovate and export. This result suggests that relationship banking may help overcome problems in financing innovation or in accessing foreign markets.

We test whether banks treated better those firms that concentrated their loans with them using data from our firm-bank loan database by regressing the yearly change in credit commitment in the recession, over the period 2007-2010, on the share of loans that the firm has with each individual bank. This dataset has 3.75 million observations, so that we restrict ourselves to a 10% random sample, and include firm and bank-year fixed effects. Table 7 shows that not all banks extend more credit to these firms, but weak banks do. This finding suggests an "evergreening" of loans by these banks and it is likely explained by banks internalizing the potentially very damning effect of denying credit to firms that are so heavily dependent on them.

Why would only weak banks behave in this way? It is likely that, while getting credit became harder for all firms, it became especially difficult for firms which were heavily dependent on weak banks. We test this stigma hypothesis using our databases on loan applications from non-current customers and on granted loans. We previously found no weak-bank employment effects for 2007 (see Table 3), so we employ monthly loan (firm-bank) data for the period 2008:1 to 2010:12, which gives us above 240,000 observations. We estimate a linear probability model for the event that a loan is requested and granted on the share that a firm had with weak banks over 2002-2006, including the same control variables as in equation (1). The results in Table 8 show no significant effect at a 70% share, but reveal a stigma effect above an 80% share (though only significant at a 10% confidence level for further thresholds).[8]

## 6.2 Matching estimates

We have so far presented estimates of the effect on a firm's employment of its working with a weak bank before the crisis. To achieve ex-ante comparability, we have controled for a long list of characteristics of the firm and of the main bank it works with. More accurate estimation may however be attained through exact matching, i.e., within $k \times k$ cells –where $k$ is the number of control variables–, so that the effect is estimated for groups of very similar firms. Sample sizes typically found in the literature imply that a very small number of cells can be constructed, whereas in our case we can use cells defined by 14 variables, which we choose according to their significance in the preceding DD regressions. Each variable is split in two, either using its 0-1 nature or the sample median value, the Primary sector and Mining v. the others regarding industry, and, whether or not it is located in the East coast of the Spanish Peninsula (plus the Balearic and Canary Islands) in the case of provinces. Out of 16,384 potential strata, we end up with 4,822 strata with observations and 3,553 of them can be matched across treated and control firms. Figure 9 shows the evolution of the pre-existing trends, making it clear that the matching method supresses any potential trend differences between treated and control firms.

Table 9 reports weighted least squares estimates, where the weights are obtained from the coarsened exact matching method (Iacus *et al.*, 2011). The estimated employment effect attached to the *Post* dummy interacted with the weak bank dummy is equal to

[8] This regression is estimated with a limited set of control variables (see the footnote to the Table), because our full standard list was not available for 2002, only for 2006.

3.2 pp, which is about half the size of our baseline DD estimate. As with the latter, we also check the stability of this estimate to the degree of exposure to weak banks. It turns out that matching estimates are even less stable than DD estimates, ranging from 5.1 pp for any exposure to nil above the ninth decile (not shown in the Table). This is to be expected, since as the degree of exposure to weak banks grows this method increasingly has to find matches for single-bank treated firms.

Our conjecture is again confirmed by estimating separately according to the number of banking relationships. The results are depicted in Figure 10, where estimates for all firms are not shown to avoid cluttering the graph. Single-bank firms attached to weak banks do not suffer additional job losses (estimates are not significant at any decile), whereas multiple-bank firms suffer losses around 4.8 pp on average (estimates are significant at all deciles). There is therefore one exception to our general result: firms working with only one weak bank did not suffer larger job losses than those working with one healthy bank. As indicated by the results in the preceding subsection, these firms obtained relatively more credit, possibly as a result of the evergreening of their loans.

### 6.3 Instrumental variables estimates

As explained in Section 5, to evaluate the impact of weak-bank attachment arising from lower access to credit –rather than other potential channels–, we estimate an instrumental variable model for the proportional change in employment using the change in total credit committed by banks to the firm, both drawn and undrawn (to minimize potential endogeneity), instrumented by the weak bank dummy. Our estimation period is 2007-2010. In this case we use firm fixed effects to absorb firm characteristics and we interact the weak bank dummy with the change in committed lending for the three different year indicators.

As shown in the lower panel of Table 10, the instrumental variable is significantly and negatively correlated with credit, increasingly so as the recession lengthens. Credit is also a significant determinant of employment changes, with a transmission coefficient of 0.42 (col. 1). The overall effect of weak-bank attachment on employment for 2010 vis-à-vis 2007 is equal to 6.5 pp. The effect for 2010 vis-à-vis 2006 is not identified from this estimation, but an instrumental variables estimation for 2007 alone provided an non-significant coefficient with respect to 2006 (as in our DD estimate of Table 3), so that the estimate for 2010 with respect to 2006 is likely to be also around 6.5 pp. This is very close to our baseline DD estimate of 6.2 pp and again twice as high as our matching estimate. In the last two columns we replace credit committed with two alternative measures of credit constraints: a dummy for rejected loan applications and a continuous variable that measures the percentage of accepted loan applications in any year. The estimate, 6.7 pp and 7.4 pp respectively, are close to the effect found for committed credit.

#### 6.3.1 Exogenous variation in exposure to weak banks

The firm's choice of bank is endogenous and firms choosing a weak bank may have been driven by motives, such as laxer credit standards, that subsequently contributed to the demise of the savings banks. In other words, to rule out selection effects and the risk of reverse causality we need an exogenous source of variation in firms' attachment to weak banks. We exploit a couple of them.

First, we exploit a legal change in December 1988 that removed all restrictions on the geographical extension of savings banks. Until that time these institutions could open at most 12 branches outside their region of origin. For this reason, we compute the share of bank branches in each of the 50 Spanish provinces that belong to our set of weak banks in December 1988 and construct a dummy that takes on the value 1 in provinces with a relatively high density of weak banks, where we use as alternative thresholds the median and the 75th and 90th percentiles. This variable should capture an exogenous variation in the probability of working with a weak bank since it is more likely that a firm will work with a bank if it is located in a province where the bank traditionally operates (determined almost twenty years before).

Next, we use that dummy and its interaction with our crisis dummy $Post$ as instruments for the weak bank dummy $WB_i$ and the interaction between $WB_i$ and $Post$ in our baseline DD equation. The correlation between these variables is weaker, but the overall effects are very much in line with our earlier results. In the lower panel of Table 11 it can be seen that high weak-bank density in 1988 significantly predicts weak-bank attachment almost 20 years later and that the second-stage estimates imply strong employment effects in the recession. If our new dummy variable is set to one for all provinces with an above-median density of weak banks in 1988, the estimated effect is equal to 5 pp (col. 1). On the other hand, if we use the 90th percentile, so that the dummy captures the five provinces with the highest weak-bank density, then the effect amounts to 7 pp (col. 3). This confirms the likely minor importance of endogeneity problems in our DD estimates.

A second way of obtaining estimates that are robust to selection is to use the exposure of weak banks to the real estate industry in 2000 –well before there were any signs of a housing price bubble– as an instrument for weak-bank attachment. The last column in Table 11 reveals the power of the instrument and a significant effect of predicted weak bank attachment on employment, with an overall effect of 3.4 pp. This finding suggests that, at least in part, weak banks got into trouble because of their historical ties to real estate firms and not simply because management of these banks aggressively pushed into real estate loans just before the crisis struck.

# 7 Conclusions

In this paper we investigate the effect of credit constraints on employment in Spain. We make our approach operational by identifying one source of credit constraints through firms that had a significant share of their funding with banks that eventually had to be bailed out due to solvency problems during the Great Recession.

We analyze employment changes from 2006, the last boom year, to 2010, well within the recession. Our most conservative estimates imply that, once selection biases are controled for, firms' attachment to weak banks caused a differential fall in employment between 3.2 and 6.2 percentage points depending on the estimation method, i.e. a 18% to 35% larger job destruction than at non-attached firms. There is one exception, in that firms attached to only one weak bank have not suffered larger job losses than those attached to one healthy bank. These firms obtained more credit than comparable firms working with several banks, possibly as a result of banks decision of "evergreening" their loans.

We therefore find relatively large job losses arising from credit constraints arising from a very large credit supply shock, which are nevertheless very narrow estimates, since they do not capture the cost of the poor risk management of Spanish savings banks that fueled the activities of firms without due concern for their ability to repay.

# A Appendix 1. Definitions of variables and descriptive statistics

**Employment.** It is measured as the average level over the year, weighting temporary employees by their number of weeks of work. The Temporary Employment Ratio is the ratio of temporary to total number of employees; for matching it is defined as 1 above the median.

**Treatment variable.** The Weak Bank Treatment (0-1) is equal to 1 if the ratio between the total value of a firm's loans from weak banks, i.e. banks bailed out by the Spanish Government, as indicated by Appendix 1, and its book value in 2006 is above the third decile of the cross-sectional distribution of firms with a strictly positive exposure to weak banks.

**Province.** There are 50 provinces. For matching the dummy is set to 1 for the East coast of the Spanish Peninsula, namely Girona, Barcelona, Tarragona, Castellón, Valencia, Alicante, Murcia, Almería, Granada, Málaga, Cádiz, Huelva, plus the islands: Baleares, Las Palmas, and Santa Cruz de Tenerife.

**Industry.** We exclude firms belonging to the following sectors (the name is preceded by the 3-digit Spanish Industrial Classification of Activities, CNAE 93, and in parentheses we show the percentage of output sold to Construction and Real Estate in 2000): 14 Extraction of Non-metallic Minerals (35.2%), 20 Wood and Cork (21.1%), 265 Cement, Lime, and Plaster (46.4%), 262-264 Clay (60.1%), 266-268 Non-metallic Mineral Products n.e.c. (85.4%), 28 Fabricated Metal Products except Machinery and Equipment (23.3%), 29 Machinery and Electric Materials (19.2%), 71 Rental of Machinery and Household Goods (26.2%).

There are nine Industry dummies, defined as follows (with the excluded subsectors in parentheses): Agriculture, Farming, and Fishing; Mining (exc. 14); Manufacturing (exc. 20, 262-268, 28, and 29); Electricity, Gas, and Water; Trade; Hotels and Catering; Transport, Storage and Communications; Rental of Machinery, Computing and R&D (exc. 71); Other Service Activities. For matching, the dummy takes on the value 1 for the first two industries.

**Age.** The Firm's Age is defined as Current year minus year of creation of the firm. For matching it is set to 1 above the median.

**Balance sheet and income statement control variables.** They are the following (flows are in nominal values and stocks in book values in December of each year): Firm Size (Total Assets), 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), and Uncollateralized Loans (Uncollateralized Loans/Total Bank Debt). For triple differences, a Small Firm is defined as one with Total Assets below 10 million euros. For matching they are set to 1 when above the median.

**Credit-related control variables.** Credit Line (the firm has at least one), Current Defaults (has any nonperforming loan in 2006), Past Defaults (any nonperforming loan over 2002-2005), Loan Applications (any over 2002-2005), All Applications Accepted (over 2002-2005), Loan Applications Rejected (any over 2002-2005). For triple differences the following composite variable is used: Defaults = Current Defaults + Past Defaults.

**Banking relationship control variables.** Banking Relationships (number of banks with outstanding loans) (for matching set to 1 for multiple-bank firms), Duration of Banking Relationship (with Main Bank, in years), and Main Bank (bank with the largest amount lent).

**Composition of the sample of firms by period.** Total: 217,025. Breakdown: (a) Both in 2006 and 2010, 153,369 (70.7%); (b) In 2006 but had closed down by 2010, 17,088 (7.9%), (c) In 2006 but not observed in 2010, 45,570 (21.0%), and (d) Available only in 2010 (other variables observed in 2006, but not employment), 998 (0.5%).

**Table A1. Returns on securities issued by Spanish Banks in 2006**
Dependent Variable: Coupon differential in basis points

| | Coeff. | s.e. |
|---|---|---|
| Constant | 16.98 | 17.78 |
| Weak Bank dummy | 2.84 | 4.74 |
| Deal Type (Ref. Asset Backed Securities) | | |
| Mortgage Backed Securities | 15.55 | 0.29 |
| Years to Maturity | 0.83 | 0.13 |
| Risk Categories (Ref. Prime) | | |
| Investment Grade | 24.37*** | 2.35 |
| Speculative Grade | 131.01*** | 25.17 |
| Collateral Type (Ref. Auto Receivables) | | |
| Collateralized Debt Obligaion | 0.32 | 17.61 |
| Customer Loans | 2.76 | 7.95 |
| Corporate Loans | 5.55 | 14.16 |
| Residential Mortgages | -18.90** | 8.82 |
| Dummy (1 = No Guarantor) | -5.65 | 6.96 |
| Guarantor Type (Ref: Central Government) | | |
| Private Sector Bank | 13.33 | 13.43 |
| State/Provincial Authority | -4.41 | 10.56 |
| Supranational | 4.65 | 5.43 |
| $R^2$ | 0.44 | |
| No. of observations | 255 | |

Note. 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. Data for 24 issuer parents drawn from Dealscan. The risk ratings of individual deal tranches are grouped in three categories: prime (AAA), investment grade (AA+ to BBB-) and speculative (BB+ to D). Month of issue dummies are included. Standard errors are adjusted for 24 clusters in the issuing bank.

## Table A2. Descriptive statistics of the main sample of firms (2006)

| | Control | | | | Treated | | | | 2-sample t-test | |
|---|---|---|---|---|---|---|---|---|---|---|
| Variable | Mean | S. D. | Min. | Max. | Mean | S. D. | Min. | Max. | Diff. | \|t\| |
| Firms | | | | | | | | | | |
| No. of Firms | 155,167 | | | | 60,860 | | | | | |
| Loans with weak banks | 0.01 | 0.20 | 0.00 | 0.06 | 0.25 | 0.17 | 0.06 | 0.99 | | |
| Share loans weak banks | 0.10 | 0.25 | 0.00 | 1.00 | 0.71 | 0.29 | <0.01 | 1.00 | 0.61 | 438.06 |
| Employment | 24.63 | 327.38 | 1.00 | 64,141.00 | 18.73 | 134.94 | 1.00 | 17,624.00 | -5.91 | 4.31 |
| Firm Size (million euros) | 5.08 | 101.32 | <0.01 | 17,843.33 | 3,01 | 22.80 | <0.01 | 1,871.56 | -2.07 | 4.99 |
| Firm Age (years) | 12.16 | 9.58 | 0.00 | 138.00 | 11.01 | 8.37 | 0.00 | 136.00 | -1.15 | 25.89 |
| Own Funds | 0.33 | 0.24 | 0.00 | 1.00 | 0.24 | 0.18 | 0.00 | 0.93 | -0.10 | 90.33 |
| Liquidity | 0.12 | 0.15 | 0.00 | 1.00 | 0.09 | 0.12 | 0.00 | 1.00 | -0.04 | 52.28 |
| Return on Assets | 0.06 | 0.11 | -0.34 | 0.63 | 0.05 | 0.09 | -0.34 | 0.63 | -0.01 | 27.52 |
| Temporary Employment | 0.21 | 0.26 | 0.00 | 1.00 | 0.24 | 0.27 | 0.00 | 1.00 | 0.03 | 19.85 |
| Bank Debt | 0.32 | 0.27 | 0.00 | 1.00 | 0.50 | 0.23 | 0.06 | 1.00 | 0.19 | 150.75 |
| Short-Term Bank Debt | 0.48 | 0.41 | 0.00 | 1.00 | 0.44 | 0.37 | 0.00 | 1.00 | -0.04 | 18.99 |
| Long-Term Bank Debt | 0.22 | 0.36 | 0.00 | 1.00 | 0.31 | 0.37 | 0.00 | 1.00 | 0.09 | 51.83 |
| Uncollateralized Loans | 0.81 | 0.34 | 0.00 | 1.00 | 0.72 | 0.36 | 0.00 | 1.00 | -0.09 | 55.65 |
| Credit Line | 0.68 | 0.47 | 0.00 | 1.00 | 0.70 | 0.46 | 0.00 | 1.00 | 0.02 | 8.87 |
| Banking Relationships | 1.94 | 1.55 | 1.00 | 61.00 | 2.98 | 2.69 | 1.00 | 59.00 | 1.03 | 111.37 |
| Current Defaults | 0.00 | 0.06 | 0.00 | 1.00 | 0.01 | 0.08 | 0.00 | 1.00 | 0.00 | 10.30 |
| Past Defaults | 0.02 | 0.13 | 0.00 | 1.00 | 0.03 | 0.17 | 0.00 | 1.00 | 0.01 | 17.56 |
| Loan Applications | 0.55 | 0.50 | 0.00 | 1.00 | 0.69 | 0.46 | 0.00 | 1.00 | 0.14 | 58.00 |
| All Applications Accepted | 0.22 | 0.42 | 0.00 | 1.00 | 0.26 | 0.44 | 0.00 | 1.00 | 0.04 | 17.94 |

| | Control | | | | Treated | | | | 2-sample t-test | |
|---|---|---|---|---|---|---|---|---|---|---|
| Variable | Mean | S.D. | Min. | Max. | Mean | S.D. | Min. | Max. | Diff. | \|t\| |
| Banks | | | | | | | | | | |
| Bank Size (billion eur) | 121,54 | 111.82 | 0.01 | 300.19 | 61.91 | 77.38 | 0.02 | 300.19 | -59.63 | 120.71 |
| Own Funds | 0.05 | 0.02 | 0.00 | 0.71 | 0.05 | 0.02 | 0.00 | 0.71 | -0.01 | 79.58 |
| Liquidity | 0.13 | 0.06 | 0.00 | 0.94 | 0.13 | 0.06 | 0.00 | 0.91 | -0.02 | 69.10 |
| Return on Assets | 0.01 | 0.00 | -0.04 | 0.05 | 0.01 | 0.00 | -0.04 | 0.05 | 0.00 | 100.02 |
| Non-performing Loans | 0.01 | 0.01 | 0.00 | 0.42 | 0.01 | 0.01 | 0.00 | 0.42 | 0.00 | 29.75 |
| Loans to Firms | 0.55 | 0.10 | 0.02 | 1.00 | 0.55 | 0.10 | 0.02 | 1.00 | -0.02 | 31.46 |
| Mortgages | 0.38 | 0.10 | 0.00 | 0.86 | 0.38 | 0.10 | 0.00 | 0.86 | 0.03 | 51.06 |

Notes. Variables are ratios unless otherwise indicated. Loans with weak banks are divided by asset value. Share of loans with weak banks in bank credit. Firm and Bank Size are measured as log(Total Assets), with Total Assets in euros.

**Table A3. The employment effect of weak-bank attachment**
**Estimates for control variables (Table 2, col. 3)**
**Difference in Differences**
Dependent variable: *log (1+Employment$_{it}$)*

| | Coeff. | s.e. |
|---|---|---|
| Firm Size | 0.442$^{**}$ | 0.008 |
| Firm Age | 0.022$^{**}$ | 0.001 |
| Firm Age squared | 0.000$^{**}$ | 0.000 |
| | | |
| Own Funds | -0.131$^{**}$ | 0.018 |
| Liquidity | 0.263$^{**}$ | 0.023 |
| Return on Assets | 0.527$^{**}$ | 0.027 |
| Temporary Employment | 0.476$^{**}$ | 0.016 |
| | | |
| Bank Debt | -0.197$^{**}$ | 0.015 |
| Short-Term Bank Debt | -0.122$^{**}$ | 0.012 |
| Long-Term Bank Debt | -0.083$^{**}$ | 0.020 |
| Uncollateralized Loans | 0.278$^{**}$ | 0.014 |
| Credit Line | 0.068$^{**}$ | 0.007 |
| | | |
| Banking Relationships | 0.051$^{**}$ | 0.005 |
| Banking Relationships Squared | -0.001$^{*}$ | 0.000 |
| | | |
| Current Defaults | -0.296$^{**}$ | 0.022 |
| Past Defaults | -0.125$^{**}$ | 0.016 |
| Loan Applications | -0.019$^{**}$ | 0.006 |
| All Applications Accepted | 0.004 | 0.004 |
| | | |
| $Post$ | 0.018 | 3.931 |
| $WB_i$ | 0.009 | 0.014 |
| $Post \times WB_i$ | -0.062$^{**}$ | 0.009 |
| Constant | -3.580 | 35.360 |

Note. OLS estimates using observations for two years: 2006 and 2010. Firm controls (see Appendix 1 for definitions): Firm Size, Firm Age, Firm Age Squared, Own Funds, Liquidity, Return on Assets, Temporary Employment, Bank Debt, Short-Term Bank Debt, Long-Term Bank Debt, Uncollateralized Loans, Credit Line, Banking Relationships, Banking Relationships Squared, Current Defaults, Past Defaults, Loan Applications, All Applications Accepted. Robust standard errors are corrected for clustering at the firm and main bank level and are reported between parentheses in the second line. Symbols: p<0.01 = **, p<0.05 = *.

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**Table 1. Heterogeneity across banks in exposure to the real estate industry and in change in credit (%)**

| | Weak banks | Healthy banks |
|---|---|---|
| A. Share of loans to the real estate industry in loans to non-financial firms (2006) | | |
| Mean | 63.8 | 33.5 |
| Standard deviation | 10.1 | 23.1 |
| Median | 64.3 | 32.3 |
| 1st decile | 50.6 | 2.9 |
| 9th decile | 76.8 | 64.9 |
| B. Change in new loans to non-financial firms (2006-2010) | | |
| Mean | -45.8 | 4.7 |
| Standard deviation | 17.8 | 195.5 |
| Median | -47.7 | -41.8 |
| 1st decile | -63.8 | -81.3 |
| 9th decile | -17.4 | 58.3 |

Notes. There are 201 healthy and 33 weak banks. Panel B reports values for 10 weak banks, which result from consolidation of the 33 banks existing in 2006. Source: Own computations on banks balance sheet data from Bank of Spain.

**Table 2. The employment effect of weak-bank attachment**
**Difference in Differences**

Dependent variable: *log (1+Employment$_{it}$)*

| | (1) | (2) | (3) Baseline |
|---|---|---|---|
| $Post \times WB_i$ | -0.085** | -0.074** | -0.062** |
| | (0.013) | (0.013) | (0.009) |
| Province and Industry Dummies | yes | yes | yes |
| Firm Controls | no | yes | yes |
| Main Bank Dummies | no | no | yes |
| $Post\times$ Province and Industry Dummies | no | no | yes |
| $R^2$ | 0.009 | 0.489 | 0.494 |
| No. of firms | 217,025 | 217,025 | 217,025 |
| No. of observations | 387,482 | 387,482 | 387,482 |

Note. OLS estimates using observations for two years: 2006 and 2010. Firm controls (see Appendix 1 for definitions): Firm Size, Firm Age, Firm Age Squared, Own Funds, Liquidity, Return on Assets, Temporary Employment, Bank Debt, Short-Term Bank Debt, Long-Term Bank Debt, Uncollateralized Loans, Credit Line, Banking Relationships, Banking Relationships Squared, Current Defaults, Past Defaults, Loan Applications, All Applications Accepted. Robust standard errors are corrected for clustering at the firm and main bank level and are reported between parentheses in the second line. Symbols: p<0.01 = **, p<0.05 = *.

**Table 3. Yearly estimates of the employment effect of weak-bank attachment**
**Difference in Differences**

Dependent variable: *log(1+Employment$_{it}$)*

| | Placebo | *Post* year | | | |
|---|---|---|---|---|---|
| | 2002 | 2007 | 2008 | 2009 | 2010 |
| $Post \times WB_i$ | -0.001 | 0.005 | -0.006 | -0.031$^{**}$ | -0.062$^{**}$ |
| | (0.001) | (0.003) | (0.005) | (0.007) | (0.010) |
| $R^2$ | 0.003 | 0.553 | 0.534 | 0.513 | 0.494 |
| No. of firms | 101,515 | 216,895 | 217,035 | 217,078 | 217,025 |
| No. of observations | 191,948 | 411,345 | 398,819 | 400,573 | 387,482 |

Note. OLS estimates using observations for two years: 2006 and, as *Post* year, subsequently, 2007 to 2010. All specifications include Industry and Province Dummies, their interaction with *Post*, and Main Bank Dummies. Firm controls (see Appendix 1 for definitions): Firm Size, Firm Age, Firm Age Squared, Own Funds, Liquidity, Return on Assets, Temporary Employment, Bank Debt, Short-Term Bank Debt, Long-Term Bank Debt, Uncollateralized Loans, Credit Line, Banking Relationships, Banking Relationships Squared, Current Defaults, Past Defaults, Loan Applications, All Applications Accepted. Robust standard errors corrected for clustering at firm and main bank level are reported between parentheses. Symbols: p<0.01 = **, p<0.05 = *.

**Table 4. The employment effect of weak-bank attachment. Robustness checks**
**Difference in Differences**
Dependent variable: *log(1+Employment$_{it}$)*

A. Timing of controls

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Firms | | Banks | |
| | 2002 | 2005 | 2000 | 2002 |
| $Post \times WB_i$ | -0.059** | -0.061** | -0.035** | -0.049** |
| | (0.010) | (0.009) | (0.006) | (0.007) |
| $R^2$ | 0.488 | 0.500 | 0.526 | 0.517 |
| No. of firms | 106,122 | 150,690 | 99,869 | 136,280 |
| No. of observations | 192,765 | 271,540 | 181,751 | 246,362 |

B. Treatment variable and sample composition

| | (5) | (6) | (7) | (8) |
|---|---|---|---|---|
| | Treatment variable | | | Surviving firms sample |
| | % loans to REI | Committed loans | Bank loans | |
| $Post \times WB_i$ | -0.062** | -0.057** | -0.054** | -0.013** |
| | (0.008) | (0.008) | (0.009) | (0.004) |
| $R^2$ | 0.494 | 0.494 | 0.494 | 0.546 |
| No. of firms | 217,025 | 217,025 | 217,025 | 199,691 |
| No. of observations | 387,482 | 387,482 | 387,482 | 353,060 |

Note. OLS estimates using observations for two years: 2006 and 2010. Cols. (1)-(4) report results for deeper lags for firm controls and main bank. Cols. (5)-(7) change Weak Bank definitions, respectively, to a bank with a high share of real estate loans, to credit committed ratio, and to weak bank loans (numerator only). Col. (8) changes the sample to surviving firms. All specifications include Industry and Province Dummies, their interaction with $Post$, and Main Bank Dummies. Firm controls (see Appendix 1 for definitions): Firm Size, Firm Age, Firm Age Squared, Own Funds, Liquidity, Return on Assets, Temporary Employment, Bank Debt, Short-Term Bank Debt, Long-Term Bank Debt, Uncollateralized Loans, Credit Line, Banking Relationships, Banking Relationships Squared, Current Defaults, Past Defaults, Loan Applications, All Applications Accepted. Robust standard errors corrected for clustering at firm and main bank level in parentheses. Symbols: p<0.01 = **, p<0.05 = *.

**Table 5. The employment effects of weak-bank attachment by industry**
**Difference in Differences**
Dependent variable: *log(1+Employment$_{it}$)*

| | |
|---|---|
| $Post \times WB_i \times$ Agriculture, Farming, and Fishing | -0.021 |
| | (0.021) |
| $Post \times WB_i \times$ Mining | 0.144 |
| | (0.096) |
| $Post \times WB_i \times$ Manufacturing | -0.084** |
| | (0.015) |
| $Post \times WB_i \times$ Electricity, Gas, and Water | -0.076 |
| | (0.043) |
| $Post \times WB_i \times$ Trade | -0.061** |
| | (0.010) |
| $Post \times WB_i \times$ Hotels and Catering | -0.043* |
| | (0.019) |
| $Post \times WB_i \times$ Transport, Storage, and Communications | -0.046** |
| | (0.012) |
| $Post \times WB_i \times$ Machinery Renting, Computing, and R&D | -0.068** |
| | (0.015) |
| $Post \times WB_i \times$ Other | -0.037 |
| | (0.020) |
| $R^2$ | 0.494 |
| No. of firms | 217,025 |
| No. of observations | 387,482 |

Note. OLS estimates using observations for two years: 2006 and 2010. All specifications include Industry and Province Dummies, their interaction with *Post*, and Main Bank Dummies. Firm controls (see Appendix 1 for definitions): Firm Size, Firm Age, Firm Age Squared, Own Funds, Liquidity, Return on Assets, Temporary Employment, Bank Debt, Short-Term Bank Debt, Long-Term Bank Debt, Uncollateralized Loans, Credit Line, Banking Relationships, Banking Relationships Squared, Current Defaults, Past Defaults, Loan Applications, All Applications Accepted. Robust standard errors are corrected for clustering at firm and main bank level. Symbols: p<0.01 = **, p<0.05 = *.

**Table 6. The employment effect of weak-bank attachment**
**Triple Differences with indicators of financial vulnerability**
Dependent variable: *log(1+Employment$_{it}$)*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| $Post \times WB_i$ | -0.043** | -0.058** | -0.029** | -0.072** | -0.010 | 0.093** |
| | (0.007) | (0.009) | (0.006) | (0.009) | (0.033) | (0.034) |
| $Post$ | -0.066** | | | | | -0.060** |
| ×Rejected applics.$_i$ | (0.004) | | | | | (0.005) |
| $Post \times WB_i$ | -0.032** | | | | | -0.013 |
| ×Rejected applics.$_i$ | (0.010) | | | | | (0.010) |
| $Post$×Defaults$_i$ | | -0.247** | | | | -0.227** |
| | | (0.026) | | | | (0.028) |
| $Post \times WB_i$ | | -0.033 | | | | -0.006 |
| ×Defaults$_i$ | | (0.022) | | | | (0.027) |
| $Post$ | | | -0.077** | | | -0.074** |
| ×Short-term debt$_i$ | | | (0.007) | | | (0.009) |
| $Post \times WB_i$ | | | -0.080** | | | -0.071** |
| ×Short-term debt$_i$ | | | (0.012) | | | (0.013) |
| $Post$×Small firm$_i$ | | | | 0.045** | | 0.010 |
| | | | | (0.013) | | (0.015) |
| $Post \times WB_i$ | | | | -0.055 | | -0.120** |
| ×Small firm$_i$ | | | | (0.033) | | (0.033) |
| $Post$ | | | | | 0.041** | 0.030** |
| ×Single bank$_i$ | | | | | (0.004) | (0.004) |
| $Post \times\ WB_i \times$ | | | | | 0.056** | 0.029** |
| Single bank$_i$ | | | | | (0.010) | (0.010) |
| $R^2$ | 0.494 | 0.494 | 0.494 | 0.492 | 0.506 | 0.396 |
| No. of firms | 217,025 | 217,025 | 217,025 | 217,025 | 217,025 | 217,025 |
| No. of observations | 387,482 | 387,482 | 387,482 | 387,482 | 387,482 | 387,482 |

Note. OLS estimates using observations for two years: 2006 and 2010. All specifications include Industry and Province Dummies, their interaction with $Post_t$, and Main Bank Dummies. Firm controls (see Appendix 1 for definitions): Firm Size, Firm Age, Firm Age Squared, Own Funds, Liquidity, Return on Assets, Temporary Employment, Bank Debt, Short-Term Bank Debt, Long-Term Bank Debt, Uncollateralized Loans, Credit Line, Banking Relationships, Banking Relationships Squared, Current Defaults, Past Defaults, Loan Applications, All Applications Accepted. Robust standard errors corrected for clustering at firm and main bank level in parentheses. Symbols: p<0.01 = **, p<0.05 = *.

**Table 7. Credit and the strength of single-bank dependence**

Dependent variable: $\Delta$*log(1+Credit$_{ijt}$)*

| | |
|---|---|
| Share of loans with the bank$_{ij}$ | -0.057 |
| | (0.056) |
| Share of loans with the bank$_{ij}$ $\times WB_i$ | 0.338** |
| | (0.096) |
| $R^2$ | 0.207 |
| No. of firms | 509,800 |
| No. of observations | 3,753,140 |

Note. OLS estimates using observations for all yearly firm-bank pairs for 2007 to 2010. Due to the large sample size, only a random sample of 10% of the observations is used. The specification includes Firm and Bank-Year fixed effects. Symbols: p<0.01 = **, p<0.05 = *.

**Table 8. The effect of weak-bank attachment on loan application acceptance**

Dependent variable: *Loan requested and granted$_{ijt}$*

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Threshold: | ≥70% | ≥80% | ≥90% | 100% |
| Loan share with weak banks in | -0.005 | -0.009* | -0.009 | -0.008 |
| 2002-2006 above the threshold$_i$ | (0.004) | (0.004) | (0.005) | (0.005) |
| $R^2$ | 0.010 | 0.010 | 0.010 | 0.010 |
| No. of firms | 109,172 | 109,172 | 109,172 | 109,172 |
| No. of observations | 240,179 | 240,179 | 240,179 | 240,179 |

Note. OLS estimates for monthly observations for 2008:1-2010:12. All specifications include Industry and Province Dummies, and Bank-time Dummies. Firm controls (see Appendix 1 for definitions): Firm Size, Firm Age, Firm Age Squared, Own Funds, Liquidity, Return on Assets and Past Defaults. Robust standard errors corrected for clustering at firm and bank level in parentheses. Symbols: p<0.01 = **, p<0.05 = *.

**Table 9. The employment effect of weak-bank attachment**
**Exact matching**

Dependent variable: *log(1+Employment$_{it}$)*

| | |
|---|---|
| $Post_t \times WB_i$ | -0.032$^{**}$ |
| | (0.014) |
| No. of strata | 4,822 |
| No. of matched strata | 3,553 |
| $R^2$ | 0.488 |
| No. of firms | 211,284 |
| No. of observations | 377,498 |

Note. Weighted least squares estimates, with weights obtained from the coarsened exact matching method (Iacus et al., 2011) using observations for two years: 2006 and 2010. The strata are based on the following 0-1 dummy variables (see Appendix 1 for definitions): Defaults, Bank Debt, Credit Line, Firm Age, Firm Size, Industry, Long-Term Bank Debt, Short-Term Bank Debt, No. of Banking Relationships, Own Funds, Province, Rejected Loan Application, Return on Assets, and Temporary Employment. Symbols: $p<0.01$ = **, $p<0.05$ = *.

**Table 10. The employment effect of weak-bank attachment**
**Instrumental variables using credit**

| | (1) | (2) | (3) |
|---|---|---|---|
| Dependent variable: | $\Delta log(1{+}Employment_{it})$ | | |
| Regressor: | $\Delta log(1{+}Credit_{it})$ | *I(Rejection)* | *% Accepted* |
| | $0.424^{**}$ | $-2.280^{**}$ | $5.364^{**}$ |
| | (0.098) | (0.461) | (1.193) |
| Overall effect | -0.065 | -0.067 | -0.074 |
| | First stage | | |
| Dependent variable: | $\Delta log(1{+}Credit_{it})$ | *I(Rejection)* | *% Accepted* |
| $d_{2008} \times WB_i$ | $-0.022^{**}$ | $0.014^{**}$ | $-0.005^{**}$ |
| | (0.006) | (0.003) | (0.001) |
| $d_{2009} \times WB_i$ | $-0.095^{**}$ | $0.024^{**}$ | $-0.011^{**}$ |
| | (0.014) | (0.004) | (0.002) |
| $d_{2010} \times WB_i$ | $-0.154^{**}$ | $0.029^{**}$ | $-0.014^{**}$ |
| | (0.016) | (0.005) | (0.003) |
| $p$-value of $F$ test | 0.00 | 0.00 | 0.00 |
| No. of firms | 716,678 | 716,678 | 502,331 |
| No. of observations | 716,678 | 716,678 | 502,331 |

Note. Instrumental variables estimates using observations for two years, 2007 and 2010, in the second stage and for four years, 2007 to 2010 in the first stage. All specifications include Industry and Province Dummies, as well as Firm and Time fixed effects. Robust standard errors are corrected for clustering at firm and main bank level. The $p$-value of the $F$ test for the exclusion restriction is reported. Symbols: $p<0.01$ = **, $p<0.05$ = *.

**Table 11. The employment effect of weak-bank attachment**
**Instrumental variables using pre-dated variables**

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | High weak-bank density province (1988) | | | Exposure to REI (2000) |
| | P50 | P75 | P90 | |
| Dependent variable: $\Delta log(1 + Employment_{it})$ | | | | |
| $Post \times WB_i$ | -0.487** | -0.512** | -0.485** | -0.239** |
| | (0.188) | (0.293) | (0.202) | (0.076) |
| Marginal effect | -0.050 | -0.068 | -0.070 | -0.034 |
| | First stage | | | |
| Dependent variable: $WB_i$ | | | | |
| High weak-bank density$_i$ | 0.034** | 0.034** | 0.042** | 0.032** |
| | (0.004) | (0.004) | (0.006) | (0.004) |
| Dependent variable: $Post \times WB_i$ | | | | |
| $Post\times$ High weak-bank density$_i$ | 0.104* | 0.132* | 0.145* | 0.141** |
| | (0.043) | (0.054) | (0.063) | (0.038) |
| $p$-value of $F$ test | 0.00 | 0.00 | 0.00 | 0.00 |
| No. of firms | 217,025 | 217,025 | 217,025 | 217,025 |
| No. of observations | 387,482 | 387,482 | 387,482 | 387,482 |

Note. Instrumental variables estimates using observations for 2006 and 2010. All specifications include Industry and Province Dummies, their interaction with $Post$, and Main Bank Dummies. Firm controls (see Appendix 1 for definitions): Firm Size, Firm Age, Firm Age Squared, Own Funds, Liquidity, Return on Assets, Temporary Employment, Bank Debt, Short-Term Bank Debt, Long-Term Bank Debt, Uncollateralized Loans, Credit Line, Banking Relationships, Banking Relationships Squared, Current Defaults, Past Defaults, Loan Applications, All Applications Accepted. Cols. (1) to (3) respectively refer to the median, and the 75th and 90th percentiles of the distribution of provinces according to their density of weak banks in December 1988. Col. (4) the exposure of banks to the Real Estate Industry in 2000 used to as an instrument for $WB_i$. Robust standard errors are corrected for clustering at firm and main bank level. The p-value of the F test for the exclusion restriction is reported. Symbols: p<0.01 = **, p<0.05 = *.

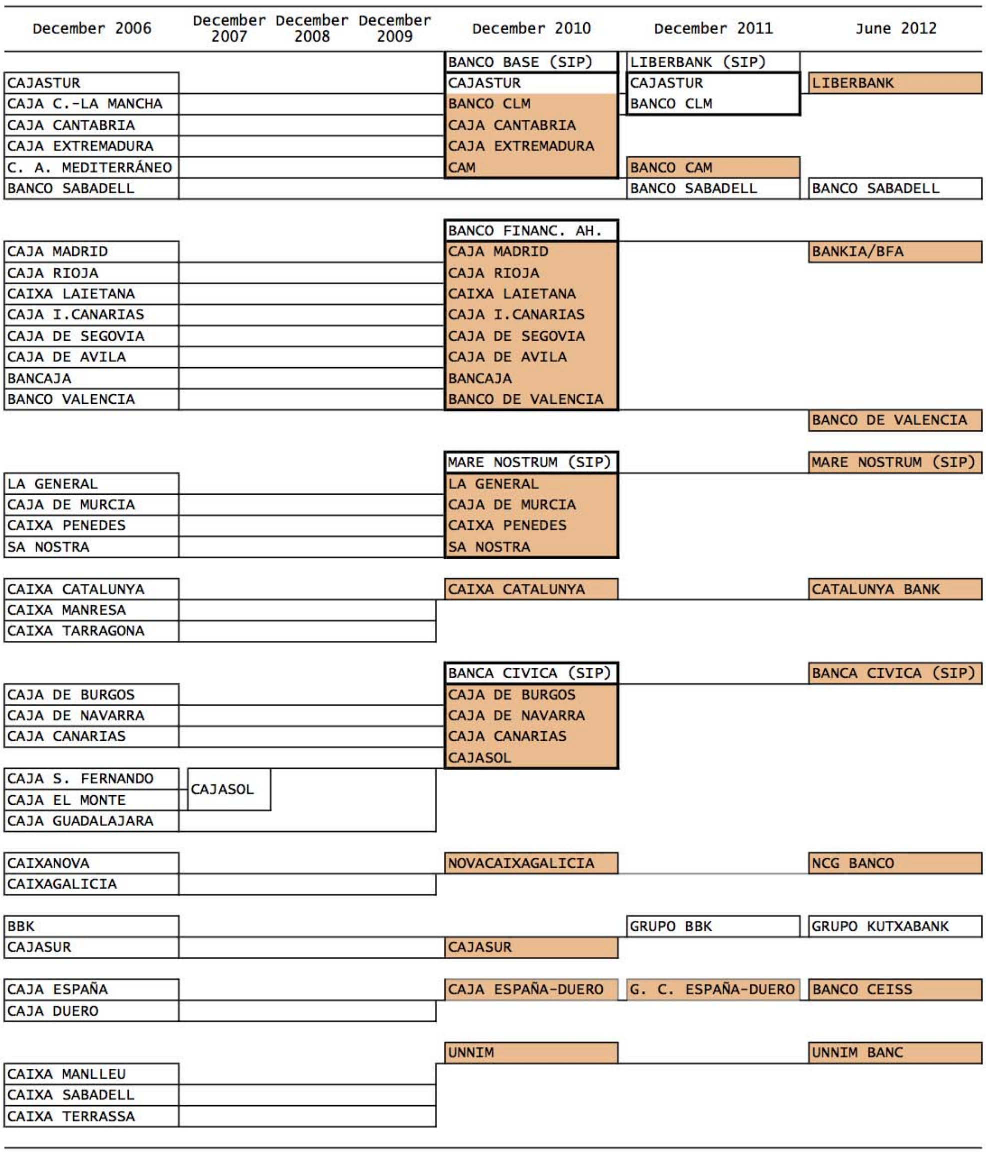


Figure 1: The weak bank bailout process

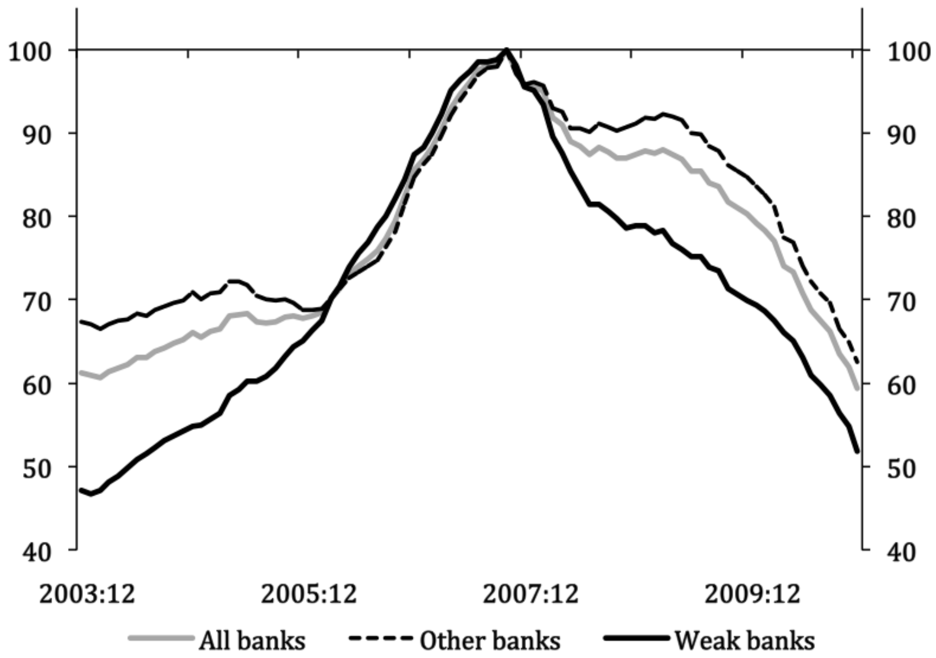


Figure 2: New credit to non-financial firms by bank type (12-month backward moving average, 2007:10=100)

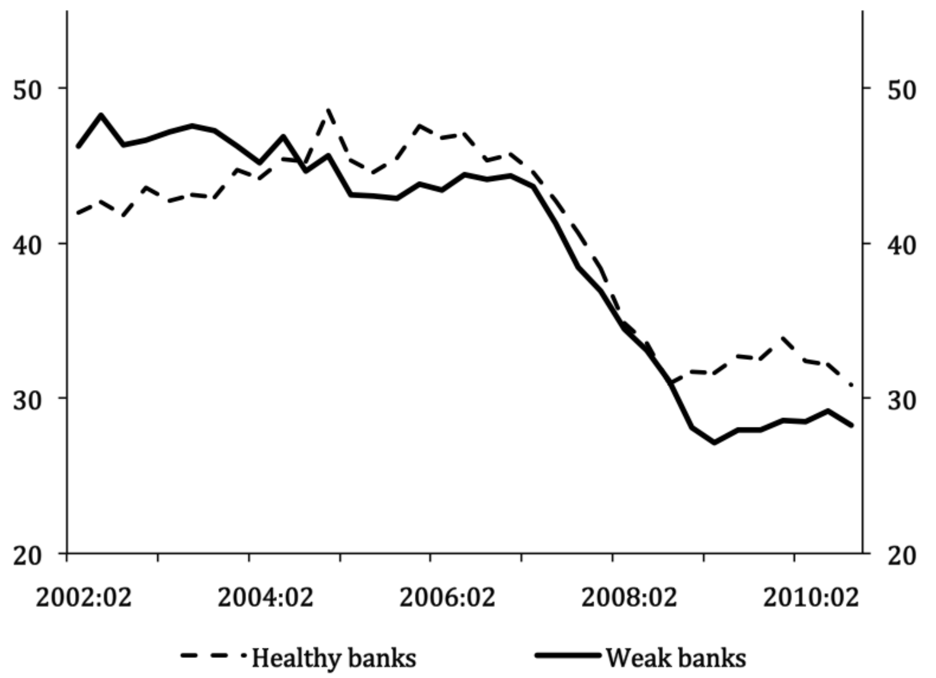


Figure 3: Acceptance rates of loan applications by non-current clients, by bank type (%)

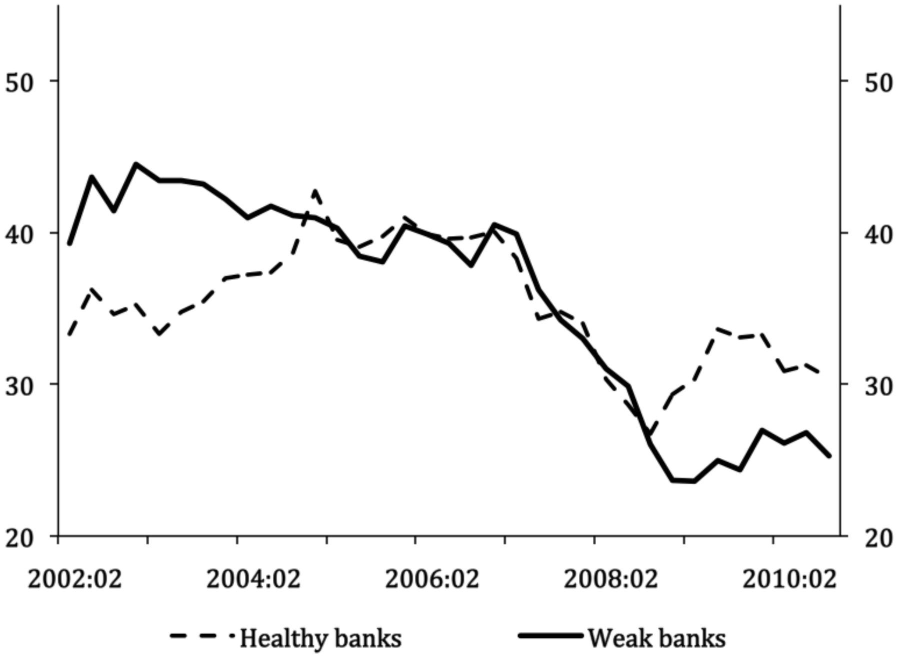


Figure 4: Acceptance rates of loan applications by non-current clients, by bank type. Firms applying to at least one bank per type (%)

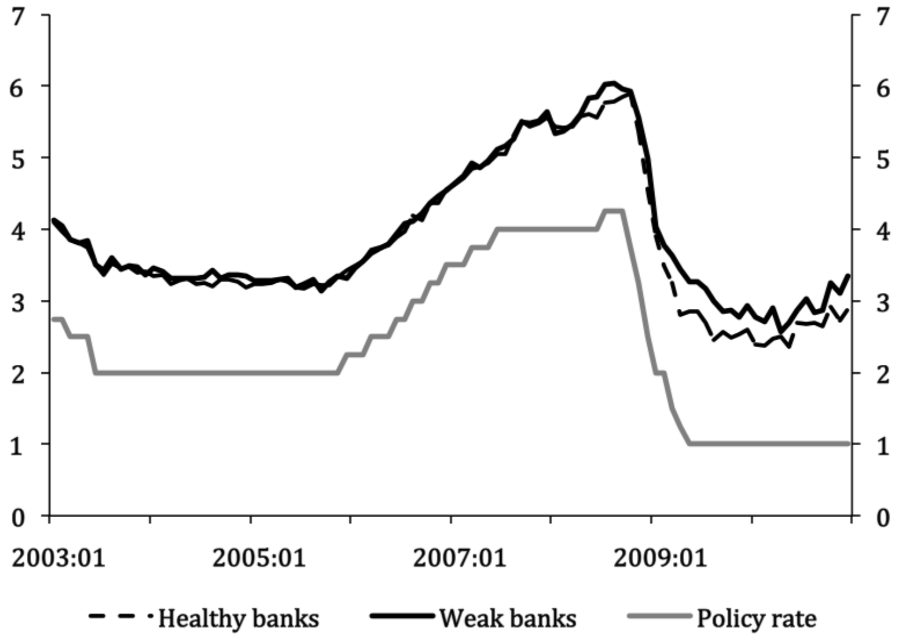


Figure 5: Average annual interest rate for new loans to non-financial firms by bank type and the policy rate (%)

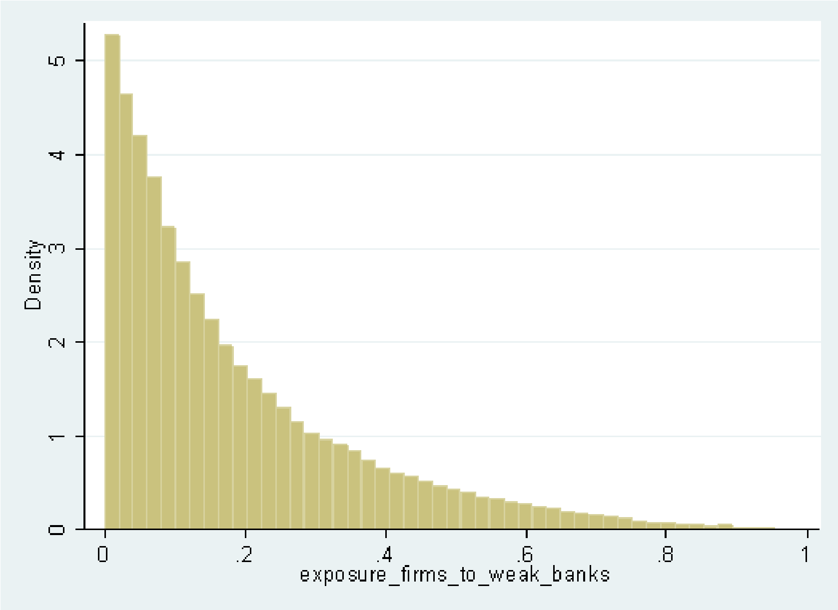


Figure 6: Histogram of the exposure of firms to weak banks (excluding no exposure) (%)

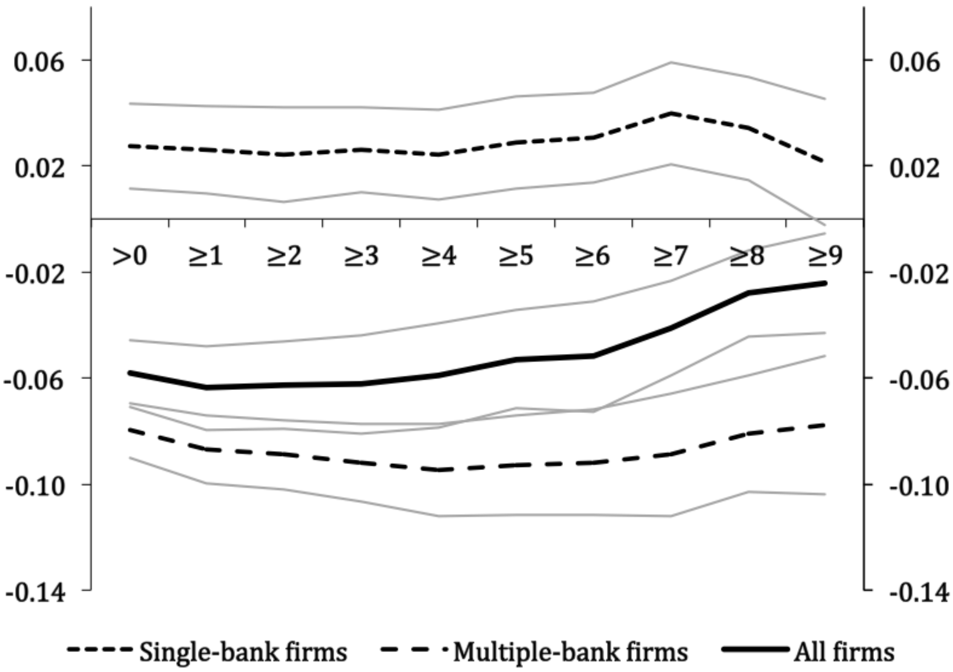


Figure 7: The employment effect of exposure to weak banks by decile and number of banks (DD estimates with 2-s.e. bands)

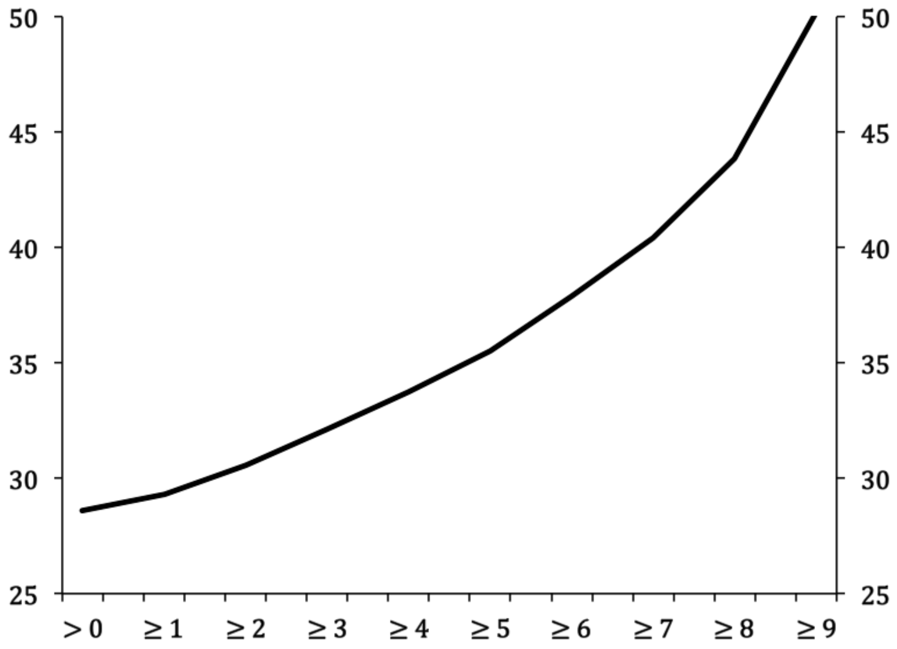


Figure 8: Share of single-bank firms by decile of exposure to weak banks (%)

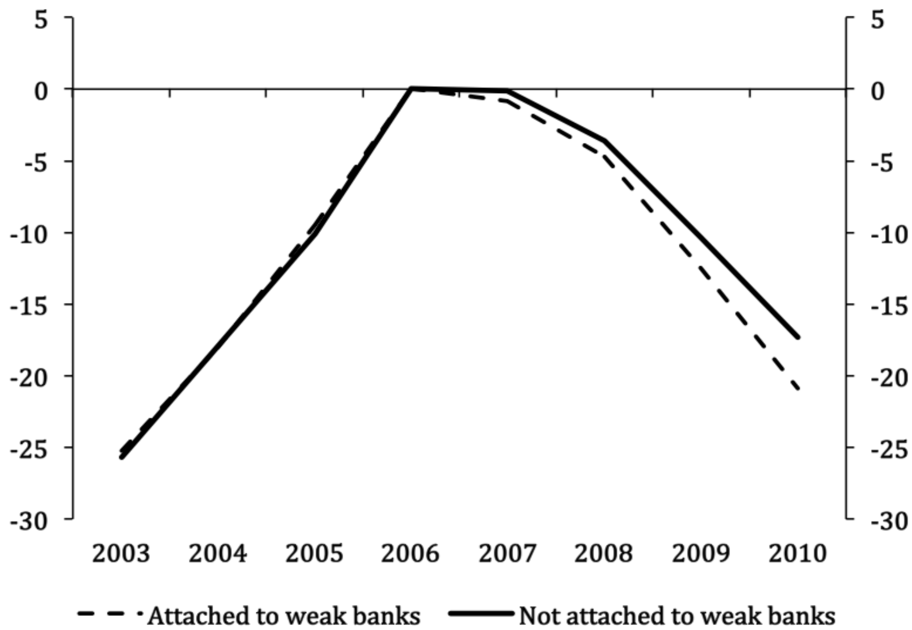


Figure 9: Evolution of employment at firms attached to weak banks and non-attached firms, weighted by matching (2006=0) (%)

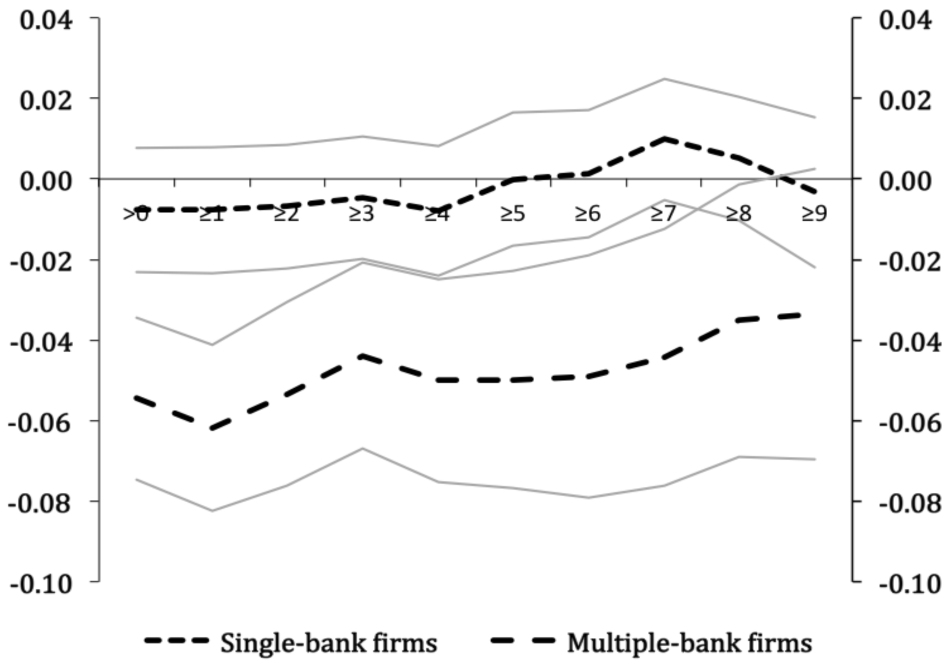


Figure 10: The employment effect of exposure to weak banks by decile and number of banks (matching estimates and 2-s.e. bands)