

"When Credit Dries Up: Job Losses in the Great Recession", by Samuel Bentolila, Marcel Jansen, Gabriel Jiménez, and Sonia Ruano.

Executive Summary and Conclusions

This paper studies whether the solvency problems of Spain's weakest banks during the Great Recession caused a reduction in credit supply and employment. The Spanish government bailed out 33 banks and the data from the Central Credit Register of the Bank of Spain shows that these weak banks strongly curtailed lending well in advance. We achieve identification by exploiting differences in lender health at start of the crisis, as evidenced by public bailouts of savings banks. The effects of the credit supply shock are assessed by comparing the change in employment between 2006 and 2010 at firms indebted with weak banks in 2006 and the rest.

We provide the first exhaustive analysis of the link between external funding and employment outcomes on the basis of loan data from an official credit register. Our data set can be considered as the census of loans to non-financial firms, predominantly small and medium-sized, for whom credit restrictions are the strongest according to the standard theory. Our exceptionally large and high-quality matched bank-loan-firm data set allows us to control exhaustively for ex-ante characteristics of firms and for potential endogeneity, as well as to perform a wide range of robustness checks. It also allows us to obtain more precise estimates and to refine the analysis in more directions than any of the existing studies in the field.

Our results show that the firms attached to weak banks indeed destroyed more jobs than similar firms working with healthier banks. At the level of firms, the additional employment losses are in the range between 6 and 7 percentage points. This represents between 25% and 35% of aggregate losses during our sample period

At the level of the average firm the additional job losses due to weak-bank attachment range from 6 to 7 percentage points and in the aggregate this channel explains between one-fourth and one-third of total job losses in our sample, though these estimates cannot be taken as an approximation to the macroeconomic effect, since we are ignoring general-equilibrium effects.

The extraordinary strength of the credit crunch in Spain is illustrated by the fact that we even find sizeable effects for the largest firms in our sample, whereas the evidence for the US only points at employment losses at the smallest firms. Furthermore, our analysis uncovers striking differences in the intensity of the credit restrictions depending on the creditworthiness of the firms and the structure of their banking relationships. In most cases, the lack of external funding forces firms to purge jobs, but we also find significant effects on the survival probability of firms: between 10% and 25% of these losses are accounted for by firm exits. Our paper is the first to offer this type of decomposition of the real effects in terms of employment losses at the intensive and extensive margins. The comparatively intense destruction of firms may help to explain why economies tend to recover slowly after a banking crisis, but more work is needed to understand its causes.

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 scarcely affected by that bank being weak.

Lastly, 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 would have been granted more credit than they did if they had not been attached to 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.

When Credit Dries Up: Job Losses in the Great Recession

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Abstract

This paper studies whether the solvency problem of Spain's weakest banks during the Great Recession caused a reduction in credit supply and employment. The Spanish government bailed out thirty-three banks and the data from the Central Credit Register of the Bank of Spain shows that these weak banks strongly curtailed lending well in advance. The effects of this credit supply shock are assessed by comparing the change in employment between 2006 and 2010 at firms indebted with weak banks in 2006 and the rest. The results show that the poor health of the weak banks caused significant employment losses at firms of all sizes. At the level of firms, the additional employment losses are in the range between 6 and 7 percentage points. This represents between 25 and 35% of aggregate losses during our sample period, and 10 to 25% of these losses are accounted for by firm exits.

KEYWORDS: Job losses, Great Recession, credit constraints.

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1 Introduction

Do shocks to the banking system have real effects and if so do they give rise to employment losses? Both questions have resurfaced with strength in the wake of the recent economic and financial crisis. In many advanced economies, bank lending to firms contracted substantially during the crisis and this decline in lending is commonly seen as a primary factor behind the slow recovery (IMF, 2013). In this paper we use data for Spain to explore the link between the contraction in bank lending to firms and the unprecedented fall in employment during the Great Recession. Our objective is to determine whether the solvency problems of Spain's weakest banks caused a reduction in credit supply and employment.

To convincingly establish such a link between the distress of banks and the drop in employment, we need to address several challenging identification issues. First of all, we need to disentangle the changes in credit supply from the concurrent changes in credit demand. Large macroeconomic shocks like the bursting of the housing bubble in Spain may force banks to curtail credit supply, but the same shock is likely to cause a reduction in credit demand, either because firms have fewer investment opportunities or because their collateral has lost value. Second, firms may have access to alternative sources of funding that allow them to cushion the effects of a contraction in the supply of bank credit. Last but not least, in the case of Spain we find evidence that the healthiest banks work with somewhat better and less vulnerable firms than the weakest banks. Without exhaustive controls for these characteristics this would cause a selection bias and, in the limit, causality could even go in the opposite direction, with the financial distress of firms causing the solvency problems of banks.

The unique features of our dataset allow us to address all these issues. The Central Credit Register (CIR) of the Bank of Spain contains detailed information on virtually all existing and newly-granted loans to non-financial firms. These data allow us to reconstruct the complete banking relationships of over 217,000 companies working with 239 banks. We also have information on loan demand through data on loan applications from non-current clients and we observe whether the applications are granted or not. All this information is matched to the balance sheets of the banks

and the balance sheets and income statements of the firms in our sample. Finally, we have access to historical data on the location of bank branches and the official register of firms so that we can trace employment changes both at the intensive margin and through firm closures. The result is, to the best of our knowledge, the most comprehensive matched firm-loan-bank dataset ever assembled to estimate the real effects of shocks to the banking system.

The case of Spain is also interesting by itself. Not only did the country suffer a very sharp contraction in bank lending, with the flow of new credit to firms falling in real terms by as much as 40% between 2007 and 2010, but Spanish firms are also more reliant on bank credit than their counterparts in most other advanced economies. For example, in 2006 the stock of loans from credit institutions to non-financial corporations represented 86% of GDP compared to 62% in the European Union.¹ Other sources of funding through financial markets are rarely used. On average only five large corporations per year issued publicly traded debt between 2002 and 2010, and the number of listed companies is very small (28 in our sample). Finally, the high leverage ratio of many, predominantly small and medium-sized, enterprises made them vulnerable to the contraction in bank lending that took place during the recession.

Our identification strategy exploits the large differences in lender health at the onset of the Great Recession. All banks suffered from the collapse of the construction sector and the global economic downturn, but the main problems were concentrated in the savings banks (*cajas de ahorros*). Indeed, out of the thirty-three banks that were rescued by the Spanish government at different stages during the crisis, all but one were savings banks. We refer to these bailed-out banks as *weak banks*. Inspection of the data reveals that these banks reduced lending more than the rest (*healthy banks*). The credit contraction started well before the first bailouts took place and our analysis tries to uncover how this differential evolution of lending affected the change in the levels of employment at the client firms of these banks. For our analysis we select the period between 2006 and 2010.

A careful study of the financial situation of banks at the onset of the crisis reveals that there is probably not a single cause for the demise of the weak banks. The

¹Source: European Central Bank (2010), Annex Tables 4 and 14.

most striking feature is their disproportionately high concentration of loans to the real estate industry – construction companies and real estate developers. In 2006 these loans represented 68% of their portfolio of loans to non-financial firms, compared to 37% in the case of healthy banks. But weak banks were also less capitalized and on average they relied more on wholesale funding than healthy banks, although this difference is not significant. Moreover, there are clear differences in the governance of savings banks and commercial banks (see Section 3 for details).

In our benchmark specification we therefore adopt an agnostic approach. We start by separating firms into two groups on the basis of a treatment dummy that takes the value one if a firm had at least one loan from a weak bank in 2006. But in an extension we also consider a continuous version of the treatment variable that is computed as the pre-crisis ratio between a firm’s loans from weak banks and the value of its assets. This version allows us to control for differences in the intensity of firms’ relationships with weak banks. Furthermore, in one of the robustness checks we also experiment with alternative definitions of weak bank based on bank leverage ratios and on the share of loans to the real estate industry in 2006. Unlike our benchmark definition, these alternative definitions of weak banks are not based on an ex-post outcome (bailout) and they capture characteristics of banks that are widely considered to affect banks’ lending capacity, but the underlying logic is the same in all cases. The weakness of a bank’s balance sheet may force it to reduce credit supply and this may have an adverse effect on its clients.

Obviously, the existence of a pre-crisis relationship with a weak bank would be irrelevant if firms could readily switch to a healthier bank to offset any unexpected reduction in credit supply. To some extent, our empirical analysis can therefore be interpreted as a test of the strength of financial market frictions and, in particular, of the importance of relationship banking (e.g. Freixas, 2005). This strand of the banking literature stresses that repeated interactions within a stable banking relationship may help to reduce the agency cost of lending as banks have an incentive to acquire soft information on their clients. Yet, the same agency costs also imply that it may be difficult for firms to switch to other lenders, and more so in recessions (e.g. Gobbi and Sette, 2014). Even so, the validity of our approach depends crucially on the

assumption that the firms in our sample could not foresee the solvency problems of the weak banks when they configured their banking relationships. We provide indirect evidence on this point by analyzing the risk premia of the two sets of banks in the run-up to the crisis. Finally, to avoid the risk of reverse causality we remove from the sample all firms in the real estate industry plus all firms in the industries that sold at least 20% of their value-added to the real estate industry.

Our final goal is to replicate as closely as possible the conditions of a natural experiment in which some firms are randomly assigned to weak banks and others to healthy banks. This strategy requires the ability to compare firms in many dimensions, in order to achieve homogeneity between treated and control firms. To that aim our benchmark difference-in-differences specification includes a full set of industry, municipality, and even firm fixed effects. But in order to minimize the risk of selection and bias due to differences in local demand conditions, we also use matching techniques to compare firms that operate in the same sector and municipality. Additionally, we guard against the potential endogeneity of banking relationships through the use of an instrument that exploits a change in banking regulation in 1988. Finally, in the rest of the paper we exploit the richness of our data to test whether there are differences in the way the two sets of banks treat financially vulnerable firms and what number of banking relationships facilitates the best access to credit during the crisis.

In sum, the quality and size of our data set, together with the nature of the event that we exploit, allow us to achieve a much more rigorous identification of the effect of interest than is available in the extant literature (see Section 2), as well as to explore new mechanisms.

Regardless of the approach followed, we find the same result. Weak-bank attachment caused large additional employment losses in the range between 6 and 7 percentage points. These extra employment losses represent between one-fourth and one-third of job losses in our sample, roughly 10% to 25% of which can be accounted for by firm exits. Finally, we find that the differences in employment destruction and firm exit are concentrated among the firms with multiple banking relationships.

The rest of the paper is organized as follows. In Section 2 we review previous theoretical and empirical work on the topic and in Section 3 we provide background

information on the Spanish economy before and during the financial crisis. Section 4 describes our data, Section 5 presents our empirical strategy and Section 6 our key results. Section 7 presents an extensive battery of robustness checks and Section 8 results on treatment heterogeneity. Section 9 shows our results for the probability of firms closing down and estimates of total job losses. Section 10 contains our conclusions. Two appendixes provide information on weak banks and securitizations, as well as detailed descriptions of the variables used.

2 Literature review

While the relationship between financial constraints and corporate investment has been studied extensively, comparatively little is known about the role that financial constraints and the availability of external finance play in determining the employment level of firms. Yet, from the literature on financial accelerators we know that such understanding is crucial as counter-cyclical fluctuations in the cost of external finance may amplify the cyclical volatility of aggregate output and employment (Bernanke and Gertler, 1995; Bernanke *et al.*, 1996; and more recently Gertler and Kiyotaki, 2010).

The theoretical literature has identified several channels through which the cost and availability of external finance may influence the employment decisions of firms (for recent discussions see Benmelech *et al.* 2011, or Boeri *et al.*, 2013). First, the mismatch between the timing of payments to workers and the generation of cash flow may force firms to finance salaries as part of their working capital. Second, turnover costs in the labor market transform labor into a quasi-fixed factor of production, creating a link between employment and external finance that is similar to the well-known link with corporate investment. This second strand of the literature includes recent contributions that study the interplay of financial and labor market frictions (Wasmer and Weil, 2004; Petrosky-Nadeau and Wasmer, 2013). Rather than focusing on the equilibrium levels of employment, these studies analyze the impact of financial frictions on the flows in the labor market. Similarly, financial frictions may also alter the optimal mix between permanent and temporary jobs, as the latter are cheaper to terminate, and this may in turn may have important implications for the cyclical volatility of employment (Caggese and Cuñat, 2009). Lastly, the availability of external finance

may have an indirect impact on the use of labor if capital and labor are complements in production. We do not explicitly study which of the above channels is driving our results, but we do provide evidence that the comparatively strong employment adjustments of the firms in the treatment group are indeed driven by their comparatively poor access to bank credit after the outbreak of the crisis.

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.² The two most closely related papers in this strand of the literature are the recent studies of Greenstone and Mas (2012) and Chodorow-Reich (2014). Both studies exploit the heterogeneity in lender health at the onset of the crisis to study the impact on firm-level employment. In the case of Greenstone and Mas (2012) this link is indirect, as they do not have access to loan-level data. To circumvent this problem, they construct a county-level credit supply shock from the product of the change in US banks' small-business lending at the national level and their predetermined credit market share at the county level. 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.

Chodorow-Reich (2014) does have access to loan-level data from the Dealscan syndicated loan database. He constructs a credit supply shock for the firms in his sample that is equal to the weighted average of the reduction in lending that the firm's last pre-crisis syndicate imposes on other firms during the crisis. 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 small 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 the largest companies in his sample he finds no significant effects.

Regarding the Spanish case, Garicano and Steinwender (2013) use survey data to

²Some studies exploit the heterogeneous impact of large external shocks to the banking system (Chava and Purnanandam, 2011; Benmelech *et al.*, 2012; or Ongena *et al.*, 2013), while others exploit cross-sectional differences in the financial vulnerability of firms at the start of the Great Recession. Almeida *et al.* (2011), Benmelech *et al.* (2011), and Boeri *et al.* (2013) exploit differences in the debt maturity structure of firms.

compare the response of different types of investment at foreign-owned and nationally-based manufacturing firms, taking this divide as a proxy for credit constraints. They also estimate the effect on employment, finding an additional job loss of 5.6 percentage points over the period 2008-2010 for a sample of 3,112 firms.³

We wish to highlight several important differences between the existing studies and our paper. A first difference is the nature of the exercise. Both Greenstone and Mas (2012) and Chodorow-Reich (2014) impute a credit-supply shock to each of the firms in their sample, while we focus on the differential strength of the credit restrictions for firms attached to weak banks and their implications for employment. We therefore do not obtain an estimate for the job losses in the control group, but this is more than compensated by the fact that we obtain extremely precise estimates; our sample of firms is roughly one-hundred times bigger than the one of Chodorow-Reich (2014).

Second, it is important to have estimations of the job losses due to credit supply shocks, but our analysis suggests that the employment losses due to firm closures may be even bigger. This aspect is vital since it is cheaper and quicker to create jobs than to rebuild firms once the economy starts to recover.

Last but not least, this is the first study in the field to have access to an official credit register. We can therefore reconstruct the entire banking relationships of the firms in our sample and the superior quality of the data allows us to refine our estimates in many directions. For example, we offer a detailed analysis of the interaction between the health of banks and the financial vulnerability of firms. Our analysis includes the typical indicators of financial vulnerability, such as firm size or the share of short-term debt, but also high-quality controls for the firm's creditworthiness. This analysis reveals that a bad credit history can cause job losses that are up to four times as big as our estimate of the average treatment effects and these additional losses are even stronger at weak banks.

A second important aspect that we study is whether there are gains from concentrating borrowing. Both the theoretical literature (Detragiache *et al.*, 2000) and the empirical literature (Hoshi *et al.*, 1990, or Houston and James, 2001) are ambiguous about the optimal number of banking relationships, but in line with a recent study

³This estimate is however dropped in the revised (November 2013) version of the paper.

for Italy by Gobbi and Sette (2014), we find that there are gains from working with a single bank. In fact, these gains are so strong that firms that obtained all their pre-crisis loans from a single weak bank suffer almost the same additional job losses as similar firms working with a healthy bank.

3 The financial crisis in Spain

The Spanish economy experienced a severe credit crunch in the Great Recession. In this section we briefly document its magnitude and origins, focusing on the weak banks.

3.1 The credit collapse

Spain went through a boom-bust bank credit cycle which was quite correlated with the business cycle. From 2002 to 2007, GDP and employment respectively grew at 3.5% and 4.2% per annum. By contrast, over 2008-2010 GDP fell by 3% and employment by 9%. Concurrently, the flow of new credit by deposit institutions to non-financial firms increased in real terms by 23% from 2003 to 2007 and then fell by 38% to 2010.

The credit crunch was induced by the interaction of the international financial crisis and domestic events. In the boom, the expansionary monetary policy of the European Central Bank (ECB) induced Spanish banks to take on more risk (the *risk-taking channel*, see Jiménez *et al.*, 2014). In particular, they fueled a housing market bubble with cheap loans to the real estate industry (REI), i.e. real estate developers and construction companies, and homeowners. The stock of loans to the REI grew from 14.8% of GDP 2002 to 43% in 2007 and over the same time period housing prices rose by 59% in real terms, while they fell by 15% between 2008 and 2010.

The fall of housing collateral values weakened bank balance sheets, mainly through a surge of non-performing loans to REI firms. In addition, banks funded a large fraction of these loans through debt. They were therefore hit by the 2008 freezing of Eurozone wholesale financial markets. Liquidity problems were largely overcome via ECB relief lending, whereas the balance sheet fragility of banks was not fully addressed until June 2012, when Spain obtained a loan of 41.4 bn euros (about 4% of GDP) from the European Financial Stability Facility to finance the recapitalization of weak banks.

The main problems were concentrated in the savings banks. These banks were subject to the same prudential regulation and the same supervision by the Bank of Spain as commercial banks, but they had a different ownership and governance structure. Not being listed in the stock market, they were quite limited in their ability to raise capital in response to the crisis. Simultaneously, they were also less exposed to the market discipline than commercial banks and *de facto* they were controlled by the corresponding regional government.⁴

Table 1 illustrates the differences in lender health between the set of weak banks that were bailed out by the State and the rest. All but one of the weak banks are savings banks while the only weak commercial bank was controlled by a savings bank. The comparison, based on data for 2006, shows that the weak banks were on average larger than healthy ones, less capitalized, and in possession of less liquidity. By contrast, the rate of return on assets and the share of non-performing loans are analogous for the two sets of firms, but this similarity hides latent losses at weak banks, which became apparent later on, as witnessed by the vastly larger non-performing loan ratio of weak banks in 2012.⁵ We conjecture that the main source of their troubles was their much larger exposure to real estate. In the case of weak banks, the loans to REI represented 68% of all the loans to non-financial firms vs. 37% for healthy banks. On the other hand, among the banks that securitize, their ratio of securitized loans to assets was larger, but not significantly so, suggesting that this was not a key difference.

Credit flows also differed markedly across bank types. Figure 1 depicts the annual flow of real new credit to non-financial firms by month, revealing that it grew significantly more at weak than at healthy banks during the boom –60% v. 12% from 2002 to 2007– and fell more during the slump –46% v. 35% from 2007 to 2010. These evolutions arose from changes in both the intensive and extensive margins. Figure 2 illustrates the latter by plotting acceptance rates for loan applications by non-client firms.⁶ During 2002-2004 acceptance rates were 4.2 percentage points (pp) higher for weak than for healthy banks, then both rates fell precipitously during 2007-2008, and

⁴See Cuñat and Garicano (2010), Fernández-Villaverde *et al.* (2013), and Santos (2014).

⁵Nonperforming loan ratios are probably noisier before 2012, the year when the authorities carried out stringent stress tests on the banks, supervised by the so-called *troika* (ECB, European Commission, and IMF).

⁶See a description of our loan application data set in Section 4.

subsequently acceptance rates became 3.4 pp lower for weak banks in 2009-2010.

It is important to stress that credit granted by weak banks did not recover after our sample period, which ends in 2010. Quite the opposite, it fell even more. The reason, as we will explain in the next Section, is that the recapitalization process was slow and imperfect until July 2012, with additional restructuring of loan policies in order to restore capital requirement ratios. Thus firms exposed to weak banks could not quickly regain access to credit after the bailouts.

Lastly, Figure 3 depicts the average interest rates charged by the two sets of banks and compares them to the policy rate of the ECB. This evidence suggests that interest rates were scarcely used by weak banks to ration credit demand.⁷ Indeed, the interest rates charged by both sets of banks closely follows the ECB policy rate and even after the freezing of the wholesale markets in late 2008 the difference in interest rates was always below 30 basis points. We can therefore safely focus our analysis on the differential evolution of the volume of credit at the two sets of firms during the crisis.

3.2 The bank restructuring process

The solvency problems of savings banks eventually had to be dealt with through State bailouts, which were of two types. First, two small banks were nationalized, Caja Castilla-La Mancha in March 2009 (resold to another bank in November 2009) and CajaSur in May 2010 (resold in July 2010). These operations jointly entailed public support of 4.6 bn euro, namely around 0.44% of Spanish GDP at the time.

In an attempt to minimize the cost to the taxpayer, the Government henceforth favored the alternative route of fostering the merger of banks (26 weak banks were involved) or the takeover of an ailing bank by a another bank (five weak banks involved) in the rest of the bailouts. The majority of these operations entailed State support – typically through the acquisition of preferential shares–, which was channeled through the Fund for the Orderly Restructuring of the Banking Sector (FROB) created in June 2009 (see Banco de España, 2014, for details). The first of these operations took place in March 2010. By the end of 2010, the FROB had provided assistance

⁷Stiglitz and Weiss (1981) show why imperfect information leads to credit rationing rather than interest rate differences and Petersen and Rajan (1994) show that US banking relationships operate more through quantities than through prices.

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

According to our definition a bank is weak if it was nationalized, it participated in a merger with State funding support or it was insolvent and bought by another bank, with or without State support. Banks that received funds to absorb other banks with solvency problems are considered to be healthy rather than weak.

In sum, a modest amount of funds, about 1.5% of GDP, had been devoted to the restructuring process by the end of 2010. Further consolidation operations and the bulk of the nationalizations took place in 2011 and 2012 (see Appendix 1 and International Monetary Fund, 2012, for details), but these operations did not restore the credit flow of weak banks as indicated in the previous section.

During our sample period, except for the two small nationalized banks (and for very short periods at that), the weak banks were run by their incumbent managers and not by government-appointed administrators. Moreover, due to pressure from the regional governments, the mergers that took place during 2010 were of the so-called Institutional Protection System (or SIP) form, a contractual agreement aimed at improving the liquidity and solvency of participating institutions, which remained separate legal entities.

4 Data

In this section we describe the nature and the sources of the variables included in our matched firm-loan-bank data set and the sample selection. We end by describing our treatment and control variables. Further details appear in Appendix 2.

4.1 Data set construction

To disentangle credit supply from credit demand shocks, it is essential to observe both bank and firm characteristics and, in particular, to have exogenous measures of firms' vulnerability to credit shortages. Our data set combines six different sources that contain such information. Although we focus on the period 2006-2010, we collect data starting in 2000.

Our data set is unique in having exhaustive loan and bank information for individual firms. The loan data is obtained from the Bank of Spain’s Central Credit Register (CIR), which contains 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 firms’ banking relationships, so that we know whether a firm has loans from weak banks. We also observe other variables like the number of bank relationships, collateralized loans, a measure of loan maturity, the firm’s main bank –namely the one with the largest share of its bank debt–, and non-performing and potentially problematic loans. We do not observe interest rates, but as noted above this is not a serious limitation.

The second unusual feature of our analysis is the use of loan application data. Banks routinely receive monthly information from the CIR on their borrowers’ credit exposure and defaults vis-à-vis all banks in Spain. But they can also get it on “any firm that seriously approaches the bank to obtain credit”. By matching these loan application data with the CIR we observe if the loan is granted. If not, either the bank denied it or else the firm obtained funding elsewhere (Jiménez *et al.*, 2012). Since the loan application data set only gives information on borrowing for firms with a credit history, we exclude loan applications from entering firms.

We gather economic and financial information for more than 300,000 private, non-financial firms from the balance sheets and income statements that Spanish corporations must submit yearly to the Spanish Mercantile Registers. Our source is the Iberian Balance Sheet Analysis System (SABI) produced by INFORMA D&B in collaboration with Bureau Van Dijk and the Central Balance Sheet Data Office (CBSO) of the Bank of Spain. We are able to perfectly match the data on loans, banks, and firms through firms’ unique tax ID (*código de identificación fiscal*). In this data set employment is measured as the average level over the year, weighing temporary employees by their weeks of work. The data also contain information on variables like the firm’s age, size, and indebtedness. For most firms we only have an abridged balance sheet with no breakdown of the firm’s liability structure, but for a subsample of about 8% of firms we have more detailed data on the structure of their liabilities, including alternative forms of credit such as trade credit, which we will use in our robustness

checks. Lastly, we observe the firm's industry and typically use a two-digit breakdown of nine industries, but in some instances we use a more detailed three-digit breakdown into 245 industries.

In order to disentangle job destruction in surviving firms from that due to firms closing down, we use the Central Business Register (DIRCE). It allows us to make sure that firms that are in the sample in 2006 but disappear from it in subsequent years have indeed closed down. We do not control for mergers and acquisitions (M&A). However, information on these operations is available in the CBSO sample of firms above 50 workers, where for 2012 we find that only 3% of all firm closures according to the DIRCE did result from M&A. Since M&A usually take place among large firms and in our sample only 5% of firms are above the 50-worker threshold, we expect to have a much lower fraction. For this reason we are confident that this issue does not affect our results. There may still be measurement error if a firm is closed down with one tax ID and subsequently opened with a different one, but we have no way to identify these cases.

Lastly, we enlarge our information set with two databases on banks. The first one, used for regulatory and supervisory purposes, records their financial statements. It includes 239 banks, comprising commercial banks, savings banks, and credit cooperatives. The second one contains historical data on the location of bank branches at the municipal level, which has never been used for research purposes before.

4.2 The treatment variable and the sample

In order to define which firms are exposed to weak banks, we construct the dummy variable WB_i which takes the value 1 if the firm had any loans with a weak bank in 2006. This treatment dummy allows us to measure the average treatment on the treated. We choose 2006 as the base year because both GDP and real credit were growing very quickly, at 4.1% and 19% p.a., respectively, so that neither the recession nor, especially, the credit crunch were generally anticipated then.

Furthermore, since it is likely that the extent of credit restrictions depends on the intensity of the relationship, we also consider a continuous treatment variable, $WB\ Intensity_i$, defined as the ratio of loans from weak banks to the firm's asset value.

This is the product of the ratio of debt with weak banks to total debt –weight of weak banks in debt– times the ratio of total debt to asset value –leverage.

Regarding sample selection, to avoid potential reverse causality –so that firms’ troubles drive the banks’ problems– we exclude firms in the REI and in two-digit industries selling at least 20% of their value added to the REI in 2000 (see Appendix 2). The date is chosen to minimize potential endogeneity through credit decisions taken in the later part of the boom. We work with a balanced sample, only including firms with reliable information from 2006 to 2010. In particular, we exclude firms that do not deposit their accounts after 2006 but still appear in the Central Business Register. Hence, firms are only considered as having closed down if they disappear from both registers. Lastly, since we are interested in bank credit, we exclude firms with no loans in 2006. This leaves us with 169,295 firms, representing 21% of firms, 32% of value added, and 48% of private sector employees in the industries included in our analysis in 2006.

Aggregate employment in our sample fell by 8.1%. Somewhat surprisingly, the job destruction rate was equal to 7.4% in firms attached to weak banks and 8.7% in non-attached firms, which hints at composition effects. A staggering 77% of job destruction was due to firm closures –though, as we will see below, the fraction that can be attributed to credit constraints is much lower. Such a large share is probably linked to Spain’s relatively high rank in the degree of stringency of employment protection legislation for permanent contracts: the ninth position out of 28 OECD countries in 2006.⁸ Temporary jobs entail much lower severance pay and, especially, the worker’s inability to challenge the dismissal in labor courts. However, these jobs represented 23% of employment at firms exposed to weak banks, so that once temporary jobs have been destroyed, it was quite costly and difficult to dismiss regular employees, so that eventually some firms had to close down.

Table 2 provides descriptive statistics for our treatment ($WB_i = 1$) and control groups for 2006. About 61% of firms had no credit from weak banks in 2006, while for those that did the average share of credit represented by weak banks was 62% and their ratio of weak-bank credit to assets was 18%. Compared with the control group,

⁸Source: OECD Indicators of Employment Protection, 2013 release.

firms in the treatment group are on average older and larger, and they have more temporary workers. On the other hand, they have a worse financial profile: they are less capitalized and profitable, they have less liquidity, and they are more indebted with banks. They work with three banks on average and over 2002-2005 they defaulted more often on their bank loans. These differences are not always large, but they are statistically significant. They may originate in different credit standards, different regional or sectoral configurations. In any event, these differences imply that we must exhaustively control for firm-level characteristics in our empirical analysis, since weak banks were more likely to grant loans to less profitable and potentially more vulnerable firms than healthy banks.

5 Empirical strategy

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

$$\log(1 + n_{ijkt}) = \alpha_i + \beta Post + \gamma Post WB_i + Post d_j \delta + Post d_k \lambda + u_{ijkt} \quad (1)$$

where n_{ijkt} is employment at firm i in municipality j , industry k , and year t ($t=2006$ and 2010), α_i is a firm fixed effect, $Post$ is a dummy variable for 2010, WB_i is a dummy variable for treated firms, d_j is a vector of municipality dummy variables (4,859), d_k is a vector of industry dummy variables (9), and u_{ijkt} denotes a random shock.

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

The fixed effects capture all potential, constant differences among firms. Alternatively, firm control variables in 2006 would play the same role, but the firm fixed effects capture unobservable differences as well. Differences across industries or municipalities in the impact of the recession are absorbed by interactions between d_j and d_k with the crisis dummy. 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 credit constraints stemming from attachment to weak banks, as opposed to other banks, measured by γ in equation (1). We estimate it in first

differences, thereby getting rid of the fixed effects. In the case of just two periods, this is identical to estimating the equation in levels (Wooldridge, 2010).

Parameter γ is an estimate of the average treatment on the treated (ATT), but it can only be unbiased under random assignment of firms to the treatment and control groups. What are the main threats to identification? To start with, it may be objected that our treatment is defined in terms of an outcome, namely bank bailout, that is realized after the crisis broke out. Using an ex-post criterion does not invalidate our results as long as the outcome was unforeseen. To study whether in 2006 firms could have anticipated the future solvency problems of weak banks, we analyze the risk premia charged to Spanish banks' securitization issues prior to the recession. We employ data on tranches of mortgage backed securities (MBS) and asset backed securities (ABS) in 2006. We group the ratings into prime (AAA), investment grade (AA+ to BBB-), and speculative (BB+ to D). We have 303 observations (deal-tranches) from Dealogic, with a floating rate, quarterly coupon frequency, and referenced to the 3-month Euribor, from 24 issuer parents.

Without any controls, weak banks actually paid 7 basis points less than healthy banks. To control for issue characteristics, we regress coupon differentials in basis points on variables capturing the type of securitization, risk category, month of issue, years to maturity, collateral type, and guarantor type. Standard errors are clustered by issuer parent. The estimated coefficient associated with the weak bank dummy is positive but 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 in 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 it either.

We aim at replicating as closely as possible the conditions of a natural experiment in which firms are randomly assigned to the treatment and control group, in our case weak and healthy banks, respectively. We see as the two key threats to identification the non-random assignment of firms to banks prior to the crisis and the presence of demand effects. The importance of the former is highlighted by the different characteristics of the firms working with weak and healthy banks before the crisis. As shown in Section

4, firms in the treatment group have worse financial statistics. Therefore, laxer loan-approval criteria at weak banks may have caused a systematic bias in the risk profile of the treated companies. To avoid such selection bias we include firm fixed effects in all our specifications and also present estimates from an instrumental variables model aimed at obtaining exogenous variation in weak-bank attachment.

Demand effects are also a very important concern (Mian and Sufi, 2014). On the one hand, lending grew especially for the real state industry and it was therefore more concentrated in certain areas, where in the recession we might observe both a larger drop in demand by households and a higher density of (non-REI) firms exposed to weak banks. In these circumstances employment reductions would stem from lower consumption demand rather than from less credit. The fact that small firms tend to be financed by local banks (Petersen and Rajan, 2002; Guiso *et al.*, 2013) would additionally contribute to the presence of local demand effects. The standard approach of analyzing employment changes within regions or even provinces may be too coarse to credibly control for local demand effects. For this reason, we deal with this issue by controlling for differential trends through the addition of interactions of *Post* with 4,859 municipality and with nine industry dummies.

After discussing the ATT we deal with other issues, such as making sure that changes in credit, absent in the DD specification, are driving the results, exogenous variation in treatment exposure, and treatment heterogeneity. We end with an analysis of the effect of weak-bank attachment on the probability of a firm exiting.

6 Baseline estimation results

We now present the estimation results for our DD equation (1). We report robust standard errors corrected for clustering at firm and main bank level. Unless otherwise noted, all estimates are significant at the 1% level. When only firm fixed effects are included, employment at firms exposed to weak banks falls by 7.7 percentage points (pp hereafter) more than at non-exposed firms. As shown in Table 3, including the interactions of *Post* with municipality and industry further reduces the treatment effect to -6.8 pp (col. 1). We adopt this specification as our baseline, since it is likely

to properly control for local demand effects.⁹

The timing of the impact of the credit constraint, measured by choosing alternative ex-post periods, is as follows (pp): 0.1 (0.3) for 2007, -1.1 (0.5) for 2008, and -3.8 (0.6) for 2009, so that it does not become significant until 2008.¹⁰ We also test for differences in pre-crisis trends for treated and control firms by running a placebo equation for the baseline specification but with 2002 and 2006 as initial and final dates, respectively. As required, this specification test delivers a null coefficient (col. 2).

As indicated, firm closures represent 77% of observed job losses in our data. For this reason, it is worth reestimating the effect of interest for surviving firms alone. The estimated job loss for treated firms is equal to 2.7 pp (col. 3), which is significantly lower than for the full sample. Credit constraints appear to have been more important in driving firm closures than in leading surviving firms to cut jobs. We return to this issue in Section 9.

6.1 Demand effects

We check the impact of alternative ways to control for demand effects. First, we include interactions of *Post* with the joint product of municipality and 3-digit industry dummies (245). Although we lose 21% of our observations, our estimate is hardly altered, becoming equal to 7 pp (col. 4).¹¹

Second, instead of introducing industry and municipality dummy variables, we compare treated and non-treated firms within municipality and two-digit industry cells. We do it by using the coarsened exact matching method (Iacus *et al.*, 2011). We end up with 18,126 strata with observations, 8,116 of which can be matched across treated and control firms. Using weighted least squares, the estimated employment effect of weak bank attachment is equal to -6.5 pp (col. 5).

⁹With 50 province dummies replacing the municipality dummies the estimated effect is -7.6 pp (1.0), and a Tobit model with firm controls (see Appendix 2) and firm random effects yields an estimate of -6.4 pp (0.4).

¹⁰The full results are not shown to save space. Similarly, we report some estimates of secondary importance in the text or in footnotes rather than in the tables, with standard errors given between parentheses. All are available upon request.

¹¹A specification including the interaction of *Post* with the product of municipality \times 3-digit industry dummies \times Main bank dummies raises the estimate to -8.3 pp (1.1), but this result is obtained for a 42% smaller sample, due to the lack of observations in many cells.

Next, Mian and Sufi (2014) argue that local demand effects should only affect output in non-traded goods sectors, while credit supply shocks should affect both traded and non-traded goods sectors. We therefore aim at filtering out local demand effects by restricting attention to traded sectors. Mian and Sufi (2014) use two classifications, one based on ad-hoc tradability criteria and another one based on geographical concentration. We prefer the latter, since more concentrated industries are likely to be more traded and hence less dependent on local demand conditions.¹² We follow these authors in computing the Herfindahl concentration index for 3-digit industries and 50 provinces, and labeling as tradable those in the highest quartile. Restricting the sample to traded goods industries yields a negative effect on employment of 8.8 pp (col. 6), which is larger than the baseline, possibly because these industries are more credit-dependent than less-concentrated ones.

An exhaustive way to control for local effects is to interact the firm fixed effects with *Post*, namely:

$$\log(1 + n_{it}) = \alpha'_i + \pi_i t + d_t \beta' + d_t W B_i \gamma' + u'_{it} \quad (2)$$

where common variables are defined as in equation (1), α'_i and π_i are both firm fixed effects, t denotes a trend, for $t = 2007, \dots, 2010$, d_t is a vector of time dummies, and u'_{it} denotes a random shock. We then take first differences on equation (2) and estimate the fixed effects model (Wooldridge, 2010):

$$\Delta \log(1 + n_{it}) = \pi_i + \theta_t \beta' + \theta_t W B_i \gamma' + \Delta u'_{it} \quad (3)$$

where $\theta_t \equiv \Delta d_t$ is another set of time dummies. Now the parameter of interest is the coefficient vector γ' . The equivalent of γ in equation (1) is the parameter for 2010, whose value is relative to 2007. As shown in Table 3, its estimate is -7 pp (col. 7).¹³ This magnitude is also valid as the effect of weak bank exposure for 2010 vs. 2006, since the estimate of our DD equation for 2006 and 2007 was not significant, and very close to the baseline estimate.

¹²As found by Mian and Sufi (2014) for the US and by Moral and Ramos (2013) for Spain.

¹³This is a fixed-effects estimation on first-differenced data (Wooldridge, 2010). The standard error is computed for a two-way cluster. It would be advisable to also cluster by year, but the low number of periods in our sample would lead to downward-biased standard errors.

6.2 Weak bank definition

Let us now characterize a bank as being weak if its 2006 loan exposure to firms in the REI, measured as the share of loans to the REI over the total value of its outstanding loans, was within the upper quartile of the distribution. Table 3 shows that now weak-bank attachment leads to an employment fall of 6.6 pp, which is very similar to our baseline (col. 8).

We saw in Section 3 that weak banks had a lower “leverage ratio” –own funds to total assets– than healthy banks. Let us then alternatively define weak banks as those having leverage ratios below the lowest quartile and treated firms as those having some exposure to them. The estimated effect on employment is now equal to -2.5 pp (1.2), which is significantly different from our baseline estimate. However, this is a less accurate definition of weak bank, since the difference in leverage ratios in 2006 is only marginally significant and because banks with low leverage ratios reduced new credit by 3.3 pp more than the rest, whereas the difference between bailed out and non-bailed out banks is equal to 11 pp. The reason is that ex-ante leverage ratio discrepancies were too small, in that they did not adequately reflect latent portfolio losses.

7 Transmission mechanism and exogenous exposure

In this section we further test the robustness of our baseline estimates, following three avenues: checking that credit is the actual channel of transmission of exposure to weak banks and whether the effect also holds for non-bank credit, addressing the potential endogeneity of the treatment, and estimating treatment intensity in terms of both firms’ financial vulnerability and their level of exposure to weak banks.

7.1 Is credit the transmission mechanism?

So far we have only estimated a closed-form relationship between weak-bank exposure and employment. To check whether credit is the key channel underlying this relationship, we estimate the following panel-data, instrumental variable (IV) model for the

proportional change in employment:

$$\begin{aligned}\Delta \log(1 + n_{it}) &= \alpha_i'' + d_t \beta'' + \gamma'' \Delta \log(1 + Credit_{it}) + u_{it}'' \\ \Delta \log(1 + Credit_{it}) &= \rho_i + d_t \eta + d_t WB_i \mu + v_{it}\end{aligned}\tag{4}$$

where common variables are defined as before, $Credit_{it}$ is total credit committed by banks to firm i in year t –both drawn and undrawn, so as to minimize potential endogeneity– for $t = 2007, \dots, 2010$, ρ_i is a firm fixed effect, and v_{it} is a random shock.

Coefficient vector μ captures the differential impact of weak banks on credit committed during the crisis, whereas γ'' captures the pass-through from credit to employment. Thus, $\gamma''\mu$, with the value of μ for 2010, is the equivalent of γ in the preceding section. The panel-data model (3) can be seen as the reduced form of this IV model. Note that we are now using growth rates because –as in the related literature– we are better able to explain credit changes than its levels. The exclusion restriction is that working with a weak bank alters employment growth only through credit changes, as opposed to other channels.

As shown in the first stage panel of Table 4, the instrumental variable is negatively correlated with credit, increasingly so as the recession lengthens (col. 1). In the second stage, credit is a significant determinant of employment changes, so that a one percentage point increase in credit raises employment by 0.58 points. This coefficient times the weak-bank effect on credit for 2010 (-0.105) yields an employment reduction of 6.1 pp in 2010 with respect to 2007 (the omitted year).¹⁴ This is very close to our baseline DD estimate, confirming that credit is the mechanism through which weak-bank exposure operates.

We have so far focused on bank credit, as the major funding source for Spanish firms. However, trade credit is an alternative source, and a firm’s suppliers may have advantages over banks as credit providers, in terms of acquiring information, monitoring, and efficient liquidation (Petersen and Rajan, 1997). We cannot fully ascertain whether trade credit may have compensated for constraints in bank credit, because we only have data on firms’ liability structure for a subsample of 13,477 firms (8% of the total). These firms provide more detailed public accounts and tend to be

¹⁴The effect vis-à-vis 2006 is unidentified, but an IV estimation for 2007 alone gives a non-significant coefficient with respect to 2006, so that the former effect is likely to be around 6.1 pp as well.

the largest ones. For example, in 2006 their median assets were equal to 9.1 million euros, vis-à-vis 0.58 million euros in our full sample. For them, at the median, financial institutions and trade credit each represent 34% of their liabilities.

Estimating the IV model (4) for this smaller sample we find that the weak bank dummy interactions are significant in the first stage, a smaller pass-through from credit to employment, and an overall effect of weak-bank attachment on employment of 3.9 pp (col. 2), which is lower than the full sample estimate. This is consistent with the vast finance literature that finds large firms to be less financially constrained than small ones. Using total credit rather than just bank credit, we find an overall employment effect which is very similar as for bank credit in this reduced sample, 4.2 pp (col. 3). We can therefore conclude that trade credit did not alleviate the bank credit constraint for large firms. While we cannot directly test this result for smaller firms, our result is consistent with Molina Pérez (2012), who finds no increase in trade credit taken by firms over the early crisis (2008-2010) with a sample of 9,602 Spanish firms, 85% of which are small and medium-sized (below 250 employees).

In the last column we return to our full sample and replace credit growth with an alternative measure of credit constraints, an indicator for having a loan application rejected in 2006. The first stage is still significant and now the causal effect of a loan rejection is a very large reduction in employment, of about 83% ($1 - e^{-1.776} = 0.83$). Note that now we are measuring a local average treatment effect for firms on the margin of having a loan approved (Imbens and Angrist, 1994),¹⁵ whose rejection may entail the firm having to close down. The overall effect coincides with the baseline estimate in Table 3.

7.2 Exogenous variation in exposure to weak banks

Firms choosing to work with a weak bank may have been driven by particular motives such as its laxer credit standards. Thus, to rule out selection effects we need an exogenous source of variation in attachment to these banks. We exploit two variants.

First, we employ a regulation-based instrumental variable. Up until 1988 savings banks could open no more than 12 branches outside their region of origin, but at

¹⁵Under the monotonicity assumption that access to credit always improves with lender health.

the end of December 1988 all location restrictions were removed (Real Decreto-ley 1582/1988). We therefore use as IV the local weak-bank density, i.e. the share of bank branches in December 1988 belonging to our set of weak banks in the firm's municipality. This variable should capture exogenous variation in the probability of weak-bank attachment, since a firm is more likely to work with a bank that has traditionally operated in the area where it is located.

The exclusion restriction is that local weak-bank density only affects a firm's employment through its exposure to weak banks. Each weak bank originated in a specific province. A problem would arise if weak banks granted more credit in their province of origin, applying lower standards, so that employment would then fall more in the crisis in firms located in those provinces. We can informally check this story by examining non-performing loan rates by province ex-post (in 2013). We find that in non-origin provinces the average rate is 2.1 times higher for weak than for healthy banks, whereas in origin provinces this ratio is equal to 2.3. This small difference suggests that there were no large differences in lending policies depending on the origin province.

In Table 5 we see that high weak-bank density in 1988 significantly predicts weak-bank attachment 18 years later (col. 1).¹⁶ The associated employment effect amounts to 7.4 pp, which is higher than the DD baseline value though not significantly so.

Lastly, we use traditional bank ties to real estate firms to make sure that credit restrictions faced by firms exposed to weak banks do not simply result from poor bank management. Our instrumental variable is now the bank's exposure to the REI in 2000, well before the beginning of the bubble –commonly thought to have started around mid-2003 (Ayuso and Restoy, 2006). The instrument is very powerful and it yields an employment effect of predicted weak-bank exposure of 10.8 pp (col. 2), which is (borderline) significantly higher than our DD baseline. This finding suggest that to some extent weak banks got into trouble because of their historical ties to real estate firms and not only due to their aggressive real estate lending just before the crisis.

¹⁶This specification includes as controls interactions of *Post* with 9 industry dummies and with a dummy variable for coastal and insular provinces.

8 Treatment heterogeneity

We explore two dimensions of treatment heterogeneity, one related to the financial vulnerability of firms and the other to the degree of exposure to weak banks.

8.1 Financial vulnerability

So far we have considered average treatment effects. However, the literature on relationship lending and financial accelerators indicates that smaller, less transparent, and financially weaker firms should be more vulnerable to changes in credit market conditions. Moreover, it is likely that the actual intensity of the banking relationship also plays a role in a bank's decision to ration credit

To find out if these features alter the real impact of credit constraints, we begin with a triple difference (DDD) model with interactions between *Post*, the weak-bank dummy, and five indicators of financial vulnerability. The first two indicators are dated in the run-up to the crisis (2002-2006): having a loan application rejected and having defaulted on any loan. The other three indicators refer to 2006. First, a standard way to measure financial vulnerability is through the value of a firm's short-term debt at the onset of the crisis (Almeida *et al.*, 2012). Accordingly, we create a dummy that takes the value 1 if the firm has a share of short-term debt in total bank debt above one-half, implying that it subsequently had to renew a sizeable fraction of it. The final two indicators capture small firms –defined following the standard EU criterion as having assets below 10 million euros– and firms indebted to just one bank.¹⁷

The estimates appear in Table 6. As expected, having a bad credit record –given by past rejected loan applications and, especially, loan defaults– or high short-term debt entails extra job losses during the crisis. These effects are stronger for firms exposed to weak banks. For example, the weak bank effect almost doubles for firms with loan defaults and it increases by one-half for firms with sizeable short-term debt. It is noteworthy that, while small firms working with healthy banks do not suffer additional losses in the crisis, those working with weak banks do.

Lastly we examine whether the impact of credit constraints varies with the number

¹⁷To avoid having to weigh estimates by the variables' average values, regressors are in deviations from their means.

of banking relations. We distinguish between firms working with only one bank from the rest. As indicated in Section 2, the empirical literature has not reached a robust conclusion on this issue but evidence for the Great Recession does find less credit constraints for single-bank firms. For Spain we expect to obtain the same because in our sample single-bank firms have better ratios of capitalization, liquidity, return on assets, bank debt, and credit record than multi-bank firms, for those working with both weak and healthy banks. Our expectations are confirmed: job losses at single-bank firms are 2.8 pp lower than at multi-bank firms. Given the ratios above, this can be interpreted as the result of a flight to quality.

Indeed, our firm-bank-loan database allows us to check which banks are rationing credit. We regress the yearly log change in credit committed in the recession to firm i by bank l , $\Delta \log(1 + Credit_{it})$, on a healthy bank dummy variable, $Healthy_l$, and the interactions $Multibank_i \times Healthy_l$ and $Weakshare_i \times Healthy_l$, where $Weakshare_i$ is the share of bank credit that firms had with weak banks in 2006 and $Multibank_i$ captures multi-bank firms.¹⁸ Firm and bank \times year fixed effects are also included to control for unobserved demand and supply factors. The data set has 2,311,840 observations on 197,597 firms. The estimated coefficient on $Multibank_i \times Healthy_l$ is -1.809 (64.254), which does not reveal a differential treatment of multi-bank firms by healthy banks vis-à-vis weak banks, whereas the coefficient on $Weakshare_i \times Healthy_l$ is -0.307 (0.071), indicating that healthy banks differentially cut credit to firms with a higher dependence on weak banks. The latter cross-effect is, as far as we know, a novel result in the literature.

8.2 Degree of exposure to weak banks

We expect the effect on employment to increase with the degree of exposure to weak banks. To find out if this is so we reestimate equation (1) using a measure of the intensity of exposure, captured by the ratio of loans from weak banks to the firm's asset value in 2006. As shown in Table 2, for exposed firms the average value of this ratio is 18%, ranging from close to zero to 98%. We allow for a non-linear effect by

¹⁸Note that the triple interaction $Weakshare_i \times Multibank_i \times Healthy_l$ coincides with the double interaction $Weakshare_i \times Healthy_l$ because the latter is equal to zero for single-bank firms.

including a cubic polynomial of this ratio.¹⁹ In view of the finding in the previous subsection, we additionally present estimates for single-bank and multi-bank firms.

The results appear in Table 7. For the total sample, all terms of the polynomial are significant. Evaluated at the average intensity of exposure (7%), it gives an overall effect of 6.8 pp, coinciding with the DD baseline. The shape of the estimated polynomial is shown in Figure 4a. While the impact is non-monotonic for the total sample, this finding stems from a composition effect.

As shown in Table 7, for single-bank firms the impact is low and generally non-significant. The polynomial in Figure 4b shows a significant effect only for exposures between 0.1 and 0.3, and evaluated at these firms' average intensity the effect is equal to -1.6 pp. On the contrary, for multi-bank firms all polynomial terms are significant and Figure 4c reveals that job losses monotonically increase with exposure, reaching a plateau of 10 pp after an exposure ratio of 14%. At the average exposure intensity, the estimated effect is -7.7 pp for multi-bank firms.

9 Probability of exit and job loss estimates

Since a large fraction of observed job losses stem from firm closures, it is natural to estimate the effect of weak-bank attachment on firm exit probability. We start with a linear probability model for exit in 2010 with respect to 2006, using the same specification as in our baseline DD equation (1). In this cross section we cannot include firm fixed effects, but we can exploit the rich contents of our data set to include many economic variables to control for selection. Thus, apart from municipality and industry dummies, we add the following firm characteristics ex-ante: (a) firm characteristics: age and its square, size (log assets), and rate of return on assets; (b) financial health: bank debt ratio, shares of short-term (up to one year) and long-term bank debt (above 5 years), and liquidity and own-funds ratios; (c) lending: number of past (2002-2005) loan applications to non-current banks and an indicator for whether all were accepted, dummy indicators for having any past loan defaults, any current loan defaults, and any credit lines, number of banking relationships and its square, and share of loan

¹⁹Given the use of a cubic, very large observations, above the 95th percentile, are winsorized at that value.

amounts that are uncollateralized; (d) a full set of main bank dummies; and (e) share of temporary contracts, proxying for both lower firing costs and future credit constraint expectations (Caggese and Cuñat, 2008).

As seen in the first column of Table 8, weak-bank exposure leads to a marginal increase in the exit probability of weak-bank dependent firms of 0.6 pp vs. non-dependent firms (col. 1). In the panel version of this specification for 2010 vs. 2007, which is the same as in col. (7) of Table 3 for the DD, we can include firm fixed effects to control for unobserved heterogeneity. This yields a larger estimate: 1.3 pp, which amounts to 13% of the baseline exit rate of 10% (col. 2).²⁰

We also try an alternative specification with our continuous treatment variable, the ratio of weak-bank credit to assets, in place of the dummy. The estimated effect is 6.3 pp for the cross section and 9.1 pp for the panel (cols. 3-4).²¹ The latter estimate implies that *ceteris paribus*, compared to a firm with a ratio of weak-bank debt to assets at the first decile –which is roughly nil–, a firm located at the ninth decile –i.e. with an exposure ratio of one-quarter– has a 22.7% higher probability of closing down.

The last column confirms that single-bank firms are hardly affected from working with a weak bank. Their marginal probability of exit is 1.1 pp., i.e. 5.2 pp less than for multi-bank firms. Therefore, the ninth-decile firm working with a weak bank would only be 2.7% more likely to close down than the first-decile firm.

9.1 Job loss estimates

With all our estimates in hand we can now compute estimates of job losses in our sample. A caveat is in order. Ours are microeconomic estimates and it would be incorrect to extrapolate them to the aggregate economy. In general equilibrium there are further effects (see Chodorow-Reich, 2014). 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. Therefore, the microeconomic effects may not be blown up to the aggregate effect.

²⁰As with the overall job loss, a linear probability model of 2006 and 2007 produced an estimate of zero, so that the panel estimate provides the effect also for 2010 v. 2006.

²¹The model for 2006 and 2007 gives an estimate of 0.4 pp (s.e. 0.3 pp).

With the 6.8 pp coefficient from our DD baseline and the initial employment share of treated firms, exposure to weak banks explains a 2.9% fall in employment.²² Since the overall employment reduction from 2006 to 2010 in our sample is equal to 8.1%, weak-bank exposure would then account for 35.5% of total job losses.

An alternative computation takes into account the divide between surviving and closing firms. On the one hand, we compute employment reductions for survivors as described above. On the other hand, we calculate the number of firm closures from the estimated probability of exit (with the cross section) and the employment drops induced. Adding up the two estimates, we get a very similar prediction: 36% of overall job losses are imputed to the weak-bank effect. Of these, 89% come from survivors and 11% from closures. This relatively small share, vis-à-vis the already mentioned 77% of observed job losses due to closures, arises both from the actual parameter estimates and from the fact that exiting firms only account for 6% of initial employment.

Lastly, we can alternatively exploit the results from the exposure intensity specification in Table 7. Aggregating estimated job losses for each individual firm, with its own exposure, we get that weak-bank attachment would account for 25.5% of total job losses. When we estimate a separate polynomial equation for survivors and perform the same calculation as above for closing firms with the exposure intensity measure, we find that the share of these job losses coming from surviving firms falls to 76%.

Our results are within the bounds 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 below 20 employees and for 16% of the total employment loss. On the other hand, Chodorow-Reich (2013) finds a firm-level maximum effect of 5 pp, as opposed to our 6-7 pp. Then, through a partial-equilibrium aggregation exercise, he calculates 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, apart from the fact that they refer to

²²Since $(1 + n_{ijkt})/(1 + n_{ijkt-1}) = \exp(\Delta \log(1 + n_{ijkt}))$, from eq. (1) the firm-specific estimated employment growth rate is equal to: $\Delta \hat{n}_{ijkt} = (1 + n_{ijkt-1})[\exp(\hat{\beta} + \hat{\gamma}WB_i + \hat{\delta}d_j + \hat{\lambda}d_k) - 1]$, where $t = 2010$ and $t - 1 = 2006$. Estimated job losses due to weak-bank attachment then equal: $\Delta \hat{n}_{ijkt} - (\Delta \hat{n}_{ijkt} | WB_i = 0) = (1 + n_{ijkt-1})[\exp(\hat{\beta} + \hat{\gamma} + \hat{\delta}d_j + \hat{\lambda}d_k) - \exp(\hat{\beta} + \hat{\delta}d_j + \hat{\lambda}d_k)]$. Total estimated job losses due to weak-bank attachment are computed by adding up individual estimated job losses for all firms with $WB_i = 1$.

shorter periods, these estimates are not directly comparable to ours. In particular, we have achieved identification by focusing on credit constraints arising from only one channel, exposure to weak banks, while controlling 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 that defaulted on a loan in the recent past is almost five times larger than for non-defaulting ones. This finding suggests that the existing literature may be overestimating the impact of credit on employment, due to the lack of sufficient controls, firms' financial history in particular, to attain homogeneity between treated and control firms.

10 Conclusions

In this paper we have analyzed the link between the solvency problems of Spain's weakest banks and the severe drop in employment during the Great Recession. We achieve identification by exploiting differences in lender health at start of the crisis, as evidenced by public bailouts of savings banks. We proceed by comparing employment changes from the expansion to the recession between firms that are heavily exposed to weak banks and less exposed firms. Our exercise is more challenging than is typical, since the bank solvency problems are linked to corporate loan portfolios.

We are not the first to study the link between external funding and employment outcomes, but we do provide the first exhaustive analysis of these links on the basis of loan data from an official credit register. For practical purposes this data set can be considered as the census of loans to non-financial firms of all sizes, but predominantly small and medium-sized, for whom credit restrictions are strongest according to the standard theory. Our exceptionally large and high-quality matched bank-loan-firm data set allows us to control exhaustively for ex-ante characteristics of firms and for potential endogeneity, as well as to perform a wide range of robustness checks. It also allows us to obtain more precise estimates and to refine the analysis in more directions than any of the existing studies in the field.

Our results show that the firms attached to weak banks indeed destroyed more jobs than similar firms working with healthier banks. At the level of the average

firm the additional job losses due to weak-bank attachment are in the range of 6 to 7 percentage points and in the aggregate this channel explains between one-fourth and one-third of the total job losses in our sample, though these estimates cannot be taken as an approximation to the macroeconomic effect, since we are ignoring any general-equilibrium effects.

The extraordinary strength of the credit crunch in Spain is illustrated by the fact that we even find sizeable effects for the largest firms in our sample, whereas the evidence for the US only points at employment losses at the smallest firms. Furthermore, our analysis uncovers striking differences in the intensity of the credit restrictions depending on the creditworthiness of the firms and the structure of their banking relationships. In most cases, the lack of external funding forces firms to purge jobs, but we also find significant effects on the survival probability of firms. Our paper is the first to offer this type of decomposition of the real effects in terms of employment losses at the intensive and extensive margins. The comparatively intense destruction of firms may help to explain why economies tend to recover slowly after a banking crisis, but more work is needed to understand its causes.

Our results for aggregate job losses are within the ranges found in the preceding US literature, but we find stronger effects at the firm level. 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 scarcely affected by that bank being weak.

We can also make a final 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 would have been granted more credit than they did if they had not been attached to 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.

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

Table A1. Spanish savings banks' restructuring process

	December 2006	Dec. 2007	Dec. 2008	Dec.2009	December 2010	December 2011	June 2012
					Banco Base (SIP)	Liberbank (SIP)	Liberbank
1 Caja C.-La Mancha					Cajastur	Cajastur	
2 Caja Cantabria					Banco CLM	Banco CLM	
3 Caja Extremadura					Caja Cantabria		
					Caja Extremadura		
4 C. A. Mediterráneo					C. A. Mediterráneo	Banco CAM	Banco Sabadell
5 Caja Madrid					BFA (SIP)		Bankia / BFA
6 Caja Rioja					Caja Madrid		
7 Caixa Laietana					Caja Rioja		
8 Caja I. Canarias					Caixa Laietana		
9 Caja de Segovia					Caja I. Canarias		
10 Caja de Ávila					Caja de Segovia		
11 Bancaja					Caja de Ávila		
12 Banco de Valencia					Bancaja		Banco de Valencia
					Banco de Valencia		
13 La General					Mare Nostrum (SIP)		Mare Nostrum (SIP)
14 Caja de Murcia					La General		
15 Caixa Penedès					Caja de Murcia		
16 Sa Nostra					Caixa Penedès		
					Sa Nostra		
17 Caixa Catalunya					CatalunyaCaixa		Catalunya Banc
18 Caixa Manresa							
19 Caixa Tarragona							
20 Caja de Burgos					Banca Cívica (SIP)		Banca Cívica (SIP)
21 Caja de Navarra					Caja de Burgos		
22 Caja Canarias					Caja de Navarra		
					Caja Canarias		
23 Caja S. Fernando		Cajasol			Cajasol		
24 El Monte							
25 Caja Guadalajara							
26 Caixanova					Novacaixagalicia		NCG Banco
27 Caixagalicia							
28 Cajasur					Cajasur	Grupo BBK	Grupo Kutxabank
29 Caja España					Caja España-Duero	G. Caja España-Duero	Banco CEISS
30 Caja Duero							
31 Caixa Manlleu					Unnim		Unnim Banc
32 Caixa Sabadell							
33 Caixa Terrassa							

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

Table A2. Returns on securities issued by Spanish banks in 2006
 Dependent Variable: Coupon differential in basis points

	Coefficient	St. error
Weak Bank	2.84	4.74
Mortgage Backed Security	15.55	0.29
Years to Maturity	0.83	0.13
Investment Grade (AA+ to BBB-)	24.37***	2.35
Speculative Grade (BB+ to D)	131.01***	25.17
Collateralized Debt Obligation	0.32	17.61
Customer Loan	2.76	7.95
Corporate Loan	5.55	14.16
Residential Mortgage	-18.90**	8.82
No Guarantor	-5.65	6.96
Private Sector Bank Guarantor	13.33	13.43
State/Provincial Authority Guarantor	-4.41	10.56
Supranational Guarantor	4.65	5.43
R^2	0.44	
No. of observations	255	

Notes. OLS estimates of coupon differentials of all asset and mortgage backed securities issued by Spanish banks in 2006 with reference to the 3-month Euribor. Reference group: Asset Backed Security, Prime Risk (AAA), Auto Receivables as collateral, Central Government as guarantor. Data for 24 issuer parents drawn from Dealscan. Month of issue dummies are included. Standard errors are adjusted for 24 clusters in issuing bank.

B Appendix 2. Definitions of variables

Employment. Computed as the average level over the year, weighing temporary employees by their weeks of work. The Temporary Employment ratio divides the temporary by the total number of employees.

Treatment. The Weak Bank dummy (0-1) is equal to 1 if the firm had any loan from a weak bank in 2006. The Weak Bank Intensity is the ratio between the total value of a firm's loans from weak banks and its book value in 2006.

Municipality. There are 4,859 municipalities, corresponding to the firm's headquarters. They need to have at least two firms in the sample. For matching, the dummy takes on the value 1 for those in the East coast of Spain and in the Balearic and Canary Islands.

Province. There are 50 provinces, see www.ine.es.

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

Industry dummies (firm's self-reported main activity in 2006): 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.

Balance sheet and income statement control variables (stocks are 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 triple differences, small firms are those with Total Assets below 10 mn euros.

Credit-related control variables. Credit Line (at least one), Current Defaults (any non-performing loan in 2006), Past Defaults (any non-performing loan in 2002-2005), Loan Applications, All Applications Accepted (in 2002-2005). For triple differences we use an indicator for any current or 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).

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Table 1. Descriptive statistics of healthy and weak banks (2006)

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

Notes. There are 206 healthy and 33 weak banks. Non-performing Loan Ratio as a ratio of the value of loans. Securitized Loans/Total Assets for banks that securitize. NFF denotes non-financial firms. Except for the ln(Total Assets), variables are ratios in percentages. The last column shows the *t* ratio on the test for the difference of the means. See definitions in Appendix 2. Source: Own computations on bank balance sheet data from the Bank of Spain.

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

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

Notes. Observations: 103,441 control firms and 65,854 treated firms. WB denotes weak banks. Variables are ratios in percentages unless otherwise indicated. The share of loans with weak banks is in bank credit. The last column shows the *t* ratio on the test for the difference of the means. See definitions in Appendix 2.

Table 3. The employment effect of weak-bank attachment. Difference in Differences
 Dependent variable: $\log(1+\text{Employment}_{it})$

	(1)	(2)	(3)	(4)
	Baseline	Placebo	Survivors	Municipality × Industry
$Post \times WB_i$	-0.068*** (0.007)	-0.002 (0.004)	-0.027*** (0.004)	-0.070*** (0.006)
<i>Post</i> interacted with:				
· Municipality and Industry	yes	yes	yes	–
· Firm fixed effects	no	no	no	no
· Municipality×3-digit Industry	no	no	no	yes
R^2	0.893	0.952	0.947	0.928
No. firms	169,295	93,911	152,284	133,409
No. observations	338,590	187,822	304,568	266,818
	(5)	(6)	(7)	(8)
	Exact Matching	Tradable Goods	Panel	Loans to REI
$Post \times WB_i$	-0.065*** (0.010)	-0.088*** (0.014)	-0.070*** (0.008)	-0.066*** (0.007)
<i>Post</i> interacted with:				
· Municipality and Industry	no	yes	–	yes
· Firm fixed effects	no	no	yes	no
· Municipality×3-digit Industry	no	no	no	no
R^2	0.898	0.902	–	0.893
No. firms	169,295	21,692	168,462	169,295
No. observations	338,590	43,484	640,906	338,590

Notes. OLS estimates for 2006 and 2010, except in col. (2), 2002 and 2006, and col. (7), 2007 to 2010. Time and Firm fixed effects are included in all specifications. In col. (5) estimates are for 8116 industry-municipality cells (see text). In col. (8) treated firms are those working with a bank whose exposure to the REI in 2006 is above the third quartile. “yes/no” indicate whether the corresponding set of variables is included, “–” indicates that the corresponding set of variables is comprised in a wider set of fixed effects. Robust standard errors corrected for clustering at the firm and main bank level appear between parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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

Instrumented variable	(1)	(2)	(3)	(4)
	$\Delta\log$ (1+Credit _{it})	$\Delta\log$ (1+Credit _{it})	$\Delta\log$ (1+T. Credit _{it})	$I(\text{Rejection}_{it})$
	0.580*** (0.087)	0.304*** (0.095)	0.285*** (0.072)	-1.776*** (0.311)
First stage				
$d_{2008} \times WB_i$	-0.003 (0.006)	0.005 (0.017)	-0.086*** (0.018)	-0.017*** (0.003)
$d_{2009} \times WB_i$	-0.058*** (0.013)	-0.108*** (0.022)	-0.125*** (0.017)	0.031*** (0.005)
$d_{2010} \times WB_i$	-0.105*** (0.015)	-0.130*** (0.025)	-0.149*** (0.019)	0.038*** (0.006)
Overall effect ($\gamma'\mu_{2010}$)	-0.061	-0.039	-0.042	-0.068
F test / p value	28.0/0.00	13.3/0.00	22.2/0.00	15.2/0.00
No. firms	168,462	13,477	13,477	168,462
No. obs.	640,906	52,078	52,078	640,906

Notes. IV estimates for 2007 to 2010. In col. (3) “T. Credit” denotes Total Credit, including bank and trade credit. Time and Firm fixed effects are included in all specifications. F test and p values for the inclusion of the IV in the first stage are reported. Robust standard errors corrected for clustering at the firm and main bank level appear between parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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

	(1)	(2)
WB_i	-0.074 ^{***} (0.025)	-0.108 ^{***} (0.019)
First stage		
Local weak-bank density _{<i>i</i>} (1988)	0.535 ^{***} (0.077)	
Exposure to REI _{<i>i</i>} (2000)		0.356 ^{***} (0.119)
<i>F</i> test / p value	48.8/0,00	8.9/0.00
No. firms	169,295	169,295
No. observations	338,590	338,590

Notes. IV estimates for 2006 and 2010. Time fixed effects, Firm fixed effects, Time \times Coast fixed effects, and Time \times Industry fixed effects are included in all specifications. *F* test and *p* values for the inclusion of the IV in the first stage are reported. Robust standard errors corrected for clustering at the firm and main bank level appear between parentheses.
 * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6. The employment effect of weak-bank attachment. Triple Differences
 Dependent variable: $\log(1+\text{Employment}_{it})$

$Post \times WB_i$	-0.043 ^{***} (0.005)
$Post \times \text{Rejected application}_i$	-0.065 ^{***} (0.004)
$Post \times WB_i \times \text{Rejected application}_i$	-0.018 ^{**} (0.008)
$Post \times \text{Defaults}_i$	-0.233 ^{***} (0.026)
$Post \times WB_i \times \text{Defaults}_i$	-0.059 ^{**} (0.029)
$Post \times \text{Short-term debt}_i$	-0.082 ^{***} (0.006)
$Post \times WB_i \times \text{Short-term debt}_i$	-0.030 ^{***} (0.008)
$Post \times \text{Small firm}_i$	-0.011 (0.011)
$Post \times WB_i \times \text{Small firm}_i$	-0.050 ^{**} (0.022)
$Post \times \text{Single bank}_i$	0.028 ^{***} (0.004)
$Post \times WB_i \times \text{Single bank}_i$	0.027 ^{***} (0.009)
R^2	0.894
No. firms	169,295
No. observations	338,590

Notes. OLS estimates for 2006 and 2010. All variables are in deviations from their means. Time dummies, Firm fixed effects and $Post \times \text{Municipality}$ and $Industry$ dummies are included in all specifications. Robust standard errors corrected for clustering at the firm and main bank level appear between parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7. The employment effect of weak-bank attachment. Exposure Intensity
 Dependent variable: $\log(1+\text{Employment}_{it})$

	(1)	(2)	(3)
	All	Single bank	Multiple banks
$Post \times WB Intensity_{it}$	-1.341*** (0.204)	0.338 (0.279)	-1.338*** (0.171)
$Post \times WB Intensity_{it}^2$	5.742*** (1,374)	0.833 (2.000)	5.543*** (1.147)
$Post \times WB Intensity_{it}^3$	-7.005*** (2.377)	0.063 (3.530)	-7.158*** (2.043)
R^2	0.893	0.878	0.894
No. firms	169,295	80,414	88,881
No. observations	338,590	162,060	177,762

Notes. OLS estimates for 2006 and 2010. All variables are in deviations from their means. Time dummies, Firm fixed effects and $Post \times Municipality$ and Industry dummies are included in all specifications. Robust standard errors corrected for clustering at the firm and main bank level appear between parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8. Effect of weak-bank attachment on the probability of exit
 Dependent variable: Probability of exit from 2006 to 2010_{*i*}

	(1)	(2)	(3)	(4)	(5)
	Cross Section	Panel	Cross Section	Panel	Cross Section
WB_i	0.006 ^{***} (0.002)	0.013 ^{***} (0.003)			
$WB Intensity_i$			0.063 ^{***} (0.015)	0.091 ^{***} (0.012)	0.063 ^{***} (0.015)
$WB Intensity_i$ × Single bank _{<i>i</i>}					-0.052 ^{***} (0.015)
Municipality and Industry	yes	–	yes	–	yes
Firm controls	yes	–	yes	–	yes
Firm fixed effects	no	yes	no	yes	no
R^2	0.077	0.581	0.078	0.691	0.078
No. firms	169,295	168,462	169,295	168,462	169,295
No. observations	169,295	640,906	169,295	640,906	169,295

Notes. OLS estimates for 2006 and 2010, cols. (1), (3), and (5), and 2007 to 2010, cols. (2) and (4). Firm controls (see Appendix 2 for definitions): 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. “yes/no” indicate whether the corresponding set of variables is included, “–” indicates that the corresponding set of variables is comprised in a wider set of fixed effects. Robust standard errors corrected for clustering at the firm and main bank level between parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

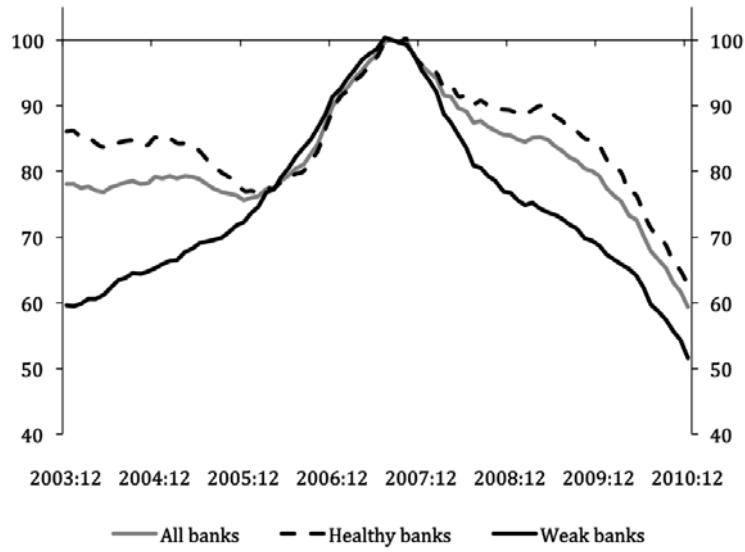


Figure 1: New credit to non-financial firms by bank type (12-month backward moving average, 2007:10=100)

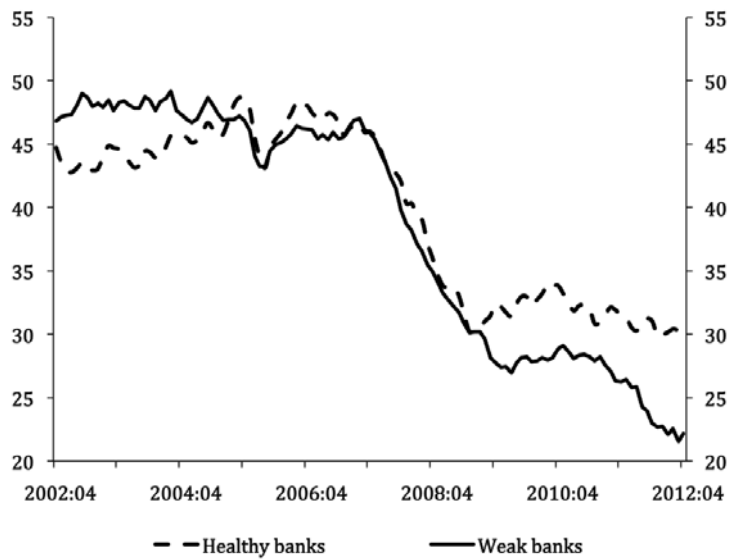


Figure 2: Acceptance rates of loan applications by non-current clients, by bank type (%)

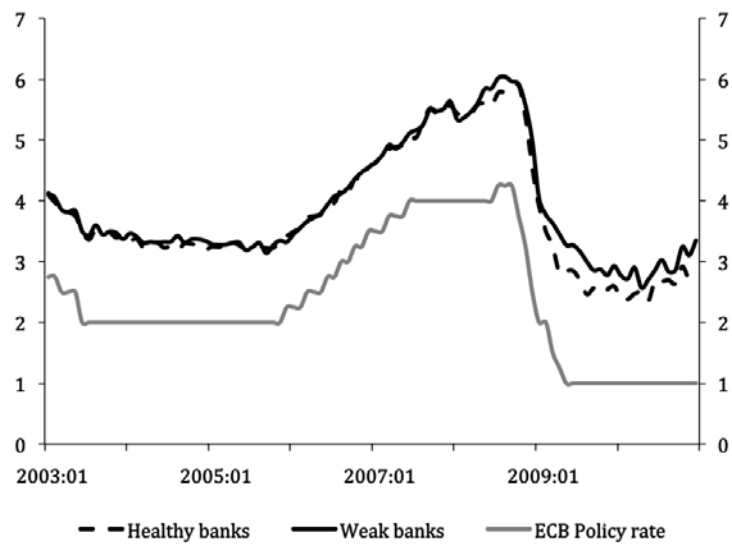


Figure 3: Average annual interest rate for new loans to non-financial firms by bank type and the policy rate (%)