Journal of Financial Stability 54 (2021) 100881

Contents lists available at ScienceDirect

Journal of Financial Stability

journal homepage: www.elsevier.com/locate/jfstabil

When central banks buy corporate bonds: Target selection and impact of the European Corporate Sector Purchase Program☆

Rients Galema*, Stefano Lugo

Utrecht School of Economics (USE.), Utrecht University, Kriekenpitplein 21-22, 3584 EC Utrecht, the Netherlands

ARTICLE INFO

Article history:
Received 17 April 2020
Received in revised form 20 April 2021
Accepted 21 April 2021
Available online 5 May 2021

JEL Classification:
G18
G28
G32

Keywords:
Corporate Sector Purchase Program
Corporate bonds
Quantitative Easing
Capital structure
Debt maturity

ABSTRACT

We study the timing of the European Corporate Sector Purchase Program and its direct effect on corporate financing decisions. Consistent with the goal of reducing credit premia, more timely purchases are observed for eligible bonds characterized by higher credit risk. Firms effectively targeted increase their relative use of market debt and the maturity of newly issued bonds more than eligible but not (yet) targeted issuers. The estimated effect is not driven by the verified relation between selection and credit risk. The program has fostered the ability to tap credit markets directly especially for eligible corporations whose bonds are actually purchased.

1. Introduction

During the last decade, central banks have implemented several Quantitative Easing (QE) programs to provide markets with extra liquidity and foster growth and inflation in low interest rate environments. After expanding its lending operations, the European Central Bank (ECB) announced in 2012 its intention to start buying securities directly. Since then, several Asset Purchase Programs (APPs) have been introduced, allowing the ECB to buy government bonds (PSPP), asset-backed securities (ABSPP), and covered bonds (CBPP3). Whereas this first round of QE has contributed to a significant reduction in market rates (Koijen et al., 2019), non-financial firms' limited access to credit remained a concern in Europe. For this reason in March 2016 the ECB announced its intention to start buying corporate bonds directly. The Corporate Sector Purchase Program (CSPP) officially started on June 8th, 2016.

In this study we assess the European Corporate Sector Purchase Program (CSPP) to address two sets of questions. First, how do central banks decide which corporate bonds to purchase? Are all bonds targeted as soon as they become eligible, or are there specific selection criteria determining which bonds are the object of more timely purchases? Second, and most importantly, does this selection matter? Is there a *differential*, *direct* impact on the financing decisions of targeted firms versus eligible but not (yet) targeted firms, or are all firms with eligible outstanding bonds equally affected by the program?

When selecting which securities to purchase from the eligible universe, the six national central banks in charge of the CSPP operations face a series of trade-offs, one of which concerns credit risk. On the one hand, the purchase of corporate bonds by central banks implies that credit risk (Benigno and Nistico, 2015) and interest rate risk (Christensen et al., 2015; Del Negro and Sims, 2015) are transferred from the private sector to the central bank's balance sheet. The ECB mentions the potential deterioration of credit quality and the possibility of defaults as explicit risks linked to the program.[1] Officially, these risks are contained by limiting the universe of

☆ For helpful discussions and suggestions, we thank Iftekhar Hasan (the editor), two anonymous referees, Sweder van Wijnbergen, Jennie Bai, Andrea Schertler (discussant), Andrew MacKinlay (discussant), Jannic Cutura (discussant), Aytekin Ertan, and participants at seminars and sessions at the Dutch Central Bank, Utrecht University, the FMA 2018 European conference, the EFMA 2018 annual meeting, and the 2018 Corporate Finance day. All remaining errors are the sole responsibility of the authors.

* Corresponding author.
E-mail addresses: R.J.Galema@uu.nl (R. Galema), S.Lugo@uu.nl (S. Lugo).

[1] http://www.ecb.europa.eu/mopo/implement/omt/html/cspp-qa.en.html

https://doi.org/10.1016/j.jfs.2021.100881
1572-3089/

eligible bonds to investment grade (IG) securities. Yet, the riskiest IG securities can already entail a substantial level of credit risk exposure. For example, in 2017 the ECB booked a loss by selling its holdings of bonds issued by Steinhoff International Holdings when a default of the company appeared to be a concrete possibility.[2] As such, in performing these unconventional operations a central bank may want to limit as much as possible the credit risk in its expanding balance sheet.

On the other hand, the main explicit goal of the CSPP is to reduce credit premia. Purchases of government bonds can affect other asset classes mostly via a "portfolio rebalancing" (e.g., Tobin, 1958, 1969) channel, i.e., investors use the proceeds from the sale of government bonds to buy other assets not directly purchased by the central bank. The portfolio rebalancing channel is expected to be weak for asset classes representing very imperfect substitutes for the asset class targeted under QE measures. Securities significantly different from government bonds, most notably in terms of credit risk, are unlikely to experience a strong positive demand shock in the short-term (Greenwood et al., 2018). As a result, an exogenous increase in the demand for government bonds has a relatively small effect on corporate bond yields (See Krishnamurthy and Vissing-Jorgensen, 2011, for the US case). This argumentation provides a rationale not only to purchase corporate bonds directly, but also to specifically target corporate bonds characterized by a relatively high level of credit risk. Central banks in charge of corporate bond purchases therefore have to weigh concerns over credit risk transferred to their balance sheets against the goal of improving the access to debt capital for non-financial firms; which of the two considerations prevails is ultimately an empirical question.

If central banks systematically purchase certain bonds within the eligible universe sooner than others, not all firms with outstanding eligible bonds may be equally affected at the same time. This is a central point of this article, and one that differentiates it substantially from other studies on the CSPP. There are two reasons why the direct effect on actually targeted firms may be stronger than the general effect experienced by all firms with eligible outstanding bonds. First, firms whose bonds are purchased under the CSPP experience a direct exogenous shock in the demand for their securities (Vayanos and Vila, 2009; Greenwood et al., 2010). If the demand for firms' bonds is not perfectly elastic, e.g. due to market segmentation (D'Amico and King, 2013), the direct effect of this shock experienced by firms whose securities are effectively purchased will be stronger than the indirect effect experienced by eligible but non-targeted firms. Second, and perhaps more important, purchases could constitute a credible signal to already targeted firms that—as long as the CSPP is in place—the central banks will also systematically buy their newly issued eligible bonds, whereas firms not yet targeted cannot rely on central banks directly increasing the demand for their newly issued eligible bonds. As shown in the results section of this article, there is clear ex-post evidence that this is indeed the case. If firms already targeted expect a CB-driven additional demand also for their new bonds should they issue any, this can accentuate the impact of the program on their financing decisions compared to otherwise similar but not yet targeted firms. We predict that targeted firms thus try to exploit the temporarily favorable credit conditions created through these demand shocks, by significantly increasing both their reliance on market debt and the maturity of their market debt, as compared to firms with eligible—but not yet purchased—outstanding bonds.

To test our predictions empirically, we start by analyzing the possible drivers of more timely purchases. We do so by estimating a logit model on a cross-section of bonds eligible during the first few weeks of the program (henceforth referred to as the first wave of the program), and a Cox proportional hazard model on a bond-week panel dataset covering the subsequent period.[3] Results for both sets of analyses indicate that, consistent with the goal of reducing credit premia, a higher level of credit risk is significantly associated with a higher hazard of a first purchase. This result is robust to the use of different proxies for credit risk and to controlling for several bond- and issuer-level characteristics that may act as confounding factors–most notably the share of the whole eligible universe represented by each security.

Do firms effectively targeted modify their financing decisions more than eligible but not (yet) targeted firms? To estimate this direct effect of the CSPP we use a difference-in-differences (DiD) approach and two samples: a firm-quarter panel dataset including all firms with eligible bonds outstanding and a cross-sectional dataset of newly issued corporate bonds. We find that targeted firms increase their use of market debt (relative to other sources of debt capital) significantly more than non-targeted firms. Consistent results are found when assessing the proportion of market debt over total debt outstanding and when, in the spirit of Becker and Ivashina (2014), we study how the program affects the probability that a targeted firm decides to issue a new bond instead of getting a new loan from a bank. Interestingly, we find that any significant heterogeneity in how firms in different rating categories react to the introduction of the program (Grosse-Rueschkamp et al., 2019) disappears once direct treatment is taken into account. There is also no evidence that the direct treatment effect varies with the firm's credit risk. These results corroborate our key point that selection matters: eligible firms with different levels of credit risk do not react differently to the program; they just face different hazard rates of being directly treated.

A number of additional meaningful robustness checks confirm the presence of a direct, differential treatment effect of the program on the debt financing decisions of firms effectively targeted. No strong treatment effect is found on leverage: the program appears to affect predominantly the relative attractiveness of different sources of debt capital (banks vs. credit markets) rather than the benefits and costs of debt capital as a whole. Finally, analyses on a cross-section of newly issued bonds show that the program also has a positive impact on the maturity of bonds issued by targeted firms. We interpret this result as evidence of firms trying to fully exploit the exceptional but temporary favorable conditions for tapping credit markets directly resulting from the CSPP. Our results obtained from security- and issuer-level analyses can help explain some macro trends observed in the data. Over our sampling period the amount of triple-B bonds over all European nonfinancial, investment grade (IG) bonds issued has increase by around 2% points; the average maturity of bonds eligible under the CSPP has increased by almost half a year. The effects of the CSPP on the composition of the European IG corporate bond markets may remain observable well after the program ends.

The rest of this paper proceeds as follow. In Section 2 we further motivate our contribution to the CSPP literature, review the general literature on asset purchase programs, and provide institutional details about the Corporate Sector Purchase Program. The dataset is described in Section 3. Section 4 presents our empirical results. Section 5 concludes.

2. Literature review and institutional background

2.1. Related literature on CSPP

Since the effective introduction of the CSPP, a number of studies have investigated its effects. Two main strands of literature can be

[2] See "The Risks in Central-Bank Balance Sheets Are Clear", Bloomberg, December 7th, 2017. https://www.bloomberg.com/opinion/articles/2017–12–07/the-risks-in-central-bank-balance-sheets-is-clear-mark-grant

[3] As discussed at length in Section 3, data availability on purchases made under the CSPP underpins our decision to divide the sampling period into two waves.

identified. The first one considers the impact of the CSPP on bond pricing. Abidi et al. (2017) use an event study to investigate the *ex ante* effect of the announcement of the CSSP program in March 2016. They distinguish the differential announcement effect on bonds that are eligible versus those that are not eligible, and find evidence of significant announcement effects. During the program, the CSPP reduced yield spreads (Zaghini, 2019) and bid-ask spreads (Todorov, 2019); it also mitigated underpricing (Rischen and Theissen, 2018) of primary issues. Several studies explicitly focus on the (announcement) effect of ECB's APP, and the Public Sector Purchase Program (PSPP; Arrata and Nguyen, 2017).

Our study belongs to a second strand of literature (Grosse-Rueschkamp et al., 2019; Ertan et al., 2020; Arce et al., 2021; Betz and De Santis, 2019; De Santis and Zaghini, 2019) which considers the effect on firms' financing decisions and access to debt capital. The main result emerging from these studies is that firms whose bonds can be purchased under the program decrease their demand for bank loans, which in turn increases the access to debt capital provided by banks to non-eligible firms, i.e., speculative grade corporations (Grosse-Rueschkamp et al., 2019, e.g.) and/or small and medium enterprises with no or very limited direct access to credit markets (Arce et al., 2021; Ertan et al., 2020, e.g.). Our main contribution to this literature is that we focus on the timing and consequences of *actual* purchases by the central banks in charge of CSPP operations.

First, we show that not all eligible bonds are immediately purchased and that the proportionality principle (i.e., banks using the market portfolio as the benchmark for their portfolio allocation) is not sufficient to explain why certain bonds are the object of more timely purchases. To the best of our knowledge no other study so far has looked at the bond-level determinants of purchasing decisions operated under the program.

Second, this verified heterogeneity in target selection and timing allows us to study the effect of the program on financing decisions using eligible but not-yet targeted firms as the reference group. Virtually all aforementioned studies use non-eligible issuers as the control group; all eligible issuers are considered to be equally treated under the program under the assumption of "likely spillovers on bonds that are eligible but have not been purchased" (e.g. Grosse-Rueschkamp et al., 2019, p. 10). Our results are not just a mere confirmation of the general effect on all eligible firms presented by e.g. Grosse-Rueschkamp et al. (2019). We argue and provide evidence for a previously undocumented *direct*, differential effect on firms whose bonds get actually purchased versus eligible but not-yet targeted firms. Importantly, we show that this direct effect is the most likely explanation for any *apparent* difference in how firms in different rating categories seem to react to the program. Once we control for direct treatment, there is no evidence that more risky firms are more heavily affected by the introduction of the program. Finally, we also show that the CSPP impacts not only the reliance on market debt by actually targeted firms but also the maturity of their newly issued bonds.

Ertan et al. (2020) also focus on actual purchases, and in particular on purchases occurring on the primary market. Their analyses however primarily focus on banks as the (indirectly) treated agents of interest. They document a positive relation between purchases by CBs of bonds newly issued by a bank's customers and the amount of credit provided by that bank to small and medium enterprises (SMEs). They interpret this result as evidence that the direct provision of credit by CBs to firms effectively targeted under the CSPP frees bank capital for SMEs. The direct effect of CSPP documented in this study can be seen as supporting their interpretation; directly treated firms do effectively shift from bank loans to bonds more than other eligible firms.

2.2. Related literature on asset purchase programs

This paper is part of a broader literature that studies the impact of central bank asset purchase programs. In frictionless financial markets, assets held by the central bank are perfect substitutes for privately held assets and the monetary policy stance can entirely be described by the current and expected policy interest rate (Wallace, 1981; Eggertsson and Woodford, 2003). Any expansion of the central bank balance sheet has zero impact on asset prices. In such a world, any impact of asset purchases could only be produced indirectly, for instance through a "signaling channel" (Eggertsson and Woodford, 2003) in which asset purchases act as forward guidance.

However, central bank asset purchases could have an impact on asset prices also via a "portfolio rebalancing channel" (Tobin, 1958, 1969; Gertler and Karadi, 2011, 2013), due to the existence of preferred habitat investors combined with limits to arbitrage (Vayanos and Vila, 2009; Greenwood and Vayanos, 2014). These theories predict that asset purchases by a central bank would have an impact on related assets, as investors selling these bonds use the proceeds to purchase substitute assets. Due to market segmentation, the "portfolio rebalancing channel" is expected to be stronger on assets that can be seen as closer equivalents of those purchased by the central bank. The key implication of the portfolio rebalancing channel argument in the context of this paper is therefore that target selection matters (Krishnamurthy and Vissing-Jorgensen, 2011): the demand shock transmitted via the portfolio rebalancing channel toward securities not purchased can be comparable in magnitude with the direct demand shock induced for effectively targeted securities only to the extent that non-targeted securities can be seen by investors as perfect substitutes (e.g., in terms of credit risk and duration) of effectively purchased bonds.

A large literature analyzes central banks' asset purchase programs. For instance, Albertazzi et al. (2016) use holdings data to study the announcement of APP and find that portfolio rebalancing seems to have been an active channel for APP. In general, many contributions find evidence consistent with a portfolio rebalancing channel. One strand of literature empirically analyzes the price impact of asset purchase programs. Eser and Schwaab (2016) analyze the effect of the European Securities Market Program and find evidence for large purchase and announcement effects. This is confirmed by De Pooter et al. (2018) who find that the SMP purchases resulted in lasting reductions in sovereign bonds' liquidity premia. Ghysels et al. (2016) use high-frequency data on purchases of the ECB and sovereign bond quotes to show that SMP interventions have been effective in reducing the yields of government bonds. Joyce et al. (2011) analyze the impact of QE in the UK and find both announcement and purchase effects on the order of 100 basis points. Steeley (2015) also looks at the UK experience and finds that QE operations reduce the costs of trading but produce predictable opportunities to earn excess returns for investors. Kettemann and Krogstrup (2014) find evidence for an announcement effect of Swiss National Bank's covered bond purchase program, even though they do not find evidence for a purchase effect. Finally, Georgiadis and Grab (2016) investigate the impact of announcing the complete Eurosystem APP program on the euro exchange rate, global equity prices and bond yields.

A second strand of empirical literature studies the effect of central bank purchases on the credit cycle. Rodnyansky and Darmouni (2017) show that quantitative easing (QE1 and QE3) increases bank lending in the US. Joyce and Spaltro (2014) and Bowman et al. (2015) find a positive effect of quantitative easing on bank lending in the UK and in Japan, respectively. Lo Duca et al. (2016) find that purchases and holdings of MBS and Treasuries by the Federal Reserve strongly affect gross corporate bond issuance in both advanced and emerging economies. Foley-Fisher et al. (2016) analyze the US maturity extension program (MEP) and find that firms more dependent on long-term debt also issue more long-term debt during the MEP.

2.3. Institutional background on CSPP

In response to the global financial crisis and the European sovereign debt crisis, the ECB initiated a series of unconventional

monetary policy instruments, including Long-Term Refinancing Operations (LTRO) and Asset Purchase Programs (APP). In January 2016 the ECB announced an "expanded asset purchase program", encompassing the existing programs for covered bonds and asset-backed securities. Under this expanded program, the combined monthly purchases of public and private securities were planned to amount to 60 billion Euros.

On March 10, 2016, the ECB decided to further expand the APP from 60 to 80 billion Euros, with an intention to continue the program to at least March 2017. In addition, the ECB introduced the CSPP aimed at the purchase of investment-grade, euro-denominated bonds issued by non-bank corporations. On April 21, 2016, the ECB announced further details on the program. Bond purchases started on June 8, 2016. Between June 2016 and July 2017, net purchases have averaged 7.22 billion euro per months according to official ECB statistics.

By the end of July 2017, the book value of holdings under the CSPP at amortized costs was 102.23 billion euro. The program was later expanded until December 2018, with net monthly purchases reduced to 30 billion euro per month.

The six national Central Banks (CBs) of Belgium, Finland, France, Germany, Italy, and Spain are in charge of purchases on the primary and secondary markets under the CSPP. Each individual CB is responsible for the purchase of corporate bonds from a specific set of countries. Since July 18, 2016 holdings under the CSPP have been made available for lending by the six CBs.[4]

Several detailed eligibility criteria have been set within the program.[5] First, purchased securities must be euro-denominated and eligible as collateral for Eurosystem credit operations according to the guideline ECB/2014/60 and its subsequent amendments. Second, the issuer is incorporated in a member state whose currency is the euro. The country of origin of the ultimate parent of the issuer is not taken into account. Third, the issuer (or its ultimate parent) cannot be a credit institution, or more generally an entity which is subject to banking supervision inside or outside the euro area. Fourth, the debt instrument has a minimum remaining maturity of 6 months and a maximum maturity less than 31 years at the time of purchase. Fifth, an issue needs to have a minimum credit rating of investment grade (i.e. BBB-/Baa3/BBBL) from at least one rating agency. Finally, purchases of assets with a (negative) yield to maturity below the deposit facility rate are permitted only to the extent necessary.[6]

Central banks can purchase bonds both on primary and on secondary markets. To mitigate the potential negative consequences of the program on second market liquidity, "The benchmark applied for purchases proportionally reflects all eligible outstanding issues." [7] We refer henceforth to this general qualitative criteria as the proportionality principle. To the best of our knowledge there is no maximum tracking error allowed, meaning CBs can take other factors into account in their target selection. By the end of the third quarter of 2017 for example triple-B bonds constitute 49% of all bonds held under the CSPP but only 45% of all eligible bonds outstanding.[8] It is nonetheless clear that the proportionality principle is a key factor influencing the general composition of the portfolio of securities held under the program.

The ECB does not publish any purchase volumes *ex ante*, but does publish data on actual holdings *ex post*. It provides the aforementioned aggregate statistics on the total Euro amounts purchased as well as detailed statistics on which bonds it purchases–but without indicating the amount held at the security level. The ECB caps holdings of each individual security to 70% of its outstanding amount; an extra (unspecified) limit per issuer group also applies. This allows for sufficient leeway to build up the portfolio, while ensuring a diversified allocation of purchases across issuers.

3. Data

In this section we first describe the procedure used to identify the universe of bonds eligible under the program and the bonds effectively purchased. Second, we present the four samples used in our analyses.

3.1. Eligible and targeted bonds

3.1.1. Eligible bonds and issuers

To identify a universe of bonds potentially eligible under the CSPP we start from the lists of marketable bonds accepted as collateral for Eurosystem credit operations that are published daily by the ECB.[9] For each week, we consider the latest list–a Friday in all but one case. All weekly data included in this study are accordingly measured as of Friday. We collect weekly lists starting from week 1 of 2015 (2015w1, i.e., 1.5 years before the start of the program) until 2018w18 (the end of our sampling period). The ECB lists include some key characteristics of the bonds such as the ISIN code, the type of instrument and issuer, the issuance and maturity date, and the haircut applied when the bond is used as collateral. Consistent with the CSPP eligibility criteria, we retain from this initial dataset only euro-denominated securities: (a) labeled as bonds (type AT01) or medium-term notes (AT02); (b) issued by corporations (issuer group IG3) and financial corporations other than credit institutions (IG9) residing in one of the EU-19 countries, and; (c) with more than 6 months of maturity left.

The final dataset includes 2414 bonds meeting all of the eligibility criteria for at least 1 week during the sampled period. For each bond, we collect from Thomson EIKON the Organization ID, the RIC code, and the SIC code for both the immediate issuer of the bond and for its parent company. Company-level data are retrieved based on the selected RIC code. Assigning each bond to a unique corporation is non-trivial, as several bonds are technically issued by financial vehicles. Whereas we use the Organization ID as our main company identifier (as it is always available) to identify the ultimate corporation issuing the bond we rely on the RIC and SIC codes. If: (a) the RIC code is available for the parent company of the issuer but not for the issuer itself, and; (b) the immediate issuer is classified as a financial company (based on its SIC code), we then treat the immediate issuer as a vehicle and consider the parent company as the ultimate issuer of the bond. In all other cases, the immediate issuer is considered to be also the ultimate issuer of the bonds. Under this procedure we identify 351 distinct issuers of the securities in our eligible bonds universe.

3.1.2. CSPP bonds

A few weeks into the program, the six national Central Banks (CBs) in charge of market operations under the CSPP started publishing weekly updates of the lists of bonds purchased and available for lending. We hand-collect these weekly updates from the websites of the six central banks to identify which bonds have been effectively purchased, and when they were first targeted. We assume bonds included in a list published on Monday to be already held by the end of the previous week; a bond is therefore considered to be

[4] https://www.ecb.europa.eu/mopo/implement/omt/lending/html/index.en.html
[5] See https://www.ecb.europa.eu/mopo/implement/omt/html/cspp-qa.en.html
[6] https://www.ecb.europa.eu/ecb/legal/pdf/celex_32017d0004_en_txt.pdf
[7] https://www.ecb.europa.eu/mopo/implement/omt/html/cspp-qa.en.html
[8] General aggregated CSPP statistics accessed at https://www.ecb.europa.eu/mopo/pdf/CSPP_breakdown_by_sector_rating_country.xlsx?96e4e336d2e7d3d3e38605a0ba7ca9c3 on October 29th, 2020.
[9] Data available at https://www.ecb.europa.eu/paym/coll/assets/html/list-MID.en.html

purchased for the first time during a given week w if it appears for the first time in the list published on Monday in week w + 1. The earliest week available for all six central banks is week 36 of 2016 (2016w36), which is the week ending with Friday 9 September 2016. We refer henceforth to the period between the start of the program and 2016w36 as the "first wave" of the program; the period between 2016w37 and 2018w18 (the end of our sampling period) is referred to as the "second wave". We can identify the exact week of the first purchase only for bonds targeted for the first time during the second wave.

As illustrated by Fig. 1, the number of bonds held under the CSPP has steadily increased over time. On March 10, 2016 the number of eligible bonds was 1386. It increased steadily to 1559 at the start of the CSPP on June 8, 2016. During the summer of 2016 the number of eligible bonds was fairly constant, with 1574 eligible bonds at the start of the second wave on September 9, 2016. After that, the number of eligible bonds steadily increased until 1762 at the end of our sample, on May 4, 2018. The CSPP started on June 8, 2016 and by September 9, 2016 (i.e., the end of the first wave period), 577 bonds are held under the CSPP. From the start of the second wave we have weekly data showing CSPP holdings steadily increasing until 1072 at the end of our sample (May 4, 2018).

3.2. Sampling

This section documents the samples and main variables used in the different analyses. Table 1 provides summary statistics; each different panel refers to one of the different samples presented in the remainder of this section.

3.2.1. First wave and second wave samples

We use two samples to investigate the drivers of more timely purchases under the CSPP. The first dataset is a cross-section of all bonds eligible under the program as of week 22 of 2016, which is the week before the official start of the program. We exclude observations where the key explanatory variables described below are missing. We refer to this henceforth as the "first wave" sample. The sample includes 1338 eligible bonds issued by 270 distinct firms; 542 of these bonds, issued by 177 distinct firms, are held under the program by the end of the first wave.

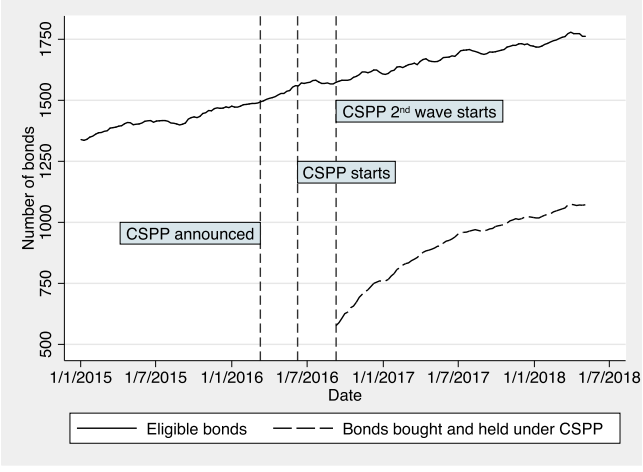

Fig. 1. Eligible bonds outstanding and bonds held under the CSPP. This figure presents the total number of eligible bonds outstanding over time and the number of bonds bought and held under the CSPP. The three vertical dashed lines represent the date the CSPP is announced (10 March 2016), the date the CSPP starts (8 June 2016) and the date the second wave starts (9 September 2016). The second wave of the program is defined as the part of our sampling period for which weekly updates of the lists of bonds held under the CSPP are available for all of the six central banks in charge of the program. Between the start of the program and the start of the second wave, a total of 577 bonds have been purchased under the CSPP.

Table 1
Summary statistics.

	Mean	SD	p5	p95	N
Panel A: First wave sample					
CSPP	0.41	0.49	0.00	1.00	1338
Haircut	14.50	10.90	2.00	36.00	1338
Rating	20.45	2.41	17.00	25.00	1246
Bid-Ask Spread	0.60	0.95	0.08	1.52	1338
% of investable	0.07	0.06	0.01	0.15	1338
Ln(1+DTM)	7.42	0.87	5.75	8.70	1338
LNTA	17.74	1.32	15.36	20.01	1016
D/E	0.89	0.83	0.10	2.40	1000
Q	1.30	0.43	0.89	2.10	999
Bond ratio	0.41	0.25	0.04	0.97	1016
Panel B: Second wave sample					
Haircut	13.36	9.45	1.00	31.00	41,077
Bid-Ask Spread	0.74	1.54	0.07	2.55	41,077
Rating	20.58	2.36	17.00	25.00	35,916
% of investable	0.04	0.03	0.00	0.10	41,077
Panel C: Firm-quarter panel sample					
Bond ratio	0.36	0.26	0.02	0.84	2768
LNTD	15.81	1.31	13.95	17.92	2768
Treatment	0.48	0.50	0.00	1.00	2768
Target	0.84	0.36	0.00	1.00	2768
Bonds issued	0.17	0.38	0.00	1.00	2768
LNTA	17.09	1.41	14.93	19.44	2229
LNPPE	15.35	2.00	11.96	18.13	2216
D/E	0.82	1.05	0.11	1.94	2216
Q	1.38	0.51	0.91	2.39	2097
Lagged return	0.09	0.23	-0.29	0.44	2485
Short-term debt ratio	0.21	0.17	0.02	0.50	2039
BBB-rated	0.56	0.50	0.00	1.00	2768
A-rated	0.27	0.44	0.00	1.00	2768
Bond versus loan	0.72	0.45	0.00	1.00	827
Panel D: Bonds issuance sample					
Maturity buckets (1–5)	3.21	0.82	2.00	4.00	612
Treatment$_w$	0.43	0.49	0.00	1.00	612
Target	0.78	0.41	0.00	1.00	612
First purchase	0.06	0.23	0.00	1.00	612
Rating	19.71	1.87	17.00	24.00	612
Ln(1+AOS)	13.18	0.77	11.51	14.04	612
LNTA	17.47	1.35	15.19	19.35	489
LNPPE	15.80	2.12	11.05	18.12	470
D/E	0.77	0.58	0.09	1.76	492
Q	1.38	0.52	0.91	2.50	460
Lagged return	0.08	0.22	-0.29	0.42	551

This table reports descriptive statistics for the variables included in each of the four samples used for the analyses presented in Table 2 (Panels A and B); Tables 4–7 (Panel C); and Table 8 (Panel D). Definitions of all variables are summarized in Table A.1 of the Appendix.

The second sample is a bond-week panel of eligible bonds covering the subsequent period until May 4, 2018 (week 18 of 2018, or 2018w18). Bonds already targeted during the first wave are excluded from this sample. Bonds exit the sample when they stop being eligible or when they get purchased under the program. Bonds issued during the sampling period also enter the dataset once eligible. We refer to this as the "second wave" sample and it includes 990 eligible bonds issued by 235 distinct firms. In our sample central banks purchase for the first time during the second wave 317 eligible bonds issued by 131 distinct firms.

The dependent variable for the analyses based on the first wave sample is a time-invariant indicator (*CSPP*) equal to one for bonds effectively purchased under the program before September 9, 2016 and zero otherwise. Analyses for the second wave focus on the number of weeks passed before the first purchase under the program. Eligible bonds issued during the second wave become at risk of being purchased since issuance.

We consider a number of bond-level and issuer-level explanatory variables in our targeting analyses. All bond-level variables are measured

as of week 22 of 2016 in the first wave sample and as 1-week lags for the second wave sample. 2015 accounting data are used for firm-level variables (source: Worldscope, retrieved via Datastream).

To measure the level of credit risk, we use two different proxies. The first one is the haircut (*Haircut*) applied to the security when used as collateral in Eurosystem credit operations (source: ECB), expressed as a percentage over the value of the bond. Higher values of *Haircut* indicate a higher level of credit risk. The second one (*Rating*) is the bond's long-term domestic rating expressed in a numerical scale (source: Thomson EIKON). The scale for Investment Grade securities ranges from 17 (Baa3/BBB-) to 27 (AAA/Aaa); when multiple ratings are available, we consider the highest rating among those assigned by Moody's, S&P, and/or Fitch, which is consistent with CSPP policies on eligibility.[10] Lower values of *Rating* indicate a higher level of credit risk.

We control for the bond's liquidity using the bid–ask spread, which is also the most commonly used proxy in the literature (e.g., Chen et al., 2007). The variable *Bid-Ask Spread* is defined as the difference between the Thomson's composite ask price (*CMPA*; source: Datastream) and the composite bid price (*CMPB*), expressed as a percentage of the mid–price. From a theoretical point of view, the expected relation between liquidity and the likelihood of a more timely purchase is non-trivial. On the one hand, liquidity is one the main factors driving the yields of fixed income securities (e.g., Duffee, 1999; Longstaff et al., 2005; Huang and Huang, 2012). Purchases of less liquid bonds may hence have a stronger impact on yields (e.g. Arrata and Nguyen, 2017) by reducing the liquidity premium asked for these bonds (Joyce et al., 2011). On the other hand, purchasing less liquid bonds may be more difficult in practice, and may also hamper the price discovery process to the extent that all (or most of) the effective trading involves a central bank as the buyer of the bond.

To account for the proportionality principle discussed in Section 2.3, we control for the relative size (at face value) of each bond over the whole eligible universe (*% of investable*), expressed in percentage points. According to the proportionality principle, we expect more timely purchases for bonds representing a larger share of the eligible universe. We proxy for the bond maturity using the natural logarithm of one plus the time to maturity expressed in days ($\text{Ln}(1 + DTM)$; source: ECB).

To proxy for the size of the issuer, we use the natural logarithm of total assets in thousands of euro (*LNTA*). To account for growth opportunities we use Tobin's *Q*, defined as the sum of the market value of equity and the book value of total assets, minus the book value of equity and deferred taxes, all divided by total assets.

Two variables are considered to account for the capital structure of the issuer. The first one is the ratio between the book value of debt and the market value of equity (D/E). The second variable, *Bond ratio*, is used to proxy for the ability of the issuer to resort to long-term bonds as a source of debt financing.[11] The variable is computed for each issuer as the ratio of the total amount of outstanding bonds and medium-term notes in the ECB database (*AOS*; measured by the end of 2015), to the book value of debt by the end of 2015.

3.2.2. Firm-quarter panel sample

To investigate if there is a direct effect of the CSPP on effectively targeted firms, we apply a DiD approach using a firm-quarter panel sample covering the period from quarter 1 of 2015 to quarter 2 of 2018. The sample includes only companies with at least one eligible bond (referred to as eligible firms). The sample includes 2768 observations for 239 distinct firms.

To measure firms' choices between bank loans and market debt, we use two different variables. First, we consider the proportion of market debt over total debt outstanding using *Bond ratio* as defined in Section 3.2.1. The variable is in this case measured at a quarterly frequency. Second, in the spirit of Becker and Ivashina (2014) we construct an indicator varying by firm and quarter (*Bond* versus *loan*) which equals one if a firm issues a new eligible bond in that quarter, equals zero if it receives a loan from a bank in that quarter, and it is set to missing if a firm does neither or both. Firms only do both in about 5% of observations. Information about the issuance of new bonds comes from the initial universe of eligible bonds discussed in Section 3.1.1. A list of new bank loans granted to the firms in the sample is retrieved from the Dealscan database, accessed via Thomson EIKON.

Our key explanatory variable of interest is *Treatment*. The variable indicates the presence of a direct treatment under the program. It is a time-varying indicator set equal to one when a firm has one or more of its bonds already purchased under the program by the end of the quarter, and set to zero as long as a firm is not (yet) targeted. *Target* is a time-invariant indicator equal to one for firms effectively targeted by the end of our sampling period and *Post* is a firm-invariant indicator equal to one from the start of the CSPP (quarter 3 of 2016) to the end of our sample period and set equal to zero otherwise. It is important to notice that *Treatment* is not defined as the product between *Target* and *Post*: not every firm targeted by the end of our sampling period becomes treated as soon as the program starts. There are 70 firms whose bonds get purchased for the first time during the second wave of the program.

We include several issuer-level control variables. *Bonds issued* is an indicator equal to one in quarters in which a firm issues a bond and zero otherwise. We use the natural logarithm of property plant and equipment (*LNPPE*) to proxy for tangibility or collateral value, and the 1-year lagged stock market return (*Lagged return*) as a performance measure (Becker and Ivashina, 2014). The ratio of current debt liabilities over total debt (*Short-term debt ratio*) is used to proxy for a firm's need to roll-over its debt. We consider two rating indicators for firms rated BBB and A, respectively, to control for credit risk at the issuer level. AAA/AA is thus the omitted investment grade rating category. Other control variables are as defined in Section 3.2.1. All issuer-level control variables except *Bonds issued* are measured with a one-quarter lag. Panel C of Table 1 provides summary statistics for the variables included in the firm-quarter sample.[12]

3.2.3. Bonds issuance sample

From the universe of eligible bonds described in Section 3.1.1, we build a cross-sectional sample of bonds issued during our sampling period. We exclude bonds issued during the first wave of the CSPP, as we cannot identify the precise week when a firm becomes targeted for the first time. The bonds issuance sample thus includes securities issued between 2015w1 and the start of the program and securities issued during the second wave of the program. After excluding observations where key variables are missing, we are left with 612 bonds issued by 171 distinct firms. The sample includes 238 bonds issued by 110 distinct firms before or at the start of the program, and 374 bonds (139 firms) issued during the second wave; among the latter group, 261 bonds (96 firms) are purchased during the second wave. Akin to Badoer and James (2016), we classify each newly issued bond as part of one of five categories by their initial maturity in years: (1,2),[2,5),[5,10), [10,20) and [20,30). As each observation represents a new bond

[10] A good illustrative example is Telecom Italia; during our sampling period, the company's bonds are rated speculative grade by Moody's and S&P but investment grade by Fitch; by mid-July 2017, 11 of the 14 eligible bonds issued by Telecom Italia have been purchased under the CSPP.

[11] Previous studies (e.g., Cantillo and Wright, 2000; Denis and Mihov, 2003) show that companies that are less likely to face difficulties in raising debt capital are also more likely to tap credit markets directly. *Bond ratio* can therefore be seen as a proxy for the ability of the issuer to collect market debt capital.

[12] Descriptive statistics for two sub-samples split according to the value taken by the variable *Target* are presented in Table B.1 of the Appendix.

Table 2
Target selection and timing.

	Logit 1st wave				Second wave duration	
	(1)	(2)	(3)	(4)	(5)	(6)
Haircut	0.024**		0.018**		0.056***	
	[0.009]		[0.009]		[0.009]	
Rating		-0.183***		-0.134		-0.103**
		[0.064]		[0.087]		[0.047]
Bid-Ask Spread	-0.279**	-0.226	-0.512**	-0.476**	-0.172***	-0.113
	[0.131]	[0.139]	[0.199]	[0.201]	[0.057]	[0.072]
% of investable	14.465***	15.815***	22.212***	21.122***	23.696***	21.014***
	[2.771]	[2.436]	[2.714]	[2.651]	[2.151]	[2.097]
Ln(1+DTM)	0.271**	0.451***	0.460***	0.578***		
	[0.116]	[0.102]	[0.124]	[0.118]		
LNTA			-0.274**	-0.232*		
			[0.115]	[0.134]		
D/E			-0.135	-0.223		
			[0.186]	[0.196]		
Q			0.379	0.265		
			[0.282]	[0.304]		
Bond ratio			-0.038	-0.313		
			[0.526]	[0.537]		
Constant	-3.891***	-0.733	-0.661	2.015		
	[1.008]	[1.574]	[2.338]	[2.392]		
Frailty variance					1.219***	0.978***
					[0.231]	[0.195]
Observations	1338	1246	999	946	41,077	36,327
Industry indicators	Yes	Yes	Yes	Yes	No	No
Pseudo R^2	0.114	0.122	0.176	0.162		
N bonds	1338	1246	999	946	990	869
N bonds targeted	542	539	448	446	317	322
N firms	270	252	181	178	235	207
N firms targeted	177	177	138	138	131	131

This table presents coefficients estimates for logit and Cox proportional hazard models for the selection and timing of purchases under the CSPP. The first four columns refer to logit models estimated on a cross-sectional sample including all bonds outstanding and eligible as of 2016w22 (just before the start of the program); the dependent variable is an indicator equal to one if the bond has been purchased under the CSPP by 2016w36 (included) and zero otherwise. Bond-level variables are measured on 2016w22, whereas issuer-level variables based on accounting variables (i.e., those included only in Models 3 and 4) are measured at the end of 2015. Industry indicators are based on 1-digit SIC codes. Robust standard errors clustered by firm are reported in square brackets. Models (5) and (6) refer to duration models estimated on a bond-week panel sample including eligible bonds not yet targeted as of 2016w37, as well as eligible bonds issued subsequently. Securities are considered at risk of being targeted since 2016w37, or since issuance for bonds created subsequently. Each security exits the analysis when it is purchased for the first time, when it becomes no longer eligible, or at the end of the sampling period (2018w18). Unobserved issuer-level effects are modeled as the result of a gamma-distributed latent variable (shared frailty model). *N bonds* (*N targeted*) is the number of distinct (purchased) bonds in the sample; *N firms* is the number of distinct firms in the sample; *N firms targeted* is the number of distinct firms in the sample whose bonds are purchased. Definitions of all variables are summarized in Table A.1 of the Appendix. *, **, and *** identify statistically significant coefficients respectively at the 10%, 5%, and 1% confidence level.

issuance, our main explanatory variable ($Treatment_w$) in this case is set equal to one when the issuer has been already targeted under the program by the end of the week before the bond is issued, and zero otherwise. There are a few cases where an issuer has its newly issued bonds purchased under CSPP for the first time and in the same week they are issued, presumably on the primary market. We control for these special cases using an indicator (*First purchase*) equal to one if the bond is purchased the same week it is issued and if it is the first bond targeted under the program for that issuer; the indicator is set equal to zero otherwise. The sample includes *Rating* and the natural logarithm of 1+*AOS* as bond-level control variables. Issuer-level control variables—described in previous sections—are measured as by the end of the last quarter before the bond is issued. Panel D of Table 1 provides summary statistics for the variables included in the bonds issuance sample.

4. Empirical results

In this section we present our empirical results. Analyses of the determinants of target selection and timing of CSPP purchases are discussed in Section 4.1. In Section 4.2 we assess to what extent firms already targeted under the program can realistically expect their newly issued bonds to be also systematically purchased under the CSPP. Sections 4.3–4.5 are devoted to assessing the impact of the program on the choice by targeted firms between markets and banks as their preferred source of debt capital. Section 4.6 analyses the impact of the program on the total amount of debt capital used by targeted firms. Section 4.7 focuses on the impact of the program on the maturity of newly issued bonds. Finally, in Section 4.8 we discuss some macro trends in the European IG corporate bond markets and how they may relate to our micro-level results.

4.1. CSPP bond selection and timing

We use two different classes of econometric models to investigate the drivers of bond selection and timing of purchases under the CSPP. For the first wave of the program (i.e., up to and including week 36 of 2016) we use logit models as the one presented in Eq. (1).

$$logit\,(\pi_i) = \alpha + \beta CR_i + \sum \gamma_t X_{v,i} + \varepsilon_i \quad (1)$$

where π_i is the probability of bond i being purchased by the end of the first wave conditional on its credit risk (*CR*, set equal to either *Haircut* or *Rating*) and the other observable factors included in the model (X_v, with v varying from 1 to V and V being the total number of control variables considered). The first four columns of Table 2 present coefficients estimates for these logit models.

Models (1) and (2) are the most parsimonious, including only bond-level proxies for credit risk, liquidity, relative size and time to maturity. *Haircut* is used to proxy for credit risk in Model (1) whereas *Rating* is used in Model (2). Both models include *Bid-Ask Spread* as a proxy for liquidity. In addition, both models include *% of*

investable to control for the proportionality principle and *ln* (1 + *DTM*) to control for the time to maturity of the bond. All models include industry (at the 1-digit SIC code level) indicators. Models (3) and (4) augment Models (1) and (2) respectively by adding issuer-level control variables, including *LNTA*, *D/E*, Tobin's *Q* and the *Bond ratio*.

To fully exploit the panel nature of the data, for the period from 2016w37 to 2018w18 we estimate Cox proportional hazard models. For the second set of analyses, as it is customary (e.g., Lugo et al., 2015) we estimate a shared frailty model with a gamma-distributed latent effect at the issuer level to account for the time-invariant characteristics of the issuer. The last two columns of Table 2 present estimates for the duration analysis performed on the second wave sample.

Consistent with the program's goal of reducing credit premia, we find credit risk to be positively associated with the hazard of a first purchase. The estimated coefficients for *Rating* (*Haircut*) are always negative (positive), and statistically significant at the 5% confidence level for all models with the exception of Model (4). The effect is also economically relevant; given the coefficient estimates for Models (2) and (4), a one-notch better rating is associated with a 3.8% decrease in the probability of being targeted during the first wave. According to Model (6) estimates for the second wave, a 1-notch higher rating is associated with a decrease in the purchase hazard by 10.3%.[13]

Larger and more liquid bonds also appear to be the target of more timely purchases, all else equal. Estimated coefficients for *Bid-Ask Spread* are always negative and statistically significant at customary confidence levels in most model specifications. This can reflect the fact that liquid bonds are merely easier to purchase in large quantities, as well as the desire by CBs to limit potential distortions in the price discovery process. The estimated coefficient for *% of investable* is always positive and highly significant, which is fully coherent with the proportionality principle discussed in Section 2.3.

4.2. First purchases as a commitment signal

Before moving to the analyses on the differential impact of the CSPP on the financing decisions of targeted versus non-targeted firms, it is important to address to what extent a first purchase can be seen—at least ex post—as a credible commitment by the central banks in charge of CSPP operations to target also new bonds issued during the program by the same firms. Our argument for a *direct* impact of the program is underpinned by the idea that the exogenous, direct increase in the demand brought by the program mostly affects firms whose bonds are actually purchased under the CSPP. If that is the case, to observe a difference in the financing decisions of targeted versus non-targeted firms one would assume that targeted firms can credibly expect to see also their new securities being purchased once they become eligible, whereas not-yet-targeted firms cannot. To check to what extent this holds, we look at the share of eligible bonds issued during the second wave that get effectively purchased under the program, and how this share varies depending on whether the issuer was already targeted by the end of the first wave or not. Statistics are reported in Table 3.

There are 374 eligible bonds issued during the second wave; 261 of those get purchased under the CSPP by the end of our sampling period. Interestingly, almost 90% of these 261 bonds are already held under the CSPP by the end of the calendar week during which they are issued, which we interpret as evidence of targeted bonds getting almost systematically purchased already in the primary market. 191 of these newly issued bonds are issued by firms already targeted by the end of the first wave: 100% of them get purchased under the CSPP. It is therefore clear, at least ex-post, that targeted firms can rely on central banks systematically purchasing any new eligible bond they issue–and that in the vast majority of cases they do so already on the primary market.[14] In contrast, only 70 of the 183 bonds (38%) issued by firms not yet targeted by the end of the first wave do get effectively purchased during the second wave. Firms eligible but not yet targeted do not face high chances that their newly issued bonds will instead be purchased by CBs.

One could argue that the statistics presented in Panel A may be driven entirely by relatively risky bonds if the latter are always systematically purchased. Statistics separated by rating category, presented in Panel B and C, suggest however that this is not the case. Being already targeted—not being in a specific rating group—is what appears to be associated with an ex-post 100% chance of newly issued bonds getting purchased.

4.3. CSPP and substitution of debt-financing towards bonds

To address the relation between CSPP purchases and corporate debt financing decisions, using the firm-quarter panel dataset covering the period 2015q1–2018q2 we estimate a series of DiD models as the one presented in Eq. (2).

$$BondRatio_{f,t} = \alpha + \beta Treatment_{f,t} + \zeta_f + \delta_t + \sum \gamma_v X_{v,f,t} + \varepsilon_{f,t} \quad (2)$$

where ζ_f and δ_t are firm and time fixed effects (or the effects of *Target* and *Post* in the simplest models). *Bond ratio*, i.e., the share of the total amount of outstanding bonds and medium-term notes in the ECB database to the book value of debt. The variable *Treatment* is our main explanatory variable of interest; its coefficient (β) represents the direct, differential effect of the CSPP on the financing decisions of actually targeted firms versus eligible but not targeted (yet) firms. Coefficients estimates are presented in the first three columns of Table 4.

Model (1) is the most parsimonious, including only *Treatment*, *Target*, and *Post* among the covariates. In Model (2) *Target* and *Post* are replaced by firm and time fixed effects; the model also includes a set of control variables measured at the issuer-quarter level. Model (3) augments Model (2) by including the two rating indicators (*BBB-rated* and *A-rated*) and the variable *Bonds issued*. Rating indicators control for changes in credit risk over time. *Bonds issued* is included to deal with a potential omitted variable bias that may affect our estimates of the treatment effect. Consider a firm not yet targeted that issues a new bond in a given quarter. Central banks, who have not targeted any outstanding bond of that firm up to that moment, may decide that this newly issued bond is instead worth purchasing. Issuing a bond may result in those cases in both an increase in the relative use of market debt and an increase in the probability of becoming treated, leading toward a spurious positive correlation between *Treatment* and the dependent variable. The inclusion of *Bonds issued* among the covariates should strongly alleviate this potential concern. Finally, in the spirit of Ertan et al. (2020), Model (3) includes industry-quarter-year indicators instead of quarter-year indicators to control for potential differences in trends between industries. Industries are defined based on the 1-digit SIC code of the issuer.

[13] As a robustness check, we re-estimate our target selection models excluding each of the six CBs in charge of CSPP purchases. Results, presented in Table B.2 of the Appendix, suggest that no individual CB alone drives our main results on target selection)

[14] CBs' purchases taking place already on the primary market would be highly consistent with our prediction for a direct demand effect on effectively targeted firms; strictly speaking however it is not a required condition for our prediction to hold. If investors other than CBs expect the latter to systematically purchase bonds issued by already targeted firms, they could themselves demand more of those bonds on the primary market as a consequence.

Table 3
Tabulation of second wave bond issues.

Panel A: Full sample				
		2^{nd} Wave Purchased		
		No	Yes	Total
1^{st} Wave	No	113	70	183
targeted	Yes	0	191	191
	Total	113	261	374
Panel B: BBB-bonds				
		2^{nd} Wave Purchased		
		No	Yes	Total
1^{st} Wave	No	73	42	115
targeted	Yes	0	103	103
	Total	73	145	218
Panel C: > BBB-bonds				
		2^{nd} Wave Purchased		
		No	Yes	Total
1^{st} Wave	No	40	28	68
targeted	Yes	0	88	88
	Total	40	116	156

This table provides in Panel A a tabulation of 374 bonds issued during the second wave according to: whether the bond get purchased under the CSPP or not (2^{nd} wave purchased; columns); and whether the issuer of the bond is already targeted under the CSPP by the end of the first wave or not (1^{st} wave targeted; rows). Panels B and Panel C split the tabulation in respectively a subsample of BBB-bonds and a subsample of bonds with a rating higher than BBB.

The effect of *Treatment* is consistently positive and statistically significant at customary confidence levels across all models. Treated firms increase their share of market debt over total debt by 4–5% points more than non-treated firms, a substantial differential increase when compared with the sample mean level (36%). It is interesting to notice how the estimated coefficient for *Post* is negative and not statistically significant from zero: on average, there seems to be no increase in the relative use of market debt for eligible but not targeted firms. The estimated coefficient for *Target* is instead positive and significant: treated and non-treated firms differ significantly in their relative use of market debt before the start of the CSPP. As we show in Section 4.5, despite this difference in level there is no indication of a divergence taking place before the program–i.e., the common trend assumption holds.

The empirical approach deployed for Models (1) to (3) is exposed to two potential criticisms. First, treatment may correlate with the unobservable need/ability to raise debt capital. Second, as capital structure choices are persistent over time our estimates may in principle still be partially affected by a reverse causality bias. As a robustness check, in Models (4) to (6) we thus follow the approach proposed by Becker and Ivashina (2014) and assess the effect of the program on the financing decisions of targeted firms by estimating logit models for the decision to raise new debt capital by issuing a bond or asking a bank for a loan in a given quarter. The observable outcome of interest is an indicator (*Bond* versus *loan*) equal to one

Table 4
Targeted issuers and debt financing.

	Bond ratio			Bonds versus loans		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.050*	0.043***	0.045**	1.553***	1.266*	2.321*
	[0.029]	[0.016]	[0.020]	[0.326]	[0.685]	[1.287]
Target	0.125***			0.281	0.348	0.562
	[0.040]			[0.276]	[0.474]	[0.709]
Post	-0.010			-0.466*	0.127	
	[0.025]			[0.262]	[0.617]	
LNTA		-0.016	-0.046		-0.167	-0.391*
		[0.031]	[0.055]		[0.165]	[0.224]
LNPPE		-0.003	0.010		0.151*	0.370***
		[0.019]	[0.022]		[0.090]	[0.107]
D/E		-0.009	-0.008		-0.086	-0.021
		[0.006]	[0.005]		[0.139]	[0.199]
Q		0.050*	0.049		-0.113	-0.355
		[0.029]	[0.035]		[0.317]	[0.474]
Lagged return		-0.052**	-0.042*		0.906	1.531*
		[0.022]	[0.025]		[0.627]	[0.887]
Short-term debt ratio		-0.030	-0.055		1.549	-0.465
		[0.040]	[0.055]		[1.093]	[1.329]
Bonds issued			0.040***			
			[0.008]			
Constant	0.237***	0.612	0.957	0.434*	1.216	0.806
	[0.035]	[0.559]	[0.898]	[0.229]	[2.218]	[3.904]
Observations	2768	1698	1593	827	351	213
Adjusted R^2	0.052	0.906	0.909			
Pseudo R^2				0.070	0.124	0.236
Quarter-year indicators	No	Yes	No	No	No	No
Firm fixed effects	No	Yes	Yes	No	No	No
Rating indicators	No	No	Yes	No	Yes	Yes
Industry-quarter indicators	No	No	Yes	No	No	Yes
N firms	239	218	209	292	156	121
N treated firms	195	180	177	192	114	82

This table reports coefficient estimates for linear models of the proportion of market debt over total debt (Models (1) to (3)) and logit models of the choice between issuing a new bond or raising a bank loan (Models (4) to (6)). All models are estimated on a firm-quarter panel dataset of firms with eligible outstanding bonds observed from 2015q1 to and including 2018q2. The dependent variable is *Bond ratio* for linear models and *Bond* versus *loan* for logit models. The sample used to estimate Models (4) to (6) excludes firm-quarter observations where no debt is raised or where the firm both issues a bond and raises a loan. All issuer-level control variables except *Bonds issued* are lagged one quarter and measured at the end of the quarter. Models (2),(3),(5) and (6) include rating indicators for BBB and A; AAA/AA is the omitted rating category. *N firms* indicates the distinct number of firms in the sample. *N treated firms* indicates the number of distinct firms in the sample for which *Target* = 1. Robust standard errors clustered by firm are reported in square brackets. Definitions of all variables are summarized in Table A.1 of the Appendix. *, ** and *** identify statistically significant coefficients respectively at the 10%, 5%, and 1% confidence level.

when a firm issues a bond in a given quarter and zero if a firm receives a bank loan. We exclude firm-quarter observations if the firm does not raise new debt capital or taps both banks and markets in the same quarter. By construction we thus condition on a firm being willing and able to raise debt capital in a given quarter.

Model (5) augments Model (4) by including control variables and two rating indicators (*BBB-rated* and *A-rated*). Model (6) also includes quarter-year-industry indicators based on 1-digit SIC codes instead of *Post*. By construction *Bond issued* is always equal to one when the dependent variable is equal to one; the variable is therefore not included in these logit models. As expected, we find a significant positive treatment effect of a firm being already targeted under the program on its likelihood of issuing a bond instead of asking a bank for a loan (conditionally on raising debt capital). For Models (4) and (5), average marginal effects (unreported in the table) indicate that the treatment results in a differential increase in the likelihood of opting for a bond in the range of 22–28%.[15] In summary, both sets of analyses provide empirical evidence for a differential, direct effect on the financing decisions of firms effectively treated by the CSPP: once they are treated these firms tilt toward the use of market debt significantly more than eligible but non-targeted firms.

4.4. Selection and treatment

Grosse-Rueschkamp et al. (2019) analyze the effects of the CSPP focusing on an "intention-to-treat" (ITT) effect on eligible firms produced by the introduction of the program. In principle, what we interpret as the direct treatment effect of the CSPP could just be generated by a general ITT effect on all eligible firms. If the firms more likely to experience more timely purchases (i.e., more risky firms) are also the firms more likely to change their financing decisions once the program is introduced–regardless of whether their own bonds are actually purchased–this would generate a spurious positive correlation between *Treatment* and the outcome of interest. However, for this alternative interpretation to be valid one would expect the estimated treatment effect to disappear once we control for potential differences in how firms with different ratings react to the introduction of the program. As shown in Table 5, the opposite is true: any significant heterogeneity in how firms in different rating categories react to the introduction of the program disappears once direct treatment is taken into account.

To control for heterogeneous responses to the introduction of the program, the models presented in Table 5 include two interaction terms between *Post* on the one hand and two rating indicators for A-rated firms and BBB-rated firms, respectively, on the other hand. AAA/AA is the omitted rating category. Models (1), (3), and (5) are specified as Models (2), (4), and (6), respectively, but they do not include *Treatment* among the regressors. The estimated coefficients for the two new interaction terms are statistically significant at customary confidence levels *only* for models that do not include *Treatment*. The estimated coefficients for *Treatment* are instead still significant at least at the 5% confidence level when the two new interaction terms are included in the models. The coefficients for *Treatment* are also remarkably similar in magnitude to those estimated for the otherwise similar models presented in Table 4. As a further check, we also investigate whether firms in different rating categories are more likely to issue bonds instead of getting a bank loan once the program starts and whether firms in different rating categories react differently to direct treatment. The results of these analyses, presented in respectively Tables B.4 and B.5 of the Appendix, are fully consistent with the idea that direct treatment, and not the level of credit risk of the firm, is what drives a stronger response to the program in terms of market versus bank debt financing choices. More risky eligible firms may appear to be more affected by the program; our results suggest this is mostly due to a higher chance of being directly treated rather than to a different response to an otherwise identical treatment.

4.5. Dynamic effect of CSPP on debt-financing towards bonds

To give a causal interpretation to the results presented in Table 4, targeted and non-targeted firms should not show a diverging trend in *Bond ratio* already in place before the former group is actually treated. To test for this common trend assumption and better assess the dynamics of the effect of the CSPP on firms financing decisions, we re-estimate Model (4) from Table 4 and include several lead and lag indicators for the presence of a treatment (Autor, 2003; Angrist and Pischke, 2008). Specifically, we include a set of indicators ($Treatment_{t+i}$) each equal to one only in quarter $t+i$, where t indicates for each firm the quarter during which it becomes effectively targeted under the CSPP, and equal to zero otherwise. We consider seven indicators corresponding to values for i equal to −4, −3, −2, −1, 0, 1, and 2. Indicators associated with negative values of i are used to test for the presence of a common trend in *Bond ratio* before the treatment actually takes place. Indicators associated with non-negative values of i capture the dynamics of the abnormal change in *Bond ratio* observed once a firm becomes treated. We also include in our analyses a "residual" treatment indicator ($Treatment_{q\geq X}$) covering the remaining post-treatment quarters. The indicator is set equal to one for every treated firm in every quarter $q \geq X$, where $X = t + i_{max} + 1$ and i_{max} is the highest i associated with indicators $Treatment_{t+i}$ effectively included in the model. In all other cases, $Treatment_{q\geq X}$ is set equal to zero.[16] We consider four general model specifications corresponding to values for X equal to t, $t+1$, $t+2$, and $t+3$. Every model includes firm fixed effects and quarter-year indicators, as well as all of the control variables included already in Model (2) of Table 4.

Table 6 presents the results. Model (1) focuses exclusively on testing for the common trend assumption. We set $X = t$, i.e., we only include indicators $Treatment_{t+i}$ where $i < 0$. In this specific case $Treatment_{q\geq X}$ is equal to *Treatment*, and the estimated coefficient captures the whole treatment effect. The estimated coefficients for indicators associated with negative values of i are not statistically different from zero at customary confidence levels; therefore, the null hypothesis of no diverging trends in *Bond ratio* between treated and non-treated firms before treatment actually takes place cannot be rejected. Notice that, since our control group is composed of eligible but not directly treated firms, this result is not in conflict with the general announcement effect documented by e.g. Grosse-Rueschkamp et al. (2019). The estimated coefficient for $Treatment_{q\geq X}$ is positive and statistically significant at the 5% confidence level, which confirms the presence of a significant direct treatment effect on effectively targeted firms. We perform a number of robustness checks using different combinations of lead and lag indicators for the presence of a treatment. In Model (2) we include $Treatment_t$ and $Treatment_{t-1}$. In Models (3) to (5) we add respectively $Treatment_{t-2}$, $Treatment_{t-3}$ and $Treatment_{t-4}$ to test the robustness of results using different reference years. In Model (6) we take Model (3) and we add $Treatment_{t+1}$; in Model (7) we add $Treatment_{t+2}$ and $Treatment_{t-3}$. $Treatment_{q\geq X}$ is therefore defined using $X = t+1$ for Models (2) to (5), $X = t+2$ for Model (6), and $X = t+3$ for Models (7) and (8). Model (8)

[15] In Table B.3 of the Appendix we present results separated for the likelihood of issuing a bond and for the likelihood of getting a bank loan. By design, these additional analyses are not conditional on the firm raising debt capital. We find some weak evidence that results presented in Table 4 are driven not only by a relatively higher probability of targeted firms to issue a bond, but also by a relatively higher probability of eligible-but-not-targeted firms of raising a loan. The latter result is generally consistent with the evidence presented by Ertan et al. (2020) on the effect of the CSPP on bank lending.

[16] Including *Treatment* instead of $Treatment_{q\geq X}$ in these models would of course lead to the same estimated coefficient. We decide to introduce a new variable with a different name and definition purely for ease of interpretation: in this setting the estimated coefficient for $Treatment_{q\geq X}$ would not necessarily represent the whole treatment effect.

Table 5
Rating category and reaction to the program.

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment		0.048***		0.046**		0.041**
		[0.018]		[0.019]		[0.017]
Post × BBB-rated	0.038**	0.027	0.037**	0.027	0.028*	0.021
	[0.018]	[0.018]	[0.019]	[0.018]	[0.015]	[0.015]
Post × A-rated	0.030*	0.014	0.029*	0.014	0.027*	0.017
	[0.017]	[0.017]	[0.017]	[0.017]	[0.015]	[0.015]
Post	0.002	-0.026				
	[0.016]	[0.020]				
LNTA					-0.018	-0.019
					[0.032]	[0.032]
LNPPE					0.003	-0.003
					[0.018]	[0.019]
Q					0.053*	0.051*
					[0.029]	[0.029]
Lagged return					-0.052**	-0.051**
					[0.023]	[0.022]
Short-term debt ratio					-0.032	-0.032
					[0.041]	[0.040]
D/E					-0.010	-0.009
					[0.006]	[0.006]
Constant	0.341***	0.341***	0.336***	0.336***	0.542	0.652
	[0.005]	[0.005]	[0.009]	[0.009]	[0.551]	[0.561]
Observations	2768	2768	2768	2768	1698	1698
Adjusted R^2	0.889	0.890	0.889	0.891	0.905	0.906
Quarter-year indicators	No	No	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
N firms	239	239	239	239	218	218
N treated firms	195	195	195	195	180	180

This table reports coefficient estimates for linear models of the proportion of market debt over total debt. All models are estimated based on a firm-quarter panel dataset of firms with outstanding eligible bonds observed from 2015q1 to and including 2018q2. The dependent variable is *Bond ratio*. All issuer-level control variables are lagged one quarter and measured at the end of the quarter. Included rating indicators for BBB and A are interacted with *Post*; AAA/AA is the omitted rating category. *Post* is a time indicator equal to one from the start of the CSPP onward and equal to zero otherwise. *N firms* indicates the distinct number of firms in the sample. *N treated firms* indicates the number of distinct firms in the sample for which *Target* = 1. Robust standard errors clustered by firm are reported in square brackets. Definitions of all variables are summarized in Table A.1 of the Appendix. *, ** and *** identify statistically significant coefficients respectively at the 10%, 5%, and 1% confidence level.

Table 6
Dynamics of the treatment effect on debt financing.

	$X = t$	$X = t+1$	$X = t+1$	$X = t+1$	$X = t+1$	$X = t+2$	$X = t+3$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Treatment_{q \geq X}$	0.036**	0.047**	0.045**	0.045**	0.037*	0.052**	0.062**	0.062**
	[0.018]	[0.019]	[0.019]	[0.021]	[0.021]	[0.021]	[0.028]	[0.028]
$Treatment_t$		0.029*	0.022	0.023	0.013	0.020	0.021	0.017
		[0.017]	[0.014]	[0.015]	[0.017]	[0.016]	[0.019]	[0.019]
$Treatment_{t+1}$						0.030*	0.034	0.036*
						[0.017]	[0.021]	[0.021]
$Treatment_{t+2}$							0.048**	0.046**
							[0.023]	[0.023]
$Treatment_{t-1}$	0.009	0.009	0.006	0.009	-0.001	0.006	0.009	0.008
	[0.014]	[0.011]	[0.012]	[0.014]	[0.015]	[0.012]	[0.016]	[0.016]
$Treatment_{t-2}$	0.008		0.006	0.009	0.003	0.008	0.011	0.011
	[0.011]		[0.010]	[0.011]	[0.014]	[0.010]	[0.012]	[0.012]
$Treatment_{t-3}$	0.006			0.007	0.003		0.008	0.009
	[0.011]			[0.011]	[0.012]		[0.012]	[0.012]
$Treatment_{t-4}$					-0.005			
					[0.012]			
Bonds issued								0.027***
								[0.007]
Observations	1335	1629	1441	1335	1159	1441	1242	1242
Adjusted R^2	0.048	0.055	0.055	0.050	0.037	0.057	0.051	0.064
Quarter-year ind.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N firms	213	217	214	213	204	214	212	212
N treated firms	170	179	172	170	160	172	170	170

This table reports coefficient estimates for linear models of the proportion of market debt over total debt. All models are estimated based on a firm-quarter panel dataset of firms with outstanding eligible bonds observed from 2015q1 to and including 2018q2. The dependent variable is *Bond ratio*. For each firm, t indicates the quarter in which it becomes treated under the CSPP. $Treatment_{q \geq X}$ is defined setting $X = t$ for Model (1), $X = t+1$ for Models (2), (3), (4) and (5), $X = t+2$ for Model (6), and $X = t+3$ for Models (7) and (8). $Treatment_{t+i}$ are indicators set equal to one only in quarter $t+i$, and set equal to zero otherwise. Controls include all issuer-level control variables included in Model (2) of Table 4. All models include firm fixed effects and quarter-year indicators. *N firms* indicates the distinct number of firms in the sample. *N treated firms* indicates the number of distinct firms in the sample for which *Target* = 1. Robust standard errors clustered by firm are reported in square brackets. Definitions of all variables are summarized in Table A.1 of the Appendix. *, ** and *** identify statistically significant coefficients respectively at the 10%, 5%, and 1% confidence level.

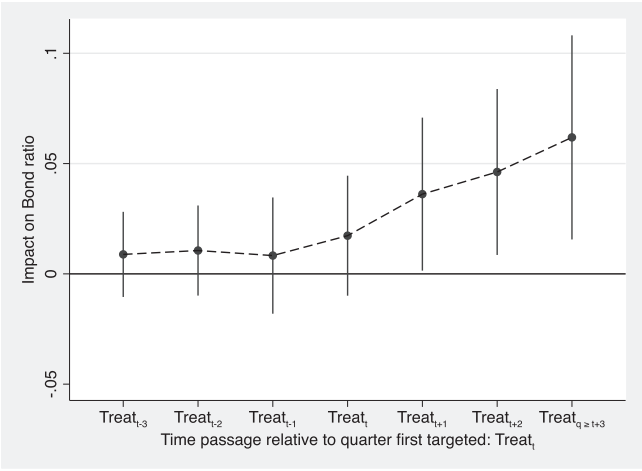

Fig. 2. Estimated dynamic impact of treatment. This figure presents the impact of CSPP purchases on firms' *Bond ratio* and is based on the coefficients estimates presented in Model (8) of Table 6. For each firm, $Treat_t$ is equal to one in quarter t and it is equal to zero otherwise, where t indicates the quarter when the bonds of the firm get purchased under the CSPP for the first time. Indicators named $Treat_{t+i}$, with $-3 \leq i \leq +2$, are set equal to one in quarter $t+i$ and set equal to zero otherwise. $Treat_{q \geq t+3}$ is a time-varying indicator set equal to one for quarters $q \geq t+3$ and set equal to zero otherwise. Vertical bars indicate 90% confidence intervals.

augments Model (7) by also including *Bonds issued* as control variable. The estimated coefficients for indicators associated with negative values of i are never statistically significant at customary confidence levels, again supporting a causal interpretation of our main results. The abnormal change in *Bond ratio* induced by the treatment is not a short-lived effect concentrated in the quarter in which a company becomes treated: across all model specifications estimated coefficients for $Treatment_{t+1}$, $Treatment_{t+2}$, and $Treatment_{q \geq X}$ are mostly positive and statistically significant at least the 10% confidence level.

Fig. 2 presents in graphical form the coefficients estimates for Model (8) of Table 6; bars indicate a 90% confidence interval. It is clear how the trend over time in *Bond ratio* between treated firms and non-treated firms diverges substantially only when targeted firms become effectively treated. In summary, the results presented in this section provide further strong empirical evidence of a direct causal effect of the program affecting specifically the financing decisions of firms effectively targeted.

4.6. Effect of the CSPP on firms' total debt

In the previous sections we show evidence consistent with a CSPP-induced shift towards market debt financing. Do targeted firms also increase their overall use of debt capital as a result? To answer this question, we perform a DiD analysis on our firm-quarter panel dataset, this time taking as the dependent variable either the natural logarithm of total debt (*LNTD*) or the debt-to-equity ratio (*D/E*) as the dependent variable. Table 7 presents the results.

The estimated coefficient for *Treatment* indicates the differential effect on the total debt (Models (1) to (3)) or the debt-to-equity ratio (Models (4) to (6)) of firms targeted by the CSPP. We do not find strong evidence that the CSPP is associated with an additional increase in the use of debt for firms directly treated. The estimated coefficient for *Treatment* is marginally significant (at the 10% confidence level) only for Model (5). There is also no clear evidence on a general increase in the use of debt for all eligible firms since the start of the program. The estimated coefficient for *Post* is not statistically significant at customary confidence levels; it is even negative for Model (1). Considering these results together with those presented in previous sections, we conclude that the CSPP mostly stimulates the use of market debt as a substitute for—rather than a complement to—bank loans.

Table 7
Targeted issuers and leverage.

	LNTD			D/E		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.348	0.031	0.006	-0.066	0.149*	0.153
	[0.231]	[0.042]	[0.047]	[0.096]	[0.081]	[0.097]
Target	0.006			-0.446*		
	[0.300]			[0.243]		
Post	-0.284			0.012		
	[0.192]			[0.087]		
LNTA		0.675***	1.026***		0.077	0.213
		[0.236]	[0.167]		[0.154]	[0.213]
LNPPE		0.484*	0.313*		0.768	0.857
		[0.264]	[0.163]		[0.657]	[0.701]
Q		0.079	0.134		-0.164*	-0.186*
		[0.099]	[0.083]		[0.089]	[0.102]
Lagged return		-0.025	-0.031		-0.351***	-0.304***
		[0.037]	[0.037]		[0.054]	[0.059]
Short-term debt ratio		-0.273	-0.303		0.424	0.500
		[0.215]	[0.198]		[0.509]	[0.530]
Constant	15.737***	-3.188	-6.691	1.205***	-12.113	-16.106
	[0.267]	[5.578]	[4.114]	[0.236]	[9.396]	[11.408]
Observations	2835	1729	1622	2800	1725	1618
Adjusted R^2	0.004	0.974	0.975	0.028	0.835	0.826
Quarter-year indicators	No	Yes	No	No	Yes	No
Firm fixed effects	No	Yes	Yes	No	Yes	Yes
Rating indicators	No	No	Yes	No	No	Yes
Industry-quarter indicators	No	No	Yes	No	No	Yes
N firms	241	219	209	239	219	209
N treated firms	197	181	177	196	181	177

This table reports coefficient estimates for a model of total debt outstanding. All models are estimated based on a firm-quarter panel dataset including all firms with outstanding eligible bonds, observed from 2015q1 to and including 2018q2. The dependent variable is the natural logarithm of the book value of debt (*LNTD*) in Models (1) to (3) and the (book) debt-equity ratio (*D/E*) in Models (4) to (6). All issuer-level control variables except *Bonds issued* are lagged one quarter and measured at the end of the quarter. Models (3) and (6) include rating indicators for BBB and A; AAA/AA is the omitted rating category. *N firms* indicates the distinct number of firms in the sample. *N treated firms* indicates the number of distinct firms in the sample for which *Target* = 1. Robust standard errors clustered by firm are reported in square brackets. Definitions of all variables are summarized in Table A.1 of the Appendix. *, ** and *** identify statistically significant coefficients respectively at the 10%, 5%, and 1% confidence level.

Table 8
Targeted issuers and maturity of newly issued bonds.

	(1)	(2)	(3)	(4)
$Treatment_w$	1.797**	2.239***	2.321*	2.434*
	[0.443]	[0.536]	[1.016]	[1.257]
Target	1.213	1.205		
	[0.294]	[0.369]		
Post	0.645*	0.788		
	[0.163]	[0.245]		
First purchase	0.807	0.812	0.686	0.475
	[0.309]	[0.249]	[0.339]	[0.223]
Ln(1+AOS)	1.349	1.763**	1.330	1.546
	[0.281]	[0.440]	[0.402]	[0.572]
LNTA		0.710**		0.035*
		[0.096]		[0.066]
LNPPE		1.030		10.053***
		[0.074]		[7.094]
D/E		0.689		4.431
		[0.231]		[6.686]
Q		0.722*		0.059**
		[0.133]		[0.065]
Observations	612	396	612	396
Pseudo R^2	0.016	0.049	0.230	0.283
Quarter-year indicators	No	No	Yes	Yes
Firm indicators	No	No	Yes	Yes
Rating indicators	Yes	Yes	Yes	Yes
N firms	171	129	171	129
N treated firms	96	70	96	70

This table reports odds ratios based on estimated coefficients for an ordered logit model of the maturity of newly issued bonds. The cross-sectional sample includes bonds issued between 2015w1 and 2016w22 and bonds issued between 2016w36 and 2018w18. The dependent categorical variable is *Maturity buckets (1–5)*, which identifies the following maturity buckets expressed as years to maturity at issuance: (1,2), [2,5),[5,10),[10,20) and [20,30). Issuer-level control variables are measured at the end of the last quarter prior to issuance. Models (3) and (4) include firm and quarter-year indicators. Rating indicators are included for BBB and A; AAA/AA is the omitted rating category. *N treatment observations* indicates the number of observations in the sample for which $Treatment_w = 1$. *N firms* indicates the distinct number of firms in the sample. *N treated firms* indicates the number of distinct firms in the sample with at least one observation where $Treatment_w = 1$. Robust standard errors clustered by firm are reported in square brackets. Definitions of all variables are summarized in Table A.1 of the Appendix. *, ** and *** identify statistically significant coefficients respectively at the 10%, 5%, and 1% confidence level.

4.7. Effect of the CSPP on firms' issuance maturity

Do targeted firms try to exploit the favorable market financing conditions brought by the CSPP also by issuing bonds with longer initial maturities? To answer this question, we investigate how the program affects the maturity of newly issued bonds using the sample described in Section 3.2.3. Following Badoer and James (2016), we estimate ordered logit models where the dependent categorical variable identifies one of the five possible maturity buckets considered.[17] The main explanatory variable of interest is $Treatment_w$, which is equal to one for bonds issued in week w by a firm who has been targeted already under the program by week $w-1$, and zero otherwise. We therefore consider a firm as not (yet) treated when it issues a bond and that security becomes its first bond purchased under the program. The rationale underpinning this decision is that the maturity of the bond is set in advance; as discussed in Section 4.2 the unconditional probability of a purchase for an eligible bond newly issued by a firm not yet targeted is less than 33%. Thus, firms that are not yet targeted cannot rely on having their new bonds purchased under the CSPP. Nonetheless, we include *First purchase* to control for cases in which a company becomes targeted the same week the bond is issued.

Table 8 shows the estimated results in terms of odds ratios. Model (1) only includes bond-level characteristics as control variables; Model (2) includes also control variables at the issuer-quarter level. Models (3) and (4) modify Models (1) and (2), respectively, by including firm and quarter-year indicators instead of *Target* and *Post*.

The estimated coefficient for $Treatment_w$ is positive and statistically significant at the 5% and 1% level in Models (1) and (2), respectively. The coefficient is still statistically significant at least at the 10% confidence level when firm and quarter-year indicators are included. The odds ratios reported in Table 8 imply that, all else equal, the probability of a bond being in the next maturity bucket is 1.8–2.4 times larger once a firm becomes treated. We can therefore conclude that effectively targeted firms not only have tilted toward bonds in their financing decisions, but have also increased the maturity of their market debt. The CSPP may have therefore not only improved the ability of effectively targeted firms to tap markets directly for debt capital, but also induced a change in market debt maturity. Longer maturities imply that the temporarily favorable financing conditions brought by the CSPP may have longer lasting consequences for the cost and composition of debt capital of effectively targeted firms.

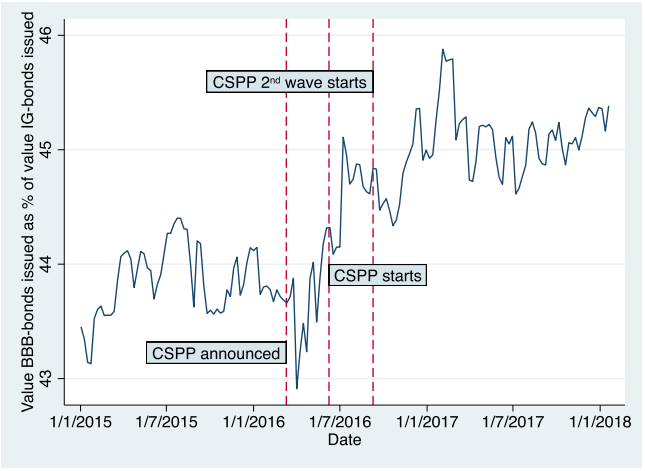

Fig. 3. Relative share of BBB-bonds over time. This figure presents the value-weighted share over time of issued bonds rated triple-B over all issued bonds meeting the eligibility criteria under the CSPP. The three vertical dashed lines represent the date the CSPP is announced (10 March 2016), the date the CSPP starts (8 June 2016) and the date the second wave starts (9 September 2016). The second wave of the program is defined as the part of our sampling period for which weekly updates of the lists of bonds held under the CSPP are available for all of the six central banks in charge of the program.

4.8. CSPP and general composition of the European IG corporate bond market

Before moving to the conclusions, in this section we briefly discuss how the general structure of the European investment grade corporate bonds market has changed since the introduction of the CSPP, and how our results can help explain the observed macro trends.

Fig. 3 shows the value-weighted share of eligible bonds issued that is rated triple-B. Before the CSPP is announced, around 43% of eligible bonds are triple-B; by the end of the sampling period, more than 45% of newly issued bonds are triple-B. This trend is consistent with our micro-level evidence on target selection and on the direct effect of the CSPP on effectively targeted firms: Relatively riskier bonds are more likely to be immediately purchased under the program and firms already targeted under the program are more likely to issue new bonds rather than obtaining a bank loan. Targeted firms are not only more likely to issue a bond; we show that their bonds are also characterized by significantly longer maturities at issuance. This result can help explain how the CSPP may have contributed to the steep increase in the average maturity of investment grade corporate bonds in Europe observed around the introduction of the

[17] As shown in Table B.6 of the Appendix, fully consistent results are found when estimating a linear model of the natural logarithm of the maturity at issuance.

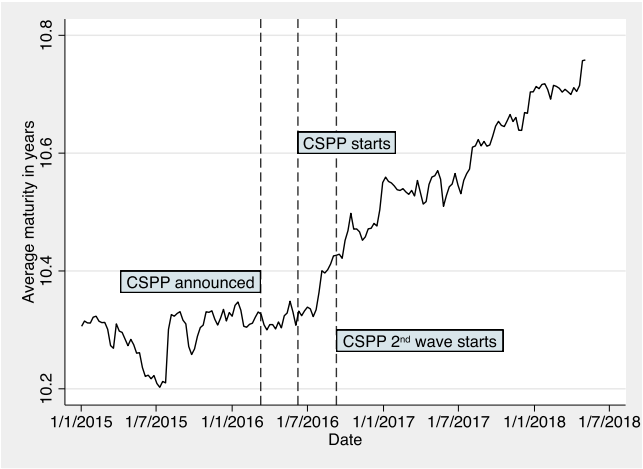

Fig. 4. Average maturity of the CSPP-eligible universe. This figure presents the average maturity of the bonds eligible for purchase under the CSPP. The three vertical dashed lines represent the date the CSPP is announced (10 March 2016), the date the CSPP starts (8 June 2016) and the date the second wave starts (9 September 2016). The second wave of the program is defined as the part of our sampling period for which weekly updates of the lists of bonds held under the CSPP are available for all of the six central banks in charge of the program.

program. As shown in Fig. 4, before the CSPP is introduced the average residual maturity in our eligible bonds sample is around 10.3 years. By July 2018, the average maturity has increased by almost half a year–an economically relevant increase.

Whereas other factors may of course contribute to these general trends, our results from micro-level analyses on the impact of the CSPP are thus fully consistent with the observed change at the macro level in the composition of the European investment-grade corporate bond market in terms of credit risk and duration.

5. Conclusions

In this paper we demonstrate that not all eligible corporate bonds have been the target of equally timely purchases under the European Corporate Sector Purchase Program (CSPP), and that this selection has material consequences for the effect of the CSPP on the access to credit markets of European firms. We argue that effectively targeted firms should experience a direct treatment effect on top of the general effect experienced by all eligible firms and already documented in previous studies.

We show that, all else equal, eligible bonds characterized by a higher level of credit risk are the target of more timely purchases by the six central banks in charge of the CSPP operations. This target selection has material consequences: firms whose bonds are effectively purchased under the program (directly treated firms) increase their use of market debt significantly more than firms with eligible but not yet purchased bonds. This result is not due to credit risk moderating how firms respond to the program; conditional on actual direct treatment, there is no difference in the reaction to the program of eligible firms in different rating categories. The increased use of bonds substitutes for the use of bank loans, and does not seem to result in a differential increase in leverage. Directly treated firms also tend to choose longer maturities for their newly issued bonds. We interpret the latter result as evidence of treated firms trying to lock in the benefits brought by the exogenous, temporary increase in the direct demand for their securities by central banks.

The presence of a direct effect of the CSPP demonstrates that target selection matters. This point is often neglected in the evaluation of quantitative easing measures under the implicit assumption of a perfect within-asset class transmission via a portfolio rebalancing channel. We show that—at least for investment grade corporate bonds as an asset class—such an assumption is not realistic. The non-neutrality of target selection and the direct effect of the CSPP on firms' financing decisions have potential long-lasting effects that central banks and supervisory institutions may want to keep under scrutiny.

Especially since the advent of the Covid-19 pandemic, programs similar to the CSPP are introduced in other countries; the US for example started the Secondary Market Corporate Credit Facility program in March 2020. Central banks in charge of these new programs may undertake different target selection choices compared to those made under the first and second wave of the CSPP. In doing so, they should be wary of the material impact of these choices. On the one hand, central banks may not want to discriminate among two otherwise similar firms by directly purchasing securities issued by only one of the two firms. On the other hand, in some situations central banks may want to consciously exploit the differential effect of direct purchases to favor the access to market credit for certain specific corporations.

Appendix A. Variables.

Table A.1.

Table A.1
Variable definitions.

Variable	Definition
CSPP	Indicator defined for eligible bonds outstanding as of 2016w22, based on the lists of bonds purchased and available for lending published by the six central banks (CBs) in charge of CSPP operations (source: CBs websites). It is set equal to one for bonds purchased under the CSPP by the end of the first wave and set equal to zero otherwise.
Haircut	The haircut, expressed in percentage points over the value of the bond, applied when the security is used as collateral in Eurosystem credit operations (source: European Central Bank eligible collateral dataset, ECB).
Rating	A numerical variable ranging from 17 (BBB-) to 27 (AAA) and corresponding to the highest long-term rating assigned to the bond by Moody's, S&P, or Fitch (source: Thomson EIKON).
Bid-Ask Spread	The difference between the ask (CMPA) and the bid (CMPB) composite closing prices of the bond, divided by the average of the two prices and expressed in percentage points (source: Thomson EIKON).
% of investable	The amount outstanding of the bond in euro (AOS) divided by the total AOS for all eligible bonds outstanding (source: Thomson EIKON).
Ln(1+DTM)	The natural logarithm of 1 plus the remaining maturity of the bond, expressed in days (source: ECB).
LNTA	The natural logarithm of total assets (WC02999) expressed in thousands of euro (source: Worldscope via Thomson EIKON; WC).
D/E	The ratio between the book value of debt (WC03255) and the market value of equity (WC08001) of the firm.
Q	The sum of the book value of total assets (WC02999) and market value of equity (WC08001), minus the book value of equity (WC03501) and deferred taxes (WC03263), all divided by total assets (WC02999).
Bond ratio	The sum of the amount (AOS) of all eligible bonds outstanding for the firm, divided by the book value of debt (WC03255).
LNTD	The natural logarithm of one plus the book value of debt (WC03255).
Treatment	A firm-quarter indicator equal to one from the first quarter onward the firm has one or more bonds purchased under the CSPP, and equal to zero otherwise.

(*continued on next page*)

Table A.1 (*continued*)

Variable	Definition
Treatment_{t+i}	A firm-quarter indicator equal to one in quarter $t+i$ and equal to zero otherwise, where t indicates the quarter in which the issuer becomes targeted under the CSPP.
$\text{Treatment}_{q\geq X}$	A firm-quarter indicator equal to one for every treated firm in every quarter $q \geq X$, where $X = t + i_{max} + 1$ and i_{max} is the highest i associated with indicators $Treatment_{t+i}$ effectively included in the model. In all other cases, it is set equal to zero.
Treatment_{w}	For each bond newly issued in week w, an indicator set equal to one if the issuer of the bond was already targeted under the CSPP as of $w-1$, and equal to zero otherwise. Newly issued eligible bonds are identified from the ECB dataset.
Bonds issued	A firm-quarter indicator set equal to one if the firm issues at least one bond in that quarter, and equal to zero otherwise.
LNPPE	The natural logarithm of one plus the net value of property, plant, and equipment (WC02501).
Lagged return	The 1 year lagged stock return based on log returns calculated from the total return index (RI) obtained from Thomson EIKON.
Short-term debt ratio	The ratio of short-term debt and current portion of long-term debt (WC03051A) to total debt (WC03255).
BBB-rated	Indicator variable equal to one when the minimum rating of a bond issuer equals BBB, and zero otherwise.
A-rated	Indicator variable equal to one when the minimum rating of a bond issuer equals A, and zero otherwise.
Bond versus loan	A firm-quarter indicator equal to one if the firm issues a bond in that quarter, equal to zero if the firm raises a bank loan, and missing if the firm does neither or both. Newly issued bonds are identified from the ECB dataset. New bank loans are retrieved from the Dealscan database via Thomson EIKON. Bank loan amounts are summed per package number and collapsed to the firm-quarter level, such that multiple loan issues by the same firm in the same quarter are counted as one, similarly for bond issues.
Maturity buckets (1–5)	A categorical variable ranging from one to five based on the maturity at issuance of the bond expressed in years. The five buckets are (1,2), [2,5), [5, 10), [10, 20), and [20, 30).
First purchase	For each newly issued bond, an indicator set equal to one if the bond is purchased under the CSPP the same week it is issued, and it is the first bond purchased under the program for that issuer. The indicator is set equal to zero otherwise.
Target	A firm indicator equal to one for issuers with at least one bond purchased under the CSPP by the end of the sampling period and set equal to zero otherwise.
Post	A time indicator equal to one from the start of the CSPP on and set equal to zero otherwise.

This table provides the sources of data and defines variables with their Worldscope (WC) or Thomson EIKON code between brackets when applicable.

Appendix B. Additional tables and robustness checks.

Table B.1Table B.2Table B.3Table B.4B.5Table B.6.

Table B.1
Averages of treated versus non-non-treated firms.

	Mean targeted	N	Mean non-targeted	N	T-stat difference
Bond ratio	0.38	2334	0.23	434	-12.37
LNTD	15.85	2334	15.61	434	-2.94
Bonds issued	0.19	2334	0.08	434	-6.61
LNTA	17.16	1883	16.70	346	-4.70
LNPPE	15.44	1871	14.84	345	-3.87
D/E	0.74	1875	1.29	341	5.22
Q	1.40	1769	1.26	328	-5.69
Lagged return	0.10	2099	0.05	386	-2.79
Short-term debt ratio	0.21	1717	0.25	322	3.78
BBB-rated	0.55	2334	0.58	434	1.06
A-rated	0.30	2334	0.11	434	-10.97

This table reports averages in which the variables in Panel C of Table 1 are split according to treated and non-treated firms where treated firms are firms that are purchased at any time during the CSPP and non-treated are never purchased during the time period of our sample. *N* indicates the number of observations and *T-stat difference* indicates a t-statistics for the null hypothesis that averages of treated and non-treated firms are equal.

Table B.2
Selection analyses: exclusion of NCB market segments.

Excluded market	(1) Belgium	(2) Belgium	(3) Finland	(4) Finland	(5) France	(6) France	(7) Germany	(8) Germany	(9) Italy	(10) Italy	(11) Spain	(12) Spain
Panel A: Bond control variables												
Haircut	0.036***		0.021**		0.011		0.028**		0.021**		0.033***	
	[0.013]		[0.010]		[0.010]		[0.011]		[0.011]		[0.013]	
Rating		-0.316***		-0.080		-0.146**		-0.266***		-0.257***		-0.293***
		[0.072]		[0.066]		[0.071]		[0.067]		[0.066]		[0.071]
Observations	896	838	1176	1101	939	880	894	817	934	850	932	849
Industry indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R-squared	0.116	0.166	0.137	0.113	0.100	0.112	0.139	0.172	0.118	0.151	0.127	0.169
N bonds	896	838	1176	1101	939	880	894	817	934	850	932	849
N targeted	330	328	506	503	414	411	297	296	312	309	349	346
Panel B: Bond and firm control variables												
Haircut	0.008		0.015*		0.017		0.013		0.003		0.017	
	[0.012]		[0.009]		[0.012]		[0.011]		[0.012]		[0.014]	
Rating		-0.265**		0.008		-0.125		-0.138		-0.208*		-0.199*
		[0.112]		[0.076]		[0.116]		[0.104]		[0.111]		[0.112]

(*continued on next page*)

Table B.2 (*continued*)

Excluded market	(1) Belgium	(2) Belgium	(3) Finland	(4) Finland	(5) France	(6) France	(7) Germany	(8) Germany	(9) Italy	(10) Italy	(11) Spain	(12) Spain
Observations	670	634	915	872	679	647	649	608	658	617	680	636
Industry indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R^2	0.172	0.179	0.185	0.164	0.183	0.175	0.202	0.194	0.184	0.183	0.181	0.175
N bonds	670	634	915	872	679	647	649	608	658	617	680	636
N targeted	289	287	423	421	327	325	257	257	261	259	292	290

This table presents coefficients estimates for logit models for the selection of purchases under the CSPP. It is a robustness check of the first four columns of Table 2 in which selected specifications exclude the bonds belonging to the purchase universe of selected national central banks (NCBs) of Belgium, Finland, France, Germany, Italy and Spain. Panel A only includes bond-level control variables, i.e. Columns (1) and (2) of Table 2. Panel B includes both bond-level and firm-level control variables, i.e. Columns (3) and (4) of Table 2. Logit models are estimated on a cross-sectional sample including all bonds outstanding and eligible as of 2016w22 (just before the start of the program); the dependent variable is an indicator equal to one if the bond has been purchased under the CSPP by 2016w36 (included) and zero otherwise. Bond-level variables are measured on 2016w22, whereas issuer-level variables based on accounting variables (i.e., those included only in Models 3 and 4) are measured at the end of 2015. Industry indicators are based on 1-digit SIC codes. Robust standard errors clustered by firm are reported in square brackets. *N bonds* (*N targeted*) is the number of distinct (purchased) bonds in the sample; *N firms* is the number of distinct firms in the sample; *N firms targeted* is the number of distinct firms in the sample whose bonds are purchased. Definitions of all variables are summarized in Table A.1 of the Appendix. *, **, and *** identify statistically significant coefficients respectively at the 10%, 5%, and 1% confidence level. respectively.

Table B.3
Targeted issuers and debt: separate bond issuance and loan granting analyses.

	Bonds			Loans		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.988***	0.783***	0.872***	-0.617***	-0.319	-0.409
	[0.183]	[0.271]	[0.329]	[0.226]	[0.445]	[0.508]
Target	0.702***	0.615	0.805*	0.607***	0.378	0.403
	[0.242]	[0.593]	[0.474]	[0.229]	[0.294]	[0.345]
Post	-0.746***	-0.395		-0.217	-0.528	
	[0.166]	[0.250]		[0.186]	[0.428]	
LNTA		-0.042	-0.061		0.059	0.115
		[0.090]	[0.098]		[0.100]	[0.111]
LNPPE		0.062	0.133		-0.082	-0.090
		[0.086]	[0.094]		[0.058]	[0.056]
D/E		-0.012	-0.007		0.028	0.159
		[0.091]	[0.114]		[0.066]	[0.097]
Q		-0.344	-0.564**		-0.121	0.007
		[0.218]	[0.275]		[0.199]	[0.221]
Lagged return		-0.295	-0.229		-0.788	-0.898*
		[0.378]	[0.432]		[0.490]	[0.543]
Short-term debt ratio		1.005*	0.758		0.311	0.268
		[0.561]	[0.601]		[0.690]	[0.780]
Constant	-2.496***	-1.616	-0.722	-3.020***	-1.979	-1.380
	[0.226]	[1.778]	[2.299]	[0.210]	[1.675]	[2.087]
Observations	5172	1789	1563	5172	1789	1168
Pseudo R-squared	0.038	0.038	0.096	0.018	0.038	0.106
Rating indicators	No	Yes	Yes	No	Yes	Yes
Industry-quarter indicators	No	No	Yes	No	No	Yes
N firms	384	221	215	384	221	208
N treated firms	256	183	178	256	183	170

This table reports coefficient estimates for logit models of the choice to issue a new bond (Models (1) to (3)) and raising a bank loan (Models (4) to (6)). All models are estimated on a firm-quarter panel dataset of firms from 2015q1 to and including 2018q2. The dependent variable is equal to one in Models (1) to (3) if a firm issues a bond and zero otherwise; it is equal to one in Models (4) to (6) when a firm is granted a loan and zero otherwise. All issuer-level control variables except are lagged one quarter and measured at the end of the quarter. Models (3), (5) and (6) include rating indicators for BBB and A; AAA/AA is the omitted rating category. *N firms* indicates the distinct number of firms in the sample. *N treated firms* indicates the number of distinct firms in the sample for which *Target* = 1. Robust standard errors clustered by firm are reported in square brackets. Definitions of all variables are summarized in Table A.1 of the Appendix. *, ** and *** identify statistically significant coefficients respectively at the 10%, 5%, and 1% confidence level.

Table B.4
Rating category and reaction to the program: bonds versus loans.

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment		1.679***		1.708**		2.392**
		[0.391]		[0.742]		[1.213]
Post × BBB-rated	0.152	-0.409	0.112	-0.628	1.373	0.268
	[0.318]	[0.418]	[1.187]	[1.063]	[2.303]	[2.367]
Post × A-rated	0.656	-0.065	0.883	0.542	2.134	1.439
	[0.422]	[0.476]	[1.013]	[0.923]	[2.183]	[2.295]
Post	0.332	-0.334				
	[0.256]	[0.294]				
Target	0.802***	0.273	0.822	0.112	1.293**	0.406
	[0.272]	[0.278]	[0.552]	[0.562]	[0.612]	[0.746]
LNTA			-0.219	-0.208	-0.330	-0.369*
			[0.158]	[0.162]	[0.207]	[0.221]

(*continued on next page*)

Table B.4 (*continued*)

	(1)	(2)	(3)	(4)	(5)	(6)
LNPPE			0.189**	0.182**	0.333***	0.359***
			[0.087]	[0.092]	[0.096]	[0.101]
D/E			-0.077	-0.045	-0.123	-0.056
			[0.151]	[0.145]	[0.169]	[0.189]
Q			-0.301	-0.318	-0.300	-0.340
			[0.331]	[0.347]	[0.441]	[0.468]
Lagged return			2.029**	2.228***	1.420	1.571*
			[0.832]	[0.802]	[0.938]	[0.891]
Short-term debt ratio			0.970	1.119	-0.529	-0.331
			[0.958]	[1.010]	[1.153]	[1.240]
Constant	0.112	0.439*	0.802	1.035	0.805	0.254
	[0.233]	[0.230]	[2.417]	[2.482]	[3.544]	[3.798]
Observations	827	827	351	351	213	213
Pseudo R^2	0.048	0.073	0.184	0.199	0.226	0.243
Quarter-year indicators	No	No	Yes	Yes	No	No
Rating indicators	No	No	Yes	Yes	Yes	Yes
Industry-quarter indicators	No	No	No	No	Yes	Yes
N firms	292	292	156	156	121	121
N treated firms	192	192	114	114	82	82

This table reports coefficient estimates for logit models of the choice between issuing a new bond or raising a bank loan. All models are estimated on a firm-quarter panel dataset of firms with eligible outstanding bonds observed from 2015q1 to and including 2018q2. The dependent variable is *Bond* versus *loan*. The sample excludes firm-quarter observations where no debt is raised or where the firm both issues a bond and raises a loan. All issuer-level control variables are lagged one quarter. Included rating indicators for BBB and A are interacted with *Post*; AAA/AA is the omitted rating category. *Post* is a time indicator equal to one from the start of the CSPP onward and equal to zero otherwise. *N firms* indicates the distinct number of firms in the sample. *N treated firms* indicates the number of distinct firms in the sample for which *Target* = 1. Robust standard errors clustered by firm are reported in square brackets. Definitions of all variables are summarized in Table A.1 of the Appendix. *, ** and *** identify statistically significant coefficients respectively at the 10%, 5%, and 1% confidence level.

Table B.5
Differential direct effect by rating category.

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.040**	0.039**	0.052***	0.044**	0.039*	0.034*
	[0.018]	[0.016]	[0.020]	[0.018]	[0.020]	[0.018]
Treatment × BBB-rated	0.014	0.006			0.015	0.012
	[0.014]	[0.015]			[0.016]	[0.015]
Treatment × A-rated			-0.012	-0.003	0.001	0.007
			[0.014]	[0.015]	[0.015]	[0.014]
LNTA		-0.016		-0.016		-0.017
		[0.031]		[0.031]		[0.032]
LNPPE		-0.004		-0.004		-0.004
		[0.019]		[0.019]		[0.019]
Q		0.050*		0.050*		0.049*
		[0.029]		[0.029]		[0.029]
Lagged return		-0.051**		-0.052**		-0.051**
		[0.022]		[0.022]		[0.022]
Short-term debt ratio		-0.031		-0.030		-0.031
		[0.040]		[0.040]		[0.040]
D/E		-0.009		-0.009		-0.009
		[0.006]		[0.006]		[0.006]
Constant	0.336***	0.618	0.336***	0.609	0.336***	0.628
	[0.009]	[0.559]	[0.009]	[0.560]	[0.009]	[0.561]
Observations	2768	1698	2768	1698	2768	1698
Adjusted R^2	0.890	0.906	0.890	0.906	0.890	0.906
Quarter-year indicators	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
N firms	239	218	239	218	239	218
N treated firms	195	180	195	180	195	180

This table reports coefficient estimates for linear models of the proportion of market debt over total debt. All models are estimated based on a firm-quarter panel dataset of firms with outstanding eligible bonds observed from 2015q1 to and including 2018q2. The dependent variable is *Bond ratio*. All issuer-level control variables are lagged one quarter and measured at the end of the quarter. Rating indicators are included for BBB and A; AAA/AA is the omitted rating category. *N firms* indicates the distinct number of firms in the sample. *N treated firms* indicates the number of distinct firms in the sample for which *Target* = 1. Robust standard errors clustered by firm are reported in square brackets. Definitions of all variables are summarized in Table A.1 of the Appendix. *, ** and *** identify statistically significant coefficients respectively at the 10%, 5%, and 1% confidence level.

Table B.6
Maturity of newly issued bonds, additional evidence.

	(1)	(2)	(3)	(4)
$Treatment_w$	0.163**	0.252***	0.228**	0.244**
	[0.078]	[0.069]	[0.102]	[0.114]
Target	0.070	0.059		
	[0.078]	[0.088]		
Post	-0.134*	-0.103		
	[0.080]	[0.086]		
First purchase	-0.002	0.096	0.003	0.068
	[0.125]	[0.100]	[0.138]	[0.140]
Ln(1+AOS)	0.131*	0.207***	0.080	0.102
	[0.070]	[0.072]	[0.098]	[0.104]
LNTA		-0.130***		-0.205
		[0.037]		[0.445]
LNPPE		0.016		0.176
		[0.018]		[0.170]
D/E		-0.110		0.249
		[0.110]		[0.400]
Q		-0.053		0.010
		[0.058]		[0.290]
Lagged return		-0.276*		-0.190
		[0.146]		[0.416]
Constant	0.150	1.297*	1.007	1.049
	[0.940]	[0.733]	[1.220]	[7.135]
Observations	612	396	612	396
Adjusted R^2	0.047	0.152	0.211	0.201
Quarter indicators	No	No	Yes	Yes
Firm indicators	No	No	Yes	Yes
Rating indicators	Yes	Yes	Yes	Yes
N firms	171	129	171	129
N treated firms	96	70	96	70

This table reports estimated coefficients for an OLS regression in which the dependent variable is the natural logarithm of maturity of newly issued bonds. The cross-sectional sample includes bonds issued between 2015w1 and 2016w22 and bonds issued between 2016w36 and 2018w18. Issuer-level control variables are measured at the end of the last quarter prior to issuance. Models (3) and (4) include firm and quarter indicators. Rating indicators are included for BBB and A; AAA/AA is the omitted rating category. *N treatment observations* indicates the number of observations in the sample for which $Treatment_w = 1$. *N firms* indicates the distinct number of firms in the sample. *N treated firms* indicates the number of distinct firms in the sample with at least one observation where $Treatment_w = 1$. Robust standard errors clustered by firm are reported in square brackets. Definitions of all variables are summarized in Table A.1 of the Appendix. *, ** and *** identify statistically significant coefficients respectively at the 10%, 5%, and 1% confidence level.

References

Abidi, N., Miquel Flores, I., Eterovic, N.A., 2017. Who benefits from the ECB's corporate sector purchase programme? A difference-in-discontinuities approach. Working Paper.

Albertazzi, U., Becker, B., Boucinha, M., 2016. Portfolio rebalancing and the transmission of large-scale asset programs: evidence from the euro area.Working Paper.

Angrist, J.D., Pischke, J.-S., 2008. Mostly Harmless Econometrics: An Empiricistas Companion. Princeton University Press.

Arce, O., Mayordomo, S., Gimeno, R., 2021. Making room for the needy: the credit-reallocation effects of the ECB's corporate QE. Rev. Financ. 25 (1), 43–84.

Arrata, W., Nguyen, B., 2017. Price impact of bond supply shocks: evidence from the eurosystemas asset purchase program. Banque de France Working Paper 623.

Autor, D.H., 2003. Outsourcing at will: the contribution of unjust dismissal doctrine to the growth of employment outsourcing. J. Labor Econ. 21 (1), 1–42.

Badoer, D.C., James, C.M., 2016. The determinants of long-term corporate debt issuances. J. Financ. 71 (1), 457–492.

Becker, B., Ivashina, V., 2014. Cyclicality of credit supply: firm level evidence. J. Monet. Econ. 62, 76–93.

Benigno, P., Nistico, S., 2015. Non-neutrality of open-market operations. Working Paper.

Betz, F., De Santis, R.A., 2019. ECB corporate QE and the loan supply to bank-dependent firms. ECB Working Paper.

Bowman, D., Cai, F., Davies, S., Kamin, S., 2015. Quantitative easing and bank lending: evidence from Japan. J. Int. Money Financ. 57, 15–30.

Cantillo, M., Wright, J., 2000. How do firms choose their lenders? An empirical investigation. Rev. Financ. Stud. 13 (1), 155–189.

Chen, L., Lesmond, D.A., Wei, J., 2007. Corporate yield spreads and bond liquidity. J. Financ. 62 (1), 119–149.

Christensen, J.H.E., Lopez, J.A., Rudebusch, G.D., 2015. A probability-based stress test of Federal Reserve assets and income. J. Monet. Econ. 73, 26–43.

D'Amico, S., King, T.B., 2013. Flow and stock effects of large-scale treasury purchases: evidence on the importance of local supply. J. Financ. Econ. 108 (2), 425–448.

De Pooter, M., Martin, R.F., Pruitt, S., 2018. The liquidity effects of official bond market intervention. J. Financ. Quant. Anal. 53 (1), 243–268.

De Santis, R.A., Zaghini, A., 2019. Unconventional monetary policy and corporate bond issuance, 2329.

Del Negro, M., Sims, C.A., 2015. When does a central bank balance sheet require fiscal support? J. Monet. Econ. 73, 1–19.

Denis, D.J., Mihov, V.T., 2003. The choice among bank debt, non-bank private debt, and public debt: evidence from new corporate borrowings. J. Financ. Econ. 70 (1), 3–28.

Duffee, G.R., 1999. Estimating the price of default risk. Rev. Financ. Stud. 12 (1), 197–226.

Eggertsson, G.B., Woodford, M., 2003. Zero bound on interest rates and optimal monetary policy. Brook. Pap. Econ. Act. 2003 (1), 139–233.

Ertan, A., Kleymenova, A., Tuijn, M., 2020. Financial intermediation through financial disintermediation: evidence from the ECB corporate sector purchase programme. Working Paper.

Eser, F., Schwaab, B., 2016. Evaluating the impact of unconventional monetary policy measures: empirical evidence from the ECB securities markets programme. J. Financ. Econ. 119 (1), 147–167.

Foley-Fisher, N., Ramcharan, R., Yu, E., 2016 nov. The impact of unconventional monetary policy on firm financing constraints: evidence from the maturity extension program. J. Financ. Econ. 122 (2), 409–429.

Georgiadis, G., Gräb, J., 2016. Global financial market impact of the announcement of the ECB asset purchase programme. J. Financ. Stab. 26, 257–265.

Gertler, M., Karadi, P., 2011. A model of unconventional monetary policy. J. Monet. Econ. 58 (1), 17–34.

Gertler, M., Karadi, P., 2013. QE 1 vs. 2 vs. 3.: a framework for analyzing large-scale asset purchases as a monetary policy tool. Int. J. Cent. Bank. 9 (1), 5–53.

Ghysels, E., Idier, J., Manganelli, S., Vergote, O., 2016. A high-frequency assessment of the ECB securities markets programme. J. Eur. Econ. Assoc. 15 (1), 218–243.

Greenwood, R., Vayanos, D., 2014. Bond supply and excess bond returns. Rev. Financ. Stud. 27 (3), 663–713.

Greenwood, R., Hanson, S.G., Stein, J.C., 2010. A gap-filling theory of corporate debt maturity choice. J. Financ. 65 (3), 993–1028.

Greenwood, R., Hanson, S.G., Liao, G.Y., 2018. Asset price dynamics in partially segmented markets. Rev. Financ. Stud. 31 (9), 3307–3343.

Grosse-Rueschkamp, B., Steffen, S., Streitz, D., 2019. A capital structure channel of monetary policy. J. Financ. Econ. 133 (2), 357–378.

Huang, J.-Z., Huang, M., 2012. How much of the corporate-treasury yield spread is due to credit risk? Rev. Asset Pricing Stud. 2 (2), 153–202.

Joyce, M., Lasaosa, A., Stevens, I., Tong, M., et al., 2011. The financial market impact of quantitative easing in the United Kingdom. Int. J. Cent. Bank. 7 (3), 113–161.

Joyce, M., Spaltro, M., 2014. Quantitative easing and bank lending: a panel data approach. Working Paper.

Kettemann, A., Krogstrup, S., 2014. Portfolio balance effects of the Swiss National Bank's bond purchase program. J. Macroecon. 40, 132–149.

Koijen, R.S.J., Koulischer, F., Nguyen, B., Yogo, M., 2019. Inspecting the mechanism of quantitative easing in the euro area. J. Financ. Econ. Forthcoming.
Krishnamurthy, A., Vissing-Jorgensen, A., 2011. The effects of quantitative easing on interest rates: channels and implications for policy. Brookings Papers on Economic Activity, pp. 215–287.
LoDuca, M., Nicoletti, G., VidalMartinez, A., 2016 feb. Global corporate bond issuance: what role for US quantitative easing? J. Int. Money Financ. 60, 114–150.
Longstaff, F.A., Mithal, S., Neis, E., 2005. Corporate yield spreads: default risk or liquidity? New evidence from the credit default swap market. J. Financ. 60 (5), 2213–2253.
Lugo, S., Croce, A., Faff, R., 2015. Herding behavior and rating convergence among credit rating agencies: evidence from the subprime crisis. Rev. Financ. 19 (4), 1703–1731.
Rischen, T., Theissen, E., 2018. Underpricing in the euro area corporate bond market: new evidence from post-crisis regulation and quantitative easing. Working Paper.
Rodnyansky, A., Darmouni, O.M., 2017. The effects of quantitative easing on bank lending behavior. Rev. Financ. Stud. 30 (11), 3858–3887.
Steeley, J.M., 2015. The side effects of quantitative easing: evidence from the UK bond market. J. Int. Money Financ. 51 (Supplement C), 303–336.
Tobin, J., 1958. Liquidity preference as behavior towards risk. Rev. Econ. Stud. 25 (2), 65–86.
Tobin, J., 1969. A general equilibrium approach to monetary theory. J. Money Credit Bank. 1 (1), 15–29.
Todorov, K., 2019. Quantify the quantitative easing: impact on bonds and corporate debt issuance. J. Financ. Econ. Forthcoming.
Vayanos, D., Vila, J.-L., 2009. A preferred-habitat model of the term structure of interest rates. NBER Working Paper.
Wallace, N., 1981. A Modigliani-Miller theorem for open-market operations. Am. Econ. Rev. 71 (3), 267–274.
Zaghini, A., 2019. The CSPP at work: yield heterogeneity and the portfolio rebalancing channel. J. Corp. Financ. 56, 282–297.