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# Has the Transmission of US Monetary Policy Changed Since 2022?

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**Has the Transmission of US Monetary Policy Changed Since 2022?\***

Authorized for distribution by Nigel Chalk

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**ABSTRACT:** Activity and inflation responded slowly to the Federal Reserve's rate hikes in 2022. Was this because the transmission of monetary policy had changed? Or did other shocks offset tighter policy? We use pre-pandemic data to estimate a VAR with monetary policy shocks identified from high-frequency data, and use it as a filter to back out the sequence of monetary policy shocks consistent with data since 2022. We compare these implied shocks to the actual shocks and find the difference statistically significant during February-July 2022. These differences imply that monetary transmission was around 25 percent weaker than normal. Our method accounts for other shocks; allowing them to change to match the post-COVID covariance of the data produces similar results but in a shorter period. We decompose changes in the uncertainty of our estimate and find that colinearity of shocks is generally more important than uncertainty over model parameters. We extend our analysis to central bank information shocks and find Federal Reserve communication was less powerful than usual during 2021.

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# 1 Introduction

As inflation reached its highest level since the mid-1980s, in March 2022 the Federal Reserve made the first of a series of interest rate hikes. In the next 19 months, the federal funds rate increased from zero to over five percent. By any standards, this was a large and fast monetary tightening. In the four preceding tightening cycles increases in the federal funds rate had been much smaller and slower, taking an average of 21 months to reach peaks typically 2.5 percentage points above their starting levels.<sup>1</sup>

By July 2022, it was clear that this tightening cycle would last some time. By then, many forecasters were predicting that interest rates would increase further and remain elevated until at least late-2023. This policy stance was widely expected to reduce output below its long-run trend and pull inflation back down towards target, in line with standard views of the transmission of monetary policy (see Figure 1).

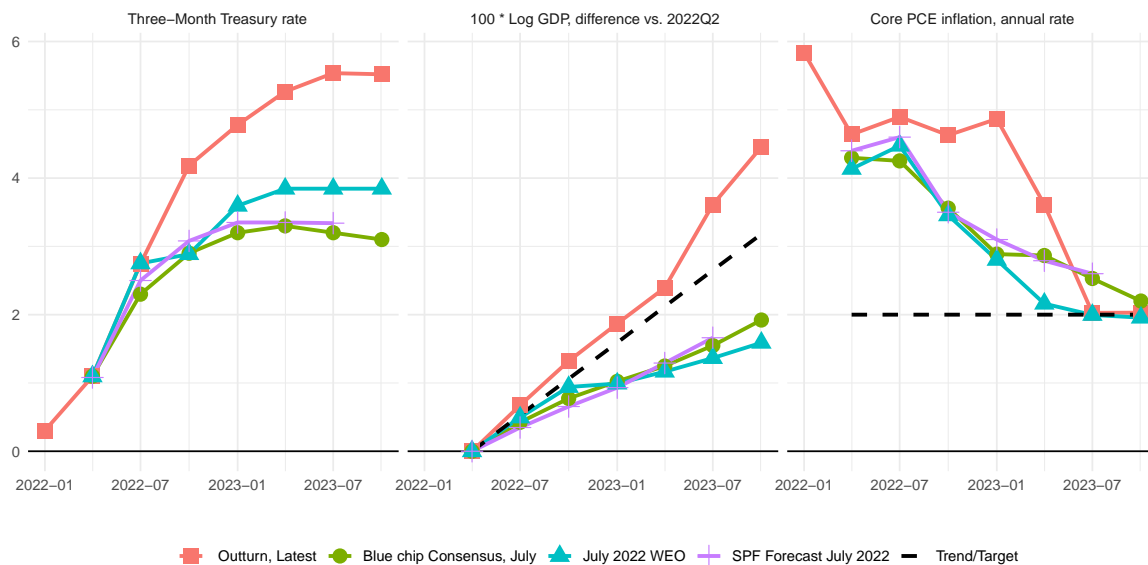


Figure 1: Outturns versus forecasts made in July 2022

Figure shows outturns for interest rates, output, and the price level versus forecasts made in July 2022. Forecasts for change in log GDP abstract from revisions by calculating relative to contemporaneous data for forecasts. The trend for log GDP is at annualized rate of 2.1, the 2000-2019 average. For core PCE the target is 2 percent, the Federal Reserve’s inflation target. Abbreviations: *WEO*, IMF’s World Economic Outlook; *SPF*, Federal Reserve Bank of Philadelphia’s Survey of Professional Forecasters.

However, outturns largely confounded these predictions. Output grew rapidly throughout 2022 and 2023, at more than double the rate of even the most optimistic forecasters. This growth surprise occurred even though interest rates ended up being much higher than expected, something which would normally be expected to have dampened real activity fur-

<sup>1</sup>Cycles starting in February 1994, June 1999, June 2004, and December 2015

ther. And despite the surge in output, inflation has since returned to levels close to the Federal Reserve’s two percent target.

The exceedingly strong performance of the real economy and the apparently unconnected disinflation during such an aggressive tightening cycle raises an obvious question: was the transmission of monetary policy somehow different during this episode? And if so, how? This paper seeks to answer these questions.

The key challenge answering these questions is in disentangling the impact of other macroeconomic shocks (or combinations of them) from any potential change in the transmission of monetary policy. For example, the mix of an unanticipated fiscal expansion and a positive supply shock could produce higher-than-expected output and interest rates and falling inflation, even if monetary policy had been acting exactly as expected.

Given this challenge, we develop a method to answer this question which accounts for the impact of other shocks but without having to identify them explicitly. Our basic approach proceeds in three steps. We first construct a series of monetary policy shocks from high-frequency intra-day financial market data in the spirit of [Jarociński and Karadi \[2020\]](#) and use a pre-COVID sample to estimate a factor-augmented vector autoregression (FAVAR) which includes the monetary policy shocks as data.<sup>2</sup> The Cholesky decomposition of the FAVAR’s reduced form shock with the monetary shocks ordered first then has a semi-structural interpretation where the monetary policy shocks are identified but non-monetary ones are not. The purpose of this step is to produce a data generating process for macroeconomic time series which embodies relatively standard views about the transmission of monetary policy in the pre-COVID period.<sup>3</sup>

In the second step, we invert the FAVAR as if the monetary policy shocks were unknown, using it as a filter to back out the most likely set of shocks from 2021 to the present. This generates the distribution of the high-frequency shocks since 2021 under the null hypothesis that the mapping from monetary policy to the macro data has not changed. A strength of our method is that it explicitly takes account of the changing composition of the other shocks. Although the inferred monetary shocks depend directly on the estimated transmission of monetary policy, they depend on other shocks only indirectly via the covariance of the reduced form residuals. Put differently, even if some unusual combination of other shocks were driving outcomes in the post-tightening period, this would not affect the inferred monetary shocks at all, so long as those other shocks were consistent with the estimated covariance of the data. This feature suggests a natural extension: to relax this assumption

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<sup>2</sup>Aside from the monetary policy shocks, the baseline VAR also includes data on the fed funds rate, industrial production, private consumption, inflation, and the excess bond premium as well as the first five principal components of a large set of other macroeconomic time series.

<sup>3</sup>As [Paul \[2020\]](#) shows, including the instrument directly in the VAR is identical to the more standard external instruments approach pioneered by [Stock and Watson \[2012\]](#) and [Mertens and Ravn \[2013\]](#), up to re-scaling by a constant. We discuss the relationship of our method to using external instruments in Section 2.

simply by replacing the estimated reduced form covariance matrix with another one, such as one estimated on the post-2021 data. This allows a calculation of the distribution of monetary shocks consistent with the data and unchanged transmission, even if the impact of other shocks has also changed. Moreover, this can be done without specifying exactly how other shocks have changed, just their impact on the covariance of the aggregate data.

In the third step, we compare these distributions to the observed high-frequency monetary policy shocks since 2021. If the null hypothesis holds—that monetary policy transmission has not changed—then the realized and inferred shocks should line up. We propose maximum likelihood tests to assess the strength of the evidence that the transmission of monetary policy has changed, as well as modifications to these tests which permit a quantitative assessment of how much weaker monetary policy might have been.

We present results for two types of monetary shocks within the same estimation framework, one a “pure” shock to the stance of monetary policy and the other a Central Bank information shock. The former corresponds to truly tighter monetary policy whereas the latter occurs when the Federal Reserve communicates its insights about other future shocks to other agents in the economy. We find that between February and July 2022, the measured high-frequency monetary stance shocks were persistently and statistically significantly higher than those implied by the filter, both on average and in (some) individual months. That is, although the Federal Reserve was repeatedly surprising the markets with unexpectedly tight monetary policy, the macro data were responding as if the opposite were true. We also show that the realized Central Bank information shocks were larger than those implied by the filter throughout 2021; the data responded as if the Fed was communicating much more negatively than it did. This suggests that monetary policy communication was perhaps less effective than usual during 2021.

We investigate the results for the pure monetary shock further, providing evidence on the magnitude of the change in the strength of monetary policy. We show that in our baseline specification, one can reject at the 5 percent confidence level a null that during February-July 2022 monetary policy was up to 25 percent less effective. This means that for every three interest rate hikes that the Fed did during this period, they would have needed to do approximately one more to deliver the same degree of tightening as one would have expected pre-COVID. When we allow for the variance of other shocks to change to replicate the covariance of the 2021-2023 data, we find that this conclusion is a little weaker – holding with the same confidence only in March and April 2022. Conversely, including policymakers’ speeches in our shock series strengthens this finding, with the same effect holding at the one percent level.

We also analyze the source of the uncertainty arising in our approach, since the quantification of this uncertainty is essential to formulating the hypothesis tests we apply. We find that uncertainty over the model parameters accounts for a slim minority of the total. Instead,

uncertainty due to the colinearity of shocks — that is the extent to which monetary policy and other shocks have similar impact on the data and are thus hard to disentangle—are slightly more important.

*Related Literature* Our work contributes to three prior areas of research.

First, we contribute to the literature that uses high-frequency changes in asset prices around specific events to construct identified monetary policy shocks (Kuttner [2001], Gürkaynak et al. [2004] and many others). Initially the literature was centered on Federal Open Market Committee (FOMC) announcements. However, there are only a limited number of scheduled FOMC meetings (typically eight per year), and the size of the asset price changes around the events is small. And so a recent literature (Swanson [2023], Swanson and Jayawickrema [2023]) extends the approach to include post-FOMC-meeting press conferences, releases of FOMC meeting minutes, and speeches by the Chair and Vice Chair of the Federal Reserve. They find that these events are important sources of variation for asset prices. In an extension, we expand the list of events further by including speeches by all FOMC Board members.

Second, a methodological literature on using high-frequency monetary policy shocks as identified shocks in vector autoregressive (VAR) models (Stock and Watson [2012], Mertens and Ravn [2013], Gertler and Karadi [2015]). These papers study the dynamic impact of identified monetary policy shocks on the economy. While we use a similar approach to estimate a VAR, our ultimate application is different. Once we estimated the VAR, we invert it and use it as a filter to back out the most likely set of shocks over a test period. We then construct a statistical test on whether the mapping of monetary policy to macro data has changed or not. Similar to Paul [2020], our pursuit of an extra step not easily represented in the external instruments framework motivates our direct inclusion of shocks in the VAR.

Third, there is an emerging literature on the monetary policy response to the COVID-19 pandemic and its aftermath (English et al. [2024], Ball et al. [2022]). Much of this work focuses on key ingredients of the monetary transmission mechanism, rather than the more general notion of transmission that we look at. For example, Stedman and Pollard [2023] highlight that services sector labor markets have been particularly tight since the pandemic. These sectors are less sensitive to changes in interest rates, potentially dampening the monetary policy impact. Similarly, Cohen [2023] argues that resilience in the labor market is connected to severe labor shortages in the post-pandemic era, leading employers to hold on to workers and hire less-skilled workers. D’Amico and King [2023] argue that the lags to monetary policy might have been shorter in the post-pandemic tightening cycle because of an increased role of the expectations channel of monetary policy. Berger et al. [2021] and Eichenbaum et al. [2022] highlight that the presence of substantial debt in

fixed-rate, prepayable mortgages can lead to state-dependent effects of monetary policy. In particular, they find that the interest rate path before – and by extension during – the pandemic potentially generated substantial headwinds for the effectiveness of monetary policy once the Federal Reserve started to hike rates. Our contribution to this literature is that we use the limited data available since the pandemic to conduct a statistical test for whether monetary policy transmission has weakened compared to before the pandemic.

The paper is organized as follows: in Section 2 we describe our method; in Section 3 we discuss the construction of our monetary policy shocks; in Section 4 we describe the estimated FAVAR and validate our method on the pre-COVID sample; in Section 5 we present our main results, comparing the filter-implied shocks with those measured in the high frequency data; in Section 6 we we discuss alternative interpretations of our results; and in Section 7 we conclude.

## 2 Methodology

### 2.1 Estimating a Semi-structural Vector Autoregression

Let  $y_t$  be a vector of  $N$  macro variables and  $v_t$  is an i.i.d. vector of proxies for  $K$  different monetary policy shocks identified from other data. We describe how to create such a series in the next section but here we take this as given. We collect monthly data on  $y_t$  and  $v_t$  since 1990 (more detail on the data we use in section 4.1) We call the period from 1990-2019 the *estimation period* and 2021-2023 the *testing period*, with lengths  $N_{est} = 336$  and  $N_{test} = 36$  respectively.

We assume that the data is generated by a VAR of the form:

$$\begin{bmatrix} v_t \\ y_t \end{bmatrix} = \sum_{j=1}^J A_j \begin{bmatrix} v_{t-j} \\ y_{t-j} \end{bmatrix} + \epsilon_t, \quad \epsilon_t \sim N(0, \Omega) \quad (1)$$

and impose that

$$A_j = \begin{bmatrix} 0_{K \times K} & 0_{K \times N} \\ 0_{N \times K} & B_j \end{bmatrix}$$

The top line of zeros follows from the assumption that  $v_t$  is i.i.d. The zeros in the other rows simply impose that  $y_t$  depend only on lagged shocks through lagged  $y_t$ , not directly (i.e. that  $y_t$  has a reduced form VAR, not VARMA, representation). We estimate this specification on the estimation period.

As is usual, we assume that the reduced form errors are a linear function of the true,

structural shocks, so that:

$$\epsilon_t = D\delta_t \quad \text{Cov } \delta_t \sim N(0, I_{N+2}) \quad (2)$$

Where  $DD' = \Omega$ . Because  $v_t$  are already externally-identified monetary shocks, any decomposition of  $\Omega$  where the non-diagonal elements of the top two rows are all zero will have monetary shocks in the first two columns.<sup>4</sup> If the shocks are orthogonal, a Cholesky decomposition will satisfy this condition.

Then we write:

$$D = \begin{bmatrix} I_K & 0 \\ D_m & D_x \end{bmatrix}$$

Then, the  $D_m$  columns thus describe the direct impact of monetary policy shocks on the macroeconomy in the estimation period. We also will find it helpful to use similar notation to divide  $\epsilon_t$ :

$$\epsilon_t = \begin{bmatrix} \epsilon_t^v \\ \epsilon_t^y \end{bmatrix}$$

And for  $\delta_t$ , we use the superscripts  $m$  and  $x$  to denote the monetary and non-monetary shocks:

$$\delta_t = \begin{bmatrix} \delta_t^m \\ \delta_t^x \end{bmatrix}$$

In which case, if we only see reduced form errors  $\epsilon_t^y$ , we can rewrite equation (2) as:

$$D_y \delta_t = \epsilon_t^y$$

where  $D_y = [D_m \ D_x]$

*The relationship to external instruments:* A common alternative approach is to use  $v_t$  as an external instrument (Stock and Watson [2012], Mertens and Ravn [2013], and Olea et al. [2021]). That is, one could rewrite equation (1) purely in terms of the  $y_t$ ,

$$y_t = \sum_{j=1}^J B_j y_{t-j} + \nu_t, \quad \nu_t \sim N(0, \Xi) \quad (3)$$

And if the elements of  $v_t$  are strongly correlated with the monetary shocks (instrument strength) but plausibly uncorrelated with the other structural shocks (the exclusion restriction), one can use the  $v_t$  as an external instrument for the monetary shocks. The result is a decomposition of  $\Xi$  where the first two columns can be interpreted as the responses to a

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<sup>4</sup>Strictly speaking, the first two entries in  $\delta_t$  are the *innovations* to  $v_t$ . But since the shocks are i.i.d. this distinction is not meaningful.



policy shocks.

Paul [2020] shows – subject to some technical conditions satisfied in our application – that the resulting impulse responses are identical to directly including the  $v_t$  series in the VAR, up to a scalar constant. That is, the two approaches differ only in their unit of measurement rather than anything more fundamental.<sup>5</sup> Given that the approaches are in this sense equivalent (something which we check in our estimation later), we prefer the direct inclusion of the shock in the VAR because it makes our next step – recasting the VAR as a filter – much easier. A similar extension motivated the derivation of the results in Paul [2020]. He estimates a VAR with time-varying parameters, which is much easier when the identified shocks are directly included rather than used as instruments.

## 2.2 Recasting the VAR as a Filter

In the VAR framework, computing the reduced form shocks is relatively straightforward. Conditional on the estimates  $\hat{\theta} = \{\hat{B}_1, \dots, \hat{B}_J, \hat{\Omega}\}$ , we can compute the fitted reduced form errors on the post-tightening period by:

$$\hat{\epsilon}_t^y = y_t - \sum_{j=1}^J \hat{B}_j y_{t-j}$$

Then because  $\delta_t \sim N(0, I_{N+2})$ , the maximum likelihood function just reduces to least squares. Thus, we can solve period-by-period the following linear-quadratic program:

$$\hat{\delta}_{t|\hat{\theta}} = \arg \min_{\delta \in \mathbb{R}^{N+K}} \delta' \delta \quad \text{s.t.} \quad \hat{D}_y \delta = \hat{\epsilon}_t^y$$

where  $\hat{D}_y$  is the sample equivalent of  $D_y$ , i.e. the bottom  $N$  rows of the Cholesky decomposition of  $\hat{\Omega}$ . This has solution:

$$\hat{\delta}_{t|\hat{\theta}} = \hat{D}_y' (\hat{\Omega}_y)^{-1} \hat{\epsilon}_t^y$$

Where  $\hat{\Omega}_y = \mathbb{E} \hat{\epsilon}_t^y \hat{\epsilon}_t^{y'} = \hat{D}_y \hat{D}_y'$  is the estimated variance covariance matrix of the reduced form residuals for the data,  $y_t$ . Because we are only interested in the monetary shock, we will find it helpful to focus only on the top  $K$  rows of this estimator:

$$\hat{\delta}_{t|\hat{\theta}}^m = \hat{D}_m' (\hat{\Omega}_y)^{-1} \hat{\epsilon}_t^y$$

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<sup>5</sup>A yet further equivalent formulation is one where the data generating process for  $y_t$  includes an explicit contemporaneous relationship, in which case  $\tilde{B}_0 y_t = \sum_{j=1}^J \tilde{B}_j y_{t-j} + \tilde{v}_t$ . This is identical to equation (3) if  $\tilde{B}_j = \tilde{B}_0 B_j$ ,  $\tilde{v}_t \sim N(0, \tilde{B}_0 \Xi \tilde{B}_0')$  and so the same equivalence between external instruments and inclusion of the external shocks in the VAR still holds.

It is straightforward to show that this estimator is unbiased, that is:

$$\mathbb{E}\hat{\delta}_{t|\hat{\theta}}^m = \delta_t^m$$

We can also compute the mean square error of the estimated structural shocks:

$$\mathbb{E}_t \left( \hat{\delta}_{t|\hat{\theta}}^m - \delta_t^m \right) \left( \hat{\delta}_{t|\hat{\theta}}^m - \delta_t^m \right)' = I_K - \hat{D}'_m \hat{\Omega}_y^{-1} \hat{D}_m \quad (4)$$

This is just a Kalman filter where the unobserved structural shocks as a hidden state and the reduced form shocks as observed signals. The only differences from the usual set-up are that there is no persistence in the state variable and that uncertainty over the state comes not from noise but from incomplete observation (in that the dimension of  $\hat{\epsilon}_t^y$  is less than  $\hat{\delta}_t$ ).<sup>6</sup>

Given that  $\delta_t$  itself is normal and that  $\hat{\delta}_{t|\hat{\theta}}^m$  is an unbiased estimator for  $\delta_t^m$  conditional on the data and the model, then the distribution of the true shocks conditional on the data and the model parameters, which we denote  $\delta_{t|\hat{\theta}}^m$ , is given by a multivariate normal:

$$\delta_{t|\hat{\theta}}^m \sim \mathcal{N} \left( \hat{D}'_m (\hat{\Omega}_y)^{-1} \hat{\epsilon}_t^y, I_K - \hat{D}'_m \hat{\Omega}_y^{-1} \hat{D}_m \right) \quad (5)$$

We will find it helpful to denote the corresponding normal density by  $f_{t|\hat{\theta}}(\delta)$ .

An important property of this filter is that the distribution for the inferred monetary shocks are a function only of  $D_m$  and the reduced form error covariance  $\hat{\Omega}_y$ . One would get exactly the same results for *any* other  $\tilde{D}_x$ , satisfying:

$$\tilde{D}_x \tilde{D}'_x = D_x D'_x$$

In other words, the filtered monetary shocks are wholly invariant to any other interpretation of the non-monetary shocks, so long as they are consistent with the data covariance. Moreover, if we want to know how any other candidate *distribution* of the non-monetary shocks affects our results we need only specify its impact on the data via the reduced form residual covariance matrix,  $\hat{\Omega}_y$ .

### 2.3 Parameter Uncertainty

The preceding section derives the maximum likelihood estimator and the mean square error for the structural shocks given the data and a model. However, the true model is unknown. The distribution in equation (5) does not account for this. And so to account for model

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<sup>6</sup>A related literature is that on conditional forecasting in VARs, initiated by Waggoner and Zha [1999], where one seeks to forecast a subset of variables in a VAR by imposing conditioning assumptions on the other variables. Our approach maps to this framework if one interprets the inferred shocks as the subset of variables to be forecast and the observed macro data as the conditioning assumptions.

uncertainty, we integrate over the distribution of the parameters via bootstrap.

Specifically, we use the point estimates  $\hat{\theta} = \{\hat{B}_1, \dots, \hat{B}_J, \hat{\Omega}\}$  to simulate  $K$  draws of the estimation sample  $\{(v_1^k, y_1^k), \dots, (v_{N_{est}}^k, y_{N_{est}}^k)\}_{k=1}^K$  from the data generating process in equation (1). On each sample, we estimate the corresponding parameters  $\hat{\theta}^k = \{\hat{B}_1^k, \dots, \hat{B}_J^k, \hat{\Omega}^k\}$ . We then compute the distribution for  $\delta_{t|t}$ , the structural shocks conditional only on the data. This is a mixture of normal distributions with density given by:

$$f_t(\delta^m) = \frac{1}{K} \sum_{k=1}^K f_{t|\hat{\theta}^k}(\delta^m) \quad (6)$$

This density represents the distribution of shocks under the null that the relationship between the data and the shocks remains unchanged, taking into account uncertainty around that relationship (i.e. model uncertainty). As such, we can use it to assess hypothesis tests of this null.

This approach has two further benefits. First, we can use the simulated parameter draws to compute bootstrapped confidence intervals for impulse responses. Second, in each testing period  $t$  we can compare the distributions  $f_{t,\hat{\theta}}(\cdot)$  and  $f_t(\cdot)$  to understand the sources of uncertainty over our estimates of the structural shocks. The former captures the irreducible uncertainty coming from the colinearity of the structural shocks. The latter adds the effect of model uncertainty, which is mitigated in longer samples. We return to this point later in Section 5.4

### 3 Constructing Monetary Policy Shocks from High-Frequency Financial Data

#### 3.1 Approach

We use the high-frequency identification approach pioneered by papers including [Kuttner \[2001\]](#) and [Gürkaynak et al. \[2004\]](#). The idea is to look at the change in the price of securities around a narrow time window of relevant monetary policy events, such as FOMC meetings. The change in price should capture the pure surprise component of the news announcement uncorrelated with other shocks, and therefore is a good candidate for an identified monetary policy shock. The choice of a narrow window aims to eliminate the possibility that other news drives the effect. The literature has traditionally focused on FOMC announcements to construct the shocks. But more recently, researchers ([Swanson \[2023\]](#), [Swanson and Jayawickrema \[2023\]](#)) have extended the approach to include other events, like speeches by Fed Leadership, FOMC press conferences, or Fed Meeting Minute releases. These papers find that other events can have at least as much effect on financial markets as FOMC

announcements.

We follow [Jarociński and Karadi \[2020\]](#) and disentangle the pure monetary policy component of a monetary policy announcement from an “information” component. The former can be thought of as a traditional monetary policy shock, where monetary policy itself is unexpectedly and exogenously tighter. The information component captures the situation when a policy announcement provides new information on (the central bank’s view on) fundamentals of the economy to the market. [Jarociński and Karadi \[2020\]](#) propose disentangling the two effects by their differential impacts on interest rates and equity prices, and present evidence that doing so improves the quality of the monetary policy shocks.

To use this approach we need high-frequency data on interest rates and the stock market. For the former we use data on “fourth”, or three-month ahead, Federal Funds Futures (FF4), as in [Gertler and Karadi \[2015\]](#); for the latter we use the S&P 500 index, as in [Jarociński and Karadi \[2020\]](#). We gather FOMC announcement dates and times from the webpage of the Federal Reserve Board. There are around 8 regular FOMC meetings per year. The event window starts ten minutes before the announcement and ends twenty minutes after the announcement, as common in the literature.<sup>7</sup> For the interest rate future we use the absolute change in the price of the security. For the S&P 500 index we use the percentage change in the index calculated as the log difference of the index.

Following [Swanson \[2023\]](#) and [Swanson and Jayawickrema \[2023\]](#), we also develop a secondary shock series, which adds changes in market prices in windows around speeches by policymaker. However, data limitations mean that this secondary series is only available back to 2010. We discuss this series more in Appendix A.2.

## 3.2 Data

We use tick-level data of two financial securities ranging from January 2008 to December 2023, purchased from Bloomberg. The two securities are the “fourth” (or three-month ahead) Federal Funds Future (FF4), and the level of the S&P 500 index (SPX). “Raw” shocks are constructed by taking the price change around events as described in the previous section. To extend our sample further back, we make use of data from [Gürkaynak et al. \[2022\]](#) which contains the same raw shocks for our two securities, but going back to the late 1980s. This data ends in June 2019, and we use the overlapping sample period to confirm that the raw shocks are close. Appendix A.1 presents full details of how we calculate the relevant changes in the high-frequency financial data. And Appendix A.2 discusses the extension to include policymakers’ speeches.

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<sup>7</sup>We use the last traded price before the window starts, and the first traded price after the window ends.

### 3.3 A rotational decomposition

To decompose high-frequency movements in interest rates and equity prices into a pure monetary shock and a Central Bank information shock, we use a rotational decomposition. The advantage of this approach is that it relies only on the high-frequency financial market data and not on other macro time series. This is important for us since we want to use macro data to formulate a null distribution for the shocks in the test period using the filter. The test statistics for the hypothesis tests will use the true high-frequency shocks. If they also depend on the macro data then they are not independent of the relationship between the shocks and the data and so the tests will be invalid. In contrast, [Jarociński and Karadi \[2020\]](#) identify their shock as a sign restriction in a VAR which includes macro data, which would be problematic given the exercise we have in mind. In practice, though, our shock series and theirs are very similar.

We let the vector  $m_t$  be the changes in interest rates and the S&P 500 in the window around FOMC announcements in each month. We write

$$m_t = \begin{bmatrix} \Delta i_t \\ \Delta q_t \end{bmatrix}$$

We assume that the data has already been transformed to be mean zero and denote the variance of  $m_t$  by  $\Gamma$ .

We want to give the high-frequency data  $m_t$  a structural interpretation, converting it into structural shocks  $v_t$ , which are related by  $m_t = H v_t$  where  $v_t$  is mean zero and  $\mathbb{E}v_t v_t' = I_2$ . In particular, if the first element of  $v_t$  exhibits a negative correlation of interest rates and equity prices and the second a positive correlation, then we can interpret the elements of  $v_t$  as a pure monetary shock and a Fed information shock respectively. This imposes a sign restriction on  $H$ :

$$H = \begin{bmatrix} + & + \\ - & + \end{bmatrix}$$

We satisfy these restrictions with an orthogonal decomposition, imposing a parametric structure on the  $H$  matrix:

$$H = \begin{bmatrix} h_1 \cos \theta & h_2 \sin \psi \\ -h_1 \sin \theta & h_2 \cos \psi \end{bmatrix}, \quad h_1, h_2 > 0 \quad \psi \in (0, \pi/2)$$

The intuition for this setup is that  $\psi$  is the angle of a line through  $(\Delta i_t, \Delta q_t)$  below the x axis. This is the prime axis of rotation. It is close to, but not necessarily identical to, the OLS line of best fit. The two components of  $v_t$  then contain instructions for getting to the corresponding point  $u_t$ , expressed as a movement along the line of best fit (the first

component) and perpendicular to it (the second component).

Solving for  $h_1, h_2, \psi$  such that  $HH' = \Gamma$ , we get that:

$$\begin{aligned}\tan 2\psi &= \frac{2\gamma_{12}}{\gamma_{22} - \gamma_{11}} \\ h_2^2 &= \frac{1}{2} \left( \gamma_{11} + \gamma_{22} + \frac{\gamma_{12}}{\sin \psi \cos \psi} \right) \\ h_1^2 &= \gamma_{11} + \gamma_{22} - (h_2)^2\end{aligned}$$

where  $\gamma_{ij}$  is the  $(i, j)$ th entry of  $\Gamma$ .<sup>8</sup>

### 3.4 The shocks

We first take a look at the raw shocks, which are the changes in the fourth Federal Funds Future and the S&P 500 stock market index over the 30 minutes event window.<sup>9</sup> Summary statistics are displayed in Table 1. The total number of observation for FF4 is 298, and for S&P 500 is 297. The shocks are small: the mean absolute change is 2.77 basis points for FF4 (36.1 basis points for S&P 500) over the full sample, with a standard deviation of 5.48 for FF4 (55.8 for S&P 500). The mean absolute change is slight smaller since the pandemic, with 1.55 basis points for FF4, and 34.3 basis points for S&P 500. Our sample includes 31 FOMC meetings since March 2020, which we consider to be the point in time in which the pandemic started in the U.S.

Figure 2 shows the cross-sectional plot of the raw shocks,  $m_t$ , over the estimation period. The solid line shows the first column of  $H$ . Each point can be uniquely decomposed into a distance along this line and a distance perpendicular to it. These distances are the entries in  $v_t$  and represent our pure monetary policy shock and Central Bank information shocks respectively.

Figure 3 shows the time series for these shocks, together with the two shock series from [Jarociński and Karadi \[2020\]](#). The latter shocks end in June 2019, while our shocks include the pandemic-period and end in January 2024.<sup>10</sup> The shocks from both approaches overlap very well. The correlations coefficient on the overlapping sample is 87 percent for the monetary policy shock and 77 percent for the Central Bank information shock, respectively. Note that some discrepancy is not surprising due to the different methodology: [Jarociński and Karadi \[2020\]](#) include macroeconomic variables when constructing the shocks, while our approach – intentionally – doesn't. The shocks series from both approaches show a higher

<sup>8</sup>This is similar to the decomposition in [Jarociński \[2022\]](#). However, there the decomposition is does not impose unit magnitude on the  $v_t$  shocks and so cannot solve uniquely for  $\psi$ .

<sup>9</sup>We transform the price  $p$  of the Federal Funds Future to an interest rate by setting  $p = 100 - R$ , where  $R$  is the arithmetic average of the daily effective federal funds rates during the contract month.

<sup>10</sup>Data limitations prevent us from extending the shock series in [Jarociński and Karadi \[2020\]](#) exactly.

| Statistic          | FF4    |          |          | SP500   |          |          |
|--------------------|--------|----------|----------|---------|----------|----------|
|                    | All    | <03/2020 | >03/2020 | All     | <03/2020 | >03/2020 |
| Mean               | -1.21  | -1.39    | 0.35     | 1.51    | 1.78     | -0.77    |
| Mean (Absolute)    | 2.77   | 2.91     | 1.55     | 36.06   | 36.26    | 34.34    |
| Standard Deviation | 5.48   | 5.71     | 2.31     | 55.76   | 56.72    | 47.46    |
| Minimum            | -37.00 | -37.00   | -6.00    | -188.03 | -188.03  | -136.58  |
| Maximum            | 12.00  | 12.00    | 5.50     | 407.58  | 407.58   | 71.32    |
| Observations       | 298    | 267      | 31       | 297     | 266      | 31       |
| Obs > Zero         | 94     | 81       | 13       | 149     | 131      | 18       |
| Obs = Zero         | 77     | 67       | 10       | 3       | 3        | 0        |
| Obs < Zero         | 127    | 119      | 8        | 145     | 132      | 13       |

Table 1: Summary Statistics for raw shocks for Federal Funds Future (FF4) and S&P 500 stock index (SP500), both in basis points.

The raw shocks are the change in the price of FF4 and the basis point change in the S&P 500 stock index over the 30 minute event windows. “All” refers to full sample ranging from January 1990 to January 2024, “< 03/2020” to before March 2020, and “> 03/2020” to since March 2020.

variance around “crisis” periods like around 2001 after the bursting of the dot-com bubble, as well as the Great Financial Crisis of 2008 and 2009. A period of some divergence between the two approaches is before 1994, where our monetary policy shocks tend to be smaller, while the Central Bank information shocks tend to be bigger. This is also the time period where the FOMC did not regularly issue a press release.

## 4 Estimation and Validation

### 4.1 Data

We aim to incorporate information from a broad set of macro time series in our estimation, while still retaining interpretable impulse responses for comparison to those in the literature. To this end, we include both commonly-used macro time series in the vector  $y_t$ , as well as factors extracted from a wider set of variables. That is, we estimate a factor-augmented vector autoregression (FAVAR). In our baseline specification,  $y_t$  includes five variables directly – the Federal Funds Rate, log industrial production, the log of the Consumer Price Index, the employment-population ratio rate, and the Excess bond premium – as well as five factors summarizing further 18 variables covering financial markets, prices, real activity, and labor markets. These variables are listed in Table 2 and we use a principal components decomposition to create the factors. These five factors explain 70 percent of the variation in these covariates. Figure 4 plots the data for the five main series after detrending and deseasonalizing. The full data sample runs from January 1990 to December 2023, with data

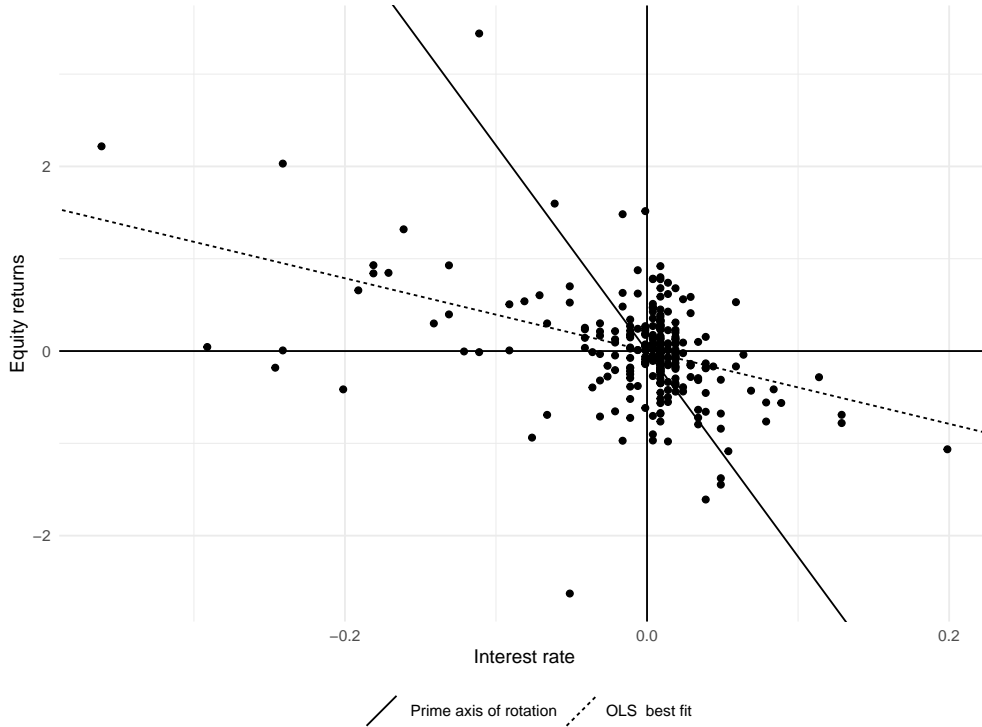


Figure 2: High frequency data: rotational decomposition

Figure shows changes in the interest rate and equity returns in thirty minute windows around FOMC meetings, aggregated monthly during 1992-2019. The prime axis of rotation corresponds to the vector in the first column of  $H$ , so  $\psi$  is the angle between it and the x axis.

before December 2019 used for estimation and after January 2021 for out-of-sample testing. Figures A2 and A1 in Appendix A.3 plot the data including the factors and the extra data series respectively.

## 4.2 A Baseline View of the Transmission of Monetary Policy

We estimate the reduced form VAR in equation (1) on the 1990-2019 period, and interpret the first two rows of the Cholesky decomposition as the impact of shocks to monetary policy and central bank information. Although our focus is not on estimating these response, the impulse responses are an important step in our analysis since they embody the view of the transmission of monetary policy to which we want to compare more recent data. We allow for  $K = 2$  lags in our baseline specification, selected by the Akaike information criterion. A short lag length is not entirely surprising since the inclusion of the factors means we have many variables in the VAR – extra lags are more heavily penalized and there is less marginal information from extra lags. Longer lag lengths produce similar results.

Figure 5 shows the response of the five headline variables to the monetary policy shock, normalized to a one percentage point increase in the Federal Funds Rate. Factors are in-



| Variable                        | Source                             | Transformation |
|---------------------------------|------------------------------------|----------------|
| <i>Included directly</i>        |                                    |                |
| Fed Funds Rate                  | Federal Reserve Board              | Level          |
| Industrial Production           | Federal Reserve Board              | Log Level      |
| Consumer Price Index            | Bureau of Labor Statistics         | Log Level      |
| Employment-Population Ratio     | Bureau of Labor Statistics         | Level, percent |
| Excess Bond Premium             | Federal Reserve Board              | Level          |
| <i>For inclusion as factors</i> |                                    |                |
| 3 month treasury rate           | Federal Reserve Board              | Level          |
| 1 year treasury rate            | Federal Reserve Board              | Level          |
| 10 year treasury rate           | Federal Reserve Board              | Level          |
| US dollar broad EER             | Bank for International Settlements | Log Level      |
| S&P 500 Index                   | Standard & Poors/Dow Jones         | Log Level      |
| CPI inflation                   | Bureau of Labor Statistics         | Percent        |
| PCE deflator                    | Bureau of Economic Analysis        | Log Level      |
| Real oil price, CPI deflated    | Energy Information Administration  | Log Level      |
| Commodity Price Index           | Commodity Research Bureau          | Log Level      |
| Federal Government Outlays      | U.S. Treasury                      | Log Level      |
| Federal Government Receipts     | U.S. Treasury                      | Log Level      |
| Unemployment Rate               | Bureau of Labor Statistics         | Level, percent |
| Average Weekly Hours            | Bureau of Labor Statistics         | Log Level      |
| Average Hourly Earnings         | Bureau of Labor Statistics         | Log Level      |
| Residential Investment          | Bureau of Economic Analysis        | Log Level      |
| Manufacturing Orders            | U.S. Census Bureau                 | Log Level      |
| Consumer Confidence             | Conference Board                   | Level          |
| Real Personal Consumption       | Bureau of Economic Analysis        | Log Level      |

Table 2: Data and sources

All variables are monthly and deseasonalized, and detrended using a Hodrick-Prescott filter. The division of series into those included directly and those which are included as factors follows the baseline specification.



Figure 3: High-frequency shocks: comparison with [Jarociński and Karadi \[2020\]](#)

cluded, but not shown; see Figures A3 and A4 in Appendix B.1 for the responses of the factors). The results are similar to those in the literature. Industrial production falls sharply in the months after the monetary shock, with a maximum contraction of around 3 percentage points. The real impact reaches its maximum around eight months after the shock although the effect persists for rather longer. We also estimate a decline in prices on impact, although this is a little less persistent than estimates elsewhere. The decline in real activity and prices also associated with a subsequent reduction in interest rates, consistent with an endogenous response to lower output and inflation.

Our estimated Central Bank information shock is also broadly consistent with other estimates, both qualitatively and quantitatively. Positive information about the future state of the economy leads to an increase in real activity and prices with interest rates rising in response and employment increasing.

In Figures 5 and 6 we scale the shocks such that the initial impulse for the Federal Funds rate is one percentage point. Typically, this requires a relatively large high frequency shock; around 120 standard deviations of the monetary policy shock and 35 of the Central Bank information shock. This occurs because the identified shock captures only a small part of the variation in the correlated with the high-frequency series,  $v_t$ . As mentioned above, an alternative approach is to treat  $v_t$  as imperfectly correlated instruments for a broader set

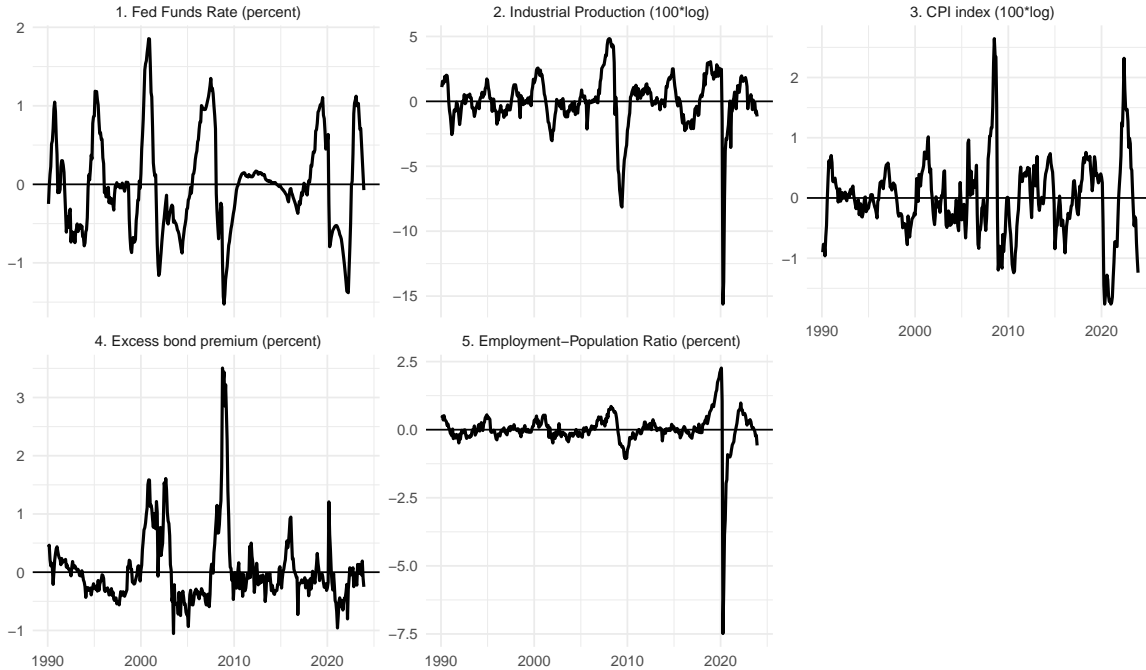


Figure 4: Time series data used in the the baseline VAR

Figure shows the data series which are in the headline vector autoregressions after transformations, excluding factors.

of monetary shocks. As Paul [2020] shows, this produces identical impulse responses, just rescaled – effectively a change-of-units to account for the fact that not all the monetary policy shocks are captured by the high-frequency series. We verify this in Figures A16 and A17 in Appendix B.4 we present the point estimate for the impulse responses using the shocks as external instruments. As they should, they match the impulse responses in Figure 5 and 6 identically up to a scalar.

### 4.3 Robustness

In Appendix B.2 we investigate the sensitivity of the estimated structural VAR model to a variety of alternatives We show there that longer lag lengths make little difference to the impulse responses (Figures A7 and A8). Changing the interest series has little impact either – the responses using a one-year government bond rate in place of the federal funds rate are identical, and when using a three-month rate, only the interest rate response changes slightly (Figures A5 and A6). A similar lack of difference holds when variously replacing our headline series with other measures of inflation, activity, or labor market activity. We suspect that this stability across alternative specifications is likely a result of the factors that we use to augment our VAR. Since they soak up much of the correlated variation in other series, replacing one or other of the headline measures makes little difference to the

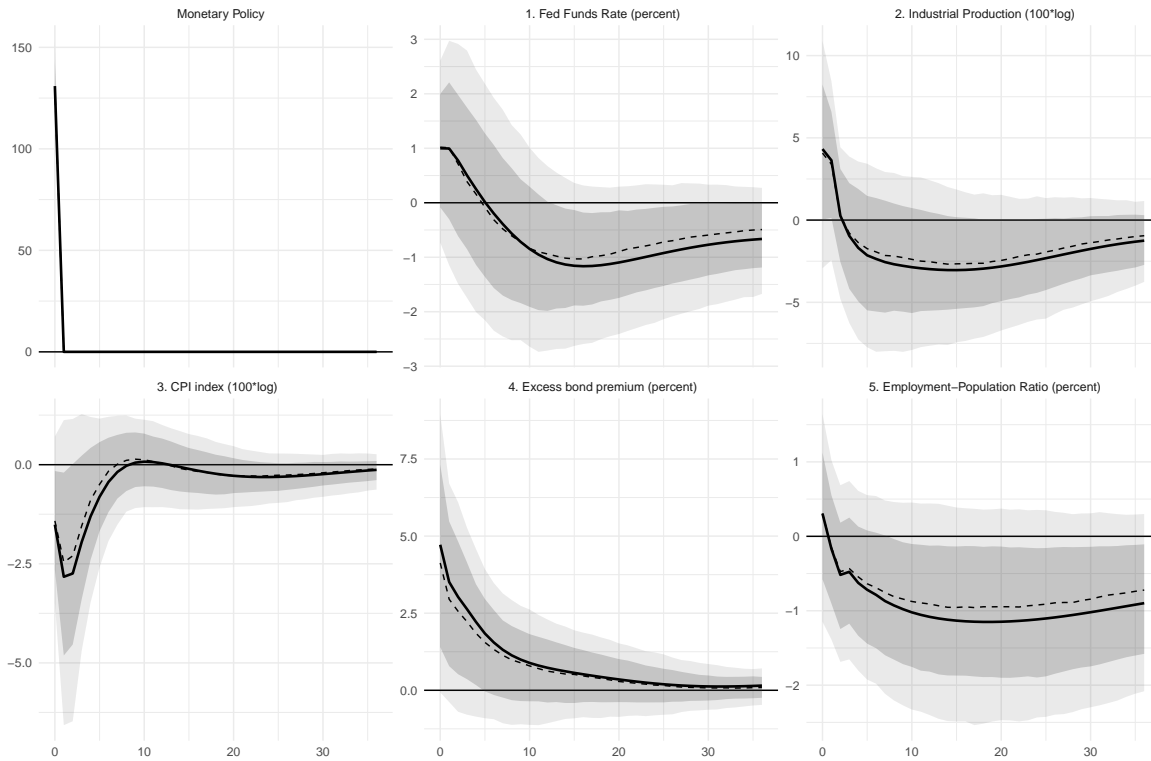


Figure 5: Impulse responses: Monetary policy shock

Figure shows the response of the headline variables to a monetary policy shock, scaled to a 1 percentage point increase in the Federal Funds Rate. The estimated VAR includes the five main variables, plus both shocks and five factors. Solid lines are point estimates and shaded regions are the 68 and 90 percent confidence intervals from a bootstrap with  $K = 1000$  replications. Dashed lines show median responses from the bootstrap. Data is monthly and the estimation sample is January 1990-December 2019

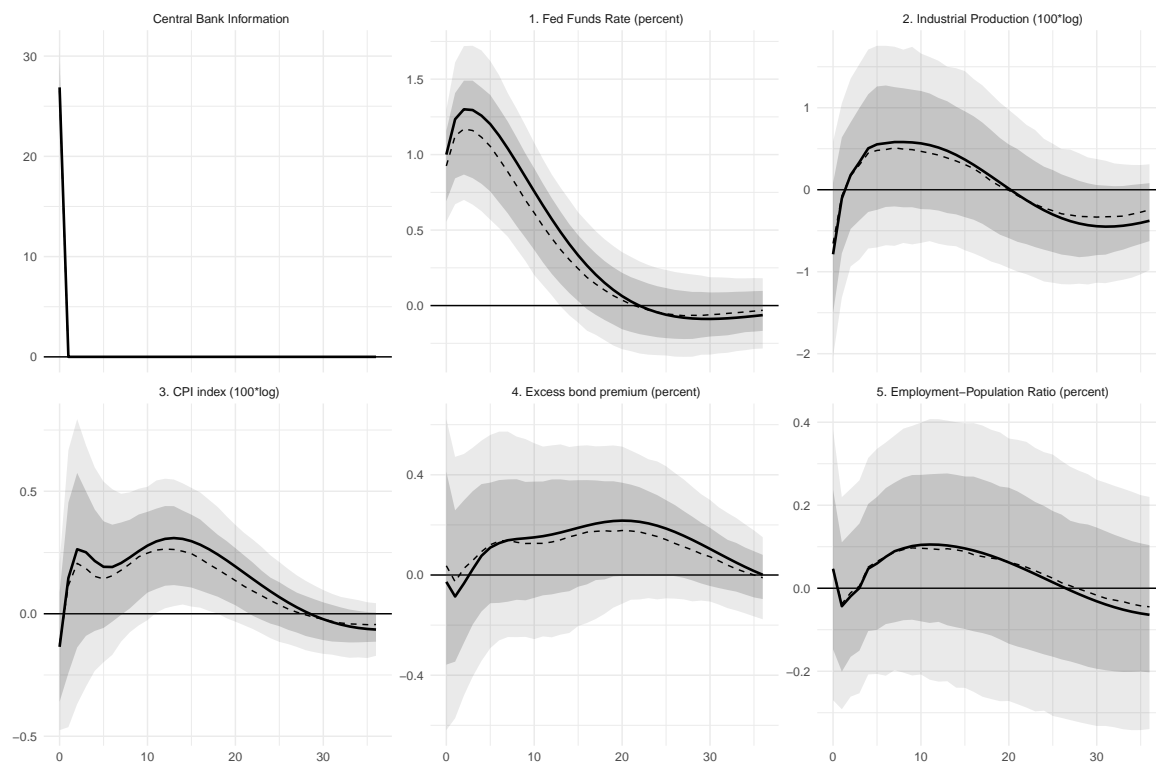


Figure 6: Impulse responses: Central Bank Information shock

Figure shows the response of the headline variables to a monetary policy shock, scaled to a 1 percentage point increase in the Federal Funds Rate. The estimated VAR includes the five main variables, plus both shocks and five factors. Solid lines are point estimates and shaded regions are the 68 and 90 percent confidence intervals from a bootstrap with  $K = 1000$  replications. Dashed lines show median responses from the bootstrap. Data is monthly and the estimation sample is January 1990-December 2019

estimated impulse responses.

We verify the importance of the factors in Appendix Figures A9 and A10 where we show that removing the factors has a relatively large impact, with interest rates staying persistently higher and output and employment slower to respond. The inclusion of a relatively rich set of factors also largely explains why our impulse responses differ from canonical estimates such as those of [Jarociński and Karadi \[2020\]](#). There, the peak decline in industrial policy per unit increase in peak short rates for a monetary policy shock is about three, and is reached at a horizon of around 10-20 months. In contrast, our baseline results have a similar peak response of industrial production, but reach it much faster, at around 7 to 8 months. They also produce a hump-shaped response for short-term interest rates, which we do not. When we exclude the factors from our VAR, though, these differences go away entirely. This is consistent with the notion that we are capturing similar shocks to [Jarociński and Karadi \[2020\]](#) (as Figure 3 suggests) but the inclusion of a wide set of factors means that we control for other sources of correlated variation. For example, because we include interest rates at a variety of horizons, our impulse response for the federal funds rate controls for correlated changes in the slope of the yield curve which may be otherwise confounded with the response of the federal funds rate.

We also check that our results are robust to the extension of our shock series to include speeches since 2008, as suggested by [Swanson \[2023\]](#). Appendix Figures A11 and A12 present the corresponding impulse responses using this shock series and find that they are broadly similar, albeit with a slightly more delayed response of the real activity variables, industrial production and employment.

To be clear, the broader point of this section and the previous one is not to claim that we have precisely the best or most definitive measure of how monetary policy affects the macroeconomy. Rather, it is to convince the reader that the view of the monetary transmission mechanism against which we test the 2022 data is reasonable. And although reasonable people may differ in the details of exactly how policy transmits, the baseline view that our impulse responses represent is consistent with mainstream views.

#### 4.4 Validation

To check that our method works, we run the filter on the estimation period (1990-2019), backing out the two implied policy shocks from the data alone. This, of course, should work—that is the point of a check—but it is also a chance for our method to fail in case there is something fundamentally wrong with it.

Table 3 assesses the point estimates from our filter, regressing the actual shocks on our inferred shocks. If our filter is truly producing an unbiased estimate of the shocks then this

regression should have a slope of one – a unit change in the estimated shock predicts a unit change in the actual shock – and have an intercept of zero – no bias. This holds true for both shocks. One less obvious point is that the  $R^2$  is low for both shocks, especially the pure monetary policy shock. This says that much of the variation in the realized shocks cannot be captured by our VAR. One possibility is that our model is mis-specified, but the other is simply that this might be a fundamentally hard task – there might not be that much information in macro data from which to learn about policy shocks.

|                                | Monetary Policy     | Central Bank Information |
|--------------------------------|---------------------|--------------------------|
|                                | (1)                 | (2)                      |
| Estimated shock                | 1.011***<br>(0.187) | 1.005***<br>(0.122)      |
| Constant                       | 0.005<br>(0.051)    | -0.025<br>(0.049)        |
| Observations                   | 356                 | 356                      |
| $R^2$                          | 0.076               | 0.161                    |
| Adjusted $R^2$                 | 0.074               | 0.158                    |
| Residual Std. Error (df = 354) | 0.966               | 0.921                    |

*Note:* \*p<0.1; \*\*p<0.05; \*\*\*p<0.01

Table 3: Model validation: Regressing actual on estimated shocks

Regression of actual shocks on the shocks estimated via the filter, monthly data 1992-2019. Spherical standard errors shown in parentheses. This is a test of our method, and if it works should return coefficients of one and zero on the estimated shock and the constant respectively.

Whatever the degree of difficulty in recovering shocks from macro variables, our measure of uncertainty should always reflect this. To check this, Table 4 reports the coverage ratios of the realized shocks computing percentile ranges at each point in the estimation sample using the mixture of normals in equation (6). If our assumptions on the distribution of  $\delta_t$  and subsequent calculations are correct, then the true shocks should be inside the  $x\%$  confidence interval  $x$  percent of the time. Table 4 shows that the coverage ratios are good for the 90 and 95 percent interval, but perhaps a little wide for the middle of the distribution and a little narrowed far out in the tails. This is consistent with the shock being highly leptokurtic, with excess kurtoses of around 13. Fat tails mean that the inner confidence intervals will be over-covered and the outer ones under-covered. Nevertheless, the mixture of normals does at least a fairly good job even in the outer percentiles. A possible extension to our hypothesis testing would be ad hoc correction to p-values based on these percentiles.

| Shock                    | 68 percent | 90 percent | 95 percent |
|--------------------------|------------|------------|------------|
| Monetary Policy          | 80.9       | 92.1       | 94.7       |
| Central Bank Information | 83.7       | 93.0       | 94.7       |

Table 4: Model validation: Coverage ratios

## 5 Results from the Filter

### 5.1 True versus filter-implied shocks

We apply the VAR-based filter to the post-2021 data. This calculates in each period the distribution of monetary (and other) shocks under the null hypothesis that the data generating process has remained unchanged. By then comparing the realized monetary shocks from the high frequency data to this sequence of distributions we perform an out-of-sample test that the data generating (and thus the monetary transmission process) remains unchanged. If the data generating process is unchanged, the realized shocks should be statistically indistinguishable from the inferred ones.

Figure 7 presents the headline results from this experiment. The shaded regions show the confidence intervals for shocks conditional on the data under the null that the data generating process is unchanged, computing using the distribution in equation (6). The blue line shows the shocks measured from the high frequency financial market data. Both series show notable and persistent departures of the realized shocks from the inferred ones. In particular, the measured monetary policy shock exceeds the point estimate from the filter throughout January to July 2022. In other words, the FOMC was repeatedly moving financial markets in ways consistent with tightening but the macro data were behaving as if they were loosening. That is, the transmission of monetary policy was looser. Likewise, the Fed information shock exceeds the point estimate From April 2021 until April 2022. This means that markets were interpreting Fed announcements at the time as if they were revealing positive information about the state of the economy, but the monthly macro data did not subsequently confirm that.

How sure can we be of these conclusions? The shaded regions in Figure 7 provides a metric to answer this question period-by-period, as they represent the distribution for the data-consistent shocks, under the null that the transmission of monetary policy is unchanged. As should be apparent from the figure, the evidence in any given month for either shock transmitting differently from usual is not strong – with a few exceptions, the blue dots are typically inside the center of the distribution. And so the statistical confidence that monetary policy transmission is weaker in any given period is typically low. However, the measured monetary policy shock is not in the upper part of the filter distribution in just one period, but rather for sequence of periods. Even if we cannot be very confident that the transmission



of monetary policy is weaker in any *given* period, perhaps we can pool evidence across these periods to conclude that monetary policy transmission was weaker on average, or at least some of the time. Much of the rest of this paper attempts to tackle this issue.

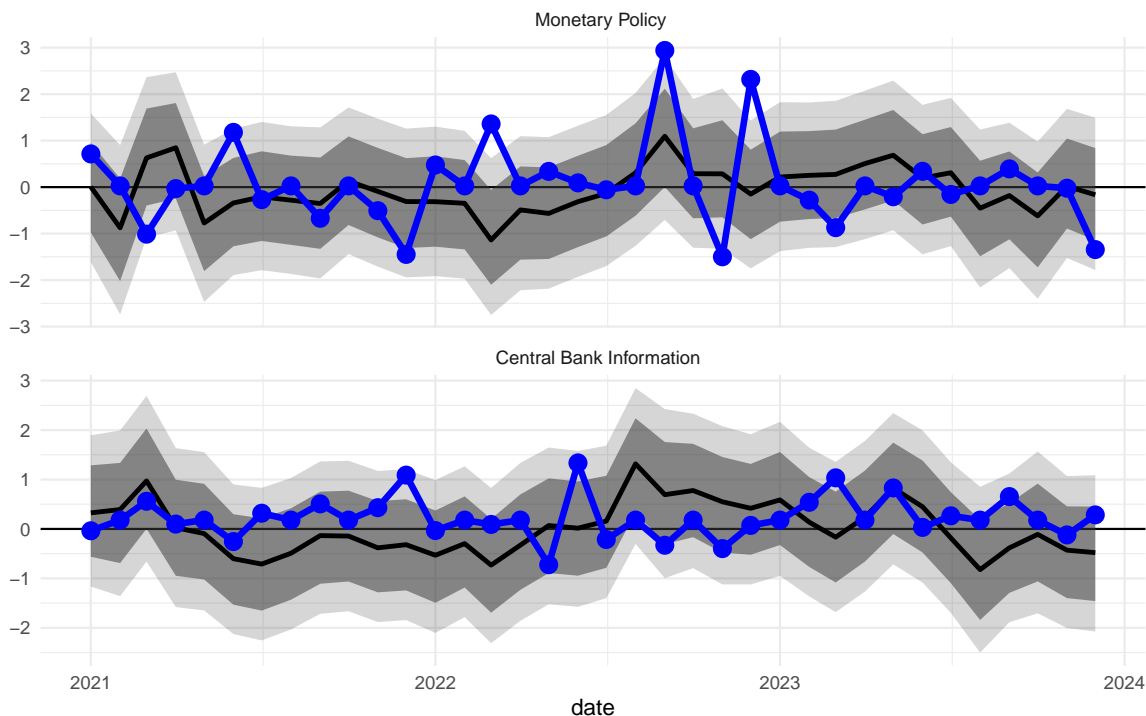


Figure 7: Inferred and realized shocks: 2021-2023

Solid lines are point estimates of the inferred shocks under the null hypothesis that the data generating process remains unchanged. The shaded regions are the 68 and 90 percent confidence intervals from a bootstrap with  $K = 1000$  replications. The blue line with large dots is the actual shock, computed from high frequency data.

## 5.2 Informal assessment

We first start with an informal test of this idea, asking: how rare is it that the observed shocks are persistently above or below the filter-implied estimates? It is very rare. For both shocks, the periods are the (sometimes joint) longest streaks where the observed data exceed the point estimate from the filter in the entire sample, starting in 1990. Table 5 puts these streaks in context, reporting the longest streaks where the observed high-frequency shocks are consistently above and below the filter-implied point estimates, across the whole sample (1990-2023). This shows that only in times of extreme stress—the Global Financial Crisis and the onset of the COVID-19 pandemic—have there been even a comparable number of consecutive observations of the monetary policy shock above or below the filter point estimates.

| Shock           | Month      | Consecutive periods above | Consecutive periods below |
|-----------------|------------|---------------------------|---------------------------|
| Monetary Policy | 2007-03-01 | 0                         | 6                         |
| Monetary Policy | 2008-04-01 | 7                         | 0                         |
| Monetary Policy | 2020-06-01 | 0                         | 9                         |
| Monetary Policy | 2022-07-01 | 7                         | 0                         |
| CB Information  | 2010-09-01 | 9                         | 0                         |
| CB Information  | 2014-12-01 | 9                         | 0                         |
| CB Information  | 2019-04-01 | 10                        | 0                         |
| CB Information  | 2022-04-01 | 13                        | 0                         |

Table 5: Historic consecutive periods of unusual shocks: Full sample, 1990-2023

Table shows terminal months of the longest streaks of high-frequency observed shocks above or below the point estimate. Episodes included in the table are the four longest consecutive sequences above or below the point estimate for each shock. For example, all other observations have 6 or fewer consecutive observations above or below the inferred monetary policy shock.

### 5.3 Formal assessment: Hypothesis testing

We can also calculate a more rigorous assessment of the extent to which these shocks are unusual. Given that we have explicit distributions for the shock conditional on the data and the data generating process in each period, we can compute p-values for the joint hypothesis test that a whole sequence of shocks is drawn from the filter-implied distribution.

Formally, we denote by  $\eta_t$  the difference between the observed high-frequency shocks and the mean of the filter-implied estimate, so  $\eta_t = v_t - \bar{\delta}_t$ . We then define  $\mu_t = \mathbb{E}\eta_t$  as the mean of  $\eta_t$ , which is a parameter. If the relationship between the high-frequency shocks and the macro data is unchanged then this will be zero in every period. Based on the preceding section, we have reason to believe that this may not be true during parts of 2021 and 2022. And thus for any particular period  $\mathcal{T}$  we test three possible hypotheses about each shock  $i$ :

**Test 1**  $H_0 : \mu_t^i \leq 0$  for some  $t \in \mathcal{T}$  vs.  $H_1 : \mu_t^i > 0$  for all  $t \in \mathcal{T}$

**Test 2**  $H_0 : \mu_t^i \leq 0$  for all  $t \in \mathcal{T}$  vs.  $H_1 : \mu_t^i > 0$  for some  $t \in \mathcal{T}$

**Test 3**  $H_0 : \bar{\mu}^i \leq 0$  vs.  $H_1 : \bar{\mu}^i > 0$ , where  $\bar{\mu}^i = \frac{1}{|\mathcal{T}|} \sum_{t \in \mathcal{T}} \mu_t^i$

Intuitively these three tests capture the idea that in a given period, the transmission of monetary policy is different 1) at all points during  $\mathcal{T}$ , 2) at least some of the time during  $\mathcal{T}$ , and 3) on average across  $\mathcal{T}$ . All three tests are explicitly one-sided, which is important in our application since we have a priori evidence that  $\mu_t$  has the same sign for multiple consecutive periods. However, this makes the rejection regions and corresponding p-values a little harder to compute than in the two-sided equivalents. Note also that in only test 3, where we compute a sample mean, is a central limit theorem likely to apply directly. In tests 1 and 2 we add an extra parameter with each extra observation, and so the power of the test

does not grow with the number of periods in  $\mathcal{T}$ . Indeed, test 1 is a very stringent test, and the null will be rejected only if we have strong evidence that transmission is changed in all periods.

The top half of Table 6 reports the p-values for these tests, evaluated by likelihood ratio, for a series of windows starting in February 2022 for the monetary policy shock.<sup>11</sup> The values in each row correspond to the maximum probability of falsely rejecting the  $H_0$  under the null for a sequence of policy shocks starting February 2022. As expected, the p-values for test 1 suggest that we cannot conclude that the relationship between the high frequency shocks and the data is always different in any part of the first half of 2022. This is because there is always at least one period where the observed high frequency shocks are not sufficiently different from the filter-implied estimates to convincingly reject the null. However, tests 2 and 3 provide stronger evidence that monetary policy is weaker than would normally be expected. They show that there is evidence at around the 1 to 3 percent confidence level that monetary policy is weaker at least some of the time, as well as on average.<sup>12</sup>

The lower half of the table repeats a similar exercise for the central bank information shock. There, there is less precision about whether the relationship between this shock and the macro data is different in any one period. However, in the longer test periods, the power of the central limit theorem starts to show. If we reject the null of unchanged average transmission of the central bank information shock during the second half of 2021, we would do so incorrectly less than 1 percent of the time. In other words, we can be very confident that the impact of the central bank information shock was on average different during this time.

## 5.4 The role of uncertainty

The measured uncertainty in our estimates is essential to computing the test statistics for the hypothesis tests above. So what drives them? In Figure 8 we decompose the 90 percent confidence intervals from Figure 7 into their contributions from model uncertainty (i.e. that coming from the fact that the true model parameters are unknown) and from the inference uncertainty (i.e. that coming from the filter). The latter is constant, since it reflects the lack of knowledge over the shocks due to missing information, which does not change. Most of the time, this is the main source of uncertainty, and arises because the shocks are fundamentally

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<sup>11</sup>Appendix C describes in detail how these are computed.

<sup>12</sup>It might seem odd that in some cases the p-value for test 3 is less than that for test 2. After all, how can one be more confident that the average of a set of values is positive than that any individual observation is? The reason is that the rejection region for test 2 is typically larger than that for test 3, and so the p-value is higher. More intuitively, rejecting test 2 requires only providing evidence that at  $v_t^i$  is sufficiently large for some  $t \in \mathcal{T}$ . With large enough  $\mathcal{T}$ , the likelihood of at least one freakishly large draw becomes quite large *even under the null*, and so the p-value for any given observation can be quite high. Appendix C presents a simple example which illustrates this point further.

| Shock                    | $\mathcal{T}$ Start | $\mathcal{T}$ End | $ \mathcal{T} $ | Test 1 | Test 2  | Test 3   |
|--------------------------|---------------------|-------------------|-----------------|--------|---------|----------|
| Monetary Policy          | 2022-02-01          | 2022-02-01        | 1               | 0.349  | 0.346   | 0.349    |
|                          | 2022-02-01          | 2022-03-01        | 2               | 0.349  | 0.012** | 0.017**  |
|                          | 2022-02-01          | 2022-04-01        | 3               | 0.349  | 0.021** | 0.021**  |
|                          | 2022-02-01          | 2022-05-01        | 4               | 0.349  | 0.025** | 0.013**  |
|                          | 2022-02-01          | 2022-06-01        | 5               | 0.349  | 0.036** | 0.014**  |
|                          | 2022-02-01          | 2022-07-01        | 6               | 0.468  | 0.054*  | 0.021**  |
| Central Bank Information | 2021-05-01          | 2021-05-01        | 1               | 0.383  | 0.382   | 0.383    |
|                          | 2021-05-01          | 2021-06-01        | 2               | 0.383  | 0.537   | 0.316    |
|                          | 2021-05-01          | 2021-07-01        | 3               | 0.383  | 0.346   | 0.149    |
|                          | 2021-05-01          | 2021-08-01        | 4               | 0.383  | 0.363   | 0.103    |
|                          | 2021-05-01          | 2021-09-01        | 5               | 0.383  | 0.379   | 0.074*   |
|                          | 2021-05-01          | 2021-10-01        | 6               | 0.383  | 0.442   | 0.072*   |
|                          | 2021-05-01          | 2021-11-01        | 7               | 0.383  | 0.407   | 0.046**  |
|                          | 2021-05-01          | 2021-12-01        | 8               | 0.383  | 0.233   | 0.017**  |
|                          | 2021-05-01          | 2022-01-01        | 9               | 0.383  | 0.260   | 0.014**  |
|                          | 2021-05-01          | 2022-02-01        | 10              | 0.383  | 0.289   | 0.013**  |
|                          | 2021-05-01          | 2022-03-01        | 11              | 0.383  | 0.270   | 0.008*** |
|                          | 2021-05-01          | 2022-04-01        | 12              | 0.383  | 0.300   | 0.007*** |

Table 6: p-values for joint hypothesis tests

Table shows two types of p-value for the joint hypothesis that consecutive observations are drawn from the null. Each line considers three tests that the observed high frequency shocks are drawn from a distribution with a higher mean than would be expected based on pre-COVID transmission of monetary policy, over a period of consecutive months. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

hard to disentangle. In some periods, however, the contribution from the model uncertainty is substantial. This is notably true in early 2021, and occurs when the reduced form shocks are largest, since uncertainty over the parameters is amplified by the magnitude of the shocks. This also explains why the contribution from model uncertainty is low in other periods, when smaller shocks imply less of an impact from model uncertainty.

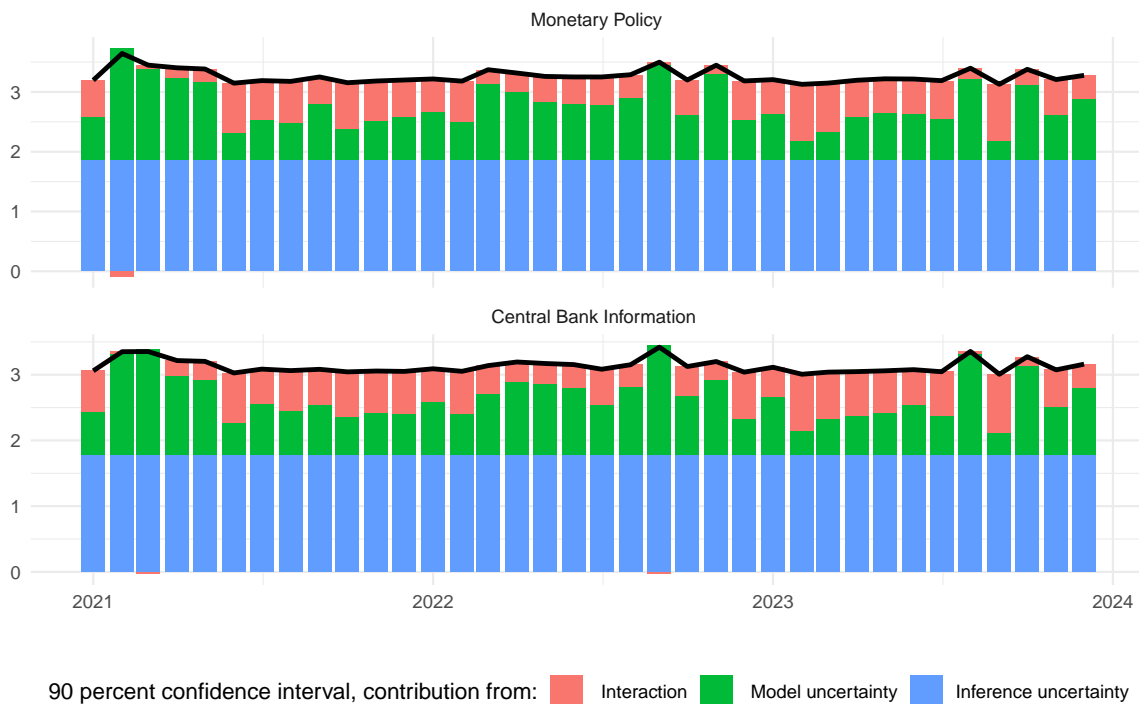


Figure 8: Uncertainty decomposition: 2021-2023

## 6 Interpretation

In the preceding section, we evaluated the strength of the evidence that the link between the data and high-frequency identified shocks had changed sometime in 2022. But what does it really mean to reject any of the tests laid out above? One obvious interpretation is that it means that the transmission of monetary policy is somehow different. But it does not tell us how much it differs, or in what ways, nor if other interpretations might be equally valid. This section thus attempts to answer three related questions. Quantitatively, how much weaker would the transmission of monetary policy have to be to reconcile the observed high-frequency shocks with the data? What other factors, such as the changing impact of other shocks, could explain the results of the previous section? And what implications do our results have for standard structural macro models?

## 6.1 Set up

The key ingredients in our filter which maps data into implied structural shocks are the matrices  $\hat{D}_y$  and  $\hat{\Omega}_y$ , which together define the the mean and covariance of the filter estimate conditional on the parameters,  $\delta_{t|\hat{t},\hat{\theta}}$ .

We can use modified  $\hat{D}_y$  matrices to consider what would happen if the null transmission of the shocks were different by multiplying by a diagonal matrix of positive numbers,  $\Lambda$ , which we partition as:

$$\Lambda = \begin{bmatrix} \Lambda_m & 0_{K \times N} \\ 0_{N \times K} & \Lambda_m \end{bmatrix}$$

Then the the modified causal impact matrix is:

$$\tilde{D}_y = D_y \Lambda = [D_m \Lambda_m D_x \Lambda_m]$$

This represents the statistical model where the the impact of each of the shocks has been scaled by the corresponding entry of  $\Lambda$ .

For example, if top-left first entry of  $\Lambda$  were 0.9 and all other entries were 1, this would capture the scenario where the transmission of the pure monetary shock was uniformly ten percent less effective, in a specific and well-defined sense: a given monetary shock moves all outcomes by ten percent less than in the baseline model. Running the filter on the same data but replacing  $D_y$  with  $\tilde{D}_y$  would then produce a distribution for the high frequency indicators under the new null, one where the impact of monetary shocks was ten percent weaker than usual. Performing our regular hypothesis tests using this modified model would then permit a test of the the hypothesis that monetary policy was at least ten percent weaker on any given sample.

A natural question to ask is how one should pick the entries of such a modifying  $\Lambda$ ? After all, if we alter some column of the impact matrix  $D$ , then without an offsetting modification of the other shocks the data generating process will no long have the same covariance as the data. In the example above, proposing that the monetary shocks have ten percent less impact than usual will mean that some of the variation in the data otherwise explained by the monetary shock is now unexplained by any shock in the model. However, because the other shocks matter only to the extent that they affect the covariance of the data,  $\hat{\Omega}_y$ , one need only be explicit about the assumptions on these other shocks *up to the extent that they affect this covariance*. This allows for something of an end-run around the problem of missing variation. For example, if one posits a modified shock matrix  $\tilde{D}_y$  where only the monetary shock is affected, then computing the filter with  $\hat{\Omega}_y$  unchanged will recover the distribution of monetary shocks under the assumption that the unidentified shocks change *in whatever ways are needed* to guarantee that the data-generating process still matches that estimated

from the sample. The cost of this shortcut is that the time series for the other, unidentified, shocks will not be correct. But we are not interested in those. Of course, if one thinks that the variance of the data has changed on the test period, one can replace  $\hat{\Omega}_y$  with something different. We return to this point later, in Section 6.3.

## 6.2 The changing impact of monetary policy

We start by trying to quantify the change in the strength of the transmission of monetary policy, assuming that the residual covariance in the post-2022 period,  $\hat{\Omega}_y$  is unchanged. To do this, we consider a series of modified structural impact matrices:

$$\tilde{D}(\lambda_1) = D_y \times \begin{bmatrix} \lambda_1 & 0 & \cdots & 0 \\ 0 & 1 & & 0 \\ \vdots & & \ddots & \vdots \\ 0 & 0 & \cdots & 1 \end{bmatrix}$$

for  $\lambda_1 \in (0, 1)$ . Then  $\tilde{D}(\lambda_1)$  embodies the idea that monetary transmission is only  $\lambda_1$  as strong as in the pre-COVID baseline. Put differently, interest rates would need to increase by around  $1/\lambda_1$  percentage points to have the same effect as a 1pp increase pre-COVID.

For each  $\lambda_1$  we then re-run our hypothesis tests from the preceding section, replacing  $D_y$  with  $D_y(\lambda_1)$ . Figure 9 plots the resulting p-values for the six monthly windows  $\mathcal{T}$  starting in February 2022. The connection to Table 6 is that the rightmost values (i.e. at  $\lambda_1 = 1$ ) match the the numbers in the corresponding columns of Table 6.

Overall, this plot suggests that the efficacy of monetary policy dropped quite considerably in March and April 2022 – during this time one would struggle reject at the 5 percent level any null where monetary policy was stronger than about 75 percent of its usual efficacy. In other words, for every three interest rate hikes that the Fed did during this period, they would have needed to do approximately one more to deliver the same degree of tightening as one would have expected pre-COVID. Later in the year, this statement continues to hold true on average. That said, this figure also makes clear that the evidence we present has some limitation. At a 1 percent significance level, we would conclude that monetary policy was incrementally weaker in just April.

Because other shocks only matter to the extent that they affect the covariance of the reduced form residuals, these results hold identically no matter how other non-monetary shocks might have changed so long as the covariance of the data is unaffected. In the next section, we relax that assumption.

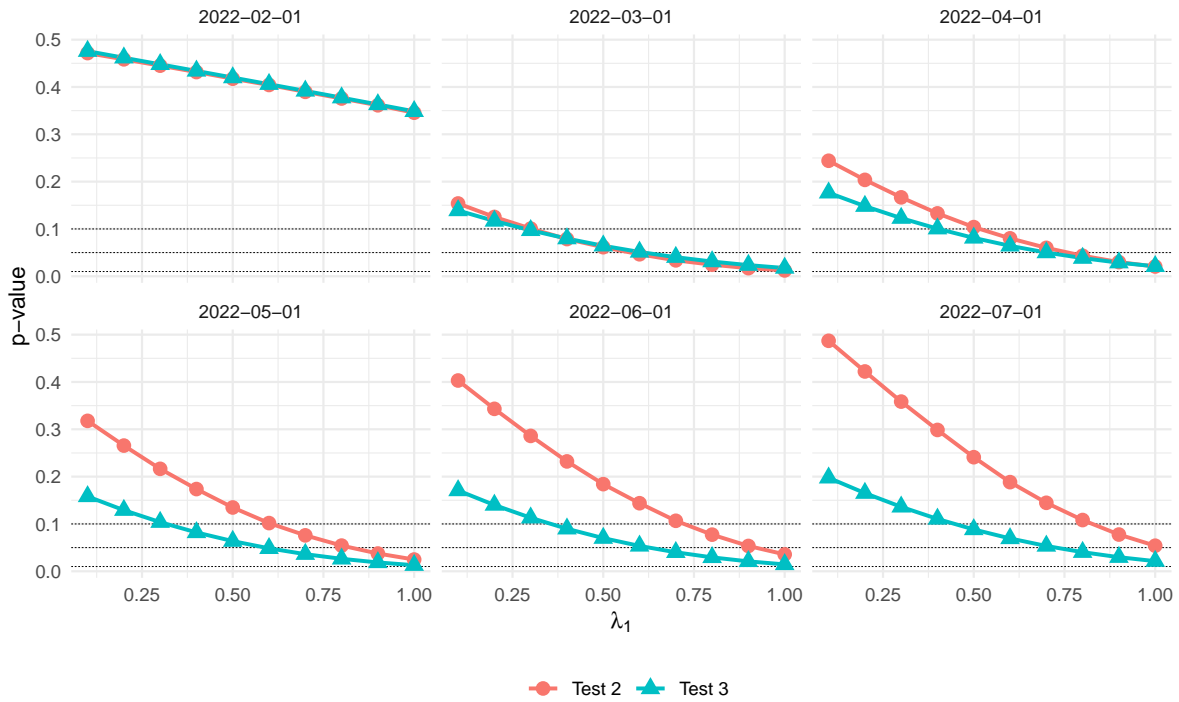


Figure 9: p-values for hypothesis tests as strength of monetary policy shock varies

Figure shows p-values for hypothesis tests described in Section 5.3, under a series of nulls, indexed by  $\lambda_1$ . Horizontal lines show 1, 5, and 10 percent confidence levels. The interpretation of  $\lambda_1$  is that it represents a null where monetary policy is only  $\lambda_1$  as effective as pre-COVID. Each panel computes the corresponding hypothesis tests on a window starting in February 2022 and ending in the titular month. The values for both lines when  $\lambda_1 = 1$  thus match the corresponding columns in Table 6.



### 6.3 Allowing for changing variances

It is, of course, possible that the nature of other shocks changed during 2022. After all, the post-COVID period was a highly unusual one with many of the after-effects of a once-in-a-lifetime global pandemic still being worked out. One of the advantages of our approach is that we can capture the impact of such changes on our filter-implied monetary shocks without specifying exactly how these other shocks changed. This is fortunate, since we do not identify them. Instead, it is enough to describe solely how any such changes would have affected the reduced form residual covariance matrix,  $\hat{\Omega}_y$ . Indeed, if the reduced form residual covariance matrix had changed during the test period, this would undermine that interpretation of our results as capturing a change in the efficacy of monetary policy.<sup>13</sup>

For example, because the filter expects that tighter monetary policy will, at the margin, produce lower inflation – see Figure 5 – it will interpret the persistence of inflation in spite of high interest rates as a sign of weaker monetary policy. Of course, other offsetting shocks could be driving the slow decline in inflation, but the filter adjusts for this based on the usual correlation of the other macro variables. However, if variance of those offsetting shocks has changed then the filter will account for them incorrectly.

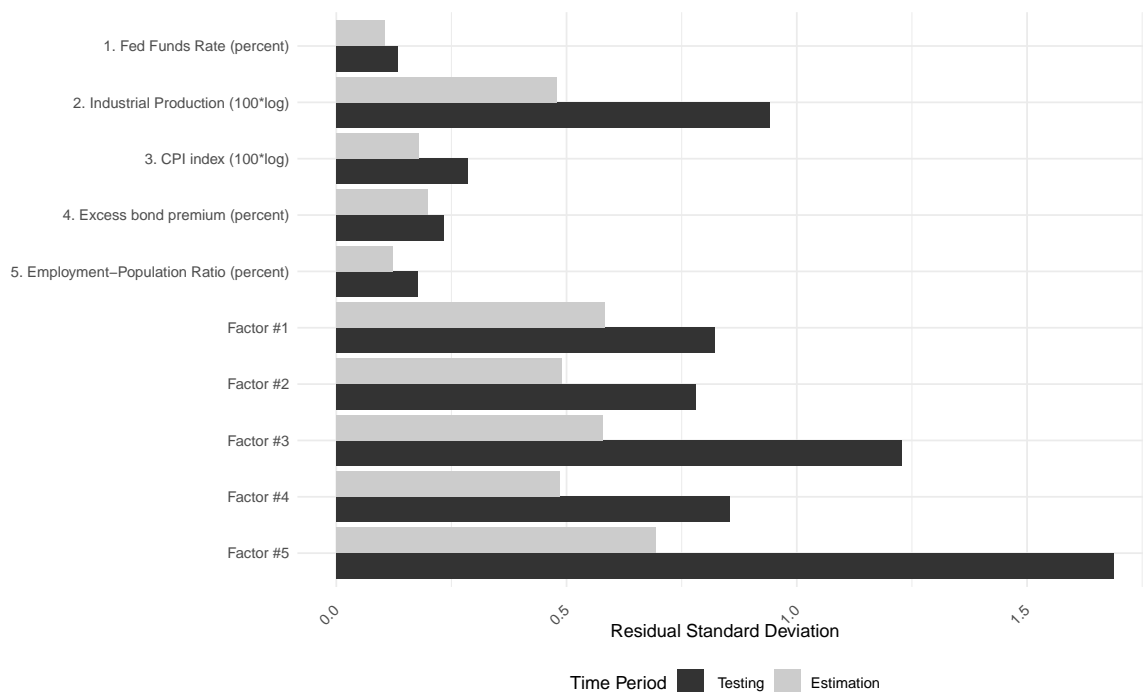


Figure 10: Reduced form residual standard deviations

Bars show the standard deviation of the reduced form shocks on the estimation and testing periods. In both cases, the autoregressive parameters,  $B_j$ , are assumed to be those estimated on the pre-COVID data.

<sup>13</sup>A further possibility that we do not investigate is that the autoregressive coefficients of the VAR also changed. The brevity of the post-tightening period means that such an exercise will likely be highly imprecise.

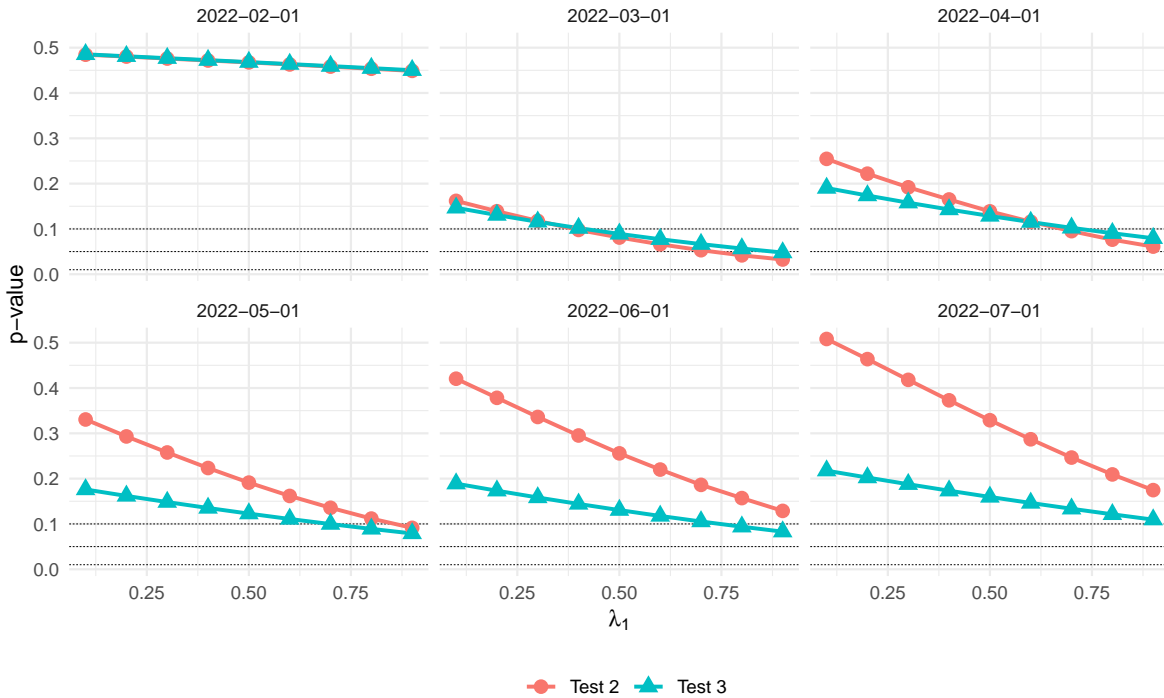


Figure 11: p-values for hypothesis tests as strength of monetary policy shock varies: Post-2021 residual covariance

Figure shows p-values for hypothesis tests described in Section 5.3, under a series of nulls, indexed by  $\lambda_1$ . This version uses the post-2021 residual covariance for  $\hat{\Omega}_y$ . Horizontal lines show 1, 5, and 10 percent confidence levels. The interpretation of  $\lambda_1$  is that it represents a null where monetary policy is only as effective as pre-COVID. Each panel computes the corresponding hypothesis tests on a window starting in February 2022 and ending in the titular month. The values for both lines when  $\lambda_1 = 1$  thus match the corresponding columns in Table 6.

Figure 10 presents preliminary evidence that the reduced form covariance has indeed changed. It shows the standard deviations of the reduced form residuals for the data series in the VAR. Every series shows an increase in the standard deviation, in some cases more than doubling. And so in Figure 11, we repeat the exercise in Figure 9, but replacing  $\hat{\Omega}_y$  with the covariance matrix of the VAR residuals from 2021 onwards. Given we use the same  $D_y$  matrix, the interpretation is that we are testing whether the impact of monetary policy has changed, allowing for any excess volatility in the data to be explained by changing variances of the other shocks. The results do somewhat mitigate our earlier findings on the transmission of monetary policy, suggesting that the evidence of given reductions in the impact of monetary policy are a little less strong. Nevertheless, the implication that policy was weaker than expected in at least March and April of 2022 remains robust. Moreover, this experiment sets a relatively high threshold for concluding that the impact of monetary policy has changed, since the residual shock covariance itself will be imprecisely estimated and including the full post-2021 period may not be representative of the population covariance during 2022.

In Appendix B.3 we repeat the hypothesis tests using the shock series which includes policymakers' speeches and show that they strengthen our results. In Appendix Figure A13 we show the post-2022 outturns compared to the filter-inferred shocks. The differences are larger than in the baseline, a fact confirmed by Appendix Table A1, which rejects unchanged monetary transmission even at the 1 percent level. Similarly, Figures A14 and A15 imply that monetary policy is statistically significantly weaker even if the covariance of other shocks is different.

## 7 Conclusions

In this paper we propose a general method for assessing whether a dynamic data generating process has changed when one has externally-identified shocks. We applied it to the question of whether the transmission of monetary policy in the US was different during the 2022 tightening cycle. We conclude that during 2022 the macro data and the shocks were not consistent with unchanged monetary transmission and that monetary policy was probably around 25 percent less effective than would have been expected pre-COVID. Importantly, our method takes into account the full set of shocks and so is robust to concerns that other shocks may be acting to mask the impact of monetary policy, and thus giving the appearance of an altered transmission mechanism. When we allow for changing shock variance, our findings are weakened but not overturned. When our series also includes policymakers' speeches, they are strengthened.

This method potentially has scope for broader application, to almost any setting where one has a well-identified shock and wishes to assess whether the propagation of that shock has changed. For example, using narrative identification of fiscal shocks one might be able to more precisely examine the extent to which public spending multipliers are state-dependent.

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## A Data

### A.1 Details on the baseline shock series

Let  $p_{t_1}^{FF4}$  be the last traded price of FF4 before the event window starts, and  $p_{t_2}^{FF4}$  be the first traded price of FF4 once the event window ends. The raw shock for FF4  $\xi_t^{FF4}$  is calculated as

$$\xi_t^{FF4} = p_{t_2}^{FF4} - p_{t_1}^{FF4}$$

Let  $p_{t_1}^{SP500}$  be the last traded value of the S&P 500 index before the event window starts, and  $p_{t_2}^{SP500}$  be the first value of the S&P 500 index once the event window ends. The raw shock for S&P 500  $\xi_t^{SP500}$  is calculated as

$$\xi_t^{SP500} = \ln p_{t_2}^{SP500} - \ln p_{t_1}^{SP500}$$

### A.2 Speeches

The fundamental idea to construct high-frequency monetary shocks from speeches by members of the FOMC is the same as for FOMC meetings. We take the change in asset prices around the event dates, as described in section 3.1 and appendix A.1 . To follow this procedure, we have to collect exact dates and times of the respective speeches, and choose an appropriate event time window. We collect dates of speeches from the homepage of the Federal Reserve Board. Starting from year 2010, this page also links the speech as a pdf document, including the time the speech was released (including the timezone). We follow [Swanson and Jayawickrema \[2023\]](#) and set the event window to 120 minutes. Using this procedure, we are able to use 714 speeches ranging from 2010 to 2023 to construct raw shocks from both the fourth Federal Funds Futures (FF4) and the S&P 500 index. As with regular meetings, we aggregate to monthly frequency by taking the sum of the individual shocks within a given month.

### A.3 Extra time series plots

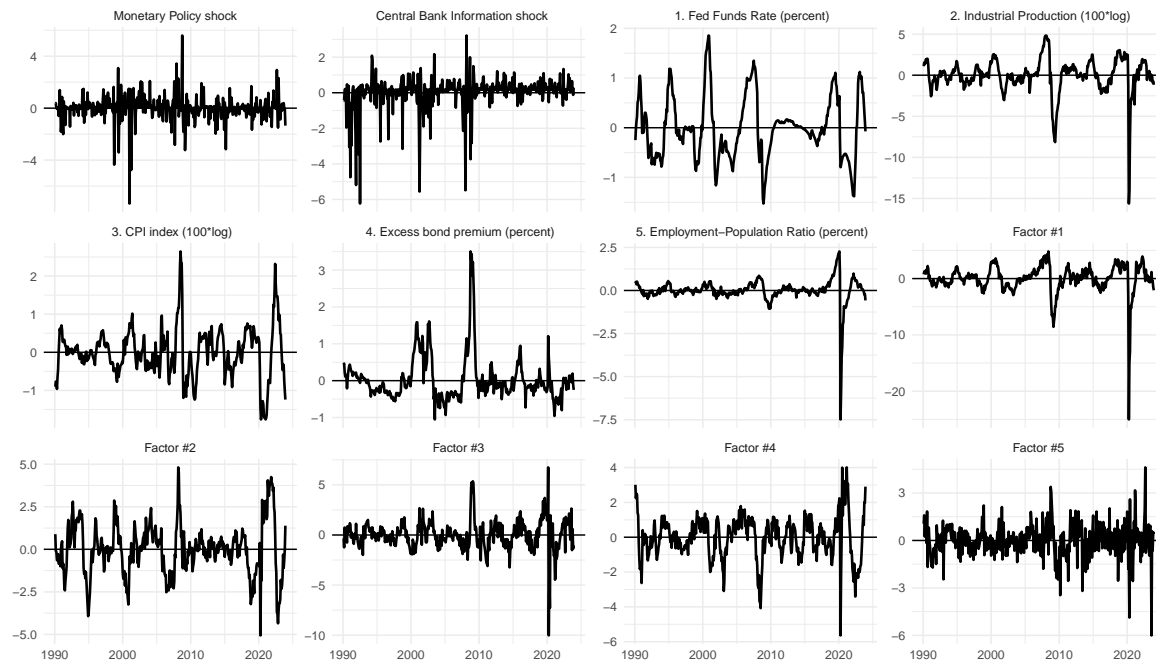


Figure A1: Data in the baseline VAR

Figure shows the data series in the baseline vector autoregression including factors, after transformations.

## B Additional results

### B.1 More results under the baseline

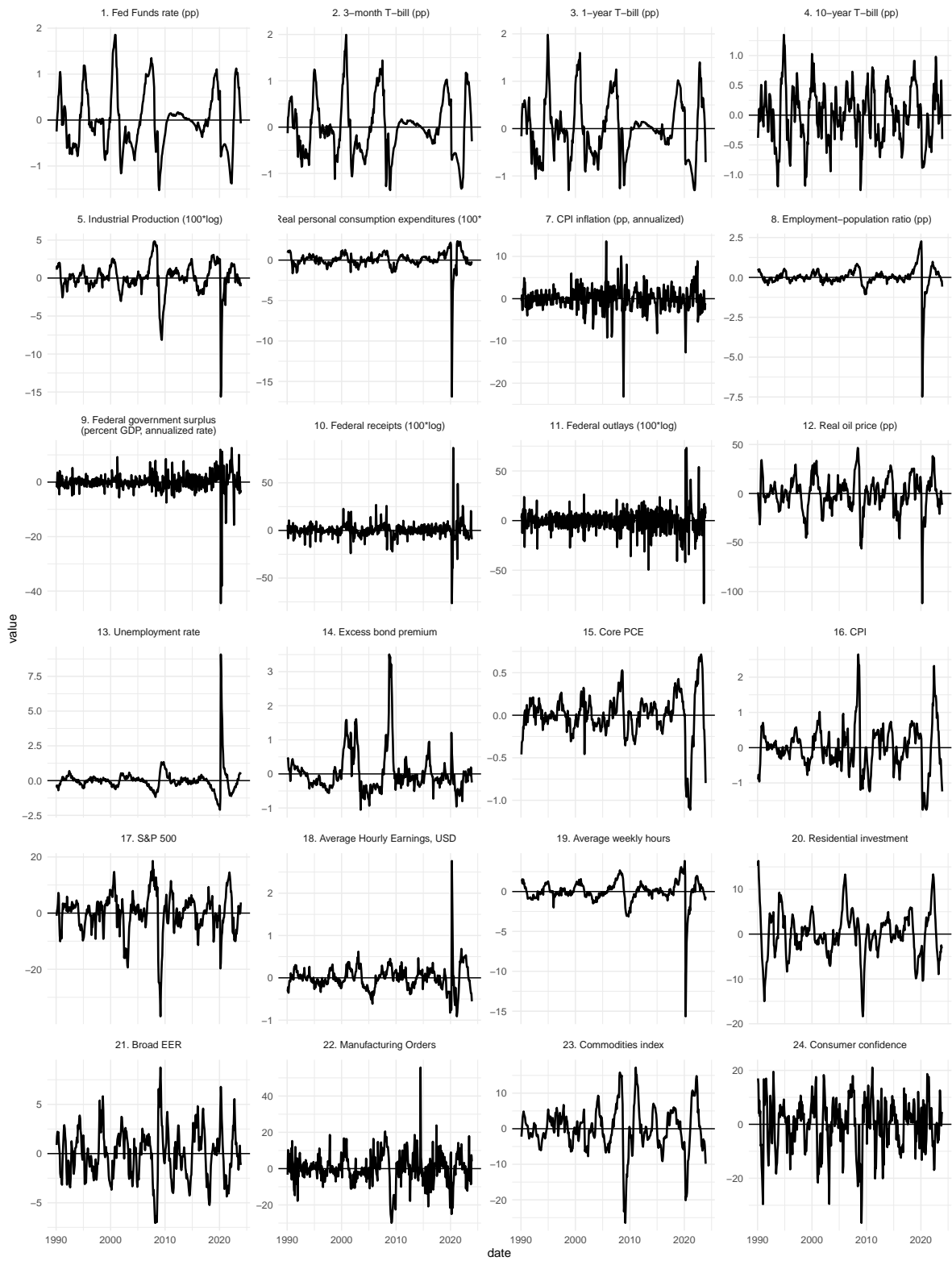


Figure A2: All time series data

Figure shows the full set of data in Table 2, after transformations.



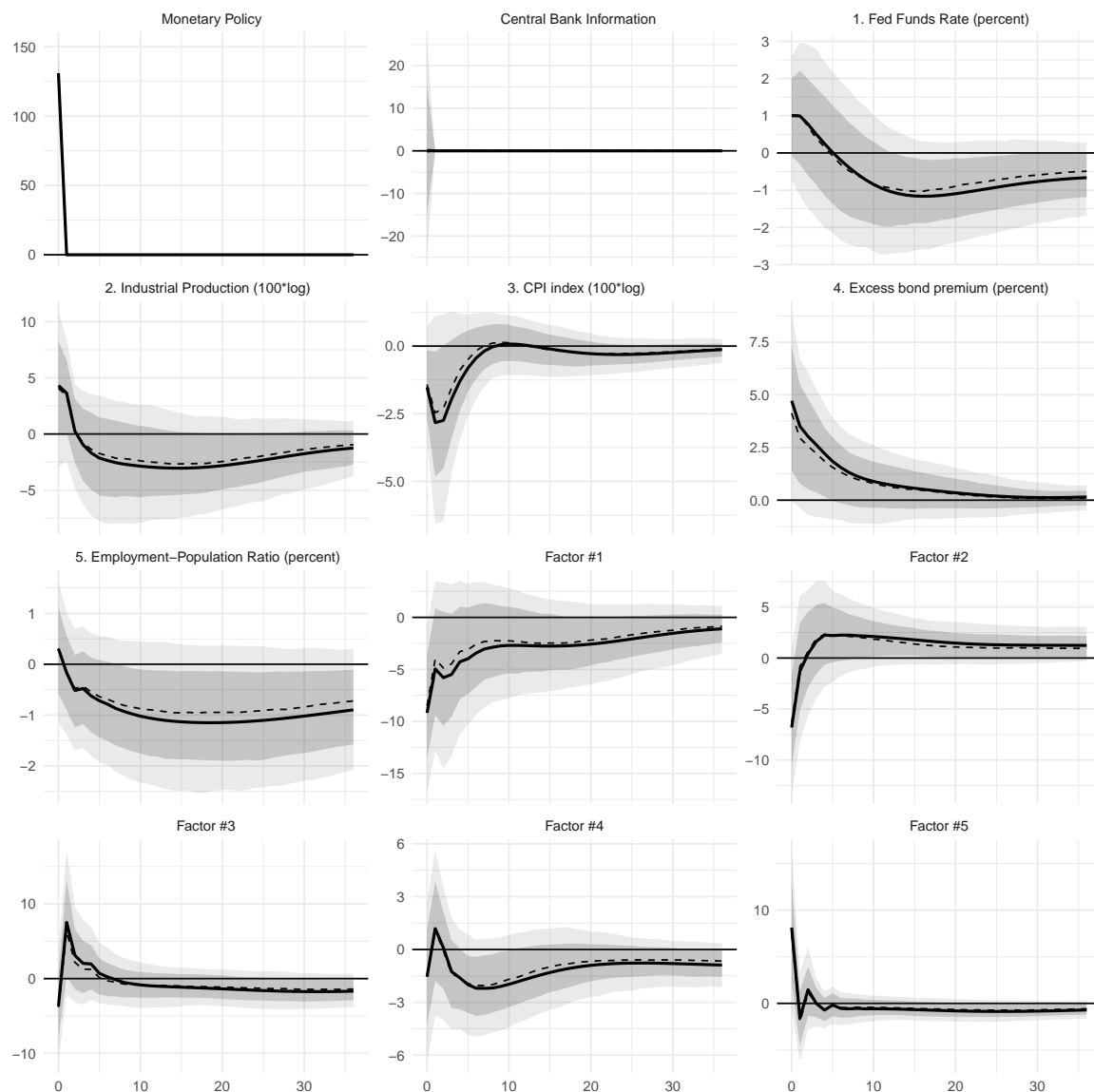


Figure A3: Impulse responses: Monetary policy shock

Figure shows the response of the headline variables to a monetary policy shock, scaled to a 1 percentage point increase in the Federal Funds Rate. Solid lines are point estimates and shaded regions are the 68 and 90 percent confidence intervals from a bootstrap with  $K = 1000$  replications. Dashed lines show median responses from the bootstrap.

## B.2 Alternative specifications

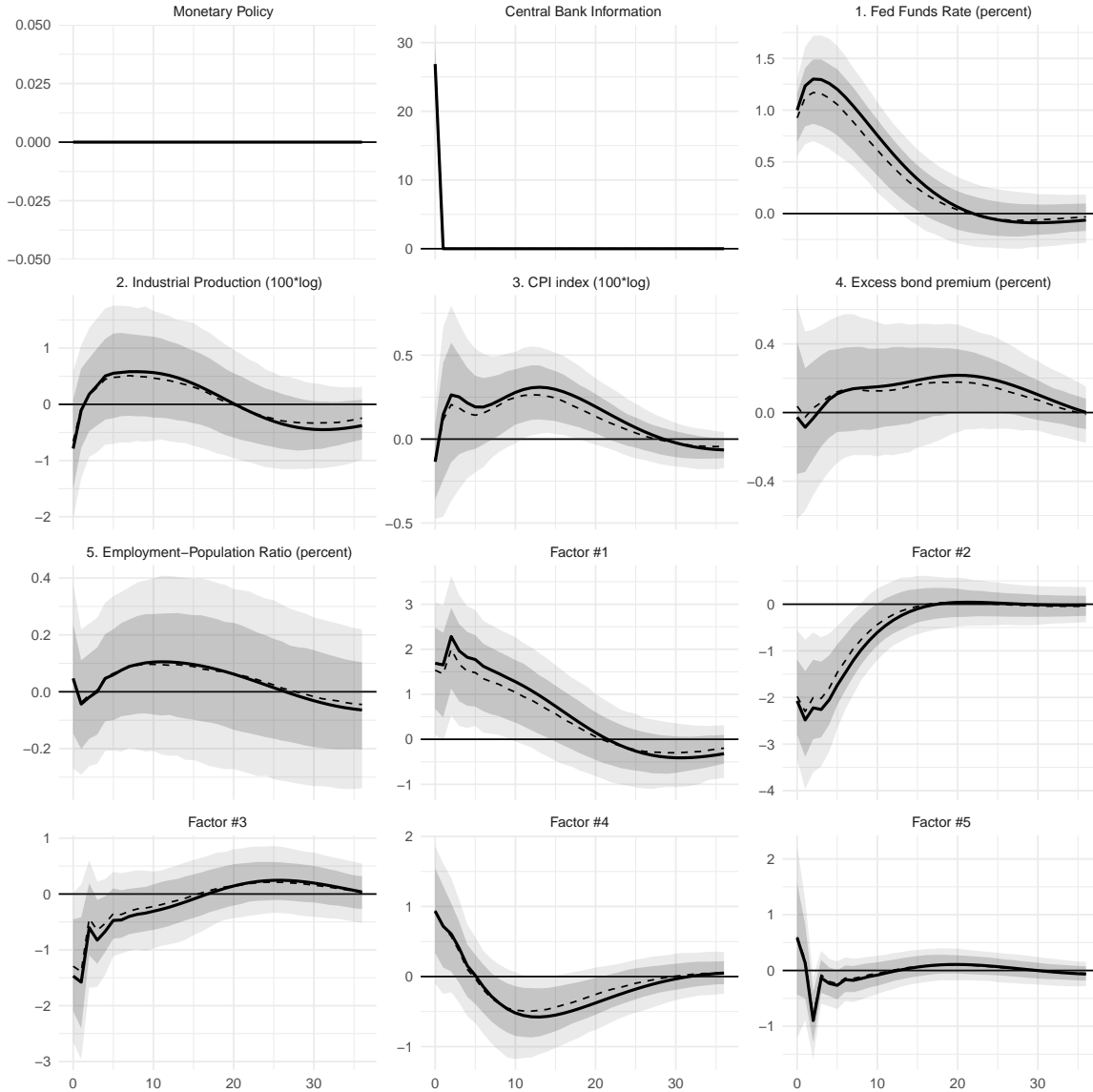


Figure A4: Impulse responses: Central Bank information shock

Figure shows the response of the headline variables to a monetary policy shock, scaled to a 1 percentage point increase in the Federal Funds Rate. Solid lines are point estimates and shaded regions are the 68 and 90 percent confidence intervals from a bootstrap with  $K = 1000$  replications. Dashed lines show median responses from the bootstrap.

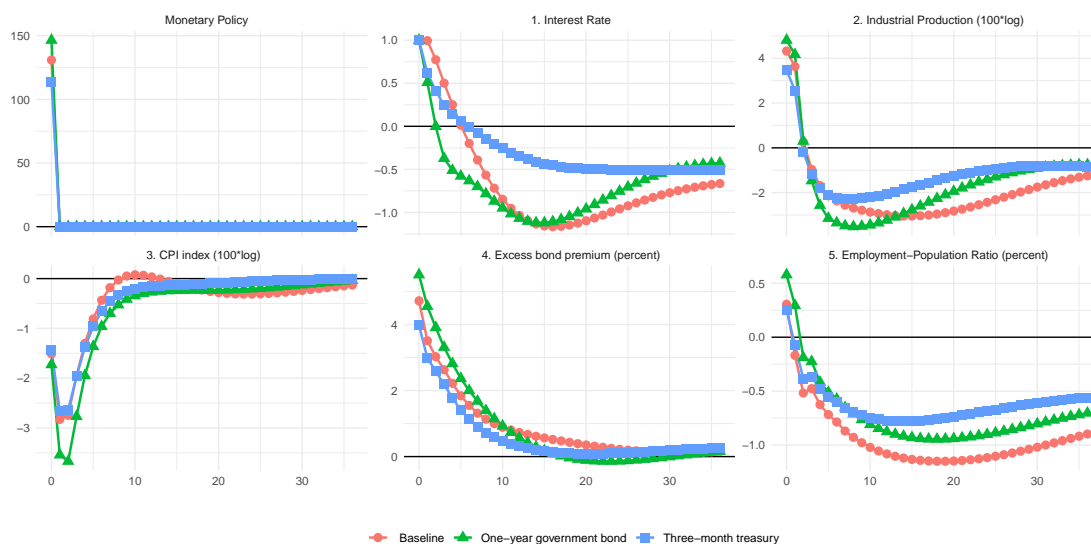


Figure A5: Impulse responses: Monetary policy shock, different interest rate series

Figure shows the point estimates of the response of a factor-augmented VAR to a monetary policy shock, scaled to a 1 percentage point increase in the interest rate. Lines differ in their measure of the interest rate, which in the baseline is the Federal Funds Rate. All specifications include five factors, responses of which are not shown here.

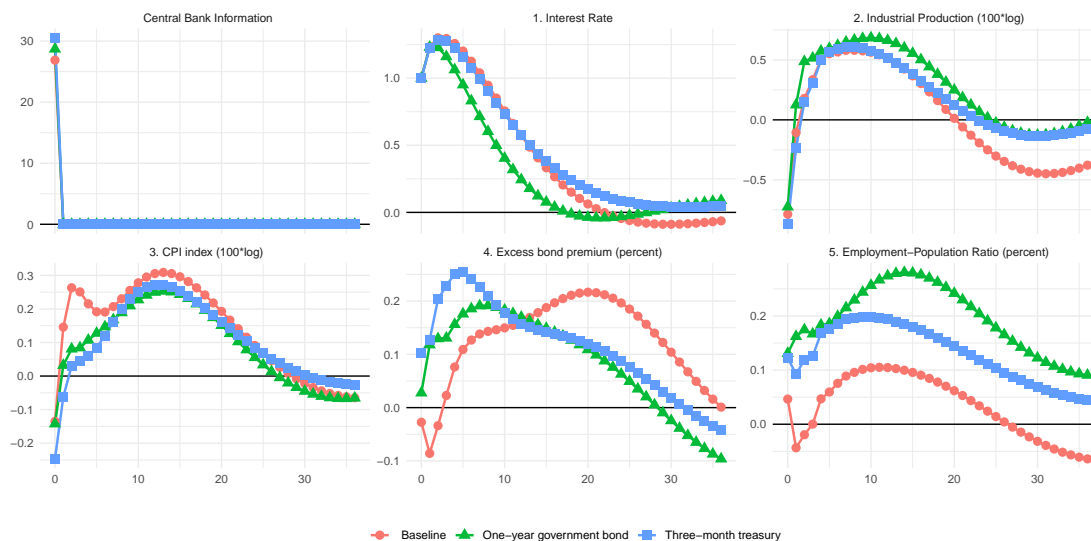


Figure A6: Impulse responses: Central Bank information shock, different interest rate series

Figure shows the point estimates of the response of a factor-augmented VAR to a Central Bank information shock, scaled to a 1 percentage point increase in the interest rate. Lines differ in their measure of the interest rate, which in the baseline is the Federal Funds Rate. All specifications include five factors, responses of which are not shown here.

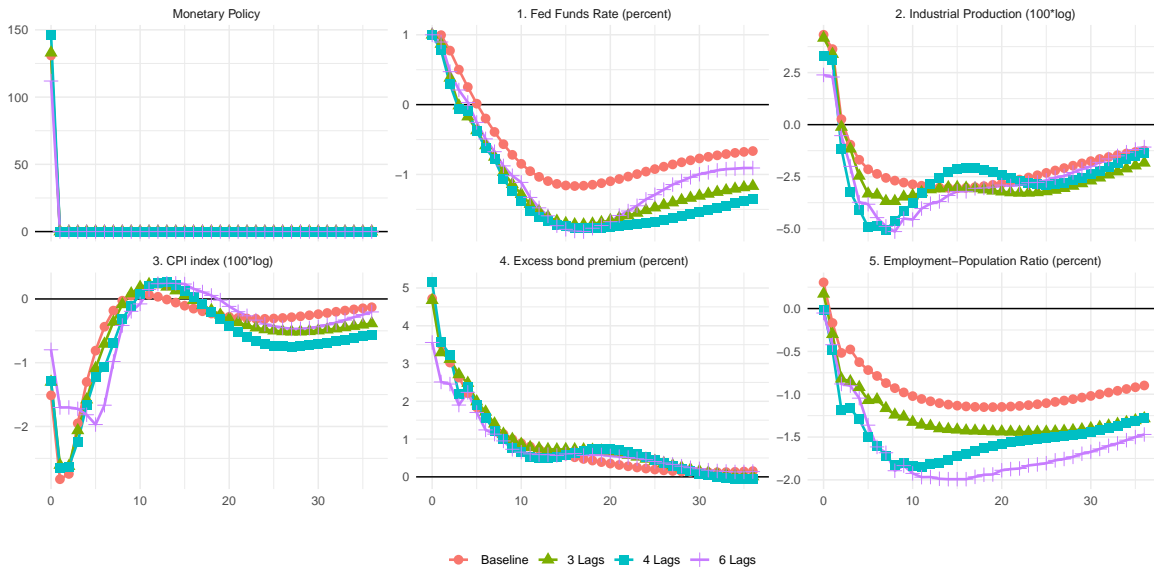


Figure A7: Impulse responses: Monetary policy shock, different lag structures

Figure shows the point estimates of the response of a factor-augmented VAR to a monetary policy shock, scaled to a 1 percentage point increase in the interest rate. Lines differ in their lag length, which in the baseline is two periods, chosen by AIC. All specifications include five factors, responses of which are not shown here.

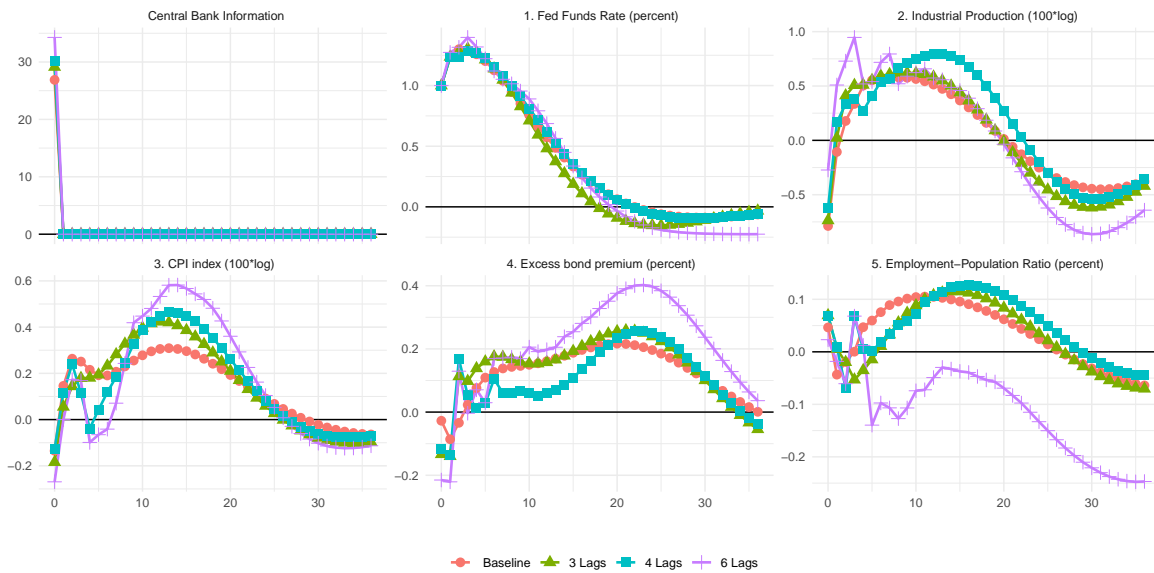


Figure A8: Impulse responses: Central Bank information shock, different lag structures

Figure shows the point estimates of the response of a factor-augmented VAR to a Central Bank information shock, scaled to a 1 percentage point increase in the interest rate. Lines differ in their lag length, which in the baseline is two periods, chosen by AIC. All specifications include five factors, responses of which are not shown here.

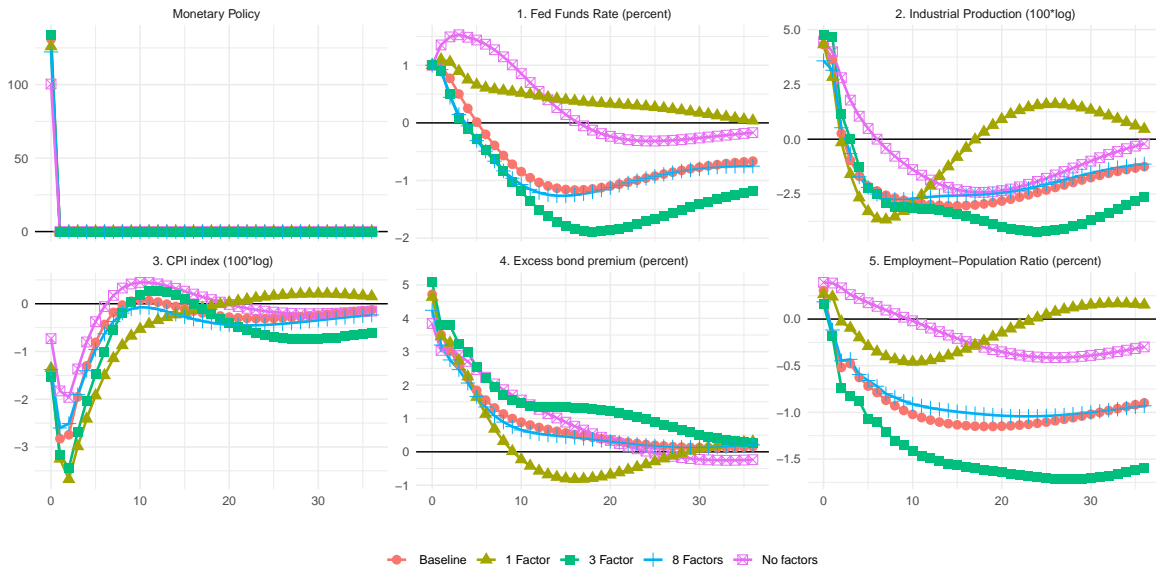


Figure A9: Impulse responses: Monetary policy shock, different number of factors

Figure shows the point estimates of the response of a factor-augmented VAR to a monetary policy shock, scaled to a 1 percentage point increase in the interest rate. Lines differ in the number of factors, which in the baseline is five. All specifications include five factors, responses of which are not shown here.

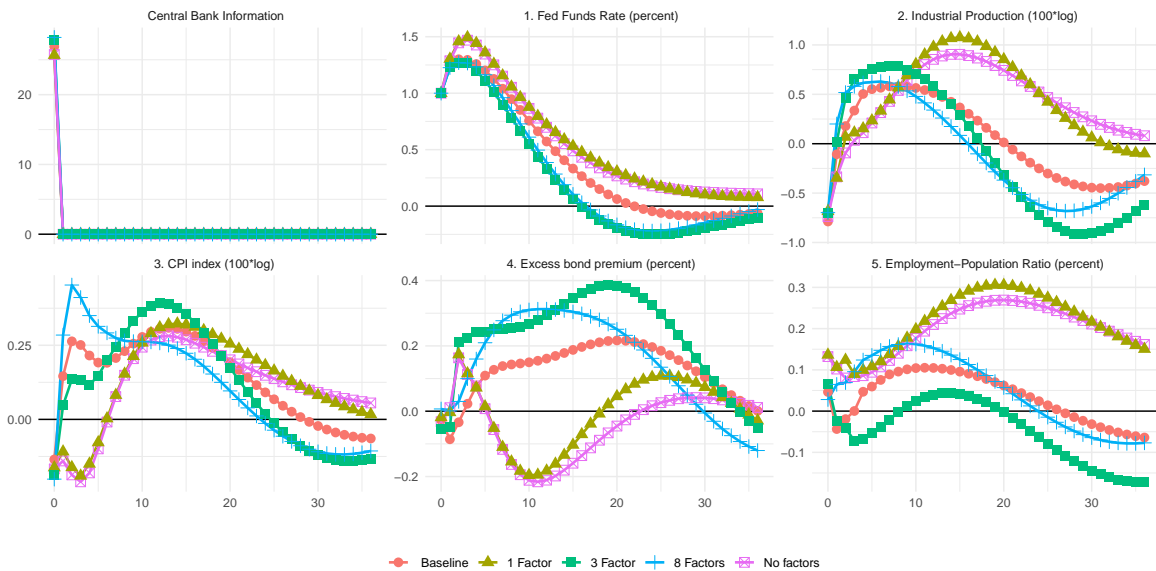


Figure A10: Impulse responses: Central Bank information shock, different number of factors

Figure shows the point estimates of the response of a factor-augmented VAR to a Central Bank information shock, scaled to a 1 percentage point increase in the interest rate. Lines differ in the number of factors, which in the baseline is five. All specifications include five factors, responses of which are not shown here.

### B.3 Including speeches

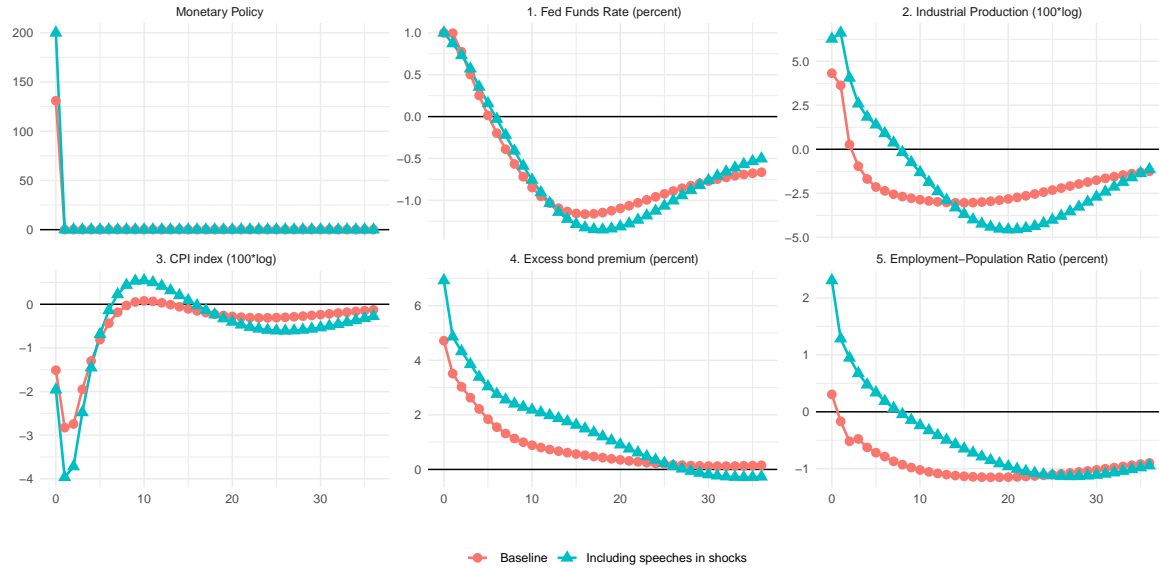


Figure A11: Impulse responses: Monetary policy shock, including speeches.

Figure shows the point estimates of the response of a factor-augmented VAR to a monetary policy shock, scaled to a 1 percentage point increase in the interest rate. Lines differ in the number of factors, which in the baseline is five. All specifications include five factors, responses of which are not shown here.

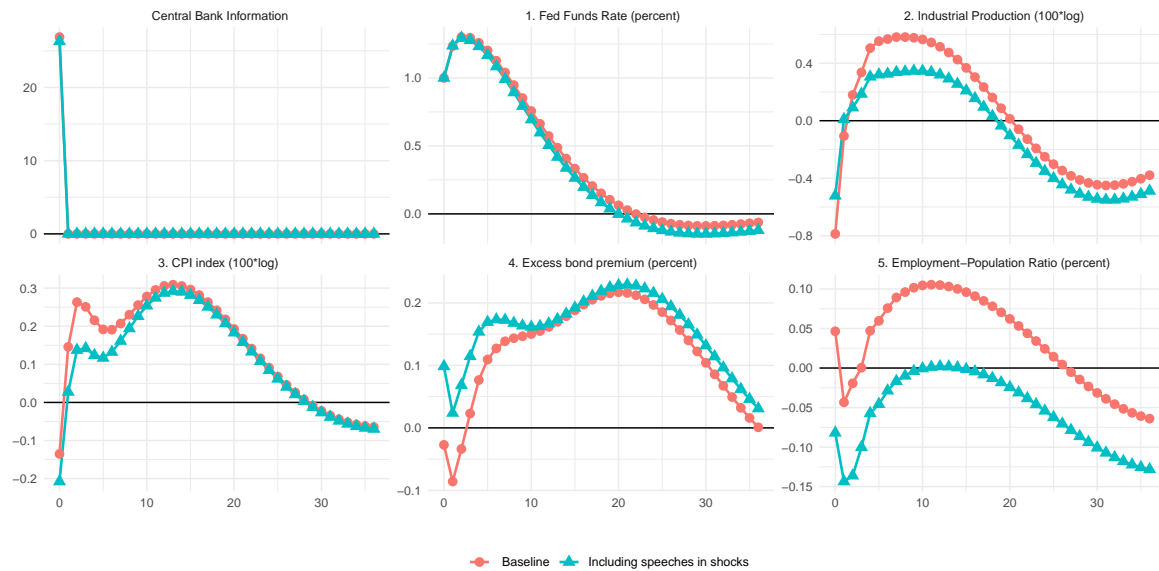


Figure A12: Impulse responses: Central Bank information shock, including speeches.

Figure shows the point estimates of the response of a factor-augmented VAR to a Central Bank information shock, scaled to a 1 percentage point increase in the interest rate. Lines differ in the number of factors, which in the baseline is five. All specifications include five factors, responses of which are not shown here.

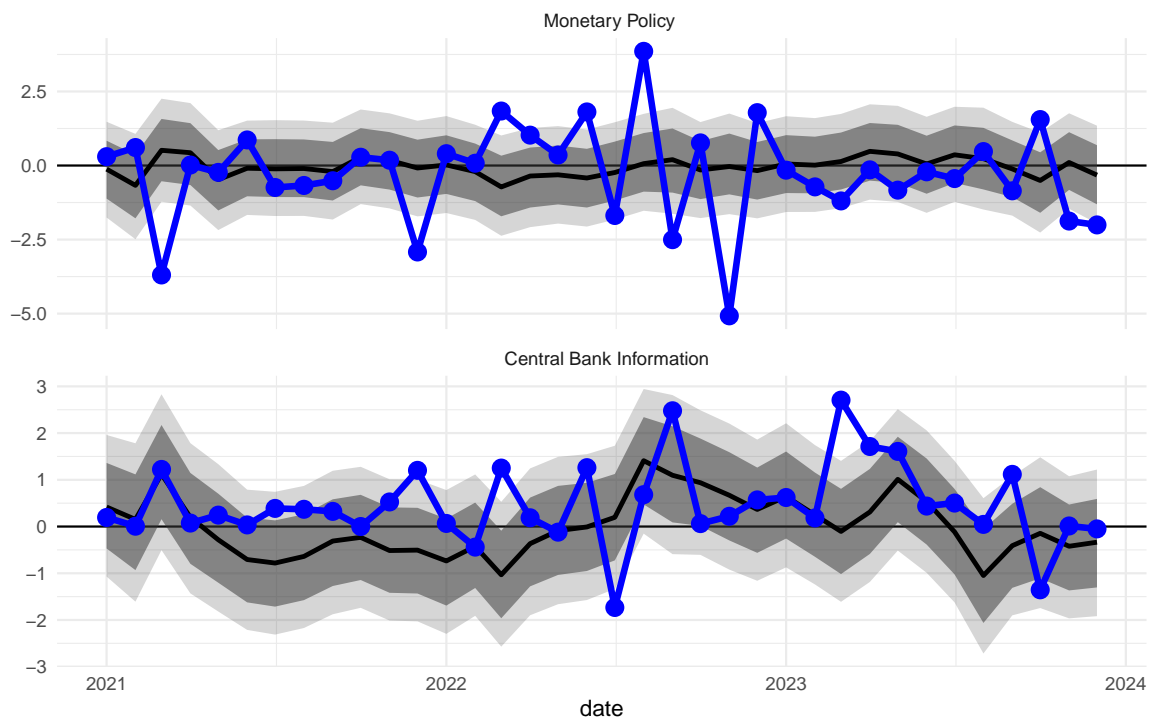


Figure A13: Inferred and realized shocks: 2021-2023, including speeches in shocks

Solid lines are point estimates of the inferred shocks under the null hypothesis that the data generating process remains unchanged. The shaded regions are the 68 and 90 percent confidence intervals from a bootstrap with  $K = 1000$  replications. The blue line with large dots is the actual shock, computed from high frequency data. This version uses shocks including policymakers' speeches.

| Shock                    | $\mathcal{T}$ Start | $\mathcal{T}$ End | $ \mathcal{T} $ | Test 1 | Test 2   | Test 3   |
|--------------------------|---------------------|-------------------|-----------------|--------|----------|----------|
| Monetary Policy          | 2022-02-01          | 2022-02-01        | 1               | 0.385  | 0.382    | 0.385    |
|                          | 2022-02-01          | 2022-03-01        | 2               | 0.385  | 0.011**  | 0.019**  |
|                          | 2022-02-01          | 2022-04-01        | 3               | 0.385  | 0.008*** | 0.006*** |
|                          | 2022-02-01          | 2022-05-01        | 4               | 0.385  | 0.013**  | 0.006*** |
|                          | 2022-02-01          | 2022-06-01        | 5               | 0.385  | 0.002*** | 0.001*** |
|                          | 2022-02-01          | 2022-07-01        | 6               | 0.933  | 0.003*** | 0.008*** |
| Central Bank Information | 2021-05-01          | 2021-05-01        | 1               | 0.281  | 0.281    | 0.281    |
|                          | 2021-05-01          | 2021-06-01        | 2               | 0.281  | 0.311    | 0.162    |
|                          | 2021-05-01          | 2021-07-01        | 3               | 0.281  | 0.192    | 0.061*   |
|                          | 2021-05-01          | 2021-08-01        | 4               | 0.281  | 0.162    | 0.029**  |
|                          | 2021-05-01          | 2021-09-01        | 5               | 0.281  | 0.182    | 0.022**  |
|                          | 2021-05-01          | 2021-10-01        | 6               | 0.399  | 0.234    | 0.026**  |
|                          | 2021-05-01          | 2021-11-01        | 7               | 0.399  | 0.185    | 0.013**  |
|                          | 2021-05-01          | 2021-12-01        | 8               | 0.399  | 0.070*   | 0.003*** |
|                          | 2021-05-01          | 2022-01-01        | 9               | 0.399  | 0.070*   | 0.002*** |
|                          | 2021-05-01          | 2022-02-01        | 10              | 0.507  | 0.091*   | 0.003*** |
|                          | 2021-05-01          | 2022-03-01        | 11              | 0.507  | 0.012**  | 0.000*** |
|                          | 2021-05-01          | 2022-04-01        | 12              | 0.507  | 0.016**  | 0.000*** |

Table A1: p-values for joint hypothesis tests, including speeches in shocks

Table shows two types of p-value for the joint hypothesis that consecutive observations are drawn from the null. Each line considers three tests that the observed high frequency shocks are drawn from a distribution with a higher mean than would be expected based on pre-COVID transmission of monetary policy, over a period of consecutive months. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ . This version uses shocks including policymakers' speeches.



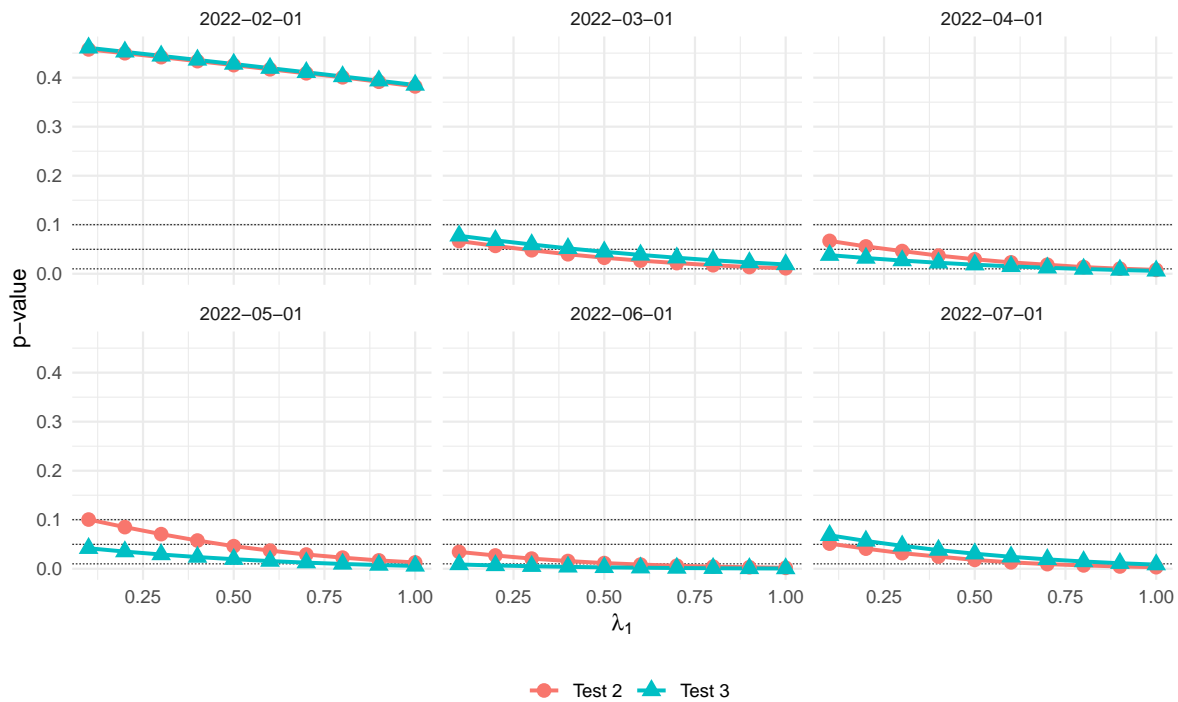


Figure A14: p-values for hypothesis tests as strength of monetary policy shock varies

Figure shows p-values for hypothesis tests described in Section 5.3, under a series of nulls, indexed by  $\lambda_1$ . Horizontal lines show 1, 5, and 10 percent confidence levels. The interpretation of  $\lambda_1$  is that it represents a null where monetary policy is only  $\lambda_1$  as effective as pre-COVID. Each panel computes the corresponding hypothesis tests on a window starting in February 2022 and ending in the titular month. The values for both lines when  $\lambda_1 = 1$  thus match the corresponding columns in Table 6. This version uses shocks including policymakers' speeches.

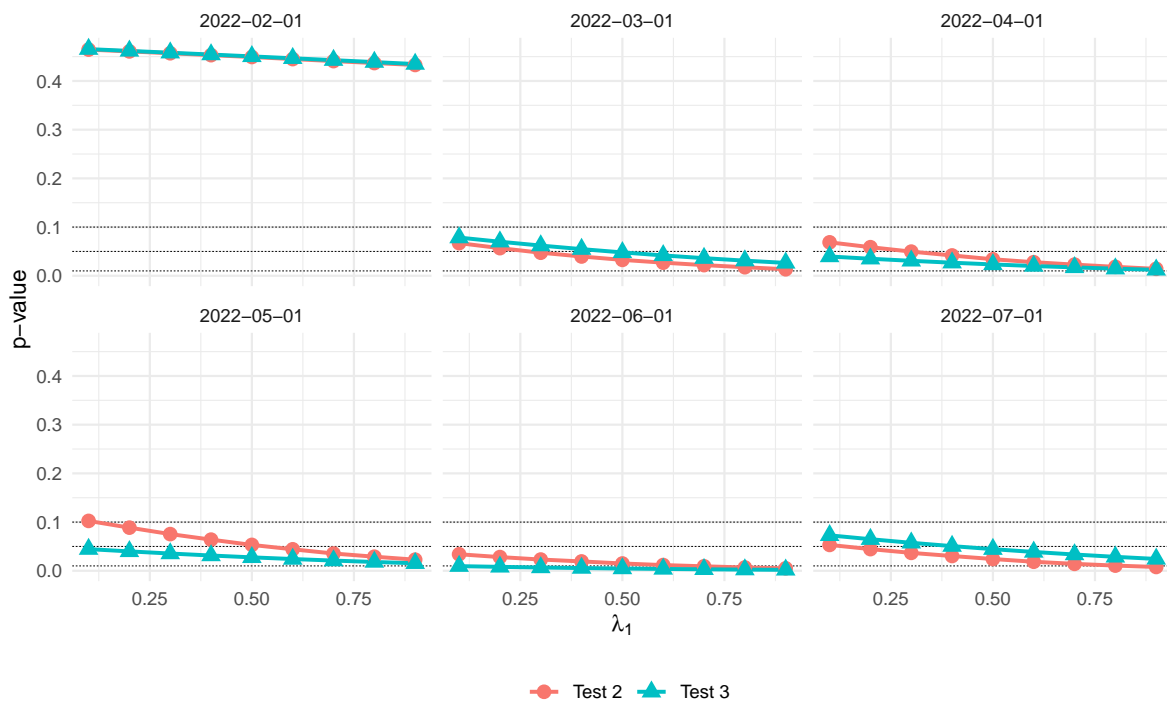


Figure A15: p-values for hypothesis tests as strength of monetary policy shock varies: Post-2021 residual covariance

Figure shows p-values for hypothesis tests described in Section 5.3, under a series of nulls, indexed by  $\lambda_1$ . Horizontal lines show 1, 5, and 10 percent confidence levels. The interpretation of  $\lambda_1$  is that it represents a null where monetary policy on only  $\lambda_1$  as effective as pre-COVID. Each panel computes the corresponding hypothesis tests on a window starting in February 2022 and ending in the titular month. The values for both lines when  $\lambda_1 = 1$  thus match the corresponding columns in Table 6. This version uses the post-2021 residual covariance for  $\hat{\Omega}_y$ , and shocks including policymakers' speeches.

## B.4 External instruments

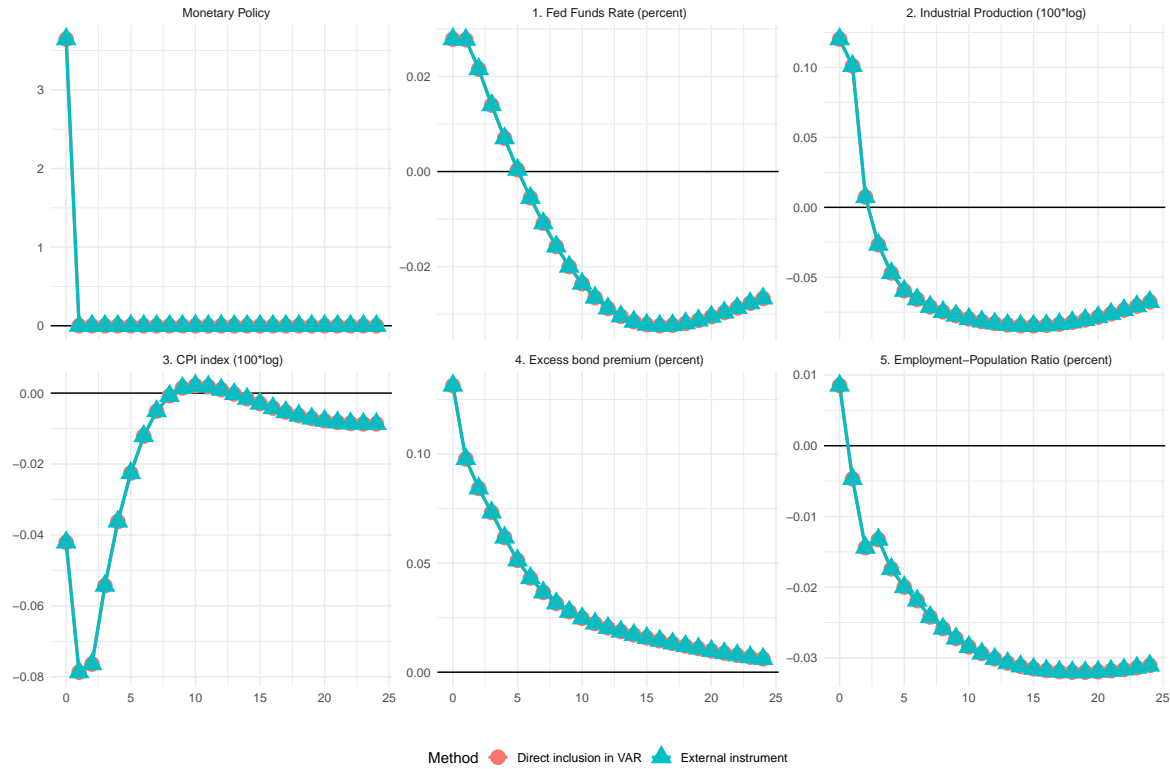


Figure A16: Direct inclusion versus external instruments: Monetary policy shock

Line labelled “External instrument” shows the point estimate of the response of the headline variables to a one-standard deviation shock computed using the high-frequency monetary policy series as an external instrument. The line labelled “Direct inclusion in VAR” are our baseline IRFs, rescaled to match the same initial impulse as for the external instrument. The five factors are included in the estimation but, for clarity, their responses are omitted.

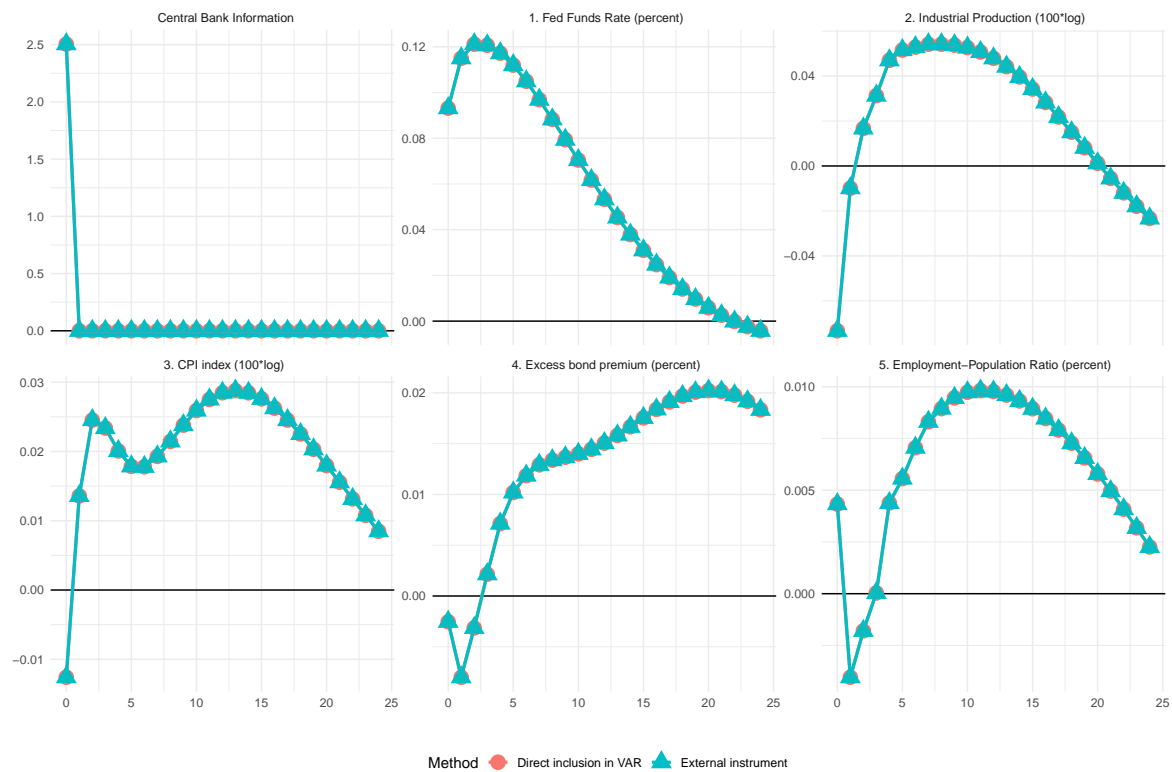


Figure A17: Direct inclusion versus external instruments: Central Bank information shock

Line labelled “External instrument” shows the point estimate of the response of the headline variables to a one-standard deviation shock computed using the high-frequency monetary policy series as an external instrument. The line labelled “Direct inclusion in VAR” are our baseline IRFs, rescaled to match the same initial impulse as for the external instrument. The five factors are included in the estimation but, for clarity, their responses are omitted.

## C Deriving the hypothesis tests

Here we derive the hypothesis tests we apply in Section 5.3.

### C.1 Set up

Recall that  $\eta_t$  is the difference between the observed high-frequency shocks and their filter-inferred equivalent, given by  $\eta = v_t - \bar{\delta}_t$  where  $\bar{\delta}_t = \int_{\delta} \delta f_t(\delta) d\delta$  and  $(\sigma_t^i)^2 = \int_{\delta} (\delta - \bar{\delta}_t)^2 f_t(\delta) d\delta$  are the mean and variance of the filter-implied distribution for the  $i$ th structural shock.

We define the parameter  $\mu_t = \mathbb{E}_t \eta_t$  as the mean of  $\eta_t$  and want to test jointly whether some or all of these parameters are positive on a sample  $t = 1, \dots, T$ . Specifically, we want to test:

**Test 1**  $H_0 : \mu_t^i \leq 0$  for some  $t \in \mathcal{T}$  vs.  $H_1 : \mu_t^i > 0$  for all  $t \in \mathcal{T}$

**Test 2**  $H_0 : \mu_t^i \leq 0$  for all  $t \in \mathcal{T}$  vs.  $H_1 : \mu_t^i > 0$  for some  $t \in \mathcal{T}$

**Test 3**  $H_0 : \bar{\mu}^i \leq 0$  vs.  $H_1 : \bar{\mu}^i > 0$ , where  $\bar{\mu}^i = \frac{1}{|\mathcal{T}|} \sum_{t \in \mathcal{T}} \mu_t^i$

It is convenient to fix  $i$  and work with the normalized shock  $x_t = \eta_t^i / \sigma_t^i$ , where  $\sigma_t^i$  is the root mean square error of the filter-inferred shock. As is common, we assume that this is known. Normality of the filter-inferred estimator implies that:

$$x_t \sim_{i,i,d} N(\mu_t, 1) \quad \forall t \in \mathcal{T}$$

Given this, we can deal with test 3 straightforwardly, since it becomes a standard univariate one-sided test of the sample mean. That is, we can construct the test statistic

$$\bar{x}_{\mathcal{T}} = \frac{1}{|\mathcal{T}|} \sum_{t \in \mathcal{T}} x_t$$

Then

$$\mathbb{E} \bar{x}_{\mathcal{T}} = \bar{\mu}^i = \frac{1}{|\mathcal{T}|} \sum_{t \in \mathcal{T}} \mu_t^i$$

where  $T = |\mathcal{T}|$ . And so

$$\bar{x}_{\mathcal{T}} \sim N(\bar{\mu}^i, 1/\sqrt{T})$$

Under the null for test 3, the mean of this distribution is zero, so we can compute one-sided p-values for the data from:

$$p_3(\bar{x}_{\mathcal{T}}) = 1 - \Phi(\bar{x}_{\mathcal{T}}/\sqrt{T})$$

However, for tests 1 and 2, things are less straightforward. We derive likelihood ratio tests more formally in the following sections.

## C.2 Rejection Regions

Tests 1 and 2 both consider the tests of the form  $H_0 : \theta \in \Theta_0$  versus  $H_1 : \theta \in \Theta_0^c$ , where  $\Theta_0$  is a restricted subset of the parameter space  $\Theta$  and  $\Theta_0^c$  is the complement of  $\Theta_0$ .

The likelihood ratio test statistic is then:

$$\lambda(\mathbf{x}) = \frac{\sup_{\Theta_0} L(\theta|\mathbf{x})}{\sup_{\Theta} L(\theta|\mathbf{x})} \quad (7)$$

where  $\mathbf{x} = (x_1, \dots, x_T)$  is the data and  $L(\cdot)$  is the likelihood function.<sup>14</sup>

In our test 1, the parameter restriction is that  $\mu_t^i \leq 0$  for some  $t \in \mathcal{T}$ . This is a fairly loose restriction, and to rejecting corresponds to the idea that one is certain that monetary policy is less effective than usual in all the periods in  $\mathcal{T}$ . We illustrate this for the simple case where  $\mathcal{T} = \{1, 2\}$  in Figure A18a,. In contrast, the parameter restriction for test 2 is much tighter, requiring that  $\mu_t^i \leq 0$  for all  $t \in \mathcal{T}$ . This corresponds to the idea that monetary policy was less effective than usual at least some of the time during  $\mathcal{T}$ . In the two-period case, this is shown by the smaller shaded region in Figure A18b.

Formally, we define the parameter restriction sets for the two tests:

$$\begin{aligned} \Theta_0^1 &= \{\boldsymbol{\mu} \in \mathbb{R}^T \mid \mu_t \leq 0 \quad \forall t \in \mathcal{T}\} \\ \Theta_0^2 &= \{\boldsymbol{\mu} \in \mathbb{R}^T \mid \mu_t \leq 0 \text{ for some } t \in \mathcal{T}\} \end{aligned}$$

The likelihood ratio test rejects  $H_0$  in favor of  $H_1$  when  $\lambda(\mathbf{x})$  falls below some critical value. For any given critical value, the set of possible observations of the data which would cause the test to reject the null is known as the rejection region. Because the data are drawn from independent normal distributions with unit variance, the likelihood ratio is simply a function of the minimum Euclidian distance from the observed data to the nearest corresponding point inside  $\Theta_c$ .

For each test  $j = 1, 2$ , we can write down the general formula for a rejection regions with a given point  $\mathbf{x}$  on the boundary as:

$$R_j(\mathbf{x}) = \{\mathbf{X}' \in \mathbb{R}^t \mid d_j(\mathbf{x}') \geq d_j(\mathbf{x})\}$$

where  $d_j(\mathbf{x})$  is the Euclidian distance between  $\mathbf{x}$  and the nearest point inside the constraint

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<sup>14</sup>See chapter 8 of [Casella and Berger \[2024\]](#) for a textbook treatment of this topic, which we follow closely here.

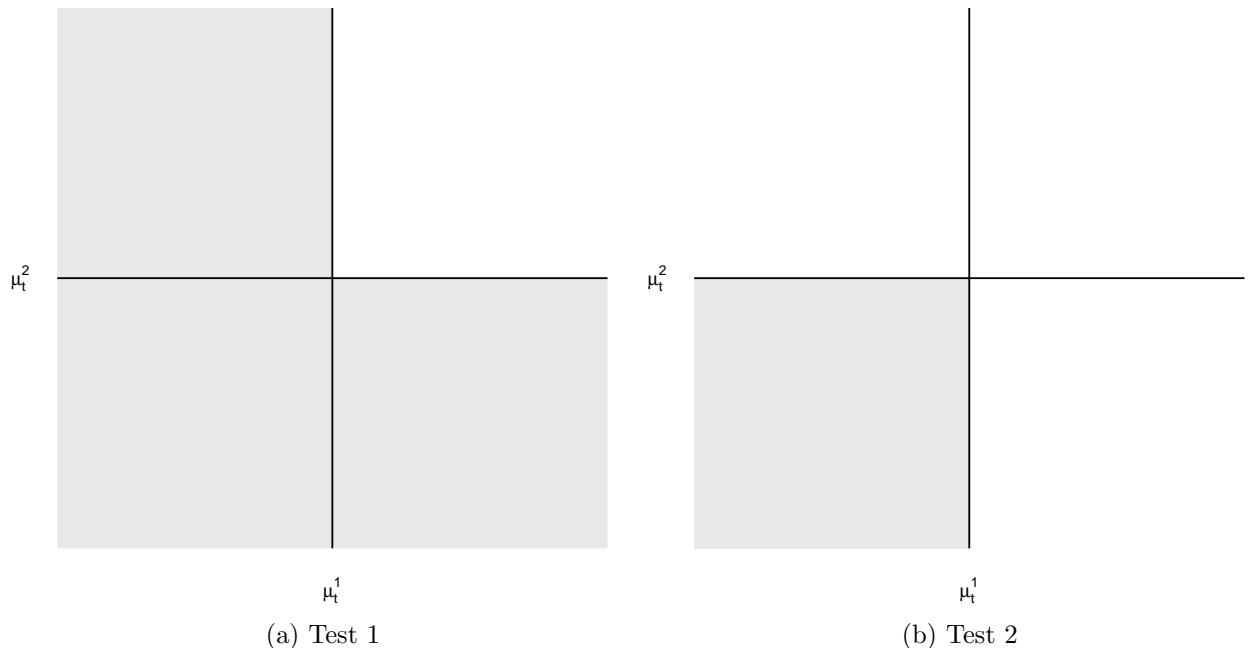


Figure A18: Parameter restrictions for tests 1 and 2

Note: Shaded regions show the restricted parameter under the null,  $\Theta_e$ , for tests 1 and 2 for the case where  $\mathcal{T} = \{1, 2\}$ .

set:

$$d_j(\mathbf{x}) = \inf_{\boldsymbol{\mu} \in \Theta_0^j} \|\mathbf{x} - \boldsymbol{\mu}\|$$

This produces rejection regions which look like the parameter restriction sets plus a buffer (see Figure A19 for the two-period example once more). This buffer is larger for smaller critical values of the likelihood ratio. More intuitively, the critical value for the likelihood ratio test sets the bar for how much the data have to disagree with the null sufficiently in order to reject it. Of course, likelihood ratio critical values are not easy to interpret. In the next section, we derive the more familiar p-values for these tests.

### C.3 Computing p-values

The p-value is a function of the data,  $p(\mathbf{x})$ . This is usually described as the probability of rejecting the observed data under the null. However, with multivariate set nulls (as we have here) this definition is insufficient. Instead, a valid p-value is the maximum probability of rejection over the null set, akin to a worst-case scenario (again see [Casella and Berger \[2024\]](#) for details).

$$p_j(\mathbf{x}) = \sup_{\boldsymbol{\mu} \in \Theta_0^j} \mathbb{P}(\mathbf{x}' \in R_j(\mathbf{x}))$$

For each test, this means we can compute the p-value in two steps. First, we calculate

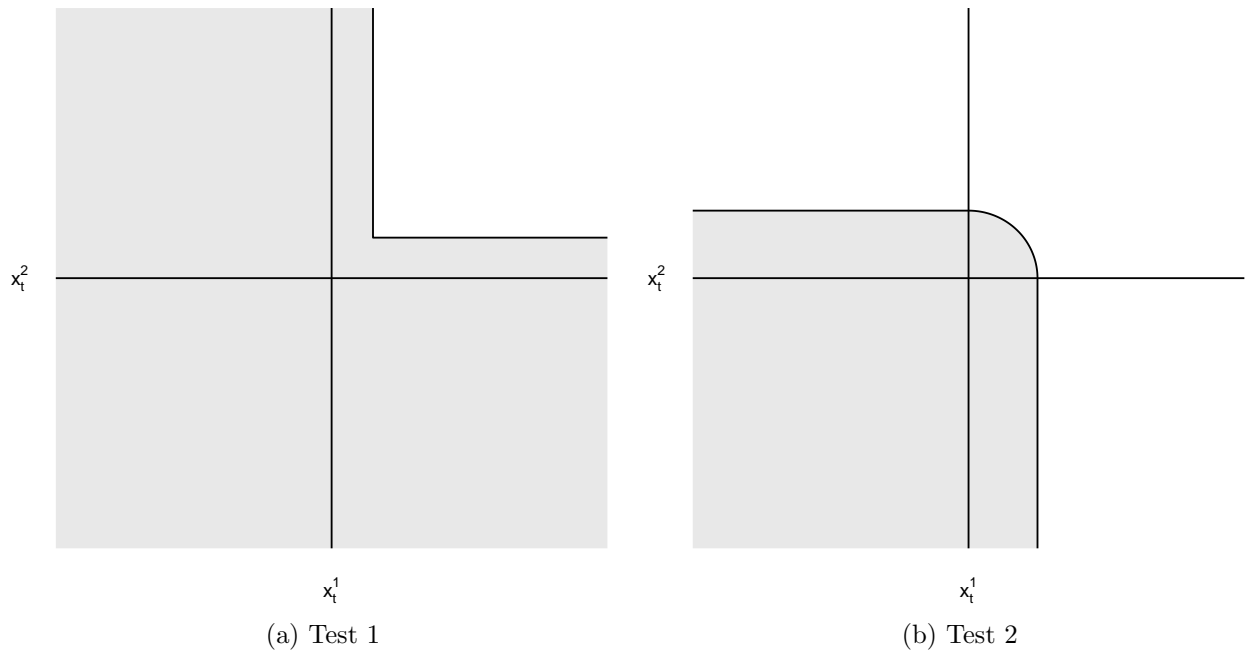


Figure A19: Rejection regions for tests 1 and 2

Note: Unshaded regions show rejection regions for tests 1 and 2 for the case where  $\mathcal{T} = \{1, 2\}$ .

$\boldsymbol{\mu}_j^* \in \Theta_0^j$ , the parameter value which maximizes this probability over the null set. Second, we then integrate over draws of the data conditional on  $\boldsymbol{\mu}^*$ .

For test 1, we can compute a closed-form solution for this test:

$$p_1(\mathbf{x}) = 1 - \Phi\left(\min_{t \in \mathcal{T}} x_t\right)$$

However, for test 2, we can only complete the first step analytically, since  $\boldsymbol{\mu}_j^* = \mathbf{0}$ , the origin. To compute the p-values, we integrate numerically, drawing 100,000 points from the mean-zero unit-variance multivariate normal and computing the fraction of observations in  $R_2(\mathbf{x})$ .





**PUBLICATIONS**