Credit Shocks, Employment Protection, and Growth: Firm-level Evidence from Spain*

Luc Laeven[a,b,], Peter McAdam[a] and Alexander Popov[a]

[a] European Central Bank, [b] CEPR

Abstract

We exploit a provision in Spanish labor laws whereby employment protection is more stringent for firms with 50+ employees. Firm-level evidence suggests that during the credit crunch of 2008-09, healthy firms with less than 50 employees borrowing from troubled banks grew faster in sectors where production factors were sufficiently substitutable. This effect is made possible by firms' substituting labor for capital when the rental cost of capital increases. Our analysis sheds new light on the importance of labor regulation and the technological substitutability of the factors of production in enabling firms to adjust to financial shocks.

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Keywords: credit crunch; employment protection; capital-labor substitution; firm growth.

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Corresponding author. Directorate Research, European Central Bank, Sonnemannstrasse 20, D-60314 Frankfurt, email: Luc.Laeven@ecb.europa.eu

1 Introduction

Flexible labor markets are a standard fixture in neoclassical economic theory. They are often credited with delivering large economic benefits to liberalizing countries because they support labor reallocation and facilitate the process of "creative destruction" (Davis and Haltiwanger, 1992; Caballero and Hammour, 1994; Nickell, 1997; Boeri, 2011). Conversely, high employment protection is often seen as one reason why economies lose their ability to create jobs and adapt to negative shocks (Blanchard and Portugal, 2001; Bertrand and Kramarz, 2002). Labor market flexibility has also been shown play a role in firm growth by shaping the interplay between labor-augmenting technologies and capital-labor substitution (Caballero and Hammour, 1998; Acemoglu, 2003).

At the same time, there is little evidence on the *interaction* between labor market frictions, such as employment protection regulation, and financial frictions, such as credit shocks. This is all the more surprising given that a large literature has shown that financial frictions matter for firm investment and growth (Fazzari et al., 1988; King and Levine, 1993a,b; Campello et al., 2010). Furthermore, a growing literature has studied how financial frictions affect firm employment directly (Chodorow-Reich, 2014; Bentolila et al., 2018; Popov and Rocholl, 2018). Ignoring how firms adjust their capital-labor mix in response to credit shocks, and how this response depends on various properties of the labor market, may limit our understanding of how such shocks work.

We go to the heart of this question by studying the joint impact of financial and labor market frictions on firm growth. Specifically, we jointly consider the role of credit shocks and of differences in employment protection, and show that they have a joint effect on firm growth. Furthermore, we argue that this effect crucially depends on the degree of substitutability between capital and labor. We focus on Spain which offers an ideal setting for identification purposes: Spanish employment protection legislation varies systematically by firm size, and Spanish firms were exposed to large negative credit shocks during the credit crisis of 2008–09. We construct a new dataset that merges two sources, the firm-level Amadeus database and the Kompass database of bank-firm relationships. The final dataset contains around 110,000 Spanish firms with full balance sheet information, observed both before and after the crisis, and which covers the full size distribution of firms, from 1 employee to over 1,000 employees.

We exploit variation along three dimensions to study the interaction between financial frictions, employment protection legislation, and firms' technological characteristics. First, labor markets in Spain are characterized by a well-defined regulatory distortion whereby dismissal rules vary by firm size. Specifically, employment protection is more stringent for firms with

more than 50 employees, greatly increasing firing costs.[1] In turn, firing costs can be seen as a fixed per-employee cost which reduces firms' incentives to hire new workers and impedes firm growth (Blanchard et al., 1985; Bentolila and Bertola, 1990).

Second, the credit crunch of 2008–09 constitutes a well-defined financial shock to firms. Spanish banks were unequally affected by the crisis, and some required large recapitalizations, to the tune of 1.1% of GDP. While the supply of credit declined across the board, new credit issued by affected banks declined significantly more than new credit issued by non-affected banks (Bentolila et al., 2018). Firms borrowing from affected banks thus experienced a larger tightening in credit constraints, giving them an incentive to substitute labor for capital.

The combination of these two factors gives rise to a well-defined empirical mechanism: all else equal, firms with fewer than 50 workers should have grown faster (or declined more slowly) than those with more than 50 workers in the wake of the financial crisis in Spain. This is because lower firing costs allowed them to hire more workers (or at least mitigated employment losses), thus making up for the decline in capital use.

At the same time, there is plenty of evidence that small firms face stiffer credit constraints during a credit crunch, because of higher credit risk and information opacity (e.g., Sharpe, 1994; Beck et al., 2008). This could also be true for firms over an arbitrarily narrow window around the 50-employee cut-off, as long as risk and opacity increase monotonically with firm size. This "credit-constraint" effect could then eclipse the "employment-flexibility" effect for small firms, leading the econometrician to erroneously conclude that employment protection does not matter.

Therefore, we add a third ingredient to our identification strategy whereby we distinguish across firms depending on their technological ability to substitute labor for capital. For firms with low elasticity of substitution between factors, a credit tightening will lead to a decline in both capital and employment. Thus, for firms with fewer than 50 employees and with low elasticity of factor substitution, the "credit-constraints" effect will dominate the "employment-flexibility" effect. However, for firms with high elasticity of factor substitution, a credit tightening will result in a lower decline in employment as firms substitute labor for capital, allowing us to identify the "employment-flexibility" effect. Our empirical hypothesis then is that credit constrained firms that benefit from firm-size-specific employment flexibility will grow faster than credit constrained firms subject to stricter employment protection, as long as their elasticity of factor substitution is large enough. Such firms will experience simultaneously higher sales growth and higher employment growth.

Our methodological approach rests on a number of conditions. First, bank-borrower rela-

[1] Spain has for long been an outlier in labor market protection, especially firing costs. This is often thought to have contributed to its structurally high unemployment (e.g., Bover et al., 2000).

tionships need to be sticky over the cycle, a fact already established in the literature in various contexts, such as the US (e.g., Chodorow-Reich, 2014), and—importantly for our paper—Spain (Bentolila et al., 2018). Second, the cross-sectional variation in banks' willingness to lend during the crisis needs to be orthogonal to firm characteristics and to pre-crisis labor market trends. The advantage of focusing on bank-specific credit supply shocks is that they are unlikely to be related to both individual borrowers' conditions and to pre-crisis labor market conditions. Third, the measure of capital-labor substitution should not be picking up variation in the demand for finance. To make sure that we have abstracted from this channel, we calculate industry-specific elasticities of substitution, and we also control for the external financial dependence of the firm.

Our main finding is that, in response to a negative credit shock, firms with fewer than 50 employees—who are subject to less stringent employment protection—grow faster (decline more slowly) relative to firms with more than 50 employees. Crucially, this effect is driven by firms whose production technology is characterized by a high substitutability between production factors. These same firms experience higher rates of employment growth, but not of investment growth. Finally, this effect is much more pronounced for firms with higher sales growth and/or with higher productivity growth before the crisis started. Our results lead us to conclude that by enabling firms to substitute labor for capital, more flexible employment protection benefits firms during a credit crunch, especially if they face healthy growth prospects.

This finding is important for two reasons. First, it demonstrates the importance of flexible labor markets for firm growth during a credit crunch. Second, it suggests that failure in the literature to integrate the margin of factor substitutability in the analysis overstates the real effect of the financial shock for firms that benefit from flexible employment protection.

The main result in our paper is robust to controlling for time-varying firm-specific factors that can affect firm growth in the absence of credit shocks or firm-size-specific labor regulation, such as size, debt, cash flow, net worth, or workers' average skill level. It is also robust to controlling throughout for unobservable firm heterogeneity with firm fixed effects, and for unobservable sector-specific trends, region-specific trends, and even sector-region-specific trends. The main effect still obtains when we compare smaller and larger firms that are closer to the 50-employee threshold; when we control for other underlying industry characteristics, such as dependence on external finance; when we look at firms with a credit relationship with only one bank; when we drop all firms in the construction sector where some of the problems of Spanish banks originated; when we account for strategic selection below the 50-employee threshold; and when we control for the potentially confounding effects of firm size and of firm quality.

We perform three separate falsification tests. First, we repeat the main test in the absence of a credit shock (i.e., before the financial crisis), assuming that the same firms were affected when

they were not. Second, we use another arbitrary firm-size threshold that does not capture a firm-size-specific employment protection rule. Third, we perform the same exercise on German data. In Germany employment protection is not different for firms with fewer and with more than 50 workers. In all three cases, the main effect goes away, suggesting that we are indeed capturing a genuine interaction of credit shocks and labor regulation in determining firm growth.

We also show that our results are not limited to one country or one episode. We show that Spanish firms with fewer than 50 employees, borrowing from banks hit by negative capital shocks, grew faster already before the crisis. This suggests that labor market flexibility allows firms to deal with credit frictions even outside of a credit crunch. We also show that German firms with fewer than 10 employees (the analogue in German labor regulation to the 50-employee threshold in Spain) grew faster during the German credit crunch of 2007–08. This finding supports the idea that the ability to grow during a financial crisis by substituting labor for capital is not limited to the case of Spain during the Global Financial Crisis.

We are the first to consider the interaction between financial frictions, employment protection, and firms' technological characteristics. There is already a large literature on the impact of labor market reforms on firms' demand for labor. Various contributions in this line of research have looked at the employment effects of changes to employment benefits (e.g., Atkinson and Micklewright, 1991; Blanchard and Wolfers, 2000; Krueger and Meyer, 2002; Card et al., 2007) and of the introduction of activation programs (Heckman et al., 1999; Dolton and O'Neill, 2002; Black et al., 2003). Others have studied the impact of employment protection laws on firm demand for labor (e.g., Boeri and Jimeno, 2005; Garibaldi and Violante, 2005; Cingano et al., 2016). In their seminal paper, Bentolila and Bertola (1990) argue that high firing costs can help explain the dynamic behavior of European employment, including the persistence of unemployment, in the 1970s and 1980s. Berton et al. (2018) present interesting evidence of the heterogeneous effects on regional Italian employment from financial shocks using a rich data set of bank-firm-job contracts linkages. We contribute to this literature by studying the interaction between employment protection and credit constraints on firm growth, by accounting for the interaction of these factors with the firm's technological elasticity of factor substitution, by performing a careful estimation of substitution elasticities, and by studying the benefits of employment protection during a financial crisis when some firms with healthy growth prospects are held back by worsening credit market frictions.

Our paper also contributes to the literature on the real effects of financial frictions, and specifically their effects on firm employment. Much of this literature has focused on the impact of negative shocks to the firm's borrowing capacity on its demand for labor. Some studies have relied on indirect measures of credit constraints such as firm size or debt to identify the

effect of monetary policy and the business cycle on employment (e.g., Sharpe, 1994; Nickell and Nicolitsas, 1999). Most recent studies have attempted to gauge the effect of shocks to external finance on employment using more direct measures. For example, Benmelech et al. (2011) find that following the large decline in real estate values in Japan, unemployment increased by about 1% in U.S. metropolitan state areas dominated by Japanese-affiliates banks. Greenstone et al. (2020) show that the predicted decline in small business lending at the regional US level maps into lower rates of new business formation and higher unemployment. Falato and Liang (2016) show that loan covenant violations are followed by simultaneous cuts in employment and wages. Giroud and Mueller (2017) show that more leveraged firms decrease employment more when they are located in an area that experiences larger declines in house prices.

In addition, a number of recent studies have used micro data to estimate the response of employment to credit constraints. For example, Bertrand et al. (2007) find that jobs were reallocated to more productive firms following the deregulation of the French banking industry. Campello et al. (2010) show that firms with credit constraints plan to cut employment more than unconstrained firms. Chodorow-Reich (2014) shows that small firms that had been borrowing from banks that became impaired during the crisis, reduced employment more than small firms associated with healthier banks. Acharya et al. (2018) find that large firms with higher exposure to syndicated lending by European periphery banks experienced lower growth of employment. Duygan-Bump et al. (2015) find that during recessions, workers in small firms are more likely to become unemployed in industries with high external financial needs. Bentolila et al. (2018) show that Spanish firms with credit relationships with weak banks reduced employment substantially more than firms borrowing from non-affected banks. Popov and Rocholl (2018) show that German firms borrowing from savings banks that had to provide funds for the recapitalization of their parent companies reduced both employment and average wages. Our contribution is to show how credit constraints interact with employment protection to determine the firm's choice of inputs of production, and ultimately, its growth. By considering the interaction between financial frictions and labor market frictions, we are able to offer a new insight: the extent to which financial shocks are amplified or dampened depends on firms' technological and regulatory environments.

The paper is organized as follows. Section 2 discusses the institutional details with respect to labor regulation and the impact of the financial crisis in Spain. Section 3 describes the data used in our empirical analysis. Section 4 presents the empirical methodology and identification strategy. Section 5 reports the main results, alongside a battery of falsification and robustness tests, and investigates the underlying mechanisms. Section 6 discusses the aggregate implications of our findings. Section 7 tests the external validity of our results. Section 8 concludes. Additional

material is included in the appendix, including a stylized theoretical model formalizing the channels of interest (Appendix A).

2 Institutional details

2.1 Employment protection in Spain

Spain is a country with structurally high unemployment rates, higher than most other OECD countries. The Spanish labor legislation at the time of the financial crisis included two dismissal rules which by default affected firms differently depending on their size.[2]

The first dismissal rule applies to negotiations between the employer and workers' representatives. According to this regulation, if a collective dismissal is going to be carried out, workers' representatives or ad hoc designated workers' representatives are entitled to negotiate the collective redundancy process. Therefore, the employer needs to first apply for authorization and open a period of consultation with the representatives of the workers. The period of required consultation is 15 days in enterprises of less than 50 workers, but 30 days in enterprises with 50+ workers. During negotiations with the workers' representatives, the employer must consider alternative measures to reduce the number of terminations, and agree on the selection criteria. As in the case of individual redundancy, the severance pay is set at 20 days of salary per year of service (capped at 12 months' pay). However, during the negotiations with the workers' representatives, severance per employee is often increased. Longer negotiations thus typically result in larger severance per employee, increasing dismissal costs for firms with 50+ employees.

Second, in enterprises with more than 50 employees, a collective dismissal has to be accompanied by a social plan aiming to mitigate the consequences for the affected workers. Firms with 50+ employees carrying out a collective dismissal should therefore offer the affected employees an external replacement plan through the authorized employment agencies. This plan, designed for a minimum period of 6 months, should include the following components: (1) measures intended to avoid or reduce the effects of restructuring, for instance, internal redeployment, functional or geographical mobility, or a substantial modifications of contractual conditions; (2) measures aimed at reducing the effects of restructuring on employees; (3) external relocation; (4) promotion of self-employment; (5) financial compensations for geographical mobility; and (6) economic, technical, organizational and other types of measures intended to make the continuation of the undertaking and its activity possible.

As part of the second dismissal rule, companies also have to carry out a special training and redeployment plan of at least 6 months, implemented by means of an authorized outplacement

[2]See the ILO Employment Protection Legislation Database for more detail.

company, if the collective dismissal affects firms with 50+ employees. The cost of carrying out this plan is borne by the firm, and not by the workers. Non-compliance with this obligation could result in a legal claim for its compliance by the workers. Companies with fewer than 50 workers do not have to implement a social plan aiming to support dismissed workers.

Why are these firing rules relevant to firms' hiring decision? The longer negotiating period and the need to provide workers with a social plan makes it considerably more costly for firms with 50+ employees to dismiss workers. As a consequence, the cost of hiring the marginal worker is higher for such firms, because they will have to pay higher dismissal costs on a larger workforce. It is therefore plausible to hypothesize that firms with fewer than 50 employees will be more inclined to hire new workers if they need to expand. This logic goes back to the seminal work by Blanchard et al. (1985) and Bentolila and Bertola (1990) who argue that when making a hiring decision today, the firm equates the discounted expected marginal revenue product of labor that the newly hired worker will provide to the discounted wage costs, plus the hiring costs today and the firing costs tomorrow. These papers show that employment protection laws have a significant effect on firms' propensity to hire by increasing the cost of future firing. In the case of the employment protection rule we focus on, this mechanism is significantly more relevant to firms with 50+ employees.

Are these rules really binding? One immediate concern is that this type of regulation may be too weak to present a binding constraint on firms' expansion. **Figure 1** plots the number of firms by number of employees, for firms with between 40 and 60 employees. The Figure shows clear evidence of a discontinuity around 50, with firms clustering at 48 and 49 employees, and with the number of firms declining sharply (from 4,727 to 3,030) at 51 employees. The presence of a dip in the firm size distribution right above the 50-employee threshold suggests that firms are indeed reluctant to pass the threshold. It is consistent with models of labor demand predicting that size-contingent employment regulation hampers the expansion of firms and generates discontinuities in their size distribution (e.g., Garicano et al., 2015).[3]

2.2 The impact of the financial crisis on the Spanish banking sector

The Spanish banking sector was severely affected during the financial crisis of 2007–08. During the crisis, the Spanish government intervened in a number of banks which were then either nationalized and quickly resold, or a bank merger or an acquisition of an ailing bank by a healthy credit institution was arranged. Most of these operations entailed government support, to the overall amount of 11.6 billion euro, i.e. about 1.1% of Spanish GDP (see Banco de Espana,

[3]The 50-employee threshold is relevant to firms in other ways, too. For example, employment subsidies are provided, in the context of a new permanent contract, to companies with fewer than 50 employees. However, this subsidy only came in place following the 2012 Spanish labor market reform (See Gamberoni et al., 2016).

2014). While some of these actions took place at the onset of the crisis, further consolidation operations and the bulk of the nationalizations took place in 2011-2012. During this period, savings banks were forced to transform into commercial banks, and the European Financial Stability Facility provided financial assistance for the recapitalization of a number of banks. Overall, a total of 71 banks were subject to some kind of intervention (see also Appendix Table D.1). For the purpose of the analysis, we classify as "affected" all firms that in 2008 had a credit relationship with a bank that was subject to government intervention at any point during the financial crisis.

It is important to note that during the crisis, banks became troubled for reasons that are not related to their credit relationship with a particular firm or a segment of the Spanish corporate landscape. In many cases, inefficient supervision exacerbated problems that could have been dealt with earlier and more forcefully. Savings banks in particular were subject to the same regulation and supervision by the Bank of Spain as commercial banks, however, they had a very different ownership and governance structure. Because they were not listed on the stock market, savings banks were less exposed to market discipline than commercial banks, while at the same time their ability to raise capital in response to the crisis was more limited. Moreover, they were de facto controlled by regional governments, which introduced a number of political inefficiencies in their operation and led to delays in their restructuring (e.g., Cuñat and Garicano, 2010; Fernández-Villaverde et al., 2013; Santos, 2017).

As a result of balance sheet problems, new credit issued by affected savings banks declined significantly more during the crisis than new credit issued by non-affected banks (a decline of 46 percent relative to a decline of 35 percent between 2007 and 2010). Importantly, the consolidation operations and nationalizations during 2011-2012 did not restore the credit flow by weak banks. The difference in new credit flow between affected and non-affected banks continued to grow, and weak banks started to ration credit by charging substantially higher average interest rates than healthy banks.

3 Data

3.1 Firm-level data from ORBIS

Our firm-level data come from the ORBIS dataset provided by Bureau van Dijk (BvD). ORBIS contains financial and ownership data for more than 170 million firms from more than 100 countries world-wide. Financial data include balance sheet information and income statements, while ownership data contains information about the company's ultimate owner and shareholders. The database has been compiled since 2004 by BvD and is currently updated quarterly.

Every vintage contains a history of up to ten years of financial information for an individual firm. In addition to this product, BvD offers to link the latest vintage with historical vintages going back to 2004. The analysis in this paper is based on the vintage as of the second quarter of 2004 linked with all historical files available from BvD.

A common case in ORBIS is that financial information for a given firm and year is updated from one vintage to the next. When constructing the historical files, special care is taken to put the latest available information for any given year and company. The resulting dataset contains many more firm-year observations than are available in the latest vintage. This is because there are more years of data for many firms. In addition, there are about 30 percent more companies in the historical files compared to the latest vintage. The reason is that BvD deletes companies that do not report for a certain period from each vintage. Such companies are nevertheless included in the linked historical files thereby reducing the survival bias that is present in a single vintage. This is crucial because any empirical estimates would be biased if the least productive firms in a country during a particular year are ultimately removed from the data. At this stage the dataset contains about 100 million firm-year observations, but about a quarter of those relate to firms that have not provided financial information in any given year.

For our analysis, we take Spanish companies with financial data in the period 2004–2011, and we work with unconsolidated accounts. We first make sure that firms' balance sheet items pass a standard consistency test, after which inconsistent firm-year observations are dropped. Our consistency checks make sure that balance-sheet identities hold within a small margin and entries are meaningful from an accounting point of view. Following Kalemli-Özcan et al. (2015), we drop firm-year observations in which total assets, fixed assets, intangible assets, sales, long-term debt, loans, creditors, other current liabilities ,or total shareholder funds and liabilities have negative values.

Next, we drop firm-year observations for which some basic accounting identities are violated by more than 10%.[4] We also drop country-specific sectors, such as agriculture and mining; sectors with high government ownership, such as public administration; and heavily regulated sectors, such as finance. We also drop firm-year observations if there are less than 10 firms in each NACE Rev. 2 digit 4 sector. In addition, we remove firm-year observations that have loans or long-term debt exceeding total liabilities. Then we drop all firms for which we do not have at least 5 years of consecutive non-missing observations of sales. This leaves us with a total of 231,843 unique firms, for a total of 1,849,170 observations.

Finally, we focus on those firms that are observed at least once before and at least once after

[4]Specifically, we drop observations if the following ratios are larger than 10% in absolute value: (total assets – total shareholder funds and liabilities) / total assets; (total assets – fixed assets – current assets) / total assets; (current liabilities – loans – creditors – other current liabilities) / current liabilities.

the beginning of the financial crisis in 2007. This is done for the purpose of comparing firms that experienced a tightening of credit constraints after the crisis started to those that did not, and studying the extent to which such tightening translates into a decline in firm growth, based on the firm's size and sector of operation. Focusing on firms with full balance sheet information, this procedure reduces the sample to 107,740 unique firms.

In terms of firm-specific information that we use in the regressions, we make use of a wide range of variables which we summarize in **Table 1**. Our main dependent variable in the paper is 'Sales growth' which denotes the log difference in the firm's total sales between the current period and the previous one. On average, firms over the sample period posted a year-on-year decline in sales of about 2.6 percent, which is consistent with the overall performance of the Spanish economy which posted negative GDP growth for five years in a row between 2009 and 2013. The median firms experienced an even larger decline in sales (2.7 percent), suggesting a slightly negatively skewed distribution of sales growth. Looking at the growth of inputs in production, we also note that employment declined considerably less during the same period, on average by 0.7 percent year on year, with the median firm neither growing nor declining. At the same time, firm-level capital investment declined on average by 5.3 percent year-on-year. This is the first indication in the raw data that the financial crisis had a more significant impact on capital than on labor. All growth variables are winsorized at -100 percent and at 100 percent.

We then use the employment data to construct the main explanatory firm-level variable which is a dummy variable equal to 1 if the firm has fewer than 50 employees. Comparing firms below and above this threshold is a direct test of the hypothesis that firms can cushion the impact of a credit shock on production by substituting labor for capital, but only if labor rules do not penalize them for adjusting employment. In order to ensure that we are not simply picking a small firm vs. large firm effect that has to do with differences in technology or opportunities, and not with labor regulation, we create other cut-offs, for example, a dummy equal to one if the firm has fewer than 10 employees. As Table 1 demonstrates, the firms in our dataset are on average very small, with 89.4 percent having fewer than 50, and 43.6 percent fewer than 10, employees. The median firm is 15 years old. Furthermore, we also employ a set of standard controls for size and creditworthiness: the logarithm of total sales and the logarithm of total assets, as well as the ratio of cash flow to total assets, the ratio of net worth (defined as assets minus liabilities) to total assets, the ratio of total debt to total assets, and total employee compensation (including both direct and indirect contributions) divided by number of employees.

3.2 Firm-bank shock

One of the main building blocks of our identification strategy is comparing firms—across size bins and across sectors—in terms of whether they are credit constrained or not. The Spanish banking sector as a whole experienced a large negative shock during the crisis. But, there were large differences across banks in pre-crisis exposure and within-crisis performance. We exploit this margin by making use of a variable called 'BANKER', available from Orbis through Kompass, which displays the name of the bank(s) with which the firm has a credit relationship. Each firm reports up to 10 credit institutions with which it has a relationship. We then match these bank names with a publicly available list—provided by the Bank of Spain—of all banks which during the financial crisis were subject to government intervention in the form of a liquidity injection, recapitalization, or a take over.[5] The firms in the final dataset report a credit link to a total of 1,506 different credit institutions. Out of these, 71 were subject to a government intervention during the crisis, and hence are classified by us as affected. Consequently, we create a dummy variable 'Shock' which is equal to one after 2007 for all firms with a credit relationship with at least one affected bank.[6]

Splitting banks into affected and non-affected based on clear criteria allows us to analyze the overall impact of the initial balance sheet shock, including latent losses not officially recognized until much later. While the Spanish banking sector as a whole reduced lending during the crisis, to a large degree because of a drop in credit demand, an "affected" bank is one with a relatively stronger deterioration in its balance sheet and lending capacity. This shocks spills over into firm growth through the channel of reduced credit access.

Table 1 reports summary statistics of the bank associations of firms in our sample that report associations with at least one bank. On average, 26 percent of firms in the final dataset have a credit association with at least one affected bank. Of course, having a credit association with an affected bank is less of an issue for firms with multiple banking relationships.

Table 1 also reports summary statistics on bank capital. The average bank has a ratio of capital to total assets of 11.65, with a minimum of 8.2 and a maximum of 28.1

3.3 Elasticity of Factor Substitution

Following León-Ledesma et al. (2010), we estimate sectoral production characteristics using a 'normalized' system of non-linear equations containing a Constant Elasticity of Substitution (CES) production function and (capital and labor) factor demands with cross-equation parameter

[5] See Appendix Table D.1 for a list of all banks classified as "affected".

[6] There is a large literature that uses the same empirical strategy for constructing a firm-level credit shock (e.g., Jiménez et al., 2012; Popov and Udell, 2012; Kalemli-Özcan et al., 2015; Beck et al., 2018).

constraints, and accounting for cross-equation residual covariances; this has proven to be a very efficient and robust estimation method in this framework (for a more detailed discussion, see Appendix B).

Our main interest is uncovering sectoral elasticities of factor substitution, denoted $\sigma \geq 0$. If $\sigma = 0$ (the Leontief case) capital and labor are used in fixed proportions. Otherwise, depending on the size of σ, factors may be substituted for one another to a great or lesser extent in response to changes in relative factor prices (i.e., wages versus user cost) or other relevant economic changes. If $\sigma = 1$ (Cobb Douglas case, CD), changes in relative factor prices lead 1-to-1 in changes in relative factor volumes (K/L ratio), such as to keep factor income shares constant. If $\sigma \to \infty$ (linear production), factors are essentially indistinguishable; accordingly, a rise in the price of one input factor can be offset by increasing use of the relatively cheaper factor.

For estimation we use the KLEMs database which contains industry-level measures of output, inputs and productivity, Jorgenson et al. (2012). This provides carefully constructed data on the labor input, labor compensation and capital (incorporating quality adjusted and compositionally adjusted measures).[7] We focus on the market economy (at the 2-digit level) rather than total economy and so exclude public administration and other non-market activities (reflecting measurement problems in these sectors; see, e.g., Klump et al., 2007; O'Mahony and Timmer, 2009).

Moreover, we estimate the substitution elasticity by using data on the US. An argument going back to Rajan and Zingales' (1998) seminal work states that the production and factor choices of firms will be least distorted by credit constraints in an economy backed by highly developed and liquid financial markets.[8] The US also has one of the most flexible set of labor regulations in the OECD, suggesting that factor adjustment in response to changes in factor prices will not be distorted by labor market rigidities. Therefore, backing out sector-level elasticities of factor substitution for US sectors should produce a reasonable empirical proxy for sectors' "natural" elasticities of substitution. However, even putting aside this argument, estimating sectoral elasticity for Spain faces data limitations (although we do additionally show those for robustness).[9]

There are two vintages of the US KLEMs database, one from 1947–2010, the other from 1970–

[7] The capital series is calculated using the perpetual inventory method for a given investment series.

[8] Gopinath et al. (2017) similarly exploit US technical parameters on Spanish data in their study of productivity and mis-allocation. Likewise Hsieh and Klenow (2009) impose US share elasticities on manufacturing plant data for China and India.

[9] For example in the case of KLEMs' sectoral data for Spain, their "Capital services, volume indices" and "Labor services, volume indices" only start in 1995. The sectoral Value Added series starts in 1970 (although there are back-casted (from 1995) estimates based on the 2012 EU KLEMs release (ISIC Rev.4/ESA 95)). Several industries (at least 5) cannot be estimated due to data availability issues. Notwithstanding, Appendix E.1 estimates for the available elasticities of factor substitution for Spanish industries and – where comparability exists – benchmarks on our US results. The correlation between the series is above 0.7 and rank correlation tests decisively reject the null of distributional independence.

2007. We rely on the more popular longer dataset. However, for robustness we also consider the shorter variant which ends just prior to the financial crisis. Although they follow the same classification, the two databases represent different vintages of the underlying BEA data and some slightly different statistical methods. Generally, though, we find that both datasets give qualitatively similar values for the sectoral elasticity values.

Table 2 shows these values across the selected industries. They range from *high* values ($\sigma > 1$: e.g., Food, beverages, and tobacco; Construction), to *medium* ($\sigma \in [0.5, 1)$: e.g, Textiles, textile, leather, and footwear; Wood and products of wood and cork) to *low* ($\sigma < 0.5$: e.g., Basic metals and fabricated metal products), with a (unweighted) across-industry median (mean) of 0.75 (0.85). In all but four cases, the elasticity is below unity. Moreover, although there is an elasticity value around unity in four other cases (e.g., $\widehat{\sigma} = 0.9$ in Textiles, textile, leather, and footwear), in no case can a unitary elasticity not be rejected.[10] Thus, our sample is characterized by a mixture of industries which are (relative to CD) constrained in their ability to substitute factors in response to economic changes, and those which are less so. As regards benchmarking, probably the closest study to ours (in terms of sectoral composition, and methodology) is Young (2013).[11] In that exercise, four more sectors (Construction; Food, beverages, and tobacco; Electricity, gas, and water supply; and Pulp, paper, printing, and publishing) are also shown to have values at or above unity; the other industries are, again similar to our results, clustered around a medium range.[12]

Elasticity values (i.e., whether high or low) reflect the interplay of several influences (see Knoblach and Stöckl, 2020). The first treats the elasticity as a purely technological parameter (most germane to a single-firm/one-sector context). However, in a multi-sectoral framework, there are also non-technological determinants (e.g., consumption preferences): the pass through of changing factor prices into final prices feeds into demand and consumer allocations across different goods, generating subsequent feedback effects into demand, supply, and industry composition, which in turn affects factor-substitution outcomes (see Hicks, 1932). Next, there are aspects of structural change (i.e., the degree of mechanization, development and industri-

[10] See the formal tests in Appendix Table Table C.1.

[11] See also Knoblach and Stöckl (2020). They show the distribution of 852 estimates of σ for the U.S. gathered from 49 studies published between 1961–2017. Estimates fell in a $0 - 2$ range for both the aggregate and Manufacturing (although for the latter there is markedly more skew towards higher σ values).

[12] Comparisons with the (limited) empirical literature that does exist on sectoral elasticities though is far from straightforward. First, many studies concentrate on particular sectors (e.g., Manufacturing, as in Berndt, 1973) or very different aggregations (e.g., Herrendorf et al., 2015) look only at the aggregates of Agriculture, Manufacturing, and Services) or use different or non-standard sectoral definitions. Second, papers tend to use different, non-nested methodologies (time series, panel, linear, non-linear, system, single equation, frequency-domain, GMM, IV), and different specifications (e.g., including additional factors of production such as land, energy etc; raw rather than quality-adjusted factors; different treatments of technical change etc). Finally, many papers are technical contributions: examining the properties of different estimation techniques without any particular recommendation or narrative. Our results notably are the first to apply the León-Ledesma et al. (2010) method, whose Monte Carlo properties have shown to good properties compared to a number of other well-known methods.

alization) that influence factor substitutability. Finally, institutional determinants matter (e.g., employment laws, ownership, production structure, product range, credit constraints).

Any particular industry can embody a value combining aspects of one or all of them. For example, industries where utilization is an important margin of adjustment (such as in Construction and some Manufacturing[13]) and where labor hoarding motives may be weak (reflecting flexible employment contracts, piece work, strongly pro-cyclical demand) can embody relatively high values of substitution. Such industries often have, over time, reduced their employment share (precisely as facilitated by a high elasticity). Moreover, Miyagiwa and Papageorgiou (2007), Zeira (1998), and Saam (2008) also demonstrate a positive relation, respectively, between capital intensity; the degree of mechanization; the degree of openness, and the subsequent value of the substitution elasticity. These elements would shed light on why the elasticity might be high in some industries. Other industries—such as Coke, refined petroleum, and nuclear fuel—may have been less subject to demand shifts, and, notwithstanding their high capital intensity, may be characterized by relatively more stable employment shares given technical complementarities, automation outcomes, and scaled proportionality between machines and manpower.[14]

4 Empirical methodology and identification

We exploit the unique setting of the Spanish labor market in the presence of firm-size specific dismissal rules combined with firm-specific credit shocks to test three separate empirical predictions. The first hypothesis is that firm growth will be negatively affected if it experiences a negative credit shock that raises the cost of renting capital. Such a shock can materialize when, for example, the firm's creditor is experiencing balance sheet problems and needs to cut lending. The second hypothesis is that a firm's decline in growth following a credit shock will be less pronounced if its production function exhibits a high elasticity of substitution between capital and labor. In this case, a firm can substitute relatively cheaper labor for capital, maintaining similar levels of output. This would not be possible with a Leontief production function where firms need to employ capital and labor in fixed, predefined proportions. The third prediction is that this mechanism is more likely to be activated if the cost of hiring is low. This will be the case when, for example, labor regulation does not impose strong restrictions on firing, making it easier for firms to expand their employment base if they need to.

[13] Fernald and Wang (2016) have ascribed the weakening procyclicality of US TFP and labor productivity to the decline of Manufacturing and Manufacturing employment.

[14] Appendix Table D3 reports summary statistics across various classes of firms. Panel A compares observable characteristics for firms with crefit relationships with affected versus non-affected banks. This Panel suggests that some of these are statistically different across the two distributions. Panel B demonstrates that firms in high-sigma industries are broadly similar to firms in low-sigma industries. Panel C compares affected and non-affected banks in terms of capital ratios, and demonstrates that those declined significantly more for affected banks between the pre-crisis and the post-crisis period. Finally, Panel D demonstrates that large and small firms differ in a number of dimensions.

Our estimand of interest is the average treatment effect of a credit shock on firms' growth, as well as on their employment and investment decisions. We exploit both the discontinuity in employment protection at the 50-employee threshold and the credit shock in 2008, to build a regression-discontinuity design combined with a difference-in-differences strategy to estimate the causal effect of the credit shock in the presence of firm-specific employment protection on various firm-level outcomes. The assumption required to interpret the effect of firm-specific employment protection as causal is that any other variable that affects firm growth is either continuous at the threshold or its discontinuity is constant over time. In this case, the average trend of sales among firms marginally above the 50-employee threshold represents a good counterfactual for the trend of those just below the threshold.

We model the sales growth of firm f using the following regression:[15]

$$\begin{aligned} \Delta Sales_{fsrt} = \quad & \beta_1 Shock_{fsrt} \times < 50\ employees_{fsr} \times \sigma_s + \\ & \beta_2 Shock_{fsrt} \times < 50\ employees_{fsr} + \\ & \beta_3 Shock_{fsrt} \times \sigma_s + \\ & \beta_4 Shock_{fsrt} + \beta_5 X_{fsrt} + \mu_f + \theta_{st} + \varphi_{rt} + \epsilon_{fsrt} \end{aligned} \tag{1}$$

The main dependent variable, $\Delta Sales_{fsrt}$ denotes the annual change in the total sales of firm f in sector s in region r during period t. We calculate average values over two periods: $t = 1$ (pre-2008) and $t = 2$ (2008 and on). By collapsing the data into two periods, we make sure that the standard errors are robust to concerns about auto-correlation (Bertrand et al., 2004).[16] We calculate the underlying growth variable as a log difference, but our results are robust to constructing the variable as a percentage change instead.

We now turn to the main explanatory variables. $Shock_{fsrt}$ is a dummy variable equal to one in $t = 2$ if firm f has a credit relationship with a bank that was affected during the financial crisis and later required government assistance. The variable is equal to 0 for such firms in $t = 1$, as well as for both periods for firms borrowing from non-affected banks. While the majority of firms in the dataset are single-bank firms, more than a third report a credit relationship with more than one bank. In the latter case, $Shock_{fsrt}$ is equal to 1 after 2008 as long as the firm has a credit relationship with at least one affected bank.

The variable $< 50\ employees_{fsr}$ is a dummy equal to one if the firm had fewer than 50

[15] With the advance of micro-level datasets, triple interactions (as here) have become increasingly common in economics and finance literatures – see for instance Antràs and Chor (2013), Beck et al. (2005), Iyer et al. (2014), Jiménez et al. (2012), and Naidu and Yuchtman (2013).

[16] Laeven et al. (2018) perform the same test using uncollapsed data.

employees in $t = 1$. This definition is based on the regulatory cut-off discussed in Section 2, whereby firms with fewer than 50 employees face substantially lower firing restrictions. In falsification tests, we move the firm-size cut-off around to test for whether we are not capturing a spurious difference between small and large firms rather than a true discontinuity effect.

Next, $\widehat{\sigma}_s$ is the estimated industry-specific elasticity of substitution between capital and labor whose construction we discussed earlier. Its inclusion in the model is crucial because it allows us to identify industries where firms can plausibly adjust the production process across inputs, in response to changes in relative prices, and sectors where firms are technologically unable to do so, therefore they keep employing production inputs in fixed proportions before and after input price shocks. This technological benchmark is different from the ratio of capital to labor, which at each point in time is an equilibrium outcome of shocks to input prices. $\widehat{\sigma}_s$ thus allows us to identify firms' growth response to changes in the cost of capital for one segment of firms (with higher capital-labor elasticity) compared to another segment of firms (with lower capital-labor elasticity).

To summarize, our expected coefficients signs in Equation (1) are $\beta_2 < 0$ and $\beta_4 < 0$, while $\beta_1 > 0$ and $\beta_3 > 0$. In addition to these three variables and the interactions thereof, we include a set of controls to make sure that we are isolating an effect that is driven by the interplay of a credit shock, firm-size-specific labor regulation, and the firm's technology. These are included in the matrix X_{fsrt}. For a start, X_{fsrt} contains a polynomial of third degree in firm employment. Given this, all effects can be read as holding labor constant. X_{fsrt} also includes the firm's age, the logarithm of the firm's sales, the ratio of the firm's cash flow to assets, the ratio of the firm's net worth to assets, the ratio of the firm's total debt to assets, and the firm's average employee compensation. These variables capture the firm-specific impact on growth of size, cash flow from operations, agency problems, and the average skill level of the firm's workforce. By including them in the regression, we control for the possibility that firms just above and just below the 50-employee threshold may differ across other dimensions that can have an impact on firm growth. All variables are 1-period lagged. Similar to $\Delta Sales_{fsrt}$, the values for all variables are aggregated over two periods, $t = 1$ and $t = 2$. Finally, X_{fsrt} contains an interaction of the polynomial of third degree in firm employment with $Shock_{fsrt}$ and σ_s. In this way, we aim to fully control for size, thus isolating local variation around the firm-size cut-off.

Second, we include a vector of firm fixed effects μ_f. This allows us to net out the independent effect of firm-specific characteristics potentially unobservable to the econometrician, such as the propensity to take risk or managerial quality, that might be fixed over the short-to-medium term and that might explain a large share of the cross-sectional variation in firm growth.[17]

[17] Note that we cannot estimate the direct effect of the interaction variable $50\ employees_{fsr} \times \sigma_s$ because it is subsumed in the firm fixed effects.

We also include a matrix of sector-period fixed effects θ_{st} and a matrix of region-period fixed effects φ_{rt}. These are crucial as they wash out any variation in the firm growth that is common to all firms in the same sector at the same point in time (e.g., shock to the demand for residential property) and to all firms in the same region at the same point in time (e.g., shocks to consumers' purchasing power in Galicia).[18] In later robustness tests, we control even more tightly for shocks to local demand by including region $\times$ sector $\times$ period fixed effects that should wash out all unobservable variation common to firms operating in the same narrowly defined geographic area and in the same sector during the same year. We specify two-way clustered standard errors at the sector and period level (Petersen, 2009). Finally, we estimate Equation (1) using OLS.[19]

The contribution of this paper is the analysis of the growth of firms experiencing a shock to their credit access by firm size and elasticity of substitution between labor and capital (or $\widehat{\sigma}_s$). By specifying a firm-size threshold at 50 employees, we are not comparing small to large firms, but rather exploiting, in a regression discontinuity sense, a firm-size-specific labor regulation. Thus, the coefficient of interest is β_1. It captures the difference in sales growth between a firm attached to an affected and a firm attached to a healthy bank, depending on size and industry. A positive coefficient β_1 would imply that a firm with fewer than 50 employees borrowing from an affected bank is experiencing a smaller decline in sales growth if it is in a high-σ sector. The economic interpretation in this case would be that in sectors where firms can substitute across inputs in production, flexible labor market rules act to counter the negative impact of credit shocks.

Distinguishing across firms' technological ability to substitute between factors of production is crucial for identification. Balance sheet shocks lead banks to reduce credit to their borrowers, and a large literature has argued that smaller firms are affected more forcefully by this process as their investment projects are more opaque and uncertain (e.g., Berger and Udell, 1995). This firm-size effect would imply that after the shock to their creditor, firms with fewer than 50 employees may suffer more in terms of growth as banks tighten credit relatively more for them. **Figure 2** plots growth rates before and after the credit shock for the firms in our sample. It clearly shows that while firms with fewer than 50 employees and firms with more than 50 employees, both of which became affected during the crisis, were growing at approximately the same rate up to 2007, in 2008 and 2009 sales growth declined considerably more for smaller firms. However, **Figure 3** demonstrated that this divergence in growth rates is driven by firms in sectors with below-median elasticity of substitution between capital and labor. At the same time, for firms

[18] There are 50 NUTS3 regions in the dataset, corresponding to the 50 Provinces of Spain.

[19] In estimation, we use cluster robust standard errors. Since we introduce σ_s from a separate regression, we examined the robustness of our results to bootstrap procedures. For a standard bootstrap, there were no change in the inference of the parameters in the table. Since we have two fixed effects in our case, however, the wild bootstrap breaks down. We thank Dimitris Georgarakos and David Roodman for discussion on these issues.

in sectors with above-median elasticity of factor substitution, affected firms with fewer than 50 employees post relatively higher growth than affected firms with more than 50 employees. This suggests that in the absence of substitutability between the factors of production, the "credit constraint" effect can dominate the "employment-flexibility" effect, and that only firms that can technologically substitute labor for capital will benefit from more flexible employment protection when the user cost of capital goes up.

The sample period is 2006–2009, including two years before and two years after the start of the financial crisis. The underlying assumption is that from an identification point of view, any effect of tightening credit constraints on individual firms would be immediate, while starting in 2010, as a result of the unfolding sovereign debt crisis, there would be more forces at play affecting firms' growth. Nevertheless, in robustness tests, we look at a longer period, to capture more medium-term effects.

We rely on two important identifying assumptions. The first one is that if firms can optimally set their size, their decision whether to stay below or above the 50-employee threshold before the crisis is orthogonal to the subsequent credit shock. The second assumption is that a credit association with an affected bank results in a decline in credit from that bank to the firm. Regarding the second assumption, Appendix Table D.2 demonstrates that firms attached to an affected bank indeed experienced a decline in overall loans (column (1)), especially if they were smaller (column (2)). Controlling for firm size, there does not appear to be a statistical difference in credit growth at firms in high- versus firms in low-σ industries (column (3)).

5 Empirical results

5.1 Main result

We begin by testing more parsimonious versions and gradually building towards the most saturated version of Equation (1). In that way, we are able to evaluate all underlying mechanisms that we had in mind when formulating our empirical model. All of these tests are reported in **Table 3**.

The first underlying mechanism relates credit constraints to firm growth. In particular, we postulate that *ceteris paribus*, tightening credit constraints due to balance sheet problems at the firm's creditor have a negative impact on firm growth. We evaluate this prediction in column (1) where we only include the variable $Shock$, alongside firm and sector $\times$ period and region $\times$ period fixed effects. The point estimate is negative and significant at the 10 percent statistical level, confirming the main intuition. In addition, the effect is economically meaningful, too: all else equal, a firm with a credit relationship with an affected bank experiences a decline in its

sales growth by around one-third of the sample mean. Note that this is a conservative estimate because we have included in the sample both single-bank firms and firms that can substitute across creditors. The estimates from this test thus confirm the intuition that firm sales decline following an increase in the user cost of capital.

We next proceed to evaluate how credit shocks interact with the other components of the triple interaction in Equation (1). In column (2) we add the interaction of the variable $Shock$ with the < 50 $employees$ dummy which is equal to one if the firm has fewer than 50 employees. The estimates from this regression make it clear that only small firms experience a decline in their sales growth when their creditor experiences balance sheet problems. In this case, the decline in sales growth for affected firms with fewer than 50 employees is 1.06 percentage points, or 0.41 percent lower than the sample mean sales growth. In column (3), we instead add the interaction of the variable $Shock$ with the empirical estimate of the technological elasticity of substitution between capital and labor in the sector in which the firm operates. This test makes it clear that the direct effect of the credit shock is statistically indistinguishable across high-$\widehat{\sigma}$ and low-$\widehat{\sigma}$ sectors.

In column (4), we introduce the triple interaction together with the two estimable double interactions and the fixed effects. This regression strongly rejects the null hypothesis that employment protection and the technological substitution between labor and capital are not associated with changes in firm growth in the presence of credit shocks. Namely, we find that all else equal, a credit shock reduces sales growth more for small firms, which are the firms that are by default more dependent on bank credit for their operations. We also find that all else equal, a credit shock reduces sales growth more for firms in high-σ industries. Crucially, the impact of a credit shock is reduced for firms with fewer than 50 employees in high-$\widehat{\sigma}$ sectors. Recall that these are the firms that can substitute labor for capital, because their technology allows them to do so. These are also the firms for which it is less costly to substitute labor for capital because employment protection regulation makes it easier for them to fire employees. Therefore, we conclude that we have identified a positive impact on firm growth of more flexible labor regulation in the presence of credit shocks. The estimates from this test thus confirm the intuition that the decline in firm sales following an increase in the user cost of capital is smaller for firms in high-sigma industries, especially if the burden imposed by labor regulation is smaller.

In column (5), we add a polynomial of third degree in firm employment. It makes sure that we are comparing sharply firms around the threshold, controlling for employment. The inclusion of the polynomial changes very little the main coefficients of interest, suggesting that the regression-discontinuity effect is remarkably stable. In column (6) we also include the set of firm-specific time-varying controls. We find that larger firms have on average lower sales

growth, while firms with higher ratio of cash flows to assets and firms with higher average employee compensation have on average higher sales growth. These results are logical and they also serve to validate the data we are using. We also find that controlling for size, age is not correlated with sales growth in an meaningful way. Importantly, we find that all variables of interest used to identify the main effect have the expected sign, just as in columns (4) and (5). The same applies to column (7) where we estimate our preferred specification where we also add the interaction of the polynomial of third degree in firm employment with the variables $Shock$ and σ. In both cases, we confirm that a credit shock reduces sales growth more for small firms, but its impact is reduced for small firms in high-$\widehat{\sigma}$ sectors. These are the firms that can both technologically substitute labor for capital and that find it relatively cheap to do so.

In terms of economic magnitudes, our empirical strategy allows us to compare firms across industries based on their technological ability to substitute between labor and capital (σ). Consider two industries, one at the 75th percentile of $\widehat{\sigma}$ (Chemicals and chemical products) and another at the 25th percentile of $\widehat{\sigma}$ (Rubber and plastic). The difference in technological capital-labor elasticity between the two sectors is 0.33. The point estimate on the triple interaction $Shock_{ft} \times < 50\ employees_f \times \sigma_s$ in column (7) is 0.0450. Our estimates thus imply that a firm with fewer than 50 employees borrowing from an affected bank grows by 1.48 percentage points faster in "Chemical and chemical products" than in "Rubber and plastic", which implies a reduction in the average decline in firm growth over the sample period of around 58 percent. Alternatively put, the point estimates in Table 3 on the interaction $Shock_{ft} \times < 50\ employees_f \times \sigma_s$ and on the interaction $Shock_{ft} \times < 50\ employees_f$ imply that while on average, a firm with fewer than 50 employees declines by more than a firm with more than 50 employees, it declines by less as long as $\widehat{\sigma} > 1.23$. Our evidence is thus a direct confirmation of the importance of more flexible employment protection rules when the rental cost of capital goes up and firms have the technological ability to substitute labor for capital.[20]

5.2 Empirical channels

We now turn to the empirical channels that are plausibly activated to cause the increase in firm growth as a result of flexible labor regulation. The underlying hypothesis is clearly that as firms are faced with a shock to external credit that raises the cost of renting capital, those which can substitute labor for capital—because both their technology and the regulatory framework make

[20] Garibaldi and Violante (2005) demonstrate that the distortions introduced by employment protection can be completely undone by efficient contracts, whereby the mandated firing costs are passed on to workers who are willing to accept a lower wage in return for a lower dismissal probability. Our evidence suggests that this mechanism is not operational in the Spanish setting, potentially because of labor market frictions that prevent wage adjustments. Furthermore, in unreported regressions, we find that while after the start of the crisis, labor compensation on average goes down at firms borrowing from affected banks, this effect does not vary with the technological elasticity of substitution between labor and capital.

it possible—will do so. Therefore, conditional on being subject to a credit shock, we should observe higher relative employment growth in firms with fewer than 50 employees in high-$\widehat{\sigma}$ industries, relative to firms with more than 50 employees and/or firms in low-$\widehat{\sigma}$ industries. The same argument should not apply to capital investment, which should decline for both types of firms, regardless of whether they can substitute labor for capital or not.

Table 4 presents a direct test of this hypothesis. We modify model Equation (1) in two ways. In column (1), we replace the dependent variable with period-average employment growth, aggregating the annual change in log employment from one year to the next. We find that the credit shock on its own has a negative impact on employment growth. This result confirms recent findings in the literature pointing to a direct effect of credit frictions on employment during the financial crisis (e.g., Chodorow-Reich, 2014; Bentolila et al., 2018; Popov and Rocholl, 2018). Crucially, our estimates clearly show that size-dependent labor regulation has a strong impact on labor demand. In particular, firms subject to a credit shock are considerably more likely to hire more workers if their technology allows them to do so (i.e., capital and labor are substitutes) and if labor regulation reduces their cost of hiring (i.e., they have fewer than 50 employees and so are not subject to strict collective dismissal restrictions). At the same time, firm-size-specific labor regulation does not appear to have a differential impact across firm sizes on capital investment (column (2)), which is consistent with our prior.

5.3 Controlling for the effect of firm size and firm quality

We now address two first-order concerns with our results. First, it is possible that our results capture the effect of firm size as opposed to employment protection. Although Spanish labor legislation contains a sharp employment-protection threshold at 50 employees, 50 employees is a threshold used in different laws to discriminate between firms in Spain. For instance, the SMEs definition in Spain, until the end of 2013 at least, required, among other factors, that companies have no more than 50 employees in two consecutive years; a workers council is required for companies with more than 50 workers; and firms with more than 50 employees are required to have a canteen. Examples like these suggests that firms with more than 50 employees face higher per-worker labor costs that are not necessarily related to employment protection. Thus it is possible that the effect reported in Table 3 is not necessarily due to dismissal costs, compromising our empirical strategy.

In order to alleviate this concern, we now include in our regression controls for firm size (namely, employment and the logarithm of total tangible assets) in interaction with $Shock_{ft}$ and with $Shock_{ft} \times \widehat{\sigma}_s$. Column (1) of **Table 5** demonstrates that controlling for employment, sales growth at firms with more tangible assets declines more after being exposed to a credit

shock. The coefficients on the main variables of interest, however, are remarkably stable: we still find that the impact of a credit shock is lower for small firms in high-σ industries, which are the firms that both can and are allowed to substitute labor for capital. The effect is still significant at the 1-percent statistical level, suggesting that other factors that make firms with more and with fewer than 50 employees different do not explain away the positive impact of flexible employment protection during a credit shock.

Second, it is possible that more affected banks are more likely to grant loans to worse firms (e.g., Bentolila et al., 2018), suggesting non-random sorting between firms and banks. In order to address this issue, we next augment our main regression to account for the independent effect of firm quality. In practice, we include in our regression the firm's ratio of cash flow to assets (a proxy for profitability) and the firm's net worth (a proxy for credit quality), in interaction with $Shock_{ft}$ and with $Shock_{ft} \times \widehat{\sigma}_s$. Column (2) of Table 5 suggests that conditional on a credit shock, more profitable firms grow faster, suggesting that indeed firm quality exerts an independent effect on the growth of firms hit by a shock to their borrowing capacity. Crucially, we still find that the impact of a credit shock is lower for small firms in high-σ industries, and the effect is still significant at the 1 percent statistical level, suggesting that the potential selection of lower-quality firms to more affected banks does not explain the main result in the paper.

Finally, we include all these additional double and triple interactions in a horse race in column (3) of Table 5. We find that while both firm size and firm quality matter for the relative performance of credit-constrained firms with fewer than 50 employees, the interaction of a firm-size-specific employment protection threshold and the technological ability to substitute labor for capital still explains a substantial part of the variation in sales growth across credit constrained firms.

5.4 Falsification tests

In this Section, we report the estimates from a number of falsification tests. In particular, the results we document should disappear once we perform our tests on samples where the labor regulation and the credit shock we base our analysis on no longer bind. Recall that the underlying mechanisms we test is that—controlling for technology—firms with fewer than 50 employees reach different outcomes than larger firms when faced with a credit shock. We now perform three different tests where we arbitrarily move first the credit shock, then the firm-size cut-off, and finally perform our test on a sample of firms derived from a different country (Germany) where some firms during the crisis are subject to a credit shock, but there is no discontinuity in labor regulation at 50 employees. We report the estimates from these tests in **Table 6**.

In order for the credit shock we use to be valid, it has to bite only once banks are suddenly

hit by balance sheet problems. In other words, it should not have an effect before the financial crisis when the same banks were posting healthy growth in both lending and profitability. To test this underlying assumption, in column (1) we perform our underlying test of Equation (1) on the period 2004–2007, using the same sample of firms, the same firm-bank matches, and the same definition of an affected bank. Similar to the main specification, we collapse all data into one observation for each of two periods, pre- (2004 and 2005) and post- (2006 and 2007). The only difference thus is that we are performing our diff-in-diff-in-diff on a sample period fully preceding the financial crisis. This should result in a random assignment of non-existing credit shocks to firms, and should yield no significant association between the shock and its interaction with firm size and firm technology with firm growth. Column (1) reports that this is indeed the case. Not only is there no statistical correlation between the shock and firm growth, but randomly "shocked" firms are also not more likely to experience different sales growth rates regardless of their size and of their technological ability to substitute labor and capital in the production process.

In column (2), we subject to a falsification test the assumption that what the 50-employee cut-off is measuring is the impact of firm-size-specific labor regulation which makes it cheaper for firms with fewer than 50 employees to hire workers when they need to. An alternative explanation is that the results we reported in Table 3 simply capture a difference between small and large firms, in that smaller firms find it naturally easier to substitute labor for capital. If so, our results would still hold when we move the size cut-off around. In column (2), we perform a test of this hypothesis. We replace the $< 50\ employees$ dummy with a $< 10\ employees$ dummy equal to 1 if the firm had fewer than 10 employees before 2008. We preserve the other components of our tests unchanged, namely, we assign firms the same credit shock and the same industry-specific elasticity of substitution between capital and labor. The data fail to reject the hypothesis that there is no difference between small and large firms when we alter the definition of "small", suggesting that firm size indeed works through the impact of firm-size-specific employment protection.

While this test suggests that the 50-employee cut-off is materially different from another way of separating small from large firms, it could still be the case that firms with fewer than 50 employees differ from larger firms in ways that are unobserved to the econometrician and are common across the global corporate landscape. However, if this is the case, then we can run Equation (1) on a sample of firms in a country without a 50-employee employment protection rule. If we continue getting a significant association between the firm-size cut-off, the firm's elasticity of substitution between capital and labor, the credit shock, and firm growth, then the underlying mechanism we have in mind will be compromised.

To address this point, we download from Orbis the exact same balance sheet information for the universe of firms in Germany. We choose this country for two different reasons. For one, during the financial crisis it experienced a similar type of credit shock whereby five of its State clearing banks, Landesbanken, needed to be recapitalized by their daughter savings banks because they had overinvested in the US mortgage-backed-securities market. The remaining 7 Landesbanken, and therefore their daughter savings banks, did not experience this shock. This makes it possible to determine which firms are linked to "affected" banks and therefore credibly experiencing a negative credit shock.[21] This makes the sample of German firms similar to the sample of Spanish firms that we are using in that some firms are subjected to an exogenous credit shock thanks to their pre-crisis association with banks which during the crisis experienced balance sheet problems. The second reason is that there is no labor regulation in Germany that distinguishes between firms with more and firms with fewer than 50 employees. If our identifying assumptions are wrong and there is an (unobservable) difference between firms with more and firms with fewer than 50 employees that is independent of labor regulation, we should register in the sample of German firms the same effect that we observe in the sample of Spanish firms. The point estimates reported in column (3) strongly suggest that this is not the case: the same firm-size cut-off that works in Spain does not affect the interaction between the credit shock, the firm's technology, and firm growth.

We conclude that the data provide us with no reason to believe that our results are due to choosing a definition of a credit shock and of a firm-size cut-off that are associated with forces which have affect firm growth outside of their impact through the relative cost of credit and the relative cost of hiring.

5.5 Robustness

5.5.1 Alternative empirical proxies and model robustness

In **Table 7**, we proceed to address a number of concerns related to the construction of our main explanatory variables. We start with the estimated elasticity of substitution between labor and capital. As we noted already, we use data on the inputs in production from the US KLEMs database. This database provides two separate data series, one encompassing the period 1947 to 2010, and one encompassing the period 1970 to 2007. In the tests so far we rely on the longer dataset which is both more popular and provides for a more robust estimation of the underlying sector-specific elasticities. An argument in favor of the second series is that it captures a period of more mature industrial development, aligning it more closely to the technological

[21] See Puri et al. (2011) for institutional details and for evidence that affected savings banks reduced retail lending. Also, see Popov and Rocholl (2018) for evidence that firms borrowing from such banks reduced employment and labor compensation.

characteristics of Spanish firms during the 2000s. Although it follows the same classification, the second database represent a different vintage of the underlying BEA data and some slightly different statistical methods.

To test for whether our main result is not driven by a particular choice of data in calculating sector-specific benchmarks for the elasticity of substitution between labor and capital, we re-estimate $\widehat{\sigma}$ using the second, shorter data series. Then we use the resulting sector-specific values to re-estimate Equation (1). The estimates from this regression are reported in column (1). They strongly suggest that the statistical association between the credit shock, labor regulation, the firm's technology, and firm growth is not a feature of the particular data series we choose to construct sector-specific elasticities of substitution between labor and capital.

In column (2), we address a similar point by noting that the distribution of estimated $\widehat{\sigma}'s$ has a median value of 0.75. At the same time, the economically relevant value is 1: when $\sigma > 1$, firms find it easier to substitute between labor and capital, while when $\sigma < 1$, capital and labor start being more of complements in production. To address this point, we use the main estimates of the sector-specific elasticities to create a dummy variable equal to 1 if for a particular sector, $\sigma > 1$, and to zero otherwise. In this way, only 4 of the 23 sectors in the dataset are defined as high-σ. Our estimates strongly suggest that it is indeed firms in high-σ sectors with fewer than 50 employees for whom a credit shock has a weaker impact on sales growth.

The empirical estimate of σ that we use throughout the paper is based on US data. Our rationale for using US data is that the observed elasticity of substitution between labor and capital is closer to the "technological" one in a country with fewer credit and labor constraints, such as the US, than in a country with strong labor constraints and less liquid financial markets, such as Spain. It is however possible that a sector-specific elasticity of substitution between labor and capital estimated from US data captures poorly the sector-specific elasticity of factor substitution in Spain. To address this, in column (3) we replicate our model after first calculating the elasticity of factor substitution using KLEMS data on Spain. Unfortunately, data are not available for 6 out of 23 sectors, relative to the US, and so the number of observations declines by about 44%. At the same time, the comparison between countries is quite well taken. For instance, some of the highest elasticity values for the US are shared by the same sectors as Spain, and the correlation between the two country series is almost 0.8, and testing for rank correlations does not reveal independence of distributions. This test confirms that firms for which funding costs are increasing grow faster if they are subject to less stringent labor regulation. This supports the idea that our main result is not an artefact of a particular choice of data to calculate technological proxies.

Our empirical model is based on the interaction of labor regulation and a credit shock with a

particular technological property that allows for the identification of the growth impact of labor regulation during a credit crunch through the substitution of labor for capital. Nevertheless, a high degree of technological substitutability between labor and capital can correlate with another industry characteristic which, if concurrently active, can introduce bias in our results. One such property is the sector's technological dependence on external finance. An argument going back to the seminal paper by Rajan and Zingales (1998) is that for technological reasons, some sectors are able to finance their operations to a higher degree with internal funds, while others rely more on external finance. Evidence from the financial crisis suggested that small firms in such sectors tend to be more affected by a credit shock as they have few alternative funding sources, and so a reduction of access to bank credit causes them to cut both employment and investment (e.g., Duygan-Bump et al. 2015; Popov and Rocholl, 2018). If the production function of firms in such sectors is also characterized by a technology where capital and labor are close substitutes, then our estimates of β_1 would partially be capturing the impact of external financial dependence. In column (4), we put this concern to the test. We first obtain data on the sectors' external dependence from Duygan-Bump et al. (2015) for the period 1980–2000. Then we create interactions of this industry benchmark with the variables $Shock$ and $Shock\times < 50\ employees$. The estimates make it clear that our main results are not biased by not accounting for the firm's natural dependence on external finance. The point estimate of β_1 is once again significant at the 1-percent statistical level, suggesting that our main results are not contaminated by firms in high-σ industries being also in industries highly dependent on external finance.

Next, we employ an alternative empirical proxy for a shock to the firm's borrowing capacity. In our main specifications, the treatment group comprises firms attached to banks that required a government intervention of some type during the financial crisis, and the control group comprises firm attached to banks that did not. This approach lumps together firms with credit relationships with banks in very poor health and firms with credit relationships with marginally affected banks, in the first case. In the second case, firms attached to unhealthy banks that did not however require government assistance are included in the control group. At the same time, some "affected" banks may have got into trouble relatively late in the cycle. To correct for such potential misclassification, we now create a dummy variable equal to one if the firm's credit has a total capital ratio of less than 10% in 2007 (during the pre-crisis period), and to 0 otherwise. In this way, we split firms into more and less likely to see their cost of funding increase by a regulatory balance-sheet criterion. The test reported in column (5) suggests that the main gist of the paper is not affected by the scheme used to classify banks as affected.

In column (6) we re-run our main specification (1) on the original annual dataset, i.e., before collapsing all data into one observation per period. The number of observations does not double

relative to Table 3, column (7) because not all firms have observations for all 4 years of the sample period. Once again, the data unequivocally reject the null hypothesis that credit shocks do not interact with employment protection in affecting firm growth during times of credit stress.

Finally, in column (7) we re-run our preferred specification after including region $\times$ sector $\times$ period fixed effects in the place of region $\times$ period and sector $\times$ period fixed effects. This allows for an even tighter identification of the effect by netting out all unobservable variation that is common to all firms in the same sector in the same region at the same point in time (i.e., real estate in Madrid after the crisis). The point estimate of the main interaction of interest is remarkably stable and still significant at the 1-percent statistical level, suggesting that the effect we document is not an artefact of unaccounted for unobservable region-sector-specific trends.

5.5.2 Sample robustness

The main advantage of the empirical design that we are using is that we are exploiting a clear policy-driven discontinuity along the firm size distribution. Firms with fewer than 50 employees find it easier to fire workers, making it in return cheaper for them to hire workers they can then fire. Conversely, hiring new workers is more difficult for firms with more than 50 employees which for whom regulation makes collective dismissals much more costly.

For this policy discontinuity to bite, we would ideally have to estimate our model for firms close to the cut-off. Otherwise, we would be running the risk of comparing a sub-sample dominated by very large firms (e.g., with more than 1000 employees) to a sub-sample of very small firms (e.g., with less than 5 workers), a concern our falsification test in Table 6, column (2) does not address. Lending weight to this concern is the fact that 89.4 percent of the firms in our sample have fewer than 50 employees, and the median firm has 11 employees. As a practical example, labor supply could be very different for workers in large firms, as these may be more high-skilled and better educated, making it more difficult to identify shocks to labor demand. While we address this point to some extent by controlling for average firm-level compensation throughout, we need to tackle the issue more forcefully.

To address this issue, in columns (1) and (2) of **Table 8** we report estimates from tests where we have restricted the sample to a narrower window around the 50-employee cut-off. In column (1), we restrict the sample to firms with more than 40 and firms with less than 85 employees, which corresponds to 4 percent of the sample on each side of the 50-employee cut-off. In column (2), we restrict the sample to an even tighter window: firms with more than 43 and firms with less than 64 employees, which corresponds to 2 percent of the sample on each side of the 50-employee cut-off. In both cases, the point estimate on the triple interaction is positive and significant at the 5-percent statistical level. The evidence thus continues suggesting that a credit

shock which raises the rental cost of capital has a smaller impact on firms that can substitute labor for capital and are not discouraged from doing so by strict employment protection rules.

Next, we extent the sample period on both sides, by one year (column (3)) and by two years (column (4)). In our main tests, we deliberately chose a sample period ending in 2010, so that our estimates are not contaminated by the sovereign debt crisis which erupted in 2010. At the same time, we would like to know how persistent the effect that we document is. In particular, the benefits of flexible labor regulation would be smaller if firms can only substitute across inputs of production in the short-run. However, the estimates from our model suggest that the increase in firm growth coming from the substitution of labor for capital does not disappear in the medium run, and the overall numerical effects is broadly similar once we look at a longer period after the initial shock. This suggests that the benefit of flexible labor regulation during times of credit distress extend beyond impact and provide longer-term benefits to firms.

Another concern associated with our sample choice is that our sample includes firms from the construction sector. It has been well-established that a number of Spanish banks had become excessively exposed to local construction, and so they encountered severe problems once the Spanish housing bubble burst (e.g., Bentolila et al., 2018). In the case of construction firms attached to troubled banks, the argument can be made that the direction of the shock ran from the real to the banking sector, which questions our assumption about the exogeneity of the credit shock. To address this concern, we drop all firms in NACE sectors 41, 42, and 43. Column (5) reports the estimates of this test, and it suggests that our main results are not sensitive to the presence of construction firms in the dataset.

There is one further, data-related concern we need to address. We have classified firms as affected by a credit shock if they have a credit association with at least one affected bank. This might be inaccurate in the context of multiple banking relationships that firms can substitute across. It is true that the potential bias goes in our favor: if firms can substitute across banks, it makes it more difficult to find any effect of the credit shock on firm growth. However, if firms could perfectly substitute away from affected banks, but we still find an effect of association with an affected bank, it would imply that the correlation between supply shocks and firm responses that we have captured is a spurious one.

To that end, in column (6), we exclude those firms that are classified as affected because their bank experienced problems during the crisis, but at the same time have a relationship with at least one other, unaffected bank. The remaining affected firms will be unable to make up for the decline in credit from their main creditor by borrowing from another bank, and due to their size, they will find it difficult to substitute for the decline of bank credit by accessing a non-bank funding source. Our estimates imply that the negative effect of the credit shock on small firms'

growth holds for the treatment group of firms that only bank with one creditor, too. Importantly, the decline in firm growth is still significantly lower for firms that are not subject to restrictions on collective dismissal, and this effect is significant at the 10 percent statistical level. We thus confirm that the statistical association between changes in financing access and changes in firm growth that we have uncovered is not spurious in that it also holds in the extreme case when firms cannot substitute between affected and non-affected banks.

Another concern is related to the comparability of firms borrowing from affected banks and those borrowing from healthy banks. Appendix Table D3 compares these two samples based on observable pre-crisis balance sheet characteristics. While the two groups are statistically similar, firms borrowing from affected banks are on average older by half a year, as well as slightly healthier (i.e., their net worth is higher by 0.01). While these statistical differences appear to be minimal, we nevertheless need to make sure that the main result in the paper is not driven by them. To that end, in column (7), we construct a smaller sample where firms in the treatment and in the control group are chosen based on propensity-score matching on pre-crisis observations of firm-specific variables whose means differ significantly across the two groups of firms. The evidence strongly suggests that statistical differences across the two groups of firms are not responsible for the main empirical regularity we document in this paper.

Finally, our results may be contaminated by selection issues, if firms that are more responsive to labor rules choose to stay below the threshold for strategic reasons. Figure 1 suggests that indeed, there is a bunching of firms right below the threshold. A McCrary test formally rejects the hypothesis of local continuity at 50. To address this concern, in column (8) we drop firms with 48 and 49 employees (which is where the distribution visually deviates from a smooth one). The evidence strongly confirms the main result of the paper, even though the numerical magnitude of the main effect declines by about 15%. We get identical results if we also exclude firms with 50, 51, and 52 employees (results not reported for brevity and available upon request).

5.5.3 Distinguishing between high- and low-growth firms

The main identifying mechanism in our paper is that when the user cost of capital goes up because of an inward shift of the supply of credit, firms with healthy growth prospects want to substitute labor for capital in order to keep growing. At the same time, they can only do so if both their technology and the extent employment protection allow them to do so. This mechanism rests on the assumption that firms have healthy growth prospects which are not affected by the same background forces which generate the credit crunch. If this assumption is violated, there is no reason to expect that firms would want to substitute between factors of production and keep growing, in which case we may have detected a spurious correlation

between credit shocks, employment protection, the elasticity of substitution between labor and capital, and firm growth.

To address this concern, in **Table 9** we split the sample between firms in the top tertile and firms in the bottom two tertiles of the distribution of sales growth and in labor productivity growth during the pre-crisis period (2006 and 2007). We calculate the latter as the log difference in value added per worker between two adjacent years. It is reasonable to assume that firms which were growing in a healthy fashion right before the credit shock were also facing better growth prospects a year or two later. The evidence suggests that the mechanism we identify is most relevant for these firms. In particular, within the sample of high-growth firms, smaller firms are more affected than larger firms by the credit shock, but they benefit more if they are in high-σ sectors, suggesting that they indeed substitute labor for capital in order to keep growing (columns (1) and (3)). We detect a much weaker such pattern in the data when we zoom in onto the sample of lower-growth firms (columns (2) and (4)), validating the underlying assumption behind our empirical strategy.

5.6 Constrained firms

In this study, we compute the impact of credit shocks on firm growth, accounting for firm-size-specific labor regulation and for firm technology. Our estimates suggest that for firms with fewer than 50 employees, tightening of financing costs following a credit shock can be compensated by an increase in employment, if these firms' technology exhibited a large enough elasticity of substitution between capital and labor.

Table 10 reports estimates from a robustness test whereby we restrict the sample to firms with a credit relationship with an affected bank, observed after the crisis (i.e., in 2008 or in 2009). This allows us to modify model (1) such that the main explanatory variable is an interaction between a dummy variable equal to 1 for firms with fewer than 50 employees and the sector-specific elasticity of substitution. The point estimates clearly show that after the crisis, smaller credit constrained firms grew more slowly than otherwise comparable larger firms, however, the effect of firm size is mitigated in high-σ industries (column (1)). Columns (2) and (3) further suggest that the higher growth of small firms in high-σ industries is entirely due to higher employment growth and not to higher investment growth, confirming the motivating empirical mechanism. Because this regression model is based on a double interaction, it is easier to interpret. Nevertheless, throughout the paper we give preference to the triple-interaction-based Equation (1) because it allows to control for the impact of unobservable time-invariant firm heterogeneity, as well as to compare constrained to unconstrained firms.

6 Aggregate effects

In this study, we compute the impact of credit shocks on firm growth, accounting for firm-size-specific labor regulation and for firm technology. Our estimates suggest that for firms with fewer than 50 employees, tightening of financing costs following a credit shock can be compensated by an increase in employment, if these firms' technology exhibited a large enough elasticity of substitution between capital and labor.

How important is this effect in the aggregate? We use our estimates to perform a back-of-the-envelope calculation of the extent to which the combination of technology and labor market flexibility dampened the impact of credit shocks during the 2008–09 financial crisis. We first note that our estimates are obtained from firm association with creditors that are affected to a different degree during the crisis. In particular, we make the simplifying assumption that banks that did not require government intervention during the crisis did not raise the cost of capital for firms borrowing from them. This assumption produces conservative estimates of the overall effect of credit if non-affected banks also reduced credit supply during the crisis. The opposite occurs if non-affected banks expanded credit supply to substitute for the unfulfilled demand by banks with higher share of foreign funding.

The estimates in Table 3 imply that while on average, sales growth by a firm with fewer than 50 employees declines by more than for an otherwise similar firm with more than 50 employees, it declines by less as long as $\sigma > 1.23$. In our dataset, 89.4% of all firms have fewer than 50 employees. In addition, 26% of all firms are borrowing from affected banks, and 26% of all firms were operating in industries with $\sigma > 1.23$. Our estimates thus imply that after the credit crunch, 6% of all firms benefited from flexible labor regulation enough to overcome the firm-size effects of credit constraints, as long as they had the right technology. According to the Structural Business Statistics, there were 1,756,620 firms in Spain in 2007. This suggests that labor market flexibility benefited a total of 105,400 firms. The average firm with less than 50 employees employs 12.7 workers, therefore, flexible employment protection rules benefited firms employing around 1.34 million workers.

7 External validity

We have established that during the Spanish credit crunch of 2008–09, flexible labor regulation benefited a number of firms by enabling them to substitute labor for capital and to keep growing. At the same time, our results would be even more powerful if they held in more than one country, and during more than one episode. We now conduct a formal investigation into whether this is

the case, and report the results in **Table 11**.

We first note that even before the crisis, there were relatively stronger and relatively weaker banks in Spain. If we account for this possibility, we would observe our crisis mechanism already before the crisis. We do so by calculating, for each firm, the change in Tier 1 capital between 2004/05 and 2006/07 (i.e., in both cases during the pre-crisis period). Then we create a dummy variable 'Weak bank' equal to one if the firm has a credit attachment to a bank whose Tier 1 capital declined between the two periods. Then we re-run the model in Equation (1) on this sample period and with this definition of 'affected firm'. The result is reported in column (1) of Table 11. The evidence suggests that a decline in the capital ratio of the firm's creditor leads to a decline in firm growth, but this decline is smaller if the firm is subject to less stringent employment protection. Importantly, this effect is observed outside of the crisis period, suggesting that the main mechanism identified in our paper is operational also during boom periods.

Next, we turn to test for our main mechanism in the context of the German credit crisis of 2007–08. Similar to Spain, employment protection in Germany is more stringent for larger firms.[22] Unlike Spain, the threshold is set at 10 employees, instead of 50. This allows us to run a test of whether the same mechanism that works in Spain works in Germany, too. We take advantage of the fact that Germany experienced a banking crisis in 2007–08, linked to the exposure of some (but not all) local savings banks to the US mortgage crisis (see Section 5.4 for more detail). We use our data to identify "affected" German firms before and after the financial crisis, and use the size threshold to run our main empirical specification. The estimates from this modification of Equation (1) are reported in column (2) of Table 11. They strongly suggest that the mechanism we identify in the case of the Spanish credit crunch worked in the same way in Germany during the financial crisis. We conclude that our results are not limited ot one country or one episode.

8 Conclusions

The recent financial crisis resulted in a dramatic increase in unemployment,[23] sparking renewed interest in how policy can help the economy withstand negative shocks and to sustain employment and output growth. While recessions may have 'cleansing' effects, deep and long recessions such as the one in Spain can leave major scars with negative long-run effects. This raises the question of whether more flexible labor regulation benefits firms not only during the upside, but also during the downside of the business cycle. In this paper, we demonstrate the

[22] I.e., protection against unfair dismissals applies; see https://www.mayr-arbeitsrecht.de/en/spectrum/german-employment-law.

[23] Spain was the arena of the most dramatic development in that regard, with youth unemployment reaching over 50 percent, higher than in any other OECD economy.

key role that employment protection and firms' technological characteristics play in this adjustment process. Failure to pay attention to these channels risks an incomplete understanding of the effect of credit shocks.

We show that relative to firms with 50+ employees, firms with fewer than 50 employees grew relatively faster when exposed to a negative credit shocks if they were operating in sectors with a technologically higher elasticity of substitution between labor and capital. Since firms with less than 50 employees face lower dismissal costs on account of labor regulation, these results support the view that labor market flexibility enhances the ability of firms to absorb large negative shocks. These results hold only for firms in sectors with a relatively high substitution elasticity, that can flexibly adjust labor following an increase in the user cost of capital. The main result is robust to controlling for time-varying firm-specific factors that can affect firm growth in the absence of credit shocks or firm-size-specific labor regulation, such as size, cash flows, and net worth. It is also robust to controlling throughout for unobservable firm heterogeneity with firm fixed effects, and for unobservable region-specific and sector-specific trends with interactions of region, sector, and year dummies. We continue to obtain our main effect when we compare smaller and larger firms that are closer to the 50-employee threshold, when we control for other underlying industry characteristics, such as dependence on external finance, and when we look at firms with a credit relationship with only one bank. Importantly, the effect is mostly present for high-growth firms, which suggests that during a credit crunch, more flexible employment protection mainly benefits firms with good growth prospects by enabling them to substitute labor for capital when the user cost of capital has increased. In terms of underlying channels, we find that the effect operates primarily through affecting employment, not capital, which indicates that the ability to substitute labor for capital is an important driver of firm growth during a credit crunch.

Our results tend to support the adoption of flexible labor laws. While such labor market reforms do not come without pain for incumbent workers, they allow firms to recover more quickly from a deep recession whose roots are in the banking sector. Consistent with this idea, Spain has indeed embarked since the financial crisis on a package of labor market reforms with a view to make labor markets more flexible and in particular ease the cost of dismissals for firms.

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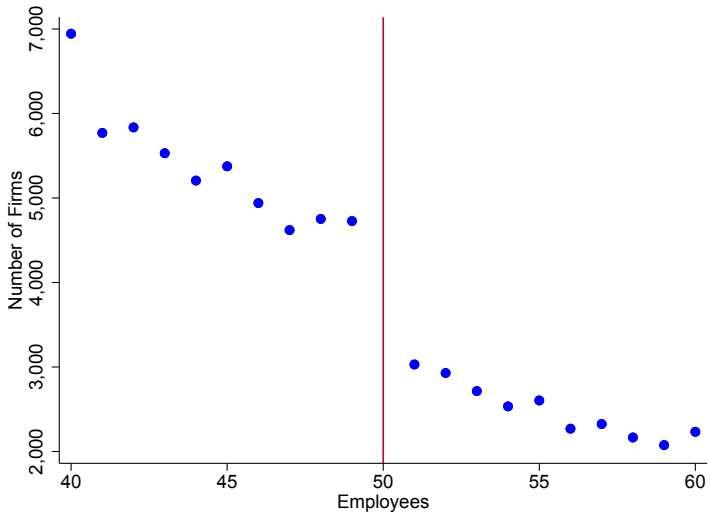
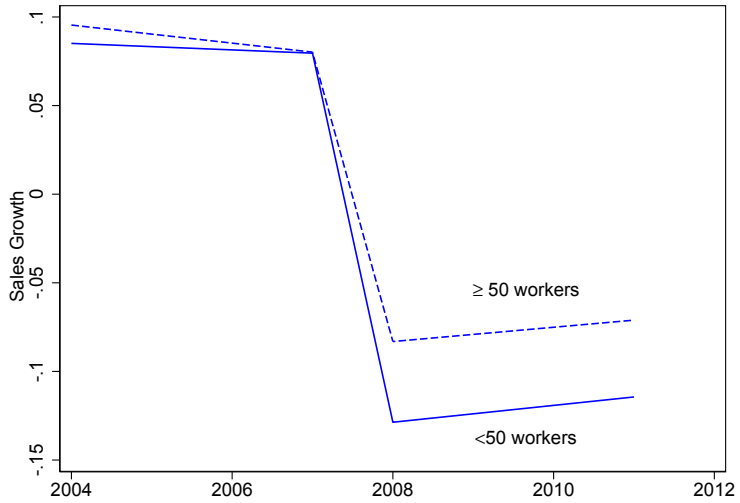
Figure 1: Distribution of firms, by number of employees



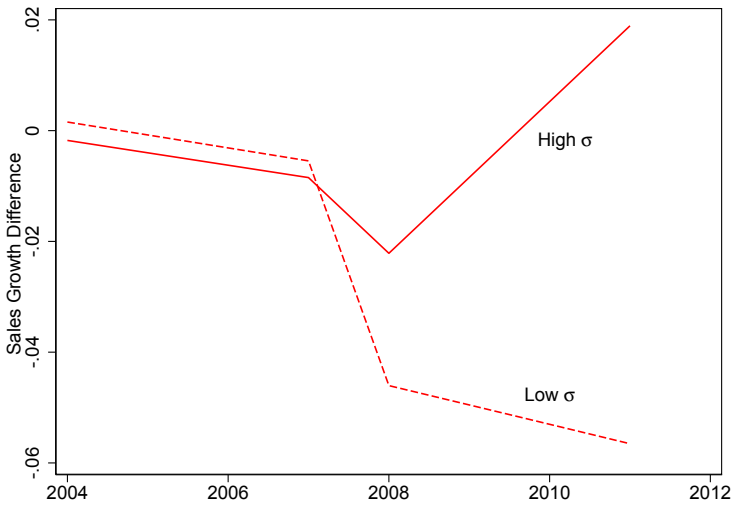
Notes: This figure plots the number of firms in the sample, by number of employees, for firms with between 40 and 60 employees. The sample period is 2006—2009. For visual convenience we mark the 50 employee mark with a vertical line. Data come from Orbis.

Figure 2: Credit constrained small versus large firms, before and after the crisis



Notes: This figure plots average sales growth for 2004, 2007, 2008, and 2011, for affected firms with less than 50 employees versus affected firms with more than 50 employees. Data come from Orbis.

Figure 3: Credit constrained small versus large firms, before and after the crisis, in high- versus low- σ industries



Notes: This figure plots the difference in average sales growth between affected firms with less than 50 employees and affected firms with more than 50 employees for 2004, 2007, 2008, and 2011, for sectors in the bottom quartile versus sectors in the top quartile in terms of the elasticity of substitution between capital and labor. Data come from Orbis.

Table 1. Summary statistics

Variable	#	Mean	Median	St. dev.	Min	Max
Firm-level						
Sales growth	215,480	-0.026	-0.027	0.330	-1	1
Employment growth	203,638	-0.007	0.000	0.191	-0.999	0.999
Investment growth	206,798	-0.053	-0.053	0.257	-1	1
<50 employees	215,480	0.894	1	0.308	0	1
<10 employees	215,480	0.436	0	0.496	0	1
Employment	215,480	35.247	10.500	412.953	1	66,124.5
Age	215,480	16.048	15	10.339	1	141
Sales (mln.)	215,480	9.554	0.099	49.548	1.101	2,289.005
Cash flow / Assets	215,480	0.068	0.060	0.096	-1.608	0.991
Net worth / Assets	215,480	0.321	0.301	0.323	-9.529	1
Assets (mln.)	215,480	9.437	0.072	35.807	1.751	188.694
Debt / Assets	215,480	0.690	0.711	0.293	0	6.368
Employee compensation	215,480	29,404.07	26,612.70	12,974.71	1	100,000
Firm-bank level						
Shock	215,480	0.264	0	0.441	0	1
Capital ratio	215,480	11.65	10.87	2.93	8.20	28.06
Industry-level						
Sigma	215,480	1.046	0.860	0.496	0.360	1.960
External dependence	215,480	0.129	0.100	0.265	-0.960	0.670

Note: The Table summarizes the variables used in the empirical tests. The sample period is 2006—2009. Only firms that report a credit association with at least one bank are included. 'Sales growth' denotes the log difference in the firm's total sales between this period and the previous one. 'Employment growth' denotes the log difference in the firm's total employment between this period and the previous one. 'Investment growth' denotes the log difference in the firm's total tangible capital between this period and the previous one. 'Shock' is a dummy variable equal to one in 2008 and in 2009 and if the firm has a credit association with at least one bank which required public assistance during the financial crisis. '<50 employees' is a dummy variable equal to one if the firm had fewer than 50 employees before the financial crisis. '<10 employees' is a dummy variable equal to one if the firm had fewer than 10 employees before the financial crisis. 'Employment' denotes the number of the firm's employees. 'Age' denotes the firm's age in years. 'Cash flow / Assets' denotes the ratio of the firm's cash flow to the firm's total assets, 1-period lagged. 'Sales (mln.)' denotes the firm's total sales, 1-period lagged, in mln. euro. 'Net worth / Assets' denotes the ratio of the firm's net worth, calculated as the difference between total assets and total liabilities, to the firm's total assets, 1-period lagged. 'Assets' denotes the firm's total tangible assets, 1-period lagged, in mln. euro. 'Debt / Assets' denotes the ratio of the firm's total debt to the firm's total assets. 'Employee compensation' denotes the firms' total compensation bill (including direct and non-direct compensation), divided by the firm's number of employees. 'Shock' is a dummy variable equal to one if the firm has a credit relationship with a bank that received government assistance during the financial crisis. 'Capital ratio' is the bank's ratio of capital to total assets. 'Sigma' denotes the sector's technological elasticity of substitution between labor and capital; see Section 3 for a description of how sigma is calculated. 'External dependence' denotes the sector's technological dependence on external finance, using the calculations in Duygan-Bump, Levkov, and Montoriol-Garriga (2015).

Table 2. Industry benchmarks

Industry	NACE codes	Sigma
Food, beverages, and tobacco	10—12	1.59
Textiles, textile, leather, and footwear	13—15	0.92
Wood and products of wood and cork	16, 31	0.69
Pulp, paper, printing, and publishing	17—18	1.80
Coke, refined petroleum, and nuclear fuel	19	0.36
Chemicals and chemical products	20—21	0.90
Rubber and plastic	22	0.57
Other non-metallic mineral products	23	0.41
Basic metals and fabricated metal products	24—25	0.42
Electrical, electronic, and optical equipment	26—27	0.79
Machinery, not else specified	28	0.54
Transportation equipment	29—30	0.64
Manufacturing, not else specified	32—33	0.66
Electricity, gas, and water supply	35—39	1.26
Construction	41—43	1.96
Sale, maintenance, and repair of motor vehicles and motorcycles	45	0.72
Wholesale trade, except of motor vehicles and motorcycles	46	0.86
Retail trade, except of motor vehicles and motorcycles	47	0.75
Transportation and storage	49—53	0.92
Hotels and restaurants	55—56	0.57
Post and telecommunications	61	0.82
IT and other information services	62—63	0.66
Real estate, renting and business activities	68—82	0.78

Note: The Table summarizes the technological elasticities of substitution between labor and capital for the respective SIC 2-digit sectors. See Section 3 on a description of how sigma is calculated.

Table 3. Main results

	Sales growth						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Shock	-0.0077*	0.0118	-0.0053	0.0543***	0.0520***	0.0493***	0.0475***
	(0.0043)	(0.0083)	(0.0092)	(0.0139)	(0.0139)	(0.0143)	(0.0171)
Shock × <50 employees		-0.0224**		-0.0686***	-0.0662***	-0.0584***	-0.0554***
		(0.0100)		(0.0142)	(0.0140)	(0.0136)	(0.0155)
Shock × Sigma			0.0023	-0.0404***	-0.0387**	-0.0328**	-0.0442**
			(0.0088)	(0.0140)	(0.0140)	(0.0133)	(0.0170)
Shock × <50 employees × Sigma				0.0440***	0.0421***	0.0360***	0.0450***
				(0.0119)	(0.0114)	(0.0098)	(0.0119)
Employment					0.0002***	0.0005***	0.0000
					(0.0001)	(0.0001)	(0.0000)
Employment squared					-0.0000***	-0.0000***	-0.0000
					(0.0000)	(0.0000)	(0.0000)
Employment cubed					0.0000***	0.0000***	0.0000
					(0.0000)	(0.0000)	(0.0000)
Age						-0.0045	-0.0055
						(0.0148)	(0.0149)
Log (Sales)						-0.1616***	-0.1622***
						(0.0064)	(0.0064)
Cash flow / Assets						0.1626***	0.1614***
						(0.0313)	(0.0316)
Net worth / Assets						-0.0435**	-0.0422**
						(0.0206)	(0.0206)
Debt / Assets						-0.0047	-0.0028
						(0.0290)	(0.0287)
Employee compensation						0.0005**	0.0005**
						(0.0002)	(0.0002)
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector × Period FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region × Period FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Employment pol × Shock	No	No	No	No	No	No	Yes
Employment pol × Sigma	No	No	No	No	No	No	Yes
Employment pol × Shock × Sigma	No	No	No	No	No	No	Yes
Observations	200,426	200,426	200,426	200,426	200,426	200,426	200,426
R-squared	0.60	0.60	0.60	0.60	0.60	0.62	0.62

Notes: The Table reports the point estimates from OLS regressions where the dependent variable is the firm's annual sales growth. 'Shock' is a dummy variable equal to one if the firm has a credit relationship with a bank that received government assistance during the financial crisis. '<50 employees' is a dummy variable equal to one if the firm has less than 50 employees. 'Sigma' is the sector's technological elasticity of substitution between labor and capital, calculated using KLEMs US data over the period 1947—2010. 'Employment' denotes the number of the firm's employees. 'Age' is the firm's age in years. 'Log (Sales)' is the logarithm of the firm's one-period-lagged total sales. 'Cash flow / Assets' is the ratio the firm's one-period-lagged cash flow to the firm's one-period-lagged total assets. 'Net worth / Assets' is the ratio of the firm's net worth, calculated as the difference between total assets and total liabilities, to the firm's total assets. 'Debt / Assets' denotes the ratio of the firm's total debt to the firm's total assets. 'Employee compensation' denotes the firms' total compensation bill (including direct and non-direct compensation), divided by the firm's number of employees. In all regressions, only firms with at least one observation before and at least one observation after 2008 are included. All regressions include fixed effects as specified. Column (7) includes interactions of 'Employment', 'Employment squared', and 'Employment cubed' with 'Shock', 'Sigma', and the interaction thereof. The sample period is 2006—2009. Standard errors clustered at the sector-period level are reported in parentheses where ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent statistical level, respectively.

Table 4. Empirical channels

	Employment growth	Investment growth
	(1)	(2)
Shock	-0.0166**	-0.0023
	(0.0082)	(0.0081)
Shock × <50 employees	0.0111	0.0044
	(0.0071)	(0.0070)
Shock × Sigma	-0.0106	-0.0012
	(0.0080)	(0.0082)
Shock × <50 employees × Sigma	0.0139**	-0.0038
	(0.0079)	(0.0057)
Firm controls and interactions	Yes	Yes
Firm FEs	Yes	Yes
Sector × Period FEs	Yes	Yes
Region × Period FEs	Yes	Yes
Observations	189,236	191,630
R-squared	0.54	0.59

Notes: The Table reports the point estimates from OLS regressions where the dependent variable is the firm's annual employment growth (column (1)) and the firm's annual investment growth (column (2)). 'Shock' is a dummy variable equal to one if the firm has a credit relationship with a bank that received government assistance during the financial crisis, and the firm is observed after 2008. '<50 employees' is a dummy variable equal to one if the firm has less than 50 employees. 'Sigma' is the sector's technological elasticity of substitution between labor and capital, calculated using KLEMs US data over the period 1947—2010. All firm controls, fixed effects, and interactions from Table 3, column (7) are also included in the regression. In all regressions, only firms with at least one observation before and at least one observation after 2008 are included. The sample period is 2006—2009. Standard errors clustered at the sector-period level are reported in parentheses where ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent statistical level, respectively.

Table 5. Employment protection, firm-size, or firm-quality effect?

	Sales growth		
	(1)	(2)	(3)
Shock	0.6788***	0.0423**	0.6863***
	(0.2051)	(0.0178)	(0.2006)
Shock × <50 employees	-0.1516***	-0.0587***	-0.1550***
	(0.0335)	(0.0136)	(0.0338)
Shock × Sigma	-0.3623*	-0.0275	-0.3578**
	(0.1951)	(0.0160)	(0.1849)
Shock × <50 employees × Sigma	0.0888***	0.0364***	0.0899***
	(0.0270)	(0.0099)	(0.0268)
Shock × Employment	-0.0000		-0.0000
	(0.0000)		(0.0000)
Shock × <50 employees × Employment	0.0000		0.0000
	(0.0000)		(0.0000)
Shock × Log (Assets)	-0.0390***		-0.0405***
	(0.0129)		(0.0130)
Shock × <50 employees × Log (Assets)	0.0202		0.0205
	(0.0126)		(0.0124)
Shock × Cash flow / Assets		0.1583*	0.1536*
		(0.0904)	(0.0856)
Shock × <50 employees × Cash flow / Assets		-0.1909*	-0.1910*
		(0.1118)	(0.1018)
Shock × Net worth / Assets		-0.0104	0.0163
		(0.0123)	(0.0136)
Shock × <50 employees × Net worth / Assets		0.0220**	0.0093
		(0.0101)	(0.0080)
Firm controls and interactions	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes
Sector × Period FEs	Yes	Yes	Yes
Region × Period FEs	Yes	Yes	Yes
Observations	200,426	200,426	200,426
R-squared	0.62	0.62	0.62

Notes: The Table reports the point estimates from OLS regressions where the dependent variable is the firm's annual sales growth. 'Shock' is a dummy variable equal to one if the firm has a credit relationship with a bank that received government assistance during the financial crisis. '<50 employees' is a dummy variable equal to one if the firm has less than 50 employees. 'Sigma' is the sector's technological elasticity of substitution between labor and capital, calculated using KLEMs US data over the period 1947—2010. 'Log (Sales)' is the logarithm of the firm's one-period-lagged total sales. 'Cash flow / Assets' is the ratio the firm's one-period-lagged cash flow to the firm's one-period-lagged total assets. 'Net worth / Assets' is the ratio of the firm's net worth, calculated as the difference between total assets and total liabilities, to the firm's total assets. All firm controls, fixed effects, and interactions from Table 3, column (7) are also included in the regression. In all regressions, only firms with at least one observation before and at least one observation after 2008 are included. The sample period is 2006—2009. Standard errors clustered at the sector-period level are reported in parentheses where ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent statistical level, respectively.

Table 6. Falsification tests

	Sales growth		
	Spain		Germany
	2004—2007	Cut-off at 10	Cut-off at 50
	(1)	(2)	(3)
Shock	-0.0152	-0.0051	0.0093
	(0.0131)	(0.0121)	(0.0093)
Shock × <50 employees	0.0100		0.0095
	(0.0141)		(0.0140)
Shock × Sigma	0.0165	-0.0098	-0.0053
	(0.0096)	(0.0088)	(0.0106)
Shock × <50 employees × Sigma	-0.0033		-0.0097
	(0.0080)		(0.0117)
Shock × <10 employees		0.0021	
		(0.0161)	
Shock × <10 employees × Sigma		0.0278	
		(0.0158)	
Firm controls and interactions	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes
Sector × Period FEs	Yes	Yes	Yes
Region × Period FEs	Yes	Yes	Yes
Observations	193,946	200,426	86,230
R-squared	0.59	0.62	0.38

Notes: The Table reports the point estimates from OLS regressions where the dependent variable is the firm's annual sales growth. 'Shock' is a dummy variable equal to one if the firm has a credit relationship with a bank that received government assistance during the financial crisis. '<50 employees' is a dummy variable equal to one if the firm has less than 50 employees. '<10 employees' is a dummy variable equal to one if the firm has less than 10 employees. 'Sigma' is the sector's technological elasticity of substitution between labor and capital, calculated using KLEMs US data over the period 1947—2010. All firm controls, fixed effects, and interactions from Table 3, column (7) are also included in the regression. In all regressions, only firms with at least one observation before and at least one observation after 2008 are included. The sample period is 2004—2007 (column (1)) and 2006—2009 (column (2)), and 2007—2010 (column (3)). In column (3), the test is performed on a sample of German firms. Standard errors clustered at the sector-period level are reported in parentheses where ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent statistical level, respectively.

Table 7. Alternative empirical proxies and model robustness

	Sales growth						
	Sigma 1970–2007	Sigma dummy	Sigma Spain	External depen-dence	Capital shock	Un-collapsed data	Region × sector trends
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Shock	0.0280***	0.0243***	0.0377*	0.0456***	0.0491*	0.0443***	0.458***
	(0.0069)	(0.0067)	(0.0244)	(0.0146)	(0.0284)	(0.0127)	(0.147)
Shock × <50 employees	-0.0338***	-0.0312***	-0.0460**	-0.0558***	-0.0716**	-0.0526***	-0.0605***
	(0.0086)	(0.0082)	(0.0239)	(0.0150)	(0.0301)	(0.0127)	(0.0132)
Shock × Sigma	-0.0067***	-0.0333**	-0.0326*	-0.0267**	-0.0256	-0.0183**	-0.0302**
	(0.0023)	(0.0162)	(0.0268)	(0.0131)	(0.0213)	(0.0093)	(0.0142)
Shock × <50 employees × Sigma	0.0067***	0.0381***	0.0373*	0.0318***	0.0496**	0.0189**	0.0382***
	(0.0015)	(0.0132)	(0.0252)	(0.0124)	(0.0246)	(0.0084)	(0.0104)
Shock × External dependence				-0.0231			
				(0.0255)			
Shock × <50 employees × External dependence				0.0168			
				(0.0260)			
Firm controls and interactions	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector × Period FEs	Yes	Yes	Yes	Yes	Yes	No	No
Region × Period FEs	Yes	Yes	Yes	Yes	Yes	No	No
Sector × Year FEs	No	No	No	No	No	Yes	No
Region × Year FEs	No	No	No	No	No	Yes	No
Sector × Region × Period FEs	No	No	No	No	No	No	Yes
Observations	200,426	200,426	110,842	200,426	55,520	391,281	199,962
R-squared	0.62	0.62	0.61	0.62	0.63	0.60	0.63

Notes: The Table reports the point estimates from OLS regressions where the dependent variable is the firm's annual sales growth. 'Shock' is a dummy variable equal to one if the firm has a credit relationship with a bank that received government assistance during the financial crisis (columns (1)—(4) and columns (6)-(7)) and a dummy variable equal to one if the firm has a credit relationship with a bank with a capital ratio of less than 10% before 2008 (column (5)). '<50 employees' is a dummy variable equal to one if the firm has less than 50 employees. 'Sigma' is the sector's technological elasticity of substitution between labor and capital. All firm controls, fixed effects, and interactions from Table 3, column (7) are also included in the regression. In all regressions, only firms with at least one observation before and at least one observation after 2008 are included. In column (1), sigma is calculated using KLEMs US data over the period 1970—2007. In column (2), sigma is first calculated using KLEMs US data over the period 1947—2010, and then replaced with a dummy equal to one if sigma is more than 1, and to zero otherwise. In column (3), data on Spain is used to calculate sigma. In columns (4)–(7), sigma is calculated using KLEMs US data over the period 1947—2010. In column (6), annual data (as opposed to data collapsed into one observation per period) are used. The sample period is 2006—2009. Standard errors clustered at the sector-period level (columns (1)—(5) and column (7)) and at the sector-year level (column (6)) are reported in parentheses where ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent statistical level, respectively.

Table 8. Sample robustness

	Sales growth							
	40—85 workers	43—64 workers	2005—2010	2004—2011	Excluding construc-tion	Single-bank firms	Matched sample	Selection
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Shock	0.0279	-0.0206	0.0405***	0.0454***	0.0562***	0.0469*	0.0365*	0.0520***
	(0.0225)	(0.0248)	(0.0131)	(0.0099)	(0.0163)	(0.0254)	(0.0219)	(0.0138)
Shock × <50 employees	-0.0499**	-0.0422	-0.0439***	-0.0480***	-0.0613***	-0.0496**	-0.0442**	-0.0609***
	(0.0211)	(0.0367)	(0.0116)	(0.0077)	(0.0150)	(0.0241)	(0.0218)	(0.0137)
Shock × Sigma	-0.0268*	0.0267	-0.0380***	-0.0455***	-0.0391**	-0.0339	-0.0407**	-0.0359***
	(0.0144)	(0.0161)	(0.0127)	(0.0089)	(0.0171)	(0.0209)	(0.0207)	(0.0129)
Shock × <50 employees × Sigma	0.0335**	0.0457**	0.0384***	0.0444***	0.0361***	0.0302*	0.0424**	0.0386***
	(0.0142)	(0.0227)	(0.0094)	(0.0063)	(0.0137)	(0.0201)	(0.0181)	(0.0105)
Firm controls and interactions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector × Period FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region × Period FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,944	5,326	210,792	211,532	184,186	141,162	105,690	198,186
R-squared	0.68	0.68	0.62	0.64	0.63	0.62	0.62	0.62

Notes: The Table reports the point estimates from OLS regressions where the dependent variable is the firm's annual sales growth. 'Shock' is a dummy variable equal to one if the firm has a credit relationship with a bank that received government assistance during the financial crisis. '<50 employees' is a dummy variable equal to one if the firm has less than 50 employees. 'Sigma' is the sector's technological elasticity of substitution between labor and capital, calculated using KLEMs US data over the period 1947—2010. All firm controls, fixed effects, and interactions from Table 3, column (7) are also included in the regression. In all regressions, only firms with at least one observation before and at least one observation after 2008 are included. The sample period is 2006—2009 (columns (1)–(2) and (5)—(8)), 2005—2010 (column (3)), and 2004—2011 (column (4)). In column (1), only firms with between 40 and 85 employees (4% of the sample on each side of 50) are included. In column (2), only firms with between 43 and 64 employees (2% of the sample on each side of 50) are included. In column (5), firms in the construction sector are excluded. In column (6), only affected firms with a credit relationship with a single bank are included in the regressions. In column (7), the treatment and control groups are chosen based on a propensity-score matching procedure. In column (8), firms with 48 and 49 employees are excluded. Standard errors clustered at the sector-period level are reported in parentheses where ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent statistical level, respectively.

Table 9. Distinguishing across pre-crisis firm growth rates

	Sales growth			
	Sales growth in 2006-07		Productivity growth in 2006-07	
	High	Low	High	Low
	(1)	(2)	(3)	(4)
Shock	0.0313	0.0380**	0.0683**	0.0333**
	(0.0237)	(0.0148)	(0.0258)	(0.0161)
Shock × <50 employees	-0.0547***	-0.0345**	-0.0708***	-0.0419***
	(0.0206)	(0.0146)	(0.0259)	(0.0130)
Shock × Sigma	-0.0090	-0.0272*	-0.0421***	-0.0204
	(0.0252)	(0.0141)	(0.0152)	(0.0158)
Shock × <50 employees × Sigma	0.0280*	0.0207	0.0321**	0.0246**
	(0.0190)	(0.0144)	(0.0155)	(0.0111)
Firm controls and interactions	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes
Sector × Period FEs	Yes	Yes	Yes	Yes
Region × Period FEs	Yes	Yes	Yes	Yes
Observations	60,026	120,922	55,070	118,202
R-squared	0.74	0.56	0.64	0.62

Notes: The Table reports the point estimates from OLS regressions where the dependent variable is the firm's annual sales growth. 'Shock' is a dummy variable equal to one if the firm has a credit relationship with a bank that received government assistance during the financial crisis, and the firm is observed after 2008. '<50 employees' is a dummy variable equal to one if the firm has less than 50 employees. 'Sigma' is the sector's technological elasticity of substitution between labor and capital, calculated using KLEMs US data over the period 1947—2010. All firm controls, fixed effects, and interactions from Table 3, column (7) are also included in the regression. In all regressions, only firms with at least one observation before and at least one observation after 2008 are included. The sample period is 2006—2009. In column (1), only firms in the top tertile of the distribution of sales growth in 2006-07 are included. In column (2), only firms outside the top tertile of the distribution of sales growth in 2006-07 are included. In column (3), only firms in the top tertile of the distribution of sales growth in 2006-07 07 are included. In column (4), only firms outside the top tertile of the distribution of sales growth in 2006-07 are included. Standard errors clustered at the sector-period level are reported in parentheses where ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent statistical level, respectively.

Table 10. Credit-constrained firms only

	Sales growth	Employment growth	Investment growth
	(1)	(2)	(3)
<50 employees	-0.0416***	0.0235***	0.0212**
	(0.0112)	(0.0055)	(0.0095)
<50 employees × Sigma	0.0219***	0.0144**	-0.0104**
	(0.0080)	(0.0070)	(0.0051)
Firm controls and interactions	Yes	Yes	Yes
Sector × Period FEs	Yes	Yes	Yes
Region × Period FEs	Yes	Yes	Yes
Observations	53,608	52,408	52,570
R-squared	0.05	0.05	0.04

Notes: The Table reports the point estimates from OLS regressions where the dependent variable is the firm's annual sales growth (column (1)), the firm's annual employment growth (column (2)) and the firm's annual investment growth (column (3)). '<50 employees' is a dummy variable equal to one if the firm has less than 50 employees. 'Sigma' is the sector's technological elasticity of substitution between labor and capital, calculated using KLEMs US data over the period 1947—2010. All firm controls, fixed effects, and interactions from Table 3, column (7) are also included in the regression. In all regressions, only firms with at least one observation before and at least one observation after 2008 are included. All regressions are run on the sub-sample of firm with a credit relationship with a bank that received government assistance during the financial crisis. The sample period is 2008 and 2009. Standard errors clustered at the sector-period level are reported in parentheses where ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent statistical level, respectively.

Table 11. External validity

	Spain 2004—2007, weak vs. strong banks	Germany 2007–2010, cut-off at 10
	(1)	(2)
Weak bank	0.1221	
	(0.0965)	
Weak bank × <50 employees	-0.1518	
	(0.1195)	
Weak bank × Sigma	-0.0756	
	(0.0682)	
Weak bank × <50 employees × Sigma	0.1332*	
	(0.0782)	
Shock		0.0124
		(0.0080)
Shock × <10 employees		-0.0091
		(0.0091)
Shock × <10 employees		-0.0312
		(0.0282)
Shock × <10 employees × Sigma		0.0422*
		(0.0261)
Firm controls and interactions	Yes	Yes
Firm FEs	Yes	Yes
Sector × Period FEs	Yes	Yes
Region × Period FEs	Yes	Yes
Observations	62,088	86,230
R-squared	0.60	0.38

Notes: The Table reports the point estimates from OLS regressions where the dependent variable is the firm's annual sales growth. 'Weak bank' is a dummy variable equal to one if the firm had a credit relationship with a bank whose Tier 1 capital declined between 2004/05 and 2006/07. 'Shock' is a dummy variable equal to one if the firm has a credit relationship with a bank that received government assistance during the financial crisis. '<50 employees' is a dummy variable equal to one if the firm has less than 50 employees. '<10 employees' is a dummy variable equal to one if the firm has less than 10 employees. 'Sigma' is the sector's technological elasticity of substitution between labor and capital, calculated using KLEMs US data over the period 1947—2010. All firm controls, fixed effects, and interactions from Table 3, column (7) are also included in the regression. In all regressions, only firms with at least one observation before and at least one observation after 2008 are included. The sample period is 2004—2007 (column (1)), and 2007—2010 (column (2)). In column (1), the test is performed on a sample of Spanish firms. In column (2), the test is performed on a sample of German firms. Standard errors clustered at the sector-period level are reported in parentheses where ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent statistical level, respectively.

Appendices

Credit Shocks, Employment Protection, and Growth:

Firm-level Evidence from Spain

Luc Laeven , Peter McAdam and Alexander Popov

A A Stylized Model

Assume firm f faces demand function $Y_{ft} = \left(\frac{P_{ft}}{P_t}\right)^{-\varepsilon} Y_t$, with Y_t and P_t being respectively the aggregate output and aggregate price level. The firm maximizes the discounted sum of profits, subject to its production constraints,

$$\max \sum_{\tau=t}^{\infty} \prod_{j=0}^{\tau-t} R_j \left\{ \Pi_\tau + P_\tau \Lambda_{f\tau}^{Y} \left[(1+\mu) F\left(K_{f\tau}, L_{f\tau}; \sigma\right) - \chi_{f\tau} - Y_{f\tau} \right] \right\} \tag{A.1}$$

where the firm's profit function and CES (Constant Elasticity of Substitution) production function are, respectively,

$$\Pi_t = P_t \left\{ Y_{ft}^{1-\frac{1}{\varepsilon}} Y_t^{\frac{1}{\varepsilon}} - \frac{W_t}{P_t} L_{ft} - \Omega\left(L_{ft}, L_{ft-1}\right) - I_{ft} - \left(1 + i_{ft-1}\right) \frac{P_{t-1}}{P_t} b_{ft-1} + b_{ft} \right\} \tag{A.2}$$

$$Y_{ft} = F\left(K_{ft}, N_{ft}; \alpha_f, \sigma\right) = \left[\alpha_f K_{ft}^{\frac{\sigma-1}{\sigma}} + \left(1 - \alpha_f\right) L_{ft}^{\frac{\sigma-1}{\sigma}} \right]^{\frac{\sigma}{\sigma-1}} \tag{A.3}$$

where $\chi_f > 0$ is the firm's fixed cost of production, $1 + \mu = \frac{\varepsilon}{\varepsilon - 1} > 1$ represents the mark-up, $\frac{W}{P} L$ captures employment costs, $\Omega\left(\bullet\right)$ represents generalized adjustment costs to labor which (consistent with the regulatory environment studied) bind only if the firm is of sufficient size,[24] investment is given by

$$I_{ft} = \Delta K_{ft} + \delta_f K_{ft-1} \tag{A.4}$$

(with depreciation rate $\delta_f \in (0,1)$), i denotes the nominal interest rate, and b_f denotes a one-period real corporate bond reflecting the possibility of external finance for the firm. $\alpha_f \in (0,1)$ is a distribution parameter.

[24] Note for expositional simplicity, we omit capital adjustment costs; although, their presence would tend to strengthen our argument since large firms tend to be more capital intensive and thus relatively less flexible in their readjustment behavior.

The general form for the elasticity of substitution between capital and labor is given by

$$\sigma \in [0, \infty) = \left. \frac{d \log (K/L)}{d \log (F_L / F_K)} \right|_Y . \tag{A.5}$$

This measures the percentage change in factor proportions due to a unit change in the marginal rate of technical substitution (along a given isoquant). The CES function nests Leontief, Cobb-Douglas, and a linear production function respectively when $\sigma \to 0, 1, \infty$. Indeed, an advantage of the CES function is that by allowing for an unconstrained σ, it facilitates a better fit to the data. For instance a well-known property of the Cobb-Douglas case is that factor income shares are constant (movements in factor prices being exactly offset by commensurate movements in factor volumes). However this stability tends to be highly counterfactual at the sectoral level (plots of sectoral income shares available on request). Typically factor prices are assumed to reflect the marginal productivities (F_K, F_L).

The first-order conditions are:

$$Y_f : \Lambda^Y_{ft} = \frac{P_{ft}}{(1+\mu) P_t} \tag{A.6}$$

$$L_f : \frac{P_{ft}}{P_t} F_{L_{ft}} = \frac{W_t}{P_t} + \frac{\partial \Omega (L_{ft}, L_{ft-1})}{\partial L_{ft}} + \mathbb{E}_t \left\{ R_{ft+1} \frac{P_{t+1}}{P_t} \frac{\partial \Omega (L_{ft+1}, L_{ft})}{\partial L_{ft}} \right\} \tag{A.7}$$

$$K_f : \frac{P_{ft}}{P_t} F_{K_{ft}} = 1 - \mathbb{E}_t \left\{ R_{ft+1} \frac{P_{t+1}}{P_t} (1 - \delta_f) \right\} \tag{A.8}$$

$$b_f : \mathbb{E}_t R_{ft+1} = \frac{1}{1 + i_{ft}} \tag{A.9}$$

$$\Lambda^Y_f : Y_{ft} = (1+\mu) F (K_{ft}, N_{ft}; \alpha_f, \sigma) - \chi_{ft} \tag{A.10}$$

where $1 + \mu = \frac{\varepsilon}{\varepsilon - 1}$ represents the equilibrium mark-up of prices over costs. Condition (A.6) defines the shadow price (or marginal cost) of output. Conditions (A.7) and (A.8) define dynamic demands for the number of employees and capital, (A.9) defines the discount factor and (A.10) retrieves the production function.

Given (A.9), the inverse of gross real interest rate is,

$$(1 + r_{ft})^{-1} = \mathbb{E}_t \left\{ R_{ft+1} \frac{P_{t+1}}{P_t} \right\} \equiv \frac{1 + \mathbb{E}_t \pi_{t+1}}{1 + i_{ft}} \tag{A.11}$$

where π denotes inflation, and $\mathbb{E}_t$ is the expectations operator. Conditions (A.9) and (A.8) solve for the firm's real user cost of capital, r^K_{ft} ,

$$r^K_{ft} = \frac{r_{ft} + \delta_f}{1 + r_{ft}} \tag{A.12}$$

The firm's optimal static capital intensity is then given by,

$$\frac{K_{ft}}{L_{ft}} = \left(\frac{r_{ft}^K}{W_t} \frac{1-\alpha_f}{\alpha_f} \right)^{-\sigma} \tag{A.13}$$

Thus $\partial \frac{K_f}{L_f} / \partial \frac{r_f^K}{w_f}$ is a negative function of σ. In other words, firms faced with relatively higher capital financing costs substitute into labor (the more so the higher is σ).[25] By contrast, firms with limited substitution possibilities will find it hard to substitute out of the expensive factor – and may, in extremes, saddled with these higher costs, exit the market altogether if fixed costs χ_f are not met.

The sum of this substitution effect plus a 'scale effect' gives the full cross-price effect of a rise in the price of one factor upon the demand of the other:

$$\frac{\partial L_f}{\partial r_f^K} = \underbrace{\frac{\partial L_f^c}{\partial r_f^K}}_{+} + \underbrace{\frac{\partial L_f^c}{\partial Y_f}}_{+} \cdot \underbrace{\frac{\partial Y_f}{\partial r_f^K}}_{-} \tag{A.14}$$

$$= (1-s_f)\,\sigma + (1-s_f)\,\varepsilon \tag{A.15}$$

where L_f^c is the conditional labor demand (i.e., conditional on output). Note that – in line with Chirinko and Mallick's (2011) correction to Hicks' (1932) original formula – the weight of the cost shares are a function of the underlying factor substitution elasticity, where, by way of illustration,

$$1 - s_f = \alpha_f^\sigma \left(\frac{r_f^K}{P_f} \right)^{1-\sigma} \in (0,1) \tag{A.16}$$

is capital's share of total cost for firm f.[26]

By definition (excluding the Leontief case), the substitution effect is positive (higher user costs expand labor demand). Note, the substitution elasticity is scaled by the capital cost share. This is intuitive: if capital's cost share is low, a change in its price should not induce a large change in labor demand. Thus, labor (cost) intensive firms will, ceteris paribus, be less affected by a rise in capital funding costs.

Note further that the price of the firm's product P_f in (A.16) will also depend on σ. This again is intuitive: the extent to which a firm's costs pass onto prices (and thus demand for their product) is a function of how it can redistribute factor intensity in response to a change in factor costs.[27]

[25] Note, this relies on observable market rates. If a firm is denied credit to finance capital expenditures, however, its borrowing rate is implicitly infinity.

[26] They demonstrate that the original Hicks' formulation implicitly assumes Cobb Douglas, implying $1 - s_f = \alpha_f \perp \sigma$.

[27] It is straightforward to show that production function (A.3) has the unit cost function: $c(W_t, r_{ft}^K) = (\alpha_f^\sigma (r_{ft}^K)^{1-\sigma} + (1-\alpha_f)^\sigma (W_t)^{1-\sigma})^{\frac{1}{1-\sigma}}$. From this we can see the pass through of factor price changes into costs, and their relation

The scale effect, meanwhile, is the product of the cost share and the demand elasticity: $(1 - s_f)\,\varepsilon$ with $\partial Y_f / \partial r_f^K < 0$ (as costs rise from an increase in user costs, prices rise and demand falls for firm f's output reflecting the price elasticity of demand ε). The extent to which cost rises impact price and output falls (and thus factor demands) is scaled by $1 - s_f$. If $\sigma > 1$ then an increase in the user cost will decrease labor's cost share which allows the firm to continue to grow and mitigates the contraction in labor demand (from the credit shock).

Accordingly, we observe the critical role for factor substitution in absorbing changes in relative factor prices. For instance, firms with low substitution possibilities and high capital cost shares will be adversely affected by punitive capital financing costs.[28]

A.1 Regulatory Effects on Labor Demand

Thus far we have considered the extent to which firms *can* technologically substitute factors as relative prices changes (and how costs and demand then adjust). An additional germane consideration is how the prevailing regulatory framework affects those choices. A simple way to accommodate that is to introduce an extensive margin cost for labor contingent upon firm size. The effect can be seen through the optimality conditions through which labor demand is derived:

$$L : \frac{P_{ft}}{P_t} F_{L_{ft}} = \frac{W_t}{P_t} + \mathbb{1}_t \left(\frac{\partial \Omega \left(L_{ft}, L_{ft-1} \right)}{\partial L_{ft}} + \mathbb{E}_t \left\{ R_{ft+1} \frac{P_{t+1}}{P_t} \frac{\partial \Omega \left(L_{ft+1}, L_{ft} \right)}{\partial L_{ft}} \right\} \right) \tag{A.17}$$

where dummy $\mathbb{1} = 1$ if $L_t \geq \bar{L}$, where $\bar{L} = 50$ is the regulated employment size threshold.

For a given level of employment, the presence of adjustment costs to labor will dampen the impact of cost and demand changes on employment. This will make firms less able to exploit inherent technological substitution characteristics, and less able to contain cost increases from tighter credit.

And it is these main two margins – factor substitutability and size-specific regulatory burdens – that informs the remainder of our analysis of how credit shocks propagate within the economy and how institutional features may interact with those margins.

to σ.

[28] Note the generality of our argument. Firms may seek external financing for many reasons beyond financing capital expenditures. However, in so far as some fraction of that borrowing at r_f^K is done to finance capital expenditure (new investment, capital replacement, upgrading), then our argument pertains – albeit to a higher or lesser degree depending on firm characteristics. Moreover, see Haskel and Westlake (2017) for a discussion of the rising importance of *intangible* capital on firms' production inputs.

Specifically, the following two testable predictions (hypotheses) follow directly from the above discussion.

$\mathcal{H}_1$ *Firm output will decline following a relative tightening in capital financing costs.*

$\mathcal{H}_2$ *The decline in firm output following an increase in the user cost of capital is less pronounced if capital and labor are highly substitutable, especially if labor is less costly due to lower (size-specific) regulatory burdens.*

B Production System Estimation

Following León-Ledesma et al. (2010) we estimate sectoral production characteristics using a normalized system of production function and factor returns with cross-equation parameter constraints.[29] Consider that real output Y for a given sector can be described by the 'normalized' CES production function,[30]

$$Y_t = \left[\alpha_z \left(a_t K_t\right)^{\frac{\sigma-1}{\sigma}} + \left(1-\alpha_z\right)\left(b_t L_t\right)^{\frac{\sigma-1}{\sigma}}\right]^{\frac{\sigma}{\sigma-1}} \tag{B.1}$$

where as before $\sigma \in [0, \infty)$ is the elasticity of substitution between the real capital stock K and the labor input L (recall (A.5)). Note, we now represent the equations with a t subscript indicating time. Distribution parameter $\alpha_z \in (0, 1)$ equals the capital income share at the point of normalization: $\alpha_z = 1 - \frac{w_z L_z}{Y_z}$, where w_z denotes the real wage rate at the normalization point (in our main text, we suppressed the 'z' subscript for simplicity). The terms a and b capture the level of technical progress associated to capital and labor, respectively (with $d \log a = \gamma_a$ being its rate of growth etc). As is standard in the literature we assume that the levels of factor augmentation are given by $J_t = e^{\gamma_{j,t}(t-t_0)}$ where t_0 is the arithmetic mean of the sample length and $J = a, b$.[31]

[29] León-Ledesma et al. (2010) use Monte-Carlo evidence to demonstrate the robustness of the system approach in comparison to single-equation approach (e.g., the estimation of the production function or one of the factor demands alone.

[30] Output, capital and labor are expressed in 'normalized' units, e.g., $Y_t = \mathbf{Y}_t/(\zeta \mathbf{Y}_z)$ where $\mathbf{Y}_t$ is the un-normalized series and $\mathbf{Y}_z$ its normali(z)ed value. Normalization essentially implies representing the production function and factor demands in consistent indexed number form. Expressed in this way, its parameters then have a direct economic and econometrically-identifiable interpretation. Otherwise they will be scale dependent and unrobust. Subscripts z denote the specific normalization points: geometric (arithmetic) averages for non-stationary (stationary) variables. See Klump, McAdam, and Willman (2011) for a survey of the normalization approach.

[31] Note, following Klump, McAdam, and Willman (2007) we also estimated all of the sector using a slightly more flexible Box-Cox function for the technical progress components. In some cases, this improved overall model fit but still identified elasticity values in the neighborhood of those estimated using the constant growth assumption.

Given this, the optimal labor and capital income shares are, respectively,

$$sh_{_{Lt}} = (1 - \alpha_z) \left(a_t \frac{L_t}{Y_t} \right)^{\frac{\sigma - 1}{\sigma}} \tag{B.2}$$

$$sh_{_{Kt}} = \alpha_z \left(b_t \frac{K_t}{Y_t} \right)^{\frac{\sigma - 1}{\sigma}} \tag{B.3}$$

Equations (A.3) – (B.3) constitute the non-linear stochastic system to be estimated.

In terms of KLEMs mnemonics, factors are taken from their volume services (LAB_QI and CAP_QI), output by Gross value added (volume indices, VA_QI), and the labor (capital) income share is labor (capital) compensation – LAB (CAP) – divided by gross value added VA.

Internet Appendices

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C Full Sectoral Estimates

We now show more detailed estimates of our core results for the chosen KLEMs US sectors. For estimation of the non-linear system of equations, we mainly used three different estimators: non-linear seemingly unrelated regression, feasible generalized non-linear least squares and the iterated feasible generalized non-linear least squares. These estimators account for cross-equation parameter restrictions as well as cross-correlated errors. Of the three, iterated feasible generalized non-linear least squares tends to be the one reported in the main text.

For additional robustness we also estimated separately the production function (A.3), individual factor demands (B.2, B.3), or the ratio of the two factor demands. In most cases these single equation approaches did not fit the data as well, but where feasible they provided a cross check on our main results. We also (for cross checking purposes) used two and three stage non-linear least square estimators (we used lags of output and capital and the labor input as instruments).[32]

C.1 presents the system parameters, as well as tests of Cobb Douglas ($\sigma = 1$), of Hicks neutrality ($\gamma_b = \gamma_a$) and the size of the technical bias ($\gamma_b - \gamma_a$). In most cases factor augmenting technical change is net labor saving; where $\widehat{\sigma}$ is estimated close to unity it becomes, as is well known in the literature, very difficult to separately identify separate capital and labor technical progress. In two cases, Hicks neutral was not rejected by the data, although imposing it did not materially affect the estimated elasticities; we therefore for space consideration show the unconstrained case.

[32]Details of all our estimation forms and results are available on request. Note $\widehat{\sigma} < 1|\sigma_0 > 1$ (likewise $\widehat{\sigma} > 1|\sigma_0 < 1$) constitutes especially strong evidence for the estimated σ given the discontinuity of the production function estimation around the unitary substitution elasticity region.

C.1 Robustness

In each of the non-linear cases, we systematically varied the initial parameter conditions to ensure the attainment of a global optimum (e.g., for the substitution elasticity, we use a grid of $\sigma_0 \in [0.2, 0.4, 0.8, 1.2, 1.6]$). For additional robustness we also estimated separately the production function (Eq. (A.3)), individual factor demands (B.2, B.3), or the ratio of the two factor demands. In most cases these single equation approaches did not fit the data as well, but where feasible they provided a cross check on our main results. We also used two and three stage non-linear least square estimators (we used lags of output and capital and the labor input as instruments).[33]

An example (for one particular sector) is given in Table C.2. In that case, there are some variations in $\widehat{\sigma}$ (although all significantly below one) with a labor augmenting growth rate of around 2% per year and a statistically zero growth rate in capital augmenting technical progress. The case IFGNLS $\forall \sigma_0$ is favored across the discriminatory metrics.

[33] Details of all our estimation forms and results are available on request. Note $\widehat{\sigma} < 1 | \sigma_0 > 1$ (likewise $\widehat{\sigma} > 1 | \sigma_0 < 1$) constitutes especially strong evidence for the estimated σ given the discontinuity of the production function estimation around the unitary substitution elasticity region.

Table C.1: Sectoral Production Parameters

	Sector (NACE Code)								
Parameter	24–25	41–43	19	26–27	35–39	10–12	28	32–33	29–30
ζ	0.996***	0.966***	1.006***	1.071***	1.053***	0.968***	0.981***	1.057***	1.034***
	{0.975::1.016}	{0.925::1.007}	{0.844::1.169}	{0.963::1.179}	{1.011::1.095}	{0.940::0.996}	{.936::1.027}	{1.004::1.064}	{1.043::1.118}
σ	0.417***	1.926***	0.364***	0.788***	1.264***	1.585***	0.542***	0.662***	0.642***
	{0.415::0.420}	{1.900::1.951}	{0.362::0.366}	{0.782::0.794}	{1.247::1.281}	{1.548::1.622}	{0.537::0.546}	{0.647::0.676}	{0.630::0.654}
γ_a	-0.023***	-0.006	0.013***	0.026***	-0.006***	0.010***	-0.016***	-0.037***	-0.019***
	{-0.025::-0.022}	{-0.014::0.002}	{0.007::0.019}	{0.018::0.035}	{-0.010::-0.003}	{0.007::0.013}	{-0.018::-0.014}	{-0.042::-0.033}	{-0.023::-0.016}
γ_b	0.010***	-0.010***	0.082***	0.117***	-0.006*	-0.006***	0.010***	0.045***	0.009***
	{0.008::0.011}	{-0.014::-0.007}	{0.074::0.090}	{0.102::0.132}	{-0.011::-0.000}	{-0.009::-0.003}	{0.008::0.013}	{0.043::0.047}	{0.006::0.012}
$\sigma = 1$	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
$\gamma_a = \gamma_b$	[0.000]	[0.363]	[0.000]	[0.000]	[0.890]	[0.000]	[0.000]	[0.000]	[0.000]
$\gamma_b - \gamma_a$	0.033	-0.004	0.069	0.091	0.000	-0.016	0.026	0.082	0.028
	23	61	17–18	68–82	47	22	13–15	49–53	46
ζ	1.000***	1.037***	0.984***	1.093***	1.015***	1.008***	1.027***	1.042***	1.000***
	{0.981::1.019}	{1.011::1.064}	{0.961::1.007}	{1.081::1.105}	{0.985::1.045}	{0.984::1.031}	{1.002::1.052}	{1.026::1.059}	{0.979::1.020}
σ	0.410***	0.823***	1.825***	0.775***	0.747***	0.574***	0.920***	0.919***	0.858***
	{0.407::0.413}	{0.793::0.853}	{1.803::1.848}	{0.755::0.794}	{0.733::0.761}	{0.571::0.577}	{0.891::0.950}	{0.905::0.934}	{0.849::0.866}
γ_a	-0.012***	-0.049***	-0.028***	0.029***	-0.058***	-0.002**	-0.091***	0.090***	0.012***
	{-0.013::-0.011}	{-0.058::-0.041}	{-0.030::-0.027}	{0.025::0.032}	{-0.068::-0.048}	{-0.004::-0.001}	{-0.129::-0.052}	{0.077::0.104}	{0.007::0.017}
γ_b	0.013***	0.076***	0.002*	-0.064***	0.030***	0.017***	0.060***	-0.017***	0.030***
	{0.012::0.014}	{0.069::0.082}	{0.000::0.004}	{-0.072::-0.056}	{0.028::0.033}	{0.015::0.018}	{0.050::0.069}	{-0.023::-0.012}	{0.027::0.034}
$\sigma = 1$	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
$\gamma_a = \gamma_b$	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
$\gamma_b - \gamma_a$	0.025	0.125	0.031	-0.093	0.088	0.019	0.151	-0.108	0.018

Notes: This table shows the full set of results for the US industries, associated fit metrics and the probability values associated to some parameter restriction tests. Symbols ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent statistical level, respectively. Numbers in braces refer to 95% confidence intervals and number is brackets refer to probability vales.

Table C.2: Illustrative Robustness: Sector Rubber & Plastic

	NLS					FGNLS					IFGNLS				
σ_0	0.2	0.4	0.8	1.2	1.6	0.2	0.4	0.8	1.2	1.6	0.2	0.4	0.8	1.2	1.6
ζ	1.002^{***}	1.002^{***}	1.002^{***}	1.002^{***}	1.002^{***}	1.014^{***}	1.014^{***}	1.014^{***}	1.014^{***}	1.014^{***}	1.008^{***}	1.008^{***}	1.008^{***}	1.008^{***}	1.008^{***}
	{0.978::1.026}	{0.978::1.026}	{0.978::1.026}	{0.978::1.026}	{0.978::1.026}	{0.990::1.037}	{0.990::1.037}	{0.990::1.037}	{0.990::1.037}	{0.990::1.037}	{0.984::1.031}	{0.984::1.031}	{0.984::1.031}	{0.984::1.031}	{0.984::1.031}
σ	0.874^{***}	0.874^{***}	0.874^{***}	0.874^{***}	0.874^{***}	0.824^{***}	0.824^{***}	0.824^{***}	0.824^{***}	0.824^{***}	0.574^{***}	0.574^{***}	0.574^{***}	0.574^{***}	0.574^{***}
	{0.806::0.942}	{0.806::0.942}	{0.806::0.942}	{0.806::0.942}	{0.806::0.942}	{0.817::0.832}	{0.817::0.832}	{0.817::0.832}	{0.817::0.832}	{0.817::0.832}	{0.571::0.577}	{0.571::0.577}	{0.571::0.577}	{0.571::0.577}	{0.571::0.577}
γ_a	-0.006	-0.006	-0.006	-0.006	-0.006	-0.004	-0.004	-0.004	-0.004	-0.004	$-0.002**$	$-0.002**$	$-0.002**$	$-0.002**$	$-0.002**$
	{-0.016::0.005}	{-0.016::0.005}	{-0.016::0.005}	{-0.016::0.005}	{-0.016::0.005}	{-0.010::0.002}	{-0.010::0.002}	{-0.010::0.002}	{-0.010::0.002}	{-0.010::0.002}	{-0.004::-0.001}	{-0.004::-0.001}	{-0.004::-0.001}	{-0.004::-0.001}	{-0.004::-0.001}
γ_b	0.019^{***}	0.019^{***}	0.019^{***}	0.019^{***}	0.019^{***}	0.018^{***}	0.018^{***}	0.018^{***}	0.018^{***}	0.018^{***}	0.017^{***}	0.017^{***}	0.017^{***}	0.017^{***}	0.017^{***}
	{0.013::0.025}	{0.013::0.025}	{0.013::0.025}	{0.013::0.025}	{0.013::0.025}	{0.014::0.022}	{0.014::0.022}	{0.014::0.022}	{0.014::0.022}	{0.014::0.022}	{0.015::0.018}	{0.015::0.018}	{0.015::0.018}	{0.015::0.018}	{0.015::0.018}
ll	341.154	341.154	341.154	341.154	341.154	343.33	343.33	343.33	343.33	343.33	352.567	352.567	352.567	352.567	352.567
aic	-674.309	-674.309	-674.309	-674.309	-674.309	-678.661	-678.661	-678.661	-678.661	-678.661	-697.133	-697.133	-697.133	-697.133	-697.133
bic	-665.673	-665.673	-665.673	-665.673	-665.673	-670.025	-670.025	-670.025	-670.025	-670.025	-688.498	-688.498	-688.498	-688.498	-688.498
$rmse(Y)$	0.096	0.096	0.096	0.096	0.096	0.097	0.097	0.097	0.097	0.097	0.096	0.096	0.096	0.096	0.096
$rmse(sh_L)$	0.057	0.057	0.057	0.057	0.057	0.059	0.059	0.059	0.059	0.059	0.092	0.092	0.092	0.092	0.092
$rmse(sh_K)$	0.092	0.092	0.092	0.092	0.092	0.091	0.091	0.091	0.091	0.091	0.106	0.106	0.106	0.106	0.106
$\sigma = 1$	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
$\gamma_a = \gamma_b$	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
$\gamma_b - \gamma_a$	0.024	0.024	0.024	0.024	0.024	0.022	0.022	0.022	0.022	0.022	0.019	0.019	0.019	0.019	0.019

Notes: The terms ll, aic and bic refer to the log likelihood, the Akaike and Bayesian information criteria, respectively, whilst $rmse(Y)$, $rmse(sh_L)$ and $rmse(sh_L)$ refer, respectively, to the root mean square error of the fitted values of equations (A.3)-(B.3). σ_0 is the starting value assigned to the non-linear solution algorithm. Symbols ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent statistical level, respectively. Numbers in braces refer to 95% confidence intervals and number is brackets refer to probability vales.

D Additional Tables

Table D.1: Credit Institutions

Credit Institution	Included in Bank Group	Date
BBVA Privanza Banco SA- Madrid	Banco Bilbao Vizcaya Argentaria SA	
Banco BBVA- Madrid		
BBVA Senior Finance SAU- Bilbao		
Catalunya Banca SA- Barcelona		March 2010
BBVA Banco de Financiacion SA- Bilbao		
Caixa d'Estalvis de Manresa-Caixa Manresa- Manresa		March 2010
Caixa d'Estalvis de Sabadell - Caixa Sabadell- Sabadell		March 2010
Caixa d'Estalvis de Terrassa-Caixa Terrassa- Terrassa		March 2010
Caixa d'Estalvis de Tarragona-Caixa Tarragona- Tarragona		March 2010
Caixa d'Estalvis de Catalunya Tarragona i Manresa-Catalunya Caixa- Barcelona		March 2010
Caixa D'Estalvis Unio De Caixes Manlleu Sabadell I Terrassa-UNNIM- Barcelona		March 2010
BBVA Privanza (Jersey) Limited- St. Helier Jersey		
Banco Bilbao Vizcaya Argentaria SA- Bilbao		
Caja de Ahorros de Murcia - Cajamurcia- Murcia	Banco Mare Nostrum SA-BMN	June 2010
Banco Mare Nostrum Group- Madrid		
Caja General de Ahorros de Granada - La General- Granada		June 2010
Banco Mare Nostrum SA-BMN- Madrid		
Caja de Ahorros del Penedes-Caixa d'Estalvis del Penedes- Vilafranca Del Penedes		June 2010
Caja de Ahorros y Monte de Piedad de las Baleares - Sa Nostra- Palma		June 2010
Banco de Sabadell SA- Sabadell	Banco Sabadell, SA	
Dexia Sabadell SA- Madrid		
Sabadell Solbank SAU- Madrid		
Banco CAM- Madrid		Dec. 2011
Banco Gallego SA- Santiago De Compostela		April 2013
Sabadell International Equity Ltd-		
Caja de Ahorros del Mediterraneo CAM- Alicante		Dec. 2011
Banco Urquijo Sabadell Banca Privada SA- Madrid		
BFA Tenedora de Acciones SAU- Madrid		

Notes: This table lists the 71 credit institutions that were subject to government intervention since the beginning of the financial crisis in 2007-08. For a number of institutions with missing "Date", we could not determine the exact point in time when the intervention took place. The Table is based on http://www.bde.es/f/webbde/GAP/Secciones/SalaPrensa/NotasInformativas/Briefing_notes/en/notabe060916en.pdf and on information obtained from the websites of individual credit institutions.

Table D.1 Credit Institutions (Cont.)

Credit Institution	Included in Bank Group	Date
Caja de Ahorros de la Rioja-Cajarioja- Logrono	BFA Tenedora de Acciones SAU (Bankia)	June 2010
Bankia SA- Valencia		
Caixa d'Estalvis Laietana-Caixa Laietana- Barcelona		June 2010
Caja Insular de Ahorros de Canarias-La Caja de Canarias- Las Palmas		June 2010
Caja de Ahorros y Monte de Piedad de Avila-Caja de Avila- Avila		June 2010
Bankia Banca Privada SA- Madrid		
Caja de Ahorros de Valencia Castellon y Alicante BANCAJA- Valencia		June 2010
Caja de Ahorros y Monte de Piedad de Madrid-Caja Madrid- Madrid		June 2010
Caja de Ahorros y Monte de Piedad de Segovia-Caja Segovia- Segovia		June 2010
CAIXABANK France- Paris	Caixabank, SA	
Caixabank S.A.- Barcelona		
Caja de Ahorros Municipal de Burgos-Caja de Burgos- Burgos		April 2010
Banco de Valencia SA- Valencia		Nov. 2012
Caja de Ahorros Provincial de Guadalajara-Caja de Guadalajara- Guadalajara		Dec. 2010
Monte de Piedad y Caja de Ahorros San Fernando de Guadalajara Huelva Jerez y Sevilla-Cajasol- Sevilla		Dec. 2010
Caixabank Electronic Money EDE S.L- Barcelona		
Caja General de Ahorros de Canarias - Caja Canarias- Santa Cruz De Tenerife		April 2010
Caja de Ahorros y Monte de Piedad de Navarra - Caja Navarra- Pamplona		April 2010
Caja Rural De Castilla-La Mancha- Toledo	Caja Rural	
Caja Rural de Casa Ibanez S. Coop. De Credito de Castilla-La Mancha- Casas Ibanez		
Caja Rural de Villamalea S Coop de Credito Agrario de Castilla-La Mancha- Villamalea		
Caja Rural de la Roda Sociedad Cooperativa de Credito de Castilla La Mancha- La Roda		

Notes: See notes above.

Table D.1 Credit Institutions (Cont.)

Credit Institution	Included in Bank Group	Date
Monte de Piedad y Caja General de Ahorros de Badajoz-Caja Badajoz- Badajoz	Ibercaja SA	July 2010
Banco Grupo Cajatres SA-Caja 3- Zaragoza		July 2010
Ibercaja Banco SAU- Zaragoza		
Caja de Ahorros y Monte de Piedad de Zaragoza Aragon y Rioja-Ibercaja- Zaragoza		July 2011
Caja de Ahorros de la Inmaculada de Aragon-Caja Inmaculada- Zaragoza		July 2010
Caja de Ahorros y Monte de Piedad del Circulo Catolico de Obreros de Burgos-Caja de Ahorros del Circulo Catolico- Burgos		July 2010
Liberbank SA- Madrid	Liberbank, SA	
Banco de Castilla-La Mancha SA- Cuenca		Nov. 2009
Caja de Ahorros y Monte de Piedad de Extremadura-Caja de Extremadura- Caceres		April 2011
Caja de Ahorros de Castilla La Mancha- Cuenca		Nov. 2009
Caja de Ahorros de Santander y Cantabria - Caja Cantabria- Santander		April 2011
Caja de Ahorros de Asturias - Cajastur- Oviedo		April 2011
Caixa de Aforros de Galicia Vigo Ourense e Pontevedra-Novacaixa Galicia- Coruna	NCG Banco, SA	June 2010
Caja de Ahorros de Galicia - Caixa Galicia- La Coruna		June 2010
Caixa de Aforros de Vigo Ourense e Pontevedra-Caixanova- Vigo		June 2010
Unicaja - Montes de Piedad y Caja de Ahorros de Ronda Cadiz Almeria Malaga Y Antequera- Malaga	Unicaja Banco SA	
Unicaja Banco SA- Malaga		
Banco de Caja Espana de Inversiones Salamanca y Soria SA- Madrid		March 2010
Caja Espana de Inversiones - Caja Espana- Leon		March 2010
Caja de Ahorros de Salamanca y Soria - Caja Duero- Leon		March 2010
Unnim Banc SA- Barcelona	Unnim Banc SA	

Notes: See notes above.

Table D2. Association with affected banks and loan growth

	Loan growth		
	(1)	(2)	(3)
Shock	-0.0330*	-0.0112	0.0238
	(0.0240)	(0.0271)	(0.0533)
Shock × <50 employees		-0.0440*	-0.0425*
		(0.0290)	(0.0291)
Shock × Sigma			-0.0349
			(0.0374)
Firm controls and interactions	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes
Sector × Period FEs	Yes	Yes	Yes
Region × Period FEs	Yes	Yes	Yes
Observations	22,096	22,096	22,096
R-squared	0.41	0.41	0.41

Notes: The Table reports the point estimates from OLS regressions where the dependent variable is the firm's annual loan growth. '<50 employees' is a dummy variable equal to one if the firm has less than 50 employees. 'Sigma' is the sector's technological elasticity of substitution between labor and capital, calculated using KLEMs US data over the period 1947—2010. All firm controls, fixed effects, and interactions from Table 3, column (7) are also included in the regression. In all regressions, only firms with at least one observation before and at least one observation after 2008 are included. The sample period is 2006—2009. Standard errors clustered at the sector-period level are reported in parentheses where ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent statistical level, respectively.

Table D3. Comparing firms, sectors, and banks

Panel A. Comparison across creditor attachment, pre-crisis

	Shock=0	Shock=1	Difference
Age	14.73	15.27	-0.64*
Sales (mln.)	32.05	32.58	-0.53
Cash flow / Assets	0.07	0.07	-0.00
Net worth / Assets	0.30	0.31	-0.01*
Debt / Assets	0.72	0.71	0.01
Employee compensation	27,439	29,037	-1,598*

Panel B. Comparison across sectors, pre-crisis

	Low-sigma	High-sigma	Difference
Age	15.10	14.97	0.13
Log (Sales)	32.27	32.45	-0.18
Cash flow / Assets	0.07	0.07	-0.00
Net worth / Assets	0.31	0.29	0.02*
Debt / Assets	0.71	0.72	-0.01
Employee compensation	28,317	28,186	131
Shock	0.53	0.52	0.01

Panel C. Comparison across banks

	Non-intervened	Intervened	Difference
Capital ratio in 2006/07	12.04	11.72	0.32
Capital ratio in 2008/09	11.81	11.24	0.57**
Difference	0.23	0.48*	-0.25*

Panel D. Comparison across firm size, pre-crisis

	<50 employees = 0	<50 employees = 1	Difference
Sales growth	0.10	0.09	0.01
Employment growth	0.05	0.03	0.02
Investment growth	0.01	-0.05	0.06**
Age	21.85	14.27	7.58***
Log (Sales)	37.10	31.76	5.34***
Cash flow / Assets	0.08	0.07	0.01
Net worth / Assets	0.35	0.30	0.05**
Debt / Assets	0.65	0.72	-0.07*
Employee compensation	32,469	27,788	4,681***

Note: The Table summarizes the main control variables used in the empirical tests across firms, for the pre-crisis period (2006 and 2007). Column (3) reports statistical differences based on a two-sided Mann-Whitney test. 'Shock' is a dummy variable equal to one in 2008 and in 2009 and if the firm has a credit association with at least one bank which required public assistance during the financial crisis. 'Age' denotes the firm's age in years. 'Log (Sales)' denotes the logarithm of the firm's total sales, 1-period lagged. 'Cash flow / Assets' denotes the ratio of the firm's cash flow to the firm's total assets, 1-period

lagged. 'Net worth / Assets' denotes the ratio of the firm's net worth, calculated as the difference between total assets and total liabilities, to the firm's total assets, 1-period lagged. 'Debt / Assets' denotes the ratio of the firm's total debt to the firm's total assets. 'Employee compensation' denotes the firms' total compensation bill (including direct and non-direct compensation), divided by the firm's number of employees. 'Shock' is a dummy variable equal to one if the firm has a credit relationship with a bank that received government assistance during the financial crisis. 'Capital ratio in 2006/07' is the bank's average ratio of capital to total assets during the period 2006–2007. 'Capital ratio in 2008/09' is the bank's average ratio of capital to total assets during the period 2008–2009. 'Sales growth' denotes the log difference in the firm's total sales between this period and the previous one. 'Employment growth' denotes the log difference in the firm's total employment between this period and the previous one. 'Investment growth' denotes the log difference in the firm's total tangible capital between this period and the previous one.

E Comparison of Spanish Sectoral Elasticity Results to the US

In this exercise, we perform the same analysis on Spanish data. For the case of Spain, KLEMs data has a less long sample: starting in 1995 and ending in 2015 (see Table E.1). We use the same estimation system and variable definitions as described in as Appendix B.

Broadly speaking – notwithstanding the imperfect nature of the comparison given shorter and poorer quality data, and an imperfect mapping between sectors – the comparison between countries is quite well taken. For instance, some of the highest elasticity value for the US are shared by the same sectors as Spain. The correlation between the two country series is almost 0.8 and testing for rank correlations does not reveal independence of distributions. Note that in five cases, there is no equivalent Spanish data to compare with the US.

The table below that, Table E.2 elaborates on the full set of Spanish results with the fit metrics and associated parameter tests.

Table E.1: Comparison of Substitution Elasticities: US and Spain

Industry	$\widehat{\sigma}_s$	
	US[1]	Spain
Food, beverages, and tobacco	1.590	1.680
Textiles, textile, leather, and footwear	0.920	1.282
Wood and products of wood and cork	0.690	0.843
Pulp, paper, printing, and publishing	1.820	–
Coke, refined petroleum, and nuclear fuel	0.360	0.751
Chemicals and chemical products	0.900	0.847
Rubber and plastic	0.570	0.759
Other non-metallic mineral products	0.410	–
Basic metals and fabricated metal products	0.420	0.705
Electrical, electronic, and optical equipment	0.790	0.623
Machinery, not else specified	0.540	0.535
Transportation equipment	0.640	0.813
Manufacturing, not else specified	0.660	0.711
Electricity, gas, and water supply	1.260	1.030
Construction	1.930	1.303
Sale, maintenance, and repair of motor vehicles and motorcycles	0.720	0.583
Wholesale trade, except of motor vehicles and motorcycles	0.860	–
Retail trade, except of motor vehicles and motorcycles[2]	0.750	–
Transportation and storage	0.920	0.714
Hotels and restaurants[3]	0.570	0.697
Post and telecommunications[4]	0.820	–
IT and other information services	0.660	0.884
Real estate, renting and business activities	0.780	0.603[5]
Unweighted Average	0.851	–
Overlapping Sample Unweighted Average	0.829	0.854
Sample Std. Dev	0.420	–
Overlapping Sample Std. Dev	0.400	0.297
Pairwise correlation	0.776 [0.000]	
Spearman's ρ	0.560 [0.017]	
Kendall's τ_a, τ_b	0.432, 0.436 [0.014]	

Notes:

This table shows estimates of the elasticity of factor substitution across selected industries. Probability values are in brackets. Full sets of results for Spain alone with full set of metrics shown in Table E.2. [1]This column is repeated from Table 2 in the main text. [2]The capital series is unavailable for this sector in Spain, so estimation is precluded. [3]On the Spanish KLEMs data there is no sector called "Post and Telecommunications", although "Postal and Courier Activities" is available and constitutes a reasonable proxy. However, capital data is unavailable for the sector in Spain, so estimation is precluded. [4]In the KLEMs Spanish data, the "Hotels and Restaurants" is relabelled "Accommodation and Food Service Activities". [5]Results for the sector "Real estate, renting and business activities" were retrieved from estimating the two first order conditions for capital and labor individually and averaging; application of the system approach, across many different robustness exercises, proved to be numerically unstable.

Table E.2: Full Sectoral Production Parameters for Spain

	Sector (NACE Code)																	
Parameter	10–12	13–15	16	31	19	20–21	22	24–25	26–27	28	29–30	32–33	35–39	41–43	45	49–53	55–56	62–63
ζ	0.986^{***}	0.971^{**}	1.003^{***}	1.056^{***}	1.033^{***}	1.006^{***}	1.008^{***}	1.022^{***}	1.013^{***}	1.015^{***}	1.057^{***}	0.981^{***}	1.002^{***}	1.003^{***}	0.991^{***}	1.035^{***}	1.059^{***}	0.994^{***}
	{0.951,1.020}	{0.955::0.987}	{0.990::1.015}	{0.970::1.141}	{1.017::1.049}	{0.991::1.022}	{0.994::1.021}	{1.004::1.040}	{1.001::1.026}	{0.979::1.051}	{1.046::1.067}	{0.951::1.01}	{0.986::1.017}	{0.994::1.012}	{0.970::1.013}	{0.993::1.076}	{1.034::1.083}	{0.985::1.003}
σ	1.680^{***}	1.282^{***}	0.843^{***}	0.751^{***}	0.847^{***}	0.759^{***}	0.705^{***}	0.623^{***}	0.535^{***}	0.813^{***}	0.711^{***}	1.030^{***}	1.303^{***}	0.583^{***}	0.714^{***}	0.697^{***}	0.884^{***}	0.603^{***}
	{1.645::1.715}	{1.185::1.379}	{0.674::1.012}	{0.413::1.090}	{0.820::0.874}	{0.737::0.782}	{0.685::0.724}	{0.587::0.658}	{0.508::0.563}	{0.801::0.825}	{0.629::0.793}	{0.998::1.005}	{1.268::1.339}	{0.550::0.617}	{0.683::0.745}	{0.687::0.708}	{0.797::0.971}	{0.600::0.631}
γ_a	0.045^{***}	0.131^{***}	0.008	-0.029	-0.104^{***}	0.009	0.020^{***}	-0.053^{***}	-0.040^{***}	-0.063^{***}	-0.126^{***}	0.050^{***}	-0.042^{***}	$-0.013*$	-0.063^{***}	0.036^{***}	-0.246^{***}	0.014^{***}
	{0.036::0.053}	{0.094::0.168}	{-0.051::0.068}	{-0.129::0.072}	{-0.122::-0.086}	{-0.000::0.019}	{0.009::0.031}	{-0.061::-0.044}	{-0.046::-0.034}	{-0.082::-0.044}	{-0.169::-0.083}	{0.042::0.057}	{-0.053::-0.031}	{-0.024::-0.003}	{-0.088::-0.037}	{0.029::0.044}	{-0.363::-0.130}	{0.011::0.016}
γ_b	-0.029^{***}	-0.031^{***}	-0.008	-0.218	0.123^{***}	-0.011^{***}	-0.002	0.065^{***}	0.046^{***}	0.085^{***}	0.080^{***}	0.001^{***}	-0.011^{***}	-0.000	-0.002	-0.088^{***}	0.097^{***}	-0.005
	{-0.037::-0.022}	{-0.046::-0.017}	{-0.040::0.024}	{-0.468::0.032}	{0.104::0.142}	{-0.017::-0.005}	{-0.013::0.010}	{0.058::0.071}	{0.043::0.049}	{0.076::0.094}	{0.068::0.091}	{0.001::0.001}	{-0.016::-0.006}	{-0.006::0.006}	{-0.016::0.012}	{-0.101::-0.076}	{0.059::0.135}	{-0.014::0.004}
ll	146.587	140.010	148.634	−21.984	161.357	147.825	124.615	115.085	151.949	124.825	123.580	144.001	181.685	145.018	109.840	117.339	77.454	180.290
aic	−285.174	−272.020	−289.268	51.969	−314.714	−287.649	−241.229	−222.169	−295.899	−241.650	−239.159	−277.13	−355.370	−282.036	−211.680	−226.678	−146.909	−354.579
bic	−280.996	−267.842	−285.090	56.147	−310.536	−283.471	−237.051	−217.991	−291.721	−237.472	−234.981	−281.11	−351.192	−277.858	−207.502	−222.500	−142.731	−351.446
$rmse(Y)$	0.085	0.028	0.031	0.149	0.037	0.047	0.105	0.055	0.026	0.079	0.026	0.033	0.042	0.030	0.056	0.093	0.059	0.026
$rmse(sh_L)$	0.037	0.039	0.044	0.460	0.041	0.053	0.105	0.078	0.033	0.061	0.044	0.040	0.019	0.049	0.090	0.063	0.097	0.088
$rmse(sh_K)$	0.053	0.100	0.085	0.763	0.035	0.073	0.112	0.079	0.059	0.085	0.135	0.070	0.042	0.094	0.155	0.055	0.280	0.030
$\sigma = 1$	[0.000]	[0.000]	[0.068]	[0.150]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.071]	[0.000]	[0.000]	[0.000]	[0.000]	[0.009]	[0.000]
$\gamma_a = \gamma_b$	[0.000]	[0.000]	[0.724]	[0.288]	[0.000]	[0.005]	[0.061]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.100]	[0.002]	[0.000]	[0.000]	[0.001]
$\gamma_b - \gamma_a$	−0.074	−0.162	−0.016	−0.190	0.227	−0.021	−0.021	0.118	0.086	0.148	0.206	0.049	0.031	0.013	0.060	−0.125	0.343	−0.019

Notes: This table summarizes the technological elasticities of substitution between labor and capital for the respective sectors, using the KLEMs data for Spain over the sample 1995–2015. See Appendix B for a description of how σ_s is calculated. See also the earlier Table E.1 for the comparison with the longer more detailed US data and Table C.2 for a reminder of the terms and tests shown. Symbols ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent statistical level, respectively. Numbers in braces refer to 95% confidence intervals and number is brackets refer to probability vales.

Appendix

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