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## The effect of TLTRO-II on bank lending

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# The Effect of TLTRO-II on Bank Lending\*

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April, 2019

## Abstract

This study applies a difference-in-differences approach to estimate the effect of the European Central Bank's second series of targeted longer-term refinancing operations (TLTRO-II) on bank lending. Effects on corporate loans, loans for house purchase and loans for consumption are analysed separately. The results indicate that TLTRO-II increased lending to non-financial corporations. The cumulative effect of TLTRO-II on participating banks' corporate lending is estimated to be about 30 per cent. The estimated effects for house purchase and consumption loans are positive, but statistically insignificant.

**Keywords:** TLTRO, unconventional monetary policy, credit supply, euro area

**JEL codes:** E44, E51, E52, G21

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# 1. Introduction

In recent years, the European Central Bank (ECB) has tried to ease private sector credit conditions and stimulate credit creation by providing banks with cheap long-term credit against adequate collateral. These credit operations were geared to increasing bank lending to the non-financial private sector in order to stimulate activity in the real economy and accelerate euro area inflation.

The ECB launched its first targeted longer-term refinancing operations (TLTRO-I) in September 2014. The operations were targeted on lending to the euro area non-financial private sector, excluding loans to households for house purchase. The operations were designed so that commercial bank borrowing from the ECB was commensurate with the lending of commercial banks to the non-financial private sector. On 10 March 2016, the ECB announced its second round of targeted longer-term refinancing operations (TLTRO-II). While the objectives of TLTRO-II were similar to those of TLTRO-I, the incentives for commercial banks were modified. Now, the maximum amount that banks could borrow was fixed. Instead, if a participating bank increased its eligible net lending sufficiently, it was entitled to a lower interest rate on its TLTRO-II borrowing. The reduction could go as low as the rate on the deposit facility (which had been lowered to -0.40 per cent in March 2016).

In the initial June 2016 TLTRO-II operation, banks borrowed 399 billion euros. Most of the money went to repaying TLTRO-I obligations. The total stock of TLTROs increased by 38 billion euros. In total, TLTRO-II borrowing amounted to 739 billion euros.

Despite the scale of the TLTROs, little is known about their effectiveness.<sup>1</sup> The lack of research is understandable in that longer-term refinancing is a novel policy tool without a track record. Moreover, empirical analysis of the effects of the operations presents non-trivial challenges. For example, applying vector autoregressive (VAR) models is very tricky as the TLTROs were a brand new policy tool. How can one identify a shock to a variable that did not exist before the shock?

The following discussion explores whether TLTRO-II accomplished its intended purpose. The impacts of TLTRO-II on bank lending are estimated using a difference-in-differences approach. The dataset consists of confidential bank-level monthly data on bank balance sheets combined with confidential information on the total take-ups of banks in TLTRO-II.

This article contributes to the literature, at least, in three ways. First, earlier studies concerning the effects of TLTROs on bank lending behaviour only deal with the effects on corporate lending (see Van Dijk and Dubovik, 2018; Benetton and Fantino, 2018; Afonso and Sousa-Leite, 2019). However, TLTROs were targeted on loans to non-financial corporations and loans for consumption (these loan types

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<sup>1</sup> Balfoussia and Gibson (2016) analyse the effect of TLTRO-I on the real economy. Ambler and Rumler (2017) and Ampudia and Van den Heuvel (2018) focus on immediate financial market reactions. Van Dijk and Dubovik (2018) and Benetton and Fantino (2018) consider the impacts of TLTRO-I on lending rates. Afonso and Sousa-Leite (2019) assess, in addition to lending rates, also the effect of TLTRO-II on the stock of corporate loans.

constitute so called eligible lending). Loans for house purchases were excluded. Therefore, it is reasonable to expect that different types of loans are affected differently. Analysing the three loan categories separately, the results show TLTRO-II increased corporate lending. However, the effects on lending for households are insignificant. This finding is peculiar as the ECB did not favour corporate loans over consumption loans.

Second, the results indicate that the quantity of TLTRO-II credit was not linked to the increment of corporate lending. The banks that took up more central bank credit did not increase their lending more than banks borrowing less. This suggests that the impact was in the decision to participate rather than intensity of participation.

Third, TLTRO-II did not increase participating banks' sovereign bond purchases. Instead, the effect is found negative. Crosignani et al. (2017) find that the earlier very long-term refinancing operations (VLTROs) were largely used to buy government bonds, which was possibly unintended. Thus, the results suggest that the ECB's targeting strategy was effective.

More broadly, the paper relates to the literature about the transmission of central bank liquidity injections to the amount of credit (e.g. Darracq-Paries and De Santis, 2015; Boeckx, Dossche and Peersman, 2017; Boeckx, De Sola Perea and Peersman, 2017; Andrade et al., 2018; Carpinelli and Crosignani, 2018).

The remainder of the paper is as follows. Section 2 represents the main features of TLTRO-II. Section 3 reviews the earlier literature. Section 4 describes the data and the methodology applied in this paper. Section 5 shows the main results. Section 6 considers additional results regarding the amount of the TLTRO-II, as well as impacts on lending rates and sovereign bond purchases. Section 7 concludes.

## **2. TLTRO-II**

TLTRO-II was launched in June 2016 to ease private-sector credit conditions and stimulate credit creation. Four operations, one each quarter, would be conducted, with the final operation taking place in March 2017. TLTRO-II loans carry a maturity of four years, so e.g. the first operation matures in June 2020. The borrower banks are also able to repay voluntarily the amounts borrowed at a quarterly frequency starting two years from the settlement of each operation.

Banks could borrow a total amount of up to 30 per cent of a specific eligible part of their loans in January 2016, less any amount previously borrowed and still outstanding under the first two TLTRO-I operations in 2014. Eligible loans included loans to non-financial corporations and households (excluding loans to households for house purchases).

**Table 1.** Main features of the ECB's longer-term refinancing operations in recent years.

	VLTRO	TLTRO-I	TLTRO-II
Implementation	2 operations (12/2011 and 2/2012)	8 operations between 9/2014 and 6/2016.	4 operations between 6/2016 and 3/2017.
Interest rate	Average MRO rate	First operation: MRO rate + 10bp at time of allotment. Subsequent operations: MRO rate only.	MRO rate at time of allotment. Possibility for lowered rate if eligible net lending increased sufficiently.
Maturity	Both operations carried maturities of 3 years.	All operations mature in 9/2018.	Every operation has a maturity of 4 years.
Amount	Full allotment	9/2014 and 12/2014: Max. 7 % of eligible loans in 4/2014. 2015-2016: Max. 3 x eligible net lending relative to bank-specific benchmark.	Max. 30 % of eligible loans in 1/2016, less any amount previously borrowed and still outstanding under the first two TLTRO operations in 2014.

The interest rate of the operations was fixed to match that of main refinancing operations (MROs) prevailing at the time of allotment. Nonetheless, the participating banks were given an incentive to increase their eligible lending by promising a lower rate if the eligible lending was increased enough in the period between February 2016 and January 2018 in comparison to bank specific benchmark. The lowered rate could be as low as the rate on the deposit facility (-0.40 per cent).

The bank-specific benchmark depended on eligible net lending as follows. For the banks with positive eligible net lending in the 12-month period before January 2016, benchmark net lending was set at zero. For the banks with negative eligible net lending, benchmark net lending was the same as eligible net lending in the 12-month period before January 2016.

The incentives in the TLTRO-II to increase eligible lending differed from the incentives in the TLTRO-I. In the TLTRO-I, the banks were pushed to increase their lending by offering them more TLTRO-I credit when they increased their eligible lending. However, the banks were able to reduce their lending after they had borrowed their preferred amount of TLTRO-I credit. A key difference between TLTRO-I and TLTRO-II was also the maturity. TLTRO-I credit borrowed in September 2014 matured after four years, but the last operation of the TLTRO-I matured after about two years. The key differences between VLTRO and TLTRO operations are summarised in Table 1.

### 3. Earlier literature

The majority of the earlier literature related to the ECB's credit operations after 2011 concerns the two untargeted 3-year very long-term refinancing operations (VLTROs). There are two approaches used in analysing VLTRO effects. The *macroeconomic approach* applies VAR models, while the *microeconomic approach* relies on difference-in-differences methods.

Darracq-Paries and De Santis (2015) estimate the effect of the VLTRO using VAR models. They identify the VLTRO shock using information from the euro area Bank Lending Survey (BLS). Darracq-

Paries and De Santis (2015) find that VLTROs had a positive temporary effect on real GDP, and that the effect peaked in summer 2013. The operations also increased the general price level and stock of corporate loans. The effect on prices and lending was more sluggish, with the full effects not seen until 2014. Boeckx, Dossche and Peersman (2017) and Boeckx, De Sola Perea and Peersman (2017) note similar effects.

Regarding microeconomic studies, Andrade et al. (2018) examine the impact of VLTROs on corporate lending in France. By using linked bank-firm data covering 2011 and 2012, they are able to control efficiently for credit demand. Linked bank-firm level data permit consideration of firms borrowing from multiple banks. Some lenders had accessed the VLTRO credit facility while others had not. Therefore, the data make it possible to observe changes in lending to a given firm depending on the extent lenders have taken up VLTRO credit. Their results show that one billion euros VLTRO credit increased the loan supply by 186 million euros. Carpinelli and Crosignani (2018) also use bank-firm data to investigate VLTRO effects based on data are from Italy running from December 2010 to June 2012. These researchers find that the aggregate effect of VLTRO on lending in Italy was about 2 per cent.

The literature on the effects of TLTRO is still quite sparse. Balfoussia and Gibson (2016) analyse the effects on the real economy using principal component analysis and VAR models. Based on their results, the cumulative effect of TLTRO-I on industrial production was nearly 6 per cent. Van Dijk and Dubovik (2018) and Benetton and Fantino (2018) find that TLTRO-I lowered lending rates. Supporting evidence for the intended effects is also found in Altavilla et al. (2019). Afonso and Sousa-Leite (2019) assess the effect of TLTROs on both lending rates and the amount of credit. Their findings regarding the effectiveness of the operations are somewhat mixed as they find some evidence about the positive effect on credit granted using data from multiple countries. However, their difference-in-differences analysis, which utilises data from Portugal, shows the effect on credit granted was not statistically significant.

In addition to these studies, there are a couple that analyse how exchange-traded assets reacted immediately after ECB announcements (see Ampudia and Van den Heuvel, 2018; Ambler and Rumler, 2017). In these studies, the immediate effects of TLTROs are found to be somewhat stronger than those of the VLTRO.

## **4. Data and methodology**

I apply a difference-in-differences approach to study the effects from bank participation in TLTRO-II. The main data are taken from the ECB's individual balance sheet items (IBSI) database. The data are monthly and at bank level. While IBSI does not cover all euro area banks the sample is quite large and



**Table 2.** Descriptive statistics grouped by decision to participate in TLTRO-II. The statistics are calculated from bank-level January 2015 to May 2016 averages, i.e. before TLTRO-II. Thus, statistics represent how the banks that participated in the credit operations and the other banks differed before treatment.

Variable	TLTRO-II participant			TLTRO-II non-participant		
	Mean	Median	n	Mean	Median	n
Balance sheet	119 345	41 634	116	69 119	18 039	181
Central bank credit to total liabilities	4.8 %	2.4 %	106	0.8 %	0.0 %	150
Deposits to total liabilities	22.6 %	23.2 %	116	24.0 %	16.2 %	180
Equity ratio	9.9 %	8.3 %	116	9.9 %	7.8 %	180
Credit to total assets	41.5 %	45.4 %	116	40.2 %	26.3 %	180

includes, depending on a variable, about 300 large banks from numerous countries. The banks cover about 85 per cent of the total corporate loans. Unfortunately, there are quite a lot missing values and all the banks cannot be used. Some key descriptive statistics of the assessed banks, grouped by the decision to participate the TLTRO-II, are shown in Table 2. In most analyses, the set of interpreted banks is smaller (this will be explained below). The used data are from January 2015 to July 2018. The IBSI data are linked to confidential information about bank's total borrowing in the TLTRO-II. In addition, individual interest rate statistics (IMIR) and the euro area Bank Lending Survey (BLS) are utilised in several analyses. IMIR corresponds to the IBSI, but covers lending and deposit interest rates instead of balance sheet items. The BLS is conducted four times a year and provides information on bank lending conditions in the euro area.

IBSI data offer several advantages. First, they make it possible to analyse TLTRO-II in several countries. Additionally, as the data are monthly and cover a sufficiently long time period after the treatment, it is possible to analyse how possible effects evolve over time. The drawbacks are the difficulty of generalising the results (IBSI, as noted, does not cover all euro area banks) and controlling for credit demand of firms.<sup>2</sup> The extent to which the results may be generalised is discussed below. When it comes to controlling credit demand, the baseline approach in this study is to add country-time fixed effects to the model. These fixed effects consider if the stock of credit increased or decreased simultaneously in all the banks within a given country.

<sup>2</sup> Some papers have used linked bank-firm data covering all banks in a single country (see Andrade et al., 2018; Carpinelli and Crosignani, 2018; Benetton and Fantino, 2018). Bank-firm data make it possible to control for credit demand of individual firms. This improves the evidentiary basis for arguing that the observed effect comes from the supply side. Unfortunately, such data are not available at the euro-area level.

To be concrete, the baseline specification is:

$$\ln(Y_{it}) = \alpha_i + \tau_{ct} + x_{it}'\beta + z_{it}'\gamma + e_{it}, \quad (1)$$

where  $Y_{it}$  is the stock of credit on the balance sheet of bank  $i$  at time  $t$ ,  $\alpha_i$  includes bank fixed effects and  $\tau_{ct}$  country-time fixed effects. The vector  $z_{it}$  includes time-varying bank-specific control variables. The vector  $x_{it}$  includes interactions between a dummy that equals 1 if the bank participated in TLTRO-II and month-dummies:  $TLTROII_i * 2015Jan_t, \dots, TLTROII_i * 2018Jul_t$ . June 2016 is the reference month. This means that the regression coefficients in the vector  $\beta$  tell how the credit granted by TLTRO banks differed from other banks in a given month relative to the difference between the groups in June 2016. A similar approach is used by Rodnyansky and Darmouni (2017) to investigate the effects of quantitative easing on bank lending behaviour in the United States. This specification is useful because it is not realistic to assume that the effect was the same in every month after treatment as is assumed in standard difference-in-differences models. If the effect was the same every month after treatment, it would mean that the stock of credit in TLTRO participant banks jumped immediately after June 2016 and remained the same thereafter. Additionally, the estimates for the interactions before the beginning of the TLTRO-II should be zero. Otherwise, the assumption of common trends would not be credible. Adding these interactions in the regression allows testing the common trend assumption.

A central challenge in this study is justifying the assumption of common development of TLTRO banks and other banks if TLTRO-II had never been conducted. Banks were free to decide whether they wanted to borrow TLTRO-II credit or not, so banks that participated in TLTRO-II may have increased their lending anyway. To mitigate this selection bias, I use propensity score matching (PSM).<sup>3</sup> This procedure brings the analysis a step closer to a randomized trial artificially.

Another problem is that the treatment possibly affects both the treatment group and the control group. Therefore, the results possibly cannot be generalized to the aggregate level. It is possible that the TLTROs increased lending within participating banks, but decreased it within other banks. For example, participating banks could charge lower interest rates than non-participant banks, and therefore increase their market share. Therefore, it is possible that the difference-in-differences estimate is clearly positive, but the effect on the aggregate level of credit is zero. Such an effect would occur if TLTRO banks increased their lending only at the expense of other banks' lending.

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<sup>3</sup> Propensity score matching was first proposed by Rosenbaum and Rubin (1983). Since then, the method has been widely applied by researchers that do not have the luxury of experimental design. For example, Rodnyansky and Darmouni (2017) apply the method before their difference-in-differences analysis.

## 5. Results

### 5.1 Propensity score matching

To reduce selection bias and help justify the common trend assumption, I use PSM with replacement in the baseline analysis. The analysis is conducted using the nearest-neighbour algorithm. Here, the idea of PSM is to estimate the likelihood of banks to borrow TLTRO-II credit, based on their characteristics before the launch of TLTRO-II. This is done by estimating and fitting a logit model. Thereafter, the banks that borrowed in the TLTRO-II are matched with other banks based on their likelihood of being treated. The procedure creates a treatment group and control group with similar ex ante probabilities of being treated.

It is not self-evident, what characteristics should be considered in the matching procedure. The baseline analysis includes the following variables in the logit model: central bank credit to total liabilities, the consumption credit growth rate, the growth rate of house purchase loans, the growth rate of loans to non-financial corporations and country dummies. The variables are averages from January 2015 to May 2016. Central bank credit to total liabilities is included because banks that had borrowed from the ECB before TLTRO-II were also more likely to participate in TLTRO-II. The country dummies are included because banks in different countries faced a wide range of macroeconomic conditions at the beginning of TLTRO-II, and thus had very different probabilities of using the TLTRO-II facility. The growth rates of different types of credit are included to ensure common trends before the TLTRO-II. Table 3 reports the estimated logit-model.<sup>4</sup>

The second step of the PSM is visualised in Figure 1. The nearest-neighbour algorithm chooses a treated bank and finds a reference bank with a propensity score as close as possible to the treated bank. Once two banks are matched, the chosen control bank is “returned to the basket,” which means that a non-TLTRO bank can be matched more than one time. Another treated bank is chosen, and so forth. The procedure is repeated until every treated bank has a match. In Figure 1, the size of the circle within matched control units illustrates the number of times the bank has been matched.

Table 4 shows some central characteristics of the formed treatment and control group. Even after the PSM, there are major differences between groups. Banks in the treatment group are much bigger than the banks in the control group. Additionally, the treated banks had much more central bank credit than

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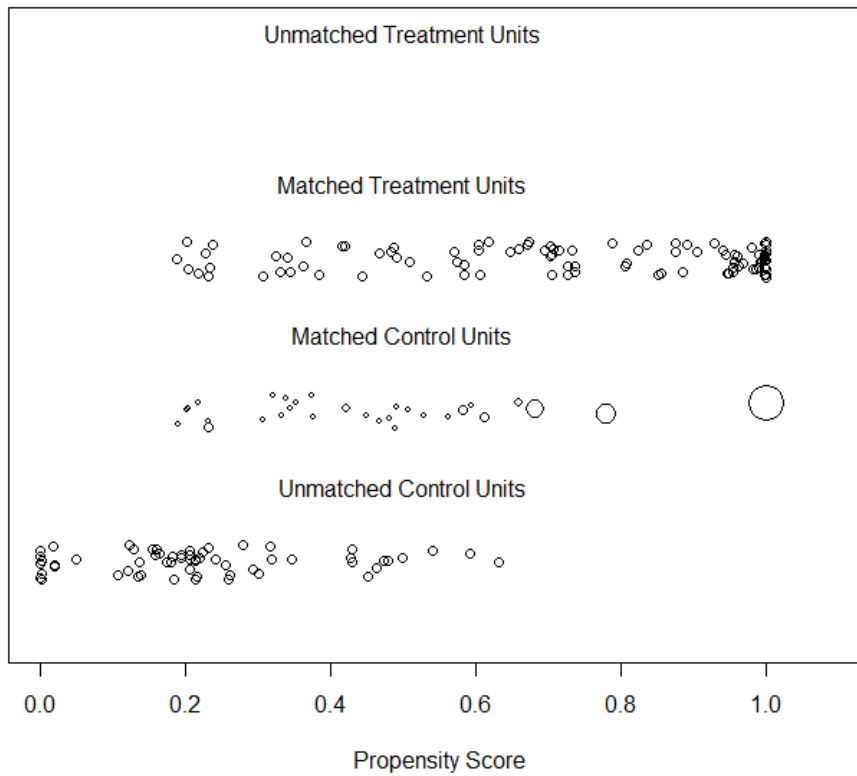
<sup>4</sup> French banks are excluded due to unavailability of information about their central bank credit. All the banks that have missing data are excluded. Also, banks that experience periods during which they have not had any corporate credit, loans for consumption or loans for house-purchase are excluded because these variables are analysed in logs. This sample selection limits generalisation of the results, but makes the analysed banks more alike.

banks in the control group before TLTRO-II – despite the fact that the variable was one of the selection criteria in the PSM. There is also a substantial difference in the average equity ratios between the groups.

**Table 3.** The logit model used in the PSM. The variables are averages prior to TLTRO-II (January 2015–May 2016).

<i>Predictor</i>	<b>Participation in TLTRO-II</b> <i>Estimate</i>
DLN (Loans to non-financial corporations)	3.10
DLN (loans for house purchase)	-46.07 *
DLN (loans for consumption)	5.78
Central bank credit to total liabilities	87.27 ***
AT	-0.96
BE	0.19
CY	-9.33
DE	-1.27 ***
EE	-0.60
ES	-0.81
FI	0.07
GR	-15.31
IE	-0.78
IT	-1.22
LT	-3.74
LU	-0.15
LV	-1.44
MT	-1.73
NL	0.99
PT	14.35
SI	-1.87
SK	0.39
Observations	187
Cox & Snell's R <sup>2</sup> / Nagelkerke's R <sup>2</sup>	0.389 / 0.518

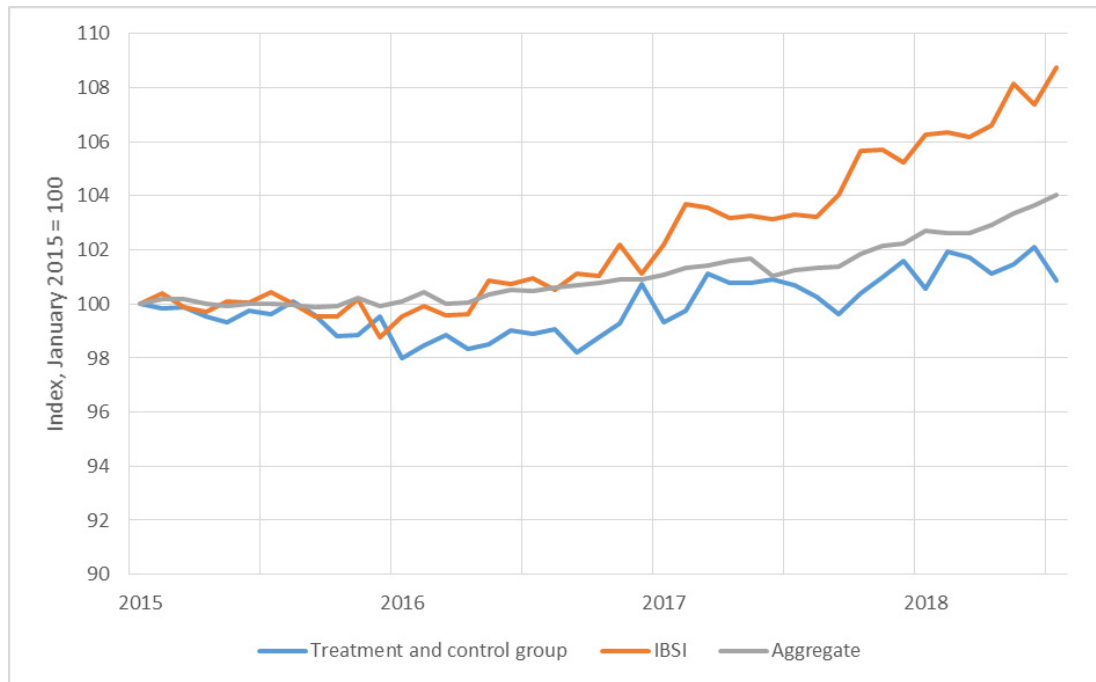
### Distribution of Propensity Scores



**Figure 1.** The estimated propensity scores from the logit model and the results of the matching procedure. Matching is done using the nearest-neighbour algorithm and with replacement. The size of the circle under the title “Matched Control Units” represents how many times a non-TLTRO bank has been matched with a TLTRO bank.

**Table 4.** Selected characteristics of banks in chosen treatment and control groups. The quantities are calculated from bank-specific averages prior to TLTRO-II (January 2015–May 2016). Credit in the credit-to-total-assets ratio means the sum of credit to non-financial corporations and credit to households. Deposits in the deposits-to-total-liabilities ratio refers to overnight deposits by households.

Variable	TLTRO-II participant (n=97)		TLTRO-II non-participant (n=30)	
	Mean	Median	Mean	Median
Balance sheet	106 988	40 042	43 149	25 960
Central bank credit to total liabilities	4.4 %	25 %	0,9 %	0.0 %
Deposits to total liabilities	25.5 %	24.3 %	29.2%	25.7 %
Equity ratio	10.4 %	9.1 %	7.6 %	6.4 %
Credit to total assets	46.1 %	46.7 %	42.9 %	45.6 %



**Figure 2.** The development of total outstanding corporate credit in the euro area, the development of average stock of corporate credit of all the IBSI banks and the development of average outstanding corporate credit of treatment and control group.

There is no single correct way to perform the PSM. The chosen set of banks vary to some extent depending on the variables included in the logit model. For example, the difference between the amounts of central banks credit would be much smaller if central bank credit to total liabilities was the only variable in the logit model. In addition, the PSM can also be conducted with calliper, which means that a treated bank can also be disregarded when there is no sufficiently close match. Doing the analysis without replacement also changes the matching as non-TLTRO banks can only be matched once. The sensitivity of the results to the matching procedure is tested later.

As the examined banks constitute a small fraction of banks in the euro area, sample representativeness needs to be addressed. Figure 2 shows the trends for total outstanding corporate credit in the euro area, average stock of corporate credit of all IBSI banks and average outstanding corporate credit of the remaining banks after the PSM. The trends differ a bit, making it harder to generalise about the results. When it comes to country distribution, the majority of the interpreted banks come from just three countries: Germany (26 per cent), Italy (11 per cent) and Spain (14 per cent).

## 5.2 Baseline results

Figure 3 shows the average development of loans to non-financial corporations, loans for house purchase and loans for consumption by groups. The groups are formed using PSM as shown in the previous section. The solid lines show the development of the TLTRO banks and dashed lines the developments of non-TLTRO banks. In all cases, the TLTRO banks increased lending compared to other banks after the beginning of TLTRO-II, which suggests that TLTRO-II increased lending overall, not just loans to non-financial corporations or consumption as targeted.

Of course, these observed developments may be caused by something other than TLTRO-II. For example, different macroeconomic conditions in different countries could explain the results. It could be that TLTRO participant banks are located in countries where bank lending has generally been increasing. In addition, as observed in the previous section, bank size and financial solidity differ considerably between the groups. To control for these possibilities, I estimate equation (1) for all the three types of credit. In vector  $z_{it}$ , I include the bank's equity ratio and natural logarithm of the bank's balance sheet.

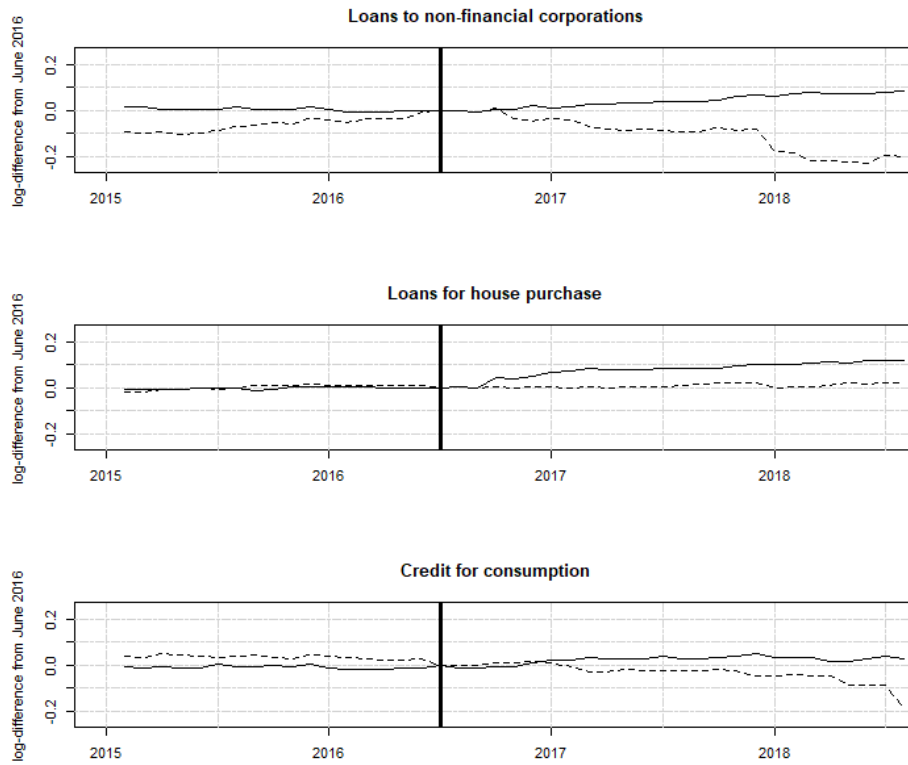
Figure 4 shows the estimated values of the vector  $\beta$ , i.e. the estimated effects of TLTRO-II in various months for different types of credit.<sup>5</sup> The solid lines represent the point estimates, and the dashed lines 95 per cent confidence intervals. In almost every month before June 2016, the estimated effects do not differ from zero, and thus supports the common trend assumption. After TLTRO-II gets underway, the point estimates for all credit types are positive on average. At the chosen confidence level, however, only the effects on loans to non-financial corporations are statistically significant.

These results are interesting as TLTRO-II was targeted at both corporate lending and lending to households (excluding housing mortgages). In light of these results, TLTRO-II clearly boosted the corporate lending of participating banks. The cumulative effect of TLTRO-II on participating banks' corporate lending is estimated to exceed 30 per cent. In contrast, the estimated effects on consumption lending seem modest and insignificant.

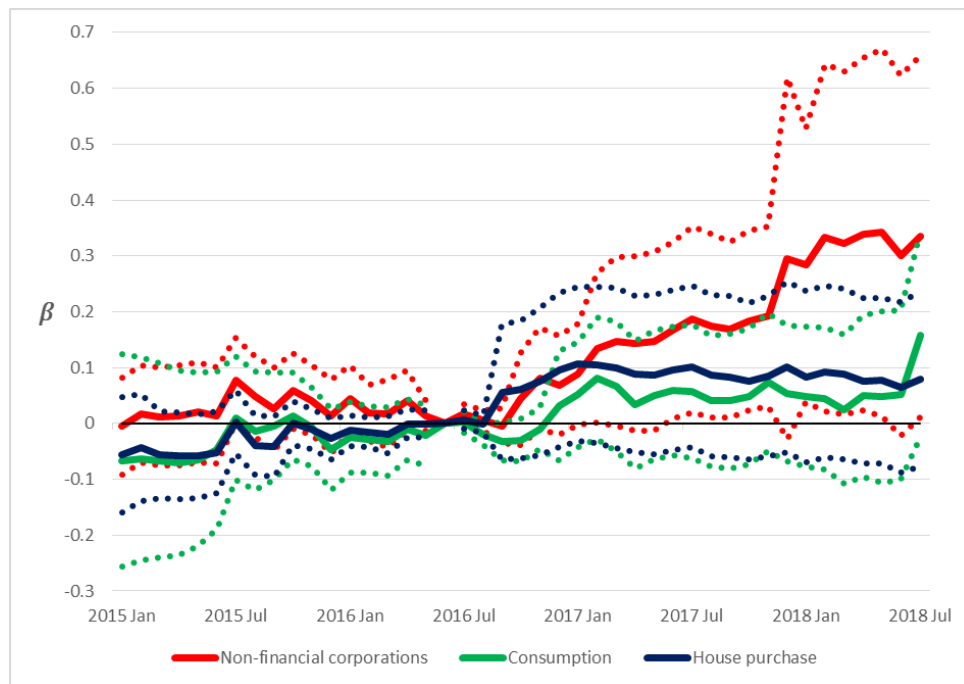
The magnitude of the effect on corporate lending seems intuitively excessive as the aggregate amount of corporate credit has grown quite slowly. If the aggregate effect was 30 per cent, it would mean that the aggregate credit stock would have contracted considerably without TLTRO-II. It is thus unlikely that these results can be interpreted as an aggregate effect. Instead, it is likely that TLTRO-II affected all banks, i.e. the increment of corporate credit in the participating banks decreased corporate lending of other banks and the difference-in-differences approach overestimates the aggregate effect. This crowding-out effect could explain the magnitude of the estimated effects.

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<sup>5</sup> The estimates are also presented in table form in Appendix A.



**Figure 3.** The development of different types of credit in the treatment (solid line) and control (dashed line) groups in comparison to the situation as of June 2016. The credit stocks are in logs. The treatment group includes 97 banks and the control group 30 banks.



**Figure 4.** The estimated effects (parameters in vector  $\beta$ ) on different types of credit. The dashed lines represent 95 per cent confidence intervals. Standard errors are clustered at the bank level.



### 5.3 Robustness

I perform several robustness checks to get a clearer picture of the effects of TLTRO-II. As mentioned earlier, it is possible that the results strongly depend on the way the PSM is conducted. I first test how the matching procedure affects the results, and then add several control variables and use an alternative method to control for credit demand.

Appendix B reports the main results after adding a calliper of 0.05 in the matching procedure. If the algorithm does not find a matching bank where the propensity score differs at most by 5 percentage points, then the bank is excluded from the treatment group. This approach homogenises the treatment and control groups as outlier banks are cut out both the treatment and control groups. On the other hand, this procedure could weaken the generalisation of the results. Adding the calliper makes the estimates for effects on corporate lending a bit smaller. It also changes the estimates for the effects on credit for household a bit. All in all, the qualitative results remain the same. The effect on corporate lending is strong and statistically significant at 5 per cent significance level. The effects on mortgages and consumer loans are modest and insignificant.

In Appendix C, matching with replacement is changed to matching without replacement. This means that one bank can be matched only once. The calliper of 0.05 is also included in this analysis. The calliper together with matching without replacement decreases the number of banks in the treatment group. In the previous analyses, there were a large number of TLTRO-banks with propensity scores close to 1. All were included in the final analysis because they all could be matched to the same one bank in the control group that also had a propensity score close to 1. If matching is done with calliper and without replacement, the estimates for effects on corporate lending are slightly smaller than in the baseline analysis. The estimated effect on loans for house purchase are a bit higher, but remain statistically insignificant.

So far, I have kept the logit model as in the baseline analysis. However, the results may depend on the specification of the logit model. To test robustness in this respect, I consider a simple model in which the sole explanatory variable is the share of central bank credit in total liabilities. The main results of this approach are reported in Appendix D. While the distributions of propensity scores change considerably in both groups, the qualitative results remain roughly similar. Again, the effect on corporate lending is clearly statistically significant, although the point estimates are a bit smaller. The effects on the two types of household credit are negative in some months and about zero on average.

Appendix E shows the results without the PSM. This approach is prone to selection bias. However, one might be interested in seeing these results for comparison. Without the PSM, the effect on corporate lending remains statistically significant, but the impacts on other types of loans are still insignificant.

All in all, it seems that the results are not very sensitive with respect to the matching procedure. One might argue, however, that there is a need to control for additional bank characteristics in the difference-

in-differences analysis. For example, the varying structures of bank liabilities or liquid assets could affect the results. Appendix F shows the results after controlling for equity ratio and size, as well as the additional control variables of share of overnight deposits by households, share of debt securities issued and share of cash. The matching procedure is as in the baseline. Adding the additional control variables hardly changes the estimates.

There are other options for controlling for credit demand besides including country-time interactions. As an alternative, I control the demand using information from the BLS. The BLS provides country-specific indices that show how banks have experience corporate demand for credit over the past three months. The indices are published quarterly, so the series are interpolated. As banks are asked to evaluate their credit demand during the last three months, the last quarterly observations are carried backward. In the regression analysis, the country-time fixed effects are replaced by these country-specific BLS indices. The equation to be estimated is then:

$$\ln(Y_{it}) = \alpha_i + BLS_{ct}'\delta + x_{it}'\beta + z_{it}'\gamma + e_{it}, \quad (2)$$

where  $BLS_{ct}$  is a vector of country-specific BLS indices. Everything else is as in the baseline. Appendix G shows the results from this approach. The estimates are close to the baseline estimates.

## 6. Additional results

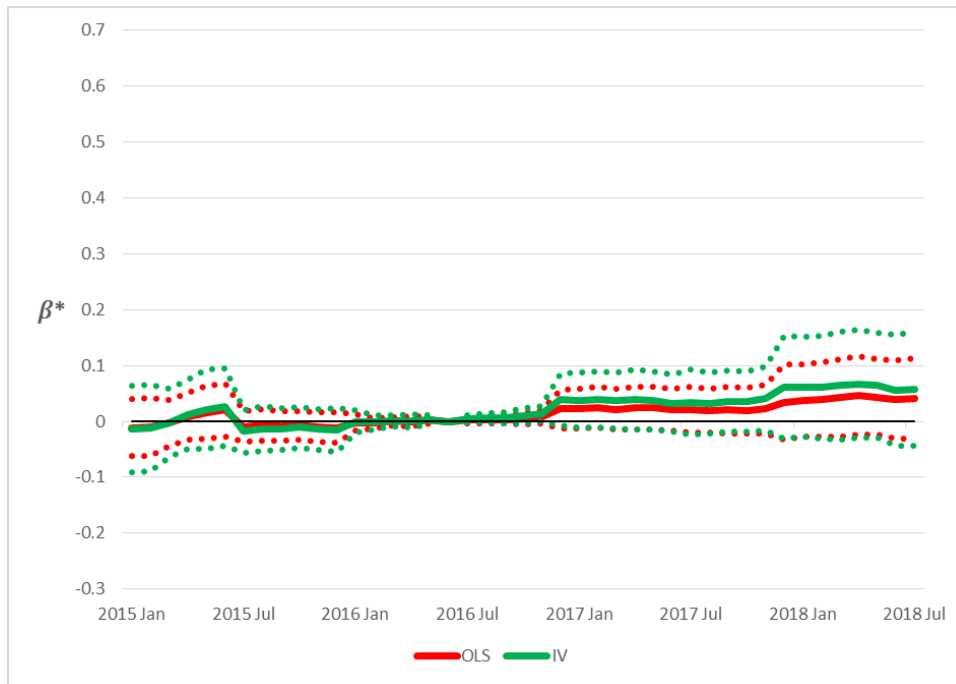
### 6.1 Amount of TLTRO-II

So far, I have only considered the effects of a decision to participate in TLTRO-II. However, one might expect that the more a bank borrowed from the central bank, the more it increased its lending to non-financial corporations. This kind of relationship is quite challenging to observe. The correlation between TLTRO-II borrowing and growth in lending to non-financial corporations is practically zero (-0.02).

To further assess this relationship, I drop all banks that did not participate in the TLTRO-II from the baseline analysis (entire control group) and add the natural logarithm of total TLTRO-II borrowing to the vector of interactions,  $\beta$ . In other words, I analyse only the banks that participated in the TLTRO (97 banks) and group them by their TLTRO-II amounts. The modified model is:

$$\ln(Y_{it}) = \alpha_i + \tau_{ct} + x_{it}'\beta^* + z_{it}'\gamma + e_{it}, \quad (3)$$

where  $\beta^*$  includes interactions:  $\ln(TLTROII\ amount)_i * 2015Jan_t, \dots, \ln(TLTROII\ amount)_i * 2018Jul_t$ . The estimates of  $\beta^*$  are shown in red in Figure 5. The estimates do not differ from zero



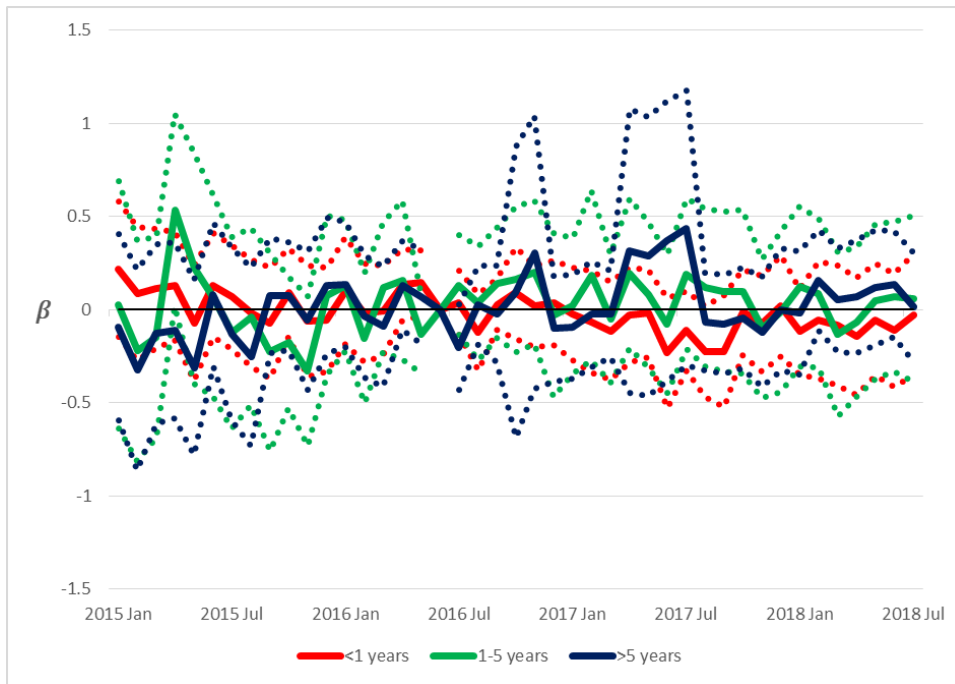
**Figure 5.** The effect of the natural logarithm of TLTRO-II borrowing on corporate lending. Only banks that participated in the TLTRO-II (and remain after the baseline PSM) are at issue here. The dashed lines represent 95 per cent confidence intervals. Standard errors are clustered at the bank level.

statistically significantly, thereby strengthening the conclusion that the amount borrowed under TLTRO-II was not as crucial as the decision to participate.

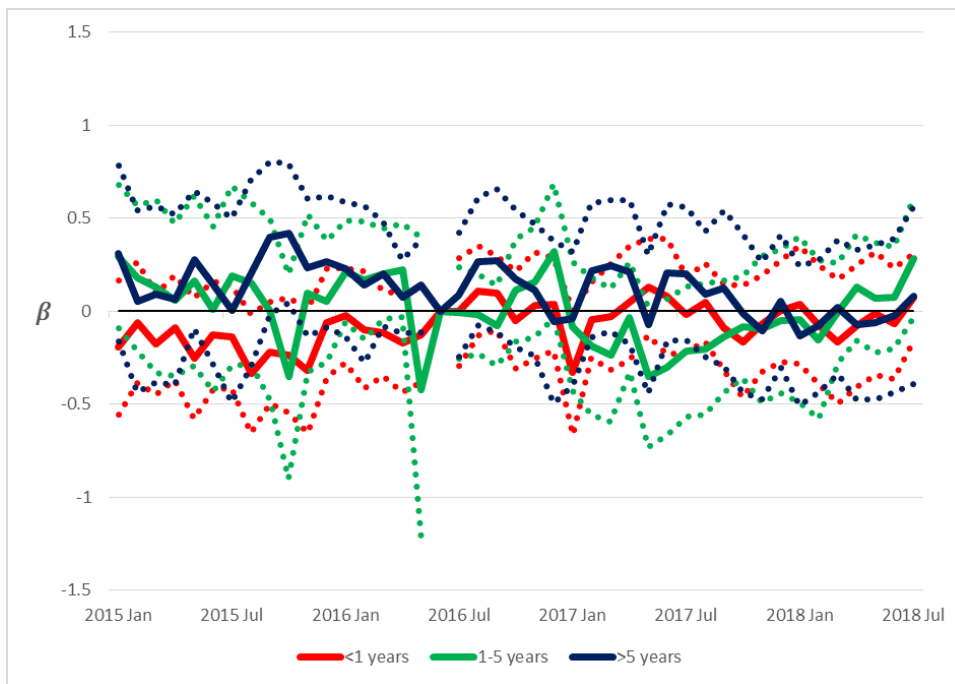
The problem with this analysis is that banks may choose their preferred amount to draw from the TLTRO-II facility. This problem was tackled in the baseline analysis using the PSM, but here the treatment is not a dichotomous variable. As the PSM is not applicable, I use an instrumental variable approach. As mentioned earlier, banks had a borrowing limit determined prior to TLTRO-II. I estimate the equation (3) instrumenting the interactions  $\beta^*$  by  $\ln(\text{Max TLTROII amount})_i * 2015Jan_t, \dots, \ln(\text{Max TLTROII amount})_i * 2018Jul_t$ . The IV-estimates of  $\beta^*$  are reported in Figure 5 in green. The estimates remain statistically insignificant.

## 6.2 Effect on lending rates

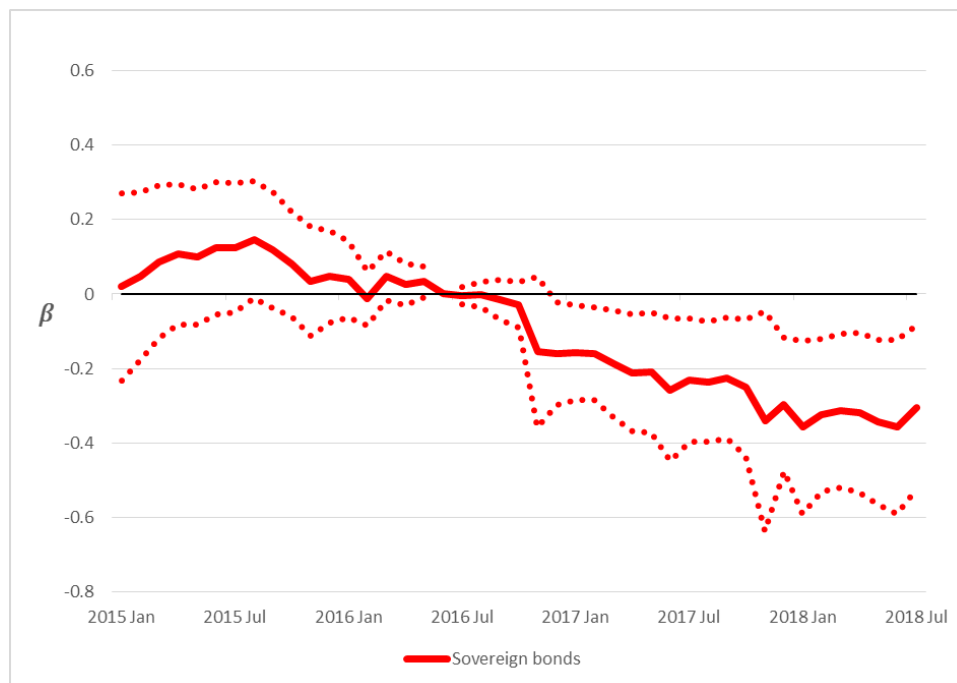
As participation in TLTRO-II had positive effect on lending of participating banks, one might expect a negative impact on lending rates. Figures 6 and 7 show the estimated effects on the interest rates of new corporate loans. The estimates are obtained using equation (1). The set of banks is as in the baseline analysis. Only thing difference is that the endogenous variable is now the interest rate applied to new corporate loans by bank  $i$  at time  $t$ . The interpreted interest rates are classified by loan maturity and size.



**Figure 6.** The effect of TLTRO-II on interest rates of new corporate loans of less than one million euros. The assessed banks are the same as in the baseline analysis. The dashed lines represent 95 per cent confidence intervals. Standard errors are clustered at the bank level.



**Figure 7.** The effect of TLTRO-II on interest rates of new corporate loans greater than one million euros. The assessed banks are the same as in the baseline analysis. The dashed lines represent 95 per cent confidence intervals. Standard errors are clustered at the bank level.



**Figure 8.** The effect of TLTRO-II on sovereign bond holdings. The assessed banks are the same as in the baseline analysis. The dashed lines represent 95 per cent confidence intervals. Standard errors are clustered at the bank level.

There are missing values as many banks have many months during which they have not issued any new loans. This problem is tackled by carrying the last observation forward.

As the figures show, there is no statistically significant effect on the lending rates. This surprising result may be explained by the fact that the observed banks operate more or less in the same markets. The interest rates they charge, therefore are subject to the law of one price and cannot diverge much from the market average. Thus, a difference-in-differences analysis in which both the treatment and control groups operate in the same market may be unsuitable to identify an effect on lending rates. TLTRO-II possibly decreased lending rates in both the treatment group and the control group, which makes it look like there was no effect at all.

### 6.3 Effect on sovereign bond purchases

Crosignani et al. (2019) find that the VLTROs mainly went to buying sovereign bonds. This was one reason the ECB chose to target its TLTROs. To investigate whether targeting worked as intended, I estimate the equation (1) as in the baseline analysis, but use the natural logarithm of the sovereign bond holdings as a dependent variable. The estimated effects are reported in Figure 8.

The results suggest that TLTRO-II worked perfectly as intended. TLTRO-II did not increase government bond holdings. Indeed, they seem to have had a negative effect, i.e. banks that participated in TLTRO-II decreased their sovereign bond holdings.

## **7. Conclusions**

The results suggest that the effect of TLTRO-II on bank lending was positive and worked as its designers had intended. In particular, TLTRO-II boosted credit to non-financial corporations, while effects on loans for house purchase and on loans for consumption are statistically insignificant. This result is surprising as TLTRO-II was targeted equally at both consumption lending and corporate lending.

The results also suggest that the main effect came from the participation decision, not from the intensity of the participation. While the positive effect of participation in TLTRO-II on corporate lending is very robust, the change in corporate lending seems unrelated to the amount borrowed under TLTRO-II.

An important result is that TLTRO-II did not increase the government bond purchases of the participating banks. Thus, the effect of TLTRO-II was quite different from the effect of the VLTROs (see Crosignani et al., 2019), and suggests that the targeting of credit operations is what matters in such policy operations.

While TLTRO-II did not induce unwanted sovereign bond purchases, it does not mean that targeting is a policy panacea. Instead, the policy implication is that if a central bank wishes to target its credit operations, the design of TLTRO-II may provide a good point of departure.

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## Appendix A

The following table shows the results from the baseline analysis. The PSM is performed with replacement and without calliper. In the difference-in-differences equation, June 2016 is the reference period. Standard errors are clustered at the bank level. Sample comprises 97 TLTRO-II banks and 30 other banks.

<i>Variable</i>	<b>LN (Loans to non-financial corporations)</b>		<b>LN (Loans for consumption)</b>		<b>LN (Loans for house purchase)</b>	
	<i>Estimate</i>	<i>Std. Error</i>	<i>Estimate</i>	<i>Std. Error</i>	<i>Estimate</i>	<i>Std. Error</i>
LN (Balance sheet)	0.79 ***	0.24	1.09 ***	0.27	0.64 *	0.28
Equity ratio	-1.75	1.51	2.10	1.58	-2.47 *	1.02
2015Jan X TLTROII	-0.01	0.04	-0.07	0.10	-0.06	0.05
2015Feb X TLTROII	0.02	0.04	-0.06	0.09	-0.04	0.05
X2015Mar X TLTROII	0.01	0.05	-0.07	0.09	-0.06	0.04
2015Apr X TLTROII	0.01	0.05	-0.07	0.08	-0.06	0.04
2015May X TLTROII	0.02	0.05	-0.06	0.08	-0.06	0.04
2015Jun X TLTROII	0.01	0.04	-0.05	0.07	-0.05	0.04
2015Jul X TLTROII	0.08 *	0.04	0.01	0.06	0.00	0.03
2015Aug X TLTROII	0.05	0.04	-0.01	0.05	-0.04	0.03
2015Sep X TLTROII	0.03	0.04	-0.01	0.05	-0.04	0.03
2015Oct X TLTROII	0.06	0.03	0.01	0.04	0.00	0.02
2015Nov X TLTROII	0.04	0.03	-0.01	0.04	-0.01	0.02
2015Dec X TLTROII	0.01	0.03	-0.05	0.04	-0.03	0.02
2016Jan X TLTROII	0.05	0.03	-0.03	0.03	-0.01	0.01
2016Feb X TLTROII	0.02	0.03	-0.03	0.03	-0.02	0.01
2016Mar X TLTROII	0.02	0.03	-0.03	0.03	-0.02	0.02
2016Apr X TLTROII	0.04	0.03	-0.01	0.03	-0.00	0.01
2016May X TLTROII	0.01	0.01	-0.02	0.03	-0.00	0.01
2016Jul X TLTROII	0.02	0.01	0.00	0.01	0.01	0.01
2016Aug X TLTROII	0.01	0.01	-0.02	0.01	-0.00	0.01
2016Sep X TLTROII	-0.00	0.02	-0.03	0.02	0.06	0.06

2016Oct X TLTROII	0.04	0.04	-0.03	0.02	0.06	0.06
2016Nov X TLTROII	0.08	0.05	-0.01	0.02	0.08	0.07
2016Dec X TLTROII	0.07	0.05	0.03	0.05	0.10	0.07
2017Jan X TLTROII	0.09	0.05	0.05	0.05	0.11	0.07
2017Feb X TLTROII	0.13 *	0.07	0.08	0.06	0.11	0.07
2017Mar X TLTROII	0.15	0.08	0.07	0.06	0.10	0.07
2017Apr X TLTROII	0.14	0.08	0.03	0.06	0.09	0.07
2017May X TLTROII	0.15	0.08	0.05	0.06	0.09	0.07
2017Jun X TLTROII	0.17 *	0.08	0.06	0.06	0.10	0.07
2017Jul X TLTROII	0.19 *	0.08	0.06	0.06	0.10	0.07
2017Aug X TLTROII	0.17 *	0.08	0.04	0.06	0.09	0.07
2017Sep X TLTROII	0.17 *	0.08	0.04	0.06	0.08	0.07
2017Oct X TLTROII	0.18 *	0.08	0.05	0.06	0.08	0.07
2017Nov X TLTROII	0.19 *	0.08	0.07	0.06	0.08	0.07
2017Dec X TLTROII	0.30	0.16	0.05	0.06	0.10	0.08
2018Jan X TLTROII	0.28 *	0.12	0.05	0.06	0.08	0.08
2018Feb X TLTROII	0.33 *	0.16	0.05	0.06	0.09	0.08
2018Mar X TLTROII	0.32 *	0.16	0.02	0.07	0.09	0.08
2018Apr X TLTROII	0.34 *	0.16	0.05	0.07	0.08	0.08
2018May X TLTROII	0.34 *	0.17	0.05	0.08	0.08	0.08
2018Jun X TLTROII	0.30	0.16	0.05	0.08	0.06	0.08
2018Jul X TLTROII	0.33 *	0.17	0.16	0.09	0.08	0.08

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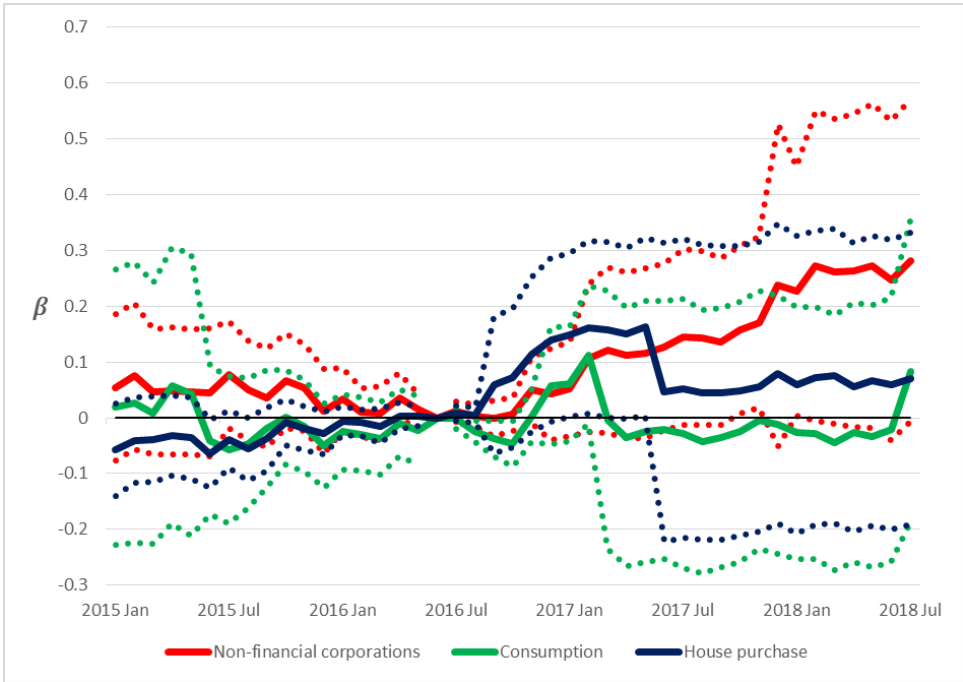
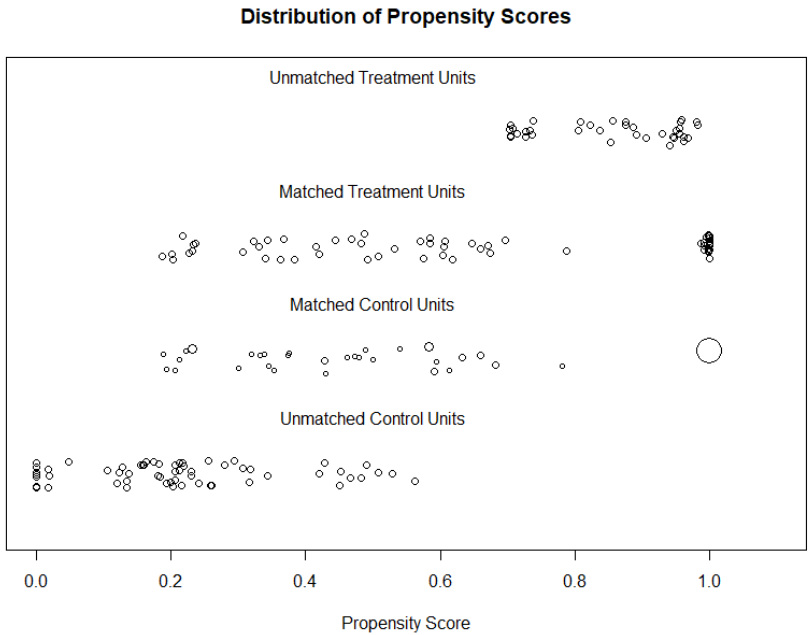
Observations	5461	5461	5461
R <sup>2</sup> / adjusted R <sup>2</sup>	0.379 / 0.252	0.346 / 0.212	0.274 / 0.126

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\*  $p < 0.05$  \*\*  $p < 0.01$  \*\*\*  $p < 0.001$

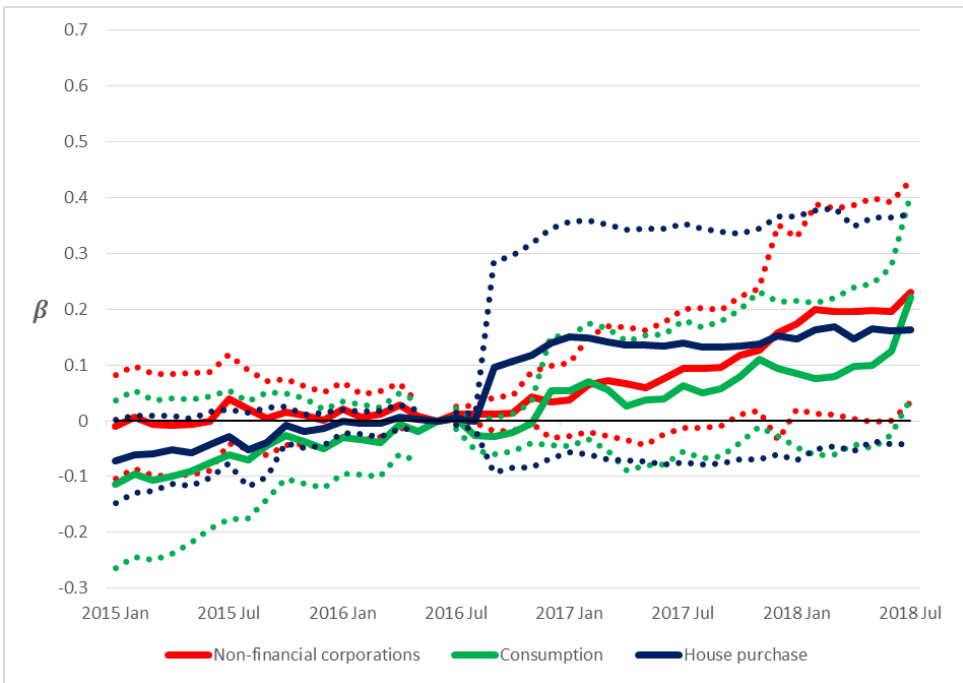
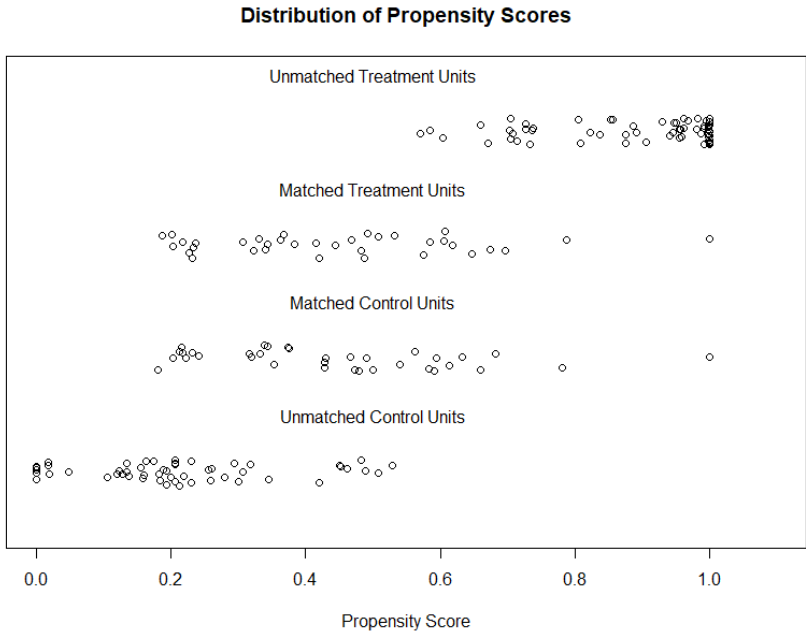
# Appendix B

The first figure represents PSM result when the analysis is done with calliper (0.05). The second figure shows the estimated effects on different types of lending after this modification in the matching procedure. Sample comprises 61 TLTRO-II banks and 31 other banks.



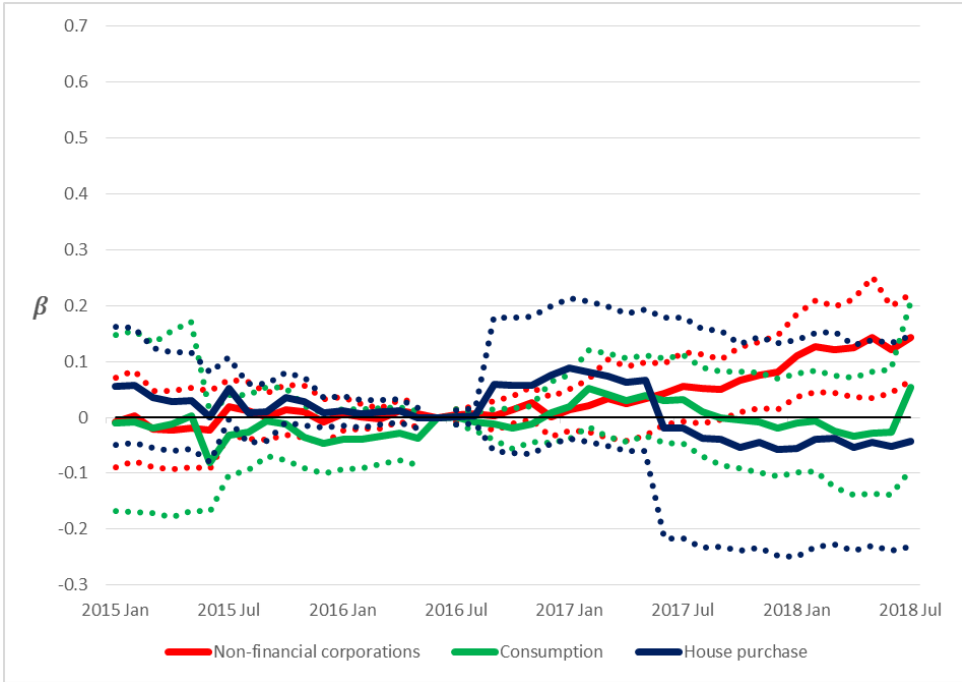
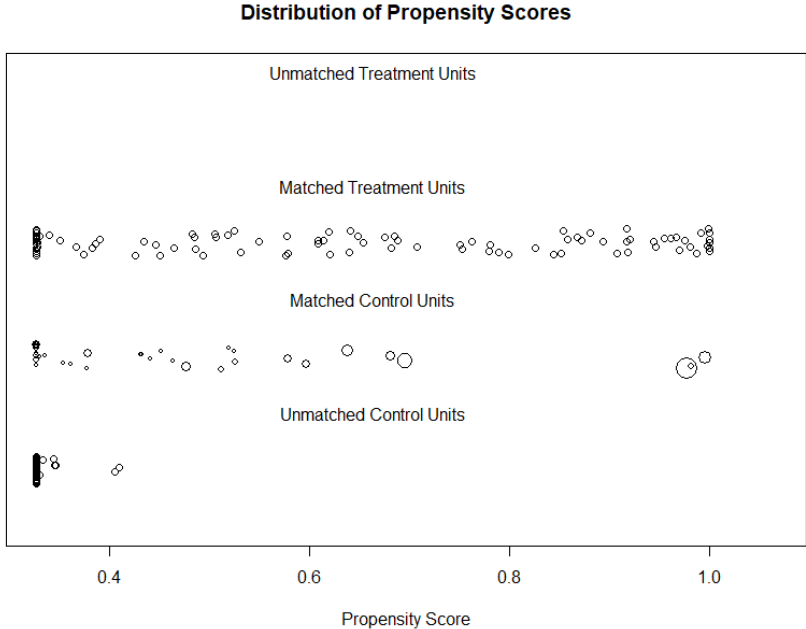
# Appendix C

The first figure represents the PSM results when the analysis is done with calliper (0.05) and without replacement. The second figure shows the estimated effects on different types of lending after this modification in the matching procedure. Sample comprise 35 TLTRO-II banks and 35 other banks.



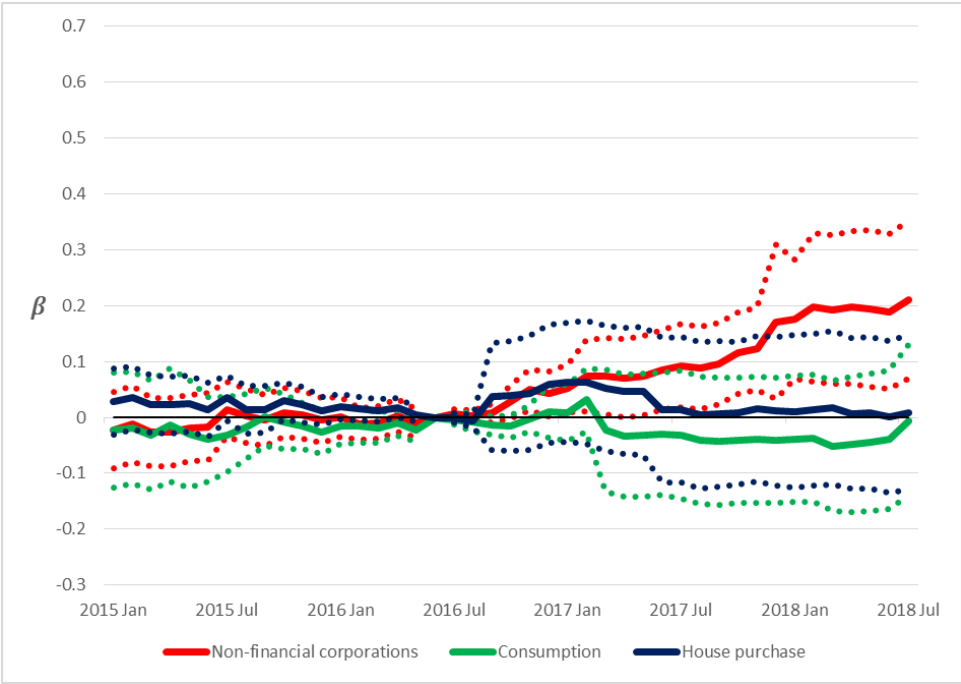
# Appendix D

The first figure represents the PSM results when the only explanatory variable in the logit model is the share of central bank credit in total liabilities. Otherwise, the PSM is as in the baseline. The second figure shows the estimated effects on different types of lending after this modification in the matching procedure. Sample comprises 97 TLTRO-II banks and 36 other banks.



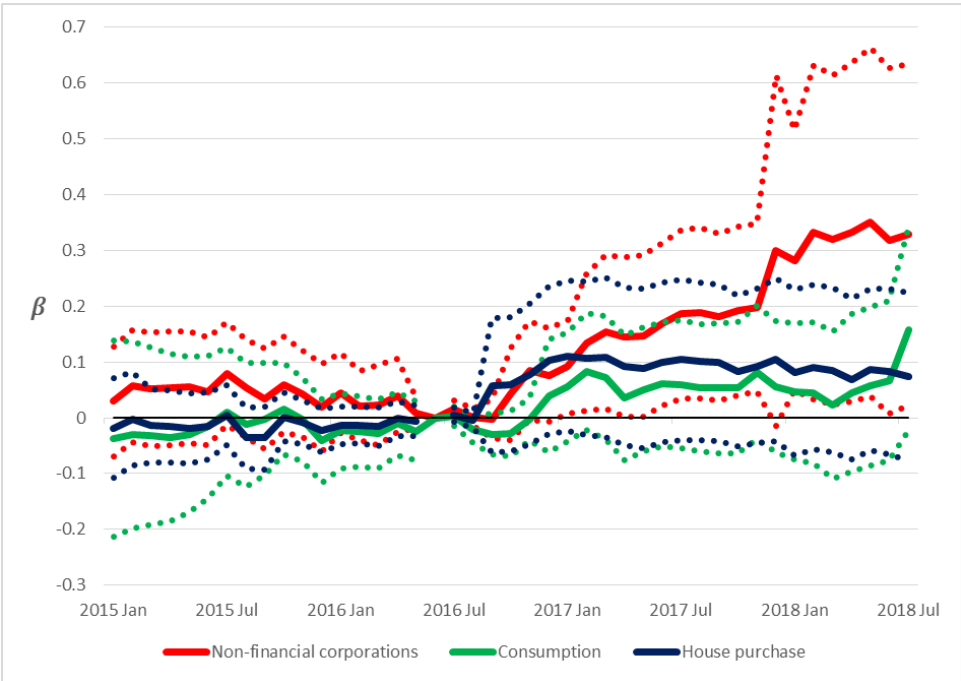
# Appendix E

The following figure represents the results without PMS. Everything else is as in the baseline. Sample comprises 97 TLTRO-II banks and 90 other banks.



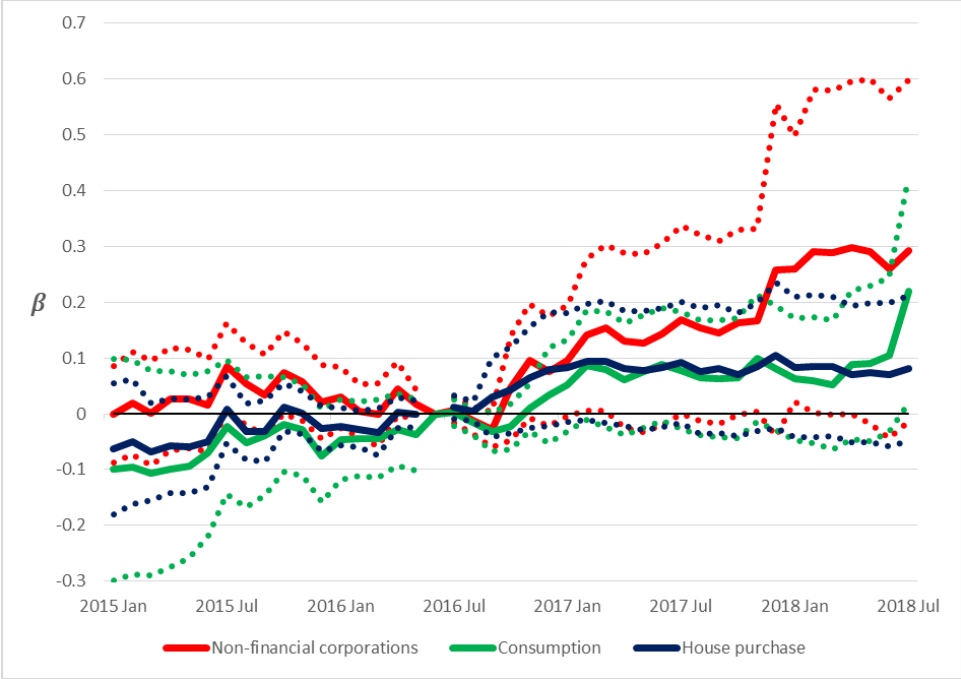
# Appendix F

The following figure represents the results after controlling for equity ratio, size, share of overnight deposits by households, share of debt securities issued and share of cash. The matching procedure is as in the baseline. Sample comprises 97 TLTRO-II banks and 30 other banks.



# Appendix G

The following figure represents the results after replacing country-time fixed effects with BLS indices. The PSM is as in the baseline. Sample comprises 97 TLTRO-II banks and 30 other banks.





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