Bank of Finland Research Discussion Papers
13 • 2023

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Bank of Finland
Research

Bank of Finland Research Discussion Papers
Editor-in-Chief Esa Jokivuolle

Bank of Finland Research Discussion Papers 13/2023
21 September 2023

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ISBN 978-952-323-446-8, online
ISSN 1456-6184, online

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# Assessing targeted longer-term refinancing operations: identification through search intensity*

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September 21, 2023

**Abstract**

We evaluate the effects of targeted credit injections of the central bank in the euro area. The aggregate policy impacts of credit easing on financial markets, bank lending and key macroeconomic variables are measured with a novel identification approach based on high-frequency web search data. Our results suggest that the targeted longer-term refinancing operations of the European Central Bank between 2014 and 2021 eased credit conditions in financial markets and had economically and statistically significant positive effects on GDP growth, bank lending and firm investment.

**JEL classification:** C36, E42, E51, E52, E58, G31
**Keywords:** Monetary policy, High-frequency identification, TLTRO, Bank lending

*We are grateful to Esa Jokivuolle, Eeva Kerola, Juha Kilponen, Jarmo Kontulainen, Markku Lehmus, Aino Silvo, Lauri Vilmi and the seminar participants at the Bank of Finland and at the University of Helsinki for their useful comments. We also thank Matias Pihlajamaa and Taina Sinivuori for their valuable assistance with the data. We thank Greg Moore for revising the language of the manuscript. The paper represents the views of the authors and are not necessarily those of the Bank of Finland. Any remaining errors are our own.

# 1 Introduction

With interest rates close to their effective lower bound in the 2010s, central banks around the world responded with a variety of monetary policy measures to stabilise financial markets and stimulate their economies. Among other unconventional tools, credit easing policies were used to promote bank lending directly. The Bank of England (BoE) led this trend with its Funding for Lending Scheme in 2012. The European Central Bank (ECB) followed in 2014 with the first in a series of targeted longer-term refinancing operations (TLTROs).

In this study, we analyse the aggregate effects of targeted credit easing in the euro area. Central bank liquidity injections used in credit easing operations are typically designed to encourage banks to increase their lending to non-financial corporations. More attractive borrowing terms for the banks, for example, may be conditioned on achieving specific lending targets. An increase in loans to the non-financial sector would eventually lead to growth in economic activity. In the euro area with its bank-based financial system, the credit easing tools gained particular prominence since 2014. The TLTRO programme was an important balance sheet tool of the ECB, used before and in parallel with other unconventional monetary policy measures such as quantitative easing.[1]

Despite their obvious relevance in adjusting the monetary stance, surprisingly little is known about the aggregate effects of credit easing policies on the economy. This blind spot likely reflects the fact that identification of their causal effects is difficult. As no directly observable policy variable is available, it is not straightforward to disentangle credit easing surprises from other monetary policy events. Moreover, announcements concerning longer-term refinancing operations are typically delivered outside the context of scheduled central bank monetary policy meetings. The news is revealed during speeches or interviews of top central bank officials or originated from central bank press releases detailing refinancing operations.

We assess the effects of ECB credit easing policies on bank lending, firm investment, financial markets and the macroeconomy using a novel identification technique that gauges the impact of news on longer-term refinancing operations in the euro area. Specifically, we construct a measure that reflects changes in the expectations on credit easing policies, measured by the financing costs of the banks conditional on the intensity of public attention to the programmes. We find that the ECB's use of targeted longer-term refinancing operations between 2014 and 2021 lowered borrowing costs and narrowed credit spreads, as well as boosted loan volumes, inflation and economic growth.

A key ingredient of this study is a policy surprise variable that we use to solve the identification problem. The variable captures the intensity of public focus on TLTRO-related news extracted from Google search data. Our intuition is that market participants begin to search for information about an operation as soon as the relevant policy announcement is made. In addition, unexpected variations are reflected in financial market data that measure surprise changes in the relative financing costs of banks.

[1] For a recent review of the unconventional monetary policy tools introduced in the wake of the global financial crisis, see Bernanke (2020). The monetary policy instruments implemented by the ECB and their effects are extensively discussed in Rostagno et al. (2021).

The daily policy surprise indicator is derived as a combination of Google search intensity and credit spread movements. Specifically, the intensity functions as a means for selecting the variation in credit spread changes stemming from news about TLTRO adjustments. Each bank credit shock is thus observed as a change in the credit spreads of the banks at times when search intensity is high. No shock is recorded if search intensity or credit spread change is negligible.[2] For a validity check, we also document that the series we derive strongly correlates with the number of news items about the TLTROs collected from other sources.

Using our daily surprise index, we estimate the effects of credit easing policies in the euro area. First, the micro-level analysis reveals that banks lower their lending rates to non-financial corporations in response to surprise readjustments in the longer-term refinancing operations. This finding is in line with the existing studies using other identification techniques. Second, based on the financial market high-frequency data, a bank credit shock decreases various credit spreads, corporate bond yields and volatility, while also leading to higher market-based inflation and bank dividend expectations. We show that an alternative identification strategy based on explicit monetary policy announcements yields ambiguous results. Third, credit easing is measured to cause an increase in loan volumes, output and prices. Finally, our tentative analysis with firm-level investment data suggests that TLTROs broadly induce growth in investment.

The contribution of this paper is three-fold. First, we propose the derivation of a policy surprise indicator based on search intensity, which can be used for the estimation of causal effects as in the high frequency identification literature (Gertler and Karadi, 2015; Altavilla et al., 2019; Jarociński and Karadi, 2020). Combining this intensity measure with financial market data makes it possible to assess the aggregate effects of specific monetary policy instruments such as TLTROs.

Unlike earlier research focusing on changes in interest rates around the time of the regularly scheduled monetary policy meetings, we use Google search data as an objective means for choosing relevant dates for a particular policy instrument.[3] Besides catching variation from relevant monetary policy meetings only, our approach takes into account important announcements made between the meetings, potentially important for a specific monetary policy tool such as TLTRO. Whereas Altavilla et al. (2019) analyse the financial market reactions to conventional and unconventional policies that influence the yield curve, we pay attention to the TLTROs whose effects are unlikely to be fully observed in the risk-free market interest rates.

Second, we complement the microeconometric evidence on the implications of TLTROs in bank lending. According to earlier studies (Benetton and Fantino, 2021; Andreeva and García-Posada, 2021; Laine, 2021; Afonso and Sousa-Leite, 2020) that rely on bank-level instrumental variable estimation, targeted operations fostered bank lending and reduced lending rates.[4] Similar to our approach, Altavilla et al.

[2] In this sense, our view on the information flow is consistent with rational inattention (Sims, 2003). The proxy moves only when the markets pay attention to the policy event.

[3] While Google Trends data are widely used in forecasting studies (e.g. Choi and Varian 2012 and Fetzer et al. 2021), no previous study to our best knowledge has used such data in identification of macroeconomic shocks.

[4] Perdichizzi et al. (2023), however, estimate negative effects of TLTROs on Italian firms identified by geographic variation.

(2023) use high-frequency identification based on bank bond yields to estimate the effects of TLTROs on bank lending. Our policy surprise indicator helps confirm the positive average effects of TLTROs on bank lending. Additionally, we are able to show that the TLTROs also induced financial market reactions and had macroeconomic implications, potentially reinforced by the indirect effects not fully taken into account in the existing studies.

Finally, we provide evidence on the aggregate effects of targeted credit easing policies. In theory, non-targeted long-term credit easing programmes in an economy with a frictional banking sector (Gertler and Karadi, 2011) should incentivise banks to increase their lending to firms both directly by providing extra liquidity and indirectly through maturity extension (Cahn et al., 2017). In line with Cúrdia and Woodford (2011), we expect pure liquidity provision of the central bank to be inefficient. However, when financial markets are incomplete, central bank balance sheet policies aimed at influencing bank lending are effective at the zero lower bound of interest rates.

Empirical studies of the effects of (non-targeted) longer-term refinancing operations (LTROs) prior to 2015 include Cahn et al. (2017), Carpinelli and Crosignani (2021), Crosignani et al. (2020) and Darracq-Paries and De Santis (2015). These studies generally find that credit easing operations lower financing costs, increase bank lending and lead to higher economic growth, but can also induce banks to increase their domestic government bond holdings (Crosignani et al., 2020).

While the literature on aggregate effects is relatively thin, the targeting element of TLTROs should in principle reinforce incentives for extending bank lending. Ambler and Rumler (2019) assert that initial TLTRO announcements only affect nominal interest rates, not inflation expectations. With the dates selected for our intensity measure, however, we find an impact on inflation expectations. With respect to macroeconomic effects, Rostagno et al. (2021) identify a general bank lending rate shock.[5] The Bank of England's credit easing policies are analysed by Churm et al. (2021). Our findings suggest that TLTROs had a non-negligible impact on macroeconomic variables during our observation period.

The remainder of the paper proceeds as follows. Section 2 provides key information about credit easing policies in the euro area and about our identification strategy. In Section 3, we present our empirical results. The final section concludes.

# 2 Background and methodology

In this section, we provide an overview of credit easing policies in the euro area and propose an approach based on a proxy variable measuring policy news.

## 2.1 Credit easing policies in the euro area

Credit easing policies in the euro area were implemented with targeted longer-term refinancing operations (TLTROs), whereby the ECB offered long-term loans to credit

[5] In addition, Balfoussia and Gibson (2016) estimate the potential impact of TLTROs based on data prior to the implementation of the programme. Nelimarkka and Laine (2021) provide tentative evidence on the effects of TLTROs with a shock identified by a set of policy announcement dates combined with other identifying restrictions.

institutions on favourable terms in order to promote bank lending to the economy. The longer maturity of TLTROs compared to the ECB's regular main refinancing operations (MROs) meant that banks were less exposed to short-term liquidity risks.[6] The operations were targeted, i.e. the favourable terms of TLTROs were only applied to banks that demonstrated a sufficient level of lending to the non-financial sector.[7]

Table 1 summarises the key properties of the ECB's three series of TLTRO operations (I, II and III) implemented between 2014 and 2021. Each series included approximately quarterly implemented operations in which banks could borrow funds from the central bank according to the lending criteria. The central bank generally set the borrowing conditions in terms of the amount, rate and maturity of loans granted to a particular borrowing bank.

Banks were entitled to a borrowing allowance that depended on their total amount of outstanding loans to the non-financial corporations and households, excluding loans for house purchases.[8] The maximum borrowing allowance varied from operation to operation as shown in the fourth row of Table 1. The maximum amount was usually set to a specific share of the eligible loan stock of the bank and according to the bank-specific benchmarks. In TLTRO I, an increase in the borrowing allowance was permitted if the bank's lending targets were satisfied.[9] In the course of the programme, the share governing the amount of available loans was adjusted multiple times. During the third series of TLTROs in particular, the availability of loans was considerably extended. With the onset of the Covid-19 pandemic, the maximum amount banks could borrow was increased to as much as 55 percent of the stock of eligible loans.

Another feature of TLTROs was that banks were incentivised to lend by offering long-term central bank credit at a rate that was low compared to the MRO rate or rates available in the bond market. In the first round of the operation, the rate was set just above the MRO rate.[10] After the first TLTRO series, the interest rate was used as an additional device to reward extra lending. In TLTRO II, the initial interest rate set to the MRO rate could be lowered if a bank sufficiently increased its eligible net lending to non-financial corporatations and households. In the third series of operations, TLTRO III, the rate at which financial institutions could borrow was ultimately lowered to a level below the deposit facility (DF) rate, provided that borrowing banks met their lending targets.

Favourable borrowing conditions for banks further enhanced by extending loan maturity. Broadly speaking, the maturity of the loans alloted in the operations varied between two and four years. The maturity was further extented under later

[6] See also the discussion of the maturity extension channel by Carpinelli and Crosignani (2021).

[7] The programme that preceded TLTROs, longer-term refinancing operations (LTROs), imposed no such conditions on lending. LTROs featured relatively low rates, but were not targeted.

[8] The sufficient lending criterion varied slightly across operations. The bank-specific lending benchmark in TLTRO II, for example, was determined so that banks with positive eligible net lending in the 12-month period before January 2016 had their benchmark net lending set at zero. For banks with negative eligible net lending, the benchmark net lending was set to the level of their lending in the 12-month period preceding January 2016. See, for example, Laine (2021) for further details.

[9] In the later rounds of TLTRO I, bank-specific benchmarks were used to determine the loan amount. These benchmarks were based on earlier lending performance.

[10] The ECB subsequently lowered the rate to the MRO rate at its January 2015 meeting.

| | TLTRO | TLTRO II | TLTRO III |
|---|---|---|---|
| Implementation | • 8 operations between 9/2014 and 6/2016 | • Four operations between 6/2016 and 3/2017 | • Initial announcement: Seven operations between 9/2019 and 3/2021.<br>• Subsequent announcement: Three additional operations to be conducted between 6/2021 and 12/2021. |
| Interest rate | • Initial operation: MRO rate + 10 bp at the time of allotment.<br>• Subsequent operations: MRO rate only. | • MRO rate at the time of allotment.<br>• Possibility for lowered rate if eligible net lending sufficiently increased. | • Intial announcement: 10 basis points above the average MRO rate over the life of each operation (DF+10 bp if lending goal achieved).<br>• Announcement in 9/2019: MRO (or DF).<br>• Announcement in 3/2020: MRO-25 bp (or DF-25 bp).<br>• Announcement in 4/2020: MRO-50 bp (or DF-50 bp). |
| Maturity | • All operations mature in 9/2018. | • All operations with maturity of 4 years | • Initial announcement: Every operation carries a maturity of two years.<br>• Announcement in 9/2019: Maturity extended to three years. |
| Amount | • 9/2014 and 12/2014: Max. 7 percent of eligible loans in 4/2014.<br>• 2015-2016: Max. 3 x eligible net lending relative to bank-specific benchmark. | • Max. 30 percent of eligible loans in 1/2016, less any amount previously borrowed and still outstanding under the first two TLTRO operations in 2014. | • Initial announcement: Max. 30 percent of the stock of eligible loans as in 2/2019.<br>• Announcement in 3/2020: Max. 50 percent.<br>• As of March 2021: Max. 55 percent. |

Table 1: Technical details of ECB targeted longer-term refinancing operations

TLTRO programmes. For instance, when TLTRO II was announced in June 2016, the ECB offered a voluntary repayment possibility for the outstanding loans from the first TLTRO to be rolled over to the TLTRO II.

To summarise the above exposition, the conditions of longer-term refinancing operations could be adjusted in multiple dimensions such as in loan volumes, maturity, interest rates and lending targets. In addition, the programme details and rules were modified on numerous occasions.[11] Thus, just how accomodative the operations eventually turned out is difficult to quantify from central bank announcements or published technical details about the TLTRO operations.

Nevertheless, the operations can generally be seen as lowering financing costs through the actual credit operations as well as through market-based bond financing. The latter occurs through the two channels mentioned above. First, the financing position of the banks improve and the related risk premium decreases as they are less vulnerable to the short-term liquidity risk. Second, the targeting element of rewarding eligible lending gives banks strong incentive to finance through the central bank credit and thereby reduces demand for market-based funding. Hence, direct effects of TLTROs may be observed as changes in the bond rates banks face in financial markets.

### 2.2 Policy surprises in longer-term refinancing operations

Our aim is to analyse the overall effects of credit easing policies on macroeconomic and financial market variables. While microeconometric studies such as Andreeva and García-Posada (2021), Benetton and Fantino (2021), Laine (2021) and Afonso and Sousa-Leite (2020) have exploited exogenous variation stemming from the allocation rule of loans, the estimation of aggregate effects requires the use of an identification strategy based on policy surprises. The latter surprises should be related to changes in the expectations about the policy stance in longer-term refinancing operations.

To be valid, the policy surprise indicator (or interchangeably a proxy) should be exogenous and relevant. By relevance it is understood that the proxy is associated with the information flows concerning TLTRO operations. The indicator is expected to move only if the public pays attention to the longer-term refinancing operations. If exogenous, the indicator measures the news component of the programmes, i.e. the unexpected, non-systematic variation in the policy.

We measure the unexpected component of the bank's financial position by daily changes in the spread of the bank bond yields relative to all corporate bonds. As discussed in the previous subsection, credit easing through TLTROs causes banks to shift their borrowing to the central bank and reduce their market-based borrowing. The decrease in the borrowing costs leads to lower bank bond yields relative to other corporate bonds and a decline in spreads.[12]

As financial markets are constantly digesting new information, changes in the

[11] Further readjustments were made, for instance, in 27 October 2022, when the ECB decided to change the specific time span during which the accommodative interest rate was applied.

[12] Similar to us, Altavilla et al. (2023) use changes in the bank bond yields to assess the effectiveness of TLTROs. We exploit, instead, variation in the bank bond spread to focus on the relative financing costs and to control for other monetary policy measures affecting the overall credit conditions.

relative financial position of banks are priced into bonds. Of course, the bank bond spread may also change on news unrelated to longer-term refinancing operations. Thus, a variable to satisfy the relevance criterion requires narrowing the set of daily changes to TLTRO-related readjustments. A general approach would be to choose only those dates on which the ECB's Governing Council held a scheduled monetary policy meeting and released a policy statement after its meeting. As a caveat to this strategy, the obtained series would likely be driven by other policy announcements unrelated to longer-term refinancing operations.

An alternative option is to consider variation only on days of TLTRO-related announcements, an approach taken e.g. by Altavilla et al. (2023). Although straightforward, the choice of these dates and the length of the monitoring window inherently involves subjective assessment. Moreover, important announcements can occur between actual policy statements.[13]

To overcome this, we extract the relevant unexpected variation in the daily changes of the bond spread by weighting the series according to the intensity with which the public pays attention to longer-term refinancing operations. If the intensity is low, the reactions of financial markets are likely unrelated to news about refinancing operations. Conversely, if the intensity is high, the movements of the bank bond spread are likely driven by the TLTRO announcement.

Implicitly, our indicator also takes the effectiveness of the TLTRO programmes into account. If the TLTROs enhance the liquidity position of banks apart from the other available monetary policy instruments, bond spreads are expected to change. If the programmes are seen by the financial markets as redundant due to the availability of liquidity from other channels, no change in the relative financing position of banks should be observed on days when the intensity is otherwise high.

### 2.2.1 Google-based TLTRO search intensity

To measure intensity, we collect Google search volumes around the subject longer-term refinancing operations. Our identification approach assumes that a certain mass of market participants begin searching the internet for details if the focus of the financial markets is on longer-term refinancing operations.

We use the Google Trends tool to collect the number of search requests over time. Google Trends provides unfiltered data samples on search items or topics submitted to the Google search engine (see also, Eichenauer et al. 2022). The Google Trends data are based on a representative sample of all searches made during the period (Google, 2023). The raw data are given as an index based on a topic's share of all searches on all topics. By Google algorithms, the available data exclude searches involving only a few people or duplicate searches.[14]

[13]Information about internal discussions within the ECB flows continuously into the public sphere, and markets react when they consider the information significant. Central bank officials also divulge their policy perspectives in speeches and interviews between monetary policy meetings. For instance, TLTRO-type monetary policy tools were already being mentioned in spring 2014. Similarly, speculation about an the impending TLTRO rate cut in January 2015 was already widespread among market participants in August 2014. Rumours about TLTRO III were rife ahead of the announcement of the programme.

[14]Google Trends data may also contain some automated searches. Google reports that some, but not all, of these irregular activities have been filtered out of the provided data. In any case, our assessment is that the distortion induced by this activity is negligible as monetary policy topics are

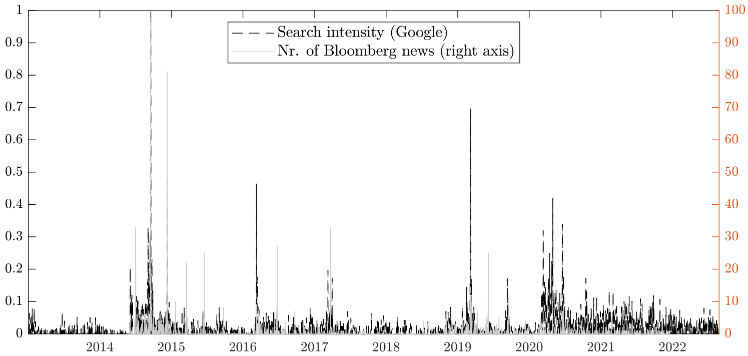



Figure 1: Google Trends TLTRO search intensity (black dashed line) and the number of Bloomberg news on TLTRO (grey solid line).

We collect from the Google Trends daily time series on search volumes based on items "LTRO" and "TLTRO", the widely used acronyms for longer-term refinancing operations in the euro area.[15] We also consider the (non-targeted) longer-term refinancing operations (LTRO), as TLTRO may have not yet been an established term in the initial sample.[16] In short, the daily index is an aggregated series computed from overlapping samples of search volumes, scaled by the monthly search volume to capture longer-term variation in the intensity. Series construction is outlined in Appendix A.1.

Figure 1 shows the constructed daily search intensity scaled on an interval $[0, 1]$ in the black dashed line. For cross-checking, we also collect from the Bloomberg terminal the daily number of headlines that contain the word "TLTRO", drawn in the figure with a grey solid line.

According to Figure 1, the intensity series has several spikes, interpreted as dates the focus of the financial markets has been on the TLTROs. The search intensity broadly aligns with the number of Bloomberg headlines. The first series of TLTROs was announced on 5 June 2014, the second on 10 March 2016 and the third on 7 March 2019. The corresponding intensities on these dates were 0.21, 0.46 and 0.70, respectively. In both measures, the largest spikes occur around these events. In addition, large variation exists in 2020 after the onset of the Covid-19 crisis when readjustments to the TLTRO conditions were introduced.

For a more in-depth analysis, Table 2 lists the 20 largest occurrences of the search intensity, along with the number of Bloomberg headlines on the same day. We subjectively assess the day's headline from the Bloomberg terminal that best captures the TLTRO-related news of the day. In practice, the Bloomberg headlines are actively followed by the financial market participants and thus inform us about

unlikely to be targeted by such activities.

[15] The use of full names yields similar results.

[16] While TLTRO typically refers to the ECB's programme, the Reserve Bank of India (RBI) also introduced its Targeted Long Term Repo Operation (TLTRO) in 2020 in response to the economic disruption of the Covid-19 pandemic. Hence, part of the global Google searches from 2020 onwards may be due to the programme of the RBI. Most notably, Google searches intensify around 17 April 2020 when the RBI released data on its fourth TLTRO. We tackle the issue by setting the search volume to 0 on dates there are headlines in the Bloomberg terminal about TLTROs of the RBI.

| Date | $I_t$ | $NR_t$ | $\Delta x_t$ | Bloomberg headline |
|---|---|---|---|---|
| 18 Sep 2014 | 1.0 | 98 | -1.80 | *ECB Provides EU82.6 BLN in TLTRO; Est. EU100 BLN to EU300 BLN |
| 19 Sep 2014 | 0.70 | 9 | -0.24 | European Banks Earnings Impact From TLTRO Is Low, Barclays Says |
| 7 Mar 2019* | 0.70 | 12 | -1.12 | European Banking Stocks Sink as TLTRO Details Disappoint |
| 11 Dec 2014 | 0.48 | 81 | 0.21 | REACT: ECB Sovereign QE Odds Rising as 2nd TLTRO Disappoints (1) |
| 10 Mar 2016* | 0.46 | 9 | 0.56 | *Draghi Says It Will Issue Four New 4-Year TLTRO Programs |
| 30 Apr 2020* | 0.42 | 5 | -2.17 | *ECB Says TLTRO Conditions Further Eased |
| 8 Mar 2019 | 0.36 | 4 | 0.45 | *MERSCH Says TLTRO Details Will Come When ECB Is Ready |
| 18 Jun 2020 | 0.34 | 7 | 2.44 | *ECB Allots EU1.31T in TLTRO Offer |
| 4 Sep 2014* | 0.33 | 7 | 0.1 | *DRAGHI Says ECB Cut Rates to Encourage TLTRO Takeup |
| 5 Sep 2014 | 0.33 | 3 | -0.74 | EU RATES OUTLOOK: Digesting ECB's Over-Delivery; TLTRO Countdown |
| 12 Mar 2020* | 0.32 | 7 | 9.49 | *ECB: TLTRO III to Have More Favorable Term June 2020-June 2021 |
| 15 Sep 2014 | 0.30 | 8 | -0.51 | PREVIEW: Size of ECB QE Depends on This Week's TLTRO |
| 12 Dec 2014 | 0.29 | 5 | 0.95 | EU MONEY MARKETS: 3Y LTRO Repayment in Focus After TLTRO Take-Up |
| 6 Mar 2019 | 0.25 | 1 | 0.43 | ECB TLTRO Already Partly Priced in by Markets: Credit Agricole |
| 14 Apr 2020 | 0.25 | 1 | -1.92 | Lagarde Needs TLTRO Program to Work |
| 17 Sep 2014 | 0.24 | 19 | -0.60 | TLTRO PREVIEW UPDATE: Underwhelming Takeup Would Boost QE Hopes |
| 13 Mar 2020 | 0.24 | 0 | 6.43 | – |
| 24 Sep 2014 | 0.23 | 0 | 0.52 | – |
| 25 Apr 2020 | 0.22 | 0 | | – |
| 11 Mar 2016 | 0.22 | 1 | -4.18 | TLTRO Likely Helpful for Europe Banks, Periphery First: Barclays |

Table 2: Largest search intensities ($I_t$), number of Bloomberg headlines ($NR_t$) and changes in the bank bond spread ($\Delta x_t$)

$I_t$ refers to the standardised Google Trends search volume, $NR_t$ to the number of Bloomberg headlines and $\Delta x_t$ to the daily change between the yields of euro area bank bonds and 5-year AAA corporate bonds. ECB monetary policy meeting were held on dates marked with an asterisk (*).

the focus of the markets.[17]

Table 2 and the reported Bloomberg headlines suggest that highest intensities align with TLTRO-related events. The dates of the highest intensities fall roughly into two categories. The first category includes dates on which actual decisions concerning TLTROs were taken. To these belong the meeting days of the ECB Governing Council (marked with an asterisk) on which a new series of TLTROs (7 March 2019 and 10 March 2016) were introduced, conditions of the existing programmes were eased (30 April 2020 and 12 March 2020) or policy rates with effects on the TLTROs were adjusted (4 September 2014). High intensities also occur on the dates 11 March 2016, 5 September 2014 and 8 March 2019 following the announcement, decisions digested by the financial markets or clarified in the interviews and speeches by the governors of the national central banks. Markets may anticipate a decision as reflected in a high value of intensity of 6 March 2019 just before the Governing Council meeting.

In the second category, high intensity is associated with the release of data on the TLTRO take-up according to the number of Bloomberg headlines. Most notably, the intensity series peaks on 18 September 2014, when the ECB alloted loans in its first TLTRO operation and published information on the take-up. Similar high

[17] Another option would be to use the number of Bloomberg news items as a measure of intensity. While transparent, the validity of this measure would hinge upon the number of headlines published on the Bloomberg terminal which may be a noisy measure of intensity. The number of headlines about TLTROs may, for instance, significantly depend on the market stress or other events of the same day. In addition, the Google-based measure is more demand-driven and reflects the attention the public pays to the ECB's credit easing policies. In this sense, the series takes rational inattention into account: the public is only seen to be revising its TLTRO expectations when search intensity is high.

intensity is seen on 11 December 2014 and 18 June 2020. On dates preceding the allotment day (15 September 2014 and 17 September 2014) and dates following the allotment day (19 September 2014 and 12 December 2014), search volumes are large. In general, it seems that the financial markets update their expectations about about the extent and effectiveness of the programme, especially when the programme was new in 2014 and uncertainty was high in 2020 at the start of the pandemic.[18]

As the above categorisation suggests, the large intensity values may be identified as TLTRO-related events. To measure market reaction, we report in the fourth column of Table 2 the daily change in the bank bond spread ($\Delta x_t$). The latter is measured as the difference between the euro area average bank bond yield and the 5-year rate for AAA-rated corporate bonds and captures the high-frequency changes in the relative financing costs of banks.[19] For the first three largest intensity values, the financial markets assess a decrease in financing costs. In general, the change in the bank bond spread determines whether the TLTRO news is more positive or negative than expected.

### 2.2.2 TLTRO policy surprise indicator

Finally, the policy surprise indicator is a product of the two variables, the search intensity scaled to an interval $[0, 1]$, $I_t$ and the daily change in the bank bond spread, $\Delta x_t$. That is,

$$m_t = I_t \Delta x_t. \tag{1}$$

Hence, the indicator measures surprise changes in the relative financial position of banks conditioned on the public attention the TLTRO generates.

It is useful to consider two bordering cases of how the series reacts to TLTRO readjustments. With maximal intensity of 1, the indicator value coincides with the daily bond spread change, implying that all variation of the variable stems from the TLTRO-related event of the day. With a small weight, the policy indicator incorporates only a minimal fraction of the change in the spread. The latter situation arises if movement in the bond spread is due to factors other than surprises related to long-term refinancing operations.[20]

Figure 2 plots the policy indicator, expressed as percentage change, multiplied by search intensity. The series obtains both positive and negative values over the sample, with the large values concentrated in episodes when new TLTROs were introduced in 2014, 2016 and 2019, listed in Table 2. At the introduction of new programmes, we see significant surprise decreases in the spread. These dates mark the first TLTRO allotment (18 September 2014) and new ECB policy decisions (11 March 2016 and 7 March 2019).[21]

[18] The additional, relatively negligible, category includes four dates that we are unable to directly link to the TLTRO-related events. Three of these appear amidst the financial turmoil induced by the Covid-19 pandemic. For 24 September 2014, no specific event can be identified.

[19] The bank bond yield is obtained from Bloomberg. The 5-year corporate AAA-bond rate for the euro area is constructed by Macrobond.

[20] In a typical event-study approach such as Altavilla et al. (2023), $I_t$ would be equal to 1 on policy announcement dates and 0 otherwise.

[21] During the first months of the Covid-19 pandemic in 2020, the daily swings in bond yields were extraordinarily large, observed as large variation in the series. As they have likely been driven by events unrelated to the TLTROs, we disregard these observations from our later analysis.

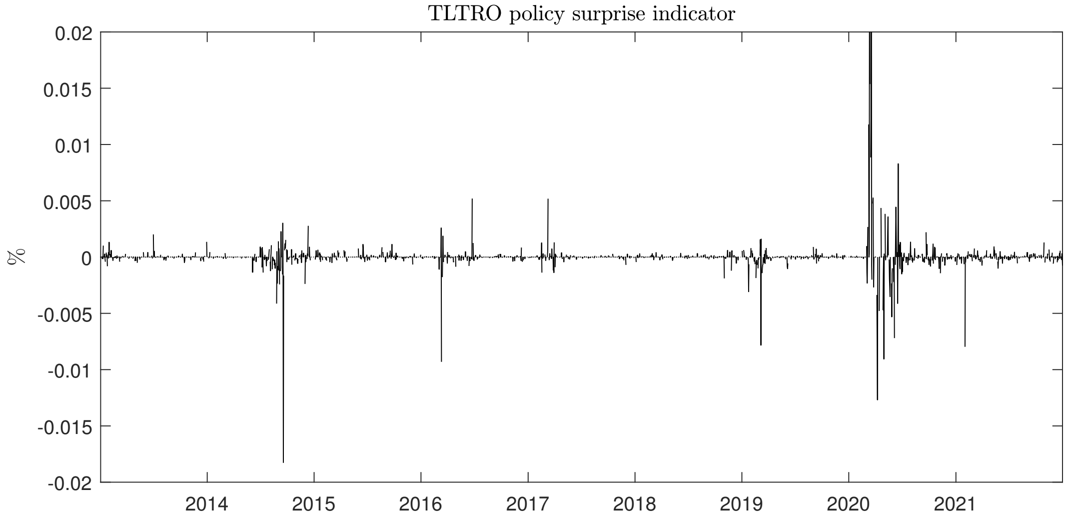



Figure 2: The TLTRO policy surprise indicator.
Daily changes in the bank bond spread (in percentage points) multiplied by the search intensity for LTROs and TLTROs. The peaks of observations on 12 March 2020 (+0.030) and 18 March 2020 (+0.054) have been cut off at the top.

It should be emphasised that the policy surprise indicator tracks unexpected variation in the TLTRO adjustments. Constructed from the daily changes in the bank bond spreads, it inherently captures short-term revisions in the expectations about bank financial positions. A positive value of the indicator implies that a surprise tightening in the lending conditions has occured. In contrast, a negative value is associated with TLTRO-induced surprise credit easing. The unexpected character of the indicator facilitates the measurement of causal effects of these operations.

# 3 Results

The following discussion lays out our estimates, based on the derived policy indicator, as to the implications of longer-term refinancing operations. Using bank-level data, we first show that the estimated effects on lending rates derived with the policy surprise indicator coincide with those obtained by other methods. We then present results for financial market reactions based on the daily data. We next use monthly macroeconomic data to derive the aggregate effects of TLTRO programmes on prices, output and loan volumes. In the final subsection, we discuss the evidence from the perspective of firm-level investment reactions.

## 3.1 Initial analysis and placebo test: the effect of credit easing surprises on bank lending rates and credit standards

We start by briefly investigating how credit easing surprises affect the lending behaviour of banks, attempting to verify whether our approach based on the policy surprise indicator yields results similar to the existing literature on the effects of TLTROs (see, e.g. Benetton and Fantino, 2021, Andreeva and García-Posada, 2021 and Altavilla et al., 2023). Earlier studies suggest that targeted longer-term refinancing operations promote bank lending by lowering lending rates. The effects in these studies are estimated by exploiting the allocation rule on how credit is allocated via TLTROs to the banks (see Benetton and Fantino 2021 for details).

Instead, we estimate the bank lending implications with the use of our policy surpise indicator. We first analyse the effects on lending rates using the euro area bank-level data, and then investigate how TLTRO surprises affect credit standards. As long as both the allocation rule and our policy surpise indicator are valid instruments, we expect the estimated effects of credit easing to coincide, irrespective of the identification technique.

For the derivation of the bank lending rate response, we use monthly panel data of euro area banks.[22] The effects are estimated by applying the local projection approach (Jordà, 2005) to the bank-level data, similar to, among others, Boeckx et al. (2020).

The identification is based on exogeneity and relevance of our policy surprise indicator as well as on the recursiveness assumption (Christiano et al., 1999). According to the latter, the lending rate is assumed not to react on impact to a TLTRO surprise. Instead, the readjustment of banks to the new TLTRO conditions takes at least a month. The lag may emerge due to the implementation lag of the new TLTRO conditions or due to the fact that new loan decisions are not taken instantly.

Specifically, we estimate a local projection regression

$$Z_{i,t+h} - Z_{i,t} = a_{i,h} + b_h m_t + e_{i,t}, h = 0, 1 \ldots, H, \tag{2}$$

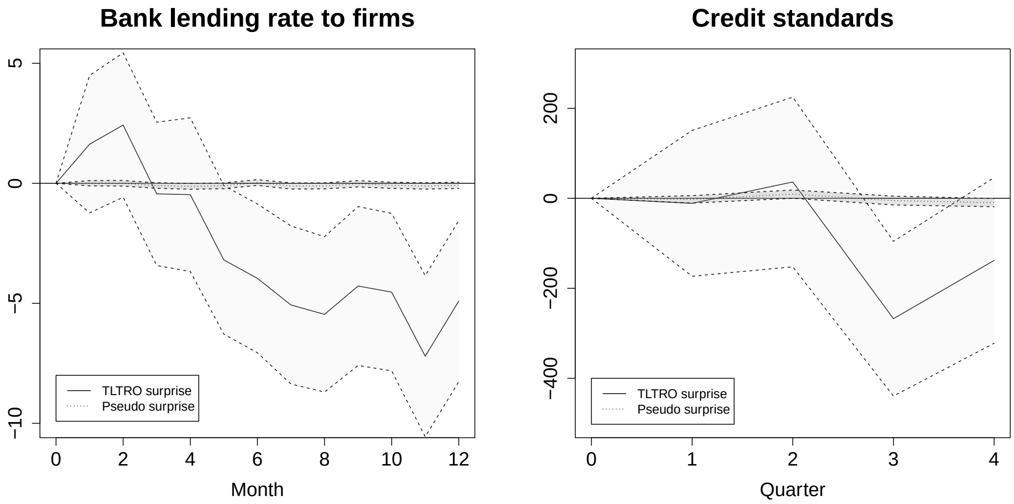
where $Z_{i,t+h}$ is the lending rate of the bank $i$ to new loans at horizon $h$ after a shock, $a_{i,h}$ is bank $i$'s fixed effects and $m_t = I_t \Delta x_t$ is the TLTRO policy surprise indicator derived in the previous subsection. [23] We use data starting from 2014, when targeted operations were first announced. Given that our series of policy surprises is exogenous and relevant, we are able to recover the average causal effects of TLTROs on bank lending rates.

Panel (a) of Figure 3 presents the estimated bank lending rate response to a unit increase in the policy surprise proxy, including 90-percent confidence intervals. The estimated impulse responses in the face of unexpected TLTRO easing are qualitatively similar to those estimated, for example, by Benetton and Fantino (2021), whose results are based on using the allocation rule as an instrumental variable. Their results show that banks subject to the treatment by the allocation rule reduced their lending rates to firms. Our estimates show that on average banks decrease their lending rates in the wake of a credit easing surprise.

Our estimates suggest that a one-basis-point TLTRO-induced bank bond yield decline lowers rate at which banks lend to the firms by approximately five basis points. The effect on the lending rate fully materialises within one year. The estimated effects are in the same ballpark with the earlier research based on different

[22]The dataset, also used by Fungáčová et al. (2023), is an unbalanced panel of 137 banks and covers periods 2010:1–2020:12. The data are confidential and compiled mainly from the ECB's iMIR, iBSI and iBLS datasets. A detailed description of the data is given in Fungáčová et al. (2023).

[23]The coefficients are estimated by ordinary least squares. Typical for the panel local projection approach, we use robust standard errors of Driscoll and Kraay (1998) to account for serial correlation and heteroscedasticity in the error term $e_{i,t}$.




(a) (b)

Figure 3: The effects of a credit easing surprise on bank lending behaviour.
Panel (a): The bank-level response of the lending rate, estimated from regression (2). 90 percent confidence intervals based on Driscoll and Kraay (1998) reported in dashed lines. Panel (b): Estimated local projection impulse response function of credit standard measure (in net percentages) from the ECB's Bank Lending Survey. 90-percent Newey-West-based confidence intervals in dashed lines.

identification techniques.[24]

Next, we analyse how credit easing surprises affect credit standards, i.e. the criteria on which banks approve loans to their customers. Andreeva and García-Posada (2021) provide evidence that credit easing policies transmit both to lending rates and credit standards. Similarly, we estimate the response of credit standards to a decrease in our policy surprise indicator. The aggregate credit standards variable is obtained from the quarterly ECB's Bank Lending Survey. It is a net percentage computed as the difference between the share of banks reporting tightening in the credit standards applied in loan approvals and banks reporting easing in their lending standards.

The change of credit standards in response to a credit easing surprise is depicted in panel (b) of Figure 3, estimated from a standard local projection regression of type (2) with the euro-area-level measure for credit standards as the dependent variable.[25] Credit easing clearly leads to looser credit standards. A one-basis-point decrease in the bank bond spread due to the TLTRO news reduces the net percentage of banks tightening credit standards by 2 percentage points, with the effect realising three quarters after the shock.

As a final check to assess the validity of our proxy variable, we run the following placebo test for the above regressions. First, we define a pseudo intensity variable

[24]The results are also robust to including bank-specific or macroeconomic control variables, see Appendix A.2. Although not shown, dropping the recursiveness assumption does not considerably change the results.

[25]As the estimation is performed at the aggregate level, fixed effects are dropped. The credit standards variable is observed at quarterly frequency, so we aggregate our proxy variable by taking the quarterly sum. The data are quarterly and cover the sample from Q1/2014 to Q4/2021.

as $\tilde{I}_t = 1 - I_t$ that gives the highest weights on dates the TLTRO intensity is measured to be the lowest. In contrast, the inverse intensity $\tilde{I}_t$ is the lowest on dates the original intensity $I_t$ is highest. Second, we multiply the pseudo intensity $\tilde{I}_t$ by the bank bond spread $\Delta x_t$ to generate a new placebo variable for the TLTRO shocks, $\tilde{m}_t = \tilde{I}_t \Delta x_t$. Third, we replicate the above local projection regressions for the lending rate and credit standards using the pseudo proxy $\tilde{m}_t$.

The dotted lines in panels (a) and (b) of Figure 3 plot the estimates to a unit increase in the pseudo variable. The responses with respect to a change in the pseudo proxy are virtually zero, reflecting the fact that the elimination of TLTRO-related dates from the bond spread variation induces the effects on bank lending activity to vanish. Consequently, our intensity measure appears to identify relevant bond yield changes as it passes through the constructed placebo test.

### 3.2 The financial market effects of longer-term refinancing operations

In this subsection, we estimate the effects of longer-term refinancing operations on financial market variables by deriving the impulse responses based on the proxy variable. The impulse responses are obtained from the vector autoregressive (VAR) model on $N$ variables in vector $y_t$:

$$y_t = c + \sum_{i=1}^{p} A_i y_{t-i} + u_t, \tag{3}$$

where $c$ is an $N$-dimensional constant parameter vector and $\{A_i\}_{i=1}^{p}$ are coefficient matrices of autoregressive parameters of dimension $N \times N$. $u_t$ is an $N$-dimensional error term with zero mean and covariance matrix $\Sigma$.

As standard in the structural VAR (SVAR) literature, the reduced-form error term $u_t$ is linearly related to the structural shocks:

$$u_t = B\varepsilon_t, \tag{4}$$

where $B$ is an $(N \times N)$ impact matrix and $\varepsilon_t$ an $N$-dimensional vector containing the structural shocks such that $BB' = \Sigma$. As our only interest here is in the TLTRO policy shock, we only need to identify the first column of $B$ which is related to the first element of $\varepsilon_t$.

We identify the bank credit shock by standard Cholesky identification, ordering the TLTRO policy surprise indicator $m_t$ as the first variable in $y_t$. In other words, the structural shock is the orthogonalised innovation to the policy surprise variable. This exclusion restriction implies that no other shock affects the policy surprise variable within a single day.

Using a simple Cholesky decomposition – in contrast to derivations of impulse responses from the proxy SVAR (Mertens and Ravn, 2013) or from local projections (Jordà, 2005) – allows us to produce efficient estimates while simultaneously tackling potential non-invertibility. The latter is a legitmate concern as TLTRO announcements can be characterised as policy news that realises with a lag as banks decide after the announcement whether they will participate in the programme. This decision lag, in turn, may cause a non-invertibility issue, i.e. the econometrician has less information than the economic agents (Leeper et al., 2013). As discussed in

Plagborg-Møller and Wolf (2021), the estimates from the VAR with the Cholesky decomposition coincide in large samples with those obtained from the local projections. In contrast to results obtained from the proxy SVAR, they are robust to non-invertibility.

The use of a VAR model also controls for potential autocorrelation and endogeneity. Bond yields and their spreads may well be autocorrelated over time. This implies that our policy surprise indicator is not necessarily serially uncorrelated. In addition, the policy indicator may depend on different types of risk premia and can partly be forecast by the lagged financial market variables. With the inclusion of a large set of financial variables and risk indicators to the VAR model, we estimate reactions to the unexpected and serially uncorrelated shock component of the policy indicator.

The following euro area variables are included to the VAR model, in addition to the policy surprise proxy. With the spread between the bank bond and AAA-rated corporate bond yields, the relative credit conditions of the financial institutions are gauged. The general level of interest rates and monetary policy are controlled for by the yield on the AAA-rated corporate bonds as well as by the 1-year, 2-year and 10-year overnight index swap (OIS) rates.

We measure the general credit conditions by the interest rate spread between the BBB and AAA-rated corporate bonds and the risk in the sovereign bond market by the spread of the 10-year yield between Italian and German bonds. Stock market movements are taken into account with the Euro Stoxx 50 and its implied volatility (VSTOXX), as well as with the Euro Stoxx Banks index.

In addition, the model is augmented with market expectation measures. We include the 1-year-after-a-year (1y1y) inflation forward rate, calculated from the inflation swaps, as well as analysts' 1-year-ahead bank dividend expectations. We use Citi's surprise index to capture changes in expectations regarding incoming economic data.[26]

The VAR model is estimated by least squares (LS). We base our results on the data from January 2014 until the end of 2021. We omit the earlier sample as any TLTRO searches were unlikely before the announcement of the first operations and as the intensity series is driven by LTRO-related searches (See Appendix A.1 for details). Thus, our results are estimated from the period dominated by targeted longer-term refinancing operations. In addition, we omit in the estimation the observations of March 2020, the period when the Covid-19-related financial market volatility was exceptionally large. Finally, the lag length is chosen by the Akaike information criterion and the pointwise confidence intervals are obtained from residual bootstrapping.

Figure 4 shows the impulse responses of the financial market variables to a one-standard-deviation credit easing shock in solid lines. We additionally report in dashed lines the estimated impulse responses from an alternative specification, where recursiveness is assumed. The latter imposes an assumption that the 1-year risk-free interest does not react on impact to a TLTRO surprise. TLTRO surprises are then

[26]Inflation swap rates and OIS rates are obtained from Bloomberg. The 1-year-ahead dividend forecast is based on analysts' consensus forecasts that are aggregated at the index level by Bloomberg (See Laine 2023a for details). Corporate and sovereign bond yields, as well as Citi's surprise index and stock price indices, are obtained from Macrobond.

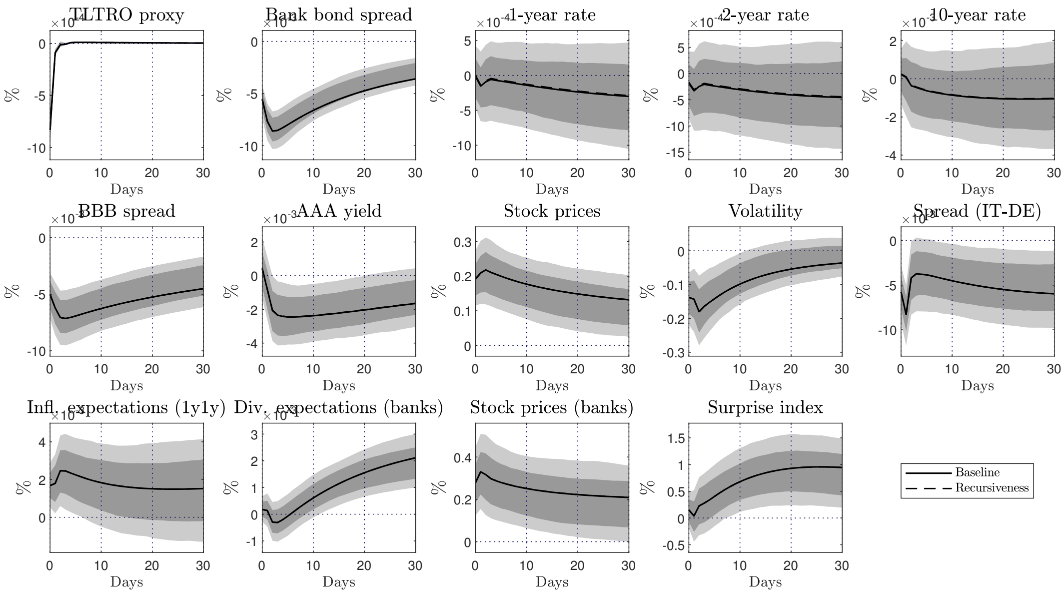


Figure 4: Estimated impulse responses to a credit easing shock.
The solid line depicts the least squares estimate of the impulse responses to the bank lending shock, derived from the daily VAR with the shock identified by the Cholesky decomposition with the TLTRO proxy ordered first. The dashed lines depict the impulse responses identified by the recursiveness assumption with the OIS rates ordered before the proxy. The light-shaded and dark-shaded regions border, respectively, the 90 and 68 percent pointwise confidence intervals of the baseline specification. Lag length $p = 2$ is chosen using the Akaike information criterion.

assumed to be contemporaneously independent of interest rate changes that could be caused by other monetary policy measures.

In general, the results suggest that TLTROs have their desired macroeconomic effects of easing overall credit conditions by lowering interest rates and suppressing risk premia. The shock decreases bank bond yields more than other corporate bond yields, which is reflected in the declining bank bond spread. The effect is expected as TLTROs should a priori lower bank funding costs. Moreover, the shock generally depresses risk premia. Corporate bond yields (especially the yields of riskier BBB-rated bonds) decrease.

Andreeva and García-Posada (2021) show that the overall macroeconomic effect of TLTROs on bank lending is theoretically ambiguous since the response of non-participating banks to the loan supply is unclear. Our results indicate that TLTROs lower average financing costs observed as lower corporate bond yields. This could be the consequence of direct effects, indirect effects or both. Directly, banks obtain central bank credit by lower costs with incentives to increase lending, and the bank competition induces lower borrowing costs for the firms. Indirectly, the decrease in the borrowing costs of the banks and firms generally lowers bond yield rates in the financial markets.

TLTRO-related credit easing also decreases the spread between Italian and German 10-year bond yields. This observation is consistent with the results of Crosignani et al. (2020) for non-targeted LTROs. They find evidence with respect to the collateral trade, whereby banks hoard government bonds to use as collateral for obtaining central bank liquidity. On the other hand, the bank-level-analyses of Benetton and Fantino (2021), Laine (2021) and de Haan et al. (2021) find no effect on the sovereign bond holdings of banks after a TLTRO take-up. The latter studies, however, do not consider the indirect effects by which the TLTROs decrease overall risk premia and thereby the sovereign bond spreads.[27]

An important observation is that a credit easing shock leaves the risk-free euro area yield curve intact, seen as statistically insignificant reactions of the 1-year, 2-year and 10-year OIS rates. In addition, as the dashed lines of Figure 4 show, assuming a zero initial reaction of the short-term interest rate implies estimates that are nearly indistinguishable from those of the baseline specification. Hence, the credit easing shock is unlikely related to surprise changes in the ECB's policy rate or other central bank measures influencing the yield curve. [28]

A bank credit shock induces reactions in stock market indices and stock market expectations. In response to credit easing, stock prices increase and stock market volatility decreases. The signs of these reactions suggest that no signalling effect (Nakamura and Steinsson, 2018; Jarociński and Karadi, 2020) exists.

Following the shock, dividend expectations gradually start to rise, consistent with increasing bank profits from favourable bank lending. With similar dynamics, the shock leads to positive surprises in economic indicators. Importantly, credit easing policies increase inflation expectations on impact, suggesting that the public expects the operations to have macroeconomic implications by creating price pressures.[29]

The baseline results of Figure 4 are robust to several alternative specifications, increasing our confidence that the constructed policy surprise indicator is a valid instrument for the measurement of causal effects. The impulse responses derived from these alternative specifications are shown in Appendix A.3. First, we eliminate the possibility that the occurrence of other monetary policy surprises would be driving the results. The estimates remain intact when the surprises are orthogonalised with respect to risk-free rate changes around the regular monetary policy meetings of the Governing Council as taken from the Euro Area Monetary Policy Event-Study Database of Altavilla et al. (2019). Hence, the shocks derived from our policy indicator seem unrelated to other surprise monetary policy measures.

Second, the measurement error or model misspecification are unlikely to be a concern in the estimation of the impulse responses. Impulse responses obtained using local projections or proxy SVAR, as well as from a VAR with longer lag length, broadly coincide with the baseline specification. Our results also do not significantly change when a more general heteroskedasticity-based identification is employed, instead of using the policy surprise indicator. As shown in Appendix A.3,

[27]In policy decisions prior to 2013, Kilponen et al. (2015) find insignificant reactions of sovereign bond spreads in response to the ECB's decisions to support liquidity through LTRO programmes.

[28]Long-term central bank funding potentially has complementary implications for the yield curve. While the shape and level of the yield curve is mainly governed by other monetary policies, TLTROs may change the expectations about the future path of short-term policy rates. The effects on the yield curve may be related, for example, to reinforcing expectations about future interest rate cuts when the short-term interest rate is at its effective lower bound. While the baseline specification does not support evidence on these complementarities, a more significant lagged decline of the risk-free interest rates may be seen in alternative specifications shown in Appendix A.3.

[29]In contrast, Ambler and Rumler (2019) find that the first TLTRO announcements in 2015 did not heighten inflation expectations. Our measure, however, covers a larger set of TLTRO news items.

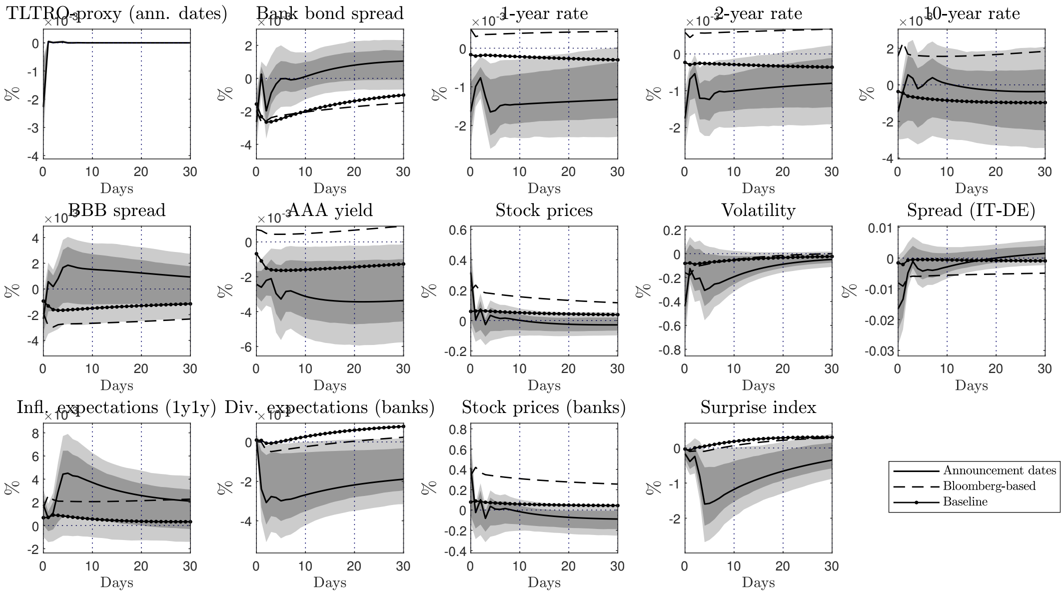



Figure 5: Estimated impulse responses based on alternative policy surprise variables. The solid line depicts the least squares estimate of the impulse responses to a one-standard-deviation bank lending shock, estimated from the daily VAR(4) with the shock identified by the Cholesky decomposition with the announcement-date-based TLTRO proxy ordered first. The dashed lines depict the impulse responses derived from the VAR(2) that includes the Bloomberg-based policy surprise indicator. The marked solid lines depict the baseline results. The light-shaded and dark-shaded regions border, respectively, the 90 and 68 percent pointwise confidence intervals of the announcement-date-based specification. The lag lengths are chosen using the Akaike information criterion. The baseline and Bloomberg-based estimates are rescaled such that the maximum impact on bank bond spread aligns with the announcement-date-based estimate.

identifying the shock by restriction in which the variance of the bank credit shock is assumed to change on days listed in Table 2 produces results similar to the baseline specification.

Third, after applying different measures for financing costs or intensity, our conclusions remain the same. Replacing the change in the bank bond spread in $\Delta m_t$ with a stock-price-index-based measure implies virtually the same impulse responses for the variables. Similarly, changing the intensity measure $I_t$ to the number of Bloomberg headlines shown in Figure 1 scaled to an interval $[0, 1]$, the estimated impact of the credit easing shock is estimated to be broadly similar as reported in Figure 5 with dashed lines.

Instead, as the solid lines in Figure 5 depict, only including those changes in the bond spread that occur on TLTRO announcement dates used by Altavilla et al. (2023) leads to counterfactual conclusions about the propagation of the credit easing shock. Most estimates become statistically insignificant. Variables such as inflation expectations and bank bond spread are estimated to have lagged responses, which contrasts with the view that financial markets immediately absorb new information. We therefore argue that including the variations that occur between TLTRO announcements matters in estimation of the effects.

### 3.3 Macroeconomic effects

According to the results presented in the above subsections, our policy surprise variable produces bank-level and financial market data results that are consistent with economic theory. After completing these validity checks, we turn to analysis of the macroeconomic effects of TLTROs.

We proceed by estimating a structural VAR model with the following monthly data. Economic activity is measured by real Gross Domestic Product (GDP) interpolated to monthly frequency by industrial production using the method of Chow and Lin (1971). The euro area harmonised consumer price index (HICP) and the stock of loans to the non-financial corporations (NFCs) capture price dynamics and lending behaviour of banks, respectively. The three latter variables are expressed in logs. We also include the lending rate of new loans to NFC and the corporate bond spread (BBB-AAA) to control for credit conditions.[30]

The credit easing shock is identified as a change in the TLTRO policy surprise indicator that is included to the model.[31]. We additionally impose the recursive identification scheme by which we assume that prices, output and loan stock respond to a credit policy surprise with a lag. As borrowing and lending decisions take time, it is reasonable to assume recursiveness. This also simultaneously tackles potential endogenous monetary policy responses. The model is estimated using least squares.[32] We use data from 2014 onwards with observations from year 2013 used as a presample.[33]

Figure 6 shows the estimated impulse responses to a one-standard-deviation credit easing shock. The solid lines depict the least squares estimates, and the light-shaded and dark-grey-shaded areas the 68 and 90 percent confidence intervals, respectively. A bank credit easing shock leads to a decrease in the borrowing rate, similar to the impulse responses of subsection 3.1 estimated from the bank-level data. The corporate bond spread simultaneously decreases and credit conditions ease, consistent with the estimates of the previous subsection.

The ECB's credit easing policies have had statistically significant macroeconomic effects. In response to a credit easing surprise, GDP and prices start to increase. The increases occur simultaneously with a growing stock of bank loans to non-financial corporations. The effects of credit easing statistically significantly materialise in GDP and prices after approximately a year. This transmission lag coincides with a decline of the borrowing rate and increase in loan volumes.

In general, the estimated incorporation of a shock into the borrowing rate and loan stock indicates that GDP and price responses are related to bank lending con-

[30]The real GDP, HICP and industrial production series are obtained from Eurostat. Loan stock and bank lending rate data are provided by the ECB.

[31]The series is the monthly sum of the daily indicator. Another option would be to consider the LS estimate of the TLTRO policy shock obtained from the daily VAR of the previous subsection. The latter measure would control for credit conditions on a daily frequency. However, the change of the shock measure does not change the results.

[32]The lag length $p = 2$ is chosen by Akaike criterion. The use of further lags does not considerably change the results. The confidence intervals are derived by obtained by standard residual bootstrapping.

[33]In addition, we omit the observations from March 2020 until December 2020 in the estimation as the large changes in GDP observed during the acute phase of the Covid-19 pandemic would otherwise dominate the results.

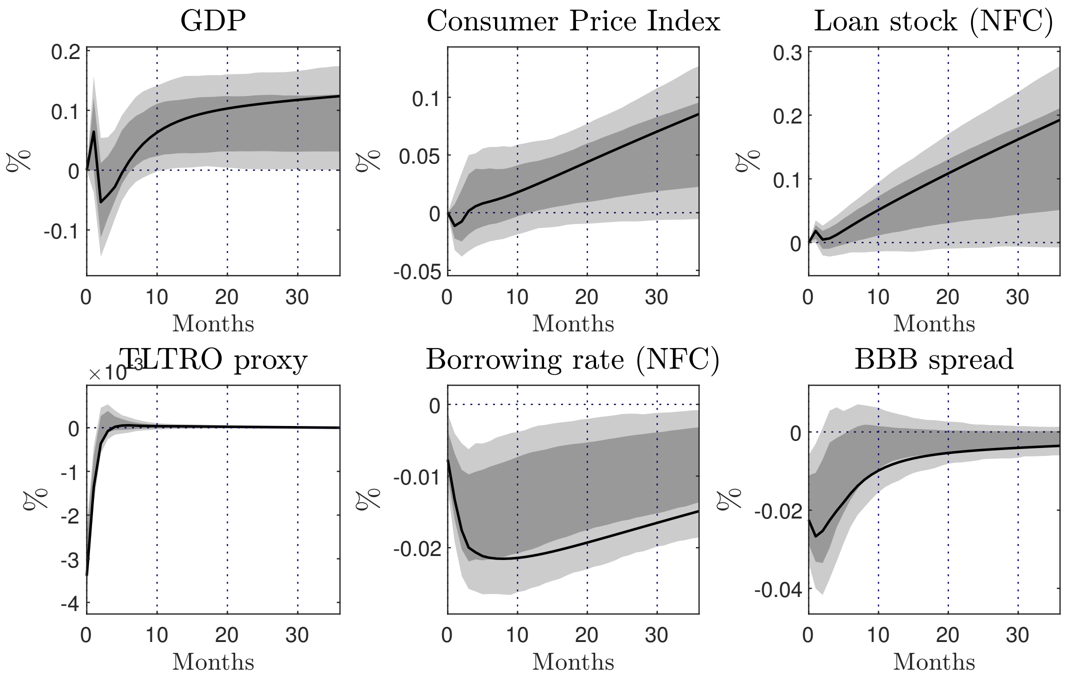



Figure 6: Impulse responses to a credit easing shock, estimated from monthly VAR. The solid lines represent the LS estimates on the impulse responses to a one-standard-deviation credit easing shock. The light-shaded and dark-shaded regions border, respectively, the 90 and 68 percent pointwise confidence intervals, obtained by residual bootstrapping.

ditions. According to our estimates, a one-basis-point decrease in the borrowing rate in one year is associated with GDP and loan stock growth of 0.05 percent. While the shock also induces prices to growth by similar magnitude in the longer run, the effect on output is observed to be more solid. However, uncertainty regarding the estimates is high and the long-run effects are difficult to measure due to the short estimation period.

The muted response of prices may be due to several reasons. The cost channel of monetary policy (Ravenna and Walsh, 2006) can be operational, although not dominating, such that the decrease in borrowing costs ease price pressures. A related theoretical reasoning is provided by Drechsler et al. (2023) who find evidence on the supply-side inflationary effects of credit tightening. The estimated less significant inflationary impact can also be the result of the estimation period characterised by exceptionally slow inflation. In addition, the variables in our model unlikely span the information set of the economic agents and thus may not tackle the price puzzle effectively.

To assess the macroeconomic relevance of TLTROs, we point to the estimates of Rostagno et al. (2021) on the impact of TLTROs on the borrowing rate. They argue that, up to 2019, TLTROs reduced lending rates on loans to non-financial corporations by approximately 0.2 percentage points. According to our impulse responses, a one-time 0.2-percentage-point cut in the lending rate results in a 0.9 and 0.4 increase of GDP and prices, respectively over the next 18 months.

### 3.4 The effect of credit easing policies at firm level

This subsection briefly presents tentative evidence on the extent to which TLTROs promoted firm-level investment. We also evaluate the effects of credit easing policies on different types of firms.

We analyse firm-level investment responses using a quarterly panel of large euro area firms. Specifically, we use financial statement data on Euro Stoxx listed firms. The collected data, originally used in Laine (2023b), span the quarters between 2014Q1 and 2020Q1. After excluding financial corporations and those with missing information on their property, plant and equipment values, and intangible assets, our sample consists of 164 firms. The latter balance sheet items represent capital used in production (fixed assets in accounting terms).

To analyse the average effect on investments we estimate a local projection regression of type (1) with a share of fixed assets to total assets, $\frac{FA_{i,t}}{A_{i,t}}$, as a response variable:

$$\frac{FA_{i,t+h}}{A_{i,t+h}} - \frac{FA_{i,t}}{A_{i,t}} = a_{i,h} + b_h m_t + e_{i,t}, h = 0, 1 \ldots, H, , \tag{5}$$

where $m_t$ is the TLTRO policy surprise variable. The results are shown in panel (a) of Figure 7.[34] The estimated impulse response shows that changes in credit easing policies transmit to investments with a lag, with the share of investment growing statistically significantly after six quarters. The estimated transmission lag is in line with the results of subsections 3.1 and 3.3 according to which bank lending rates react to a policy change with a lag of approximately one year.

The estimated effects of TLTROs are economically significant. A one-basis-point TLTRO-induced bank bond yield decrease that (according to subsection 3.1) lowers the bank lending rate by approximately 5 basis points raises the share of fixed assets to total assets by approximately 0.5 percentage points. This result is even more clearly statistically significant if firm-specific characteristics are controlled for (see Appendix A.4).

Next, we assess whether the investment responses hinge upon the firm-specific characteristics. According to the previous literature, monetary policy incorporates heterogenously to the firms. The credit channel of monetary policy suggests that more financially constrained firms are the most sensitive to interest rate changes (Bernanke and Gertler, 1995). Accordingly, since the TLTROs primarily influence bank lending, firms with high leverage could be expected to be affected the most. On the other hand, if the investment channel (Ottonello and Winberry, 2020) dominates, monetary policy affects the highly-debted firms less.

The heterogeneity also matters for the effectiveness of the credit easing policies. For instance, if the TLTROs promote investments in less profitable firms, these policies might hamper productivity and lower the long-term growth potential. Some studies suggest that monetary policy easing may benefit “zombie” firms, which could explain the absence of inflationary pressure in the euro area (Acharya et al., 2019, 2020). Regarding the TLTROs, Perdichizzi et al. (2023) provide evidence based on Italian firms using province-level identification that these unintended effects on investment may exist.

[34] We also performed the estimations with log-difference of fixed assets as a response variable. The results were qualitatively similar.

To examine the potential heterogeneities, we estimate the equation (5) after augmenting it with interaction terms between our shock proxy and several lagged firm characteristics.[35] Our characteristics variables are firm-specific market value (market cap) in logarithms, profit margin and leverage ratio.

The estimated coefficients of the interaction terms are shown in panels (b), (c) and (d) of Figure 7. Somewhat surprisingly, we find no evidence that credit easing policies produce heterogenous investment responses with respect to firm-specific chacteristics. Although the aggregate effect estimated from this specification (see Appendix A.4) has the expected sign, the interaction terms are insignificant in all specifications and at all lags.

We offer a couple possible explanations for the lack of heterogeneities in our estimates. The transmission mechanism of credit easing policies may differ from other types of monetary policies. The credit channel of monetary policy via change in the collateral values may be of negligible magnitude as credit easing policies are conducted on the effective lower bound of interest rates. Hence, collateral values of borrowers may only negligibly change in the face of news about credit easing policy. While interesting, a detailed assessment of this issue is beyond the scope of this study.

Additionally, our dataset only covers large, listed companies. Hence, there may be insufficient variation in our firm characteristics of interest.[36] Moreover, listed companies often finance themselves through the bond market and bond yields are affected by TLTROs only indirectly.

# 4 Conclusions

This study analysed the effects of the ECB's targeted longer-term refinancing operations between 2014 and 2021 using a novel identification technique based on Google search intensity. The policy surprise variable we derive facilitates estimation of aggregate effects of credit easing policies.

Using a variety of datasets, we examined bank-level, firm-level, financial market and macroeconomic responses to the ECB's TLTRO programmes I, II and III. In general, the ECB's credit easing policies achieved their desired effects on bank lending at the macro level. We find that the operations transmitted to the economy directly via bank lending and indirectly by easing credit conditions in financial markets. Finally, we conclude that TLTROs positively contributed to GDP growth and inflation in the euro area.

Although we focus on the ECB's credit easing policies in this study, our proposed policy surprise variable and the identification technique could readily be applied in other instances as well. Our policy surprise variable, for example, could be used to analyse the transmission and effectivess of longer-term refinancing operations in greater detail. While we find no evidence of heterogeneity in our assessment of firm-level investment responses, the question could be analysed in more depth using other data.

[35] Firm characteristic variables are added as control variables to the regression.

[36] However, the evidence by Laine (2023b) based on the same data suggests that different types of monetary policies may have heterogenous effects even in large firms.

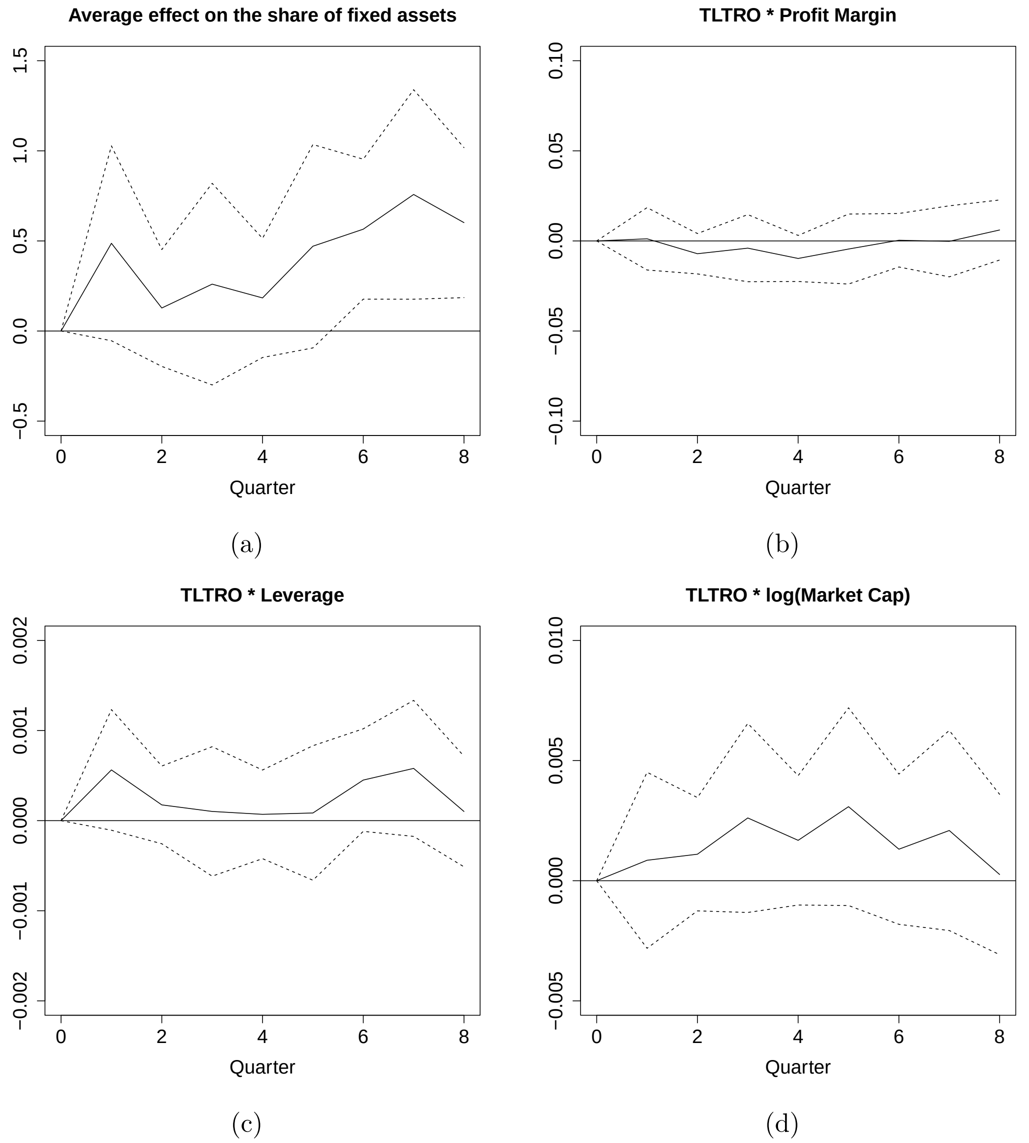



Figure 7: Firm level local projection results.

The data are quarterly and cover the sample from 2014/Q1 to 2020/Q1. The dataset covers 164 firms. 90-percent confidence intervals suggested by Driscoll and Kraay (1998) are reported. The proxy variable is given in basis points and the response variable in percentage points. Profit margin and leverage (debt to equity) are in percentage points. Log(Market Cap) is multiplied by 100. The responses are multiplied by -1 to represent expansionary policy. (a) IRF from TLTRO proxy (no interaction terms in the model) (b) IRF from the interaction between our proxy and the lagged profit margin (c) IRF from the interaction between our proxy and the lagged leverage ratio (d) IRF from the interaction between our proxy and the lagged log(market capitalisation)

Finally, we should mention that the proposed identification strategy based on search intensity is readily available for the analysis of other policy measures or events where the relevant variation is otherwise hard to observe. The approach is particularly suitable for the analysis of events with relatively few announcement dates that are objectively difficult to select or that coincide with other major events.

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# A Appendix

## A.1 Construction of the search intensity index

Here, we provide details on the construction of the intensity index. First, we download daily data in 9-month-long 1-month rolling intervals for the period starting from 2010 until the end of 2022 as longer daily data series are not available from Google Trends. For longer samples, only weekly and monthly series are available. We denote these daily data sets by $x_t = \{x_{1,t}, \ldots, x_{269,t}\}$, where the sample length is 269 days. These overlapping samples do not coincide with each other, but are standardised based on data from the considered period (for a more detailed discussion, see Eichenauer et al. 2022).

Second, to construct a long daily time series, we aggregate the overlapping 9-month daily samples as follows. We normalise the overlapping daily samples by dividing each element of $x_t$ by the maximum element of $x_t$. For each day, we take an average over all samples that includes observations from the corresponding month.[37]

Third, we separately download a monthly series covering the whole period starting from 2010 by which we aim to capture long-term trends in the search intensity. Using this series, we scale the normalised daily series by the corresponding monthly observation of the long-term series to obtain a high-frequency measure for the search intensity, which is finally standardised by its maximum value to a range $[0, 1]$. Figure A.1 shows the aggregated daily index, monthly index and the daily index scaled by the monthly index separately for the both search items “LTRO” and “TLTRO”. Figure A.2 shows the final daily search volumes.

[37] We use R package gtrendsR to download Google data.

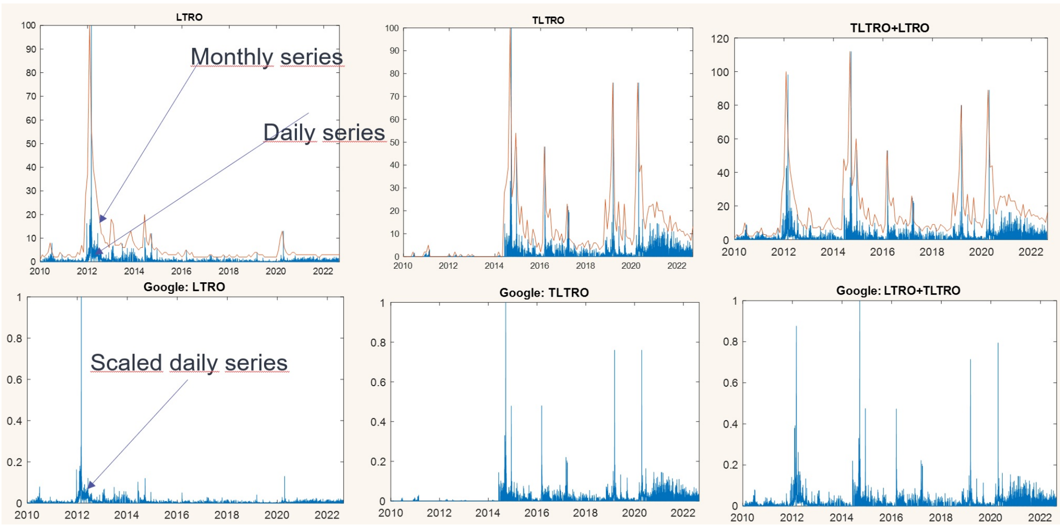
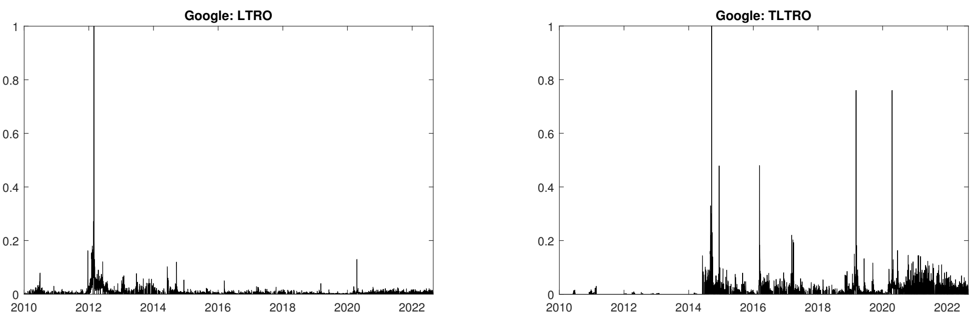



Figure A.1: Construction of the search intensity: daily and monthly intensities for the search items.




(a) LTRO

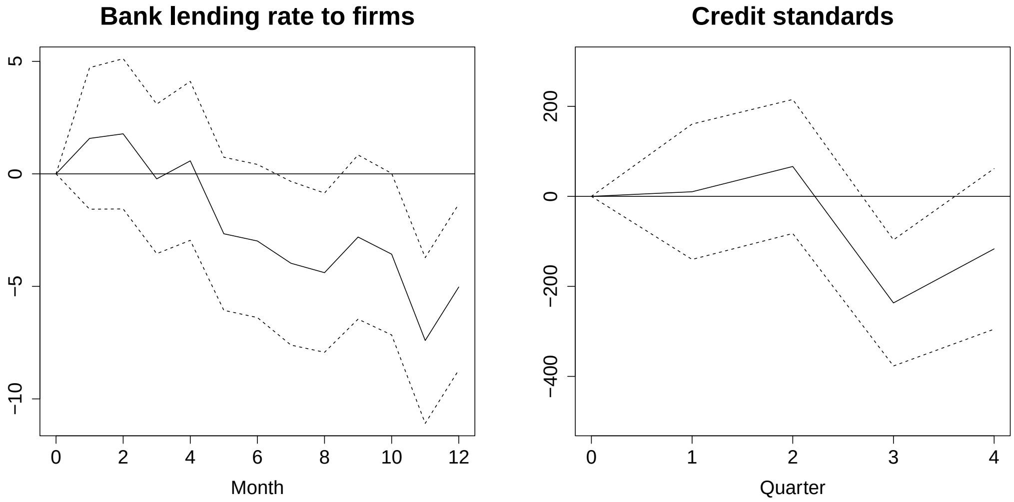
(b) TLTRO

Figure A.2: Google Trends search volumes for items LTRO and LTRO.

Figure A.2 shows the aggregated daily indices of the search intensity separately for the search items LTRO and TLTRO. These two search items are combined by summing the daily intensities as $x_t = x_t^{LTRO} + x_t^{TLTRO}$. As Figure A.2 suggests, the two search items largely occur in two distinct time periods, with LTRO dominating before 2014 and TLTRO thereafter.

## A.2 Further results on the bank lending effects

Panel (a) of Figure A.3 shows the impulse responses of bank lending rate when control variables are added to the regression (2). The following bank-specific variables are added. We control for liquidity (liquid assets to total assets), the capitalisation ratio (capital and reserves to total assets), the bank size (log of total assets) and bank-specific credit demand. Credit demand is measured by confidential data on the individual banks' answers to the ECB's Bank Lending Survey. Specifically, two dummy variables are added. The first obtains the value of 1 if demand has increased, and the second one obtains the value of 1 if demand has decreased. The control variables are lagged by one period in the regression.




(a) (b)

Figure A.3: The effects of credit easing surprise to bank-lending behaviour: estimation with bank-specific and macroeconomic control variables.
Panel (a): The bank-level response of the lending rate, estimated from regression (2), after controlling for bank-specific liquidity, capitalisation, size and credit demand variables. 90-percent confidence intervals based on Driscoll and Kraay (1998) reported in dashed lines. Panel (b): Estimated local projection impulse response function of credit standard measure (in net percentages) from the ECB's Bank Lending Survey, after controlling for GDP growth and the VSTOXX volatility index. 90-percent Newey-West-based confidence intervals in dashed lines.

Panel (b) of Figure A.3 shows the responses of aggregate credit standards, when the log difference of real GDP and VSTOXX volatility index are included as lagged control variables.

## A.3 Robustness checks for estimation of financial market reactions

Figure A.4 plots the results derived from the VAR model, where the conventional monetary policy surprises are controlled for. The latter is done by including the 1-week OIS rate reactions around the regular monetary policy meetings of the ECB, obtained from the Euro Area Monetary Policy Event Study Database (EA-MPD) of Altavilla et al. (2019) to the VAR. The credit easing shock is then identified by Cholesky decomposition with the 1-week OIS reaction ordered first and the LTRO policy surprise proxy second. Switching the order or using an OIS rate of other maturity as a proxy for a monetary policy surprise does not significantly change the results.

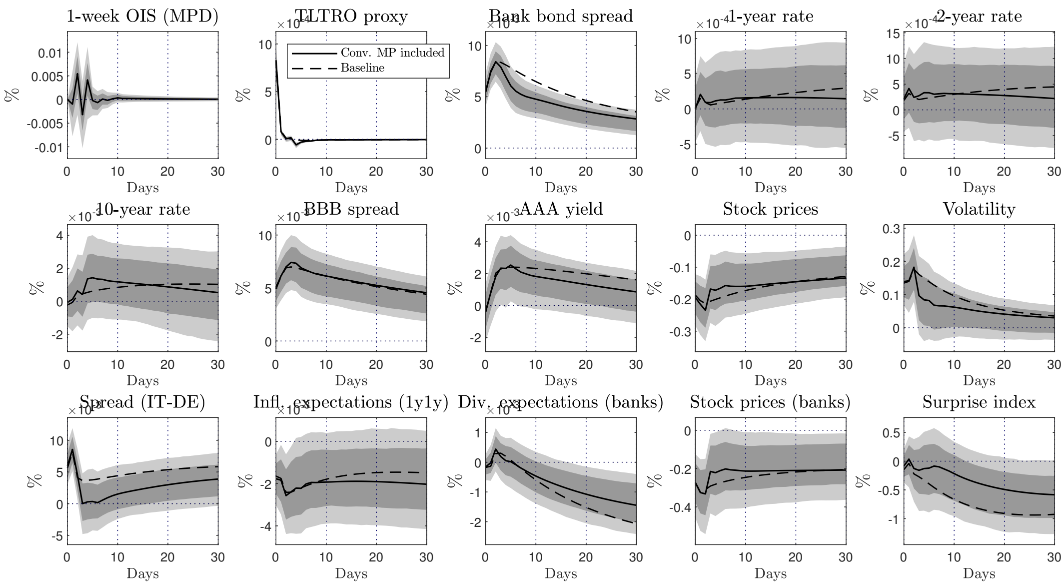



Figure A.4: Estimated impulse responses to a credit easing shock after controlling for conventional monetary policy surprises.
The solid line depicts the least squares estimate of the impulse responses to a one-standard-deviation bank lending shock, estimated from the daily VAR(4), where the shock is identified by the Cholesky decomposition with the conventional monetary policy proxy ordered first and the TLTRO policy surprise index second. The dashed lines depict the baseline results from the VAR(2). The light-shaded and dark-shaded regions border, respectively, the 90 and 68 percent pointwise confidence intervals of the model augmented with the conventional monetary policy surprise proxy. The lag lengths are chosen by the Akaike information criterion. The baseline estimates are rescaled such that the maximum impact on bank bond spread aligns with the estimates from the augmented model.

Figure A.5 depicts impulse responses estimated by local projections (Jordà, 2005) regression

$$y_{i,t+h} - y_{i,t-1} = a_i + b_{i,h} m_t + \varepsilon_{i,t} \tag{A.1}$$

in solid lines.[38] For comparison, the VAR-based impulse responses of the baseline specification are shown in dashed lines.

[38] The impulse responses are similar in case more controls are added to the regression or if the local projections are implemented by $y_{i,t+h}$ on the left-hand side and the lags of $y_{i,t}$ on the right-hand side.

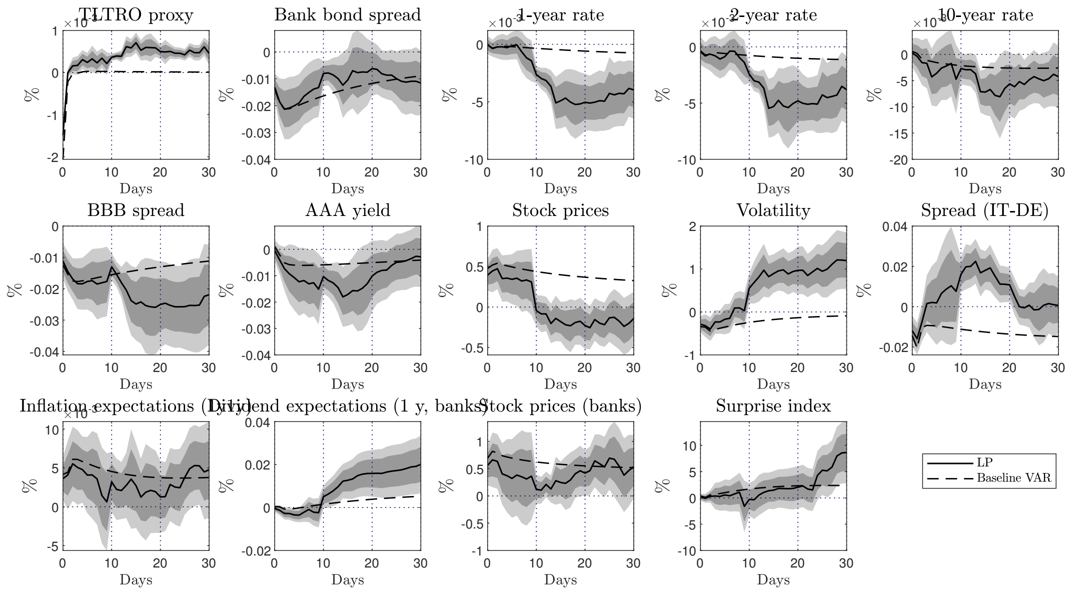



Figure A.5: Impulse responses to a credit easing shock: local projection estimates. The solid line depicts the ordinary least squares impulse responses to a credit easing shock estimated by local projections. The impulse responses are scaled by the standard deviation of the policy surprise indicator. The dashed line depicts the baseline VAR results. The light-shaded and dark-shaded regions border, respectively, the 90 and 68 percent Newey–West-based pointwise confidence intervals. The VAR impulse responses are scaled to align with the maximum impact of the LP estimate on bank bond spread.

In Figure A.6, the impulse responses are derived from the VAR model with the credit easing shock identified by an alternative stock-price-based proxy. The latter policy surprise indicator uses as the measure of relative financing cost the difference in the daily rate of return of the Euro Stoxx Banks and the Euro Stoxx 50 indices.

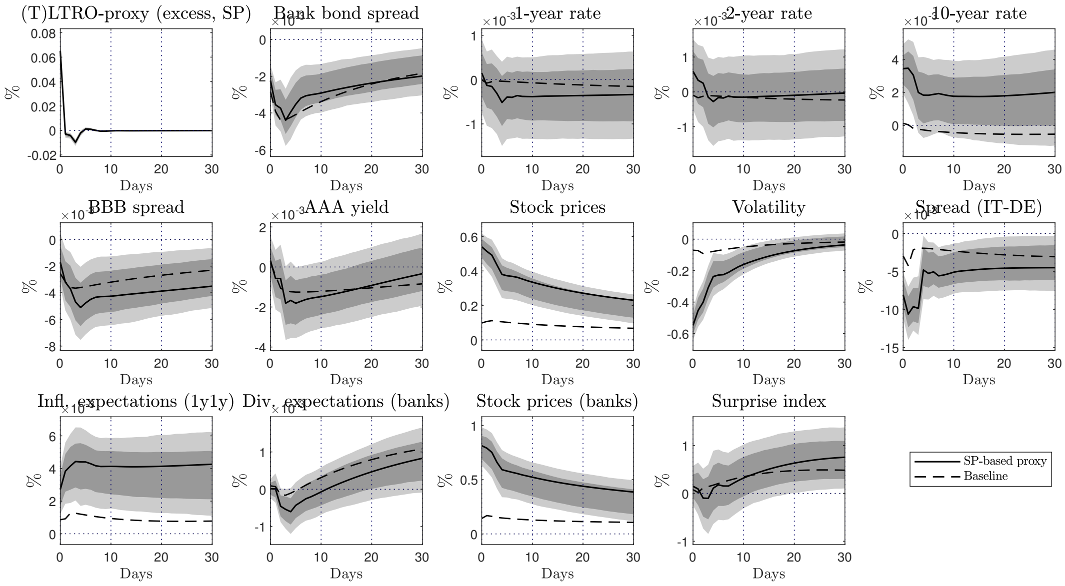



Figure A.6: Estimated impulse responses to a credit easing shock: stock-price-based policy surprise indicator.
The solid line depicts the least squares estimate of the impulse responses to a one-standard-deviation bank lending shock, estimated from the daily VAR(4), where the shock is identified by the Cholesky decomposition with the stock-price-based policy surprise indicator ordered first. The dashed lines depict the baseline results from the VAR(2). The light-shaded and dark-shaded regions border, respectively, the 90 and 68 percent pointwise confidence intervals of the model augmented with the conventional monetary policy surprise proxy. The lag lengths are chosen by the Akaike information criterion. The baseline estimates are rescaled such that the maximum impact on bank bond spread aligns with the estimates from the other model with stock-price-based proxy.

Figure A.7 plots impulse responses from a VAR model that identifies the credit easing shock using the heteroskedasticity-based approach of Rigobon (2003). Accordingly, we assume that a credit easing shock has a different variance on days the intensity measure has obtained its 20 largest values, listed in Table 2.[39]. Given this assumption, the first column of the impact matrix referring to the credit easing shock can be identified. The VAR from which the impact matrix is derived includes all variables as in the baseline specification except the policy surprise indicator. The impact matrix is estimated by the Generalised Methods of Moments (GMM) estimation technique of Wright (2012).

[39]As before, we omit the observations of March 2020 from the estimation due to the extreme volatility during that period.

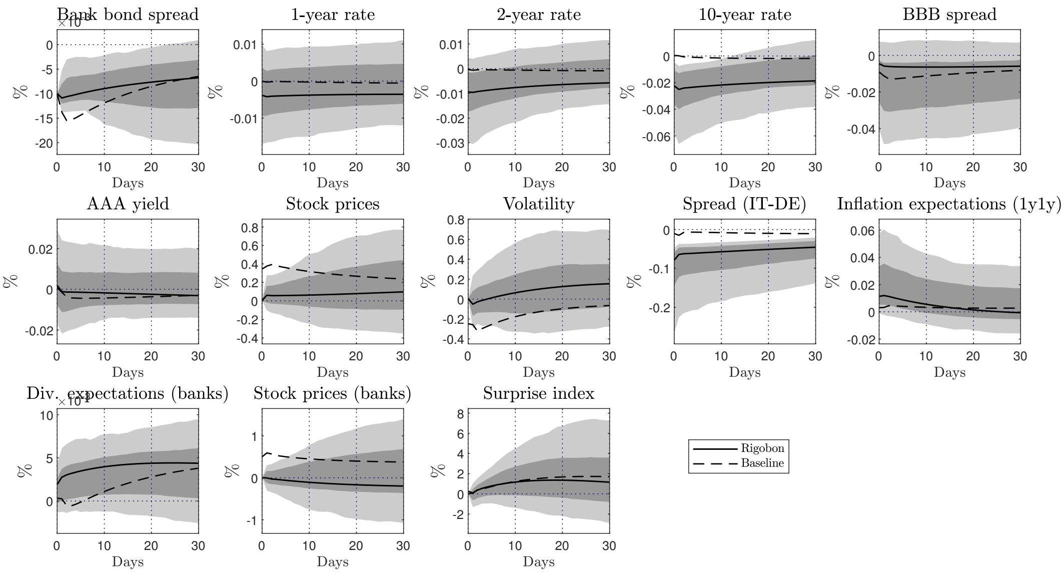


Figure A.7: Estimated impulse responses to a credit easing shock – heteroskedasticity-based identification.

The solid line depicts the least squares estimate of the impulse responses to a bank lending shock, estimated from the daily VAR(2). The dashed lines depict the baseline results from the VAR(2). The light and dark, respectively, shaded regions border the 90 and 68 percent pointwise confidence intervals, obtained by wild bootstrap. The lag lengths are chosen by the Akaike information criterion. Impulse responses from the both models are scaled to have an initial effect on the bank bond spread of 1 basis point.

In Figure A.8, the impulse responses are derived from two different specification in addition to the baseline SVAR results shown in dashed lines. First, the solid lines and the confidence intervals draw the impulse responses from the proxy SVAR (Mertens and Ravn, 2013). The model includes all variables except the policy surprise indicator. The credit easing shock is recovered by using the policy surprise indicator as a proxy. Second, in the marked lines, results are shown for the baseline specification estimated with a VAR of 12 lags.

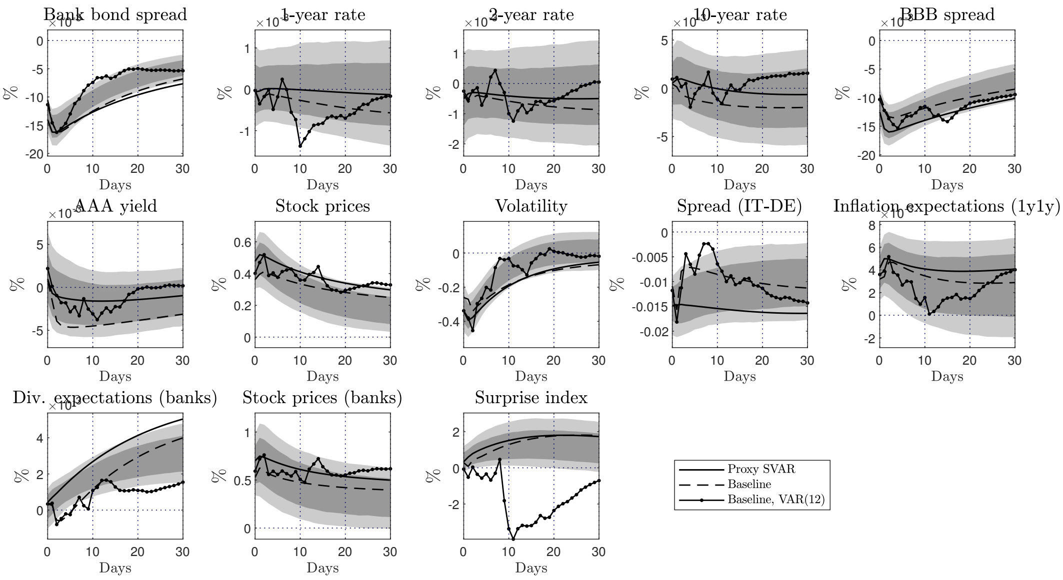



Figure A.8: Estimated impulse responses to a credit easing shock: proxy SVAR and longer lag length

The solid line depicts the least squares estimate of the impulse responses to a one-standard-deviation bank lending shock, estimated from the daily proxy SVAR with 2 lags. The dashed lines depict the baseline results from the VAR(2). The marked line depicts the estimates from the baseline VAR(12) model. The light-shaded and dark-shaded regions border, respectively, the 90 and 68 percent pointwise confidence intervals of the proxy SVAR model, obtained by block bootstrapping. Impulse responses are scaled such that the maximum impact of the bank bond spread coincides with the estimate of the proxy SVAR estimate.

## A.4 The effect on investments after controlling for firm-specific variables

Figure A.9 shows the results regarding firms' investment after controlling for firm-specific profitability, leverage and (log) market capital. The control variables are lagged by one period.

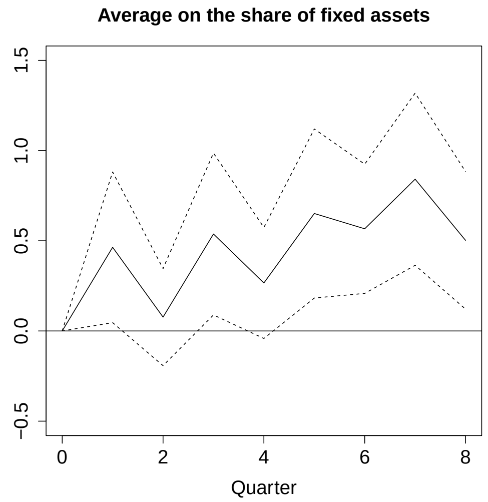



Figure A.9: Firm-level local projection results after controlling for firm-specific profitability, leverage and size.
The data are quarterly and cover the quarters from 2014Q1 to 2020Q1. The dataset is a balanced panel of 164 firms. 90-percent confidence intervals of Driscoll and Kraay (1998) are reported.

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