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ISSN: 2365-9793

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ABSTRACT

The Long Run Earnings Effects of a Credit Market Disruption*

This paper studies the long term consequences on workers' labour earnings of the credit crunch induced by the 2007-2008 financial crisis. We study the evolution of both employment and wages in a large sample of Italian workers followed for nine years after the start of the crisis. We rely on a unique matched bank-employer-employee administrative dataset to construct a firm-specific shock to credit supply, which identifies firms that, because of the collapse of the interbank market during the financial crisis, were unexpectedly affected by credit restrictions. We find that workers who were employed before the crisis in firms more exposed to the credit crunch experience persistent and sizable earnings losses, mainly due to a permanent drop in days worked. These effects are heterogeneous across workers, with high-type workers being more affected in the long run. Moreover, firms operating in areas with favorable labor market conditions react to the credit shock by hoarding high-type workers and displacing low-type ones. Under unfavorable labor market conditions instead, firms select to displace also high-type (and therefore more expensive) workers, even though wages do react to the slack. All in all, our results document persistent effects on the earnings distribution.

JEL Classification: E24, E44, G21, J21, J31, J63

Keywords: credit crunch, employment, wages, long run effects, administrative data, linked bank-employer-employee panel data

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* We thank Andrew Ellul, Filippo Mezzanotti, Thomas Neuber, Giovanni Pica, Steve Pischke, Alexander Popov, Antoinette Schoar, Josef Zweimuller, and the participants at the Bank of Italy internal conference on Banking, the University of Mannheim Applied seminar, the University of Zurich seminar, the CUHK-RCFS conference in financial intermediation in Hong Kong, the MoFiR Workshop on Banking in Chicago, the Barcelona GSE Summer forum, the Endless Summer Conference on Financial Intermediation and Corporate Finance in Athens, the CRC TR 224 conference in Montabaur, the Inps/Bank of Italy "Labour market analysis: new opportunities with administrative data" workshop in Rome, the ECB/CEPR Labour Market Workshop "Labour market adjustments" in Frankfurt, and the Brucchi Luchino Labour Economics Workshop in Turin for helpful comments. The views expressed in this paper are those of the authors and do not necessarily reflect the views of the Bank of Italy. Financial support by the German Research Foundation through CRC TR 224 (Project A03) is gratefully acknowledged.

1 Introduction

How does the labour market adjust to major shocks? Do displaced workers find a new job or exit the labour force? What is the impact on the distribution of wages and earnings? These are key questions to assess the implications of major adverse economic events for the efficiency of the allocation of labour as well as for labour earnings inequality.

Since the burst of the Global financial crisis in 2007-2008 the economic literature has extensively investigated the short run consequences of that credit shock on employment, firm size and labor cost (see e.g. Chodorow-Reich, 2014, Greenstone et al., 2020 for the US, and Bentolila et al., 2018, Berton et al., 2018 for Europe). Less is known, however, about the impact on labour earnings and, more generally, about the long-run consequences of that shock on workers.

In this paper we study the effects of the credit crunch following the 2007-2008 financial crisis on employment, wages, and labour earnings of workers. We track their working history both in the short and in the long term, up to nine years after the shock first hit the economy. In particular, we look at both displaced workers and workers who remain attached to the firm where they were employed before the shock.

We rely on a unique dataset from Italy, which allows us to identify the shock that hits each firm in the sample and to disentangle its impact from other local labour market or industry-specific shocks. Our dataset is obtained by matching four administrative data sources: banks' balance sheet data, bank-firm level credit relationships taken from the Credit register, firms' balance sheet data and longitudinal matched employer-employee data, drawn from the Social Security database. The richness of our data allows us to address several important identification challenges. We obtain a plausibly exogenous measure of credit shock at the firm-level. Following the existing literature (for instance, Iyer et al., 2013 and Cingano et al., 2016) our identification strategy is based on the fact that the 2007-2008 financial shock (originated by the US subprime mortgage crisis and Lehman's default) caused a dramatic liquidity drought in interbank markets. As a consequence, banks that relied more on interbank borrowing before the crisis suffered more than banks relying on other liquidity sources. Firm's exposure to credit restrictions is then obtained as a credit-share weighted average of the pre-2007 interbank funding-to-assets ratio of all the banks lending to the firm the year before the beginning of the crisis. We perform several tests to support the hypothesis that our measure of firm exposure to the credit crunch is unrelated to previously observed firm specific trends. In particular, we show that when the first signals of tensions in the interbank market started to emerge there was no strategic sorting of firms into banks depending on the banks' reliance to the interbank market; moreover, we document that firms' exposure to the credit shock

is not systematically correlated with other labour market shocks at the province-year level. Finally, we provide evidence that the European sovereign debt crisis that came after the financial crisis did not hit banks differently depending on their pre-2007 reliance on interbank funding.

We focus on Italy, a large country with labor market institutions comparable to those of most major countries worldwide. Italy represents an ideal setting to identify the job market impact of the 2007-2008 crisis, since the crisis originated in the US housing market and was unrelated to conditions and developments of the Italian economy. In addition, Italy did not experience a housing bubble and Italian banks had an extremely small exposure to the US housing-related assets (Cingano et al., 2016).

We first present firm-level evidence on the effect of credit restrictions on firms' survival probability, size and average wages per employee. We find that firms with limited access to credit face a higher probability of exiting the market and that they shrink permanently more than less affected firms. They also end up paying their employees lower wages on average, but only in the short run.

Then, by relying on matched employer-employee administrative data, we compare the working histories of individuals employed before the crisis in firms differently exposed to the credit shock. Our setting has several advantages in this respect. Differently from the literature on mass layoffs (e.g. Jacobson et al., 1993, Davis and Von Wachter, 2011, Lachowska et al., 2018, Schmieder et al., 2019), which is confined to the analysis of displaced workers, we are able to study both displaced workers and stayers within mostly hit firms. In this way, we can identify which workers bear the largest cost and whether some form of insurance within the firm is taking place. Moreover, contrary to the trade literature (Autor et al., 2014, Utar, 2018, and Dauth et al., 2020), that analyzes the effect of the increased availability of lower-cost factor inputs – which are typically substitutes for low-skilled workers –, the credit shock that we are exploiting is fully exogenous to workers' characteristics. As a result, we can analyze whether and how firms select workers to be displaced. Lastly, since our measure of the shock is at the firm level rather than at the industry or local labour market level – and it appears to be unrelated to other shocks –, our setting allows us to assess how the firm-level credit shock interacts with external conditions, and in particular whether its effects vary under different local labour market conditions.

We distinguish among workers who remain in the same firm as in 2006 and those who move to another firm and we analyze three main outcomes: (i) total yearly labour earnings; (ii) the number of days worked per year; (iii) daily wages. We find evidence of highly persistent earnings losses on average for workers who in 2006 were employed in firms more exposed to the credit shock. A worker employed in a firm 10 pp more

exposed to the shock (which corresponds to the difference between the 90th and the 10th percentiles of the shock distribution) experiences a drop of around 2% in average yearly labour earnings over the period 2007-2016. Note that these are average effects on all workers. If we solely focus on displaced workers, the reduction in earnings is about 20%, in line with the findings of the literature on mass layoffs (Lachowska et al., 2018, Schmieder et al., 2019). This drop in labour earnings is mainly due to a persistent reduction in days worked. Workers employed in more exposed firms experience a loss in the number of days worked in the same firm as in 2006, consistently with firm-level findings. Some of the displaced workers manage to find a new job in another firm, but the aggregate number of days worked does not fully recover even several years after displacement.

Overall, the effects are highly heterogeneous, with potentially large consequences on economic efficiency and earnings inequality for the population of workers we consider.[1] First, we document that earnings losses are more persistent among high-paid workers. This is partly explained by a supply-side mechanism as these workers are more likely to retire early after displacement. This implies a reduction of earnings inequality, but through a worsening of the conditions of high paid workers rather than through an improvement for low paid workers. In addition, to the extent that high paid workers are more productive, this also implies that economic efficiency may be negatively affected. Second, we find that the effects critically depend on the average labour market conditions of the local labor market where workers used to be employed, in particular for high-paid workers. We find that firms operating in areas with structurally favorable labor market conditions hoard high-type workers and displace low-type workers. Under unfavorable labor market conditions instead, firms select to displace also high-type (and therefore more expensive) workers. This is true even though wages do react to the labour market slack. Wage growth decreases in provinces with high unemployment rate both among low-type workers, who accept wage cuts to stay in the 2006 firm, and among high-type workers, who obtain lower wages when —once fired— move to different firms. This also explains why firms in high unemployment provinces choose to fire also high type workers in the first place: they are then able to find similar workers at lower wages. Third, we document that firm specific human capital - as measured by workers' tenure - explains part of the observed wage losses among more exposed workers, as wages drop only for long-tenured workers, especially if they move to a new firm where they cannot transfer their firm specific skills. Finally, we show that persistent earnings losses did not arise as a consequence of a negative reallocation process that induced displaced workers to systematically find jobs in lower quality firms offering worse average wage prospects. We

[1] These are workers who were already present in the labor market in 2006, without considering new entrants.

show instead evidence of a positive reallocation of workers towards better firms, although not all workers manage to completely recover their days worked.

Taken together, our results suggest that earnings losses of more exposed workers do not consist in a fixed penalty attached to laid-off workers, but largely depend on the conditions of the local labour markets where they are employed, as well as on the propensity or ability of workers of different type to move to a new job. Moreover, our results stress the importance of going beyond the short term impacts on workers. Indeed, we find that, while in the short term low- and high-type workers are similarly affected, the long term consequences are mostly concentrated on high-type individuals.

Our work relates to several strands of the literature. On the one hand, it speaks to the literature on the real effects of credit supply shocks (Acabbi et al., 2019; Amiti and Weinstein, 2018; Cingano et al., 2016; Paravisini et al., 2015). More specifically, we contribute to the literature on the impact of credit shocks on employment (Acharya et al., 2018; Berton et al., 2018; Bentolila et al., 2018; Giroud and Mueller, 2017; Popov and Rocholl, 2016; Chodorow-Reich, 2014; Greenstone et al., 2020; Benmelech et al., 2011). We extend this literature in two key dimensions. First, we do not only look at individual workers' employment status, but we also observe workers' wages and labour earnings. We find that the negative impact on earnings is mainly related to the difficulty of finding a new job for high wage employees in high unemployment areas. Second, we focus on the long-run effects of the shock and we study their persistence. This allows us to fully gauge the effect of the shock on the working histories of individuals. Indeed, we show that the reduction in firm size translates very heterogeneously into a reduction in workers' earnings, and that the type of heterogeneity is different in the short and in the long run.

Our work also relates to the literature on the long-term consequences on workers of industry-level shocks, such as trade liberalizations (e.g. Autor et al., 2014; Utar, 2018; Dauth et al., 2020) or of displacements following a mass layoff (e.g. Jacobson et al., 1993 and more recently Lachowska et al., 2018, Schmieder et al., 2019, Davis and Von Wachter, 2011, Gathmann et al., 2020, among others). Differently from the literature on industry-level shocks, our shock is heterogeneous across firms, thus allowing us to dig into the mechanisms behind aggregate results. We study possible effects on workers' reallocation not only across sectors but also across firms of different types and we look at the heterogeneity of the response depending on the type of the local labour market that the firm is facing. We contribute to the literature on mass layoffs in several dimensions. On the one hand, we are able to analyze how firms choose whether and who to fire. On the other hand, we can study what are the effects on workers who remain in the most affected firms. Moreover, mass layoffs are usually dramatic events for the local labour market of the failing firm. In our setting instead, we can also observe hardly affected firms

operating in barely hit local labour markets. We can therefore study the heterogeneity of the response of wages and employment under very heterogeneous local labour market conditions. Our results show that the level of local unemployment, together with firm-specific factors, is a very important determinant of the wage and employment losses of affected workers in the long run.

Finally, our paper speaks to the recently growing literature that studies the extent to which shocks to firms' performance are transmitted to their employees' pay (for instance, Macis and Schivardi, 2016; Card et al., 2015, 2014; Guiso et al., 2005; Blanchflower et al., 1996). We add to this literature by showing that rent-sharing largely depends on the local labor market conditions the firm is facing and that employees who separate from the original firm bear a much larger cost than those who stay in the firm.

This paper is organized as follows. Section 2 describes the data and the construction of our sample. Section 3 describes the empirical strategy and shows some supporting evidence on its validity. Section 4 presents the main results at the firm and at the worker level and explores the possible mechanisms behind the employment and wage losses. Finally, Section 5 briefly concludes.

2 The data

We rely on a unique dataset combining data from five different sources. The first and most important for our paper is the employer-employee matched dataset, provided by the Italian Social Security Institute (INPS). This includes a random and representative sample of 6.5% of the Italian workforce, drawn from the universe of private employees. The data contain information on the main demographic characteristics (age, gender, country of birth) and on all job spells (daily wages, occupation, type of contract, length of the spell). This dataset also contains the unique firm tax identifiers, which allow us to match any type of firm characteristics to the worker sample.

At the firm level we obtain information from two sources. The Social Security Institute (INPS) provides data on some firm characteristics (size, average monthly wage, sector and location of businesses) for all private sector firms with at least one employee. We match this dataset with the register of incorporated firms, CERVED, which collects balance sheet data from the Italian Chambers of Commerce. Hence, the sample includes incorporated firms for which we observe balance sheet information from the firm register (this includes between 600,000 and 700,000 firms per year).

Finally, the last two data sources are related to banks and credit relationships. We obtain information on bank-firm credit relationships from the Italian Credit Register administered by the Bank of Italy, which includes all credit commitments above 75,000

euros (30,000 since 2009) by banks operating in Italy. For each firm-bank pair, we recover the end-of-year total granted credit. The banks' exposure to the interbank market is obtained from the bank balance sheet information taken from the Supervisory Reports. We compute the reliance of banks on interbank funding as the ratio between interbank deposits, including repurchase agreements (repos) and banks' total assets. In particular, we use the average bank exposure between 2003 and 2006.[2] We match these data using the unique bank identifier, and then we use the unique firm tax identifier to match them with the social security and firm balance sheet datasets. This allows us to obtain unique quality information at the worker level matched with firm-level data and a measure of firm exposure to the credit restrictions following the Lehman collapse.

We construct our worker sample as follows. We consider individuals aged between 20 and 50 in 2006, who were at that time employed in corporations with outstanding credit relationships appearing in the Italian credit register. We drop workers younger than age 15 at the beginning of the period, since 15 is the minimum working age in Italy. We also drop workers older than age 60 at the end of the period, who may have already access to retirement programs in Italy during the period we consider. Moreover, we restrict attention to workers with strong labor market attachment, who had at least three years of experience in 2006 (had worked in the 2006 firm for at least 200 days per year in the period 2003-2005). As a result, our sample only includes permanent workers. We do so for two main reasons. First, these restrictions follow largely the existing literature, allowing us to better compare our results to those of the literature on mass layoffs (see, e.g. Lachowska et al., 2018, Schmieder et al., 2019) and on the long term effects of trade shocks (Autor et al., 2014). Second, while firing costs in Italy are null for temporary workers, permanent workers are protected by employment protection rules that are similar to those in place in other countries (i.e. firing costs are increasing in firms' size and workers' tenure, see Bentolila et al., 2020 for a review), making our results easier to generalize and interpret.[3] Temporary workers represent in any case a small portion of the employees in the Italian private sector (about 12%), and their exclusion implies, if anything, that we are underestimating the employment losses, as there are no firing costs for temporary workers. We then follow these individuals from 2002 till 2016.[4] Overall, our data include around 1.6 million worker-year observations.

[2]We use data consolidated at the bank holding company level for banking group and individual bank data from stand alone entities. This is important to net out the flows of funds among banks within the same banking group.

[3]See, for instance, Sestito and Viviano, 2018 for an assessment of the role of firing costs in firms' firing and hiring decisions in Italy.

[4]While for the firm level analysis we have data till 2018, worker level data (matched employer-employee) are available at the moment only till 2016. Data on firms' access to credit are instead available till 2019.

Our three worker-level outcomes of interest are defined as follows: (i) total earnings, that consist of the sum of all labour earnings obtained by an individual from all his/ her employers in the considered year (0 if he/she is not employed) as reported to the Social Security Institute, i.e. net of employers' social security contributions and before income taxes; (ii) number of days, which is the total number of days worked (including paid holidays but excluding Sundays); (iii) daily wages, computed as the ratio between total labour earnings and number of days worked (normalized to 1 in 2006).

In our analysis we use nominal daily wages.[5] Our measure of wages includes the base-fixed component as well as premia and bonuses, that vary over time. On average, about 20% of Italian workers' wages are individually negotiated with the employers and consist of a flexible part, mainly composed by premia and bonuses (see, for instance, Adamopoulou et al., 2016). The remaining part of the wages is centrally negotiated through nation-wide collective agreements, which set minimum wages at the sector level for different occupation classes. These contracts are typically renewed every three years (it used to be every two years before 2009) by the main social partners. In the period we consider, nominal increases of the base (minimum) wage were usually benchmarked to an independent 3-year-ahead forecast of inflation net of imported energetic goods, which was always positive. Similar forms of collective bargaining are present in many other European countries, e.g. France, Spain, Portugal etc., see Villanueva (2015).

In the firm-level analysis, we use a sample of firms that includes the same corporations as the ones we observe in the workers' sample. We consider four main outcomes: i) credit granted to each firm every year, which we use to estimate the impact of interbank exposure on firms' ability to obtain credit, ii) a dummy that takes the value 1 when the firm exits the market and 0 otherwise, iii) a variable that measures the firms' yearly average number of employees (and is equal to 0 if the firm exits the market) and iv) a variable measuring average monthly wages of all employees in the firm (normalized to 1 in 2006).

3 Identification strategy

3.1 Measuring the exposure of firms to the credit crunch

Our first step is to identify which firms were more exposed to the credit crunch generated by the Global financial crisis. The Italian banking system has been severely hit by the 2007-2008 financial crisis. The shock was arguably exogenous with respect to the conditions of Italian banks: The Global financial crisis originated in the U.S. subprime mortgage market, a small market segment to which Italian banks were not exposed to

[5] All our regressions include year fixed effects and thus account for changes in average inflation over time.

(Panetta and Signoretti, 2010). Italian banks have been instead largely hit by the liquidity drought in interbank markets that started in August 2007 (Brunnermeier, 2009). This shock has been heterogeneous across banks, depending on their exposure to interbank (wholesale) funding. This is asserted by the existing literature (Iyer et al., 2013; Cingano et al., 2016) showing that banks' reliance on interbank funding is a good proxy for their exposure to the 2007-2008 financial crisis and for the extent to which they restricted credit supply. We refer to these works for further details on the transmission mechanism from tensions in interbank funding markets to lending. Figure A.1 in the Appendix shows that in our data overall credit growth, whose variations can be driven by both changes in supply and in demand of credit, started dropping in 2007, fell very sharply in 2008, and kept contracting at a high pace in subsequent years, as economic activity contracted and banks kept deleveraging.

We measure the average exposure to interbank funding for each bank operating in Italy using the balance sheet data contained in the Supervisory Reports. In practice, for each bank b we calculate the interbank funding (deposits plus repurchase agreements) to total assets ratio averaged over the period from 2003 to 2006.[6] We label this variable as $Interbank_{b,06}$, and this is our measure of banks' exposure to the Global financial crisis. Importantly, we find that the interbank funding to total assets ratio is not correlated with key bank characteristics, which could drive banks' lending policies or the quality of banks' loans. In particular, it shows no correlation with capital profitability, bad loans ratio, and bank size (Table A.1). The interbank funding ratio is negatively correlated with the ratio of retail deposits to total assets and with liquid assets. This is reassuring, because both retail deposits and liquid assets are substitutes for interbank funding.

Since firms have multiple relationships with banks, for each firm-bank relationship we define a variable $Interbank^{f}_{b,06}$ equal to the exposure to the interbank market of bank b lending to firm f in 2006. We calculate our measure of firm's f specific risk of credit crunch by averaging bank exposure $Interbank^{f}_{b,06}$ over f. For each firm, $Interbank^{f}_{b,06}$ is weighted by the share of loans granted to firm f by bank b as of end-2006 ($credit_{f,b,06}$) over total loans granted to firm f in 2006, $credit_{f,06}$.

Formally:

$$Interbank_{f,06} = \Sigma_b Interbank^{f}_{b,06} * \frac{credit_{f,b,06}}{credit_{f,06}}, \tag{1}$$

In our sample, the exposure to the credit shock ($Interbank_{f,06}$) is 15%, on average, with a standard deviation of 6%.

For this to be a good measure to identify firms' exposure to the credit crunch following the crises, two conditions need to be met. First, we should exclude any possible sorting

[6]Data on bank balance sheet variables are observable at semi-annual frequency, thus the average comprises 8 dates from June 2003 to December 2006.

of firms into banks at the very first signals of tensions in the interbank market in 2007. To ensure this is not the case, we measure banks' interbank exposures as the average of 2003-2006, i.e. before the beginning of the crisis. We also run regressions of the change in the share of credit in each bank-firm relationship between 2000 and 2005 on the bank's exposure to interbank market in that period ($Interbank_{b,06}$, Table A.2). We find that interbank exposure is never statistically significant (and by a wide margin). This shows that banks more reliant on interbank funding in the period before the crisis were not systematically expanding credit.

Second, the interbank shock had to be unforeseen. This is the case as far as we assume that Italian firms in 2006 were unable to forecast both the abrupt and sudden stop in the interbank market occurred since 2007 (worsening after the Lehman Brothers' default in 2008) and their bank's exposure to it. This is indeed a reasonable and widely shared assumption. The firm-level exposure measure we adopt is analogous to the ones used for instance by Iyer et al. (2013) and Cingano et al. (2016), and it also follows the same logic as Chodorow-Reich (2014), who uses different proxies of banks' exposure to the crisis, i.e. their participation in loan syndicates in which Lehman was the lead arranger or the exposure to asset backed securities (ABS) measured by the correlation between banks' stock prices and an index of the value of ABS. Italian banks had limited direct exposure to Lehman-related assets or to ABS (Cingano et al., 2016), which prevents us from using the same measure.

To test whether our measure of interbank exposure correlates with the evolution of firms' loans we estimate the following regression at the firm-year level:

$$\Delta credit_{f,t} = \theta Interbank_{f,06} * \alpha_t + \alpha_f + \epsilon_{f,t}, \tag{2}$$

where $\Delta credit_f$ is the year-on-year growth rate of loans granted to firm f, $Interbank_{f,06}$ is our measure of the risk of being subject to the crunch, that ranges between 0 and 100, α_t are year dummies and α_f are firm fixed effects. This specification allows us to check for pre-trends which would invalidate our identification strategy and to look at the evolution over time of credit to firms after the shock. We estimate the model from 2002 to 2018.

Figure 1 reports the estimated coefficients. Relative to the omitted year (2005), before 2006 the elasticity of firm loans to firm exposure to the shock ($interbank_{f,06}$) is small, positive, and often not statistically different from zero. Afterwards it becomes negative and significant, signalling a structural break in 2007. After 2007, a percentage point increase in our index of credit crunch is associated to a -0.3 pp average reduction in the growth rate of loans (that corresponds to about one standard deviation in credit growth). Interestingly, the elasticity remains stable also after the sovereign debt crisis (which burst in mid-2011). Overall, Figure 1 suggests that the structural break in the

elasticity of credit to interbank market exposure has occurred since 2007. After that, banks more reliant on interbank funding kept supplying less credit than other banks. This evidence supports our strategy to identify firms more affected by the credit crunch as those that borrowed relatively more from banks that ex-ante used to rely more on interbank funding.

A last point to notice is that Italy has been hit by a second shock, the European sovereign debt crisis, in the summer of 2011 after private sector agents were involved in the restructuring of Greek debt in late June 2011 (Bofondi et al., 2017, Correa et al., 2018). Italy became involved in the crisis because of its high level of sovereign debt. This is a long standing problem of the Italian economy and was not due to the rescue of Italian banks as it occurred, e.g., in Ireland or Spain, as documented in Bofondi et al. (2017) and the references therein. We argue that this second shock did not hit banks depending on their reliance on interbank funding before the 2009 crisis and therefore, it does not represent a threat to the identification of our effects.

We support this point based on several results. First, Figure 1 does not show any change after 2011 in the difference in credit growth between firms more and less exposed to banks affected by the Global financial crisis. The larger differential effect occurs in 2007-2008 and then the difference remains rather flat. Second, we run eq. 2 for the change in credit between 2011 and 2015, controlling for the change in credit between 2010 and 2006 and its interaction with $Interbank_{f,06}$. This allows us to assess whether, during the 2011 sovereign debt crisis, banks more exposed to the interbank market in the 2003-2006 period restricted credit more than banks less exposed, after controlling for the reduction already occurred during the Global financial crisis (2008-2009). The results, shown in Table A.3, indicate that when controlling for the credit growth between 2006 and 2010 (after the Global crisis), $Interbank_{f,06}$ does not have a significant effect on credit growth between 2015 and 2011 (after the sovereign debt crisis). Third, Table A.4 shows that banks' interbank funding ratio as of 2006, $Interbank_{b,06}$, is not correlated with other key bank characteristics as of 2010-2011 (capital, profitability) and it is actually negatively correlated with the share of government bonds to assets, which could be used as proxy for the impact of the sovereign debt crisis on Italian banks. Fourth, banks' interbank funding ratio as of 2006, $Interbank_{b,06}$ is positively correlated with the change in banks' average cost of funding between 2007 and 2010, but not between 2011 and 2015 (Table A.5). Finally, existing evidence shows that the drop in credit during the sovereign debt crisis has been more homogeneous across banks. For instance, Bofondi et al. (2017) show that the contraction in credit supply after the sovereign debt crisis was independent of banks' characteristics, including the funding structure, and instead driven by the nationality of the bank holding company. Taken together, all these pieces of evidence suggest that the

interbank funding ratio as of 2006 is a good proxy for the impact of the post-Lehman shock on banks, but not for that of the eurozone sovereign debt crisis.

A final concern is that firms' exposure to the credit shock may be systematically correlated with the labour market shocks at the province-year level. Figure 2 plots the provincial yearly unemployment rate against the share of firms highly exposed to the interbank market. The graph clearly shows that there is a large degree of heterogeneity across provinces, and that the firm-level exposure to the interbank shock is not correlated with the cross sectional variability in the provincial unemployment rate.

3.2 Identifying affected firms and workers

The goal of the paper is to document the long term effects on the career of workers employed in firms hit by the credit shock. To this aim, we classify as affected workers, those employed before the crisis in firms that used to borrow from banks more exposed to the interbank market (as measured by the variable $Interbank_{f,06}$). We then compare more and less affected workers adopting a difference-in-differences strategy.

Table 1 shows summary statistics for the worker-level sample, distinguishing by the intensity of the shock: top 33-percentile of exposure (column 1) and rest of the sample (column 2). The upper part of Table 1 reports the characteristics of the firms where the workers were employed in 2006. The table suggests that most treated firms are slightly larger, pay on average higher wages, and are younger than less treated firms.[7] This occurs because smaller firms tend to be customers of smaller banks, less exposed to the interbank market. Nevertheless, as shown in the previous section, this structural characteristic of the Italian banking system has not changed in response to the expansion of credit before the Global financial crisis (i.e. we do not find evidence of strategic firm-bank sorting or of differential pre-trends). The lower part of the Table reports summary statistics for workers. Workers in most exposed firms are more likely to be men, of a higher occupational level (white collars or managers), and tend to earn higher daily wages on average.

Since we adopt a difference-in-differences strategy, for our identification to hold, there can be differences in the type of workers and firms that are more exposed to the credit supply shock, as long as trends in the outcomes of treated and control workers would have been parallel absent the shock. In the regression analysis we always check that trends were parallel before the crisis. However, the credit shock that occurred during the Global financial crisis was followed by a series of other, non-necessarily credit related, negative shocks, whose impact was heterogeneous depending on firms' characteristics, industries

[7] Our sample includes a much larger fraction of small firms than the sample used in Cingano et al., 2016 in which exposure to interbank market was balanced across firm characteristics.

and local labour markets. For our strategy to identify only the effect of the credit shock, we need to ensure that the workers and the firms we consider were comparable in terms of the impact of other possible shocks.

To this aim, since Table 1 suggests the presence of some differences in observable characteristics across more and less exposed firms and workers, we rely on a propensity score matching procedure to obtain a balanced sample. In particular, we use a step matching estimator in the spirit of Schmieder et al., 2019 and Dauth et al., 2020, and we match workers within 1-digit industries based on a number of matching variables measured before the credit crunch. Specifically for each 1-digit industry, we estimate the propensity of being employed in 2006 in a firm belonging to the top 33 percentile of the distribution of exposure to the interbank market. The matching variables, both at the worker and the firm level, all refer to the base year (2006) and are the following: gender, age (linear and squared), full-time contract, tenure within the firm (distinguishing between less than 2 years, 2-5 years, 6-9 years, 10 years or more), average (log) daily wage (linear, squared and cubic) and earnings in 2006 (linear, squared and cubic), firms' leverage (if firm's debt to asset ratio is above the 75th percentile), firms' age (linear and squared), firms' size and firms' average monthly wage. Columns 3 and 4 of Table 1 display the average characteristics of workers employed in 2006 in more and less exposed firms, weighting for the propensity score. With this procedure, we obtain a highly balanced sample.

Before estimating the results at the worker level, we carry out firm level estimates to show how firms exposed to the credit shock adjusted their labour input, i.e. their size and average wage paid to their workers. We estimate the following equation on the sample of firms included in the worker level analysis:

$$y_{f,t} = \gamma Interbank_{f,06} * \delta_t + \delta_f + u_{f,t}, \tag{3}$$

where $y_{f,t}$ is the firm f outcome in year t, and $interbank_{f,06}$ is firm f exposure to the credit shock. Year fixed effects δ_t capture general shocks; δ_f are firm fixed effects.

Note that the combination of Equation 2 and 3 gives us also the IV estimate for the impact of credit on firm size and average wages.

Next, we estimate the response of workers' earnings, daily wage and days worked to the credit shock faced by the firm where the employee used to work in 2006. The model is:

$$y_{i,f,t} = \beta Interbank_{f06} * \alpha_t + \alpha_i + \alpha_p * \alpha_t + \epsilon_{i,f,t}, \tag{4}$$

where $y_{i,f,t}$ is the outcome variable of worker i in year t employed in firm f and $interbank_{f06}$ is the exposure to the credit shock of firm $f06$ employing the worker i in

2006. The term α_i indicates worker fixed effects (which implicitly also controls for $f06$ fixed effects). We saturate the models by including province*time fixed effects ($\alpha_p * \alpha_t$). In this way we control for possible time-varying confounders, most notably business cycle fluctuations at the local level.

A potential important issue is how to treat the individuals who lose their job and those who move from one firm to another. Workers may decide to switch to other firms, either because they get fired, because they are subject to wage cuts, or because they see that the perspective of the firm they are currently employed is deteriorating. In this case, we track workers across firms and attribute to them the interbank exposure of the firm they were employed in 2006, as any successive movement may be part of the endogenous response to the shock. This choice, aimed at strengthening identification, implies that in our worker-level regressions we can only estimate the intention-to-treat.

Importantly, to better understand the mechanisms behind our results, we distinguish between all workers and stayers (for whom we use observations only for the years they remained in the same firm they were employed in 2006) and between the number of days worked per year in any firm during the period 2000-2016 and in the same firm workers were employed in 2006. This allows us to fully assess the impact of the credit shock: if workers were fired but then could easily find a new job, the impact of the credit crunch on earnings would be short lived. It could still be sizable if workers had to accept a lower wage in the new job. The richness of our data allows us to perform these tests.

We further test whether the effects of the credit shock are heterogenous across types of workers. Throughout the paper we define high- and low-type workers as workers with wages above/below the median in 2006 (net of age, gender, part time contract, tenure, firm size, province and sector fixed effects). We compute this measure by regressing individuals' wages in 2006 on these controls and taking the residual.[8] To the extent that high paid workers are more productive, this allows us to understand how the crisis affected the most or the least productive workers and, potentially, the implications this may have also on the efficiency ground (Hellerstein et al., 1999). In addition, it allows us to contribute to the debate on the distributional impact of negative shocks (Autor et al., 2014). Table 2 reports summary statistics by worker type. Their characteristics turn out to be well-balanced across more and less exposed workers, also within each type. High type workers are usually employed in larger and higher paying firms, they tend to be similar in terms of age and gender, they are more likely to be white collars or managers, they are more mobile in the labour market and their wage is on average about two times higher than that of low-type workers.

[8]We implement a similar procedure to distinguish between high- and low-type firms. We define as high- and low-type firms those with average wage per employee (net of size, age and province and industry fixed effects) above/below the 50th percentile in 2006.

4 Results

4.1 Firm-level effects

We first provide direct firm level evidence of the effects of the credit shock on firms' outcomes between 2002 and 2018. Table 3 reports the reduced form and first stage estimates of the effect of the credit crunch on the firms' probability of exiting the market, on their size, and on their average monthly wage per employee (normalized to 1 in 2006). Column 1 shows the effect of being more exposed to the credit supply shock on the change in credit granted to firms (first stage). The results are consistent with the evidence presented in Figure 1; they indicate that a 10 pp higher exposure to the interbank market of the banks lending to the firm (which corresponds to the difference between the 90th and the 10th percentile of the shock distribution) implies a 3 pp reduction per year in credit growth at the firm level.[9] Column 2 displays the effect on the probability of exiting the market, column 3 reports the estimates on firm's size and column 4 those on average monthly wage per employee (normalized to 1 in 2006). In particular, our results imply that a 10 pp higher exposure to the shock leads to a 0.4 pp increase in the probability of exiting the market and a 3% reduction in firm size (which is on average 40 in our sample). As shown in Panels a. and b. of Figure 3 these effects are persistent, especially the latter. Moreover, firms more exposed to the credit shock end up paying their employees less on average compared to less affected firms. According to our estimates, a 10 pp reduction in the exposure to the shock implies a 0.3 pp reduction in average monthly wage per employee relative to its 2006 level. However, as shown in Panel c. of Figure 3, this effect is less persistent as the difference in average wage per employee between more and less affected firms fades away in the medium run (after 2014). The negative effect on average wage per employee at the firm level could be driven either by a downward adjustment of the wages of those workers who stay in the firm they were employed before the shock or by a re-composition of the firms' workforce towards lower paid employees. The worker level analysis in the next section allows us to disentangle these effects and it provides supportive evidence of the latter channel, as workers who are more likely to permanently lose their job following the credit shock are the most expensive ones.

4.2 Worker-level effects

Next, we present evidence at the worker level. Table 4 shows the effect of having been employed in 2006 in firms borrowing from banks that used to be more exposed to the

[9]This is the regression version of Figure 1 in which the effect of the credit shock is averaged over the period 2007–2018, by the inclusion of a dummy equal to one for the years 2007–2018 interacted with the shock ($interbank_{f,06} * post06$).

interbank market on (i) total labour earnings, (ii) the yearly number of days worked, and (iii) daily wages (normalized to 1 in 2006). To understand whether the shock was mostly born by workers who remained employed in the same firm as in 2006 (stayers) or by individuals who were forced or preferred to change firm (movers), we also re-run estimates for stayers only (columns 2 and 5).

Column 1 of Table 4 and Panel a. of Figure 4 show that labor earnings are persistently lower for workers, who before the crisis were employed in more exposed firms. The earnings losses are mainly due to a drop in the number of days worked in the original, 2006, firm (Column 2 and Panel b). In other words, workers lose their jobs as a result of the credit shock. Subsequently, some of these workers manage to find new jobs in other firms but the number of days worked does not fully recover (Column 3 and panel c). Moreover, workers who move to new jobs earn on average lower wages (Column 4 and Panel d). A worker employed in a firm at the 90th percentile of exposure to the credit shock experiences a drop of around 2% in average yearly labour earnings over the period 2007-2016 (which were on average 27,000 euros in our sample) relative to workers employed in firms at the 10th percentile of exposure to the shock.[10] This amounts to around 480 euros per year, which is not negligible. However, these are average effects on all workers, not only on displaced workers. If we focus solely on displaced workers, the reduction in earnings is about 20%, which lies within the range of earnings losses typically found in the literature on mass layoffs (e.g. Lachowska et al., 2018, and Schmieder et al., 2019).

Overall, our results indicate that in order to quantify the longer-term labor market impact of a major shock, such as a credit shock, it is crucial to consider not only the workers' probability of remaining employed but also their probability of finding a job after displacement. While being fired or induced to leave the original firm may even trigger a beneficial labor reallocation process, the ability of displaced workers to find new jobs as well as the wage losses that may have to bear are crucial to assess the impact on earnings inequality, and thus get a sense of the overall longer-term effects of the shock.

4.3 Investigating the mechanisms

4.3.1 Heterogeneity by type of workers

In order to better understand the mechanisms behind our main findings, we first explore the heterogeneity by workers' type (Table 5 Panel a and Figure 5). We find that high-type workers experience larger and more persistent earnings losses than low-type workers.

[10]The difference between the 90th and the 10th percentile in the distribution of the shock corresponds to a 10 pp higher exposure to the interbank market of the banks lending to the firm, which implies a 3 pp reduction in the growth of credit granted to the firm.

More specifically, labour earnings drop twice as much for high-type workers than for low-type workers. This is because, although in the short run low- and high-type workers experience on average a similar loss in days worked, in the long run days worked by high-type workers do not completely recover while those of low-type workers go back to the original level.[11] These results underline that, to fully gauge the distributional consequences of a firm-level shock, it is crucial to look not only at the short-run effects for different types of workers, but also at their differential propensity or ability to find a new job and to accept possible wage cuts in the medium-long term. Otherwise, the conclusions in terms of the distributional consequences of earnings losses could be misleading, due to the large heterogeneity in the future employment prospects of workers of different type.[12]

To uncover the causes of the persistent employment losses among high-type workers, in Figure 6 we run our estimates for high- and low-type workers distinguishing by age (aged 20-40 and 40-50 in 2006). We observe that high-type workers of older age are the ones who bear the toll of the credit shock. First, credit constrained firms, when firing high-type workers, select the older ones. Second, these workers experience persistent losses in days worked when separated from their original firm: while days worked by low-type older workers recover after a few years, days worked by high-type older individuals remain persistenly lower. On the one hand, this may happen because older workers have greater difficulty in finding a new job, consistently with a demand-side story; however, this is hard to reconcile with the evidence that, among low type workers, the elderly manage to recover days worked after some years. On the other hand, high paid older workers may be less willing to accept new jobs at lower wages, in line with a supply-side story. This would be consistent with the existing evidence (Koenig et al., 2016) that reservation wages are rigid, and they do not adjust easily to changes in economic conditions. Indeed, by merging our data to the register of pensioners, we find that the probability of early retirement for older high-type workers increases substantially (by around 5 percent) after the shock, supporting the supply-side explanation.

4.3.2 The role of local labor market conditions

Our setting allows us to test whether the way firms adjust to the credit shock differs depending on the economic conditions of the local labour market where they operate. While this is crucial to get a full understanding of the anatomy of wage and employment losses, the existing evidence is still rather scarce due to the high requirements in terms of identification. Our measure of shock is instead particularly suitable for this test: it is

[11] Note that high-type workers are also high-paid workers who are more costly for firms.

[12] Table 6, panel b confirms the robustness of our estimates by showing that we obtain similar heterogeneity in the responses when, instead of looking at the distinction between high and low type workers, we look at the difference between blue collars and white collars or managers.

at the firm rather than at the local level, and it is spread across geographical areas in a way that is uncorrelated with the local average unemployment rate (Figure 2). It is therefore straightforward to compare workers employed in similarly hit firms, located in local labour markets facing heterogeneous economic conditions.

Table 5, panel b and Figure 7 show that the effects largely depend on the type of local labour market where the firms are located. In particular, we distinguish Italian provinces by whether their average unemployment rate in the period we consider was above or below the median. We find that in low-unemployment provinces the employment losses induced by the credit shock are concentrated entirely among low-type workers. This suggests that credit constrained firms operating in areas with more favorable labor market conditions tend to hoard high-type workers and displace low-type workers, in line with the analysis of Berton et al. (2018) for Veneto, a typical low unemployment region. In high-unemployment provinces instead, employment losses are more persistent and entail both high-type and low-type workers. This is true even though wages do react to the labour market slack. We find that in high-unemployment provinces wage growth decreases among low-type workers, who accept wage cuts to remain in the 2006 firm. Moreover, wage dynamics are subdued also among high-type workers, mostly because they have to accept jobs on a lower wage trajectory once they are fired and need to move to different firms. This may also explain why, in the first place, firms in high unemployment provinces choose to displace also high type workers: these firms are afterwards able to find similar workers at lower wages.

Overall, our results provide new evidence on the sources of cyclicality of wage and employment losses. First, consistently with some macro literature (Mueller, 2017), we show that the type of separated individuals is very different depending on the economic conditions: when facing high unemployment, firms are more likely to displace high-type workers, who would otherwise hoard when facing good economic prospects. Second, this result complements the recent evidence on the cyclicality of earnings losses after displacement (Schmieder et al., 2019), as it uncovers that not only the extent of wage losses but also the type of displaced individuals differs along the cycle, as well as the recently growing literature on rent-sharing (see for instance Friedrich et al., 2019; Kline et al., 2019; Lamadon et al., 2019; Leonardi et al., 2019; Saez et al., 2019), as it shows that the way firms transmit shocks to workers differs widely depending on the cyclical conditions that they face. Finally, our results point out that the existing evidence on the decision of credit constrained firms to lay-off workers that would not optimally have displaced otherwise, i.e. those with steeper productivity profiles or lower firing costs (Caggese et al., 2019), also crucially depends on the conditions of the local labour markets where these firms operate.

4.3.3 Firm-specific human capital, reallocation, and firm type

Firm-specific human capital, whose associated premium cannot be transferred to other employers, is often considered as an explanation for the persistent earnings losses of displaced workers (Lachowska et al., 2018). We now evaluate its role for our findings. In particular, we proxy firm-specific human capital with tenure within the firm. Table 6 and Figure A.2 suggest that, while the effect on days worked is similar across short- and long-tenured workers, wages drop only for long-tenured workers, especially when they move to a new firm. This is consistent with the notion that firm-specific human capital explains part of the observed wage losses among more exposed workers. At the same time, given that long-tenured workers tend to earn higher wages, this finding also supports the idea that credit constrained firms choose to lay-off the most expensive workers.

The literature on mass layoffs has also discussed the role of the quality of the new firm in explaining the size of the earnings losses borne by workers when displaced (e.g. Lachowska et al., 2018, Schmieder et al., 2019 and Gulyas and Pytka, 2019). We therefore assess whether the type of reallocation of separated workers into new firms is different after a credit shock, and whether this explains part of the negative effect on wages we estimate. We first define firms' type, as the firms' average wage paid to their employees (net of firm size, province and sector fixed effects), in the spirit of Haltiwanger et al. (2018). Using a method similar to the one adopted by Utar (2018) and Dauth et al. (2020) to estimate the role of mobility for earnings losses, we decompose the total effect on workers' labour earnings and days worked into three components: i) that occurred among workers who continue to be employed in the original firm, ii) that occurred among displaced workers who find jobs in a new firm of higher type and iii) that occurred among displaced workers who find jobs in a new firm of lower or equal type. As Table 7 shows, displaced workers are reallocating mostly towards firms of higher type, suggesting that wage losses are not generated by a negative change in the composition of firms where displaced workers become employed. Moreover, the table confirms that the main source of earnings losses is the fact that not all workers manage to find a new job, even nine years after the shock. We confirm that firms' quality does not seem to play any significant role by running a further heterogeneity exercise by type of the firm where the worker was employed in 2006. Table 8 shows that there is basically no heterogeneity in the effect depending on whether workers used to work in high- or low- type firms in 2006, as high- and low-type firms tend to adjust similarly to shocks if they face comparable local labour markets conditions.

Overall, our findings suggest that the credit crunch induced by the 2007-2008 financial crisis induced persistent earnings losses on workers. Importantly, these have been highly heterogeneous across the sample of workers we consider, with potential large con-

sequences on inequality and economic efficiency. First, the effects have been stronger on high paid workers. This may have reduced earnings inequality, but through a worsening of the high-paid workers' labour earnings rather than through an improvement of low-paid workers' earnings. In addition, to the extent that high paid workers are more productive, economic efficiency may have been negatively affected. The earnings losses among high type workers are mainly due to a permanent reduction in days worked, especially among the elderly. This seems to be explained by a supply side mechanism, as their probability of early retirement increases. Second, we find that the effects critically depend on the economic conditions of the local labor market where the firms operate. This is true in particular for high-paid workers, who are displaced only under unfavorable local labor market conditions. Third, firm-specific human capital, as measured by workers' tenure, has an impact on wages, but not much on days worked, as long and short tenured workers experience similar employment losses. Finally, persistent earnings losses did not arise as a consequence of a negative reallocation process, that induces displaced workers to systematically find jobs in lower quality firms offering worse wage prospects. We find instead that workers, who find a new job, tend to do it in a better firm; what generates earning losses is that some workers do not manage to find a new employment even nine years after the shock. Taken together, these results suggest that earnings losses of displaced workers do not consist in a fixed penalty attached to lay-offed workers, but largely depend on the propensity and the ability of workers of different types to move to a new job, which is in turn strongly affected by the conditions of the local labour market.

5 Conclusions

A decade after the Global financial crisis it is now possible to analyze whether and to what extent that dramatic event had persistent effects on the labour market. To answer this question we depart from the existing literature. Using a unique matched bank-employer-employee dataset we are able to construct a firm-specific credit shock, which allows us to disentangle the effect of the Global financial crisis on workers' labour earnings and in their probability of employment.

According to our results, the Global financial crisis caused a drop in labour earnings for more exposed workers, compared with those less exposed, i.e. it directly affected the earnings distribution. The effect is sizeable. The earnings of workers employed in firms at the 90th percentile of the distribution of our firm-level measure of exposure to the shock were around 2% lower per year than those of workers employed in firms at the 10th percentile of the shock distribution; the loss amounts to about 20% if we only consider displaced workers. This permanent loss in earnings is mainly caused by

a reduction in days worked by more exposed individuals. Employment losses are more persistent among high-type workers. This is partly explained by a supply-side mechanism, as these workers are more likely to opt for early retirement schemes after displacement. Moreover, consistently with the existing evidence on reference dependence in reservation wages (Koenig et al., 2016), workers are probably unwilling to accept jobs at much lower wages than those earned in their previous positions.

Our results shed light on the mechanisms behind the observed earnings losses. We document that firms operating in areas with structurally favorable labor market conditions hoard high-type workers and displace low-type workers. Under unfavorable labor market conditions instead, firms select to displace high-type (and therefore more expensive) workers, even though wages do react to the labour market slack. The earnings losses do not seem to depend on the type of firms the workers are moving to, since we find evidence of a positive reallocation of workers towards better firms. The potential loss of firm-specific human capital may have played some role, since workers with longer tenure suffer larger wage losses, but does not explain the permanent drop in terms of employment, as the effect on days worked is similar among long- and short-tenured employees.

Our findings lead us to conclude that policy makers should react immediately to any shock that leads to a halt in corporate financing; this would prevent workers from ending up in a path of unemployment and low wage growth, which is difficult for them to abandon even in the long term.

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Tables

Table 1: Summary statistics

	Most treated	Least treated	Most treated	Least treated
	Unweighted		Weighted (PSM)	
	top 33^{th} exp.	others	top 33^{th} exp.	others
	[1]	[2]	[3]	[4]
Firm level variables (characteristics in 2006 of 2006 firms)				
Interbank exp.	17.054	10.980	17.303	11.375
	(5.679)	(2.822)	(5.995)	(2.653)
Size	3427.941	1023.040	1391.272	954.373
	(10909.354)	(3585.282)	(3576.524)	(2624.294)
Av. wage	2205.160	2025.280	2212.513	2143.238
	(826.717)	(730.407)	(808.955)	(776.437)
High type firm	0.653	0.594	0.656	0.637
	(0.476)	(0.491)	(0.475)	(0.481)
Firm age	18.434	20.450	18.711	19.922
	(12.487)	(12.916)	(12.650)	(12.827)
Manufacturing	0.550	0.547	0.579	0.591
	(0.497)	(0.498)	(0.494)	(0.492)
Worker level variables (in 2006)				
Age	41.360	41.214	41.291	41.314
	(7.960)	(8.036)	(7.963)	(7.987)
Female	0.304	0.323	0.298	0.313
	(0.460)	(0.468)	(0.457)	(0.464)
Blue Collar	0.533	0.586	0.545	0.558
	(0.499)	(0.493)	(0.498)	(0.497)
High type worker	0.517	0.474	0.540	0.527
	(0.500)	(0.499)	(0.498)	(0.499)
Tenure <6 years	0.421	0.424	0.437	0.425
	(0.494)	(0.494)	(0.496)	(0.494)
Daily wage	93.801	86.984	94.266	93.879
	(78.271)	(58.546)	(80.696)	(71.879)
Days worked	297.756	296.701	297.839	297.316
	(44.202)	(45.502)	(44.213)	(45.059)
Yearly labour earning	27708.767	25607.227	27835.817	27777.453
	(17899.854)	(14809.806)	(17757.345)	(19936.336)
Observations	1088970	2206800	826575	790170

Note: standard deviation in parenthesis (column 1-4), standard errors (column 5). Treated and control are the treatment and control groups, after having matched the workers working in most exposed firms (top 33th percentile of exposure) with the workers working in least exposed firms (all other firms). High-type workers are workers whose wage in 2006 (net of age, gender, part time contract, tenure, firm size, province and sector fixed effects) was above the median. High-type firms are firms whose average monthly wage per employee in 2006 (net of firm size, province and sector fixed effects) was above the median.

Table 2: Summary statistics by type of worker

	High type workers			Low type workers		
	All	Most treat top 33^{th} exp.	Least treat others	All	Most treat top 33^{th} exp.	Least treat others
	[1]	[2]	[3]	[4]	[5]	[6]
Firm level variables (characteristics in 2006 of 2006 firms)						
Interbank exp.	14.163	17.166	11.573	13.996	17.464	11.154
	(5.153)	(5.796)	(2.451)	(5.632)	(6.217)	(2.846)
Size	1200.138	1509.314	933.682	1101.382	1252.635	977.473
	(2898.191)	(3466.564)	(2264.606)	(3322.156)	(3696.693)	(2974.741)
Av. wage	2448.436	2491.746	2411.110	1862.369	1884.564	1844.186
	(851.695)	(858.696)	(843.835)	(576.825)	(598.215)	(558.035)
High type firm	0.746	0.756	0.738	0.530	0.537	0.524
	(0.435)	(0.429)	(0.440)	(0.499)	(0.499)	(0.499)
Firm age	18.832	18.179	19.395	19.981	19.336	20.510
	(13.003)	(12.857)	(13.101)	(12.449)	(12.373)	(12.487)
Manufacturing	0.572	0.567	0.576	0.601	0.592	0.607
	(0.495)	(0.496)	(0.494)	(0.490)	(0.491)	(0.488)
Worker level variables (in 2006)						
Age	41.328	41.342	41.317	41.274	41.231	41.310
	(7.957)	(7.949)	(7.965)	(7.997)	(7.980)	(8.011)
Female	0.312	0.308	0.315	0.299	0.285	0.311
	(0.463)	(0.462)	(0.465)	(0.458)	(0.452)	(0.463)
Blue Collar	0.365	0.355	0.373	0.766	0.767	0.764
	(0.481)	(0.479)	(0.484)	(0.424)	(0.423)	(0.424)
Tenure <6 years	0.450	0.460	0.442	0.408	0.409	0.407
	(0.498)	(0.498)	(0.497)	(0.491)	(0.492)	(0.491)
Daily wage	119.717	119.144	120.211	64.737	65.048	64.482
	(96.372)	(102.820)	(90.445)	(12.790)	(12.765)	(12.805)
Days worked	298.351	298.915	297.864	296.646	296.575	296.705
	(46.563)	(45.743)	(47.254)	(42.397)	(42.311)	(42.468)
Yearly labour earning	35210	34992	35397	19343	19430	19271
	(23161)	(21260)	(24682)	(4897)	(4902)	(4891)
Observations	846330	442590	403740	770415	383985	386430

Note: standard deviation in parenthesis (column 1-4), standard errors (column 5). Treated and control are the treatment and control groups, after having matched the workers working in most exposed firms (top 33th percentile of exposure) with the workers working in least exposed firms (all other firms). High-type workers are workers whose wage in 2006 (net of age, gender, part time contract, tenure, firm size, province and sector fixed effects) was above the median. High-type firms are firms whose average monthly wage per employee in 2006 (net of firm size, province and sector fixed effects) was above the median.

Table 3: Firm level evidence

Dep var:	Credit growth	1=Exit	N. employees	Av.wage (2006=1)
	[1]	[2]	[3]	[4]
interb.*post2006	-0.300***	0.037***	-10.501***	-0.033***
	(0.025)	(0.008)	(2.661)	(0.013)
N	700848	810246	948583	807087
Firm FE	Yes	Yes	Yes	Yes
t FE	Yes	Yes	Yes	Yes

Note: Firm level analysis. The regressions include firm and time fixed effects. Standard errors clustered at the firm level. The dummy exit is a dummy equal to 0 for all the years a firm operates in the market and 1 for the first year the firm exits the market. Sample column [1]: only years when receiving credit; sample column [2]: only years when the firm operates in the market and the first after the firm exits; sample column [3]: all years between 2002 and 2018 (employees=0 if the firm exits the market); sample column [4]: only years the firm operates in the market. Robust standard errors clustered at the firm level in parenthesis.

Table 4: Worker level evidence

Dep var:	Earnings	N days empl		Daily wage (2006=1)	
		2006 firm (0 if moving)	anyfirm	any firm	if they stay (. if moving)
	[1]	[2]	[3]	[4]	[5]
interb.*post2006	-4790.253***	-59.365***	-20.548***	-0.032**	-0.014
	(1730.178)	(6.625)	(4.882)	(0.015)	(0.012)
N	1616745	1616745	1616745	1471446	1193214
Worker FE	Yes	Yes	Yes	Yes	Yes
pt FE	Yes	Yes	Yes	Yes	Yes

Note: Worker level analysis. The regressions include worker and province (of the firm in 2006) times year fixed effects. Standard errors clustered at the 2006 firm level in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Worker level evidence

Dep var:	Earnings	N days empl		Daily wage (2006=1)	
		2006 firm (0 if moving)	anyfirm	any firm	if they stay (. if moving)
	[1]	[2]	[3]	[4]	[5]
		a. By type of workers			
		Low type workers			
interb.*p2006	-1703.319***	-54.205***	-14.990**	-0.040*	-0.018
	(609.047)	(8.773)	(6.951)	(0.022)	(0.015)
N	770415	770415	770415	689878	564878
		High type workers			
interb.*p2006	-7621.987***	-61.506***	-26.250***	-0.021	-0.005
	(2555.657)	(9.580)	(6.705)	(0.020)	(0.017)
N	846330	846330	846330	781568	628336
		b. By average local unemployment rate			
		All workers			
interb.*post2006	-3175.067	-30.544***	-5.414	-0.013	0.010
	(2764.026)	(8.767)	(6.271)	(0.019)	(0.019)
shock*high unempl prov	-4155.108	-74.141***	-38.930***	-0.049	-0.064**
	(2931.247)	(13.272)	(9.995)	(0.031)	(0.031)
N	1616745	1616745	1616745	1471446	1193214
		Low type workers			
interb.*post2006	-406.985	-46.188***	-0.465	-0.042	0.013
	(827.820)	(11.957)	(9.191)	(0.029)	(0.030)
shock*high unempl prov	-3072.504**	-19.001	-34.426**	0.007	-0.073
	(1217.568)	(17.516)	(14.048)	(0.045)	(0.046)
N	770415	770415	770415	689878	564878
		High type workers			
interb.*post2006	-5992.493	-14.614	-14.637*	0.020	0.017
	(3818.448)	(12.007)	(8.367)	(0.025)	(0.024)
shock*high unempl prov	-4627.703	-133.173***	-32.982**	-0.119***	-0.044
	(4238.503)	(19.658)	(13.990)	(0.042)	(0.042)
N	846330	846330	846330	781568	628336
Worker FE	Yes	Yes	Yes	Yes	Yes
pt FE	Yes	Yes	Yes	Yes	Yes

Note: Worker level analysis. The regressions include worker and province (of the firm in 2006) times year fixed effects. Standard errors clustered at the 2006 firm level in parenthesis. High-type workers are workers whose wage in 2006 (net of age, gender, part time contract, tenure, firm size, province and sector fixed effects) above the median. Average local unemployment rate refers to the average unemployment rate of the province were the worker was employed in 2006, for the period between 2002 and 2016. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Additional heterogeneity tests: worker level heterogeneity

Dep var:	Earnings	N days empl		Daily wage (2006=1)	
		2006 firm (0 if moving)	anyfirm	any firm	if they stay (. if moving)
	[1]	[2]	[3]	[4]	[5]
		a. By occupation			
		Blue collar			
interb.*post2006	-1458.901**	-50.220***	-14.140**	-0.012	-0.002
	(600.107)	(7.944)	(6.173)	(0.017)	(0.013)
N	905490	905490	905490	819696	669386
		White collar and managers			
interb.*post2006	-3246.688**	-62.455***	-27.899***	-0.021	0.010
	(1281.303)	(11.097)	(7.946)	(0.027)	(0.018)
N	668205	668205	668205	613227	493412
		b. By tenure within the firm			
		Short tenure			
interb.*post2006	-2779.071***	-71.175***	-20.834***	-0.017	0.005
	(962.322)	(10.092)	(7.697)	(0.024)	(0.017)
N	601770	601770	601770	536885	399052
		Long tenure			
interb.*post2006	-6360.978*	-39.310***	-13.070*	-0.059**	-0.033*
	(3506.063)	(10.304)	(7.153)	(0.023)	(0.018)
N	726630	726630	726630	678701	577396
Worker FE	Yes	Yes	Yes	Yes	Yes
pt FE	Yes	Yes	Yes	Yes	Yes

Note: Worker level analysis. The regressions include worker and province (of the firm in 2006) times year fixed effects. Long-tenured workers are workers with more than 5 years of experience within the firm in 2006. Standard errors clustered at the 2006 firm level in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Reallocation after the shock, days worked and earnings decomposition

	Overall		Stayers		Movers		
	Any firm		2006 firm		in better firms		in worse (or =) firms
	[1]	=	[2]	+	[3]	+	[4]
			a. Days worked decomposition				
			High type workers:				
interb.*post2006	-26.250***		-61.506***		23.036***		12.270**
	(6.705)		(9.580)		(6.406)		(6.052)
N	846330		846330		846330		846330
			Low type workers:				
interb.*post2006	-14.990**		-54.205***		25.496***		13.739***
	(6.951)		(8.773)		(5.588)		(5.296)
N	770415		770415		770415		770415
			b. Labour earnings decomposition				
			High type workers:				
interb.*post2006	-7621.987***		-9747.487***		1717.647		407.853
	(2555.657)		(3080.219)		(1561.948)		(893.614)
N	846330		846330		846330		846330
			Low type workers:				
interb.*post2006	-1703.319***		-4814.033***		2062.114***		1048.600**
	(609.047)		(715.890)		(459.911)		(428.428)
N	770415		770415		770415		770415
Worker FE	Yes		Yes		Yes		Yes
pt FE	Yes		Yes		Yes		Yes

Note: Worker level analysis. The table displays coefficients of regressions similar to the ones reported in Table 4, where the dependent variable in column 3 is equal to the number of days worked or labour earnings accumulated in better firms for displaced workers (and 0 otherwise); in column 4 the number of days worked or earnings accumulated in worse or equal firms. The definition of better or worse firms depends on firm type, which is defined by the average wage per employee paid by firm j, net of size, sector and province fixed effects. The sum of the coefficients of columns 2, 3 and 4 gives the coefficient displayed in column 1. The regressions include worker and province (of the firm in 2006) times year fixed effects. Standard errors clustered at the 2006 firm level in parenthesis. High-type workers are workers whose wage in 2006 (net of age, gender, part time contract, tenure, firm size, province and sector fixed effects) was above the median. *** p<0.01, ** p<0.05, * p<0.1.

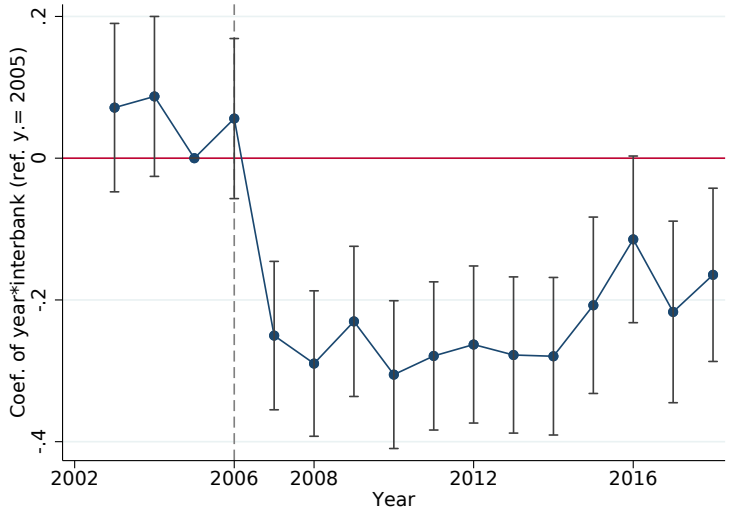
Table 8: Additional heterogeneity tests: type of the 2006 firm

Dep var:	Earnings	N days empl		Daily wage (2006=1)	
		2006 firm (0 if moving)	anyfirm	any firm	if they stay (. if moving)
	[1]	[2]	[3]	[4]	[5]
			High type firms		
interb.*post2007	-3448.792*	-47.639**	-19.896**	-0.002	-0.001
	(1998.070)	(18.914)	(9.393)	(0.024)	(0.023)
N	1034235	1034235	1034235	953373	783217
			Low type firms		
interb.*post2007	-2105.201*	-55.388***	-8.316	-0.062	-0.019
	(1103.817)	(20.345)	(10.222)	(0.047)	(0.051)
N	582510	582510	582510	518073	409993
Worker FE	Yes	Yes	Yes	Yes	Yes
pt FE	Yes	Yes	Yes	Yes	Yes

Note:Worker level analysis. The regressions include worker and province (of the firm in 2006) times year fixed effects. Standard errors clustered at the 2006 firm level in parenthesis. Firms' type is whether the firms' average wage per employee, net of size, sector and province fixed effects, is above or below the median. *** p<0.01, ** p<0.05, * p<0.1.

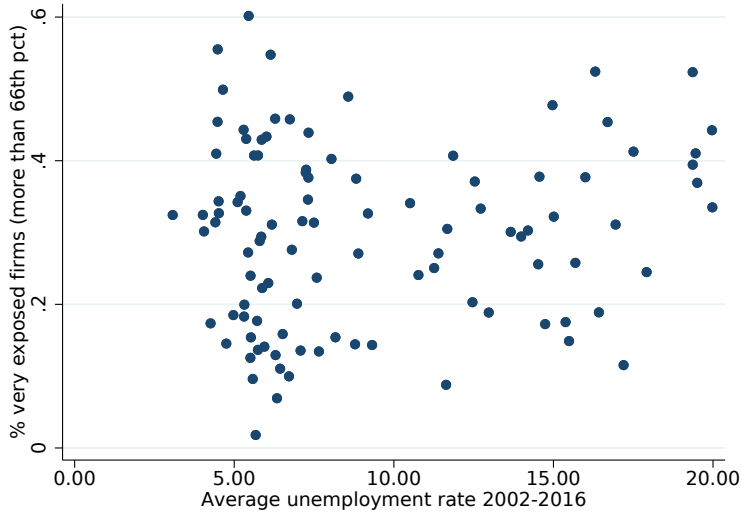
Figures

Figure 1: Effect on delta credit at the firm level



Note: interactions of 2006 exposure to interbank of the firms with year dummies. Vertical bars represent 95% confidence intervals. Standard error clustered at the firm level. Additional controls: firm and year fixed effects.

Figure 2: Province level dispersion of interbank exposure



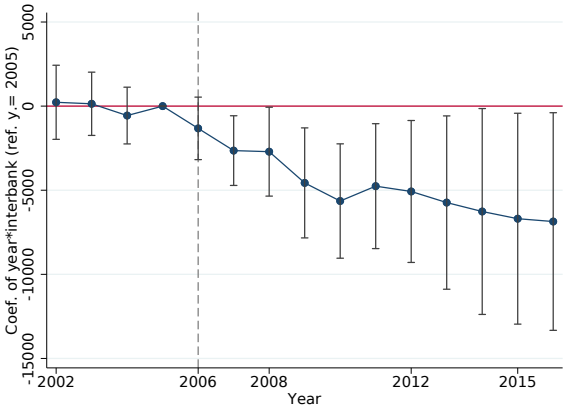
Note: Unemployment rate at the province level (average 2002-2016) and share of firms highly exposed to the interbank market (above the 66^{th} percentile in 2006) in the same province.

Figure 3: Effect on exit probability, firms' size, and average wage per employee

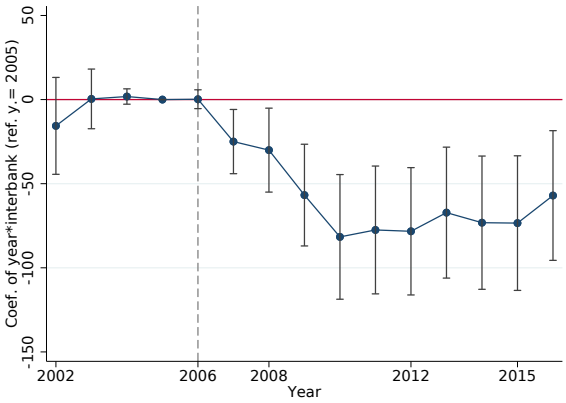
Note: interactions of 2006 exposure to interbank of the firms with year dummies. Vertical bars represent 95% confidence intervals. Standard error clustered at the firm level. Additional controls: firm and year fixed effects.

Figure 4: Effect on workers' earnings, days worked and daily wages

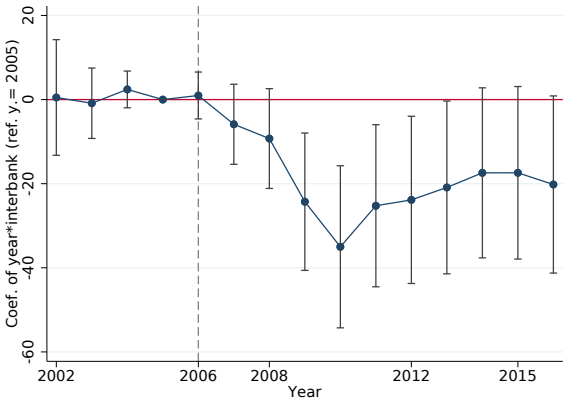
a. Labour Earnings



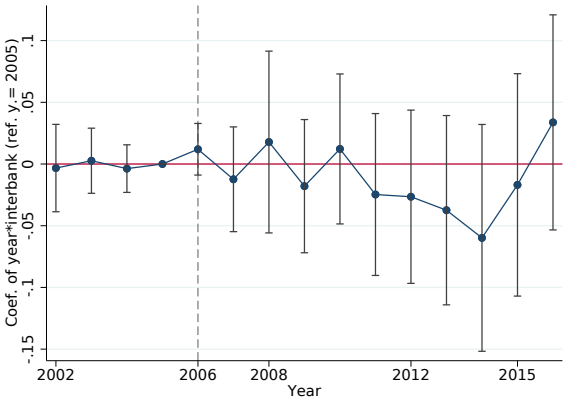
b. Days worked in 2006 firm



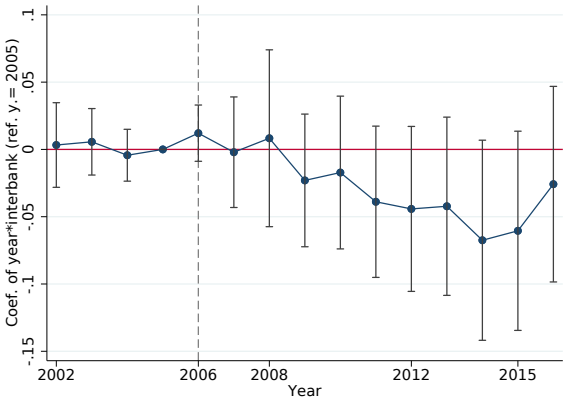
c. Days worked in any firm



d. Daily wages in 2006 firm (2006=1)

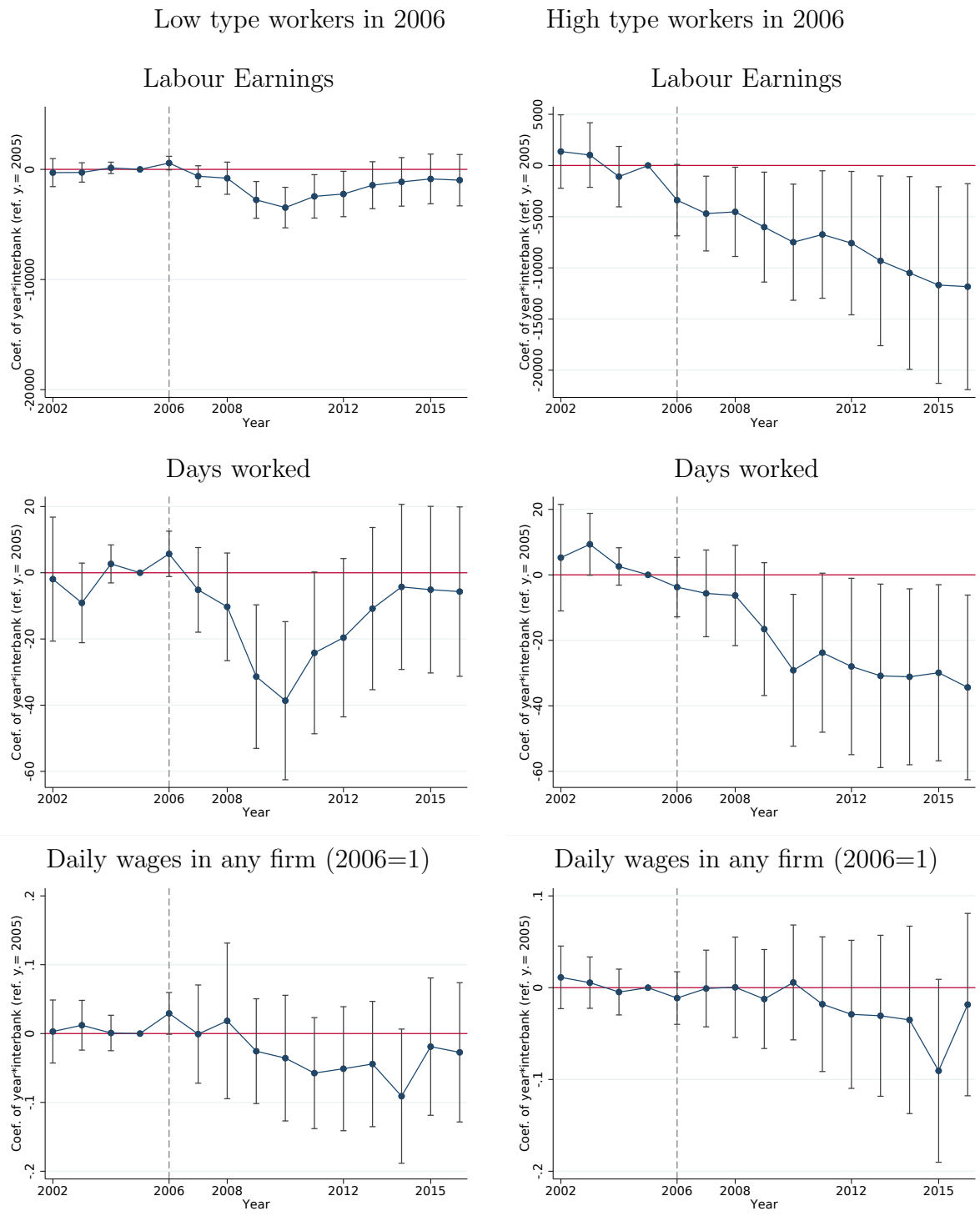


e. Daily wages in any firm (2006=1)



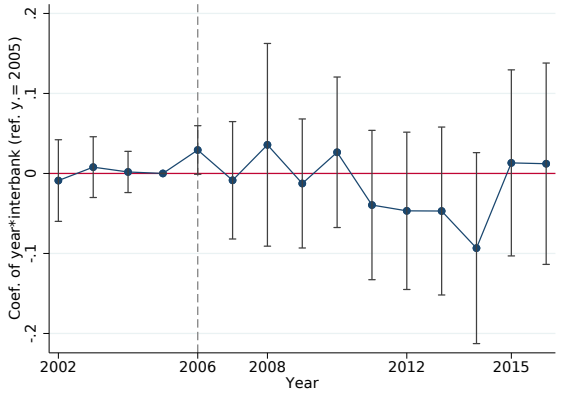
Note: interactions of exposure to interbank of the firms where the worker was employed in 2006 with year dummies. Vertical bars represent 95% confidence intervals. Standard error clustered at the 2006 firm level. Additional controls: worker, year*province fixed effects.

Figure 5: Effect by type of workers



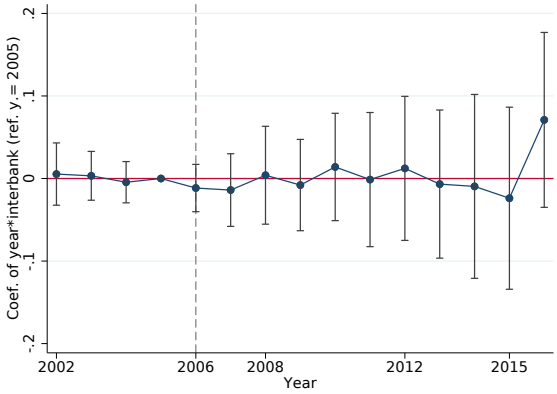
Low type workers in 2006

Daily wages in 2006 firm (2006=1)



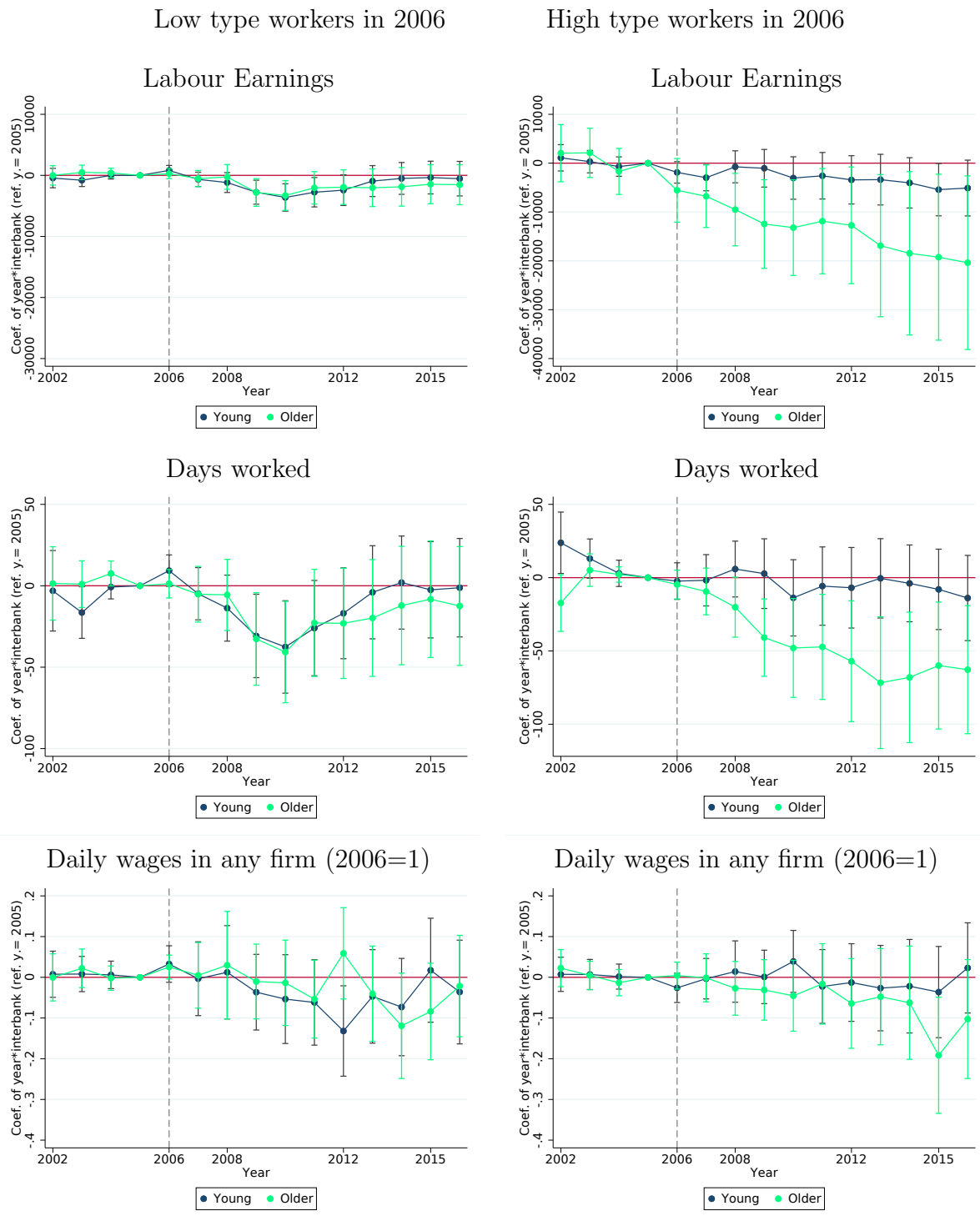
High type workers in 2006

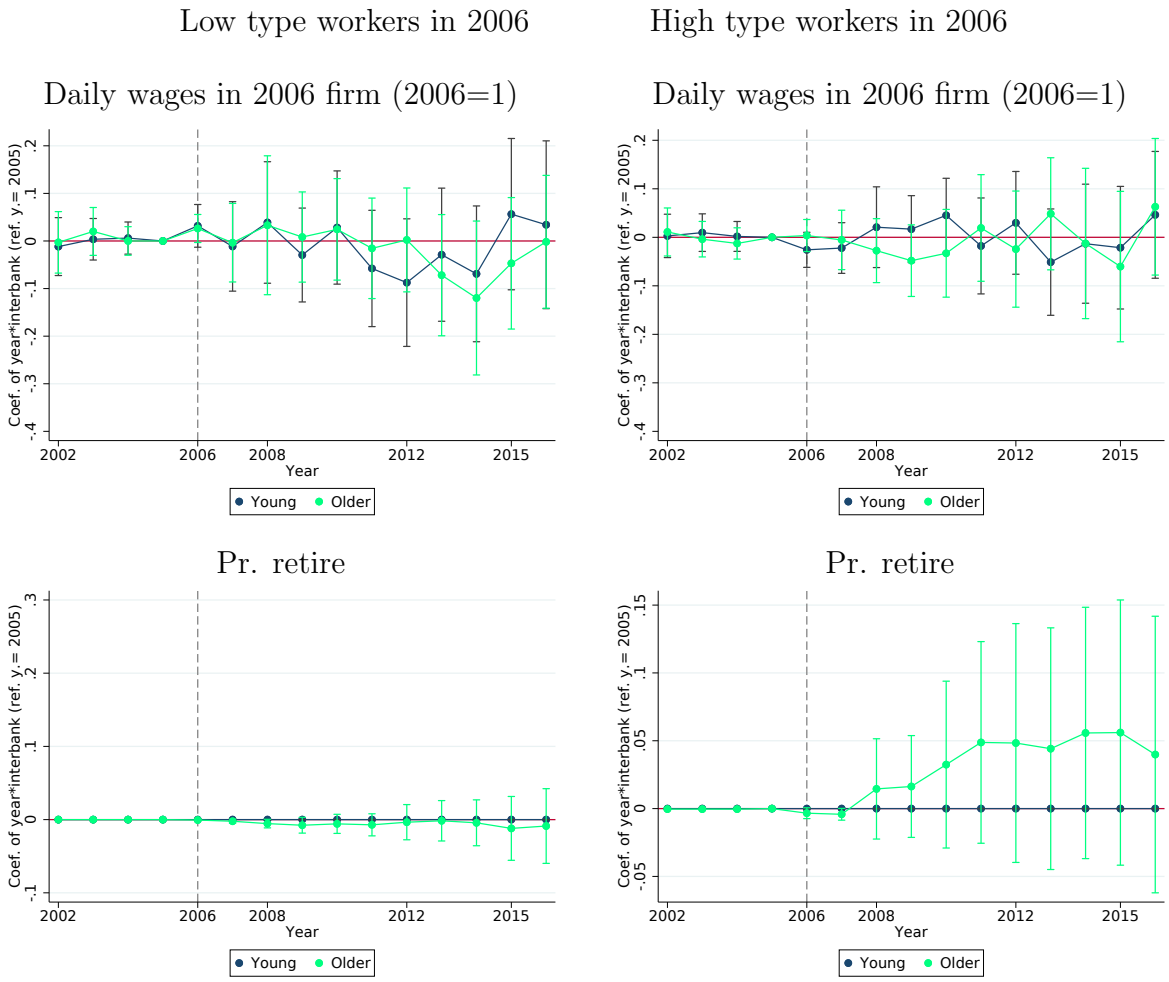
Daily wages in 2006 firm (2006=1)



Note: interactions of exposure to interbank of the firms where the worker was employed in 2006 with year dummies. Vertical bars represent 95% confidence intervals. Standard error clustered at the 2006 firm level. Additional controls: worker, year*province fixed effects. High-type workers are workers whose wage in 2006 (net of age, gender, part time contract, tenure,firm size, province and sector fixed effects) above the median.

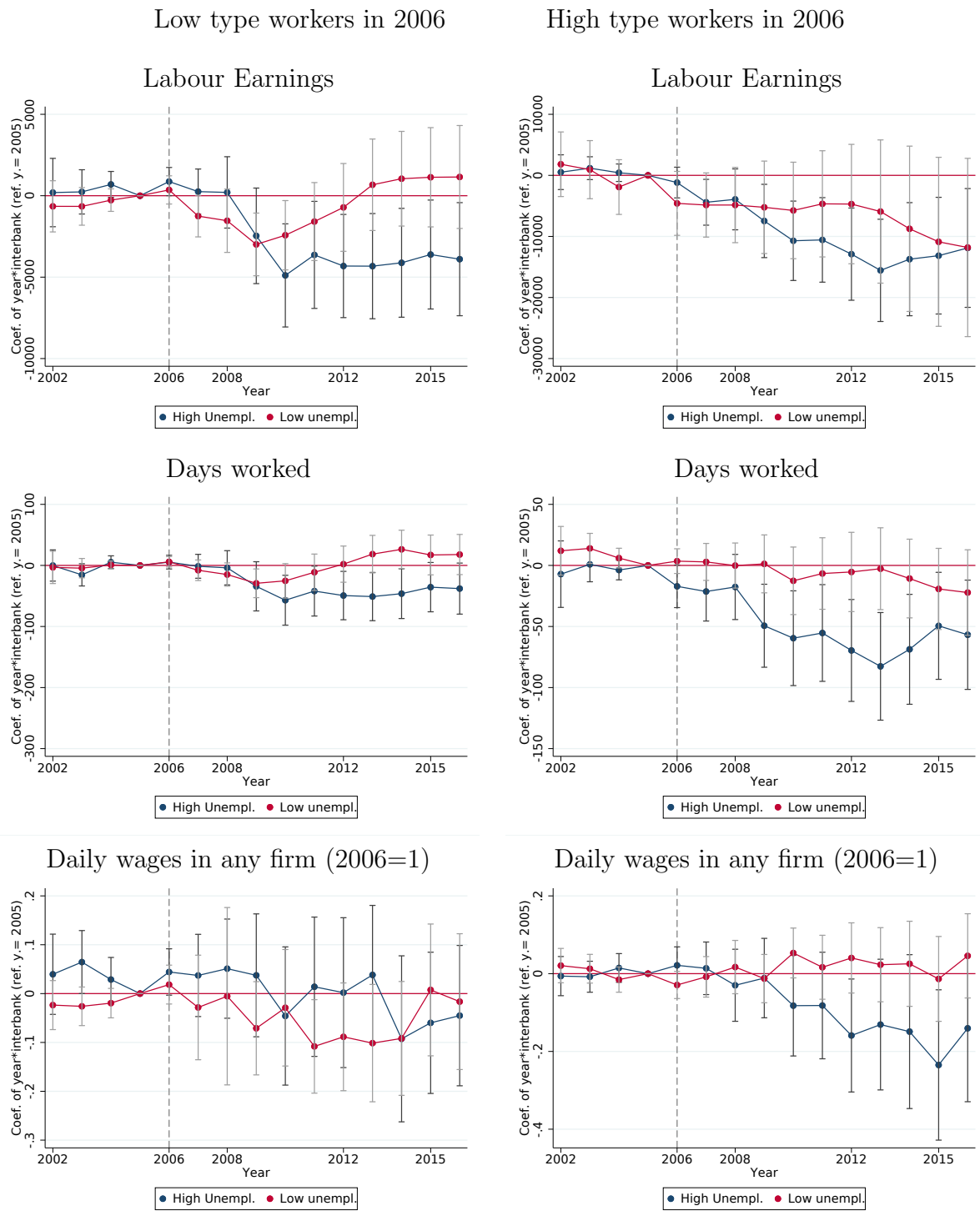
Figure 6: Effect by type of workers - Older and younger individuals





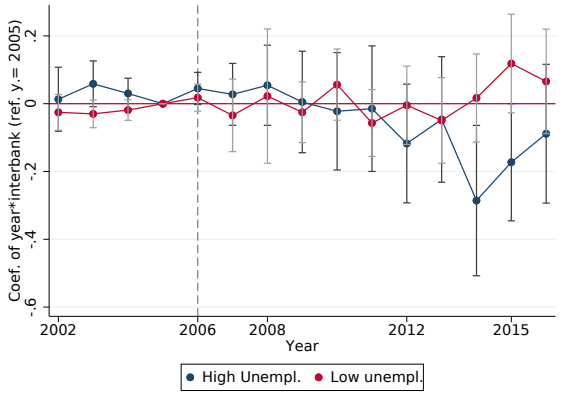
Note: interactions of exposure to interbank of the firms where the worker was employed in 2006 with year dummies. Vertical bars represent 95% confidence intervals. Standard error clustered at the 2006 firm level. Additional controls: worker, year*province fixed effects. High-type workers are workers whose wage in 2006 (net of age, gender, part time contract, tenure, firm size, province and sector fixed effects) above the median. Old workers if aged 40-50 in 2006; young worker if aged 20-40 in 2006.

Figure 7: Effect by type of workers - high and low unemployment provinces



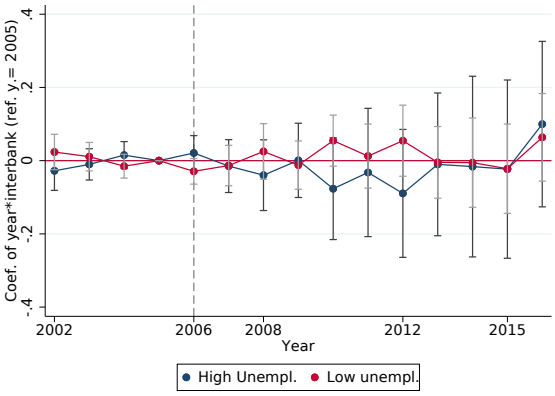
Low type workers in 2006

Daily wages in 2006 firm (2006=1)



High type workers in 2006

Daily wages in 2006 firm (2006=1)



Note: interactions of exposure to interbank of the firms where the worker was employed in 2006 with year dummies. Vertical bars represent 95% confidence intervals. Standard error clustered at the 2006 firm level. Additional controls: worker, year*province fixed effects. High-type workers are workers whose wage in 2006 (net of age, gender, part time contract, tenure, firm size, province and sector fixed effects) above the median. High unemployment provinces if average unemployment level above the media between 2002-2016.

A Appendix A

A.1 Additional Tables

Table A.1: Correlation between interbank funding ratio and bank characteristics

Dep var:	Interbank/Assets	
	(1)	(2)
capital ratio	-0.00337	-0.00972
	(0.0101)	(0.0111)
bank ROA	0.727	0.683
	(0.738)	(0.726)
liquidity ratio	-0.104***	-0.101***
	(0.0333)	(0.0345)
retail deposits/assets	-0.185***	-0.178***
	(0.0433)	(0.0438)
bad loans/assets	0.283	0.290
	(0.209)	(0.219)
log bank assets	0.404	
	(0.396)	
Constant	10.78***	13.23***
	(4.038)	(2.748)
Dummies for deciles of bank size	No	Yes
N	473	473
R^2	0.274	0.290

Note: Regression at the bank level of the interbank funding ratio on bank characteristics. Data are from bank balance sheet data from the Supervisory reports (average 2003-2006). Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table A.2: Share of firm-level credit from banks more exposed to the interbank market in 2006

Dep var:	Delta % credit 2005-2000	
	[1]	[2]
interbank06$_b$	0.001	0.001
	(0.000)	(0.003)
% credit 2000		-1.009***
		(0.006)
N	538169	538169
Firm FE	Yes	Yes

Note: Regression at the bank-firm level, it shows whether the change in the pre-crises share of credit of different banks lending to firm f is correlated to the banks' exposure to interbank markets (average 2003-2006). Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table A.3: Change in firm-level credit to firms more exposed to the interbank market in 2006

	Delta cred 2010-2006 [1]	Delta cred 2015-2011 [2]	Delta cred [3]
interbank06*post 2011			-0.009
			(0.046)
post 2011			-32.0505***
			(0.597)
l.delta cred			0.1150***
			(0.00461)
l.delta cred*post 2011			-1.268***
			(0.0101)
interbank06	-0.285***	-0.0904***	
	(0.0268)	(0.0342)	
l.delta cred	-0.101***	-0.128***	
	(0.00389)	(0.00588)	
N	223263	209206	294690
Firm FE	No	No	Yes

Note: Regression at the firm level, it shows whether the change in credit (2015 and 2010) of different firms is correlated to the banks' exposure to interbank markets (average in 2003-2006, weighted by firm-bank outstanding credit in 2006), after controlling for the drop in credit observed between 2010 and 2006. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table A.4: Correlation between bank characteristics as of 2010-2011 and interbank exposure as of 2006 at the bank level

	[1] capital/assets	[2] tier1/assets	[3] capital/rwa	[4] roa	[5] govt bonds/assets
interbank06$_b$	-0.0221	-0.0116	-0.0217	-0.000242	-0.162***
	(0.0169)	(0.0173)	(0.0388)	(0.00467)	(0.0509)
N	469	469	469	469	469
R^2	0.192	0.252	0.132	0.017	0.151

Note: The table shows correlations between interbank funding to total assets (average in 2003-2006) of each bank and measures of capital, profitability, and exposure to the sovereign debt crisis. These measures are averages between June 2010 and June 2011. All regressions include dummies for deciles of bank assets. Data are from the Supervisory Reports. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

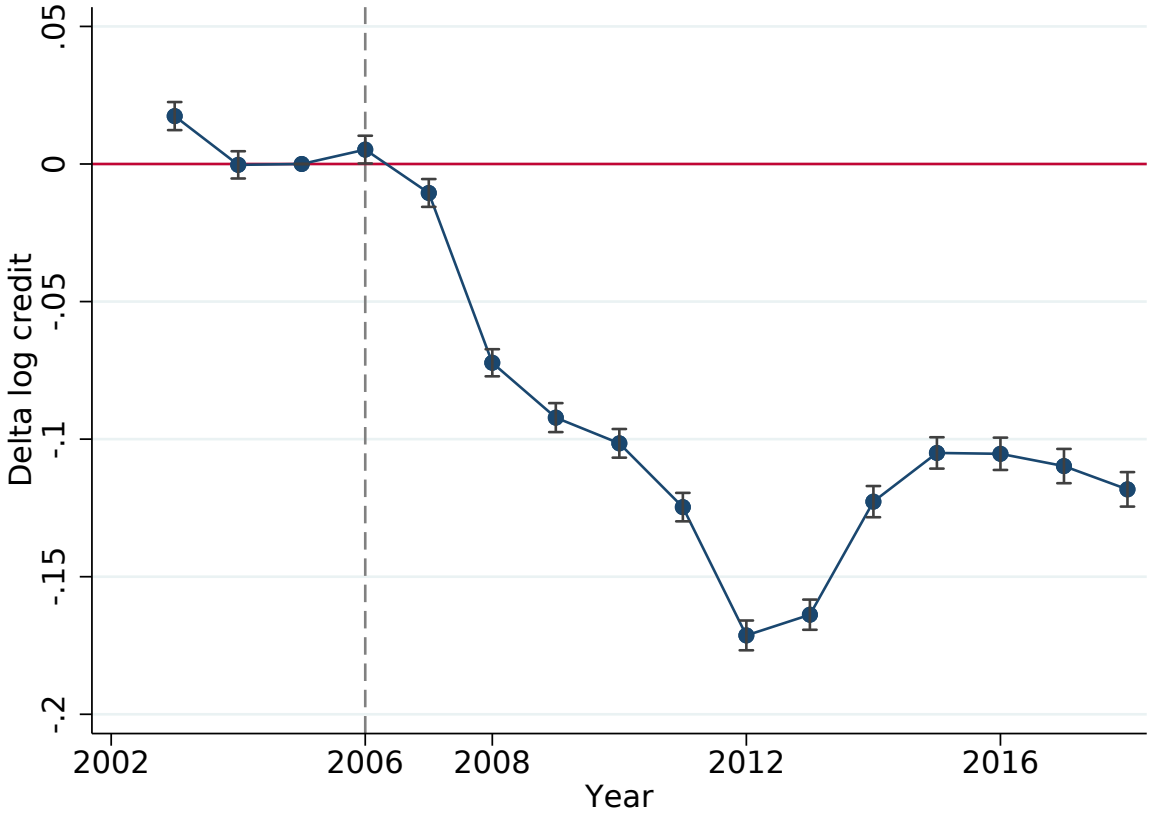
Table A.5: Change in bank's average cost of funding

	(1) 2006	(2) 2010
interbank06$_b$	0.0484**	0.000735
	(0.0238)	(0.0130)
initial cost of funding (level)	-1.285**	-0.388**
	(0.521)	(0.166)
N	448	443
R^2	0.119	0.085

Note: The table shows correlations between interbank funding to total assets (average in 2003-2006) of each bank and the change in the average cost of funding between 2006 and 2010 in column 1 and 2011-2015 in column 2. All regressions include dummies for deciles of bank assets. Data are from the Supervisory Reports. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

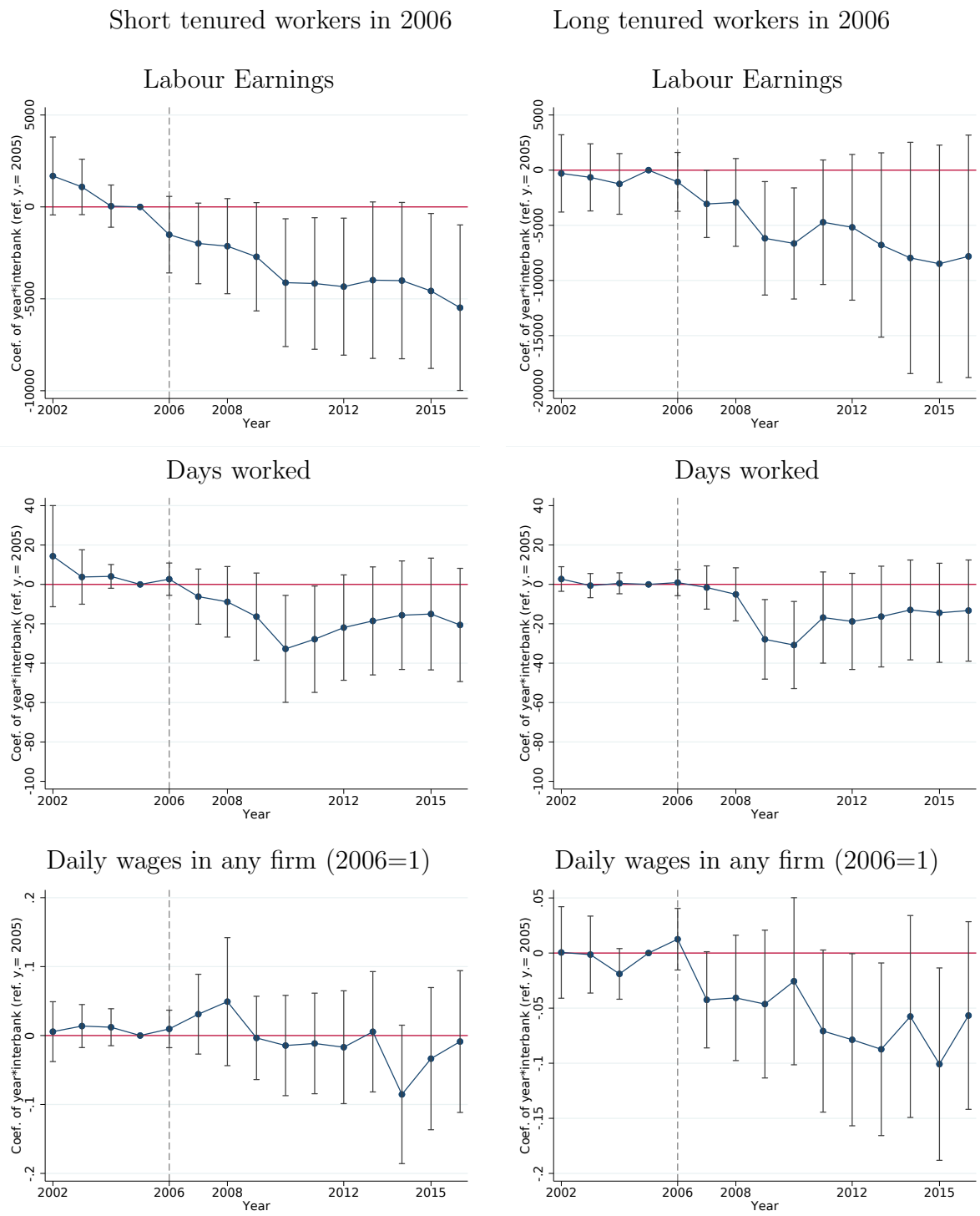
A.2 Additional figures

Figure A.1: Dynamics of aggregate credit growth



Note: This figure plots the level of overall credit growth experienced by all firms in our sample, relative to 2005.

Figure A.2: Effect by worker tenure in 2006 firm



Note: interactions of exposure to interbank of the firms where the worker was employed in 2006 with year dummies. Vertical bars represent 95% confidence intervals. Standard error clustered at the 2006 firm level. Additional controls: worker, year*province fixed effects. Long tenured workers are workers with more than 5 years of experience within the firm in 2006.