

**Do Credit Shocks Affect Labor Demand?
Evidence for Employment *and* Wages during the Financial Crisis**

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Abstract

We study the impact of exogenous funding shocks to German savings banks during the U.S. subprime mortgage crisis on the labor decisions of 30,000+ private and public firms in Germany. We find that firms with credit relationships with affected banks experience a significant decline in labor demand relative to firms with credit relationships with healthy banks, manifested in a simultaneous reduction in firm-level employment and average wages. The employment effect is more pronounced in larger firms, while the wage effect is stronger in smaller firms. Both employment and wages go back to pre-shock levels three years after the shock.

JEL classification: D92, G01, G21, J23, J31.

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The question of how financial stress affects employment ranks at the top of the academic and policy agenda, mainly due to the significant increase in unemployment during the crisis and the “jobless recovery” that followed suit in many countries. In this paper, we study empirically the impact of adverse shocks to firms’ credit access on their demand for labor. We exploit a clean and unique experimental setting, in which we analyse shocks that originate in U.S. mortgage markets, propagate to German public banks, and affect their loan provision to local firms, with a material impact on these firms’ employment decisions.

There are three important aspects of this setting that our empirical design takes advantage of. First, the exogenous variation in financing access for firms is induced by the heterogeneous impact of the U.S. subprime mortgage crisis across banks. The German economy exhibited stable growth and record-low levels of unemployment until 2008, and the German housing market experienced no significant increase and rapid decline in prices as in the United States, Ireland, or Spain. At the same time, some of the German Landesbanken had accumulated large exposures to the U.S. subprime market and were thus substantially affected right at the onset of the crisis. Landesbanken are in turn owned by their network of savings banks, which were required to provide guarantees or equity injections for the affected Landesbanken. As described in Puri, Rocholl, and Steffen (2011), shocks to external financing in this set-up are thus reliably orthogonal to local investment opportunities.

Second, on the bank side we observe actual financial distress in the form of publicly announced support for Landesbanken with large losses on their exposures to the U.S. subprime mortgage market. On the firm side, we have data on both employment and wages and can thus reliably identify changes in firm-specific *labor demand* due to the increase in the cost of external finance that are not contaminated by potential contemporaneous shocks to local *labor*

supply, such as the migration of workers away from localities with deteriorating credit conditions.¹

Third, law mandates savings banks to serve only their respective local customers and thus operate in precisely and narrowly defined geographic regions, following a version of narrow banking. This creates an ideal experimental setting in which we can estimate—in a classic difference-in-differences set-up—the employment and wage adjustment by firms attached to affected banks relative to the employment and wage adjustment by similar firms attached to non-affected banks.

We find that firms with a credit relationship with at least one affected bank experience a significant decline in employment and average labor compensation after the start of the financial crisis. Relative to firms attached to non-affected banks, firms with credit relationships with affected banks reduce employment by up to 1.5% and average wages for the retained employees by up to 1.8%. We find evidence that the employment effect increases and the wage effect decreases with firm size, suggesting that small firms face higher firing costs—potentially because they have stronger relationships with their workers—so that their adjustment in response to impaired external finance is primarily on the labor compensation margin. We also find that both effects are stronger in industries in which firms have high external financing needs for technological reasons such as project scale, gestation period, the ratio of hard vs. soft information, the ratio of tangible vs. intangible assets. Finally, the employment effect of

¹ Migration effects are potentially large; for example, Coen-Pirani (2010) reports a combined outflow and inflow of population of 16 percent for the average U.S. state between 1995 and 2000. Migration flows in Germany are economically large, too, with Wirth (2013) reporting an average annual net migration level of 1.3 percent between German states, and 3.2% between counties within the same state. Dustmann (2003), Boustan, Kahn, and Rhode (2010), and Boustan, Fishback, and Kantor (2010) provide evidence on migration in response to current or anticipated negative income shocks. Brown and Matsa (2015) show that a firm-specific financial shock leads to a decline in job applications at that firm. In an early seminal paper, Katz and Murphy (1992) demonstrate the importance of observing both employment and wage data in order to determine the relative contribution of labor demand and labor supply to changes in the college wage premium.

impaired financing access is not more long-lasting than the wage effect, with both employment and wages back to pre-shock levels on the third year after the shock.

Our empirical design takes advantage of shocks to external financing that are reliably orthogonal to local investment opportunities. Nevertheless, our difference-in-differences strategy might still be subject to a number of concerns. First, our estimates could be driven by shocks to labor demand that are unrelated to the supply of credit, e.g., agency cost problems at firms becoming more severe for firms borrowing from affected banks. To that end, we employ a rich set of time-varying firm-level balance sheet characteristics and firm fixed effects. We show that the effect of impaired access to finance on employment and compensation is also observed for firms with no access to foreign markets, alleviating concerns that the reported empirical patterns are generated by a disproportionate dependence on credit granted by affected banks of firms whose exports were hit by the decline in global demand.

Second, there could be a non-random assignment of firms to banks. It is important to note that the nature of the German banking market makes it highly unlikely that firms that anticipate a future need to reduce employment and wages, choose to be associated with a bank that later becomes affected. Our results survive specifications with *region-specific trends* that allow us to identify the effect off cross-firm within-region variation at a particular point in time. This strategy makes sure that we are not comparing affected firms in one region with unaffected firms in another region, but rather affected and unaffected firms within the same region, alleviating concerns that the effects are driven by unobservable changes in labor regulation that may vary across regions.

The question of how impaired access to capital affects labor demand is highly important. There is evidence for the negative effect of credit constraints on capital (Love, 2003), R&D investments (Brown, Fazzari, and Petersen, 2009), and on-the-job training (Popov, 2014), however, evidence on the effect of access to finance on firms' employment is scarce. Second, unemployment spells can have significant negative effect on workers' employability. Job-specific skills deplete quickly in an environment of continuous adoption of new technologies, and this process can turn cyclical unemployment into permanently high structural one

(Ljungqvist and Sargent, 1998). Third, by potentially inducing higher unemployment, tighter credit can have important negative social consequences, such as an increase in income inequality and crime (Raphael and Winter-Ebmer, 2001; Garmaise and Moskowitz, 2006).

Our paper is closely related to studies on the effect of financial market imperfections on employment. Some studies rely—in the spirit of Gertler and Gilchrist (1994)—on indirect measures of credit constraints such as firm size or debt to identify the effect of monetary policy and the business cycle on employment (e.g., Sharpe, 1994; Nickell and Nicolitsas, 1999). Lichtenberg and Siegel (1990) provide evidence that a leveraged buyout is followed by a reduction in employment and wages. Hanka (1998) shows that highly levered firms reduce employment more often and pay lower wages. Falato and Liang (2015) show that loan covenant violations are followed by simultaneous cuts in employment and wages.

Recent studies have attempted to gauge the effect of shocks to external finance on employment using more direct measures. For example, Benmelech, Bergman, and Seru (2011) find that following the large decline in real estate values in Japan, unemployment increased by about 1% in U.S. metropolitan state areas dominated by Japanese-affiliates banks. Greenstone and Mas (2012) show that the predicted decline in small business lending at the regional US level maps into lower rates of new business formation and higher unemployment. Boeri, Garibaldi, and Moen (2012) shows that more leveraged sectors exhibit higher employment-to-output elasticities during banking crises. Pagano and Pica (2012) show that during banking crises, employment grows less in industries more dependent on external finance. There are several studies that have used micro data to estimate the response of employment to credit constraints. Campello, Graham, and Harvey (2010) show that firms with credit constraints plan to cut investment and employment more than unconstrained firms. Chodorow-Reich (2014) uses syndicated loan data to show that small firms that before the crisis were borrowing from banks that subsequently became impaired, reduced employment more than small firms associated with healthier banks. Acharya, Eisert, Eufinger, and Hirsch (2014) find that large firms with higher exposure to syndicated lending by European periphery banks experienced lower growth of employment, sales, and capital expenditures. Bentolila, Jansen, Jimenez, and

Ruano (2013) show that Spanish firms with credit relationships with weak banks had substantially lower employment levels than firms borrowing from non-affected banks. Duygan-Bump, Levkov, and Montoriol-Garriga (2015) find that during recessions, workers in small firms are more likely to become unemployed in industries with high external financial needs.

Our paper extends upon these studies in a number of important ways. Most importantly, we look at the wage aspect in addition to the employment effect. This is crucial because a reduction in employment can take place even if labor demand does not change, as long as there is an inward shift in labor supply in response to credit shocks. Observing a reduction in employment *and* in wages is therefore necessary to identify an inward shift in labor demand. Moreover, the negative welfare implications of impaired credit access can be considerably larger if wages for retained employees go down, too. Second, we employ matched bank-firm data to study the effect of shocks to bank balance sheets on firms' labor demand. This allows us to control for firm-specific time-varying observable characteristics related to creditworthiness, as well as for time-invariant firm-specific unobservable characteristics (such as project or management quality) in order to identify a supply effect that is not contaminated by a concurrent demand effect. Third, we directly identify affected banks as they are subject to a publicly announced intervention. Furthermore, we have small and medium firms in our sample, rather than large firms only, which enables us to draw aggregate implications from our micro results. Finally, we do not have to worry about selection in bank-firm relationships due to the matching of firms and banks on a strict geographic principle mandated by German law.

1. Institutional setting and the recession in Germany

The German banking system comprises three pillars: private banks, cooperative banks, and public banks. The latter group consists of Landesbanken and savings banks. In 2011, there were 11 Landesbanken, which cover different federal states, and 431 savings bank within the federal states. The owners of the Landesbanken are the federal states in which the Landesbanken are headquartered and the savings banks in these federal states. A strict regional principle applies, i.e. a savings bank can only become the owner of the Landesbank that

operates in its federal state. The matching between Landesbank and savings banks is thus uniquely driven by geography. While savings banks concentrate on providing financial services for customers in their municipality, often with a focus on retail customers, Landesbanken concentrate on serving as commercial banks, being the clearing banks for their local savings banks, and offering wholesale business such as syndicated lending or underwriting, cooperating with their respective savings banks. In case of distress, Landesbanken can rely on the support of their owners, including the savings banks in their region. These support mechanisms are either agreed upon formally, or they arise from the various ways in which savings banks benefit from the well-being of the respective Landesbank in their federal state.

The economic outlook in Germany before the global financial crisis of 2008-09 was positive. The German economy grew robustly and unemployment kept declining, over the period 2005-2008. In addition to sustaining a robust real growth, Germany experienced none of the housing bubbles that emerged in other countries during the early-to-mid 2000s. In fact, according to the OECD (2008), even in nominal terms German house prices did not increase in any single year since 1999. Thus at the onset of the crisis, German banks did not face particular risks related to domestic developments.

At the same time, a number of banks had invested substantially in the US subprime mortgage market before the financial crisis and incurred significant losses. Several Landesbanken were hit in particular: Sachsen LB in the summer of 2007, West LB in November 2007, Bayern LB in February 2008, and finally Landesbank Baden-Württemberg (LBBW) and HSH Nordbank in November 2008. Savings banks in the respective federal states were directly affected and had to provide significant support to their Landesbanken. As described in detail in Puri, Rocholl, and Steffen (2011), the geographical location of the affected Landesbanken is dispersed over Germany, while the affected federal states are highly heterogeneous in their degree of economic development.

Germany's economy experienced a growth of 2.5% in 2007 and it kept expanding at the beginning of 2008. Overall GDP growth for 2008 amounted to 1.3% and became slightly negative only in the second half of 2008, while unemployment reached a 16-year low in

October 2008. In 2009, the German economy shrank by 5.1%, which represented the largest annual economic decline in post-war Germany. However, the economy recovered very quickly and showed an increase of 2.1% in 2010. At the same time, there were large differences across affected and unaffected regions in unemployment dynamics. Germany-wide, unemployment rose by 0.8 percentage points in one year, from a low of 7.2% in 2008 to a high of 8% in 2009. On average, the population-unweighted unemployment rate in the 18 affected German regions increased by 0.7% between 2008 and 2009, while it actually declined by 0.1% on average in the 20 unaffected German regions.

2. Data

2.1. Firm-level data

Our firm-level data come from the Dafne database. Dafne is a commercial database provided by Bureau van Dijk, containing financial information on over 1 million public and private companies in Germany. The notably rich database contains detailed firm-level accounting data on 568 separate balance sheet items, including financial ratios, activities, ownership, sector, etc. Crucially, for the purpose of our paper, Dafne provides information on the number of bank relationships that the firm has, including the names of the banks. The database also reports firm-level information on total employment and on total labor expenses. All variables are reported with annual frequency. Dafne also reports each firm's industrial sector at the 2-digit NACE level of disaggregation, which allows us to control for a number of technological differences across firms. We focus on firms that have at least 1 observation on employment and on labor expenses before the financial crisis, and at least one observation after that. We focus on the period 2005-2012. In the final sample, there are 56,773 firms with sufficient data on employment, and 37,935 firms with sufficient data on wages.

Table 1 shows definitions and summary statistics of the main firm-level variables that we use in the paper. Around 22% of the firms in our final dataset have an association with an "affected" bank. The median firm in the sample has 22 employees, but the firm size distribution is heavily positively skewed, with mean employment of 71. Median average wage (calculated as

total annual labor expenses divided by total employment) is around €37,300, or around €3,000 per month. The median firm also has €3.42 million worth of physical assets and sells goods worth around €41 thousand; its capital (equity-to-assets) ratio is 0.27; its profit-to-assets ratio is 0.03; and its cash flow-to-assets ratio is 0.08. Finally, each firm has on average credit relationships with 2 banks. At the same time, 48% of the firms have a credit relationship with a single bank, and 10% of the firms have credit relationships with 4 banks or more.

2.2. Bank data

We match the 64,745 firms in our final dataset to all banks with which they have a lending relationship. Dafne reports these relationships for a minimum of 1 bank and a maximum of 6 banks. The total number of banks to whom the firms in the dataset are credit-related is 3034. Out of those, 359 are savings banks: 169 are related to one of the five Landesbanken that required financial assistance in late 2007 and throughout 2008, and 190 are related to one of the remaining Landesbanken. The number of Landesbank-savings bank associations ranges from 75 savings banks connected to West LB, and only two savings banks connected to Bremen LB.

We define an “affected” firm to be a firm that has a relationship with at least one savings bank that is a shareholder of an affected Landesbank. These relationships are fixed throughout the sample period. Clearly, because half of the firms have more than one credit relationship, this classification makes it more difficult to find a negative effect of credit distress on labor demand, as firms are capable of substituting credit across their multiple bank relationships. At the same time, the literature has documented that even when such substitutions take place, overall borrowing tends to decline (Jimenez, Ongena, Peydro, and Saurina, 2012).

For main variables definitions and sources, see Table A1.

3. Empirical strategy and identification

We analyze whether impaired access to finance, proxied by a credit relationship with a bank affected by the US subprime mortgage crisis, has a negative impact on the firm’s labor decisions. In particular, we employ a difference-in-differences approach to analyze two

questions. First, do firms associated with affected banks adjust their labor demand on the *employment* margin, relative to firms associated with unaffected banks? Second, do firms associated with affected banks adjust their labor demand on the *compensation* margin, relative to firms associated with unaffected banks? We address these two questions by exploiting the specific setting in Germany, where savings banks represent a homogenous group of banks that operate according to a model of narrow banking throughout the country and are the owners of their respective regional Landesbanken. The identification strategy is based on the fact that some but not all of the Landesbanken and thus some but not all of the savings banks were affected by the financial crisis, resulting in different shocks to access to finance for firms associated with different banks.

The Landesbanken in Saxony, North Rhine-Westphalia, Baden-Wuerttemberg, Bavaria, and the states of Hamburg and Schleswig-Holstein are the only Landesbanken that publicly announced losses from the US subprime crisis in late 2007 and throughout 2008. The savings banks in these regions are thus affected as well due to their respective ownership. We define the exact crisis event date for these savings banks based on the first public announcement of losses by their respective Landesbanken, which is the third quarter of 2007 for Sachsen LB, the fourth quarter of 2007 for West LB, the first quarter of 2008 for Bayern LB, and November 2008 for LBBW and HSH Nordbank. Because firms need a reasonable adjustment period to adjust their production inputs in response to a change in credit conditions, we treat 2009 as the first post-crisis year. Puri, Rocholl and Steffen (2011) show that the recognition of losses by Landesbanken and their respective savings banks is followed by a substantial reduction in credit to private borrowers. Hence, in this paper we take the reduction of credit by affected banks to their corporate clients as given, and evaluate its real consequences in terms of firm-level employment and compensation. The remaining Landesbanken do not show losses from the US subprime crisis during the sample period. Therefore, the savings banks in these regions are thus classified as non-affected banks, and all firms associated with these banks are treated as non-affected in the empirical tests.

We use two sources of identifying variation: the time before and after the financial crisis as well as the cross section of firms affected and not affected by the crisis because of the negative balance sheet shocks experienced by their bank(s). We estimate the following two regressions:

$$\text{Log}(\text{Employment})_{it} = \beta_1 \text{Post}_t \times \text{Affected}_i + \beta_2 X_{it} + \beta_3 \varphi_i + \beta_4 \eta_t + \varepsilon_{it} \quad (1)$$

$$\text{Log}(\text{Average wage})_{it} = \beta_1 \text{Post}_t \times \text{Affected}_i + \beta_2 X_{it} + \beta_3 \varphi_i + \beta_4 \eta_t + \varepsilon_{it} \quad (2)$$

Here Affected_i is a dummy variable equal to 1 if firm i has a credit relationship with at least one savings bank that became affected in 2007 or 2008, and to 0 otherwise; Post_t is a dummy variable equal to 1 in and after 2009, and to 0 otherwise; X_{it} is a vector of time-varying firm-level control variables; φ_i is a firm fixed effect; η_t is a year fixed effect; and ε_{ijt} is an i.i.d. error term. Affected_i and Post_t are not included in the specification on their own because the effect of the former is subsumed in the firm fixed effects, and the effect of the latter is subsumed in the year fixed effects. We estimate the two equations separately using OLS, and we cluster the standard errors at the firm level to account for the fact that the unobservable component of firms' investment decisions can be correlated over time. The main results of the paper are robust to clustering at the regional level, to account for cross-sectional dependence. The sample period is 2005–2012, with four pre-crisis and four post-crisis years.

The vector of firm-level controls X_{it} allows us to capture the independent impact of various firm-specific developments, such as shocks to overall debt, profits, or cash flow, as well as losses on assets. We also include firm fixed effects and year fixed effects. By including firm fixed effects, we address the possibility that changes in labor demand are driven by a time-invariant firm-specific unobservable factor, such as managerial risk appetite or growth opportunities. Firm fixed effects also subsume other types of time-invariant variation across firms, such as sectoral and regional variation. By including year fixed effects we aim to alleviate concerns that our results might be driven by product market demand shocks that vary heterogeneously over the business cycle.

The coefficient of interest is β_1 . In a classical difference-in-differences sense, it captures the change in the variable of interest (employment or average labor compensation), from the pre-treatment to the post-treatment period, for the treatment group (firms associated with affected banks) *relative to* the control group (firms associated with non-affected banks). A negative coefficient β_1 would imply that all else equal, employment or average labor compensation increased less (decreased more) for the group of affected banks. The numerical estimate of β_1 captures the difference in the change in the variable of interest between the pre- and the post- period induced by switching from the control group to the treatment group.

Figures 1 and 2 represent the employment and wage dynamics captured by Models (1) and (2), where we interact the variable $Affected_i$ with year dummies that span the sample period 2005–2012. We do not include an indicator for 2008, so that the regression coefficients measure the year-by-year dynamics of firm-level employment relative to the year before the shock. The solid lines represent the regression coefficients on the indicator variables, and the dashed lines plot 95% confidence intervals for the point estimates. The patterns clearly show that both employment and wages at the firm level declined sharply immediately after the shock, for firms associated with affected banks relative to firms associated with non-affected banks. The difference between affected and non-affected firms for both employment and wages becomes once again insignificant on the third year after the shock.

The combination of time-varying firm-level characteristics and firm fixed effects addresses the concern that our estimates can be contaminated by shocks to labor demand unrelated to the supply of credit. While agency cost problems may have become more severe and/or growth opportunities may have deteriorated more for firms borrowing from affected banks, this will be picked by the rich set of balance sheet information, as well as by the firm fixed effects.

Nevertheless, we need to address a number of remaining issues with our empirical strategy. The first one is related to the assumption that the “treatment” (association with a bank affected by the crisis) is random. The nature of the German banking market, i.e., the regulatory dependence of firms on their local banks and in turn on their Landesbank, makes it unlikely that firms which anticipated a future need to reduce employment and wages, have

chosen a credit association with a bank that later became affected. However, there are other ways in which this assumption can be violated. For example, affected banks may be operating in regions where the local economy is skewed towards sectors with naturally high elasticity of labor adjustment to shocks to external financing. Table 2 illustrates the difference between firms associated with affected banks and firms associated with non-affected banks, with respect to a number of variables (all in terms of average pre-financial crisis values). Before the crisis, firms associated with banks that subsequently became affected were on average larger (in terms of employment, but not in terms of assets) and had slightly lower capital ratios. They also had, on average, relationships with a larger number of banks (2.2 vs. 1.7). To the degree that observable differences across the two types of firms may result in distributions of balance sheet characteristics that exhibit insufficient overlap across the two groups of firms, simply controlling for these differences in an OLS setting may not be enough to address such potential selection bias. To that end, we control in some specifications for the number of credit relationships. We employ a propensity matching procedure whereby we choose a control group of firms that is observationally identical to the treatment group of firms, based on pre-crisis characteristics.

Another related concern is that if there were different trends between the two types of firms prior to the crisis (for example, because of systematic differences in risk taking between the two groups of firms) we might incorrectly interpret our results as being driven by exposure to affected banks. To test for different trends between the two types of banks, we perform a placebo test on an earlier period.

4. Empirical results

4.1. Main result

We analyse the effect of credit access, proxied by association with at least one affected bank, on firms' labor demand in a difference-in-differences framework aimed at identifying a differential effect on affected versus nonaffected firms. The key identifying assumption is that trends related to labor demand are the same among affected and nonaffected firms in the

absence of changes in access to finance that are induced by the financial crisis. We evaluate this assumption explicitly later.

The main results of the paper are reported in Table 3. In it, we estimate two different versions of Models (1) and (2). The effect of credit access on labor demand in the dimension of employment is estimated in the first two columns. In column (1), we include $Affected_i$ and $Post_t$ on the right-hand side, but do not include firm and year fixed effects. We also control for a number of time-varying firm-specific characteristics: the firm's size (proxied by the natural logarithm of firm assets), sales, capital-to-assets ratio, profit-to-assets ratio, and cash flow-to-assets ratio. We also control for the number of credit relationships, which is time-invariant. We find that larger firms—in terms of assets and sales—have more employees, and that firms with a higher average cash flow also employ more workers. As labor and capital are complements in production, our evidence is consistent with the evidence in Blanchard, Lopez-de-Silanes, and Shleifer (1994), Lamont (1997), and Rauh (2006), among others, who establish a robust link between cash flow and investment. Firms that use more equity also tend to have a larger employment base, and so do firms with credit relationships with more banks. Controlling for those, profit is negatively correlated with the size of the firm's labor force.

Turning to the main variable of interest, the estimate of the regression implies that credit association with an affected bank had a negative effect on the firm's employment after the occurrence of the credit supply shock. The interpretation of the coefficient is in a difference-in-differences sense: relative to a firm whose credit relations are to non-affected banks only, a firm with a credit relation with at least one affected bank reduced employment by 1.5% on average, between the 2005-2008 and the 2009-2012 period. In addition to that, the estimates on the individual components of $Affected_i \times Post_t$ imply that affected firms employ more workers, and that firm-level employment was on average higher after 2008, suggesting that the shocks to access to external finance arrested this secular decline in unemployment by affecting negatively a sub-sample of the population of firms.

In column (2), we drop $Affected_i$ and include firm fixed effects instead, to account for the fact that changes in labor demand may be correlated with a time-invariant component of the firm's production function that is unobservable to the econometrician. We also exclude $Post_t$, but add year fixed effects, to account for shocks to the business cycle that are common to all firms in the sample. In this way, we aim to identify the firm-specific time-varying component of labor demand.

Importantly, the effect of an association with an affected bank on employment is still significant at the 1% level. The magnitude of the coefficient is even higher than in column (1), implying that between the 2005-2008 and the 2009-2012 period, and relative to a firm whose credit relations are to non-affected banks only, a firm with a credit relation with at least one affected bank reduced employment by 1.5% on average (or by 1 employees, given a sample mean of 71).²

In columns (3) and (4), we repeat the same set of exercises, but this time the outcome variable is the natural logarithm of average labor compensation. The estimates imply that firms respond to shocks to external financing by adjusting the labor compensation margin as well as the employment margin. In both specifications, the effect is significant at the 1% statistical level. The estimates from the model that accounts for time-varying firm-level balance sheet characteristics reported in column (4) imply that larger and better capitalised firms tend to pay higher compensation to their employees. The coefficient on the main variable of interest implies that between the 2005-2008 and the 2009-2012 period, and relative to a firm whose credit relations are with non-affected banks only, a firm with a credit relation with at least one affected bank reduced average labor compensation by about €435 per year.

² In unreported regressions, we find that after the shock, firms attached to affected banks reduce their asset base and post a decline in sales and in profits, which is consistent with a mechanism whereby companies experience a reduction in lending by their creditors, therefore investing less and, as a consequence, employing fewer workers than before (results available upon request).

The reported number may overstate or understate the “true” effect because it is based on the assumption that all workers receive the average wage, both before and after the reduction in firm-level employment. However, in reality there is probably large within-firm across-worker heterogeneity in wages. If only the highest-paid worker is laid off, average wages for the remaining workers will mechanically go down, in which case our estimates will overestimate the “true” level effect. As the structure of the Dafne dataset does not allow us to observe the wage structure of the firms in our sample, or who gets laid off following a credit shock, we cannot calculate this “true” effect. However, Hochfellner, Montes, Schmalz, and Sosyura (2016) document that it is the *lowest-paid* workers that are more likely to leave the firm during a recession, and so the decline in average wages that we document is incompatible with a labor-supply-driven employment composition effect.

The totality of our results implies that shocks to lenders have a significant negative effect on the labor demand of firms attached to affected banks. At the same time, the small economic effect in the employment dimension underscores the importance of observing wage data to identify changes in labor demand: with an average annual net migration level of 3.2 percent between counties within the same German state (see Wirth, 2013), a 1.5 percent decline in employment at firms banking with affected lenders could simply be an equilibrium adjustment to the migration of workers away from counties where credit conditions are deteriorating. However, the simultaneous reduction in average firm-level wages rejects this hypothesis. The small firm-specific effect on employment also assuages concerns that our results may be driven by a composition effect. For example, Brown and Matsa (2015) show that firms hit by a financial shocks experience an outflow of skilled workers, resulting in a labor-supply-driven decline in wages. However, with a loss of 1 worker for the average affected firm, it is highly unlikely that the decline in wages is driven by shocks to labor supply.

Our empirical strategy allows us to come up with a rough partial equilibrium estimate of the aggregate labor consequences of the credit shock stemming from exposure of a number of German banks to the US mortgage market. In 2008, the German firms employed 35.8 million persons. Our data implies that 22 percent of the firms are associated with affected banks (Table

1), and that before the crisis, the average affected firm had 71 employees relative to 59 for a non-affected firm (Table 2). Consequently, around 26.5 percent of German workers were employed at affected firms, or around 9.5 million persons. Our estimates imply that all else equal, overall employment declined by around 142,500 persons, and aggregate compensation declined by €4.05 billion, due to the credit shock.

4.2. Accounting for alternative explanations

While the negative effect of impaired financing access on labor demand and compensation detected so far appears robust, a number of alternative mechanisms could be at play, diluting the causal interpretation of this result. In particular, firms associated with affected banks could be systematically different from firms associated with non-affected banks, in ways that matter for their labor demand. For example, they may be systematically different in terms of size and net worth; they may be predominantly exporting firms that are facing the dual shock of collapsing global demand and tightening access to credit; or they may have been subject to different trends already before the crisis started. In what follows, we review these potential problems and discuss the strategies we have employed to deal with them.

4.2.1. Systematic balance sheet differences between treatment and control firms

One concern related to systematic differences across firms stems from different industrial dynamics across German regions. The divergence in unemployment trends across affected and non-affected regions could in theory be due to the activation of mechanisms that affect firms' labor demand, other than shocks to credit supply. One such mechanism is related to shocks to investment opportunities: because the state participated as well in the recapitalization of troubled Landesbanken, state budgets were adversely affected. An alternative mechanism is related to labor regulation: if non-affected regions have local regulation that makes it more difficult to fire workers during a recession, this could explain a more gradual decline in unemployment for the firms in the control group. Finally, there could be a feedback effect from retail lending which declined in the wake of the crisis (Puri, Rocholl, and Stefen, 2011).

We thus modify our main regression in two different ways. First, we download regional information for all firms in the dataset for which such information is available, and add Region×Year fixed effects to the regressions. The inclusion of this interaction term should net out all trends that are region-specific but common to all firms within a region. We can thus identify the effect of tightening credit constraints off cross-firm variation within the same region at the same point in time. Columns (1) and (2) of Table 4 report that our main results are immune to this alternative procedure. However, the magnitude of the effect goes down and its statistical significance declines, suggesting that unobservable time-varying regional heterogeneity plays an important role in explaining changes in firms' labor demand. Next, we only compare affected and non-affected firms within affected regions (column (3) and (4)), eliminating the effect of cross-regional differences. Once again, our main results survive.³

4.2.2. Exporters vs. non-exporters

Another observable dimension across which affected and non-affected firms might differ is the export dimension. The German economy is heavily dependent on foreign markets, with exports accounting for around 48% of GDP in 2008 (compared with 13% for the US). Global trade fell by around 30% between late 2008 and early-to-mid 2009, driven to a large degree by a decline in the global demand for the products that the Germany economy specializes in, such as manufacturing goods, and in particular durables (Eaton, Kortum, Neimar, and Romalis, 2011). German exports declined by about 17% over the course of 2009 relative to 2008. Some of the German states whose Landesbanken were affected by the crisis, are also home to a large manufacturing base: for example, the headquarters of three of the largest German automobile producers are in states whose Landesbanken became impaired in late 2007 or in 2008.

³ Note that in the absence of wage data, an empirical specification with Region×Year fixed effects would not allow us to separate the labor demand and the labor supply effect on firm-level changes in employment, as long as bank health and unobserved changes in labor supply are not orthogonal at a level of observation finer than the Region – for example, in the presence of intra-regional migration of workers away from towns where credit conditions are deteriorating.

While the latter concern is addressed in the previous table where we account for region-specific trends over time, it is still possible that within the same region, exporting firms are systematically more likely to be attached to affected banks. If so, the estimates reported in Table 3 could be biased by the unobservable decline in global demand which is firm-specific and therefore captured neither by the firm fixed effects nor by the year fixed effects. To address this concern, we split the sample in exporting and non-exporting firms. We classify non-exporting firms as firms whose production is fully sold in the domestic market. Conversely, exporting firms are ones for whom at least 1% of total output is sold in foreign markets. We then run our saturated Models (1) and (2) on the two subsets of firms.

The estimates from this test are reported in Table 5. They imply that between the 2005-2008 and the 2009-2012 period, and relative to firms associated with non-affected banks, firms associated with affected banks reduced employment and wages significantly regardless of whether they were exporters or not. However, we also record important differences in the behaviour of exporters vs. non-exporters. In particular, affected exporters were somewhat more likely to reduce employment than affected non-exporters (columns (1) and (2)). At the same time, affected exporters were almost twice as likely to reduce wages as were affected non-exporters (columns (3) and (4)). One potential interpretation of these differences is related to the fact that exporting firms are usually the most productive firms in the market (Helpman, Melitz, and Yeaple, 2004) and so their workforce may to a larger degree be endowed with valuable industry-specific skills. The termination of labor relationships would then be more costly to exporters than to nonexporters, and so they would prefer to adjust to shocks to external financing by renegotiating wages rather than by reducing employment.⁴

⁴ Another concern relates to the possibility that adverse economic conditions are increasing bank agency costs and firm agency costs at the same time—for example, because low-net worth firms may be more likely to be borrowing from low-net worth banks (Gertler and Gilchrist, 1994). While the structure of the German banking market discussed in the previous section goes some way towards alleviating this concern. Table 2 does suggest that affected and non-affected firms differed in their pre-crisis characteristics. We address this problem by applying a propensity score matching procedure whereby we choose a sub-sample of control (non-affected) firms that are as close as possible a match for the sample of affected firms, based on pre-crisis size, capital, profitability, cash flow, geographic region, and number of banking relationships. We continue to record a strong negative effect of

4.2.3. Parallel trends and duration of effect

Another possible concern is related to the timing of the effect. For one, the different trends in labor demand we observe during the crises may have already been in place before the shock to credit supply. While in the previous two sub-sections we conditioned on observables, there could still be pre-existing trends related to unobservable factors, such as growth opportunities. If this were to be the case, we might incorrectly interpret pre-determined trends as evidence of the negative effect of impaired financing access (see Roberts and Whited, 2011). Second, are the effects we estimate short-lived or permanent? Bentolila, Jansen, Jimenez, and Ruano (2013) show that employment in 2010 was lower than in 2006 at Spanish firms borrowing from affected banks. As we have four post-crisis years, we can differentiate the immediate response from the long-term one.

We interact $Post_t \times Affected_i$ with a dummy equal to 1 for each year during the 2005–2012 period, taking the years 2007 and 2008 as a basis, and merging 2005 and 2006 in one category. We report these results in Table 6. If treatment and control observations were already facing different prospects before the crisis, one should observe statistically significant differences between the same firms observed during 2005–2006 and during 2007–2008. However, the estimates reported in Table 6 imply that this is not the case. The employment coefficient is positive, suggesting that employment increased less for affected firms between 2005–2006 and 2007–2008, it is nowhere near any meaningful level of statistical significance. The same applies to the evolution of average wages. The evidence thus suggests that the effect is indeed due to changes in firms' behavior specific to the period after the shocks to the asset side of a subset of banks.

Regarding the duration of the effect, the data suggest a very strong short-run effect on both employment (column (1)) and on wages (column (2)) in that the decline in employment

impaired financing access on labor demand, both in the employment and in the compensation dimension, and the results continue to be statistically significant at the 1 percent statistical level (results available upon request).

and in labor compensation is very pronounced in 2009 and in 2010, and then it dissipates over time. While the wage effect is most pronounced immediately after the shock (in 2009), the employment effect takes more time to build, with the biggest decline in firm-level employment taking place two years after the shock, in 2010. Our results thus suggest that negative shocks can have a relatively long-lasting negative effect on aggregate employment levels, taking three years to return to pre-crisis levels.

4.3. Heterogeneous impact of financing access on firms' labor demand

We next study which firms are most sensitive to the transmission of bank balance sheet conditions. We expect the firms' risk and the tangibility of their assets to play an important role in explaining differences in credit availability across firms. High-risk firms tend to be most affected by changes in credit conditions, and these tend to be small and young (Berger, Klapper, and Udell, 2001). Regarding asset tangibility, Berger, Ofek, and Swary (1996) show that firms with fewer tangible assets are more likely to lose access to credit when banks reprice risk. In Table 7, we evaluate the effect of financing access on employment after differentiating across firm-specific characteristics that proxy for the above considerations. The regression estimates imply that in response to a negative shock to access to finance, firms with less than 20 employees are *less* likely to reduce employment (column (1)) while firms with less than 20 employees are *more* likely to reduce wages (column (4)). There are a number of potential explanations for this effect. For instance, smaller firms may be facing lower flexibility in the employment dimension because labor regulation preventing firms from firing employees is tighter for smaller firms. Alternatively, smaller firms may be benefiting more from state assistance which compensates businesses for retaining workers during a recession. Finally, solidarity between employers and workers may be stronger when there are fewer hierarchical levels. The latter is corroborated by the evidence in a recent paper by Ellul, Pagano, and Schivardi (2014) who show that family firms (who are likely to be smaller) provide greater employment protection but less wage stability. Consequently, smaller firms are more likely adjust on the wage margin in the presence of binding constraints on external financing. The

estimates also imply that firms with higher cash flows are less likely to reduce wages if their credit institutions are hit by a funding shock (column (5)). Finally, more profitable firms are less likely to reduce employment (column (3)) and wages (column (6)) when the cost of external financing goes up, albeit in both cases the effect is insignificant.

While informative, this latest test is intrinsically imperfect because firm characteristics such as size and cash flow are endogenous in that they can be adjusted by the firm in response to credit shocks and are in general determined together with labor demand. In Table 10, we take advantage of the predetermined technological characteristics of the industry the firm operates in. We use data on mature US firms and construct industry benchmarks for risk and asset tangibility. Rajan and Zingales (1998) argue that the actual corporate structure of small firms is a function of financial constraints, while the corporate structure of large mature firms is more representative of the cross-industry variations in the scale of projects, gestation period, the ratio of tangible vs. intangible assets, R&D investment, etc. The idea is that the financing decisions by large listed US firms reflect the industry's natural demand for external funds.

Table 8 reports a version of the main model where the interaction of the *Affected* and the *Post* dummies is also interacted with one of three industry characteristics: “External dependence”, “Employment-capital ratio”, and “R&D intensity”.⁵ The estimates suggest that firms are more likely to reduce employment if their production process is more R&D-intensive (column (3)). Unlike firms in physical capital-intensive industries, firms with R&D-intensive processes are less able to pledge more tangible collateral in order to convince a bank in distress to keep lending to them. We also find that firms in industries that rely on external finance for technological reasons are more likely to reduce wages in response to an impaired access to

⁵ The three benchmarks are constructed by calculating—for each mature Compustat firm between 1990 and 2000—the firm's ratio of capital expenditures not financed with internal funds (in the case of “External dependence”); the ratio of the number of employees to total physical capital used in production (in the case of “Employment-capital intensity”); and of research and development expenses to sales (in the case of “R&D intensity”). We then take the industry median value. The three benchmarks should ideally capture partially risk and partially asset tangibility.

financing (column (4)). Finally, firms are less likely to reduce wages if their production process is more labor dependent (i.e., there are more employees per unit of capital to begin with; column (5)), which in combination with the negative coefficient in column (2) implies that firms where labor is relatively abundant are more likely to adjust in the employment margin, while firms where labor is relatively scarce are more likely to keep their employees and adjust in the wage margin.

5. Conclusion

In this paper, we analyse the effect of shocks to lenders on firms' labor demand using a sample of 64,745 German firms over the period 2005–2012. We find that firms with a credit relationship with at least one affected bank experience a significantly larger decline in employment and in average labor compensation after the beginning of the global financial crisis. Our estimates imply that relative to firms associated with non-affected banks, firms associated with affected banks reduce employment by up to 1.5% and average wages for the retained employees by up to 1.8%. We also find that this effect takes three years to dissipate, suggesting that financial (and in particular, banking) crises can induce a long-lasting disruption in labor markets. By providing evidence on the negative employment effect of balance sheet shocks to financial institutions, our paper informs the current debate on whether fiscal expansion or addressing bank balance sheet weaknesses is the best policy response to high unemployment during financial crises (e.g., Guajardo, Leigh, and Pescatori, 2011; Calvo, Coricelli, and Otonello, 2013).

Our work leaves open a number of interesting and important extensions. First, how does employment legislation affect the incentive of credit constrained firms to retain workers during recession? Second, employment shocks could be a catalyst for change in the industrial structure of the economy, leading to better long-term human capital allocation and higher wages. Investigating these mechanisms presents itself as an attractive avenue for future research.

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Figure 1. Employment dynamics, affected firms relative to non-affected ones

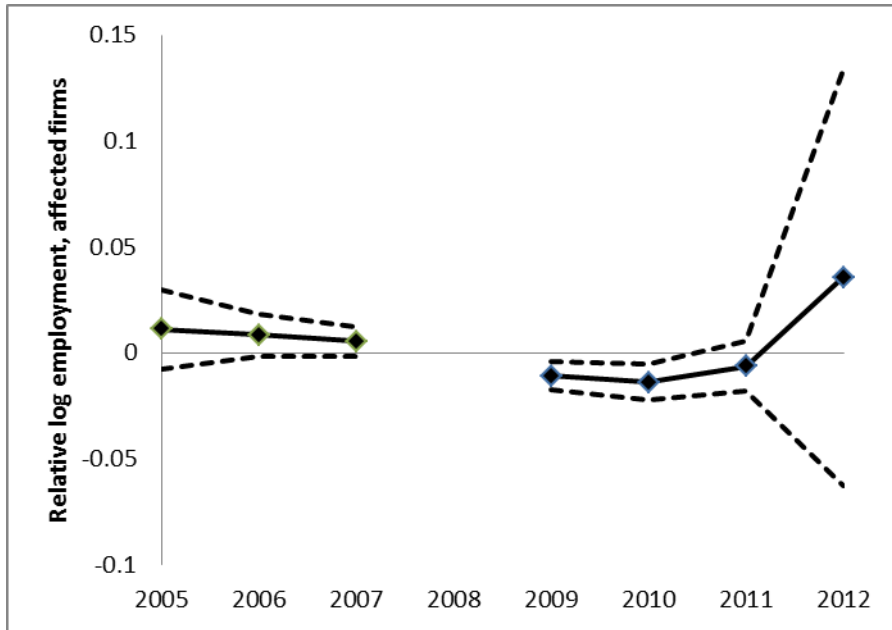
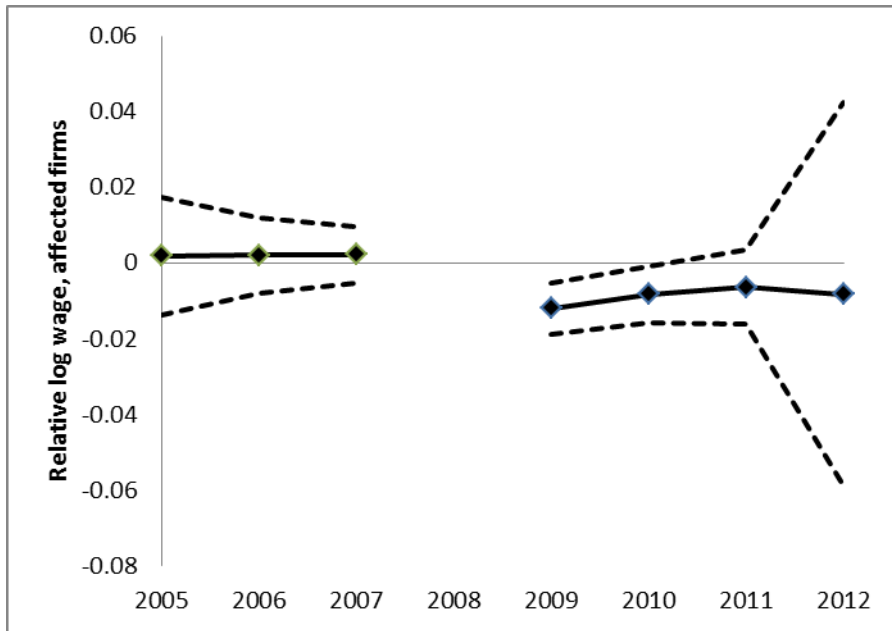


Figure 2. Wage dynamics, affected firms relative to non-affected ones



Note: The figures plot coefficients from regressions of log employment (Figure 1) and log wages (Figure 2) on indicator variables extending from 2005 to 2012, interacted with a dummy equal to 1 if the firm is associated with an affected bank. The indicator variable for 2008 is omitted, so that coefficients are measured relative to log employment and log wages in 2008. Firm-specific data, firm fixed effects, and year fixed effects are also included in the regressions (see Models (1) and (2)). The dashed lines represent 95% confidence intervals, with standard errors clustered by firm.

Table 1. Summary statistics

This table presents summary statistics for the variables used in the empirical tests. ‘Affected’ is a dummy variable equal to 1 when at least one of the firm’s banks is an owner of one of the five Landesbanken that were affected by the US subprime mortgage crisis after August 2007, and to 0 otherwise. ‘Employment’ denotes the number of the firm’s total employees. ‘Average wage’ denotes the firm’s total wage bill in euro, divided by total number of employees. ‘Assets’ denotes the firm’s total assets, in € million. ‘Sales’ denotes the firm’s total sales, in € million. ‘Capital’ denotes the ratio of the firm’s equity to total assets. Profit denotes the ratio of the firm’s total profits to total assets. ‘Cash flow’ denotes the ratio of the firm’s total cash flows to total assets. ‘No. bank relationships’ reports the total number of banks with which the firm has a credit relationship. The sample period is 2005–2012. Only firms with at least one observation during the pre- period and at least one observation during the post- period are included. The values reported are calculated over all firm-year observations.

Variable	# firms	Mean	Median	St. dev	Min	Max
Affected	64,745	0.22	0.00	0.42	0.00	1.00
Employment	56,773	70.62	22.00	302.73	1.00	131,313.00
Average wage	37,935	41,097.95	37,313.70	29,490.08	200.00	500,000.00
Assets	64,745	6.04	2.14	81.17	0.00	21,625.59
Sales	64,745	6.24	0.04	45.55	0.00	19,170.71
Capital	64,711	0.31	0.27	0.27	-28.81	2.08
Profit	40,072	0.05	0.03	0.53	-47.53	161.39
Cash flow	40,072	0.10	0.08	0.92	-25.73	250.36
No. bank relationships	64,745	1.84	2.00	1.06	1.00	6.00

Table 2. Affected vs. non-affected firms, pre-crisis

This table presents difference-in-differences estimate from a Mann-Whitney two-sided test on pre-2008 mean values of the variables used in the empirical tests, for affected vs. non-affected firms. ‘Affected’ is a dummy variable equal to 1 when at least one of the firm’s banks is an owner of one of the five Landesbanken that were affected by the US subprime mortgage crisis after August 2007, and to 0 otherwise. ‘Employment’ denotes the number of the firm’s total employees. ‘Average wage’ denotes the firm’s total wage bill in euro, divided by total number of employees. ‘Assets’ denotes the firm’s total assets, in € million. ‘Sales’ denotes the firm’s total sales, in € million. ‘Capital’ denotes the ratio of the firm’s equity to total assets. ‘Profit’ denotes the firm’s the ratio of the firm’s total profits to total assets. ‘Cash flow’ denotes the ratio of the firm’s total cash flows to total assets. ‘No. bank relationships’ reports the total number of banks with which the firm has a credit relationship. The sample period is 2005–2008. Only firms with at least one observation during the pre- period and at least one observation during the post- period are included. Standard errors are reported in parentheses, where *** indicates significance at the 1% level, ** at the 5% level, and * at the 10% level.

Variable	Non-affected	Affected	Difference
Employment (pre-2009)	59.80	70.51	-10.71***
Average wage (pre-2009)	43,489.27	38,207.43	5,281.84
Assets (mln.) (pre-2009)	5.88	5.98	-0.10
Sales (mln.) (pre-2009)	6.24	5.62	0.62*
Capital (pre-2009)	0.30	0.29	0.01***
Profit (pre-2009)	0.05	0.05	0.00
Cash flow (pre-2009)	0.11	0.10	0.01
No. bank relationships	1.73	2.22	-0.49***

Table 3. The effect of financing constraints on firms' employment and compensation: Main result

This table presents difference-in-differences estimates of changes in firm employment (columns (1)–(2)) and average wage (columns (3)–(4)). 'Affected' is a dummy variable equal to 1 when at least one of the firm's banks is an owner of one of the five Landesbanken that were affected by the US subprime mortgage crisis after August 2007, and to 0 otherwise. 'Post_2008' is a dummy equal to 1 after 2008 and to 0 before that. 'Log (Assets)' denotes the natural logarithm of the firm's total assets. 'Log (Sales)' denotes the natural logarithm of the firm's total sales. 'Capital' denotes the ratio of the firm's equity to total assets. 'Profit' denotes the firm's the ratio of the firm's total profits to total assets. 'Cash flow' denotes the ratio of the firm's total cash flows to total assets. The sample period is 2005–2012. Only firms with at least one observation during the pre- period and at least one observation during the post- period are included. All regressions include fixed effects as specified. Standard errors (clustered at firm level) are reported in parentheses, where *** indicates significance at the 1% level, ** at the 5% level, and * at the 10% level.

	Log (Employment)		Log (Average wage)	
	(1)	(2)	(3)	(4)
Affected × Post_2008	-0.0154*** (0.0038)	-0.0158*** (0.0038)	-0.0105*** (0.0030)	-0.0108*** (0.0030)
Affected	0.0876*** (0.0139)		0.0010 (0.0061)	
Post_2008	0.0214*** (0.0021)		0.0244*** (0.0016)	
Log (Assets)	0.2596*** (0.0078)	0.2076*** (0.0097)	0.1077*** (0.0061)	0.1182*** (0.0172)
Log (Sales)	0.1478*** (0.0059)	0.1190*** (0.0062)	0.0398*** (0.0053)	0.0483*** (0.0071)
Capital	0.0573*** (0.0146)	0.0371** (0.0160)	0.0874*** (0.0096)	0.0501*** (0.0146)
Profit	-0.0559** (0.0151)	-0.0479** (0.0120)	0.0286 (0.0250)	0.0168 (0.0285)
Cash flow	0.0098 (0.0065)	0.0071 (0.0048)	0.0323** (0.0165)	0.0365** (0.0171)
No. bank relationships	0.1339*** (0.0055)		-0.0154*** (0.0025)	
Year fixed effects	No	Yes	No	Yes
Firm fixed effects	No	Yes	No	Yes
No. Observations	132,548	132,548	164,507	164,212
No. Firms	30,732	30,732	32,760	32,760
R-squared	0.27	0.28	0.13	0.14

Table 4. The effect of financing constraints on firms' employment and compensation: Accounting for regional differences

This table presents difference-in-differences estimates of changes in firm employment (columns (1) and (3)) and average wage (columns (2) and (4)). The regressions are performed on the sub-sample of firms with information of region of incorporation (columns (1)–(2)) and on the sub-sample of firms in affected regions only (columns (3)–(4)). 'Affected' is a dummy variable equal to 1 when at least one of the firm's banks is an owner of one of the five Landesbanken that were affected by the US subprime mortgage crisis after August 2007, and to 0 otherwise. 'Post_2008' is a dummy equal to 1 after 2008 and to 0 before that. The sample period is 2005–2012. Only firms with at least one observation during the pre- period and at least one observation during the post- period are included. All regressions include all firm controls from Table 3, as well as fixed effects as specified. Standard errors (clustered at firm level) are reported in parentheses, where *** indicates significance at the 1% level, ** at the 5% level, and * at the 10% level.

	Log (Employment) (1)	Log (Average wage) (2)	Log (Employment) (3)	Log (Average wage) (4)
Affected × Post_2008	-0.0083* (0.0048)	-0.0079** (0.0040)	-0.0112*** (0.0048)	-0.0082** (0.0039)
Firm controls	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Region × Year fixed effects	Yes	Yes	No	No
No. Observations	114,420	111,850	70,853	69,716
No. Firms	26,876	23,134	16,339	14,311
R-squared	0.25	0.10	0.26	0.11

Table 5. The effect of financing constraints on firms' employment and compensation: Exporters vs. non-exporters

This table presents difference-in-differences estimates of changes in firm employment (columns (1)–(2)) and average wage (columns (3)–(4)). The regressions are performed on the sub-samples of exporting (columns (1) and (3)) and non-exporting (columns (2) and (4)) firms. 'Affected' is a dummy variable equal to 1 when at least one of the firm's banks is an owner of one of the five Landesbanken that were affected by the US subprime mortgage crisis after August 2007, and to 0 otherwise. 'Post_2008' is a dummy equal to 1 after 2008 and to 0 before that. The sample period is 2005–2012. Only firms with at least one observation during the pre- period and at least one observation during the post- period are included. All regressions include all firm controls from Table 3, as well as fixed effects as specified. Standard errors (clustered at firm level) are reported in parentheses, where *** indicates significance at the 1% level, ** at the 5% level, and * at the 10% level.

	Log (Employment)		Log (Average wage)	
	Exporters (1)	Non-exporters (2)	Exporters (3)	Non-exporters (4)
Affected × Post_2008	-0.0160*** (0.0056)	-0.0133** (0.0050)	-0.0148*** (0.0053)	-0.0078** (0.0037)
Firm controls	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
No. Observations	35,340	97,208	43,880	120,332
No. Firms	7,387	23,345	8,444	24,316
R-squared	0.29	0.28	0.11	0.14

Table 6. The effect of financing constraints on firms' employment and compensation: Comparison over time

This table presents difference-in-differences estimates of changes in firm employment (column (1)) and average wage (column (2)). 'Affected' is a dummy variable equal to 1 when at least one of the firm's banks is an owner of one of the five Landesbanken that were affected by the US subprime mortgage crisis after August 2007, and to 0 otherwise. 'Dummy_2005_2006' is a dummy equal to 1 during 2005 and 2006 and to 0 otherwise. The sample period is 2005–2012. Only firms with at least one observation during the pre- period (2005–2008) and at least one observation during the post- period (2009–2012) are included. All regressions include all firm controls from Table 3, as well as fixed effects as specified. Standard errors (clustered at firm level) are reported in parentheses, where *** indicates significance at the 1% level, ** at the 5% level, and * at the 10% level.

	Log (Employment)	Log (Average wage)
	(1)	(2)
Affected × Dummy_2005_2006	0.0064 (0.0048)	0.0009 (0.0045)
Affected × Dummy_2009	-0.0133*** (0.0034)	-0.0131*** (0.0032)
Affected × Dummy_2010	-0.0166*** (0.0043)	-0.0093*** (0.0036)
Affected × Dummy_2011	-0.0090 (0.0060)	-0.0074 (0.0048)
Affected × Dummy_2012	0.0328 (0.0503)	-0.0093 (0.0258)
Firm controls	Yes	Yes
Year fixed effects	Yes	Yes
Firm fixed effects	Yes	Yes
No. Observations	132,548	164,212
No. Firms	30,732	32,760
R-squared	0.28	0.13

Table 7. The effect of financing constraints on firms' employment and compensation:
Differentiating by firm characteristics

This table presents difference-in-differences estimates of changes in firm employment (columns (1)–(3)) and average wage (columns (4)–(6)). 'Affected' is a dummy variable equal to 1 when at least one of the firm's banks is an owner of one of the five Landesbanken that were affected by the US subprime mortgage crisis after August 2007, and to 0 otherwise. 'Post_2008' is a dummy equal to 1 after 2008 and to 0 before that. 'Small' is a dummy variable equal to 1 if the firm has fewer than 20 employees, and to 0 otherwise. 'Cash flow' denotes the ratio of the firm's total cash flows to total assets. 'Profit' denotes the firm's the ratio of the firm's total profits to total assets. The sample period is 2005–2012. Only firms with at least one observation during the pre- period and at least one observation during the post- period are included. All regressions include all firm controls from Table 3, as well as fixed effects as specified. Standard errors (clustered at firm level) are reported in parentheses, where *** indicates significance at the 1% level, ** at the 5% level, and * at the 10% level.

	Log (Employment)			Log (Average wage)		
	(1)	(2)	(3)	(4)	(5)	(6)
Affected × Post_2008 ×						
Small	0.1217*** (0.0187)			-0.0492*** (0.0151)		
Cash flow		-0.0058 (0.0214)			0.0565*** (0.0170)	
Profit			-0.0025 (0.0261)			0.0267 (0.0242)
Double interactions	Yes	Yes	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
No. Observations	132,548	132,548	132,548	164,212	164,212	164,212
No. Firms	30,732	30,732	30,732	32,760	32,760	32,760
R-squared	0.34	0.28	0.28	0.14	0.14	0.14

Table 8. The effect of financing constraints on firms' employment and compensation:
Differentiating by industry characteristics

This table presents difference-in-differences estimates of changes in firm employment (columns (1)–(3)) and average wage (columns (4)–(6)). 'Affected' is a dummy variable equal to 1 when at least one of the firm's banks is an owner of one of the five Landesbanken that were affected by the US subprime mortgage crisis after August 2007, and to 0 otherwise. 'Post_2008' is a dummy equal to 1 after 2008 and to 0 before that. 'External dependence' is the industry median fraction of capital expenditures not financed with internal funds for mature COMPUSTAT companies during 1980–1990. 'Employment-capital ratio' is the industry median ratio of employment to total physical capital for mature COMPUSTAT companies during 1980–1990. 'R&D intensity' is the industry median ratio of R&D expenditures to total sales for mature COMPUSTAT companies during 1980–1990. The sample period is 2005–2012. Only firms with at least one observation during the pre- period and at least one observation during the post- period are included. All regressions include all firm controls from Table 3, as well as fixed effects as specified. Standard errors (clustered at firm level) are reported in parentheses, where *** indicates significance at the 1% level, ** at the 5% level, and * at the 10% level.

	Log (Employment)			Log (Average wage)		
	(1)	(2)	(3)	(4)	(5)	(6)
Affected × Post_2008 ×						
External dependence	-0.0070 (0.0189)			-0.0237** (0.0126)		
Employment-capital ratio		-0.0001 (0.0003)			0.0004** (0.0002)	
R&D intensity			-0.1438* (0.0854)			-0.0626 (0.1001)
Double interactions	Yes	Yes	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
No. Observations	62,437	62,437	59,197	80,437	80,437	76,616
No. Firms	14,016	14,016	13,236	15,769	15,769	15,012
R-squared	0.32	0.32	0.30	0.15	0.14	0.15

Table A1. Variables: Definitions and sources

Variable	Definition	Source
Affected	Dummy variable equal to 1 when at least one of the firm's banks is an owner of one of the five Landesbanken that were affected by the US subprime mortgage crisis after August 2007: Sachsen LB, West LB, Bayern LB, LBBW, and HSH Nordbank.	Puri, Rocholl, and Steffen (2011)
Post_2008	Dummy variable equal to 1 after 2008.	
Employment	The number of the firm's total employees.	Dafne
Average wage	The firm's total wage bill in euro, divided by total number of employees.	Dafne
Assets (mln.)	The firm's total assets, in € million.	Dafne
Sales (mln.)	The firm's total sales, in € million.	Dafne
Capital	The ratio of the firm's equity to total assets.	Dafne
Profit	The firm's the ratio of the firm's total profits to total assets.	Dafne
Cash flow	The firm's total cash flows to total assets.	Dafne
No. bank relationships	The number of banks with which the firm has a credit relationship.	Dafne
Small	Dummy variable equal to 1 if the firm has fewer than 20 employees.	Dafne
External dependence	The median fraction of capital expenditures not financed with internal funds for mature COMPUSTAT companies during 1980-1990, for the firm's 2-digit NACE industry.	Compustat North America
Employment-capital ratio	The median ratio of employment to total physical capital for mature COMPUSTAT companies during 1980-1990, for the firm's 2-digit NACE industry.	Compustat North America
R&D intensity	The median ratio of R&D expenditures to total sales for mature COMPUSTAT companies during 1980-1990, for the firm's 2-digit NACE industry.	Compustat North America