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* Views expressed are those of the author and do not necessarily reflect official positions of De Nederlandsche Bank.

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The local supply effect of asset purchases: evidence from the Eurosystem's CSPP

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Abstract

We estimate to what extent the Eurosystem's Corporate Sector Purchase Programme (CSPP) impacted the price of securities that were actually bought, or their close substitutes, more than the price of other securities. For *own* bond purchases we do not find significant local supply effects, which is in line with the Eurosystem's market neutrality principle of asset purchases. We do, however, find significant local supply effects caused by the purchases of *substitute* bonds defined by similar maturities; we estimate that these effects reduce bond yields by about 40-45 basis points. Such local supply effects are more pronounced for bonds that were eligible under the CSPP than for non-eligible bonds, for bonds that have been issued more than a year ago and for bonds with relatively low credit ratings.

Keywords: monetary policy, quantitative easing, preferred habitat

JEL Codes: C26, E43, E52, E58

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1 Introduction

On 10 March 2016, the European Central Bank (ECB) announced its Corporate Sector Purchase Programme (CSPP). Under this programme, the Eurosystem purchased debt securities issued by non-bank corporations established in the euro area. The CSPP was part of the Asset Purchase Programme (APP), also known as quantitative easing (QE). The scope of CSPP purchases, which started in June 2016, included investment-grade securities denominated in euro, both in the primary and secondary market and over a wide range of remaining maturities (between 6 months and 30 years).[2]

In this paper, we investigate the relevance of local supply effects under the CSPP. With full arbitrage and homogeneous markets and investors, it should be irrelevant for the effectiveness of a purchase programme which specific securities are bought, as purchase effects will be distributed evenly across all securities. But in practice, there may be frictions related to scarcity of specific securities or close substitutes, whose yields would then be impacted more than other assets. D'Amico and King (2013) denote such specific responses as "local supply effects" and present evidence that these could be observed in the United States following the Fed's 2009 US Treasury purchases.

We consider the first stage of the CSPP, which was announced on March 10, 2016 and continued up to 20 December 2018. Additional (i.e. net) purchases under the APP – including the CSPP – were suspended as of that date; subsequent purchases were only done to maintain the existing portfolio size. The announcement of the CSPP came as a surprise for financial markets, (see Abidi and Miquel-Flores, 2018, for a discussion and evidence), which allows us to ignore any anticipation effects in market prices. The APP was resumed in November 2019 and continued during the COVID crisis, but this is not part of our study. Analyzing this second

[2] See for further details the press releases of the announcement (ECB, 2016a) and the details as communicated on 21 April 2016 (ECB, 2016b).

stage would more challenging, as it was anticipated and therefore likely reflected in market prices before the official announcement.

Our analysis focuses on stock effects that can be assessed over the entire duration of the programme. Flow effects, which particularly affect yields on the day of a purchase, are not considered. D'Amico and King (2013) show that flow effects are far less pronounced than stock effects and most relevant when markets are dysfunctional. Presumably, there was no significant market dysfunction in our sample period, as can be inferred from the absence of any interventions by the ECB to stabilize specific markets.[3]

We find evidence of local supply effects due to purchases of bonds that can be considered substitutes, defined by maturity class. In particular, individual bond prices significantly increase (and yields significantly decrease) following purchases of bonds in the same maturity buckets. In our regressions, these buckets are defined by remaining maturities, which my deviate up to two years. On average, we find that local supply effects of substitute purchases have reduced bond yields by about 40-45 basis points. These effects vary significantly across individual bonds, due to the heterogeneity of our sample in terms of issuers and other characteristics, such as remaining maturities and ratings. We also find that local supply effects are more pronounced for bonds that are eligible under the CSPP than for non-eligible bonds, for bonds that have been issued more than a year ago and for bonds that have relatively low credit ratings.

Interestingly, we hardly find evidence for local supply effects that can be linked to *own* purchases, i.e. the purchase of the bond itself. This may be attributed to the Eurosystem's aim for market-neutral implementation of the CSPP, avoiding any distortionary impact of

[3] The ECB implemented or announced asset purchases to address market dysfunction in the years before and after our sample period, like the Securities markets Programme (SMP) in 2010 and Outright Monetary Transactions (OMT) in 2012, as well as the Pandemic Emergency Purchase Programme (PEPP) in 2020 and the Transmission Protection Instrument (TPI) in 2022. No such asset purchase programmes were developed or implemented during the period we consider.

individual purchases on the prices of bonds that were bought. In addition, we present evidence for increased bond issuance by corporates whose securities were purchased, which likely counteracts any potential local supply effects on prices following own purchases.

While several studies have investigated the overall impact of the Eurosystem's asset purchases including the CSPP programme, local supply effects have received only limited attention so far. Our findings imply that through their impact on specific market segments, central bank asset purchases may directly affect allocation decisions in the economy, which would go beyond the traditional (macro) focus of monetary policy. This may complicate monetary policy aimed at price stability, but could support potential secondary objectives to influence allocation by tilting asset portfolios towards specific segments. This could be done, for example, towards green assets in order to pursue climate-related goals.

The rest of our paper is organised as follows. In Section 2, we discuss background literature including empirical work on local supply effects. In Section 3, we discuss our data and methodology. Section 4 presents our main results, and Section 5 concludes.

2 Literature

A key transmission mechanism for asset purchases is the portfolio-rebalancing channel, which traces back to Tobin (1969) and has been formalized by Andrés et al. (2004). The idea is that asset purchases have a price impact due to imperfect substitutability across asset classes. As investors who sold their securities adjust their position by rebalancing their portfolio towards alternative assets, this drives up prices and reduces yields more broadly. A more specific strand within the portfolio balance literature is the preferred habitat theory (Culbertson, 1957; Modigliani and Sutch, 1966; recently revived by Vayanos and Vila, 2021). These models assume that investors prefer to stick to a specific market segment or "habitat". For instance,

pension funds and life insurers tend to prefer long-term maturities to match their liabilities, while money market funds typically only invest in short-term assets. Alongside preferred habitat investors, arbitrageurs play an essential role in these models as they exploit risk-return trade-offs across the segments. With risk-neutral arbitrageurs, the impact of asset purchases through duration extraction is spread over the entire maturity spectrum. If arbitrageurs are risk averse, however, their role remains limited and purchases have a specific impact on the securities that are actually bought and their close substitutes, which creates local supply effects (see also Cochrane, 2009; d'Amico and King, 2013).

Some studies specifically investigate the relevance of local supply effects. In a pioneering study, d'Amico and King (2013) find evidence for local supply effects for the Fed's initial QE programmes in the Treasury market. More specifically, they find a significant impact of own purchases as well as – albeit more limited – purchases of bonds with a similar duration. In addition, most of the impact (about 30 basis points) works through *persistent* stock effects (i.e. measured over the entire programme) while the impact of *temporary* flow effects (measured on the day of the purchase) only account for a few basis points (likely related to improved market liquidity). Cahill et al. (2013) provide further evidence for local supply effects as a transmission channel of the Fed's asset purchases, on top of the more general duration risk channel. Schlepper et al. (2020) investigate the impact of the Eurosystem's Public Sector Purchase Programme (PSPP) on Bund yields, using intraday transaction data, and find evidence of a local supply effect. D'Amico and Kaminska (2017) investigate local supply effects in the Bank of England's Corporate Bond Purchase Scheme and find that bond prices respond to own purchases but not to purchases of close substitutes.

Other studies analyse the impact of the Eurosystem's CSPP but don't consider local supply effects. Zaghini (2019) analyses the CSPP during its first implementation year and finds evidence for portfolio rebalancing. More specifically, the study shows that the impact on non-

eligible bond spreads was similar (albeit more delayed) to that of eligible bond spreads. Abidi and Miquel-Floris (2018) find a similar result with a significant decline in borrowing costs after the CSPP announcement, for eligible as well as non-eligible bonds but particularly for firms that with ratings around BBB- (i.e. that just qualify to be eligible). Focusing on primary market purchases, Mäkinen et al. (2022) do not find a significant difference between eligible and non-eligible bonds. DeSantis et al. (2018) also observe a significant reduction in spreads and an improvement in supply conditions for both eligible and non-eligible bonds. Moreover, they present evidence supporting that the Eurosystem pursued a neutral implementation of the CSPP, where purchase volumes across countries, sectors and primary versus secondary markets were calibrated in such a way that any distortionary impact on market functioning was avoided. Betz and DeSantis (2022) present evidence for spillover effects of CSPP to bank-dependent firms. Such firms were not directly affected by the CSPP programme as they do not have direct access to the bond market, but benefited indirectly from a shift in demand and supply conditions in credit markets more generally.

To our knowledge, the only study so far that local supply effects of the CSPP programme is Bua and Kapp (2023). They focus on local supply effects of own purchases and apply the resulting elasticity to estimate the implications of tilting the portfolio towards securities with better climate scores. They conclude that this could lower yields in specific sectors (like energy and utilities, which would be bought more) and push up yields in other sectors (technology, industrial, which would be sold) while the overall impact on monetary stance is negligible.

3 Data and methodology

In our analysis, we apply the methodology of d'Amico and King (2013) to estimate local supply effects using security-level data. We estimate the impact of own purchases as well as close

substitutes defined by maturity buckets. We only consider stock effects since the announcement of the CSPP, i.e. measured over the entire period from March 2016 till the end of net purchases in December 2018. This means that we do not consider possible flow effects, which may materialise immediately after purchases; such effects are particularly relevant in periods of market dysfunction but are otherwise likely to be very small (as also found by d'Amico and King, 2013).

3.1 Data

Our data consists of observations just before the CSPP announcement on 10 March 2016, and the day after the programme ended on 20 December 2018. Table 1 presents an overview of the main variables. The primary variables of interest are the volumes purchased by the Eurosystem and the price change over the entire period at the individual bond level.[4]

Table 1 Stylised facts of our data

Variable	N	Mean	p50	Min	Max	SD
Own purchases (% amount outstanding)	944	7.9%	3.2%	0.0%	56.5%	9.8%
1Y bucket purchases (% amount outstanding)	944	8.4%	8.9%	0.0%	12.4%	2.6%
2Y bucket purchases (% amount outstanding)	944	8.5%	8.8%	0.0%	11.7%	2.2%
Remaining maturity (years)	944	8.2	6.9	2.8	49.1	5.5
Rating: 1 (AAA) to 10 (BBB-)	944	6.7	7	1	10	2.2
Credit spread vis-a-vis Bunds (%)	944	1.1%	1.0%	0.0%	4.2%	0.7%
Initial price (logs)	944	2.0	2.0	1.9	2.2	0.0
Weeks since issuance	944	175.4	130.9	0.0	1128.8	157.0
Outstanding amount March 9, 2016 (EUR mln)	944	635.8	550.0	0.0	3850.0	458.1
Outstanding amount, 1Y bucket (EUR bln)	944	114.9	135.0	0.1	189.2	61.2
Outstanding amount, 2Y bucket (EUR bln)	944	214.5	239.4	0.1	357.4	110.6
Outstanding amount, sovereign debt 1Y bucket (EUR bln)	944	667.6	686.2	1.0	1139.0	314.7
Outstanding amount, sovereign debt 2Y bucket (EUR bln)	944	1338.1	1382.1	1.8	2414.5	623.9

Note: Purchases are measured over our sample period (10 March 2016 – 20 December 2018); other variables are measured just before the CSPP announcement on 10 March.

[4] More specifically, at the ISIN-level (International Securities Identification Number).

We use the ECB's Eligible Assets Database (EADB) to select the securities for our analysis, which is published on a daily basis on the ECB website.[5] From this database, we take two snapshots, on 9 March 2016 and 20 December 2018, respectively. The bonds included in our dataset should be present on both of these dates.[6] This set is further reduced to 1,488 bonds that are either eligible under the CSPP or not eligible but comparable (in terms of denomination, rating and issuer).[7,8] In total, we retain 944 bonds of which 603 were eligible under the CSPP.

The ECB published weekly data on the ISINs held by the Eurosystem as well as a semi-annual breakdown of the CSPP portfolio by sector, rating and country of risk. The Eurosystem's CSPP holdings were representative for the eligible bond universe (ECB, 2017). As we need securities holdings at the ISIN-level to investigate local supply effects, we use confidential purchase data.

Our dependent variable is the price change between 9 March 2016 and 20 December 2018. Following d'Amico and King (2013), we use the gross return (in percentage points) as the price change variable, which includes price changes, accrued interest and coupon payments over the period under investigation. This is more appropriate than 'clean' price changes, as these tend to moderate over time due to the decreasing number of upcoming coupon payments.

Our main explanatory variables are the Eurosystem's purchases of bonds under the CSPP, as well as their substitutes defined by maturity buckets. These purchases are normalised by amounts outstanding one day before the CSPP announcement, as further explained in Section

[5] See List of eligible marketable assets (https://www.ecb.europa.eu/mopo/coll/assets/html/index.en.html)
[6] This means that we remove bonds that were not present on one of both dates (typically because they were issued after 10 March 2016 or matured before 20 December 2018).
[7] We kept securities of the following types (ECB Eligible Assets Classification): AT01 (bond), AT02 (medium-term note), AT03 (commercial paper), AT11 (asset-backed security) and AT12 (multi cédulas – Spanish covered bonds) denominated in euro, and removed bonds from international organizations, local governments. Furthermore, we only kept securities from issuer groups IG3 (corporates) and IG9 (non-credit financial institutions).
[8] For example, ISIN XS1061430051, with a remaining maturity of just over 31 years on March 9th 2016 issued by SNCF Réseau (the French railway company) is present in the dataset, even though it is not eligible under the CSPP.

3.2. Purchases are taken from the internal Eurosystem database; amounts outstanding are taken from Bloomberg.

Our control variables are all based on the observation at the announcement date: 10 March 2016. Yield curve effects are captured by remaining maturity (measured in years) and maturity squared (to control for changes in the slope and curvature of the yield curve), taken from Bloomberg. Credit risk is controlled for by each bond's credit rating, which is taken from Moody's, S&P and Fitch, translated into numerical values ranging from 1 (AAA) to 10 (BBB-). In cases where ratings differ across agencies, we take the average. We also include the credit spread of each bond, which is calculated vis-à-vis the corresponding Bund rate.[9] Finally, as will be further explained in Section 3.2 we construct instrumental variables using the weeks since issuance and the amount outstanding of corporate bonds (both taken from Bloomberg) as well as government bonds (from the EADB).

3.2 Empirical specification

Following d'Amico and King (2013), our empirical specification reflects that the local supply effect may influence the price of a bond that is purchased under the CSPP as well as close substitutes. These substitutes are defined by buckets $S_n(i)$, where i represents an individual bond at the ISIN level and n is the bucket range, defined by remaining maturity. The narrowest bucket $S_0(i)$ with $n = 0$ contains solely the own purchases of security i itself. By varying the size of the buckets, the scope of the local supply effect can be investigated. In our baseline specification, the buckets are defined on the basis of remaining maturities that deviate up to,

[9] German bond yields are available for 1 to 10, 15, 20, 25 and 30 years. Each bond in the dataset is matched with its nearest Bund maturity. For example, ISIN FR0010033381, issued by Veolia Environnement S.A., has a remaining maturity of 17.72 years and its credit spread is calculated vis-à-vis the 20-year Bund rate.

respectively, 1 and 2 years. For example, $S_2(i)$ includes all bonds that have been purchased with remaining maturities up to two years above or below the maturity of bond *i*.

The euro amount purchased under the CSPP in bucket *n* for bond *i* is denoted as $Q_{i,n} = \sum_{j \in S_n(i)} Q_j$, where $Q_{i,0}$ captures own purchases. Purchases are scaled by the amounts outstanding in the relevant bucket, to make them more comparable across ISINs and bucket sizes. The resulting normalized purchases are denoted as $q_{i,n} = Q_{i,n}/O_{i,n}$ where $O_{i,n}$ is the outstanding amount in bucket *n* for bond *i*. This means that own purchases of a bond are normalized by its own outstanding amount.

The baseline is a cross-sectional regression and takes the following form:

(1) $R_i = \beta_{n,0} + \beta_n q_n + \varphi_n{}' x_i + e_{n,i}$

R_i is bond *i*'s gross return between 9 March 2016 and 20 December 2018 and x_i is a vector of bond-specific control variables. We do not take into account possible anticipation effects, as the CSPP announcement came as a surprise for financial markets (see Abidi and Miquel-Flores, 2018, for a discussion and evidence). β_n is our main coefficient of interest, representing the price elasticity of bond purchases in bucket *n*. Our explanatory variables include remaining maturity and maturity squared to control for a bond's sensitivity to shifts in the yield curve. Ratings and the spread vis-à-vis bunds are included to control for credit risk. Following d'Amico and King (2013) and d'Amico and Kaminska (2019), the log of the initial price is included to account for mean reversion or expectations of future returns.

Asset purchases are prone to endogeneity concerns, as they may be driven by bond-specific market conditions and hence their yields. For instance, a central bank may be inclined to buy bonds they believe are underpriced, which could result in reverse causality. To address possible endogeneity, we use two-stage least squares (2SLS) by instrumenting purchase amounts with bond characteristics just before the CSPP announcement.

So, the first-stage regression looks as follows:

(2) $q_{i,n} = \gamma_0 + \gamma_1' z_i + \gamma_2' x_i + u_i$

where $q_{i,n}$ is purchases and z_i is a vector of instrumental variables. These are (1) weeks since issuance (reflecting that securities issued longer ago are traded less heavily, expected to have a negative impact on $q_{i,n}$); (2) an 'on-the-run' dummy that is one if the number of weeks since issuance is less than three and zero otherwise (a supplementary liquidity indicator, expected to have a positive impact); and (3) the amount outstanding of the bond or bucket on the day of the announcement (10 March 2016, reflecting the scope for purchases, expected to have a positive impact). For the 1–2-year maturity buckets, we include as a fourth instrument the amount outstanding of the sovereign debt with corresponding maturity (following d'Amico and Kaminska (2019), to reflect potential substitutability between CSPP and PSPP purchases within the overall APP, expected to have a negative impact).[10] All four instruments are constructed specifically for each bucket. This means that for own purchases, we use the observations of the corresponding ISINs. For the wider buckets, we calculate the average value of weeks-since-issuance and the on-the run dummy for all securities in the bucket (weighted by the proportion of their outstanding amounts). The outstanding CSPP and corresponding PSPP amounts are aggregated over the securities in the bucket and are corrected for the Eurosystem's already existing holdings when the CSPP was launched.

4 Results

4.1 Baseline regressions

Table 2 presents first-stage regressions for own purchases and 1-year and 2-year buckets). As expected, weeks-since-issuance has a negative impact, while the on-the-run dummy has a

[10] Weeks since issuance is taken from Bloomberg, outstanding amounts are taken from the EADB.

positive impact on the normalised amount of purchases. The volume of outstanding amounts also has a positive impact, in line with the idea that a larger volume facilitates purchases while still complying with the market neutrality objective. The amount outstanding of sovereign debt in the corresponding maturity bucket has a negative sign for the 1-2 year buckets, consistent with the interpretation of these securities as potential substitutes.

Table 2 First-stage regression

	Own	1Y bucket	2Y bucket
Weeks since issuance	-0.011 ***	-0.005 ***	-0.007 ***
	(0.000)	(0.000)	(0.000)
On the run	8.149 *	52.812 ***	89.216 ***
	(0.049)	(0.052)	(0.076)
Outstanding amount in bucket	0.002 ***	0.020 ***	0.008 ***
		(0.000)	(0.000)
Idem, sovereign debt		-0.006 ***	-0.004 ***
		(0.000)	(0.000)
Maturity	0.089	-0.173	-0.672 ***
	(0.001)	(0.002)	(0.001)
Maturity squared	-0.007 *	-0.004	0.006 **
	(0.000)	(0.000)	(0.000)
Rating	1.038 ***	0.064 *	-0.011
	(0.000)	(0.000)	(0.000)
Credit spread	-0.858	0.125	0.105
	(0.006)	(0.001)	(0.001)
Initial price (log)	-1.402	0.515	-0.278
	(0.076)	(0.023)	(0.013)
Constant	5.119	10.603 **	18.417 ***
	(0.154)	(0.052)	(0.03)
Instrument tests:			
Hansen J	0.125	0.208	0.011
Kleibergen-Paap LM	0.000	0.000	0.000
Cragg-Donald F statistic	17.54	59.27	182.64
Endogeneity	0.071	0.08	0.08

Notes:

The dependent variable is the total purchase of bonds in the relevant bucket between one day before the announcement (9 March 2016) and the end of the net purchase phase (20 December 2018).

The Hansen J test is a test of overidentifying restrictions, with a joint null hypothesis that the instruments are valid, i.e. uncorrelated with the error term. The Kleibergen-Paap LM test is an underidentification test. The Cragg-Donald F statistic is also used to test instrument relevance; typically, a value of at least 10 is considered an indication that instruments are sufficiently correlated with the endogenous regressor. Finally, the Durbin-Wu-Hausman endogeneity test (reported by the endog option in Stata's ivreg2 code) is carried out to check whether the instruments are indeed endogenous; under the null hypothesis, the assumed endogenous regressors can actually be treated as exogenous variables.

Standard errors are reported between brackets. Superscripts *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

The amount of purchases is negatively related to maturity for the wider buckets, although only statistically significant for the 2-year bucket, which is at odds with the idea that the intended impact on the term premium is most pronounced when longer maturities are bought. This may reflect that the Eurosystem's purchase programmes were implemented by defining monthly purchase volumes and not by specific targets for duration extraction. Maturity squared is statistically significant only in two cases, with the opposite sign as maturity itself, implying that it is important to take into account yield curve curvature.

Credit quality (measured by ratings and credit spread) appears to have limited relevance for purchases. Only in two cases (own purchases and the 1-year bucket) the impact of a lower rating is significantly positive, for the 2-year bucket the impact is insignificant. This may reflect that the Eurosystem's purchases were restricted to investment grade (BBB-) and within this limited scope there was no policy to focus on particular credit quality classes.[11]

We performed statistical tests to investigate the quality of our instruments. All three specifications meet the Kleibergen-Paap LM (2006) underidentification test and the Cragg-Donald (1993) weak instrument test. The first two specifications also meet the Hansen (1982) J-statistic to test instrument exogeneity; the 2-year bucket specification does not, which implies that outcomes of this regression should be interpreted with caution. Finally, exogeneity of purchases can be rejected for all three specifications at the 10 percent level. Overall, the tests support using an instrumental variables approach for own purchases and the 1-year bucket: instruments are adequate and exogeneity can be rejected. For the 2-year bucket, the tests are not conclusive as instrument exogeneity cannot be rejected. We did not further investigate this; a relevant aspect may be that for wider buckets, the instruments are constructed based on a

[11] For bonds close to the lower bound of investment grade ratings (BBB-) there would be an increased probability to become non-eligible due to a downgrade. In that case, the Eurosystem may choose, but is not obliged, to sell the bond. In principle, this possibility could be a consideration not to buy lower-rated bonds.

wider range of (substitute) securities, which may introduce information that contaminates the instrument.

Table 3 Results baseline regressions

	Two-stage least squres			Ordinary least squres		
	Own	1Y bucket	2Y bucket	Own	1Y bucket	2Y bucket
Purchases	1.295	32.946 **	27.296 **	2.327 **	19.812 ***	26.940 ***
	(9.785)	(15.456)	(12.521)	(1.114)	(4.795)	(6.147)
Maturity	0.283 ***	0.266 ***	0.276 ***	0.283 ***	0.272 ***	0.276 ***
	(0.09)	(0.088)	(0.088)	(0.055)	(0.055)	(0.055)
Maturity squared	-0.008 ***	-0.005 **	-0.006 **	-0.008 ***	-0.006 ***	-0.006 ***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)
Rating	-0.073	-0.067	-0.054	-0.082	-0.065	-0.054
	(0.124)	(0.065)	(0.064)	(0.065)	(0.064)	(0.064)
Credit spread	4.068 ***	3.977 ***	4.004 ***	4.072 ***	4.011 ***	4.004 ***
	(0.418)	(0.388)	(0.39)	(0.232)	(0.231)	(0.231)
Initial price (log)	11.063	11.227 **	11.569 **	11.339 ***	11.023 ***	11.558 ***
	(7.014)	(5.21)	(5.239)	(2.697)	(2.664)	(2.667)
Constant	-23.620 *	-26.716 **	-27.085 **	-24.217 ***	-25.183 ***	-27.030 ***
	(14.3)	(10.577)	(10.687)	(5.451)	(5.405)	(5.454)
# Obs	944	944	944	944	944	944
R^2	0.43	0.43	0.44	0.43	0.43	0.44
Yield effect (bps)	-0.22	-5.54	-4.59	-0.39	-3.25	-4.41

Notes: The dependent variable is the gross return over the CSPP programme between one day before the announcement (9 March 2016) and the end of the net purchase phase (20 December 2018). The yield effect is the purchases coefficient divided by the average duration of the bonds in the corresponding bucket.

Standard errors are reported between brackets. Superscripts *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

The first three columns in Table 3 present second-stage regressions explaining the change in bond prices over the CSPP programme, using the instrumented purchases as our most important explanatory variable. As expected, purchases have a significantly positive impact on bond prices for the 1-year and 2-year buckets. For own purchases, the impact is also positive but insignificant. The bottom line in Table 3 translates the coefficient of purchases into a yield effect, thereby taking into account the average duration of included bonds. For the 1-year and 2-year buckets, the yield impact is around 5 basis points if one percent of the bucket is purchased. As on average 8.5 percent of amounts outstanding in each bucket were bought

(Table 1), the mean total supply effect is around 40-45 basis points, which is higher than the 30 basis points d'Amico and King (2013) found for the Fed's Treasury programme.

We repeated the regressions with ordinary least squares (OLS), as a robustness check (Table 3, last three columns). These regressions ignore possible endogeneity issues, but are in principle more efficient than an instrumental variables approach. The impact of own purchases is now statistically significant, although still very small; for the one-year bucket, the impact of purchases is found to be somewhat lower than in our 2SLS regression. The other coefficients are very similar to the 2SLS results.

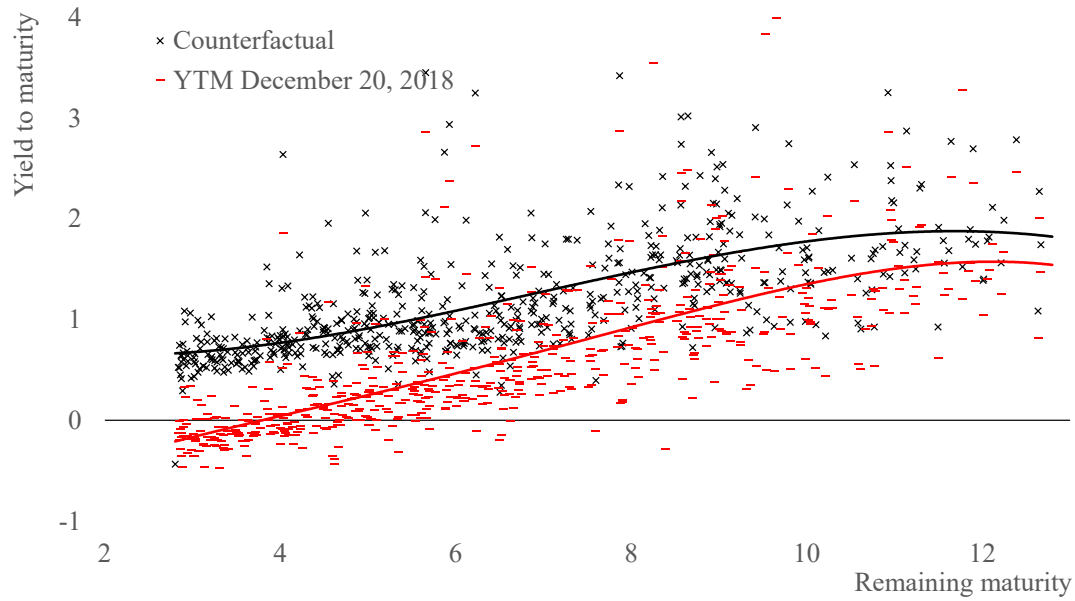
We also investigated a more granular grid of buckets beyond own purchases, with 0.1-year steps (not presented). Applying 2SLS, local supply effects for the narrowest buckets are statistically insignificant, but they become significant from a 0.5-year bucket size onwards with an impact similar to that of the one-year bucket. In addition, instrument validity tests are valid for buckets up to 1.7 years but also suggest (from bucket size 1.3 onwards) that exogeneity of asset purchases can no longer be rejected. Hence, OLS estimates may be preferred over 2SLS for these wider buckets, as endogeneity seems less of a concern. Applying OLS, the impact is statistically significant for all buckets with steps of 0.1-year, and the narrowest bucket already has a significantly larger effect than own purchases. Both for 2SLS and OLS, the outcomes for narrower buckets confirm that local supply effects are only significant and meaningful for wider buckets beyond own purchases.

Overall, we find a significant local supply effect for near substitutes but not for own purchases. This is the opposite of what d'Amico and King (2013) find for asset purchases in the US Treasury market and d'Amico and Kaminska (2019) for the Bank of England's corporate bond programme, as both these studies find more pronounced effects for own purchases than for wider buckets. In Section 4.2, we discuss possible explanations why we do not find a significant effect for own purchases.

To illustrate the estimated impact on individual bond yields, Figure 1 compares the actual yield to maturity of the bonds in our sample with a counterfactual that corrects for the local supply effect. More specifically, we calculate a bond-specific price impact by multiplying the 1-year bucket purchase coefficient (32.946) by the normalized purchases in this bucket for each bond. To translate it into a yield effect, it is divided by the bond-specific duration in the beginning of our sample period. This yield effect is then added back to the actual yield to maturity, which results in the counterfactual. The chart confirms that the average impact has been substantial, i.e. about 40 basis points. At the same time, the estimated impact differs substantially across individual securities, which reflects the heterogeneity of the corporate bond market. This differs from sovereign debt purchase programmes, which are implemented in relatively homogeneous markets leading to less dispersion across individual securities, as shown by d'Amico and King (2013) who present a similar graph based on the Fed's asset purchases.

Figure 1 Yield to maturity end-2018 versus counterfactual

Percent



Note: The red dashes are bond-specific yields to maturity on December 20, 2018, i.e. at the end of the CSPP net purchases. The black crosses are a counterfactual, using our estimates based on the 1-year bucket purchases. The red and black trend lines are third-order polynomials.

4.2 Non-response of own purchases

The non-response of own purchases in our results is remarkable, and to some extent counterintuitive as own purchases are arguably the most direct way to impact the yield of a particular bond. We have two explanations for this finding. First and foremost, it is consistent with Eurosystem's commitment to implement the CSPP in a neutral way. This means that purchases were selected and calibrated in such a way that a distortionary impact on markets was avoided as much as possible. Neutrality of CSPP purchases has been established empirically by DeSantis et al. (2018), who present an event study showing that in the days following specific bond purchases there was hardly any impact on the pricing of these bonds. This finding implies that any persistent impact of own purchases is unlikely as well. A second explanation is that own purchases may have triggered corporates to issue more bonds, counteracting the negative supply shock we analysed in our regressions and its impact on yields. Simple correlations between amounts issued by corporates and own purchases in previous quarters are significantly positive at a level around 0.2, which suggests that there is indeed such a such a link (Table 4). Correlations between purchases and amounts issued in wider maturity buckets are very low, implying that issuance effects are only relevant for own purchases. One way to interpret this is that while own purchases did not have local supply effects on *prices*, they stimulated *issuance* and therefore contributed to the goal of the CSPP.

Table 4 Correlation between bond issuance and previous CSPP purchases

	Own	**1Y bucket**	**2Y bucket**
Purchases up to and including previous quarter	0.19	-0.01	0.01
Purchases up to previous quarter	0.23	-0.04	0.06

Note: simple correlations between bonds issued by a specific corporate and CSPP purchases of bonds issued by the same corporate prior to the issuer. The upper row is based on purchases from 2016Q2 up to the quarter preceding the issuance; the lower low is based on purchases only in the preceding quarter.

4.3 Impact on different subsets of bonds

Next, we investigate whether effects have been different across subsets of bonds in our sample. We consider three splits: (1) eligible versus non-eligible bonds, (2) bonds that have been issued relatively recently when the CSPP was announced (up to one year before the announcement) versus bonds that have been issued longer ago and (3) bonds with a relatively high rating (up to A, or A2 for Moody's) versus lower rated bonds. The second and third splits are made in such a way that the resulting subsamples have nearly equal sizes. Like d'Amico and King (2013), we separate the subsets by using dummies, which divide the bond purchases into two components:

$$(3) \quad R_i = \beta_{n,0} + d_{n,1}\beta_{n,1}q_n + d_{n,2}\beta_{n,2}q_n + \varphi_n{}'x_i + e_{n,i}$$

where $d_{n,1}$ and $d_{n,2}$ are dummies that equal one for bonds that are part of the first and second subset, respectively, and zero otherwise. This means that there are two purchase coefficients $\beta_{n,1}$ and $\beta_{n,2}$, i.e. one for each subset.

Table 5 presents the resulting purchase coefficients for each of the buckets. The other explanatory variables are not presented, but remain very similar to those based on the aggregate sample. For own purchases, the impact remains statistically insignificant (2SLS regressions) or very low (OLS), but for 1-year and 2-year buckets there are clear asymmetric effects. The impact of purchases is statistically significant for both eligible and non-eligible bonds, though less pronounced for the latter. This result aligns with the Eurosystem's focus on eligible bonds but contrasts with Zaghini (2019) and Mäkinen et al. (2022), who find similar effects across both groups.[12] Mature bonds exhibit stronger effects than recently issued ones. This may reflect that older bonds tend to be less liquid as these are typically held by buy-and-hold investors,

[12] It should be noted, though, that these studies do not focus on local supply effect but on other aspects, such as the impact on the primary market (Mäkinen et al., 2022) and the portfolio rebalancing channel (Zaghini, 2019). The latter study also observes that initially, eligible bonds were affected earlier while the impact on non-eligible bonds followed with a lag, which is in line with the rebalancing mechanism.

requiring larger price adjustments to induce sales, which is in line with the preferred habitat theory. Finally, bonds with lower ratings tend to be more affected than bonds with better ratings for the wider buckets. Apparently, purchases have a dampening effect on the pricing of credit risk that particularly affect bonds with lower credit quality.

Table 5 Different effects between subsets of bonds

	Two-stage least squares			Ordinary least squares		
	Own	1Y bucket	2Y bucket	Own	1Y bucket	2Y bucket
	Overall impact (Equation 1)					
Aggregate purchases	1.3	32.9 **	27.3 **	2.3 **	19.8 ***	26.9 ***
Purchases split by:	*Impact after decomposition (Equation 3)*					
Eligibility						
Not eligible	..	24.7 *	20.0	..	13.0 **	19.1 ***
Eligible	..	29.6 **	26.4 **	..	20.5 ***	27.7 ***
Reject equality	..	*5.8* **	*14.3* ***	..	*8.1* ***	*10.9* ***
Issuance date						
Recently issued	-1.9	35.9 **	27.7	-1.0	13.8 **	20.9 ***
Issuance long ago	7.3	47.7 ***	39.2 **	3.4 ***	21.8 ***	29.7 ***
Reject equality	*9.4* ***	*14.9* ***	*14.0* ***	*4.8* ***	*5.9* ***	*7.2* ***
Credit quality						
Better rating	0.4	28.7	27.3 *	4.4 **	14.9 ***	22.3 ***
Worse rating	1.4	38.7 *	36.2 **	1.5	24.2 ***	31.1 ***
Reject equality	*0.0* ***	*5.0* ***	*4.7* ***	*1.7* ***	*5.6* ***	*4.7* ***

Notes:

The dependent variable is the gross yield over the CSPP programme between one day before the announcement (9 March 2016) and the end of the net purchase phase (20 December 2018). Same specifications as in Table 3, except that the purchase coefficient is estimated separately for two subsets: eligible vs non-eligible bonds, recently issued (up to 1 year) vs issued longer ago and better rating (up to and including A) vs worse rating (other investment grade). The regressions presented here are estimated with two-stage-least squares with the same instruments as our baseline. The only extension we made is that two of the instruments for the wider buckets – the outstanding amounts of purchased securities and corresponding government bonds – have also been split into two variables according to eligibility, time since issuance and rating class.

The equality tests investigate the null hypothesis that the coefficients of the two subcategories are not significantly different from each other; presented are Chi2 statistics.

Superscripts *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

OLS regressions show the same asymmetries between subgroups as 2SLS, with one exception: for own purchases, ratings have the opposite asymmetry (more impact for better ratings). But the low coefficient implies that this asymmetry is not very pronounced, while for the 1-year and 2-year buckets the asymmetry is in line with the 2SLS estimates.

5 Concluding remarks

We find evidence for local supply effects of the Eurosystem's asset purchases under the CSPP, particularly for assets that can be considered substitutes of securities that are actually bought, as defined by maturity buckets. For own bond purchases, we do not significant price effects, which is in line with the Eurosystem's neutrality principle of asset purchases. In addition, the absence of local supply effects for own purchases may by partly attributed to the fact that these often trigger new bond issuances by corporates whose securities have been purchased. Arguably, the latter would still be an indication that CSPP purchases affected individual bonds directly, but through quantity rather than price effects.

We also find that, within the maturity buckets, local supply effects are more pronounced for bonds that were eligible under the CSPP programme (rather than non-eligible bonds), mature bonds (rather than recently issued bonds) and bonds with lower (rather than higher) credit quality.

Our findings imply that asset purchases affect some market segments more than on others. Such an varying impact may affect allocation decisions in the economy, which goes beyond the traditional (macro) focus of monetary policy aiming at price stability. Hence, a central bank may choose to spread out its purchases as much as possible over the universe of eligible bonds, to avoid specific stimulus for particular segments. However, if the central bank pursues secondary objectives to support specific sectors, our results present some preliminary evidence that tilting asset portfolios could be an effective way to achieve these goals.

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