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## A Reassessment of Monetary Policy Surprises and High-Frequency Identification

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### I. Introduction

Over the past two decades, high-frequency interest rate changes around the Federal Reserve's Federal Open Market Committee (FOMC) announcements, or *monetary policy surprises*, have become an important tool for identifying the effects of monetary policy on asset prices and the macroeconomy. For example, Kuttner (2001); Bernanke and Kuttner (2005); Gürkaynak, Sack, and Swanson (2005); Hanson and Stein (2015); and Swanson (2021) use monetary policy surprises to estimate the effects of monetary policy on asset prices, while Cochrane and Piazzesi (2002); Faust et al. (2003); Faust, Swanson, and Wright (2004); Gertler and Karadi (2015); Ramey (2016); and Stock and Watson (2018) use them to help estimate the effects of monetary policy on macroeconomic variables in a structural vector autoregression (SVAR) or Jordà (2005) local projections (LP) framework.

Monetary policy surprises are appealing in these applications because their focus on interest rate changes in a narrow window of time around FOMC announcements plausibly rules out reverse causality and other endogeneity problems. For example, FOMC decisions are completed an hour or two before the decision is announced, implying that the FOMC could not have been reacting to changes in financial markets in a sufficiently narrow window of time around the announcement, so the asset price changes are clearly caused by the announcements themselves, rather than

vice versa. For lower-frequency changes in monetary policy and asset prices, the direction of causality is generally not clear (see, e.g., Rigobon and Sack 2003, 2004).

Monetary policy surprises are also typically viewed as being unpredictable with any publicly available information that predates the FOMC announcement. This view is supported by the standard argument that, otherwise, financial market participants would be able to trade profitably on that predictability and drive it away in the process. Thus, monetary policy surprises are plausibly exogenous with respect to all macroeconomic variables that are publicly known prior to the FOMC announcement itself, making them a valid instrument for the effects of monetary policy in SVARs and LPs, as discussed in Stock and Watson (2018).

A few recent studies, however, have questioned whether monetary policy surprises possess these desirable properties to the extent that the literature has typically assumed. For example, Cieslak (2018), Bauer and Swanson (2023), and Miranda-Agrippino and Ricco (2021) all document substantial correlation of monetary policy surprises with publicly available macroeconomic or financial market data that predate the FOMC announcement, with Bauer and Swanson (2023) reporting  $R^2$  of 10%–40%. These results undermine the standard assumption that monetary policy surprises represent exogenous changes and call into question the results of the empirical studies cited above. In addition, Ramey (2016) finds that the macroeconomic effects of monetary policy are often poorly estimated in samples that begin after about 1984, likely because monetary policy was conducted more systematically over this period, so the set of structural monetary policy shocks (estimated using high-frequency monetary policy surprises or other methods) is much smaller and less informative than in earlier years. In other words, the results in Cieslak (2018) and the other studies cited above question the exogeneity of high-frequency monetary policy surprises, and Ramey (2016) questions whether those surprises are sufficiently relevant. As discussed in Stock and Watson (2018), both conditions are required for monetary policy surprises to be a good instrument for estimating the effects of monetary policy.

In this paper, we address these challenges in two main ways. First, we improve the relevance of monetary policy surprises by substantially expanding the set of monetary policy announcement events to include press conferences, speeches, and testimony by the Federal Reserve chair (which we will refer to as “speeches” for brevity), in addition to the FOMC announcements. As shown by Swanson and Jayawickrema (2021), speeches by the Fed chair are even more important for financial markets

than FOMC announcements themselves, and thus should more than double the relevance of the monetary policy variation in our analysis, relative to previous studies that focused on FOMC announcements alone. Thus, we respond to Ramey's (2016) critique by increasing the number and total variation of monetary policy announcement shocks in our sample. Moreover, Swanson and Jayawickrema (2021) extend the sample for all of these monetary policy announcements back to 1988, giving us a few more years of data during a period when monetary policy was more variable than in the 1990s, which increases the variation in our monetary policy surprise series further still.

Second, for this expanded set of monetary policy surprises, we address the exogeneity issue by removing the component of the monetary policy surprises that is correlated with economic and financial data, following the recommendations of Bauer and Swanson (2023). In particular, we regress those surprises on the economic and financial variables that predate the announcements and are correlated with them, and take the residuals. These orthogonalized monetary policy surprises should help eliminate any attenuation bias or "price puzzle" types of effects in SVARs and LPs and provide better estimates of monetary policy's true effects on macroeconomic variables.

We thus produce a new measure of monetary policy surprises that is both more relevant and more likely to be exogenous than those used by previous researchers. We use our new measure to reassess previous empirical estimates of the effects of monetary policy on financial markets and the macroeconomy, using high-frequency event-study regressions, SVARs, and LPs. Our reassessment leads to two main findings: first, estimates of the effects of monetary policy on financial markets with high-frequency event-study regressions are largely unchanged. The correlation of monetary policy surprises with macroeconomic and financial data that predate the announcements has essentially no effect on these estimates, consistent with the simple theoretical model that we develop shortly. Second, conventional estimates of the effects of monetary policy on the macroeconomy using high-frequency identification are substantially biased, due to the econometric endogeneity of the monetary policy surprises. Using our new, improved monetary policy surprise measure produces stronger, more plausible, and more precise estimates. In addition, our correction of monetary policy surprises uses publicly available data, so our results do not support the view that Fed information effects are an important confounding factor for monetary policy surprises, in contrast to Nakamura and Steinsson (2018), Miranda-Agrippino and Ricco (2021), and others.

We begin our analysis with a simple theoretical model of private-sector learning about the Fed’s monetary policy rule in Section II. The model extends an earlier model in Bauer and Swanson (2023) and helps to organize our thinking and to make testable empirical predictions. In the model, the Fed’s responsiveness to the economy is both time varying and unobserved by the private sector. A key result is that monetary policy surprises arise not only from exogenous monetary policy shocks but also from incomplete information about the Fed’s monetary policy rule. As a consequence, monetary policy surprises can be correlated with economic variables observed prior to the policy announcements.<sup>1</sup> A precondition for this effect, which Bauer and Swanson (2023) termed the “Fed response to news” channel, is that the public systematically underestimated how strongly the Fed would respond to economic news. We provide empirical evidence that the Fed has become more responsive to the economy over our sample, 1988–2019, which can explain why, on average, the Fed has responded more strongly to economic conditions than the private sector expected. Additional evidence in Cieslak (2018) and Schmeling, Schrimpf, and Steffensen (2022) also supports this view.<sup>2</sup> The model has additional implications for our subsequent empirical analysis: it predicts that monetary policy surprises can be used without correction for estimating asset price responses to monetary policy in high-frequency regressions, but they are unlikely to be valid instruments for monetary policy shocks in SVARs or LPs.

In Section III, we review and extend previous studies of the predictability of high-frequency monetary policy surprises. We document a strong correlation of monetary policy surprises with information that is publicly available prior to the FOMC announcements. We argue that this correlation is unlikely to be driven entirely by time-varying risk premia, because survey forecast errors for the federal funds rate are also significantly correlated with the same preannouncement information. Instead, we argue that a violation of the Full-Information Rational Expectations (FIRE) hypothesis is a more likely explanation. Monetary policy surprises were likely unpredictable *ex ante* but predictable *ex post*, consistent with our simple theoretical model and imperfect information on the part of the private sector.

We then begin our empirical reassessment of the transmission of monetary policy to financial markets and the macroeconomy. In Section IV, we revisit high-frequency empirical estimates of the effects of monetary policy announcements on financial markets, as in Kuttner (2001) and Gürkaynak et al. (2005), using our expanded set of orthogonalized monetary policy surprises. In line with previous estimates, we find very strong effects of

monetary policy surprises on Treasury yields and the stock market. A comparison of the estimates using conventional versus orthogonalized monetary policy surprises shows that the two have very similar effects on asset prices, in line with the prediction of our theoretical model. The implication for empirical research is that standard event studies using conventional high-frequency monetary policy surprises can reliably estimate the financial market effects of monetary policy announcements.

In Section V, we turn to high-frequency identification of the effects of monetary policy on macroeconomic variables in an SVAR or LP framework, as in Gertler and Karadi (2015), Ramey (2016), Miranda-Agrippino and Ricco (2021), and Plagborg-Møller and Wolf (2021, 2022). Our expanded set of monetary policy surprises greatly improves the first-stage  $F$ -statistic for our high-frequency instrument, solving one of the main difficulties faced by those earlier authors. Our orthogonalized monetary policy surprises produce estimates of monetary policy's effects that do not suffer from price or activity puzzles and are up to four times larger than when conventional, unadjusted monetary policy surprises are used. Thus, we find substantial evidence that the econometric endogeneity of conventional monetary policy surprises used by previous authors leads to a significant bias that attenuates or even reverses the sign of their estimates. We collect lessons learned from revisiting previous empirical work and present new "best practice" estimates of the dynamic macroeconomic effects of monetary policy shocks using our orthogonalized monetary policy surprises.

We also revisit the role of the Fed's internal "Greenbook" forecasts for explaining the endogeneity of monetary policy surprises. Miranda-Agrippino and Ricco (2021) documented that Greenbook forecasts (and forecast revisions) have predictive power for monetary policy surprises, and that removing this correlation changes SVAR estimates that use these surprises as instruments for policy shocks. We show that there is nothing particularly special about the Greenbook forecasts in these results: both in predictions of monetary policy surprises and in SVARs that use adjusted monetary policy surprises as instruments, the use of Greenbook and Blue Chip forecasts produces almost identical results. Because Blue Chip forecasts are publicly observable, our findings challenge the view that the Fed has significant private information, consistent with the findings in Bauer and Swanson (2023) that both types of forecasts are equally (in)accurate. Hence, they call into doubt the presence of strong Fed information effects and support our interpretation in terms of a "Fed response to news" channel.

In Section VI, we conclude and discuss the implications of our results for monetary policy and central bank communication in practice. For

example, we address the question of whether policy makers should be concerned about information effects or other effects that might attenuate or counteract the intended effects of monetary policy announcements. We also discuss what our new estimates imply about the effectiveness of policy communication in speeches by the Fed chair versus official communication by the FOMC itself. Finally, we lay out some ideas that hold promise for future research.

Our work is related to three different strands of the macroeconomic literature. First, several recent studies have documented that high-frequency monetary policy surprises around FOMC announcements are in fact significantly correlated *ex post* with information that was publicly available prior to the FOMC announcement. For example, Cieslak (2018) documents correlation with the lagged federal funds rate and employment growth; Miranda-Agrippino (2017) and Miranda-Agrippino and Ricco (2021) with broad-based macroeconomic factors from a dynamic factor model; Bauer and Swanson (2021) with major macroeconomic data release surprises—such as for nonfarm payrolls, unemployment, gross domestic product (GDP), and inflation—and changes in financial markets, such as the Standard and Poor's 500 index (S&P 500), yield curve slope, and commodity prices; Karnaugh and Vokata (2022) with the most recent Blue Chip GDP forecast revisions; Bauer and Chernov (2023) with option-implied skewness of Treasury yields; and Sastry (2021) with the consumer sentiment release, recent S&P 500 stock returns, and the most recent Blue Chip GDP forecast. Relative to these previous studies, we extend the predictability findings to additional predictors and an expanded sample. We also present new evidence that Blue Chip forecasts have predictive power for monetary policy surprises that is just as strong as the predictive power of the Fed's Greenbook forecasts documented by Miranda-Agrippino and Ricco (2021).

The above studies have also proposed a number of possible explanations for the predictability they document. For example, Karnaugh and Vokata (2022) argues that bond markets were slow to incorporate the information in the Blue Chip forecasts, although this raises the question of why competition for profits by market participants would not drive the sluggish response away. Miranda-Agrippino (2017) argues that there are substantial, predictable risk premia on short-term interest rate securities; however, Piazzesi and Swanson (2008) and Schmeling et al. (2022) estimate that the risk premia on such short-term securities is small, and Cieslak (2018) argues that those risk premia would need to be implausibly large to explain the observed predictability in the data and that

a risk premium interpretation is inconsistent with a variety of other financial market evidence. Miranda-Agrippino and Ricco (2021) argue that the predictability is evidence of a “Fed information effect,” according to which the Fed’s monetary policy surprises reveal to the markets information about the Fed’s forecast for the economy.<sup>3</sup> However, we show in this paper that Blue Chip forecasts have equally strong predictive power for those policy surprises, indicating that the Fed is unlikely to have significant private information, and that Fed information effects may not be an important source of that predictability. Moreover, Bauer and Swanson (2023) show that the Fed’s Greenbook forecasts are no more accurate than Blue Chip forecasts, that Blue Chip forecasters do not revise their forecasts in response to FOMC announcements in a way consistent with the Fed information effect, and that previous authors’ results that supported a Fed information effect can be explained by major macroeconomic data releases and financial market changes that were omitted from those previous studies.<sup>4</sup> Instead, in this paper and in Bauer and Swanson (2023), we argue that the predictability of monetary policy surprises is due to financial markets not having full information about the Fed’s monetary policy rule and underestimating *ex ante* how responsive the Fed would be to economic data; this interpretation of the evidence is also very similar to Cieslak (2018) and Schmeling et al. (2022). Note, however, that our analysis in the present paper does not hinge on this particular interpretation, because we investigate the practical consequences of the predictability of monetary policy surprises, no matter what the source of that predictability is.

The second strand of literature related to the present paper uses high-frequency monetary policy surprises to estimate the effects of monetary policy on asset prices. Kuttner (2001) uses daily changes in the current-month or next-month federal funds futures rate around an FOMC announcement to measure the surprise component of the announcement and the effects of changes in the federal funds rate on short- and longer-term Treasury yields, and Bernanke and Kuttner (2005) estimate the effects of those changes on the stock market. Gürkaynak et al. (2005) extend Kuttner’s analysis by focusing on intradaily changes in financial markets around FOMC announcements and by looking at interest rate futures with several months to maturity, allowing them to separately estimate the effects of changes in the federal funds rate from changes in forward guidance on bond yields and stock prices. Brand, Buncic, and Turunen (2010) extend the Gürkaynak et al. analysis to the euro area, and D’Amico and Farka (2011) consider a more detailed and updated

analysis of the stock market. Swanson (2021) extends the Gürkaynak et al. analysis to separately identify the effects of the Fed's asset purchases as well as federal funds rate changes and forward guidance, and Altavilla et al. (2019) apply the analysis in Swanson to the euro area. We revisit this type of analysis in Section IV, reestimating the effects of monetary policy surprises on asset prices both with and without corrections for the predictability discussed above.

The third strand of literature related to our study uses high-frequency monetary policy surprises to help estimate and identify the effects of monetary policy on macroeconomic variables in an SVAR or LP framework. Early examples are Cochrane and Piazzesi (2002), Faust et al. (2003), and Faust et al. (2004). Stock and Watson (2012, 2018) discuss how to use high-frequency monetary policy surprises as an external instrument to identify the effects of monetary policy in a VAR, and Gertler and Karadi (2015) and Ramey (2016) follow this approach to obtain estimates that are now regarded as benchmarks. In the present paper, we reassess the VAR and LP analysis in these studies in light of our expanded set of monetary policy surprises and our corrections for the predictability of those surprises discussed above.

## II. A Simple Model with Incomplete Information

To gain intuition and guide our empirical work shown later, we present a simple theoretical model of incomplete information and private-sector learning about the Fed's monetary policy rule. Readers who are interested only in our empirical results can skip this section and proceed directly to the beginning of our empirical analysis in Section III.

The basic idea is that monetary policy surprises can arise from a discrepancy between the true and perceived responsiveness of the Fed to the state of the economy. For example, if the Fed is more responsive to the output gap than the public expects, then a high output gap will lead to a positive monetary policy surprise. If the private sector's underestimate persists for several periods, as will typically be the case in a model of learning, then the monetary policy surprises will end up being correlated with the output gap *ex post* even though they were unpredictable by the private sector *ex ante*.

### A. *The Simple Model*

In the interest of clarity, we make the model as simple as possible, following along the lines of the model in Bauer and Swanson (2023), but

extended in two ways: first, we explicitly consider the case where the parameters of the Fed's monetary policy rule may change over time. Second, we allow for changes in the interest rate to feed back directly to the economy.

For simplicity, the state of the economy in the model is captured by a scalar variable  $x_t$ . For concreteness,  $x_t$  is taken to be procyclical (e.g., the output gap). We assume that  $x_t$  follows a simple backward-looking linear process,

$$x_t = \rho x_{t-1} - \theta i_{t-1} + \eta_t, \quad (1)$$

where time  $t$  is discrete,  $|\rho| < 1$  and  $\theta \geq 0$  are parameters,  $i_t$  denotes the interest rate, and  $\eta_t$  is an exogenous *i.i.d.* Gaussian process with mean zero and variance  $\sigma_\eta^2$ . In contrast to Bauer and Swanson (2023), we allow  $\theta \neq 0$  in equation (1), which complicates the model but explicitly allows the interest rate  $i_t$  to affect future values of  $x_t$ . Intuitively, equation (1) is a simple, backward-looking IS curve, with the negative sign on  $\theta$  corresponding to the standard intuition that higher interest rates reduce future economic activity.

Each period  $t$  is divided into two subperiods, with  $x_t$  realized in the first subperiod and  $i_t$  set by the Federal Reserve in the second subperiod. The Fed sets  $i_t$  according to the monetary policy rule

$$i_t = \alpha_t x_t + \varepsilon_t, \quad (2)$$

where  $\alpha_t$  denotes the Fed's responsiveness to  $x_t$ , and  $\varepsilon_t$  is the monetary policy shock, an exogenous *i.i.d.* Gaussian process with mean zero and variance  $\sigma_\varepsilon^2$ . In contrast to Bauer and Swanson (2023), we explicitly allow the parameter  $\alpha_t$  in equation (2) to be time varying; for simplicity, we assume that it follows a random walk,

$$\alpha_t = \alpha_{t-1} + u_t, \quad (3)$$

where  $u_t$  is an exogenous *i.i.d.* Gaussian process with mean zero and variance  $\sigma_u^2$ .

We assume that the Fed has full information and perfectly observes all variables and parameters of the model. The private sector knows the parameters  $\rho$ ,  $\theta$ ,  $\sigma_\eta^2$ ,  $\sigma_\varepsilon^2$ , and  $\sigma_u^2$  and observes  $x_t$  and  $i_t$  each period, but does not observe  $\alpha_t$  (or  $\varepsilon_t$  or  $u_t$ ), and thus must form beliefs about  $\alpha_t$  based on the history of the observed  $x_t$  and  $i_t$ . We assume that the private

sector's belief formation is fully Bayesian and thus rational. We let  $\mathcal{H}_t \equiv \{i_t, x_t, i_{t-1}, x_{t-1}, \dots\}$  denote the history of variables observed by the private sector up to time  $t$ . At the beginning of period  $t$ , before  $x_t$  and  $i_t$  are realized, we assume that the private sector's prior beliefs about  $\alpha_t$  are Gaussian with mean  $a_t = E[\alpha_t | \mathcal{H}_{t-1}]$  and variance  $\sigma_t^2 = \text{Var}[\alpha_t | \mathcal{H}_{t-1}]$ .

Once the private sector observes  $x_t$ , it expects the interest rate to be  $E[i_t | x_t, \mathcal{H}_{t-1}] = a_t x_t$ . The Fed's actual interest rate decision in the second subperiod then leads to the monetary policy surprise

$$\begin{aligned} \text{mps}_t &\equiv i_t - E[i_t | x_t, \mathcal{H}_{t-1}] \\ &= (\alpha_t - a_t)x_t + \varepsilon_t. \end{aligned} \tag{4}$$

Equation (4) illustrates that monetary policy surprises can be due either to exogenous policy shocks  $\varepsilon_t$  or to imperfect information about the Fed's monetary policy rule,  $\alpha_t \neq a_t$ .

After observing  $i_t$ , the private sector updates its beliefs about  $\alpha_t$  optimally using Bayesian updating (i.e., Kalman filtering):

$$a_{t+1} = E[\alpha_t | H_t] = a_t + k_t \text{mps}_t, \tag{5}$$

where the Kalman gain parameter  $k_t$  is given by

$$k_t = \frac{\omega_t}{x_t}, \quad \omega_t = \frac{x_t^2 \sigma_t^2}{x_t^2 \sigma_t^2 + \sigma_\varepsilon^2}, \tag{6}$$

and the belief variance evolves according to

$$\sigma_{t+1}^2 = \sigma_t^2 (1 - \omega_t) + \sigma_u^2. \tag{7}$$

The direction of the parameter update naturally depends upon the signs of both  $x_t$  and  $\text{mps}_t$ : the private sector will raise its belief about  $\alpha_t$  for a hawkish surprise ( $\text{mps}_t > 0$ ) during an expansion ( $x_t > 0$ ), as well as for a dovish surprise ( $\text{mps}_t < 0$ ) during a recession ( $x_t < 0$ ).

The model in Bauer and Swanson (2023) assumed constant  $\alpha_t = \alpha$ , that is,  $\sigma_t^2 = 0$ , for simplicity. In that case, the belief variance  $\sigma_{t+1}^2 = \sigma_t^2 (1 - \omega_t)$  tends to zero as  $t \rightarrow \infty$ , so the private sector would gradually learn the true value of  $\alpha$  over time. In the more general case here, the private sector can never fully learn the Fed's policy rule.

Because the updating in equations (4)–(5) is optimal, the monetary policy surprise  $\text{mps}_t$  is unpredictable *ex ante*, based on any information that is available to the private sector before the Fed sets the interest rate

$i_t$ . This is evident from equation (4), which implies that  $E[\text{mps}_t | x_t, H_{t-1}] = 0$ . Nevertheless, the monetary policy surprises  $\text{mps}_t$  can be correlated with  $x_t$  ex post if  $\alpha_t > a_t$  for several periods in a row. From equation (4),  $\text{Cov}(\text{mps}_t, x_t) = (\alpha_t - a_t)\text{Var}(x_t)$ , which is positive if  $\alpha_t > a_t$  on average over a given sample.<sup>5</sup> If the private sector tends to underestimate the Fed's responsiveness to the economy, then the monetary policy surprise  $\text{mps}_t$  will be ex post positively correlated with a procyclical business cycle indicator such as  $x_t$ . Such ex post predictability in financial markets—without any true ex ante predictability due to variation in risk premia and expected returns—is a common implication of models of imperfect information and learning by investors (Timmermann 1993; Lewellen and Shanken 2002; Johannes, Lochstoer, and Mou 2016).

Our empirical analysis in Section III documents significant procyclical correlation between monetary policy surprises and macroeconomic and financial variables. The model suggests a straightforward explanation of this correlation: financial markets have underestimated how responsive the Fed would be to the economy (i.e.,  $\alpha_t > a_t$  on average over our sample).

One way we could have  $\alpha_t > a_t$  over our sample is if the Fed became more responsive to the economy, so that  $\alpha_t$  increased over time. In fact, several pieces of evidence presented later are consistent with such a pattern. If  $\alpha_t$  increases, then a logical consequence of Bayesian learning is that the private sector's beliefs  $a_t$  will tend to lag behind, and thus on average  $a_t < \alpha_t$ . The reason is that signals about  $\alpha_t$  are downweighted in the update of the parameter belief because  $\omega_t \in [0, 1]$ . To see this more clearly, rewrite the updating rule (eq. [5]) as

$$a_{t+1} = (1 - \omega_t)a_t + \omega_t\alpha_t + \frac{\omega_t}{x_t}\varepsilon_t. \quad (8)$$

For example, suppose that at the end of period  $t = 1$ , the private sector's beliefs are correct, so that  $a_2 = \alpha_1$ , and then the Fed becomes more responsive, so that  $\alpha_2 - \alpha_1 = u_2 > 0$ . Assume for simplicity that there is no policy shock, so  $\varepsilon_2 = 0$ . After the interest rate  $i_2 = \alpha_2 x_2$  is observed, the private sector's belief update is  $a_3 - a_2 = \omega_2(\alpha_2 - a_2) = \omega_2 u_2$ , which is smaller than the actual parameter change,  $u_2$ . This example illustrates a general pattern: if the Fed becomes more responsive over time, then the perceived responsiveness parameter will tend to be smaller than the true parameter.

There are a number of plausible reasons to think that private-sector learning about the Fed's monetary policy rule would be quite slow in practice, with the result that changes in  $\alpha_t$  would cause a persistently

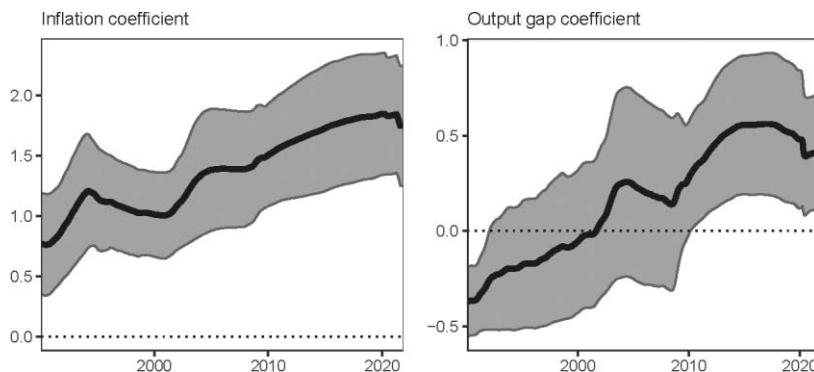
large discrepancy  $\alpha_t - a_t$ . First, learning about a persistent component ( $\alpha_t$ ) from a noisy time series ( $i_t$ ) is difficult and happens only gradually, with long-lasting biases in beliefs; see Farmer, Nakamura, and Steinsson (2021) for a recent discussion. Second, the private sector in reality faces a multidimensional learning problem: realistic policy rules are of course multivariate, requiring the public to learn about several parameters at once, which greatly slows down the learning process (Johannes et al. 2016). Third, the private sector must form beliefs about which macroeconomic and financial variables enter the Fed's monetary policy rule (i.e., about its functional form). Fourth, the Fed's monetary policy rule could contain nonlinearities—which we have also abstracted from here—so that, in practice, the Fed responds most aggressively to the economy when the economic data are most extreme. These extreme events occur only very rarely, so it is extraordinarily difficult for the private sector to learn the Fed's true responsiveness to the economy during these rare episodes.

### *B. Evidence for Increasing Fed Responsiveness*

Empirically, there is substantial evidence that the Fed's monetary policy has in fact become more responsive to the economy over the past few decades. First, a number of studies have investigated shifts in the parameters of the Fed's monetary policy rule, going back to the seminal work of Clarida, Gali, and Gertler (2000), who documented a substantial increase in the Fed's responsiveness to inflation and output when Paul Volcker became Fed chairman in 1979. Empirical monetary policy rules with explicitly time-varying parameters also generally suggest a tendency for the Fed's responsiveness to inflation and real activity to have increased since the 1980s (Cogley and Sargent 2005; Primiceri 2005; Boivin 2006; Kim and Nelson 2006; Bianchi, Lettau, and Ludvigson 2022). In figure 1, we report results from estimating a simple time-varying monetary policy rule for the Fed, obtained using recursive, exponentially weighted least squares estimates as described in appendix A. There is a clear upward trend in the Fed's response coefficients to both inflation and output over the past 30 years.

These empirical estimates are also supported by numerous speeches by Federal Reserve officials. For example:

- In 2001, Chairman Greenspan noted, “The Federal Reserve has seen the need to respond more aggressively than had been our wont in earlier decades” (*Wall Street Journal* 2001).



**Fig. 1.** Recursive least squares estimates of Fed monetary policy rule parameters. Exponentially weighted recursive least squares estimates of the Federal Reserve's monetary policy rule parameters using expanding windows beginning in 1976 and ending between 1990 and 2021, with shaded two-standard-error bands based on Newey and West (1987) with 12 lags. Regressions are estimated at monthly frequency, inflation is measured using the 1-year change in the log core Personal Consumption Expenditures price index, and the output gap is the Congressional Budget Office estimate. See text and appendix for details. A color version of this figure is available online.

- In 2008, Chairman Bernanke stated, “By way of historical comparison, this policy response stands out as exceptionally rapid and proactive” (Bernanke 2008).
- In 2012, Vice Chair Yellen introduced an “optimal control” approach to monetary policy. Under this approach, which Yellen characterized as consistent with the current strategy of the FOMC, monetary policy responds more strongly to unemployment than policy rules that had characterized past Fed behavior (Yellen 2012).
- Both Chairs Bernanke and Yellen have emphasized and elaborated on a “balanced approach” to monetary policy (e.g., Bernanke 2013; Yellen 2017), which puts more weight on resource utilization than historical policy rules. The Fed makes this explicit in its Monetary Policy Report to Congress, which regularly compares policy rules: the coefficient on the unemployment gap in the “balanced-approach rule” is two, whereas this coefficient in the classic Taylor (1993) rule is one.<sup>6</sup>

It is also reasonable to think that the Fed’s view of optimal monetary policy has become more responsive to the economy over time. Many prominent theoretical and empirical studies of monetary policy over the past 30 years have increasingly supported the view that more systematic and proactive monetary policy leads to better macroeconomic

outcomes (e.g., Taylor 1999; Clarida et al. 2000; Stock and Watson 2002; Woodford 2009).<sup>7</sup>

Finally, empirical evidence from surveys provides direct support for the view that the private sector has typically underestimated the responsiveness of the Fed to the economy. In particular, Cieslak (2018) and Schmeling et al. (2022) show that survey forecasts systematically underpredicted changes in the federal funds rate over our sample, particularly during easing episodes.<sup>8</sup>

### C. Implications of the Model

The simple model of incomplete information and learning outlined above has a number of implications for empirical analysis with high-frequency monetary policy surprises. First, as discussed above, equation (4) shows that as a result of imperfect public information about the policy rule, monetary policy surprises can be correlated with information that is publicly available prior to the FOMC announcements. This is true even if the surprises are unpredictable *ex ante* because financial markets are perfectly rational and risk premia on short-term rate securities are negligible.

Second, the model suggests that the effects of monetary policy surprises on asset prices can be estimated using standard high-frequency regressions. The reason is that revisions to interest rate expectations—the only asset prices in this model—are affected by monetary policy announcements only through  $mps_t$  and not separately by  $\varepsilon_t$ . To show this, we introduce new notation for the change in private-sector expectations in response to the monetary policy announcement in period  $t$ ,  $\Delta E_t(z) = E[z|H_t] - E[z|H_{t-1}, x_t]$  for expectations about a generic variable  $z$ . Bayesian updating and the fact that  $\alpha_t$  is a martingale imply that changes in beliefs about all future rule coefficients are simply

$$\Delta E_t(\alpha_{t+n}) = k_t mps_t, \quad \text{for all } n \geq 0. \quad (9)$$

Changes in expectations of future interest rates are

$$\begin{aligned} \Delta E_t(i_{t+n}) &= \Delta E_t(\alpha_{t+n} x_{t+n}) \\ &\approx \Delta E_t(\alpha_{t+n}) E[x_{t+n} | \mathcal{H}_{t-1}, x_t] + \Delta E_t(x_{t+n}) E[\alpha_{t+n} | \mathcal{H}_{t-1}, x_t], \end{aligned} \quad (10)$$

where the first equality follows from the policy rule (eq. [2]) and the fact that the policy shock  $\varepsilon_t$  is unpredictable, and the second line is a first-order approximation that simplifies the argument in the presence of an endogenous output gap ( $\theta \neq 0$ ). In the simpler case with an exogenous output

gap ( $\theta = 0$ ), as in the model of Bauer and Swanson (2023), revisions to rate expectations are exactly equal to the first term in equation (10), which from equation (9) depends only on  $\text{mps}_t$  and not on  $\varepsilon_t$ :

$$\Delta E_t(i_{t+n}) = \Delta E_t(\alpha_{t+n})E[x_{t+n}|\mathcal{H}_{t-1}, x_t] = \rho^n \omega_t \text{mps}_t. \quad (11)$$

In the more general case, we need to account for revisions to output gap expectations, which from equation (1) and recursive substitution are

$$\Delta E_t(x_{t+n}) = -\theta \sum_{j=0}^{n-1} \rho^{n-j-1} \Delta E_t(i_{t+j}). \quad (12)$$

From induction on equations (10) and (12), with initial condition  $\Delta E_t(i_t) = \text{mps}_t$ , it is evident that the revisions  $\Delta E_t(x_{t+1}), \Delta E_t(i_{t+1}), \Delta E_t(x_{t+2}), \Delta E_t(i_{t+2}),$  and so forth all depend only on  $\text{mps}_t$  and not separately on  $\varepsilon_t$ . That is, up to first order, a monetary policy announcement at time  $t$  changes private-sector expectations of future interest rates  $i_{t+n}$  by an amount that is a function of the surprise  $\text{mps}_t$ , with no separate role for  $\varepsilon_t$ . Accordingly, the effects of a monetary policy shock  $\varepsilon_t$  manifest themselves entirely through  $\text{mps}_t$ .

As a result, an econometrician can use high-frequency data on monetary policy surprises  $\text{mps}_t$  to estimate the effects of those surprises on the yield curve (or other asset prices) using high-frequency regressions of the form

$$\Delta E_t(i_{t+n}) = b_0 + b_1 \text{mps}_t + e_t, \quad (13)$$

and those estimates will also be representative of the effects of an exogenous change in monetary policy  $\varepsilon_t$ . Although the high-frequency monetary policy surprises  $\text{mps}_t$  may be correlated with  $x_t$ , our model predicts that there is no omitted variable issue: once we condition on  $\text{mps}_t$ , there is no separate role for  $x_t$  or  $\varepsilon_t$ . Thus,  $\text{mps}_t$  can still be used, without adjustment, to estimate the effects of an exogenous change in monetary policy  $\varepsilon_t$  on asset prices in a narrow window of time around an FOMC announcement. This implies that the high-frequency empirical estimates in Kuttner (2001), Bernanke and Kuttner (2005), Gürkaynak et al. (2005), and others should reliably estimate the effects of an exogenous change in monetary policy ( $\varepsilon_t$ ) on the yield curve, the stock market, and other asset prices. We check this prediction of our model in Section IV.

A third implication of our model is that it may be problematic to use monetary policy surprises for estimation of the dynamic effects of monetary policy on macroeconomic variables in an SVAR or LP framework. To be a valid external instrument for a monetary policy shock,  $\text{mps}_t$  must be exogenous with respect to the other structural shocks and the lagged

variables of the VAR (Stock and Watson 2018). However, according to our model,  $\text{mps}_t$  can be correlated with  $x_t$  ex post, and the evidence in Section III confirms that  $\text{mps}_t$  is strongly correlated with various macroeconomic and financial variables in practice. Therefore, it is likely that the econometric exogeneity condition is violated, and  $\text{mps}_t$  is not a valid instrument for the monetary policy shock.

In Bauer and Swanson (2023), we recommend orthogonalizing  $\text{mps}_t$  with respect to the macroeconomic and financial variables that are observed before the FOMC announcement to remove this correlation. According to our model, such a procedure would: (i) isolate the component of  $\text{mps}_t$  that is due to the monetary policy shock  $\varepsilon_t$ , (ii) leave estimates of the effects of monetary policy on asset prices largely unchanged,<sup>9</sup> and (iii) increase the likelihood that the resulting series is a valid instrument for monetary policy shocks in a VAR. In Sections IV and V, we implement this correction and assess to what extent it affects empirical estimates typical of those in the literature.

### III. Monetary Policy Surprises and Predictability

In this section, we present new evidence for the predictability of high-frequency monetary policy surprises around FOMC announcements, extending the results of previous studies such as Cieslak (2018), Bauer and Swanson (2023), and Miranda-Agrippino and Ricco (2021).<sup>10</sup> We expand on earlier work in three main ways: first, we use a new, more extensive data set of high-frequency monetary policy surprises from Swanson and Jayawickrema (2021). Second, we document predictive power for additional macroeconomic and financial variables, which we show to be robust across different sample periods and measures of monetary policy surprises. Third, we assess the information content in macroeconomic forecasts for subsequent monetary policy surprises and find that the Blue Chip survey consensus and the Fed's Greenbook forecasts contain the same amount of information. We interpret these results through the lens of our model and argue that they support the view that predictability arises from imperfect information in the private sector about the Fed's monetary policy rule.

#### A. Monetary Policy Surprises around FOMC Announcements

The Swanson and Jayawickrema (2021) data set covers the period from 1988 to 2019, which begins earlier and ends later than the studies cited above and includes 322 FOMC announcements and 880 speeches by the

Fed chair. For comparability to previous work, we focus first on FOMC announcements.

From 1994 onward, FOMC announcement dates and times are relatively easy to collect, because each announcement was communicated clearly to the markets through a press release.<sup>11</sup> Prior to 1994, the FOMC typically did not issue such press releases (except after a discount rate change), and market participants had to infer whether there had been a change in the federal funds rate from the size and type of open market operation conducted by the Fed each morning. In this case, the term “FOMC announcement” corresponds to the date and time of the corresponding open market operation.<sup>12</sup> Swanson and Jayawickrema (2021) measure intradaily interest rate changes over a 30-minute window starting 10 minutes before each FOMC announcement and ending 20 minutes afterward, using intradaily data from Tick Data.

To construct high-frequency monetary policy surprises, some authors use the change in the current-month federal funds futures contract (e.g., Kuttner 2001), some use the change in a farther-ahead federal funds futures contract (e.g., Gertler and Karadi 2015), and others use a range of federal funds and Eurodollar futures contracts (e.g., Gürkaynak et al. 2005; Nakamura and Steinsson 2018).<sup>13</sup> In this paper, we follow the last approach and use the first four quarterly Eurodollar futures contracts, ED1–ED4.<sup>14</sup> Rather than focus on two dimensions of monetary policy, as in Gürkaynak et al. (2005), we follow Nakamura and Steinsson (2018) and take just the first principal component of the changes in ED1–ED4 around FOMC announcements, which we rescale so that a one-unit change in the principal component corresponds to a 1 percentage point change in the ED4 rate. Gürkaynak et al. (2005) showed that FOMC announcements cause surprises about both the current federal funds rate target and the expected path of the federal funds rate for the next several months (i.e., their “target” and “path” factors). Because the first principal component is essentially equal to a weighted average of the target and path factors, it parsimoniously captures some of the main features of both types of monetary policy surprises.

### *B. Predictability with Macroeconomic and Financial Data*

The literature cited earlier has documented several variables that predict upcoming monetary policy surprises. For our analysis here, we focus on macroeconomic and financial variables that were previously found by Cieslak (2018), Bauer and Chernov (2023), and Bauer and Swanson

(2021) to be good predictors, but we also explored a number of other variables. In all cases, we make sure that the relevant data were available to financial markets prior to the FOMC announcement itself. Our goal was to choose a parsimonious and robust set of predictors that also have an intuitive relationship to the Fed's monetary policy rule, consistent with our simple model from Section II. We ultimately settled on the following six predictors:

- Nonfarm payrolls surprise: the surprise component of the most recent nonfarm payrolls release prior to the FOMC announcement, measured as the difference between the released value of the statistic minus the median expectation for that release from the Money Market Services survey.<sup>15</sup>
- Employment growth: the log change in nonfarm payroll employment from 1 year earlier to the most recent release before the FOMC announcement, as used in Cieslak (2018).
- S&P 500: the log change in the S&P 500 stock market index from 3 months (65 trading days) before the FOMC announcement to the day before the FOMC announcement.
- Yield curve slope: the change in the slope of the yield curve from 3 months before the FOMC announcement to the day before the FOMC announcement, measured as the second principal component of 1-to-10-year zero-coupon Treasury yields from Gürkaynak, Sack, and Wright (2007).
- Commodity prices: the log change in the Bloomberg Commodity Spot Price index from 3 months before the FOMC announcement to the day before the FOMC announcement.
- Treasury skewness: the implied skewness of the 10-year Treasury yield, measured using options on 10-year Treasury note futures with expirations in 1–3 months, averaged over the preceding month, from Bauer and Chernov (2023).

With these predictors, we estimate regressions of the form

$$\text{mps}_t = \alpha + \beta' X_{t-} + u_t, \quad (14)$$

where  $t$  indexes FOMC announcements in our sample,  $\text{mps}_t$  denotes a measure of the monetary policy surprise,  $X_{t-}$  contains the six predictors described above (which are known prior to the announcement  $t$ , indicated by the time subscript  $t-$ ), and  $u_t$  is a regression residual.

The results from four different versions of regression (eq. [14]) are reported in table 1. The first column considers our baseline measure of the monetary policy surprise, described above, over our full sample of 322 FOMC announcements from 1988 to 2019. The  $R^2$  is about 16%, most predictors are statistically significant, and the signs of the estimated coefficients are intuitive and, consistent with the model in Section II, indicate procyclical correlations: strong nonfarm payroll employment, a strong stock market, and high commodity prices predict a hawkish monetary policy surprise. Similarly, when the yield curve becomes more upward-sloping (i.e., when short-term interest rates fall relative to long-term rates, as they do during monetary easing cycles), or when implied skewness on the 10-year Treasury yield is negative (suggesting markets are most concerned about a decrease in interest rates), the Fed is likely to follow with an easing surprise.

**Table 1**  
Predictive Regressions Using Macroeconomic and Financial Data

	(1)	(2)	(3)	(4)
Nonfarm payrolls	.094 (2.425)	.113 (1.977)	.082 (1.788)	.155 (3.696)
Empl. growth (12 m)	.005 (2.144)	.004 (1.402)	.005 (1.217)	.003 (1.601)
$\Delta$ log S&P 500 (3 m)	.084 (1.446)	.112 (1.578)	.154 (1.943)	.020 (.350)
$\Delta$ Slope (3 m)	-.010 (-1.393)	-.010 (-1.154)	-.011 (-1.035)	-.016 (-2.024)
$\Delta$ log Comm. price (3 m)	.119 (2.380)	.093 (1.461)	.224 (3.489)	.103 (1.944)
Treasury skewness	.032 (3.017)	.035 (2.917)	.050 (2.127)	.023 (2.159)
$R^2$	.162	.173	.192	.163
Sample	1988:1–2019:12	1994:1–2019:12	1988:1–2007:6	1990:1–2019:6
N	322	218	216	259
Policy surprise	mps	mps	mps	FF4

Note: Coefficient estimates  $\beta$  from predictive regressions  $mps_t = \alpha + \beta'X_{t-} + u_t$ , where  $t$  indexes Federal Open Market Committee (FOMC) announcements. Columns 1–3 use our baseline monetary policy surprise measure mps described in the text, and column 4 uses the change in FF4 (also used in Gertler and Karadi 2015). Predictors  $X$  are observed prior to the FOMC announcement: the surprise component of the most recent nonfarm payrolls release, employment growth over the last year, the log change in the Standard & Poor's 500 index (S&P 500) from 3 months before to the day before the FOMC announcement, the change in the yield curve slope over the same period, the log change in a commodity price index over the same period, and the option-implied skewness of the 10-year Treasury yield from Bauer and Chernov (2023). Heteroskedasticity-consistent  $t$ -statistics are in parentheses. See text for details.

The other three columns of table 1 report results for alternative estimation samples and monetary policy surprises. The second column repeats regression (eq. [14]) with the same data but begins the sample in 1994, when the FOMC started explicitly announcing its monetary policy decisions. The results over this sample are very similar to the first column, with an  $R^2$  that is even a bit higher. The third column reports results for a sample period that stops in June 2007, before the financial crisis and zero lower bound period, again with similar estimates and a higher  $R^2$ . The last column shows results for a different measure of the monetary policy surprise: specifically, the change in the 3-month-ahead federal funds futures contract, FF4, as used by Gertler and Karadi (2015).<sup>16</sup> We estimate this regression over the largest sample for which we have FF4 data, 1990:1–2019:6, obtained from an extension of the Gürkaynak et al. (2005) data set used in Bauer and Swanson (2023). Again, the results in this column are very similar to the first three columns.

The results in table 1 confirm the substantial predictability of high-frequency monetary policy surprises found by previous authors, for a variety of different monetary policy surprise measures and samples. Notably, these results show *ex post* predictability based on full-sample estimates, and we do not claim that investors could have taken advantage of it in real time. Indeed, our model implies that monetary policy surprises are not predictable *ex ante*, similar to the implications of learning models for the predictability of stock returns (e.g., Timmermann 1993). In additional, unreported analysis, we have investigated the out-of-sample predictability of monetary policy surprises using expanding estimation windows, mimicking the prediction problem faced by investors at each point in time.<sup>17</sup> Out-of-sample predictability was generally much lower than in-sample predictability, if it was at all present, and the forecast gains from including the six predictors in table 1 were never statistically significant. This evidence is consistent with the absence of *ex ante* predictability.

### C. *Predictability with Macroeconomic Forecast Data*

In an influential recent paper, Miranda-Agrippino and Ricco (2021) showed that the Fed’s internal “Greenbook” forecasts contain substantial information that is correlated with the high-frequency monetary policy surprise around the subsequent FOMC announcement. The interpretation given by Miranda-Agrippino and Ricco is based on a Fed information effect, discussed above, whereby the monetary policy surprise

reveals information to the private sector about the Fed's internal macroeconomic forecast. However, our predictability evidence in table 1, based on publicly available information, raises the question whether one might obtain similar results if in the Miranda-Agrippino and Ricco regressions the internal Greenbook forecasts were replaced with publicly observable forecasts from the Blue Chip survey of professional forecasters. This would then suggest a very different interpretation of the Miranda-Agrippino and Ricco monetary policy surprise predictability findings.

To investigate this question, we repeat the monetary policy surprise predictability regressions in Miranda-Agrippino and Ricco, who followed Romer and Romer (2004) closely. We use exactly the same predictors as Miranda-Agrippino and Ricco: forecasts for real GDP growth and GDP deflator inflation for the previous quarter to 3 quarters ahead; the unemployment rate forecast for the current quarter; and forecast revisions for all three macro series for the previous quarter to 2 quarters ahead. As an alternative to the Fed's Greenbook forecasts, we also consider the publicly available Blue Chip consensus forecasts and forecast revisions for the exact same macro variables and forecast horizons.<sup>18</sup>

The results are reported in table 2. The top panel reports results analogous to those in Miranda-Agrippino and Ricco's table 1, using the Fed's internal Greenbook forecasts, and the bottom panel repeats the analysis using the publicly available Blue Chip forecasts instead. Each column corresponds to a different sample period, along the lines of table 1, albeit ending in 2015 rather than 2019 because the Fed only releases its Greenbook forecast data with a 5-year lag. For simplicity and brevity, for each regression we report only the  $R^2$ , the adjusted  $R^2$ , and the  $p$  value for the robust Wald test that all 23 regression coefficients (aside from the intercept) are equal to zero.

The results in the top panel of table 2 confirm those of Miranda-Agrippino and Ricco (2021): there is strong evidence that the Fed's internal Greenbook forecasts are correlated with the subsequent monetary policy surprises. However, the results in the bottom panel of table 2 show that this predictability is essentially identical when we use the publicly available Blue Chip forecasts instead. Thus, the Greenbook and Blue Chip forecasts seem to contain very similar information for upcoming FOMC announcement surprises. This observation is also consistent with Bauer and Swanson (2023), who showed that Greenbook and Blue Chip forecasts are about equally accurate predictors of future macroeconomic data.

**Table 2**  
Predictive Regressions Using Macroeconomic Forecasts

	(1)	(2)	(3)	(4)
Greenbook forecasts:				
$R^2$	.158	.225	.183	.153
Adjusted $R^2$	.085	.114	.085	.059
$p$ value	.0003	.0002	.0010	.0225
Blue Chip forecasts:				
$R^2$	.144	.217	.179	.168
Adjusted $R^2$	.070	.105	.080	.076
$p$ value	.0058	.0000	.0004	.0040
Sample	1988:1–2015:12	1994:1–2015:12	1988:1–2007:6	1990:1–2015:12
N	289	185	216	231
Policy surprise	mps	mps	mps	FF4

Note: Predictive regressions for monetary policy surprises using macroeconomic forecasts and their revisions. The regressors are forecasts and forecast revisions for the same variables and horizons as in Miranda-Agrippino and Ricco (2021) (see text), using the Fed's own Greenbook forecasts in the top panel, and the consensus forecast in the Blue Chip Economic Indicators survey in the bottom panel. We use the most recent forecasts before each Federal Open Market Committee announcement. Columns 1–3 use our baseline monetary policy surprise measure mps described in the text, and column 4 uses the change in FF4 (also used in Gertler and Karadi 2015). We report  $p$  values for robust Wald tests (using White covariance estimates) of joint significance of all predictors.

The implication of these findings is that the predictive power of Greenbook forecasts for policy surprises that was documented by Miranda-Agrippino and Ricco does not appear to be due to a Fed information effect. Instead, it seems to be a reflection of the empirical pattern we have documented above and in Bauer and Swanson (2023): monetary policy surprises are systematically correlated with macroeconomic and financial data that are publicly available prior to the monetary policy announcement.

#### *D. Interpretation of the Predictability Evidence*

How should we think about the predictability evidence documented above? First, note that these high-frequency interest rate changes should be unpredictable if (a) bond risk premia are zero or constant, and (b) investor beliefs satisfy the FIRE hypothesis.<sup>19</sup> We discuss deviations from each of these two assumptions in turn.

The first possible explanation for the predictability results in table 1 is that risk premia on the underlying interest rate securities are substantial and time varying. Indeed, Miranda-Agrippino (2017) makes exactly this

argument. Through the lens of our model in Section II, this implies that  $\text{mps}_t$  in equation (4) should include an additional risk premium term that is time varying and correlated with  $x_t$ . One problem with this explanation is that risk premia for these short-maturity interest rate futures seem to be relatively small (Piazzesi and Swanson 2008; Schmeling et al. 2022). Cieslak (2018) argues that these risk premia would have to be implausibly large to explain the observed correlation in the data and that a risk premium interpretation is inconsistent with a variety of other financial market evidence.<sup>20</sup> Thus, we view this explanation as relatively implausible, although we cannot rule it out entirely.

Instead of arguing that risk premia on short-term interest rates are large, our preferred explanation is based on moderate deviations from the strong assumption of FIRE. Much empirical work in macroeconomics has documented that expectations—of households, firms, or investors—do not satisfy the FIRE assumption.<sup>21</sup> Directly relevant for our setting here, Cieslak (2018) shows that the forecast errors for the federal funds rate in the Blue Chip survey of professional forecasters are strongly predictable. The online appendix of Bauer and Swanson (2023) updates and extends that evidence, showing that close to one-fourth of the variation in federal funds rate survey forecast errors is predictable with information observed before the survey responses were collected.<sup>22</sup> Under the FIRE assumption, forecast errors should be unpredictable using information that is publicly observable at the time the forecasts are made. Thus, this body of evidence strongly supports the view that public expectations of the Fed’s policy rate do not satisfy the FIRE assumption.<sup>23</sup>

A simple and plausible deviation from FIRE that can explain the predictability results in table 1 is that the private sector has incomplete information about the Fed’s monetary policy reaction function, as in our model of Section II. Specifically, if financial markets underestimated the Fed’s responsiveness to the economy, then that could explain the procyclical correlation of macroeconomic and financial variables with monetary policy surprises documented in table 1. For further arguments in support of this explanation, see Cieslak (2018), Bauer and Swanson (2023), and Schmeling et al. (2022).

An alternative explanation of the ex post predictability of monetary policy surprises relies on information effects. As the learning model in Miranda-Agrippino and Ricco (2021) shows, if the Fed’s announcements reveal information that the private sector uses to update its beliefs about the state of the economy, then high-frequency monetary policy surprises can be correlated with past macroeconomic data. However,

the evidence above and in Bauer and Swanson (2023) suggests that the Fed does not seem to possess an information advantage concerning the state of the economy and the future economic outlook. Thus, it seems unlikely that the Fed's monetary policy announcements reveal significant new information about the economy to the private sector.

Overall, our view is that the evidence best supports the story of imperfect information about the Fed's monetary policy rule. However, the exact reason for the predictability of the monetary policy surprises is not particularly important for the rest of our paper. What matters is that those high-frequency monetary policy surprises are correlated with macroeconomic and financial variables predating the policy announcements, which has important implications for estimating the transmission of monetary policy to financial markets and the macroeconomy using these surprises. This is what we turn to next.

#### IV. Monetary Policy Effects on Asset Prices

In this section, we estimate the effects of monetary policy announcements on asset prices. Relative to previous studies, we make two contributions: first, we use a novel measure of monetary policy surprises that is orthogonal to macroeconomic and financial data observed before the announcement and compare the estimates to those obtained for a conventional measure of the monetary policy surprise. Second, we consider not only policy announcements made by the FOMC but also those communicated in post-FOMC press conferences, speeches, and testimony by the Federal Reserve chair.

##### A. *The Event-Study Approach*

Monetary policy influences inflation and real activity through its effects on financial conditions. Changes in the current target and future expectations for the federal funds rate affect interest rates all along the yield curve, stock prices, corporate bond yields, exchange rates, and other asset prices. A large empirical literature in monetary economics estimates the transmission of monetary policy to financial markets. Starting with the landmark studies by Cook and Hahn (1989) and Kuttner (2001), event studies have been the method of choice for such empirical analysis, due to their promise to sharply identify the causal effects of monetary policy actions on interest rates and other asset prices.<sup>24</sup>

These event-study regressions are usually of the form

$$y_t = \alpha + \beta \text{mps}_t + u_t, \quad (15)$$

where  $t$  indexes monetary policy announcements,  $y_t$  is an asset return or interest/exchange rate change,  $\text{mps}_t$  is a measure of the policy surprise, and both  $y_t$  and  $\text{mps}_t$  are measured over tight windows around the announcement. The idea is that the monetary policy surprise  $\text{mps}_t$  captures a monetary policy shock and we can estimate the effects of this shock on financial markets using regression (eq. [15]). But accurate estimation of such causal effects on asset prices requires four crucial assumptions.

The first assumption is that there is no reverse causation; that is, that changes in asset prices do not affect the monetary policy action (Cook and Hahn 1989). With intradaily data and the usual 30-minute announcement windows, this assumption is very plausible: the policy decision is made, and the FOMC statement formulated, up to several hours in advance of the actual announcement via the release of the statement. It is therefore hard to argue that the FOMC decision could react in some way to asset price changes in a sufficiently narrow window of time around the announcement.<sup>25</sup>

The second assumption is that there are no omitted variables that are correlated with  $\text{mps}_t$  and independently affect  $y_t$ . News released during the event window on day  $t$  will generally affect  $y_t$  but is unlikely to be correlated with the (predetermined) policy action  $\text{mps}_t$  for the same reason as above.<sup>26</sup> However, information prior to the FOMC announcement may predict both  $\text{mps}_t$  and  $y_t$ , which would call this assumption into question. Previous event studies have generally not considered this possibility, based on the premise that high-frequency asset price changes are unpredictable. By contrast, our simple model in Section II predicted that  $\text{mps}_t$  may well be correlated with macroeconomic and financial variables observed before  $t$ , and our evidence in Section III confirmed this. Importantly, our model also predicted that the effects on  $y_t$  would be completely captured by the monetary policy surprise, and that once we condition on  $\text{mps}_t$  there is no separate role for monetary policy shocks ( $\varepsilon_t$  in the model) or macroeconomic and financial data ( $x_t$ ). Thus, according to our model, ordinary least squares (OLS) estimates of  $\beta$  in equation (15) would not suffer from omitted variable bias.

Third, the surprise  $\text{mps}_t$  must be truly unanticipated.<sup>27</sup> If the regressor contains a component that is anticipated by financial market participants, and if asset prices do not respond to this anticipated component, then this will tend to make the estimated coefficient small and insignificant

due to the presence of classical measurement error. Cook and Hahn (1989) regressed yield changes on the target rate change around FOMC decisions, but the target changes are partly anticipated by financial markets. The important contribution of Kuttner (2001) was to separate the unexpected from the expected component of the target rate change using federal funds futures, which allowed him to uncover strong and highly significant effects on bond yields. Many researchers have followed this approach since. The predictability of  $mps_t$ , documented in Section III, challenges the assumption that we have completely isolated the unexpected component of the policy surprise, and it raises the possibility of measurement error. However, estimates of the asset price response will only be affected if financial markets react differently to the predicted component of the policy surprise than to the orthogonal component. Again, our model in Section II predicts that all components of the policy surprise should lead to the same asset price reaction, so that there are no measurement error in the classical sense and no bias of the OLS estimate of  $\beta$  in equation (15).

The fourth and last assumption is that the surprise should not contain any information effects (Romer and Romer 2000; Campbell et al. 2012; Nakamura and Steinsson 2018). Such effects would be present if the central bank's monetary policy decision reveals private information about the economic outlook that directly affects macroeconomic expectations, in addition to the actual monetary policy shock. For some assets, such as stocks, information effects would typically have an effect opposite to that of a monetary policy shock. Thus, their presence could in principle lead to estimates of  $\beta$  that are smaller or even of the opposite sign than if  $mps_t$  only captured a monetary policy shock.<sup>28</sup> However, Bauer and Swanson (2023) found that the responses of macroeconomic surveys, stock prices, and exchange rates show little evidence of information effects.

### *B. Conventional and Orthogonalized Monetary Policy Surprises*

We update and extend previous results in the literature with an event study that uses our new data set of 322 FOMC announcements from 1988 to 2019, described in Section III. We estimate the event-study regression in equation (15) using two alternative measures of the policy surprise  $mps_t$ . First, as a natural starting point, we use a conventional, unadjusted high-frequency monetary policy surprise measure described in Section III: the first principal component of high-frequency changes in the

Eurodollar futures rates ED1 to ED4. This measure is essentially equal to a weighted average of the target and path factors of Gürkaynak et al. (2005) and therefore captures news about both the current federal funds rate target and the future policy path.

Our second measure of the monetary policy surprise addresses the predictability issues raised in Section III. Specifically, we construct an orthogonal measure of the monetary policy surprise by taking the residuals from the regression (eq. [14]); that is,

$$\text{mps}_t^\perp = \text{mps}_t - \hat{\alpha} - \hat{\beta}' X_{t-}, \quad (16)$$

where  $X_{t-}$  and  $\hat{\beta}$  correspond to the predictors and estimated regression coefficients in the first column of table 1. The orthogonal surprise  $\text{mps}_t^\perp$  is, by construction, uncorrelated with those macroeconomic and financial data observed before the FOMC announcement, and thus is more likely to satisfy the crucial event-study assumptions noted above. In the remainder of this section we compare the effects of  $\text{mps}_t$  and  $\text{mps}_t^\perp$  on asset prices, and in Section V we compare the effects of the two different monetary policy surprise measures on macroeconomic variables in an SVAR or LP framework.

### C. Asset Prices and FOMC Announcements

We estimate the effects of monetary policy surprises on Treasury yields and stock prices using high-frequency event-study regressions of the form (eq. [15]). The Treasury yield responses are measured using 30-minute changes in Treasury futures prices around each FOMC announcement, and the stock market response is measured using S&P 500 futures price changes over the same 30-minute windows.<sup>29</sup>

The results for the unadjusted monetary policy surprises  $\text{mps}$  are reported in the first column of table 3. All of the Treasury yields and stock prices respond very strongly to monetary policy surprises, with  $t$ -statistics of six or more. The Treasury yield responses decline with maturity, but even for the 30-year yield there is still a 25-basis-point (bp) increase per 100 bp monetary policy surprise, a  $t$ -statistic greater than 6 and an  $R^2$  greater than 20%.<sup>30</sup> The same surprise leads to a 5.4% drop in the S&P 500, with a  $t$ -statistic close to 8. These large and highly statistically significant estimates are similar to those documented by previous authors, such as Kuttner (2001), Bernanke and Kuttner (2005), Gürkaynak et al. (2005), Hanson and Stein (2015), and Swanson (2021), among others.

**Table 3**  
Asset Price Responses to Monetary Policy Surprises

	FOMC		Fed Chair Speeches	
	$\text{mps}_t$	$\text{mps}_t^\perp$	$\text{mps}_t$	$\text{mps}_t^\perp$
2-year yield:	.73	.74	.73	.72
<i>t</i> -statistic	(18.6)	(16.7)	(23.4)	(22.0)
$R^2$	.784	.689	.856	.827
5-year yield:	.63	.64	.66	.66
<i>t</i> -statistic	(14.4)	(13.8)	(16.5)	(15.8)
$R^2$	.626	.550	.737	.714
10-year yield:	.41	.41	.49	.49
<i>t</i> -statistic	(9.5)	(9.9)	(13.9)	(13.2)
$R^2$	.435	.363	.651	.627
30-year yield:	.25	.25	.39	.38
<i>t</i> -statistic	(6.3)	(6.7)	(10.5)	(10.1)
$R^2$	.206	.173	.479	.455
S&P 500:	-5.39	-5.50	-1.59	-1.56
<i>t</i> -statistic	(-7.7)	(-6.6)	(-2.5)	(-2.5)
$R^2$	.304	.266	.027	.025
Observations	322	322	295	295

Note: Estimated coefficients  $\beta$  and regression  $R^2$  from high-frequency event-study regressions  $y_t = \alpha + \beta \text{mps}_t + u_t$ , where  $t$  indexes Federal Open Market Committee (FOMC) announcements or Fed chair speeches,  $y_t$  denotes the change in the 2-, 5-, 10-, or 30-year Treasury yield or log S&P (Standard & Poor's) 500 price index in a narrow window of time around each announcement, and the regressor  $\text{mps}_t$  is either the unadjusted high-frequency monetary policy surprise measure  $\text{mps}_t$  or  $\text{mps}_t^\perp$ , the residual from regressing  $\text{mps}_t$  on the predictors in table 1. Heteroskedasticity-consistent  $t$ -statistics are in parentheses. Sample: 1988:1–2019:12. See text for details.

Analogous results for our orthogonalized monetary policy surprise measure,  $\text{mps}_t^\perp$ , are reported in the second column of table 3, and they are similar to the first column. The point estimates are almost identical, the  $t$ -statistics are very similar, and the regression  $R^2$  are similar, albeit a little lower in the second column. Additional, unreported estimates of an alternative regression specification that includes  $\text{mps}_t$  together with the macroeconomic and financial variables from table 1 yielded similar coefficient estimates on  $\text{mps}_t$  as in the first column of table 3, and coefficients on the additional variables that were statistically insignificant.

These estimates suggest that the predictability of monetary policy surprises does not cause any noticeable problems for standard high-frequency event-study regressions estimating the effects of monetary policy surprises on financial markets. This predictability appears to cause neither omitted variable bias nor classical measurement error in these

regressions, consistent with the implications of our model in Section II. The economic and financial news variables are correlated with  $\text{mps}_t$ , but once we account for the effects of  $\text{mps}_t$ , there are no independent effects of these other variables on asset prices. In addition, the component of  $\text{mps}_t$  correlated with news variables predating  $t$  apparently leads to a similar asset price response as the orthogonal component of  $\text{mps}_t$ .

The key takeaway is that conventional monetary policy surprises can be used to estimate the effects of monetary policy on financial markets, even though these policy surprises are partly predictable. This empirical conclusion is consistent with a simple model in which the predictability of monetary policy surprises arises as a consequence of the private sector's imperfect information about the Fed's monetary policy rule.

#### *D. Monetary Policy Surprises around Fed Chair Speeches*

News about monetary policy is released not only through FOMC announcements but also through other communication by FOMC members and the Feb. Speeches by the Fed chair are particularly important, given the influence of the chair on the committee's decisions. Leveraging the work of Swanson and Jayawickrema (2021), we construct measures of the monetary policy surprise around post-FOMC press conferences, speeches, and congressional testimony by the Federal Reserve chair and investigate their effects on asset prices. (For brevity, we refer to these types of communication by the Fed chair as "speeches.") Over our sample period, 1988–2019, there are 880 such speeches by the Fed chair (compared with 322 FOMC announcements), but many of those speeches are on topics unrelated to monetary policy.<sup>31</sup> To identify those speeches that did contain significant news about monetary policy, we did the following: first, we included all 40 post-FOMC press conferences and all 126 semiannual monetary policy report testimonies by the Fed chair to Congress, because these press conferences and testimonies always discuss US monetary policy at length.<sup>32</sup> Second, we included all 22 speeches by the Fed chair at the Fed's annual Jackson Hole symposium for central bank leaders, because these speeches also typically discuss US monetary policy in detail and are closely followed by the markets. Third, we identified all of the remaining speeches by the Fed chair that led to a substantial (3 bp or more) reaction in the 2-quarter-ahead Eurodollar futures contract (ED3). We checked whether these additional speeches contained news about monetary policy, or whether the market was moved by news unrelated to the speech, by reading the market commentary in *The Wall*

*Street Journal* or *New York Times* that afternoon or the following morning.<sup>33</sup> This resulted in an additional 107 speeches by the Fed chair that contained significant news about monetary policy.

All together, the above criteria leave us with 295 Fed chair speeches that contained significant news about monetary policy. For each of these 295 speeches, we have the exact date and time of the speech and high-frequency asset price changes around that speech from Swanson and Jayawickrema (2021).<sup>34</sup>

The last two columns of table 3 report the estimated effects of Fed chair speeches on financial markets. The 2- and 5-year Treasury yields respond almost identically to Fed chair speeches as they do to FOMC announcements, and 10- and 30-year Treasury yields respond even more strongly. The  $R^2$  for Fed chair speech effects are also even higher than those for FOMC announcements. Together, these observations confirm the general point in Swanson and Jayawickrema (2021) that speeches by the Fed chair are even more important for the Treasury market than FOMC announcements themselves.

By contrast, the response of the stock market is substantially weaker, with an  $R^2$  around 3%. The modest stock market response to Fed chair speeches is somewhat puzzling in light of the fact that monetary policy typically has pronounced effects on the stock market (Bernanke and Kuttner 2005; Gürkaynak et al. 2005). One possible explanation is based on information effects: speeches by the Fed chair could potentially have larger information effects than FOMC announcements, given the extensive conversations the chair is having with the public or Congress about the Fed's outlook for monetary policy and the US economy. For example, many of the chair's speeches are semiannual monetary policy reports to Congress, which are 3 hours long and include extensive question-and-answer sessions about many aspects of the US economy as well as monetary policy. As argued in Nakamura and Steinsson (2018), Cieslak and Schrimpf (2019), and Jarocinski and Karadi (2020), information effects could mute the negative stock market response to changes in the expected policy path, or even reverse its sign. Another explanation is that other news besides the chair's speech could have moved interest rates and stock prices during the event window. Our announcement windows for chair speeches are necessarily longer than for FOMC announcements (2 hours for regular speeches and press conferences and 3.5 hours for testimony, vs. 30 minutes for FOMC announcements) and sometimes occur in the mornings, when economic data are released.<sup>35</sup> Any news about employment or output would tend to move interest rates and stock prices in the

same direction, in contrast to news about monetary policy (Andersen et al. 2007), explaining why the stock market response is less negative.<sup>36</sup> A third possible explanation is that the stock market is more sensitive to actual federal funds rate changes than to forward guidance, as found by Gürkaynak et al. (2005). The chair's speeches do not change the current federal funds rate and thus can be thought of as pure forward guidance. Of course, all of these mechanisms could be at work, and without further evidence we cannot distinguish between them.

For monetary policy surprises around Fed chair speeches, we also estimate predictive regressions using macroeconomic and financial data that predate the speeches. The predictability is generally quite a bit lower than for FOMC announcements, with  $R^2$  in the single digits. As shown in the last column of table 3, using the orthogonalized monetary policy surprise  $\text{mps}_t^\perp$  in asset price regressions has little effect on the high-frequency estimates relative to using the unadjusted  $\text{mps}_t$  itself.

## V. Monetary Policy Effects on the Macroeconomy

Many recent studies use high-frequency changes in interest rates around FOMC announcements as an instrument to help estimate the effects of monetary policy on macroeconomic variables such as output and inflation; for a survey, see Ramey (2016). Our results in Section III, however, imply that these high-frequency monetary policy surprises are correlated with those economic variables, violating the standard exogeneity condition that is required for the instrument to be valid. Our orthogonalization procedure discussed above corrects the monetary policy surprises for this correlation and should alleviate the problem.

We now investigate to what extent the high-frequency identifications of the effects of monetary policy shocks in SVARs and LPs are affected by this correlation and our proposed correction. We begin, in Subsection V.A, by laying out the basic proxy-SVAR method and revisiting the analysis in Gertler and Karadi (2015), which has become a canonical benchmark specification for monetary policy SVARs. In Subsection V.B, we estimate LPs similar to those in Ramey (2016). In Subsection V.C, we consider the alternative estimation method of Plagborg-Møller and Wolf (2021) that uses a recursive SVAR with the monetary policy instrument ordered first. In Subsection V.D, we revisit some of the analysis in Miranda-Agrippino and Ricco (2021) and show that similar SVAR results are obtained when either Blue Chip consensus forecasts or Greenbook forecasts are used to orthogonalize the policy surprises. Finally,

in Subsection V.E, we summarize lessons learned and present new “best practice” estimates of the macroeconomic effects of monetary policy shocks.

#### A. *Revisiting Gertler and Karadi (2015)*

##### Baseline VAR Specification

As in Gertler and Karadi (2015), we begin by estimating a reduced-form monthly VAR with four macroeconomic variables as our baseline specification: the log of industrial production (IP), the log of the consumer price index (CPI), the Gilchrist and Zakrajšek (2012) excess bond premium (EBP), and the 2-year Treasury yield. IP and the CPI are taken from the FRED database at the Federal Reserve Bank of St. Louis. We include the GZ EBP (available from the Federal Reserve Board’s website) for comparability to Gertler and Karadi and because Caldara and Herbst (2019) found it to be important for the estimation of monetary policy VARs. The 2-year Treasury yield is from the Gürkaynak, Sack, and Wright (2007) database on the Federal Reserve Board’s website. As discussed in Swanson and Williams (2014) and Gertler and Karadi (2015), the 2-year Treasury yield was essentially unconstrained during the 2009–15 zero lower bound period in the United States, making it a better measure of the stance of monetary policy than a shorter-term interest rate such as the federal funds rate. Note that Gertler and Karadi used the 1-year Treasury yield rather than the 2-year yield but only because they were unable to get a sufficiently large  $F$ -statistic for their first-stage instrumental variables regression; as shown later, we do not have this problem, which makes use of the 2-year Treasury yield feasible for our analysis.<sup>37</sup> We stack these four variables into a vector  $Y_t$  and estimate the reduced-form VAR

$$Y_t = \alpha + B(L)Y_{t-1} + u_t, \quad (17)$$

where  $B(L)$  denotes a matrix polynomial in the lag operator,  $u_t$  is a  $4 \times 1$  vector of regression residuals that are serially uncorrelated, and  $\text{Var}(u_t) = \Omega$ , which is not necessarily a diagonal matrix. We follow Gertler and Karadi (2015), Ramey (2016), and many others and use a specification with 12 monthly lags.

We estimate regression (eq. [17]) from January 1973 to February 2020 via OLS. The GZ EBP data begin in 1973, preventing us from beginning the sample earlier. We choose to end our sample in February 2020 to

avoid the dramatic swings in IP that begin with onset of the COVID-19 pandemic in the United States. We also consider and discuss alternative sample periods, because this was a main point discussed by Ramey (2016).

We follow standard practice and assume that the economy is driven by a set of serially uncorrelated structural shocks,  $\varepsilon_t$ , with  $\text{Var}(\varepsilon_t) = I$  (see, e.g., Ramey 2016). Because the dynamics of the economy are determined by  $B(L)$ , the effects of different structural shocks  $\varepsilon_t$  on  $Y_t$  are completely determined by differences in their impact effects on  $Y_t$  in period  $t$ , that is, by their effects on  $u_t$ . We assume that this relationship is linear,

$$u_t = S\varepsilon_t, \quad (18)$$

where  $S$  is a matrix of appropriate dimension. If the number of shocks in  $\varepsilon_t$  equals the number of variables in the VAR, a common assumption in the SVAR literature, then equation (18) implies invertibility.<sup>38</sup> However, we do not need to impose that restriction for our purpose of estimating impulse response functions to a monetary policy shock, so  $\varepsilon_t$  can in principle include any number of additional structural shocks.<sup>39</sup> We will return to the issue of invertibility in Subsection V.C.

We assume that one of the structural shocks is a “monetary policy shock,” and we order that shock first in  $\varepsilon_t$  and denote it by  $\varepsilon_t^{\text{mp}}$ . The idea of a structural monetary policy shock is that sometimes the Fed is faced with a decision that is a “close call” between two options and must pick one option or the other; the difference in effects between these two choices is the outcome of a structural monetary policy shock (see Ramey 2016 for additional discussion). Given our choice of high-frequency instrument—the first principal component of the first 4 Eurodollar futures contracts, ED1–ED4—this shock should be thought of as a change in the outlook for the path of short-term interest rates over the next 4 quarters. Intuitively, this includes changes in the current federal funds rate as well as some degree of “forward guidance” about the near-term path of future values of the federal fund rate.

The first column of  $S$  describes the impact effect of the structural monetary policy shock  $\varepsilon_t^{\text{mp}}$  on  $u_t$  and  $Y_t$ . The variances of  $u_t$  and  $\varepsilon_t$  imply that

$$SS' = \Omega. \quad (19)$$

The identification problem is that there are infinitely many potential matrices  $S$  that satisfy equation (19), so that  $S$  cannot be uniquely determined by the data (even with infinitely many observations of  $Y_t$ ). The econometrician must bring additional information to bear on the problem—either theoretical or empirical—to estimate  $S$  and the dynamic

effects of a structural shock on  $Y_t$ . Our identification problem is simplified somewhat by the fact that estimation of the effects of monetary policy shocks does not require identification of the entire matrix  $S$  but only of its first column,  $s_1$ , and only up to scale, because we follow common practice and estimate impulse responses to a policy shock that is normalized to have a 0.25 percentage point impact effect on the interest rate.

### High-Frequency Identification

To identify the impact effect  $s_1$  of a structural monetary policy shock  $\varepsilon_t^{\text{mp}}$ , we use the high-frequency identification approach of Gertler and Karadi (2015), described in detail by Stock and Watson (2012, 2018). Let  $z_t$  denote our set of high-frequency monetary policy surprises, converted to a monthly series by summing over all of the high-frequency surprises mps within each month. For  $z_t$  to be a valid instrument for  $\varepsilon_t^{\text{mp}}$ , it must satisfy an instrument *relevance* condition,

$$E[z_t \varepsilon_t^{\text{mp}}] \neq 0, \quad (20)$$

and an instrument *exogeneity* condition,

$$E[z_t \varepsilon_t^{-\text{mp}}] = 0, \quad (21)$$

where  $\varepsilon_t^{-\text{mp}}$  denotes any element of  $\varepsilon_t$  other than the first.<sup>40</sup> Stock and Watson (2012, 2018) refer to  $z_t$  as an *external instrument* because it comes from information outside of the VAR—in particular, from high-frequency financial market data.

The appeal of high-frequency monetary policy surprises is that they plausibly satisfy conditions (eqs. [20]–[21]). First, consider instrument relevance: the monetary policy shock  $\varepsilon_t^{\text{mp}}$  is the total amount of exogenous news about monetary policy in month  $t$ . FOMC announcements and Fed chair speeches are an important part of this news, so it is reasonable to expect that the correlation between  $z_t$  and  $\varepsilon_t^{\text{mp}}$  is positive and may be large.<sup>41</sup> Crucially, monetary policy surprises that include Fed chair speeches will provide a more relevant instrument than those based solely on FOMC announcements.

Second, consider instrument exogeneity: high-frequency monetary policy surprises capture interest rate changes in very narrow windows of time around policy announcements. It would therefore appear unlikely that other structural shocks in  $\varepsilon_t^{-\text{mp}}$  can significantly affect financial

markets at the same time, so that these other shocks should be uncorrelated with  $z_t$ , implying equation (21).<sup>42</sup>

However, the predictability documented in Section III suggests a potential violation of the exogeneity condition (eq. [21]) and calls the validity of  $z_t$  as an instrument into question. In particular, equation (21) is violated if  $z_t$  is correlated with macroeconomic news that occurs within the month, and all of the financial market predictors in table 1 are very plausibly correlated with shocks to output, inflation, and the EBP.<sup>43</sup> Thus, the structural VARs estimated by previous authors using high-frequency identification likely have an endogeneity problem that biases their estimates. For example, as shown in table 1, news about higher output or inflation reflected in the stock market or commodity prices tends to predict a higher value of  $z_t$ ; thus, the estimated effects of a monetary policy tightening are contaminated by the fact that tighter monetary policy is correlated with news about higher output and inflation, biasing the estimated effects of a monetary policy tightening on real activity and inflation in the positive direction (attenuating or even reversing the sign of the estimated effects).<sup>44</sup>

To eliminate this endogeneity problem, we project out the correlation of  $z_t$  with the macroeconomic and financial predictors from Section III, as suggested by Bauer and Swanson (2023). We construct an orthogonalized version of our monthly monetary policy instrument,  $z_t^\perp$ , by regressing  $z_t$  on the predictors in table 1 and taking the residuals.<sup>45</sup> This instrument is more likely to satisfy the exogeneity condition (eq. [21]), leading to estimates of the effects of monetary policy on the economy that are free from the bias. Moreover,  $z_t^\perp$  should still satisfy the relevance condition (eq. [20]), because most of the variation in mps was not predictable by macroeconomic and financial variables and represents information about the future path of monetary policy.

Given our external instrument,  $z_t$  or  $z_t^\perp$ , we estimate the impact effects  $s_1$  in the SVAR as described in Stock and Watson (2012, 2018) and Gertler and Karadi (2015). For concreteness, order the 2-year Treasury yield last in  $Y_t$ , and denote it by  $Y_t^{2y}$ . We then estimate the regression

$$Y_t = \alpha + B(L)Y_{t-1} + s_1 Y_t^{2y} + \tilde{u}_t \quad (22)$$

via two-stage least squares, using  $z_t$  or  $z_t^\perp$  as the instrument for  $Y_t^{2y}$ , where  $s_1$  is the first column of  $S$  described above,  $\alpha$  and  $B(L)$  are as in equation (17), and  $\tilde{u}_t$  is a regression residual.<sup>46</sup> Because the reduced-form residuals in equation (17) satisfy  $u_t = S\varepsilon_t$ , it is straightforward to show that equations (20) and (21) imply this regression produces an unbiased and

consistent estimate of  $s_1$ , with the last element normalized to unity. (In our empirical results shown later, we rescale  $s_1$  so that the last element corresponds to an impact effect of 25 basis points, rather than 1 percentage point.)

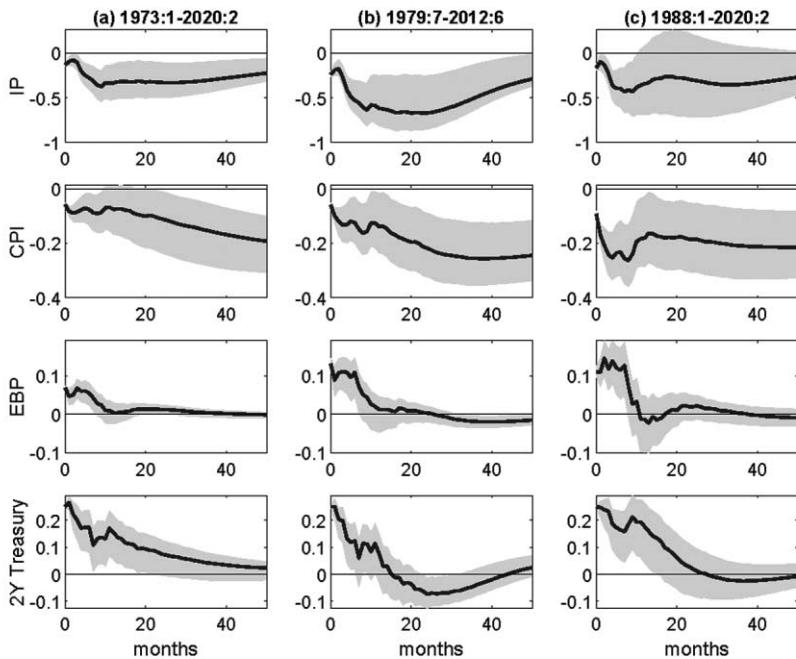
Importantly, the sample for the two-stage least squares regression (eq. [22]) used to estimate  $s_1$  does not have to be the same as for the reduced-form VAR (eq. [17]) used to estimate  $\alpha$  and  $B(L)$ , as discussed by Stock and Watson (2012, 2018) and Ramey (2016). In our data set, the high-frequency monetary policy surprises underlying  $z_t$  and  $z_t^\perp$  are available only from 1988:1 to 2019:12. By contrast, we are able to estimate the reduced-form VAR coefficients  $\alpha$  and  $B(L)$  over the longer sample from 1973:1 to 2020:2.

### Results Based on FOMC Announcements

Figure 2 reports impulse response functions to a 25 bp monetary policy shock in our baseline structural VAR, described above, using the unadjusted high-frequency monetary policy surprise instrument,  $z_t$ . This specification corresponds very closely to that in Gertler and Karadi (2015), Ramey (2016), and others. Column *a* reports the results for our full sample, January 1973 to February 2020, and columns *b–c* report results for two different subsamples. The solid lines report the estimated impulse response functions, and the shaded gray regions report 90% standard-error bands around those point estimates, computed using 10,000 bootstrap replications.<sup>47</sup>

The results in column *a* of figure 2 are very similar to those in Gertler and Karadi (2015), which is not surprising given the very similar specification and data, although we have used the 2-year Treasury yield instead of the 1-year yield, a longer sample (1973:1–2020:2), and a slightly different measure of the high-frequency monetary policy surprise with several more years of data (1988:1–2019:12). The 2-year Treasury yield increases 25 bp on impact, by construction, and then declines gradually back toward steady state. The EBP increases about 5 bp on impact, remains at about that level for several months, and then declines back toward steady state. IP drops slightly on impact and then declines more significantly afterward, with a trough response of about  $-0.35\%$  after about 1 year. The CPI drops slightly on impact, by about 0.05%, and then declines gradually a bit more over the next several years.

Column *b* of figure 2 repeats the analysis in column *a*, but for Gertler and Karadi's sample, July 1979 to June 2012. The standard-error bands



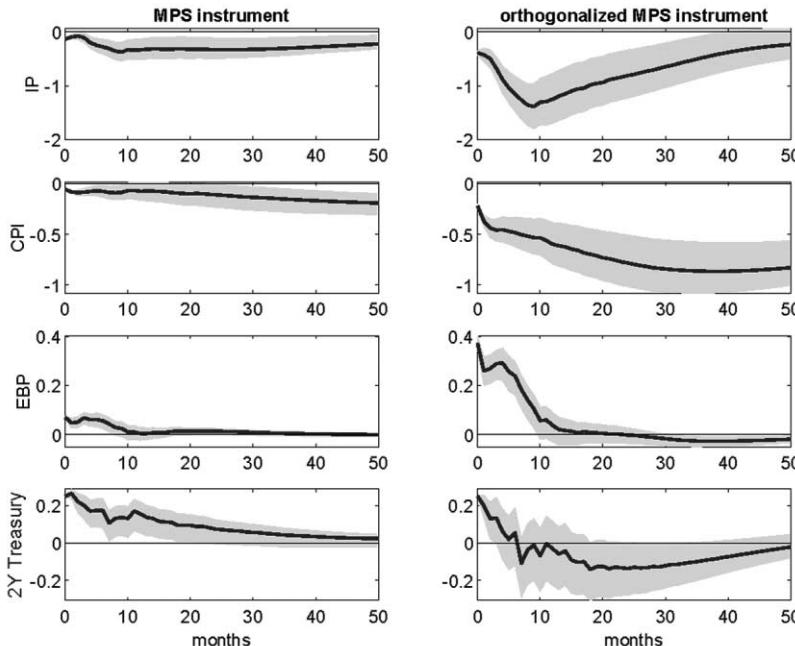
**Fig. 2.** Structural VAR with external instrument, different sample periods. Structural vector autoregression impulse response functions to a 25-basis-point monetary policy shock, identified using the unadjusted high-frequency mps measure around Federal Open Market Committee announcements for three different sample periods: (a) full sample, 1973:1–2020:2; (b) Gertler and Karadi’s sample, 1979:7–2012:6; and (c) 1988:1–2020:2, because our high-frequency mps data begin in 1988. Shaded regions report bootstrapped 90% standard-error bands. EBP = excess bond premium, CPI = Consumer Price Index, IP = industrial production. See text for details. A color version of this figure is available online.

in column *b* are somewhat larger, due to the smaller sample size, but the impulse response functions are otherwise similar. Output, inflation, and the EBP respond by somewhat more on impact for this sample but have very similar shapes and are within the range of sampling variability.

Column *c* of figure 2 repeats the analysis once more, for the sample beginning in 1988, when our high-frequency mps data are first observed. Although Ramey (2016) suggests that samples beginning after the mid-1980s may not have enough variation in monetary policy to produce good estimates of its effects, we find no evidence of such a problem here: our results in column *c* are very similar to those in the first two columns, albeit with larger standard errors than in column *a*, due to the shorter sample.

The impulse response functions in figure 2 are also robust to standard variations in our baseline specification, such as using the 1-year Treasury yield instead of the 2-year yield or including the unemployment rate as an additional variable. We do not report those results here in the interest of space, but figure A1 provides them for four variations of our baseline specification that match those used by previous authors, and they are all very similar to those in figure 2.<sup>48</sup>

We now turn to one of the main research questions of this paper: How much difference does orthogonalizing the high-frequency surprises make for estimating the effects of monetary policy on the economy? Figure 3 provides an answer to this question, with the left column repeating the baseline results for our full sample from figure 2 column *a*, and the



**Fig. 3.** Structural VAR with external instruments. Structural vector autoregression impulse response functions to a 25-basis-point monetary policy shock (MPS), identified in the left column using the unadjusted high-frequency mps measure around Federal Open Market Committee (FOMC) announcements, and in the right column using high-frequency change in mps around FOMC announcements orthogonalized with respect to economic news available prior to the announcement. Sample: 1973:1–2020:2. Shaded regions report bootstrapped 90% standard-error bands. EBP = excess bond premium, CPI = Consumer Price Index, IP = industrial production. See text for details. A color version of this figure is available online.

right column reporting results for the same specification and sample but using the orthogonalized monetary policy surprise instrument,  $z_t^\perp$ .

The first point to note in figure 3 is that the persistence of the 2-year Treasury yield response is much lower in the right-hand column, returning back to steady state in less than 1 year rather than 4 years. This is intuitive if we think of economic data as being persistent, so that the Fed's response to that data—which we have projected out in the right column—leads to an upwardly biased estimate of interest rate persistence in the left column.

The second key point to take away from figure 3 is that the responses of output, inflation, and the EBP in the right column are all larger than in the left column, by a factor of about four. For example, IP has a trough response of about  $-1.4\%$  in the right column versus  $-0.35\%$  in the left column. These stronger impulse responses are intuitive if we think of the right column as being free of the bias that is likely contaminating the estimates in the left column. For example, standard macroeconomic models such as Christiano, Eichenbaum, and Evans (2005) imply that positive news about output or inflation causes the Fed to raise interest rates and also causes output or inflation to increase; this is exactly opposite to the standard effects of monetary policy and leads to an upward bias in the top two panels of the left column.<sup>49</sup> In the right column, the monetary policy instrument is orthogonalized with respect to this news, eliminating the bias.<sup>50</sup>

Although our estimates in the right column of figure 3 are four times larger than in the left column, the magnitudes are quite reasonable. Coibion (2012) surveys estimates of the effects of monetary policy in the literature, with the estimates in Gertler and Karadi (2015) being similar to those from other SVARs, which Coibion regards as small.<sup>51</sup> In contrast, the estimates in Romer and Romer (2004) are six times larger than those in the SVARs. Coibion (2012) argues that the true effects of a monetary policy shock lie in between these two sets of estimates, which is consistent with what we estimate in the right column of figure 3.

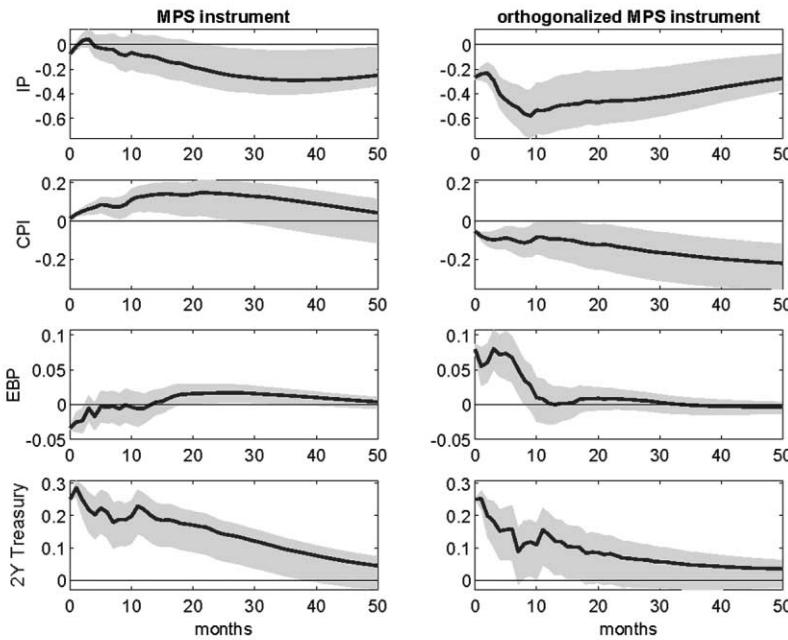
It is also interesting to note that, in the right column of figure 3, the responses of output and the policy instrument have very different persistences, with a relatively transitory effect on the 2-year yield and a long-lasting effect on IP. This endogenous persistence of output can be explained with medium-scale dynamic stochastic general equilibrium (DSGE) models that feature, for example, consumption habits, staggered wage contracts, and variable capital utilization (e.g., Christiano et al. 2005).

Finally, a potential concern with high-frequency identification is that the instrument may be weak, with relatively little relevance. Stock and Watson (2012) use a rule of thumb according to which the instrument is weak if the first-stage  $F$ -statistic in the two-stage least squares regression is less than 10. In our SVAR results above, the first-stage  $F$ -statistic for  $z_t$  is 8.19 in the left column and only 1.83 for  $z_t^\perp$  in the right column.<sup>52</sup> Thus, the orthogonalization procedure reduces the relevance of our instrument—which was already not very strong—to the point where weakness is a serious concern. Even for our unadjusted instrument  $z_t$ , the Stock and Watson rule of thumb suggests potential weakness. Indeed, it was precisely this problem that led Gertler and Karadi (2015) to modify their specification to use the month-average 1-year Treasury yield rather than the end-of-month 2-year Treasury yield we have used here. Instead of modifying our baseline specification, as Gertler and Karadi did, we propose increasing the power of our high-frequency instrument by bringing to bear additional data on high-frequency interest rate responses to speeches by the Fed chair, which Swanson and Jayawickrema (2021) showed have been an even more important source of information about monetary policy than FOMC announcements themselves.

#### Results Based on FOMC Announcements and Fed Chair Speeches

High-frequency monetary policy surprises around FOMC announcements are an imperfect measure of the true monetary policy shock each month, because a great deal of information about the course of monetary policy is communicated to the public outside of FOMC announcements, such as in speeches by the Fed chair and other FOMC members. To improve the relevance of our high-frequency monetary policy instrument and avoid a potential weak-instrument problem, we now include information from speeches, press conferences, and congressional testimony by the Federal Reserve chair, as discussed earlier (and recall that, for brevity, we refer to all of these communications as “Fed chair speeches”).

In figure 4, we repeat the structural VAR estimation and identification from figure 3, but this time including Fed chair speeches as well as FOMC announcements in our high-frequency measure of monetary policy surprises. As before, we sum up all of the high-frequency monetary policy surprises in a given month to arrive at a monthly instrumental variable,  $z_t$ . The power of the instrument  $z_t$  is greatly increased by this addition, with the first-stage  $F$ -statistic in the two-stage least squares regression rising from 8.19 in the previous section to 30.44 here. For the



**Fig. 4.** Structural VAR with external instruments, including Fed chair speeches. Structural vector autoregression impulse response functions to a 25-basis-point monetary policy shock (MPS), identified in the left column using raw high-frequency mps measure around Federal Open Market Committee (FOMC) announcements and speeches by the Fed chair, and in the right column using high-frequency mps around FOMC announcements and Fed chair speeches orthogonalized with respect to economic news available prior to the announcement. Sample: 1973:1–2020:2. Shaded regions report bootstrapped 90% standard-error bands. EBP = excess bond premium, CPI = Consumer Price Index, IP = industrial production. See text for details. A color version of this figure is available online.

orthogonalized instrument  $z_t^\perp$ , the first-stage  $F$ -statistic increases from 1.83 to 12.37.

Comparing the left column of figure 4 to figure 3, the 2-year Treasury yield response is almost identical. The response of IP is also similar, albeit with a slight output puzzle for about 2 months shortly after the shock's impact. The CPI in the left column of figure 4 displays a true price puzzle, responding positively for more than 4 years after the shock, and the EBP response also displays a puzzle, dropping on impact and remaining at zero or below for about a year.

Thus, several of the impulse responses in the left-hand column of figure 3 exhibit puzzling behavior. One possible explanation for this is that speeches by the Fed chair convey more information about the economy

and financial markets—either through a “Fed information effect” or a “Fed response to news” channel—than do FOMC announcements. Many speeches by the Fed chair, especially the semiannual monetary policy reports to Congress, do in fact discuss the US economy and how the Fed is responding to the economy at length, so this explanation is plausible. Thus, the endogeneity problem for the unadjusted high-frequency mps instrument may be even larger in figure 4 than it was in figure 3.

The right column of figure 4 eliminates this endogeneity by using the orthogonalized monetary policy surprise instrument  $z_t^\perp$  rather than the unadjusted  $z_t$ . Orthogonalization has substantial effects on the estimated impulse responses. First, all of the output, price, and EBP puzzles are eliminated once we switch to the orthogonalized instrument. Second, the 2-year Treasury yield response is somewhat less persistent in the right column than in the left, consistent with our finding in figure 3. Third, the impulse response functions in the right-hand column of figure 4 are very similar to those in figure 3 in shape and timing, although they are a bit smaller. Thus, despite the low first-stage *F*-statistics using just FOMC announcements, the estimated effects of monetary policy are robust when we extend the instrument set to include speeches by the Fed chair. Overall, the differences between the columns are similar to those in figure 3 and are consistent with the orthogonalized monetary policy instrument being purged of endogenous Fed responses to economic data.

## Summary

To summarize, there are three main points to take away from our reassessment of the high-frequency SVAR estimates in Gertler and Karadi (2015). First, we have consistently found that estimates using unadjusted monetary policy surprises as an external instrument are biased, leading to attenuated or “puzzling” dynamic responses. That is, estimates of the effects of monetary policy on output or inflation using unadjusted monetary policy surprises generally produce estimates that are either too small or even go in the opposite direction from what standard economic theory would predict. Using our adjusted, orthogonalized monetary policy surprise instrument consistently produced better results. This is not too surprising, given that our corrected monetary policy surprises should be largely free of the econometric endogeneity problems that we documented for the unadjusted surprises.

Second, using Fed chair speeches as well as FOMC announcements to measure the monetary policy surprise each month also helps to produce

more reliable estimates. This is most evident comparing our LP estimates in figure 5 of Subsection V.B to figure A2, but we have also found this to be the case more generally as well. This finding is also not too surprising, because the larger set of monetary policy announcement events roughly doubles the explanatory power of the external instrument and leads to first-stage instrumental variables  $F$ -statistics that are much higher than those using FOMC announcements alone.

Third, the results are generally robust to variations in sample period and specification, as in figures 2 and A1, especially when using our orthogonalized monetary policy surprise measure. This robustness to using a later sample period is an important point when comparing our SVAR results to those using LPs, shown later.

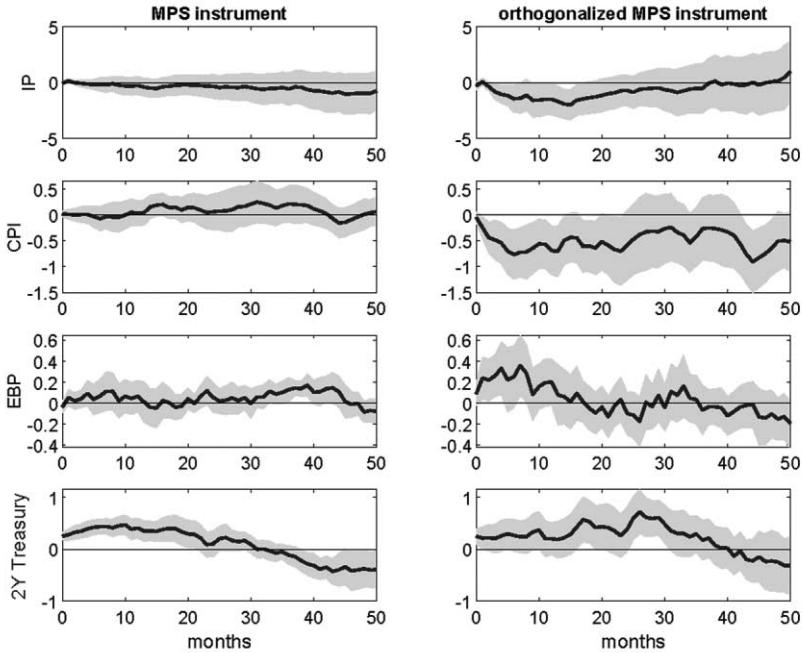
### B. Revisiting Ramey's (2016) Local Projections Estimates

An alternative approach to structural VARs is to estimate the dynamic effects of a monetary policy shock via Jordà (2005) local projections (LPs). The idea is to directly regress future values of macroeconomic variables on the identified monetary policy shock, with controls for lags and other relevant macroeconomic variables. When the monetary policy shock is unobserved but we have an external instrument, such as our high-frequency monetary policy surprise measures  $z_t$  and  $z_t^\perp$ , we can perform the LP regressions on the 2-year Treasury yield using these instruments. This procedure, known as LP-IV, is performed by Ramey (2016) and discussed in detail in Stock and Watson (2018). In this section, we revisit Ramey's LP estimates to assess the importance of monetary policy surprise predictability for those results.

We match our LP-IV specification to our VAR as closely as possible by using the same variables and the same number of lags (12 months). Although Ramey (2016) used only three monthly lags for her LP-IV specification, we found that using so few lags led to substantial differences relative to using a larger number more consistent with a VAR (see also the discussion in Ramey 2022). Thus, our LP-IV regressions have the form

$$Y_{t+h} = \alpha^{(h)} + A^{(h)}(L)Y_{t-1} + \theta^{(h)}Y_t^{2y} + \eta_t^{(h)}, \quad (23)$$

where  $Y$  includes the same variables as in our VAR,  $h \geq 0$  indexes the horizon of the impulse response function, the regression (eq. [23]) is estimated separately for each horizon  $h$ ,  $\alpha^{(h)}$  is a constant,  $A^{(h)}(L)$  is a matrix



**Fig. 5.** Local projections. Local projections impulse response functions to a 25-basis-point monetary policy shock (MPS), identified in the left column using unadjusted high-frequency mps measure around Federal Open Market Committee (FOMC) announcements and speeches by the Fed chair, and in the right column using high-frequency mps measure around FOMC announcements and Fed chair speeches orthogonalized with respect to economic news available prior to the announcement. Sample period: 1988:1–2020:2. Shaded regions report 90% standard-error bands. EBP = excess bond premium, CPI = Consumer Price Index, IP = industrial production. See text for details. A color version of this figure is available online.

polynomial of degree 11 (allowing for 12 lags),  $\theta^{(h)}$  is the coefficient of interest,  $Y_t^{2y}$  denotes the 2-year Treasury yield, and  $\eta_t^{(h)}$  is the regression residual. Equation (23) is estimated via two-stage least squares using either the unadjusted  $z_t$  or orthogonalized  $z_t^\perp$  as the instrument for  $Y_t^{2y}$ . Our sample period for the estimation runs from 1988:1 to 2020:2, because our high-frequency mps data begin in 1988. Standard errors are computed using Newey and West (1987) with  $h$  lags.

The results from this procedure are generally more poorly estimated than for our SVAR specifications above: they have large standard errors, suffer from month-to-month volatility, and also show large differences when speeches by the Fed chair are excluded versus included in the

monetary policy surprise instrument. Figure 5 reports results for the latter case, when Fed chair speeches are included in the monetary policy surprise measure. (The corresponding results when Fed chair speeches are excluded from the instrument have even larger standard errors and are reported in fig. A2.)<sup>53</sup>

Although the impulse responses in figure 5 are imprecisely estimated and somewhat more erratic, they are otherwise qualitatively consistent with those for SVARs shown in figures 3–4. Comparing the left and right columns of figure 5, the estimates in the right column produce stronger responses of output, inflation, and the EBP to the monetary policy shock, and eliminate the slight output puzzle, price puzzle, and EBP puzzle that are present in the left column. Thus, as in figures 3–4, using the unadjusted high-frequency mps instrument seems to produce results that are biased, with attenuated or puzzling responses, and that bias is largely eliminated when we use the mps measure that has been orthogonalized with respect to macroeconomic and financial news.

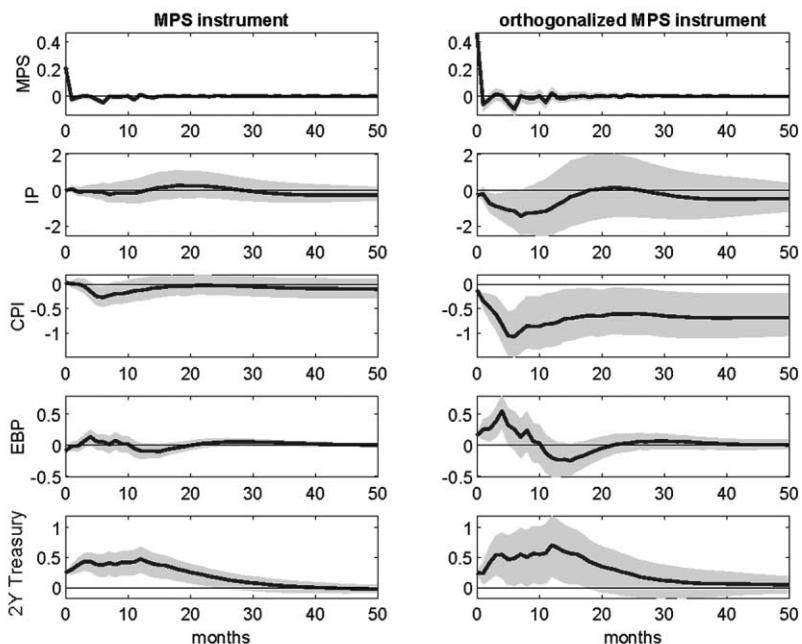
We conclude from this exercise that the estimated impulse responses to a monetary policy shock using LP-IV are generally similar to those from a structural VAR, but substantially less precisely estimated. This conclusion contrasts somewhat with Ramey (2016), who found more substantial differences between LP-IV and SVAR impulse responses, but we found those differences to be primarily due to the shorter, 3-month lag length Ramey used for her LP-IV specification.<sup>54</sup> Our main point, however, is that conventional, unadjusted high-frequency surprises are a poor choice of instruments for monetary policy shocks in LPs, which agrees with Ramey's conclusions, and we have shown how one can construct instruments that are more relevant and more likely to be exogenous.

### C. *Revisiting Plagborg-Møller and Wolf (2021)*

Plagborg-Møller and Wolf (2021) recommend an alternative procedure for estimating impulse response functions using an external instrument, which they call the “internal instrument” approach. Instead of estimating a standard proxy-SVAR or LP-IV regression, they recommend including the instrument in the VAR, ordering it first, and using a recursive (Cholesky) ordering to estimate its effects. Intuitively, this allows the other variables in the VAR to respond to the instrument on impact, and the dynamics are asymptotically the same as a conventional VAR or (in population, and for infinite lag length) LP-IV estimation.

Here we revisit the estimates of Plagborg-Møller and Wolf using our new instrument series, based on monetary policy surprises around both FOMC announcements and Fed chair speeches. Because our high-frequency surprise data run from 1988:1 to 2019:12 and are included in the VAR, the sample for the estimation is 1988:1–2019:12. As in our other SVARs and LP-IV regressions, we include 12 monthly lags in the VAR and normalize the monetary policy shock to have an impact effect of 25 bp on the 2-year Treasury yield.

The results are shown in figure 6. Overall, they are quite similar to our proxy-SVAR results in figure 4, but they are less precisely estimated due to the shorter sample and larger number of parameters (because the



**Fig. 6.** Recursive structural VAR with internal instrument. Structural vector autoregression (SVAR) impulse response functions to a 25-basis-point monetary policy shock (MPS), identified in the left column using raw high-frequency mps measure around Federal Open Market Committee (FOMC) announcements and speeches by the Fed chair, and in the right column using high-frequency mps around FOMC announcements and Fed chair speeches orthogonalized with respect to economic news available prior to the announcement. Instrument is ordered first in a recursive SVAR, following the methodology of Plagborg-Møller and Wolf (2021). Sample: 1988:1–2019:12. Shaded regions report bootstrapped 90% standard-error bands. EBP = excess bond premium, CPI = Consumer Price Index, IP = industrial production. See text for details. A color version of this figure is available online.

coefficients on the lags of  $z_t$  must be estimated). Recall from our estimates across different subsamples in figure 2, starting the estimation in 1988 instead of 1973 does not substantially affect the point estimates, but it does noticeably reduce the precision. Comparing the left and right columns of figure 6, we see again that orthogonalizing the monetary policy surprises substantially increases the size of the estimated effects and removes any price puzzle types of responses in the left column.

Figure 6 is also interesting because including the instrument in the VAR automatically orthogonalizes it with respect to lags of all the variables in the VAR. Despite this, the unadjusted mps instrument in the left-hand column does a relatively poor job of estimating the effects of monetary policy on the economy, with estimates that are similar to the left column of figure 4. By contrast, our orthogonalization with respect to the predictors in table 1 seems to do a much better job of removing the econometric endogeneity. Apparently the endogeneity that is present in the mps variable is not well captured by the lags of the variables in the VAR.

As was the case with our previous SVARs in figures 3–4, the VAR structure here seems to improve the quality of our estimates, relative to unrestricted LPs. However, restricting the sample to begin in 1988, when our high-frequency data become available, reduces the precision of the estimated dynamics in figure 6. Based on these findings, an SVAR specification with identification using external instruments, as in Sub-section V.A, seems preferable to a recursive SVAR with an internal instrument.

With respect to invertibility, discussed at length in Stock and Watson (2018) and Wolf (2020), we have found that the Granger-causality test suggested by Plagborg-Møller and Wolf (2022) does not reject the null of invertibility for any of the specifications and instruments that we consider.<sup>55</sup> Overall, lack of invertibility does not seem to be of much concern in this context, and there are good reasons to prefer the SVAR-IV approach for estimation of the dynamic effects of monetary policy with high-frequency identification.

#### *D. Revisiting Miranda-Agrippino and Ricco (2021)*

We now turn to the SVAR analysis of Miranda-Agrippino and Ricco (2021), who orthogonalized monetary policy surprises with respect to the Fed’s internal “Greenbook” forecasts and demonstrated that this leads to substantially different impulse responses to monetary policy

shocks when using the resulting series for high-frequency identification.<sup>56</sup> They interpreted these results as supporting a strong role for a Fed information effect (Romer and Romer 2000; Campbell et al. 2012; Nakamura and Steinsson 2018), given the apparent importance of the Fed's own private forecasts. However, the results in Subsection III.C showed that the Blue Chip survey forecasts, which are publicly available on a monthly basis, have very similar predictive power for monetary policy surprises as the Fed's own Greenbook forecasts, which the public does not see until 5 years after the FOMC meeting. This raises the question of whether orthogonalizing monetary policy surprises with respect to public Blue Chip forecasts—in line with our general approach of orthogonalizing monetary policy surprises with respect to publicly available information—yields results similar to those of Miranda-Agrippino and Ricco. If so, this would raise further doubts about the Fed information effect.

Before going into the details of this analysis, it is helpful to compare, at a high level, the approach of Miranda-Agrippino and Ricco to the one we propose in this paper. Overall, Miranda-Agrippino and Ricco suggest a very similar correction to monetary policy surprises as we do. However, they recommend the use of a different set of predictors and base their approach on a different motivation. Because they document predictability of monetary policy surprises based on the information in Greenbook forecasts, they argue that this predictability is caused by a Fed information effect. They therefore recommend orthogonalizing the policy surprises with respect to the Greenbook forecasts. Our prescription is based on a different premise, and it is also practically simpler in that the data for the orthogonalization are publicly available in real time.

Most of the analysis of Miranda-Agrippino and Ricco closely follows the specification of Gertler and Karadi (2015). The key is a comparison of the impulse responses obtained using the Gertler-Karadi monetary policy surprise instrument, FF4GK, to the results obtained using a new monetary policy instrument, MPI, which Miranda-Agrippino and Ricco construct according to the following three-step approach:

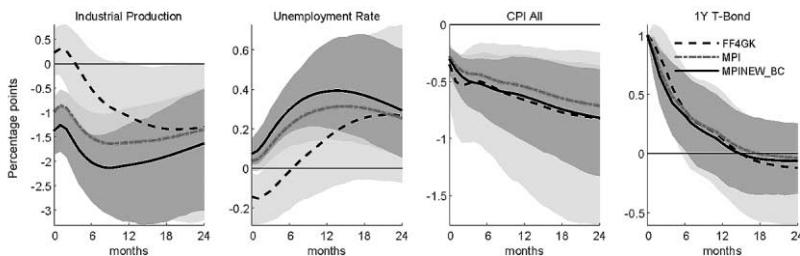
1. Regress the high-frequency announcement surprises FF4 on Greenbook forecasts and forecast revisions for real GDP growth, inflation and unemployment (for details, see Subsec. III.C or Miranda-Agrippino and Ricco's table 1) and calculate the residuals.
2. Aggregate the announcement-frequency residual series to a monthly time series, with zeros for months without monetary policy announcements.

3. Regress these monthly values onto 12 lags and again calculate the residual.<sup>57</sup>

As a result, the Miranda-Agrippino and Ricco monthly instrument series MPI is orthogonal to the Fed's own macroeconomic forecasts and does not exhibit any serial correlation.

We construct an alternative instrument series, MPINEW\_BC, using the same three-step approach, but with the Blue Chip consensus forecasts instead of the Greenbook forecasts in the first step. We use exactly the same policy surprise, sample period, variables, methods, and forecast horizons as Miranda-Agrippino and Ricco. For each FOMC announcement, we regress FF4 on the most recent available Blue Chip forecasts and revisions, as in Subsection III.C. The resulting monthly instrument series is therefore orthogonal to publicly available forecasts but does not take into account any private information that the Fed may possess, which might be contained in the Greenbook forecasts.

Figure 7 is analogous to figure 3 in Miranda-Agrippino and Ricco and shows the responses of IP, the unemployment rate, the CPI, and the 1-year Treasury yield to a 100 bp monetary policy shock. (Thus, the monetary policy shock in fig. 7 is four times larger than in figs. 2–6, for comparability to Miranda-Agrippino and Ricco.) The three different lines correspond to the three different external instruments used to identify the monetary policy shock. The lines for FF4GK and MPI exactly replicate the responses shown in Miranda-Agrippino and Ricco's figure 3.<sup>58</sup> One of



**Fig. 7.** Greenbook versus Blue Chip forecasts in Miranda-Agrippino and Ricco SVARs. Structural vector autoregression impulse response functions to a 100-basis-point monetary policy shock identified using three different external instrument series: the unadjusted Gertler-Karadi instrument (FF4GK), the Miranda-Agrippino and Ricco instrument orthogonalized to Greenbook forecasts (MPI), and a new instrument we construct orthogonalized to Blue Chip rather than Greenbook forecasts (MPINEW\_BC). Specification, sample period, and estimation method are exactly as in figure 3 of Miranda-Agrippino and Ricco (2021). Shaded areas are 95% credibility bands based on the simulated posterior distribution. CPI = Consumer Price Index. A color version of this figure is available online.

their main points was that the response of IP and unemployment are very different for MPI than for the FF4GK instrument. In particular, using MPI they find no output or unemployment puzzle, with strong and significantly negative responses of IP and positive responses of the unemployment rate to a monetary policy tightening.

The third line in figure 7, labeled MPINEW\_BC, shows the same impulse responses but using our new external instrument for identification. Strikingly, the response of IP to a monetary policy shock is at least as negative, and in fact even more negative, as when using MPI. Similarly, the response of the unemployment rate is at least as positive for our instrument as for Miranda-Agrippino and Ricco's instrument.

The results of this exercise suggest that there is nothing special in the Greenbook forecasts, and that the publicly available Blue Chip forecasts contain very similar information about upcoming monetary policy surprises. Thus, there appears to be little to no role for a Fed information effect in explaining the different macroeconomic responses to a policy shock documented by Miranda-Agrippino and Ricco. Instead, their results may well be driven by the "Fed response to news" channel of Bauer and Swanson (2023). What is clear is that their results are due to the correlation between monetary policy surprises and publicly available macroeconomic and financial news predating the FOMC announcement, which we emphasize in this paper.

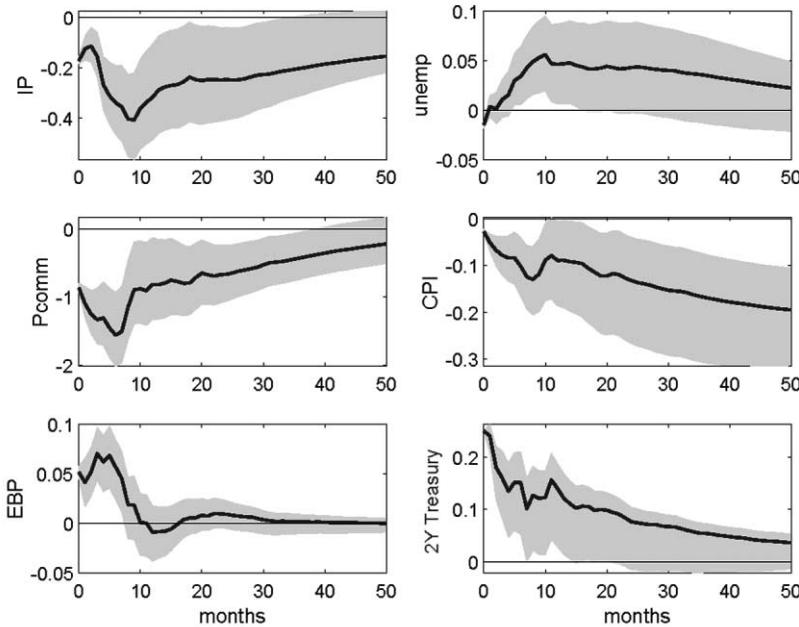
The main point of Miranda-Agrippino and Ricco, however, is that one should not use unadjusted high-frequency surprises as instruments for monetary policy shocks. Our analysis very much supports this conclusion, and we similarly propose to orthogonalize the observed high-frequency surprises to construct better instruments. However, we emphasize that one can use publicly available data to do so, and that there is no need to rely on Greenbook forecasts that are made public only after a lag of 5 years. Although our preferred explanation of the endogeneity of conventional monetary policy surprises differs from that of Miranda-Agrippino and Ricco, because it does not rely on information effects, this is not crucial for the main points we make in this paper.<sup>59</sup>

#### *E. Best Practice Estimates of Monetary Policy's Effects*

We close our empirical analysis of the effects of monetary policy on the macroeconomy with a summary of what we have found to produce the most reliable estimates, and a final set of estimates that incorporate these lessons learned:

- High-frequency monetary policy surprises need to be orthogonalized with respect to macroeconomic and financial data observed before the policy announcements, to avoid estimation bias and create instruments that are more likely to be exogenous.
- Including additional monetary policy announcements, such as speeches by the Fed chair, improves the relevance of the instruments and the precision of the estimates.
- Estimates from SVAR models tend to be more precise and less erratic than those based on LPs, but the two are qualitatively similar.
- Using a longer sample period for estimation of the reduced-form VAR helps improve the precision of the estimates and leads to qualitatively similar results. Although there is a trade-off for using longer samples between improved efficiency and robustness to potential structural breaks, our results in figure 2 suggest that the estimated effects of monetary policy shocks are rather stable across subsamples.
- Including the instrument series in a recursive SVAR does not fix the endogeneity problem and still requires an orthogonalization of the monetary policy surprises with respect to macroeconomic and financial data.
- Invertibility of the SVAR does not seem to be an important concern in this context.
- Including additional variables in the VAR, such as the unemployment rate or commodity prices, makes relatively little difference for the other impulse responses (see, e.g., fig. A1). Nevertheless, the effects of monetary policy on these other variables may be interesting for their own sakes, and hence worth including.

Taking these lessons to heart, we report a benchmark set of impulse response functions in figure 8. These are computed using a structural VAR with external instruments, as in Subsection V.A. Because we do not reject invertibility, there are several reasons to prefer this methodology, including the ability to use longer samples for estimation of the reduced-form dynamics, higher precision, and less erratic estimates.<sup>60</sup> We combine FOMC announcements and Fed chair speeches to construct the monthly monetary policy surprise instrument, and we use the orthogonalized instrument series  $z_t^\perp$ . We estimate the reduced-form VAR over our full sample period from 1973:1 to 2020:2, and we use the instrument series from 1988:1 to 2019:12 to estimate the impact effects of the structural monetary policy shock on the variables of the VAR. Finally, we include the unemployment rate and an index of commodity prices in the



**Fig. 8.** Best practice estimates of structural VAR. Structural vector autoregression impulse response functions to a 25-basis-point monetary policy shock, identified using high-frequency mps measure around Federal Open Market Committee announcements and speeches by the Fed chair orthogonalized with respect to economic news available prior to the announcement. Sample: 1973:1–2020:2. Shaded regions report bootstrapped 90% standard-error bands. EBP = excess bond premium, Pcomm = commodity prices, IP = industrial production, CPI = Consumer Price Index. See text for details. A color version of this figure is available online.

VAR because the responses of these variables are often of interest and have been included by many previous authors, even though all of our other impulse response functions are very similar if unemployment and commodity prices are excluded.<sup>61</sup>

As in our previous estimates, we normalize the monetary policy shock in figure 8 to increase the 2-year Treasury yield 25 bp on impact. After the initial jump, we estimate that the 2-year yield gradually returns to steady state over the next several years (although only the first 4 years are plotted in fig. 8, as in our previous figures). In response to this shock, we estimate that the EBP jumps 5 bp in the impact month and commodity prices fall almost 1%. The EBP rises a bit further over the next 6 months before returning to steady state after about a year, and commodity prices fall further for the first 8 months before gradually returning to steady state over the next 4–5 years.

IP falls almost 0.2% in the impact month and declines further over the next 9 months before turning around and gradually returning to baseline over the next several years. The unemployment rate is essentially unchanged on impact, rises slightly over the next 10 months by about 0.05 percentage points, and then very slowly returns back toward steady state over the next several years. Finally, the CPI response is the most sluggish, dropping 0.05% in the impact month and then gradually decreasing about 0.2% over the next 5 years before very slowly starting to head back toward baseline.

It is interesting to compare the large and rapid response of commodity prices in figure 8 to the sluggish response of the CPI. This difference is consistent with standard medium-scale New Keynesian DSGE models that imply inflation inertia, such as Christiano et al. (2005). If we replace the CPI in the VAR with the core CPI, the core CPI response is even more sluggish.

Overall, the results in figure 8 are consistent with those we presented earlier and consistent with standard macroeconomic models. Our hope is that these may serve as a guideline and benchmark for future estimates.

## VI. Conclusion

This paper investigates the use of high-frequency monetary policy surprises to estimate the effects of monetary policy on financial markets and the real economy. This investigation is necessitated by the emerging consensus in the literature that high-frequency monetary policy surprises are significantly correlated with macroeconomic and financial data that predate the monetary policy announcements. An additional motivation is the concern that these surprises may have become less relevant over time as measures of monetary policy shocks (Ramey 2016).

We confirmed and extended previous evidence on the predictability of high-frequency monetary policy surprises. We also presented substantial evidence—and a simple theoretical model—that suggest this predictability can be attributed to the “Fed response to news” channel of Bauer and Swanson (2023), according to which financial markets simply underestimated how responsive the Fed would be to the economy. Our explanation is a plausible alternative to a “Fed information effect,” according to which the Fed’s monetary policy announcements reveal information about the state of the economy that the private sector did not previously have. We then investigated the consequences of the predictability of monetary policy surprises for empirical work, independent of the precise economic reason for this predictability.

When measuring the effects of monetary policy on financial markets, we found that standard, high-frequency OLS regressions using unadjusted monetary policy surprises produced reliable estimates. This observation follows both from our simple theoretical model and from our empirical reassessment comparing the effects of monetary policy surprises that are unadjusted versus orthogonalized with respect to macroeconomic and financial news that predates the announcement.

However, when estimating the effects of monetary policy on macroeconomic variables using a structural VAR or LPs, we found that unadjusted monetary policy surprises led to estimates that are biased. The bias arises because the macroeconomic data in the VAR are correlated with the monetary policy surprise, so that, for example, a monetary policy tightening is correlated with positive innovations to output and inflation, which attenuates or even reverses the estimated effects of the tightening. In this case, using our orthogonalized high-frequency monetary policy surprises provides us with an instrument that is more likely to be exogenous with respect to the other variables in the VAR and produces impulse response functions that are substantially stronger and devoid of opposite-signed puzzles such as the “price puzzle.”

An additional difficulty of working with high-frequency monetary policy surprises in SVARs and LPs, especially for our orthogonalized monetary policy surprises, is that they can have low explanatory power for monthly changes in interest rates. In other words, even though our orthogonalized monetary policy surprise instrument is exogenous, it may not be very relevant, a concern that has also been expressed by Ramey (2016). We addressed this concern by bringing to bear additional monetary policy surprise data in the form of speeches, press conferences, and congressional testimony by the Federal Reserve chair. Using this larger set of monetary policy surprises avoids potential weak-instrument problems and still confirms the general pattern of the effects of monetary policy on the economy.

Our results also have important implications for central bank communication and the conduct of monetary policy. First, along with Bauer and Swanson (2023), we find little or no evidence that FOMC announcements have a substantial “Fed information effect” component. Although the minutes of recent FOMC meetings reveal that some participants worried about the potential for counterproductive information effects,<sup>62</sup> our results indicate that policy makers have little need to fear that information effects might attenuate the effects of their announcements, except possibly in exceptional circumstances (which our results cannot rule out).

Second, our estimates of the effects of monetary policy on financial markets confirm previous estimates in the literature, despite the fact that those monetary policy surprises are correlated with economic and financial data that predate the FOMC announcement.

Third, our estimates of the effects of monetary policy on the macroeconomy are stronger than many previous high-frequency-based estimates, because our orthogonalization of the high-frequency monetary policy surprises removes an estimation bias that was present in those studies. Thus, like Coibion (2012), we estimate larger effects of monetary policy on real activity and inflation.

Going forward, our results suggest several avenues for future research. The predictability—or rather, *ex post* correlation—of high-frequency monetary policy surprises with macroeconomic and financial data certainly deserves further investigation, extending the analysis to other central banks, additional predictors, and decompositions of monetary policy surprises into changes in risk premia and short-rate expectations. Explicitly incorporating empirical monetary policy rules into this analysis would also be valuable to learn more about the exact sources of this predictability. Regarding information effects, our empirical evidence here and in Bauer and Swanson (2023) suggest that they are unlikely to be strong on average, but it does not rule out that some exceptional FOMC announcements convey information about the economic outlook. Further research is needed to understand when this channel may be relevant, and recent work by Cieslak and Pang (2021) using comovement of asset prices is an important step in that direction. Regarding the macroeconomic effects of monetary policy, our analysis has documented large impulse responses to monetary policy shocks but leaves open the question of what our improved identification implies for the overall quantitative importance of monetary policy for business-cycle fluctuations. Future research could combine our identification strategy and methods for historical and variance decompositions, including methods recently developed by Plagborg-Møller and Wolf (2022), to address this important question. Finally, our SVAR analysis focused on policy surprises that shift the current target rate and expected policy path, but it did not consider the effects of forward guidance separately or of balance-sheet policies such as quantitative easing. Based on the lessons in this paper, methods for high-frequency identification may be combined with unconventional monetary policy surprises, such as those measured by Swanson (2021), to yield new insights in this area.

## Appendix

### Recursively Estimated Monetary Policy Rule

We estimate the following monetary policy rule:

$$i_t = r_t^* + \pi_t^* + \beta_t(\pi_t - \pi_t^*) + \gamma_t(y_t - y_t^*) + u_t,$$

according to which the Fed reacts to year-over-year core Personal Consumption Expenditures inflation,  $\pi_t$ , and the output gap,  $y_t - y_t^*$ . The dependent variable,  $i_t$ , is the 2-year Treasury yield, which we use instead of the federal funds rate to somewhat alleviate the effects of the zero lower bound. All data series are from FRED, including the Congressional Budget Office's estimates of potential GDP ( $y_t^*$ ). Our data are monthly from June 1976 to July 2021, and we linearly interpolate the quarterly output gap series.<sup>63</sup> We estimate the response coefficients  $\beta_t$  and  $\gamma_t$ , as well as the combined intercept  $r_t^* + (1 - \beta_t)\pi_t^*$ , using exponentially weighted least squares and an expanding estimation window.<sup>64</sup> The forgetting factor is set to  $\nu = 0.005$ , which implies an effective sample size of 200 months. That is, estimation at time  $t$  uses data from the beginning of the sample to time  $t$ , and the weights for data at  $t - j$  are proportional to  $(1 - \nu)^j$ . We begin our estimation in January 1990 and estimate the parameters for each month until July 2021. We obtain Newey-West standard errors using 12 lags to construct 95% confidence intervals.

Figure 1 plots the estimated response parameters  $\hat{\beta}_t$  and  $\hat{\gamma}_t$  and confidence intervals. An upward trend is clearly present in both estimated series. The inflation coefficient starts out slightly below 1 but increases quickly, satisfying the "Taylor principle" ( $\beta_t > 1$ ) for most of the sample, and reaches its peak of about 1.8 near the end of the sample. The output gap coefficient is close to zero and statistically insignificant for most of the first 20 years of our sample period, and increases toward a peak around 0.6 in 2017, before declining somewhat toward the end of the sample. In both series, the estimates over the past decade are substantially higher than the earlier estimates. In sum, this evidence supports the view that the Fed has become more responsive to economic conditions, including both inflation and real activity.

### Structural VAR Robustness

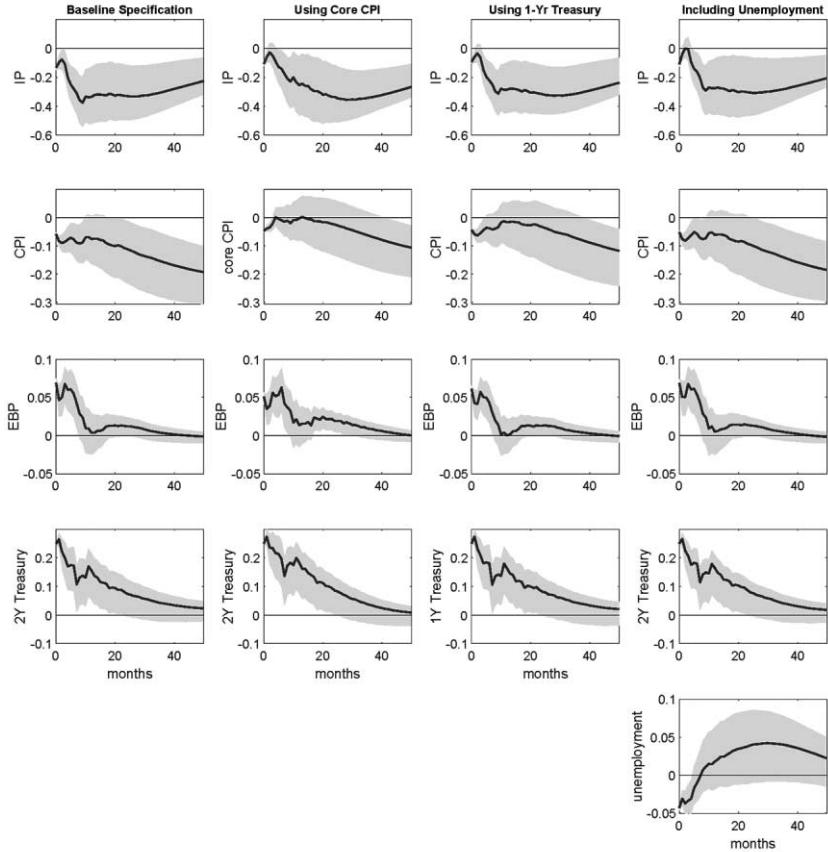
This section demonstrates the robustness of the results from our baseline structural VAR specification presented in the main text.

In figure A1, we present results from four variations of our baseline specification. The first column of the figure repeats the results from our baseline specification over our full sample, 1973:1–2020:2, and using the unadjusted monetary policy surprise measure  $mps$  around FOMC announcements as our high-frequency instrument, because that corresponds most closely to the instrument used by previous authors. The results in the first column of figure A1 thus are the same as in column  $a$  of figure 2 and the left-hand column of figure 3. In the second column of figure A1, we repeat the analysis using the core CPI instead of the headline CPI; in the third column, we repeat the analysis using the 1-year Treasury yield instead of the 2-year Treasury yield; and in the fourth column, we repeat the analysis including the unemployment rate as a fifth variable in the specification, as is sometimes done in the literature (e.g., Ramey 2016).

As can be seen in figure A1, the impulse response functions are very similar across all of these specifications. The different specifications also generally yield differences in the first-stage  $F$ -statistics for the regression of the reduced-form residual  $u_t^{2y}$  on the high-frequency monetary policy instrument,  $z_t$ . In the first column, the first-stage  $F$ -statistic is 8.19, in the second column 7.92, in the third column 13.12, and in the fourth column 8.12. Note that the higher first-stage  $F$ -statistic in the third column was exactly why Gertler and Karadi (2015) used that specification as their baseline. Nevertheless, Gertler and Karadi found that their estimated SVAR results were very similar using the 2-year Treasury yield instead of the 1-year yield, which we likewise find in figure A1.

### Local Projections

Figure A2 reports estimated impulse response functions using the LP-IV specification (eq. [23]) with the high-frequency monetary policy instrument around FOMC announcements each month as the external instrument (excluding speeches by the Fed chair). The impulse response functions in figure A2 are larger than in figures 3–6, but the standard errors are also much larger, so we would not reject these other estimates. Ramey (2016) suggests that the later sample period may be partly responsible for the difference between the LP-IV and VAR results, but our results in figure 2 suggest that the different sample period is not a major issue. The impulse response functions for IP in particular in figure A2 are very large, especially for the orthogonalized  $mps$  instrument, although the standard errors are correspondingly large. It is likely that part of the problem here is that the orthogonalized surprises  $z_t^\perp$  are a weak instrument—recall that the first-stage  $F$ -statistic for this instrument is only

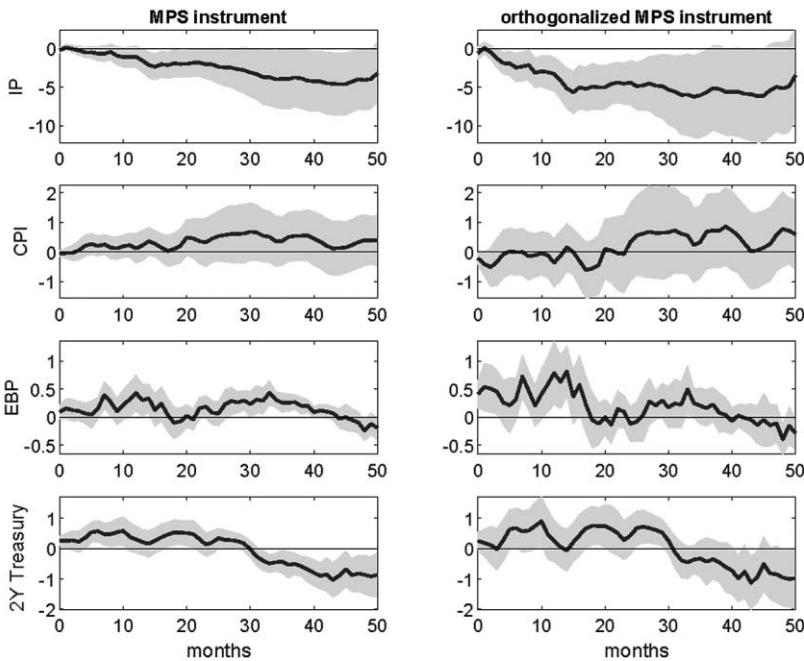


**Fig. A1.** Structural VAR impulse responses for four specification variations, using unadjusted monetary policy surprises around FOMC announcements. Structural vector auto-regression impulse responses to a 25-basis-point monetary policy shock (MPS), identified using the unadjusted high-frequency mps measure around Federal Open Market Committee (FOMC) announcements, for four different specifications. The baseline specification includes the log of industrial production (IP), log of the Consumer Price Index (CPI), Gilchrist and Zakrajsek (2012) excess bond premium (EPB), and the 2-year Treasury yield. Sample: 1973:1–2020:2. Shaded regions report bootstrapped 90% standard-error bands. See text for details. A color version of this figure is available online.

1.83. Overall, the results in figure A2 are imprecise and should be treated very cautiously.

### Miranda-Agrippino and Ricco (2021)

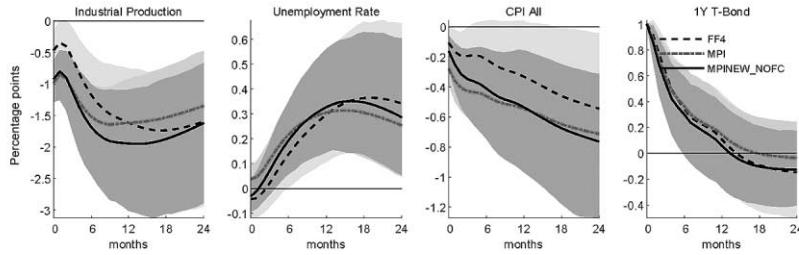
We noticed two issues in our reassessment of the results in Miranda-Agrippino and Ricco (2021) that are only tangentially related to our



**Fig. A2.** Local projections impulse responses, identified using raw versus orthogonalized monetary policy surprises around FOMC announcements. Local projections impulse response functions to a 25-basis-point monetary policy shock (MPS), identified in the left column using the unadjusted high-frequency mps measure around Federal Open Market Committee (FOMC) announcements, and in the right column using high-frequency change in mps around FOMC announcements orthogonalized with respect to economic news available prior to the announcement. Sample: 1988:1–2020:2. Shaded regions report 90% standard-error bands. EBP = excess bond premium, CPI = Consumer Price Index, IP = industrial production. See text for details. A color version of this figure is available online.

main points but which are helpful for interpreting the results in their paper and in ours.

First, it is important to consider the properties of the unadjusted monetary policy surprises. As also noted by Ramey (2016), the Gertler-Karadi version of FF4, which is a 30-day moving average of the underlying high-frequency FF4 surprises, introduces serial correlation into the resulting series FF4GK. As a result, using FF4 or FF4GK leads to quite different results. In particular, impulse responses obtained using FF4 are more similar to those obtained using MPI in figure 7. Figure A3 shows that results for FF4 are more similar to results for MPI than the results for FF4GK are. That is, the orthogonalization of high-frequency surprises with respect to



**Fig. A3.** Additional results for Miranda-Agrippino and Ricco. Structural vector autoregression impulse response functions to a monetary policy shock identified with three different external instrument series: raw FF4 series, Miranda-Agrippino and Ricco instruments using Greenbook forecasts (MPI), and a new instrument series that does not orthogonalize FF4 with respect to macroeconomic forecasts, and only removes serial correlation (MPINEW\_NOFC). Specification, sample period, and estimation method are exactly as in figure 3 of Miranda-Agrippino and Ricco (2021). Shaded areas are 95% credibility bands based on the simulated posterior distribution. CPI = Consumer Price Index. A color version of this figure is available online.

macro forecasts and the removal of serial correlation actually makes a smaller difference for the SVAR results than it initially appeared. By contrast, our results in Subsections V.A–V.C showed that simple orthogonalization of the surprises with respect to macroeconomic and financial data makes a very substantial difference for the resulting impulse responses.

Second, we have also found that an instrument series that does not use any information in macroeconomic forecasts but only removes serial correlation leads to results not too different from those obtained using MPI or MPINEW\_BC. This is evident in figure A3, which shows results for an instrument series MPINEW\_NOFC obtained in exactly the same way as MPI except for the fact that we did not orthogonalize the surprises with respect to Greenbook forecasts. The similarity of the impulse response functions (IRFs) for MPI and for MPINEW\_NOFC suggests that orthogonalizing with respect to macro forecasts has a very modest impact on the resulting estimates.

Overall, it appears that most of the differences in the impulse responses shown in figure 7—between those for FF4GK on the one hand, and those for MPI and MPINEW\_BC on the other hand—appear to be due to the serial correlation in the FF4GK series.

## Endnotes

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1. Although there is *ex post* correlation between the policy surprises and economic variables predating the announcements, the monetary policy surprises were in fact unpredictable *ex ante* by financial market participants, according to this explanation. Imperfect information can lead to full-sample, *ex post* predictability even without any *ex ante* predictability (e.g., Timmermann 1993).

2. Bauer and Swanson (2023) show that controlling for the Fed response to news channel—by controlling for these macroeconomic and financial variables—eliminates the “Fed information effect” puzzle in survey regressions documented by Campbell et al. (2012) and Nakamura and Steinsson (2018).

3. See Romer and Romer (2000), Campbell et al. (2012), Nakamura and Steinsson (2018), and Bauer and Swanson (2023) for extensive discussions and evidence for and against the Fed information effect.

4. Using an alternative, more model-based approach, Sastry (2021) similarly concludes that there is little or no evidence of a Fed information effect in the data.

5. Although  $mps_t$  would also be correlated with  $x_t$  if, on average,  $\alpha_t < a_t$ , the resulting negative correlation would be at odds with the procyclical correlations we document in Sec. III.

6. See, for example, the July 2021 Monetary Policy Report, available at <https://www.federalreserve.gov/monetarypolicy/2021-07-mpr-part2.htm>.

7. Changes in the Fed's preferences over economic outcomes or in the biases of its own forecasts could also have caused monetary policy to become more responsive to the economy. For example, Lakdawala (2016) documents changes in the Fed's preferences, and Capistrán (2008) found that the Fed underpredicted inflation before Volcker and then overpredicted inflation, which would be consistent with a shifting asymmetric loss function.

8. See also the online appendix to Bauer and Swanson (2023), which provides related evidence on the predictability of Fed funds rate survey forecast errors.

9. For high-frequency asset price regressions such as eq. (13), orthogonalizing the monetary policy surprises  $mps_t$  and isolating the component due to  $\varepsilon_t$  is not necessary and may actually reduce the efficiency of the regression estimates. The reason is that, according to our model, yield changes are related to the full monetary policy surprise  $mps_t$ , and not just the exogenous component  $\varepsilon_t$ .

10. Throughout our paper, we use the term “monetary policy surprises” to denote high-frequency interest rate changes around FOMC announcements. Given the predictability of these changes, it may seem odd to speak of “surprises.” However, this is standard terminology in the literature, so we stick with it. In addition, our simple model in Sec. II is consistent with the view that these surprises are unpredictable *ex ante* and that the predictability is due to imperfect information on the part of the private sector, which leads to correlation between the economy and the monetary policy surprises *ex post*.

11. From 1994 to May 1999, the absence of such a press release at 2:15 p.m. following an FOMC meeting indicated to the markets that there was no change in the federal funds rate target. Beginning in May 1999, the FOMC began issuing explicit press releases in those cases as well. See Swanson (2006).

12. Note that in the early years of the sample, 1988–90, changes in the federal funds rate were more frequent and there were several cases where the FOMC's decision was not immediately obvious to markets after just one open market operation. In those cases, there can effectively be two or three announcements in a row, corresponding to the consecutive days of open market operations, which gradually clarified the Fed's position to the markets. See Swanson and Jayawickrema (2021) for details.

13. Some authors have also used other measures—see Gürkaynak, Sack, and Swanson (2007) for examples.

14. Federal funds futures are also often included in the construction of monetary policy surprises but are not available in Tick Data until 2010. Gürkaynak, Sack, and Swanson (2007) show that Eurodollar futures are the best predictor of future values of the federal funds rate at horizons beyond 6 months and are virtually as good as federal funds futures at horizons less than 6 months.

15. Prior to each major macroeconomic data release, Money Market Services conducted a survey of financial market participants to determine the market expectation for the release. The survey was continued by Action Economics and is now owned by Haver Analytics. See Bauer and Swanson (2023) for additional details. The units are in thousands of workers, and the surprise is typically around 100; we divide these values by 1,000 to make the scale similar to the other predictors in our analysis.

16. Ramey (2016) and Miranda-Agricoppino and Ricco (2021) also use FF4 as their primary measure of the monetary policy surprise, for comparability to Gertler and Karadi (2015). Gertler and Karadi also take a 30-day moving average of the high-frequency monetary policy surprises to create their high-frequency external instrument; we do not do that here because, as Ramey (2016) points out, the 30-day moving average induces extra serial correlation and predictability in those surprises that is not present in the underlying high-frequency changes in FF4 itself.

17. Note, however, that investors at the time had neither the same macro data as we do nor the same conceptual understanding of monetary policy surprises, which would have put them at an even bigger disadvantage.

18. The Blue Chip consensus forecasts are from the Blue Chip Economic Indicators survey and correspond to the arithmetic mean of the individual forecasts. The Blue Chip Economic Indicators forecasts, which we use in this analysis, are usually released on the tenth day of the month; we take the tenth day of the month as the date that the forecasts are publicly available. In recent years, the Blue Chip consensus forecast data do not include observations for the previous quarter when the macroeconomic data have already been released. In those cases, we add real-time data from ALFRED; see <https://alfred.stlouisfed.org>. The Greenbook forecasts are publicly released with a 5-year lag and are obtained from the database maintained by the Philadelphia Fed at <https://www.philadelphiafed.org/surveys-and-data/real-time-data-research/philadelphia-data-set>.

19. One way of seeing this is to note that high-frequency interest rate changes are essentially identical to negative excess returns on the underlying security, because over the very short holding period there is no material change in maturity or risk-free return. Excess returns are unpredictable when conditions (a) and (b) are satisfied. Schmeling et al. (2022) provide a recent discussion.

20. As discussed later, Cieslak (2018) also shows that the forecast errors for the federal funds rate in the Blue Chip survey of professional forecasters are also strongly predictable with the same variables that predict the market's forecast errors, implying that risk premia cannot be the whole story.

21. Prominent examples are Greenwood and Shleifer (2014) and Coibion and Gorodnichenko (2015).

22. Bauer and Chernov (2023) show related evidence, using conditional Treasury skewness and the shape of the yield curve as predictors for funds rate forecast errors.

23. Another possible explanation of our predictability results is the heterogeneous use of common information, as argued by Sastry (2021).

24. Event studies have been used to study the effects of both conventional monetary policy (e.g., Bernanke and Kuttner 2005; Gürkaynak et al. 2005; Bauer 2015; Hanson and Stein 2015; Nakamura and Steinsson 2018) and unconventional monetary policy such as forward guidance and large-scale asset purchases (LSAP) (e.g., Gürkaynak et al. 2005; Gagnon et al. 2011; Swanson 2011; Bauer and Neely 2014; Bauer and Rudebusch 2014; Swanson 2021). Work on unconventional monetary policy is surveyed by Kuttner (2018).

25. This assumption is possibly more problematic with daily data. However, Cook and Hahn (1989) argue that it is likely to be satisfied even with daily data, and even before the FOMC released policy statements at predetermined times (i.e., even before 1994).

26. In addition, our narrow intraday announcement windows keep the amount of other news about the economy that is released during these times to a minimum.

27. See also Kuttner (2018) for a discussion of this assumption in the context of LSAP event studies.

28. An example would be a more positive assessment of the current economic outlook by the central bank than by the public, and a hawkish policy surprise,  $mps_t > 0$ , as a result. Such an information effect might raise forecasts for output, inflation, and dividends, whereas a contractionary policy shock would lower them.

29. These 30-minute windows are the same as for the monetary policy surprise. The data source is Tick Data. In all cases, we use the current-quarter futures contract, which has the highest liquidity. Data for the 2-year Treasury note contract begin in January 1991 and those for the 5-year Treasury note contract begin in July 1988, so for these two Treasury yields some FOMC announcements are missing from our regressions. Changes in futures prices are converted to changes in yields using the duration of the notional underlying security obtained from Bloomberg. For the S&P 500, we use the S&P 500 futures changes up to August 1997 and switch to the e-mini S&P 500 futures changes from September 1997 onward, due to the e-mini futures having higher liquidity and longer trading hours. For additional details, see Swanson and Jayawickrema (2021).

30. Recall from Sec. III that the monetary policy surprise is normalized to move the ED4 futures rate one-for-one.

31. For example, the Fed chair has often been called on by Congress to testify about bank regulation, fiscal policy, Treasury debt policy, Social Security, Government-Sponsored Enterprises, the exchange rate, and other economic issues of national significance.

32. Although the monetary policy report testimonies are semiannual, they are given to each house of Congress, with extensive question-and-answer sessions each day. This results in a total of four of these testimonies per year.

33. Although this methodology necessarily involves some degree of personal judgment, most of the time it is quite clear from the market commentary whether the Fed chair's speech was interpreted as containing news about the likely path of monetary policy.

34. Because speeches, testimony, and press conferences take time, often an hour or more, Swanson and Jayawickrema (2021) do not use 30-minute windows for them, but instead use wider intradaily windows that are tailored to the length of the speech or testimony, typically about 90 minutes for a speech or press conference and 210 minutes for testimony. In addition, if there is a macroeconomic data release that occurs during one of these windows, they adjust the window start and end times to exclude the effects of the macro data release. See Swanson and Jayawickrema (2021) for details.

35. As discussed above, we minimized this contamination as much as possible by excluding macroeconomic data releases from our Fed chair speech event windows and by reading *The Wall Street Journal* and *New York Times* market commentary to determine whether the chair's speech was the main news moving markets, but there could always be some remaining effects of macroeconomic news in these windows.

36. A strong correlation of yield changes with the policy surprise could still be observed because interest rate changes across maturities are generally very highly correlated, and the "policy surprise" is just a measure of changes in short-term interest rates. In fact, the correlation of yield changes across maturities is even stronger for other types of news than for monetary policy news, as the latter is inherently multidimensional (Bauer 2015). The muted stock market response could be explained by the fact that the bond-stock correlation depends on the types of news.

37. Gertler and Karadi (2015) also used the month-average Treasury yield in their analysis; we use the end-of-month values. The end-of-month value corresponds more naturally to our high-frequency monetary policy surprise instrument; because Gertler and Karadi use the month-average Treasury yield, they also take a 30-day moving average of their high-frequency monetary policy surprise instrument. This 30-day moving average creates extra serial correlation and predictability in their instrument, which leads to concerns about the instrument's validity, as discussed by Ramey (2016). Nevertheless, our results shown later are all very similar whether we use the 1- or 2-year Treasury yield or the end-of-month or month-average yield in our analysis.

38. See Stock and Watson (2018) and Plagborg-Møller and Wolf (2021) for discussions of invertibility.

39. This generalization allows for a certain type of noninvertibility, but we still rule out the most common type of noninvertibility: that lagged structural shocks affect current reduced-form innovations (Wolf 2020).

40. LP estimation of impulse response functions, which we also consider later, requires an additional *lead-lag exogeneity* condition,  $E[z_t \varepsilon_{t+j}] = 0 \ \forall j \neq 0$  (Stock and Watson 2018). In an SVAR framework, eqs. (17)–(18) and the serial independence of the  $\varepsilon_t$  make this condition unnecessary.

41. Of course, this correlation is not perfect, and  $z_t \neq \varepsilon_t^{\text{mp}}$ , because not all of the information about the policy shock is released in FOMC announcements and Fed chair speeches. For example, speeches by other FOMC members, minutes of FOMC meetings, interviews, and so on also contain important information about the course of monetary policy.

42. For lead-lag exogeneity, discussed in endnote 40, previous studies have typically assumed that monetary policy surprises are uncorrelated with all information that predates the FOMC announcement; thus, it is natural to view the lead-lag exogeneity condition as being satisfied for  $j < 0$ , and the case  $j > 0$  holds due to the standard VAR assumption that the shocks  $\varepsilon_{t+j}$  are exogenous.

43. The nonfarm payrolls surprise in table 1 is also plausibly correlated with  $\varepsilon_t^{\text{mp}}$ . Even though the released data describe month  $t - 1$ , the surprise is realized in month  $t$ , and a VAR which recognized this information structure would classify the surprise as an information shock in month  $t$ . In addition, the lead-lag exogeneity condition in endnote 40 is violated if  $z_t$  is correlated with macroeconomic or monetary policy shocks from previous months, which is the case for all of the macroeconomic and financial market predictors in table 1.

44. Note that this endogeneity bias could create the illusion of a “Fed information effect” (Romer and Romer 2000; Campbell et al. 2012; Nakamura and Steinsson 2018) even if there is no such information effect in the data, a point emphasized by Bauer and Swanson (2023).

45. If a month contains more than one FOMC announcement, we use the values of the predictors for the first announcement that month.

46. One can obtain the same point estimates for  $s_1$  by regressing the reduced-form residuals  $u_t$  from eq. (17) on  $u_t^{2y}$  using  $z_t$  or  $z_t^\perp$  as the instrument. Stock and Watson (2018) recommend using specification (eq. [22]) instead to avoid a generated regressor and correctly estimate the standard errors.

47. We compute these standard-error bands using the wild bootstrap procedure of Mertens and Ravn (2013) and Gertler and Karadi (2015). This method accounts for the uncertainty both in the estimated impact effect vector  $s_1$  and in the reduced-form VAR coefficient matrices  $B(L)$ .

48. Where these specification changes make the most difference is in the first-stage  $F$ -statistics for the two-stage least squares regression. In general, the specification chosen by Gertler and Karadi (2015) (headline CPI, month-average 1-year Treasury yield, no unemployment in the VAR) helps to maximize the first-stage  $F$ -statistic. This is a problem that we generally do not have to worry about, because our data set includes Fed chair speeches as well as FOMC announcements, substantially increasing our first-stage  $F$ -statistics and helping to avoid a weak-instrument problem.

49. Similarly, good economic news about output or the EBP typically causes the Fed to raise interest rates and the EBP to fall; this is again opposite from the standard effects of monetary policy on the EBP and leads to a downward bias of the EBP response in the left column as well.

50. There are two reasons for the larger impulse response functions obtained using the orthogonalized policy surprise. Recall that the only difference between the two columns of fig. 3 is the instrument, and thus our estimate of the impact vector  $s_1$ ; the reduced-form dynamics  $B(L)$  are the same in both columns. The estimation procedure for  $s_1$ , described previously, amounts to a regression of the reduced-form residuals  $u_t$  on the instrument  $z_t$ , with the results scaled so that the last element of  $s_1$  equals 0.25 (the impact effect on the 2-year yield). The first reason for the larger macro effects in the right column of fig. 3 is that the orthogonalized policy surprise has a larger impact effect on log IP, the log

CPI, and the EBP, because we eliminate bias arising from an endogenous response of monetary policy. The second reason is that the orthogonalized policy surprise has a slightly smaller impact effect on the 2-year yield, and the normalization of that effect further increases the other elements of  $s_1$ .

51. Note that most SVARs use the federal funds rate as their measure of monetary policy, and Gertler and Karadi (2015) and the present paper use the 1-year or 2-year Treasury yield. Estimates in Gürkaynak et al. (2005) imply that a 100 bp change in the federal funds rate corresponds approximately to a 50 bp or 45 bp effect on the 1-year or 2-year yield, respectively. Thus, when comparing SVAR estimates, this difference in scaling must be kept in mind: the estimated effects of a 25 bp increase in the 1- or 2-year Treasury yield are comparable to the effects of a 50 bp or 55 bp increase in the federal funds rate.

52. We compute these first-stage  $F$ -statistics as the squared  $t$ -statistic of the instrument in the regression of  $Y_t^{2y}$  on a constant, the 12 lags of  $Y_t$ , and the instrument, using Huber-White heteroskedasticity-consistent standard errors.

53. Recall that the first-stage  $F$ -statistics for the instrumental variable when we include Fed chair speeches are much higher (30.44 and 12.37) than when the chair's speeches are excluded (8.19 and 1.83), so it is not too surprising that the estimates in fig. 5 are more precisely estimated than in fig. A2.

54. See also Ramey (2022). Ramey also suggested part of the difference between her LP-IV and SVAR results was due to the later sample period for the former, but our results in fig. 2 suggest that the different sample period is not a major issue.

55. Other previously reported results of invertibility tests in the Gertler-Karadi setting are consistent with our findings: Stock and Watson (2018) used an alternative test based on impulse response function (IRF) differences between LP-IV and SVAR-IV and did not reject invertibility. Plagborg-Møller and Wolf (2022) applied their Granger-causality test in that same empirical setting, and only rejected the null when the policy instrument was taken to be the federal funds rate; in the baseline Gertler-Karadi setting with the 1-year yield they did not reject invertibility either. We thank Christian Wolf for suggesting this line of inquiry.

56. Relatedly, Lakdawala (2019) orthogonalizes monetary policy surprises with respect to the difference between Greenbook and Blue Chip forecasts.

57. Only observations with a nonzero dependent variable are used in the regression. That is, zeros in the monthly time series are not affected by this step.

58. We are grateful for excellent replication code that the authors made available via the journal's website; see <https://www.openicpsr.org/openicpsr/project/116841/version/V1/view>.

59. Our analysis of the Miranda-Agrippino and Ricco (2021) monetary policy instruments and results yielded some additional insights about different high-frequency surprises that are somewhat tangential to our main points; see app. D.

60. Li, Plagborg-Møller, and Wolf (2022) use simulation studies to show the advantages of SVAR-IV in the presence of invertibility and even mild noninvertibility.

61. Since Sims (1992), commodity price series have often been included in VARs to avoid a price puzzle. We emphasize that even without commodity prices, our VAR estimates do not exhibit a price puzzle, as long as orthogonalized monetary policy surprises are used as instruments for monetary policy shocks (see, e.g., figs. 3 and 4). The Bloomberg spot commodity price index is not available back to 1973, so we use the log of the Commodity Research Bureau's monthly index of commodity prices, downloaded from Bloomberg.

62. For example, in the minutes of the FOMC meeting on March 15, 2020, participants were concerned that a strong monetary easing surprise "ran the risk of sending an overly negative signal about the economic outlook." See <https://www.federalreserve.gov/monetarypolicy/fomcminutes20200315.htm>.

63. We use the fully revised output gap series due to the difficulties in constructing a long and consistent real-time output gap series. Although revisions to the output gap may affect estimated policy rules (Orphanides 2001), they are unlikely to affect our overall result.

64. Exponentially weighted least squares is equivalent to constant-gain recursive least squares.

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