Flow Effects of Central Bank Asset Purchases on Euro Area Sovereign Bond Yields: Evidence from a Natural Experiment

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Abstract

We estimate the response of euro area sovereign bond yields to purchase operations under the ECB's Public Sector Purchase Programme (PSPP), using granular data on all PSPP-eligible securities. To avoid simultaneity bias in the estimated relationship between yields and purchase volumes, we exploit a PSPP design feature that renders certain securities temporarily ineligible for reasons unrelated to their yields. Using these temporary purchase restrictions as an instrument to identify exogenous variation in purchase volumes, we find that the "flow effect" of PSPP has, on average, led to a temporary 7 basis-point decline in sovereign yields on the day of purchase.

JEL Classification: E52, E58, E65, G12.

Keywords: Quantitative Easing; Monetary Policy; Sovereign Yields; Natural Experiment.

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1 Introduction

The literature on the interest-rate term structure has emphasised two channels by which changes in the net supply of debt securities in specific yield-curve segments may create relevant local price pressures.[1] One of these channels derives from preferred habitat motives by which certain investors prefer specific types of securities, defined for instance over their issuer or maturity characteristics. Due to these preferences, a decline in the available supply of specific securities induces investors to bid up their prices – instead of diversifying into other, less-richly valued, market segments. The other channel derives from limited-arbitrage models in which investors transmit local price pressures to other market segments through portfolio rebalancing, but face constraints to their risk-bearing capacity that render the transmission imperfect.

Depending on the relative strength of these channels, variations in the net supply of securities available in specific market segments – for instance due to new issuance or sizeable purchases by market participants – may give rise to very different yield curve constellations. In particular, according to theory, the more relevant are preferred-habitat motives or the more binding are limits to arbitrage, the narrower is the market segment impacted by net supply shocks (or, in other words, the narrower is the interpretation of "local" price pressures). But the relative relevance of these different channels ultimately remains an empirical question.

The asset purchase programmes adopted by several major central banks over recent years provide a rich laboratory to address this question. These programmes have entailed a massive withdrawal in the supply of targeted securities from the market and the scale and pace of withdrawal has varied along the time-series and cross-section dimensions of the respective eligible universe. In this vein, the current paper exploits the Public Sector Purchase Programme (PSPP), which the European Central Bank (ECB) announced in January 2015 and launched two months later, to study the effects of central bank purchases on sovereign bond yields in the euro area.

As such, our analysis adds to a large and growing literature on the strength and timing with which central bank purchase programmes affect sovereign bond markets. Several recent contributions to this literature document that central bank asset purchase programmes have

[1] See, for instance, Culbertson (1957), Modigliani and Sutch (1966), and Tobin (1969) for seminal contributions and Andrés et al. (2004), Vayanos and Vila (2009), and Greenwood and Vayanos (2010) for more recent contributions to this literature.

triggered strong and persistent declines in sovereign bond yields. Most of these yield impacts materialised at the date of programme announcement, or at prior instances when market participants received information that may have altered their expectations on the likely size and modalities of the respective programme.[2]

By contrast, the yield impact of actual purchase operations by which central banks have followed up on their previous announcements is generally held to be small; but while this conjecture has received empirical support in the US and UK contexts, only limited evidence is available for the Public Sector Purchase Programme (PSPP) that the ECB announced in January 2015 and launched two months later.[3]

The aim of the current paper is to close this gap by estimating the "flow effect" of PSPP purchase operations on euro area sovereign bond yields, using security-level data at daily frequency. This issue is relevant because: first, the findings from other major economies might have limited external validity for the euro area due to differences in the structure of financial markets and the modalities of central bank asset purchases in the different jurisdictions; second, the presence of economically relevant flow effects, beyond the anticipation and announcement effects at the onset of the programme, would alter the overall impact to be expected from PSPP on financial conditions and, ultimately, on the broader economy; and, third, the granular structure of our data allows us to assess the relative relevance of narrow versus broader channels by which variations in the net supply of securities in specific market segments affect bond yields more broadly.

From an econometric perspective, a key contribution of this paper is to address simultaneity bias in the estimated relationship between the yields and central bank purchases of a specific security – an issue that has been acknowledged, but never been dealt with, in the related literature on flow effects. Specifically, simultaneity bias may arise if the central bank, in choosing which securities within the eligible universe to buy on a given day (and in which quantities to buy them), accounts for their prevailing yield levels. In the PSPP context, such

[2]For evidence on the US case, see Doh (2010), Gagnon et al. (2011), Krishnamurthy and Vissing-Jorgensen (2011), Meaning and Zhu (2011), D'Amico et al. (2012), D'Amico and King (2013) and Li and Wei (2013); on the UK, see Meier (2009), Joyce et al. (2011), Joyce and Tong (2012), Meaning and Zhu (2011), Breedon et al. (2012), Christensen and Rudebusch (2012) and McLaren et al. (2014); on the euro area, see Altavilla et al. (2015), Andrade et al. (2016), De Santis (2016), and Blattner and Joyce (2016).

[3]See D'Amico and King (2013) and Kandrac and Schlusche (2013) for the US; Joyce and Tong (2012) for the UK; Andrade et al. (2016) for the euro area; and Schlepper et al. (2017) for the German Bund market.

concerns appear warranted because: *(i)* the PSPP legal set-up does not fully determine the precise allocation of purchases to individual securities each day but merely specifies broad programme parameters that do not have to be met on a daily basis;[4] accordingly, the daily allocation of purchases to individual securities is the outcome of a decision problem rather than an exogenous process; *(ii)* the ECB has stated that, in the conduct of PSPP, *"flexibility will be applied, also taking into account the relative values of bonds (...)"*,[5] thus suggesting that prevailing market conditions may indeed enter this decision problem. Accordingly, PSPP purchase volumes and sovereign bond yields are likely to be jointly determined at the level of individual securities.

To address this identification problem, we exploit the "blackout periods" embedded in the PSPP legal set-up, which render certain securities trading in the secondary market temporarily ineligible for reasons unrelated to their prevailing yields. In particular, Article 4(1) of the ECB decision on PSPP stipulates that:

> *"(...) no purchases shall be permitted in a newly issued or tapped security and the marketable debt instruments with a remaining maturity that are close in time, before and after, to the maturity of the marketable debt instruments to be issued, over a period to be determined by the Governing Council ('blackout period')."*
>
> (Decision (EU) 2015/774 of the European Central Bank; Article 4(1))

The blackout periods may be understood as one of the safeguards that ensure compatibility of ECB sovereign bond purchases with European Union law and, specifically, with the monetary financing prohibition.[6] From an econometric perspective, the blackout periods entail an occasionally binding temporary purchase restriction for individual securities in the secondary market, resulting from a design feature that was hardwired in the PSPP legal set-up before the start of the programme and superordinate to the decision problem of purchase officers. We exploit this design feature of PSPP as an instrument to identify exogenous variation in central bank purchase volumes, using a two-stage least squares regression set-up.

We find that the flow effects of PSPP operations have reduced sovereign bond yields by, on average, 7 basis points on the day of purchase. This impact derives from both, a reduction

[4] See section 3 for further detail.
[5] See, ECB (2016): https://www.ecb.europa.eu/mopo/implement/omt/html/pspp-qa.en.html.
[6] See, for instance, ECB (2015).

in the yields of the securities being purchased and from a transmission of the yield impact to other securities that market participants may perceive as “substitutes”. This transmission, in turn, extends over large yield-curve segments, with purchases exerting significant effects on the yield of securities with a remaining maturity of up to eight years different from the security being purchased. The yield impact on the securities being purchased fades out after three trading days, whereas the impact on substitutes is more persistent, only loosing statistical significance six trading days later.

Overall, these findings corroborate the view that, on the one hand, changes in the supply of securities in specific yield curve segments create relevant local price pressures and, on the other hand, arbitrage activity partly transmits these price pressures along the yield curve. From an academic perspective, the paper thus lends support to models combining preferred habitat and limited arbitrage theories of the term-structure (such as that developed in Vayanos and Vila (2009)) that point to local supply effects as an important transmission channel of quantitative easing – on top of the (duration) risk extraction and signalling effects emphasised in earlier contributions to the related literature (see Gagnon et al. (2011) and Bauer and Rudebusch (2014), respectively).

From a policy perspective, the relevance of local price pressures implies that central banks may be able to steer the shape of the yield curve through the calibration of purchases (as regards, for instance, their maturity composition and, in a multi-country setting such as the euro area’s, the weight of different issuers in the purchase envelope). This, in turn, provides a potential avenue to calibrate the sectoral and geographical incidence of the monetary stimulus originating from asset purchases (and, in a normalisation phase, the withdrawal of that stimulus).

In terms of overall size, our flow effects estimates amount to around twice the impact D’Amico and King (2013) estimate for the “LSAP1” Treasury securities purchases by the US Federal Reserve that took place from March to October 2009 and that entailed a broadly similar monthly purchase envelope as PSPP, relative to the respective eligible universe.[7] This discrepancy is particularly striking in view of the tranquil financial market conditions prevail-

[7]The scaling of purchase flows per unit of time by the respective eligible universe is consistent with the practice adopted in the related literature; see section 2 for further detail. At the same time, the comparison between the estimates from different studies is subject to the caveat that, due to imperfect overlap in model set-ups and variable definitions, we cannot establish whether the discrepancy in impact assessments is significant in a statistical sense.

ing over most of the PSPP implementation period and US evidence indicating that the flow effects of Treasury purchases tended to disappear during later US Federal Reserve asset purchases programmes that took place under lower financial distress than LSAP1 (see Kandrac and Schlusche (2013)). As one potential driver, the discrepancy in impact estimates across economies may reflect differences in the structure of the sovereign bond market which, in the US, consists of a deep and liquid pool of fairly homogenous debt securities whereas, in the euro area, it displays substantial heterogeneity in terms of market depth and issuer characteristics; the latter environment, in turn, is likely to give rise to a more sluggish price discovery mechanism and, hence, a higher impact of ongoing purchase operations on yields. However, the discrepancy may also derive from methodological differences, since our empirical set-up – in contrast to the related literature – addresses endogeneity concerns that may lead to a downward bias in flow effect estimates (in absolute terms).

Notwithstanding these relatively sizeable estimates, our results indicate that flow effects accounted for only a limited share of the overall impact of PSPP on sovereign yields in the euro area, which is in line with the patterns observed for other economies. Instead, most of the downward pressure of PSPP on sovereign bond yields seems to have derived from "stock effects", which materialised in anticipation and upon announcement of key programme parameters. According to event study evidence, these effects have lowered 10-year sovereign bond yields by around 50 basis points.[8]

The remainder of the paper is structured as follows. Section 2 provides details on the identification strategy and specification used in the empirical analysis. Section 3 describes the data and institutional background. Section 4 presents results and section 5 concludes.

2 Model and identification strategy

We estimate the following equation:

$$y_{it} = \beta Q_{it}^{0} + \sum_{j=1}^{J} \gamma_j Q_{it}^{j} + u_i + v_t + \varepsilon_{it} \qquad (1)$$

[8] See Altavilla et al. (2015), Andrade et al. (2016), and De Santis (2016).

where y_{it} denotes the yield of security i on day t; Q_{it}^{0} is the nominal amount of central bank purchases of that security on day t (in % of its nominal outstanding amount); Q_{it}^{j} is the amount of central bank purchases of "substitutes" for security i on day t, defined as securities issued in the same country but located in different maturity buckets than security i (in % of the sum of outstanding amounts of all securities in substitute category j; see section 4 for further detail); u_i are security-fixed effects; v_t are day-fixed effects; and ε_{it} is an error term. β and γ_j are the slope parameters on the respective central bank purchase variables.

This basic set-up closely follows the related literature in that it allows central bank asset purchases to not only exert a direct effect on the yield of the respective security being purchased (as captured by β), but also an indirect effect on non-targeted securities with similar features that market participants may perceive as substitutes (as captured by γ_j). Direct effects on purchased securities may derive from the presence of preferred-habitat investors (see Vayanos and Vila (2009)) or from impairments in market liquidity (see Babbel et al. (2004)).[9] Indirect effects on the yield of substitute securities may derive from arbitrageurs' exploiting price differentials along the yield curve and thereby transmitting the local effect of central bank purchases to other maturity segments (Vayanos and Vila (2009)).

The use of normalized central bank purchase variables (in % of outstanding amounts) is motivated by the assumption that the scarcity induced by a given euro amount of purchases depends inversely on the total size of the respective security or market segment (see Joyce and Tong (2012); D'Amico and King (2013); and Kandrac and Schlusche (2013)). Like the related literature, we record the purchase variables at nominal value, rather than market values at the time of purchase, since the latter also reflect the prevailing price of the security which may give rise to a mechanical relationship between the dependent and explanatory variables in equation 1. But robustness checks with purchase variables based on market values produced very similar results.

In section 4.5, we also assess the robustness of our choice of dependent variable to non-stationarity concerns and, in this context, re-run the main specifications with daily bond returns (defined as the first difference of bond prices) as dependent variable.

[9] Preferred habitat investors value specific security characteristics and are willing to bid up their prices in order to restore their portfolios in response to the local scarcity in the supply of such securities induced by central bank purchases. Impairments in market liquidity may lead to sluggish price discovery so that arbitrage only gradually removes price differentials.

As explained above, we face a complex identification problem in estimating equation 1: OLS is likely to yield inconsistent estimates since central bank purchasing officers, in allocating overall purchase volumes to individual bonds, may be attentive to the constellation of yields, $Y_t = (y_{1t}, ..., y_{Nt})$, prevailing in the market on a given day. If indeed the case, yields and purchase amounts would be jointly determined such that, for each security i, $Q_{it} = f(Y_t)$ with $f' \neq 0$ for at least some of the elements in Y_t. This, in turn, would render estimates of the coefficients of interest, β and γ_j, subject to simultaneity bias.

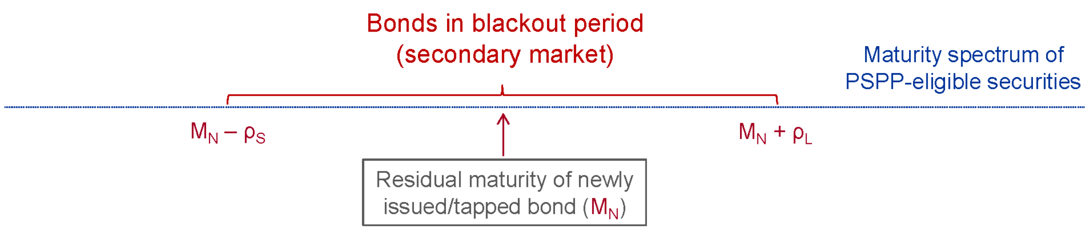
While the direction of bias is unobservable, ECB communication has emphasised its intention to ensure that PSPP purchases "create as little distortion as possible",[10] which may indicate an inclination to favour bonds that are trading at a discount over those trading at a premium – an approach that would resemble the practice observed for the US Fed (see D'Amico and King (2013)). In this case, OLS would risk underestimating the flow effects of purchases on sovereign bond yields (*i.e.* the coefficients β and γ_j would be biased upward) as the estimated slope coefficients would capture not only the (downward) impact of central bank purchases on bond yields but also the tendency for bonds with relatively high yields to attract higher purchase volumes.[11]

To address this concern, we exploit the blackout periods embedded in the PSPP legal set-up in a two-stage least squares (2SLS) instrumental variable estimation, using the time-window during which a specific security is subject to the blackout period as an excluded instrument. To illustrate the mechanism, Figure 1 plots a hypothetical segment of the PSPP-eligible maturity spectrum at the time of a primary market issuance on that segment. The ECB is not permitted to participate in the primary market under the EU Treaties, so the newly issued security N (with maturity M_N at issuance) is not PSPP-eligible. For securities trading in the secondary market, where the EU Treaties in principle permit sovereign bond purchases, eligibility depends on whether a security falls within the scope of the blackout period or not. In particular, the blackout periods prohibit purchases only of securities with residual maturities *"that are close in time, before and after"* the newly issued security, as illustrated by ρ_S and ρ_L. Accordingly, securities in the secondary market whose residual maturity falls within the inter-

[10]See https://www.ecb.europa.eu/mopo/implement/omt/html/pspp-qa.en.html.

[11]For similar considerations in the context of the ECB's Securities Markets Programme, see Ghysels et al. (2014).

Figure 1: Illustration of identification strategy using blackout periods



val $[M_N - \rho_S, M_L + \rho_L]$ are ineligible due to the blackout periods, whereas otherwise similar securities whose residual maturity falls just outside this interval can still be bought. Since the blackout periods have constituted a fixed and binding constraint since the inception of PSPP, the resultant variation in purchase volumes is exogenous to their prevailing market value and to other factors that may confound the identification of β and γ_j.

We formalise this institutional setting with a dummy variable D^0_{it} that takes value 1 if security i is in blackout on day t and 0 otherwise. For substitutes we apply a similar logic but, given the substitute ranges consist of a number of securities that may be in blackout at different points in time, we define the blackout variable D^j_{it} for each substitute category j as the average number of securities in the respective substitute category that was in blackout on day t.

Accordingly, the first-stage regressions in the 2SLS set-up take the form:

$$\begin{pmatrix} Q^0_{it} \\ Q^j_{it} \end{pmatrix} = \delta_0 D^0_{it} + \sum_{j=1}^{J} \delta_j D^j_{it} + \upsilon_i + \phi_t + \eta_{it} \tag{2}$$

where υ_i and ϕ_t again denote security- and day-fixed effects, respectively, η_{it} is an error term, and all other variables are defined as explained above. These regressions are estimated separately for the "own-purchase" variable, Q^0_{it}, and for each of the "substitute-purchase" variables, Q^j_{it}, with J ranging between 1 and 4, depending on the specification (see section 4). Unless noted otherwise, the panel estimations are based on the full set of eligible securities. Since each day only a sub-set of these securities is in the blackout period and the blackout-status is temporary, the sample provides for non-trivial cross-section and time-series variation in the excluded instruments, D_{it}^k for $k = 0, ..., J$. We exploit this variation to estimate the coefficients $\hat{\delta}_k$ and, using these coefficients, compute fitted values of the purchase variables, $\hat{Q}_{it}^k$. Under the exogeneity assumption for the excluded instruments, variation in $\hat{Q}_{it}^k$ is independent from prevailing yields. The second-stage regression corresponds to equation 1, but replaces each of the purchase variables with their fitted values $\hat{Q}_{it}^k$ from equation 2 to obtain causal estimates of the slope coefficients, β and γ_j.

3 Data and institutional background

The data consist of daily observations on the entire PSPP-eligible universe, comprising 3,061 securities over the period from March 9, 2015 (the day PSPP was launched) to June 21, 2016.[12] Overall, the sample includes around 900,000 observations, thus extending well beyond the sample size available for similar studies in the US or UK contexts.[13]

The eligible universe, as specified in the PSPP legal set-up over the sample period, comprised securities with a residual maturity between 2 and 30 years issued by euro area central, regional and local governments, as well as recognised agencies, international organisations and multilateral development banks located in the euro area.[14] Moreover, for securities to be eligible, the credit rating of the issuer has had to exceed a certain minimum threshold, except for countries that participate in a financial assistance programme by the European Stability Mechanism, in which case eligibility could be restored despite a lower rating. Over the sample period, securities trading at a yield below the ECB deposit facility rate were not eligible; and the cumulative Eurosystem holdings per security and per issuing entity could not exceed 33%, respectively, except for EU supranational institutions, where these limits have stood at 50%.[15]

[12] The sample is unbalanced since some securities dropped out of the eligible range and some were newly issued.

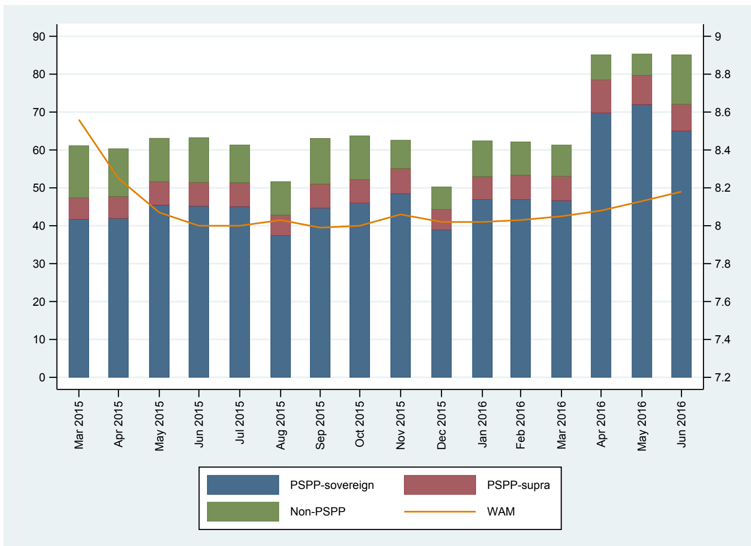
[13] As a comparison, D'Amico and King (2013) and Kandrac and Schlusche (2013) use unbalanced panels with around 1,000 and 16,000 observations, respectively, while Joyce and Tong (2012) work with around 2,500 observations in their baseline specifications.

[14] The database includes all euro area countries except Greece, which has not been eligible for PSPP, and Estonia, which has no presence in sovereign debt markets. Note that, technically, PSPP is conducted in a decentralised manner meaning that a large part of actual purchases are conducted by the National Central Banks that, together with the ECB, make up "the Eurosystem". For expositional ease, however, we will henceforth refer to these different entities as "the ECB".

[15] The ECB increased the issue share limit from 25% to 33% in its Governing Council monetary policy meeting on 3 September 2015 and to 50% for bonds issued by EU supranational institutions in its Governing Council monetary policy meeting on 10 March 2016.

Beyond these eligibility criteria, the PSPP legal set-up also specifies broad purchase modalities, including an overall monthly envelope which amounted to €60 billion from March 2015 to March 2016 and €80 billion thereafter. This envelope, however, has applied to the full set of ECB purchase programmes (*i.e.* including – besides PSPP – also the covered bond, asset-backed securities (ABS) and corporate sector purchase programmes), so the precise PSPP purchase amounts have not been determined by the legal acts. Over the sample period, the share of PSPP in the overall monthly purchase envelope fluctuated around an average of 83% (see Figure 2).

Figure 2: Allocation of ECB asset purchases to different programmes and issuer categories (in €-billion, left axis) and weighted average maturity of purchases (in years, right axis)



Source: European Central Bank.

Note: PSPP-sovereign refers to securities issued by the general government sector or recognised agencies in euro area countries. PSPP-supra refers to securities issued by international organisations and multilateral development banks. Non-PSPP refers to securities eligible under the covered bond, ABS and corporate sector purchase programmes. WAM is the weighted average maturity of PSPP portfolio holdings.

A second set of modalities specified in the PSPP set-up relates to the allocation of purchases across different types of entities and across countries. The former distinguishes between: *(i)* government bonds and recognised agencies, to which 90% of the PSPP purchase volumes are allocated, and *(ii)* international organisations and multilateral development banks, which receive the remainder.[16] The allocation of purchase volumes across countries follows

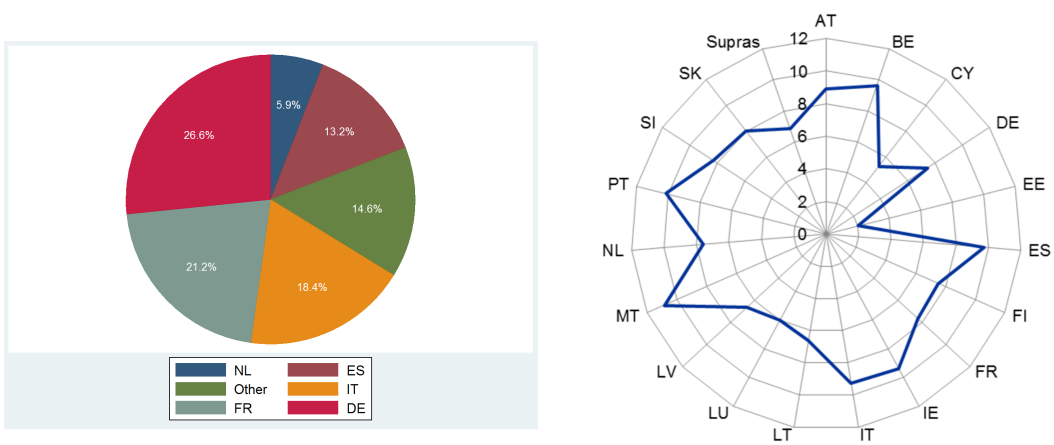
[16]The initial split from March 2015 to March 2016 allotted 88% of the total purchases to government bonds and recognised agencies and 12% to securities issued by international organisations and multilateral development banks.

their respective shares in the ECB capital key, which broadly corresponds to the relative size of the respective economy (see Figure 3, left panel).

For the allocation of purchases across maturity buckets, the PSPP set-up does not specify a precise target, but the ECB has expressed an intention to weigh different maturity buckets in a "market-neutral" manner and, within each jurisdiction, to allocate purchases "roughly according to the nominal amount outstanding".[17] The resultant average maturity, weighted by relative purchase amounts, has hovered around 8 years (see Figure 2), but with substantial cross-country heterogeneity, ranging from 7 to 10 years for the five largest euro area countries and showing an even larger dispersion among smaller countries (see Figure 3, right panel).

Figure 3: Composition of PSPP purchases

Country-allocation (in percent) *Weighted average maturity (in years)*



Source: European Central Bank.
Note: the left panel shows the share in overall monthly net PSPP purchase volumes (excluding purchases of EU-supras) for the five largest euro area countries – comprising Germany (DE), France (FR), Italy (IT), Spain (ES), and the Netherlands (NL) – as well as for the other euro area countries. The right panel shows the weighted average maturity by issuer (equivalent to the euro area aggregate measure shown in Figure 2).

In contrast to the asset purchase programmes adopted by the US Federal Reserve and the Bank of England, PSPP implementation generally does not rely on auctions.[18] Instead, the ECB retains a permanent presence in the markets and conducts purchases via bilateral transactions with eligible counterparties on a daily basis. Regarding the precise conduct of

[17] See: https://www.ecb.europa.eu/mopo/implement/omt/html/pspp-qa.en.html and Cœuré (2015).

[18] As an exception, some purchases have been conducted via auctions by a small subset of National Central Banks. But these auctions have taken place on a trial basis and made up only a very limited share of overall purchases.

purchases, the programme parameters leave substantial flexibility – as visible, for instance, from the monthly variation in overall ECB asset purchase volumes, their allocation to different constituent programmes and, within PSPP, to different issuer categories and maturity buckets. Taken together, the PSPP has thus created a rich data set with variation along several dimensions, including maturities, credit quality, and other characteristics such as market size and liquidity. The use of security-level data allows us to exploit this heterogeneity, while using the natural experiment deriving from the "blackout periods" to draw causal inference.

In the analysis, the key variables of interest are bond yields, the daily amounts purchased under the PSPP, the outstanding amounts of the respective securities, and the assignment of securities to the blackout period. The security-specific information on purchase volumes and blackout periods is based on a proprietary and confidential data set of the ECB and the data on bond yields and outstanding amounts are from Bloomberg.

4 Results

4.1 Instrumental variables estimation and comparison to OLS

Table 1 presents flow effects estimates from our baseline specification. Besides "own purchases" (Q_{it}^0), this specification includes, as explanatory variables, four categories of substitutes, with the closest ($j = 1$) consisting of all securities issued in the same country with a residual maturity up to two years different from security i and further substitute categories being defined in two-year intervals.[19] The furthest substitute category consists of securities with maturities between six and eight years different from security i. Facilitated by the large sample, this breakdown of substitute categories is more granular and broader than that adopted in the literature.[20] We experimented with alternative definitions of substitute categories, moving to one- or three-year intervals and changing the furthest maturity buckets considered in

[19] That is: securities in category $j = 2$ have a residual maturity between two and just under four years different from security i. Securities in category $j = 3$ have a residual maturity between four and just under six years different from security i. And securities in category $j = 4$ have a residual maturity between six and just under eight years different from security i.

[20] For instance, Kandrac and Schlusche (2013) only consider one substitute category spanning securities with a maturity up to one year different from security i; D'Amico and King (2013), in their baseline flow effects regressions, consider two substitute categories, one with a maturity difference of up to two years and the other with a maturity difference between two and six years; in some of their specifications, they also add another category with a maturity difference between six and fourteen years which is the same as the set considered in Joyce and Tong (2012).

the substitute categories; but formal specification tests (based on the Akaike and Bayesian information criteria) pointed to the above baseline as the most informative choice.[21]

The estimates from this baseline specification point to statistically significant and economically relevant negative flow effects of PSPP on euro area sovereign bond yields (see first column of Table 1). The coefficients on the substitute purchase variables are all statistically significant at a 1% level and the coefficient on own purchases is significant at a 5% level (based on heteroskedasticity-robust standard errors clustered by security).[22] The overall flow effect of PSPP purchase operations, derived by applying the regression coefficients to the sample averages of the respective purchase variables, amounts to an average reduction in sovereign bond yields of 7 basis points on the day of purchase.[23] Accordingly, central bank asset purchases have exerted considerable downward pressure on euro area sovereign yields in addition to the market adjustments that materialised on the back of (pre-)announcement effects.

To interpret the individual regression coefficients and to compare them across the own- and substitute purchase categories, it is convenient to back out the implied impacts for each €-billion in own purchases. For own purchases, this impact is calculated as $\tilde{F}^0 = \hat{\beta} \times 1bn/\bar{B}^0$ where $\hat{\beta}$ is the estimated coefficient for purchase variable Q^0_{it} and $\bar{B}^0$ is the average of the outstanding amounts used to normalize that variable (see section 2). For substitute purchase variable j, the effect is calculated as $\tilde{F}^j = \frac{\bar{Q}^j}{\bar{Q}^0} \times \hat{\gamma}_j \times 1bn/\bar{B}^j$ where the notation is analogous to the previous sentence and the first term on the right-hand side accounts for the fact that, on average, own purchases do not translate into an equivalent amount of substitute purchases.[24]

Re-scaling coefficients accordingly, the baseline regression coefficients imply that a €1 billion increase in the purchases of security i on day t, on average, triggers a 4 basis point

[21] Specifically, the baseline specification with four substitute purchase categories consisting of two-year intervals had lower AIC and BIC (amounting to, respectively, -486,216.9 and -486,158.4) than alternative specifications with: eight categories consisting of one-year intervals (AIC: -448,606.0; BIC: -448,500.8); three categories consisting of three-year intervals (AIC: -472,162.9; BIC: -472,116.2); and the substitute categories considered in D'Amico and King (2013) (AIC: -484,518.4; BIC: -484,483.4).

[22] The analyses were performed using Stata and, specifically, the reghdfe package for instrumental variables estimation; see Correia (2014).

[23] Formally, the combined effect is calculated as $\hat{\beta}\bar{Q}^0 + \sum_{j=1}^{4} \hat{\gamma}_j \bar{Q}^j$ where $\hat{\beta}$ and $\hat{\gamma}_j$ are the estimated coefficients for own purchases and the substitute purchase variables $j = 1, \ldots, 4$, respectively; and $\bar{Q}^0$ and $\bar{Q}^j$ are the sample averages of the respective purchase variables as a ratio of their outstanding amounts.

[24] The exact mapping between $\bar{Q}^0$ and $\bar{Q}^j$ depends on the distribution of purchases and outstanding amounts across maturity segments. Intuitively, wedges between these variables may arise because not all securities are within the same substitute category. Suppose, as an illustrative example, that the central bank purchases 1% of the respective outstanding amounts of only two securities with remaining maturities that are more than 8 years apart (so they are not considered substitutes). Then, $\bar{Q}^0 = 1$ and $\bar{Q}^j = 0$ for all $j = 1, \ldots, 4$.

Table 1: Flow effect estimates – 2SLS versus OLS

	2SLS	OLS	2SLS	2SLS	2SLS	2SLS
Own purchase	-0.108**	-0.002**	-0.138***	-0.092*	-0.100*	-0.087*
	(0.055)	(0.001)	(0.049)	(0.050)	(0.052)	(0.052)
Substitute <2y	-0.575***	-0.059***		-0.461***	-0.483***	-0.576***
	(0.148)	(0.007)		(0.123)	(0.131)	(0.148)
Substitute <4y	-0.404***	0.031***			-0.473***	-0.379***
	(0.136)	(0.009)			(0.140)	(0.137)
Substitute <6y	-0.329***	0.038***				-0.427***
	(0.102)	(0.008)				(0.101)
Substitute <8y	-0.375***	0.037***				
	(0.076)	(0.008)				
Observations	876038	876039	876038	876038	876038	876038
K-P weak IV stat	64.184	-	420.619	201.928	126.088	98.300
S-Y critical values	-	-	16.38	7.03	-	-

Note: this table reports estimates of equation 1, with cluster-robust standard errors in parentheses. Dependent variable is the yield to maturity. Asterisks indicate statistical significance at 10% (*), 5% (**), and 1% (***) levels. "*K-P* weak IV stat" refers to Kleibergen-Paap rk Wald F-statistic for the respective specification and "*S-Y* critical values" to the corresponding Stock-Yogo critical values for 10% maximal Wald test size distortion.

decline in the yield of that security and a 0.6-1.4 basis point decline in the yield of securities falling into the different substitute categories. Hence, the flow effects are strongest on the securities being purchased but also trigger some non-negligible transmission across the maturity spectrum.[25] The estimated effect of own purchases broadly corresponds to the mid-point of the range of price impacts that Schlepper et al. (2017) estimate for the German government bond market.

Given our emphasis on the need to instrument the purchase variables, the second column of Table 1 presents results from a simple OLS estimation of equation 1 that does not instrument the purchase variables and, thus, abstracts from potential endogeneity concerns. The resultant coefficients, in absolute terms, are substantially smaller than those from the baseline estimation (for own purchases, for instance, amounting to merely 2% of the IV regression estimate); and for some of the substitute categories, the coefficients even have a counterintuitive positive sign. Accordingly, the comparison between the baseline IV estimates and the OLS results is

[25] In interpreting the re-scaled impact ($\tilde{F}^0$ and $\tilde{F}^j$), it is also important to recall that the substitute categories typically include a large number of securities, so the outstanding amounts by which the estimated coefficients are divided in the previous step are much larger than for own purchases, which only affect one security at a time.

qualitatively consistent with our conjecture that failing to account for simultaneity between yields and purchase volumes may lead to an underestimation of flow effects in absolute terms. This may also explain why related analyses for the PSPP have not found statistically significant flow effects (Andrade et al. (2016)).

In view of the large number of endogenous regressors in the baseline specification, the last four columns present estimates from more parsimonious models with no or fewer substitute categories as explanatory variables. The resultant coefficient estimates are very close to the baseline specification and remain statistically significant (albeit, in some of the specifications, only at a 10% level for Q^0_{it}).

4.2 First-stage regression for endogenous regressors

Table 2 turns to the first stage regression of the endogenous regressors on the excluded instruments and the full set of security- and day-fixed effects. In the regression for own purchases (Q^0_{it}), the coefficient on the blackout dummy (D^0_{it}) reported in the first column is highly significant (with a *t-statistic* of 20.5) and has the expected negative sign, consistent with the fact that blackout periods impose a purchase volume of zero, whereas securities outside the blackout period, on average, attract positive purchase volumes.[26] The remaining columns show the equivalent first-stage estimates for the different substitute categories. Again, the coefficients on the instruments for the respective purchase categories (*i.e.* D^1_{it} for Q^1_{it} etc.) are highly significant (with *t-statistics* between 14.2 and 19.0) and have the expected negative sign.

Interestingly, in some of the first-stage regressions, the explanatory variables corresponding to other substitute categories have a statistically significant, albeit small, positive effect (see *e.g.* the coefficients on D^1_{it} and D^3_{it} in the regression explaining Q^2_{it}, shown in column "$Sub < 4y$" of table 2). This pattern indicates that purchase managers may respond to a higher share of securities in blackout period in one maturity bucket by temporarily shifting purchase volumes to other maturity buckets.

For each of the first-stage regressions, the conditional *F-statistics* (based on Sanderson and Windmeijer (2016)) are well above the *Stock-Yogo* critical values for relative bias and *Wald-test* size distortions (see Stock and Yogo (2005)), thus clearly refuting weak identification

[26] Recall that there are three sorts of securities: those being purchased, those not being purchased on account of their being in a blackout period, and those not being purchased but, in principle, eligible on that day.

Table 2: PSPP purchase variables and instruments – first-stage regression

	Own	Sub <2y	Sub <4y	Sub <6y	Sub <8y
Blackout (own)	-0.070***	-0.000	0.001	0.000	0.004
	(0.003)	(0.001)	(0.002)	(0.001)	(0.003)
Blackout <2y	-0.002	-0.126***	0.033***	0.029**	0.026***
	(0.018)	(0.007)	(0.012)	(0.014)	(0.006)
Blackout <4y	0.023*	0.011**	-0.121***	0.012	0.013**
	(0.012)	(0.006)	(0.009)	(0.012)	(0.006)
Blackout <6y	-0.005	0.028***	0.031**	-0.151***	-0.005
	(0.010)	(0.006)	(0.014)	(0.008)	(0.009)
Blackout <8y	0.048**	0.020***	0.006	0.025***	-0.165***
	(0.023)	(0.005)	(0.005)	(0.005)	(0.009)
Observations	876038	876038	876038	876038	876038
S-W weak IV stat	385.92	225.50	301.52	221.61	299.96
S-Y critical values	26.87	26.87	26.87	26.87	26.87

Note: this table reports estimates of equation 2, with cluster-robust standard errors in parentheses. Each column corresponds to a different first-stage regression with one of the purchase variables as dependent variable. "Own" refers to Q^0_{it}; "Sub <2y" to Q^1_{it}; "Sub <4y" to Q^2_{it}; "Sub <6y" to Q^3_{it}; and "Sub <8y" to Q^4_{it}. The rows show estimates for the excluded instruments. "Blackout (own)" refers to D^0_{it}; "Blackout <2y" to D^1_{it}; "Blackout <4y" to D^2_{it}; "Blackout <6y" to D^3_{it}; and "Blackout <8y" to D^4_{it}. Asterisks indicate statistical significance at 10% (*), 5% (**), and 1% (***) levels. "S-W weak IV stat" refers to Sanderson-Windmeijer conditional F-statistic for each of the individual endogenous regressors and S-Y critical values to the corresponding Stock-Yogo critical values for 10% maximal Wald test size distortion.

concerns for the single endogenous regressors. This assessment also finds support for the full regression given the high *Wald F-statistic* (based on Kleibergen and Paap (2006); see bottom of Table 1).[27]

4.3 Heterogeneity across sub-samples

This section explores whether flow effects differ across sub-samples of the PSPP-eligible universe. The related literature has identified various market and security characteristics that may alter the impact of central bank asset purchase programmes on yields. First, purchase pro-

[27] The *Stock-Yogo* critical values commonly used as a benchmark for this *Kleibergen-Paap (K-P)* statistic are only computed for regressions with up to two or three endogenous regressors. For the more parsimonious specifications in Table 1 that meet this constraint, the *K-P statistic* is firmly above the reported *Stock-Yogo* critical values. For the larger models, the direct comparison is not feasible, but the respective *K-P statistics* remain above the critical values reported for the smaller specifications. Together with the fact that the critical values reported in Stock and Yogo (2005) tend to decline with the number of endogenous regressors, this provides further reassurance that the regressions pass the weak identification test (see Stock and Yogo (2005) Table 5.1 and Kraay (2015) for an application).

grammes may compress credit risk premia, thus exerting stronger effects on asset classes or issuers whose securities, ceteris paribus, trade at higher yields.[28] Second, yield impacts may depend on the maturity of targeted securities, albeit with different implications at different stages of a programme: while (pre-)announcement effects are typically stronger for securities with a higher residual maturity, consistent with a higher expected duration risk extraction per unit of purchases, the pattern is less clear-cut for flow effects, which have been found to fall (rise) in the maturity of targeted securities in the US (UK).[29] Finally, flow effects may differ depending on market liquidity conditions as the price response to purchase activity is likely to be amplified and prolonged when it is more costly to exploit price differentials through market trading.[30]

To explore these potential sources of heterogeneity, we augment the basic flow effects model with a set of interaction terms that allow the coefficients on the purchase variables to differ across sub-samples. Since this approach implies a doubling of the endogenous regressors and gives rise to weak identification problems when applied to the full model with five purchase variables, we focus on a more parsimonious setup, restricting the set of explanatory variables to own purchases, Q^0_{it}, and the closest substitute category, Q^1_{it}, as well as the corresponding interaction terms (and the full set of security- and day-fixed effects).[31] The resultant specification is given by:

$$y_{it} = \beta Q^0_{it} + \gamma_1 Q^1_{it} + \beta^H Q^0_{it} \times H_i + \gamma^H_1 Q^1_{it} \times H_i + u_i + v_t + \varepsilon_{it} \tag{3}$$

where H_i is a dummy variable that groups observations according to different characteristics (see below); $Q^0_{it} \times H_i$ and $Q^1_{it} \times H_i$ are interactions between this dummy and the purchase variables; and all other variables are defined as in equation 1 (since H_i is perfectly collinear with

[28] For differences across asset classes see, e.g., Krishnamurthy and Vissing-Jorgensen (2011). For differences across issuers see, e.g., Altavilla et al. (2015) and De Santis (2016) (who study (pre-)announcement effects) and Eser and Schwaab (2016) (who study programme implementation effects).

[29] For a discussion on the role of duration risk extraction for announcement effects, see Gagnon et al. (2011); D'Amico et al. (2012); Cahill et al. (2013); Altavilla et al. (2015); and Andrade et al. (2016). For evidence on differences in flow effects for bonds with different residual maturities, see Joyce and Tong (2012) and D'Amico and King (2013).

[30] See, in particular, Pelizzon et al. (2016) on the role of bond market liquidity and its interaction with non-standard monetary policy in the euro area. For the interaction between central bank asset purchases and bond market liquidity, see Babbel et al. (2004); D'Amico and King (2013); and Kandrac and Schlusche (2013).

[31] An alternative approach would be to split the sample along the respective dimensions and estimate separate regressions, but we encountered the same weak identification concerns when using this approach.

the fixed effects it is not included in the specification as a separate regressor). We again estimate equation 3 via 2SLS, with D^0_{it}, D^1_{it}, $D^0_{it} \times H_i$, and $D^1_{it} \times H_i$ acting as excluded instruments.

In the specification considering the role of credit premia, H_i takes value 1 (value 0) for all countries whose benchmark 10-year sovereign bond yield spread vis-à-vis Germany exceeded (fell below) the median among the sample countries on 6 March 2015, the last business day before PSPP purchases started.[32] The grouping for liquidity follows the same approach but uses bid-ask spreads on 6 March 2015 as a criterion to rank and subdivide the countries into groups.[33] Finally, differences in maturity are captured by assigning value 1 to H_i for all securities with a remaining maturity that exceeds the mid-point of the eligibility range (standing at 16 years) and 0 otherwise.

The sub-sample analysis does not point to significant differences in the flow effects of own purchases in low- versus high-yield jurisdictions, while showing a stronger transmission to close substitutes in the latter group (see Table 3). Considering only the effect of own purchases (first column), the estimated coefficient for the group of low-yield countries (at -0.098) is similar to the corresponding coefficient for the full sample (see third column in Table 1); but it does not meet the critical value for significance at a 10% level ($p = 0.107$). The combined coefficients for the high-yield group (*i.e.* $\partial y_{it}/\partial Q^0_{it}$ for $H_i = 1$) are significant at a 1% level as visible from the *t-statistic* in the bottom of the table (calculated based on the variance-covariance matrix of $\hat{\beta}$ and $\hat{\beta}^H$); the combined coefficients also point to a somewhat stronger flow effect of own purchases in high-yield countries but the difference to the low-yield group is not statistically significant. By contrast, the impact on close substitutes is significantly stronger in high-yield countries (second column). In line with the findings for the (pre-)announcement

[32]The use of yields prevailing before the start of purchases aims at avoiding that the country-ranking is influenced by differential flow effects (we also experimented with interactions between purchase variables and real-time country-specific continuous variables for bid-ask spreads and credit-default swap spreads but did not find robust evidence for heterogeneity at this level of granularity). The countries falling in the high-yield group include: Belgium, Ireland, Spain, Italy, Portugal, and Slovakia; the countries in the low-yield group include Germany, the Netherlands, Austria, France, and Finland. The remaining countries are excluded, either due to the limited size of their bond markets, lack of a benchmark bond that could be used to include them in the ranking, or ineligibility. While no comparable benchmark yield curve is available for the securities issued by different types of EU supranational institutions, these securities are assigned to the low-yield group given their average yields fall well below those recorded for most of the countries.

[33]On the use of bid-ask spreads as a measure of market liquidity, see Amihud and Mendelson (1986), as well as Beber et al. (2009) in the euro area context. The countries falling in the high bid-ask spread group include: Belgium, Finland, Ireland, Spain, Portugal, and Slovakia; the countries in the low bid-ask spread group include Germany, the Netherlands, Austria, France, and Italy. Securities issued by EU institutions are also assigned to the low bid-ask spread group.

effects of PSPP, this pattern implies that actual purchase operations may have contributed to a narrowing of sovereign spreads across euro area countries.

Table 3: Sub-sample analysis across jurisdictions and maturity segments

Distinction by:	Yield		Bid-ask spread		Maturity	
Own purchase	-0.098	-0.101	-0.114*	-0.090	-0.114*	-0.077
	(0.061)	(0.062)	(0.064)	(0.066)	(0.059)	(0.061)
Own purchase × dummy	-0.134	-0.015	-0.143	-0.051	-0.383*	-0.139
	(0.116)	(0.121)	(0.127)	(0.131)	(0.209)	(0.238)
Substitute <2y		0.215		-0.286		-0.448**
		(0.209)		(0.245)		(0.188)
Substitute × dummy		-1.188***		-0.494		-0.722**
		(0.359)		(0.370)		(0.326)
K-P weak IV stat	117.885	42.187	150.750	62.033	163.423	79.917
S-Y critical values	7.03	-	7.03	-	7.03	-
t-statistic for $H_i = 1$:						
Own purchase	2.375	1.122	2.355	1.236	2.530	0.952
Substitute <2y	-	3.977	-	3.407	-	4.840

Note: this table reports estimates of equation 3, with cluster-robust standard errors in parentheses. The first two columns split the sample into countries whose spreads relative to Germany exceeded (fell below) the median before the start of purchases; the third and fourth columns split the sample into countries whose bid-ask spreads exceeded (fell below) the median before the start of purchases; the third and fourth columns split the sample into securities with residual maturity above (below) 16 years. Dependent variable is the yield to maturity. Asterisks indicate statistical significance at 10% (*), 5% (**), and 1% (***) levels. "*K-P* weak IV stat" refers to Kleibergen-Paap rk Wald F-statistic for the respective specification and "*S-Y* critical values" to the corresponding Stock-Yogo critical values for 10% maximal Wald test size distortion.

There are no discernible differences between flow effects for countries characterised by relatively high or low bid-ask spreads – neither on the securities being purchased, nor on close substitutes (see third and fourth column). A potential interpretation is that the relatively calm financial conditions prevailing over the sample period have rendered liquidity premia less relevant for the price response to purchase activity.

Interesting differences emerge across the maturity segments in which purchases take place. While the specification restricted to own purchases points to a stronger flow effect on securities in the upper half of the maturity-range, this effect disappears when adding substitutes (as visible from the last two columns of Table 3). At the same time, the estimates for substitutes continue to show significant differences, with the combined impact for securities in the high-maturity group amounting to almost three times the estimate for the securities in the lower-maturity group. These increasing impact estimates are consistent with the patterns detected for stock effects, which also tend to rise when moving out on the maturity-spectrum, and may reflect a greater duration extraction per unit of purchases taking place at the longer end of the yield curve.[34]

Table 4: Robustness – dropping countries from estimation sample

Country dropped:	DE	FR	IT	ES	NL
Own purchase	-0.141**	-0.096*	-0.125**	-0.121*	-0.106*
	(0.061)	(0.058)	(0.056)	(0.062)	(0.056)
Substitute <2y	-0.560***	-0.507***	-0.571***	-0.407**	-0.568***
	(0.158)	(0.152)	(0.150)	(0.160)	(0.149)
Substitute <4y	-0.454***	-0.349**	-0.408***	-0.332**	-0.406***
	(0.146)	(0.141)	(0.140)	(0.154)	(0.138)
Substitute <6y	-0.302***	-0.309***	-0.356***	-0.072	-0.322***
	(0.110)	(0.106)	(0.105)	(0.121)	(0.103)
Substitute <8y	-0.395***	-0.344***	-0.358***	-0.403***	-0.362***
	(0.086)	(0.079)	(0.072)	(0.099)	(0.077)
Observations	511414	758955	818765	774830	843943

Note: this table reports estimates of equation 1, with cluster-robust standard errors in parentheses. Each regression drops all observations from the country indicated in the column name (DE stands for Germany, FR for France, IT for Italy, ES for Spain and NL for the Netherlands). Dependent variable is the yield to maturity. Asterisks indicate statistical significance at 10% (*), 5% (**), and 1% (***) levels. The Kleibergen-Paap rk Wald F-statistic ranges between 33.9 (when excluding Spain) and 59.6 (when excluding the Netherlands). Given the large number of endogenous regressors, no Stock-Yogo critical values are available.

Finally, Tables 4 and 5 adopt a more agnostic approach to assess the robustness of our main findings to the choice of estimation sample. In particular, Table 4 presents estimates for the baseline model but, in each regression, dropping one of the five largest euro area countries – Germany, France, Italy, Spain and the Netherlands (which together account for more than 80% of euro area GDP). Table 5 conducts a similar exercise but now dropping two months at a time from the (sixteen month) sample period. The estimated coefficients are essentially unaffected by these adjustments, indicating that our conclusions are not driven by any specific sub-sets of the estimation sample.

[34]The increase in flow effects estimates along the maturity-range may also derive from stronger local-supply or market-functioning channels. However, if these channels were dominant, they would be expected to also show up in a stronger impact of own purchases in the high-maturity group. By contrast, the coefficient on substitute purchases captures broader transmission channels that go beyond pure local-supply effects and may also be related to duration extraction.

Table 5: Robustness – removing months from estimation sample

Months dropped:	1-2	3-4	5-6	7-8	9-10	11-12	13-14	15-16
Own purchase	-0.093*	-0.082	-0.113**	-0.102*	-0.121**	-0.096*	-0.094*	-0.131**
	(0.054)	(0.055)	(0.056)	(0.058)	(0.058)	(0.058)	(0.054)	(0.058)
Substitute <2y	-0.426***	-0.522***	-0.484***	-0.626***	-0.620***	-0.694***	-0.384***	-0.647***
	(0.128)	(0.144)	(0.146)	(0.151)	(0.163)	(0.162)	(0.135)	(0.182)
Substitute <4y	-0.439***	-0.394***	-0.360***	-0.417***	-0.414***	-0.467***	-0.248**	-0.516***
	(0.126)	(0.136)	(0.132)	(0.147)	(0.143)	(0.150)	(0.108)	(0.164)
Substitute <6y	-0.312***	-0.306***	-0.230**	-0.346***	-0.377***	-0.399***	-0.263***	-0.322***
	(0.095)	(0.100)	(0.106)	(0.105)	(0.109)	(0.109)	(0.094)	(0.110)
Substitute <8y	-0.258***	-0.348***	-0.386***	-0.374***	-0.428***	-0.413***	-0.260***	-0.394***
	(0.076)	(0.072)	(0.082)	(0.076)	(0.083)	(0.079)	(0.086)	(0.077)
Observations	831952	823780	817849	818888	820508	820214	814816	817549

Note: this table reports estimates of equation 1, with cluster-robust standard errors in parentheses. Each regression drops all observations from the months indicated in the column name (with the count starting from March 2015). Dependent variable is the yield to maturity. Asterisks indicate statistical significance at 10% (*), 5% (**), and 1% (***) levels. The Kleibergen-Paap rk Wald F-statistic ranges between 34.253 (when excluding months 15 and 16) and 60.915 (when excluding months 1 and 2). Given the large number of endogenous regressors, no Stock-Yogo critical values are available.

4.4 The dynamics of flow effects

The results summarised in Table 1 demonstrate that purchase operations under PSPP have exerted sizeable downward pressure on sovereign yields in the euro area on the day the respective purchases occurred. The final question we turn to now is whether this flow effect is purely transitory or shows some persistence.

To this end, we re-estimate the flow effects equation with 2SLS, again sticking to the more parsimonious set-up with Q_{it}^{0} and Q_{it}^{1} as endogenous regressors, but adopt different lag-structures across separate regressions (similar in spirit to Jordà (2005)). In particular, we use as dependent variable the yield of security i on trading day $t+k$, such that:

$$y_{it+k} = \beta_k Q_{it}^{0} + \gamma_{1k} Q_{it}^{1} + u_{ik} + v_{t+k} + \varepsilon_{it+k}. \tag{4}$$

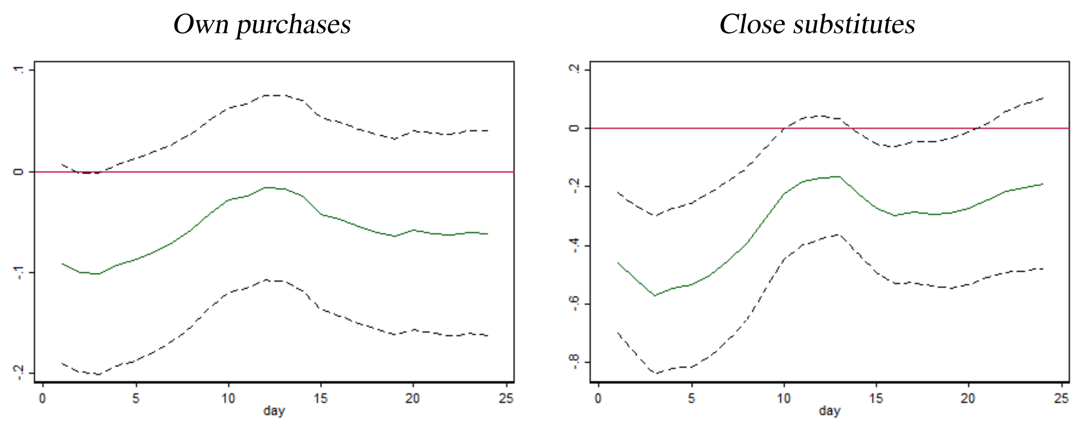
for all $k = 0, \ldots, 24$ (thus covering the equivalent of five weeks of trading).

Figure 4 presents the point estimates and 95% confidence intervals for each of the 25 regressions we ran in this exercise, with the x-axis indicating the number of trading days by which the purchase variables have been lagged.

The coefficients on the own- and substitute purchase variables display very different dynamic patterns. The former loses statistical significance after only three trading days and remains indistinguishably close to zero for the remainder of the horizon. This relatively short-lived response of the yields on securities being purchased is consistent with the findings in D'Amico and King (2013) where the own-price response of US Treasury notes and bonds to purchase operations by the US Federal Reserve vanishes after around two to six trading days.

By contrast, the flow effect on close substitutes is considerably more persistent (albeit much smaller per €-billion in purchases; see section 4.1). In fact, this effect fades out only nine trading days after the purchases take place and hovers around borderline significant levels until briefly before the end of the 25-trading day horizon. Since related studies for other economies do not report the dynamic patterns of the coefficients on substitute purchase variables, we cannot assess whether this persistence of flow effects also carries over to other contexts. At the same time, Joyce and Tong (2012) refer to analysis they conducted for the 10 to 25-year maturity segment of the UK Gilts market showing that "*rather than declining over time, the*

Figure 4: Evolution of flow effects across horizons



Note: the figures report estimates of equation 4. Solid lines show point estimates from separate regressions lagging the explanatory variables by as many trading days as indicated by the respective tick on the x-axis. Dashed lines show upper and lower bounds of the 95% confidence interval.

impact from purchases on the yields of these longdated gilts remained significantly high" (see page 372); this seems to point to some persistence in flow effects also in the UK. The greater persistence of effects on close substitutes indicates that the process of transmitting yield changes across maturity segments is more gradual than the "own-price" effect on the security being purchased.

4.5 Unit root tests and daily returns as alternative dependent variable

Throughout the analysis, we have assessed the impact of central bank asset purchase flows on the *level* of sovereign bond yields (y_{it}). To test the robustness of our findings to this choice of dependent variable, this section presents the results from two further exercises. The first consists of a set of unit root tests that aim at scrutinising whether the results may suffer from spurious regression problems. The second consists of re-running our key specifications, but replacing yield levels with daily bond returns (r_{it}) as the dependent variable (defined as the difference between the bond price on day t and the bond price on day $t-1$, both measured at close of business). This specification replicates the approach adopted in some of the related literature, where the use of this first-differenced measure of bond prices has presumably been motivated by non-stationarity concerns.[35]

[35] For instance, D'Amico and King (2013), Kandrac and Schlusche (2013), and Schlepper et al. (2017) use daily bond returns and Joyce and Tong (2012) use the first-difference of bond yields as dependent variable.

For the panel used in the current analysis, the tests clearly reject the null hypothesis of a unit root in the level of yields. Table 6 presents test statistics proposed by Im et al. (2003), which are well-suited to our data given they allow for unbalanced panels and cross-sectional heterogeneity in the autoregressive parameter.[36] The H_0 of non-stationarity is rejected, independently of whether: *(i)* the specification includes lags (and in the former case, whether we just impose one lag for all securities or use the Akaike Information Criterion to determine the most appropriate number of lags for each of the securities); or *(ii)* it includes a time trend.

Table 6: Panel unit root tests for yield levels

Specification	(1)	(2)	(3)	(4)	(5)	(6)
Test statistic	-22.330	-26.422	-26.985	-59.513	-26.779	-27.129
p-value	0.000	0.000	0.000	0.000	0.000	0.000
Number of lags	0	1	0.55	0	1	0.52
Linear time trend	No	No	No	Yes	Yes	Yes

Note: the tests follow Im et al. (2003). The H_0 is that y_{it}, as defined in section 2, has a unit root. Following the notation in Im et al. (2003), the test statistic corresponds to $Z_{\tilde{t}-bar}$ for specifications (1) and (4) that do not include lags and to W_{t-bar} for all other specifications, which include lags. The tests are based on demeaned series (*i.e.* subtracting the cross-sectional average from each observation in each time period), consistent with the use of time-fixed effects in all regressions presented in sections 4.1 to 4.4. In specifications (3) and (6), the number of lags refers to the cross-sectional average number of lags per security chosen based on the Akaike Information Criterion. The tests are based on 3,020 securities over an average time horizon of 290 days.

Table 7 presents estimates of equation 1, but replacing yields with daily returns as dependent variable. As in the baseline, the coefficients on all purchase variables point to statistically significant flow effects, now manifest in upward pressure on daily returns. Also, the pattern of coefficients on substitute purchases declining with maturity distance remains intact (*i.e.* substitutes with a maturity up to two years different from security i display the highest coefficient and substitutes between six and eight years different display the lowest coefficient). The OLS-estimates fail to show any impact of bond purchases on returns, again confirming the results from the regressions for yields. Like in the baseline, the size of the coefficients overall remains stable when including a lower number of purchase variables.

[36]We implemented the tests using the Stata routine "xtunitroot ips". Since this routine does not allow for gaps in the data, we dropped a subset of observations for securities to which this situation applied. However, the resultant reduction of observations is negligible, relating to only five (out of more than 3,000) securities; and, even for those securities, more than half of the observations could be retained since gaps occurred only in parts of the sample. A further five securities had to be dropped since the number of time series observations available for them was lower than ten – the minimum necessary for the test by Im et al. (2003) to be conducted.

Table 7: Flow effect estimates with daily returns as dependent variable

	2SLS	OLS	2SLS	2SLS	2SLS	2SLS
Own purchase	0.134**	-0.000	0.207***	0.128**	0.137**	0.120*
	(0.066)	(0.002)	(0.060)	(0.062)	(0.064)	(0.065)
Substitute <2y	0.896***	-0.010		0.758***	0.783***	0.897***
	(0.186)	(0.008)		(0.154)	(0.168)	(0.188)
Substitute <4y	0.459***	0.002			0.558***	0.443***
	(0.117)	(0.016)			(0.117)	(0.118)
Substitute <6y	0.459***	-0.003				0.524***
	(0.095)	(0.006)				(0.098)
Substitute <8y	0.246***	-0.003				
	(0.082)	(0.004)				
Observations	873009	873009	873009	873009	873009	873009
K-P weak IV stat	63.412	-	413.467	198.884	125.439	97.053
S-Y critical values	-	-	16.38	7.03	-	-

Note: this table reports estimates of equation 1, but replacing yields with daily returns as dependent variable. Cluster-robust standard errors are in parentheses. Asterisks indicate statistical significance at 10% (*), 5% (**), and 1% (***) levels. "K-P weak IV stat" refers to Kleibergen-Paap rk Wald F-statistic for the respective specification and "S-Y critical values" to the corresponding Stock-Yogo critical values for 10% maximal Wald test size distortion. The reduction in the number of observations compared to the baseline (see table 1) is due to the calculation of daily returns using the first lag of the price level, which are unavailable on the first day on which a security enters the data set.

Taken together, these estimates further underpin the evidence of significant flow effects in euro area sovereign bond markets. At the same time, the regressions based on yield levels appear more appropriate as our baseline specification. From a conceptual perspective, this is because the specification in first differences, in a literal interpretation, implies that a one-off purchase of a certain security exerts a *permanent* effect on its price (or yield) level; this is intuitively less appealing in view of the notion of flow effects as triggering only temporary price action. From an empirical perspective, the rejection of non-stationarity in yield levels eliminates an important econometric motivation for using first-differenced variables in our sample.

5 Conclusion

A large body of literature has documented sizeable and persistent stock effects of central bank asset purchase programmes on sovereign bond yields and other financial market variables.

These stock effects tend to materialise whenever market participants receive information that alters their assessment of the overall size of the bond portfolio that the central bank is expected to accumulate and maintain over the life of the programme, or on other key programme modalities, such as its maturity composition or allocation to different asset classes. Stock effects operate, *inter alia*, by suppressing duration, liquidity and credit risk premia; by signalling a lower future path of policy-controlled short-term interest rates; and by creating local scarcity in market segments preferred by certain classes of investors.

In contrast to the extensive stock effects literature, there is only sparse evidence on the flow effects that may arise around the time of central bank asset purchase operations on account of the temporary scarcity they induce in specific market segments and potential impairments in market liquidity that may reinforce the price response to purchase activity. Moreover, the few empirical studies that analyse flow effects tend to focus on the US and UK economies, whereas no systematic assessment has been available for the euro area context as of yet.

This paper broadens the scope of the flow effects literature to also cover the euro area and, specifically, the impact of the ECB's Public Sector Purchase Programme (PSPP). Our results point to statistically significant and economically relevant flow effects of PSPP on euro area sovereign bond yields. These effects materialise not only via a reduction in the yields of securities being purchased, but also via a transmission of the yield impact to similar securities that market participants may perceive as substitutes. Overall, the sizeable impact estimates corroborate models of the interest-rate term-structure that combine preferred habitat with limited arbitrage elements.

In terms of method, our results show that it is important to address simultaneity bias in the estimation of flow effects. In most markets, prices and purchase quantities are jointly determined and it is plausible to assume that this condition also applies to the market for sovereign debt securities in which central banks operate when implementing their asset purchase programmes. This conjecture receives further support from central bank communication suggesting that the decision of which securities to buy and in which quantities to buy them at a given point in time is not independent from prevailing yield levels. Against this background, we resort to a natural experiment that arises from the legal set-up of PSPP, and in particular from the temporary purchase restrictions during blackout periods, to identify exogenous variation in purchase volumes.

We find the choice of estimation approach to be consequential for the implications of our analysis as the OLS estimates range closer to zero than our preferred instrumental-variables estimates and, in some cases, show the opposite sign. This pattern is consistent with the hypothesis that the slope coefficients estimated with OLS do not only capture the downward impact of central bank purchases on bond yields but also the tendency for bonds with relatively high yields to attract higher purchase volumes. Since a similar constellation may apply to the asset purchase programmes of other central banks, these findings may argue for a review of the conclusions from related studies that do not address the risk of simultaneity bias in the estimation of flow effects.

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