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Firm-level employment, labour market reforms, and bank distress

Abstract

We explore the interaction between labour market reforms and financial frictions. Our study combines a new cross-country reform database on labour market reforms with matched firm-bank data for nine euro area countries over the period 1999 to 2013. While we find that labour market reforms are overall effective in increasing employment, restricted access to bank credit can undo up to half of long-term employment gains at the firm-level. Entrepreneurs without sufficient access to credit cannot reap the full benefits of more flexible employment regulation.

Non-technical summary

There is a consensus among policymakers and academics that labour market reforms entail long-term employment gains. At the same time, several macro-level studies suggest that there is substantial heterogeneity in the magnitude of these benefits across countries and across time. For example, the short-term benefits of some labour market reforms seem to be lower when they are implemented during recessions. While there is ample evidence for asymmetric effects of reforms at the macro level, few studies investigate heterogeneity in the effects of reforms at the firm-level.

Labour market reforms incentivise entrepreneurs to undertake new projects and hire new employees. However, entrepreneurs will need additional financing to pay the wages of new employees and to purchase the new machines needed to put them to productive use. Given that the typical European firm is small and bank-dependent, we hypothesise that a firm's ability to profit from labour market reforms will be limited if it is connected to a weak relationship bank as opposed to a strong one. This hypothesis seems especially relevant for countries in the periphery of the euro area featuring weaknesses in their banking sector at the same time as high levels of labour market rigidity and unemployment.

We study the interdependency of labour market reforms and bank financing conditions by combining data on reforms in Austria, Germany, Spain, France, Greece, Ireland, Luxembourg, the Netherlands, and Portugal during the period of 1999 to 2013 with a matched bank-firm sample of roughly 390,000 firms and 1,100 banks. The advantage of using micro-data is that we can disentangle the effect of country-level developments like business cycles from the effect of bank financing conditions faced by individual firms. We construct an index that proxies firm-specific bank financing conditions based on the health of each firm's main relationship bank taking into account the bank's capitalisation, NPL ratio, return on assets, z-score, and liquidity. Based on this index we categorise firms as either being connected to a weak or to a strong bank.

We then compare the employment gains of these two groups of firms after the implementation of three different types of labour market reforms: i) reforms that liberalise employment protection for regular workers, ii) reforms that liberalise unemployment benefit schemes, where liberalising would imply reducing the generosity of the benefit scheme in terms of size and/or duration, and iii) reforms that liberalise regulation of temporary employment, where liberalising would, for example, imply loosening rules restricting the use of temporary vs. regular employ-

ment contracts.

Using difference-in-difference regressions, we find that all three types of labour market reforms induce a long-term increase in firm-level employment. This increase is strongest in the case of reforms that reduce the generosity of unemployment benefit schemes. At the same time, this reform type exhibits the strongest differential effect of bank financing conditions on employment outcomes: Firms that were connected to a weak relationship bank after the implementation of an unemployment benefit reform only increase employment by half as much as firms connected to strong banks.

In order to uncover the channel through which bank distress affects the success of labour market reforms, we use several criteria to further differentiate among firms connected to weak banks after the implementation of labour market reforms. In line with our hypothesis, the negative link between bank health and firm-level reform outcomes is tighter for particularly bank dependent firms. We find that especially smaller firms, which have fewer alternative sources of financing, are impeded in their ability to profit from labour market reforms when their relationship bank is in distress. Moreover, the negative link between bank distress and reform success is stronger for firms operating in sectors with high external financial dependence and for firms in sectors that produce goods of high durability. The intensity of the interdependence is also affected by the business cycle. The negative interdependence is stronger during recessions when firms' balance sheets are weaker. However, the negative relationship between bank distress and firm-level employment outcomes of reforms is also present during normal times.

We support our main result with several robustness tests addressing the endogeneity of bank-firm matching, reverse causality from firm health to bank health and alternative econometric specifications.

Note that the micro-level results in our paper do not necessarily translate into macroeconomic effects and that we are abstracting from short-term costs and distributional implications of reforms. Nonetheless, the differential effect of bank financing conditions on firm-level reform outcomes suggests that countries set on implementing a labour market reform can benefit from combining it with policies strengthening bank balance sheets.

1 Introduction

We study how financial frictions impact the effectiveness of labour market reforms. Entrepreneurs need to have ample financing to fully take advantage of labour market deregulation. Being connected to a stressed relationship bank might prevent entrepreneurs from obtaining sufficient funding to finance the employees and machines needed for the new projects induced by a reform.

A number of studies have shown that labour market rigidities are a significant impediment to labour market clearing, resulting in high and persistent unemployment.[1] Several euro area countries feature both a high level of labour market rigidity and weaknesses in their banking sector. Moreover, the typical European firm is small and heavily dependent on bank financing. If financial frictions matter for labour market reform outcomes, the presence of weak banks might modify the employment gains from labour market liberalisation in these countries. This raises the issue of the sequencing of reforms. Our results suggest that countries can gain from complementing labour market reforms with a comprehensive clean-up of the banking sector.

Our research question also touches a deeper economic issue, namely the way financial market and factor market frictions interact in shaping firm policies. We analyse how the benefits of removing frictions in one market (labour) can be undermined by existing frictions in another market (financial). Our hypothesis is based on the theoretical framework on the interaction of labour and credit market frictions by Wasmer and Weil (2004). Their study suggests that entrepreneurs need bank financing to take full advantage of labour market deregulation. The standard argument for labour market reforms is that the resulting decrease in the user cost of labour will induce entrepreneurs to undertake the marginal project, which they were hesitating to implement before. However, insufficient access to funding can make entrepreneurs unable to finance the employees and machines needed for this marginal project. Hence, the new project might not be implemented at all or only on a smaller scale. The same holds for complementary follow-up projects, with potential long-term negative consequences for firm-level and economy-wide growth.[2]

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[1] For an extensive overview of the literature on structural reforms and their interdependencies with other policies see Masuch et al. (2018).

[2] One can think of entrepreneurs connected to weak banks as being trapped in a bad equilibrium which lowers the long-term employment gains from the reform. Thus, cleaning up the banking sector in combination with a labour market reform might help to push more firms towards the good equilibrium. For an exposition on how complementary projects can lead to a "big push" in a multiple equilibria setting see the study by Murphy et al. (1989).

We test our hypothesis empirically by studying the interaction of labour market reforms and bank financing conditions in a matched bank-firm sample with annual data from nine euro area countries over the period 1999 to 2013. We find that the gains in firm-level employment induced by labour market reforms are significantly reduced if a firm is connected to a weak bank after the implementation of a reform. We also find that the negative link between bank distress and reform success is strongest among firms that are more bank dependent and have less capacity for internal financing. Specifically, we find that the negative link is stronger for firms that are small, sell goods of high durability, or operate with a production technology inducing high external financial dependence. Moreover, the link is stronger during firm-years characterised by recessions. At the same time, our analysis of recessions also shows that bank distress reduces firm-level employment gains from reforms even during normal times.

We study three labour market reform types of major interest for policymakers in advanced economies: i) those that liberalise or tighten employment protection for regular workers, ii) those that reduce or increase the generosity of unemployment benefit schemes in terms of size and/or duration, and iii) those that liberalise or tighten the regulation of temporary employment.[3] Our empirical setup extends the difference-in-differences approach employed in Simintzi et al. (2015) to compare firms in countries that implement one of our three reform types with similar firms that are not implementing the same reform at the same point in time. To explore the interdependency of reform success of banking financing, we then compare firms connected to either weak or strong banks within the same country after a reform has been implemented.

Among our three reform types, the negative relationship between bank weakness and reform success is most pronounced for unemployment benefit reforms. While a reform reducing the generosity of unemployment benefit schemes can induce a long-term shift in firm-level employment of up to 8.5%, the same effect is only 4.8% for firms connected to a weak bank. We estimate this differential effect of bank financing conditions after controlling for country-level factors with a rich fixed effects structure. Disentangling the effect of bank financing conditions from general macroeconomic developments like the business cycle is only possible with firm-level micro data. Note, however, that according to Gal and Hijzen (2016) unweighted firm-level regressions only capture the response of the typical incumbent firm which means that one must be careful in deriving conclusions about the aggregate effect of reforms. We are focusing on the differen-

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[3]In the following, we will refer to liberalising reforms simply as "reforms" while calling tightening reforms "counter-reforms". Note that our dataset provides too few tightening counter-reforms to allow us to provide a separate estimate for counter-reforms, which is why we also do not discuss them separately.

tial employment impact of reforms on financially constrained firms, which means that we are also abstracting from the effect of reforms on productivity and sectoral reallocation. Moreover, our focus on employment effects in the long-run means that we are abstracting from potential short-term costs and distributional implications of reforms.[4]

Firms and banks match in an unobservable process, which might drive our results. We address this issue by excluding firms whose relationship bank is relatively weak already at the time these firms enter the sample. This set of firms is more likely to have matched with weak banks purposefully (see Schwert (2018)). The sample restriction thus leaves us with a set of particularly bank-dependent firms that start out with a strong bank which subsequently becomes weak. This restriction increases the power of our model to detect the negative interaction between bank weakness and reforms. While our baseline aproach only allows us to find a negative relationship between bank financing and unemployment benefit reforms, we can now find this relationship for all three reform types.

Another confounding factor could be reverse causality between bank and firm health. Large firms might be able to drag down their relationship bank once their own health deteriorates. To address this concern, we re-estimate our model using only small- and micro firms. Banks are better able to diversify a portfolio of exposures to small- and micro firms and it is less likely that a deterioration in their health turns a strong bank into a weak one. Small- and micro firms also are a set of firms that are particularly bank-dependent. Thus, our main result that bank financing limits the employment gains of unemployment benefit reforms holds and even grows in magnitude.

Our paper contributes to three strands of literature. It relates to the literature focusing on the macroeconomic impact of structural reforms. A host of recent studies look at product and labour market reforms and their interdependence with macroeconomic policies and conditions.[5] Duval et al. (2017), Adhikari et al. (2018), and Duval and Furceri (2018) focus on labour market reforms in particular and find that their short-term impact on employment is only positive during upturns. We contribute by showing that bank financing conditions have a similar but independent impact on reform success by using our micro-data setting to control for time-varying developments at the country-level.

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[4]Bassanini and Cingano (2019) show that labour market reforms can lead to transitory employment losses and Krebs and Scheffel (2013) show how labour market reforms can create winners and losers with unevenly distributed welfare losses.

[5]See Bouis et al. (2016), Banerji et al. (2017), Ciminelli et al. (2018b), Duval et al. (2017), Adhikari et al. (2018), and Duval and Furceri (2018).

Several studies explore the impact of labour market reforms using cross-country firm-level data. Simintzi et al. (2015) find that reforms increasing employment protection decrease firm leverage. They argue that an increase in labour market protection constitutes an increase in operating leverage and thus crowds out financial leverage.[6] We provide a new angle by studying firm policies when there is a decrease in operating leverage through a liberalising reform but firm leverage cannot rise due to financial frictions. Some recent studies also focus on the interaction of labour market reforms and firm-level financing conditions. Using data on syndicated loans to large public companies, Alimov (2015) shows that higher employment protection decreases firms' access to credit in terms of higher loan spreads and less favorable non-pricing terms. Using survey data, Moro et al. (2017) show that lower employment protection increases firms' access to credit. Cingano et al. (2010) and Calcagnini et al. (2009, 2014, 2015) find that financial constraints exacerbate the negative impact of increases in employment protection on firm investment. In these studies, financial constraints are proxied with different measures of firms' internal liquidity. Antoun de Almeida and Balasundharam (2018) proxy financial constraints with firm leverage and find no significant interaction between firm leverage and the employment outcomes of labour market reforms. We contribute by showing that firm-level employment outcomes of labour market reforms depend on the differential impact of banks' ability to grant credit. More specifically, the effects of labour market policies depend on firm-specific bank financing conditions, which are arguably independent of firms' own growth and financing decisions at different stages of their life cycle.[7] This is important from a policy-making perspective because regulators can more easily target bank health than wide-spread firm balance sheet weaknesses. Thus, a comprehensive clean-up of the banking sector might ensure successful reform outcomes even when firms have few internal funds based on observable liquidity measures. Furthermore, we contribute relative to the studies mentioned above by differentiating between three different types of labour market reforms classified in a novel reform database.[8]

Finally, our study contributes to the literature on firm financing and employment protection.

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[6]Serfling (2016) obtains a similar result using data on US state-level legislation and Karpuz et al. (2018) find that the increase in operating leverage also induces firms to increase cash holdings.

[7]Farre-Mensa and Ljungqvist (2016) test different firm-level measures of financial constraints commonly used in the literature and conclude that they mostly reflect differences in firms' own policies.

[8]The database by Duval et al. (2018b) has several advantages relative to other databases commonly used in the literature. Rather than providing a long list of reforms as the European Commission's LABREF database, it focuses on major reforms, which are identified by textual analysis of OECD country reports. While changes in widely used OECD labour market indicators also feed into the mechanism identifying major reforms, the database by Duval et al. (2018b) has the additional advantage of providing the exact implementation dates of the underlying policies.

Cingano et al. (2016) show how the introduction of employment protection for previously exempt small Italian firms leads to higher investment, especially among firms endowed with higher internal liquidity. Claessens and Ueda (2018) find that the staggered introduction of wrongful-discharge laws in US states increased growth in knowledge intensive industries, especially in states with simultaneous bank branch deregulation.[9] In contrast, Bai et al. (2019) and Lee and Shin (2018) find that wrongful-discharge laws in the US lead to lower firm-level investment, especially among financially constrained firms. Laeven et al. (2018) compare the performance of firms subject to different employment protection rules during the financial crisis in Spain. While they study the effect of financial shocks at given levels of labour market frictions, we study the effect of changes in labour market frictions given different levels of financial frictions.

2 Theoretical background

2.1 Labour market reforms and employment

We consider three types of labour market reforms. We look at reforms that decrease employment protection for regular workers. We analyse reforms that reduce the generosity and/or duration of unemployment benefit schemes and we look at reforms that decrease the regulation of temporary employment.

Lazear (1990) shows how reforms that liberalise employment protection for regular workers can increase firm-level employment by decreasing dismissal costs and encouraging new hiring. However, he points out that in the short run these types of reforms can have a countervailing effect on employment by making it easier to fire existing workers. In the short-run, the countervailing effect might dominate since laying off incumbent employees can be more swiftly implemented than searching for and hiring adequate new ones. As we focus on long-run effects, we would expect to find an overall positive effect of regular employment reforms on firm-level employment.

There are two different mechanisms how reforms that reduce the generosity of unemployment benefit schemes can decrease the user cost of labour and thereby stimulate firm labour demand. Firstly, unemployment benefit reforms can make hiring more attractive by decreasing aggregate wages. According to Schmieder et al. (2016), making benefits less generous effectively reduces

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[9]According to theoretical work by Janiak and Wasmer (2014), increasing employment protection from very low levels can lead to higher capital-labour ratios due to the complementarity between physical and firm-specific human capital.

workers' outside option. This will lead to lower equilibrium wages for new hires either by the reduction in workers' bargaining power or by the reduction in workers' reservation wages.[10] Krause and Uhlig (2012) describe a second mechanism how unemployment benefit reforms can stimulate firm labour demand. In their model, a decrease in unemployment benefits lowers the relative cost of posting a vacancy by increasing the probability that a given vacancy will be filled. Here, the reform leads to higher firm labour demand through lower entry cost of employers into the matching market rather than through lower wages.

Temporary employment reforms can encourage hiring by decreasing dismissal costs for fixed-term employees or by lifting restrictions on hiring on a temporary basis rather than on an open-ended basis.[11] In addition, our reform dataset also counts reforms that ease restrictions on the use of temporary agency workers as temporary employment reforms. Given our focus on within-firm changes, this particular manifestation of a temporary employment reform could actually lead to a negative effect on regular firm-level employment.[12] These countervailing effects decrease our power to determine the effects of temporary employment reforms on firm-level employment and obscure the role of financing in moderating the size of employment effects.

2.2 Labour market reforms and financing

After having discussed how labour market reforms can increase employment, we now examine how financing can limit the magnitude of this increase. The most relevant study in this respect is the paper by Wasmer and Weil (2004), which combines credit market and labour market frictions in a DGSE model where employers need to obtain financing from a bank before they can post a vacancy. They show that even when labour markets move towards a frictionless state, financing will still be a limiting factor for job creation. Other theoretical studies that suggest a complementarity between access to credit and labour market deregulation are Koskela

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[10]As described in Pissarides (2001), the wage setting process can be modelled either as a bargaining process or as a wage posting mechanism. In the latter mechanism, the decrease in workers' outside options lowers their reservation wage and makes them accept take-it-or-leave-it offers with relatively lower wages. If one assumes a wage bargaining mechanism, as we do in Section 2.3, the diminished outside option lowers workers' bargaining power. Employers take this into account and push for lower wages when negotiating with potential new hires.

[11]An example for such a reform would be the extension of the continuous time period during which workers can be employed on the basis of renewed fixed-term contracts as opposed to regular open-ended contracts.

[12]The actual size of this negative effect depends on country-specific reporting rules on whether temporary agency workers are counted on the payroll of the firm employing agency workers or the temporary work agency itself. Note that while this measurement uncertainty diminishes our ability to estimate effects for temporary employment reforms in particular, it is unlikely to introduce much bias into the empirical results for the other reform types. This is because among the countries in our sample, temporary agency workers only make up between 0.3% (Greece) and 3.5% (Netherlands) of total employment (OECD, 2013).

and Stenbacka (2004) and Rendon (2013). Recent models combining credit and labour market frictions focus more on the financial crisis and try to explore how their interaction amplifies financial shocks. They shed more light on the interaction between employment and credit frictions and identify small, young and highly-leveraged firms as the ones most likely to be affected by financial frictions in their employment decisions (Boeri et al., 2013; Buera et al., 2015).

2.3 Graphical analysis

We visualise the main theoretical mechanism of our study by using a small diagram, which is based on the stylised right-to-manage model from Ciminelli et al. (2018a). We assume a standard constant elasticity of substitution (CES) production function with constant returns to scale:

$$
\begin{aligned}
Y &= F(K, AL) \\
&= (\alpha(K)^\varepsilon + (1-\alpha)(AL)^\varepsilon)^{1/\varepsilon}
\end{aligned}
\tag{1}
$$

where K denotes capital, L denotes labour, and A denotes labour-augmenting technical change. ε relates to the elasticity of substitution between labour and capital according to $\sigma = 1/(1-\varepsilon)$. Capital and labour are complements if $\varepsilon < 0 \rightarrow \sigma < 1$ and substitutes if $\varepsilon > 0 \rightarrow \sigma > 1$.

Labour market frictions take the form of bargaining between employers and workers. After the wage is determined, employers take the wage as given and set employment.[13] We consider reforms of employment protection that decrease the bargaining power of workers and thereby lower the wage. This in turn increases firms' labour demand.

Figure 1 depicts how the decrease in wages lowers the user cost of labour, shifts the employers' isocost curve outwards and thereby increases firm demand for labour and capital. At this point, the interaction between labour market reforms and financing comes into play. Financing constraints, which are visualised by the black crosses inhibiting the movement of K, L, and the jump of the production isoquant, can prevent firms from increasing labour and capital to the new optimum. Note that we are assuming that $\varepsilon < 0 \rightarrow \sigma < 1$, which implies that labour and capital are complements so that the income effect dominates the substitution effect.[14] The

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[13]In a right-to-manage model, the manager retains the "right" to unilaterally set employment levels. This contrasts with the efficient bargaining model where employers and employees bargain over both wages and employment. According to Ciminelli et al. (2018a), the right-to-manage describes rather well the actual functioning of labour markets in Europe. Nevertheless, employees might also have some power over employment levels so that the actual bargaining process likely also contains elements from an efficient bargaining model.

[14]In theory, in a case where capital and labour are highly substitutable, the decrease in the user cost of labour

underlying narrative is that employers have to get financing before they can hire new workers and buy the new machines needed to put them to productive use.[15]

3   Data

3.1   Data and sample selection procedure

Our sample contains firms from nine out of a total of nineteen euro area countries, namely Austria, Germany, Spain, France, Greece, Ireland, Luxembourg, the Netherlands, and Portugal. For this subset of countries, we have extensive data on firm and bank characteristics, firm-bank links, and structural reforms.

We obtain information on labour market reforms for 13 euro area countries during the period of 1970-2013 from a newly released IMF database.[16] The database is described in Duval et al. (2018b) and follows a narrative approach, which relies on text-search of OECD country reports and a set of rules and cross-checks to identify "major" reforms that have a significant impact on labour (and product) markets. Table 1 shows the distribution of reforms across countries. Among the reforms in our sample, roughly 20% are tightening reforms. There is substantial heterogeneity in the sense that some countries enact a range of different reforms for each of our three reform types while, for example, Luxembourg does not enact a single reform that qualifies as "major" according to the IMF classification during our sample period. In the following section, we will describe how these reforms map into the recursive reform indicator used in our empirical analysis.

Our firm-level data is from Bureau van Dijk's ORBIS database and includes all euro area countries from 1999-2015. We remove firms with inconsistent balance sheets by dropping observations with zero, missing or negative total assets and observations in which the sum of total equity, current-, and non-current liabilities is below 98% or above 102% of total assets.[17] We use

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might even lead firms to demand less capital. However, according to several empirical studies most firms in Europe and the US operate with technologies that exhibit some degree of complementarity (Ciminelli et al., 2018a; Laeven et al., 2018; Antras, 2004; Oberfield and Raval, 2014; Lawrence, 2015). In a robustness check in Section 6.2, we explicitly look at firms where labour and capital are substitutes.

[15]According to the Ajello (2016)'s analysis of US firms' cash flow statements, roughly a third of the need for external financing emanates from working capital needs, which includes financing the wages of new workers, while the remaining part emanates from the accumulation of fixed capital.

[16]The database relies on the analysis of textual OECD data and OECD indicators, which is why euro area countries that are not part of the OECD, or have only joined the OECD very recently, are not covered. The countries with missing reform data are Cyprus, Estonia, Lithuania, Latvia, Malta, and Slovenia. Also note that the IMF reform database assigns a given reform to the year t if it was implemented within the first half of the year t while it assigns it to the year t+1 if it was implemented in the second half of the year t.

[17]Note that in order to decrease survivorship bias, we do not drop all inactive firms. Our results are robust

unconsolidated annual accounts to avoid double-counting when both the consolidated account of the parent (with all its subsidiaries) and the unconsolidated account of the parent (without subsidiaries) are available. Our analysis concentrates on the non-financial business economy, which excludes, for example, the government sector. For details on our sample composition and a discussion of representativeness of the database, we refer the reader to Section A in the appendix.

ORBIS also provides data on bank-firm linkages for 15 out of 19 euro area countries. The countries without data on bank-firm linkages in ORBIS are Belgium, Finland, Italy and Slovakia and are therefore excluded from our study. Note that the bank-firm link is taken from the 2017 ORBIS extract and is not time-varying. Thus, we are relying on the assumption put forward by Kalemli-Ozcan et al. (2015) and commonly used in the literature that bank-firm relationships are sticky.[18] We assume that a firm's reported relationship banks also reflect its borrowing relationships. We manually match the name of the firm's relationship banks with unconsolidated bank data from Bureau van Dijk's Bankscope database. Over 95% of bank names from the relationship database can be matched to a bank-identifier in Bankscope. For firms that report more than one bank relationship, we assign the largest domestic bank among the reported banks, in terms of total assets in the year 2000, as the company's main relationship bank. Only if there is no domestic bank available, we take the largest foreign bank as the firm's main relationship bank.[19]

3.2   Variable construction

In our analysis, we use variables at the firm-, bank-firm-, country- and sector level. The dependent variable is the natural logarithm of the total number of employees. To capture variation in a firm's indebtedness we control for leverage, defined as financial debt relative to total assets.[20] We proxy a firm's liquidity by dividing cash holdings by total assets and a firm's access to marketable collateral by controlling for tangibility, defined as tangible fixed assets over total assets.

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to excluding inactive firms as defined in Kalemli-Ozcan et al. (2015). This encompasses companies with a status that reads either "inactive", "dissolved", "in liquidation" or "bankruptcy". Regressions excluding inactive firms are available upon request.

[18]Giannetti and Ongena (2012) explicitly compare the 2005 and 2013 versions of the ORBIS relationship database and Kalemli-Ozcan et al. (2019) do the same for the 2013 and 2015 versions. Both confirm that bank-firm relationships are sticky. This seems also to be the case in the US as discussed in Chodorow-Reich (2014). Other recent studies that use ORBIS and rely on the assumption of sticky relationships are Storz et al. (2017), Andrews and Petroulakis (2019) and Duval et al. (2019).

[19]Only 1.69% of firms have a foreign bank as their main relationship bank.

[20]Financial debt includes, e.g., loans and bonds but excludes non-financial debt like deferred tax liabilities.

To capture changes in a firm's profitability, we calculate the return on assets by relating net income to total assets. Each firm-level explanatory variable is winsorised at the 1st and 99th percentile.[21]

We use the bank stress measure proposed in Storz et al. (2017) to gauge bank health based on balance sheet data from Bankscope. We use principal component analysis to construct an indicator consisting of bank characteristics commonly associated with bank stress. The indicator is defined as the first principal component of a bank's capitalisation, NPL ratio, return on assets, z-score, and liquidity.[22] Capitalisation is measured as bank equity over total assets. The NPL ratio is defined as nonperforming loans over total loans. Return on assets is defined analogously to our firm control variable. Bank z-score is measured as equity and net income over the standard deviation of the return on assets.[23] Bank liquidity is defined as the difference between liquid assets and runnable short-term liabilities normalised by total assets, where runnable short-term liabilities are the sum of deposits and short-term funding.[24]

The advantage of our bank stress measure relative to market-based measures, such as CDS spreads, is that it is available also for small and non-listed banks. For the small subsample of banks in the ORBIS firm-bank link database for which CDS data is available, Storz et al. (2017) compare their bank stress measure with CDS spreads and find that the correlation between both variables is 68%. Principal component analysis has by now become a standard tool in economics and finance.[25] Other recent papers using principal component analysis to gauge bank stress are Chodorow-Reich and Falato (2018) for US banks and Andrews and Petroulakis (2019) for European banks. While Andrews and Petroulakis (2019) use a slightly different set of bank variables, the variation of their measure across countries and over time is similar to the variation displayed in our Table A.3.

We dichotomise our continuous bank stress measure by defining a weak bank indicator equal to one if bank stress is higher than the 75h percentile over all observations within the regression

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[21] Note that we do not winsorise the dependent variable since we do not want to treat firms with very few or very many employees as outliers.

[22] The bank stress indicator loads positively on the NPL ratio with an eigenvector of 0.26 and negatively on capitalisation (-0.66), return on assets (-0.66), z-score (-0.02) and liquidity (-0.24).

[23] According to Laeven and Levine (2009), a bank's z-score reflects the inverse of the probability of insolvency.

[24] Our measure is related to the concept of illiquidity risk described in Morris and Shin (2016) and the liquidity coverage ratio defined in the Basel III framework. Note that we define liquid assets as the sum of securities, derivatives and loans and advances to banks.

[25] See Aït-Sahalia and Xiu (2018) for a discussion of principal component analysis and an overview over different applications in the economic literature.

sample.[26] In all regressions we also control for bank size, which we define as the natural logarithm of inflation-adjusted total assets.

Our labour market reform variables are modelled after the recursive reform indicators from Simintzi et al. (2015). For each reform type we use a recursive reform indicator that starts out with zero at the beginning of the sample and increases by one for a liberalising reform while decreasing by one for a tightening reform. Thus, a liberalising reform shifts the reform indicator upwards permanently until the end of the sample period (unless there is a counter-reform or a second reform). We try to capture the long-term effect of reforms on firm-level employment.[27] Tables A.4, A.5, and A.6 show the evolution of each of the three reform indicators over time.

Whenever our fixed effects structure does not absorb variation at the country-sector-year level, we also control for GDP growth and sector growth which are calculated based on AMECO and Eurostat data, respectively.

A detailed description of all variables can be found in Table A.1.

4 Empirical approach

We estimate the following cross-country panel regression using data at the firm-year level:

$$
\begin{aligned}
ln(employees)_{jsct} = \ & \beta_1 WeakBank_{jsct-1} \\
& + \beta_2 Regular_{ct-2} + \beta_3 Regular_{ct-2} * WeakBank_{jsct-1} \\
& + \beta_4 Unempl_{ct-2} + \beta_5 Unempl_{ct-2} * WeakBank_{jsct-1} \\
& + \beta_6 Tempor_{ct-2} + \beta_7 Tempor_{ct-2} * WeakBank_{jsct-1} \\
& + ReformInteractions \\
& + \beta Controls + FEs + \varepsilon_{jsct},
\end{aligned}
\tag{2}
$$

where j denotes firms, s denotes sectors, c denotes countries and t denotes years. $Regular$ denotes reforms of employment protection for regular workers, $Unempl$ denotes reforms of unemployment benefit schemes, $Tempor$ denotes reforms of the regulation of temporary employment, and $WeakBank$ denotes our binary bank stress indicator. The vector of control variables contains our firm-level covariates, bank size, sector growth and GDP growth. Moreover, we include all possible double interactions and the triple interaction between all three reform types. We

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[26] Thus, the weak bank indicator takes into account the entire variation across firms and over time to determine whether a given firm-year observation is characterised by easy or tight access to credit.

[27] There are two main reasons why we do not look at short-run effects. Firstly, our data is at a low, annual, frequency which reduces our power to find short-run effects. Secondly, in the short-run employment deregulations are likely to increase layoffs and reductions in unemployment benefit schemes can lead to negative short-run aggregate demand effects. These two negative short-run mechanisms do not have clear link to bank financing conditions, as opposed to the long-run mechanism increasing employment, which we have described in Section 2.

cluster standard errors at the firm-level.[28] We lag the reform indicators by two years to allow for an impact lag (e.g. due to a recognition lag on the side of entrepreneurs), while all other variables are lagged by one year. This means that we assume that changes in the health of a firm's main bank have a more immediate impact than legislative changes at the country-level.[29] Our main coefficients of interest are β_3, β_5, and β_7 and we expect them to have negative values reflecting the negative interaction between bank weakness and reforms. If the data did not support our hypothesis, this would suggest that European firms can easily finance new workers out of internal internal cash-flows or non-bank sources of external finance. It would also suggest that capital and labour are relatively substitutable so that firms do not need to purchase costly new machines to increase employment in response to reforms. One could even imagine a positive interaction effect if firms that are both operating with high-substitutability technologies and are connected to weak banks use labour market reforms as an opportunity to reduce their dependence on their relationship bank by making production less capital intensive.[30]

We estimate equation (2) with two different levels of fixed effects. In the less restrictive case, we use sector-year fixed effects and firm fixed effects. Sector-year fixed effects control for time-varying industry trends, such as the growing importance of the ICT sector. Firm-fixed effects control for time-invariant differences among the firms in our sample, including time-invariant differences at the country-level, such as different legal systems. Moreover, since in our setting each firm is connected to exactly one bank, the firm fixed effects encompass bank fixed effects. The interaction terms between each reform type and the weak bank indicator capture the difference across firms within the treatment group along our weak vs. strong bank dichotomy: In other words, we compare firms that were connected to weak banks after the implementation of a reform with firms that were connected to strong banks at the same point in time.

We can sharpen our analysis by substituting our sector-year fixed effects with country-sector-year fixed effects. While this does no longer allow us to identify the effect of reforms per se, we are able to compare treated firms connected to either weak or strong banks within the same country, industry and year. This way we can also control for very specific but relevant confounding factors like country-sector-year specific wage bargaining agreements.

We do not necessarily claim that the empirical relationships we uncover are causal. The difference in reform outcomes between firms connected to weak and firms connected to strong

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[28]In a robustness check in Section C, we verify that our main result is robust to clustering at the country-level.
[29]We verify that our main result is robust to lagging also reforms by one year only.
[30]We specifically analyse the role of the elasticity of substitution in Section 6.2.

banks is policy-relevant even if it merely constitutes a conditional correlation. The following two assumptions are helpful for reducing the potential for spurious correlations.

Firstly, we assume that labour market reforms are orthogonal to firm-level decisions. The biggest threat to our identification would come from firm- or sector-level lobbying increasing the probability of reforms. However, there is no empirical or theoretical support for the lobbying explanation of reforms. Instead, both theory and empirics point to a host of country-level determinants that affect the probability of the implementation of labour market reforms. The theoretical literature points to recessions (Fernandez and Rodrik, 1991), a country's degree of unionisation (Saint-Paul, 2002), and voting systems (Pagano and Volpin, 2005) as the most important determinants of labour market regulation. On the empirical side, Simintzi et al. (2015) find that the degree of employment protection is linked to a country's degree of unionisation and income equality. According to Botero et al. (2004), a country's legal origin is the most important determinant of the stringency of employment protection. Duval et al. (2018a) and Dias da Silva et al. (2017) look at structural reforms in general and identify crises, high unemployment rates and political factors like EU directives or the EU accession process as the most important country-level determinants. We automatically control for all of these time-varying and time-invariant country-level factors in our most restrictive specification featuring firm- and country-sector-year fixed effects.

Our second identification assumption is that bank financing is orthogonal to both reforms and firm-level employment. It is unlikely that variations in the health of a single bank drive a reform at the country-level. A more serious threat would be reverse causality emanating from large firms that are able to drag down their creditors as their own health worsens. We address this concern by excluding large firms from our sample. Moreover, it is possible that only a certain type of firm matches with weak banks. Then it might be that the firm-level employment patterns we observe are driven by some unobservable factor driving both bank health and firm-level employment. We address the issue of endogenous bank-firm matching by excluding a group of firms likely to have matched with weak banks purposefully.

Note that the positive credit supply effects found by Alimov (2015) and Moro et al. (2017) do not threaten our identification. According to the these studies, labour market deregulation reduces banks' perception of borrower credit risk because it increases firms' flexibility to react to financial distress. As long as weak banks do not perceive the change in borrower risk differently from strong banks, this positive credit supply effect of labour market reforms will bias our

analysis against finding a negative differential effect of bank distress on employment gains at the firm-level.

In Table 2, we show summary statistics for the firm-year observations in our regression sample.[31] We show descriptive statistics for firm-level variables (Panel A), variables describing the balance sheet of each firm's main bank (Panel B), country-level variables (Panel C) and sector-level variables (Panel D). In Appendix Section B, we discuss observable differences between firms connected to strong banks and firms connected to weak banks. Table 3 shows the distribution of banks and firms by country within our final regression sample. We refer the reader to Appendix Section C for robustness checks addressing concerns about coverage heterogeneity.

5 Main results

5.1 Baseline results

In Table 4, we show the results of our baseline regression based on equation (2). Column 1 shows the results for the specification using the less restrictive set of fixed effects, which allows us to gauge the effect of labour reforms per se. Here we capture variation in employment within firms and across countries. A firm located in a country implementing a reform will belong to the treatment group while a firm located in a country that is not implementing a reform will belong to the control group. The results in column 1 suggest that labour market reforms have a positive long-term effect on firm-level employment. Recall, again, that we are using unweighted regressions focusing on the employment response of the average firm. It might well be that the aggregate employment impact of the reforms we study is significantly different from the effects we are measuring.

Among the three reform types, unemployment benefit schemes has the most positive effect amounting to a permanent shift of 8.5% in employment for firms connected to strong banks.[32] The same effect is only 4.8% for firms connected to weak banks after the implementation of

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[31]Note that due to the lag structure of equation (2), our regression sample effectively uses reform data from 1999-2013, bank-and firm explanatory variables from 2000 to 2013 and firm-level employee data from 2001 to 2014.

[32]This is calculated based on the $(exp(\beta) - 1) * 100$ formula needed in log-level specifications. Also note that, strictly speaking, this number only describes the effect of implementing an unemployment benefit reform when a country has not yet implemented any other reform type, i.e. at a point in time where all three reform indicators are still equal to zero. Furthermore, note that our results only refer to the intensive margin since we are capturing within firm variation and because newly created firms might not immediately start reporting balance sheet information to chambers of commerce or other data providers.

the reform.[33] This confirms our hypothesis of a negative interaction between bank weakness and labour market reforms. The reforms induce a shift in firm-level employment but the size of this shift is moderated by bank financing conditions. If firms do not have sufficient access to financing in the aftermath of the reform, some part of the potential employment boost is permanently lost.

We also find a positive effect of regular employment reforms on firm-level employment and a negative interaction between regular employment reforms and bank weakness. However, the coefficients are much smaller in magnitude than the coefficients for the case of unemployment benefit schemes. The fact that our results are less strong for regular employment reforms can be explained by the potentially very negative short-term effects discussed in Section 2.1. Even though we are capturing long-term shifts in employment, a very large negative short-term effect will reduce the size of the overall estimate.

We find a positive interaction effect between temporary employment reforms and bank weakness. This result could be driven by country-specific peculiarities in the way labour markets react to bank stress. Caggese and Cuñat (2008) analyze the dualism of Italy's labour market where fixed-term employees absorb most of the firm-level employment volatility. They find a substitutability between access to credit and fixed-term employment. Firms with insufficient access to credit try to regain flexibility by hiring mostly on a temporary basis. It might be that firms in the European periphery, where dual labour markets are common, use temporary employment reforms as an opportunity to further intensify this strategy.[34]

In column 2, we compare treated firms connected to strong banks with treated firms connected to weak banks within the same country-sector-year cell. The advantage of this framework is that we are able to control for a range of confounding factors at the country-sector-year level. For example, the effect of country-specific business cycle conditions, which might drive both firm-level employment and the decision to implement reforms, and events like a country- and sector-specific wage-bargaining agreement are controlled for. The significantly negative interaction between unemployment benefit reforms and bank weakness is preserved and only slightly

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[33]This is calculated using $(exp(\beta_3 + \beta_4) - 1) * 100$. Note that we dichotomise the weak bank indicator based on the 75h percentile of bank stress in the regression sample containing the 2,075,151 observations from column 1 and do not re-define it for subsequent regressions.

[34]Recall, however, that our estimates concerning temporary employment reforms are subject to some measurement error induced by country-specific counting rules for temporary agency workers. This measurement error will be especially large in the specification of column 1, where the less restrictive fixed effects structure allows for cross-country variation.

decreases in magnitude. The coefficient on the interaction between temporary employment reforms and bank weakness becomes insignificant and even turns around in terms of sign. The country-sector-year fixed effects have absorbed the cross-country differences in labour market dualism which were likely driving the positive interaction in column (2). The negative interaction between regular employment reforms and bank weakness is preserved but loses statistical significance.

5.2 Channels

To explore the channels behind our main result, we use a triple difference-in-differences framework. We build on the specification with firm- and country-sector-year fixed effects and introduce a third factor, denoted as η, that arguably influences the link between the success of labour market reforms and bank financing conditions. We want to explore whether we are indeed identifying the mechanism we have in mind: A bank-dependent firm will benefit less from labour market deregulation if it cannot get a loan to fund the hiring of new employees and the machines needed to provide a productive work environment. We look into three different measures of η: i) a measure of sector-specific external financial dependence, ii) a measure of the durability of the goods produced by each sector, and iii) an binary indicator for negative country-level GDP growth.

In column 1 of Table 5, we let η denote each sector's reliability on external financial dependence, which is calculated in a study by Duygan-Bump et al. (2015). The authors construct a sector-specific measure of external financial dependence by calculating the percentage of capital expenditure financed with external funds. Their measure reflects technological reasons that constrain a firm financially. For example, certain industries, such as pipelines or metal mining, require higher fixed costs and feature longer time periods until a given investment pays off relative to less constrained sectors like the tobacco industry. The identification approach in this triple interaction regression is inspired by the argumentation of Rajan and Zingales (1998). Since the external financial dependence measure is constructed based on US data, we get a source of variation in the tightness of the link between the health of a firm's main bank and firm-level employment that is orthogonal to the variation within our dataset.[35]

We find a significantly negative coefficient on the triple interaction between unemployment

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[35]Moreover, Rajan and Zingales (1998) argue that the US is the perfect economy to obtain an estimate of external financial dependence that is capturing actual variation in sector-level technologies rather than country-specific variation since it is the economy with the most developed financial system.

benefit reforms, bank weakness and the external financial dependence of a firm's sector-level technology. This implies that the negative interaction between bank weakness and labour market reforms is especially high for firms in sectors using technology characterised by high external financial dependence. In contrast, firms operating in sectors with low fixed costs and abundant cash flows will not be as affected if their main bank becomes troubled.

Another factor relevant for a firm's financial vulnerability is its products durability. In recessions, consumers cut spending more quickly on durable goods than on non-durable goods. Thus, durable-good producers are more financially vulnerable in recessions and less able to make up reduced access to credit from their main bank by using own funds. In column 2 of Table 5, η captures the durability of each sector's output taken from Bils et al. (2013). We find that our negative interaction between reforms and bank weakness is especially high for firms operating in high-durability sectors. This is in line with economic intuition because durable goods producers are especially vulnerable in business cycle downturns. However, the triple interaction using the measure of durability is less sizable and significant than the triple interaction which uses our measure of external financial dependence.[36]

In line with the previous result, column 3 shows that the negative interaction between unemployment benefit reforms and bank weakness is especially strong during recessions. Here, η is a time-varying indicator that is equal to one if a country's GDP growth turns negative. The coefficient on the constituent term for the interaction of bank financing and recessions is significantly negative. Being connected to a weak bank has a particularly detrimental effect on employment during recessions when firms themselves have weaker balance sheets.[37] The constituent term on unemployment benefit reforms and bank weakness shows that the interaction between bank financing and labour market reforms is also significant outside of recessions. Crises exacerbate but do not fully drive the link between reform success and bank financing conditions.

According to theoretical work by Buera et al. (2015), another important factor that tightens the link between bank financing and firm employment is a firm's size, since small firms are usually more reliant on credit to finance their production. When examining the role of firm size, we refrain from using a triple interaction model since our results would be entirely driven by the few firms that switch the size class during the sample period. Instead, we run subsample

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[36]Note that Bils et al. (2013) are able to calculate their durability measure only for a subset of sectors, which reduces the sample size and the power of our model.

[37]See the discussion in Giroud and Mueller (2017) on how weak household-, firm- and bank balance sheets reinforced each other exacerbating the impact of the Great Recession in the US.

regressions and compare the coefficients to the ones in our preferred specification. We use the European Commission's definition of micro, small, medium, and large firms described in European Commission (2015) and run our baseline regression in the subsample of micro and small firms only. The results in column 2 of Table 6 suggest that the link between bank financing conditions and reforms indeed gets stronger for smaller firms, which is in line with economic intuition.[38] The results in column 3 suggest that our baseline effect is not present among medium and large firms.[39] Smaller firms are more dependent on bank financing because they do not have easy access to alternative sources of funding. Thus, they will be especially inhibited in their ability to benefit from labour market reforms if their main bank is not in good health.

6 Robustness checks

6.1 Endogenous bank firm matching and reverse causality

Table 7 deals with the concern that our results might driven by endogenous bank-firm matching. Ultimately, we cannot fully rule out that the ex-ante process of relationship building between banks and firms is driving our results without actually observing this process. However, we can use the variation at hand to identify a subset of firms for which endogenous bank-firm matching is especially relevant. Specifically, we exclude firms that are already connected to a weak bank in the year they enter the sample. These could be firms that have picked relatively weak banks on purpose because they were enticed by better loan conditions or were unable to build a relationship with stronger banks. By excluding this set of firms, we are only left with variation that comes from firms starting out with a link to a bank in good health. Then a firm's relationship bank can either remain in good health throughout the sample period or become stressed (i.e. become a weak bank) over time.

In this specification, our main result of the negative interaction between bank weakness and unemployment benefit reforms remains intact. Even the negative coefficients on the interaction terms for temporary and regular employment reforms turn significant. Thus, the exclusion of firms for which endogenous bank-firm matching is especially relevant has actually increased the power of our model. This suggests that, if anything, endogenous bank-firm matching will make us

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[38]Note that in this specification also the interaction between temporary employment reforms and bank financing is significantly negative, albeit only on a 10%-level.

[39]This could also be due to smaller sample size which is why we prefer to compare the coefficient for micro and small firms in column 2 with the coefficient from the full sample in column 1.

underestimate the negative interaction effect between bank weakness and labour market reforms. According to Schwert (2018), bank-dependent firms are more likely to match with healthy banks while less dependent firms purposefully match with banks that are less well capitalised. This would explain why excluding firms more likely to match with weak banks actually increases our ability to find a negative interaction between labour market reforms and bank weakness.

Another concern is that banks might become stressed due to the deteriorating health of their borrowers. This argument is most relevant for large firms that are able to drag down their creditors once they become distressed. This could happen for example by outright default or by missed interest payments by a firm whose size makes it a significant contributor to its main bank's overall loan portfolio. The regression in column 2 of Table 6 already addresses this concern by estimating our model only in the subsample of micro and small firms, which automatically excludes firms that do not belong to the group of small and medium enterprises (SMEs).[40]

6.2 Elasticity of substitution between labour and capital

Our analysis in Section 2 relies on the assumption that labour and capital are to some extent complementary. In the few sectors where capital and labour are substitutes, i.e. where the elasticity of substitution is above one, the interaction between bank financing conditions and labour market reforms is driven by two countervailing forces. On the one hand, the need for working capital financing still generates a negative interaction effect between bank weakness and employment gains induced by labour market reforms. On the other hand, a firm operating with a technology characterised by substitutability could respond to the reform by increasing its use of labour at the expense of capital. If such a firm is also connected to a weak bank it might be especially prone to sell some of its capital stock to finance new employees and to make its production process less dependent on external financing by its weak relationship bank. Depending on which of the the countervailing forces is stronger, the overall interaction effect between bank financing and labour market reforms might even be positive.[41]

We want to investigate the role of the elasticity of substitution by splitting our sample

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[40]In an unreported regression we only exclude large firms, rather than excluding medium and large firms. Thus we are left with a sample of only SME-firms. The results are very similar and available upon request.

[41]Laeven et al. (2018) look at a financial shock instead of a labour market shock but their study suggests a mechanism similar to the one described above. They find that Spanish firms that are both subject to an elasticity of substitution greater than one and face lower levels of financial frictions because they fall below a size threshold react to the financial crisis by increasing employment.

above and below the economically relevant threshold of an elasticity of one. Unfortunately, the estimation of sector-specific elasticities of substitution relies on a myriad of assumptions and is therefore highly method dependent. As a case in point, the construction sector has the lowest estimated elasticity of substitution among all sectors in the study by Ciminelli et al. (2018a), whereas it has the highest elasticity in the study by Laeven et al. (2018). We opt to use both measures in two separate sample splits while excluding the construction sector to make both sets of estimates more comparable.

In Table 8, we compare firms in sectors with an elasticity of substitution below and above one. In columns 1 and 2, we use the estimates by Ciminelli et al. (2018a), while we use the estimates by Laeven et al. (2018) in columns 3 and 4. Columns 1 and 3 show our baseline results for a sample of sectors with an elasticity below one. Here the theoretical prediction is unambiguous and in fact our baseline results remains intact. In columns 2 and 4, we look at firms operating in sectors with an elasticity above one. We do not find any evidence for a positive interaction effect between bank financing and labour market reforms.[42] Notably, in column 2 we are able to find a negative interaction effect between regular employment reforms and bank weakness which is not statistically significant in the corresponding regression for firms operating with complementarity in column 1. This might be driven by different magnitudes of the short-term effect of regular employment reforms. As we discussed in Section 2, laying off employees can be implemented faster than searching for and hiring new ones. This short-term negative effect might be especially strong for firms that are operating with substitutability and are connected to weak banks. The labour market reforms might allow them to lay off employees without fearing unduly disruption in their production. In contrast, firms with technologies characterised by complementarity might be more cautious in implementing layoffs, given the risk that machines might become idle and cannot be put to good use without adequate numbers of employees.

In section C in the appendix, we show further robustness checks in order to verify that our results are neither driven by specific subsamples nor overly sensitive to specification choices. We show that our results are robust to excluding certain countries, different firm-age subsamples, controlling for the weighted average cost of capital (WACC), excluding all control variables, clustering on the country level, lagging reforms only by one year, using continuous or three-category measures of bank stress, and excluding firms connected to more than one bank.

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[42]Our inability to find a positive effect could, of course, also be due to the severely diminished sample size in these two subsample regressions.

7 Conclusion

Economic theory suggests that labour market deregulation can increase employment by decreasing the user cost of labour and thus encouraging firms to implement new projects. However, entrepreneurs can only take advantage of labour market deregulation if they can get sufficient financing to be able to pay the new employees and purchase the new machines required for the marginal project. We find that bank financing is an important moderator of the impact of labour market deregulation on firm-level employment. We look at reforms of employment protection for regular workers, unemployment benefit reforms and reforms of the regulation of temporary employment. The negative interaction effect between labour market reforms and bank weakness is most robust in the case of unemployment benefit reforms. Specifically, we find that firms that were connected to a weak bank after the implementation of an unemployment benefit reform increase employment by only half as much as similar firms connected to stronger banks.

Moreover, we find that the negative effect of weak banks on firm-level employment gains after reforms is especially strong for firms operating in sectors with a high dependence on external financing, for durable goods producers, for small firms, and during recessions.

In a series of robustness checks, we address concerns about endogenous bank-firm matching, and reverse causality. Moreover, we verify that our results are robust to different subsamples and do not depend on specification choices.

Our results highlight the importance of policies complementing labour market reforms with a comprehensive strengthening of bank balance sheets. Ensuring that banks are adequately capitalised, unburdened by high levels of non-performing loans, and sufficiently liquid will help entrepreneurs to realise the full potential of employment gains after the implementation of labour market reforms. Our findings are particularly relevant for countries in the euro area periphery where high levels of labour market regulation go together with bank weaknesses.

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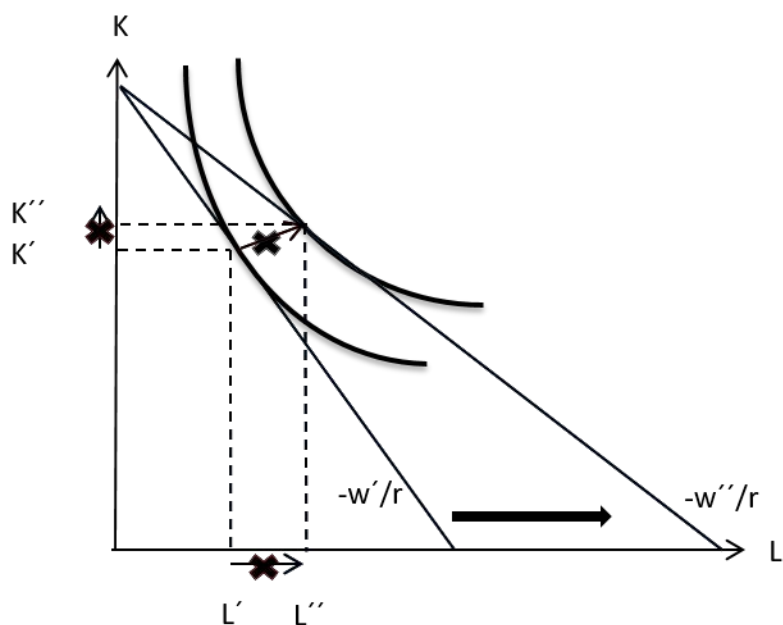
Figure 1: CES production function with intermediate σ
This figure visualises a CES production function of the form $Y = F(K, AL) = (\alpha(K)^\varepsilon + (1-\alpha)(AL)^\varepsilon)^{1/\varepsilon}$. The elasticity of substituion, σ, is assumed to take on an intermediate value between zero and one. The labour reform shock lowers the user cost of labour shifting the isocost curve outwards. Financing, visualised with black crosses prevents the firm from increasing K and L as much as it would desire thereby inhibiting the jump to a higher profit curve.

Table 1: Labour market reforms across countries

This table shows the incidence of reforms concerning employment protection of regular workers, the replacement rate and duration of unemployment benefit schemes and reforms of the regulation of temporary employment within our sample. The incidence numbers relate the years in which reforms were enacted to the total number of country-years over 1999-2013. The total number of country-years excludes Luxembourg due to its small size and the reform incidence excludes an unemployment benefit reform enacted in Portugal in 1999 because this country-year is not in our sample. Please see Appendix Tables A.4, A.5, and A.6 for the evolution of the recursive reform indicator built by using the implementation dates of the reforms below.

Country	Regular	Unempl.	Tempor.
Austria	1	0	0
Germany	1	1	3
Spain	3	0	3
France	2	0	0
Greece	2	0	2
Ireland	2	1	1
Luxembourg	0	0	0
Netherlands	0	1	1
Portugal	2	1	3
Total	13	4	13
Incidence	11%	3%	11%

Table 2: Summary statistics

This table shows summary statistics for the observations in our regression sample. Panel A reports summary statistics for our dependent variable, the log of employment and several firm-level control variables. Panel B reports summary statistics on the variables describing the health of each firm's main bank. Panel C reports summary statistics on our country-level control variables. Panel D reports summary statistics for several time-invariant sector-level measures that are meant to characterize the technology used in each sector. Refer to Appendix Table A.1 for variable definitions.

Panel A: Firm-level variables						
	N	Mean	SD	P25	Median	P75
Ln(Employment)	2,075,151	2.277	1.392	1.386	2.197	3.091
Leverage	2,075,151	0.190	0.224	0.003	0.106	0.303
Cash	2,075,151	0.142	0.174	0.019	0.072	0.201
Tangibility	2,075,151	0.214	0.221	0.042	0.136	0.322
Return on assets	2,075,151	0.019	0.123	0.000	0.019	0.062
WACC	1,747,585	0.015	0.016	0.003	0.009	0.021
Operating rev.	2,050,924	1.594	1.329	0.732	1.317	2.078
Interest	1,792,566	0.015	0.017	0.003	0.010	0.022
Savings	1,591,302	0.067	0.107	0.016	0.053	0.109

Panel B: Firm-bank-level variables						
Bank size	2,075,151	11.402	1.591	10.629	11.525	12.858
Bank stress	2,075,151	-0.028	0.309	-0.200	-0.056	0.106
Weak bank (bank stress >= p75)	2,075,151	0.250	0.433	0.000	0.000	1.000

Panel C: Country-level variables						
Real GDP growth	2,075,151	-0.001	0.025	-0.018	0.002	0.019
Sector growth	2,075,151	-0.006	0.074	-0.033	0.001	0.027
Recession	2,075,151	0.372	0.483	0.000	0.000	1.000

Panel D: Sector-level variables						
EFD	1,953,760	0.184	0.283	-0.050	0.160	0.380
Elasticity of substitution (Ciminelli)	2,075,151	0.535	0.257	0.390	0.470	0.530
Elasticity of substitution (Laeven)	2,054,937	0.979	0.475	0.720	0.780	0.920
Durability	1,841,488	1.391	1.364	0.100	0.960	2.270

Table 3: Firms and banks by countries

This table shows the distribution of firms and banks within the regression sample across countries. Note that banks can be present in multiple countries.

Country	Firms	Banks
AT	3,105	21
DE	10,631	791
ES	151,844	86
FR	115,512	159
GR	16,996	17
IE	4,572	23
LU	311	5
NL	1,412	16
PT	83,539	69

Table 4: Labour market reforms, weak banks and employment

This table reports estimates of equation (2). The sample covers the period 2001-2014 and has a firm-year structure. Note that each firm is connected to one main bank so that firm fixed effects implicitly encompass bank fixed effects. In addition, firm fixed effects also encompass country fixed effects. The dependent variable is the log of employment. The explanatory variables include firm-level balance sheet information, time-varying information on the balance sheet of each firm's main bank, country-level time-varying information and the three recursive reform indicators. All independent variables are lagged by one year except for the reform indicators which are lagged by two years. Standard errors are clustered as indicated below. t-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Ln(employment)	
	(1)	(2)
Regular empl. reform	0.004***	
	(2.71)	
Regular empl. ref. × weak bank	-0.012***	-0.001
	(-10.92)	(-0.58)
Unemployment benefit reform	0.082**	
	(2.36)	
Unempl. benefit ref. × weak bank	-0.035***	-0.023***
	(-5.80)	(-3.92)
Temporary empl. reform	0.007**	
	(2.44)	
Temporary empl. ref. × weak bank	0.049***	-0.002
	(18.93)	(-1.06)
Regular empl. ref. × temp. ref.	-0.023***	
	(-15.97)	
Regular empl. ref. × unemployment ref.	-0.028**	
	(-2.39)	
Temporary empl. ref. × unemployment ref.	-0.070**	
	(-2.40)	
Reg. × temp. × unempl.	0.025	
	(1.23)	
Weak bank	-0.011***	-0.001
	(-6.72)	(-0.62)
Sector growth	0.033***	
	(5.03)	
Real GDP growth	0.837***	
	(18.26)	
Leverage	-0.017***	-0.013***
	(-4.30)	(-3.24)
Cash	-0.034***	-0.047***
	(-6.90)	(-9.79)
Tangibility	0.061***	0.061***
	(8.58)	(8.62)
Return on assets	0.228***	0.218***
	(50.92)	(49.28)
Bank size	0.004***	-0.004***
	(3.83)	(-3.51)
Firm FE	Yes	Yes
Sector-year FE	Yes	No
Country-sector-year FE	No	Yes
Observations	2,075,151	2,070,262
Adjusted R^2	0.934	0.935
Number of firms	387,922	387,113
Number of banks	1,121	1,116
Mean dep. var.	2.277	2.272
Clustering	Firm	Firm
Sample period	2001-2014	2001-2014

Table 5: Channels: External financial dependence, durability, and recessions

This table reports estimates from a triple interaction framework complementing the double interaction model from equation (2). The sample covers the period 2001-2014 and has a firm-year structure. The dependent variable is the log of employment. The explanatory variables include firm-level balance sheet information, time-varying information on the balance sheet of each firm's main bank, time-invariant sector-level measures and the three recursive reform indicators. In column 1, η relates to external financial dependence and in column 2 it relates to the durability of goods produced, both at a time-invariant sector level. In column 3, η reflects a binary country-year indicator equal to one in years of negative GDP growth. All independent variables are lagged by one year except for the reform indicators which are lagged by two years. Standard errors are clustered as indicated below. t-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Ln(employment)		
	(1)	(2)	(3)
Regular empl. ref. × weak bank	-0.002	-0.002	-0.001
	(-1.06)	(-1.40)	(-0.74)
Regular empl. ref. × weak bank × η	0.004	0.001	0.001
	(0.96)	(1.45)	(0.29)
Unempl. benefit ref. × weak bank	-0.013*	-0.010	-0.026***
	(-1.87)	(-1.12)	(-3.16)
Unempl. benefit ref. × weak bank × η	-0.053**	-0.009*	-0.427*
	(-2.50)	(-1.70)	(-1.65)
Temporary empl. ref. × weak bank	-0.002	-0.001	0.001
	(-0.78)	(-0.46)	(0.15)
Temporary empl. ref. × weak bank × η	0.001	-0.000	-0.001
	(0.11)	(-0.13)	(-0.08)
Weak bank× η	-0.005	-0.001	-0.005**
	(-0.83)	(-0.93)	(-2.18)
Weak bank	-0.001	-0.000	-0.000
	(-0.49)	(-0.04)	(-0.01)
Firm controls	Yes	Yes	Yes
Bank size	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Sector-year FE	No	No	No
Country-sector-year FE	Yes	Yes	Yes
Observations	1,870,553	1,764,339	2,070,262
Adjusted R^2	0.939	0.939	0.935
Number of firms	354,845	335,332	387,113
Number of banks	1,106	1,099	1,116
Mean dep. var.	2.246	2.221	2.272
Clustering	Firm	Firm	Firm
Sample period	2001-2014	2001-2014	2001-2014

Table 6: Small and micro firms

This table reports estimates of equation (2). The sample covers the period 2001-2014 and has a firm-year structure. The explanatory variables include firm-level balance sheet information, time-varying information on the balance sheet of each firm's main bank and the three recursive reform indicators. In column 1, we re-state the results from our preferred specification. In column 2, we only include micro firms and small firms according to the definition of the European Commission (European Commission, 2015). In column 3, we only include medium-sized and large firms. All independent variables are lagged by one year except for the reform indicators which are lagged by two years. Standard errors are clustered as indicated below. t-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Ln(employment)		
	(1)	(2)	(3)
	Baseline	Small and micro only	Medium and large only
Regular empl. ref. × weak bank	-0.001	0.001	-0.001
	(-0.58)	(1.18)	(-0.34)
Unempl. benefit ref. × weak bank	-0.023***	-0.025***	0.001
	(-3.92)	(-4.29)	(0.05)
Temporary empl. ref. × weak bank	-0.002	-0.004*	0.000
	(-1.06)	(-1.84)	(0.03)
Weak bank	-0.001	-0.002	-0.002
	(-0.62)	(-1.15)	(-0.53)
Firm controls	Yes	Yes	Yes
Bank size	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Sector-year FE	No	No	No
Country-sector-year FE	Yes	Yes	Yes
Observations	2,070,262	1,793,468	258,586
Adjusted R^2	0.935	0.898	0.928
Number of firms	387,113	344,437	51,571
Number of banks	1,116	1,021	814
Mean dep. var.	2.272	1.912	4.675
Clustering	Firm	Firm	Firm
Sample period	2001-2014	2001-2014	2001-2014

Table 7: Robustness: Firms that are less likely to match with weak banks purposefully

This table reports estimates of equation (2). The sample covers the period 2001-2014 and has a firm-year structure. The dependent variable is the log of employment. The explanatory variables include firm-level balance sheet information, time-varying information on the balance sheet of each firm's main bank and the three recursive reform indicators. We exclude firms that are already connected to a weak bank in the moment they enter the sample. All independent variables are lagged by one year except for the reform indicators which are lagged by two years. Standard errors are clustered as indicated below. t-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Ln(employment)
	(1)
	Excl. firms with weak bank in t_0
Regular empl. ref. $\times$ weak bank	-0.003*
	(-1.86)
Unempl. benefit ref. $\times$ weak bank	-0.021***
	(-3.62)
Temporary empl. ref. $\times$ weak bank	-0.005**
	(-2.14)
Weak bank	0.002
	(0.95)
Firm controls	Yes
Bank size	Yes
Firm FE	Yes
Sector-year FE	No
Country-sector-year FE	Yes
Observations	1,810,791
Adjusted R^2	0.935
Number of firms	339,273
Number of banks	1,055
Mean dep. var.	2.298
Clustering	Firm
Sample period	2001-2014

Table 8: Robustness: Sectors where labour and capital are substitutes

This table reports estimates of equation (2). The sample covers the period 2001-2014 and has a firm-year structure. The explanatory variables include firm-level balance sheet information, time-varying information on the balance sheet of each firm's main bank, time-invariant sector-level measures and the three recursive reform indicators. In columns 1 and 2, EOS relates to the elasticity of substitution between labour and capital estimated by Ciminelli et al. (2018a). In columns 3 and 4 we use the alternative estimates by Laeven et al. (2018). Columns 1 and 3 exclude and columns 2 and 4 only include firms from sectors with an EOS above one. All independent variables are lagged by one year except for the reform indicators which are lagged by two years. Standard errors are clustered as indicated below. t-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Ln(employment)			
	(1)	(2)	(3)	(4)
	$EOS_{Cim} < 1$	$EOS_{Cim} \geq 1$	$EOS_{Laev} < 1$	$EOS_{Laev} \geq 1$
Regular empl. ref. × weak bank	-0.001	-0.009*	-0.002	0.000
	(-0.68)	(-1.87)	(-1.38)	(0.03)
Unempl. benefit ref. × weak bank	-0.016***	-0.033	-0.016***	-0.028
	(-2.58)	(-1.49)	(-2.65)	(-1.24)
Temporary empl. ref. × weak bank	-0.003	0.004	-0.002	-0.011
	(-1.31)	(0.59)	(-0.64)	(-1.61)
Weak bank	0.001	-0.002	0.001	-0.003
	(0.26)	(-0.22)	(0.24)	(-0.49)
Firm controls	Yes	Yes	Yes	Yes
Bank size	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Sector-year FE	No	No	No	No
Country-sector-year FE	Yes	Yes	Yes	Yes
Observations	1,590,016	172,217	1,621,997	140,236
Adjusted R^2	0.940	0.929	0.938	0.942
Number of firms	292,430	33,738	300,502	25,666
Number of banks	1,034	671	1,059	585
Mean dep. var.	2.322	2.092	2.260	2.757
Clustering	Firm	Firm	Firm	Firm
Sample period	2001-2014	2001-2014	2001-2014	2001-2014

Appendix for

"Firm-Level Employment, Labour Market Reforms, and Bank Distress"

A   Firm sample composition

We exclude firms operating in special sectors, such as agriculture and mining, or firms from the financial sector, given their specific leverage characteristics. Thus, we focus on eleven NACE sections (sections C-N, excluding K) with most firms operating in manufacturing, wholesale and retail trade, real estate and construction services, as well as food and accommodation services. The advantage of the ORBIS database relative to databases containing only public companies is that it also includes SMEs, which are an important part of the euro area economy. See Kalemli-Ozcan et al. (2015) for a thorough discussion of the representativeness of ORBIS relative to Eurostat data. Most relevant to our study is the representativeness in terms of employment. In Table A.2 we show the total number of employees in our cleaned firm-level dataset from the nine countries relevant for our study and relate it to Eurostat data. Coverage is very heterogeneous across time and countries because ORBIS collects data from different national providers that in turn collect data based on public disclosure rules subject to changes over time.[43] In the following section, we address concerns on coverage heterogeneity of ORBIS.

B   Differences between firms connected to strong and firms connected to weak banks

Table A.7 compares firms connected to strong banks with firms connected to weak banks in the year 2003. Assuming a two-year reform impact lag, we would not expect to see any significant effects on employment in 2003 since nearly all countries implemented their first reforms in 2002 or 2003.[44] As shown by the t-test statistic, the two groups of firms do not differ significantly in terms of employment growth, which lends support to the parallel trends assumption.[45] In contrast, the two groups of firms differ substantially in the level of employment, our four main

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[43]The data providers and the disclosure rules determining coverage are described in detail in Table A.1 in Kalemli-Ozcan et al. (2015)

[44]The Netherlands already implemented its first reform in 2001 but Dutch firms only constitute a small part of our sample.

[45]Employment growth in the second year of our sample period, 2002, is relatively higher for firms connected to strong banks which is likely due to the dotcom-recession of 2001. We verify in an unreported regression that our baseline result remains qualitatively unchanged if we let the sample period begin in 2003.

firm characteristics and three additional firm variables, namely savings over total assets, interest expenses over total liabilities and operating revenue over total assets. Note, however, that since we are using firm fixed effects throughout the paper, our four main firm variables are only meant to control for within-firm dynamics. We do not need to claim that they adequately capture the differences between different types of firms, e.g. between firms connected to weak or strong banks. Nonetheless, we provide a robustness check in columns 2, 3, and 4 of Table A.10 where we successively add the three additional firm controls to the four control variables from our baseline specification and verify that our results remain qualitatively unchanged.

C Further sensitivity checks

As discussed in Section A, coverage in ORBIS can be uneven across countries and over time. Two things stand out in Table A.2, which relates ORBIS employment numbers to the corresponding Eurostat figures. Firstly, our ORBIS sample over-represents employment for Luxembourg, the Netherlands, and Ireland. The disadvantage of using unconsolidated accounting data is that corporate profit shifting motives might especially affect balance sheet data of large multinational companies. According to Tørsløv et al. (2018), the three countries mentioned above feature corporate tax systems particularly attractive for multinationals, which is why profit shifting is especially relevant in their case. Column 1 of Table A.8 verifies that our main result is robust to excluding all three countries. Secondly, due to missing data on employees, the coverage for Portuguese companies from 2002 to 2005 is almost non-existent. Column 2 verifies that our results are also robust to excluding data from Portuguese companies prior to 2006.

Firm age is another relevant factor for our purposes since it moderates the need for financing. While young firms might still be struggling to establish themselves on the market and build relationships with creditors, older firms are more likely to have established stable business- and bank relationships. In columns 3 and 4 of Table A.8, we show subsample regressions for young and old firms using the 25-years threshold from Gal and Hijzen (2016). Our main result of a negative interaction between bank weakness and unemployment benefit reforms can be reproduced in both subsamples.

In our baseline regression, we focus on the general relationship between access to credit, proxied by the health of a firm's main bank, and firm-level employment dynamics. Ideally, we would also have very detailed information on the interest payments on specific loans provided by

a firm's main bank that we can then compare to an appropriate benchmark interest rate for each firm's peer group. Due to data availability, we can only construct a relatively crude measure of a firm's weighted average cost of capital (WACC). To estimate the WACC, we use a measure that lumps together dividends and interest paid on debt and divide this measure by total assets. Table A.9 shows that controlling for this measure leaves our main result qualitatively unchanged.

In column 1 of Table A.10, we want to assure the reader that our results are not driven by our choice of control variables. We re-estimate our baseline regression without any firm-level controls and without controlling for bank size. Omitting all control variables decreases the explanatory power of our model and lowers the coefficient for our main result, the negative interaction between unemployment benefit reforms and the binary weak bank indicator. At the same time, we also gain some explanatory power relative to our baseline by including a larger set of firms in our analysis. This allows us to find a slightly significant negative interaction effect between weak banks and regular employment reforms, while this effect is insignificant in our baseline regression.

In our baseline regression, we cluster standard errors on the firm-level to account for firm-specific unobserved shocks that induce correlation between all within-firm observations. Clustering at the country-level is a sensible alternative to clustering at the firm-level since the reforms we are studying are legislated at the country-level (see the discussion in Bertrand et al. (2004)). In Table A.11, we cluster standard errors at the country- instead of at the firm-level. The significance of our main results is preserved. Note, however, that this regression has to be taken with a grain of salt due to the low number of clusters, given that our sample only includes nine countries.

Recall that in our baseline regression we lag reforms by two years while all other explanatory variables are lagged by one year in order to allow for a bigger impact lag for country-level developments. In Table A.12, we lag all explanatory variables by one year and find that our main result remains intact.

In our baseline model, we dichotomise bank stress based on the 75th percentile within the regression sample in order to ease interpretability of the interaction effects. In Table A.13, we use the continuous version of the bank stress variable instead of the binary weak bank indicator. We can reproduce our main result of a negative interaction between unemployment benefit reforms and bank weakness. Moreover, we find a negative interaction between the employment impact of temporary employment reforms and bank weakness, which we do not find in our

preferred specification. Note that in this regression we are sacrificing ease of interpretability for the sake of more detailed variation in bank stress. Here we can only conclude that the effect of unemployment benefit reforms and temporary employment reforms diminishes with *increasing* bank stress. In Table A.14, we divide bank stress into three terciles instead of dichotomising it. Here we find that the interaction of unemployment benefit reforms with the highest tercile of bank stress features an even higher coefficient than the one in our baseline regression (where the weak bank indicator captures the highest quartile).[46]

Lastly, Table A.15 features a robustness check that excludes firms having reported more than one bank relationship in the bank-firm linkage part of the ORBIS dataset.[47] As noted by Kalemli-Ozcan et al. (2019), multi-bank firms are common in exactly those countries that feature relatively stressed banking sectors, i.e. Spain, Portugal, and Greece. Thus, dropping multi-bank firms amounts to a rather extreme change in our sample composition. We therefore opt to re-define the binary weak bank indicator on the basis of the new subsample instead of defining it based on our original baseline regression subsample. Column 1 shows the result of using this newly defined binary weak bank measure. With this estimation framework we are unable to recover our main result in the new subsample. Given the considerable reduction in the variation of bank stress that is available in this subsample, we think it is a sensible choice to use the continuous version of our bank stress indicator rather than the binary measure. When using the continuous bank stress variable (also used in Table A.13), we are indeed able to reproduce our main result also within the subsample of firms that have reported only one relationship bank.

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[46]Note that the positive coefficient on the constituent term of medium and high bank stress is not a cause for concern. Due to the interactive structure of our model, these coefficients only measure the relative effect during those years in which a country has not implemented a single one of the three reform types in our sample. Thus it captures the difference between firms connected to banks with low stress relative to firms connected with higher levels of stress during limited and relatively early country-year subsamples.

[47]Recall that in these cases we keep the bank with the highest total assets in the year 2000.

D   Appendix tables

Table A.1: Definition of variables

Variable	Source	Definition
Firm-related variables:		
Ln(employees)	ORBIS	The natural logarithm of the total number of employees.
Leverage	ORBIS	Financial debt (e.g. loans and bonds) over total assets.
Cash	ORBIS	Cash over total assets.
Tangibility	ORBIS	Tangible fixed assets over total assets.
Return on assets	ORBIS	Net income over total assets.
WACC	ORBIS	Dividends and interests paid on debt over total asset.
Operating rev.	ORBIS	Operating revenue over total assets.
Interest	ORBIS	Interest expenses over total liabilities.
Savings	ORBIS	EBITDA less tax expenses and interest expenses over total assets.
Firm-bank variables:		
Bank size	Bankscope	Size of a given firm's main relationship bank. Size is based on the natural logarithm of a bank's total assets in m EUR of 2015.
Bank stress	Bankscope	Financial constraints faced by a given firm proxied by the health of its main relationship bank. The indicator is based on the principal component derived from the following bank-level variables:
Bank capitalization	Bankscope	Equity over total assets.
Bank non-performing loans	Bankscope	Non-performing loans over total loans.
Bank return on assets	Bankscope	Net income over total assets.
Bank Z-score	Bankscope	Equity and net income over SD(return on assets).
Bank liquidity	Bankscope	Liquid assets (securities, derivatives and loans and advances to banks) less deposits and short-term funding over total assets.
Weak bank	Bankscope	The bank stress indicator for a given firm-year is above the 75th percentile of the distribution of bank stress within the regression sample.
Country level variables:		
Real GDP growth	AMECO	Change in annual real GDP relative to previous year.
Recession	AMECO	Binary indicator equal to one if a country's GDP growth is negative in a certain year.
Sector growth	Eurostat	Change in annual real value added relative to previous year (for each sector in each country).
Regular employment reform	Duval et al. (2018b)	Recursive indicator reflecting major reforms that change employment protection for regular workers. The indicator starts out with 0 and increases by one for liberalizing reforms while decreasing by one for tightening reforms.
Unemployment benefit reform	Duval et al. (2018b)	Recursive indicator reflecting major reforms that change unemployment benefit replacement rates or the duration of unemployment benefits. The indicator starts out with 0 and increases by one for liberalizing reforms while decreasing by one for tightening reforms.
Temporary employment reform	Duval et al. (2018b)	Recursive indicator reflecting major reforms that change the regulation of temporary employment. The indicator starts out with 0 and increases by one for liberalizing reforms while decreasing by one for tightening reforms.
Sector-related variables:		
EOS (Ciminelli)	Ciminelli et al. (2018a)	Technological elasticity of substitution between labour and capital estimated through a production function approach. Ciminelli et al. (2018a) estimate the elasticity of substitution through a production function approach and use industry-level data on output, inputs and productivity from the 2017 EU KLEMS database for 13 EU countries.

(Continued)

Table A.1: – *Continued*

EOS (Laeven)	Laeven et al. (2018)	Technological elasticity of substitution between labour and capital estimated through a production function approach. Laeven et al. (2018) et al use industry-level data on output, inputs and productivity from the US KLEMS database from 1947-2007.
EFD	Duygan-Bump et al. (2015)	External financial dependence (EFD). Duygan-Bump et al. (2015) calculate the EDF as the proportion of capital expenditure financed with external funds for mature Compustat firms from 1980-1996.
Durability	Bils et al. (2013)	Bils et al. (2013) create a measure of durability for the products sold by firms in each US industry based on life expectancy tables from insurance companies and estimates from the US Bureau of Economic Analysis.

Table A.2: Orbis coverage

This table shows the employment coverage (in %) of our ORBIS dataset relative to Eurostat. Employment numbers from ORBIS are based on the total number of employees reported by the firms in our sample while Eurostat numbers are based on national account data on total employees in the sectors included in our sample.

Country	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014
AT	12	14	14	14	17	11	12	13	18	35	54	55	60	66
DE	23	20	21	22	29	38	39	40	43	44	51	59	62	62
ES	62	68	68	68	69	68	65	65	67	67	66	67	68	68
FR	84	52	59	57	52	49	49	42	41	46	43	38	46	51
GR	33	41	42	43	43	42	41	40	45	45	46	45	45	48
IE	11	25	39	77	94	140	154	159	171	171	179	193	195	276
LU	32	139	95	81	104	113	199	190	205	204	207	187	199	192
NL	73	83	88	99	101	102	87	79	111	110	120	123	122	120
PT	14	6	1	1	1	83	87	89	89	87	88	89	89	89

Table A.3: Bank stress

This table shows the evolution of our bank stress indicator. It reflects the financial constraints faced by the firms in our sample as proxied by the health of their main relationship bank. The bank stress indicator captures the principal component of a bank's capitalization, non-performing loans, return on assets, Z-score and liquidity. Note that this table shows the variation of the continuous bank stress measure rather than the dichotomized weak bank indicator. Refer to Appendix Table A.1 for variable definitions.

Country	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014
AT	-0.06	-0.05	-0.03	-0.10	-0.11	-0.23	-0.16	-0.09	0.02	0.05	0.04	-0.02	0.03	-0.05
DE	-0.43	-0.39	-0.34	-0.28	-0.20	-0.26	-0.24	-0.25	-0.21	-0.19	-0.00	-0.02	-0.02	-0.03
ES	-0.01	-0.02	-0.04	-0.19	0.10	0.04	-0.01	0.03	-0.27	-0.23	-0.17	0.03	0.08	-0.02
FR	0.13	0.15	0.13	0.12	0.03	-0.01	0.01	-0.08	-0.11	-0.10	-0.09	-0.10	-0.10	-0.12
GR	-0.07	-0.00	-0.02	-0.10	-0.03	-0.15	-0.18	-0.03	-0.00	0.13	1.13	0.92	0.58	0.83
IE	-0.16	-0.13	-0.13	-0.21	-0.29	-0.35	-0.26	-0.10	0.03	0.32	0.49	0.64	0.84	0.51
LU	NA	NA	NA	-0.41	-0.44	-0.48	-0.29	-0.32	-0.37	-0.42	0.14	-0.17	-0.18	-0.20
NL	-0.15	-0.07	-0.10	-0.11	-0.20	-0.25	-0.22	-0.11	-0.24	-0.17	0.06	0.21	0.36	0.33
PT	-0.01	-0.01	-0.03	-0.04	-0.08	-0.07	-0.09	-0.05	-0.17	-0.14	-0.04	0.16	0.36	0.47

Table A.4: Regular employment protection reforms

This table shows the evolution of the recursive reform indicator for regular employment protection reforms within our sample. The indicator jumps by one for (liberalizing) reforms and decreases by one for counter-reforms. (Note that this indicator only reflects changes but not the level of employment protection for regular workers.)

Country	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013
AT	0	0	0	0	1	1	1	1	1	1	1	1	1	1	1
DE	0	0	0	0	0	1	1	1	1	1	1	1	1	1	1
ES	0	0	0	1	1	1	1	1	1	1	1	1	2	2	3
FR	0	0	0	0	-1	-1	-1	-1	-1	-1	0	0	0	0	0
GR	0	0	0	0	0	0	0	0	0	0	0	0	1	2	2
IE	0	0	0	0	0	0	0	1	1	1	1	1	1	0	0
LU	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
NL	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
PT	NA	0	0	0	0	1	1	1	1	1	1	2	2	2	2

Table A.5: Unemployment benefit scheme reforms
This table shows the evolution of the recursive reform indicator for reforms of unemployment benefit schemes within our sample. The indicator jumps by one for (liberalizing) reforms and decreases by one for counter-reforms. (Note that this indicator only reflects changes and not level of the generosity of unemployment benefit schemes.)

Country	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013
AT	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
DE	0	0	0	0	0	0	1	1	1	1	1	1	1	1	1
ES	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
FR	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
GR	0	0	0	0	0	0	0	0	0	0	0	0	0	1	1
IE	0	0	0	0	0	0	0	0	0	0	0	0	1	1	1
LU	0	0	0	0	0	0	0	0	1	1	1	1	1	1	0
NL	0	0	0	0	0	0	0	0	0	0	0	1	0	1	1
PT	NA	0	0	0	0	0	0	0	0	0	0	0	0	1	1

Table A.6: Temporary employment reforms

This table shows the evolution of the recursive reform indicator for temporary employment reforms within our sample. The indicator jumps by one for (liberalizing) reforms and decreases by one for counter-reforms. (Note that this indicator only reflects changes but not the level of temporary employment regulation.)

Country	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013
AT	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
DE	0	0	0	1	2	2	2	2	2	2	2	2	2	1	1
ES	0	0	0	0	0	0	0	0	0	0	0	0	1	0	1
FR	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
GR	0	0	0	0	0	1	1	1	1	1	1	1	2	2	2
IE	0	0	0	0	0	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
LU	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
NL	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
PT	NA	0	0	-1	-1	0	0	0	0	1	1	1	1	1	1

Table A.7: Two-sample t-tests for firms connected to strong and firms connected to weak banks

This table shows two sample t-tests for the observations in our regression sample in 2003. We compare variable means for firms connected to strong banks and firms connected to weak banks. Column 5 shows the differences in means between the two groups of firms and column 6 reports the test statistic for a two-sample t-test. Refer to Appendix Table A.1 for variable definitions.

	Firms connected to strong banks		Firms connected to weak banks			
	N	Mean	N	Mean	Difference	*t*-stat
Ln(employment)	57,239	2.699	17,896	2.232	0.468	40.498
Leverage	57,239	0.145	17,896	0.097	0.048	32.733
Cash	56,487	0.149	17,684	0.194	-0.045	-30.316
Tangibility	57,091	0.220	17,888	0.168	0.052	29.664
Return on assets	57,225	0.043	17,896	0.062	-0.019	-22.045
Operating rev.	56,653	1.775	17,894	2.126	-0.351	-31.464
Interest	51,671	0.016	13,858	0.012	0.004	29.062
Savings	47,521	0.088	13,755	0.103	-0.014	-14.265
Empl. growth	52,376	0.131	14,557	0.097	0.033	1.022

Table A.8: Robustness: Countries with over-coverage or uneven coverage; young vs. old firms
This table reports estimates of equation (2). The sample covers the period 2001-2014 and has a firm-year structure. The dependent variable is the log of employment. The dependent variable is the log of employment. The explanatory variables include firm-level balance sheet information, time-varying information on the balance sheet of each firm's main bank and the three recursive reform indicators. In column 1 we exclude Luxembourg, Ireland and the Netherlands and in column 2 we exclude employment data from Portugal prior to 2006. In column 3 we only include firms less than 25 years old while we only look at firms older than 25 years in column 4. All independent variables are lagged by one year except for the reform indicators which are lagged by two years. Standard errors are clustered as indicated below. t-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Ln(employment)			
	(1)	(2)	(3)	(4)
	Excl. LU,IE,NL	Excl. PT pre-2006	Age<25	Age>=25
Regular empl. ref. × weak bank	-0.001	-0.001	-0.003*	-0.000
	(-0.56)	(-0.59)	(-1.94)	(-0.24)
Unempl. benefit ref. × weak bank	-0.023***	-0.023***	-0.017**	-0.017*
	(-4.14)	(-3.93)	(-2.27)	(-1.89)
Temporary empl. ref. × weak bank	-0.002	-0.002	-0.001	-0.003
	(-1.00)	(-1.09)	(-0.29)	(-0.70)
Weak bank	-0.001	-0.001	0.002	-0.002
	(-0.63)	(-0.56)	(0.91)	(-0.56)
Firm controls	Yes	Yes	Yes	Yes
Bank size	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Sector-year FE	No	No	No	No
Country-sector-year FE	Yes	Yes	Yes	Yes
Observations	2,044,823	2,069,640	1,481,856	556,692
Adjusted R^2	0.934	0.935	0.927	0.956
Number of firms	381,108	387,104	300,933	114,284
Number of banks	1,096	1,116	1,037	806
Mean dep. var.	2.258	2.272	2.103	2.709
Clustering	Firm	Firm	Firm	Firm
Sample period	2001-2014	2001-2014	2001-2014	2001-2014

Table A.9: Robustness: Weighted average cost of capital

This table reports estimates of equation (2). The sample covers the period 2001-2014 and has a firm-year structure. The dependent variable is the log of employment. The explanatory variables include firm-level balance sheet information, time-varying information on the balance sheet of each firm's main bank and the three recursive reform indicators. All independent variables are lagged by one year except for the reform indicators which are lagged by two years. Standard errors are clustered as indicated below. *t*-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Ln(employment)
	(1)
Regular empl. ref. × weak bank	-0.001
	(-0.72)
Unempl. benefit ref. × weak bank	-0.026***
	(-3.75)
Temporary empl. ref. × weak bank	-0.002
	(-0.77)
Weak bank	-0.001
	(-0.73)
WACC	-0.007
	(-0.14)
Firm controls	Yes
Bank size	Yes
Firm FE	Yes
Sector-year FE	No
Country-sector-year FE	Yes
Observations	1,772,610
Adjusted R^2	0.934
Number of firms	344,486
Number of banks	1,097
Mean dep. var.	2.403
Clustering	Firm
Sample period	2001-2014

Table A.10: Robustness: Excluding control variables; adding additional control variables

This table reports estimates of equation (2). The sample covers the period 2001-2014 and has a firm-year structure. The dependent variable is the log of employment. In column 1, the explanatory variables include the weak bank indicator reflecting the health of a firm's main bank and its interaction with the three recursive reform indicators and no firm control variables. In columns 2, 3, and 4, we successively add three new firm control variables to the four main firm control variables from our baseline specification. All independent variables are lagged by one year except for the reform indicators which are lagged by two years. Standard errors are clustered as indicated below. t-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Ln(employment)			
	(1)	(2)	(3)	(4)
Regular empl. ref. × weak bank	-0.002*	-0.001	-0.001	-0.000
	(-1.85)	(-0.45)	(-0.47)	(-0.09)
Unempl. benefit ref. × weak bank	-0.012***	-0.024***	-0.027***	-0.026***
	(-2.67)	(-4.09)	(-3.92)	(-3.38)
Temporary empl. ref. × weak bank	-0.001	-0.002	-0.002	-0.003
	(-0.63)	(-1.15)	(-0.90)	(-1.21)
Weak bank	-0.001	-0.001	-0.002	-0.002
	(-0.59)	(-0.64)	(-0.80)	(-1.07)
Leverage		0.005	0.012***	0.033***
		(1.26)	(2.70)	(6.64)
Cash		-0.044***	-0.046***	-0.047***
		(-9.07)	(-8.28)	(-8.11)
Tangibility		0.074***	0.075***	0.087***
		(10.46)	(9.74)	(10.47)
Return on assets		0.203***	0.230***	0.158***
		(45.79)	(43.87)	(17.01)
Operating rev.		0.051***	0.057***	0.055***
		(50.98)	(48.85)	(44.01)
Interest			-0.496***	-0.549***
			(-9.35)	(-9.54)
Savings				0.073***
				(7.98)
Bank size		-0.004***	-0.003***	-0.003**
		(-3.45)	(-2.93)	(-2.24)
Firm FE	Yes	Yes	Yes	Yes
Sector-year FE	No	No	No	No
Country-sector-year FE	Yes	Yes	Yes	Yes
Observations	2,419,003	2,044,530	1,755,181	1,547,647
Adjusted R^2	0.934	0.936	0.935	0.937
Number of firms	503,155	381,295	339,966	309,823
Number of banks	1,351	1,052	1,033	1,024
Mean dep. var.	2.213	2.270	2.401	2.491
Clustering	Firm	Firm	Firm	Firm
Sample period	2001-2014	2001-2014	2001-2014	2001-2014

Table A.11: Robustness: Country-level clustering

This table reports estimates of equation (2). The sample covers the period 2001-2014 and has a firm-year structure. The dependent variable is the log of employment. The explanatory variables include firm-level balance sheet information, time-varying information on the balance sheet of a firm's main bank and the three recursive reform indicators. All independent variables are lagged by one year except for the reform indicators which are lagged by two years. Standard errors are clustered as indicated below. t-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Ln(employment)
	(1)
Regular empl. ref. × weak bank	-0.001
	(-0.44)
Unempl. benefit ref. × weak bank	-0.023***
	(-7.16)
Temporary empl. ref. × weak bank	-0.002
	(-1.42)
Weak bank	-0.001
	(-0.36)
Firm controls	Yes
Bank size	Yes
Firm FE	Yes
Sector-year FE	No
Country-sector-year FE	Yes
Observations	2,070,262
Adjusted R^2	0.935
Number of firms	387,113
Number of banks	1,116
Mean dep. var.	2.272
Clustering	Country
Sample period	2001-2014

Table A.12: Robustness: Lagging reforms by one year only

This table reports estimates of equation 2. The sample covers the period 2001-2014 and has a firm-year structure. The dependent variable is the log of employment. The explanatory variables include firm-level balance sheet information, time-varying information on the balance sheet of each firm's main bank and the three recursive reform indicators. All independent variables are lagged by one year including, in this robustness check, the reform indicators. Standard errors are clustered as indicated below. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Ln(employment)
	(1)
Regular empl. ref. × weak bank	-0.000
	(-0.20)
Unempl. benefit ref. × weak bank	-0.022***
	(-5.19)
Temporary empl. ref. × weak bank	0.004
	(1.50)
Weak bank	-0.003
	(-1.56)
Firm controls	Yes
Bank size	Yes
Firm FE	Yes
Sector-year FE	No
Country-sector-year FE	Yes
Observations	2,310,709
Adjusted R^2	0.930
Number of firms	410,796
Number of banks	1,132
Mean dep. var.	2.253
Clustering	Firm
Sample period	2001-2014

Table A.13: Robustness: Continuous bank stress
This table reports estimates of equation (2). The sample covers the period 2001-2014 and has a firm-year structure. The dependent variable is the log of employment. The explanatory variables include firm-level balance sheet information, time-varying information on the balance sheet of each firm's main bank and the three recursive reform indicators. All independent variables are lagged by one year except for the reform indicators which are lagged by two years. Standard errors are clustered as indicated below. t-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Ln(employment)
	(1)
Regular empl. ref. $\times$ bank stress	0.000
	(0.15)
Unempl. benefit ref. $\times$ bank stress	-0.050***
	(-6.34)
Temporary empl. ref. $\times$ bank stress	-0.011***
	(-3.25)
Bank stress	-0.002
	(-0.53)
Firm controls	Yes
Bank size	Yes
Firm FE	Yes
Sector-year FE	No
Country-sector-year FE	Yes
Observations	2,070,262
Adjusted R^2	0.935
Number of firms	387,113
Number of banks	1,116
Mean dep. var.	2.272
Clustering	Firm
Sample period	2001-2014

Table A.14: Robustness: Three categories of bank stress

This table reports estimates of equation (2). The sample covers the period 2001-2014 and has a firm-year structure. The dependent variable is the log of employment. The explanatory variables include firm-level balance sheet information, time-varying information on the balance sheets of each firm's main bank and the three recursive reform indicators. All independent variables are lagged by one year except for the reform indicators which are lagged by two years. Standard errors are clustered as indicated below. t-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Ln(employment)
	(1)
Regular empl. ref. × medium stress	-0.002
	(-1.13)
Regular empl. ref. × high stress	-0.002
	(-0.96)
Unempl. benefit ref. × medium stress	-0.022
	(-1.19)
Unempl. benefit ref. × high stress	-0.048**
	(-2.51)
Temporary empl. ref. × medium stress	-0.001
	(-0.45)
Temporary empl. ref. × high stress	-0.001
	(-0.48)
Medium stress	0.009***
	(3.61)
High stress	0.011***
	(4.12)
Firm controls	Yes
Bank size	Yes
Firm FE	Yes
Sector-year FE	No
Country-sector-year FE	Yes
Observations	2,070,262
Adjusted R^2	0.935
Number of firms	387,113
Number of banks	1,116
Mean dep. var.	2.272
Clustering	Firm
Sample period	2001-2014

Table A.15: Robustness: Excluding multi-bank firms

This table reports estimates of equation (2). The sample covers the period 2001-2014 and has a firm-year structure. The dependent variable is the log of employment. The explanatory variables include firm-level balance sheet information, time-varying information on the balance sheet of each firm's main bank and the three recursive reform indicators. In column 1, we use a binary weak bank indicator that is re-defined based on the new regression sample using only single-bank firms. In column 2, we use the same subsample but the continuous bank stress indicator that is also used in Appendix Table A.13. All independent variables are lagged by one year except for the reform indicators which are lagged by two years. Standard errors are clustered as indicated below. t-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Ln(employment)	
	(1)	(2)
	Excl. multi-bank firms	
Regular empl. ref. × weak bank (new)	0.001	
	(0.59)	
Regular empl. ref. × bank stress		0.004
		(1.37)
Unempl. benefit ref. × weak bank (new)	0.002	
	(0.44)	
Unempl. benefit ref. × bank stress		-0.027***
		(-2.93)
Temporary empl. ref. × weak bank (new)	-0.005	
	(-1.53)	
Temporary empl. ref. × bank stress		-0.004
		(-0.85)
Weak bank (new)	-0.001	
	(-0.31)	
Bank stress		-0.003
		(-0.81)
Firm controls	Yes	Yes
Bank size	Yes	Yes
Firm FE	Yes	Yes
Sector-year FE	No	No
Country-sector-year FE	Yes	Yes
Observations	1,239,891	1,239,891
Adjusted R^2	0.943	0.943
Number of firms	249,345	249,345
Number of banks	1,086	1,086
Mean dep. var.	2.083	2.083
Clustering	Firm	Firm
Sample period	2001-2014	2001-2014

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