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

Dismissal rules are less stringent for Spanish firms with fewer than 50 employees, lowering the cost of hiring new workers. We find that during the financial crisis, healthy firms with fewer than 50 employees borrowing from troubled banks grew faster in sectors where capital and labor are technological substitutes. This result does not obtain when we use a different cut-off for Spain or the same cut-off for firms in Germany. Our evidence suggests that labor market flexibility can dampen the negative effect of credit shocks by allowing firms to keep growing by substituting labor for capital.

JEL classification: G21; J80; D20.

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

*We thank Francesca Barbiero and Carlo Medici for outstanding research assistance, and Jakub Mućk and Lucciano Villacorta for discussions. The opinions expressed herein are those of the authors and do not necessarily reflect those of the ECB or the Eurosystem.

†Corresponding author. European Central Bank, Financial Research Division, Sonnemannstrasse 20, D-60314 Frankfurt, email: Alexander.Popov@ecb.int

1 Introduction

Labor market liberalization is typically on top of the policy agenda when countries embark on a program of structural reforms. Besides being a policy darling, flexible labor market institutions are a mainstay of neoclassical economic theory, and are often credited with delivering large economic benefits to liberalizing countries, such as the UK in 1989, Italy in 1997–2003, or Germany in the early 2000s (Boeri, 2011; Dustmann et al., 2014). Conversely, high employment protection is often seen as making economies more sclerotic, losing their ability to adapt to negative shocks (Blanchard and Portugal 2001).[1] While empirical evidence to the success of labor market reforms abounds, it mostly focuses on the up phase of the cycle (Engellandt and Riphahn, 2005; Ichino and Riphahn, 2005). This approach is consistent with Davis and Haltiwanger's (1990, 1992) seminal work on the importance of labor reallocation for growth and macroeconomic fluctuations. At the same time, there are equally good arguments to expect that flexible labor markets will deliver large benefits to a country's economy during downturns, including by facilitating the process of "creative destruction" (Caballero and Hammour, 1994). Yet, to our knowledge, there is no evidence to the role that employment protection plays during financial crises.

In this paper, we go to the heart of this question by studying the role of labor market flexibility in the ability of firms to absorb large negative shocks during recessions. Our study focuses on shocks to the supply of credit during the recent financial crisis and their negative impact on the demand for labor. We construct a new data set that merges the Amadeus database for Spanish firms with data on each firm's main banking relationship from Kompass. We exploit variation in bank health to capture negative shocks to the supply of credit, using information on whether the bank received government assistance to determine bank health during the crisis. We then capture the impact on the demand for labor using the elasticity of substitution between capital and labor. Over short and medium run frequencies, capital tends to adjusts highly inflexibly, with most of the adjustment falling on labor (Caballero and Hammour, 1998). A higher value of substitution thus denotes a higher degree of flexibility with which labor can be adjusted to absorb shocks (Hicks, 1932). Finally, we exploit a unique feature of Spanish labor market regulation at the time of the financial

[1] For excellent early reviews of the role played by differences in the level of employment protection, unemployment benefit systems, payroll taxes and subsidies on labor, as well as wage setting institutions, in explaining the higher level and duration of unemployment in Europe vis-à-vis the US, see Bean (1994), Nickell (1997), and Bertola (1999).

crisis to capture the influence of labor market protection. Specifically, according to Spanish law, employment protection is notably more stringent for firms with more than 50 employees, requiring collective dismissals accompanied by a social plan, greatly increasing firing costs. These firing costs can be seen as a fixed per employee cost holding back firm growth (Bentolila and Bertola 1990). We then compare the growth performance of Spanish firms during the crisis at firms with less than 50 employees and that had pre-existing relationships with relatively weak banks and that operate in sectors with a relatively high value of capital-labor substitution with otherwise similar firms that have more than 50 employees. We focus on sales growth as a measure of firm performance but also consider employment growth as a potential transmission channel.

Our methodological approach rests on a number of conditions. First, bank-borrower relationships need to be sticky over the cycle, a fact that has been established by Chodorow-Reich (2014). Second, cross-sectional variation in bank's willingness to lend during the crisis needs to be independent of pre crisis firm characteristics and labor market conditions. The advantage of focusing on credit supply shocks is that they are unlikely to be related to pre-crisis labor market conditions. Third, capital needs to adjust inflexibly over the crisis period, with all the adjustment falling on labor. To the extent this is not the case, it would bias the results against finding an effect. And finally, firm size needs to primarily capture variation in employment protection and not other factors that influence demand for labor and/or the supply for credit. This condition is questionable because firm size is likely to capture financial frictions, with smaller firms being more affected by credit market disruptions. However, to the extent that the small firm dummy captures financial frictions and not employment protection it should bias the results against finding an effect. Moreover because the employment protection kicks in at a firm size of exactly 50 employees and financial frictions are likely monotonically decreasing in firm size, we will exploit regression discontinuity techniques around the firm size threshold of 50 employees to differentiate between the effects of employment protection and financial frictions. Additionally, to address concerns that the measure of capital-labor substitution may be picking up variation in the demand for finance we also control for the financial dependence of the firm.[2]

Spain is an ideal setting to study the role of labor market flexibility in affecting firm growth during recessions. First, Spain differentiates its labor market protection by firm

[2]Another concern is that small firms that are more likely to be exposed to shocks stay below a size of 50 to avoid high firing costs, introducing a possible selection bias. However, we find no bunching of firms around 50 employees in the data.

size, offering exogenous variation in the degree of labor market protection needed to identify the impact of labor market protection on firm outcomes.[3] Second, Spain has for long been an outlier in labor market protection, especially firing costs, contributing to structurally high unemployment (Bover et al. 2000). Third, the recent financial crisis prompted a dramatic increase in unemployment in Spain, with youth unemployment reaching over 50 percent, higher than in any other OECD economy, prompting the question how more flexible labor markets could have enhanced the ability of firms to withstand the shock and preserve employment and output. While recessions may have cleansing effects, with some role for creative destruction, deep and long recessions such as the one in Spain can generate major scars with long-run negative effects. Finally, for Spain we have detailed firm-level data on firm characteristics and bank-firm relationships, covering the full size distribution of firms from 1 employee to over 1000 employees. With very few exceptions, previous studies on the role of credit market shocks have focused on large firms (e.g., Chodorow-Reich, 2014). Notable exceptions are Duygan-Bump et al. (2015), Bentolila et al. (2017), and Popov and Rocholl (2017) who study the impact of credit shocks on small-firm employment in the US, Spain, and Germany, respectively. However, these studies do not consider the role of labor market flexibility which is a key focus of our study.

Our main finding is that Spanish firms with fewer than 50 employees grew relatively faster during the financial crisis when exposed to a negative credit shock than larger firms in sectors with a technologically higher substitutability between labor and capital. This 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 sector-specific trends with interactions of sector and year dummies. 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, and when we look at firms with a credit relationship with only one bank. It also appears to be long-lasting, since it is still present in the data 4 years after the initial shock.

We also perform three separate falsification tests of the underlying credit and labor market mechanisms. First, we repeat the main test on the same sample of firms, but in the

[3] Schivardi and Torrini (2008) and Cingano et al. (2016) similarly exploit firm size cutoffs to identify the effects of firing restrictions in Italy where firing restrictions are more stringent for firms with more than 15 employees.

absence of a credit shock (i.e., before the financial crisis). Second, we compare how sales growth adjusts to credit shocks, depending on the firm's elasticity of substitution between labor and capital, for firms smaller and larger than another arbitrary firm-size threshold that does not capture a firm-size-specific employment protection rule. Third, we compare sales growth across the 50-worker threshold, for credit-constrained versus unconstrained firms and depending on their elasticity of substitution between labor and capital, in Germany where 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.

Our paper contributes to the 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; Ichino and Riphahn, 2005; Card et al., 2007) and of the introduction of activation programs (Heckman et al., 1999; Dolton and O'Neill, 2002; Kluve and Schmidt, 2002; Black et al., 2003). More closely related are papers that have studied the impact of employment protection laws on firm demand for labor. For example, 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. Boeri and Jimeno (2005) study the effects of employment protection on dismissal probabilities and on the equilibrium size distribution of firms. They find that workers under permanent contracts in firms with less restrictive employment protection are more likely to be dismissed, while at the same time there is no effect of the exemption threshold on the growth of firms. Garibaldi and Violante (2005) study empirically the two separate dimensions of firing costs—the transfer from the firm to the laid-off worker, and the tax paid outside the firm-worker pair—and show that they do not have the same effect in the presence of wage rigidities. Messina and Valanti (2007) use firm level data of manufacturing and non-manufacturing industries to study the impact of firing restrictions on job flow dynamics across 14 European countries, and find that more stringent firing laws make job turnover less counter-cyclical. Marinescu (2009) uses a 1999 British reform that increased job security for workers with 1–2 years of tenure, and finds that the firing hazard for these workers decreased by 26% relative to the hazard for workers with 2–4 years of tenure. Methodologically most closely related to our analysis is the paper by Cingano et al. (2017) which also uses a firm-size threshold to identify the impact of employment protection on capital investment in Italy. We make two contributions to the literature relative to the cited papers. First, we study the interaction between employment protection and credit constraints on firm growth, whereby we take advantage of the firm's technological ability to substitute labor for capital when relative factor prices change. Second, we study 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 is closely related to studies on the effect of financial market frictions on 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). Lichtenberg and Siegel (1990) provide evidence that a leveraged buyout is followed by a reduction in employment and wages. Hanka (1998) shows that highly levered firms reduce employment more often and pay lower wages. Falato and Liang (2017) show that loan covenant violations are followed by simultaneous cuts in employment and wages. 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 and Mas (2014) 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. Boeri et al. (2012) shows that more leveraged sectors exhibit higher employment-to-output elasticities during banking crises. Pagano and Pica (2012) show that during banking crises, employment grows less in industries more dependent on external finance. There are several studies that have used micro data to estimate the response of employment to credit constraints. Campello et al.(2010) show that firms with credit constraints plan to cut investment and employment more than unconstrained firms. Chodorow-Reich (2014) uses syndicated loan data to show that small firms that before the crisis were borrowing from banks that subsequently became impaired, reduced employment more than small firms associated with healthier banks. Acharya et al. (2014) find that large firms with higher exposure to syndicated lending by European periphery banks experienced lower growth of employment, sales, and capital expenditures. 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. (2017) show that Spanish firms with credit relationships with weak banks had substantially lower employment levels than firms borrowing from non-affected banks. Popov and Rocholl (2017) show that German firms borrowing from savings banks that during 2007–2008 had to provide funds for the recapitalization of their head institutions reduced employment and average wages, both in the short and medium term.

The remainder of 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** details the calculations of the industry-specific elasticity of substitution between labor and capital. **Section 4** describes the data. **Section 5** presents the empirical methodology. **Section 6** provides the main test, alongside an exhaustive battery of falsification and robustness tests, and we investigate the mechanisms involved. **Section 7** concludes.

2 Institutional details

2.1 Employment protection in Spain

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. The first one applied to negotiation 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 more than 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, oftentimes during the negotiations with the workers' representatives, severance per employee is increased.

In addition, in enterprises with more than 50 employees, a collective dismissal should be accompanied by a social plan aiming to mitigate the consequences for the affected workers. Such firms carrying out a collective dismissal should 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:

- 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;
- measures aimed at reducing the effects of restructuring on employees;
- external relocation;
- promotion of self-employment;
- financial compensations for geographical mobility;
- economic, technical, organizational and other types of measures intended to make the continuation of the undertaking and its activity possible.

Companies also have to carry out a special training and redeployment plan of at least 6 months, implemented by means of an authorized outplacement company, if the collective dismissal affects over 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. Importantly, companies with fewer than 50 workers do not have to implement a social plan aiming to support dismissed workers.

The longer negotiating period and the need to provide workers with a social plan makes it considerably more costly for firms with more than 50 employees to dismiss workers. As a consequence, the cost of hiring the marginal worker, given that she may have to be let go in the future, is higher for such firms. It is therefore plausible to hypothesize that firms with fewer than 50 employees will be more inclined to hire more workers if they need to expand. This logic goes back to studies such as Bentolila and Bertola (1990) who argue that employment protection laws – in their view, the main source of firing costs in Europe – have a significant effect on firms' propensity to fire, as well as to hire.

One immediate concern is that this type of regulation may be too weak to present a binding constraint on firms' expansion. Figure 1A plots the number of firms by number of employees, for firms with between 40 and 60 employees (Figure 1B plots the share of firms by all employees). The Figure shows clear evidence of a discontinuity around 50, with firms clustering at 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, as well as with evidence thereof (e.g., Garicano et al., 2015).

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, in the amount of 11.6 bln. euro, i.e. about 1.1% of Spanish GDP (Banco de España, 2014). While some of these actions took place at the very 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 Table A1). 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.[4]

As a result of balance sheet problems, new credit issued by affected savings banks de-

[4] See Cunat and Garicano (2010), Fernandez-Villaverde et al. (2013), and Santos (2014).

clined 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 (see Bentolila et al., 2015).

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, affecting firm growth through the channel of reduced credit access.

3 Estimation of the sectoral capital-labor elasticities

A key parameter in our analysis is firms' factor substitution possibilities. How easy it is to shift (substitute) between factors (typically, capital K and labor L) is captured by the elasticity of factor substitution. This provides a powerful tool for answering analytical questions about the distribution of income and the response of the economy to various shocks. To illustrate, for production function $Y = F(K, L)$ the elasticity is given by the formula $\sigma \in (0, \infty] = \frac{d \log(K/L)}{d \log(F_K/F_L)}$. In other words, it is the percentage change in factor proportions due to a unit change in the marginal rate of technical substitution (MRTS) (along a given isoquant). Under the assumption that the marginal productivities (F_K, F_L) reflect factor prices, the MRTS would match up with the wage/capital rental ratio.

Accordingly, if there is a 'shock' to relative factor prices (i.e., one factor becomes more expensive compared to another), then optimizing firms respond by changing their factor intensity. The extent to which they can do so is captured by 'σ'. To illustrate, increasing the minimum wage in sectors in which the substitution elasticity is 'high' (say > 1) would, ceteris paribus, contract labor demand since the representative firm could shift into (now relatively cheaper) capital inputs. Likewise, and more relevant in our context, a firm which faced higher or more variable capital costs, may react by increasing its demand for labor (to the extent allowed by its substitution elasticity).

Following León-Ledesma et al. (2010) we estimate sectoral production characteristics using a normalized system of equations containing the production function and factor demands with cross-equation parameter constraints. The full details are relegated to **Section A** of the appendix but the general principles are straightforward. Consider that real output Y for a given sector can be described by a Constant Elasticity of Substitution (CES) production function,

$$Y_t = F(K_t, L_t) = \left[\pi \left(\Gamma_t^K K_t\right)^{\frac{\sigma-1}{\sigma}} + (1-\pi)\left(\Gamma_t^L L_t\right)^{\frac{\sigma-1}{\sigma}}\right]^{\frac{\sigma}{\sigma-1}} \tag{1}$$

where $\pi \in (0,1)$ is a distribution parameter that determines the relative importance of factors in the production of the final good, and the term Γ_t^J capture the level of technical progress associated to the jth factor over time t.

The CES function is attractive since it nests other well-known production types.[5] In the Leontief case ($\sigma = 0$), substitution between factors is not possible: capital and labor must always be used in fixed proportions. In the linear case ($\sigma = \infty$), substitution is such that factors are essentially indistinguishable from one another. Cobb Douglas ($\sigma = 1$) is an intermediate case. Figures 2 and 3 illustrate the mechanism. For firms which cannot substitute labor for capital, an increase in the user cost of capital results in a decline in both factors of production (Figure 2). However, for firms with high elasticity of substitution, an increase in the user cost of capital can result in an increase in employment (Figure 3), as firms substitute labor for capital, with overall output declining less than in the Leontief case.

4 Data

4.1 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

[5] An additional 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 unitary-elasticity Cobb-Douglas production function is that factor income shares are constant, given that movements in factor prices are exactly offset by commensurate movements in factor volumes. However as **Figure B.1** makes clear for all the sectors in the database, this is widely counterfactual.

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 2004–2013 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 that have negative values.

Next, we drop firm-year observations for which some basic accounting identities are violated by more than 10%.[6] 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. For our analysis we retain only firms in Manufacturing (NACE Rev. 2 section C), Construction (F), Wholesale and retail trade (G), Trans-

[6] 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.

portation and storage (H), Accommodation and food service activities (I), Information and communication (J), Professional, scientific and technical activities (M) and we 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 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. This reduces the sample to 135,799 unique firms, for a total of 1,351,331 observations.

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. The main dependent variable in the paper is 'Sales growth' which denotes the log difference in the firm's total sales between this period and the previous one. On average, firms over the sample period posted a year-on-year decline in sales of about 3.2 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 (3.6 percent), suggesting a 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 5.2 percent year-on-year, with the median firm posting an even larger decline (6.7 percent). 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. This definition is based on a piece of regulation which makes it considerably easier for smaller firms to fire employees, making it less costly for them to expand their labor force. 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 hiring workers. In order to make sure 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 43.5 percent fewer than 10, employees. We also employ a set of standard controls for size and net worth. They take the logarithm of total sales, as well as the ratio of cash flow to total assets and of net worth to total assets.

4.2 Firm-bank shock

One of the main blocks of our identification strategy is trying to compare firms across size bins and sectors in terms of whether they are credit constrained or not. While the Spanish banking sector as a whole experienced a large negative shock during the crisis, mostly deriving from its exposure to the country's pre-crisis housing boom, 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 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.[7] 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.[8]

Table 1 reports the percentage of firms in our sample that are associated with one or with multiple banks. On average, 51 percent of firms in the final dataset have a credit association with at least one affected bank, translating into 26 percent of all observations. Of

[7]For information on bank recapitalizations, see
http://www.bde.es/f/webbde/GAP/Secciones/SalaPrensa/NotasInformativas/Briefing_notes/en/notabe060916en.pdf.
See also Appendix Table 1 for a list of all banks classified as "affected".

[8]There is a large literature that uses the same empirical strategy for constructing a firm-level credit shock (e.g., Jimenez et al. (2012); Popov and Udell (2012); Kalemli-Özcan et al. (2015); Beck et al. (2017); Popov and Rocholl (2017).

course, having a credit association with an affected bank is less of an issue for firms with multiple banking relationships. For this reason, in robustness tests we look at firms attached to a single bank only. In the case of these, the variable 'Shock' is equal to one in 18.9 percent of all cases (corresponding to 40.3 percent of all such firms).

4.3 Elasticity of substitution between labor and capital

We estimate the technological elasticity of substitution between labor and capital 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 that is backed by highly developed and liquid financial markets. Moreover, the US 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 substitution between labor and capital for US sectors should produce a reasonable empirical proxy for the sectors' "natural" elasticities of substitution.

For our estimation of the US sectors, we use the KLEMs database which contains industry-level measures of output, inputs and productivity, see Jorgenson et al. (2012). The database provides carefully constructed data on the labor input, labor compensation and capital (incorporating quality adjusted and compositionally adjusted measures). We focus on the market economy rather than total economy and so exclude public administration and other non-market activities. This common practice mainly reflects measurement problems in these sectors (see, e.g., Klump et al., 2007; O'Mahony and Timmer, 2009).[9]

There are two vintages of the US KLEMs database, one from 1947–2010, the other from 1970–2007.[10] In this application, we rely on the more popular longer data set. 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 data sets give qualitatively similar values for the sectoral elasticity values.

Table 2 shows that the elasticity of substitution values across the selected sectors. They

[9] Specifically, these discarded industries are Education (code M), Extra-Territorial Organizations & Bodies (Q), Health & SocialWork (N), Public Admin. & Defence; Compulsory Social Security (L).

[10] The respective databases are available from http://www.worldklems.net/data.htm and http://www.euklems.net.

range from 0.36 (Coke subsector) to 1.96 (Construction), with a (unweighted) across-industry median of 0.85. In all but four cases, the elasticity is below unity. Moreover, although there is an elasticity value relatively close to unity in four other cases (e.g., $\widehat{\sigma} = 0.9$ in Textiles, textile, leather, and footwear, see tables C.2-C.1), we conclude that in no case can a unitary elasticity not be rejected. Given the prevalence of below-unity results, there are many sectors which are (compared, say, to the unitary elasticity benchmark) constrained in their ability to substitute factors for one another in the face of economic shocks or changes to relative factors prices.

5 Empirical methodology and identification

Our identification strategy is based on three separate arguments. The first argument is that a firm's sales growth will be negatively affected if it experiences a negative credit shocks that raises the cost of renting capital. Such shock can materialize when, for example, the firm's creditor is experiencing balance sheet problems and needs to cut lending. The second argument is that a firm's decline in sales 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 proportions. The third argument 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.

Exploiting this identification mechanism, we model the sales growth of firm f in sector s in year t as follows:

$$\begin{aligned} \Delta Sales_{fst} = \quad & \beta_1 Shock_{ft} \times < 50\, employees_f \times \widehat{\sigma}_s + \\ & \beta_2 Shock_{ft} \times < 50\, employees_f + \beta_3 Shock_{ft} \times \widehat{\sigma}_s + \\ & \beta_4 Shock_{ft} + \beta_5 X_{ft} + \mu_f + \theta_{st} + \varepsilon_{fst} \end{aligned} \tag{2}$$

$\Delta Sales_{fst}$ denotes the change in the total sales of firm f in sector s between year $t-1$ and year t. We calculate the variable as a log difference. Our results are robust to constructing the variable as a percentage change instead.

We now turn to the main explanatory variables. $Shock_{ft}$ is a dummy variable equal to one if firm f is borrowing from a bank that was affected during the financial crisis and later required government assistance. For firms linked to an affected bank, the dummy variable becomes one in 2008. While the majority of firms in the dataset are single-bank firms, more than a third of firms report a credit relationship with more than one bank. In the latter case, $Shock_{ft}$ is equal to 1 after 2008 as long as the firm has a credit relationship with at least one affected bank. Variable $< 50\ employees_f$ is a dummy equal to one if the firm has fewer than 50 employees. This definition is based on the regulatory cut-off discussed in **Section 4**, 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.

Finally, $\widehat{\sigma}_s$ is the estimated industry-specific elasticity of substitution between capital and labor whose construction we detailed in **Section 3**. 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. σ_s thus allows us to identify firms' growth response to changes in the cost of capital for one segment of firms (with high capital-labor elasticity) compared to another segment of firms (with low capital-labor elasticity).

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. For one, we include a vector of time-varying firm-specific variables X_{ft}. It includes the logarithm of the firm's sales, the ratio of the firm's cash flow to assets, and the ratio of the firm's net worth to assets. These variables capture the firm-specific impact on growth of size, cash flow from operations, and agency problems. All variables are lagged 1-period. 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. We also include a matrix of sector-year fixed effects θ_{st}. 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).[11] We specify two-way clustered standard errors at the sector and year level (Petersen, 2009). Finally, we estimate model (2) using OLS.

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 sigma). 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-sigma 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 4 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 at smaller firms. However, Figure 5 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 in sectors with

[11] Note that we cannot estimate the direct effect of the interaction variable $50\ employees_f \times \sigma_s$ because it is subsumed in the fixed effects.

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 firm-size effect can dominate the employment-protection effect, and that only firms that can technologically substitute labor for capital will benefit from lax 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 beginning 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.

One final concern with our identification strategy is that small firms that are more likely to be exposed to shocks can choose to stay below a size of 50 to avoid high firing costs, introducing a possible selection bias. However, **??**, where we plot the firm size distribution for the firms in our sample, shows no bunching of firms around 50 employees in the data, alleviating concerns related to the self-selection of firms into a particular size class.

6 Credit shocks, employment protection, and firm growth: Empirical results

In this section, we present the full battery of empirical tests employed to evaluate the relationship between credit constraints, employment protection, and firm growth. In **Section 6.1**, we present the deadline results based on model (1). In **Section 6.2**, we present estimates from a set of falsification tests. In **Section 6.3**, we perform robustness tests using alternative cut-offs within our regression discontinuity set-up, as well as alternative empirical proxies, sample periods, and samples. Finally, **Section 6.4** tests for the underlying mechanisms that are activated to deliver the main result.

6.1 Main result

We begin by testing more parsimonious versions and gradually building towards the most saturated version of Model (2). 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$\times$Year fixed effects. The point estimate is negative and significant at the 5 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 30 percent 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.

We next proceed to evaluate how credit shocks interact with the other components of the triple interaction in Model (2). 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 less 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 is 1.09 percentage points, or 0.34 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-sigma and low-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-sigma industries. Crucially, the impact of a credit shock is reduced for firms with fewer than 50 employees in high-sigma industries. 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 legislation makes it easier for them to fire employees. Therefore, we conclude that we have identified a positive impact on firm growth of laxer labor regulation in the presence of credit shocks.

Finally, in column (5) we estimate our preferred specification which includes on the right-hand side the triple interaction, the estimable double interactions, the variable $Shock$, the fixed effects as specified, and the set of firm-specific time-varying controls. We find that larger firms have on average lower sales growth, while firms with higher net worth have on average higher sales growth. These results are logical and they also serve to validate the data we are using. Importantly, we find that all variables of interest used to identify the main effect have the expected sign, just as in column (4). Namely, we confirm that a credit shock reduces sales growth more for small firms and for firms in high-sigma industries, but its impact is reduced for small firms in high-sigma industries, which are the firms that both can and are allowed to substitute labor for capital.

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 σ (Chemicals and chemical products) and another at the 25th percentile of σ (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 (5) is 0.0213. Our estimates thus imply that a firm with fewer than 50 employees borrowing from an affected bank grows by 0.7 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 22 percent, and is a direct confirmation of the importance of the technological ability to substitute labor for capital when the rental cost of capital goes up. Alternatively put, a firm in the median-sigma industry (Retail trade, except of motor vehicles and motorcycles, $\sigma = 0.75$) that is borrowing from an affected bank grows by 1.6 percentage points faster if it has fewer than 50 than if it has more than 50 employees. This implies a reduction in the average decline in firm growth over the sample period of around 50 percent, and is a direct confirmation of the importance of lax employment protection rules when the rental cost of capital goes up and firms have the technological ability to substitute labor for capital.

6.2 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 4.

In order for the credit shock we use to be valid, it has to bite only during the financial crisis once banks were suddenly hit by balance sheet problems. In other words, it should not have an effect before the financial crisis when the Spanish banking sector posted healthy growth in both lending and profitability. To test this underlying assumption, in column (1) we perform our underlying test of Model (2) on the period 2004–2007, using the same sample of firms, the same firm-bank matches, and the same definition of an affected bank. 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 capita. 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 has fewer than 10 employees. 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 Model (2) 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.[12] This makes it possible to determine which firms are linked to "affected" banks and therefore credibly experiencing a negative credit shock.[13] 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

[12] See Puri et al. (2011) for more institutional details.

[13] See Puri et al. (2011) for evidence savings banks affected in this fashion reduced retail lending, and Popov and Rocholl (2017) for evidence that firms borrowing from such banks reduced employment and labor compensation.

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.

6.3 Robustness

6.3.1 Narrower window around the 50-employee cut-off

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. Namely, while all 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—this becomes 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 500 employees) to a sub-sample of very small firms (e.g., with less than 5 workers), a concern our test in Table 4, column (2) does not address. Lending gravity to this concern is the fact that 95.3 percent of the firms in our sample have fewer than 50 employees, 90.4 percent of the firms have fewer than 30 employees, 69.4 percent of the firms have fewer than 10 employees, and the median firm has 6 employees.

To address this issue, in Table 5 we reports estimates from tests where we have restricted the sample to a narrower window around the 50-employee cut-off. We make three such sample choices. In the first regression, we restrict the sample to firms with more than 30 and firms with less than 750 employees, which corresponds to 4 percent of the sample on each side of the 50-employee cut-off. In the second regression, we restrict the sample to firms with more than 35 and firms with less than 110 employees, which corresponds to 2.5 percent of the sample on each side of the 50-employee cut-off. Finally, we restrict the sample to firms with more than 38 and firms with less than 75 employees, which corresponds to 1.5 percent of the sample on each side of the 50-employee cut-off.

Table 5 reports the results from these regressions. In all of these cases, the point estimate on the triple interaction is positive and significant at least at the 5 percent statistical level, suggesting that a credit shock which increases the price of renting capital has a smaller impact on firms that can substitute labor for capital and are allowed to do so by labor rules. Numerically, in all three cases, the point estimates of β_1 are larger than the one reported in Table 3, column (5). These tests thus provide a direct confirmation of the assumption that a regulatory firm-size-specific restriction has an effect on firms close to the cut-off.

6.3.2 Alternative empirical proxies, sample periods, and samples

In Table 6, 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 data set 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 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 Model (1) using the second, shorter data series. Then we use the resulting sector-specific values to re-estimate Model (2). The estimate from this regression is reported in column (1) of Table 6. They strongly suggest that 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 $\sigma's$ has a median value of 0.77. 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-sigma. Our estimates strongly suggest that it is indeed firms in high-sigma sectors with fewer than 50 employees for whom a credit shock has a weaker impact on sales growth.

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, 2017). 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 (3), 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 5 percent statistical level.

Next, we extent the sample period on both sides, by one year (column (4)) and by two years (column (5)). 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 Model (2) suggest that the increase in firm growth coming from the substitution of labor for capital not only does not disappear in the medium run, but becomes even larger 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.

The final concern we address is data-related. 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 favour: 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) of Table 6, we reduce the sample to firms with a single bank relationship. Such 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 sub-sample of firms that only bank with one creditor, too. The magnitude of this effect is substantially larger than in column (5) of Table 3, confirming the intuition that single-bank firms are more affected by the same credit shock than firms that can substitute across creditors. Importantly, the decline in firm growth is 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.

6.3.3 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, and they can only do so if both their technology and 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 would 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 7 we split the sample between firms with below-median and firms with above-median sales growth in 2007. 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 later. The evidence in Table 7 suggests that the mechanism we identify is only 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-sigma sectors, suggesting that they indeed substitute labor for capital in order to keep growing (column (1)). We detect no such patter in the data when we zoom in onto the sample of low-growth firms (column (2)), validating the underlying assumption behind our empirical strategy.

6.4 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 it possible—will do so. Therefore, conditional on being subject to a credit shock, we should observe higher labor growth in firms with fewer than 50 employees in high-sigma industries, relative to firms with more than 50 employees and/or firms in low-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 7 presents a direct test of this hypothesis. We modify Model (2) in two ways. In column (1), we replace the dependent variable with employment growth, i.e., the log difference in the firm's total employment between this period and the previous one. In column (2), we replace the dependent variable with investment growth, i.e., the log difference in the firm's total tangible assets between this period and the previous one. 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, which is consistent with our prior.

7 Conclusions

We show that Spanish firms with fewer than 50 employees grew relatively faster during the financial crisis when exposed to a negative credit shock than larger firms in sectors with a technologically higher substitutability 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 during recessions. These results hold only for for firms in sectors with a relatively high elasticity of substitution between labor and capital which are firms that offer more flexibility to adjust labor (since capital tends to be sticky over short periods). This 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 sector-specific trends with interactions of 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. The effect is also long-lasting, as it is still present in the data 4 years after the initial shock. 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.

Our results strongly 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. Since the financial crisis, Spain has indeed embarked 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. This has led to a reduction in the wage bill of the average firm and generated an economic recovery and a fall in the exorbitantly high levels of unemployment.

References

[1] Acharya, V., Eisert, T., Eufinger, C., and C. Hirsch, 2014. Real effects of the sovereign debt crisis in Europe: Evidence from syndicated loans. NYU mimeo.

[2] Alesina, A., and F. Giavazzi, 2006. *The Future of Europe: Reform or Decline*. MIT Press: Cambridge, MA.

[3] Atkinson, A., and J. Micklewright, 1991. Unemployment compensation and labor market transitions: A critical review. *Journal of Economic Literature* 29, 1679–1727.

[4] Beck, T., Degryse, H., De Haas, R., and N. van Horen, 2017. When arm's length is too far: Relationship banking over the credit cycle. *Journal of Financial Economics*, forthcoming.

[5] Benmelech, E., Bergman, N., and A. Seru, 2011. Financing labor. NBER Working Paper 17144.

[6] Bentolila, S., and G. Bertola, 1990. Firing costs and labor demand: How bad is eurosclerosis? *Review of Economic Studies* 57, 381–402.

[7] Bentolila, S., Jansen, M., and G. Jimenez, 2017. When credit dries up: Job losses in the Great Recession. *Journal of the European Economic Association*, forthcoming.

[8] Berger, A., and G. Udell, 1995. Relationship lending and lines of credit in small business finance. *Journal of Business*, 68, 351-381.

[9] Bertola, G., 1999. Microeconomic perspectives on aggregate labor markets. In *Handbook of Labor Economics*, Ashenfelter, O., and D. Card (Eds.): North Holland, Elsevier.

[10] Black, D., Smith, I., Berger, M., and B. Noel, 2003. Is the threat of training more effective than training itself? Evidence from random assignments in the UI system. *American Economic Review*, 93, 1313–1327.

[11] Blanchard, O., and P. Portugal, 2001. What hides behind an unemployment rate: Comparing Portuguese and U.S. labor markets, *American Economic Review* 91, 187–207.

[12] Blanchard, O., and J. Wolfers, 2000. The role of shocks and institutions in the rise of European unemployment: The aggregate evidence. *Economic Journal*, 110, 1–33.

[13] Boeri, T., 2011. Institutional reforms and dualism in European labor markets. In *Handbook of Labor Economics*, Ashenfelter, O., and D. Card (Eds.): North Holland, Elsevier.

[14] Boeri, T., Garibaldi, P., and E. Moen, 2012. The labor market consequences of adverse financial shocks. IZA Discussion Paper 6826.

[15] Boeri, T., and J. Jimeno, 2005. The effects of employment protection: Learning from variable enforcement. *European Economic Review*, 49, 2057–2077.

[16] Bover, O., P. Garcia-Perea, and P. Portugal, 2000. Labor market outliers: Lessons from Portugal and Spain. *Economic Policy* 15, 379–428.

[17] Caballero, R., and M. Hammour, 1994. The cleansing effect of recessions. *American Economic Review*, 84, 1350–1368.

[18] Caballero, R., and M. Hammour, 1998. Jobless growth: Appropriability, factor substitution and unemployment, *Carnegie-Rochester Conference Series on Public Policy*, 48, 51–94.

[19] Campello, M., Graham, J., and C. Harvey, 2010. The real effects of financial constraints: Evidence from a financial crisis. *Journal of Financial Economics* 97, 470–487.

[20] Card, D., Chetty, R., and A. Weber, 2007. The spike at benefit exhaustion: Leaving the unemployment system or starting a new job? *American Economic Review*, 97, 113–118.

[21] Chodorow-Reich, G., 2014. The employment effects of credit market disruptions: Firm-level evidence from the 2008-09 financial crisis. *Quarterly Journal of Economics* 129, 1–59.

[22] Cingano, F., Leonardi, M., Messina, J., and G. Pica, 2016. Employment protection legislation, capital investment and access to credit: Evidence from Italy. *Economic Journal* 126, 1798–1822.

[23] Davis, S., and J. Haltiwanger, J., 1990. Gross job creation and destruction: Microeconomic evidence and macroeconomic implications. *NBER Macroeconomic Annual*, 5, 123–168.

[24] Davis, S., and J. Haltiwanger, J., 1992. Gross job creation, gross job destruction, and employment reallocation. *Quarterly Journal of Economics*, 107, 819–863.

[25] Dolton, P., and D. O'Neill, 2002. The long-run effects of unemployment monitoring and work-search programs: experimental evidence from the United Kingdom. *Journal of Labor Economics*, 20, 381–403.

[26] Dustmann, C., Fitzenberger, B., Schoenberg, U., and A. Spitz-Oener, 2014. From sick man of Europe to economic superstar: Germany's resurgent economy. *Journal of Economic Perspectives* 28, 167–188.

[27] Duygan-Bump, B., A. Levkov, and J. Montoriol-Garriga, 2015. Financing constraints and unemployment: Evidence from the Great Recession. *Journal of Monetary Economics*, 75, 89–105.

[28] Engellandt, A., and R. Riphahn, 2005. Temporary contracts and employee effort. *Labour Economics* 12, 281–299.

[29] Falato, A., and N. Liang, 2017. Do creditor rights increase employment risk? Evidence from loan covenants. *Journal of Finance*, forthcoming.

[30] Garibaldi, P., and G. Violante, 2005. The employment effects of severance payments with wage rigidities. *Economic Journal*, 115, 799–832.

[31] Garicano, L., LeLarge, C., and J. Van Reenen, 2015. Firm size distortions and the productivity distribution: Evidence from France. *American Economic Review*, 106, 3439–3479.

[32] Greenstone, M., and A. Mas, 2012. Do credit market shocks affect the real economy? Quasi-experimental evidence from the Great Recession and 'normal' economic times. MIT Department of Economics Working Paper 12–27.

[33] Hanka, G., 1998. Debt and the terms of employment. *Journal of Financial Economics* 48, 245–282.

[34] Heckman, J., LaLonde, R., and I. Smith, 1999. The economics and econometrics of active labor market programs. In: Ashenfelter, O., Card, D. (Eds.), *Handbook of Labor Economics*, pp. 1865–2097. Elsevier: Amsterdam.

[35] Hicks, J. R. 1932. *The Theory of Wages*. London: Macmillan.

[36] Ichino, A., and R. Riphahn, 2005. The effect of employment protection on worker effort: Absenteeism during and after probation. *Journal of the European Economic Association*, 3, 120–143.

[37] Jimenez, G., Ongena, S., Peydro, J.-L., and J. Saurina, 2012. Credit supply and monetary policy: Identifying the bank balance-sheet channel with loan applications. *American Economic Review*, 102, 2301–2326.

[38] Jorgenson, D., Ho, M., and J. Samuels, 2012. A prototype industry-level production account for the United States, 1947-2010. Technical report, Second World KLEMS Conference, Harvard University.

[39] Kalemli-Özcan, Ş., Laeven, L., and D. Moreno, 2015. Debt overhang in Europe: Evidence from firm-bank-sovereign linkages. University of Mariland mimeo.

[40] Klump, R., P. McAdam, and A. Willman, 2007. Factor substitution and factor augmenting technical progress in the US. *Review of Economics and Statistics*, 89, 183–92.

[41] Klump, R., P. McAdam, and A. Willman, 2012. The normalized CES production function: Theory and empirics. *Journal of Economic Surveys*, 26, 769–799.

[42] Kluve, I., and C. Schmidt, 2002. Can training and employment subsidies combat European unemployment? *Economic Policy*, 35, 411–448.

[43] Krueger, A., and B. Meyer, 2002. Labor supply effects of social insurance. In: Auerbach, A., Feldstein, M. (Eds.), *Handbook of Public Economics*. Elsevier: Amsterdam.

[44] León-Ledesma, M., P. McAdam, and A. Willman, 2010. Identifying the elasticity of substitution with biased technical change. *American Economic Review* ,100, 1330–1357.

[45] Lichtenberg, F., and D. Siegel, 1990. The effects of leveraged buyouts on productivity and related aspects of firm behaviour. *Journal of Financial Economics*, 27, 165–194.

[46] Marinescu, I., 2009. Job security legislation and job duration: Evidence from the UK. *Journal of Labor Economics* 27, 475–486.

[47] Messina, J., and J. Valanti, 2007. Job flow dynamics and firing restrictions: Evidence from Europe. *Economic Journal*, 117, 279–301.

[48] Nickell, S., 1997. Unemployment and labor market rigidities: Europe versus North America. *Journal of Economic Perspectives* 11, 55–74.

[49] Nickell, S., and D. Nicolitsas, 1999. How does financial pressure affect firms? *European Economic Review* 43, 1435–1456.

[50] O'Mahony, M., and M. Timmer, 2009. Output, input and productivity measures at the industry level: The EU KLEMS Database. *Economic Journal*, 119, 374–403.

[51] Pagano, M., and G. Pica, 2012. Finance and employment. *Economic Policy* 27, 5–55.

[52] Petersen, M., 2009. Estimating standard errors in finance panel data sets: Comparing approaches. *Review of Financial Studies* 22, 435–480.

[53] Popov, A., and J. Rocholl, 2017. Do credit shocks affect labor demand? Evidence for employment and wages during the financial crisis. *Journal of Financial Intermediation*, forthcoming.

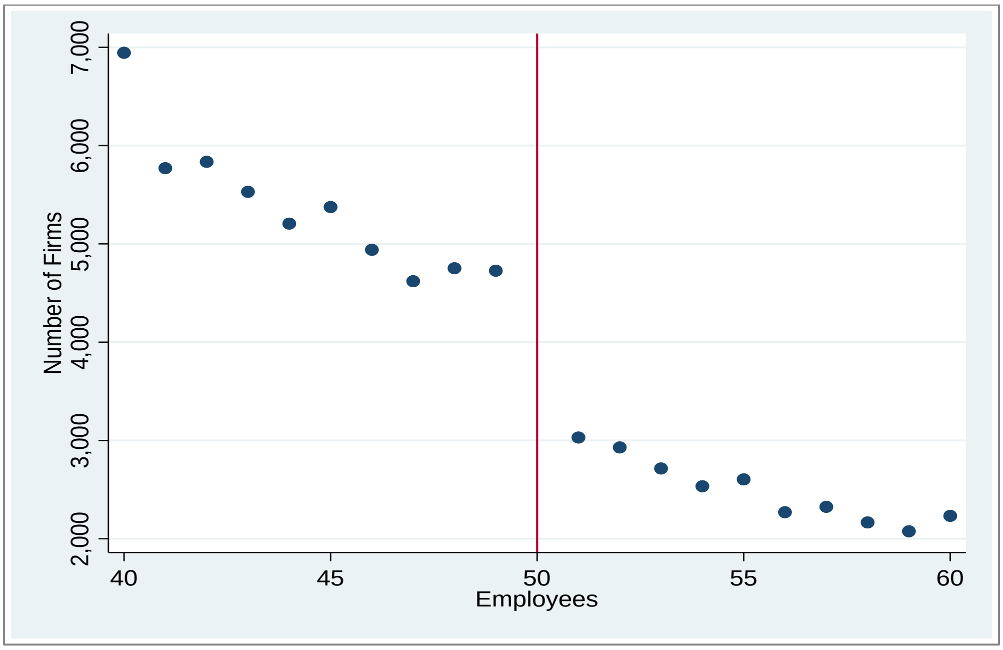
[54] Popov, A., and G. Udell, 2012. Cross-border banking, credit access, and the financial crisis. *Journal of International Economics*, 87, 147–161.

[55] Rajan, R., and L. Zingales, 1998. Financial dependence and growth. *American Economic Review* 88, 559–586.

[56] Schivardi, F., and R. Torrini, 2008. Identifying the effects of firing restrictions through size-contingent differences in regulation, *Labour Economics* 15, 482–511.

[57] Sharpe, S., 1994. Financial market imperfections, firm leverage, and the cyclicality of employment. *American Economic Review* 84, 1060–1074.

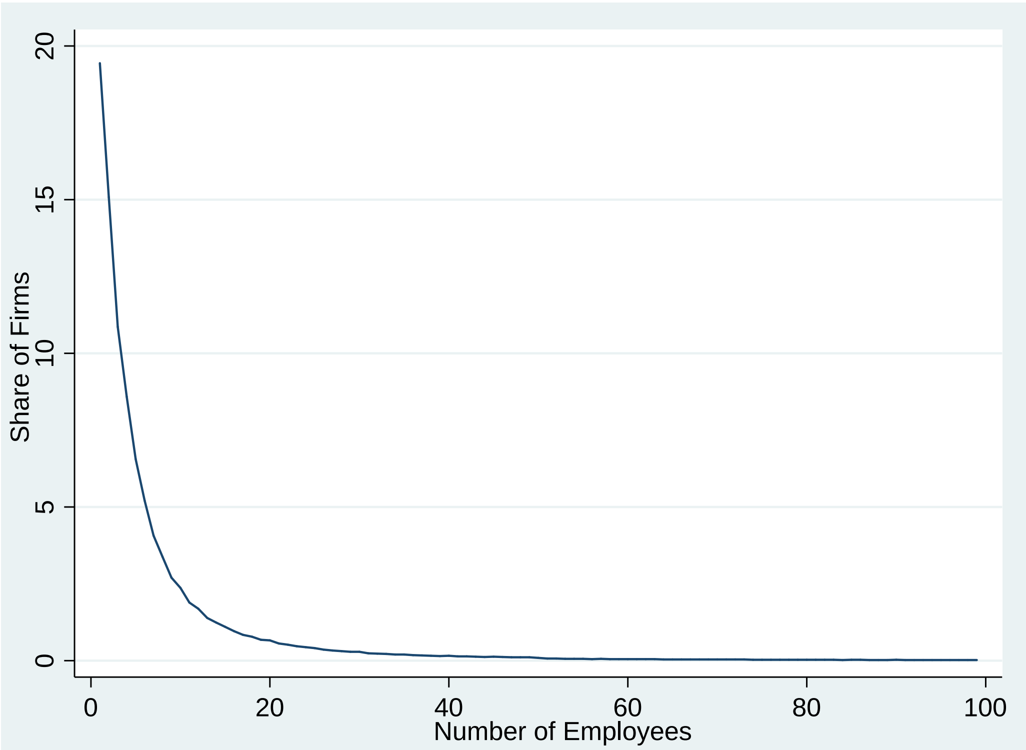
Figure 1 A. Distribution of firms, by number of employees



Note: This Figure plots the number of firms in the sample, by number of employees, for firms with between 40 and 60 employees.

Source: Orbis, sample period is 2006—2009.

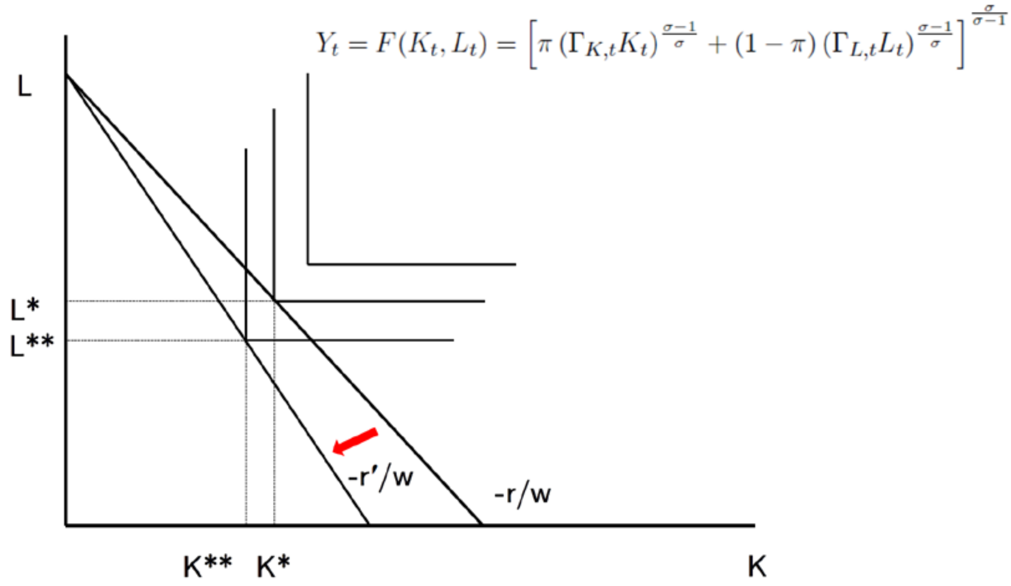
Figure 1 B. Share of firms, by number of employees



Note: This Figure plots the share of firms in the sample, by number of employees.

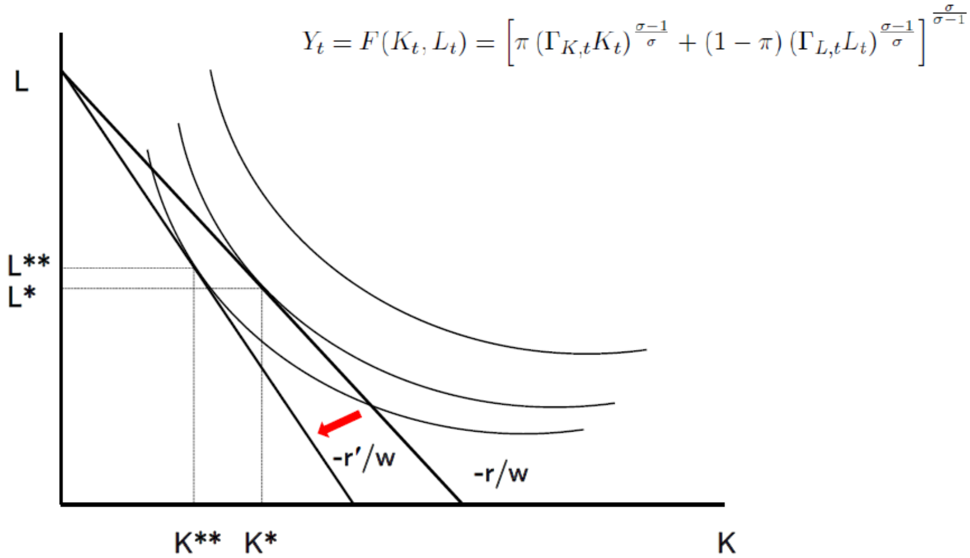
Source: Orbis, sample period is 2006—2009.

Figure 2. Factor adjustment in response to a rise in the user cost of capital: Leontief case



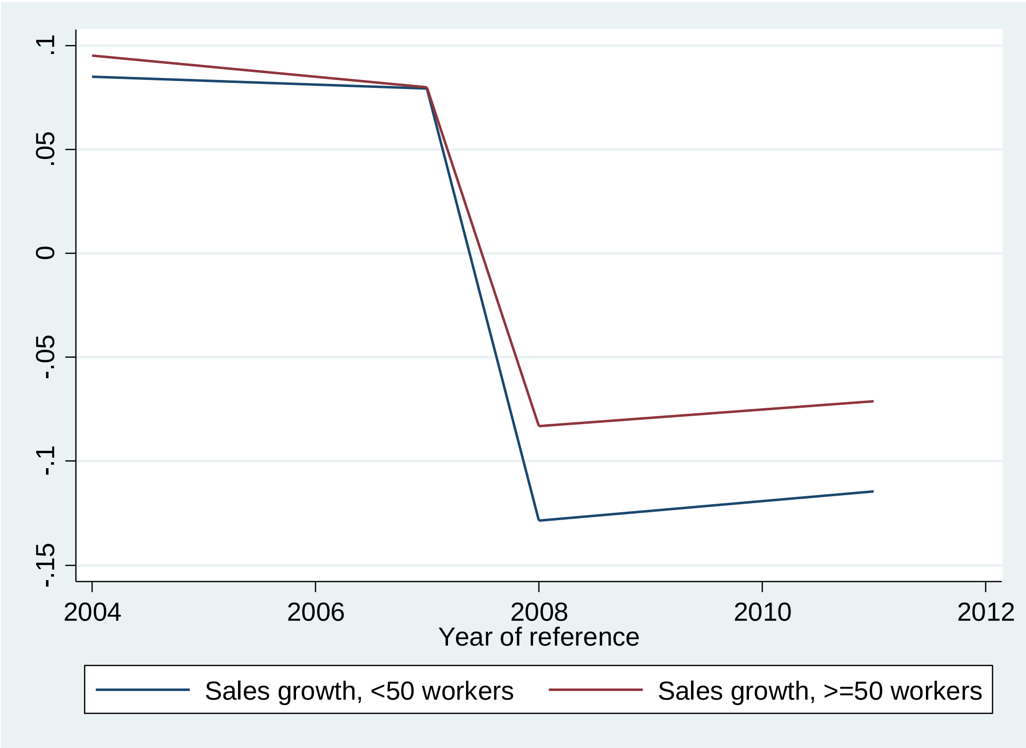
Note: This Figure graphs the factor adjustment and corresponding change in output following an increase in the user cost of capital, for a Leontief production function.

Figure 3. Factor adjustment in response to a rise in the user cost of capital: Standard Sigma case



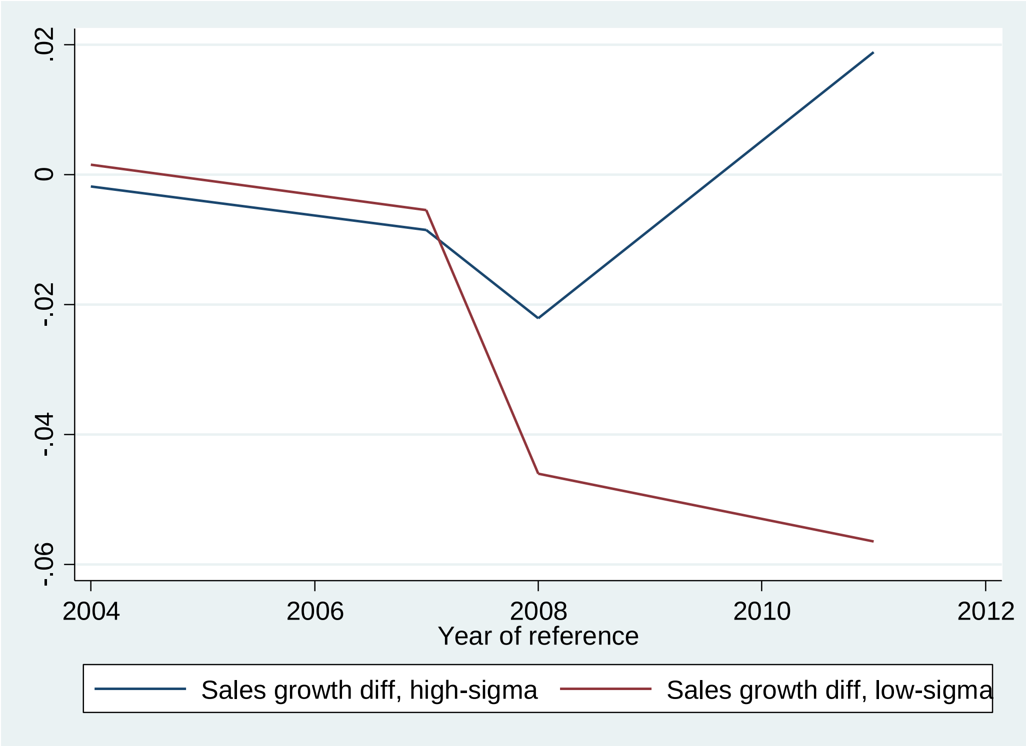
Note: This Figure graphs the factor adjustment and corresponding change in output following an increase in the user cost of capital, for a standard-sigma production function (i.e., where the elasticity is between the Leontief and Linear cases).

Figure 4. Credit constrained small versus large firms, before and after the crisis



Note: 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 5. Credit constrained small versus large firms, before and after the crisis, in high-sigma versus low-sigma industries



Note: 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	485,822	-0.032	-0.036	0.392	-1.000	1.000
Employment growth	406,078	-0.007	0.000	0.248	-0.999	0.999
Investment growth	456,166	-0.052	-0.067	0.302	-1.000	1.000
<50 employees	442,452	0.894	1.000	0.308	0.000	1.000
<10 employees	442,452	0.435	0.000	0.496	0.000	1.000
Log (Sales)	522,579	18.596	18.423	3.090	2.308	28.459
Cash flow / Assets	523,049	0.065	0.057	0.116	-1.608	0.991
Net worth / Assets	511,125	0.314	0.293	0.345	-9.529	1.000
Firm-bank level						
Shock	526,268	0.260	0.000	0.439	0.000	1.000
Industry-level						
Sigma	526,268	1.054	0.860	0.502	0.360	1.960
External dependence	526,268	0.131	0.100	0.268	-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. '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. '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)
Shock	-0.0093**	0.0154**	-0.0101	0.0440***	0.0498***
	(0.0040)	(0.0064)	(0.0084)	(0.0128)	(0.0130)
Shock × <50 employees		-0.0263***		-0.0581***	-0.0571***
		(0.0079)		(0.0138)	(0.0130)
Shock × Sigma			0.0009	-0.0275**	-0.0211**
			(0.0070)	(0.0116)	(0.0092)
Shock × <50 employees × Sigma				0.0306***	0.0213***
				(0.0093)	(0.0080)
Log (Sales)					-0.3522***
					(0.0107)
Cash flow / Assets					-0.0061
					(0.0206)
Net worth / Assets					0.0549***
					(0.0200)
Firm FEs	Yes	Yes	Yes	Yes	Yes
Sector × Year FEs	Yes	Yes	Yes	Yes	Yes
Observations	485,822	410,793	485,822	410,793	400,148
R-squared	0.17	0.17	0.17	0.17	0.31

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 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. 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 level are reported in parentheses where ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent statistical level, respectively.

Table 4. Falsification tests

	Sales growth		
	2004—2007	Cut-off at 10	Germany
	(1)	(3)	(4)
Shock	0.0039	-0.0005	0.0093
	(0.0097)	(0.0114)	(0.0093)
Shock × <50 employees	0.0078		0.0093
	(0.0121)		(0.0139)
Shock × Sigma	0.0092	-0.0065	-0.0053
	(0.0076)	(0.0072)	(0.0106)
Shock × <50 employees × Sigma	-0.0092		-0.0095
	(0.0074)		(0.0116)
Shock × <10 employees		-0.0021	
		(0.0164)	
Shock × <10 employees × Sigma		0.0134	
		(0.0124)	
Firm controls	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes
Sector × Year FEs	Yes	Yes	Yes
Observations	393,464	400,148	87,177
R-squared	0.26	0.31	0.11

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. '<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 data over the period 1947—2010. All firm controls from Table 1, column (5) are also included in the regression. The sample period is 2004—2007 (column (1)) and 2006—2009 (columns (2)—(3)), and 2007—2010 (column (4)). In column (4), the test is performed on a sample of German firms. Standard errors clustered at the sector level are reported in parentheses where ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent statistical level, respectively.

Table 5. A narrower window around the 50-employee cut-off

	Sales growth		
	30 to 750	35 to 110	38 to 75
	(1)	(2)	(3)
Shock	0.0019	-0.0131	-0.0248
	(0.0120)	(0.0170)	(0.0228)
Shock × <50 employees	-0.0309*	0.0024	0.0195
	(0.0175)	(0.0177)	(0.0196)
Shock × Sigma	0.0001	0.0056	0.0116
	(0.0114)	(0.0128)	(0.0170)
Shock × <50 employees × Sigma	0.0330***	0.0287**	0.0259**
	(0.0124)	(0.0128)	(0.0134)
Firm controls	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes
Sector × Year FEs	Yes	Yes	Yes
Observations	70,616	42,950	28,180
R-squared	0.36	0.37	0.39

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 data over the period 1947—2010. All firm controls from Table 1, column (5) are also included in the regression. In column (1), only firms with between 30 and 750 employees (4% of the sample on each side of 50) are included. In column (2), only firms with between 35 and 110 employees (2.5% of the sample on each side of 50) are included. In column (3), only firms with between 39 and 75 employees (1.5% of the sample on each side of 50) are included. The sample period is 2006—2009. Standard errors clustered at the sector level are reported in parentheses where ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent statistical level, respectively.

Table 6. Robustness

	Sales growth					
	Alternative sigma	Sigma dummy	External dependence	2005—2010	2004—2011	Single-bank firms
	(1)	(2)	(3)	(4)	(5)	(6)
Shock	0.0352***	0.0343***	0.0492***	0.0530***	0.0629***	0.0613**
	(0.0066)	(0.0066)	(0.0149)	(0.0115)	(0.0111)	(0.0298)
Shock × <50 employees	-0.0410***	-0.0419***	-0.0582***	-0.0546***	-0.0626***	-0.0636**
	(0.0071)	(0.0068)	(0.0152)	(0.0105)	(0.0092)	(0.0291)
Shock × Sigma	-0.0039***	-0.0238**	-0.0203	-0.0216***	-0.0272***	-0.0447*
	(0.0014)	(0.0100)	(0.0127)	(0.0072)	(0.0060)	(0.0273)
Shock × <50 employees × Sigma	0.0032***	0.0258***	0.0232**	0.0223***	0.0263***	0.0373*
	(0.0011)	(0.0083)	(0.0131)	(0.0068)	(0.0059)	(0.0261)
Shock × Ext. dependence			-0.0026			
			(0.0253)			
Shock × <50 employees × External dependence			-0.0069			
			(0.0270)			
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes
Sector × Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	400,148	400,148	400,148	592,417	775,149	201,273
R-squared	0.31	0.31	0.31	0.26	0.23	0.18

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 data over the period 1947—2010. All firm controls from Table 1, column (5) are also included in the regression. The sample period is 2006—2008 (columns (1)—(3) and (6)), 2005—2010 (column (4)), and 2004—2011 (column (5)). In column (1), sigma is calculated on a shorter, pre-crisis time period (1970—2007). In column (2), sigma is replaced with a dummy equal to one if sigma is more than 1, and to zero otherwise. In column (6), only firms with a credit relationship with a single bank are included in the regressions. Standard errors clustered at the sector level are reported in parentheses where ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent statistical level, respectively.

Table 7. Distinguishing across pre-crisis firm growth rates

	Sales growth	
	High-growth firms in 2007	Low-growth firms in 2007
	(1)	(2)
Shock	0.0552***	0.0582***
	(0.0154)	(0.0180)
Shock × <50 employees	-0.0680***	-0.0588***
	(0.0135)	(0.0195)
Shock × Sigma	-0.0163	-0.0263*
	(0.0102)	(0.0139)
Shock × <50 employees × Sigma	0.0261***	0.0168
	(0.0105)	(0.0139)
Firm controls	Yes	Yes
Firm FEs	Yes	Yes
Sector × Year FEs	Yes	Yes
Observations	193,286	186,700
R-squared	0.45	0.32

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 data over the period 1947—2010. All firm controls from Table 1, column (5) are also included in the regression. The sample period is 2006—2009. In column (1), only firms with above-median sales growth in 2007 are included. In column (2), only firms with below-median sales growth in 2007 are included. Standard errors clustered at the sector level are reported in parentheses where ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent statistical level, respectively.

Table 8. Channels (Employment and Investment)

	Employment growth	Investment growth
	(1)	(2)
Shock	-0.0097	-0.0085
	(0.0094)	(0.0074)
Shock × <50 employees	0.0024	0.0121**
	(0.0097)	(0.0062)
Shock × Sigma	-0.0195**	-0.0003
	(0.0095)	(0.0076)
Shock × <50 employees × Sigma	0.0245**	-0.0053
	(0.0110)	(0.0053)
Firm controls	Yes	Yes
Firm FEs	Yes	Yes
Sector × Year FEs	Yes	Yes
Observations	397,928	381,297
R-squared	0.29	0.41

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 data over the period 1947—2010. All firm controls from Table 1, column (5) are also included in the regression. The sample period is 2006—2009. Standard errors clustered at the sector level are reported in parentheses where ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent statistical level, respectively.

Appendix Table 1.

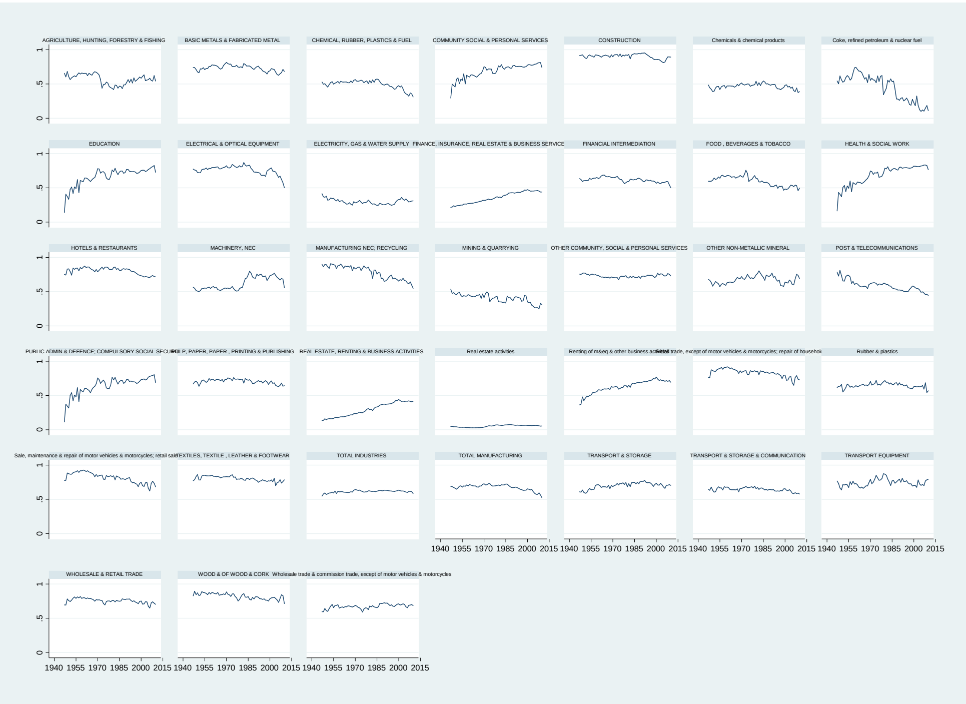
Credit Institution	Included in Bank Group	Date
BBVA Privanza Banco SA- Madrid	Banco Bilbao Vizcaya Argentaria SA	
Banco Depositario BBVA- Madrid		
BBVA Senior Finance SAU- Bilbao		
Catalunya Banc SA- Barcelona		March 2010
BBVA Banco de Financiacion SA- Bilbao		
Caixa d'Estalvis de Manresa-Caixa Manresa- 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		December 2011
Banco Gallego SA- Santiago De Compostela		April 2013
Sabadell International Equity Ltd-		
Caja de Ahorros del Mediterraneo CAM- Alicante		December 2011
Banco Urquijo Sabadell Banca Privada SA- Madrid		
BFA Tenedora de Acciones SAU- Madrid		

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		November 2012
Caja de Ahorros Provincial de Guadalajara-Caja de Guadalajara- Guadalajara		December 2010
Monte de Piedad y Caja de Ahorros San Fernando de Guadalajara Huelva Jerez y Sevilla-Cajasol- Sevilla		December 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		
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		November 2009
Caja de Ahorros y Monte de Piedad de Extremadura-Caja de Extremadura- Caceres		April 2011
Caja de Ahorros de Castilla La Mancha- Cuenca		November 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: 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.

Figure 4. Labor Income Share by Sector



A 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.[14] Consider that real output Y for a given sector can be described by the 'normalized' CES production function,[15]

$$Y_t = \left[\pi_z \left(\Gamma_t^K K_t\right)^{\frac{\sigma-1}{\sigma}} + (1-\pi_z)\left(\Gamma_t^L L_t\right)^{\frac{\sigma-1}{\sigma}}\right]^{\frac{\sigma}{\sigma-1}} \tag{A.1}$$

where as before $\sigma \in [0, \infty)$ is the elasticity of substitution between the real capital stock and the labor input. Distribution parameter $\pi_z \in (0,1)$ equals the capital income share at the point of normalization: $\pi_z = 1 - \frac{w_z N_z}{Y_z}$, where w_z denotes the real wage rate at the normalization point (in our main text, we suppressed the 'z' subscript for simplicity). The term Γ_t^J capture the level of technical progress associated to the j^{th} factor over time t. As is standard in the literature we assume that the rates of factor augmentation are given by $\Gamma_t^j = e^{\gamma_t^j (t-t_0)}$ where t_0 is the arithmetic mean of the sample length and γ_t^j is the average growth rate of technical progress associated to factor j.[16]

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

$$\tau_{L,t} = (1-\pi_z)\left(\Gamma_t^L \frac{L_t}{Y_t}\right)^{\frac{\sigma-1}{\sigma}} \tag{A.2}$$

$$\tau_{K,t} = \pi_z \left(\Gamma_t^K \frac{K_t}{Y_t}\right)^{\frac{\sigma-1}{\sigma}} \tag{A.3}$$

Equations (A.1) – (A.3) constitute the non-linear stochastic system to be estimated.

In terms of KLEMs mnemonics, factors are taken from their volume services (LAB_QI

[14] 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.

[15] 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 unnormalized 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.

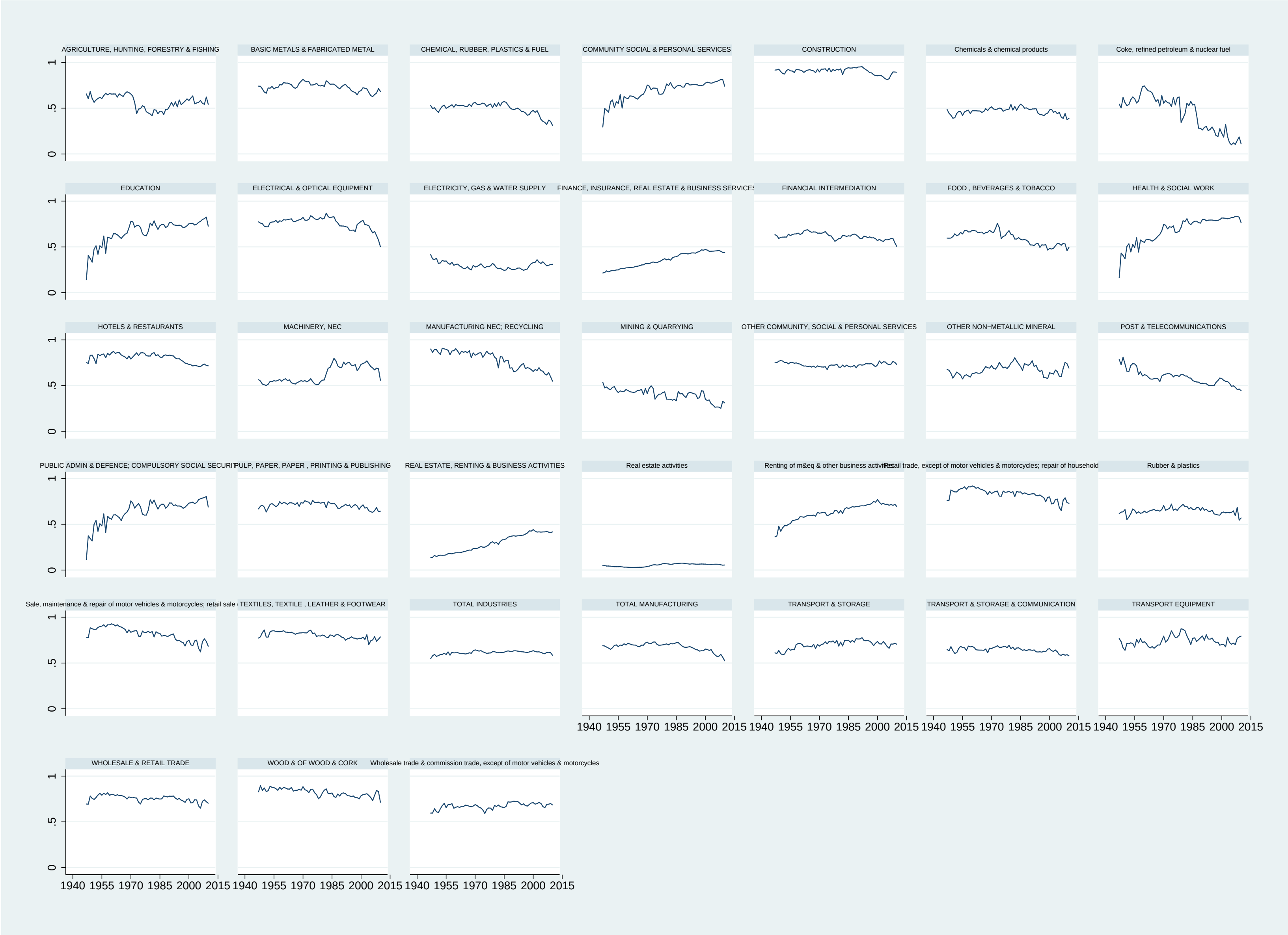
[16] Note, following Klump, McAdam, and Willman (2007) we also estimated all of the sector using a slightly more flexible Box-Cox function for the technical progress components. In some cases, this improved overall model fit but identified elasticity values in the neighborhood of those estimated using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

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.

B Labor Shares Across Sectors

Figure B.1 shows the labor shares by sector taken from the KLEMs $1947-2010$ database. As per the KLEMs database, the capital share is given by one minus the labor share. The pure profit component is therefore subsumed within the capital income share component.

Figure B.1: Labor Share by Sector



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.1), individual factor demands (A.2, A.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).[17]

Table C.1-**Table C.2** present the system parameters, as well as tests of Cobb Douglas ($\sigma = 1$), of Hicks neutrality ($\gamma^L = \gamma^K$) and the size of the technical bias ($\gamma^L - \gamma^K$). 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.

[17] 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

Parameter	Sector											
	AtB	27t28	F	23	30t33	E	JtK	J	15t16	29	36t37	34t35
ζ	0.999***	0.996***	0.966***	1.006***	1.071***	1.053***	1.054***	1.014***	0.968***	0.981***	1.057***	1.034***
	{0.973::1.024}	{0.975::1.016}	{0.925::1.007}	{0.844::1.169}	{0.963::1.179}	{1.011::1.095}	{1.044::1.064}	{0.981::1.047}	{0.940::0.996}	{.936::1.027}	{1.004::1.064}	{1.043::1.118}
σ	1.683***	0.417***	1.926***	0.364***	0.788***	1.264***	0.777***	0.895***	1.585***	0.542***	0.662***	0.642***
	{1.577::1.789}	{0.415::0.420}	{1.900::1.951}	{0.362::0.366}	{0.782::0.794}	{1.247::1.281}	{0.761::0.792}	{0.890::0.900}	{1.548::1.622}	{0.537::0.546}	{0.647::0.676}	{0.630::0.654}
γ^K	0.022***	-0.023***	-0.006	0.013***	0.026***	-0.006***	0.022***	-0.080***	0.010***	-0.016***	-0.037***	-0.019***
	{0.018::0.025}	{-0.025::-0.022}	{-0.014::0.002}	{0.007::0.019}	{0.018::0.035}	{-0.010::-0.003}	{0.019::0.024}	{-0.090::-0.070}	{0.007::0.013}	{-0.018::-0.014}	{-0.042::-0.033}	{-0.023::-0.016}
γ^L	0.030***	0.010***	-0.010***	0.082***	0.117***	-0.006*	-0.039***	0.039***	-0.006***	0.010***	0.045***	0.009***
	{0.028::0.033}	{0.008::0.011}	{-0.014::-0.007}	{0.074::0.090}	{0.102::0.132}	{-0.011::-0.000}	{-0.043::-0.034}	{0.030::0.048}	{-0.009::-0.003}	{0.008::0.013}	{0.043::0.047}	{0.006::0.012}

Notes: Joint system estimation of equations (A.1)-(A.3). Numbers in curly (square) parentheses indicates 95% confidence intervals (probability values). Asterisks denotes significance level based on robust standard errors, where $^{***} < 0.01, ^{**} < 0.05, ^{*} < 0.1$.

Table C.2: Sectoral Production Parameters Cont.

Parameter	Sector										
	26	64	21t22	K	71t74	52	25	17t19	60t63	G	51
ζ	1.000***	1.037***	0.984***	1.093***	1.106***	1.015***	1.008***	1.027***	1.042***	1.005***	1.000***
	{0.981::1.019}	{1.011::1.064}	{0.961::1.007}	{1.081::1.105}	{1.090::1.122}	{0.985::1.045}	{0.984::1.031}	{1.002::1.052}	{1.026::1.059}	{0. 986::1.024}	{0.979::1.020}
σ	0.410***	0.823***	1.825***	0.775***	0.875***	0.747***	0.574***	0.920***	0.919***	0.854***	0.858***
	{0.407::0.413}	{0.793::0.853}	{1.803::1.848}	{0.755::0.794}	{0.844::0.906}	{0.733::0.761}	{0.571::0.577}	{0.891::0.950}	{0.905::0.934}	{0. 847::0.860}	{0.849::0.866}
γ^K	-0.012***	-0.049***	-0.028***	0.029***	0.048***	-0.058***	-0.002**	-0.091***	0.090***	-0.044***	0.012***
	{-0.013::-0.011}	{-0.058::-0.041}	{-0.030::-0.027}	{0.025::0.032}	{0.035::0.060}	{-0.068::-0.048}	{-0.004::-0.001}	{-0.129::-0.052}	{0.077::0.104}	{-0.053::-0.036}	{0.007::0.017}
γ^L	0.013***	0.076***	0.002*	-0.064***	-0.034***	0.030***	0.017***	0.060***	-0.017***	0.042***	0.030***
	{0.012::0.014}	{0.069::0.082}	{0.000::0.004}	{-0.072::-0.056}	{-0.041::-0.028}	{0.028::0.033}	{0.015::0.018}	{0.050::0.069}	{-0.023::-0.012}	{0.039::0.045}	{0.027::0.034}
$\sigma = 1$	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	0.000
$\gamma_L = \gamma_K$	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	0.000
$\gamma_L - \gamma_K$	0.025	0.125	0.031	-0.093	-0.082	0.088	0.019	0.151	-0.108	0.087	0.018
	{0.019::0.026}	{0.110::0.140}	{0.028::0.033}	{-0.105::-0.082}	{-0.101::-0.063}	{0.077::0.100}	{0.017::0.022}	{0.103::0.198}	{-0.127::-0.088}	{0.074::0.099}	{0.010::0.026}

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 (A.1), individual factor demands (A.2, A.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).[18]

An example (for one particular sector) is given in **Table C.3**. 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.

[18]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, e.g., [**?**].

Table C.3: Illustrative Robustness: Sector 25 (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}
$\gamma^{(K)}$	-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}
$\gamma^{(L)}$	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
$rmse(\tau^{(L)})$	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
$rmse(\tau^{(K)})$	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]
$\gamma^{(L)} = \gamma^{(K)}$	[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]
$\gamma^{(L)} - \gamma^{(K)}$	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
	{0.008::0.041}	{0.008::0.041}	{0.008::0.041}	{0.008::0.041}	{0.008::0.041}	{0.013::0.031}	{0.013::0.031}	{0.013::0.031}	{0.013::0.031}	{0.013::0.031}	{0.017::0.022}	{0.017::0.022}	{0.017::0.022}	{0.017::0.022}	{0.017::0.022}

Notes: The terms ll, aic and bic refer to the log likelihood, the Akaike and Bayesian information criteria, respectively, whilst $rmse(Y)$, $rmse(\tau^{(L)})$ and $rmse(\tau^{(K)})$ refer, respectively, to the root mean square error of the fitted values of equations (A.1)-(A.3).