

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

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## Abstract

We study how labor rigidities affect firms' responses to credit shocks. Using administrative data on workers, firms and banks in Portugal, we establish three results. First, a short-term credit supply shock leads to a decrease in firms' employment and a greater probability of exit, but effects are concentrated in firms deriving greater value added from labor. Second, the shock disproportionately affects productive firms with a highly-skilled labor force, requiring greater investment in on-the-job training. Third, high firm productivity does not attenuate the effects of credit shocks. Our findings indicate that labor rigidities hinder productivity-enhancing reallocation throughout financial crises.

JEL codes: D24, D25, E24, G21, G30.

Keywords: credit shocks, labor rigidities, human capital, productivity.

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# 1 Introduction

Labor costs constitute a sizable fraction of firms' cost structure. The labor share, albeit declining, is still above 60 percent of value added in advanced economies ([Karabarbounis and Neiman, 2014](#)) and varies substantially across firms. Despite their large share in total costs, labor costs are often overlooked as a source of financial risk, and macroeconomic models often assume that labor is a flexible input in production.

In this paper, we argue that labor costs affect the transmission of credit shocks and amplify their real effects on firms 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 exposes them to variations in the costs of external financing. By giving rise to a credit supply shock, an increase in credit spreads 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 credit shortfalls to which firms might be exposed.

Understanding the interaction between labor cost adjustment and financial frictions is not just relevant at the firm level. It is also important on an aggregate level as it provides insights into the potential effect of financial crises on allocative efficiency. Recessions have in principle the positive by-product 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 and even turn it into "sully" ([Barlevy, 2003](#); [Ouyang, 2009](#)), an overall increase in resources misallocation.<sup>1</sup>

We document how firms' financial flexibility, and in particular their ability to adjust labor costs, determines their responsiveness to credit shocks, and in turn affects their economic performance. To do so, we analyze the impact and 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

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<sup>1</sup>Along those lines, [Foster et al. \(2016\)](#) observe that the Great Recession featured less productivity-enhancing reallocation of inputs and a weaker cleansing effect in firm exits.

the supply of short-term credit to Portuguese firms. This event represents 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, 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 short-term fungible credit to cover their labor costs, the shock has a potential to generate sizable real effects.

An analysis of these topics is typically difficult, given the data limitations in tracking at the same time labor and credit market dynamics and due to the lack of exogenous variation in liquidity. We overcome these issues by combining extremely rich administrative data on banks' and firms' balance sheets with a matched employer-employee dataset and a credit register covering the universe of bank 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 cost rigidity, we exploit two sources of variation at the firm level. First, we leverage the quasi-exogenous exposure of firms to the interbank market freeze, depending on the banks they hold loans with, by using a shift-share instrument for credit growth. Thus, we compare otherwise similar firms that are hit by shocks of different magnitude depending on the banks that finance them. Second, we explore heterogeneity in the impact of the shock depending on firms' (pre-determined) labor share. In this way, we compare firms at the opposite ends of the labor share distribution within their industry while controlling for other firm 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 investigate heterogeneous responses along their (predetermined) labor costs exposure.

We document three sets of results. First and foremost, the incidence of labor costs in value added is a fundamental driver of the transmission and effects of credit shocks. On average, firms hit by the shock decrease employment and assets and experience a greater probability of exit. However, the average estimates mask substantial heterogeneity as effects are concentrated in firms with a higher labor share. Second, we observe that this heterogeneity appears to be correlated with the firms' investment in hiring or training high skilled workers. We observe the largest negative effects on productive labor-intensive firms that employ a more specialized workforce and cor-

respondingly offer more generous compensation. We dig deeper into these dynamics and find that firms with a workforce that requires a high level of on-the-job training decrease labor more and exit with a relatively higher probability than the others. Third, the credit shock has a potentially disruptive effect on the productivity distribution, as we do not detect any heterogeneous effects across firms with different average productivity.

Our findings highlight that the disruption in the operations of constrained firms triggered by the credit shock could be so severe that firms that are more labor-intensive 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. Overall, we conclude that productivity-enhancing reallocation dynamics are weaker as a consequence of the shock.

Finally, we assess the aggregate implications of our results. 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 falls entirely 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 misallocation. 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 impacted by a recession would accumulate less human capital over their life time ([Acabbi et al., 2023](#)).

Overall, our findings indicate that credit shocks tend to increase misallocation, weaken productivity growth and hamper the cleansing effect of recessions. We show that these effects are due to financial frictions interacting with labor adjustment, which we label as the financial channels of labor rigidities.

**Contribution to the Literature.** Our work contributes to several strands of the literature. First, our research relates to the analysis of the propagation of financial

shocks through banks' credit supply and their real effects (Peek and Rosengren, 2000; Khwaja and Mian, 2008; Ivashina and Scharfstein, 2010; Chodorow-Reich, 2014).<sup>2</sup> 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' cost structure. Similar to Fonseca and Van Doornik (2022) and Sforza and Acabbi (2022), we show that credit supply shocks have different effects on the employment of workers with different skill levels and are mostly correlated to skill and capital deepening. Different from those works though, we highlight that the effects of a short-term credit shock give rise to previously overlooked important dynamics. The main contribution of our study is to show empirically how investment in skilled labor feeds into operating leverage, which is the degree to which the firm is exposed to fixed operating costs in its cost structure. We show how credit cuts not only give rise to decreases in the investment in skilled workers but also spill over to other categories of workers and real outcomes through this labor-induced operating leverage.<sup>3</sup>

Second, 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; Falato and Liang, 2016; Caggese et al., 2019; El-lul and Pagano, 2019; Favilukis et al., 2020; Benmelech et al., 2021; Baghai et al., 2021; Kuzmina, 2023).<sup>4</sup> In particular, Donangelo et al. (2019) present theoretical and empirical support for using the labor share as a proxy for firm-level labor leverage. They also rationalize the mechanism by which labor leverage explains the cross-sectional differences in firms' volatility of cash flows and expected returns along the business cycle, and thus riskiness. We contribute to this line of research by providing novel causal empirical evidence of the critical role that labor rigidities play in the reaction of firms' real outcomes to liquidity shortages, through a direct interaction between finan-

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<sup>2</sup>For more recent studies along the same line of research, see Pagano and Pica (2012) and Jermann and Quadrini (2012), Bentolila et al. (2017), Giroud and Mueller (2017), Blattner et al. (2023) and Barrot et al. (2019). Berton et al. (2018), Barbosa et al. (2020), and Moser et al. (2020) focus on the credit channel effect on employment composition and compensation.

<sup>3</sup>Other recent studies recently focusing on the interaction of financial friction, human capital, inequality and entrepreneurship are Bernstein et al. (2022) and Fonseca and Matray (2023).

<sup>4</sup>See Matsa (2019) and Pagano (2019) for a review of the literature regarding the relationship between labor composition and firm financing.

cial frictions and labor adjustment costs.<sup>5</sup> By showing that the labor rigidity channel matters particularly for firms with a highly-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), 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, 2022).

Third, 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).<sup>6</sup> Our work provides novel evidence of the impact of a negative credit supply shock on aggregate misallocation by means of a causally identified event study.<sup>7</sup> Furthermore, we are the first to show the existence of a perverse non-cleansing selection mechanism in firm exit and inputs reallocation, which is related to the degree of labor rigidities measured at the firm level.

**Plan of the paper.** Section 2 describes the data and sample selection. Section 3 sets the empirical strategy that we employ to identify the credit shock. Section 4 reports the average firm-level results and the heterogeneous effects. Section 5 analyzes the macroeconomic effects of labor rigidities. Finally, Section 6 concludes.

## 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.<sup>8</sup>

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<sup>5</sup>Schoefer (2022) provides a theoretical setting in which business cycle amplification stems from rigidities in *incumbents’* wage rigidity. Mueller (2017) rationalizes the greater cyclicity of separations for high skilled workers in a model with financial frictions and working capital constraints.

<sup>6</sup>Davis and Haltiwanger (1990, 1992); Acabbi et al. (2023) confirm the existence of a cleansing effect of recessions by analyzing job flows and firm dynamics using US Census data up to the mid-1990s. 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 it or turn it into a “sully” effect.

<sup>7</sup>A similar approach is adopted in recent studies by Bai et al. (2018), Fonseca and Van Doornik (2022), and Bau and Matray (2022).

<sup>8</sup>We refer the reader to Section A in the Online Appendix for the sample selection criteria. An Internet Addendum with a detailed description of each dataset is also available on the authors’ websites.

## 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 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.<sup>9</sup> 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 can 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 (Iyer et al., 2014; Bonfim and Dai, 2017).

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, 2022). 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 pa-

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<sup>9</sup>See the early 2009 editions of the ECB "Survey on the Access to Finance of Small and Medium Enterprises" (SAFE, ECB, Sep 2009b) 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.



pers and subprime lending ([Acharya and Schnabl, 2010](#)), and featured a stable if not mildly increasing aggregate credit supply (see Online Appendix Figure 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 ([Upper, 2006](#); [Cocco et al., 2009](#); [ECB, Apr 2009a](#)). 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 1a 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.<sup>10</sup>

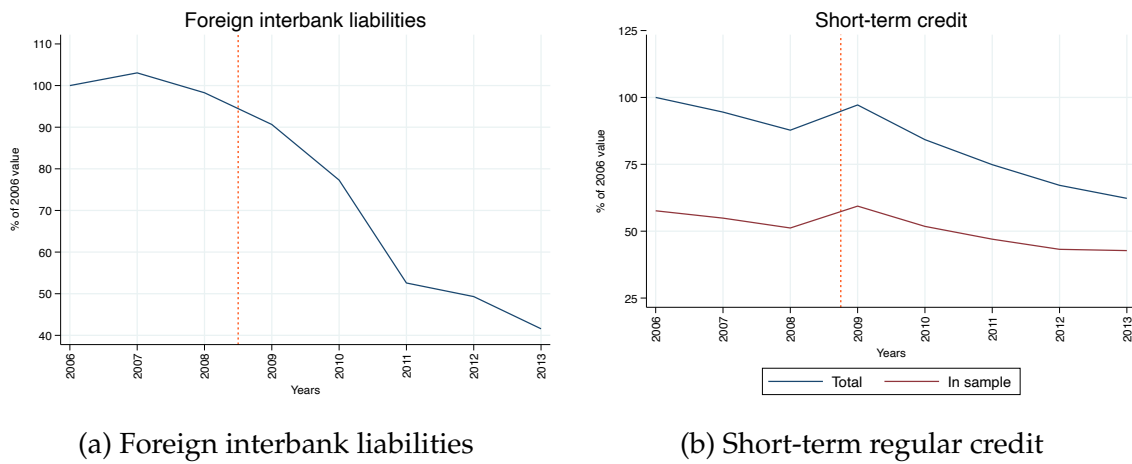
Given the inability to obtain liquidity for their day-to-day operations in a period of global financial turmoil, banks around the world increased spreads and haircuts, and reduced the amount of credit supplied to the real economy and non-financial businesses, as shown for instance by the ECB “Bank Lending Surveys” at the time ([Banco de Portugal, Jan 2009](#)). Figure 1b shows the aggregate trends for regular (neither overdue nor under renegotiation) short-term credit (with maturity less than one year, or liquid credit lines with no defined maturity). Credit supply was still increasing after the first signs of financial distress in 2007, and 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 1a) and 40 percent (short-term credit). In this way, the financial crisis that originated in the US spread to the Portuguese real economy.

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<sup>10</sup>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.



Figure 1: Credit dynamics in Portugal



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 a maturity of less than one year, or liquid credit lines with no defined maturity. The orange dotted line splits the sample into pre-period and post-period. Totals are expressed as a percentage of foreign interbank liabilities (left) and short-term 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) [8] .

## 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 credit records. We construct the bank-firm matched credit dataset from the Bank of Portugal's 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 coun-

terparty (extra-EU or each EU country separately).<sup>11</sup>

## 2.3 Sample selection and descriptives

We combine all 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 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.<sup>12</sup> 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.<sup>13</sup> Our final sample spans 14,846 firms and 31 banking groups.<sup>14</sup> 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.<sup>15</sup> 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

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<sup>11</sup>Throughout the analysis we resort to some other minor datasets, either confidential or publicly available. We refer the reader to the Internet Addendum on our website for a more detailed description of all datasets.

<sup>12</sup>Appendix Table 1 shows how representative our sample is in QP in terms of different measures.

<sup>13</sup>Throughout the text, we often refer to consolidated banking groups as "banks", too.

<sup>14</sup>Most of the regressions that require also balance-sheet variables feature a sample of 13,806 firms.

<sup>15</sup>Appendix Table 2 reports workforce composition descriptive statistics for firms in the sample.

Table 1: Firm level descriptive statistics, sample of analysis

	Mean	SD	p25	p50	p75
<b>Pre - 2009</b>					
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
<b>Post - 2009</b>					
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 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 [11] .

(median 0.47).<sup>16</sup>

### 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 productiv-

<sup>16</sup>The credit market in Portugal is heavily concentrated. Figure 2 in the Online Appendix shows the distribution of the number of credit relationships by firms in 2005, both for firms in our sample and the full dataset.

ity – 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.

### 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:<sup>17</sup>

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

Building on [Iyer et al. \(2014\)](#), we propose an instrument for 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. 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. 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).<sup>18</sup> 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, we 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

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<sup>17</sup>This measure ranges between -2 and 2 and it is particularly appealing in 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 *before* calculating the symmetric growth rate.

<sup>18</sup>In our dataset, reasonable values of foreign interbank exposure range from 10 percent to slightly more than 25 percent.

defined as:

$$Z_i = \sum_{b \in B_i} \omega_{i,b} FD_b, \quad (2)$$

where  $B_i$  is firm's  $i$  set of banks with a credit relationship in 2005 and  $\sum_{b \in B_i} \omega_{i,b} = 1$ .<sup>19</sup>

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 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, did not drive in any way the credit dynamics we identify. Importantly, the Appendix shows in Table C.1 that it is extremely implausible, following the methodology in [Altonji et al. \(2005\)](#) and [Oster \(2019\)](#), that also unobservable match-specific characteristics drive banks' credit supply reductions.

Given the paper's 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 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 [Akerberg et al. \(2015\)](#). Our baseline specification features TFP calculated through a gross-output three-factor Cobb-Douglas production function.<sup>20</sup> 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:

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

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 Equa-

<sup>19</sup>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.

<sup>20</sup>Productivity and productivity rankings are highly persistent for firms over time. Section B in the Internet Addendum on the authors' websites 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*** (0.276)	-2.347*** (0.308)	-2.467*** (0.289)	-2.328*** (0.315)
, Med. TFP	-1.941*** (0.309)	-2.314*** (0.312)	-2.029*** (0.312)	-2.519*** (0.318)
, High TFP	-2.376*** (0.294)	-1.927*** (0.274)	-2.462*** (0.314)	-2.109*** (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 \(2012\)](#) and [Akerberg et al. \(2015\)](#). In columns 1 and 2, firm fixed effects control for unobservable firms' characteristics time-trends, and the sample contains only firms with loans with more than one bank. In columns 3-4, we control for fixed effects for observables, but no firm fixed effect. Additional fixed effects include 3-digit 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) [14].

<sup>+</sup>  $p < 0.1$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

tion 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 percent 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, increasing the overall volatility of firms' cash flows and fundamentals (Donangelo et al., 2019).

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 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.<sup>21</sup>

In this section, we investigate whether the firm overall compensation policy –

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<sup>21</sup>Section B in the Online Appendix provides an extensive analysis of the average effects of the credit shock on employment, workers' composition, firm investment and exit.

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.<sup>22</sup> 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. The specification for exit is a linear probability model:

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

while the specification for employment is:

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

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.<sup>23</sup> We cluster the standard errors at the main bank-industry pair level.<sup>24</sup>

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<sup>22</sup>In the Online Appendix we provide a simplified theoretical framework guiding our empirical specification choices.

<sup>23</sup>Appendix A reports the full list of controls and fixed effects in the empirical specifications.

<sup>24</sup>Our choice is driven by the fact that [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 (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). Industry is defined as 3-digits CAE (Codificação de Actividades Económicas).

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. Our baseline definition of labor costs includes all labor-related costs of the firm, such as wages, provisions, training and other staffing costs. 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 compensation-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). 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, we compute labor shares in two ways: by considering total costs related to labor (baseline results in text) 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 labor share as the ratio of labor costs to total sales. Lastly, we also provide results using the labor share in total costs instead of value added in order to remove the influence of variations in labor productivity.<sup>25</sup>

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. In order to violate this assumption, one should need to think about shocks relating our labor-intensive firms to their individual credit dynamics, but importantly *not* through our hypothesized causal channel. We have already shown in the current Section that there is no evidence that 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 value added per employee. We run the following regression at the exposure level:

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

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<sup>25</sup>Robustness results for employment and the probability of exiting are in Online Appendix Figures 3 and 4.

Table 3: Loan level regressions: labor share

	(1)	(2)
	$\Delta D_{st,pre-post}$	
$FD_b$ , Low LS	-1.778*** (0.327)	-2.068*** (0.320)
, Med LS	-2.234*** (0.255)	-2.328*** (0.273)
, High LS	-2.311*** (0.348)	-2.272*** (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) [18].

<sup>+</sup>  $p < 0.1$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

where we partition firms according to our baseline measure of labor share into quartiles and define as “medium” labor shares the two central quartiles. 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.

We further address threats to identification by showing in Section 4.2 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

We show estimates for the baseline specifications by labor share quantiles, for both exit and employment in figures 2a and 2b (for 7 quantiles) and Figures 2c and 2d (for quartiles). Both the estimated elasticity of employment and the effect on the proba-

bility of firm exit as a consequence of a (negative) credit shock are almost monotonically increasing across the labor-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).<sup>26</sup>

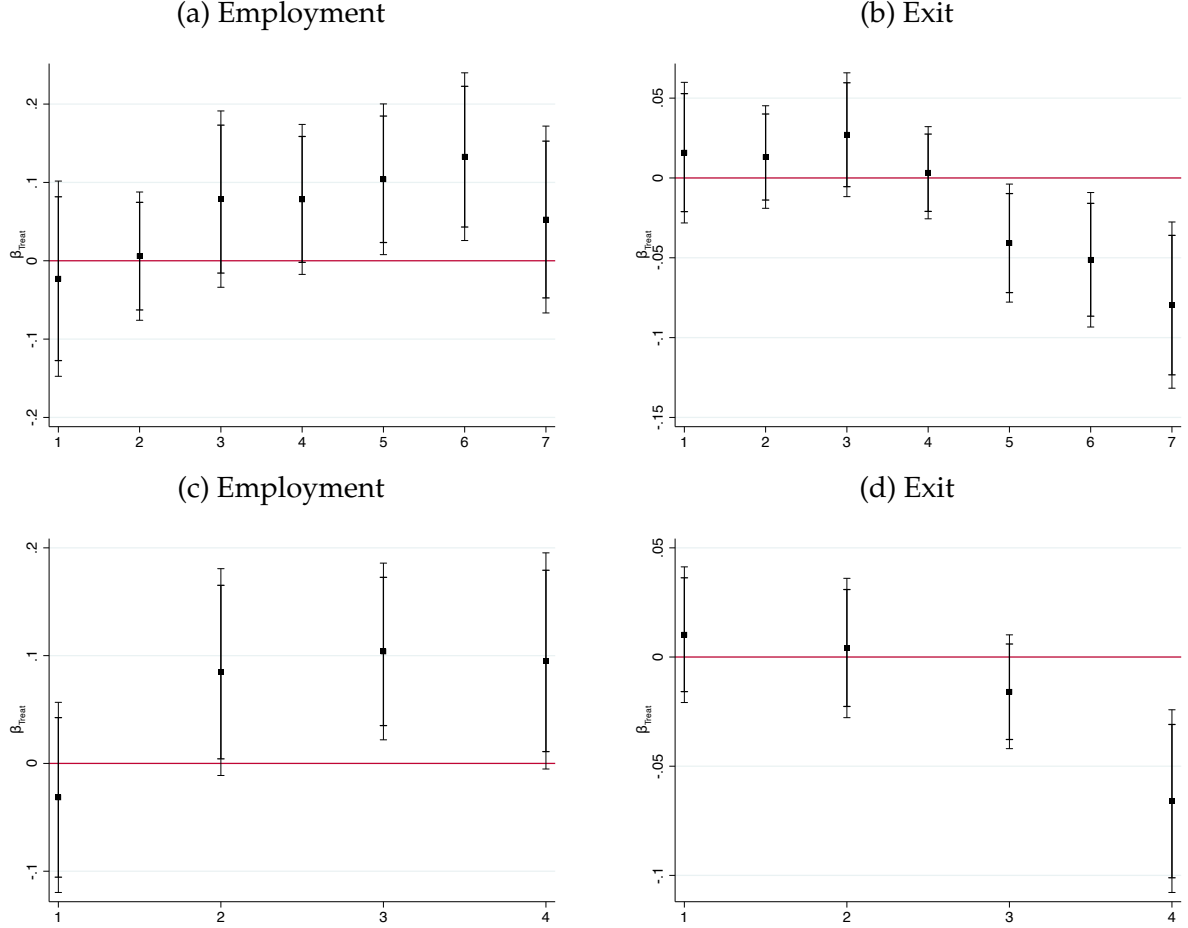
Online Appendix B reports in detail results of the firm level analysis of average effects of the shock, without partitioning firms by labor-share bins. According to that analysis, a one standard deviation in the shock could explain between 14 and 17 percent of a standard deviation in firm level employment variation in the period, and around 17 percent of the average exit probability (Online Appendix Tables 4 and 5). 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.

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<sup>26</sup>Appendix Figure 5 reports 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. 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 are available on request.

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 Appendix 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. Coefficients and standard errors referring to these empirical specifications are reported in Online Appendix Table 3. Referenced on page(s) [19].

## 4.1 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 fire workers with the steepest productivity profiles but lowest firing costs (Caggese et al., 2019), often the youngest and lower-tenured workers, a result we also show in Online Appendix Table 9 and Online Appendix Figure 6. 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 (Oi, 1962). Thus, a relevant way to measure the degree of fixity of labor is through measures of required training and skill specificity of their tasks.

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.

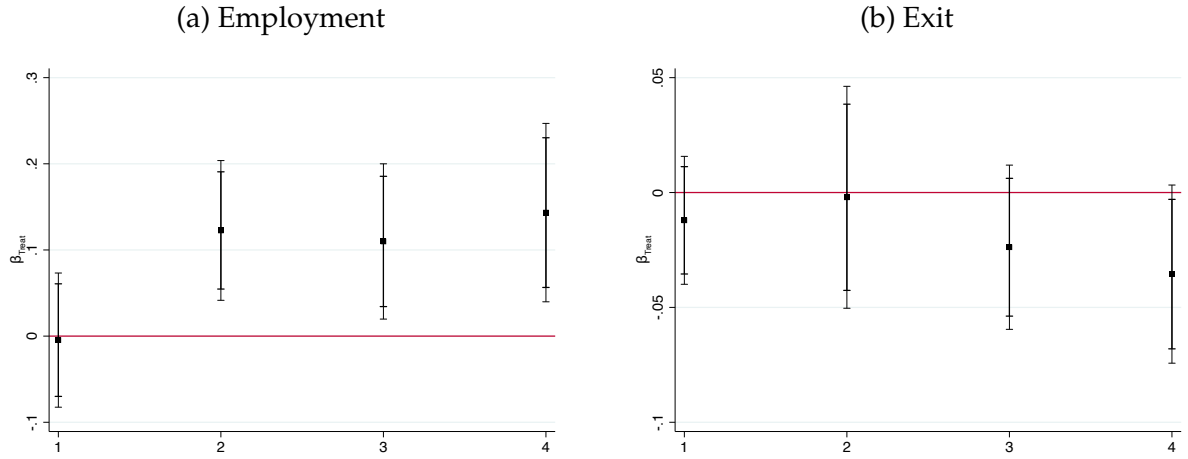


We use the OTJ scores and estimate the difference-in-differences specifications as in equations (4) and (5), where we substitute  $LabSh_{bins}$  with  $OTJ_{bins}$ . Figure 3 shows the results of these estimations. Consistent with the idea of labor as a quasi fixed factor (Oi, 1962), we find greater 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 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 the compensation policy is used in these firms to attract talent.

## 4.2 Additional supporting evidence and robustness

**Sectors and workers heterogeneity.** In the Online Appendix Tables 7, 8 and 9 and Figure 6 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. In Figure 6 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. We show this by displaying how 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. 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

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



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 Appendix 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 the internet addendum on the authors' website 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. Coefficients and standard errors referring to these empirical specifications are reported in Online Appendix Table 6. Referenced on page(s) [22] .

human capital.<sup>27</sup>

**Institutional rigidities.** 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. In order to do that we focus on firms for which collective bargaining agreements had just been renewed within a year before October 2008, the start of the financial crisis.<sup>28</sup> Table 10 in the Online Appendix 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.<sup>29</sup>

**Separations and wage rigidity.** One of the major drivers of labor leverage and the amplification of the real effects of credit shocks on firms' employment, size and exit is the past wage policy for incumbent workers. Consistently with [Card et al. \(2017\)](#); [Kline et al. \(2019\)](#), one would expect wages to incorporate part of the training costs in which the firm had incurred in order for the workers to be productive, given their implicit lower substitutability with outsiders. Moreover, risk-sharing consideration between firms and workers might affect wage dynamics ([Guiso et al. 2005](#); [Balke and Lamadon 2022](#); [Souchier 2023](#)). For example, [Xiaolan \(2014\)](#), [Acabbi et al. \(2023\)](#) show that the optimal contract to provide insurance to workers against negative productivity shocks, while at the same time providing the incentives for high-skill workers to remain in the firm, necessarily implies a degree of downward wage rigidity and an increasing trajectory over time as a function of human capital accumulation. In other

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<sup>27</sup>[Mueller \(2017\)](#) shows in a structural search model of the labor market with a working capital constraint, as is the case in our model in the Online appendix and in [Caggese et al. \(2019\)](#), that high skill, high potential workers should be the ones generating the strongest cyclicalities in employment adjustment, as their surplus creation might be more backloaded than current liquidity needs.

<sup>28</sup>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 on the results. A similar strategy is undertaken in [Faia and Pezone \(2023\)](#).

<sup>29</sup>The result is confirmed by conducting a heterogeneous treatment analysis by partitioning firms based on their expected statutory firing costs. Results available upon request.

words, downward rigidity in compensation is not determined by explicit firing costs, but by risk-sharing considerations, human capital accumulation and implicit leverage. In such cases, even if more risk sharing with high skill workers would be optimal ex-post, it is generally not optimal ex-ante. Moreover, we do not observe in the data that the use of wage cuts is widespread (as in [Carneiro et al. \(2014\)](#)), and novel survey evidence seems to suggest that firms try to avoid wage cuts for many reasons. [Bertheau et al. \(2022\)](#) show that, when asked about their downsizing choices, firm managers tends to state that layoffs give a better control of the workforce. Wage cuts, especially if selective, tend on the other hand to be disruptive from an organizational standpoint, demotivating and potentially alienating especially the higher skilled workers. Coordinating selective wage cuts only for some workers is thus deemed infeasible by most firms. Lastly, firms' managers state that the primary reasons for not laying off workers is the fear of losing skills embedded in the most specialized and valued ones (coherently with findings in [Oi \(1962\)](#), in this paper and in [Baghai et al. \(2021\)](#)).

**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 \(2016\)](#).<sup>30</sup> 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.

To summarize the results, considering institutional rigidities or standard measures of financial risk such as financial leverage does not by any means explain the results we find. What emerges from the workforce composition results in Table 4 is that labor rigidities matter a lot in the reaction to short-term credit (liquidity) shocks. Exposure to working and human capital financing makes employment and real activity susceptible to credit variations, and the effect is stronger the greater the share of value added is generated by labor. Firms with such characteristic tend to feature a more expensive, expert, educated, specialized workforce, requiring more time-consuming training to

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<sup>30</sup>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		Labor share
<b>Workforce Composition</b>		<b>Professional scores</b>	
Sh. managers	(+)**	OJT score	(+)**
Sh. specialized workers	(+)**	ONET zone score	(+)**
Sh. temporary workers	(-)**	<b>Financial variables</b>	
Median tenure (perm.)	(+)**	Financial leverage (debt/ass.) (2005)	(-)**
Sh. workers 55+	(+)**	ST debt/ass. (2005)	(-)**
Sh. highly educated workers	(+)**	Financial leverage (2008)	(-)**
<b>Growth rates (06-08)</b>		ST debt/ass. (2008)	(-)+
Managers	.	Credit growth (06-08)	(-)**
Specialized workers	.	Cash per worker (2005)	(-)**
Highly educated workers	.	Sh. ST credit (2005)	(+)**
<b>MRP - gaps</b>		Sh. ST debt fully secured (2009)	.
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). Highly educated workers are defined as having obtained an undergraduate degree. See Section B.2 in the Internet Addendum for details regarding the estimation of MRP - cost gaps. Referenced on page(s) [22,26,26,37] .

+  $p < 0.10$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

fully become productive. The brunt of the adjustment to the shock in employment is however concentrated on relatively younger, lower tenured 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.

## 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 a firm's exposure to human capital in determining its real outcomes' responses and its sur-

vival probability. In this section, we tackle two related questions: first, whether the credit supply shock had a cleansing effect, and how labor rigidities relate to it; second, 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 when 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](#)). For example, in an influential study on employment dynamics and reallocation, [Foster et al. \(2016\)](#) show that the Great Recession was “less cleansing” than previous downturns, and hypothesize that financial frictions might be relevant to justify 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 effect for each tercile by running the following joint regression at the firm level for exit:

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

while the specification for employment is:

$$\begin{aligned} \log(Y_{i,t}) = & \gamma_i + \tau_t + \left( \sum_{k=\{L,M,H\}} \beta_k S_i \cdot \mathbb{1}\{TFP_{bin} = k\} + \Gamma \mathbf{X}_{i,pre} \right) \cdot \mathbb{1}\{t = Post\} + \\ & + FE_{i,t} + \varepsilon_{i,t} \quad t \in \{Pre, Post\}. \end{aligned} \quad (8)$$

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

Table 5: Regressions by CD productivity bins ([Akerberg et al., 2015](#))

	(1) $\log(\#emp)_{i,t}$	(2) $P(exit)_{i,t}$
$S_i$ , Low TFP	0.070+ (0.037)	-0.033* (0.013)
, Med. TFP	0.087* (0.042)	-0.015 (0.013)
, High TFP	0.080+ (0.042)	-0.017 (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 the method proposed in [De Loecker and Warzynski \(2012\)](#); [Akerberg 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 [28,30].

+  $p < 0.10$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

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.<sup>31</sup>

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 productivity 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

<sup>31</sup>The results are qualitatively unchanged under different production functions specifications and estimation procedures. See for instance Online Appendix Table 11 for the Translog specification.



between. We estimate the following specification for firm exit:

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

and the following specification for employment:

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

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.

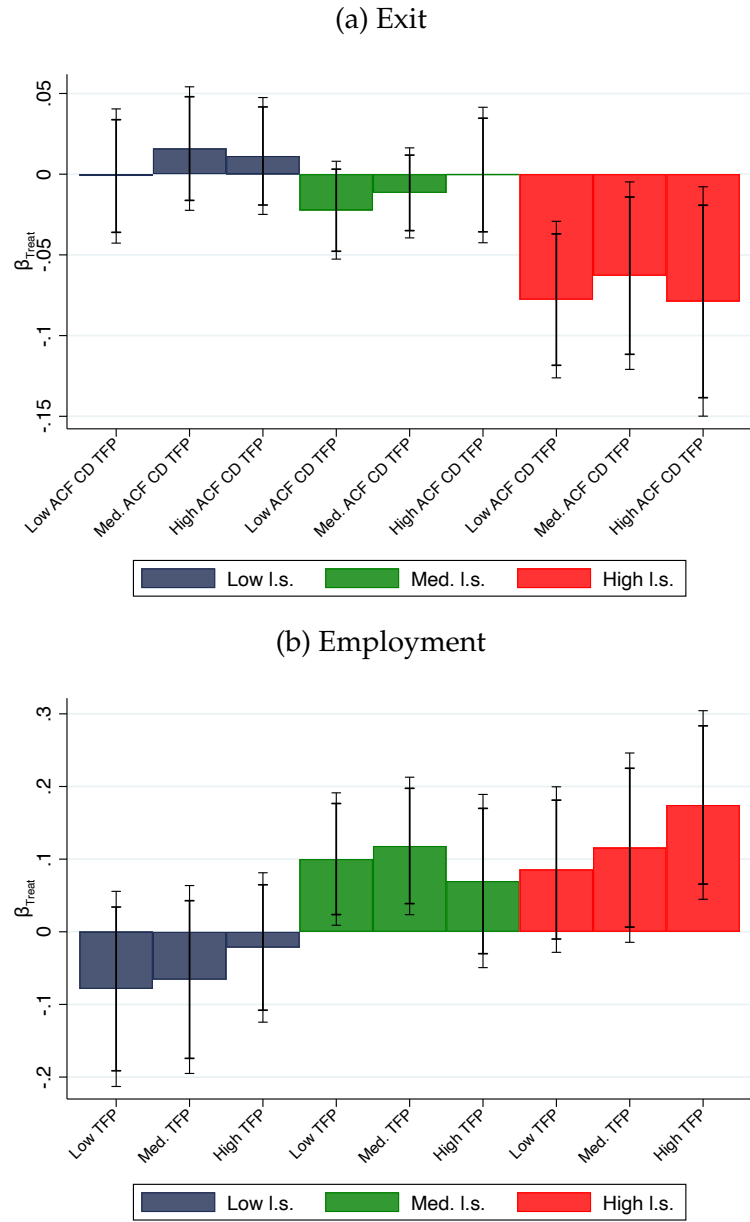
The results of this empirical exercise are presented in Figure 4.<sup>32</sup> Three considerations emerge. First, within a labor-share bin firms respond to the shock in a very similar way across different levels of productivity. Second, high labor share firms suffer more from the shock, both in terms of the probability of exit and the employment adjustments. The coefficients for the high and low labor-share bins across productivity levels are statistically different from each other, at the 5 to 10 percent level of significance depending on the specification and the specific productivity bin (see coefficients and standard errors in Online Appendix Table 12). 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 of financial frictions that hinder productivity-enhancing resource reallocation.

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<sup>32</sup>TFP is estimated according to [Akerberg et al. \(2015\)](#) from a Cobb-Douglas production function. In Online Appendix Figure 7 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.

Figure 4: Regressions by labor-share and productivity bins



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 [Akerberg et al. \(2015\)](#), by 2-digit industrial sectors. See Appendix 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. Coefficients and standard errors referring to these empirical specifications are reported in Online Appendix Table 12. Referenced on page(s) [30,30].

## 5.2 Firm survival and resources 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 with respect to 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.<sup>33</sup> We run the following regressions at the firm-year level:

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

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. 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 [Akerberg 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.

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

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<sup>33</sup>We still exclude micro-firms, with less than 3 employees of Euro 1000 in turnover.

Table 6: Reallocation and TFP - full dataset

	(1) <i>exit</i> <sub><i>i,t</i></sub>	(2)	(3) $\Delta \log(emp)_{i,t+1}$	(4) $\Delta \log(ftemp)_{i,t+1}$	(5) $\Delta \log(fixed\ cap.)_{i,t+1}$
<i>TFP</i> <sub><i>i,t</i></sub>	-0.0529*** (0.0052)	-0.0536*** (0.0051)	0.0378*** (0.0069)	0.0379*** (0.0067)	0.0338** (0.0107)
<i>TFP</i> <sub><i>i,t</i></sub> · <i>Post Lehman</i> <sub><i>t</i></sub>		0.0013* (0.0006)	-0.0063* (0.0029)	-0.0065* (0.0028)	-0.0088* (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) [32].

+  $p < 0.10$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

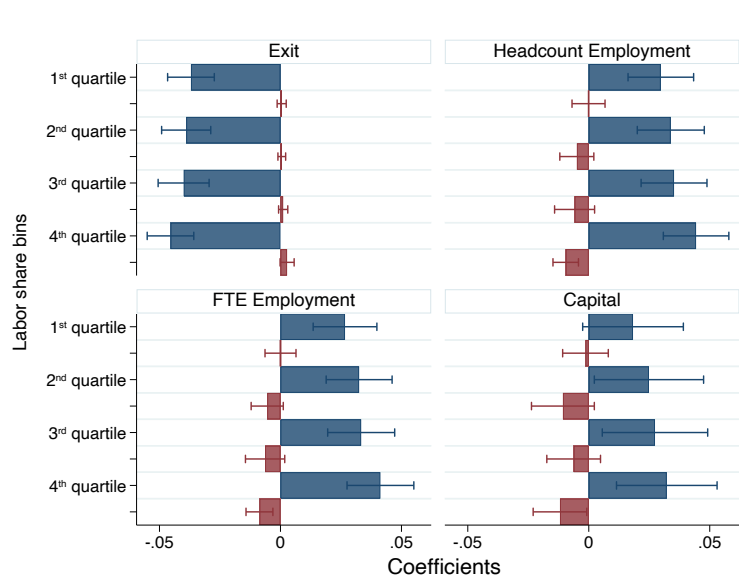
the crisis period for each quartile. We run the following empirical specifications:

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

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 Online Appendix Table 13. 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.

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.

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. Coefficients and standard errors referring to these empirical specifications are reported in Online Appendix Table 13. Referenced on page(s) [33].

### 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 \(2012\)](#); [Baqae and Farhi \(2019\)](#), who generalize the classic decomposition by [Hulten \(1986\)](#). 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:

$$APG = \sum_i P_i dY_i - \sum_i \sum_{X_i \in \{L, K, M\}} W_{i, X_i} dX_i, \quad (13)$$

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:

$$APG = \sum_i D_i d \log A_i + \sum_i D_i \sum_{X_i \in \{L, K, M\}} (\theta_i^X - s_i^X) d \log X_i, \quad (14)$$

where  $A_i$  is technical efficiency (i.e. firm-level TFP),  $D_i = (P_i Q_i) / (\sum_i V A_i)$  are the Domar weights,  $\theta_i^X$  are the output elasticities of input  $X_i$  through the production function estimation and  $s_i^X$  are revenue shares for each input  $X_i$ .<sup>34</sup> The first term in Equation 14 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.

We perform an aggregate-productivity growth-accounting exercise between 2008 and 2012; we use a discrete-time (Tornquist-Divisia) approximation of Equation 14, and take averages across periods for the Domar weights, elasticities and revenue shares. We use TFP and output elasticities calculated according to the [Akerberg et al. \(2015\)](#) method on a translog production function.<sup>35</sup> We estimate an overall  $-12.37$  percent

<sup>34</sup>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.

<sup>35</sup>The exercise is carried out on the sample of relatively large firms that we use for most of the causal

aggregate-productivity 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.<sup>36</sup>

We perform a counterfactual exercise in which we calculate the productivity effects determined by decreases in misallocation as a consequence of the shock (Baqee and Farhi, 2019; Bau and Matray, 2022). For the aggregation exercise, we assume that the technical productivity variations are not correlated with our reduced-form estimates from the event study.<sup>37</sup> 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 Baqee and Farhi (2019). 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 average estimates from Online Appendix Table 4, 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. 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 (Online Appendix Table 14). The relevant wedge in the case the treatment effect is accounted for is

$$\widehat{\theta_i^L - s_i^L}|_T = \hat{\beta}S_i + \hat{\gamma}_i + \hat{\tau}_{post} + \hat{\mathbf{X}}_{i,pre} + \hat{F}E_{i,post}, \quad (15)$$

whereas the counterfactual wedge had the shock not hit is

$$\widehat{\theta_i^L - s_i^L}|_{NT} = \hat{\gamma}_i + \hat{\tau}_{post} + \hat{\mathbf{X}}_{i,pre} + \hat{F}E_{i,post}. \quad (16)$$

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

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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.

<sup>36</sup>Baqee 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 Baqee and Farhi (2019, 2020) for a discussion.

<sup>37</sup>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.



used in the aggregation exercise are:

$$\overline{\theta_i^L - s_i^L}|_j = \frac{1}{2}((\theta_i^L - s_i^L)_{2008} + (\widehat{\theta_i^L - s_i^L})|_j) \quad j \in \{T, NT\}. \quad (17)$$

Therefore, the portion of aggregate productivity growth exclusively attributable to the shock is:

$$APG|_T \approx \sum_i \bar{D}_i \left( (\overline{\theta_i^L - s_i^L})|_T \widehat{d\log L_i}|_T - (\overline{\theta_i^L - s_i^L})|_{NT} \widehat{d\log L_i}|_{NT} \right), \quad (18)$$

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 (18) 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.

Once we apply the results from the reduced form aggregation exercise for wedges and employment to Equation 18, 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.<sup>38</sup>

## 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

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<sup>38</sup>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.

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 institutional 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 sully effects for the economy through young workers' scarred labor careers ([Acabbi et al., 2023](#)).

Our results are also relevant for firms' optimal financing decisions, especially with regard 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., 2023). 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, as found in Barrot and Nanda (2019).

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 constrains internal funds and liquidity management in periods of scarce liquidity, and impairs 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.

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# Appendices

## 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 identification of a causal effect of a variation in credit supply to a firm hinges on the possibility of setting apart banks' effective willingness to provide credit (supply) from firms' unobserved demand for it. As long as credit demand is correlated with a

firms' investment decisions 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 thus use an instrument to identify banks' supply decisions: banks' foreign interbank market funds exposure as a share of total assets.

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 the possibly endogenous 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. its distribution across firms is plausibly random conditional on the observables. 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 C.1 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 em-

pirical 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. We thus show for completeness in Online Appendix Figure 8 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.

## B.2 Credit exposure 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 short-fall. 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:<sup>1</sup>

$$S_{i,b} = \beta FD_b + \mu_i + \varepsilon_{i,b}, \quad (\text{B.1})$$

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 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 we are only able to implement this exposure-level specification only on firms with multiple bank-

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<sup>1</sup>This is the average exposure analogue of the empirical specification of Equation 3 in Section 3.1.

ing relationships (Khwaja and Mian, 2008).

Table C.1 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. 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 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 (column 4). The bound between the estimated and the “bias-corrected” coefficients is tight and far from 0.<sup>2</sup>

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 2 and 3 in which we add controls for the exposure of banks to sovereign debt by the Portuguese government. In column 2 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 3 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

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<sup>2</sup>Oster (2019) developed a framework to evaluate coefficient stability by observing how much estimated coefficients and  $R^2$  vary in regressions when one varies the number 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.

1.<sup>3</sup> 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:

$$\Delta D_{i;st,2013-2010} = \beta \Delta D_{i;st,2010-2006} + \gamma Z_i + \Gamma \mathbf{X}_i + \varepsilon_i. \quad (\text{B.2})$$

Results are shown in Table 15 in the Online Appendix. 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)). We either consider for the weights matched banks in 2005 (column 4) or in the fourth quarter of 2009 (for the banks that have active regular short-term loans, column 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 4 to 7 of Table C.1. 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 assess whether the match between firms and banks based on interbank funding 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 4 and 5 we replicate the exposure-level regression with no controls, while in columns 6 and 7 we saturate the model with a series of fixed effects characterizing the firm operations, such as industry, location and other characteristics. In columns 5 and 7 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 6) of around 2–3 percent of the base estimate of column 1. All estimates are statistically indistinguishable at standard confidence levels.<sup>4</sup>

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<sup>3</sup>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, while interbank exposure typically ranges between 10 and 20 percent.

<sup>4</sup>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

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 (taking into account both domestic and foreign exposures). 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.

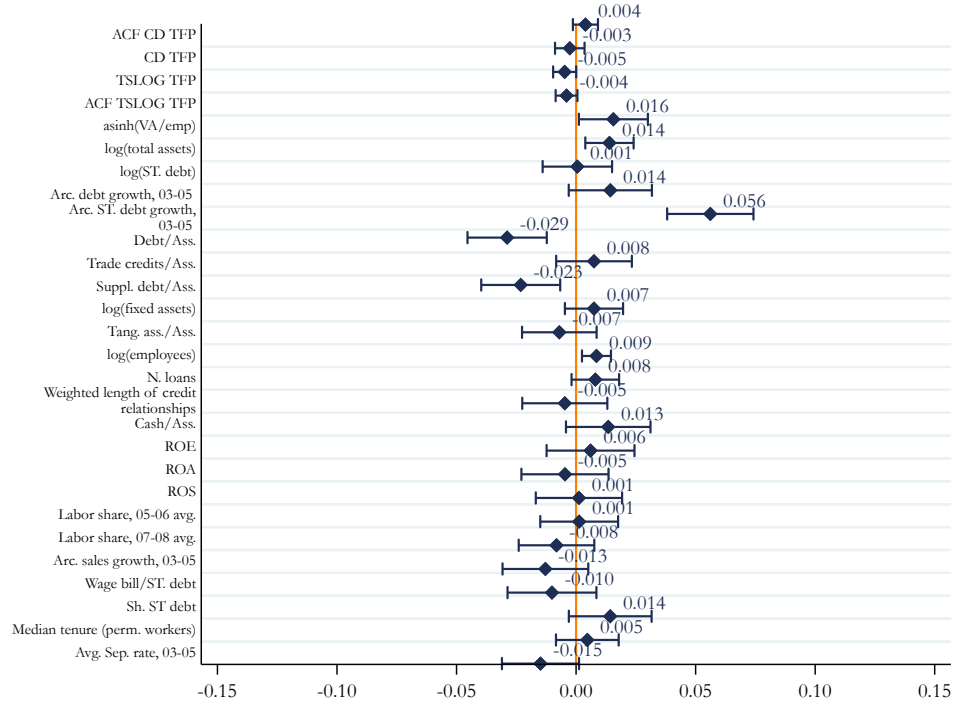
In conclusion, the average exposure results reported in this Appendix further lend credibility to the causal identification of our main effects. On the one hand, they strongly lend support to the argument that the matching of firms to banks in our natural experiment is plausibly orthogonal to the instrument. 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 the observables, especially labor costs and productivity, on which our analysis is based.

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exposure or other characteristics were making the distribution of shares not-exogenous, one would surely expect semi-elasticities of credit with respect to interbank exposure to visibly vary, which is never the case.

## C Appendix Figures and Tables

Figure C.1: 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.



Table C.1: Loan level regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$\Delta D_{st,pre-post}$							$\Delta D_{tot,pre-post}$
$FD_{2005}$	-2.104*** (0.229) [-2.104 -1.971]	-2.151*** (0.221)	-2.186*** (0.218)	-2.145*** (0.251)	-2.192*** (0.251)	-2.159*** (0.248)	-2.237*** (0.247)	
$\frac{Sovs.}{Ass. 2009}$		-6.501*** (0.576)						
$\frac{Sovs.}{Ass. 2009,q4}$			-4.226*** (0.369)					
$ID_{2005}$								-0.432*** (0.121)
Firms	9927	9927	9927	9927	13937	9927	13933	10413
Firm FE	Yes	Yes	Yes	No	No	No	No	Yes
Other FE	No	No	No	No	No	Yes	Yes	No
Sample	Multi-loans	Multi-loans	Multi-loans	Multi-loans	All firms	Multi-loans	All firms	Multi-loans

In columns 1-7 the dependent variable is the symmetric growth rate of average short term debt between 2006-2007 and 2009-2010. In column 8 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 is the *overall* ratio of interbank funds' liabilities to assets in 2005 (domestic *and* foreign). In columns 1-3 and column 8 firm fixed effects control for unobservable firms' characteristics time-trends. In columns 4-5 there are no additional controls, whereas in columns 6-7 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 2-3 following [Buera and Karmakar \(2017\)](#) 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.. In column 1 I include a bound for the coefficient for robustness to OVB calculated by following [Oster \(2019\)](#), pag. 7 (with parameterization  $R_{max} = 1, \delta = 2$ ). We implicitly compare specifications in column 1 and 4. 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.

<sup>+</sup>  $p < 0.1$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$