The Effect of Targeted Monetary Policy on Bank Lending

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ABSTRACT

This paper studies the effect of central banks' targeted refinancing operations on bank lending. It utilizes data from the European Central Bank's targeted longer-term refinancing operations (TLTROs) together with monthly bank level balance sheet data from multiple countries. The effect of targeted policy is identified utilizing the institutional setting that provides natural instrumental variables and a proxy for credit demand. Unlike previous papers, this paper studies the effects on corporate loans and loans for consumption separately. The cumulative effect of TLTROs on participating banks' stock of corporate loans is estimated to be significant (about 20 per cent). However, the effect on lending for consumption is found close to zero. Furthermore, the positive effects on corporate loans are found to be driven by crisis countries suggesting that the effectiveness of monetary policy depends on the economic conditions. The paper also finds some evidence that the effect on government bond purchases is negative. This result is very different from the earlier results regarding non-targeted liquidity operations.

JEL Classification: E44; E51; E52; G21

Keywords: unconventional monetary policy, credit supply, TLTRO, bank lending

1. INTRODUCTION

When policy rates have been close to the effective lower bound, central banks have adopted a range of unconventional tools to stimulate the economy. One channel through which these tools operate is bank lending.[2] The unconventional tools have included providing banks with cheap long-term credit. For example, the European Central Bank (ECB) has conducted several longer-term credit operations that have been 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. Andrade et al. (2018) find that these operations have increased bank lending to non-financial corporations. Though the earlier literature has provided some evidence that supports the effectiveness of these

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[2] See for example Jiménez et al. (2012), Rodnyansky and Darmouni (2017), Altavilla et al. (2020), Di Maggio et al. (2020).

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tools, many questions have remained unanswered. Especially, the literaure concerning so called targeted refinancing operations is scarce. These targeted operations are the focus of this paper.

The first shortage of the literature is that it has not studied the effects of these liquidity operations to other types of loans than loans to firms. Because the credit market is quite different for households and firms, it is likely that the liquidity operations have very different effects on lending to non-financial corporations and lending to households, though the banks' are given equal reward for lending to households and lending to firms. Second, the literature finds that liquidity operations have increased bank lending both on the extensive margin and on the intensive margin (e.g. Benetton and Fantino, 2021). In other words, both the participation to the operations and the borrowed ammount matters. However, central banks have launched several targeted and non-targeted operations that have rather different incentive structures. Therefore, the results regarding one operation cannot be necessarily generalised to another operation. Another issue regarding the generalisation of the previous results is that the earlier literature has focused on the effects in single countries, though the effects may be very different in different economic conditions.[3] Finally, one important reason for targeting the liquidity operations in the euro area was probably the observation that non-targeted longer-term refinancing operations seemed to be used for buying sovereign debt (see Crosignani et al., 2020). Therefore, it should be analysed whether the targeted operations have had an effect of sovereign bond holdings of the banks or not.

This paper applies a difference-in-differences estimation to bank level dataset from multiple countries to analyse the effects of the second series of the ECB's targeted longer-term refinancing operations (TLTRO-II). The paper shows that the ECB's liquidity operations have boosted lending to non-financial corporations, but not the lending to households for consumption. This finding is interesting as the ECB does not favour corporate loans over loans for consumption. Unlike the previous literature, the paper finds that the positive impact is mainly explained by the effect of participation (extensive margin). The allotted amount of TLTRO-II does not seem to have been very important. In addition, the paper shows that the positive effects on corporate lending are largely driven by crisis countries. This suggests that the effectiveness of longer-term refinancing operations depends on the economic conditions under which they are implemented. The results also show that TLTRO-II did not increase participating banks' sovereign bond purchases. Instead, the effect is found negative. Thus, the results suggest that the ECB's targeting strategy was effective in this respect.

The remainder of the paper is as follows. Section 2 reviews the earlier literature. Section 3 describes the data and the used methodology. It is dividied into three subsections. The first one describes the institutional setting, the second one represents the data and the third one explains the methods used in this study. Section 4 shows the results. It begins from the baseline results that focus on the participation effect (or the extensive margin). Then it shows that assuming continuous treatment (the amount of TLTRO) yields different results. After that the section analyses potential cross-country differences between the effectiveness of TLTROs and the issue related to sovereign bond purcahses. Finally, the section considers the robustnsess of the results. Section 5 concludes.

2. LITERATURE REVIEW

Conventionally, the maturity of refinancing operations provided by central banks has been very short. For example, the maturity of the ECB's main refinancing operations is one week. In recent years, central banks have began to refinance banking sector with loans that have maturity of multiple years. The rationale of this policy change is, as Carpinelli and Crosignani (2021) note, that "In presence of uncertainty about the future role of the central bank as a liquidity provider,

[3] García-Posada and Marchetti (2016) study the effects in Spain, Andrade, Cahn, Fraisse, Mésonnier (2018) in France, Benetton and Fantino (2021) and Carpinelli and Crosignani (2021) in Italy.

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short-term liquidity is ineffective in stopping an ongoing credit contraction". Furthermore, central banks have began to incentivise banks to use this credit for lending to non-financial private sector (e.g. TLTROs in the euro area and Funding for Lending in the UK). In the euro area, the ECB launched the first series of TLTROs in the year 2014.

As these targeted tools are rather new, there are not many published papers that study their effectiveness. When it comes to non-targeted operations, Andrade et al. (2018), Carpinelli and Crosignani (2021) and García-Posada and Marchetti (2016) provide some evidence about their effectiveness using bank level data from single countries. VAR evidence is provided by Darracq-Paries and De Santis (2015).

When it comes to targeted operations, that is the main interest of this paper, Balfoussia and Gibson (2016) show that the first series of TLTROs (TLTRO-I) incresed lending to firms. In addition, contemporaneously with this paper, Benetton and Fantino (2021) show using data from Italy that TLTRO-I lowered the rates of corporate loans and incresed their amount. In addition, they find that the competition between banks matters for the effectiveness of targeted lending progmmes.

3. DATA AND METHODOLOGY

3.1. TLTRO-II

TLTRO-II was launched in June 2016 to ease private-sector credit conditions and stimulate credit creation. Four operations, one each quarter, were 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 matured 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 purchase).

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 TLTRO-II to increase eligible lending differed from the incentives in TLTRO-I. In 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 TLTRO-I matured after about two years. The key differences between VLTRO operations of 2011–2012 and TLTRO operations are summarised in Table 1.

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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 operations: 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.

Source: ECB's press releases.

Table 2
Descriptive statistics grouped by decision to participate in TLTRO-II

Variable	TLTRO-II participant (n = 97)		TLTRO-II non-participant (n = 90)	
	Mean	Median	Mean	Median
Balance sheet (million €)	106 989	40 043	72 290	14 203
Central bank credit to total liabilities	4.4%	2.5%	0.7%	0.0%
Household deposits to total liabilities	25.5%	24.3%	33.7%	36.5%
Equity ratio	10.4%	9.1%	10.0%	8.2%
Eligible credit to total assets	26.0%	24.9%	27.1%	27.0%

Note: 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.

Source: Author's calculation

3.2. Data

The main data are taken from the ECB's individual balance sheet items (IBSI) database. The data are monthly and at bank level. The used data are from January 2015 to July 2018. The IBSI data are linked to confidential information about bank's total borrowing in TLTRO-II.

IBSI data offer several advantages. First, they make it possible to analyse TLTRO-II in multiple 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. While IBSI does not cover all euro area banks the sample is quite large and includes about 300 large banks that are from all the euro area countries. The final dataset covers 187 banks from 18 countries due to missing data.[4] However, the data are still very representative as the interpreted bank

[4] All the banks that have missing data from necessary variables 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. All the banks from France are excluded because the data about central bank credit are missing.

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covered about 62 per cent of the total corporate loans in the euro area prior TLTRO-II. Some key descriptive statistics of the assessed banks, grouped by the decision to participate TLTRO-II, are shown in Table 2.

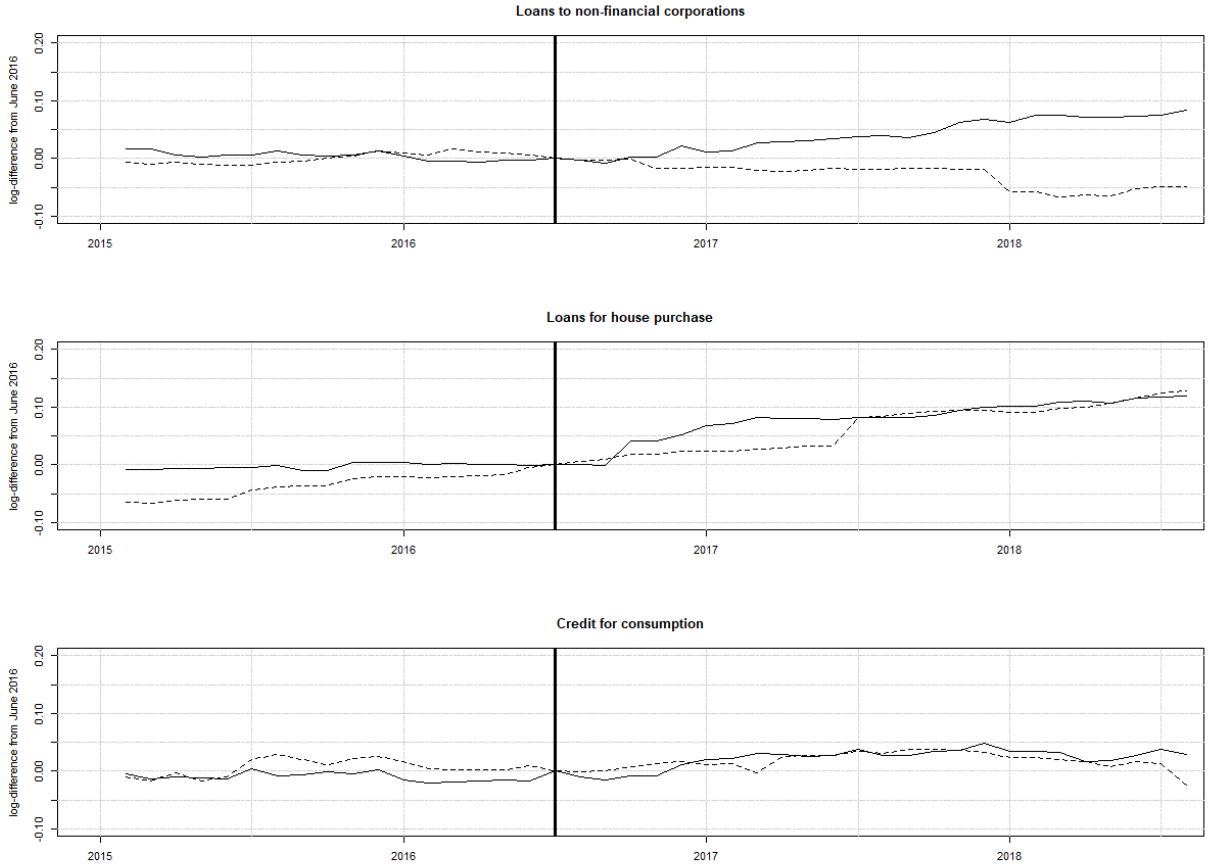
Figure 1 shows the average development of loans to non-financial corporations, loans for house purchase and loans for consumption by groups. The solid lines show the development of the TLTRO banks and dashed lines the developments of non-TLTRO banks. The TLTRO banks increased corporate lending compared to other banks after the beginning of TLTRO-II. Instead, it is rather difficult to observe significant diverging in other types of loans.

Figure 2 shows the average development of loans to non-financial corporations, loans for house purchase and loans for consumption among the banks that participated in TLTRO-II. Now, the grouping is based on the share of TLTRO-II in total liabilities. The size of balance sheet is from May 2016 (before TLTRO-II). The solid lines show the development of the banks that had the share of TLTRO-II above the median and dashed lines the developments of the banks that had a ratio below the median. The differences between groups remain rather constant. This suggests that the allotted amount of TLTRO-II was not essential.

This preliminary analysis has not taken into account the fact that banks could choose whether to participate in TLTRO-II or not. Additionally, this analysis has not considered the role of credit demand. These issues are assessed in the remaining sections.

Figure 1

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



Note: The credit stocks are in logs. The treatment group includes 97 banks and the control group 90 banks.

Source: Author's calculation.

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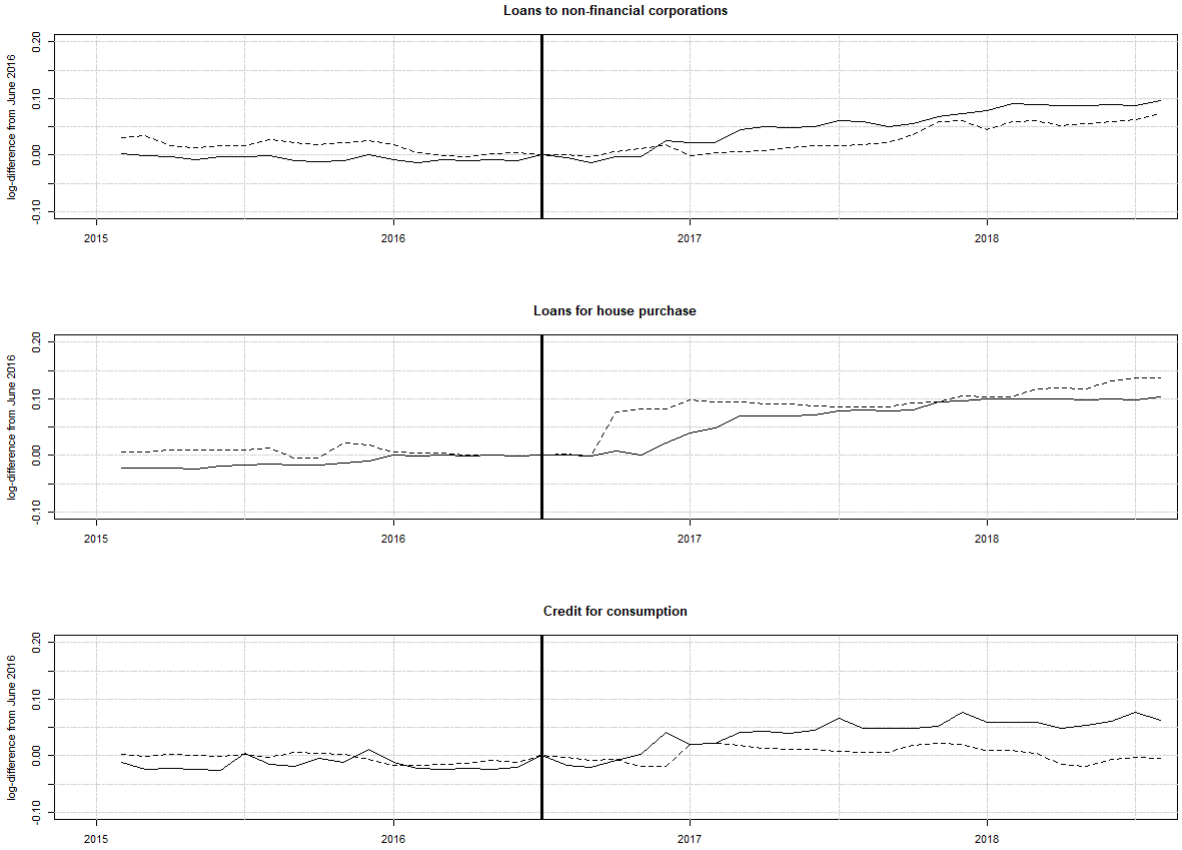
Figure 2
The development of different types of credit in the high-intensity participants (solid line) and low-intensity participants (dashed line) groups in comparison to the situation as of June 2016



Note: The credit stocks are in logs. The high-intensity group includes 49 banks and the low-intensity group 48 banks. High-intensity participants are those that had the ratio of TLTRO-II take-up to total liabilities (in May 2016) above the median. Low-intensity participants are the banks that borrowed in TLTRO-II, but had the ratio below the median.

Source: Author's calculation.

3.3. Methodology

The paper applies a difference-in-differences approach to study the effects of TLTRO-II on bank lending. It uses two types of specifications. First, in the baseline regression the treatment is assumed to be binary: participation in the TLTRO-II or not. This specification is used to assess the participation effect of TLTROs. Second, it is possible that the impact of TLTROs depends on the amount allotted to the banks. As was discussed earlier, some other studies find that the targeted operations have a positive effect on bank lending both on the extensive and on the intensive margin. The second specification is used to analyse this issue.

To be concrete, the baseline specification is:

$$\ln(Y_{ict}) = \alpha_{ic} + \tau_{ct} + \Sigma_h \beta_h (D_h \cdot TLTRO_{ci}) + \gamma Z_{ict} + e_{ict}, \quad (1)$$

where Y_{ict} is the stock of credit on the balance sheet of bank i in country c at time t, α_{ic} includes bank fixed effects, τ_{ct} includes country-time fixed effects, Z_{ict} includes time-varying bank-specific control variables that are the size of balance sheet in logs and equity ratio in the baseline analysis. $TLTRO_{ic}$ equals 1 if the bank participated in TLTRO-II and D_h, where $h \in \{2015Jan, \ldots, 2018Jul\} \backslash \{2016Jun\}$, includes indicators for time periods. June 2016 is the reference month. This means that the regression coefficients β_h tell how the credit granted by TLTRO banks differed from other banks in a given month relative to the difference between the

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groups in June 2016. Standard errors are clustered at bank and month level to allow for serial correlation and heteroscedasticity in the error term e_{ict}.

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 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.

To assess whether the allotted amount of TLTRO-II was important, we use a specification slightly different from Eq. (1). The modified model is:

$$\ln(Y_{ict}) = \alpha_{ic} + \tau_{ct} + \Sigma_h \beta_h^* (D_h \cdot \log(TLTRO\ amount_{ci})) + \gamma Z_{ict} + e_{ict}, \quad (2)$$

where the binary treatment variable is replaced by the natural logarithm of the amount borrowed in TLTRO-II.

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. The coefficients may also be biased downwards, if participating banks had strong deleveraging pressures.

To tackle this selection bias, we use instrumental variable estimation. We utilise two different novel properties of TLTRO-II. First, TLTRO-II, launched in June 2016, was mainly used to replace earlier TLTROs that were mainly borrowed in 2014 and in the beginning of 2015.[5] In May 2016, TLTRO-I covered about 83 per cent of the total credit from the ECB. Therefore, the amount of credit from the ECB prior TLTRO-II is highly correlated to the amount borrowed in TLTRO-II. The amount of earlier TLTROs is also a valid instrument as it is quite difficult for a bank to forecast its lending opportunities multiple years ahead. In addition, in the first series of TLTROs, the incentive structure was such that it motivated banks to increase their lending at very beginning of the operations.[6] Therefore, it is probable that participation in TLTRO-I was not affected by the expected lending opportunities during the years 2016–2018. Thereby, the amount of TLTRO-I is a valid instrument for the amount of TLTRO-II. In Eq. (1), where the treatment is binary, we use $\frac{Credit\ from\ the\ ECB\ in\ May\ 2016}{Balance\ sheet\ in\ May\ 2016}_{ci}$ as an instrument for the participation in TLTRO-II. In Eq. (2), the used instrument is $\log(Credit\ from\ the\ ECB\ in\ May\ 2016)_{ci}$.

Another novel property of TLTRO-II is the fact that the amount a bank could borrow was predetermined by the ECB. This property provides another potential instrumental variable. The maximum amount a bank could borrow in TLTRO-II was based on its amount of loans to non-financial corporations and loans for consumption (so called eligible loans) in January 2016. This constraint was predetermined by the ECB and hence exogenous. Thus, the amount of eligible loans in January is another potential instrument for the participation in TLTRO-II. A similar identification strategy is used by Benetton and Fantino (2021) to analyse the effects of TLTRO-I. Because all the banks in the sample had eligible loans in January 2016, the share of eligible

[5] In the initial operation of TLTRO-II in June 2016, banks borrowed 399 billion euros. Nevertheless, the total stock of TLTROs increased only by 38 billion euros.

[6] In TLTRO-I, the participating banks were motivated to increase their eligible lending by promising a possibility to borrow more TLTRO credit if they increased lending. Because all TLTRO-I credit had to be paid back in 2018, the incentive structure motivated banks to increase their lending in the beginning of TLTRO-I. The reason for this is that the last operations of TLTRO-I had only a maturity of about two years. Thus, it was reasonable to increase lending as early as possible, and then be able to borrow more TLTRO credit with a long maturity.

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loans in total assets is a weak instrument. Therefore, the amount of eligible loans is used as an instrument only in Eq. (2) where the treatment is continuous. Specifically, the used instrument is $\log(\textit{Eligibje loans in January } 2016)_{ci}$.

In addition to the instrumental variables, the paper considers propensity score matching as a robustness check and shows that the results are robust to controlling for many observable variables.

In addition to the selection bias, another problem is the role of credit demand which is difficult to control for. Many earlier studies have utilised the approach of Khwaja and Mian (2008) and controlled the demand at firm level. Because we have no data about firms or households that had loans from multiple banks, we use country-time fixed effects. The problem in the approach of Khwaja and Mian (2008) and country-time fixed effects is the possibility of capturing supply side effects as well. If TLTROs increased the lending of all the banks and not just the lending of participating banks, then country-time fixed effects (or firm-time fixed effects) would unintendedly capture these indirect effects as well. The problem with country-time fixed effects is also the assumption that all the banks within a country faced identical credit demand. To mitigate these concerns, we test the robustness of the results by replacing τ_{ct} by τ_t and adding $\log(\textit{Loans for house purchase})_{ict}$ into Z_{ict}. The idea behind this control variable is the following. Loans for house purchase were excluded from the eligible loans. Therefore, it is likely that changes in loans for house purchase reflect mainly changes in credit demand.

4. RESULTS

4.1. Baseline results

First, we estimate Eq. (1) using 2SLS. The instrument we use is the average share of central bank credit in total liabilities in May 2016. Specifically, we instrument the interactions $D_h \cdot TLTRO_{ci}$ by $D_h \cdot \left(\frac{\textit{Credit from the ECB in May } 2016}{\textit{Balance sheet in May } 2016}\right)_{ci}$. The banks that participated in the first series of TLTROs were likely to participate also in TLTRO-II. Therefore, it is not surprising that the F-statistics of the first-stage regressions are about 41. Thus, weak instruments are not an issue.

Figure 3 shows the estimated values of the vector β_h, i.e. the estimated effects of TLTRO-II in various months for different types of credit. The solid lines represent the point estimates, and the dashed lines 90 per cent confidence intervals. Appendix A provides some more information about the model. In every month before June 2016, the estimated effects do not differ from zero, which supports the common trend assumption. The effect on corporate loans is positive and statistically significant. F-statistic for the joint significance of interactions from July 2016 to July 2018 is 2.9 ($p = 0.001$). The cumulative effect of TLTRO-II on participating banks' corporate lending is estimated to exceed 20 per cent. Instead, the estimated effect on loans for consumption is actually negative, though not statistically significantly. F-statistic for the joint significance of interactions from July 2016 to July 2018 is 0.6 ($p = 0.935$). This is surprising as TLTROs were also targeted on loans for consumption.

In the sample, the banks that took up TLTRO-II had lent about 50 per cent of the outstanding corporate loans in June 2016. If this share could be generalised to the whole population and if TLTRO-II did not affect to the banks that did not participate, it would mean that TLTRO-II increased the total stock of corporate credit about 10 per cent cumulatively from June 2016 to July 2018.

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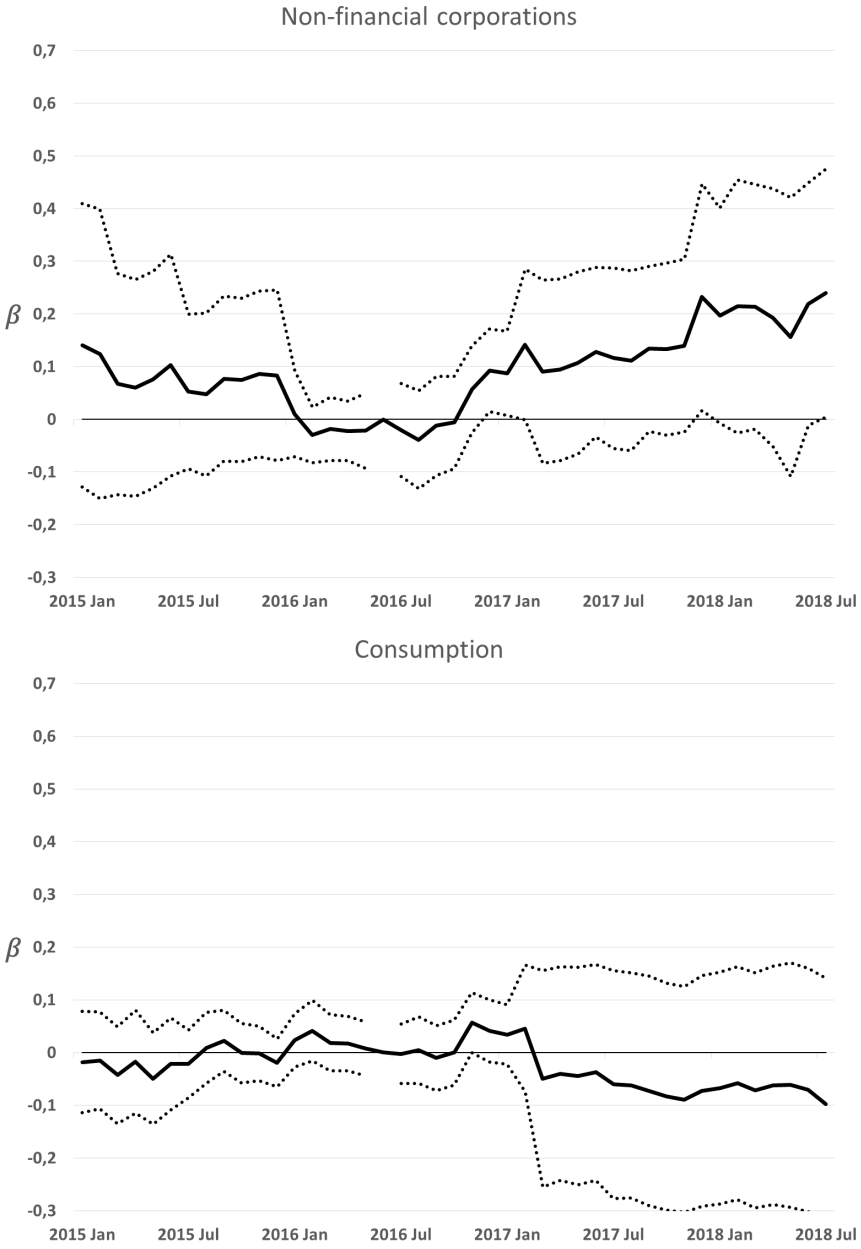
Figure 3
The estimated effects of TLTRO-II (parameters in vector β) on different types of credit



Note: The dashed lines represent 90 per cent confidence intervals. Standard errors are clustered at bank and month level. The share of central bank credit in total liabilities prior TLTRO-II is used as an instrument for participation in TLTRO-II.

Source: Author's calculation.

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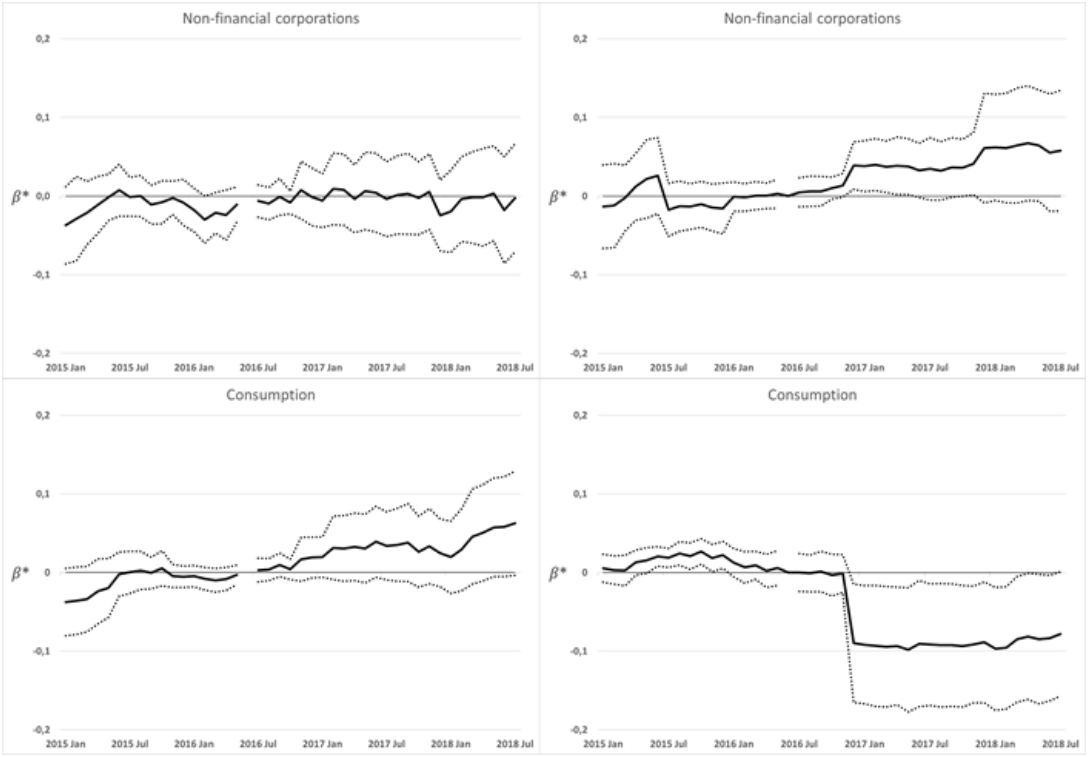
Figure 4
The estimated effects of the amount of TLTRO-II (parameters in vector β^*) on different types of credit



Note: The dashed lines represent 90 per cent confidence intervals. Standard errors are clustered at bank and month level. On left, the (log) amount of central bank credit prior TLTRO-II is used as an instrument for the (log) total borrowing in TLTRO-II. On right, the (log) amount of eligible loans in January 2016 is used as an instrument for the (log) total borrowing in TLTRO-II.

Source: Author's calculation.

4.2. The amount of TLTRO-II

So far, we 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 and to households for consumption. This kind of relationship is quite challenging to observe (see Figure 2). The correlation between TLTRO-II borrowing and growth in lending to non-financial corporations is practically zero (Pearson correlation is -0.02 and it is clearly insignificant).

To further asses this relationship, we drop all banks that did not participate in TLTRO-II from the baseline analysis (entire control group) and add the natural logarithm of total TLTRO-II into Eq. (2). In other words, we analyse only the banks that participated in TLTRO-II (97 banks) and group them by their TLTRO-II amounts. We instrument the (log) total take-up in TLTRO-II by the (log) amount of central bank credit in May 2016. Additionally, we use the (log) amount of eligible loans in January 2016 as an alternative instrumental variable.

Figure 4 shows the estimated effects. The estimates on the left-hand side are based on the amount of central bank credit in May 2016 and the estimates on the right-hand side are based on the amount of eligible loans in January 2016. The estimates based on eligible loans suggest that the allotted amount of TLTRO-II had an impact on bank lending. Instead, the estimates that are based on the amount of central bank credit are insignificant. The values of F-statistics for these two alternative instruments are about 14 and 269. Thus, assuming that both instrumental variables are valid, one should give more weight to the results based on the stronger instrument: amount of eligible loans in January 2016.

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The results are potentially unintuitive and puzzling, but there are also some good reasons for the conclusion that the amount of TLTRO did not matter so much. As was explained in Subsection 3.1, the banks were expected to achieve a certain threshold for their bank lending to receive lower interest rate. Therefore, banks with high TLTRO take-ups could use the part of TLTRO to something else than eligible lending without losing the low interest rate.

4.3. Cross-country differences

There are large cross-country differences when it comes to the state of banking sector or economic conditions. Therefore, it is likely that the effects of TLTRO-II were different in different countries. For example, Albertazzi, Nobili and Signoretti (2021) observe that the transmission of conventional monetary policy is stronger for weaker banks. However, their results suggest that when it comes to unconventional monetary policy, the transmission is stronger among strong banks. Boeckx, de Sola Perea and Peersman (2020) find some evidence in favour of the opposite conclusion. Thus, the literature regarding the bank lendig channel of uncoventional monetary policy tools is rather mixed. In addition there may some other reasons, why monetary policy may have different effects in different countries. More generally, the cross-country differences in the effects of monetary policy has been studied by Burriel and Galesi (2018). They find that countries with more fragile banking systems benefit the least from unconventional monetary policy measures.

To assess this question, we calculate a dummy variable that equals 1 if the bank's home country is Spain, Italy, Greece or Portugal. These countries form a group that we call "crisis countries". We replace the interactions $D_h \cdot TLTRO_{ci}$ in Eq. (1) by interactions $crisis_c \cdot D_h \cdot TLTRO_{ci}$. This means that the treatment group consists of the banks that participated in TLTRO-II and were located in the crisis countries. Otherwise, model specification and estimation are as in the baseline analysis.

The coefficient estimates are shown in Figure 5. The results hint that the effect on bank lending has been stronger in the crisis countries than elsewhere. This result indicates that it is problematic to generalise results obtained from a single country to euro area level. However, this issue requires more research. It is not clear, what is the underlying reason for heterogeneous effects. One potential reason is the state of the banking sector, but deeper analysis regarding this topic is left for the future research.

4.4. Effect on sovereign bond purchases

Crosignani et al. (2020) find that a large part of VLTROs went to buying sovereign bonds in Portugal. The fact that VLTROs was used to buy bonds in crisis countries was possibly one reason why the ECB chose to target its TLTROs. In principle, TLTROs create an incentive to replace government bonds by eligible loans. However, as discussed in the previous sections, the banks had to achieve a certain lending threshold, after which they were rewarded with a lower rate by the ECB. After achieving this threshold, the incentives in favour of eligible lending disappear. Therefore, the effect of TLTROs on sovereign bond purchases is ambiguous.

To investigate whether targeting worked as intended, we estimate the Eq. (1) as in the baseline analysis, but use the natural logarithm of sovereign bond holdings as a dependent variable and keep the treatment as in the previous section. The results are shown in Figure 6.

The results suggest that TLTRO-II worked as intended. TLTRO-II did not increase government bond holdings. Instead, the operations seem to have had a negative effect. However, the reason for this result is not necessarily the design of TLTRO-II. The different effect from Crosignani et al. (2020) might be driven, for example, by different macroeconomic conditions.

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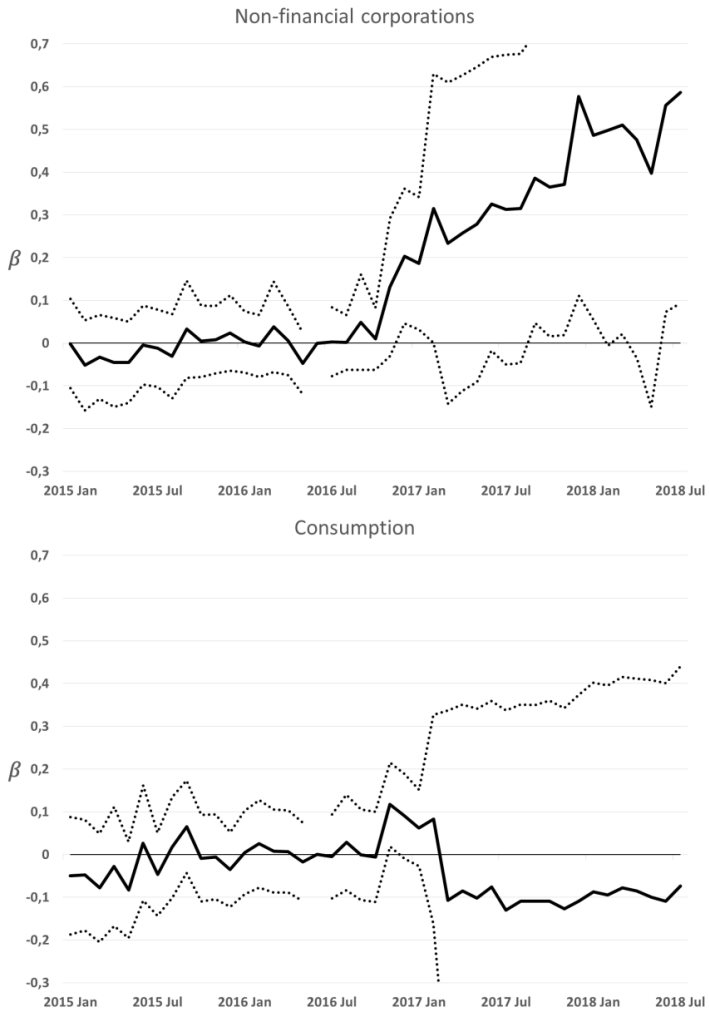
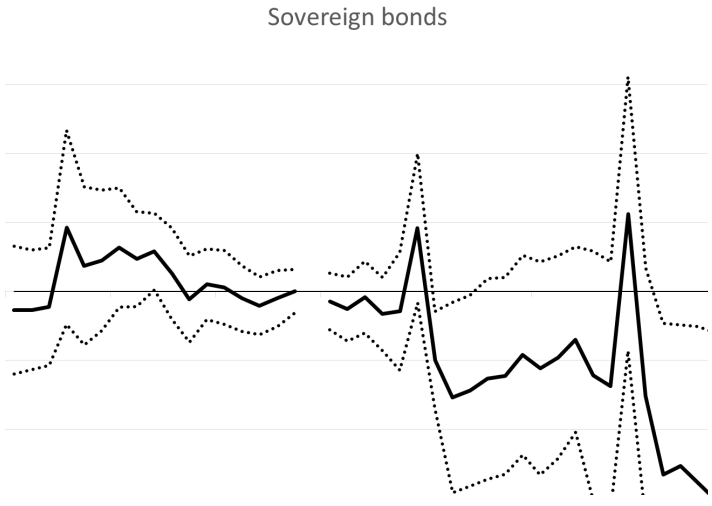
Figure 5
The estimated effects of TLTRO-II in crisis countries on different types of credit



Note: The treatment group consists of the banks that participated in TLTRO-II and are located in Spain, Italy, Greece or Portugal. The dashed lines represent 90 per cent confidence intervals. Standard errors are clustered at bank and month level. The share of central bank credit in total liabilities prior TLTRO-II is used as an instrument for participation in TLTRO-II.

Source: Author's calculation.

Figure 6
The effect of TLTRO-II on sovereign bond holdings



Note: The endogenous variable is (log) government bond holdings. The treatment group consists of the banks that participated in TLTRO-II and are located in Spain, Italy, Greece or Portugal. The dashed lines represent 90 per cent confidence intervals. Standard errors are clustered at bank and month level. The share of central bank credit in total liabilities prior TLTRO-II is used as an instrument for participation in TLTRO-II.

Source: Author's calculation.

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Table 3
Logit model used in the propensity score matching

	Participation in TLTRO-II
Predictors	**Log-Odds**
(Intercept)	-3.30*
Dlog(Loans to non-financial corporations)	-0.96
Dlog(loans for house purchase)	-16.58
Dlog(loans for consumption)	1.97
log(Balance sheet)	0.33**
Cash to total assets	-7.72
Household deposits to total liabilities	-0.91
Equity ratio	2.82
Observations	187
R^2 Tjur	0.100

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

Note: The used variables are calculated from bank-level January 2015 to May 2016 averages, i.e. before TLTRO-II.

Source: Author's calculation.

4.5. Robustness

As was shown earlier, the banks included in the sample were quite heterogeneous, for example, when it comes to their size (Table 2). With a perfect instrumental variable, this should not bias the results. However, one may always argue that the used instrumental variables are not valid, and there may be selection bias present. To analyse, if the results are driven by the differences in the treatment and control group, we use propensity score matching. Specifically, we estimate a logit model that predicts the participation in TLTRO-II based on banks' observable characteristics before TLTRO-II. Thereafter, the banks that borrowed in TLTRO-II are matched with other banks based on their estimated likelihood to participate using nearest-neighbour algorithm with replacement and calliper of 0.1.

In the logit model, we include such variables that could potentially affect the participation decision. Specifically, we include average growth rates of different types of lending before TLTRO-II. It is possible that such banks that were already increasing their lending self-selected into TLTRO-II because they believed that continuing increasing lending would be easy. On the other hand, it also possible that banks that were doing poorly self-selected into TLTRO-II, because they were unable to receive market-based funding. In addition, choosing loan growth variables makes the common trends assumption more reliable: we choose such banks that shared the common trend in loan growth. We also include the average size of the banks before TLTRO-II as the participating banks were much larger than the others. Additionally, we consider the share of cash, share of household deposits and equity ratio. The estimated logit model is reported in Table 3, and Figure 7 shows the results from the propensity score matching. The matching drops 7 banks from the treatment group (participants) and 40 from the control group (non-participants). The results show that it is rather difficult to find observable variables that could explain the participation decision. In other words, based on the observable variables, it is difficult to argue that selection bias plays a significant role.

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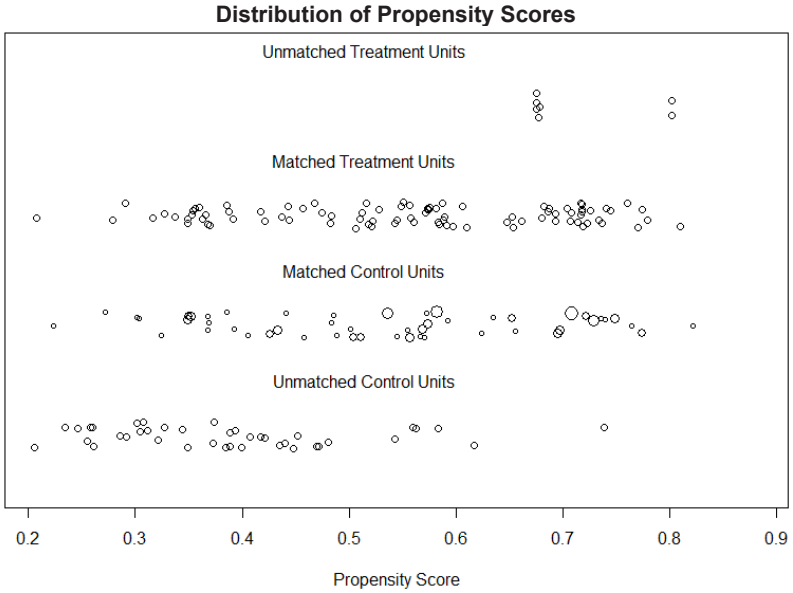
Figure 7
Results after using loans for house purchase as a proxy for credit demand



Note: The matching is done using nearest neighbour algorithm with replacement and 0.1 calliper. 7 banks are dropped from the treatment group and 40 from the control group. Thus, the final sample consists of 90 TLTRO banks and 50 other banks.

Source: Author's calculation.

The results from the baseline analysis with this subsample of banks is shown in Figure 8. The results remain roughly the same. Actually, the positive effect on corporate lending is now even more clearly statistically significant. The effect on lending for consumption is still close to zero and statistically insignificant. Therefore, it is difficult to argue that the results were biased downwards or upwards due to self-selection.

Another potential issue that may affect the results is demand. There are many ways to control for credit demand, and so far, we have used country-time fixed effects only. Figure 9 shows the results when country-time fixed effects are replaced by time fixed effects and $\log(\textit{Loans for house purchase}_{ict})$ is used as a control variable. Because TLTROs were targeted on loans to households excluding loans for house purchases, it is likely that the variation in the stock of mortgages reflects mainly variation in loan demand. The estimation is done using the full sample. This modification lowers the estimate for the effect on corporate lending a bit. The estimated effect on lending for consumption is still close to zero and statistically insignificant. Therefore, our results seem not to depend on the chosen way of controlling for credit demand.

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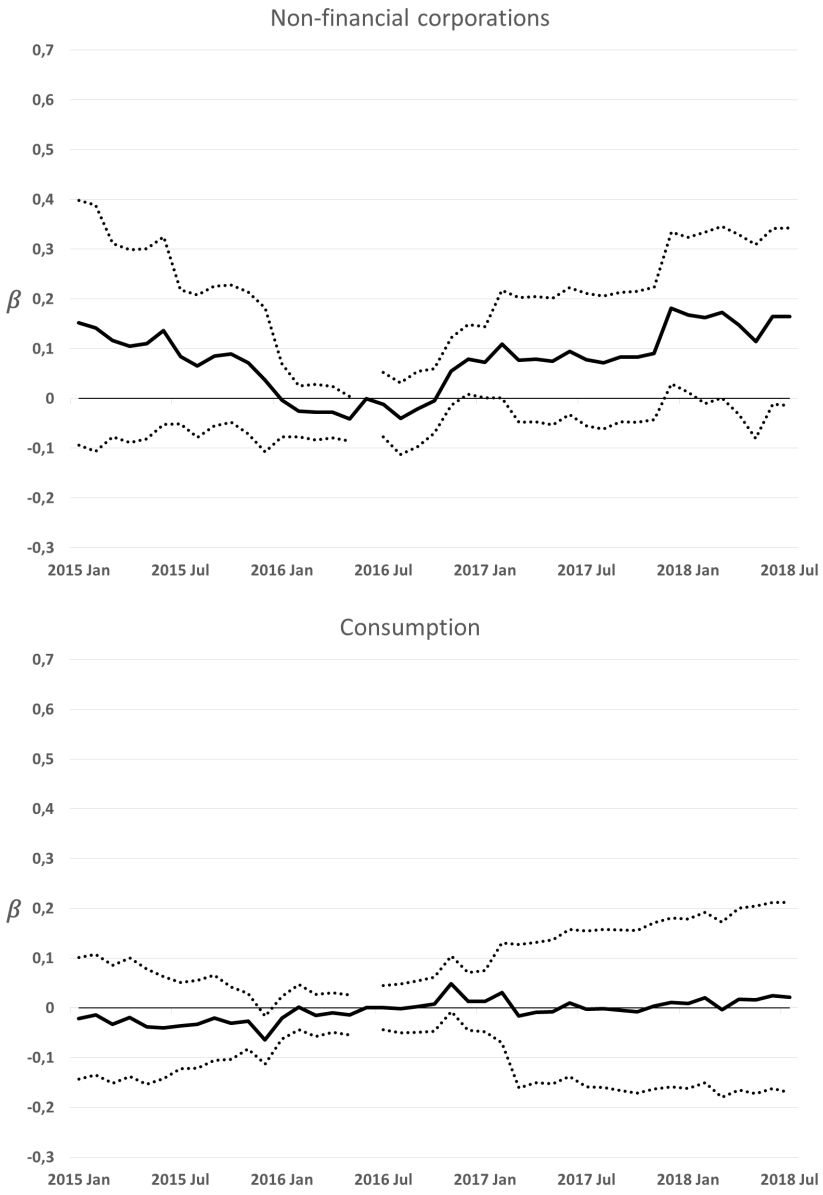
Figure 8
Results after propensity score matching



Note: The dashed lines represent 90 per cent confidence intervals. Standard errors are clustered at bank and month level. The share of central bank credit in total liabilities prior TLTRO-II is used as an instrument for participation in TLTRO-II.

Source: Author's calculation.

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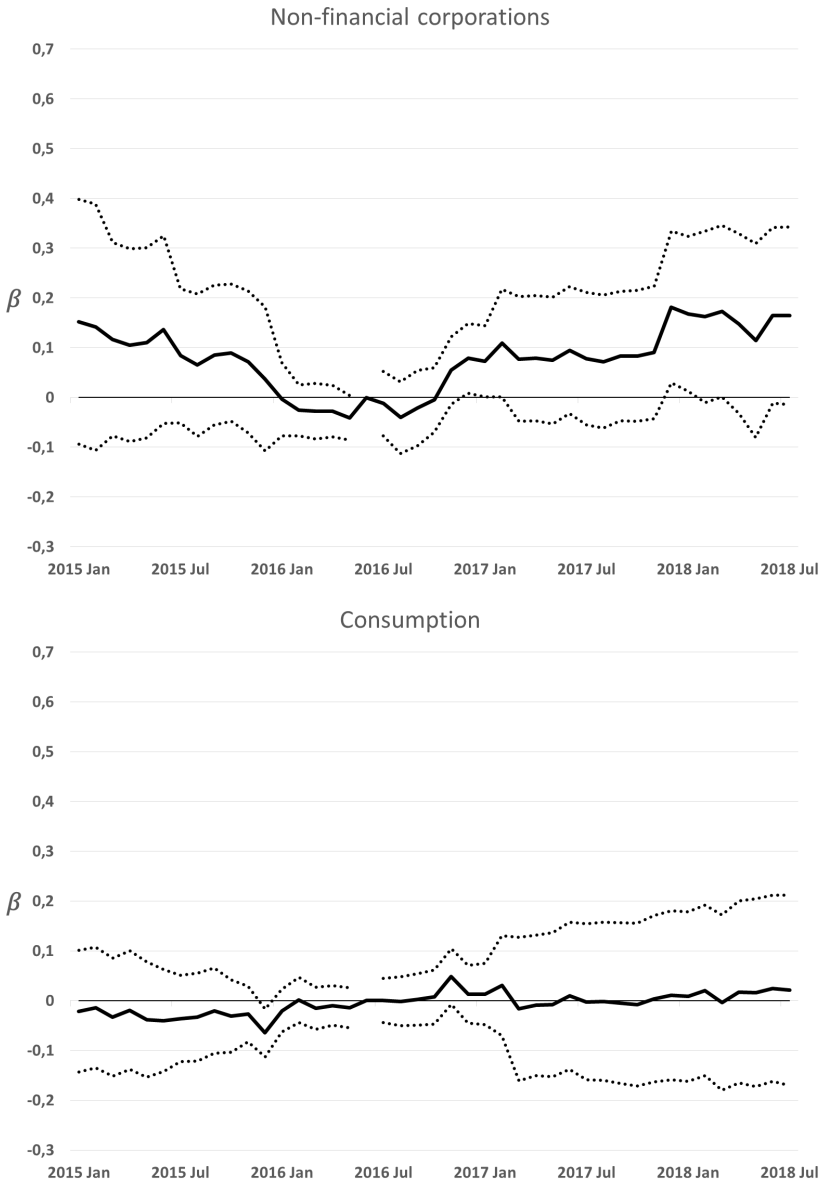
Figure 9
Results after using loans for house purchase as a proxy for credit demand



Note: The dashed lines represent 90 per cent confidence intervals. Standard errors are clustered at bank and month level. The share of central bank credit in total liabilities prior TLTRO-II is used as an instrument for participation in TLTRO-II.

Source: Author's calculation.

5. CONSLUSIONS

The results show that the effect of TLTRO-II on bank lending was positive. In particular, TLTRO-II boosted credit to non-financial corporations, while the effect on loans for consumption is estimated to be close to zero and statistically insignificant. This result is surprising as TLTRO-II was targeted equally at both consumption lending and corporate lending. Because the zero effect was unexpected, we do not have any obvious theoretical explanation for this in our mind. The explanation for the result might be related to, for example, differences in market power in different credit markets (Benetton and Fantino, 2021). One of the usual suspects for strange results in this field is the way loan demand is controlled for. In the baseline analysis, we use country-time fixed effects. This technique has its drawbacks, and therefore we assess the robustness of the results by controlling credit demand using loans to households for house purchase, which is

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excluded from the eligible lending and thus a good proxy for credit demand (especially regarding households). This alternative way of controlling for credit demand does not change the results. Another issue that might drive the results is the fact that the banks were rather heterogeneous before the treatment. If one had a perfect instrumental variable, this fact should not affect the results. Because the validity of instrumental variables is in the end a matter of argumentation, and one might maybe argue that central bank borrowing prior the treatment is not necessarily a perfectly valid instrument, we consider also propensity score matching as a supplementary technique for tackling potential selection bias. Controlling for potential variables that might explain the selection to the treatment does not change the results.

The results also suggest that the effects of TLTROs have not been the same in all the countries. This is not surprising as there are many papers that show that the effects of monetary policy are different in different countries. However, the result is important, because the earlier studies that analyse the effects of longer-term refinancing operations with microdata focus on single countries. Thus, these results are difficult to generalise to other countries. According to the results, the effects have been strongest in countries most affected by the crisis.

The results show as well that TLTRO-II did not increase the government bond purchases of the participating banks in crisis countries. Thus, the effect of TLTRO-II was quite different from the effect of the VLTROs (see Crosignani et al. 2020) and suggests that the targeting of credit operations mattered.

Though this paper has covered many open questions related to the targeted monetary policy, there are certainly many questions that should be answered in the future research. One shortage in the current literature is that it is mainly empirical. As the targeted tools are becoming more and more "conventional" in the central banks' toolboxes, it would be necessary to understand better how and why these tools work.

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APPENDIX

Table A1
Some key information about the baseline regressions

	Dependent variable:	
	Log(Loans to NFCs)	**Log(Loans for consumption)**
	(1)	(2)
log(Total Assets)	0.915**	0.914**
	(0.202)	(0.242)
Equity ratio	0.085	2.368*
	(0.886)	(1.186)
Observations	8,041	8,041
R^2	0.990	0.987
Adjusted R^2	0.989	0.986
Residual Std. Error (df = 7054)	0.191	0.213

* $p < 0.05$ ** $p < 0.01$

Note: The interactions are reported in Figure 3. Standard errors are clustered at bank and month level.

Source: Author's calculation.

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