The Post-2015 German Lending Surge - What Role for QE?[†]

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Some tables and graphs are missing in this version because they have not been cleared by the Bundesbank for publication.
Please contact me if you are interested in the complete results.

This paper uses matched bank-firm loan data from Germany to test whether the ECB's quantitative easing (QE) spurred bank lending to non-financial firms. Using the share of bonds in banks' total assets before QE as treatment proxy, my dataset also allows me to control for loan demand at firm level by using firm dummies. While the effects my regression reveals are positive and statistically significant, their economic relevance is questionable: Increasing the bond/asset ratio by one standard deviation increases the deviation of outstanding loans to a firm from its own pre-QE trend by only 3-4% of its within-sample mean. At firm level, no effect can be observed.

Keywords: Unconventional monetary policy, portfolio rebalance, panel regression.
JEL classification: C23, E51, E52, G11, G21.

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1 Introduction

In 2015, the Eurosystem launched its quantitative easing (QE) program with the intention to bring inflation back to its 2% target via spurring bank lending and hence stimulating the real economy. At the same time, the German economy experienced a surge in private debt that was strongly driven by bank lending (see section 2). This seems to make it obvious that QE was successful. The empirical literature on the effects of QE, however, does not reach unanimous conclusions. While some papers find a strong impact (e.g. Rodnyansky and Darmouni 2017), others find only mixed effects (e.g. Acharya et al. 2019; Chakraborty et al. 2020). These differences in the results might stem from differences in the countries under observation, the time period, or which outcome and explanatory variables were used. Also see Dell'Ariccia et al. (2018) for further information on this.

This paper investigates whether QE stimulated bank lending in the German economy between 2015 and 2018. With previous research revealing ambiguous results, this question warrants further investigation. The issue is important because QE has evolved from an unconventional monetary policy measure to a standard component of central banks' toolbox and central banks need to know how their instruments work. For the Eurosystem it is particularly important as it oversees a very heterogeneous currency area comprised of 20 national economies which, most of the time, take different positions in both the business and financial cycle. The Eurosystem's monetary policy, however, is carried out on equal terms throughout the euro area, and QE was no exception.[1] Hence, it is important to analyze the effects of QE at the national level as results might be very different. The German banking system was not under any stress in 2015, unlike those of Southern European countries like Spain or Italy that were still suffering from the preceding financial crises.

Using data on individual German banks and their loans to individual German firms, my proxy to measure the cross-sectional exposure of a bank towards QE is the share of fixed-income bonds over bank total assets before the start of QE in 2015. This identification was used before in the literature (Bittner et al. 2024; Chakraborty et al. 2020; Rodnyansky and Darmouni 2017). The channel I test is the portfolio rebalancing channel[2]: when the central bank purchases large quantities of government bonds and other FI assets, this additional demand from the monetary authority increases the prices of those

[1] To be precise, the Public Sector Purchase Program (PSPP) launched in 2015 was no exception as the amount of government bonds the ECB purchased from each member state was determined by the state's share in the ECB's capital. Under the Pandemic Emergency Purchase Program (PEPP) launched in 2020, purchases deviated from the shares indicated by the capital key. PEPP, however, is not part of my investigation.

[2] Portfolio rebalancing hinges on preferred habitat investors, see Tobin (1969) and Vayanos and Vila (2009, 2021) for details and a formalization.

assets and hence squeezes the yield. Assuming a yield-searching motive on the banks' side, this should then trigger a portfolio rebalancing: in order to keep their overall return on assets stable, they switch into other assets that might be riskier but generate a higher yield, for instance corporate loans. The argument of QE proponents is then that this should increase the supply of corporate loans and squeeze the interest rate on those as well. That, in turn, would then stimulate corporate lending and investment and eventually lead to a higher inflation via increased demand in the goods market (Hammermann et al. 2019). My dependent variable is the deviation of an individual bank's outstanding loan volume to an individual firm from its pre-QE trend. Using a subsample of firms with more than one bank connection allows me to control for loan demand at the firm level, following Khwaja and Mian (2008). The expectation is to observe a positive relationship: the higher the share of bonds in bank total assets, the more should this bank's loan volume deviate from its pre-QE trend compared to other banks lending to the same firm.

In my research design, I follow a two-step approach as is common in this line of literature. First, I run a loan-level regression which allows me to control for loan demand at firm level as described above. The results of this regression, however, only show me whether banks in the treatment group increase their *relative* loan volume compared to banks in the control group *within* the same firm. For this reason, second, I run a firm-level regression to see whether the receiving firm actually increases its total loan volume or merely substitutes loans from control group banks with loans from treatment group banks. To measure the exposure of a firm towards QE, I take the share of bonds in total assets of all banks lending to that firm and compute the weighted firm-level average across all banks, with the weights being the share of each bank's loan volume in all banks' loan volumes. Since firm fixed effect dummies cannot be used to control for firm-level loan demand in the firm-level regression, I employ a mathematical method derived by Jiménez et al. (2020) to compute an unbiased coefficient in the firm-level regression – see section 4.2 for details.

What are my main findings? At the loan level, I find a mild positive effect of QE (section 5.1). When the share of bonds over total bank assets is increased by one standard deviation (roughly 12%-points), the deviation of the outstanding loan volume from a bank to a firm from its pre-QE trend increases by 2-4% of the mean outstanding loan volume over the observation period (2011-2018). At the firm level, I cannot find a statistically significant or economically relevant effect (5.2). This indicates substitution by borrowers between different lenders.

All my results are very robust across a number of sub-samples: all firms in the dataset, only non-financial firms for which balance sheet data are available in the firm dataset, only firms that issue no market debt, and only firm where the sum of bilateral loans in the credit register makes up at least 75% of total bank loans reported by the same firm in the firm dataset[3]. Also, the inclusion of newly established lending relationships does not change the overall picture (6.1), although the effect strength increases when taking the extensive margin into consideration. This indicates that borrowing firms not only substitute loans between existing bank relationships, but also through establishing new relationships. Furthermore, the results are robust against using different proxies for QE (6.2): only bonds that eventually become eligible for being purchased by the Eurosystem, only bonds that mature during QE, only government bonds from GIIPS countries, and the volume of bond redemptions during QE.

Related Literature. The use of matched bank-firm data has become increasingly popular over the course of the previous decade, following Khwaja and Mian (2008). These authors use matched bank-firm data to investigate the effect of an unexpected liquidity drop on credit supply in the Pakistani bank system following international sanctions in 1998. Using only a sub-sample of firms with multiple bank relations allows them to control for firm-level credit demand via firm dummies. Their key finding is that banks more strongly affected by the negative liquidity shock reduce their lending compared to their less-affected peers. A further investigation of the evolution borrowing at firm level reveals that firms with strong pre-shock ties to affected banks were not able to compensate this supply drop by increased borrowing from less-affected banks. Amiti and Weinstein (2018) extend the Khwaja and Mian (2008) method to compute aggregate bank shocks, weighted by banks' lending, in order to investigate the impact of idiosyncratic bank shocks on total lending and investment in the economy. Using Japanese data, they find a strong impact of bank shocks on firm investment activity both at the granular and at the macroeconomic level.

In this paper I will use the Khwaja and Mian (2008) (henceforth KM) method to investigate whether the increase in loan growth observed in Germany after 2015 can be tracked to the Eurosystem's Quantitative Easing program (the Asset Purchase Program, APP). This paper adds to the existing literature in that I investigate a *positive* shock to banking system while the rest of the literature, to the best of my knowledge, almost exclusively investigates *negative* lending shocks. This is important because previous literature has found monetary policy to have asymmetric effects with negative monetary shocks having a larger impact than positive ones.

[3]Due to a reporting threshold in the credit register, the sum of individual loans is usually below the total bank debt reported by the same firm in the firm dataset, see section 3.

The three papers that, to the best of my knowledge, are closest to the present study are Bittner et al. (2024), Rodnyansky and Darmouni (2017) and Chakraborty et al. (2020). Bittner et al. (2024) use largely the same data to investigate interactions between quantitative easing and negative interest rate policy (NIRP). They measure banks' exposure towards QE through banks' share of bonds in total assets and banks' exposure to negative interest rates through banks' share of household deposits in total assets. Their main finding is that while both QE and NIRP by themselves have a positive impact on lending, there is a strong negative interaction effect between the two: banks with high exposure to QE lend relatively less when they are also strongly exposed to NIRP. My paper differs from Bittner et al. (2024) in that I focus on QE, but take a more detailed look. Specifically, I run the regression on various subsets of borrowers (see above) and also test other QE proxies (e.g. only bonds that mature during QE).

Rodnyansky and Darmouni (2017) investigate the effect of the Federal Reserve System's three rounds of QE on US bank lending. Using the securities-to-asset ratio as proxy for a bank's exposure to QE, their bank-level regression finds that banks in the top quartile of that measure increase their loan volume by more than 3% compared to banks in the bottom quartile. Controlling for loan demand in a loan-level (bank-firm) regression cuts the effect in half. These results, however, only hold for the first and third round of US QE during which MBS were purchased. For the second round, which focused on the purchase of Treasury securities, the authors do not find any positive effects on lending. Building on Rodnyansky and Darmouni (2017), Chakraborty et al. (2020) find that MBS purchases by the Federal Reserve induced affected banks to increase mortgage lending at the expense of commercial lending; firms negatively affected by this crowing-out effect decreased their total borrowing and investment. For treasury purchases, the authors cannot find any large positive impact on lending.

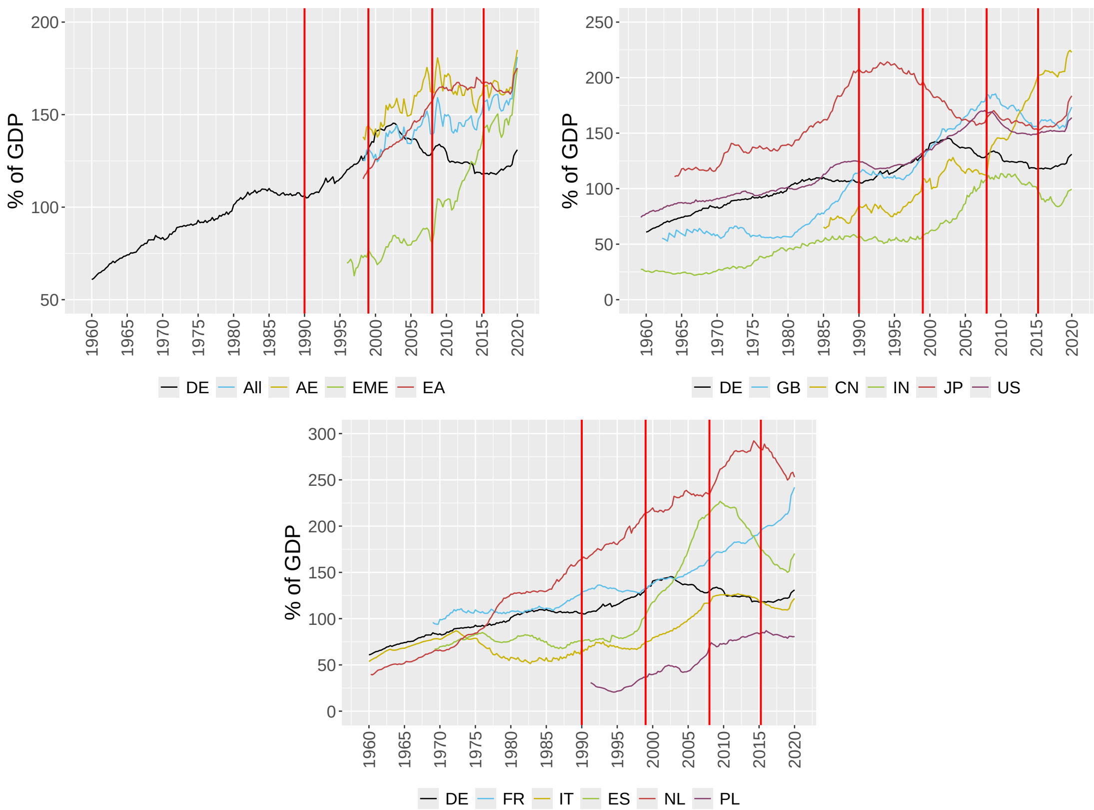
One final paper that deserves special mentioning is Jiménez et al. (2020)[4] insofar as they also investigate the impact of a positive shock to banks. Linking the Spanish credit register (CIR) with bank and firm balance sheet data, they find that Spanish banks which were strongly exposed to the booming domestic housing market during the 2000s also increased their lending to non-real-estate firms compared to banks less active in the real estate business. However, they cannot detect a noteworthy effect at firm level, meaning in this case borrowers merely substituted lending from control group banks by lending from treatment group banks. Further literature that uses similar data than the present study can be found in the online appendix.

[4]The working paper version of Jiménez et al. (2020) is Jiménez et al. (2011) which is often referred to by papers published even before Jiménez et al. (2020).

The rest of the paper is structured as follows. Section 2 provides some more detail on the evolution of private sector debt in Germany over time using data from public sources. Section 4 outlines the empirical strategy. Section 3 shows information on the data and descriptive statistics. In section 5 I present and discuss the results of the main specification and in section 6 the robustness checks. Section 7 concludes.

2 Private debt and bank lending in Germany

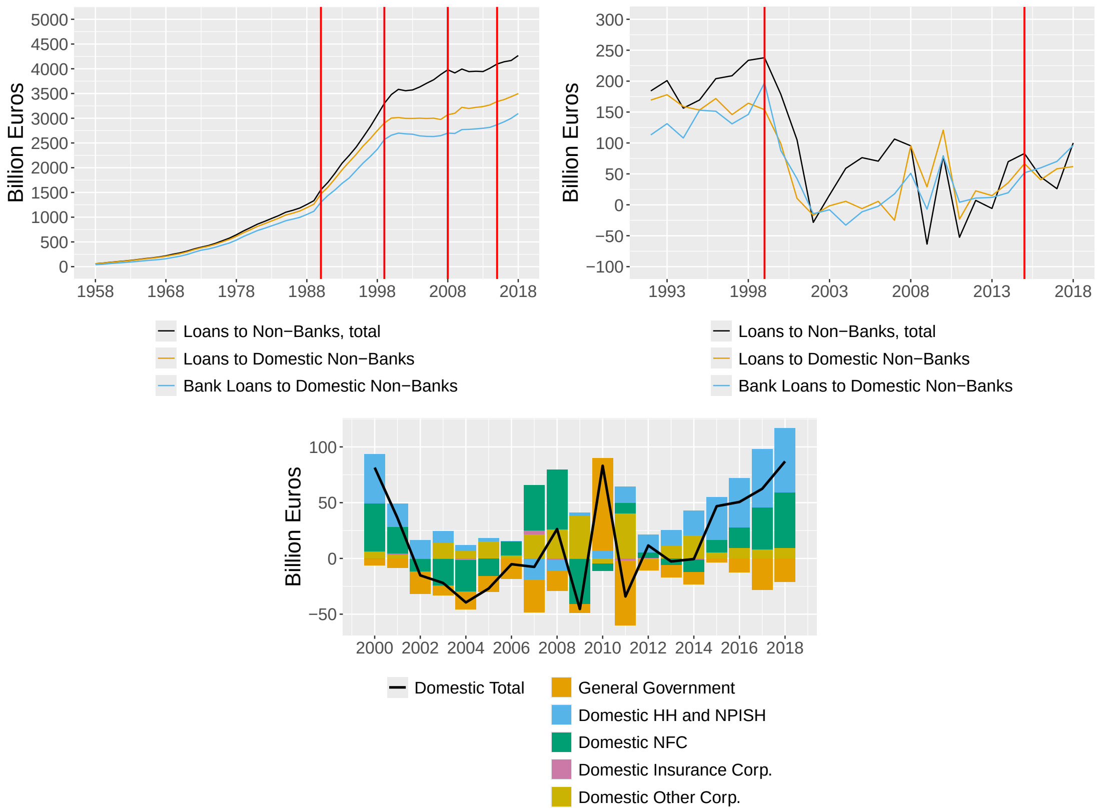
Figure 1: Credit to Private Non-Financial Sector



The figure shows total credit to domestic private non-financial sectors. The vertical red lines indicate important events in 1990 (German Reunification), 1999 (introduction of the Euro), 2008 (Global Financial Crisis), and 2015 (start of QE). The drop in the German time series in 2014 is due to a data revision. Source: Bank for International Settlements.

Figure 1 compares total credit from all sources (bank and non-bank) to the domestic non-financial private sectors as share of GDP in Germany to various country aggregates and countries, based on data from the Bank for International Settlements (BIS). The vertical red lines indicate important events in Germany. Germany experienced an unprecedented private credit surge between reunification in 1990 and the early 2000s when credit started to decline. From 2014, private credit rose at par with GDP and eventually

Figure 2: Loan Volume of German Banks



Outstanding loans (top left) and y-o-y change of outstanding loans (top right) in Billion Euros of German Banks. The black and orange lines show total lending (through bank loans and securities) to non-banks and domestic non-banks respectively, the blue line shows only bank loans to domestic non-banks. The bottom panel shows the y-o-y change of outstanding bank loans by institutional sectors. Source: Deutsche Bundesbank.

outpaced GDP growth from 2017 on. Two things can be seen here. First, while many countries ran through a boom-bust cycle during the 2000s and 2010s, Germany's predates those of almost all other countries by ten years and hence ran against the international trend after the turn of the millennium. Second, the amplitude in Germany's cycle is less pronounced than elsewhere. In any case, the long-run downward trend in private credit seems to have stopped in 2015. Further details on the German and euro area financial cycles can be found in Bundesbank (2019).

That 2015 represents a break in the lending and borrowing behavior in Germany can also be seen in figure 2 which shows the lending of German banks, based on publicly available Bundesbank data. The top left panel shows the outstanding stock of loans in the long run. The top right panel depicts year-on-year changes and zooms in on the period between German Reunification, which posed a major macroeconomic shock, and the end of QE in 2018. The data echo the pattern from the total credit data shown in figure 1: a surge after 1990 which ended after 2000 and a return to growth after 2014, and this rebound is particularly pronounced when zooming in on bank loans (blue line). The bottom panel decomposes the growth of bank loans to domestic non-banks into the growth towards various institutional sectors. Here we can see that the post-2015 growth was strongly driven by a rebound in the growth of loans to non-financial corporations (NFC). The other major contributor to loan growth, loans to households, started to increase in 2010 already.

These data clearly show that the German private non-financial sector stepped up its borrowing after 2014, that the vast majority of that debt seems to have been sourced by the domestic banking system, and that a significant and increasing share of the new debt went to the non-financial corporate sector. Explaining lending from German banks to German non-financial firms hence explains a non-trivial share of this post-2014 debt surge. That the pivot point coincides with the launch of QE in the euro zone makes QE the prime suspect in explaining this pivot.

3 Data

To conduct the analysis, I merge various datasets containing microdata. The centerpiece is the monthly balance sheet statistics (BISTA – Bilanzstatistik)[5] which contains detailed data on balance sheet positions of all banks resident in Germany. The BISTA can be linked with the SHS (Securities Holding Statistics)[6] in which banks report their monthly holdings of securities by ISIN. Further details on individual ISINs can be taken from the CSDB (Centralized Securities Database)[7]. All three databases are complete surveys and available on a monthly basis from January 2013 on; before that, the SHS is only available on a quarterly basis. Further detail on the industry structure of banks' lending portfolio is provided by the Quarterly Borrower Statistics (VJKRE - Viertljährliche Kreditnehmerstatistik)[8]. Here, banks have to report their lending volume to 22 different industry sectors at the end of each quarter. The VJKRE is aligned with the BISTA to ensure that the sum of loans over all sectors equals the total loan volume reported in the BISTA.

To this I add the German Credit Register (MIO - Millionenkreditevidenz) and the statistics of individual financial statements of non-financial firms (JANIS - Jahresabschlüsse-

[5]DOI = 10.12757/BBk.BISTA.99Q1-21Q4.01.01; dataset description in Gomolka et al. (2022)
[6]DOI = 10.12757/Bbk.SHSBaseplus.05122112; dataset description in Blaschke et al. (2022)
[7]DOI = 10.12757/BBk.CSDB.200903-202012.02.01; dataset description in Yalcin et al. (2021)
[8]DOI = 10.12757/BBk.VJKRE.99Q1-21Q4.01.01; dataset description in Krodel et al. (2022)

nichtfinanzieller-Unternehmen-Statistik)[9] in order to create bank-firm links. While linking the MIO to the BISTA is comparatively straightforward as both are raised from banks, linking the MIO to the JANIS is not a trivial exercise; details can be found in the web appendix. What is important to understand here are two things. First, the Credit Register does not contain individual loans, but the total outstanding credit of the reporting bank vis-à-vis individual borrowers, whereas total credit is defined as the sum of debt and equity instruments. So if a bank reports credit of one million Euros it cannot be determined whether the bank granted a loan or bought a bond or a share of that borrower or any combination thereof. I used cross-referencing with the CSDB to ensure that the bank-firm pairs I observe in my analysis are through debt and not through equity - see section A of the web appendix.[10] Second, in the Credit Register banks have to report their credit vis-à-vis an individual borrower at the end of a quarter if the credit to the borrower *unit* to which that individual borrower belongs exceeded a threshold[11] at any point during that quarter; a borrower unit is defined as a combination of connected borrowers. Hence, the outstanding credit reported in the dataset can be well below the reporting threshold and even be zero. Still, the data have the important limitation that we cannot know whether a new lender-borrower relationship is actually new or an old one that previously was just below the reporting threshold. Details on the MIO can be found in Bundesbank (2021) (only available in German).

The JANIS contains – as its name implies – annual financial statements of non-financial corporations. It covers a plethora of items of the balance sheet, income statements, and, in some cases, statement of changes in tangible fixed assets. Additionally, it covers information on the industry sector and the legal form of each firm.

Table 1: Descriptive Stats of Loan-Level Variables

– not cleared for publication yet –

Table 2: Descriptive Stats of Bank-Level Variables

– not cleared for publication yet –

Tables 1 through 3 provide descriptive statistics for loan level, bank level, and firm level variables, each for two different sub-sets of the data: one with all non-financial firms that have at least two bank connections and for which firm data are available in the JANIS

[9] DOI = 10.12757/Bbk.JANIS.9722.11.11; dataset description in Becker et al. (2023)

[10] Besides, it's probably save to assume that banks generally do not own equity of non-financial firms at a large scale.

[11] 1.5 million Euros until 2014 Q4, 1 million thereafter.

Table 3: Descriptive Stats of Firm-Level Variables

– not cleared for publication yet –

Figure 3: Share of Banks

– not cleared for publication yet –

Figure 4: Share of Firms

– not cleared for publication yet –

(dataset 2), and one in which all firms have been dropped that issued no bonds before QE and for which the sum of the bilateral lending relationships from the credit register is less than 75% of the total bank loans reported by the firm in the JANIS (dataset 4). The descriptive statistics of the two datasets are remarkably similar with one exception: firms in dataset 4 are a lot smaller on average.

Figures 3 and 4 show the share of the full BISTA and JANIS datasets that are covered by my datasets. Coverage of the BISTA is very good: bank total assets make up around 85% of the full dataset. Since the BISTA is a full survey, this is also 85% of the German banking systems aggregate total assets. Coverage of the JANIS is a lot lower: while the broader dataset covers around 30% of total assets of the full dataset, the narrower dataset covers 10%. This is due to the structure of both the credit register and the JANIS which both contain lots of unbalanced panels. In fact, many of the firms in the JANIS are only there for one or two years. Additional descriptive data can be found in the online appendix.

4 Empirical Strategy

The key appeal of matched bank-firm data is that both the bank lending channel and the balance sheet channel can be thoroughly tested by controlling for credit demand at the firm level and by controlling for credit supply at the bank level. Instrumental for the use of firm respectively bank dummies is that there are firms with multiple bank connections (to control for demand) and banks with multiple firm connections (to control for supply). While the latter condition is easily fulfilled, the former is only true for a sub-sample because particularly smaller firms often only have one bank from which they borrow. Plus, the reporting threshold must be expected to cut off secondary lending relationships, again most notably for smaller firms. In this paper, the focus is on the bank lending channel.

The economic mechanism I test is called the portfolio rebalancing channel. According to it, banks with a larger bond portfolio should be particularly sensitive to quantitative easing as a reduction in bond yields through central bank purchases creates an incentive for banks to reduce their bond holdings and instead increase their lending to firms and households (portfolio rebalancing).

In order to estimate the portfolio rebalancing channel, I follow a three-stage procedure well established in the previous literature. First, I use loan-level data to see whether banks with strong exposure to QE – as measured by their bond-to-asset ratio – increased their loans compared to banks with weak exposure to QE lending to the same firm. Second, I test whether receiving firms actually increased their total borrowing or merely used loans from treatment group banks to replace loans from control group banks. Finally, I test whether firms with strong credit relations to treatment group banks changed their economic activity vis-à-vis firms with weak relations to treatment group banks.

4.1 Estimating loan-level effects

In testing the bank lending channel, the KM framework has become a standard approach, as has been mentioned in section 1. The basic equation for estimating the bilateral loan volume between bank and firm is as follows:

$$y_{bf} = \beta_1 * \text{TREAT} \times \gamma_b + \boldsymbol{\beta_2'} * \mathbf{A}_{\boldsymbol{b}} + \delta_f + \epsilon_{bf} \tag{1}$$

The dependent variable y_{bf} is the deviation of the bilateral loan volume from bank b to firm f from its pre-QE trend, divided by the mean outstanding loan volume over the observation period:

$$y_{bf} = \frac{mean_T\left[\text{DetrendedLoans}_{bf,t}\right]}{mean_{11-18}\left[\text{Loans}_{bf,t}\right]} \tag{2}$$

whereas T represents the control period (2011 to 2014) and the treatment period (2015-2018). The variables in the numerator are defined as:

$$\text{DetrendedLoans}_{bf,t} = \text{Loans}_{bf,t} - \text{LoansLinearTrend}_{bf,11-14} \tag{3}$$

In words, starting with equation 3: I take the loan volume of bank b to firm f in year t and subtract the linear trend of that bilateral loan volume for the years 2011 to 2014, i.e. before QE started. By this I control for possible violations in the common trends assumption.[12] Then I compute the means of that detrended loan volume before QE (2011 to 2014) and during QE (2015 to 2018) and normalize it by the mean of the loan volume

[12] I also run a specification in which I take the non-detrended loan volume. The results are strongly different and are shown in the online appendix.

of bank b to firm f over the entire observation period[13]. This is my dependent variable (equation 2). Importantly, I censor the linear trend to be non-negative because negative trends would lead to all kinds of nonsensical results as the actual lending volume can never be negative.

On the right-hand side of equation 1, γ_b is a bank-level loan supply shock, interacted with a dummy TREAT that is zero for 2011 to 2014 and 1 for 2015 to 2018. In my case, γ_b is the exposure of a bank b to quantitative easing via the mean share of fixed-income assets (bonds) over total assets over the years 2011 to 2014:

$$\gamma_b = mean_{11-14}\left[\frac{\mathrm{bonds_{b,t}}}{\mathrm{TA_{b,t}}}\right] \tag{4}$$

Vector $\mathbf{A}$ includes the following set of control variables, :

$$mean_T\left[\frac{\text{deposits}_{b,t}}{\text{TA}_{b,t}}\right], \qquad mean_T\left[\frac{\text{wholesale funding}_{b,t}}{\text{TA}_{b,t}}\right], \qquad mean_T\left[\frac{\text{equity}_{b,t}}{\text{TA}_{b,t}}\right]$$

$$mean_T\left[\frac{\text{interbank claims}_{b,t}}{\text{TA}_{b,t}}\right], \qquad mean_T\left[\frac{\text{central bank liquidity}_{b,t}}{\text{TA}_{b,t}}\right]$$

$$mean_T\left[\frac{\Delta\mathrm{TA_{b,t}}}{\text{TA}_{b,t-t}}\right]$$

ϵ_{bf} is the error term and δ_f a firm-level loan demand shock. A simple OLS estimation of β_1 produces biased results if γ_b and δ_f are correlated: $\hat{\beta}_1^{OLS} = \beta + \frac{Cov(\gamma_b,\delta_f)}{Var(\gamma_b)}$. This problem can be remedied with data in which there are multiple bank connections per firm. Then, a (within-firm) fixed effects regression delivers unbiased results. The size and sign of the correlation can easily be shown by running both FE and OLS regressions on the same subsample and comparing the coefficients: $\hat{\beta}_1^{OLS} > \hat{\beta}_1^{FE}$ implies a positive correlation. See Jiménez et al. (2020) who also provide proof from simulations that this is a viable method.

I will also test an adaption of equation 1 in which I substitute the firm fixed effect δ_f with a number of firm-level control variables $\mathbf{B_f}$ in order to test how they perform in controlling for firm-level loan demand:

$$y_{bf} = \beta_1 * \text{TREAT} \times \gamma_b + \boldsymbol{\beta_2'} * \mathbf{A_b} + \boldsymbol{\beta_3'} * \mathbf{B_f} + \epsilon_{bf} \tag{5}$$

[13] I do not use the pre-QE mean of the outstanding loan volume because if the loan volume is zero before 2015 and then increases, I cannot compute y_{bf} even though there is a change in the loan volume. This is particularly relevant when the extensive margin is included (see section 6.1)

Vector $\mathbf{B}_{\boldsymbol{f}}$ contains:

$$mean_T\left[\frac{\text{cash holdings}_{f,t}}{\text{TA}_{f,t}}\right], \qquad mean_T\left[\frac{\text{cash flow}_{f,t}}{\text{TA}_{f,t}}\right],$$

$$mean_T\left[\frac{\text{net interest income}_{f,t}}{\text{TA}_{f,t}}\right], \qquad mean_T\left[\frac{\Delta\text{TA}_{\text{f,t}}}{\text{TA}_{f,t-1}}\right]$$

Higher cash holdings and a higher cash flow over total assets should both reduce the requirement for external finance. The net interest income is a proxy for the debt burden the firm is facing. Finally, I include the growth in total assets as a broad control for firm growth: strongly growing firms can be expected to have a higher demand for finance.

4.2 Estimating firm-level effects

Looking at bilateral lender-borrower relations does not give a sufficient picture of the firm-level borrowing. This is because a loan-level regression as described by equations 1 and 5 tells us how loans from banks in the treatment group evolve *relative* to loans from banks in the control group *within* the same firm. Any positive (or negative) effect might simply represent substitution of a given firm between different banks rather than an increase (or decrease) in overall borrowing by that very firm. From a theoretical point of view it sounds reasonable for a firm to exploit more favorable loan conditions from one bank to replace loans from another bank with less favorable loan conditions. To test whether firms increase their overall borrowing following a bank-specific supply shock, we need to look at the firm level:

$$\bar{y}_f = \bar{\beta}_1 * \text{TREAT} \times \bar{\gamma}_f + \delta_f + \bar{\epsilon}_f \tag{6}$$

Here, $\bar{y}_f$ is computed the same way as y_{bf} from equation 1, but this time with the total borrowing of the firm from all banks, as reported in the JANIS. Since this variable also includes lender-borrower relationships that are below the credit register's reporting threshold, it is not necessarily identical to the aggregate over all bilateral relationships that enter equation 1. We will come back to this point later. Again, $\bar{\epsilon}_f$ is the error term. δ_f is the same firm-level demand shock as in equation 1. $\bar{\gamma}_f$ is the weighted average exposure of the firm to the bank supply shocks:

$$\bar{\gamma}_f = \sum_{b \in N_f} (W_{\gamma_b} * \gamma_b) \tag{7}$$

N_f is the set of banks lending to firm f. W_{γ_b} is the weight of bank b in firm f's total borrowing over the observation period, defined as follows:

$$W_{\gamma_b} = \frac{\sum_{11-18} \text{Loans}_{bf}}{\sum_{b \in N_f} \sum_{11-18} \text{Loans}_{bf}} \tag{8}$$

I.e. I take the sum of the end-of-year loan volumes of bank b to bank firm f over the years 2011 to 2018 and divide it by the sum of the end-of-year loans to that firm from all banks over the years 2011 to 2018. In section 6.3 I will discuss alternative weights. If the firm does not substitute between its various sources of loans at all, then $\bar{\beta}_1 = \beta_1$. In the other extreme case, in which the firm undertakes perfect substitution, $\bar{\beta}_1 = 0$.

The problem with the firm-level regression is that it suffers from the same potential correlation between supply shock $\bar{\gamma}_f$ and demand shock δ_f as the loan-level regression, but this time it cannot be controlled for through firm-level fixed effects because both shocks are now invariant at firm level. There are various solutions for this problem in the literature. Khwaja and Mian (2008), for instance, luckily escape these troubles because in their case the shocks are actually negatively correlated which means that their firm-level OLS results are even underestimates. Bentolila et al. (2018) and Bottero et al. (2020) saturate their firm-level regressions with firm controls and industry-location dummies. Cingano et al. (2016) include the estimates of δ_f from their loan-level equation in their firm-level equation. Jiménez et al. (2020) derive a mathematical solution to compute an unbiased coefficient at firm level, starting with the fact that the coefficient computed with a firm-level OLS regression is biased:

$$\hat{\bar{\beta}}_1^{OLS} = \beta_1 + \frac{Cov(\gamma_b, \delta_f)}{Var(\bar{\gamma}_f)} \tag{9}$$

While $Cov(\gamma_b, \delta_f)$ is not observable, the fact that a loan-level fixed effects estimation in the vein of equation 1 delivers an unbiased estimate of β_1, namely $\hat{\beta}^{FE}$, allows us to re-arrange the previous equation to express the covariance as follows:

$$Cov(\gamma_b, \delta_f) = \left(\hat{\beta}_1^{OLS} - \hat{\beta}_1^{FE}\right) * Var(\bar{\gamma}_f) \tag{10}$$

Plugging equation 10 into equation 9 then gives us the following formula with which we can compute an unbiased estimate of β at firm level:

$$\hat{\bar{\beta}}_1 = \hat{\bar{\beta}}_1^{OLS} - \left(\hat{\beta}_1^{OLS} - \hat{\beta}_1^{FE}\right) * \frac{Var(\gamma_b)}{Var(\bar{\gamma}_f)} \tag{11}$$

$\hat{\bar{\beta}}_1^{OLS}$ is from equation 6, $\hat{\beta}_1^{OLS}$ is from equation 1 without firm fixed effects, and $\hat{\beta}_1^{FE}$ is from equation 1 with firm fixed effects.

5 Results

5.1 Loan-level effects

Table 4 shows the results of the loan-level regression as defined by equation 1. I run the regression on four different sub-sets of the dataset and with three different specifications. Within each dataset, panel (a) simply regresses the dependent variable as defined by equation 2 on the share of bonds in banks' balance sheets as defined by equation 4. Panel (b) adds firm fixed effects and panel (c) further adds bank control variables.

Dataset (1) contains all firms with at least two bank connections. Including the firm FEs has hardly any effect on the diff-in-diff coefficient, indicating that supply and demand effects happen to be uncorrelated. Further saturating the model with bank controls slightly decreases the coefficient compared to models (a) and (b). This pattern holds throughout all datasets. The reason why I run all regressions with and without bank controls separately is that in the firm-level regression I undertake later I cannot include bank control variables, so I need specification (b) to maintain comparability of the coefficients of the QE shock. Yet still, bank characteristics other than the share of bonds in total assets must be expected to play a role in banks' loan supply. In lieu of this, it is rather fortunate that leaving those bank controls does not impact the coefficient too much.

In dataset (2), I only use non-financial corporations for which firm balance sheet data are available in the JANIS. In dataset (3), I additionally drop all firms which have issued bond debt before 2015 to keep the analysis focused on firms which are arguably dependent on bank loans. In dataset (4), I drop those firms where the sum of bilateral loans to that firm reported in the credit register makes up less than 75% of total bank loans reported by the same firm in the JANIS. The effect strength of TREAT$\times\gamma_b$ is remarkably stable throughout most datasets and stays within conventional boundaries of statistical significance with the exception of model 4 (p-values are shown in brackets).

What do the results of table 4 mean in terms of economic relevance? Remember, the dependent variable is the mean deviation of loans from bank b to firm f from their pre-QE trend, divided by the mean outstanding loan volume from b to f during the observation period. Hence, the coefficient 0.329 in model (1a) means that an increase in the share of bonds in bank total assets by 1 unit (i.e. by 100%-points) implies that the deviation of outstanding loans from their pre-QE trend increases by 32.9% of the mean outstanding loan volume over 2011 to 2018. At first glance, this appears to be a massive effect, but a variation in the explanatory variable by 1 unit would imply comparing a bank with no bonds at all with a bank where all assets are bonds. A more reasonable gauge would be to check the impact of a change by one standard deviation. Additionally, how does the

Table 4: Results of the Loan-Level Regression with Firm Fixed Effects (equation 1)

Dataset	(1)			(2)			(3)			(4)		
Model	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)
– not cleared for publication yet –												
Firm FE												
Bank Controls												
N Banks												
N Firms												
N Pairs												
N												
R^2												

Dataset (1) contains all firms with at least two bank connections. Dataset (2) contains only non-financial corporations for which balance sheet data are available. Dataset (3) contains only firms that never issued bonds before 2015. Dataset (4) contains only firms where bilateral lending in the credit register sums up to at least 75% of total bank loans reported in the JANIS. γ_b is the share of bonds in bank total assets as defined in equation 4. Standard errors (in parentheses) are clustered at the bank level. p-values are shown in square brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

change in the dependent variable implied by the model compare to its variance we observe in the data? Table 5 shows the coefficient of each regression (column (2)), the standard deviation of the bank-level shock γ_b (column (3)), the product of the two (column (4)), the standard deviation of the change of y_{bf} in 2011-2014 compared to 2015-2018 (column (5)) and the ratio of the latter two (column (6)).

Table 5: Effect strengths in regression equation 1

(1)	(2)	(3)	(4)	(5)	(6)
Model	Coefficient (table 4)	SD of γ_b (table 2)	(2)*(3)	SD of Δy_{bf} (table 1)	(4)/(5)
– not cleared for publication yet –					

Column (4) shows how y_{bf} in regression model 1 changes as a reaction to a change of γ_b by one standard deviation. Column (5) is the standard deviation of Δy_{bf} which is the change of y_{bf} between the control period (2011-2014) and the treatment period (2015-2018) as observed in the data. Column (6) sets the two in relation.

In words: in model (1a), a change in bonds over bank total assets by one standard deviation leads to a change in the mean deviation of loans from their pre-QE trend by 3,9% of the mean of outstanding loan volume. This is not a large number by itself and it is also small compared to the standard deviation of the change of the dependent variable between the two time periods, which happens to be 1. Taken together, a typical change in the independent variable (bonds over bank total assets) triggers a change in the dependent variable (deviation of outstanding loans from their pre-QE trend as share over outstanding loans) that is less than 4% of the standard deviation of the change of that dependent variable. However, remember that this is not a final assessment because at this point we can only say that banks increase their lending relative to other banks when they are arguably more exposed to QE. From this regression we cannot say whether the receiving firm actually increases its total volume of bank loans or whether it is merely substituting between banks.

In table 6, I test whether specific firm variables can sufficiently replace firm fixed effects as controls for firm-level loan demand. Model (2a) repeats model (2b) from table 4 to serve as benchmark. In model (2b) I drop the firm FE in favor of the firm controls (vector $\boldsymbol{B_f}$ from equation 5); additionally, I add the ratio of bank debt reported by the firm in the JANIS over its tangible assets before QE (model 2b) and the share of tangible assets in firm total assets before QE (model 2c). My motivation here is that loan supply does not only depend on the lending bank's characteristics but also on the borrowing firm's characteristics. Tangible assets are often used as collateral in loan contracts, so the higher the ratio between bank loans and tangible assets already is, the less inclined should a bank

Table 6: Results of the Loan-Level Regression with Firm Control Variables (equation 5)

Dataset	(2)				(4)			
Model	(a)	(b)	(c)	(d)	(a)	(b)	(c)	(d)

– not cleared for publication yet –

Firm FE								
Bank Controls								
N Banks								
N Firms								
N Pairs								
N								
R^2								

Datasets (2) and (4) are as defined in table 4. γ_b is the share of bonds in bank total assets as defined in equation 4. Standard errors (in parentheses) are clustered at the bank level. p-values are shown in square brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

be to extend further loans to that firm. However, QE has been known to push tangible asset prices[14] and hence those firms which are arguably supply-restricted via their bank loan / tangible asset ratio should receive further lending. The coefficient of this variable is zero, though. Tangible assets by themselves have the expected positive impact.[15] The coefficient of TREAT$\times\gamma_b$ is smaller than in the benchmark specification. This means that replacing firm FEs by firm controls does not lead to an overestimation of the effect of QE. In model (4) the coefficients of interest are no longer statistically significant just as in table 4.

5.2 Firm-level effects

Table 7 shows the results of the firm-level regression (equation 6). Again, model (2) runs on the dataset with all non-financial firms for which firm data are available in the JANIS and which have at least two bank connections. Panel (a) shows the base model without additional controls. The coefficient of TREAT$\times\bar{\gamma}_f$ is drastically reduced in its size compared to its loan-level counterpart (model (2a) in table 4) and even turns negative.

As has been lined out in section 4.2, firm fixed effects cannot be used to control for loan demand in the firm-level regression. One option to tackle this issue is to extract the firm fixed effects from the loan-level regression and include them in the firm-level regression, as has been done by Cingano et al. (2016). I mimic this approach in panels (b) of table 7. This decreases the coefficient of the diff-in-diff term even further, mirroring the results of table 4. The effect can be observed in both models 2 and 4 which is important because model 4 runs on the dataset in which the sum of bilateral loans from the credit register and the total bank loans from the firm balance sheets have the best match. In other words: the model 4 results are the ones suited best for comparison between loan-level and firm-level.[16]

A second way to control for loan demand in the firm-level regression is to include firm control variables, as has been done by Bentolila et al. (2018) and Bottero et al. (2020). I mimic this approach in panels (c) through (e) of table 7. As in table 6, the inclusion of vector $\boldsymbol{B_f}$ and bank loans over tangible assets does not have much of an impact. Including vector $\boldsymbol{B_f}$ and tangible assets over total assets reduces the coefficient of TREAT$*\bar{\gamma}_f$ in

[14] see, e.g., Berg et al. (2023), Huber and Punzi (2020), and Rahal (2016). In fact, property price growth in Germany gained momentum after 2015 (Federal Statistical Office of Germany 2020-12-28).

[15] The is a sizeable literature on this collateral channel of monetary policy which finds conflicting results of the effect of real estate prices on firm borrowing and investment. Bednarek et al. (2021) find a positive impact of real estate prices in German firm activity. Chaney et al. (2012) do so for the US. In China, in contrast, the collateral channel seems hardly at work and displaced by other real estate related channels (Wu et al. 2024).

[16] Also interesting to note is that the fixed effects implanted from the loan-level regression have a much stronger effect on the dependent variable than any other regressor. This indicates that firm-level loan demand effects are much more important than any supply effects.

Table 7: Results of the Firm-Level Regression (equation 6)

Dataset	(2)				(4)			
Model	(a)	(b)	(c)	(d)	(a)	(b)	(c)	(d)
– not cleared for publication yet –								
Firm FE								
Bank Controls								
N Banks								
N Firms								
N Pairs								
N								
R^2								

Datasets (2) and (4) are as defined in table 4. $\bar{\gamma}_f$ is the firm-level shock as defined in equation 7. Standard errors (in parentheses) are clustered at the bank level. p-values are shown in square brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

both models, mirroring the results from table 6.

A third way to compute an unbiased coefficient in the firm-level regression is via equation 11, as has been done by Jiménez et al. (2020). Table 8 shows the results for models 2 and 4. In both cases the unbiased coefficients table 7 are drastically smaller than their loan-level correspondents, negative even, but effectively zero. Remember, the case of perfect substitution between banks would result in $\hat{\bar{\beta}}_1 = 0$, while no substitution implies $\hat{\bar{\beta}}_1 = \hat{\beta}_1^{FE}$. These results mean, then, that firms use what they newly borrow from banks strongly exposed to QE to replace loans from banks that are less exposed.

Table 8: Unbiased $\hat{\bar{\beta}}_1$ from Equation 6 Using the Equation 11 Formula

Model	$\hat{\bar{\beta}}_1^{OLS}$	$\hat{\beta}_1^{OLS}$	$\hat{\beta}_1^{FE}$	$Var(\gamma_b)$	$Var(\bar{\gamma}_f)$	$\hat{\bar{\beta}}_1$
– not cleared for publication yet –						

The table shows the values that are plugged into equation 11 to compute an unbiased coefficient from the firm-level regression (equation 6). The first two column depicts which model in the respective output table the line is referring to. $\bar{\beta}_1^{OLS}$ is the coefficient from the firm-level regressions. β_1^{OLS} is the coefficient from the loan-level regressions without firm fixed effects. β_1^{FE} is the coefficient from the loan-level regression with firm fixed effects. $Var(\gamma_b)$ and $Var(\bar{\gamma}_f)$ are the variance of γ_b and $\bar{\gamma}_f$ (see tables 1 and 3).

5.3 Discussion

How do these results relate to the remaining literature on QE? Paludkiewicz (2021) and Tischer (2018) also use German bank data to investigate whether the APP triggered additional firm lending, both finding a sizeable effect. Both papers' level of analysis is the bank level, however, i.e. neither of them uses loan-level data. On top of that, Paludkiewicz (2021) uses the volume of new loans at bank level as a dependent variable rather than the volume of total outstanding loans. Two further papers that use bank-level data to investigate the effect of QE on bank lending are Bowman et al. (2015) and Joyce and Spaltro (2014). The former investigate the effects of the Bank of Japan's QE (2001-2006), finding a statistically significant positive but economically negligible effect in the early stages and no effect in the later stages of QE. The latter investigate the effects of the Bank of England's QE (2009-2010) and their findings mirror those of Bowman et al. (2015): positive and statistically significant, but of little economic relevance. None of these papers' findings contradict mine as my loan-level regression also shows a positive loan growth of exposed banks vis-à-vis control group banks. How firm-level borrowing reacts to QE is a question not tackled by any of these authors.

The same is true for Chakraborty et al. (2020) and Rodnyansky and Darmouni (2017) which have been mentioned in the introduction. Both papers go one step further than those mentioned in the previous paragraph in that they employ loan-level regressions in addition to bank-level regressions. Remember, Rodnyansky and Darmouni (2017) find a positive effect of QE1 and QE2, but not of QE3 on US bank lending and Chakraborty et al. (2020) put further light on those findings in that They unveil that those positive effects stem from increased mortgage lending while they fail to find any positive effects on commercial lending. Again, these findings are complementary to mine.

Taken together, the literature employing micro data (be it at bank, loan, or firm level) to find out whether QE stimulated lending to the real economy finds mild effects at best. Assuming the portfolio rebalancing channel is at work, we would expect to see banks which are more exposed to QE to increase their lending compared to less exposed banks. Evidently, this channel is only weak, however, in most cases. The question is: Is that surprising? From the perspective of the loanable funds hypothesis and the money multiplier model, the answer would be yes. This is because those two concepts postulate a mechanical relationship between reserves and bank deposits with the causality running from the former to the latter. So if the central bank injects additional reserves into the bank system, banks would increase deposits via additional lending. In this line of thinking, lending is supply-led. On the other end of the spectrum would be Post-Keynesian theory which argues that lending is purely demand-led. According to this reasoning, the banking system as a whole can only increase the outstanding amount of loans if aggregate loan demand increases accordingly. Any attempt to push lending via supply stimulus would only lead of a shift of lending between banks – just as I observe in my analysis.

Does all this mean that QE is generally ineffective? The answer is no. First, QE can have positive effects on the financial market, e.g. through the stabilization of asset prices and the reduction of uncertainty. Second, there can be other channels through which QE is beneficial for the real economy, e.g. through this very stabilization of financial markets. In fact, the results of Rodnyansky and Darmouni (2017) are in line with this as they find that purchase of stressed assets (MBS) exercises a positive stimulus on bank lending while the purchases of non-stressed assets (Treasuries) does not. What these results clearly show, however, that a central bank's ability to directly stimulate the economy has limits.

6 Robustness checks

6.1 Including the extensive margin

The results in section 5 are drawn from regressions that only take pre-existing bank-firm relationships into consideration. Put technically: they are limited to the intensive margin. In reality, though, banks and firms are free to establish new relationships or terminate

Table 9: Robustness Check of table 4: Including the Extensive Margin

Dataset	(1)			(2)			(3)			(4)		
Model	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)
– not cleared for publication yet –												
Firm FE												
Bank Controls												
N Banks												
N Firms												
N Pairs												
N												
R^2												

Datasets (1) through (4) are as defined in table 4, but include new and terminal bank-firm relationships. γ_b is the share of bonds in bank total assets as defined in equation 4. Standard errors (in parentheses) are clustered at the bank level. p-values are shown in square brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Robustness Check of table 7: Including the Extensive Margin

Dataset	(2)				(4)			
Model	(a)	(b)	(c)	(d)	(a)	(b)	(c)	(d)
Firm FE								
Bank Controls								
N Banks								
N Firms								
N Pairs								
N								
R^2								

– not cleared for publication yet –

Datasets (2) and (4) are as defined in table 4, but include new and terminal bank-firm relationships. $\bar{\gamma}_f$ is the firm-level shock as defined in equation 7. Standard errors (in parentheses) are clustered at the bank level. p-values are shown in square brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

existing ones. Because of the reporting threshold in the credit register[17] a lender-borrower relationship that newly pops up after the beginning of the observation period is not automatically genuinely new; I might as well just be an existing relationship that simply did not exceed the reporting threshold before. For this reason, I flag a relationship as being new when it exceeds 3 million euros – i.e. twice the threshold – at its first appearance. Even if such a relationship is not genuinely new, it is at least one that shows a sizeable increase.

With lender-borrower relationships that are terminated during the observation period, the same problem with the reporting threshold holds: was the relationship really terminated or did it simply drop below the threshold? And in this case, the problem is worse than with new relationships because typically borrowers take in a loan at one point in time and the gradually pay it down.[18] So when we observe the volume of outstanding loans, we observe a steep increase at the beginning and then a gradual decline. This means that we can be certain that a relationship that lies well above the reporting threshold at its first appearance, at least shows a significant increase even if it was not a genuinely new relationship but merely crossed the threshold. With relationships *exiting* the dataset, however, we do not have this certainty. They could still linger below the threshold for years. The only exception are loans that are zero in their final, and only their final, entry. Because remember, in the German credit register, a bank must report its outstanding loan volume to a borrower at the end of the quarter when it exceeds the reporting threshold at any point during that quarter. Hence, a sole zero final entry indicates payback. Besides those loans, I also keep all loans which closely follow a negatively sloped quadratic time trend (R^2 must be above 0.8) which I use to extend the time series until after its final appearance in the dataset.

Keeping those new and terminal pairs in the dataset increases the number of borrowers by 20 to 40%, depending on which sub-set is used. The results of running the loan-level and firm-level regressions with new and terminal pairs are shown in tables 9 and 10. In the loan-level regression, coefficients increase by around 50 to 100% compared to the main specification. Also the model 4 coefficients are now statistically significant. In the firm-level regression, the coefficients of interest now tend to be positive rather than negative, but are still very close to zero. Taken together, these results indicate that establishing new borrower relationships are important for exposed banks to increase their lending to a given firm compared to their less-exposed peers; this is why we observe a larger effect-strength in the loan-level setup. At firm level, however, there is still no effect that differs

[17] 1.5 million euros until end-2014, 1 million thereafter.

[18] This might not be the case with consumer or, above all, student loans which might also show a gradual increase. In this study, however, we only observe corporate loans.

from zero in any statistically or economically significant way. This confirms the results from the main specification that the rebalancing channel of QE triggers a shift in loan volumes between banks rather than an increase.

6.2 Different QE proxies

Identifying an individual bank's exposure to QE is not straightforward. In my main specification I used the share of bonds in banks' total asset which is the broadest possible measure because even if the central bank is targeting only specific types of assets, there can be spillover effects to non-targeted assets. Spillover effects must be expected to be non-perfect, however, and hence it seems prudent to check the effect of different sub-classes of bonds that might proxy a bank's exposure to QE more effectively than the top aggregate. Tables 11 through 15 repeat the regression analyses of table 4 with different explanatory variables.

In table 11, I use the pre-QE share of bonds in bank total assets that, at some point during the observation period, become eligible to be purchased under the Eurosystem's APP. This is arguably the narrowest measure of QE exposure. The pattern from table 4 with the coefficient grossly staying unaffected when introducing firm fixed effects and dropping slightly when further adding bank controls remains throughout all specifications.

In table 12, I use the pre-QE share of bonds in bank total assets that mature during QE. The motivation here is that it is only at maturity of a bond that a bank is actually exposed to the yield-squeezing effect that central bank purchases exercise on the bond. This is because then the bank gets paid the nominal value of the maturing bond and has to make a decision: does it want to re-invest into bonds or rather rebalance into other assets? Again, the pattern from table 4 is upheld, with coefficients staying below conventional thresholds of statistical significance.

In table 13, I use the pre-QE share of bonds in bank total assets that where issued by GIIPS governments. The motivation here is that those bonds might have experienced the largest increase in prices – and hence the largest drop in yields – during QE. Once more the known pattern regarding coefficients prevails.

In table 14, I use the mean share of bond redemptions over bank total assets during QE (2011-2015) as explanatory variable. Bond redemptions are the volume of bonds that a bank actually holds until maturity. With German banks being strong buy-and-hold investors this measure is similar to the one in table 12, but not identical because even German banks sell some of their bonds before maturity (see Tischer (2018)). While, again,

Table 11: Robustness Check of table 4: Using Only Bonds Eligible for QE as Proxy for Bank Exposure

Dataset	(1)			(2)			(3)			(4)		
Model	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)
– not cleared for publication yet –												
Firm FE												
Bank Controls												
N Banks												
N Firms												
N Pairs												
N												
R^2												

Datasets (1) through (4) are as defined in table 4. γ_b is the share of bonds in bank total assets as defined in equation 4, but only bonds that become eligible to be purchased by the Eurosystem at some point during 2011-2018. Standard errors (in parentheses) are clustered at the bank level. p-values are shown in square brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 12: Robustness Check of table 4: Using Only Bonds Maturing During 2015-2018 as Proxy for Bank Exposure

Dataset	(1)			(2)			(3)			(4)		
Model	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)

– not cleared for publication yet –

Firm FE												
Bank Controls												
N Banks												
N Firms												
N Pairs												
N												
R^2												

Datasets (1) through (4) are as defined in table 4. γ_b is the share of bonds in bank total assets as defined in equation 4, but only bonds that mature during 2015-2018. Standard errors (in parentheses) are clustered at the bank level. p-values are shown in square brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 13: Robustness Check of table 4: Using Only GIIPS Government Bonds as Proxy for Bank Exposure

Dataset	(1)			(2)			(3)			(4)		
Model	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)

– not cleared for publication yet –

Firm FE												
Bank Controls												
N Banks												
N Firms												
N Pairs												
N												
R^2												

Datasets (1) through (4) are as defined in table 4. γ_b is the share of bonds in bank total assets as defined in equation 4, but only GIIPS government bonds. Standard errors (in parentheses) are clustered at the bank level. p-values are shown in square brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 14: Robustness Check of table 4: Using Volume of Redemptions as Proxy for Bank Exposure

Dataset	(1)			(2)			(3)			(4)		
Model	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)
– not cleared for publication yet –												
Firm FE												
Bank Controls												
N Banks												
N Firms												
N Pairs												
N												
R^2												

Datasets (1) through (4) are as defined in table 4. γ_b is the mean over 2015-2018 of the ratio of the volume of bond redemptions over total bank assets. Standard errors (in parentheses) are clustered at the bank level. p-values are shown in square brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 15: Robustness Check of table 4: Using Volume of Redemptions of Bonds Held Before 2015 as Proxy for Bank Exposure

Dataset	(1)			(2)			(3)			(4)		
Model	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)
– not cleared for publication yet –												
Firm FE												
Bank Controls												
N Banks												
N Firms												
N Pairs												
N												
R^2												

Datasets (1) through (4) are as defined in table 4. γ_b is the mean over 2015-2018 of the ratio of the volume of bond redemptions over total bank assets, only using bonds that were held before 2015. Standard errors (in parentheses) are clustered at the bank level. p-values are shown in square brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

adding firm fixed effects has no impact on coefficients and adding bank controls slightly decreases them, coefficients drop below conventional levels of statistical significance in almost all specifications.

In table 15, I again use redemptions but this time only of bonds that banks already held before QE to control for any adaptions that banks might have undertaken in their bond portfolio in response to QE. Here, coefficients are statistically significant, even in model 4.

What do these results mean economically? The coefficients are not directly comparable because the explanatory variables have different levels and hence different variances across banks. Hence, we repeat the exercise from table 5 and multiply the coefficient of each specification with the standard deviation of the respective regressor and express the result as share of the dependent variable's standard deviation. Table 16 shows the results. Column (2) repeats column (6) from table 5 for easy comparison. With the exception of bonds eligible for QE and GIIPS government bonds which both show much weaker effects, these values confirm that the main specification's results are extremely robust against the use of different proxies for banks' exposure towards QE.

Table 16: Effect strength of robustness checks

(1)	(2)	(3)	(4)	(5)	(6)	(7)
Model	Bonds	Bonds eligible	Bonds matur-ing	Bonds GIIPS	Redemp-tions	Initial Redemp-tions
– not cleared for publication yet –						

This table repeats the calculation from table 5. Column (2) repeats column (6) from table 5 for easy comparison.

6.3 Different weights for firm-level shocks

In the main specification I used the share of a particular bank's loans in all loans to a firm over the entire observation period as a weight to compute the firm-level exposure to QE. It is not entirely straightforward, however, which is the ideal way to compute the QE-induced supply shock a firm level. For instance, one might argue that one should compute the firm-level exposure to QE by using only lending before QE as weight. At first glance this sounds reasonable: A firm is flagged as exposed if it has strong ties to treated banks before QE because when a bank has an incentive to increase its lending its main loan customers will be the first ones it might turn to. One could just as well

Table 17: Robustness Check of table 4

Dataset	(1)			(2)			(3)			(4)		
Model	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)
TREAT × $BONDS_b$	-0.123***	-0.123***	-0.123***	-0.123***	-0.123***	-0.123***	-0.123***	-0.123***	-0.123***	-0.123***	-0.123***	-0.123***
– not cleared for publication yet –												
Firm FE												
Bank Controls												
N Banks												
N Firms												
N Pairs												
N												
R^2												

Dataset (1) contains all firms with at least two bank connections. Dataset (2) contains only non-financial corporations for which balance sheet data are available. Dataset (3) contains only firms that never issued bonds before 2015. Dataset (4) contains only firms where bilateral lending in the credit register sums up to at least 75% of total bank loans reported in the JANIS.

Table 18: Robustness Check of table 4

Dataset	(1)			(2)			(3)			(4)		
Model	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)
TREAT × $BONDS_b$	-0.123***	-0.123***	-0.123***	-0.123***	-0.123***	-0.123***	-0.123***	-0.123***	-0.123***	-0.123***	-0.123***	-0.123***
						– not cleared for publication yet –						
Firm FE												
Bank Controls												
N Banks												
N Firms												
N Pairs												
N												
R^2												

Dataset (1) contains all firms with at least two bank connections. Dataset (2) contains only non-financial corporations for which balance sheet data are available. Dataset (3) contains only firms that never issued bonds before 2015. Dataset (4) contains only firms where bilateral lending in the credit register sums up to at least 75% of total bank loans reported in the JANIS.

Table 19: Robustness Check of table 4

Dataset	(1)			(2)			(3)			(4)		
Model	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)
TREAT × $BONDS_b$	-0.123***	-0.123***	-0.123***	-0.123***	-0.123***	-0.123***	-0.123***	-0.123***	-0.123***	-0.123***	-0.123***	-0.123***
– not cleared for publication yet –												
Firm FE												
Bank Controls												
N Banks												
N Firms												
N Pairs												
N												
R^2												

Dataset (1) contains all firms with at least two bank connections. Dataset (2) contains only non-financial corporations for which balance sheet data are available. Dataset (3) contains only firms that never issued bonds before 2015. Dataset (4) contains only firms where bilateral lending in the credit register sums up to at least 75% of total bank loans reported in the JANIS.

Table 20: Unbiased $\hat{\bar{\beta}}_1$ from Equation 6 Using the Equation 11 Formula

Model	$\hat{\bar{\beta}}_1^{OLS}$	$\hat{\beta}_1^{OLS}$	$\hat{\beta}_1^{FE}$	$Var(\gamma_b)$	$Var(\bar{\gamma}_f)$	$\hat{\bar{\beta}}_1$
Table 17						
– not cleared for publication yet –						
Table 18						
– not cleared for publication yet –						
Table 19						
– not cleared for publication yet –						

The table shows the values that are plugged into equation 11 to compute an unbiased coefficient from the firm-level regression (equation 6). The first two columns depict which model in the rective output table the line is referring to. $\bar{\beta}_1^{OLS}$ is the coefficient from the firm-level regressions. β_1^{OLS} is the coefficient from the loan-level regressions without firm fixed effects. β_1^{FE} is the coefficient from the loan-level regression with firm fixed effects. $Var(\gamma_b)$ and $Var(\bar{\gamma}_f)$ are the variance of γ_b and $\bar{\gamma}_f$ (see tables 1 and 3).

argue, however, that it will be the firms which have weak ties to strongly exposed banks before treatment start that eventually are more exposed. The argument goes as follows: Assume QE, as measured by the bond-to-asset ratio, sets an incentive for the bank to increase its lending. Further assume that a firm's total borrowing is primarily determined by factors that are not influenced by the bank, e.g. its expected output growth, which are also not directly affected by QE. Then the bank's only way to increase its loan volume is to increase its market share at the expense of its competitors by offering more favorable loan conditions. Then a firm would not be strongly exposed to QE when it had strong ties to treated banks *before* QE, but when it develops strong ties *during* QE. Since it is not per se clear which weight gives the best measure of firm-level QE exposure, I will undertake robustness checks in this section using different weights: From only the pre-QE period and from only the QE period. Additionally, I also compute an unweighted firm-level shock where I simply take the mean of bonds-to-bank asset ratios across all banks that lend to a firm. The results are shown in tables 17 to 19. Table 20 shows the unbiased coefficients, computed with the Jiménez et al. (2020) method.

In table 17 I used the unweighted mean of banks' bond-to-asset ratio across all banks lending to a firm as the firm-level shock. The results for models 2 and 4 are grossly similar. In table 18 I used the weighted mean with only loan volumes before QE (2011-2014) to compute the weights, in table 19 I used the weighted mean with only loan volumes during QE (2015-2018). In all cases, coefficients remain insignificant and small, as in the main specification. Somewhat of an effect can be seen in model 2, but remember that in the firm-level specification model 4 is a lot more reliable as the sum of the loans from the credit register that are used to compute the weights sum up to at least 75% of all bank loans that the receiving firm reports in the firm dataset. In model 4, both weighted shocks' effect strength is almost exactly zero.

7 Conclusion

In this paper I used loan-level data from the German credit register in combination with balance sheet data on both lenders and borrowers in order to assess the impact of quantitative easing on bank lending. To measure the cross-sectional exposure of individual banks to QE I use the bond-to-asset ratio because large-scale asset purchases by the Eurosystem will increase those bonds' prices and squeeze their yields, hence setting an incentive for holding banks to rebalance into other assets like corporate loans. Using a subset of firms with multiple bank connections allows me to control for loan demand at borrower level via firm fixed effects. I find that exposed banks increase their lending towards the same firm compared to less exposed banks. At the firm level, however, no such effect can be observed: there is no evidence in the data that firms that are more exposed to QE via their bank linkages increase their total borrowing compared to their less exposed peers. The results from the loan-level and firm-level regressions imply that firms used funds from strongly exposed banks to merely substitute loans from other banks rather than to increase their total external financing. These findings are in line with the empirical literature. While they do not necessarily imply that QE is generally ineffective, they clearly show is that a central bank's ability to directly stimulate the economy has limits.

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