ANNOUNCEMENT AND IMPLEMENTATION EFFECTS OF CENTRAL BANK ASSET PURCHASES*

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Abstract

What is the overall impact of announcement and implementation effects of QE on financial conditions? Existing research lacks a unified approach for answering this question. We fill this gap by estimating a VAR model based on two pillars: a unique daily dataset covering ECB's asset purchases over the period 2014-2021 and a novel identification strategy combining survey-based external instruments and narrative sign restrictions. The findings underscore the relevance of both purchase announcements and actual purchases in influencing bond yields and stock prices. Neglecting how purchases are actually implemented may severely distort the assessment of QE effectiveness.

JEL classification: E52, E58, E44, C32, C54.

Keywords: monetary policy, asset purchases, stock effects, flow effects, high-frequency, VAR.

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1. INTRODUCTION

Central bank asset purchases (APs) have become an integral part of the monetary policy toolkit of many central banks. Since the Global Financial Crisis (GFC) of 2007-08, APs have been used to ease the monetary policy stance when policy rates were close to their effective lower bound (ELB) and to preserve the smooth transmission of monetary policy during periods of financial market stress.

The increasing reliance of central banks on APs has spurred a vast literature on the effects of purchase announcements and actual purchases. These effects are typically referred as stock and flow effects. Stock effects are set in motion by changes in the stock of assets held by the central bank in its balance sheet. Because financial markets are forward looking, stock effects typically arise upon purchase announcements. This has led many studies to proxy stock effects with announcement effects. By contrast, flow effects – or implementation effects, as we call them in this paper – emerge when purchases are actually made. The existence of both these effects is a robust empirical fact, supported by a large body of evidence gathered across many countries and historical episodes.

Notwithstanding the extensive literature, an important gap remains unaddressed. Existing research does not allow assessing the relative importance of announcement and implementation effects in influencing financial conditions. There are two main reasons for this. First, these effects are typically analyzed using different frameworks (event studies and granular-data settings, respectively), which are not necessarily consistent with each other. Second, existing studies typically focus on the average impact of an *isolated* AP shock. However, such a focus is *per se* uninformative about the overall effectiveness of APs, which depends on (*i*) the cumulative effects of a *repeated sequence* of AP shocks over time[1] and on (*ii*) those stemming from the systematic response of APs to changes in economic and financial conditions.

The lack of a unified empirical framework has recently led to somewhat controversial views. On the one hand, it is often argued in the policy debate that the actual implementation of APs is of little importance in explaining their effectiveness, which can instead be assessed by focusing mainly on announcements (BIS, 2019). Such conclusion is motivated by the finding that the yield impact of a typical purchase announcement is larger and more persistent than the impact of a typical actual purchase. On the other hand, anectodal evidence suggests that asset purchase programmes cannot be fully assessed solely on the basis of announcement effects on yields, as their actual implementation also appears to play a key role in compressing longer-term bond yields (Vissing Jørgensen, 2021; BIS, 2023).

This paper fills this gap by studying the joint impact of announcement and implementation effects of central bank asset purchases. We develop a comprehensive empirical framework based on a Vector Autoregressive (VAR) model that allows us to compare and combine the announcement and implementation effects of APs. Our framework relies on two main pillars: a unique daily dataset

[1] See for instance the point raised by Ludvigson et al. (2021).

covering all the APs implemented in the euro area from 2014 to 2021 and an identification strategy combining external instruments, zero-sign restrictions, and narrative restrictions. These two pillars are highly interconnected. As explained below, the focus on daily data is indeed central to our identification: in the case of lower-frequency data, our identification assumptions would either be much restrictive (e.g., using weekly or monthly data) or completely unreasonable (e.g., using quarterly or annual data).

Two policy variables are central to our analysis. First, we construct an indicator that tracks the maximum stock of assets under the Eurosystem's asset purchase programmes that the ECB Governing Council intends to reach when it makes an announcement of a new round of purchases.[2] By construction, it accounts for past, present, and (announced) future purchases. Second, using confidential data on actual purchase flows, we construct a simple indicator that signals how these announced purchases are actually implemented on a day-by-day basis. More specifically, we derive the average pace at which announced purchases are anticipated to be carried out, which is known in advance by financial markets on the basis of announcements and press releases. We then subtract this average pace from the actual series of gross purchases. The resulting indicator has two main advantages. From an econometric perspective, it helps controlling for expectations not already absorbed by the VAR. From an economic perspective, it provides a simple measure of the degree of "temporal flexibility" in the conduct of asset purchases: that is, to what extent the actual implementation of asset purchases deviates from a constant-pace implementation of the announced stock of purchases.

Our high-frequency identification strategy rests on three main assumptions. First, we identify announcement shocks using surveys conducted on the eve of ECB's monetary policy meetings and use them as an external instrument in the model. Second, we assume that implementation shocks generate a within-day negative co-movement between actual purchases and bond yields, consistently with the findings documented by the empirical literature on flow effects. This assumption is necessary in order to account for the well-known simultaneity bias between purchase flows and bond yields; while shocks to actual purchases compress bond yields, those to bond yields may induce an endogenous increase in purchases by central banks' market desks. If unaddressed, the bias would lead to the finding of a null or even positive correlation between purchases and yields. We strengthen the identification of implementation shocks by using (narrative) evidence that this negative co-movement was strong at the launch dates of the APP and the PEPP. Third, we assume that all other shocks in the economy do not induce any within–day reaction by the central bank in terms of gross purchase flows. Imagine that a positive news about the demand-side of the economy is released to the public. This news would exert an immediate (i.e., within-day) rise in bond yields and stock prices. As prescribed by the standard macro-textbook, the central bank may decide to react to this positive aggregate demand shock by tightening monetary policy. This reaction, however, will never materialize on the same day of the news

[2] We refer here to the Asset Purchase Programme (APP) and the Pandemic Emergency Purchase Programme (PEPP).

release, as central bank's governing bodies would wait until the next monetary policy meeting before eventually re-calibrating their AP programmes. This residual group of shocks, therefore, is cleanly identified by exploiting the high-frequency lags that characterize the response of APs to macroeconomic developments. To further validate our approach, we also consider an extended version of our model in which we shed more light on the nature of these shocks by disentangling them into narrower categories, among which aggregate demand and aggregate supply shocks.

Our model is able to reproduce well-documented stylized facts on the transmission of AP programmes. First, we document that both purchase announcements and actual purchases have a significant impact on financial conditions, as evidenced by a reduction in the slope of the government yield curve, an increase in stock prices, and a rise in inflation expectations. Second, these effects differ on their degree of persistence. In particular, purchase announcements exert very long-lasting effects on yields, stock prices, and inflation expectations. Actual purchases, instead, generate much shorter-lived effects. Third, we find that for both types of asset purchase shocks the reduction in the slope of the government yield curve is largely driven by a compression of sovereign spreads.

Our model also sheds new light on the monetary policy reaction function in the conduct of asset purchases, providing results that align closely with prevailing narratives about the ECB's monetary policy response during the analyzed period. First, we find that over the longer run a large share of the average variation in purchase announcements is explained by the reaction of the ECB to demand shocks (about 30% after 5 years). The relevance of demand shocks as a driver of the ECB's decisions to announce changes in the stock of purchases is particularly evident in the period leading up to the resumption of asset purchases in September 2019, when our model signals that the decision was taken largely in response to a sharp slowdown in aggregate demand that had started at the beginning of that year and posed risks to price stability. Second, we find that actual purchases are partly driven by a systematic policy response to financial shocks (more than 30% on average). This was particularly the case at the height of the Covid-19 crisis in the spring of 2020, when our model signals that the observed increase in actual purchases was largely triggered by the sudden and pronounced deterioration in financial market conditions. This result, which captures well the Eurosystem prompt intervention at that time, provides further support for the validity of our identification strategy. Third, we find that financial conditions and inflation expectations are mainly driven by macroeconomic and financial shocks, both directly and through the systematic change they induce in the asset purchase programmes. In particular, over the sample period under review, demand shocks are the main driver of the variation in the slope of the yield curve, stock prices, and inflation expectations. Financial shocks, on the other hand, are the main driver of sovereign spreads.

We then test the robustness of our evidence to several changes in the baseline specification. First, we examine the extent to which our main results are affected by the use of an informative prior or by specifying a limited number of lags. We find that the results are unaffected by choosing a flat prior or by significantly increasing the number of lags in the model. Second, we examine the extent to which our results depend on the inclusion of the Covid-19 crisis in the sample. If we drop observations after

2019, we obtain results that are quantitatively similar to the baseline results, with bond yields falling slightly less in response to purchase announcements and inflation expectations rising moderately more on impact. Third, we examine the role of narrative restrictions on the identification of the implementation shocks. We show that their exclusion increases the average responsiveness of actual purchases to financial-sector shocks up to 60% (from 30% under the baseline), a value that appears at odds with the typical degree of responsiveness of central bank market desks.

Finally, we use our new framework to obtain a stock-flow decomposition of the overall impact of APs on financial conditions. To this end, we construct for a given sample period two alternative structural policy scenarios that allow assessing the role of both the systematic and the unexpected components of purchase announcement and actual purchases. First, to estimate the overall impact of APs stemming from announcements and implementation decisions, we analyze what would have happened had the ECB not announced a recalibration of its purchase stock and implemented the announced purchases at a constant pace (instead of frontloading or backloading them). Second, in order to isolate the specific contribution of the actual implementation to this overall effect, we analyze what would have happened had the ECB announced a recalibration of its stock of purchases (as historically occurred) but implemented announced purchases at a constant and predictable pace. The combination of these two sets of counterfactual effects provides the stock-flow decomposition.

We apply our stock-flow decomposition to examine the impact of APs following the pandemic recalibration of March 2020, a period characterized by a significant increase in the announced stock of purchases (€870 billion) and by a marked frontloading of purchases (around €60 bn in total over the period analyzed, about 45 days between 11 March and 29 April). Our model suggests that both the announcement and the implementation contributed to a flattening of the slope of the euro-area government yield curve by about 40 bps, half of which was due to the increase in the stock and the other half to the frontloading of purchase flows. To verify if implementation choices always matter in shaping the overall effectiveness of AP programmes, we apply the stock-flow decomposition in the aftermath of the Public Sector Purchase Programme (PSPP) announcement in January 2015, a period in which the ECB announced a similar increase in its stock of APs that, however, was implemented at a constant and predictable pace (i.e., with a cumulative frontloading of only around €4 bn over the period analyzed). According to our model, the total effect of asset purchases on the slope of the government yield curve at that time was around 21 bps at the peak, of which 20 bps due to the increase in the announced stock and just 1 bp due to implementation choices. Taken together, these results suggest that AP programmes cannot be fully assessed on the basis of purchase announcements alone, as the actual implementation of purchases can enhance or diminish their effectiveness over time in a non-negligible way.

Related literature and contributions. The literature on APs can be sorted-out along three broad dimensions.[3] First, the way APs are modeled: indirectly (through price impacts) or directly (through quantities). Second, the type of event under consideration: purchase announcements or actual purchases. Third, the stages of the transmission mechanism over which the effect of APs is assessed: early stages (financial variables) or latest stages (real variables). In this respect, this paper assesses the financial effects of APs by focusing directly on the amount of purchases that are announced and implemented.[4]

Our paper adds to three relevant strands of the literature. First, we contribute to the literature on announcement and flow effects of APs by providing a unified empirical macroeconomic framework that allows assessing and comparing these effects over time. In this regard, we provide evidence of a non-negligible role of the implementation in shaping the effectiveness of APs. A paper closely related to ours is the one by Sudo and Tanaka (2021), who estimate a DSGE model of the Japanese economy in order to assess the relevance of stock and flow effects. Although the authors find that flow effects have been at work, they conclude that their quantitative role has been limited, around 10% of the reduction in the term premium due to the purchase of government bonds. With respect to their fully-fledged model, our empirical framework has the advantage of imposing less structure on the data and being better suited at capturing financial markets features.

Second, we contribute to the literature on the effects of APs conducted during the pandemic crisis. Recent studies tackle the same issue relying on different frameworks and data. On the one hand, Gilchrist et al. (2021) and Vissing-Jørgensen (2021) use single-equation models to study the effectiveness of the pandemic APs conducted by the Fed. The first paper evaluates the efficacy of the Secondary Market Corporate Credit Facility (SMCCF), a program designed to stabilize the US corporate bond market during the Covid-19 pandemic. It finds that purchase announcements significantly lowered credit spreads and worked almost entirely through a reduction in credit risk premia. As for actual purchases, they were instead deemed negligible. In contrast to this evidence, Vissing-Jørgensen (2021) finds that during the COVID crisis APs by the Fed worked more via actual purchases than purchase announcements. On the other hand, Costain et al. (2022) and Motto and Özen (2022) use multivariate models to study the effectiveness of the pandemic APs conducted by the ECB. Costain et al. (2022) extend the influential term structure model of Vayanos and Vila (2021) to allow,

[3] A non comprenhensive list of papers includes (in chronological order) Krishnamurty and Vissing Jørgensen (2011), Kapetanios et al. (2012), Wright (2012), D'Amico et al. (2012), Wright (2012), Kapetanios et al. (2012), Baumeister and Benati (2013), D'Amico and King (2013), Gambacorta et al. (2014), Gambacorta et al. (2014), Falagiarda and Reits (2015), Szczerbowicz (2015), Falagiarda and Reitz (2015), Wu and Xia (2016), Eser and Schwaab (2016), Casiraghi et al. (2016), Eser and Schwab (2016), Boeckx et al. (2017), Ghysels et al. (2017), Arrata and Nguyen (2017), Schlepper et al. (2017), Krishnamurty et al. (2018), De Pooter et al. (2018), De Santis and Holm-Hadulla (2020), Bernardini and De Nicola (2020), Gambetti and Musso (2020), De Santis (2020), Altavilla et al. (2021), Lhuissier and Nguyen (2021), Altavilla et al. (2021), Lhussier and Nguyen (2021), Droste et al. (2022).

[4] The choice of working with quantities provides an important advantage. Indeed, Swanson (2021) notes that one drawback of the literature that extracts APs surprises or shocks from interest rates comes from the impossibility of getting direct estimates of central bank policies in terms of policy tools. One has therefore resort to back-of-the envelope calculations in order to get these numbers. Focusing directly on purchases fixes this issue.

in addition to duration risk, for sovereign default in a monetary union. They find that the flexible design of the PEPP substantially enhanced its impact on financial conditions, especially through a compression of credit spreads. Motto and Özen (2022) focus on intra-daily changes in asset prices around policy announcements – in the spirit of Gürkanayak et al. (2005) and Swanson (2021) – to disentangle a new underlying driver called "market-stabilization QE". They find that the latter factor lowers sovereign spreads and raises stock prices. While both papers draw these conclusions by looking at purchase announcements only, we also stress the role played by their actual implementation.

Third, we add to the most recent empirical literature on the assessment of the effects of monetary policy based on high-frequency data pioneered by Gürkaynak et al., (2005; see, among others, Gertler and Karadi, 2015; Nakamura and Steinsson, 2018; Altavilla et al., 2019; Bauer and Swanson, 2022a, 2022b; Jarocinski and Karadi, 2020; Andrade and Ferroni, 2021; Miranda-Agrippino and Ricco, 2021; Aruoba and Drechsel, 2022). Our contribution is to apply the beauty of high-frequency data to identify the causal effects of APs more directly (i.e., by looking at quantities rather than prices). We do that by relying on a parsimonious set of high-frequency identifying assumptions that are obtained by combining external instruments, zero-sign restrictions (Piffer and Podstawski, 2018: Arias et al, 2021; Cesa-Bianchi and Sokol, 2022; Braun and Brueggeman, 2022), and narrative restrictions (Antolìn-Diaz and Rubio-Ramìrez, 2018).

Outline of the paper. The rest of the paper is organized as follows. Section 2 illustrates our empirical framework. Section 3 discusses the transmission and relevance of shocks to purchase announcements and actual purchases and discusses a number of robustness exercises and extensions. Section 4 presents the counterfactual exercises conducted to evaluate the relative contribution of purchase announcements and actual purchases to the overall impact of AP recalibrations. Finally, Section 5 concludes.

2. EMPIRICAL FRAMEWORK

2.1. Model specification and data

We adopt a simple Vector Autoregressive (VAR) model to capture the dynamic feedbacks between central bank APs and financial conditions. In particular, we specify the following VAR:

$$y_t = c + A(L)y_{t-1} + u_t \tag{1}$$

where y is a vector of endogenous variables, c is a constant term, and u is a vector of forecast errors. $A(L)$ is a matrix polynomial in the lag operator L, where t denotes the time frequency, which in our setting is daily (weekdays).

The vector y of endogenous variables is composed of two main blocks: a block of asset purchase variables and a block of financial variables. All variables are sampled at daily frequency and cover the time interval ranging from 20 October 2014 to 10 December 2021. Our main sources for the data are

Eurosystem's confidential data on purchases, the ECB's Survey of Monetary Analysts (SMA),[5] Bloomberg and Refinitiv.

Asset purchase variables: announced stock. The first policy variable is the announced stock of purchases under the Eurosystem's asset purchase programmes. It measures the maximum stock of assets under the APP and PEPP that the Governing Council anticipates to hold and that is announced to the public in the press release or during the press conference following the monetary policy meetings. We construct this variable in the spirit of Weale and Wieladek (2016) and Lhuissier and Nguyen (2021). All the details are reported in the Appendix (see Table A1).

Figure 1 compares this variable with the actual stock of purchases. By construction, the announced stock measures the sum of all past, present, and announced future purchases. A positive gap, therefore, indicates all the purchases that have been announced but not yet implemented, while a zero gap signals that the Eurosystem's programmes have been halted (i.e., redemptions are fully-reinvested in order to keep the overall stock constant), as occurred in large part of 2019.

[FIGURE 1 ABOUT HERE]

Asset purchase variables: implemented flows. The second policy variable is related to the implemented purchase flows under the Eurosystem's asset purchase programmes. We construct this variable using confidential Eurosystem data on daily gross purchases under the APP and the PEPP. Part of the implementation is mechanical and known in advance. First, gross purchases are implemented according to an announced (net) target and the expected amount of redemptions coming due. Second, gross purchases are implemented on a weekday schedule and are halted during periods of preannounced holidays. All this information is released to the public well in advance and allows to infer the average (or constant-pace) trend of daily purchases needed to deliver the announced stock. All the details are reported in the Appendix (see Table A2).

Figure 2 compares the actual gross flows under the APP and the PEPP with its underlying trend. The latter tracks well the underlying movements in the actual implementation of gross purchases and drops temporarily to zero during preannounced holiday periods, when gross purchases are halted.[6] The gap between these two variables measures the amount of flexibility in the conduct of APs: that is, the historical deviations from a constant-pace implementation. The ECB has always retained some margins of flexibility, as shown by the volatility in the implemented-flow gap since the start of the APP. The volatility in this variable, however, has substantially increased since March 2020, due to the enhanced flexibility under the PEPP. Overall, historical deviations in the gap have been symmetric around zero.[7]

[5] The Survey of Monetary Analysts (SMA) collects information on market participants' expectations about the future evolution of key monetary policy parameters, financial market variables, and the economy. It runs eight times a year, with a frequency aligned to the six-week cycle of the Governing Council's monetary policy meetings. For further details, please see the ECB's website.

[6] The underlying trend is a daily-frequency version of the graph published on the ECB's APP website using monthly data.

[7] Mean is €0.011 bln, median is 0.023.

[FIGURE 2 ABOUT HERE]

Changes in the announced stock tend to be followed by changes in implemented flows. Such policy lags, which are announced to the public in advance, can be hardly predicted by the VAR for two reasons. First, they are quite long (on average around 3 months; see Table A2). This would require the specification of a very large lag order when working with daily observations. Second, and more importantly, they are quite heterogeneous, ranging from a few weeks to several months ahead. This would imply that, at best, the model would only capture the average policy lag between an announcement and its subsequent implementation, which however would be very different from the actual one. To effectively take into account the presence of long and variable implementation lags, we include the implemented-flow gap in Figure 2 as our second policy variable, which by construction takes into account the correct implementation lag of each announcement. By doing so, our second policy variables centers exclusively around the role played by implementation in shaping the actual pace at which purchases are implemented (in deviation from the announced underlying one).

Financial market variables. Figure 3 shows the four variables that belong to the financial block. The first variable measures the slope of the yield curve, which is defined as the difference between the long-term government bond yield (i.e., the GDP-weighted average of 10-year government bond yields of the eleven largest countries in the euro area) and the short-term risk-free rate (i.e., the 1-month Overnight Indexed Swap rate, OIS). It encompasses the total contribution of the expected path of policy rates and several type of premia (to compensate for duration risk, credit risk, liquidity risk, and convenience) to the slope of the government bond yield curve.[8]

[FIGURE 3 ABOUT HERE]

The second variable measures the sovereign spread embedded on the long-end of the yield curve, which is defined as the difference between the long-term government bond yield and the long-term risk-free rate (i.e., the 10-year OIS rate) and captures the compensation for credit-and liquidity-risk as well as the non-pecuniary benefit associated with holding government bonds (e.g., the so-called convenience yield). We focus on the government yield curve because government yields typically serve as benchmarks for the private sector's financing costs and, exactly for this reason, asset purchase programmes typically target this bond category. The third variable is a market-based measure of medium-term expected inflation (i.e., the 2-year 1-year forward Inflation Linked Swap rate, ILS). It measures the (risk-neutral) expected average inflation rate over the two-year period that begins one year from today. Finally, the last variable is a stock price index, composed of 50 stocks from 11 countries in the Eurozone stocks (i.e., the Euro Stoxx 50). As it is standard in models based on daily data, we use this variable as a high-frequency proxy for expected economic activity.

[8] An alternative could be using the 1-month government bond yield. However, the latter may be affected by the presence of premia (in particular, the so-called convenience yield). We therefore prefer to use the short-term risk-free rate. Nevertheless, the results are largely unaffected by any of these choices.

2.2. High-frequency identification

We identify two distinct AP shocks using a novel approach. As shown in equation (2), the standard identification problem in VARs consists in defining a matrix B such that the forecast errors of the model $\boldsymbol{u}_t$ can be expressed as a linear combination of shocks $\boldsymbol{\varepsilon}_t$:

$$u_t = B\varepsilon_t \tag{2}$$

In particular, we use a combination of external instruments, zero and sign restrictions, and narrative restrictions to isolate two APs shocks: a shock to the announced stock and a shock to implemented flows. All the remaining structural innovations in the system (e.g., global and domestic shocks, such as additional demand, supply and financial shocks), labeled non-AP shocks, are not explicitly identified but grouped into two broad categories: those that exert a within-day response of actual purchases and those that only exert a lagged response. All restrictions are imposed on impact only, meaning that they must hold merely on the day in which a given shock hit. This ensures that the persistence of the effects of shocks is entirely driven by the data. The identifying assumptions are summarized in Table 1.[9]

[TABLE 1 ABOUT HERE]

Asset purchase shocks: announced stock. Shocks to the announced stock of purchases are identified using an instrumental variable. Recalibrations of the announced stock of purchases are often anticipated by the private sector and followed by little-to-none changes in market rates. To isolate the exogenous and unanticipated component of this variable, we rely on an external instrument (or proxy variable) z_t. In particular, we follow Lhuissier and Nguyen (2021) and set z_t equal to a time series of survey-based surprises on the unexpected stock of additional purchases under the APP and the PEPP. All the details are reported in the Appendix (see Table A3). Figure 4 compares the external instrument with the observed changes in the announced stock. While some announcements came as a full surprise (e.g., in March 2020), in many cases they were either perfectly anticipated by market analysts (e.g., in October 2017) or even associated with a negative surprise (e.g., in December 2015).

[FIGURE 4 ABOUT HERE]

The use of survey-based surprises as an external instrument for announcement shocks allows properly estimating the elasticity of financial variables to programmes' announcements. Formally, our first identifying assumption is that the external instrument z_t is correlated with an unobserved series of announced stock shocks but is uncorrelated with the other series of shocks (Mertens and Ravn, 2013; Stock and Watson, 2018):

[9] Notice that we are collapsing the three remaining shocks (last three columns in Table 1) in one convolution. This is common practice in the literature, when the focus of the empirical analysis hinges on partial rather than full identification (see, for example, Jarocinski and Karadi, 2020). In Section 3.3 we dig deeper into this convolution of shocks, identifying demand, supply and (lagged) financial shocks.

$$\begin{cases} E[\varepsilon_{1t} z_t'] \neq 0 \\ E[\varepsilon_{it} z_t'] = 0 \; for \; i = 2, \dots, N \end{cases} \tag{3}$$

where ε_{it} is the *i-th* shock (sorted as in Table 1) and N is the number of endogenous variables.

Asset purchase shocks: implemented flows. Shocks to the implemented purchase flows are identified through sign and narrative restrictions. Our second identifying assumption is that implementation shocks generate on impact a negative comovement between actual purchases and bond yields. The assumption is supported by a growing body of evidence documenting a negative response of bond yields to unexpected increases in purchase flows.[10] The other contemporaneous responses are left unrestricted, as there is little evidence in the literature supporting a specific sign or size.

We strengthen the identification of implementation shocks using narrative restrictions (Antolìn-Diaz and Rubio-Ramìrez, 2018). These types of restrictions allow researchers to exploit external information to discipline the data on the relevance of a given shock at a given point in time. By their very nature, they are particularly appropriate in high-frequency settings such as ours as they allow the narrative about a given episode to be narrowed down to a very specific day, rather than tying it to a specific month or quarter.[11] Specifically, we further assume that implementation shocks were a key driver of the frontloading of actual purchases that occurred on the launch dates of the PSPP and the PEPP. In practice, we implement our narrative assumption by retaining only those draws for which we observe, for a given date (i.e., 9 March 2015 and 26 March 2020), that: (*i*) the contribution of the implementation shock to the one-step ahead forecast error of implemented flows is the largest in absolute value; (*ii*) the sign of the contribution of the implementation shock to the one-step ahead forecast error of implemented flows matches the sign of the same forecast error.

Figure 5 provides the main intuition for our narrative assumption. In March 2015, the two largest reductions in the slope of the government yield curve occurred on the 9th and the 10th, the first two days of PSPP implementation (Panel a).[12] Likewise, the largest reduction in the slope in March 2020 occurred on the 26th, the first day of PEPP implementation, and was very similar in magnitude to the one that occurred a few days earlier when the PEPP was announced (Panel b). While other shocks may have as well contributed to the flattening of the yield curve over these two days (note that we do not

[10] In the case of the euro area, see Casiraghi et al. (2016), Eser and Schwaab (2016) and Ghysels et al. (2017) for the Securities Markets Programme (SMP); De Santis and Holm-Hadulla (2020) for the APP; Bernardini and De Nicola (2020) for the PEPP.

[11] Narrative restrictions are increasingly used in the applied macroeconomics literature interested in identifying monetary and financial-sector shocks (Antolìn-Diaz and Rubio-Ramìrez, 2018; Conti et al., 2023), but they heve not yet been applied to high-frequency data.

[12] On March 9, 2015 a Eurogroup meeting was held in Brussels to discuss the evolution of the Greece economic situation after the financial programme provided by the IMF, the ECB and the European Commission. President Dijsselbloem announced that "the discussions between the Greek authorities and the institutions must and will start as from Wednesday March 11, 2015, with a view to achieve a speedy and successful conclusion of the current review". Hence, this announcement could be a possible confounding factor, driving the yield slope downwards. However, (*i)* the remarks by President Dijsselbloem came at 8.45 pm, after European markets had already closed and (*ii*) the market assessment on the reform package proposed by the Greek Government resulted in an *increase* of the Greek 10-year government yield.

impose any restriction on the yield slope itself), the timing, magnitude and direction of the observed changes in market yields provide strong anecdotal support for our narrative assumption.

[FIGURE 5 ABOUT HERE]

Non-asset purchase shocks. All other shocks are grouped in two broad categories. The first category includes those that trigger a stabilizing within-day response by the central bank in terms of gross purchase flows. These shocks are often associated with events related to the financial sector and their existence has been extensively documented in the literature on flow effects. On the one hand, Ghysels et al. (2017) show that when Eurosystem's interventions are triggered by sudden and sharp price deteriorations, regressions of changes in yields on the size of purchase flows give null or even positive coefficients, misleadingly suggesting that the interventions were ineffective or counterproductive even if they did in fact contribute to stabilizing bond yields in spite of unceasing upward pressures. On the other hand, De Santis and Holm-Hadulla (2020) show that the simultaneity bias between bond yields and purchase flows can arise even in good times (i.e., when purchases do not have any explicit market stabilization purpose), as central bank's portfolio managers tend to take into account the relative values of bonds when allocating purchases. This implies that, even in good times, they may react to temporary changes in bond yields that are related to bond spreads rather than policy rate expectations. In our identification strategy we therefore assume that these shocks cause an increase in yields slope (partly as a result of a widening of spreads) and a reduction of stock prices.

The second category (the last three columns in Table 1) includes shocks that induce a lagged response of gross purchase flows (i.e., the within-day response of actual purchases is zero). These shocks are mainly macroeconomic and financial shocks whose effects are assessed approximately every 6-7 weeks by the ECB Governing Council. As such, they are identified by exploiting the policy lags that characterize monetary policy responses to macroeconomic and financial developments. This is a key advantage of estimating our model with high-frequency data: while timing restrictions are very much plausible at the daily frequency, they become much less acceptable at lower frequencies, such as quarterly or even monthly.[13] To further validate our approach, in Section 3.3 we present an extended version of our model in which we shed more light on the nature of the shocks belonging to this second category by disentangling them into narrower categories: aggregate demand, aggregate supply, and financial shocks.

2.3. Estimation and inference

Our unique dataset of APs and financial variables covers the whole history of the Eurosystem's APs up until 2021. More specifically, our sample extends from 20 October 2014 to 10 December 2021. Based on the usual lag-length selection criteria[14], we specify six lags and all variables are entered in

[13] In Bernardini and Conti (2021) we claim that this assumption, despite being stronger, holds also at the weekly frequency, for which data on ECB's asset purchases are publicly available.

[14] In more detail, the Bayesian Information Criterion (BIC) suggests a too parsimonious number of lags p=1, while the Akaike Information indicate p=6. We therefore select p=6 in our baseline specification, and then perform robustness checks along this dimension in Section 3.

(log-)levels, in order to allow for possible cointegration relationships between the endogenous variables. Specifically, the yield slope and spread are included in levels, as well as the implemented flow gap (the difference between actual purchases and their constant-pace trend). Announced stock of APs and stock prices are taken in log-levels.

The combination of external instruments and zero-sign restrictions underlying the identifying assumptions shown in Table 1 is implemented using the methodology proposed by Cesa-Bianchi and Sokol (2022), which is adapted to the Bayesian framework. Following their strategy, we partition the B matrix as $B = [B^{IV}\, B^{ZSR}]$, where B^{IV} is an $n \times 1$ vector which captures the impact of the announcement shock, while B^{ZSR} is an $n \times (n-1)$ matrix which captures the effects of the remaining shocks identified by means of zero and sign restrictions. After having pinned down B^{IV} by using the external instrument, conditional on that we recover B^{ZSR} by using a standard Graham-Schmidt decomposition (for more details please refer to Appendix B and Cesa-Bianchi and Sokol, 2022).

The model is estimated using Bayesian techniques, with a standard Minnesota prior and a Gibbs sampling, similar to the daily VAR used by Wright (2012) to assess the impact of US monetary policy on long-term interest rates at the effective lower bound. The elicitation of the priors is also fairly standard.[15] All the figures presented in the following sections are based on 5,000 retained draws. Appendix B provides further details and the algorithm used to estimate and identify the model and relates our paper to the strand of the literature on the informativeness of the prior on the orthonormal rotation matrix.

3. MAIN FINDINGS

This section presents the effects of the identified shocks in terms of transmission and relevance. Specifically, in Section 3.1 we first discuss the dynamic effects of announcement and implementation shocks, focusing on the reaction of market interest rates and stock prices. We then present the dynamic effects of non-AP (financial) shocks that trigger a within-day response by the ECB, in order to validate our identification strategy. In Section 3.2 we show the contribution of the identified shocks to the average and historical variation of the endogenous variables. In Section 3.3 we further disentangle the "other" macroeconomic shocks – those triggering only a delayed reaction by the ECB – into demand, supply and financial shocks and analyze their effects. Finally, in Section 3.4 we test the sensitivity of our results to several perturbations of the baseline model and we show that they are robust to (*i*) possible structural changes induced by the Covid-19 crisis, (*ii*) the adoption of an uninformative prior in the BVAR estimation, (*iii*) the specification of a larger number of lags in the BVAR covering the 6-7 weeks period between two consecutive monetary policy meetings, and (*iv*) alternative identifications of the implementation shocks (i.e., removing the narrative restrictions).

[15] In particular, the overall degree of shrinkage is set to 0.3, the cross-variable shrinkage is set to 0.5, and the prior on the deterministic components is set to 10^5. The prior on the orthonormal rotation matrix is assumed to be uniform. See also Appendix B.

3.1. Transmission of shocks

Announcement shocks. The first row of Figure 6 shows the dynamic effects of announcement shocks, together with their 68% and 90% credibility intervals. For illustrative purposes, the responses are scaled so that the median response of the announced stock of purchases is equal to €400 bn on impact. We highlight two main findings. First, announcement shocks have very persistent effects on yields, stock prices, and inflation expectations. Second, the negative co-movement between the IRFs of yields and stock prices suggests that the identified shock is not strongly affected by any substantial central bank information effect (see Nakamura and Steinsson, 2018, Jarocinski and Karadi, 2020, Bauer and Swanson, 2022a and Miranda-Agrippino and Ricco, 2021).

[FIGURE 6 ABOUT HERE]

To relate our quantitative results on announcement shocks to those found in the literature, we refer to Lhuissier and Nguyen (2021), which is the closest contribution to our paper and from which we borrow the survey-based identification block. There are two potentially important differences with their analysis. First, while we include the pandemic purchases in our estimation sample, they stop their analysis in 2019. When we restrict the estimation sample until December 2019 (see Section 3.3) our estimates are qualitatively and quantitatively in line with their ones. Second, following Weale and Wieladek (2016), Lhuissier and Nguyen (2021) normalize the stock of announced purchases on the value of euro area GDP at the end of 2014, before the announcement of the APP. Remapping our shock to a 1% of euro-area GDP in 2014, we obtain a peak-impact on the yield slope of around 2 bp, which is almost twice the effect estimated by Lhuissier and Nguyen (2021), but falls within their 90% credibility interval.[16]

Implementation shocks. The second row of Figure 6 shows the dynamic effects of implementation shocks. For illustrative purposes, the responses are scaled so that the median response of the implemented flow gap is equal to €0.5 bn on impact, which corresponds to the typical daily change in purchase flows in our sample. We highlight three key findings. First, implementation shocks have significant but short-lived effects relative to announcement shocks. Second, they trigger a sizable easing of financial conditions. Third, the reaction of inflation expectations is not significant.

The reaction of all variables is qualitatively consistent with that obtained in previous theoretical and empirical work on flow effects (see for instance, D'Amico and King, 2013; De Santis and Holm-Hadulla, 2020; Bernardini and De Nicola, 2020). Looking at the policy variables, after rising on impact implemented flows slowly decay to their pre-shock level in about four weeks. Moreover, the implementation shock does not affect the announced stock of purchases: the IRF of the latter is almost nil (compared to the typical changes observed during announcements), though in negative territory, and surrounded by elevated uncertainty. This negative sign may reflect the relation between

[16] Garcia Pascual and Wieladek (2016b) apply to euro-area data the empirical strategy proposed by Weale and Wieladek (2016) for evaluating the impact of central bank asset purchases on the US and UK economy. Their VAR model does not include the 10-year long-term rate, but they report an impact of 5 bp on the 20-year long-term yield.

implementation and announcements, as a positive permanent deviation from the announced stock requires a future decrease in the announced stock itself to keep the overall stance unaffected. The sovereign yield slope and the sovereign yield spread are lowered on impact by about 0.7 and 0.4 bp, respectively; the effect of implementation shocks lasts for almost 20 – 30 days, before converging to zero.[17] Stock prices rise on impact and then peak to almost 0.5 pp, before reverting to zero. Interestingly, medium-term inflation expectations – which are left unrestricted in our identification scheme (see Table 1) – display a positive reaction, although with a high degree of uncertainty.[18]

With respect to the literature, the closest contribution to our paper is the work by Ghysels et al. (2017), which, to best of our knowledge, is the only other paper addressing this issue in a dynamic multivariate framework. With respect to Ghysels et al. (2017), we also jointly identify announcement shocks, thus enabling to assess the relative importance of both effects. On the other hand, one advantage of the paper by Ghysels et al. (2017) is the possibility of relying on higher-frequency data than ours, as they use confidential intra-daily data on actual purchases. In this respect, however, we believe that (*i*) the identification of the within-day endogenous reaction shocks and (*ii*) the adoption of narrative restrictions help in dampening the former disadvantage.

Shocks exerting a within-day response of the central bank. The third row of Figure 6 shows the dynamic effects of shocks that exert a stabilizing within-day response by the central bank in terms of gross purchase flows.[19] These shocks have been often associated with shocks which entail market dysfunctions and increase in financial stress; their existence has been extensively documented in the literature on flow effects (see the discussion in Ghysels et al., 2017). In our framework, assessing their impact on financial conditions is crucial, since they imply a positive comovement between actual purchases and yields. Identifying such shocks therefore ensures to take care of the endogeneity problem highlighted by the literature on flow effects (see Ghysels et al, 2017; De Santis and Holm-Hadulla, 2020; Bernardini and De Nicola, 2020).

With respect to implementation shocks, the main difference is that the return of the financial variables to their pre-shock level is much slower. This finding seems consistent with this shock well-capturing the reaction of the ECB to market dysfunctions: with respect to implementation shocks, purchases are lower at the beginning but they last for a longer period of time, likely necessary to restore the proper market functioning. The response of the announced stock is modestly positive and, again, surrounded by a significant share of uncertainty. The yield slope and the yield spread increase by about 2 and 1 bp. Stock prices are lowered, as a typical spillover of market dysfunctions.

[17] The percentage of negative draws is equal to 93% after 5 days, 84% after 10 days, 75% after 20 days and 67% after 30 days.

[18] The percentage of positive draws in response to implementation shocks is equal to 55% on impact, but grows up to 70% after 9 days, and hovers around that figure thereafter.

[19] Also in this case, for illustration purposes the responses are scaled so that the median response of the implemented flow gap is equal to €0.5 bn on impact.

3.2. Relevance of shocks

We now turn to the relevance of the identified shocks as drivers of the variation in the analyzed variables. The top panel of Figure 7 shows how much the shocks are relevant *on average*. Each plot reports for a given variable the shares of the variance of the forecast error explained by the shocks (forecast error variance decomposition, FEVD). The bottom panel shows how much the shocks are relevant *over time.* In this case, each plot reports the cumulative effect of the shocks to the cyclical movements of a given variable at given point in time (the Historical Decomposition, HD).[20]

[FIGURE 7 ABOUT HERE]

Our model sheds new light on the reaction function of asset purchases. First, we find that purchase announcements are mostly driven by a systematic reaction of the central bank to economic and financial shocks. In particular, after one year about 35% of the variation in purchase announcements is explained by a reaction of the ECB to non-AP shocks (which becomes 60% after 5 years – not shown). Second, we find that actual purchases are partly triggered by a systematic reaction of the central bank to financial shocks. In particular, over 30% of the average variation in actual purchases is explained by a within-day reaction of the ECB to shocks arising in the financial sector. The bottom panel digs deeper into this result by looking at the contributions of AP and non-AP shocks in the period surrounding the Covid-19 crisis. In March 2020, at the height of the crisis, the model signals that the observed step-up in actual purchases was largely triggered by the sudden and marked deterioration of financial markets' conditions occurred in the Spring of 2020 (red bars). Considering that this period provides the main episode of market dysfunction available in our sample, this result constitutes a strong validation of the empirical framework. The model, despite being linear by construction, is in fact able to correctly distinguish, in line with historical narratives, autonomous increases in purchases from those related to the endogenous reaction to heightened market tensions (Ghysels et al., 2017; De Santis and Holm-Hadulla, 2020). Third, we find that AP shocks are not a major driver of inflation expectations and the slope of the yield curve, as they account for no more than 25% of the variation in financial conditions (around 15% on average). This result is consistent with the literature that reports a dominant role of macro shocks (Kurmann and Otrok, 2013; Moench and Soofi-Siavash, 2022).

Overall, the evidence displayed in Figure 7 indicates that APs are largely endogenous, as they tend to respond to changes in economic and financial conditions in a non-negligible way. Importantly, it also provides a noteworthy further validation of our empirical framework and in particular of the proposed identification scheme of implementation shocks.[21]

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[20] More precisely, Figure 7.b reports the cumulative contribution of the shocks to the deviation of a variable from its model-implied steady state since the beginning of 2020. Notice that the model-implied steady states (shown in Figure C1 in the appendix) turn-out to be remarkably unbiased by the pandemic crisis, a strong feature that adds strength to our model.

[21] Moreover, the HDs provide a further validation of our empirical framework as they capture some interesting "narrative developments", such as, for example, the 2016 collapse in stock prices driven by non-AP shocks after Brexit (not shown).

3.3. Further disentangling of the non-AP shocks

The previous section has shown that a non-negligible share of the variation in purchase announcements is explained by a lagged reaction of the ECB to changes in economic and financial conditions (labeled as non-AP shocks). What are exactly these shocks underpinning?

To better gauge the former result and to further validate our approach, we develop an extended version of the model in which all the non-AP shocks are explicitly assigned to three narrower categories: aggregate demand, aggregate supply, and financial shocks. Notice that, by construction, all the results discussed so far remain largely unaffected, given the zero restrictions imposed on the actual purchases in response to all these shocks (Jarocinski and Karadi, 2020). This implies that this extension does not play any major role in answering the main questions that are at the heart of this study. Nevertheless, it provides an additional validation test of the model, as it allows to check to what extent it is capable of capturing established narratives of the main drivers of the economic and financial outlook during selected historical periods.

[TABLE 2 ABOUT HERE]

Table 2 summarizes the additional identifying restrictions (highlighted in red). Notice that each of the three shocks is defined as an adverse one, that is they are normalized to lower stock prices. First, aggregate demand shocks are disentangled from aggregate supply shocks based on the reaction of inflation expectations, which decrease after an adverse demand shock, while they increase after an adverse supply shock. To further narrow-down the set of admissible models, we assume that they also differ on the response of the yield slope. In the case of an adverse demand (supply) shock, the yield slope is lowered (raised) by the expectations of future cuts (hikes) in the policy rate. Second, financial shocks are disentangled from aggregate demand disturbances as they cause the slope of the yield curve to rise (partly as a result of an increase in spreads); like aggregate-demand shocks, they generate a fall in inflation expectations. This latter restriction allows disentangling them from aggregate-supply shocks. Finally, notice that the additional class of financial shocks identified in this extension is very similar to the one identified in the baseline except for the fact that the central bank does not *immediately* respond to them by frontloading actual purchases. This is a flexible feature of our framework: within a standard linear model, by having both class of shocks in the model we indirectly allow for the possibility that the degree of the central bank's reactiveness to shocks arising from the financial sector changes over time.

This extension provides three additional insights.[22] First, purchase announcements are largely driven by aggregate demand shocks. The top panel of Figure 8 shows that the systematic reaction of purchase announcements is mainly driven by this type of shocks (around 15% after 1 year, almost 30% after 5 years – not shown). The bottom panel of Figure 8 digs deeper into this result by looking at the contribution of AP and non-AP shocks in the period surrounding the restart of QE in September 2019.

[22] The underlying impulse responses are reported in the appendix (see Figure C3).

According to the model, this decision and the related announcement was largely made in response to a severe slowdown in aggregate demand that has been going on from the beginning of that year, a finding that matches the historical narratives at the time. Second, demand shocks are the major driver of the variation in the yield slope, stock prices, and inflation expectations. The finding that demand shocks are more relevant than supply shocks in driving fluctuations of stock prices and inflation expectations, both on average and over time, is in line with the prevailing narrative on the drivers of the euro-area business cycle in the period 2014-2019, stemming from lower-frequency models of real and nominal variables (see Koester et al., 2021, and references therein). Notice also that supply shocks exert a significant impact on inflation expectations at short horizons, likely capturing (also) global commodity prices movements. Third, financial shocks are the major driver of sovereign spreads. This result may be also explained by the fact that these spreads primarily embed credit and liquidity risk premia whose variation, due to the incompleteness of the European monetary union, is often linked to disorderly market dynamics arising in the financial sectors unrelated to changes in fundamentals.

[FIGURE 8 ABOUT HERE]

3.4. Robustness

The robustness of our findings is assessed by considering alternative choices in the specification of the baseline model (see Figures 9 and 10).

[FIGURES 9 AND 10 ABOUT HERE]

Sample. Our findings may be specific to the analyzed sample, which includes the Covid-19 period. They could reflect a bias induced by structural changes in the overall state of economy or monetary policy rules. Unfortunately, the limited number of purchase announcements limits us in estimating the model only for the Covid-19 period or to conduct state-dependent analysis. However, to check to what extent the inclusion of the Covid-19 period (around one-fourth of the baseline sample) affects our results we consider a shorter sample ending on December 31, 2019. Overall, the results are consistent with those of the baseline. The main difference relates to the response of market-based inflation expectations to announcement shocks, which appears to be more pronounced on impact (although it remains very close to the baseline at longer horizons).

Prior. In the baseline, we use a standard Minnesota prior. This choice is motivated by the fact that all the variables show strongly-persistent dynamics. However, one may question whether the use of a prior that has been commonly associated with lower-frequency VAR models (such as monthly or quarterly) is appropriate in case of higher-frequency VARs (such as daily). In this respect, it should be noted that – if anything – the Minnesota-prior assumption that all the variables follow random-walk processes is even weaker than usual, as the degree of persistence in the analyzed variables is typically stronger at higher frequencies. Nevertheless, we re-estimate the model by shutting-down the role of the Minnesota prior (that is, by specifying the prior to be extremely flat) and find that it has very little influence on the posterior estimates of the model parameters and, hence, on the baseline results.

Lag order. We specify 6 lags to keep our baseline specification parsimonious. One week of data, however, does not reflect the horizon over which the ECB's Governing Council assesses incoming data in order to take monetary policy decisions, which usually spans a period of 6 weeks. To this end, we verify to what extent our results change when we include a lag order of 30 weekdays. Unsurprisingly, we find that the impulse responses became more erratic as they now reflect a more complex linear combination of parameters; nonetheless, they remain quantitatively very similar to the baseline ones.

Narrative restrictions. An important role in our identification of implementation shocks is played by the narrative restrictions described in Section 2. To obtain a simple but clean (and formal) assessment of their role, we run the BVAR model under the baseline identification, except for the fact that we remove the narrative restrictions on the forecast errors of the implemented flows gap. Figure 10 shows the results of this exercise, for the transmission (panel a) and the relevance (panel b) of implementation shocks. Removing the narrative restrictions produces IRFs that are qualitatively similar, albeit with a much higher degree of uncertainty (Figure 10, top panel). Notably, when we do not use the narrative restrictions, the credibility interval of the yield spread includes the zero. This suggests that the narrative sign restrictions on the launch of the APP and the PEPP do indeed provide valuable information to disentangle implementation shocks from the (financial) shocks that trigger an endogenous response of the ECB.

Importantly, the latter conclusion is further corroborated by the picture emerging from the FEVD (Figure 10, bottom panel). The comparison between the baseline FEVDs and those obtained when removing the narrative restrictions is striking as well and helps shedding further light on the role played by narrative events in our identification. The share of variance of the implemented flows gap explained by implementation shocks is much larger when the narrative restrictions are imposed, while it decreases from about 75% to 40% when the latter are not imposed, a result that seems at odds with the typical degree of responsiveness of central banks' market desks. Overall, we interpret the evidence obtained by remove removing narrative restrictions as an indication that the joint combination of narrative restrictions and zero-sign restrictions sharpens our identification of implementation shocks.

4. A STOCK-FLOW DECOMPOSITION OF THE IMPACT OF ASSET PURCHASES

4.1. Design of policy counterfactuals

What is the contribution of announcements and implementation choices to the overall effectiveness of APs on financial conditions? Does the decision of frontloading or backloading the actual implementation of announced purchases matter? The evidence reported in the previous section shows that, to answer these questions, we need to evaluate the effects of AP changes in their entirety: that is, regardless of whether they are expected or unanticipated. In other words, we need to evaluate *both* the discretionary and the systematic components of APs. This is particularly important as we have shown in Section 3.2 that AP shocks account for a relatively important but not overwhelming proportion of variation in announced and implemented APs and that historically they have not been a dominant force behind changes in financial conditions. This finding suggests that, similar to conventional tools, APs affect financial conditions mainly through their systematic component, i.e. by responding to changes in economic and financial conditions.

To decompose the overall impact of APs into the contributions of announcement and implementation effects, we rely on the estimation of policy counterfactuals, an approach dating back to Bernanke et al. (1997) and carefully discussed in Kilian and Lütkepohl (2017). In particular, for a given historical episode, we consider two alternative policy scenarios, as follows:

1. in the first scenario, we examine what would have happened *if the ECB had not kept its announced stock of purchases unchanged* (instead of recalibrating it) *and had implemented announced purchases at a constant pace* (instead of frontloading or backloading them). This scenario provides a measure of the overall impact of APs resulting from the announcement and implementation choices;
2. in the second scenario, we instead examine what would have *happened if the ECB had announced a recalibration of its stock of purchases* (as historically occurred) *but had implemented the purchases at a constant pace* (instead of frontloading or backloading them). This scenario provides a measure of the contribution of implementation choices to the overall impact of the APs.

The difference between the first and the second scenario provides an indication of the contribution of the announcement component to the total impact of APs.

Our policy counterfactuals have two important and desirable features, that are crucial for minimizing concerns related to the Lucas' critique, typically raised to warn against the plausibility of counterfactual scenario analyses. First, they are structural, as the counterfactual paths of the AP variables are attributed to the AP shocks only (see, for example, the discussion in Antolín-Díaz et al., 2021). Technically, the estimated time series of the AP shocks are replaced by counterfactuals calculated so that the predetermined paths of the AP variables under the two scenarios are realized, while all the remaining estimated (non-AP) shocks in the system remain unchanged (Kilian and Lütkepohl, 2017). This feature ensures that counterfactual effects are not driven by changes in economic and financial conditions but rather by autonomous changes in monetary policy decisions. Second, they have a short-term nature, as the daily frequency of our model allows to conduct our analysis over a relatively narrow time window (around 45 days). This feature makes it less likely that agents, if surprised, will immediately conclude that the policy reaction function has changed; they would rather wait a few weeks until the next monetary policy meeting of the Governing Council to gather more information. To further address concerns about the Lucas' critique, we follow Leeper and Zha (2003), Kilian and Lütkepohl (2017) and Antolìn-Diaz et al. (2021) and check to what extent the sequence of counterfactual shocks is very different in magnitude from the original ones.[23] Intuitively, if the difference in the (absolute) size of counterfactual and original shocks is small and occurs over a short period of time, the counterfactual scenarios are considered to be plausible exercises.

4.2. Evidence from two case studies

The announcements of the APP temporary envelope and the PEPP (March 2020). We first use our stock-flow decomposition to analyze the effects of APs in the aftermath of the PEPP announcement in March 2020, a period in which the ECB announced a large increase in its stock of APs and decided to strongly frontload these announced purchases in order to preserve the smooth functioning of the market. This feature, labeled "flexibility" in APs by the Governing Council, has been often cited as a key feature of the ECB's response to the pandemic crisis (Lane, 2022).

More in detail, in March 2020 two significant AP recalibrations were announced in order to offset the severe consequences triggered by the outbreak of the Covid-19 pandemic. On March 12, a temporary envelope of additional net APs of €120 billion was added to the existing APP programme until the end of the year. However, the announcement of this further envelope was somewhat overlooked by markets after the press conference, in which some miscommunication occurred about the ECB intentions and will to fight fragmentation.[24] As a result, sovereign spreads soared in the following days and on 18 March, in an unscheduled meeting, the ECB's Governing Council decided to launch the Pandemic Emergency Purchase Programme (PEPP), a new temporary asset purchase programme of private and public sector securities. The PEPP was initially endowed with an overall envelope of net APs of €750 billion to be implemented until the end of 2020. A key difference with the APP concerned the implementation of the APs. Indeed, purchases of public sector securities under the PEPP were allowed to be made in a flexible manner across asset classes, among jurisdictions and – particularly important from the perspective of our model – over time.

Figure 11 presents the results of our exercise. The top panel displays the actual and counterfactual paths of the analyzed variables: in particular, the magenta dashed line corresponds to the scenario in which the ECB does not recalibrate the size of its monetary policy portfolio and implements purchases at a constant and predictable pace, whereas the green dotted line refers to the scenario in which the

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[23] A visual inspection of our counterfactual shocks shows that they are not quantitatively distant from the identified ones (see Figure C7). This means that they are plausible in the sense of Leeper and Zha (2003): it is unlikely that the perturbation to the policy (AP announcements and implementation) that we are assuming is vulnerable to the Lucas' critique.

[24] President Lagarde stated: "My point number two has to do with more debt issuance coming down the road depending on the fiscal expansion that will be determined by policymakers. Well, we will be there, as I said earlier on, using full flexibility, but *we are not here to close spreads*". The latter sentence was then clarified in a CNBC interview after the press conference, in which President Lagarde noted that she was "fully committed to avoid any fragmentation in a difficult moment for the euro area. High spreads due to the coronavirus impair the transmission of monetary policy. We will use the flexibility embedded in the asset purchase programme, including within the public sector purchase programme. The package approved today can be used flexibly to avoid dislocations in bond markets, and we are ready to use the necessary determination and strength".

ECB recalibrate its monetary policy portfolio as historically occurred but implements purchases at a constant pace. The bottom panel decomposes the total impact of APs as the contribution of the announcement and implementation choices made by the ECB.[25]

[FIGURE 11 AND TABLE 3 ABOUT HERE]

Two main findings stand out. First, the monetary policy response to the outbreak of the Covid-19 pandemic had a significant impact on the yield curve, inflation expectations, and stock prices. Indeed, had the ECB not launched the PEPP in March 2020 the 10-year yield slope and the 10-year yield spread would have been higher by around 40 and 25 bp, respectively, with a consequent increase in the 10-year OIS rate of around 15 bp. In addition, stock prices and inflation expectations would have been lower by slightly less than 10 percentage points and 15 bp, respectively. Notably, the probability that these counterfactual effects are greater than zero at their peak (or lower than zero in the case of stock prices) is estimated to be between 90 and 95 per cent for all financial variables, with the exception of inflation expectations (around 70 per cent; see Table 3).[26] Second, the implementation of asset purchases (in particular, the decision of strongly frontloading announced purchases) accounts for almost half of this overall effect of APs. In fact, the total impact of APs on financial variables is almost equally split between announcement and implementation effects. Indeed, when evaluated at its peak, the flexible implementation embedded in the PEPP lowered the yield slope and the yield spread by about 20 and 10 bp, respectively, while raising stock prices and inflation expectations by 5 percentage points and 6 bp. This finding suggests that the use of temporal flexibility in the implementation of the announced stock of purchases contributed significantly to the effectiveness of the APs launched by the ECB to counter the effects of the Covid-19 crisis.

Finally notice that these effects can be likely interpreted as a lower bound of the true impact. In particular, the VAR model assumes that announcement and flow elasticities are constant over time, implying that the effectiveness of central bank asset purchases do not depend on the underlying state of the economy. As already discussed in Section 3.4, we are forced to make this assumption because the availability of few official announcements limits us in specifying a fully-fledged model with time-varying or state-dependent coefficients. In this respect, the recent euro-area evidence (Schnabel, 2021; Bernardini and De Nicola, 2020) showing that the elasticities of market rates to purchase announcements and actual purchases were particularly large at the height of the Covid-19 crisis suggest

[25] The overall impact shown in the figure reflects indeed the sum of two policy decisions: (*i*) the one of announcing a higher stock of purchases and (*ii*) the one of frontloading purchases instead of implementing them at a constant pace. The counterfactual effects shown in Figure 11.a provide the specific contribution of temporal flexibility (decision *ii*) to the overall impact. The specific impact coming from the higher announced stock of purchases (decision *i*) is approximately provided by the differences between the two counterfactual effects.

[26] For each variable and for each day in the analysed time window, the probability is computed as the percentage of draws for which the counterfactual effect has the same sign of the median effect taken across draws.

that the assumption of a linear model is likely to be associated with a downward bias in the estimated impact of asset purchases during this particular episode.

The announcement of the PSPP (January 2015). To provide a benchmark for comparison, we now use our stock-flow decomposition to analyze the impacts of APs in the aftermath of the PSPP announcement in January 2015, a period in which the ECB announced a similar increase in its stock of APs, but implemented APs at a constant and predictable pace (i.e., with a cumulative frontloading of only around €4 bn over the period analyzed).[27] While the results of this comparison are conditional on the linear nature of the model and by the absence of small amount of frontloading/backloading in the case of the PSPP,[28] comparing this episode to the previous one allows to understand to what extent implementation choices matter, are negligible, or somewhat in between.

In particular, on January 22, 2015 the Governing Council of the ECB announced an expanded asset purchase programme (APP; see ECB, 2015)[29]. The main novelty of the programme was the addition of purchases of sovereign bonds to the already existing purchases of private sector bonds, in order to counter the risks of a too prolonged period of low inflation and the de-anchoring of inflation expectations due to a lack of aggregate demand (Draghi, 2014). More specifically, the Governing Council announced an expanded asset purchase programme encompassing the asset-backed securities purchase programme (ABSPP) and the covered bond purchase programme (CBPP3), both launched at the end of 2014, with a monthly amount of €60 billion. They were intended to be carried out until at least September 2016 and in any case until the occurrence of a sustained adjustment in the path of inflation consistent with the ECB inflation aim of achieving inflation rates below, but close to, 2% over the medium term. President Draghi announced that purchases would have started in March, without referring to any explicit form of frontloading.

The results of our counterfactual exercise conducted upon the announcement of the APP are presented in Figure 13 and Table 4. According to our model, in January and February 2015 the total effect of APs on the slope of the government yield curve was around 21 bps at the peak, of which around 20 bps due to the increase in the announced stock and just 1 bps due to implementation choices.

[FIGURE 13 AND TABLE 4 ABOUT HERE]

Overall, the results obtained from our two case studies and presented in Figures 12-13 and Tables 3-4 indicate that the AP programmes cannot be fully evaluated only from purchase announcements, as the actual implementation of purchases can strengthen or dampen their effectiveness over time in a non-negligible way. Thus, the results indicate that the smaller impact of APs on financial conditions

[27] The PSPP was announced on January 22 and actual purchases started on March 9: between these dates, the Eurosystem continued to implement purchases under the already existing programmes (the CB3PP and the ABSPP; see also Appendix A).
[28] Given the linear nature of our framework, the size of the effect attributable to flexible implementation depends on the scale and duration of frontloading of the announced programme.
[29] See https://www.ecb.europa.eu/press/pr/date/2015/html/pr150122_1.en.html.

upon the APP announcements, as opposed to the PEPP announcement, stems from the contribution of the implementation, which was almost nil at that time.

5. Conclusions

We have developed a novel empirical approach that allows to combine and compare announcement (stock) and implementation (flow) effects of APs within a unified framework estimated using a unique daily dataset covering the history of the APs conducted in the euro area between October 2014 and December 2021. The results offer several novel insights into the effectiveness of asset purchases, also depending on the modalities of implementation, and on the reaction function that guides their announcement and execution. From a policy perspective, our results show that flexibility in the implementation of asset purchases is effective in reducing market fragmentation, therefore preserving the smooth transmission of monetary policy to the real economy.

Our paper improves the literature on two dimensions. First, it proposes a novel approach to obtain a joint identification of announcement and implementation effects. Second, our unified framework allows to compare the relative importance of stock and flow effects of central bank APs in a consistent manner, taking into account the role played by both the systematic and unexpected monetary policy reactions. Both features fill an important gap in the literature.

As new data will become available, our analysis could be extended along two dimensions. First, while our model is linear, the effects of announcement and implementation shocks may be larger in periods of financial market stress. Second, while the paper is tailored to the case of QE, the proposed framework can be easily adapted to study the effects of QT.

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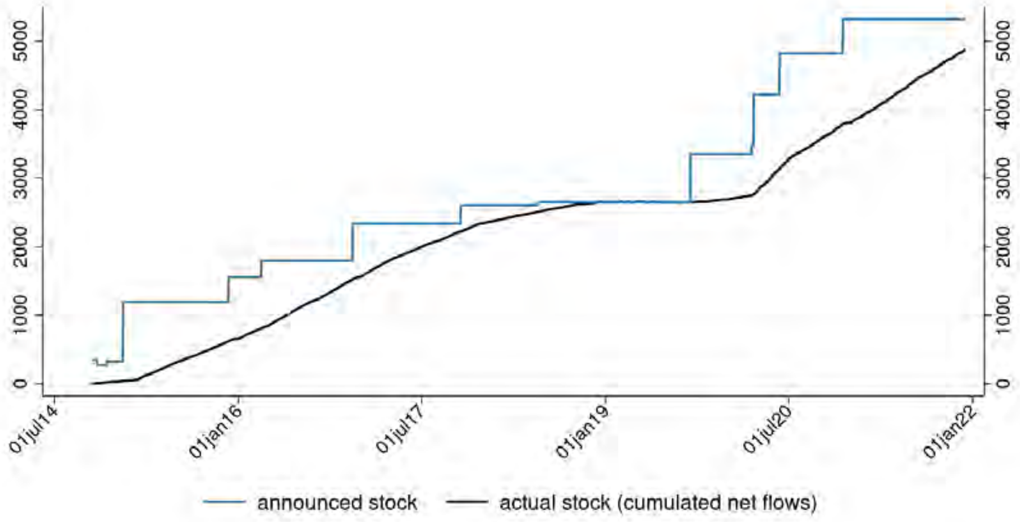
FIGURES AND TABLES

Figure 1 – Announced stock of purchases

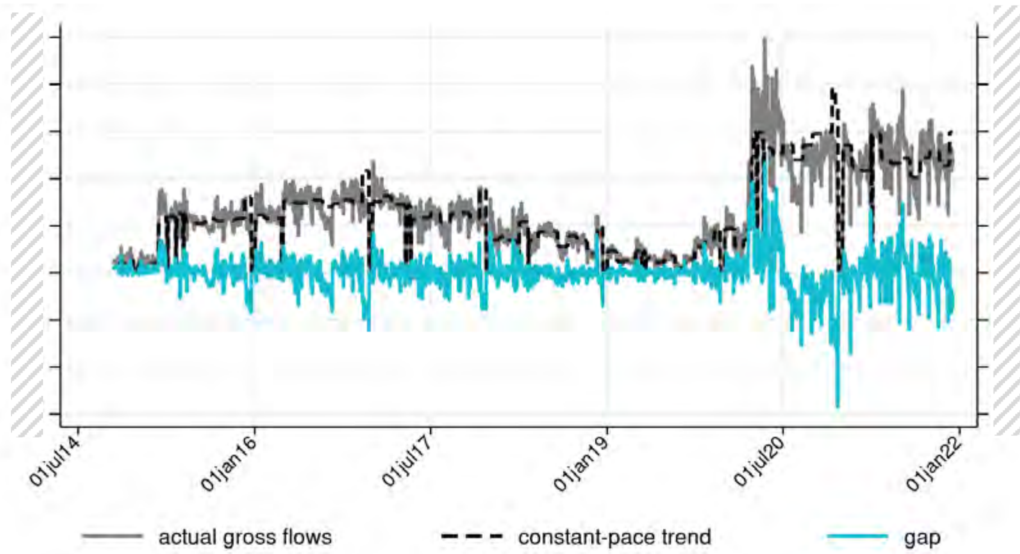
(*€ bln*)



Note. The figure shows the announced stock of purchases, together with the actual stock, sampled at the daily frequency over the period 20 October 2014 – 10 December 2021. *Announced stock* is the overall size of assets under the APP and the PEPP that the ECB's Governing Council anticipates to hold and which makes public after its monetary policy meetings. It is the sum of all past, present, and future purchases. *Actual stock* reflects past and present purchases. The *x*-axis shows weekdays, while the *y*-axis is in billions of euro.

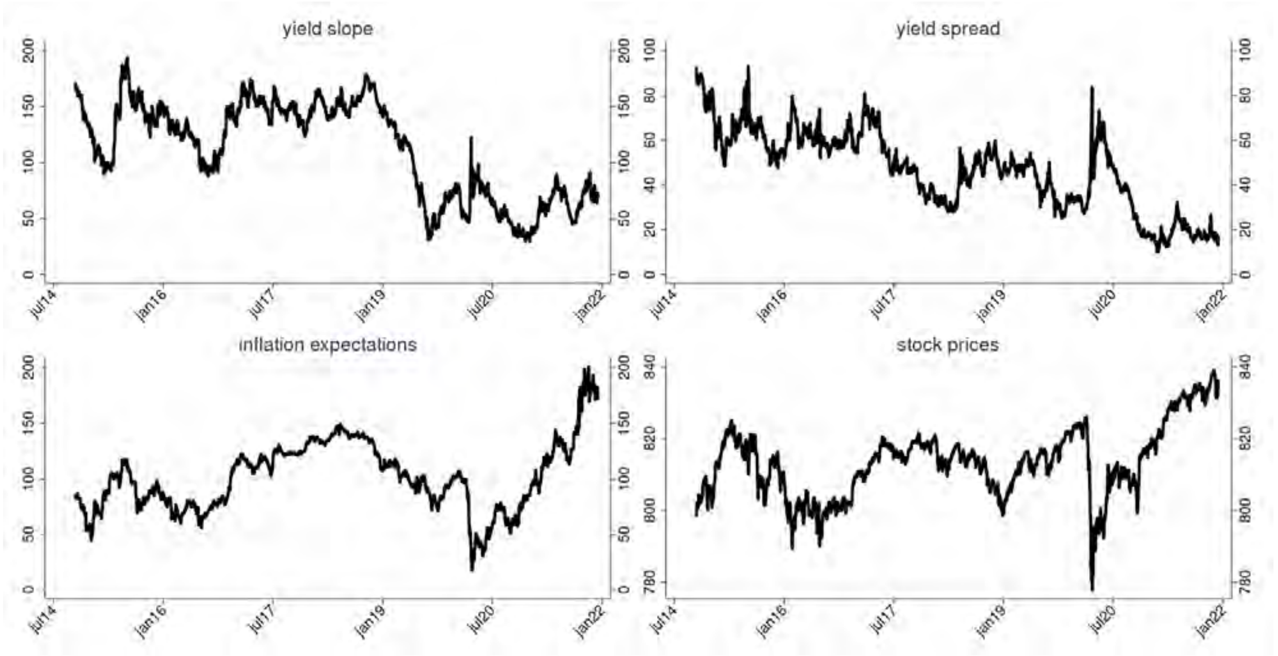
Figure 2 – Implemented purchase flows

(*€ bln*)



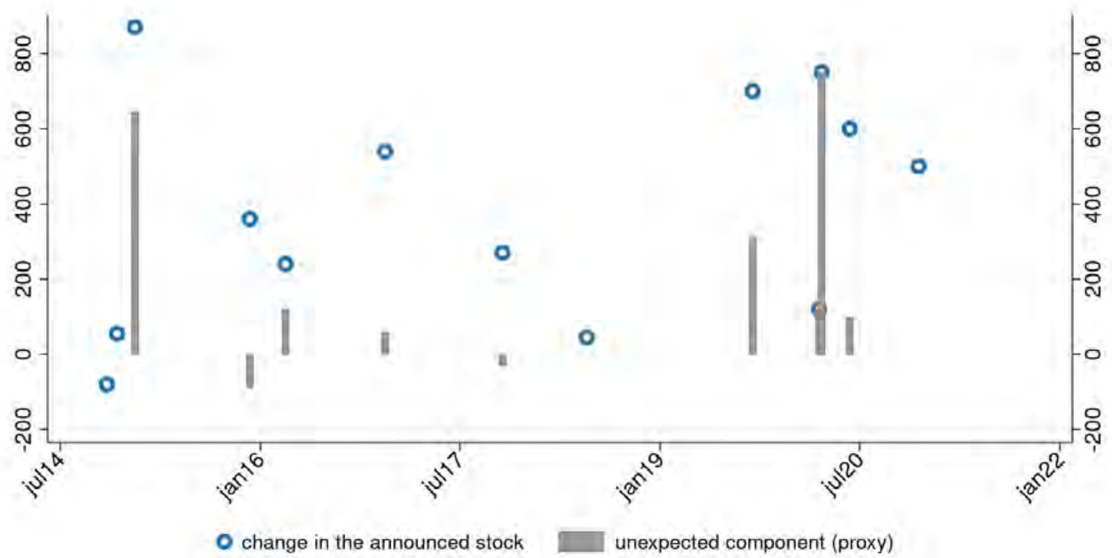
Note. The figure shows the actual gross flows, together with its constant-pace trend, sampled at the daily frequency over the period 20 October 2014 – 10 December 2021. *Actual gross flows* are the gross purchase flows under the APP and the PEPP that the ECB's Governing Council anticipates to hold and which makes public after its monetary policy meetings. The *x*-axis shows weekdays, while the *y*-axis is in billions of euro.

Figure 3 – Financial market variables



***Note*.** The figure shows the block of financial variables used in the model, sampled at the daily frequency over the period 20 October 2014 – 10 December 2021. The x-axis shows weekdays. *Yield slope* is the difference between the long-term government bond yield (i.e., the GDP weighted average of 10-year government bond yields in the euro area) and the short-term risk-free rate (i.e., the 1-month Overnight Indexed Swap rate). *Yield spread* is the difference between the long-term government bond yield and the corresponding risk-free rate. *Inflation expectations* is a market-based measure of medium-term expected inflation (i.e., the 2-year 1-year forward Inflation Linked Swap rate). *Stock prices* is a stock index of Eurozone stocks (i.e., the Euro Stoxx 50). Purchases are expressed in €bn. Yields and inflation expectations are expressed in basis points. The stock price index is expressed in log-levels (multiplied by 100).

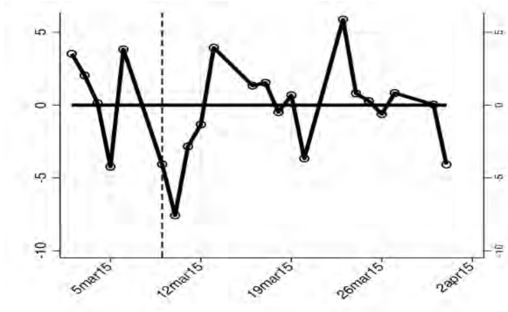
Figure 4 – Survey-based external instrument for the announced stock



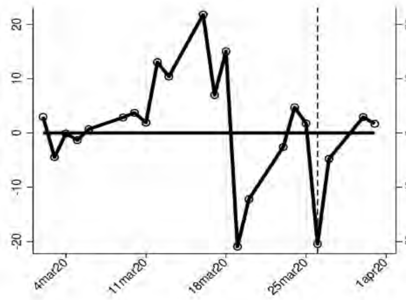
***Note*.** The figure compares the change in the announced stock of purchases (circles) with its unexpected component (bars), computed using the median expectation in the surveys conducted by Bloomberg and by the ECB (Survey of Monetary Analysts). The latter is the external instrument used to identify the announcement shock (see Table 1). The x-axis shows weekdays, while the y-axis is in billions of euro.

Figure 5 – Changes in the yield slope around kick-off dates

(a) Start of the PSPP (March 2015)



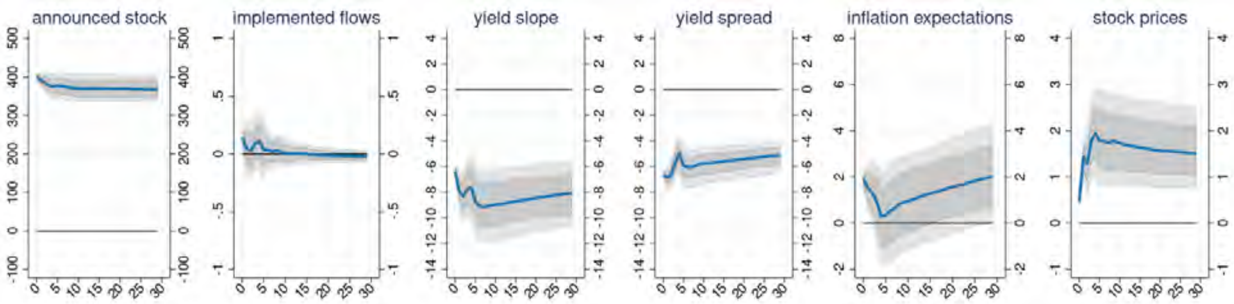
(b) Start of the PEPP (March 2020)



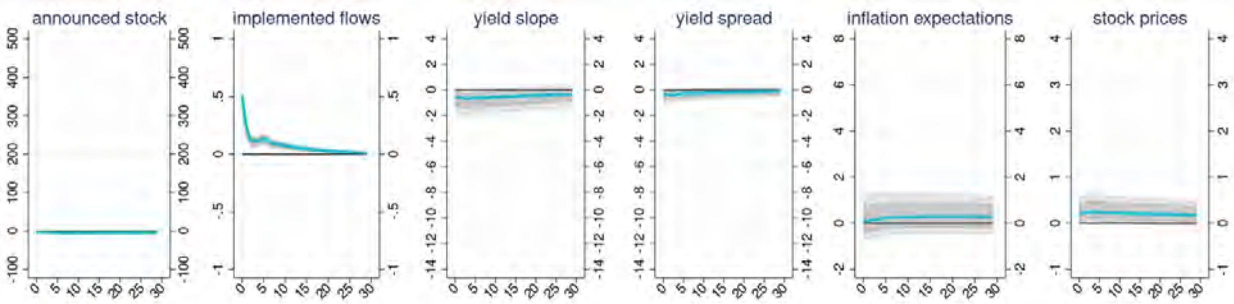
Note. The figure shows the daily change in the slope of the government yield curve at the kick-off dates of the PSPP in March 2015 and of the PEPP in March 2020 (marked by the vertical dashed lines). The x-axis shows weekdays, while the y-axis is in basis points.

Figure 6 – Transmission of shocks

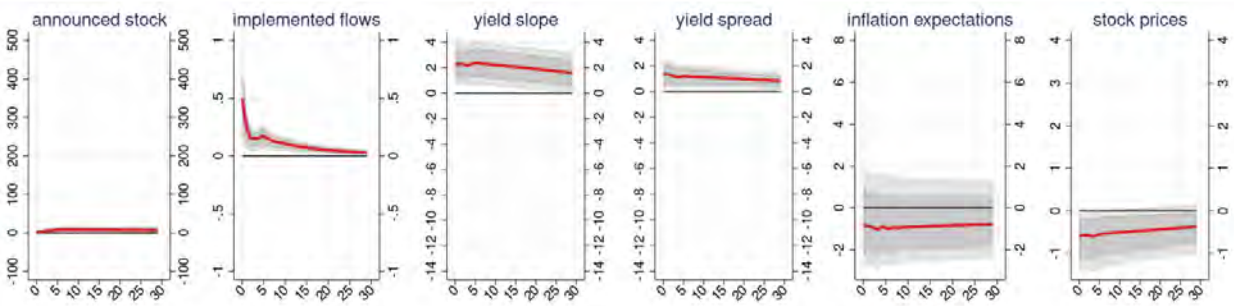
(a) Responses to an announcement shock



(b) Responses to an implementation shock



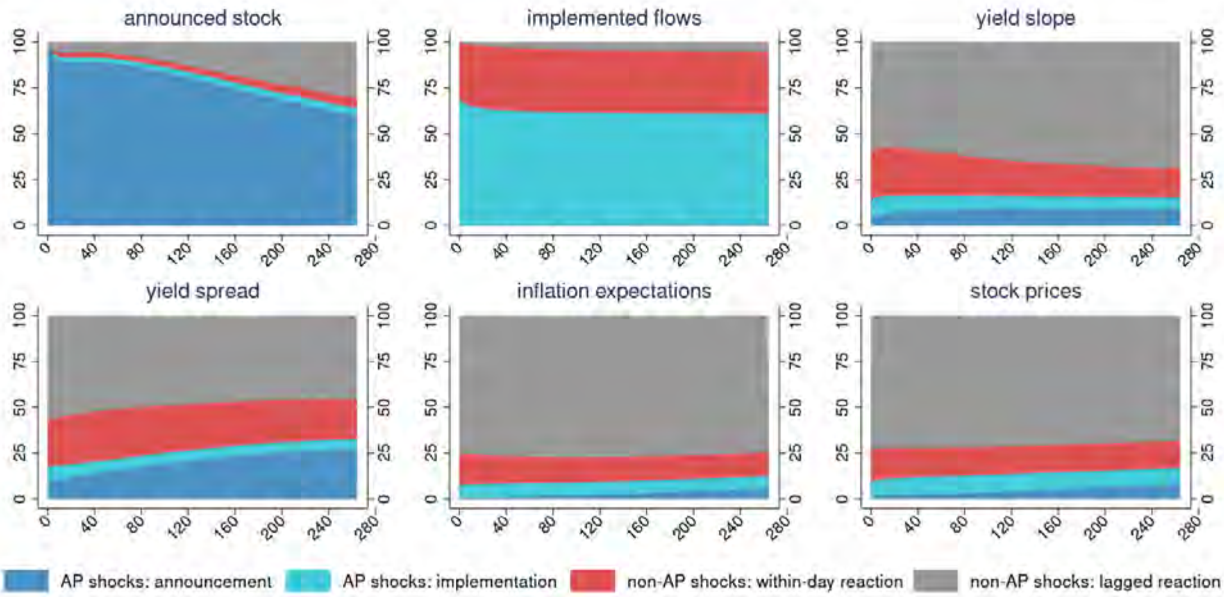
(c) Responses to shocks exerting a within-day response of actual purchases



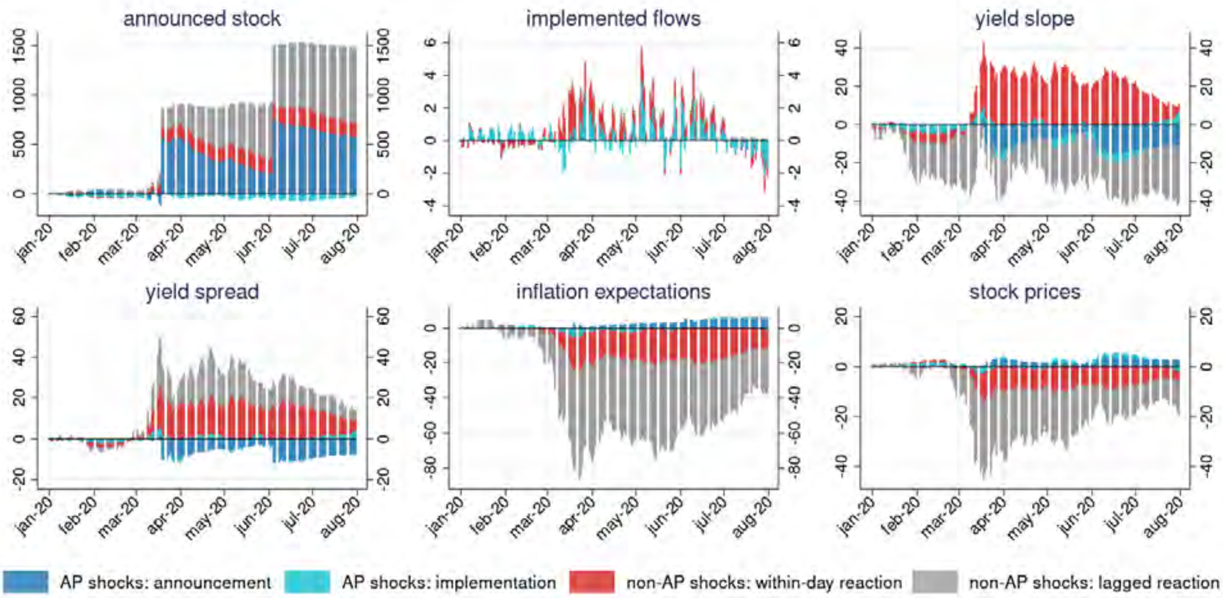
Note. The figure shows the dynamic effects of announcement, implementation and within-day financial shocks. Solid lines and the dark (light) grey bands denote, respectively, posterior medians and 68% (90%) credible intervals. The horizon is the number of working days after the shock. Responses are normalized so that the median response of the announced stock (implemented flows) to an announcement (implementation) shock increases on impact by €400 bn (€0.5 bn), which denotes the typical change observed over the analyzed sample.

Figure 7 – Relevance of shocks

(a) On average



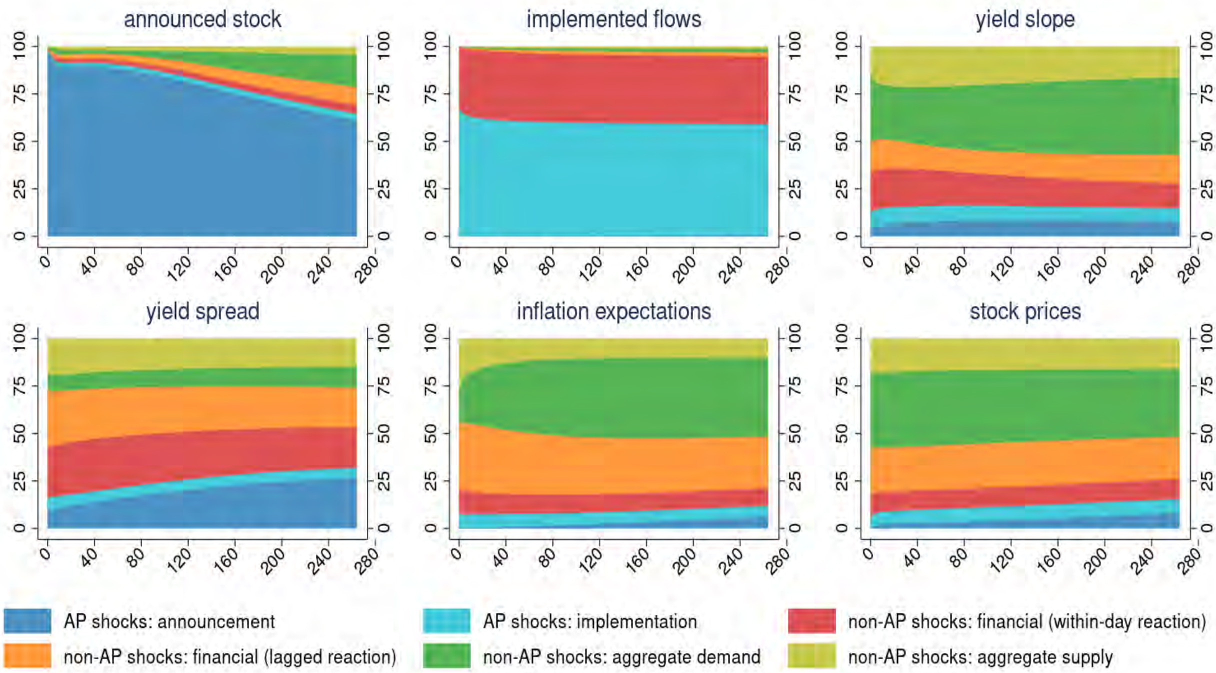
(b) Around the height of the Covid-19 crisis



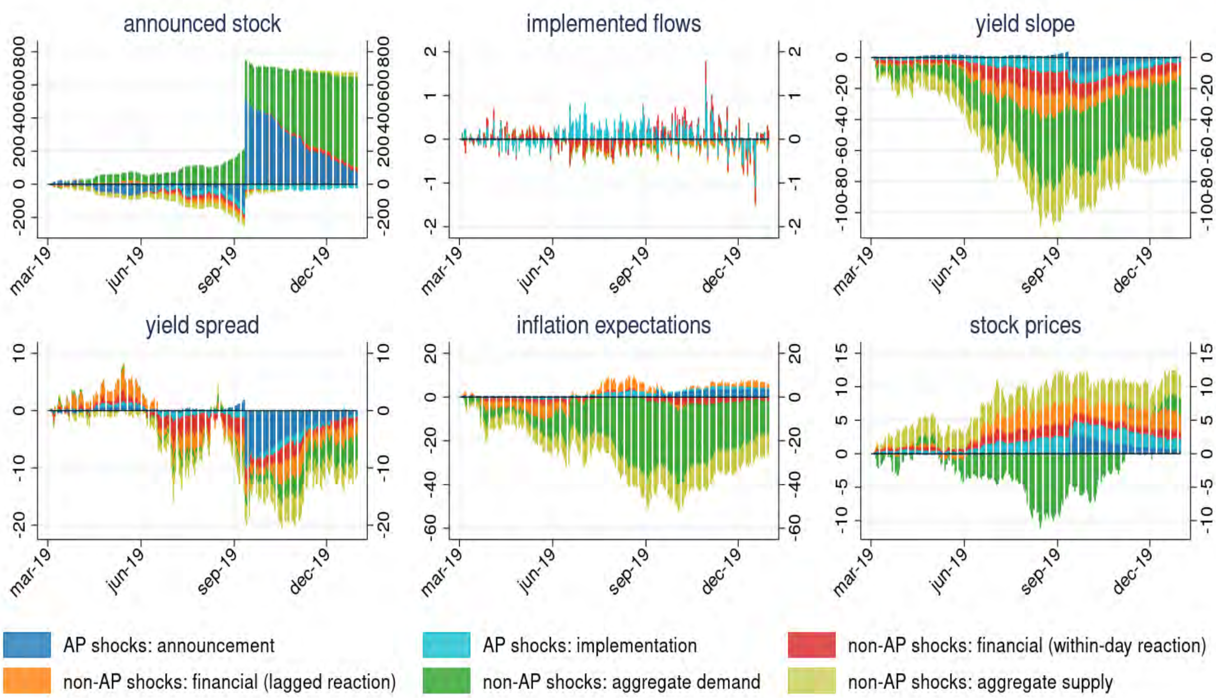
Note. *Top panel*. Forecast Error Variance Decompositions (FEVDs). For each variable, the colored areas show the share of the forecast error variance explained by AP shocks and non-AP shocks. *Bottom panel*. Historical Decompositions (HDs). Each panel shows the contribution of the identified structural shocks to the unexpected change in each variable, i.e. the difference between the actual dynamics and its steady-state component (i.e., the unconditional forecast produced by the BVAR model). Notice that each contribution is normalized to zero in December 31, 2019.

Figure 8 – Relevance of shocks under extended version

(a) On average



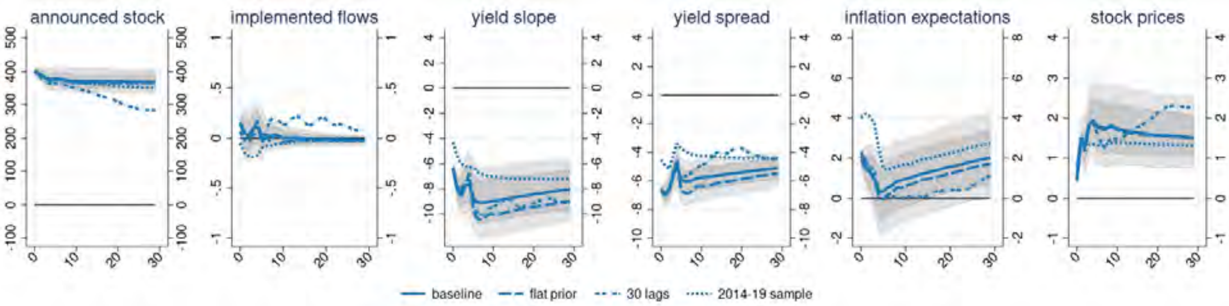
(b) Around the APP restart announcement (September 2019)



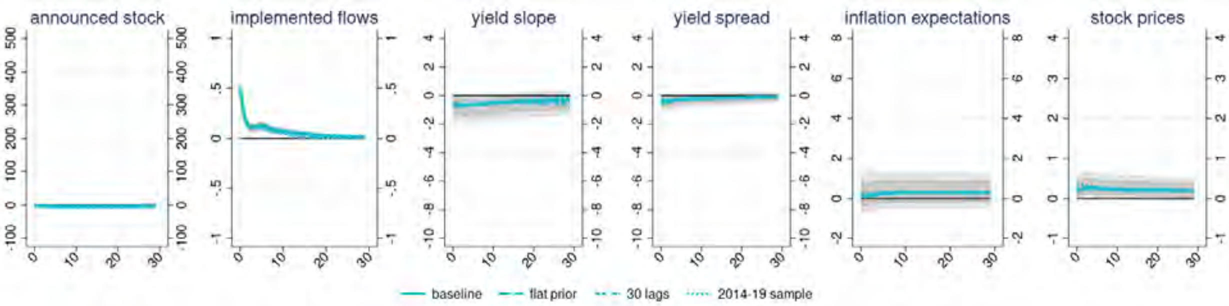
Note. *Top panel*. Forecast Error Variance Decompositions (FEVDs) under the shock identification summarized in Table 2. For each variable, the colored areas show the share of the forecast error variance explained by AP shocks and non-AP shocks. *Bottom panel*. Historical Decompositions (HDs) under the shock identification summarized in Table 2. Each panel shows the contribution of the identified structural shocks to the unexpected change in each variable, i.e. the difference between the actual dynamics and its steady-state component (i.e., the unconditional forecast produced by the BVAR model). Notice that each contribution is normalized to zero in March 1, 2019.

Figure 9 – Transmission of shocks under alternative specifications

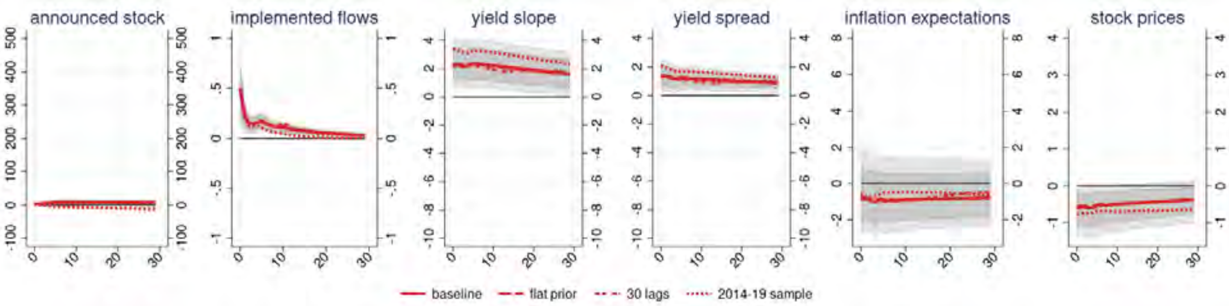
(a) Responses to announcement shocks



(b) Responses to implementation shocks



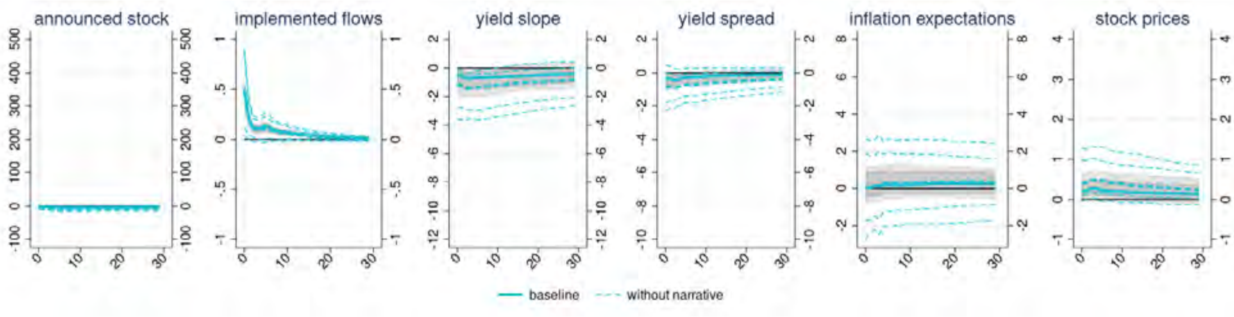
(c) Responses to shocks exerting a within-day response of the actual purchases



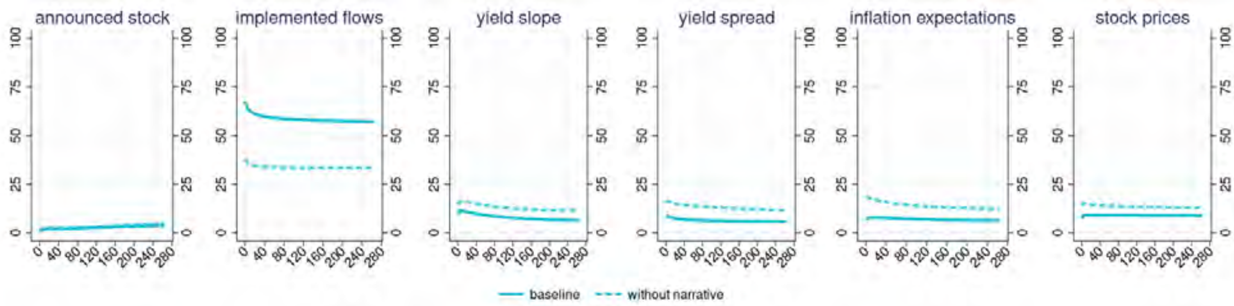
Note. Impulse responses (IRFs). The solid lines and the dark (light) grey bands denote, respectively, posterior medians and 68% (90%) credible intervals. The other lines indicate perturbations of the baseline model. In more detail, dashed lines refer to replacing Minnesota prior with a flat prior. Dotted lines refer to including 30 lags (instead of 6). Dashed-dotted lines denote pre-Covid-19 estimation sample. The horizon denotes the number of working days after the shock.

Figure 10 – Transmission and relevance of implementation shocks the role of narrative restrictions

(a) Transmission



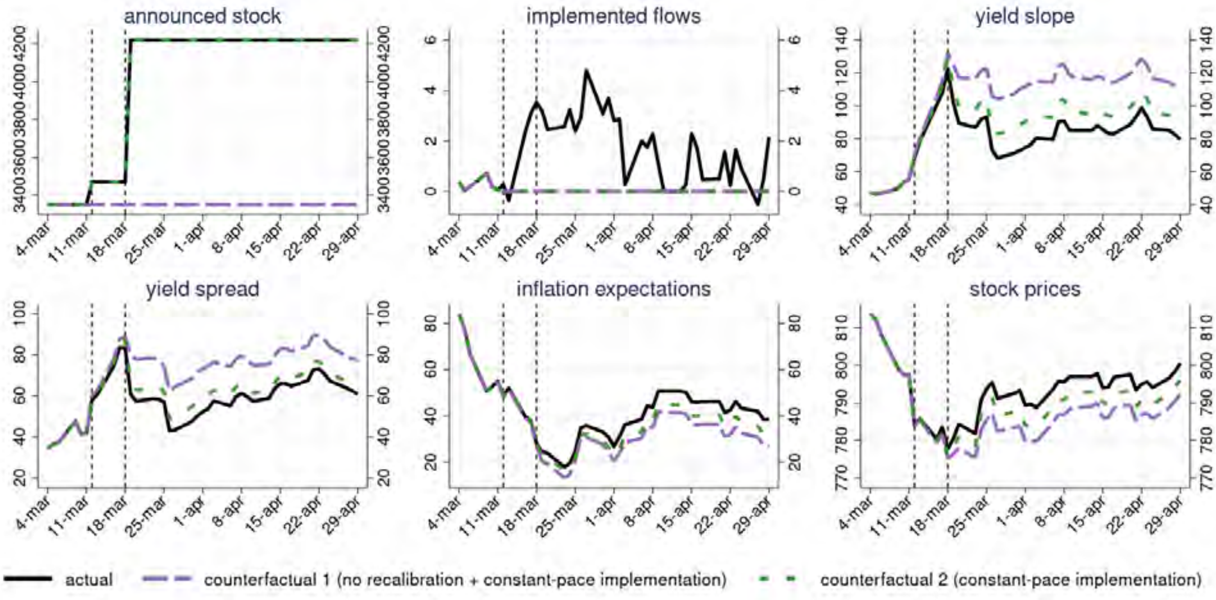
(b) Relevance (on average)



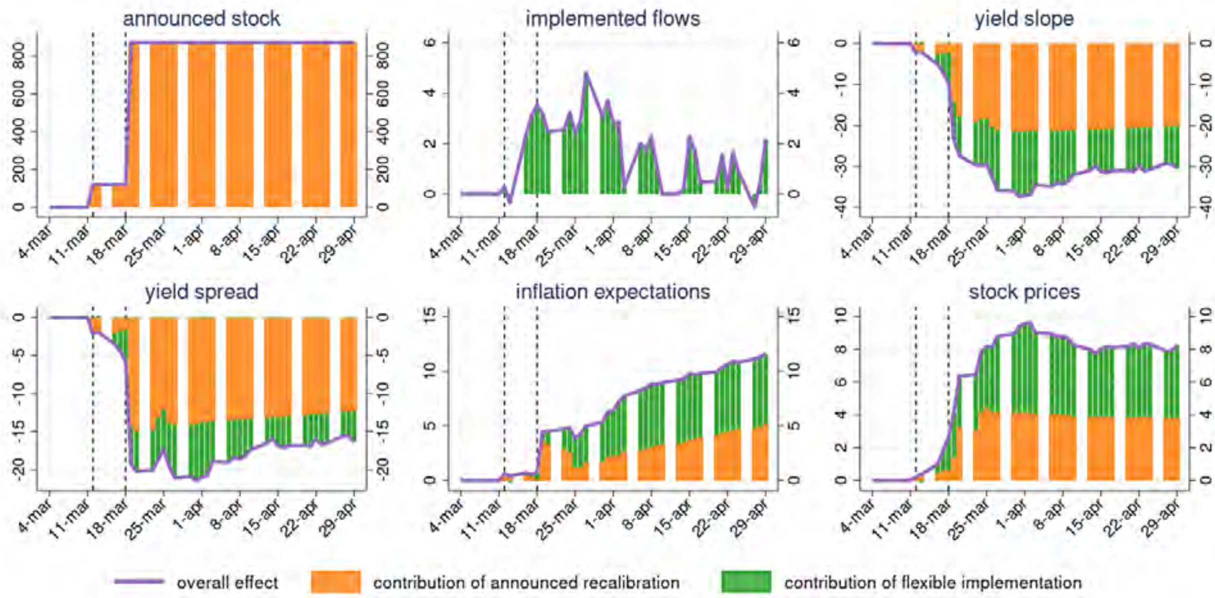
Note. Panel a: Impulse responses (IRFs). The solid lines and the dark (light) grey bands denote, respectively, posterior medians and 68% (90%) credible intervals under the baseline identification. The thick dashed lines and the thinner dashed lines refer to posterior medians and 68% (90%) credible intervals obtained when removing narrative restrictions. The horizon denotes the number of working days after the shock. Panel b: Forecast Error Variance Decompositions (FEVDs). In each plot the solid line represents the share of variance explained by implementation shocks under the baseline identification, while the dashed line represents the share of variance explained by implementation shocks when removing narrative restrictions.

Figure 11 – Effects of APs in the aftermath of the PEPP announcement

(a) Actual and counterfactuals paths



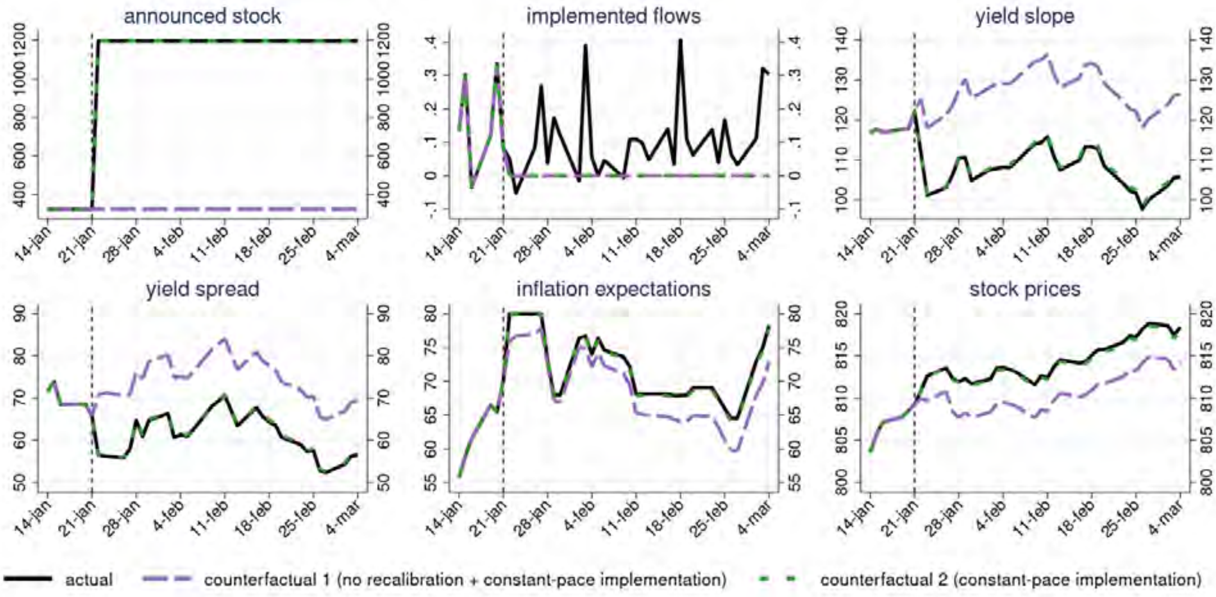
(b) Stock-flow decomposition



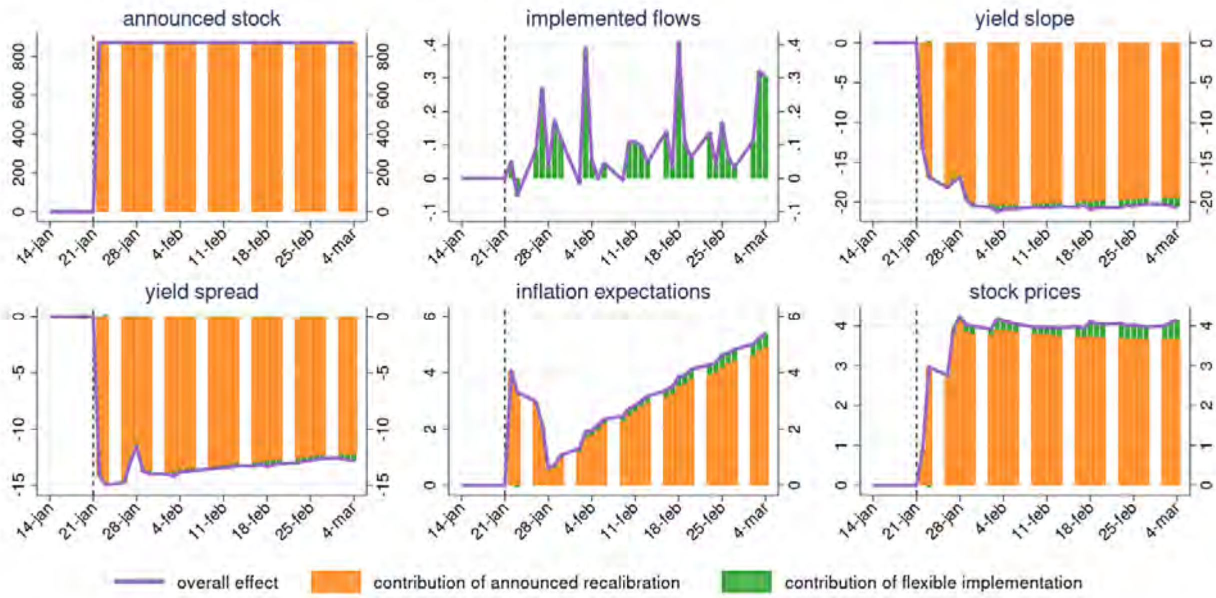
***Note*.** Panel (a) shows the actual and counterfactual paths of all the endogenous variables included in the model. All the paths coincide until the day before the announcement made on 12 March 2020 (marked by the vertical dashed line). *Counterfactual #1* assumes that the ECB neither announced a recalibration of its APs nor operated in flexibility. *Counterfactual #2* assumes that the ECB announced the recalibration, as actually occurred, but implemented them at a constant monthly pace. Panel (b) shows the resulting effects. *Overall effect* is the difference between the actual path and the counterfactual path #1. *Contribution of announced recalibration* is the difference between the actual path and the counterfactual path #2. *Contribution of flexible implementation* is the difference between counterfactual 1# and counterfactual #2. In each plot, the thin dashed lines represent March 12, 2020, i.e. the day in which the ECB Governing Council announced the temporary APP envelope, and March 18, 2020, in which an unscheduled meeting launched the PEPP.

Figure 13 – Effects of APs in the aftermath of the PSPP announcement

(a) Actual and counterfactuals paths



(b) Stock-flow decomposition



Note. Panel (a) shows the actual and counterfactual paths of all the endogenous variables included in the model. All the paths coincide until the day before the announcement made on 22 January 2015 (marked by the vertical dashed line). *Counterfactual #1* assumes that the ECB neither announced a recalibration of its APs nor operated in flexibility. *Counterfactual #2* assumes that the ECB announced the recalibration, as actually occurred, but implemented them at a constant monthly pace. Panel (b) shows the resulting effects. *Overall effect* is the difference between the actual path and the counterfactual path #1. *Contribution of announced recalibration* is the difference between the actual path and the counterfactual path #2. *Contribution of flexible implementation* is the difference between counterfactual 1# and counterfactual #2. In each plot, the thin dashed line represents January 22, 2015, i.e. the day in which the ECB Governing Council announced the launch of the APP.

Table 1 – High-frequency identifying assumptions

(a) IRF restrictions

	AP shocks		non-AP shocks			
	announcement	implementation	within-day response (financial)	lagged response		
announced stock	proxy					
implemented flows	proxy	>0	>0	$=0$	$=0$	$=0$
yield slope	proxy	<0	>0			
yield spread	proxy		>0			
inflation expectations	proxy					
stock prices	proxy		<0			

(b) Narrative restrictions

(i) The contributions of the implementation shocks to the one-step ahead forecast errors of implemented flows estimated on 9 March 2015 and 26 March 2020 are (in absolute value) the largest.

(ii) The signs of the contributions of the implementation shocks to the one-step ahead forecast errors of implemented flows estimated on 9 March 2015 and 26 March 2020 coincide with those of the same forecast errors.

Note. The Table summarizes the set of restrictions used to identify the shocks. All restrictions are imposed at high-frequency, meaning that they must hold only the day in which the shock occurs. Panel (a) shows the set of restrictions imposed on the responses of the variables to the shocks. Proxy denotes the external instrument used to identify the announcement shocks. Blank entries denote unrestricted responses. Panel (b) shows the set of restrictions imposed on the contribution of the shocks to the forecast errors made on specific days.

Table 2 – Further disentangling the non-AP shocks

(a) IRF restrictions

	AP shocks		non-AP shocks			
	announcement	implementation	within-day response	lagged response		
			financial	aggregate demand	aggregate supply	financial
announced stock	proxy					
implemented flows	proxy	>0	>0	$=0$	$=0$	$=0$
yield slope	proxy	<0	>0	<0	>0	>0
yield spread	proxy		>0			>0
inflation expectations	proxy			<0	>0	<0
stock prices	proxy		<0	<0	<0	<0

(b) Narrative restrictions

(i) The contributions of the implementation shocks to the one-step ahead forecast errors of implemented flows estimated on 9 March 2015 and 26 March 2020 are (in absolute value) the largest.

(ii) The signs of the contributions of the implementation shocks to the one-step ahead forecast errors of implemented flows estimated on 9 March 2015 and 26 March 2020 coincide with those of the same forecast errors.

Note. The table summarizes the set of restrictions used to identify the shocks. All restrictions are imposed at high-frequency, meaning that they must hold only the day in which the shock occurs. Panel (a) shows the set of restrictions imposed on the responses of the variables to the shocks. Proxy denotes the external instrument used to identify the announcement shocks. Blank entries denote unrestricted responses. Panel (b) shows the set of restrictions imposed on the contribution of the shocks to the forecast errors made on specific days.

Table 3 – Effects of APs in the aftermath of the PEPP announcement

days after the announcement	announced stock		implemented flows		yield slope		yield spread		expected inflation		stock prices	
	overall effect	flexible impl.	overall effect	flexible impl.	overall effect	flexible impl.	overall effect	flexible impl.	overall effect	flexible impl.	overall effect	flexible impl.
	(€ billion)		*(€ billion)*		*(bp)*		*(bp)*		*(bp)*		*(pp)*	
h=1	120.0	0.0	0.3	0.3	-2.2	-0.3	-2.1	-0.2	0.6	0.0	0.2	0.1
	–	–	–	–	*100.0*	*100.0*	*100.0*	*87.0*	*99.4*	*54.2*	*100.0*	*92.1*
h=5	120.0	0.0	3.5	3.5	-9.6	-7.3	-5.7	-4.1	0.7	0.7	2.7	2.1
	–	–	–	–	*100.0*	*99.4*	*99.2*	*90.6*	*55.8*	*54.9*	*99.6*	*97.7*
h=10	870.0	0.0	2.4	2.4	-29.5	-11.3	-17.4	-5.3	3.9	2.7	8.2	3.8
	–	–	–	–	*100.0*	*96.3*	*100.0*	*87.4*	*68.2*	*64.4*	*99.9*	*98.4*
h=20	870.0	0.0	2.3	2.3	-34.3	-13.1	-18.6	-5.2	8.9	5.9	8.8	4.8
	–	–	–	–	*99.9*	*87.2*	*100.0*	*76.7*	*78.5*	*70.9*	*99.6*	*92.4*
h=30	870.0	0.0	0.2	0.2	-30.1	-9.6	-16.0	-3.3	10.6	6.2	8.1	4.3
	–	–	–	–	*99.7*	*78.5*	*99.9*	*70.7*	*82.9*	*72.1*	*98.7*	*89.2*

Note. For each variable and considered horizon (i.e., days after the announcement of a policy recalibration) the Table shows the effect attributable to the overall announcement and the flexible implementation (straight entries), together with the associated probability – based on the posterior distribution of the VAR model – that the counterfactual effect is greater (if positive) or lower (if negative) than zero (*italics* entries). The announcement is made on March 12, 2020 and it is then followed by the launch of the PEPP on March 19, 2020. Notice that since the counterfactual paths of the policy variables are imposed, no *associated probability* is reported. All entries refer to the counterfactuals shown in Figure 12b.

Table 4 – Effects of APs in the aftermath of the PSPP announcement

days after the announcement	announced stock		implemented flows		yield slope		yield spread		expected inflation		stock prices	
	overall effect	flexible impl.	overall effect	flexible impl.	overall effect	flexible impl.	overall effect	flexible impl.	overall effect	flexible impl.	overall effect	flexible impl.
	(€ billion)		*(€ billion)*		*(bp)*		*(bp)*		*(bp)*		*(pp)*	
h=1	870.0	0.0	0.1	0.1	-13.4	-0.1	-14.2	0.0	4.1	0.0	0.9	0.0
	–	–	–	–	*100.0*	*100.0*	*100.0*	*87.0*	*100.0*	*54.2*	*99.8*	*92.1*
h=5	870.0	0.0	0.0	0.0	-17.0	-0.3	-11.5	-0.2	0.6	0.0	4.2	0.1
	–	–	–	–	*100.0*	*96.7*	*100.0*	*89.8*	*56.8*	*54.6*	*99.8*	*98.2*
h=10	870.0	0.0	0.1	0.1	-20.9	-0.6	-13.8	-0.3	1.9	0.1	4.1	0.2
	–	–	–	–	*100.0*	*95.2*	*100.0*	*87.2*	*72.2*	*59.8*	*99.8*	*98.3*
h=20	870.0	0.0	0.4	0.4	-21.0	-1.0	-13.4	-0.5	3.9	0.3	4.1	0.3
	–	–	–	–	*100.0*	*93.5*	*100.0*	*83.1*	*88.1*	*63.6*	*99.8*	*94.9*
h=30	870.0	0.0	0.3	0.3	-20.7	-1.3	-12.8	-0.6	5.4	0.4	4.1	0.4
	–	–	–	–	*100.0*	*89.3*	*100.0*	*80.6*	*94.0*	*65.8*	*99.8*	*94.1*

Note. For each variable and considered horizon (i.e., days after the announcement of a policy recalibration) the Table shows the effect attributable to the overall announcement and the flexible implementation (straight entries), together with the associated probability – based on the posterior distribution of the VAR model – that the counterfactual effect is greater (if positive) or lower (if negative) than zero (*italics* entries). The announcement is made on March 9, 2015. Notice that since the counterfactual paths of the policy variables are imposed, no *associated probability* is reported. All entries refer to the counterfactuals shown in Figure 13b.

APPENDIX TO

ANNOUNCEMENT AND IMPLEMENTATION EFFECTS OF CENTRAL BANK ASSET PURCHASES

Marco Bernardini and Antonio M. Conti

September 2023

APPENDIX A: CONSTRUCTION OF VARIABLES

Announced stock. Table A1 reports the relevant information for the construction of our first policy variable – the announced stock of purchases – for which we largely build over the methodology originally proposed by Weale and Wieladek (2016) and recently refined by Lhuissier and Nguyen (2021).

Table A1 – Construction of the announced stock of purchases

Relevant meeting	Date	Announced recalibration	Announced stock	Notes
Oct-14	*start*	---	350	There was no announced recalibration at the time. We assume that the announced stock is equal to the expected stock as derived from a Bloomberg survey conducted immediately after the meeting.
Nov-14	06Nov14	---	270	
Dec-14	04Dec14	---	325	
Jan-15	22Jan15	1140	1195	The announced stock is the sum between €55 bn (the realized stock under the CB3PP and the ABSPP as of end-of-February, which could be easily forecasted as of end-of-January) and the announced recalibration.
Dec-15	03Dec15	360	1555	
Mar-16	10Mar16	240	1795	
Dec-16	08Dec16	540	2335	
Oct-17	26Oct17	270	2605	
Jun-18	14June18	45	2650	
Sept-19	12Sept19	700	3350	
Mar-20	12Mar20	120	3470	
Mar-20 (emergency meeting)	19Mar20	750	4220	The decision was announced on 18 March 2020 after financial markets closed. Therefore, announcement effects on financial markets were observed only on 19 March 2020.
Jun-20	04June20	600	4820	
Dec-20	10Dec20	500	5320	

Note. Unless otherwise specified under "Notes", the announced recalibration is taken from Lhuissier and Nguyen (2021) and the announced stock is the sum between the previous announced stock and the announced recalibration.

Implemented-flow gap. Our second policy variable, the gap between actual purchases and their underlying constant-pace trend, is constructed in four steps. First, for each asset purchase programme, we construct the *average monthly pace of net asset purchases*, a figure that can be derived (directly or indirectly) by official announcements (Table A2).

Table A2 – Construction of the average monthly pace of net asset purchases

(a) APP component

Relevant meeting	Date	APP component	Notes
---	*start*	10	Before the implementation of the PEPP, we assume a monthly pace of €10bn per month, which is the approximately the average constant pace of net purchases under the CB3PP and the ABSPP.
Jan-15	09Mar15	60	Although announced in January, the "legal" decision of the PSPP was taken on 4 March, effective from 9 March (https://eur-lex.europa.eu/legal-content/EN/TXT/HTML/?uri=CELEX:32015D0010&from=EN)..
Mar-16	01Apr16	80	
Dec-16	01Apr17	60	
Oct-17	01Jan18	30	
June-18	01Oct18	15	
June-18	01Jan19	0	
Sept-19	01Nov19	20	
Mar-20	13Mar20	20+120/9.5	Baseline pace (€20bn/m) + the ratio between the announced envelope and the announced horizon (9 months and a half).
Mar-20	01Jan21	20	

(b) PEPP component

Relevant meeting	Date	PEPP component	Notes
Mar-20 (emergency meeting)	26Mar20	750/9	Although announced on March 18, a legal decision of the PEPP was taken on 24 March, effective from 26 March (https://eur-lex.europa.eu/legal-content/EN/TXT/HTML/?uri=CELEX:32020D0440&from=EN).
Jun-20	05Jun20	(750+600-235)/13	Ratio between the new remaining envelope and the new remaining horizon. The "used" envelope is taken from the Weekly Financial Statement that was publicly available at the time.
Dec-20	11Dec20	(750+600+500-718)/16	

Note. Unless otherwise specified under "Notes", the constant-pace trend is equal to the announced pace and the date is the first day in which the constant-pace trend becomes effective.

Second, we construct the corresponding average monthly pace of gross asset purchases. We do that by taking into account the monthly redemptions coming due each month, an information that is publicly available to market participants. Third, we derive the average daily pace of gross asset purchases (i.e., what we label the "underlying constant-pace trend"). We impose that the latter variable takes value zero when the Eurosystem pauses its actual purchases (typically during holiday periods), an information that is also publicly available. On all the other days, we set it equal to the ratio between the average monthly pace of gross asset purchases and the number of days in the month in which the market desk is expected to carry-out actual purchases. Finally, we take the difference between the series of actual gross purchases and the underlying constant-pace trend.

Announced-stock surprise. Table A3 reports the relevant information for the construction of our external instrument, which is largely consistent with the one proposed by Lhuissier and Nguyen (2021). The main novelty is, however, the extension of the proxy to the purchase announced after 2019, which is the end of the sample considered by Lhuissier and Nguyen (2021). In particular, we assume a full surprise for the launch of both the temporary APP envelope and the PEPP, while we assume a 100 million surprise for the June 2020 recalibration (based on expectation taken from the SMA) and a fully expected recalibration for the December 2020 meeting of the Governing Council (based on the SMA as well).

Table A3 – Construction of the external instrument

Relevant meeting	**Date**	**Announced recalibration**	**Announced stock**			**Notes**
		expectation	***expectation***	***realized***	***surprise (external instrument)***	
Oct-14	---	---	350	350	0	By construction, the expected stock is equal to the announced stock (see Table A1).
Nov-14	06Nov14	---	270	270	0	
Dec-14	04Dec14	---	325	325	0	
Jan-15	22Jan15	---	550	1195	645	
Dec-15	03Dec15	450	1645	1555	-90	
Mar-16	10Mar16	120	1675	1795	120	
Dec-16	08Dec16	480	2275	2335	60	
Oct-17	26Oct17	300	2635	2605	-30	
Jun-18	14Jun18	45	2650	2650	0	
Sept-19	12Sep19	390	3040	3350	310	
Mar-20	12Mar20	0	3350	3470	120	We assume full surprise.
Mar-20 (emergency meeting)	19Mar20	0	3470	4220	750	We assume full surprise.
Jun-20	04Jun20	500	4720	4820	100	The expected recalibration is taken the SMA.
Dec-20	10Dec20	500	5320	5320	0	The expected recalibration is taken from the SMA.

***Note*.** The "realized" announced stock is taken from Table A1. A surprise is defined as the difference between the realized announced stock and its expectation. Unless otherwise specified under "Notes", the "expected" announced stock is constructed as the sum between the previous announced stock and the expected announced recalibration calculated in Lhuissier and Nguyen (2021).

Financial variables. Table A4 reports the sources and the description of the financial variables used construct the endogenous variables employed in the baseline VAR model. Notice that inflation expectations used in the VAR are the 2-year 1-year forward Inflation Linked Swap rate, which is constructed using the 3-year and 1-year Inflation Linked Swap rate.

Table A4 – Construction of financial conditions

(a) Underlying variables

Variable	Source	Description
OIEUR1M	Eikon Refinitiv	1-month Overnight Index Swap (OIS) rate with floating rate EONIA.
OIEU10Y	Eikon Refinitiv	10-year Overnight Index Swap (OIS) rate with floating rate EONIA.
EURIS1Y	Eikon Refinitiv	1-year Inflation Linked Swap (ILS) rate with floating rate HICP.
EURIS3Y	Eikon Refinitiv	3-year Inflation Linked Swap (ILS) rate with floating rate HICP.
GDBR10	Bloomberg	10-year government bond yield for Germany.
GAGB10YR	Bloomberg	10-year government bond yield for Austria
GBGB10YR	Bloomberg	10-year government bond yield for Belgium
GFIN10YR	Bloomberg	10-year government bond yield for Finland
GFRN10	Bloomberg	10-year government bond yield for France
GGGB10YR	Bloomberg	10-year government bond yield for Greece
GBTPGR10	Bloomberg	10-year government bond yield for Italy
GIGB10YR	Bloomberg	10-year government bond yield for Ireland
GNTH10YR	Bloomberg	10-year government bond yield for Netherlands
GSPT10YR	Bloomberg	10-year government bond yield for Portugal
GSPG10YR	Bloomberg	10-year government bond yield for Spain
STOXX50	Eikon Refinitiv	STOXX Europe 50 stock index.

(b) Model variables

Variable	Notes
Yield slope	Difference between the GDP-weighted 10-year government bond yield and the 1-month OIS rate. The GDP-weighted 10-year government bond yield is a weighted average of the 10-year government bond yields of the 11 largest countries in the euro area, weighted by the GDP level realized over the previous year. OIS rates are expressed in terms of €STR by subtracting a constant spread of 8.5 bp (the official "spread" used to convert EONIA in terms of €STR).
Yield spread	Difference between the GDP-weighted 10-year government bond yield and the 10-year OIS rate. The GDP-weighted 10-year government bond yield is a weighted average of the 10-year government bond yields of the 11 largest countries in the euro area, weighted by the GDP level realized over the previous year. OIS rates are expressed in terms of €STR by subtracting a constant spread of 8.5 bp (the official "spread" used to convert EONIA in terms of €STR).
Inflation expectations	2-year 1-year forward ILS rate.
Stock prices	STOXX Europe 50 stock index.

Note. Daily data obtained from the corresponding source (see Panel a). The baseline estimation sample goes from December 20, 2014 to December 10, 2021, for a total number of observations $T = 1859$.

APPENDIX B: METHODOLOGICAL ASPECTS

Let us recall our baseline VAR model:

$$y_t = c + A(L)y_{t-1} + u_t, \quad u_t \sim i.i.d. N(0, \Sigma) \tag{B.1}$$

where y is a vector of $N = 6$ endogenous variables, c is a constant term, u is a vector of forecast errors with zero mean and variance-covariance matrix Σ. $A(L)$ is a matrix polynomial in the lag operator L; t denotes the time frequency, which in our setting is daily (weekdays). Notice that the standard identification problem in VARs implies that the daily forecast errors of the model $\boldsymbol{u}_t$ can be expressed as a linear combination of daily shocks $\boldsymbol{\varepsilon}_t$, having zero mean and variance-covariance matrix I:

$$u_t = B\varepsilon_t, \quad \varepsilon_t \sim i.i.d. N(0, \mathrm{I}) \Rightarrow \Sigma = BB' \tag{B.2}$$

As explained in Section 2, we use a combination of external instruments and zero-sign restrictions to identify announcement and implementation shocks, and, in order to do so, we follow and adapt the algorithm developed by Cesa-Bianchi and Sokol (2022). Let us rewrite the 6×6 B matrix after partitioning it in columns, as follows: $B = [b_1\, b_2\, b_3\, b_4\, b_5\, b_6\,]$. Our strategy is to pin down the first column b_1 by means of the external instruments methodology proposed by Stock and Watson (2012, 2018) and Mertens and Ravn (2013), relying on the proxy described in Section 2 and presented in Figure 4 of the paper. Then, we focus on columns b_2 and b_3 using sign restrictions to identify the implementation shock and the within-day response shock. Also, we impose zeros on the reaction of the actual purchases gap to "other" (macro) shocks, that is we impose the following restriction on three coefficients: $b_{42} = b_{52} = b_{62} = 0$.

We now explain more in detail how we adapt the algorithm to our Bayesian framework. In practice, our work involves two main steps:

1. First, estimating the reduced-form VAR and the first column of B, which amounts to estimating $A(L)$, Σ and b_1.
2. Second, estimating the other columns of the B matrix (i.e., the remaining five columns, given that the model includes six variables; *n*=6).

Estimating the reduced-form VAR model and the first column of B. The first step entails the estimation of $A(L)$, Σ and b_1. In order to do so, we rely on a Gibbs sampling algorithm akin to the one developed by Caldara and Herbst (2019). Recall that the external instrument z_t is related to the announcement shock ε_t^{ann} through the following equation:

$$z_t = \beta\varepsilon_t^{ann} + \sigma_v v_t, \quad v_t \sim i.i.d. N(0,1), \; v_t \perp \varepsilon_t \tag{B.2}$$

where v_t is the measurement error. We use a standard Minnesota prior for $A(L)$, Σ. Since data are in (log-)levels, we center the prior on the autoregressive coefficient around 1 for the first lag and around 0 for the other lags. As for the shrinkage parameters, in our baseline specification we set λ_1=0.3 for the overall tightness, while the relative tightness of the prior variance in the other lags in a given equation compared to the own lags is set λ_2=0.5. The prior on the constant and the deterministic components is diffuse – $\lambda_3 = 10^5$ – and we assume a linear decay (the parameter which governs the decay is λ_4=1.0). Finally, we assume a flat (non-dogmatic) prior on β and σ_v, that is we assume $\beta \sim N(0,1)$ and an inverse-gamma distribution on σ_v with degrees of freedom s_1=2 and centering parameter s_2=0.02, so that the prior is not very informative.

Estimating the remaining columns of B. In order to recover columns $[b_2\, b_3\, b_4\, b_5\, b_6]$ we need to find values fulfilling two important conditions: (i) they must satisfy the sign and zero restrictions summarized in Table 1 and (ii) conditional on b_1, they must verify the relation $\Sigma = BB'$. We achieve this feature by closely follow the methodology proposed by Cesa-Bianchi and Sokol (2022), just adapting it to our Bayesian framework.[1] Let C be the Cholesky factor of the variance/covariance matrix Σ, i.e. $\Sigma = CC'$, and Q an orthonormal matrix $QQ' = I$. We assume a uniform prior on Q. The problem then consists of finding a specific Q such that:

$$B = CQ = C\,[q_1\, q_2\, q_3\, q_4\, q_5\, q_6] \tag{B.3}$$

It is immediate to see that conditioning on b_1 implies restricting the first column of Q such that $q_1 = C^{-1} b_1$. After doing that, in order to find such an orthonormal matrix Q that the remaining five columns of B satisfy the zero and sign restrictions reported in Table 1, we implement the following steps:

1. draw $A(L)$, Σ and b_1 using the Gibbs sampler similarly to Caldara and Herbst (2019);
2. compute C and $\widehat{q_1} = C^{-1} b_1$;
3. draw a candidate 6×6 Q following the algorithm proposed by Uhlig (2005) and further developed by Peersman (2005), then extended to zero restrictions in Boeckx et al. (2017)[2];
4. replace q_1 with $\widehat{q_1}$;
5. orthogonalize $[q_2\, q_3\, q_4\, q_5\, q_6]$ with respect to $\widehat{q_1}$ by using the Graham-Schmidt methodology as suggested by Cesa-Bianchi and Sokol (2022), obtaining a 5×5 matrix $\tilde{Q}$;
6. compute $B = C\tilde{Q}$ and the structural shocks $\varepsilon_t = B^{-1} u_t$;
7. if columns $[b_2\, b_3\, b_4\, b_5\, b_6]$ satisfy *jointly* the sign restrictions imposed on impact to identify implementation shocks (that is, restrictions on b_2) and within-day reaction shocks (that is, restrictions on b_3) and the zero restrictions $b_{42} = b_{52} = b_{62} = 0$, then retain the draw; otherwise, discard it and go back to step 1.

Informativeness of the prior on the orthonormal matrix Q. Some recent influential contributions on set-identified models have raised critiques to the machinery that most of the literature works with, namely the adoption of the uniform or Haar prior on the orthonormal matrix Q that is rotated to obtain our identified shocks. In particular, Baumeister and Hamilton (2015, 2018, 2022) note that in set-identified models the prior may affect the posterior in spite of a deep sample length. In this case, a researcher may unintentionally end up with using an informative prior which drives the results. The authors propose to overcome this shortcoming by eliciting priors on the matrix containing the contemporaneous relations between the endogenous variables instead of the one which includes the

[1] Cesa-Bianchi and Sokol (2022) work with a frequentist framework.

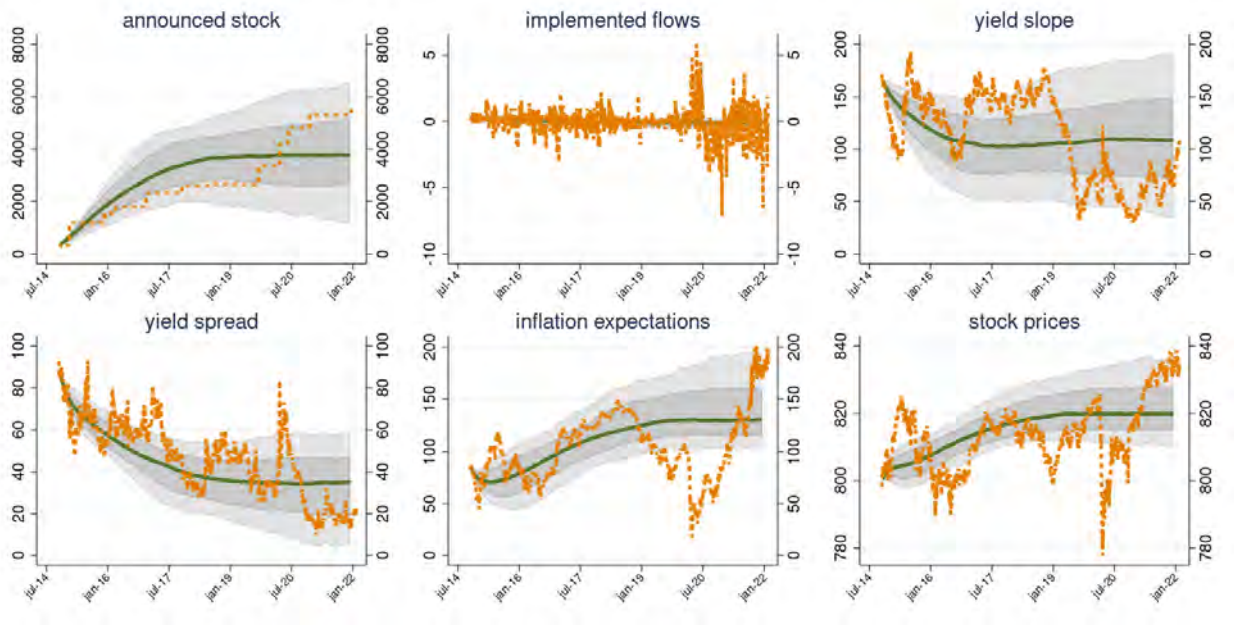
[2] Benati (2014) and Boeckx et al., (2017) show that the results of this algorithm, which is implemented using R.A.T.S. 10 and its procedure @forcedfactor, are robust to the adoption of the sampler proposed by Arias et al. (2018).

contemporaneous relations between shocks. Such an approach is useful, especially when working with models for which such prior information is readily available (from theory or empirical structural models) and can therefore be promptly exploited in the empirical analysis. Unfortunately, this is not (the case for the literature on APs, especially at such a high-frequency as the daily one used in our framework. We therefore proceed acknowledging that our prior may, in principle, play an informative role, but we share the view of Braun and Brueggemann (2022) that this type of prior implies that for a given correlation structure, all SVAR models that satisfy the identifying restrictions are equally likely a priori. In our view, this is a reasonable prior to work with when no further identifying information is available. At the same time, we are confident that the undue influence of the Haar prior on the model posterior should not represent a large factor from a quantitative point of view, for several reasons. First, a consensus on the (un)informativeness of the Haar prior and its consequences is far from being reached (see, for example, Rubio-Ramirez, 2022; Arias et al., 2022). Second, with respect to the models considered by Baumeister and Hamilton (2018) we strongly refine the set-identification of implementation and within-day reaction shocks by adding zero and narrative restrictions to the pure sign-restrictions approach. Both of these tools tend to tighten the identification of the structural responses of interest compared to the pure sign-restrictions approach (Inoue and Kilian, 2023).

Appendix C: Additional Figures and Tables

Reduced-form model

Figure C1 – Model-implied steady states

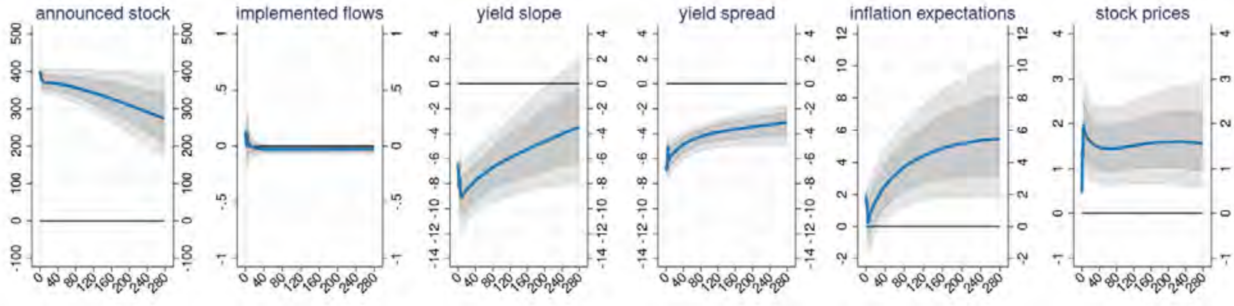


Note. Steady states computed by the BVAR model. For each variable, the dark green line is the unconditional forecast computed by the BVAR (i.e. the forecast in absence of any shock), while the dark (light) grey shaded area represents its 68% (90%) credibility interval. The orange line is the actual dynamics.

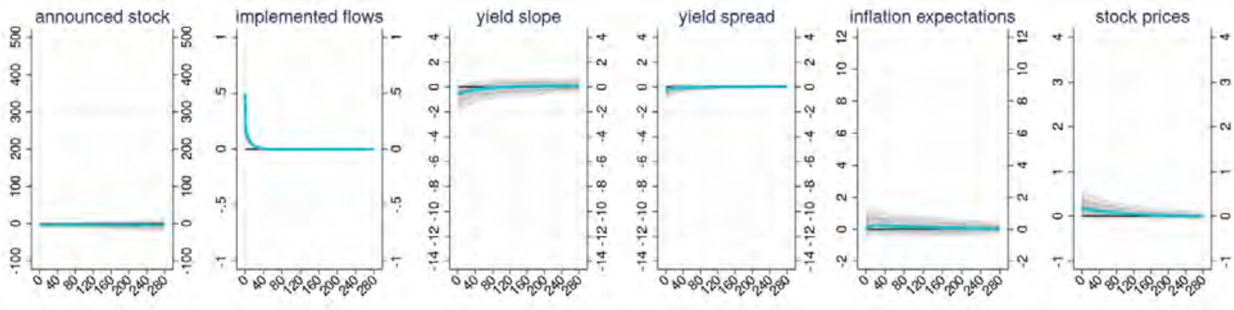
Transmission of shocks

Figure C2 – Transmission of shocks at longer horizons (baseline model)

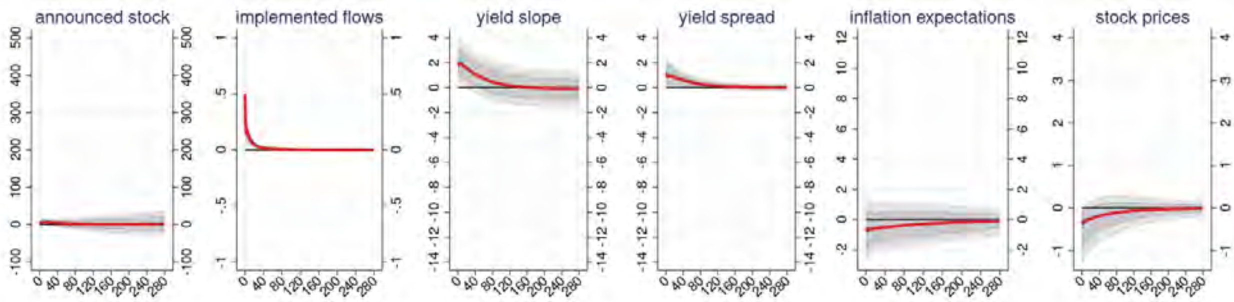
(a) Responses to an announcement shock



(b) Responses to an implementation shock



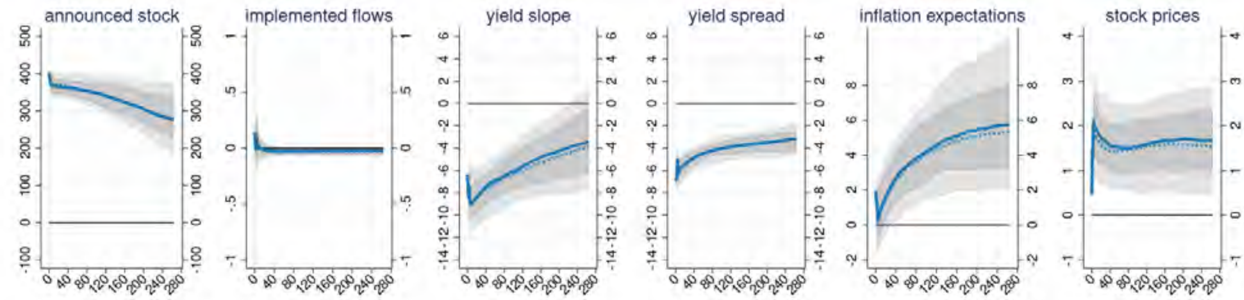
(c) Responses to shocks exerting a within-day response of actual purchases



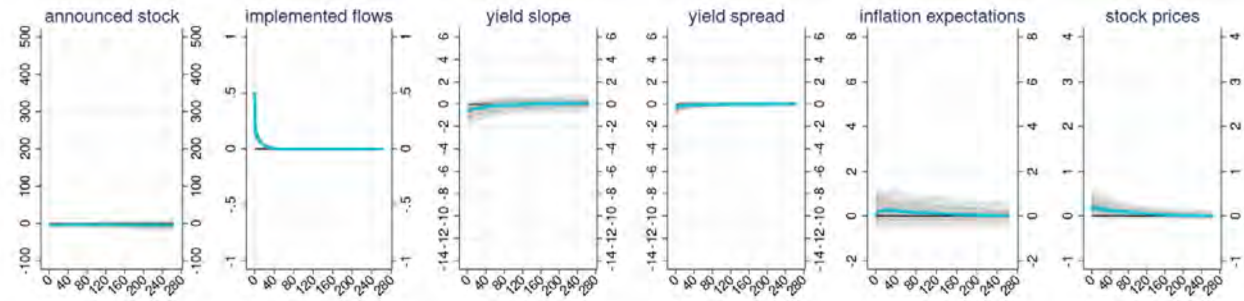
Note. The figure shows the dynamic effects of announcement and implementation shocks. Solid lines and the dark (light) grey bands denote, respectively, posterior medians and 68% (90%) credible intervals. The horizon is the number of working days after the shock. Responses are normalized so that the median response of the announced stock (implemented flows) to an announcement (implementation) shock increases on impact by €400 bn (€0.5 bn), which denotes the typical change observed over the analyzed sample.

Figure C3 – Transmission of shocks at longer horizons (extended model)

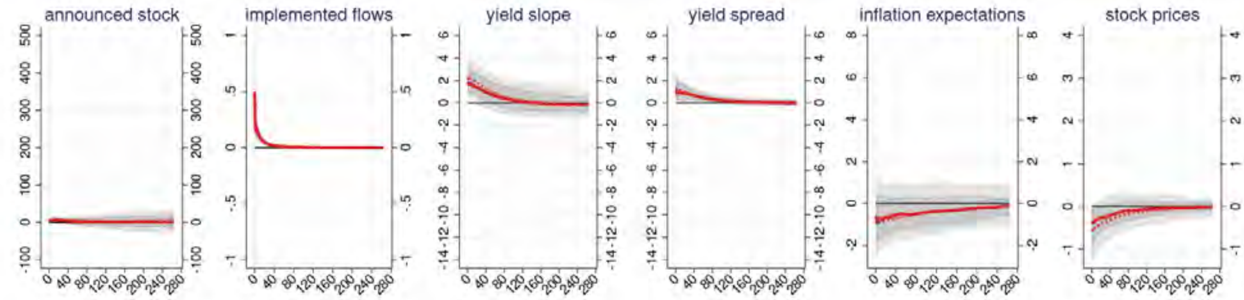
(a) Responses to announcement shocks



(b) Responses to implementation shocks



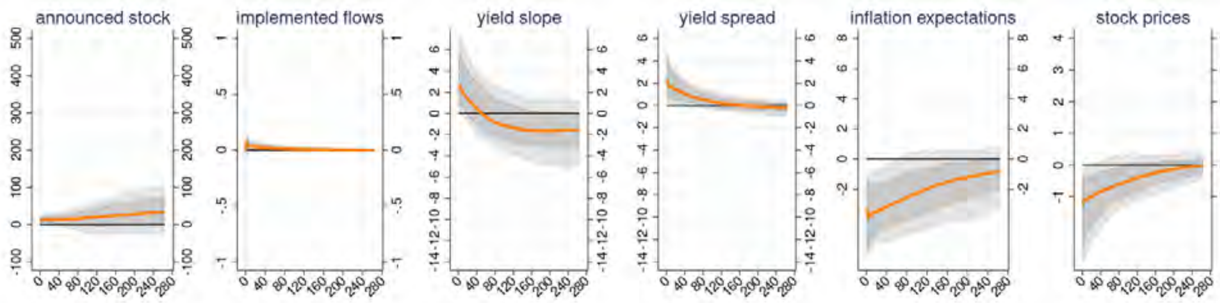
(c) Responses to shocks exerting a financial shock (within-day reaction of actual purchases)



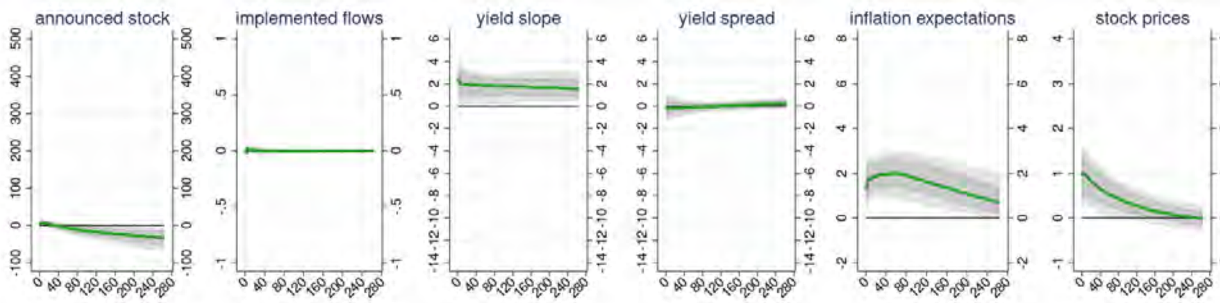
Note. The figure shows the dynamic effects of announcement, implementation and within-day financial shocks. Solid lines and the dark (light) grey bands denote, respectively, posterior medians and 68% (90%) credible intervals. The horizon is the number of working days after the shock. Responses are normalized so that the median response of the announced stock (implemented flows) to an announcement (implementation) shock increases on impact by €400 bn (€0.5 bn), which denotes the typical change observed over the analyzed sample.

Figure C3– Transmission of shocks at longer horizons (extended model) (*cont.*)

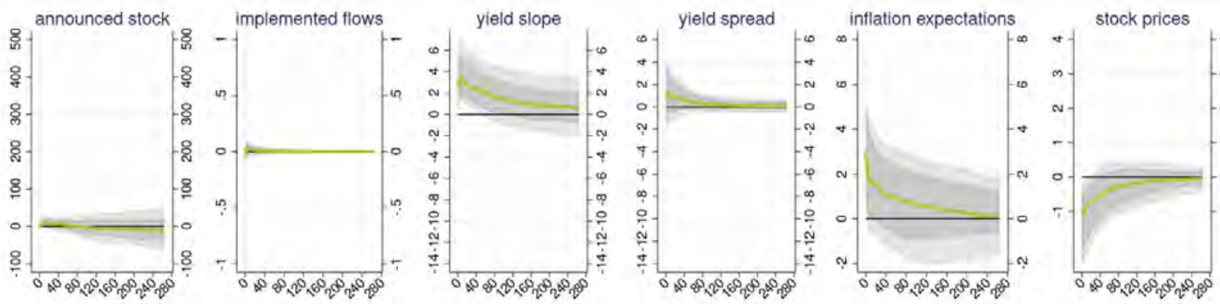
(d) Responses to a financial shock (lagged reaction of actual purchases)



(e) Responses to an aggregate demand shock



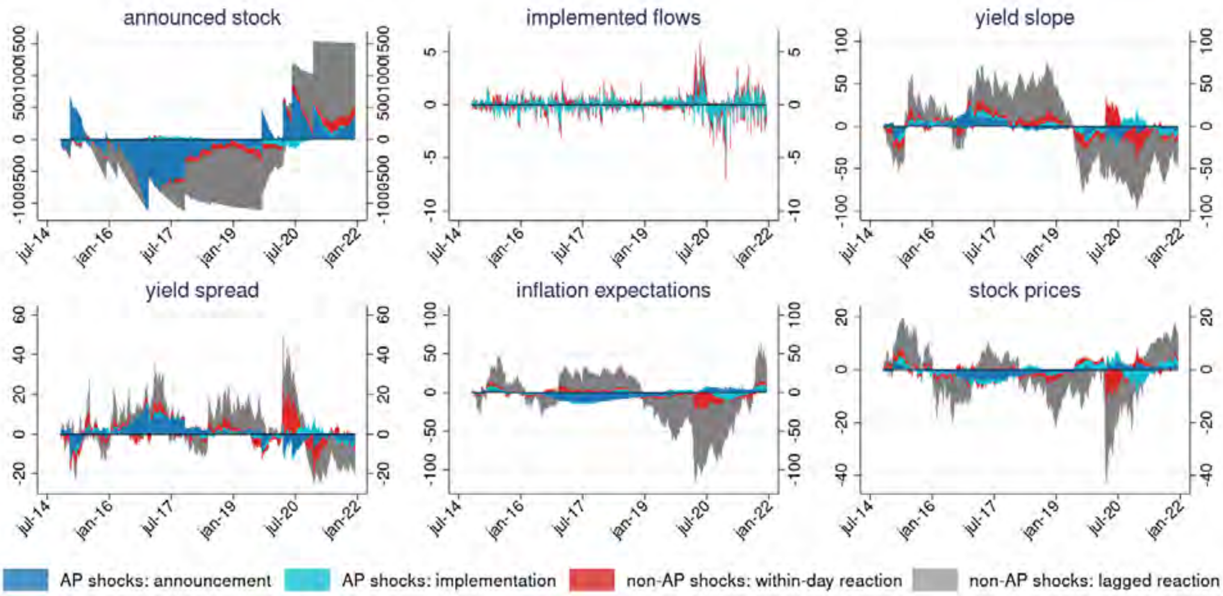
(f) Responses to an aggregate supply shock



Note. The figure shows the dynamic effects of lagged financial, demand and supply shocks. Solid lines and the dark (light) grey bands denote, respectively, posterior medians and 68% (90%) credible intervals. The horizon is the number of working days after the shock. Responses are normalized so that the median response depicts the reaction of financial variables to a rise in the yield slope, that is a favorable demand shock and an unfavorable lagged financial/supply shock.

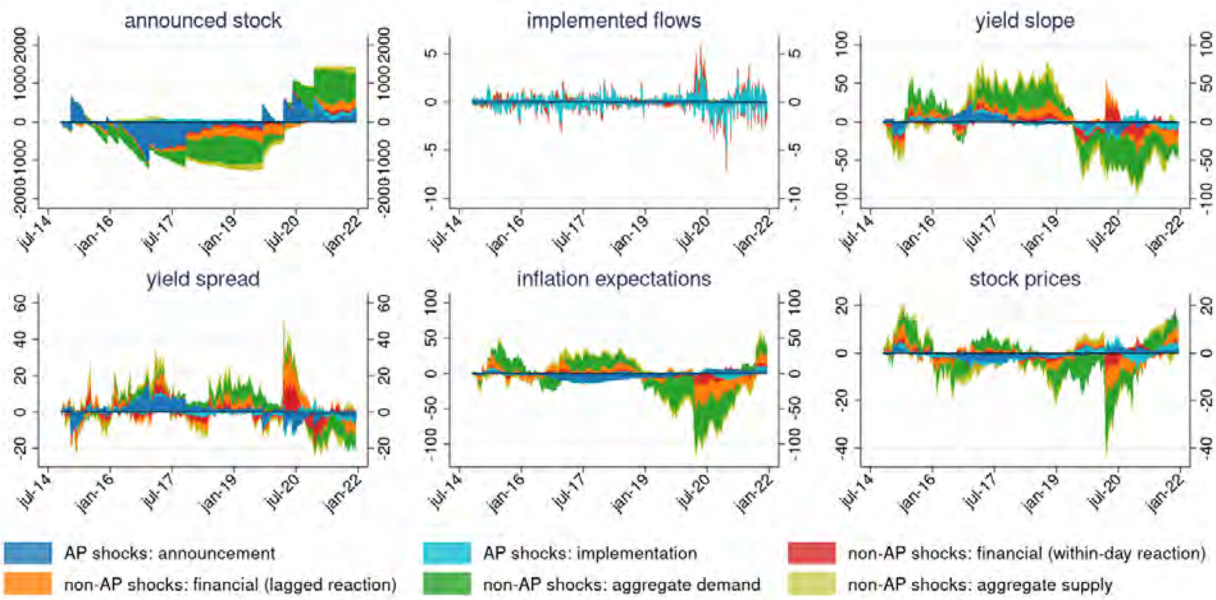
Relevance of shocks

Figure C4 – Full historical decomposition (baseline model)



Note. Historical Decompositions (HDs) under the shock identification summarized in Table 1. Each panel shows the contribution of the identified structural shocks to the unexpected change in each variable, i.e. the difference between the actual dynamics and its steady-state component (i.e., the unconditional forecast produced by the BVAR model).

Figure C5 – Full historical decomposition (extended model)

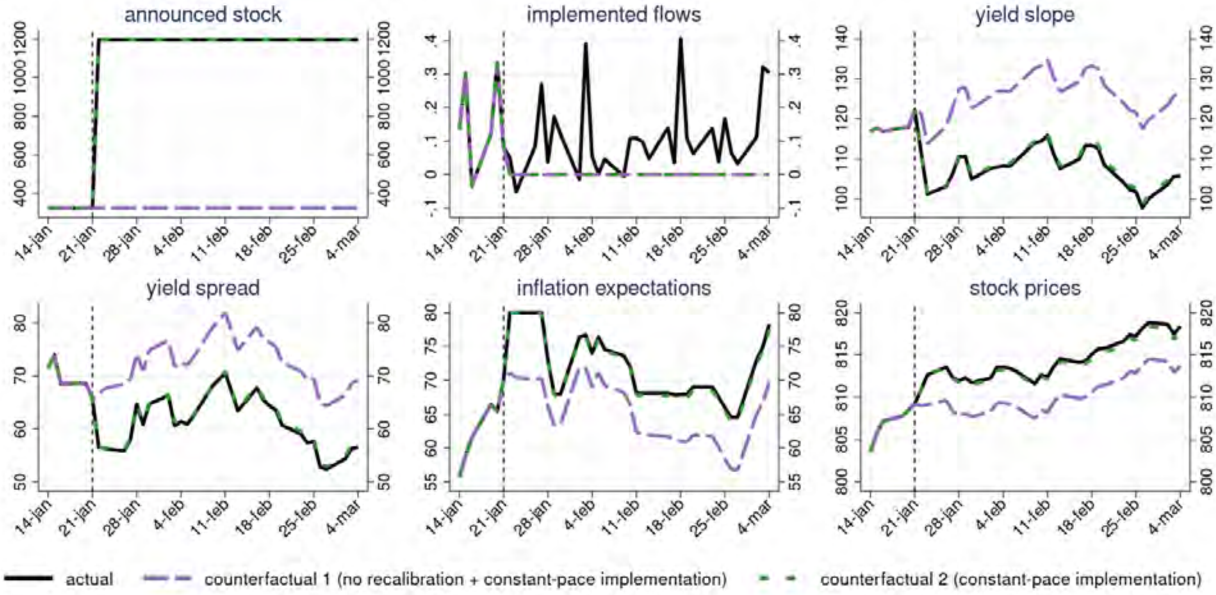


Note. Historical Decompositions (HDs) under the shock identification summarized in Table 2 (extended model). Each panel shows the contribution of the identified structural shocks to the unexpected change in each variable, i.e. the difference between the actual dynamics and its steady-state component (i.e., the unconditional forecast produced by the BVAR model).

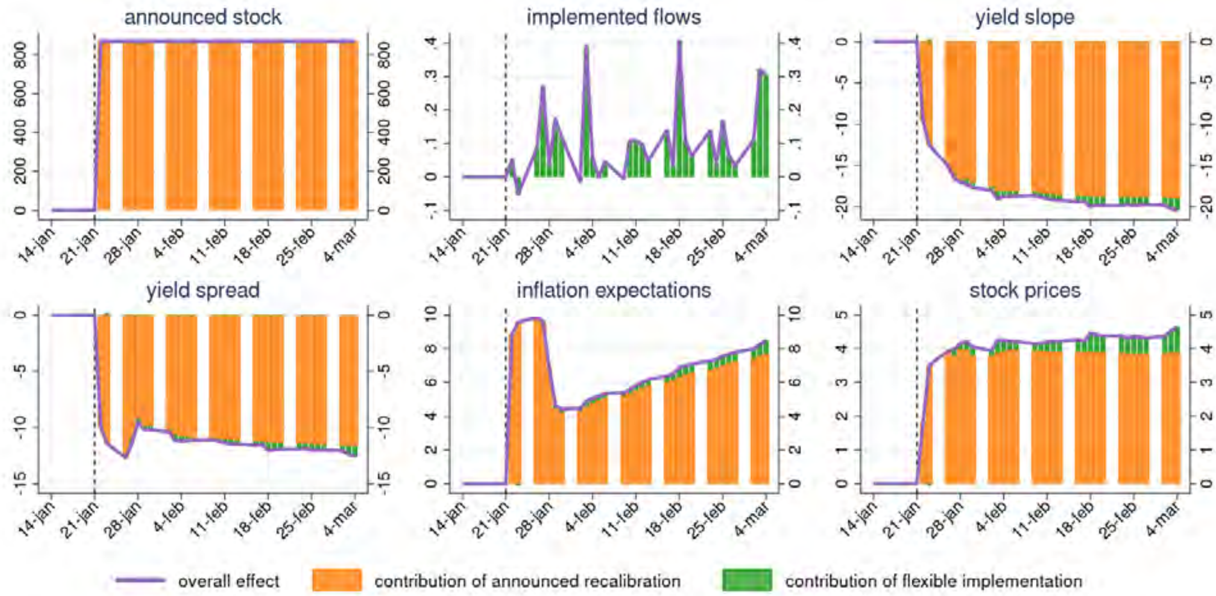
Policy counterfactuals

Figure C6 – Effects of APs in the aftermath of the PSPP announcement using the 2014-19 subsample (*cont.*)

(a) Actual and counterfactuals paths



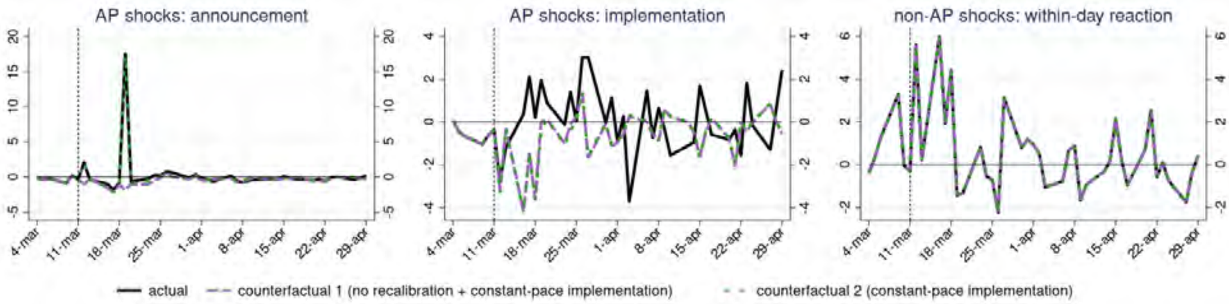
(b) Stock-flow decomposition



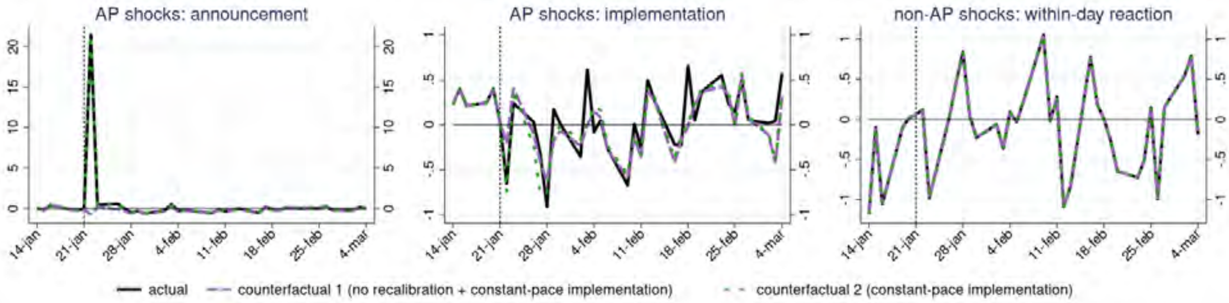
Note. The results are obtained estimating the model until December 31, 2019. Panel (a) shows the actual and counterfactual paths of all the endogenous variables included in the model. All the paths coincide until the day before the announcement made on 22 January 2015 (marked by the vertical dashed line). *Counterfactual #1* assumes that the ECB neither announced a recalibration of its APs nor operated in flexibility. *Counterfactual #2* assumes that the ECB announced the recalibration, as actually occurred, but implemented them at a constant monthly pace. Panel (b) shows the resulting effects. *Overall effect* is the difference between the actual path and the counterfactual path #1. *Contribution of announced recalibration* is the difference between the actual path and the counterfactual path #2. *Contribution of flexible implementation* is the difference between counterfactual 1# and counterfactual #2. In each plot, the thin dashed line represents January 22, 2015, i.e. the day in which the ECB Governing Council announced the launch of the APP.

Figure C7 – Plausibility of counterfactuals effects

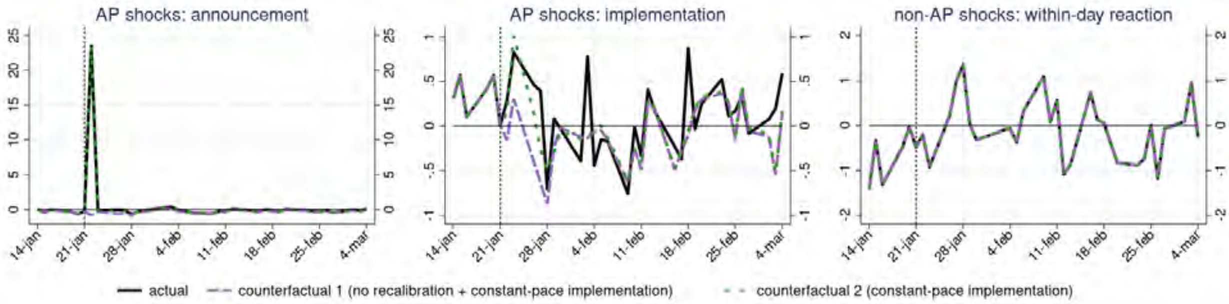
(a) PEPP announcement



(b) PSPP announcement (full sample)



(c) PSPP announcement (2014-19 sample)



Note. The figure shows the actual and counterfactual paths of all the structural shocks estimated by the model. All the paths coincide until the day before the announcements made respectively on January 22, 2015 and March 12, 2020 (marked by the vertical dashed lines). *Counterfactual #1* assumes that the ECB neither announced a recalibration of its APs nor operated in flexibility. *Counterfactual #2* assumes that the ECB announced the recalibration, as actually occurred, but implemented them at a constant monthly pace. By construction, under the counterfactual scenarios only the AP shocks are allowed to differ from the actual ones. In Panel (a) and Panel (b) the results are based on the full sample estimation (i.e. until December 10, 2021), while in Panel (c) results are based on the pre-Covid estimation (i.e. until December 31, 2019).

Table C1 – Effects of APs in the aftermath of the PSPP announcement using the 2014-19 subsample

days after the announcement	announced stock		implemented flows		yield slope		yield spread		expected inflation		stock prices	
	overall effect	flexible impl.	overall effect	flexible impl.	overall effect	flexible impl.	overall effect	flexible impl.	overall effect	flexible impl.	overall effect	flexible impl.
	(€ billion)		(€ billion)		(bp)		(bp)		(bp)		(pp)	
h=1	870.0	0.0	0.1	0.1	-9.6	-0.1	-10.0	-0.1	8.8	0.0	1.8	0.0
	–	–	–	–	*100.0*	*100.0*	*100.0*	*92.5*	*100.0*	*69.2*	*100.0*	*99.1*
h=5	870.0	0.0	0.0	0.0	-17.0	-0.4	-9.5	-0.3	6.9	0.1	4.2	0.2
	–	–	–	–	*99.9*	*93.8*	*100.0*	*97.0*	*98.4*	*68.7*	*99.9*	*99.6*
h=10	870.0	0.0	0.1	0.1	-18.8	-0.7	-11.2	-0.4	5.1	0.3	4.3	0.3
	–	–	–	–	*99.9*	*87.6*	*100.0*	*90.6*	*91.4*	*68.6*	*99.9*	*98.4*
h=20	870.0	0.0	0.4	0.4	-20.0	-1.3	-12.0	-0.7	6.9	0.5	4.5	0.5
	–	–	–	–	*99.8*	*87.1*	*100.0*	*85.3*	*95.5*	*70.3*	*99.9*	*96.7*
h=30	870.0	0.0	0.3	0.3	-20.4	-1.5	-12.6	-0.9	8.5	0.8	4.6	0.8
	–	–	–	–	*99.8*	*81.2*	*100.0*	*85.1*	*96.3*	*70.6*	*99.8*	*96.0*

Note. For each variable and considered horizon (i.e., days after the announcement of a policy recalibration) the Table shows the effect attributable to the overall announcement and the flexible implementation (straight entries), together with the associated probability – based on the posterior distribution of the VAR model – that the counterfactual effect is greater (if positive) or lower (if negative) than zero (*italics* entries). The announcement is made on March 9, 2015. Notice that since the counterfactual paths of the policy variables are imposed, no *associated probability* is reported. All entries refer to the counterfactuals shown in Figure 11b.

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