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 the impact of such a credit shock on employment in Spain. Our identification strategy exploits the marked differences in banks' health at the onset of the crisis. A number of banks with solvency problems have been rescued by the state and we show that these weak banks reduced credit supply more than the other banks. To analyze the implications for employment, we compare the changes in employment from 2006 to 2010 at two groups of firms: those that obtained a significant share of their funding in the pre-crisis period from weak banks and those receiving it from the banks that survived without state support. Our most conservative estimates imply that the firms in the first group suffered an additional fall in employment of between 3 and 6 percentage points due to the stronger fall in credit supply. This corresponds to 15 to 33% of the total difference in the growth rate of employment for the two groups of firms.

Keywords: Job losses, Great Recession, credit constraints.

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1 Introduction

Policymakers in both Europe and the US are concerned about the economic implications of the current shortage of credit. As the International Monetary Fund puts it in a recent review of advanced economies' efforts to revive their credit markets, "policymakers want to support markets because the decline in lending is seen to be a primary factor in the slow recovery" (IMF, 2013). The available evidence on the economic implications of the recent decline in lending is however still rather limited. For some of the most affected countries, like for instance the US, there is a lack of good data on bank credit to firms and this poses a problem for identification.

In this study we contribute to the literature by providing new data that help to resolve the identification problems. Our objective is to estimate the impact of the fall in bank lending on employment in Spain during the Great Recession. The Spanish economy offers an ideal setting to explore this type of question. To start with, Spanish firms rely heavily on bank credit and the high leverage ratio of many firms, mostly SME's, made them vulnerable to the contraction in bank lending that took place during the recession. The origin of this credit shock was a boom-bust cycle in domestic housing prices that had a large impact on bank solvency. Thus, the Spanish example may help us draw lessons that are applicable to other countries, like Ireland or the US, that also experienced a collapse of their housing markets and a strong rise in unemployment. Last but not least, the extraordinary quality of our data allows us to address the challenge of disentangling credit demand from credit supply shocks.

Our dataset contains information from six different sources. We have access to the official credit register of the Bank of Spain (CIR) which contains detailed information on virtually all existing and newly-granted loans to non-financial firms. Using these data we are able to reconstruct the complete banking relationships of over 217,000 companies working with almost 230 banks. We also have information on loan demand through loan applications from non-current customers of banks, and we know wether the applications are granted or not. All this information is linked to the balance sheets of all banks operating in Spain and to the balance sheets and income statements of the firms in our sample. The result is, as far as we know, the most comprehensive matched firm-loan-bank data set ever assembled to estimate the real effects of shocks to the banking system.

Our empirical strategy exploits the large differences in lenders' health at the onset of the crisis. The collapse of the housing bubble affected all Spanish banks, but the impact on banks' solvency was far from uniform and only a subset of the banks, all but one of them savings banks (called *Cajas de Ahorros* in Spanish), needed to be rescued by the State. Before the crisis, these bailed-out or *weak banks* accumulated a disproportionately large share of the loans to the real estate industry, and between the outbreak of the crisis and the first bail-outs in late 2010 these same banks reduced credit more than the other banks. To capture the real effects of this credit supply shock, we compare the changes in employment from 2006 to 2010 at two sets of firms: those with a high and those with a low exposure to weak banks, where exposure is measured as the pre-crisis ratio between a firm's loans from weak banks and its asset value.

The underlying assumption is that the client firms of weak banks could not predict this credit shock when they configured their banking relations. In addition, they must not have been able to readily switch to healthier banks after the outbreak of the crisis. We will provide evidence to corroborate both claims. Finally, it is important to stress that we removed from our sample all the firms belonging to the real estate industry (henceforth REI) — namely, construction firms and real estate developers — as well as those selling a significant share of their output to this industry. In doing so, we avoid the risk of reverse causality.

Our final goal is to replicate as closely as possible the conditions of a natural experiment in which some of the firms are randomly assigned to weak banks and others to healthy banks. This strategy requires the possibility of comparing firms in many dimensions, in order to achieve homogeneity between treated and control firms. In our benchmark, we estimate the impact of weak-bank attachment on employment — the so-called average treatment effect on the treated (ATT) — using a difference-in-differences specification with a large set of firm controls. But in order to minimize the risk of selection, we also make use of matching estimators. Furthermore, we show that there is a causal link between the differences in employment growth for the firms in the treatment and control group and their access to credit during the crisis. In these exercises, weak-bank attachment is used as an instrument for the observed changes in credit and the predicted changes are subsequently used to explain the differences in the growth of employment at the firm level. Finally, in what is our most ambitious test, we check on the potential endogeneity of banking relationships by exploiting a change in banking regulation in 1988. This legal change liberalized the location decisions of savings banks, allowing them to expand freely beyond their region of origin. Our data allow us to calculate the share of bank branches at the municipal level that belonged to weak banks right before this legal change, and we use this variable as an instrument for weak-bank attachment in 2006.

Regardless of the approach followed, we find the same qualitative result. Firms with a relatively large exposure to weak banks at the start of the crisis destroyed a larger share of their jobs between 2006 and 2010 than other firms. Once selection effects are controled for, our estimates indicate that they destroyed an additional 3.0 to 13.5 percentage points, i.e. 7.4% to 16% larger job losses. We also find large differences across firms belonging to different industries and across firms with different credit histories. Moreover, credit constraints can be shown to have operated mostly by firm closedowns than through employment adjustment at surviving firms.

The rest of the paper is organized as follows. In Section 2 we review previous theoretical and empirical work on our topic 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 key results, and Section 6 presents an extensive battery of robustness checks. Section 7 contains our conclusions. Appendix 1 provides some details on the variables used.

2 Theory and literature review

Our identification strategy requires the existence of some financial market friction. In particular, as explained above, the firms attached to weak banks must not have been able to readily switch to healthier banks. Two separate strands of the literature provide the theoretical basis for this result.

The literature on financial accelerator mechanisms has shown that endogenous changes in credit market conditions may amplify shocks to the real economy. In these models, asymmetric information drives a wedge between the cost of internal and external funds that depends negatively on a firm's net worth. Negative shocks to net worth are therefore associated with a rise in the external finance premium and this may force firms to cut back on their scale of operation (the *net-worth effect*).[1] Furthermore, the theory suggests that the firms with weak balance sheets should be the main victims of this tightening of credit constraints (*the flight to quality*).

The bulk of the financial accelerator literature treats financial intermediation as a veil, but Gertler and Kiyotaki (2009) illustrate how the theory can be adapted to incorporate agency problems between capital-constrained banks and their lenders. In their setup, a negative shock to the net worth of banks may generate a disruption of both the inter-bank market and the credit flow from banks to firms. The result is an inefficient allocation of capital and a fall in investment. It is important to stress that the fall in credit supply is not necessarily uniform. Banks with a relatively high leverage ratio are more vulnerable to shocks and this has negative repercussions for their clients.

The second line of research focuses on the role of relationship banking (*e.g.* Freixas, 2005). Banks that repeatedly interact with the same client often acquire soft information that allows a better assessment of the firm's future profitability. This explains why banks may give a preferential treatment to their incumbent clients when capital is scarce and why a profitable firm may not be able to find alternative sources of funding.

The same literature is less clear-cut on the optimal number of banking relations. A strong relationship with a single bank reduces transaction costs and makes it easier to restructure the firm's debt in case of financial distress. But attachment to a single bank may also impede the firm to undertake a profitable project due to financial distress on the part of the bank, as the presence of asymmetric information may impose high switching costs for borrowers. Firms may therefore prefer to establish relationships with several

[1] The initial studies focused on investments in physical capital. For early contributions that consider the effect of financial constraints on employment, see Greenwold and Stiglitz (1993) or Sharpe (1994).

banks to insure themselves against this type of liquidity risk (Detragiache *et al.*, 1990).

Finally, firms that are more prone to suffer from credit constraints can use several strategies to reduce the impact of these future constraints. One option is to maintain a buffer stock of liquid assets. Another option is to maintain a fringe of flexible workers on fixed-term contracts. Ex ante this makes firms less vulnerable to financial shocks, but ex post it may also provoke quick and sizeable adjustments in employment levels (Caggese and Cuñat, 2009).

Moving now to the empirical literature, in recent years there has been a surge of studies exploiting quasi-experimental techniques to estimate the real effects of credit supply shocks. Broadly speaking, we can divide them into three groups depending on their identification strategy. A first strand of papers exploits the heterogeneous impact of large external shocks to banks in the US (*e.g.* Chava and Purnanandam (2011) or Benmelech *et al.* (2012)). A second line of work exploits cross-sectional differences in the financial vulnerability of firms at the start of the Great Recession. Almeida *et al.* (2011), Benmelech *et al.* (2011), and Boeri *et al.* (2013) exploit differences in the debt maturity structure of firms. Since this maturity is often determined years in advance, it leads to fairly exogenous differences in firms' refinancing needs at a time when capital becomes very scarce. 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.

The third route, which is the one adopted here, is to exploit cross-sectional differences in banks' health. 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 national level and the predetermined credit market share of these banks at the county level. They find that this measure is highly predictive of the considerable reduction in county-level credit to small, standalone firms and in their employment levels in the period going 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. This data is matched to confidential data from the Bureau of Labor Statistics Longitudinal Database for a sample of just over 2,000 firms. 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 clients of healthier banks. By contrast, for larger companies there are no significant effects.

It should be stressed that none of the above papers have access to a credit register. Nor do they have access to information about the loan applications or the credit history of firms. As explained in the Introduction, the access to loan level data with detailed financial information about lenders and borrowers is crucial to identify shocks to credit supply. Moreover, our loan application data are a unique source of information about the extent of the credit constraints faced by Spanish firms. They allow us not only to control for cross-sectional differences in the financial vulnerability of both borrowers and lenders, but also to perform a wide range of robustness tests that cannot be replicated with the available data for the US. Finally, our sample of firms is roughly one-hundred times bigger than the one in the closest-related study of Chodorow-Reich (2012) and it predominantly contains SME's that according to the theory are most susceptible to changes in credit market conditions.

3 The financial crisis in Spain

The Spanish economy has experienced an acute credit crunch in the Great Recession. In this section we briefly document the magnitude and the origins of this credit crunch, focusing on the role played by weak banks. We end with some evidence that shows that financial markets failed to anticipate the economic troubles of these weak banks.

3.1 The credit collapse

Spain provides an ideal setting to study credit constraints arising from bank credit supply shocks. To start with, Spanish firms rely more on bank credit than their counterparts in most other developed countries. For example, in 2006 the stock of loans from credit institutions to non-financial corporations represented 86% of GDP vis-à-vis 62% on average in the EU.[2] Moreover, alternative sources of funding are hard to come by. In particular, corporate debt issue is not an option: over the period 2002-2010 on average only five very large companies issued debt in the market each year. And very few firms are quoted in the stock market. Indeed, our sample only contains 28 listed firms (i.e. 0.01% of all the firms in our sample).

Secondly, the latest Spanish business cycle coincided with a boom-bust cycle in the credit market. The Spanish economy experienced an expansion from 1996 to 2007, with GDP and employment respectively growing at 3.7% and 4.1% per annum. By contrast, during the Great Recession, GDP fell by 1.1% per annum over 2008-2010 and by the end of 2010 employment had fallen by 10%, while the unemployment rate had soared from 8.6% to 20.3%. At the same time, credit grew very rapidly during the boom and fell precipitously in the bust. The annual average flow of new credit to non-financial firms by deposit institutions increased in real terms by 23% from 2003 to its peak in 2007, to subsequently fall by 38% in the period until 2010.

The credit crunch resulted from the interaction of the international financial crisis and domestic events. During the boom, the expansionary monetary policy pursued by the European Central Bank (ECB) induced Spanish banks to take on more risks (the *risk-taking channel* of monetary policy). In particular, they fueled a housing market bubble with cheap loans to real estate developers and construction companies –real estate industry, REI, hereafter–, as well as homeowners (Jiménez *et al.*, 2013). The stock of loans to the REI grew from 14.8% of GDP 2002 to 43% in 2007. As a result, housing prices rose by 56% in real terms over 2003-2007, while by the end of 2010 they had fallen by 15%.

Two features of the Spanish banking regulation helped to protect banks in the initial stages of the crisis (Jiménez *et al.*, 2012b). The Spanish regulator, namely the Bank of Spain, had forced banks to keep securitized assets on their balance sheets and in 2000 it implemented the system of dynamic provisioning. This macroprudential tool obliged

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

banks to build up provisions against unrealized loan losses. The banks had however funded a significant fraction of their new lending by issuing debt abroad and they were therefore acutely hit by the freezing of wholesale Eurozone markets in 2008. The European Central Bank offered relieve to banks throughout the Euro area, but the losses at REI firms increasingly threatened the solvency of many banks and this induced them to curtail lending.

What happened to interest rates? The average interest rate on loans to non-financial companies rose from 3.3% in 2005:11 to 5.9% in 2008:09 –closely following the path of the ECB's policy rate. However, it steadily fell thereafter to 2.4% in 2010:5, rising again to 3% by the year's end. Thus, while there was tightening at the beginning of the recession, it was sharply reversed upon Lehman Brothers' bankruptcy. For this reason, in our empirical approach we focus on the volume of credit rather than on interest rates.

3.2 The demise of the savings banks

As explained in the Introduction, the buildup of risks was not uniform across banks, with major risks being concentrated in the savings banks. One factor that may have contributed to the differential buildup of risks is the peculiar governance of savings banks. The savings banks were not exposed to the same market discipline as private banks as they were not listed on the stock market and *de facto* they were controled by the corresponding regional government (see Cuñat and Garicano, 2010).

Solvency problems at savings banks eventually had to be dealt with through State bailouts. These entailed either a merger of banks or a solvent bank taking over an ailing bank, usually with loans and guarantees from the public sector, or a bank's nationalization and recapitalization, sometimes followed by reprivatization via auction (see Table A1 for a summary).[3]

In the period between 2006 and 2011 the number of savings banks went down from 47 to 11, but over our sample period (2006-2010) nationalization only affected two very small

[3] Weak banks are defined as those that obtained funding to remain alive or to be acquired, not banks receiving funds to acquire other banks.

savings banks.[4] Throughout the rest of the analysis we define weak banks as those banks that obtained funding from the state in order to survive. We refer to the rest of the banks as healthy banks and this group includes the banks that received funds to acquire ailing banks. It is important to stress that the set of weak banks only includes one private bank. To fund the recapitalization of these banks, the Spanish Government obtained a loan of 41.4 billion euros (around 4% of GDP) from the European Financial Stability Facility in June 2012.

Our empirical strategy exploits the differences between weak (bailed-out) and healthy banks. In 2006 the former accounted for about one-third of outstanding credit to the non-financial sector. Yet, as shown in Panel A of Table 1, while the REI represented one-third of loans at healthy banks, it comprised almost two-thirds at weak banks. This explains the considerable differences in lender health at the onset of the crisis.

Furthermore, credit grew more at weak than at healthy banks during the boom –in real terms, 60% v. 12% from 2003 to 2007– and it fell more during the slump –46% v. 35% from 2007 to 2010. Looking at individual loans, new credit to non-financial firms fell by 46% from 2006 to 2010 for weak banks and 5% for healthy banks (Panel B of Table 1). These evolutions stemmed from changes in both the extensive margin (credit to firms that were new to the bank) and the intensive margin (new credit to current clients). Figure 1 depicts acceptance rates for loan applications by non-client firms. As a rough control for firm quality, we report acceptance rates for firms applying simultaneously to at least one weak and one healthy bank. During 2002-2004 acceptance rates were 6.5 pp higher for weak than for healthy banks, then both rates fell precipitously during 2007-2008, and subsequently acceptance rates switched to being 6.3 pp lower for weak banks in 2009-2010.[5]

[4] See International Monetary Fund (2012) or "The restructuring of the banking sector in Spain" at bde.es/bde/en.

[5] Results from a panel regression for loan approvals, controlling for firm and month fixed effects, their interaction, and bank characteristics (clustering standard errors at the bank level) shows that the approval rate for weak banks was above that for healthy banks from 2002:02 to 2004:12, by 4.6 pp (s.e. 1.6), and below during 2008:12-2010:12, by 7.6 pp (s.e. 1.2).

3.3 Were weak bank troubles anticipated?

The differential buildup in risks at the two sets of banks is striking. So, in this section we ask the question whether firms could predict the solvency problems of the weak banks. Such anticipation effects would invalidate our identification procedure.

To study this issue, we analyze the risk premia charged to Spanish banks' securitization issues prior to the recession. We employ data on tranches of mortgage backed securities (MBS) and asset backed securities (ABS) in 2006. During this year the ratio of securitization to total assets was equal to 16.7% for weak banks and 13.5% for healthy banks. Thus, in relative terms weak banks depended more on securitization than healthy banks.

We group the 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) with a floating-rate, quarterly coupon frequency, and referenced to the 3-month Euribor from Dealogic. Our final sample contains securities by 24 issuer parents.

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

4 Data

In this section we briefly describe the variables included in our matched firm-loan-bank data set. Further details on procedures and and definitions are provided in Appendix 1.

We end this section with a description of our treatment and control variables.

4.1 Data set construction

As noted, a negative aggregate shock may reduce both credit supply and demand. To disentangle them, it is essential to observe both bank and firm characteristics and, in particular, to have exogenous measures of firms' vulnerability to bank credit shortages. Our data set contains such information. It combines six separate sources on firm-level economic and financial data, firm creation and destruction, individual firms' loan applications to banks, individual loans granted and their subsequent history, and bank balance sheets and location.

We gather economic and financial information for more than 300,000 private, non-financial firms from balance sheets and income statements that Spanish corporations must submit yearly to the Spanish Mercantile Registers. These data are extracted from the Iberian Balance sheet Analysis System (SABI) produced by INFORMA D&B in collaboration with Bureau Van Dijk and the Central Balance Sheet Data Office of the Bank of Spain. In particular, it contains information on employment, measured as a yearly average, as well as on variables like the firm's age, size or indebtedness, which are used as controls in our analysis.

To avoid the risk of reverse causality –so that the troubles of firms drive the solvency of banks–, we exclude firms belonging to the Construction and Real Estate industries, as well as firms in related industries, defined as those that, according to the 2000 input-output tables, sold at least 20% of their output to the first two sectors (see Appendix 1). The date is chosen to minimize potential endogeneity through credit decisions taken in the later part of the expansion. We are left with a sample of 217,025 firms, representing 27% of firms, 37% of value added, and 61% of private sector employees, in the industries included in our analysis, in 2006. We complement this information with data from the Central Business Register on firm entry and exit, so as to disentangle job destruction at surviving firms from that due to firms closing down.

We match these data sets with loan and bank information. The loan information is obtained from the Central Credit Register of the Bank of Spain (CIR), a proprietary database with information on all loans above 6,000 euros (around 8,100 dollars) granted to companies by all banks operating in Spain. Since we only consider loans to non-financial firms and given the low threshold, this data set can be taken as a census. From the CIR we construct exhaustive information on the banking relationships of the firms in our sample and we compute the ratio of loans from weak banks to the firm's asset value, which is our key treatment variable. We also observe the number of bank relationships, collateralized loans, and credit lines, as well as a measure of loan maturity, so that we can control for firms' refinancing needs at the onset of the crisis. Since we are interested in bank credit, we exclude firms with no loans in 2006. We also identify each firm's main bank, defined as the one that accounts for the largest share of a firm's outstanding loans. Though firms' creditworthiness is typically unobservable, in our case information on non-performing loans and, especially, potentially problematic loans is available.

We also employ information on loan applications. All banks receive monthly-updated information from the CIR on their borrowers' credit exposure and defaults vis-à-vis all banks in Spain. But banks can also costlessly obtain this information on any potential borrower, defined as "any firm that seriously approaches the bank to obtain credit". By matching the loan application data set with the CIR we can observe, for each application, whether the loan is granted or not. If a bank does not grant the loan, it either denied it or else the firm obtained funding somewhere else (see Jiménez *et al.*, 2012a). Since the loan application data set only gives information on whether a firm borrowed from its bank(s) if it has a credit history, we exclude loan applications from entering firms.

Lastly, we enlarge our information with two data sets on banks. The first one records their financial statements and it is used by the Bank of Spain for regulatory and supervisory purposes. It includes 226 banks, comprising commercial banks, savings banks, and credit cooperatives. The second dataset contains historical data on the location of bank branches at the municipal level and has never been used for research purposes before.

4.2 The treatment variable and the sample

As already explained, we aim at measuring the employment losses caused by the differential effect of the financial crisis on the lending capacity of banks due to the heterogeneity in their financial health at the onset of the crisis. We do so by comparing the evolution of employment in firms with high and low exposure to weak banks. Exposure is captured by the firm's pre-crisis level of debt with weak banks normalized by its asset value. This ratio jointly reflects the overall leverage ratio and the relative importance of weak banks in the firm's indebtedness.[6]

About one-third of firms had no credit from weak banks. The histogram of those which did in 2006 is shown in Figure 2. In our benchmark treated firms are defined as those above the third decile of this cross-sectional distribution, which implies a weak-bank loan-to-asset ratio of 6.3%. This figure corresponds to a share of weak banks in total bank credit of 51.4%, so that above half of bank credit comes from weak banks. On average, their ratio of credit with weak banks to assets is 25% and their share of bank credit with weak banks equals 71%. Going beyond this benchmark threshold, we will also show that our results qualitatively hold for all other deciles of the distribution.

On average, in 2006 the firms in our sample had 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%, and a bank debt ratio of 37%, and they had loans from slightly more than two banks. 2% of them defaulted on loans over 2002-2005.

Table 2 provides descriptive statistics for our treatment and control groups, revealing different characteristics across groups. 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 have a worse financial profile: they are less capitalized and profitable, they have less liquidity, and they are more indebted with banks. They work with three banks on average and between 2002 and 2005 they defaulted more often on their bank

[6] We focus on drawn credit, but we also check the robustness of our results to the inclusion of undrawn credit in several of our empirical specifications.

loans. Table A3 in the Appendix gives the statistics on all control variables and shows that treated 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. These differences are not always large, but they are all significant, which is not surprising given the large sample sizes. This feature indicates that we must exhaustively control for firm-level characteristics in any empirical exercise, since weak banks were more likely to grant loans to less profitable and potentially more vulnerable firms than healthy banks.

5 Empirical strategy and results

In this Section we first discuss our empirical strategy and then show the estimation results, both for standard difference in differences and for our instrumental variables models.

5.1 Difference in differences

We start with the standard difference-in-differences (DD) approach. In our benchmark we 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 is either 2006 or 2010, WB_i is a dummy variable for treated firms, $Post$ is a dummy variable for 2010, d_S is a joint vector 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 can jointly measure employment changes both at surviving firms and due to firm closures. In additional analyses below we also study them separately.

Our main hypothesis is that firms working more intensely with weak banks in the expansion suffer more stringent credit constraints during the crisis and this translates into larger job losses. We do not intend to estimate all potential effects of credit constraints on employment, but only a partial effect that can be identified as being causal, namely the differential impact of those credit constraints stemming from being attached to a weak bank, as opposed to other banks, which is measured by γ in equation (1).

We aim at isolating 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, so that selection effects which may bias our estimates are absent. The group controls (d_S) and other characteristics (X_i) are intended to achieve such ex-ante homogeneity across firms, allowing allows us to estimate the average treatment effect on the treated (ATT) by comparing firms in the treatment group to similar firms in the control group.

The firm characteristics in X_i are as follows (see definitions in the Appendix): Main Bank Dummies (226), 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, and All Applications Accepted. The rationale for their inclusion is discussed next.

5.2 Threats to identification

The two main challenges for identification are the non-random assignment of firms to banks prior to the crisis and the possibility of firms avoiding treatment through a successful application for loans at healthy banks. The relevance of the first threat is highlighted by the different characteristics of the firms working with weak and healthy banks before the crisis. As shown in Section 4, the firms in the treatment group have worse financial statistics. It therefore seems that laxer loan-approval criteria at weak banks may have caused a systematic bias in the risk profiles of the companies in the treatment group or, alternatively, they may have been a motive for self-selection of firms into weak banks.

The exceptionally rich contents of our data set helps us avoid many threats to identification. To start with, our data go back four years before the outbreak of the recession, so that we can test for differences in pre-existing trends in employment at attached and non-attached firms after conditioning on controls. Secondly, potential biases arising from a different geographical or sectoral concentration of the activities of either borrowers or banks are dealt with by including province and industry dummies in all specifications. Similarly, differences across industries or provinces in the depth of the recession are absorbed by interactions between d_S and the crisis dummy $Post$. For example, coastal areas were more strongly hit than the rest of the regions due to their concentration in construction activities.

We also introduce a set of covariates controling for firm characteristics ex-ante (2006, unless otherwise indicated) that could lead to differential employment outcomes, like the firm's age and its square (to capture nonlinear effects), size (in terms of assets), temporary employment ratio, and rate of return on assets. A second set of variables is linked to financial health, such as a firm's indebtedness with banks and its shares of short-term (up to one year) and long-term bank debt (above 5 years) –intermediate terms being captured by the reference firm. A third set of variables captures the firm's financial vulnerability, several of them serving as direct proxies for expected credit constraints: liquidity and own-funds ratios, the number of past loan applications and an indicator for whether all were accepted, indicators for having any past loan defaults (2002-2005), any current loan defaults, and any credit lines, the number of banking relationships and its square, and the share of loan amounts that are uncollateralized. Lastly, a full set of dummies (226) captures synthetically the characteristics of the main bank that a firm works with.

In the Spanish context it is also vital to control for cross-sectional differences in the share of temporary contracts, which represent almost one-fourth of employment in our sample. These contracts can be terminated at much lower costs than permanent ones and therefore, other things equal, we expect larger employment adjustments at firms with a larger temporary rate. Moreover, firms expecting to face financing constraints in future have an incentive to maintain a buffer stock of temporary contracts (Caggese and Cuñat, 2008).

This rich set of controls allows much better identification than is typical in the literature. The breakdown into 50 provinces affords a more accurate control of firms' location than in research work that uses regions or states (in the US) instead. Moreover, most of the firm characteristics we introduce are simply unavailable in standard data sets. In particular, what makes our exercise exceptional is the use of firms' banking relationships, in terms of the number and identity of the banks they work with, and the proxies for the banks' assessment of a firm's creditworthiness via its credit history: its decisions to apply for loans and its success in such applications, as well as its ability to meet repayment obligations. Lacking this information, researchers have resorted to proxying firms' access to credit either by responses to questions about past loan denials (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 to both attain very high precision in our estimates and apply matching methods using many controls, so that very similar firms, attached and non-attached to weak banks, are being compared. Self-selection through unobservables is however still possible, and we therefore need to rely on the assumption of randomness of the assignment of firms to the control and treatment groups conditional on observables.

Our approach would still be incorrect if treated firms could easily find alternative funding from healthy banks or other sources. As highlighted by the relationship banking framework (see Section 2), banks usually obtain information on firms' profitability and solvency through long-standing relationships. This makes switching banks very costly for firms, since it takes time for other banks to acquire such knowledge. Thus, when the Great Recession arrived, obtaining loans from new banks became harder and many firms were largely limited to the funding provided by banks with which they had long-established relationships. As previously shown, acceptance rates for loan applications at all banks from non-current customers did sharply fall starting in early 2007. Below we will also check whether it was even harder for firms with a high exposure to weak banks to get them approved. Moreover, as shown below in the context of an IV model, committed credit fell significantly more for firms attached to weak banks and neither did bank nor non-bank credit sources allow firms to replace bank lending.

Lastly, it may be objected that the treatment is defined in terms of an outcome, namely bank bailout, that is realized several years after the outbreak of the crisis. The use of an ex-post criterion does not invalidate our results as long as the outcome was not foreseen. And we have shown in Section 3 that expected differences in bank default risk were insignificant, since financial markets did not recognize them in the runup to the crisis. Nevertheless, 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 (see Section 6).

5.3 DD estimates

Table 3 presents the estimation results for our difference-in-differences specification (1). We report robust standard errors corrected for clustering at firm and main bank level, unless otherwise stated, and we typically do not discuss the significance of our estimates, since most are significant at the 1% level. The raw mean difference between the proportional loss of employment at firms in the treatment and control group is equal to 8.5 percentage points (pp hereafter). This figure remains unaltered after the inclusion of province and industry dummies, while it falls to 7.4 pp when firm characteristics are controled for (cols. 1 and 2). Adding main bank dummies and controling for differential trends by province and industry reduces the treatment effect to 6.2 pp (col. 3).[7] We take this as the baseline specification, in particular with respect to the set of control variables.[8] This estimate implies that weak-bank attachment accounts for 7.4% of job losses at treated firms.

We test for differences in the pre-crisis trends for the treatment and control groups by running a placebo equation (1) where we have chosen 2002 and 2006 as initial and final dates. As required, this specification test delivers a coefficient that is not significantly different from zero.

[7] The estimated coefficient did not change when main bank dummies were replaced by either main bank characteristics (those in Table A3) or main, secondary, and tertiary bank dummies.

[8] Table A4 reports the coefficients on the firm control variables obtained for this specification, which are highly significant and show the expected signs.

5.4 Instrumental variables estimates

We wish to ascertain that the impact of weak-bank attachment on employment is driven by credit constraints as opposed to other potential avenues. To this end we estimate the following instrumental variable (IV) model for the proportional change in employment:

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

where all variables are defined as in equation (1), except that $Credit_{it}$ is total credit committed by banks to firm i in year t –both drawn and undrawn, so as to minimize potential endogeneity– , $Post_t$ is a vector of year dummies for t =2007,..., 2010, and d_i is a firm fixed effect. Coefficient μ in the first-stage regression captures the differential impact of weak banks on credit committed during the crisis, whereas δ' captures the passthrough from credit to employment. The exclusion restriction is that working with a weak bank alters employment growth only through credit changes, as opposed to other channels.

This model differs from the DD equation in important ways. First, it is estimated in first differences, as opposed to levels, because –in keeping with the literature– we are better able to explain credit changes than its levels. Secondly, it is a panel of four rather than two periods, so that we exploit information for each recession year and we capture firm-specific characteristics via fixed effects rather than through initial-year control variables.

As shown in the lower panel of Table 4 (col. 1), the instrumental variable is significantly and negatively correlated with credit, increasingly so as the recession lengthens. Credit is also found to be a significant determinant of employment changes, with a large transmission coefficient of 0.42. The product of this second-stage coefficient and the weak-bank effect on credit for 2010 (-0.154) yields an employment reduction of 6.5 pp in 2010 with respect to 2007 (the ommitted year). The effect vis-à-vis 2006 is not identified, but an IV estimation for 2007 alone provided a non-significant coefficient with respect to 2006, so that the effect for 2010 with respect to 2006 is likely to be around 6.5 pp as well. This is very close to our baseline DD estimate of 6.2 pp, in spite of the different nature of the two models. These results support the idea that credit is the key channel through which the weak-bank attachment operates.

In the second column of Table 4 we replace credit growth with an alternative measure of credit constraints, namely an indicator for having a loan application rejected. The effect of weak-bank attachment again increases over time in the first stage, and now the causal effect of a loan rejection is a very large reduction in employment, of about 90% ($1 - e^{-2.28}$=0.90). Note that here we are measuring a local average treatment effect (LATE) for firms on the margin of having a loan approved (Imbens and Angrist, 1994).[9] This finding is not so surprising once we realize that these losses stem from firms closing down, which as already noted represent 77% of aggregate job losses in our sample. In any event, this underlines the need to examine the effect of weak-bank attachment on the probability of exit, as is done in the next Section.

5.4.1 Exogenous variation in exposure to weak banks

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 convincingly rule out selection effects we need an exogenous source of variation in firms' attachment to weak banks. We exploit two variants.

First, we use a regulation-based instrumental variable. Up until 1988 savings banks could open at most 12 branches outside their region of origin, but in December 1988 a new law removed all location restrictions (Real Decreto-ley 1582/1988 of 29 December 1988). In order to better exploit this variation, we compute for each municipality the share of bank branches that belonged to our set of weak banks in December 1988. Our instrumental variable is this weak-bank density in the municipality where the firm is located. This variable should capture exogenous variation in the probability of weak-bank attachment, since it is more likely that a firm will work with a bank if it is located in a municipality where the bank traditionally operates. In Table 5 (col. 1) we see that high weak-bank density in 1988 significantly predicts weak-bank attachment 18 years

[9] Under the monotonicity assumption that access to credit always improves with lender health.

later. The associated employment effect amounts to 8.4 pp, which is higher than the DD baseline value of 6.2 pp, though not significantly so. This estimate gives rise to an attribution of 10% of job losses being due to weak-bank attachment.

Alternatively, we use traditional bank ties to real estate firms to make sure that credit restrictions faced by firms indebted with weak banks do not simply result from poor bank management. Our instrumental variable is now banks' exposure to the real estate industry in 2000, well before the beginning of the bubble, which is commonly thought to have started around mid-2003, see Ayuso and Restoy (2006). The second column reveals that the instrument is very powerful. The employment effect of predicted weak-bank attachment of 13.5 pp is now significantly higher than our DD baseline, implying that a share of 16% of job losses at attached firms being attributed to weak-bank attachment. This finding suggests that to some extent weak banks got into trouble because of their historical ties to real estate firms and not only because they aggressively pursued real estate lending just before the crisis struck.

6 Robustness checks

In this section we check the robustness of our baseline estimates in many ways. The checks are presented in terms of the dimension of variation: (a) timing, treatment variable, and reference population, (b) firms' financial vulnerability, (c) level of exposure to weak banks, (d) probability of exit, (d) measure of credit, and (e) estimation method.

6.1 Timing, treatment variable, and reference population

We first explore the timing of the impact of the credit constraint on firms by choosing alternative ex-post periods. Our estimates are as follows (in pp, s.e. between parentheses): 0.5 (0.3) for 2007, -0.6 (0.5) for 2008, and 3.1 (0.7) for 2009. Thus the weak-bank effect does not become significant until 2009. Secondly, to avoid potential anticipation effects, we progressively restrict the analysis to firms with long-run banking relationships, established years before the outbreak of the crisis. In Table 6 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.

We also shift back the dating of the treatment. The assignment of firms to the treatment is now based on their weak-bank exposure in either 2000 or 2002. This approach involves a tradeoff: it potentially weakens endogeneity concerns but it also brings us farther away from the conditions faced by firms just before the crisis –which are likely to be more relevant to outcomes. As Table 6 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, which are still sizeable and the former is significantly different from the baseline (cols. 3 and 4).

To check whether using a treatment defined by an ex-post event, i.e. the bailout, may be biasing our results, we re-classify banks on the basis of their exposure to the real estate industry in 2006. The latter is measured as the share of a bank's loans that are granted to REI-firms and all banks with an exposure above the third decile of the corresponding distribution across banks are classified as weak banks. This change leads to an estimated employment effect of 6.2 pp (col. 5), which is identical to the baseline, confirming that REI exposure drives weak-bank troubles.[10]

Lastly, we estimate the effect only for surviving firms, i.e. leaving out those that close down. The estimated extra job loss for firms in the treatment group is equal to 1.3 pp, which is much lower than for the full sample. As indicated, firms that close down comprise 77% of overall job losses and compared to continuing firms, they are smaller, younger, less capitalized, and less profitable. This finding indicates that in Spain credit constraints have been more important in driving firm closures than in leading surviving firms to cut jobs. We return to this issue below, when we examine the probability of firm exit.

6.2 Firms' financial vulnerability

In this section we allow for heterogeneity of the treatment effect across firms with different characteristics. To this effect we interact the product of the *Post* dummy and the

[10] Including committed but undrawn loans when constructing the treatment variable leads to a job loss of 5.7 pp (s.e. 0.8 pp).

weak bank dummy with the firm characteristic of interest. We begin with nine industry dummies. For five out of the nine we find significant effects, as follows: Manufacturing (8.4 pp), Machinery, Renting, Computing, and R&D (6.8), Trade (6.1), Transport, Storage, and Communications (4.6), and Hotels and Catering (4.3). These results are quite reasonable, since the effects tend to be larger for industries requiring more capital and therefore typically more credit.

A second set of triple difference specifications examines whether the employment cost of weak-bank attachment depends on a firm's financial vulnerability.[11] The first measure is an indicator for whether the firm had a loan application rejected over 2002 to 2006. Table 7 reveals that these firms suffered an additional loss of jobs of 6.4 pp in the recession but no extra loss if they were very attached to weak banks. Similarly, firms that defaulted on a loan over the same period experience an additional loss in employment of 22.9 pp, though working with a weak bank again does not add to it. Note that, at face value, this estimate implies an employment impact which is almost five times higher than for other treated firms.

Next, firms with a share of short-term bank debt in total debt above one-half in 2006, implying that they subsequently had to renew a sizeable fraction of it, suffer an additional job loss of 9.4 pp, and another 7.1 pp if they worked with weak banks. Further, small firms (defined as those with assets below 10 million euros) suffer an extra 12 pp job losses, but only if they were attached to weak banks (the coefficient of the interaction with *Post* is only significant at the 10% level). These findings are in accordance with standard theoretical predictions that smaller, less transparent, and financially weaker firms should be more vulnerable to changes in credit market conditions. In contrast to the literature, however, our results are based on direct measures of credit constraints and of credit records (such as rejected applications and loan defaults, respectively). A noteworthy result of our analysis is that there are no significant differences in the penalties that imposed on firms with a bad credit record.

The next issue we examine is whether the impact of credit constraints varies with

[11] To avoid having to weigh coefficients by average values, we include regressors as deviations from their means.

the number of banking relations. In particular, we distinguish between firms working with only one bank from the rest. The empirical literature has not reached a robust conclusion on this issue. According to Hoshi *et al.* (1990), in Japan firms whose debt is concentrated with a single bank, within a group of firms or *keyretsu*, have better access to credit in periods of distress. On the contrary, in the case of the US, Houston and James (2001) find that cash-flow sensitivity is larger at firms that establish an exclusive banking relationship. In the case of Spain, we find that job losses at single-bank firms are 3.8 pp lower than at multi-bank firms as shown in Table 7, and another 2.9 pp lower if the single bank is a weak bank. In the next subsection we explore this last finding in more depth.

6.3 Degree of exposure to weak banks

We have so far presented results for the degree of exposure to weak banks using the third decile of the cross-sectional distribution in 2006 as the threshold (which corresponds to having at least half of bank debt with weak banks). To check the sensitivity of our results to this choice, we reestimate equation (1) for firms with any positive loan balances with weak banks and for exposure levels above each decile of the distribution of firms in that set. The picture that emerges from Figure 3 is that the weak-bank effect is present at all deciles and that there is relatively little variation in the estimates for any exposure up to the sixth decile, ranging from 5.2 to 6.3 pp (continuous line). The magnitude however falls for higher deciles.

This decline may reflect a composition effect since the share of firms with a single banking relationship grows from 29% above the first decile of exposure to weak banks to 50% above the ninth decile and we know that these firms suffered smaller employment losses. To check this hypothesis, we reestimate equation (1) separately for single- and multiple-bank firms. The estimates, depicted in Figure 3, are now stable and significantly different: job losses from weak-bank attachment are on average 8.8 pp higher for multiple-bank firms but 2.8 pp lower for single-bank firms.

It may be that single-bank firms are better borrowers. In our sample, they have better ratios of capitalization, liquidity, return on assets, and bank debt, and they are less likely to have defaulted on their debt obligations. Thus they may also be better along other dimensions we have not controled for. Alternatively, there may be an advantage for a firm in working with a single bank, which acquires more information about it and has a stronger stake in its economic success. Some 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 that bank to the firm's asset value– has a positive impact on its propensity to innovate and export. This result suggests that relationship banking helps with funding of innovation and in accessing foreign markets.

To make progress on this issue, we test whether banks treated better those firms that had concentrated their loans with them. Using our firm-bank loan database, we regress the yearly change in credit committed in the recession (2007-2010) on the share of loans of the firm with each individual bank in 2006, including firm and bank-year fixed effects. Since this data set has 37.5 million observations, we restrict ourselves to a 10% random sample. In Table 8 we see that only weak banks extend more credit to single-bank firms. This result hints at an "evergreening" of loans by these banks.

Why would only weak banks behave in this way? On the one hand, during this period weak banks were more closely monitored by the markets than healthier banks, due to their large exposure to real estate, so that they would be more eager to avoid increases in their non-performing loan rate. On the other hand, while obtaining credit became harder for all firms, this effect may have been stronger for firms which were heavily dependent on weak banks. We test this hypothesis of a stigma using our data on loan applications from non-current customers. We previously found no significant effects from weak-bank attachment for 2007, so we employ monthly loan (firm-bank) data from 2008:01 to 2010:12, which gives us more than 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, reported in Table 9, reveal a stigma effect for firms with a pre-crisis share of loans from weak banks above 80% (though the effect is only significant at a 10% confidence level for higher exposure thresholds).[12]

6.4 Probability of exit

Since the bulk of job losses stem from firm closures, it is natural to estimate the effect of weak bank attachment on the probability of firm exit. We start by estimating a linear probability model for exit in 2010 with respect to 2006, using the same specification as in our baseline DD equation (1). The sample consists of the 170,457 firms with either a positive employment level or a zero level of employment in 2010 because the firm is known not to have survived the crisis.

As shown in the first column of Table 10, the treatment effect is significant. Weak-bank attachment leads to a marginal exit probability which is 8.4% (0.8 pp) higher than the baseline exit rate of 10%. We also try a second specification in which we use the actual ratio of weak-bank credit to assets rather than the treatment dummy. The effect, presented in col. (2), is again significant, with a coefficient of 5.8 pp. This implies that *ceteris paribus*, compared to a firm with a ratio of weak bank debt to assets at the first decile –which is roughly nil–, a firm at the ninth decile, i.e. with an exposure of one-quarter, has a 14.4% higher probability of closing down. The last column of the Table confirms preceding results, in that single-bank firms benefit from this condition by having a lower probability of exit. At the average exposure ratio (0.17), they have an 8% lower probability of exit than multi-bank firms.

Job losses via firm destruction carry a larger economic cost than downsizing at surviving firms, and it probably makes the recession more protracted. It is therefore worth asking why do credit constraints cause employment losses mostly through closures. This is likely linked to the fact that in 2006 Spain ranked relatively high –in the ninth position out of 28 OECD countries– in the degree of stringency of employment protection legislation for permanent contracts (OECD Indicators of Employment Protection, 2013 release, www.oecd.org). Therefore, once temporary jobs have been destroyed at a relatively low cost, it is quite costly and difficult –due to labor court procedures– to dismiss regular

[12] This equation is estimated with a limited set of control variables (see the footnote to the Table), because our full standard list was unavailable for 2002.

employees, so that eventually the firm has to close down.

6.5 Credit measure

So far we have focused on bank credit, which is the major source of funding for firms in Spain. However, trade credit is an alternative source, and a firm's suppliers may have advantages over banks as credit providers, in terms of acquiring information, monitoring, and efficiency in liquidation (Petersen and Rajan, 1997).

We cannot fully address the question of whether trade credit may have compensated for restrictions in bank credit. The reason is that we only have data on firms' liability structure for a subsample, namely on 15,323 firms (7% of the total). These are the firms that provide more detailed public accounts, which not surprisingly tend to be the largest ones. For example, in 2006 their median assets were equal to 9.1 millions, vis-à-vis 0.58 millions in our full sample. For them, at the median, financial institutions and trade credit each represent 34% of their liabilities.

We then estimate our instrumental variables model (2) for the credit channel with these firms. The weak bank dummy is significant in the first stage and the overall effect of weak-bank attachment on employment is 4.0 pp, lower than the full sample estimate of 6.2 pp, see Table 12 (col. 1). This is consistent with the larger effect found for small firms in our triple difference estimation. Estimating the IV model with total credit rather than bank credit (col. 2), we find again that the weak bank dummy is significant and, contrary to the case of bank credit, it reveals a credit contraction in 2007. The overall effect is slightly higher than for bank credit, 4.4 pp, but not significantly so. Thus we conclude that trade credit did not alleviate the credit constraint. We cannot directly check whether the same is true also for smaller firms, which are usually more dependent on trade credit. However, our finding is consistent with the results by Molina Pérez (2012), who finds no increase in trade credit taken by firms over the period 2008-2010 with a sample of 9,602 Spanish firms, 85% of which are small and medium-sized firms (below 250 employees).

6.6 Estimation technique

In order to achieve ex-ante comparability across firms, so far we have controled for a long list of firm characteristics and included dummy variables for the firm's main bank. More accurate control for selection may however be attained through the use of matching techniques. Here we apply the coarsened exact matching method (Iacus *et al.*, 2011). Sample sizes typically found in the literature severely limit the number of cells that can be constructed, whereas in our case we can use cells defined by 14 control variables, which we choose according to their significance in the baseline DD regression.

The coarsening entails each variable becoming a 0-1 dummy. For variables that were not originally of this type, we use the sample median value as the cutoff, except for the number of banks, where we the distinction is between firms with one or multiple banking relationships. Regarding industry, we separate the Primary sector and Mining from the others, and for provinces we differentiate those in the East coast of the Spanish Peninsula plus the Balearic and Canary Islands from the rest (see Appendix for definitions). Out of 16,384 potential strata, we end up with 4,822 strata with observations. 3,553 of which can be matched across treated and control firms. Figure 4 makes it clear that the matching method supresses any potential preexisting trend differences between treated and control firms.

Using weighted least squares, the estimated employment effect attached to the weak bank dummy variable is equal to 3.0 pp (s.e. 1.4 pp), which is about half the size of our baseline DD estimate,[13] so that weak-bank attachment accounts for 10.8% of job losses at treated firms. The sizeable difference between these estimates suggests that rigorous controling for selection can significantly alter estimated effects.

We also check the stability of this estimate with respect to the degree of exposure to weak banks. Matching estimates are less stable than DD estimates, ranging from 5.2 pp for exposure above the third decile to nil above the ninth decile (not shown). Estimating separately for single- and multi-bank firms, see Figure 5, we find that the former do not

[13] The number of fims is 211,284 and the number of observations 377,498. This sample is not exactly the same as for DD; for the same sample the estimated effect is -0.063 (s.e. 0.009).

suffer additional job losses from being attached to weak banks, whereas multiple-bank firms suffer losses around 4.8 pp on average.[14] As already indicated, these firms obtained relatively more credit, possibly as a result of the evergreening of their loans.

7 Conclusions

In this paper we aim at measuring the impact of credit constraints on employment during the Great Recession in Spain, for firms outside the real estate sector. We achieve identification by exploiting differences in lender health at start of the crisis, as evidenced by public bailouts of savings banks. We proceed by comparing employment changes from the expansion to the recession between firms that are heavily exposed to weak banks and less exposed firms. Our exceptionally large matched bank-loan-firm data set allows us to control exhaustively for ex-ante characteristics of firms and for potential endogeneity, as well as to perform a wide range of robustness checks.

The estimated effects are quite sizeable. Controling for selection, attachment to weak banks caused a larger fall in employment from 2006 to 2010 ranging from 3.0 to 13.4 percentage points, i.e. 7.4% to 16% of job losses at treated firms. We also find significant heterogeneity according to the ex-ante financial vulnerability of firms.

Our results are at the low end of the ranges of estimated effects found in the preceding US literature. Greenstone and Mas (2012) infers that the decline in lending from 2007 to 2009 accounted for up to 20% of the employment decline in US firms with less than 20 employees and for 16% of the total employment loss. On the other hand, Chodorow-Reich (2013) finds that the withdrawal of credit explains between one-third and one-half of job losses at small and medium-sized firms in the year following the Lehman Brothers bankruptcy. However, these estimates are not directly comparable with ours for a number of reasons. In particular, we have achieved identification by focusing on credit constraints arising from only one channel –namely, exposure to weak banks–, while controling for other firm characteristics that are also traditionally thought to capture credit restrictions.

[14] The precision of our estimates falls as the exposure to weak banks grows, because finding matches for treated firms becomes increasingly harder within smaller groups, but coefficients are generally significant.

Our estimates reveal that the joint consideration of both sets of characteristics lead to a wider range of estimated employment effects. For example, the impact for firms which ever defaulted on a loan is almost five times larger. This finding suggests that, lacking information on firms' financial histories, the existing literature may be mis-estimating the impact of credit on employment, due to the lack of sufficient controls to attain homogeneity between treated and control firms.

We also contribute to the literature on the interaction between credit constraints and the number of banks a firm works with. Our results clearly show that, in the Spanish case, firms that relied on a single bank did were not adversely affected by that bank being a weak one. Lastly, we have also found that credit constraints caused employment losses mainly by driving firms to close down rather than to downsize. This channel had not been identified in the existing literature, as far as we know. And there are potentially important welfare implications, since job losses via firm destruction carry a larger economic cost than downsizing at surviving firms, and they probably make the recession more protracted.

At a more general level, while we find relatively large job losses caused by credit constraints, the domain of application of our results is limited. On the one hand, we clearly cannot extrapolate our firm-level estimates to the aggregate economy. We are only able to estimate the differential employment effect of weak banks on employment at firms attached them vis-à-vis the remaining firms. In general equilibrium, there would be further effects (see Chodorow-Reich, 2013). A drop in aggregate demand generally reduces labor demand by both constrained and unconstrained firms, but product demand may be shifted from the former to the latter, thus inducing an increase in their labor demand. The microeconomic effect need therefore not coincide with aggregate effects.

On the other hand, our estimates do not capture the social cost of the poor risk management practices of Spanish savings banks, which fueled the activities of firms seemingly without due concern for their expected ability to repay. Still, we can make a statement regarding efficiency. Assuming that our quasi-experimental approach is valid, the assignment of firms to weak banks, as opposed to healthy banks, is as good as random. In other words, given our controls, these firms could have been granted as much credit from healthy rather than weak banks. In this sense, while the overall job losses suffered by firms attached to weak banks may or may not have been efficient, the estimated employment effects of the credit constraints suffered by firms attached to weak banks, once selection has been taken into account, can be considered to be inefficient.

A Appendix. Definitions of variables and descriptive statistics

Employment. It is computed as the average level over the year, weighing temporary employees by their number of weeks of work. The Temporary Employment Ratio divides the temporary by the 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. List of 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.

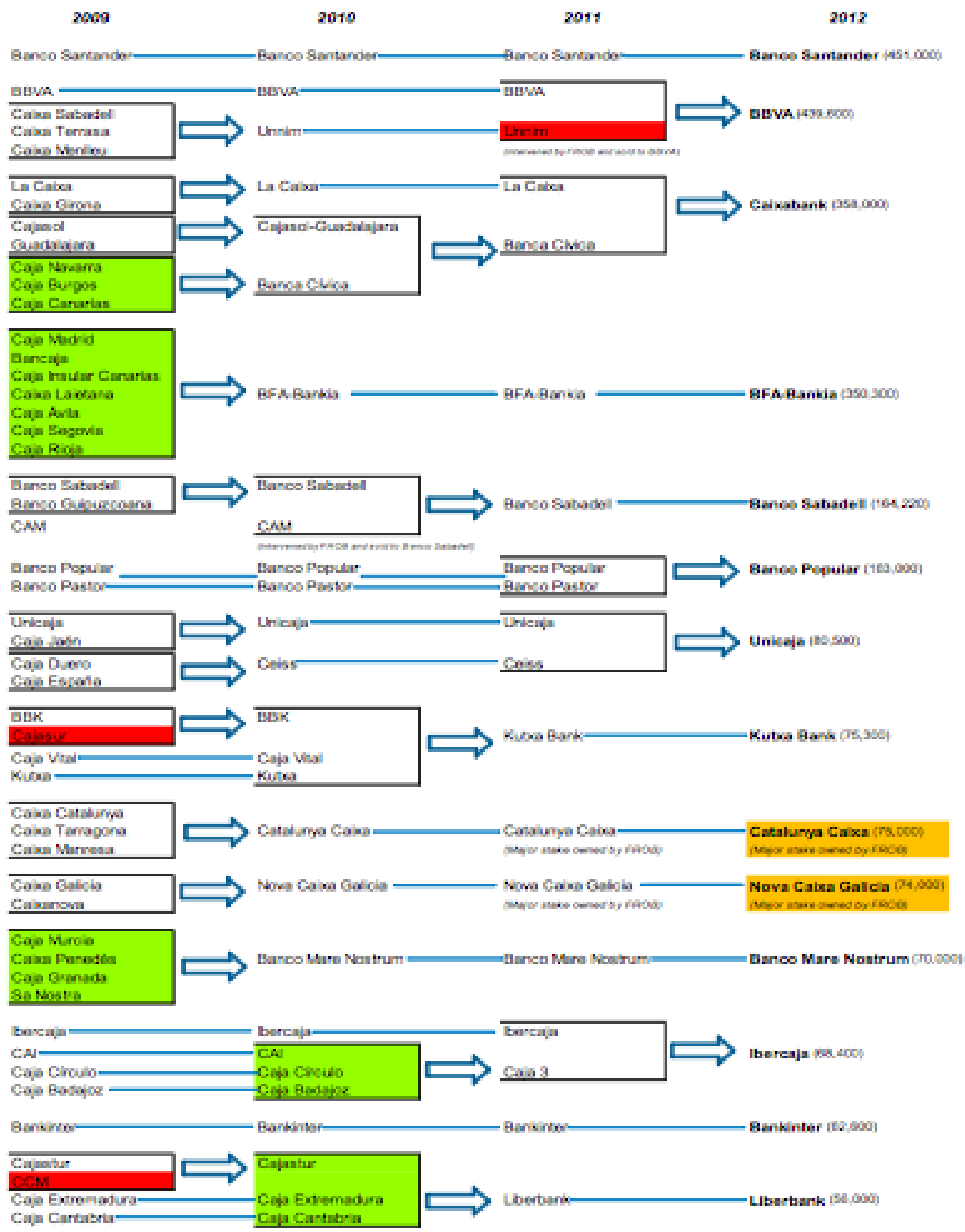
Balance sheet and income statement control variables. They are the following (flows are in nominal values and stocks in book values in December of each year): Firm Size (Total Assets), Own Funds (Own Funds/Total Assets), Liquidity (Liquid Assets/Total Assets), Return on Assets (Earnings before interest, 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, 170,475 (78.5%); (b) In 2006 but had closed down by 2010, 17,088 (7.9%), (c) In 2006 but not observed in 2010, 28,482 (13.1%), and (d) Observed only in 2010 (other variables observed in 2006, but not employment), 998 (0.5%).

Table A1. Spanish savings banks' integration process



Sources: Data from the authorities; and IMF staff estimates.
Note: Assets for each bank are reported in millions of euro and only correspond to assets in Spain in 2011. Banks coded in red were intervened; banks coded in green were part of the institutional protection scheme; banks coded in orange have been intervened and will be auctioned.

Source: International Monetary Fund (2012), Figure 2.

Table A2. 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 A3. Descriptive statistics of the main sample of firms (2006)

	Control		Treated		2-sample t-test	
Firms	Mean	S. D.	Mean	S. D.	Diff.	|t|
Loans with weak banks	0.01	0.20	0.25	0.17	0.24	
Share loans weak banks	0.10	0.25	0.71	0.29	0.61	438.06
Employment	24.63	327.38	18.73	134.94	-5.91	4.31
Firm Size (million euros)	5.08	101.32	3,01	22.80	-2.07	4.99
Firm Age (years)	12.16	9.58	11.01	8.37	-1.15	25.89
Own Funds	0.33	0.24	0.24	0.18	-0.10	90.33
Liquidity	0.12	0.15	0.09	0.12	-0.04	52.28
Return on Assets	0.06	0.11	0.05	0.09	-0.01	27.52
Temporary Employment	0.21	0.26	0.24	0.27	0.03	19.85
Bank Debt	0.32	0.27	0.50	0.23	0.19	150.75
Short-Term Bank Debt	0.48	0.41	0.44	0.37	-0.04	18.99
Long-Term Bank Debt	0.22	0.36	0.31	0.37	0.09	51.83
Uncollateralized Loans	0.81	0.34	0.72	0.36	-0.09	55.65
Credit Line	0.68	0.47	0.70	0.46	0.02	8.87
Banking Relationships	1.94	1.55	2.98	2.69	1.03	111.37
Current Defaults	0.00	0.06	0.01	0.08	0.00	10.30
Past Defaults	0.02	0.13	0.03	0.17	0.01	17.56
Loan Applications	0.55	0.50	0.69	0.46	0.14	58.00
All Applications Accepted	0.22	0.42	0.26	0.44	0.04	17.94
Banks						
Bank Size (billion eur)	121,54	111.82	61.91	77.38	-59.63	120.71
Own Funds	0.05	0.02	0.05	0.02	-0.01	79.58
Liquidity	0.13	0.06	0.13	0.06	-0.02	69.10
Return on Assets	0.01	0.00	0.01	0.00	0.00	100.02
Non-performing Loans	0.01	0.01	0.01	0.01	0.00	29.75
Loans to Firms	0.55	0.10	0.55	0.10	-0.02	31.46
Mortgages	0.38	0.10	0.38	0.10	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 thousand euros.

Table A4. 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 in bank 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 the Bank of Spain.

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

	Control		Treated	
Variable	Average	Standard deviation	Average	Standard deviation
Loans with weak banks to assets	0.01	0.20	0.25	0.17
Share of loans with weak banks	0.10	0.25	0.71	0.29
Employment (employees)	24.63	327.38	18.73	134.94
Temporary Employment	0.21	0.26	0.24	0.27
Firm Age (years)	12.16	9.58	11.01	8.37
Firm Size (million euros)	5.08	101.32	3.01	22.80
Own Funds	0.33	0.24	0.24	0.18
Liquidity	0.12	0.15	0.09	0.12
Return on Assets	0.06	0.11	0.05	0.09
Bank Debt	0.32	0.27	0.50	0.23
Banking Relationships	1.94	1.55	2.98	2.69
Past Defaults	0.02	0.13	0.03	0.17

Notes. Observations: 155,167 control firms and 60,860 treated firms. Variables are ratios unless otherwise indicated. The share of loans with weak banks is in bank credit.

Table 3. The employment effect of weak-bank attachment
Difference in Differences

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

	(1)	(2)	(3) Baseline	(4) Placebo
$Post \times WB_i$	-0.085***	-0.074***	-0.062***	-0.001
	(0.013)	(0.013)	(0.009)	(0.001)
Province and Industry Dummies	yes	yes	yes	yes
Firm Controls	no	yes	yes	yes
Main Bank Dummies	no	no	yes	yes
$Post\times$ Province and Industry D.	no	no	yes	yes
R^2	0.009	0.489	0.494	0.003
No. firms	217,025	217,025	217,025	101,515
No. observations	387,482	387,482	387,482	191,948

Note. OLS estimates using observations for two years: 2006 and 2010, in column (4), 2002 and 2006. 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: $p<0.01$ = ***, $p<0.05$ = **, $p<0.10$ = *.

Table 4. The employment effect of weak-bank attachment
Instrumental variables for credit
Dependent variable: $\Delta log(1{+}Employment_{it})$

	(1)	(2)
Instrumented variable:	$\Delta log(1{+}Credit_{it})$	$I(Rejection)$
	0.424^{***}	-2.280^{***}
	(0.098)	(0.461)
Overall effect	-0.065	-0.067
First stage		
$d_{2008} \times WB_i$	-0.022^{***}	0.014^{***}
	(0.006)	(0.003)
$d_{2009} \times WB_i$	-0.095^{***}	0.024^{***}
	(0.014)	(0.004)
$d_{2010} \times WB_i$	-0.154^{***}	0.029^{***}
	(0.016)	(0.005)
p-value of F test	0.00	0.00
No. firms	196,978	196,978
No. observations	716,678	716,678

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 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: $p<0.01$ = **, $p<0.05$ = *, $p<0.10$ = *.

Table 5. The employment effect of weak-bank attachment
Instrumental variables for weak banks
Dependent variable: *log(1+Employment$_{it}$)*

	Weak-bank density (1988)	Exposure to REI (2000)
$Post \times WB_i$	-0.084***	-0.135***
	(0.038)	(0.024)
First stage		
Dependent variable: WB_i		
Weak-bank density$_i$	0.101***	
	(0.009)	
Exposure to REI (2000)		0.276***
		(0.076)
Dependent variable: $Post \times WB_i$		
$Post\times$ Weak-bank density$_i$	0.431***	
	(0.076)	
$Post\times$ Exposure to REI (2000)		0.373***
		(0.122)
p-value of F test	0.00	0.00
No. firms	217,025	217,025
No. observations	387,482	387,482

Note. Instrumental variables estimates using observations for 2006 and 2010. All specifications include Firm and Time fixed effecdts. In the firsc column the density of weak banks in December 1988 is used as instrument for WB_i. In the second column the exposure of banks to the Real Estate Industry in 2000 used to as an instrument. Robust standard errors are corrected for clustering at firm level. The p-value of the F test for the exclusion restriction is reported. Symbols: p<0.01 = **, p<0.05 = *, p<0.10 = *.

Table 6. The employment effect of weak-bank attachment. Robustness checks
Difference in Differences
Dependent variable: *log(1+Employment$_{it}$)*

	(1)	(2)	(3)	(4)	(5)	(6)
	Timing of controls				Treatment	Sample
	Firms		Banks		% loans	Surviving
	2002	2005	2000	2002	to REI	firms
$Post \times WB_i$	-0.059***	-0.061***	-0.035***	-0.049***	-0.062***	-0.013***
	(0.010)	(0.009)	(0.006)	(0.007)	(0.008)	(0.004)
R^2	0.488	0.500	0.526	0.517	0.494	0.546
No. firms	106,122	150,690	99,869	136,280	217,025	199,691
No. observ.	192,765	271,540	181,751	246,362	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, col. (5) changes the definition of weak bank, and col. (6) changes the sample to surviving firms. All specifications include Industry and Province Dummies, their interaction with $Post$, and Main Bank Dummies (in cols. 3 and 4, for both 2006 and the year of reference). 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 = *, p<0.10 = *.

Table 7. The employment effect of weak-bank attachment
Triple Differences with indicators of financial vulnerability

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

$Post \times WB_i$	-0.047***
	(0.007)
$Post\times$ Rejected application$_i$	-0.064***
	(0.004)
$Post \times WB_i\times$ Rejected application$_i$	-0.013
	(0.011)
$Post\times$ Defaults$_i$	-0.229***
	(0.025)
$Post \times WB_i\times$ Defaults$_i$	-0.006
	(0.027)
$Post\times$ Short-term debt$_i$	-0.094***
	(0.008)
$Post \times WB_i\times$ Short-term debt$_i$	-0.071***
	(0.013)
$Post\times$ Small firm$_i$	-0.024*
	(0.013)
$Post \times WB_i\times$ Small firm$_i$	-0.120***
	(0.033)
$Post\times$ Single bank$_i$	0.038***
	(0.004)
$Post \times WB_i\times$ Single bank$_i$	0.029***
	(0.010)
R^2	0.389
No. firms	217,025
No. observations	387,482

Note. OLS estimates using observations for two years: 2006 and 2010. All specifications include Industry and Province Dummies, their interaction with $Post_t$, 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 = *, p<0.10 = *.

Table 8. Credit and the strength of single-bank dependence

Dependent variable: Δ*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. firms	509,800
No. 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 = *, p<0.10 = *.

Table 9. The effect of weak-bank attachment on loan application acceptance

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

	(2)	(3)	(4)
Threshold:	≥80%	≥90%	100%
Loan share with weak banks in 2002-2006 above the threshold$_i$	-0.009**	-0.009*	-0.008*
	(0.004)	(0.005)	(0.005)
R^2	0.010	0.010	0.010
No. firms	109,172	109,172	109,172
No. observations	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 = *, p<0.10 = *.

Table 10. Effect of weak-bank attachment on the probability of exit
Linear probability model
Dependent variable: $I(Exit_i)$

	(1)	(2)	(3)
WB_i	0.008**		
	(0.004)		
% Credit with weak banks$_i$		0.058***	0.057***
		(0.014)	(0.007)
% Credit with weak banks$_i \times$ Single bank$_i$			-0.050***
			(0.012)
R^2	0.056	0.056	0.056
No. firms	170,457	170,457	170,457
No. observations	170,457	170,457	170,457

Note. OLS estimates using firms with observations for two years: 2006 and 2010. All specifications include Industry, Province, 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 = *, p<0.10 = *.

Table 11. The employment effect of weak-bank attachment
Difference in Differences with a reduced sample
Dependent variable: $\Delta log(1{+}Employment_{it})$

	(1)	(2)
Instrumented variable:	$\Delta log(1{+}Credit_{it})$	$\Delta log(1{+}Total\ Credit_{it})$
	0.266***	0.301***
	(0.096)	(0.082)
Overall effect	-0.040	-0.044
	First stage	
$d_{2008} \times WB_i$	0.015	-0.072***
	(0.012)	(0.013)
$d_{2009} \times WB_i$	-0.100***	-0.118***
	(0.020)	(0.013)
$d_{2010} \times WB_i$	-0.150***	-0.147***
	(0.025)	(0.021)
p-value of F test	0.00	0.00
No. firms	15,323	15,323
No. observations	57,013	57.013

Note. OLS estimates using observations for two years: 2006 and 2010, in column (4), 2002 and 2006. 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 = **, p<0.10 = *.

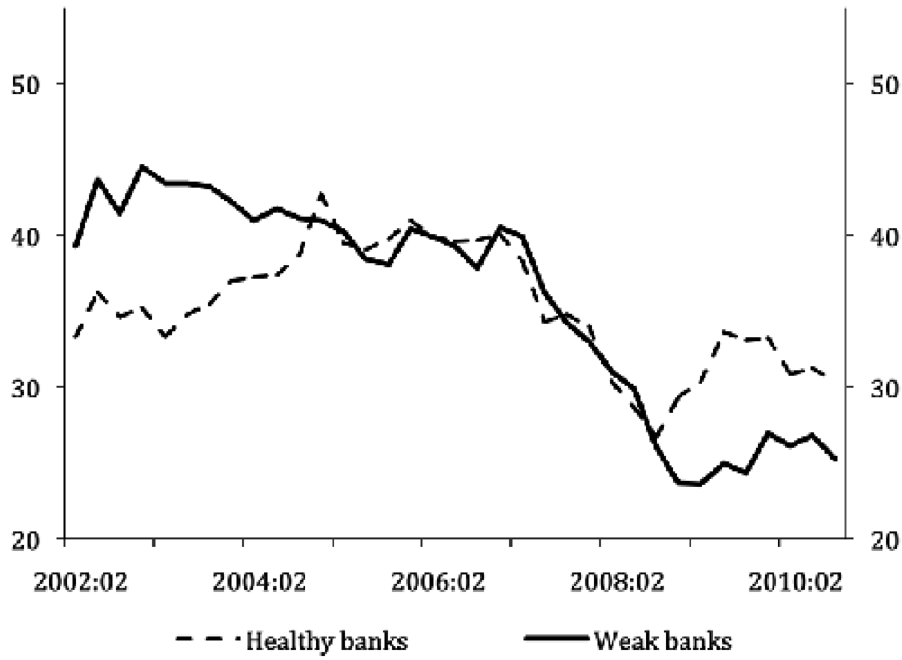

Figure 1: Acceptance rates of loan applications by non-current clients, by bank type. Firms applying to at least one bank per type (%)

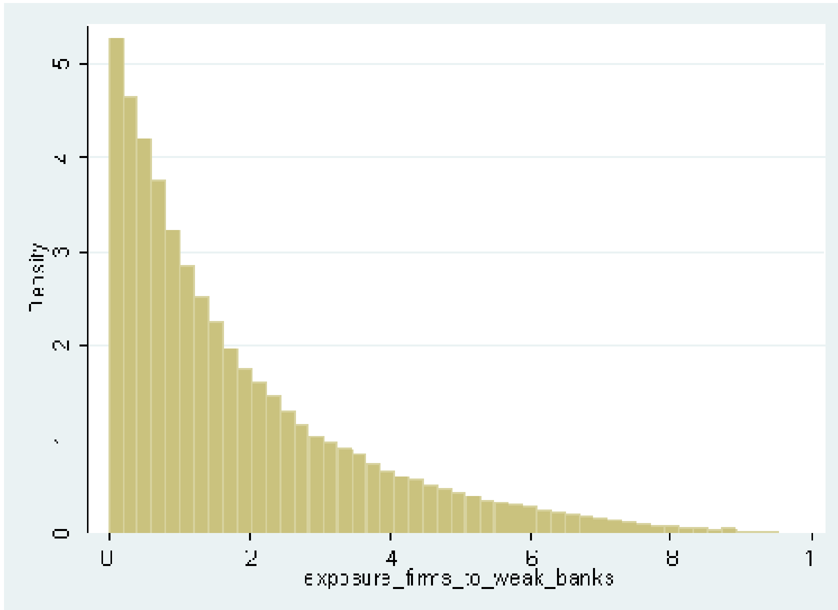

Figure 2: Histogram of the exposure of firms to weak banks (excluding no exposure) (%)

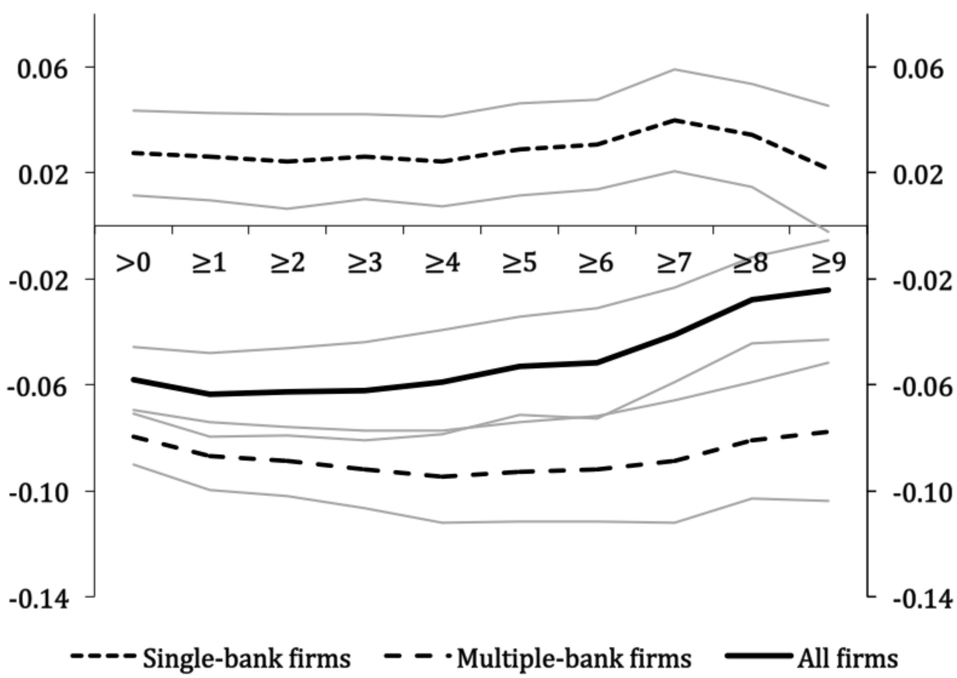

Figure 3: The employment effect of exposure to weak banks by decile and number of banks (DD estimates with 2-s.e. bands)

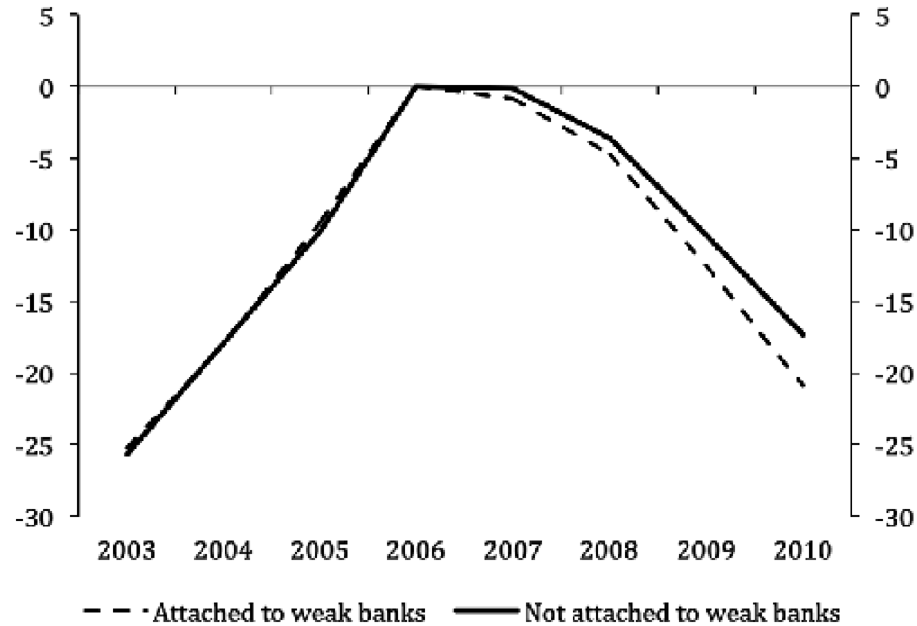

Figure 4: Evolution of employment at firms attached to weak banks and non-attached firms, weighted by matching (2006=0) (%)

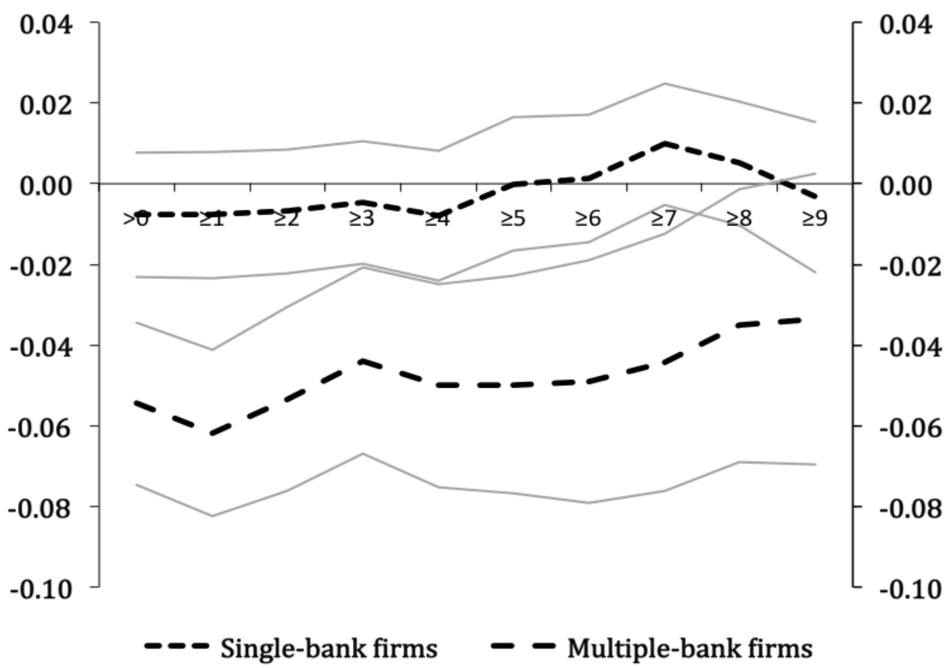

Figure 5: The employment effect of exposure to weak banks by decile and number of banks (matching estimates and 2-s.e. bands)