Rollover Risk and Bank Lending Behavior

Abstract

How does a sudden extension of bank debt maturity affect bank lending in times of crisis? We use the provision of long-term funding by the 2011 European Central Bank's (ECB) very long-term refinancing operations (vLTRO) as a quasi-natural experiment to address this question. Our analysis employs a novel dataset that matches the ECB monetary policy and market operations data with the firm credit registry and banks' security holdings in Portugal. We show that lengthening of bank debt maturity in crisis times has a positive and economically sizable impact on bank lending and the real economy. The effects are stronger on the supply of credit to smaller, younger, riskier firms and firms with shorter lending relationships. We also find that loan-level results translate to real and credit effects at the firm level. Finally, we discuss policy side-effects and show how the unrestricted liquidity provision provided incentives to banks to purchase more securities and partially substituted away from lending to the real economy.

JEL classification: E44, E52, E58, G21, G32

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Introduction

The global financial crisis raised attention to the crucial amplification role of banks' short-term debt, maturity mismatch and rollover risk (Brunnermeier (2009) and Krishnamurthy (2010)). Positive and negative aspects of short-term debt issued by banks has long been discussed in the banking literature. On the one hand, short-term debt plays a disciplining role for bank managers and alleviates moral hazard problem (Calomiris and Kahn (1991), Diamond (1991), Diamond and Rajan (2001)). On the other hand, more recently, Brunnermeier and Oehmke (2013), Farhi and Tirole (2012), He and Xiong (2012), Segura and Suarez (2017) highlight that market forces lead to the issuance of too much short-term debt, excessive maturity mismatch, unnecessary rollover risk and higher credit risk.

The maturity structure of debt has an important impact on bank investment. Diamond and He (2014) argue that in good times short-term debt is better at inducing banks to invest due to a lower debt overhang.[1] In bad times, however, when bank assets are more volatile, short-term debt generates high rollover risk and hurts banks incentives to undertake profitable investment. The theory in Stein (2012) suggests that cheaper debt of longer maturity can decrease bank funding costs, reduce fire-sale externalities and increase investment. Both papers imply that policies of lengthening debt maturity in the times of crisis can therefore decrease the rollover risk and have positive effects on bank lending and real activity. Empirical evidence on the impact of debt maturity extension and the reduction in rollover risk is however scarce as it faces a number of challenges. Most importantly, the banks' debt structure is endogenous to the composition of assets, investors' preferences and the cost of financing.

This paper contributes to the literature by providing causal evidence on how lengthening of bank debt structure in times of crisis reduces rollover risk and affects bank lending. To this end, we address the empirical challenges by using 2011 European Central Bank's (ECB) very long-term refinancing operations (vLTRO) as a quasi-natural experiment of lengthening bank debt maturity. vLTRO allowed banks to costlessly swap existing short-term debt into newly offered long-term debt. Banks participating in the vLTRO could receive unlimited funding of long-term (three-year) maturity at the same financing conditions as in case of the short-term borrowing (the same interest rate, pool of eligible securities and haircut policy). At the time of the ECB policy decision, euro area banks were relying on short-term debt and were largely exposed to rollover risk. By ensuring stable funding for a horizon of three years, the vLTRO substantially lengthened the maturity of bank liabilities and sizably reduced uncertainty about the financing conditions over a longer period. As a result, these institutional features are particularly suitable

[1] This argument for the lower debt overhang associated with short-term debt was first formalized by Myers (1977). Debt overhang problem arises when the burden of existing debt is so large that it reduces firm's incentives to invest in new profitable projects. As short-term debt is less sensitive to firm value, a larger part of the return on new investment is captured by the equity holder.

to regard the vLTRO as a large shock to a bank debt structure that lengthened debt maturity and reduced rollover risk.[2]

The main hypothesis that we test in this paper is that during a period of bank funding uncertainty, banks more exposed to a reduction in rollover risk induced by the vLTRO policy provide more credit to the real economy than less exposed banks. We test this hypothesis by using a novel dataset that perfectly matches the ECB monetary policy and market operations data with the firm credit registry and banks' security holdings in Portugal.

The intuition behind the effect of elongating debt maturity on bank lending can be illustrated as follows. Banks simultaneously choose the composition of their assets and liabilities. Due to creditors' preferences for short-term debt, higher cost of long-term financing and lower debt overhang, banks use short-term debt to finance long-term projects. The maturity mismatch, intrinsic to the maturity transformation function of banks, exposes them to rollover risk. As a reaction to the vLTRO, Portuguese banks converted most of their short-term secured funding into long-term debt. Specifically, they swapped a substantial amount of their existing short-term borrowing granted by the ECB into the newly available three-year loans (Figure 1). The vLTRO induced an increase in the overall maturity of banks' liabilities and, hence, reduced their maturity mismatch (Figure 2). Following the vLTRO, banks also adjusted the asset-side of their balance sheets against their new maturity structure of liabilities. This led to an overall increase of maturity of assets (Figure 3).

In our empirical analysis, we use a difference-in-differences framework and exploit the variation in banks' exposure to the lengthening of debt maturity prior to the policy announcement. That is, we compare the evolution of lending to firms by banks that were more exposed to the policy relative to the lending by banks with less exposure. We construct two exposure measures: a (i) liability-side and (ii) asset-side measure. On the liability side, we use total ECB secured short-term bank borrowing that could be costlessly swapped into the three-year funding. The asset-side measure captures total banks holdings of securities that are eligible as a collateral for ECB funding. Banks with larger holdings of these securities are eligible to subscribe to more vLTRO funding. In line with Khwaja and Mian (2008), we incorporate firm-time fixed effects to isolate the causal effect of the policy by comparing lending outcomes of the same firm borrowing from at least two differently exposed banks. We also include bank-firm fixed effects to absorb any time-invariant bank, firm and bank-firm characteristics.

This paper provides several insightful results. First, we show that a reduction in rollover risk through lengthening of bank debt maturity has positive and economically sizeable impact on bank lending. In terms of sensitivities, we find that a one standard deviation increase in bank exposure to the reduction in rollover risk is associated with a 5.3 percent increase both on the

[2] As a re ection of severe funding needs, more than 800 euro area banks took a total amount of liquidity of about EUR 1 trillion in the vLTRO. Thus the policy intervention represents an unprecedented provision of central bank liquidity.

existing and new lending. We also find that more exposed banks are less prone to terminate existing lending and are more probable to make a loan to a new client after the vLTRO. Our results on the positive effect of a reduction in rollover risk on bank lending support the theoretical work by Diamond and He (2014), Stein (2012) and Segura and Suarez (2017).

Second, we examine the impact of a reduction in rollover risk on bank lending to different types of firms. We find stronger bank-lending channel for smaller, younger, riskier firms and firms with shorter lending relationship. Further, we also investigate whether lengthening of bank debt maturity transmitted into firm-level credit. We find that the policy had significant effect not only on loan-level credit but more importantly also on the firm-level, i.e. firms did not merely substitute borrowing from less to more exposed banks but they effectively increased their total borrowing. We estimate that for every EUR 100 of debt swapped from short-term to long-term, firms received EUR 2.5 in new lending. Put differently, we estimate that the reduction in rollover risk triggered by the vLTRO contributed to EUR 1 bn in lending to firms in Portugal. If we compare the credit development to the counterfactual world without any intervention, we find that although the policy did not stop the ongoing credit contraction, it significantly reduced its pace. We estimate that without the policy the credit would have contracted by additional 4 percentage points.[3] Third, we document that the policy contributed to higher investment and for small firms also to an increase in employment.

Finally, we highlight how the unrestricted liquidity provision provided incentives to banks to purchase more securities and substitute lending to the real economy (the so-called collateral trade). We document that after the policy announcement some banks purchased new eligible securities and immediately pledged these securities with the central bank to take up more vLTRO funding. Banks used the newly obtained ECB funding to decrease their dependence on other non-core deposits (in particular loans from other financial institutions). We show that as banks did not expand the balance sheet with the new vLTRO funding, this behavior led to a partial substitution away from lending in favor of security holdings. We find that the magnitudes of the strategic purchases depend on the bank business model. As a result, we find that the positive effects of a lengthening of debt maturity on lending was partially downsized by the strategic security purchases.

Literature Review

This paper relates to several streams of literature. First, our empirical evidence contributes to the theoretical literature on rollover risk (Brunnermeier and Oehmke (2013), Diamond (1991), Diamond and Rajan (2001), Farhi and Tirole (2012), Flannery (1986) and He and Xiong (2012)). Our results on the positive effect of a reduction in rollover risk on bank lending support the

[3] The observed credit contraction in the period after the vLTRO was -4.5% while we estimate that in the absence of the policy the contraction would have been -8.5%.

theoretical work by Diamond and He (2014), Stein (2012) and Segura and Suarez (2017). In the model of Diamond and He (2014) short-term debt may lead to stronger debt overhang compared to the long-term debt. A combination of high asset volatility and short-term debt deters equity holders from investment and induces them to default earlier. Instead, long-term debt provides incentives to undertake investment in bad times. In Stein (2012), an excessive amount of short-term debt and its refinancing during the crisis forces banks to sell some of their assets at fire-sale prices, which contributes to the tightening of their financial constraints. Cheaper debt of longer maturity (like in case of vLTRO) would be socially beneficial by reducing this externality and increasing lending to firms in general. Similarly, Segura and Suarez (2017) find that social surplus can be increased by lengthening the maturity of bank debt thanks to reduced costs of crisis financing and pecuniary externalities produced by banks refinancing needs during the crisis. In terms of existing empirical evidence, Almeida *et al.* (2011) find real effects (decrease in investment) among firms for which long-term debt matured during the 2007–2009 financial crisis. To the best of our knowledge, however, ours is the first paper that links debt structure of banks with the impact on borrowing to firms and firms' real outcomes through the bank-lending channel.

Second, we contribute to a growing field of empirical literature that investigates the lending and real outcomes of unconventional monetary policies (see, among others, Ferrando *et al.* (2015), Chakraborty *et al.* (2017), Luck and Zimmermann (2017), Rodnyansky and Darmouni (2017)). More generally, we also relate to the literature on the bank lending channel of monetary policy transmission (Bernanke and Blinder (1992), Kashyap *et al.* (1994)) and in particular to the latest work that relies on loan-level data (Jimenez *et al.* (2012), Jimenez *et al.* (2014b)) extending the Khwaja and Mian (2008) methodology. Our research also relates to the literature studying the effect of liquidity shocks or financial crisis on bank lending channel and the real economy (Chodorow-Reich (2014), Ivashina and Scharfstein (2010), Iyer *et al.* (2014), Schnabl (2012)).

In addition, we relate to the existing work that analyzes different aspects of the ECB vLTRO using credit registry data. Carpinelli and Crosignani (2017) investigate how the extension of a pool of eligible collateral by the Italian government for the vLTRO restored bank credit supply after the previous wholesale funding dry-up. van Bekkum *et al.* (2018) study the impact of the lower rating requirement for residential mortgage-backed securities announced in the context of the vLTRO on bank lending in the Netherlands. Different from these previous studies, we identify the direct effects of the reduction in rollover risk induced by the ECB policy action and offer original evidence on the effects of the vLTRO on bank lending in Portugal.

Finally, our work contributes to the literature on the perils of the unrestricted liquidity provision and changes in risk behavior of banks. Acharya and Steffen (2015) document that the vLTRO incentivized banks to engage in strategic security trade and in particular to purchase government debt securities. This also closely relates to the model of sovereign-bank diabolic loop as banks increased their sovereign exposures (Brunnermeier *et al.* (2016a)). Crosignani *et al.* (2017) provide evidence of a collateral carry trade for government debt securities in Portugal. Our paper contributes to existing literature by providing new evidence on how security purchases impacted the propagation of the policy stimulus to bank-lending and real outcomes.

The remainder of the paper is organized as follows. Section 1 presents institutional background. Section 2 discusses the data and Section 3 presents the empirical strategy. Section 4 presents the results on loan-level effects, Section 5 discusses the heterogeneous results by firm characteristics and Section 6 presents firm-level credit and real outcomes. Section 7 address the impact of policy side-effects. Finally, Section 8 concludes.

1 Background

1.1 ECB's non-standard monetary policy measures

The ECB provides liquidity through open market operations in the form of repurchase agreements against eligible collateral. The amount of liquidity a bank can receive depends on a riskiness of an underlying asset expressed through the haircut policy, i.e. the riskier the asset, the higher the haircut, and the lower is the funding a bank can receive. Until early 2008, the ECB provided liquidity to banks through auctions with a maximum maturity of three months[4] at variable rate (American) auction.[5] As a reaction to the financial crisis, the ECB implemented two main changes in its liquidity provision policy. First, it replaced the existing tender procedure with a *xed-rate full allotment policy*. This change allowed for unlimited funding at a fixed interest rate as long as banks could pledge sufficient collateral. Second, the ECB introduced a series of longer-term refinancing operations (LTRO) with maturity of 6 and 12 months.

In December 2011, the ECB unexpectedly announced a new very Long-Term Refinancing Operation (vLTRO). The crucial features of the policy were the following: (i) unlimited amount of funding against eligible collateral and unconditional on its use, (ii) extraordinary three-year long maturity, and (iii) the same interest rate as the existing short-term (weekly) repo operations. Financing conditions were fixed for the first year, during which the loan could not be repaid.

In 2011, European debt markets were very fragile. Banks mostly borrowed in short-term maturities and they were expected to roll over EUR 700 bn for each year in the coming period 2012–2014 (Brunnermeier *et al.* (2016b)). vLTRO provided the banking sector with stable and predictable funding. Banks could reduce funding liquidity risk associated with the debt rollover and avoid a potential liquidity dry-up on interbank market and central bank policy changes

[4] Weekly Main Re nancing Operations (MRO) and 1- and 3-month Longer-Term Re nancing Operations.

[5] See Cassola *et al.* (2013) for the analysis of banks' bidding behavior under the multiple rate auction during the 2007 subprime market crisis.

in the medium term. vLTRO was announced on December 8, 2011 and administered in two operations on December 21, 2011 and February 29, 2012. In total, more than 800 European banks subscribed to approximately EUR 1 trillion. To date, vLTRO is the largest liquidity provision in the history of modern central banking.

1.2 Portuguese banking sector

We choose Portugal as a laboratory for our analysis for a number of reasons. First, Portuguese banks represent the second-largest intake of vLTRO with respect to the size of banking sector with an individual bank vLTRO uptake on average 11% of total assets. Second, the degree of dependence of Portuguese banks on the ECB was very sticky and did not increase before the implementation of the vLTRO, thus, indicating that the policy intervention had been unexpected by the Portuguese banking system. Third, the focus on Portugal also overcomes main data limitations and subsequent identification challenges. In particular, we are able to exploit the loan-level credit registry data to isolate the movement in the credit supply. Also, we can match the banks' security holdings portfolios (and their evolution over time) with the securities pledged as a collateral with the ECB. This allows us to provide a new evidence on strategic timing of security purchases by banks after the vLTRO announcement. Finally, Portugal provides a very rich coverage for small and medium enterprises (SMEs) which are generally underrepresented in a number of existing datasets. This large variation in firms size, allows us to investigate if the lending and real outcomes significantly vary by firm size, an analysis of limited scope in existing samples that disproportionately favor large firms. Finally, in Portugal the vLTRO was not accompanied by any country-specific additional institutional changes in regulation that would confound the results.

In May 2010, Portuguese banks lost access to the international wholesale market and increased their dependence on the ECB liquidity operations (Figure 1).[6] Prior to the introduction of vLTRO, banks borrowed from the ECB in shorter maturities (from 1 week to 6 months)[7]. All Portuguese banks that had been previously using ECB funding subscribed to the vLTRO. In total, they borrowed EUR 20.2bn in December and an additional EUR 26.8 bn in February. Individual bank vLTRO uptake constituted on average 15.3% of total assets. Banks shifted most of their shorter funding needs into vLTRO. Moreover, an additional vLTRO uptake was obtained against newly pledged securities. In particular, since the policy announcement Portuguese banks purchased EUR 14 bn eligible securities that they immediately pledged with the ECB in order to obtain additional liquidity through vLTRO.

[6] Alves *et al.* (2016) nd that the banks did not freeze lending to the real economy as banks e ectively substituted their source of funding with the central bank liquidity operations.

[7] Two months before the vLTRO, ECB also introduced a one-year re nancing operations.

2 Data

For the purpose of our analysis, we create a novel dataset that matches the data of the European Central Bank and the Bank of Portugal. This dataset allows us to observe a full transmission of the unconventional monetary policy into bank lending and real economy. Below we describe the data top-down:

(Un)conventional monetary policy: We start with the ECB monetary policy and market operations data. The dataset collects the information on all ECB liquidity operations split by categories (i.e., weekly main refinancing operations (MRO); longer-term refinancing operations (LTROs) for 1, 3, 6, 12, and 36 months, respectively). The data are reported for all banks on a daily basis. In this paper, we analyze the effect of unconventional 36-month liquidity operations that were organized in two allotments, on December 21, 2011 and February 29, 2012.

Second, we use the Eurosystem Collateral Data to extract the information on the securities pledged with the ECB and used as collateral to obtain ECB funding. The ECB maintains a list of eligible assets, most of which are marketable securities. We observe the following characteristics of the pledged assets at bank-security-month level: ISIN-code, nominal value, haircut adjusted value, haircut category, quantity, issuance date, and maturity date.

Bank-level data: We use several sources maintained by the Bank of Portugal. Most notably, Securities Statistics Integrated System (SIET) contains information on the pool of all marketable securities held by banks such as quantity, book value, and market value available at bank-security-month level. We match the SIET database with the Eurosystem Collateral database to clearly disentangle the securities that were held by banks before the policy announcement (December 2011) from securities that were both purchased and pledged after the policy announcement.

Next, we use a set of bank balance sheet and prudential databases to construct controls for observable bank characteristics such as size, equity ratio, capital ratio, liquidity ratio, loan-to-assets ratio, and equity-to-assets ratio. These datasets are reported on the bank-month level. We restrict our analysis to domestic banks and domestic subsidiaries of foreign banks.[8] We drop mutual agricultural banks, whose business model differs from the rest of the banking sector.

Credit register: Central de Responsabilidades de Credito (CRC) provides monthly loan-level information on the universe of outstanding loans to Portuguese firms above the reporting threshold of EUR 50. CRC includes data on loan amounts and key loan characteristics (maturity, currency, type of the loan, and guarantee/collateral used to secure the loan, if any). CRC allows us to observe both drawn and potential credit (unused credit lines, credit cards, etc.) We focus on all outstanding loans granted by banks to non-financial firms residing in Portugal and

[8] We do not include in our analysis branches of foreign banks because those banks are not subject to prudential regulation by the Central Bank of Portugal.

borrowing in euro currency between June 2011 and June 2012. In the core part of the analysis we focus on private non-financial firms with multiple bank lending relationships, this accounts for almost 1.5 million observations and 135,751 bank-firm relationships.

Financial institutions are legally required to report data to the CRC. In return, they can access information on their current borrowers (such as their total debt and overdue or default loans from all institutions). Banks consult the CRC as part of a borrower screening for a new loan. If a bank does not currently have a relationship with a potential borrower, it can request information from the CRC at a negligible cost. All the new loan consultations are recorded and stored in the consultation database. It includes a time stamp when each bank accessed a CRC record of a potential borrower who is currently not its client. We match the consultation database with the CRC to construct the measure of a credit approval rate for the extensive margin analysis.

Firm-level data: Lastly, we match the dataset with firm-level annual census data which contain balance sheet and financial reports as well as regional and sectoral classification of the firms. We use this information to control for firm characteristics (total assets, employment, age, industry, and district) as a substitute to firm fixed effects. Additionally, we focus on investment outcomes when we examine real effects of the policy. We also use firm establishment level employee-employer database Quadros de Pessoal and following Chodorow-Reich (2014) we construct a measure of firm employment.

3 Empirical strategy

We use the difference-in-differences framework to compare lending before and after the policy intervention by exploiting the variation in the cross-section of banks' exposure to lengthening of bank debt maturity triggered by the vLTRO policy. We examine the time series evolution of bank credit supply following the baseline specification:

$$log(credit_{i;j;t}) = \alpha_{jt} + \alpha_{ij} + \beta(X_i^{RR} \quad Post_t) + \epsilon_{i;j;t} \tag{1}$$

Our outcome variable $log(credit_{i;j;t})$ is log amount of all credit that firm j obtains from bank i at time t. In the main analysis we focus on drawn credit. We define drawn credit as a sum of regular, renegotiated and under 90 days overdue loans. In the robustness, we also present results on total credit which is measured as a sum of drawn and potential credit.

We define X_i^{RR} as an exposure to the elongating of debt maturity triggered by the vLTRO policy. X_i^{RR} captures the total borrowing capacity of a bank measured three months before the policy announcement (September 2011).

First, in the baseline, we construct a liability-side measure as a sum of all liquidity providing operations from the ECB taken up by banks and normalized to their total assets. These resources were readily available to banks to be swapped from existing short-term maturities to three-year loans by keeping all other conditions unchanged (e.g. eligible securities, haircuts, interest rates). In other words, banks were able to costlessly increase maturity of their liabilities with the ECB and hence lower their rollover risk. We choose our treatment to be a continuous variable to address a concern that the policy did not affect all treated banks equally. In fact, banks swapped on average 86 % of their short-term ECB funding into the vLTRO.

As a robustness, we also consider the asset-side measure $X^{RR}_{i;eligSec}$ that captures total banks holdings of securities that are eligible as a collateral for ECB funding.[9] Banks with larger holdings of these securities are eligible to subscribe to more vLTRO funding.

In order to check how our measures of exposure correlate with the realized vLTRO uptake ($vLTRO_i$), we run the following regression:

$$vLTRO_i = \alpha + \beta X^{RR}_i + \epsilon_i \tag{2}$$

As evidenced in Table 1, our measures of exposure are strongly, positively correlated with the actual 3-year ECB liquidity uptake.

To minimize endogeneity, we focus on cross-bank variation in September 2011 (three months before the policy announcement).[10] Nonetheless, we observe that banks were very rigid in changing their exposure to the ECB (Figure 4) and security holdings, and the policy announcement on 8 December 2011 came to a large extent as an unexpected shock. In fact, we do not observe any significant changes in the bank security holdings or pledging behavior in the period prior to the policy announcement.

Next, given that the dependence on short-term ECB repo funding prior to the policy announcement is a bank's choice, a potential concern is that our choice of treatment can be correlated with bank's observables. To address this concern, we run univariate regressions to regress our baseline exposure measure X^{RR}_i against a wide set of bank's characteristics:

$$X^{RR}_i = \alpha + \zeta W^{Sep2011}_i + \epsilon_i \tag{3}$$

We examine a wide range of observables such as banks' size, capital ratio, loan composition, deposits, profitability, liquidity, leverage and equity ratios and we report the estimates of univariate regressions (ζ) in Table 2. The results suggest that banks' dependence on ECB funding in September 2011 is only correlated with banks' dependence on market repo funding before the financial crisis (we use the measure from 2005). This observation suggests that banks that were more reliant on ECB secured repo funding in 2011 used to be generally more reliant on secured

[9] This approach is also analogous to the exposure measure to the quantitative easing in the US by Rodnyansky and Darmouni (2017).

[10] We do so also to avoid a potential threat of capturing the e ect of 1Y-LTRO which was announced on 6 October 2011 and replaced by the vLTRO in December 2011.

market funding prior to the crisis. As a reaction to the financial crisis, banks swapped their secured repo loans obtained in the market into the ECB as a safer form of financing. Drechsler *et al.* (2016) also show that the ECB offered lower haircuts to securities issued in distressed countries (such as Portugal) and hence banks could also receive more favorable conditions from the ECB as when borrowing on the market.[11]

In our difference-in-difference regressions, we analyze a period of 13 months: June 2011–June 2012 and we use the data of monthly frequencies. $POST$ is a dummy variable equal to one in the post-treatment period after the second allotment (February–June 2012). We end our sample period in June 2012 to avoid an overlap with the announcement of Outright Monetary Operations (OMT) – the "whatever it takes" speech of the president of the ECB Mario Draghi in July 2012.

We also saturate our model with fixed effects to address some of the main empirical challenges. First, a potential bias in estimating the causal effects of the liquidity injection can stem from the interaction of demand and supply for credit. In line with Khwaja and Mian (2008) we incorporate firm fixed effects to isolate the causal effect of the policy by comparing lending outcomes of the *same firm* borrowing from at least two differently exposed banks. In our time series set-up, we rely on monthly frequency credit registry data and implement firm-time fixed effects.[12]

Second, bank-firm matching is not exogenous as banks choose their borrowers (or vice-versa) and firm borrowing relationships can also be of a different quality across different banks. In our preferred specification, we address a potential bias related to i) the correlation of our exposure measure with observable and unobservable bank characteristics and ii) non-exogenous bank-firm matching by including bank-firm fixed effects that absorb any time-invariant bank, firm and bank-firm variation.

To sum up, our empirical specification relies on a combination of *firm-time* and *bank-firm* fixed effects (Equation 1). The main results are presented for domestic and foreign non-financial firms and non-profit firms which we denote as *Private NFCs*. We also report results which further include self-employed entrepreneurs and public companies denoted as *All firms*.

Finally, we take several steps to support our identification. First, we examine the existence of parallel trends by comparing lending dynamics of exposed and non-exposed in the period leading up to policy. Second, we use the 2007 unexpected freeze of European interbank market as a placebo sample. Still, a potential threat to our identification strategy is the existence of time-varying bank specific characteristics that would be correlated with our choice of exposure.

[11] Drechsler *et al.* (2016) show that at the end of 2010 the ECBs haircut on 10-year Portugal bonds was 4%, while LCH Clearnet, a private repo exchange, applied a 10% haircut on these bonds.

[12] We also performed the same analysis on a collapsed model by comparing average outcomes of the pre- and post- period.

One thing that we can do to address this issue is to check for any concurrent policy actions. At the time of vLTRO, four banks were undergoing the stress tests conducted by the European Banking Authority. To examine if our results are robust to this potential confounding factor, we exclude these banks from our sample as a part of the robustness exercise.

4 Results

4.1 Intensive margin

Table 3 summarizes the main result on the intensive margin following the Equation 1. We exploit the variation in the cross-section of the ex ante bank exposure to the vLTRO. We construct the measure of exposure X_i^{RR} as the amount of total secured short-term funding received from the central bank one quarter before the policy announcement. This represents a measure of existing borrowing that can be costlessly swapped into the three-year repo loan.

Column (1) presents the results using the full sample of loans (bank-firm pairs). Here we introduce bank fixed effects (to absorb any time invariant bank characteristics) and time fixed effects. From Column (2) onward we restrict the loan sample to only firms that at each month borrow from at least two banks. We denote this as "multiple bank relationships". In Column (3) we replace time fixed effects with firm-time fixed effects to absorb any variation from firm-level (demand) changes in line with Khwaja and Mian (2008).

It is well plausible that a firm has a different relationship with different banks. To address this challenge, we first introduce a set of bank-firm control variables (Column 4) and finally in our preferred specification shown in Column (5) we also saturate the model with bank-firm fixed effects to address any potential threat coming from non-exogenous matching between banks and firms.

The coefficient estimate of β for all specification is positive and statistically significant suggesting that lengthening of bank debt maturity induced by the vLTRO had positive impact on bank lending to firms. In terms of economic significance, the coefficient estimate of 0.631 (Column 5) implies that a one standard deviation increase in bank exposure to vLTRO (8.4 percent from Table 14) is associated with an 5.3 percent increase on the intensive margin.

In both cases, we can also observe that specifications which are not controlling for overall firm's credit demand overestimate the true effect of the policy exposure on lending. The coefficient of interest β decreases in magnitude when we introduce the firm-time fixed effects but it remains stable, positive and statistically significant across all specifications.

The results suggest that more exposed banks could transform larger share of their funding from the short-term into long-term debt which in times of the sovereign debt crisis in Portugal contributed to the reduction in rollover risk and caused positive effect on bank lending to firms. This empirical evidence is consistent with implications from theoretical models of He and Xiong

(2012) and Stein (2012). In the model of Stein (2012), excessive amount of short-term debt and its refinancing during the crisis forces banks to sell some of their assets at fire-sale prices that contributes to tightening of their financial constraints. Cheaper debt of longer maturity (like in the case of vLTRO) would be socially beneficial by reducing this externality and increasing lending to firms in general. Similarly, He and Xiong (2012) find that in bad times, when assets prices are more volatile, long-term debt reduces rollover risk and incentivizes equity-holders to undertake profitable investment.

In Table 4, we show that our findings are robust to the collapsed specification of difference-in-differences which compares the average lending before and after the policy announcement. While the effects are quantitatively similar, we prefer to use the time-series specification as a baseline since it allows us to absorb any observable and unobservable time-invariant bank-firm characteristics.

In Table 5, we present that our findings are also robust to sample changes and alternative measures of the exposure to the vLTRO. Columns (2)–(6) show variations to the baseline. Column (2) uses an alternative exposure measure $X^{RR}_{i;eligSec}$ defined as a sum of all securities eligible for the vLTRO and held by the banks prior to the vLTRO (as a share of total assets). Column (3) shows results for the dependent variable measured as a log of total credit. We define total credit as a sum of drawn credit and potential credit (i.e. unused credit lines which are reported off-balance sheet). Our results are also robust to different definitions of firms. While the baseline results are reported for private non-financial corporations, we also extend the definition of firms to account for public firms and individual entrepreneurs which leads to an increase of the sample by additional 900,000 observations (denoted as *All rms* in Column(4)).

A possible concern is that banks that did not participate in the ECB's open market operations could be significantly different from the exposed banks. Based on the balancing checks reported in Table 2, we do not find any observable differences between more and less exposed banks. Nonetheless, in Column (5) of Table 5 we only focus on the variation in the cross-section of exposed banks and the results remain robust.

Yet, a potential threat to our identification strategy is the existence of time-varying bank specific characteristics that would be correlated with our choice of exposures. One thing that may allow us at least in part to look into potential threads is to check for any concurrent policy events. In case of Portugal, four main banks were undergoing the stress test exercise of the European Banking Authority (EBA) around the same time as the vLTRO As a result, in Column (6) we drop these banks from our sample but the results remain robust to the baseline.

We report estimates using two-way clustered standard errors at bank-time and firm level. We choose this method of clustering as it allows us to address a threat that firm-shocks can be serially correlated and also bank-time shocks (our source of variation) can be correlated across firms. Our results are robust to alternative clustering either at the bank and firm level; or only at the bank level and we report these estimates in the Appendix Table 16.

4.2 Dynamic difference-in-differences version

We modify the empirical framework into a dynamic difference-in-differences version (leads-and-lags around the treatment effect). This specification allows us to observe how quickly banks reacted to the policy and whether the impact accelerates, stabilizes or reverts to the mean (Autor 2003). One might also infer causality via the timing of policy and visually examine pre-existing parallel trends. Our dynamic difference-in-differences is summarized as follows:

$$log(credit_{i;j;t}) = \alpha_{ij} + \sum_{k \neq 2011m9} \beta_k \mathbf{1}_{t=k} + \sum_{k \neq 2011m9} \gamma_k (X_i^{RR} \quad \mathbf{1}_{t=k}) + \epsilon_{i;j;t} \tag{4}$$

We start with a simple binary set-up where we divide banks into two groups: exposed and non-exposed. Here, we consider both measures of exposure: as in the baseline, we first set X_i^{RR} to 1 if a bank i was borrowing from the central bank in repo operations prior to the vLTRO announcement and 0 otherwise. Second, as in the robustness, we set the X_i^{RR} to 1 if a bank i was holding securities eligible for the vLTRO prior to the policy announcement and 0 otherwise.

Figure 5 represents the sequence of coefficient γ_k estimates for each month t from Equation 4. Panel (a) shows the results for short-term secured borrowing and panel (b) for eligible securities. The key identifying assumption of the DID framework is the existence of parallel trends prior to the policy announcement. Figure 5 indicates that there was no significant difference in lending by exposed and non-exposed banks prior to the policy implementation. This serves as an evidence of no pre-trend. However, after the vLTRO we can observe significant differences in lending behavior between exposed and non-exposed banks.

Second, we replace the binary X_i^{RR} with the continuous measure which we present in Section 4.1. As in our main result, X_i^{RR} is defined as a sum of all liquidity providing operations from the ECB taken up by banks and normalized to their total assets. These resources were readily available to banks to be swapped from existing short-term maturities to three-year loans by keeping all other conditions unchanged. Figure 6 illustrates the full dynamic impact of exposure to the vLTRO. Again, in months leading up to the vLTRO we find no difference in lending between more exposed and less exposed banks which is in support of parallel trends. Notably, after the vLTRO the coefficients rise and remain persistently positive and statistically significant. This further confirms the positive effect of lengthening maturity of bank debt on bank lending.

4.3 Placebo test: 2007 freeze of the European interbank market

What if banks more exposed to the 2011 vLTRO are generally more prone to react to bank-specific shocks? This would mean an important threat to our identification strategy as our measure of exposure would merely capture banks which are inherently stronger propagators of shocks. To address this question, we use the finding of Iyer *et al.* (2014) who show the negative credit supply effects of the 2007 unexpected liquidity crunch using Portuguese credit registry data. They find that that banks that relied more on interbank borrowing decreased their credit supply more following the sudden freeze of the European interbank market in August 2007. We use the 2007 liquidity freeze as a placebo sample to investigate whether the banks more exposed to the vLTRO in 2011 were also more sensitive to the 2007 liquidity dry-up.

We follow the dynamic set-up specification from Equation 4 and we replace the LHS lending outcomes in 2011–2012 with the lending outcomes in 2007. Figure 7 depicts the sequence of coefficient γ_k estimates for each month t in 2007. If the 2007 and 2011 exposures were spuriously correlated, we would expect negative and statistically significant coefficients after August 2007. Instead, we however find that the plotted estimates of γ_k are not statistically different from zero throughout 2007. As a result, we have no evidence that the banks exposed to vLTRO are generally more affected by liquidity shocks.

4.4 Extensive margin

Did vLTRO affect bank-lending behavior also on the extensive margin by enabling banks to ***not*** stop lending to firms altogether in the period of the sovereign debt crisis or even helping them to establish new lending relationships?

We start by looking into the probability of the "exit rate" of existing loans (bank-lending relationships). Consider a simple collapsed version of difference-in-differences where we compare the period after the vLTRO (2012m2–2012m6) with the period before the vLTRO (2011m6–2011m10). We set the variable EXIT to 1 if the loan only appears in the pre-treatment period but ceases to exist in the post-treatment period. In other words, the loan is not renewed and the bank-lending relationship is terminated. In case the loan appears both in pre- and post-period, EXIT is classified as 0. As before, we follow the Khwaja and Mian (2008) approach and we absorb firm fixed effects to investigate whether the same firm borrowing from multiple banks is less likely to exit the loan from a bank that was more exposed to the positive shock to the debt maturity. This set up is summarized as follows:

$$EXIT_{i;j} = \alpha_j + \beta X_i^{RR} + \tau B_i + \epsilon_{i;j} \tag{5}$$

Table 6 summarizes the results. Column (1) uses a combination of bank controls (size, capital ratio, liquidity ratio, equity-to-assets and loan-to-assets ratios) as well as firm controls (firm size, district NACE-2 industry FE), while Column (2) replaces firm controls with firm fixed effects as described in Equation 5 above. Both results are quantitatively similar and suggest that a 1 standard deviation increase in the exposure to the vLTRO (8.4 percent) decreases the exit rate by 6.6 percent (8.4 0.782). This confirms the hypothesis that in the period of a severe crisis, banks that are more exposed to the policy of elongating debt maturity are less prone to terminate relationships with firms and hence also have a slower pace of financial deleveraging.

Are more exposed banks also more prone to start lending to new clients? To address this question, we match the firm credit registry data with the firm credit consultation data. Utilization of consultation data allows us to directly observe which banks consulted the loan application and whether this application successfully translated into new lending.

Consultation data: We exploit the matching of the Portuguese credit registry with the loan consultation database. The consultation database has a time stamp when a bank reviewed a credit record of a firm that is currently not its client (i.e., bank and firm do not have a lending relationship). Banks have access to the credit records of existing borrowers without any need to use the consultation database. To obtain records of new firms, banks can access the consultation database upon firm's consent at a low monetary cost. Banks tend to use the consultation database as part of the screening in the loan-granting process.[13]

We analyze all loan consultations after the announcement of the vLTRO and we match them with actual entries in the credit registry. We construct a dummy variable $newLoan_{i;j}$ which takes a value of 1 if a bank-firm consultation entry is matched with a new bank-firm record in the credit registry and 0 otherwise.[14] In the main analysis we focus on consultations made between December-April and we match them with credit registry outcomes in the period December–June. The majority of the loans are approved within two months. Roughly 10% of loan consultations are successful and appear in the credit registry as new loans.[15] We estimate the extensive margin with the following specification:

$$newLoan_{i;j} = \alpha_j + \beta X_i^{RR} + \tau B_i + \epsilon_{i;j} \quad (6)$$

We focus on the effect of elongating bank debt triggered by the vLTRO policy on the probability of making a new loan. We start by controlling for both bank and firm-specific characteristics and later we replace firm controls with firm fixed effects. We cluster the errors on the bank level. The main results are ran as linear probability models as it is the most standard approach in the literature (see Khwaja and Mian (2008), Jimenez *et al.* (2012)) and it is comparable to FE specification and the rest of the results in the paper. Logit and probit versions, however,

[13]Consultation database o ers only a subset of information the banks can see in the credit registry for their existing clients. Hence it would be suboptimal for banks to use consultation database for their current clients during the loan approval process.

[14]Our construction of the extensive margin is similar to Jimenez *et al.* (2014b) and Jimenez *et al.* (2012) who study loan approvals in Spain. Spanish CIR credit registry has a very similar data structure to the Portuguese CRC.

[15]We also perform robustness tests for changes in the consultation window and the results are not a ected if we change the consultation sample by +/- 1 month: December-March and December-May.

provide us with very similar results.

Firm fixed effects absorb any changes at the firm level, including credit demand. In practice, this condition may seem restrictive – we require a firm to consult a potential lending condition with at least two banks from which it is currently not borrowing. Although the FE sample size drops by a half, we still record 41,000 loan consultations. Table 7 shows that the coefficient is very stable with or without including firm FE. In terms of sensitivities, a 1 standard deviation increase in the exposure to the reduction in rollover risk increases the probability of making a loan to a new client by 5.3 percent (0.63 0.84) – surprisingly, this is identical to the magnitude previously reported for the intensive margin.

4.5 Do changes in maturity of bank debt affect maturity of loans to firms?

In this section we explore if and how the longer maturity of liabilities affected maturity transformation practiced by banks. In an ideal experiment, we want to isolate the changes in maturity from the changes in loan size within the credit supply and at the same time we want to control for the firm demand side. As a result, we restrict the analysis on 10,863 bank-firm relationships for which the total outstanding amount of all lending is constant over the entire period of June 2011–June 2012. Unfortunately, this restriction does not allow us to further implement preferred firm FE as the sample would drop to only 800 observations. Instead, we opt to focus on a fixed loan size bank-firm sample while controlling for firm characteristics by using firm controls, firm-bank controls, and firm industry-district fixed effects.

In the data, we observe two measures of maturity: original and residual. Original maturity is a maturity at origination and unless a bank/firm renegotiated the maturity extension, it remains unchanged until the loan is repaid. Residual maturity, on the other hand, decreases as the borrower repays the loan. We estimate changes in the original maturity by running a difference-in-difference estimation on the sample of bank-firm pairs with constant outstanding loans.

$$\Delta Maturity_{i;j} = \alpha + \beta X_i^{RR} + \delta Q_{ij} + \tau B_i + \mu F_j + \epsilon_{i;j} \quad (7)$$

Equation 7 summarizes the main set-up.[16] The outcome variable $\Delta Maturity_{i;j}$ is a change in the median original maturity between the *pre-* (June–October 2011) and *post-* (March–June 2012) treatment period. As before, $X_i^R R$ refers to the measure of exposure to the ECB policy in September 2011. B_i, Q_{ij}, F_j are sets of bank, bank-firm and firm controls respectively, which we progressively introduce to the specification. Table 8 summarizes the results. The positive and statistically significant coefficients suggest that the reduction in rollover risk and extension of maturity of liabilities might have a positive effect on the maturity of assets (loans) of banks –

[16] This set-up is analogous to the collapsed version of the intensive margin in Online Appendix.

in the post-treatment period banks more exposed to the vLTRO modify more of their existing loans by extending their original maturities.

Our estimation strategy might provide a more conservative estimate of the actual effects on a loan maturity change. It is very plausible that banks also extended maturities along with changes in the outstanding amount as a part of the loan renegotiation process or that newly issued loans were now granted with longer maturities. These hypotheses are, however, more challenging to test in the absence of a valid counterfactual.

5 Firm characteristics

In this section we examine the impact of the liquidity provision by firm type. We exploit the detailed matching of the credit registry data with firm annual census data and we analyze heterogeneous treatment outcomes by main firm characteristics (F_j): size, age, riskiness, and the length of a bank-firm relationship. We start by estimating the most generalized specification of intensive margin with both firm-time and bank-firm fixed effects and we introduce a triple interaction term $X_i^{RR} \quad Post_t \quad F_j$ to measure which firms were more affected by the vLTRO funding. Equation 8 summarizes the set-up:

$$log(credit_{i;j;t}) = \alpha_{jt} + \alpha_{ij} + \beta_1(X_i^{RR} \quad Post_t) + \beta_2(X_i^{RR} \quad Post_t \quad F_j) + \epsilon_{i;j;t} \qquad (8)$$

Table 9 presents the results. We find that positive effect of banks' debt maturity extension is stronger for smaller and younger firms (negative and statistically significant triple-interaction coefficients in Column (2) and (3)). Furthermore, vLTRO induced stronger bank lending channel to riskier firms (Column (4)). For firms' riskiness we utilize the measure of Antunes *et al.* (2016) who develop default probabilities for Portuguese firms. Last, we find that firms with shorter relationship with banks are more affected by the positive credit supply shock.

Our findings on the heterogeneous effects of vLTRO varying by firm size are symmetric to the existing work on the propagation of negative shocks to small firms (Gertler and Gilchrist (1994), Kashyap *et al.* (1994), Khwaja and Mian (2008)). Our results also suggest that banks pass through positive debt maturity shock more to risky firms. This observation is in line with the findings on bank risk-taking behavior in response to changes in overnight interest rates (Jimenez *et al.* (2014b)) or positive liquidity shock (Crosignani *et al.* (2015)).

6 Firm-level credit and real outcomes

As a next step, we collapse credit registry loan-level data to the firm level. Following the literature on the bank-lending channel, we examine whether the significant loan-level results reported so far also translate to firm-level credit and real outcomes. In other words, we want to investigate if firms "hedged" against changes in bank lending following the vLTRO. We estimate the following equation:

$$\Delta log(y_j) = \alpha + \beta\overline{X_j^{RR}} + \tau\overline{B_j} + \delta\overline{Q_j} + \mu F_j + \epsilon_j \tag{9}$$

We start by analyzing the impact on firm-level credit. We compute our outcome variable as a change in sum of all bank lending on the firm level between the ***pre-*** (June–October 2011) and ***post-*** (March–June 2012) treatment period. For each firm, we compute an indirect measure of X_j^{RR} as a weighted average of the X_i^{RR} of all banks a firm j is borrowing from. The weights are based on the firm's credit in the pre-treatment period:

$$\overline{X_j^{RR}} = \frac{\sum_i (X_i^{RR} \times credit_{i,j,t=pre})}{credit_{j,t=pre}}$$

We repeat the same procedure for computing an indirect measure of bank and bank-firm control variables (denoted as $\overline{B_j}$ and $\overline{Q_j}$). We also include firm controls (log of firm's total assets and industry-district fixed effects). Table 10 presents the results. Firm-level effects on credit (Columns (1) and (2)) are consistent with the results reported on the loan level. In particular, the effect of the exposure to the reduction in rollover risk on aggregate firm-level credit is positive and statistically significant and stronger for smaller firms. For column (1) we further compute bias-corrected estimates as proposed by Jimenez *et al.* (2014a), the corrections cause the coefficient to drop by a half but the final estimates still remains positive, suggesting that the effect of vLTRO remains positive also on the firm level.[17]

Next, we use those results to compute aggregate effects of the vLTRO on credit to firms. For this back-of-the-envelope calculation, we assume partial equilibrium setting. We plug the estimates from Column (1) of Table 10 into Equation 9 in order to compare the predicted firm-level credit with the policy ($\beta = 0.378$) and without it ($\beta = 0$). We estimate that for every EUR 100 of debt swapped from short-term to long-term, firms received EUR 2.5 in new lending. Put differently, we estimate that the reduction in rollover risk triggered by the vLTRO contributed to approximately EUR 1 bn in lending to firms in Portugal. If we compare the credit development to the counterfactual world without any intervention, we find that although the policy did not stop the ongoing credit contraction, it significantly reduced its pace. We estimate that without the policy the credit would have contracted by additional 4 percentage points. The observed credit contraction in the period after the vLTRO was -4.5% while we estimate that in the absence of the policy the contraction would have been -8.5%.

[17] Following Jimenez *et al.* (2014a), we compute the bias-correct coefficient as:

$$\hat{\beta}_j = \hat{\beta}_{j,OLS} - (\hat{\beta}_{OLS} - \hat{\beta}_{FE}) \frac{var(X_i^{RR})}{var(\overline{X_j^{RR}})} = 0.700 - (1.485 - 1.428) \frac{0.0844^2}{0.03549^2} = 0.378$$

Our analysis so far suggests strong effects of the unconventional liquidity provision on lending. But are these changes substantial enough to have real effect? To examine this question, we follow the same procedure and estimate the impact on investment and employment (Equation 9).

We measure $\Delta log(investment_j)$ as an annual log change of investment using firm-level census data. Columns (3) and (4) of Table 10 summarize the regression results. Two important observations stand out. First, the indirect measure of the reduction in rollover risk $(\overline{X_j^{RR}})$ is consistent with our previous results. Second, these estimates suggest that smaller firms are more exposed to the positive effects of the reduction in bank rollover risk propagated through the bank lending channel.

Finally, we examine $\Delta employment_j$, which we measure using establishment-level employment microdata following employment growth rate definition also used in Chodorow-Reich (2014). While we find no average firm-level effect on employment (Column (5) Table 10), when differentiating firms by their size, we report a positive effect on employment for small firms (Column (6)). This is also consistent with our previous findings that highlight stronger bank-lending channel responses for smaller firms in Portugal.

7 Policy side-effects and the impact on lending

Until this point, we have discussed the effects of a bank debt maturity extension induced by vLTRO which allowed banks to shift maturity of central banks' liquidity they held before the vLTRO announcement towards longer maturities. In this section, we address the changes in the bank risk behavior between the policy announcement and the allotment, and their implications on lending.

After learning the details of the policy announcement, banks increased their pledging of eligible securities with the central bank. More importantly, banks did so by purchasing additional eligible securities. This phenomenon has been discussed in the literature as collateral trade (Acharya and Steffen (2015), Crosignani *et al.* (2017)). The perils of this behavior included strengthening of the diabolic loop (Brunnermeier *et al.* 2016a) between the sovereign and the banking sector as banks increased their home bias by purchasing more domestic government bonds.

One potential challenge is to quantify the magnitude of the security carry trade. We design a measure of *purchased & pledged securities* (PP) that matches ISIN-level security holdings database maintained by the Bank of Portugal with ISIN-level security pledging database of the European Central Bank. This perfect matching allows us to precisely measure the pledging uptake from eligible assets purchased only *after the policy announcement*. Hence, we do not capture the delayed pledging of pre-existing holdings nor the security purchases that were not used with the vLTRO. Construction of the PP thus allows us to precisely measure the pledging uptake from securities purchased after the banks learned the details of vLTRO in the announcement. To the best of our knowledge, this is the first paper that directly measures the magnitude of the security carry trade of banks. We start by exploring possible hypotheses to explain the bank behavior and in the next step we examine its potential impact on bank lending.

7.1 Motives behind purchasing eligible securities

Why did some banks purchase eligible securities in order to obtain vLTRO funding and others not? In this section, we test three hypotheses that were previously emphasized in the literature (see Acharya and Steffen (2015), Crosignani *et al.* (2017), Abbassi *et al.* (2016) for details).

First, undercapitalized banks could shift their exposure from private lending to government and bank securities in order to strengthen their regulatory capital position without issuing new equity. This motive arises due to the lower risk weights applied to bank and government securities when calculating the capital ratios in the Basel II regime. Second, banks short on liquid assets could substitute illiquid lending to firms with liquid securities. As a result, they could respond better to negative funding liquidity shocks or dry-ups. Third, banks with different business model (i.e., specializing in security trading) may react differently to the newly emerged trading opportunities. They could purchase additional high-yield securities to profit from their potential price increase without exposing themselves to large liquidity risk thanks to the vLTRO. In order to test which of the motives was present between the announcement and the final vLTRO allotment, we run the following OLS regression:

$$PP(bv)_i = \alpha + \theta BankChar_i + \epsilon_i \tag{10}$$

where $BankChar_i$ is a proxy for one of the three motives mentioned above. We proxy the capital hypothesis with different measures of capital ratio (capital adequacy ratio, Tier 1 ratio and Core Tier 1 ratio) in September 2011. To test for the liquidity motive, we use three measures of liquidity: (i) cash only, (ii) cash and short-term MFI loans and (iii) cash, short-term MFI loans and short-term securities, all measured in September 2011 and normalized to total assets. According to the reasoning behind those motives, banks, that are undercapitalized or have a shortfall of liquid assets (and have a lower capital or a lower liquidity ratio), are expected to purchase more securities.

To proxy for a business model we use the average amount of security holding in a period of one year prior to the policy shock. Additionally, we also use the variation in sovereign bonds purchases in 2009. The first measure stems from the notion that banks active in the financial markets generally hold more securities on their balance sheets. The rationale behind the second one is connected with banks' strategic behavior at the beginning of the sovereign debt crisis in the Eurozone in 2009. As documented in a number of studies (see for example Acharya and Steffen (2015), Drechsler *et al.* (2016), Becker and Ivashina (2014), Battistini *et al.* (2014)), banks from peripheral countries purchased additional domestic government bonds after investors started to perceive those securities as riskier and the yields on those securities increased. Figure 10 in the Appendix documents a strong uptake of government debt holdings for *some* Portuguese banks in 2009.[18] If the purchases of eligible securities are correlated with bank business model, one would expect that banks that held more securities and purchased high-yield government bonds during the sovereign debt crisis would have also purchased securities during the vLTRO inter-allotment period.

Table 11 presents the results of Equation 10. We find that none of the estimates that proxy for a capital or liquidity motive is statistically significant which gives us no evidence to assume that capital or liquidity needs might explain strategic security purchases after the policy announcement. In contrast, we observe positive and statistically significant coefficient for both proxies for the bank business model hypothesis. This positive relationship is consistent with the empirical evidence in Abbassi *et al.* (2016). Using bank security-level data and credit register in Germany, the authors find that when fire sales occur, banks with higher trading expertise withdraw funding from corporate lending and invest in securities after their price drops significantly.

The trading strategy based on purchases & pledging of eligible securities can be justified from the perspective of both associated risk and profitability. First, by offering very long-term repo loans, the ECB implicitly increased the pool of eligible assets that mature within the repo period and it hence lowers banks' repayment risk, i.e. the risk that the price of the asset at the end of the repo will be lower than the price of the loan to be repaid. 90% of purchased & pledged securities had remaining maturity below 3 years and 44% of them had maturity between 6 months and 3 years. Second, in terms of returns, 99% of securities offered high yields that were well above the vLTRO interest rate (which, following the ECB accommodative policy, gradually decreased from 1% in 2012 to 0.05% in 2015). Figure 9 presents the histogram of yields to maturity offered by the purchased securities. We also estimate that on average banks obtained an annual return of 7% in capital gains from this strategy. Here, we assume that banks held securities until they matured if the maturity date was before the final vLTRO repayment date and they sold securities with remaining maturity above three years at the vLTRO repayment date. This also confirms the evidence of a "divine loop," as an opposite to a "diabolic loop" (Brunnermeier *et al.* 2016b) . With a benefit of hindsight, we see that the securities appreciated in value, bank realized capital gains and improved their balance sheet positions – a mechanism also related to "stealth recapitalization" by Brunnermeier and Sannikov (2016). Brunnermeier *et al.* (2016b) further point out that stronger balance sheets eased banks' position in subsequent

[18] We observe signi cant heterogeneity in banks' government security purchases in 2009. As a result, this exercise compares the correlation in the cross-section of bank security purchases in 2009 and in the vLTRO period (2011{20012.)

stress tests.

7.2 Security carry trade and lending

Next, we extend our framework to incorporate the impact of strategic security purchases on lending to firms. During the sovereign debt crisis, peripheral banks were financially constrained and could not expand their balance sheets freely. To test this hypothesis, we first regress the size of the balance sheet on the amount of purchased & pledged securities:

$$\frac{BSitem_{i;t}}{TotalAssets_{i;Sep11}} = \alpha_i + \alpha_t + \xi PP_i \quad Post_t + \epsilon_{i;t} \tag{11}$$

We start with the specification where $BSitem_{i;t}$ refers to the total size of banks' assets. We document that banks involved in the security trade did not use vLTRO funding to expand their balance sheet (Column (1) of Table 12).

Next, we replace $BSitem_{i;t}$ with the total size of central bank funding and non-core liabilities, respectively and we report the regression coefficients in Columns (2) and (3) of Table 12. We confirm that as expected banks that purchased & pledged more securities increased their dependence on the central bank (positive and statistically significant coefficient estimate in Column (2)). Subsequently, we also find that banks that purchased & pledged securities to obtain additional vLTRO funding used the ECB liquidity to decrease their non-core liabilities.[19]

As a consequence, the purchase of eligible securities might come at the cost of balance sheet re-balancing. In order to test whether banks substituted lending to firms with security purchases, we exploit the variation *within exposed banks*. We run the baseline regression on intensive margin with an additional explanatory variable *purchased* & *pledged securities* (PP) normalized to total assets in September 2011. Equation 12 summarizes the set-up:

$$log(credit_{i;j;t}) = \alpha_{jt} + \alpha_{ij} + \beta(X_i^{RR} \quad Post_t) + \gamma(PP_i \quad Post_t) + \delta Q_{ij} + \epsilon_{i;j;t} \tag{12}$$

In a frictionless world, banks can expand their balance sheets to accommodate higher security holdings. The impact of the sovereign debt crisis however negatively affected balance sheets of European banks, in particular in periphery countries. As we have shown in Table 12, banks in fact did not expand their assets and, hence, the purchase of eligible securities might come at the cost of substitution.

In Table 13, we examine whether banks substituted lending to firms for security holdings. We take advantage of the variation *within exposed banks* and the fact that not all banks that subscribed to the vLTRO purchased additional securities after the policy announcement. In other words, we consider only banks that participated in the vLTRO and we exploit additional

[19] We measures non-core liabilities as a sum of unsecured deposits of nancial institutions and issued debt securities.

variation in their purchase & pledge behavior (measured as PP in Equation 12). We report the results both for PP measured in the book value (*bv*) and as a value-adjusted haircut (*vah*). We normalize both measures to total assets in September 2011.

We find that the effect of the exposure to the lengthening of bank debt maturity remains positive and statistically significant as when estimating the baseline Equation 1. By contrast, the coefficient for the purchase & pledge behavior on lending to firms is negative and statistically significant. The estimate is stable for both book and value-adjusted haircut valuations. This result favors the existence of a balance-sheet substitution from lending towards security holdings after the vLTRO announcement and suggests the existence of policy side-effects.

Our result is analogous to the crowding out effect of commercial and industrial lending (C&I) induced by quantitative easing (QE) in the US documented by Chakraborty *et al.* (2017). They find that banks decreased C&I lending and moved funds into mortgage origination and creation of the mortgage-backed securities in response to announced purchases of these securities by the Federal Reserve within the QE program. Both empirical findings can also be related to the "winner-picking" practice of actively shifting resources from one project to another observed in theoretical models featuring internal capital markets (see for example Stein (1997) and Scharfstein and Stein (2000)). From the theoretical perspective, there is also a growing literature analyzing the impact of security trading by banks on credit supply during a crisis. Diamond and Rajan (2011) find that banks load their portfolios with high-yield securities prone to fire sales which lead to contraction in credit supply. Shleifer and Vishny (2010) show that banks prefer to shift capital from new loan investment into buying underpriced securities when the latter bear higher return.

This part of our analysis is also related to the work of Becker and Ivashina (2014) and Crosignani *et al.* (2017). Becker and Ivashina (2014) find that increased holding of government debt by the Eurozone banks generated a crowding out of corporate lending. While Becker and Ivashina (2014) measure this effect using the notion of firm switching from loan to bond financing as a proxy for higher cost of bank credit, we can directly observe changes in loan quantities for firm borrowing from two banks, one of which purchased securities while the other did not. Crosignani *et al.* (2017) show that banks in Portugal acquired eligible securities after the vLTRO announcement and those purchases are correlated with the vLTRO uptake during second allotment. While authors mainly focus on the role of short-maturity government bonds, we exploit the detailed bank-security level data on both sides of the vLTRO contract to identify which securities were used as collateral to obtain this funding. We find that banks purchased not only government bonds but also bank bonds. Furthermore, we show how this strategic activity transmitted into the bank-lending channel.

8 Conclusion

This paper contributes to the literature by providing new empirical evidence on how lengthening of maturity of bank debt in times of crisis reduces the rollover risk and affects bank lending. To this purpose, we exploit the policy change of the ECB's unconventional liquidity provision as a quasi-natural experiment of lengthening debt maturity. Using a novel dataset that perfectly matches the ECB monetary policy and market operations data with the firm credit registry and banks' security holdings in Portugal, we find that banks more exposed to the lengthening of debt maturity deleveraged more slowly than did the less exposed banks. Our findings are economically significant for both existing and new credit. We show that a reduction in rollover risk has a positive and economically sizable impact on both bank lending to the real economy, investment and employment of firms.

Furthermore, we highlight how unrestricted liquidity provision contributed to banks' incentives to hold more securities and substitute lending to the real economy for security holdings. We find that the magnitude of these side effects are dependent on the business model of banks. This finding might be helpful in thinking about a design of policy regulation in the future. In fact, some of the subsequent central banks' initiatives aimed to enforce conditionality behind liquidity provisions to make sure banks pass through positive stimulus to the real economy – examples include ECB's Targeted Longer-Term Refinancing Operation (TLTROs) and Funding for Lending Scheme by the Bank of England. Yet, there is still little evidence to how successful stronger conditionality of policy stimulus would translate into lending and in particular real outcomes that opens new avenues for future research.

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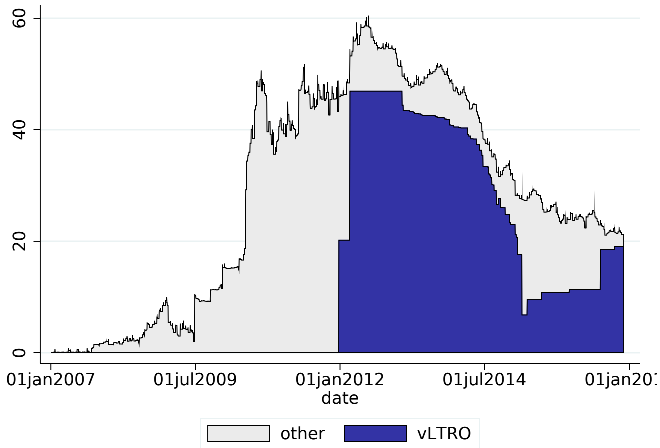
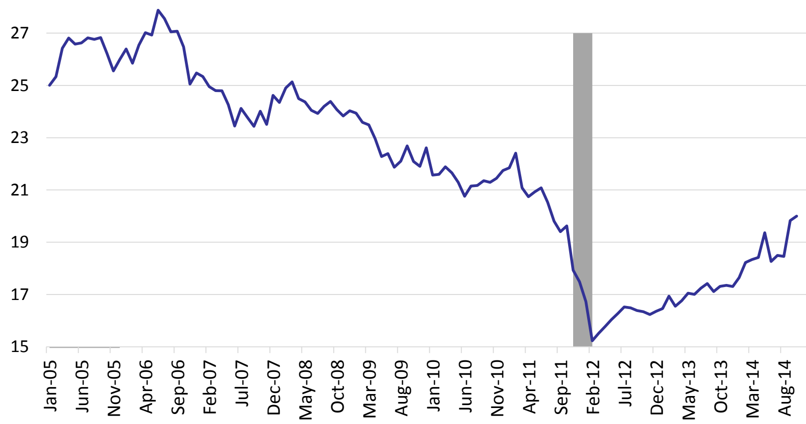
9 Figures and Tables

Figure 1: Liquidity provisions received by Portuguese banks from the central bank



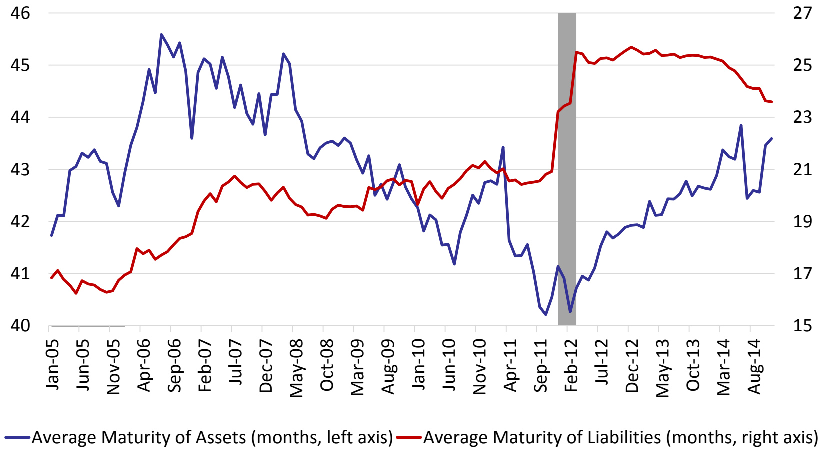
Notes: In billion EUR. Split by maturity: *vLTRO* includes the analyzed 3Y LTRO (2011-2014) and the subsequent targeted-LTRO (from 2014 onwards), *other* denotes all other liquidity operations with a maturity below three years (1 week{1 year).

Figure 2: Mechanism: maturity mismatch



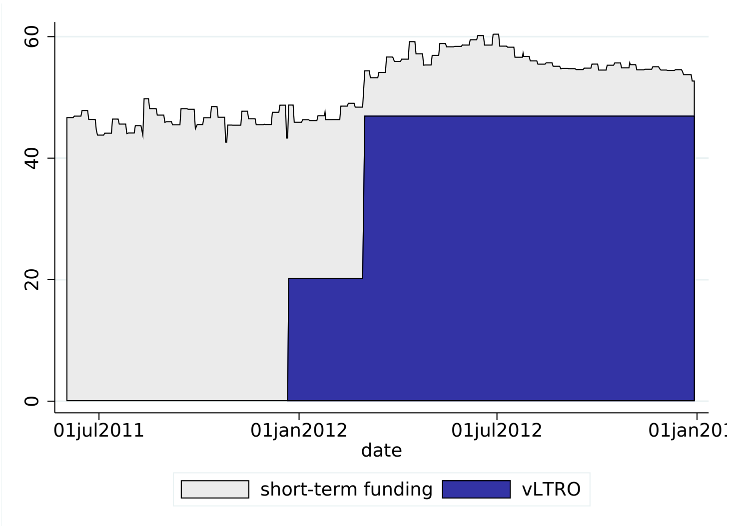
Notes: In months. Averages for Portuguese banking sector. Grey-shaded area represents the period between the vLTRO announcement and nal (second) allotment.

Figure 3: Mechanism: maturity of assets and liabilities



Notes: In months. Averages for Portuguese banking sector. Grey-shaded area represents the period between the vLTRO announcement and nal (second) allotment.

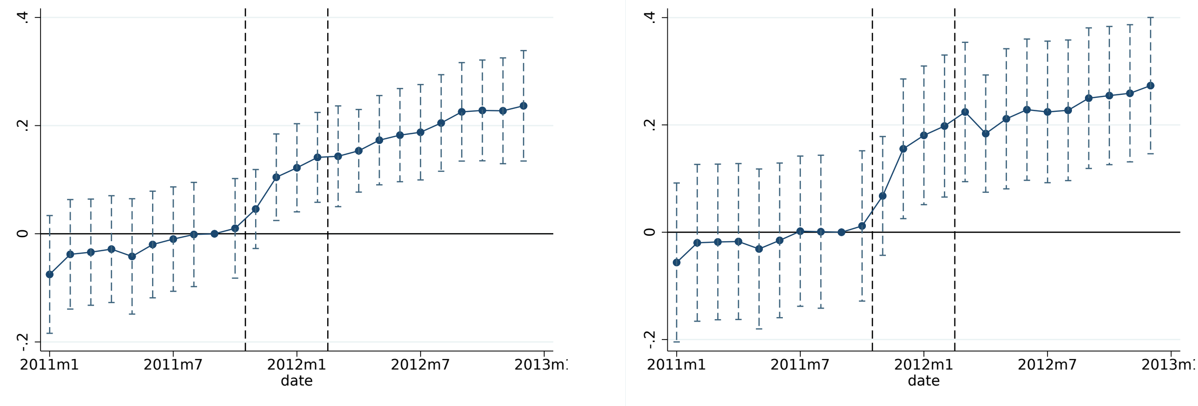
Figure 4: Dependence on the ECB is sticky prior to vLTRO announcement



Notes: Liquidity provisions received by Portuguese banks during the period Jun 2011{Dec 2012. Reported in billion EUR. Short-term funding includes all liquidity provision with a maturity of less that one year.

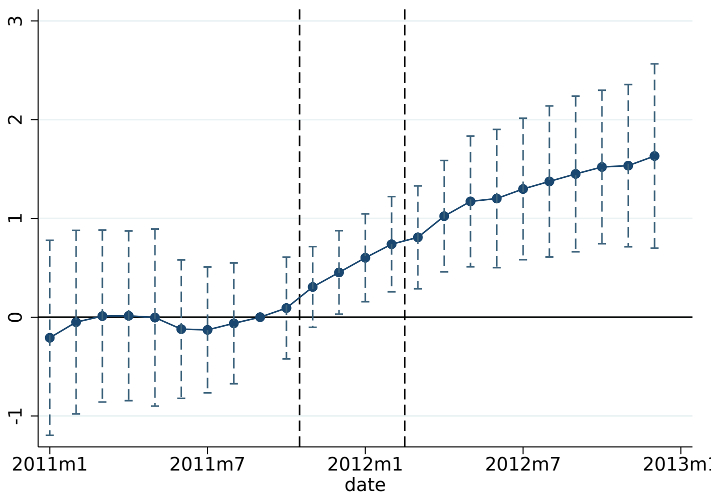
Figure 5: Leads and lags around the treatment effect: binary treatment

(a) ECB repo funding (b) Eligible securities



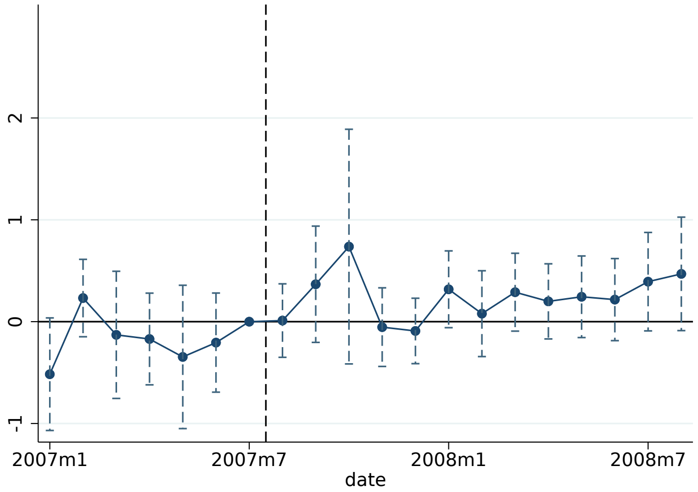
Notes: This gure presents coe cient estimates of $_k$ for each month *t* from Equation 4. We divide banks into two groups: exposed and non-exposed. Panel (a) sets X_i^{RR} to 1 if a bank *i* was borrowing from the central bank in repo operations prior to the vLTRO announcement and 0 otherwise. Panel (b) sets the X_i^{RR} to 1 if a bank *i* was holding securities eligible for the vLTRO prior to the policy announcement and 0 otherwise. Vertical bands represent +/- 1.96 times standard error of each point estimate. Vertical dotted lines separate the vLTRO period. Standard errors are two-way clustered at the bank-time and rm level.

Figure 6: Leads and lags around the treatment effect: continuous treatment



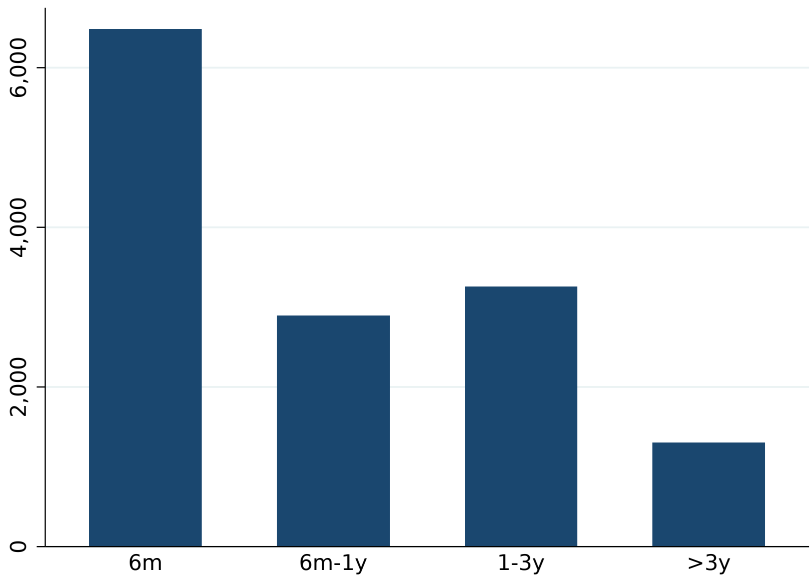
Notes: This gure presents coe cient estimates of $_k$ for each month *t* from Equation 4. Exposure measure X_i^{RR} is de ned as a sum of all secured borrowing from the central bank prior to the vLTRO announcement (as a share of total assets). Vertical bands represent +/- 1.96 times standard error of each point estimate. Vertical dotted lines separate the vLTRO period. Standard errors are two-way clustered at the bank-time and rm level.

Figure 7: Placebo test: freeze of the European interbank market in August 2007



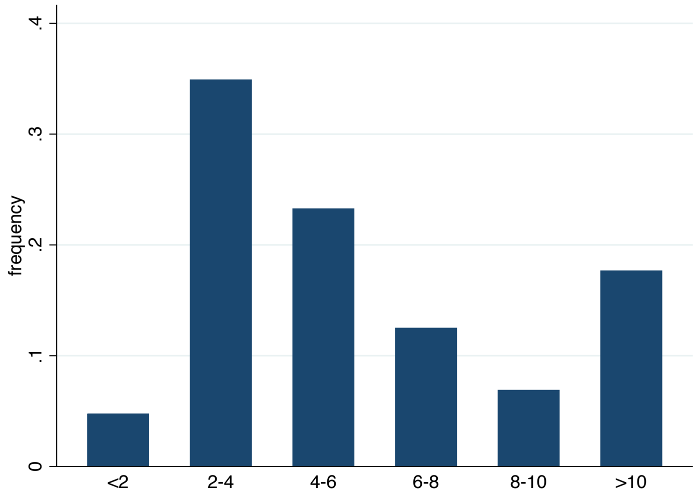
Notes: The gure uses an unexpected freeze of the European interbank market in August 2007 as a placebo test. The negative e ects of the interbank liquidity crunch on lending in Portugal were previously shown by Iyer *et al.* (2014). This gure presents coe cient estimates of $_k$ for each month *t* from Equation 4. There is no evidence that banks more exposed to the vLTRO in 2011 were generally more sensitive to the liquidity dry-up in 2007 (the plotted estimates of $_k$ are not signi cantly di erent from zero). Vertical bands represent +/- 1.96 times standard error of each point estimate. Vertical dotted line shows the European interbank market in August 2007. Standard errors are two-way clustered at the bank-time and rm level.

Figure 8: Remaining maturity of purchased & pledged securities



Notes: As of February 2012. In million EUR.

Figure 9: Distribution of yield to maturity of purchased & pledged securities (in percent)



Source: Bloomberg.

Table 1: Exposure to the reduction of rollover risk and the actual vLTRO uptake

	$vLTRO_i$	
	(1)	(2)
X_i^{RR}	0.780***	
	(0.080)	
$X_{i;eligSec}^{RR}$		0.415***
		(0.046)
Observations	30	30
R^2	0.78	0.75

Notes: This table presents coe cients from a univariate regression (Equation 2) that regresses the actual value of vTLRO liquidity against our treatment variable: X_i^{RR} denotes the total secured borrowing before the policy announcement and $X_{i,eligSec}^{RR}$ denotes the total holdings of eligible securities. All variables are normalized to total assets in September 2011. Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.

Table 2: Exposure to the reduction of rollover risk and other bank characteristics

	Est	S.E.	R^2
log(TA)	0.001	0.006	0.00
Capital ratio	0.081	0.080	0.04
Loans (% Assets)	-0.083	0.050	0.09
Loans (% Deposits)	-0.021	0.019	0.04
Deposits (% Assets)	-0.030	0.084	0.00
ROA	-0.506	0.498	0.04
Cash reserves (% Assets)	0.113	1.500	0.00
Liquidity ratio	-0.067	0.059	0.04
Leverage ratio	-0.001	0.001	0.01
Equity (% Assets)	-0.041	0.084	0.01
Average REPO funding ('05) (% Assets)	0.753***	0.150	0.50

Notes: This table presents coe cients from set of univariate regressions that regress our treatment variable X_i^{RR} against a set of bank observable characteristics (see Equation 3). $log(TA)$ is log of total assets of a bank i, *CapitalRatio* is a capital adequacy ratio, *Loans (% Assets)* is a share of loans to private sector normalized by total assets, *Loans (% Deposits)* is a share of loans to private sector normalized by total deposits, *Deposits (% Assets)* is a share of deposits normalized by total assets, *ROA* is bank's return on assets, *Cash reserves (% Assets)* is a share of cash reserves normalized by total assets, *Equity (% Assets)* is a share of equity normalized by total assets, *Average REPO funding ('05) (% Assets)* is a bank funding coming from repo market (average in 2005) normalized by total assets. Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Intensive margin: time-series version

	$log(credit\_drawn_{i;j;t})$				
	(1)	(2)	(3)	(4)	(5)
$X_i^{RR} \quad Post_t$	0.702***	0.740***	0.623***	0.607***	0.631***
	(0.088)	(0.071)	(0.076)	(0.090)	(0.063)
Time FE	Yes	Yes			
Bank FE	Yes	Yes	Yes	Yes	
Firm-Time FE			Yes	Yes	Yes
Bank-Firm controls				Yes	
Bank-Firm FE					Yes
Observations	2,914,218	1,488,436	1,488,436	1,488,436	1,487,089
R^2	0.0674	0.123	0.568	0.862	0.993
Loan Sample	Full	Multiple bank relationships	Multiple bank relationships	Multiple bank relationships	Multiple bank relationships

Notes: This table presents coe cients from regressions related to loan-level intensive margin, as described in Equation 1. The dependent variable is log credit granted to private non- nancial corporations in Portugal. Standard errors are two-way clustered at the bank-time and rm level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Intensive margin: collapsed version

	$\Delta log(credit\_drawn_{i;j})$			
	(1)	(2)	(3)	(4)
X_i^{RR}	1.464**	1.502**	1.485**	1.428***
	(0.590)	(0.577)	(0.546)	(0.418)
Bank Controls	Yes	Yes	Yes	Yes
Bank-Firm Controls	Yes	Yes	Yes	Yes
Firm Controls		Yes	Yes	
Firm FE				Yes
Observations	231,350	231,350	135,751	135,751
R^2	0.021	0.034	0.047	0.442
Sample	Full	Full	Multiple bank relationships	Multiple bank relationships

Notes: This table presents coe cients from regressions related to loan-level intensive margin described as $log(credit\_drawn_{ij}) = \; _j + X_i^{RR} + B_i + Q_{i,j} + \; _{i,j}$. The dependent variable $log(credit\_drawn)_{ij}$ is a change in the log of average credit drawn in the period before (2011m6{2011m10) and after (2012m2{2012m6) the vLTRO. Bank controls are *log*(*TotalAssets*), *CapitalRatio*, *LiqRatio*, *Equity=TA* and *Loans=TA*. Bank-Firm controls are variables *Length*, *Share* and *Secured*. Firm controls are log of rm total assets and 2-digit NACE industry district xed e ects. Standard errors clustered at the bank level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Intensive margin: robustness

	Baseline	Alternative exposure	Total credit	All firms	Exposed banks	EBA shock
	(1)	(2)	(3)	(4)	(5)	(6)
X_i^{RR} $Post_t$	0.631***		0.613***	0.639***	0.532***	2.043***
	(0.063)		(0.059)	(0.053)	(0.078)	(0.215)
$X_{i;eligSec}^{RR}$ $Post_t$		0.089**				
		(0.045)				
Firm-Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,487,089	1,487,089	1,673,619	2,387,616	1,243,816	765,574
R^2	0.993	0.993	0.995	0.994	0.993	0.951
Firm sample	Private NFC	Private NFC	Private NFC	All firms	Private NFC	Private NFC
Bank sample	All banks	All banks	All banks	All banks	Only exposed	Excl. EBA banks
Credit	Drawn	Drawn	Total	Drawn	Drawn	Drawn

Notes: This table presents coe cients from regressions related to loan-level intensive margin, as described in Equation 1. Column (1) represents the baseline speci cation as shown in the last column of Table 3. The dependent variable is log credit granted to private non- nancial corporations in Portugal. The exposure X_i^{RR} is de ned as a sum of all available borrowing from the ECB as a share of total assets prior to the vLTRO. Columns (2){(6) present variations to the baseline. Column (2) uses an alternative exposure measure X_i^{sec} de ned as a sum of all securities eligible for the vLTRO and held by the banks prior to the vLTRO (as a share of total assets). Column (3) shows results for the dependent variable measured as a log of total credit (a sum of credit granted and potential credit). Column (4) extends the sample to all rms in Portugal, including not only private NFC but also independent entrepreneurs and publicly-owned companies. Column (5) focuses only on the variation across banks that were exposed to the ECB open market operations prior to the vLTRO (in September 2011). Column (6) drops four largest banks which were subject to the stress test exercise conducted by the European Banking Association (EBA) around the same time as the vLTRO. Standard errors are two-way clustered at the bank-time and rm level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Extensive margin: exit

	$EXIT_{i;j}$	
	(1)	(2)
X_i^{RR}	-0.840***	-0.782***
	(0.168)	(0.137)
Bank controls	Yes	Yes
Firm controls	Yes	
Firm FE		Yes
Observations	284,179	274,605
R^2	0.242	0.526

Notes: This table presents coe cients from regressions related to loan-level extensive margin, as described in Equation 5. The sample includes all loans that were outstanding in the period prior to the vLTRO (2011m6{ 2011m10). For a given loan, $EXIT$ is classi ed as 1 if the loan is not renewed and the bank- rm relationship ceases to exist. Bank controls include $ln(TA)$, $CapitalRatio$, $LiqRatio$, $Equity=TA$ and $Loans=TA$. Firm controls include log of total assets of a rm j and industry-district xed e ects. Standard errors clustered at the bank level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Extensive margin: entry using loan consultation data

	$newLoan_{i;j}$	
	(1)	(2)
X_i^{RR}	0.630*	0.633***
	(0.312)	(0.211)
Bank controls	Yes	Yes
Firm controls	Yes	
Firm FE		Yes
Observations	92,115	40,938
R^2	0.055	0.459

Notes: This table presents coe cients from regressions related to loan-level extensive margin, as described in Equation 6. The dependent variable $newLoan$ takes a value of 1 if a bank- rm consultation entry is matched with a new bank- rm record in the credit registry and 0 otherwise. Bank controls include $ln(TA)$, $CapitalRatio$, $LiqRatio$, $Equity=TA$ and $Loans=TA$. Firm controls include log of total assets of a rm j and industry-district xed e ects. Standard errors clustered at the bank level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Maturity of loans

	$\Delta Maturity_{i;j}$		
	(1)	(2)	(3)
X_i^{RR}	0.792***	0.850***	0.618***
	(0.181)	(0.218)	(0.173)
Bank controls	Yes	Yes	Yes
Bank-Firm controls		Yes	Yes
Firm Controls			Yes
Observations	10,863	10,863	7,539
R^2	0.007	0.011	0.099

Notes: This table presents coe cients from regressions related to loan-level regression, as described in Equation 7. The dependent variable $\Delta Maturity$ is a change in the median original maturity between the *pre-* (June{ October 2011) and *post-* (March{June 2012) treatment period. Bank controls include $ln(TA)$, $CapitalRatio$, $LiqRatio$, $Equity/TA$ and $Loans/TA$. Bank- rm controls are *Length*, *Share* and *Secured*. Firm controls are log of total assets of a rm j and industry-region xed e ects. Standard errors clustered at the bank level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 9: Heterogeneous outcomes by firm type

	$log(credit\_drawn_{i;j;t})$			
	(1)	(2)	(3)	(4)
X_i^{RR} $Post_t$	1.722***	0.814***	0.628***	1.050***
	(0.182)	(0.0704)	(0.0326)	(0.0341)
X_i^{RR} $Post_t$ $Size_j$	-0.077***			
	(0.013)			
$X_{i;}^{RR}$ $Post_t$ Age_j		-0.052*		
		(0.027)		
X_i^{RR} $Post_t$ $Risky_j$			0.948**	
			(0.466)	
X_i^{RR} $Post_t$ $Length_{ij}$				-0.006***
				(0.000)
Firm-Time FE	Yes	Yes	Yes	Yes
Bank-Firm FE	Yes	Yes	Yes	Yes
Observations	1,166,299	1,166,299	1,166,299	1,166,299
R^2	0.96	0.96	0.96	0.96

Notes: This table presents coe cients from regressions related to loan-level intensive margin, as described in Equation 8. The dependent variable $log(credit\_Drawn)$ is log credit drawn of rms in Portugal. $Size_j$ is log of total assets of rm j, *Age* is rm's age in years, *Risky* is a measure of riskiness based on default probabilities for Portuguese rms (Antunes *et al.* 2016), *Length* is a length of bank-borrowing relationship in months. Standard errors are two-way clustered at the bank-time and rm level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 10: Firm-level outcomes

	$\Delta log(credit\_drawn_j)$		$\Delta log(investment_j)$		$\Delta employment_j$	
	(1)	(2)	(3)	(4)	(5)	(6)
$\overline{X_j^{RR}}$	0.700***	2.257***	0.595**	6.030***	-0.016	55.538**
	(0.058)	(0.366)	(0.287)	(1.805)	(6.134)	(1.805)
$\overline{X_j^{RR}}$ $Size_j$		-0.118***		-0.411***		-4.182**
		(0.027)		(0.135)		(0.135)
Bias corrected $\overline{X_j^{RR}}$	0.378					
$\overline{\text{Bank controls}}$	Yes	Yes	Yes	Yes	Yes	Yes
$\overline{\text{Bank-Firm controls}}$	Yes	Yes	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	51,978	51,978	27,029	27,029	37,368	37,368
R^2	0.038	0.038	0.046	0.047	0.001	0.001

This table presents coe cients from regressions related to rm-level intensive margin, as described in Equation 9. The dependent variable $log(credit\_drawn_j)$ a log change in total bank lending on the rm level between the *pre-* (June{October 2011) and *post-* (March{June 2012) treatment period or $log(investment_j)$ as a annual log change in investment (2012 vs. 2011) or $employment_j$ as a annual change in employment (2012 vs. 2011). $\overline{\text{Bank controls}}$ are indirect measures of $ln(TA)$, $CapitalRatio$, $LiqRatio$, $Equity{=}TA$ and $Loans{=}TA$. $\overline{\text{Bank-Firm controls}}$ are indirect measures of *Share*, *Length* and *Secured*. Firm controls are log of total assets. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 11: Motives behind purchasing eligible securities

$\theta BankChar_i$	Estimate	Std. error	R^2
1. Capital motive			
Capital ratio	0.024	(0.024)	0.033
2. Liquidity motive			
Liquidity ratio	-0.014	(0.018)	0.022
Liquidity ratio (noSec)	-0.017	(0.018)	0.032
Cash/TA	0.057	(0.454)	0.001
3. Business model			
Securities/TA	0.033**	(0.016)	0.139
Δ govSecurities2009/TA	0.378***	(0.095)	0.362

Notes: This table presents coe cients from bank-level regressions as described in Equation 10. *Capital ratio* is a capital adequacy ratio, *Liquidity ratio* is a sum of cash, short term securities and short term MFI borrowing normalized by total assets, *Liquidity ratio (noSec)* is a sum of cash and short term MFI borrowing normalized by total assets. $Cash{=}TA$ is a ratio of cash to total assets, $Securities{=}TA$ is a ratio of average security holdings one year prior to vLTRO to total assets, $govSecurities2009{=}TA$ is a sum of purchases of the government debt securities in 2009. Number of observations is 30. ***p<0.01, **p<0.05, *p<0.1.

Table 12: Strategic security purchases: deeper into balance sheet

	Total Assets (1)	Central bank funding (2)	Non-core liabilities (3)
$PP_i \quad Post_t$	0.366	0.248**	-0.587***
	(0.474)	(0.124)	(0.174)
Observations	360	360	360
R^2	0.017	0.043	0.021
Bank sample	All banks	All banks	All banks

Notes: This table presents coe cients from regressions as described in Equation 11, where the outcome variable $BSitem_{i,t}$ refers to the total assets, central bank funding and non-core liabilities, respectively. Standard errors in parentheses. ***p<0.01 **p<0.05, *p<0.1

Table 13: Intensive margin: policy side effects and substitution

	$log(credit\_drawn_{i;j;t})$	
	(1)	(2)
$X_i^{RR} \quad Post_t$	0.725***	0.741***
	(0.140)	(0.144)
$PP(vah)_i \quad Post_t$	-1.524***	
	(0.566)	
$PP(bv)_i \quad Post_t$		-1.191***
		(0.458)
Bank-Firm FE	Yes	Yes
Firm-Time FE	Yes	Yes
Observations	1,695,671	1,695,671
R^2	0.97	0.97
Bank sample	Exposed banks	Exposed banks

Notes: This table presents coe cients from regressions related to loan-level intensive margin, as described in Equation 12. The dependent variable $log(credit\_drawn_{i,j,t})$ is a log credit drawn of non- nancial rms in Portugal. Standard errors are two-way clustered at the bank-time and rm level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

A Appendix

Table 14: Bank characteristics

		Mean	S.D.	p25	p50	p75
		September 2011				
Total assets	bn EUR	16.9	32.7	0.6	1.9	11.1
Capital ratio	Capital/RWA	11.2	16.3	8.8	10.3	14.4
ROA	Profit/Assets	0.4	2.6	-0.2	0.0	0.4
Cash reserves	% Assets	0.7	0.9	0.0	0.5	1.1
Security holdings	% Assets	19.4	17.2	5.6	19.6	29.7
Eligible securities	% Assets	12.9	15.6	1.1	11.1	20.1
Loans	% Assets	66.3	20.5	25.5	43.0	69.2
Equity	% Assets	13.8	15.8	6.4	8.4	13.2
All LTROs	% Assets	4.6	7.0	0.0	0.0	7.2
Total ECB liquidity	% Assets	5.9	8.4	0.0	1.1	9.2
		December 2011				
All LTROs	% Assets	5.3	7.5	0.0	1.1	8.8
vLTRO	% Assets	3.1	4.6	0.0	0.0	4.9
Total ECB liquidity	% Assets	5.8	7.8	0.0	2.1	9.7
		March 2012				
All LTROs	% Assets	7.1	9.0	0.0	4.7	10.8
vLTRO	% Assets	6.2	7.5	0.0	4.7	10.5
Total ECB liquidity	% Assets	7.5	9.9	0.0	4.7	11.2

Table 15: Loan characteristics

	N	Mean	S.D.	p25	p50	p75
	Main sample: private NFCs					
Credit drawn	4,150,391	284,501	2,968,372	6,417	23,283	90,635
Total credit	4,712,155	323,571	3,630,589	5,465	22,475	91,693
	All firms					
Credit drawn	4,910,118	252,791	2,967,164	6,302	24,890	89,344
Total credit	5,685,298	263,194	3,356,880	4,109	20,000	80,997

Notes: Reported in EUR. Minimum reporting threshold is EUR 50. Credit drawn represents regular, renegotiated and less than 60 days overdue credit, Total credit represents credit drawn and potential credit (unused credit lines, credit cards etc.) Main sample consists of domestic and foreign non-financial firms and non-profits. Full sample additionally includes public firms and individual entrepreneurs.

Table 16: Intensive margin: robustness to standard error clustering

	$log(credit\_drawn_{i;j;t})$					
S.E. Clustering	Bank-Time and Firm		Bank and Firm		Bank	
	Baseline					
	(1)	(2)	(3)	(4)	(5)	(6)
$X_i^{RR} \quad Post_t$	0.607***	0.631***	0.607***	0.631***	0.607***	0.631***
	(0.090)	(0.063)	(0.167)	(0.168)	(0.024)	(0.214)
Bank FE	Yes		Yes		Yes	
Bank-Firm Controls	Yes		Yes		Yes	
Bank-Firm FE		Yes		Yes		Yes
Firm-Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,488,436	1,487,089	1,488,436	1,487,089	1,488,436	1,487,089
R^2	0.862	0.993	0.862	0.993	0.862	0.993

Notes: This table presents coe cients from regressions relating to loan-level intensive margin, as described in Equation 1. The dependent variable is log credit granted to private non- nancial corporations in Portugal. Clustered standard errors in parentheses. Level of standard error clustering is bank-time and rm, bank and rm and rm, respectively. *** p<0.01, ** p<0.05, * p<0.1.

Figure 10: Government debt holding by Portuguese banks