REVISITING THE DYNAMIC IMPACT OF ASSET PURCHASES: A SURVEY-BASED IDENTIFICATION

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Abstract. We propose a novel instrument for identifying central bank asset purchase shocks in a proxy-VAR. Our instrument exploits the deviations between asset purchase announcements and expectations inferred from quantitative surveys. Using euro-area data, we find a positive impact of purchases on macroeconomic variables with high posterior probability. An asset purchase shock of one percent of GDP leads to median impacts on output and prices of 0.12 and 0.07 percent, respectively. The effects are three times as small as those in the U.S. economy. Finally, we discuss the relevance of our approach compared with that of widely used high-frequency instruments.

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I. Introduction

In the past 15 years, most central banks have conducted asset purchases as part of their unconventional monetary policy, often called "quantitative easing" (QE). In this paper, we propose a new method based on quantitative surveys to identify and estimate the aggregate effects of central bank asset purchases.

Many empirical studies have investigated the dynamic effects of asset purchases by employing vector autoregressions (VARs), using different identification strategies. Early contributions relied on traditional sign and zero restrictions to identify asset purchase shocks (e.g., Gambacorta, Hofmann, and Peersman, 2014; Weale and Wieladek, 2016). However, this method has been criticized, as economic theory rarely justifies such exact restrictions. In response, more recent studies have shifted toward using external instruments for identification, commonly known as proxy-VARs.[1] Building on the methodology developed by Kuttner (2001) and Gürkaynak, Sack, and Swanson (2005), high-frequency (HF) changes in asset prices around policy announcements have typically served as instruments for identifying asset purchase shocks (e.g., Rogers, Scotti, and Wright, 2018; Kim, Laubach, and Wei, 2020; Swanson, 2024).[2] However, the presence of multiple dimensions of policy and multiple transmission channels complicates the task of isolating purely exogenous variations in asset purchases. The HF identification strategy must therefore rely on strong assumptions, such as the pre-selection of variables and the implementation of exclusion and narrative restrictions, to identify the structural policy shock.[3]

We contribute to this literature on identification by proposing a novel instrument that is instead built on quantitative surveys to identify asset purchase shocks and their effects on the economy. In our core empirical exercise, we use Bloomberg and Reuters surveys conducted with market participants ahead of each monetary policy meeting of the European Central Bank (ECB). Since late 2014, these surveys have included questions about market

[1] Stock and Watson (2012) and Mertens and Ravn (2013) pioneered this methodology via a frequentist approach, whereas Caldara and Herbst (2019), Drautzburg (2020), and Arias, Rubio-Ramírez, and Waggoner (2021) introduced Bayesian inference.

[2] See, for example, Gertler and Karadi (2015), Caldara and Herbst (2019), Hachula, Piffer, and Rieth (2020), Jarociński and Karadi (2020), and Lhuissier and Szczerbowicz (2021) for a discussion of the effects of conventional and unconventional monetary policy measures using proxy-VARs and Hartmann and Smets (2018), Pfister and Sahuc (2020) and Rossi (2020) for a recent review.

[3] See, for example, Wright (2019) and Lewis (forthcoming), for a critique. Furthermore, Brennan, Jacobson, Matthes, and Walker (2024) benchmark six different commonly used HF instruments for monetary policy shocks in the U.S. and find that *"different series of HF monetary shocks can have a correlation coefficient as low as 0.5 and the same sign in only two-thirds of observations. Both data and methods drive these differences"*, thus suggesting that some constructions may result in nonrobust inferences.

participants' expectations regarding the future course of asset purchases and, crucially, regarding the amount of expected additional asset purchases to be announced at the upcoming monetary policy meeting. For each policy announcement on asset purchases, we compute the surprise component, as the difference between the additional amount effectively announced and the median additional amount expected by market participants.[4] The resulting series directly determines the size of the unexpected component of each asset purchase announcement without imposing any additional assumptions; in particular, this method does not rely on exclusion and narrative restrictions required in HF identification.

We then use our surprise measure as an instrument within a proxy-VAR framework to trace out the dynamic effects of asset purchases on aggregate activity. In line with the literature, we adopt a Bayesian estimation procedure to address the relatively small sample size inherent to the period of asset purchases, which is nonetheless not materially smaller than those of comparable studies. In our baseline specification, we find that an immediate increase in euro-area asset purchases of one percent of GDP (which is roughly equivalent to Eur 100 billion) leads to positive macroeconomic effects with high posterior probability. At the median, industrial production and consumer prices rise by 0.12 and 0.07 percent, respectively. Turning to financial markets, we find that stimulative asset purchase shocks lower the government bond spread, but these effects die off fairly quickly. The contribution of these shocks to macroeconomic variability appears modest but non-negligible: the shocks account for slightly less than a fifth of the variability in long-run output and consumer prices. The minor historical role of asset purchase policy in generating business cycle fluctuations is mainly due to the fact that much of the observed variations in asset purchases are systematically responsive to the state of the economy. Therefore, we run a number of counterfactual exercises to assess the effects of the systematic component of asset purchases, as opposed to the effects of the unpredictable component of the policy, and show that each major phase of recalibration in the euro area has been instrumental in supporting inflation.

We apply the same identification method to the U.S. economy via QE surprises extracted from the New York Federal Reserve's Survey of Primary Dealers, following D'Amico and Seida (2024). The findings are qualitatively similar but quantitatively different. The estimated

[4] A parallel can be drawn between the construction of our instrument and Rudebusch (1998)'s definition of a monetary policy shock: the difference between the realized policy rate target and the market-based expectation. Our approach has also similarities with the literature on fiscal policy shock identification. Notable examples include Auerbach and Gorodnichenko (2012) and Forni and Gambetti (2016). Both papers make extensive use of surveys of forecasters to better identify fiscal policy shocks. In Auerbach and Gorodnichenko (2012), forecasts help purge fiscal variables of "innovations" that were predicted by forecasters. In Forni and Gambetti (2016), forecast errors (i.e., deviations of actual spending from such expectations) are directly used in the VAR model.

effects on U.S. data are indeed three times as large as those in the euro area, corroborating the results of several previous studies.

In addition to estimating the macroeconomic impact of asset purchases, we compare our approach with that of HF instruments. To operationalize this comparison, we run alternative VARs using well-known HF instruments based on intra-daily changes in Overnight Index Swaps (OIS) rates (Altavilla, Brugnolini, Gürkaynak, Motto, and Ragusa, 2019) and in sovereign bond yields, in line with the literature. We demonstrate that our survey-based instrument is a more valid and stronger instrument than HF instruments in two ways. First, we show that financial press accounts on announcements systematically corroborate our instrument, in contrast to HF instruments. Second, we formally compare both instruments by testing their strengths via standard statistical tests, following Gertler and Karadi (2015). We show that our surprises pass the usual weak instrument tests, whereas the HF instruments do not. This benchmarking exercise demonstrates that survey-based measures contain useful information for identification, in addition to HF asset price changes.

To the best of our knowledge, our paper is the first to exploit surveys on market participants' views of the central bank's asset purchases to construct an instrument for asset purchase shocks in a proxy-VAR. Hesse, Hofmann, and Weber (2018) and Kim, Laubach, and Wei (2020) use similar surveys but to build a time series of 1-year-ahead expected asset purchases, which directly enters into the VAR system as an endogenous variable. Kim, Laubach, and Wei (2020) then instrument the VAR residuals of this variable with intra-daily changes in asset prices around policy announcements for identification. In contrast, instead, we instrument the VAR residuals of asset purchases effectively announced by the ECB with survey-based surprises, which offer advantages over HF surprises, as mentioned above. Our approach is also related to that of Gambetti and Musso (2020), who examine the macroeconomic effects of the initial announcement launching the APP in January 2015. They use Bloomberg surveys to extract the unexpected component of APP announcements, which is inserted into a recursive VAR structure but not as an instrument.

The paper is organized as follows. Section II discusses the identification via external instruments in the proxy-VAR framework. Section III introduces the construction of the proxy for asset purchase shocks used to identify the VAR model. Section IV presents the main results, and Section V compares our approach to alternative HF proxies, tests their relative strengths and discusses their differences. Section VI concludes the paper.

II. The proxy-VAR model

This section outlines the empirical approach that is used to estimate the macroeconomic impact of asset purchase shocks in the euro area. Since the seminal paper by Sims (1980), VAR models have been widely employed to estimate the effects of monetary policy shocks on the economy. Identified VAR modeling allows us to analyze and interpret the data while avoiding potentially "incredible restrictions" on the structure of the economy. In this respect, and following the methodology established by Stock and Watson (2012) and Mertens and Ravn (2013), we propose using an external instrument to identify asset purchase shocks.

We consider that the euro area economy can be described by a VAR with the following general form

$$\mathbf{y}_t'\mathbf{A}_0 = \sum_{\ell=1}^{p} \mathbf{y}_{t-\ell}'\mathbf{A}_\ell + \mathbf{c} + \boldsymbol{\varepsilon}_t' \text{ for } 1 \leq t \leq T, \tag{1}$$

where $\mathbf{y}_t$ is an $n \times 1$ vector of endogenous variables, $\boldsymbol{\varepsilon}_t$ is an $n \times 1$ vector of exogenous structural shocks, $\mathbf{A}_\ell$ is an $n \times n$ matrix of parameters for $0 \leq \ell \leq p$ with $\mathbf{A}_0$ invertible, $\mathbf{c}$ is a $1 \times n$ vector of parameters, p is the lag length, and T is the sample size. The vector $\boldsymbol{\varepsilon}_t$, conditional on past information and the initial conditions $\mathbf{y}_0, ..., \mathbf{y}_{1-p}$, is Gaussian with mean zero and covariance matrix $\mathbf{I}_n$, the $n \times n$ identity matrix. The model described in equation (1) can be compactly written as

$$\mathbf{y}_t'\mathbf{A}_0 = \mathbf{x}_t'\mathbf{A}_+ + \boldsymbol{\varepsilon}_t' \text{ for } 1 \leq t \leq T, \tag{2}$$

where $\mathbf{A}_+' = \begin{bmatrix}\mathbf{A}_1' & \cdots & \mathbf{A}_p' & \mathbf{c}'\end{bmatrix}$ and $\mathbf{x}_t' = \begin{bmatrix}\mathbf{y}_{t-1}' & \cdots & \mathbf{y}_{t-p}' & 1\end{bmatrix}$ for $1 \leq t \leq T$. The dimension of $\mathbf{A}_+$ is $m \times n$, where $m = np + 1$.

The reduced-form representation implied by equation (2) is

$$\mathbf{y}_t' = \mathbf{x}_t'\mathbf{B} + \mathbf{u}_t' \text{ for } 1 \leq t \leq T,$$

where $\mathbf{B} = \mathbf{A}_+\mathbf{A}_0^{-1}$, $\mathbf{u}_t' = \boldsymbol{\varepsilon}_t'\mathbf{A}_0^{-1}$, and $\mathbb{E}[\mathbf{u}_t\mathbf{u}_t'] = \boldsymbol{\Sigma} = (\mathbf{A}_0\mathbf{A}_0')^{-1}$. The matrices $\mathbf{B}$ and $\boldsymbol{\Sigma}$ are the reduced-form parameters, while $\mathbf{A}_0$ and $\mathbf{A}_+$ are the structural parameters.

In the benchmark specification, the vector of endogenous variables $\mathbf{y}_t$ consists of five monthly euro area variables: the logarithm of industrial production (ip_t); the logarithm of the Harmonized Index of Consumer Prices, HICP (p_t); the De Santis (2018a,b)'s excess bond premium, EBP (ebp_t); the spread between the 10-year euro area government bond yields of the four largest euro area countries (Germany, France, Italy, and Spain) and the 10-year OIS rate (sp_t); and the cumulative amount of asset purchases announced scaled by the annualized 2014 euro area GDP (b_t).

All variables are monthly time series covering November 2014 through December 2019.[5] Our main sources for the data are the ECB Data Portal and Bloomberg. The asset purchase

[5]Our dataset does not cover the COVID-19 pandemic period because of unprecedented variation in our macroeconomic variables, severely distorting parameter estimates. See, for example, Lenza and Primiceri (2020) for further details. Our sample size aligns with other empirical studies examining the macroeconomic effects of asset purchases for other countries. For instance, Gambacorta, Hofmann, and Peersman (2014) use

announcement series (Figure 1) is constructed from ECB official announcements in the Weale and Wieladek (2016)'s manner: asset purchase announcements are simply cumulated over time, i.e., it reflects the total purchases announced. Regarding our financial variables, we take the 10-year bond yield-OIS spread, widely used by the ECB in assessing asset purchases (e.g., Coeuré, 2017) and also justified by our focus on asset purchases: retrenching the OIS rate controls for the expected path of short-term interest rates which is more directly connected to other instruments like forward guidance. Our second financial variable is a euro area excess bond premium (De Santis, 2018a,b) computed similarly to Gilchrist and Zakrajsek (2012). The series is the credit spread component capturing the investors' appetite for bearing corporate debt exposure, correcting from default risk. Put differently, it measures the compensation investors ask for investing in the corporate debt market above the simple compensation they require for default risk. The series is computed from secondary market prices of senior unsecured bonds, purged from observable determinants at firm and bond levels, like default risk, maturity, etc. Favara, Gilchrist, Lewis, and Zakrajšek (2016) show that the excess bond premium is a good predictor of economic downturns. More details about data are presented in the online Appendix. Based on a standard lag-length selection criterion (BIC), our proxy-VAR is estimated over the sample period January 2015—December 2019, so that the data allow the inclusion of a two-period lag.[6]

Our structural approach requires the identification of the coefficients of $\mathbf{A}_0^{-1}$. Since we only study the effects of asset purchases shocks, only the coefficients of the fifth column of $\mathbf{A}_0^{-1}$ have to be identified. Our approach to identify asset purchases is based on the use of an external instrument, z_t, along the lines of Stock and Watson (2012) and Mertens and Ravn (2013). Both studies show how the external instrument can be used to identify structural shocks. Specifically, the identification must satisfy several critical assumptions in order to identify movements in the policy indicator that are due to purely exogenous monetary policy disturbances. In particular, the instrument must be correlated with asset purchases shocks ε_t^p but uncorrelated with all other structural shocks $\varepsilon_t^{\neq p}$. This assumption can be summarized as follows

$$\mathrm{E}\left[z_t \varepsilon_t^p\right] = \psi, \tag{3}$$

$$\mathrm{E}\left[z_t \varepsilon_t^{\neq p}\right] = 0. \tag{4}$$

monthly data from January 2008 to June 2011, Weale and Wieladek (2016) use monthly data from March 2009 to May 2014, Hesse, Hofmann, and Weber (2018) use data from November 2008 to October 2014.

[6]The small lag length corroborates other monthly VAR studies that report a lag length of 2 when studying the period of unconventional monetary policy. See, for example, Gambacorta, Hofmann, and Peersman (2014) and Weale and Wieladek (2016).

Given the small sample size, we adopt a Bayesian approach along the lines of Arias, Rubio-Ramírez, and Waggoner (2021) to estimate our proxy-VAR. The authors have developed an efficient algorithm to independently draw from the posterior distribution over the structural parameterization of a proxy-VAR conditional on exogeneity restrictions as shown in equations (3) and (4). The prior and posterior distributions of the VAR belong to the normal-generalized-normal family. The choice of prior density parameterization implies an uninformative prior and leads to prior densities that are equivalent to those in Uhlig (2005). For more details and implementation, we refer to Arias, Rubio-Ramírez, and Waggoner (2021).[7] By adopting a Bayesian approach, we are able to fully characterize the uncertainty of our results by generating draws from the posterior distributions of functions of parameters (such as impulse responses, variance decompositions, etc...). Below, we introduce our novel instrument to identify asset purchases shocks.

III. A proxy for asset purchases shocks

Our focus is on the asset purchase programme (APP) conducted by the ECB since January 2015. Institutional details about the programme are provided in the online Appendix. In this section, we introduce the construction of our proxy, which is the unexpected size of additional asset purchase announcements and recalibrations for the euro area. To infer market expectations over APP announcements, we rely on surveys conducted by Reuters and Bloomberg ahead of each Governing Council.[8] These surveys include questions about the interest rate path, the size, pace, and composition of APP, or the macroeconomic forecasts for the euro area. Both surveys ask participants about their expectations for monetary policy decisions at the next Governing Council. In particular, we use two types of information:

i. the likelihood of an APP announcement at the next Governing Council: e.g., *"Do you expect the ECB to announce QE at its January 22 meeting?"* or *"'(on APP) When Will ECB Make Next Policy Change?'*

ii. the median amount of additional purchases to be announced, e.g., *"What total do you expect the ECB to announce?"*

Questions were often asked in different ways (e.g., a pace + a length of purchases, or a total additional amount) but it is rather straightforward to infer the next Governing Council

[7]Following Arias, Rubio-Ramírez, and Waggoner (2021), we set the truncation parameter at $\gamma = 0.2$.

[8]In the euro area, there is no publicly-available equivalent to the Survey of primary dealers conducted by the New York Fed, used for instance by Cao and Foerster (2013) and Kim, Laubach, and Wei (2020). Both Bloomberg and Reuters survey results are typically published three or four business days before the Governing councils.

expectations in terms of APP. Table 2 details the sources and questions we have exploited at each date.

We preferably use the surveys conducted by Bloomberg, as they gather more respondents (around 60) and are available during the whole period, while Reuters asked direct questions on APP less frequently and on a smaller sample. When the two surveys overlap, we find both are remarkably similar regarding APP expectations. On a few occasions, when the Bloomberg survey is unclear or incomplete, this allows us to complement the answers with the Reuters one. For instance, in March 2016, Bloomberg reported a majority of market participants believed the ECB would *"expand QE purchases above Eur 60 billion a month"*, while in the Reuters survey, market participants were asked to give directly their expectations of monthly amount. The median was 70 billion Eur a month, which is what we use.

We also check the consistency of the expectations with two internal sources: a Banque de France survey (BDF) among market participants since 2017 and the ECB Survey of Monetary Analysts (SMA) conducted by the ECB since April 2019, both confidential and conducted before each Governing Council. Both largely convey the same information and corroborate our measure from Bloomberg and Reuters surveys. In total, this allows us to cross-check our expectation measure using four independent sources.

We then compute a surprise as the difference between the additional purchases effectively announced by the ECB and the median additional purchases expected by market participants.[9] Since there is no Governing Council meeting where the median expectation pointed to an increase in asset purchases while none was announced, we obtain 7 surprises (initial announcement in January 2015 followed by 6 recalibrations).

Figure 2 shows the distribution of additional amount expectations across market participants ahead of 3 major announcement dates. Interestingly, the dispersion of expectations varies considerably: while some (small) recalibrations seem perfectly consensual (e.g. June 2018), the most important recalibrations in terms of amount take place with a larger uncertainty. In September 2019 the 25/75 percentiles of expectations lay between Eur 300 and 500 billion, with min and max between Eur 200 and 700 billion.

In all but one case during our period of interest, computing these surprises is straightforward as the ECB announced a fixed and predetermined additional amount. For instance, in December 2015, the Governing Council announced an extension of the APP by 6 months at

[9] Our preferred measure of surprises considers that sometimes a proportion of respondents expected no announcement at all at the *upcoming* Governing Council. We set their expectations at 0. In the robustness section, we also recompute the surprises using only the answers by the respondents who believed in an announcement at the next Governing Council.

the same pace of Eur 60 billion per month: *"we decided to extend the asset purchase programme (APP). The monthly purchases of Eur 60 billion under the APP are now intended to run until the end of March 2017"* (See Table 1).

However, in September 2019, the ECB announced an open-ended restart of the APP: *"Net purchases will be restarted under the Governing Council's asset purchase programme (APP) at a monthly pace of Eur 20 billion as from 1 November. The Governing Council expects them to run for as long as necessary to reinforce the accommodative impact of its policy rates, and to end shortly before it starts raising the key ECB interest rates."*[10]. This ECB statement conveys two important indications: the APP extension is linked to the horizon of the first hike, and the net asset purchases will stop "shortly before" this happens. In the September polls, the first DFR hike was expected around mid-2022 and "shortly before" was interpreted as around 3 months. In the Bloomberg poll conducted on 16-Oct-2019, at the question *"The ECB says asset purchases will end "shortly" before the first rate increase. How do you define "shortly"?"*, the median answer was 3 months, while the first rate hike was expected around end-2022. As purchases restarted in November 2019, this means 2 months in 2019 + 3*12 months (in 2020, 2021 and 2022) - 3 months ("shortly before"). This implies market participants understood what ECB announced in September 2019 as around Eur 700 billion APP extension (assuming a Eur 20 billion/month pace, which is expected by virtually all the poll respondents), while they expected Eur 384 billion (See Table 2). To cross-check, we also use the OIS curve on the days just before the September 2019 Governing Council to derive the horizon of the next 10 basis points rate hike priced by the swap market. We take all Eur OIS contracts with standard maturities (3,6,9,12 months and 2 to 10 years) and build the OIS curve using the Nelson-Siegel procedure in order to obtain a precise data point at each date in the future. The OIS curve just before the September 2019 Governing Council (Figure C.1 in the online Appendix) suggests one or two further cuts and a first rate hike in January 2023, perfectly consistent with the survey. We also look at how much did the market participants rescale their expectations over the total size of the APP between September 2019 (before the Governing Council) and October 2019 (next survey) using internal sources (SMA and BDF described above). This information broadly corroborates the magnitude of our back-of-the-envelope estimation.

Figure 3 shows our proxy: surprises in Eur amount scaled by the euro area nominal GDP, that we lag as of 2014 – before any discussion on APP. The value of the proxy is positive when the ECB announces a bigger extension in APP purchases than expected by the market. The average surprise is 1.9 % of GDP. The first surprise in January 2015 and the last one in September 2019 are by far the most important, at 6.1% and 4.9%, respectively.

[10] https://www.ecb.europa.eu/press/pr/date/2019/html/ecb.mp190912~08de50b4d2.en.html

Interestingly, there is always a large majority of market participants on the timing according to which actual APP decisions are announced: for each of the 7 APP decisions, all have been predicted by at least 60% of respondents (and 4 by at least 80%). In addition, the median expected additional purchases outside of Governing Councils that effectively announced a recalibration is never different from zero. There is no example of a significant proportion of respondents expecting an APP decision at a Governing Council which turned out to be a non-event, even though there is not always a consensus. Put differently, this excludes negative surprises occurring because market participants expected a decision when none was taken.[11] This suggests market participants have inferred correctly the timing of APP announcements, probably because most decisions are prefigured via different communication means – e.g., Governing Council members interviews, speeches, past Governing Council accounts, etc. On the contrary, we show and exploit the fact the announced size was not perfectly anticipated. Note also that in our particular exercise we are primarily interested in the upcoming Governing Council, and it can be the case that market participants do not expect a decision to be taken at the next policy meeting but in the same time start to build additional purchases expectations ahead of a future announcement date.

It may be worth mentioning the fact that many observations are censored to zero and that the number of respondents in quantitative surveys is limited is not an issue with our proxy-VAR approach because it allows to explicitly account for possible measurement errors in the construction of the proxy for asset purchases shocks. Indeed, our proxy-VAR methodology is robust to many types of measurement problems, as explained in Mertens and Ravn (2013). As long as conditions (3)-(4) hold, the precise nature of the measurement error does not affect the identification of the impulse responses.

Finally, we check the narrative with external sources for all our surprises to further substantiate our approach. To ensure, for instance, that what we compute as negative surprises correspond to the market sentiment, we check how the ECB decision has been reported in major economic newspapers. In all cases, the press corroborates the sign of our surprises, as seen in Table 3.

IV. Main Results

This section presents the main results. First, Section IV.1 presents the time series of asset purchase shocks. Second, Section IV.2 reports the impulse responses of asset purchase shocks. Third, Section IV.3 assesses the quantitative importance of asset purchase shocks through a variance decomposition. Fourth, to establish the contribution of asset purchase shocks to business cycle fluctuations over time, Section IV.4 displays the historical decomposition.

[11] Note also that no APP decision has been made outside of a Governing Council during our sample period.

Fifth, Section IV.5 conducts counterfactual exercises to assess the role of asset purchases as a source of business cycle fluctuations. Sixth, Section IV.6 applies our methodology to US data. Seventh, Section IV.7 conducts some robustness checks.

IV.1. **Identified asset purchases shocks.** Before describing the dynamic effects of APP shocks on the economy, we provide an empirical interpretation of the evolution of these disturbances over time. Figure 4 displays the time series of APP shocks from January 2015 to December 2019. The black line reports the median, while the blue areas report the 68% and 90% probability intervals. A positive value means an expansionary APP shock.

As can be seen, APP shocks capture remarkably well the dates of the introduction and recalibrations of APP, notably January 2015, December 2017 and September 2019. The largest of the historical positive APP shocks occurred in January 2015 when ECB announced the introduction of APP to further ease the monetary policy stance.[12]

Overall, this pattern is in line with the corresponding instrument, as shown by the high correlation between the instrument and the identified shock in Figure 5. The posterior distribution of correlation, which is relatively tight, has the entire mass far from zero with a median value equal to 0.67, thus reflecting the relevance of our APP instrument.[13]

In the spirit of Forni and Gambetti (2014), we perform a test of "structuralness" of the estimated APP shocks to verify that the VAR contains sufficient information to identify the structural APP shocks. We do so by projecting the structural APP shocks onto the factors summarizing the information content of a large set of information available ahead of each Governing Council. The results, reported in the online Appendix, indicate that our structural shocks are not predictable based on past information available, and thus the structuralness is accepted.[14]

IV.2. **Impulse Response Analysis.** The macroeconomic impact of an asset purchase shock is traced out in Figure 6. The median is reported in solid black line, and the 68% and the

[12]We checked formally the persistence of shocks by using autocorrelation function (ACF) with a confidence interval. Clearly, the assumption of independence of residuals holds.

[13]Following Gertler and Karadi (2015), we have also regressed the residuals of the fifth equation ("asset purchases" equation) of the baseline VAR equation on our proxy and we have computed the robust F statistic to check that a weak instrument problem is not present. Clearly, our econometric test confirms the validity of our proxy.

[14]By contrast, Andrade and Ferroni (2020) and Miranda-Agrippino and Ricco (2021) emphasize that HF monetary policy surprises are predictable using information available at the time of monetary policy decisions.

90% error bands in blue areas.[15] The size of an APP shock is scaled to induce an immediate increase in asset purchases of 1% of GDP.[16]

After the initial rise, asset purchases stay persistently above the level expected prior to the shock, gradually converging to the pre-shock expected level in the longer run. Most importantly, the rise in asset purchases provides a substantial short-run output and price stimulus. Both variables immediately rise and then steadily return to their pre-shock levels. The maximum impact is 0.12 percent on industrial production and 0.07 percent on prices. Interestingly, the response of prices appears much more persistent, which is also a pattern observed in the effects of conventional monetary policy shocks. Regarding the uncertainty surrounding those estimates, the response of prices is positive with high posterior probability (both at 68% and 90%) for the first 20 months after the shock. The output response is slightly less precisely estimated; while the corresponding 68% posterior probability band lies within the positive region, there is a small part of the 90% posterior probability mass that implies a zero response.

The estimated proxy-VAR suggests similar price effects of APP shocks than those found in previous studies for the euro area, including Garcia Pascual and Wieladek (2016) and Gambetti and Musso (2020). For example, Garcia Pascual and Wieladek (2016), using VARs with four different identification schemes, find that the peak price effect is between 0.05 and 0.10 percent after an APP shock of 1% of GDP. While we employ industrial production as a proxy for output, the two previous studies use real GDP, making it difficult to compare output effects.

Our proxy-VAR suggests much lower effects than the existing literature on U.S. and U.K. asset purchases. In the U.S., Weale and Wieladek (2016), Hesse, Hofmann, and Weber (2018), and Kim, Laubach, and Wei (2020) find an asset purchases shock equivalent to 1% of GDP leads to a rise in industrial production by about 0.58, 0.20 and 0.68 percent, respectively. For the U.K., the peak output effect is about 0.20 and 0.25 percent according to Hesse, Hofmann, and Weber (2018) and Weale and Wieladek (2016), respectively.[17] Thus, the effect is roughly 1.5 to 5 times smaller than in the U.S. and the U.K. For consumer prices, our estimated model also suggests lower effects than U.S. and U.K estimates. For the U.S., Hesse, Hofmann, and Weber (2018), Weale and Wieladek (2016), and Kim, Laubach, and

[15] Our approach to generate the posterior distribution of impulse responses is as follows. First, we generate draws from the posterior distribution of VAR parameters using Arias, Rubio-Ramírez, and Waggoner (2021). Second, for each draw, we compute impulse responses and then gather all of them to compute sample statistics.

[16] 1% of the euro area GDP is roughly equivalent to a Eur 100 billion.

[17] Kim, Laubach, and Wei (2020) report the effects of a "asset purchases" shock equivalent to 2.5% of nominal GDP. To make the comparison possible, we standardized it to a shock equivalent to 1% of GDP.

Wei (2020) estimate a maximum impact of an asset purchases shock equivalent to 1% of GDP on prices of about 0.20, 0.62, and 0.16 percent, respectively, while for the U.K., Hesse, Hofmann, and Weber (2018) and Weale and Wieladek (2016) document a rise of about 0.20 and 0.32 percent, respectively. In Section IV.6, we apply our methodology to U.S. economy and confirm that the effects are indeed higher than those for the euro area. We also discuss potential underlying factors that may account for these differences.

Turning to financial variables, the excess bond premium declines on impact roughly 2 basis points and then remains below its pre-level shock for about one year. Recall that the excess bond premium is a component of corporate bond credit spreads that is not directly attributable to expected default risk. As argued by Gilchrist and Zakrajsek (2012), it provides a measure of investors' sentiment and risk appetite in the corporate bond market. Its sizeable decrease signals a positive effect on corporates' financial conditions. Interestingly, the effect is far more persistent (lasting more than 1 year) than for the 10-year sovereign rates (see below), which means that APP has a persistent effect on private market conditions on top and not only related to the fall of risk-free rates.

Our 10-year bond-OIS spread falls immediately about 1.5 basis points. This effect fades in about one month and is typically lower than other estimates using event study methodology. Andrade, Breckenfelder, De Fiore, Karadi, and Tristani (2016) compile a selection of event studies on the APP announcement, which give an impact on the 10-year yield between -2.45 and -5.8 basis points (27–64 basis points for a 11% GDP shock). The impact is typically lower on the 10-year rate than in the US or UK. Altavilla, Carboni, and Motto (2021) among others attribute this difference to the financial conditions at the start of APP, noting financial stress was already low in January 2015, and *"various yields and spreads [were] already compressed"*.

A back-of-the-envelope estimation suggests the January 2015 announcement (around 10% of GDP) lowers the 10-year spread by around -15 basis points based on our results, which is not far from what Altavilla, Carboni, and Motto (2021) find in their controlled event study on the January 2015 announcement: -17 basis points on a 1-day change in the 10-year Bund yield and -29 basis points in the euro area average 10-year yield. They also find that this immediate effect starts to fade from the day after by looking at 2-day windows. By contrast, Eser, Lemke, Nyholm, Radde, and Vladu (2019) using a calibrated term-structure model with a supply factor, find an impact of the January 2015 announcement on the 10-year term premium (average of 4 biggest euro area countries) of about -50 basis points, with long-persisting effects.

These differences may come first by our data frequency. With monthly data, we capture a 10-year yield compression persistency rather than relying uniquely on the day of the announcement. Second, our 10-year spread controls for the evolution of the OIS curve, meaning that potential effects coming purely from a signaling channel are, in principle, taken out. This is an advantage of our measure as these signaling effects can be confounded with other instruments like the forward guidance strategy.

Finally, we study the dynamic effects of asset purchases on a range of macroeconomic and financial variables to shed further light on the transmission mechanisms of a such policy. First, we analyze the impact of APP shocks on long-term government bond yields and find evidence consistent with the portfolio balance channel, as policy shocks exert a negative effect on yields at longer maturities. Second, we assess the response of financial market uncertainty and observe a short-term, sharp decline in the VSTOXX index — a proxy for overall economic uncertainty in Europe — suggesting that asset purchases also operate through the uncertainty channel. Lastly, additional evidence from market-implied inflation expectations and ECB macroeconomic projections supports the view that the expectations channel has played a significant role. All related results are presented in the online Appendix.

IV.3. **Variance Decomposition.** Using variance decomposition, we now assess the relative importance of APP shocks in driving fluctuations in endogenous variables. Table 4 reports the percentage of the variances of the error made (at the median) in forecasting each endogenous variable due to APP shocks at forecasting horizons between the first (1M) and the forty-eighth months (48M) after the initial shock. The 68 percent error bands are indicated in brackets.

Variance decomposition shows that the contribution of disturbances to asset purchases to business cycle fluctuations is modest but nonnegligible. These shocks explain about 13 (21) percent of long-run output (prices) variability. APP shocks account for about 20 percent of long-run fluctuations in the excess bond premium and for about 12 percent in the 10-year spread. The majority of their fluctuations are, therefore, caused by non-policy innovations, in line with the conventional wisdom that monetary policy contributes little to business cycle fluctuations. Note also that posterior uncertainty surrounding these estimates is large.

Interestingly, long-run variation in asset purchases is dominated by sources of variation other than policy. APP shocks explain less than half of its variation in the long run. This implies most APP actions have historically been systematic reactions to the state of the economy.

IV.4. **Historical Decomposition.** This section looks at the cumulative role played by the estimated APP shocks in driving the variables of the model. Figure 7 shows the historical decomposition of variables with respect to the impact of the APP shock. In particular, we suppress APP shocks throughout the sample in order to quantify their importance. To do so, we simply set the disturbances to APP to zero. The actual data (solid line) and the median counterfactual paths (dotted line) with the 68% and 90% error bands in blue areas for each endogenous variable are displayed.

The estimated model shows modest but nonnegligible responses of output and prices to APP shocks, and thus rules out from the start a strong explanatory role in macroeconomic movements by APP shocks. The contribution of APP shocks at the median is sometimes positive, sometimes negative, but uncertainty about the estimates remains relatively large, as shown by the 68% and 90% posterior probability bands, which makes the interpretation of the results difficult. Finally, the history of asset purchases is attributed almost entirely to non-policy sources since the counterfactual path closely follows its actual path throughout the sample, meaning that most of the observed variations in asset purchases are systematically responsive to the state of the economy.

IV.5. **Counterfactual Exercises.** Our estimated model implies not only that asset purchase shocks have accounted for little of the historical pattern of business cycles but also that they account for a relatively small proportion of variation in asset purchases. This implies that most ECB actions have historically been systematic reactions to the state of the economy. Assessment of the effects of APP policy, as opposed to the effects of unpredictable changes in policy, must therefore consider what would have happened if the systematic component of APP were different. In this section, we now run a number of counterfactual exercises to assess the role of the systematic part of asset purchases by considering what would have happened if major APP recalibrations had not been implemented. We discuss simulations for the four major APP recalibrations (December 2015 and March 2016 together, December 2016, October 2017 and June 2018 together, and September 2019).

The main conclusion is that the estimated APP policy changes have successfully boosted output and prices with high posterior probability. The positive effects of APP on output turn out to be less precisely estimated, as the 90% probability interval exhibits significant uncertainty. We also quantify, for each counterfactual, how implausible our APP counterfactual scenarios are using the "modesty statistics" developed by Antolín-Díaz, Petrella, and Rubio-Ramírez (2020). For brevity, the results of this section are available in the online Appendix.

IV.6. **US Evidence.** This section analyzes the robustness of our results by applying our methodology to U.S. data. This application deserves particular attention because the sample size of our analysis is relatively short, which can raise some doubts about the robustness of the results. We show that the estimation on U.S. data produces qualitatively similar but quantitatively different results. The estimated effects are indeed much larger in the US economy. This corroborates the results of a number of previous studies.

In the VAR specification, the vector of endogenous variables $\mathbf{y}_t$ consists of five monthly US variables: the logarithm of industrial production; the logarithm of Consumer Prices Index (CPI) ; the Gilchrist and Zakrajsek (2012)'s excess bond premium, EBP; the Adrian, Crump, and Moench (2013)'s 10-year term premium; and the cumulative amount of asset purchases announced scaled by the annualized 2009 US GDP. We follow Weale and Wieladek (2016) to construct the asset purchase series. Asset purchases associated with the maturity extension program (Operation Twist) are treated as additional asset purchases, attaching the same weight to them as asset purchase announcements of government bonds funded through the issue of central bank reserves. All variables are monthly time series covering January 2009 through December 2015.

Similarly to euro area, our U.S. instrument represents the unexpected component of the asset purchase announcements, that is the amount of asset purchases that was not correctly anticipated by market participants. To do so, we rely mostly on the work of D'Amico and Seida (2024) to measure asset purchase surprises. The authors use the Survey of Primary Dealers (SPD) compiled by the New York Fed before each Federal Open Market Committee (FOMC) meeting to extract market participants' expectations. These expectations are then subtracted from asset purchases effectively announced by FOMC to derive the unexpected component. According to D'Amico and Seida (2024), the surprises associated with the first large-scale asset purchase (LSAP1) announcement (March 18, 2009), the first maturity extension program (MEP1) announcement (September 21, 2011), and the MEP2 announcement (June 20, 2012) are equal to \$142.5 bn, \$146.5 bn, and \$174.75 bn, respectively. Regarding LSAP2 (November 3, 2010), we exploit the Bloomberg survey conducted ahead FOMC meeting. Survey shows that the Fed will probably Start \$500 bn of bond buys. Since FOMC announced \$600 bn, the unexpected component amounts to \$100 bn. For more details and the computation of each surprise, we refer to D'Amico and Seida (2024).

Figure 8 shows that an asset purchases shock provides a substantial output and price stimulus in the short-run. Both variables immediately rise and then steadily return to their pre-shock levels. The maximum impact is 0.35 percent on industrial production and 0.20 percent on prices, which is more than three times as large as in the euro area. Turning to financial variables, the excess bond premium declines and reaches its minimum at −5 basis points after 5 months. The 10-year term premium falls immediately but its effects fade in about one month.

We are not the first to report lower asset purchases effects in the euro area compared to the U.S. (see Section IV.2). For example, Garcia Pascual and Wieladek (2016) find *"The effect* *is roughly 2/3 times smaller than in the UK/US"* and establish asset purchases had limited impact on financial market uncertainty and lower impact on stock markets in the euro area in comparison to the U.S. Relatedly, it is worth noting that asset purchases have been launched in the U.S. and in the euro area in very different contexts, QE1 in the US *"at the height of the 2008-09 global financial crises"* while in the euro area APP was launched in 2015 in a deflationary environment but relatively stable and well-functioning financial markets. Finally, structural differences between the US and the euro area economies in combination with major differences in terms of asset classes targeted by the central bank likely affect the effect and transmission of asset purchases. Heterogeneity in the set of institutional sectors directly benefiting from purchases has also distributional consequences on wealth effects stemming from QE. In the US, QE rounds with MBS purchases are associated with a larger effect on lending than QE round targeting Treasuries only (Rodnyansky and Darmouni, 2017). By the same token, a plausible factor is that the US also differ by a greater reliance on financial markets, while the euro area largely relies on banking intermediation.

IV.7. **Robustness.** In order to assess the robustness of our results, we studied a number of alternative specifications. First, we excluded January 2015 from the sample. Second, we excluded the last APP announcement, which was open-ended. Third, we employed an alternative proxy, which is built only from market participants who expect changes in ECB's announcements. Fourth, our structural estimation was carried out by ordering the proxy first in a recursive VAR. Fifth, instead of scaling the size of asset purchases with GDP, we used the volume of free float. Sixth, we purged our proxy from the effects of central bank information shocks that could potentially bias our estimates. Seventh, we scaled the size of asset purchases by the previous calendar year's nominal GDP. All of these alternative specifications confirm our main results. For brevity, the results of this section are available in the online Appendix.

V. Comparison with HF instruments

We now turn to the comparison of our survey-based instrument with HF instruments, which have been widely used to identify conventional and unconventional monetary policy shocks. We first benchmark our instrument with HF instruments, then provide additional evidence that our surprises capture only asset purchases and no other monetary policy instrument. We conclude the section by discussing the sources of the differences between our approach and HF identification.

V.1. **Benchmarking our approach with two HF instruments.** We analyze how our survey-based surprises compare to different HF surprises. Our first HF instrument relies on the methodology developed by Altavilla, Brugnolini, Gürkaynak, Motto, and Ragusa (2019). The authors extract an orthogonalized factor from a set of OIS rates, associated with asset purchases, and call it "QE factor". We replicate their HF factor and extend it through the end of 2019.[18]

As stressed by Wright (2019), APP should not be identified solely from HF movements in the OIS curve but primarily from variations in government bond yields. For this reason, we construct an alternative QE factor, mostly based on adapting Swanson and Jayawickrema (2024)'s methodology to the euro area. This factor represents the change in the 10-year government bond yields, orthogonalized with respect to changes in the target factor and forward guidance (ie, path factor). This identifying assumption is intuitive and is similar to that in Rogers, Scotti, and Wright (2018); it is also simpler than the one in Swanson (2021). The construction of the instrument is as follows. First, we compute HF changes of GDP-weighted average of 10-year government bond yields of Germany, France, Italy and Spain, available in the EA-MPD database.[19] Second, we purge the resulting factor from conventional monetary policy developments. To do so, we regress the factor on the Altavilla, Brugnolini, Gürkaynak, Motto, and Ragusa (2019)'s target and path factors, which capture variations in the short-term policy rate and in forward guidance. The residuals of such a regression become our alternative QE factor.

As shown in Figure 9, movements in survey-based and HF surprises do not always align. For example, the December 2016 recalibration is classified as an expansionary policy surprise using the survey-based approach but as a neutral or restrictive surprise with the HF approach. Notably, survey-based surprises correspond more closely to financial press reactions to ECB decisions (Table 3), suggesting that they offer a more reliable instrument.[20] For instance on the December 2019 announcement, QE factor obtained with HF suggests a negative surprise, while our survey method finds a positive surprise, in line with the financial press (NYT: "*The European Central Bank took unexpectedly aggressive steps on Thursday [...] The measures [...] go beyond what many analysts were expecting.*")

We provide a formal comparison of instruments by testing their strength along the lines of Gertler and Karadi (2015). A strong instrument is an instrument that is sufficiently

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[18]Thanks to the data and code made available in Julia via R. Gürkaynak's website: http://refet.bilkent.edu.tr/research.html

[19]The EA-MPD database is available here: https://www.ecb.europa.eu/pub/pdf/annex/Dataset_EA-MPD.xlsx

[20]A comparison of APP shocks associated with each instrument is provided in the online Appendix. We find that APP shocks identified using HF instruments generate greater uncertainty than those derived from our survey-based instrument. Furthermore, financial press reactions to ECB decisions generally align more closely with APP shocks associated with our instrument.

correlated with the regression residual of a particular policy indicator. In addition to the two HF surprises mentioned previously, we also consider two additional extensions of the alternative QE factor by including speeches. Specifically, the first additional instrument includes, in addition to Governing Council announcements, key ECB President's speeches associated to APP: 04/12/2015, 27/06/2017, 27/03/2019, and 18/06/2019. These dates are drawn from Rostagno, Altavilla, Carboni, Lemke, Motto, and Saint Guilhem (2021). The choice of including key speeches about asset purchases is motivated by the work of Kim, Laubach, and Wei (2020). The second additional instrument includes, in addition to Governing Council announcements, all speeches by the ECB President.[21] This instrument is in line with the recent work of Swanson and Jayawickrema (2024).

Table 5 summarizes the results. The columns considered are regression residuals of a particular policy indicator regressed on each instrument. Columns (1)-(5) use the residuals of the fifth equation of our baseline VAR model such that the policy indicator is the cumulative amount of asset purchases announced by the ECB. Columns (6)-(10) propose an alternative policy indicator, that is the VAR residuals of the 10-year euro area government bond yields, which are more in line with existing HF studies.

Our results show that the best instrument is our survey-based proxy, with an F-statistic of 84.97, well above the threshold value recommended by Stock and Watson (2012). Concerning HF surprises, none of the instrument combinations meet the threshold. That being said, the second best instrument is the alternative QE factor (including all speeches), with a F-statistic of 8.02, below the recommended threshold but still near it. This suggests that while the alternative factor may have some relevance, it falls short of being a strong instrument.

V.2. **Survey-based surprises and forward guidance.** By using surprises only referring to the size of asset purchases, our approach is agnostic about the transmission channels of APP and may capture for instance a signaling channel of asset purchases, that would be in other methodologies be only captured as a forward guidance (FG) shock.

We believe this would not be problematic for inference, unless the magnitude of our surprises is correlated with surprises in terms of announcements on FG made during the same Governing Council, for instance. Based on Rostagno, Altavilla, Carboni, Lemke, Motto, and Saint Guilhem (2021)'s list of the ECB announcements, in 4 out of our 7 APP surprises, an announcement of FG has been made in parallel with recalibrations of APP, in December 2016, October 2017, June 2018 and September 2019.

[21]For both additional instruments, we use the database of Istrefi, Odendahl, and Sestieri (2024) to extract the surprises around speeches.

However, the correlation between our surprises and FG turns out to be statistically insignificant. Table 6 reports the correlation coefficients of our proxy with respectively the target factor, FG factor and QE factor of Altavilla, Brugnolini, Gürkaynak, Motto, and Ragusa (2019). The FG factor is never correlated at a statistical significance of 5%. Even before the orthogonalization of factors, HF changes in medium-term OIS prices — which should be directly affected by FG announcements (5-year and 5-year in 1 month, to control for the next policy meeting expectations) — are not correlated with our instrument.

V.3. **On the usefulness of surveys on top of HF approach.** Our previous exercises suggest that our survey-based measure contains useful information not captured by HF price changes. First, it may be the case that the price changes of financial instruments, specifically, movements in OIS rates, do not fully encompass, or solely, the information regarding a monetary policy surprise in terms of asset purchases. For instance, Wright (2019) suggests that asset purchases cannot solely be identified from HF movements of the OIS curve, but primarily from term premia and intra-euro area sovereign spreads.

Second, OIS price changes may reflect information about the anticipated decisions regarding asset purchases by the central bank and its decisions in future meetings, whereas our proxy only reflects APP surprises at the upcoming policy meeting.

Third, while the first two points apply specifically to the Altavilla, Brugnolini, Gürkaynak, Motto, and Ragusa (2019)'s OIS-based QE factor, a broader issue arises when considering any types of HF factor. Indeed, a key challenge lies in factor extraction for HF instruments, which requires imposing questionable restrictions to disentangle different dimensions of monetary policy (such as the target rate, forward guidance, and asset purchases). In contrast, our survey-based method directly quantifies the magnitude of the unexpected component of each APP announcement and does not contain information about other ECB actions.

VI. Conclusion

In this paper, we assessed the macroeconomic impact of the ECB's asset purchase programme and its recalibrations within a proxy-VAR framework. To identify exogenous asset purchase shocks, we proposed a novel proxy derived from quantitative surveys capturing market participants' expectations ahead of key policy announcements. Specifically, the proxy measures the surprise component of APP announcements, defined as the deviation between the actual purchase amounts communicated by the ECB and the amounts anticipated in the survey responses. Estimating the effects of asset purchase shocks identified according to our methodology shows a positive impact of asset purchases on both output and prices with high posterior probability. We applied the same identification to U.S. economy. Qualitatively, the findings are similar, asset purchases stimulate both output and prices. Quantitatively, the results suggest that the macroeconomic effects are larger in the U.S. economy. Finally, we demonstrated the relevance of our instrument compared to commonly used instruments that depend on HF changes in asset prices.

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Appendix A. Tables

Table 1. APP announcements 2015M1 — 2019M12

Event	Date	Start	End	Length (month)	Add. Pace (/month)	Add. amount (bn Eur)	Cumulated (bn Eur)
Announcement	22/01/2015	Mar-15	Sep-16	19	60	1140	1140
Extension	03/12/2015	Sep-16	Mar-17	6	60	360	1500
Extension	10/03/2016	Apr-16	Mar-17	12	20	240	1740
Extension	08/12/2016	Apr-17	Dec-17	9	60	540	2280
Extension	26/10/2017	Dec-17	Sep-18	9	30	270	2550
Extension	14/06/2018	Sep-18	Dec-18	3	15	45	2595
Re-start	12/09/2019	Nov-19	open-ended	-	20	700*	3315

Source: ECB, Bloomberg. *The September 2019 restart is announced in an "open-ended" way, i.e., the ECB commits only to a monthly size, and gives two indications: net purchases will stop "shortly before" the next interest rate hike. We infer the total size from these parameters, their qualitative interpretation by market participants from surveys and quantitatively from the OIS curve. See section III for the detailed explanation.

Table 2. Market expectations on APP announcement/re-calibration

Dates	Source and comments
22-Jan-2015	We use of a Bloomberg poll conducted on 19-Jan-2015. At the question *"Do you expect the ECB to announce QE at its Jan. 22 meeting ?"*, 93% of respondents answered "Yes", 7% 'No" (over 60 respondents). The median estimate for the total size of purchases was 550 billion Eur.
03-Dec-2015	We use a Bloomberg poll conducted on 30-Nov-2015. 100% of respondents (over 53) expected an announcement at the Dec-2015 GovC of an extension of APP. However information is sparse on the expected additional amount. We use the information contained in the Reuters ECB December pre-meeting poll: the median extension is 6 months to end-March 2017 and the pace of purchases is expected to increase to 75 bn Eur (+6*75 = 450)
10-Mar-2016	We use a Bloomberg poll conducted on 07-Mar-2016. At the question *"what new measures will Draghi announce on March 10?"*, 73% of respondents (over 59) answered *"Expand QE purchases above EUR60b/month"* with a median increase by Eur 15 bn a month. The large majority (72%) expected an unchanged end in March 2017. The Reuters poll gives the median expected (*"What monthly total do you expect the ECB to announce"*) at 70 billion Eur/month. Therefore the median additional purchases were at Eur 120 billion (+10*12 months).
08-Dec-2016	We use the additional questions asked in a Reuters poll "European Central Bank Monetary Policy Poll - December 8, 2016", as the Bloomberg poll is less explicit about the next GovC expectations. 87% (52 of 60 respondents) said *'the ECB will announce on Dec. 8 an extension to its QE programme beyond the current plan of March 2017"*. The median estimate was an extension from Mar-2017 to Sep-2017 at a monthly pace of 80 billion Eur/month, or an additional amount of purchases of 480 billion Eur.
26-Oct-2017	We use a Bloomberg poll conducted on 18-Oct-2017. 98% of the 57 respondents expected a decision at the 26-Oct meeting regarding APP. The median estimate of additional purchases was 300 billion Eur.
14-Jun-2018	We use a Bloomberg poll conducted on 7-Jun-2018. At the question *"When Will ECB Announce QE End Date?"*, 30% of respondents (over 56) answered "June 2018". The median estimates of additional purchases to be announced after Sep-2018 was 45 billion Eur.
12-Sep-2019	We use a Bloomberg poll conducted on 6-Sep-2019. 59% of respondents expected a decision regarding the restart of APP at the Sep. 12 meeting. The median estimate of additional purchases was 12 months at a pace of 32.5 billion Eur, or a cumulative additional amount of 390 billion Eur.

TABLE 3. Selected newspapers' accounts on APP announcement/re-calibrations

Dates	Source and comments
22-Jan-2015	FT: "Mario Draghi's bond-buying plan outstrips expectations" https://www.ft.com/content/8f215db8-a256-11e4-9630-00144feab7de
03-Dec-2015	FT: "the measures seem to have disappointed market participants who were expecting even bolder steps" http://blogs.ft.com/the-world/liveblogs/2015-12-03/. The Guardian: "European stocks slide after ECB dashes hopes of major QE expansion" https://www.theguardian.com/business/2015/dec/03/ecb-launches-new-stimulus-package-eurozone
10-Mar-2016	FT: "The European Central Bank has unleashed a bigger than expected package of measures to stimulate the eurozone economy, [...] The ECB raised the amount of bonds the eurozone's central bankers buy each month under QE from Eur 60bn to Eur 80bn — a greater sum than many analysts had expected." https://www.ft.com/content/9a45a960-e6ac-11e5-a09b-1f8b0d268c39
08-Dec-2016	WSJ: "ECB Extends but Scales Back Stimulus, Whipsawing Markets" https://www.wsj.com/articles/ecb-to-extends-stimulus-program-by-nine-months-at-reduced-rate-1481201978
26-Oct-2017	FT: "Although purchases under its historic quantitative easing programme will from January be cut from the current pace of Eur 60bn a month to Eur 30bn, the scale of the reduction matched the consensus among investors, economists and traders." https://www.ft.com/content/2849d4ec-ba24-11e7-9bfb-4a9c83ffa852
14-Jun-2018	FT: "ECB to phase out Eur 2.4tn bond-buying programme by year end euro falls as markets respond to cautious elements in plan to end QE" https://www.ft.com/content/7514a734-6fbe-11e8-92d3-6c13e5c92914
12-Sep-2019	NYT: "The European Central Bank took unexpectedly aggressive steps on Thursday [...] The measures [...] go beyond what many analysts were expecting. Recent comments by members of the Governing Council had cast doubt on whether the bank would restart purchases of government and corporate bonds." https://www.nytimes.com/2019/09/12/business/ecb-europe-recession-stimulus.html

TABLE 4. Forecast Error Variance Decomposition

Horizon	IP	Prices	EBP	Spread	APP
1M	5.91 [2.69;11.46]	9.71 [3.15;19.69]	17.74 [8.01;31.14]	9.32 [3.02;19.26]	90.84 [84.54;94.82]
6M	8.78 [3.13;19.59]	15.55 [4.70;29.71]	22.63 [10.47;36.21]	9.33 [4.31;16.66]	77.02 [66.40;85.84]
12M	11.69 [3.73;25.88]	17.98 [5.37;35.40]	22.14 [10.22;36.05]	10.36 [5.23;17.88]	64.01 [46.69;76.93]
24M	12.78 [3.72;28.18]	20.33 [5.73;41.89]	21.06 [9.75;35.27]	11.07 [5.62;18.76]	50.07 [30.98;68.01]
36M	12.86 [4.19;28.60]	21.14 [6.01;43.08]	20.60 [9.72;35.20]	11.48 [6.16;19.07]	45.60 [25.48;64.35]
48M	13.41 [4.40;28.93]	21.52 [6.19;43.57]	20.48 [9.55;35.59]	11.71 [6.36;19.74]	43.52 [23.22;63.17]

Note: Fraction of variances (at the median) of each endogenous variable explained by APP shocks at various horizons. The 68 percent probability intervals are indicated in brackets.

Table 5. Tests on the strength of the instruments

	Asset purchases Announcement					10-year government bond yields				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Our survey-based proxy	1.084***					-0.008				
	(0.118)					(0.009)				
Altavilla et al.'s QE factor		0.001					-0.000			
		(0.004)					(0.000)			
Alternative QE factor			-0.619*					0.035**		
			(0.347)					(0.014)		
Alternative QE factor				-0.766**					0.032**	
(*including major APP speeches*)				(0.295)					(0.015)	
Alternative QE factor					-0.778***					0.030*
(*including all speeches*)					(0.275)					(0.017)
Observations	60	60	60	60	60	60	60	60	60	60
Adjusted R-squared	0.51	-0.01	0.14	0.22	0.22	-0.01	0.01	0.05	0.04	0.03
F-statistic	84.97	0.03	3.18	6.72	8.02	0.79	1.89	6.18	4.67	3.28

Note: The sample period is 2015.M01 to 2019:M12. The models estimated are $u_{5,t} = \alpha + \beta z_t + \eta_t$ with $u_{5,t}$ the residual in the 5$^{\text{th}}$ equation of the VAR corresponding to that of asset purchases announcement series for columns (1) to (5), and that of 10-year euro area government bond yields for columns (6)-(10). Proxy variables z_t used are: our survey-based proxy, Altavilla, Brugnolini, Gürkaynak, Motto, and Ragusa (2019)'s QE factor, an alternative QE factor (i.e., changes in the 10-year government bond yields around the time of Governing Council meeting, purged from the target and path factors) and its variants including speeches. Statistical significance is shown for $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors that are robust against heteroskedasticity are given in parentheses.

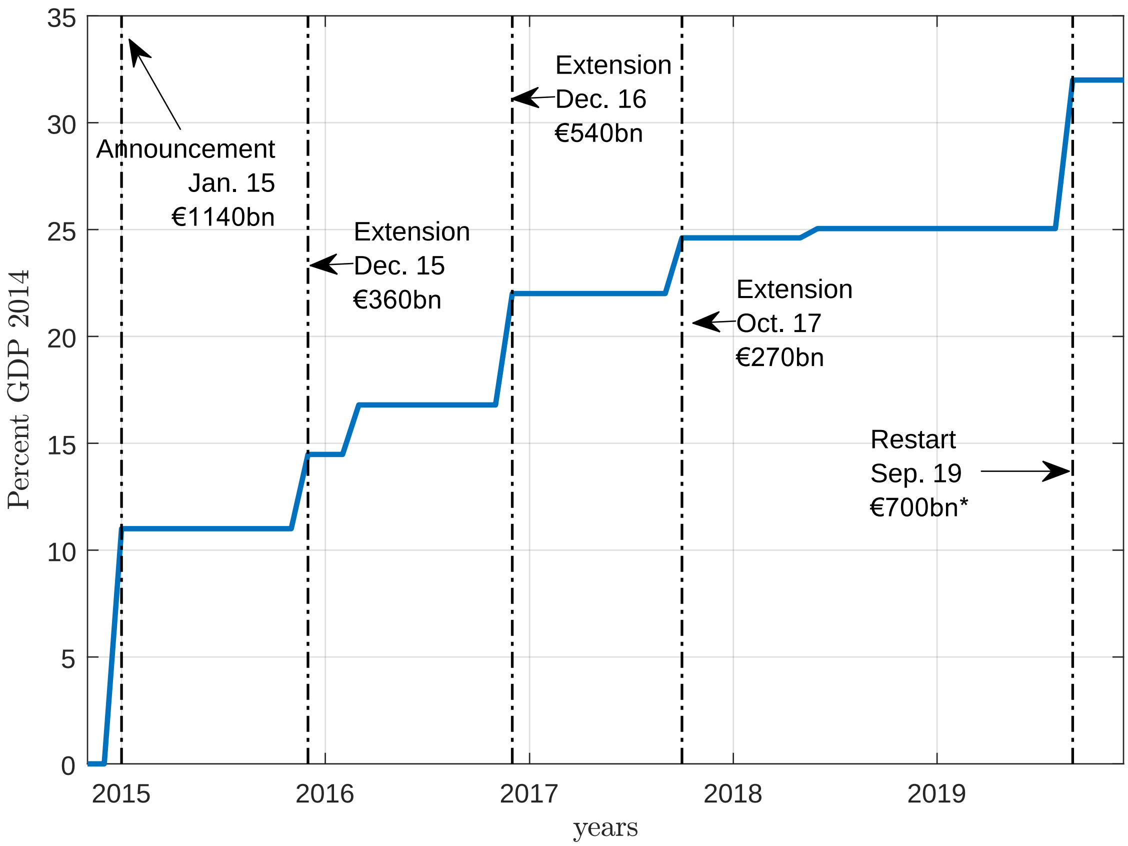
Table 6. OLS Regressions

	Survey-based APP shocks				
	(1)	(2)	(3)	(4)	(5)
Altavilla et al.'s Target Factor	-0.00603				
	(-0.77)				
Altavilla et al.'s FG Factor		-0.0115^{*}			
		(-1.77)			
Altavilla et al.'s QE Factor			0.00230		
			(0.71)		
5-year OIS				-0.000278	
				(-0.28)	
5-year/5-year OIS in 1-month					-0.000282
					(-0.29)
Observations	60	60	60	60	60
Adjusted R-squared	0.014	0.138	0.042	-0.012	-0.012

Note: The sample period is 2015.M01 to 2019:M12. The dependent variable is the time series of APP shocks. The explanatory variables are the three monetary policy factors of Altavilla, Brugnolini, Gürkaynak, Motto, and Ragusa (2019) (target, path (i.e., forward guidance), and QE), the 5-year OIS, and the 5-year/5-year OIS in 1 month (to control for the next policy meeting expectations). Statistical significance is shown for $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors that are robust against heteroskedasticity are given in parentheses. t-statistics in parentheses.

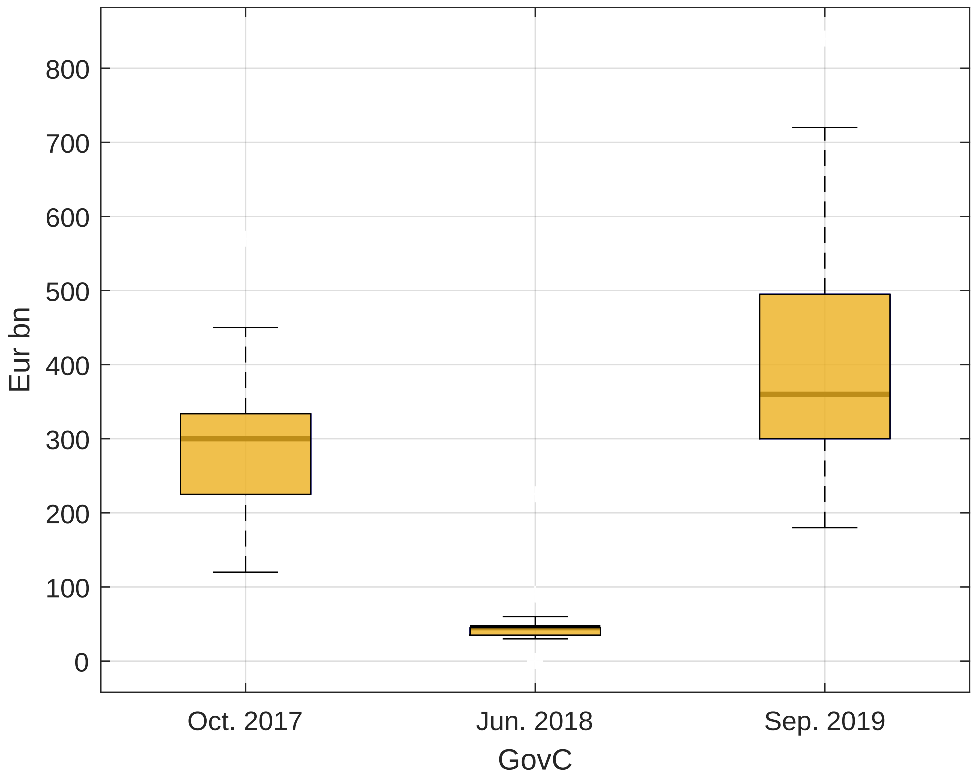
Appendix B. Figures

Figure 1. Total APP size, as announced by the ECB



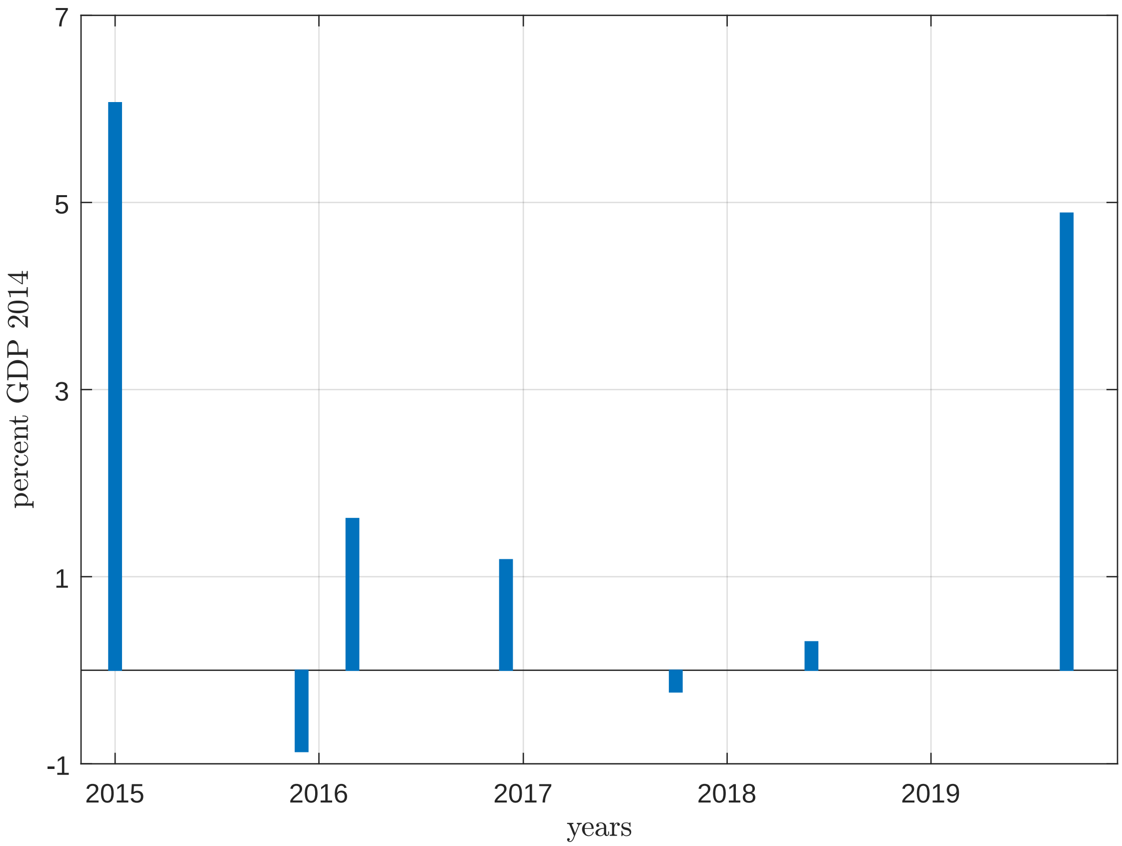
Note: Sample period: 2014.M11 – 2019.M12. * The Sept 2019 restart is announced in an "open-ended" way ie. ECB commits only to a monthly size, and gives two indications: net purchases will stop "shortly before" the next interet rate hike. We infer the total size from these parameters, their qualitative interpretation by market participants from surveys and quantitatively from the OIS curve. See section III for the detailed explanation.
Source: ECB, ECB Statistical Data Warehouse.

Figure 2. Distribution of market expectations on APP recalibrations



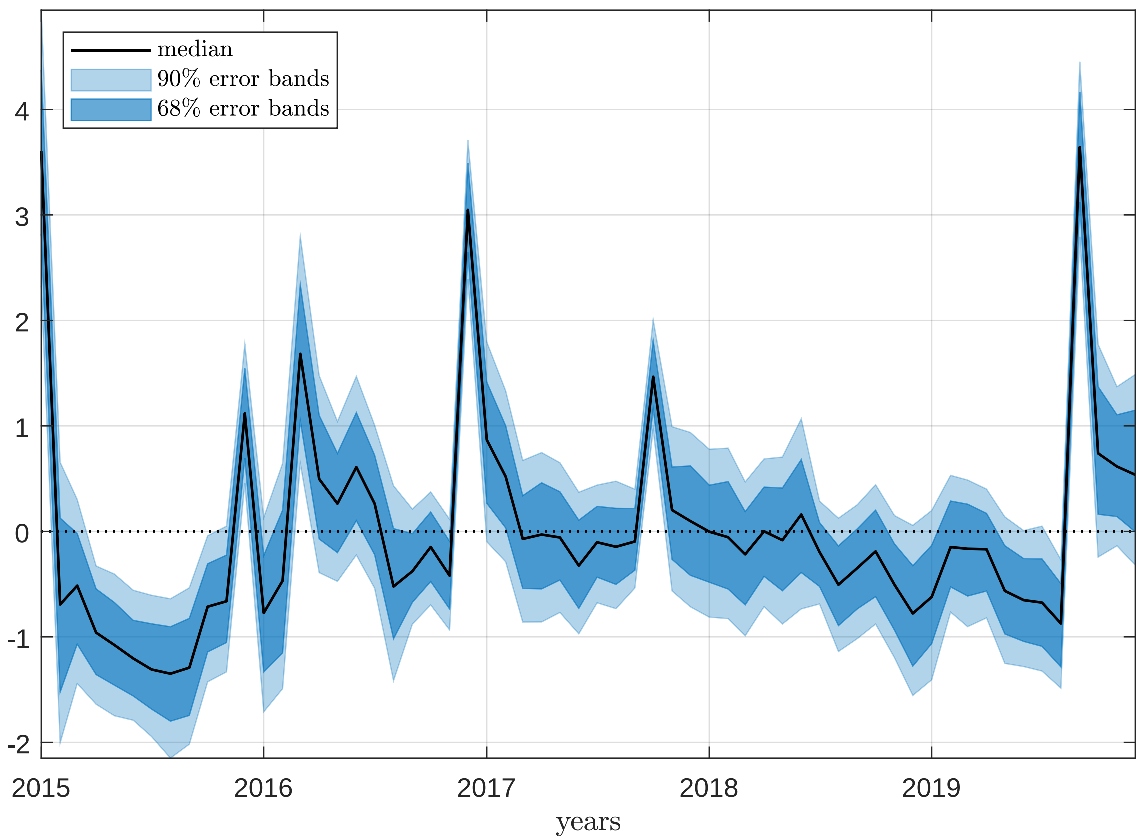
Note: This figure illustrates the distribution of market expectations on the additional APP purchases ahead of three key Governing Councils (GovC). Sources used are described in Table 2, the respondents' distribution come from Bloomberg polls. We take the median value to compute our surprise measure. Boxes represent the 25/75 percentiles and bars min/max once outliers are removed.

Figure 3. Proxy



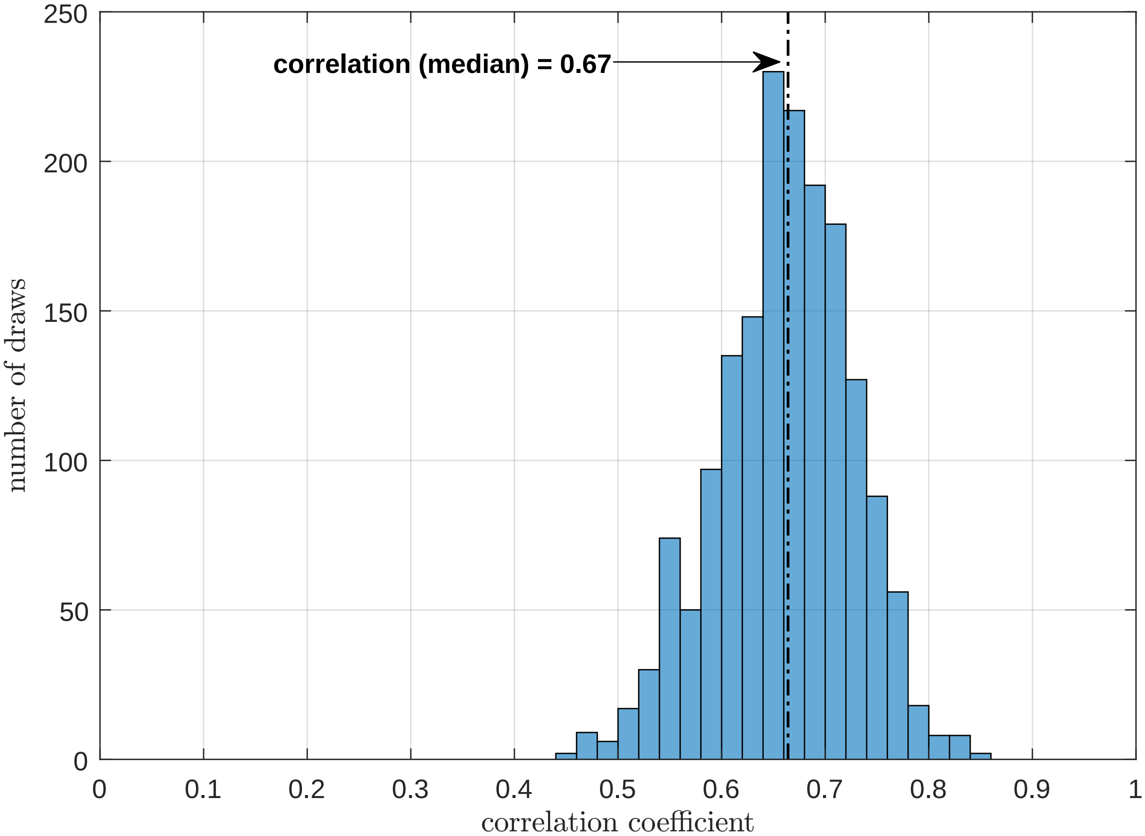
Note: Sample period: 2014.M11 — 2019.M12. Unexpected Components around Governing Council Announcements. Unexpected components are scaled to nominal GDP 2014.

Figure 4. Historical Path of Structural APP Shocks



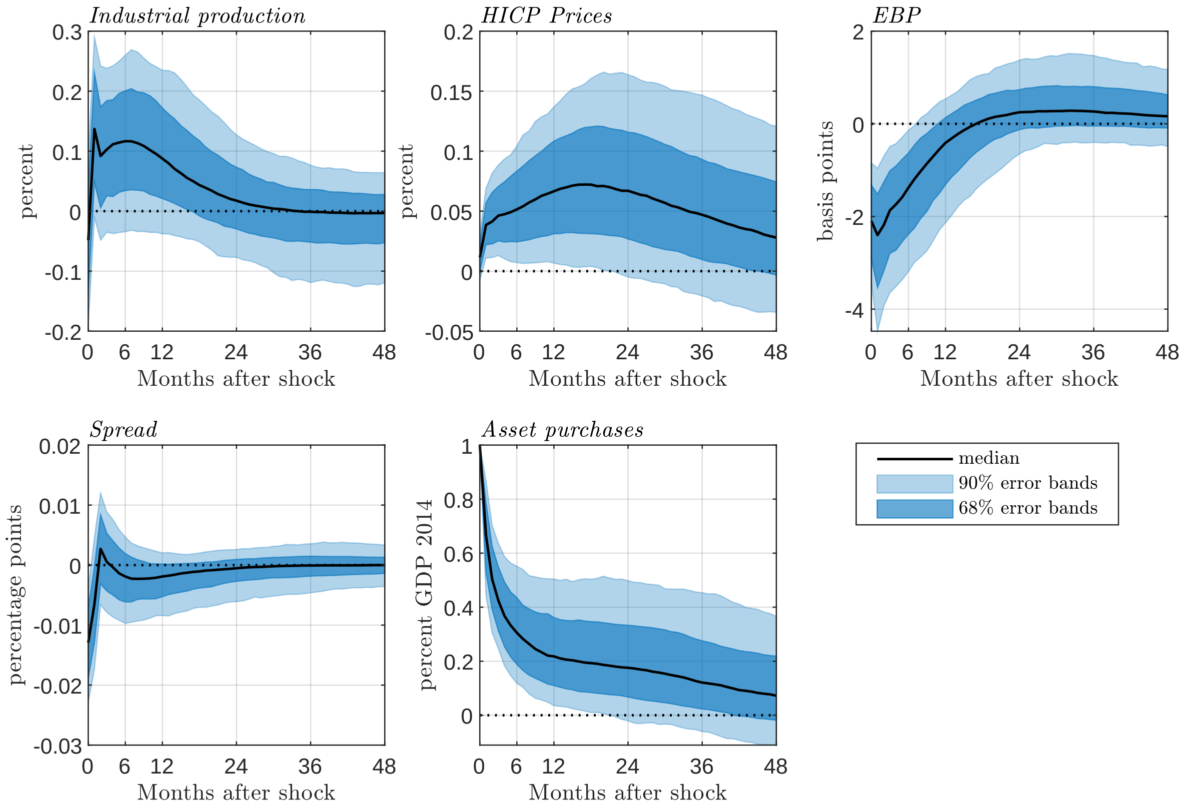
Note: Sample period: 2015.M01 — 2019.M12. Historical path of structural APP shocks (at the median). The blue areas denote 68% and 90% error bands areas.

FIGURE 5. Correlation between Proxy and APP Shock



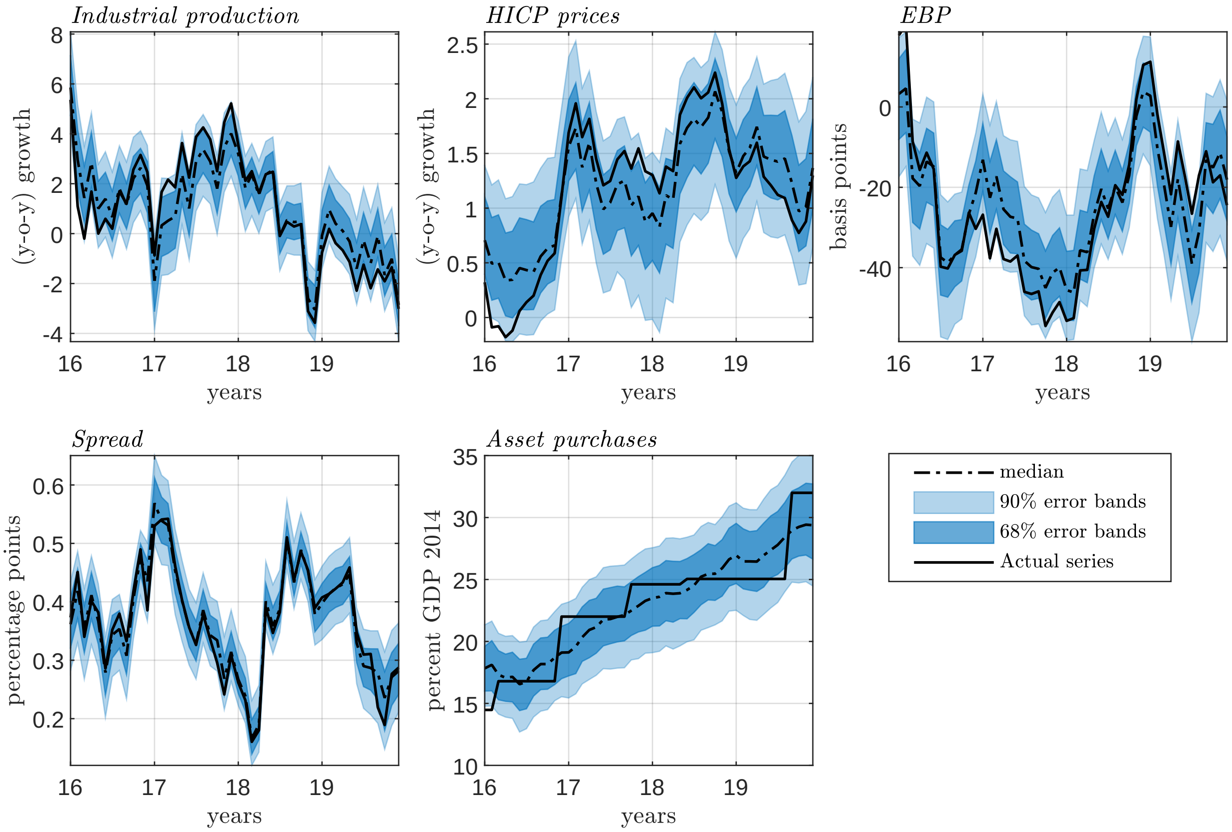
Note: Histogram based on 1805 independent draws generated from the algorithm developed by Arias, Rubio-Ramírez, and Waggoner (2021).

FIGURE 6. Impulse responses to an APP shock



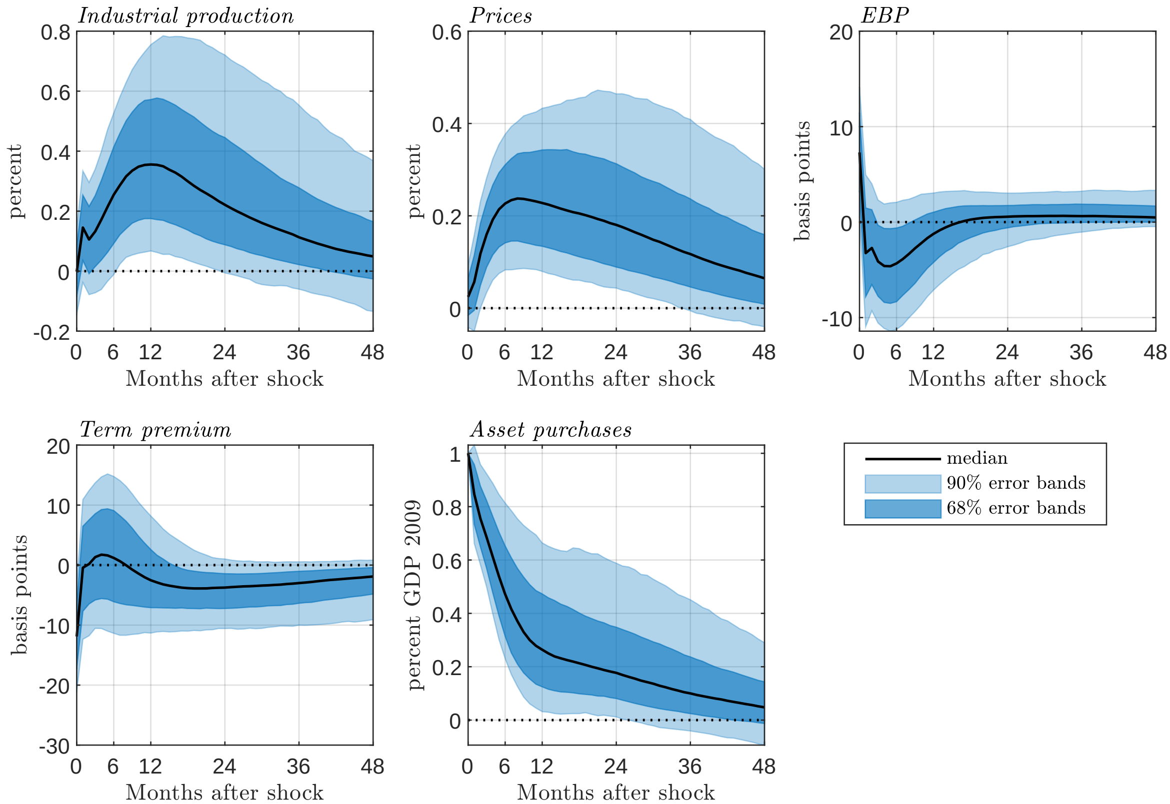
Note: The size of the shock is scaled to induce an immediate increase in asset purchases of 1% of GDP. In each panel, the median is reported in solid line, while the 68% and 90% error bands are shown by blue areas.

FIGURE 7. Suppressing APP shocks



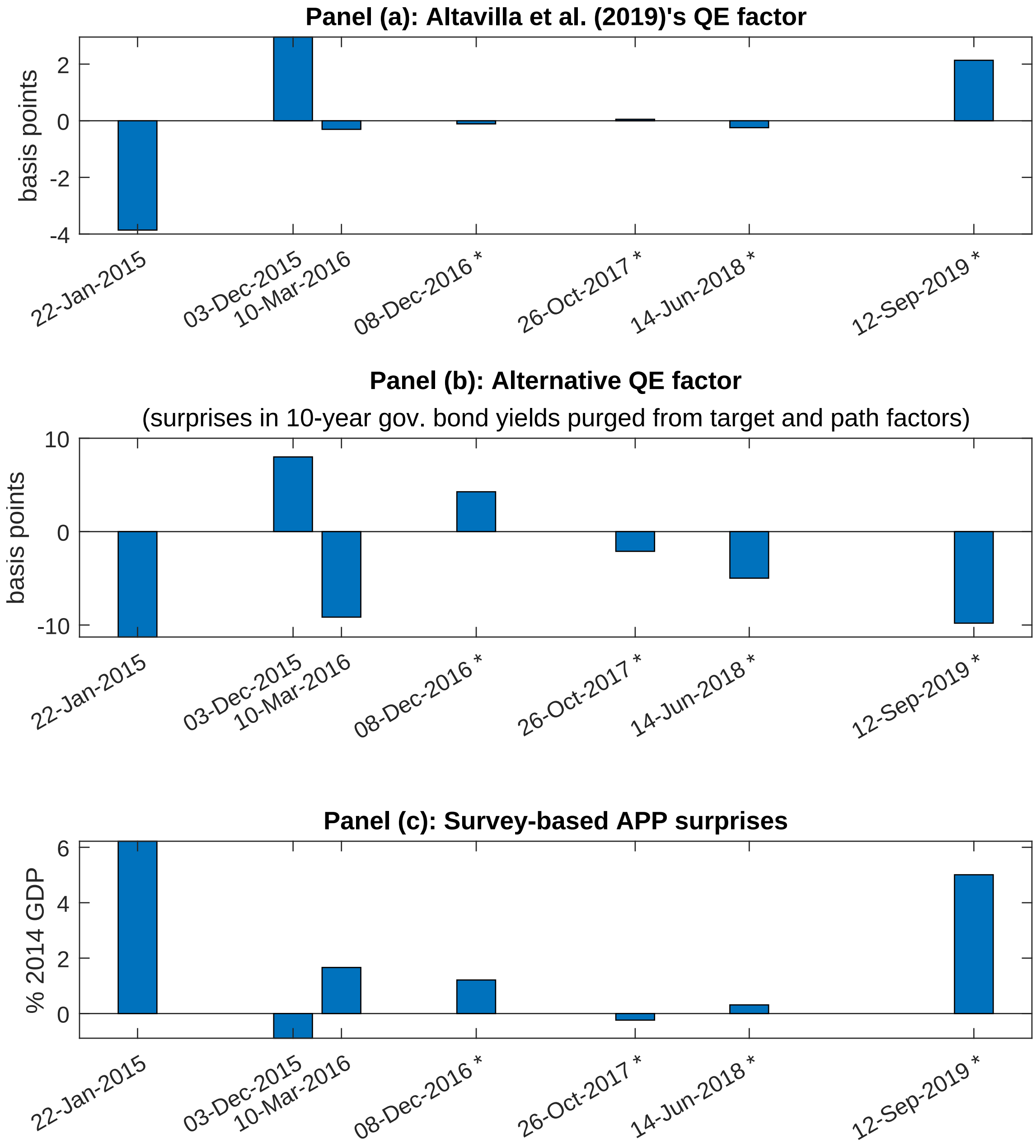
Note: Sample period: 2016.M01 — 2019.M12. Historical decomposition — i.e., suppressing APP shocks throughout the entire period. In each panel, actual and median counterfactual paths of endogenous variables are in black solid and dotted lines, respectively. The blue areas denote the counterfactual's 68% and 90% error bands areas.

FIGURE 8. Impulse responses to an asset purchase shock — US evidence



Note: The size of the shock is scaled to induce an immediate increase in asset purchases of 1% of GDP. In each panel, the median is reported in solid line, while the 68% and 90% error bands are shown by blue areas.

FIGURE 9. Comparison of HF surprises with survey-based surprises



Note: Panel (a) reports the change in OIS rates during the monetary policy window, from Altavilla, Brugnolini, Gürkaynak, Motto, and Ragusa (2019) and using their public EA-MPD dataset. Their QE factor is extracted from the changes in long-term OIS rates. Panel (b) reports changes in 10-year government bond yields around monetary policy windows, purged from target and path factors. Panel (c) reports our APP surprises, extracted from surveys and scaled by 2014 nominal GDP. To convey the same information, the two HF metrics should go in opposite directions from APP surprises. The "*" signals Governing Councils in which an APP recalibration has been announced in parallel with a change in FG, as identified by Rostagno, Altavilla, Carboni, Lemke, Motto, and Saint Guilhem (2021).

ONLINE APPENDIX: REVISITING THE DYNAMIC IMPACT OF ASSET PURCHASES: A SURVEY-BASED IDENTIFICATION

Not for Publication

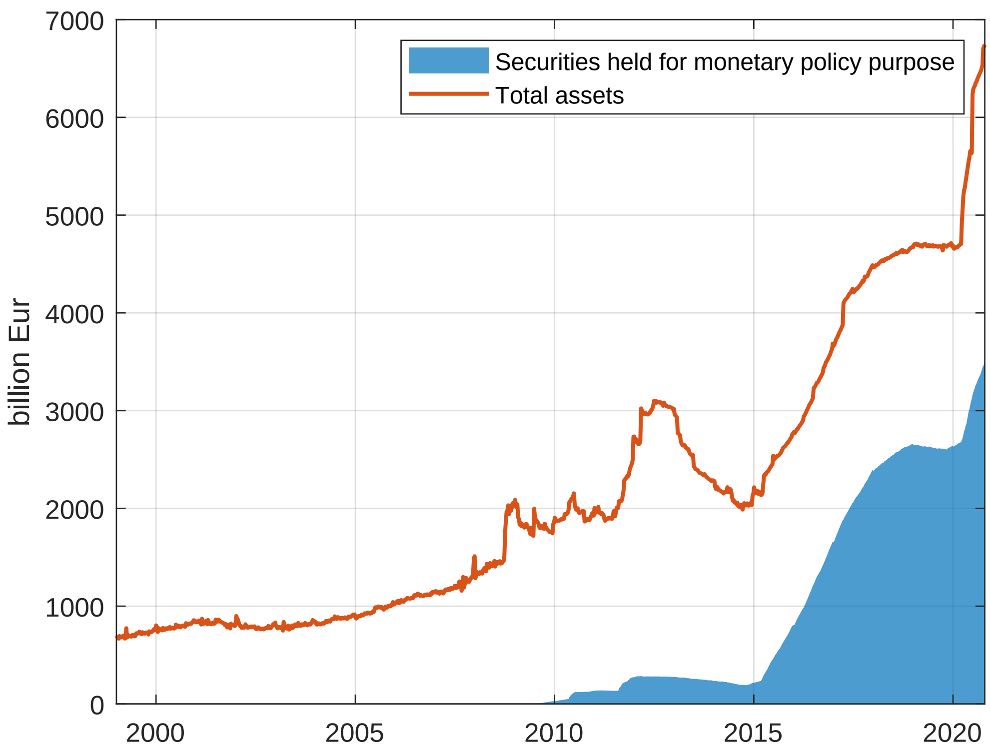
STÉPHANE LHUISSIER AND BENOÎT NGUYEN

This Appendix consists of the following sections:

Appendix A. Institutional details

The ECB launched its extended Asset Purchase Programme (APP) in January 2015 to address the mounting risks of a prolonged period of low inflation. As shown in Figure A.1, the APP has been the main factor driving the expansion of the balance sheet of the ECB. In February 2020, the amount of securities held for monetary policy purposes by the ECB accounted for about Eur 2,700 billion — approximately 25% of the euro area nominal Gross Domestic Product (GDP).

Figure A.1. Size and composition of the Eurosystem's balance sheet



Note: Sample period: Jan 1999 – Oct 2020. Source: ECB Statistical Data Warehouse.

The extended APP was announced on January 22, 2015, for an initial size of Eur 1140 billion. The programme consisted for the first time in large-scale purchases of public securities[22], which completed the existing smaller scale purchases of covered bonds (CBPP) and ABS (ABSPP). In March 2016, the ECB added a programme on corporate bonds (CSPP)

[22]Prior to the extended APP, the ECB ran several smaller-scale purchase programmes, on peripheral debts (SMP 2010-2011) and on specific market segments (covered bonds and ABS).

consisting of the purchase of investment-grade (rated higher than BBB-) bonds issued by non-financial companies.

The APP has been re-calibrated several times in terms of pace and length of purchases. The initial announcement in January 2015 has been followed by 6 recalibrations during our period under review, see Table 1. The first recalibration was announced in December 2015, as a 6 months extension from the initial ending date (September 2016) to the end of March 2017. In March 2016 the monthly pace of purchases was increased from 60 to 80 billion – starting from April 2016 – with an unchanged ending date in March 2017. In December 2016, the Governing council announced a 9-month extension to December 2017 at 60 billion per month. In October 2017 APP purchases were extended until September 2018 at a lower monthly pace of 30 billion. In June 2018, the ECB added 3 additional months at Eur 15 billion per month until the end-December 2018. Finally, in September 2019, the Governing council restarted the APP purchases, from November 2019 at Eur 20 billion per month.

While net purchases stopped between December 2018 and November 2019, the securities purchased by the Eurosystem under its various sub-APP programmes are meant to be held until maturity and redemptions in the monetary policy portfolio are reinvested – for a horizon linked by the ECB to the next policy rate hike – which explains why the ECB balance sheet did not shrink since 2015. In February 2020 – before the Covid-19 crisis triggered additional emergency stimulus – the amount of securities held by the Eurosystem accounted for Eur 2,700 billion, or around 25% of the euro area nominal GDP.

The purchases are intended to be implemented in a *market neutral* way, meaning they are conducted in proportion of the market outstanding (See Hammermann, Leonard, Nardelli, and von Landesberger (2019)). This means, notably, that the weighted average maturity (WAM) of the APP portfolio closely follows the WAM of the market, which can be verified each month on the ECB website.[23] In addition, set aside one technical change in the eligibility criteria (eg. bond residual maturity lowered to 1 year instead of 2 years in Dec 2016), there has been no change in the maturity composition of purchases. As a result, the WAM of the PSPP has been remarkably stable around 7 years. Put differently, by analogy to the metrics used in Eser, Lemke, Nyholm, Radde, and Vladu (2019) this means the ratio of 10-year equivalent of 100 Eur bn additional purchases scaled by 10-year equivalent of the eligible outstanding remains stable over time.

In terms of the asset composition of purchases, the share between public and private securities has been remarkably stable (around 80% in public securities). The purchases under the CSPP merely substitute to those in the small covered and ABS markets.

[23] https://www.ecb.europa.eu/mopo/implement/omt/html/index.en.html#pspp

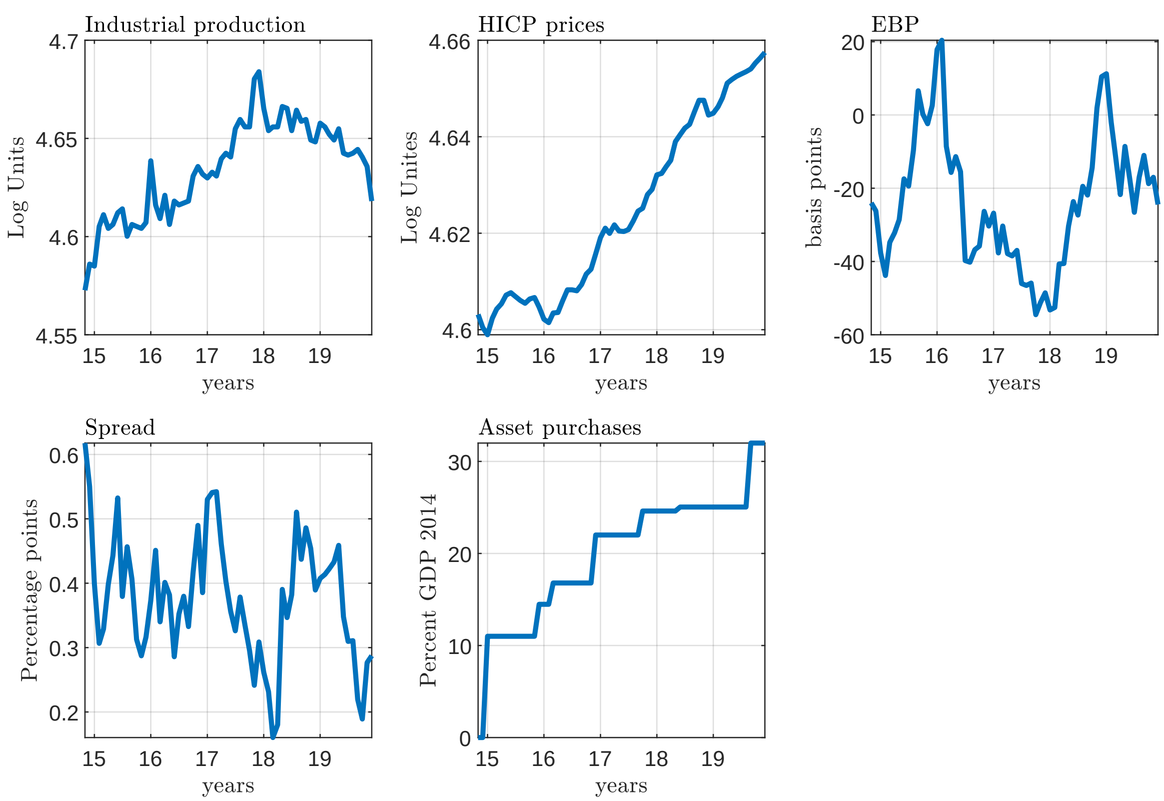
To sum up, for these two reasons, we think the information on the size of APP is an appropriate summary of the degree of accommodation provided by APP in the euro area, while we acknowledge this can be more complex in the U.S. for instance, where the Federal Reserve also actively used as a policy tool the maturity of asset purchases (MEP) or their relative composition between MBS and Treasuries.

Appendix B. Data

All data are organized monthly from November 2014 to December 2019. Data comes from the ECB - Statistical Data Warehouse, Reuters and Bloomberg, except for the excess bond premium which has been generously given by Roberto De Santis. Figure B.1 displays the time series data.

- ip_t: Output is the logarithm of industrial production index, working day and seasonally adjusted.
- p_t: Prices is the logarithm of harmonized index of consumer price (HICP), working day and seasonally adjusted.
- app_t: Total size of asset purchases announced by the ECB, divided by 2014.Q1 GDP. The end-of-month value is used;
- ebp_t: De Santis (2018a,b)'s excess bond premium (regularly updated by Roberto De Santis). The end-of-month value is used;
- sp_t: Difference between GDP-weighted (Big4) 10-year government bond yields and the 10-year overnight index swaps (OIS). The end-of-month value is used;

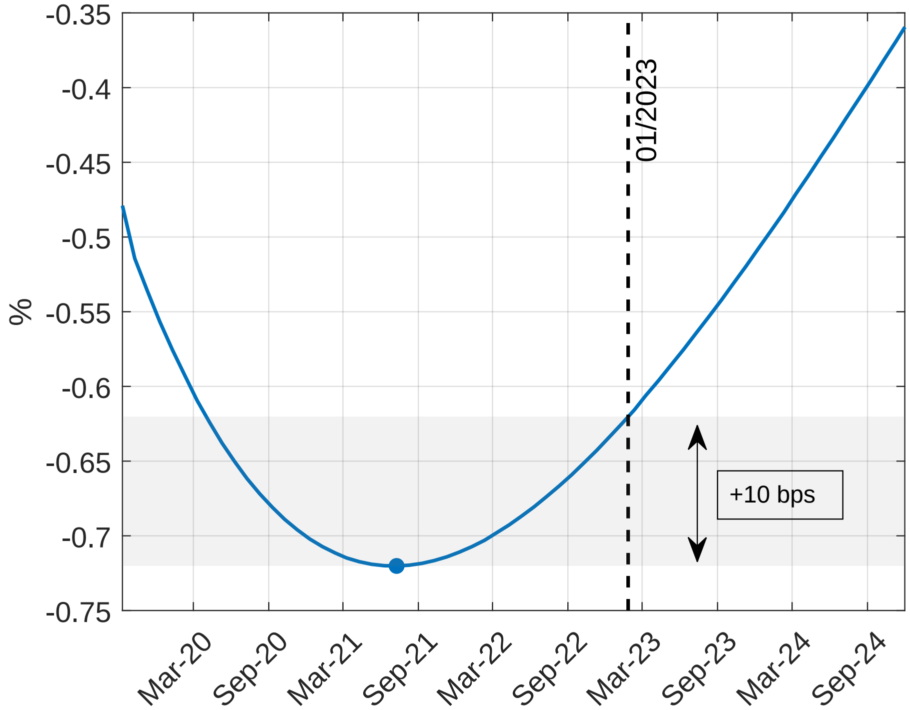
Figure B.1. Time series of data (November 2014 — December 2019)



Appendix C. OIS curve

Results from Section III.

Figure C.1. Forward OIS curve pre-September 2019 Governing Council meeting



Note: Liftoff date is defined as +10bps above the minimum reached by the OIS curve. OIS contracts from Bloomberg, as of 10-Sep-2019. We build the curve using standard Nelson Siegel procedure.

Appendix D. Testing for structuralness

Our approach to APP identification relies on a relatively small amount of information drawn from a 5-variables Proxy-VAR. This small number of variables is unlikely to span the information sets used by the ECB, who has access to richer information about the state of the economy, or by the the financial market participants. If the information sets used in our VAR does not span that of the agents, then the structural shocks might be contaminated (i.e., non-fundamental).

In the spirit of Forni and Gambetti (2014), we perform a test of "structuralness" of the estimated APP shocks to verify whether our VAR suffers from this informational problem. To do so, we project the structural APP shocks onto the factors summarizing the information content of a large set of information available ahead of each Governing Council.

More formally, let ε_t be the vector containing APP shocks at time t, and let X_t be a vector collecting a number of macroeconomic, survey and financial variables. We define the following system:

$$X_t = \Lambda \mathbf{f}_t + u_t,$$

$$\varepsilon_{t+1} = \mathbf{f}_t' B + e_{t+1},$$

where $\mathbf{f}_t$ are factors summarizing the information content in X_t; u_t and e_t are independent identically distributed (i.i.d) shocks; and B is the matrix that loads factors onto structural APP shocks. if B is statistically significant, then shocks can be predicted using past common information, meaning that they are not structural. Following Andrade and Ferroni (2020), we consider a set of 38 variables in X_t that are related to macroeconomic, survey and financial time series. The list of variables is reported in Table D.1. We then extract their principal components using the factor extraction technique developed by Bai and Ng (2002). The number of factors is equal to 5. We then regress APP shocks on these factors and look at their statistical significance.

Table D.2 reports the individual p-values of the coefficients of the regression of APP shocks on lagged factors. Clearly, APP shocks are not predictable by the available information ahead of each Governing Council.

TABLE D.1. List of variables included in X_t to test the predicability of APP shocks. Transformations: 1=first difference; 2=growth rate.

Variables	*Code Series (ECB SDW)*	*Transf*
Adjusted loans to euro area private sector	BSI.M.U2.Y.U.A20TA.A.1.U2.2200.Z01.E	2
Monetary aggregate M3	BSI.M.U2.Y.V.M30.X.1.U2.2300.Z01.E	2
Japanese yen/Euro	EXR.M.JPY.EUR.SP00.A	1
Unemployment rate (as a % of labour force)	STS.M.I8.S.UNEH.RTT000.4.000	1
Euribor 3-month	FM.M.U2.EUR.RT.MM.EURIBOR3MD_.HSTA	1
EER-42/Euro	EXR.M.E7.EUR.EN00.A	1
CPI deflated EER-42/Euro	EXR.M.E7.EUR.ERC0.A	1
Dow Jones Euro Stoxx 50 Price Index	FM.M.U2.EUR.DS.EI.DJES50I.HSTA	2
Standard and Poors 500 Composite Index	FM.M.US.USD.DS.EI.S_PCOMP.HSTA	2
HICP - Overall index	ICP.M.U2.Y.000000.3.INX	2
ECB Commodity Price index	STS.M.I8.N.UWIE.CTOTNE.3.000	2
Unemployment rate, Male	STS.M.I8.S.UNEH.RTM000.4.000	1
New passenger car registration	STS.M.I8.Y.CREG.PC0000.3.ABS	2
Industrial new orders; total	STS.M.I8.Y.ORDT.NSC002.3.000	2
Industrial production for the euro area	STS.M.I8.Y.PROD.NS0020.4.000	2
Industrial production; intermediate goods	STS.M.I8.Y.PROD.NS0040.4.000	2
Industrial production; consumer goods	STS.M.I8.Y.PROD.NS0080.4.000	2
Industrial production; energy	STS.M.I8.Y.PROD.NS0090.4.000	2
Industrial production; including construction	STS.M.I8.Y.PROD.NS0010.4.000	2
Industrial production; excl. construction, energy	STS.M.I8.Y.PROD.NS0021.4.000	2
Industrial production; durable consumer goods	STS.M.I8.Y.PROD.NS0060.4.000	2
Industrial turnover, nominal; manufacturing	STS.M.I8.Y.TOVT.2C0000.4.000	2
Retail trade turnover	STS.M.I8.Y.TOVT.NS4701.4.000	2
UK pound sterling/Euro	EXR.D.GBP.EUR.SP00.A	1
EONIA	EON.D.EONIA_TO.RATE	1
US dollar/Euro	EXR.D.USD.EUR.SP00.A	1
HICP; excluding energy and unprocessed food	ICP.M.U2.Y.XEFUN0.3.INX	2
Euribor 1-year	RTD.M.S0.N.C_EUR1Y.E	1
Brent crude oil 1-month Forward	RTD.M.S0.N.P_OILBR.E	2
Consumer Confidence Indicator	RTD.M.S0.S.Y_CSCCI.F	2
Economic Sentiment Indicator	RTD.M.S0.S.Y_ESIND.F	2
U.S. Consumer Price Index	CPIAUCSL (FED FRED)	2
U.S. all Employees, Total Nonfarm	PAYEMS (FED FRED)	2
U.S. 10-Year Treasury Constant Maturity Rate	DGS10 (FED FRED)	1
U.S. Advance Real Retail and Food Services Sales	RRSFS (FED FRED)	2
U.S. 3-Month Treasury Bill	TB3MS (FED FRED)	1
U.S. Unemployment Rate	UNRATE (FED FRED)	1
U.S. ISM Manufacturing PMI	NAPMPMI Index (Bloomberg)	2

Table D.2. Predictability of structural APP shocks

Factors in set of 38 variables	APP shocks
f_1	0.97
f_2	0.66
f_3	0.81
f_4	0.42
f_5	0.94

Note: P-values of the regression of APP shocks on macroeconomic, survey, and financial lagged factors.

Appendix E. Evidence on other financial and macroeconomic variables

We study the dynamic effects of asset purchases on other variables to explore further the transmission of asset purchases to the economy. Our analysis is conducted by adding each variable to the baseline VAR, one at a time. We consider the following series:

- 20-year and 30-year government bond yields
- The implied-volatility of stock market prices (VSTOXX)
- Stock market prices (stoxx Europe 600 and stoxx Europe 50)
- Market-implied inflation expectations (1y1y, 2y2y, 5y5y inflation-linked swaps)
- ECB macroeconomic projections for real GDP growth and inflation

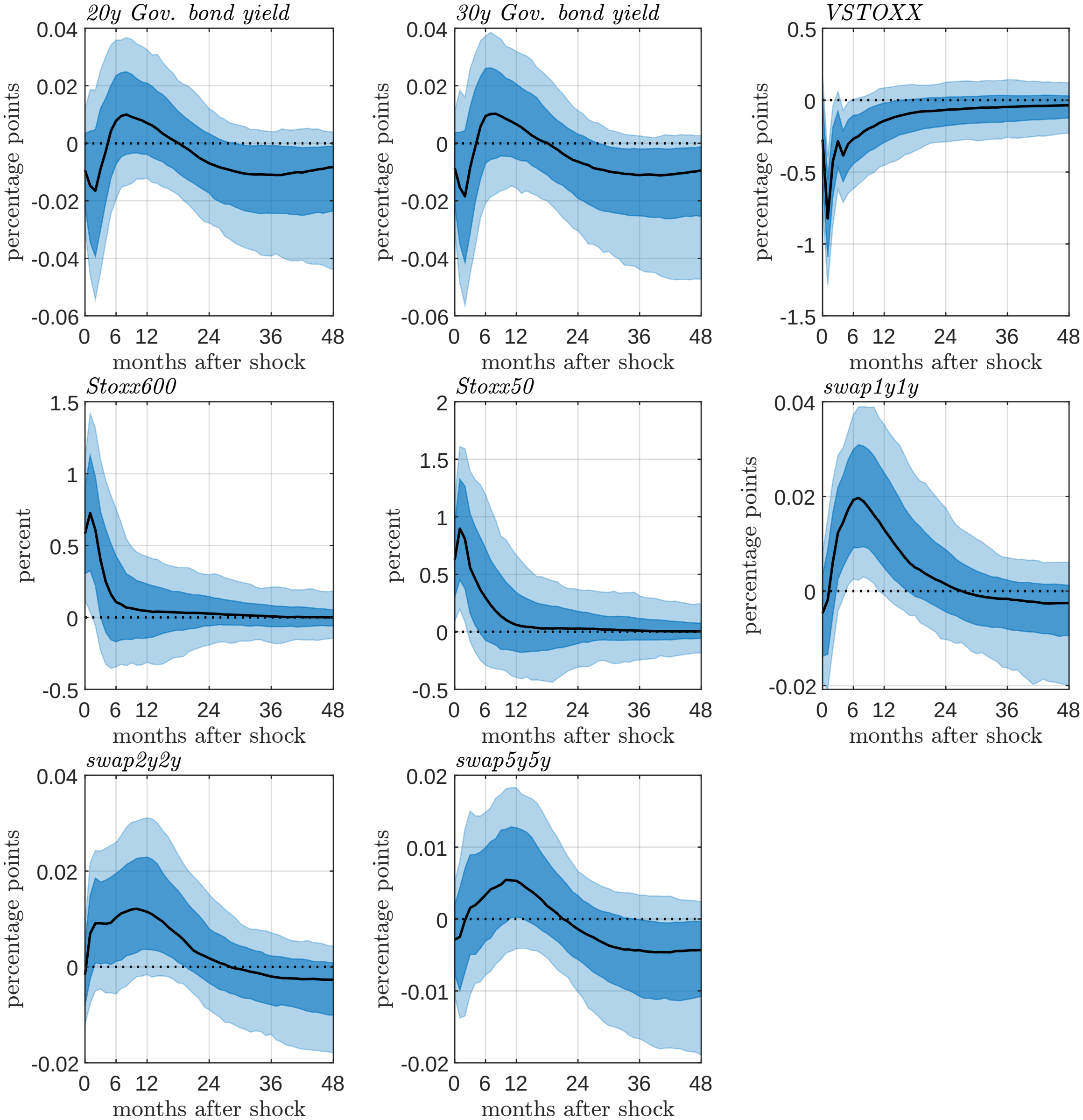
Figures E.1 and E.2 report the results. After an APP shock, both 20-year and 30-year government bond yields decline immediately, though the uncertainty about these estimates remains relatively large, as shown by the 68% and 90% posterior probability bands. Nevertheless, we can reasonably say that these results provide evidence of the portfolio balance channel, since one would expect that asset purchases influence yields at longer maturities.

Looking at the impact on financial market uncertainty, the short-term and rapid decline in VSTOXX, a measure reflecting overall economic uncertainty in Europe, indicates that asset purchases have also a direct impact on the economy by reducing uncertainty.

Further evidence from market-implied inflation expectation suggests that the expectations channel has been a powerful channel. Indeed, 1y1y, 2y2y and 5y5 inflation-linked swaps increase after the initial shock, although the rise in inflation expectations appears to be less significant at longer horizons. This corroborates the impulse responses of one-year ahead ECB projections for GDP growth and inflation.[24]. As shown in Figure E.2, APP shocks positively influence expectations about GDP growth and inflation in the medium-term. A reasonable conclusion is that this is an evidence that the ECB has been able to manage expectations about future economic outcomes.

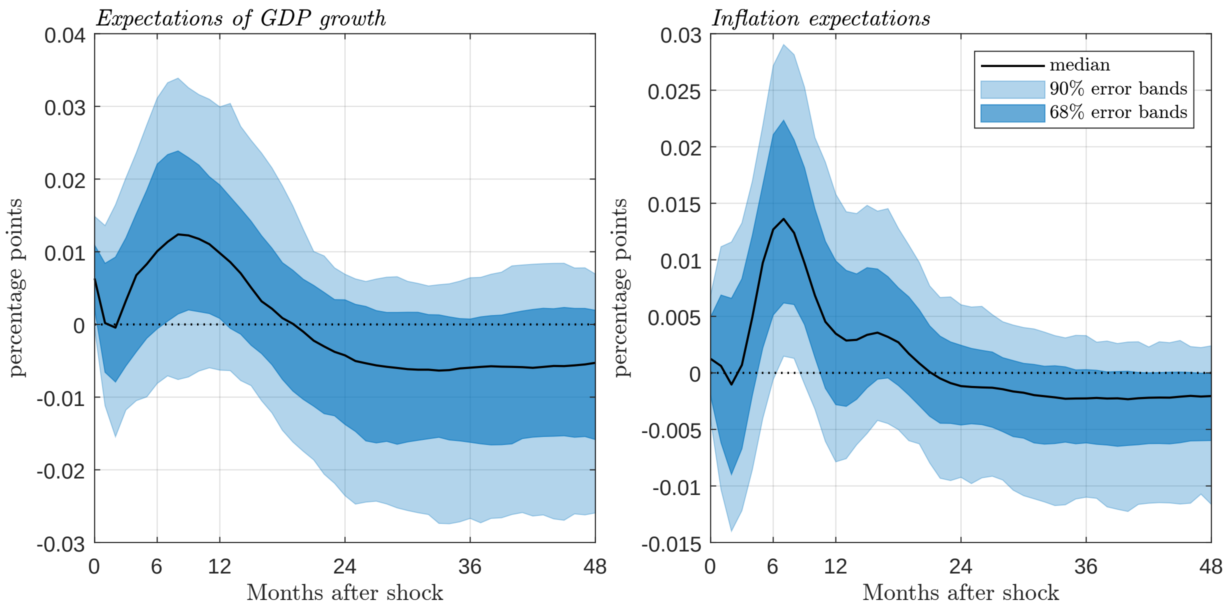
[24]Macroeconomic projections produced by the ECB (available here) are published at a quarterly frequency, that is four times a year (in March, June, September and December). We recover 1-year ahead inflation and real GDP expectations, and then convert them into monthly frequency using a simple linear interpolation

Figure E.1. Impulse responses of financial instruments to an APP shock.



Note: The size of the shock is scaled to induce an immediate increase in asset purchases of 1% of GDP. In each panel, the median is reported in solid line, while the 68% and 90% error bands are shown by blue areas.

Figure E.2. Impulse responses of macroeconomic expectations (one-year ahead) to an APP shock.



Note: The size of the shock is scaled to induce an immediate increase in asset purchases of 1% of GDP. In each panel, the median is reported in solid line, while the 68% and 90% error bands are shown by blue areas.

Appendix F. Historical Counterfactuals

Our estimated model implies not only that asset purchase shocks have accounted for little of the historical pattern of business cycles but also that they account for a relatively small proportion of variation in asset purchases. This implies that most ECB actions have historically been systematic reactions to the state of the economy. Assessment of the effects of APP policy, as opposed to the effects of unpredictable changes in policy, must therefore consider what would have happened if the systematic component of APP were different. In this section, we now run a number of counterfactual exercises to assess the role of the systematic part of asset purchases by considering what would have happened if each major APP recalibration had not been implemented.

The procedure is straightforward. Given the actual data, a set of draws is generated from the posterior distribution using the algorithm developed by Arias, Rubio-Ramírez, and Waggoner (2021). For each draw, we recover the sequence of APP shocks in the model. We then simulate the history (i.e., a set of new series), but replace the actual APP shocks with shocks such that APP recalibrations would not have happened. As a result, the counterfactual simulations report what would have happened if the systematic part of APP policy had not responded to the economy. In these exercises, all other equations of the system are held fixed, which implied that changes in dynamics of the private sector are potentially ignored since private agents may change their behavior under the new policy. Our counterfactual scenarios potentially ignore the Lucas critique. That being said, and according to Leeper and Zha (2003), a counterfactual exercise may imply "*a change in policy that does not significantly shift agents' belief about policy regime and does not generate quantitatively important expectations-formation effects of the kind Lucas (1976) emphasizes*". Therefore, the required distribution of structural APP shocks may not necessarily violate Lucas's critique.

In Section F.1, we discuss simulations for the four major APP recalibrations (December 2015 and March 2016 together, December 2016, October 2017 and June 2018 together, and September 2019)[25] in Figures F.1 to F.4. The main conclusion is that the estimated APP policy changes have successfully boosted prices with high posterior probability. The positive effects of APP on output turn out to be slightly less precisely estimated, as the 90% probability interval exhibits significant uncertainty. In each of these figures, the solid lines represent the actual series, while the dotted lines represent the series under the modified

[25]We study jointly December 2015 and March 2016 recalibrations due to their proximity. Also, given the relatively small amount announced in June 2018, we investigate its cumulative effects with the October 2017 recalibration. See Table 1 for the details about the amount announced by the ECB.

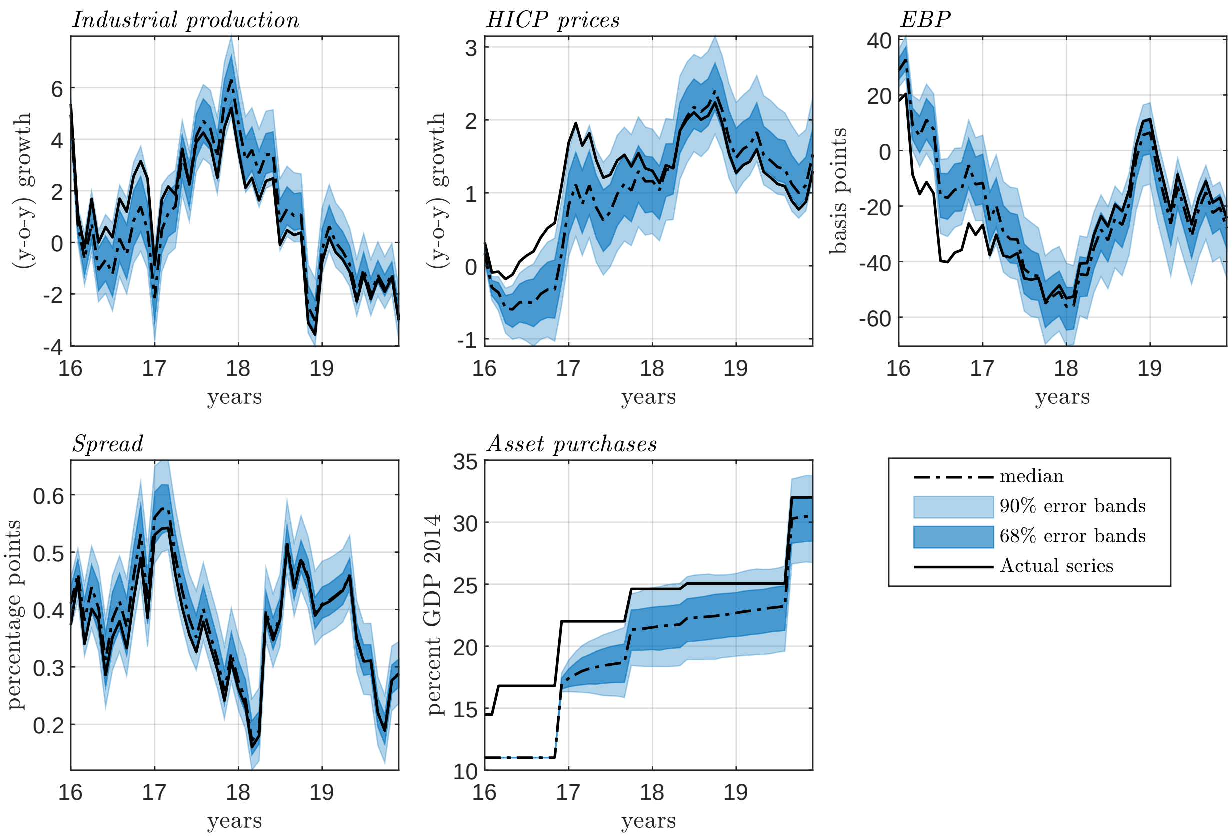
policies, along with their 68% and 90% error bands in blue areas. In Section F.2, we quantify, for each counterfactual, how implausible our APP counterfactual scenarios are using the "modesty statistics" developed by Antolín-Díaz, Petrella, and Rubio-Ramírez (2020).

F.1. **Counterfactual scenarios.** We run a first simulation in which the APP extensions by Eur 360 billion in December 2015 and Eur 240 billion in March 2016 would not have been implemented. The results are shown in Figure F.1. Here, we impose a sequence of APP shocks such that asset purchases remain at their pre-December 2015 level until November 2016, the last month before the date of the next major recalibration. We can see that (year-over-year) industrial production output growth would have reached a lower value by around one percentage point at its peak – although the posterior probability bands are very wide – and the (year-over-year) HICP inflation level would have lowered at its maximum by 0.90 percentage points with high posterior probability. This counterfactual simulation implies that the two first major APP recalibrations have mitigated the decline in output while preventing inflation from reaching negative values.

When we repeat our exercise with December 2016 recalibration (Eur 540 billion), we obtain the results in Figure F.2. This policy counterfactual, in which the path of asset purchases remains constant from December 2016 to September 2017, would have kept output lower by around 0.90 percentage points at its peak and would have delivered a lower inflation by about the same amount. Clearly, this recalibration has been successful in preventing mounting deflationary pressures. The response of output is less precisely estimated since there is a posterior probability mass that lies above the actual series in 2017. Furthermore, without the policy intervention taken by the ECB, the excess bond premium would have been a great deal higher, while the 10-year spread would be slightly higher but only during a very short period.

The effects of the absence of October 2017 (Eur 270 billion) and June 2018 (Eur 45 billion) recalibrations are shown in Figure F.3. The major difference with the previous simulations is that the size of this APP extension has been relatively smaller but its duration were much longer. Indeed, we assume an intervention that maintains the level of asset purchases to its pre-October 2017 level until August 2019, the last month before the announcement date of the "open-ended" recalibration. Interestingly, our counterfactual simulation produces modest posterior median effects on output in the beginning of 2018 – with a 90% probability interval exhibiting significant uncertainty – and then leaves the time path of output almost unchanged for the rest of the sample. By contrast, the simulation cuts inflation in late 2017, and keeps it well below historical values (by about 0.4 percentage points) for most of 2018 and 2019.

Figure F.1. Effects of December 2015 & March 2016 APP recalibrations.

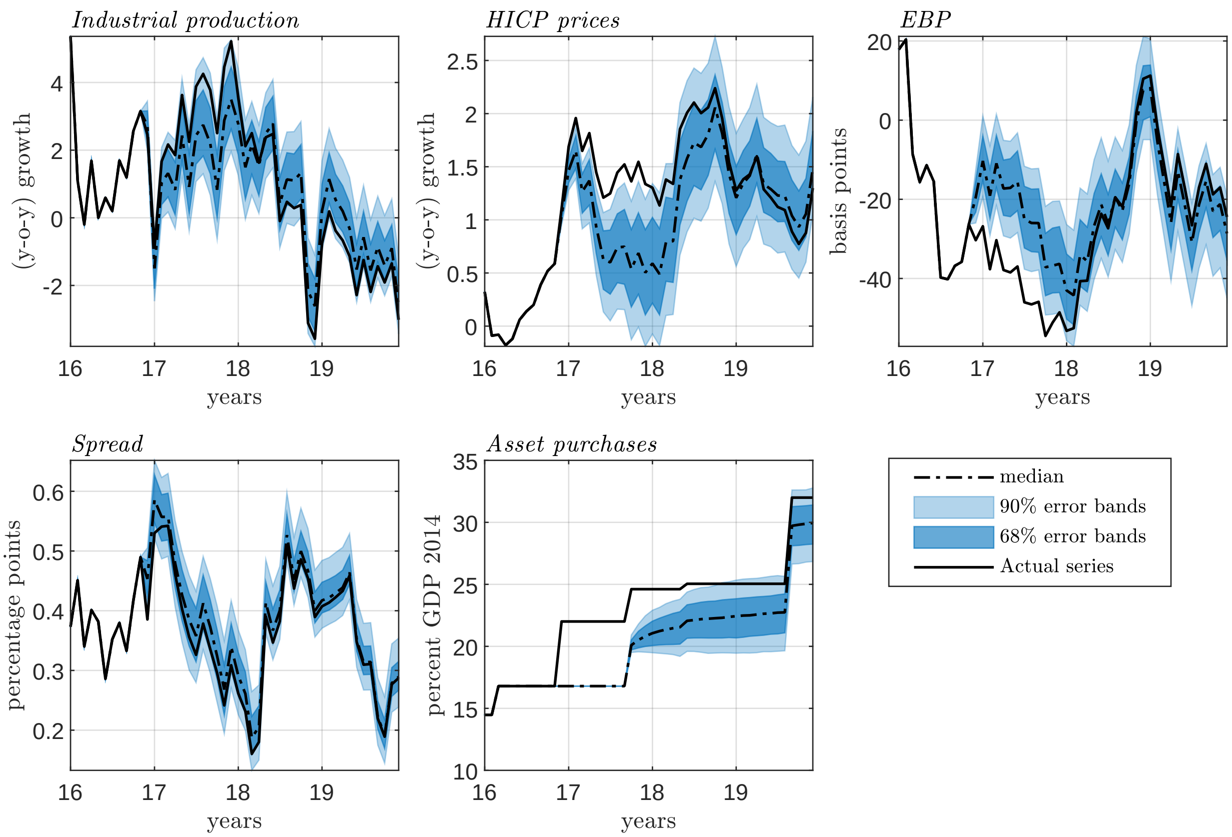


Note: Sample period: 2016.M01 — 2019.M12. Counterfactual — i.e., modifying APP shocks such that asset purchases remain unchanged from December 2015 to November 2016. In each panel, actual and median counterfactual paths of endogenous variables are in black solid and dotted lines, respectively. The blue areas denote the counterfactual's 68% and 90% error bands areas.

Also noticeable is the long-term deterioration of financial intermediaries' financial positions as shown through the excess bond premium.

The last simulation in which September 2019 recalibration (Eur 700 billion) would not have been put in place is reported in Figure F.4. At its peak, inflation would have reached a lower value by around 0.25 percentage point with high posterior probability, while effects on output remain relatively modest and uncertain. As shown by both financial variables, financial conditions would have been tighter without the ECB intervention. Note however that, since our data sample ends in December 2019, we cannot fully evaluate the impact of this recalibration on the economy.

Figure F.2. Effects of December 2016 APP recalibration.

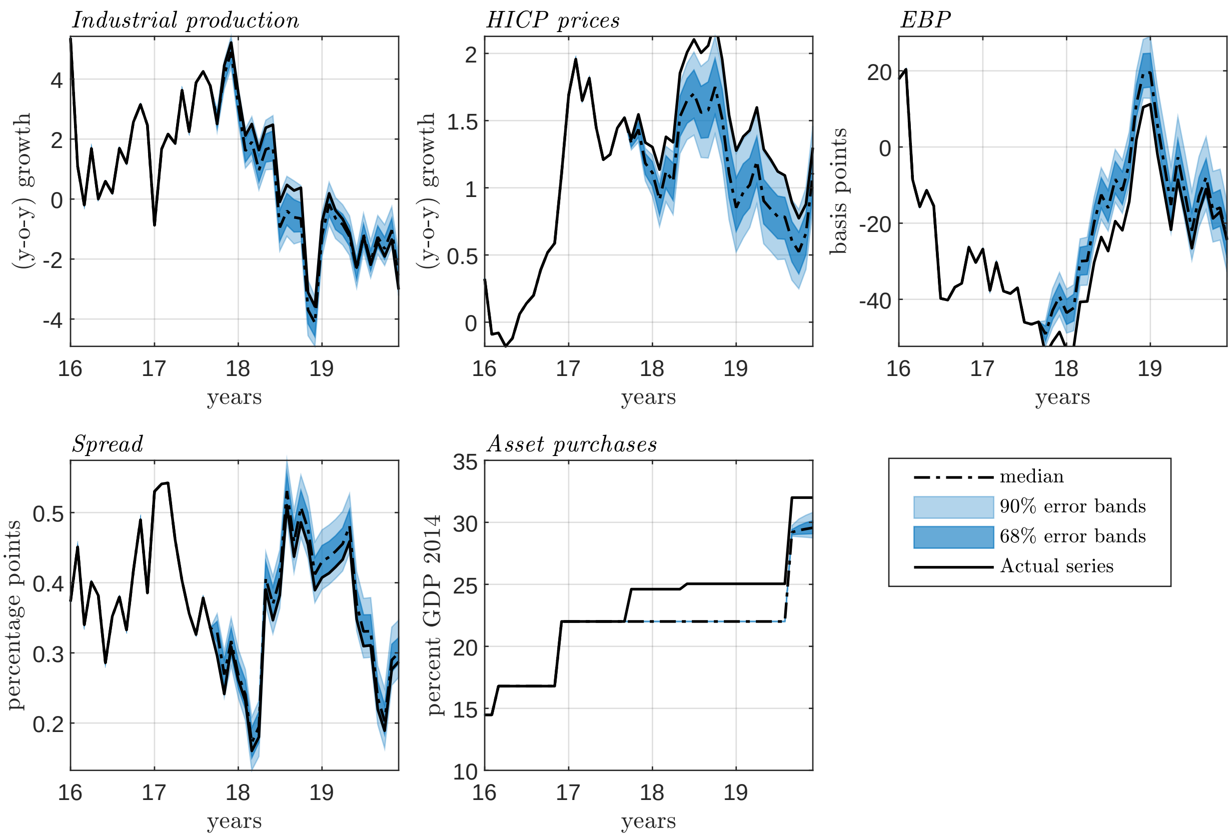


Note: Sample period: 2016.M01 — 2019.M12. Counterfactual — i.e., modifying APP shocks such that asset purchases remain unchanged from December 2016 to September 2017. In each panel, actual and median counterfactual paths of endogenous variables are in black solid and dotted lines, respectively. The blue areas denote the counterfactual's 68% and 90% error bands areas.

Overall, our counterfactual simulations provide strong evidence that APP recalibrations had beneficial effects both on the real and financial economy since 2015, and prevented several times inflation from falling into negative territory.

To compare our estimates with the literature, we record the estimated effects of the systematic part of asset purchases on output (i.e., real GDP or industrial production) and inflation (consumer prices) based on major studies' baseline model. Following the Fabo, Jančoková, Kempf, and Pástor (2021)'s rule, we record the effects on the level — the level of output and the price level. We denote Y the actual level of the outcome variable (i.e., with asset purchases) and $\hat{Y}$ its counterfactual level (i.e., without asset purchases), and derive the percentage difference, $(Y - \hat{Y})/\hat{Y}$. We standardize the effects to a common asset purchase

Figure F.3. Effects of October 2017 & June 2018 APP recalibrations.

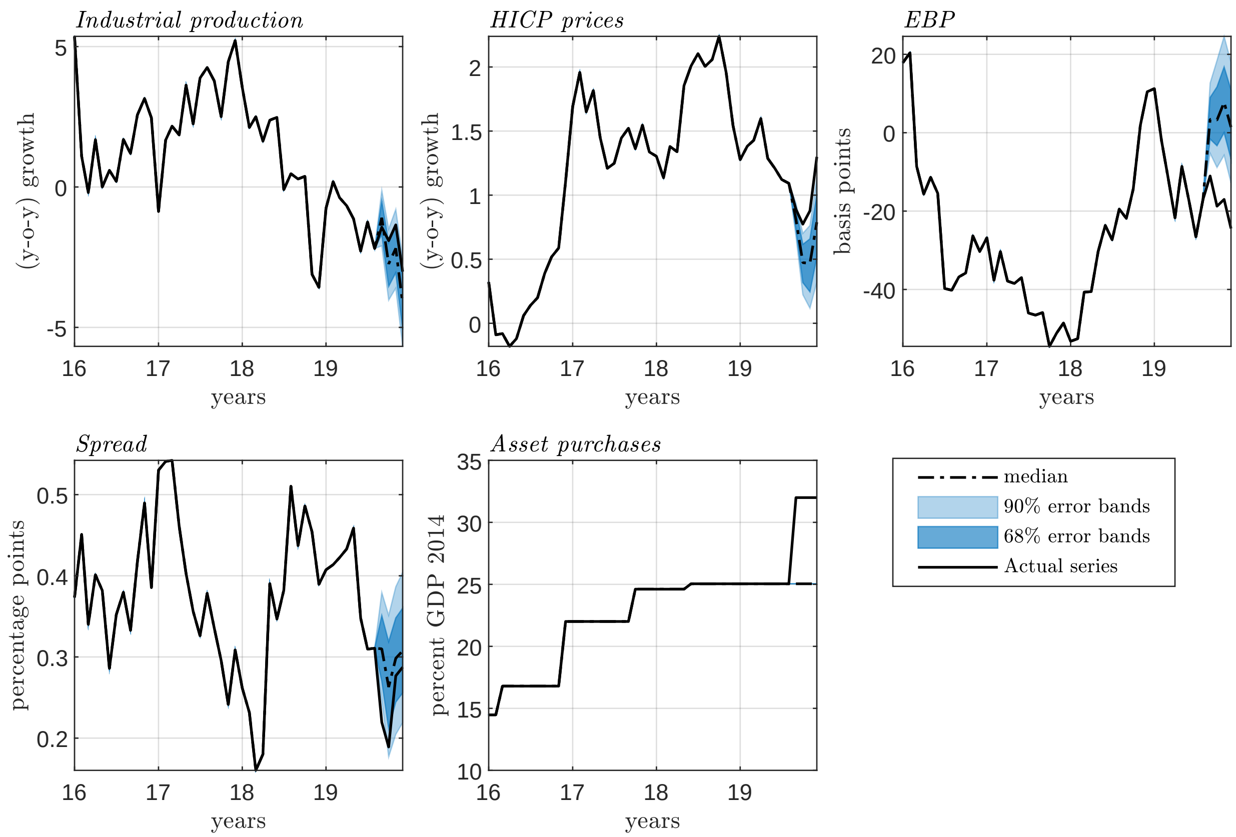


Note: Sample period: 2016.M01 — 2019.M12. Counterfactual — i.e., modifying APP shocks such that asset purchases remain unchanged from October 2017 to August 2019. In each panel, actual and median counterfactual paths of endogenous variables are in black solid and dotted lines, respectively. The blue areas denote the counterfactual's 68% and 90% error bands areas.

increase equal to 1% of the respective country's GDP around the time asset purchases were first implemented. We proceed similarly with our estimates by taking the average of the median peak effects on the level of both variables among the first three APP recalibrations: December 2015 & March 2016, December 2016, and October 2017 & June 2018, and then standardize the effects in the same way as above. We do not include the last APP recalibration since we have not fully evaluated its impact. The (standardized) peak effects drawn from our baseline estimates are reported in Figures F.5 and F.6.

Table F.1 shows the estimated effects of asset purchases on the levels of output and prices for our baseline and major studies' estimates. For prices, our peak effects are smaller than those from Andrade, Breckenfelder, De Fiore, Karadi, and Tristani (2016) and Weale and

Figure F.4. Effects of September 2019 APP recalibration.

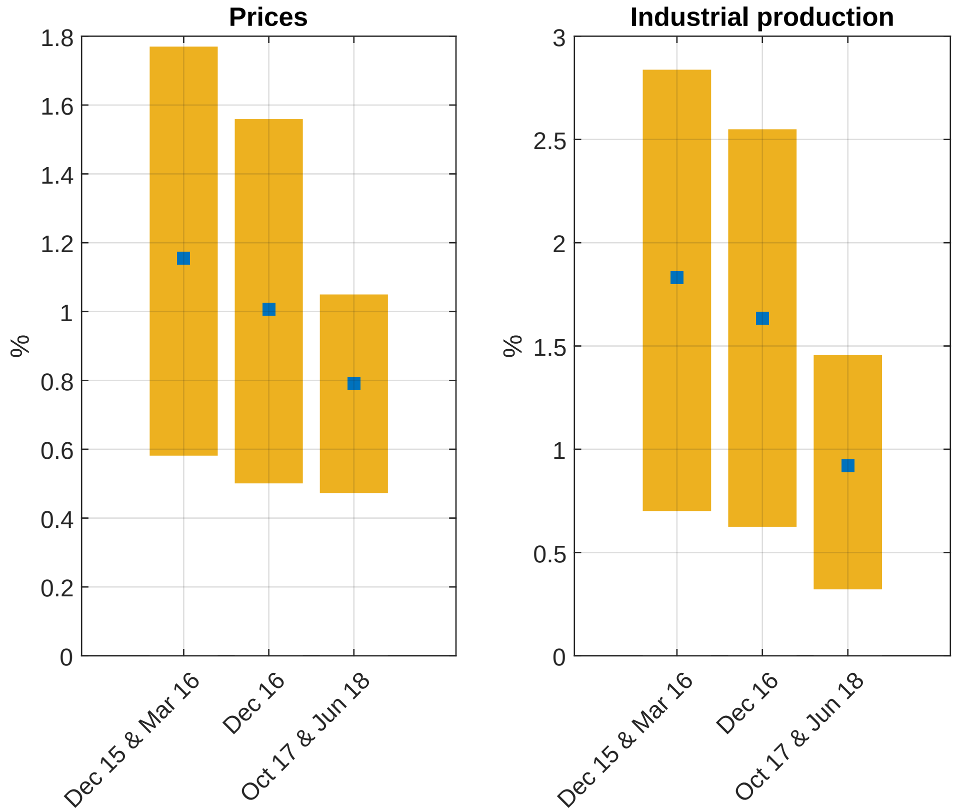


Note: Sample period: 2016.M01 — 2019.M12. Counterfactual — i.e., modifying APP shocks such that asset purchases remain unchanged from September 2019 to December 2019. In each panel, actual and median counterfactual paths of endogenous variables are in black solid and dotted lines, respectively. The blue areas denote the counterfactual's 68% and 90% error bands areas.

Wieladek (2016), but much larger than those from Garcia Pascual and Wieladek (2016), Gambetti and Musso (2020), and Baumeister and Benati (2013). For output, the estimate of the peak effect lies within the high side of the range of values reported by the literature.

F.2. **The plausibility of counterfactual scenarios.** We examine whether our counterfactual simulations can be regarded as "modest policy interventions" in the sense of Leeper and Zha (2003). The idea is to assess the plausibility of the counterfactual paths for each endogenous variable of the VAR system from the perspective of a forecast. If the distribution of a conditional forecast under a policy intervention deviates significantly from the unconditional distribution, then the alternative policy should be deemed implausible; there is something that has changed in the VAR system. As a consequence, conditional forecasts

Figure F.5. Peak effect of APP recalibration rounds

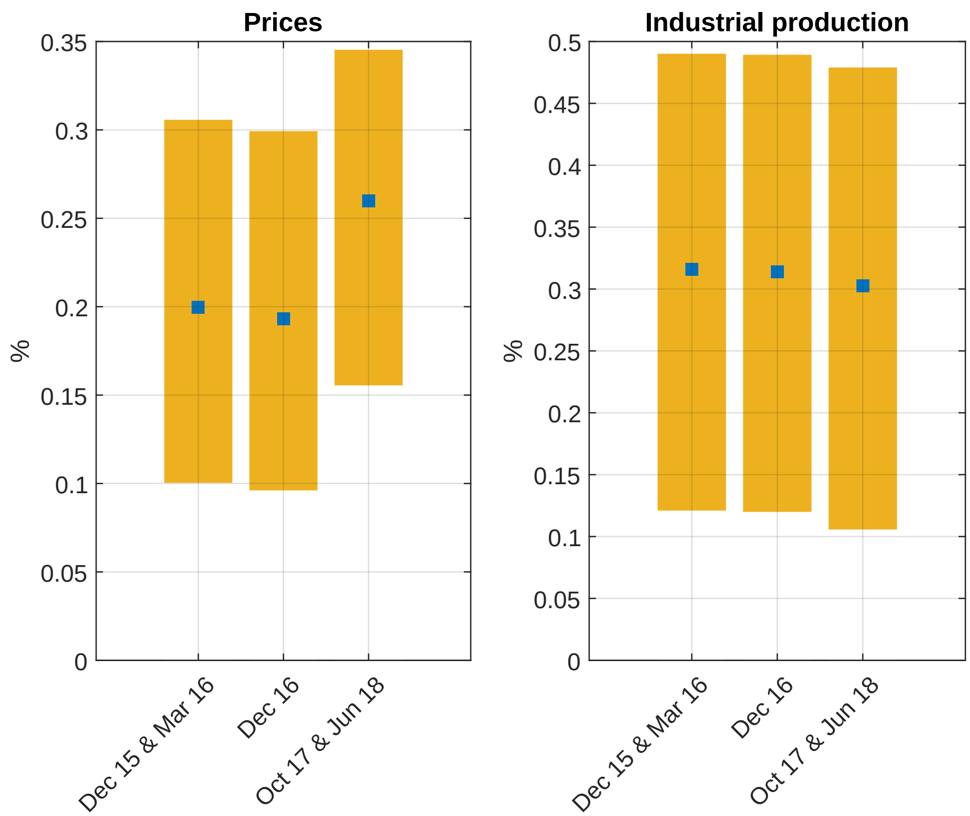


Note: Blue squares denote the median and yellow bars the 68% uncertainty bands. Sep 2019 recalibration not shown as its full impact may not be reflected in our sample ending in Dec 2019.

are not viewed as credible, and the formation of expectations needs to be taken into account when forecasting under this alternative policy.

Using the methodology developed in Antolín-Díaz, Petrella, and Rubio-Ramírez (2020), we construct our conditional forecasts (or also called "structural scenarios") by choosing a sequence of APP shocks that keep asset purchases constant over a predefined horizon, which is specific to each round of interest. Other shocks of the model are perturbed stochastically but without any constraint. Conditional forecasts are done directly on the structural form of the VAR, as opposed to the reduced form as in Waggoner and Zha (1999), which does not require identifying the structural shocks. This allows us to guarantee that the path of variables results from a sequence of APP shocks that keep asset purchases constant over a predefined horizon.

Figure F.6. Peak effect of APP recalibration rounds - rescaled for 1% of euro area GDP.



Note: Blue squares denote the median and yellow bars the 68% uncertainty bands. We rescale each recalibration round by 2014Q1 euro area GDP. Sep 2019 recalibration not shown as its full impact may not be reflected in our sample ending in Dec 2019.

We compute four conditional and unconditional forecasts over a horizon of $h = 48$ months by taking as initial conditions the data up to the announcement date of each major recalibration. Our forecasts are based on the VAR estimated over the entire sample period from November 2014 to December 2019. The reason is simply that the sample period would be otherwise too short to produce reasonable estimates, which would lead to more dispersed posterior distributions. The conditional and unconditional forecasts are available in Figure F.7.

Following Antolín-Díaz, Petrella, and Rubio-Ramírez (2020), we now quantify how plausible a structural scenario is by determining how "far" the distribution of conditional forecast is from the unconditional distribution using the Kullback-Leibler (KL) divergence as "modesty statistics". Equivalently, this amounts to determining how different the distribution of the

Table F.1. Standardized peak effects on prices and output

	Country	Model	Prices (%)	Output (%)
Baseline estimates	E.A	VAR	0.22	0.31
Andrade, Breckenfelder, De Fiore, Karadi, and Tristani (2016)	E.A	DSGE	0.41	0.12
Garcia Pascual and Wieladek (2016)	E.A	VAR	0.09	0.13
Gambetti and Musso (2020)	E.A	VAR	0.04	0.02
Baumeister and Benati (2013)	U.K	VAR	0.06	0.08
Baumeister and Benati (2013)	U.S	VAR	0.06	0.09
Weale and Wieladek (2016)	U.K	VAR	0.46	0.30
Weale and Wieladek (2016)	U.S	VAR	0.96	0.96
Chen, Cúrdia, and Ferrero (2012)	U.S	DSGE	0.02	0.02
Kim, Laubach, and Wei (2020)	U.S	VAR	0.19	0.93

Note: This table summarizes the standardized peak effects on prices and output in the literature. The effects are standardized to a common increase size equal to 1% of the respective country's GDP around the time asset purchases was first introduced.

structural shocks compatible with the structural scenario is from the unconditional distribution of the structural shocks (i.e., from the standard normal distribution). By doing so, we take into account the uncertainty induced by the future realization of all shocks in the model, as opposed to the original framework of Leeper and Zha (2003), in which only policy shocks are active.[26] The KL divergence between the distribution of the structural shocks, $\mathcal{N}_U$, and the unconditional distribution of structural shocks, $\mathcal{N}_{SS}$, is given as follows

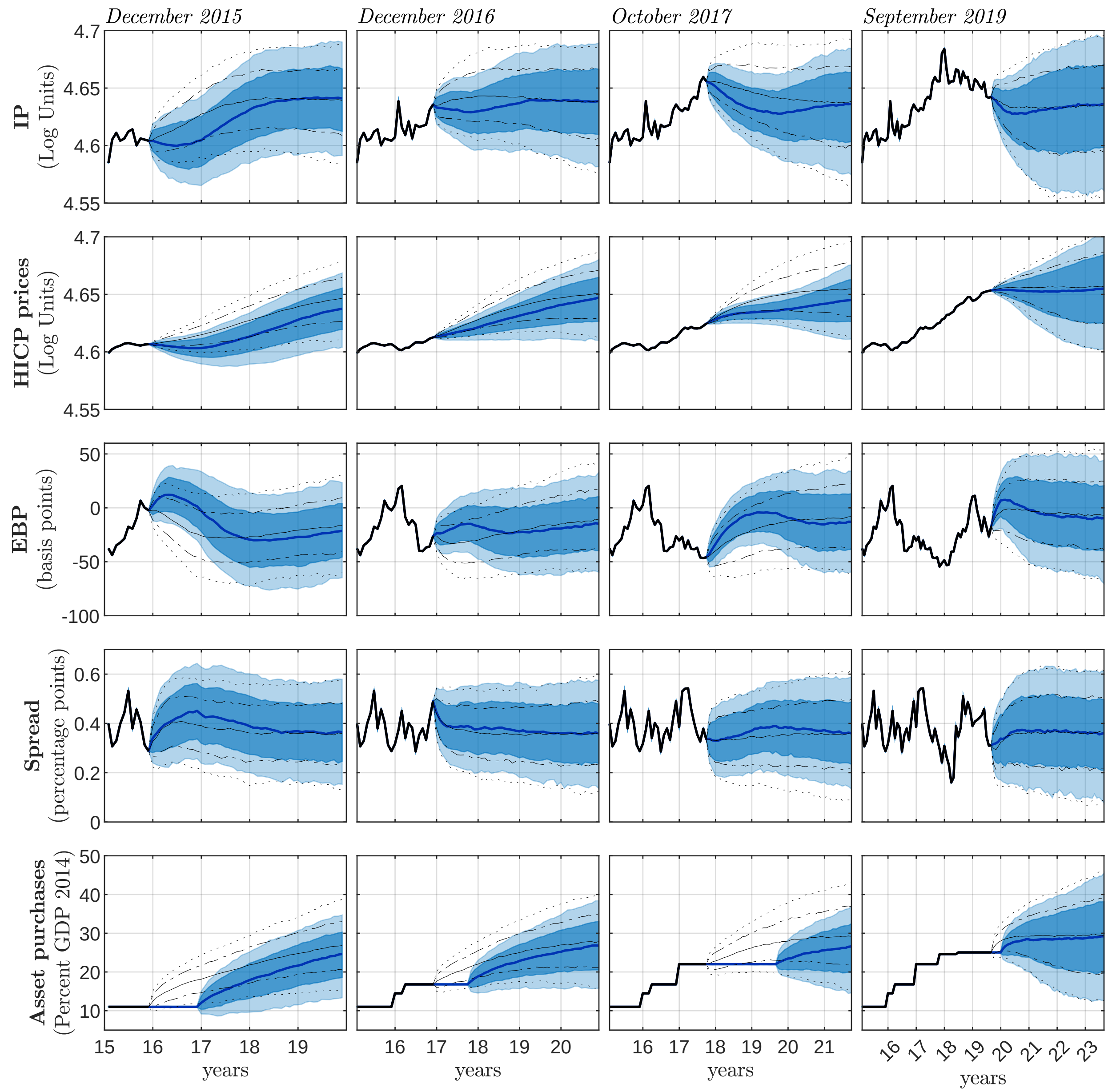
$$D_{KL}(\mathcal{N}_U||\mathcal{N}_{SS}) = \frac{1}{2}\left(\text{tr}\left(\mathbf{\Sigma}_\varepsilon^{-1}\right) + \mu_\varepsilon'\mathbf{\Sigma}_\varepsilon^{-1}\mu_\varepsilon - nh + \ln\left(\det\mathbf{\Sigma}_\varepsilon\right)\right), \tag{5}$$

where tr denotes the trace of a matrix, det denotes the determinant, and μ_ε and $\mathbf{\Sigma}_\varepsilon$ are the mean and variance of the shocks under the conditional forecasts.

In order to ease the interpretation of the KL divergence and to say how far the conditional forecast is from the unconditional forecast, we "calibrate" the KL divergence along the lines of McCulloch (1989); i.e., the discrimination information between the flips of a fair coin and a biased coin. $D_{KL}(\text{Bern}(0.5)||\text{Bern}(q))$, is proposed to calibrate the KL divergence, where $\text{Bern}(q)$ is a Bernoulli distribution with probability q. Given the dimensional issues with the Bernoulli distribution, we follow Antolín-Díaz, Petrella, and Rubio-Ramírez (2020) and

[26] Adolfson, Laséen, Lindé, and Villani (2005) and Benati (2021) have also taken into account the uncertainty induced by all shocks of the model to assess the plausibility of some conditional forecasts. However, they do not use the KL divergence as modesty statistics.

Figure F.7. Conditional versus Unconditional Forecasts.



Note: Each column represents the conditional and unconditional forecasts starting at different periods: December 2015, December 2016, October 2017, and September 2019. The median of conditional forecasts is reported in solid blue line with 68% and 90% error bands in blue areas. The median of unconditional forecasts is reported in solid black line, along with 68% and 90% error bands in dotted lines.

replace it with a binomial distribution. It follows that

$$q = \frac{1 + \sqrt{1 - e^{-\frac{2z}{nh}}}}{2}, \tag{6}$$

where $z = D_{KL}(\mathcal{N}_U||\mathcal{N}_{SS})$. It can be shown that $q \in [0.5; 1]$. Thus, a value of q close to 1 suggests that the distribution of the structural shocks under the structural scenario considered is very far from the unconditional distribution of the shocks, and therefore that such a policy intervention can be regarded as implausible or, say differently, not "modest" in the sense of Leeper and Zha (2003). By contrast, a value of q close to 0.5 makes the intervention highly plausible.

Table F.2 reports the median of "modesty statistics" for each counterfactual experiment using the KL divergence and its calibration as proposed above along with 68 probability intervals. The first striking result is that the calibrated q is below 1 with narrow probability intervals for all four cases, meaning that our counterfactual experiments are all plausible. The least likely policy intervention among the ones considered is October 2017. Although the intervention is relatively small, the overall intervention is very persistent since the intervention is in place until August 2019. As a consequence, this requires a sequence of APP shocks that substantially deviate from their unconditional distribution. This result corroborates the Leeper and Zha (2003)'s evidence, namely that the longer the intervention last the least likely the policy is plausible. Say it differently, expectations-formation effects become apparent at longer horizons.

Table F.2. Plausibility of Counterfactual Scenarios

	KL divergence	Calibrated q
December 2015	31.80	0.72
	[29.36; 35.19]	[0.71; 0.73]
December 2016	24.46	0.70
	[23.44; 25.58]	[0.69; 0.70]
October 2017	55.01	0.78
	[52.72; 57.45]	[0.78; 0.79]
September 2019	10.25	0.63
	[9.46; 11.55]	[0.63; 0.64]

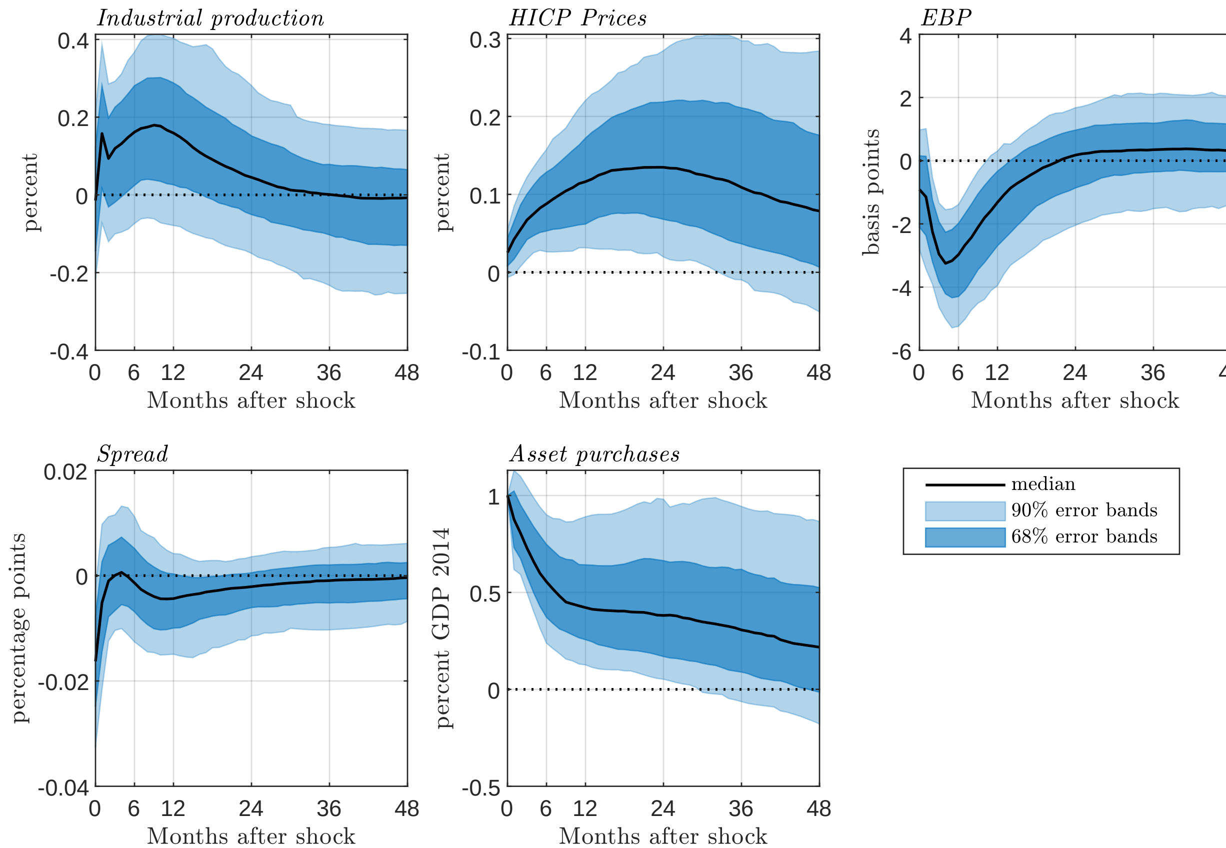
Note: Median of the KL divergence and the calibrated q. The 68 percent probability intervals are indicated in brackets.

Finally, it may worth mentioning that we have also conducted a counterfactual in which we assume that the overall balance sheet did not expand throughout the sample. Nevertheless, after conducting this exercise, we have noticed that this kind of scenario is highly implausible (and substantially less useful) in the sense that the implied APP shocks are so “unusual” that the analysis falls into the criticism put forward by Lucas (1976). Indeed, the posterior mode of the plausibility metric is very close to 1 (calibrated q), indicating a large distortion of the distribution of APP shocks (with respect to shocks from unconditional distribution) to generate the new desired path of asset purchases. Once again, this result corroborates the Leeper and Zha (2003)'s evidence, namely that the longer the intervention last the least likely the policy is plausible. By contrast, our scenarios focusing on each APP calibration are highly plausible.

Appendix G. Robustness Analysis

G.1. **Excluding January 2015.** In absolute terms, the largest APP shock occurred in January 2015, the date of the introduction of APP. Given the small sample size, this date might be the main driver of our results. To check whether our results depend on this date, we re-estimate our model on a sample that starts in February 2015. We obtain similar results, as shown in G.1.

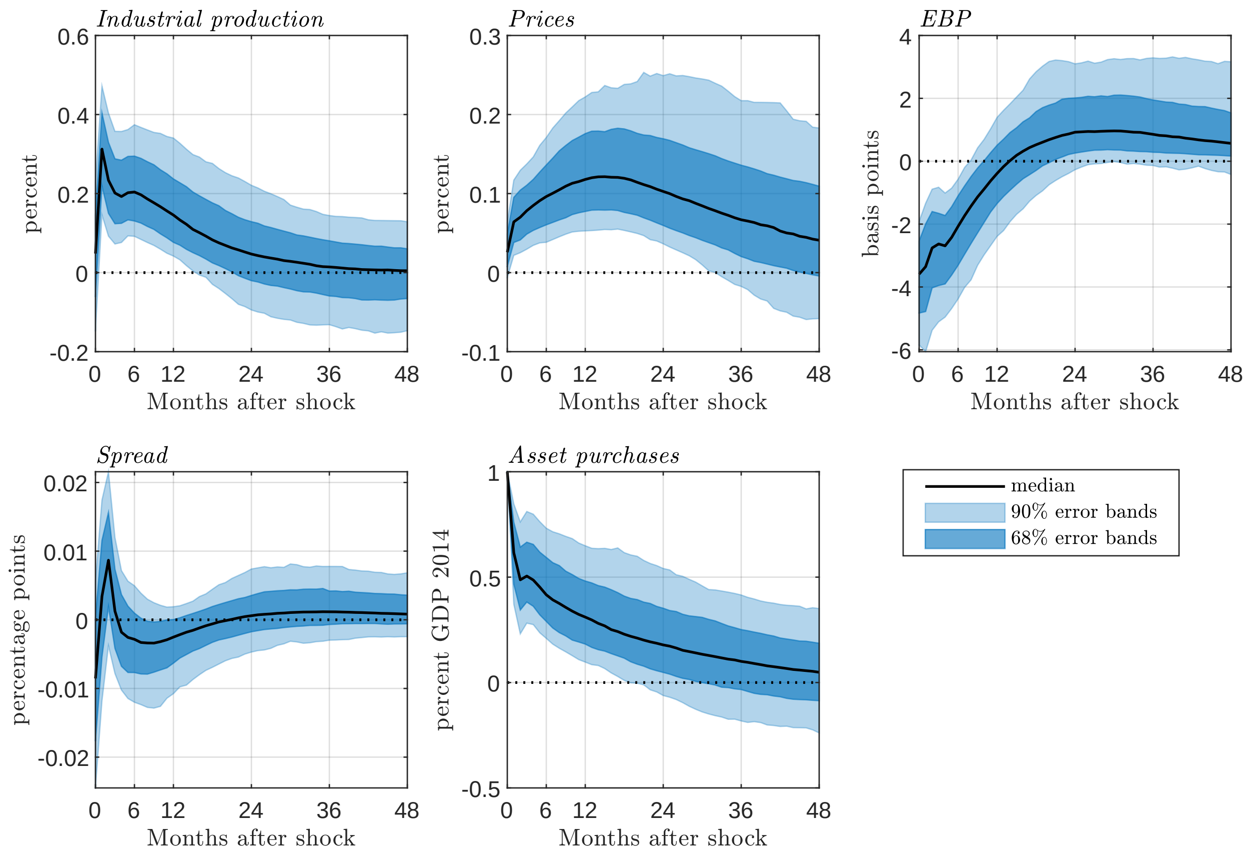
Figure G.1. Excluding January 2015 from the sample



Note: The size of the shock is scaled to induce an immediate increase in asset purchases of 1% of GDP. In each panel, the median is reported in solid line, while the 68% and 90% error bands are shown by blue areas.

G.2. **Excluding open-ended announcement.** Our sample includes the last APP announcement (September 2019), which was open-ended. This particular announcement required an elaborated method for constructing the surprise (see the main manuscript). Therefore, this surprise is more subject to uncertainty compared to other constructed surprises of our sample. To reduce such an uncertainty, we re-estimated our model on a sample that ends in August 2019. As shown in Figure G.2, our results remain unchanged.

FIGURE G.2. Excluding Open-ended Announcement



Note: The size of the shock is scaled to induce an immediate increase in asset purchases of 1% of GDP. In each panel, the median is reported in solid line, while the 68% and 90% error bands are shown by blue areas.

G.3. **Alternative proxy.** In our baseline specification, our preferred measure of surprises used as a proxy takes into account the fact that a (small) proportion of respondents were not expected the announcement of a new APP recalibration. Their expectations were set to zero. In this section, we recompute the surprises using only the answers from the respondents who expected an APP recalibration at the next Governing Council. The resulting new time series proxy is depicted in Figure G.3 (thereafter called "reweighted proxy"), together with the baseline proxy. Qualitatively, the proxy delivers similar values to the baseline proxy. Quantitatively, there are however several major differences. The most striking difference between both proxies is found in the September 2019 recalibration. While the baseline proxy indicates unexpected changes in APP by about 5 percent of GDP, such a surprise is only about 3 percent of GDP for the reweighted proxy. Another difference with the baseline proxy is that there is no significant surprise in APP in June 2018. Finally, the small difference between both proxies in January 2015 reveals that the introduction of APP was almost fully expected by all market participants.

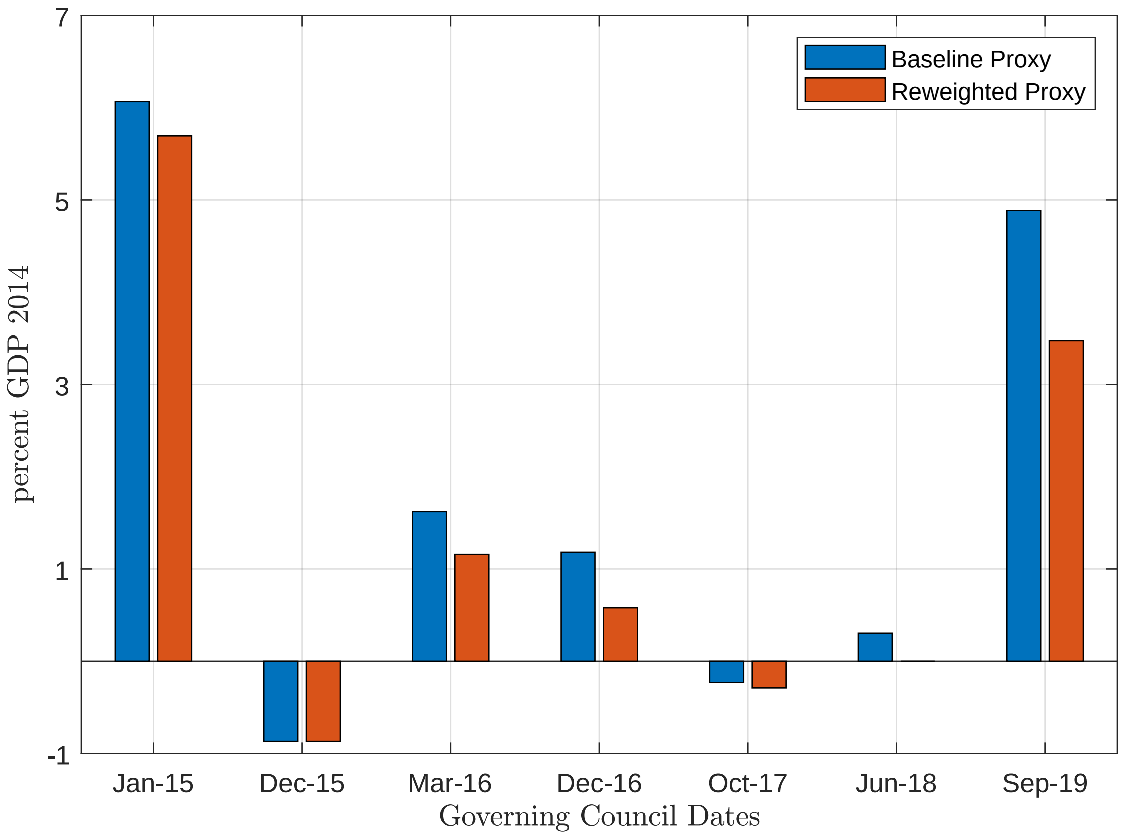
Given the sizeable differences between proxies, we re-estimate our VAR model by replacing our baseline proxy with our reweighted proxy. Figure G.4 displays the impulse responses. Clearly, changing the construction of our proxy does not affect the dynamics effects of APP shocks. Impulse responses are close to those reported with the VAR identified from the baseline proxy; the effects of APP shocks are still expansionary.

G.4. **Proxy within a recursive VAR.** A number of studies employ an "internal instrument" strategy consisting in ordering the proxy first in a recursive (i.e., Cholesky) VAR instead of the "external instrument" approach. Notable examples include Kilian (2009), Ramey (2011), Jarociński and Karadi (2020), and Miranda-Agrippino and Ricco (2021). Through the properties of the Cholesky decomposition, the identifying restrictions given by equations (3) and (4) still hold. According to Plagborg-Møller and Wolf (2021), the "internal instrument" strategy leads to valid impulse response estimates even if the proxy is contaminated with measurement error that is unrelated to the shock of interest. Furthermore, it yields to estimates that are closely tied to those obtained from the "local projections" approach proposed by Jordà (2005) at short horizons (see Corollary 1 in Plagborg-Møller and Wolf (2021)).

As a robustness check, we estimate our baseline VAR model using the "internal instrument" strategy. We augment our VAR to include the proxy and order it first using a recursive ordering. By doing so, we are also able to check whether our results remain valid through a "local projections" approach. Impulse responses are displayed in Figure G.5. The responses are qualitatively similar to the responses of Figure 6 which are obtained from the "external instrument" approach. But there are notable quantitative differences. First, the response of industrial production is less precisely estimated, and there is a posterior probability mass that lies within a region of negative values when looking at 90% error bands. Second, the impact on the excess bond premium appears slightly stronger. At its peak, the decline is about one basis point lower.

G.5. **Free float.** In this section, we explore an alternative scaling of our shock variable. In our baseline specification, we normalize the APP shocks by the euro area GDP. While this scaling is consistent with most of the literature, we may use instead the stock of debt really

Figure G.3. Baseline Proxy versus Reweighted Proxy around Governing Council Announcements.

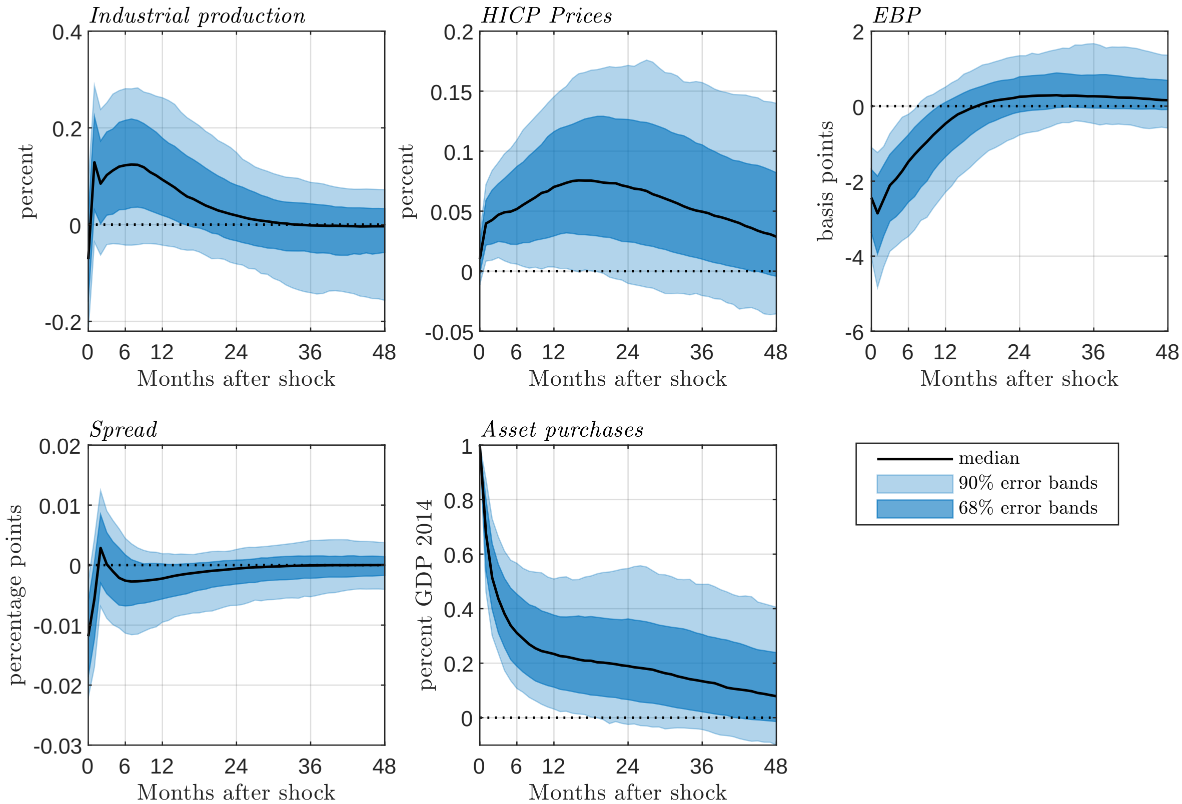


Note: Unexpected components are scaled to GDP 2014. Our baseline measure of surprises takes into account the fact that a proportion of respondents were not expected the announcement of a new APP recalibration at the upcoming GovC. Their expectations were then set to zero. The reweighted proxy recompute the surprises using only the answers by the respondents who expected an APP recalibration at the next Governing Council.

available in the market ("free float"), in the spirit of Eser, Lemke, Nyholm, Radde, and Vladu (2019) and Altavilla, Carboni, and Motto (2021).

This approach is notably guided by the preferred habitat theory. Intuitively, the amount of debt held by inelastic investors (e.g. "preferred habitat" debt with particular characteristics in terms of maturity, invested in held-to-maturity portfolios) is in a way retrenched from the free float available for trading in the market. Vayanos and Vila (2021) develop formally a term-structure model in which the yield curve response to a bond supply shock varies depending on the residual debt to be held by non-preferred habitat investors.

Figure G.4. Impulse responses to an APP shock

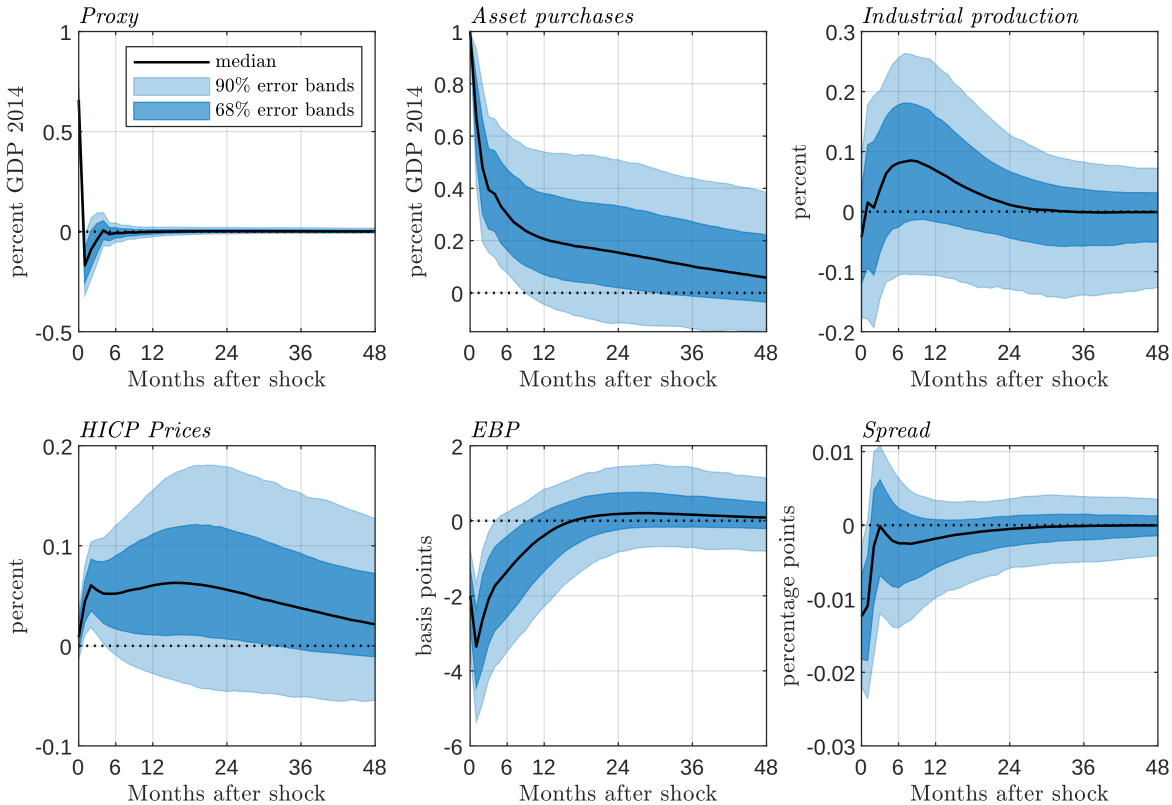


Note: The size of the shock is scaled to induce an immediate increase in asset purchases of 1% of GDP. In each panel, the median is reported in solid line, while the 68% and 90% error bands are shown by blue areas.

We follow Eser, Lemke, Nyholm, Radde, and Vladu (2019) in defining the "free float" as the amount outstanding of market debt held by investors other than the ECB in its monetary policy portfolio and by insurance and pension funds (ICPF), known to be inelastic investors.[27] To do so, we use the Securities Holdings statistics Database (SHS-S), which tracks quarterly the holdings of European investors at the security levels. Free float hovers around Eur 6,000 billion from 2013Q4 and 2015Q1 and then steadily decreases to Eur 4,500 billion, reflecting the APP purchases. The free float rises again after December 2018 and the end of net purchases. We rescale our APP surprises and the APP announced size in the VAR on this free float measure (contemporaneous and lagged in 2014Q1 as for GDP), Impulse responses yield extremely similar results in both cases, and we do not report them for this reason. These results are available upon request.

[27]Due to data limitation, we cannot include foreign officials in the inelastic holdings.

Figure G.5. Impulse responses to an APP shock using an "Internal instrument" approach



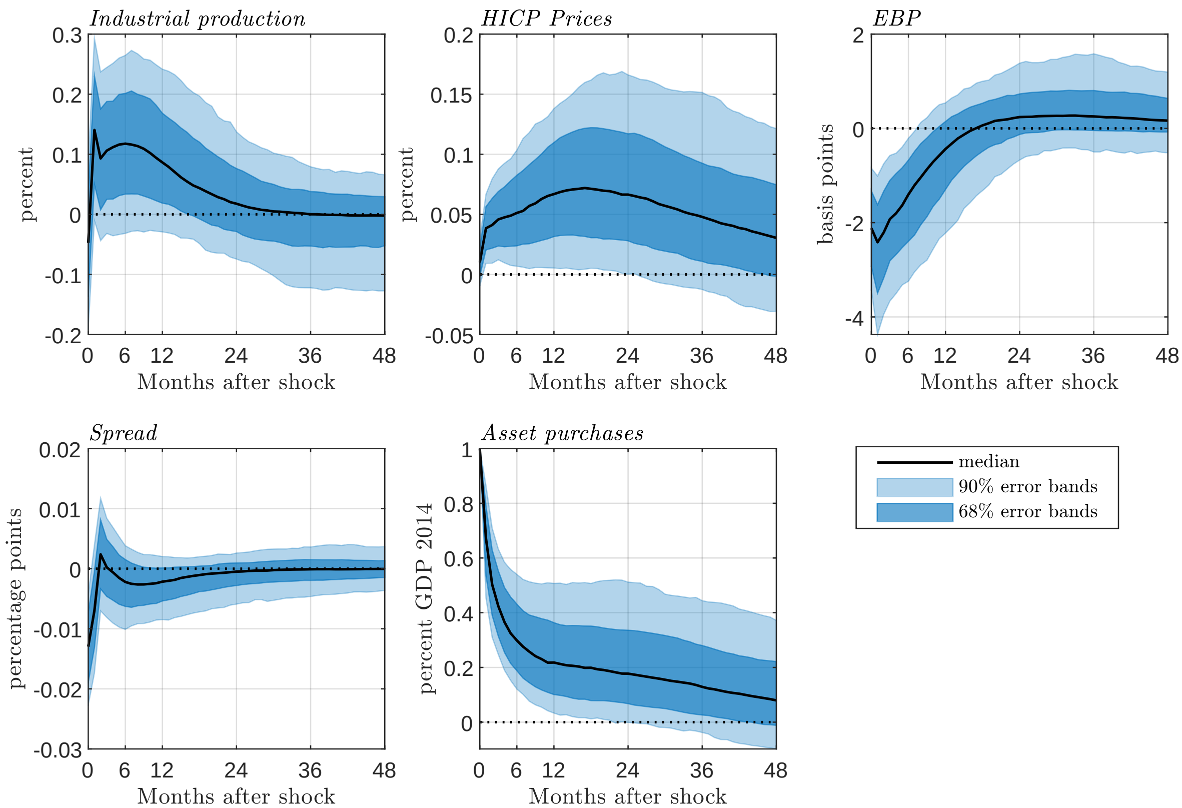
Note: The size of the shock is scaled to induce an immediate increase in asset purchases of 1% of GDP. In each panel, the median is reported in solid line, while the 68% and 90% error bands are shown by blue areas.

G.6. **Instrument purged from ECB information effects.** It is well known that monetary policy announcements reveal information not just about policy, but also sometimes about the central bank's assessment of the economic outlook (e.g., Miranda-Agrippino and Ricco, 2021; Jarociński and Karadi, 2020). Thus, our instrument for the identification of APP disturbances is potentially a mixture of the true policy shock and information about the state of the economy due to the information reported by the ECB action.

In this section, we adjust our proxy for central bank information effects by means of Jarociński and Karadi (2020)'s poor man sign restrictions. This restriction involves setting the APP surprise to zero in cases where stock price (Euro Stoxx 50) on announcement days move in the opposite direction as the surprise in APP. The resulting proxy is very close to our baseline proxy since there is only one announcement where the APP surprise and the stock prices do not positively co-move (October 2017).

Impulse responses are displayed in Figure G.6. They are indistinguishable from our baseline responses.

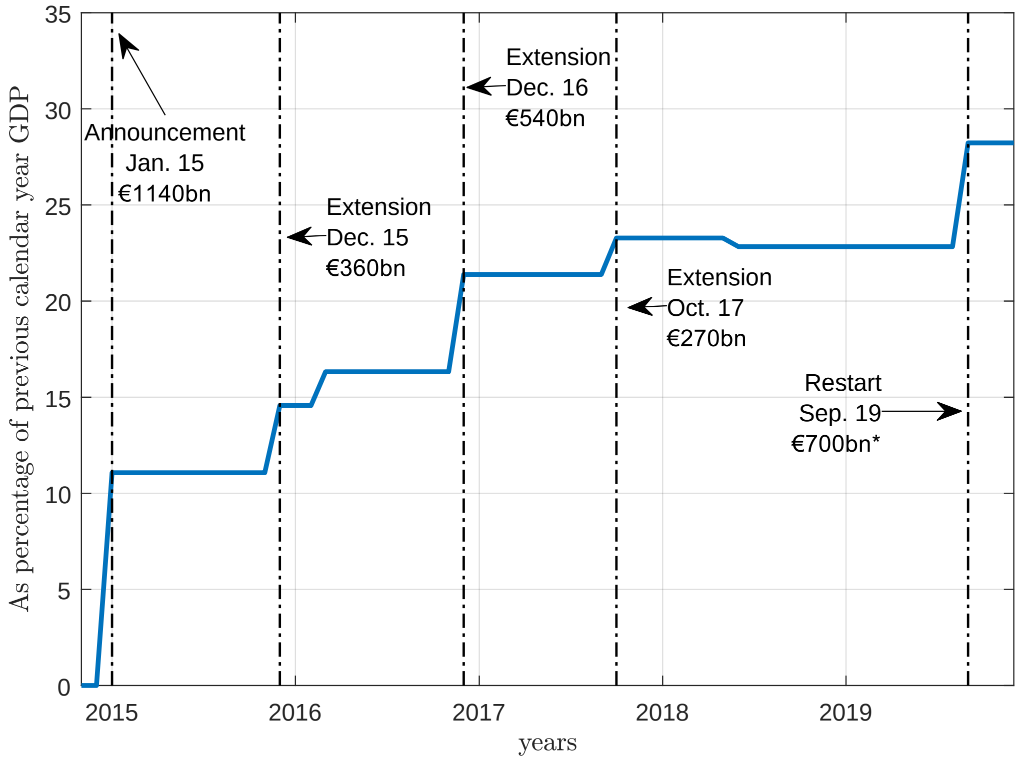
Figure G.6. Impulse responses to an APP shock that are purged from the effects of the ECB information



Note: The size of the shock is scaled to induce an immediate increase in asset purchases of 1% of GDP. In each panel, the median is reported in solid line, while the 68% and 90% error bands are shown by blue areas.

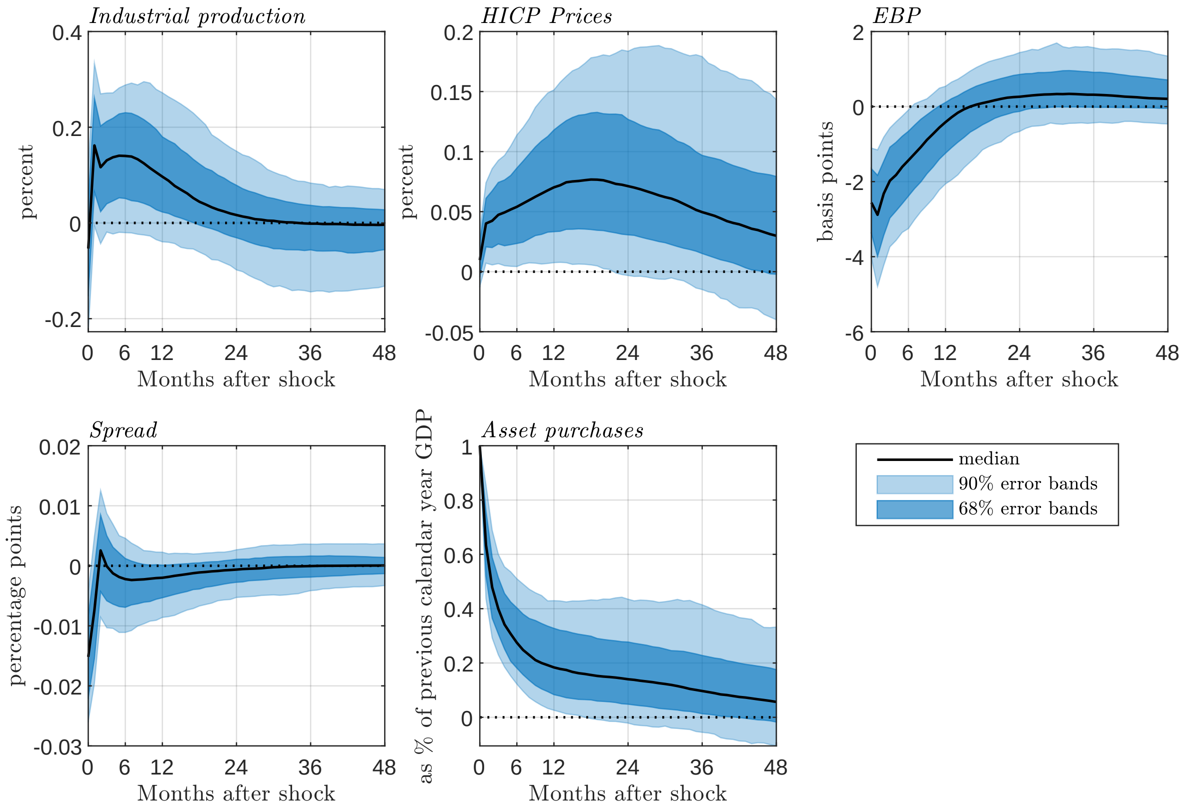
G.7. **Scaling with the previous calendar year GDP.** In this section, we explore an alternative scaling of our policy variable. Instead of scaling the cumulative amount of asset purchases announced by 2014 nominal GDP, we do it by the previous calendar year's GDP (Figure G.7). It may be worth mentioning that the series is very similar to the one previously used in the baseline analysis. The only difference occurs in June 2018, where the new time series exhibits a minor decline. It seems that the ratio of APP to GDP (of previous calendar year) were higher prior to that of the announcement in June 2018. We have also applied this new scaling to construct the surprises of APP. Our results remain unchanged with this new scaling procedure. In particular, impulse responses yield extremely similar results, as shown in Figure G.8.

Figure G.7. Total APP size, as announced by the ECB.



Note: Sample period: 2014.M11-2019.M12.

Figure G.8. Impulse responses to an APP shock - Scaling with the previous calendar year's GDP



Note: The size of the shock is scaled to induce an immediate increase in asset purchases of 1% of GDP. In each panel, the median is reported in solid line, while the 68% and 90% error bands are shown by blue areas.

Figure H.1. US Asset Purchase Announcements.

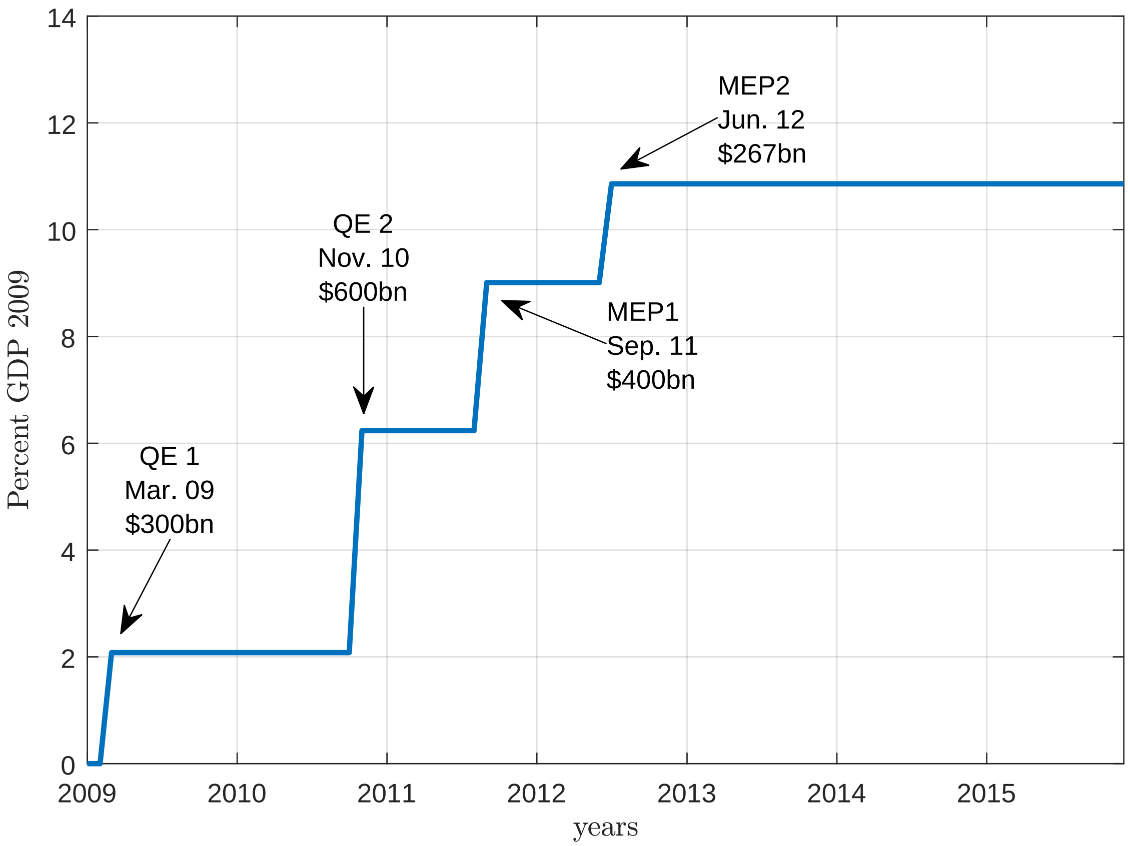


Figure H.2. US Proxy.

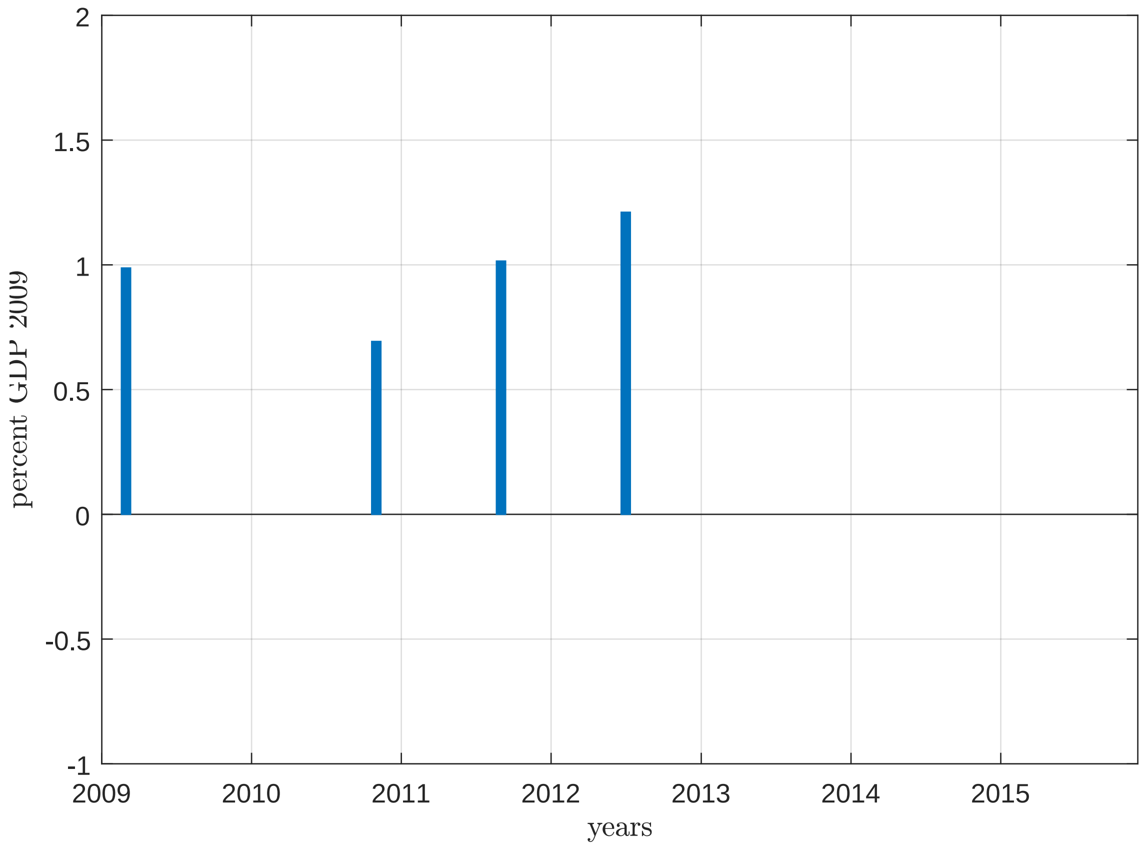
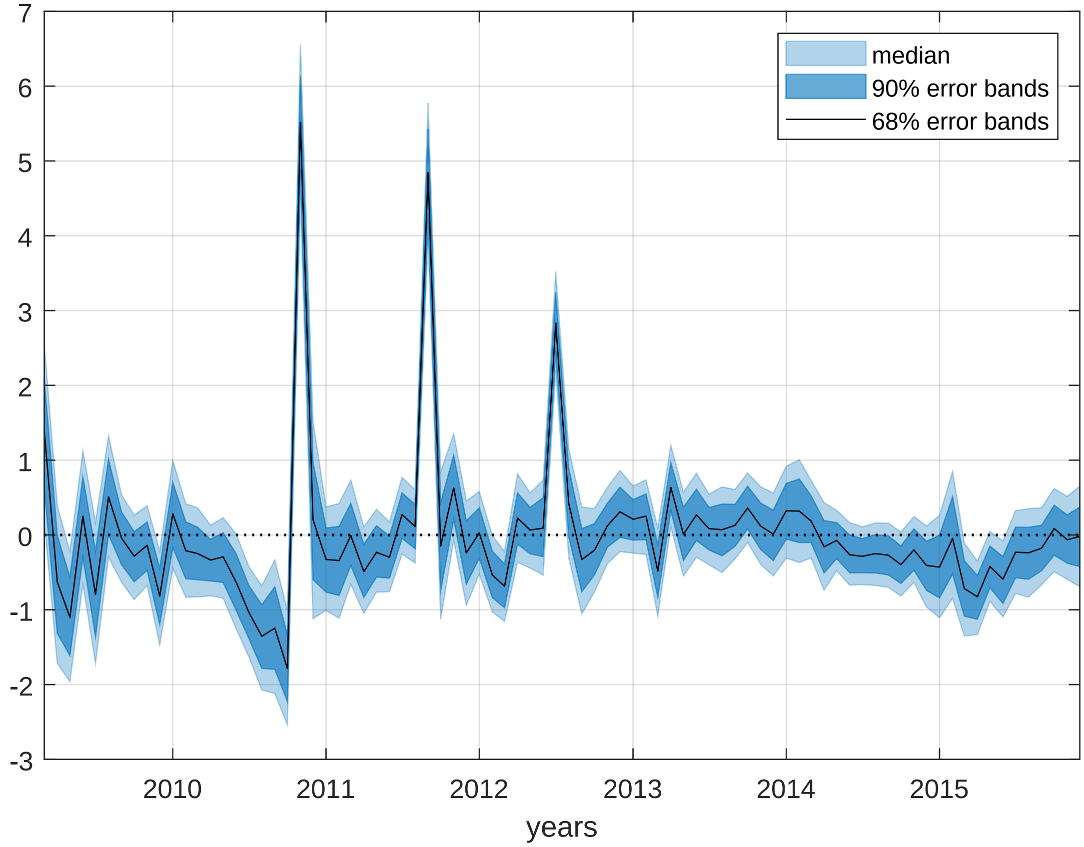
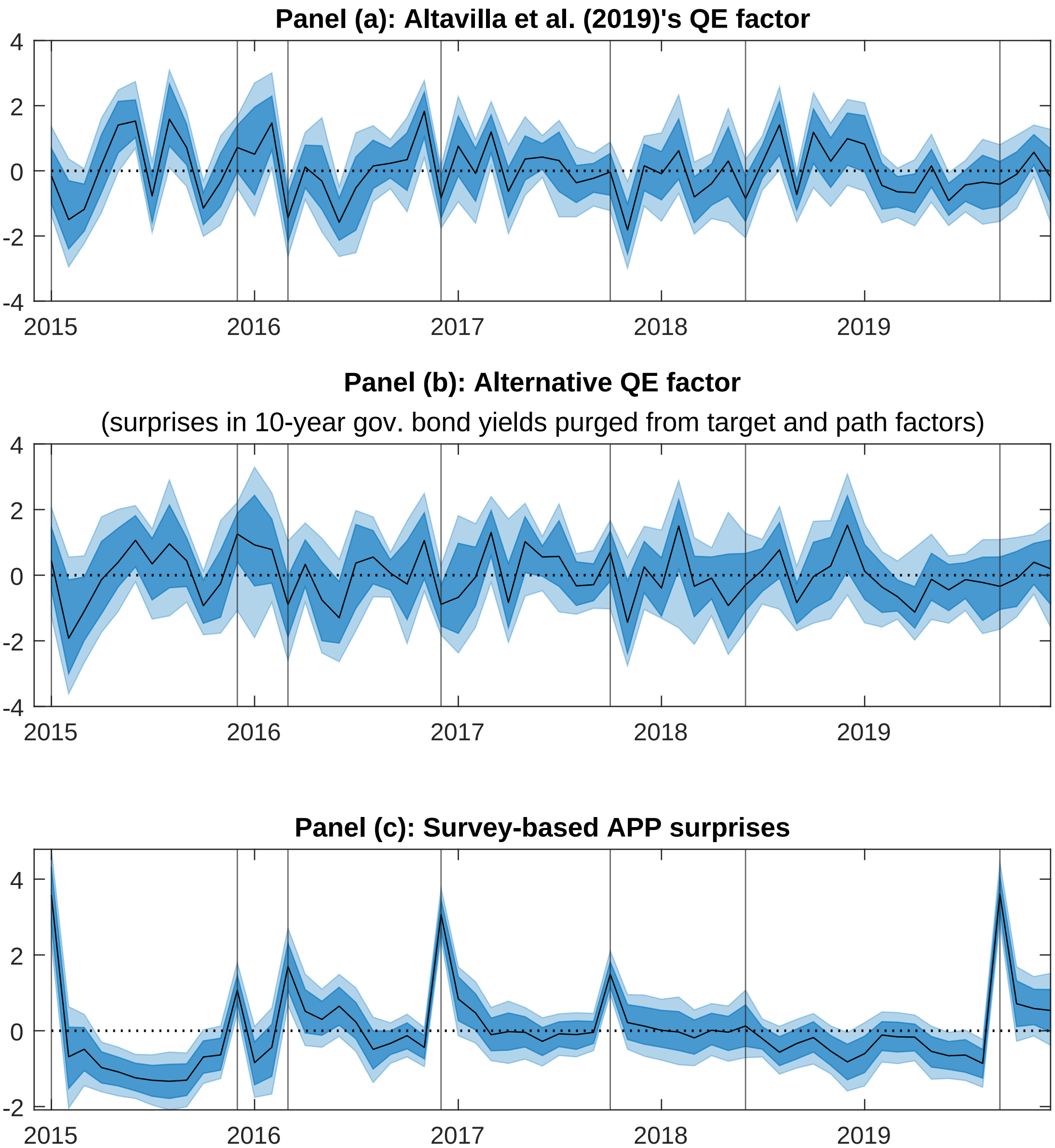


Figure H.3. US APP Shocks.



Appendix I. APP shocks associated with each instrument

Figure I.1. Comparison of APP shocks associated with each instrument



Note: Panel (a), (b), and (c) report APP shocks associated with the Altavilla, Brugnolini, Gürkaynak, Motto, and Ragusa (2019)'s QE factor, the alternative factor (purged from target and path factors), and our survey-based instrument, respectively. To convey the same information, the two former APP shocks (Panel (a) and (b)) should go in opposite directions from the latter APP shocks (Panel (c)).