The Effects of Credit Supply on Wage Inequality between and within Firms*

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Abstract

In this paper, we study the effect of a credit supply shock on the distribution of wages within and between firms. We construct a novel dataset combining administrative linked employer-employee data with information on firms' preexisting bank relationships in the credit market. We use the introduction of negative monetary policy rates in the euro area as a source of variation in banks' credit supply to firms in Germany. We find that this credit supply shock leads to higher within-firm wage inequality at more affected employers. At the same time, we find a reduction in between-firm wage inequality due to relatively higher average wages among initially lower-paying employers. Our results suggest that monetary policy can have important distributional consequences through affecting credit supply and firm pay heterogeneity.

JEL classification: D22, G21, G31, G32, J31

Keywords: credit supply, monetary policy, negative policy rates, wage inequality, bank lending relationships, worker and firm heterogeneity, linked employer-employee data

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1 Introduction

A salient characteristic of many labor markets is that seemingly identical workers receive large differences in pay across jobs. Motivated by this observation, there has been growing interest in employer heterogeneity as an important determinant of worker-level outcomes, including income inequality and income risk (Abowd, Kramarz, and Margolis (1999); Guiso, Pistaferri, and Schivardi (2005)). A recent strand of work in this area documents that large changes in the earnings distribution are explained by the evolution of between-firm pay differences over time (Card, Heining, and Kline (2013); Alvarez, Benguria, Engbom, and Moser (2018); Song, Price, Guvenen, Bloom, and von Wachter (2019)). Yet the drivers behind the evolution of both between-firm and within-firm pay differences remain insufficiently understood.

Dustmann, Ludsteck, and Schönberg (2009) show that wage inequality in Germany has increased from the 1980s to the mid-1990s for similar reasons as in the U.S., in particular changes in labor market institutions, such as de-unionization, and (some variant of) skill-biased technological change. In addition, Biewen, Fitzenberger, and de Lazzer (2018) point out the role of compositional changes in education, age, and recent labor market histories for the rise of wage inequality in Germany between 1985 and 2010.

However, these trends that lasted for almost three decades might have come to a turning point. As documented by Möller (2016), wage inequality in Germany has ceased to increase since $2010 - 2011$, coinciding with the European sovereign debt crisis and the policy response by the European Central Bank (ECB).

In this paper, we put forward credit supply as a novel explanation for the development of wage inequality between and within firms in Germany. In particular, we exploit the ECB's introduction of negative monetary policy rates in 2014 as a source of variation in credit supply. We estimate how this monetary policy affects different workers' wages through firms' predetermined banking relationships and banks' exposure to the negative rates.

Our empirical strategy builds on Heider, Saidi, and Schepens (2018), who highlight the transmission of negative monetary policy rates to firm-level outcomes through banks' funding structure. Their empirical identification is centered on the idea that banks are reluctant, or unable, to pass on negative rates to their depositors. Therefore, banks that rely more on deposit funding—as opposed to interbank lending—experience a negative shock to their net worth, leading to a reduction in bank lending and increased risk taking. In line with Heider, Saidi, and Schepens (2018), we show that this risk taking by high-deposit banks takes on the form of more (concentrated) lending to riskier firms.

We extend this identification strategy to study the effects of credit supply on the distribution of wages across workers. We find that reduced credit supply by high-deposit, rather than low-deposit, banks to firms in preexisting lending relationships leads to higher within-firm wage inequality.

At the same time, we find an inequality-reducing treatment effect on between-firm wage inequality. Such reduction in between-firm wage inequality can stem from two fundamental explanations. First, firms with low-paid workers and, thus, low average wages could start paying their employees more. Alternatively, formerly low-paid workers may gain the ability to switch to firms that generally pay higher wages than their previous employers.

To disentangle these two channels, we further decompose the estimated effects of the credit supply shock on the wage distribution into components due to changes in the allocation of workers across firms, and changes in firms' pay policies. The results of this decomposition show that the observed decline in between-firm wage inequality is mostly due to average wages decreasing less among initially lower-paying firms that are more exposed to the negative policy rate through their banking relationships. We characterize these initially lower-paying firms as risky firms that are previously constrained to pay higher average wages, and whose constraints are relaxed due to the non-decreasing credit supply by high-deposit banks.

Our contribution can be viewed from two angles. On the one hand, this paper is among the first to identify variation in credit supply as a source of wage inequality between and within firms. Related work by Chodorow-Reich (2014) and, more recently, by Barbosa, Bilan, and Célérier (2017) and Berton, Mocetti, Presbitero, and Richiardi (2018) explores the effect of credit supply shocks on employment. While our analysis incorporates this extensive margin, we further test for the effects of credit supply on wages. In particular, we focus on the effects of credit supply shocks on firms' responses in wage setting for standing employees and new hires throughout the wage distribution.

On the other hand, this paper contributes to our understanding of the distributional consequences of monetary policy. While addressing inequality is not a direct objective of monetary policy, it is known—at least in theory—to affect the distribution of income, for example through heterogenous balance-sheet compositions between low-income vs. high-income households, as described in Auclert (2017).

A small but growing literature has provided direct evidence on the comovement between inequality and financial conditions (Rajan (2010); Kumhof, Rancière, and Winant (2015); Coibion, Gorodnichenko, Kueng, and Silvia (2017)). In this paper, we build a new administrative dataset, and propose variation in banks' credit supply as a novel channel through which monetary policy affects wage inequality. By uncovering important distributional effects of negative policy rates, our results can inform the debate on the (unintended) consequences of monetary policy that is of interest to academic researchers and policymakers alike.

2 Linking Credit Supply and Wage Inequality: Hypothesis Development

We start from the premise that firms that are financially constrained set wages in response to such constraints. Financial constraints can, in turn, be relaxed through credit provision by banks, while credit supply is partly governed by monetary policy.

Variations in credit supply to firms may have an effect on the wage distribution and, thus, inequality *between and within* firms. Risky firms tend to be more credit constrained (Neuhann and Saidi (2018)). These firms may, in turn, be constrained to pay higher wages or to hire more workers. Credit supply can relax these firms' constraints. This can have consequences for between-firm wage inequality in at least two ways.

First, initially low-paying firms may adjust their pay policies in the face of changes in credit access. Second, workers may gain the ability to switch to firms that pay differently than their previous employers, and that change their hiring policies in response to a credit supply shock.

Credit supply shocks may also affect within-firm wage inequality. Fagereng, Guiso, and Pistaferri (2018) document that workers are partially insured against firm-level shocks, such as credit supply shocks, in that there exists some passthrough of such shocks to workers' wages. In particular, they show that firms offer less insurance to workers with higher wealth, particularly against permanent, rather than transitory, firm-level shocks.

The implications for the distribution of wages within firms depends on the differential passthrough of positive vs. negative firm-level shocks to workers according to their position in the wage distribution. In particular, while top earners' wages may covary more with firm-level shocks *overall* (Fagereng, Guiso, and Pistaferri (2018)), this may be primarily due to the greater passthrough of positive firm-level outcomes. This, in turn, would imply greater within-firm wage inequality.

Similarly, bottom earners may be affected by greater passthrough of negative firm-level outcomes, in that they are laid off more frequently following the realization of such negative firm-level outcomes.[1]

Variations in firms' credit supply may also have an effect on the wage distribution of new hires, and differentially so across workers according to their position in the wage distribution, as suggested by Guiso, Pistaferri, and Schivardi (2013). Besides contributing

[1] Note that this implies a sample bias in Fagereng, Guiso, and Pistaferri (2018) if they limit their sample to workers that remain employed at the same firm undergoing any shocks.

to within-firm wage inequality, this is also a component that may govern between-firm wage inequality, through the two channels mentioned above.

As a shock to credit supply, we use the transmission of negative monetary policy rates to the real sector via bank lending following the implementation of a negative deposit facility rate, one of the main policy rates, in the euro area in June 2014.

Heider, Saidi, and Schepens (2018) argue that banks are reluctant to pass on negative rates to their depositors. Therefore, banks that rely more on deposit funding experience a negative shock to their net worth. In line with Heider, Saidi, and Schepens (2018), we show that this leads to less lending overall by high-deposit banks that, instead, focus their lending on risky firms.

We exploit this credit supply shock as follows. Following the introduction of negative policy rates in June 2014, we show that firms that are in relationships with high-deposit, rather than low-deposit, banks are less likely to receive loan financing. This credit contraction is, however, confined to safe, rather than risky, firms.

Against this background, we hypothesize that the transmission of negative monetary policy rates through bank lending can affect between-firm wage inequality within the group of firms in relationships with high-deposit banks vis-à-vis the group of firms in relationships with low-deposit banks. This is because, as we will show, high-deposit banks reallocate credit towards risky firms that are more likely to have been constrained to pay higher average wages, thereby enabling them to pay relatively more on average than other firms in relationships with high-deposit banks. This should, in turn, lead to less between-firm wage inequality within the group of firms in relationships with high-deposit banks.

In addition, negative monetary policy rates may also affect within-firm wage inequality. Firms in relationships with high-deposit banks experience, on average, a credit contraction in comparison to firms in relationships with low-deposit banks. It is plausible that the former group of firms passes through this negative credit supply shock to the wages of workers depending on their position in the wage distribution. For instance, if wages at the bottom are more likely to be rationed than wages at the top, then this leads to greater within-firm wage inequality at firms in relationships with high-deposit banks than at firms in relationships with low-deposit banks.

3 Empirical Strategy and Implementation

We wish to estimate the causal effect of credit supply shocks stemming from monetary policy on individual wages of workers at different percentiles of the wage distribution both between and within firms.

For the sake of simplicity, we denote credit supply shocks at the annual firm level jt by *Shock*$_{jt}$. To estimate the effect of credit supply on between-firm inequality, we conduct the analysis at the worker level ijt, and estimate the following regression specification, where the dependent variable is a function of individual wages (or employment status):

$$y_{ijt} = \beta_1 Shock_{jt} \times Position_{it-1} + \beta_2 Position_{it-1} + \mu_{jt} + \theta_i + \varepsilon_{ijt}, \quad (1)$$

where y_{ijt} is worker i's employment outcome—e.g., her annualized wage—at firm j in year t, $Position_{it-1}$ is a function of the position of worker i's wage in the distribution of firm j or elsewhere in year $t-1$, $Shock_{jt}$ captures credit supply for firm j in year t, μ_{jt} denotes firm-time fixed effects, and θ_i are individual fixed effects. Standard errors are clustered at the firm level.

For workers that do not switch firms, it holds that firm j associated with both $Position_{it-1}$ and the fixed effects μ_{jt} are identical. We set $\mu_{jt} = \mu_{jt-1}$ for all workers i that are unemployed in period t but were employed by firm j in period $t-1$.

In our implementation of (1), we replace $Position_{it-1}$ by *Bottom* $20\%_i$ and *Middle* $60\%_i$, and use *Top* $20\%_i$ as the omitted category. These are pre-determined indicator variables for whether worker i's wage is in the bottom 20%, middle 60%, or top 20% of the wage distribution of firm j where i was employed, or elsewhere, in the last available year during the pre-period from 2010 to 2013.

Our analysis of the effect of credit supply on within-firm wage inequality is conducted at the firm-year level jt:

$$inequality_{jt} = \beta_1 Shock_{jt} + \mu_j + \eta_t + \varepsilon_{jt}, \tag{2}$$

where $inequality_{jt}$ is a measure of within-firm wage inequality at firm j in year t, $Shock_{jt}$ captures credit supply for firm j in year t, μ_j denotes firm fixed effects, and η_t are year fixed effects. Standard errors are clustered at the firm level.

We use syndicated-loan data from DealScan to sort firms into those in financing relationships with high-deposit banks (treatment group) vs. low-deposit banks (control group). In particular, we define $Treatment_j$ as the average deposits-to-assets ratio of all banks that provided firm j with a syndicated loan, in the role of a lead arranger, anytime from 2010 to 2013. In regression specifications (1) and (2) above, $Shock_{jt}$ can thus be understood as an interaction term between $Treatment_j$ and $After(2014)_t$, where $After(2014)_t$ is a post dummy for the period from 2014 onwards.

4 Data and Sample Construction

For the first time, this paper combines three major datasets covering firms' bank relationships in the syndicated-loan market and the worker level. Specifically, we merge administrative linked employer-employee (ORBIS-ADIAB) data (Schild (2016)) with privately-held and publicly-listed firms' balance-sheet data (Amadeus). In doing so, we focus on holding companies in Amadeus.

The match between establishments in the employer-employee data and firms in Amadeus furthermore enables us to merge our data with information on syndicated loans granted to German firms in DealScan (by hand-matching the latter with firms in the Amadeus database, similar to Acharya, Eisert, Eufinger, and Hirsch (2017) and Heider, Saidi, and Schepens (2018)). Finally, we match the DealScan data with SNL Financial's data on European banks.

Our sample period is from 2010 to 2016. Using the matched employer-employee data, we are able to build a worker panel that records working stints of worker i at any firm j in year t. At any given point in time, an individual i can be employed or unemployed. If individual i is employed at firm j in year t, there exist three possibilities regarding worker i's employment status in the previous year $t-1$: she could have been unemployed, employed at the same firm j, *and/or* employed by another firm that is not j (the qualification *and* refers to cases where worker i switched jobs in $t-1$).

We start by keeping all worker-year observations when worker i is full-time employed at any firm j in year t. Within a given worker-year, we keep the main job j. By main job, we define the job with the highest nominal wage held by worker i. Then, for each worker, we expand the dataset to include zero wages—so as to capture unemployment—in year $t+1$ if worker i is employed by firm j in year t but not in $t+1$. We keep these zero-wage observations but do not label them as unemployment stints if worker i re-joins the same firm j (as in t) in year $t+2$ after earning a zero wage in year $t+1$, e.g., leave of absence.

Finally, we limit our sample to firms active in the syndicated-loan market, as our treatment-intensity variable *Deposit ratio*$_j \in [0,1]$ captures the average deposit ratio of all euro-area banks that firm j received a syndicated loan from anytime from 2010 to 2013.

Summary statistics. Table 1 contains firm-level summary statistics. In particular, Panel A shows summary statistics for 293 German firms (holding companies) in the administrative linked employer-employee (ORBIS-ADIAB) data with available banking data, in particular on the deposit ratio of European banks that firms borrowed from in the syndicated-loan market anytime in the pre-treatment period (from 2010 to 2013).

In Panels B and C, we split up the firms from Panel A, namely into firms in the top and bottom quartile, respectively, of the distribution of *Deposit ratio*$_j$, our treatment variable (the summary statistics of which are in the first row of Panel A). As is already the case in Panel A, there are striking differences within panels between average and median assets, sales, and the number of employees. This indicates that we have very few extremely large firms in our dataset. Note, however, that we difference out time-invariant firm characteristics by including firm fixed effects in all regression specifications. In our worker-level regressions, we additionally include firm-year fixed effects which capture time-varying heterogeneity at the firm level, including different time trends among small vs. large firms.

Comparing firms in Panel B, which have greater exposure to the negative policy rate through their banking relationships, and Panel C, they are very similar in terms of their average size – irrespective of whether we measure it through assets, sales, or the number of employees. Most importantly, average wages are extremely similar across all panels in Table 1. This holds in particular when comparing the composition of workers at treatment (Panel B) vs. control firms (Panel C): besides earning similar wages, they are also similar in terms of their proportion of female, foreign, and university-educated workers.

5 Results

We present our results in two main steps. First, we characterize the nature of the credit supply shock stemming from the introduction of negative monetary policy rates for German firms. Then, we discuss how firms' exposure to negative policy rates through their banking relationships affects wage inequality between and within firms.

5.1 Monetary-policy Transmission through Banks' Deposit Funding

We start by using transaction-level data on syndicated loans of German firms in DealScan in order to estimate whether high-deposit, rather than low-deposit, banks lend to different firms after the introduction of negative policy rates.

As in Heider, Saidi, and Schepens (2018), and to replicate their main finding for German firms relevant for our analysis, we characterize bank risk taking by the ex-ante riskiness of bank-financed firms. In analyzing bank lending behavior, we focus on banks that act as lead arrangers in the syndication process. Lead arrangers are those members of a syndicate that are typically responsible for traditional bank duties including due diligence, payment management, and monitoring of the loan (Ivashina and Scharfstein (2010)).

We estimate the following regression specification:

$$\ln(\sigma(\mathrm{ROA}_{jkt})^{6y} = \beta \textit{Deposit ratio}_k \times \textit{After}(06/2014)_t + \gamma X_{jt} + \delta_t + \eta_k + \epsilon_{jkt}, \tag{3}$$

where $\ln(\sigma(\mathrm{ROA}_{jkt})^{6y}$ is the logged six-year standard deviation of firm j's return on assets (ROA, using P&L before tax) from year $t-5$ to t associated with firm j's loan provided by syndicate k at time t. *Deposit ratio*$_k \in [0,1]$ is equal to the 2013 deposit ratio of the euro-area lead arranger in syndicate k when there is only one lead arranger. When there are multiple lead arrangers, *Deposit ratio*$_k$ is equal to the average deposit ratio in 2013 across all euro-area lead arrangers of syndicate k. *After*$(06/2014)_t$ is a dummy variable for the period from June 2014 onwards, X_{jt} denotes interactions of industry (of firm j) and year fixed effects, and δ_t denotes month-year fixed effects. η_k is a bank fixed effect in syndicates with only one lead arranger. When there are multiple lead arrangers, η_k denotes a vector of bank fixed effects containing one fixed effect for each lead arranger in syndicate k.

The results are in Table 2. In the first two columns, we estimate (3) for a short time window, from 2013 to 2015, around the introduction of negative policy rates in June 2014. High-deposit banks finance significantly riskier firms than do low-deposit banks after the introduction of negative policy rates. This result holds up to the inclusion of borrowers' industry-year fixed effects in column 2.

In the last two columns, we expand the sample so as to encompass 2010 to 2016. The estimate in column 3 is similar to that in column 2. A one-standard-deviation increase in

Deposit ratio$_k$ ($= 0.077$) translates into a 15.5% increase in ROA volatility ($0.077 \times 2.012 = 0.155$). This matches the economic significance that Heider, Saidi, and Schepens (2018) document for the (Amadeus-matched) universe of European firms financed by euro-area banks.

In column 4, we add a placebo treatment by using the rate cut in July 2012, when the ECB lowers the DF rate from 0.25% to zero. The estimated coefficient on the interaction term *Deposit ratio*$_k$ $\times$ *After*(07/2012)$_t$, is negative and marginally significant, while the estimated coefficient on the interaction *Deposit ratio*$_k$ $\times$ *After*(06/2014)$_t$ remains positive and strongly significant. This lends further support to the idea that the transmission of negative rates to the real sector depends on banks' funding structure, which matters only in times of rate decreases when rates are negative, rather than positive.

In order to supplement our characterization of the nature of the credit supply shock, we next examine the impact of negative policy rates on the volume of bank lending. For this purpose, we move our analysis to the borrower-bank level, and analyze individual banks' participation in firms' syndicated loans, irrespective of whether banks serve as lead arrangers or participants. For each syndicated loan, we use information on each bank's share from DealScan, which underlies our calculation of each bank's total loan amount granted to a firm in a given time period.[2] On this basis, we construct a balanced panel of all borrower-bank pairs at the semi-annual frequency. That is, the unit of observation is bank k's lending to firm j in half-year t.

In the first three columns of Table 3, we implement the same difference-in-differences strategy as before, except that we use each individual euro-area bank's *Deposit ratio*$_k$, and use as dependent variable the natural logarithm of the total loan volume granted to firm j by bank k in half-year t plus one.

After including bank and firm-time fixed effects, we find that high-deposit banks lend less overall, but this effect is explained substantially by time-varying heterogeneity at the

[2] We use loan shares from DealScan whenever available. Otherwise, similar to Chodorow-Reich (2014), we set the total loan share retained by lead arrangers (participants) in the syndicate equal to the sample mean, and divide it equally among all lead arrangers (participants) in the syndicate.

firm level, including but not limited to firms' loan demand (column 1). In columns 2 and 3, we then split the sample by firm risk, and find that the credit contraction is confined to safer firms, which is in line with our results on high-deposit banks' risk taking (see Table 2).

In the last three columns of Table 3, we investigate firms' loan volume granted by any euro-area or non-euro area bank—serving as a lead arranger or a participant in a syndicated loan—as a function of their pre-period banking relationships.

In doing so, we use the same firm-level exposure variable as in the main part of our analysis where we estimate the effect of firms' exposure to negative policy rates through their banking relationships on wage inequality. *Deposit ratio*$_j$ is the average deposit ratio across all euro-area lead arrangers from which firm j received syndicated loans anytime from 2010 to 2013. This implies that we need to drop firm-time fixed effects, so that the estimated treatment effect reflects the average change in a German firm's loan volume from European banks.

Column 4 shows that treated firms—i.e., those in preexisting relationships with high-deposit banks—receive less debt financing in the form of syndicated loans following the introduction of negative policy rates, although the effect is significant only at the 14% level. In columns 5 and 6, we see that this is, again, confined to safer firms for which the effect is significant at the 2% level. In sum, these findings imply that high-deposit banks in the euro area contract their lending to safe German firms, but not to risky ones. In addition, German firms in relationships with high-deposit banks cannot compensate for the loss in loan financing by attaining loans from other, possibly non-euro area banks.

In Table A.1 of the Online Appendix, all of these insights remain to hold true when we replace the dependent variable by an indicator for any involvement of bank k in syndicated loans granted to firm j in half-year t, i.e., when testing whether firms are more or less likely to attain any loans—from any euro-area or non-euro area bank serving as a lead arranger or a participant in a syndicated loan—as a function of their pre-period banking relationships.

We finish with estimating real effects, namely on investment and employment. In Table 4, we move our analysis to the firm-year level, and use the same treatment-intensity variable, *Deposit ratio*$_j$, as in the last four columns of Table 3. That is, we estimate the effect of firms' preexisting banking relationships on investment and employment following the introduction of negative policy rates.

Again, we split firms into ex-ante safe and risky ones. The estimates in the first two columns suggest that while there is no effect for safe firms, risky firms that are exposed to the negative policy rate through their banking relationships experience an increase in investment by 32.3% for a one-standard-deviation increase in the deposit ratio (which is equal to 0.063 in this particular sample). In the last two columns, however, we find no effect on employment for either group of firms. Thus, any effects on wage inequality are unlikely to stem from this margin of adjustment.

5.2 Worker-level Evidence

Having described the nature of the credit supply shock, we now turn to estimating its effect on wage inequality between and within firms.

We start with graphical evidence. Figure 1 plots overall wage growth (blue line), decomposed into its between-firm (red line) and within-firm (green line) components for the difference between treated and control firms, namely firms in the top and bottom quartiles, respectively, of the deposit-ratio distribution of their banking relationships. We employ the above-mentioned sample selection, but keep all (including part-time) workers.

The blue line indicates that the overall treatment effect—the difference between treatment and control firms that are differentially affected by the introduction of negative policy rates—on wage growth is negative. That is, workers at treated firms have seen their earnings grow less than those at control firms. There is, however, considerable heterogeneity. Workers at the bottom of the wage distribution experience positive but volatile wage growth, allowing them to catch up to the middle. At the same time, previously

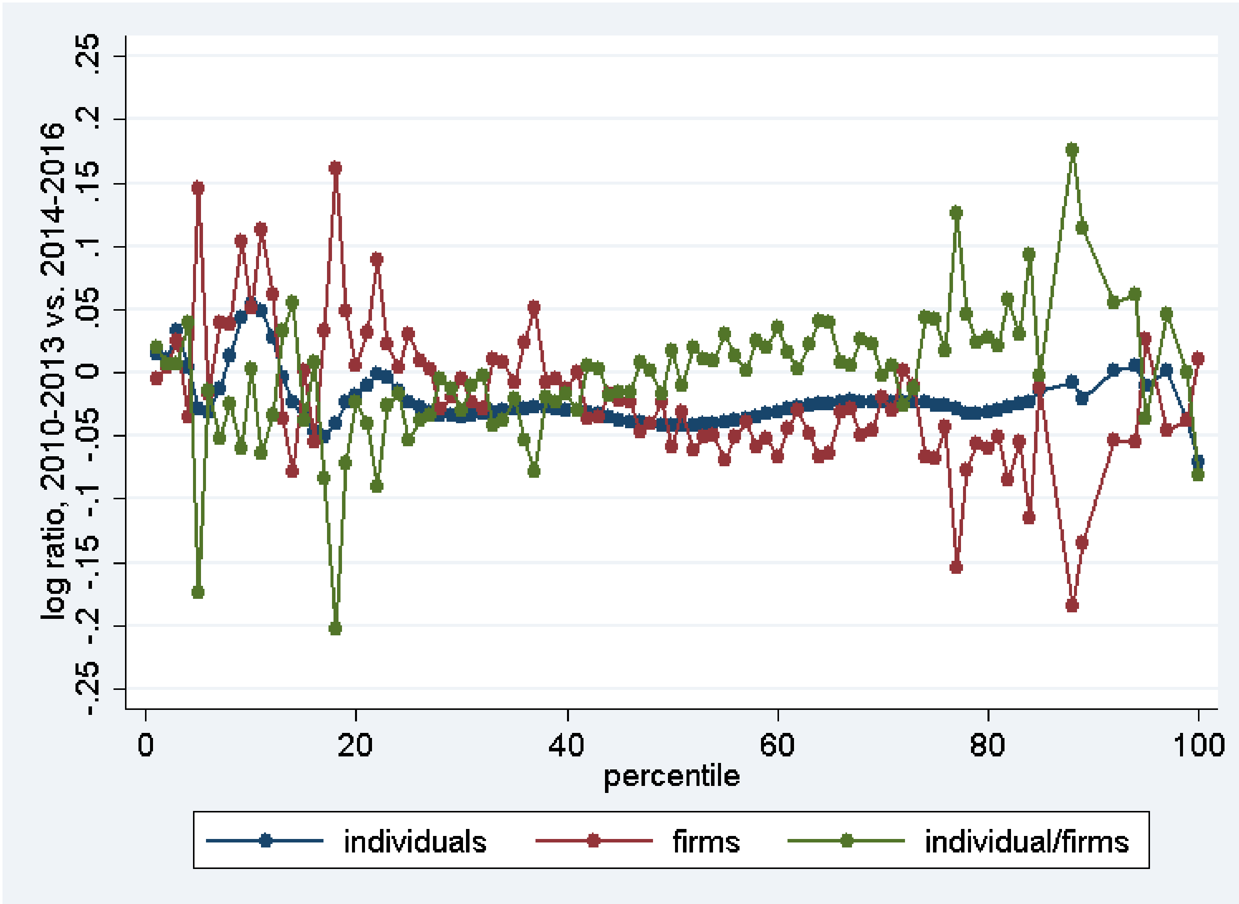

Figure 1: Decomposition of Treatment Effect on Within-firm and Between-firm Wage Inequality. For firms in the top and bottom quartiles of the deposit-ratio distribution of their banking relationships, this figure plots the difference in the average logarithm of the ratio of the average annualized wage from $2010 - 2013$ vs. $2014 - 2016$ (blue line), and decomposes it into its between-firm (red line) and within-firm (green line) components. The sample is limited to all employees at German corporations that are active in the syndicated-loan market, as recorded in DealScan.

already high-paid workers drift further away from the middle.

The green and red lines decompose this overall treatment effect on wage growth into its within-firm and between-firm components, respectively. In particular, the green line is negative at low income percentile and positive at high income percentiles. This positive slope indicates a drastic increase in within-firm inequality. Low-paid workers at treated firms, as compared to untreated firms, have seen their wages grow by less than high-paid workers in those same firms.

In contrast, the red line is positive at low income percentiles and negative at high income percentiles. This negative slope reflects a reduction in between-firm inequality.

This can imply one or both of the following developments. First, treated firms that previously employed low-paid workers have raised wages more than untreated firms, while those that previously employed high-paid workers have raised wages less than untreated firms. Second, in treatment vs. control firms, workers that used to earn low wages switch to firms that generally pay more (i.e., without necessarily adjusting their average pay).

We now turn to regressions that lend further support to these two main findings. In order to assess the effect of negative policy rates on wage inequality at the worker level, we modify regression specification (1). In particular, we define *Position*$_i$ as worker i's position in the wage distribution of firm j or elsewhere in the pre-period, when policy rates in the euro area are still nonnegative. We then estimate the following specification:

$$\begin{aligned} y_{ijt} = \; & \beta_1 Deposit\ ratio_j \times After(2014)_t \times Bottom\ 20\%_i \\ & + \beta_2 Deposit\ ratio_j \times After(2014)_t \times Middle\ 60\%_i \\ & + \beta_3 Deposit\ ratio_j \times Bottom\ 20\%_i + \beta_4 Deposit\ ratio_j \times Middle\ 60\%_i \\ & + \beta_5 After(2014)_t \times Bottom\ 20\%_i + \beta_6 After(2014)_t \times Middle\ 60\%_i \\ & + \mu_{jt} + \theta_i + \varepsilon_{ijt}, \end{aligned} \tag{4}$$

where y_{ijt} is worker i's employment outcome—e.g., her annualized wage—at firm j in year t, *Bottom* $20\%_i$ (*Middle* $60\%_i$) is an indicator variable for whether worker i's wage is in the bottom 20% (middle 60%) of the wage distribution of firm j where i was employed, or elsewhere, in the last available year during the pre-period from 2010 to 2013, *Deposit ratio*$_j$ is the average deposit ratio of all euro-area banks that firm j received a syndicated loan from anytime from 2010 to 2013, $After(2014)_t$ is a dummy variable for the post-period from 2014 to 2016, μ_{jt} denotes firm-time fixed effects, and θ_i are individual fixed effects.

The coefficients of interest are β_1 and β_2 which capture to what extent firms' exposure to negative policy rates through their (pre-period) banking relationships differentially affects employment outcomes of workers at the bottom 20% and middle 60% of the wage distribution in comparison to the top 20%.

We always include individual fixed effects, θ_i, controlling for time-invariant heterogeneity at the individual worker level. In addition, firm-year fixed effects, μ_{jt}, control for time-varying heterogeneity at the firm level, such as firm-wide developments that may be correlated with firms' heterogenous exposure to negative policy rates through their banking relationships.

In order to ascertain any effect on wage inequality, we use as dependent variable the logged annualized wage of individual i that works at firm j in year t. Note that firm j does not vary within a worker-year it as we pick only the most important wage source in case of i working at multiple firms in a given year.

In the absence of firm(-year) fixed effects, β_1 and β_2 effectively capture the shape of the blue line in Figure 1. The blue line is somewhat more positive at the lower end of the wage distribution, indicating that β_1 should be positive and larger than β_2.

Figure 1 suggests that this reduction in wage inequality is driven primarily by a reduction in between-firm wage inequality. Once we add firm-year fixed effects, we control for firm-level changes in employment. Such firm-level changes comprise, but are not limited to, changes in wage policies, e.g., an increase in the average wage paid by treated and untreated firms, irrespective of their initial level of pay.

If one additionally controls for worker-firm fixed effects, then the treatment effect is identified off individuals that work at the same firm before and after the introduction of negative policy rates in 2014. Should a (more) positive coefficient on β_1 (rather than on β_2) persist after the inclusion of worker-firm fixed effects, then this would suggest that the reduction in between-firm wage inequality could—at least partly—be explained by firms with low-paid workers starting to pay them more, rather than by previously low-paid workers switching to firms that generally pay more than their previous employers.

In Table 5, we estimate regression specification (4) to assess the differential effect of firms' exposure to the introduction of negative policy rates in 2014 through their banking relationships on the natural logarithm of nonzero wages of workers whose wages in the pre-period put them in the top 20%, middle 60%, and bottom 20%.

In columns 1 and 2, we start with the average effect on wages across treatment and control firms. Employees at high-deposit banks tend to earn less after the introduction of negative policy rates, but the effect is not statistically significant and explained largely by time-invariant unobserved heterogeneity at the worker level (column 2).

In columns 3 and 4, we include interaction terms with workers' positions in the pre-period wage distribution. Doing so, we find that even after including firm-year fixed effects in column 4, individuals that used to earn a wage in the bottom 20% of their respective firms' wage distribution experience significantly greater positive wage growth at treated firms after the introduction of negative policy rates than the top 20% (the omitted category).

These estimates suggest that the observed reduction in between-firm wage inequality is consistent with both initially low-paying firms increasing their average pay and low-paid workers switching to firms that generally pay more. However, the actual explanatory power of the latter depends on the frequency with which workers switch firms.

In the last column, our estimates hold up to additionally controlling for worker-firm fixed effects. This implies that the observed reduction in between-firm wage inequality is consistent with the notion that firms with low-paid workers start paying them more, rather than workers switching firms.

In Table 6, we replace the dependent variable by an indicator variable that equals one if worker i is no longer employed at any firm in year $t + 1$. In this manner, we find that the inequality-reducing treatment does not only hold in terms of wages of employed individuals, but also in terms of individuals in the bottom 20% of the wage distribution at treated firms facing lower risk of being laid off than workers in the top 20%.

In Table 7, these insights generally remain to hold true when we replace *Bottom* 20% (*Middle* 60%) *within firm*$_i$ by an indicator variable for whether worker i's wage is in the bottom 20% (middle 60%) of the wage distribution across all firms in the sample in the last available year during the pre-period from 2010 to 2013. However, the differential treatment of the bottom 20% vs. top 20% (the omitted category) becomes significant only after the inclusion of firm-year fixed effects. Furthermore, the significantly lower risk of being laid off applies more to the middle 60%, as compared to the top 20%, rather than the bottom 20%.

Our estimates surviving the inclusion of firm-year fixed effects suggests that workers switching firms *can* contribute to the observed decline in between-firm wage inequality, but offers no indication as to *how much*. Analogously, our inequality-reducing treatment effect holding up to the inclusion of worker-firm fixed effects suggests that the decline in between-firm wage inequality could also be driven by firms with low-paid workers starting to pay them more. However, a pressing issue is to quantify the explanatory power of each one of these channels.

We will address explicitly this shortcoming of our reduced-form framework in a decomposition exercise below. The main reason for why our present framework cannot quantify the explanatory power of each channel separately is that the treatment effect is identified using different samples according to the set of fixed effects employed.

This is best illustrated by considering the inequality-reducing treatment effect in the last column of Tables 5, in the presence of worker-firm fixed effects. In that sample, the treatment effect is identified off workers staying at their respective firms in the pre- and post-period. If there were no new entrants into firms, then this result would also imply lower *within-firm* wage inequality.

As we have already seen in Figure 1, the opposite is the case. Besides pointing out the importance of general time-varying trends at the firm level, this suggests that the increase in within-firm wage inequality should be driven by the wage distribution of new entrants into treatment vs. control firms.

5.3 Decomposition of the Reduction in Between-firm Wage Inequality

As discussed above, the inequality-reducing treatment effect that we capture so far holds up to the inclusion of firm-year fixed effects. This suggests that the reduction in between-firm wage inequality that we document can, in part, be due to formerly low-paid workers switching to firms that pay them more.

On the other hand, the treatment effect is similarly strong after including worker-firm fixed effects, which suggests that the reduction in between-firm wage inequality would also be consistent with low-paid workers staying at firms that now start paying them more.

That is, while our evidence rules in both explanations, it does not indicate their relative importance for the observed decline in between-firm wage inequality. To shed light on this issue, we extend the methodological approach from Alvarez, Benguria, Engbom, and Moser (2018) to our quasi-experimental setting by introducing a two-stage regression framework that enables us to disentangle different sources of between-firm wage inequality.

In our first stage, we model wages for individual i working at firm j in year t, denoted $\ln(wage_{ijt})$ as:

$$\ln(wage_{ijt}) = \beta X_{it} + \psi_{J(i,t)t} + \varepsilon_{it}, \tag{5}$$

where X_{it} contains worker controls, and $\psi_{J(i,t)t}$ is a firm-year fixed effect with the function $J(i,t)$ identifying the employer of worker i in year t.

We include in X_{it} dummies for workers' gender, linear and quadratic terms in age, interacted with education dummies, and occupation dummies.

The results of estimating (5) are in Table A.2 of the Online Appendix. We do not include individual fixed effects to avoid spurious negative correlation bias between worker and firm-year fixed effects (Andrews, Gill, Schank, and Upward (2008)).

In our second stage, we wish to relate between-firm wage inequality to the treatment due to firms' exposure to negative policy rates through their (pre-period) banking relationships. To this end, we start with a worker-level dataset with estimated firm-year fixed effects $\widehat{\psi}_{J(i,t)t}$, based on column 2 of Table A.2.

To test for effects on between-firm inequality, we are interested in how the treatment affects the distribution of firm average wages, $\widehat{\psi}_{J(i,t)t}$. For this purpose, we estimate the following regression specification at the worker-year level:

$$\begin{aligned} \widehat{\psi}_{J(i,t)t} = \; & \beta_1 Deposit\ ratio_{J(i,t)} \times After(2014)_t \times Firm\ rank_{J(i,t)} \\ & + \beta_2 Deposit\ ratio_{J(i,t)} \times After(2014)_t + \beta_3 Firm\ rank_{J(i,t)} \times After(2014)_t \\ & + \theta_i + \mu_j + \delta_t + \eta_{J(i,t)t}, \end{aligned} \tag{6}$$

where $\widehat{\psi}_{J(i,t)t}$ is the estimated firm-year effect based on column 2 of Table A.2, *Deposit ratio*$_{J(i,t)}$ is the average deposit ratio of all euro-area banks that firm j, at which individual i works at time t, received a syndicated loan from anytime from 2010 to 2013, *After*(2014)$_t$ is a dummy variable for the post-period from 2014 to 2016, *Firm rank*$_{J(i,t)}$ is the rank (from 0 = lowest to 1 = highest) of firm $j = J(i,t)$ in the distribution of average firm-year effects during the pre-period from 2010 to 2013, θ_i and μ_j denote individual and firm fixed effects, respectively, and δ_t denotes year fixed effects.

We are interested in the estimate of β_1 in equation (6), which we interpret as the treatment effect on pay at previously higher-paying vs. lower-paying firms. If $\beta_1 < 0$, then initially lower-paying firms increase their pay by relatively more compared to initially higher-paying firms, hence the treatment leads to a decline in between-firm pay inequality. Conversely, $\beta_1 > 0$ implies an increase in between-firm inequality due to the treatment.

The results from estimating (6) are in the first two columns of Table 8. On average, firms in relationships with high-deposit banks pay less after the introduction of negative policy rates (column 1).

However, there is considerable heterogeneity as a function of firms' pre-period pay policies. In particular, the estimates in column 2 suggest that following the introduction of negative policy rates, lower-paying firms in relationships with high-deposit banks increase their pay by relatively more than higher-paying firms, thereby reducing between-firm wage inequality.[3]

Our two-stage regression approach first identifies firm-specific pay components, and then correlates these estimates to firms' exposure to negative policy rates through their banking relationships, the effect of which we allow to differ throughout the initial firm-pay ranks. The second-stage regression therefore isolates the treatment-induced changes in the wage structure that could come about through two alternative channels. The first channel entails workers moving between firms that differ in their pay policies. The second channel entails firms changing their pay policies for a given distribution of workers across employers.

To isolate the effects of worker mobility separately from the effects of changes in firm pay policies, we conduct two counterfactual experiments, each holding one of the two channels fixed while allowing the other to vary. Specifically, in our first counterfactual experiment, we allow for worker mobility as observed in the data, but hold fixed firms' pay policies in the last year of the pre-period. Conversely, in our second counterfactual experiment, we fix worker mobility, but allow firm pay policies to change as observed in the data.

We then ask how each of the two counterfactual distributions of estimated firm-pay components evolves over time, relative to the baseline second-stage regression results in column 2 of Table 8.

Comparing the results of each counterfactual with the baseline results suggests which of the two channels, worker mobility or changes in firm pay policies, is more important in explaining the observed changes in the overall distribution of firm-pay components. Of course, there could be interaction effects between the two counterfactual exercises, which this approach will not capture. Due to such nonlinearities, our counterfactual decomposition of the overall effect need not add up to the overall effect from the baseline.

[3] These results are virtually invariant to omitting firm fixed effects in both regressions.

Decomposition 1. For every firm j, fix across all years its value of $\widehat{\psi}_{J(i,t)t}$ in year $t = 2013$, which we denote $\tilde{\psi}_{J(i,t)}$. We then estimate the following regression:

$$
\begin{aligned}
\tilde{\psi}_{J(i,t)} \quad = \quad & \tilde{\beta}_1 Deposit\ ratio_{J(i,t)} \times After(2014)_t \times Firm\ rank_{J(i,t)} \\
& + \tilde{\beta}_2 Deposit\ ratio_{J(i,t)} \times After(2014)_t + \tilde{\beta}_3 Firm\ rank_{J(i,t)} \times After(2014)_t \\
& + \tilde{\beta}_4 Deposit\ ratio_{J(i,t)} \times Firm\ rank_{J(i,t)} + \tilde{\beta}_5 Deposit\ ratio_{J(i,t)} \\
& + \tilde{\beta}_6 Firm\ rank_{J(i,t)} + \theta_i + \delta_t + \eta_{J(i,t)t}. \qquad (7)
\end{aligned}
$$

Note that firm fixed effects cannot be included in this regression as the dependent variable is time-invariant within firms.

The above equation relates changes in the firm component of pay to the pattern of workers switching firms. We are interested in the estimate of $\tilde{\beta}_1$, which captures the differential effects on firm pay due to only worker mobility.

The respective estimation results are in the third and fourth column of Table 8. After holding firm pay policies fixed, there is no economically meaningful (albeit statistically significant) treatment effect—neither on average nor for previously higher-paying vs. lower-paying firms. This suggests that the reduction in between-firm wage inequality documented in the first two columns is primarily due to changes in firm pay policies, and not due to worker mobility.

Decomposition 2. To assess the importance of changes in firm pay policies, we limit the sample to workers i that spent the same number of years at firm j as the respective firm exists in the sample period. We then estimate the following regression:

$$
\begin{aligned}
\widehat{\psi}_{J(i,t)t} \quad = \quad & \tilde{\tilde{\beta}}_1 Deposit\ ratio_{J(i,t)} \times After(2014)_t \times Firm\ rank_{J(i,t)} \\
& + \tilde{\tilde{\beta}}_2 Deposit\ ratio_{J(i,t)} \times After(2014)_t + \tilde{\tilde{\beta}}_3 Firm\ rank_{J(i,t)} \times After(2014)_t \\
& + \theta_i + \delta_t + \eta_{J(i,t)t}, \qquad (8)
\end{aligned}
$$

which is in essence the same regression specification as (6). However, in the presence of individual fixed effects θ_i, one can no longer estimate firm fixed effects μ_j as we limit the sample to stayers, which eliminates all time-invariant firm-level variation.

The above equation relates changes in the firm component of pay to the within-firm changes in pay. We are interested in the estimate of $\tilde{\tilde{\beta}}_1$, which captures the differential effects on firm pay due to only firms adjusting their pay policies.

As can be seen in the last two columns of Table 8, firms in relationships with high-deposit banks adjust their average pay downwards after 2014. However, firms with initially lower levels of pay significantly more following the introduction of negative policy rates. Our sample-selection criterion is rather restrictive.[4]

The negative average effect on wages at firms in relationships with high-deposit, rather than low-deposit, banks is in line with the reduced lending by high-deposit banks following the introduction of negative policy rates (see Table 3). While high-deposit banks lend less overall, they do lend relatively more to risky than to safe firms (see Tables 2 and 3). This indicates a reallocation of credit towards risky, potentially more constrained firms. Indeed, firms with higher ROA and stock-return volatilities tend to have a lower *Firm rank*$_{J(i,t)}$ in our data.

Alternatively, low-wage firms may be unable to reduce their pay in response to the credit supply shock because they hit minimum-wage boundaries and therefore cannot lower wages within the law. Coinciding with two full years of our post-period, a minimum-wage reform was implemented in Germany on January 1, 2015, introducing a minimum wage (a gross hourly wage of at least 8.50 €) for the first time in the country.

To show that risky firms are constrained to pay higher wages on average, and that this constraint is relaxed in comparison to safe firms following the treatment, we replace *Firm rank*$_{J(i,t)}$ by *Risk rank*$_{J(i,t)}$, which is the rank (from 0 = lowest to 1 = highest) of firm $j = J(i,t)$ in the distribution of the average ROA volatility of loan-financed firm i during the pre-period from 2010 to 2013. The results in Table 9 are qualitatively and quantitatively

[4] These insights are invariant even when we use a less strict criterion, such as limiting the sample to workers who stay at one firm their entire career, of which the sample in the last two columns of Table 8 is a subset.

similar to those in Table 8, indicating that risky firms are less likely to adjust their average pay downwards thanks to the treatment.

This holds true also for the subset of publicly listed firms when we define *Risk rank*$_{J(i,t)}$ based on the distribution of the average standard deviation of firm i's monthly stock returns over 36 months during the pre-period from 2010 to 2013 (Table A.3).

Therefore, the lending behavior of high-deposit banks following the introduction of negative policy rates contributes to a reduction in between-firm wage inequality in the following way. On the one hand, high-deposit banks lend less than do low-deposit banks, and this credit contraction leads to an overall decrease in average wages at firms connected to high-deposit banks. However, risky firms that are previously constrained to pay higher wages receive relatively more loans from high-deposit banks.

This reallocation of credit towards risky firms relaxes their constraints, enabling them to pay relatively more than other firms in relationships with high-deposit banks. Finally, we show that between worker mobility and changes in firm pay policies, only the latter has the potential to explain the observed decline in between-firm wage inequality stemming from the treatment.

5.4 Firm-level Evidence on Within-firm Wage Inequality

So far, we have estimated the treatment effect of negative policy rates on low-paid vs. high-paid workers at treated vs. control firms, which corresponds to the blue line in Figure 1. We have also presented evidence that the overall inequality-reducing effect is symptomatic of a reduction in between-firm inequality, in line with the downward-sloping red line in said figure.

The green line in Figure 1 suggests that within-firm wage inequality simultaneously increases. This development is reflected in our previous estimates, but it is overshadowed by the reduction in between-firm inequality. An increase in within-firm wage inequality at firms in relationships with high-deposit, rather than low-deposit, banks would be consistent with the overall contraction in lending by high-deposit banks, forcing them to cut (some) wages.[5]

To specifically assess the treatment effect on within-firm wage inequality, we need to move the analysis to the firm-year level jt. We start with graphical evidence at this less granular level (compared to our worker-level evidence). In Figure 2, we plot the average difference in the logarithm of the annualized wage at the 95$^{\text{th}}$ vs. 5$^{\text{th}}$ percentile, separately for treated and control firms. As before, we discretize the otherwise continuous exposure to treatment by considering firms in preexisting relationships with banks in the top and bottom quartiles of the deposit-ratio distribution.

We observe parallel movements in within-firm wage inequality during the entire pre-period from 2010 to 2013, with firms in relationships with high-deposit banks exhibiting at most 10% higher inequality. This gap widens significantly, to 20%, in the post-period starting in 2014.

To show this more formally, we estimate regression specification (2), and use as dependent variable the difference in the logarithm of the annualized wage at the 95$^{\text{th}}$ vs. 5$^{\text{th}}$ percentile of firm j's wage distribution in year t. The results are in the top panel of Table 10. In the first column, the coefficient on our difference-in-differences estimator is positive and significant. In line with Figure 2, firms in relationships with high-deposit banks exhibit higher within-firm wage inequality after the introduction of the negative policy rate, compared to firms in relationships with low-deposit banks. This result is robust to the inclusion of state-year fixed effects, controlling for time-varying unobserved heterogeneity at the level of the state in which firm j is incorporated.

In the second column, we run the same specification as in the first column, but add an interaction term between *After*(2014)$_t$ and *Non-euro area deposit ratio*$_j \in [0,1]$, which captures the average deposit ratio across all *non-euro area* lead arrangers that firm j received a syndicated loan from in the pre-period. This is a falsification test as these banks are

[5] Note that this would not necessarily be at odds with the fact that within the group of firms in relationships with high-deposit banks, risky, potentially constrained firms receive more loan financing, and subsequently do not reduce their wages as much.

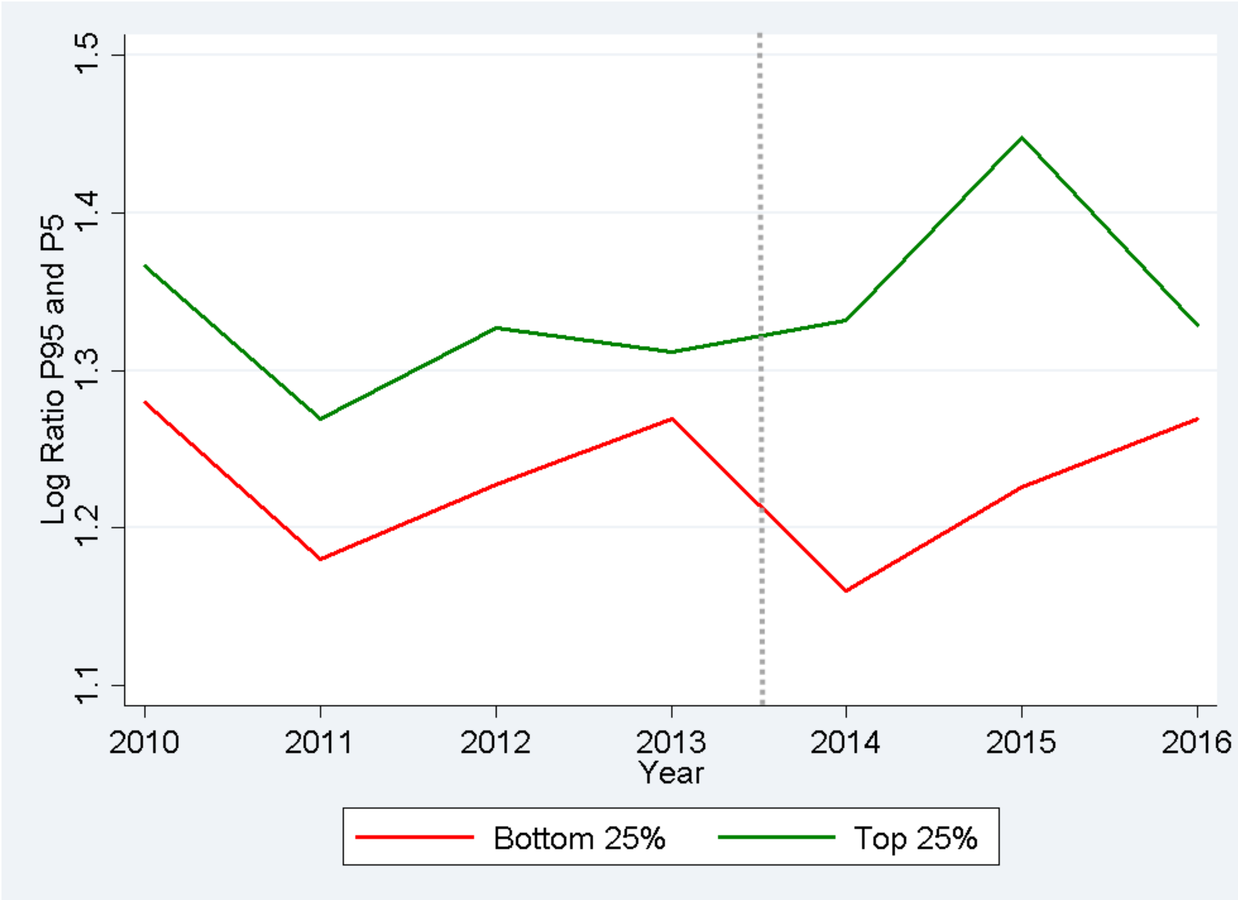

Figure 2: Within-firm Wage Inequality at Treated vs. Control Firms. This figure plots the average difference in the logarithm of the annualized wage at the 95$^{\text{th}}$ vs. 5$^{\text{th}}$ percentile over time, separately for firms in the top and bottom quartiles of the deposit-ratio distribution of their banking relationships. The sample is limited to German corporations that are active in the syndicated-loan market, as recorded in DealScan.

not affected by euro-area monetary policy. The respective coefficient amounts to only one-fifth of our difference-in-differences estimate, and is statistically insignificant.

In the last two columns of Table 10, we split the sample into privately-held and publicly-listed firms, and find that the increase in within-firm inequality prevails primarily at private, rather than public, firms. As we will argue later, this is likely due to the fact that topcoding in our wage data is less severe at private firms.

All of these findings remain to hold true when we replace the dependent variable by the difference in the logarithm of the annualized wage at the 90$^{\text{th}}$ vs. 10$^{\text{th}}$ percentile of firm j's wage distribution in year t. As can be seen in the bottom panel of Table 10, the difference between our treatment effect stemming from euro-area banks' as opposed to non-euro area banks' deposits (as a falsification test) becomes even more emphasized.

We next revisit an implication of our worker-level results, namely that newly hired workers are more important for the development of within-firm wage inequality than for between-firm wage inequality. This conjecture is based on two observations. First, we document an inequality-decreasing treatment effect on between-firm wage inequality that persists even after including worker-firm fixed effects. As in column 5 of Tables 5 and 6 identification is achieved through stayers, the inequality-decreasing treatment effect would imply lower within-firm inequality if it was not for the distribution of wages of newly hired workers.

Given that we do find an inequality-increasing treatment effect on within-firm inequality in Table 10, however, this implies that the latter must be driven nontrivially by the distribution of wages of newly hired employees. As not all of these workers are switchers but, in fact, new entrants into the labor market with no prior track record, the firm-year level is the appropriate level of observation for us to test this conjecture.

For this purpose, we run the same regressions as in Table 10, but re-calculate the dependent variable on the basis of all full-time employees at firm j except for newly hired workers in year t. The results are in Table 11. In line with our conjecture, the coefficients on *Deposit ratio*$_j$ $\times$ *After*(2014)$_t$ in the first three columns are much smaller (sometimes even halved) and are all insignificant. As can be seen in the last column, the inequality of the wage distribution for new employees holds the largest explanatory power for the treatment effect on wage inequality in publicly-listed firms.

In Table A.4 of the Online Appendix, we find that the increase in within-firm wage inequality at treated vs. control firms is driven primarily by lower wages at the bottom of the within-firm wage distribution. This is consistent with a rational response by treated firms facing lower (average) credit supply. As the labor market for top managers is more competitive, and higher wages are an important means of attracting such talent, the wages of new hires at the bottom are more likely to be rationed.

So far, we have failed to find any significant effect on wage inequality in publicly-listed firms. This may in part be due to the fact that while very rich, the administrative linked employer-employee (ORBIS-ADIAB) wage data are censored from above. This type of topcoding may affect many public firms. To deal with such concerns, and to scrutinize to what extent the observed increase in within-firm wage inequality may be driven by the very top wages, we enrich our firm-level analysis by considering executive compensation (see also Bertrand and Schoar (2003)).

To this end, we merge our wage data with executive-compensation data from BoardEx (and do so via the Amadeus bridge available to us). We use total compensation for all executive board members by dropping data for board members whose roles are merely representative (e.g., employee representatives) or of other nonexecutive nature. In this manner, we yield information on 26 (typically public) firms in Germany.

In the first two columns of Table 12, we run the same regressions as in Table 10, but replace the dependent variable by the natural logarithm of the ratio of the median total compensation of executive board members to the median wage (top panel) and to the bottom 5% wage (bottom panel) of the same firm in our administrative data.[6] We yield a positive coefficient that has grown considerably in size for our treatment effect.

In the last two columns of Table 12, we dissect the median total compensation of executive board members into a salary component and any bonus component. In comparison to the estimates in the second column, the increase in within-firm wage inequality is most emphasized when considering the ratio between the median bonus of executive board members and wages of other employees in our administrative data (column 4).

These results imply that not only privately-held firms that are more exposed to the negative policy rate through their banking relationships exhibit greater within-firm wage inequality after 2014 (as seen in column 3 of Table 10), but we are able to detect a similar effect among publicly-listed firms once we incorporate compensation data for a population

[6] We drop state-year fixed effects from these estimations for the sake of maintaining a larger sample, although all results are robust to the inclusion of state-year fixed effects.

that more closely resembles these firms' top earners. This also reconciles our findings with general evidence in the United Kingdom that larger, publicly-listed firms exhibit greater within-firm wage inequality (Mueller, Ouimet, and Simintzi (2017)).

6 Conclusion

In this paper, we study the effect of credit supply on wage inequality. We exploit the introduction of negative monetary policy rates in the euro area combined with banks' balance-sheet exposure and preexisting lending relationships as a source of variation in credit supply to firms in Germany. We find that this credit supply shock has substantial effects on the distribution of wages between and within employers.

On the one hand, the credit supply shock leads to a reduction in between-firm wage inequality. This first effect is due to average wages increasing more among initially lower-paying firms exposed to the shock. On the other hand, within-firm wage inequality at treated firms increases. This second effect operates through a disproportionate rise in compensation for executives, as well as a wider range of wages for newly hired employees at firms more affected by the credit supply shock.

Besides providing evidence from micro data on the link between credit supply and wage inequality, our results highlight firm pay heterogeneity as a novel channel through which monetary policy can have important distributional consequences.

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7 Tables

Table 1: Summary Statistics

Variable	Mean	Median	Standard deviation	Min	Max	# firms
Panel A: German firms with banking information in DealScan						
Deposit ratio	0.363	0.362	0.079	0.024	0.645	293
Non-euro area deposit ratio	0.159	0	0.216	0	0.678	293
Sales in million €	5,449.143	635.545	18,218.501	0.007	197,007	283
Assets in million €	6,554.497	577.562	26,643.666	0.473	324,333	293
ROA volatility	0.047	0.036	0.041	0.002	0.256	260
# employees in thousands	2.288	0.278	11.381	0.001	131.742	293
Average wage in thousand €	49.858	50.561	10.146	9.033	69.598	293
Proportion female	0.300	0.288	0.179	0	0.800	293
Proportion foreigners	0.061	0.043	0.070	0	0.500	293
Proportion university	0.332	0.274	0.258	0	1	293
Panel B: German firms related to banks in the highest quartile of the deposit-ratio distribution						
Sales in million €	2,599.459	258.817	9,603.690	25.326	73,973	71
Assets in million €	2,019.115	211.014	7,782.365	16.255	64,204	74
ROA volatility	0.060	0.042	0.050	0.007	0.256	66
# employees in thousands	0.857	0.187	3.365	0.001	28.288	74
Average wage in thousand €	47.717	48.031	10.581	15.646	64.698	74
Proportion female	0.311	0.281	0.196	0	0.800	74
Proportion foreigners	0.052	0.048	0.051	0	0.294	74
Proportion university	0.301	0.189	0.268	0	1	74
Panel C: German firms related to banks in the lowest quartile of the deposit-ratio distribution						
Sales in million €	2,369.729	495.064	8,336.613	11.295	60,132	72
Assets in million €	2,993.536	414.310	14,076.203	2.705	118,148	76
ROA volatility	0.037	0.032	0.026	0.003	0.123	66
# employees in thousands	1.307	0.352	4.512	0.002	38.323	76
Average wage in thousand €	48.743	49.041	9.471	27.553	69.598	76
Proportion female	0.315	0.299	0.197	0	0.727	76
Proportion foreigners	0.067	0.043	0.084	0	0.500	76
Proportion university	0.314	0.262	0.244	0.012	1	76

Notes: This table shows firm-level summary statistics for the last pre-treatment year 2013. Panel A includes all German corporations in the administrative linked employer-employee (ORBIS-ADIAB) data with available banking data, in particular on the deposit ratio of the lead arrangers from which they attained loans in the pre-treatment period from 2010 to 2013. Panels B and C split up these firms that are active in the syndicated-loan market into those in the top and bottom quartile, respectively, of the distribution of *Deposit ratio*$_j$, which is the average deposit ratio (measured in 2013) across all euro-area lead arrangers from which firm j received syndicated loans anytime in the pre-treatment period from 2010 to 2013.

Table 2: ROA Volatility of German Firms Financed by Banks Following Negative Policy Rates

	$\ln(\sigma(\text{ROA})^{6y})$			
Sample	$2013-2015$		$2010-2016$	
Variable	(1)	(2)	(3)	(4)
Deposit ratio × After(06/2014)	2.130**	2.389**	2.012**	2.817***
	(1.076)	(1.105)	(0.915)	(1.019)
Deposit ratio × After(07/2012)				-0.014*
				(0.008)
Bank FE	Y	Y	Y	Y
Month-year FE	Y	Y	Y	Y
Industry-year FE	N	Y	Y	Y
N	346	314	568	568

Notes: The sample consists of all completed syndicated loans (package level) of German corporations j at date t granted by any euro-area lead arranger(s) k, from January 2013 to December 2015 in the first two columns and from January 2010 to December 2016 in the last two columns. The dependent variable is the logged six-year standard deviation of firm j's return on assets (ROA, using P&L before tax) from year $t-5$ to t associated with firm j's loan provided by loan syndicate k at time t. *Deposit ratio*$_k \in [0,1]$ is the average ratio (in %) of deposits over total assets in 2013 across all euro-area lead arrangers of syndicate k. *After*(06/2014)$_t$ is a dummy variable for the period from June 2014 onwards. *After*(07/2012)$_t$ is a dummy variable for the period from July 2012 onwards. Bank fixed effects are included for all euro-area lead arrangers. Industry-year fixed effects are based on two-digit SIC codes. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the bank level) are in parentheses.

Table 3: Impact of Negative Policy Rates on Lending to German Firms

	ln(1+Total loan volume)					
Lenders	Euro-area banks			All European banks		
Sample	2010 – 2016	Low ROA volatility	High ROA volatility	2010 – 2016	Low ROA volatility	High ROA volatility
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Deposit ratio × After(06/2014)	-0.746	-1.430**	-0.261	-1.894	-7.888**	-0.741
	(0.632)	(0.627)	(0.872)	(1.260)	(3.201)	(2.075)
Bank FE	Y	Y	Y	Y	Y	Y
Firm-time FE	Y	Y	Y	N	N	N
Firm FE	N	N	N	Y	Y	Y
Time FE	N	N	N	Y	Y	Y
N	20,384	6,580	6,580	23,296	6,874	6,846

Notes: Based on all (participating and lead) banks' shares of completed syndicated loans of German corporations j anytime from January 2010 to December 2016, the sample is extended so as to represent a balanced panel of all borrower-bank pairs at the semi-annual frequency. Time therefore refers to the semi-annual level. Furthermore, the sample in the first three columns (last three columns) includes all euro-area (European) banks that lend to German firms anytime during the sample period. All singletons are dropped from the total number of observations N. In the second and fifth (third and sixth) column, the sample is split into borrower firms in the bottom (top) third of the distribution in terms of firms' ROA volatility. The dependent variable is the total loan volume granted to firm j by (participating or lead) bank k. In the first three columns, *Deposit ratio*$_k \in [0, 1]$ is euro-area bank k's ratio (in %) of deposits over total assets in 2013. In the last three columns, *Deposit ratio*$_j \in [0, 1]$ is the average deposits-to-assets ratio (in %) across all euro-area banks from which firm j received syndicated loans anytime from 2010 to 2013. *After*(06/2014)$_t$ is a dummy variable for the period from June 2014 onwards. Public-service, energy, and financial-services borrower firms are dropped. Robust standard errors (clustered at the bank level) are in parentheses.

Table 4: Real Effects of Negative Policy Rates

	$\ln(Investment_t)$		$\ln(Employment_t)$	
Sample	Low ROA volatility	High ROA volatility	Low ROA volatility	High ROA volatility
Variable	(1)	(2)	(3)	(4)
Deposit ratio $\times$ After(06/2014)	1.982	5.129**	0.451	-0.078
	(2.582)	(2.453)	(0.784)	(0.754)
Firm FE	Y	Y	Y	Y
Month-year FE	Y	Y	Y	Y
N	444	329	690	659

Notes: The unit of observation is the firm-year level jt, for German corporations j from January 2010 to December 2016. All singletons are dropped from the total number of observations N. In the first and third (second and fourth) column, the sample is split into borrower firms in the bottom (top) third of the distribution in terms of firms' ROA volatility. In the first two columns, the dependent variable is the natural logarithm of firm j's investment in year t, where investment is measured as the difference in tangible fixed assets between year t and $t-1$. In the last two columns, the dependent variable is the natural logarithm of firm j's number of employees. *Deposit ratio*$_j \in [0, 1]$ is the average deposits-to-assets ratio (in %) across all euro-area banks from which firm j received syndicated loans anytime from 2010 to 2013. *After*(06/2014)$_t$ is a dummy variable for the period from June 2014 onwards. Public-service, energy, and financial-services firms are dropped. Robust standard errors (clustered at the firm level) are in parentheses.

Table 5: Effect of Negative Policy Rates on Wages

	ln(Annualized wage)				
Variable	(1)	(2)	(3)	(4)	(5)
Deposit ratio × After(2014) × Bottom 20% within firm			0.722**	0.640***	0.652***
			(0.290)	(0.243)	(0.223)
Deposit ratio × After(2014) × Middle 60% within firm			0.200	0.228	0.231
			(0.244)	(0.208)	(0.189)
Deposit ratio × After(2014)	-0.349	-0.027	-0.234		
	(0.268)	(0.271)	(0.340)		
Deposit ratio × Bottom 20% within firm			0.622	0.710	
			(0.642)	(0.678)	
Deposit ratio × Middle 60% within firm			-0.005	-0.118	
			(0.355)	(0.367)	
After(2014) × Bottom 20% within firm			-0.280***	-0.261***	-0.267***
			(0.103)	(0.089)	(0.082)
After(2014) × Middle 60% within firm			-0.061	-0.065	-0.067
			(0.087)	(0.076)	(0.069)
Worker FE	N	Y	Y	Y	N
Firm FE	Y	Y	Y	N	N
Worker-firm FE	N	N	N	N	Y
Year FE	Y	Y	Y	N	N
Firm-year FE	N	N	N	Y	Y
N	5,533,824	5,336,619	4,884,518	4,884,455	4,881,876

Notes: The sample consists of full-time employees i at German corporations j that are active in the syndicated-loan market in year t from 2010 to 2016. The dependent variable is the natural logarithm of the annualized wage of individual i at firm j in year t. *Deposit ratio*$_j \in [0, 1]$ is the average deposits-to-assets ratio across all euro-area banks from which firm j received syndicated loans anytime from 2010 to 2013. *After*(2014)$_t$ is a dummy variable for the years $2014 - 2016$. *Bottom* 20% (*Middle* 60%) *within firm*$_i$ is an indicator variable for whether worker i's wage is in the bottom 20% (middle 60%) of the wage distribution of firm j where i was employed in the last available year during the pre-period from 2010 to 2013. Robust standard errors (clustered at the firm level) are in parentheses.

Table 6: Effect of Negative Policy Rates on Layoffs

Variable	Unemployed next year $\in \{0,1\}$ (1)	(2)	(3)	(4)	(5)
Deposit ratio × After(2014) × Bottom 20% within firm			-0.253*	-0.313**	-0.318**
			(0.136)	(0.147)	(0.134)
Deposit ratio × After(2014) × Middle 60% within firm			-0.052	-0.067	-0.068
			(0.056)	(0.054)	(0.049)
Deposit ratio × After(2014)	0.054	-0.094	-0.036		
	(0.034)	(0.071)	(0.069)		
Deposit ratio × Bottom 20% within firm			0.027	0.082	
			(0.219)	(0.229)	
Deposit ratio × Middle 60% within firm			0.011	-0.008	
			(0.095)	(0.097)	
After(2014) × Bottom 20% within firm			0.178***	0.206***	0.209***
			(0.048)	(0.052)	(0.048)
After(2014) × Middle 60% within firm			0.024	0.033*	0.033*
			(0.019)	(0.019)	(0.017)
Worker FE	N	Y	Y	Y	N
Firm FE	Y	Y	Y	N	N
Worker-firm FE	N	N	N	N	Y
Year FE	Y	Y	Y	N	N
Firm-year FE	N	N	N	Y	Y
N	4,984,235	4,839,953	4,457,995	4,457,943	4,455,298

Notes: The sample consists of full-time employees i at German corporations j that are active in the syndicated-loan market in year t from 2010 to 2016. The dependent variable is an indicator variable for whether individual i is unemployed in year $t+1$. *Deposit ratio*$_j \in [0,1]$ is the average deposits-to-assets ratio across all euro-area banks from which firm j received syndicated loans anytime from 2010 to 2013. *After*(2014)$_t$ is a dummy variable for the years $2014-2016$. *Bottom* 20% (*Middle* 60%) *within firm*$_i$ is an indicator variable for whether worker i's wage is in the bottom 20% (middle 60%) of the wage distribution of firm j where i was employed in the last available year during the pre-period from 2010 to 2013. Robust standard errors (clustered at the firm level) are in parentheses.

Table 7: Effect of Negative Policy Rates on Wages and Layoffs—Robustness

	ln(Annualized wage)			Unemployed next year $\in \{0,1\}$		
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Deposit ratio $\times$ After(2014) $\times$ Bottom 20%	-0.124	1.044***	1.055***	-0.076	-0.086	-0.088
	(0.626)	(0.366)	(0.333)	(0.157)	(0.148)	(0.133)
Deposit ratio $\times$ After(2014) $\times$ Middle 60%	0.201	0.267	0.272	0.002	-0.094**	-0.095***
	(0.232)	(0.198)	(0.179)	(0.053)	(0.040)	(0.036)
Deposit ratio $\times$ After(2014)	-0.171			-0.068		
	(0.307)			(0.076)		
Deposit ratio $\times$ Bottom 20%	0.740	0.377		-0.042	-0.032	
	(0.816)	(0.800)		(0.200)	(0.221)	
Deposit ratio $\times$ Middle 60%	-0.332	-0.449		0.033	0.036	
	(0.351)	(0.349)		(0.100)	(0.101)	
After(2014) $\times$ Bottom 20%	-0.056	-0.385***	-0.392***	0.134**	0.154***	0.156***
	(0.195)	(0.121)	(0.110)	(0.055)	(0.048)	(0.043)
After(2014) $\times$ Middle 60%	-0.064	-0.072	-0.074	-0.001	0.043***	0.044***
	(0.085)	(0.077)	(0.070)	(0.019)	(0.016)	(0.014)
Worker FE	Y	Y	N	Y	Y	N
Firm FE	Y	N	N	Y	N	N
Worker-firm FE	N	N	Y	N	N	Y
Year FE	Y	N	N	Y	N	N
Firm-year FE	N	Y	Y	N	Y	Y
N	4,926,740	4,926,683	4,924,078	4,498,521	4,498,475	4,495,802

Notes: The sample consists of full-time employees i at German corporations j that are active in the syndicated-loan market in year t from 2010 to 2016. The dependent variable in the first three columns is the natural logarithm of the annualized wage of individual i at firm j in year t. The dependent variable in the last three columns is an indicator variable for whether individual i is unemployed in year $t+1$. *Deposit ratio*$_j \in [0,1]$ is the average deposits-to-assets ratio across all euro-area banks from which firm j received syndicated loans anytime from 2010 to 2013. *After*(2014)$_t$ is a dummy variable for the years $2014-2016$. *Bottom* $20\%_i$ (*Middle* $60\%_i$) is an indicator variable for whether worker i's wage is in the bottom 20% (middle 60%) of the wage distribution across all firms in the sample in the last available year during the pre-period from 2010 to 2013. Robust standard errors (clustered at the firm level) are in parentheses.

Table 8: Decomposition of Between-firm Wage Inequality: Second-stage Regressions

	Estimated firm-year fixed effects					
Decomposition			Fix firm pay		No worker mobility	
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Deposit ratio × After(2014) × Firm rank		-0.131***		-0.003*		-0.232***
		(0.005)		(0.001)		(0.007)
Deposit ratio × After(2014)	-0.126***	-0.114***	0.001***	0.003***	-0.133***	-0.112***
	(0.001)	(0.002)	(0.000)	(0.001)	(0.001)	(0.003)
Firm rank × After(2014)		0.064***		0.000		0.109***
		(0.002)		(0.001)		(0.003)
Deposit ratio × Firm rank				0.359**		
				(0.160)		
Deposit ratio			-0.201***	-0.629***		
			(0.066)	(0.097)		
Firm rank				0.318***		
				(0.058)		
Worker FE	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	N	N	N	N
Year FE	Y	Y	Y	Y	Y	Y
N	5,271,313	5,271,294	5,271,745	5,271,745	3,358,266	3,358,258

Notes: The sample consists of full-time employees i at German corporations j that are active in the syndicated-loan market in year t from 2010 to 2016. In the last two columns, we furthermore limit the sample to workers i that spent the same number of years at firm j as the respective firm exists in the sample period. Generally, the dependent variable is the estimated firm-year effect, based on column 2 of Table A.2, for firm j at which individual i works at time t. In the third and fourth column, we fix across all years each firm j's estimated firm-year fixed effect in 2013, which we use as dependent variable. *Deposit ratio*$_{J(i,t)} \in [0, 1]$ is the average deposit ratio of all euro-area banks that firm j, at which individual i works at time t, received a syndicated loan from anytime from 2010 to 2013, *After*(2014)$_t$ is a dummy variable for the post-period from 2014 to 2016, and *Firm rank*$_{J(i,t)}$ is the rank (from 0 = lowest to 1 = highest) of firm $j = J(i, t)$ in the distribution of average firm-year effects during the pre-period from 2010 to 2013. Robust standard errors (clustered at the individual level) are in parentheses.

Table 9: Decomposition of Between-firm Wage Inequality by Firms' ROA Volatility: Second-stage Regressions

	Estimated firm-year fixed effects					
Decomposition			Fix firm pay		No worker mobility	
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Deposit ratio × After(2014) × Risk rank		0.152***		0.005***		0.146***
		(0.003)		(0.001)		(0.004)
Deposit ratio × After(2014)	-0.118***	-0.211***	0.001***	-0.002***	-0.124***	-0.214***
	(0.001)	(0.002)	(0.000)	(0.001)	(0.001)	(0.002)
Risk rank × After(2014)		-0.051***		-0.001***		-0.049***
		(0.001)		(0.000)		(0.001)
Deposit ratio × Risk rank				-2.397***		
				(0.236)		
Deposit ratio			-0.177**	1.282***		
			(0.078)	(0.130)		
Risk rank				0.800***		
				(0.082)		
Worker FE	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	N	N	N	N
Year FE	Y	Y	Y	Y	Y	Y
N	4,559,074	4,559,074	4,559,681	4,559,681	2,936,727	2,936,727

Notes: The sample consists of full-time employees i at German corporations j that are active in the syndicated-loan market in year t from 2010 to 2016. In the last two columns, we furthermore limit the sample to workers i that spent the same number of years at firm j as the respective firm exists in the sample period. Generally, the dependent variable is the estimated firm-year effect, based on column 2 of Table A.2, for firm j at which individual i works at time t. In the third and fourth column, we fix across all years each firm j's estimated firm-year fixed effect in 2013, which we use as dependent variable. *Deposit ratio*$_{J(i,t)} \in [0,1]$ is the average deposit ratio of all euro-area banks that firm j, at which individual i works at time t, received a syndicated loan from anytime from 2010 to 2013, *After*(2014)$_t$ is a dummy variable for the post-period from 2014 to 2016, and *Risk rank*$_{J(i,t)}$ is the rank (from 0 = lowest to 1 = highest) of firm $j = J(i,t)$ in the distribution of the average ROA volatility of loan-financed firm i during the pre-period from 2010 to 2013. Robust standard errors (clustered at the individual level) are in parentheses.

Table 10: Effect of Negative Policy Rates on Within-firm Inequality

	ln(p95/p5)			
Sample	All	All	Private firms	Public firms
Variable	(1)	(2)	(3)	(4)
Deposit ratio × After(2014)	0.543*	0.539*	0.665**	0.214
	(0.296)	(0.297)	(0.327)	(0.713)
Non-euro area deposit ratio × After(2014)		-0.108	-0.183	-0.065
		(0.112)	(0.146)	(0.180)
Firm FE	Y	Y	Y	Y
State-year FE	Y	Y	Y	Y
N	2,639	2,639	1,730	885
	ln(p90/p10)			
Sample	All	All	Private firms	Public firms
Variable	(1)	(2)	(3)	(4)
Deposit ratio × After(2014)	0.330*	0.329*	0.390*	0.187
	(0.199)	(0.199)	(0.220)	(0.432)
Non-euro area deposit ratio × After(2014)		-0.033	-0.037	-0.049
		(0.064)	(0.091)	(0.112)
Firm FE	Y	Y	Y	Y
State-year FE	Y	Y	Y	Y
N	2,654	2,654	1,735	895

Notes: The sample consists of all German corporations j that are active in the syndicated-loan market in year t from 2010 to 2016, and the unit of observation is the firm-year level jt. In the third and fourth column, we furthermore limit the sample to privately-held and publicly-listed firms, respectively. In the top panel, the dependent variable is the delta log of the annualized wage at the 95$^{\text{th}}$ vs. 5$^{\text{th}}$ percentile of firm j's wage distribution in year t. In the bottom panel, the dependent variable is the delta log of the annualized wage at the 90$^{\text{th}}$ vs. 10$^{\text{th}}$ percentile of firm j's wage distribution in year t. *Deposit ratio*$_j \in [0, 1]$ is the average deposits-to-assets ratio across all euro-area banks from which firm j received syndicated loans anytime from 2010 to 2013. *Non-euro area deposit ratio*$_j \in [0, 1]$ is the average deposits-to-assets ratio across all non-euro area banks from which firm j received syndicated loans anytime from 2010 to 2013. *After*(2014)$_t$ is an indicator variable for the years $2014 - 2016$. State-year fixed effects are based on firm j's state of incorporation. Robust standard errors (clustered at the firm level) are in parentheses.

Table 11: Effect of Negative Policy Rates on Within-firm Inequality—No New Hires

	ln(p95/p5), without new employees in year t			
Sample	All	All	Private firms	Public firms
Variable	(1)	(2)	(3)	(4)
Deposit ratio × After(2014)	0.383	0.381	0.598**	-0.446
	(0.269)	(0.269)	(0.286)	(0.694)
Non-euro area deposit ratio × After(2014)		-0.048	-0.032	-0.118
		(0.086)	(0.107)	(0.159)
Firm FE	Y	Y	Y	Y
State-year FE	Y	Y	Y	Y
N	2,598	2,598	1,695	880
	ln(p90/p10), without new employees in year t			
Sample	All	All	Private firms	Public firms
Variable	(1)	(2)	(3)	(4)
Deposit ratio × After(2014)	0.189	0.191	0.288*	-0.123
	(0.157)	(0.157)	(0.162)	(0.426)
Non-euro area deposit ratio × After(2014)		0.047	0.139**	-0.070
		(0.050)	(0.065)	(0.110)
Firm FE	Y	Y	Y	Y
State-year FE	Y	Y	Y	Y
N	2,612	2,612	1,701	888

Notes: The sample consists of all German corporations j that are active in the syndicated-loan market in year t from 2010 to 2016, and the unit of observation is the firm-year level jt. In the third and fourth column, we furthermore limit the sample to privately-held and publicly-listed firms, respectively. In the top panel, the dependent variable is the delta log of the annualized wage at the 95$^{\text{th}}$ vs. 5$^{\text{th}}$ percentile of firm j's wage distribution in year t, calculated on the basis of all full-time employees at firm j except for newly hired workers in year t. In the bottom panel, the dependent variable is the delta log of the annualized wage at the 90$^{\text{th}}$ vs. 10$^{\text{th}}$ percentile of firm j's wage distribution in year t, calculated on the basis of all full-time employees at firm j except for newly hired workers in year t. *Deposit ratio*$_j \in [0, 1]$ is the average deposits-to-assets ratio across all euro-area banks from which firm j received syndicated loans anytime from 2010 to 2013. *Non-euro area deposit ratio*$_j \in [0, 1]$ is the average deposits-to-assets ratio across all non-euro area banks from which firm j received syndicated loans anytime from 2010 to 2013. *After*(2014)$_t$ is an indicator variable for the years $2014 - 2016$. State-year fixed effects are based on firm j's state of incorporation. Robust standard errors (clustered at the firm level) are in parentheses.

Table 12: Effect of Negative Policy Rates on Within-firm Inequality: Executive Board Members vs. Employees in Administrative Data

	ln(median board total/p50)		ln(med. board salary/p50)	ln(med. board bonus/p50)
Variable	(1)	(2)	(3)	(4)
Deposit ratio $\times$ After(2014)	0.974	4.427*	2.460*	7.204*
	(1.706)	(2.538)	(1.433)	(4.209)
Non-euro area deposit ratio $\times$ After(2014)		-1.221*	-0.649**	-1.019
		(0.645)	(0.288)	(0.866)
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
N	177	177	177	175
	ln(median board total/p5)		ln(med. board salary/p5)	ln(med. board bonus/p5)
Variable	(1)	(2)	(3)	(4)
Deposit ratio $\times$ After(2014)	2.685*	7.127***	5.160**	9.877**
	(1.402)	(2.309)	(2.043)	(3.953)
Non-euro area deposit ratio $\times$ After(2014)		-1.571***	-0.999**	-1.345*
		(0.522)	(0.457)	(0.752)
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
N	177	177	177	175

Notes: The sample consists of all German corporations j that are active in the syndicated-loan market in year t from 2010 to 2016, and for which we have board-compensation data from BoardEx; the unit of observation is the firm-year level jt. In the top panel, the dependent variable in the first two columns is the delta log of the median total compensation, consisting of a salary and a potential bonus, of executive board members at firm j in year t vs. the median annualized wage at firm j in year t. The dependent variable in the third column is the delta log of the median salary of executive board members at firm j in year t vs. the median annualized wage at firm j in year t. The dependent variable in the fourth column is the delta log of the median bonus (conditional on being nonzero) of executive board members at firm j in year t vs. the median annualized wage at firm j in year t. In the bottom panel, the median annualized wage at firm j in year t is replaced by the annualized wage at the 5^{th} percentile of firm j's wage distribution in year t. *Deposit ratio*$_j \in [0, 1]$ is the average deposits-to-assets ratio across all euro-area banks from which firm j received syndicated loans anytime from 2010 to 2013. *Non-euro area deposit ratio*$_j \in [0, 1]$ is the average deposits-to-assets ratio across all Non-euro area banks from which firm j received syndicated loans anytime from 2010 to 2013. *After*(2014)$_t$ is an indicator variable for the years $2014 - 2016$. Robust standard errors (clustered at the firm level) are in parentheses.

Online Appendix

A Supplementary Tables

Table A.1: Impact of Negative Policy Rates on Lending to German Firms—Robustness

	Any loan share $\in \{0,1\}$					
Lenders	Euro-area banks			All European banks		
Sample	$2010-2016$	Low ROA volatility	High ROA volatility	$2010-2016$	Low ROA volatility	High ROA volatility
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Deposit ratio $\times$ After(06/2014)	-0.044	-0.080**	-0.019	-0.101	-0.437**	-0.061
	(0.035)	(0.036)	(0.047)	(0.073)	(0.181)	(0.118)
Firm-time FE	Y	Y	Y	N	N	N
Firm FE	N	N	N	Y	Y	Y
Time FE	N	N	N	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y	Y
N	20,384	6,580	6,580	23,296	6,874	6,846

Notes: Based on all (participating and lead) banks' shares of completed syndicated loans of German corporations j anytime from January 2010 to December 2016, the sample is extended so as to represent a balanced panel of all borrower-bank pairs at the semi-annual frequency. Time therefore refers to the semi-annual level. Furthermore, the sample in the first three columns (last three columns) includes all euro-area (European) banks that lend to German firms anytime during the sample period. All singletons are dropped from the total number of observations N. In the second and fifth (third and sixth) column, the sample is split into borrower firms in the bottom (top) third of the distribution in terms of firms' ROA volatility. The dependent variable is an indicator for any non-zero share of firm j's loans retained by (participating or lead) bank k. In the first three columns, *Deposit ratio*$_k \in [0,1]$ is euro-area bank k's ratio (in %) of deposits over total assets in 2013. In the last three columns, *Deposit ratio*$_j \in [0,1]$ is the average deposits-to-assets ratio (in %) across all euro-area banks from which firm j received syndicated loans anytime from 2010 to 2013. *After*(06/2014)$_t$ is a dummy variable for the period from June 2014 onwards. Public-service, energy, and financial-services borrower firms are dropped. Robust standard errors (clustered at the bank level) are in parentheses.

Table A.2: First-stage Regressions

	ln(Annualized wage)	
Variable	(1)	(2)
Female	-0.114***	-0.096***
	(0.001)	(0.001)
German	0.060***	0.064***
	(0.001)	(0.001)
University	-1.104***	-1.134***
	(0.008)	(0.008)
Age	0.091***	0.140***
	(0.000)	(0.000)
Age^2	-0.001***	-0.001***
	(0.000)	(0.000)
College × Age	0.052***	0.052***
	(0.000)	(0.000)
College × Age^2	-0.001***	-0.001***
	(0.000)	(0.000)
Year FE	Y	N
Occupation FE	Y	Y
Firm-year FE	N	Y
R^2	0.337	0.383
N	5,465,787	5,465,750

Notes: The sample consists of full-time employees i at German corporations j that are active in the syndicated-loan market in year t from 2010 to 2016. The dependent variable is the natural logarithm of the annualized wage of individual i at firm j in year t. Robust standard errors are in parentheses.

Table A.3: Decomposition of Between-firm Wage Inequality by Firms' Stock-return Volatility: Second-stage Regressions

	Estimated firm-year fixed effects					
Decomposition			Fix firm pay		No worker mobility	
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Deposit ratio × After(2014) × Risk rank		0.105***		0.002		0.083***
		(0.003)		(0.001)		(0.003)
Deposit ratio × After(2014)	-0.104***	-0.167***	0.001*	-0.000	-0.122***	-0.173***
	(0.001)	(0.002)	(0.000)	(0.000)	(0.001)	(0.002)
Risk rank × After(2014)		-0.027***		-0.000		-0.018***
		(0.001)		(0.000)		(0.001)
Deposit ratio × Risk rank				-2.630***		
				(0.345)		
Deposit ratio			-0.508***	1.059***		
			(0.135)	(0.134)		
Risk rank				0.890***		
				(0.119)		
Worker FE	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	N	N	N	N
Year FE	Y	Y	Y	Y	Y	Y
N	3,411,171	3,411,171	3,411,101	3,411,101	2,268,420	2,268,420

Notes: The sample consists of full-time employees i at publicly listed German corporations j that are active in the syndicated-loan market in year t from 2010 to 2016. In the last two columns, we furthermore limit the sample to workers i that spent the same number of years at firm j as the respective firm exists in the sample period. Generally, the dependent variable is the estimated firm-year effect, based on column 2 of Table A.2, for firm j at which individual i works at time t. In the third and fourth column, we fix across all years each firm j's estimated firm-year fixed effect in 2013, which we use as dependent variable. *Deposit ratio*$_{J(i,t)} \in [0, 1]$ is the average deposit ratio of all euro-area banks that firm j, at which individual i works at time t, received a syndicated loan from anytime from 2010 to 2013, *After*(2014)$_t$ is a dummy variable for the post-period from 2014 to 2016, and *Risk rank*$_{J(i,t)}$ is the rank (from 0 = lowest to 1 = highest) of firm $j = J(i,t)$ in the distribution of the average standard deviation of firm i's monthly stock returns over 36 months during the pre-period from 2010 to 2013. Robust standard errors (clustered at the individual level) are in parentheses.

Table A.4: Effect of Negative Policy Rates on Wages at Different Percentiles

Variable	ln(p5) (1)	ln(p10) (2)	ln(p90) (3)	ln(p95) (4)
Deposit ratio × After(2014)	-0.448	-0.247	0.095	0.103
	(0.288)	(0.197)	(0.068)	(0.065)
Non-euro-area deposit ratio × After(2014)	0.111	0.056	0.028	0.017
	(0.112)	(0.067)	(0.021)	(0.019)
Firm FE	Y	Y	Y	Y
State-year FE	Y	Y	Y	Y
N	2,639	2,654	2,669	2,669

Notes: The sample consists of all German corporations j that are active in the syndicated-loan market in year t from 2010 to 2016, and the unit of observation is the firm-year level jt. The dependent variable is indicated at the top of each column, and is the natural logarithm of the annualized wage at the 5^{th}, 10^{th}, 90^{th}, and 95^{th} percentile of firm j's wage distribution in year t. *Deposit ratio*$_j \in [0, 1]$ is the average deposits-to-assets ratio across all euro-area banks from which firm j received syndicated loans anytime from 2010 to 2013. *Non-euro area deposit ratio*$_j \in [0, 1]$ is the average deposits-to-assets ratio across all non-euro area banks from which firm j received syndicated loans anytime from 2010 to 2013. *After*(2014)$_t$ is an indicator variable for the years $2014 - 2016$. State-year fixed effects are based on firm j's state of incorporation. Robust standard errors (clustered at the firm level) are in parentheses.