CPB Netherlands Bureau for Economic Policy Analysis

CPB Discussion Paper | 372

Effects of Unconventional Monetary Policy on European Corporate Credit

Machiel van Dijk
Andrei Dubovik

Effects of Unconventional Monetary Policy on European Corporate Credit*

Machiel van Dijk[a] and Andrei Dubovik[a,b]

[a]CPB Netherlands Bureau for Economic Policy Analysis
[b]Erasmus University Rotterdam

January 23, 2018

Abstract

In this paper we investigate whether the targeted longer-term refinancing operations (TLTRO) and the asset purchase programme (APP) led to lower interest rates on new corporate credit, and whether the signalling channel and the capital relief channel played any role in the transmission of these ECB policies. We find that both APP and TLTRO contributed to lower long-term interest rates on new corporate credit and to flatter yield curves, with APP having a stronger effect. However, we find no support that either the signalling or the capital relief channel were conducive in this respect.

JEL Codes: E43, E58, G21.

1 Introduction

Towards the end of 2014, the rate on the main refinancing operations set by the ECB was down to 0.05%. Having essentially reached the limits of its standard open market operations, the ECB proceeded with Unconventional Monetary Policy (UMP). In particular, starting in September, it resumed the targeted longer-term refinancing operations (TLTRO) on a quarterly basis. Then, in March 2015, the ECB started the Asset Purchase Programme (APP) with a monthly allotment of €60 bln. A year later, in March and April of 2016, the rate on the main refinancing operations was lowered to 0%, and APP was extended to €80 bln. a month (see Fig. 1, left panel).

The goal of UMP is to bring inflation back to its 2% target. It aims to achieve this goal by facilitating investment and consumption through lower

*The authors are thankful to their colleagues at CPB, the participants of the 3rd Benelux Banking Research Day, and Dirk Bezemer for valuable suggestions.

interest rates for businesses and households. Previous research (see, e.g., Andrade et al., 2016) has identified a number of channels through which UMP could achieve lower interest rates. One channel, common both to APP and TLTRO, is the *signalling channel*. By influencing expectations of future short-term interest rates, central banks can affect asset prices and economic activity (Bernanke and Reinhart, 2004). For this policy commitment to be credible, central banks ideally signal their commitment by acting accordingly. The ECB, by accumulating long-term obligations on its balance sheet, signals its willingness to maintain lower interest rates for a longer period of time. This signal results in lower interest rates overall, including those on corporate credit and household savings.

Another channel, which is specific to APP, is the *capital relief channel*. The commitment by the ECB to regular purchases of sovereign bonds should result in a permanent price increase for those bonds. A permanent increase in sovereign bond prices is equivalent to a capital injection for banks holding those bonds. In turn, a capital injection alleviates capital constraints, increases supply of corporate credit and lowers the corresponding interest rates.

In this paper we aim to investigate whether UMP worked as expected. Specifically, we ask the following questions. Did APP and TLTRO result in lower interest rates on new corporate credit? If so, did the signalling and the capital relief channel play a role? Furthermore, which of the programmes, APP or TLTRO, had a bigger effect?

These research questions are relevant not only for economists, but also for policymakers. After all, UMP instruments are controversial, because they bring along various macroeconomic risks. APP bears the risk that the ECB indirectly finances EU countries. Lower interest rates on sovereign debt make it easier for governments to meet the goals of the Stability and Growth Pact of the European Commission, thus reducing the discipline for prudent fiscal policy and increasing the risk of moral hazard. The moral hazard risks resulting from UMP are not exclusive to governments either. They are also relevant for banks that may act less cautious knowing that the ECB stands ready to help with large scale asset purchases and cheaper long-term credit. Furthermore, both APP and TLTRO flatten the yield curve, which results in capital losses for market players with short assets and long liabilities such as insurers and pension funds.

While there are potential risks to UMP, it is furthermore not immediately evident that UMP has been pivotal in raising inflation towards its intended level of 2%. Other confounding factors could have played a role. One way to judge the effectiveness of UMP is to check whether UMP indeed contributed to lower rates on corporate credit, and whether UMP worked through its expected channels in doing so.

Our results show that there was a statistically significant decrease in the long-term interest rates on corporate credit in the second half of 2014–

beginning of 2015. We find no statistically significant change in the short-term interest rates. Furthermore, our data shows that both monthly APP purchases and quarterly TLTRO auctions had an effect, with the effect of APP being the strongest. However, we find no support that the outstanding balances of either APP or TLTRO were important. Thus, we find no evidence in our data for the efficacy of the signalling channel.

For the purpose of assessing whether the capital relief channel was important, we construct an expected capital relief indicator. Our indicator measures how much banks in a given country should have benefited from APP given their exposures to specific sovereign bonds and given how strongly those bonds have reacted to the announcement of APP.[1] If the capital relief channel was important, then we expect to find a stronger effect of APP in countries for which our indicator is higher. Yet, we find no statistically significant effects with respect to our capital relief indicator.

We do find a statistically significant effect with respect to leverage. However, this effect is the opposite of what we would expect if the capital relief channel played a role. If the latter was important, then we should observe a stronger effect of APP in countries that had a more leveraged banking system prior to APP. We find, however, that APP had a bigger impact in countries with a less leveraged banking system. Both our results, with respect to the expected capital relief indicator and with respect to leverage, suggest that the capital relief channel did not play a noticeable role.

The remainder of this paper is organized as follows. Section 2 reviews the relevant literature, Section 3 describes our data, Section 4 introduces the model and the estimation strategy, Section 5 presents the results, and Section 6 concludes with some discussion over the limitations of the current research and the potential avenues for future exploration.

2 Literature Overview

Several authors highlight the disruptive effect of financial crises on transmission of monetary policy. For instance, Gambacorta et al. (2015) show that transmission of policy rates to lending rates broke down during the global financial crisis. In such circumstances, quantitative easing (QE) can be an effective substitute for the traditional monetary policy instruments. Indeed, Swanson and Williams (2014) show that central banks have ample room to affect medium and long term interest rates, despite the main refinancing operations rate being at its zero lower bound. Still, there is limited understanding of the exact mechanisms through which QE and similar policies affect the real economy. One recent paper that explores this question is Rodnyansky and Darmouni (2017). The authors look at QE3, which con-

[1] The exposure data comes from the transparency tests conducted by the European Banking Authority.

sisted of a large-scale purchase of mortgage-backed securities (MBS), and they show that the US banks with higher exposures to MBS increased their lending more following QE3 than did the banks which were less exposed. In effect, their finding suggests that QE3 impacted corporate credit through the capital relief channel. We follow the idea of Rodnyansky and Darmouni but apply it to the European countries, where instead of banks' exposures to MBS we use banks' exposures to sovereign debt.

A necessary condition for the capital relief channel to work is that APP had an effect on sovereign bond prices in the first place. That this effect took place is generally undisputed. Andrade et al. (2016) show that APP led to lower interest rates on sovereign bonds, with the effect being the largest when new interventions were announced. Further, De Santis and Holm-Hadulla (2017) show that not only the announcements, but also the actual flow of purchases by the ECB, had an impact on sovereign yields, albeit the latter effect was smaller than the former.

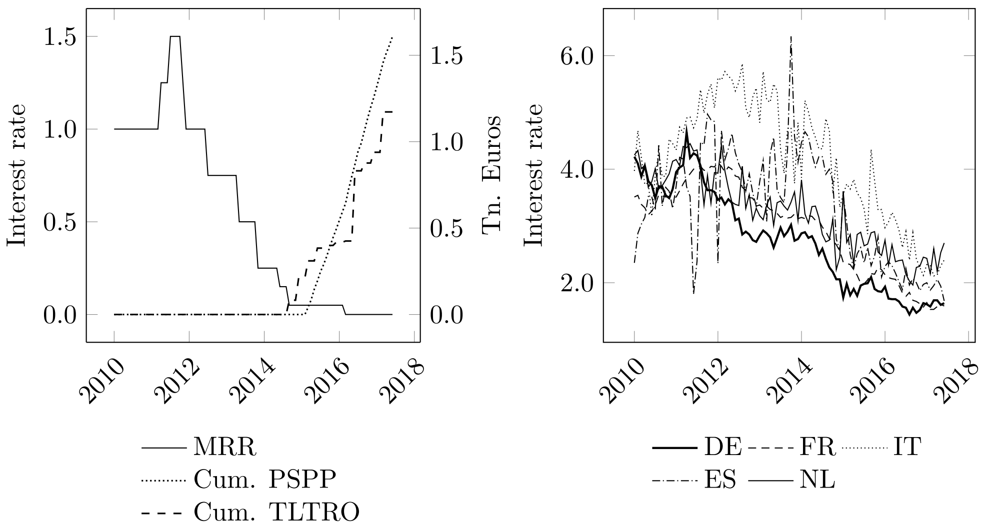
Similar negative effects of QE on sovereign bond yields were found for the UK by Joyce et al. (2011), who show that the largest part of the impact was through a portfolio effect. Lenza et al. (2010) compare the impact of (early) monetary policy responses of the ECB, the FED and the Bank of England and find a high degree of similarity in the responses and their effects on interest rates and spreads.

While there is strong evidence that APP had an effect on sovereign bond yields, it is not guaranteed that lower bond yields translate to lower rates on corporate credit in post-crisis circumstances. The empirical evidence of the effect of UMP on credit supply to firms is indeed mixed.

A number of papers find a positive effect. Andrade et al. (2015) show that French banks that bid more on the LTROs also lent more to non-financial corporations. Similarly, Carpinelli and Crosignani (2017) find that 3-year LTROs had a positive effect on corporate credit supply in Italy. The effect was stronger for those banks that were more exposed to the wholesale market, and thus were more liquidity constraint after the preceding wholesale bank run. Boeckx et al. (2017) estimate a VAR model and find that an exogenous expansion of the ECB's balance sheet led to an increase in the volume of bank lending to consumers and firms, as well as to lower interest rates initially.

Other papers find a mixed or no effect. Cycon and Koetter (2015) use changes in regional taxes in Germany to distinguish supply and demand effects for loans. They find that SMP and CBPP, the precursors of APP, did not themselves reduce corporate loan rates. However, these monetary policies were effective in mitigating the effects that an expansionary fiscal policy had on interest rates. Creel et al. (2016) estimate a VAR model and find that UMP had mixed effects: some instruments were effective in some countries, while other instruments were effective in other countries. The authors summarize that the effect of UMP was primarily on interest rates on

Figure 1: ECB Policies and Banks' Rates on New Corporate Loans



Notes: on the left panel, *MRR* is the main refinancing rate of the ECB, *Cum. PSPP* and *Cum. TLTRO* are outstanding balances of the respective programmes; source: ECB. The right panel shows banks' interest rate on new corporate loans (excluding revolving loans); source: ECB.

banks' credit rather than on volumes. Lojschova (2017) finds that APP—as proxied by the sales of government bonds by banks—improved banks' lending to households in Slovakia, but had no discernible effect regarding lending to firms.

So, whilst some empirical literature regarding the effect of UMP on corporate credit has developed, many studies focus only on particular countries, and mostly look at the earlier interventions such as LTRO's. To the best of our knowledge, the evidence regarding the impact of APP and TLTRO on corporate credit and the efficacy of their transmission channels, is still scarce.

3 Data and Descriptive Statistics

We study the effect that UMP had on interest rates on new corporate credit (excluding revolving loans). Within UMP we look specifically at APP, its component the Public Sector Purchase Programme (PSPP), and TLTRO. Additionally, we also use the ECB's rate on its main refinancing operations. All these data are on monthly basis and come from the ECB website. The data on interest rates on new corporate credit are on monthly/country basis, and come from the ECB website as well (the MIR dataset). We use data for the period January 2010–June 2017. The left boundary has been chosen as

Table 1: Descriptive Statistics

	N	Min	Max	Mean	SD	SD Betw.	SD With.
Rate ($\geq$ 5 years)	900	-0.248	2.102	1.188	0.386	0.182	0.345
Rate ($\leq$ 1 year)	1260	0	1.924	0.978	0.426	0.371	0.231
MRR	90	0	1.500	0.518			0.483
APP	90	0	0.0800	0.0236			0.0340
Cum. PSPP	90	0	1.609	0.248			0.459
TLTRO	90	0	0.406	0.0391			0.0859
Cum. TLTRO	90	0	1.172	0.215			0.348
BCI	900	96.05	102.6	100.0	1.005	0.472	0.900
Leverage	10	7.050	20.08	12.51		4.800	
Capital Relief	9	0.000584	0.00462	0.00252		0.00120	
HHI	10	0.0300	0.331	0.114		0.0934	

Notes: *Rate (maturity)* is the logarithm of the interest rate on new corporate loans, excluding revolving loans; *MRR* is the main refinancing rate of the ECB; *APP* is the expected size of APP, in trillion euros, it equals 0.6 starting January 2015 and 0.8 starting March 2016; *TLTRO* is the TLTRO purchases in the last three months, in trillion euros (ECB organizes TLTRO auctions once per quarter); *Cum. PSPP* and *Cum. TLTRO* are the outstanding balances of the PSPP and TLTRO purchases respectively; *BCI* is the business confidence indicator from the World Bank; *Leverage* is the ratio of total assets to capital for all banks in a given country; *Capital Relief* is the expected capital relief indicator, see Sec. 3; *HHI* is the Herfindahl-Hirschman index for the banking industry in a given country. Until stated otherwise, the data are public data provided by the ECB.

the first year outside the 2007/2008 financial crisis. The data cover 10–14 countries (depending on the credit maturity).

A preview of the data can be found in Fig. 1, and the descriptive statistics are given in Table 1. Additional information on a number of miscellaneous variables is provided in the table's legend. In what follows we discuss specific aspects of the data, including the construction of our expected capital relief indicator.

Flow and stock variables. Absent financial frictions, the excess demand for sovereign bonds must be perfectly elastic. The ECB purchases should then have no effect on the price of sovereign bonds. In reality, the ECB purchases had a positive effect, see Andrade et al. (2016); Georgiadis and Gräb (2016); Koijen et al. (2016), which suggests a negatively sloped excess demand curve. In this case, the announcement of APP should result in price changes, and no further price changes should be observed absent shocks. Therefore, we construct an APP dummy following the ECB announcements: 0 till the end of 2014, 60 bln. euros starting January 2015, and 80 bln. euros starting March 2016. As we discuss in Section 4, we use an error-correction specification, and so shifting the APP dummy back or forward one month does not change the results.

The ECB runs TLTRO auctions once a quarter. If a bank obtains TLTRO funding in a given month, we expect that this funding will propagate to the corporate sector in the coming months. Indeed, if the mechanism was very different, then ECB would have likely chosen a different frequency for its TLTRO auctions. So, we use TLTRO loans in the last 3 months as our flow regressor for TLTRO.

To test for the signalling channel, we compute the cumulative stock of APP and TLTRO. Interest rates change when new information arrives. The rates can react to both, hard information like the outstanding APP and TLTRO balances, and soft information like, for example, the following ECB announcement: "They [asset purchases] are intended to be carried out until at least September 2016 and in any case until the Governing Council sees a sustained adjustment in the path of inflation that is consistent with its aim of achieving inflation rates below, but close to, 2% over the medium term." In this paper we only use the outstanding balances to test for the singalling role of UMP. An alternative would be to encode all ECB announcements with respect to their potential impact on interest rates, similar to Falagiarda and Reitz (2015). Given that the rates on corporate credit react with a lag, and given significant monthly fluctuations that we observe in the rates data, we do not expect that a more precise encoding of the ECB signals would be beneficial. We therefore opt for the outstanding balances as an approximate but simple and well-defined proxy for the signalling channel.

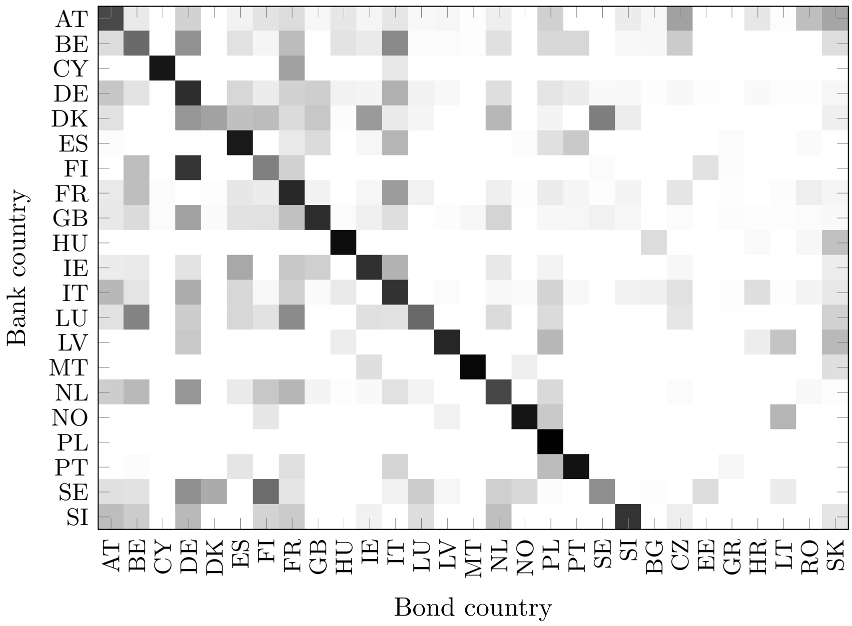
Since we proxy the singalling channel with outstanding balances, we use actual PSPP purchases instead of the APP dummy. As for TLTRO, we simply accumulate all TLTRO auctions to date. We do not need to account for the expiring TLTRO loans as the expiration dates fall outside our data period.

Expected capital relief indicator. We construct the indicator as follows:

$$\mathrm{ECR}_i = \frac{\sum_{k,m} B_{i,k}^m \cdot \Delta p_k^m}{A_i}, \quad \Delta p_k^m = \left. \frac{p_{k,t+1}^m - p_{k,t-1}^m}{p_{k,t-1}^m} \right|_{t=22 \text{ jan } 2015},$$

where $B_{i,k}^m$ is the net exposures of banks in country i to sovereign debt of country k with maturity m at the end of 2014 (source: transparency exercises of the European Banking Authority); A_i is the total risk-weighted assets of banks in country i at the end of 2014 (same source); and $p_{k,t}^m$ is the sovereign bond price for country k and maturity m on date t (source: Thomson Reuters Datastream). We use all available data for the construction of the indicator, however we drop Dexia N.V. due to its specific situation (it was designated a "bad bank" and was undergoing restructuring at the time).

Figure 2: Banks' Net Exposures to Sovereign Debt in 2014



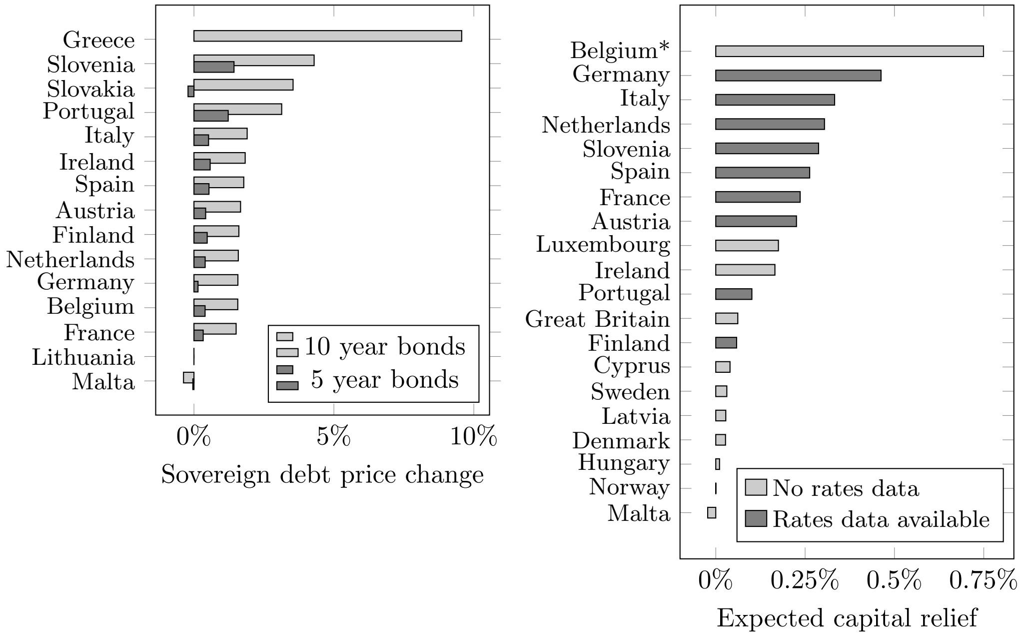
Notes: the figure shows relative holdings of European sovereign bonds by European banks, i.e. each row "sums up" to one. Darker color means larger holdings. Source: EBA.

Fig. 2 illustrates the average $B^m_{i,t}$ for maturities above 5 years. In line with Koijen et al. (2016) we observe that most sovereign bonds are held domestically. Fig. 3 shows Δp^m_k for selected maturities with a breakdown per country (left panel), and the resulting expected capital relief indicator ECR_i (right panel). The set of countries for which we can construct the capital relief indicator is larger than the number of countries for which we have data on interest rates on new corporate loans (long maturities). This discrepancy is highlighted in Fig. 3.

The ECB aimed to execute the PSPP programme in a neutral manner. This is consistent with the pattern shown by the left panel of Fig. 3: there is no discernible difference in the price increases of sovereign bond among the major European countries. Consequently, there is little information contained in Δp^m_k. On the other hand, there was substantial heterogeneity in the net holdings of sovereign debt by banks, and so the expected capital relief indicator that we construct still has substantial variance across major European countries.

Unconventional monetary policies could have had an impact on the real economy via other channels than the bank landing channel, e.g. through expectation formation or through exchange rates. Arguably, these other channels would have increased the demand for credit, which would have led to higher interest rates. Therefore, if we do not correct for possible demand

Figure 3: Expected Capital Relief Indicator



* Belgium figures exclude Dexia N.V.

Notes: the left panel shows a relative price change of sovereign bonds between 21 and 23 of January, 2015; source: Thomson Reuters Datastream. The right panel shows how much the banks should have benefited from APP, in terms of their risk-weighted assets. The countries for which we miss the interest rate data on new corporate loans are shown in light gray; these countries are therefore not used in the regression analysis.

effects, we might underestimate the effect that UMP had on credit supply. We use the Business Confidence Indicator (BCI) from the World Bank as a proxy for demand. Another possible proxy is production in manufacturing. However, this latter proxy directly depends on the corporate interest rates and is therefore endogenous. To alleviate the endogeneity bias we use BCI instead.

4 Methodology

We assume that interest rates on new corporate loans, for a given maturity, are determined as follows:

$$\begin{aligned} \ln r_t^i =& \ln \tilde{r}_t^i + u_t^i, \\ \ln \tilde{r}_t^i =& \alpha_i + \beta \ln \tilde{r}_{t-1}^i + \gamma \text{MRR}_t + \text{UMP}_t \delta + Z_t^i \lambda + \varepsilon_t^i, \end{aligned} \tag{1}$$

where r_t^i is the observed interest rate on new corporate loans (excluding revolving loans) in country i in month t, $\tilde{r}_t^i$ is the unobserved structural interest rate, MRR_t is the main refinancing rate of the ECB, UMP_t is a row-vector of UMP instruments (APP, Cum. PSPP, TLTRO, Cum. TLTRO), and Z_t^i is a row-vector of control variables (currently, BCI). We further assume that all errors are independent and normally distributed (we allow for country specific heteroskedasticity).

Specification (1) is our baseline specification. For some estimates we relax the assumption that δ is the same across all countries, or we add interaction terms between UMP regressors and various cross-country measures (expected capital relief indicator, leverage).

The monetary policy is executed at the European level, and it is based on inflation expectations. Interest rates on corporate credit in a specific country are therefore less likely to have a direct impact on contemporeneous monetary policy, and so we assume that MRR_t and UMP_t are predetermined variables.

The error-in-measurement specification is motivated by Fig. 1, right panel. The panel shows substantial monthly variation in interest rates on new corporate loans. This variation could be caused by temporary shocks that banks experience, e.g. due to reevaluations of their mark-to-market portfolios, or simply due to selection effects: banks receive different clients in different months and therefore the observed interest rate varies from month to month. The selection effects could be partially mitigated if interest rate data were available per risk class, but such data are not available.

In order to avoid inconsistent estimates, we therefore need to allow explicitly for the short-term noise in the interest rates. As we will show in Section 5, if we do not allow for this noise, the resulting bias would lead to estimates of the marginal APP effect that are about eleven times larger than their consistent counterparts.

The interest rates that banks charge on corporate loans are not directly linked to market securities through no-arbitrage conditions. Therefore it is unreasonable to assume that these interest rates will adjust immediately to changes in the main refinancing rate or UMP. Most likely, the rates will adjust gradually towards their new equilibrium levels. We capture this reasoning with an error-correction specification, or—equivalently—by including the lagged interest rate as one of the regressors.

We estimate (1) using Kalman filtering and maximum likelihood. It is possible to relax the normality assumption and estimate the model with GMM using lagged exogenous regressors plus 2^{nd} and further lags of the dependent variable as instruments. However, as we demonstrate in Section 5, these semi-parametric estimates are not robust in our sample—they vary substantially depending on the choice of instruments and/or regressors. In other words, while GMM estimates are theoretically consistent, our finite sample does not have enough information to draw any inference based on them. Consequently, we have opted for parametric estimation as our preferred approach.

Table 2: State Space Estimations

	$\leq$ 1 year	$\leq$ 1 year	$\geq$ 5 years	$\geq$ 5 years	$\geq$ 5 years
L.Rate	0.947***	0.946***	0.905***	0.912***	0.928***
	(0.00850)	(0.00829)	(0.0301)	(0.0221)	(0.0322)
MRR	0.0283***	0.0282***	0.0367***	0.0339***	0.0301***
	(0.00368)	(0.00355)	(0.00809)	(0.00664)	(0.00753)
APP	-0.130	-0.108	-0.408*	-0.397**	-0.694**
	(0.0999)	(0.0619)	(0.159)	(0.141)	(0.229)
Cum. PSPP	-0.00381		-0.0261		
	(0.0152)		(0.0238)		
APP * Leverage					0.0279*
					(0.0109)
TLTRO	-0.0194	-0.0150	-0.0759	-0.0611*	-0.0627*
	(0.0244)	(0.0197)	(0.0389)	(0.0303)	(0.0298)
Cum. TLTRO	0.00797		0.0339		
	(0.0271)		(0.0451)		
BCI	0.00126	0.00130	0.00529	0.00527*	0.00624*
	(0.00130)	(0.00128)	(0.00277)	(0.00263)	(0.00257)
Observations	1260	1260	900	900	900

Notes: the dependent variable is the interest rate on new corporate loans (log-form), *L.Rate* is its lagged value. Column headings give the maturity of the interest rate. The exogenous regressors are described in the notes of Table 1. The standard errors are in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The specification allows for errors in the measurement of the dependent variable. There are country fixed effects and the errors are heteroskedastic.

5 Empirical Results

Maximum likelihood estimates of Model (1) are given in Table 2. The first two columns give estimates for short-term interest rates, the remaining columns give estimates for long-term rates. In all cases we find that interest rates on corporate credit react positively to changes in the main refinancing rate. Importantly, this reaction is gradual: the coefficient for lagged interest rates is significant and is about 0.9–0.95.

We find that neither flow nor stock indicators for APP/PSSP or TLTRO are significant for the dynamic of the short-term interest rates. When we look at the long-term rates, then flow indicators for APP/PSPP and TLTRO are significant and negative. APP and TLTRO are targeted at longer-term loans and so these results conform with the expectation that APP and TLTRO should flatten the yield curve. We still find that stock indicators are insignificant, i.e. we find no support for the signalling channel.

Table 3: Determinants of the Marginal APP Effect

	(1)	(2)	(3)
Leverage	0.0310	0.0362	0.0363*
	(0.0317)	(0.0181)	(0.0148)
Capital Relief	48.20	31.28	
	(116.9)	(76.69)	
HHI	0.357		
	(1.724)		
Observations	9	9	10
AICc	13.75	6.628	0.787

Notes: cross-country regression, the dependent variable is a point estimate of the effect of APP on long-term interest rates; observations with more accurate point estimates have higher weights. The regressors are described in the notes of Table 1. The standard errors are in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

As can be seen from Fig. 1, left panel, *Cum. PSPP* and *Cum. TLTRO* are strongly correlated. The large standard errors that we obtain for these regressors could simply be a consequence of the strong correlation between them. We therefore also run regressions with *APP*, *TLTRO*, and either *Cum. PSPP* or *Cum. TLTRO*. Our conclusions do not change, we still observe no significant effect with regard to the stock indicators.

The results are economically significant. In the long run, monthly APP purchases of €80 bln. yield approximately a 36% reduction in the long-term interest rates.[2] Quarterly TLTRO auctions of €100 bln., which was roughly the average till March 2017, yield approximately a 7% reduction in the long-term rates.[3] The corresponding numbers for the short-term rates are 16% and 2.8%.

If $\text{var}(u_t^i) = 0$ in Model (1), i.e. if we assume that there is no monthly noise associated with interest rates on corporate loans, then Model (1) can be estimated consistently with OLS. However, if $\text{var}(u_t^i) > 0$, then OLS yields inconsistent estimates. Table A4, last column, gives OLS estimates that we obtain from our data. Following these, the adjustment of the interest rates to the ECB policy is much faster. For example, the marginal APP effect is about 11 times larger in comparison with the maximum likelihood estimate. The long term effect is also larger: following OLS, monthly APP purchases of €80 bln. yield a 43% reduction in the long-term interest rates. Having said that, if we test for $H_0 : \text{var}(u_t^i) = 0$ with ML, then the hypothesis is always rejected. Therefore, we do not use OLS estimates.

[2] According to Table 2, column 4: $-0.397/(1-0.912)\cdot 0.08 = -0.36$. The 95% confidence interval equals $[-0.52, -0.17]$.

[3] According to Table 2, column 4: $-0.0611/(1-0.912)\cdot 0.1 = -0.069$. The 95% confidence interval equals $[-0.17, -0.0017]$.

If $\text{var}(u_t^i) > 0$, then in theory the model can be estimated robustly using GMM. Lagged exogenous variables, starting with lag 1, and the lagged endogenous variable (interest rate), starting with lag 2, can be used as instruments. However, with our data these instruments are weak: the first stage F-statistic is 7.47, which is considered small (see, e.g., Staiger and Stock 1997). Potentially as a consequence, GMM estimates also change a lot depending on the regressors or instruments that we use, see columns 3 and 4 in Table A4. We conclude that there is not enough data for a robust semi-parametric estimation, and so we have chosen to present and interpret the parametric estimates (maximum likelihood based on Kalman filtering).

We have data with small N (countries) and large T (months), therefore we can consistently estimate country specific APP effects. We can then check whether countries with a higher capital relief indicator experienced a larger APP effect. Table 3 gives the results when we regress point estimates of the marginal APP effect on our capital relief indicator, the leverage of the banking sector, and the HHI index; see also Fig 4. Further, Table 2, last column, presents estimates of Model (1), when we interact the marginal APP effect with leverage. We do not find that capital relief was correlated with the size of the APP effect. However, we do find that countries with a more leveraged banking system experienced a smaller APP effect, which contradicts rather than supports the capital relief mechanism.

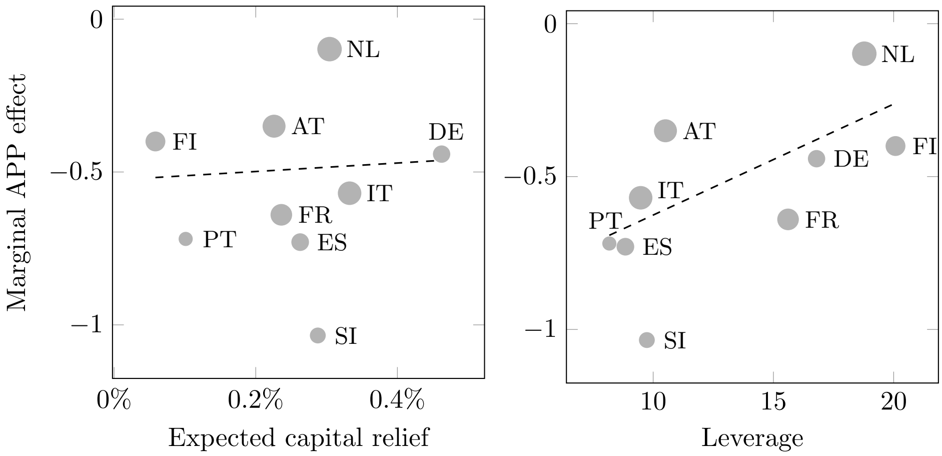
Interest rates are normally positive and so it is customary to model the dynamics of their logarithms rather than the dynamics of the rates themselves. We have done so for all the preceding estimates. It could be hypothesized, however, that UMP lowers all interest rates by the same amount of basis points, rather than proportionally. We test this alternative specification and report our results in Table A5, column "Levels." According to AIC, it is extremely unlikely that the levels specification minimizes the information loss when compared against the log specification.

The interest rates that we use are not offered rates but realized rates, computed for a given month and across all customer categories. We do not observe whether the composition of customers changes from month to month, e.g. with respect to their risk profiles, which can introduce a bias to our estimates. An alternative source of information on the interest rates that banks offer to corporate clients comes from the SAFE survey conducted by the ECB. Based on the survey, we construct the following index:

$$r_{i,t}^{\text{SAFE}} = r_{i,t-1}^{\text{SAFE}} + \ln(1 + s_{i,t}), \quad r_{i,0}^{\text{SAFE}} = 0,$$

where $s_{i,t}$ is the percentage of respondents that say that the interest rate has increased in the last 6 months minus the percentage of respondents that say that the interest rate has decreased (Question 10a). We then repeat our analysis but using r^{SAFE} instead of r, and on a biannual basis instead of a monthly basis.

Figure 4: Determinants of the Marginal APP Effect



Notes: *Marginal APP effect* denotes point estimates of the effect of APP on long-term interest rates. The area of any circle is inversely proportionate to the variance of the corresponding point estimate, i.e. larger circles denote more accurate estimates. The slope of the regression line in the left panel is not statistically different from zero; the slope in the right panel is significant at 5%.

The results are given in Table A5, column "SAFE." We do find that the offered rate, as reported by the SAFE survey, is highly persistent. While it has decreased in the recent years, the whole decrease could be explained as a delayed response to a decrease in the main refinancing rate; the UMP instruments come out insignificant in the analysis. That being said, since the rate is highly persistent, the 95% confidence interval for the long term effect of APP is very large. Say, for monthly purchases of €80 bln it is $[-11800\%, +11500\%]$. Effectively, there is not enough data from the biannual SAFE survey to accurately estimate the effects of UMP.

6 Concluding Remarks

Our empirical results suggest that TLTRO and APP have led to lower interest rates on new corporate loans. However, we find no evidence that the signalling channel and the capital relief channel have been effective in transmitting UMP. Other transmission channels, such as the confidence channel—when decisive actions of the ECB lead to lower market volatility and thus lower interest spreads—may well have played a role. For instance, reduced form macro models such as in Elbourne et al. (2017) do find positive effects of UMP.

Further research, ideally with bank-level data, can perhaps clarify which of the remaining explanations is more valid. If other transmission channels, such as the confidence channel, explain how corporate interest rates have declined, then perhaps less risky monetary policy measures can achieve the same goals in the future. So, as long as strong empirical evidence for specific transmission channels of UMP on its objectives is absent, ECB should perhaps be more restrained in the future with its unconventional interventions. Furthermore, without good empirical understanding of the transmission channels, it is challenging to make accurate forecasts about the consequences of the UMP phaseout.

References

Andrade, P., Breckenfelder, J., Fiore, F. D., Karadi, P., and Tristani, O. (2016). The ECB's asset purchase programme: an early assessment. ECB Working Paper No. 1956.

Andrade, P., Cahn, C., Fraisse, H., and Mésonnier, J. (2015). Can unlimited liquidity provision help to avoid a credit crunch? evidence from the eurosystem's filtros. Banque de France Working Paper No. 540.

Bernanke, B. S. and Reinhart, V. R. (2004). Conducting monetary policy at very low short-term interest rates. *The American Economic Review*, 94(2):85–90.

Boeckx, J., Dossche, M., and Peersman, G. (2017). Effectiveness and transmission of the ecb's balance sheet policies. *International Journal of Central Banking*, 13(1):297–333.

Carpinelli, L. and Crosignani, M. (2017). The effect of central bank liquidity injections on bank credit supply. FEDS Working Paper No. 2017-38.

Creel, J., Hubert, P., and Viennot, M. (2016). The effect of ecb monetary policies on interest rates and volumes. *Applied Economics*, 48(47):4477–4501.

Cycon, L. and Koetter, M. (2015). Monetary policy under the microscope: Intra-bank transmission of ecb asset purchase programs. Working Paper.

De Santis, R. A. and Holm-Hadulla, F. (2017). Flow effects of central bank asset purchases on euro area sovereign bond yields: evidence from a natural experiment. ECB Working Paper Series No. 2052.

Elbourne, A., Ji, K., and Duijndam, S. (2017). The effects of unconventional monetary policy in the euro area. CPB Discussion Paper.

Falagiarda, M. and Reitz, S. (2015). Announcements of ECB unconventional programs: Implications for the sovereign spreads of stressed euro area countries. *Journal of International Money and Finance*, 53:276–295.

Gambacorta, L., Illes, A., and Lombardi, M. J. (2015). Has the transmission of policy rates to lending rates changed in the wake of the global financial crisis? *International Finance*, 18(3):263–280.

Georgiadis, G. and Gräb, J. (2016). Global financial market impact of the announcement of the ecb's asset purchase programme. *Journal of Financial Stability*, 26:257–265.

Joyce, M., Lasaosa, A., Stevens, I., and Tong, M. (2011). The financial market impact of quantitative easing in the united kingdom. *International Journal of Central Banking*, 7(3):113–161.

Koijen, R. S., Koulischer, F., Nguyen, B., and Yogo, M. (2016). Quantitative easing in the euro area: The dynamics of risk exposures and the impact on asset prices. Banque de France Working Paper No. 601.

Lenza, M., Pill, H., and Reichlin, L. (2010). Orthodox and heterodox monetary policies. *Economic Policy*, 62:295–339.

Lojschova, A. (2017). Did quantitative easing boost bank lending? The Slovak experience. MPRA Paper No. 79567.

Rodnyansky, A. and Darmouni, O. M. (2017). The effects of quantitative easing on bank lending behavior. *The Review of Financial Studies*, 30(11):3858–3887.

Staiger, D. and Stock, J. H. (1997). Instrumental variables regression with weak instruments. *Econometrica*, 65(3):557–586.

Swanson, E. T. and Williams, J. C. (2014). Measuring the effect of the zero lower bound on medium-and longer-term interest rates. *The American Economic Review*, 104(10):3154–3185.

A Additional Tables

Table A4: Alternative Estimators

	State Space	State Space	Weak IV	Weak IV	OLS
L.Rate	0.905***	0.912***	0.631***	0.799***	0.187
	(0.0301)	(0.0221)	(0.139)	(0.126)	(0.109)
MRR	0.0367***	0.0339***	0.0956*	0.0643	0.178**
	(0.00809)	(0.00664)	(0.0386)	(0.0371)	(0.0449)
APP	-0.408*	-0.397**	-0.814	-1.004	-4.342***
	(0.159)	(0.141)	(0.632)	(0.692)	(0.901)
Cum. PSPP	-0.0261		-0.00900		
	(0.0238)		(0.103)		
TLTRO	-0.0759	-0.0611*	-0.0227	-0.00514	-0.195**
	(0.0389)	(0.0303)	(0.129)	(0.111)	(0.0592)
Cum. TLTRO	0.0339		-0.107		
	(0.0451)		(0.151)		
BCI	0.00529	0.00527*	0.0127	0.00594	0.00851
	(0.00277)	(0.00263)	(0.0101)	(0.00978)	(0.0113)
Observations	900	900	870	870	890

Notes: the dependent variable is the interest rate on new corporate loans (log-form), *L.Rate* is its lagged value. Column headings denote the estimation procedures. State space and GMM estimates (IV) allow for errors in the measurement of the dependent variable. OLS is consistent only if there are no such measurement errors. The exogenous regressors are described in the notes of Table 1. The standard errors are in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. There are country fixed effects and the errors are robust to heteroskedasticity. First-stage 2SLS (IV columns) F-statistic is 7.47.

Table A5: Robustness Checks

	Baseline	Levels	SAFE
L.Rate	0.912***	0.911***	0.971***
	(0.0221)	(0.0236)	(0.0131)
MRR	0.0339***	0.113***	0.440***
	(0.00664)	(0.0240)	(0.0862)
APP	-0.397**	-0.898*	-0.422
	(0.141)	(0.400)	(1.221)
TLTRO	-0.0611*	-0.106	-0.436
	(0.0303)	(0.0862)	(0.563)
BCI	0.00527*	0.0248**	0.0436
	(0.00263)	(0.00909)	(0.0483)
Observations	900	900	105
AIC	-1100.0	1010.1	49.37

Notes: the dependent variable is the interest rate on new corporate loans, either in logs (column "Baseline"), or in levels (column "Levels"). The third column is the evolotion of the interest rate on corporate loans as reported by comanies in the SAFE survey. *L.Rate* is the lagged value of the corresponding interest rate. The exogenous regressors are described in the notes of Table 1. The standard errors are in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The specification allows for errors in the measurement of the dependent variable. There are country fixed effects and the errors are heteroskedastic (all columns except "SAFE").

Publisher:

CPB Netherlands Bureau for Economic Policy Analysis
P.O. Box 80510 | 2508 GM The Hague
T (088) 984 60 00

February 2018