Online Appendix for: Labor Rigidities and Firms' Resilience to Liquidity Shocks

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A 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.

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First, we perform some quality checks on the QP and remove workers' for which identifiers are not consistent over time. 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). As regards monetary balance-sheet variables, wages and credit variables, we deflate all nominal values in the analysis by the 2013 consumer price index.[1]

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.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.[2] In order to reduce measurement noise, we consider only firms with no gaps in the data in the pre-period.[3]

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

[1]We refer the reader to Section B in the Internet Addendum on the authors' websites for details regarding deflation of nominal values in the productivity estimation.

[2]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.

[3]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.

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, while we average interbank funding between entities in case of mergers happening at the time of instrument measurement.

To limit the influence of outliers in the regressions, we drop firms in the top 2.5 percentile of positive percentage credit variation between the pre- and post-periods, *before* calculating the symmetric growth rate.[4] 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, once again *before* calculating the symmetric growth rate. 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.[5]

B Average firm level results

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

[4]We only drop firms with unreasonably high levels of growth of credit, as they are likely to start from very low initial amounts, and their percentage variation is thus a clear outlier. As we want to consider firms closing their credit relationship or reducing their debt to almost 0, we do *not* drop the lowest percentiles.

[5]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 Appendix Section A), and cover the entire sample.

following regression:

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

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. We cluster the standard errors at the main bank-industry pair level.

Table 5 reports the results from estimating Equation (1) with the logarithm of the number of employees as an outcome variable. The first column of Table 5 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 C.2 in the main Appendix) and in column 4 we get to the main specification that we will use throughout the paper, with the full set of controls. Results in columns 3 and 4 show that the correlation of the instrument in 2005 with some observables does not have a relevant effect on the coefficient of interest. Given that we are averaging the outcome variable over a different number of observations in the post period due to firm exit or attrition from the QP, in column 5 we report the estimated coefficient over a sample of survivor firms. This last estimated

coefficient more precisely characterizes the intensive margin of firm adjustment. The first stage effective F-statistic is always above 30 and not far from the 5-percent Nagar bias threshold (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.[6]

Our preferred estimates range from 0.071 (column 4, full sample) to 0.086 (column 5, survivors). To add context to the magnitude of our estimates, in the post-period the predicted treatment after the first stage regression has an average of -0.183 (median -0.204), a standard deviation of 0.565 and a 10–90 percentile range of 1.532. A one negative standard-deviation variation in the treatment would decrease the average firm employment by approximately 4 percentage (log) points (4.9 percentage points according to the survivors' estimate). Given that the average employment variation in our sample is -0.044 (median -0.040) and the standard deviation in employment is 0.288, the shock has significant economic size. The economic impact is even more prominent, given that in the later years in our sample Portugal was suffering from 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.[7] One standard deviation in our shock explains between 14 and 17 percent of the standard deviation in employment. The amount of variation explained by the shock is comparable to recent related studies Bentolila et al. (2017); Berton et al. (2018).

Table 6 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

[6]In an acid regression the instrument is not significant, whereas the variation in credit obviously is.

[7]Consistently with the evidence presented in Table C.1, 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.

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.[8] In the columns (5) and (6) we show the results of estimating a euro-to-euro sensitivity of payroll with respect to the cash-flow shock generated by the credit-supply variation. We scale the level variation in salaries between the pre- and post-periods and the variation in credit at the numerator of S_i in Equation (1) by the pre-period average level of sales. The estimated euro-to-euro sensitivity is 0.17 for the full sample and 0.23 for the survivor sample, which should deliver more precise estimates for the wage bill and employment for the entirety of the post-period. These values are close to standard values estimated in the literature.[9] The last column reports results of running the following linear probability model on a dummy which is 1 upon firm exit:

$$P(exit_{i,t}) = \tau_t + \beta S_i + \mathbf{\Gamma X_{i,pre}} + FE_{i,t} + \varepsilon_{i,t}, \tag{2}$$

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

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

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

In Table 3, for robustness and to confirm the logic of our identification strategy and analysis, we show that applying the same logic of credit variation S_i and instrument Z_i, but using long-term credit variation and weights, does not deliver any significant result. Quite importantly, first-stage F-stats are extremely low, suggesting that banks' exposure to the interbank market freeze did not predict in any way banks decreases in long-term credit supply. This makes sense, given that banks themselves were facing a sudden and unexpected shortage in liquidity, and could not arguably immediately adjust long-term credit supply, as this kind of credit might have had a maturity structure which did not allow any adjustment in the moment.

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

$$Y_{i,t} = \gamma_i + \tau_t + \sum_{k \neq 2008} (\beta_k S_i + \mathbf{\Gamma_k X_{i,pre}}) \cdot \mathbb{1}\{t = k\} + FE_{i,t} + \varepsilon_{i,t} \tag{3}$$

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

As evident from Figure 9, the treatment does not show pre-trends. Moreover, it has persistent effects that accumulate over time, weakly waning only in 2013.[10]

Table 17 shows the results of estimating Equation (1) on balance-sheet and other

[10]These results are qualitatively robust to checking for potential violations in parallel trends as in Rambachan and Roth (2023). Results available upon request.

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.[11] 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 firms.[12]

C Conceptual framework

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. We develop a simplified model of firm decision of credit and labor demand, subject to a working-capital constraint, in a similar fashion to Schoefer (2022) and Mueller (2017).

[11] We take the logarithm of the variables with positive support, and the asinh of the variables that can take negative values.

[12] In unreported results we show that, by using an empirical specification as in Almeida et al. (2011), fixed capital growth is not responsive to short term capital variations, but only to long term credit variations for those firms which had a big share of long term credit (more than 20 percent) maturing right before the shock.

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 $P = 1$. The firm starts the initial period with some retained earnings Π, a fixed amount of capital $\bar{K}$ and incumbent workers I who receive a salary w^I.[13] The firm sets its employment policy by choosing whether to hire new workers N at the prevailing salary w^N on the market.[14] The firm incurs in a training/vacancy costs cN in order for workers 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 cN.

The presence of mismatches in cashflow timing and the presence of training costs make incumbent and prospective workers imperfectly substitutable for the firm in the short term. Incumbent, trained labor can thus be seen as a quasi-fixed factor (Oi, 1962). The firm, having to incur in these upfront costs, can finance them through past retained earnings or, if they are insufficient, it needs to borrow an amount D from the banking sector. The need to borrow to finance labor expenditures exposes the firm to the cost of external finance (Chodorow-Reich, 2014).

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 also 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

[13] For simplicity we do not directly model the determination of the incumbents' salaries. They would depend on current market wages and past wage levels, depending on the presence of downward wage rigidity and human capital accumulation through training and on-the-job learning (Acabbi et al., 2024).

[14] 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. This assumption is sensible as we are focusing on a short-term sudden liquidity decrease.

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:

$$\max_{N,D} \; V = AF(L, \bar{K}) - w^N N - w^I I - cN - f, \tag{4}$$

subject to the working-capital constraint:

$$D + \Pi \geq cN + \lambda(w^N N + w^I I), \tag{5}$$

where A is the firm TFP. $L = N + I$, as we assume that in the last period all workers are eventually properly trained and productive.

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

$$AF_L(L, \bar{K}) = w^N + c'(N). \tag{6}$$

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 (5). In this case the incumbents' salaries have no effect on the optimal level of hiring. Moreover, notice that absent unforeseen shocks, rational firms will not be firing incumbents. In presence of implicit or explicit adjustment costs as in Oi (1962), Bentolila and Bertola (1990) firms will not find it optimal to fire workers, unless unexpected and strongly adverse shocks materialize. This is also a crucial point to distinguish different predictions regarding a sales or a credit supply shock. One might expect the working capital constraint to bind and thus constrain employment choices also vis-a-vis an unforeseen sales shock. This might well be the case, *provided* lenders are not willing to help a firm smooth temporary fluctuations in sales. As long

as shocks are temporary and not too severe, though, it is unlikely that banks in a heavily bank-dependent system like Portugal would let their attached firms go under. However, a sudden *liquidity* shock to firms might be more difficult to confront, especially if there are no easily obtainable alternative sources of financing. Iyer et al. (2014) provide extensive evidence that firms during the interbank market freeze were not able to substitute their credit across banks.

Suppose now that, as in Mueller (2017), the cash-flow constraint is exogenously tightened: the bank unexpectedly cuts its credit supply and the firm can only obtain a level $\bar{D}$.[15] In this case, the firm's working-capital constraint yields the firm's labor demand as:

$$\bar{N} = \frac{(D + \Pi) - \lambda w^I I}{c + \lambda w^N}. \tag{7}$$

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

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

$$\left.\frac{\partial V}{\partial D}\right|_{D=\bar{D}} = \mu = \frac{AF_L(\bar{N} + I, \bar{K}) - w^N - c}{c + \lambda w^N}, \tag{8}$$

where μ is the shadow value of the working-capital constraint (5).

The effect on firm's value through new hires is:

$$\frac{\partial N}{\partial D} = \frac{1}{\lambda w^N + c} - \frac{\lambda}{\lambda w^N + c}\left(\frac{\partial w^I}{\partial D} I + \frac{\partial I}{\partial D} w^I\right) \tag{9}$$

A credit tightening will have a direct affect on hirings, which can be muted if

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

adjustment to incumbent employment or wages are feasible. As wages of incumbents tend to be inflexible (Donangelo et al. 2019; Schoefer 2022), wage cuts are rare (Bertheau et al., 2022) and firings of incumbents with implicit firing costs can be infeasible or not optimal, 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:*

$$\varepsilon_{L,D} = \frac{\partial \log L}{\partial \log D} = \frac{N^*}{N^* + I} + \frac{\lambda w^I I - \Pi}{(\lambda w^N + c)(N^* + I)}. \tag{10}$$

The first term proxies a standard investment channel, which defines firms' employment choices given cash-flows availability and the cost of external funds, absent liquidity pre-commitments. The second term proxies the labor-as-leverage amplification channel. The greater the incumbents' wages w^I, the greater the share of the wage bill λ that has to be paid in advance, and the smaller the retained earnings Π from the previous period, the stronger the amplification of a credit shock through operating leverage determined by labor costs will be on employment.[16] 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.

The fact that employment elasticity to credit is amplified by incumbents' labor costs might not be per se a signal of fundamental unprofitability of the firm. For instance, the firm might still have a good operating margin in normal times after taking capital costs and other costs into account. This can happen if the firm employs high skill specialized labor, and generates value added trough it. In the data, the labor share is unconditionally negatively correlated with past profits. The empirical

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

exercise, however, aims to remove this effect, exploiting the variation in labor costs *conditional* on a certain level of profitability. 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 inefficiency, overpayment or a operating leverage effect through labor costs on an otherwise productive firm.[17]

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.*

Our result makes clear that a major driver of labor leverage and the amplification of the real effects of credits shocks on firms' employment, size and exit is the past wage policy for incumbent workers. In the main body of the paper we show, to this point, that the elasticity of firm employment to credit scales up with the degree of complexity of professions carried out in the firms, proxied by a measure of training intensity from ONET.[18] Incumbents wages w^I can be heterogeneous, and high for different reasons, which are left unmodeled here given the static nature of the model, but are discussed in Section 4 in the main body of the paper.

[17]Keep in mind that the employment elasticity is not attenuated by future profitability past next period. If anything, future profitability or productivity of workers leads to a greater effect on impact, as firms would hire more in the optimal undistorted case in anticipation of greater profits in the future, even in absence of current cash-flows (Mueller 2017; Caggese et al. 2019.)

[18]Acabbi et al. (2024) show, by means of a structural model, that human capital accumulation over the tenure of workers at firms eventually diminishes firms' resilience throughout recessions, especially if labor markets fluidity is scarce.

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D Appendix 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 - 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 3: Employment regressions, long-term credit variation and instrument

	(1) $log(\#emp)_{i,t}$	(2) $log(wage\ bill)_{i,t}$
$S_{i,lt}$	-0.006	-0.025
	(0.127)	(0.146)
Firms	11,012	11,012
WID F	4.24	4.24

The regressions refer to the empirical specification in Equation (1) in the text. All regressions feature firm and time fixed effects. The regression is run for the complete sample of firms for which long-term credit is available. The instrument and the credit variations are calculated as for the main specification in the text, but using long-term instead of short-term credit variation, and using long-term credit shares in 2005 to calculate the shift-share instrument. Long-term credit refers to credit with fixed-term maturity above 1 year. See the Appendix Section A 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 4: Regressions by labor share bins

	(1)	(2)	(3)	(4)
	(7*bins*)		(4*bins*)	
	Employment	*Exit*	*Employment*	*Exit*
$S_i \cdot \mathbb{1}(labsh\_q. = 1)$	-0.023	0.016	-0.032	0.010
	(0.064)	(0.022)	(0.045)	(0.016)
$\mathbb{1}(labsh\_q. = 2)$	0.006	0.013	0.085+	0.004
	(0.042)	(0.016)	(0.049)	(0.016)
$\mathbb{1}(labsh\_q. = 3)$	0.079	0.027	0.104*	-0.016
	(0.057)	(0.020)	(0.042)	(0.013)
$\mathbb{1}(labsh\_q. = 4)$	0.078	0.003	0.095+	-0.066*
	(0.049)	(0.015)	(0.051)	(0.021)
$\mathbb{1}(labsh\_q. = 5)$	0.104*	-0.041*		
	(0.055)	(0.019)		
$\mathbb{1}(labsh\_q. = 6)$	0.133*	-0.051*		
	(0.061)	(0.021)		
$\mathbb{1}(labsh\_q. = 7)$	0.053	-0.080**		
	(0.061)	(0.027)		
Firms	13760	13750	13760	13750

The table reports the coefficients in Figure 2. We estimate a coefficient for each of the seven (columns 1 and 2) or four (columns 3 and 4) labor-share bins, while controlling linearly for baseline effects. Each interacted treatment is instrumented by the interacted instrument. See Appendix A for the list of controls and fixed effects present in the regressions. Given that we cannot control for unobservable characteristics in the exit specifications through firm fixed effects, we characterize the bank-firm matching by augmenting the set of controls with the share of loans that each firm has with micro banks and with banks failing up to 2014. All fixed effects are interacted with a year dummy, while regressors are constant in the exit regressions. In the employment regressions fixed effects and regressors are interacted with the Post dummy. Number of firms: 13,750 (exit) and 13,760 (employment). Standard errors clustered at the bank-industry pair level.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5: Employment regressions

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

The regressions refer to the empirical specification in Equation (1) in the text. All regressions feature firm and time fixed effects. We refer to *Fail b.c.* as the estimation sample in which only the controls for which the balance checks have failed are included and controlled for. See the Appendix Section A for the list of controls and fixed effects in the regressions. Standard errors clustered at the bank-industry pair level.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 6: Wage bill and exit regressions

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

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

Table 7: Regressions by quartiles of on-the-job training scores

	(1)	(2)
	Employment	*Exit*
$S_i \cdot \mathbb{1}(otj\_q. = 1)$	-0.005	-0.012
	(0.040)	(0.014)
$\mathbb{1}(otj\_q. = 2)$	0.123**	-0.002
	(0.041)	(0.025)
$\mathbb{1}(otj\_q. = 3)$	0.110*	-0.024
	(0.046)	(0.018)
$\mathbb{1}(otj\_q. = 4)$	0.143**	-0.036+
	(0.053)	(0.020)
Firms	13756	13746

On-the-job (otj) training is defined as work carried out under the supervision of more experienced workers, and ranges from 1 (short demonstration) to 9 (several years of training). We estimate a coefficient for each of the four otj training quartiles, while controlling by means of a third order polynomial of the otj score. Each interacted treatment is instrumented by the interacted instrument. See Appendix Section A for the list of controls and fixed effects present in the regressions. Given that we cannot control for unobservable characteristics in the exit specifications through firm fixed effects, we characterize the matching of firms to banks by augmenting the set of controls with the share of loans that each firm has with micro banks and with banks failing up to 2014. Additional controls for these specification using O*NET variables comprehend the scores for: required education, required previous experience and required amount of training on site. Results are unchanged if these additional controls are not added. See Section A.7 in the Internet Addendum 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. Standard errors clustered at the bank-industry pair level.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 8: Employment - wage bill regressions: Manufacturing

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

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

Table 9: Heterogeneous employment regressions: Qualifications

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

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

Table 10: Heterogeneous employment regressions: Age cohorts

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

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

Table 11: Employment and Exit regressions: collective agreements renewals

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

The dependent variable for columns 1 and 2 is the logarithm of the number of employees, whereas for columns 3 and 4 it a dummy equal to 1 if in the specific year the firm exits the market. The exit regression is a yearly linear probability model. In this specification we add to the controls the share of credit that a firm gets from micro-banks (i.e. excluding the the 10 largest banks) and the share of credit that the firm is getting from the banks failing before 2014, as we try to control indirectly for the unobservable characteristics related to these kinds of matching. Columns 1 and 3 report baseline estimates in Appendix Tables 5 and 6. Columns 2 and 4 report results of specifications with heterogeneous effects for firms which have renewed their collective bargaining agreement in the last year, where treatment and instrument are interacted with a dummy for this characteristic. Coefficients need to be interpreted as deviations from average ones. See Appendix Section A for the list of controls and fixed effects in the regressions. Regressions in columns 1 and 2 feature a full set of 2005-06 controls and fixed effects, interacted with a *Post* dummies. In the linear probability model fixed effects are interacted with year dummies, whereas 2005-06 controls are not. Standard errors clustered at the bank-industry pair level.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 12: 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*	-0.033*
	(0.039)	(0.015)
, *Med. TFP*	0.077*	-0.014
	(0.037)	(0.012)
, *High TFP*	0.073	-0.022
	(0.045)	(0.017)
Firms	13285	13277
WID F	11.15	11.59
Firm FE	Yes	No

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

Table 13: Regressions by labor-share and productivity bins

	(1) *Employment*	(2) *Exit*
S_i, *Low Lab.Sh., Low TFP*	-0.078	-0.001
	(0.068)	(0.021)
S_i, *Low Lab.Sh., Med. TFP*	-0.066	0.016
	(0.066)	(0.020)
S_i, *Low Lab.Sh., High TFP*	-0.021	0.011
	(0.052)	(0.018)
S_i, *Med. Lab.Sh., Low TFP*	0.099*	-0.022
	(0.046)	(0.015)
S_i, *Med. Lab.Sh., Med. TFP*	0.120*	-0.012
	(0.049)	(0.014)
S_i, *Med. Lab.Sh., High TFP*	0.069	-0.001
	(0.060)	(0.021)
S_i, *High Lab.Sh., Low TFP*	0.085	-0.078**
	(0.058)	(0.025)
S_i, *High Lab.Sh., Med. TFP*	0.117**	-0.063*
	(0.067)	(0.030)
S_i, *High Lab.Sh., High TFP*	0.175**	-0.079*
	(0.066)	(0.036)
Firms	13258	13248

We estimate a coefficient for each of the nine interacted bins, while controlling linearly for baseline effects and their interaction. Each interacted treatment is instrumented by the interacted instrument. Labor share is defined as the ratio between employment-related costs and total value added (average of 2005-2006 levels). Productivity is estimated on a 3-inputs gross output Cobb-Douglas production function following Ackerberg et al. (2015), by 2-digit industrial sectors. See the Online Appendix Section A for the list of controls and fixed effects present in the regressions. Given that we cannot control for unobservable characteristics in exit specifications through firm fixed effects, we characterize the matching of firms to banks by augmenting the set of controls with the share of loans that each firm has with micro banks and with banks failing up to 2014. All fixed effects are interacted with a year dummy, while regressors are constant in the exit specifications. In the employment specifications All fixed effects and controls are interacted with a *post* dummy. Sample size depends 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 14: Reallocation and TFP by labor share - full dataset

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

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

Table 15: Wedge regressions

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

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

Table 16: Robustness: instrument effects on credit post 2010

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

The regressions refer to the empirical specification in equation (B.2) in the text. See Appendix Section A for the list of controls and fixed effects in the regressions. The sample consists of firms with (short-term) credit relationships in 2010. Columns 4 and 5 control directly for firms (weighted) exposure to banks average sovereign debt holdings over assets in 2009 (Q4), either considering banks with which the firms has a relationship in 2005 (4) or 2009 (5). Standard errors clustered at bank-industry pair level.

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

Table 17: Balance sheet and financials regressions

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

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

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

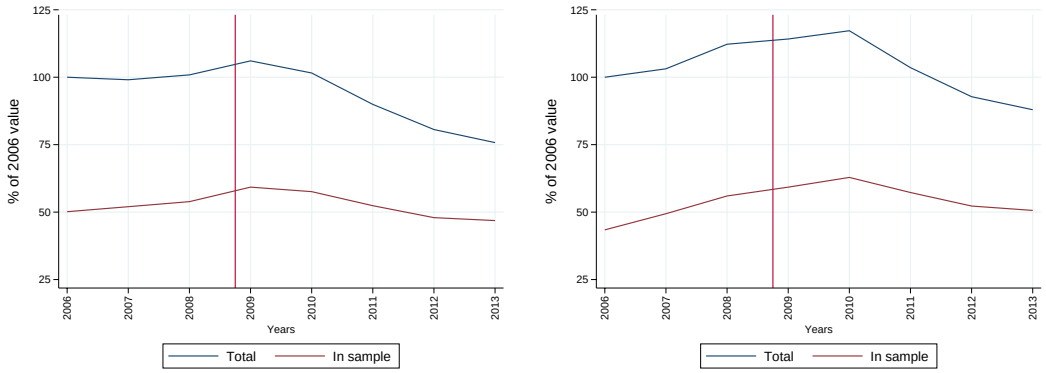
Table 18: Robustness: Almeida et al. (2011) identification, effects on fixed investments

	(1)	(2)
	$log(fixed\ assets)_{i,t}$	
S_i	0.017	0.030
	(0.074)	(0.120)
$S_i \cdot exp\_lt_i$		-0.023
		(0.119)
exp_lt_i	-0.090***	-0.095***
	(0.015)	(0.026)
Firms	11797	11797

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 trends 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 Appendix A for a list of the added controls and fixed effects present in the regressions. All regressions feature the full set of fixed effects and controls. Standard errors clustered at the bank-industry pair level.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

E Appendix Figures

Figure 1: Credit dynamics in Portugal

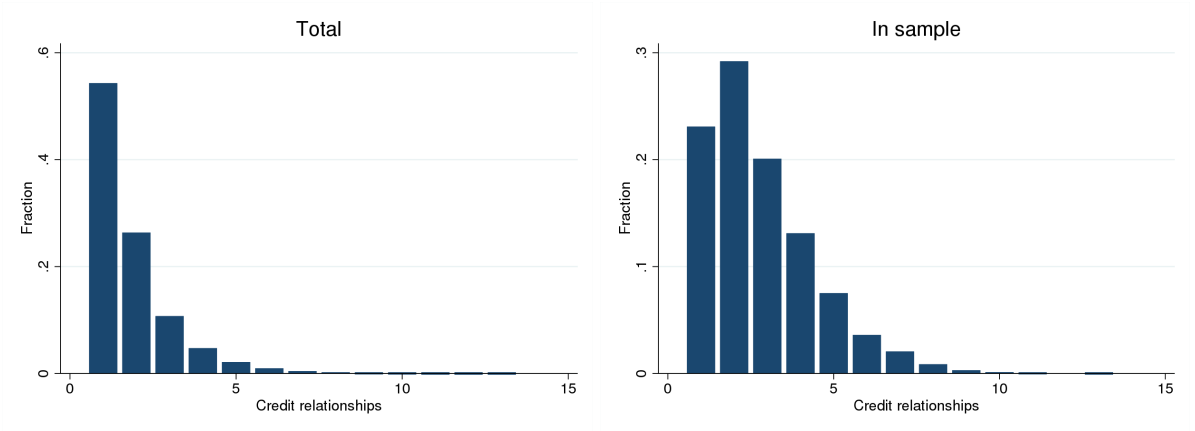


(a) Total regular credit

(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.

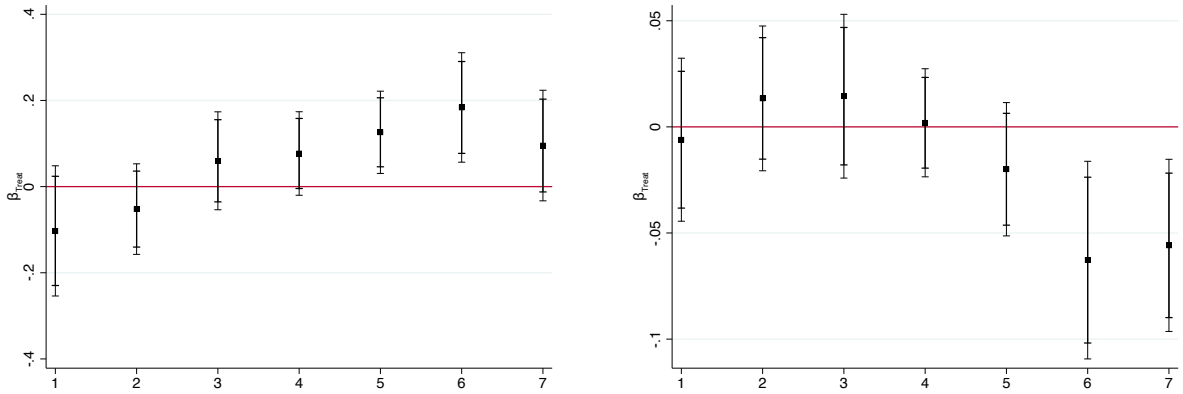
Figure 2: Number of credit relationships



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.

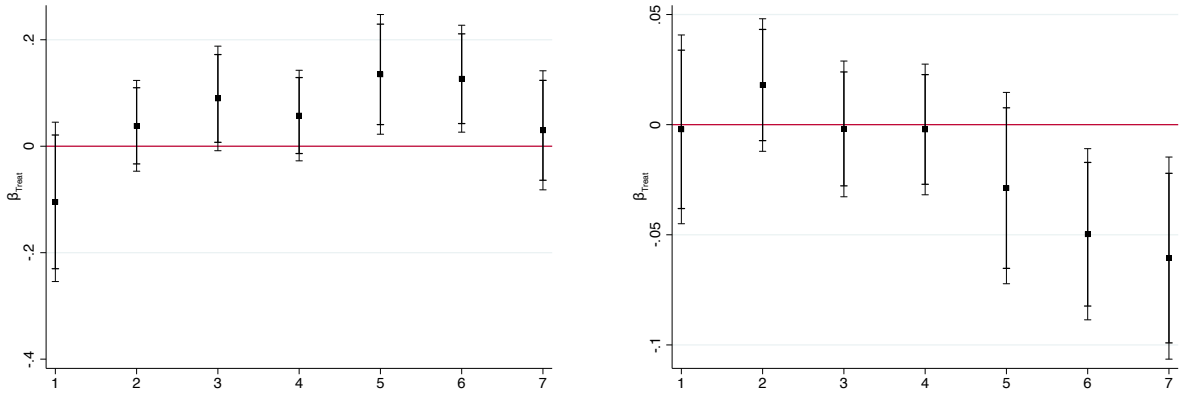
Figure 3: Robustness regressions by labor share bins

(a) Residualized labor share



Employment Exit

(b) Wage bill labor share

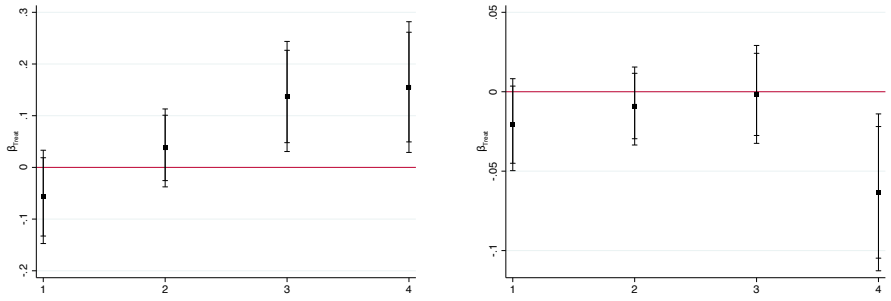


Employment Exit

For subfigures 3a we obtain the residualized labor shares by calculating the residuals in a regression of the labor share in value added per employee (2005-2006 average values) and the set of fixed effects that we control for in the regressions. For subfigures 3b we calculate labor share as the average of the labor share in value added per employee (2005-2006 average values), considering only the wage bill as labor costs. We estimate a coefficient for each of the seven labor-share bins, while controlling linearly for baseline effects. Each interacted treatment is instrumented by the interacted instrument. See Section A for the list of controls and fixed effects present in the regressions. Given that we cannot control for unobservable characteristics in the exit specifications through firm fixed effects, we characterize the bank-firm matching by augmenting the set of controls with the share of loans that each firm has with micro banks and with banks failing up to 2014. All fixed effects are interacted with a year dummy, while regressors are constant in the exit regressions. In the employment regressions fixed effects and regressors are interacted with the Post dummy. Number of firms: 13,750 (exit) and 13,760 (employment). 95 and 90% confidence intervals displayed, standard errors clustered at the bank-industry pair level.

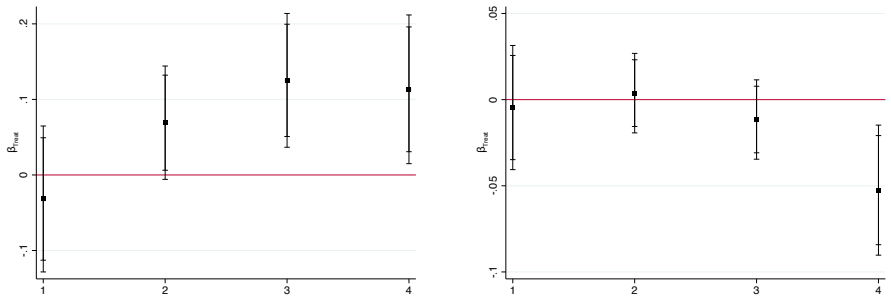
Figure 4: Robustness regressions by labor share bins

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



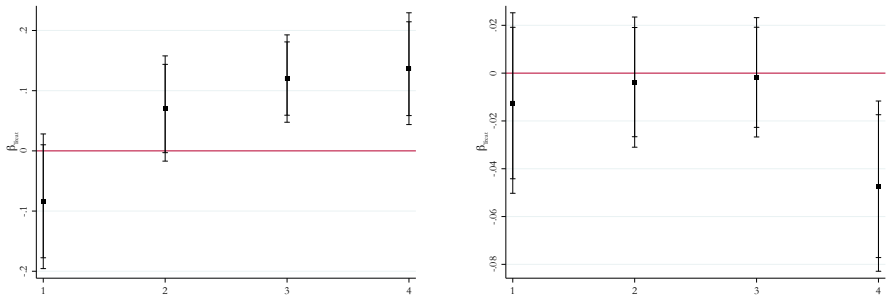
Employment Exit

(b) Labor share in sales



Employment Exit

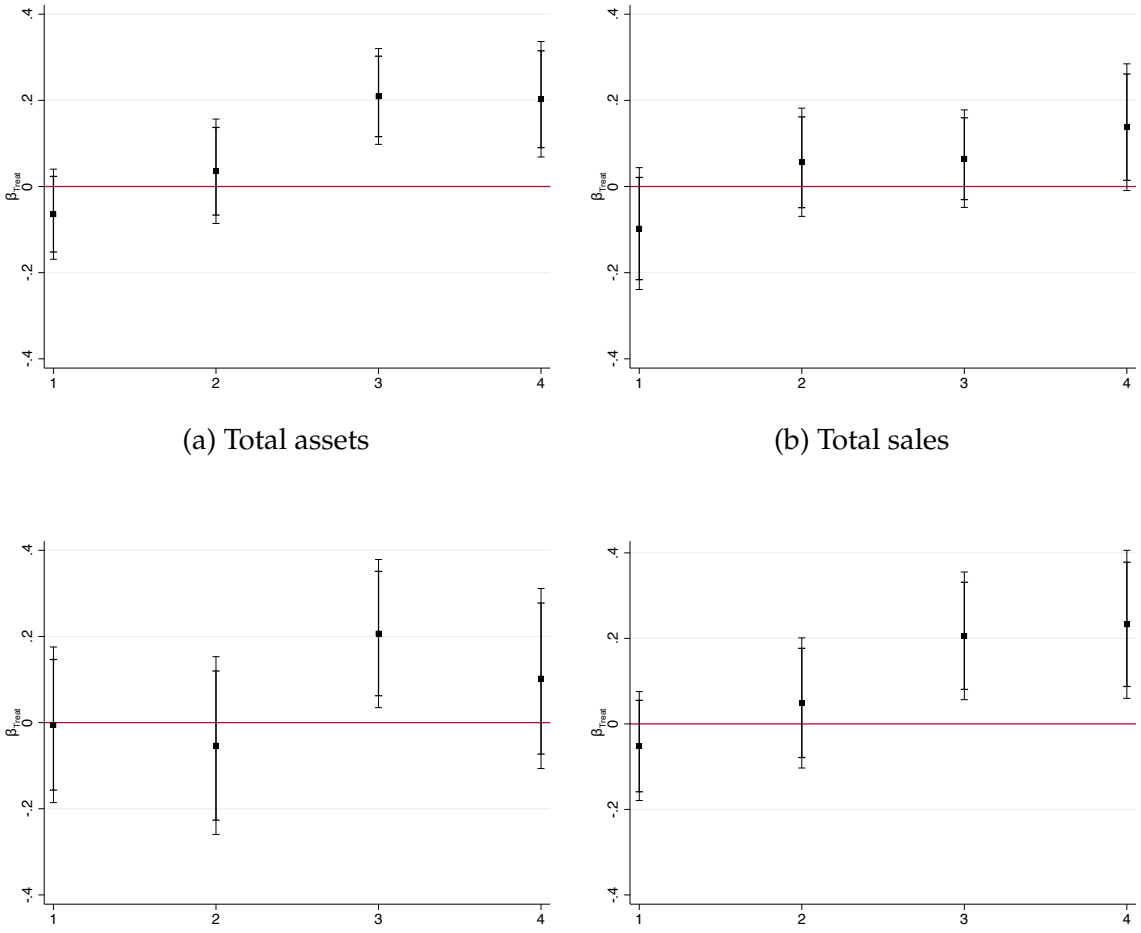
(c) Labor share in total costs



Employment Exit

For subfigures 4a we calculate labor share as the average of the labor share in value added per employee (2007-2008 average values). For subfigures 4b we calculate labor share as the average of the labor share in sales (2005-2006 average values). For subfigures 4c we calculate labor share as the average of the labor share in total firm costs (2005-2006 average values). We estimate a coefficient for each of the seven labor-share bins, while controlling linearly for baseline effects. Each interacted treatment is instrumented by the interacted instrument. See Section A for the list of controls and fixed effects present in the regressions. Given that we cannot control for unobservable characteristics in the exit specifications through firm fixed effects, we characterize the bank-firm matching by augmenting the set of controls with the share of loans that each firm has with micro banks and with banks failing up to 2014. All fixed effects are interacted with a year dummy, while regressors are constant in the exit regressions. In the employment regressions fixed effects and regressors are interacted with the Post dummy. Number of firms: 13,750 (exit) and 13,760 (employment). 95 and 90% confidence intervals displayed, standard errors clustered at the bank-industry pair level.

Figure 5: Balance sheet items and sales regressions by labor-share bins



(a) Total assets

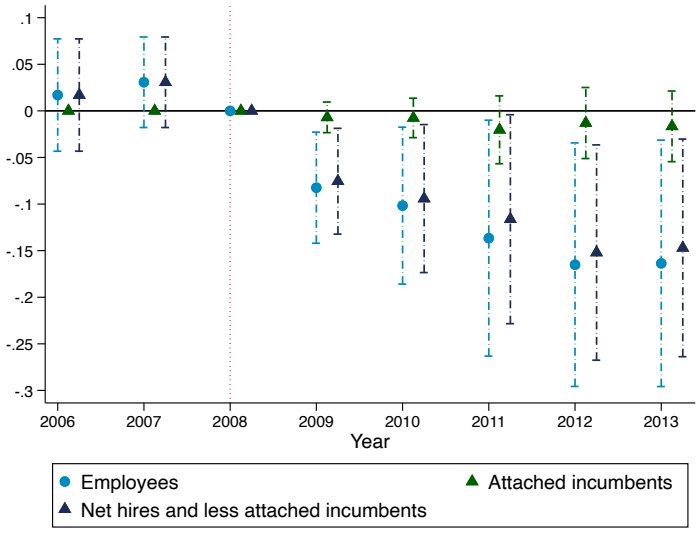
(b) Total sales

(c) Fixed assets

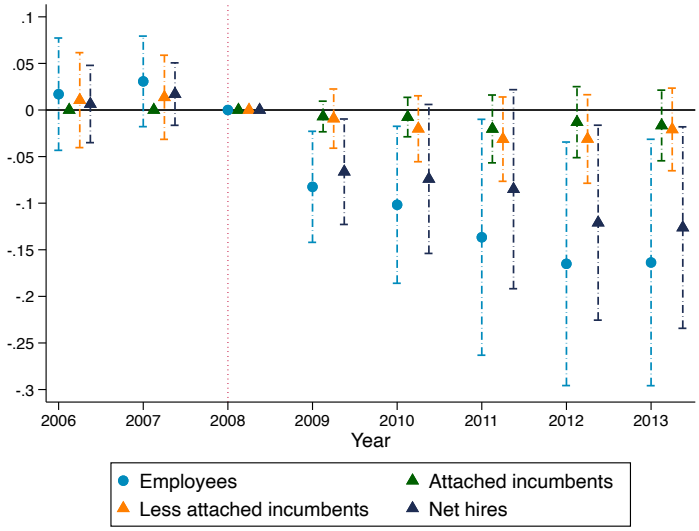
(d) Current assets

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

Figure 6: Employment adjustment by tenure



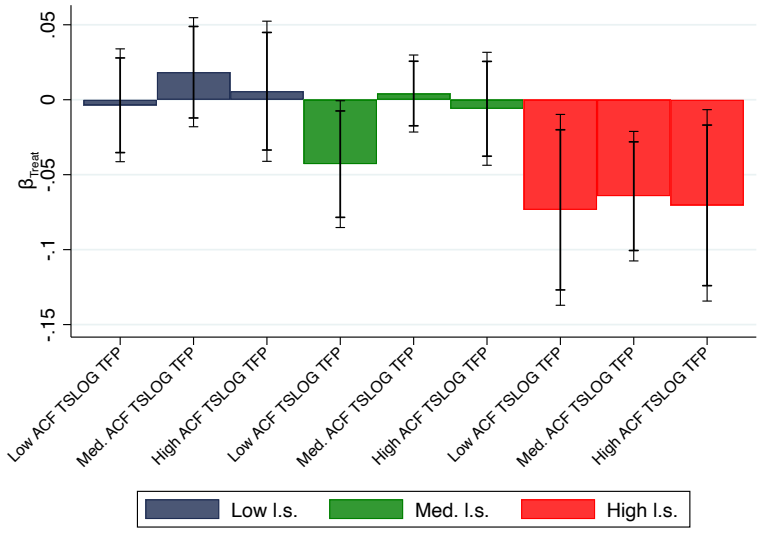
(a) Attached incumbents vs. all other workers



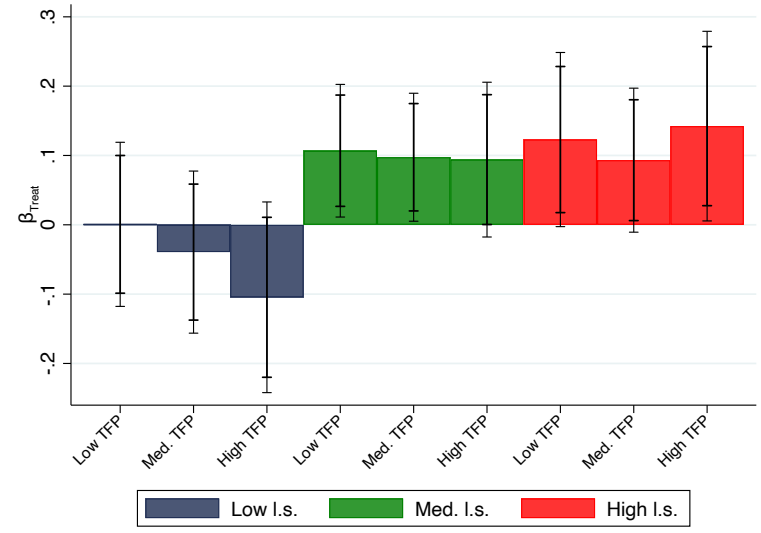
(b) Attached incumbents vs. less attached incumbents and net hires

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 percent of the workforce in the pre-period. The share of less attached incumbent is the remaining share of workers in 2008, and is always less than half of the attached incumbents share throughout the post-period. The sample includes only survivor firms (N = 11,801), but is not balanced. The graph displays the effect of a negative shock. See Section A for the list of controls and fixed effects present in the regressions. All regressors and fixed effects are interacted with a year dummy. 95% confidence intervals displayed, standard errors clustered at the bank-industry pair level.

Figure 7: Regressions by labor-share and TSLOG-ACF-productivity bins



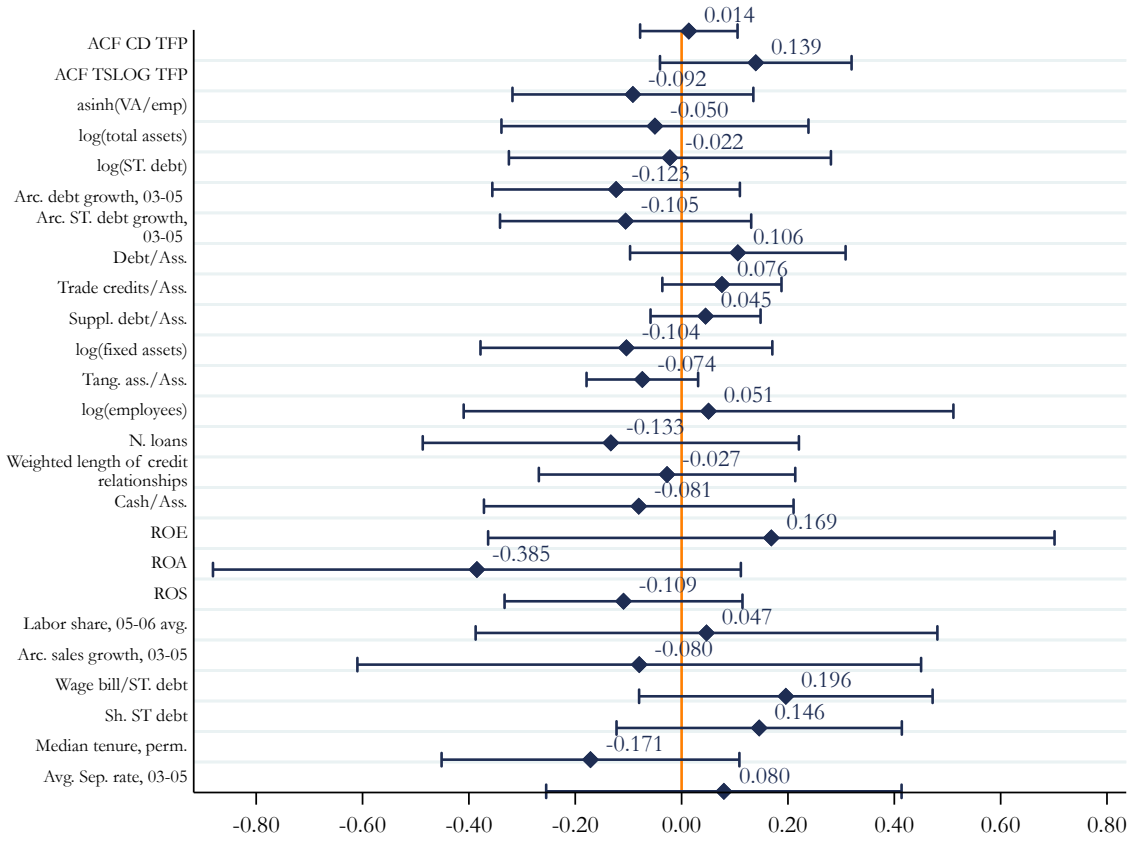
(a) Exit



(b) Employment

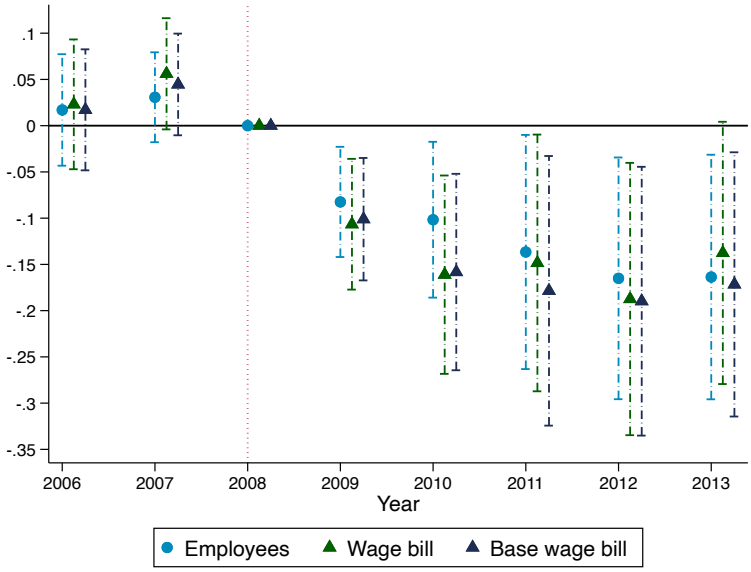
We estimate a coefficient for each of the nine interacted bins, while controlling linearly for baseline effects and their interaction. Each interacted treatment is instrumented by the interacted instrument. Labor share is defined as the ratio between employment-related costs and total value added (average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

Figure 8: Balance checks (Borusyak et al., 2022)



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

Figure 9: Employment and wage bill regressions: event study



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