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by

Samuel Bentolila[*], Marcel Jansen[**]
Gabriel Jiménez[***] and Sonia Ruano[***]

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[*] CEMFI, CEPR and CESifo.
[**] Universidad Autónoma de Madrid, Fedea e IZA.
[***] Banco de España

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Jorge Juan, 46, 28001, Madrid – España | Te..: +34 914 359 020 | Fax: +34 915 779 575 documentos.fedea.net | infpub@fedea.es

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Samuel Bentolila
CEMFI

Marcel Jansen
Universidad Autónoma de Madrid

Gabriel Jiménez
Banco de España

Sonia Ruano
Banco de España

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Abstract

We use a unique dataset to estimate the impact of a large credit supply shock on employment in Spain. We exploit marked differences in banks' health at the onset of the Great Recession. Several weak banks were rescued by the State and they reduced credit more than other banks. We compare employment changes from 2006 to 2010 at firms heavily indebted to weak banks before the crisis and the rest. Our estimates imply that these firms suffered an additional employment drop between 3 and 13.5 percentage points due to weak-bank attachment, representing between 8% and 36% of aggregate job losses.

KEYWORDS: Job losses, Great Recession, credit constraints.
JEL CODES: D92, G33, J23.

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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 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. We aim at estimating 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 question. To start with, Spanish firms rely heavily on bank credit and the high leverage ratio of many firms, mostly small and medium-sized enterprises (SMEs), made them vulnerable to the contraction in bank lending that took place during the recession. This credit shock originated in a boom-bust cycle in domestic housing prices that had a large impact on bank solvency. Thus, the Spanish example may help us to draw lessons that are applicable to other countries, like Ireland or the US, which 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 draws from several sources. We have access to the official credit register of the Bank of Spain, 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 on whether 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 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 their solvency was far from uniform. 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. The rescue 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. Before the crisis, these bailed-out or *weak banks* accumulated a very large share of the loans to the real estate industry and, between the outbreak of the crisis and the end of 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 chose 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, we avoid the risk of reverse causality by removing from our sample all firms belonging to the real estate industry as well as those selling a significant share of their output to this industry.

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– 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 between firms in the treatment and the control group and their access to credit during the crisis. In these exercises, weak-bank attachment is used as an instrument for observed changes in credit and the predicted changes are subsequently used to explain the differences in employment growth at the firm level. Lastly, 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. Thus we use the share of bank branches at the municipal level that belonged to weak banks right before this legal change 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, which in our sample would *ceteris paribus* represent between 8% and 36% of aggregate job losses. We also find large differences across firms belonging to different industries and across firms with different credit histories. Moreover, credit constraints are shown to have operated mostly through firm closures 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 background information 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. The Appendix provides a detailed description of the variables used.

2 Literature review

Our identification strategy requires the existence of financial market frictions. In particular, firms attached to weak banks must not have been able to readily switch to healthier banks. Two 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 in recessions 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 bank's net worth may generate a disruption in both the interbank market and the credit flow from banks to firms. The result is an inefficient allocation of capital and a drop in investment. We should stress that the fall in credit supply is not necessarily

[1] The initial studies focused on investment in physical capital. Early contributions considering the effect of financial constraints on employment are Greenwald and Stiglitz (1993) and Sharpe (1994).

uniform. Banks with a relatively high leverage ratio are more vulnerable to shocks and this negatively affects their clients.

The second line of research focuses on the role of relationship banking (*e.g.* Freixas, 2005). Repeated interaction with a client often provides 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 from undertaking 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 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 bank health. Greenstone and Mas (2012) construct a county-level credit supply shock from the product of the change in US banks' small-business lending at the national level and their predetermined credit market share at the county level. 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 papers above 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 for identifying 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 SMEs 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 its magnitude and origins, focusing on the role played by weak banks. We end with some evidence showing 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 unanticipated reductions in bank lending. 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 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, 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

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

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, subsequently falling by 38% in the period to 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 or 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 stage of the crisis (Jiménez *et al.*, 2012b). The Spanish regulator, namely the Bank of Spain, forced banks to keep securitized assets on their balance sheets, and in 2000 it implemented a system of dynamic provisioning, which obliged 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 were therefore acutely hit by the freezing of wholesale Eurozone markets in 2008. The European Central Bank offered relief to Euro area banks, 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. Moreover, while in the recession weak banks started charging firms a higher interest rate for their loans than healthy banks, the difference was quite small, on average less than 30 basis points. For this reason, we focus on the volume of credit rather than on interest rates.

3.2 The demise of the savings banks

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. These 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. 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 savings banks (International Monetary Fund, 2012). Throughout the rest of the analysis we define weak banks as those banks that obtained funding from the State in order to survive (this set only includes one private, non-savings bank). We refer to the remainder as healthy banks, including those that received funds to acquire ailing banks. To fund the recapitalization of weak 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 and healthy banks. In 2006 the former accounted for about one-third of outstanding credit to the non-financial sector. While the REI represented one-third of loans at healthy banks, it comprised almost two-thirds at weak banks (Panel A of Table 1). 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. 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 at both the extensive margin (credit to new customers) 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 percentage points (hereafter 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.

3.3 Were weak bank troubles anticipated?

The differential buildup in risks at the two sets of banks is striking. But could firms anticipate the solvency problems of weak banks? If so, our identification procedure would be invalid. 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. By the end of this year, the ratio of securitization to total assets was significantly higher for weak than for healthy banks, 16.7% and 13.5% respectively.

We group the ratings into three 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 24 issuer parents (source: Dealogic).

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). 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 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 describe the variables included in our matched firm-loan-bank data set. We end this section with a description of our treatment and control variables. Further details appear in the Appendix.

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 combines six separate sources and contains such information.

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.[3] 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

[3]The source is the Iberian Balance sheet Analysis System produced by INFORMA D&B in collaboration with Bureau Van Dijk and the Central Balance Sheet Data Office of the Bank of Spain.

used as controls in our analysis.

To avoid the risk of reverse causality –so that the troubles of firms drive the solvency problems of banks–, we exclude the REI, as well as industries that in the year 2000 sold at least 20% of their output to it (see the Appendix). 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. 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 potentially problematic loans is available.

We also use 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 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 not, either the bank denied it or else the firm obtained funding elsewhere (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 it 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. We do so by comparing the evolution of employment in firms with high and low exposure to weak banks. Exposure is captured by the ratio of the firm's pre-crisis level of debt with weak banks to its asset value. This ratio jointly reflects the overall leverage ratio and the relative importance of weak banks in the firm's bank debt.[4]

About one-third of firms had no credit from weak banks. In our benchmark treated firms are defined as those above the third decile of the cross-sectional distribution of firms with positive exposure, which takes a value 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%. We will also show that our results hold qualitatively

[4] 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.

for all other deciles of the distribution.

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 loans. 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, although they are statistically significant. We must therefore 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 discuss our empirical strategy and show the estimation results, both for standard difference in differences and for two instrumental variables models.

5.1 Difference in differences

We start by estimating the following standard difference-in-differences (DD) equation:

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

where n_{it} is employment at firm i in year t (t=2006 and 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.[5] 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 in 2010 –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. Below we will 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, which 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, 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 list of variables in X_i 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

[5]The total number of firms is 217,025. The breakdown is as follows: both in 2006 and 2010, 153,369; in 2006 but closed down by 2010, 17,088; in 2006 but not observed in 2010, 45,570, and observed only in 2010, 998.

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, that 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 impact of the recession are absorbed by interactions between d_S and the crisis dummy.

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), its size (in terms of assets), and its 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 to non-current banks (where "past" refers to 2002-2005) and an indicator for whether all were accepted, indicators for having any past loan defaults, 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.

It is also vital to control for differences in the share of temporary contracts, which represent almost one-fourth of jobs 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 and to 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, 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 of loan applications at all banks from non-current customers sharply fell starting in early 2007. Below we will also check whether it was even lower for firms with a high exposure to weak banks. Moreover, as shown below in the context of an instrumental variables 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 unforeseen. 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 our 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 REI.

5.3 DD estimates

Table 3 presents the estimation results for our difference-in-differences equation (1). We report robust standard errors corrected for clustering at firm and main bank level, unless otherwise stated. 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. 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 (first and second columns). Adding main bank dummies and controling for differential trends by province and industry reduces the treatment effect to 6.2 pp (third column).[6] We take this as the baseline specification, in particular with respect to the set of control variables. We can perform a very rough estimate of the aggregate effect in our sample. Restricting ourselves to those firms in our sample whose employment is known for both 2006 and 2010, since firms attached to weak banks comprise 21.7% of employment in 2006, the estimated effect would explain an aggregate 1.3% employment fall. Given that the overall employment reduction at those firms from 2006 to 2010 was equal to 8%,[7] weak-bank attachment would then account for 16.7% of aggregate job losses. Obviously this estimate cannot be taken as an approximation to the macroeconomic effect, since we are completely abstracting from any general-equilibrium effects.

We test for differences in the pre-crisis trends for the treatment and control groups by running a placebo equation 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 (last column in Table 3).

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:

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

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

[6] The estimated coefficient did not change when main bank dummies were replaced by either main bank characteristics or main, secondary, and tertiary bank dummies.

[7] This decline is very close to the fall in the number of private sector employees in Spain over the period, which was equal to 8.3%.

t =2007,...,2010, and d_i is a firm fixed effect. Coefficient μ 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 several ways. First, it is estimated in first differences because –in keeping with the literature– we are better able to explain credit changes than levels. Second, it is a panel of four rather than two periods, so that we exploit information for each recession year and we capture all firm-specific characteristics via fixed effects rather than through initial-year control variables.

As shown in the lower panel of Table 4, the instrumental variable is significantly and negatively correlated with credit, increasingly so as the recession lengthens (first column). Credit is also found to be a significant determinant of employment changes, so that a one pp increase in credit raises employment by 0.42 points. 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 omitted year).[8] This is very close to our baseline DD estimate of 6.2 pp, in spite of the different nature of the two models. This result supports the idea that credit is the key channel through which the weak-bank attachment operates.

In the second column 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 for firms on the margin of having a loan approved

[8]The effect vis-à-vis 2006 is unidentified, but an IV estimation for 2007 alone gave a non-significant coefficient with respect to 2006, so that the former effect is likely to be around 6.5 pp as well.

(Imbens and Angrist, 1994).[9] This finding is not so surprising once we realize that these losses stem from firms closing down, which 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 below.

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). 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 (6,101 municipalities with bank branches). 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 we see that high weak-bank density in 1988 significantly predicts weak-bank attachment 18 years later (first column). 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. According to this estimate, 22.7% of aggregate job losses would be attributed 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 bank

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

exposure to the REI 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. 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. Repeating the simple extrapolation exercise performed above, this estimate would imply that 36.4% of job losses would be attributed to weak-bank attachment.

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: timing, treatment variable and reference sample, firms' financial vulnerability, level of exposure to weak banks, probability of exit, measure of credit, and estimation method.

6.1 Timing, treatment variable, and reference sample

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 (first two columns). This restriction moderates the effect to 5.9 pp when 2002 is used and to 6.1 pp for 2005.

We also shift back the date of 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. 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 with the former being significantly different from the baseline (third and fourth columns).

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 REI in 2006. This 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 distribution are classified as weak banks. This specification leads to an estimated employment effect of 6.2 pp (fifth column), which is identical to the baseline, confirming that REI exposure drives weak-bank troubles.[10]

Lastly, we estimate the effect only for surviving firms. The estimated job loss for treated firms is equal to 1.3 pp, which is significantly 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 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 weak bank dummy with the firm characteristic of interest. We begin with nine industry dummies. For five out of the nine we find significant

[10]Including committed but undrawn loans in the treatment yields a job loss of 5.7 pp (s.e. 0.8 pp).

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 2002-2006. Table 7 reveals that these firms suffer an additional loss of jobs of 6.4 pp in the recession but no extra loss if they were 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 implies an impact that 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 attached to 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 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 credit records. A noteworthy result is that we find no significant differences in the penalties that weak banks impose on firms with a bad credit record.

We next examine whether the impact of credit constraints varies with the number of banking relations. We distinguish between firms working with only

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

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 with an exclusive banking relationship. For 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 still 2.9 pp lower if the single bank is weak. In the next subsection we further explore the latter finding.

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. 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. Figure 2 shows 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 are now stable and significantly different: job losses from weak-bank attachment are on average 8.8 pp higher for multi-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 favored firms that 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 bank in 2006, including firm and bank-year fixed effects to control for unobservable demand and supply factors.[12] This data set has 3.75 million observations on 509,800 firms. We find that in general banks did not extend more credit to firms whose credit was more concentrated with them, except for weak banks: each pp of loans with the bank raises credit growth by 0.34 pp (s.e. 0.09). This result hints at an *evergreening* of loans by these banks.

Why would only weak banks behave in this way? During this period, weak banks were more closely monitored by the markets than healthy banks, due to their large exposure to real estate, so that they might have been more eager to avoid increases in their non-performing loan rate. Alternatively, while obtaining credit became harder for all firms, it may have been more difficult for firms heavily depending on weak banks. We test this stigma hypothesis 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.[13] We estimate a linear prob-

[12]Standard errors are clustered at firm and bank level.

[13]The dataset has 240,179 observations on 109,172 firms. The available variables in 2002, which are used in this estimation are: Industry and Province Dummies, Size, Age, Age Squared, Own Funds, Liquidity, Return on Assets, and Past Defaults. Standard errors are clustered at firm and bank level.

ability model for the event that a loan is requested and granted on the share of bank credit that a firm had with weak banks over 2002-2006, including the same control variables as in equation (1) and bank-time dummies to control for time-varying unobservable supply factors. The result is a decrease in that probability of 3% for each pp increase in that share, as long as the firm has a share with weak banks above 80%.

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 8, 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 the second column, 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 the 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.

Firm destruction carries job losses with larger economic costs than downsizing at surviving firms, and it probably makes recessions more protracted. It is therefore worth asking why do credit constraints cause employment losses mostly through closures. This is likely linked to Spain ranking relatively high –in the ninth position out of 28 OECD countries in 2006– in the degree of stringency of employment protection legislation for permanent contracts (OECD Indicators of Employment Protection, 2013 release). 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 employees, so that eventually many firms have to close down.

6.5 Non-bank credit

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 for 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 reestimate our instrumental variables model (2) for the credit channel for 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 9 (first column). 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 (second column), we find again that the weak bank dummy is significant and, contrary to the case of bank credit, it reveals a credit contraction starting in 2008. 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 Exact matching

To achieve ex-ante comparability across firms, so far we have controled for a long list of firm characteristics. 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 distinguish between firms with one and with 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 the Appendix for a full list). 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. Reassuringly, we have ascertained that the matching method suppresses 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.[14] In this case, the aggregate impact of weak-bank attachment would also be halved, to 8.1%. The sizeable difference between these estimates suggests that a rigorous control for selection can significantly alter estimated effects.

We also check the stability of the estimate with respect to the degree of exposure to weak banks. Matching estimates are less stable than DD estimates, ranging from 5.2 pp above the third decile to nil above the ninth decile (not shown). However, estimating separately for single- and multi-bank firms, we find that the former do not suffer additional job losses from weak bank attachment, whereas multiple-bank firms suffer losses around 4.8 pp on average.[15] 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 sizeable. Controling for selection, attachment to weak banks caused a larger fall in employment from 2006 to 2010 ranging from 3.0 to 13.5 percentage points, i.e. 8% to 36% of aggregate job losses in our sample, though of course these

[14] There are 377,498 observations on 211,284 firms. This sample is not exactly the same as for DD; for the same sample the DD estimated effect is -0.063 (s.e. 0.009).

[15] Estimate precision falls as exposure to weak banks grows, because finding matches for treated firms becomes increasingly harder within smaller groups, but coefficients are generally significant.

estimates cannot be taken as an approximation to the macroeconomic effect, since we are ignoring any general-equilibrium effects. We also find significant heterogeneity according to the ex-ante financial vulnerability of firms.

Our results are within the ranges found in the preceding US literature. Greenstone and Mas (2012) infer 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, exposure to weak banks, while controling for other firm characteristics that are also traditionally thought to capture credit restrictions. 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 overestimating 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 that firms work with. Our results clearly show that in the Spanish case firms that relied on a single bank were not adversely affected by that bank being weak. Lastly, we have also found that credit constraints caused employment losses mainly by driving firms to close down rather than to just downsize. This channel had not been identified in the existing literature, as far as we know. And it has 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.

We can also 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 total job losses suffered by firms attached to weak banks may or may not have been efficient, the estimated employment effects of the credit constraints we identify, once selection has been taken into account, were inefficient.

In order to achieve better identification of a causal effect, we have focused on a single channel through which credit constraints operate, namely estimating the differential effect on employment at firms attached to weak banks vis-à-vis the non-attached firms. For this reason, it would be incorrect to extrapolate our estimates to the aggregate economy. 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 effects need therefore not coincide with the aggregate effect.

A Appendix. Definitions of variables

Employment. Computed as the average level over the year, weighing temporary employees by their weeks of work. The Temporary Employment ratio divides the temporary by the total number of employees. Set to 1 above the median for matching.

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 and its book value in 2006 is above the third decile of the cross-sectional distribution of firms with positive exposure to weak banks.

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

Industry. Firms belonging to the following industries are excluded (share of output sold to Construction and Real Estate in 2000 shown between parentheses): Extraction of Non-metallic Minerals (35.2%), Wood and Cork (21.1%), Cement, Lime, and Plaster (46.4%), Clay (60.1%), Non-metallic Mineral Products n.e.c. (85.4%), Fabricated Metal Products except Machinery and Equipment (23.3%), Machinery and Electric Materials (19.2%), and Rental of Machinery and Household Goods (26.2%).

There are nine Industry dummies, for: Agriculture, Farming, and Fishing; Mining; Manufacturing; Electricity, Gas, and Water; Trade; Hotels and Catering; Transport, Storage and Communications; Rental of Machinery, Computing and R&D; and Other Service Activities. For matching, the dummy takes on the value 1 for the first two.

Balance sheet and income statement control variables (flows are in nominal values and stocks in book values in December). 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). Age is defined as current year minus year of creation. For matching they are set to 1 when above the median. For triple differences, a Small Firm is defined as one with Total Assets below 10 million euros.

Credit-related control variables. Credit Line (at least one), Current Defaults (any nonperforming loan in 2006), Past Defaults (any nonperforming loan over 2002-2005), Loan Applications, All Applications Accepted (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).

Further tables and figures can be found in an **Online Apppendix** at: https://dl.dropboxusercontent.com/u/15338248/creditjobsonlineappendix.pdf.

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

	Weak banks	Healthy banks
A. Share of loans to the real estate industry (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. Panel A gives shares in loans to non-financial firms. 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		2-sample
Variable	Average	Standard deviation	Average	Standard deviation	t-test
Loans with WB/Assets	<0.01	0.01	0.25	0.17	551.46
Share of loans with WB	0.10	0.25	0.71	0.29	438.06
Employment (employees)	24.63	327.38	18.73	134.94	4.31
Temporary Employment	0.21	0.26	0.24	0.27	19.85
Age (years)	12.16	9.58	11.01	8.37	25.89
Size (million euros)	5.08	101.32	3.01	22.80	4.99
Own Funds	0.33	0.24	0.24	0.18	90.33
Liquidity	0.12	0.15	0.09	0.12	52.28
Return on Assets	0.06	0.11	0.05	0.09	27.52
Bank Debt	0.32	0.27	0.50	0.23	150.75
Banking Relationships (no.)	1.94	1.55	2.98	2.69	111.37
Past Defaults	0.02	0.13	0.03	0.17	17.56

Notes. Observations: 155,167 control firms and 60,860 treated firms. WB denotes weak banks. 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$_{it}$)

			Baseline	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

Notes. OLS estimates for 2006 and 2010; in the last column, 2002 and 2006. Firm controls: Size, Age, 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 the firm and main bank level between parentheses. p<0.01=***, p<0.05=**, p<0.10=*.

Table 4. The employment effect of weak-bank attachment. Instrumental Variables
Dependent variable: Δlog(1+Employment$_{it}$)

Instrumented variable:	Δlog(1+Credit$_{it}$)	I(Rejection)
	0.424***	-2.280***
	(0.098)	(0.461)
	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

Notes. IV estimates for 2007 to 2010. All specifications include Firm and Time Fixed effects. Robust standard errors corrected for clustering at firm and main bank level between parentheses. p-value of the F test for the exclusion restriction reported. p<0.01=***, p<0.05=**, p<0.10=*.

Table 5. The employment effect of weak-bank attachment. Instrumental Variables
Dependent variable: $\log(1+\text{Employment}_{it})$

	Weak-bank density	Exposure to REI
$Post \times WB_i$	-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

Notes. Instrumental variables estimates for 2006 and 2010. All specifications include Industry Dummies, their interaction with *Post*, and Main Bank Dummies. Firm controls as in Table 3. Robust standard errors corrected for clustering at firm and main bank level between parentheses. p-value of the F test for the exclusion restriction reported. p<0.01=***, p<0.05=**, p<0.10=*.

Table 6. Robustness checks. Difference in Differences
Dependent variable: $\log(1+\text{Employment}_{it})$

	Timing: Firms		Timing: Banks		% loans to REI	Surviving firms
	2002	2005	2000	2002		
$Post \times WB_i$	-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. obs.	192,765	271,540	181,751	246,362	387,482	353,060

Notes. OLS estimates for 2006 and 2010. The first four columns report results for deeper lags of Firm controls and Main Bank, the fifth changes the definition of weak bank, and the sixth changes the sample to surviving firms. All specifications include Industry and Province Dummies, their interaction with *Post*, and Main Bank Dummies. Firm controls as in Table 3. Robust standard errors corrected for clustering at firm and main bank level between parentheses. p<0.01=***, p<0.05=**, p<0.10=*.

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

Notes. OLS estimates for 2006 and 2010. All variables are in deviations from their means. All specifications include Industry and Province Dummies, their interaction with $Post_t$, and Main Bank Dummies. Firm controls as in Table 3. Robust standard errors corrected for clustering at firm and main bank level between parentheses. p<0.01=***, p<0.05=**, p<0.10=*.

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

WB_i	0.008**		
	(0.004)		
Share of credit with weak banks_i		0.058***	0.057***
		(0.014)	(0.007)
Share of credit with weak banks_i			-0.050***
× Single bank_i			(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

Notes. OLS estimates using firms with observations for 2006 and 2010. All specifications include Industry, Province, and Main Bank Dummies. Firm controls as in Table 3. Robust standard errors corrected for clustering at firm and main bank level between parentheses. p<0.01=***, p<0.05=**, p<0.10=*.

Table 9. The employment effect of weak-bank attachment. Differences in Differences
Dependent variable: $\Delta\log(1+\text{Employment}_{it})$

Instrumented variable:	$\Delta\log(1+\text{Credit}_{it})$	$\Delta\log(1+\text{Total Credit}_{it})$
	0.266***	0.301***
	(0.096)	(0.082)
	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

Notes. IV estimates for 2007 to 2010. All specifications include Firm and Time Fixed effects. Robust standard errors corrected for clustering at firm and main bank level between parentheses. p-value of the F test for the exclusion restriction reported. p<0.01=***, p<0.05=**, p<0.10=*.

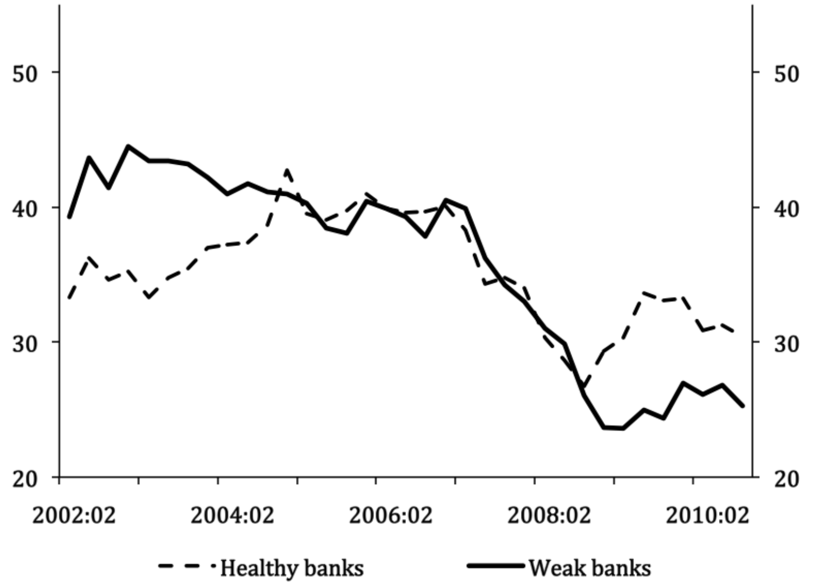

Figure 1: Acceptance rates of loan applications by non-current clients, by bank type. Firms applying to at least one bank per type (%)

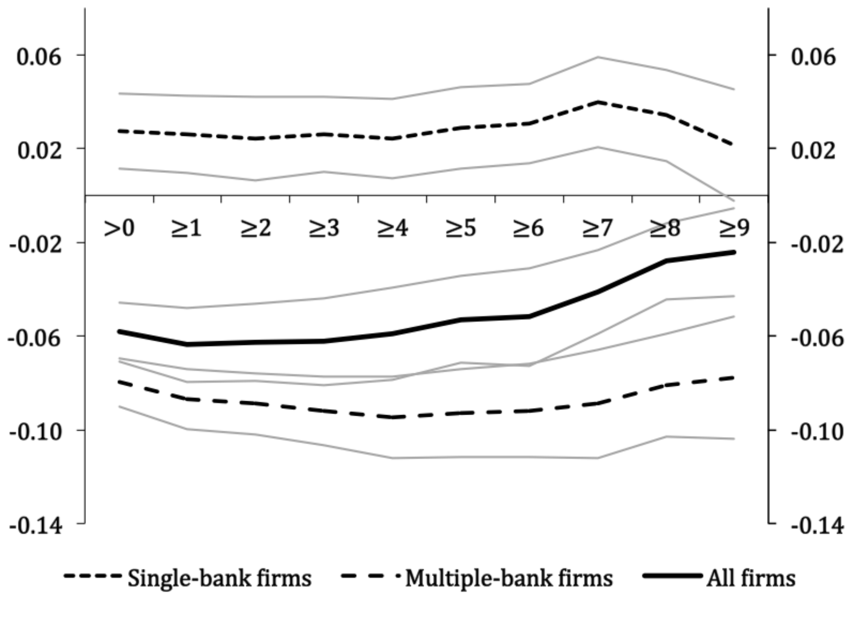

Figure 2: The employment effect of exposure to weak banks by decile and number of banks (DD estimates with 2-s.e. bands)

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