DIW BERLIN

2163

Discussion Papers

Deutsches Institut für Wirtschaftsforschung 2026

Reassessing Proxy-based Identification of Multiple Monetary Policy Shocks for the Euro Area, the US, and the UK

Martin Bruns, Helmut Lütkepohl and James McNeil

IMPRESSUM

DIW Berlin
German Institute for Economic Research
Anton-Wilhelm-Amo-Str. 58
10117 Berlin

Tel. +49 (30) 897 89-0
Fax +49 (30) 897 89-200
http://www.diw.de

ISSN electronic edition 1619-4535

Papers can be downloaded free of charge from the DIW Berlin website:
http://www.diw.de/discussionpapers

Discussion Papers of DIW Berlin are indexed in RePEc and Econstor:
http://ideas.repec.org/s/diw/diwwpp.html
https://www.econstor.eu/handle/10419/10

Reassessing Proxy-based Identification of Multiple Monetary Policy Shocks for the Euro Area, the US, and the UK

by

Martin Bruns
University of East Anglia, School of Economics,
Norwich Research Park, NR4 7TJ, Norwich, United Kingdom
Email: martin.j.bruns@gmail.com

Helmut Lütkepohl
DIW Berlin and Freie Universität Berlin, Boltzmannstr. 20, 14195 Berlin, Germany
Email: luetkepohlh@gmail.com

James McNeil
Department of Economics, Dalhousie University, Halifax, Nova Scotia, Canada
Email: mcneilj@dal.ca

This version: April 28, 2026[1]

Abstract. Several recent studies consider a set of proxies to identify different monetary policy shocks for different regions in the world. We show that the way the proxies are used to identify the monetary policy shocks may lead to correlated shocks and dubious structural analysis and we demonstrate how to overcome the problem of correlated shocks. We illustrate that, if correlated shocks are used in applied studies, key statistics of interest such as impulse responses and forecast error variance decompositions can be severely distorted and we consider benchmark studies on monetary policy in the euro area (EA), the US and the UK to demonstrate the problems.

Key Words: Structural vector autoregression, proxy VAR, GMM, correlated structural shocks
JEL classification: C32

[1]The authors thank Gregor Smith, Robin Braun, and Silvia Miranda-Agrippino for helpful comments on earlier versions of this paper. Earlier versions of the research were presented at the Statistics group seminar at University of East Anglia, 12 March 2026, and at the annual meeting of the Econometrics Committee of the Verein für Socialpolitik in Berlin, 19 - 21 March 2026.

1 Introduction

In recent empirical studies of monetary policy, structural vector autoregressive (VAR) type methods are standard analysis tools. In those studies it has become increasingly popular to explicitly account for the different nature of monetary policy shocks due to the range of instruments at the disposal of central banks. For example, for the euro area (EA), Altavilla, Brugnolini, Gürkaynak, Motto and Ragusa (2019) consider four potentially different monetary policy shocks, a Target, a Timing, a Forward Guidance (FG), and a Quantitative Easing (QE) shock. For the US, Swanson (2021) distinguishes between a Fed funds rate (FFR) shock, a FG shock and a large scale asset purchases (LSAP) shock while for the UK, Braun, Miranda-Agrippino and Saha (2025) look at Bank rate (BR) shocks, communication shocks, and QE shocks. All these studies have appeared recently in the *Journal of Monetary Economics* and they all use proxy variables to identify the various monetary policy shocks which are constructed from high frequency data around central bank announcements. For all three geographical regions, similar high-frequency financial data around central bank announcements is available in publicly accessible and regularly updated datasets (see Appendix A.1). This suggests that proxies constructed from such data will be extensively used in future research.

In this study, we show that the identification strategies used to recover the different monetary policy shocks may lead to correlated shocks which are inconsistent with the standard assumptions for a structural VAR analysis. If the structural shocks are correlated, standard VAR tools such as impulse responses, forecast error variance decompositions (FEVDs) and historical decompositions will typically be distorted, in some cases severely so, and cannot be interpreted in the usual way. For example, if FEVDs are computed in the usual way, the sum of the forecast error variance components due to the different shocks may not add up to 100% and, hence, cannot be interpreted as shares of the forecast error variance explained by a specific shock. One may, of course, argue that such a problem will become immediately obvious and can perhaps be accounted for in the interpretation of the shares of the forecast error variance explained by specific shocks. Unfortunately this may not be the case if the shocks are only partially identified such that only the shares of the forecast error variances of a subset of shocks is available which may all be between 0 and 100% although the sum of the forecast error variance components of all shocks including the unknown, unidentified ones may be smaller or larger than 100%. Clearly, there are good reasons why Sims (1980) required the shocks to be uncorrelated in his seminal article promoting VAR models for macroeconomic analysis.

It may also be worth emphasizing that the proxies being uncorrelated is not sufficient for the different shocks to be uncorrelated. In fact, in Section 3 we will show that correlated shocks may be obtained even when the identifying proxies are uncorrelated. To understand why this problem occurs, consider two proxies z_{it}, $i = 1, 2$, which are each equal to an associated shock, w_{it}, plus a measurement error, e_{it}, such that $z_{it} = w_{it} + e_{it}$, $i = 1, 2$. But if the measurement errors e_{1t} and e_{2t} are correlated, then the estimated shocks w_{1t} and w_{2t} will have to be correlated for the proxies z_{1t} and z_{2t} to be uncorrelated. In monetary policy analysis, such correlation between the measurement errors may be due to measuring all the proxies on the same announcement days where specific economic conditions are prevailing.

It is important that valid economic analysis avoids correlated shocks. While orthogonality of the structural shocks has traditionally been treated as an identifying assumption, this condition has not been imposed systematically for VARs identified by proxies.[2] One way to do so is to use a GMM estimation approach that takes the uncorrelatedness of the shocks explicitly into account. A method that can be used for this purpose was proposed by Bruns, Lütkepohl and McNeil (2025). It can be applied if suitable identifying assumptions are used as they are often considered explicitly or implicitly in monetary policy analysis based on multiple proxies.[3]

We use the studies by Altavilla et al. (2019), Swanson (2021) and Braun et al. (2025) as benchmark studies to illustrate the problems related to correlated shocks. We show that all three studies use correlated shocks in some of their empirical analyses and it makes a difference if uncorrelated shocks are used instead. The three benchmark studies represent a range of different setups, none of which are immune to getting correlated shocks. For example, Altavilla et al. (2019) uses daily data while Braun et al. (2025) uses monthly data and Swanson (2021) uses data at the frequency of FOMC meetings. As already mentioned, the data are from different regions in the world. Moreover, the three studies consider different estimation techniques. While Altavilla et al. (2019) and Braun et al. (2025) use standard proxy VAR estimation techniques, Swanson (2021) applies local projections (LPs) for structural estimation. Furthermore, in Altavilla et al. (2019) all shocks are identified while in the other two studies partial identification is used. In other words, correlated shocks can be obtained in a wide range of setups used in structural VAR analysis.

Our focus on the properties of the identified shocks follows in a long line of monetary policy papers, dating at least to Rudebusch (1998), who showed that shocks identified from different VARs displayed only a low correlation with each other and with shocks derived from financial markets. Such low correlations indicate that at least some of these models are misspecified, which he used to cast doubt on their findings. Our contribution is to show instead that shocks identified in the *same* proxy VAR can be highly correlated, violating a basic assumption of the structural VAR. Such high correlations indicate that at least some of the identified shocks are problematic and conclusions based on those shocks may be misleading.

We find that for the EA between 2014 and 2018, a model with correlated shocks (and considering the standard estimate of the forecast error variance component) suggests that over 80% of the forecast error variance in the expected overnight policy rate could be attributed to Target shocks. Instead, when the GMM approach is used to enforce uncorrelated shocks, this proportion falls to less than 10%. This suggests that a model with correlated shocks overstates the importance of Target shocks for the expected policy rate by a factor of eight. For the US between 2015 and 2019, models with correlated shocks suggest that between 10% and 19% of the forecast error variance of 30-year treasury bills is explained by FFR shocks. Instead, if the model is estimated by GMM to obtain uncorrelated shocks it attributes only a share of less than 3% to FFR shocks. Hence, for the US the importance of FFR shocks for the long end of the yield curve is overstated by a factor of more than three. For the UK, we also find evidence for correlated shocks if uncorrelatedness is not explicitly taken into account. These correlations lead to some quantitative differences in FEVDs compared

[2]A non-exhaustive list of notable exceptions includes Mertens and Ravn (2013) and Lakdawala (2019).

[3]Alternative methods include the maximum likelihood approach proposed by Angelini and Fanelli (2019) and the Bayesian approach proposed by Hou (2024).

to a model with uncorrelated shocks but change the qualitative conclusions in a more limited way. It is therefore unclear *ex ante* how severely correlated shocks distort key statistics of interest.

Our paper is structured as follows. In the next section we present the basic model setup and identification of the structural shocks based on proxies. We discuss under which conditions the identified shocks will be correlated and why this is problematic when using standard VAR tools for structural analysis. We also briefly present the GMM approach that is designed so as to avoid correlated shocks. In Section 3 we illustrate the problems associated with correlated shocks by presenting the empirical studies and Section 4 concludes.

2 Model Setup

2.1 The Model

We adapt the model setup from Bruns et al. (2025) for the purposes of this study. The data generating process (DGP) is assumed to be the following K-dimensional reduced-form VAR process,

$$y_t = \nu + A_1 y_{t-1} + \cdots + A_p y_{t-p} + u_t = (\nu, A_1, \ldots, A_p)Y_{t-1} + u_t, \tag{1}$$

where $(\nu, A_1, \ldots, A_p)$ are the reduced-form slope coefficients, $Y_{t-1} = (1, y'_{t-1}, \ldots, y'_{t-p})'$ is a $(Kp+1)$-dimensional column vector and u_t is a zero-mean white noise process with covariance matrix Σ_u, i.e., $u_t \sim (0, \Sigma_u)$. We acknowledge that in some studies authors use local projections to estimate effects of shocks and may not explicitly assume a VAR DGP. However, if they are interested in the transmission of shocks they implicitly assume a dynamic model as the DGP. The problems of interest here are more easily understood in the VAR context. Therefore we use this model framework here.

The vector of structural shocks, $w_t = (w_{1t}, \ldots, w_{Kt})'$, is obtained by a linear transformation, $w_t = B^{-1}u_t$ from the reduced-form errors, u_t, where the $(K \times K)$ matrix $B = [b_{ij}]$ contains the impact effects of the structural shocks. The shocks are assumed to be instantaneously uncorrelated. Hence, the transformation matrix B has to be such that the covariance matrix $\Sigma_w = B^{-1}\Sigma_u B^{-1\prime}$ of the shocks w_t is diagonal. The impact effects, B, are the structural parameters of the model. Given the reduced form VAR and the matrix B, we can compute the structural shocks as $w_t = B^{-1}u_t$.

In practice the option to compute the shocks in this way is not available if the shocks are only partially identified. Each column of B contains the impact effects of a single shock on all the K variables. In the partially identified case, only some columns of B are available. In Sec. 3 we will sometimes face that situation. Without loss of generality we assume that only the first $K_1 \leq K$ shocks are of interest and properly identified. If $K_1 < K$, we partition the vector of shocks correspondingly as $w'_t = (w_t^{(1)\prime}, w_t^{(2)\prime})$, where $w_t^{(1)}$ and $w_t^{(2)}$ are $(K_1 \times 1)$ and $((K - K_1) \times 1)$ vectors, respectively. We denote the columns of B associated with $w_t^{(i)}$ by B_i, $i = 1, 2$, and partition the impact effects matrix as $B = [B_1 : B_2]$, where B_1 is $(K \times K_1)$ and B_2 is $(K \times (K - K_1))$.

When only a subset of the columns of B are identified such that $w_t = B^{-1}u_t$ is not available to compute the structural shocks, one may still recover the shocks associated with individual columns of B. Denoting by b_k the k-th column of B, the k-th shock can be obtained from the reduced-form residuals as

$$w_{kt} = b_k' \Sigma_u^{-1} u_t / b_k' \Sigma_u^{-1} b_k \tag{2}$$

(see, e.g., Stock and Watson (2018), Bruns and Lütkepohl (2022, Appendix A.1)). Note, however, that the derivation of this formula crucially depends on the structural shocks, w_t, being instantaneously uncorrelated (Σ_w being diagonal). If B is such that the corresponding shocks are correlated, then the shocks computed by Eq. (2) are not the desired structural shocks.[4] Hence, if the quantities computed with Eq. (2) are correlated, then they cannot be the shocks of interest because they violate a basic condition of our model setup.

Even if the true structural matrix B_1 ensures uncorrelated structural shocks, the estimated empirical shocks may be correlated if orthogonality of the shocks is not enforced in the estimation of the columns of B_1. For example, in practice columns of B_1 are sometimes estimated one-by-one using the proxies individually which is then likely to result in correlated shocks. That fact is important to note because in the empirical models we study in Sec. 3, we will deal with this situation.

2.2 Identification via Proxy Variables

Identification of the structural parameters and, hence, the structural shocks can be done by instrumental variables, often called proxies in the related literature. These are external variables satisfying suitable conditions to qualify as proxies. To identify the k-th shock, w_{kt}, by a proxy z_{kt}, the latter has to satisfy the following relevance and exogeneity conditions:

$$\mathbb{E}(w_{kt} z_{kt}) = \sigma_{w_k z_k} \neq 0, \quad \text{(relevance)}, \tag{3}$$
$$\mathbb{E}(w_{it} z_{kt}) = 0, \quad \forall i \neq k \quad \text{(exogeneity)}. \tag{4}$$

If these conditions are satisfied,

$$\mathbb{E}(u_t z_{kt}) = B\mathbb{E}(w_t z_{kt}) = \sigma_{w_k z_k} b_k. \tag{5}$$

In other words, the covariance of the reduced-form residuals and the k-th proxy is just a scalar multiple of the k-th column of B. Thus, we can estimate a scalar multiple of b_k as

$$\overline{\hat{u} z_k} = \frac{1}{T} \sum_{t=1}^{T} \hat{u}_t z_{kt}, \tag{6}$$

where the $\hat{u}_t$ are reduced-form least squares (LS) residuals.

If we have a valid proxy for each of the K_1 shocks of interest and estimate the impact effects one-by-one using (6), we can write the estimator for B_1 as

$$\overline{\hat{u} z} = \frac{1}{T} \sum_{t=1}^{T} \hat{u}_t z_t'. \tag{7}$$

[4] If B is such that $\Sigma_w = B^{-1} \Sigma_u B^{-1\prime}$ is not a diagonal matrix, then using $\Sigma_u = B \Sigma_w B'$ and denoting the k-th column of the $(K \times K)$ identity matrix I_K by e_k, $b_k' \Sigma_u^{-1} u_t = b_k' B'^{-1} \Sigma_w^{-1} B^{-1} u_t = e_k' \Sigma_w^{-1} w_t$ is a linear combination of w_t which is generally not equal to a scalar multiple of w_{kt}.

Note, however, that this estimator is suitable only if each proxy individually satisfies the relevance and exogeneity conditions (3)/(4). In that case the covariance matrix $\Sigma_{w^{(1)}z} = \mathbb{E}(w_t^{(1)} z_t')$ is a diagonal matrix. Even if that condition is satisfied, it is well known that the estimator (6) or (7) does not ensure uncorrelated shocks because uncorrelatedness of the shocks is not explicitly taken into account in the estimation procedure (see, e.g., Gregory, McNeil and Smith (2024)). Therefore we will estimate the impact effects matrix by the following GMM approach proposed by Bruns et al. (2025).

Using $\mathbb{E}(u_t z_t') = B\mathbb{E}(w_t z_t') = B_1 \mathbb{E}(w_t^{(1)} z_t')$, we get KK_1 moment conditions

$$\mathbb{E}(u_t z_t' - B_1 \Sigma_{w^{(1)}z}) = 0. \tag{8}$$

Moreover, $\Sigma_u = B\Sigma_w B'$ implies that

$$\mathbb{E}(B_1' \Sigma_u^{-1} u_t u_t' \Sigma_u^{-1} B_1) = [I_{K_1} : 0]\Sigma_w^{-1} \begin{bmatrix} I_{K_1} \\ 0 \end{bmatrix},$$

where I_{K_1} denotes the $(K_1 \times K_1)$ identity matrix. Diagonality of Σ_w provides a set of $\frac{1}{2}K_1(K_1 - 1)$ moment conditions

$$\mathbb{E}[\mathrm{vh}(B_1' \Sigma_u^{-1} u_t u_t' \Sigma_u^{-1} B_1)] = 0, \tag{9}$$

where $\mathrm{vh}(\cdot)$ is the vectorisation operator that collects the elements below the main diagonal of the square matrix in the argument in a column vector.

Note that there are $KK_1 + K_1^2$ parameters in B_1 and $\Sigma_{w^{(1)}z}$ and we have $KK_1 + \frac{1}{2}K_1(K_1 - 1)$ moment conditions in (8) and (9). Thus, in general there are fewer moment conditions than parameters to estimate. Hence, we have an under-identified estimation problem. We note, however, that the diagonal elements of $\Sigma_{w^{(1)}z}$ can be set to 1, without loss of generality, because the analyst is free to choose the size of the shocks. Of course, that will be reflected in the covariance matrix Σ_w. The diagonal elements of that matrix are not of interest in the GMM procedure, however, because they are not part of the parameter set estimated in that procedure. In the following we assume that $\Sigma_{w^{(1)}z}$ has a unit diagonal which means that there are at most $KK_1 + K_1(K_1 - 1)$ free parameters in B_1 and $\Sigma_{w^{(1)}z}$. Moreover, it may be possible to choose the proxies such that further restrictions can be imposed on $\Sigma_{w^{(1)}z}$. For example, if each of the proxies satisfies the relevance and exogeneity conditions (3)/(4) individually, then $\Sigma_{w^{(1)}z}$ is a diagonal matrix and, hence, a unit matrix under our assumption of unit diagonal elements. In that case, the number of free parameters reduces to KK_1 and the moment conditions become over-identifying for $K_1 \geq 2$ such that we can use GMM estimation of the unrestricted parameters.

Bruns et al. (2025) set up the GMM objective function $J(\eta)$, where η is the vector of unrestricted elements in B and $\Sigma_{w^{(1)}z}$, such that it accounts for the parameters of the reduced-form VAR model which can be estimated by LS in a first step. They have to be treated as nuisance parameters in the GMM objective function, $J(\eta)$, for efficient estimation, however, and for getting a standard asymptotic distribution of Hansen's J-test based on $J(\eta)$. The test has an asymptotic χ^2-distribution with as many degrees of freedom as there are over-identifying restrictions, if the moment conditions are specified correctly. We will use that GMM approach in the following section where we discuss the identification of monetary policy shocks and we will use the J-test to assess the validity of over-identifying restrictions.

The GMM procedure should eliminate or at least substantially reduce the correlation between the estimated shocks. Note that due to the over-identifying moment conditions, the minimum of the GMM objective function will be nonzero, i.e., $J(\hat{\eta}) > 0$. Hence, the empirical moment conditions will not hold exactly and the estimated structural shocks may still have some small correlation empirically. That issue will be relevant in the examples considered in the following.

3 Empirical Analysis

In this section we use examples from the literature to illustrate problems related to correlated structural shocks in empirical monetary economics. The examples are based on articles, in chronological order, by Altavilla et al. (2019), Swanson (2021) and Braun et al. (2025). We would like to emphasize that all three articles make important contributions to the literature and use a range of models and tools for their analysis. We only take up specific aspects of these studies and point out problems related to those aspects. We do not wish to create the impression that all their analysis is problematic.

All three studies use multiple proxies to identify a set of monetary policy shocks and they all use the same principles for constructing the proxies which were pioneered by Gürkaynak, Sack and Swanson (2005). The basic idea is that monetary policy announcements contain information about both conventional and unconventional monetary policy. Gürkaynak et al. (2005) propose to extract a small number of factors from changes in financial variables in a narrow window around monetary policy announcements using principal components analysis. These factors are then rotated to satisfy additional restrictions such that they have an economic interpretation. The factors are the basis for identifying and estimating different types of monetary policy shocks.

3.1 European Monetary Policy Analysis

Altavilla et al. (2019) investigate the impact of monetary policy in the EA by a proxy VAR model. They consider four shocks related to monetary policy to be identified by four proxies in a daily financial VAR model containing the following variables: the 2-year Overnight Index Swap rate (OIS2Y), the log euro-US dollar exchange rate (EURUSD), the log Euro Stoxx 50 stock index (STOXX), and the 2-year inflation linked swap rate (ILS2Y). They use their four proxies $z_t = (z_t^{Target}, z_t^{Timing}, z_t^{FG}, z_t^{QE})'$ one-by-one to identify four shocks, labelled Target, Timing, Forward Guidance (FG), and Quantitative Easing (QE) shocks. The Target shock is meant to capture conventional monetary policy action reflected in changes in the OIS2Y, the Timing and FG shocks are associated with central bank communication and capture short-term and medium-term guidance, respectively. Finally, the QE shock is relevant for the time of unconventional monetary policy starting in 2014. It captures the longer term assessment of the economic situation by the central bank.

The overall sample considered by Altavilla et al. (2019) is based on daily data from January 2002 till September 2018. They conduct a subsample analysis for three subperiods: Jan 1, 2002 - Dec 31, 2007, Jan 1, 2008 - Dec 31, 2013 and Jan 1, 2014 - Sept 19, 2018. The ILS2Y variable is not available for the first subperiod. Therefore only a three-dimensional model is considered for that subperiod and the QE shock is not identified, which may be justified as no such policy actions were undertaken by the ECB during that time. For all subperiods, VAR(12) models with constant terms are used as in Altavilla et al. (2019).

The proxies are based on rotated principle components on a set of changes in yields of risk-free rates of seven different maturities considering two intra-day windows on days of ECB Governing Council meetings. The first intra-day window covers the press-release time and the second window is the period of the press conference following each ECB Council meeting. Altavilla et al. (2019) find a single factor in the press-release window capturing primarily changes in short-term rates and, hence, is viewed as reflecting a Target shock representing conventional monetary policy action. Altavilla et al. (2019) find two factors in the press conference window before January 2014 when the ECB started using QE policy tools. For the full sample they find three factors. Upon rotation one of them is identified as a QE factor and the other two are regarded as communication factors, one of them related to short-term expectations (Timing) and the other one related to medium and longer term expectations (FG). The QE factor is not available for the first subperiod. For getting daily proxies from the factors which are available for ECB Council meeting days, the z_t^{Target}, z_t^{Timing}, z_t^{FG} and z_t^{QE} are set to the corresponding factor values on days of ECB Council meetings and zero elsewhere.

Table 1: Empirical correlations of proxies for EA

	Jan 1, 2002 - Dec 31, 2007			Jan 1, 2008 - Dec 31, 2013				Jan 1, 2014 - Sept 19, 2018			
	z_t^{Target}	z_t^{Timing}	z_t^{FG}	z_t^{Target}	z_t^{Timing}	z_t^{FG}	z_t^{QE}	z_t^{Target}	z_t^{Timing}	z_t^{FG}	z_t^{QE}
z_t^{Target}	1.000	0.036	0.096	1.000	-0.090	0.133	-0.055	1.000	0.094	0.253	0.147
z_t^{Timing}		1.000	-0.076		1.000	0.084	-0.106		1.000	-0.558*	0.428*
z_t^{FG}			1.000			1.000	-0.140			1.000	-0.015
z_t^{QE}							1.000				1.000

Note: The asterisk * denotes correlations which are significantly different from zero at the 5% level.

It is perhaps worth noting that, because the proxies are constructed using the full sample of data, they are only restricted to be instantaneously uncorrelated over the full sample and not for the individual subperiods. Therefore we present the correlations among the proxies for the three subperiods in Table 1. Clearly, some of the proxies are highly correlated, in particular in the third subperiod two of them are even significantly different from zero at a 5% level.[5] In that subperiod, z_t^{Timing} has correlation -0.558 with z_t^{FG} and correlation 0.428 with z_t^{QE}. Although our earlier discussion suggests that correlation between the proxies is no problem as long as each of the proxies is correlated with a single shock only, many researchers would take correlated proxies as a warning signal that the shocks cannot be identified one-by-one if no further identifying information or additional assumptions are used. Therefore it is worth keeping the correlation of the proxies in mind in the following analysis.

We mention that Martínez-Hernández (2020) and Ricco, Savini and Tuteja (2024)

[5] We have generated 95% confidence intervals for the correlations by the bootstrap method presented in the Online Supplement OS.3 of Bruns et al. (2025) using 10,000 bootstrap replications. If zero does not fall in the interval the correlation is called significant at the 5% level.

question the construction of the proxies and propose modifications to properly reflect monetary policy action in the EA. We will still focus on the Altavilla et al. (2019) proxies to compare our results to an already established benchmark.

Table 2: Correlations of EA shocks estimated by PVAR approach

	Jan 1, 2002 - Dec 31, 2007			Jan 1, 2008 - Dec 31, 2013				Jan 1, 2014 - Sept 19, 2018			
	w_t^{Target}	w_t^{Timing}	w_t^{FG}	w_t^{Target}	w_t^{Timing}	w_t^{FG}	w_t^{QE}	w_t^{Target}	w_t^{Timing}	w_t^{FG}	w_t^{QE}
	Shocks computed as $w_{kt} = b_k' \Sigma_u^{-1} u_t / b_k' \Sigma_u^{-1} b_k$										
w_t^{Target}	1.000	0.369*	0.900*	1.000	0.837*	0.852*	-0.396*	1.000	0.990*	0.979*	0.991*
w_t^{Timing}		1.000	0.656*		1.000	0.978*	-0.790*		1.000	0.959*	0.992*
w_t^{FG}			1.000			1.000	-0.796*			1.000	0.972*
w_t^{QE}							1.000				1.000
	Shocks computed as $w_t = B^{-1} u_t$										
w_t^{Target}	1.000	0.677*	-0.939*	1.000	-0.246*	-0.731*	-0.898*	1.000	-0.558*	-0.618*	-0.173*
w_t^{Timing}		1.000	-0.802*		1.000	-0.442*	0.260*		1.000	0.444*	-0.634*
w_t^{FG}			1.000			1.000	0.712*			1.000	-0.332*
w_t^{QE}							1.000				1.000

Note: The asterisk * denotes correlations which are significantly different from zero at the 5% level.

We follow Altavilla et al. (2019) and use the proxies one-by-one to identify the three shocks for the first subsample and the four shocks for the second and third subsamples. This identification approach will be referred to as the PVAR (proxy VAR) approach in the following. The correlations between the estimated shocks are presented in Table 2. If the shocks are actually uncorrelated both possibilities to compute the shocks from the estimated impact effects presented in Sec. 2.1 are feasible because there are as many shocks as there are variables in each of the models. The first one is based on Eq. (2). As emphasized in Sec. 2.1, it requires that the shocks are uncorrelated. Looking at the numbers in the upper part of Table 2 the latter condition seems to be violated: All the shocks are highly correlated in all three subperiods and significantly so at a 5% level. In fact, the estimated correlations are very high, in particular in the last subperiod, where they all exceed 0.95 and in some cases are nearly one. This near perfect correlation suggests that there is in fact only a single source of variation driving all four of the shocks identified by the PVAR. In other words, the PVAR is in fact identifying the same shock regardless of which instrument is used. This is clear also from Fig. 5 of Altavilla et al. (2019), where the IRFs in the final subperiod (red dashed lines) are essentially identical up to a scalar multiple. This identified shock may be one of the four monetary policy shocks or some linear combination of the four shocks. In either case, this changes the economic interpretation of the findings considerably.

In the present situation, we also have the option to estimate the shocks directly as $\hat{w}_t = \hat{B}^{-1} \hat{u}_t$. We show the corresponding empirical correlations of these shocks in the lower part of Table 2. They are also very high and significant at the 5% level. Hence, the shocks violate a basic condition of a standard structural VAR analysis.

To overcome the correlation of the shocks, we can use the GMM method presented in Section 2.2 and impose uncorrelatedness of the shocks as an additional restriction. As mentioned in Section 2.2, in that case there are even over-identifying restrictions if we continue to assume that each proxy is only correlated to exactly one shock. We

Table 3: Correlations of EA shocks estimated by GMM approach

	Jan 1, 2002 - Dec 31, 2007			Jan 1, 2008 - Dec 31, 2013				Jan 1, 2014 - Sept 19, 2018			
	w_t^{Target}	w_t^{Timing}	w_t^{FG}	w_t^{Target}	w_t^{Timing}	w_t^{FG}	w_t^{QE}	w_t^{Target}	w_t^{Timing}	w_t^{FG}	w_t^{QE}
	Shocks computed as $w_{kt} = b_k' \Sigma_u^{-1} u_t / b_k' \Sigma_u^{-1} b_k$										
w_t^{Target}	1.000	-0.000	0.003	1.000	0.001	0.007	0.000	1.000	0.013	-0.054	0.016
w_t^{Timing}		1.000	-0.003		1.000	-0.002	0.001		1.000	0.019	0.009
w_t^{FG}			1.000			1.000	-0.024			1.000	0.025
w_t^{QE}							1.000				1.000
	Shocks computed as $w_t = B^{-1} u_t$										
w_t^{Target}	1.000	0.000	-0.003	1.000	-0.001	-0.007	0.000	1.000	-0.014	0.054	-0.017
w_t^{Timing}		1.000	0.003		1.000	0.002	-0.001		1.000	-0.019	-0.009
w_t^{FG}			1.000			1.000	0.024			1.000	-0.026
w_t^{QE}							1.000				1.000

Note: None of the correlations is significantly different from zero at the 5% level.

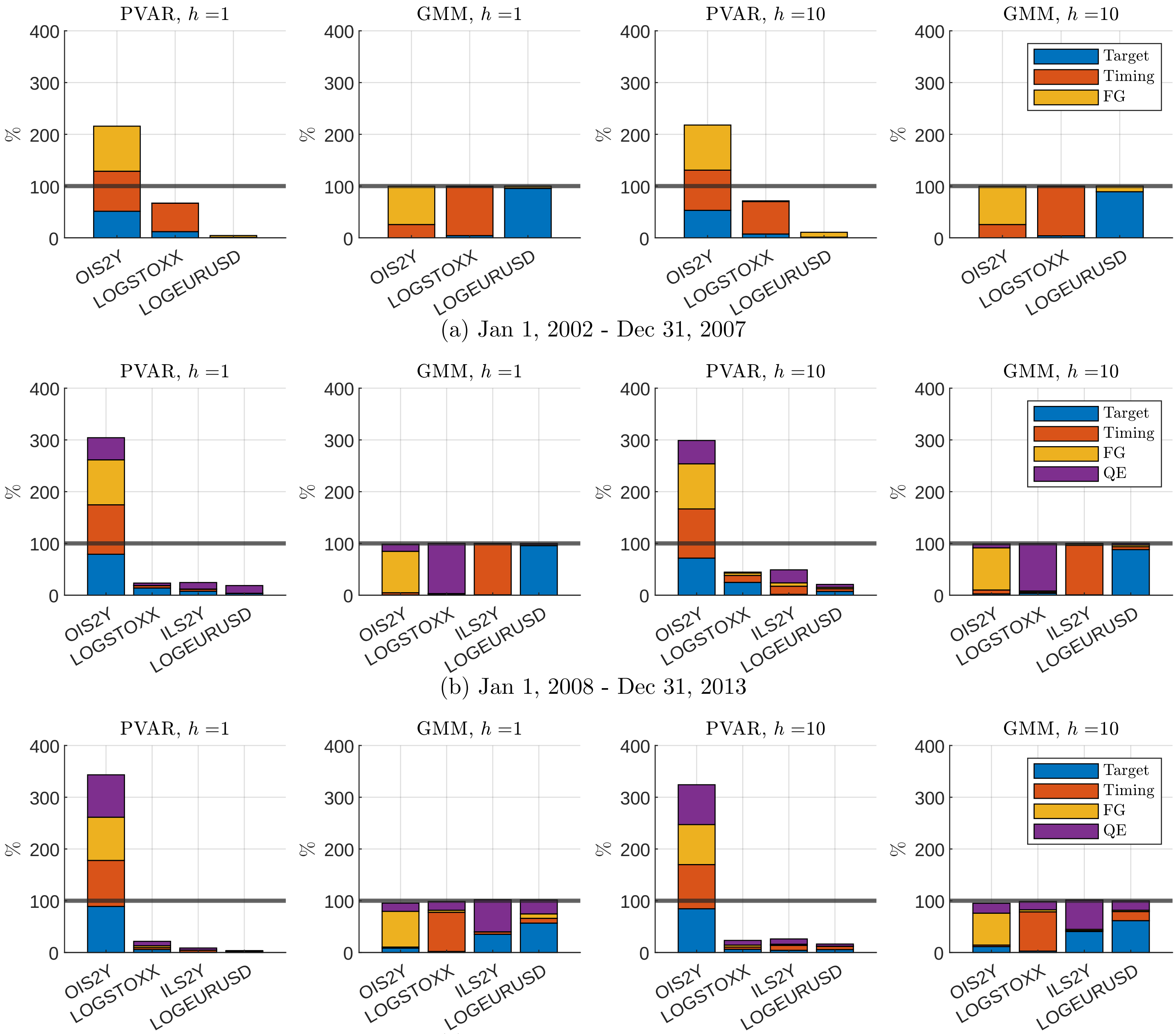
have estimated the shocks using this GMM procedure and show their correlations in Table 3, where again both ways to compute the shocks are presented. In this case, as the shocks are even empirically close to being uncorrelated, the two ways to compute the shocks should provide very similar results. Indeed all off-diagonal elements in all the correlation matrices are very similar apart from sign and close to zero in Table 3. Note that the sign of the shocks is not identified and has to be chosen by the user for a specific analysis. Therefore the signs of the correlations in Table 3 are somewhat arbitrary and are reversed because the shock signs are reversed when the shocks are computed as $\hat{w}_t = \hat{B}^{-1}\hat{u}_t$ instead of using Eq. (2). In any case, we now have shocks that at least satisfy the basic assumption of being uncorrelated.

Table 4: GMM J-test results for EA models

Jan 1, 2002 - Dec 31, 2007 (1563 obs)		Jan 1, 2008 - Dec 31, 2013 (1566 obs)		Jan 1, 2014 - Sept 19, 2018 (1227 obs)	
test stat	p-value	test stat	p-value	test stat	p-value
6.375	0.095	9.009	0.173	4.279	0.639

Now, as we have imposed over-identifying restrictions we can even check the underlying identifying assumption that each proxy is correlated with one shock only. Recall that this assumption was at least implicitly made by Altavilla et al. (2019) by using the proxies one-by-one to identify the corresponding shocks. These authors did not report evidence that the data support this assumption. Using our GMM approach it now becomes possible to formally test the assumption of each proxy being correlated with a single shock only. Test results based on the GMM J-test proposed by Bruns et al. (2025) for this purpose (see also Sec. 2.2) are presented in Table 4. All p-values are larger than 5%, and in the second and third subsamples substantially so. In the first subsample the p-value is 0.095 and, hence, close to 10% which one might interpret as mild evidence against the over-identifying restrictions.

Although a more detailed analysis of the over-identifying restrictions, e.g., by considering individual correlations between proxies and shocks, may produce some evidence against the over-identifying restrictions, it may be instructive to compare the implications of using the PVAR and GMM shocks for structural analysis. In particular, considering a FEVD may be of interest in this case because we have as many shocks as we have variables in the system such that the contributions of the shocks identified by proxies to the forecast error variances of each of the variables should add up to 100% for all forecast horizons in a standard FEVD based on uncorrelated shocks. We show the FEVDs for the PVAR and GMM shocks for forecast horizons $h = 1$ and $h = 10$ graphically in Fig. 1 and the precise numbers in Table A.1 in the Appendix.



(a) Jan 1, 2002 - Dec 31, 2007

(b) Jan 1, 2008 - Dec 31, 2013

(c) Jan 1, 2014 - Sept 19, 2018

Figure 1: EA forecast error variance decompositions (in percent)

An obvious difference between the PVAR and GMM FEVDs is that the former do not add up to 100%. For the OIS2Y interest rate, the sums of the contributions are substantially larger than 100%, e.g., 343.3% in the last subperiod, while for the other variables they are estimated to be substantially smaller than 100%, e.g., 21.5% for STOXX in the third subperiod. In sharp contrast, for the GMM estimates the forecast error variance components for each of the variables add up at least roughly to 100% in each subperiod, as they should. Clearly, forecast error variance components that do not add up to 100% across all shocks are difficult to interpret. For example, looking at the PVAR estimates of the last subperiod (Jan 1, 2014 - Sept 19, 2018) the forecast error variance contribution of the Target shock on the OIS2Y variable turns out to be 89.0% for forecast horizon $h = 1$, which indicates a substantial contribution of the Target shock to the variability of the two-year rate. Even if one takes into account that all the estimates of all the other shocks for this variable are of a similar magnitude, one might conclude that the impact of the Target shock on OIS2Y is substantial. Looking instead at the GMM estimates, it is seen that the orthogonalized Target shock contributes less than 10% (namely 8.5%) to the variance of OIS2Y. Clearly, that result sheds doubt on the interpretation of the shock as reflecting conventional monetary policy because that would be expected to have an immediate impact on the short end of the term structure. Instead, the FG shock explains by far the largest part of the forecast error variance of the variable at forecast horizon $h = 1$, namely 69.0% (see Table A.1). A very similar situation is also observed for longer forecast horizons (see, e.g., the results for $h = 10$ in Table A.1).

The PVAR FEVDs can also be very misleading when they add up to less than 100%, as, e.g., for STOXX. Looking again at the results for the last subperiod, one may get the impression from the PVAR estimates that specifically the Timing shock contributes very little to the development of the stock market whereas the GMM estimates disclose that the Timing shock contributes quite substantially to the forecast error variance of the STOXX variable. Generally, it makes a great difference whether one considers the correlated (PVAR) or nearly uncorrelated (GMM) estimates. Readers may draw their own conclusions from the results in Fig. 1. Given our FEVD results, it should also be clear that impulse responses and historical decompositions based on the PVAR estimates are highly problematic.

At this point it may be useful, however, to keep in mind that even the GMM estimates may not paint a realistic picture of EA monetary policy because the GMM estimates are based on the over-identifying assumptions used by Altavilla et al. (2019). As noted earlier, it is not clear that they can stand up against the data in a more detailed analysis. Of course, given that the identifying assumptions in addition with an assumption of uncorrelatedness of the shocks leads to over-identification, one could relax the assumption of each proxy being correlated with one shock only and instead assume that the proxies collectively identify the shocks but may be correlated with more than one shock. In that case one would need to make further assumptions about how to identify the shocks individually. Such further assumptions are not suggested by Altavilla et al. (2019) and therefore not considered here.

Another obvious problem in the context of the present analysis is that in each of the subsamples, models are considered with as many monetary shocks as there are endogenous variables. That implies that all the variables are fully determined by the monetary shocks. Such an assumption may be questioned for variables such as the stock index. One might conjecture that stock prices are not fully determined by monetary shocks but are also driven by events that happen in the real economy in between the central bank committee meetings.

3.2 US Monetary Policy

Our second example is based on Swanson (2021) who studies the effects of three US monetary policy shocks on various US bond yields and asset prices. One set of variables considered by Swanson (2021) are intradaily changes in bond yields at maturities of 6-months, and 2-, 5-, 10-, and 30-years in a 30-minute window around a monetary policy announcement.[6] These changes are the components of y_t. Hence, $K = 5$. Our data are at the frequency of FOMC meetings as in Swanson (2021). Thus, the subscript t for y_t refers to the periods of FOMC meetings. We also consider the same four sample periods as Swanson (2021): the full sample, which runs from July 1991 until June 2019, the pre-zero lower bound period from July 1991 until December 2008, the zero lower bound period from January 2009 until November 2015, and the post-zero lower bound period from December 2015 until 2019.

Swanson (2021) does not perform a standard structural VAR analysis based on proxies but he constructs three shock measures that may be viewed as proxies for US monetary policy shocks. He uses principal components constructed from the changes around FOMC meetings of a set of federal funds futures, Eurodollar futures, Treasury bond yields with different times to maturity, the S&P 500 and two exchange rates as a basis for three different proxies for monetary policy action: conventional policy based on the federal funds rate (z_t^{FFR}), forward guidance (z_t^{FG}), and large-scale asset purchases (z_t^{LSAP}). Three principle components are rotated such that the resulting three factors used as proxies are such that (a) changes in forward guidance have no effect on the current FFR, (b) changes in LSAPs have no effect on the FFR and (c) the LSAP factor is as small as possible in the period before the zero lower bound was reached by the interest rate. The factors are constructed to be orthogonal for the full sample period July 1991 until June 2019. Further details on the proxies are provided in Swanson (2021).

All three proxies, $z_t^{FFR}, z_t^{FG}, z_t^{LSAP}$, are relevant for the full sample and the last subperiod Dec 2015-June 2019. For subperiod Jul 1991-Dec 2008, only z_t^{FFR} and z_t^{FG} are relevant because there were no large scale asset purchases at that time and in the subperiod Jan 2009-Nov 2015 the FFR reached the zero lower bound such that only z_t^{FG} and z_t^{LSAP} are relevant in that subperiod.

Given the orthogonality of the proxy variables over the full sample, it makes perhaps sense to view them as measures of uncorrelated shocks and Swanson (2021) regresses each component of y_t on the set of his proxies considering the model

$$y_t = \nu + B_1 z_t + v_t, \tag{10}$$

where z_t contains the three proxies in the full sample and the last subperiod, while it includes only the relevant two proxies for the other subperiods. The matrix B_1 is accordingly a (5×3) or (5×2) matrix of impact effects of the relevant monetary policy shocks. Assuming for simplicity that we work with mean-adjusted variables y_t and z_t so that we can drop the intercept ν from model (10), the ordinary LS estimator of B_1 used by Swanson (2021) is

$$\hat{B}_1 = \left(\sum_t y_t z_t'\right)\left(\sum_t z_t z_t'\right)^{-1}.$$

[6]We thank Eric Swanson for providing the data.

Since the regressors z_t are orthogonal in the full sample such that $(\sum_t z_t z_t')$ is a diagonal matrix, the LS estimator $\hat{B}_1$ is equivalent (apart from scaling which is not relevant for the correlation of the shocks) to the PVAR estimator obtained from (7) if in that equation $\hat{u}_t$ is replaced by the mean-adjusted y_t. In other words, Swanson's estimator corresponds to the PVAR estimator for a VAR(0) model for the full sample. For the present data set, a VAR lag order zero can actually be justified because the BIC or SIC criterion (see, e.g., Kilian and Lütkepohl (2017, Section 2.6.3)) is minimized for lag order zero if a maximum order of $p = 8$ (the typical number of FOMC meetings over a year) is considered. Thus, for the full sample, we can interpret the estimator used by Swanson (2021) as a PVAR estimator for a VAR(0).

Table 5: Empirical correlations of proxies for US example

	Jul 1991-Jun 2019			Jul 1991-Dec 2008		Jan 2009-Nov 2015		Dec 2015-Jun 2019		
	z_t^{FFR}	z_t^{FG}	z_t^{LSAP}	z_t^{FFR}	z_t^{FG}	z_t^{FG}	z_t^{LSAP}	z_t^{FFR}	z_t^{FG}	z_t^{LSAP}
z_t^{FFR}	1.000	0.000	0.000	1.000	0.009	-	-	1.000	-0.174	-0.134
z_t^{FG}		1.000	0.000		1.000	1.000	-0.451*		1.000	0.243
z_t^{LSAP}			1.000	-	-		1.000			1.000

Note: The asterisk * denotes correlations which are significantly different from zero at the 5% level.

For the shorter subperiods (Jul 1991-Dec 2008, Jan 2009-Nov 2015, Dec 2015-Jun 2019) the proxies are in fact not empirically orthogonal. We present the actual correlations of the proxies for all subperiods in Table 5, where it can be seen that the proxies in the subperiods Jan 2009-Nov 2015 and Dec 2015-Jun 2019 actually have substantial empirical correlation. The correlation of z_t^{FG} and z_t^{LSAP} in the Jan 2009-Nov 2015 subperiod is even significantly different from zero at the 5% level if we use our bootstrap test mentioned in Footnote 5. As the proxies are not orthogonal in sample in the subperiods, Swanson's estimates will differ from the PVAR estimates based on a VAR(0). In the following we will call Swanson's estimates *direct* estimates to distinguish them from the PVAR estimates. Based on Swanson's model (10), the PVAR estimates correspond to estimating the columns of B_1 one-by-one by including only a single proxy in z_t at a time.

We have used the direct and the PVAR method to estimate the impact effects and the corresponding shocks based on Eq. (2). The correlations of the shocks are presented in Table 6. Note that only Eq. (2) is available here to compute the shocks because we have more variables ($K = 5$) than identified shocks. Clearly, the direct as well as the PVAR shocks are correlated and in some cases even significantly. We note that this occurs even over the full sample, despite the proxies being uncorrelated over the full sample. Therefore we have also computed the GMM estimates of the impact effects and show the correlations of the corresponding shocks also in Table 6. As expected, the correlations of the shocks estimated by GMM over the full sample are all very close to zero. Given that the shocks are over-identified, the correlations of the GMM shocks are not exactly zero, as explained earlier. However, for the subperiods some correlations of the GMM shocks are actually quite substantial and in one case even significantly different from zero based on a 5% level test (see the correlation of w_t^{FG} and w_t^{LSAP} in the subperiod Jan 2009–Nov 2015 in Table 6). That outcome suggests that the data resist the moment conditions underlying the GMM approach which may be based on invalid moment conditions in this case. In other words, our over-identifying assumption that each proxy is correlated with exactly one shock only may not be in line with the data.

Table 6: Correlations of shocks of US example

	direct			PVAR			GMM		
	w_t^{FFR}	w_t^{FG}	w_t^{LSAP}	w_t^{FFR}	w_t^{FG}	w_t^{LSAP}	w_t^{FFR}	w_t^{FG}	w_t^{LSAP}
Jul 1991–Jun 2019 (241 observations)									
w_t^{FFR}	1.000	0.333*	0.276*	1.000	0.333*	0.276*	1.000	-0.065	-0.014
w_t^{FG}		1.000	-0.304*		1.000	-0.304*		1.000	0.055
w_t^{LSAP}			1.000			1.000			1.000
Jul 1991–Dec 2008 (151 observations)									
w_t^{FFR}	1.000	0.272*	-	1.000	0.288*	-	1.000	-0.175	-
w_t^{FG}		1.000	-		1.000	-		1.000	-
Jan 2009–Nov 2015 (55 observations)									
w_t^{FG}	-	1.000	0.388*	-	1.000	-0.510*	-	1.000	0.386*
w_t^{LSAP}	-		1.000	-		1.000	-		1.000
Dec 2015–Jun 2019 (29 observations)									
w_t^{FFR}	1.000	0.239	0.655*	1.000	0.034	0.626*	1.000	0.039	-0.224
w_t^{FG}		1.000	-0.276		1.000	0.121		1.000	-0.068
w_t^{LSAP}			1.000			1.000			1.000

Note: The asterisk * denotes correlations which are significantly different from zero at the 5% level.

In order to assess our over-identifying assumptions, we can apply the J-test. Note, however, that there are different numbers of over-identifying restrictions in the different subperiods. There is one over-identifying restriction in the subperiods Jul 1991–Dec 2008 and Jan 2009–Nov 2015, while there are three over-identifying restrictions in the final subsample and the full sample period. Table 7 shows the results of the J-tests; the over-identifying restrictions over the full sample period and the first two subperiods are rejected at a 5% level. Of course, it is not surprising that the restrictions are rejected for the full sample period, given that they are rejected for two of the subperiods. Only in the last subperiod (Dec 2015–Jun 2019) the J-test does not reject at common significance levels, the p-value being 0.545.

The J-test rejecting the null hypothesis indicates general misspecification. However, Bruns et al. (2025) illustrate that the test has power against failure of the over-identifying restrictions, which occurs when some proxies are correlated with more than one shock. Thus, the basic identifying assumptions for the structural shocks are rejected by the data for the full period and the first two subperiods and, hence, the results embedded in the VAR framework are on soft grounds. Note that this also applies for Swanson's estimates although one could argue that they are not based on proxies but on actual shocks. In that case, one may not view the shocks obtained via Eq. (2) as relevant. If instead the proxies are regarded as shocks this does not solve the problem of working with correlated shocks because we know from Table 5 that the proxies are also instantaneously correlated, at least over the zero-lower-bound period and perhaps also in the post-zero-lower-bound period, the only subperiods where the quantitative easing shocks are identified.

Of course, if a set of identifying assumptions cannot stand up against the data, a

Table 7: Over-identification tests for US example

Period	J-statistic	df	p-value
Jul 1991–Jun 2019	11.518	3	0.009
Jan 1996–Dec 2008	3.829	1	0.050
Jan 2009–Nov 2015	6.288	1	0.012
Dec 2015–Jun 2019	2.132	3	0.545

natural response might be to use alternative models or assumptions for identification. In the present context this is of course an option. Indeed a number of other identifying assumptions for monetary policy shocks have been considered in the literature. As a J-test rejecting the null hypothesis may indicate that the VAR is misspecified, we have tried including a stock market index, and the euro-USD and the JPY-USD exchange rates, as well as experimenting with the lag order and find that our results are unchanged. There are of course many alternative specifications one might consider and we did not attempt a more thorough exploration. As we are just interested in using the example to illustrate our points regarding the use of correlated shocks and since the identifying restrictions are not rejected for the last subperiod, we focus on that subperiod in the following and maintain our identifying assumption.

For the last subperiod, we have calculated FEVDs and show them in Fig. 2. In the present VAR(0) setting, only the results for forecast horizon $h = 1$ are needed and, hence, given in the figure because the results for all other horizons are identical. As the estimates are based on data at the meeting level, we expect the total variability of especially the short-run bonds explained by the three identified shocks to be close to 100%. Since the shocks recovered from the direct and PVAR approaches are correlated, there is nothing to ensure that the total variation does not exceed 100%, and we see that that occurs for all five yields for the direct shocks and for the 6-month and 2-year yields for the PVAR shocks. There are also two cases where the sums of the contributions to the variances estimated by GMM very slightly exceed 100%, which occurs because the model is over-identified so that the moment conditions hold only approximately. The exact FEVD results are shown in Table A.2 in the Appendix.

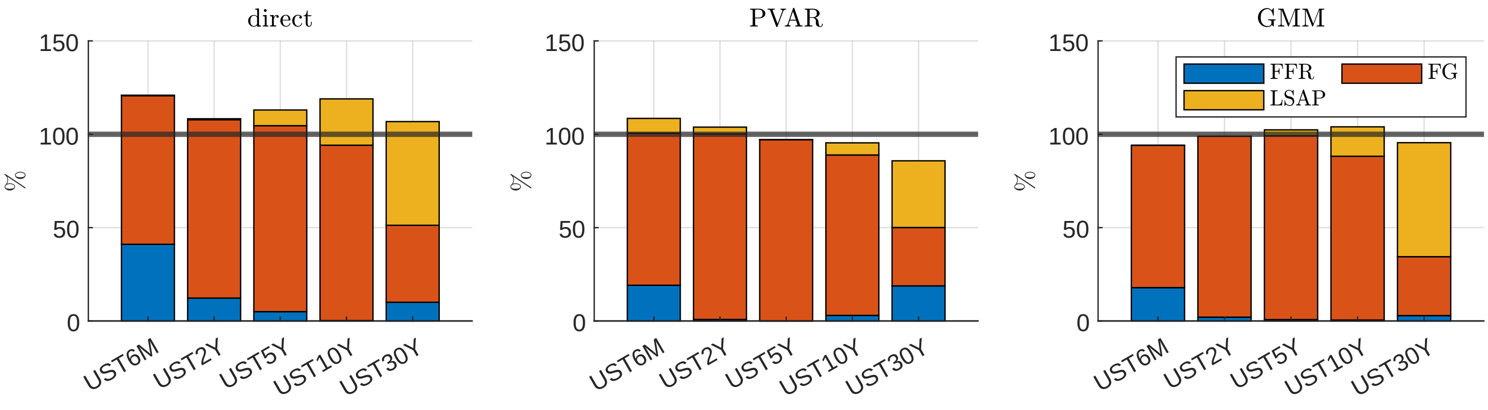


Figure 2: US Forecast error variance decompositions (in percent) for subperiod Dec 2015–Jun 2019 (29 observations), forecast horizon $h = 1$

In some cases the discrepancies between the FEVDs estimated by the different approaches are large in economic terms. The PVAR estimates a sizeable role for FFR shocks at the long end of the yield curve, accounting for more than 18% of the forecast error variance of 30-year bonds. By comparison, when the model is estimated by GMM, this estimate falls to only 2.9%. The GMM estimates also find a much larger role for LSAP shocks in the variation of long-run bonds compared to the PVAR estimates, rising from 6.5% to 15.8% for 10-year yields and 35.7% to 61.1% for 30-year yields. Another notable difference is that according to the direct estimates, FFR shocks explain a larger share of variance at the short end of the yield curve, with the estimated shares of 6-month and 2-year yields declining by more than half in each case when the model is estimated by GMM.

Overall this example demonstrates that even without using the VAR framework explicitly, problems with correlated structural shocks can arise if regression or local projection type methods are used that can be embedded in the structural VAR framework. In any case, caution is necessary in the interpretation of the results when multiple instruments, proxies or even artificially constructed shocks are used.

3.3 UK Monetary Policy

Braun et al. (2025) study UK monetary policy with proxy VAR models. They consider a monthly VAR model with lag order 12 with seven endogenous variables as their baseline model for the UK for a sample period 1997M01-2019M12. The variables are the bank rate (BR), the 1-year gilt yield (1YGY), a corporate bond spread (CBS), the FTSE All Share index (FTSE), a measure of monthly Gross Domestic Product (GDP), the consumer price index (CPI), and the nominal sterling exchange rate index (Ex). All variables not already expressed in percentage points enter in log levels. We use the data set of Braun et al. (2025) and refer the reader to that paper for further information on the data.

Braun et al. (2025) construct a target-proxy, z_t^{Target}, a path-proxy, z_t^{Path}, and a quantitative-easing-proxy, z_t^{QE}. All three are based on high-frequency revisions in asset prices in a narrow window around monetary policy announcements. The construction of these proxies proceeds as follows: First, the authors use a factor model based on four short sterling futures as well as the 2-, 5-, and 10-year gilts. From these seven series, the authors extract three factors: The target factor is designed to summarise the immediate policy rate decision, the path factor captures information about the future path of policy, and the QE factor captures the effects of quantitative easing announcements. Second, to disentangle the factors, the authors impose the following restrictions: The target factor is the only factor which loads on the short sterling futures. The variance of the QE factor is minimised over the pre-2009 sample, disentangling it from the path factor. At the policy-announcement frequency all three factors are orthogonal to each other. Lastly, to build monthly proxies from the factors (which are at policy-announcement frequency), Braun et al. (2025) construct three monthly measures which are equal to 0 in months with no monetary policy announcements, equal to the factor in months with one announcement and equal to the sum of the respective factor values in months with more than one policy announcement. The correlations of the proxies constructed in this way are presented in Table 8. They are not uncorrelated but have reasonably small correlations which are not significantly different from zero at a 5% level. The reason for the small non-zero correlation is that orthogonal factors at policy-announcement frequency are aggregated to the monthly level and because we are considering a subsample ending in 2019M12, while the orthogonality condition is imposed for a sample ending in

2023M12.

Table 8: Correlations of proxies for UK

	z_t^{Target}	z_t^{Path}	z_t^{QE}
z_t^{Target}	1.000	0.048	0.002
z_t^{Path}		1.000	-0.029
z_t^{QE}			1.000

Note: None of the correlations is significantly different from zero at the 5% level.

We consider two separate setups: The first setup, called the baseline setup in the following, includes the seven baseline variables of Braun et al. (2025) and two proxies, z_t^{Target} and z_t^{Path}, to recover the shocks w_t^{Target} and w_t^{Path}. In a second setup we use as an additional variable the 10-year-gilt yield (10YGY) and consider all three proxies to recover the three shocks jointly. We call this the extended setup and acknowledge that the latter setup was not used by Braun et al. (2025) who consider other tools for analyzing the shock transmission instead. We recover shocks based on Eq. (2) since in both setups the number of model variables exceeds the number of estimated shocks.

Table 9: Correlations of UK shocks estimated by PVAR

	Baseline		Extended		
	w_t^{Target}	w_t^{Path}	w_t^{Target}	w_t^{Path}	w_t^{QE}
w_t^{Target}	1.000	-0.251*	1.000	-0.296*	-0.529*
w_t^{Path}		1.000		1.000	-0.139*
w_t^{QE}					1.000

Note: Shocks computed as $w_{kt} = b_k' \Sigma_u^{-1} u_t / b_k' \Sigma_u^{-1} b_k$. The asterisk * denotes correlations which are significantly different from zero.

Table 9 reports the correlations between the shocks estimated using the PVAR approach. Although the proxies z_t^{Target} and z_t^{Path} show little correlation (see Table 8), the corresponding PVAR shocks show quite a high and significant at the 5% level absolute correlation in both the baseline and extended setups. Hence, we see as in the US example, that having uncorrelated proxies is no guarantee for uncorrelated shocks in the PVAR approach.

Given the correlation of the PVAR shocks, we have also applied the GMM procedure and show the correlations of the estimated shocks in Table 10. As these estimates are based on the over-identifying assumption that each proxy is correlated with one shock only we first note that the J-test for the baseline setup produces a test value of 0.896 and a corresponding p-value of 0.344 based on a $\chi^2(1)$-distribution, while the J-test value for the extended setup is 7.191 with a p-value 0.066 based on a $\chi^2(3)$-distribution. Hence, the J-test does not reject the over-identifying restrictions at the 5% significance level, although there is mild evidence against the restrictions for the extended model as the p-value is less than 10%. We will still proceed with the over-identifying restrictions for illustrative purposes for both models in the following.

Table 10 presents the correlations among the shocks obtained via the GMM approach. As expected, these correlations are all close to zero and not significantly

Table 10: Correlations of UK shocks estimated by GMM

	Baseline		Extended		
	w_t^{Target}	w_t^{Path}	w_t^{Target}	w_t^{Path}	w_t^{QE}
w_t^{Target}	1.000	0.029	1.000	0.040	0.057
w_t^{Path}		1.000		1.000	0.016
w_t^{QE}					1.000

Note: Shocks computed as $w_{kt} = b_k' \Sigma_u^{-1} u_t / b_k' \Sigma_u^{-1} b_k$. None of the correlations is significantly different from zero at the 5% level.

different from zero under a 5% level test. Thus, the GMM approach again remedies the issue of correlated shocks.

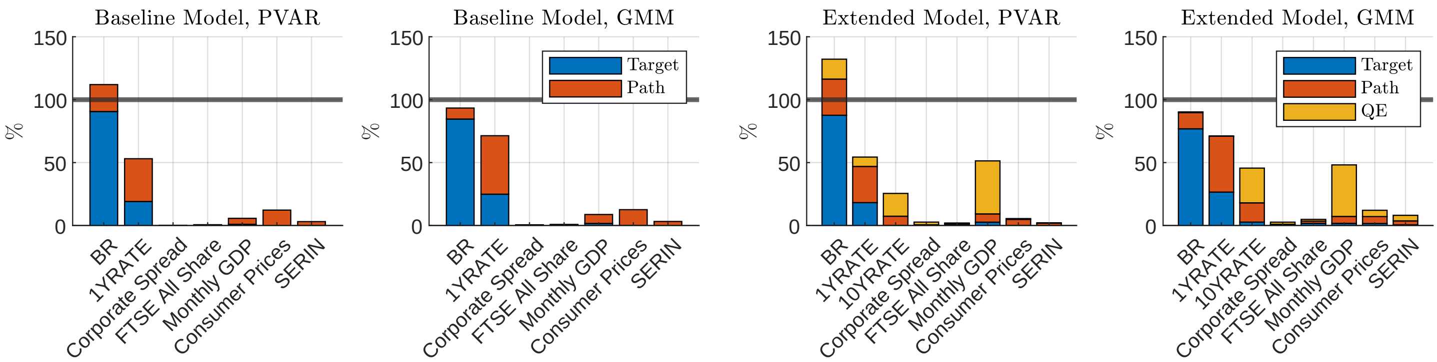


Figure 3: UK forecast error variance decompositions (in percent), forecast horizon $h = 1$.

For the present example, FEVD estimates obtained by the PVAR and GMM approaches are presented in Fig. 3 for the baseline and extended setups for forecast horizon $h = 1$. The first notable observation is that the forecast error variance shares across the identified shocks for the PVAR approach for the BR alone contribute more than 100% to the FEVD for both the baseline and the extended model. Recall that the baseline model contains seven variables and, hence, all seven shocks together should contribute 100% of the forecast error variance while here already the first two of them contribute more than 100% of the forecast error variance. Similarly, for the eight-dimensional extended model the contributions of three shocks exceed already 100%. Apart from that the PVAR and GMM estimates are such that one would draw similar conclusions from both sets of estimates, perhaps with some exceptions. For example, based on the PVAR approach one might assign a larger importance to Path shocks for BR than based on the GMM shocks. Interestingly, this observation can be made for both the baseline setup as well as for the extended setup. For the extended model, the PVAR setup, taken at face value, suggests a contribution of 15.8% of QE shocks to the FEVD of BR. Instead, the GMM approach attributes almost none of the FEVD of BR to QE shocks. The exact FEVD estimates are reported in Tables A.3 and A.4 in the Appendix. The results for forecast horizon $h = 10$ confirm that the estimates obtained from PVAR and GMM are qualitatively similar as can be seen in Fig. A.1 in the Appendix. Still, given that the PVAR shocks are correlated, interpreting the PVAR estimates is on shaky grounds. Of course, using proxies based on such extended data sets, it is important to make sure that the corresponding structural shocks are uncorrelated.

4 Conclusions

In empirical monetary economic analysis, multiple proxies are used in a number of recent structural VAR studies to investigate the impact of different tools of central banks such as interest rate setting, communication and quantitative easing. Proxies based on high frequency data are often considered to identify the corresponding monetary shocks one-by-one or in similar ways. We show that this type of identification strategy will typically lead to correlated structural shocks if orthogonality of the shocks is not explicitly imposed in the estimation of the structural parameters and we demonstrate that interpreting standard analysis tools such as FEVDs based on correlated shocks can be quite misleading.

We have used benchmark studies by Altavilla et al. (2019), Swanson (2021) and Braun et al. (2025) that consider monetary policy in the EA, the US and the UK, respectively, to illustrate our points and to demonstrate that the problems discussed actually come up in applied work. We find that the shocks considered in these three studies are correlated and that standard analysis tools invite misleading interpretations. Our benchmark studies cover a range of data sets with different characteristics. In fact, the data frequency ranges from daily to monthly and FOMC meeting frequency. The variables are financial quantities as well as macroeconomic data and they are from different regions in the world. The proxies are partly uncorrelated and in some cases correlated. The estimation methods used in the benchmark studies cover standard VAR approaches and LP techniques. Moreover, in one of the benchmark studies all shocks are identified by proxies while in the other two studies the shocks are only partially identified. Thus, the problems we have pointed out and illustrated cover a broad range of different scenarios considered in applied work. They are not only due to specific data frequencies or correlated proxies. In fact, they can arise whenever uncorrelatedness of the shocks is not properly enforced in the estimation procedure.

Although we have chosen benchmark studies from the *Journal of Monetary Economics*, it should be understood that there are further studies, partly published in other journals, for which our arguments are relevant. For example, other studies of monetary policy analysis using multiple proxies are Rogers, Scotti and Wright (2018), Jarociński and Karadi (2020), Andrade and Ferroni (2021) and Kubota and Shintani (2025). Moreover, our arguments are, of course, not limited to monetary policy studies but are equally relevant for other structural VAR studies that use multiple proxies. For example, other economic studies using multiple proxies to identify multiple shocks in structural VAR analyses are Stock and Watson (2012), Känzig (2021), Degasperi (2023) and Lunsford (2015). However, it is not our intention to question the results of all previous proxy VAR studies using multiple proxies. Some of them have, in fact, considered alternative methods or identification assumptions that ensure uncorrelated shocks (e.g., Mertens and Ravn (2013), Lakdawala (2019), Angelini and Fanelli (2019), and Hou (2024)). Even our benchmark studies make useful contributions in some dimensions. It is our hope, however, that the problems we have spelled out in this article will be avoided in future studies.

In addition to imposing the orthogonality conditions outlined in Section 2, identification of monetary policy shocks could be improved with more informative proxy variables. Disentangling valid proxies for target, forward guidance, and quantitative easing shocks from changes in financial data around monetary policy meetings is challenging because these meetings provide information about all three of these shocks, as discussed by Swanson (2021). One possible solution, then, is to augment these datasets with additional events which are more likely to distinguish between these three types of policies. For the US, Swanson and Jayawickrema (2023) and Swanson (2023) extend the standard set of FOMC meetings with speeches made by the Fed chair. They show that these speeches have more explanatory power than FOMC meetings for long-run assets, which is consistent with these speeches containing more information about policies impacting the long end of the yield curve, such as forward guidance and quantitative easing. Altavilla, Gürkaynak, Laeven and Kind (2025) perform a similar exercise for the EA.

We emphasize that our analysis has focussed on frequentist methods while many structural VAR studies use Bayesian VAR analysis. In that context, proxies are often internalized by including them in the set of endogenous variables (see Bruns and Lütkepohl (2025) for a formal comparison of using proxies as external versus internal instruments). If multiple proxies are available and they are included in the VAR one-by-one, the same problems of potentially correlated shocks will arise as discussed in the present study for the frequentist case. If all proxies are included at once and a recursive ordering of the shocks can be justified as sometimes assumed in large Bayesian VARs (see, e.g., Bańbura, Giannone and Reichlin (2010)) then uncorrelated shocks will be obtained. Note, however, that this is at the expense of additional identifying assumptions.

A Appendix

A.1 Data for Monetary Policy Proxies

Datasets and accompanying studies for monetary policy proxies in different regions:

Euro area: Altavilla et al. (2025), https://www.ecb.europa.eu/pub/pdf/other/EA-EMPD.en.xlsx

US: Acosta, Ajello, Bauer, Loria and Miranda-Agrippino (2025), https://www.frbsf.org/wp-content/uploads/USMPD.xlsx

UK: Braun et al. (2025), https://www.bankofengland.co.uk/-/media/boe/files/working-paper/2023/measuring-monetary-policy-in-the-uk-the-ukmpesd.xlsx

A.2 Additional Results

Table A.1: EA forecast error variance decompositions (in percent)

	Forecast horizon $h = 1$				$h = 10$			
	OIS2Y	STOXX	EURUSD		OIS2Y	STOXX	EURUSD	
PVAR				Jan 1, 2002 - Dec 31, 2007				
w_t^{Target}	51.3	11.9	1.0		53.1	7.4	0.4	
w_t^{Timing}	77.4	55.0	0.0		77.6	62.6	0.9	
w_t^{FG}	87.1	0.1	3.4		87.2	1.5	9.4	
Total	215.8	67.1	4.4		217.9	71.4	10.7	
GMM								
w_t^{Target}	0.0	4.3	95.5		0.1	4.0	89.0	
w_t^{Timing}	25.7	94.2	0.0		25.6	95.7	0.1	
w_t^{FG}	74.0	1.6	4.6		74.0	0.4	11.0	
Total	99.7	100.1	100.1		99.7	100.0	100.2	
	OIS2Y	STOXX	ILS2Y	EURUSD	OIS2Y	STOXX	ILS2Y	EURUSD
PVAR				Jan 1, 2008 - Dec 31, 2013				
w_t^{Target}	79.0	13.8	7.5	3.1	71.6	24.7	1.6	7.2
w_t^{Timing}	95.6	4.5	4.0	0.5	95.0	13.4	15.6	5.2
w_t^{FG}	86.9	0.2	0.2	0.0	87.2	4.7	6.5	2.7
w_t^{QE}	42.7	4.8	12.7	15.0	45.0	1.8	25.2	5.7
Total	304.2	23.4	24.4	18.6	298.8	44.5	48.9	20.8
GMM								
w_t^{Target}	0.5	1.7	0.7	95.7	2.5	3.5	0.7	88.1
w_t^{Timing}	4.2	1.2	98.4	1.3	7.5	2.3	95.8	5.5
w_t^{FG}	80.1	0.1	0.4	0.2	81.3	2.3	2.6	3.4
w_t^{QE}	13.7	97.1	0.6	2.9	7.6	91.4	0.9	3.2
Total	98.4	100.1	100.1	100.1	98.9	99.4	99.9	100.1
PVAR				Jan 1, 2014 - Sept 19, 2018				
w_t^{Target}	89.0	5.7	0.6	0.9	84.5	6.2	4.2	5.6
w_t^{Timing}	88.8	3.7	3.9	1.2	85.3	4.2	9.6	6.2
w_t^{FG}	83.6	3.7	0.1	1.1	77.3	3.9	2.2	0.4
w_t^{QE}	81.9	8.5	4.1	0.2	77.0	9.0	10.2	4.2
Total	343.3	21.5	8.7	3.4	324.1	23.3	26.2	16.3
GMM								
w_t^{Target}	8.5	1.9	35.2	56.8	11.5	2.6	40.5	61.5
w_t^{Timing}	2.0	75.7	4.3	9.2	2.9	75.9	1.7	17.5
w_t^{FG}	69.0	4.0	0.3	8.6	61.6	4.0	2.5	2.6
w_t^{QE}	15.9	17.0	62.0	27.0	19.1	16.0	56.7	18.3
Total	95.4	98.6	101.9	101.6	95.1	98.6	101.4	99.9

Table A.2: U.S. forecast error variance decompositions (in percent) for subperiod Dec 2015–Jun 2019 (29 observations), forecast horizon $h = 1$

	direct				PVAR				GMM			
	FFR	FG	LSAP	Total	FFR	FG	LSAP	Total	FFR	FG	LSAP	Total
6-month	41.1	79.5	0.3	120.9	19.1	81.5	7.9	108.5	17.9	76.2	0.0	94.1
2-year	12.3	95.5	0.6	108.3	0.8	99.4	3.6	103.8	2.1	96.8	0.0	98.9
5-year	5.0	99.6	8.4	113.0	0.0	97.0	0.1	97.2	0.7	98.4	3.2	102.4
10-year	0.2	93.9	24.8	119.0	3.0	85.9	6.5	95.4	0.5	87.7	15.8	104.0
30-year	10.0	41.2	55.6	106.8	18.8	31.3	35.7	85.8	2.9	31.5	61.1	95.5

Table A.3: UK forecast error variance decompositions (in percent) for baseline model, forecast horizon $h = 1$

	BR	1YGY	CBS	FTSE	GDP	CPI	Ex
PVAR							
w_t^{Target}	90.6	19.1	0.1	0.5	1.0	0.0	0.1
w_t^{Path}	21.4	34.0	0.1	0.2	4.7	12.3	3.0
Total	112.0	53.1	0.2	0.7	5.7	12.3	3.1
GMM							
w_t^{Target}	84.6	24.9	0.4	0.5	1.6	0.1	0.0
w_t^{Path}	8.7	46.5	0.1	0.3	7.1	12.5	3.2
Total	93.4	71.4	0.5	0.9	8.8	12.6	3.2

Table A.4: UK forecast error variance decompositions (in percent) for extended model, forecast horizon $h = 1$

	BR	1YGY	10YGY	CBS	FTSE	GDP	CPI	Ex
PVAR								
w_t^{Target}	87.6	18.2	0.1	0.1	1.1	2.7	0.1	0.0
w_t^{Path}	28.7	28.8	7.3	0.6	0.7	6.6	4.8	1.9
w_t^{QE}	15.8	7.5	18.1	2.1	0.2	42.1	0.6	0.2
Total	132.2	54.4	25.5	2.7	2.0	51.4	5.5	2.1
GMM								
w_t^{Target}	76.8	26.6	2.7	0.4	1.8	1.7	1.6	0.6
w_t^{Path}	12.9	44.5	15.3	0.5	1.7	5.5	5.5	3.1
w_t^{QE}	0.5	0.0	27.6	1.7	1.4	41.0	5.0	4.4
Total	90.3	71.1	45.6	2.7	4.9	48.2	12.1	8.1

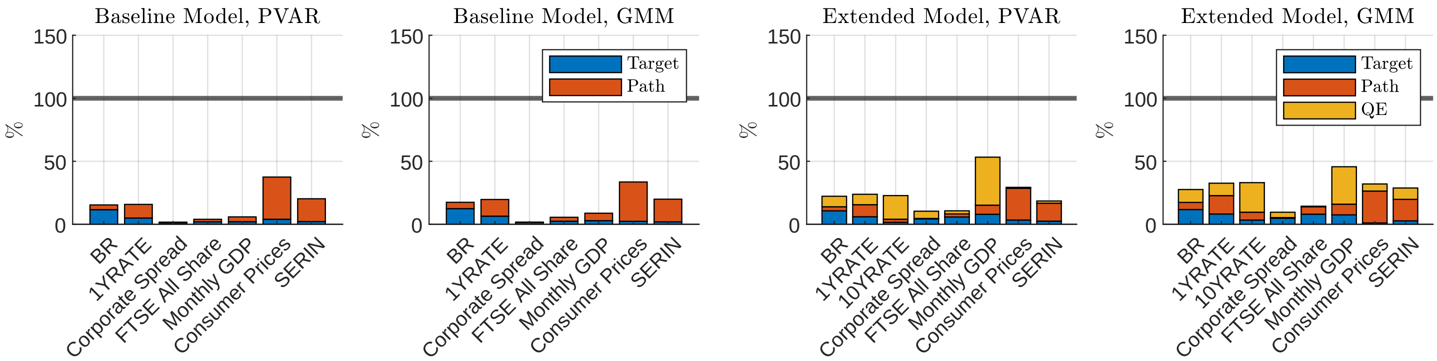


Figure A.1: UK forecast error variance decompositions (in percent), forecast horizon $h = 10$.

References

Acosta, M., Ajello, A., Bauer, M. D., Loria, F. and Miranda-Agrippino, S. (2025). Financial market effects of FOMC communication: Evidence from a new event-study database, *Technical report*, IMFS Working Paper Series.

Altavilla, C., Brugnolini, L., Gürkaynak, R. S., Motto, R. and Ragusa, G. (2019). Measuring euro area monetary policy, *Journal of Monetary Economics* **108**: 162–179.

Altavilla, C., Gürkaynak, R. S., Laeven, L. and Kind, T. (2025). Monetary transmission with frequent policy events.

Andrade, P. and Ferroni, F. (2021). Delphic and Odyssean monetary policy shocks: Evidence from the euro area, *Journal of Monetary Economics* **117**: 816–832.

Angelini, G. and Fanelli, L. (2019). Exogenous uncertainty and the identification of structural vector autoregressions with external instruments, *Journal of Applied Econometrics* **34**(6): 951–971.

Bańbura, M., Giannone, D. and Reichlin, L. (2010). Large Bayesian vector autoregressions, *Journal of Applied Econometrics* **25**: 71–92.

Braun, R., Miranda-Agrippino, S. and Saha, T. (2025). Measuring monetary policy in the UK: The UK monetary policy event-study database, *Journal of Monetary Economics* **149**: 103645.

Bruns, M. and Lütkepohl, H. (2022). Comparison of local projection estimators for proxy vector autoregressions, *Journal of Economic Dynamics & Control* **134**: 104277.

Bruns, M. and Lütkepohl, H. (2025). Comparing external and internal instruments for vector autoregressions, *Journal of Economic Dynamics and Control* **177**: 105131.

Bruns, M., Lütkepohl, H. and McNeil, J. (2025). Avoiding unintentionally correlated shocks in proxy vector autoregressive analysis, *Journal of Business and Economic Statistics* **43**: 1119–1131.

Degasperi, R. (2023). Identification of expectational shocks in the oil market using OPEC announcements, *Technical report*, University of Warwick, Department of Economics.

Gregory, A. W., McNeil, J. and Smith, G. W. (2024). US fiscal policy shocks: Proxy-SVAR overidentification via GMM, *Journal of Applied Econometrics* **39**(4): 607–619.

Gürkaynak, R. S., Sack, B. and Swanson, E. T. (2005). Do actions speak louder than words? The response of asset prices to monetary policy actions and statements, *International Journal of Central Banking* **1**(1): 55–93.

Hou, C. (2024). Large Bayesian SVARs with linear restrictions, *Journal of Econometrics* **244**(1): 105850.

Jarociński, M. and Karadi, P. (2020). Deconstructing monetary policy surprises - The role of information shocks, *American Economic Journal: Macroeconomics* **12**(2): 1–43.

Känzig, D. R. (2021). The macroeconomic effects of oil supply news: Evidence from OPEC announcements, *American Economic Review* **111**(4): 1092–1125.

Kilian, L. and Lütkepohl, H. (2017). *Structural Vector Autoregressive Analysis*, Cambridge University Press, Cambridge.

Kubota, H. and Shintani, M. (2025). Macroeconomic effects of monetary policy in Japan: An analysis using interest rate futures surprises, *Empirical Economics* **68**(2): 783–801.

Lakdawala, A. (2019). Decomposing the effects of monetary policy using an external instruments SVAR, *Journal of Applied Econometrics* **34**(6): 934–950.

Lunsford, K. (2015). Identifying structural VARs with a proxy variable and a test for a weak proxy, *Technical report*, Federal Reserve Bank of Cleveland.

Martínez-Hernández, C. (2020). Disentangling the effects of multidimensional monetary policy on inflation and inflation expectations in the euro area, *School of Business & Economics Discussion Paper 2020/18*, Freie Universität Berlin.

Mertens, K. and Ravn, M. O. (2013). The dynamic effects of personal and corporate income tax changes in the United States, *American Economic Review* **103**(4): 1212–1247.

Ricco, G., Savini, E. and Tuteja, A. (2024). Monetary policy, information and country risk shocks in the euro area, *Discusion Paper DP19679*, CEPR.

Rogers, J. H., Scotti, C. and Wright, J. (2018). Unconventional monetary policy and international risk premia, *Journal of Money, Credit and Banking* **50**: 1827–1850.

Rudebusch, G. D. (1998). Do measures of monetary policy in a VAR make sense?, *International Economic Review* **39**(4): 907–931.

Sims, C. A. (1980). Macroeconomics and reality, *Econometrica* **48**(1): 1–48.

Stock, J. H. and Watson, M. W. (2012). Disentangling the channels of the 2007–2009 recession, *Working Paper No. 18094*, National Bureau of Economic Research.

Stock, J. H. and Watson, M. W. (2018). Identification and estimation of dynamic causal effects in macroeconomics using external instruments, *Economic Journal* **128**(610): 917–948.

Swanson, E. T. (2021). Measuring the effects of federal reserve forward guidance and asset purchases on financial markets, *Journal of Monetary Economics* **118**: 32–53.

Swanson, E. T. (2023). The importance of Fed chair speeches as a monetary policy tool, *AEA papers and proceedings* **113**: 394–400.

Swanson, E. T. and Jayawickrema, V. (2023). Speeches by the Fed chair are more important than FOMC announcements: An improved high-frequency measure of US monetary policy shocks, Mimeo. Unpublished Manuscript.