# The Financial Channels of Labor Rigidities: Evidence from Portugal*

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December 2, 2022

**Abstract**

We study how labor rigidities affect firms' responses to credit shocks. Using novel data on the universe of workers, firms, banks and credit in Portugal, we establish three main facts. First, a short-term credit supply shock leads to a decrease in firms' employment and size and to a greater probability of exit, but the effects are concentrated on firms deriving greater value added from labor within their industries. Second, this exposure to liquidity risk stems from exposure to high-skill workers' compensation: the shock disproportionately affects productive firms with a high-skilled specialized labor force that requires greater investment in on-the-job training. Third, given labor costs exposure, productivity does not attenuate the effects of credit shocks. Our findings suggest that labor rigidities are an important driver of the lack of productivity-enhancing reallocation throughout financial crises.

*JEL: D24, D25, E24, G21, G30. Edoardo M. Acabbi thanks Gabriel Chodorow-Reich, Emmanuel Farhi, Samuel G. Hanson and Jeremy Stein for their advice. The authors thank Filipe Correia, Isaac Hacamo and Abhinav Gupta for their thoughtful discussions and Andrea Alati, António Antunes, Omar Barbiero, Diana Bonfim, Andrea Caggese, Andrew Ellul, Andrew Garin, Simon Jäger, Sudipto Karmakar, Lawrence Katz, Chiara Maggi, Carlos Robalo Marques, Luca Mazzone, Steven Ongena, Matteo Paradisi, Pedro Portugal, Gianluca Rinaldi, Martina Uccioli, Boris Vallé and Silvia Vannutelli for their comments. The authors also thank Paulo Guimarães, Pedro Próspero, Marta Silva, Fátima Teodoro, Maria Lucena Vieira, Sujiao Zhao and the staff of the "Laboratório de Investigação com Microdados" at Banco de Portugal (BPLim). Pedro Moreira and Antonio Santos provided excellent research assistance. Edoardo Acabbi gratefully acknowledges financial support from the Comunidad de Madrid (Programa Excelencia para el Profesorado Universitario, convenio con Universidad Carlos III de Madrid, V Plan Regional de Investigación Científica e Innovación Tecnológica), the Spanish Ministry of Science and Innovation, project PID2020-114108GB-I00, and the Fundación Ramón Areces. The analyses, opinions and findings of this paper represent the views of the authors, and are not necessarily those of Banco de Portugal or the Eurosystem.
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# 1 Introduction

Why do some non-productive firms survive through a crisis, and why some very productive firms exit the market? Studying the interaction between firms' labor costs structure and credit shocks throughout a financial crisis can help explain these findings.

Labor costs constitute a sizable fraction of firms cost structure. In advanced economies the labor share, albeit declining, is still above 60 percent of value added (Karabarbounis and Neiman, 2014). Despite their large share in total costs, labor costs are often overlooked as a source of financial risk, and most macroeconomic models assume that labor is a flexible input in production. However, labor adjustment costs can amplify the effect of credit shocks and put firms' operations at risk for at least two reasons.

First, the possible mismatch in the timing of payments to workers and the cash flows from production makes firms subject to a working capital financing channel. In fact, the need on the part of firms to obtain liquidity to pay salaries directly expose their employment decisions to variations in the costs of external financing. An increase in credit spreads, by giving rise to a credit supply shock, increases the marginal costs of hiring, thus potentially leading to hiring and investment cuts. Second, firms frequently hire specialized human capital. Training, search and hiring costs make these kinds of workers a quasi-fixed factor of production (Oi, 1962) with inflexible labor costs. The rigidity of compensation owed to incumbent workers in the short run creates operating leverage, which tends to amplify the effects of liquidity shortfalls firms might be exposed to.

Understanding the interaction between labor costs adjustment and financial frictions is important, as it provides insights into the potential effect on allocative efficiency of financial crises. Recessions have in principle the positive byproduct of "cleansing" low-productivity firms out of the market, thus improving the allocation of resources (Schumpeter, 1942). However, financial frictions might attenuate or even reverse this effect, even turning it into a "sullying" one (Barlevy, 2003, Ouyang, 2009). [1]

In this study we document how firms' financial flexibility, and in particular their ability to adjust their labor costs, determines their responsiveness to liquidity shocks and in turn affects their economic performance. In order to do so, we analyze the impact and the propagation of the global interbank market freeze in 2008 on labor market reallocation, real activity and firm survival in Portugal. The freeze in the interbank market led to a sharp decrease in the supply of short-term liquidity to Portuguese firms. This event repre-

[1] Along those lines, Foster et al. (2016) observe that the Great Recession featured less productivity-enhancing inputs' reallocation and a weaker cleansing effect in firms' exit.

sents a unique opportunity to analyze the real effect of credit shocks and their interaction with labor-market rigidities for two reasons. First, the failure of Lehman Brothers was sudden and unexpected, and exogenous to the Portuguese economy. Second, the event led to a considerable dry-up of the interbank market, which Portuguese banks heavily relied upon to finance their corporate short-term credit. Since Portuguese firms are highly dependent on bank credit (especially short-term) to cover their labor costs, the shock has a strong potential to generate sizable real effects.

We combine detailed administrative data on banks and firms balance sheets with a matched employer-employee dataset and a credit register covering the universe of banks loans in Portugal to trace out the heterogeneous propagation of the credit shock across the corporate sector. To isolate the effect of the interaction between credit shocks and labor costs rigidity, we exploit two sources of variation at the firm level. First, we leverage quasi-exogenous exposure of firms to the interbank market freeze depending on the banks they hold loans with using a shift-share instrument for credit growth. Thus, we compare otherwise similar firms who are hit by shocks of different magnitude depending on the banks that finance them. Second, we look at the heterogeneity of the impact of the shock depending on firms' (pre-determined) labor share, thus comparing firms at the opposite spectrum of the labor share distribution, while controlling for other firms' characteristics, including productivity. This design is similar in spirit to a triple difference-in-differences design, where we compare firms that are differently exposed to a credit shock and then differentiate the impact by the extent of labor costs.

We document three sets of results. First, the credit shock has significant effects on employment dynamics and on the likelihood of firms' survival. Firms hit by the credit shock decrease employment, assets and experience a greater probability of exit. Second and more importantly, the estimated average effects hide substantial heterogeneity across firms. Effects are concentrated among firms with a higher labor share, whereas firms on the opposite side of the labor share distribution are unaffected by the credit shock. Interestingly, we observe the largest negative treatment effects for productive labor-intensive firms that employ a more specialized workforce and correspondingly offer more generous compensations, thus suggesting that the effects might be correlated with the investment in workers' human capital. We dig deeper into these dynamics and find that firms with a workforce that requires high level of on-the-job training decrease labor more and exit with a relatively higher probability. Third, the credit shock has a potentially disruptive effect on the productivity distribution, as we do not detect any heterogeneous effects across differently productive firms on average. Our findings highlight that the disruption in the operations of constrained firms triggered by the credit shock could be so severe that more labor-intensive firms are also more likely to fail, irrespective of their productivity ranking. As we show that banks reduce credit supply *similarly* across firms regardless of their productivity and observable characteristics, the dynamics that we identify are consistent with a "non-cleansing" effect of credit shocks at the micro level. We do not observe a strengthening of productivity-enhancing reallocation dynamics as a consequence of the shock.

The credit shock explains 29 percent of the employment loss among large Portuguese firms between 2008 and 2013, and the burden of the estimated loss entirely falls on firms with relatively greater exposure to employee-related expenditures in their cost structure. The negative shock also exacerbates labor misallocation at the firm level, thus impairing productive labor reallocation in the economy. By conducting an aggregate productivity growth accounting exercise, we find that our shock explains approximately 4.3 percent of the overall deterioration in productivity during the period of analysis, entirely through labor mis-allocation. Moreover, the rigidities in labor adjustments seem to disproportionately harm younger cohorts of workers, who suffer a greater likelihood of undergoing job separations (Caggese et al., 2019). This result points to an important potential and understudied source of productivity losses in the long run, since younger generations accumulate less human capital (Acabbi et al., 2022).

Overall, our findings indicate that credit shocks tend to increase mis-allocation, weaken productivity growth and diminish the cleansing effect of recessions. We attribute these effects to the presence of financial frictions, which we identify as driven by the financial channels of labor rigidities.

**Contribution to the Literature** Our work contributes to several strands of the literature. First, we complement the literature on firm dynamics along the business cycle and the cleansing properties of recessions (Davis and Haltiwanger, 1990, Barlevy, 2003, Ouyang, 2009, Haltiwanger et al., 2022).[2] To the best of our knowledge, our work provides the first causally identified analysis of the impact of a negative credit supply shock on aggregate misallocation.[3] Furthermore, we are the first to show the existence of a perverse non-cleansing selection mechanism in firm exit and inputs reallocation, which we show is

[2]Davis and Haltiwanger (1990, 1992) confirmed the existence of a cleansing effect of recessions by analyzing job flows and firm dynamics using US Census data up to the mid-90s. In contrast, some authors such as Barlevy (2003), Ouyang (2009), Osotimehin and Pappadà (2015) and Kehrig (2015), question the unconditional existence of the cleansing effect, and argue that financial frictions might attenuate or even reverse it, possibly even turning it into a "sullying" effect.

[3]A similar approach is adopted in recent studies by Bai et al. (2018), Fonseca and Van Doornik (2022) and Bau and Matray (2022).

related to the degree of labor rigidities measured at the firm level.

Second, our research relates to the analysis of the propagation of financial shocks through banks' credit supply (Peek and Rosengren (2000), Khwaja and Mian (2008), Ivashina and Scharfstein (2010) and especially Chodorow-Reich (2014)) [4] We contribute to this line of research by causally identifying the propagation of a credit shock on large firms in a small open economy, providing results on firms' propensities to adjust the employment of different kinds of workers and the reaction of related investment decisions at the firm level as a function of firms' own cost structure.We highlight that the effects of a short-term credit shock on employment growth and firms' survival are heterogeneous, and give rise to interesting dynamics depending on firms' endogenous exposure to liquidity risk.[5]

Third, our work is related to the literature analyzing the implications for firms of inflexibility in labor inputs (Oi, 1962, Hamermesh, 1989, Hamermesh and Pfann, 1996, Benmelech et al., 2019). A recent strand of studies has focused on the increasing importance of analyzing the financing of labor (Danthine and Donaldson, 2002, Simintzi et al., 2015, Serfling, 2016, Caggese et al., 2019, Ellul and Pagano, 2019, Donangelo et al., 2019, Favilukis et al., 2020, Baghai et al., 2021).[6] We contribute to these lines of research by showing how labor rigidities become a sizable constraint on firms' internal funds usage vis-a-vis a liquidity shortage, through a direct interaction between financial frictions and labor adjustment costs. [7] By showing that the labor rigidity channel matters particularly for firms with a high-skilled workforce, we also relate to the literature analyzing the relevance of firm-specific or general human capital in turnover decisions (Oi, 1962, Becker, 1962, Jovanovic, 1979*a*,*b*), rent-sharing within the firm (Guiso et al., 2013, Card et al., 2017, Kline et al., 2019), and workers' substitutability (Jäger, 2020, Garin and Silverio, 2018).

[4]For more recent studies along the same line of research, see Pagano and Pica (2012) and Jermann and Quadrini (2012), who provide a theoretical analysis of the working-capital financial propagation channel, and Paravisini et al. (2015), Bentolila et al. (2017), Giroud and Mueller (2017), Bottero et al. (2018), Amiti and Weinstein (2018), Blattner et al. (2019), and Barrot et al. (2019) for empirical works.

[5]We are among the first to document how credit supply matters for firms' liquidity management especially with respect to the financing of heterogeneous workers, alongside Berton et al. (2018), Moser et al. (2020), Adamopoulou et al. (2020), Barbosa et al. (2020) and Sforza and Acabbi (2022).

[6]See Matsa (2019) and Pagano (2019) for a review of the literature regarding the relationship between labor composition and firm financing.

[7]Schoefer (2022) provides one of the first instances of a theoretical setting in which business cycles amplification is modeled to stem from rigidities in *incumbents'* wage rigidity. Faia and Pezone (2020) show how heterogeneous renewal timings of collective bargaining agreements has substantial effects on firms' employment and hiring policies.

# 2 Data and sample selection

We start with a summary of the dynamics of Portugal's economy around 2008–2009. Then, we describe the data and the sample of analysis, and report some descriptive statistics relative to firm and workforce characteristics. We refer the reader to Appendix C for a more detailed description of each dataset and the sample selection criteria.

## 2.1 The global financial crisis in Portugal

We use Portugal during the global financial crisis as a laboratory. We focus on the firm response to variations in short-term credit supply around the end of 2008, when the US investment bank and global financial services firm Lehman Brothers filed for bankruptcy, thus initiating a global financial crisis that spread internationally through the banking system and financial networks.

The global financial crisis and the ensuing credit shock to the Portuguese banking system feature some peculiar characteristics, which make them particularly suitable to isolate the mechanism we are after. First, Portugal is a small open economy, and the credit shock arguably originated outside of its own economy. Thus, conditional on controlling for possible endogeneity or selection in banks' portfolios, our setting offers the best conditions to causally identify the real effects of an exogenous credit shock via banks. Second, the drop in bank credit is mostly driven by short-term credit. This is particularly interesting for the analysis of employment decisions and firm dynamics, as such shortages are likely to be unexpected to firms, and directly related to their day-to-day liquidity management. Given the need to smooth liquidity mismatches between cash-flows and revenues, short-term credit is commonly used to finance current expenditures, such as stipends, labor costs and in general working capital, and a shortage would likely impair a firm' s smooth functioning.[8]Third, the Portuguese economy is characterized by medium- and small-sized firms, heavily reliant on bank credit. Portuguese firms are in general not able to access alternative means of financing, as very few of them are able to issue bonds. Moreover, they are likely to be involved in relationship lending with their banks, which makes it difficult for them to switch to different banks in case of shocks.[9]

[8]See the early 2009 editions of the ECB "Survey on the Access to Finance of Small and Medium Enterprises" (SAFE, ECB, Sep 2009*b*) and the Banco de Portugal "Bank Lending Survey" (BLS, Banco de Portugal, Jan 2009) for information on firms' use of banks' short-term debt, the tightening of credit standards and the rise in working capital financing at the onset of the global financial crisis. Our empirical analysis confirms these findings.

[9]See Bonfim and Dai (2017) for evidence on relationship lending in Portugal. Iyer et al. (2014) document

Before the end of 2008, the Portuguese economy did not suffer from the global financial crisis directly, but rather through indirect channels, such as the collapse in global export demand (Garin and Silverio, 2018). Moreover, unlike the United States or Spain, Portugal did not suffer from the burst of a real estate bubble (Fradique Lourenço and Rodrigues, 2015), had in place regulations discouraging the set-up of off-balance-sheet vehicles for banks which could have been used to get exposure to US commercial papers and subprime lending (Acharya and Schnabl, 2010), and featured a stable if not mildly increasing aggregate credit supply (see Appendix Figure F.1).

The failure of Lehman Brothers in September 2008 led to a worldwide confidence crisis in the banking sector, and to a dramatic decrease in the liquidity available to the Portuguese financial sector. At that time, Portuguese banks relied heavily on very short-term interbank loans for financing and managing their day-to-day liquidity needs.[10] Liquidity suddenly dried up, as these financial instruments were often unsecured and the market for them was based on trust across financial institutions. These facts determined a collapse in the volume of funds exchanged. Figure 1b reports the aggregate volume of foreign interbank liabilities in the Portuguese banking system, measured as the sum of short-term deposits (up to 1 year) and repos where the counterparty is a foreign financial institution (excluding central banks). The volume of credit intermediated started shrinking in 2007, but the fall substantially accelerated after 2008, so that by 2013 the total volume was approximately 40 percent of its peak 2007 value.[11]

Given the inability to obtain liquidity for their day-to-day operations in a period of global financial turmoil, banks around the world increased spreads and haircuts, and reduced the amount of credit supplied to the real economy and non-financial businesses, as shown for instance by the ECB "Bank Lending Surveys" at the time (Banco de Portugal, Jan 2009). Figure 1a shows the aggregate trends for regular (neither overdue nor under renegotiation) short-term (with maturity less than one year, or liquid credit lines with no defined maturity) and foreign short-term interbank funds (for banks in our final sample). Credit supply was still increasing after the first signs of financial distress in 2007, and

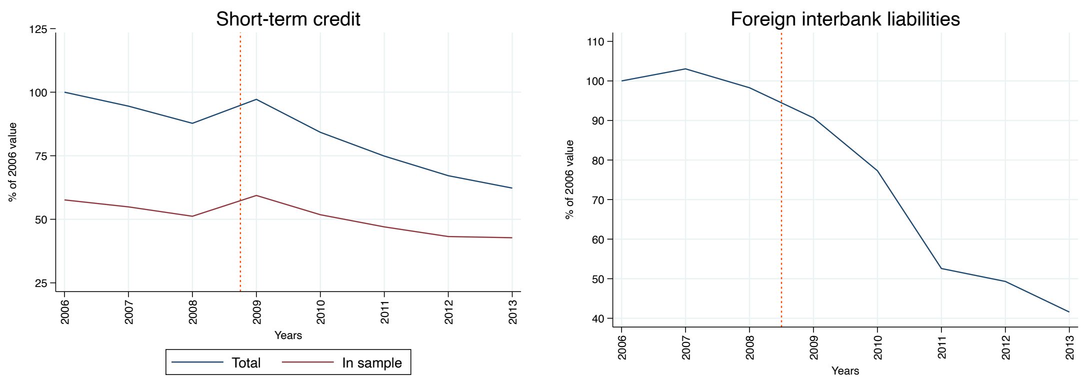
that Portuguese firms in that period did not seem able to compensate for the lost credit supply with other forms of credit.

[10]See for instance Upper (2006). Cocco et al. (2009) show that interbank lending relationships were quite important for the Portuguese banking sector. As regards the dynamics of the dry-up of the interbank funds market, see the ECB Financial Integration Report of April 2009 ECB (Apr 2009*a*).

[11]A similar trend is observed for the overall interbank funds. We focus on foreign interbank exposures because at the time of the failure of Lehman Brothers banks were particularly worried about counterparty risk, and it is plausible to assume that this concern was especially prevalent vis-à-vis foreign counterparties. All our results are robust if in our empirical analysis we consider total interbank funds instead of just foreign ones as measure of exposure.

rapidly fell from 2009 onwards, primarily because of a strong decrease in the supply of short-term credit and credit lines. Overall, from the start of the financial crisis to the end of 2013 the total volume of credit shrank by 30 percent (regular credit, see Appendix Figure F.1a) and 40 percent (short-term credit). In this way, the financial crisis originated in the US spread to the Portuguese real economy.

Figure 1: Credit dynamics in Portugal




(a) Short-term regular credit

(b) Foreign interbank liabilities

The Figures show the time series for the aggregate amount of short-term credit (left) and foreign interbank liabilities (right) for the firms and banks in the sample. Foreign interbank liabilities are the sum of short-term deposits (up to 1 year) and repos where the counterparty is a foreign financial institution (not central banks). Short-term credit is credit with maturity less than one year, or liquid credit lines with no defined maturity. The red dotted line splits the sample in pre-period and post-period. Totals are expressed as a percentage of foreign interbank liabilities (left) and total regular credit (right) in 2006.
Source: *Central de Responsabilidades de Crédito* merged with *Quadros de Pessoal*, authors' calculations and sample selection. Referenced on page(s) [7] and [7] .

## 2.2 Data

Our analysis combines four main datasets: (i) a matched employer-employee dataset *Quadros de Pessoal* (QP), covering the universe of firms and attached workforce in manufacturing and services in Portugal; (ii) a firms' balance-sheet dataset *Central de Balanços* (CB), covering the universe of firms; (iii) a bank-firm matched credit registry *Central de Responsabilidades de Crédito* (CRC), with data at the credit exposure level for the universe of loans; (iv) a banks' balance-sheet dataset (BBS).

QP contains detailed data at the worker and firm level for approximately 350,000 firms and 3 million employees per year. For each firm, the dataset features location, industry, annual revenues, structure of ownership and total employment at the establishment level, and age, gender, occupation, qualification, level of education, type of contract, date of hire and last promotion, hours worked, base stipend and extra compensation at the worker level.

CB consists of a repository of yearly economic and financial information on the universe of non-financial corporations operating in Portugal from 2005 to 2013. It includes information on sales, balance-sheet items, profit and loss statements, and cash flow statements (after 2009). It is the most reliable dataset in terms of coverage of firms active in Portugal, which is why we also use it in our analysis to determine firm exit.

A distinctive feature of the Portuguese data is the possibility of linking the workers' information from QP to measures of credit exposure at the firm level using the credit records. We construct the bank-firm matched credit dataset from the Bank of Portugal' s own credit registry CRC, which features the universe of bank-firm monthly exposures by Portuguese credit institutions. The dataset contains detailed information on the number of credit relationships, the corresponding amounts and the kind of exposure: short- or long-term, credit overdue, written-off or renegotiated.

Finally, we also access one of the Bank of Portugal's proprietary datasets with balance sheets for the universe of financial institutions operating in the country (BBS). For each balance-sheet item it is possible to see the kind of counterparty involved (i.e. the kind of institution, government, private or non-governmental body, creditor or debtor), the maturity of the item in question if relevant (time deposits, on-demand deposits, interbank long-term or short-term exposures) and the nationality of the counterparty (extra-EU or each EU country separately).[12]

### 2.3 Sample selection and descriptives

We combine all the four administrative datasets to obtain a complete picture of firms' and workers' conditions and their linkages to banks through credit. We restrict our attention to firms in mainland Portugal, and exclude the agricultural sector, the fishing sector, the energy sector (extraction, mining and distribution), the construction sector and the financial sector itself. The period covered in our analysis spans from 2005 to 2013. In our empirical exercise we refer to the years between 2006 and 2008 as the "pre-period", and to the years from 2008 to 2013 as the "post-period". To study firms' response to the shock, we consider firms with a credit relationship with any bank in 2005 (before the shock), conditional on their survival until the start of 2009 (after the shock). Moreover, we focus on firms with at least 9 employees, which is the threshold for the fourth quartile (75th

[12]Throughout the analysis we resort to some other minor datasets, either confidential or publicly available. We refer the reader to Appendix C for a more detailed description of all datasets.

percentile) in the distribution of firm size in the years before 2009, and covers more than 60 percent of the workforce in the QP in the pre-period.[13] Finally, we exclude firms with gaps in employment data in QP for the entirety of the pre-period (from 2006 to 2008).

We consolidate banks into banking-groups.[14] Our final sample spans 14,846 firms and 31 banking groups.[15] Given that the level of observation for workers' and balance-sheet data at the firm level is yearly, we collapse banks' balance sheets and the credit dataset to the yearly level. Credit exposures are averaged over the entire year. Table 1 presents firm level descriptive statistics for the firms included in our sample for the pre- and post-period.[16] The average firm has 59 employees and a turnover of approximately €10 million. However, the distribution is heavily skewed to the right, as the median firm has 25 employees and a turnover of around €2.3 million. For the average firm, the leverage ratio – intended as regular credit over total assets – is 24 percent (median 20 percent), and the ratio of short-term credit to wage bill – intended as liquid credit with less than one year of maturity or credit lines over wage bill – is 1.19 (median 0.47).[17]

# 3 Empirical exercise and identification

In this section we show how we exploit the granularity of our data and employ a triple-differences specification combined with an instrumental variable approach to estimate the extent to which labor rigidities determine the firm level responses to a negative short-term credit shock.

We start by describing whether and to what extent banks cut credit to firms in the aftermath of the Lehman Brothers' failure in 2009. We then investigate firm level responses to the credit shock and the extent to which firms' labor share explain these patterns. We mean to analyze whether – conditional on the same observed productivity – a firm which derives a greater share of its own value added from labor is more likely to be heavily impacted by a negative shock than a lower value added labor share firm.

[13]Appendix Table E.1 shows how representative our sample is in QP in terms of different measures.

[14]We use the term "banks" throughout the text, even though they refer to consolidated banking groups.

[15]Most of the regressions that require also balance-sheet variables features a sample of 13,806 firms.

[16]Appendix Table E.2 reports workforce composition descriptive statistics for firms in the sample.

[17]Appendix Figure F.2 shows credit market concentration in Portugal over the years, in terms of the regular credit of the largest banks in the country. The figure clearly shows how the credit market in Portugal is heavily concentrated, as also found by Amador and Nagengast (2016). Appendix Figure F.3 shows the distribution of the number of credit relationships by firm in 2005, both for firms in our sample and in the full dataset. The Portuguese credit market tends to be concentrated, and a lot of firms only have a single relationship with one bank. However, our sample consists of relatively bigger and more organizationally sophisticated firms. Thus, it features a substantially lower share of firms with single banking relationships.

Table 1: Firm level descriptive statistics, sample of analysis

| | Mean | SD | p25 | p50 | p75 |
|---|---|---|---|---|---|
| **Pre - 2009** | | | | | |
| FTE employment | 59.48 | 234.99 | 16.00 | 25.00 | 46.00 |
| Wage bill | 891,949.78 | 4,042,165.30 | 159,911.37 | 287,558.01 | 607,804.17 |
| Avg. wage | 14,427.21 | 6,480.97 | 9,960.92 | 12,792.20 | 16,877.77 |
| Sales | 9,917,213.22 | 59,168,827.51 | 1,014,851.32 | 2,295,683.43 | 5,771,160.42 |
| Tot. assets | 8,597,381.81 | 70,475,275.42 | 837,323.08 | 1,864,513.03 | 4,554,633.37 |
| # loans | 3.08 | 1.84 | 2.00 | 3.00 | 4.00 |
| Regular debt/assets | 0.24 | 0.20 | 0.08 | 0.20 | 0.35 |
| ST debt/sales | 0.12 | 0.20 | 0.01 | 0.06 | 0.16 |
| ST debt/wage bill | 1.19 | 2.72 | 0.08 | 0.45 | 1.31 |
| **Post - 2009** | | | | | |
| FTE employment | 70.25 | 337.52 | 16.00 | 26.00 | 50.00 |
| Wage bill | 1,088,347.62 | 5,258,404.35 | 176,668.35 | 322,748.09 | 710,588.65 |
| Avg. wage | 15,159.90 | 6,630.58 | 10,723.99 | 13,505.00 | 17,480.19 |
| Sales | 1,0860,932.69 | 68,885,027.20 | 942,522.93 | 2,213,365.53 | 5,896,964.44 |
| Tot. assets | 11,748,679.36 | 1.62e+08 | 947,493.93 | 2,129,300.94 | 5,508,795.18 |
| # loans | 3.24 | 2.02 | 2.00 | 3.00 | 4.00 |
| Regular debt/assets | 0.24 | 0.32 | 0.07 | 0.20 | 0.36 |
| ST debt/sales | 0.14 | 1.16 | 0.00 | 0.05 | 0.15 |
| ST debt/wage bill | 0.97 | 2.65 | 0.03 | 0.30 | 1.03 |

Descriptive statistics for the full (unbalanced) sample of analysis, with N=14,864 distinct firms. Monetary values expressed in euros, deflated by 2013 CPI. Full-time equivalent employment, salaries and revenues taken from CB, in order to have consistency of representation over time. Referenced on page [10] .

## 3.1 Characterizing banks' credit supply

Studying the impact of a variation in credit supply on any firm-level outcomes is complex: the amount of credit supplied to each firm is an equilibrium outcome of demand and supply. In order to isolate the component of credit variation that is only related to banks' supply decision we thus use an instrumental variable approach.

We measure credit variation $S_i$ for firm $i$ as a symmetric growth rate:[18]

$$S_i = \frac{D_i^{post} - D_i^{pre}}{\frac{1}{2}(D_i^{post} + D_i^{pre})}. \tag{1}$$

Building on Iyer et al., 2014 and Paravisini et al., 2015, we propose an instrument for

[18]This measure ranges between -2 and 2 and it is particularly appealing ion the literature (Davis et al., 1996, Chodorow-Reich, 2014) because it allows us to consider credit variation that ranges from the creation of a credit relationship (value 2) to its complete termination (value -2). Moreover, it limits the influence of outliers on empirical specifications.We still eliminate the outliers in the credit-growth data, dropping the 2.5 percent greatest positive variations.

credit supply $S_i$ based on banks' exposures to the interbank market as a means of financing: the ratio of foreign interbank liabilities to total assets at the bank level in the year 2005, i.e. a year before the sample period of analysis.[19] Foreign interbank liabilities are measured as the sum of short-term deposits (up to 1 year) and repos where the counterparty is a foreign financial institution (excluding central banks).[20] As this is defined at the bank level, we need to compute a measure of firm indirect exposure to the interbank market through its bank networks. We build a shift-share instrument at the firm level, in which the shift component is the bank's exposure to the foreign interbank market and the shares are the shares of a firms' short-term credit with each bank in 2005. Formally, define the foreign exposure of bank $b$ as $FD_b$ and firm $i$' s share of short-term credit with bank $b$ in 2005 as $\omega_{i,b}$. Then, the instrument $Z_i$ is defined as:

$$Z_i = \sum_{b \in B_i} \omega_{i,b} FD_b, \tag{2}$$

where $B_i$ is firm's $i$ set of banks with a credit relationship in 2005 and $\sum_{b \in B_i} \omega_{i,b} = 1$.[21]

Before analyzing the impact of the credit shock on firm real outcomes, we show how accurate our proposed instrument is at characterizing exposure-level credit dynamics around the Lehman Brothers' failure. The exposure-level analysis of the shock is important for providing evidence that banks did *not* selectively cut credit to some firms in response to the liquidity shortfall. If that is true, we can be confident that any difference in firms' reaction to the shock is the by-product of the firms' own decisions or ex-ante characteristics. In Appendix section B we run extensive checks showing how, at the bank-firm exposure level, there is no evidence that banks selectively cut credit to firms based on observables, and that important and concurring events, such as the onset of the Sovereign Debt Crisis in 2010, do drive in any way the credit dynamics we identify.[22]

Given the paper emphasis on productivity dynamics, we show here that banks did not selectively cut credit differently based on firms' productivity. We partition firms into terciles of TFP within their own 2-digit sector, and label the terciles as low, medium and

[19] The choice of an out-of-period of analysis year for the measurement of the instrument allows us to mitigate the concern that firms and banks alter their matching in anticipation of the shock. 2005 is the earliest year for which firm level balance sheet data are reliably available for the universe of firms.

[20] In our dataset, reasonable values of foreign interbank exposure range from 10 percent to slightly more than 25 percent.

[21] In Appendix B, we provide evidence on some further instruments' properties, balance checks, and an analysis of credit dynamics as a function of banks' interbank exposure at the loan level.

[22] Importantly, the Appendix shows that it is extremely implausible, following the methodology in Altonji et al. (2005), Oster (2019), that also *unobservable* match-specific characteristics are a plausible driver of banks' credit supply reductions.

high productivity bins. The productivity measure used in the analysis is the average of 2005–2006 TFP. Our preferred estimation methodology follows De Loecker and Warzynski, 2012 and Ackerberg et al., 2015. Our baseline specification features TFP calculated through a gross-output three-factor Cobb-Douglas production function.[23] Then, we jointly estimate an effect of banks' exposure to foreign interbank funds for each tercile by running the following regression at the exposure level:

$$S_{i,b} = \sum_{k \in \{L,M,H\}} \beta_k FD_b \cdot \mathbb{1}\{TFP_{bin} = k\} + \mu_i + \varepsilon_{i,b}, \tag{3}$$

where $S_{i,b}$ is the symmetric growth rate of credit variation for each credit exposure of firm i to bank b between 2006–2007 and 2009–2010 averages, calculated as in Equation 1 but at the firm-bank exposure level, and $FD_b$) is the bank's foreign exposure. The definition of the outcome variable allows us to simultaneously consider extensive and intensive margins of the treatment effect. Firm-level fixed effects $\mu_i$ control for the within-firm variation in credit supply, i.e. the average change in lending to the same firm by banks with different levels of exposure. They thus allow us to control for any firm-specific heterogeneity over the period, subject to the caveat that we are only able to implement this exposure-level specification for firms with multiple banking relationships (Khwaja and Mian, 2008). As a further robustness check, we also run an analogous regression without firm fixed effects but saturating the model with additional controls, in order to analyze the exposure-level relationship also for firms attached to only one bank. The results of these specifications, reported in Table 2, show that for all specifications we cannot reject the null hypothesis that banks transmit the shock to firms with different productivity in the same way. In the next section we make use of this quasi experimental set-up to shed light on the firm level responses to a drop in the availability of credit.

## 3.2 The role of labor rigidities

Labor share at the firm level accounts for around 60% or more of total firm value added in all advanced economies, and is thus a very relevant component of firms' operating costs and value creation (Favilukis et al., 2020, Gouin-Bonenfant, 2022). For this reason, any friction in labor costs adjustment might have relevant effects on firms' operating leverage.

Different factors might determine a firm' s labor share. Observing a high labor share might indicate that a firm is inefficient at using its labor, hence that it has a very low value

[23]Productivity and productivity rankings are highly persistent for firms over time. Appendix D describes in details the different methodologies and robustness exercises used to estimate TFP.

Table 2: Loan level regressions: productivity

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | $\Delta D_{i;st,pre-post}$ | | | |
| $FD_b$, *Low TFP* | -2.237*** | -2.347*** | -2.467*** | -2.328*** |
| | (0.276) | (0.308) | (0.289) | (0.315) |
| , *Med. TFP* | -1.941*** | -2.314*** | -2.029*** | -2.519*** |
| | (0.309) | (0.312) | (0.312) | (0.318) |
| , *High TFP* | -2.376*** | -1.927*** | -2.462*** | -2.109*** |
| | (0.294) | (0.274) | (0.314) | (0.289) |
| Firms | 9,206 | 9,206 | 12,703 | 12,703 |
| Firm FE | Yes | Yes | No | No |
| Other FE | No | No | Yes | Yes |
| TFP Measure | CD ACF | TSLOG ACF | CD ACF | TSLOG ACF |

In columns 1 and 3 TFP is the residual of a CD three factors production function, whereas in columns 2 and 4 the production function is TSLOG. The estimation always follows De Loecker and Warzynski (2012), Ackerberg et al. (2015). In columns 1 and 2 firm fixed effects control for unobservable firms' characteristics time-trends. In columns 6-7 we control for fixed effects for observables, but no firm fixed effect. Samples are firms with loans with more than one bank (essential to identify the firm fixed effect) across all specifications in the table. Additional fixed effects include 3 digits industry, commuting zone, age and size quintiles, dummy for exporter in 2005, dummy for overdue loans in 2007, dummy for firm capable of issuing bonds, dummy indicating whether the firm has any loan with banks failing up to the year 2014. Sample sizes depend on availability of non-missing variables in CB. Standard errors in parentheses, clustered at the firm and bank-by-3 digits industry level. Referenced on page(s) [13] .
$^{+}$ $p < 0.1$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$

added per employee. Additionally, a firm could be hit by a temporary negative shock and have low value added per worker, which in turn would justify a higher than usual labor share. Finally, given a certain level of labor productivity, a firm might have a cost structure that compensates labor more than other factors of production. This could happen when a firm needs a specialized production workforce because of a highly sophisticated production structure, or when the complexity of the goods (or services) produced requires a specific knowledge of the production process or of the markets supplied. In the latter case, labor becomes an investment for the firm, which incurs high costs of hiring and training its workforce. In our analysis we aim at focusing on this last aspect, explicitly controlling for other channels related to labor productivity.

Traditional economic theory treats labor as a purely variable factor, especially if compared to capital investment. However, in many cases this does not turn out to be the case. In the presence of hiring and training costs labor becomes a quasi-fixed factor of production (Oi, 1962). Differently from capital, which can be easily used as collateral for its own financing, labor cannot be pledged by firms to collateralize stable financing. This implies that, especially if there is a mismatch in the timing of cash flows in production, firms will resort to short-term credit to finance working capital, which importantly entails paying their workforce. When faced with a tightening of credit supply, a higher cost of labor can become a pivotal factor in determining firm decisions.[24]

In this section, we investigate whether the firm overall compensation policy – which changes slowly over time, especially if firms are in need of highly specialized workers – affects firms' survival and investment decisions in response to a negative credit shock.[25] First, we partition firms by labor-share quantiles, where labor share is measured as an average value in 2005–2006, some years before the financial crisis. Then, we run regressions measuring a different coefficient for the causal effect of the credit shock on real outcomes for each quantile. We run specifications by splitting firms into 7 equally sized bins (for the main specifications) or 4 quartiles (for most robustness exercises and the specifications on financial variables). We control for baseline effects linearly.[26] The specification for exit is a linear probability model:

$$P(exit)_{i,t} = \tau_t + \sum_{k=1}^{n} \beta_k S_i \cdot \mathbb{1}\{LabSh_{bin} = k\} + \mathbf{\Gamma X_{i,pre}} + FE_{i,t} + \varepsilon_{i,t}, \tag{4}$$

while the specification for employment is:

$$\begin{aligned} log(Y_{i,t}) = \gamma_i + \tau_t + &\left( \sum_{k=1}^{n} \beta_k S_i \cdot \mathbb{1}\{LabSh_{bin} = k\} + \mathbf{\Gamma X_{i,pre}} \right) \cdot \mathbb{1}\{t = Post\} + \\ &+ FE_{i,t} + \varepsilon_{i,t} \quad t \in \{Pre, Post\}, \end{aligned} \tag{5}$$

where $n \in \{4, 7\}$ depending on the specification. The outcome variable in equation (4), defined at the yearly level, is a dummy variable equal to 1 if a firm exited in any year between 2009 and 2013, while $\tau_t$ is a year fixed effect. In equation (5), $Y_{i,t}$ is the average outcome variable in the period of consideration, $\gamma_i$ is a firm fixed effect, $\tau_t$ is a time fixed effect. In both equations $S_i$ is the treatment variable – the symmetric growth rate for a firm $i$ defined in equation (1) that we instrument in the 2SLS regression with the instrument $Z_i$, $X_{i,pre}$ are a set of pre-determined controls at the firm level in 2005, and $FE_{i,t}$ is a set of fixed effects by post period. In equation (5), we interact controls with a dummy equal to 1 for the post-period years (from 2009 to 2013) to allow differential trends over the post-period. Fixed effects and baseline effects in the pre-period are absorbed by the firm fixed effect $\gamma_i$, and thus their influence captures differential group-specific trends in the post

[24]Section B provides an extensive analysis of the average effects of the shock on employment, workers' composition, firm investment and exit.

[25]Another reason why compensation policy on the part of firms might be sticky is that collective bargaining is prevailing the Portuguese market.

[26]Results are qualitatively unchanged if we control by means of a polynomial.

period.[27] We cluster the standard errors at the main bank-industry pair level.[28]

We employ two strategies to isolate the source of variation in the generosity of compensation and the relative importance of labor costs for the cost structure of the firm. In our main specifications, we control for the level of value added per employee, while in robustness exercises we first regress the labor share on value added per employee and then use the residuals as a counterfactual labor share based only on the wage-policy component. Moreover, we control for the labor share by using: (i) its average level between 2005 and 2006 (in base results) or (ii) its average for 2007 and 2008 (for robustness).[29]

The causal identification for this exercise relies on the assumption that firms are not suffering from within-sector labor-share quantile-specific shocks that are correlated with the credit supply shock of banks. 'These kinds of shocks should relate our labor-intensive firms to credit dynamics, but importantly *not* through our hypothesized causal channel. We have already shown in the current Section that there is no evidence banks transmitted the shock differently across firms based on observable characteristics within their own sector. We apply the same logic of the previous specification here and analyze the transmission of the credit shock at the loan level for firms with different exposure to labor costs. We partition firms in quantiles of labor share in value added, while controlling for underlying labor productivity through valued added per employee. We run the following regression at the exposure level:

$$S_{i,b} = \sum_{k \in \{L,M,H\}} \beta_k FD_b \cdot \mathbb{1}\{LabSh_{bin} = k\} + \mu_i + \varepsilon_{i,b}, \tag{6}$$

where we partition firms according to our baseline measure of labor share into quar-

[27]The full list of controls and fixed effects in empirical specifications are listed in Appendix Section A.

[28]Borusyak et al. (2022) and Adão et al. (2019) recently expressed concerns on clustering standard errors in shift-share designs at the level of the unit of analysis (which would be the firm in our case), given that the variation in the treatment actually comes from "shifts" at a more aggregate level (in our case banks). To speak to that, we cluster standard errors by main bank-by-industry pair, where the industry is defined as 3-digits CAE (Codificaçao de Actividades Económicas). The results are robust, if not more precise, to different clustering choices, namely straight industry clustering or double-clustering by industry and commuting zone. We always admit potentially different industry-related trends in credit and outcomes in all specifications.

[29]The consistency of our results across all specifications mitigates the concerns that results might be driven by temporary shocks in productivity or profitability, or by persistent low productivity. As further robustness checks, first we compute labor shares in two ways: by considering total costs related to labor as reported in the CB in our main specification, or just wages from QP. Moreover, given that utilization of intermediate inputs might be an additional choice affecting firms' ability to adjust the cost structure in response to unexpected shocks, we also report results with the labor share computed as the ratio of labor costs to total sales. Robustness results for employment and the probability of exiting are in Appendix Figures F.4 and F.5.

tiles and define as "medium" labor shares the two central quartiles.[30] Once again, we run the specification with firm fixed effects or saturating it with many observable controls, in order to be able to include in the estimation sample also firms with only one bank relationship. Results shown in Table 3 show that no statistically or economically significant differences can be detected across the coefficients of interest.

Table 3: Loan level regressions: labor share

| | (1) | (2) |
|---|---|---|
| | $\Delta D_{st,pre-post}$ | |
| $FD_b$, *Low LS* | -1.778*** | -2.068*** |
| | (0.327) | (0.320) |
| , *Med LS* | -2.234*** | -2.328*** |
| | (0.255) | (0.273) |
| , *High LS* | -2.311*** | -2.272*** |
| | (0.348) | (0.367) |
| Firms | 9,509 | 13,147 |
| Firm FE | Yes | No |
| Other FE | No | Yes |

Labor share is calculated as share of labor costs in value added. The "medium" bin comprehends the second and third quartiles. In columns 1 firm fixed effects control for unobservable firms' characteristics time-trends. In column 2 we control for fixed effects for observables, but no firm fixed effect. Samples are firms with loans with more than one bank (essential to identify the firm fixed effect) across all specifications in the table. Additional fixed effects include 3 digits industry, commuting zone, age and size quintiles, dummy for exporter in 2005, dummy for overdue loans in 2007, dummy for firm capable of issuing bonds, dummy indicating whether the firm has any loan with banks failing up to the year 2014 and fixed effects for the labor share bins. Standard errors in parentheses, clustered at the firm and bank-by-3 digits industry level. Referenced on page(s) [16] .
$^{+}$ $p < 0.1$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$

We further address threats to identification by showing in Section 4.3 that firms mostly affected by the shock do *not* exhibit in the pre-period any feature that might make them particularly susceptible to liquidity droughts through alternative channels, such as financial leverage or pre-existing credit cycles determined by investment.

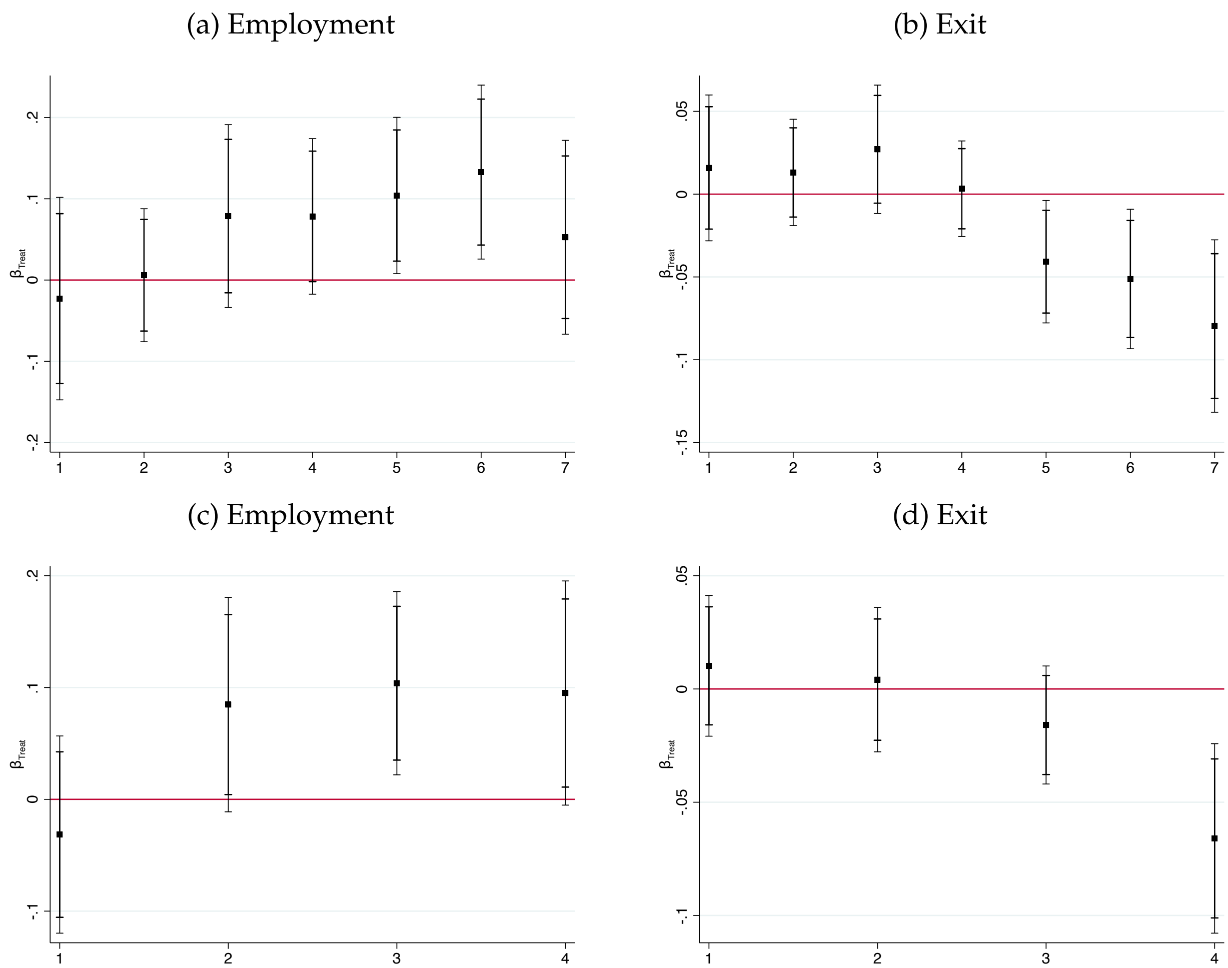
# 4 Results

## 4.1 Main Results

Figures 2a and 2b (for 7 quantiles) and Figures 2c and 2d (for quartiles) show estimates for the baseline specifications of labor share, for both exit and employment. Both the estimated elasticity of employment and the effect on the probability of firm exit as a consequence of a (negative) credit shock are almost monotonically increasing across the labor-

[30] Results wth different quantile splits, unreported, give qualitatively identical results.

Figure 2: Regressions by labor share bins




We estimate a coefficient for each of the seven labor-share bins, while controlling linearly for baseline effects. Each interacted treatment is instrumented by the interacted instrument. See Section A for the list of controls and fixed effects present in the regressions. Given that we cannot control for unobservable characteristics in the exit specifications through firm fixed effects, we characterize the bank-firm matching by augmenting the set of controls with the share of loans that each firm has with micro banks and with banks failing up to 2014. All fixed effects are interacted with a year dummy, while regressors are constant in the exit regressions. In the employment regressions fixed effects and regressors are interacted with the Post dummy. Number of firms: 13,750 (exit) and 13,760 (employment). 95 and 90% confidence intervals displayed, standard errors clustered at the bank-industry pair level. Referenced on page(s) [17] .

share bins. There is substantial heterogeneity across bins, with the lowest labor-share bins virtually unaffected by the shock. Importantly, the estimated coefficients are statistically different across far bins in the distribution. For instance, focusing on the specifications with 7 bins, the sixth and fifth bins in the employment specification are statistically different (significance between 5 and 10 percent) from the second and the first. For the exit regression the sixth and seventh bins are statistically different from the first and the second (with significance ranging from 1 to 10 percent depending on the bins).

Appendix Figures F.6 show results for firm level financial variables. One can notice that the labor-share amplification dynamics of the effect of the credit shock are present for other firms' real outcomes, such as total assets, current assets (likely to be closely related to labor financing) and sales.[31]

Appendix Section B reports in detail results of the firm level analysis of average effects of the shock, without partitioning firms by labor-share bins. According to that analysis, a one standard deviation in the shock could explain between 14 and 17 percent of a standard deviation in firm level employment variation in the period, and around 17% of the average exit probability. Extrapolating the reduced form employment estimates to aggregate employment variation in Portugal up to 2013, this shock alone explains approximately 29 percent of the employment loss.

The results in this section highlight that these estimated adverse effects of the credit shock can be almost entirely attributed to a selected subset of firms. We show that the component of the variation in labor share determined by the generosity in labor compensation increases firms' exposure to a working-capital channel, which affects the hiring margin, and acts as a risk factor for the firms as regards total employment and firm survival. As a consequence, firms with more exposure to labor costs experience a relatively more volatile employment adjustment and a greater likelihood to fail following a negative credit shock.

## 4.2 On-the-job training

In section 3.2 we show that firms with greater exposure to labor in value added – everything else, especially labor productivity, equal – are more exposed to a negative short-term credit shock. Recent empirical evidence shows that financially constrained firms

[31]As a further robustness check we show that the shock to short term credit was not a driver of capital investment, which responds more to long-term credit unexpected variations. We confirm this fact by looking only at firms with a high share of long term debt expiring right before the failure of Lehman Brothers, using the same logic as in Almeida et al. (2011). Results available on request.

fire workers with the steepest productivity profiles but lowest firing costs (Caggese et al., 2019), often the youngest workers.[32] Additionally, the risk of losing talent may affect the leverage strategy of the firm ex-ante: losing talent is a serious concern for the firm, and highly levered firms lose talented employees earlier when facing financial distress (Baghai et al., 2021). Productive firms invest in talent, face higher labor costs per worker and in general a greater degree of attachment of workers to the firm. We posit that labor fixity can be a strong driver of operating leverage. We thus aim to show that the most affected firms in our analysis are also the ones with a greater degree of labor fixity. Firms will be more attached to their own workers the more specialized they are and the more training the tasks they perform need. Their skill will either be scarce in the market, thus commanding a wage premium upon hiring and substantial search and hiring costs, or will take a lot of time and investment in training to build up within the firm. For this reason the more specialized workers' tasks, the more important they will be for value added creation, the more firms should be hesitant to part ways with specialized workers, especially if highly tenured. Thus, a relevant way to measure the degree of fixity of labor is through measures of required training and skill specificity of their tasks(Oi, 1962). [33]

In order to identify the firms' investment intensity in workers' training we exploit the detailed information available in the QP. We match profession definitions for each worker in the QP to characteristics in the Occupational Information dataset O*NET, a widely used dataset in labor economics categorizing professions according to different criteria. O*NET provides information on the educational requirements and training necessary to master a task and the complexity of performing it. We use scores from the "education, training and experience" tables, and more specifically the "on-the-job training required" (OTJ) score, to classify each worker by the amount of on-the-job training that they require given the job they perform. We then aggregate the scores at the firm level, taking averages across employees within a firm, and partition firms in quartiles based on the average scores in 2005.

We use the OTJ scores and estimate the difference-in-differences specifications as in equations (4) and (5), where we substitute $LabSh_{bin}$s with $OTJ_{bin}$s. Figure 3 shows the results of these estimations. Consistent with the idea of labor as a quasi fixed factor (Oi, 1962), we find greater elasticities of employment to credit in firms that employ workers with higher training costs, as proxied by the OTJ score. Moreover, we also find a stronger

[32] Appendix Table E.5 and Appendix Figure F.7 show that this is the case in our event study too.

[33] Tenure is another standard way to measure the degree of attachment of workers to firms, which might however be correlated with factors other than worker specialization per se. We investigate the importance of tenure in a series of robustness exercises, available upon request.

probability of exiting for those firms, even if the results for exit are noisier and not statistically significantly different across coefficients. In Table 4, we report the significance of correlations of observables with the portion of variation in the labor share determined by workers' compensation. We find very strong positive correlations of the OTJ score, the share of managers and specialized workers, highly educated workers and highly tenured workers with our measure of cost rigidity through labor. We also find, unsurprisingly, that these firms feature higher AKM (Abowd et al., 1999) compensation premia, which further strongly suggests compensation is used in these firms to attract talent.[34]

## 4.3 Additional supporting evidence and robustness

**Institutional rigidities:** In Appendix Tables E.4, E.5 E.3 and Figure F.7 we provide additional evidence regarding our financial channel of labor rigidity. We confirm that the real effects of the shock are stronger in manufacturing firms, where cash-flow mismatches are more likely to be present. We show that employment is cut especially among specialized and young workers. We also show that employment adjustment within the firm follows a precise priority order: we observe that firms first cut replacement hires, then recent hires, and almost do not transfer any effect of the shock to long-term incumbents in the firm.[35] These results strongly suggest that firms direct their employment adjustments to young and high-skilled workers, while preserving long-term incumbents. In turn, the evidence is consistent with the hypothesis that firms are unwilling to separate from the more experienced workers, which required more training or might possess more firm-specific human capital.

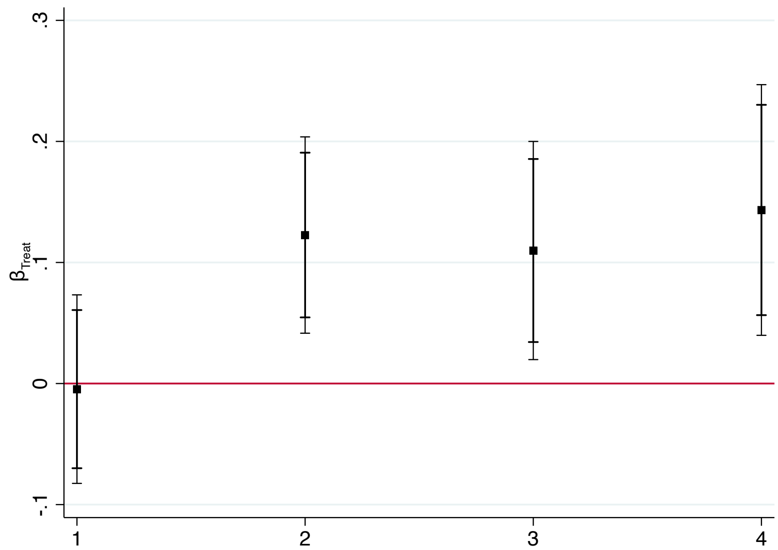
The evidence might also be consistent with an institutional rigidity story, whereby firms just cut the workers that are least expensive to lay off due to firing costs. We argue however that the effects measured in Section 3.2 cannot be fully explained, if at all, by institutional rigidities determined by labor market regulations. First, we analyze whether the effects might just be determined by wage rigidity and no notion of investment in labor by focusing on firms for which collective bargaining agreements had just been re-

[34]We also consider the "zone" score, which is a score provided by O*NET that serves as a summary index for all the scores in the "education, training and experience" tables scores (required education, required experience, amount of training on-site and the OTJ score itself). Results of regressions based on quartiles of this variable are qualitatively analogous to workers' training's ones.

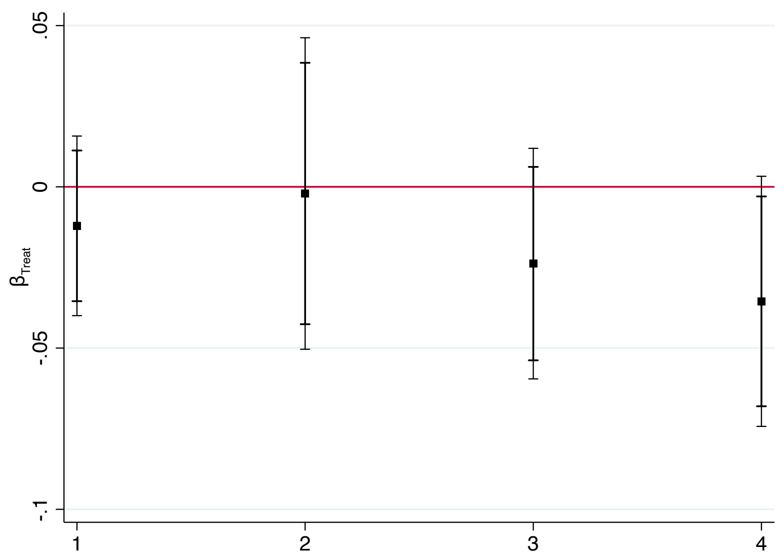
[35]In Figure F.7 the average effect of the shock is split across different tenure groups. If all groups had the same elasticity to the credit shock, the share of the effect should equal their share of employment in the firm. This is clearly not the case, as long-term incumbents in the firm in the pre-period are on average 3 times as many as recent hires.

Figure 3: Regressions by quartiles of on-the-job training scores

(a) Employment




(b) Exit




On-the-job (otj) training is defined as work carried out under the supervision of more experienced workers, and ranges from 1 (short demonstration) to 9 (several years of training). We estimate a coefficient for each of the four otj training quartiles, while controlling by means of a third order polynomial of the otj score. Each interacted treatment is instrumented by the interacted instrument. See Section A for the list of controls and fixed effects present in the regressions. Given that we cannot control for unobservable characteristics in the exit specifications through firm fixed effects, we characterize the matching of firms to banks by augmenting the set of controls with the share of loans that each firm has with micro banks and with banks failing up to 2014. Additional controls for these specification using O*NET variables comprehend the scores for: required education, required previous experience and required amount of training on site. Results are unchanged if these additional controls are not added. See Appendix C.1.7 for a description of each of these variables, and the on-the-job training score as well. All fixed effects are interacted with a year dummy, while regressors are constant in the exit regressions. In the employment regressions fixed effects and regressors are interacted with the Post dummy. Number of firms: 13,746 (exit) and 13,756 (employment). 95 and 90% confidence intervals displayed, standard errors clustered at the bank-industry pair level. Referenced on page(s) [20] .

newed within a year before October 2008, the start of the financial crisis.[36] Appendix Table E.6 reports the results of the analysis. It is immediately clear that the employment adjustment is in no way determined by the sudden increase in wages upon signing the contract, whereas shifts in rigid wages appear to be important in explaining the exit result. All specifications by labor share bins are however unaffected by the addition of these measures of wage rigidity as controls.[37]

Given the workforce composition documented in Table 4, what emerges is that labor rigidities matter a lot in the reaction to short-term credit (liquidity) shocks. Exposure to working and human capital financing make employment and and real activity susceptible to credit variations, and the effect is stronger the greater the share of value added is generated by labor. The brunt of the adjustment in employment is concentrated on young and specialized workers, while expensive high-skill incumbents are preserved. The rigidity in compensation generates a leverage effect, which becomes problematic in case firms are at the same time exposed to unexpected increases in wages, which cannot legally be cut in Portugal, and can lead to firms' demise altogether.

**Financial leverage and labor leverage:** Is it possible that our financial channel of labor rigidity is just confounding a more standard financial leverage effect? Are the effects of the credit shock just a manifestation of a boom-bust cycle in credit? The data dismiss these hypotheses. Correlations in Table 4 show that more constrained firms do exhibit overall lower credit growth and financial leverage for the entirety of the pre-period, consistent with findings in (Simintzi et al., 2015, Serfling, 2016).[38] At the same time, there is also no evidence that firms are correctly anticipating the riskiness from labor rigidity, despite lower financial leverage: highly constrained firms do not have greater liquidity per worker and are either unable or unwilling to get a greater share of collateralized debt, despite most of their debt being short-term.

The evidence in this section supports our argument that the financial channel of labor rigidity is an underappreciated but quite relevant risk factor for firms.

[36]We look at the last renewal of the modal collective bargaining agreement before the failure of Lehman Brothers. The choice of the time interval before that date has no qualitative bearing ont he results.

[37]The result is confirmed by conducting an heterogeneous treatment analysis by partitioning firms based on their expected statutory firing costs. Results available upon request.

[38]Unreported results available on request show that an heterogeneous treatment analysis of the effect of the credit shock by leverage quantiles does not exhibit any pattern.

Table 4: Correlations of observables with labor share

| | **Labor share** |
|---|---|
| **Workforce variables** (pre 2009) | |
| Avg. wage | (+)*** |
| AKM firm FE | (+)*** |
| Sh. managers | (+)*** |
| Sh. specialized workers | (+)*** |
| Sh. temporary workers | (-)** |
| Median tenure (perm.) | (+)*** |
| Sh. workers 55+ | (+)*** |
| Sh. high education workers | (+)*** |
| OJT score | (+)*** |
| ONET zone score | (+)*** |
| **Financial variables** | |
| Financial leverage (debt/ass.) (2005) | (-)*** |
| ST debt/ass. (2005) | (-)*** |
| Financial leverage (2008) | (-)*** |
| ST debt/ass. (2008) | (-)+ |
| Credit growth (06-08) | (-)*** |
| Cash per worker (2005) | (-)*** |
| Sh. ST credit (2005) | (+)*** |
| Sh. ST debt fully secured (2009) | . |
| **MRP - gaps** | |
| Labor gap | (+)*** |
| Capital gap | . |

Correlations are measured in regressions controlling by the full set of fixed effects and controls in the diff-in-diff specifications, which implies that we are also always controlling for value added per employee. Labor share and value added for employee are calculated as averages for the years 2005 and 2006. When controlling for a variable post-2006, its previous level in 2005 is excluded from the controls. Abowd et al. (1999) (AKM) firm fixed effects obtained from regressing (full) individual wages in the pre-period on individual fixed effects, firm fixed effects, year fixed effects, gender dummy, educational level fixed effects (less than high school, high school and undergraduate degree and higher), a third order polynomial of age. Results for AKM FE and avg. wage robust to controlling for workforce composition variables (share of specialized workers and managers, shares of workers with different education levels). See appendix D.2 for details regarding the estimation of MRP - cost gaps. Referenced on page(s) [20,23,23,34,7] .

$^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$

# 5 Productivity dynamics and aggregate effects

In the previous sections we presented evidence on the effect of a negative short-term credit supply shock on firm dynamics, highlighting the importance of firm's exposure to human capital in determining both the employment and other real outcomes' responses and the probability of survival of a firm. In this section we tackle two related questions. First, we investigate whether the credit supply shock had a cleansing or non-cleansing effect. Second, we study whether the labor rigidities that we identified in previous sections matter on an aggregate scale.

## 5.1 The Schumpeterian hypothesis

The hypothesis that recessions are periods of enhanced creative destruction in which the least productive agents in the economy are weeded out by economic forces dates back to Schumpeter (1942). According to the "cleansing" hypothesis, one should expect a greater likelihood of failure for the least productive firms in recessions, and a stronger productivity-enhancing reallocation of resources across firms. Some recent empirical studies have questioned whether this cleansing effect should arise, especially if financial frictions hinder resource reallocation (Barlevy, 2003, Ouyang, 2009).[39] In an influential study on the employment dynamics and reallocation in the US, Foster et al. (2016) argue that the Great Recession was way "less cleansing" than previous downturns, with less productivity-enhancing labor reallocation. The authors surmise that financial frictions might be relevant explanatory factors for justifying their findings.

In order to directly inspect the relationship between the firm dynamics and productivity levels, we once again partition firms into terciles of productivity within their own 2 digit sector, and label the terciles as low, medium and high productivity. Then, we jointly estimate a separate credit shock effect for each tercile by running the following joint regression at the firm level for exit:

$$P(exit)_{i,t} = \tau_t + \sum_{k=\{L,M,H\}} \beta_k S_i \cdot \mathbb{1}\{TFP_{bin} = k\} + \mathbf{\Gamma X_{i,pre}} + FE_{i,t} + \varepsilon_{i,t}, \tag{7}$$

[39]The cleansing hypothesis has generally been confirmed in the data in studies of labor reallocation in post-war recessions during the $20 * th$ century (Davis and Haltiwanger, 1990, Davis and Haltiwanger, 1992, Davis et al., 1996). Osotimehin and Pappadà (2015) argue that the cleansing effect could actually be weaker in recessions than in normal times, while Ouyang (2009) explain that the cleansing effect could even reverse and turn into a "scarring" effect.

while the specification for employment is:

$$log(Y_{i,t}) = \gamma_i + \tau_t + \left( \sum_{k=\{L,M,H\}}^{n} \beta_k S_i \cdot \mathbb{1}\{TFP_{bin} = k\} + \mathbf{\Gamma X_{i,pre}} \right) \cdot \mathbb{1}\{t = Post\} + \\ + FE_{i,t} + \varepsilon_{i,t} \quad t \in \{Pre, Post\}, \tag{8}$$

Table 5 reports the results of the estimation by productivity bins. The estimated coefficients for the terciles of productivity are not significantly different across each other in either specifications. Regarding exit, we find a significant effect for low-productivity firms, whereas estimates of the effect for medium- and high-productivity firms are lower. In terms of employment, the effects are evenly distributed across productivity bins, indicating that labor reallocation is clearly not stronger for the worst firms in the economy.[40]

Table 5: Regressions by CD productivity bins (Ackerberg et al., 2015)

| | (1) $log(\#emp)_{i,t}$ | (2) $P(exit)_{i,t}$ |
|---|---|---|
| $S_i$, *Low TFP* | 0.070+ | -0.033* |
| | (0.037) | (0.013) |
| , *Med. TFP* | 0.087* | -0.015 |
| | (0.042) | (0.013) |
| , *High TFP* | 0.080+ | -0.017 |
| | (0.042) | (0.016) |
| Firms | 13287 | 13277 |
| WID F | 10.84 | 11.47 |
| Sample | Complete | Complete |
| Firm FE | Yes | No |
| Other FE | Yes | Yes |

See Appendix Section A for a list of the added controls and fixed effects present in the regressions. All regressions feature the full set of fixed effects and controls. We control for average TFP in 2005 and 2006, estimated according to to the method proposed in De Loecker and Warzynski (2012), Ackerberg et al. (2015) by means of a three factors of production gross output Cobb-Douglas production function. TFP can be estimated for less firms than the full samples depending on availability of the variables to compute it in CB. Given that we cannot control for unobservable characteristics in the exit specifications through firm fixed effects, we characterize the bank-firm matching by augmenting the set of controls with the share of loans that each firm has with micro banks and with banks failing up to 2014. We control linearly for the baseline effect of productivity. In the exit specification the fixed effects are interacted with year dummies, whereas the controls are kept constant and not interacted with any year dummy. In the employment specifications all variables are interacted with a post-period dummy. Standard errors clustered at the bank-industry pair level. Referenced on pages [26,27] .

$^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$

In light of the evidence of section 3.2, we ask ourselves whether and how labor rigidities might interact with the propagation of the shock across firms at different productiv-

[40]The results are qualitatively unchanged under different production functions specifications and estimation procedures. See for instance Appendix Table E.7 for the Translog specification.

ity levels. We thus combine the previous analyses, and estimate coefficients for different combinations of labor-share and productivity tiers. We keep three terciles of TFP, and consider quartiles of labor share: we define as low and high labor share the lower and upper quartiles respectively, and as medium labor share anything in between. We estimate the following specification for firm exit:

$$P(exit)_{i,t} = \tau_t + \sum_{k,j\in\{L,M,H\}}\sum \beta_{k,j} S_i \cdot \mathbb{1}\{LabSh_{bin} = k, TFP_{bin} = j\} + \mathbf{\Gamma X_{i,pre}} + FE_{i,t} + \varepsilon_{i,t} \quad (9)$$

and the following specification for employment

$$\begin{aligned} log(Y_{i,t}) = \gamma_i + \tau_t + &\left(\sum_{k,j\in\{L,M,H\}}\sum \beta_{k,j} S_i \cdot \mathbb{1}\{LabSh_{bin} = k, TFP_{bin} = j\} + \mathbf{\Gamma X_{i,pre}}\right) \cdot \\ &\cdot \mathbb{1}\{t = Post\} + FE_{i,t} + \varepsilon_{i,t} \quad t \in \{Pre, Post\}. \end{aligned} \quad (10)$$

We estimate a different coefficient for each bin, while once again controlling for the baseline effects linearly and interacting labor share and productivity as well.

The results of this empirical exercise are presented in Figure 4.[41] Three considerations emerge. First, within a labor-share bin firms respond to the shock in a very similar way across different levels of productivity. Second, high labor share firms suffer more from the shock, both in terms of the probability of exit and the employment adjustments.[42] Third, we find significant effects on high-productivity firms with high labor share, both for employment and for firm exit. All in all, Table 5 and Figure 4 provide evidence of a non-cleansing effect of the credit shock: high productivity firms are strongly affected by a reduction in the supply of credit, especially so if they have a high labor share.

Our results strengthen the argument in Foster et al. (2016) that during the last financial crisis the cleansing dynamics that were typically observed in recessions were weaker. Moreover, our results show that labor rigidities are a fundamental driver of the propagation of the credit shock across the economy, and might ultimately be one relevant source

[41]TFP is estimated according to Ackerberg et al. (2015) from a Cobb-Douglas production function. In Appendix Figure F.8 we provide as robustness the same specifications with TFP estimated with a translog production function. This specification is more flexible and does not force a unitary elasticity of substitution across factors of production. Results are qualitatively identical.

[42]The coefficients for the high and low labor-share bins across productivity levels are statistically different from each other, at the 5 to 10% level of significance depending on the specification and the specific productivity bin.

of financial frictions that hinder productivity-enhancing resource reallocation.

## 5.2 Firm survival and factor reallocation

We revisit the evidence in Foster et al. (2016) and show that in the period around the financial crisis following the collapse of Lehman Brothers the cleansing dynamics throughout the whole economy were weaker than in normal times, both as regards firms survival and the reallocation of factors of production. Moreover, we show that at the aggregate level labor rigidities drive the non-cleansing effects. We thus now consider firms for the whole QP-CB matched dataset, which comprises *all* firms observed in our data with workers, regardless of their credit position.[43] We run the following regressions at the firm-year level:

$$y_{i,t+1} = \tau_t + \beta TFP_{i,t} + \gamma TFP_{i,t} \cdot \mathbb{1}\{t \in Post\} + FE_i + \varepsilon_{i,t}, \tag{11}$$

where $Post \equiv \{2009, 2010, 2011, 2012\}$. In the exit regression, the dependent variable is a dummy equal to 1 if the firm exits in year $t$ and is not in the dataset anymore at $t+1$, while it is equal to $\Delta log(x)_{i;t+1,t}$ for the regressions of employment growth, full-time-equivalent employment growth and capital growth. We estimate a coefficient for TFP that reports the average effects for the pre-period years, and a variation of slope for the crisis years.[44]

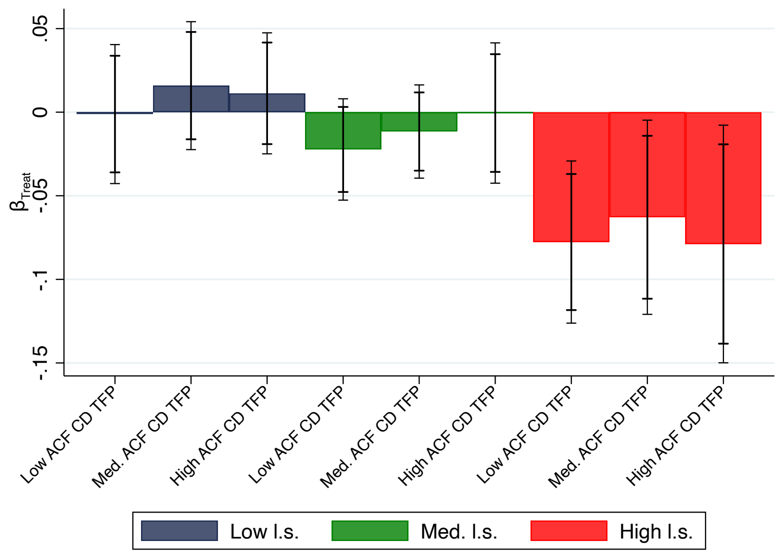
We report our results in Table 6. Higher-productivity firms have lower probability of exit and greater input growth on average. In all cases, however, the effect is weaker in the post-Lehman years, consistent with Foster et al. (2016). We find evidence that the cleansing effect during the financial crisis was weaker overall, which is possibly related to the labor frictions we analyzed in the previous sections.

We then analyze whether our measure of labor rigidities might contribute to explaining these aggregate results. We split the firms by labor-share quartiles and we estimate a different effect of productivity on our outcomes in both normal times and the crisis period for each quartile. We run the following empirical specifications:

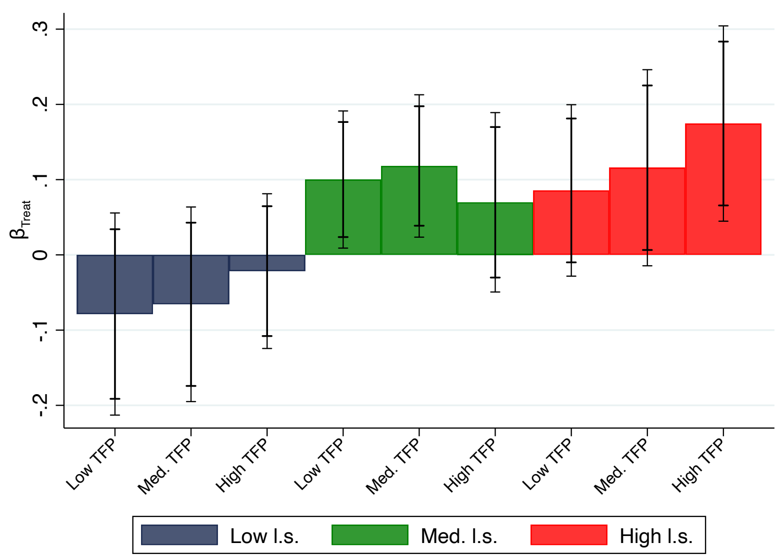
[43] We still exclude micro-firms, with less than 3 employees of Euro 1000 in turnover.

[44] The sample includes all firms for which we can compute the TFP, measured on the full residual of the estimation of a Cobb-Douglas production function with the Ackerberg et al. (2015) methodology. We control for year fixed effects and 3-digits industry fixed effects, and cluster standard errors at the 3-digits industry level.

(a) Exit




(b) Employment




We estimate a coefficient for each of the nine interacted bins, while controlling linearly for baseline effects and their interaction. Each interacted treatment is instrumented by the interacted instrument. Labor share is defined as the ratio between employment-related costs and total value added (average of 2005-2006 levels). Productivity is estimated on a 3-inputs gross output Cobb-Douglas production function following Ackerberg et al. (2015), by 2-digit industrial sectors. See Section A for the list of controls and fixed effects present in the regressions. Given that we cannot control for unobservable characteristics in exit specifications through firm fixed effects, we characterize the matching of firms to banks by augmenting the set of controls with the share of loans that each firm has with micro banks and with banks failing up to 2014. All fixed effects are interacted with a year dummy, while regressors are constant in the exit specifications. In the employment specifications All fixed effects and controls are interacted with a *post* dummy. Number of firms: 13,248 (exit) and 13,258 (employment). Sample size depends on availability of non-missing variables in CB. 95 and 90% confidence intervals displayed, standard errors clustered at the bank-industry pair level. Referenced on page(s) [27,27] .

Table 6: Reallocation and TFP - full dataset

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | $exit_{i,t}$ | | $\Delta\log(emp)_{i,t+1}$ | $\Delta\log(ftemp)_{i,t+1}$ | $\Delta\log(fixed\,cap.)_{i,t+1}$ |
| $TFP_{i,t}$ | -0.0529*** | -0.0536*** | 0.0378*** | 0.0379*** | 0.0338** |
| | (0.0052) | (0.0051) | (0.0069) | (0.0067) | (0.0107) |
| $TFP_{i,t} \cdot Post\ Lehman_t$ | | 0.0013* | -0.0063* | -0.0065* | -0.0088* |
| | | (0.0006) | (0.0029) | (0.0028) | (0.0042) |
| Firms | 199746 | 199746 | 189766 | 188450 | 197253 |
| N | 848309 | 848309 | 809584 | 801842 | 889125 |
| Industry FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |

The regressions refer to the empirical specification in equation (11) in the text. All regressions feature 3-digits industry fixed effects. The sample consists of all firms in CB for which TFP can be computed (with the exclusion of the energy and construction sector). We also exclude very small firms, with less than 2 employees or less than a thousand euros in total assets or revenues on average for all years in which they are observed. All variables refer to the outcomes from $t$ to $t+1$. We measure employment either as total headcount of full time equivalent employment, as reported in CB. The exit regression excludes the year 2005, given the CB structure. Standard errors clustered at the 3-digits industry level. Referenced on page(s) [28] .

$^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$

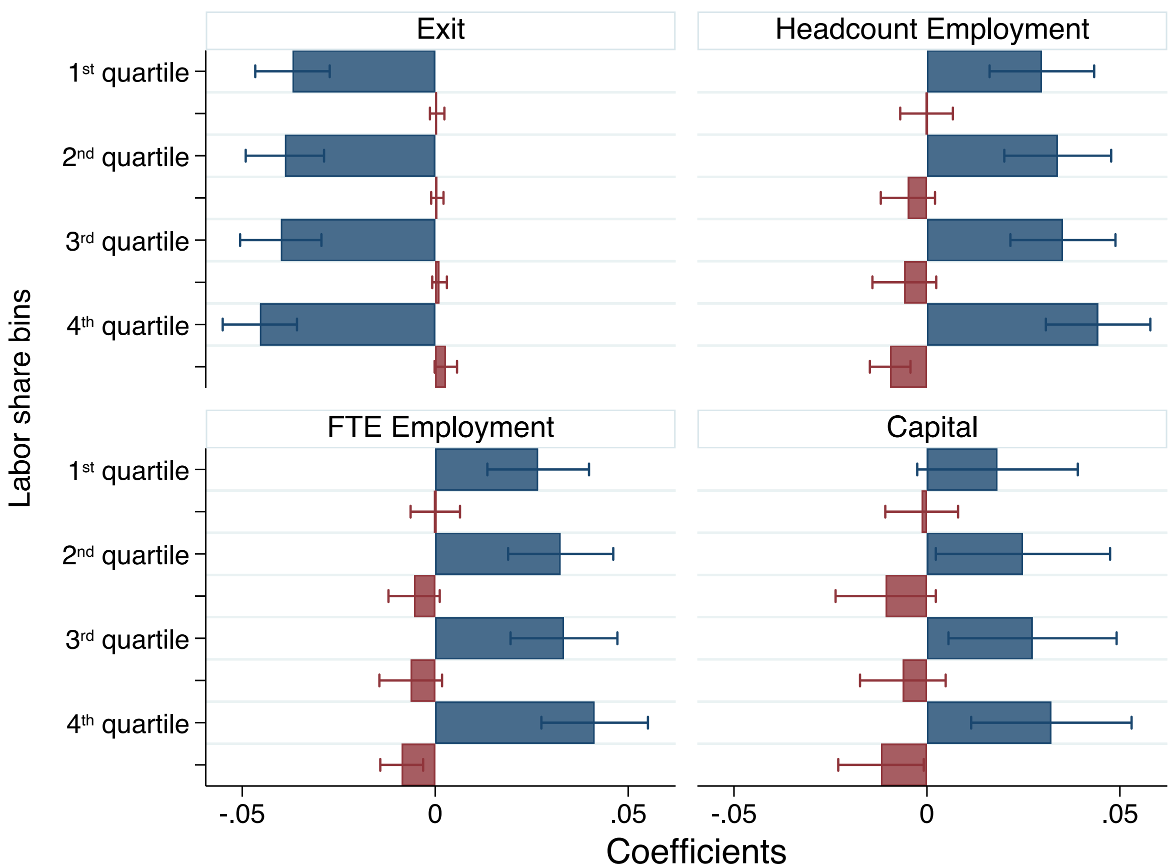
$$y_{i,t+1} = \tau_t + \sum_{k=1}^{4} \left(\beta_k TFP_{i,t} + \gamma_k TFP_{i,t} \cdot \mathbb{1}\{t \in Post\}\right) \cdot \mathbb{1}\{LabSh_{bin} = k\} + FE_{i,t} + \varepsilon_{i,t}, \quad (12)$$

where $Post \equiv \{2009, 2010, 2011, 2012\}$. We control for labor-share quartile-by-pre/post-period fixed effects together with industry fixed effects, and for the average effect of value added per employee. We report our results in Figure 5 and Appendix Table E.8. The cleansing effects are stronger in firms with a greater labor share and labor rigidity in normal times. At the same time, the prevalence of the cleansing effect falls more sharply (both in absolute and relative terms) for firms with a high labor share and thus stronger frictions in costs' adjustment throughout the crisis period.[45]

These results indicate that in the post-Lehman period the cleansing effect was particularly weak with respect to the pre-period *exactly* for the firms with the most rigid cost structure with respect to labor, both in terms of firm survival and inputs reallocation. In other words, even if this exercise only points to correlations, this evidence lends credibility to the relevance of the financial channels of labor rigidity from the event study in previous sections in impacting productivity enhancing reallocation at an aggregate level.

[45]The only case in which the weakening of cleansing effects is not exactly monotonic in labor share regards capital reallocation.

Figure 5: Reallocation and TFP by labor share - full dataset




The regressions refer to the empirical specification in Equation (12) in the text. A different coefficient is jointly estimated for each labor share bin (blue bars), and a variation of slope is estimated for the years post 2008 (red bars). Labor share is computed as the average ratio of employment costs over value added for the years from 2005 to 2008. In all specifications a control for the average value added per employee in the period from 2005 to 2008 is added. All regressions feature 3-digits industry fixed effects, and labor share quartile by post-Lehman dummy fixed effects. The sample consists of all firms in QP matched with CB for which TFP can be computed (with the exclusion of the energy and construction sector). All variables refer to the outcomes from $t$ to $t+1$. We measure employment either as total headcount of full time equivalent employment, as reported in CB. The exit regression excludes the year 2005, given the CB structure. Standard errors clustered at the 3-digits industry level. Referenced on page(s) [30] .

## 5.3 Labor rigidities and macroeconomic effects

Do the labor rigidities and their potential non-cleansing effects matter in aggregate? In order to address this question, we perform an aggregate productivity accounting exercise.

We aim to aggregate the previous results and estimate the share of productivity variation explained by the credit shock. We follow Levinsohn and Petrin (2012), Baqaee and Farhi (2019). We define aggregate productivity growth as the change in the Solow residual, that is the variation in aggregate demand minus the change in expenditures in production inputs. More formally,

$$APG = \sum_i P_i dY_i - \sum_i \sum_{X_i \in \{L,K,M\}} W_{i,X_i} dX_i$$

where $Y_i$ is total output, $X_i$ is an input in production (either labor $L$, capital $K$ or intermediate inputs $M$) and $W_{i,X_i}$ are costs of inputs. These are the wage $W_i$, the user cost of capital $R_i$ (depreciation and interest rates on debt financing capital), or the price of intermediate inputs $P_i^m$. The previous relationship can be re-written in growth rates as:

$$APG = \sum_i D_i d\log A_i + \sum_i D_i \sum_{X_i \in \{L,K,M\}} (\theta_i^X - s_i^X) d\log X_i \tag{13}$$

where $A_i$ is technical efficiency (i.e. firm-level TFP), $D_i = (P_i Q_i)/(\sum_i VA_i)$ are the Domar weights, $\theta_i^{X_i}$ are the output elasticities of input $X_i$ through the production function estimation and $s_i^{X_i}$ are revenue shares for each input $X_i$.[46] The first term in Equation 13 defines variation in technical efficiency at the aggregate level, while the second term represents the effect of input reallocation. Intuitively, it captures the benefit of reallocating inputs from low marginal product firms to high marginal product ones. [47]

We perform an aggregate-productivity growth-accounting exercise between 2008 and 2012; we use a discrete-time (Tornquist-Divisia) approximation of Equation 13, and take averages across periods for the Domar weights, elasticities and revenue shares. We use TFP and output elasticities calculated according to the Ackerberg et al. (2015) method on a translog production function.[48] We estimate an overall $-12.37\%$ aggregate-productivity

[46]The Domar weights are the ratio of sales to total value added at the firm level. They thus can sum to more than 1, and depending on value added can also be negative.

[47]This goes beyond the technical-efficiency effect, which would be aggregate productivity growth in efficient economies (Hulten (1986))

[48]The exercise is carried out on the sample of relatively large firms that we use for most of the causal empirical analysis, as we need to obtain estimates of the effects of the credit shock on input wedges. For this part of the exercise we implicitly assume that our sample represents the whole Portuguese economy.

growth for Portugal between 2008 and 2012, due to variation in TFP not explained by input reallocation. The combined contribution of allocative efficiency is close to a positive 1 percentage point.[49]

We perform a counterfactual exercise in which we calculate the productivity effects determined by decreases in misallocation as a consequence of the shock. [50]

For the aggregation exercise, we assume that the technical productivity variations are not correlated with our reduced-form estimates from the event study.[51] Under this assumption, reduced form estimates identify a source of variation in aggregate productivity stemming only from input misallocation, and we can adopt decomposition 2 from Baqaee and Farhi (2019), which is a generalization of the results in Levinsohn and Petrin (2012), under the assumption of no TFP changes due to the shock. We then compute the amount of aggregate productivity growth attributable to the shock only through misallocation. As regards labor variation, we use the credit elasticity estimates from Appendix Section B, and extrapolate the aggregate effects of the shock by comparing the difference between predicted employment growth with and without the partial equilibrium effect we estimate (see Appendix Table E.12). As regards variation in the labor wedge, we take averages between the 2008 labor wedge and the 2012 wedges implied by the reduced form estimates variation. Specifically, the relevant wedge in the case the treatment effect is accounted for is, according to results in Appendix Table E.9,

$$\widehat{\theta_i^L - s_i^L}|_T = \hat{\beta} S_i + \hat{\gamma}_i + \hat{\tau}_{post} + \mathbf{\hat{\Gamma} X_{i,pre}} + \hat{FE}_{i,post}, \tag{14}$$

whereas the counterfactual wedge had the shock not hit is

$$\widehat{\theta_i^L - s_i^L}|_{NT} = \hat{\gamma}_i + \hat{\tau}_{post} + \mathbf{\hat{\Gamma} X_{i,pre}} + \hat{FE}_{i,post}. \tag{15}$$

According to the use of the Tornquist-Divisia approximation, the wedges to be used

[49] Baqaee and Farhi (2019) show that the decomposition by Levinsohn and Petrin (2012) might be incorrect because in the aggregation they use the wrong Domar weights (that is, the "right" Domar weights should be cost, and not revenue weights). However, we still use the results of Levinsohn and Petrin (2012) to provide at least suggestive evidence of the decomposition of productivity growth, and its overall size, over the period. See Baqaee and Farhi (2019, 2020) for a discussion.

[50] The counterfactual exercise is similar to Baqaee and Farhi (2019) and Bau and Matray (2022).

[51] We do not find any direct effect of our shock on TFP. However, the assumption here is stronger, because it also implies that TFP variations are not directly or indirectly (through general-equilibrium adjustments) correlated with our estimated inputs and wedge variations.

in the aggregation exercise are thus

$$\overline{\theta_i^L - s_i^L}|_j = \frac{1}{2}((\theta_i^L - s_i^L)_{2008} + (\widehat{\theta_i^L - s_i^L})|_j) \quad j \in \{T, NT\} \tag{16}$$

The portion of aggregate productivity growth exclusively attributable to the shock is thus

$$APG|_T \approx \sum_i \bar{D}_i \left( \overline{(\theta_i^L - s_i^L)}|_T \, \widehat{dlogL_i}|_T - \overline{(\theta_i^L - s_i^L)}|_{NT} \, \widehat{dlogL_i}|_{NT} \right), \tag{17}$$

where $\bar{D}_i$ is the firm level average Domar weight between the years 2008 and 2012, which is used as firm level weight in both scenarios (treatment and counterfactual scenario with no treatment). The first term on the right hand side of Expression (17) is the allocative efficiency aggregate productivity variation implied by reduced form estimates. The second term represents the same measure in a counterfactual scenario where treatment effects are uniformly set to 0.

Aggregating the estimated reduced form effects on employment indicates that our shock explains 29 percent of the aggregate employment decrease throughout the post period.[52]

Once we apply the results from the reduced form aggregation exercise for wedges and employment to Equation 17 we find that the misallocation attributable to labor would increase by 4 percentage points absent the shock, whereas the effect of the shock further increases misallocation by 0.52 percentage points, i.e. 13 percent of the estimated partial-equilibrium effect. In aggregate terms, the implied variation attributable to the shock corresponds to approximately 4.2 percent of the total. Moreover, implied misallocation according to the event study reverses the contribution of allocative efficiency to aggregate productivity growth, from positive to negative.[53]

[52]At the firm level, as shown in Appendix Section B, one standard deviation in the shock explains between 14 and 17 percent of the standard deviation in employment (and a similar percentage in the variation in the probability of exiting the market).

[53]The correlation in Table 4 shows that firms in our sample with high labor shares display markedly greater labor marginal product-cost gaps. This suggests that these firms are constrained, and a reallocation of labor to them would actually *increase* allocative efficiency, despite them being already labor intensive.

# 6 Conclusion

In this paper, we study how labor-market rigidities impact the propagation of credit shocks to firms' employment, exit and other real outcomes, and whether in turn this propagation reinforces or impairs productivity-enhancing reallocation dynamics. To answer these questions, we conduct an event study to analyze the real effects of the interbank market freeze in Portugal following the failure of Lehman Brothers at the end of 2008, and dissect the way in which the credit shock generated by that episode spreads to the corporate sector. Our main results highlight that the credit shock has significant effects on employment dynamics and firms' survival, irrespective of firms' measured productivity. These findings are entirely driven by the interaction of the credit shock with labor-market frictions, determined by rigidities in labor costs and exposure to working-capital financing and operating leverage induced by investment in specialized workers. The credit shock explains about 29 percent of the employment loss among large Portuguese firms between 2008 and 2013, and contributes to slightly less than 5 percent of the productivity losses due to labor misallocation. Our findings also support the argument that the presence of financial frictions, as determined by the financial channels of labor rigidities, weakens the cleansing effect of recessions.

Our study sheds light on the macroeconomic relevance of financial frictions at the firm level determined by labor rigidities, and poses interesting questions about how policy-makers should think about regulating their influence. The operating leverage determined by labor costs calls for policies that alleviate labor market frictions, and especially wage rigidity. The presence of labor-induced operating leverage would by itself call into question the efficacy of hiring credits in recessions in order to preserve employment levels. Instead of facilitating new hiring, policy-makers should evaluate measures that alleviate the burden of firms' current employment costs. Recent empirical evidence regarding short-term work and furlough programs of labor hoarding point exactly to this direction (Giupponi and Landais, 2018, Cahuc et al., 2018). These labor market policies, on top of preserving employment, might also have beneficial effects on productivity if labor rigidities and financial frictions are relevant. The adoption of these schemes might allow to distinguish cases in which a firm wants to fire unproductive workers but is constrained by explicit labor-market rigidities from cases in which it does not want to fire any worker given their potential productivity in the future. This is likely to be helpful for young workers (see Caggese et al., 2019), and diminish the sullying effects for the economy through young workers' scarred labor careers (Acabbi et al., 2022).

Our results are also relevant for firms' optimal financing decisions, especially with regards to high-skilled workers' salaries and human capital accumulation. The existence of an investment component in labor is deeply ingrained in how firms carry out some production processes, and its relevance will only increase over time as intangible human capital becomes more salient in both manufacturing and services (Sun and Xiaolan, 2018, Acabbi et al., 2022). These trends make it essential for economists to improve their understanding of labor financing, and rationalize why firms expose themselves to the risk inherent in the maturity mismatch in financing an investment good as high-skill labor with short-term credit. In turn, this understanding will enable policy-makers and firm managers to develop policies to support more stable forms of labor financing, or alleviate the exposure of firms to the liquidity risk of incurring upfront costs.[54]

Our findings point to a pivotal role of labor compensation and financing in affecting firms' performance. The relevance of labor costs in input cost structure at the firm level constrain internal funds and liquidity management in periods of scarce liquidity, and impair firms' productive activities, leading to weakened cleansing in the economy. Our results show that, because of frictions in labor adjustment, employment losses and productivity distortions resulting from credit shocks can be significant. Our mechanisms also help providing an explanation for why the effects of credit shock can be persistent over time. As the amplification of any shock through cost rigidity aggravates economic downturns, many firms end up either dramatically down-scaling or exiting the market altogether. Future research should thus continue to analyze these frictions, their determinants, and most importantly policies to alleviate their influence.

[54]An interesting piece of evidence on this issue is Barrot and Nanda (2019), who show that accelerating payments in arrears on the part of the US government in 2011 led to an alleviation in creditor firms financing constraint, which benefited employment. Barrot et al. (2019) show that, in the context of France, countercyclical government's loan guarantees to alleviate financial frictions for small firms helped short-term debt roll-over and employment stabilization during the Great Financial Crisis.

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# APPENDIX

## A Controls for empirical specifications

All empirical specification of differences-in-differences in the text, unless otherwise specified, include the following set of controls:

- **Fixed effects**: 3-digit industrial sector, commuting zone, quintiles of firm age and size in 2005, and dummies for exporter status, firm with overdue loans in 2007, banking relationship with the banks failed before 2014, firm capable of issuing bonds, firm with a single banking relationship.
- **Additional controls**: log level of short-term credit, financial leverage, log of total assets, short-term credit growth between 2004 and 2005, debt towards suppliers over assets, number of loans, (weighted) length of banking relationships, cash over assets, share of temporary workers, trade credits over assets, log number of employees, asinh of the value added per employee, log of sales, firm age, share of short-term credit in regular credit, share of fixed tangible assets in total assets, share of exports in sales in 2006, ROA, ROS, log of the average wage, average workers' turnover rate between 2003 and 2005.

When not differently specified, additional controls are measured as the 2005 values. All controls and fixed effects are interacted with a post-period dummy (or year dummies in dynamic specifications).

# B Transmission of the shock: instrument properties and loan level evidence

## B.1 Instrument properties

The complication with measuring the causal impact of a variation in credit supply on any firm-level real variable, which is in itself the equilibrium object, is that observing credit variation on its own does not convey any information on the unobserved effective willingness of a bank to supply credit vis-à-vis the unobserved credit demand by the firm. As long as the firm's credit demand is correlated with a firm's investment decision and idiosyncratic investment opportunities, the econometrician needs some way to isolate the component of credit variation that is only related to banks' supply decision. We use an instrument to identify banks' supply decisions.

In order for an instrument to be valid in disentangling the exogenous variation in credit supply, it must correlate to firms' real outcomes only through credit variation. Moreover, its assignment to firm-bank pairings must be as good as random conditional on observables. In the case of firms' real investment decisions, this implies that the econometrician should verify that there are parallel trends in firms' behavior absent the treatment, in our case the credit shock. It is also necessary to avoid bias in the estimates stemming from endogenous non-random matching of banks and firms in the years leading to 2008. Relationship ties between banks and firms in Portugal are sticky, and the average duration of a relationship is around 9 years Bonfim and Dai (2017). Nonetheless, it is possible that firms and banks re-sorted themselves in anticipation of the credit shock. To indirectly control for these dynamics, we measure our instrument while referring to the bank-firm network in 2005, which is outside of our sample of analysis. Observing a strong first stage in the regressions would imply that endogenous firm-bank re-sorting in anticipation of the crisis is unlikely to be a relevant issue.

To further control for other sources of observed and unobserved heterogeneity that might affect our estimates, we saturate our empirical model with multiple fixed effects and firm level observables interacted with a time variable. This allows us to explicitly allow for differential trends in the outcome variables. As a consequence, our estimates effectively compare variations across firms with similar starting characteristics, and allow for differential trends depending on a firm's location, industry and many other observables. In this way, we can identify the effect onto firms as similar as possible to each other, but attached to banks with differential exposure to the credit shock.

In order for our instrument to disentangle a causal effect, we need to verify that it is quasi-randomly assigned, i.e. it's distribution across firms is plausibly random conditional on the observables that the econometrician can control for. Passing this test guarantees that the estimated effects are not the spurious by-product of other dynamics stemming from the non-random matching between a firm and a bank based on the bank's foreign funding exposure. Figure F.9 shows pairwise correlations of the instrument $Z_i$ with firm-level observable characteristics, conditional on the set of fixed effects that we include in the main empirical difference-in-differences specification. In almost all cases coefficients are very small and close to 0. Still, given that some observables are significantly correlated with the instrument, we control explicitly for trends related to these observables in our regressions, plus other observables that we include to improve precision and robustness.

In a recent influential paper on the identification properties of shift-share instruments, Borusyak et al. (2022) highlight that the exogeneity of the instrument could stem from the quasi-randomness of the underlying shifts with respect to firm characteristics, but does not per se stem from the overall shift-share structure. They show that the standard shift-share IVs can be implemented through a re-weighted shock-level specification. We show for completeness in Figure F.10 that the interbank exposures at bank level are not significantly correlated to weighted bank exposures to firm-level observables (where the weights are shares of bank-level short-term credit exposures with a firm). The absence of any significant correlation lends credibility to the assumption that our identification stems from shocks quasi-randomness. Moreover, the stability of semi-elasticities of credit to interbank exposure in Table E.10 in Appendix Section B.2 indicate that observables, firm or match-specific characteristics do not significantly affect the transmission of the credit shock, which is also consistent with the validity assumptions in Goldsmith-Pinkham et al. (2020).

## B.2 Loan level evidence

The exposure-level analysis of the shock is important for providing evidence that banks did not selectively cut credit to some firms in response to the liquidity shortfall. If that is true, we can be confident that any difference in firms' reaction to the shock is the by-product of the firms' own decisions. For this empirical analysis we run the following specification:[1]

$$S_{i,b} = f(FD_b) + \mu_i + \varepsilon_{i,b}, \tag{18}$$

where $S_{i,b}$ is the symmetric growth rate of credit variation for each credit exposure of firm $i$ to bank $b$ between 2006–2007 and 2009–2010 averages, calculated as the endogenous treatment in Equation (1) but at the firm-bank exposure level, and $f(FD_b)$ represents different functions of bank foreign exposure. The definition of the outcome variable allows us to simultaneously consider extensive and intensive margins of the treatment effect. In the baseline specification we also add firm-level fixed effect $\mu_i$ so that we are effectively controlling for the within-firm variation in credit supply, i.e. the change in lending to the same firm by banks with different levels of exposure. This feature allows us to control for any firm-specific time-invariant heterogeneity, but but we are only able to implement this exposure-level specification only on firms with multiple banking relationships (Khwaja and Mian, 2008).

Table E.10 shows the results of multiple specifications testing the robustness of the exposure-level relationship. We find highly significant negative (semi-)elasticities of firm short-term credit to our measure of a bank's exposure to the foreign interbank funds' market. In our preferred specification, in column 1, a 1 percentage point increase in a bank's exposure determines approximately a 2.1 percentage points decrease in the amount of short-term credit provided by that bank until 2010. Columns 2 and 3 show analogous results for different functions of the level of interbank exposure (firms above the mean or median banks' exposure). Given that one might be concerned about the effects of omitted variable bias, which might imply that the estimated effects are biased by unobservable firm-level characteristics of effects specific to the matching between firms and banks, we perform several robustness checks to show that the estimated effect is very stable and quite precisely estimated. In column 1 we report a bounding set to evaluate coefficient stability, following Oster (2019), which should give the reader an idea of how much one would expect the

[1]This is the exposure average analogue of the empirical specification of Equation 3 in Section 3.1.

estimated coefficient to move because of the presence of match-specific unobservable influences. We use the results of the specification in column 1, which control for firm specific trends in short-term credit dynamics determined by unobservable characteristics through firm fixed-effects, as benchmark results, and compare them to results obtained when an analogous regression is run on the same sample with no controls at all. The bound between the estimated and the "bias-corrected" coefficients is tight and far from 0, which is strongly reassuring.[2]

In order to show that the credit channel proxied by the interbank foreign funds exposure is not influenced by the dynamics of the sovereign debt crisis, we perform a robustness exercise in columns 4 and 5 in which we add controls for the exposure of banks to sovereign debt by the Portuguese government. In column 4 we control for the ratio of the average amount of sovereign debt on a bank's balance sheet over total assets in 2009, and in column 5 we control for the same measure from the last quarter of 2009, which is the period when the sovereign debt crisis dynamics started to unfold. Even if these controls are highly significant in these specifications at the exposure level, our estimated coefficients for the effect of exposure to foreign interbank funds remain stable and are not statistically distinguishable from the estimated coefficient in column 1.[3] As a further check, we test whether our instrument $Z_i$ computed at the firm level as in Equation (2) predicts credit dynamics after 2010, after controlling for credit variation up to that year. We run the following regression at the firm level on the set of firms active in 2010:

$$\Delta D_{i;st,2013-2010} = \beta \Delta D_{i;st,2010-2006} + \gamma Z_i + \mathbf{\Gamma X_i} + \varepsilon_i. \tag{19}$$

Results are shown in Table E.11. From columns 1 to 5 we gradually add controls, including the fixed effects, 2005 observable firm characteristics and controls for weighted exposure of firm to sovereign holdings of their banks (with shares of short term loans with each bank as weights, as for the instrument in Equation (2)), either considering matched banks in 2005 or in the fourth quarter of 2009 (for the banks that have active regular short-term loans), in columns 4 and 5. In all specifications our instrument does not predict short-term debt dynamics after 2010, lending credibility to our proposed identification channel.

Following Khwaja and Mian (2008) and Chodorow-Reich (2014) we perform further robustness tests in columns 6 to 9 of Table E.10. By removing the firm level fixed effect and observing how much the semi-elasticity of credit to the bank shock varies across specifications, we can indirectly asses whether the match between firms and banks is not as good as random. The variation in the estimate should proxy the amount of bias implied by the not as-good-as-random matching determined by firm-specific unobservable characteristics. In columns 6 and 7 we replicate the exposure-level regression with no controls, while in columns 8 and 9 we saturate the model with a series of fixed effects characterizing the firm operations, such as industry, location and other characteristics. In columns 7 and 9 we run the regressions on the full sample of firms that we use in the firm-level specifications in our sample of analysis, including firms with only a single bank relationship. The estimates are remarkably stable across specifications, with a range of variation for our preferred specifications (columns 1 and 8) of around 2–3 percent of the base estimate of

[2]Oster (2019) developed a framework to evaluate coefficient stability by observing how much estimated coefficients and $\mathbb{R}^2$ vary in regressions when one varies the amount of observable controls. The framework builds on the work by Altonji et al. (2005), and is based on the logic according to which, if a researcher includes relevant observable controls in a linear regression and the coefficient of interest does not vary, it is unlikely that omitted unobservable controls are significantly biasing the results.

[3]In our dataset, the exposure of banks to sovereign-issued financial liabilities is one order of magnitude smaller than the exposure to foreign interbank exposure. Exposures rates are rarely above 4 percent.

column 1. All estimates are statistically indistinguishable at standard confidence levels.[4]

Finally, to lend further credibility to our estimates, in column 10 we run a regression analogous to the specifications by Iyer et al. (2014), and analyze the impact on total credit of the banks' exposure to the interbank market as a whole (both domestic and foreign exposures are accounted for in this specification, to adhere closely to the specification in Iyer et al., 2014). Our estimate with the 2005 exposure is on the same order of magnitude as theirs (-0.432 versus -0.556), despite the fact that they have estimates from a different set of firms, with a more recent measure of exposure, and a wider set of banks, given that Iyer et al. (2014) do not consolidate banks into banking groups. Our estimates also show that the credit shock had an immediate and very strong impact on the volume of short-term credit, as the semi-elasticities imply that most of the variation in total credit determined by the Lehman shock comes from relatively fickle short-term exposures.

In conclusion, the average exposure results reported in this Appendix further lend credibility to the causal identification of our main effects. On the one hand, they strongly lend support to the argument that the matching of firms to banks in our natural experiment is plausibly orthogonal to the instrument, banks' interbank exposure in 2005. On the other hand, the stability of the estimated semi-elasticities of credit along multiple empirical specifications supports the thesis that banks did *not* selectively cut credit to firms based on observables, especially labor costs and productivity, our analysis is based on.

[4]Together with balance checks, observing that the credit transmission of the shock does not appear to be affected by the possible impact of observables, firm or match-specific characteristics lends credibility to the identification, given the validity assumptions in Goldsmith-Pinkham et al. (2020). If credit exposure or other characteristics were making the distribution of shares not-exogenous, one would surely expect semi-elasticites of credit with respect to interbank exposure to visibly vary, which is never the case.

# INTERNET APPENDIX

## A Conceptual framework

The aim of this section is to understand the mechanism underlying the evidence that we observe in the data, and derive some further empirical predictions that we can test in support. To this end, we develop a theoretical framework in which both low- and high-productive firms might have a high labor share, and use it to study the reaction of employment and exit to credit shocks.

The economy lives for two identical periods, labelled $t = 1, 2$, and is populated by a large number of monopolistically competitive firms, each producing a single variety of the consumption good, that they sell at price $p$. Firms incur in fixed production costs $f_e$ in every period and produce using some inelastically supplied labor $L$ as only input.

We extend this basic framework in three ways. First, we assume that labor costs are made of two components. The first one is a salary $w$ per efficiency-units of labor, which is assumed exogenous for simplicity. The second one is a fixed component $F$. Both variable and fixed labor costs depend on a productivity draw $\phi$ at $t = 1$ that is observable only by firms, so that total labor costs are equal to $\frac{wL}{\phi} + F(\phi)$. Given that the model features just two periods, we take fixed costs to incorporate longer term dynamics in labor costs. We interpret the fact that the fixed component is increasing in productivity as a proxy for the fact that firms might need to train highly-productive workers, thus making labor a quasi-fixed factor of production, like in Oi (1962).[1]

The second feature of the environment is a working capital constraint, that forces firms to obtain credit from banks in order to pay in advance a fraction of total salaries. At $t = 1$, banks make take-it-or-leave-it credit offers $M$ at a fixed interest rate $R$. Firms use credit to pay a fraction $\delta$ of total salaries before production, hence:

$$M \geq \delta \left( \frac{wL}{\phi} + F(\phi) \right). \tag{20}$$

The working capital constraint is a way to account for the fact that Portuguese firms have labor as their main production input, and depend on banks as their almost unique source of credit. Moreover, the assumption that banks observe productivity stands for the significance of relationship banking in Portugal (Bonfim and Dai, 2017). Finally, the take-it-or-leave-it structure of credit offers replicates the balance of bargaining power that we observe in Portugal, between few large banks and several small borrowers.

The last feature of the environment is an incentive constraint, that rationalizes the working capital constraint. More precisely, firms, after selling their products, can run away from their previously agreed commitments and retrieve a fraction $1 - \theta$ of their total proceeds, while not paying the remaining part of workers' salaries $1 - \delta$. As in Bigio (2015), this assumption is a consequence of the information structure of the environment, in particular that workers do not observe productivity. Firms can also partially run away from banks, in which case are forced to repay a fraction $1 - \theta^B$ of the credit obtained. Taken together, these

[1] The model does not feature capital. Given this simplification productivity can refer to labor and total factor productivity altogether.

assumptions imply that firms are subject to the incentive compatibility constraint:

$$pq - (1-\delta)\left(\frac{wL}{\phi} + F(\phi)\right) - (1+R)M \geq (1-\theta)pq - (1-\theta^B)(1+R)M. \tag{21}$$

Firms maximize profits by choosing price and the fraction $\delta$ of total salaries to pay in advance:

$$\max_{\delta,p} \ \Pi = (p-c)q, \tag{22}$$

subject to the working capital constraint in (20) and the incentive compatibility constraint in (21). By monopolistic competition, the CES demand function is:

$$q = \frac{p^{-\sigma}}{P^{1-\sigma}} E, \tag{23}$$

where $\sigma > 1$ is the elasticity of substitution across varieties, $P$ is the CES price aggregator, and $E$ is total expenditure. The definition of marginal costs is:

$$c = \frac{1}{q}\left[(1-\delta)\left(\frac{wL}{\phi} + F(\phi)\right) + (1+R)M\right]. \tag{24}$$

Attach the Lagrange multipliers $\chi$ to the incentive compatibility constraint and $\lambda$ to the working capital constraint. The first-order condition with respect to $\delta$ reads:

$$\left(\frac{wL}{\phi} + F(\phi)\right)(1-\lambda+\chi) = 0. \tag{25}$$

This clearly yields $\lambda = 1 + \chi > 0$, meaning that the working capital constraint is always binding and in equilibrium:

$$\delta = \frac{M}{\frac{wL}{\phi} + F(\phi)}. \tag{26}$$

The first-order condition with respect to $p$ instead reads:

$$\left[q(p) + pq'(p)\right](1+\theta\chi) - cq'(p) = 0, \tag{27}$$

which yields prices as a mark-up over marginal costs:

$$p = \frac{\sigma}{(\sigma-1)(1+\theta\chi)} c. \tag{28}$$

The mark-up is a decreasing function of the elasticity of substitution across varieties, of the tightness of the incentive compatibility constraint (as represented by the Lagrange multiplier $\chi$) and of the fraction $\theta$ of total proceeds that firms cannot retrieve by running away and renege on their commitments with workers. From (28), we derive the labor share as:

$$S \equiv \frac{cq}{pq} = \frac{\sigma - 1}{\sigma}(1 + \theta\chi). \tag{29}$$

The labor share turns out to be increasing in the elasticity of substitution across varieties, in the tightness of the incentive compatibility constraint and on the irretrievable fraction of total proceeds. While the first channel is a straightforward consequence of monopolistic competition, the other two come directly from the incentive compatibility constraint. The higher a firm's incentives to renege on salaries are (low $\theta$), the higher equilibrium mark-ups need to be in order for firms to stick to their commitments with workers, and as a consequence the lower the labor share.

The characterization of labor shares together with the definition of marginal costs allows us to study the relationship between productivity and labor shares. We distinguish two cases, depending on whether the incentive compatibility constraint is slack or not. In the first case (i.e. $\chi = 0$), the problem yields the standard result of prices as a constant mark-up over marginal costs: $p = \frac{\sigma}{\sigma-1}c$. A direct consequence of this is that labor shares are also constant across productivity draws, i.e. $S = \frac{\sigma-1}{\sigma}$.

If the incentive compatibility constraint is instead binding (i.e. $\chi > 0$), by complementary slackness the incentive compatibility constraint must hold with equality, and total firm proceeds are:

$$pq = \frac{1}{\theta}\left[\frac{wL}{\phi} + F(\phi) + (\theta^B(1+R) - 1)M\right], \tag{30}$$

where we also made use of the fact that the working capital constraint must hold with equality in equilibrium. Consequently, labor shares now read:

$$S = \frac{\frac{wL}{\phi} + F(\phi) + RM}{\frac{1}{\theta}\left[\frac{wL}{\phi} + F(\phi) + (\theta^B(1+R) - 1)M\right]}. \tag{31}$$

From the latter expression, we can prove the following:

**Proposition 1.** *When the incentive compatibility constraint is binding, labor shares are increasing (decreasing) in productivity if*

$$\phi\frac{F'(\phi)}{F(\phi)} < (>)\frac{wL/\phi}{F(\phi)}.$$

*Proof.* The derivative of $S$ with respect to $\phi$ is:

$$\frac{\partial S}{\partial \phi} = \frac{1}{pq}\left(1 - \frac{S}{\theta}\right)\left(-\frac{wL}{\phi^2} + F'(\phi)\right). \tag{32}$$

From (31), notice that $\frac{S}{\theta}$ is always larger than 1 as $\theta^B < 1$. Then, the result follows. □

The proposition highlights the role played by fixed and variable labor costs for the relationship between productivity and labor shares. The condition states that it is possible find similarly productive firms adopting different labor shares, depending on their fixed (or better, slowly moving in a dynamic sense) component of labor costs. If fixed costs loom large in the labor costs structure and the elasticity of fixed costs is relatively low, it will be possible to find highly productive firms featuring a high labor share. These are the firms that in our event studies invest more in highly specialized workers. On the other hand, if the fixed component of costs if elastic to productivity, productive firms will prefer not to invest in this component of labor costs.

We employ this framework to analyze the reaction of firms' employment and exit to a credit shock. To this end assume that, fixed at $t = 1$ prices and the fraction of salaries paid in advance, at $t = 2$ firms are hit by an unexpected credit tightening that reduces the amount $M$ that they can borrow from banks. In equilibrium, by the binding working capital constraint the demand for labor is:

$$L = \frac{\phi}{w}\left(\frac{M}{\delta} - F(\phi)\right). \tag{33}$$

This implies that:

$$\frac{\partial L}{\partial M} = \frac{\phi}{\delta w} > 0 \qquad \frac{\partial^2 L}{\partial M \partial \phi} = \frac{1}{\delta w} > 0. \tag{34}$$

As the working capital constraint is binding, firms react to an unexpected credit tightening (i.e. a drop in $M$) by lowering employment, and the more so the more productive they are.

We use (30) to derive total profits of a constrained firm as:

$$\Pi^{CONS} = \left(\frac{1}{\theta} - 1\right)\left[\frac{wL}{\phi} + F(\phi)\right] + \left[\frac{\theta^B(1+R) - 1}{\theta} - R\right] M. \tag{35}$$

From here, it is straightforward to derive the following result:

**Proposition 2.** *Profits of a constrained firm are increasing in credit if:*

$$\theta^B > \frac{1 + \theta R}{1 + R}. \tag{36}$$

Intuitively, both a firm's marginal costs and total proceeds are increasing in credit size. The second effect dominates the first if the fraction of loan repayment that the bank forbears is sufficiently high.

The result in Proposition 2 allows us to characterize firm exit. When profits drop below the fixed costs $f_e$ as a consequence of a credit tightening, the firm does not operate. In other words, a credit tightening can lead to exit, and the more often the higher a firm' s labor share is, under the conditions of Propositions 1 and 2. Therefore, the labor share is critical to understand the effect of a credit shock. Intuitively, the labor share is increasing in the tightness of the incentive compatibility constraint, which is more binding the higher a firm' s marginal costs. Hence, what matters to explain the relationship between the labor share and labor productivity is the relationship between marginal costs and productivity. In turn, this depends on marginal fixed labor costs: if they are sufficiently low, total marginal costs turn out increasing in productivity, and more productive firms will have high labor shares.

To sum up, the model predicts that if marginal fixed labor costs are sufficiently low, high-productive firms will be hit the worst by the credit shock. This result motivates our focus on fixed labor costs like training, that make labor a quasi-fixed factor of production.

# B Average firm level results

Our baseline empirical specification for average firm level results follows a standard difference-in-differences design. We collapse our dataset at a pre- and post-period level, by averaging our outcome variables over the two periods. Then, we run the following regression:

$$log(Y_{i,t}) = \gamma_i + \tau_t + \left(\beta S_i + \mathbf{\Gamma X_{i,pre}}\right) \cdot \mathbb{1}\{t = Post\} + FE_{i,t} + \varepsilon_{i,t} \quad t \in \{Pre, Post\}. \tag{37}$$

In the specification, $Y_{i,t}$ is the average outcome variable in the period of consideration, $\gamma_i$ is a firm fixed effect, $\tau_t$ is a time fixed effect, $S_i$ is the treatment variable that we instrument in the 2SLS regression with the instrument $Z_i$, $X_{i,pre}$ are a set of out-of-sample controls at the firm level in 2005, and $FE_{i,t}$ is a further set of fixed effects by pre/post period. We interact controls with a dummy equal to 1 for the post-period years (from 2009 to 2013) to allow differential trends over the post-period (their baseline effect is captured by the firm fixed effect $\gamma_i$). Fixed effects in the pre-period are absorbed by the firm fixed effect, and thus their influence captures differential group-specific trends in the post period. Consistent with the loan level specifications in Table E.10, we cluster the standard errors at the main bank-industry pair level.

Table E.12 reports the results from estimating Equation (37) with the logarithm of the number of employees as an outcome variable. The first column of Table E.12 reports the results of a standard difference-in-differences with no additional controls. In the second column we add the set of fixed effects that we use in all regressions throughout the empirical analysis. In column 3 we add as controls all the variables for which the balance-check test fails (see Figure F.9) and in column 4 we get to the main specification that we will use throughout the paper, with the full set of controls. Results in columns 3 and 4 show that the correlation of the instrument in 2005 with some observables does not have a relevant effect on the coefficient of interest. Given that we are averaging the outcome variable over a different number of observations in the post period due to firm exit or attrition from the QP, in column 5 we report the estimated coefficient over a sample of survivor firms. This last estimated coefficient more precisely characterizes the intensive margin of firm adjustment. The first stage effective F-statistic is always above 30 and not far from the 5% Nagar bias thresholds (which is at 37.42 according to the methodology in Montiel Olea and Pflueger, 2013), showing that the instrument very strongly predicts the variation in credit.[2]

Our preferred estimates range from 0.071 (column 4, full sample) to 0.086 (column 5, survivors). To add context to the magnitude of our estimates, in the post-period the predicted treatment after the first stage regression has an average of -0.183 (median -0.204), a standard deviation of 0.565 and a 10–90 percentile range of 1.532. A one negative standard-deviation variation in the treatment would decrease the average firm employment by approximately 4 percentage (log) points (4.9 percentage points according to the survivors' estimate). Given that the average employment variation in our sample is -0.044 (median -0.040) and the standard deviation in employment is 0.288, the shock has significant economic size. The economic impact is even more prominent, given that in the later years in our sample Portugal was suffering form the EU sovereign debt crisis. The debt crisis dynamics may have been correlated with our shock, but they are unlikely to have been predominately determined by it.[3] One standard deviation in our shock explains

[2]In an acid regression the instrument is not significant, whereas the variation in credit obviously is. Results available upon request.

[3]Consistently with the evidence presented in Table E.10, the coefficients in these regressions hardly move if we add direct controls for firm-level weighted exposure to sovereign debt at the end of 2009.

between 14 and 17 percent of the standard deviation in employment. The amount of variation explained by the shock is comparable to recent related studies (Bentolila et al., 2017, Berton et al., 2018).

Table E.13 shows the estimates of the elasticity of the wage bill (either full or base wage) in specifications where we control for the full set of controls and fixed effects, and consider both the full sample and the survivors. The estimates have a similar degree of precision as the employment ones, and the wage bill appears to have a higher elasticity to the shock with respect to employment. This might indicate that wages were being cut, or that there were compositional effects in firing/hiring. The flexible components of pay do not display a different volatility to the shock compared to base pay, indicating that firms cannot cut extra compensation more easily than base wages.[4] In the columns (5) and (6) we show the results of estimating a euro-to-euro sensitivity of payroll with respect to the cash-flow shock generated by the credit-supply variation. We scale the level variation in salaries between the pre- and post-periods and the variation in credit at the numerator of $S_i$ in Equation (1) by the pre-period average level of sales. The estimated euro-to-euro sensitivity is 0.17 for the full sample and 0.23 for the survivor sample, which should deliver more precise estimates for the wage bill and employment for the entirety of the post-period. These values are close to standard values estimated in the literature.[5] The last column reports results of running the following linear probability model on a dummy which is 1 upon firm exit:

$$P(exit_{i,t}) = \tau_t + \beta S_i + \mathbf{\Gamma X_{i,pre}} + FE_{i,t} + \varepsilon_{i,t}, \tag{38}$$

The credit shock has a substantial impact on the chances of firm survival. According to the estimates, a one-percent standard-deviation drop in the predicted treatment would increase the probability of firm exit between 0.63 percentage points per year, against an average exit rate of approximately 5 percent. The difference in the likelihood of firm exit for a firm exposed to the $10^{th}$ percentile of treatment as opposed to the $90^{th}$ would be 1.6 percentage points per year.

To assess the timing and persistence of the effects of the credit shock, we run a dynamic specification of the previous difference-in-differences:

$$Y_{i,t} = \gamma_i + \tau_t + \sum_{k \neq 2008} (\beta_k S_i + \mathbf{\Gamma_k X_{i,pre}}) \cdot \mathbb{1}\{t = k\} + FE_{i,t} + \varepsilon_{i,t} \tag{39}$$

where a different treatment coefficient is estimated for each year $k$. We normalize the treatment to be 0 in 2008, so that all the other treatment coefficients in the regressions can be interpreted as variation in the outcome with respect to its level in 2008. In this specifications the outcome variables are always expressed as ratios of the level of the outcome over its average in the pre-period. This means that the regressions are performed on the percentage change with respect to the average pre-period level of the outcome. We run these event-study regressions on survivor firms only, whom we identify through the CB.

As evident from Figure F.11, the treatment does not show pre-trends. Moreover, it has persistent effects

[4]This finding is confirmed by results on hours and normal hours, available upon request, which show that estimated elasticities of work hours to the shock are almost identical to the elasticities of employment.

[5]Schoefer (2022) provides a review of the values of the cash-flow dollar-to-dollar sensitivity estimated in the literature. Plausible estimates range from 0.2 to 0.6, and he calibrates his model for the US economy to obtain a 0.25 sensitivity, close to our intensive margin estimate. Barbiero (2019) estimates cash-flow sensitivities between 0.2 and 0.3.

that accumulate over time, weakly waning only in 2013.[6]

Table E.14 shows the results of estimating Equation (37) on balance-sheet and other financial variables for our sample of survivor firms. The outcome variables are total assets, fixed assets (sum of tangible and intangible assets) and current assets, cash, sales, trade credits and debts to suppliers.[7] Total assets appear strongly responsive to the shock, with an estimated coefficient of 0.098, which has similar magnitude as the employment and wage bills coefficients. When we break down the effects by fixed and current assets, we see that the result is entirely driven by current assets, whereas the elasticity of fixed assets is not significantly different from 0, despite the fact that its magnitude is quite comparable to the employment estimate. In a similar fashion, we estimate a sizable and significant (at the 10-percent level) elasticity of trade credits to the credit shock, possibly indicating that negatively hit firms ran down their existing trade credits over time while positively hit firms were willing to let their trade credits stock grow vis-à-vis their customers. We do not identify a significant elasticity for sales, cash or debt with respect to suppliers, possibly indicating that this alternative means of extracting liquidity from suppliers up the production chain was not readily available to affected firms.[8]

# C Data description and cleaning

## C.1 Available datasets

### C.1.1 Labor market data: Quadros de Pessoal

The *Quadros de Pessoal* (henceforth QP) is a longitudinal matched employer-employee dataset, containing detailed data at the workers' and firms' level on employment composition for the firms and individual worker characteristics. The data are collected and managed by the Ministry of Labour and Social Solidarity, that draws on a compulsory annual census of all the firms employing at least one worker at the end of October each year. It does not cover the public administration and non-market services, whereas it covers partially or fully state-owned firms, provided that they offer a market service. The dataset covers approximately 350,000 firms and 3 million employees per year. In 2010 the structure of the survey was reformed

[6]Interestingly, the only year in which the point estimates for employment and wage bill are clearly different is 2010, when Portugal briefly exited the recession and the EU sovereign debt crisis dynamics had not fully materialized. This is the only year in our sample in which we have some weak evidence that firms adjusted employment through temporary contracts, and it is plausible that some of the terminated workers were substituted with new hires at lower wages. These dynamics would explain why, despite wage rigidity, we observe a higher elasticity of the wage bill with respect to employment.

[7]We take the logarithm of the variables with only positive support, and the asinh of the variables that can take negative values, because defined in net terms.

[8]In results available upon request we show that, by using an empirical specification as in Almeida et al. (2011), fixed capital growth is not responsive to short term capital variations, but only to long term credit variations for those firms which had a big share of long term credit (more than 20%) maturing right before the shock. As we find in Appendix Table 4 we find that firms more subject to labor rigidities, for which we indeed detect significant elasticities of capital investment adjustment, are also firms that have a relatively higher share of short-term financing to start with. Consequently, the average effect on capital investment is also plausibly a by-product of compositional differences in financing across firms.

and the QP was incorporated into the *Relatório Único*, an integrated reporting system to enable employers to easily provide more extensive information on workers to the Ministry. As a consequence, some very small entrepreneurial firms were exempted from filing compulsorily the questionnaire, which is why after 2009 the coverage of QP is less complete than in previous years.[9]

The dataset is available at the Bank of Portugal from 1982 to 2013, and is hierarchically made up by a firm-level dataset, an establishment-level dataset and a worker-level dataset. The firm level dataset contains information on the firm location (from regional to very narrowly defined parish level, which roughly corresponds to a neighborhood, industry of operation (CAE rev. 2.1 until 2006 and CAE rev. 3, based on NACE-Rev. 2 Statistical classification of economic activities in the European Community), total employment, total sales, ownership structure and legal incorporation. Analogous information is available on the establishment-level dataset.

The worker level dataset provides detailed information on worker characteristics and contracts. Information included comprehends workers' gender, age, detailed occupational code (the *Classificação Nacional de Profissões* (CNP94) up to 2009 and the *Classificação Portuguesa das Profissões* (CPP2010) from 2010 onward, which is based on ISCO08 International Occupational Classification Codes), detailed educational level, qualification within the firm (managerial qualification, specialized workforce or generic workers, besides trainees). At the contract level it is possible to know the precise hiring date, the kind of contract (various typologies that generally define the contract as fixed-term or open-ended), the hours arrangement (full-time versus part-time), the effective number of hours worked, and information on the compensation. More specifically, for each worker it is possible to obtain information on the base pay, any extra paid in overtimes or other extra-ordinary payments and other irregularly paid components. In contrast, there is no information on social security contributions. We winsorize the extreme 0.5% tails of the distribution of wages.[10]

The unique worker identifier is based on the workers' social security number, and given the extensive work on the part of the Ministry to control and certify the quality of the data in this administrative dataset, the coverage and reliability of the data is quite high (except for the discrete break in coverage for only a subset of firms in 2010 due to the new reporting requirement in the *Relatório Único*. Given that other datasets in our analysis cannot cover the same time-span, we only focus on years from 2005 to 2013 (but potentially control for observables up to 2003 in some empirical exercises).

We use the QP to extract information regarding wage policies at the firm level. This is the dataset we use in order to compute AKM firm level fixed effects, which describe what component of workers' compensation is firm specific and pertains to average firm wage policy. We compute AKM fixed effects through an AKM regression, which is a wage regression at the worker level on firm and worker fixed effects and other workers' characteristics. The firm fixed effect captures overall generosity of payments while the individual fixed effect should capture unobservable skills. In our AKM specifications we control for sex, a third polynomial of age and educational categories dummies, all variables present in QP itself.

[9]Despite this inconvenience, we use the firms' balance sheet dataset, the *Central de Balanços*, which covers all non-financial corporations in Portugal, to correctly disentangle firms' failures. This also means that in our analysis the "survivors" sample is not necessarily a balanced sample.

[10]As regards the qualification categories, the Portuguese Decree-Law 380/80 established that firms should indicate the qualification level as in the Collective Agreement. If this is not available, firms should select the qualification level of the worker. These categories are based on the degree of complexity of tasks that the worker performs within the firm (from more basic, routine tasks to more discretionary managerial ones). The categories are defined within a 9 levels hierarchy, that we simplify into three broad categories.

We run the AKM regressions on worker level data from 2003 to 2008.

### C.1.2 Firm level financial statement data: Central de Balanços

The *Central de Balanços* (henceforth CB) is a firms level balance-sheet and income statements database, managed by the Bank of Portugal. It consists of a repository of yearly economic and financial information on the universe of non-financial corporations operating in Portugal from 2005 to 2013. It includes information on sales, balance-sheet items, profit and loss statements, and cash flow statements (after 2009) for all private firms in Portugal.The CB builds on the *Informaçào Empresarial Semplificada*, an administrative firms' balance-sheet dataset managed by the Ministry of Finance and Public Administration. The Bank of Portugal obtains the data from the Ministry and performs extensive consistency checks to guarantee that the data are reliable and consistent over the years.
The dataset in its present form covering the universe of firms is based on information reported in the starts in 2006, even if almost the entirety of firms already existing in 2005 provided balance sheet data for that year as well together with the 2006 filing. For this reason, we actually have a very high coverage of firms' balance sheets for 2005 as well. Before 2005 the CB maintained by the Bank of Portugal was actually a survey only for the biggest firms in the country. However, given the substantially lower coverage of the population of firms before 2005, we do not rely on that data.
After 2009, in order for the data to comply with international accounting standards, there has been a major overhaul of the variables definitions in the dataset, from the *Plano Oficial de Contabilidade* (POC) to the *Sistema de Normalização Contabilística* (SNC). In all our computations, unless otherwise noted, we have personally gone through a variables' harmonization process, in collaboration with the statistics department managing the administrative datasets for researchers at the Bank of Portugal, BPLim, to guarantee comparability across periods.

The dataset contains a great amount of information on firms' balance sheets and income statements, even if the harmonization process between 2009 and 2010 makes it at time difficult if not impossible to keep consistent records for all balance sheet variables in the dataset. We use the dataset to obtain information on total assets, fixed assets, current assets total debt (not just bank debt) and interest expenditures, cash-flow and capital expenditures (after 2009), cash balances, exports and export status, trade credits, debt towards suppliers, inventories, return on equity, assets and sales, salaries, total employee related , revenues, costs and breakdowns (among which intermediate inputs, materials and services), profits. We computer value added from this dataset by adding back employee related expenditures to the firm EBITDA (which should correspond to subtracting expenditures on intermediate goods from total sales).

Given the dataset time-consistent coverage of firms operating in Portugal, we use it to identify firm exits as well. The procedure to identify a firm exit combines different criteria. Firstly, we rely on the CB on categorization of whether a firm is active, suspended activity or closed down. Secondly, we flag all the cases in which the firm will end up having 0 employees the next year but does have a positive number of employees in a given year. Thirdly, we actually check whether a firm disappears from the dataset in any given year that is not 2013 and does not re-appear at any time (and does not simply have, consequently, a gap in the data). Lastly, we label as exits the instances in which a firm disappears for more than two years, as it is likely that if the identifier reappears later it has just been reassigned to another firm (an assumption that seems to be validated by the observation that when such instance takes place the firm seems different in terms of size and sector between the two periods). In all the cases we select the criterion of exit, in case a firm matches more than one at different points in time, by looking at the case in which the firm "closed down" with the highest number of employees or, if ties are not resolved, with the lowest EBITDA.

### C.1.3 Credit exposure level dataset: Central de Responsabilidades de Crédito

The *Central de Responsabilidades de Crédito* (henceforth CRC), is the credit registry of the Central Bank of Portugal. The dataset features available for our period of analysis (up to 2013) features bank-firm exposures above EUR50 by the universe of Portuguese credit institutions at the monthly level. The dataset does not contain credit exposure by foreign banks towards Portuguese firms, but can obviously contain credit from Portuguese banks to foreign owned firms residing and operating in Portugal.[11]

The dataset is regularly employed for supervisory purposes, and by the credit institutions themselves to obtain information on potential debtors. It contains detailed information on the number of credit relationships, the corresponding amounts and the kind of exposure: short- and long-term, credit granted but still not materialized (potential), credit overdue, written-off or renegotiated. From 2009 onwards, but unfortunately not before, it is possible to obtain information closer to loan-level (i.e. it is possible to keep track of exposures which consist into the sum of loans with very detailed similar characteristics instead of seeing an aggregate number by kind of coarsely defined exposure) and more details about the exact maturity of each exposure and the collateral posted by each firm, if any (real collateral or guarantees, fraction of the value of the loan backed by it). Given the nature of our analysis and the period of interest, we mostly focus on obtaining a consistent representation of the information available in the dataset before 2009. For our analysis and given the time frequency in other data sources we average debt exposures at the yearly level. We use "regular" credit in our specifications as measure of credit, which corresponds to credit in good standing and in use by the firm. Credit is defined as short-term if the maturity is below 1 year or it is a credit line with undefined maturity (post-2009 data) or is categorized as commercial, discount or other funding short-term pre-2009. We group together short-term loans, credit lines with defined short-term maturity and credit lines with undefined maturity because the latter category of credit lines comprehends all those exposures that, once withdrawn by the customer, should undergo renegotiation with the bank in order to be rolled-over. This feature makes them very liquid instruments that, similarly to short-term loans, is subject to short-term credit rates volatility and rollover risk. Credit lines always constitute above 3/4 of short-term credit as we define it. Long-term credit is thus obtained as the remainder in regular credit.

### C.1.4 Banks balance sheet dataset: Balanço das Instituições Monetárias e Financeiras

The "Balanço das Instituições Monetárias e Financeiras" (henceforth BBS) is the balance-sheet dataset for credit institutions that we employ. It is a proprietary dataset of the Bank of Portugal with the balance sheets of the universe of financial monetary institutions operating in the country. The dataset is utilized by officers of the bank in order to monitor the health of financial monetary institutions operating in the country and

[11] We do not believe that this fact could be a source of significant bias in any of our results, as the Portuguese economy mostly features relatively small and arguably bank-dependent firms, and for the biggest firms it is more likely for them to access directly debt markets instead of creating ties with foreign banks. Most foreign banks, moreover, operate Portugal incorporated subsidiaries in the country, the credits of which would regularly appear in the CRC.

the overall stability of the system. In the dataset, for each balance-sheet item (liability or asset) it is possible to see which is the kind of counterparty involved (i.e. the kind of institution, government, private or non-governmental body, creditor or debtor), the maturity of the item in question if relevant (time deposits, on demand deposits, interbank long-term or short-term exposures) and the nationality of the counterparty (extra-EU or each EU country separately). The data are reported at the monthly level.

The measure of interbank funding which is the basis of our instrument is computed from this dataset as the ratio of the average (yearly) short-term foreign interbank borrowing by the bank over total assets. Foreign short-term interbank borrowing is computed as the sum of short-term deposits with maturity up to 1 year and repos where the counterparty if a foreign financial institution (obviously not a central bank).

In matching the BBS and the CRC, we also took care of harmonizing and making bank definitions consistent across datasets given the existence of many mergers and acquisitions in the Portuguese banking system during the period. Each M&A event between 2000 and 2013 (for institutions with at least 1 percent of total credit in a given month) was taken into consideration in order to make sure that credit flows across institutions were rightly accounted for, and definitions of bank codes across datasets and across time were consistent.

### C.1.5 Banks balance sheet dataset: Sistema Integrado de Estatísticas de Títulos

The *Sistema Integrado de Estatísticas de Títulos* (henceforth SIET) is a proprietary dataset of the Bank of Portugal. It includes debt securities (i.e. banknotes, commercial papers, bonds, etc.) with maturity both short term (up until 1 year) and long term (more than 1 year), and capital (i.e. shares and other means of participation) but neither derivatives nor REPOs. For both debt securities and capital, SIET collects data about emissions and portfolio holdings. For emissions, SIET collects flows and stocks relative to national issuers, on a title-by-title and issuer-by-issuer bases. For portfolio holdings, SIET collects flows and stocks on an investor-by-investor and title-by-title basis. Through SIET we obtain holdings of sovereign debt, or more in general any government-issued debt instrument held by banks on their balance sheet.

### C.1.6 Commuting zone definitions

Given the relevance of the concept of commuting zone, especially for the analysis of labor market reallocation, we obtained data on the definition of commuting zones for Portugal from Afonso and Venâncio (2016).

### C.1.7 Labor market data: Occupational Information Network

Given the availability of definitions of occupations at the worker level in the QP, we were able to obtain occupation characteristics through the Occupational Information Network (O*NET) database. The O*NET database is a widely used database in labor economics and is the primary source of data in the United States for categorization of occupation characteristics. It is based on the combination of the analysis of responses to questionnaires on occupations administered to sampled employers and employees, and is updated four times a year with new data or updates to current categorizations.

We used O*NET in order to create indexes on job categorizations in terms of education, experience and training requirements. For each occupation a categorization is provided regarding the level of experience required (with possible scores ranging from 1 to 12, from less than high-school to post-graduate level), the level of previous experience (from 1 to 11, from none to more than 10 years), the level of on-site training (classes, courses, instructions sessions organized by the employer) or on-the-job training (that is, work carried out under the supervision of more experienced workers) required to being able to carry out the required tasks (from 1 to 9, from a short demonstration to years of training). Moreover, we also extracted for each occupation the categorization of the "job zone" (with a score from 1 to 4 in ascending order of "sophistication" of required vocational preparation levels), which is a further categorization created by expert O*NET analysts that combines all the previous four categories in a unique index. We obtained a separate occupational index as well for each category by averaging the scores, taking into account the frequency of each score for each response.

In order to combine the data, we first worked on making profession definitions consistent across time in our dataset, and then merged our occupational code to O*NET through a ISCO08-ONETSOC10 crosswalk. Given the change in occupational codes from the *Classificação Nacional de Profissões* (CNP94) to the new *Classificação Portuguesa das Profissões* (CPP2010) in 2010 in order to update the categorization and making in compliant to the *International Standard Classification of Occupations* (ISCO2008) categorization, we created a crosswalk based on the frequency of cross-occupational code changes from 2009 to 2010 in the QP within the same firms. We used the cross-walk in Hardy et al. (2018) to merge our ISCO08 codes to (ONET)SOC10 (Standard Occupational Codes). We then averaged all the occupational scores and indexes obtained from ONET across occupations in order to obtain a time consistent 3-digits ISCO08 occupational categorization.[12]

We used O*NET version 23.3, and more specifically the education, training and experience files.[13]

## C.2 Sample selection

In order to prepare the data for the analysis in our event-study, we need to combine all the different sources of data available, and perform cleaning checks to obtain a relevant sample of analysis depending on variables availability and firms' and banks' characteristics.

Given that the focus of our analysis is predominantly the adjustment of employment and other real variables as a function of the different measures that we label as different sources of "labor rigidities" in the text, the main firm-level dataset around which we combine the other datasets is the QP.

First, we perform some quality checks on the QP and remove workers' for which identifiers are not consistent over time.[14] We then select only workers listed as "employees", full-time, between 16 and 65

[12]The fact that obviously the occupational categorizations are neither bijections nor injections across sets made it difficult in some cases to reassign the occupational codes. We tried to use the official crosswalk at first, but noticed that it created very big discontinuities for the frequency of observation of some professions. We noticed on the other hand that within firms changes in occupational codes seemed to be very consistent, and as such a more valid "revealed preference" categorization on the part of employers of their employees actual occupation. We then decided to limit ourselves to a 3-digits categorization in order to have a meaningful number of workers for occupation, and in order to minimize the inconsistencies in the cross-categorizations of occupational codes between CNP and CPP.

[13]https://www.onetcenter.org/dictionary/23.3/excel/education_training_experience.html

[14]For the period 2005-2013 the problem is actually marginal.

years of age, and receiving a full wage in the October of every year (e.g. not on sick leave or other forms of leave).[15] As regards monetary balance-sheet variables, wages and credit variables, we deflate all nominal values in the analysis by the 2013 consumer price index.[16]

In order to define the final sample of analysis we merge all datasets and select firms based on some defined criteria. Given that we are interested in both firm dynamics and employment adjustment, we mostly consider firms present both in QP and CB.[17] We restrict our attention to firms in mainland Portugal, and exclude from the sample industries like agriculture, fishing, energy (extraction, mining and distribution), the construction sector and the financial sector itself. For the event study we only consider firms with a credit relationship with any bank in 2005, which of course must survive until 2009 to be present in the period of time after the credit shock. We focus on firms with at least 9 employees, which is approximately the threshold for the fourth quartile in the distribution of firms' sizes in the years before 2009, and covers more than 60 percent of the workforce in the QP matched to CRC in the pre-period. In order to reduce measurement noise, we consider only firms with no gaps in the data in the pre-period.[18,19]

We also perform some consistency and sanity checks in selecting the relevant banks to be included in the analysis. More precisely, we exclude from the analysis the very small banks that disappear from the dataset before 2009. We also exclude from the set of banks for which the instrument is computed those banks for which foreign interbank funding is actually intra-banking-group funding from the foreign headquarter to the Portuguese subsidiary.[20]

To limit the influence of outliers in the regressions, we drop firms in the top 2.5 percentile of positive credit variation between the pre- and post-periods. For the same reason we drop all the firms with a percentage of exposure-amount growth above the top 2.5 percent of the distribution in the exposure level specifications. This effectively amounts to eliminating more than 2.5 percent of firms for those particular regressions, but we still think that this kind of cut is more sensible than leaving the firms in the estimation sample without accounting for all their loans.

Our final sample spans 14,846 firms and 31 banks.[21]

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[15]We also remove records with unreasonable number of hours worked and perform other sanity checks or within-worker's records harmonization on other variables, such as date of birth, hiring dates, workers' characteristics.

[16]For the productivity estimation the deflation of nominal values is performed at a much greater level of detail and precision depending on each item and industry. We refer the reader to Appendix D for the details of the estimation.

[17]Most of the firms with partial state ownership are not in CB. Hence, they can be included in the most basic analyses, but not in those that feature firm balance-sheet controls.

[18]Considering firms already existing in 2005 allows us to have at least 3 years of pre-period in our event study framework. We implicitly exclude entrants in the three years before 2009 from the event study analysis.

[19]We focus on relatively big firms, at least by Portuguese standards, as we are interested in measuring employment adjustment at the firm level, which becomes increasingly noisy and lumpy for very small firms, and because of the fact that the QP coverage of Portuguese firms is full for relatively big firms but decreases for very small firms after 2009.

[20]The cases for which this happens are very few and do not represent more than 1 percent of total credit at any point time. We cannot disclose any detail on the names of the banks in question.

[21]Most of the regressions which require also balance-sheet variables consists of 13,804 firms, while the sample of surviving firms consists of 11,802 firms. At least for the employment and balance-sheet items regressions, though, results are virtually unchanged if we just restrict our attention to specifications in which fixed effects that do not require the CB are utilized (see Section A), and cover the entire sample.

# D Production function estimation

## D.1 Productivity and output elasticities estimation

For the estimation of output elasticities, markups and ultimately revenue total factor productivity (TFPR) we use different methodologies. First of all, we consider a three-factors of production gross output ($y$) function, where factors are labor ($l$), physical capital ($k$) and an intermediate input ($m$). We consider both a simple Cobb-Douglas specification where the elasticity of substitution among the factors of production is restricted to be 1 and a translog specification, which relaxes the above assumption. The (log-)production function is thus expressed as a function of log-inputs as:

$$y_{i,t} = f(l_{i,t}, k_{i,t}, m_{i,t}) + \omega_{i,t} + \varepsilon_{i,t} \tag{40}$$

where $f(l_{i,t}, k_{i,t}, m_{i,t})$ is

$$\beta_l l_{i,t} + \beta_k k_{i,t} + \beta_m m_{i,t} \tag{41}$$

in the Cobb-Douglas case, and:

$$\beta_l l_{i,t} + \beta_k k_{i,t} + \beta_m m_{i,t} + \sum_{x\in\{l,k,m\}} \beta_{xx} x_{i,t}^2 + \sum_{j\in\{l,k,m\},j\neq x} \quad \sum_{x\in\{l,k,m\}} \beta_{jx} j_{i,t} x_{i,t} \tag{42}$$

in the translog case. $\omega$ in the equation represents the firm's level of technical efficiency (or total factor productivity, TFP).[22]

In our estimation gross output is measured as total firm sales (coming from QP when available and using CB firm revenues, which correspond to the QP definition of sales, for all other firms), deflated by 2-digit industry gross output deflators. Labor is measured as the firm wage bill (coming from QP when available or using CB total salaries for all other firms), which differently from total headcount (or full-time equivalent count) partially accounts for labor quality, and is deflated by the consumer price index. The intermediate input is the sum of the cost of intermediate goods and supplied services, deflated by 2-digit industry intermediate inputs deflators. For physical capital we use a capital series that we constructed following the perpetual inventory method (PIM) in the baseline specifications or the book value of (net) fixed assets (both tangible and intangible). In the latter case the book value of (net) fixed assets is deflated by 2-digit industry capital goods formation deflator.[23] For the PIM on the other hand we estimate the following equation:

$$K_{i,t} = (1 - \delta_{i,t})K_{i,t-1} + \frac{I_{i,t}}{def_t} \tag{43}$$

at the firm level. Instead of using the book value of yearly depreciation for fixed assets, we use a level of 7

[22] The CES production function is a specific case of the general translog production function, and can be obtained by applying a second order Maclaurin approximation (which implies the parameterization of the Cobb-Douglas case as point around which the approximation is performed) to the log of $y = \left(\sum a_x x_i^{\rho}\right)^{\frac{\nu}{\rho}}$. The CES entails some specific parameters restrictions with respect to an unconstrained translog specification, which should thus be considered as a more general specification.

[23] All the price indexes for Portugal, apart from the CPI, are obtained from the OECD STructural ANalysis Database (STAN) (http://www.oecd.org/sti/ind/stanstructuralanalysisdatabase.htm).

percent for all firms.[24] From 2009 onwards we can measure directly firm level capital investment form the cash-flow statement (unavailable for earlier years) as the total yearly capital expenditure in both tangible and intangible capital formation. For the other years, or when the variable is missing, we use the variation in book fixed assets, deflated by the yearly capital goods formation deflator, as a measure of investment. We take the earliest year available level of fixed assets, deflated by the industry capital goods formation deflator, as starting value for the series. For incumbents firms in the dataset, the earliest year is 2005, and their starting value of real capital is thus just an approximation. We use the results based on PIM capital as our baseline.[25]

The estimation is carried out yearly, for all firms in the CB from 2005 to 2013, at a level of aggregation that is close to the 2-digit industry level.[26] For the estimation of output elasticities we remove from the dataset firms with a revenue labor share lower than or greater than 1 percent, firms with a revenue material labor share lower than 10 percent or greater than 1, and the firms with a sum of labor and material shares above 1.2. We also drop the lowest and highest 1-percent quantiles of labor and material shares. We are left with 275,093 unique firms and 139,735 firm-year observations on average.

Firm level (log) TFP is calculated as the residual from the estimation of the production function according to the various specifications. The estimated residual, the productivity shock, can be written as

$$\xi_{i,t} = \hat{\omega}_{i,t} + \nu_{i,t} = \omega_{i,t} + \varepsilon_{i,t} \tag{44}$$

where $\hat{\omega}$ represents the "transmitted" component of productivity (that is, the one that the firm takes into account while making input decisions) and $\nu_{i,t}$ should represent an unexpected shock. Given that the residual $\nu_{i,t}$ in the estimation might also arise because of any measurement error in output, inputs and prices, we calculate productivity either as the full residual from the production function estimation, or the residual

$$\hat{\omega}_{i,t} = \hat{y}_{i,t} - \hat{f}(l_{i,t}, k_{i,t}, m_{i,t}) \tag{45}$$

where $\hat{y}_{i,t}$ is obtained as the estimated gross output from a regression of output on a third order polynomial of all inputs of production. The latter form aims to eliminate any component of the realization of gross output that appears not to be related to the planned input choice, and remains unexplained by it, thus limiting the concern on the influence of measurement error. In the main text we show results based on this latter measure of productivity, but our results are qualitatively unchanged regardless of the measure we use. We use the full residual, as standard in the literature, for the productivity decomposition.[27]

The estimation of output elasticities and productivity generally presents problems related to the nature of input choice itself. On the one hand, input choice is likely to be very strongly correlated with (expected) productivity itself, and as such the direct estimation of the log production function by OLS would very

[24] 7% is less than the maximum level of depreciation tax deduction that firm would get by deflating capital the most each year. For this reason, even if imperfect, it is a plausible measure of yearly depreciation. Given that we are unable to decompose in a time-consistent way the subcomponents of capital formation, the approximation is necessary.

[25] Our results are qualitatively insensitive to the measure of fixed capital that we used for the estimation of output elasticities and productivity, and in many cases are also almost quantitatively indistinguishable.

[26] Given that there is a change in industry definitions in QP (see Appendix C) and some subgroups are small, we aggregate some of the subgroups. The resulting industry definitions are conceived to be time-consistent across the different CAE versions of industrial definitions.

[27] See Petrin and Sivadasan (2013) for a similar exercise.

likely be subject to biases given endogeneity determined by simultaneity.[28] On the other hand, there is generally an implicit selection bias for the firms observed in the dataset, given that more productive firm tend to be more resilient in normal times.

We address the first issue by following the literature in industrial organization on the identification by means of proxy variables (Olley and Pakes (1996), Levinsohn and Petrin (2003)).[29] This methodology consists into substituting unobserved productivity in the production function by a proxy variable, a choice variable assumed to have an invertible mapping with productivity itself. In our case, we use the intermediate input as the proxy variable (as in Levinsohn and Petrin (2003)).

The estimation is subdivided in two stages: in the first stage output is non-parametrically regressed on the inputs (and importantly, the proxy variable, which is an input in our case), in order to retrieve expected output and an estimate of the residual:[30]

$$y_{i,t} = \phi(l_{i,t}, k_{i,t}, m_{i,t}) + \varepsilon_{i,t} \tag{46}$$

We follow De Loecker and Warzynski (2012) and Ackerberg et al. (2015) in the estimation of all the relevant output elasticities at the second stage, which allows for consistent estimation even in presence of dynamic effects of the labor choice on the other inputs. The second stage estimation relies on the assumption that productivity at the firm level follows a Markov process:

$$\omega_{i,t} = g(\omega_{i,t-1}) + \eta_{i,t} \tag{47}$$

For a given guess of parameters **fi** one can obtain an estimate of productivity:

$$\hat{\omega}_{i,t}(\mathbf{fi}) = \hat{\phi} - (\hat{\beta}_l l_{i,t} + \hat{\beta}_k k_{i,t} + \hat{\beta}_m m_{i,t}) \tag{48}$$

for the Cobb-Douglas case of

$$\hat{\omega}_{i,t}(\mathbf{fi}) = \hat{\phi} - \left( \hat{\beta}_l l_{i,t} + \hat{\beta}_k k_{i,t} + \hat{\beta}_m m_{i,t} + \sum_{x \in \{l,k,m\}} \hat{\beta}_{xx} x_{i,t}^2 + \sum_{j \in \{l,k,m\}, j \neq x} \sum_{x \in \{l,k,m\}} \hat{\beta}_{jx} j_{i,t} x_{i,t} \right) \tag{49}$$

in the translog case. One can thus non-parametrically regress $\hat{\omega}_{i,t}$ on its own lag and obtain the estimated innovation to productivity $\nu_{i,t}(\mathbf{fi})$). It is then possible to estimate all the output elasticities and subsequently TFP by GMM relying on moment conditions of the form:

$$\mathbb{E}[\eta_{i,t}(\mathbf{fi}) z^j] = 0 \qquad j \in \{l, k, m\} \tag{50}$$

[28]Given Portugal's labor market institutional features, it would not look unreasonable to consider labor as a quasi-fixed input in production, with a greater degree of flexibility than capital but still less flexible. Our method of estimating labor elasticity is consistent regardless of this matter, but if labor not a fully flexible input in production it cannot be utilized to estimate firms' markups.

[29]The proxy-variable approach is the most frequently used in the industrial organization literature. Alternatives are fixed effects, first order conditions, the dynamic panel approach or the use of plausible instruments.

[30]We use a third order polynomial of inputs in this first stage regression.

in the Cobb Douglas case and

$$\mathbb{E}[\eta_{i,t}(\mathbf{fi})z^j] = 0 \qquad j \in \{l,k,m\} \tag{51}$$

$$\mathbb{E}[\eta_{i,t}(\mathbf{fi})z^j z^h] = 0 \qquad j,h \in \{l,k,m\} \tag{52}$$

in the translog case. The $z$ variables are instruments for the various inputs. Given the standard assumptions on input dynamics, $k$ can be a valid instrument of itself, whereas we use lags of labor and intermediate inputs as instruments, and according interactions for higher order terms.[31,32,33]

We address the problem of possible selection bias of firms into the dataset by trying to control for the probability of survival in the law of motion of productivity, as suggested in Olley and Pakes (1996). We actually augment the estimation of Equation (47) by adding the estimated survival probability obtained by fitting a probit model on year dummies and input levels.[34]

We compute productivity and elasticities for robustness by estimating the Cobb-Douglas and translog productions functions by straight OLS as well, adding year fixed effects to the estimation. All results in the main body of the paper are qualitatively (and quantitatively) robust to these different estimation procedures.

In the Cobb-Douglas case the estimated coefficients for each input are also output elasticities, which are consequently fixed within each industry (the Cobb-Douglas specification does not admit any variation in input revenue shares and elasticities across firms within the same estimation sample). In the translog case, on the other hand, the elasticity of substitution across any inputs is not restricted to be 1 and elasticities can vary depending on each firms' input mix utilized. For any input $x$, given the other two inputs $j$ and $h$, the estimated output elasticity can be obtained as:

$$\hat{\theta}^x_{i,t} = \hat{\beta}_x + 2\hat{\beta}_{xx}x_{i,t} + \hat{\beta}_{xj}j_{i,t} + \hat{\beta}_{xh}h_{i,t} \tag{53}$$

Tables E.15 and E.16 show average estimates of input elasticities using all the different estimation methodologies, and with different measures of the capital input. Reassuringly, the estimated elasticities and markups are in line with the recent studies performing similar estimations ( Blattner et al. (2019) for Portugal, Fonseca and Van Doornik (2022) for Brazil and Lenzu and Manaresi (2018) for Italy).

## D.2 Markups and marginal products

The estimation of output elasticities makes it possible to also estimate firms' markups and evaluate the marginal revenue product of inputs in production.

[31] In order for lags of wage bill and intermediate inputs to be valid instrument for their respective current values, one would need the prices to be correlated over time, an assumption that is quite plausible and surely confirmed in our data as regards the dynamics of wages.

[32] In the Cobb Douglas case we also add orthogonality conditions for the lag of capital and the second lag of intermediate inputs. Given the amount of parameters to estimate and the computing time required for the procedure, we do not add overidentifying restrictions in the translog case.

[33] If labor was indeed a dynamic input, the estimation of its elasticity would remain consistent anyway, as the orthogonality condition would a fortiori be valid for its lag.

[34] We carried out a the same procedure by augmenting Equation (47) with the estimated failure probability as in Antunes et al. (2016), but did not notice any material difference in final outcomes.

In order to estimate firm level markups, we rely on the procedure laid out by De Loecker and Warzynski (2012), who use the first-order condition of the flexible inputs to impute the ratio of prices to costs. We use the intermediate input for this task, given that, as discussed above, labor is likely to be a dynamic input in our context, and is surely subject to some degree of adjustment costs. The markup can be obtained as

$$\hat{\mu}_{i,t} = \hat{\theta}^m_{i,t} \left( \frac{P_{i,t} Q_{i,t}}{P^m_{i,t} M_{i,t}} \right) \tag{54}$$

As in De Loecker and Warzynski (2012), we can only imperfectly measure the expenditure share of materials in gross output, given the likely presence of measurement error in the estimation of Equation (40). For this reason, we divide gross output in equation (54) by $\exp(\hat{\varepsilon}_{i,t})$, the residual from the first stage regression in the production function estimation procedure. Per De Loecker and Warzynski (2012) this correction helps eliminating any variation in expenditure shares coming from variation in output not correlated with $\phi(l_{i,t}, k_{i,t}, m_{i,t})$, that is "output variation not related to variables impacting input demand".[35]

Given the estimated markups and elasticities, it is possible to obtain estimates of the distortion in labor and capital utilization, namely the differences (gaps) between their estimated marginal products and their cost. Taking into account a model in which firms compete monopolistically and choose their input demand level at each period, we can derive revenue marginal product (MRP) as

$$MRP^X_{i,t} \equiv \frac{\partial (P_{i,t}(Q_{i,t}) Q_{i,t})}{\partial X_{i,t}} = \underbrace{P_{i,t} \frac{\partial Q_{i,t}}{\partial X_{i,t}}}_{VMP^X_{i,t}} \Bigg( \underbrace{1 + \frac{Q_{i,t}}{P_{i,t}} \frac{\partial P_{i,t}}{\partial Q_{i,t}}}_{\mu^{-1}_{i,t}} \Bigg) = \theta^X_{i,t} \frac{P_{i,t} Q_{i,t}}{X_{i,t}} \frac{1}{\mu_{i,t}} \tag{55}$$

and as such MRP - cost gaps as

$$\text{MRPK-cost gap}_{i,t} = \hat{\theta}^k_{i,t} \frac{P_{i,t} Y_{i,t}}{K_{i,t}} \frac{1}{\hat{\mu}_{i,t}} - R_{i,t} \tag{56}$$

$$\text{MRPL-cost gap}_{i,t} = \hat{\theta}^l_{i,t} \frac{P_{i,t} Y_{i,t}}{L_{i,t}} \frac{1}{\hat{\mu}_{i,t}} - W_{i,t} \tag{57}$$

$R_{i,t}$ consists of the depreciation rate, which we keep at 7 percent as in the PIM exercise, and the average interest rate paid by the firm on its debt, which is the ratio of interest expenditures to total debt. When the information is missing, similarly to Fonseca and Van Doornik (2022) we impute interest rates as the average yearly interest rate at the 2-digit industry level.[36] For the average wage $W_{i,t}$, we divide the total wage bill by the number of employees (either taken from the QP when available, or as the full-time equivalent count in the CB for the remaining firms).[37]

[35] We mainly focus on the estimates of markups and marginal products coming from the Ackerberg et al. (2015) translog specification, as in the Cobb-Douglas case elasticities do not vary within industry, and as such markups for instance are solely determined by the ranking in corrected expenditure shares, and not by possible variation in output elasticities and inputs utilization.

[36] It is not possible to obtain more precise interest rates estimates for different kind of loans and credit instruments for the years of the analysis. The variation in results is minimal if using finer definitions of industry.

[37] For this estimation, one would ideally want to have more precise estimates of the marginal costs of inputs of production than the average yearly estimates of firm wage and user cost of capital. Reassuringly, studies in which data allow to gauge the distinction between average and marginal cost levels do not seem

These gaps convey information on how much a firm is constrained in the demand for an input (in case the gap is positive) or is overusing it and likely the optimal downward adjustment in its usage is hindered by adjustment costs (negative gaps).

Table E.17 displays our estimates of costs, marginal revenue products and gaps. Even in this case, quite reassuringly, our estimates of gaps are in the same ballpark of magnitude of recent studies performing similar exercises (Blattner et al. (2019) for Portugal, Fonseca and Van Doornik (2022) for Brazil and Lenzu and Manaresi (2018) for Italy).

to find dramatic differences in gaps estimated according to the different costs definitions (see Lenzu and Manaresi (2018) for the difference in estimated gaps using average versus marginal wages.

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# E Appendix Tables

Table E.1: Sample representativeness, 2005 firms with credit, QP

| | FTE empl. | Wage bill | ST credit | Sales | # Firms |
|---|---|---|---|---|---|
| 2006 | 0.55 | 0.62 | 0.58 | 0.60 | 0.14 |
| 2007 | 0.58 | 0.65 | 0.58 | 0.62 | 0.15 |
| 2008 | 0.62 | 0.67 | 0.58 | 0.64 | 0.16 |
| 2009 | 0.65 | 0.70 | 0.61 | 0.67 | 0.16 |
| 2010 | 0.66 | 0.71 | 0.61 | 0.68 | 0.17 |
| 2011 | 0.67 | 0.71 | 0.63 | 0.69 | 0.18 |
| 2012 | 0.67 | 0.72 | 0.64 | 0.69 | 0.18 |
| 2013 | 0.69 | 0.73 | 0.69 | 0.70 | 0.19 |

Shares of quantities per year, firms active in 2005 (QP) and with credit. Short-term credit is defined as a regular credit exposure with a maturity of less than one year (or a credit line, which is highly liquid and readily accessible). Full-time equivalent employment, salaries and sales from CB, in order to have consistency of representation over time.
By definition the potential set of firms under considerations excludes firm entry after 2005, but takes into account firms' exit from 2005 onwards. This is the reason why the coverage shares are increasing over time. Referenced on page(s) [9] .

Table E.2: Firm level descriptive statistics, sample of analysis - workforce composition

| | Mean | SD | p25 | p50 | p75 |
|---|---|---|---|---|---|
| **Pre - 2009** | | | | | |
| Share of managers | 0.13 | 0.15 | 0.02 | 0.09 | 0.17 |
| Specialized workers | 0.33 | 0.27 | 0.10 | 0.24 | 0.52 |
| Generic workers | 0.51 | 0.31 | 0.22 | 0.56 | 0.79 |
| High education | 0.11 | 0.17 | 0.00 | 0.05 | 0.12 |
| Medium education | 0.47 | 0.24 | 0.28 | 0.45 | 0.65 |
| Low education | 0.42 | 0.29 | 0.16 | 0.41 | 0.65 |
| Under 30 | 0.25 | 0.17 | 0.12 | 0.22 | 0.35 |
| Att. incumbents | 0.68 | 0.19 | 0.58 | 0.72 | 0.82 |
| **Post - 2009** | | | | | |
| Share of managers | 0.15 | 0.18 | 0.04 | 0.10 | 0.19 |
| Specialized workers | 0.37 | 0.27 | 0.14 | 0.31 | 0.56 |
| Generic workers | 0.47 | 0.30 | 0.20 | 0.50 | 0.73 |
| High education | 0.13 | 0.19 | 0.00 | 0.07 | 0.16 |
| Medium education | 0.52 | 0.24 | 0.34 | 0.52 | 0.70 |
| Low education | 0.35 | 0.27 | 0.10 | 0.32 | 0.55 |
| Under 30 | 0.18 | 0.16 | 0.07 | 0.15 | 0.27 |
| Att. incumbents | 0.55 | 0.23 | 0.40 | 0.58 | 0.73 |

Descriptive statistics for the full (unbalanced) sample of analysis, with N=14,864 distinct firms. All workforce decomposition variables from QP. Referenced on page(s) [10] .

Table E.3: Employment - wage bill regressions: Manufacturing

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | $log(\#emp)_{i,t}$ | | $log(Wage\ bill)_{i,t}$ | | $log(Base\ wage\ bill)_{i,t}$ | |
| $S_i$ | 0.118** | 0.137** | 0.166** | 0.187** | 0.153** | 0.170** |
| | (0.041) | (0.047) | (0.056) | (0.062) | (0.052) | (0.057) |
| Firms | 6347 | 5403 | 6347 | 5403 | 6347 | 5403 |
| WID F | 23.76 | 21.77 | 23.76 | 21.77 | 23.76 | 21.77 |
| Sample | Complete | Survivors | Complete | Survivors | Complete | Survivors |

The regressions refer to the empirical specification in Equation (37) in the text. The dependent variables are the logarithm of: the number of employees (columns 1 and 2), the total wage bill (columns 3 and 4) or the base wage bill, which does not comprehend extraordinary or overtime payments (columns 5 and 6). See the Appendix Section A for the list of controls and fixed effects in the regressions. All regressions feature the full set of fixed effects and controls. Standard errors clustered at the bank-industry pair level. Referenced on page(s) [21] .
$^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$

Table E.4: Heterogeneous employment regressions: Qualifications

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | $Managers_{i,t}$ | | $Spec.\ workers_{i,t}$ | | $Generic\ workers_{i,t}$ | |
| $S_i$ | 0.075 | 0.136 | 0.339** | 0.402** | 0.077 | 0.107 |
| | (0.103) | (0.107) | (0.129) | (0.135) | (0.063) | (0.066) |
| Firms | 11404 | 9757 | 13000 | 11154 | 13174 | 11270 |
| WID F | 32.05 | 36.02 | 37.40 | 38.16 | 36.79 | 38.74 |
| Sample | Complete | Survivors | Complete | Survivors | Complete | Survivors |

The dependent variable in these regressions is the ratio of the number of specific workers to the average level of the pre-period corresponding amount. As such, the regressions are defined only for the firms for which the kind of worker is present in the pre-period (even if missing values for some years are possible). Workers' categories are derived by aggregating the 9 levels of qualification defined by the Portuguese Law (Decree-Law 380-80). The levels are based on the nature and complexity of the tasks performed by the workers within the firm. Generic workers carry out basic, routine and/or repetitive tasks that do not require any particular decision making. Specialized workers (team-leaders) on the other hand deal with more complex tasks that might require discretionary decision-making. Managers directed the general policy and are in charge of defining strategies and organization of the firm. The outcome variable is winsorized at the top 1% level. See the Appendix Section A for the list of controls and fixed effects in the regressions. All regressions feature the full set of fixed effects and controls. Standard errors clustered at the bank-industry pair level. Referenced on page(s) [21] .
$^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$

Table E.5: Heterogeneous employment regressions: Age cohorts

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | *Young* $w_{\cdot i,t}$ | | *Prime age* $w_{\cdot i,t}$ | | *Old* $w_{\cdot i,t}$ | |
| $S_i$ | 0.171+ | 0.223* | 0.087** | 0.101** | 0.078 | 0.040 |
| | (0.098) | (0.102) | (0.030) | (0.031) | (0.058) | (0.054) |
| Firms | 13208 | 11313 | 13804 | 11800 | 10677 | 9122 |
| WID F | 32.01 | 32.55 | 35.73 | 36.35 | 29.42 | 35.61 |
| Sample | Complete | Survivors | Complete | Survivors | Complete | Survivors |

The dependent variable in these regressions is the ratio of the number of specific workers to the average level of the pre-period corresponding amount. As such, the regressions are defined only for the firms for which the kind of worker is present in the pre-period (even if missing values for some years are possible). The age categories are: young workers (between 16 and 30), prime age workers (between 30 and 55) and old workers (between 56 and 65). Age cohorts are fixed over the period of analysis and defined depending on the age of the worker in 2008. The outcome variable is winsorized at the top 1% level. See the Section A for the list of controls and fixed effects in the regressions. All regressions feature the full set of fixed effects and controls. Standard errors clustered at the bank-industry pair level. Referenced on page(s) [19,21] .

$^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$

Table E.6: Employment and Exit regressions: collective agreements renewals

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | $log(\#emp)_{i,t}$ | | $P(exit)_{i,t}$ | |
| $S_i$ | 0.071* | 0.102* | -0.019+ | -0.001 |
| | (0.034) | (0.041) | (0.011) | (0.012) |
| $S_i \cdot jren_{1y}$ | | -0.073 | | -0.044* |
| | | (0.051) | | (0.020) |
| Firms | 13804 | 13804 | 13796 | 13796 |
| WID F | 35.73 | 13.12 | 36.20 | 15.15 |

The dependent variable for columns 1 and 2 is the logarithm of the number of employees, whereas for columns 3 and 4 it a dummy equal to 1 if in the specific year the firm exits the market. The exit regression is a yearly linear probability model. In this specification we add to the controls the share of credit that a firm gets from micro-banks (i.e. excluding the the 10 largest banks) and the share of credit that the firm is getting from the banks failing before 2014, as we try to control indirectly for the unobservable characteristics related to these kinds of matching. Columns 1 and 3 report baseline estimates in Appendix Tables E.12 and E.13. Columns 2 and 4 report results of specifications with heterogeneous effects for firms which have renewed their collective bargaining agreement in the last year, where treatment and instrument are interacted with a dummy for this characteristic. Coefficients need to be interpreted as deviations from average ones. See Section A for the list of controls and fixed effects in the regressions. Regressions in columns 1 and 2 feature a full set of 2005-06 controls and fixed effects, interacted with a *Post* dummies. In the linear probability model fixed effects are interacted with year dummies, whereas 2005-06 controls are not.Standard errors clustered at the bank-industry pair level. Referenced on page(s) [21] .

$^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$

Table E.7: Regressions by TSLOG productivity bins (Ackerberg et al. (2015))

| | (1) | (2) |
|---|---|---|
| | $log(\#emp)_{i,t}$ | $P(exit)_{i,t}$ |
| $S_i$, *Low TFP* | 0.080* | -0.033* |
| | (0.039) | (0.015) |
| , *Med. TFP* | 0.077* | -0.014 |
| | (0.037) | (0.012) |
| , *High TFP* | 0.073 | -0.022 |
| | (0.045) | (0.017) |
| Firms | 13285 | 13277 |
| WID F | 11.15 | 11.59 |
| Sample | Complete | Complete |
| Firm FE | Yes | No |
| Other FE | Yes | Yes |

See Section A for a list of the added controls and fixed effects present in the regressions. All regressions feature the full set of fixed effects and controls. In addition to that specification we control for average TFP in 2005 and 2006, estimated by the Ackerberg et al. (2015) methodology by means of a three factors of production gross output translog production function. TFP can be estimated for less firms than in the full samples depending on availability of the variables to compute it in CB. Given that we cannot control for unobservable characteristics in the exit specifications through firm fixed effects, we characterize the bank-firm matching by augmenting the set of controls with the share of loans that each firm has with micro banks and with banks failing up to 2014. We control linearly for the baseline effect of productivity. In the exit specification the fixed effects are interacted with year dummies, whereas the controls are kept constant and not interacted with any year dummy. In the employment specifications all variables are interacted with a post-period dummy. Standard errors clustered at the bank-industry pair level. Referenced on page(s) [26] .
$^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$

Table E.8: Reallocation and TFP by labor share - full dataset

| | (1) $exit_{i,t}$ | (2) $\Delta \log(emp)_{i,t+1}$ | (3) $\Delta \log(ftemp)_{i,t+1}$ | (4) $\Delta \log(fixed\,cap.)_{i,t+1}$ |
|---|---|---|---|---|
| $TFP_{i,t} \cdot \mathbb{1}(labsh\_q. = 1)$ | -0.0370*** | 0.0298*** | 0.0267*** | 0.0183+ |
| | (0.0049) | (0.0069) | (0.0067) | (0.0105) |
| $\mathbb{1}(labsh\_q. = 2)$ | -0.0390*** | 0.0339*** | 0.0325*** | 0.0249* |
| | (0.0051) | (0.0070) | (0.0069) | (0.0115) |
| $\mathbb{1}(labsh\_q. = 3)$ | -0.0400*** | 0.0352*** | 0.0334*** | 0.0274* |
| | (0.0053) | (0.0069) | (0.0070) | (0.0110) |
| $\mathbb{1}(labsh\_q. = 4)$ | -0.0454*** | 0.0444*** | 0.0413*** | 0.0322** |
| | (0.0049) | (0.0069) | (0.0070) | (0.0105) |
| $Post\ Lehman_t \cdot \mathbb{1}(labsh\_q. = 1)$ | 0.0005 | -0.0001 | 0.0000 | -0.0014 |
| | (0.0010) | (0.0035) | (0.0032) | (0.0048) |
| $\mathbb{1}(labsh\_q. = 2)$ | 0.0005 | -0.0049 | -0.0055 | -0.0107 |
| | (0.0008) | (0.0036) | (0.0034) | (0.0066) |
| $\mathbb{1}(labsh\_q. = 3)$ | 0.0011 | -0.0059 | -0.0064 | -0.0062 |
| | (0.0010) | (0.0042) | (0.0041) | (0.0056) |
| $\mathbb{1}(labsh\_q. = 4)$ | 0.0027+ | -0.0095*** | -0.0087** | -0.0119* |
| | (0.0015) | (0.0027) | (0.0028) | (0.0056) |
| $asinh(VA/emp)_{2005-2008}$ | -0.0112*** | -0.0031* | 0.0014 | 0.0077+ |
| | (0.0011) | (0.0013) | (0.0013) | (0.0040) |
| Firms | 178294 | 170044 | 169324 | 176376 |
| N | 802568 | 767934 | 762156 | 845980 |
| Industry fixed effects | Yes | Yes | Yes | Yes |
| Labor share quartile by post-Lehman FE | Yes | Yes | Yes | Yes |

The regressions refer to the empirical specification in equation (12) in the text. A different coefficient is jointly estimated for each labor share bin, and a variation of slope is estimated for the years post 2008. Labor share is computed as the average ratio of employment costs over value added for the years from 2005 to 2008. In all specifications a control for the average value added per employee in the period from 2005 to 2008 is added. All regressions feature 3-digits industry fixed effects, and labor share quartile by post-Lehman dummy fixed effects. The sample consists of all firms in QP matched with CB for which TFP can be computed (with the exclusion of the energy and construction sector). All variables refer to the outcomes from $t$ to $t+1$. We measure employment either as total headcount of full time equivalent employment, as reported in CB. The exit regression excludes the year 2005, given the CB structure. Standard errors clustered at the 3-digits industry level. Referenced on page(s) [30] .
$^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$

Table E.9: Wedge regressions

| | (1) $Lab.\ wedge_{i,t}$ | (2) $Cap.\ wedge_{i,t}$ | (3) $Mat.\ wedge_{i,t}$ |
|---|---|---|---|
| $S_i$ | -0.0088+ | 0.0023 | -0.0136 |
| | (0.0046) | (0.0049) | (0.0083) |
| Firms | 11708 | 12821 | 12986 |
| WID F | 29.42 | 35.47 | 35.00 |
| Sample | Complete | Complete | Complete |

Outcome variables are winsorized at $0.5^{th}$ and $99.5^{th}$ percentiles. See Section A for a list of the added controls and fixed effects present in the regressions. All regressions feature the full set of fixed effects and controls. Regressions are run on the full sample for all firms for which it was possible to calculate the input wedges. Standard errors clustered at the bank-industry pair level. Referenced on page(s) [33] .
$^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$

Table E.10: Loan level regressions

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | $\Delta D_{st,pre-post}$ | | | | | $\Delta D_{tot,pre-post}$ |
| $FD_{2005}$ | -2.104*** | | | -2.151*** | -2.186*** | -2.145*** | -2.192*** | -2.159*** | -2.237*** | |
| | (0.229) | | | (0.221) | (0.218) | (0.251) | (0.251) | (0.248) | (0.247) | |
| $FD_{2005} > p50(FD_{2005})$ | | -0.215*** | | | | | | | | |
| | | (0.028) | | | | | | | | |
| $FD_{2005} > mean(FD_{2005})$ | | | -0.095*** | | | | | | | |
| | | | (0.025) | | | | | | | |
| $Sovs./Ass._{2009}$ | | | | -6.501*** | | | | | | |
| | | | | (0.576) | | | | | | |
| $Sovs./Ass._{2009,q4}$ | | | | | -4.226*** | | | | | |
| | | | | | (0.369) | | | | | |
| $ID_{2005}$ | | | | | | | | | | -0.432*** |
| | | | | | | | | | | (0.121) |
| Firms | 9927 | 9927 | 9927 | 9927 | 9927 | 9927 | 13937 | 9927 | 13933 | 10413 |
| Firm FE | Yes | Yes | Yes | Yes | Yes | No | No | No | No | Yes |
| Other FE | No | No | No | No | No | No | No | Yes | Yes | No |
| Sample | Multi-loans | Multi-loans | Multi-loans | Multi-loans | Multi-loans | Multi-loans | All firms | Multi-loans | All firms | Multi-loans |

In columns 1-9 the dependent variables is the symmetric growth rate of average short term debt between 2006-2007 and 2009-2010. In column 10 the dependent variable is an analogous growth rate for total debt as in Iyer et al. (2014). The main regressor of interest $ID$ in column 10, again as in Iyer et al. (2014), is the *overall* ratio of interbank funds' liabilites to assets in 2005 (dometic *and* foreign). In columns 1-5 and column 10 firm fixed effects control for unobservable firms' characteristics time-trends. In columns 6-7 there are no additional controls, whereas in columns 8-9 additional fixed effects for observables are added. Samples are either firms with loans with more than one bank (essential to identify the firm fixed effect) or the complete sample of firms (also firms with one loan only). In columns 4-5 we control for the ratio of sovereign debt on balance sheet over total assets, where the amount of government-issued debt is calculated as either the average of 2009 holdings, or the average of the last quarter of 2009 holdings. The logic of the control in the analysis follows Buera and Karmakar (2017). Additional fixed effects include 3 digits industry, commuting zone, age and size quintiles, dummy for exporter in 2005, dummy for overdue loans in 2007, dummy for firm capable of issuing bonds, dummy indicating whether the firm has any loan with banks failing up until the year 2014. Standard errors in parentheses, clustered at the firm and bank-by-3 digits industry level. Referenced on page(s) [2,3,4,5,5] .

$^{+}\ p < 0.1$, $^{*}\ p < 0.05$, $^{**}\ p < 0.01$, $^{***}\ p < 0.001$

Table E.11: Robustness: instrument effects on credit post 2010

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | | | $\Delta D_{st,2013-2010}$ | | |
| $\Delta\ D_{st,2010-2006}$ | -0.220*** | -0.231*** | -0.252*** | -0.253*** | -0.250*** |
| | (0.011) | (0.011) | (0.012) | (0.012) | (0.012) |
| $Z_i$ | 0.240 | -0.041 | -0.142 | -0.170 | -0.147 |
| | (0.277) | (0.257) | (0.259) | (0.261) | (0.260) |
| W. Sov. | | | | -0.990+ | |
| share in Q4-2009, 2005 banks | | | | (0.589) | |
| W. Sov. | | | | | -1.235* |
| share in Q4-2009, 2009 banks | | | | | (0.629) |
| Firms | 12883 | 12865 | 12059 | 12059 | 11880 |
| Fixed effects | No | Yes | Yes | Yes | Yes |
| Controls | No | No | Yes | Yes | Yes |

The regressions refer to the empirical specification in equation (19) in the text. See Appendix Section A for the list of controls and fixed effects in the regressions. The sample consists of firms with (short-term) credit relationships in 2010. Columns 4 and 5 control directly for firms (weighted) exposure to banks average sovereign debt holdings over assets in 2009 (Q4), either considering banks with which the firms has a relationship in 2005 (4) or 2009 (5). Standard errors clustered at bank-industry pair level. Referenced on page(s) [4] .
$^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$

Table E.12: Employment regressions

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | | | $log(\#emp)_{i,t}$ | | |
| $S_i$ | 0.066+ | 0.072* | 0.070* | 0.071* | 0.087* |
| | (0.040) | (0.033) | (0.033) | (0.034) | (0.035) |
| Firms | 14846 | 14830 | 13833 | 13804 | 11800 |
| WID F | 34.18 | 38.35 | 37.66 | 35.73 | 36.35 |
| Sample | Complete | Complete | Complete | Complete | Survivors |
| Fixed effects | No | Yes | Yes | Yes | Yes |
| Controls | No | No | Fail b.c. | Yes | Yes |

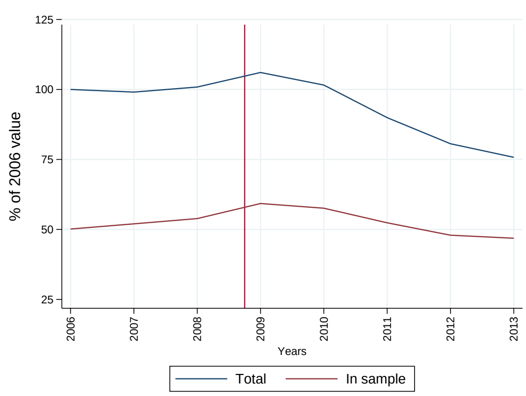
The regressions refer to the empirical specification in Equation (37) in the text. All regressions feature firm and time fixed effects. We refer to *Fail b.c.* as the estimation sample in which only the controls for which the balance checks have failed are included and controlled for. See the Appendix Section A for the list of controls and fixed effects in the regressions. All regressions feature the full set of fixed effects and controls. Standard errors clustered at the bank-industry pair level. Referenced on page(s) [33,5,5,21] .
$^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$

Table E.13: Wage bill and exit regressions

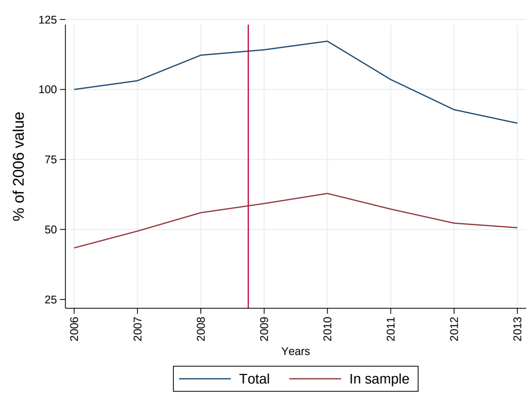
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | $log(wage\ bill)_{i,t}$ | | $log(base\ wage\ bill)_{i,t}$ | | $wage\ bill_{i,t}/sales_{i,pre}$ | | $P(exit)_{i,t}$ |
| $S_i$ | 0.092* | 0.115** | 0.094* | 0.112** | 0.168* | 0.253** | -0.019+ |
| | (0.038) | (0.041) | (0.038) | (0.040) | (0.083) | (0.083) | (0.011) |
| Firms | 13804 | 11800 | 13804 | 11800 | 13804 | 11800 | 13796 |
| WID F | 35.73 | 36.35 | 35.73 | 36.35 | 42.43 | 46.43 | 36.20 |
| Sample | Complete | Survivors | Complete | Survivors | Complete | Complete | |

The regressions refer to the empirical specification in equation (37) in the text, except for column 7. The dependent variables are either the total wage bill (columns 1 and 2) or the base wage bill, which does not comprehend extraordinary or overtime payments (columns 3 and 4). In columns 5 and 6 the dependent variable is the ratio of wage bill to the pre-period average value of sales, whereas the treatment is the variation in average short term credit (as for the standard treatment) scaled by the pre-period average value of sales. The coefficients in columns 5 and 6 should be interpreted as dollar-on-dollar cash-flow pass through. The exit regression is a yearly linear probability model. In this specification we add to the controls the share of credit that a firm gets from micro-banks (i.e. excluding the the 10 largest banks) and the share of credit that the firm is getting from the banks failing before 2014, as we try to control indirectly for the unobservable characteristics related to these kinds of matching. See Appendix Section A for the list of controls and fixed effects in the regressions. All regressions feature a full set of 2005-06 controls and f.e., interacted with a *Post* dummies. In the linear probability model fixed effects are interacted with year dummies, whereas 2005-06 controls are not. Standard errors clustered at the bank-industry pair level. Referenced on page(s) [6,21] .
$^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$

Figure F.1: Credit dynamics in Portugal




(a) Total regular credit




(b) Long-term regular credit

The Figures show the time series for the aggregate amount of total regular (left) and long-term credit (right) for the firms and banks in the sample. Total regular credit is credit not overdue or in renegotiation available to the firm. Long-term credit is any credit exposure with maturity greater than one year, with the exclusion of credit lines with no maturity. The red dotted line splits the sample in pre-period and post-period. Totals are expressed as a percentage total regular credit in 2006.
Source: *Central de Responsabilidades de Crédito* merged with *Quadros de Pessoal* (left), *Central de Responsabilidades de Crédito* merged with *Quadros de Pessoal* and banks' balance sheets (right), authors' calculations and sample selection. Referenced on page(s) [7] .

Table E.14: Balance sheet and financials regressions

| | (1) $log(assets)_{i,t}$ | (2) $log(sales)_{i,t}$ | (3) $arsinh(cash)_{i,t}$ | (4) $arsinh(trade\ credits)_{i,t}$ | (5) $arsinh(suppliers'\ debt)_{i,t}$ | (6) $log(fixed\ assets)_{i,t}$ | (7) $log(current\ assets)_{i,t}$ |
|---|---|---|---|---|---|---|---|
| $S_i$ | 0.098* | 0.041 | -0.128 | 0.409+ | 0.020 | 0.062 | 0.109* |
| | (0.041) | (0.044) | (0.129) | (0.225) | (0.117) | (0.071) | (0.054) |
| Firms | 11800 | 11552 | 11800 | 11800 | 11800 | 11797 | 11790 |
| WID F | 36.35 | 35.81 | 36.35 | 36.35 | 36.35 | 36.32 | 36.75 |

When the *arsinh* is used, the variable is expressed in net terms and can take negative values. Outcome variables are winsorized. Variables expressed in logs that can take only positive values are right-tail winsorized at the $97.5^{th}$ percentile. Variables expressed as arsinh are winsorized on both tails, at the $1^{st}$ and $99^{th}$ percentiles. Sample size varies depending on the availability of the balance sheet item in a consistent way in CB (after harmonization of balance sheet data across the two different accounting systems, pre- and post- 2010). The regressions are carried out on the sample of firms surviving up to 2013. See Section A for a list of the added controls and fixed effects present in the regressions. All regressions feature the full set of fixed effects and controls. Standard errors clustered at the bank-industry pair level. Referenced on page(s) [7] .

$^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$

Table E.15: Revenue elasticities and markups, PIM capital

| | CD | | TSLOG | | ACF CD | | ACF TSLOG | |
|---|---|---|---|---|---|---|---|---|
| | Mean | IQR | Mean | IQR | Mean | IQR | Mean | IQR |
| $\theta^L$ | 0.20 | 0.13 | 0.21 | 0.18 | 0.22 | 0.11 | 0.21 | 0.19 |
| | (0.0001) | | (0.0003) | | (0.0001) | | (0.0003) | |
| $\theta^K$ | 0.03 | 0.02 | 0.03 | 0.03 | 0.02 | 0.01 | 0.03 | 0.02 |
| | (0.00003) | | (0.00005) | | (0.00001) | | (0.00004) | |
| $\theta^M$ | 0.74 | 0.19 | 0.73 | 0.21 | 0.71 | 0.14 | 0.72 | 0.21 |
| | (0.0002) | | (0.0003) | | (0.0002) | | (0.0003) | |
| RS | 0.97 | 0.04 | 0.97 | 0.05 | 0.96 | 0.02 | 0.96 | 0.04 |
| | (0.0001) | | (0.0001) | | (0.0002) | | (0.0001) | |
| $\mu$ | | | | | 1.33 | 0.34 | 1.26 | 0.16 |
| | | | | | (0.0009) | | (0.0003) | |

The table displays descriptive statistics regarding firm-level production function parameters, returns to scale and markups. Mean, interquartile ranges and block bootstrapped standard errors (by firm) for the mean. We show estimates for the two specifications of the gross production function (Cobb-Douglas and translog) and two methodologies we use. The first two columns are estimated by simple OLS, whereas the second two are estimated following the method by Ackerberg et al. (2015), which accounts for endogeneity in the choice of inputs use and we correct for firm selection. See appendix D for details regarding the estimation procedure. Returns to scale are computed as $\sum_X \theta_{i,t}^X$ $X \in \{L, K, M\}$. Markups are estimated according to the method laid out by De Loecker and Warzynski (2012), see appendix D.2 for details regarding the estimation procedure. The table results are based on estimates of the production function where capital is measured according to the perpetual inventory method (PIM). Referenced on page(s) [17] .

Table E.16: Revenue elasticities and markups, book v. of capital

| | CD | | TSLOG | | ACF CD | | ACF TSLOG | |
|---|---|---|---|---|---|---|---|---|
| | Mean | IQR | Mean | IQR | Mean | IQR | Mean | IQR |
| $\theta^L$ | 0.20 | 0.13 | 0.21 | 0.18 | 0.23 | 0.14 | 0.21 | 0.18 |
| | (0.0001) | | (0.0003) | | (0.0002) | | (0.0003) | |
| $\theta^K$ | 0.03 | 0.02 | 0.03 | 0.03 | 0.02 | 0.01 | 0.04 | 0.02 |
| | (0.00003) | | (0.00005) | | (0.00001) | | 0.00005 | |
| $\theta^M$ | 0.74 | 0.19 | 0.73 | 0.21 | 0.72 | 0.17 | 0.71 | 0.20 |
| | (0.0002) | | (0.0003) | | (0.0002) | | (0.0003) | |
| RS | 0.97 | 0.04 | 0.97 | 0.05 | 0.97 | 0.02 | 0.95 | 0.04 |
| | (0.0001) | | (0.0001) | | (0.0001) | | (0.0001) | |
| $\mu$ | | | | | 1.34 | 0.33 | 1.25 | 0.14 |
| | | | | | (0.0008) | | (0.0002) | |

The table displays descriptive statistics regarding firm-level production function parameters, returns to scale and markups. Mean, interquartile ranges and block bootstrapped standard errors (by firm) for the mean. We show estimates for the two specifications of the gross production function (Cobb-Douglas and translog) and two methodologies we use. The first two columns are estimated by simple OLS, whereas the second two are estimated following the method by Ackerberg et al. (2015), which accounts for endogeneity in the choice of inputs use and we correct for firm selection. See appendix D for details regarding the estimation procedure. Returns to scale are computed as $\sum_X \theta_{i,t}^X$ $X \in \{L, K, M\}$. Markups are estimated according to the method laid out by De Loecker and Warzynski (2012), see appendix D.2 for details regarding the estimation procedure.
The table results are based on estimates of the production function where capital is measured as the net book value of balance sheet. Referenced on page(s) [17] .

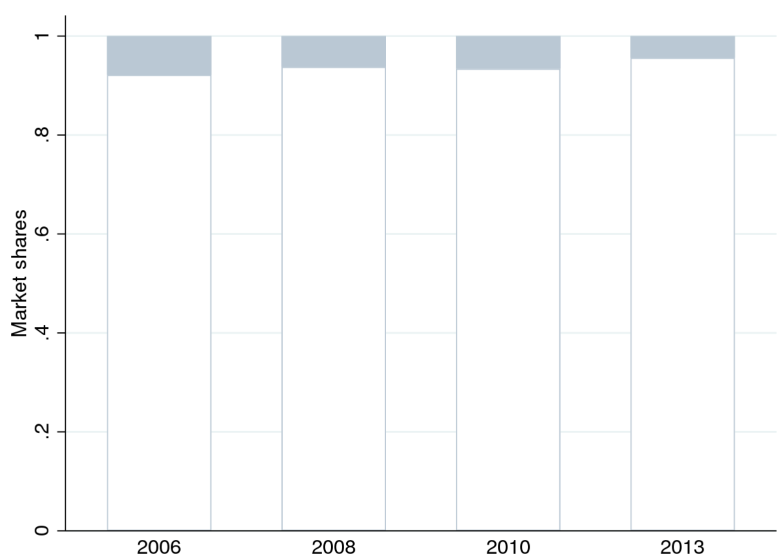
Table E.17: MRPs, user costs and gaps, full CB

| | Mean | p50 | p10 | p90 | Mean | p50 | p10 | p90 |
|---|---|---|---|---|---|---|---|---|
| **Panel A** | | | | | | | | |
| $r$ | 0.07 | 0.05 | 0.00 | 0.14 | | | | |
| $w$ | 10.59 | 9.27 | 5.21 | 17.35 | | | | |
| **Panel B** | PIM capital | | | | Book v. capital | | | |
| $MRP^L$ | 13.39 | 10.53 | 3.64 | 25.54 | 13.19 | 10.29 | 3.96 | 24.64 |
| | (0.0271) | | | | (0.0262) | | | |
| $MRP^K$ | 0.38 | 0.21 | 0.06 | 0.83 | 0.33 | 0.18 | 0.05 | 0.68 |
| | (0.0010) | | | | (0.0010) | | | |
| $Lab. Gap$ | 2.37 | 1.23 | -4.19 | 9.81 | 2.20 | 0.93 | -3.68 | 8.80 |
| | (0.0153) | | | | (0.0136) | | | |
| $Cap. Gap$ | 0.23 | 0.09 | -0.09 | 0.70 | 0.18 | 0.05 | -0.10 | 0.54 |
| | (0.0010) | | | | (0.0009) | | | |

Panel A reports descriptive statistics regarding the distribution of measured interest rates and firm level (average) wages. Interest rates are measured as the ratio of interest expenses of the firm over the total stock of debt (as reported in CB, which comprehends both bank debt and any other form of debt financing for the firm). The average wage is simply calculated as the ratio of salaries to employees, where total salaries are taken from CB and employees are either employment as measured from QP or full time equivalent employment from CB is the former data is missing. Panel B reports descriptive statistics regarding marginal products and marginal products-cost gaps. The labor marginal product and gap are measured in thousands of Euros. Block bootstrapped standard errors (by firm) displayed for the means. We report statistics both for the marginal products and gaps based on elasticities and values of variables when capital is computed according to the perpetual inventory method (PIM) or the net book value. Referenced on page(s) [19] .
See appendix D.2 for details regarding the computations.

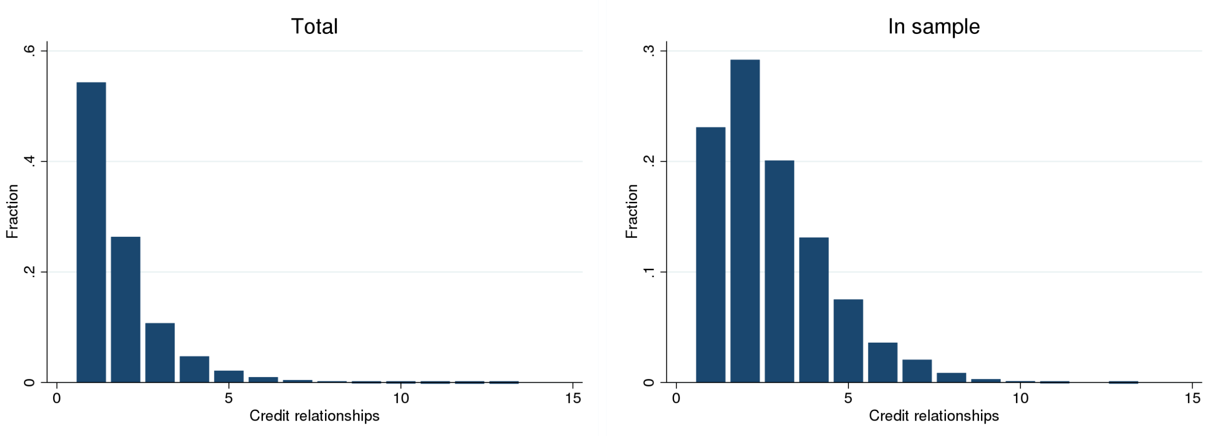
# F Appendix Figures

Figure F.2: Bank concentration by total regular credit




Banking groups market shares in terms of total regular credit, top 10 vs. all other banks. The sample of reference is all firms in QP with credit. Only banks surviving up to 2009 are included, consistently with the sample definition for the empirical event study.
Source: *Central de Responsabilidades de Crédito* merged with *Quadros de Pessoal*, authors' calculations and sample selection. Referenced on page(s) [10] .

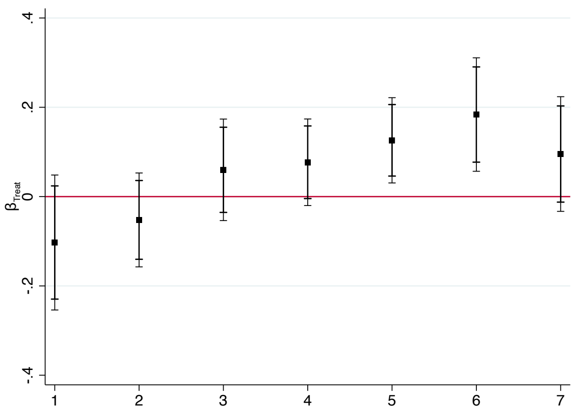
Figure F.3: Number of credit relationships



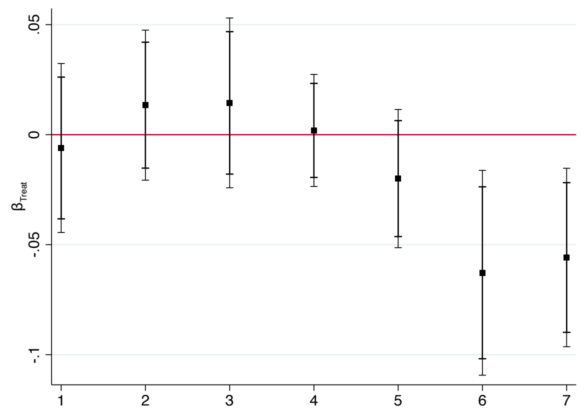

The Figure shows the distribution of the number of credit relationships by firm in 2005 for all firms with credit and in the QP (left) and for the firms in the sample of analysis (right).
Source: *Central de Responsabilidades de Crédito* merged with *Quadros de Pessoal*, authors' calculations and sample selection. Referenced on page(s) [10] .

Figure F.4: Robustness regressions by labor share bins

(a) Residualized labor share

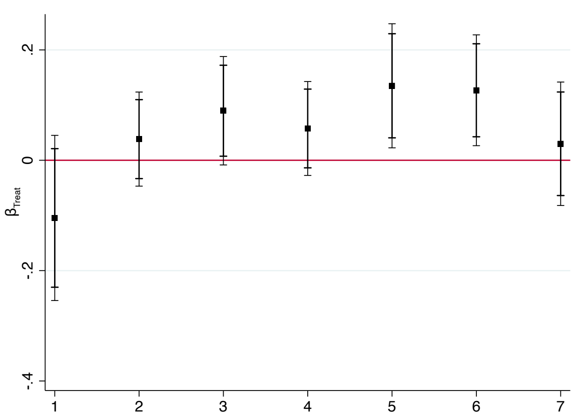



Employment

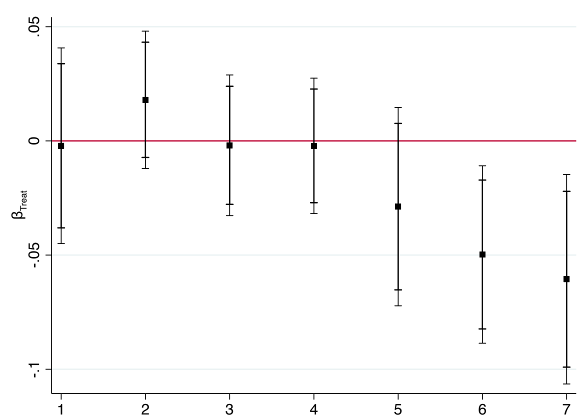



Exit

(b) Wage bill labor share




Employment

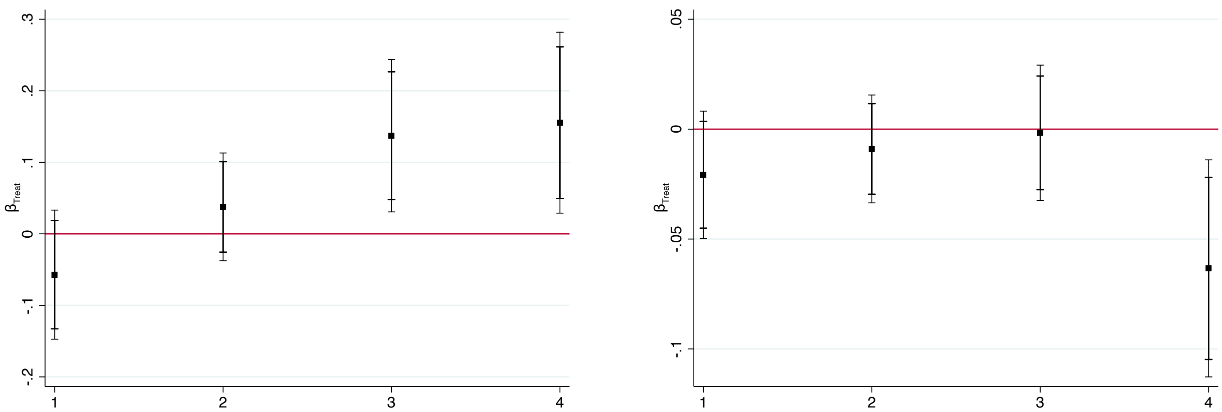



Exit

For subfigures F.4a we obtain the residualized labor shares by calculating the residuals in a regression of the labor share in value added per employee (2005-2006 average values) and the set of fixed effects that we control for in the regressions. For subfigures F.4b we calculate labor share as the average of the labor share in value added per employee (2005-2006 average values), considering only the wage bill as labor costs. We estimate a coefficient for each of the seven labor-share bins, while controlling linearly for baseline effects. Each interacted treatment is instrumented by the interacted instrument. See Section A for the list of controls and fixed effects present in the regressions. Given that we cannot control for unobservable characteristics in the exit specifications through firm fixed effects, we characterize the bank-firm matching by augmenting the set of controls with the share of loans that each firm has with micro banks and with banks failing up to 2014. All fixed effects are interacted with a year dummy, while regressors are constant in the exit regressions. In the employment regressions fixed effects and regressors are interacted with the Post dummy. Number of firms: 13,750 (exit) and 13,760 (employment). 95 and 90% confidence intervals displayed, standard errors clustered at the bank-industry pair level. Referenced on page(s) [16] .

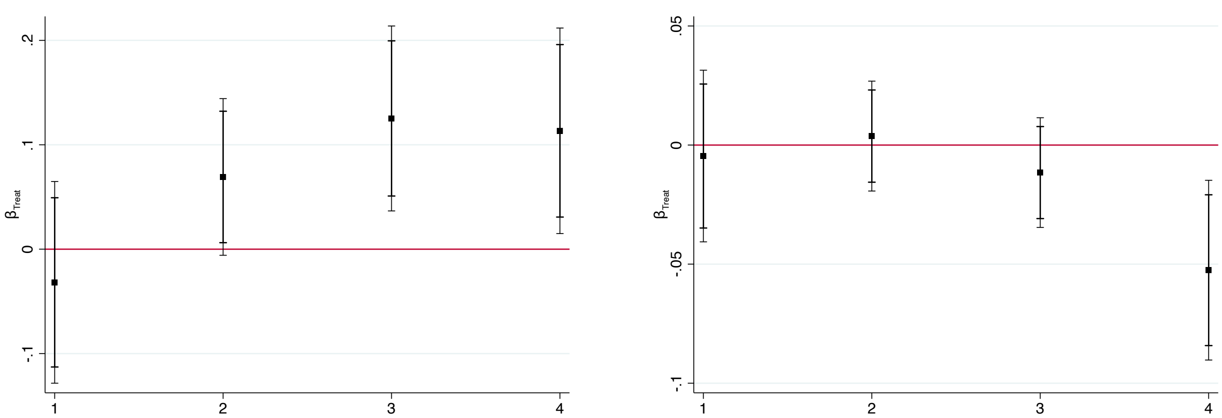
Figure F.5: Robustness regressions by labor share bins

(a) Labor share (average of 2007 and 2008)




Employment Exit

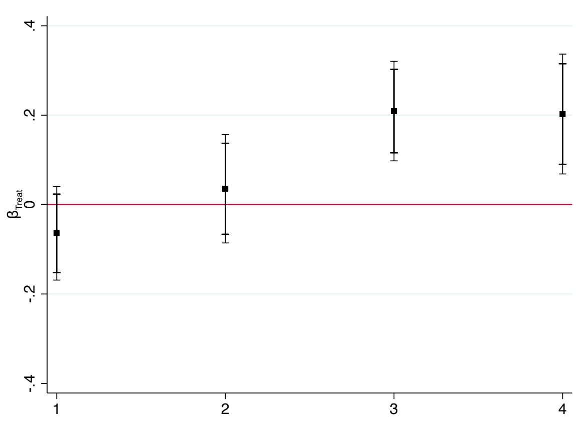
(b) Labor share in sales



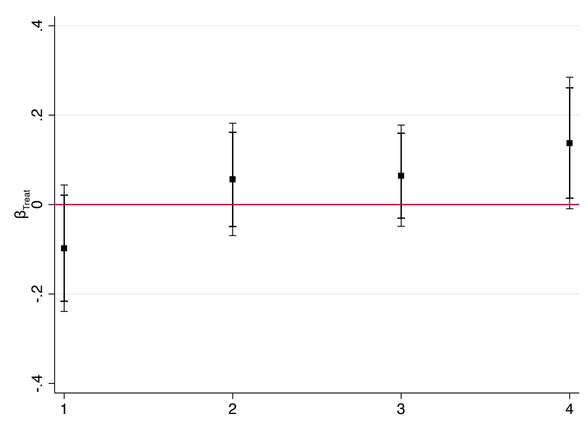

Employment Exit

For subfigures F.5a we calculate labor share as the average of the labor share in value added per employee (2007-2008 average values). For subfigures F.5b we calculate labor share as the average of the labor share in sales (2005-2006 average values). We estimate a coefficient for each of the seven labor-share bins, while controlling linearly for baseline effects. Each interacted treatment is instrumented by the interacted instrument. See Section A for the list of controls and fixed effects present in the regressions. Given that we cannot control for unobservable characteristics in the exit specifications through firm fixed effects, we characterize the bank-firm matching by augmenting the set of controls with the share of loans that each firm has with micro banks and with banks failing up to 2014. All fixed effects are interacted with a year dummy, while regressors are constant in the exit regressions. In the employment regressions fixed effects and regressors are interacted with the Post dummy. Number of firms: 13,750 (exit) and 13,760 (employment). 95 and 90% confidence intervals displayed, standard errors clustered at the bank-industry pair level. Referenced on page(s) [16] .

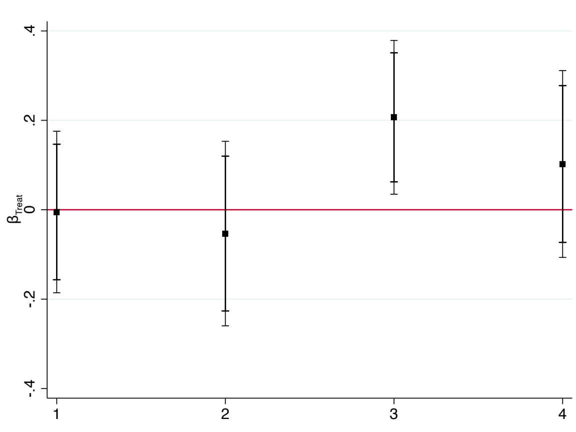
Figure F.6: Balance sheet items and sales regressions by labor-share bins



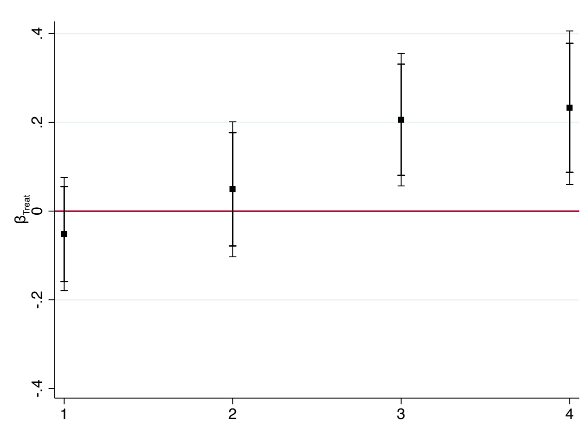

(a) Total assets




(b) Total sales



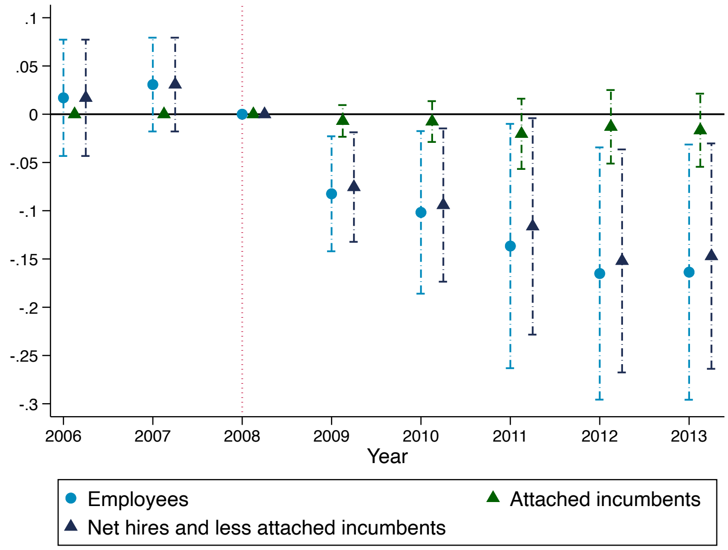

(c) Fixed assets



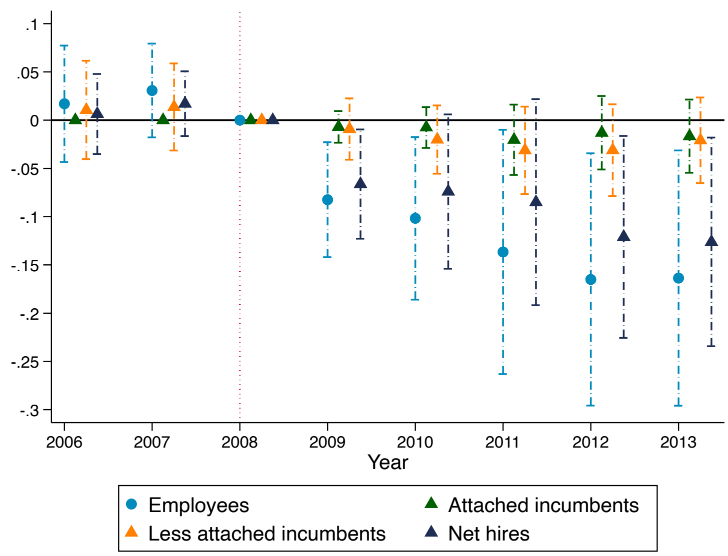

(d) Current assets

We estimate a coefficient for each of the four labor-share quartiles, while controlling linearly for baseline effects. Each interacted treatment is instrumented by the interacted instrument. Current assets are defined residually by subtracting fixed assets from total assets. See Section A for the list of controls and fixed effects present in the regressions. Fixed effects and regressors are interacted with the Post dummy. All fixed effects and controls are interacted with a *post* dummy. Number of firms: 13,760. 95 and 90% confidence intervals displayed, standard errors clustered at the bank-industry pair level. Referenced on page(s) [19] .

Figure F.7: Employment adjustment by tenure



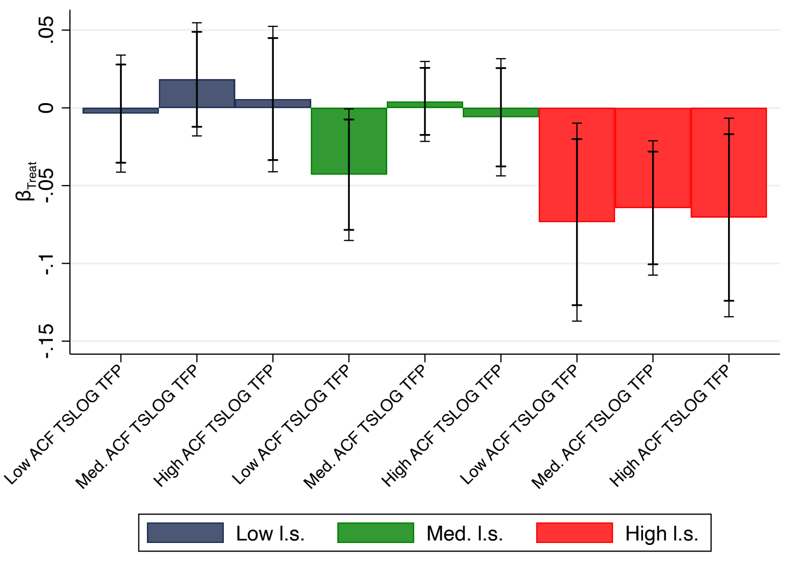

(a) Attached incumbents vs. all other workers



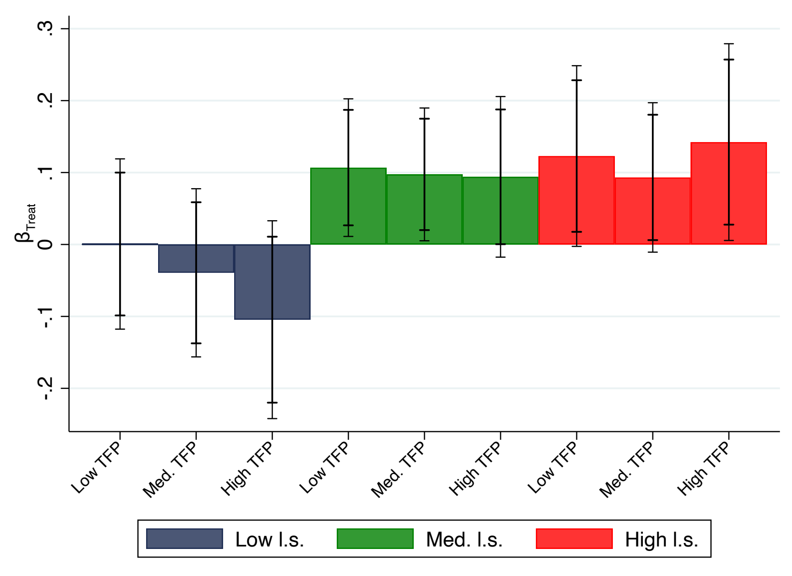

(b) Attached incumbents vs. less attached incumbents and net hires

The dependent variables in these regressions are the ratio of the number of employees of the specific category over the average number of employees in the pre-period (2006-2008). By construction, the sum of the coefficients of each regression should be equal to the overall employment effect. In the specifications the coefficients for 2008 are normalized to 0, so that all the other coefficients should be interpreted as the effect on the percentage variation of each kind of employment with respect to the 2008 level. Attached incumbents are defined as workers present at the firm for the entirety of the pre-period. Less attached incumbents are all other incumbents in 2008, whereas net hires are all the other workers (hires/separations in the post period). In order to get a sense of the implied elasticities of adjustment, one should divide the estimated coefficient by the share of workers in the pre-period. Attached workers constitute more than 67% of the workforce in the pre-period. The share of less attached incumbent is the remaining share of workers in 2008, and is always less than half of the attached incumbents share throughout the post-period. The sample includes only survivor firms (N = 11,801), but is not balanced. The graph displays the effect of a negative shock. See Section A for the list of controls and fixed effects present in the regressions. All regressors and fixed effects are interacted with a year dummy. 95% confidence intervals displayed, standard errors clustered at the bank-industry pair level. Referenced on page(s) [19,21,21] .

Figure F.8: Regressions by labor-share and TSLOG-ACF-productivity bins



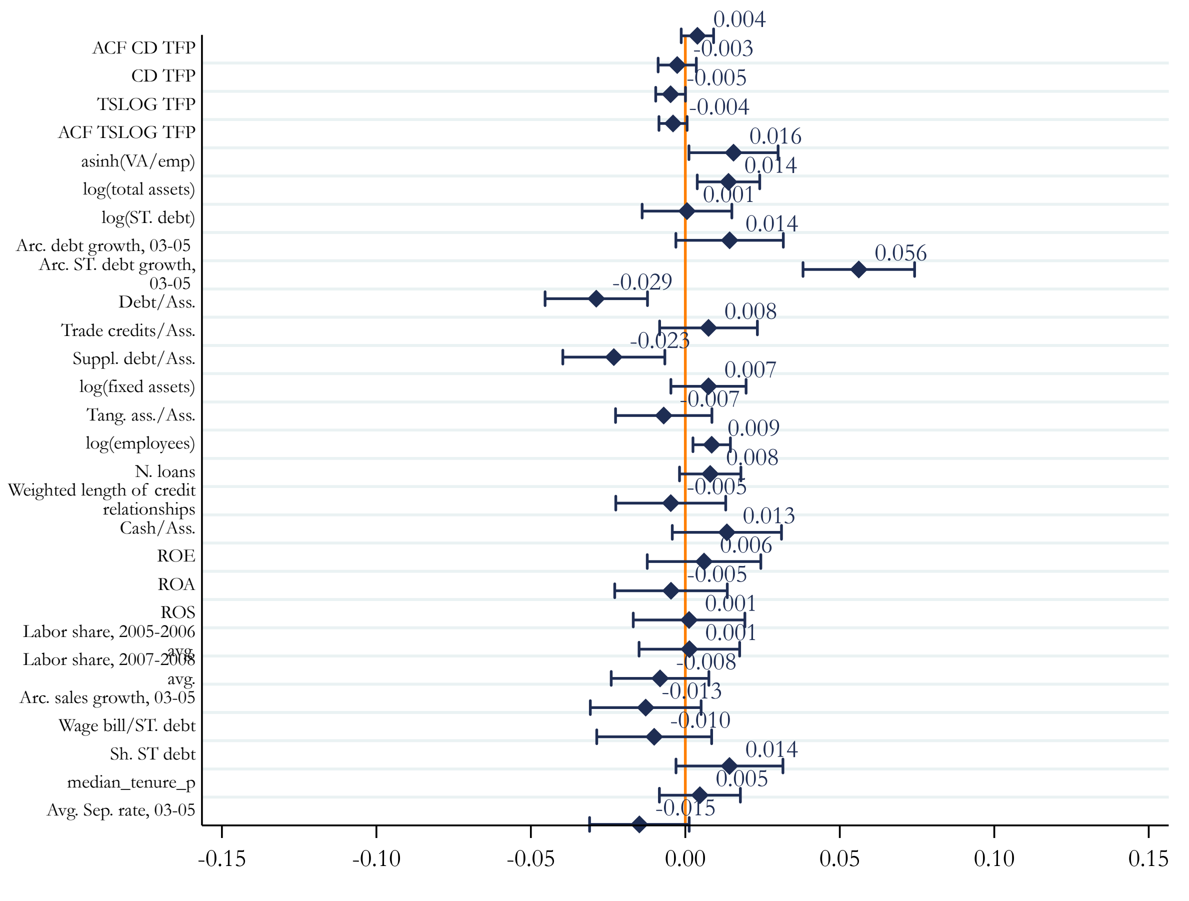

(a) Exit




(b) Employment

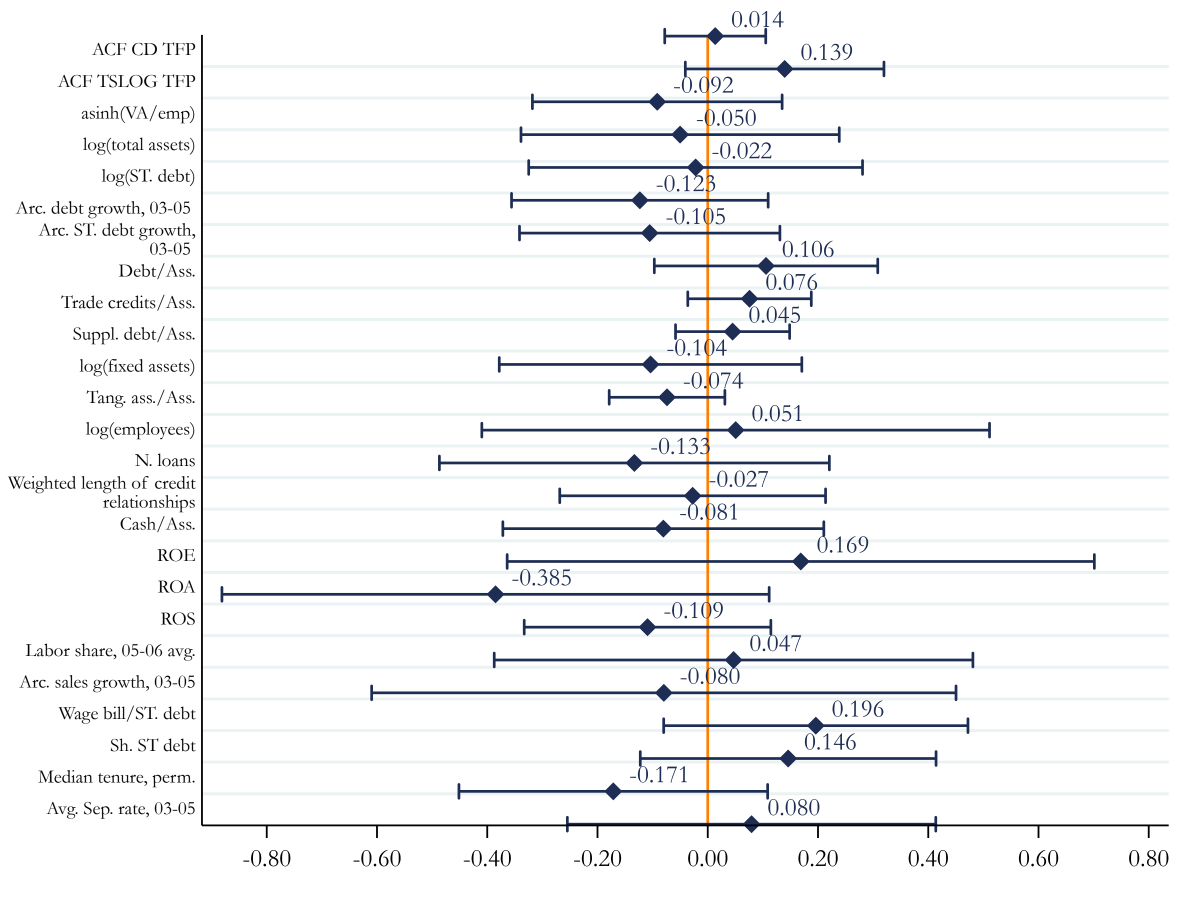
We estimate a coefficient for each of the nine interacted bins, while controlling linearly for baseline effects and their interaction. Each interacted treatment is instrumented by the interacted instrument. Labor share is defined as the ratio between employment-related costs and total value added (average of 2005-2006 levels). Productivity is estimated on a 3-inputs gross output translog production function following Ackerberg et al. (2015), by 2-digit industrial sectors. See Section A for the list of controls and fixed effects in the regressions. Given that we cannot control for unobservable characteristics in the exit specifications through firm fixed effects, we characterize the matching of firms to banks by augmenting the set of controls with the share of loans that each firm has with micro banks and with banks failing up to 2014. All fixed effects are interacted with a year dummy, while regressors are constant in the exit regressions. In the employment regressions fixed effects and regressors are interacted with the Post dummy. Number of firms: 12,927 (exit) and 12,927 (employment). Sample size depends on availability of non-missing variables in CB. 95 and 90% confidence intervals displayed, standard errors clustered at the bank-industry pair level. Referenced on page(s) [27] .

Figure F.9: Balance checks



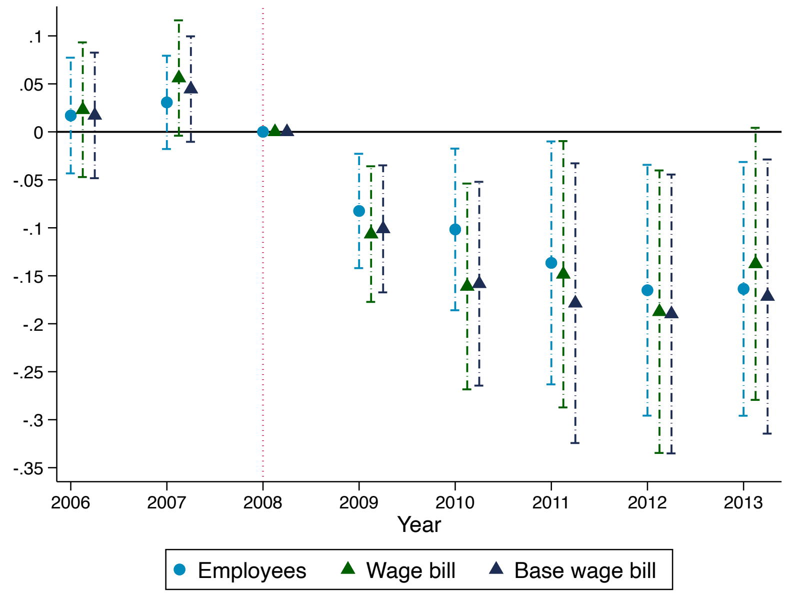

The Figure shows the coefficients (with 95% confidence intervals) of pairwise regressions of the standardized value of each variable in 2005 (unless reported otherwise) on the instrument $Z_i$. All regressions include the same set of fixed effects of the main specification, which are 3-digit industrial sector, commuting zone, quintiles of firm age and size in 2005, and dummies for: exporter, overdue loans in 2007, loans with banks failing up until 2014, bond issuance, exporter, single loans. Standard errors robust to heteroskedasticity. Referenced on page(s) [2,5] .

Figure F.10: Balance checks (Borusyak et al., 2022)




The Figure shows the coefficients (with 95% confidence intervals) of pairwise regressions of the standardized value of each variable in 2005 (unless reported otherwise) on the (standardized value of the) instrument $Z_i$. The regressions are run at the bank level, and all regressors are weighted bank exposures to firm characteristics, according to the method exposed in (Borusyak et al., 2022). Before weighting firm characteristics at the bank level, the variables are regressed on the fixed effects used throughout the analysis in the paper (see Section A for a list), and residuals are calculated and used in the analysis. Standard errors robust to heteroskedasticity. Referenced on page(s) [2] .

Figure F.11: Employment and wage bill regressions: event study




The dependent variables in these regressions are the ratio of the number of employees (wage bill) over the average of their level in the pre-period (2006-2008). In the specifications the coefficient for the year 2008 are normalized to 0, so that all the other coefficients have to be interpreted as the effect on the percentage variation of employment or wage bill with respect to the 2008 level. The sample includes only survivor firms (N = 11,801), but is not balanced. The graph displays the effect of a negative shock. See Appendix Section A for the list of controls and fixed effects in the regressions. All regressors and fixed effects are interacted with a year dummy. 95% confidence intervals displayed, standard errors clustered at the bank-industry pair level. Referenced on page(s) [6] .