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A unified measure of Fed monetary policy shocks[☆]

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ABSTRACT

We develop a U.S. monetary policy shock series that stably bridges periods of conventional and unconventional policymaking, is largely unpredictable, and contains no significant central bank information effect. We attribute differences between our measure and often-used alternatives to our econometric procedure, a partial least squares approach, and our using the full maturity spectrum of interest rates in estimating the shock. We find that shocks to our monetary policy series have particularly large effects on maturities in the middle of the term structure and produce conventionally-signed impulse responses of output and inflation.

1. Introduction

The adoption of unconventional monetary policy tools by the Federal Reserve in the wake of the Great Financial Crisis (GFC) brought policymaking into new territory. The Fed's forceful response to rapidly deteriorating conditions due to coronavirus, including a 100 basis point interest rate cut and resumption of large scale asset purchases (LSAPs), marks a return to reliance on unconventional tools with rates at the effective lower bound. These policy moves have rekindled challenges for empirical work on measuring monetary policy shocks and estimating their effects on financial markets and the macro economy.

In this paper, we develop a new measure of Fed monetary policy shocks with three appealing features. First, our measure stably bridges periods of conventional and unconventional policymaking. Second, our estimation approach has very mild data requirements. Compared to the path-breaking work of Romer and Romer (2004), our method involves no need to parse through Federal Reserve transcripts and forecasts. Nor does it require intra-daily data, which is costly to acquire and can have spotty coverage. Thus, our method can be implemented over longer sample periods and for more countries, for which data requirements often render existing procedures untenable.[1] Third, our series exhibits important differences with alternatives, as our shock series is (1) largely unpredictable from available information on the economy, and (2) contains

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[1] As evidence, we construct and analyze a new shock series for ECB monetary policy in online Appendix D.

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no significant central bank information effect. This permits cleaner inference on the transmission of exogenous shocks to monetary policy.

The general idea behind construction of our measure is to use Fama and MacBeth (1973) two-step regressions to estimate the unobservable monetary policy shock. The method works initially through the sensitivity of "outcome variables" to FOMC announcements. In the first step, we run time-series regressions to estimate the sensitivity of interest rates at different maturities to FOMC announcements. This is equivalent to the asset beta in the original Fama-MacBeth method. In order to filter out non-monetary policy news, we also employ the heteroskedasticity-based estimator of Rigobon (2003) and Rigobon and Sack (2004), implemented with instrumental variables (IV), into this step. In the second step, we regress all outcome variables onto the corresponding estimated sensitivity index from step one, for each time t. In this way, we derive the new monetary policy shock as the series of estimated coefficients from the Fama-MacBeth style second step regressions. The application of this partial least squares (PLS) procedure to estimating monetary policy shocks is novel as far as we are aware.[2]

In order to explain how our methodology gives rise to the appealing features of our shock series noted above, we begin with a prototype econometric example in which we consider one short-term interest rate and two long rates. All three may be influenced by both the true monetary policy shock and the information effect. We show that our PLS procedure eliminates the information effect if the short rate and long rates are affected differently by the information effect, as in Hansen et al. (2019), or if long rates are unaffected by the information effect. Following that, we discuss the importance of our choice in data. Whereas alternative measures are constructed from only short rates, we use the entire yield curve. This is important because we find that the Fed information effect is essentially non-existent in maturities of five years and longer, thus supporting the latter condition from our simple conceptual example.[3]

Related literature

Much of the post-GFC empirical research on monetary policy built on influential work that used bond market data at daily or intra-daily frequencies.[4]. This was a departure from traditional approaches using orthogonalized Fed Funds rate innovations in recursive VARs (Christiano et al., 1996) or the narrative approach of Romer and Romer (2004). An advantage of the newer papers is that, under certain assumptions, the resulting shock series captures both conventional policymaking, through shocks to the target Fed Funds rate, as well as unconventional policymaking, as reflected in identified shocks to forward guidance (FG) and LSAPs. The use of narrow time windows around FOMC announcements enhances identification, it is argued, because no other economic news is (routinely) released. Monetary policy surprises are measured as the change in interest rate futures in narrow windows around FOMC announcements.

However, papers on the central bank private information effect have called into question this measurement of monetary policy shocks.[5] Under this view, the central bank reveals in its meeting day announcements not only pure monetary policy news but also private information on the economy, its preferences, or the model it uses to analyze the economy. This in turn causes the private sector to change its outlook for macroeconomic developments. Thus, monetary policy surprises, even those measured in tight windows around FOMC announcements, may be correlated with developments in non-monetary policy economic fundamentals. Further confounding identification, these studies document that (1) monetary policy surprises are predictable, and/or (2) private sector expectations (and possibly stock prices) go in the "wrong" direction. Concerning the latter, following a positive monetary policy surprise, expectations of future GDP growth (or stock prices) rise. The predictability of monetary policy surprises and the Fed information effect call into question the central assumption that these surprises are appropriate to identify (pure) monetary policy shocks.

Our contribution to the literature

Our new shock is a single-factor, summary measure of the monetary policy actions (or inactions) on FOMC announcement days, and thus follows Nakamura and Steinsson, Jarocinski and Karadi, and much of the literature examining distributional effects of monetary policy on firms and households also uses a single-factor shock to characterize Fed policy.[6] Gürkayanak et al. (2005), Swanson (2018b), and Rogers et al. (2018) argue that monetary policy has more than one dimension. Changes in the federal funds rate may be different from forward guidance announcements, and both of these may be different from LSAP announcements. Furthermore, other researchers distinguish between different types of forward guidance shocks: those that convey information about the economy and those that convey information about monetary policy

[2] See Wold (1966) and Kelly and Pruitt (2013) for applications to equity returns.

[3] The yield curve is also used as a function in contemporaneous work by Inoue and Rossi (2018). Like us, they propose a new way to identify monetary policy shocks, in what they refer to as "functional shocks", and then estimate transmission effects during periods of conventional and unconventional policy. We differ in several important ways: (1) we use a much simpler method involving only linear regressions; and (2) we focus on the information effects of identified shocks while Inoue and Rossi focus more on econometric issues. Conclusions concerning the transmission effect of shocks are consistent, however.

[4] See Kuttner (2001), Cochrane and Piazzesi (2002), Rigobon and Sack (2004), Gurkayanak, Sack, and Swanson (2005), Wright (2012), Gertler and Karadi (2015), Nakamura and Steinsson (2018), Rogers et al. (2018), Swanson (2018b), and Jarocinski and Karadi (JK) (2018)

[5] See Romer and Romer (2000), Campbell et al. (2012), Nakamura and Steinsson (2018), Miranda-Agrippino (2016), Jarocinski and Karadi (2018), Hansen et al. (2019), Cieslak and Schrimpf (2019), Paul (2019), and Bauer and Swanson (2020).

[6] See Gorodnichenko and Weber (2016) and Ottonello and Winberry (2018) on firms and Coibion, Gorodnichenko, Keung, and Silvia (2017); Kaplan et al. (2018); Auclert (2019); Wong (2019); Cravino et al. (2020); and Ravn and Sterk (2020) for households.

Campbell et al. (2012) and JK (2018). The shock measures from these papers thus reflect, under the maintained identifying assumptions, the separate effects of changes in the target rate, forward guidance, and asset purchases. Our single-factor measure represents outcomes on FOMC meeting day that reflect the combined effect of all words and actions (or lack thereof) undertaken by the Committee. Our measure is best thought of as an average effect of Fed funds rate changes, forward guidance, and LSAPs following the FOMC meeting, just like those in the applications cited above. By analogy, the forward guidance shocks identified by Swanson (2018b) and Rogers et al. (2018) are averages of guidance that conveys information about monetary policy and guidance that reveals private information.

We conduct four empirical exercises with our new measure. First, we show that both the short end and long end of the yield curve respond less to our shock than do medium-horizon maturities (2-yr and 5-yr rates), and in this sense our shock looks very similar to the NS shock and the Swanson forward guidance shock. Moreover, there are many days in which the stock market co-moves positively with our series, consistent with Jarocinski and Karadi (2018). Focusing on the period surrounding lift-off in December 2015, we show that our shock series reflects the strong forward guidance delivered at the October 2015 FOMC meeting, implying that a contractionary monetary policy shock took place *before* the actual interest rate hike, consistent with existing measures.

Second, we show that our shock is largely unpredictable from available information, including Blue Chip forecasts, "big data" measures of economic activity, news releases, and consumer sentiment. This contrasts with alternative measures of monetary policy shocks, as emphasized by Ramey (2016), Miranda-Agrippino (2016), and Bauer and Swanson (2020), among others.

Third, we empirically test for the Fed information effect, following two approaches: the Nakamura-Steinsson expectations-based test and Jarocinski-Karadi "indirect" test. Using the NS test, we do not find a statistically significant information effect in our new shock series, while we confirm its presence in those of Nakamura and Steinsson (2018) and Swanson (2018b). We also follow Jarocinski and Karadi (2018) and examine the high-frequency co-movement of interest rates and stock prices around FOMC announcements. Monetary policy announcements that lead to positive co-movement (within the day) are defined to be those that reveal central bank private information. Jarocinski and Karadi construct their monetary policy surprises also using only a short rate, the three-month Fed Funds futures rate (FF4). Using their data, we find evidence of the Fed information effect, in the sense of Nakamura-Steinsson, on (JK) information effect days. However, using our new measure, and even confining our analysis to observations that occur on days with positive co-movement between stock prices and interest rates, we find no evidence of an information effect in the sense of Nakamura and Steinsson (2018).

Finally, we demonstrate using our series that a positive monetary policy shock leads to significantly negative effects on output and prices, consistent with standard theory, in both vector autoregressions and local projections (LP). This is true in the full sample and for sub-samples before and during the ZLB. We also find conventional signs using our monetary policy shock measure irrespective of whether the stock market rose or fell on FOMC announcement day. These macro transmission effects differ from those of monetary policy shocks that contain the information effect.

In the next section, we describe our econometric approach and provide a conceptual analysis of why our procedure for estimating the new shock series helps render insignificant the information effect. In Section 3, we display estimates of our new series, compare it to alternatives in the literature, and perform several robustness exercises: constructing real-time versions of the shock, series with alternative benchmark policy indicators from 3m to 10yr, and sub-sample stability of our measure. We also estimate its effects on financial variables. In Section 4, we demonstrate that our shock is relatively unpredictable, estimate its effects on the yield curve, and empirically test for the information effect. In Section 5, we demonstrate that shocks to our series stably produce impulse responses that are consistent with conventional theory. Section 6 concludes.

2. A New Monetary policy shock: Methodology

2.1. Estimation approach

We assume that the true monetary policy shock e_t is unobservable. We further assume that the (observable) changes in Treasury yields around FOMC announcement days are driven by a monetary policy shock and nonmonetary policy shock. Our objective is to estimate the former. We normalize the unobserved shock to have a one to one relationship with the 2-yr Treasury yield.[7] We choose the 2 year rate because (1) it is widely used in the literature (e.g., Gilchrist et al., 2015); (2) captures crucial aspects of Fed monetary policy, while not significantly constrained by the Zero Lower Bound Swanson and Williams (2014))[8]; and (3) normalizing to a relatively short-term interest rate helps reduce the information effect from our estimates, as we make clear below.

Our Fama-MacBeth two-step procedure extracts monetary policy shocks e_t from the common component of the outcome variables $\Delta R_{i,t}$. In the first step, we estimate the sensitivity of each outcome variable to monetary policy via time-series regressions. We assume that the outcome of monetary policy decisions is reflected in movements of zero-coupon yields with

[7] Normalizing to other interest rates gives effectively similar results. We check robustness to the choice of monetary policy indicator from 3-mo. to 10-yr rates (Appendix C).

[8] Swanson and Williams (2014) argue that after 2008, "The 2-year Treasury yields sensitivity to news was generally not significantly attenuated until late 2011, and even then remained partially responsive to news until late2012." (the end of their sample; see their Figure 3(d)).

maturities of 1 year to 30 years. As we demonstrate below, our use of the full maturity structure is important, most notably in producing a shock series with no significant information effect. In the estimation, we use a 1-day window, capturing policy surprises between FOMC announcement day (end) and the previous day (end). Because the Fed released no public statements about monetary policy decisions until 1994, we begin estimation then. These outcome variables are also affected by background noise:

$$\Delta R_{i,t} = \alpha_i + \beta_i e_t + \epsilon_{i,t} \tag{1}$$

where $\Delta R_{i,t}$ is the change in the zero-coupon yield with i-year maturity and $\epsilon_{i,t}$ denotes factors unrelated to monetary policy news, including factors associated with the Fed information effect.[9]

From our normalization, we can rewrite (1) as,

$$\Delta R_{i,t} = \theta_i + \beta_i \Delta R_{2,t} + \xi_{i,t} \tag{2}$$

where $\xi_{i,t} = -\beta_i \epsilon_{2,t} + \epsilon_{i,t}$ and θ_i is a constant. Recalling that $\epsilon_{2,t}$ is the error term in the policy indicator (see Eq. (1)), we see that the regressor $\Delta R_{2,t}$ is correlated with the error term $\xi_{i,t}$ due to the component "$-\beta_i \epsilon_{2,t}$". The OLS estimate of β_i is thus biased. More importantly, the estimate of β_i can be biased because the information content in $R_{2,t}$ and $\xi_{i,t}$ is correlated. The former problem, which is a traditional errors-in-variables problem, can be reconciled using the heteroskedasticity-based estimator of Rigobon (2003) and Rigobon and Sack (2004). As demonstrated formally in Appendix B, β_i in (1) can be consistently estimated using instrumental variables (IV).[10] The bias caused by the second problem is more important, and we discuss it thoroughly in Section 2.2.

The second step of our approach, by analogy to Fama and MacBeth, is to recover the aligned monetary policy shock from cross-sectional regressions of $\Delta R_{i,t}$ on the estimated sensitivity index $\hat{\beta}_i$ for each time t,

$$\Delta R_{i,t} = \alpha_i + e_t^{aligned} \hat{\beta}_i + \nu_{i,t} \quad t = 1, 2, \cdots, T \tag{3}$$

where $e_t^{aligned}$ is the coefficient of interest. This series of T estimated coefficients from the second step regressions is the BRW monetary policy shock series.

2.2. Econometric methodology and the fed information effect

Monetary policy announcements contain information about central bank forecasts of economic fundamentals, which is referred to as the "Fed information effect" (see Nakamura and Steinsson, 2018; Romer and Romer, 2000, and Jarocinski and Karadi (2018)). As a by-product, macroeconomic variables such as output and inflation may be influenced not only by the announced policy itself but also by the forecasting information contained in the announcement. The opposite forces from these two sources may cause puzzling impulse responses such as output rising after a contractionary policy shock. Use of even narrow windows around central bank announcements may not alleviate the issue for researchers.[11]

We use a simple example to show that our two-step procedure can substantially reduce the Fed information content from our estimated monetary policy shock, under testable assumptions. For simplicity, consider three interest rates: one short-term interest rate y_{1t} that is affected by both the monetary policy shock e_t and the information effect u_{St}; and two long-term interest rates y_{2t} and y_{3t} that are affected by the monetary policy shock e_t and the information effect u_{Lt}. We differentiate u_{St} from u_{Lt} based on Hansen et al. (2019), who show that information effects in short- and long-term interest rates are different. Normalize the unobserved monetary policy shock e_t to have a one-to-one relationship with the short-term interest rate y_{1t}:

$$y_{1t} = e_t + u_{St} + \nu_{1t} \tag{4}$$

$$y_{2t} = \gamma_2 e_t + u_{Lt} + \nu_{2t} \tag{5}$$

$$y_{3t} = \gamma_3 e_t + u_{Lt} + \nu_{3t} \tag{6}$$

where ν_{1t}, ν_{2t}, and ν_{3t} are background noise that are assumed to be orthogonal to each other, and $Cov(e_t, u_{St}) = 0$, $Cov(e_t, u_{Lt}) = 0$, $Cov(u_{St}, u_{Lt}) = 0$. The first and second equality follow by definition. The third equality follows according

[9] For example, the market interpreting an FOMC announcement as revealing private information it has on the state of the economy, its own preferences for inflation versus output stabilization, etc. The fact that Federal Reserve Board staff construct the index of Industrial Production is one potential source of such private information. Fed staff are situated particularly auspiciously, for example, to ascertain and report to the FOMC in private how noisy is a particular release of the IP series. See Nakamura and Steinsson (2018) for further discussion of "background noise".

[10] The underlying assumption is that on days of FOMC meetings the variance of the "true" monetary policy shock increases while that of non-monetary policy news is unchanged. We have tested this assumption and find that it holds for all interest rates used in this paper (Appendix B).

[11] Campbell et al. (2012) also provide evidence of a Fed information effect. Hansen et al. (2019) find that information signals affecting the short and long-run yields are different. While short-run yields are driven by expectations about economic conditions, long-run yields are mostly driven by economic uncertainty. Faust et al. (2004) and Zhang (2019) find no evidence of an information effect, while Lunsford (2018) argues that in his sample from February 2000 to May 2006 the information effect is present in the first half only.

to our discussion about information effects along the yield curve. It holds when *either* $u_{Lt} = 0$, *or* u_{St} and u_{Lt} are different. We show empirical evidence for the first condition ($u_{Lt} = 0$) in Section 4.3. For more discussions about the second condition (u_{St} and u_{Lt} are different), see Hansen et al. (2019).

From the first step time-series regression of $y_{it} \quad (i = 2, 3)$ on y_{1t} we obtain the coefficient $\hat{\beta}_i \quad (i = 2, 3)$,

$$\hat{\beta}_i = \frac{Cov(y_{it}, y_{1t})}{Var(y_{1t})} = \frac{\gamma_i Var(e_t)}{Var(y_{1t})} = \frac{\gamma_i Var(e_t)}{Var(e_t) + Var(u_{St}) + Var(v_{1t})} \propto \gamma_i \tag{7}$$

$\hat{\beta}_i$ approaches $\gamma_i * c$ where c is a constant.

In the second step, run a cross-section regression month by month of $y_t = \binom{y_{2t}}{y_{3t}}$ on $\beta^* = \binom{\hat{\beta}_2}{\hat{\beta}_3} \rightarrow c * \binom{\gamma_2}{\gamma_3} = c \times \gamma *$. The coefficient extracted through this regression, i.e., our shock at time t, is thus,

$$\hat{shock_t} = \frac{Cov(\beta^*, y_t)}{Var(\beta^*)} = \frac{c * Cov(\gamma^*, y_t)}{c^2 Var(\gamma^*)} = \frac{e_t Var(\gamma^*)}{c * Var(\gamma^*)} \propto e_t \tag{8}$$

Therefore, under the maintained assumptions, our shock is effectively devoid of the information effect. Note that we drop the benchmark interest rate y_{1t} in the second step to avoid our estimation failing as a result of the disproportionate coefficient of y_{1t} on e_t. Recall that y_{1t} is only used to normalize our monetary shock in the first step.[12]

This contrasts with Principal Components Analysis (PCA), as used in NS, in which the factors are linear combinations of the original variables (e.g., interest rate futures changes around FOMC announcements) that contain the largest amount of time-series variation. These factors are likely to embody information effects since they always weight the original variables based on their loadings.[13] We compare our shock series and to one constructed using PCA below.

3. The new shock series

We collect data on the monetary policy indicator from the Federal Reserve Board public website. As noted above, we examine 2-year, 5-year, and 10-year Treasury rates, with 2-year as benchmark. We also use data on estimated term premia, from Adrian et al. (2013), which are available through the New York Fed website https://www.newyorkfed.org/research/data_indicators/term_premia.html. The policy outcome variables, the zero coupon yields with maturities of 1 to 30 years and instantaneous forward rates, are estimated by Gürkayanak et al. (2007), and available at https://www.federalreserve.gov/pubs/feds/2006/200628/200628abs.html. To estimate impulse responses, we use monthly industrial production and CPI, both taken from https://fred.stlouisfed.org, the core commodity price index from Thompson Reuters, and the excess bond premium from Gilchrist and Zakrajsek (2012).

We display our monetary policy shock series in Fig. 1.[14]. There are sizable movements before, during, and after the ZLB period. The announcements of QE1, QE2, and QE3, marked by navy lines, all generate large expansionary shocks. Monetary policy shocks during Operation Twist, denoted by the orange lines, are instead contractionary. We mark with the blue line the FOMC meeting in October 2015, the meeting *preceding* lift-off in December. Zooming in on the last three meetings of 2015, our shock series takes the values -0.045 (September), 0.064 (October), and 0.020 (December). Expectations of a lift-off had been growing throughout the summer and heading into the October meeting. For a variety of reasons, including turmoil in global equity markets, the FOMC decided to keep the target Fed Funds rate unchanged at that meeting but sent a clear signal of a likely rise in December 2015.[15] Our measure indicates that this forward guidance gave rise to a sizable contractionary monetary policy shock in October 2015, one meeting before the actual rate increase. This is consonant with alternative measures. For example, the corresponding values of the policy shock of Nakamura and Steinsson (2018) are (-0.042, 0.032, 0.016); the forward guidance surprise in Rogers et al. (2018) are (-0.09, 0.09, 0.03); and in Swanson (2018b) (-1.50, 1.67, NA).[16]

3.1. Comparison with shocks in the literature

Moving beyond the plausibility of specific observations around liftoff and QE announcements, we compare our shock series to well-known measures in the literature: Kuttner (2001),Romer and Romer (2004), Nakamura and Steinsson (2018),Swanson (2018b) and Jarocinski and Karadi (2018). The updated R&R shock series runs through the end of 2007, as

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[12] As noted below, Bauer and Swanson (2020) argue persuasively that Fed information effects are due to the Fed's and, simultaneously, private forecasters' responses to economic news. The "direct test" of NS suffers from an omitted variables problem, it is argued, which is precisely news releases about, e.g., non-farm payrolls that both the Fed and private sector are reacting to. One could use our framework to think about this issue, under the assumption that u_{St} and u_{Lt} can be interpreted as the Fed's response to economic news. Hansen et al. (2019) show that economic factors affecting short term and long term interest rates are different. Information about economic conditions drives movements in short term yields while news about uncertainty (second or higher order information) surrounding economic conditions moves long term yields.

[13] For example, the first component X is calculated by applying weights w to each of the original variables, $w_t = arg\ \max\ \{w^T X^T X w\}$.

[14] Regular updates of our measure are available at: https://www.federalreserve.gov/econres/feds/a-unified-measure-of-fed-monetary-policy-shocks.html.

[15] As headlined in the Financial Times on October 29, 2015: "Federal Reserve drops warnings on global risks to US economy: Central bank hawkish statement increases chances of December rise."

[16] Magnitudes differ due to different normalization choices, especially by Swanson, whose series ends with liftoff.

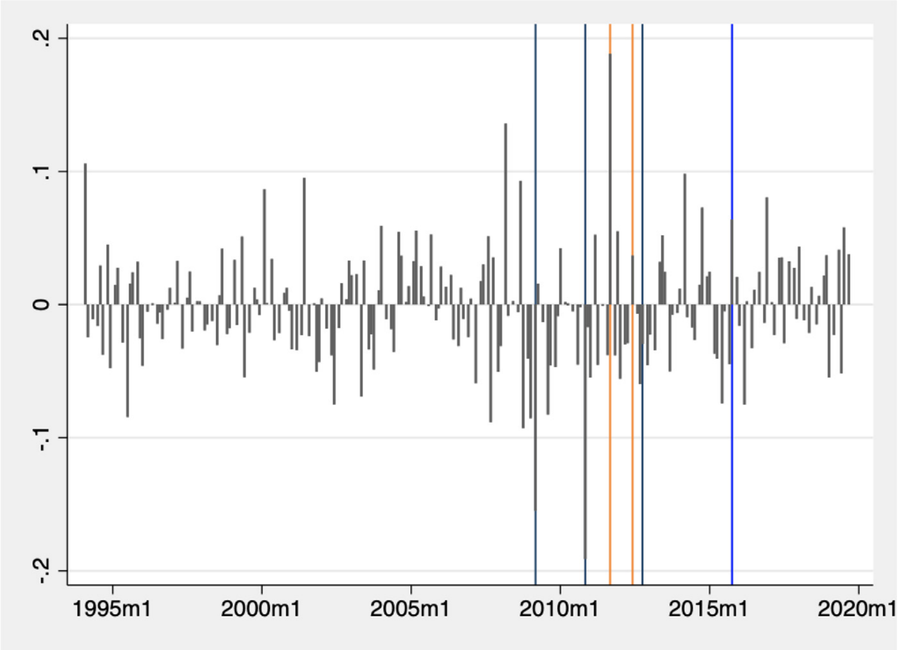


Fig. 1. BRW Shock Series Jan 1994 to Sep 2019. Note: The BRW shock series is estimated from Eqs. (2) and (3). The navy vertical lines denote announcements of QE1, QE2, and QE3; the orange vertical lines denote the Operation Twist period; and the blue line denotes Oct. 2015, the FOMC meeting prior to liftoff. Regular updates of this measure are available at: https://www.federalreserve.gov/econres/feds/a-unified-measure-of-fed-monetary-policy-shocks.htm. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

constructed by Wieland and Yang (2020) (see also Coibion, Gorodnichenko, Kueng, and Silvia (2017). Kuttner (2001) shocks are extracted from changes in Federal Funds futures rates in 30-minute windows around FOMC announcements. Nakamura and Steinsson also examine high-frequency movements around FOMC announcements. Their monetary policy shock is the first principal component of changes in the current month Federal Funds futures rate, the Federal Funds futures rate immediately following the next FOMC meeting, and two, three and four quarter ahead euro dollar futures in the 30-minute event window.[17] Jarocinski and Karadi (2018) use three-month Fed Funds futures (FF4) changes in 30-minute windows around FOMC announcements, while Swanson (2018b) separately identifies the effects of forward guidance, large-scale asset purchases, and target Federal Funds rate shocks, also using principal components.[18]

In Table 1 we present the correlation between our measure and the alternatives. As seen in column 1, over the full sample, our shock is reasonably well correlated (around 0.5) with the NS and Swanson shocks, which themselves are relatively large before and during the ZLB. The next two columns compare sub-periods before and during the ZLB. Before the ZLB, our series is correlated with NS, JK, and the Swanson FG shock at around 0.6. During the ZLB, the largest correlation, at 0.52, is with the Swanson FG shock. In online appendix Figure A.1, we display plots of our shock series against the alternatives. Consistent with the correlations above, prior to 2008 our shock series exhibits a similar pattern to the NS, Kuttner, and R&R shocks. After 2008, the alternative series are quite small given that the Fed Funds rate is at zero during the ZLB. In contrast, our new shock series exhibits relatively large movements, consistent with Fed monetary policy being about more than the target FFR. Our shock series is more similar to the FG shocks of Swanson.

3.2. Shock construction robustness (Table 2)

We examine several modifications to the construction of our baseline series. Our first check extends estimation backward to 1969. From the first column of Table 2, we see that the correlation with our baseline shock is over 93%.[19] Our next

[17] We obtain these shocks from Nakamura and Steinsson (2018) through 2014m3 (their sample period) and then follow their procedures to update to the present. For this exercise and all of our work using intra-daily data, we obtain the data from the "Event Study" database maintained by Federal Reserve Board staff.

[18] Rogers et al. (2018) implement an approach similar to Swanson (2018b) in computing their three separate components of Fed policy shocks. The series are very highly correlated with those of Swanson, around 0.96.

[19] One feature of our methodology is the need to check the stability of the sensitivities of interest rates to monetary policy shocks. Here, we do the rolling sample test for each period of 15 years, expanding the sample size to 1969 - 2017. When we use different monetary policy indicators of 1-, 2-, 5- and 10-year Treasury rates, the coefficients are stable beginning in the early 1990s (see the online appendix Figure A.2). Thus we start the sample in 1994, when the Fed first released a statement about FOMC policy decisions, and after which our sensitivity index is flat.

Table 1
Correlation with BRW Shock Series.

	Full Sample	Pre-ZLB	ZLB
NS Shock	0.545	0.677	0.473
SS shock	0.567	0.691	0.388
R&R Shock*		0.164	
Kuttner Shock		0.340	
SS_FFR		0.408	
SS_FG	0.469	0.601	0.523
SS_LSAP			0.187
FF4	0.453	0.609	0.338

Note: The benchmark shock is our BRW shock series estimated from Eqs. (2) and (3). *NS* Shock refers to the policy factor shock of Nakamura and Steinsson (2018), which we update to the present. *SS Shock* refers to the sum of the shock series of the federal funds rate, the forward guidance and the large asset purchases in Swanson (2018a). *R&R Shock** refers to the updated shock series following the narrative method in Romer and Romer (2004). *Kuttner Shock* refers to the 30-minute Fed Funds rate changes around FOMC announcements. *SS_FFR*, *SS_FG*, *SS_LASP* refers to the shock series of the Federal Funds rate, forward guidance and large asset purchases in Swanson (2018). *FF4* is the 30-minute change in 3 month federal funds futures rate around the FOMC announcement used in Jarocinski and Karadi (2018). Sample periods are: Full sample 1994m1-2017m12, Pre ZLB 1994m1-2008m12, ZLB 2009m1-2015m12.

Table 2
Shock Series Robustness: Correlations with Baseline BRW Shock Series.

	BRW69	R5	QE	Unschedule	Day2	IV2	BRW (RT1)	BRW (RT2)	TP	OLS	PCA	Tight (NS)	Tight (Full)
BRW Shock	0.936	0.949	0.993	0.904	0.828	0.990	0.960	0.955	0.807	0.995	0.104	0.363	0.535
Observations	191	191	190	183	191	191	191	191	191	191	190	191	191

Note: *BRW Shock* refers to our BRW shock series estimated from Eqs. (2) and (3). *BRW69* refers to our BRW shock series estimated from the sample 1969m1 to 2017m12. *R5* refers to the BRW shock series aligned using zero-coupon yields with only the 1, 2, 5, 10, 30-year maturities as outcome variables. *QE* refers to the BRW shock series excluding the announcement of QE1 in March 2009. *Unschedule* refers to the BRW shock series aligned including all of the unscheduled FOMC meeting dates since 1995. *Day2* refers to the BRW shock series aligned using a 2-day event window around FOMC announcement days. *IV2* refers to the BRW shock series aligned using daily movements in the policy indicator 1-day before FOMC announcement day rather than one week as the instrumental variable. *BRW (RT1)* refers to BRW shock series combining the rolling sample method post 2008 and original BRW shock before 2008. *BRW (RT2)* refers to BRW shock series aligned from sensitivity indexes from the pre-2008 subsample. *TP* refers to the BRW shock series generated free of the estimated term premium. *OLS* refers to the alternative BRW shock series aligned from the simple Fama-Macbeth method without the IDH procedure. *PCA* refers to the shock series generated from extracting the first principal component of our underlying data, i.e., all outcome variables (daily changes of 1 to 30-year zero coupon rate around FOMC meeting). *Tight(NS)* refers to the BRW shock series using the data underlying Nakamura and Steinsson (2018), i.e., the 30-minute changes of the current month Fed funds futures rate, the Fed funds futures rate immediately following the next FOMC meeting, and two, three, four quarter ahead euro dollar futures. *Tight(Full)* refers to the BRW shock series using the NS data and the 30-minute changes of the 3 month, 6 month, 2 year, 5 year, 10 year, 30 year interest rates around FOMC announcements.

modification is to use only the zero-coupon yields with 1-, 2-, 5-, 10-, and 30-year maturities as outcome variables. The correlation with the baseline shock series is 0.949 (column 2 of Table 2). We also assess robustness to leaving out the March 2009 QE1 announcement. This was a sufficiently big event occurring at a time when financial markets were so sluggish that the market response might not represent a typical effect of monetary policy. The shock series without QE1 is highly correlated with our baseline series (Column 3). Next, we include all unscheduled FOMC meeting dates and reconstruct our shock. We find a correlation of 0.904 (Column 4). We then use a 2-day event window for both policy indicator and outcome variables, and find a correlation with the baseline series of 0.828 (Column 5). Next, we construct the instrumental variable as the daily movement in the policy indicator *one day* (as opposed to one week) before FOMC announcement day. As presented in Column 6 of Table 2, this alternative shock series has a correlation of 0.99 with the baseline series. Finally, we examine robustness to leaving out identification through heteroskedasticity in the first step (leaving only Fama-MacBeth procedures). This shock series is highly correlated with our baseline series (Column 10).

As an additional robustness check, we construct real-time versions of the series.[20] We use two methods: in the first, we estimate step 1 on the sample up to 2007:12, use the betas from that in the second step regression to compute the monetary policy shock for 2008:1, then roll through the sample one month at a time to construct a real-time shock for 2008:2, 2008:3, ... using these rolling window sensitivity indexes. In the second method, we estimate the step 1 regression *only* up through 2007:12 and use the estimated betas from that regression to generate the monetary policy shock series for

[20] One advantage of using raw surprises as in Kuttner (2001) and JK (2018) is that the resulting shocks are precisely what occurred in real time. Series such as Nakamura and Steinsson (2018), Swanson (2018b), and our baseline measure above are (full-sample) estimation-based, do not account for estimation error, and are thus not strictly speaking real-time.

each observation beginning in 2008:1. The correlations of these two real-time measures with our baseline series are 0.960 and 0.955, respectively (see columns 7 and 8 of Table 2).

Finally, we examine different interest rates to normalize on, per Eq. (1). Table C.1 of Appendix C reports the correlation between our benchmark shock series, normalized on the 2-yr Treasury rate, with alternative series normalized on the 3-mo, 6-mo, 1-yr, 5-yr, and 10-yr Treasury rates. The correlations are all very high. Furthermore, we examine robustness to constructing our shock series using instantaneous forward rates over the yield curve, rather than zero coupon rates. The correlation between this series and our baseline measure is 0.88.[21]

3.3. Time variation

Evolution in the primary tool used in Fed monetary policymaking over our sample period naturally leads us to investigate whether such changes give rise to instabilities associated with our measure over sub-periods like the ZLB. We investigate time variation associated with our new measure in four ways. First, as noted above, the betas estimated from step 1 of our procedure exhibit no time variation starting with samples from the early 1990s. Second, we re-estimate our shock series separately using pre and post-2008 subsamples. The new, conjoined series is highly correlated (0.96) with the baseline series estimated over the full sample. Third, we plot in Appendix Figure A.3 a time varying version of the yield curve regressions above. We estimate time-varying effects of monetary policy shocks (ours and the alternatives) on the 2-yr Treasury rate, again using a five-year rolling window. Our shock has very stable loadings over the full sample, plotted as the solid line with 90% confidence intervals, in contrast to alternative measures. Finally, in the shock transmission analysis of Section 5 we demonstrate constancy of the impulse responses estimated over different sub-periods. Together, these four investigations suggest that there is no detectable break at the ZLB in our shock series, which captures monetary policy shocks in the medium part of the yield curve.

These results accord with Debortoli et al. (2019), who present impulse responses from a time-varying structural VAR model and conclude that there was not a significant change in U.S. monetary policy transmission during the ZLB. Swanson (2018a) reaches a similar conclusion that Fed monetary policy was not very constrained by the ZLB. Swanson and Williams (2014) measure the effects of the ZLB on interest rates of different maturities by estimating a time-varying, high-frequency sensitivity of those interest rates to macroeconomic announcements, compared to a pre-ZLB period. They conclude that monetary and fiscal policy were about as effective during the 2008–2010 period as they usually were.

4. Predictability, yield curve, information effects

4.1. Predictability of monetary policy shocks

A large recent and concurrent literature on monetary policy transmission emphasizes the role that monetary policy shock predictability could play. Ramey (2016), Miranda-Agrippino (2016), Miranda-Agrippino and Ricco (2017), Bauer and Swanson (2020), Karnaukh (2019), and Sastry (2020), present evidence that standard measures of monetary policy shocks, including those of Romer and Romer (2004); Nakamura and Steinsson (2018), Gertler and Karadi (2015), Jarocinski and Karadi (2018), and (2005)/Swanson (2018a,b) are predictable. These papers use as regressors a variety of available information on the state of the economy, including Blue Chip forecasts, news releases, and consumer sentiment. In this section, we show that our shock series is much less predictable than those examined in this literature.

As noted by Bauer and Swanson (2020), if the Fed and Blue Chip forecasters are responding to the same economic news and the Fed responds by more than financial markets expected them to, the consequent positive correlation between monetary policy surprises and forecast revisions would be consistent with a Fed information effect. For example, if monetary policy tightens in response to positive economic news by an unexpectedly large amount, the resulting positive monetary policy surprises occur in times of positive economic news. Bauer and Swanson (2020) propose an alternative explanation to the information effect, which they label the “Fed response to news” channel. An important implication of their analysis is that standard information effect regressions, such as those we will estimate below, suffer from an omitted variables problem. Because economic news released prior to FOMC announcements predicts subsequent monetary policy surprises, the authors find, including pre-FOMC economic news in the NS regressions removes the evidence of a Fed information effect. Bauer and Swanson (2020) consider three measures of news: the monthly change in nonfarm payrolls, the Chicago Fed’s “big data” business cycle indicator (Brave et al., 2019)), and the percent change in the S&P 500 index from one quarter prior to the FOMC announcement to the day before the subsequent FOMC announcement.

Sastry (2020) investigates why the FOMC is able to affect economic activity by shaping expectations through forward guidance. He provides a new interpretation of this that works through public sentiment about where the economy is headed. He shows that the Michigan Survey of Consumers’ unemployment outlook, measured in the three months prior to an FOMC meeting, significantly predicts the high-frequency monetary surprises of Nakamura and Steinsson (2018). When the public is pessimistic, markets are consistently surprised about the extent to which the Fed plans to cut rates, so that negative

[21] We also test for information effects in the differently-benchmarked series, using the approaches described in section 4.3, and find effectively none (see Table C.2).

Table 3
Predictability of Monetary Policy Shocks.

a. With *crisis* dummy for 2009Q1					
	BRW	FF4	NS	SS_FFR	SS_FG
Blue Chip	0.010	0.060**	0.058***	0.926**	1.025**
	(0.021)	(0.026)	(0.019)	(0.406)	(0.456)
Nonfarm payrolls	0.004	0.014**	0.010*	0.164	0.158
	(0.004)	(0.007)	(0.005)	(0.110)	(0.103)
ADS index	0.001	0.015*	0.014**	0.184	0.162
	(0.005)	(0.009)	(0.006)	(0.134)	(0.110)
Brave et. al. index	0.002	0.006**	0.006***	0.087*	0.077*
	(0.002)	(0.003)	(0.002)	(0.046)	(0.044)
R^2 of Unemp sentiment	0.02	0.07	0.12	0.08	0.04
Obs	208	213	181	178	178
b. Without *crisis* dummy for 2009Q1					
	BRW	FF4	NS	SS_FFR	SS_FG
Blue Chip	0.021	0.058**	0.057***	0.896**	1.064**
	(0.020)	(0.025)	(0.019)	(0.388)	(0.443)
Nonfarm payrolls	0.009**	0.011*	0.009*	0.135	0.170*
	(0.004)	(0.006)	(0.004)	(0.089)	(0.087)
ADS index	0.008	0.012	0.011**	0.146	0.177*
	(0.006)	(0.007)	(0.005)	(0.106)	(0.091)
Brave et. al. index	0.004**	0.005**	0.005**	0.067*	0.078**
	(0.002)	(0.002)	(0.002)	(0.037)	(0.036)
R^2 of Unemp sentiment	0.02	0.07	0.12	0.08	0.04
Obs	208	213	181	178	178

Note: Sample period: 1994m1-2019m12. Constant term not displayed. Robust standard error (White) in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *FF4* is the 30-minute change in 3 month federal funds futures rate around the FOMC announcement. *NS* Shock is the policy factor shock of Nakamura and Steinsson (2018), updated to 2015m12. *SS_FFR,SS_FG* are the federal funds rate and forward guidance shocks of Swanson (2018). *BRW* is our baseline shock series. *Blue Chip* is the monthly revision of 1-quarter ahead GDP growth forecast in Blue Chip. *Nonfarm payrolls* is the monthly change in the nonfarm payrolls release. *ADS index* is the Aruoba, Diebold, and Scotti (2009) business conditions index. *Brave et. al. index* is the business cycle index in Brave et al. (2019). *Unemp sentiment* refers to consumer sentiment about how unemployment will evolve in the next year, from the Michigan Survey of Consumers, following Sastry (2020). In panel a, row 1–4 reports the coefficient of an OLS regression of each shock series on news released at the beginning of the corresponding FOMC meeting month: $shock_t = \alpha + \beta news_t + crisis_t + \epsilon_t$, where $crisis_t$ is a dummy equal to 1 for 2009Q1. We exclude any observation if $news_t$ is released after the FOMC meeting at time t. This adjustment is applied to Blue Chip, which is typically released in the first three business days of a month, and to nonfarm payrolls, typically released on the first Friday. We use the Brave et al. index for month $t-1$ as measure of $news_t$, as it is computed using economic data released in month t. Row 5 reports the adjusted R^2 of an OLS regression of each shock series on three lags of unemployment sentiment index: $shock_t = \alpha + \sum_{l=1,2,3} \beta_l UnempSent_{t-l} + \epsilon_t$. Panel b reports results excluding the *crisis* dummy.

economic news predicts negative measured monetary policy surprises. Similarly, Karnaukh (2019) finds that Blue Chip GDP growth forecast revisions explain 15% of the NS monetary policy surprises around scheduled FOMC meetings. A positive (negative) GDP growth revision predicts a contractionary (expansionary) policy news shock. She argues that failing to account for this predictability biases estimates of monetary policy transmission. Echoing Miranda-Agrippino and Ricco (2017), she finds that policy news shocks that are orthogonal to professional forecaster beliefs about the economy have more negative effects on GDP than the raw policy shocks.

We examine the predictability of our new shock series using an approach similar to that of these recent papers. In Table 3 we report the coefficient of an OLS regression of the monetary policy shock series on news released at the beginning of the corresponding FOMC meeting month:

$$shock_t = \alpha + \beta news_t + \gamma crisis_t + \epsilon_t \tag{9}$$

where $crisis_t$ is a dummy equal to 1 for 2009Q1. In panel (a) we report estimates with the dummy and in panel (b) without it. We run regressions separately on one of four measures of economic news: *Blue Chip* is the monthly revision of 1-quarter ahead GDP growth forecast in Blue Chip; *Nonfarm payrolls* is the monthly change in the nonfarm payrolls release; *ADS index* is the Aruoba et al. (2009) business conditions index; and *Brave et. al. index* is the business cycle index in Brave et al. (2019).[22] Finally, we report the adjusted R^2 of an OLS regression of the shock on three lags of the Michigan Survey

[22] In all regressions, we exclude any observation if $news_t$ is released after the FOMC meeting at time t. This adjustment is applied to Blue Chip, which is typically released in the first three business days of a month, and to nonfarm payrolls, typically released on the first Friday. We use the Brave et al. index

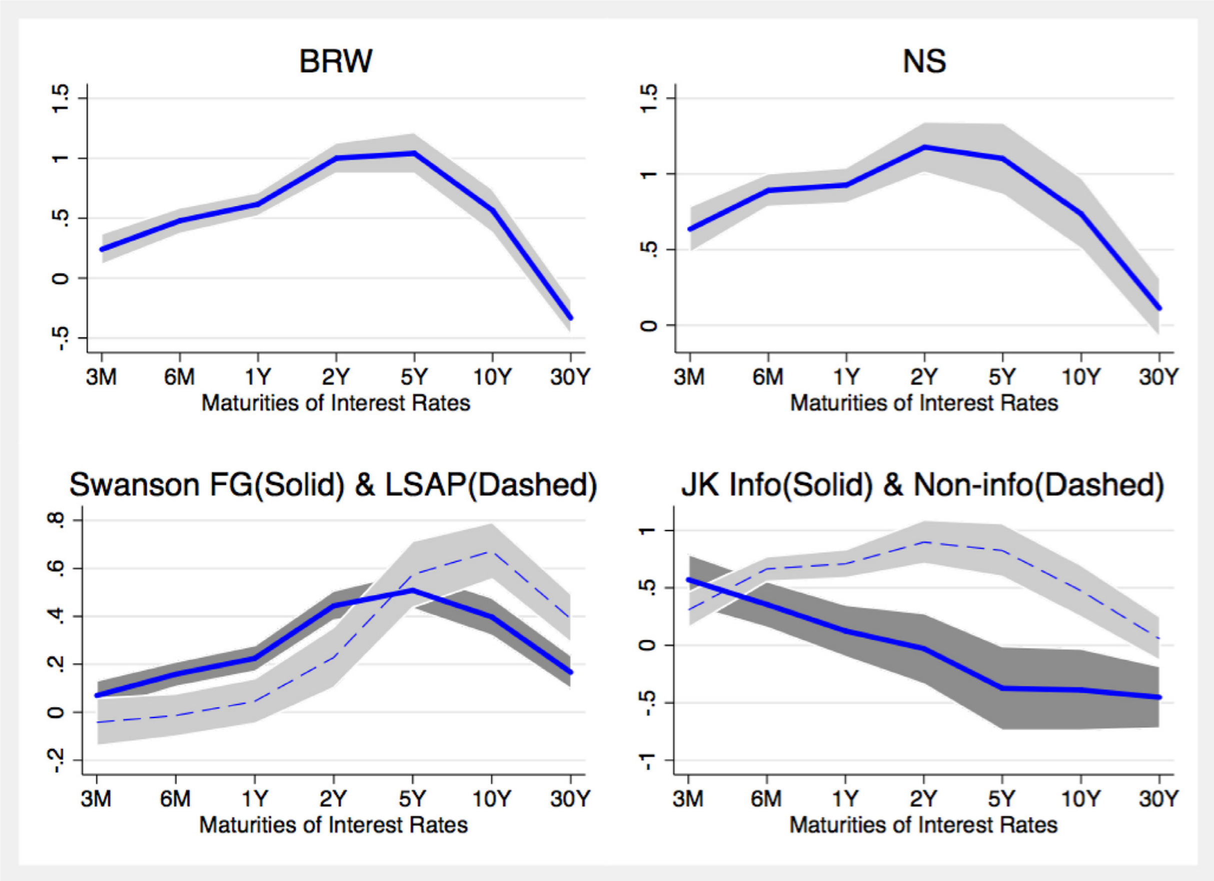


Fig. 2. Monetary Policy Shocks and the Yield Curve. Note: 90% confidence intervals are displayed for all shock series. *3M, 6M, 1Y, 2Y, 5Y, 10Y, 30Y* refers to the daily change in the 3 month, 6 month, 1, 2, 5, 10, and 30 year treasury bond yields around the FOMC announcement. *BRW* refers to our benchmark shock series. *NS* Shock refers to the policy factor shock of Nakamura and Steinsson (2018), which we update to the present. *Swanson FG*, *Swanson LASP* refers to the shock series of forward guidance and large asset purchases in Swanson (2018). *JK Info* and *JK Non-info* are the surprises of the 3-month federal funds futures focusing only on the Fed information effect days and non-information effect days, as used by Jarocinski and Karadi (2018).

unemployment sentiment index (Sastry, 2020):

$$shock_t = \alpha + \sum_{l=1,2,3} \beta_l UnempSent_{t-l} + \epsilon_t \tag{10}$$

As seen in panel (a), none of the news measures significantly predicts our series but all four are significant for the FF4 and NS shocks. Furthermore, the regression R^2 value from the unemployment sentiment index is a mere 0.02, compared to 15% reported by Sastry (2020). In panel (b), where we exclude the dummy for 2009Q1, there is some evidence of predictability of our measure from non-farm payrolls and the Brave et. al. index. However, as noted by the authors and by Bauer and Swanson (2020), this index includes data that are released *after* the FOMC announcement as well as before, thus contaminating interpretation.[23] Finally, predictability from non-farm payrolls clearly results from the two large negative releases in January and March 2009 amid sizable FOMC loosenings, as seen in online appendix Figure A.4.

4.2. Effects on the yield curve

Comparisons above suggest that our shock is closely related to forward guidance, which is well captured by movements in 2-year interest rates. Fig. 2 provides further evidence of this, displaying estimates of our shock on the yield curve. We regress interest rates of different maturities on the monetary policy shock,

$$\Delta y_{i,t} = \alpha_i + \beta_i e_t + \epsilon_{i,t} \tag{11}$$

for month $t-1$ as measure of $news_t$, as it is computed using economic data released in month t. We also examined revisions of 1-quarter ahead GDP growth forecasts in the Greenbook. Results are consistent with those displayed here.

[23] The ADS measure, for which there is no predictability in our regressions with or without the crisis dummy, does not suffer from this timing problem.

Table 4
Fed Information Effect Regressions of Nakamura and Steinsson (2018).

	BRW	SS	NS	BRW (RT1)	BRW (RT2)	BRW (JK Info)	BRW (JK Ninfo)	FF4 (JK Info)	FF4 (JK Ninfo)	PCA	BRW Tight (NS data)	BRW Tight (full data)
1995–2015	-0.05	1.80**	0.75***	-0.13	-0.11	0.32	-0.10	1.240***	0.31	0.56**	-0.18	-0.10
	(0.26)	(0.71)	(0.22)	(0.19)	(0.15)	(0.49)	(0.29)	(0.24)	(0.21)	(0.26)	(0.12)	(0.33)
Obs	132	134	133	132	132	45	87	24	74	131	135	135

Note: Monthly change (current month to next) in Blue Chip survey expectations of output growth over the next 3 quarters regressed on the shock series in that month plus a constant (not displayed). Sample periods are listed at top. Robust standard error (White) in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *BRW* refers to our shock series: our data and our PLS estimation procedure. *SS* refers to the sum of the shock series of the Federal Funds rate, forward guidance and large scale asset purchases of Swanson (2018), scaled by 10. *NS* refers to the policy news shocks of Nakamura and Steinsson (2018). *BRW (RT1)* refers to BRW shock series combining a rolling sample method after 2008 and original BRW shock before 2008. *BRW (RT2)* refers to BRW shock series aligned from sensitivity indexes of the pre-2008 subsample.*BRW(JK Info)* and *BRW(JK Ninfo)* are two sub-sample regressions focusing only on the Fed information effect days and non-information effect days, as defined by Jarocinski and Karadi (2018). *FF4(JK Info)* and *FF4(JK Ninfo)* are the surprises of the 3-month federal funds futures focusing only on the Fed information effect days and non-information effect days, as used by Jarocinski and Karadi (2018). *PCA* refers to the shock series generated from extracting the first principal component of our underlying data, i.e., all outcome variables (daily changes of 1 to 30-year zero coupon rate around FOMC meeting), scaled by 100. *BRW Tight(NS data)* refers to the BRW shock series computed using PLS with the data in Nakamura and Steinsson (2018), i.e., the 30-minute changes of the current month Fed funds futures rate, the Fed funds futures rate immediately following the next FOMC meeting, and two, three, four quarter ahead euro dollar futures. *BRW Tight(full data)* refers to the BRW shock series using the NS data *plus* the 30-minute changes of the 3 month, 6 month, 2 year, 5 year, 10 year, 30 year interest rates around FOMC announcements. *BRW (RT1)* refers to BRW shock series combining rolling sample method post 2008 and original BRW shock before 2008.

where $\Delta y_{i,t}$ is the change in interest rate of maturity i around the FOMC announcement and e_t is, alternatively, the BRW, NS, Swanson, and JK shock series.

As seen from plots of the β_i, our shock has a hump-shaped effect: the coefficient reaches its maximum at the 2-year interest rate, which we normalize to be 1. The response of the 5-year interest rate is also large and significant. Coefficients in regressions for all other maturities (3-mo., 6-mo., 1-yr, 10-yr and 30-yr) are significant but smaller: both the short and long ends of the yield curve respond to our shock by less than do the 2- and 5-yr rates. In the remaining panels of Fig. 2, we estimate the same regressions using the NS, JK, and Swanson shocks, and plot the coefficients. Shocks to our series have effects similar to those of Swanson's forward guidance shock and JK's non-info day shock series. All of them move the 2-yr, 5-yr, and 10-yr interest rate the most. The NS shock series and Swanson's LSAP shock series affect the yield curve at the short end and long end, respectively. The JK shock affects the yield curve significantly differently on information effect and non-information effect days, as expected. As seen in the last panel of the figure, FF4 shocks on non-information effect days affect the yield curve in much the same way as BRW shocks, while on information effect days the shock is strongest at the very short end of the yield curve, with zero effect on the 2-year or 5-year rates.[24]

4.3. The Fed information effect

We begin with the test of Nakamura and Steinsson (2018). We confirm their results for their series and examine robustness to our shock and Swanson (2018b)'s. Specifically, we run regressions of monthly changes in Blue Chip survey expectations of output growth on the monetary policy shock series of that month, and test for the Fed information effect based on the sign of the estimated coefficient. Table 4 displays the results. While the information effect is significant in the measures of Nakamura-Steinsson and Swanson, it is insignificantly different from zero in ours (see the first three columns). We also find that the two real-time BRW measures do not feature significant information effects (fourth and fifth columns).[25]

Jarocinski and Karadi (2018) construct their information shock by examining the high-frequency co-movement of interest rate and stock price surprises on FOMC announcement days. They argue that when the stock market moves in the same direction as interest rates, the Fed information effect dominates the monetary policy news effect of the announcement. Following Jarocinski and Karadi, we depict in the scatterplot of Fig. 3 daily returns on the S&P 500 on FOMC announcement days against the BRW shock (blue dots) as well as the JK surprises – FOMC announcement day high-frequency changes in the fourth Fed Funds futures contract (in orange). Although the relationship is negative overall, there are many points falling in the first and third quadrants. As emphasized by JK, these are difficult to explain as purely monetary policy shocks. We re-estimate the NS information effect regressions, separately on Fed information effect days and non-information days, for both BRW and JK measures. The results are displayed in columns six and seven (BRW) and eight and nine (JK) of Table 4. In regressions with the BRW measure, the point estimates are very small and have no statistical significance. Thus, even during the Jarocinski-Karadi information effect days, our shock does not display economically or statistically important Fed

[24] Results for our ECB shock are similar (see Appendix D). The shock series normalized on the 2-year (5-year) rate captures relatively more information at the short to medium (medium to long) end of the yield curve.

[25] We find no evidence of a significant relationship, either in the full sample or when running the tests on the NS sub-samples: 1995–2014, 2000–2014, and 2000–2007 (see online appendix Table A.1). Extending through 2018 does not alter our conclusions. Also following NS, we exclude from these regressions all observations when FOMC meetings occurred in the first week of the month, as that likely precedes the time that the Blue Chip survey forecast was made for that month.

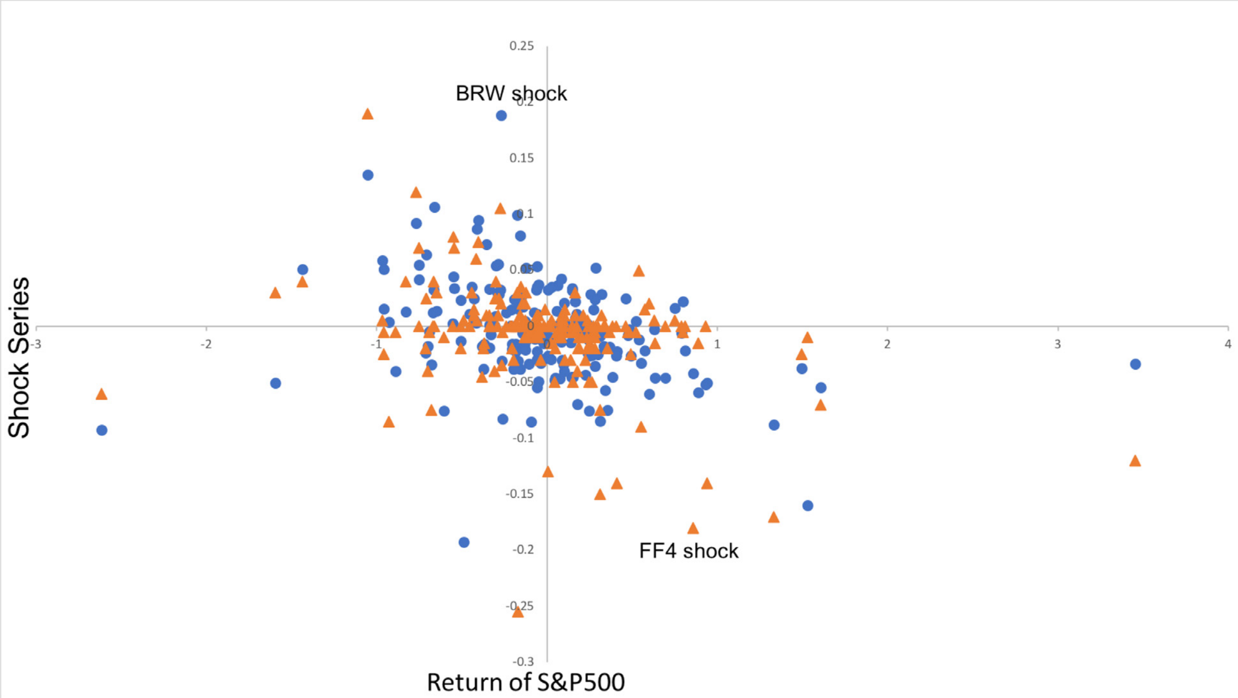


Fig. 3. S&P 500, the BRW Shock, and the JK Shock. Note: Shock series plotted against the S&P 500 returns. The S&P 500 returns are computed over a 30-minute window around FOMC meeting announcements. The blue dots represent the BRW shocks, and the orange triangles are the surprises of the 3-month federal funds futures that are used by Jarocinski and Karadi (2018). (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

Table 5
Fed Information Effect in Interest Rates with Different Maturities.

	Kuttner	6-month	2-yr.	5-yr.	10-yr.	30-yr.
Coef.	0.296***	0.395*	0.348**	0.254	0.279	0.193
	(0.11)	(0.22)	(0.16)	(0.17)	(0.21)	(0.30)
Observations	168	168	168	168	168	168
R-squared	0.039	0.025	0.032	0.015	0.010	0.003

Note: Constant term not displayed. Robust standard error (White) in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. We regress the monthly change (current month to next) in survey expectations of output growth over the next 3 quarters from Blue Chip Economic Indicators on the shock series in that month. *Kuttner Shock* refers to monetary policy shock of Kuttner(2001). *6 month* refers to the 30-minute change in 6 month treasury note yield around the FOMC announcement. *2, 5, 10, and 30 year* refer to the 30-minute changes in 2, 5, 10, and 30 year treasury bond yields around the FOMC announcement. The sample period is 1994m1-2019m12. Following NS, we exclude the Great Recession period.

information effects in the sense of NS. The next two columns of Table 4 confirm that the information effect *is* present in the Jarocinski-Karadi data. This naturally sparks the question we address next.

Why does our shock series have less of an information effect?

We investigate empirically why tests for the information effect are rejected using our shock series but not the alternatives. This exercise also validates the assumptions made in our conceptual example of Section 2.2. We begin by comparing tests using the shock series generated by the PCA approach. To see the differences, we input our data into the principal components estimation procedure to construct an alternative monetary policy shock, which we label the “PCA shock”. As seen in column 11 of Table 2, the correlation between this and our baseline BRW shock is only 0.104. Moreover, estimating the NS information effect regressions with this PCA shock, we find that a positive shock leads to a significant *increase* in the Blue Chip real GDP growth rate forecast in the next quarter, consistent with Fed information effects (Table 4, column 10). Thus, the PCA approach does not remove the Fed information effect even when the underlying data include long-term interest rates.

Next, we show that long-term interest rates are less associated with Fed information effects. Nakamura and Steinsson construct their monetary policy shock using short-term interest rates up to one year. By contrast, we use the whole yield curve. In Table 5, we report results of the NS information effect regressions–monthly changes in Blue Chip survey expectations of output growth on the 30-minute changes of interest rates–with maturities from 1 day (Fed funds future rate) to 30-year treasury bond yields. This table is similar to Table 4. It is clear that as the maturity of interest rates increases, the coefficients become less significant. This indicates that one reason our shock contains less of a Fed information effect is because we incorporate longer term interest rates than do alternative measures of Fed monetary policy shocks. Another possibility is that relatively large amounts of noise in long term interest rates attenuate information effects. Hansen et al. (2019) show that information signals driving movements in short-term and long-term interest rates are different. They find that news about economic uncertainty can have a larger impact on long-term yields rather than short-term yields.

We conclude by inputting data in tight windows around FOMC announcements, as in NS, into our estimation procedure. This includes data on the expected 3-month eurodollar interest rates with horizons of 2 to 4 quarters, the current month Fed funds futures rate and the Fed funds futures rate immediately following the next FOMC announcement.[26] The “Tight(NS) shock” generated in this way has a correlation of 0.36 with the BRW shock (Table 2, column 12). The information effect regressions of Table 4 indicate that a positive shock to this series is unrelated to changes in the Blue Chip real GDP growth rate forecast (column 11). What happens when we expand the NS data set to include longer horizon maturities? The “Tight(full) shock” is generated with our PLS estimation procedure but with the NS data expanded to further include the expected 3-month eurodollar interest rates with horizons of 1 to 8 quarters and on-the-run Treasury rates of 3 months, 6 months, 2 years, 10 years and 30 years. Using this expanded data increases the correlation with the BRW shock up to 0.54 (Table 2, column 13). Again, the information effect is absent from this Fama-MacBeth aligned shock (Table 4, column 12). This confirms the importance of using the Fama-Macbeth procedure in accounting for differences in results on the information effect in monetary policy shock series.

5. Impulse responses

The literature has offered the information effect as one reason why responses to monetary policy shocks could have signs that differ from traditional theory. In this section, we show that shocks to our measure display conventionally-signed responses. We first estimate a standard monthly VAR model. Following Romer and Romer (2004), we place our cumulative shock series first, thus allowing our monetary policy shock to contemporaneously affect all variables: output, inflation, commodity prices (CP), and excess bond premium (EBP).[27] We include CP in light of the “price puzzle” (CEE, 1996), though results are robust without (see online appendix Figure A.6), and the EBP because of its ability to explain business cycles (Gilchrist and Zakrajsek, 2012) and as an indicator of the price of risk Creal and Wu (2016).[28] In light of standard concerns about mis-specification in VARs, we also estimate the IRFs using Jordá (2005) local projections.[29]

Conventionally-signed IRFs using our shock

Panel a of Fig. 4 presents impulse responses to a contractionary monetary shock using the full sample (1994–2017). Here and throughout we normalize to a 100 basis point positive monetary policy shock on impact. The 68% and 90% standard error confidence intervals, displayed as deep and shallow gray areas respectively, are generated by the bootstrap. Both output and inflation decrease after a monetary policy tightening. The responses trough after about 10 months in the VAR and 12 months with LP. The EBP increases and peaks after about 8 months under both estimation strategies. These results are conventional, in line with Gertler and Karadi (2015), for example. Panel b of Fig. 4 shows similar results when the impulse responses are estimated after 2008. Output and inflation significantly decrease for the first 10–15 months after a policy tightening, while the EBP increases significantly.[30] Thus, responses to BRW shocks are conventional and stable across the ZLB sub-period.

We check robustness to subtracting from the raw interest rates the corresponding term premium on the 2-year Treasury rate and all zero-coupon yields with 1 to 10-year maturity, as estimated by Adrian et al. (2013). We then reconstruct our monetary policy shock series excluding the term premium in this way. Inserting the cumulative values of that series into the baseline VAR model, we find that the impulse responses are quantitatively identical to the baseline results of Fig. 4, although the negative effect on IP is dampened for the first few months (see online appendix Figure A.7). As shown in column 9 of Table 2, the correlation between the term-premium free shock and our baseline shock is high, 0.81.

[26] We use 4-quarter ahead 3 month eurodollar to normalize the shock.

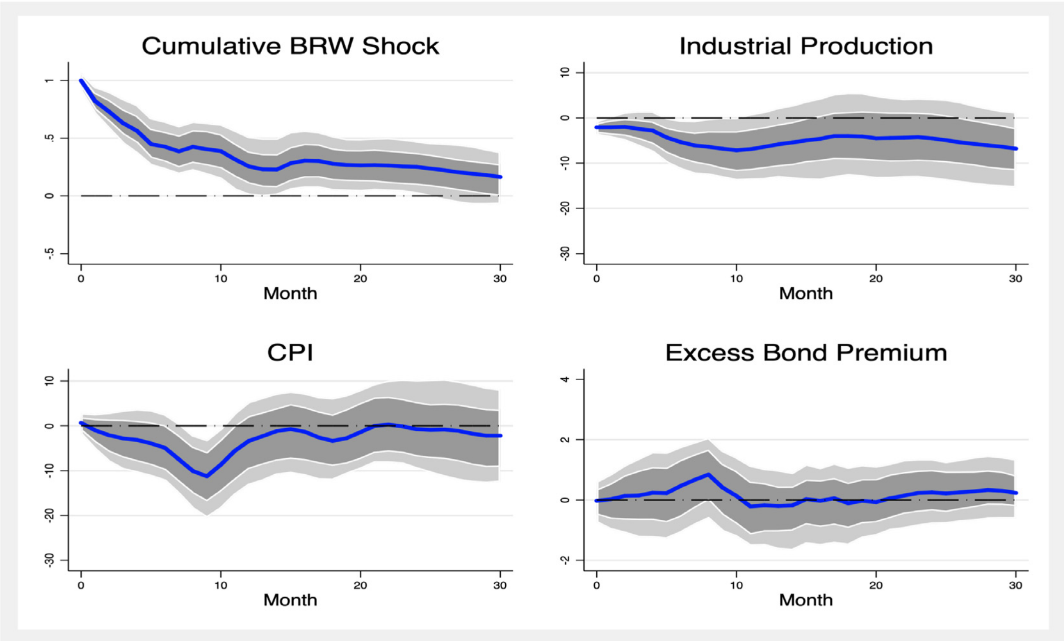
[27] This also follows Romer and Romer. Our series and theirs are plausibly exogenous, given how they are constructed.

[28] We also examine systems with the 5-year interest rate as an additional variable in the VAR model. These generate similar impulse responses.

[29] Again this follows Romer and Romer (2004), who estimate a VAR with cumulative monetary policy shocks and also estimate a version of local projections. We smooth the impulse response functions of local projections using a simple moving average. Results are similar if we employ the Smooth Local Projections developed by Barnichon and Brownlees (2019).

[30] Estimates from the pre-2008 sub-sample are highly similar and omitted for brevity.

a. 1994:01 to 2017:12
Structural VAR



Local Projection

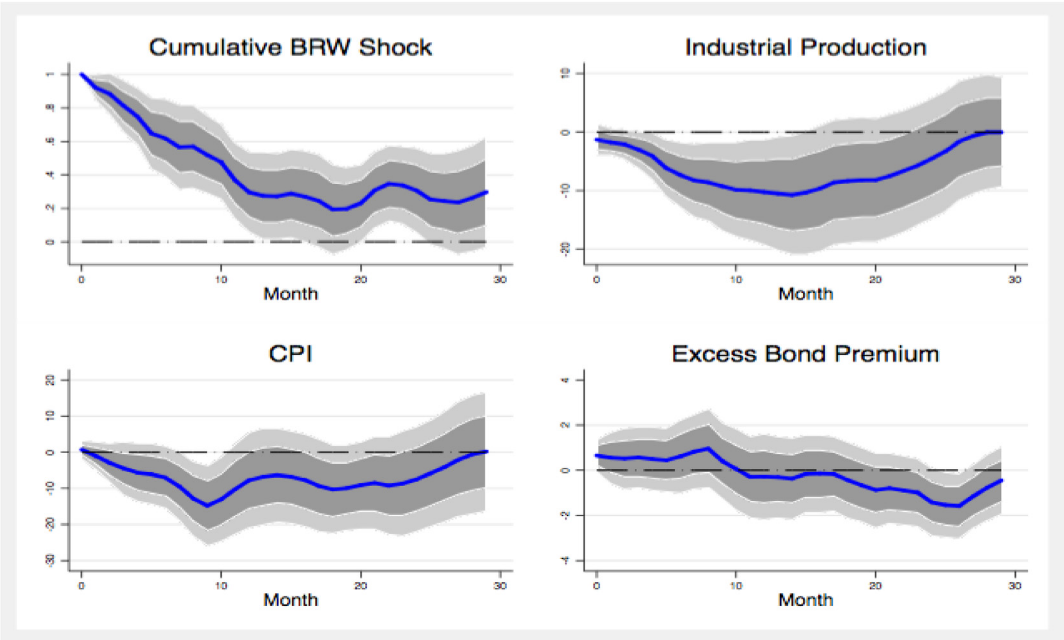
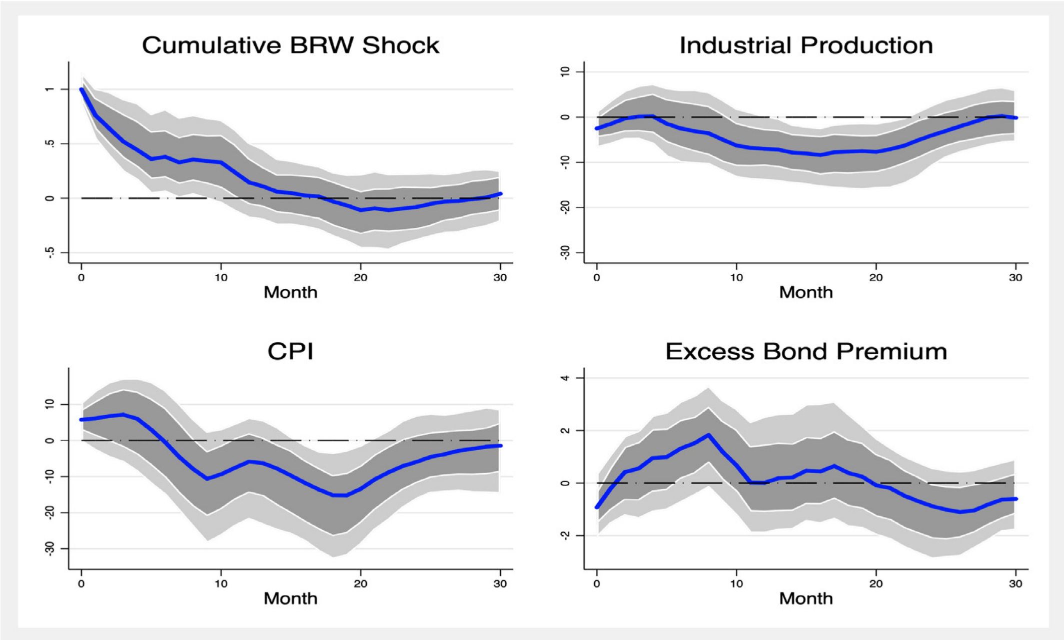


Fig. 4. BRW Shock Series Impulse Responses: (a) Note: The upper (lower) panel is IRFs of industrial production from the VAR (Local Projections) with monthly data using 12 lags. Variables are ordered: cumulative BRW shock series, log IP, log CPI, log commodity prices, and EBP. Impulse responses to a 100 basis point increase in the cumulative BRW shock series. Deep and shallow gray shaded areas are 68% and 90% confidence intervals produced by bootstrapping 1000 times, respectively.; (b) Note: The upper (lower) panel is IRFs of industrial production from the VAR (Local Projections) with monthly data using 12 lags. Variables are ordered: cumulative BRW shock series, log IP, log CPI, log commodity prices, and EBP. Impulse responses to a 100 basis point increase in the cumulative BRW shock series. Deep and shallow gray shaded areas are 68% and 90% confidence intervals produced by bootstrapping 1000 times, respectively.

b. 2008:01 to 2017:12
Structural VAR



Local Projection

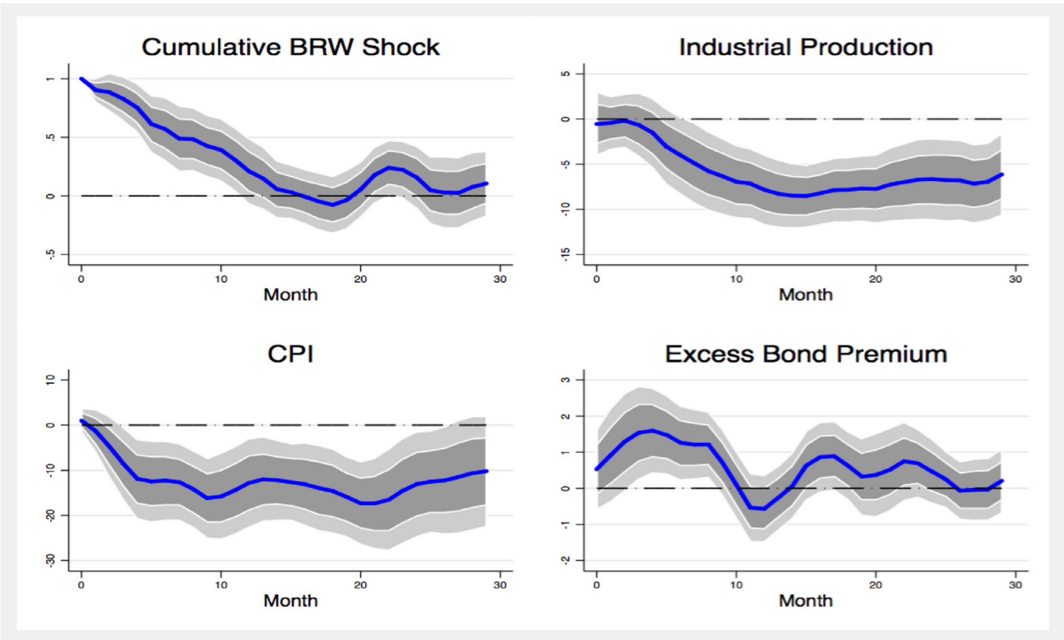
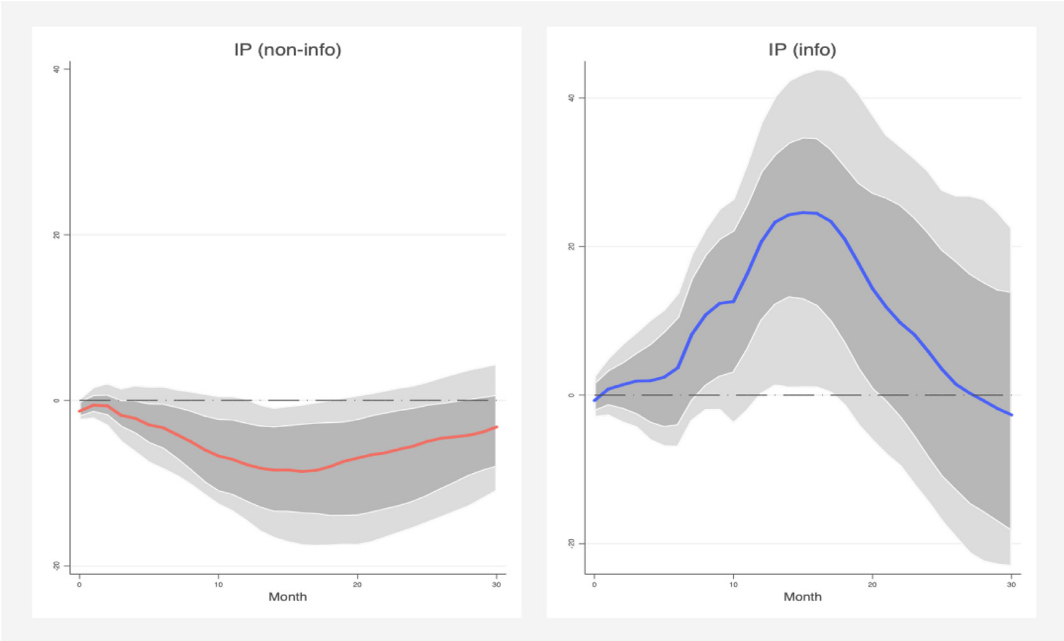


Fig. 4. Continued

a. Using the Jarocinski-Karadi FF4 Shock
Structural VAR



Local Projection

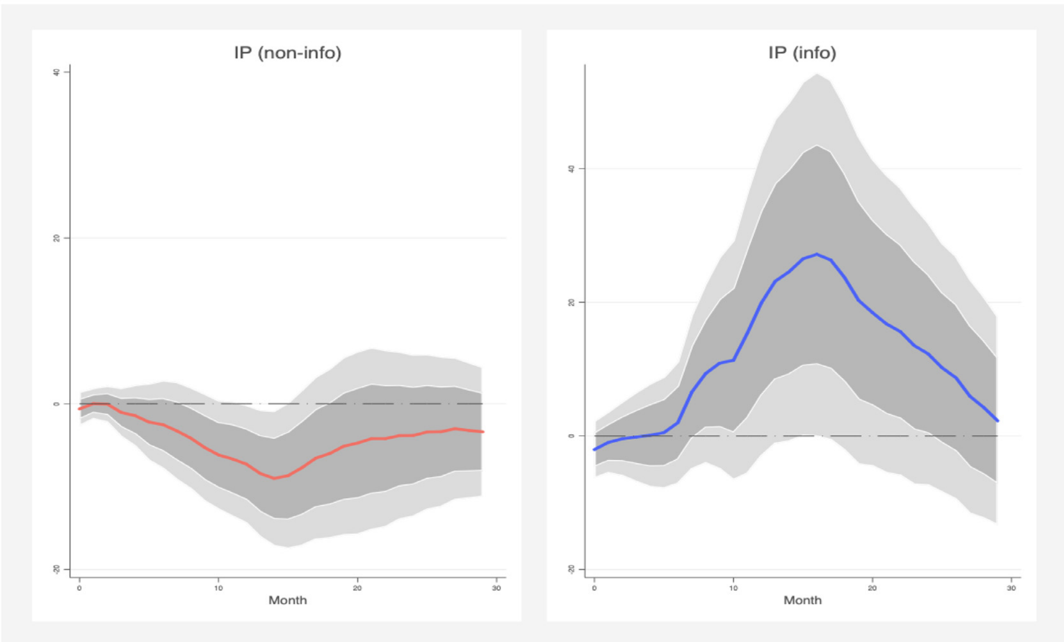
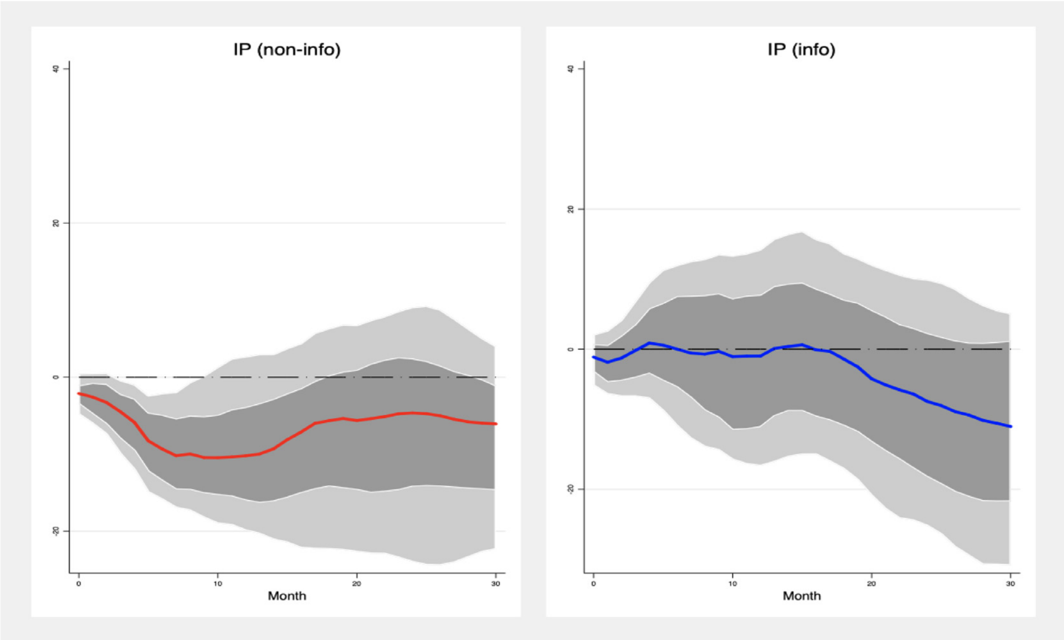


Fig. 5. Industrial Production Responses on Non-information Effect Days (red) and Information Effect Days (blue). (a) Note: Sample is 1994m1-2017m12. The upper (lower) panel uses the VAR (Local Projections). The Jarocinski-Karadi shock is the accumulated FF(4) change in the 30-minute FOMC announcement window. The 68% and 90% confidence intervals are produced by bootstrapping 1000 times. (b) Note: Sample is 1994m1-2017m12. The upper (lower) panel uses the VAR (Local Projections). The 68% and 90% confidence intervals are produced by bootstrapping 1000 times. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

b. Using the BRW Shock
Structural VAR



Local Projection

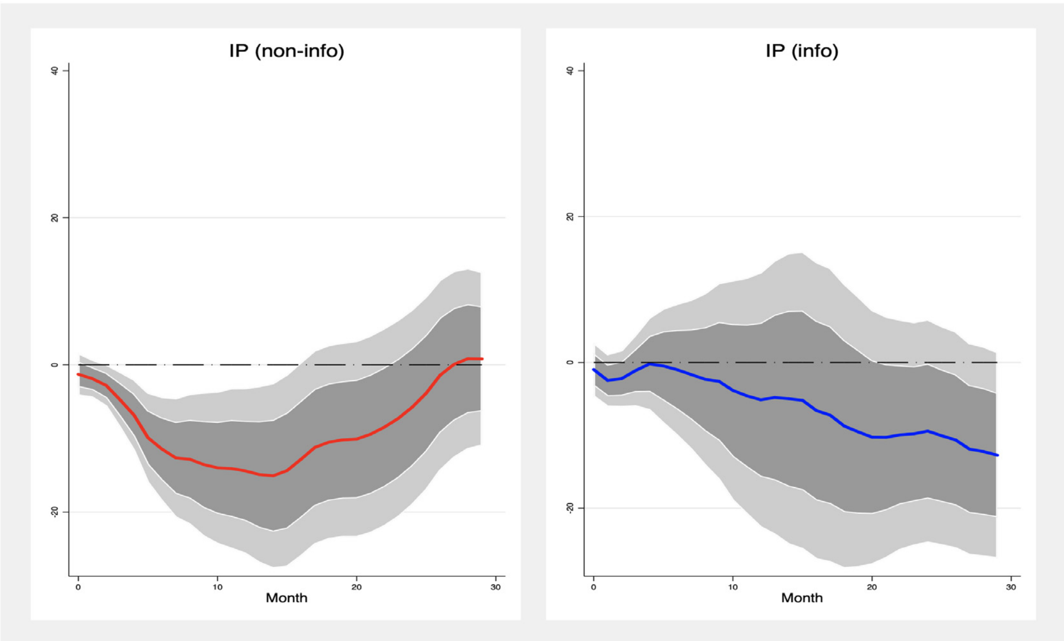


Fig. 5. Continued

Finally, we provide evidence demonstrating robustness to constructing our shock series with instantaneous forward rates (BRW_FR). In online appendix Figure A.5, we show that there are conventionally-signed and significant impulse responses to BRW_FR from the VAR model. For the ZLB subsample, it takes about 15 months for the shock to have a significant effect on IP, a bit longer than in the baseline. As seen in the online appendix Table A.2, BRW_FR contains no significant information effects, according to the NS tests.

Conditioning on FOMC announcement day stock market responses

As noted above, Jarocinski and Karadi (2018) argue that the information effect is empirically important by showing in a VAR that output responds with significantly different signs to a monetary policy shock compared to the shock conditioned on stock prices and interest rates co-moving positively, which they label central bank information shocks. In Fig. 5, we replicate the JK results, using both a VAR and local projections, and examine robustness to using the BRW shocks. The upper panel, Fig. 5a, displays results using the JK surprise FF4, while the lower panels of Fig. 5b use our measure of the monetary policy shock. We focus on the responses of IP in order to save space, but display the full set of results in the online appendix (Figure A.8 and Figure A.9). The left columns display VAR impulse responses while the right columns are estimated from local projections. Responses to a monetary tightening on non-information effect days, points in the second and fourth quadrants of Fig. 3, are shown in red and responses on information effect days are in blue.[31]

Consider Fig. 5a first, results with the JK monetary policy surprise. On non-information effect days, the top row, the impulse responses exhibit traditional signs: output falls in response to a monetary contraction. On information-effect days, however, the impulse responses depicted in the second row (in blue) are significantly different, with the transmission effect changing signs. This result holds equally well using Jorda's local projections estimator, as shown in the right columns. Now consider the same experiment but replacing the JK monetary policy shock with ours, as in Fig. 5b. Transmission to output (and prices and EBP; available online) exhibit conventional signs, both on information effect days and non-information effect days, although the response is weakened on information effect days to become insignificantly different from zero. These results hold using VARs and local projections. Recalling Table 4, this is consistent with there being no detectable evidence of an information effect, in the sense of NS, in the BRW series even on JK information effect days.

6. Conclusion

The question of how monetary policy affects the economy has long been a focus of research and is of course important to central banks. As is well known, identification is difficult, hampered by the endogeneity of monetary policy and macroeconomic aggregates like GDP. The properties of the identified monetary policy shocks commonly used in the literature have come under scrutiny in important recent work. This new research calls into question the assertion that surprises are capturing exogenous changes in monetary policy, even if measured in narrow windows around central bank policy announcements when very little other economic news is revealed. As these papers document, traditional measures of monetary policy shocks are predictable and/or have properties consistent with the central bank information effect.

In this paper, we derive a new U.S. monetary policy shock series that stably bridges periods of conventional and unconventional policy, is relatively unpredictable, has plausible effects on the yield curve, and has no significant information effect. Our series has very mild data requirements and is easily applied to many countries and historical episodes. It is a summary measure of all monetary policy actions on FOMC announcement days. We also construct real-time versions of our series. These too have properties consistent with forward guidance aspects to monetary policy and contain no evidence of the information effect. We demonstrate that our approach extracts a monetary policy shock without significant Fed information effects as long as either of two conditions holds: (1) information effects in long-term interest rates are very small, something that we document empirically; or (2) information effects in short and long yields are present but they are different, as in Hansen et al. (2019). In a macroeconomic context, we find that in response to tightening shocks to our new measure, output and prices fall significantly, consistent with conventional theory. Our new shock measure thus should be highly useful for guiding empirical work and quantitative theoretical modeling of the effects of Fed monetary policy.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jmoneco.2020.11.002.

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[31] We use all available VAR data in these experiments, and simply set shocks on the other days to zero. This is equivalent to the second estimation procedure used by Jarocinski and Karadi, labelled "poor man sign restrictions."

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