The Financial Channels of Labor Rigidities: Evidence from Portugal*

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JOB MARKET PAPER

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January 29, 2020

MOST RECENT VERSION

Abstract

How do credit shocks affect labor market reallocation and firms' exit, and how does their propagation depend on labor rigidities at the firm level? To answer these questions, we match administrative data on worker, firms, banks and credit relationships in Portugal, and conduct an event study of the interbank market freeze at the end of 2008. Consistent with other empirical literature, we provide novel evidence that the credit shock had significant effects on employment and assets dynamics and firms' survival. 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, which we label "labor-as-leverage" and "labor-as-investment" financial channels. The credit shock explains about 29 percent of the employment loss among large Portuguese firms between 2008 and 2013, and contributes to productivity losses due to increased labor misallocation.

*Edoardo M. Acabbi thanks Gabriel Chodorow-Reich, Emmanuel Farhi, Samuel G. Hanson and Jeremy Stein for their extensive advice and support. The authors thank Martina Uccioli, Andrea Alati, António Antunes, Omar Barbiero, Diana Bonfim, Andrea Caggese, Luca Citino, Andrew Garin, Edward L. Glaeser, Ana Gouveia, Simon Jäger, Sudipto Karmakar, Lawrence Katz, Chiara Maggi, Carlos Robalo Marques, Fernando Martins, Luca Mazzone, Simon Mongey, Steven Ongena, Luca Opromolla, Matteo Paradisi, Andrea Passalacqua, Pedro Portugal, Gianluca Rinaldi, Stefanie Stantcheva, Ludwig Straub, Adi Sunderam and Boris Vallée, and the seminar participants at the briq workshop on "Firms, Jobs and Inequality", Barcelona GSE Summer Forum in "Financial Shocks, Channels and Macro Outcomes", the Petralia job-market 2019 bootcamp, the Federal Reserve Bank of Boston and Harvard macroeconomics, finance and public economics and fiscal policy lunches. The authors also thank Paulo Guimarães, Pedro Próspero, Fátima Teodoro, Maria Lucena Vieira and the staff of the "Laboratório de Investigação com Microdados" at Banco de Portugal (BPLim). Pedro Moreira and Antonio Santos provided excellent research assistance. The analyses, opinions and findings of this paper represent the views of the authors, and are not necessarily those of Banco de Portugal or the Eurosystem.
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1 Introduction

How do credit shocks affect labor market reallocation and firms' exit, and how does their propagation depend on labor rigidities at the firm level? Do credit shocks reinforce or hinder productivity-enhancing reallocation dynamics? We answer these questions by using an event study of the real effects of the interbank market freeze during the global financial crisis, and dissect how the shock to short-term credit supply spread to the corporate sector.

The recent global financial crisis (2008–2009) and the EU joint bank and sovereign debt crisis (2010–2012) revamped the interest of politicians, policy-makers and the general public in how financial markets affect the real economy. The severity of these recessionary episodes make it of paramount importance for economists and policy-makers to provide an accurate analysis of the implications and propagation of financial shocks. Moreover, the central role of the financial system in channeling funds across different sectors of the economy may have non-trivial implications for the overall productive system; financial frictions can hamper productive reallocation of resources and limit long-run growth.

We document how the responsiveness of firms to credit shocks is determined by their own financial flexibility, and in particular by their ability to adjust their labor costs. Labor costs constitute a sizable fraction of firms' cost structure, as in many advanced economies the labor share, albeit declining, is still above 50 percent (Karabarbounis and Neiman, 2014). Despite their large share in total costs, labor costs are often overlooked as a source of financial risk, and most macroeconomic models assume that labor is a flexible input in production. However, there are at least two important financial channels through which intrinsic labor inflexibility can amplify the effect of credit shocks and put firms' operations at risk. We label these channels "labor-as-leverage" and "labor-as-investment".

Labor-as-leverage refers to the operating leverage that results from the rigidity of compensation owed to incumbent workers. It is determined by significant adjustment costs at the firm level, in the form of hiring, search and firing costs, together with wage rigidity. Labor-as-leverage exposes the firm to greater liquidity risk, given the firm's inability to adjust partially fixed operating costs. Should it be hit by an unexpected adverse liquidity shock, a firm with relatively greater pre-commitments to labor, which is senior to many other expenses, might have to cut employment of its least protected workers and existing investments in order to honor these pre-commitments. The presence of institutional adjustment costs in the labor market, and/or internal incentives to retain workers with the greatest human capital (Oi, 1962), would then lead the firm to cut its most liquid investments, hiring, and separate from the least protected workers. Eventually, if the liquidity drought is too intense, the firm might not be able to honor its debt and workforce commitments, and exit the market.

In parallel, the labor-as-investment channel arises because of the possible mismatch in the timing of payments to workers and the cash flows from production, which makes workers akin to an investment good. This channel would be stronger in firms that need to employ a more qualified workforce, the hiring of which is likely to be costlier if these workers are a scarce input of production. The same channel would be present if firms have to pay training costs in order for the workers to be productive. As firms incur these labor-related costs in advance of realizing

revenue cash flows, they become subject to a working-capital channel, through which the labor costs are directly affected by the costs of external financing. As a consequence, an increase in credit spreads, by giving rise to a credit supply shock, increases the marginal costs of hiring, thus leading to hiring and investment cuts.[1]

We use the conceptual framework underlying these two channels to address our research questions. We analyze the impact and the propagation on labor-market reallocation, real outcomes for firms and firm survival of the interbank market freeze in 2008 in Portugal, which led to a sharp decrease in the supply of short-term liquidity to Portuguese firms. This event represents a unique opportunity to analyze the real effect of credit shocks and their interaction with labor-market rigidities for several reasons. First, the failure of Lehman Brothers was sudden and unexpected, and plausibly exogenous to the Portuguese economy. Second, the event led to a considerable dry-up of the interbank market, which Portuguese banks heavily relied on to finance their corporate short-term credit. Since Portuguese firms are highly dependent on bank credit (especially short-term) to cover their labor costs the shock has a strong potential to generate sizable real effects.[2]

We combine detailed administrative data on banks' and firms' balance sheets, a matched employer-employee dataset, and a credit register covering the universe of banks loans in Portugal to trace out the heterogeneous propagation of the credit shock across the corporate sector. We obtain a causal identification by leveraging a shift-share instrument for credit growth, calculated as the weighted exposure of a firm to the interbank market freeze through the banks with which it holds loan relationships. Our results highlight that the credit shock had statistically and economically significant effects on employment dynamics, the average likelihood of firms' survival and other firm level real outcomes.

More importantly, the estimated average effects hide substantial heterogeneity across different firms. The findings are in fact driven by our two financial channels of labor rigidities. In the presence of adjustment costs and wage rigidities, consistent with the labor-as-leverage story, firms are unable to alter their overall incumbents' wage bill. At the same time the requirement for some firms to pay search and hiring costs or part of the payroll in advance, exposes the firm to liquidity risk, consistent with the labor-as-investment channel. If firms use bank short-term credit to smooth liquidity outflows and inflows to pay labor costs, or to finance fixed operating costs, then wage policies, incumbents' wage bills and hiring and training expenses will constrain their responses and adjustments to shocks. Firms characterized by relatively more generous commitments to employees will experience higher cash-flow sensitivities of hiring, employment and investment as a consequence of both the labor-as-investment and labor-as-leverage channels.

[1]There is another potential indirect channel, closely related to labor-as-investment, that might explain the employment effects that we identify. If a firm does not finance labor through credit, but is subject to a borrowing constraint on financing capital, the effects that we identify would directly impact capital investment, and by the complementarity of production inputs would affect employment decisions, too (Fonseca and Van Doornik, 2019). While we do not rule out this channel, we argue that many features in our results point to the existence of labor-as-investment and labor-as-leverage. Complementarity effects, likely present in our results, do not undermine the validity of our conceptual framework.

[2]Administrative data indicate that the labor share in value added in Portugal is around 60 percent. According to the employment laws index by Botero et al. (2004) and several other international employment protection legislation (EPL) indexes, Portugal has among the strictest employment protection laws among advanced economies.

We estimate different treatment effects across firms with relatively greater labor costs exposure than otherwise observationally similar firms. We show that reliance on labor in production appears to constrain firms' ability to respond to unexpected credit shocks, through pre-commitments to labor and upfront labor-related costs. The average effects are predominantly determined by the firms most exposed to labor costs. Interestingly, we observe stronger treatment effects for labor-intensive firms that employ a more specialized workforce and correspondingly offer more generous compensation, thus suggesting that leverage effects induced by labor costs might be correlated with the investment in workers' human capital. Our findings highlight that the disruption in the operations of constrained firms triggered by the credit shock can be so severe that more labor-intensive firms are also more likely to fail, irrespective of their productivity ranking.

Our results show that, conditional on firms' exposure to labor financing and pre-commitments to incumbents, overall total factor firm productivity does not seem to meaningfully affect the ability of firms to respond to shocks. The dynamics that we identify are consistent with a "non-cleansing" effect of credit shocks at the micro level, meaning that we do not observe a strengthening of existing productivity-enhancing reallocation dynamics as a consequence of the shock. In fact, the intensity of the credit shock transmitted by banks is evenly distributed across firms, and thus the shock appears to be as good as randomly assigned across the corporate sector. As such, the heterogeneous results are plausibly not flawed by reverse causality, and we can successfully tie the observation of non-cleansing effects to labor rigidity. Put differently, the credit shock idiosyncratically spreads to the real economy through bank-firm linkages, and has the potential to prompt non-cleansing effects, driven by firms' endogenous exposure to liquidity risk through the financial channels of labor rigidity.

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

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

Contribution to the Literature Our work contributes to several strands of the academic literature. First, we contribute to the strand of the literature in corporate finance that analyzes the real effects of financial frictions, both at the microeconomic and macroeconomic level, with

a particular focus on the propagation of different kinds of financial shocks, chiefly credit shocks.[3] Our research relates to the analysis of the propagation of financial shocks through banks' credit supply, such as in Peek and Rosengren (2000), Khwaja and Mian (2008), Ivashina and Scharfstein (2010) and especially Chodorow-Reich (2014), who focuses on the financial crisis following the demise of Lehman Brothers.[4] We contribute to this line of research by using a dynamic event study to trace out the propagation of a credit shock on large firms in a small open economy, providing results on firms' propensities to adjust the employment of different kinds of workers, and the reaction of related investment decisions at the firm level.[5] We highlight that the effects of a short-term credit shock on employment growth and firms' survival are heterogenous, and give rise to interesting dynamics depending on firms' endogenous exposure to liquidity risk.

Second, our study is related to a literature in corporate finance analyzing capital structure determination in connection with financial frictions (Froot et al., 1993; Rampini and Viswanathan, 2013).The literature has mostly focused on financial leverage and capital financing, whereas our study is closer to a more recent strand of literature focusing on the financial frictions that might emerge in relationship to labor composition and financing. We especially relate to the literature analyzing the characteristics of labor that can make it akin to a quasi-fixed factor in production and an investment good from the firm's standpoint, which should in turn require long term financing (Oi, 1962; Hamermesh, 1989; Hamermesh and Pfann, 1996; Benmelech et al., 2015, 2019). In this context, some recent papers have focused on the increasing importance of analyzing the financing of labor, firm intangible capital, especially in the form of human capital (Danthine and Donaldson, 2002; Xiaolan, 2014; Simintzi et al., 2015; Serfling, 2016; Favilukis and Lin, 2016b,a; Sun and Xiaolan, 2018; Favilukis et al., 2019; Kuzmina, 2018; Caggese et al., 2019; Ellul and Pagano, 2019).[6] We contribute to this line of research by showing how labor rigidities become a sizable constraint on firms' internal funds, through the two

[3]Among the very first examples of this strand of literature are Bernanke and Gertler (1989), Gertler and Gilchrist (1994) and Bernanke et al. (1999), who theorized the existence of a "financial accelerator" in the propagation of both real and financial shocks, and Fazzari et al. (1988) and Kaplan and Zingales (1997), who focus on the cash-flow sensitivities of firms in relation to measures of financial frictions.

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

[5]In a recent study Farinha et al. (2019) document, in accordance with our results, that the funding shock to banks during the Sovereign Debt Crisis significantly increased firms' exit probability in Portugal.

[6]Xiaolan (2014) describes how, in order to provide incentives for workers to invest in human capital and not leave the firm, firms have an incentive to insure risk-averse workers against income risk, thus promising stable compensation and implicitly generating a leverage effect in downturns. Guiso et al. (2005) also analyze workers' insurance against income risk provided by firms, and finds that firms tend not to transmit idiosyncratic adverse shocks to workers. Favilukis et al. (2019) shares our focus on labor financing as a fundamentally important source of risk for firms, and call labor obligations "the elephant in the room" for explaining firm risk and credit spreads. Kuzmina (2018) shows that flexibility in employment contracts increases debt capacity and reduces operating leverage for firms. See Matsa (2019); Pagano (2019) for a review of the literature regarding the relationship between labor composition and firm financing.

financial channels of propagation that we define, labor-as-investment and labor-as-leverage.[7,8]

Third, we complement the literature on firm dynamics along the business cycle and the cleansing properties of recessions. Based on the seminal argument by Schumpeter (1942), there has been a general consensus that recessions have the positive by-product of cleansing low-productivity firms out of the market, thus improving the allocation of resources. Davis and Haltiwanger (1990, 1992) revamped interest in this subject and confirmed the existence of a cleansing effect, by analyzing job flows and firm dynamics using US Census data. In contrast, some authors such as Barlevy (2003), Ouyang (2009), Osotimehin and Pappadà (2015) and Kehrig (2015) question the unconditional existence of the cleansing effect, and argue that financial frictions might attenuate or even reverse it, possibly even turning it into a "scarring" effect. Along those lines, Foster et al. (2016) observe that the Great Recession featured less productivity-enhancing inputs' reallocation and a weaker cleansing effect in firms' exit. They argue (but do not provide causal evidence) that the fact that that recessions started as a financial crisis could rationalize this finding, consistent with the possible "scarring" effects of financial shocks. We directly contribute to this literature by showing that, in the context of the propagation of the financial crisis to Portugal from 2008 onwards, there is no evidence of increased cleansing. We employ a causally identified event study, and document that financial shocks generate non-cleansing dynamics, which appear to be correlated with the specific financial friction we identify.[9]

Finally, we contribute to the literature analyzing productivity growth and factors' misallocation at the macroeconomic level, and the relationship between productivity growth, financial development and financial factors in general.[10] We adopt the methodology from some recent studies in productivity estimation (De Loecker and Warzynski, 2012; Ackerberg et al., 2015; Lenzu and Manaresi, 2018) to measure firm-level distortions, and connect their evolution to the

[7]For an outline of theoretical macroeconomic models addressing the impact on labor and financial frictions, see among others Petrosky-Nadeau (2014); Schoefer (2015). Schoefer (2015) provides one of the first instances of a theoretical setting in which business cycles amplification is modeled to stem from rigidities in *incumbents'* wage rigidity, and not new hires' rigidity, which has been the focus of the literature on macroeconomic fluctuations and wage rigidity (Gertler and Trigari, 2009; Gertler et al., 2016). The Portuguese setting provides some of the best available empirical evidence on the different wage cyclicality between new and incumbent workers (Martins et al., 2012; Carneiro et al., 2012).

[8]We implicitly relate also to the literature in labor economics analyzing the relevance of firm specific or general human capital in turnover decisions (Oi, 1962; Becker, 1962; Jovanovic, 1979b,a; Portugal and Varejão, 2007), wage setting under monopsony (Manning, 2011), rent-sharing within the firm (Card et al., 2017; Kline et al., 2019), and workers' substitutability (Jäger, 2019). Garin and Silverio (2018) provide an analysis of rent-sharing around the same period of our analysis in Portugal, as a consequence of unexpected demand shocks determined by the exposure of different trade flows at the firm level.

[9]Dias and Robalo Marques (2018) apply methods from this strand of literature (Davis and Haltiwanger, 1990; Melitz and Polanec, 2015; Foster et al., 2016) to show that the period of the EU sovereign debt crisis was more cleansing than previous periods for the Portuguese economy. We show that if one takes into account the period after the Lehman Brothers failure as the "crisis" period as well and considers the years before as "normal times" there is actually no evidence of enhanced cleansing.

[10]Seminal studies in this literature are Hulten (1986) for the first theorization of an aggregation result from individual firm or establishment to aggregate productivity, and Hall (1988), Restuccia and Rogerson (2008) and Hsieh and Klenow (2009) for the formal definition of misallocation through the measurement of wedges between marginal costs and prices of input utilization and output. Seminal studies analyzing firms productivity and size distributions as a function of the presence of adjustment costs are, among others, Hopenhayn (1992); Hopenhayn and Rogerson (1993); Gomes (2001). More recent studies providing advancements in methodologies in order to identify and compute aggregate productivity growth and misallocation are Levinsohn and Petrin (2012), Petrin and Sivadasan (2013), Bartelsman et al. (2013), Baqaee and Farhi (2019a,b).

impact of a financial shock, as in Bai et al. (2018), Fonseca and Van Doornik (2019) and Bau and Matray (2019). To the best of our knowledge, our work provides the first analysis of the impact of a negative credit supply shock on misallocation in the context of a causally identified event study.[11] Furthermore, we are the first to show the existence of a perverse non-cleansing selection mechanism in firm exit and inputs reallocation, which we show is related to the degree of labor rigidities measured at the firm level.

Section 2 briefly summarizes the conceptual framework that will guide our empirical analysis. Section 3 describes the data and sample selection, and Section 4 describes the empirical strategy we employ to identify the credit shock. Section 5 reports the average firm-level results, and Section 6 the heterogeneous results. Section 7 analyzes the macroeconomic effects of labor rigidities.

2 Conceptual framework

We start our analysis with a brief description of the conceptual framework underlying the financial channels of labor rigidities and their real effects at the firm level.

A firm may need to finance part of its payroll and other production inputs with credit (i.e. a form of working capital) because of the possible time mismatch between cash inflows and outflows due to the nature of its economic activity or to the presence of upfront training and search costs. In this case the marginal costs of output and inputs incorporate the cost of credit, and variations in the cost of credit feed back into inputs demand. Hence, labor costs directly depend on the cost of credit, and a sudden increase in credit spreads demanded by banks would decrease the amount of labor demanded by firms.

In the most extreme cases, firms may have to forgo any working capital and only finance production inputs out of retained earnings. In general, any firm that discounts future profits and does not hire its labor on the spot market in each period (due to hiring and firing costs, search costs, and long-term labor training cycles) will incorporate credit rates into the long-term value of hiring, exactly as for any investment good. Then, if a firm does not match the maturity of the investment with its financing, and finances long-term investment in labor with short-term credit exposures or fungible credit lines, it will be subject to liquidity risk in the short run, and to the volatility of short-term credit rates in credit markets. A rise in these credit spreads will thus immediately decrease labor demand. This is the labor-as-investment channel.

This channel would manifest itself in firms that need to employ a more qualified and selected workforce, the hiring of which is costlier if these workers possess specific skills or education that make them a scarce input of production. Moreover, the same channel would be present if there are significant training costs that firms have to incur for the workers to be employable in the production process.

A credit supply shock not only affects labor demand by changing the effective cost of labor,

[11] Schivardi et al. (2018) analyze credit supply in Italy during the Sovereign Debt Crisis, and provide evidence of limited effects of credit misallocation for the real economy. On the contrary, Blattner et al. (2019) use the 2012 EBA capital requirements exercise as a natural experiment to show that Portuguese banks distorted the allocation of credit towards firms with underreported loan losses, which led to sizable negative productivity effects.

but it also drains the firm of liquidity to finance current expenditures. In such a scenario, rigidities in compensation and employment adjustments might impair firm production and ultimately cause it to exit the market. We label this channel labor-as-leverage. The partial fixity of labor expenditures related to incumbent workers creates a channel of operating leverage, which impacts the firm's ability to adjust costs following unexpected shocks to productivity or liquidity. Given that pre-committed payments to labor are generally senior to other committed payments, this kind of labor-induced operating leverage becomes a source of financial risk for the firm, similarly to (if not more pressing than) financial leverage itself.

This transmission channel for credit shocks is conceptually distinct from labor-as-investment, which predominantly matters for hiring and investment decisions. In fact, labor-as-leverage amplifies the effects of a shock, and has the potential to impact firing decisions and firm survival itself. The rigidity in incumbents' costs might affect firm outcomes through the working-capital channel, if a share of the whole workforce compensation has to be paid in advance, but might also result from the need for firms to finance other sources of fixed costs to continue operating. In turn, labor-as-leverage can be determined by the presence of institutional rigidities in the labor market that firms need to comply with, or by dynamics of human capital accumulation at the firm level. In fact, the firms' need to employ a skilled and highly trained workforce makes labor a quasi-fixed input in production (Oi, 1962; Jovanovic, 1979a) which determines rigidities in employment and cost adjustment even without explicit firing costs and downward wage rigidity in the labor market. Thus, it is important to notice that labor-as-leverage and labor-as-investment can depend on and reinforce each other in many cases. If workers with greater hiring and training costs command higher wages because of their lower substitutability with outside workers, it is reasonable to expect that labor-as-investment would give rise to labor-as-leverage. On the other hand, if firms can use higher salaries and overall compensations to attract and retain workers of higher quality, labor-as-leverage might lead to more labor-as-investment, too. In other words, the two channels are very likely to coexist in the same firm, and interact and reinforce each other.[12]

If these dynamics are present, they give rise to sharp predictions regarding how firms with different exposures to labor rigidities through the two channels should respond to an unexpected shock to short-term credit, which is akin to a cash-flow shock.

Prediction 1 *A negative credit shock will lead to lower hiring and employment, and will reduce other expenditures at the firm level.*

A direct link between the cost of credit and the marginal cost of hiring through labor-as-investment, or the direct and indirect influence of rigidities in compensation adjustment and fixity in labor through labor-as-leverage, would directly impact hiring. The amplification of

[12]There is evidence in the recent literature that the interaction and amplification effects of the channel might depend on the cycle and whether human capital is firm-specific or general. Baghai et al. (2020) show, by using Swedish micro-data, that firms in financial distress because of idyosincratic demand shocks are more likely to lose high-skill workers. The cost of losing this particularly scarce labor input adds to the costs of financial distress. This mechanism is consistent with our finding that firms more reliant on high skill, specialized and imperfectly substituable labor have more coservative capital structures (as in Simintzi et al. (2015); Favilukis et al. (2019)). Being able to parse out specific and general human capital would allow to identify in which circumstances labor is more likely to create operating leverage.

the liquidity shock through cost fixity could also affect current employees, especially the least expensive to fire. The pecking order of firings would likely be LIFO (last-in-first-out), because of firing costs that are increasing in incumbents' tenure (which is indeed part of the story in Portugal) or the possibility that tenured incumbents accumulate firm-specific human capital through which they hold up the firm's operations, or general skills that they could use to increase their bargaining power and obtain employment protection and higher salaries. This conceptual experiment highlights the fact that both channels might interact, and that investment in labor inputs might support the labor-as-leverage channel, too.

The two channels might also impact other outcomes at the firm level: firms might try to overcome the liquidity shortage by extracting liquidity through other means, such as trade credits or debt towards suppliers, depending on their market power in the supply chain. They might have to reduce the investment in current assets or, by amplification through leverage or by complementarity through the investment channel, even decrease capital investment.

Prediction 2 *A negative credit shock will decrease the value of the firm, and in the presence of fixed costs will increase firm exit.*

Following the same logic as in Prediction 1, the distortion in production inputs demand and allocation will necessarily decrease firm value. In the presence of fixed operating costs this means that there is an increased chance for a firm hit by a negative credit shock to have a productivity too low to satisfy its fixed commitments. Given that labor-as-leverage amplifies the effects of credit shocks, it is reasonable to attribute the increased probability of firm failure to this channel.[13]

Prediction 3 *The effects of Predictions 1 and 2 will be stronger in firms more exposed to labor costs, in the presence of rigidities in compensation and adjustment costs.*

This is the main contribution of our paper. We isolate the variation related to firms' reliance on labor costs in their operating-cost structure. We show that firms more exposed to labor costs through their own compensation and wage policy decisions over time end up having greater elasticities of employment and investment, and eventually a greater likelihood to fail. This finding implies that it is more likely to observe exits of relatively high productivity firms if these firms are particularly exposed to labor rigidities, which underlies the logic of the non-cleansing hypotheses for financial shocks. Section 6 provides a detailed description of how we identify labor rigidity through labor costs, and how we think the two channels of labor rigidities determine the results and affect each other.

Appendix A develops a simple model of a firm's credit and labor demand subject to a working-capital constraint, which formalizes and provides intuition regarding some of the chan-

[13] A firm that, absent other fixed commitments, is unable to operate profitably after the shock is likely to be ex-ante unproductive, and we would thus not define this an instance of firm exit as a result of the credit shock per se. The fact that in our results we identify high productivity firms failing as a result of the credit shock should at least partly dismiss this story. Moreover, in our sample there is no evidence that firms with high incumbents' turnover face higher exit rates as a consequence of the credit shock, a finding which would be an instance of failure determined directly by labor-as-investment. Results are not reported but are available upon request.

nels that we state here. The model also provides proofs of Predictions 1, 2, 3 in that simple setting.

3 Data and sample selection

We start with a summary of Portugal and its economy in 2008–2009. Then, we describe the data and the criteria according to which we define the sample of analysis, and reports some descriptive statistics relative to firm and workforce characteristics. We refer the interested reader to Appendix B for a more detailed description of each dataset and the sample-selection criteria.

3.1 The credit shock in Portugal in 2008-2009

We aim to analyze the firm response to variations in credit supply around the end of 2008, when the US investment bank and global financial services firm Lehman Brothers filed for bankruptcy, thus initiating a global financial crisis that spread internationally through the banking system and financial networks. 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".

The global financial crisis and the ensuing credit shock to the Portuguese banking system feature some peculiar characteristics, which make them particularly apt for analyzing the impact of credit shocks on real variables, the effects on firm survival and in general the ability of firms to cope with unexpected liquidity shocks. First, Portugal is a small open economy, predominantly characterized by medium- and small-size firms, heavily reliant on bank credit as in other southern European countries. Firms are not likely to be able to access alternative means of financing during a financial crisis, as very few of them are able to issue bonds (which can in any case be controlled for by the econometrician). Moreover, firms 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 a shock.[14] Second, the fact that the decrease is mostly driven by short-term credit is particularly interesting for the analysis of employment decisions and firm dynamics, as such shortages are likely to be unexpected for firms, and to be directly related to their day-to-day liquidity management. Given the need to smooth liquidity mismatches between cash-flows and revenues, this form of credit is commonly used as a mean of financing current expenditures, such as stipends, labor costs and in general working capital, and a shortage would likely impair a firm's smooth functioning.[15] Third, the shock originated outside the real economy and the Portuguese economy altogether. Thus, conditional on controlling for possible endogeneity or selection in banks' portfolios, our setting offers the best conditions for causally identifying an exogenous credit shock for banks.

[14]See Bonfim and Dai (2017) for evidence on relationship lending in Portugal. Iyer et al. (2014) document that firms, in the context of the same credit shock we study, did not seem able to compensate for the lost credit supply with other forms of credit.

[15]See the ECB "Survey on the Access to Finance of Small and Medium Enterprises" (SAFE) and the "Bank Lending Survey" (BLS) for information on firms' use of banks' external finance and especially short-term debt. We also address this point in the empirical analysis, where we show that the shock seems to be mostly correlated with working capital dynamics, but not with long-term financing of capital investment.

Before the end of 2008, the Portuguese economy did not suffer from the global financial crisis directly, but rather through indirect channels, such as the collapse in global export demand (Garin and Silverio, 2018). Moreover, unlike the United States or Spain, Portugal did not suffer from the burst of a housing bubble (Fradique Lourenço and Rodrigues, 2015), had in place regulations discouraging the set-up of off-balance-sheet vehicles for banks, which would have likely been used to get exposure to US commercial paper and subprime lending from the US (Acharya and Schnabl, 2010), and aggregate credit supply was stable if not mildly increasing. 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.[16] The fact that liquidity suddenly dried up, that these financial instruments were often unsecured and, consequently, that the market for them was based on trust across financial institutions effectively determined a collapse in the volume of funds exchanged. Figure 1b reports the aggregate volume of foreign interbank liabilities in the Portuguese banking system, measured as the sum of short-term deposits (up to 1 year) and repos where the counterparty is a foreign financial institution (excluding central banks). The volume of credit intermediated in this market 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.[17][18]

Given the inability to easily obtain liquidity for their own day-to-day operations in a period of global financial turmoil during which capital injections were arguably hampered as well, banks around the world had to increase spreads and required collateral, and reduce the amount of credit that they supplied to the real economy and non-financial businesses, as shown for instance by the ECB "Bank Lending Surveys" at the time. Iyer et al. (2014) document these dynamics in Portugal, and analyze how credit supplied at the credit exposure and firm level changed as a function of the exposure of Portuguese banks to interbank funds at the beginning of 2007.[19] Figure 1a shows the aggregate trends for regular (neither overdue nor under renegotiation) short-term (with maturity less than one year, or liquid credit lines with no defined maturity) and foreign short term interbank funds (for banks in our final sample).[20] Credit supply was

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

[17]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 we consider total interbank funds instead of just foreign ones.

[18]Right after the collapse of Lehman Brothers, central banks around the world quickly acted to provide liquidity to the banking systems. The ECB, for instance, introduced a new fixed rate tender procedure with full allotment, which allowed banks to obtain virtually unlimited liquidity by posting a wide variety of collateral. The fact that we find significant effects of interbank exposure on short-term credit dynamics up to 2010 (see Table D.4) suggests that, coherently with findings in the recent literature (Cornett et al., 2011; Acharya and Skeie, 2011; Acharya and Merrouche, 2013), banks were hoarding liquidity without relaxing credit conditions.

[19]The first episode of distress associated to the global financial crisis was BNP Paribas' decision in August 2007 to freeze redemptions on three of its money market funds, due to the inability to price the assets in the portfolios exposed to the US subprime housing market.

[20]We group credit lines with no defined maturity as "short-term" because in the dataset this category comprehends all those exposures that, once withdrawn by the customer, should undergo renegotiation with the bank in order to be rolled-over.

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. Appendix Figure E.1 shows analogous aggregate trends for long-term and total regular credit. Overall, from the start of the financial crisis to the end of 2013 the total volume of credit under consideration shrank by 30 percent (regular credit) and 40 percent (short-term credit). In this way, the financial crisis originated in the banking system in the US spread to the Portuguese real economy. Figure E.2 documents how the unemployment rate started to steadily increase in 2008, especially for the youngest workers, reaching a peak at above 16 percent in 2013 (40 percent for workers under 25).

3.2 Data

Our analysis combines four main datasets. These are confidential administrative datasets provided by the Bank of Portugal, featuring: a matched employer-employee dataset, covering almost the universe of firms and attached workforce in manufacturing and services in Portugal; a firms' balance-sheet dataset, covering the universe of firms; a bank-firm matched credit registry, with data at the exposure level for the universe of loans; a banks' balance sheets' dataset. The period covered in our analysis spans from 2003 to 2013.

Our employer-employee dataset, containing detailed data at the worker and firm level, is the *Quadros de Pessoal* (henceforth QP). It is a dataset collected and managed by the Ministry of Labour, Solidarity and Social Security, that draws on a compulsory annual census of all firms employing at least one worker at the end of October each year. The dataset covers approximately 350,000 firms and 3 million employees per year. The dataset features detailed data at the firm level (location, industry, annual revenues, structure of ownership and total employment), at the establishment level, and at the worker level (age, gender, occupation, qualification, level of education, type of contract, date of hire and last promotion, hours worked, base stipend and extra compensation).

The firms' balance-sheet database is the *Central de Balanços* (henceforth CB), managed by the Bank of Portugal. It consists of a repository of yearly economic and financial information on the universe of non-financial corporations operating in Portugal from 2005 to 2013. It includes information on sales, balance-sheet items, profit and loss statements, and cash flow statements (after 2009). It is the most reliable dataset in terms of coverage of firms active in Portugal; we also use it also to determine firm exits.

We construct the bank-firm matched credit dataset from the Bank of Portugal's own credit registry, the *Central de Responsabilidades de Crédito* (henceforth 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 granted but still not in use (potential), 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 (henceforth, BBS). For each balance-sheet item (liability or asset) 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).

Throughout the analysis we resort to some other minor datasets, both confidential and publicly available. We refer the reader to Appendix B for a detailed description of their features, what variables we use and what procedures or computations we perform.

3.3 Sample selection and descriptives

Our main analysis consists of an event study in which we estimate the adjustment of real outcomes at the firm level to an unexpected variation in short-term credit supply.

We combine all the four administrative datasets to obtain a complete picture of firms' and workers' conditions and their linkages to banks through credit. We restrict our attention to firms in mainland Portugal, and exclude the agricultural sector, the fishing sector, the energy sector (extraction, mining and distribution), the construction sector and the financial sector itself. To study firms' response to the shock, we consider firms with a credit relationship with any bank in 2005 (before the shock), and condition on their survival until 2009 (after the shock). Moreover, we focus on firms with at least 9 employees, which is approximately the threshold for the fourth quartile (75th percentile) in the distribution of firms' sizes in the years before 2009, and covers more than 60 percent of the workforce in the QP in the pre-period. 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.[21] Our final sample spans 14,846 firms and 31 banking groups, even if most of the regressions that require also balance-sheet variables features a sample of 13,806 firms. 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 summarizes the representativeness of our sample with respect to the set of active firms in 2005 with bank credit. Our sample has an approximate coverage between 60 and 70 percent of the QP's workforce and revenues in the pre-period. Tables 2 and D.3 present firm level descriptive statistics for the firms included in our sample for the pre-period.[22] The average firm in our sample has 55 employees and a turnover of approximately EUR6.4 million. However, the distribution is heavily skewed to the right, as the median firm has 22 employees and a turnover of around EUR2.1 million. For the average firm, the leverage ratio —intended as regular credit over total assets —is 26 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 1.24 (median 0.47).

Figure E.3 shows credit market concentration in Portugal over the years, in terms of the regular credit of the largest banks in the country. The figure clearly shows how the credit market

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

[22]We deflate all nominal values in the analysis by the 2013 CPI, except for the productivity and marginal products estimation.

in Portugal is heavily concentrated, as also found by Amador and Nagengast (2016). Figure E.4 shows the distribution of the number of credit relationships by firm in 2005, both for firms in our sample and in the full dataset. The figures provide further evidence that the Portuguese credit market tends to be concentrated, and a lot of firms only have a single relationship with one bank. However, our sample consists of relatively bigger and more organizationally sophisticated firms, and thus features a substantially lower share of firms with single banking relationships.

4 Empirical strategy

4.1 Characterizing banks' credit supply

In this section, we present our empirical strategy to causally identify the impact of a variation in short-term credit supply on different real measures at the firm level, such as employment and other forms of real investment, and on the probability of failure. We analyze firms survival with a linear probability model, and other firms' real adjustments with a difference-in-differences empirical strategy.

The complication with measuring the causal impact of a variation in credit supply on any firm-level real variable, which is in itself the equilibrium outcome of a firm-level decision, is that the simple observation of credit variation does not convey any information on the unobserved effective willingness of a bank to supply credit vis-à-vis the unobserved credit demand by the firm. As long as the firm's credit demand is correlated with a firm's investment decision, and with idiosyncratic investment opportunities, the econometrician needs some way to isolate the component of credit variation that is only related to banks' supply decision. In the presence of a credit shock that affects the ability of a bank to supply credit to firms regardless of their credit demand, the econometrician also needs to also find an indirect measure of the bank's credit supply decision. We use an instrument to identify banks' supply decisions. [23]

In order for an instrument to be valid in disentangling the exogenous variation in credit supply, it must correlate to firms' real outcomes only through credit variation. Moreover, its assignment to firm-bank pairings must be as good as random conditional on observables. In the case of firms' real investment decisions, this implies that the econometrician should verify that there are parallel trends in firms' behavior absent the treatment, in our case the credit shock. It is also necessary to avoid bias in the estimates stemming from endogenous non-random matching of banks and firms in the years leading to 2008. Relationship ties between banks and firms in Portugal are sticky, and the average duration of a relationship is around 9 years Bonfim and Dai (2017). Nonetheless, it is possible that firms and banks re-sorted themselves

[23]Several empirical works in the recent years (Khwaja and Mian, 2008; Chodorow-Reich, 2014; Amiti and Weinstein, 2018) address the endogeneity issue by exploiting the fact that firms tend to have multiple bank relationships at the same time. By observing how much banks vary credit supply to other attached firms, while controlling for each firm's demand through firm-level fixed effects, these works indirectly obtain a measure of the predicted variation in credit that a firm should expect, independently of its own idiosyncratic demand. However, this strategy has the weakness that it does not allow to compute a measure of the credit-supply shock for firms with only one lending relationship. Thus, it would not be particularly appropriate in our setting (see Figure E.4) but would better apply to the analysis of big and financially sophisticated firms which optimally choose to diversify their credit exposures among different financial intermediaries.

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. If switching across firms and banks in anticipation of the shock is relevant, it is possible that firms in 2008 are no longer associated with the same banks from 2005. Thus, observing a strong first stage in the regressions should lend credibility to our strategy and mitigate any concern of endogenous switching or re-sorting between firms and banks.

Finally, to further control for other sources of observed and unobserved heterogeneity that might affect our estimates, we saturate the model with multiple fixed effects and firm level observables interacted with a time variable, 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 our empirical exercise the endogenous treatment variable that we instrument, labeled D_i^j, is the variation in the average short-term credit of firm i in period j, where j is either two years in the pre-period (between 2006 and 2007) or in the post-period (between 2009 and 2010). We leave out 2008 because some signs of financial distress were already present but the financial crisis had not yet escalated. In a similar fashion we calculate the shock up until 2010, in order to avoid considering credit dynamics pertaining to the EU sovereign debt crisis. Following the literature analyzing firm-level employment flows with micro-data (Davis et al., 1996) and the more recent one on the real effect of financial shocks (Chodorow-Reich, 2014) we measure the credit shock S_i to a firm i as a symmetric growth rate:

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

This measure ranges between -2 and 2. It is a particularly appealing measure in this context because it allows us to consider credit variation that ranges from the creation of a credit relationship (value 2) to its complete termination (value -2). Moreover, it generally limits the influence of outliers on empirical specifications.[24]

Following Iyer et al. (2014), we propose an instrument for credit supply 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, a year out of sample. Foreign interbank liabilities are measured as the sum of short-term deposits (up to 1 year) and repos where the counterparty is a foreign financial institution (excluding central banks). As this is defined at the bank level, we need to compute a measure of firm indirect exposure to the interbank market through its bank networks. We build a shift-share instrument at the firm level, in which the shift component is the bank's exposure to the foreign interbank market and the shares are the shares of a firms' short-term credit with each bank in 2005. Formally, define the foreign exposure of bank b as FD_b and firm i's share of short-term credit with bank b in

[24]We still eliminate the outliers in the credit-growth data, dropping the 2.5 percent greatest positive variations.

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 the set of banks with a credit relationship with firm i in 2005 and $\sum_{b \in B_i} \omega_{i,b} = 1$.[25]

In order for such an instrument to disentangle a causal effect, we need to verify that it is quasi-randomly assigned, i.e. it's distribution across firms is plausibly random conditional on the observables that the econometrician can control for. Passing this test guarantees that the estimated effects are not the spurious by-product of other dynamics stemming from the non-random matching between a firm and a bank based on the bank's foreign funding exposure. Figure 2 shows pairwise correlations of the instrument Z_i with firm-level observable characteristics, conditional on the set of fixed effects that we include in the main empirical difference-in-differences specification. The instrument appears to be as good as randomly distributed conditional on most of the observables we consider (and the fixed effects in the regressions).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.[26,27]

4.2 Testing the channel

Before analyzing the impact of the credit shock on firm real outcomes, we show how accurate our proposed instrument is at characterizing exposure-level credit dynamics around the Lehman Brothers' failure.[28]

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,

[25]We extensively scrutinize BBS data before admitting banks to the estimation sample. For instance, we exclude all intra-group funding between subsidiaries of foreign banking groups, and harmonize the dataset to account for all bank mergers between 2003 and 2013. For confidentiality reasons, we cannot disclose any information on the position of any specific bank.

[26]Given that the years preceding the financial crisis were years of sustained credit growth, also fueled through interbank liquidity financing, it is not surprising to see a strong positive correlation between the indirect exposure of a firm to the foreign interbank market and credit growth until 2005. Even so, we control for pre-2005 credit growth in all specifications.

[27]In a recent influential paper on the identification properties of shift-share instruments, Borusyak et al. (2019) highlight that the exogeneity of the instrument could stem from the quasi-randomness of the underlying shifts with respect to firm characteristics, but does not per se stem from the overall shift-share structure. They show that the standard shift-share IVs can be implemented through a re-weighted shock-level specification. We show for completeness in Figure E.6 that the interbank exposures at bank level are not significantly correlated to weighted bank exposures to firm-level observables (where the weights are shares of bank-level short-term credit exposures with a firm). The absence of any significant correlation lends credibility to the assumption that our identification stems from shocks quasi-randomness. In concurrent work, Goldsmith-Pinkham et al. (2019) show that the identification can come from exogeneity in the shares, too. We present balance checks both for the bank level shocks and the full shift-share, in order to show that there is no evidence regarding sorting between banks and firm observables based on interbank exposure, according to the logic in Borusyak et al. (2019). Nonetheless, stability of semi-elasticities of credit to interbank exposure in Table 3 in section 4.2 indicate that observables, firm or match-specific characteristics do not significantly affect the transmission of the credit shock, which is consistent with the validity assumptions in Goldsmith-Pinkham et al. (2019).

[28]The structure of the CRC for the years of interest does not allow us to track loans over time, but more broadly defines firm-bank exposure types. We use the terms exposure and loan interchangeably, if not otherwise specified. We refer the reader to Appendix B for a description of exposure categories in the CRC.

we can be confident that any difference in firms' reaction to the shock is the by-product of the firms' own decisions. For this empirical analysis, which resembles the exposure-level first stage of the firm-level analyses that we perform in section 5, we run the following specification:

$$S_{i,b} = f(FD_b) + \mu_i + \varepsilon_{i,b}, \tag{3}$$

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 the endogenous treatment in Equation (1) but at the firm-bank exposure level, and $f(FD_b)$ represents different functions of bank foreign exposure. The definition of the outcome variable allows us to simultaneously consider extensive and intensive margins of the treatment effect. In the baseline specification we also add firm-level fixed effect 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 but we are only able to implement this exposure-level specification only on firms with multiple banking relationships (Khwaja and Mian, 2008).

Table 3 shows the results of multiple specifications testing the robustness of the exposure-level relationship.[29] 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.[30] Columns 2 and 3 show analogous results for different functions of the level of interbank exposure (firms above the mean or median banks' exposure). Given that one might be concerned about the effects of omitted variable bias, which might imply that the estimated effects are biased by unobservable firm-level characteristics of effects specific to the matching between firms and banks, we perform several robustness checks to show that the estimated effect is very stable and quite precisely estimated. In column 1 we report a bounding set to evaluate coefficient stability, following Oster (2019), which should give the reader an idea of how much one would expect the estimated coefficient to move because of the presence of match-specific unobservable influences. We use the results of the specification in column 1, which control for firm specific trends in short-term credit dynamics determined by unobservable characteristics through firm fixed-effects, as benchmark results, and compare them to results obtained when an analogous regression is run on the same sample with no controls at all. The bound between the estimated and the "bias-corrected" coefficients is tight and far from 0, which

[29]Borusyak et al. (2019) and Adão et al. (2019) recently expressed concerns on clustering standard errors in shift-share designs at the level of the unit of analysis (which would be the firm in our case), given that the variation in the treatment actually comes from "shifts" at a more aggregate level (in our case banks). To speak to that, we cluster standard errors by bank-by-industry pair, where the industry is defined as 3-digits CAE (Codificaçao de Actividades Económicas). The results are robust, if not more precise, to different clustering choices, namely straight industry clustering or double-clustering by industry and commuting zone. We maintain this choice of clustering throughout the analysis (we use main bank in firm-level estimations), and we always admit potentially different industry-related trends in credit and outcomes in all specifications. In results available upon request we found evidence that, controlling by bank specialization as in Paravisini et al. (2017), banks seemed to be supplying credit while discriminating across industries ex-ante, but not within.

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

is strongly reassuring.[31]

In order to show that the credit channel proxied by the interbank foreign funds exposure is not influenced by the dynamics of the sovereign debt crisis, we perform a robustness exercise in columns 4 and 5 in which we add controls for the exposure of banks to sovereign debt by the Portuguese government. In column 4 we control for the ratio of the average amount of sovereign debt on a bank's balance sheet over total assets in 2009, and in column 5 we control for the same measure from the last quarter of 2009, which is the period when the sovereign debt crisis dynamics started to unfold. Even if these controls are highly significant in these specifications at the exposure level, our estimated coefficients for the effect of exposure to foreign interbank funds remain stable and are not statistically distinguishable from the estimated coefficient in column 1.[32] As a further check, we test whether our instrument Z_i computed at the firm level as in Equation (2) predicts credit dynamics after 2010, after controlling for credit variation up to that year. We run the following regression at the firm level on the set of firms active in 2010:

$$\Delta D_{i;st,2013-2010} = \beta \Delta D_{i;st,2010-2006} + \gamma Z_i + \mathbf{\Gamma X_i} + \varepsilon_i. \tag{4}$$

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

Following Khwaja and Mian (2008) and Chodorow-Reich (2014) we perform further robustness tests in columns 6 to 9 of Table 3. By removing the firm level fixed effect and observing how much the semi-elasticity of credit to the bank shock varies across specifications, we can indirectly asses whether the match between firms and banks is not as good as random. This can be done if the firm characteristics affecting loan demand, which would normally be absorbed by the firm fixed effect, are additively separable in the empirical specification under consideration (Altonji et al., 2005; Khwaja and Mian, 2008). The variation in the estimate should proxy the amount of bias implied by the not as-good-as-random matching determined by firm-specific unobservable characteristics. In columns 6 and 7 we replicate the exposure-level regression with no controls, while in columns 8 and 9 we saturate the model with a series of fixed effects characterizing the firm operations, such as industry, location and other characteristics. In columns 7 and 9 we run the regressions on the full sample of firms that we use in the firm-level specifications in our sample of analysis, including firms with only a single bank relationship. The estimates are remarkably stable across specifications, with a range of variation for our preferred specifications

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

[32]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.

(columns 1 and 8) of around 2–3 percent of the base estimate of column 1. All estimates are statistically indistinguishable at standard confidence levels.[33]

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

5 Average firm-level results

In this section, we present average results on the effect of our identified credit shock on firms' real outcomes, employment, other forms of investment and adjustment of balance-sheet aggregates, and ultimately the decision to operate vis-à-vis exiting the market. We also highlight how firms present different propensities to adjust employment depending on worker characteristics, such as education, qualification, tenure within the firm and age. We provide evidence that firms appear to be constrained in their ability or willingness to adjust employment of their longer-tenured employees. This finding, together with the presence of substantial downward nominal-wage rigidity in Portugal (see Figure E.5) lends support to the likely presence of operating-leverage channel through financial frictions (Schoefer, 2015). We also show that firms' employment adjustment is stronger in industries with time lags between the moment when (labor) costs are paid out and the moment when revenues are realized and cash-flows are received, namely manufacturing. Employment adjustment is also concentrated on more skilled, specialized workers, who are likely to be more expensive to hire and train.. These findings point to the existence of a labor-as-investment channel, possibly coexisting with and complementing the labor-as-leverage channel.

5.1 Labor outcomes

Before analyzing the interaction of the effects of the credit shock on firm employment, financial outcomes and survival with the influence of labor rigidities, we show average results on employment to give a sense of the average magnitude of the dynamics caused by the credit shock. This exercise is useful both to validate our results and to present a dynamic event study showing the

[33]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. (2019). If credit exposure or other characteristics were making the distribution of shares not-exogenous, one would surely expect semi-elasticites of credit with respect to interbank exposure to visibly vary, which is never the case.

absence of a pre-trend in our employment dynamics, which lends credibility to our identification strategy.

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

$$log(Y_{i,t}) = \gamma_i + \tau_t + (\beta S_i + \mathbf{\Gamma X_{i,pre}}) \cdot \mathbb{1}\{t = Post\} + FE_{i,t} + \varepsilon_{i,t} \quad t \in \{Pre, Post\}. \tag{5}$$

In the specification, $Y_{i,t}$ is the average outcome variable in the period of consideration, γ_i is a firm fixed effect, τ_t is a time fixed effect, S_i is the treatment variable that we instrument in the 2SLS regression with the instrument Z_i, $X_{i,pre}$ are a set of out-of-sample controls at the firm level in 2005, and $FE_{i,t}$ is a further set of fixed effects by pre/post period. We interact controls with a dummy equal to 1 for the post-period years (from 2009 to 2013) to allow differential trends over the post-period (their baseline effect is captured by the firm fixed effect γ_i). Fixed effects in the pre-period are absorbed by the firm fixed effect, and thus their influence captures differential group-specific trends in the post period. Consistent with the loan level specifications in Table 3, we cluster the standard errors at the main bank-industry pair level.

Table 4 reports the results from estimating Equation (5) with the logarithm of the number of employees as an outcome variable, while Tables D.5 and D.6 in the Appendix show the first stage and the reduced-form estimates. The first column of Table 4 reports the results of a standard difference-in-differences with no additional controls. In the second column we add the set of fixed effects that we use in all regressions throughout the empirical analysis. In column 3 we add as controls all the variables for which the balance-check test fails (see Figure 2) and in column 4 we get to the main specification that we will use throughout the paper, with the full set of controls. Results in columns 3 and 4 show that the correlation of the instrument in 2005 with some observables does not have a relevant effect on the coefficient of interest. Given that we are averaging the outcome variable over a different number of observations in the post period due to firm exit or attrition from the QP, in column 5 we report the estimated coefficient over a sample of survivor firms. This last estimated coefficient more precisely characterizes the intensive margin of firm adjustment.[34] The first stage effective F-statistic is always above 30 and not far from the 5% Nagar bias thresholds (which is at 37.42 according to the methodology in Montiel Olea and Pflueger, 2013), showing that the instrument very strongly predicts the variation in credit.[35]

Our preferred estimates range from 0.071 (column 4, full sample) to 0.086 (column 5, survivors). To add context to the magnitude of our estimates, in the post-period the predicted treatment after the first stage regression has an average of -0.183 (median -0.204), a standard deviation of 0.565 and a 10–90 percentile range of 1.532.[36] A one negative standard-deviation

[34]Given that we do not add zeros to the panel to balance it, estimation on the full sample does not simultaneously capture extensive and intensive margins adjustment. At the same time, estimating the elasticity on the sample of survivors is only potentially subject to bias due to self-selection into survival.

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

[36]Recall that the baseline growth rate takes values between -2 and 2, and nothing prevents the treatment predicted by the first stage from taking values outside this domain.

variation in the treatment would decrease the average firm employment by approximately 4 percentage (log) points (4.9 percentage points according to the survivors' estimate). Given that the average employment variation in our sample is -0.044 (median -0.040) and the standard deviation in employment is 0.288, the shock has significant economic size. The economic impact is even more prominent, given that in the later years in our sample Portgual was suffering form the EU sovereign debt crisis. The debt crisis dynamics may have been correlated with our shock, but they are unlikely to have been predominately determined by it.[37] One standard deviation in our shock explains between 14 and 17 percent of the standard deviation in employment. Even though our estimated elasticity is approximately between one quarter and one sixth of the estimated corresponding values in other studies (Bentolila et al., 2017; Berton et al., 2018b), the amount of variation explained by the shock is comparable.[38]

Table 5 shows the estimates of the elasticity of the wage bill (either full or base wage) in specifications where we control for the full set of controls and fixed effects, and consider both the full sample and the survivors. The estimates have a similar degree of precision as the employment ones, and the wage bill appears to have a higher elasticity to the shock with respect to employment. This might indicate that wages were being cut, or that there were compositional effects in firing/hiring. The flexible components of pay do not display a different volatility to the shock compared to base pay, indicating that firms cannot cut extra compensation more easily than base wages. This finding is confirmed by results in Table D.7, which shows that estimated elasticities of work hours to the shock are almost identical to the elasticities of employment. Moreover, elasticities are indistinguishable between the cases referring to total hours worked and the ones referring to normal hours (with no extra-ordinary or over-time hours). In the last two columns in Table 5, we show the results of estimating a euro-to-euro sensitivity of payroll with respect to the cash-flow shock generated by the credit-supply variation. We scale the level variation in salaries between the pre- and post-periods and the variation in credit at the numerator of S_i in Equation (1) by the pre-period average level of sales. The estimated euro-to-euro sensitivity is 0.17 for the full sample and 0.23 for the survivor sample, which should deliver more precise estimates for the wage bill and employment for the entirety of the post-period. These values are close to standard values estimated in the literature.[39]

In Table 6 we show the results of estimating our difference-in-differences specifications only on the firms in manufacturing. For most production activities labor is not immediately paid on the moment in which a revenue is generated, and this timing mismatch is particularly strong in manufacturing industries. Consistently with this reasoning, the estimated employment and wage

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

[38]The corresponding share of one standard-deviation variation in employment explained by the shock in Berton et al. (2018b) is 18 percent. However, in our study credit is much more volatile than in the other cited studies, which might imply that the difference in the estimated elasticities could be a by-product of the non-linear relationship between the credit-supply variation and the real variables, and the fact that short-term credit is likely much more volatile than total credit. Interestingly, the reduced-form estimates are quite consistent with similar analyses in a similar period (Cingano et al., 2016).

[39]Schoefer (2015) provides a review of the values of the cash-flow dollar-to-dollar sensitivity estimated in the literature. Plausible estimates range from 0.2 to 0.6, and he calibrates his model for the US economy to obtain a 0.25 sensitivity, close to our intensive margin estimate. In contemporaneous works on how exchange rate shocks affect French firms, Barbiero (2019) estimate cash-flow sensitivities between 0.2 and 0.3.

bill elasticities are highly significant, between 60 and above 80 percent higher, and estimated with a very similar degree of precision in the manufacturing sector despite the fact that the sample of analysis is approximately half the size of the full sample. The results are a clear sign of the existence of a labor-as-investment channel in our sample, at least for manufacturing firms.

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

$$Y_{i,t} = \gamma_i + \tau_t + \sum_{k \neq 2008} (\beta_k S_i + \mathbf{\Gamma_k X_{i,pre}}) \cdot \mathbb{1}\{t = k\} + FE_{i,t} + \varepsilon_{i,t} \tag{6}$$

where a different treatment coefficient is estimated for each year k. We normalize the treatment to be 0 in 2008, so that all the other treatment coefficients in the regressions can be interpreted as variation in the outcome with respect to its level in 2008. In this specifications the outcome variables are always expressed as ratios of the level of the outcome over its average in the pre-period. This means that the regressions are performed on the percentage change with respect to the average pre-period level of the outcome.[40] We run these event-study regressions on survivor firms only, whom we identify through the CB.

As evident from Figure 3, the treatment does not show pre-trends. Moreover, it has persistent effects that accumulate over time, weakly waning only in 2013. Interestingly, the only year in which the point estimates for employment and wage bill are clearly different is 2010, when Portugal briefly exited the recession and the EU sovereign debt crisis dynamics had not fully materialized. This is the only year in our sample in which we have some weak evidence that firms adjusted employment through temporary contracts, and it is plausible that some of the terminated workers were substituted with new hires at lower wages. These dynamics would explain why, despite wage rigidity, we observe a higher elasticity of the wage bill with respect to employment.

Figures 4a, 4b and E.8 show that firms are constrained in how they adjust to the credit shock, and tend to concentrate their employment and wage-bill adjustments on the least attached workers, possibly as a consequence of wage rigidity. The results highlight a pecking order of firm adjustment along the extensive margin similar to a LIFO (last-in-first-out) logic, where less protected workers are disproportionately impacted. However, the evidence is inconclusive on whether the results are determined by firing costs increasing with tenure (Lindbeck and Snower, 1986), the presence of a dual labor market (Boeri and Jimeno, 2016) or the firms' revealed-preference unwillingness to separate from their most skilled and expert workers (Jovanovic, 1979a,b), consistent with the view that labor is a costly investment good.

In these dynamic specifications, we regress the ratio of each category of workers (attached incumbents and net hires) over the pre-period level of employment (or analogously their wage bill over the average pre-period wage bill), and plot the series of estimated coefficients along

[40]This specification is very convenient for the dynamic model, as it allows to graphically depict how the treatment effect is subdivided across subgroups of workers, which we are going to exploit to show tenure dynamics within the firm. Running this specification does not present particular problems in terms of handling outliers. We present in Appendix Table D.8 the results of the difference-in-differences in specification (5) run on these outcome variables, and show that they are closely consistent with the log specification.

with the overall total employment and wage-bill effect. We define attached incumbents as the workers present in the firm for the entirety of the pre-period, whereas all the other workers are either less attached workers (present in the firm for less than three years, or discontinuously present in the firm) or net hires throughout the period. Given that each worker is assigned to a category, their estimated coefficients must by construction sum up to the total effect. Therefore, we are not estimating the own-elasticity of employment for each kind of worker, but we are allocating the treatment to the two different categories. By dividing the estimated share of treatment going to a category by the share that it represents in the pre-period, one can indirectly obtain the implied elasticity. If the share of apportioned treatment corresponded closely to the pre-period share represented by the category, one could conclude that the workers' category exhibits the same elasticity of employment adjustment as for the average employee.[41] It is important to keep in mind that the attached incumbents represent approximately 70 percent of employment in the pre-period, and they are always at least double the size of the less-attached workers. Figures 4a, 4b and E.8 provide evidence that that firms pass the treatment effect to workers' employment and labor costs asymmetrically. Attached workers, less attached workers and net hires have very different implied elasticities of employment and compensation: only a small fraction of the treatment impacts attached workers, whereas less attached workers and net hires seem to bear the brunt, if not almost the entirety, of the treatment effect, coherently for instance with the logic of results in Buhai et al. (2014). The evidence on the different adjustment channels depending on attachment and tenure within the firm indirectly supports the existence of a hiring effect through labor-as-investment but, perhaps more relevantly, also supports the argument that incumbents' wages and overall labor costs constrain firms' choices in reaction to shocks, consistent with labor-as-leverage. In fact, the higher implied elasticity for the less attached workers with respect to attached ones implies that separations take place as a consequence of the shock, and these separations are more common precisely for workers whose separation is plausibly cheaper.[42] In other words, incumbents' costs act as a sluggish operating-leverage factor to which firms do not easily adjust, which makes the most attached workers in the firm a quasi-fixed factor (Oi, 1962).

In Tables 7, 8, D.9 and D.10 we calculate the elasticities of employment to the credit shock for subgroups of workers based on their qualification within the firm, age cohort in 2008, contract type and education level.[43] In these regressions the dependent variable is the ratio of the number of employees in a specific category over their average number in the pre-period. This effectively amounts to performing the regressions on the percentage variation between the pre- and post-periods.[44] We define the categories as follows: for qualifications, we subdivide the

[41]See for instance Berton et al. (2018b) for a similar decomposition methodology for different types of workers.

[42]Table D.13 in the Appendix reports elasticities of the employment of attached and less attached workers through the difference-in-differences specification analogous to the one in Equation (5). The estimated elasticity for the less attached workers is more than double the elasticity for attached workers.

[43]Additional results on heterogeneous effects for workers in different kind of occupations (routine vs. non-routine, manual vs. cognitive) along the lines of Autor et al. (2003) and Autor and Dorn (2013) are available upon request.

[44]We use this particular measure because it allows us to admit 0 values in the post-period, as the measure is defined only if a firm has a positive number of workers in the firm of the specific category in the pre-period. The dependent variable is always winsorized at the top 1-percent level. We prefer this measure to the $asinh()$ or the $log(1+x)$, which are very similar in practice, because in many cases we are dealing with small numbers of workers, hence adding one "fictitious" worker to the estimation could make a material difference in the estimated

9-level hierarchy defined by the Portuguese law into managers, specialized workers and skilled professionals or semi-skilled and unskilled professionals and trainees; for age cohorts, we define young workers as those younger than 30 in 2008, prime workers between 31 and 55, and old workers between 56 and 65; for contracts, we distinguish between workers with permanent or fixed-term contracts; finally, for education, we define low educated workers as those who did not complete a high school or equivalent schooling level, as medium educated those who did not get any undergraduate degree or specialized secondary education, and as high educated those with any higher level of schooling.

The results show high and significant elasticities for specialized workers/team-leaders, and high but noisier estimates for low-skilled generic workers. Sforza (2018) argues that the observed adjustment dynamics are likely related to the complementarity of specialized workers with some specific forms of capital, such as specific equipment, that are directly affected by the credit shock. He argues that these workers are a layer in firms' organization, that would likely be cut as a consequence of a credit shock downsizing the firm. As shown in section 6.1, these workers seem to be relatively prevalent in more labor-intensive firms, featuring a more specialized and highly paid workforce. Thus, part of the higher elasticity of employment that we detect in relation to the credit shock is arguably related to the implicit constraint that their compensation imposes on these firms' liquidity management. More importantly, the hiring and training of these kinds of workers is likely to entail greater costs. As a consequence, firms are likely to consider these workers investment goods. If firms pay wages and employment costs out of short-term loans or credit lines, a maturity mismatch in their financing can generate an implicit exposure to cash-flow shocks.

In line with the previous tenure-related findings (Figures 4a and E.8), we estimate the highest elasticity of employment for young workers, and non significant elasticities with respect to older workers. This finding is particularly relevant for the long-term effects on growth and productivity, if human-capital investment requires time and there are persistent "scarring" effects on workers belonging to cohorts that were at some point in time highly exposed to unemployment and recessions.[45] Regarding contracts, temporary workers have a higher estimated elasticity of employment than permanent workers, but the estimates are noisy and not significant.[46] Finally, regarding education, we find that the credit sock effects, coherently with the previous results,

coefficients.

[45]In a recent paper Caggese et al. (2019) show similar results, both theoretically and empirically, and argue that financial frictions and adjustments costs in hiring and firing distort the firing choices of firms. We plan to analyze the possible long-term adverse effects on productivity and growth determined by these dynamics in the future research.

[46]A common anecdotal explanation for the fact that the estimates of the elasticity of temporary employment are not statistically different from 0 is that, during the crisis and especially from 2011 onwards, the Portuguese government enacted some ad-hoc measures to allow firms to retain temporary workers beyond their contractual duration. Given that these workers are in general paid less than permanent workers, many firms actually exploited this concession to retain them at lower relative cost (See OECD (2017), page 33). Another candidate explanation might be that firms performed a large part of the replacement hires that they could afford with temporary contracts instead of permanent ones, and given that we only see a snapshot of firm employment every year in October the overall numbers do not vary much. Finally, the reforms to the Employment Protection Legislation following the Memorandum of Understanding with the International Monetary Fund led to a reduction in the costs related to permanent contracts vis-à-vis temporary ones. For more details on the Portuguese labor laws and the reforms during the EU sovereign debt crisis we refer the reader to OECD (2017).

are concentrated on medium education workers.[47,48]

5.2 Firm exit

In order to analyze firm survival we estimate a linear probability model on the probability of exiting the market as a consequence of our instrumented treatment. We use the CB for all the firms in which the merged QP-CB exists in order to identify firm exit (as explained in Appendix B.1.2).

We run the following linear probability model specification:

$$P(exit_{i,t}) = \tau_t + \beta S_i + \mathbf{\Gamma X_{i,pre}} + FE_{i,t} + \varepsilon_{i,t}, \tag{7}$$

where the outcome variable, defined at the yearly level, is a dummy variable equal to 1 if a firm exited in any year between 2009 and 2013, and τ_t is a year fixed effect. Table 9 shows estimates of Equation (7). The fixed effects are the same as for the empirical specifications in Table 4, interacted with yearly dummies and controls fixed at their 2005 levels (not interacted with year dummies). The set of controls $\mathbf{X_{i,pre}}$ is the same as for the specifications in Table 4, but here we add to the controls the share of credit that a firm gets from micro-banks (i.e. excluding the the 10 largest banks) and the share of credit that the firm is getting from the banks failing before 2014, as we try to control indirectly for the unobservable characteristics related to these kinds of matching.[49] In column 2 we show results on the restricted sample for which we can reliably calculate the TFP.[50]

The credit shock has a substantial impact on the chances of firm survival, especially in the sub-sample of firms for which we have enough data to estimate productivity. According to the estimates, a one-percent standard-deviation drop in the predicted treatment would increase the probability of firm exit between 0.63 and 0.76 percentage points per year, against an average exit rate of approximately 5 percent. The difference in the likelihood of firm exit for a firm exposed to the 10th percentile of treatment as opposed to the 90th would be between 1.6 and 1.9 percentage points per year. Interestingly, the estimated coefficient for TFP is very close to 0 and not significant: high productivity three years before the crisis does not seem to unconditionally protect firms from the shock, despite the fact that TFP is very persistent over time.[51] The estimated effect, far from having a "scarring effect", does not seem to be "cleansing" on average either.

[47]Several workers carrying out non-trivial specialized tasks, especially if older in the workforce, would not possess an advanced schooling degree, despite the recent progresses in Portugal's average level of schooling (Cardoso et al., 2018).

[48]In Appendix Figures E.9 and E.10 we report the breakdown, in a dynamic event study specification analogous to the ones underlying Figures 4a and E.8, of the treatment effects across workers with different qualification levels and different ages respectively.

[49]For confidentiality reasons, we are not allowed to discuss the position and role of any specific bank, firm and in general any unit of observation in the empirical specifications.

[50]Our preferred TFP specification follows the estimation technique by De Loecker and Warzynski (2012) and Ackerberg et al. (2015), also used on the QP by Caliendo et al. (2016). For further details about the estimation techniques we refer the reader to the aforementioned papers and to Appendix C.

[51]The correlation coefficient between TFP in 2008 and 2005 is between 0.9 and 0.97 depending on the different TFP specifications.

5.3 Financial outcomes

In this section, we document the effects of the credit shock on firms adjustment over balance-sheet variables. We focus on short-term credit and identify a liquidity shock that impairs the short-term liquid finances of a firm and thus its ability to manage its day-to-day activities. We argued that the financing of the so-called "working capital" of the firm is most likely to be affected by the dynamics of short-term credit supply to the firm. In contrast, one would not expect fixed capital investment to be directly affected by this type of shock, as such kind this investment is the result of advanced planning by the firm, and it is probably financed through long-term credit. Any effect on capital investment, in this setting, should come as a result of complementarity of production function inputs.

Table 10 shows the results of estimating Equation (5) on balance-sheet and other financial variables for our sample of survivor firms. The outcome variables are total assets, fixed assets (sum of tangible and intangible assets) and current assets, cash, sales, trade credits and debts to suppliers. We take the logarithm of the variables with only positive support, and the asinh of the variables that can take negative values, because defined in net terms. Total assets appear strongly responsive to the shock, with an estimated coefficient of 0.098, which has similar magnitude as the employment and wage bills coefficients. When we break down the effects by fixed and current assets, we see that the result is entirely driven by current assets, whereas the elasticity of fixed assets is not significantly different from 0, despite the fact that its magnitude is quite comparable to the employment estimate. In a similar fashion, we estimate a sizable and significant (at the 10-percent level) elasticity of trade credits to the credit shock, possibly indicating that negatively hit firms ran down their existing trade credits over time while positively hit firms were willing to let their trade credits stock grow vis-à-vis their customers. We do not identify a significant elasticity for sales, cash or debt with respect to suppliers, possibly indicating that this alternative means of extracting liquidity from suppliers up the production chain was not readily available to affected firms.

Our results show that fixed assets growth, i.e. capital investment, is not significantly affected by the shock on average. This average result is reasonable if long-term credit is the relevant variable for predicting an effect on capital investment. To verify this, we use an empirical strategy similar to the one in Almeida et al. (2011). Given that from 2009 onwards the CRC features more precise measures of a loan's original and residual maturity, we measure the value of long-term credit maturing for a firm in the first two quarters of 2009. Then, we rerun the difference-in-differences model of Equation (5) adding as further control the dummy exp_lt_i, which is equal to 1 if a firm has more than 20 percent of its long-term credit maturing in that period (the same threshold used in Almeida et al. (2011), corresponding roughly to the 75th percentile in the distribution of the variable), and estimate the following specification:

$$\begin{aligned} log(Y_{i,t}) = \gamma_i + \tau_t + (\beta S_i + \lambda exp\_lt_i + \delta S_i \cdot exp\_lt_i + \mathbf{\Gamma X_{i,pre}}) \cdot \mathbb{1}\{t = Post\} + \\ + FE_{i,t} + \varepsilon_{i,t} \quad t \in \{Pre, Post\}, \end{aligned} \tag{8}$$

in which the dummy is added as a control and interacted with our treatment (which implies that the interaction is also instrumented by the interaction of the dummy with the instrument).

We report our results in Table D.11. The credit shock has no significant effect on capital investment on average, even when the firm has a large portion of its long-term credit maturing and it is hard to roll it over with banks. This considerable share of long-term credit maturing for the firm has a strong significant effect on capital investment, as any firm in this category experiences between 5.75 and 6.13 percentage (log) point smaller increase in fixed assets over the period. These results support our hypothesis that the credit shock is predominantly a liquidity shock, and firms' longer term investment decisions do not seem to depend on short-term liquid sources of financing (at least as long as the shock does not impair their overall operations in a definitive way and/or the firm is strongly exposed to liquidity risk though labor costs, as we show in section 6.1).[52] It is thus sensible to consider inputs that need financing on a more frequent basis and by means of short-term liquidity for our analysis on firms adjustment to the shock.

6 Heterogeneity results

In this section, consistent with the conceptual framework in section 2, we present heterogeneous results showing how labor rigidities affect the propagation of the credit shock and amplify its effects. The first section focuses on labor rigidities, how we define them and which frictions underlie them and their financial channels. We then show that the propagation of the shock through labor rigidities takes place regardless of firms' productivity levels. The dynamics of the shock thus appear to have a "non-cleansing" connotation, in that the shock affects high- and low-productive firms similarly, depending on the degree to which they are subject to labor rigidities.

6.1 The role of labor rigidities

Our goal is to show that the empirical results of the previous sections are driven by rigidities related to labor financing and firms' inability to adjust their workforce in response to unexpected credit shocks. Consistent with recent research in labor and finance, we argue that the rigidities that we aim to identify can generate real effects through firms' financing channels.

We present evidence on the overall influence of labor rigidities on the propagation of the credit shock across firms and the relevance of its effects. Controlling for how much labor compensation matters in the cost structure of the firm is likely to proxy for both how much a firm needs to finance labor as an investment good and how much it is likely to be constrained by rigidity in labor adjustments and costs in case of a sudden liquidity drought.

Our results are based on the variation of the labor share at the firm level that is determined by the variation, size and generosity of wage and compensation policies intrinsic in the firm economic activity. We measure the degree to which labor costs constrain cost-structure adjustments by ranking firms by their labor shares, i.e. the share of employment costs in value added.

[52] In Section 6.1 we find that firms more subject to labor rigidities, for which we indeed detect significant elasticities of capital investment adjustment, are also firms that have a relatively higher share of short-term financing to start with. Consequently, the average effect on capital investment is also plausibly a by-product of compositional differences in financing across firms.

The ideal experiment is to identify the effect of a difference in the relevance of labor and labor costs in the firm cost structure for otherwise observationally similar firms. For instance, even within the same industry, firms producing different varieties of the same product which require a different input mix can end up having similar degrees of productivity and profitability, but a different reliance on labor costs in production. Given that we argue that labor costs would in many instances need to be financed with external liquidity, and would also be partially rigid to adjust in the short-term, this variation in the cost structure would capture exactly the channels we describe in section 2.

Different factors might determine a firm's labor share: for instance, observing a high labor share might indicate that a firm is very inefficient at using its labor (that is, it has a very low value added per employee), that it is temporarily unproductive (value added is low because of a temporary shock) or that, given a certain level of labor productivity, it has a cost structure that compensates labor more than other factors of production. The source of variation we want to isolate pertains to this last component of labor share characterizing the firm overall compensation policy, which likely changes slowly over time. We address this point and employ two strategies to isolate a source of variation in the generosity of compensation and the relative importance of labor costs for the cost structure of the firm: either we control for the level of value added per employee in our main specifications or, in robustness exercises, we first regress the labor share on value added per employee and then use the residuals as a counterfactual labor share based only on the wage-policy component.[53] Moreover, we control for the labor share by using its average level between 2005 and 2006 in base results, but confirm the results in specifications with average labor share for the entire pre-period or just 2007 and 2008. Given that the robustness exercises always qualitatively confirm our findings we are less worried that the base results are just driven by temporary shocks in productivity or profitability, or by persistently low productivity. As a further robustness check, we compute labor shares in two ways: by considering total costs related to labor as reported in the CB in our main specification, or just wages from QP. As a final robustness check, given that utilization of intermediate inputs might be an additional choice affecting firms' ability to adjust the cost structure in response to unexpected shocks, we also report results with the labor share computed as the ratio of labor costs to total sales.

We partition firms by labor-share quantiles, and perform regressions measuring a different coefficient for the credit shock for each quantile.[54] 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, but the results are qualitatively unchanged if we control with a polynomial. The specification for exit is:

$$P(exit)_{i,t} = \tau_t + \sum_{k=1}^{n} \beta_k S_i \cdot \mathbb{1}\{LabSh_{bin} = k\} + \mathbf{\Gamma X_{i,pre}} + FE_{i,t} + \varepsilon_{i,t}, \tag{9}$$

[53]Some recent studies (Schoefer, 2015; Donangelo et al., 2019; Favilukis et al., 2019) highlight the importance of labor share as an indicator of the degree to which labor costs inflexibility constrain firms' operations, especially in presence of wage rigidity.

[54]The results are the same regardless of whether we rank firms within sector. For the reported results we did not rank within sector. All regressions feature an industry fixed effect, so all results should be interpreted as within industry.

while the specification for employment is:

$$log(Y_{i,t}) = \gamma_i + \tau_t + \left(\sum_{k=1}^{n} \beta_k S_i \cdot \mathbb{1}\{LabSh_{bin} = k\} + \mathbf{\Gamma X_{i,pre}} \right) \cdot \mathbb{1}\{t = Post\} + \\ + FE_{i,t} + \varepsilon_{i,t} \quad t \in \{Pre, Post\}, \tag{10}$$

where $n \in \{4, 7\}$ depending on the specification.

Figures 5a and 5b (for 7 quantiles) and Figures E.13a and E.13b (for quartiles) show estimates for the baseline specifications of labor share, for both exit and employment. Both the estimated elasticity of employment and the effect on the probability of firm exit as a consequence of a (negative) credit shock are almost monotonically increasing across the labor-share bins, as labor-rigidity channels would imply. 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). There is substantial heterogeneity across bins, with the lowest labor-share bins virtually unaffected by the shock. Consistent with the results in the previous section and our conceptual framework in section 2, these results confirm 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 leverage/risk factor for the firms as regards total employment and firm survival. As a consequence, firms with high labor shares experience a relatively more volatile employment adjustment and a greater likelihood to fail following a negative credit shock. The estimated coefficients for most affected firms have substantial economic relevance, with the magnitude approximately 33 percent greater than the average or more for employment, and more than 2 times larger for exit for the most extreme labor-share bins. In Appendix Figures from E.11 through E.16b, we report the employment specification of the "survivors" sample and the reduced forms and robustness checks with alternative measures of the labor share (residual from the regression of labor share on value added per employee, alternative definition with wage bill only and labor share in sales). The results are qualitatively coherent and quantitatively very close to the main ones across all specifications.

In order for the exercise to make empirical sense, we also need the labor share to be a firm's persistent feature. The correlation between the average labor share between 2007 and 2008 and the average lavor share between 2005 and 2006, which we use in the main specifications, is higher than 0.7, and we should actually expect the compensation component to be more persistent than the productivity component. As a robustness check we show in Figures E.17a and E.17b the results obtained by estimating the exit and employment specifications on quartiles of average labor share in 2007 and 2008. The results are very similar and confirm the findings in our main specifications, validating our results and indicating that labor share is a slowly changing firm characteristic. Nonetheless, we prefer not to use this as our main specification because the evolution of labor share during the pre-period might be endogenous to firms' contemporaneous

outcomes, or to the possible anticipation of idiosyncratic outcomes, which might be correlated with having obtained credit from banks through the 2005 sorting process.

In Figures 6a through 6c we report the results of running the specification on quartiles of financial variables, in the sample of firms not exiting the market. There is a very strong similarity in how total assets, current assets and sales respond to the shock with respect to the employment specification. Even in these cases, there is an upward trend in the estimated elasticities with respect to the labor-share quartiles, and the magnitudes of amplification of the elasticities for the highest labor share bins are approximately double the average estimated effect. Regarding fixed assets, we showed in section 5.3 that we do not find any significance on average, and that fixed capital investment is much more reactive to shocks to long-term credit supply. Nonetheless, comparing estimated elasticities between the two lowest and the two highest quartiles, elasticities in higher quartiles are generally larger, possibly indicating that there are complementarities in the adjustment of employment and fixed capital investment for the most exposed firms.[55] We observe similar, albeit less significant, results and trends regarding specification with total sales as the dependent variable.

In this section we showed that the share of (arguably inflexible) labor costs in the firm cost structure amplifies the effect of a shock to short-term credit, which firms use to finance current operations, such as compensating labor, on real outcomes. We now dissect the financial channel of labor rigidities into the two main channels underlying it, the labor-as-leverage and the labor-as-investment channels.

6.1.1 Labor as leverage, labor as investment

From the previous analyses we can see that the variation in labor shares determined by compensation policies can both create an alternative source of leverage for firms and expose them to mismatches in the timing of cash-flow receipt and payouts. The evidence implicitly shows that firms do not sufficiently correct for these time and maturity mismatches, as many firms finance their current expenses with short-term liquidity even when these expenses underlie longer-term predictable liquidity needs, which is surely the case for some kinds of labor. Thus, inflexible labor costs can be a strong source of financial frictions during a liquidity shock.

There is likely no clear-cut way to definitely separate the two channels that give rise to these results in our empirical setting, because they likely support and reinforce each other.[56]

[55] In a recent study Fonseca and Van Doornik (2019) analyze the adjustment of both labor and fixed-capital investment following a reform of the bankruptcy law in Brazil, which effectively increased recovery rates from bankruptcy for lenders and eased firms' financial constraints. They show how capital investment and high-skill employment react to exogenous variations in credit availability by virtue of their complementarity in production. If a firm does not finance labor through credit, but is subject to a borrowing constraint on financing capital, the effects that of a credit shock would directly impact capital investment at first, and then by the complementarity of production inputs it would affect employment decisions as well. Importantly, Fonseca and Van Doornik (2019) argue that their findings are consistent with our financial channels, which they cannot set apart from their proposed complementary channel (page 3, footnote 2). Our results seem to confirm that the channel identified by Fonseca and Van Doornik (2019) is in place, but also suggest that whether a firm is hit by a shock or not crucially depends on the prevalent means of financing. In other words, while we do not rule out this channel, we argue that many features in our results point to the existence of labor-as-investment and labor-as-leverage.

[56] The model in Appendix A also conveys this intuition, as it highlights how the exposure to a working-capital channel through labor-as-investment also gives rise to labor-as-leverage if incumbents' salaries must be paid in

Nevertheless, we can show that numerous empirical findings and results in our data point to their existence.

First of all, there is abundant descriptive evidence supporting our hypotheses on the financial channels of labor rigidities. As reported in Figure E.5, Portugal seems to be subject to a considerable degree of nominal wage rigidity, as demonstrated by the observed spike in the share of contracts from 2009 onwards that are subject to wage freezes (and not cuts). As previously stated, the Portuguese labor market is generally considered as one of the most inflexible among advanced economies in terms of rigidity of contractual agreements and severance costs, as shown for instance by Botero et al. (2004) or by indicators of the strictness of Employment Protection Legislation (EPL), such as those by the OECD or the ILO.

Furthermore, we regress some 2005 or full pre-period observables on the full set of controls and fixed effects in the main empirical specifications plus labor share, and analyze which of these characteristics are strongly correlated with the average labor share after controlling for the average value added per employee for 2005 and 2006. This exercise does not identify any inherently causal relationship, but it can indicate what firms' characteristics correlate with the effects that we estimate, or which stylized facts might explain them. Table 11 displays the signs of the correlations for these regressions, along with the significance level of the statistical estimate.

Firms with high labor shares are similar in terms of employment size to the most productive firms with low labor shares, but have a lower level of total assets. In regressions where we do not control for the average 2005 wages, which are strongly persistent, the labor share is strongly positively correlated with the firm fixed effect estimated through an AKM model (Abowd et al., 1999) on the full QP dataset for pre-period years, even after controlling for labor force composition (share of managers, specialized workers). The labor share is almost by definition positively correlated with average wages: firms with high labor shares (especially if productive) seem to be paying their workers more, even after conditioning on workers' ability (share of highly educated or highly qualified workers). These correlations are consistent with the existence of labor-as-leverage, possibly correlated with the labor-as-investment channel, but likely going beyond it. In fact, it appears plausible that relatively more labor-intensive firms actively use their compensation policies as a means of attracting qualified workers. However, given labor costs rigidity, these same compensation policies likely constrain firms' use of internal funds, especially when liquidity is scarce. We also observe that the shares of older workers in the firm, of managers, of specialized workers and highly educated workers are strongly positively correlated with the labor share. Consistent with this finding, firm-level median tenure of permanent workers positively correlates with labor share. This fact indicates that, despite a firm's workforce being more inflexible at high labor shares, the preserved matches might be of higher quality (Jovanovic, 1979a). We also observe a negative correlation with the share of temporary workers, who might be used by firms to alleviate the influence of labor-as-leverage.

We might also expect a negative correlation between measures of financial leverage and the component of labor share representing generosity of compensation if the labor-as-leverage

advance and are subject to some form of rigidity. In contrast, labor-as-leverage does not necessarily require a labor-as-investment channel to exist, as discussed in Section 2, but is plausibly determined by it in practice.

channel is active (Simintzi et al., 2015; Matsa, 2019; Favilukis et al., 2019). Indeed this correlation is present in the data, which supports the view that firms realize that labor costs can generate operating leverage, which needs to be traded off against financial leverage. Given the identified results on the credit shock, it is also evident that firms do not sufficiently take the necessary steps to isolate themselves from cash-flow shocks. We do not find evidence of greater collateralization in short-term credit (see Table D.2), which is the prevalent form of financing for those firms with a highly inflexible cost structure related to labor costs, and we actually find lower cash holdings per employee. To further show that our results are not consistent with a standard amplification through financial leverage, we show in Appendix Figures E.18a and E.18b that performing regressions by financial-leverage bins instead of labor-share ones does not reveal any clear trend or relationship between the response to the shock and financial leverage. The estimated coefficients are mostly flat, and noisily estimated. Therefore, despite this negative correlation with financial leverage, our results show that the rigidity determining the labor-as-leverage channel represents a risk factor for firms' operations and survival that is distinct from the standard financial-leverage channel. Our results complement the recent results in Favilukis et al. (2019). We find that after controlling for measures of technical and labor productivity, the variation in labor share determined by the generosity in labor compensation policies is strongly positively correlated with measures of risk, such as the default probabilities for Portuguese firms estimated by Antunes et al. (2016). This correlation does not stem from the labor productivity component of labor share, which is independently accounted for.

The results in section 5 implicitly support the existence of the two channels of labor-as-leverage and labor-as-investment. We see that most of the employment adjustment takes place with respect to specialized workers (Table 7), whose abundance is heavily correlated with a firm's exposure to rigid labor costs.

The results in Figures 4a, 4b and E.8, and Tables 8 and D.13, are also consistent with rigidities in firm adjustment with respect to incumbents, which might be the result of firing costs that are increasing in tenure, as surely is the case in Portugal, or by the reluctance of firms to part away with employees with greater general and firm-specific human capital (Jovanovic, 1979a), as one would expect if labor is a long-term investment. It is important to stress two points. First, the higher employment elasticities of the less-attached workers imply that separation as a consequence of the shock is possible, which reinforces the intuition that the employment effect is at least partially determined by the leverage channel, and not just the investment channel, which should primarily impact hiring. Second, the insiders/outsiders dynamics (Lindbeck and Snower, 1986) that we observe towards young workers is very likely to have implications for the long-run trajectory of labor productivity and the stock of human capital in the economy.

Additional results partially support the two channels as well. Table D.12 reports the results of running our difference-in-differences regressions on average wages at the firm level, either for all employees or for the remaining incumbents from 2008. We do not find any evidence of meaningful adjustments in the average wages in either case, as one would expect if wages were rigid.[57]

[57]This piece of evidence is not conclusive, as any significant effect might indicate that the adjustment took

Finally, in section 5 we showed stronger effects in manufacturing industries, which is consistent with the labor-as-investment channel. We perform a further check to identify effects plausibly driven by labor-as-investment. We verify that, as the existence of the channel would imply, we should measure stronger employment effects in firms that employ workers with greater hiring and training costs. We exploit the detailed information available at the worker level in the QP. We match profession definitions for each worker in the QP to profession definitions and characteristics in the Occupational Information dataset O*NET. This is a widely used dataset in labor economics that categorizes professions according to different criteria. For instance, it has information on the educational requirements and training necessary to master a task, and the complexity of performing it. We use scores from the "education, training and experience" tables, and more specifically the "on-the-job training required" (otj) score, to classify each worker by the amount of on-the-job training that he or she is supposed to require given the job that he or she performs. We aggregate these scores at the firm level taking averages of the scores for each employee, and partition the firms in quartiles based on the scores in 2005. Then, we run our difference-in-differences specifications for employment and firm survival, estimating a different coefficient for each quartile. Figures 7a and 7b show the results of these estimations. Consistent with the labor-as-investment channel, we find greater elasticities of employment to credit in firms that employ workers with higher training costs, as proxied by the average otj score, and a correspondingly stronger effect on the probability of exiting for those firms, even if the results for exit are noisier and do not allow to detect statistically significant differences across coefficients of interest.

In Table 11, we report the significance of correlations of the O*NET variables with the portion of variation in the labor share determined by workers' compensation. We consider the firm-level otj score, and the "zone" score, which is a score provided by O*NET itself that serves as a summary index for all the scores in the "education, training and experience" tables scores, namely required education, required experience, amount of training on-site and the otj score itself. We find very strong positive correlations of these measures with our measure of cost rigidity through labor costs.[58]

The fact that our results are determined by both investment and leverage effects lends credibility to the hypothesis that labor-as-investment can over time create labor-as-leverage. By accumulating skills, workers become a source of high and rigid costs for the firm. On top of that, because of a possibly long training process for replacements, workers become quasi-fixed inputs from which firms do not want to separate (Oi, 1962). This is a way in which labor-as-investment plausibly turns into labor-as-leverage even in absence of explicit labor market rigidities.

place through either the intensive (wages) or the extensive (hirings and firings) margin. Not finding any significant effect is however consistent with the possibility that wage adjustments did not take place because of wage rigidity.

[58]The finding that firms with more tenured workers or a greater share of specialized highly educated workers experience a stronger employment adjustment in our event study echoes the findings in Portugal and Varejão (2007) for the Portuguese labor market. The authors show that firms with this workforce composition are more subject to lumpy adjustment in employment. It is thus not surprising that, the moment in which a liquidty shock hits them, they are relatively less prepared to counter it and have less financial flexibility.

6.2 Labor rigidities and non-cleansing effects

In the previous section, we showed that how the credit shock propagates and which firms are affected the most crucially depend on the firms' exposure to illiquidity risk, as proxied by their exposure to labor costs. Either because labor costs or a rigid component of firm costs or because firms must expose themselves to liquidity risk to pay wages (or plausibly both), we find stronger effects of a credit shock for firms with relatively greater labor costs. We now tackle a related question about the effects of the shock on the productivity of firms, which has been the subject of recent research in macroeconomics.

There is a hypothesis in macroeconomics, dating back as far as Schumpeter (1942), that recessions are periods of enhanced creative destruction, and the worst, least productive agents in the economy are more easily weeded out by economic forces. 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 from the least to the most productive firms. This hypothesis has generally been confirmed in the data, especially regarding labor reallocation in post-war recessions during the 20th century (Davis and Haltiwanger (1990), Davis and Haltiwanger (1992), Davis et al. (1996)). However, some empirical studies have questioned whether this cleansing effect should arise, especially if financial frictions hinder resource reallocation (Barlevy, 2003), with some studies arguing that the cleansing effect could actually be weaker in recessions than in normal times (Osotimehin and Pappadà, 2015) or even reverse itself and turn into a "scarring" effect (Ouyang, 2009). In an influential study about the employment dynamics and reallocation in the US, Foster et al. (2016) argue that the Great Recession was "less cleansing" than previous downturns, with less productivity-enhancing labor reallocation. They argue (but do not prove) that financial frictions might be relevant explanatory factors for justifying their findings.

We put this argument to test, and show that the credit shock that we identify affects both productive and unproductive firms in similar ways. Moreover, we show that productivity by itself is not a strong driver of the shock propagation, as opposed to our measures of labor rigidities: conditional on those measures, total factor productivity does not determine any heterogeneity in firms' reactions to the shock.[59]

In order to evaluate the effects of the credit shock on firms with different productivities, we compute TFP at the firm level, by 2 digit industrial sector. Our preferred methodology follows De Loecker and Warzynski (2012) and Ackerberg et al. (2015), which adopt the control-function approach of Olley and Pakes (1996) and Levinsohn and Petrin (2003) and provide updates to the estimation techniques. Our main specification features TFP calculated through a gross-output three-factor Cobb-Douglas production function. We measure capital both as the net book value or by the perpetual inventory method (PIM), but present results based on the latter measure in our main analysis. Labor is measured as the wage bill, which should capture differences in quality more precisely than the headcount, and intermediate inputs are measured as the cost of

[59]We already partly tested this theory in Table 9, where we showed that the 2005 level of TFP, even if very persistent across time and up to 2008, does not seem to be positively correlated in a significant way to the probability of survival in our sample of relatively big firms.

intermediate goods and services reported in income statements.[60] Input costs are deflated by appropriate price indexes at the industry level.

To test for the cleansing effect of the credit shock, we check that banks did not transmit the shock differently across firms. If this was the case, any possible heterogeneity that we detect in the firm real outcomes might be determined by the different strength with which banks decided to transmit the credit shock depending on firms characteristics, and not by non-cleansing dynamics determined by a shock with the same intensity on different firms. In Table 3 we showed that there is no evidence that banks are on average transmitting the shock differently to different firms, and we offered evidence supporting this view by showing that estimated exposure-level semi-elasticities of credit to the instrument are stable across specifications with and without firm-level fixed effects, and across samples with firms with more than one loans and with all firms. We now go one step further, and test directly whether the same semi-elasticities vary by productivity level.

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

$$S_{i,b} = \sum_{k \in \{L,M,H\}} \beta_k FD_b \cdot \mathbb{1}\{TFP_{bin} = k\} + \mu_i + \varepsilon_{i,b}, \tag{11}$$

where, as in Equation (3), $S_{i,b}$ is the symmetric growth rate of credit variation for each credit exposure of firm i to bank b between 2006–2007 and 2009–2010, calculated as the endogenous treatment in (1) but at the firm-bank level, and FD_b represents bank foreign exposure. We again control for the baseline effects linearly, through results are unchanged with polynomial controls or fixed effects. We estimate the specification in (11) with firm fixed effects for the multi-loan sample (first two columns) and with fixed effects included in the empirical specifications throughout the paper as in columns 8 and 9 in Table 3 for the full sample (second two columns). The results of these specifications reported in Appendix Table D.14 show that across all specifications we cannot rule out the null hypothesis that banks transmit the shock to firms with different productivity in the same way. We are however unable to argue whether this was the case because of "evergreening" incentives, as Blattner et al. (2019) argue for the later period of the EU bank and sovereign crisis, or because, when the unexpected liquidity drought

[60] We conducted several robustness tests, by also estimating a gross-output production function as a three-factors Cobb-Douglas (by OLS) or a translog (by OLS or again following De Loecker and Warzynski (2012); Ackerberg et al. (2015)). In the main text, we show results where TFP is computed as the "transmitted" component of productivity, which is the part of productivity predictable by the firm, which affects input choices. This is computed as the difference between output predicted through a regression on a third order polynomial of all inputs of production (first stage) and the predicted output after the (consistent) estimation of all relevant coefficients of the production function (second stage). All the results presented in the paper are qualitatively robust to different specifications of the production function or to using as TFP the overall residual after the second-stage estimation (without purging the error from the first stage). The full residual, as opposed to the transmitted component, might include unexpected productivity innovations, but also some measurement error and to some extent the influence of prices, given that we perform a revenue TFP estimation. See Appendix C for details about the estimation, and see Tables D.22 and D.23 for descriptive statistics on output elasticities according to the different estimation methods.

hit them, banks could not afford to discriminate among borrowers in cutting short-term credit.

We also estimate analogous specifications at the firm level, similar to what we did for the analysis of labor rigidities. We run the following specification for firm exit:

$$P(exit)_{i,t} = \tau_t + \sum_{k \in \{L,M,H\}} \beta_k S_i \cdot \mathbb{1}\{TFP_{bin} = k\} + \mathbf{\Gamma X_{i,pre}} + FE_{i,t} + \varepsilon_{i,t}, \tag{12}$$

and the following specification for employment:

$$\begin{aligned} log(Y_{i,t}) = \gamma_i + \tau_t + \left(\sum_{k \in \{L,M,H\}} \beta_k S_i \cdot \mathbb{1}\{TFP_{bin} = k\} + \mathbf{\Gamma X_{i,pre}} \right) \cdot \mathbb{1}\{t = Post\} + \\ + FE_{i,t} + \varepsilon_{i,t} \quad t \in \{Pre, Post\}, \end{aligned} \tag{13}$$

and again control for baseline effects linearly.

Table D.15 reports the results of the estimation by productivity bins. The estimated coefficients across productivity levels are not significantly different across each other in either specifications, more evidently so in the case of the employment specifications. Regarding exit, we find a significant effect for low-productivity firms, whereas medium- and high-productivity firms seem to be less affected by the credit shock. Regarding employment, the effects are relatively evenly distributed across productivity levels. This seems to indicate that labor reallocation is not necessarily benefiting high-productivity firms at the expenses of lower-productivity ones. The estimates are not precise enough to yield any definitive conclusion on the presence of a cleansing effect. Still, labor reallocation does not seem to be stronger for the worst firms in the economy, and even the results on firm exit do not clearly indicate that highly productive firms fare better after being hit by an unexpected shock.

6.2.1 Heterogeneity results by labor share and productivity

How do labor rigidities 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 between. We estimate the following specification for firm exit:

$$P(exit)_{i,t} = \tau_t + \sum_{k,j \in \{L,M,H\}} \sum \beta_{k,j} S_i \cdot \mathbb{1}\{LabSh_{bin} = k, TFP_{bin} = j\} + \mathbf{\Gamma X_{i,pre}} + FE_{i,t} + \varepsilon_{i,t}, \tag{14}$$

and the following specification for employment:

$$\begin{aligned} log(Y_{i,t}) = \gamma_i + \tau_t + \left(\sum_{k,j \in \{L,M,H\}} \sum \beta_{k,j} S_i \cdot \mathbb{1}\{LabSh_{bin} = k, TFP_{bin} = j\} + \mathbf{\Gamma X_{i,pre}} \right) \cdot \\ \cdot \mathbb{1}\{t = Post\} + FE_{i,t} + \varepsilon_{i,t} \quad t \in \{Pre, Post\}. \end{aligned} \tag{15}$$

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

We show the results from this empirical exercise in Figures 8a and 8b for TFP estimated according to the Ackerberg et al. (2015) method from a Cobb-Douglas production function, while in Figures E.22a and E.22b we provide as robustness the same specifications with TFP estimated according to Ackerberg et al. (2015) but with a translog production function. Three considerations emerge. First, within labor-share bins different levels of productivity across firms do not seem to determine remarkable differences in the estimated coefficients, consistent with the predictions of Table D.15. Second, the results from section 6.1 on the labor-share heterogeneous effects carry over in a very stark way to this empirical exercise, as evident from the different magnitudes of the coefficients across labor-share bins. Moreover, the coefficients for the high and low labor-share bins across productivity levels are statistically different from each other, at the 5 to 10% level of significance depending on the specification and the specific productivity bin. Third, although there is no significant effect on high-productivity firms for most bins, we do find strong significant effects on high-productivity firms with high labor share, both for employment and, unlike the results in Table D.15, for firm exit. In other words, if we consider the detection of similar effects across different levels of firm productivity as evidence of a non-cleansing effect of the credit shock, we can readily conclude that the estimated effect is not cleansing.[61]

Our results strengthen the argument in Foster et al. (2016) that, at least during the last financial crisis, the cleaning dynamics that are typically observed in recessions were weakened. Moreover, our results show that labor rigidities are a fundamental driver of the propagation of the credit shock across the economy, and might ultimately be the source of financial frictions that hindered productivity-enhancing resource reallocation. In the next section we try to verify this conjecture, and revisit recent empirical evidence on the cleansing effects of the financial crisis in our Portuguese sample.

7 Labor rigidities and macroeconomic effects

Do the labor rigidities that we identify have effects on an aggregate scale? In this section we address this question, and show evidence that suggests the macroeconomic relevance of the financial channels of the labor rigidities.

First, we show results on the overall share of employment loss that the credit shock explains for the post-period, by means of a back-of-the-envelope calculation on the event-study sample. We aggregate the implied effects of the credit shock at the firm level through the reduced-form estimates from the event study, following a counterfactual partial-equilibrium approach. The counterfactual we consider is the variation in employment that would have occurred, per the

[61] In Figures E.19a and E.19b we report the reduced form estimates. In Figures E.20a, E.20b, E.21a, E.21b, E.23a, E.23b, E.24a and E.24b we again provide robustness evidence, through the specifications with residualized labor share, labor share in sales and specifications with CD and TSLOG TFP estimated by OLS. Additional robustness exercises with TFP estimated as the full residual of the production function, or with capital as the net book value of assets, in all the interactions of production functions and estimation methods are available upon request.

event study reduced form estimates, had the credit shock not hit the Portuguese economy.

Second, we revisit the evidence in Foster et al. (2016), and show that their story is confirmed in Portugal, as the cleansing effect we detect around the Lehman shock is present but weaker than in normal times.[62] For this aggregate exercise, we consider all firms in a CB-QP matched dataset, excluding the industries outside the event study. Put differently, we are considering a much bigger set of firms, many of which are much smaller than those in our the event study.

Finally, we show that our shock impacts labor misallocation at the firm level, by measuring the variation in the gaps between the marginal product of labor and the cost of labor. We use this result to perform an aggregate productivity decomposition exercise along the lines of Levinsohn and Petrin (2012) in our sample of large firms. We then show, again by means of a counterfactual partial-equilibrium approach, that labor misallocation at the firm level explained by our shock accounts for slightly less than 5 percent of the aggregate productivity loss in Portugal from 2008 to 2012.[63]

7.1 The credit shock and aggregate employment

We showed in section 5 that a standard deviation in the credit shock can explain approximately between 14 and 17 percent of the standard deviation in employment growth over the period. In this section, we aggregate the reduced-form estimates and show that our identified credit shock can explain a substantial share of employment variation in the post-period. We do so by estimating the counterfactual aggregate employment variation assuming that the treatment effect of the credit shock was uniformly equal to zero. Because of the nature of the aggregation exercise, we implicitly ignore general-equilibrium effects, the imputation of which would require a fully specified structural model.

We estimate Equation (5) in first differences, with the variation in log employment between the pre- and post-periods as outcomes, and obtain the estimated employment growth rate for each firm given the average treatment effect (T):

$$\hat{g}_{i,E}|_T = \hat{\beta} S_i + \hat{\gamma}_i + \hat{\tau}_{post} + \hat{\mathbf{\Gamma}} \mathbf{X_{i,pre}} + \hat{FE}_{i,post} \tag{16}$$

Then, we obtain the analogous predicted employment growth rate assuming that the treatment effect of the credit shock is uniformly 0 (NT):

$$\hat{g}_{i,E}|_{NT} = \hat{\gamma}_i + \hat{\tau}_{post} + \hat{\mathbf{\Gamma}} \mathbf{X_{i,pre}} + \hat{FE}_{i,post} \tag{17}$$

We can thus compute the implied employment growth rate due to the treatment effect, by

[62] In a recent paper, Dias and Robalo Marques (2018) show that the 2010–2012 EU bank and sovereign debt crisis was more cleansing than previous years for Portugal, albeit not for all firms. We do not think their results are at odds with ours, given that shifting back two years the timing that they label as "crisis" considerably weakens their result, which stands given their choice of reference years to define the "crisis" period. However, our results are instrumental in showing one of the possible channels through which financial frictions weaken, but do not necessarily eliminate, cleansing effects (Osotimehin and Pappadà, 2015; Foster et al., 2016).

[63] For the Levinsohn and Petrin (2012) decomposition, we implicitly only consider incumbent firms in 2008 surviving until 2012.

aggregating the treatment effects across all firms:

$$\hat{G}_E|_T = \frac{\sum_i \Delta\hat{E}_{i,Post}|_T}{\sum_i E_{i,Pre}} = \frac{\sum_i E_{i,Pre}[exp(\hat{g}_{i,E}|_T) - exp(\hat{g}_{i,E}|_{NT})]}{\sum_i E_{i,Pre}} \tag{18}$$

The credit shock explains between 1.1 and 1.2 percentage points of the 3.9 percent aggregate employment loss in our sample of medium to large Portuguese firms, depending on whether one uses the estimate for the full sample or only the survivor firms. Put differently, we account for approximately 29 percent of employment losses in relatively big firms in Portugal.[64] The entirety of these employment losses comes from the firms in the highest labor-share quartiles, as showed by the estimation of Equation 10. These estimates show that the credit shock is critical for explaining employment losses between 2008 and 2013, a period which was characterized by a global trade collapse and a Europe-wide financial crisis, which seem to be driving factors of neither the estimated credit shock nor its effects.[65]

7.2 Firm survival and factor reallocation throughout the financial crises

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 were weaker than in normal times, both as regards firms survival and the reallocation of factors of production. Moreover, we show that even at the aggregate level labor rigidities seem to drive the weakening of cleansing effects. We run the following regressions at the firm-year level:

$$y_{i,t+1} = \tau_t + \beta TFP_{i,t} + \gamma TFP_{i,t} \cdot \mathbb{1}\{t \in Post\} + FE_i + \varepsilon_{i,t}, \tag{19}$$

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. All outcome variables come from the CB. 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 12. Productivity seems to underlie a cleansing dynamic on average: higher-productivity firms have relatively lower probability of exit and greater input growth on average. In all cases, however, the effect seems to be weaker in the post-Lehman years, with a variation in the effect always significant at the 5% level. This is true for all the results related to exit, employment growth and fixed-capital growth. There is thus evidence,

[64] We do not explicitly account for firm exit in this back-of-the-envelope calculation, and use average employment levels in the pre- and post periods for the calculations, even in the case of failed firms. The estimate of employment loss is stable if we restrict our attention to survivor firms.

[65] An analogous back-of-the-envelope calculation for total assets predicts a loss in the aggregate stock of slightly more than 1 percent, even if in our sample total assets actually increased by almost 2 percent. This is probably due to capital-intensive firms, which we find are mostly unaffected by the credit shock.

consistent with Foster et al. (2016) or Osotimehin and Pappadà (2015), that the cleansing effect during the financial crisis was weaker, which could be plausibly related to the financial channels and frictions we analyzed in the event study.

We then analyze whether our measure of labor rigidities affects these aggregate results. We again split the firms by labor-share quartiles and, while controlling for the underlying effects of labor share and value added per employee, 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:

$$y_{i,t+1} = \tau_t + \sum_{k=1}^{4} (\beta_k TFP_{i,t} + \gamma_k TFP_{i,t} \cdot \mathbb{1}\{t \in Post\}) \cdot \mathbb{1}\{LabSh_{bin} = k\} + FE_{i,t} + \varepsilon_{i,t}, \quad (20)$$

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 9 and Table D.19 in the Appendix. In all cases, in normal times the cleansing effects are stronger in firms with a greater labor share and labor rigidity. At the same time, the prevalence of the cleansing effect falls most sharply for firms with a high labor share and thus stronger frictions in costs' adjustment. This finding is valid both in absolute terms, for all specification under consideration, and in relative terms, apart from results for the second and forth bins in the capital specification. In relative terms, the greatest weakening in correlation ranges from low values for the exit specification (around 6 percent of the pre-period level) to relatively large and significant values (slightly less than 25 percent of the pre-period value for headcount employment, slightly less than 40 percent for capital).

These results indicate that in the post-Lehman period the cleansing effect was particularly weak with respect to the pre-period for the firms with the most rigid cost structure with respect to labor, both in terms of firm survival and inputs reallocation. In other words, despite the inherent lack of causal identification underlying this exercise, our evidence lends credibility to the relevance of the financial channels of labor rigidity from the event study in impacting reallocation at an aggregate level.

7.3 The credit shock and aggregate misallocation

Having established a connection between the real effects of the credit shock and labor rigidities, we finally turn to the direct imputation of an aggregate productivity effect of the shock. The exercise consists of two parts. First, we analyze the direct effect of the shock on the misallocation of factors of production at the firm level. Second, we use our imputed partial-equilibrium effects to calculate an aggregate productivity effect. Given the nature of our event study, we restrict ourselves to analyzing aggregate productivity in our sample of relatively large Portuguese firms. The logic behind our calculation of counterfactual productivity is similar to the exercise we conducted for aggregate employment variation in section 7.1.

7.3.1 The credit shock and misallocation in labor

In order to obtain firm-level measures of misallocation, we directly analyze the effect of our identified credit shock on the gaps in marginal revenue products and user costs (MRP-cost gaps). In efficient economies the first-order conditions for the optimal input demand would yield zero MRP-cost gaps. As a consequence, any friction in the adjustment process and imperfections in the inputs market might lead to positive or negative gaps. Following the literature on misallocation (Hsieh and Klenow, 2009; Bartelsman et al., 2013; Petrin and Sivadasan, 2013; Asker et al., 2014; Lenzu and Manaresi, 2018) we estimate MRP-cost gaps by measuring wedges in implicit first-order conditions.

In order to compute MRP-cost gaps, we obtain plausible measures of input costs from our dataset, and estimates of output elasticities from the parameters in the estimation of the production function. Before computing the gaps from the data, given that we do not observe prices, we estimate markups following De Loecker and Warzynski (2012).[66] Define MRP-cost gaps as:

$$\text{MRPK-cost gap}_{i,t} = \hat{\theta}^k_{i,t} \frac{P_{i,t} Y_{i,t}}{K_{i,t}} \frac{1}{\hat{\mu}_{i,t}} - R_{i,t}, \tag{21}$$

$$\text{MRPL-cost gap}_{i,t} = \hat{\theta}^l_{i,t} \frac{P_{i,t} Y_{i,t}}{L_{i,t}} \frac{1}{\hat{\mu}_{i,t}} - W_{i,t}, \tag{22}$$

for capital and labor, respectively. We obtain the marginal-revenue product part of the gap by multiplying the relevant output elasticity from production function estimation ($\hat{\theta}^x_{i,t}$, where $x \in \{L, K\}$) by the inverse of the revenue share of the input and the estimated price markup ($\hat{\mu}_{i,t}$). The marginal-cost part depends on whether we consider capital or labor. For capital, the marginal cost consists of $R_{i,t}$, that summarizes the depreciation rate, which we keep at 7 percent (as for the PIM computations), and the average interest rate paid by the firm on its debt, which is the ratio of interest expenditures to total debt.[67] For labor the marginal costs is instead the average wage $W_{i,t}$, for which we divide the total wage bill by the number of employees (taken from the QP when available, or as the full-time equivalent count in the CB).

Table 11 reports the correlation of the estimated gaps with the labor share, after controlling for labor productivity. Even if the labor share by itself, almost by construction, correlates negatively with the MRPL-cost gap, the portion of variation attributable to variation in labor costs and wage policies correlates positively with it. This correlation indicates that those firms with a more rigid cost structure because of labor costs are also the ones that would benefit the most by reallocating labor more efficiently, and for which the marginal product of labor is above marginal costs despite wage policies. This finding suggests that these firms employ a more generous wage policy to attract high-quality labor, but these wages are dangerous double-edged sword when unexpected liquidity shocks hit, as losing workers for these firms hurts aggregate

[66] As Lenzu and Manaresi (2018) and Fonseca and Van Doornik (2019) show, the gaps can also be used to obtain an implicit index of input allocation distortion across firms, and thus which firms are credit-constrained or which firms have more problematic labor/capital wedges that prevent them from optimizing their employment choices.

[67] When the information is missing, similarly to Fonseca and Van Doornik (2019) we impute interest rates as the average yearly interest rate at the 2-digit industry level.

productivity growth.[68]

We reinforce this intuition by showing the direct effect of the shock on misallocation at the firm level. We run the difference-in-differences specification from Equation (5) with the average MRP-cost gaps in the pre- and post-periods as outcomes. We report our results in Table 13, where the dependent variables are the gaps calculated with elasticities from a translog production function with the Ackerberg et al. (2015) methodology, using both PIM and book value of capital.[69]

These effects are predominantly determined by an increase in marginal products of labor, and not by variations in wages. In Appendix Table D.20 we decompose the labor-gaps effects into MRPL and average wages effects and find that after a negative credit shock the MRPL tends to increase at the firm level. This suggests that it is implausible that firms are firing exclusively the most inefficient workers in response to the shock.

The identified shock does not have any meaningful effect on capital misallocation, whereas it has sizable effects (albeit significant only at the 10% level) on the labor gaps, with negative shocks considerably worsening labor allocation. Taking into account that the labor gaps are in thousand of euros, a negative standard deviation shock would increase labor gaps by approximately EUR500, which is slightly more than 20 percent of the average and around 40 percent of the median of the MRPL-cost gap distributions.

Finally, we run the specification from Equation (10) on the MRPL-cost gap to determine whether there are heterogeneous treatment effects through labor rigidities. Figure E.25 shows that even in this case our measure of labor rigidity is strongly correlated with the estimated treatment effects, as we detect the strongest effect in the highest labor-share quartile. Nonetheless, even if not significant, the estimates of the effect are stably negative in three out of four quartiles, which might indicate that the misallocation effects are present in most of the firms in the economy. In Appendix Table D.21 we show analogous results when the outcome variable is the difference (wedge) between output elasticity and revenue share. In these specifications the shock had a significant effect on the labor wedge, and a close to significant effect on the intermediate inputs wedge. These wedge measures can be used to calculate overall allocative efficiency component of aggregate productivity growth as in Levinsohn and Petrin (2012), as we proceed to do in the next section.

7.3.2 Aggregate productivity effects

In order to aggregate the previous results and estimate the share of productivity variation explained by the credit shock, we follow Levinsohn and Petrin (2012) and define aggregate productivity growth (APG) as the change in aggregate demand minus the change in expenditures

[68]Consistent with Fonseca and Van Doornik (2019), we show that labor rigidities strongly characterize constrained firms, for which labor allocation seems to be heavily distorted. This is especially true for highly productive firms (the correlation of both gaps with TFP, not shown, is always significant and positive at the 0.1% level).

[69]We use this methodology as it implicitly allows firm-specific elasticities that vary with input utilization, a more empirically general assumption. In contrast, with Cobb-Douglas the elasticities are fixed at the industry level and the elasticity of substitution across inputs in production is fixed to 1, as inputs are neither complements nor substitutes. Hence, in that case the variation in gaps would only come from variation in revenue shares and markups.

in production inputs (the usual Solow residual). More formally,

$$APG = \sum_i P_i dY_i - \sum_i \sum_{X_i \in \{L,K,M\}} W_{i,X_i} dX_i, \tag{23}$$

where Y_i is final output (total sales), 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 (either the wage W_i or the user cost of capital R_i, which includes 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:

$$APG = \sum_i D_i d\log A_i + \sum_i D_i \sum_{X_i \in \{L,K,M\}} (\theta_i^X - s_i^X) d\log X_i, \tag{24}$$

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 obtained through the production function estimation and $s_i^{X_i}$ are revenue shares for each input X_i.[70] The first term in Equation (24) defines variation in technical efficiency at the aggregate level and the second term represents the effect of input reallocation. This term, beyond the technical-efficiency effect (which would be APG in efficient economies (Hulten, 1986) incorporates the benefit from reallocating inputs from firms in which the marginal product of the input is relatively smaller with respect to the cost to firms in which the marginal product is relatively higher.

We perform an aggregate-productivity growth-accounting exercise between 2008 and 2012 by using a discrete-time (Tornquist-Divisia) approximation of Equation(24), using 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. The exercise is carried out on the sample of relatively large firms that we use for most of the causal empirical analysis, as we aim to use the estimates of wedge effects from section 7.3.1 to compute the contribution of the shock. For this part of the exercise we implicitly assume that our sample represents the whole Portuguese economy.[71] We estimate an overall -12.37 percent APG for Portugal between 2008 and 2012, due to variation in TFP not explained by input reallocation. The combined contribution of alocative efficiency is on the other hand close to a positive 1-percenage point variation (due to labor reallocation in the aggregate).[72]

Then, we perform a counterfactual exercise similar to the one for aggregate employment in section 7.1, following the derivations in the second decomposition of Baqaee and Farhi (2019a). This exercise follows a very similar exercise run in Bau and Matray (2019), in which the same decomposition is used to calculate the productivity effects determined by decreases in misallocation as a consequence of liberalizations in India over the 2000s.

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

[71]See Appendix Table 1 for the sample representativeness.

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

For the aggregation exercise, we assume that the technical productivity variations are not correlated with our reduced-form estimates from the event study.[73] Thus, 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 (2019a), which is a generalization of the results in Levinsohn and Petrin (2012), under the assumption of no TFP changes (due to the shock), and compute the amount of aggregate productivity growth attributable to the shock only through misallocation. As regards labor variation, we use the estimates from Section 7.1, specifically Equations 16 and 17. As regards variation in the labor wedge, we take averages between the 2008 labor wedge and the 2012 wedges implied by the reduced form estimates variation. Specifically, the relevant wedge in the case the treatment effect is accounted for is, according to results in Appendix Table D.21,

$$\widehat{\theta_i^L - s_i^L}|_T = \hat{\beta} S_i + \hat{\gamma}_i + \hat{\tau}_{post} + \hat{\mathbf{\Gamma}} \mathbf{X_{i,pre}} + \hat{FE}_{i,post}, \tag{25}$$

whereas the counterfactual wedge had the shock not hit is

$$\widehat{\theta_i^L - s_i^L}|_{NT} = \hat{\gamma}_i + \hat{\tau}_{post} + \hat{\mathbf{\Gamma}} \mathbf{X_{i,pre}} + \hat{FE}_{i,post}. \tag{26}$$

According to the use of the Tornquist-Divisia approximation, the wedges to be used in the aggregation exercise are thus

$$\overline{\theta_i^L - s_i^L}|_j = \frac{1}{2}((\theta_i^L - s_i^L)_{2008} + (\widehat{\theta_i^L - s_i^L})|_j) \quad j \in \{T, NT\} \tag{27}$$

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

$$APG|_T \approx \sum_i \bar{D}_i \left(\overline{(\theta_i^L - s_i^L)}|_T \, \widehat{dlogL_i}|_T - \overline{(\theta_i^L - s_i^L)}|_{NT} \, \widehat{dlogL_i}|_{NT} \right), \tag{28}$$

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 (28) is the allocative efficiency aggregate productivity variation 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.

We find that the misallocation attributable to labor would increase by 4 percent absent the shock, whereas the effect of the shock further increases misallocation by 0.52 percentage points, i.e. 13 percent of the estimated partial-equilibrium effect. In aggregate terms, the implied variation attributable to the shock corresponds to approximately 4.2 percent of the total. Moreover, implied misallocation according to the invent study reverses the conribution of allocative efficiency to aggregate productivity growth, from positive to negative.

[73] We do not find any direct effect of our shock on TFP. However, the assumption here is actually 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.

The effect that we identify has economic significance, but the methodology used is based on a partial-equilibrium exercise that has some caveats and would call for caution when interpretating results. This is especially true given that the general-equilibrium exercise does not detect a large change in misallocation, whereas the partial-equilibrium estimates from the event study imply an increase in misallocation, at least for labor. The analysis of the financial channels of labor rigidities through the lens of a structural model of the economy, in this sense, would provide a more complete characterization of the productivity dynamics and the underlying propagation channels. We argue that our exercise provides evidence that the financial channels that we identify have plausible macroeconomic relevance for part of the productivity dynamics observed at the aggregate level.[74]

8 Conclusion

We study how credit shocks affect labor market reallocation, firms' exit and other real outcomes, how labor-market rigidities impact the propagation of credit shocks, and whether in turn this propagation reinforces productivity-enhancing reallocation dynamics. To answer these questions, we conducted 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 dissected the way in which the credit shock generated by that episode spread 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, which we label "labor-as-leverage" and "labor-as-investment" financial channels. 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.

The relevance of labor obligations for financial risk and real activity of firms has recently been the focus of empirical research in finance and macroeconomics (Serfling, 2016; Donangelo et al., 2019; Favilukis and Lin, 2016a). Our study sheds light on the macroeconomic relevance of financial frctions at the firm level determined by labor rigidities, and poses interesting questions about how policy-makers should think about regulating their influence. The fact that several of our results are determined by both labor-as-investment and labor-as-leverage channels lends credibility to the hypothesis that labor-as-investment can over time create labor-as-leverage,

[74]Blattner et al. (2019) perform an analogous exercise in their paper. Cumulating their estimated APG over the years, one obtains a productivity decrease of approximately 16.4 percent, with an overall 7 percent coming from TFP and most of the rest coming from capital misallocation. We obtain a number in the same ballpark of theirs, but have most of the decrease coming from TFP (13 percent) with no significant contributions from capital misallocation. However, it is hard to compare the results, as we limit ourselves to a smaller sample of relatively bigger firms, where it is reasonable to expect a smaller productivity decrease overall. We also use different techniques in order to estimate the production function elasticities (they use cost-shares for the APG, whereas we use Ackerberg et al. (2015) control function methods on a translog, which is a more flexible production function specification) and even the sample of analysis varies, as we only focus on incumbent firms in 2008 that are alive until 2012.

given that, by accumulating skills, workers become a source of high and rigid costs for firms (Oi, 1962). In that sense, it is paramount to deepen our understanding of the workings of the two financial channels by developing a structural model, more so to study their general-equilibrium effects and elaborate counterfactuals that would allow the evaluation of policy responses. In fact, the two conceptually distinct financial channels that we identify, despite their intrinsic connection, demand different policy responses. The labor-as-leverage financial channel calls for policies that alleviate labor market frictions, and especially wage rigidity. This channel would by itself call into question the efficacy of hiring credits in recessions in order to preserve employment levels. Instead of facilitating new hiring, it should actually motivate policy-makers to evaluate measures that alleviate the burden of firms' current employment costs.[75]

The existence of the labor-as-investment financial channel, in contrast, is deeply ingrained in how firms carry out some production processes, and its relevance will only increase over time as intangible human capital becomes more salient in both manufacturing and services (Xiaolan, 2014; Sun and Xiaolan, 2018). These trends make it essential for economists to improve their understanding of labor financing, and rationalize why firms expose themselves to the risk inherent in the maturity mismatch in financing an investment good as high-skill labor with short-term credit. In turn, this understanding will enable policy-makers and firm managers to develop policies to support more stable forms of labor financing, or alleviate the exposure of firms to the liquidity risk of incurring upfront costs.[76]

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

[75]Recent evidence in empirical macroeconomics and labor point exactly to this direction. Among others, Giupponi and Landais (2018) and Cahuc et al. (2018) show that short-term-work schemes in Italy and France were particularly helpful during the Great Recession to subsidize labor hoarding and attenuate the employment effects of the recession. Our results imply that these programs, on top of preserving employment, might also have beneficial effects on productivity. Moreover, the attenuation of the adjustment frictions in labor and labor costs implied by these scheme might allow to distinguish cases in which a firm wants to fire unproductive workers but is constrained by explicit labor-market rigidites 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).

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

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9 Tables

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

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

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

Table 2: Firm level descriptive statistics, sample of analysis

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

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

Table 3: Loan level regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
					$\Delta D_{i;st,pre-post}$					$\Delta D_{i;tot,pre-post}$
$FD_{b;2005}$	-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]									
$FD_{b;2005}$		-0.215***								
$> p50(FD_{b;2005})$		(0.028)								
$FD_{b;2005}$			-0.095***							
$> mean(FD_{b;2005})$			(0.025)							
$Sovs./Ass._{b;2009}$				-6.501***						
				(0.576)						
$Sovs./Ass._{b;2009,q4}$					-4.226***					
					(0.369)					
$ID_{b;2005}$										-0.432***
										(0.121)
Firms	9927	9927	9927	9927	9927	9927	13937	9927	13933	10413
Firm FE	Yes	Yes	Yes	Yes	Yes	No	No	No	No	Yes
Other FE	No	No	No	No	No	No	No	Yes	Yes	No
Sample	Multi-loans	Multi-loans	Multi-loans	Multi-loans	Multi-loans	Multi-loans	All firms	Multi-loans	All firms	Multi-loans

In columns 1-9 the dependent variables is the symmetric growth rate of average short term debt between 2006-2007 and 2009-2010. In column 10 the dependent variable is an analogous growth rate for total debt as in Iyer et al. (2014). The main regressor of interest ID in column 10, again as in Iyer et al. (2014), is the overall ratio of interbank funds' liabilities to assets in 2005 (domestic and foreign).
In columns 1-5 and column 10 the firm fixed effects control for unobservable firms' characteristics time-trends. In columns 6-7 there are no additional controls, whereas in columns 8-9 additional fixed effects for observables are added. Samples are either firms with loans with more than one bank (essential to identify the firm fixed effect) or the complete sample of firms (also firms with one loan only).
In columns 4-5 we control for the ratio of sovereign debt on balance sheet over total assets, where the amount of government-issued debt is calculated as either the average of 2009 holdings, or the average of the last quarter of 2009 holdings. The logic of the control in the analysis follows Buera and Karmakar (2017).
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 6. We refer the interested reader to Oster (2019) for details regarding how the bounds are conceived.
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.
$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4: Employment regressions

	(1)	(2)	(3)	(4)	(5)
			$log(\#emp)_{i,t}$		
S_i	0.066+	0.072*	0.070*	0.071*	0.086*
	(0.040)	(0.033)	(0.033)	(0.034)	(0.035)
Firms	14846	14830	13833	13806	11802
EFF. WID F	34.15	37.29	36.52	34.56	35.55
Sample	Complete	Complete	Complete	Complete	Survivors
Fixed effects	No	Yes	Yes	Yes	Yes
Controls	No	No	Fail b.c.	Yes	Yes

The regressions refer to the empirical specification in equation (5) in the text. All regressions feature firm and time fixed effects.
We refer to as the estimation sample in which only the controls for which the balance checks have failed are included and controlled for.
Additional fixed effects are 3-digit industrial sector, commuting zone, quintiles of firm age and size in 2005, and dummies for: exporter, 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 include the 2005 values of: log level of short-term credit, financial leverage, log of total assets, short-term credit growth betweeen 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, ROA, ROS, log of the average wage, average workers' turnover rate between 2003 and 2005. All controls and fixed effects are interacted with a post-period dummy.
The number of firms in the regressions change depending on the missing values in the balance-sheet variables.
Standard errors clustered at the bank-industry pair level.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5: Wage bill regressions

	(1)	(2)	(3)	(4)	(5)	(6)
	$log(wage\ bill)_{i,t}$		$log(base\ wage\ bill)_{i,t}$		$wage\ bill_{i,t}/sales_{i,pre}$	
S_i	0.090*	0.114**	0.093*	0.111**	0.163+	0.233**
	(0.038)	(0.041)	(0.038)	(0.040)	(0.084)	(0.083)
Firms	13806	11802	13806	11802	13806	11802
WID F	35.63	36.33	35.63	36.33	42.37	46.45
Sample	Complete	Survivors	Complete	Survivors	Complete	Complete

The dependent variables are either the total wage bill (columns 1 and 2) or the base wage bill, which does not comprehend extraordinary or overtime payments (columns 3 and 4). In columns 5 and 6 the dependent variable is the ratio of wage bill to the pre-period average value of sales, whereas the treatment is the variation in average short term credit (as for the standard treatment) scaled by the pre-period average value of sales. The coefficients in columns 5 and 6 should be interpreted as dollar-on-dollar cash-flow pass through.
See the table 4 for the list of controls and fixed effects in the regressions. All regressions feature the full set of fixed effects and controls.
Standard errors clustered at the bank-industry pair level.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 6: Employment - wage bill regressions: Manufacturing

	(1)	(2)	(3)	(4)	(5)	(6)
	$log(\#emp)_{i,t}$		$log(Wage\ bill)_{i,t}$		$log(Base\ wage\ bill)_{i,t}$	
S_i	0.118**	0.136**	0.165**	0.186**	0.153**	0.169**
	(0.041)	(0.047)	(0.056)	(0.062)	(0.052)	(0.057)
Firms	6348	5404	6348	5404	6348	5404
EFF. WID F	24.06	22.27	24.06	22.27	24.06	22.27
Sample	Complete	Survivors	Complete	Survivors	Complete	Survivors

See the table 4 for the list of controls and fixed effects in the regressions. All regressions feature the full set of fixed effects and controls.
Regressions estimated only on the subsample of firms in manufacturing industries.
Standard errors clustered at the bank-industry pair level.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 7: Heterogeneous employment regressions: Qualifications

	(1)	(2)	(3)	(4)	(5)	(6)
	$Managers_{i,t}$		$Spec.\ workers_{i,t}$		$Generic\ workers_{i,t}$	
S_i	0.072	0.134	0.343**	0.403**	0.076	0.106
	(0.103)	(0.107)	(0.129)	(0.135)	(0.063)	(0.066)
Firms	11406	9759	13002	11156	13176	11272
EFF. WID F	30.66	34.82	36.30	37.36	35.47	37.48
Sample	Complete	Survivors	Complete	Survivors	Complete	Survivors

The dependent variable in these regressions is the ratio of the number of specific workers to the average level of the pre-period corresponding amount. As such, the regressions are defined only for the firms for which the kind of worker is present in the pre-period (even if missing values for some years are possible).
Workers' categories are derived by aggregating the 9 levels of qualification defined by the Portuguese Law (Decree-Law 380-80). The levels are based on the nature and complexity of the tasks performed by the workers within the firm. "Generic" workers carry out basic, routine and/or repetitive tasks that do not require any particular decision making. Specialized workers (team-leaders) on the other hand deal with more complex tasks that might require discretionary decision-making. Managers directed the general policy and are in charge of defining strategies and organization of the firm.
The outcome variable is winsorized at the top 1% level.
See the table 4 for the list of controls and fixed effects in the regressions. All regressions feature the full set of fixed effects and controls.
Standard errors clustered at the bank-industry pair level.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 8: Heterogeneous employment regressions: Age cohorts

	(1)	(2)	(3)	(4)	(5)	(6)
	Young $w_{\cdot i,t}$		*Prime age* $w_{\cdot i,t}$		*Old* $w_{\cdot i,t}$	
S_i	0.170+	0.223*	0.087**	0.101**	0.079	0.040
	(0.098)	(0.102)	(0.030)	(0.032)	(0.058)	(0.054)
Firms	13210	11315	13806	11802	10679	9124
EFF. WID F	30.70	31.64	34.56	35.55	28.76	34.81
Sample	Complete	Survivors	Complete	Survivors	Complete	Survivors

The dependent variable in these regressions is the ratio of the number of specific workers to the average level of the pre-period corresponding amount. As such, the regressions are defined only for the firms for which the kind of worker is present in the pre-period (even if missing values for some years are possible).
The age categories are: young workers (between 16 and 30), prime age workers (between 30 and 55) and old workers (between 56 and 65). Age cohorts are fixed over the period of analysis and defined depending on the age of the worker in 2008.
The outcome variable is winsorized at the top 1% level.
See the table 4 for the list of controls and fixed effects in the regressions. All regressions feature the full set of fixed effects and controls.
Standard errors clustered at the bank-industry pair level.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9: Exit regressions

	(1)	(2)
	$P(exit)_{i,t}$	
S_i	-0.019+	-0.023+
	(0.011)	(0.012)
$ACF\ TFP_{i,05-06}$		0.001
		(0.002)
Firms	13796	13277
EFF. WID F	36.20	35.74

See the table 4 for the list of controls and fixed effects in the regressions. In addition to that specification we control for average TFP in 2005 and 2006 (estimated according 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) in column 2. TFP can be estimated for less firms depending on availability of the variables to compute it in CB. Moreover, given that we cannot control for unobservable characteristics through firm fixed effects any more, we try to 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 that will fail until the year 2014.
In this specification the fixed effects are interacted with year dummies, whereas the controls are kept constant and not interacted with any year dummy.
Standard errors double-clustered at the firm and bank-industry level.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 10: Balance sheet and financials regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	$log(assets)_{i,t}$	$log(sales)_{i,t}$	$arsinh(cash)_{i,t}$	$arsinh(trade\ credits)_{i,t}$	$arsinh(suppliers'\ debt)_{i,t}$	$log(fixed\ assets)_{i,t}$	$log(current\ assets)_{i,t}$
S_i	0.098*	0.041	-0.123	0.408+	0.021	0.063	0.108*
	(0.041)	(0.044)	(0.128)	(0.226)	(0.117)	(0.071)	(0.054)
Firms	11802	11554	11802	11802	11802	11799	11792
EFF. WID F	35.55	34.93	35.55	35.55	35.55	35.55	35.55

When the *arsinh* is used, the variable is expressed in net terms and can take negative values.
Outcome variables are winsorized. Variables expressed in logs that can take only positive values are right-tail winsorized at the 97.5^{th} percentile. Variables expressed as arsinh are winsorized on both tails, at the 1st and 99th percentiles.
Sample size varies depending on the availability of the balance sheet item in a consistent way in CB (after harmonization of balance sheet data across the two different accounting systems, pre- and post- 2010).
The regressions are carried out on the sample of firms surviving up to 2013.
See Table 4 for a list of the added controls and fixed effects present in the regressions. All regressions feature the full set of fixed effects and controls.
Standard errors clustered at the bank-industry pair level.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 11: Correlations of observables with labor share

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

Correlations are measured in regressions controlling by the full set of fixed effects and controls in the diff-in-diff specifications, which implies that we are also always controlling for value added per employee. Labor share and value added for employee are calculated as averages for the years 2005 and 2006.
When controlling for a variable post-2006, its previous level in 2005 is excluded from the controls.
Abowd et al. (1999) (AKM) firm fixed effects obtained from regressing (full) individual wages in the pre-period on individual fixed effects, firm fixed effects, year fixed effects, gender dummy, educational level fixed effects (less than high school, high school and undergraduate degree and higher), a third order polynomial of age.
Results for AKM FE and avg. wage robust to controlling for workforce composition variables (share of specialized workers and managers, shares of workers with different education levels).
See appendix C.2 for details regarding the estimation of MRP - cost gaps.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 12: 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 (19) 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.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

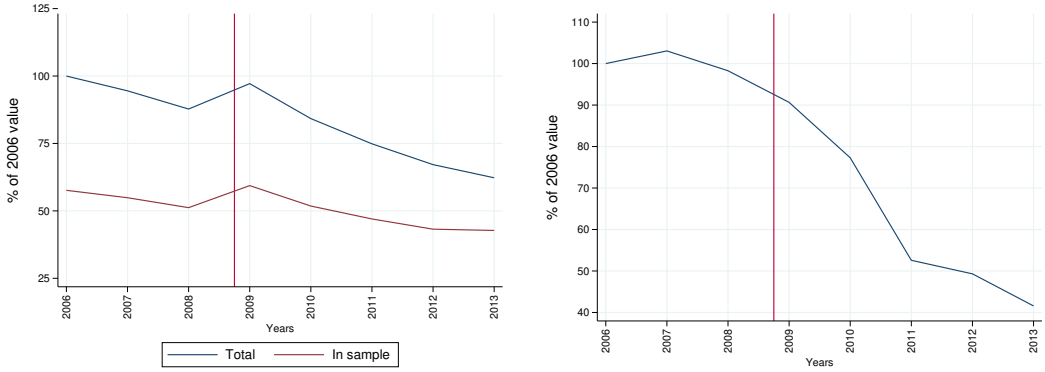
Table 13: Marginal revenue products - cost gaps regressions

	(1)	(2)	(3)	(4)
	$Lab.\ gap_{i,t}$	$Cap.\ gap_{i,t}$	$Lab.\ gap_{book;i,t}$	$Cap.\ gap_{book;i,t}$
S_i	-0.968+	-0.008	-0.888	0.019
	(0.537)	(0.027)	(0.547)	(0.022)
Firms	11710	12823	12005	12895
WID F	29.34	35.37	29.06	34.99
Sample	Complete	Complete	Complete	Complete

The gaps are expressed in thousand euros (labor) and percentage (capital).
In columns 1 and 2 gaps are obtained given estimation of output elasticities with capital computed through PIM.
In columns 3 and 4 gaps are obtained given estimation of output elasticities with book value of capital.
Outcome variables are winsorized at 0.5^{th} and 99.5^{th} percentiles.
See Table 4 for a list of the added controls and fixed effects present in the regressions. All regressions feature the full set of fixed effects and controls.
Regressions are run on the full sample for all firms for which it was possible to calculate the MRP-cost gaps.
Standard errors clustered at the bank-industry pair level.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

10 Figures

Figure 1: Credit dynamics in Portugal

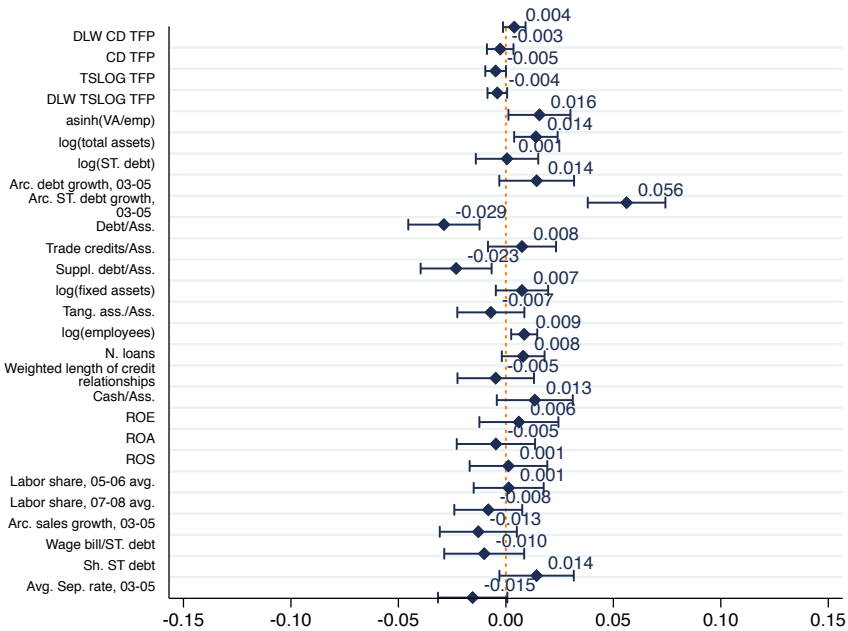


(a) Short-term regular credit

(b) Foreign interbank liabilities

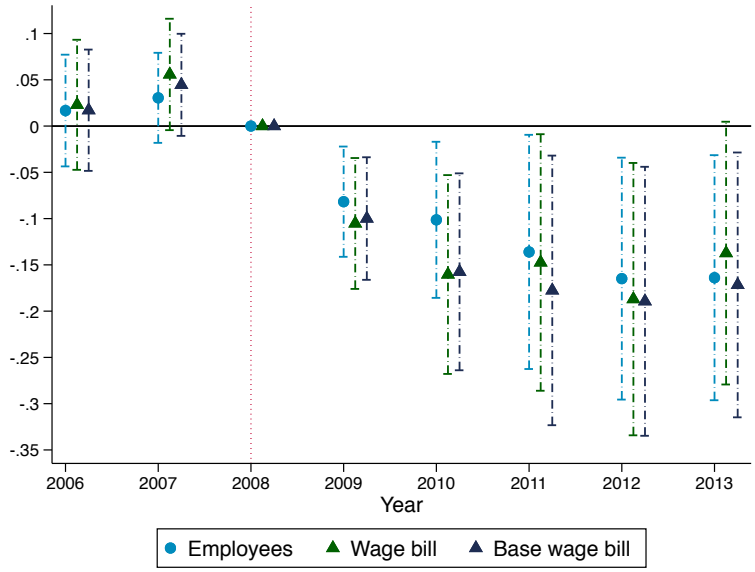
The Figures show the time series for the aggregate amount of short-term credit (left) and foreign interbank liabilities (right) for the firms and banks in the sample. Foreign interbank liabilities are the sum of short-term deposits (up to 1 year) and repos where the counterparty is a foreign financial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

Figure 2: Balance checks



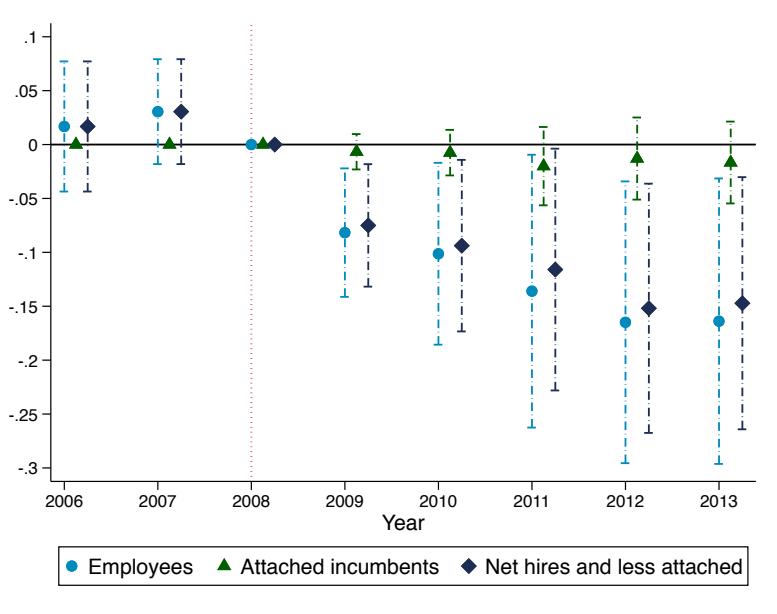
The Figure shows the coefficients (with 95% confidence intervals) of pairwise regressions of the standardized value of each variable in 2005 (unless reported otherwise) on the instrument Z_i. All regressions include the same set of fixed effects of the main specification, which are 3-digit industrial 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.

Figure 3: Employment and wage bill regressions: event study

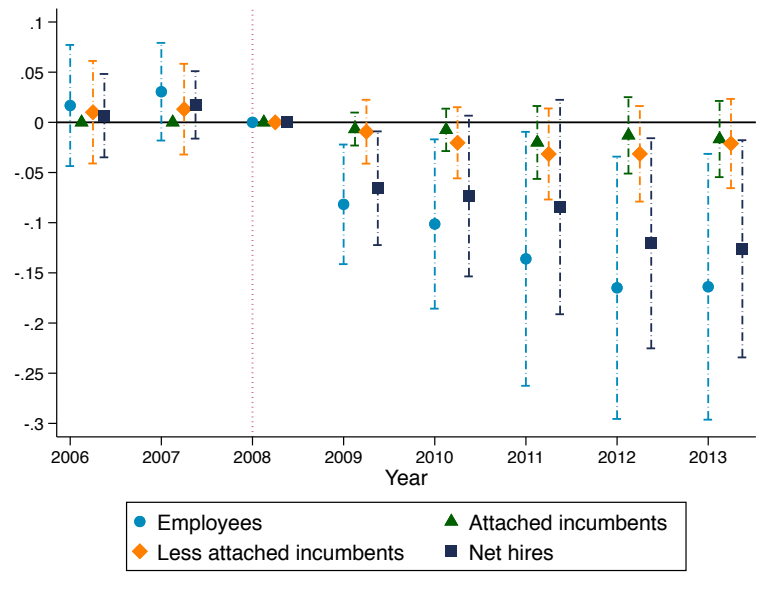


The dependent variables in these regressions are the ratio of the number of employees (wage bill) over the average of their level in the pre-period (2006-2008). In the specifications the coefficient for the year 2008 are normalized to 0, so that all the other coefficients have to be interpreted as the effect on the percentage variation of employment or wage bill with respect to the 2008 level.
The sample includes only survivor firms (N = 11,801), but is not balanced. The graph displays the effect of a negative shock.
See Table 4 for the list of controls and fixed effects in the regressions. All regressors and fixed effects are interacted with a year dummy.
95% confidence intervals displayed, standard errors clustered at the bank-industry pair level.

Figure 4: Employment adjustment by tenure



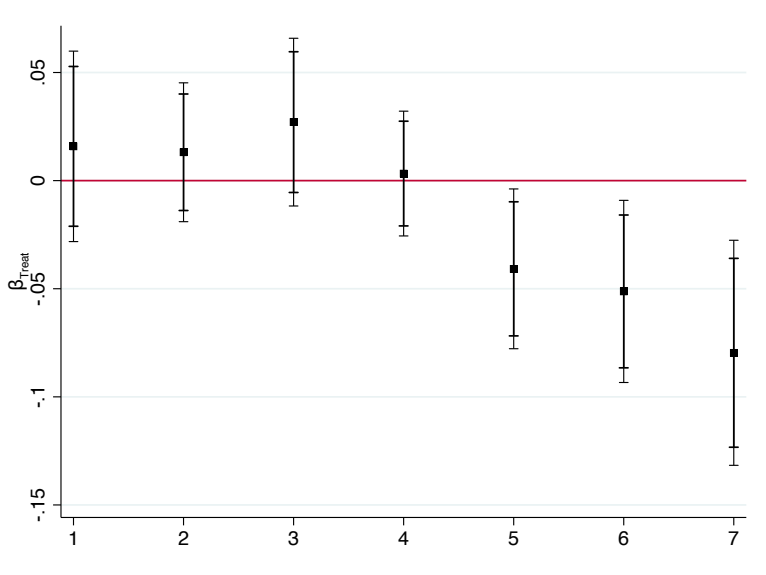
(a) Attached incumbents vs. all other workers



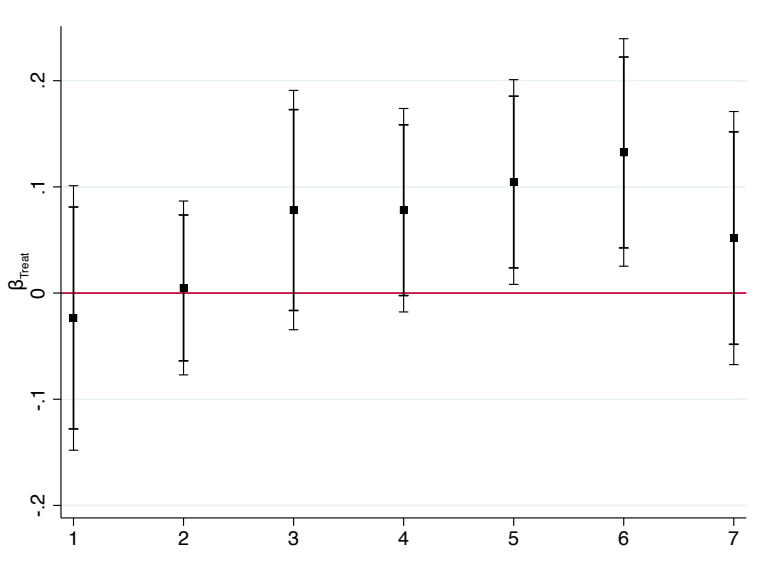
(b) Attached incumbents vs. less attached incumbents and net hires

The dependent variables in these regressions are the ratio of the number of employees of the specific category over the average number of employees in the pre-period (2006-2008). By construction, the sum of the coefficients of each regression should be equal to the overall employment effect. In the specifications the coefficients for 2008 are normalized to 0, so that all the other coefficients should be interpreted as the effect on the percentage variation of each kind of employment with respect to the 2008 level.
Attached incumbents are defined as workers present at the firm for the entirety of the pre-period. Less attached incumbents are all other incumbents in 2008, whereas net hires are all the other workers (hires/separations in the post period). In order to get a sense of the implied elasticities of adjustment, one should divide the estimated coefficient by the share of workers in the pre-period. Attached workers constitute more than 67% of the workforce in the pre-period. The share of less attached incumbent is the remaining share of workers in 2008, and is always less than half of the attached incumbents share throughout the post-period.
The sample includes only survivor firms (N = 11,801), but is not balanced. The graph displays the effect of a negative shock. See Table 4 for the list of controls and fixed effects present in the regressions. All regressors and fixed effects are interacted with a year dummy. 95% confidence intervals displayed, standard errors clustered at the bank-industry pair level.

Figure 5: Regressions by labor-share bins



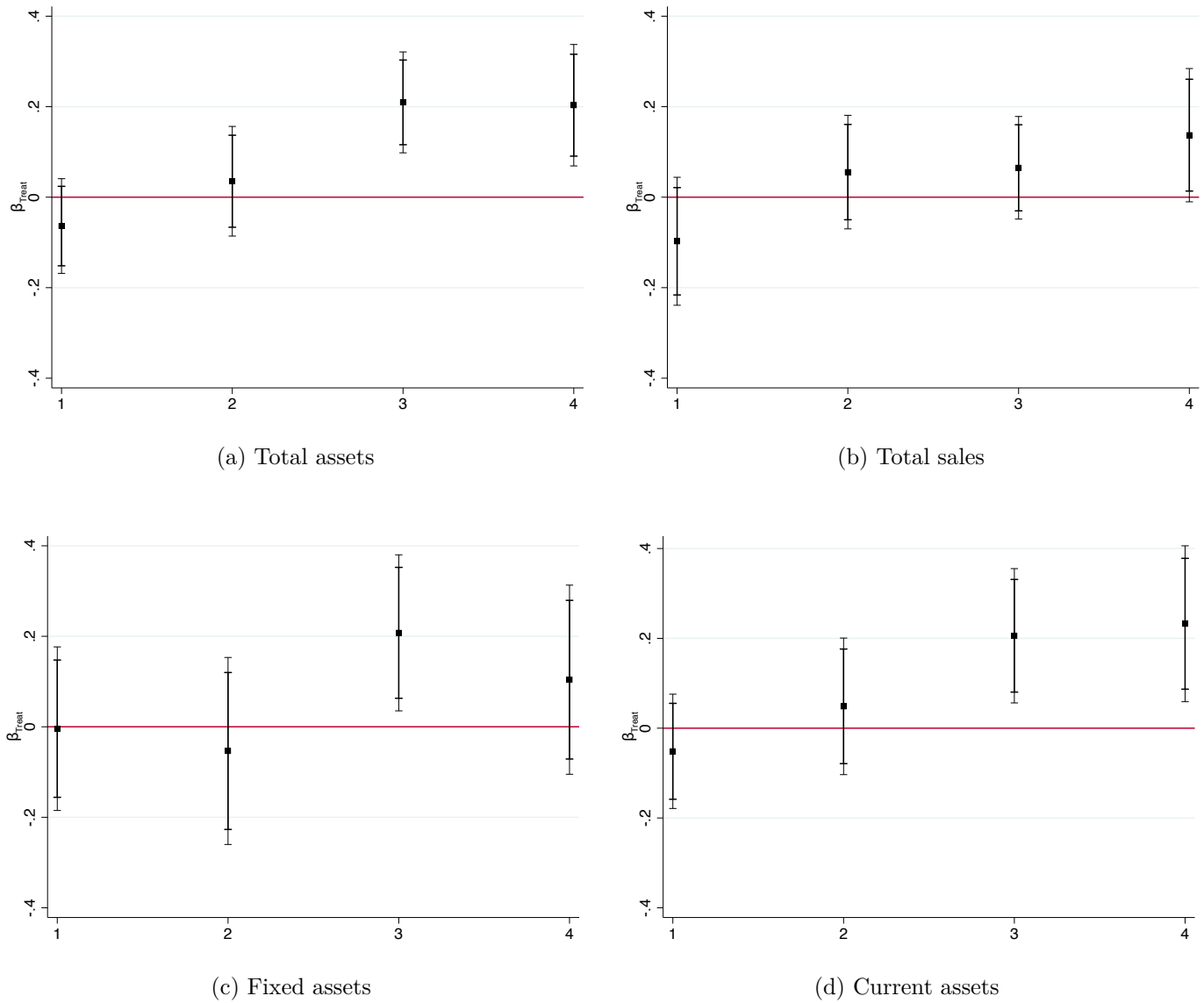
(a) Exit



(b) Employment

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 Table 4 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). 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.

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



(a) Total assets

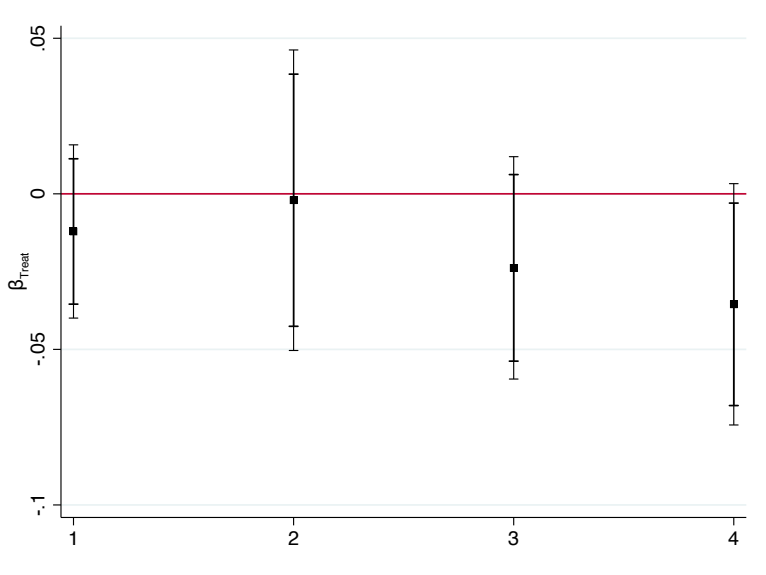
(b) Total sales

(c) Fixed assets

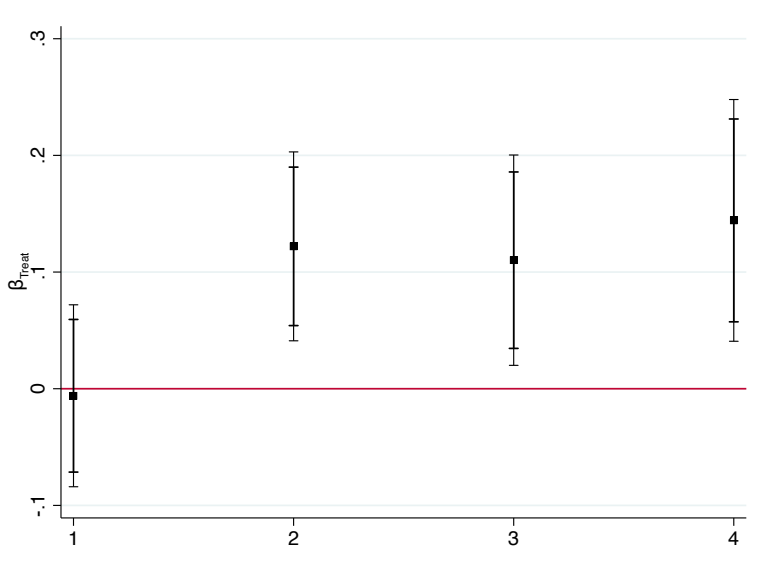
(d) Current assets

We estimate a coefficient for each of the four labor-share quartiles, while controlling linearly for baseline effects. Each interacted treatment is instrumented by the interacted instrument. Current assets are defined residually by subtracting fixed assets from total assets.
See Table 4 for the list of controls and fixed effects present in the regressions. Fixed effects and regressors are interacted with the Post dummy.
All fixed effects and controls are interacted with a *post* dummy.
Number of firms: 13,760.
95 and 90% confidence intervals displayed, standard errors clustered at the bank-industry pair level.

Figure 7: Regressions by quartiles of otj training scores



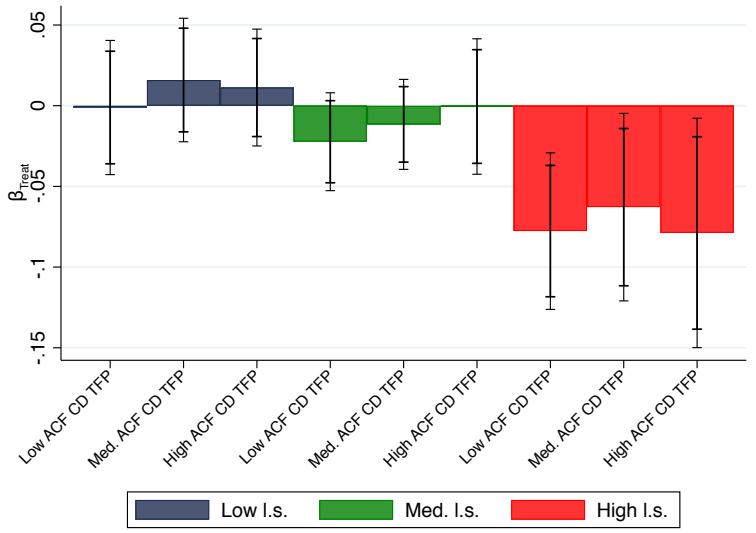
(a) Exit



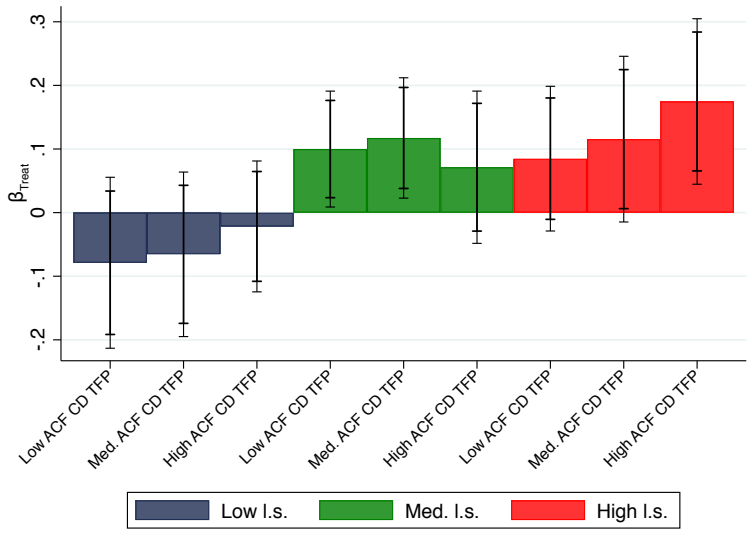
(b) Employment

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 (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 Table 4 for the list of controls and fixed effects present in the regressions. Given that we cannot control for unobservable characteristics in the exit specifications through firm fixed effects, we characterize the matching of firms to banks by augmenting the set of controls with the share of loans that each firm has with micro banks and with banks failing up to 2014. Additional controls for these specification using O*NET variables comprehend the scores for: required education, required previous experience and required amount of training on site. Results are unchanged if these additional controls are not added. See Appendix B.1.7 for a description of each of these variables, and the on-the-job training score as well.
All fixed effects are interacted with a year dummy, while regressors are constant in the exit regressions. In the employment regressions fixed effects and regressors are interacted with the Post dummy.
Number of firms: 13,746 (exit) and 13,756 (employment). 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.

Figure 8: Regressions by labor-share and CD-ACF-productivity bins



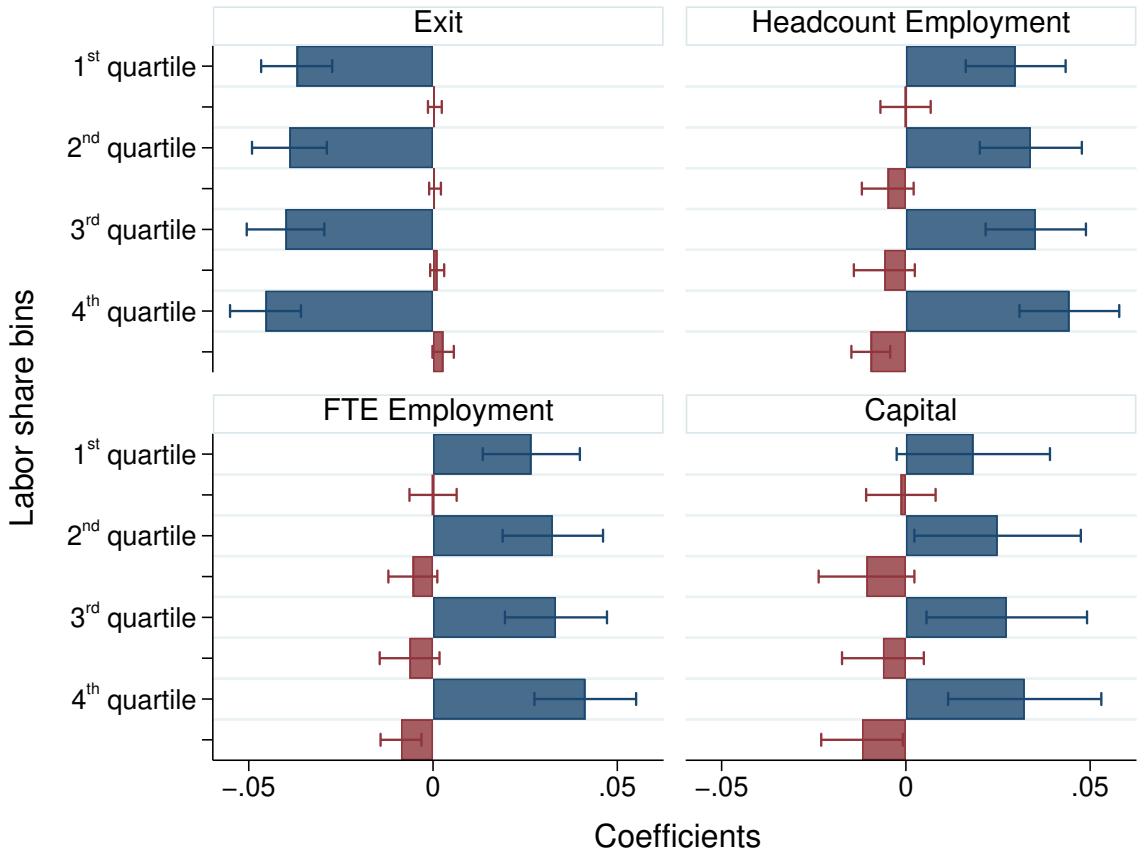
(a) Exit



(b) Employment

We estimate a coefficient for each of the nine interacted bins, while controlling linearly for baseline effects and their interaction. Each interacted treatment is instrumented by the interacted instrument. Labor share is defined as the ratio between employment-related costs and total value added (average of 2005-2006 levels). Productivity is estimated on a 3-inputs gross sales Cobb-Douglas production function following Ackerberg et al. (2015), by 2-digit industrial sectors.
See Table 4 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.

Figure 9: Reallocation and TFP by labor share - full dataset



The regressions refer to the empirical specification in Equation (20) 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.

APPENDIX

A A model of the financial channels of labor rigidities

The aim of the present section is to provide a conceptual framework for the emergence of the financial channels of labor rigidities, which are at the core of the empirical analysis of the paper. To this end, it develops a simple model of firm decision of credit and labor demand, subject to a working-capital constraint.

The economy lives for two periods, and is populated by a profit-maximizing firm. The firm has a constant returns-to-scale technology F that employs capital and labor to produce a consumption good to be sold at price $P = 1$.[1] The firm starts the initial period with some retained earnings Π, a fixed amount of capital $\bar{K}$ and incumbent workers I (a share of the workforce of the previous period) who receive a salary w^I.[2] The firm has to choose whether to adjust the workforce by hiring new workers N in a competitive labor market.[3] To this end, it offers to them a salary w^N and incurs in a training/vacancy cost c in order for them to be productive in the final period. The firm has to pay a share λ of the total wage bill of the final period in advance, together with the full training cost c. We are assuming that, given the natural of the production activity carried out, there might be time gaps between cash inflows and wage payouts, or that it takes time for workers to become productive and the training has to be financed in advance.[4] For this reason, it needs to borrow an amount D of working capital from a banking sector.

For the present exercise, we assume that the firm starts in an unconstrained condition, in which bank credit D is available at a rate $R = 1/\beta$, where β is an intertemporal discount factor and is equal to 1 for simplicity. The firm incurs a fixed cost of operation f, and exits if is unable to honor it. In that sense, credit is only useful to transfer resources across periods, but absent any unexpected shock would have no effect on the unconstrained equilibrium. Hence, we assume for simplicity that the firm takes external credit only up to the amount needed to finance its current expenditures, but no more. Formally, the firm maximizes its value V:

$$\max_{N,D} V = AF(N + I, \bar{K}) - w^N N - w^I I - cN - f, \tag{29}$$

subject to the working-capital constraint:

$$D + \Pi \geq cN + \lambda(w^N N + w^I I), \tag{30}$$

where A is the firm TFP, and Π are retained earnings.

In normal times, when the working-capital constraint is not binding, the first-order condition with respect to new hires yields:

$$AF_L(N + I, \bar{K}) = w^N + c. \tag{31}$$

This relationship defines the optimal level of new hires N^* and as a consequence the optimal amount of credit D^* to achieve it from the working-capital constraint (30).[5]

Now suppose that the bank unexpectedly cuts its credit supply and the firm can only obtain a level $\bar{D}$.[6] In

[1]This market setting comes at no loss of generality, and can be easily extended to a monopolistically competitive environment.

[2]We do not model the determination of the incumbents' salary. That would arguably depend on current market wages, past wage levels in case of downward wage rigidity and some incurred training cost.

[3]De facto, this amounts to analyzing a setting with decreasing returns to scale in the short run, and necessarily ignores any feedback effect on fixed capital investment through labor-capital complementarity in production.

[4]Another way to rationalize this constraint is by limited enforcement in labor contracts. Before production takes place, the firm promises salaries w^I and w^N to incumbents and new hires, respectively. However, it can renege on the promise and defaults on his payroll, in which case the workers are able to seize only a fraction of the production. To relax this problem, the firm pays a fraction of the wage bill upfront.

[5]Notice that in this case the incumbents' salaries have no effect on the optimal level of hiring. Moreover, we do not allow the firm to fire incumbents. We discuss the effect of this assumption below.

[6]Remember that in our empirical analysis we do not have a direct measure of credit spreads at the loan level. Hence, we model the credit shock as a decrease in credit supply below the level that would be optimally demanded by firms. In any case, the two settings are isomorphic in our simple model, as any suboptimal level of

this case, the firm's working-capital constraint yields the firm's labor demand as:

$$\bar{N} = \frac{(D + \Pi) - \lambda w^I I}{c + \lambda w^N}. \tag{32}$$

The credit tightening has a negative impact on the firm's value, summarized by the shadow value of credit.

Result 1 *A negative credit shock decreases firm value:*

$$\left.\frac{\partial V}{\partial D}\right|_{D=\bar{D}} = \mu = \frac{AF_L(\bar{N} + I, \bar{K}) - w^N - c}{c + \lambda w^N}, \tag{33}$$

where μ is the shadow value of the working-capital constraint (30)*.*

The effect on firm's value through new hires:

$$\frac{\partial N}{\partial D} = \frac{1}{\lambda w^N + c} - \frac{\lambda}{\lambda w^N + c}\left(\frac{\partial w^I}{\partial D} I + \frac{\partial I}{\partial D} w^I\right) \tag{34}$$

highlights that a credit tightening can affect them not only directly, but also through the effect on incumbents wages and employment. Yet, if we assume that the incumbents' wages are fully inflexible (which, at least in nominal terms, is almost entirely the case in Portugal) and that firings are not feasible or optimal (see discussion below), then the term in parentheses of the previous expression cancels out, and we can state the following:

Result 2 *The elasticity of employment to credit around the optimal undistorted quantities is:*

$$\varepsilon_{L,D} = \frac{\partial \log L}{\partial \log D} = \frac{N^*}{N^* + I} + \frac{\lambda w^I I - \Pi}{(\lambda w^N + c)(N^* + I)}. \tag{35}$$

We interpret the first term as the labor-as-investment channel, which depends on how much the firm wants to extend its workforce with respect to the current one (the elasticity of new hires to credit is always 1 in this setting). The second term represents instead the labor-as-leverage channel: The greater the incumbents' wages w^I, or the greater the share of the wage bill λ that has to be paid in advance, or the smaller the retained earnings Π from the previous period, the stronger the amplification of a credit shock from the labor-as-leverage channel will be on new hires.[7] The leverage effect through a firm's exposure to the compensation-policy component of its labor share, which we identify in the empirical analysis, exactly proxies this leverage effect in the model through the greater incumbents' wage level.

As a matter of fact, this amplification result might not be per se a signal of firm unprofitability, as it is possible that retained earnings Π do not fall too much for greater levels of labor share in the past, if the firm has a good operating margin in the aggregate after taking capital costs and other costs into account. In the data, the labor share is indeed negatively correlated with past profits. The variation in labor costs conditional on a certain level of profitability is exactly what the empirical exercise measures. What matters for the leverage effect to materialize is that a firm is exposed to rigid labor costs that it has to honor. Controlling for different degrees of productivity, such as value added per employee and overall TFP, helps distinguishing whether what one observes in the data is the results of simple overpayment or a leverage effect on an otherwise productive firm.[8]

Result 3 *Given Π, a greater incumbents' labor share to finance (proxied by $\lambda w^I I$) amplifies the employment elasticity to credit, and further raises the TFP threshold below which a firm exits the market.*

Discussion Given the necessary complication that analyzing the discontinuity in the hiring/firing decision would entail with respect to the discontinuity in adjustment costs at the 0 point, our model does not include

credit supplied by banks can rationalize an increase in credit spreads through the shadow value of liquidity for the firm.

[7]Keep in mind that λ and Π do not affect the optimal amount of hiring in the undistorted state.

[8]Keep in mind that the employment elasticity does not directly depend on future profitability and is not attenuated by it. If anything, future profitability would lead to a greater effect on impact through the investment channel, as firms would hire more in the optimal undistorted case.

optimal firing due to TFP shocks or market conditions. In principle, the presence of implicit or explicit firing costs would arguably determine a pecking order from the least to the most protected workers. A way to rationalize this could be either by assuming firing costs that are increasing in tenure (which is for sure part of the story in Portugal) or the possibility that tenured incumbents accumulate firm-specific human capital through which they hold up the firm's operations, or general skills that they could use in increasing their bargaining power with the firm. However, these dynamics would not eliminate the leverage effect, given that the workers creating the most leverage are at the same time the most skilled and likely essential to the firm operations.

Incumbents wages w^I can be high for different reasons, which are left unmodeled here given the static nature of the model. Consistently with Card et al. (2017) or Kline et al. (2019), one would expect wages to incorporate part of the training costs in which the firm had incurred in order for the workers to be productive, given their implicit lower substitutability with outsiders. Moreover, risk-sharing consideration between firms and workers might affect wage dynamics (Guiso et al., 2005, 2013). For example, Xiaolan (2014) shows that the optimal contract to provide insurance to workers against negative productivity shocks, while at the same time providing the incentives for high-skill workers to remain in the firm, necessarily implies a degree of downward wage rigidity and an increasing trajectory over time as a function of human capital accumulation. In other words, in Xiaolan's setting downward rigidity in compensation is not determined by explicit firing costs, but by risk-sharing considerations. These kinds of dynamics are exactly what one should have in mind regarding the way in which labor-as-investment turns into labor-as-leverage in our framework.

From the empirical analysis, it is clear that the amplification of a credit shock, which in this setting is isomorphic to a cash-flow shock, is predominantly driven by leverage.[9] Thus, the effects on exit can almost surely be attributable to the labor-as-leverage channel.[10] The labor-as-investment channel instead, absent any non-convexity in training costs, should at most eliminate new hires but, given the presence of capital in place and incumbent workers, it is unlikely to drive exit. Labor-as-investment plausibly creates amplification through the effect that it has on wage dynamics over time, which is not captured in this static model, but does not per se generate a greater volatility of firms' employment and investment decisions to cash-flow shocks. Put differently, in this very simple model working capital is needed to create the leverage effect through incumbents wages, but does not create amplification per se. In fact, training and vacancy costs expose the firms to financing costs, but firms might react to cash-flow shocks by scaling them back in a proportionate way. For example, a firm having to finance 80 percent of the wage bill in advance will exhibit the same elasticity of employment as a firm having to finance only 20 percent, provided that they receive the shock in a similar proportional amount. Hence, the elasticity of hiring to cash-flow, absent other channels, should be 1 in the simple model, which is why the overall elasticity of employment is the hiring share itself.

It is important to point out that, even if one would generally expect the labor-as-investment and labor-as-leverage channels to frequently coexist and possibly reinforce each other, these two channels are separate on a substantive level, and it is conceivable that they can exist independently. In the model, labor-as-leverage implicitly depends on the presence of the working-capital constraint, which makes labor akin to an investment good. However, it is possible to conceive other situations in which its effects would materialize even in the absence of an investment channel. In fact, it is possible that the presence of other fixed or complementary costs at firm level would set the leverage effect in motion, even if workers immediately generate the cash flow needed for their payments. In a similar way, if a firm had projects or other expenses that need priority financing for the firm to operate, the need to finance them would determine a real effect on employment and other forms of investments if costs are rigid. As long as the cash inflows that the workers generate are used by the firm to finance other prioritized payments, the quasi-fixity of labor will once again reverberate on new hiring and even firing decisions

[9]An alternative possibility is that the firm is hit by the credit shock at a point in time when it needs to replace a high share of its incumbent workers, and the credit shock makes it impossible to pay the training costs for new hires, thus leaving the firm in the impossibility to produce. Through such a mechanism, exit would also depend on the labor-as-investment channel. However, in our sample there is no evidence that firms with high incumbents' turnover face higher exit rates. Results are available upon request.

[10]The effect of the credit shock might also translate into a greater volatility of capital investment and eventually a greater exit probability, given that the decrease in D diminishes firm value. In turn, stronger leverage effects from past rigid costs, by distorting more a firm employment decision, might amplify the effect on V, consistently with the empirical results.

for the less attached workers.

All these arguments do not dismiss the labor-as-investment channel as irrelevant: to some extent, the channel creates an exposure to credit fluctuations through investment costs (which might have a leverage effect later on), and the fact that firms do not seem to insure themselves against the exposure to short-term liquidity risk is still puzzling, given that training costs are possibly long-term and easily predictable.

B Data description and cleaning

B.1 Available datasets

B.1.1 Labor market data: Quadros de Pessoal

The *Quadros de Pessoal* (henceforth QP) is a longitudinal matched employer-employee dataset, containing detailed data at the workers' and firms' level on employment composition for the firms and individual worker characteristics. The data are collected and managed by the Ministry of Labour and Social Solidarity, that draws on a compulsory annual census of all the firms employing at least one worker at the end of October each year. It does not cover the public administration and non-market services, whereas it covers partially or fully state-owned firms, provided that they offer a market service. The dataset covers approximately 350,000 firms and 3 million employees per year. In 2010 the structure of the survey was reformed and the QP was incorporated into the *Relatório Único*, an integrated reporting system to enable employers to easily provide more extensive information on workers to the Ministry. As a consequence, some very small entrepreneurial firms were exempted from filing compulsorily the questionnaire, which is why after 2009 the coverage of QP is less complete than in previous years.[11]

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

The worker level dataset provides detailed information on worker characteristics and contracts. Information included comprehends workers' gender, age, detailed occupational code (the *Classificação Nacional de Profissões* (CNP94) up to 2009 and the *Classificação Portuguesa das Profissões* (CPP2010) from 2010 onward, which is based on ISCO08 International Occupational Classification Codes), detailed educational level, qualification within the firm (managerial qualification, specialized workforce or generic workers, besides trainees). At the contract level it is possible to know the precise hiring date, the kind of contract (various typologies that generally define the contract as fixed-term or open-ended), the hours arrangement (full-time versus part-time), the effective number of hours worked, and information on the compensation. More specifically, for each worker it is possible to obtain information on the base pay, any extra paid in overtimes or other extra-ordinary payments and other irregularly paid components. In contrast, there is no information on social security contributions. We winsorize the extreme 0.5% tails of the distribution of wages.[12]

The unique worker identifier is based on the workers' social security number, and given the extensive work on the part of the Ministry to control and certify the quality of the data in this administrative dataset, the coverage and reliability of the data is quite high (except for the discrete break in coverage for only a subset of firms in 2010

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

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

due to the new reporting requirement in the *Relatório Único*. Given that other datasets in our analysis cannot cover the same time-span, we only focus on years from 2005 to 2013 (but potentially control for observables up to 2003 in some empirical exercises).

We use the QP to extract information regarding wage policies at the firm level. This is the dataset we use in order to compute AKM firm level fixed effects, which describe what component of workers' compensation is firm specific and pertains to average firm wage policy. We compute AKM fixed effects thourgh an AKM regression, which is a wage regression at the worker level on firm and worker fixed effects and other workers' characteristics. The firm fixed effect captures overall generosity of payments while the individual fixed effect should capture unobservable skills. In our AKM specifications we control for sex, a third polynomial of age and educational categories dummies, all variables present in QP itself. We run the AKM regressions on worker level data from 2003 to 2008.

B.1.2 Firm level financial statement data: Central de Balanços

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

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

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

two periods). In all the cases we select the criterion of exit, in case a firm matches more than one at different points in time, by looking at the case in which the firm "closed down" with the highest number of employees or, if ties are not resolved, with the lowest EBITDA.

B.1.3 Credit exposure level dataset: Central de Responsabilidades de Crédito

The *Central de Responsabilidades de Crédito* (henceforth CRC), is the credit registry of the Central Bank of Portugal. The dataset features available for our period of analysis (up to 2013) features bank-firm exposures above EUR50 by the universe of Portuguese credit institutions at the monthly level. The dataset does not contain credit exposure by foreign banks towards Portuguese firms, but can obviously contain credit from Portuguese banks to foreign owned firms residing and operating in Portugal.[13]

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

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

The "Balanço das Instituições Monetárias e Financeiras" (henceforth BBS) is the balance-sheet dataset for credit institutions that we employ. It is a proprietary dataset of the Bank of Portugal with the balance sheets of the universe of financial monetary institutions operating in the country. The dataset is utilized by officers of the bank in order to monitor the health of financial monetary institutions operating in the country and the overall stability of the system. In the dataset, for each balance-sheet item (liability or asset) it is possible to see which is the kind of counterparty involved (i.e. the kind of institution, government, private or non-governmental body, creditor or debtor), the maturity of the item in question if relevant (time deposits, on demand deposits, interbank long-term or short-term exposures) and the nationality of the counterparty (extra-EU or each EU country separately). The data are reported at the monthly level.

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

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

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

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

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

B.1.6 Commuting zone definitions

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

B.1.7 Labor market data: Occupational Information Network

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

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

In order to combine the data, we first worked on making profession definitions consistent across time in our dataset, and then merged our occupational code to O*NET through a ISCO08-ONETSOC10 crosswalk. Given the change in occupational codes from the *Classificação Nacional de Profissões* (CNP94) to the new *Classificação Portuguesa das Profissões* (CPP2010) in 2010 in order to update the categorization and making in compliant to the *International Standard Classification of Occupations* (ISCO2008) categorization, we created a crosswalk based on the frequency of cross-occupational code changes from 2009 to 2010 in the QP within the same firms. We used the cross-walk in Hardy et al. (2018) to merge our ISCO08 codes to (ONET)SOC10 (Standard Occupational Codes). We then averaged all the occupational scores and indexes obtained from ONET across occupations in order to obtain a time consistent 3-digits ISCO08 occupational categorization.[14]

[14]The fact that obviously the occupational categorizations are neither bijections nor injections across sets made

We used O*NET version 23.3, and more specifically the education, training and experience files.[15]

B.2 Sample selection

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

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

First, we perform some quality checks on the QP and remove workers' for which identifiers are not consistent over time.[16] We then select only workers listed as "employees", full-time, between 16 and 65 years of age, and receiving a full wage in the October of every year (e.g. not on sick leave or other forms of leave).[17] As regards monetary balance-sheet variables, wages and credit variables, we deflate all nominal values in the analysis by the 2013 consumer price index.[18]

In order to define the final sample of analysis we merge all datasets and select firms based on some defined criteria. Given that we are interested in both firm dynamics and employment adjustment, we mostly consider firms present both in QP and CB.[19] We restrict our attention to firms in mainland Portugal, and exclude from the sample industries like agriculture, fishing, energy (extraction, mining and distribution), the construction sector and the financial sector itself. For the event study we only consider firms with a credit relationship with any bank in 2005, which of course must survive until 2009 to be present in the period of time after the credit shock. We focus on firms with at least 9 employees, which is approximately the threshold for the fourth quartile in the distribution of firms' sizes in the years before 2009, and covers more than 60 percent of the workforce in the QP matched to CRC in the pre-period. In order to reduce measurement noise, we consider only firms with no gaps in the data in the pre-period.[20][21]

We also perform some consistency and sanity checks in selecting the relevant banks to be included in the analysis. More precisely, we exclude from the analysis the very small banks that disappear from the dataset before 2009. We also exclude from the set of banks for which the instrument is computed those banks for which foreign interbank funding is actually intra-banking-group funding from the foreign headquarter to the Portuguese subsidiary.[22]

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it difficult in some cases to reassign the occupational codes. We tried to use the official crosswalk at first, but noticed that it created very big discontinuities for the frequency of observation of some professions. We noticed on the other hand that within firms changes in occupational codes seemed to be very consistent, and as such a more valid "revealed preference" categorization on the part of employers of their employees actual occupation. We then decided to limit ourselves to a 3-digits categorization in order to have a meaningful number of workers for occupation, and in order to minimize the inconsistencies in the cross-categorizations of occupational codes between CNP and CPP.

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

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

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

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

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

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

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

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

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

Our final sample spans 14,846 firms and 31 banks.[23]

C Production function estimation

C.1 Productivity and output elasticities estimation

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

$$y_{i,t} = f(l_{i,t}, k_{i,t}, m_{i,t}) + \omega_{i,t} + \varepsilon_{i,t} \tag{36}$$

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

$$\beta_l l_{i,t} + \beta_k k_{i,t} + \beta_m m_{i,t} \tag{37}$$

in the Cobb-Douglas case, and:

$$\beta_l l_{i,t} + \beta_k k_{i,t} + \beta_m m_{i,t} + \sum_{x \in \{l,k,m\}} \beta_{xx} x_{i,t}^2 + \sum_{j \in \{l,k,m\}, j \neq x} \sum_{x \in \{l,k,m\}} \beta_{jx} j_{i,t} x_{i,t} \tag{38}$$

in the translog case. ω in the equation represents the firm's level of technical efficiency (or total factor productivity, TFP).[24]

In our estimation gross output is measured as total firm sales (coming from QP when available and using CB firm revenues, which correspond to the QP definition of sales, for all other firms), deflated by 2-digit industry gross output deflators. Labor is measured as the firm wage bill (coming from QP when available or using CB total salaries for all other firms), which differently from total headcount (or full-time equivalent count) partially accounts for labor quality, and is deflated by the consumer price index. The intermediate input is the sum of the cost of intermediate goods and supplied services, deflated by 2-digit industry intermediate inputs deflators. For physical capital we use a capital series that we constructed following the perpetual inventory method (PIM) in the baseline specifications or the book value of (net) fixed assets (both tangible and intangible). In the latter case the book value of (net) fixed assets is deflated by 2-digit industry capital goods formation deflator.[25] For the PIM on the other hand we estimate the following equation:

$$K_{i,t} = (1 - \delta_{i,t}) K_{i,t-1} + \frac{I_{i,t}}{def_t} \tag{39}$$

[23]Most of the regressions which require also balance-sheet variables consists of 13,804 firms, while the sample of surviving firms consists of 11,802 firms. At least for the employment and balance-sheet items regressions, though, results are virtually unchanged if we just restrict our attention to specifications in which fixed effects that do not require the CB are utilized (see 4), and cover the entire sample.

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

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

at the firm level. Instead of using the book value of yearly depreciation for fixed assets, we use a level of 7 percent for all firms.[26] From 2009 onwards we can measure directly firm level capital investment form the cash-flow statement (unavailable for earlier years) as the total yearly capital expenditure in both tangible and intangible capital formation. For the other years, or when the variable is missing, we use the variation in book fixed assets, deflated by the yearly capital goods formation deflator, as a measure of investment. We take the earliest year available level of fixed assets, deflated by the industry capital goods formation deflator, as starting value for the series. For incumbents firms in the dataset, the earliest year is 2005, and their starting value of real capital is thus just an approximation. We use the results based on PIM capital as our baseline.[27]

The estimation is carried out yearly, for all firms in the CB from 2005 to 2013, at a level of aggregation that is close to the 2-digit industry level.[28] For the estimation of output elasticities we remove from the dataset firms with a revenue labor share lower than or greater than 1 percent, firms with a revenue material labor share lower than 10 percent or greater than 1, and the firms with a sum of labor and material shares above 1.2. We also drop the lowest and highest 1-percent quantiles of labor and material shares. We are left with 275,093 unique firms and 139,735 firm-year observations on average.

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

$$\xi_{i,t} = \hat{\omega}_{i,t} + \nu_{i,t} = \omega_{i,t} + \varepsilon_{i,t} \tag{40}$$

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

$$\hat{\omega}_{i,t} = \hat{y}_{i,t} - \hat{f}(l_{i,t}, k_{i,t}, m_{i,t}) \tag{41}$$

where $\hat{y}_{i,t}$ is obtained as the estimated gross output from a regression of output on a third order polynomial of all inputs of production. The latter form aims to eliminate any component of the realization of gross output that appears not to be related to the planned input choice, and remains unexplained by it, thus limiting the concern on the influence of measurement error. In the main text we show results based on this latter measure of productivity, but our results are qualitatively unchanged regardless of the measure we use. We use the full residual, as standard in the literature, for the productivity decomposition.[29]

The estimation of output elasticities and productivity generally presents problems related to the nature of input choice itself. On the one hand, input choice is likely to be very strongly correlated with (expected) productivity itself, and as such the direct estimation of the log production function by OLS would very likely be subject to biases given endogeneity determined by simultaneity.[30] On the other hand, there is generally an implicit selection bias for the firms observed in the dataset, given that more productive firm tend to be more resilient in normal times.

We address the first issue by following the literature in industrial organization on the identification by means

[26] 7% is less than the maximum level of depreciation tax deduction that firm would get by deflating capital the most each year. For this reason, even if imperfect, it is a plausible measure of yearly depreciation. Given that we are unable to decompose in a time-consistent way the subcomponents of capital formation, the approximation is necessary.

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

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

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

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

of proxy variables (Olley and Pakes (1996); Levinsohn and Petrin (2003)).[31] This methodology consists into substituting unobserved productivity in the production function by a proxy variable, a choice variable assumed to have an invertible mapping with productivity itself. In our case, we use the intermediate input as the proxy variable (as in Levinsohn and Petrin (2003)).

The estimation is subdivided in two stages: in the first stage output is non-parametrically regressed on the inputs (and importantly, the proxy variable, which is an input in our case), in order to retrieve expected output and an estimate of the residual:[32]

$$y_{i,t} = \phi(l_{i,t}, k_{i,t}, m_{i,t}) + \varepsilon_{i,t} \tag{42}$$

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

$$\omega_{i,t} = g(\omega_{i,t-1}) + \eta_{i,t} \tag{43}$$

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

$$\hat{\omega}_{i,t}(\beta) = \hat{\phi} - (\hat{\beta}_l l_{i,t} + \hat{\beta}_k k_{i,t} + \hat{\beta}_m m_{i,t}) \tag{44}$$

for the Cobb-Douglas case of

$$\hat{\omega}_{i,t}(\beta) = \hat{\phi} - \left(\hat{\beta}_l l_{i,t} + \hat{\beta}_k k_{i,t} + \hat{\beta}_m m_{i,t} + \sum_{x \in \{l,k,m\}} \hat{\beta}_{xx} x_{i,t}^2 + \sum_{j \in \{l,k,m\}, j \neq x} \sum_{x \in \{l,k,m\}} \hat{\beta}_{jx} j_{i,t} x_{i,t} \right) \tag{45}$$

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

$$\mathbb{E}[\eta_{i,t}(\beta) z^j] = 0 \qquad j \in \{l, k, m\} \tag{46}$$

in the Cobb Douglas case and

$$\mathbb{E}[\eta_{i,t}(\beta) z^j] = 0 \qquad j \in \{l, k, m\} \tag{47}$$

$$\mathbb{E}[\eta_{i,t}(\beta) z^j z^h] = 0 \qquad j, h \in \{l, k, m\} \tag{48}$$

in the translog case. The z variables are instruments for the various inputs. Given the standard assumptions on input dynamics, k can be a valid instrument of itself, whereas we use lags of labor and intermediate inputs as instruments, and according interactions for higher order terms.[33][34][35]

We address the problem of possible selection bias of firms into the dataset by trying to control for the probability of survival in the law of motion of productivity, as suggested in Olley and Pakes (1996). We actually augment the estimation of Equation (43) by adding the estimated survival probability obtained by fitting a probit model on year dummies and input levels.[36]

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

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

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

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

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

[36] We carried out a the same procedure by augmenting Equation (43) with the estimated failure probability as in Antunes et al. (2016), but did not notice any material difference in final outcomes.

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

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

$$\hat{\theta}^x_{i,t} = \hat{\beta}_x + 2\hat{\beta}_{xx}x_{i,t} + \hat{\beta}_{xj}j_{i,t} + \hat{\beta}_{xh}h_{i,t} \tag{49}$$

Tables D.22 and D.23 show average estimates of input elasticities using all the different estimation methodologies, and with different measures of the capital input. Reassuringly, the estimated elasticities and markups are in line with the recent studies performing similar estimations (Blattner et al. (2019) for Portugal, Fonseca and Van Doornik (2019) for Brazil and Lenzu and Manaresi (2018) for Italy).

C.2 Markups and marginal products

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

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

$$\hat{\mu}_{i,t} = \hat{\theta}^m_{i,t}\left(\frac{P_{i,t}Q_{i,t}}{P^m_{i,t}M_{i,t}}\right) \tag{50}$$

As in De Loecker and Warzynski (2012), we can only imperfectly measure the expenditure share of materials in gross output, given the likely presence of measurement error in the estimation of Equation (36). For this reason, we divide gross output in equation (50) by $\exp(\hat{\varepsilon}_{i,t})$, the residual from the first stage regression in the production function estimation procedure. Per De Loecker and Warzynski (2012) this correction helps eliminating any variation in expenditure shares coming from variation in output not correlated with $\phi(l_{i,t}, k_{i,t}, m_{i,t})$, that is "output variation not related to variables impacting input demand".[37]

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

$$MRP^X_{i,t} \equiv \frac{\partial(P_{i,t}(Q_{i,t})Q_{i,t})}{\partial X_{i,t}} = \underbrace{P_{i,t}\frac{\partial Q_{i,t}}{\partial X_{i,t}}}_{VMP^X_{i,t}}\underbrace{\left(1 + \frac{Q_{i,t}}{P_{i,t}}\frac{\partial P_{i,t}}{\partial Q_{i,t}}\right)}_{\mu^{-1}_{i,t}} = \theta^X_{i,t}\frac{P_{i,t}Q_{i,t}}{X_{i,t}}\frac{1}{\mu_{i,t}} \tag{51}$$

and as such MRP - cost gaps as

$$\text{MRPK-cost gap}_{i,t} = \hat{\theta}^k_{i,t}\frac{P_{i,t}Y_{i,t}}{K_{i,t}}\frac{1}{\hat{\mu}_{i,t}} - R_{i,t} \tag{52}$$

$$\text{MRPL-cost gap}_{i,t} = \hat{\theta}^l_{i,t}\frac{P_{i,t}Y_{i,t}}{L_{i,t}}\frac{1}{\hat{\mu}_{i,t}} - W_{i,t} \tag{53}$$

$R_{i,t}$ consists of the depreciation rate, which we keep at 7 percent as in the PIM exercise, and the average interest

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

rate paid by the firm on its debt, which is the ratio of interest expenditures to total debt. When the information is missing, similarly to Fonseca and Van Doornik (2019) we impute interest rates as the average yearly interest rate at the 2-digit industry level.[38] For the average wage $W_{i,t}$, we divide the total wage bill by the number of employees (either taken from the QP when available, or as the full-time equivalent count in the CB for the remaining firms).[39]

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

Table D.24 displays our estimates of costs, marginal revenue products and gaps. Even in this case, quite reassuringly, our estimates of gaps are in the same ballpark of magnitude of recent studies performing similar exercises (Blattner et al. (2019) for Portugal, Fonseca and Van Doornik (2019) for Brazil and Lenzu and Manaresi (2018) for Italy).

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

[39]For this estimation, one would ideally want to have more precise estimates of the marginal costs of inputs of production than the average yearly estimates of firm wage and user cost of capital. Reassuringly, studies in which data allow to gauge the distinction between average and marginal cost levels do not seem to find dramatic differences in gaps estimated according to the different costs definitions (see Lenzu and Manaresi (2018) for the difference in estimated gaps using average versus marginal wages.

D Appendix Tables

Table D.1: Average prevailing interest rates on non-financial bank loans

	ST rates	MT rates	LT rates	Euribor - 1y
2005	4.57	4.14	4.04	2.33
2006	5.23	4.72	4.66	3.44
2007	6.13	5.58	5.54	4.45
2008	6.76	6.12	5.97	4.81
2009	4.69	4.10	3.78	1.62
2010	4.23	3.32	2.99	1.35
2011	5.72	4.55	3.85	2.00
2012	6.33	5.16	3.87	1.11
2013	5.91	4.99	3.46	0.54

Yearly (simple) average of monthly rates on loans for non-financial corporations.
Short term loans defined as loans with maturity less than one year. Medium term loans defined as loans with maturity between 1 and 5 years. Long term loans defined as loans with maturity greater than 5 years.
Source: Monetary and financial statistics, Bank of Portugal.

Table D.2: Share of secured credit, firm level

	(1) ST	(2) LT	(3) Diff.
Sh. secured loans	0.25	0.50	
Sh. secured credit	0.40	0.62	0.22***
	(0.43)	(0.40)	138.46
Sh. fully secured credit	0.21	0.38	0.17***
	(0.33)	(0.40)	122.03
Sh. financial collateral	0.04	0.07	0.03***
	(0.18)	(0.21)	33.11
Sh. real collateral mortgage	0.07	0.21	0.14***
	(0.23)	(0.35)	121.83
Sh. personal guarantee	0.37	0.50	0.14***
	(0.42)	(0.42)	83.50
N	140592	131845	

The table refers to all firms in a CRC-QP match in the sectors of analysis (excluding, agriculture, mining, energy production and distribution and construction). The share of secured loans refers to the actual share of loans that have any sort of collateralization backing it. All other statistics are aggregated at the firm level and refer to shares of total regular credit available to firms.
Column (3) shows the results of t-test for the difference of the means, T-stats reported.

Table D.3: Firm level descriptive statistics, sample of analysis - workforce composition

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

Descriptive statistics for the full (unbalanced) sample of analysis, with N=14,864 distinct firms. All workforce decomposition variables from QP.

Table D.4: Robustness: instrument effects on credit post-2010

	(1)	(2)	(3)	(4)	(5)
			$\Delta D_{st,2013-2010}$		
$\Delta D_{i;st,2010-2006}$	-0.220***	-0.231***	-0.253***	-0.254***	-0.251***
	(0.011)	(0.011)	(0.012)	(0.012)	(0.012)
Z_i	0.240	-0.041	-0.148	-0.176	-0.154
	(0.277)	(0.257)	(0.260)	(0.262)	(0.261)
W. Sov.				-0.980+	
share in Q4-2009, 2005 banks				(0.588)	
W. Sov.					-1.242*
share in Q4-2009, 2009 banks					(0.629)
Firms	12883	12865	12061	12061	11882
Fixed effects	No	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes	Yes

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

Table D.5: Employment regressions, first stage

	(1)	(2)	(3)	(4)	(5)
			S_i		
Z_i	-1.761***	-1.706***	-1.716***	-1.702***	-1.766***
	(0.301)	(0.276)	(0.280)	(0.285)	(0.293)
Firms	14846	14830	13833	13806	11802
Sample	Complete	Complete	Complete	Complete	Survivors
Fixed effects	No	Yes	Yes	Yes	Yes
Controls	No	No	Fail b.c.	Yes	Yes

The table shows the estimation results of the first stage regressions of the credit variation S_i on the instrument Z_i. All regressions feature firm and time fixed effects.
See the table 4 for the list of controls and fixed effects in the regressions.
Standard errors clustered at the bank-industry pair level.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.6: Employment regressions, reduced form

	(1)	(2)	(3)	(4)
		$log(\#emp)_{i,t}$		
Z_i	-0.123**	-0.120**	-0.120**	-0.152***
	(0.051)	(0.052)	(0.053)	(0.054)
Firms	14830	13833	13806	11802
Sample	Complete	Complete	Complete	Survivors
Fixed effects	Yes	Yes	Yes	Yes
Controls	No	Fail b.c.	Yes	Yes

All regressions feature firm and time fixed effects. See the table 4 for the list of controls and fixed effects in the regressions.
Standard errors clustered at the bank-industry pair level.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.7: Hours regressions

	(1)	(2)	(3)	(4)
	$log(\#\ hours)_{i,t}$		$log(\#\ base\ hours)_{i,t}$	
S_i	0.067*	0.085*	0.067*	0.085*
	(0.034)	(0.035)	(0.034)	(0.035)
Firms	13806	11802	13806	11802
EFF. WID F	34.56	35.55	34.56	35.55
Sample	Complete	Survivors	Complete	Survivors

The dependent variables are either the total amount of work hours (columns 1 and 2) or the total amount of normal contract hours, which does not comprehend extraordinary or overtime hours of work.
See the table 4 for the list of controls and fixed effects in the regressions. All regressions feature the full set of fixed effects and controls.
Standard errors clustered at the bank-industry pair level.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.8: Employment - wage bill regressions

	(1)	(2)	(3)	(4)	(5)	(6)
	$Employees_{i,t}$		$Wage\ bill_{i,t}$		$Base\ wage\ bill_{i,t}$	
S_i	0.069*	0.083*	0.100**	0.122**	0.102**	0.116**
	(0.031)	(0.033)	(0.038)	(0.041)	(0.038)	(0.040)
Firms	13806	11802	13806	11802	13806	11802
EFF. WID F	34.56	35.55	34.56	35.55	34.56	35.55
Sample	Complete	Survivors	Complete	Survivors	Complete	Survivors

The dependent variable in these regressions is the ratio of the number of workers (or the wage bill) to the average level of the pre-period corresponding amount.
The outcome variable is winsorized at the top 1% level.
See the table 4 for the list of controls and fixed effects in the regressions. All regressions feature the full set of fixed effects and controls.
Standard errors clustered at the bank-industry pair level.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.9: Heterogeneous employment regressions: Contracts

	(1)	(2)	(3)	(4)
	$Permanent\ w_{\cdot i,t}$		$Temporary\ w_{\cdot i,t}$	
S_i	0.098+	0.158*	0.174	0.145
	(0.057)	(0.062)	(0.147)	(0.150)
Firms	13657	11681	11829	10129
EFF. WID F	37.00	38.37	31.02	32.94
Sample	Complete	Survivors	Complete	Survivors

The dependent variable in these regressions is the ratio of the number of specific workers to the average level of the pre-period corresponding amount. As such, the regressions are defined only for the firms for which the kind of worker is present in the pre-period (even if missing values for some years are possible).
Temporary workers are workers hired with a fixed-term contract.
The outcome variable is winsorized at the top 1% level.
See the table 4 for the list of controls and fixed effects in the regressions. All regressions feature the full set of fixed effects and controls.
Standard errors clustered at the bank-industry pair level.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.10: Heterogeneous employment regressions: Education

	(1)	(2)	(3)	(4)	(5)	(6)
	$High\ educ._{i,t}$		$Medium\ educ._{i,t}$		$Low\ educ._{i,t}$	
S_i	0.030	-0.048	0.222**	0.271***	0.057	0.065+
	(0.138)	(0.133)	(0.071)	(0.080)	(0.036)	(0.036)
Firms	9539	8228	13699	11710	12679	10833
EFF. WID F	30.45	32.25	33.01	34.07	38.51	41.89
Sample	Complete	Survivors	Complete	Survivors	Complete	Survivors

The dependent variable in these regressions is the ratio of the number of specific workers to the average level of the pre-period corresponding amount. As such, the regressions are defined only for the firms for which the kind of worker is present in the pre-period (even if missing values for some years are possible).
High educated obtained at least an undergraduate degree. Medium educated workers completed high school or an equivalent level professional school. Low educated workers did not complete high school or an equivalent professional school.
The outcome variable is winsorized at the top 1% level.
See the table 4 for the list of controls and fixed effects in the regressions. All regressions feature the full set of fixed effects and controls.
Standard errors clustered at the bank-industry pair level.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.11: Robustness: Almeida et al. (2011) identification, effects on fixed investments

	(1)	(2)
	$log(fixed\ assets)_{i,t}$	
S_i	0.017	0.031
	(0.074)	(0.120)
$S_i \cdot exp\_lt_i$		-0.025
		(0.119)
exp_lt_i	-0.090***	-0.095***
	(0.015)	(0.026)
Firms	11799	11799
Sample	Survivors	Survivors

The regressions refer to the empirical specification in equation (8) in the text.
The treatment is interacted with a dummy variable equal to 1 if the firm has long-term debt maturing in the first two semester of 2009. As a consequence, we instrument the interacted treatment with the interaction of the same dummy and the baseline instrument Z_i. We also control for the dummy variable itself, interacted with the post-period dummy to allow for different rends depending on the long-term debt maturing soon after the credit shock.
The regressions are carried out on the sample of firms surviving up to 2013.
See Table 4 for a list of the added controls and fixed effects present in the regressions. All regressions feature the full set of fixed effects and controls.
Standard errors clustered at the bank-industry pair level.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.12: Average wage regressions

	(1)	(2)
	$log(Avg.\ wage)_{i,t}$	$log(Avg.\ wage)_{inc08;i,t}$
S_i	0.018	0.023
	(0.015)	(0.017)
Firms	13806	13804
EFF. WID F	34.56	34.56
Sample	Complete	Complete

The dependent variable for column 1 is the logarithm of the average wage for all employees. The dependent variable for column 3 is the logarithm of the average wage for incumbent workers in 2008 who remain in the firm. See the table 4 for the list of controls and fixed effects in the regressions. All regressions feature the full set of fixed effects and controls.
Standard errors clustered at the bank-industry pair level.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.13: Regressions by attachment

	(1)	(2)
	$log(Att.\ empl.)_{i,t}$	$log(Low\ att.\ empl.)_{i,t}$
S_i	0.040	0.104*
	(0.028)	(0.049)
Firms	13726	13198
EFF. WID F	37.21	36.63
Sample	Complete	Complete

The dependent variable for column 1 is the logarithm of the number of attached employees in 2008, whereas the dependent variable for column 2 is the logarithm of the number of less attached employees in 2008. We define attached workers as workers present in the firm for the entirety of the pre-period. We define less attached employees as all other incumbent employees present in the firm in 2008.
See the table 4 for the list of controls and fixed effects in the regressions. All regressions feature the full set of fixed effects and controls.
Standard errors clustered at the bank-industry pair level.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.14: Loan level regressions: productivity

	(1)	(2)	(3)	(4)
		$\Delta D_{i;st,pre-post}$		
FD_b, $Low\ TFP$	-2.237***	-2.347***	-2.318***	-2.235***
	(0.276)	(0.308)	(0.256)	(0.264)
, $Med.\ TFP$	-1.941***	-2.314***	-2.278***	-2.457***
	(0.309)	(0.312)	(0.268)	(0.274)
, $High\ TFP$	-2.376***	-1.927***	-2.346***	-2.245***
	(0.294)	(0.274)	(0.275)	(0.263)
Firms	9206	9206	12703	12703
Firm FE	Yes	Yes	No	No
Other FE	No	No	Yes	Yes
TFP Measure	CD ACF	TSLOG ACF	CD ACF	TSLOG ACF

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

Table D.15: Regressions by CD productivity bins (Ackerberg et al. (2015))

	(1)	(2)
	$log(\#emp)_{i,t}$	$P(exit)_{i,t}$
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 Table 4 for a list of the added controls and fixed effects present in the regressions. All regressions feature the full set of fixed effects and controls. In addition to that specification we control for average TFP in 2005 and 2006 (estimated 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. Moreover, given that we cannot control for unobservable characteristics in the exit specification through firm fixed effects any more, we try to 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 failed up until the year 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.
Sample sizes depend on availability of non-missing variables in CB.
Standard errors clustered at the bank-industry pair level.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.16: Regressions by OLS CD productivity bins

	(1) $log(\#emp)_{i,t}$	(2) $P(exit)_{i,t}$
S_i, $Low\ TFP$	0.048	-0.023+
	(0.037)	(0.012)
, $Med.\ TFP$	0.104*	-0.026+
	(0.042)	(0.016)
, $High\ TFP$	0.088*	-0.019
	(0.045)	(0.017)
Firms	13287	13277
WID F	10.91	10.64
Sample	Complete	Complete
Firm FE	Yes	No
Other FE	Yes	Yes

See Table 4 for a list of the added controls and fixed effects present in the regressions. All regressions feature the full set of fixed effects and controls. In addition to that specification we control for average TFP in 2005 and 2006 (estimated by OLS 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. Moreover, given that we cannot control for unobservable characteristics in the exit specification through firm fixed effects any more, we try to 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 failed up until the year 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.
Sample sizes depend on availability of non-missing variables in CB.
Standard errors clustered at the bank-industry pair level.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.17: Regressions by OLS TSLOG productivity bins

	(1) $log(\#emp)_{i,t}$	(2) $P(exit)_{i,t}$
S_i, $Low\ TFP$	0.079*	-0.029*
	(0.040)	(0.015)
, $Med.\ TFP$	0.070+	-0.019
	(0.038)	(0.013)
, $High\ TFP$	0.080+	-0.020
	(0.041)	(0.015)
Firms	13287	13277
WID F	10.87	11.50
Sample	Complete	Complete
Firm FE	Yes	No
Other FE	Yes	Yes

See Table 4 for a list of the added controls and fixed effects present in the regressions. All regressions feature the full set of fixed effects and controls. In addition to that specification we control for average TFP in 2005 and 2006 (estimated by OLS by means of a three factors of production gross output translog production function). TFP can be estimated for less firms than the full samples depending on availability of the variables to compute it in CB. Moreover, given that we cannot control for unobservable characteristics in the exit specification through firm fixed effects any more, we try to 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 failed up until the year 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.
Sample sizes depend on availability of non-missing variables in CB.
Standard errors clustered at the bank-industry pair level.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.18: Regressions by TSLOG productivity bins (Ackerberg et al. (2015))

	(1) $log(\#emp)_{i,t}$	(2) $P(exit)_{i,t}$
S_i, $Low\ TFP$	0.080*	-0.034*
	(0.039)	(0.015)
, $Med.\ TFP$	0.077*	-0.015
	(0.037)	(0.012)
, $High\ TFP$	0.072	-0.022
	(0.045)	(0.017)
Firms	13287	13277
WID F	11.12	11.59
Sample	Complete	Complete
Firm FE	Yes	No
Other FE	Yes	Yes

See Table 4 for a list of the added controls and fixed effects present in the regressions. All regressions feature the full set of fixed effects and controls. In addition to that specification we control for average TFP in 2005 and 2006 (estimated by the Ackerberg et al. (2015) methodology by means of a three factors of production gross output translog production function). TFP can be estimated for less firms than in the full samples depending on availability of the variables to compute it in CB. Moreover, given that we cannot control for unobservable characteristics in the exit specification through firm fixed effects any more, we try to 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 failed up until the year 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.
Sample sizes depend on availability of non-missing variables in CB.
Standard errors clustered at the bank-industry pair level.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.19: Reallocation and TFP by labor share - full dataset

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

The regressions refer to the empirical specification in equation (20) in the text. A different coefficient is jointly estimated for each labor share bin, and a variation of slope is estimated for the years post 2008.
Labor share is computed as the average ratio of employment costs over value added for the years from 2005 to 2008. In all specifications a control for the average value added per employee in the period from 2005 to 2008 is added.
All regressions feature 3-digits industry fixed effects, and labor share quartile by post-Lehman dummy fixed effects.
The sample consists of all firms in QP matched with CB for which TFP can be computed (with the exclusion of the energy and construction sector).
All variables refer to the outcomes from t to $t+1$. We measure employment either as total headcount of full time equivalent employment, as reported in CB. The exit regression excludes the year 2005, given the CB structure.
Standard errors clustered at the 3-digits industry level.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.20: Marginal revenue products - cost gaps regressions

	(1)	(2)	(3)
	$MRPL_{i,t}$	$MRPL_{book;i,t}$	$Avg.\ wage_{i,t}$
S_i	-0.993+	-0.987	0.018
	(0.599)	(0.606)	(0.015)
Firms	11710	12005	13806
WID F	29.34	29.06	35.63
Sample	Complete	Complete	Complete

MRPLs are expressed in thousand euros.
In columns 1 the MRPL is obtained given estimation of output elasticities with capital computed through PIM.
In column 2 MRPL is obtained given estimation of output elasticities with book value of capital.
Outcome variables are winsorized at 0.5^{th} and 99.5^{th} percentiles.
See Table 4 for a list of the added controls and fixed effects present in the regressions. All regressions feature the full set of fixed effects and controls.
Regressions are run on the full sample for all firms for which it was possible to calculate the MRP-cost gaps.
Standard errors clustered at the bank-industry pair level.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.21: Wedge regressions

	(1)	(2)	(3)
	$Lab.\ wedge_{i,t}$	$Cap.\ wedge_{i,t}$	$Mat.\ wedge_{i,t}$
S_i	-0.0080+	0.0023	-0.0081
	(0.0047)	(0.0049)	(0.0092)
Firms	11710	12823	12988
WID F	29.34	35.37	34.90
Sample	Complete	Complete	Complete

Outcome variables are winsorized at 0.5^{th} and 99.5^{th} percentiles.
See Table 4 for a list of the added controls and fixed effects present in the regressions. All regressions feature the full set of fixed effects and controls.
Regressions are run on the full sample for all firms for which it was possible to calculate the input wedges.
Standard errors clustered at the bank-industry pair level.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.22: Revenue elasticities and markups, PIM capital

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

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

Table D.23: Revenue elasticities and markups, book v. of capital

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

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

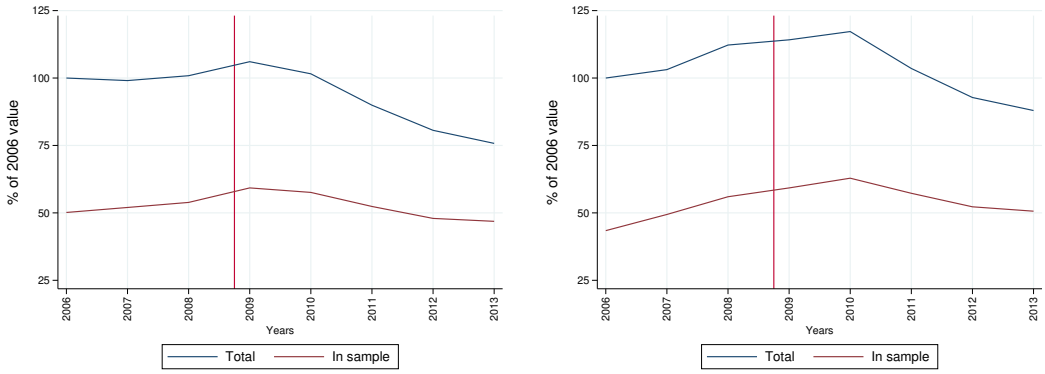
Table D.24: MRPs, user costs and gaps, full CB

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

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

E Appendix Figures

Figure E.1: Credit dynamics in Portugal

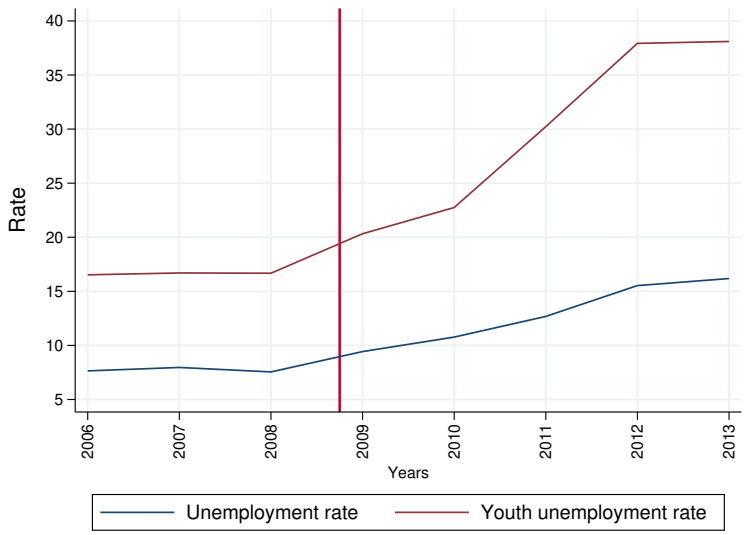


(a) Total regular credit

(b) Long-term regular credit

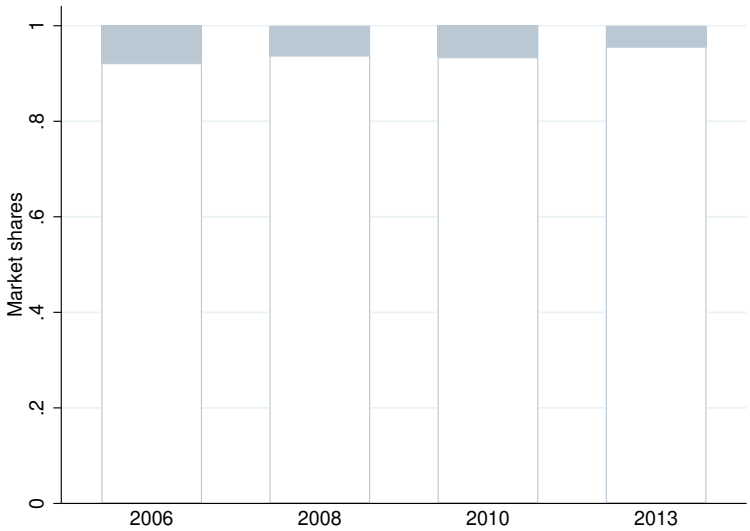
The Figures show the time series for the aggregate amount of total regular (left) and long-term credit (right) for the firms and banks in the sample. Total regular credit is credit not overdue or in renegotiation available to the firm. Long-term credit is any credit exposure with maturity greater than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

Figure E.2: Unemployment rate



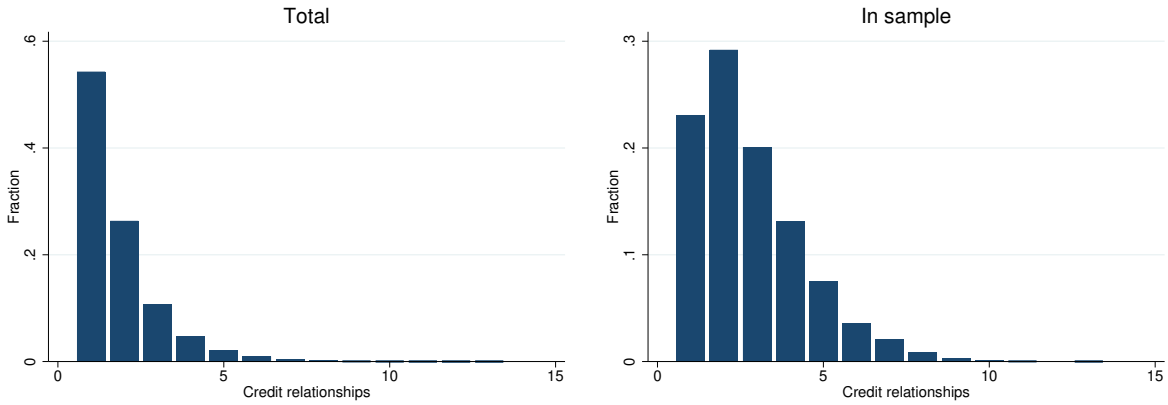
The Figure shows the time series of the unemployment rate in Portugal, both for all workers (blue) and for workers below 25 years old (red).
Source: OECD.

Figure E.3: Bank concentration by total regular credit



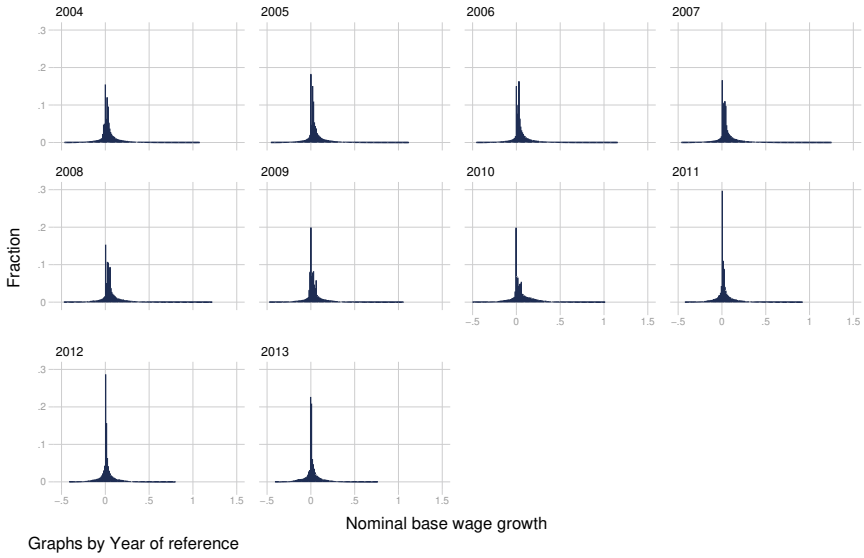
Banking groups market shares in terms of total regular credit, top 10 vs. all other banks. The sample of reference is all firms in QP with credit. Only banks surviving up to 2009 are included, consistently with the sample definition for the empirical event study.
Source: *Central de Responsabilidades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

Figure E.4: Number of credit relationships



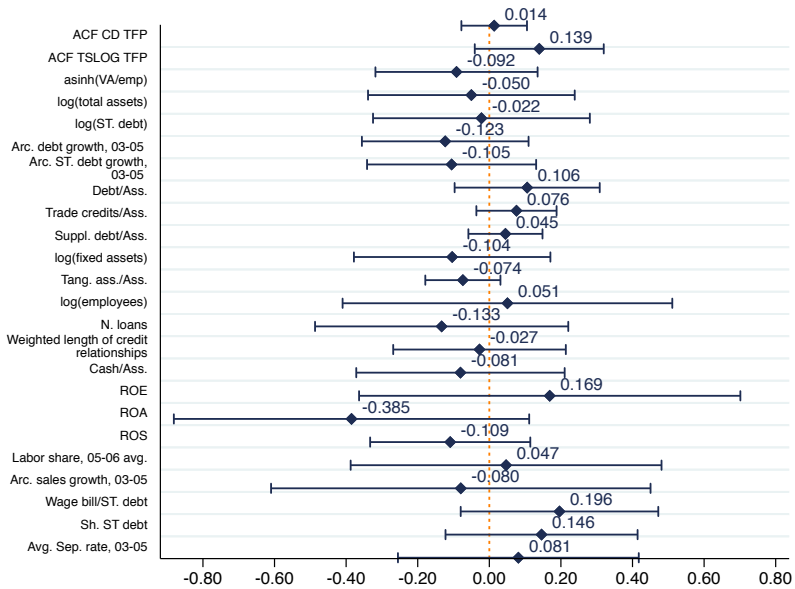
The Figure shows the distribution of the number of credit relationships by firm in 2005 for all firms with credit and in the QP (left) and for the firms in the sample of analysis (right).
Source: *Central de Responsabilidades de Crédito* merged with *Quadros de Pessoal*, authors' calculations and sample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

Figure E.5: Distribution of nominal base-wage growth in Portugal



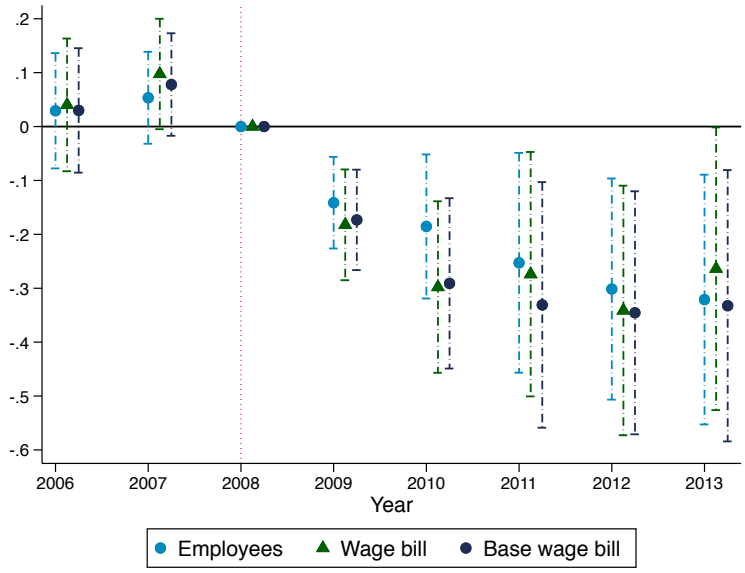
Distribution of the nominal base-wage growth at the worker's level in Portugal, by year. Base wage comprehends only regular compensations, without accounting for any overtime payments, and refers to the October of each year. Source: Full *Quadros de Pessoal* dataset, cleaned for inconsistent workers' identifiers. Authors' calculations and sample selection.

Figure E.6: Balance checks (Borusyak et al., 2019)



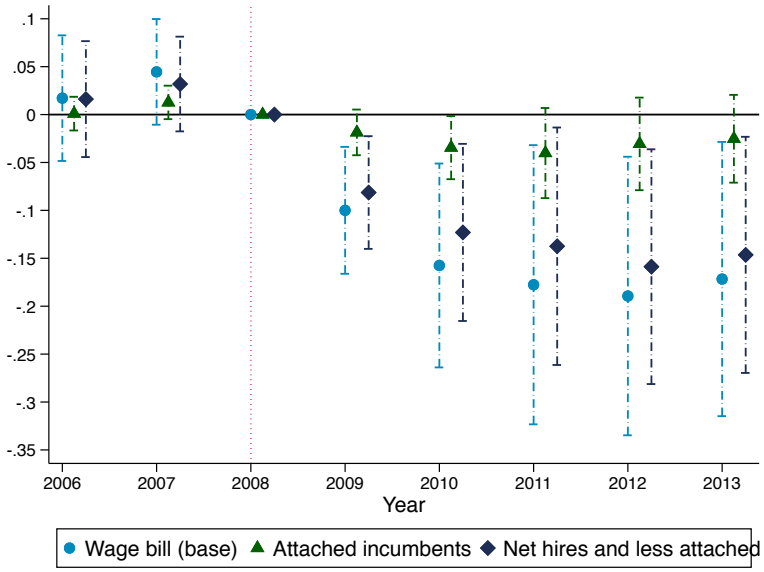
The Figure shows the coefficients (with 95% confidence intervals) of pairwise regressions of the standardized value of each variable in 2005 (unless reported otherwise) on the (standardized value of the) instrument Z_i. The regressions are run at the bank level, and all regressors are weighted bank exposures to firm characteristics, according to the method exposed in (Borusyak et al., 2019). Before weighting firm characteristics at the bank level, the variables are regressed on the fixed effects used throughout the analysis in the paper (see Table 4 for a list), and residuals are calculated and used in the analysis. Standard errors robust to heteroskedasticity.

Figure E.7: Employment and wage-bill regressions: event study, reduced form



The dependent variables in these regressions are the ratios of the number of employees and the wage bill (total and base) over the average of their levels in the pre-period (2006-2008). In the specifications the coefficient for the year 2008 are normalized to 0, so that all the other coefficients have to be interpreted as the effect on the percentage variation of employment or wage bill with respect to the 2008 level.
See Table 4 for the list of controls and fixed effects in the regressions. All regressors and fixed effects are interacted with a year dummy.
Number of firms: 11,802, survivors sample.
95% confidence intervals displayed, standard errors clustered at the bank-industry pair level.

Figure E.8: Wage bill adjustment by tenure: event study

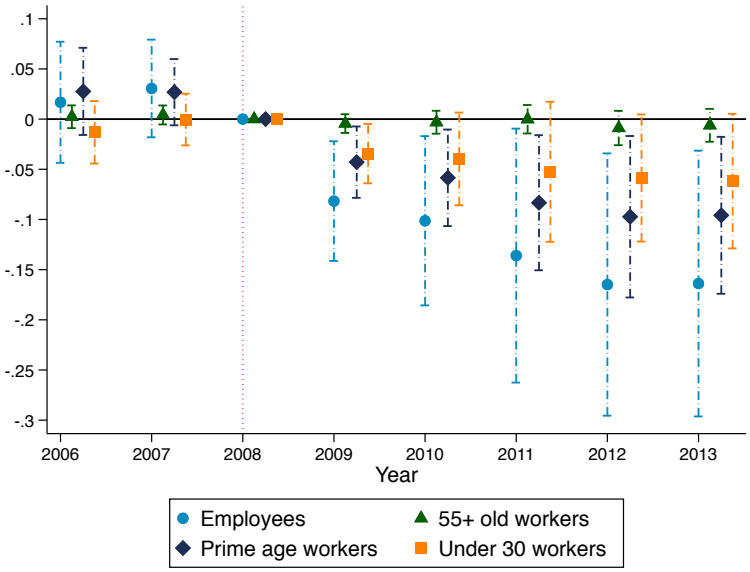


The dependent variables in these regressions are the ratio of the the base-wage bill going to employees of the specific category over the average level of base-wage bill in the pre-period (2006-2008). By construction, the sum of the coefficients for the attached incumbents and net hires should be equal to the overall base-wage bill effect. In the specifications the coefficients for 2008 are normalized to 0, so that all the other coefficients should be interpreted as the effect on the percentage variation of (each kind of) wage bill with respect to the 2008 level.
Attached incumbents are defined as workers present at the firm for the entirety of the pre-period, and net hires are all other workers (less attached workers or hires/separations in the post period). In order to get a sense of the implied elasticities of adjustment, one should divide the estimated coefficient by the share of wage bill going to attached workers in the pre-period.
The sample includes only survivor firms (N = 11,809), but is not balanced. The graph displays the effect of a negative shock.
See Table 4 for the list of controls and fixed effects present in the regressions. All regressors and fixed effects are interacted with a year dummy.
95% confidence intervals displayed, standard errors clustered at the bank-industry pair level.

Figure E.9: Qualifications regressions, event study

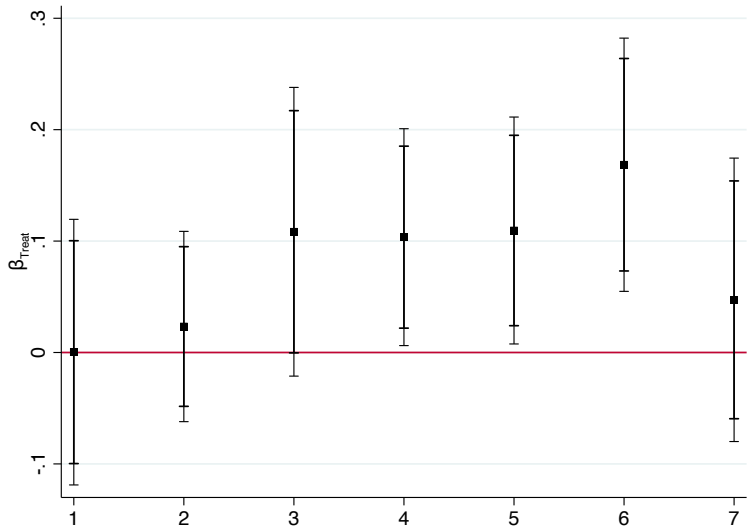
The dependent variables in these regressions are the ratios of the total number of employees and the number of employees by qualification over the average number of employees in the pre-period (2006-2008). See Appendix B.1.1 for more details. In the specifications the coefficient for the year 2008 are normalized to 0, so that all the other coefficients have to be interpreted as the effect on the percentage variation of employment with respect to the 2008 level.
See Table 4 for the list of controls and fixed effects in the regressions. All regressors and fixed effects are interacted with a year dummy.
See Table D.3 for descriptives statistics on the relative shares of workers in the workforce in the pre- and post-period.
Number of firms: 11,802, survivors sample. Pre-period sh. of managers: 13%. Share of specialized workers: 33%. 95% confidence intervals displayed, standard errors clustered at the bank-industry pair level.

Figure E.10: Age regressions, event study



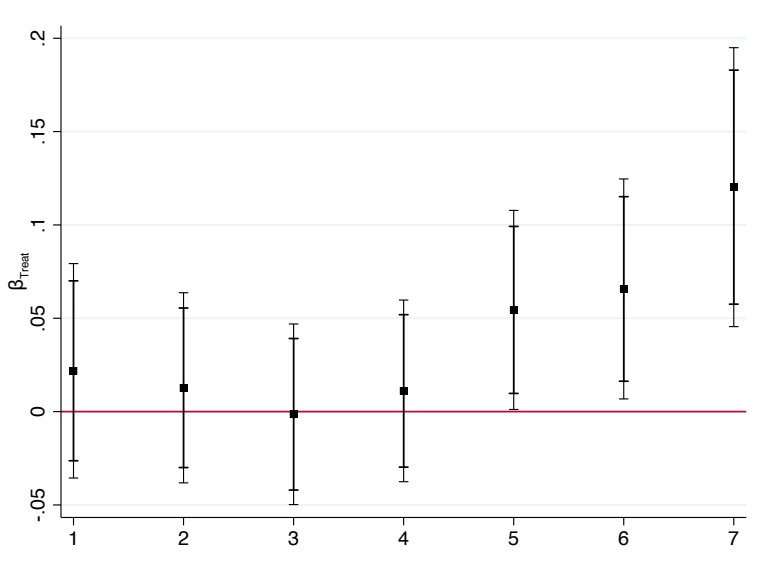
The dependent variables in these regressions are the ratios of the total number of employees and the number of employees by age over the average number of employees in the pre-period (2006-2008). See Appendix B.1.1 for more details. In the specifications the coefficient for the year 2008 are normalized to 0, so that all the other coefficients have to be interpreted as the effect on the percentage variation of employment with respect to the 2008 level.
See Table 4 for the list of controls and fixed effects in the regressions. All regressors and fixed effects are interacted with a year dummy.
See Table D.3 fothe r descriptives statistics on the relative shares of workers in the workforce in the pre- and post-period.
Number of firms: 11,802, survivors sample. Pre-period share of under 31: 25%. Share of above 54: 8%.
95% confidence intervals displayed, standard errors clustered at the bank-industry pair level.

Figure E.11: Employment regressions by labor-share bins, survivors sample

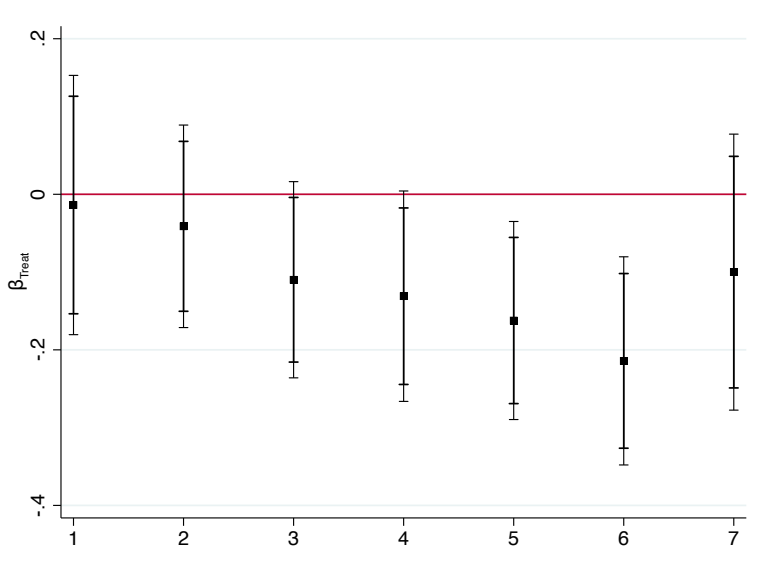


We estimate a coefficient for each of the seven labor-share bins, while controlling linearly for baseline effects. Each interacted treatment is instrumented by the interacted instrument. Labor share is defined as the ratio between employment-related costs and total value added (average of 2005-2006 levels).
See Table 4 for the list of controls and fixed effects in the regressions. All fixed effects and controls are interacted with a *post* dummy.
Number of firms: 11,768, survivors sample.
95 and 90% confidence intervals displayed, standard errors clustered at the bank-industry pair level.

Figure E.12: Regressions by labor-share bins, reduced form



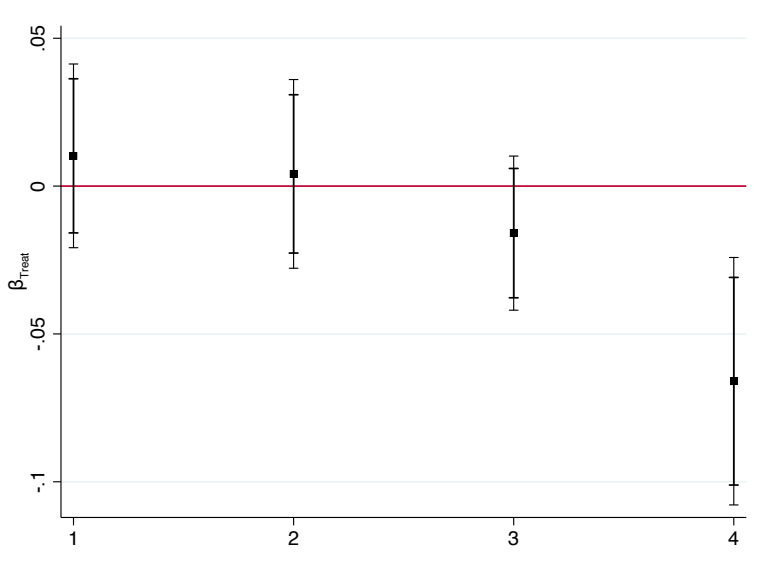
(a) Exit



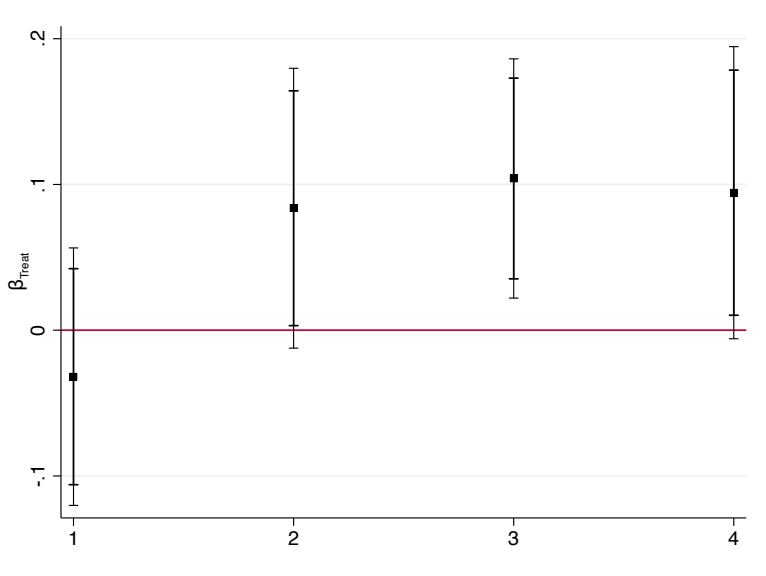
(b) Employment

We estimate a coefficient for each of the seven labor-share bins, while controlling for baseline effects linearly and for the interacted instruments. Labor share is defined as the ratio between employment-related costs and total value added (average of 2005-2006 levels).
See Table 4 for the list of controls and fixed effects in the regressions. Given that we cannot control for unobservable characteristics in the exit specifications through firm fixed effects any more, 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 specification. In the employment specification all fixed effects and controls are interacted with a *post* dummy.
Number of firms: 13,750 (exit), 13,760 (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.

Figure E.13: Regressions by labor-share quartiles



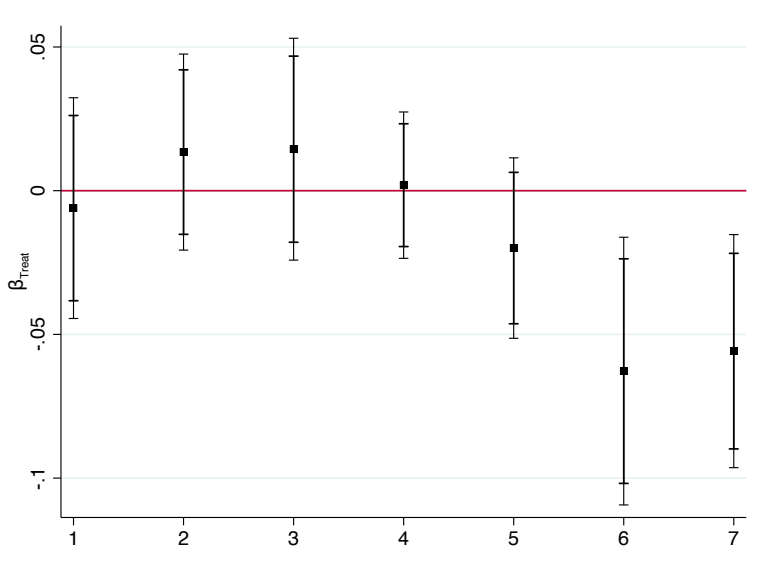
(a) Exit



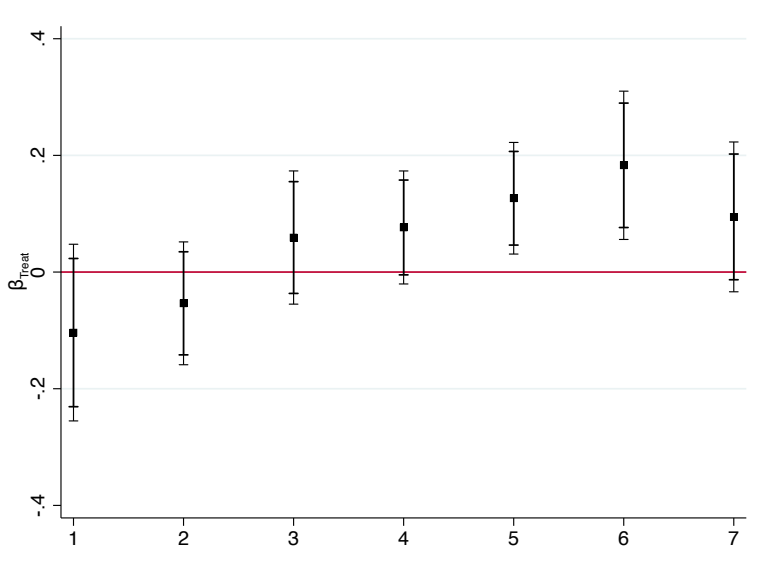
(b) Employment

We estimate a coefficient for each of the four labor-share quartiles, while controlling linearly for baseline effects. Each interacted treatment is instrumented by the interacted instrument.
See Table 4 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). 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.

Figure E.14: Regressions by residualized labor-share bins



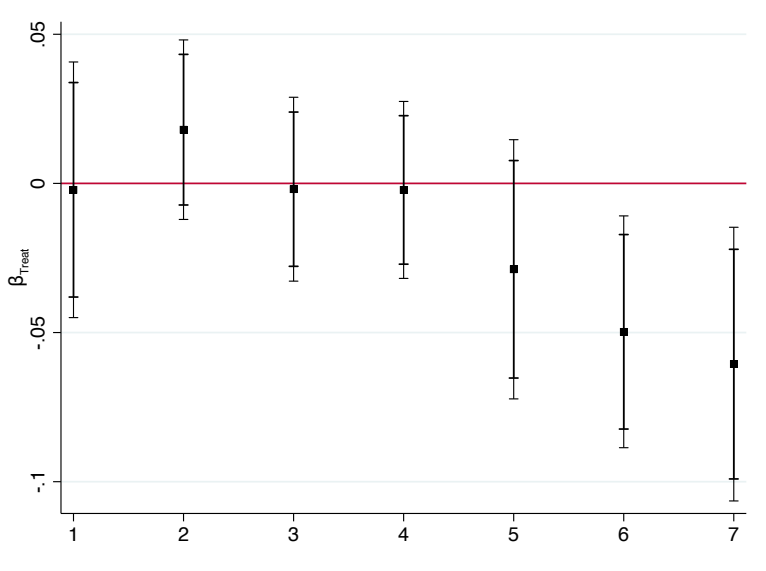
(a) Exit



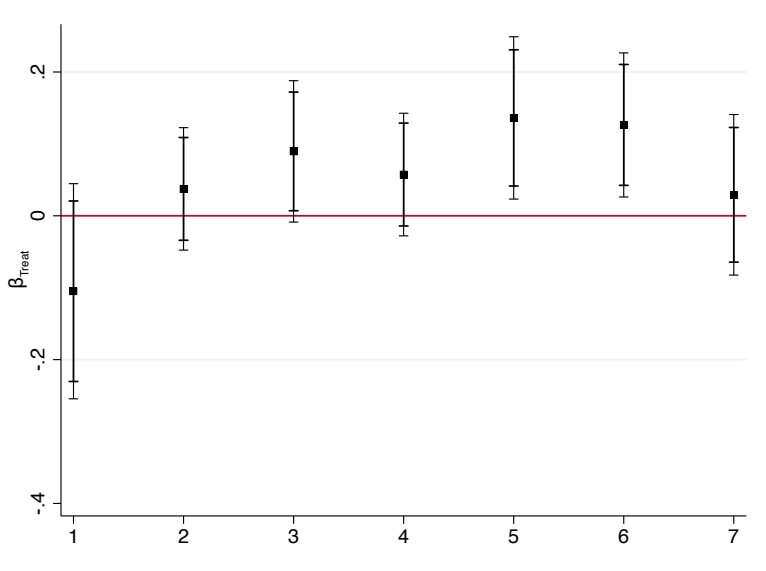
(b) Employment

We obtain the residualized labor shares by calculating the residuals in a regression of the labor share over value added per employee (2005-2006 average values) and the set of fixed effects that we control for in the regressions. 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 Table 4 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: 13,740 (exit) and 13,750 (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.

Figure E.15: Regressions by labor-share bins (alternative measure)



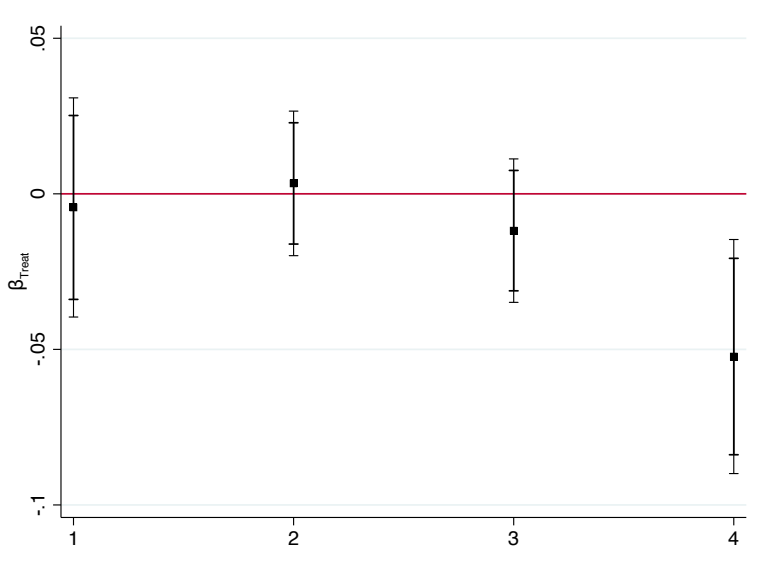
(a) Exit



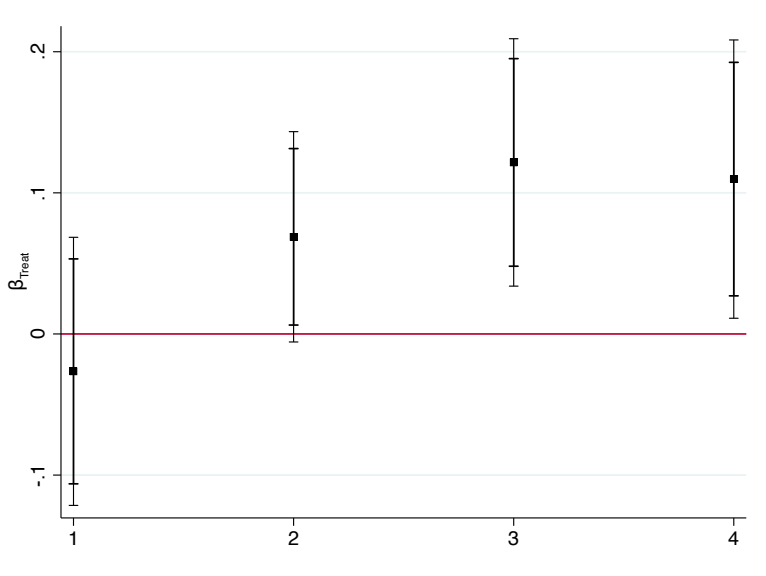
(b) Employment

The alternative labor-share measure uses wage bills calculated from the QP as a measure of labor costs. We estimate a coefficient for each of the seven labor-share bins, while controlling linearly for baseline effects. Each interacted treatment is instrumented by the interacted instrument.
See Table 4 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: 13,714 (exit) and 13,724 (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.

Figure E.16: Regressions by labor-share quartiles in sales



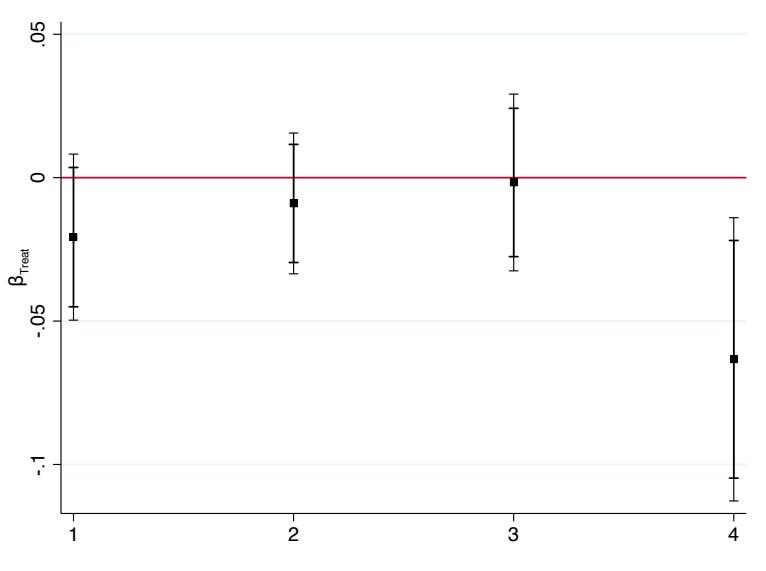
(a) Exit



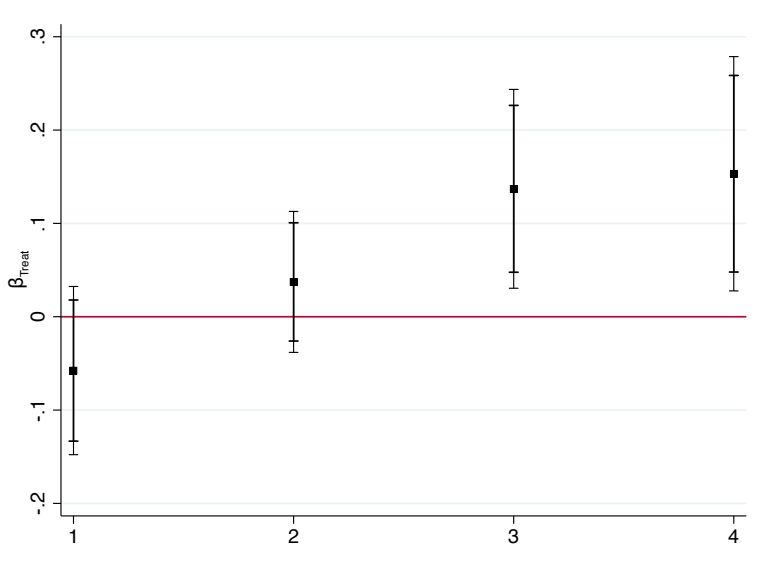
(b) Employment

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. Labor share is defined as the ratio between employment-related costs and total sales.
See Table 4 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.
In order to be consistent with the value added specifications, we use sales per employee instead of value added per employee as the measure of labor productivity.
Number of firms: 13,750 (exit) and 13,760 (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.

Figure E.17: Regressions by labor-share bins (2007-2008 averages)



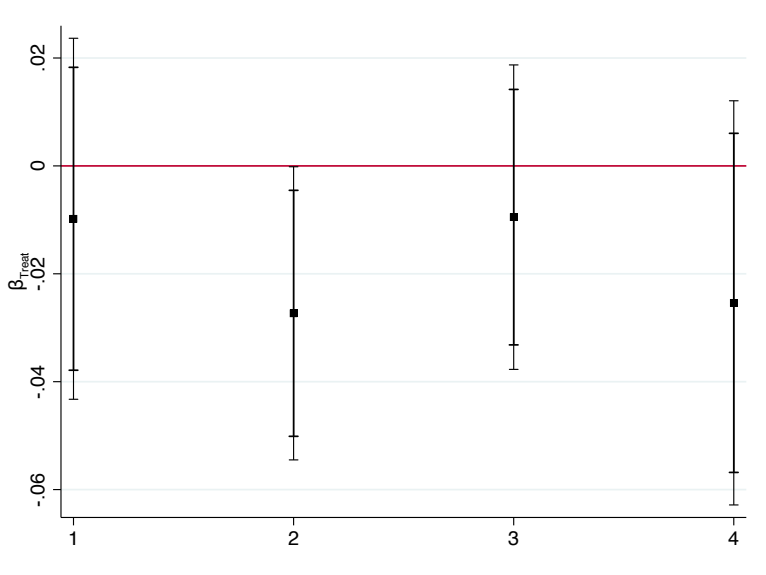
(a) Exit



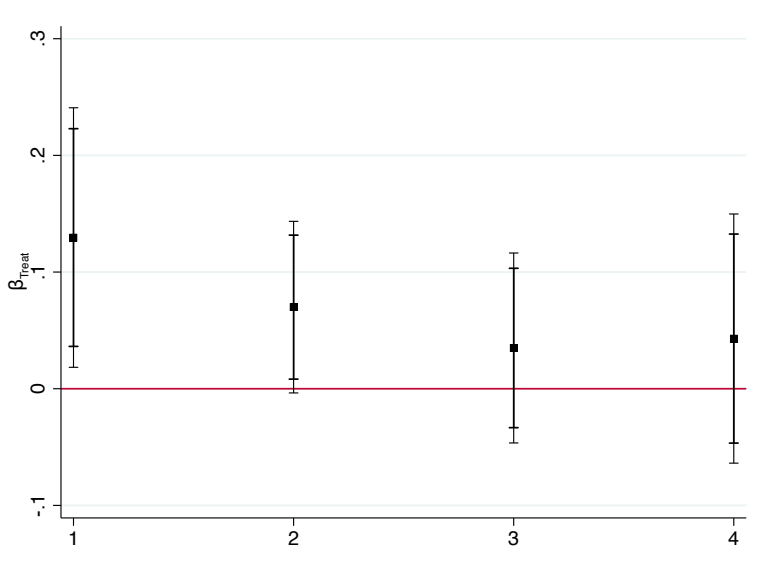
(b) Employment

The alternative labor-share measure uses wage bills calculated from the QP as a measure of labor costs. We estimate a coefficient for each of the seven labor-share bins, while controlling linearly for baseline effects. Each interacted treatment is instrumented by the interacted instrument. Labor share is defined as the ratio between employment-related costs and total value added (average of 2007-2008 levels).
See Table 4 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). 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.

Figure E.18: Regressions by leverage bins



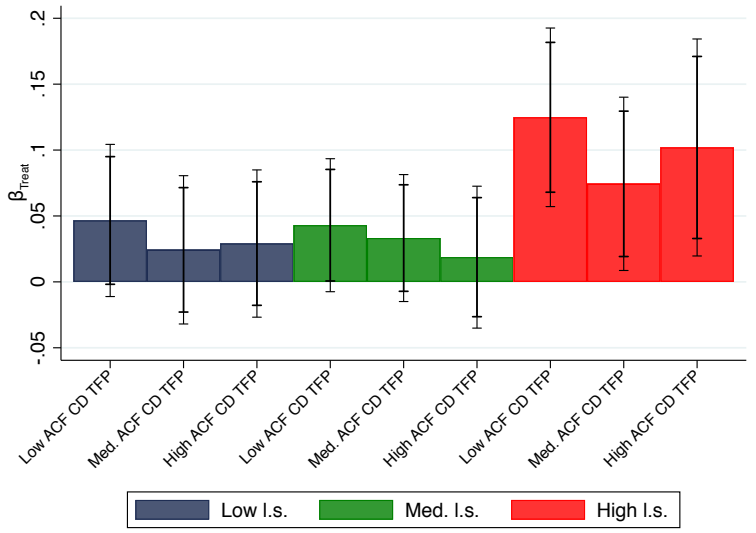
(a) Exit



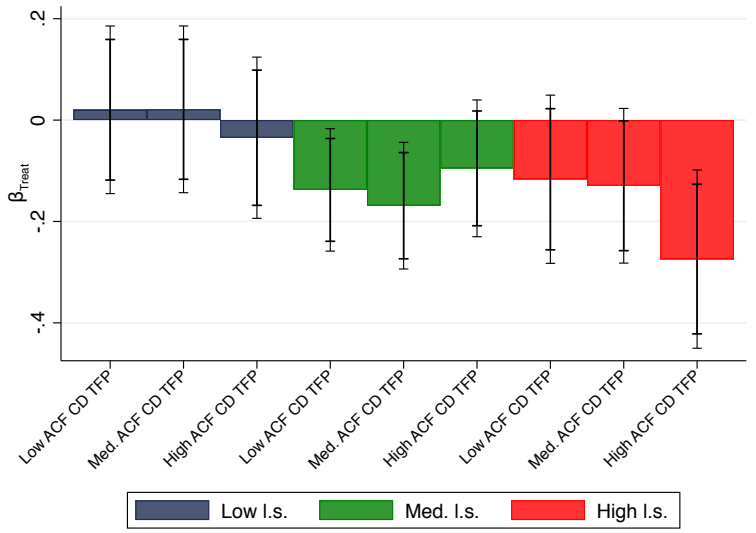
(b) Employment

We estimate a coefficient for each of the four leverage quartiles, while controlling linearly for baseline effects. Each interacted treatment is instrumented by the interacted instrument. Leverage is defined as the ratio between total regular credit to total assets (2005 levels).
See Table 4 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: 13,796 (exit) and 13,806 (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.

Figure E.19: Regressions by labor-share and productivity bins, reduced form



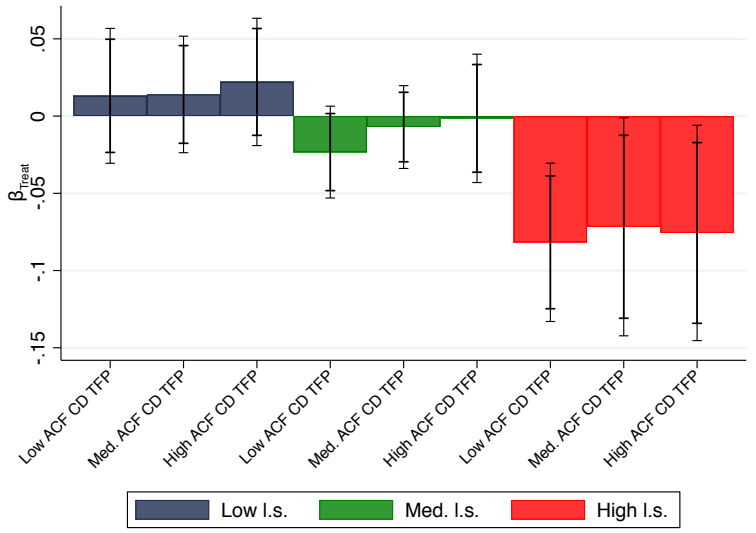
(a) Exit



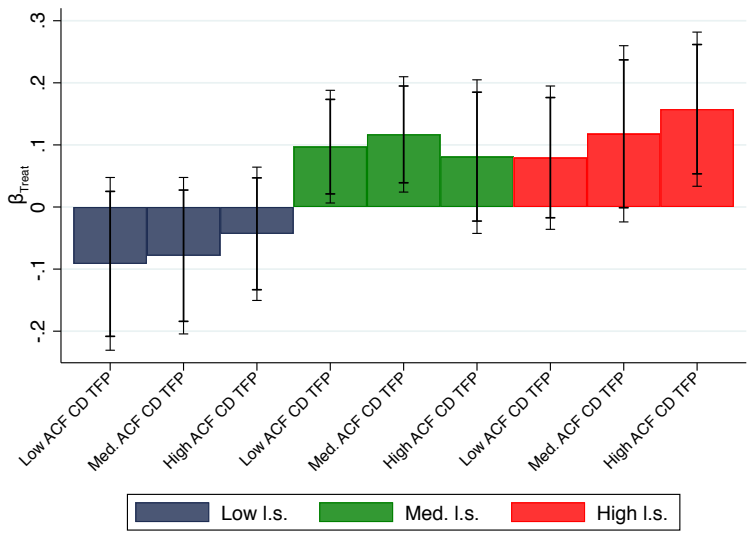
(b) Employment

We estimate a coefficient for each of the nine interacted bins, while controlling linearly for baseline effects and their interaction. 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 sales Cobb-Douglas production function following Ackerberg et al. (2015), by 2-digit industrial sectors.
See Table 4 for the list of controls and fixed effects in the regressions. Given that we cannot control for unobservable characteristics in the exit specifications through firm fixed effects, we characterize the matching of firms to banks by augmenting the set of controls with the share of loans that each firm has with micro banks and with banks failing up to 2014.
All fixed effects are interacted with a year dummy, while regressors are constant in the exit regressions. In the employment regressions fixed effects and regressors are interacted with the Post dummy.
Number of firms: 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.

Figure E.20: Regressions by residualized labor-share and productivity bins



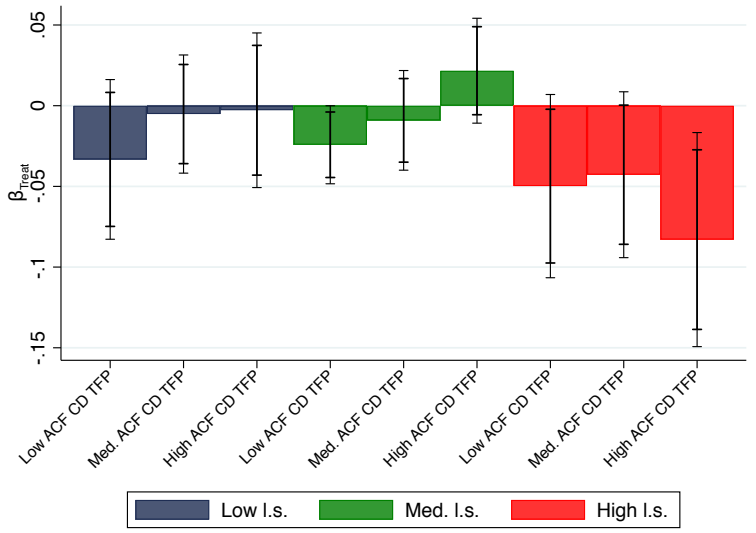
(a) Exit



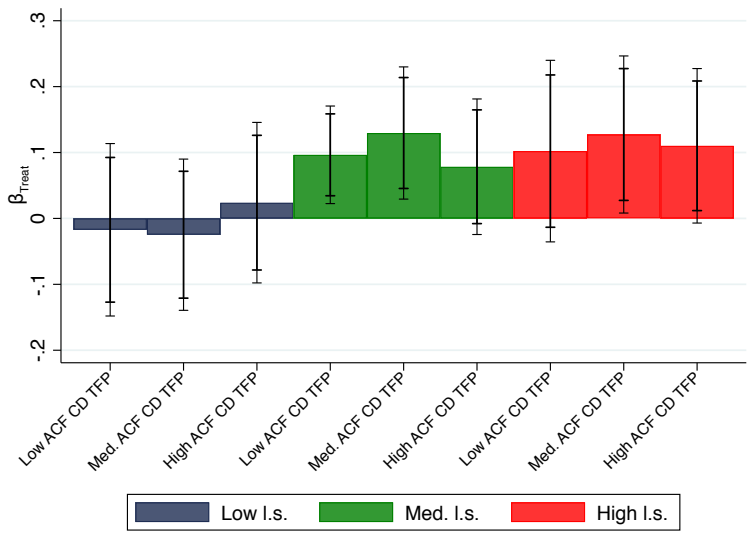
(b) Employment

We obtain the residualized labor shares by calculating the residuals in a regression of the labor share over value added per employee (2005-2006 average values) and the set of fixed effects that we control for in the regressions. 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 sales Cobb-Douglas production function following Ackerberg et al. (2015), by 2-digit industrial sectors.
See Table 4 for the list of controls and fixed effects in the regressions. Given that we cannot control for unobservable characteristics in the exit specifications through firm fixed effects, we characterize the matching of firms to banks by augmenting the set of controls with the share of loans that each firm has with micro banks and with banks failing up to 2014.
All fixed effects are interacted with a year dummy, while regressors are constant in the exit regressions. In the employment regressions fixed effects and regressors are interacted with the Post dummy.
Number of firms: 13,238 (exit) and 13,248 (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.

Figure E.21: Regressions by labor-share in sales and productivity bins



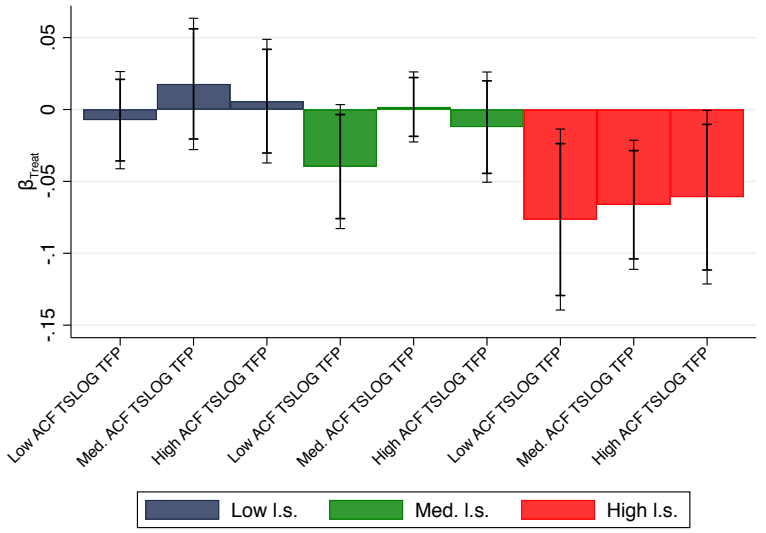
(a) Exit



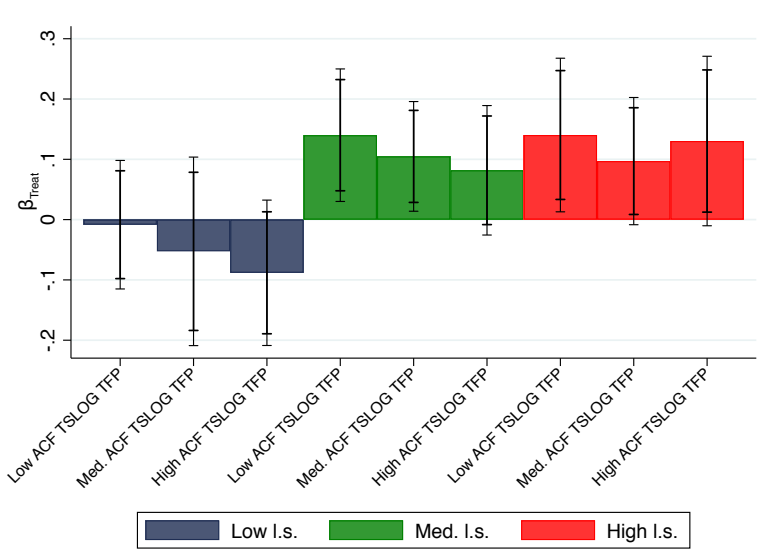
(b) Employment

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 sales (average of 2005-2006 levels). Productivity is estimated on a 3-inputs gross sales Cobb-Douglas production function following Ackerberg et al. (2015), by 2-digit industrial sectors.
See Table 4 for the list of controls and fixed effects in the regressions. Given that we cannot control for unobservable characteristics in the exit specifications through firm fixed effects, we characterize the matching of firms to banks by augmenting the set of controls with the share of loans that each firm has with micro banks and with banks failing up to 2014.
All fixed effects are interacted with a year dummy, while regressors are constant in the exit regressions. In the employment regressions fixed effects and regressors are interacted with the Post dummy.
Number of firms: 13,258 (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.

Figure E.22: Regressions by labor-share and TSLOG-ACF-productivity bins



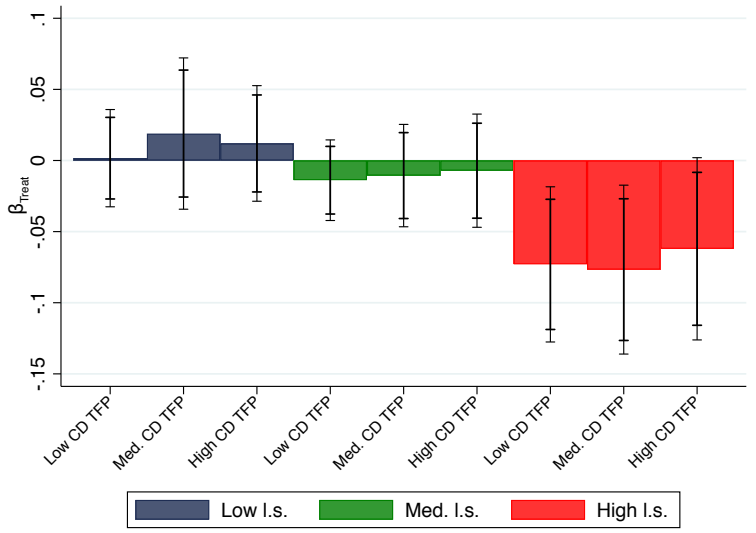
(a) Exit



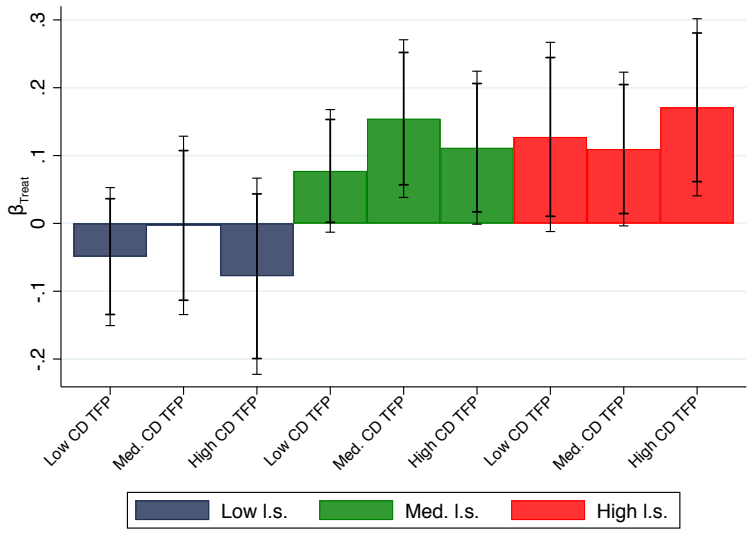
(b) Employment

We estimate a coefficient for each of the nine interacted bins, while controlling linearly for baseline effects and their interaction. Each interacted treatment is instrumented by the interacted instrument. Labor share is defined as the ratio between employment-related costs and total value added (average of 2005-2006 levels). Productivity is estimated on a 3-inputs gross sales translog production function following Ackerberg et al. (2015), by 2-digit industrial sectors.
See Table 4 for the list of controls and fixed effects in the regressions. Given that we cannot control for unobservable characteristics in the exit specifications through firm fixed effects, we characterize the matching of firms to banks by augmenting the set of controls with the share of loans that each firm has with micro banks and with banks failing up to 2014.
All fixed effects are interacted with a year dummy, while regressors are constant in the exit regressions. In the employment regressions fixed effects and regressors are interacted with the Post dummy.
Number of firms: 12,927 (exit) and 12,927 (employment). Sample size depends on availability of non-missing variables in CB.
95 and 90% confidence intervals displayed, standard errors clustered at the bank-industry pair level.

Figure E.23: Regressions by labor-share and CD-productivity bins, OLS



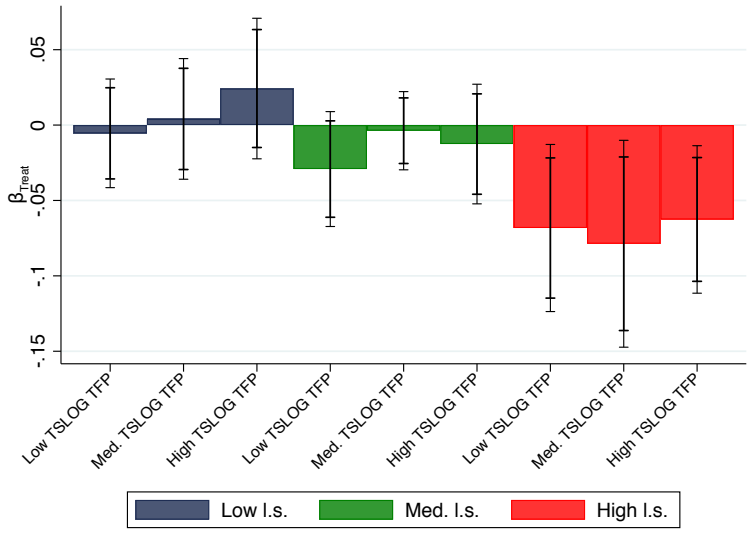
(a) Exit



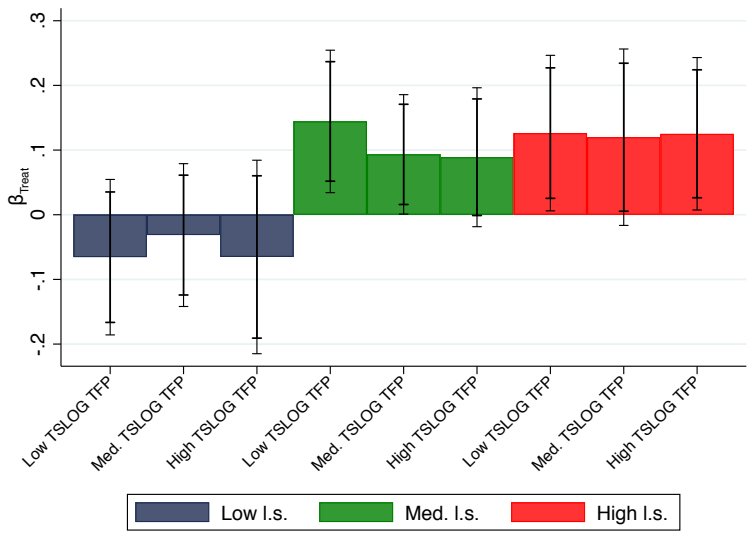
(b) Employment

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 sales Cobb-Douglas production function by OLS and by 2-digit industrial sectors. See Table 4 for the list of controls and fixed effects in the regressions. Given that we cannot control for unobservable characteristics in the exit specifications through firm fixed effects, we characterize the matching of firms to banks by augmenting the set of controls with the share of loans that each firm has with micro banks and with banks failing up to 2014.
All fixed effects are interacted with a year dummy, while regressors are constant in the exit regressions. In the employment regressions fixed effects and regressors are interacted with the Post dummy.
Number of firms: 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.

Figure E.24: Regressions by labor-share and TSLOG-productivity bins, OLS



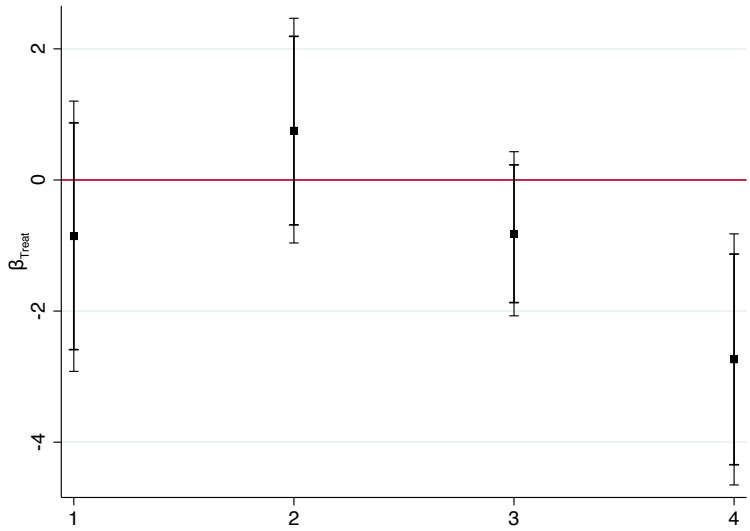
(a) Exit



(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 sales translog production function by OLS and by 2-digit industrial sectors.
See Table 4 for the list of controls and fixed effects in the regressions. Given that we cannot control for unobservable characteristics in the exit specifications through firm fixed effects, we characterize the matching of firms to banks by augmenting the set of controls with the share of loans that each firm has with micro banks and with banks failing up to 2014.
All fixed effects are interacted with a year dummy, while regressors are constant in the exit regressions. In the employment regressions fixed effects and regressors are interacted with the Post dummy.
Number of firms: 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.

Figure E.25: Labor MRP-cost gap regressions by labor-share quartiles



The labor MRP-cost gap is defined as the difference between the estimated marginal revenue product of labor and the average firm wage. See Appendix C.2 for more details. We estimate a coefficient for each of the four labor-share quartiles, while controlling linearly for baseline effects. Each interacted treatment is instrumented by the interacted instrument. Labor share is defined as the ratio between employment-related costs and total value added (average of 2005-2006 levels).
See Table 4 for the list of controls and fixed effects present in the regressions. Additional controls for these specification using O*NET variables include the scores for: required education, required previous experience and required amount of training on site. Results are unchanged if these additional controls are not added. See Appendix B for a description of each of these variables, and the on-the-job training score as well.
All fixed effects and controls are interacted with a *post* dummy.
Number of firms: 13,750.
95 and 90% confidence intervals displayed, standard errors clustered at the bank-industry pair level.