Labor Rigidities and Firms' Resilience to Liquidity Shocks*

Edoardo Maria Acabbi† Ettore Panetti‡ Alessandro Sforza§

June 2024

Abstract

We study how labor rigidities affect firms' responses to liquidity shocks. Using administrative data on workers, firms and banks in Portugal, we establish two key results. First, the the negative effects on employment and firm survival of an unexpected liquidity shock are concentrated in firms with greater share of value added from labor. These firms feature a higher-skill, less replaceable workforce, requiring greater investment in on-the-job training. Second, firm productivity does not mitigate the impact of liquidity shocks. Our findings suggest that labor rigidity impedes productivity-enhancing reallocation during financial crises.

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

Keywords: liquidity, operating leverage, labor rigidities, human capital.

*The paper previously circulated as "The Financial Channels of Labor Rigidities: Evidence from Portugal". Edoardo Acabbi thanks Gabriel Chodorow-Reich, Emmanuel Farhi, Samuel G. Hanson and Jeremy Stein for their guidance and support. We thank Andrea Alati, Diana Bonfim, Andrea Caggese, Sergey Chernenko, Andrew Ellul, Emilia Garcia Appendini, Andrew Garin, Abhinav Gupta, Isaac Hacamo, Simon Jäger, Sudipto Karmakar, Lawrence Katz, Hyunseob Kim, David Martinez Miera, Adrien Matray, David Matsa, Steven Ongena, Marco Pagano, Pedro Portugal, Pablo Ruiz Verdu, Amit Seru, Boris Vallé, Vikrant Vig, Silvia Vannutelli, for their comments. We also thank António Antunes, Paulo Guimarães, Pedro Próspero, Marta Silva, Fátima Teodoro, Maria Lucena Vieira, Sujiao Zhao and the staff of the BPLim at Banco de Portugal. Edoardo Acabbi acknowledges the financial support from the Comunidad de Madrid, the Spanish Ministry of Science and Innovation (project PID2020-114108GB-I00, CNS2022-135749 and PID2022-137707NB-I00), and the Fundación Ramón Areces. Ettore Panetti acknowledges financial support from the European Union – NextGenerationEU – and the Italian Ministry of Universities and Research (PRIN Project No. 20227N3BMK).

†Corresponding author, University of Mannheim and CEMFI, edoardo.maria.acabbi@uni-mannheim.de.

‡University of Naples Federico II, CSEF, SUERF, and UECE-ISEG, ettore.panetti@unina.it

§University of Bologna, CEPR and CESifo, alessandro.sforza3@unibo.it

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 most advanced economies (Karabarbounis and Neiman, 2014), and varies substantially across firms. Given their large share in total costs, frictions in labor costs adjustment can affect firms' effective operating leverage, increasing the overall volatility of firms' fundamentals. In presence of credit markets disruptions, firms with a less substitutable workforce (Oi, 1962) can thus be more exposed to liquidity risk. As labor is often regarded as a fully flexible input in production, this channel has been previously overlooked.

In this paper, we show how labor costs affect the transmission of liquidity shocks and amplify their real effects on firms. While recent evidence shows that firms take labor costs into account for determining their capital structure (Agrawal and Matsa 2013, Simintzi et al. 2015), we argue that firms remain exposed to liquidity risk stemming from labor rigidity for 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: the need to obtain liquidity to pay salaries exposes them to variations in the costs of external financing. Second, firms frequently employ workers with specialized human capital, who entail high hiring and training costs. Thus, firms cannot easily adjust in the short run by firing these workers as they would face high costs to replace them in the future. This implicit rigidity in workers' compensation creates operating leverage, which amplifies the effects of liquidity shortfalls. The standard definition of operating leverage (Lev, 1974) - the ratio of fixed to variable operating costs - does not encompass labor costs as possibly fixed. Our analysis however shows that operating leverage through labor is a major driver of the real effects of a liquidity contraction for firms.

Understanding the interaction between labor cost adjustment, liquidity management and financial frictions also 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, thereby improving the allocation of resources (Schumpeter, 1942; Davis et al., 1996). Financial frictions might attenuate or even reverse this effect and turn it into "sullying" (Barlevy, 2003; Ouyang, 2009), an overall increase in resources misallocation.[1]

Our key objective is to document how firms' financial flexibility, and in particular their ability to adjust labor costs, determines their responsiveness to sudden liquidity downfalls and how it affects their economic performance. Analyzing the consequences of liquidity shocks on firms with different labor cost structures is challenging, given the data limitations in tracking at the same time labor and liquidity management 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 achieve causal identification, we analyze the global interbank market freeze in 2008 in the Portuguese banking system. This event presents a unique setting to analyze the real effect of contractions in liquidity and their interaction with financial frictions and labor-market rigidities. The failure of Lehman Brothers was sudden, unexpected, and exogenous to the Portuguese economy. 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 and credit lines. Since Portuguese firms are highly dependent on bank short-term fungible credit to cover their working capital financing, the shock has a potential to generate sizable real effects (Jermann and Quadrini, 2012; Chodorow-Reich, 2014).

To isolate the effect of the interaction between the credit shock and labor cost rigidity, we exploit two sources of variation at the firm level. First, following Iyer et al. (2014), we leverage the quasi-exogenous exposure of firms to the interbank market freeze by using a shift-share instrument for credit growth that leverages pre-determined

[1]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.

variation in firm-bank relationships. Thus, we compare otherwise similar firms that were financed by different banks and are hit by shocks of different magnitude depending on the differential exposure of their banks to the interbank market freeze. We saturate all regressions with non-parametric time trends interacted with firms pre-determined observable characteristics, in order to control for any dynamics related to the financial crisis and firms' characteristics, and thus improve the precision of the estimates. Second, we explore heterogeneity in the impact of the shock depending on firms' pre-determined labor share. We compare firms that are exposed to similar credit shocks but lie at the opposite ends of the labor share distribution within their industry. This design allows us to control for a wide set of firm characteristics, including labor productivity. Our identification is similar in spirit to a triple difference-in-differences design, where we compare the evolution over time of outcomes for firms that are more vs. less exposed to a credit shock and then investigate heterogeneous responses for exposed firms based on their pre-determined labor share.

Importantly, our identification does not depend on the dynamics of the financial crisis, other than through firm heterogeneous exposures to the interbank market freeze. Moreover, our identification also does not require to assume that labor share or financial leverage exposures are exogenous to firms' outcomes. Instead, our design simply relies on the assumption that outcomes of firms with similar levels of pre-determined labor share but different bank relationships would have evolved along parallel trajectories absent the interbank shock. Put differently, we assume that there are no other unobserved shocks generating a *difference in differential trends* between firms' outcomes for firms with different pre-determined levels of labor share.[2]

We provide several pieces of evidence in favor of the robustness of our design. First, we show that firms do not differentially sort into banks with different shock exposure depending on their labor share and many other pre-determined observables. Second, we show that banks do not differentially transmit the liquidity shock to firms

[2]Gruber (1994) and Olden and Møen (2022) show that the identifying assumptions for this kind of exercise are substantially *weaker* than the ones for standard diff-in-diffs, and violations are less plausible. We discuss both the assumptions and evidence in support of our design in detail in Sections 3.2 and 4.

with different labor share. Third, we provide evidence of pre-existing parallel trends for firms more vs. less exposed to the shock that are robust to the inclusion of non-parametric trends by labor share.

We document two sets of results. First, the incidence of labor costs in value added is a fundamental driver of the transmission and effects of credit liquidity shocks. Firms hit by the shock decrease employment and assets and experience a greater probability of exit. However, average estimates mask substantial heterogeneity as effects are concentrated in firms with a higher labor share. As we set out to explain why these heterogeneous effects materialize, we observe that they are correlated with 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 correspondingly offer more generous compensation (Abowd et al. 1999, Card et al. 2017). We dig deeper into these dynamics and find that firms with a workforce that requires a high level of on-the-job training and is less substitutable decrease labor more, and exit with a higher probability than the others. Consistently with the literature (Matsa 2019), we find that these firms are *not* highly exposed to financial leverage, but derive this greater riskiness from operating leverage through labor costs. Second, the liquidity shock has a distortionary effect on the productivity distribution. The disruption in the operations of firms exposed to labor leverage triggered by the liquidity shock is so severe that these firms are also more likely to downsize and 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 liquidity 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. Our estimates imply that the liquidity 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 operating leverage through labor costs. 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 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 for the economy in the long run, since younger generations impacted by a recession would accumulate less human capital over their life time (Acabbi et al., 2024).

Overall, our findings indicate that liquidity shocks increase misallocation, weaken productivity growth and hamper the cleansing effect of recessions. We show that these effects are due to financial frictions interacting with frictions in (skilled) 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 work is related to the literature analyzing the implications for firms of inflexibility in labor inputs (Oi, 1962; Hamermesh, 1989; Hamermesh and Pfann, 1996; Danthine and Donaldson, 2002; Le Barbanchon et al., 2023). A recent strand of studies has focused on the increasing importance of analyzing the financing of labor (Simintzi et al., 2015; Serfling, 2016; Falato and Liang, 2016; Caggese et al., 2019; Ellul and Pagano, 2019; Benmelech et al., 2019; Favilukis et al., 2020; Bai et al., 2020; Benmelech et al., 2021; Baghai et al., 2021).[3] 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. Faia and Pezone (2023) show that firms with greater (statutory) wage rigidity determined by bargaining agreement renewals are more susceptible to monetary policy shocks. The models and empirical results

[3]See Matsa (2019) and Pagano (2019) for a review of the literature.

from these studies do however abstract from analyzing the interaction of labor and financial frictions.[4] While their results point to greater cyclical volatility of firms due to a standard operating leverage effect, we focus on the importance of liquidity and credit management for these firms. We provide novel causal empirical evidence of the critical role that labor rigidities play in the reaction of firms' real outcomes to liquidity shortages. Differently from other kinds of fundamental shocks, which can be partly absorbed through access to financial markets, we document how financial risk stems from liquidity risk of firms subject to labor leverage, as liquidity through bank credit is hardly substitutable when financial markets are in distress (Iyer et al., 2014). We thus document the direct interaction between financial frictions and labor adjustment costs, which we further characterize by analyzing the employment composition of *labor levered* firms. By showing that the labor rigidity channel is particularly relevant for firms with a highly-skilled workforce, we also importantly 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, 2022).

Second, our research relates to the analysis of the propagation of financial shocks through banks' credit supply and their real effects on firms (Peek and Rosengren, 2000; Khwaja and Mian, 2008; Ivashina and Scharfstein, 2010; Chodorow-Reich, 2014).[5] We contribute to this line of research by causally identifying the propagation of a credit shock on large firms in a small open economy, focusing on its relationship with labor costs' composition within firms. We provide 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), we show that credit supply shocks have different effects on the

[4]Schoefer (2022) provides a theoretical setting with business cycle amplification through *incumbents'* wage rigidity. Mueller (2017) characterizes greater cyclicality of separations for high skilled workers in a model with financial frictions and working capital constraints. None of the two studies focuses on labor leverage spillovers on other workers, firm survival and its interaction with productivity.

[5]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), Bai et al. (2018), Blattner et al. (2023) and Barrot et al. (2019). Berton et al. (2018), Barbosa et al. (2020), Moser et al. (2020), Jasova et al. (2021) focus on the credit channel effect on employment composition and compensation.

employment of workers with different skill levels. Fonseca and Van Doornik (2022) focus on a reform of bankruptcy law in Brazil, *permanently* decreasing financial frictions in obtaining bank credit, and highlight how greater access to credit allows firms to expand investment in capital and skilled workers operating it. In that, they do not focus on rigidities in labor adjustment. We highlight on the other hand that the effects of a liquidity shock, in the form of a reduction in *short-term* and *fungible* credit, give rise to previously overlooked different dynamics. The main contribution of our study is in fact 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 a temporary credit cut not only gives rise to decreases in the investment in skilled workers but also spills over to other categories of workers and real outcomes through this labor-induced operating leverage.[6] Our findings thus provide novel insights regarding liquidity risk and labor costs' adjustment.

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).[7] 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.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.

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

[6]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).

[7]Davis and Haltiwanger (1990, 1992) 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), Kehrig (2015) question the unconditional existence of the cleansing effect, and argue that financial frictions might attenuate it or turn it into a "sullying" effect.

statistics relative to firm and workforce characteristics.[8]

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, 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. Conditional on controlling for 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 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.[9]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

[8]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.

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

(Iyer et al., 2014; Bonfim and Dai, 2017).

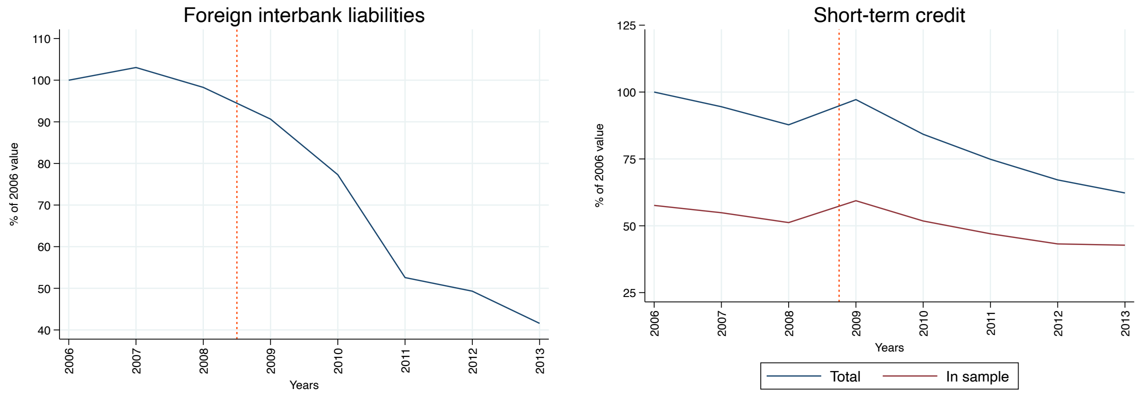
Before the end of 2008 the Portuguese economy did not suffer from the global financial crisis directly, but rather through indirect channels, such as the decrease in global export demand (Garin and Silverio, 2022). Moreover, unlike the United States or Spain, Portugal did not suffer from the burst of 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 papers 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 2009*a*). 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.[10]

Given the inability to obtain liquidity for their day-to-day operations in a period of 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 Por-

[10]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



(a) Foreign interbank liabilities

(b) Short-term regular credit

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) [10] .

tugal, Jan 2009). Figure 1b shows the aggregate trends for regular (neither overdue nor under renegotiation) short-term credit, where we define credit to be short-term if it has maturity less than one year or, if is is a liquid and fungible credit line 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 Online 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.

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 counterparty (extra-EU or each EU country separately).[11]

[11]Throughout the analysis we resort to some other minor datasets, either confidential or publicly

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.[12] 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.[13] Our final sample spans 14,846 firms and 31 banking groups.[14] 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.[15] 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

available. We refer the reader to the Internet Addendum on our website for a more detailed description of all datasets.

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

[13]Throughout the text, we use the terms banking groups and banks interchangeably.

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

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

Descriptive statistics for the full (unbalanced) sample of analysis, with N=14,864 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.

(median 20 percent), and the ratio of short-term credit to wage bill – intended as liquid credit with less than one year of maturity or credit lines over wage bill – is 1.19 (median 0.47).[16]

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 liquidity shock though short-term credit.

We start by describing whether and to what extent banks cut credit to firms in the aftermath of the interbank market freeze, which took place around Lehman Brothers'

[16]The credit market in Portugal is 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.

failure in 2008. We then investigate firm level responses to the credit shock and the extent to which firms' pre-determined labor share explain these patterns. We mean to analyze whether – conditional on the same observed productivity – a firm which derives a greater share of its own value added from labor is more likely to be 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 use an instrumental variable approach.

We measure credit variation S_i for firm i as a symmetric growth rate:[17]

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

As in Section 2, we define D_i, our measure of short-term bank liquidity for the firm, as the sum of credit with maturity less than one year or credit lines, which are by their nature a fungible credit instrument.[18]) 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

[17]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 termination (value -2). Moreover, it limits the influence of outliers on empirical specifications.

[18]In Online Appendix B we show that banks' interbank exposures do not predict long-term credit variation (Table 3.

central banks).[19] 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 defined as:

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

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

The logic behind the causal identification strategy is as follows. The key idea is to leverage the quasi-random exposure of firms to the interbank market freeze through pre-determined variation in firm-bank relationships. We compare otherwise similar firms that were financed by different banks and are hit by shocks of different magnitude depending on the differential exposure of their banks to the interbank market freeze. Importantly, our identification does not depend in any way on the dynamics of the financial crisis and the failure of Lehman Brothers. Any aggregate shock is in fact absorbed by year effects, and we also saturate all regressions with non-parametric time trends interacted with firms pre-determined observable characteristics, in order to control for any specific dynamics related to the interaction of the financial crisis and firms' characteristics.[20] In Appendix B we provide evidence on instruments' properties, balance checks, and an analysis of credit dynamics as a function of banks' interbank exposure at the credit exposure level

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 interbank market freeze episode. The exposure-level analysis of the shock is important for providing evidence that banks did not selectively cut credit

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

[20] Our identification does nonetheless not depend in any way on the addition of these controls, the purpose of which is just to improve the precision of the estimates.

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 that we identify. The Appendix shows in Table C.1 that it is 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 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 Ackerberg et al. (2015). Our baseline specification features TFP calculated through a gross-output three-factor Cobb-Douglas production function.[21] Then, we jointly estimate an effect of banks' exposure to foreign interbank funds for each tercile by running the following regression at the exposure level:

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

where $S_{i,b}$ is the symmetric growth rate of credit variation for each credit exposure of firm i to bank b between 2006–2007 and 2009–2010 averages, calculated as in Equation 1 but at the firm-bank exposure level, and FD_b is the bank's foreign exposure. The definition of the outcome variable allows us to consider extensive and intensive margins of the treatment effect at the same time. Firm-level fixed effects μ_i control for the within-firm variation in credit supply, i.e. the average change in lending to the same

[21] Productivity and productivity rankings are 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. All results are robust to different estimaiton methods.

Table 2: Loan level regressions: productivity

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

In columns 1 and 3, TFP is the residual of a CD three factors production function, whereas in columns 2 and 4 the production function is TSLOG. The estimation always follows De Loecker and Warzynski (2012) and Ackerberg 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 6-7, 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) [17] .
$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

firm by banks with different levels of exposure. They 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

In this section, we investigate whether the firm overall compensation policy – which changes slowly over time, in particular if firms are in need of specialized workers – affects firms' survival and investment decisions in response to a liquidity contraction through a sudden decrease in short-term credit supply.

Labor share at the firm level accounts for around 60 percent or more of total firm value added in all advanced economies, and is a very relevant component of firms' operating costs and value creation (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). This implies that firms exposed to "labor leverage" should also be more likely so see their real outcomes affected in a negative way in the presence of unanticipated liquidity shocks originating from the financial system.[22]

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. Moreover, 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 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 while controlling for other channels related to labor productivity, sectoral dynamics, product cycle dynamics, credit cycle dynamics.

[22]In Online Appendix C we characterize a simple model in which firms' operating leverage is determined by firms' incumbents' salaries, which are positively correlated with workers' skill levels and qualification. We also discuss why our predictions would not be the same for sales shocks, as long as financial markets are functioning.

Traditional economic theory treats labor as a 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 used as collateral for its own financing, labor cannot be pledged by firms to collateralize stable financing. This implies that if there is a mismatch in the timing of cash flows in production, firms will resort to short-term fungible credit to finance working capital. When faced with a tightening of credit supply, a higher cost of labor can become a pivotal factor in determining firm decisions.[23]

For the empirical analysis, we first 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:

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

while the specification for employment is:

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

where $n \in \{4, 7\}$ depending on the specification. The outcome variable in equation (4), defined at the yearly level, is a dummy variable equal to 1 if a firm exited in any year between 2009 and 2013, while τ_t is a year fixed effect. In equation (5), $Y_{i,t}$ is the average

[23] Section B in the Online Appendix provides an analysis of the average effects of the credit shock on employment, workers' composition, firm investment and exit. We do not detect any variation in long term credit as a function of the interbank market exposure of banks, as the long term credit cannot immediately be adjusted by banks in response to their liquidity disruption.

outcome variable in the period of consideration, γ_i is a firm fixed effect, τ_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.[24] We cluster the standard errors at the main bank-industry pair level.[25]

Our identification is similar in spirit to a triple difference-in-differences design, where we compare the evolution over time of outcomes for firms that are more vs. less exposed to a credit shock and then further investigate heterogeneous responses for exposed firms based on their pre-determined labor share. Importantly, our causal identification does not assume, in line with the literature (Simintzi et al., 2015; Matsa, 2019; Favilukis et al., 2020), that the labor share is randomly or exogenously distributed, or unrelated to firms' capital structure decisions. Instead, our design relies on the assumption that outcomes of firms with similar levels of pre-determined labor share but different bank relationships would have evolved along parallel trajectories absent the interbank shock. In other words, we are assuming that there are no other unobserved shocks generating a difference in differential trends in firms' outcomes for firms with different pre-determined levels of labor share (Olden and Møen, 2022).

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

[24]Appendix A reports the full list of controls and fixed effects in the empirical specifications.

[25]Our choice is driven by the fact that Borusyak et al. (2022) and Adão et al. (2019) expressed concerns on clustering standard errors in shift-share designs at the level of the unit of analysis (in our case, the firm) given that the variation in the treatment comes from "shifts" at a more aggregate level (in our case, banks). Industry is defined as 3-digits CAE (Codificaçao de Actividades Económicas).

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. [26]

We further analyze the transmission of the credit shock at the credit exposure level for firms with different exposure to labor costs. We partition firms in quantiles of labor share in value added, while controlling for underlying labor productivity through valued added per employee. We run the following regression at the exposure level:

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

where we partition firms according to our baseline measure of labor share into 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 in Table 3 show that no statistically or economically significant differences can be detected across the coefficients of interest.

We further address potential threats to identification by showing in Section 4.2 that firms affected by the shock do not exhibit in the pre-period any feature that might make them susceptible to liquidity shocks through alternative channels, such as finan-

[26]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***	-2.068***
	(0.327)	(0.320)
, *Med LS*	-2.234***	-2.328***
	(0.255)	(0.273)
, *High LS*	-2.311***	-2.272***
	(0.348)	(0.367)
Firms	9,509	13,147
Firm FE	Yes	No
Other FE	No	Yes

Labor share is calculated as share of labor costs in value added. The "medium" bin comprehends the second and third quartiles. In columns 1 firm fixed effects control for unobservable firms' characteristics time-trends. In column 2 we control for fixed effects for observables, but no firm fixed effect. Samples are firms with loans with more than one bank (essential to identify the firm fixed effect) across all specifications in the table. Additional fixed effects include 3 digits industry, commuting zone, age and size quintiles, dummy for exporter in 2005, dummy for overdue loans in 2007, dummy for firm capable of issuing bonds, dummy indicating whether the firm has any loan with banks failing up to the year 2014 and fixed effects for the labor share bins. Standard errors in parentheses, clustered at the firm and bank-by-3 digits industry level. Referenced on page(s) [22] .
$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

cial leverage or pre-existing credit cycles determined by investment.

4 Results

In this section we provide causal evidence of the consequences of labor leverage for firm outcomes in response to a liquidity shock. 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 probability 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 almost 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). Online Appendix Figure C.1 reports the coefficients of a dynamic event study based on equation 5, with firms partitioned in labor share quartiles, for the first and fourth quartiles. The specification, which is very demanding as *all* observable controls and fixed effects are interacted with non-parametric year trends, shows that firms at the opposite spectrum of the labor share distribution have diverging trends up until at least 2012. However, there is not enough statistical power to statistically distinguish different coefficients. No clear pre-trends emerge.[27]

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 5 and 6). Extrapolating the reduced form employment estimates to aggregate employment variation in Portugal up to 2013, this shock alone explains around 29 percent of the employment loss. Online Appendix Figures 9 and 6 report dynamic event studies that shows the absence of pre-trends in employment variables with respect to the shock. The dynamic specifications feature non-parametric yearly controls by labor share-levels, further supporting the assumption that no differences in differential trends related to labor share are impacting our results.

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

[27]Online Appendix Figure 5 reports results for firm level financial variables. The labor-leverage amplification dynamics are present for other firms' real outcomes, such as total assets, current assets 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) (table 18).

firm survival. As a consequence, firms with more exposure to labor costs experience a relatively more volatile employment adjustment and a greater likelihood to fail following a negative credit shock.

4.1 On-the-job training

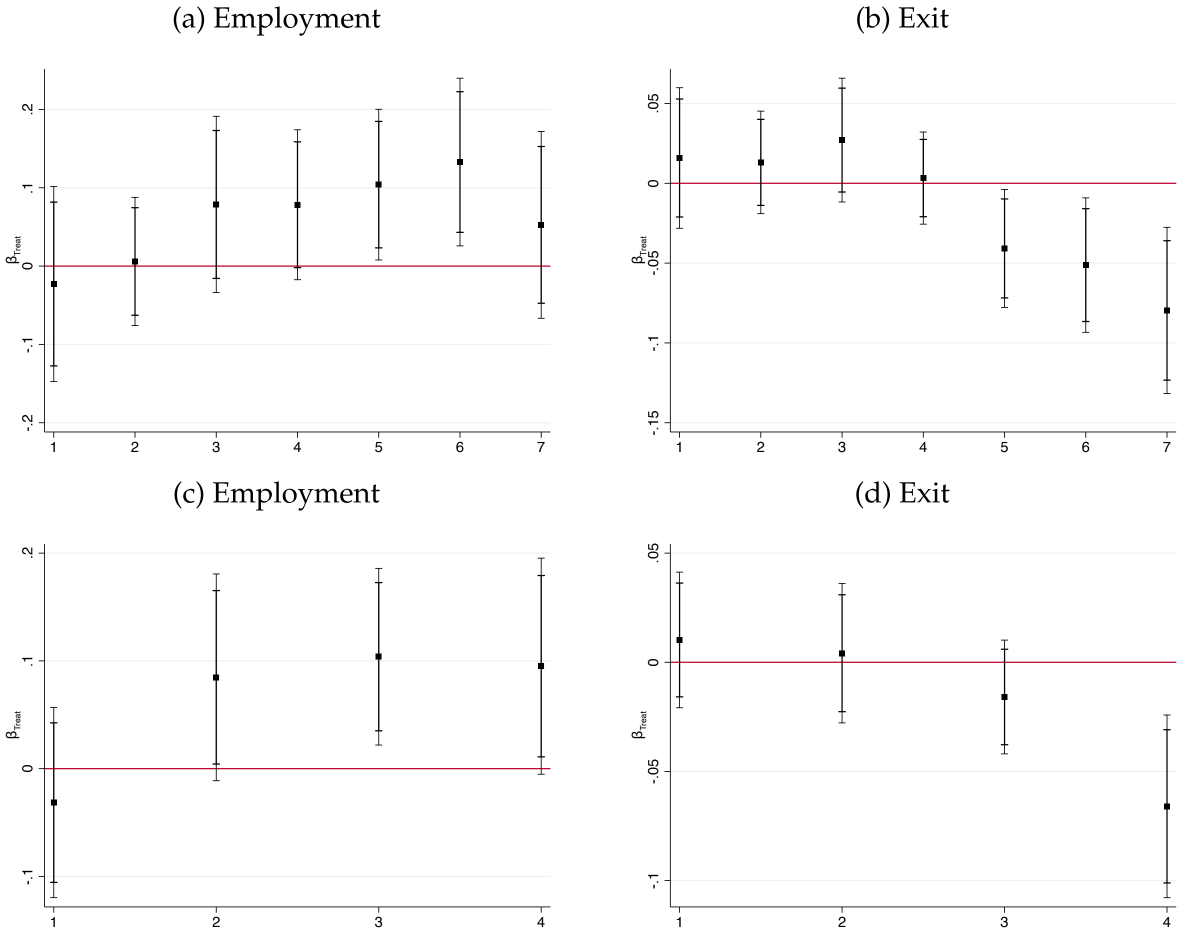
Our results up to now show that otherwise-similar firms with different labor share are differentially exposed to liquidity risk through a negative short-term credit shock. We now turn to investigate some of the mechanisms that could explain the observed results. We start by exploring the role played by workers' specialization. Intuitively, firms should exhibit stronger attachment to their workers when the tasks in their occupations require specialization and extensive training. Specialized tasks commanding a wage premium and incurring substantial search and hiring costs, would make firms hesitant to part ways with tenured, specialized workers (Oi, 1962; Le Barbanchon et al., 2023).[28] Therefore, a pertinent measure of labor fixity involves assessing the required training and skill specificity of tasks.[29]

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 in particular 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 based on a 3-digits occupation definition. We then aggregate the scores at the firm level, taking averages across employees within a

[28]Notice that, from the firm perspective, the distinction between firm-specific and general human capital is not crucial. Firm will be reluctant to part ways with workers with greater human capital due to firm specific training, or search frictions given scarce general human capital.

[29]A further consequence of firms' retention incentives should be that financially constrained firms are more likely to fire workers with steepest productivity profiles, often the young and low-tenured (Caggese et al., 2019). We corroborate this observation in Online Appendix Table 10 and Figure 6.

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 4. Referenced on page(s) [23] .

firm, and partition firms in quartiles based on the average scores in 2005.[30]

We use the OTJ scores and estimate the difference-in-differences specifications as in equations (4) and (5), where we substitute $LabSh_{bin}$s with OTJ_{bin}s. Figure 3 shows the results of these estimations. Consistent with the idea of labor as a quasi fixed factor (Oi, 1962), we find greater elasticities of employment to credit in firms that employ workers with higher training costs, as proxied by the OTJ score. Moreover, we also find a stronger probability of exiting for those firms, even if the results for exit are noisier and not statistically different across quantiles. 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 that these firms feature higher AKM (Abowd et al., 1999) compensation premia, which further suggests that the compensation policy is used in these firms to attract talent.

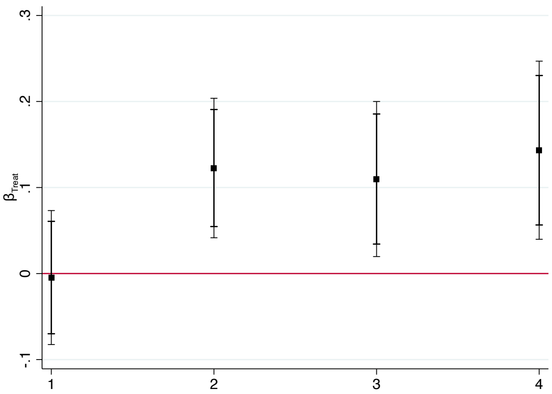
4.2 Additional supporting evidence and robustness

Heterogeneity by sector and worker characteristics. We provide additional evidence that firms direct their employment adjustments to young and high-skilled workers, while preserving long-term incumbents. The evidence is also consistent with the hypothesis that firms are unwilling to separate from the more experienced workers, which underwent more training or might possess more firm-specific human capital. In Online Appendix Tables 8, 9 and 10 and Figure 6 we provide additional evidence regarding our financial channel of labor rigidity on workers with different characteristics and across manufacturing and non-manufacturing firms. 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 further show that employment is cut more among specialized and young workers. In Online Appendix Figure 6 we also show that em-

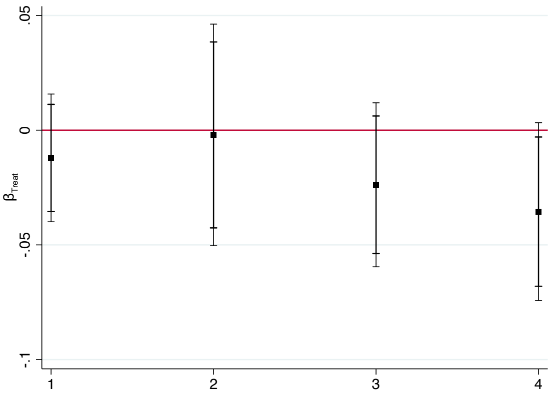
[30]We rely on ESCO to O*NET-SOC crosswalks, as developed by the ESCO data science team for the European Commission, to match 3-digit occupations across the datasets.

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

(a) Employment



(b) Exit



On-the-job (otj) training is defined as work carried out under the supervision of more experienced workers, and ranges from 1 (short demonstration) to 9 (several years of training). We estimate a coefficient for each of the four otj training quartiles, while controlling by means of a third order polynomial of the otj score. Each interacted treatment is instrumented by the interacted instrument. See 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 7. Referenced on page(s) [27] .

ployment 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 not the case, as long-term incumbents in the firm in the pre-period are on average 3 times as many as recent hires.[31]

Institutional rigidities. Our baseline results from Section 4 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.[32] Table 11 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.[33]

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. A natural question to ask is therefore

[31] 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, young and high potential workers should be the ones generating the strongest cyclicality in employment adjustment, as their surplus creation might be more backloaded than current liquidity needs.

[32] 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).

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

why we don't observe wage cuts instead of layoffs in the data in order to decrease operating leverage (Carneiro et al., 2014). We discuss here many potential reasons.

In line with Card et al. (2017); Kline et al. (2019), wages are expected to incorporate part of the training costs incurred by firms. Risk-sharing considerations between firms and workers can also affect wage dynamics (Guiso et al. 2005; Balke and Lamadon 2022; Souchier 2023). Xiaolan (2014) and Acabbi et al. (2024) show that the optimal dynamic contract balancing workers insurance against negative productivity shocks and retention incentives implies downward rigid wages and backloading of wages over time, as a function of human capital accumulation. In other words, downward rigidity in compensation emerges even in absence of explicit firing costs, through risk-sharing considerations, human capital accumulation and implicit leverage.

Despite the potential benefits of increasing risk-sharing with high-skill workers, firms tend to avoid wage cuts also for managerial reasons. Bertheau et al. (2022), by using survey data, show that firm managers tend to prefer layoffs as a method for donwsizing labor costs, as it provides a better control of the workforce. Wage cuts, and in particular selective ones, can be disruptive, demotivating, and alienate higher-skilled workers. Therefore, coordinating selective wage cuts is often deemed infeasible. Managers also state that the primary reasons for not laying off some specific workers is the fear of losing specialized skills embedded in them (coherently with findings in Oi 1962, in this paper and in Baghai et al. 2021).

Financial leverage and labor leverage. Another concern with the results might be that labor leverage could only be an imperfect indicator of financial leverage. Our results might be driven by a surge in credit demand before the shock, coinciding with an expansion in employment expenses. We show however that this does not seem to be the case at all. Correlations in Table 4 show that more constrained firms do exhibit overall low credit growth and financial leverage for the entirety of the pre-period, consistent with findings in the literature in Agrawal and Matsa (2013), Simintzi et al.

(2015).[34] This finding also suggests that these firms were *not* levering up to invest before the shock. All specifications control for different trends in credit and sales at the firm. At the same time, there is no evidence that firms are anticipating the liquidity risk stemming 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 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 base wages, 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 survival 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

[34] 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* discernible pattern.

Table 4: Correlations of observables with labor share

	Labor share		Labor share
Workforce Composition			
Sh. managers	(+)***	**Professional scores**	
Sh. specialized workers	(+)***	OJT score	(+)***
Sh. temporary workers	(-)**	ONET zone score	(+)***
Median tenure (perm.)	(+)***	**Financial variables**	
Sh. workers 55+	(+)***	Financial leverage (debt/ass.) (2005)	(-)***
Sh. highly educated workers	(+)***	ST debt/ass. (2005)	(-)***
Growth rates (06-08)		Financial leverage (2008)	(-)***
Managers	.	ST debt/ass. (2008)	(-)+
Specialized workers	.	Credit growth (06-08)	(-)***
Highly educated workers	.	Cash per worker (2005)	(-)***
MRP - gaps		Sh. ST credit (2005)	(+)***
Labor gap	(+)***	Sh. ST debt fully secured (2009)	.
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) [27,30,31,42] .

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

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 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:

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

while the specification for employment is:

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

Table 5 reports the results of the estimation by productivity bins. The estimated coefficients for the terciles of productivity are not significantly different across each other

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

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

See Appendix Section A for a list of the added controls and fixed effects present in the regressions. All regressions feature the full set of fixed effects and controls. We control for average TFP in 2005 and 2006, estimated according to to the method proposed in De Loecker and Warzynski (2012); Ackerberg et al. (2015) by means of a three factors of production gross output Cobb-Douglas production function. TFP can be estimated for less firms than the full samples depending on availability of the variables to compute it in CB. Given that we cannot control for unobservable characteristics in the exit specifications through firm fixed effects, we characterize the bank-firm matching by augmenting the set of controls with the share of loans that each firm has with micro banks and with banks failing up to 2014. We control linearly for the baseline effect of productivity. In the exit specification the fixed effects are interacted with year dummies, whereas the controls are kept constant and not interacted with any year dummy. In the employment specifications all variables are interacted with a post-period dummy. Standard errors clustered at the bank-industry pair level. Referenced on pages [33,35] .
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

in either specifications. Regarding exit, we find a significant effect for low-productivity firms, whereas estimates of the effect for medium- and high-productivity firms are lower. In terms of employment, the effects are evenly distributed across productivity bins, indicating that labor reallocation is not stronger for the worst firms in the economy.[35]

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 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

[35]The results are qualitatively unchanged under different production functions specifications and estimation procedures. See for instance Online Appendix Table 12 for the Translog specification.

between. We estimate the following specification for firm exit:

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

and the following specification for employment:

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

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.[36] 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 13). 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 affected by a reduction in the supply of credit, and 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

[36] TFP is estimated according to Ackerberg 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.

the propagation of the credit shock across the economy, and might be a relevant source of financial frictions that hinders productivity-enhancing resource reallocation.

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 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.[37] We run the following regressions at the firm-year level:

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

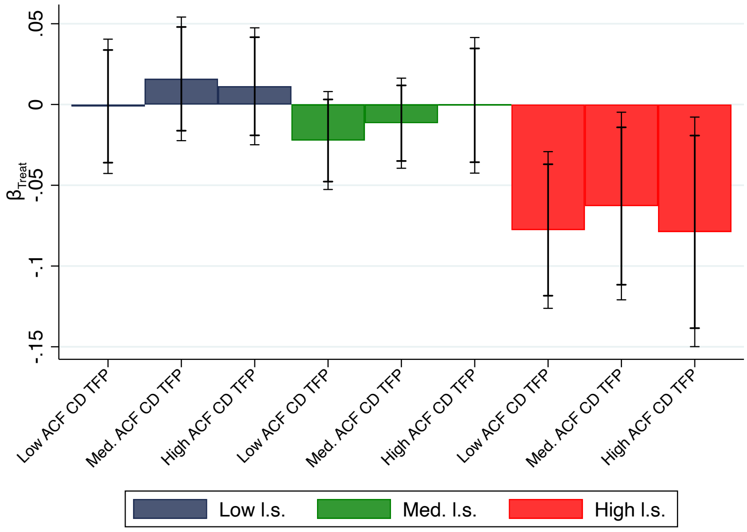
where $Post \equiv \{2009, 2010, 2011, 2012\}$. In the exit regression, the dependent variable is a dummy equal to 1 if the firm exits in year t and is not in the dataset anymore at $t+1$, while it is equal to $\Delta log(x)_{i;t+1,t}$ for the regressions of employment growth, full-time-equivalent employment growth and capital growth. We estimate a coefficient for TFP that reports the average effects for the pre-period years, and a variation of slope for the crisis years. The sample includes all firms for which we can compute the TFP, measured on the full residual of the estimation of a Cobb-Douglas production function with the Ackerberg et al. (2015) methodology. We control for year fixed effects and 3-digits industry fixed effects, and cluster standard errors at the 3-digits industry level.

We report our results in Table 6. Higher-productivity firms have lower probability of exit and greater input growth. 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 related to the

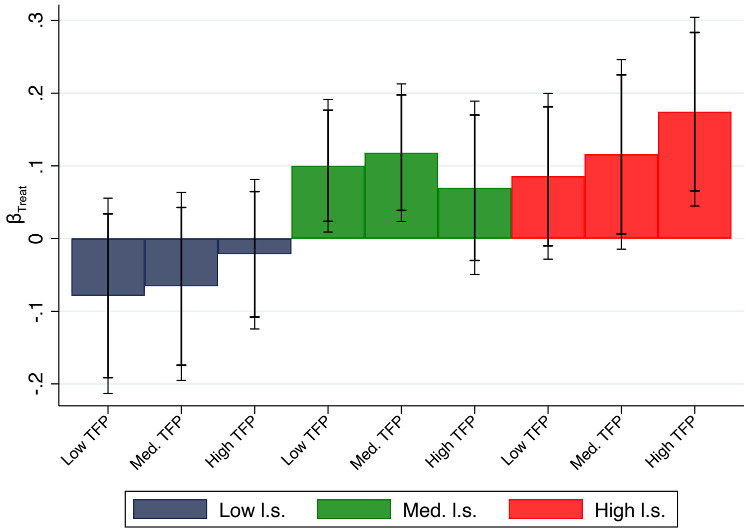
[37]We still exclude micro-firms, with less than 3 employees of Euro 1000 in turnover.

Figure 4: Regressions by labor-share and productivity bins

(a) Exit



(b) Employment



We estimate a coefficient for each of the nine interacted bins, while controlling linearly for baseline effects and their interaction. Each interacted treatment is instrumented by the interacted instrument. Labor share is defined as the ratio between employment-related costs and total value added (average of 2005-2006 levels). Productivity is estimated on a 3-inputs gross output Cobb-Douglas production function following Ackerberg 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 13. Referenced on page(s) [35,35] .

Table 6: Reallocation and TFP - full dataset

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

The regressions refer to the empirical specification in equation (11) in the text. All regressions feature 3-digits industry fixed effects. The sample consists of all firms in CB for which TFP can be computed (with the exclusion of the energy and construction sector). We also exclude very small firms, with less than 2 employees or less than a thousand euros in total assets or revenues on average for all years in which they are observed. All variables refer to the outcomes from t to $t+1$. We measure employment either as total headcount of full time equivalent employment, as reported in CB. The exit regression excludes the year 2005, given the CB structure. Standard errors clustered at the 3-digits industry level. Referenced on page(s) [36] .
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

labor frictions we analyzed in the previous sections.

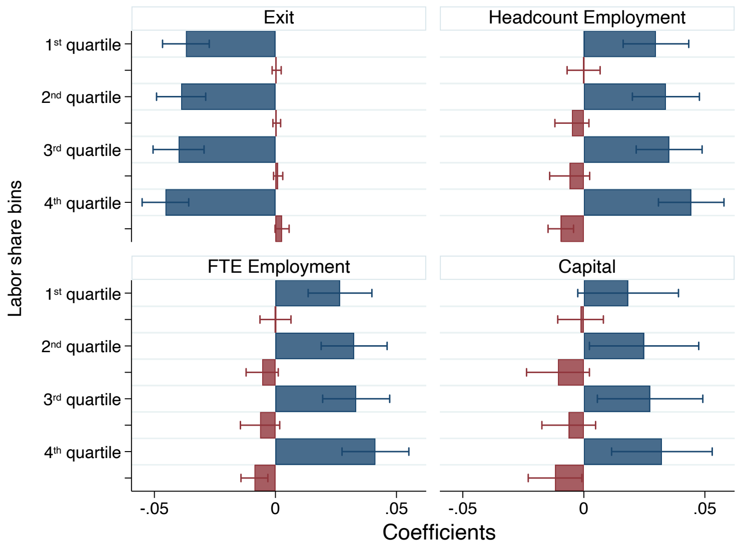
We then analyze whether our measure of labor rigidities might contribute to explaining these aggregate results. We split the firms by labor-share quartiles and we estimate a different effect of productivity on our outcomes in both normal times and the crisis period for each quartile. We run the following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

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

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 14. 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 stronger frictions in costs' adjustment throughout the crisis period.

These results indicate that in the post-Lehman period the cleansing effect was 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

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 14. Referenced on page(s) [38] .

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.

5.3 Labor rigidities and macroeconomic effects

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

We aim to aggregate the previous results and estimate the share of productivity variation explained by the credit shock. We follow Levinsohn and Petrin (2012);

Baqaee and Farhi (2019), 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:

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

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:

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

where A_i is technical efficiency (i.e. firm-level TFP), $D_i = (P_i Q_i)/(\sum_i VA_i)$ are the Domar weights, $\theta_i^{X_i}$ are the output elasticities of input X_i through the production function estimation and $s_i^{X_i}$ are revenue shares for each input X_i.[38] 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 Ackerberg et al. (2015) method on a translog production function.[39]We estimate an overall -12.37 percent aggregate-productivity growth for Portugal between 2008 and 2012, due to variation in TFP not explained by input reallocation. The combined contribution of allocative

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

[39]The exercise is carried out on the sample of firms that we use for the causal empirical analysis.

efficiency is close to a positive 1 percentage point.[40]

We perform a counterfactual exercise in which we calculate the productivity effects determined by decreases in misallocation as a consequence of the shock (Baqaee 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.[41] Under this assumption, reduced form estimates identify a source of variation in aggregate productivity stemming only from input misallocation, and we can adopt decomposition 2 from Baqaee and Farhi (2019).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 5, 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 15). The relevant wedge in the case the treatment effect is accounted for is

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

whereas the counterfactual wedge had the shock not hit is

$$\widehat{\theta_i^L - s_i^L}|_{NT} = \hat{\gamma}_i + \hat{\tau}_{post} + \mathbf{\hat{\Gamma} X_{i,pre}} + \hat{FE}_{i,post}. \tag{16}$$

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

[40] Baqaee and Farhi (2019, 2020) 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). Our results provide at least suggestive evidence of the decomposition of productivity growth, and its overall size, over the period.

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

in the aggregation exercise are:

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

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

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

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 around 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.[42]

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

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

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

Our study sheds light on the macroeconomic relevance of financial frictions at the firm level determined by labor rigidities, and poses interesting questions about how policy-makers should think about regulating their influence. The operating leverage determined by labor costs calls for policies that alleviate labor market frictions, and especially wage rigidity. The presence of labor-induced operating leverage would by itself call into question the efficacy of hiring credits in recessions in order to preserve employment levels. Instead of facilitating new hiring, policy-makers should evaluate measures that alleviate the burden of firms' current employment costs. Recent empirical evidence regarding short-term work and furlough programs of labor hoarding point 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 sullying effects for the economy through young workers' scarred labor careers (Acabbi et al., 2024).

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 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; Crouzet et al., 2022). These trends make it essential for economists to improve their understanding of labor financing, and rationalize why firms expose themselves to the risk inherent in the maturity mismatch in financing an investment good as high-skill labor with short-term credit. In turn, this understanding will enable policymakers 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 down-scaling or exiting the market altogether. Future research should 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 credit exposure 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 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: 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 identify 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. We provide suggestive of the exclusion restriction in Figure C.2. We show pairwise correlations of the instrument Z_i with firm-level observable characteristics, conditional on the set of fixed effects that we include in the main empirical difference-in-differences specification. In almost all cases coefficients are very small and close to 0. Still, given that some observables are significantly correlated with the instrument, we control explicitly for trends related to these observables in our regressions, plus other observables that we include to improve precision and robustness.

In a recent influential paper on the identification properties of shift-share instruments, Borusyak et al. (2022) highlight that the exogeneity of the instrument could stem from the quasi-randomness of the underlying *shifts* with respect to firm characteristics, but does not per se stem from the overall shift-share structure. We thus show for completeness in Online Appendix Figure 8 that the interbank exposures at bank level are not significantly correlated to 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 shortfall. If that is true, we can be confident our empirical analysis identifies average elasticities of firm outcomes to unexpected credit variation as depending on firms' decisions. For this empirical analysis we run the following specification:[1]

$$S_{i,b} = \beta FD_b + \mu_i + \varepsilon_{i,b}, \tag{B.1}$$

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 μ_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 banking 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

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

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.[2]

In order to show that the credit channel proxied by the interbank foreign funds exposure is not influenced by the dynamics of the sovereign debt crisis, we perform a robustness exercise in columns 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 1.[3] As a further check, we test whether our instrument Z_i computed at the firm level as in Equation (2) predicts credit dynamics after 2010, after controlling for credit variation up to that year. We run the following regression at the firm level on the set of firms

[2] Oster (2019) developed a framework to evaluate coefficient stability by observing how much estimated coefficients and $\mathbb{R}^2$ vary in regressions when one varies the 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.

[3] In our dataset, the exposure of banks to sovereign-issued financial liabilities is one order of magnitude smaller than the exposure to foreign interbank exposure. Exposures rates are rarely above 4 percent, while interbank exposure typically ranges between 10 and 20 percent.

active in 2010:

$$\Delta D_{i;st,2013-2010} = \beta \Delta D_{i;st,2010-2006} + \gamma Z_i + \mathbf{\Gamma X_i} + \varepsilon_i. \tag{B.2}$$

Results are shown in Table 16 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 asses 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.[4]

[4]Together with balance checks, observing that the credit transmission of the shock does not appear to be affected by the possible impact of observables, firm or match-specific characteristics lends credibility to the identification, given the validity assumptions in Goldsmith-Pinkham et al. (2020). If credit exposure or other characteristics were making the distribution of shares not-exogenous, one would

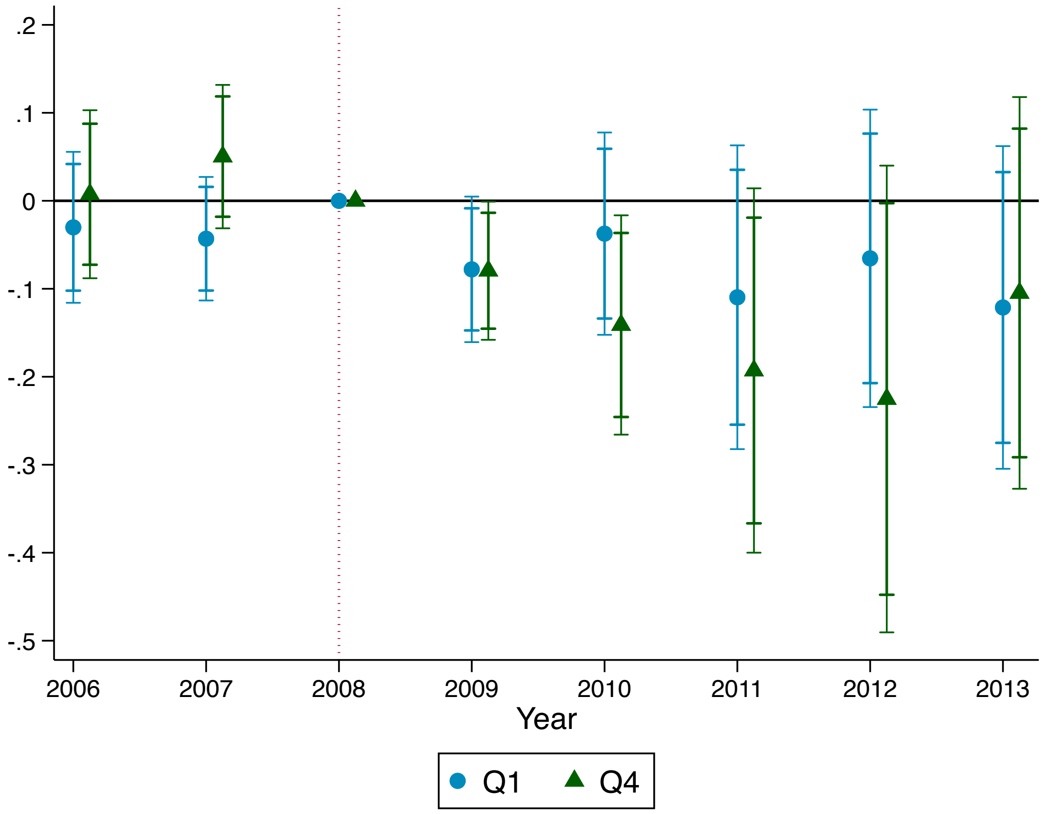
Finally, to lend further credibility to our estimates, in column 10 we run a regression analogous to the specifications by Iyer et al. (2014), and analyze the impact on total credit of the banks' exposure to the interbank market as a whole (taking into account both domestic and foreign exposures). Our 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.

surely expect semi-elasticites of credit with respect to interbank exposure to visibly vary, which is never the case.

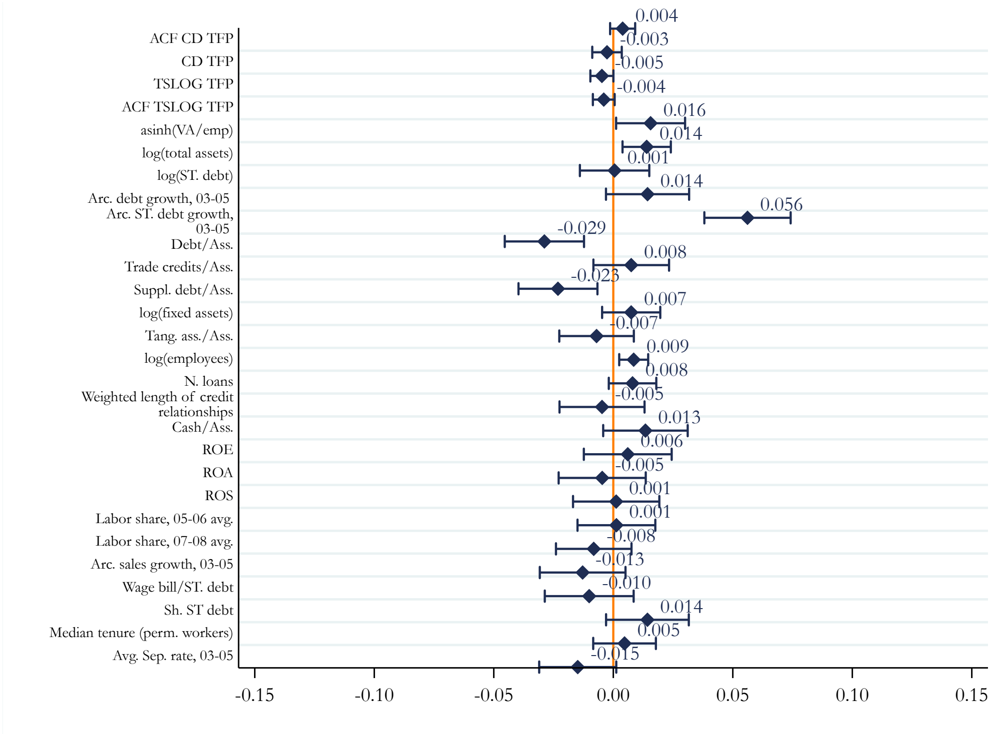
C Appendix Figures and Tables

Figure C.1: Dynamic event study, by firm labor share quartiles



We estimate a coefficient for each of the four 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. All fixed effects and pre-period observables are interacted with a year dummy. 1^{st} and 4^{th} quartiles' series coefficients reported. Number of firms: 11,766. 95 and 90% confidence intervals displayed, standard errors clustered at the bank-industry pair level.

Figure C.2: 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: Credit exposure level regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				$\Delta D_{st,pre-post}$				$\Delta D_{tot,pre-post}$
FD_{2005}	-2.104***	-2.151***	-2.186***	-2.145***	-2.192***	-2.159***	-2.237***	
	(0.229)	(0.221)	(0.218)	(0.251)	(0.251)	(0.248)	(0.247)	
	[-2.104 -1.971]							
$\frac{Sovs.}{Ass.}_{2009}$		-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 variables 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' liabilites to assets in 2005 (dometic *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.

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