The Liquidity State-Dependence of Monetary Policy Transmission*

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COMMENTS WELCOME

Abstract

The large reactions of long-term government bond yields to monetary policy shocks occur during periods of higher market liquidity, and there is very little reaction during periods of lower liquidity. This newly documented liquidity state-dependence persistently affects real yields, term premia as well as long-term mortgage rates. Conditioning on market liquidity yields stronger state-dependence than simply conditioning on macroeconomic indicators. Balance sheet constraints on both hedge funds and dealers contribute to the liquidity state-dependence. In addition to using publicly observable time-series data, we also exploit a unique, granular dataset which covers virtually all secondary-market trades of US Treasuries executed in London, and contains detailed information on each transaction including the identities of both counterparties. Consistent with our baseline results, we find that arbitrage activity is significantly higher when FOMC meetings occur during periods of higher market liquidity. Overall, our results underscore the importance of market functioning, and the financial health of key intermediaries that support it, for implementing stabilisation policies.

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1 Introduction

How does liquidity in government bond markets affect the monetary policy transmission? The importance of this question is underscored by recent liquidity problems observed even in the most developed government bond markets, coinciding with substantial challenges faced by monetary policy in achieving inflation targets. While government bond markets are the backbone of the financial system and vital for the implementation of monetary policy, the role played by these markets' liquidity conditions in shaping the efficacy of monetary policy remains incompletely understood.

Our paper fills this gap by estimating the role of bond market liquidity in the transmission of monetary policy shocks to government bond yields. Using various standard measures of monetary policy shocks and market liquidity for the US, our empirical analysis yields five main results. First, monetary policy shocks have larger effects on bond yields when market liquidity is higher. We refer to this interaction between market liquidity and monetary policy as the liquidity state-dependence of the monetary policy transmission. We find that the amplified response of bond yields persists to longer maturities and is almost entirely driven by the real component. Second, the liquidity state-dependence is driven by the real term premium component, and not by changing expectations about future short-term interest rates. Third, we analyse the mechanism underlying the liquidity state-dependence and find that the balance sheet constraints both on hedge funds and dealers independently contribute to our baseline results, but hedge fund constraints are more influential in driving effects on long-maturities. Fourth, conditioning on market liquidity yields stronger state-dependence than simply conditioning on macroeconomic conditions. Importantly, liquidity state-dependence is particularly strong during economic downturns, underscoring the importance of market functioning for implementing stabilisation policies. Fifth, our baseline results are similar for the UK, highlighting the significance and generalisability of liquidity state-dependence in shaping the monetary policy transmission.

To arrive at these results, we employ a two-pronged approach using standard time-series regressions (as used in the macro-finance literature) as well as a granular, transaction-level dataset (as used in the market micro-structure literature). The first approach uses time-series regressions to estimate the impact of monetary policy shocks on daily changes in bond yields. As standard in the literature, we calculate these shocks using high-frequency interest rate changes surrounding FOMC announcements (Nakamura and Steinsson, 2018; Swanson, 2021; Bauer and Swanson, 2023b). We use daily changes in 1-year instantaneous forward rates up to 20 years, examining nominal yields, real yields, and inflation compensation components, thus providing a comprehensive view of yield curve dynamics. As our baseline measure bond market liquidity, we utilise the yield curve fitting error series, referred to as yield curve 'noise', proposed by Hu, Pan, and Wang (2013). By partitioning the data into high and low noise days, we effectively distinguish between periods of low and high market liquidity, enabling a detailed assessment of yield curve reactions to monetary policy shocks. High (low) yield curve noise indicates low (high) market liquidity, as there are large (small) discrepancies between the price of bonds with similar maturities that are otherwise close to perfect substitutes. The second empirical approach uses a unique, transaction-level dataset on the US Treasury market which allows us to investigate in detail how different market participants (such as hedge funds, pension funds, foreign entities, dealers etc.) actually trade around FOMC meetings. We find that arbitrage activity is significantly larger when FOMC meetings occur during periods of higher market liquidity, consistent with our time-series evidence on the liquidity state-dependence.

Our results suggest that the puzzling degree of non-neutrality of monetary policy, reflected in significant changes in real yields far into the future, is stronger than previously documented when we focus on liquid markets. For example, a shock that increases the 1-year nominal yield by 100 bps, increases the 10-year nominal yield by 38 bps in the baseline sample (Nakamura and Steinsson, 2018). We show that this effect is increased to 124 bps (driven entirely by the real interest rate components) when the shock hits during periods of higher market liquidity, and the effect is virtually zero during periods of lower market liquidity. Moreover, the liquidity state-dependence is driven by variations in term premia, supporting the growing focus on risk premium effects of monetary policy (Bauer, Bernanke, and Milstein, 2023; Kashyap and Stein, 2023).

There are at least two contrasting hypotheses regarding how market liquidity might shape the monetary policy transmission, which depend on the nature or underlying driver of liquidity. First, one could think of liquidity in the classical market microstructure sense where illiquid markets are characterised by larger price impact because of asymmetric information and adverse selection (Kyle, 1985). Given that asymmetric information tends to increase around macroeconomic announcements (Green, 2004), the impact of (monetary policy) shocks could be higher when markets are less liquid. Second, one could think of liquidity through the lens of intermediary asset pricing and limits to arbitrage models and expect the impact to be less significant in less liquid markets because price discovery is impaired due to constraints on arbitrageurs (Vayanos and Vila, 2021; Kekre, Lenel, and Mainardi, 2022) or dealers (Adrian, Etula, and Muir, 2014; He, Kelly, and Manela, 2017).[1] Our empirical results provide ample support for the second hypothesis.

We directly investigate the mechanism underlying the liquidity state-dependence, we find evidence in support of the predictions of the intermediary asset pricing and limits to arbitrage models, where dealers' and arbitrageurs' balance sheets are a key determinant of risk premia and market conditions. To show that, we proceed in two steps. First, we establish the empirical link be-

[1]Most relevant to our empirical framework is the seminal model of Vayanos and Vila (2021), and the recent generalization of Kekre, Lenel, and Mainardi (2022), who offer a rationalisation of the role of arbitrage capital for yield curve noise and liquidity state-dependence, as well as the existence of a term premium effect. In Vayanos and Vila (2021), when arbitrageurs' capital is sufficiently high, they transmit shocks to short-term interest rates along the yield curve, ensuring a smooth curve. We interpret the low noise periods as periods in which arbitrageurs' wealth is high, and hence their role in transmitting short-rate shocks to longer bond yields and smoothing the yield curve is not impaired.

tween these two factors and our baseline yield curve noise measure, by regressing monthly changes in noise on measures of intermediary capital (He, Kelly, and Manela, 2017) and past returns of fixed-income arbitrage hedge funds (Siriwardane, Sunderam, and Wallen, 2022).[2] Past hedge fund returns appear to be the single most important determinant of noise (with an adjusted R^2 of 35-37% from univariate regressions) whereas the effect of intermediary capital is also significant (with an adjusted R^2 of 16-18% from univariate regressions).[3] Second, we consider the impact of both of these two conditioning variables on the monetary policy transmission, and find that both measures make independent and statistically significant contributions to the state-dependent effects of monetary policy.

Our empirical findings have important policy implications. The liquidity state-dependence (which appears to be quantitatively stronger than simply conditioning on macroeconomic indicators) speaks to the coordination between monetary policy and financial policy which has received increasing attention in the policy debate. It is well known that a liquid and well-functioning government bond market hugely benefits society by facilitating government financing, providing safe assets and determining the risk-free benchmark for all other borrowing rates in the economy. However, more recent arguments point to a more nuanced *interaction* whereby market functioning itself can shape macroeconomic stabilisation policies (Duffie and Keane 2023). Our paper provides evidence related to this interaction: while we have mixed results regarding the role of market liquidity in affecting the efficacy of monetary policy in favourable economic environments, liquidity is a robust and crucial determinant of the monetary policy transmission in depressed economic conditions.

Related Literature Our paper is closely related to a large literature quantifying the effects of monetary policy on asset prices. Most related to our paper are Cochrane and Piazzesi (2002), Gürkaynak, Sack, and Swanson (2005b), Hanson and Stein (2015) and Nakamura and Steinsson (2018), who focus on the response of government bond yield curve to monetary policy shocks.[4] Cochrane and Piazzesi (2002) first documented the surprisingly large response of long term bond yields to monetary policy shocks identified with high-frequency data. Hanson and Stein (2015) and Nakamura and Steinsson (2018) further document a large response in real yields that slowly decays with maturity, implying a significant amount of non-neutrality that is at odds with textbook macroeconomic models.[5] Our contribution to this literature is to show that the response of long

[2]We also consider other determinants of the noise measure during this exercise.

[3]To our knowledge, the empirical result pertaining to the quantitatively important role of past hedge fund returns in explaining the noise measure of Hu, Pan, and Wang (2013) is also novel in the literature.

[4]This literature builds on earlier work of Kuttner (2001) using high-frequency responses of interest rates to identify monetary policy shocks, which was also used to study the reaction of equities in Bernanke and Kuttner (2005).

[5]Hanson and Stein (2015) emphasise the role of key financial intermediaries in affecting the risk premium component of yields, while Nakamura and Steinsson (2018) emphasise a Fed information effect that works through the expectation components of yields.

term bond yields mainly depends on market liquidity, driven primarily by arbitrageurs' balance sheet health.

Our evidence pertaining to a strong term premium component in the liquidity state-dependence of monetary policy transmission is related to the growing literature on the term premium effects of monetary policy (Abrahams, Adrian, Crump, Moench, and Yu, 2016; Kekre, Lenel, and Mainardi, 2022; Hanson, Lucca, and Wright, 2021; Bauer, Bernanke, and Milstein, 2023). Our results also reveal a significant expectation component at long horizons, also consistent with learning (Hillenbrand, 2020) or an information effect (Nakamura and Steinsson, 2018). The presence of both effects is consistent with models featuring limits to arbitrage and segmentation such as Vayanos and Vila (2021); Kekre, Lenel, and Mainardi (2022). Our results picture a nuanced view on the role of term premium and expectation components: the term premium response can be large (as argued by Hanson and Stein (2015)), but these responses manifest exclusively during periods of higher market liquidity.

More generally, our paper is connected with the expanding literature on intermediary asset pricing (He and Krishnamurthy, 2013; Brunnermeier and Sannikov, 2014; Du, Tepper, and Verdelhan, 2018; Andersen, Duffie, and Song, 2019; Fleckenstein, Longstaff, and Van Nieuwerburgh, 2020; Cenedese, Corte, and Wang, 2021; Infante, Favara, and Rezende, 2020; Du, Hebert, and Huber, 2022). Our contribution to this literature is to provide a detailed empirical analysis on how constraints on financial intermediation affect the monetary policy transmission to the yield curve.

Our results also relate to the macroeconomic literature on the state-dependence of monetary policy, showing that monetary policy has a weaker effect during recessions than in expansions (Tenreyro and Thwaites, 2016; Jorda, Schularick, and Taylor, 2020; Alpanda, Granziera, and Zubairy, 2021; Eichenbaum, Rebelo, and Wong, 2022; Li, 2022). While macroeconomic conditions are correlated with measures of market liquidity, we show that market liquidity carries independent information that is not captured by business cycle conditions.

The remainder of the paper is organized as follows. Section 2 describes the data used in this paper; Section 3 presents the main results documenting state-dependence in the monetary policy transmission to bond yields; Section 4 tries to disentangle the economic factors that explain the state-dependence; Section 5 provides extensions to our baseline empirical design; Section 6 presents various robustness checks; Section 7 provide evidence from transaction-level data from the US treasury market; Section 8 concludes.

2 Data

Our baseline analysis uses publicly available aggregate time-series for the US, summarised as follows.

Bond Yields We use daily nominal yields from Gurkaynak, Sack, and Wright (2007), and real and inflation compensation components from Gurkaynak, Sack, and Wright (2010) available from the Federal Reserve Board.[6] Availability of real yields is the main constraint on the starting date, which is why all of our analysis is from 2000 onwards. We use 1-year instantaneous forward rates spanning the first 20 years of maturities.

Monetary Policy Shocks We use the high-frequency measure of monetary policy surprises of Nakamura and Steinsson (2018), which uses the first principal component of the unanticipated change over the 30-minute window around scheduled FOMC announcements in various interest rates.[7] We extend this shock series (originally covering the period from 1/1/2000 to 3/19/2014) to cover the more recent period, using the data provided by Acosta (2022) who extends the sample to 2019.

In addition, we consider three alternative monetary policy shock series as robustness checks. First, we employ the series proposed by Jarocinski and Karadi (2020) (also used subsequently by Kekre, Lenel, and Mainardi (2022)), which uses a structural vector autoregression model to disentangle variation in monetary policy shocks caused by policy changes from that caused by central bank information. Second, we use the monetary policy surprise measure of Bauer and Swanson (2023b), which substantially expands the set of monetary policy announcement events (to include press conferences, speeches etc.) and removes the component of the monetary policy surprises that is correlated with economic and financial data. Third, we also consider the shocks computed by Swanson (2021) that uses principal component analysis to decompose monetary policy surprises into a short factor, a path factor and a long-term factor.

Market Liquidity Proxies We consider the yield curve noise measure of Hu, Pan, and Wang (2013) as our baseline proxy for liquidity conditions in the US Treasury market.[8] This is motivated by the fact that this series is one of the most frequently used measures of treasury market liquidity in the recent literature.[9]

[Figure 1]

[6]The data can be downloaded from the Federal Reserve website. We use the original dataset of Nakamura and Steinsson (2018) (available in their replication package on this website) in our baseline analysis to increase comparability with their results. In the different extensions we subsequently consider, we extend their original dataset with more recent data. However, this approach has the drawback that the data vintage for real interest rates used in Nakamura and Steinsson (2018) differs from the most recent vintage available on the website of the Federal Reserve Board due to subsequent revisions to the underlying methodology. We assess the robustness of our results to the choice of different data vintages in Appendix A.1.

[7]Specifically, Nakamura and Steinsson (2018) uses the following five interest rates: the Fed funds rate immediately following the FOMC meeting, the expected Fed funds rate immediately following the next FOMC meeting, and expected three-month eurodollar interest rates at horizons of two, three, and four quarters.

[8]Fontaine and Garcia (2012) also study yield curve noise as a proxy for liquidity supply.

[9]See Vogt, Fleming, Shachar, and Adrian (2017); Foucault, Kozhan, and Tham (2017); Duffie (2020); Goldberg (2020); Goldberg and Nozawa (2021); Boyarchenko, Crump, Kovner, and Shachar (2021) among many others.

As shown in Figure 1, the noise measure spiked during the 2008-2009 financial crisis. We follow Nakamura and Steinsson (2018) and drop the period spanning the height of the financial crisis in the second half of 2008 and the first half of 2009. Given the visible trend in the noise series, we also check in Appendix A.1 that our results are robust to detrending the series. To proxy the mechanisms underlying the noise measure, we use the intermediary leverage measure of He, Kelly, and Manela (2017) and past returns of fixed-income arbitrage hedge funds as in Siriwardane, Sunderam, and Wallen (2022).

As robustness, we also experiment with alternative measures of liquidity, including the VIX index (Adrian and Shin, 2010; Nagel, 2012; Goldberg, 2020; Goldberg and Nozawa, 2021) as well as the T-Bill Eurodollar (TED) spread (Garleanu and Pedersen, 2011; Friewald and Nagler, 2019; Goldberg and Nozawa, 2021) that have been widely used as proxies for market liquidity conditions.

3 Empirical Analysis

To measure the transmission of monetary policy, we start with the following baseline regression specification:

$$\Delta f^i_{t,\tau} = \alpha + \gamma^i_{all,\tau} \Delta mps_t + \varepsilon_t, \tag{1}$$

where t is the date of scheduled FOMC announcements, $\Delta f^i_{t,\tau}$ is the daily change in the forward rate of denomination $i \in \{n, r, \pi\}$ (for nominal, real and inflation, respectively) and maturity τ, and Δmps_t is the high-frequency monetary policy surprise identified in a narrow window around the FOMC announcements. For example, the parameters $\gamma^n_{all,\tau}$ for $\tau \in (2, 20)$ trace out the estimated impact of the monetary policy shocks Δmps_t on the nominal forward yield curve from the 2-year to 20-year horizon. We follow Nakamura and Steinsson (2018) and scale Δmps_t such that the effect on the 1-year nominal Treasury yield is 100 bps ($\gamma^n_{1,1} = 1$).

Our goal is to assess whether market liquidity conditions around announcements systematically impact the transmission of monetary policy shocks to nominal yield curve, and its real and inflation components, at different maturities. To capture liquidity state-dependence we interact the monetary policy surprise with indicator functions capturing whether our proxy for market liquidity is above (HighLiq_{t-1}) or below its median value (LowLiq_{t-1}) prior to a given FOMC announcement. This yields the regression:

$$\Delta f^i_{t,\tau} = \alpha + \gamma^i_{hl,\tau} \cdot [\Delta mps_t \times \text{HighLiq}_{t-1}] + \gamma^i_{ll,\tau} \cdot [\Delta mps_t \times \text{LowLiq}_{t-1}] + \varepsilon_t, \tag{2}$$

Our parameters of interest, $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$, are therefore estimated on the same number of announcements. Each coefficient measures the degree of monetary policy transmission to bond yields under different market liquidity conditions. This setting nests the baseline specification (1) in which the impact of monetary policy shocks is assumed constant over the entire estimation sample, corresponding to the coefficient restriction $\gamma^i_{hl,\tau} = \gamma^i_{ll,\tau}$.

To quantify the economic and statistical significance of the state-dependence (the difference in estimates in days of high vs. lower liquidity) we also estimate the related specification:

$$\Delta f^i_{t,\tau} = \alpha + \gamma^i_{ll,\tau} \Delta mps_t + \gamma^i_{h-l,\tau} \cdot [\Delta mps_t \times \text{HighLiq}_{t-1}] + \varepsilon_t, \tag{3}$$

where $\gamma^i_{h-l,\tau}$ captures the incremental reaction of bond yields to monetary policy shocks on days of higher liquidity on forward i of maturity τ. Section 6.4 will provide a detailed analysis on the statistical significance of the state-dependence using our baseline liquidity measure as well as alternative measures of liquidity and monetary policy shock series.

3.1 Baseline Results

As our baseline specification we replicate Nakamura and Steinsson (2018), using their measure of monetary policy shocks and their sample choice. We estimate regressions (1) and (2) by OLS for each forward maturity separately. We follow their convention of restricting the sample to exclude all announcements taking place between July 2008 and June 2009 to ensure that our results are not driven by anomalous liquidity conditions in the Treasury market during the height of the Global Financial Crisis.

Liquidity State-Dependence in Nakamura and Steinsson (2018) As a starting point, we reproduce Table 1 of Nakamura and Steinsson (2018) so that we can transparently demonstrate how the baseline coefficients change as we split the sample into low-noise and high-noise days (equation 2). Table 1 summarises the results.

[Table 1]

The policy news shock is scaled such that the effect on the one-year Treasury yield is 100 bps. Looking across different maturities, the average effect of the shock is somewhat smaller for shorter maturities, peaks at 110 bps for the 2-year nominal yield and then declines monotonically to 38 bps for the 10-year nominal yield. When looking across the columns, we find that the effect on 10-year yields is 124 bps (driven entirely by the real component) when the shock hits during periods of higher market liquidity, and the effect is virtually zero during periods of lower market liquidity. We find strong liquidity state-dependence of the real rate when we consider forward rates as well. For example, Nakamura and Steinsson (2018) finds that the average effect on the 10-year forward is insignificant (-0.08), but we find that the effect turns significantly positive (0.58) during periods of higher liquidity.

The Effects on Longer Maturity Forwards In what follows, we only depart from Nakamura and Steinsson (2018) by focusing on forward rates and extending the maturity breakdown of bond yields by including all the 1-year instantaneous forwards from the 2-year forward up to 20 years.[10] Figure 2 presents the results for the sample from January 2000 to March 2014 used in Nakamura and Steinsson (2018). The first column shows the estimates of $\gamma^i_{all,\tau}$ from specification (1) for forward rates with maturities $\tau \in (2, 20)$. The state-dependent parameters estimated using regression (2) shown in columns two ($\gamma^i_{hl,\tau}$, corresponding to FOMC announcements on higher liquidity/low noise days) and three ($\gamma^i_{ll,\tau}$, corresponding to FOMC announcements on lower liquidity/high noise days), for $i \in \{n, r, \pi\}$. We estimate the regressions by OLS for each forward maturity τ separately.

[Figure 2]

The first row of Figure 2 plots the estimates of regressions (1) and (2) for nominal forward rates ($\gamma^n_{j,\tau}$), while the second row and third row display the corresponding estimates for the real ($\gamma^r_{j,\tau}$) and inflation ($\gamma^\pi_{j,\tau}$) forwards, respectively, for $j \in \{all, hl, ll\}$. In all charts in Figure 2, 90% confidence bands based on robust standard errors are shown around the point estimates for each maturity.

Estimates replicating the results in Nakamura and Steinsson (2018) using our finer forward maturity breakdown are shown in the first column of Figure 2, labeled 'Baseline'. The point estimates of the response of nominal forward yields to monetary policy shocks ($\gamma^n_{all,\tau}$) are shown in the top left chart. They are decreasing in maturity from the 2-year forward onwards, with only estimates for very short maturity forwards (2 and 3 years) statistically significantly different from zero. This highlights the benefit of using forwards instead of spot yields, as it clearly shows that the statistically significant response of 10-year spot yields is entirely driven by the reaction of very short-term forwards.[11]

The dominant role of real rates in explaining the positive response of the nominal bond yield curve to monetary policy shocks, which had been highlighted earlier (see Hanson and Stein (2015), Nakamura and Steinsson (2018) and Kekre, Lenel, and Mainardi (2022)), can be seen from the charts on the second and third row of the first column. The real forward curve reaction is positive for all maturities (though significant only through medium maturities), while the inflation forward curve reaction is negative (and statistically significant) except for very short-term maturities. Before considering the state-dependence, which is the focus here, we note that in the baseline specification of Nakamura and Steinsson (2018) the results are less significant for longer maturities than found in Hanson and Stein (2015).[12]

[10] This is the most complete breakdown possible in our dataset. Focusing on forwards on such an extended maturity spectrum provides a comprehensive account of yield curve dynamics, and allows greater comparability of our results with Hanson and Stein (2015) and Kekre, Lenel, and Mainardi (2022) as well.

[11] The spot yield in a N-year zero-coupon bond is simply the average of all 1-year forward yields from year 1 to N. As shown in Table 1, though the response of the 10-year spot yield to monetary policy shocks is positive and significant, the 10-year forward yield point estimate is actually negative, though statistically insignificant.

[12] We also find no U-shaped pattern in the coefficients, which is the focus of Kekre, Lenel, and Mainardi (2022). We return to these differences when considering alternative samples and different measures of monetary policy shocks.

The essence of our key results – the state-dependence in the response of the yield curve to monetary policy shocks – is illustrated by the second and third column of Figure 2. We find a larger nominal response to monetary policy shocks (first row, second column) in higher liquidity (low yield curve noise) days, and the state-dependence is almost entirely driven by the real component (second row, second column). The response of nominal forwards in higher liquidity days is even larger than the baseline estimates, and statistically significant through all forward maturities considered (first row, second column). The larger response of nominal yields in liquid markets is entirely driven by the larger and more significant real components (second row, second column), with an insignificant response of inflation forwards of all maturities (third row, second column). When we condition on lower liquidity (high yield curve noise) days, the response of nominal yields (first row, third column) is not statistically different from zero for short to medium forward maturities, and is even negative and statistically significant for forward maturities above 10 years, This is the result of an insignificant response of the real forward curve (second row, third column), combined with the increasingly negative response of the inflation component at longer horizons (third row, third column).

Our baseline results survive an extensive list of robustness checks – including different sample periods and different definitions and/or measurements for market liquidity and monetary policy shocks – which are summarized in Section 6. The main takeaway from these robustness exercises is that our key result on the liquidity state-dependence of monetary policy transmission is not driven by the data and sample period used in Nakamura and Steinsson (2018), by our choice of measure for market liquidity conditions, or by unconventional monetary policy operations carried out since the GFC.[13]

Role of Risk Premium vs Expectations We now turn to model decompositions of forwards into term premium and expectation components to determine what accounts for the state-dependence shown above. As our baseline, we employ the decomposition of Abrahams, Adrian, Crump, Moench, and Yu (2016), and we consider alternative decompositions (Kim and Wright, 2005; D'Amico, Kim, and Wei, 2018) in robustness Section 6.5.[14]

The state-dependence of the components of real forwards are shown in Figure 3, using the estimates of Abrahams, Adrian, Crump, Moench, and Yu (2016). The corresponding results for expectations and risk premia components of the nominal forwards are shown in Figure 89 of the appendix, and confirm that the state-dependence is driven by the real component.

[Figure 3]

[13]Most notably, our main result remains valid when considering an extended sample covering the period from 2000 to 2019 – particularly once the long-term downward trend in the noise measure is properly taken into account. See Section 6.1 and Appendix A.1 for further details.

[14]We thank Min Wei for sharing with us a finer 1-year instantaneous forward maturity decomposition than is available online, to allow us to match our regression design and compare with Abrahams, Adrian, Crump, Moench, and Yu (2016).

This clear role for the risk premium component is in line with the earlier results of Hanson and Stein (2015), and the interpretation of Kekre, Lenel, and Mainardi (2022),[15] but at odds with the results of Nakamura and Steinsson (2018). Here we show that there is a term premium effect using the high-frequency surprise measure of Nakamura and Steinsson (2018), but this is present only in higher liquidity states. Given the large response in nominal yields is predominantly driven by the real response in high-liquidity states, our results suggest the 'information effect' emphasized by Nakamura and Steinsson (2018) cannot be the only, or even main, mechanism at work. Kekre, Lenel, and Mainardi (2022) emphasises the effects on arbitrageur wealth in driving the positive effect of monetary policy surprises on real term premium – we explore this explanation in the following section.

4 Inspecting the Mechanism

In this section, we explore potential mechanisms which could rationalise our evidence on the link between poor liquidity conditions in the Treasury market and impaired transmission of monetary policy shocks. We build on the expanding literature highlighting the role played by primary dealers and hedge fund arbitrageurs (e.g. Barth and Kahn, 2021; Kruttli, Monin, Petrasek, and Watugala, 2023 and references above).

4.1 What Explains the Noise Measure?

To estimate the determinants of our main state variable, we regress monthly changes in the noise measure over the sample period from 2000 to March 2014 on the other widely used proxies for liquidity conditions introduced in Section 2.[16] We additionally consider the role of financial intermediaries' leverage (using the measure of He, Kelly, and Manela, 2017) as well as arbitrageurs' balance sheet health as proxied by Siriwardane, Sunderam, and Wallen (2022).[17]

[Table 2]

As shown by Table 2, all the variables used in the regression have a statistically significant relation with the noise measure when considered individually (columns 1 to 5). Higher volatility and increased cost of funding, respectively proxied by the VIX index and the TED spread, are

[15] Kekre, Lenel, and Mainardi (2022) do not directly test the role of term premium using real term premium estimates, instead they infer it must be driven by risk premium from the U-shaped response they find. We do not find evidence of U-shaped response, but find a direct term premium effect which is mainly present in higher liquidity states.

[16] See also Section 6.3 for evidence that our main result on the liquidity state-dependence of monetary policy transmission is robust to the use of these alternative liquidity measures.

[17] The measure of Siriwardane, Sunderam, and Wallen (2022) uses Barclay's hedge fund return indices. The rationale is that past negative returns would be associated with tighter balance sheet constraints on these investors. See also Greenwood and Vayanos (2014).

both positively associated with higher noise and a worsening in liquidity conditions in the Treasury market. Financial intermediaries' balance sheet constraints, captured by the leverage ratio measure, also have a positive relation with the noise measure. Likewise, lower hedge fund returns, which proxy for tighter balance sheet constraints for arbitrageurs, are associated with rising illiquidity in the Treasury market. Particularly, returns for fixed-income arbitrage (FIA) funds active in this market can explain the changes in the noise measure with an adjusted R^2 of 36.68%.

[Table 3]

The important role played by financial intermediaries and arbitrageurs is confirmed by the results of the multivariate regressions (columns 6 to 10). Intermediary leverage and FIA returns jointly explain 40% of the monthly variations in the noise measure. Other liquidity proxies such as the VIX and TED spread are no longer significant in regressions including proxies for balance sheet constraints of financial intermediaries and arbitrageurs. Moreover, their incremental contribution to the adjusted R^2 is negligible. Our results are confirmed in the extended sample ending in December 2019, although the share of monthly variations in the noise measure explained by the regressors decreases slightly (see Table 3).

In light of the importance of hedge fund returns in explaining yield curve noise, we revisit our baseline results on liquidity state-dependence by estimating a variant of regression (3) with FIA hedge fund returns used to sort our sample into higher and lower liquidity periods.

[Figure 4]

As shown by Figure 4, the majority of the monetary policy transmission to the long-term real forward rates is concentrated in periods with looser balance sheet constraints on hedge funds (middle column).

4.2 Conditioning on Primary Dealers and Hedge Fund Proxies

We now turn to disentangling the relative contributions of primary dealers and hedge funds to the transmission of monetary policy shocks. To that end, we first condition on the state of fixed-income arbitrage (FIA) returns (i.e. by taking the subset of FOMC announcements where FIA hedge funds have been experiencing below/above median returns), and consider the marginal impact of primary dealers' leverage on MP transmission.

Empirically, this amounts to applying a double sorting approach on the set of FOMC announcement dates whereby announcements are first sorted into two buckets depending on whether the FIA hedge fund returns are below/above median and, then, further dividing each bucket into two subgroups depending on whether the intermediary leverage measure of He, Kelly, and Manela (2017) is below/above median. We estimate an extended version of regression (2) on the four subsets of FOMC announcements, respectively, corresponding to "Low FIA returns/Low leverage", "Low FIA returns/High leverage", "High FIA returns/Low leverage", and "High FIA returns/High leverage". We consider the extended sample period from 01/2000 to 12/2019 in this part of the analysis to increase the number of observations in each subset of FOMC announcements.

[Figures 5–6]

Figure 5 focuses on FOMC announcements around which FIA hedge funds have been experiencing above-median returns and are thus less likely to be balance-sheet constrained. The first column reproduces the estimates of $\gamma^i_{hl,\tau}$ in regression (2) for the subset of scheduled FOMC announcements for which the FIA hedge fund return index is above its median level (see also the second column of Figure 4).

The second and third columns respectively report the estimates for the "High FIA returns/Low leverage" and "High FIA returns/High leverage". We observe that once conditioning on hedge funds being unconstrained in their risk-bearing capacity, the incremental impact on monetary policy transmission of dealers' balance sheet being unconstrained becomes negligible. However, there are strong signs of asymmetries in the interactions between hedge funds' and primary dealers' balance-sheet constraints. The third column of Figure 5 shows that unconstrained hedge funds might amplify monetary policy transmission far out in the yield curve (at maturities of 10-12 years) for both nominal and real forward rates when primary dealers are balance-sheet constrained. This result is suggestive of a beneficial role of liquidity provision played by hedge funds active in the Treasury market which supports monetary policy transmission when primary dealers have limited risk-bearing capacity.

Figure 6 shows the corresponding results for FOMC announcements around which FIA hedge funds have been experiencing below-median returns and are thus more likely to be balance-sheet constrained. While the second column indicates some mild attenuation of the impaired transmission of shocks when dealers' balance sheet is unconstrained, the third columns points to a sharp worsening of monetary policy transmission when dealers' balance-sheet constraints reinforce hedge funds' impaired risk-bearing capacity. We even record significant decreases in nominal and real forward rates at longer maturities (above 5 years) following a monetary policy tightening.

[Figures 7–8]

Figures 7 and 8 present the results respectively for the cases where primary dealers' are less (resp. more) likely to be balance-sheet constrained. The third column of Figure 7 shows that hedge funds being unconstrained has a negligible incremental impact on monetary policy transmission when conditioning on primary dealers being unconstrained (in line with the results above).

The results in the case where hedge funds' risk-bearing capacity is likely to be constrained (second column) are informative about the role played by each group of financial intermediaries in the transmission of shocks across the yield curve. While transmission in the short end of the yield curve (maturities below 5 years) is broadly unaffected by hedge funds' balance-sheet constraints for both nominal and real forward rates – albeit with more uncertainty around the estimates indicated by wider confidence bands – the transmission to longer-maturity real forward rates becomes ineffective (with corresponding impact on nominal rates). These results are indicative of a more important role played by primary dealers' risk-bearing capacity in shock transmission to short maturities and hedge funds playing a more important role in transmitting monetary policy shocks across the yield curve.

Figure 8 presents the results when dealers are constrained. The second column confirms our result of a worsening in monetary policy transmission when both dealers and hedge funds have impaired risk-bearing capacity. The third column confirms the beneficial role of liquidity provision played by hedge funds when dealers are balance-sheet constrained.

5 Extensions

In this section, we extend our baseline analysis to address a number of possible issues around our empirical design.

5.1 Does Liquidity State-Dependence Capture Recession State-Dependence?

Periods of low (high) market liquidity tend to coincide with macroeconomic recessions (expansions). Given that the macroeconomics literature has shown that monetary policy has a weaker effect during recessions than in expansions (Tenreyro and Thwaites, 2016; Jorda, Schularick, and Taylor, 2020; Alpanda, Granziera, and Zubairy, 2021; Eichenbaum, Rebelo, and Wong, 2022), a natural question is whether liquidity state-dependence is simply picking up recession state-dependence.

To address this issue, we estimate a variant of our baseline regression (2) whereby we replace the yield curve noise measure with the ISM manufacturing Purchasing Managers Index (PMI) – a timely indicator of US business cycle activity closely monitored by market participants (Berge and Jorda, 2011; Andreasen, Engsted, Moller, and Sander, 2021).[18]

[Figures 9–10]

Results in Figures 9 and 10 support the previous literature pointing to a less powerful transmission of monetary policy when economic conditions are depressed, which corresponds in our case to the PMI being below its median level (third column). We note that the results for nominal

[18] Berge and Jorda (2011) document that indices of economic activity such as the Chicago Fed National Activity Index (CFNAI) or the Philadelphia Fed ADS index (Aruoba, Diebold, and Scotti, 2009) have a higher accuracy than the PMI at classifying US economic activity into expansions and recessions. However, the CFNAI and ADS index provide model-based measures of economic activity subject to ex-post revisions. We therefore opt for the PMI as it is more likely to accurately reflect the information available to market participants about the state of the economy around FOMC announcements.

forward rates in the baseline sample indicate a less strong transmission of monetary policy shocks to longer maturities (above 5 years) in favourable economic conditions compared to our baseline results for low noise announcements dates.

[Figures 11]

To test whether the monetary policy transmission is significantly different during high and low PMI periods and whether this difference is larger compared to using our baseline noise measure as conditioning variable, Figure 11 presents the estimates of $\gamma^i_{h-l,\tau}$ in regression (3) for nominal and real forward rates in the baseline sample. For the PMI, the evidence of statistically significant state dependence in real rates in the baseline sample are concentrated in short and medium-term maturities (below 8 years) with a magnitude comparable to the results for the noise measure. However, the differences between high and low PMI periods are visibly weaker in long maturities, where our baseline liquidity measure continues to yield significant heterogeneity.

To disentangle the relative contributions of market liquidity and economic conditions to monetary policy transmission, we follow the same methodology as in the previous section and adopt a double sorting strategy.[19] Figures 12 and 13 highlight another source of asymmetry arising from the interactions between economic conditions and market liquidity. While market liquidity plays a minor role in favourable economic environments (Figure 12), we observe in column 2 of Figure 13 that higher liquidity in the market can support monetary policy transmission across the yield curve for both nominal and real rates in depressed economic conditions. We document in Appendix A.1 that the downward trend in the noise measure over the sample can bias the results of our double sorting analysis. Once corrected, we observe that market liquidity plays a key role in monetary policy transmission over the business cycle. The transmission of policy shocks can become muted during favourable economic conditions if market liquidity is poor, and higher liquidity in the market can support monetary policy transmission during periods of depressed economic activity.

[Figures 12–13]

Figures 14 and 15 provide complementary evidence on the role of market liquidity for monetary policy transmission over the business cycle. First, the marginal impact of economic conditions on monetary policy transmission becomes negligible once we condition on the subset of FOMC announcements associated with high market liquidity (Figure 14). Again there is some degree of asymmetry in these results, as the third column of Figure 15 indicates that low market liquidity is less detrimental to monetary policy transmission in favourable economic environments. However, the asymmetry in the results is driven by the trend in the noise measure. We show in Appendix A.1 that market liquidity plays a key role in monetary policy transmission as we do not observe a

[19] As in the previous section, we consider the extended sample period from 01/2000 to 12/2019 in this part of the analysis to increase the number of observations in each subset of FOMC annoucements.

significant impact of (un)favourable economic conditions once we condition on liquidity conditions in the market being good or bad.

[Figures 14–15]

5.2 Possible Correlations between Market Liquidity and Monetary Policy

A potential concern about our empirical design is related to the possible correlations between market liquidity and monetary policy. For example, it might be that market liquidity may be systematically different during monetary policy announcements, leading to endogeneity problems in our empirical analysis. Alternatively, the size of monetary policy shocks itself could depend on market liquidity, which could affect the interpretation of our results. To address these concerns, we first check how the average values of the noise measure are distributed across the week days with and without FOMC announcements. As shown by Appendix Table 10, the noise measure is relatively stable across the different days of the week, although it tends to be higher on Fridays. We do not see a statistically significant difference in the mean noise during FOMC weeks, which indicates that this measure is a good candidate for our approach conditioning on liquidity conditions prior to the arrival of the shock.

Moreover, we estimate the relation between the size of monetary policy shocks and market liquidity by regressing the absolute value of the realised surprises on the previous day's noise measure. As shown by Appendix Table 11, there is some evidence that higher noise days are followed by larger surprises (column 1).[20] Given that the variance of the monetary policy surprises could differ across higher/lower liquidity announcement dates, we standardize the shocks in each subgroup to have unit variance. This allows us to isolate the state-dependence in the covariance between interest rates and monetary policy surprises independently of differences in the magnitude of the response driven by conditional heteroskedasticity across liquidity states. Note that for comparability, we also standardize the shocks in the baseline results to have unit variance.

[Figure 16]

Figure 16 presents the results for the baseline. The results confirm that the state-dependence of monetary policy transmission with respect to liquidity conditions is not driven by the conditional heteroskedasticity of the shocks across liquidity states.

[20]Though the effect is weaker and statistically insignificant once we remove trends from the noise measure (column 2-3).

5.3 *Dynamic Effects over Longer Horizon*

We assess the impact of market liquidity on the transmission of monetary policy shocks over a horizon of 60 trading days. To this end, we extend the specification in Equations (1)–(2) as follows:

$$f^i_{t+h-1,\tau} - f^i_{t-1,\tau} = \alpha^h + \gamma^{i,h}_{all,\tau} \Delta mps_t + \varepsilon_{t+h-1}, \tag{4}$$

$$f^i_{t+h-1,\tau} - f^i_{t-1,\tau} = \alpha^h + \gamma^{i,h}_{hl,\tau} \cdot [\Delta mps_t \times \text{HighLiq}_{t-1}] + \gamma^{i,h}_{ll,\tau} \cdot [\Delta mps_t \times \text{LowLiq}_{t-1}] + \varepsilon_{t+h-1}, \tag{5}$$

where the left-hand side variable measures the cumulative change over an horizon of h days after the announcement. Figure 17 presents the results for real forward rates in the baseline sample, supporting the thesis of a state-dependent transmission of monetary policy shocks. The initial increase in real forwards on higher liquidity announcement days has prolonged effects up to 3 months after the initial shock. The estimated effects are strongly significant over the first month for intermediate maturities of 5 and 10 years, and the overall increase after 3 months is significant for the 5 and 20-year forwards. These results contrast starkly with the muted response of real forwards on lower liquidity announcement days. The majority of the estimates are not significantly statistically different from zero and we even observe a significant cumulative decrease 3 months after the initial shock for 5 and 10-year forwards.

[Figure 17]

Note that the lack of clear state-dependence in the results for the 2-year forward rate might be explained by the shorter sample used for the estimation (starting in 2004). While the results for the extended sample (Figure 57 in the Appendix) might lend support to this hypothesis, they are actually driven by the downward trend in the noise measure. Once corrected, the results in the extended sample using the detrended noise measure are in line with our baseline analysis (see Figure 60 and Section A.1 in the Appendix).

We can gain further insights on the differences between the results for the baseline and extended sample by examining how nominal and inflation forward rates react to the monetary policy shock at longer horizons. Figure 55 shows that nominal forward rates also exhibit signs of a persistent impact of the state-dependent transmission of monetary policy shocks in the baseline sample which parallels the results documented for real forwards. This is associated with moderate signs of state-dependence for inflation forwards, with estimates which are not statistically significant (Figure 56). These results are confirmed by our analysis for the extended sample using the detrended noise measure. See Figures 61–62, as well as Section A.1 in the Appendix for further details.

5.4 Macroeconomic Implications: The Response of Mortgage Rates

As the results in Section 5.3 are suggestive of a persistent dynamic impact of liquidity conditions on the transmission of monetary policy, a natural question to ask is whether there are potential implications for the real economy. As an application, we consider the effects on mortgage rates given the importance of the monetary policy transmission to the housing market (Iacoviello 2005; Davis and Van Nieuwerburgh 2015).

[Figure 18]

Figure 18 provides the results for US fixed mortgage rates in the baseline sample and Figure 63 in the appendix covers the extended sample. In line with our main results, we find a strong degree of state-dependence in monetary policy transmission, with a large and statistically significant cumulative increase in mortgage rates over an horizon of 3 months when the shock occurs on a higher liquidity day.[21] Conversely, a significant cumulative decrease follows shocks occurring on lower liquidity announcement days. The magnitude of these estimated effects is somewhat lower in the extended sample but we still find a strong and significant persistence of monetary transmission over the first two months following a monetary policy tightening on higher liquidity announcement days. As in previous sections, the somewhat weaker liquidity state-dependence of monetary policy transmission in the extended sample is resulting from the downward trend in the noise measure which needs to be properly taken into account in the analysis. We document in Appendix A.1 that the results based on the detrended noise measure confirm our baseline analysis. Figure 64 shows that the estimates for the higher liquidity days are more precisely estimated and the cumulative increase in mortgage rates is significantly statistically different from zero at all horizons up to 3 months after the initial shock.

5.5 Evidence from the UK

We now turn to the results using UK data. The measure of monetary policy shocks is the first principal component extracted from the high-frequency surprises in the the first four quarterly Short Sterling Futures contracts. As such, the monetary policy shock incorporates information about the short-term target rate and the expected path of interest rates over the next few months, similar to the measures proposed by Nakamura and Steinsson (2018) and Bauer and Swanson (2023b) for the US which were used in the previous sections.

[Figure 19]

Figure 19 shows the results using all Bank of England MPC announcements for the sample 2000-2019. We find a similar pattern of state-dependence for the real forward curve as found for the

[21]Such quick propagation of the monetary policy shock to mortgage rates is consistent with recent evidence, showing that the monetary policy transmission to the mortgage market may be quicker than the previous literature had suggested (Gorea, Kryvtsov, and Kudlyak 2022).

US in a similar sample. The only difference is that the inflation forwards is also state-dependent, with a more negative response to policy shocks in higher liquidity states. This state-dependent reaction of inflation forwards offsets the larger impact on real forwards, leaving the nominal forward reaction similar across liquidity states, though the pattern in each of its components is clearly state-dependent.

6 Additional Results and Robustness Checks

In this section we demonstrate the robustness of our baseline results. We first show they hold when using a number of different measures of the monetary policy shocks proposed by the recent literature, and using different sample choices (Section 6.1). We also consider whether a predictable component in these shocks might affect our results – an issue that has been shown to plague many of these measures (Section 6.2).[22]

We show in Section 6.3 that our main result on the liquidity state-dependence of monetary policy transmission is robust to the use of alternative liquidity measures. Section 6.4 provides an in-depth assessment of the economic and statistical significance of our state-dependence results. Finally, we show in Section 6.5 that our results on risk premia are robust to alternative methods of decomposing yields and forward rates.

In addition, Appendix A.1 provides further results on the potential impact of our choice of timing to measure market liquidity conditions around FOMC announcements; the potential role played by long-term trends in market liquidity; and the implications for our results of using different data vintages for interest rates.

6.1 Alternative Measures of Monetary Policy Shocks and Sample Choice

One of the key differences among previous studies on the response of bond yields to monetary policy shocks is how these shocks are measured. Hanson and Stein (2015) use the daily change in 2-year yields on FOMC announcement days, while Nakamura and Steinsson (2018) use changes over 30-min windows around the announcement, and argue that their identification accounts for the difference in the results relative to Hanson and Stein (2015). The literature in monetary policy identification has increasingly focused on high-frequency measures similar to Nakamura and Steinsson (2018) because of the additional background noise in daily changes. Refinements include the exclusion of days when changes in equity prices suggest the presence of an information effect (Jarocinski and Karadi, 2020), and decomposing shocks into federal funds rate, forward

[22] See Miranda-Agrippino (2016); Miranda-Agrippino and Ricco (2021); Karnaukh and Vokata (2022); Bauer and Swanson (2023a,b) among others.

guidance and LSAP shocks (Swanson, 2021).[23]

[Figure 20]

The robustness results are summarized in Figure 20, showing the estimates of the additional impact on real forward yields at times of higher liquidity ($\gamma^r_{h-l,\tau}$, for $\tau \in (2, 20)$, from Equation (3)) for different samples and monetary policy shock measures.

We start by changing the samples, while keeping the same monetary policy shock measure. The first row shows the baseline Nakamura and Steinsson (2018) results discussed in the previous section, and two alternative samples: an extended sample through 2019 (using the series provided by Acosta, 2022) and the extended sample with all unconventional monetary policy announcements identified by Cieslak and Schrimpf (2019) removed. Figures 72 and 73 in the appendix report the complete set of results for these two cases and Figure 74 covers the case where all the announcements in the 2008-2009 period are also included in the analysis. The same pattern of higher estimates in liquid markets, with statistically significant estimates for many maturities, remain, though the statistical significance of the state-dependence is not as uniform across the curve as in the baseline case. We show in Appendix A.1 that the apparent weaker liquidity state-dependence of monetary policy transmission in the extended sample is likely due to the downward trend in the noise measure which is unrelated to shorter-term market liquidity conditions around FOMC announcements.[24] We find very similar estimates compared to our baseline results once the long-term trend in the noise measure is properly taken into account.

We next consider two recent alternative monetary policy surprise measures, by Jarocinski and Karadi (2020) and Bauer and Swanson (2023b). These are shown in the second row of Figure 20. The results with the Jarocinski and Karadi (2020) shocks, which are the shocks used in Kekre, Lenel, and Mainardi (2022), confirm the results using the shocks from Nakamura and Steinsson (2018), with similar magnitude in the extended sample and even stronger state-dependence for the original sample.[25]

When using the individual shocks from Swanson (2021) shown in the third row of Figure 20, only the forward guidance shock has any statistically significant state-dependence. There is

[23]These are how Swanson (2021) refers to them. The LSAP shocks, however, are nearly identical before and after 2009 except for 3 dates (March 2009, and two Taper Tantrum dates), which suggests that literally assigning them to LSAP shocks may be misleading. The first two shocks correspond to what others (Gürkaynak, Sack, and Swanson, 2005a) have referred to as short-rate and path shocks. We instead refer to the LSAP shock as a curve twist shock (with opposite loading on short-term and long-term yields, and higher loadings on long-term yields).

[24]Adrian, Fleming, and Vogt (2023) document a downward trend in their liquidity index for the US Treasury market over the same period. They also note an upward trend in trading activity during this period linked to the progressive adoption of fully automated electronic trading platforms.

[25]Figure 75 in the appendix reports the same set of results using the monetary policy shock of Jarocinski and Karadi (2020) and their original sample, as used in Kekre, Lenel, and Mainardi (2022). Figure 76 considers the extended sample ending in June 2019. We note that we find almost no sign of a U-shape pattern in the estimates, particularly for the whole sample. Furthermore, just as with the Nakamura and Steinsson (2018) shocks, the whole sample response of bond yields to monetary policy shocks is only statistically different from zero for short maturities.

no evidence of state-dependence in the LSAP shock, with point estimates for most maturities close to zero. The full estimates for each of the shocks from Swanson (2021), shown in Figures 78-80 of the appendix, reveal an even greater role for forward guidance shocks. All estimates (across maturities, type of forward and for full sample and each liquidity state) are insignificant for the Federal Funds rate shock, while all point estimates for LSAP shocks are very similar across liquidity states. In contrast, the marginal significance of the incremental reaction in higher liquidity states to the forward guidance shock is purely an issue of statistical precision. The results for the LSAP shocks, together with the evidence for state-dependence discussed above for Nakamura and Steinsson (2018) shocks when we exclude all of the unconventional monetary policy announcements identified by Cieslak and Schrimpf (2019), suggest our results are not influenced by QE/LSAP operations since the global financial crisis.

6.2 Information Effect and Response to News

Bauer and Swanson (2023a) show that the Fed information effect can be explained by the Fed's response to publicly available news in financial and macroeconomic variables. This introduces a predictable component in high-frequency monetary policy surprises which needs to be purged out to satisfy the exogeneity assumption of the instrument. Though they emphasize this is not an issue for event study regressions with asset prices as we do here, we nevertheless check whether this affects our state-dependence results. To that end, we orthogonalize the monetary policy shocks of Nakamura and Steinsson (2018) and Bauer and Swanson (2023b) and repeat our analysis with the orthogonalized shocks. Appendix A.2 details how the monetary policy shocks are regressed on the set of macroeconomic and financial predictors proposed in Bauer and Swanson (2023b). Table 4 presents the results for each monetary policy shock series and different samples. Figures 21 and 22 compare the original shock series with the orthogonalized ones for the baseline and extended samples.[26]

[Figures 21–22]

The robustness of state-dependence to orthogonalized monetary policy shocks in the case of real forward rates is shown in Figure 23 for both the Nakamura and Steinsson (2018) and Bauer and Swanson (2023b) shocks. Figures 81 and 82 of the appendix report the same results for nominal and inflation forward rates and the complete set of results can be found in Figures 83-86. For our baseline sample the results are unaffected, while they lose statistical significance for the extended sample. A closer inspection of Figures 21 and 22 indicates that this is likely due to the fact that orthogonalized shocks are much smaller in the more recent sample, making inference more noisy.

[26]The orthogonalized shock series correspond to the residuals from the regression results reported in columns (4)-(5) of Table 4 for the baseline sample and columns (8)-(9) for the extended sample.

[Figure 23]

We also consider the alternative shock series proposed by Karnaukh and Vokata (2022). They remove the component in the Nakamura and Steinsson (2018) shock and the Gürkaynak, Sack, and Swanson (2005a) path shock that can be forecast using the revision in private sector forecasts of growth from Blue Chip Surveys. Figure 24 shows the results using the path shock, which confirms the importance of the similar 'forward guidance' shock from Swanson (2021), and is robust to orthoganalizing the series to private sector updates of growth expectations.

[Figure 24]

6.3 Alternative Measures of Market Liquidity

Our baseline measure of liquidity is correlated with other popular measures that proxy liquidity conditions. For example, variations in the VIX index can reflect changes in financial intermediaries balance sheet constraints (Adrian and Shin, 2010; Nagel, 2012), and can also proxy broader market conditions (Goldberg, 2020; Goldberg and Nozawa, 2021). In addition, the T-Bill Eurodollar (TED) spread is often used to proxy variations in funding conditions (Garleanu and Pedersen, 2011) and dealers' funding costs (Friewald and Nagler (2019); Goldberg and Nozawa (2021)).

We find some evidence of state dependence using either measure, as shown in Figures 25 and 26; and the results for the extended sample are displayed in Figures 87 and 88 of the appendix.

[Figures 25–26]

For the TED spread, we see a stronger reaction in the short-end of the yield curve on high liquidity days compared to the results with the noise measure as state variable. This might be indicative of funding liquidity/constraints playing a more important role for monetary policy transmission in this part of the curve, particularly for the results in the extended sample (Figure 88). One caveat is that we use a proxy for unsecured funding while the main source of funding would be through secured repo funding. Results for the VIX are broadly comparable to the ones with the noise measure.

6.4 Statistical Significance of the State Dependence for the Different Conditioning Variables

We next turn to the economic and statistical significance of the state-dependence in the response of bond yields to monetary policy shocks. Since we have shown the state-dependence is driven by the real component of nominal yields (Section 3.1), we here focus on estimates of the differential impact of monetary policy shocks on real forwards ($\gamma^r_{h-l,\tau}$) from Equation (3).[27] The top left

[27]Figures 70 and 71 in the appendix present the corresponding results for the nominal and inflation forward rates.

chart in Figure 20 shows the point estimates for $\gamma^r_{h-l,\tau}$ in the baseline specification (regression (3) with same monetary policy shocks and sample choice of Nakamura and Steinsson (2018)) for real forward rates. The difference in the coefficient estimates for low minus high noise days are positive and statistically significant at the 5% for all maturities, and at the 1% level for real forwards between 4 and 16 years. The point estimates for the incremental response of real forwards in liquid markets first increases from the 2-year forward (1.21) up to the 4 year forward (1.42), before monotonically decreasing to 0.52 at the 20 year horizon.

[Figure 20]

In addition, we illustrate whether the monetary policy transmission is significantly different during periods of high and low values of VIX index and TED spread. This is shown in Figure 27, which presents the estimates of $\gamma^r_{h-l,\tau}$ in regression (3) respectively for nominal and real forward rates in the baseline sample. The corresponding results for the extended sample are reported in Figure 28.

[Figures 27–28]

In line with our main results (top-left panel of Figure 20), the differences in the coefficient estimates for low and high noise announcement dates are both economically and statistically significant at all maturities in the baseline sample. However, the differences in coefficient estimates are no longer statistically significant for maturities above 5-8 years (for nominal and real resp.) when results in the extended sample are considered. The pattern of the results for the VIX is broadly comparable, with the exception of the baseline sample for which the differences in coefficient estimates are not statistically significant for medium and long-term maturities. For the TED spread, the results in the baseline sample are broadly comparable to the ones for the noise measure, although with slightly larger estimated differences in the short end of the yield curve (below 5 years). The results in the extended sample are not weaker in the case of the TED spread and we even observe a stronger state-dependence at longer maturities for nominal forward rates.

For dealers' leverage, the difference in coefficient estimates in the baseline sample is slightly less pronounced at short and medium maturities but we observe a pick up at longer maturities – particularly for nominal forward rates with maturities larger than 15 years. We still observe a statistically significant state-dependence for medium and long-term maturities in the extended sample, whose magnitude is comparable to the results for the TED spread.

[Figures 29–30]

The results for FIA hedge fund returns are comparable to the ones for dealers' leverage: the evidence supporting a statistically significant state-dependence are weaker in the baseline sample compared to other conditioning variables but we find strong supporting evidence in the extended sample for both nominal and real forward rates.

6.5 *Alternative Risk Premium Estimates*

In this section, we check whether our results on risk premia are robust to alternative methods of decomposing yields and forward rates. The importance of the term premium in accounting for the state-dependence of nominal forward rates is confirmed by using the decomposition of nominal forwards based on the approach introduced in Kim and Wright (2005). Figures 90–91 confirm that our baseline results are robust to using this decomposition. Using the joint decomposition of D'Amico, Kim, and Wei (2018), shown in Figures 92–95 of the appendix, confirms the role of real term premium, though it also suggests a role for the inflation risk premium.[28]

7 Evidence from Transaction-Level Data

In this section, we complement the previous sections (using aggregate time-series data) by employing a granular, transaction-level dataset on the US treasury market. Our sample covers virtually all secondary-market trades of US Treasuries executed in London, and contains detailed information on each transaction. This unique dataset helps further analyse the mechanisms underlying the liquidity-state dependence, because it helps understand how different market participants actually trade around FOMC meetings.

Our dataset has three crucial advantages compared to the existing literature. First, it is high frequency, observing minute-by-minute trades. Second, we observe unique client and counterparty identifiers. Third, its coverage is much larger than other micro-level dataset in terms of both sectors and maturities covered. The data covers a bit less than 15% of volume of the entire US treasury volume and is representative of the wider market as we explain below.

7.1 *Data and Measurement*

7.1.1 *Data Sources*

To study the trading activity in US Treasuries, we use the MIFID II database. This is a confidential transaction-level dataset, maintained by the Financial Conduct Authority, which provides information for almost all secondary market transactions on execution time, transaction price and quantity, as well as the International Securities Identification Number (ISIN), the Legal Entity Identifier (LEI) of both counterparties, and buyer-seller flags among others.[29] The dataset covers virtually all transactions where one side of the trade involved a UK-based institution between 2018 and 2022. since most US Treasury trades are dealer-intermediated and some of those dealers are European entities, this means we observe a substantial fraction of US Treasury trading.

[28] The method of D'Amico, Kim, and Wei (2018) addresses concerns with the liquidity or mispricing of real bonds (see also Fleckenstein, Longstaff, and Lustig, 2014; Barria and Pinter, 2023).

[29] Further information on the MIFID II dataset can be found in the Reporting Guidelines: ESMA 2016. Recent applications of the datasets can be found in Pinter, Wang, and Zou (2021) among others.

The identities of the participants involved in trades are denoted by the LEIs, which allows us to classify clients into different sectors. We focus our analysis on banks, asset managers, hedge funds, foreign official (central banks and sovereign wealth funds) and insurance companies and pension funds (ICPFs). We develop a sectoral classification based on a text-based algorithm which we complement with the Bank of England's internal classification system. Insurance companies in our sample include liability-driven investors (LDI) which are usually managed by asset managers. Our definition of hedge funds includes both discretionary and systematic funds featuring both macro and relative value strategies. Our definition of asset managers include both wealth and asset managers as well as other mutual funds. Foreign officials include foreign central banks, sovereign wealth funds and any state-owned entity operating in this market.

We complement the trade-level data with the Daily CRSP US Treasury Dataset. We collect information on duration, coupon rate, end-of-day price, and amount outstanding for all the bonds in our dataset. We restrict our analysis to coupon bonds with at least 1 year left to maturity. We do so because inflation-protected securities and floating-rate notes are distinct assets with a distinct clientele of investor.

7.1.2 Representativeness of the Sample

We first document that US Treasuries trading in London makes up a fraction that is substantially higher than previously documented. Based on GovPX data, Fleming (1997) reports that US Treasury trading in London accounted for less than 5% of volume in 1997. In contrast, we find that between 2018 and 2021,[30] London accounts between $10 - 15\%$ of the average daily traded volume, see Figure 32. Besides the different sample period, the discrepancy between Fleming (1997) and our figure is because Fleming (1997) accounts for London trading by the hour of the day, so that even trades that executed in London but during hours when New York is open are counted as US trading. We find instead the even when New York opens, a large fraction of US Treasury trading continues to be routed via London.

Also Fleming (1997) reports that the maturity composition of US Treasuries traded in London is systematically skewed towards certain maturities. We argue instead that our sample is representative in terms of maturity distribution. In Figure 33 we compare the distribution of maturities traded between our data and that reported by Securities and Industry Financial Market Association (SIFMA). We find that the distribution of maturities is relatively similar, with our data is slightly lighter on shorter maturities $(0 - 2y)$ and slightly heavier on intermediate ones $(3 - 7y)$.

[Figures 8]

[30] The last year for which we have official SIFMA data.

7.2 *Client Trading Around FOMC Meetings*

7.2.1 *Stylised Facts*

To empirically characterise the trading behaviour of clients, we provide summary statistics on average daily volume, trade size and daily number of transactions in the *dealer-to-client* (D2C) segment of our data.

Panel A in Table 5 reports aggregate statistics. On the average day, we observe volume of $10 billions, 586 transactions and an average trade size of $17 millions. Our data covers 3000 unique clients trading in US Treasuries.

Panel B shows the same statistics broken down by maturity. There is substantial trading across all maturities, with a slight tilt toward the $3 - 7y$ bucket. There are larger transactions on the short end of the curve and roughly the same number of players are active across maturities. Trade size is decreasing in maturity.

Panel C shows the sectoral composition of the market. Banks and hedge funds represent more than 60% of the volume in this market, followed by asset managers and foreign officials. Banks and asset managers trade more frequently but in smaller ticket size ($13 and $8 millions respectively). Hedge funds and Foreign Officials trade in substantially larger ticket sizes.

[Table 5]

Since our focus is on FOMC events, Panel A in Table 6 provides descriptive statistics around those events. Given the time-zone difference between the US and UK, the FOMC decision is usually released at 19.00pm UK time, which we take to be close to market close in London.[31] For this reason, we call the FOMC day, *Pre-FOMC* and the day after *Post-FOMC*, and we will have no *FOMC* day separately. On average, activity increases around FOMC events with both higher volumes as well more transactions. It is worth noting that not everyone trades those events: of the 3000 unique identifiers, only half are active around FOMC events.

The main finding of our paper is that Treasury market reaction to monetary policy surprises is significantly different depending on market liquidity conditions. To stay close in spirit to our evidences in Section 3, we define the same measure in our sample. Specifically, we update the noise measure of Hu, Pan, and Wang (2013) up to 2022 and we compute its median the day before the FOMC from 2018-2022. We define *High Noise* FOMC meetings as those above the median, and *Low Noise* meeings those below the median. Panel B shows descriptive statistics split between High and Low noise FOMCs. On average, volume is slightly higher pre-FOMC when noise is low, while not substantially different post-FOMC. However, we do see more transactions in low liquidity post-FOMC. Table 6 reports the p-values of the t-test for the difference in average volume across different days. The main take-away is that the test rejects equality of means for the volume between high and low noise FOMC periods.

[31] Even though this is mostly a OTC market.

[Table 6]

7.2.2 Identifying Arbitrageurs

Recent theories of the term structure (Vayanos and Vila, 2021) place a central role on arbitrage activity in transmitting shocks to the entire curve. It is therefore essential to identify who carries out such an activity. Previous studies have assumed that certain sectors carried out such activity: for example hedge funds and trading desks of large broker-dealers. We adopt a different approach and develop a new methodology to classify individual institutions into arbitrageurs and non-arbitrageurs. Our paper is to our knowledge the first to attempt such an exercise using trade-level data directly. We can do so because of the frequency and granularity of our dataset, which provides a considerable advantage relative to the existing literature.

Our baseline measure classifies a client as an arbitrageurs if two criteria are satisfied. First, we must observe trading across multiple maturities. In reality and in our models, fixed-income arbitrage exploits price discrepancies *across* the curve. Such an activity incorporates information at both maturities, imparting a specific shape to the yield curve. We operationalise this criteria by computing the standard deviation of the maturities traded (weighted by trade size). We denote the standard deviations of maturities traded by trader i over period t by $\sigma_{i,t}$. Second, we require that the combined exposure of all the trades is market neutral. The arbitrageur is interested in exploiting the price discrepancy between two different maturities without necessarily a view on the level of interest rates. We implement this measure by computing the *net* duration exposure of the combined trades. We denote the net duration exposure of trader i over period t as $d_{i,t}$. Since we are not interested in the direction of the exposure, e.g. whether it is a net long or net short, we take the absolute value. We also multiply by -1 so that an increase in duration will be penalized by our composite score. An important consideration is the time horizon (t) over which we compute the two measures. We experiment with different horizons (daily, weekly, monthly) and we report the main results with a monthly window[32]

Each period, we rank traders across each of the two measures and combine them into a single composite index:[33]

$$I_{i,t} = R^{\sigma}_{i,t} * R^{d}_{i,t}, \tag{6}$$

where $R^M_{i,t} \in [0,1]$ is the (standardized) ranking of trader i at time t on metric $M = \{\sigma, d\}$. We take the average of index value over the entire sample and end up with a single score for each trader:

[32]Section A.3 in the Appendix shows the results using the index at different time horizons.

[33]We standardize the ranking of each measure between zero and one.

$$I_i = \frac{1}{N_i} \sum_{t=1}^{N_i} I_{i,t}. \tag{7}$$

We then define arbitrageurs as those that score above the third tercile on the combined metric over the entire sample. According to our measure, the majority of arbitrageurs are from the hedge fund sector (Figure 34), accounting for about 60% of total arbitrage volume. The remaining 40% is accounted by banks and asset managers, which gives us confidence that our measure is picking up sophisticated institutional investors. The non-arbitrageurs in our sample are a mix of sectors, with a bit over 40% of volume generated by commercial banks. Note also that, according to our ranking, not all the hedge funds in our sample engage in fixed-income arbitrage.

[Figure 34]

We start by looking at the evidence in Panel A of Table 7. On a given day, our measure finds the arbitrageurs (RV) account for a smaller volume, but with a substantially larger trade size. We find a total of 699 clients that resembles arbitrageurs. We describe trading behaviour by the arbitrageurs in the next section.

[Table 7]

7.2.3 Arbitrage Activity During Liquid vs Illiquid Periods

We revisit our finding that the Treasury market reacts more to monetary policy when it is more liquid through the prism of our micro-data. Guided by the liquidity measure, we expect low noise days to be associated with more intense arbitrage activity. In particular, we hope to see arbitrageurs, as identified in the previous section, to be *increasingly* more active when noise is low, and more so than non-arbitraguers. Panel B of Table 7 shows that irrespective of liquidity conditions, on FOMC days volume increases for both arbitrageurs (RV) and non-arbitrageurs (non RV). Interestingly, and consistent with our hypothesis, a difference emerges when we sort FOMC days by liquidity in Panel C. Arbitrageurs increase their volume both Pre-FOMC as well as post-FOMC. In contrast, non-arbitrageurs increase volume when the noise is high but decrease when noise is low. This is our first key finding using the micro-data. This point is made more evident by Figure 35, where we compute the percentage change in volume across high and low noise periods. Arbitrageurs increase their pre-FOMC volume by 32% in Low noise compared to High noise environment, whereas non-arbitrageurs increase by 17%. More importantly, post-FOMC non-arbitrageurs cut down their volume by 6% whereas arbitrageurs increase it by 17% when noise is lower.

[Figure 35]

Our finding was that monetary policy transmits more strongly to longer maturities when liquidity is high. This finding implies that if arbitrageurs are responsible for transmitting monetary policy shocks to longer maturities, we should see them more active at those maturities. We repeat the same exercise as above, this time splitting by maturity, in Figure 36. We can see the pattern between high- and low-noise FOMC days even more clearly over the curve. Arbitrageurs are substantially more active than non-arbitrageurs, increasing volume (almost) monotonically over the entire term structure. Indeed, volume is 50% higher at longer maturities pre-FOMC (Figure 37) and by more than 30% post-FOMC (Figure 38) in a low noise relative to high noise. In contrast non-arbitrageurs increase volume by about 10% and uniformly over all maturities pre-FOMC, while cutting down post-FOMC across all maturities when noise is low. We take those patterns as highly suggestive that it is specialized arbitrage capital behind the liquidity-state dependence of monetary policy.

[Figures 36–38]

7.3 Intermediation Around FOMC Meetings

While we have provided evidence that the demand of arbitrageurs vs non-arbitrageurs exhibit substantially different behaviour in high vs low liquidity environment, one plausible concern is that the change in market liquidity and monetary transmission is due to their counterparties, the primary dealers, affecting the supply. In this section we explore the connection between dealers' constraints and trading activity.

The recent OTC literature argues that interdealer trading activity strongly correlates with constraints on the intermediation capacity of primary dealers. Specifically, the dispersion of transaction prices in the interdealer sector is a strong predictor of measures of liquidity (Eisfeldt, Herskovic, and Liu, 2022).[34] Building on their analysis of corporate bond markets, our measure of interdealer price dispersion, D_T, is as follows:

$$D_T = \sqrt{\frac{1}{N}\sum_{v}^{N}\left(\ln\left(P_v^{\star}\right) - \ln\left(\overline{P}\right)\right)^2}, \tag{8}$$

where $P_v^{\star}$ is the transaction price corresponding to trade v in the interdealer market, and $\overline{P}$ is the average hourly interdealer price in a given bond. Figure 39 shows the time series of the dispersion measure. The measure picks up the repo spike in 2019, the illiquidity during Covid-19 and the steady deterioration of liquidity since 2021 consistent with Duffie, Fleming, Keane, Nelson, Shachar, and Van Tassel (2023).

[Figure 39]

[34] Since Jankowitsch, Nashikkar, and Subrahmanyam (2011), aggregate price dispersion has often been used as an alternative measure of market liquidity (see Friewald, Jankowitsch, and Subrahmanyam (2012); Uslu (2019); Uslu and Velioglu (2019) among many others).

We test whether inter-dealer price dispersion is significantly different in high- versus low-noise FOMC periods. Importantly, as shown by Table 8, we find that price dispersion decreases when noise is low. This result is consistent with dealers constraints playing a significant role in the liquidity-state-dependence of Section 3.

[Table 8]

8 Conclusion

A rapidly expanding literature in macroeconomics has studied the identification of monetary policy surprises, their impact on asset prices and the role of financial intermediaries in amplifying and propagating shocks through the economy. In this paper we have used the intuition and results from all three strands to zoom in on the role of market liquidity in shaping the transmission of monetary policy shocks to the yield curve.

We have found robust evidence of a liquidity state-dependence in the impact of monetary policy on yields that is both economically and statistically significant. These results have important policy implications as they point to the need to coordinate financial policy with monetary and stabilisation policies.

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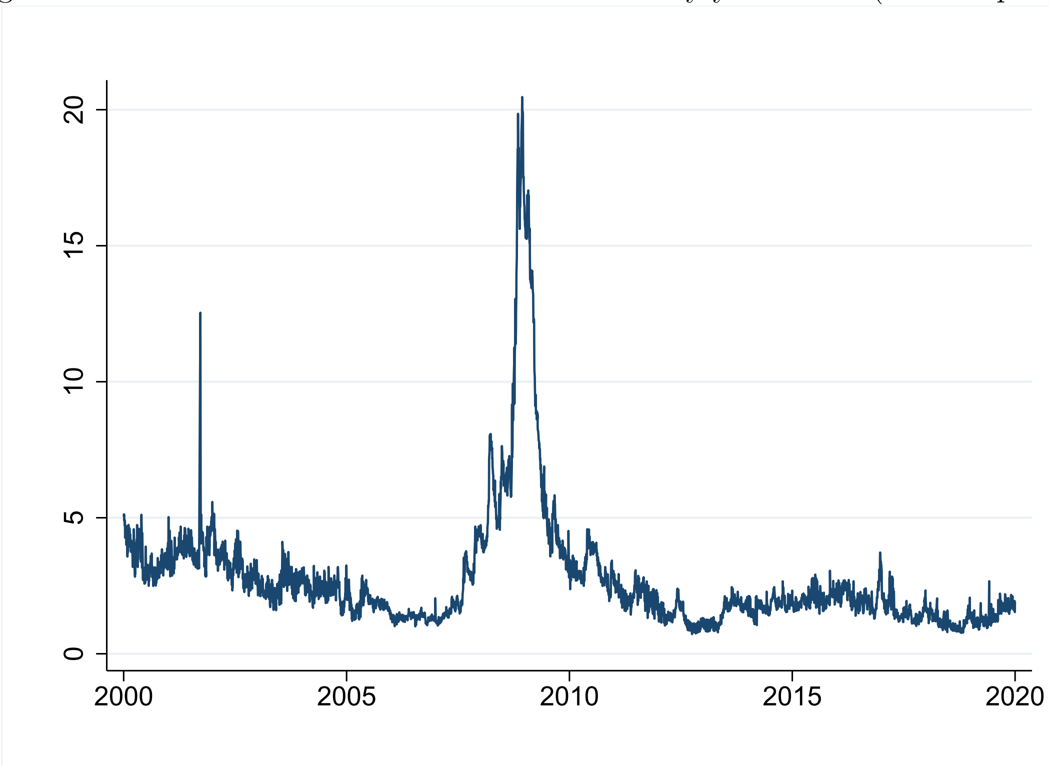
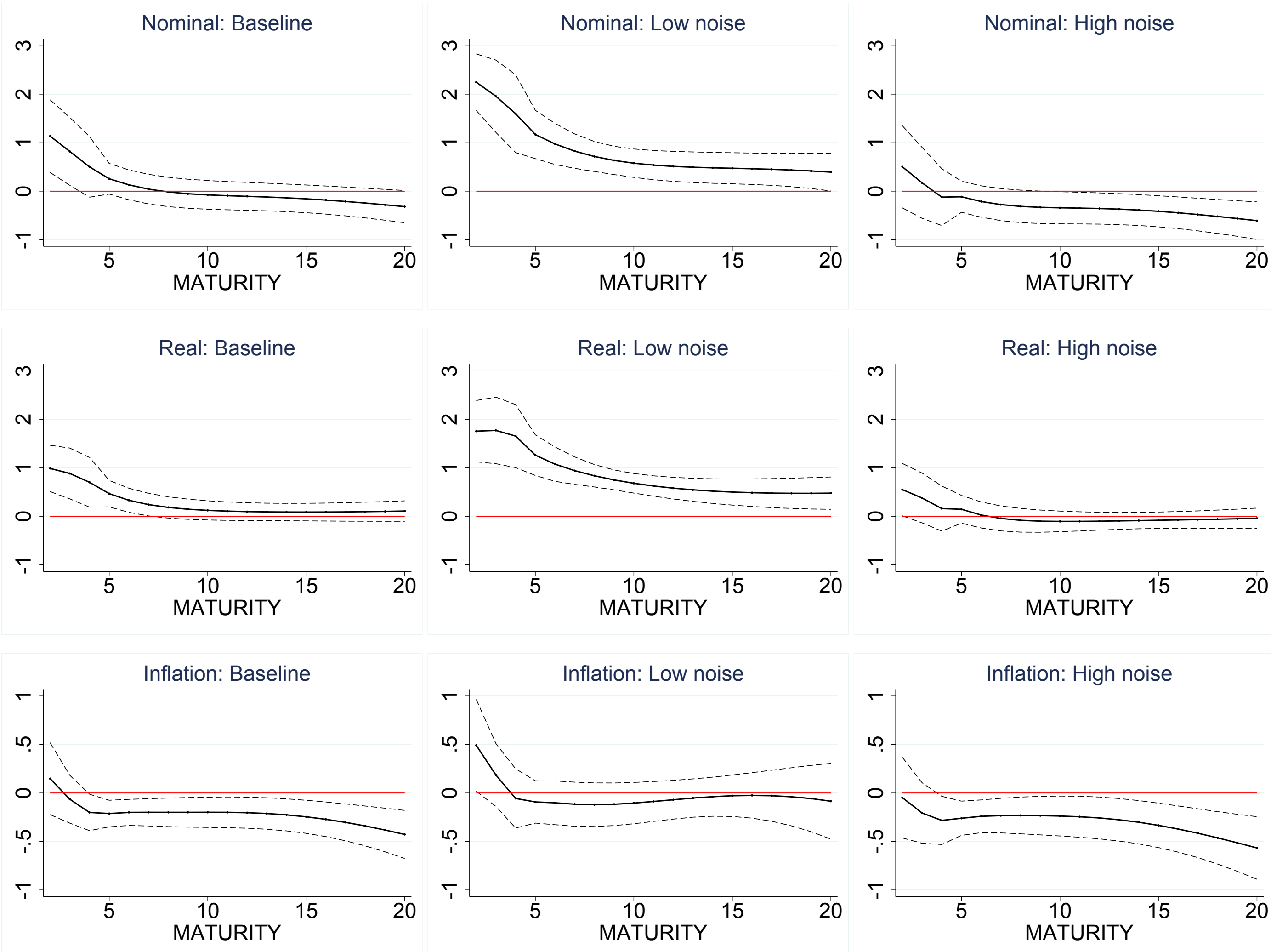
Figures and Tables

Figure 1: Noise measure for the US nominal Treasury yield curve (in basis points)



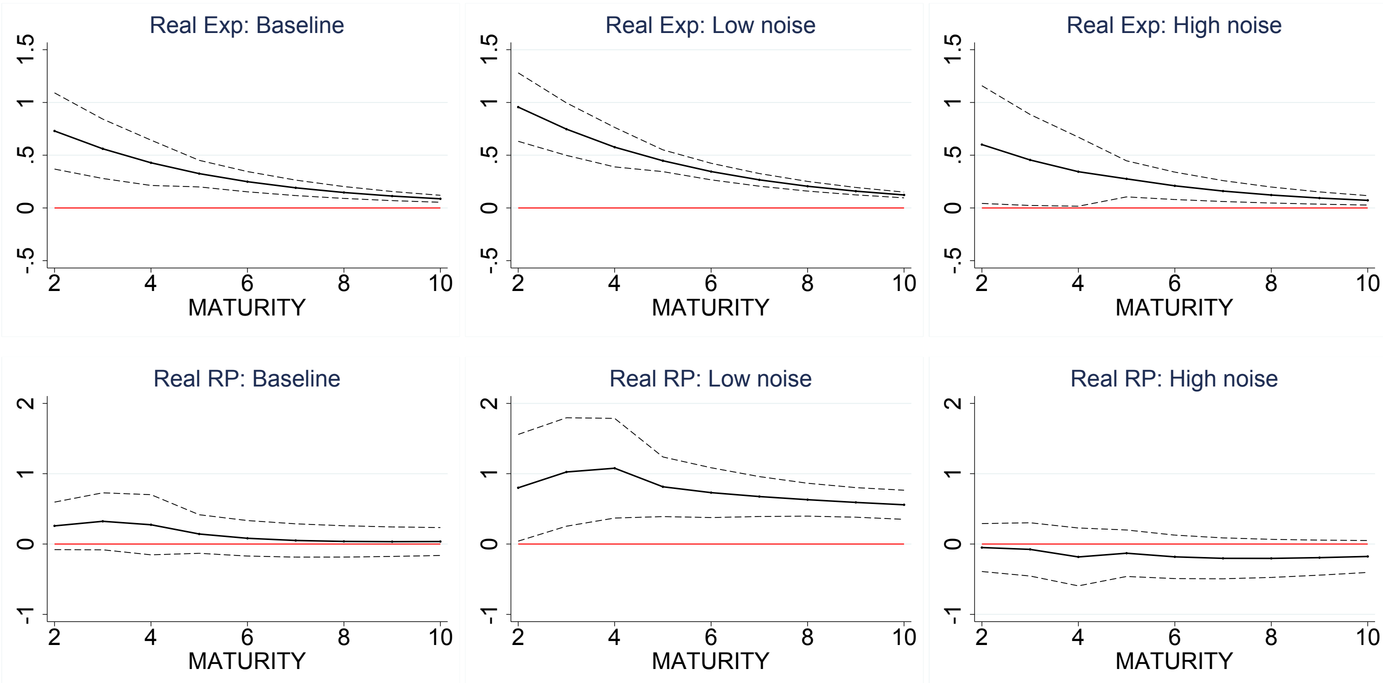
Note: This figure plots the time series evolution of the noise measure of Hu, Pan, and Wang (2013) for the US nominal Treasury yield curve (in basis points). The period covered is from 2000/01 to 2019/12.

Figure 2: Impact of yield curve noise on the transmission of MP shocks to US forward rates



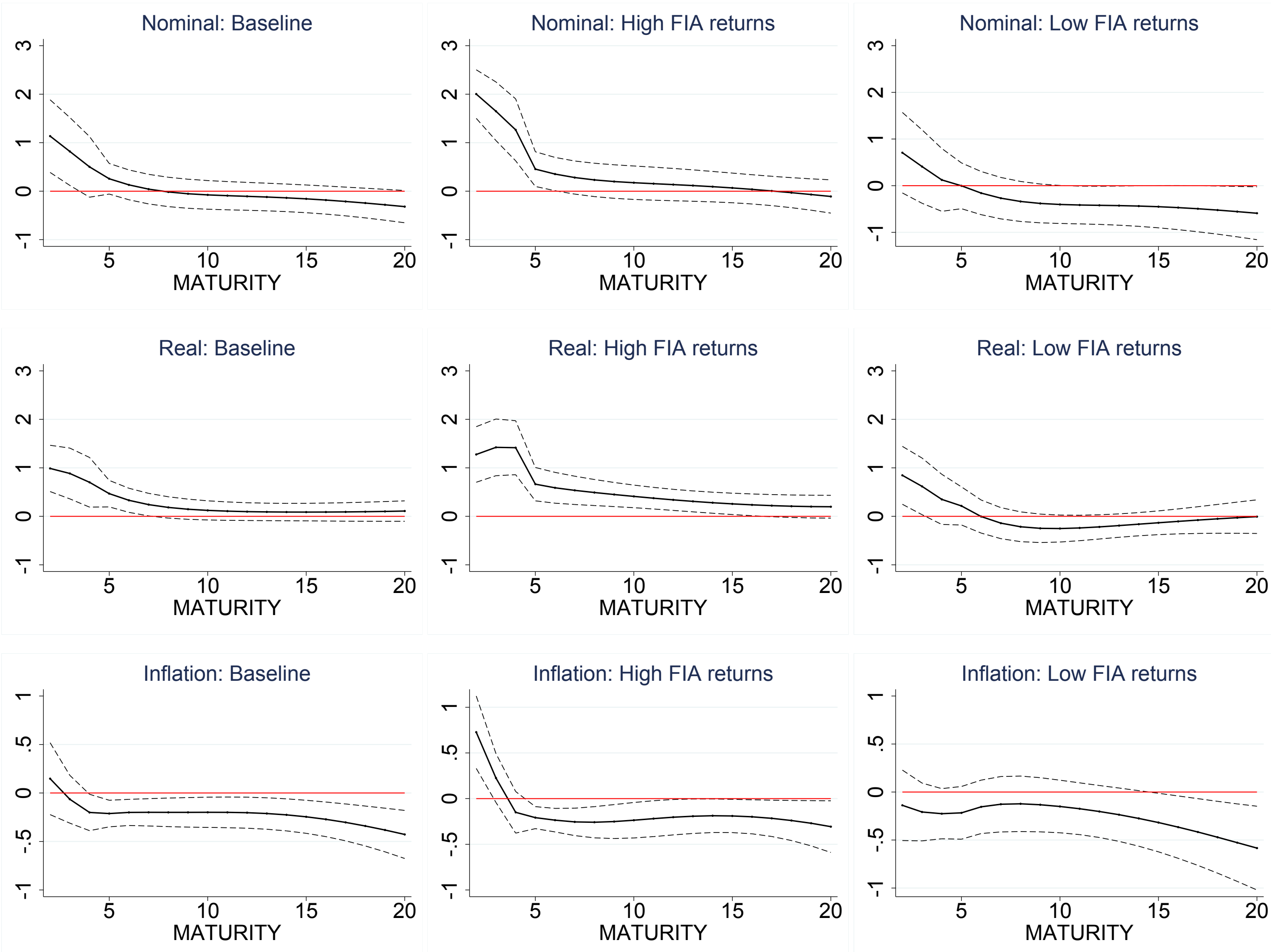
Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/01/2000 to 19/03/2014, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 106 observations on which the policy news shock is computed and each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 74 observations (starting in 2004).

Figure 3: Decomposition of US real forward rates into expected and risk premia components



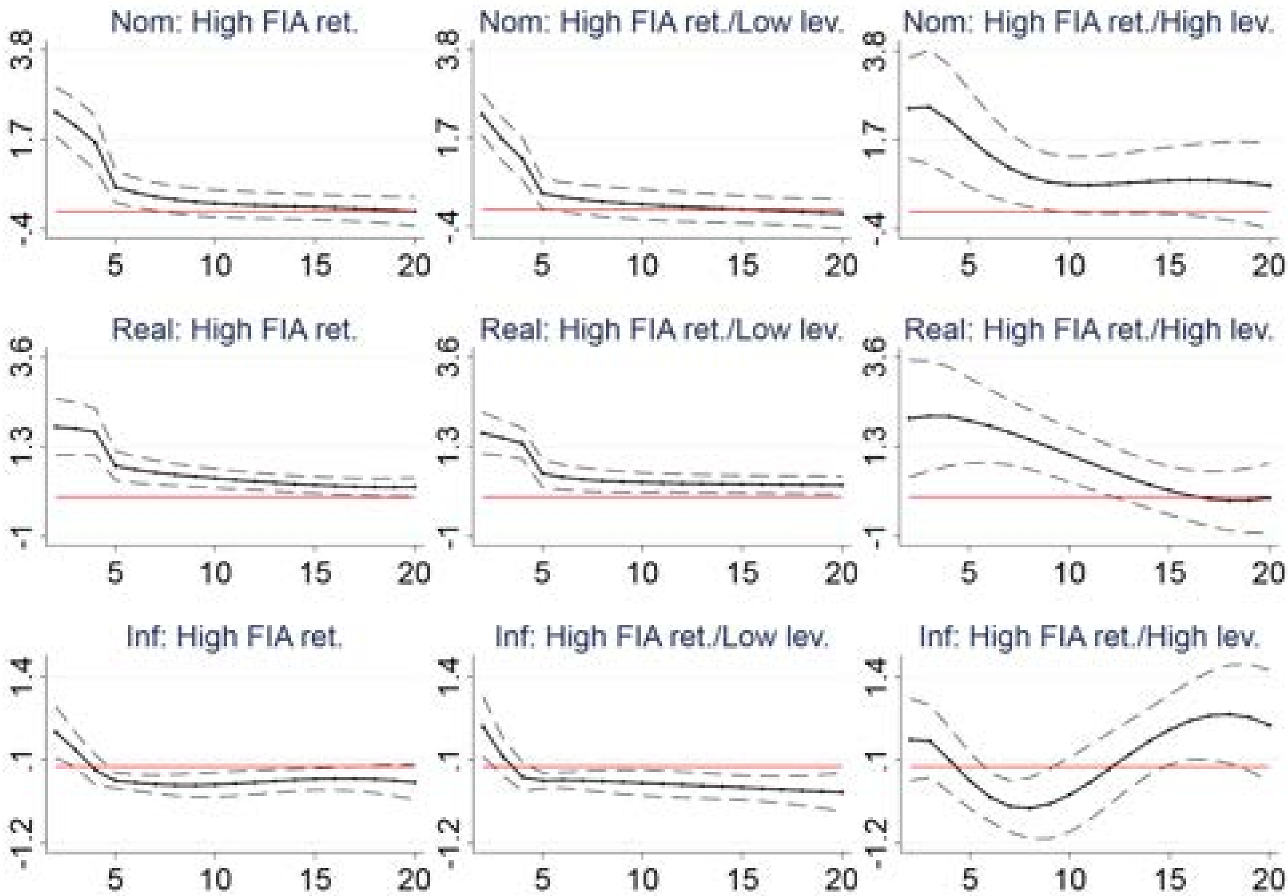
Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for the expected average future real forward rate (first row) and real risk premium (second row) at maturities $\tau \in (2, 10)$, together with 90% confidence intervals based on robust standard errors. The measures are based on the decomposition introduced in Abrahams, Adrian, Crump, Moench, and Yu (2016) and we follow Nakamura and Steinsson (2018) by grouping the term premium, liquidity premium and model error into a single risk premium component. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the noise measure is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/01/2000 to 19/03/2014, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 106 observations on which the policy news shock is computed and each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 74 observations (starting in 2004).

Figure 4: Impact of FIA hedge fund returns on the transmission of MP shocks to US forward rates



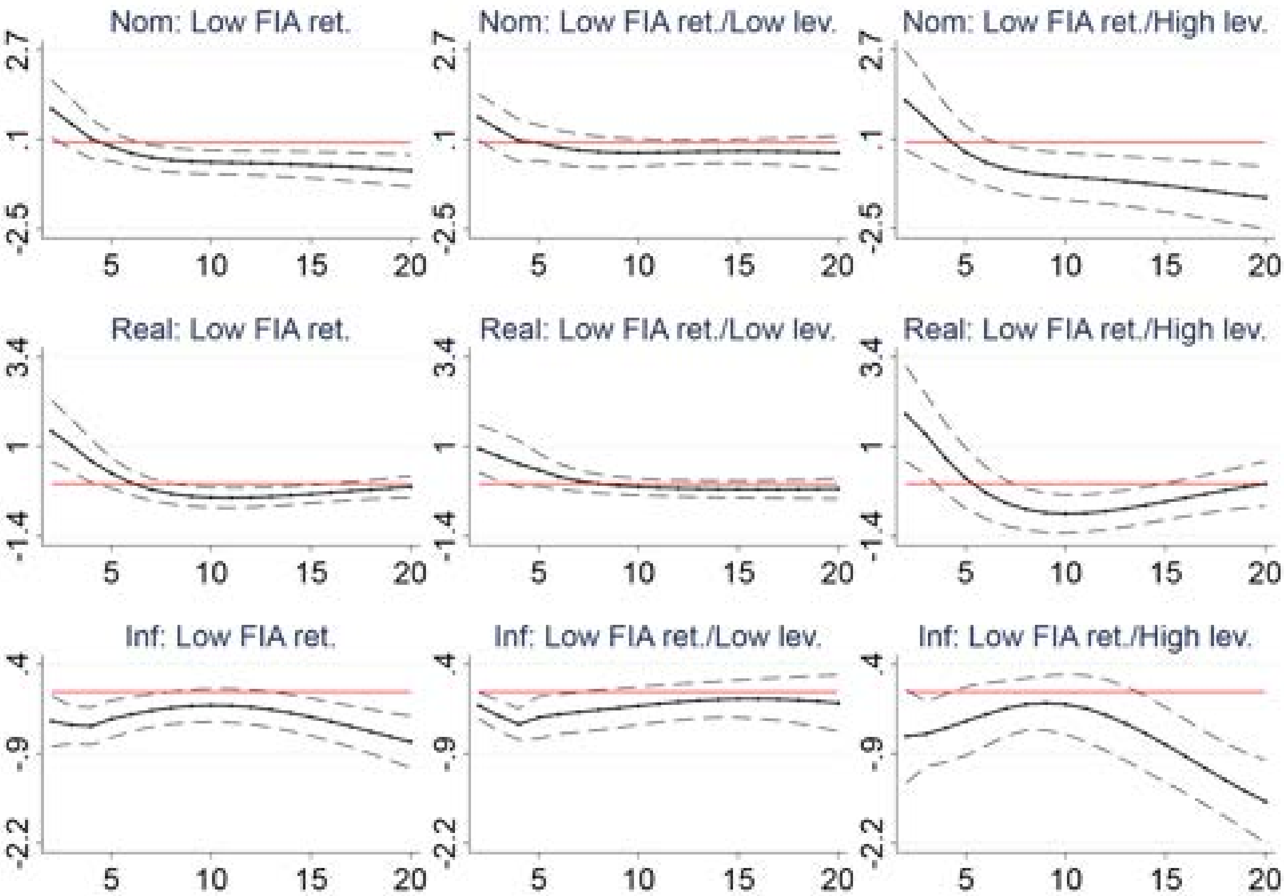
Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (High FIA returns) and third (High FIA returns) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the hedge fund return index is above (resp. below) its median level. The index measures the average return of the fixed-income arbitrage hedge funds in the Barclays database. The sample includes all regularly scheduled FOMC meetings from 01/01/2000 to 19/03/2014, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 106 observations on which the policy news shock is computed and each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 74 observations (starting in 2004).

Figure 5: Impact of dealers' leverage on monetary policy transmission when hedge funds are unconstrained



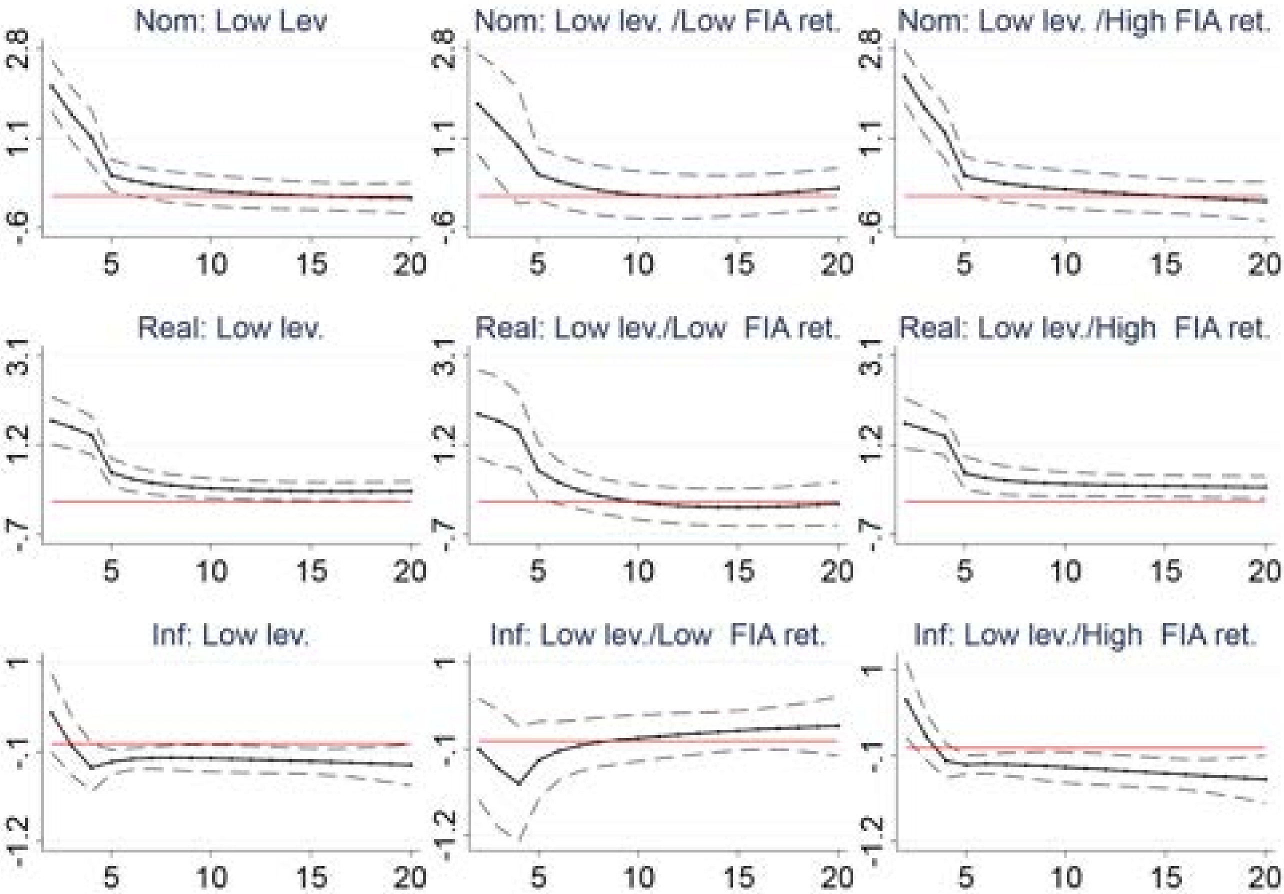
Note: The first column (High FIA returns) plots estimates of $\gamma^{i}_{hl,\tau}$ in regression (2) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$ for the subset of scheduled FOMC announcements for which the hedge fund return index is above its median level. The index measures the average return of the fixed-income arbitrage hedge funds in the Barclays database. 90% confidence intervals based on robust standard errors are provided around the estimates. The second (High FIA returns/Low leverage) and third (High FIA returns/High leverage) columns respectively present parameter estimates for the two subgroups obtained by further dividing the "High FIA returns" announcements into two subgroups depending on whether the intermediary leverage measure of He, Kelly, and Manela (2017) is below/above median. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 76 observations (out of 152) for the "High FIA returns" results and 38 observations for the "High FIA returns/Low leverage" and "High FIA returns/High leverage" subgroups. Regression results for the 2, 3 and 4-year forward rates are based on a sample starting in 2004.

Figure 6: Impact of dealers' leverage on monetary policy transmission when hedge funds are constrained



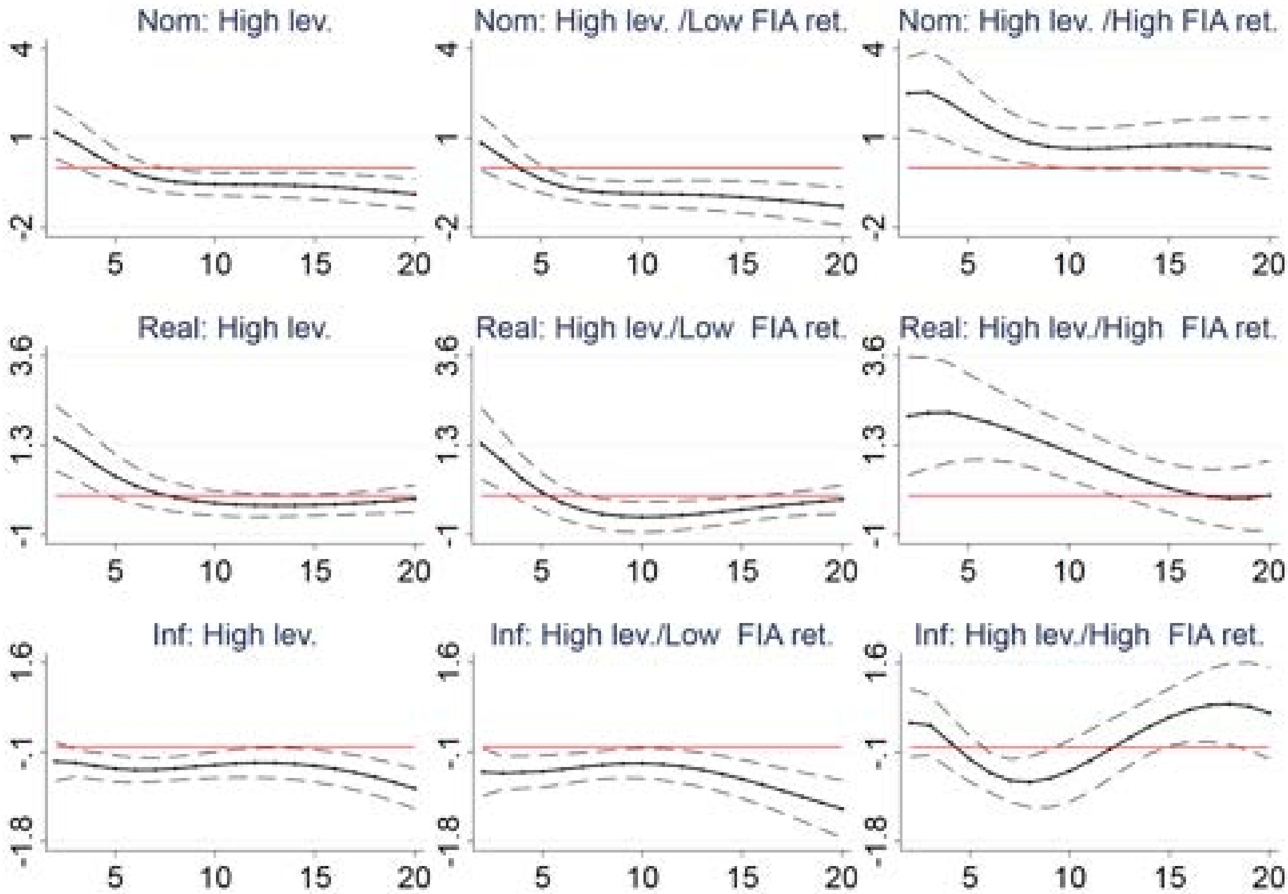
Note: The first column (Low FIA returns) plots estimates of $\gamma^{i}_{ll,\tau}$ in regression (2) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$ for the subset of scheduled FOMC announcements for which the hedge fund return index is below its median level. The index measures the average return of the fixed-income arbitrage hedge funds in the Barclays database. 90% confidence intervals based on robust standard errors are provided around the estimates. The second (Low FIA returns/Low leverage) and third (Low FIA returns/High leverage) columns respectively present parameter estimates for the two subgroups obtained by further dividing the "Low FIA returns" announcements into two buckets depending on whether the intermediary leverage measure of He, Kelly, and Manela (2017) is below/above median. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 76 observations (out of 152) for the "High FIA returns" results and 38 observations for the "High FIA returns/Low leverage" and "High FIA returns/High leverage" subgroups. Regression results for the 2, 3 and 4-year forward rates are based on a sample starting in 2004.

Figure 7: Impact of hedge funds' returns on monetary policy transmission when dealers are unconstrained



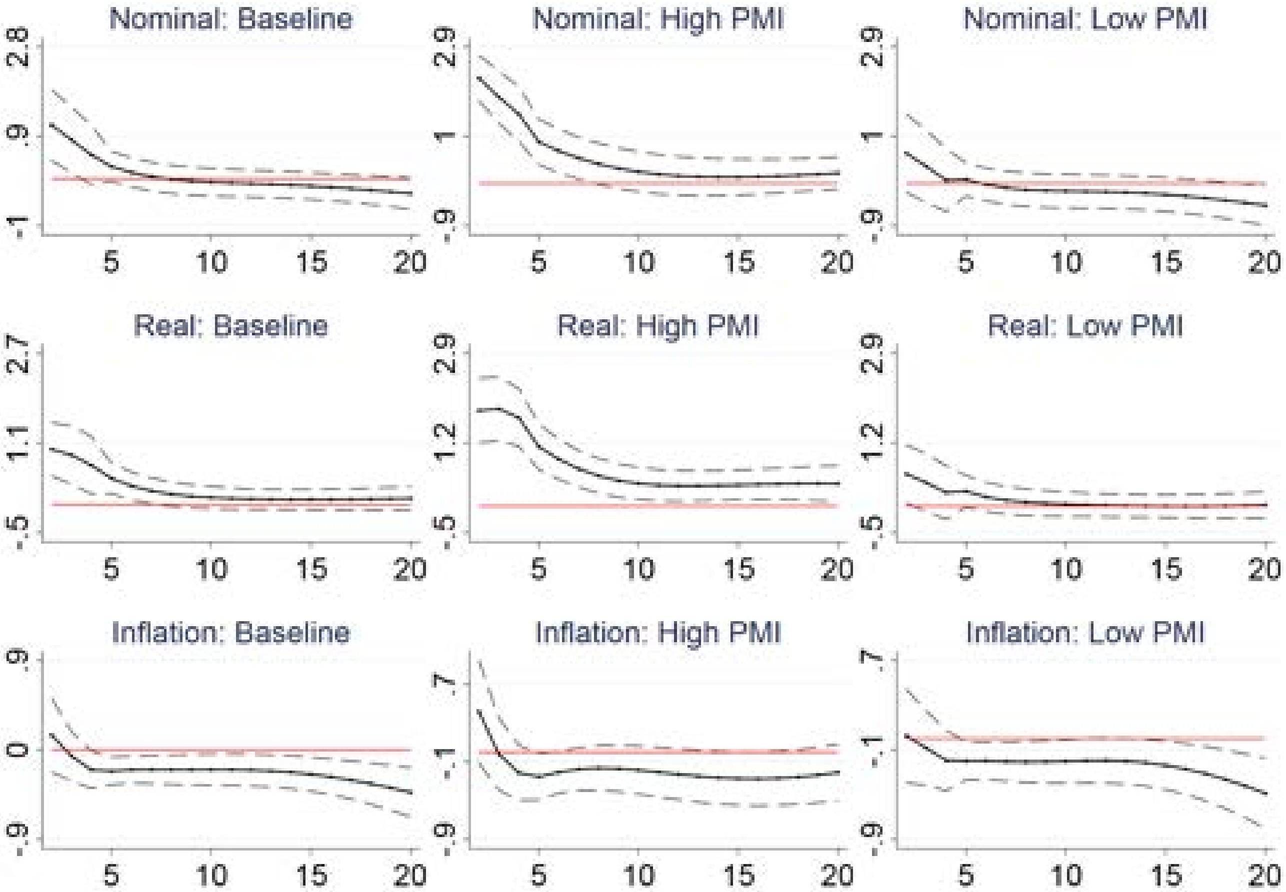
Note: The first column (Low leverage) plots estimates of $\gamma^{i}_{hl,\tau}$ in regression (2) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$ for the subset of scheduled FOMC announcements for which the intermediary leverage measure of He, Kelly, and Manela (2017) is below its median level. 90% confidence intervals based on robust standard errors are provided around the estimates. The second (Low leverage/Low FIA returns) and third (Low leverage/High FIA returns) columns respectively present parameter estimates for the two subgroups obtained by further dividing the "Low leverage" announcements into two buckets depending on whether the FIA hedge fund return index is below/above median. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 76 observations (out of 152) for the "Low leverage" results and 38 observations for the "Low leverage/Low FIA returns" and "Low leverage/High FIA returns" subgroups. Regression results for the 2, 3 and 4-year forward rates are based on a sample starting in 2004.

Figure 8: Impact of hedge funds' returns on monetary policy transmission when dealers are constrained



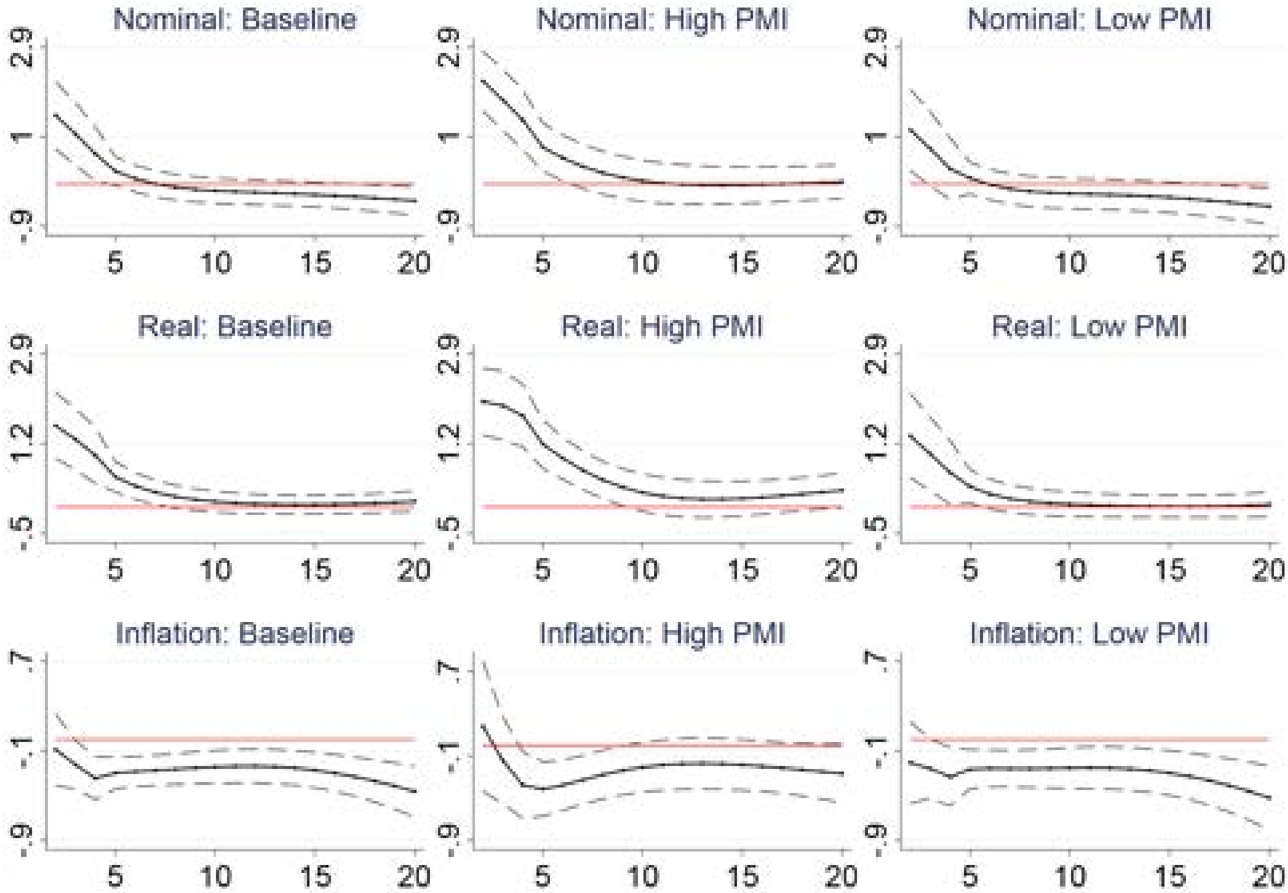
Note: The first column (High leverage) plots estimates of $\gamma^{i}_{ll,\tau}$ in regression (2) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$ for the subset of scheduled FOMC announcements for which the intermediary leverage measure of He, Kelly, and Manela (2017) is above its median level. 90% confidence intervals based on robust standard errors are provided around the estimates. The second (High leverage/Low FIA returns) and third (High leverage/High FIA returns) columns respectively present parameter estimates for the two subgroups obtained by further dividing the "High leverage" announcements into two buckets depending on whether the FIA hedge fund return index is below/above median. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 76 observations (out of 152) for the "High leverage" results and 38 observations for the "High leverage/Low FIA returns" and "High leverage/High FIA returns" subgroups. Regression results for the 2, 3 and 4-year forward rates are based on a sample starting in 2004.

Figure 9: Impact of economic conditions on the transmission of MP shocks to US forward rates



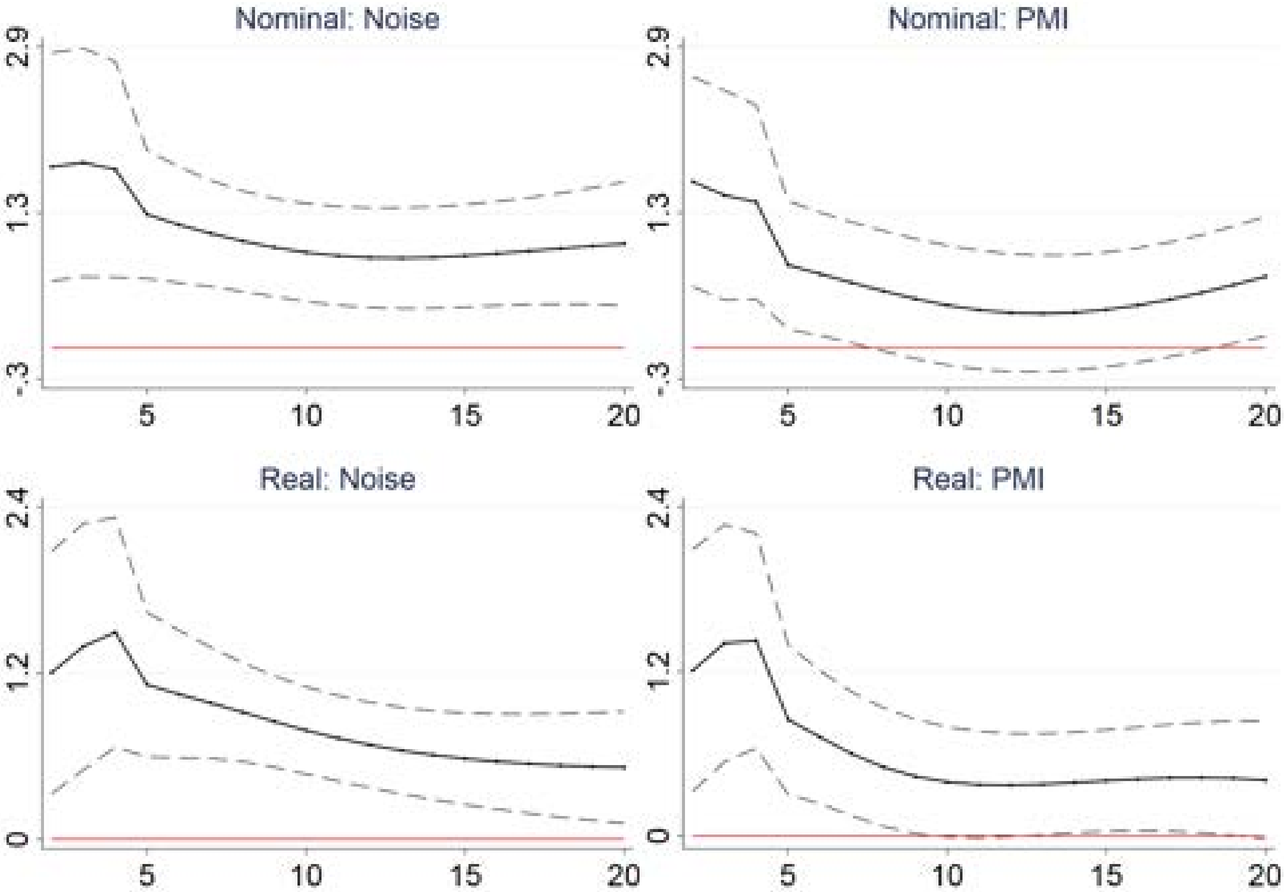
Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (High PMI) and third (Low PMI) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the ISM manufacturing Purchasing Managers Index (PMI) is above (resp. below) its median level. The sample includes all regularly scheduled FOMC meetings from 01/01/2000 to 19/03/2014, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 106 observations on which the policy news shock is computed and each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 74 observations (starting in 2004).

Figure 10: Impact of economic conditions on the transmission of MP shocks to US forward rates: extended sample



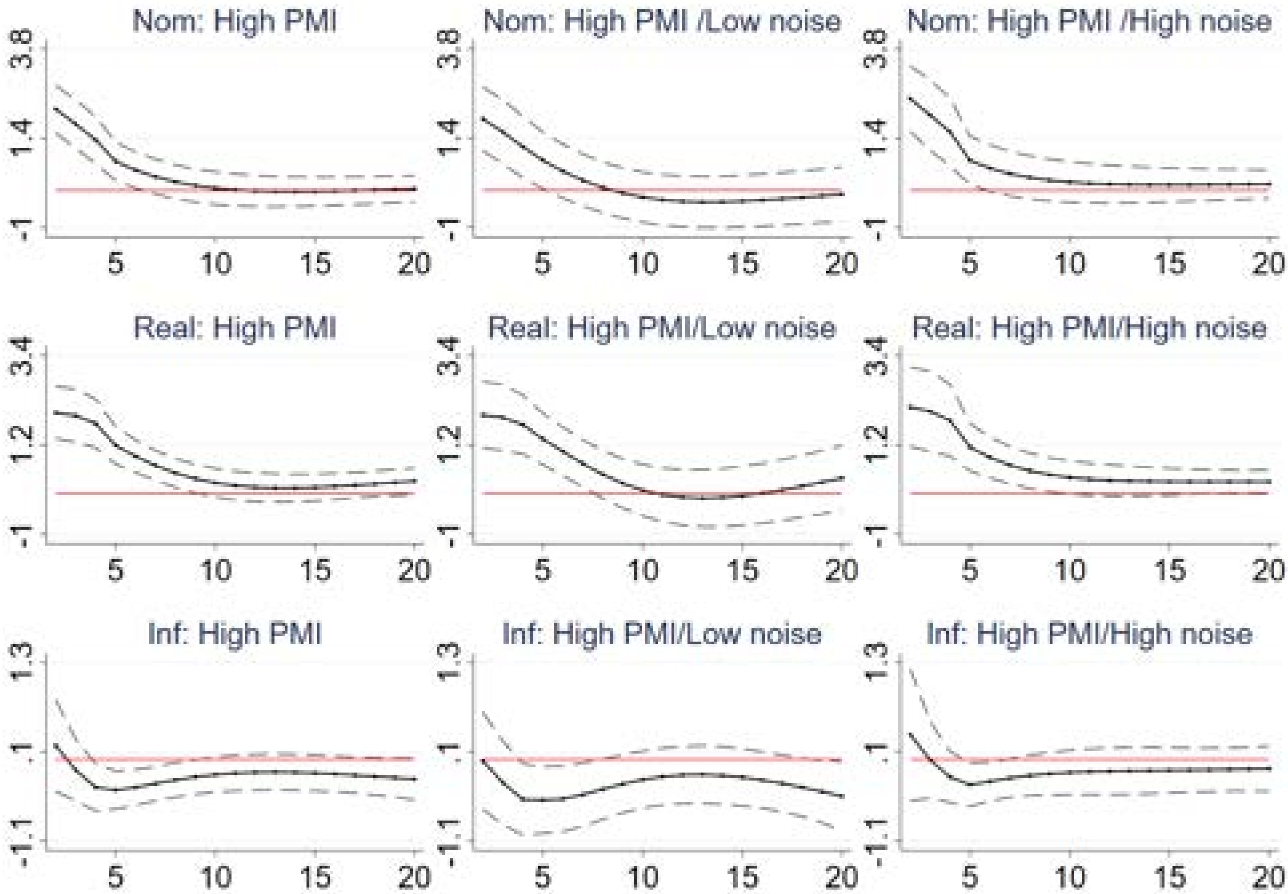
Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (High PMI) and third (Low PMI) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the ISM manufacturing Purchasing Managers Index (PMI) is above (resp. below) its median level. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 152 observations on which each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 120 observations (starting in 2004).

Figure 11: Statistical significance of the state dependence: yield curve noise and PMI



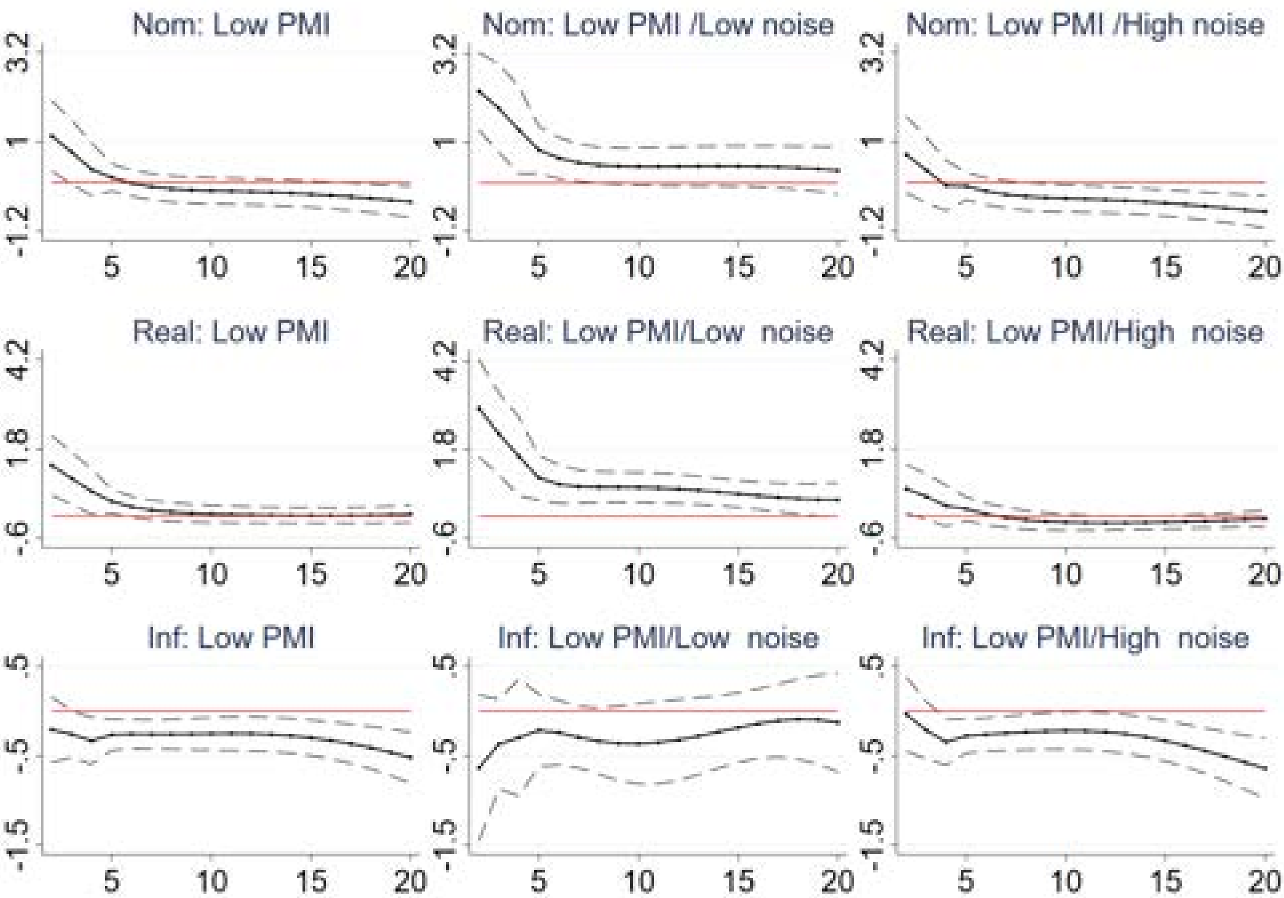
Note: The charts plot the estimates of $\gamma^{i}_{h-l,\tau}$ in regression (3) for each nominal (first row) and real forward rate (second row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The first column presents the results for the yield curve noise and the second column for the ISM manufacturing Purchasing Managers Index (PMI). The sample includes all regularly scheduled FOMC meetings from 01/01/2000 to 19/03/2014, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 106 observations on which the policy news shock is computed and each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 74 observations (starting in 2004).

Figure 12: Impact of yield curve noise on monetary policy transmission when economic conditions are favourable



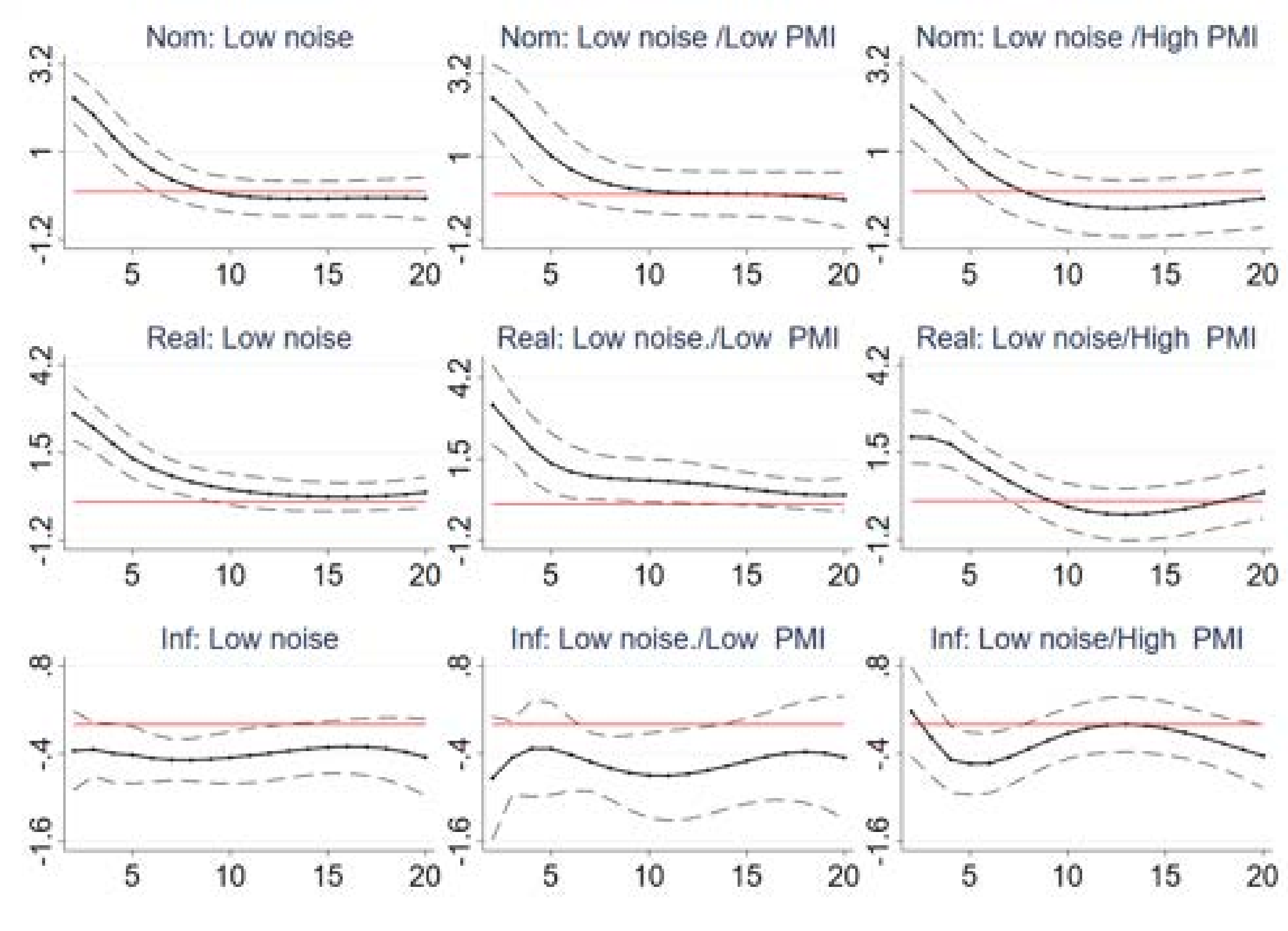
Note: The first column (High PMI) plots estimates of $\gamma^i_{hl,\tau}$ in regression (2) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$ for the subset of scheduled FOMC announcements for which the manufacturing PMI is above its median level. 90% confidence intervals based on robust standard errors are provided around the estimates. The second (High PMI/Low noise) and third (High PMI/High noise) columns respectively present parameter estimates for the two subgroups obtained by further dividing the "High PMI" announcements into two subgroups depending on whether the yield curve noise measure is below/above median. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 76 observations (out of 152) for the "High PMI" results and 38 observations for the "High PMI/Low noise" and "High PMI/High noise" subgroups. Regression results for the 2, 3 and 4-year forward rates are based on a sample starting in 2004.

Figure 13: Impact of yield curve noise on monetary policy transmission when economic conditions are depressed



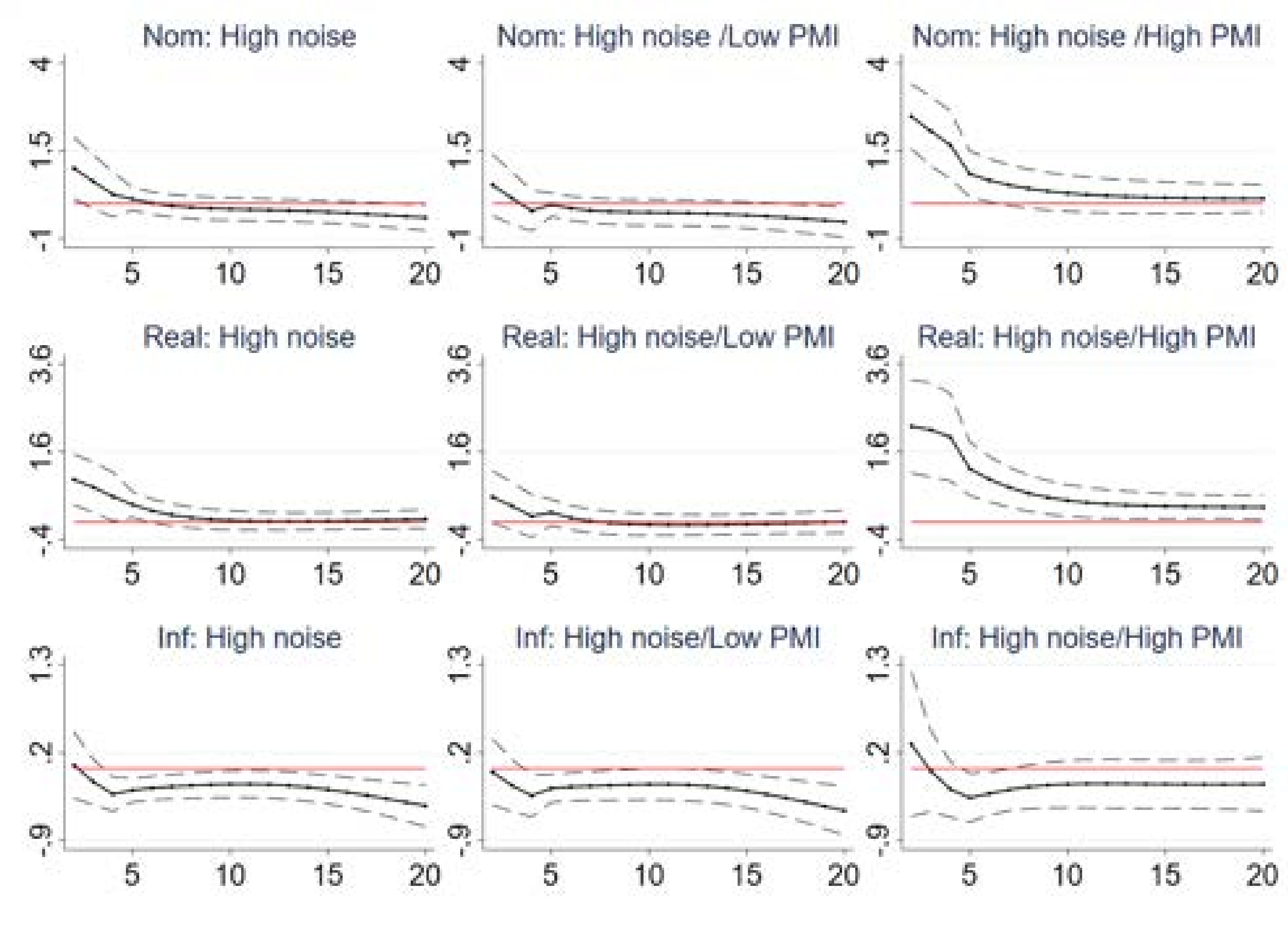
Note: The first column (Low PMI) plots estimates of $\gamma^i_{hl,\tau}$ in regression (2) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$ for the subset of scheduled FOMC announcements for which the manufacturing PMI is below its median level. 90% confidence intervals based on robust standard errors are provided around the estimates. The second (Low PMI/Low noise) and third (Low PMI/High noise) columns respectively present parameter estimates for the two subgroups obtained by further dividing the "Low PMI" announcements into two subgroups depending on whether the yield curve noise measure is below/above median. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 76 observations (out of 152) for the "Low PMI" results and 38 observations for the "Low PMI/Low noise" and "Low PMI/High noise" subgroups. Regression results for the 2, 3 and 4-year forward rates are based on a sample starting in 2004.

Figure 14: Impact of economic conditions on monetary policy transmission when yield curve noise is low



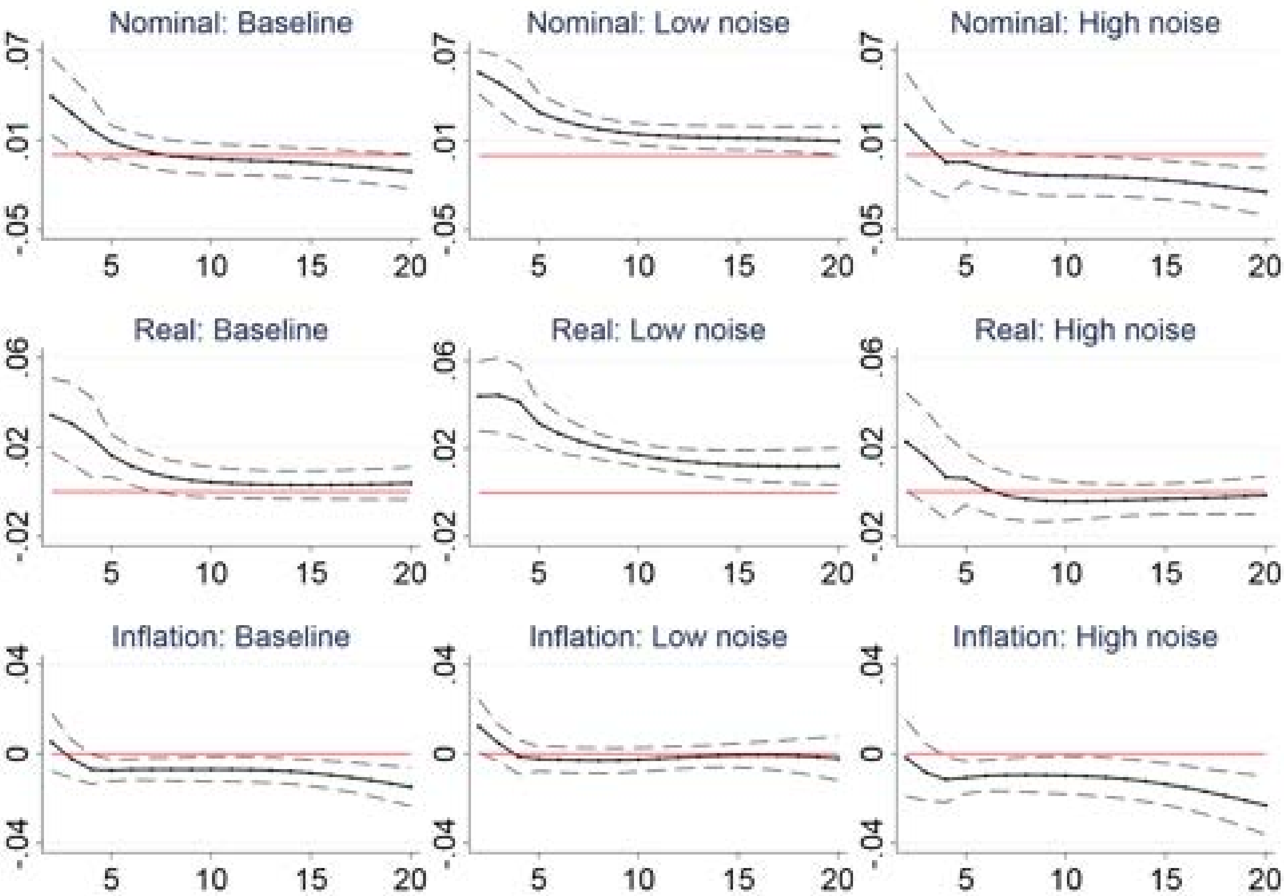
Note: The first column (Low noise) plots estimates of $\gamma^i_{hl,\tau}$ in regression (2) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$ for the subset of scheduled FOMC announcements for which the yield curve noise measure is below its median level. 90% confidence intervals based on robust standard errors are provided around the estimates. The second (Low noise/Low PMI) and third (Low noise/High PMI) columns respectively present parameter estimates for the two subgroups obtained by further dividing the "Low noise" announcements into two buckets depending on whether the manufacturing PMI is below/above median. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 76 observations (out of 152) for the "Low noise" results and 38 observations for the "Low noise/Low PMI" and "Low noise/High PMI" subgroups. Regression results for the 2, 3 and 4-year forward rates are based on a sample starting in 2004.

Figure 15: Impact of economic conditions on monetary policy transmission when yield curve noise is high



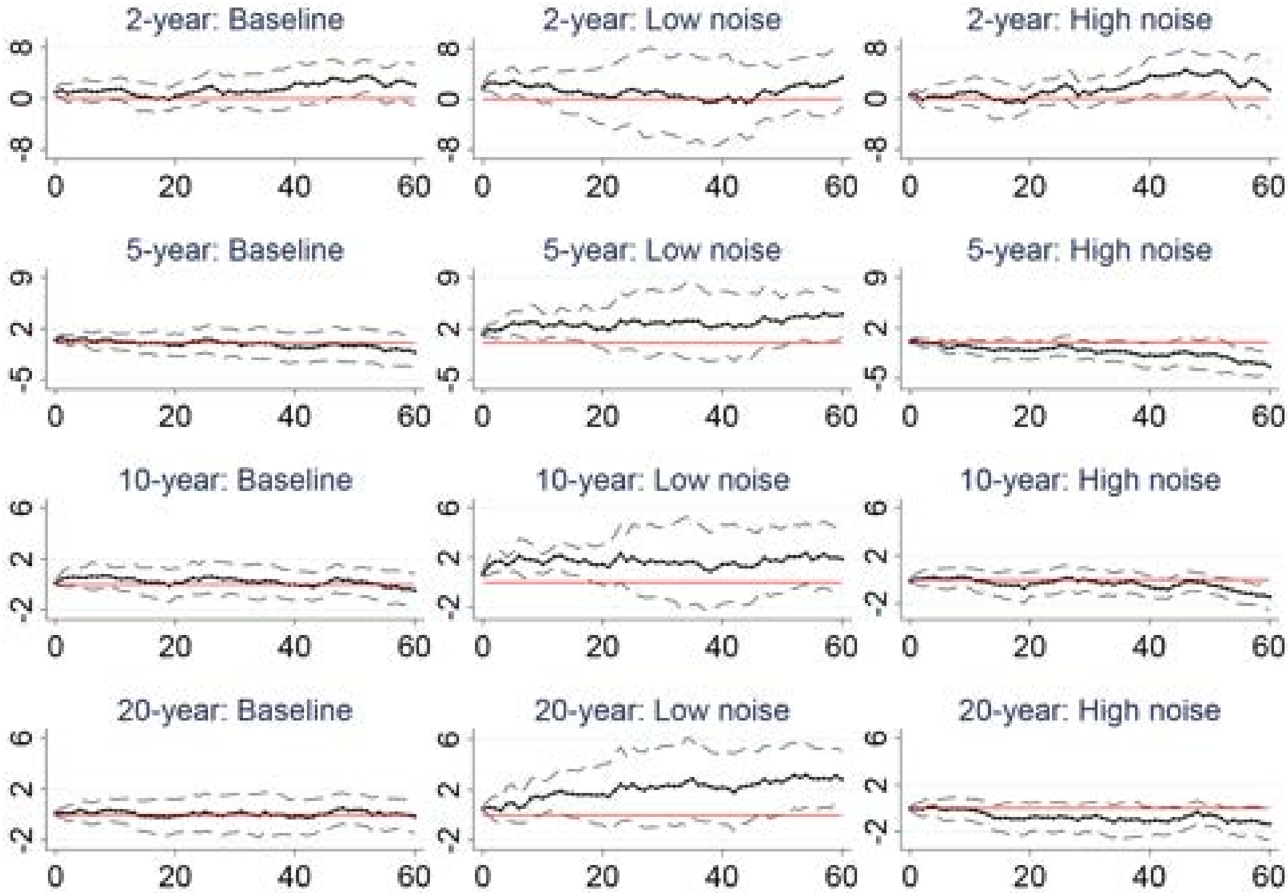
Note: The first column (High noise) plots estimates of $\gamma^{i}_{hl,\tau}$ in regression (2) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$ for the subset of scheduled FOMC announcements for which the yield curve noise measure is above its median level. 90% confidence intervals based on robust standard errors are provided around the estimates. The second (High noise/Low PMI) and third (High noise/High PMI) columns respectively present parameter estimates for the two subgroups obtained by further dividing the "High noise" announcements into two buckets depending on whether the manufacturing PMI is below/above median. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 76 observations (out of 152) for the "High noise" results and 38 observations for the "High noise/Low PMI" and "High noise/High PMI" subgroups. Regression results for the 2, 3 and 4-year forward rates are based on a sample starting in 2004.

Figure 16: Impact of yield curve noise on the transmission of MP shocks to US forward rates: standardized shocks



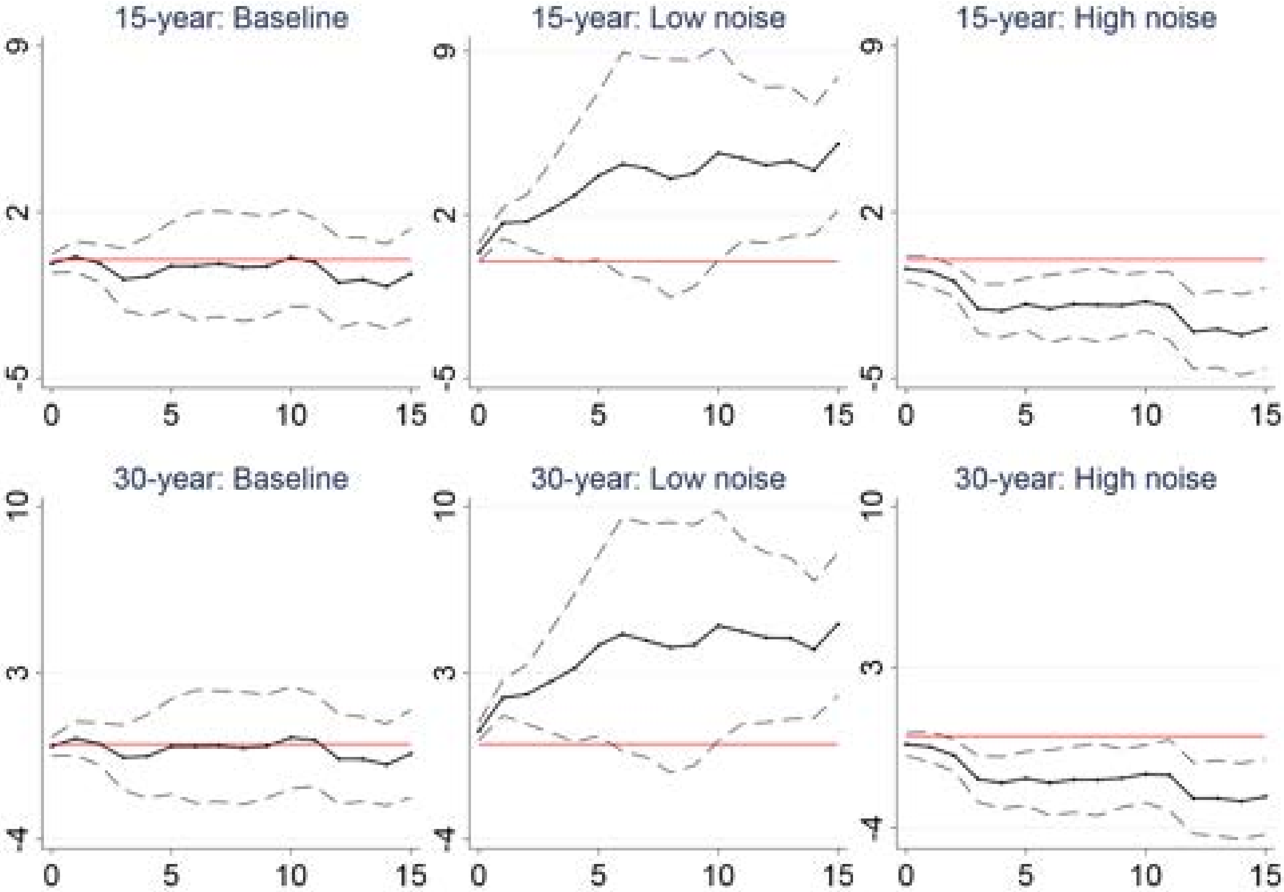
Note: The first column (Baseline) plots estimates of $\gamma^{i}_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^{i}_{hl,\tau}$ and $\gamma^{i}_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/01/2000 to 19/03/2014, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 106 observations on which the policy news shock is computed and each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 74 observations (starting in 2004). Note that the shock series in each estimation are standardized such that the coefficient estimates represent the impact of a one-standard-deviation shock respectively for the full sample (first column) and in the low/high noise buckets (resp. second and third columns).

Figure 17: Impact of yield curve noise on the transmission of MP shocks to US real forward rates at longer horizons



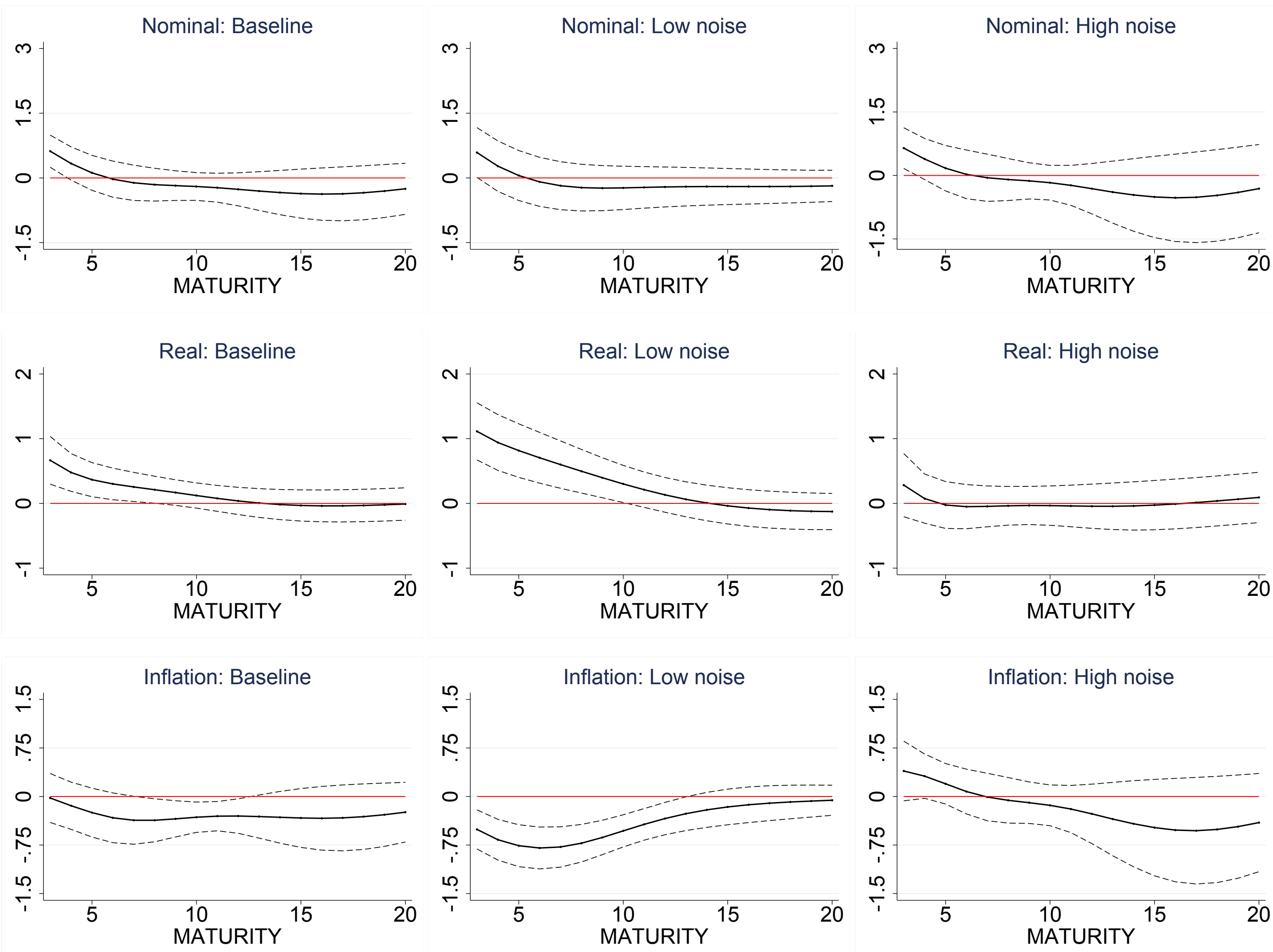
Note: The first column (Baseline) plots estimates of $\gamma^{r,h}_{all,\tau}$ in regression (4) for real forward rates with maturities of 2, 5, 10, and 20 years (in each row) and at horizons ranging from 1 to 60 trading days after the announcement, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^{r,h}_{hl,\tau}$ and $\gamma^{r,h}_{ll,\tau}$ in regression (5), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/01/2000 to 19/03/2014, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 106 observations on which the policy news shock is computed and each regression is estimated. Regression results for the 2-year forward rate are based on a sample size of 74 observations (starting in 2004).

Figure 18: Impact of yield curve noise on the transmission of MP shocks to US fixed mortgage rates



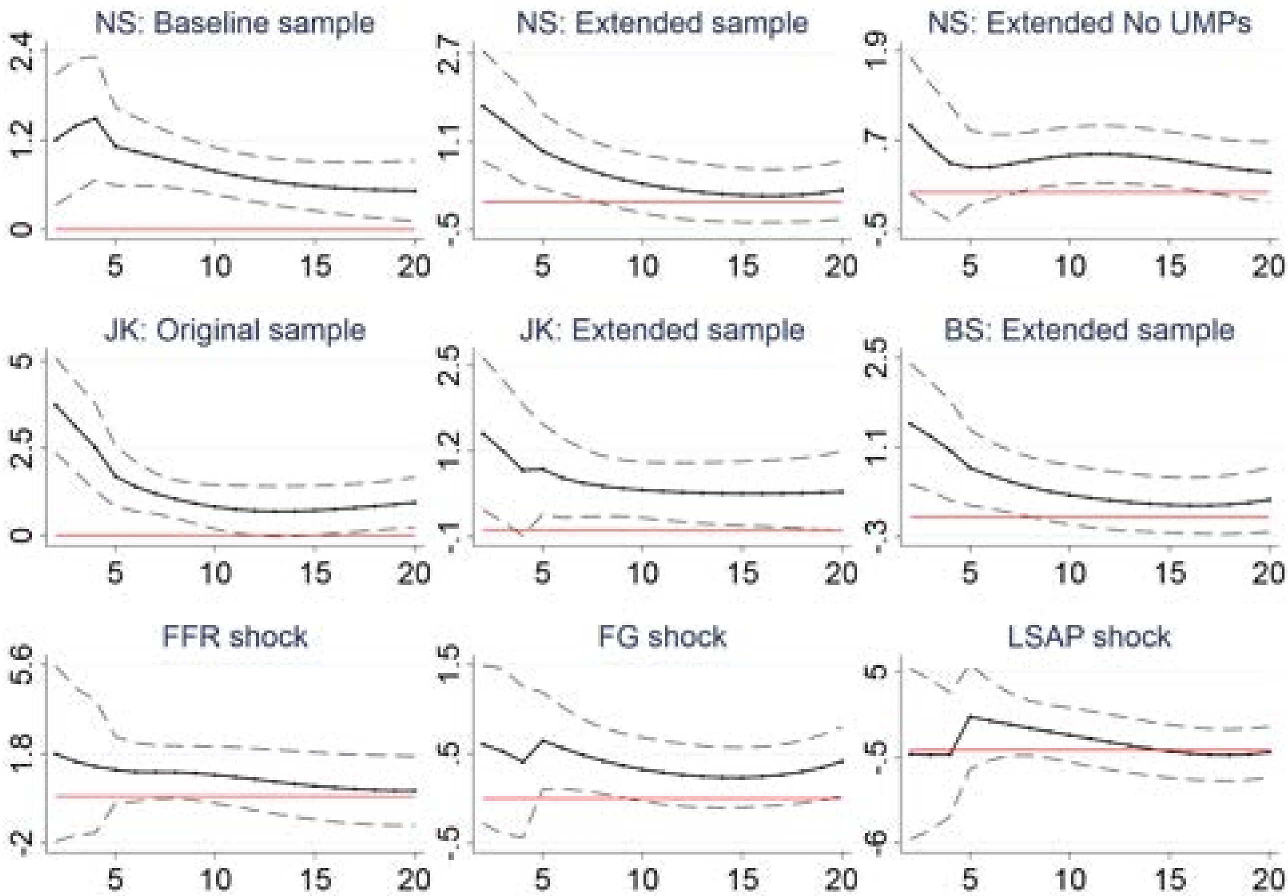
Note: The first column (Baseline) plots estimates of $\gamma^h_{all,\tau}$ in regression (4) for US fixed mortgage rates with maturities of 15 and 30 years (in each row) and at horizons ranging from 1 to 15 weeks after the announcement, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^h_{hl,\tau}$ and $\gamma^{r,h}_{ll,\tau}$ in regression (5), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/01/2000 to 19/03/2014, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 106 observations on which the policy news shock is computed and each regression is estimated.

Figure 19: Impact of yield curve noise on the transmission of MP shocks to UK forward rates: all MPC announcements



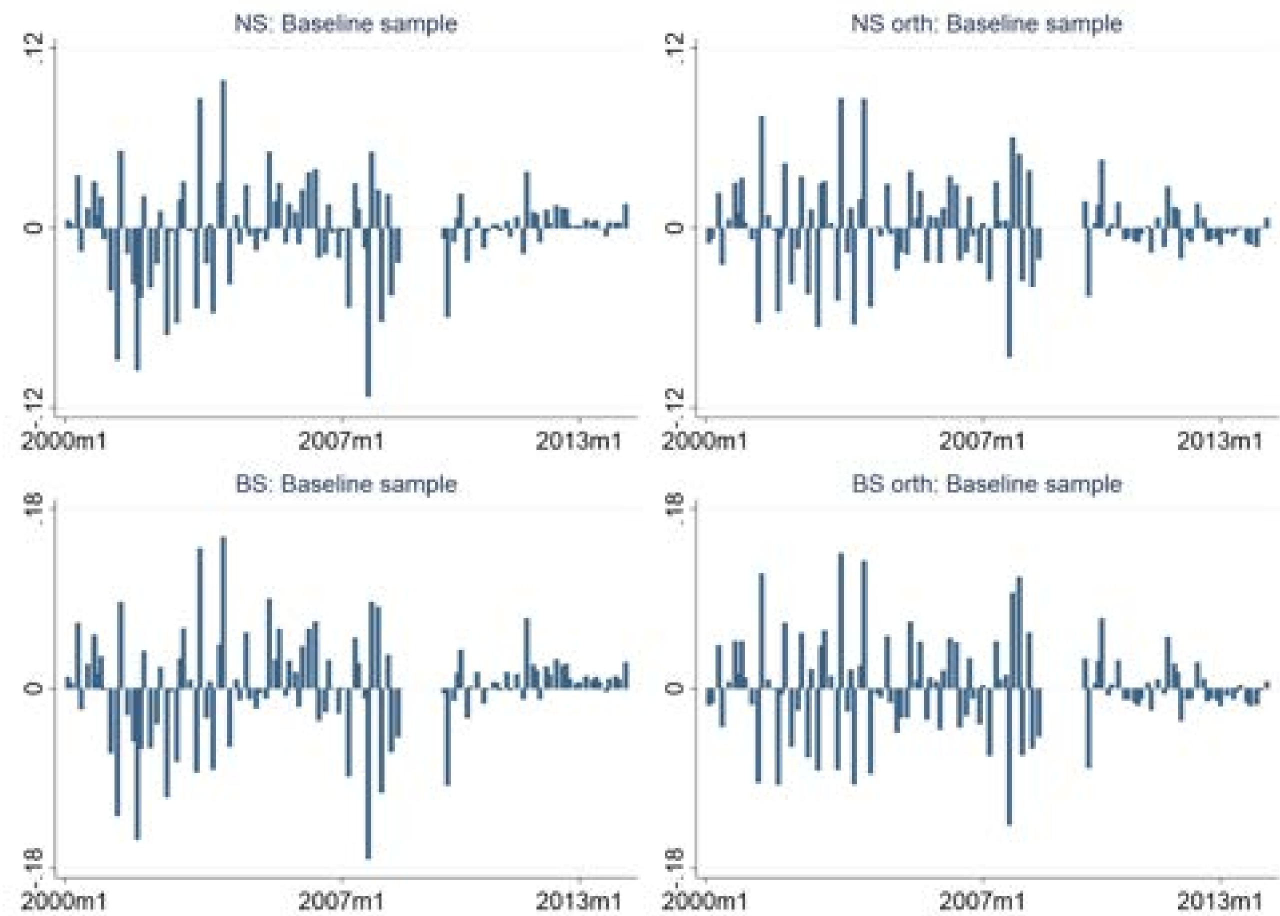
Note: The first column (Baseline) plots estimates of $\gamma^{i}_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (3, 20)$, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^{i}_{hl,\tau}$ and $\gamma^{i}_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled MPC announcements for which the UK yield curve noise measure is below (resp. above) its median level. The sample includes all regularly scheduled MPC meetings from 01/2000 to 12/2019. This corresponds to a sample size of 226 observations on which the policy news shock—obtained as the first principal component extracted from the high-frequency surprises in the first four quarterly Short Sterling Futures contracts—is computed and each regression is estimated.

Figure 20: Robustness of state-dependence for real forward rates to different samples and monetary policy shocks



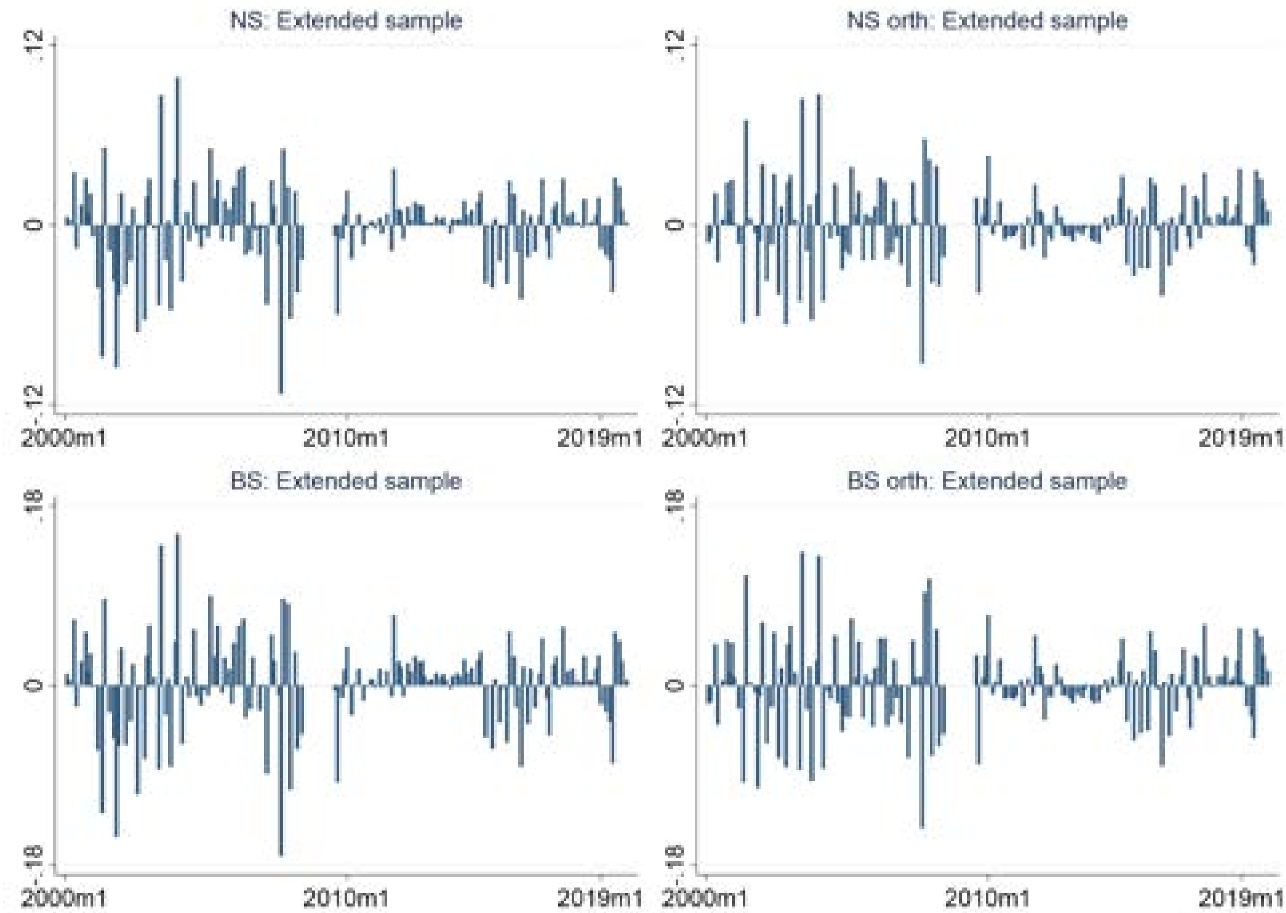
Note: The charts plot the estimates of $\gamma^{r}_{h-l,\tau}$ in regression (3) for each real forward rate with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The first row reports the results for the monetary policy shock of Nakamura and Steinsson (2018) respectively using their baseline sample from 2000/01 to 2014/03 (first column); an extended sample from 2000/01 to 2019/12 with the updated data provided in Acosta (2022) (second column); and the extended sample excluding unconventional monetary policy announcements identified in Table 5 of Cieslak and Schrimpf (2019) (third column). The first two columns of the second row report results for the MP shock of Jarocinski and Karadi (2020) using their orginal sample 2000/01-2016/12 and an extended sample from 2000/01 to 2019/06. The third column of the second row reports the results for the MP shock of Bauer and Swanson (2023b) for the sample from 2000/01 to 2019/12. The last row reports the results for the Federal Funds Rate (FFR), Forward Guidance (FG), and Large Scale Asset Purchase shocks of Swanson (2021) using a sample from 2000/01 to 2019/06. We exclude FOMC meetings taking place between July 2008 and June 2009 from the estimation except in the results based on the MP shock of Jarocinski and Karadi (2020) which only considers the subset of policy announcements for which the high-frequency changes in the S&P 500 and interest rates have opposite signs. Regression results for the 2, 3 and 4-year forward rates are based on samples starting in 2004/01.

Figure 21: Comparison of monetary policy shocks with their orthogonalized version: baseline sample



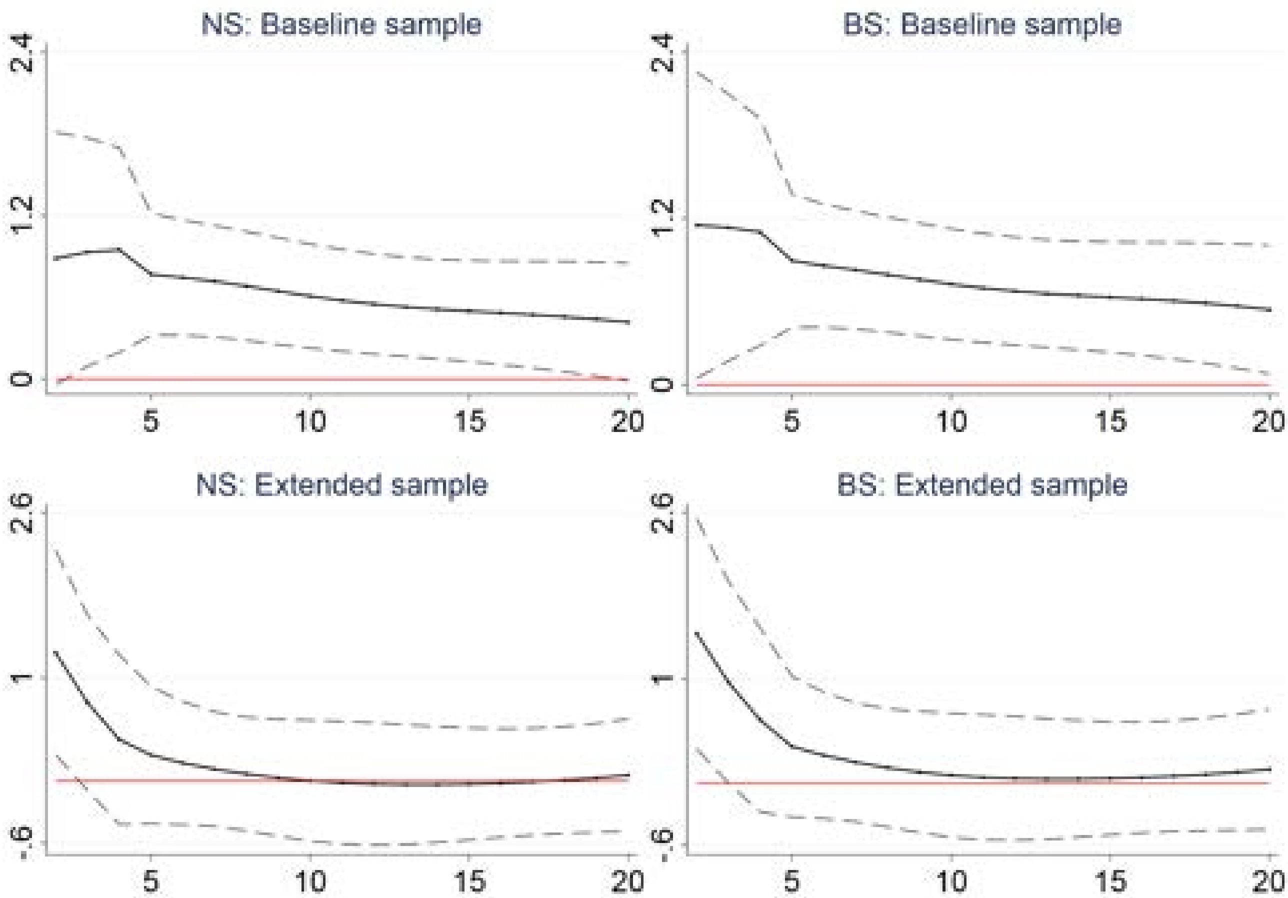
Note: The first column presents the monetary policy shock series of Nakamura and Steinsson (2018) and Bauer and Swanson (2023b), respectively denoted by NS and BS, in the baseline sample from 2000/01 to 2014/03 (excluding FOMC meetings between 2008/07 and 2009/06). The second column displays the corresponding orthogonalized shock series obtained using the approach introduced in Bauer and Swanson (2023b). See Appendix A.2 for additional details. Note that the orthogonalized shock series correspond to the residuals from the regression results reported in columns (4)-(5) of Table 4.

Figure 22: Comparison of monetary policy shocks with their orthogonalized version: extended sample



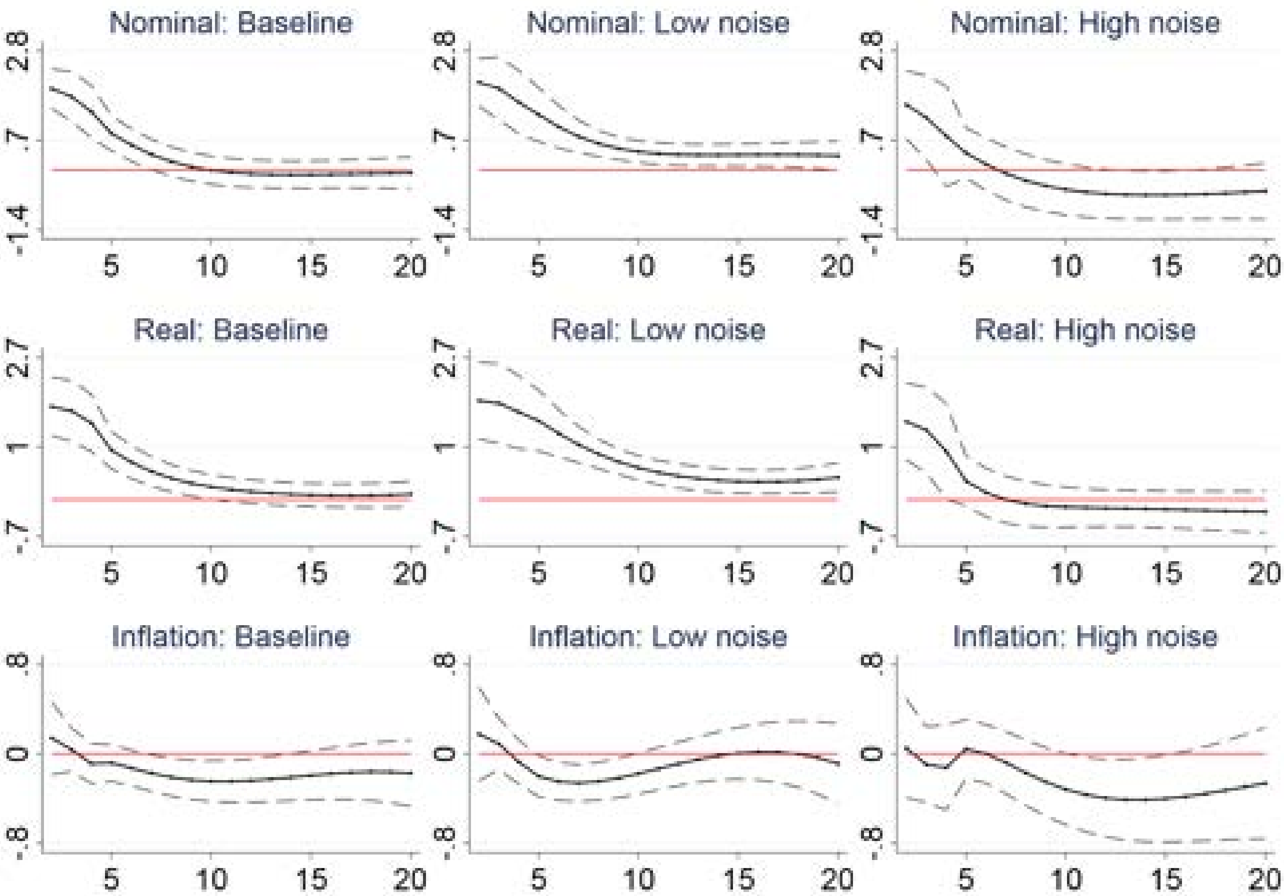
Note: The first column presents the monetary policy shock series of Nakamura and Steinsson (2018) and Bauer and Swanson (2023b), respectively denoted by NS and BS, in the baseline sample from 2000/01 to 2019/12 (excluding FOMC meetings between 2008/07 and 2009/06). The second column displays the corresponding orthogonalized shock series obtained using the approach introduced in Bauer and Swanson (2023b). See Appendix A.2 for additional details. Note that the orthogonalized shock series correspond to the residuals from the regression results reported in columns (8)-(9) of Table 4.

Figure 23: Robustness of state-dependence for real forward rates to orthogonalized monetary policy shocks



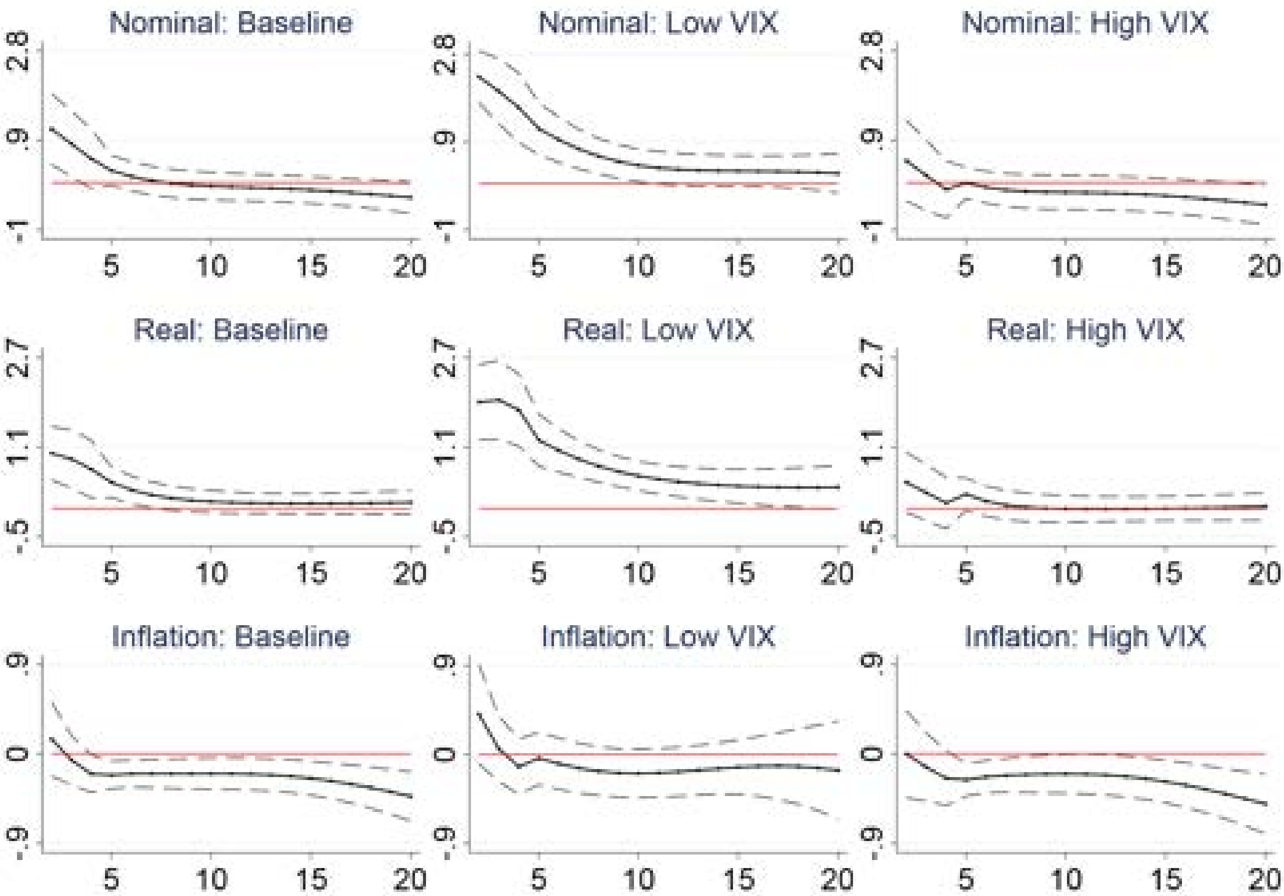
Note: The charts plot the estimates of $\gamma^{r}_{h-l,\tau}$ in regression (3) for each real forward rate with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The first row reports the results for the orthogonalized shocks of Nakamura and Steinsson (2018) and Bauer and Swanson (2023b), respectively denoted by NS and BS, in the baseline sample from 2000/01 to 2014/03. The second row reports the same results for the extended sample from 2000/01 to 2019/12. We exclude FOMC meetings taking place between July 2008 and June 2009 from the estimation. Regression results for the 2, 3 and 4-year forward rates are based on samples starting in 2004/01. Details on the approach used to obtain the orthogonalized shock series can be found in Appendix A.2.

Figure 24: Impact of yield curve noise on the transmission of MP shocks to US forward rates using the orthogonalized Gürkaynak, Sack, and Swanson (2005a) path shock



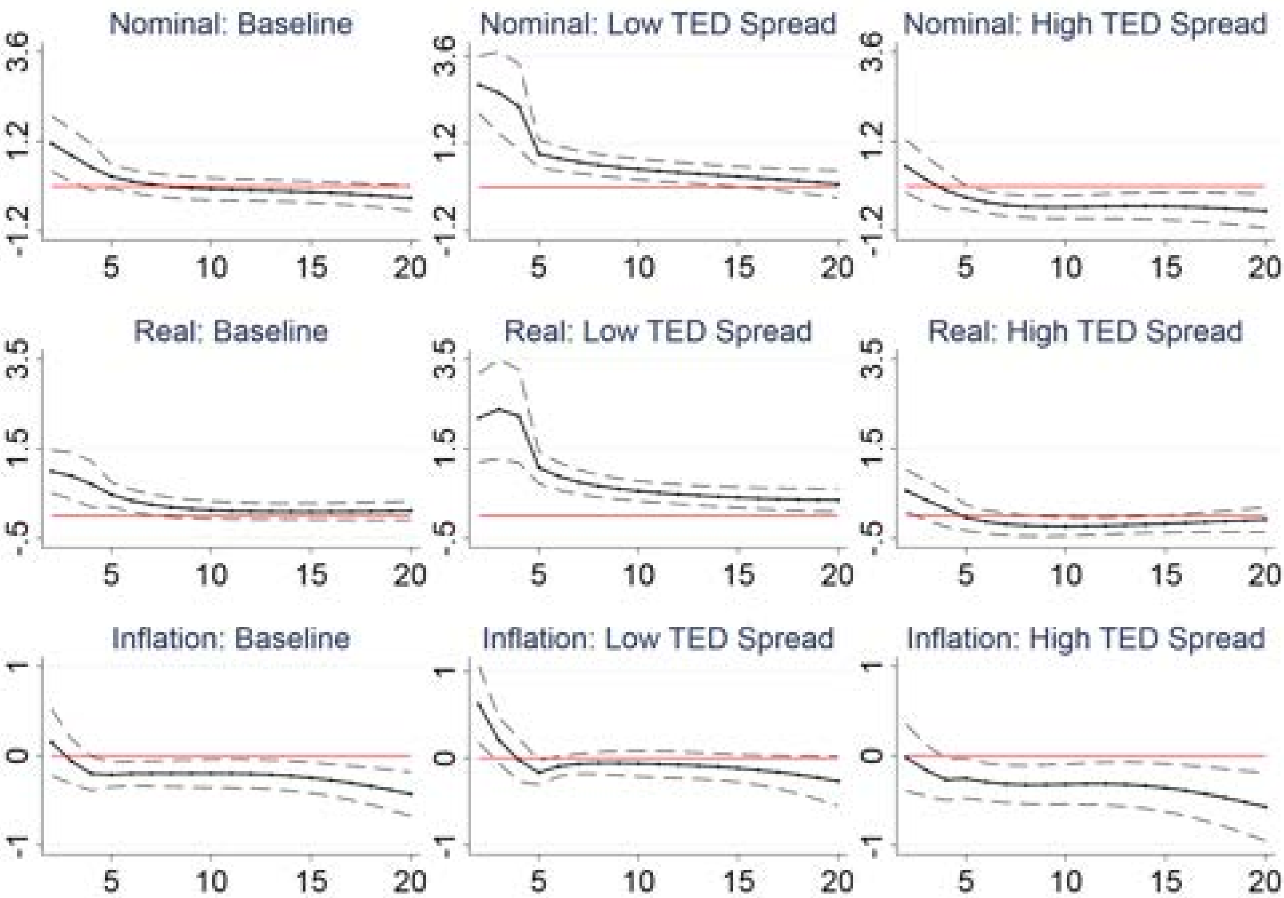
Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/01/2000 to 28/10/2015, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 119 observations on which each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 87 observations (starting in 2004). The monetary policy path shock of Gürkaynak, Sack, and Swanson (2005a) is orthogonalized with respect to private sector growth forecasts obtained from the Blue Chip Surveys following the approach proposed by Karnaukh and Vokata (2022).

Figure 25: Impact of volatility (VIX) on the transmission of MP shocks to US forward rates



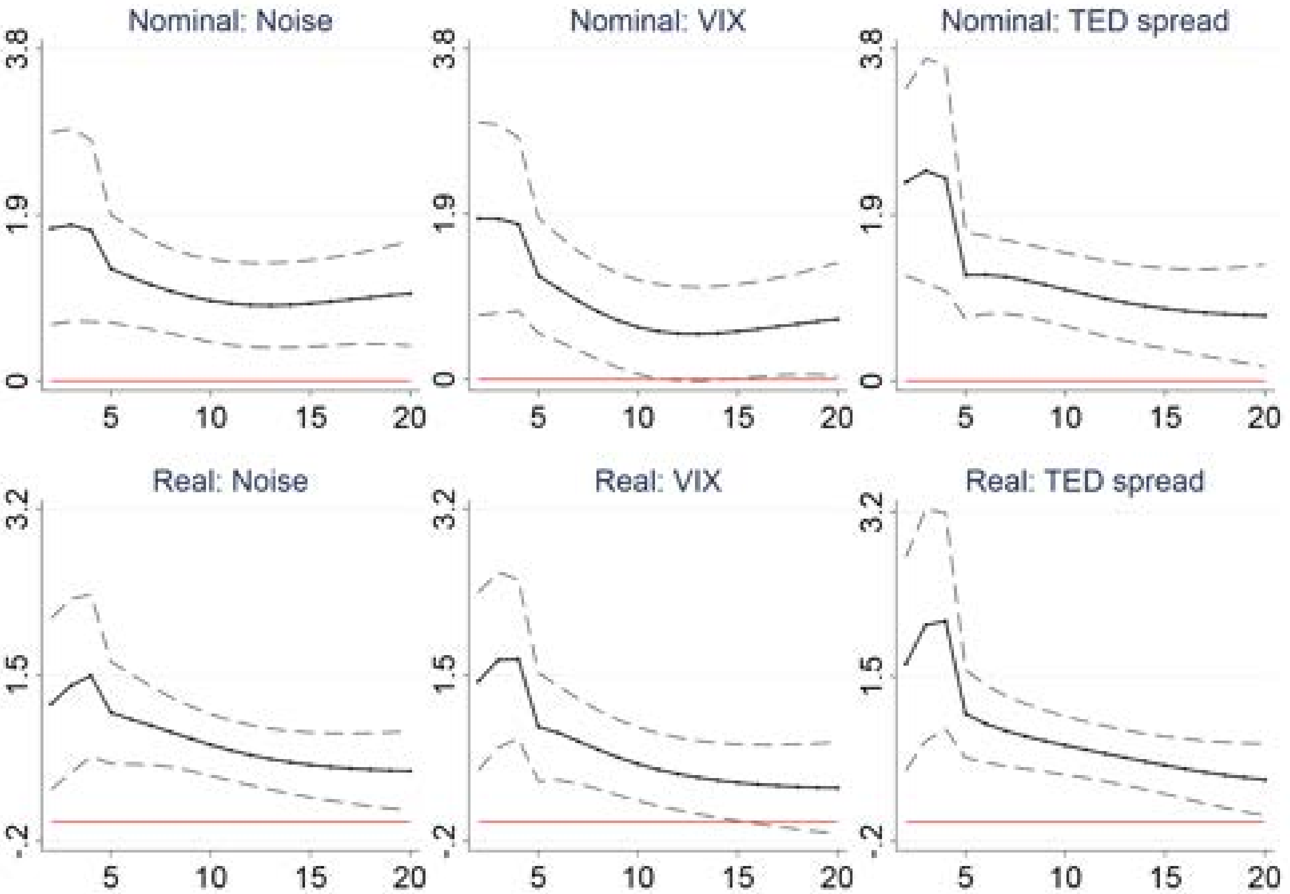
Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (Low VIX) and third (High VIX) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the CBOE VIX index is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/01/2000 to 19/03/2014, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 106 observations on which the policy news shock is computed and each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 74 observations (starting in 2004).

Figure 26: Impact of the TED spread on the transmission of MP shocks to US forward rates



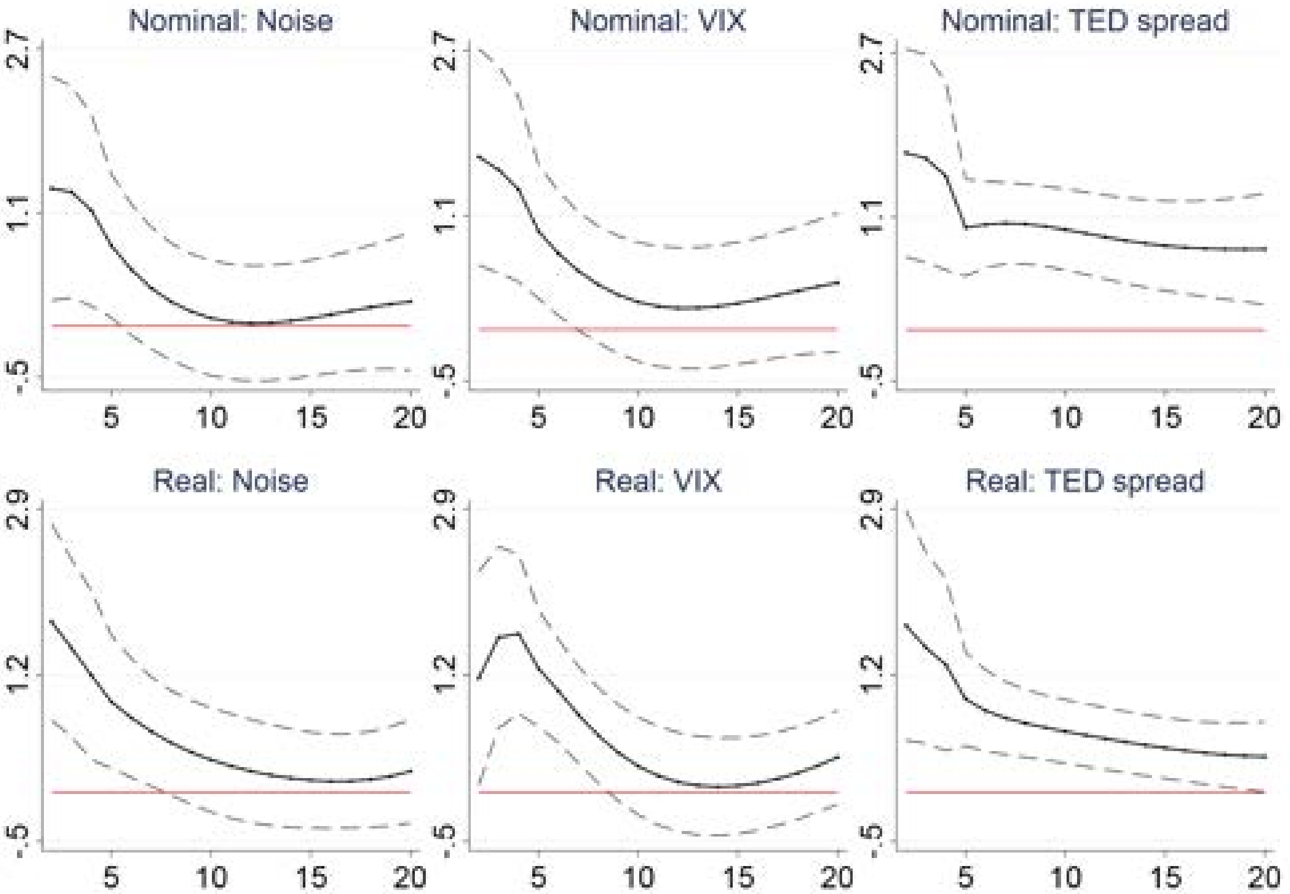
Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (Low TED Spread) and third (High TED Spread) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the T-Bill Eurodollar (TED) spread is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/01/2000 to 19/03/2014, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 106 observations on which the policy news shock is computed and each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 74 observations (starting in 2004).

Figure 27: Statistical significance of the state dependence: yield curve noise, VIX and TED spread



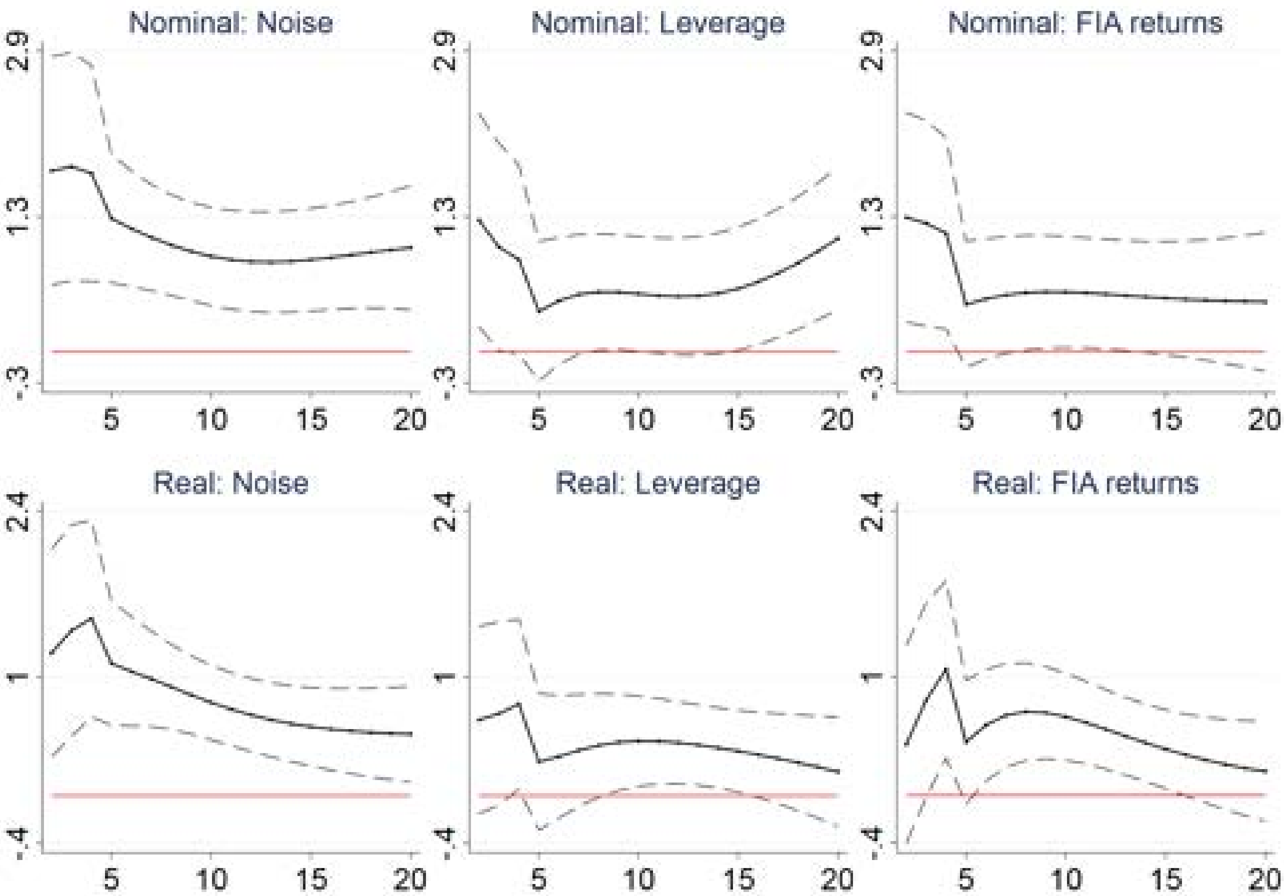
Note: The charts plot the estimates of $\gamma^i_{h-l,\tau}$ in regression (3) for each nominal (first row) and real forward rate (second row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The first column presents the results for the yield curve noise, the second column for the CBOE VIX index, and the third one for T-Bill Eurodollar (TED) spread. The sample includes all regularly scheduled FOMC meetings from 01/01/2000 to 19/03/2014, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 106 observations on which the policy news shock is computed and each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 74 observations (starting in 2004).

Figure 28: Statistical significance of the state dependence: yield curve noise, VIX and TED spread in the extended sample



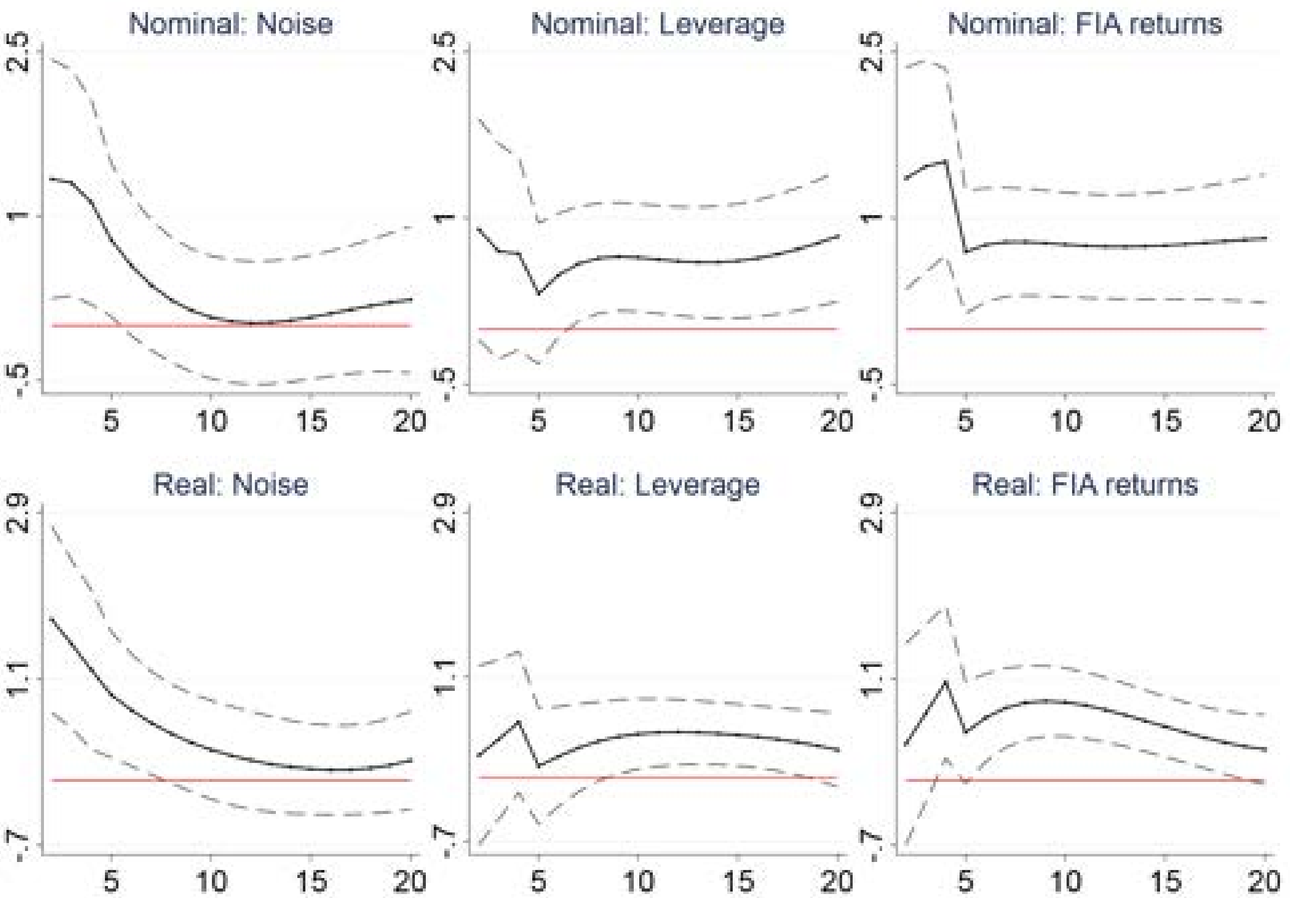
Note: The charts plot the estimates of $\gamma^{i}_{h-l,\tau}$ in regression (3) for each nominal (first row) and real forward rate (second row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The first column presents the results for the yield curve noise, the second column for the CBOE VIX index, and the third one for T-Bill Eurodollar (TED) spread. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 152 observations on which each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 120 observations (starting in 2004).

Figure 29: Statistical significance of the state dependence: yield curve noise, dealers' leverage and FIA returns



Note: The charts plot the estimates of $\gamma^i_{h-l,\tau}$ in regression (3) for each nominal (first row) and real forward rate (second row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The first column presents the results for the yield curve noise, the second column for the intermediary leverage measure of He, Kelly, and Manela (2017), and the third one for the Barclays return index for fixed-income arbitrage (FIA) hedge funds. The sample includes all regularly scheduled FOMC meetings from 01/01/2000 to 19/03/2014, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 106 observations on which the policy news shock is computed and each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 74 observations (starting in 2004).

Figure 30: Statistical significance of the state dependence: yield curve noise, dealers' leverage and FIA returns in the extended sample



Note: The charts plot the estimates of $\gamma^{i}_{h-l,\tau}$ in regression (3) for each nominal (first row) and real forward rate (second row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The first column presents the results for the yield curve noise, the second column for the intermediary leverage measure of He, Kelly, and Manela (2017), and the third one for the Barclays return index for fixed-income arbitrage (FIA) hedge funds. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 152 observations on which each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 120 observations (starting in 2004).

Figure 31: Representativeness of our Sample of US Treasury Transactions

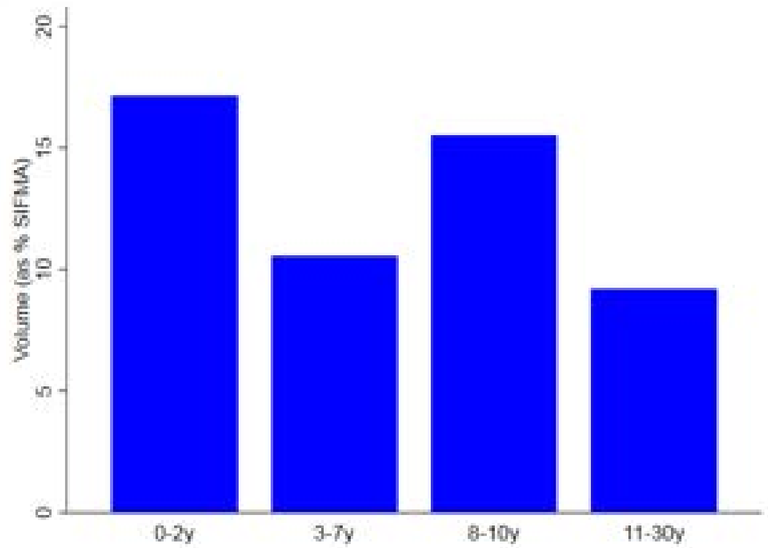


Figure 32: Volume (% SIFMA)

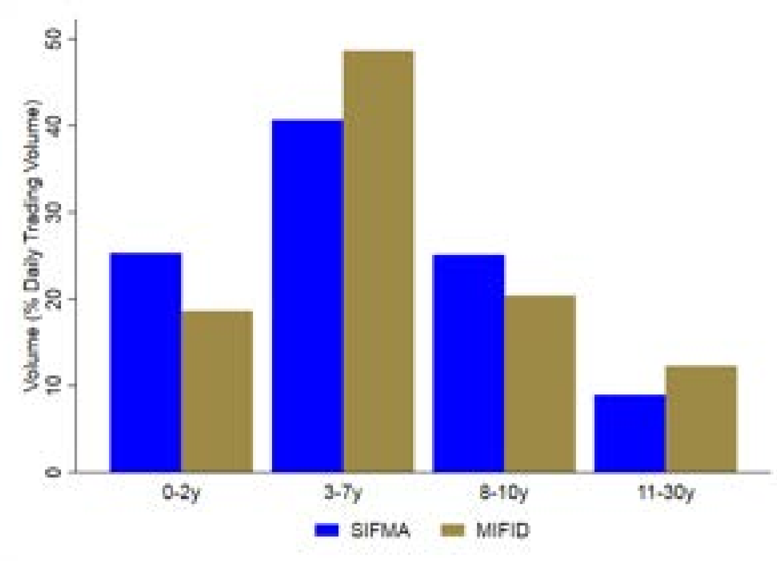
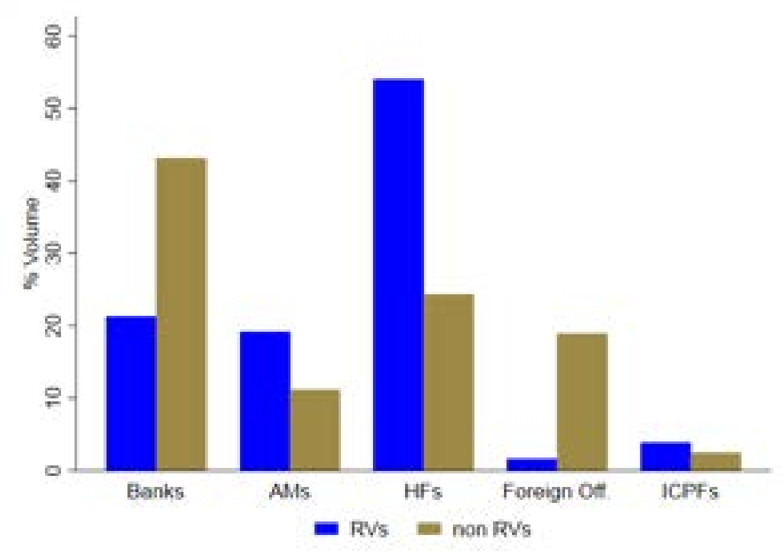


Figure 33: Composition

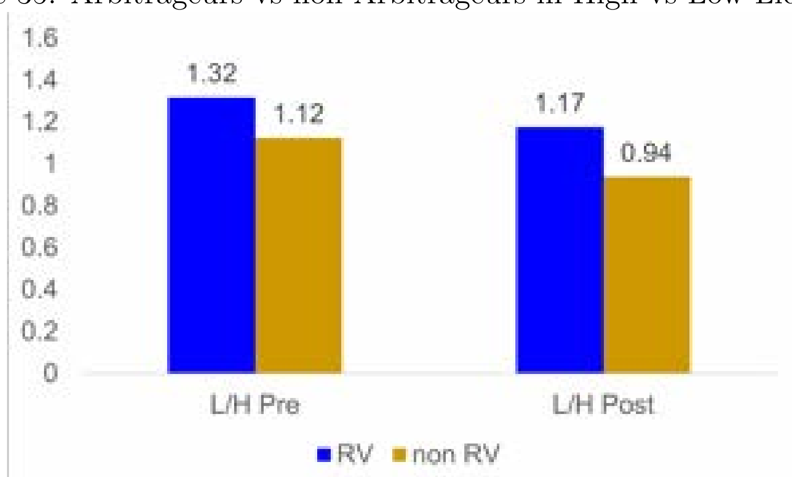
Notes: Panel (a) reports the average daily trading volume by maturity bucket as a percentage of total trading volume as reported by SIFMA using the Primary Dealer Statistics of Federal Reserve of New York. Panel (b) reports the composition by maturity (volume in % of total). The figures are based on the average over 2018-2021.

Figure 34: Distribution of Volume by By Sectors



Notes: The figure shows the percentage of volume within each category (RV/non-RV) attributed to different sectors.

Figure 35: Arbitrageurs vs non-Arbitrageurs in High vs Low Liquidity



Notes: The figure shows the average volume traded by RV and non-RV in Low liquidity (L) relative to High liquidity (H) FOMC events. A number greater than 1 indicates that volume is higher in L events than in H events.

Figure 36: Volume Traded by Maturity

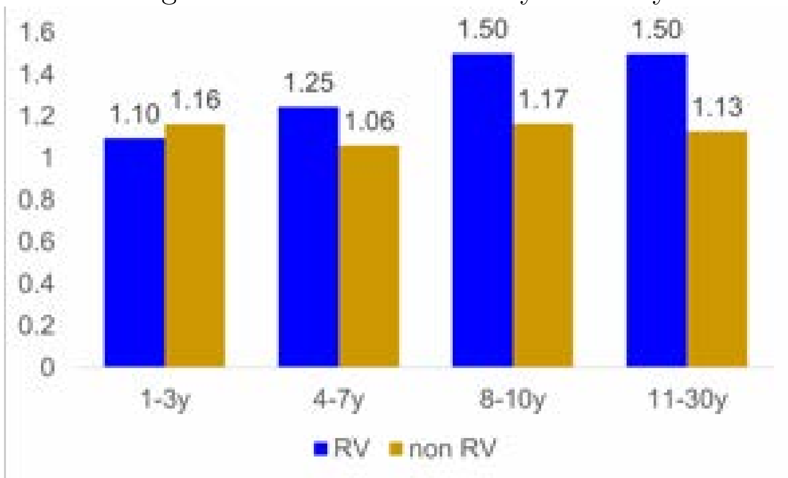


Figure 37: Pre-FOMC

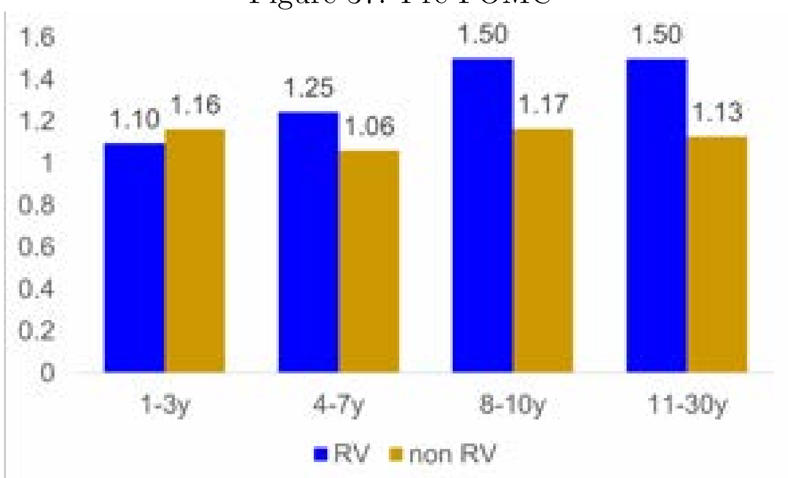
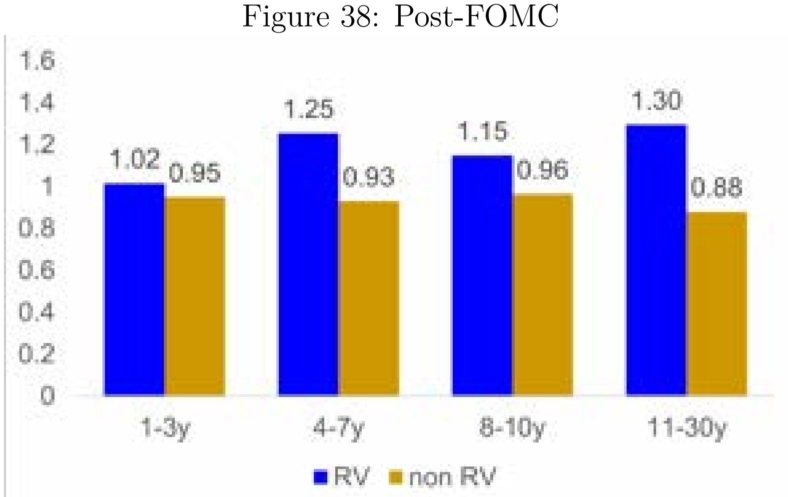
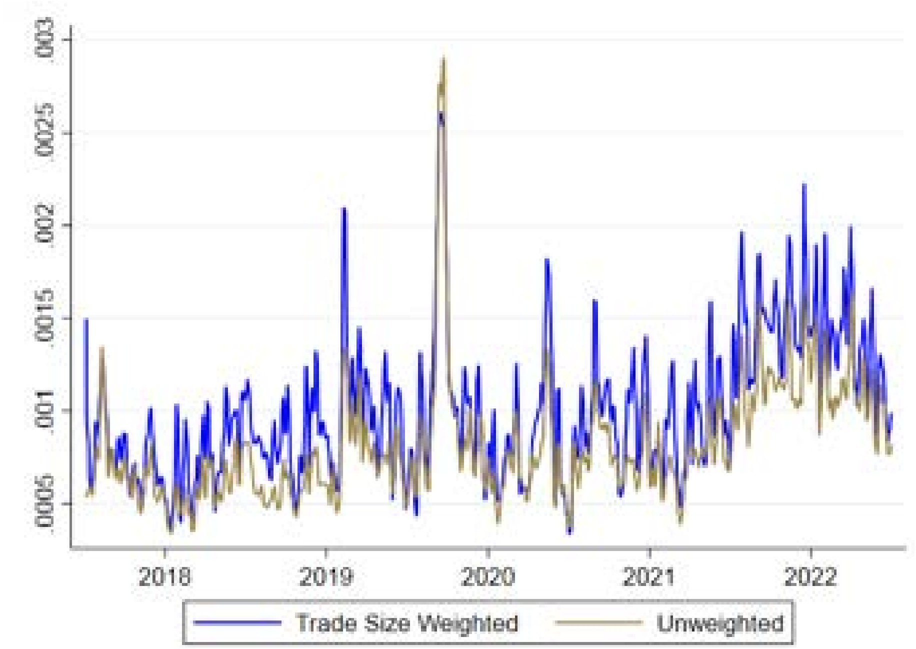


Figure 38: Post-FOMC



Notes: The figure shows the average volume traded by RV and non-RV in Low liquidity (L) relative to High liquidity (H) FOMC events, split by maturity. A number greater than 1 indicates that volume is higher in L events than in H events.

Figure 39: Interdealer Price Dispersion



Notes: The figure shows the 5-day moving average of the interdealer price dispersion measure, D_T.

Table 1: Impact of noise in the nominal Treasury yield curve on the response of US interest rates and inflation to MP shocks

	Baseline			Low noise			High noise			P-values		
	Nom.	Real	Inf.	Nom.	Real	Inf.	Nom.	Real	Inf.	Nom.	Real	Inf.
3M Treasury yield	0.67***			0.61***			0.69***			0.766		
	(0.14)			(0.16)			(0.19)					
6M Treasury yield	0.85***			0.74***			0.90***			0.475		
	(0.11)			(0.16)			(0.14)					
1Y Treasury yield	1.00***			1.48***			0.81***			0.007		
	(0.14)			(0.12)			(0.18)					
2Y Treasury yield	1.10***	1.06***	0.04	1.83***	1.69***	0.14	0.69*	0.70**	-0.01	0.034	0.034	0.715
	(0.33)	(0.24)	(0.18)	(0.23)	(0.32)	(0.33)	(0.41)	(0.29)	(0.20)			
3Y Treasury yield	1.06***	1.02***	0.04	1.92***	1.72***	0.20	0.57	0.62**	-0.05	0.018	0.021	0.482
	(0.36)	(0.25)	(0.17)	(0.27)	(0.33)	(0.28)	(0.43)	(0.29)	(0.20)			
5Y Treasury yield	0.73***	0.64***	0.09	1.68***	1.58***	0.10	0.34	0.26*	0.08	0.000	0.000	0.925
	(0.20)	(0.15)	(0.11)	(0.24)	(0.20)	(0.18)	(0.21)	(0.14)	(0.14)			
10Y Treasury yield	0.38**	0.44***	-0.06	1.24***	1.24***	0.00	0.03	0.11	-0.08	0.000	0.000	0.656
	(0.17)	(0.13)	(0.08)	(0.20)	(0.16)	(0.12)	(0.17)	(0.12)	(0.11)			
2Y Treasury inst. forward rate	1.14**	0.99***	0.15	2.25***	1.76***	0.49*	0.50	0.55*	-0.05	0.011	0.027	0.182
	(0.46)	(0.29)	(0.23)	(0.35)	(0.38)	(0.29)	(0.51)	(0.33)	(0.25)			
3Y Treasury inst. forward rate	0.82*	0.88***	-0.06	1.96***	1.77***	0.18	0.17	0.38	-0.21	0.009	0.012	0.193
	(0.43)	(0.32)	(0.15)	(0.45)	(0.42)	(0.20)	(0.44)	(0.31)	(0.19)			
5Y Treasury inst. forward rate	0.26	0.47***	-0.21**	1.17***	1.26***	-0.09	-0.12	0.15	-0.26**	0.001	0.001	0.353
	(0.19)	(0.17)	(0.08)	(0.30)	(0.25)	(0.13)	(0.19)	(0.17)	(0.11)			
10Y Treasury inst. forward rate	-0.08	0.12	-0.20**	0.58***	0.68***	-0.10	-0.34*	-0.10	-0.24*	0.002	0.000	0.490
	(0.18)	(0.12)	(0.09)	(0.18)	(0.12)	(0.13)	(0.20)	(0.13)	(0.13)			

Notes: The first panel (Baseline) reports estimates of γ in regression (1). The second (Low noise) and third (High noise) panels respectively present parameter estimates for γ_1 and γ_2 in regression (2), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. The last panel reports the p-values of the tests of equality between the coefficients estimates for low- and high-noise FOMC announcements. The dependent variable in each regression is the one-day change in the variable stated in the left-most column. The sample includes all regularly scheduled FOMC meetings from 01/01/2000 to 19/03/2014, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 106 observations on which the policy news shock is computed and each regression is estimated. Regression results for the 2- and 3-year yields and forward rates are based on a sample size of 74 observations (starting in 2004). Robust standard errors are in parentheses. Asterisks denote significance levels (* $p<0.1$, ** $p<0.05$, *** $p<0.01$).

Table 2: Yield curve noise and other financial variables: Baseline sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
ΔVIX	0.09***							0.04	0.02	0.02
	(0.03)							(0.03)	(0.03)	(0.02)
ΔTED Spread		1.07*						0.69	0.25	0.25
		(0.59)						(0.66)	(0.78)	(0.78)
ΔIntermed. Lev.			0.19***			0.10*	0.09*	0.13**	0.09*	0.09*
			(0.07)			(0.06)	(0.05)	(0.06)	(0.05)	(0.05)
HF Ret.				-0.20***		-0.15**		-0.05		-0.01
				(0.07)		(0.06)		(0.03)		(0.03)
FIA Ret.					-0.32***		-0.28***		-0.22**	-0.22**
					(0.05)		(0.05)		(0.10)	(0.10)
Adj. R^2 (%)	23.13	8.12	18.06	22.49	36.68	26.08	40.03	32.38	40.90	40.55
# months	171	171	171	171	171	171	171	171	171	171

Notes: The dependent variable is the monthly change in the noise measure, obtained by averaging daily observations over a given month. VIX is the volatility index from CBOE. TED spread is the T-Bill Eurodollar spread. Intermediary leverage is the measure of He, Kelly, and Manela (2017). HF return is a measure of the average return of all hedge funds in the Barclays database. FIA return is a measure of the average return of the fixed-income arbitrage hedge funds in the Barclays database. The period is from 01/2000 to 03/2014, corresponding to a sample size of 171 observations. Newey-West standard errors with 4 lags are in parentheses. Asterisks denote significance levels (* p<0.1, ** p<0.05, *** p<0.01).

Table 3: Yield curve noise and other financial variables: Extended sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
ΔVIX	0.08***							0.03	0.01	0.01
	(0.03)							(0.02)	(0.02)	(0.02)
ΔTED Spread		1.04*						0.80	0.29	0.29
		(0.57)						(0.56)	(0.73)	(0.72)
ΔIntermed. Lev.			0.18***			0.11*	0.09*	0.14**	0.09*	0.09*
			(0.06)			(0.06)	(0.05)	(0.06)	(0.05)	(0.05)
HF Ret.				-0.17***		-0.12**		-0.04		0.00
				(0.06)		(0.05)		(0.03)		(0.03)
FIA Ret.					-0.32***		-0.27***		-0.23**	-0.23**
					(0.05)		(0.05)		(0.09)	(0.10)
Adj. R^2 (%)	18.02	7.63	16.35	18.53	34.52	22.47	37.37	28.35	37.91	37.65
# months	240	240	240	240	240	240	240	240	240	240

Notes: The dependent variable is the monthly change in the noise measure, obtained by averaging daily observations over a given month. VIX is the volatility index from CBOE. TED spread is the T-Bill Eurodollar spread. Intermediary leverage is the measure of He, Kelly, and Manela (2017). HF return is a measure of the average return of all hedge funds in the Barclays database. FIA return is a measure of the average return of the fixed-income arbitrage hedge funds in the Barclays database. The period is from 01/2000 to 12/2019, corresponding to a sample size of 240 observations. Newey-West standard errors with 4 lags are in parentheses. Asterisks denote significance levels (* p<0.1, ** p<0.05, *** p<0.01).

Table 4: Predictive Regressions Using Macroeconomic and Financial Data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Nonfarm payrolls	0.094**	0.044	0.026	-0.014	-0.007	0.034	0.020	-0.007	-0.002
	(0.039)	(0.064)	(0.039)	(0.060)	(0.038)	(0.051)	(0.032)	(0.049)	(0.031)
Employment growth (12m)	0.005**	0.006*	0.003*	0.007**	0.005**	0.006**	0.003**	0.008**	0.005***
	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)
Δ log S&P500 (3m)	0.084	0.153	0.081	0.095	0.052	0.137*	0.081*	0.091	0.057
	(0.059)	(0.092)	(0.055)	(0.089)	(0.055)	(0.071)	(0.042)	(0.065)	(0.041)
Δ Slope (3m)	-0.010	-0.012	-0.008	-0.012	-0.008	-0.011	-0.008	-0.013	-0.008
	(0.007)	(0.009)	(0.006)	(0.009)	(0.006)	(0.008)	(0.005)	(0.008)	(0.005)
Δ log Comm. price (3m)	0.119**	0.065	0.051	0.118	0.082*	0.072	0.049	0.093	0.062*
	(0.051)	(0.089)	(0.051)	(0.079)	(0.049)	(0.068)	(0.038)	(0.057)	(0.035)
Treasury skewness	0.032***	0.027	0.020	0.035	0.025	0.029**	0.018**	0.031**	0.019**
	(0.011)	(0.026)	(0.015)	(0.027)	(0.016)	(0.013)	(0.008)	(0.013)	(0.008)
R^2 (%)	16.16	21.32	22.19	16.92	18.03	18.96	19.72	14.71	15.41
Adj. R^2 (%)	14.56	16.90	17.82	11.88	13.07	15.78	16.58	11.18	11.91
Sample	88-19	00-14	00-14	00-14	00-14	00-19	00-19	00-19	00-19
GFC	YES	YES	YES	NO	NO	YES	YES	NO	NO
N	322	114	114	106	106	160	160	152	152
Policy surprise	BS	BS	NS	BS	NS	BS	NS	BS	NS

Notes: The table reports the coefficient estimates from Equation (9) for the set of macroeconomic and financial predictors considered in Bauer and Swanson (2023b), which are observed prior to the FOMC announcements. These include the surprise component of the most recent nonfarm payrolls release, employment growth over the last year, the log change in the S&P500 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 (2021). The first column present the results for the monetary policy surprise shock of Bauer and Swanson (2023b) with their set of 322 FOMC announcements over the period 1988/01-2019/12. The next 4 columns consider all scheduled FOMC announcements taking place in the baseline sample (2000/01-2014/03) for the Bauer and Swanson (2023b) and Nakamura and Steinsson (2018) shocks, respectively denoted by BS and NS. Columns (4)-(5) discard the 8 FOMC announcements taking place during the peak of the GFC between 2008/07 and 2009/06. The last 4 columns report the results for the extended sample (2000/01-2019/12), with columns (8)-(9) discarding FOMC announcements taking place between 2008/07 and 2009/06. Robust standard errors are in parentheses. Asterisks denote significance levels (* p<0.1, ** p<0.05, *** p<0.01).

Table 5: Summary Statistics

	Volume	No. Transactions	Trade Size	No. LEI
	Panel A: Full Sample			
	9,887	586	16.9	3,020
	Panel B: By Maturity			
1-3y	2,676 (26.3%)	132 (22.0%)	20.2	2,146
3-7y	3,433 (33.8%)	153 (25.5%)	22.4	2,067
7-10y	2,831 (27.9%)	189 (31.5 %)	15.0	2,199
11-30y	1,218 (12.0%)	126 (21.0%)	9.7	1,806
	Panel C: By Sector			
Banks	3,829 (37.2%)	293 (48.5%)	13.0	524
AMs	1,329 (12.9%)	168 (27.8%)	7.9	1,365
HFs	3,160 (30.7%)	83 (13.7%)	38.1	596
Foreign Off.	1,654 (16.1%)	38 (6.3%)	43.5	126
ICPFs	308 (3.0%)	22 (3.6%)	14.1	409

]

Notes: The table shows daily averages for different splits of the sample. No. LEI is the *total* number of unique LEI and not the daily average.

Table 6: Summary Statistics: FOMC Days

	Volume	No. Transactions	Trade Size	No. LEI
	Panel A: FOMC			
no FOMC	9,739	577	16.9	2,984
Pre-FOMC (t)	11,546	682	16.9	1,416
Post-FOMC (t+1)	13,700	806	17.0	1,401
	Panel B: FOMC in High and Low Liquidity			
H-Noise Pre-FOMC	10,561	586	18.0	925
H-Noise Post-FOMC	13,804	777	17.8	966
L-Noise Pre-FOMC	12,307	757	16.3	1,177
L-Noise Post-FOMC	13,620	828	16.5	1,155

Table 7: Summary Statistics: RV vs non-RV on FOMC days

		Volume	No. Transactions	Trade Size	No. LEI
		Panel A: Any Day			
RV		2,372	103	23.0	699
non RV		7,610	488	15.6	2,321
		Panel B: FOMC			
RV	no FOMC	2,342	101	23.1	699
RV	Pre-FOMC (t)	2,716	127	21.4	459
RV	Post-FOMC (t+1)	3,155	143	22.0	479
non RV	no FOMC	7,498	482	15.6	2,285
non RV	Pre-FOMC (t)	8,830	556	15.9	957
non RV	Post-FOMC (t+1)	10,545	662	15.9	922
		Panel C: High vs Low Liquidity FOMC			
RV	H-Noise Pre-FOMC	2,305	111	20.8	310
RV	H-Noise Post-FOMC	2,873	137	21.0	329
RV	L-Noise Pre-FOMC	3,034	139	21.8	375
RV	L-Noise Post-FOMC	3,374	148	22.8	394
non RV	H-Noise Pre-FOMC	8,255	475	17.4	615
non RV	H-Noise Post-FOMC	10,931	640	17.1	637
non RV	L-Noise Pre-FOMC	9,274	618	15.0	802
non RV	L-Noise Post-FOMC	10,246	680	15.1	761

Table 8: Interdealer Price Dispersion in High and Low Liquidity

	Unweighted	Weighted	Unweighted	Weighted
	Interdealer Price Dispersion D_T (smoothed)			
L-Noise Pre-FOMC	-7.33***	-7.15***		
L-Noise Post-FOMC			-7.27***	-7.08***
Observations	39	39	39	39

Notes: The table reports the results of the regression $\log(D_t) = \beta \mathbb{1}_{LowNoise}$ where $i = \{Pre - FOMC, Post - FOMC\}$.

A Appendix

A.1 *Different Timings for the Noise Measure, Slow-Moving Trend, and Different Data Vintages*

A.1.1 Different timings for the noise measure

We explore different definitions for the timing of the liquidity measure around FOMC announcements. Figure 43 of the appendix presents the results using a 3-day average of the noise measure before FOMC announcement (i.e. $t-3$ to $t-1$) and Figure 44 considers the same treatment on the extended sample. Moreover, Figure 45 considers a 3-day window around announcements (i.e. $t-1$ to $t+1$) and Figure 46 considers the same treatment on the extended sample. The results are unaffected by the timing used to measure market liquidity conditions around FOMC announcements.

A.1.2 Slow-Moving Trend in the noise measure

Another concern is that the declining trend in the noise measure over the sample period could be driving our results. Figure 47 presents the results using a detrended version of the noise measure, obtained by fitting a quadratic trend to the series. Figure 48 considers the same treatment on the extended sample. We find very similar estimates compared to our baseline results presented in Section 3.1.

We note that properly accounting for the downward long-term trend in the noise measure also has implications for the extensions considered in Section 5, which we revisit below.

Does Liquidity State-Dependence Capture Recession State-Dependence? Section 5.1 highlights the implications for monetary policy transmission of the interactions between market liquidity and economic conditions. We show in Figure 12 that market liquidity might play a minor role for monetary policy transmission when economic conditions are favourable. However, Figure 49 indicates that the downward trend in the noise measure introduces a bias in our sorting of "High PMI" FOMC announcement dates into high and low noise buckets. Once corrected, we observe that market liquidity plays a key role in monetary policy transmission over the business cycle. The transmission of policy shocks can become muted during favourable economic conditions if market liquidity is poor (third column of Figure 49), and high liquidity in the market can support monetary policy transmission during periods of depressed economic activity (second column of Figure 50). The results in Figures 51 and 52 lend further support to the key role played by market liquidity in monetary policy transmission as we do not observe a significant impact of (un)favourable economic conditions once we condition on liquidity conditions in the market being better (Figure 51) or worse (Figure 52).

Possible Correlations between Market Liquidity and Monetary Policy Figure 53 presents the results in the extended sample from the analysis developed in Section 5.2 to remove the potential counfounding effect of conditional heteroskedasticity in monetary policy shocks across higher vs. lower liquidity states. These results would suggest that the weaker state-dependence in the extended sample is still present after standardizing the shocks and, therefore, cannot be completely explained by the smaller variance of the monetary policy surprises in the more recent part of the sample. However, Figure 54 shows that the results based on the detrended noise measure confirm our baseline results in Section 5.2 that the state-dependence of monetary policy transmission with respect to liquidity conditions is not driven by the conditional heteroskedasticity of the shocks across liquidity states.

Dynamic Effects over Longer Horizon Figure 60 shows that the results in the extended sample using the detrended noise measure are in line with our baseline results from Section 5.3. The estimated effects for real forward rates on higher liquidity announcement days are strongly significant over the first month for intermediate maturities of 5 and 10 years, and the overall increase after 3 months is significant for the 5 and 20-year forwards. Monetary policy transmission on lower liquidity announcement days is muted – except for short maturities – and long maturities actually experience a statistically significant decrease over a horizon of 3 months. Nominal forward rates show similar signs of a long-lasting impact of the liquidity state-dependence of monetary policy transmission (Figure 61), leaving inflation forward rates broadly unaffected except for longer maturities (Figure 62).

Macroeconomic Implications: The Response of Mortgage Rates Figure 64 reports the results for the extended sample when we use the detrended noise measure to sort FOMC announcements into high and low noise days. We find strong evidence of state dependence in monetary policy transmission, which confirms the results obtained in Section 5.4 for the baseline sample. The estimates for the higher liquidity days are more precisely estimated and the cumulative increase in mortgage rates is statistically different from zero at all horizons up to 3 months after the initial shock.

A.1.3 Impact of different data vintages

As highlighted in Section 2, the data vintage for real interest rates used in Nakamura and Steinsson (2018) differs from the most recent vintage available on the website of the Federal Reserve Board due to subsequent revisions to the underlying methodology. While we took the approach of extending the initial dataset of Nakamura and Steinsson (2018) with more recent data to increase comparability between our baseline analysis and their results, we would also like to ensure that our main finding on the liquidity state-dependence of monetary policy transmission – which operates primarily through real interest rates – is not driven by our choice of data vintage. Figure 65 shows that the baseline estimates for real forward rates (first column, second row) are qualitatively comparable to our baseline results, with significant coefficient estimates for short and medium maturities. We also observe a slightly more pronounced U-shaped pattern in the coefficients, although differences in estimates are unlikely to be statistically significant, which helps reconciling some of the differences between our results and the analysis in Kekre, Lenel, and Mainardi (2022).

As in our baseline results, we find larger responses in real forward rates on higher liquidity days (second column, second row) and no statistically significant response on lower liquidity days (third column, second row). We note however that the difference in coefficient estimates between higher and lower liquidity days is somewhat less pronounced than in our baseline analysis, and the transmission of shocks to longer real forward rates on higher liquidity days tapers off after the 10-year maturity.

Figure 66 presents the results for the extended sample using the detrended noise measure. The results are in line with the ones for the baseline sample, with the exception of a statistically significant response of short-term real forward rates (maturities below 5 years) on lower liquidity days. The magnitude of the coefficient estimates for these maturities is comparable across liquidity states, with noticeably wider confidence intervals around the estimates on lower liquidity days. Taken together, these evidence suggest that our main results are robust to the choice of different data vintages for real interest rates.

A.2 Information Effect and Response to News

We follow the approach proposed by Bauer and Swanson (2023b) to remove the predictable component from the monetary policy surprises. We estimate the following regression:

$$\Delta mps_t = \beta_0 + \beta_1^\top X_{t-} + v_t \ , \tag{9}$$

where Δmps_t is a measure of monetary policy surprises, X_{t-} is a set of macroeconomic and financial predictors known at the announcement time t, and v_t is a regression residual term orthogonal to the predictors. We consider the predictors used in Bauer and Swanson (2023b)—namely, the surprise in the latest nonfarm payrolls release, the 12-month log-change in nonfarm payroll employment, the 3-month log change in the S&P500 stock price index prior to the FOMC announcement, the 3-month changes in the yield curve slope and in the Bloomberg Commodity Spot Price index, and the average value of the Treasury skewness measure of Bauer and Chernov (2021) over the previous month.

Table 4 reports the results for the monetary policy surprise shocks of Nakamura and Steinsson (2018), denoted by NS, and Bauer and Swanson (2023b), denoted by BS. For comparison purpose, the first column reproduces the results of Bauer and Swanson (2023b) based on their set of 322 FOMC announcements over the period 1988/01-2019/12. The rest of the columns report the results for the set of all scheduled FOMC announcements taking place in the baseline (2000/01-2014/03) and extended (2000/01-2019/12) samples. In both cases, we consider the implication of dropping the 8 FOMC announcements taking place at the peak of the GFC between July 2008 and June 2009. The orthogonalized shocks $\hat{v}_t$ used in the analysis of Section 6.2 correspond to columns (4)-(5) for the baseline sample and (8)-(9) for the extended sample.

A.3 Robustness of the RV Measure

In this section we show the key trade-off in choosing the window over which to compute our two metrics of arbitrage capital. We immediately rule out windows that are too long (e.g. quarterly, annual, or over entire sample) since they would fail even to distinguish between preferred-habitat investors and arbitrageurs. We test our measure with three windows: daily, weekly and monthly. The trade off is the following: suppose on day t I see you trading only at the $2Y$ point; then next period, $t+1$, I see you trading the $7Y$ maturity. We would not be able to compute a standard deviation here. If we extend our window to a two-day window (t to $t+1$) we would say you have $\sigma > 0$. This is the benefit of having a longer window.

However, as the window expands, I also capture behaviour that not necessarily implies arbitrage across the curve. For example, suppose there is a preferred-habitat investor with a preference for maturity $30Y$. At time t we see him buying at the $30Y$. One year later the bond has become a $29Y$ but the investor has a preference/mandate for $30Y$ so rebalancing is needed. I would observe the preferred-habitat investor selling a $29Y$ and buying a $30Y$ thus implying a positive standard deviation.

In other words, extending the window increase type I error: categorizing non-arbitrageurs as arbitrageurs, while restricting the window causes type II error. Table 9 shows our measure at different windows. The daily window captures more volume and more frequent trading. That is, seems to be capturing traders hit by more frequent liquidity shocks that go in and out to manage liquidity. When we move to weekly, Panel B, we capture less volume and less transaction but they start increasing in size. Finally when we move to monthly, Panel C, we capture slightly less volume and transactions but larger trade sizes.

Figure 40 shows the composition across the monthly and daily index. While picking up a smaller volume, the composition becomes more tilted towards hedge funds and picks up fewer banks, which could be trader hit by more frequent liquidity shock (i.e. deposit withdrawals).

Figure 40: Distribution of Arbitrage Capital

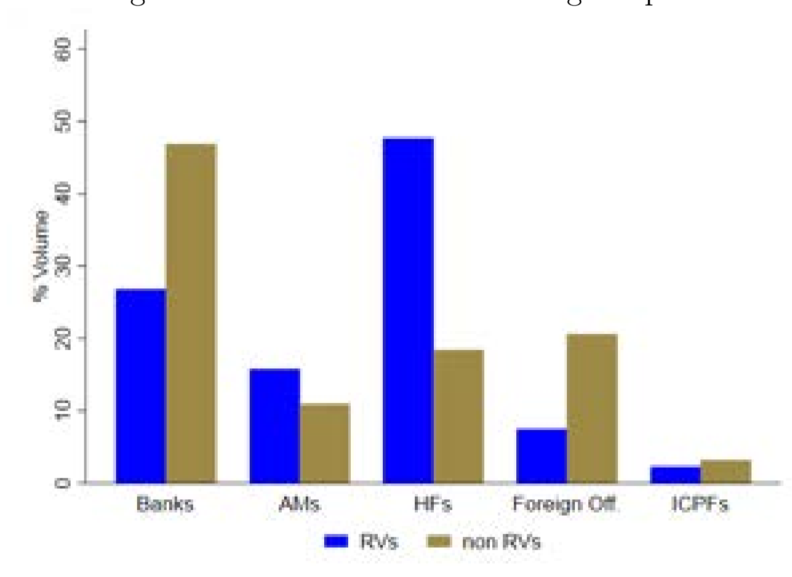


Figure 41: Daily Index

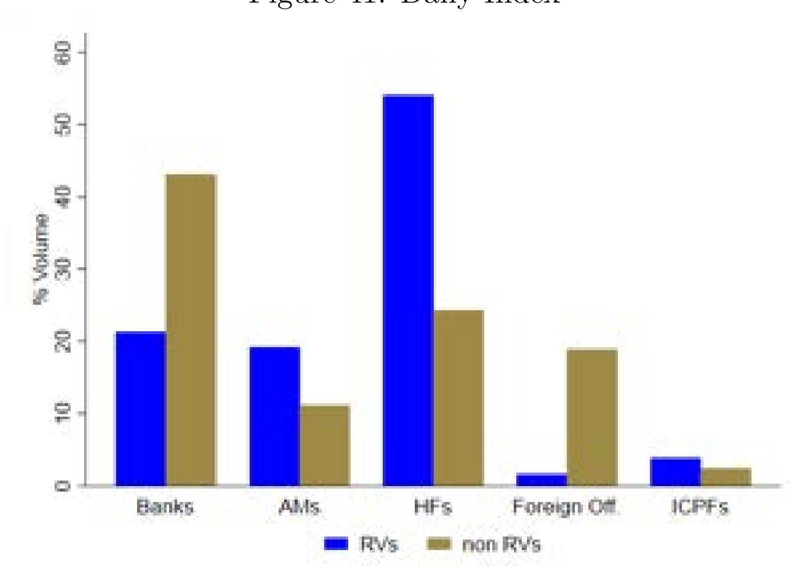


Figure 42: Monthly Index

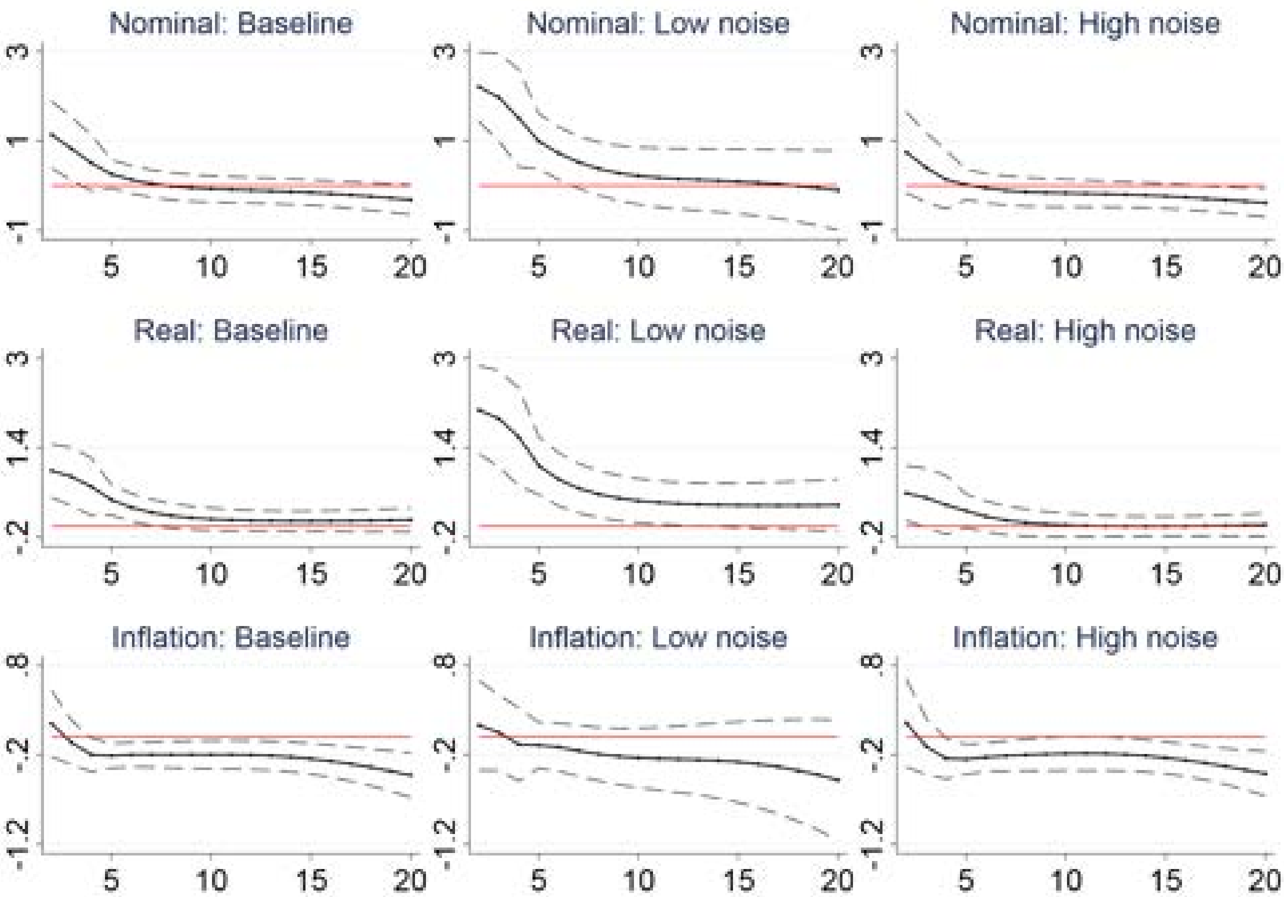
Table 9: Summary Statistics at different windows

	Volume	No. Transactions	Trade Size	No. LEI
		Panel A: Daily Measure		
RV	4,408	247	17.9	617
non RV	5,573	345	16.2	2,403
		Panel B:Weekly Measure		
RV	2,972	144	20.6	655
non RV	6,992	446	15.7	2,365
		Panel C: Monthly Measure		
RV	2,372	103	23.0	699
non RV	7,610	488	15.6	2,321

A.4 Additional Tables and Figures

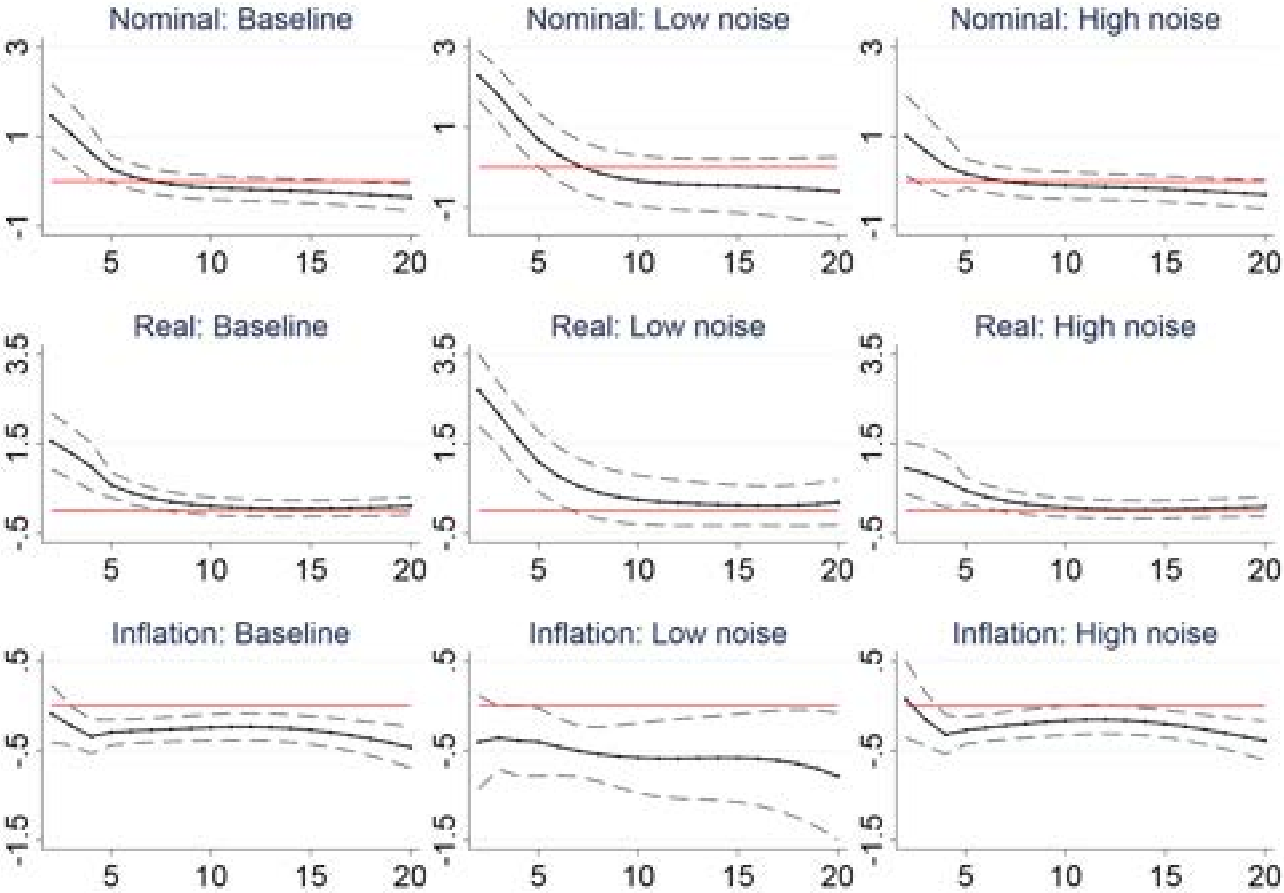
A.4.1 Different Timings and Slow-Moving Trend in the Noise Measure

Figure 43: Impact of yield curve noise on the transmission of MP shocks to US forward rates: backward-looking window



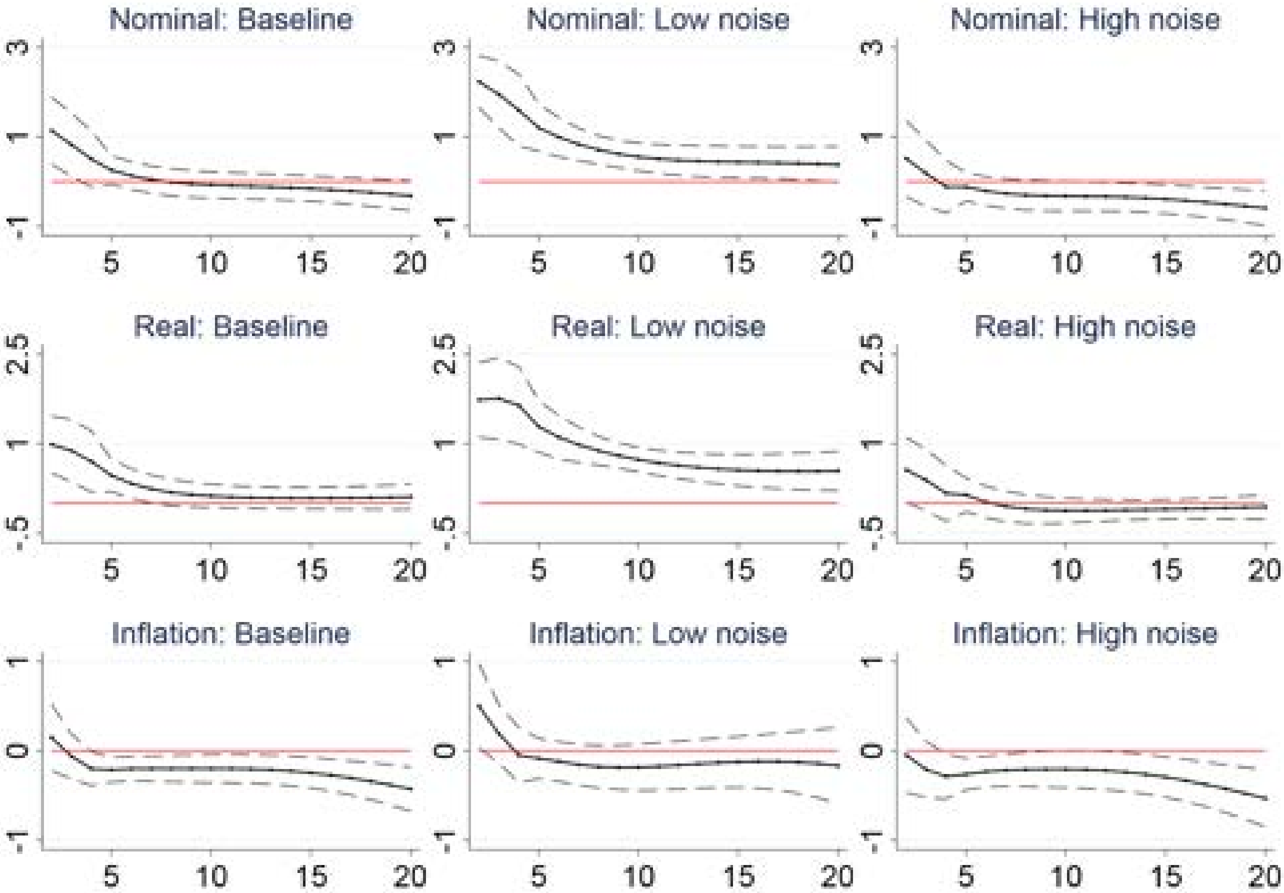
Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. We use a 3-day average of the noise measure before the announcement ($t-3$ to $t-1$). The sample includes all regularly scheduled FOMC meetings from 01/01/2000 to 19/03/2014, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 106 observations on which the policy news shock is computed and each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 74 observations (starting in 2004).

Figure 44: Impact of yield curve noise on the transmission of MP shocks to US forward rates: backward-looking window in the extended sample



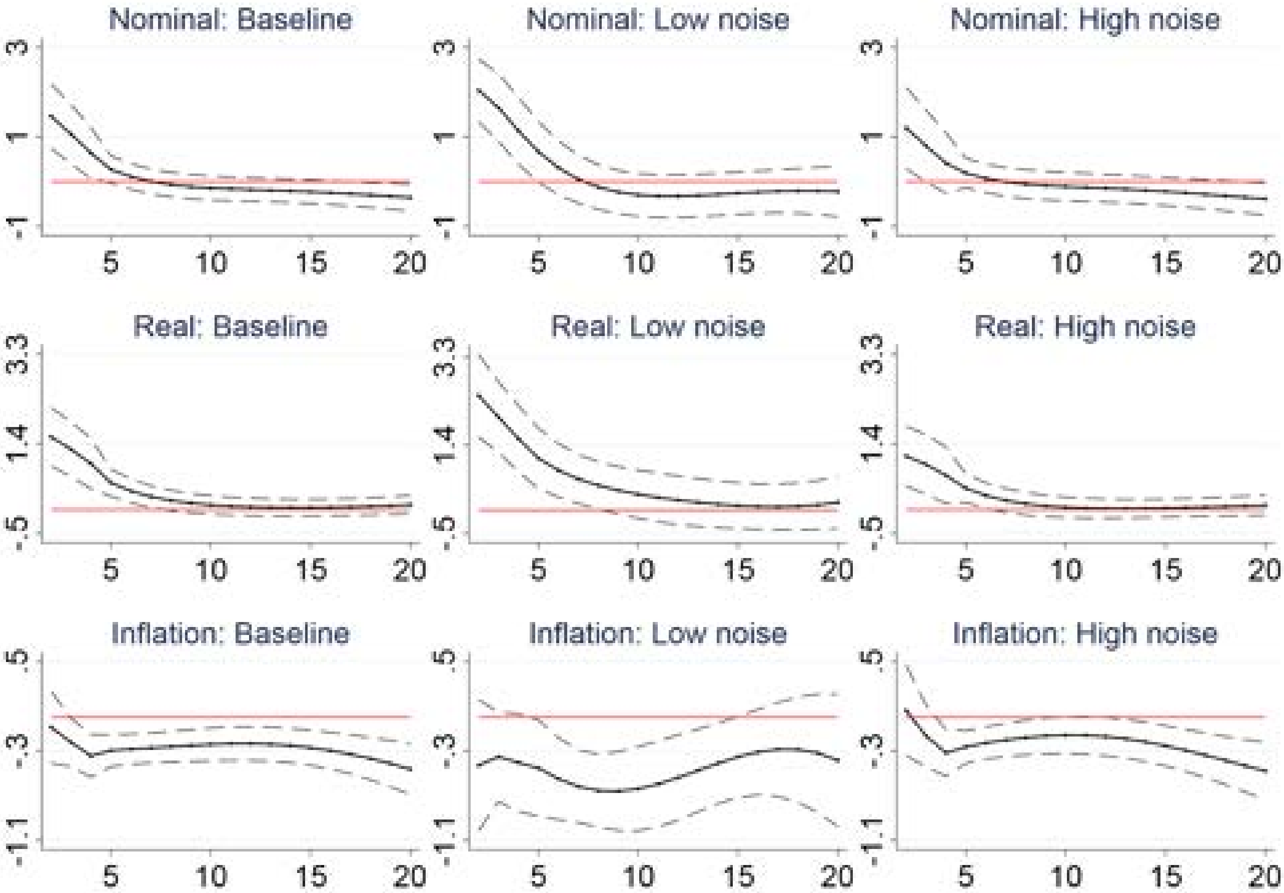
Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. We use a 3-day average of the noise measure before the announcement ($t-3$ to $t-1$). The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 152 observations on which each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 120 observations (starting in 2004).

Figure 45: Impact of yield curve noise on the transmission of MP shocks to US forward rates: forward-looking window



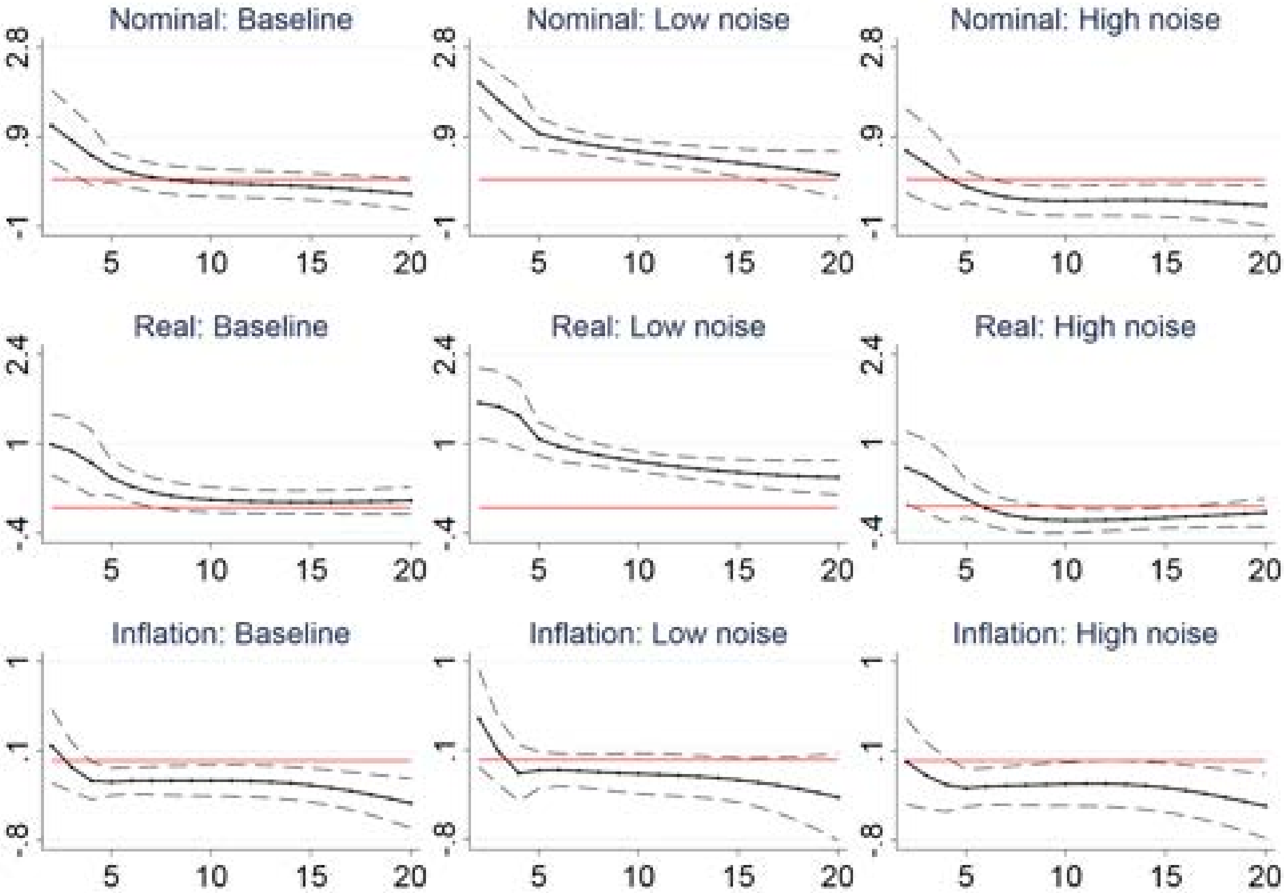
Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. We use a 3-day average of the noise measure around the announcement ($t-1$ to $t+1$). The sample includes all regularly scheduled FOMC meetings from 01/01/2000 to 19/03/2014, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 106 observations on which the policy news shock is computed and each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 74 observations (starting in 2004).

Figure 46: Impact of yield curve noise on the transmission of MP shocks to US forward rates: forward-looking window in the extended sample



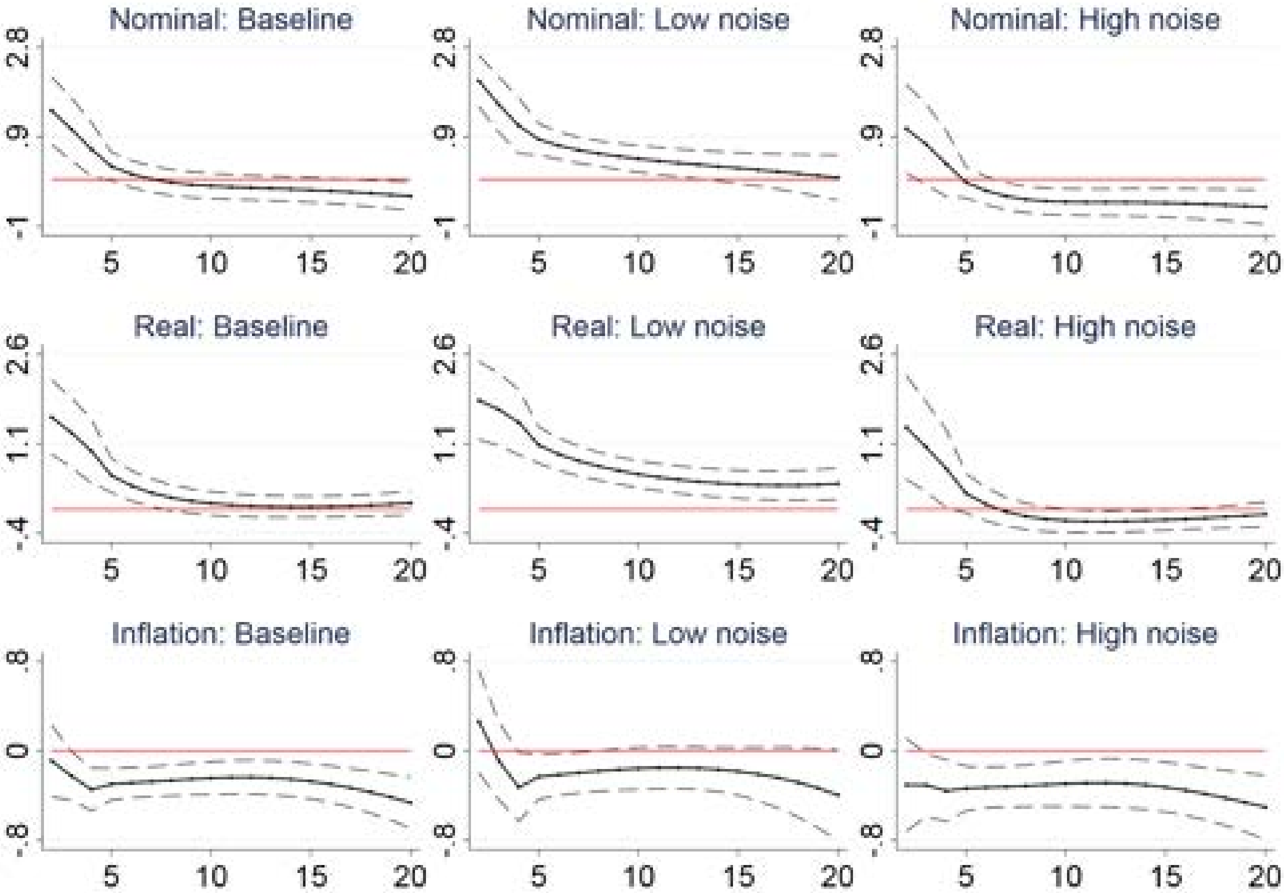
Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. We use a 3-day average of the noise measure around the announcement ($t-1$ to $t+1$). The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 152 observations on which each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 120 observations (starting in 2004).

Figure 47: Impact of yield curve noise on the transmission of MP shocks to US forward rates: accounting for trends



Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. We use a detrended version of the noise measure, obtained by fitting a quadratic trend to the series. The sample includes all regularly scheduled FOMC meetings from 01/01/2000 to 19/03/2014, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 106 observations on which the policy news shock is computed and each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 74 observations (starting in 2004).

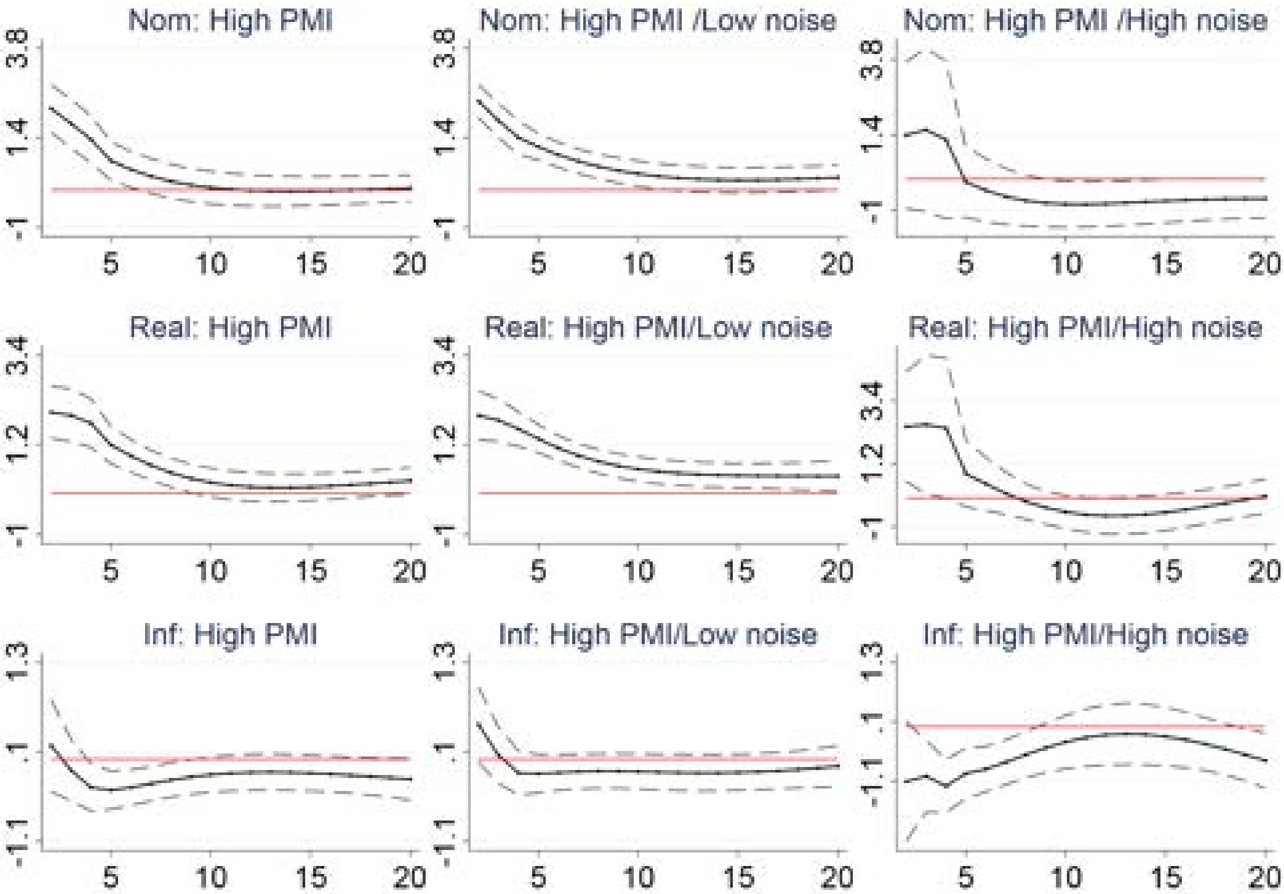
Figure 48: Impact of yield curve noise on the transmission of MP shocks to US forward rates: accounting for trends in the extended sample



Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. We use a detrended version of the noise measure, obtained by fitting a quadratic trend to the series. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 152 observations on which each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 120 observations (starting in 2004).

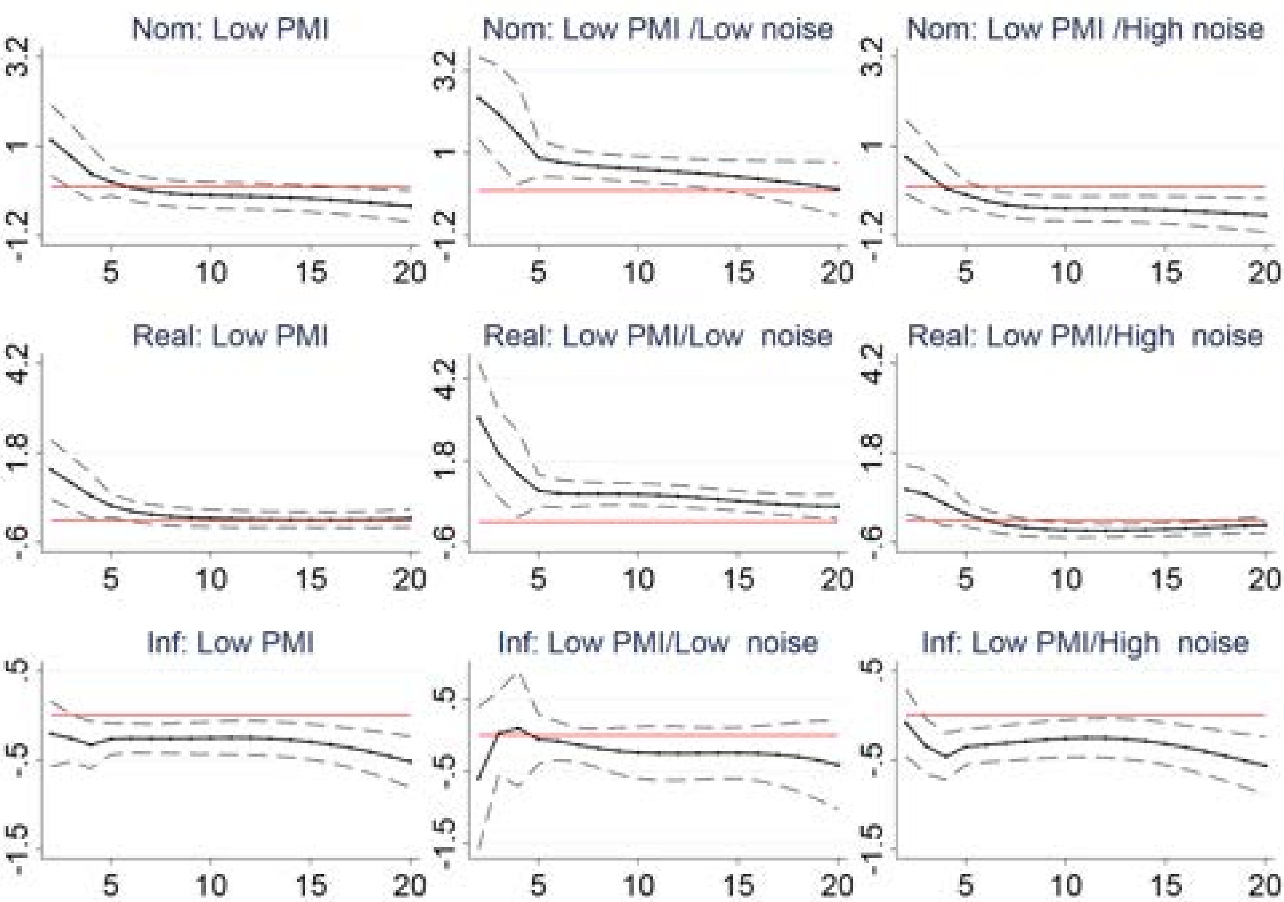
A.4.2 Does Liquidity State-Dependence Capture Recession State-Dependence?

Figure 49: Impact of yield curve noise on monetary policy transmission when economic conditions are favourable: detrended noise



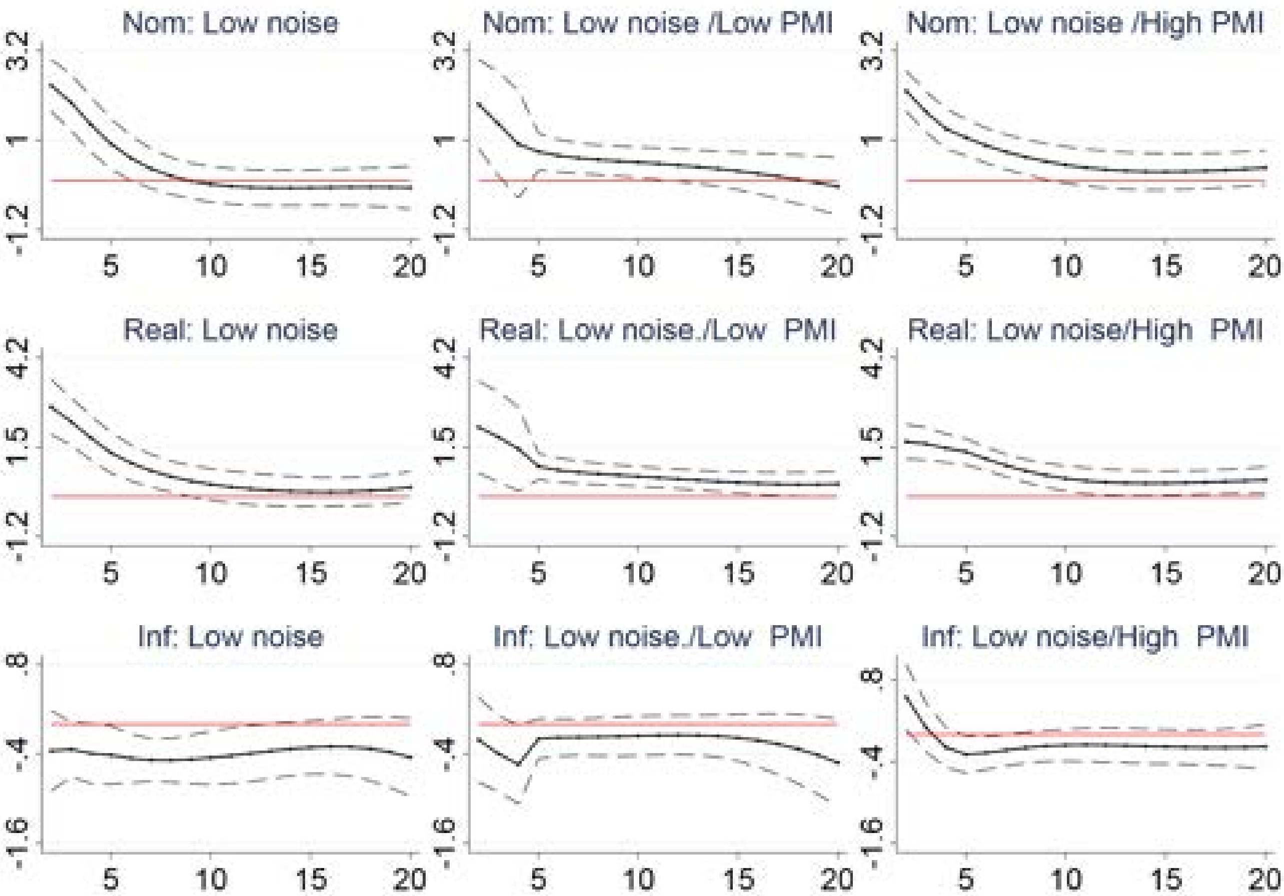
Note: The first column (High PMI) plots estimates of $\gamma^{i}_{hl,\tau}$ in regression (2) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$ for the subset of scheduled FOMC announcements for which the manufacturing PMI is above its median level. 90% confidence intervals based on robust standard errors are provided around the estimates. The second (High PMI/Low noise) and third (High PMI/High noise) columns respectively present parameter estimates for the two subgroups obtained by further dividing the "High PMI" announcements into two subgroups depending on whether the yield curve noise measure is below/above median. We use a detrended version of the noise measure, obtained by fitting a quadratic trend to the series. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 76 observations (out of 152) for the "High PMI" results and 38 observations for the "High PMI/Low noise" and "High PMI/High noise" subgroups. Regression results for the 2, 3 and 4-year forward rates are based on a sample starting in 2004.

Figure 50: Impact of yield curve noise on monetary policy transmission when economic conditions are depressed: detrended noise



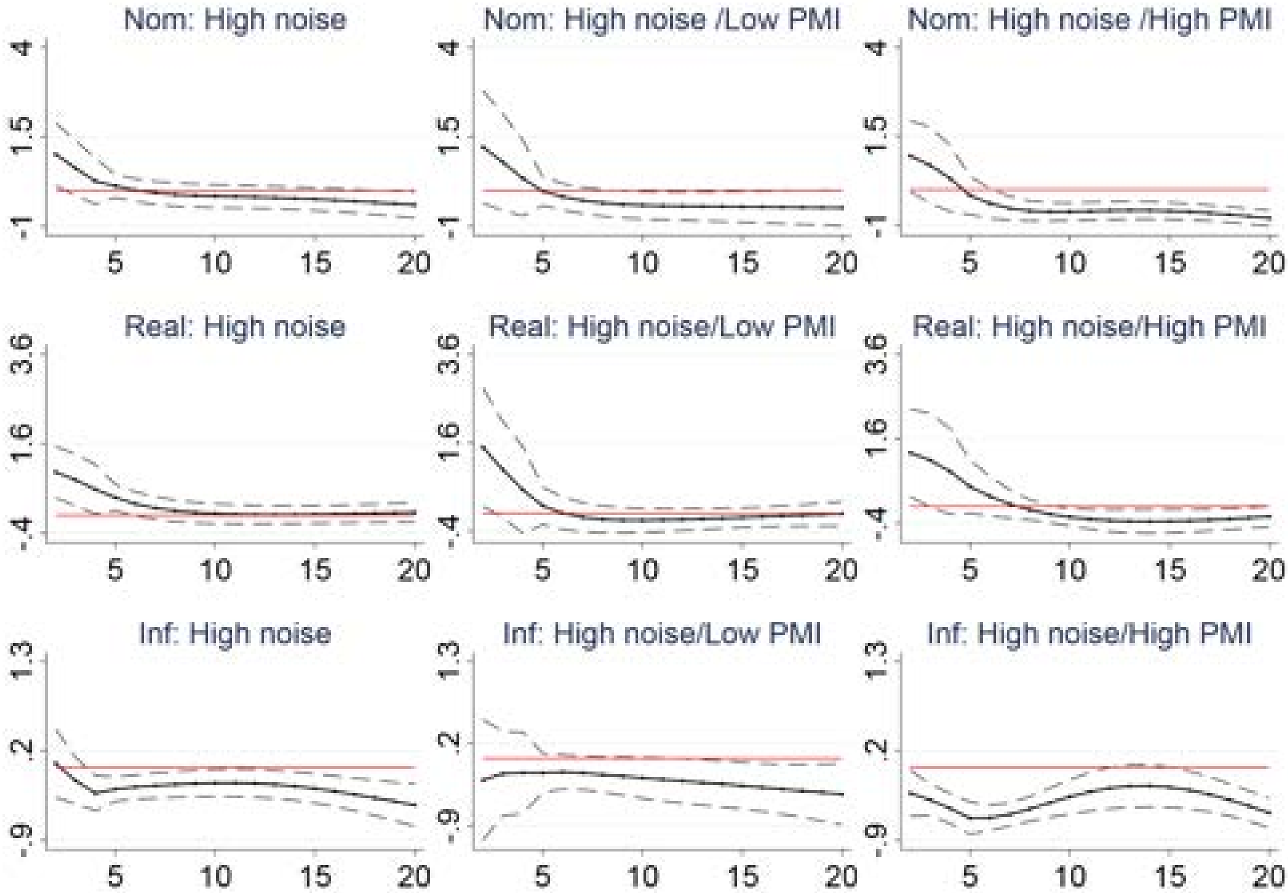
Note: The first column (Low PMI) plots estimates of $\gamma^i_{hl,\tau}$ in regression (2) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$ for the subset of scheduled FOMC announcements for which the manufacturing PMI is below its median level. 90% confidence intervals based on robust standard errors are provided around the estimates. The second (Low PMI/Low noise) and third (Low PMI/High noise) columns respectively present parameter estimates for the two subgroups obtained by further dividing the "Low PMI" announcements into two subgroups depending on whether the yield curve noise measure is below/above median. We use a detrended version of the noise measure, obtained by fitting a quadratic trend to the series. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 76 observations (out of 152) for the "Low PMI" results and 38 observations for the "Low PMI/Low noise" and "Low PMI/High noise" subgroups. Regression results for the 2, 3 and 4-year forward rates are based on a sample starting in 2004.

Figure 51: Impact of economic conditions on monetary policy transmission when yield curve noise is low: detrended noise



Note: The first column (Low noise) plots estimates of $\gamma^{i}_{hl,\tau}$ in regression (2) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$ for the subset of scheduled FOMC announcements for which the yield curve noise measure is below its median level. 90% confidence intervals based on robust standard errors are provided around the estimates. The second (Low noise/Low PMI) and third (Low noise/High PMI) columns respectively present parameter estimates for the two subgroups obtained by further dividing the “Low noise” announcements into two buckets depending on whether the manufacturing PMI is below/above median. We use a detrended version of the noise measure, obtained by fitting a quadratic trend to the series. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 76 observations (out of 152) for the “Low noise” results and 38 observations for the “Low noise/Low PMI” and “Low noise/High PMI” subgroups. Regression results for the 2, 3 and 4-year forward rates are based on a sample starting in 2004.

Figure 52: Impact of economic conditions on monetary policy transmission when yield curve noise is high: detrended noise



Note: The first column (High noise) plots estimates of $\gamma^{i}_{hl,\tau}$ in regression (2) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$ for the subset of scheduled FOMC announcements for which the yield curve noise measure is above its median level. 90% confidence intervals based on robust standard errors are provided around the estimates. The second (High noise/Low PMI) and third (High noise/High PMI) columns respectively present parameter estimates for the two subgroups obtained by further dividing the "High noise" announcements into two buckets depending on whether the manufacturing PMI is below/above median. We use a detrended version of the noise measure, obtained by fitting a quadratic trend to the series. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 76 observations (out of 152) for the "High noise" results and 38 observations for the "High noise/Low PMI" and "High noise/High PMI" subgroups. Regression results for the 2, 3 and 4-year forward rates are based on a sample starting in 2004.

A.4.3 Possible Correlations between Market Liquidity and Monetary Policy

Table 10: Impact of FOMC meetings on US yield curve noise

	Noise	HP filter	Quadratic trend
Panel A: mondays			
FOMC week	-0.02	-0.01	-0.01
	(0.13)	(0.11)	(0.12)
Constant	2.56***	-0.61***	-0.34***
	(0.05)	(0.05)	(0.05)
N	603	603	603
Panel B: tuesdays			
FOMC week	0.03	0.04	0.03
	(0.13)	(0.12)	(0.12)
Constant	2.53***	-0.63***	-0.36***
	(0.05)	(0.04)	(0.05)
N	673	673	673
Panel C: wednesdays			
FOMC week	0.04	0.05	0.04
	(0.12)	(0.11)	(0.12)
Constant	2.53***	-0.63***	-0.36***
	(0.05)	(0.04)	(0.05)
N	675	675	675
Panel D: thursdays			
FOMC week	0.06	0.05	0.06
	(0.13)	(0.12)	(0.13)
Constant	2.56***	-0.60***	-0.32***
	(0.05)	(0.04)	(0.05)
N	667	667	667
Panel E: fridays			
FOMC week	0.01	0.01	0.01
	(0.13)	(0.12)	(0.11)
Constant	2.69***	-0.47***	-0.20***
	(0.05)	(0.05)	(0.05)
N	667	667	667

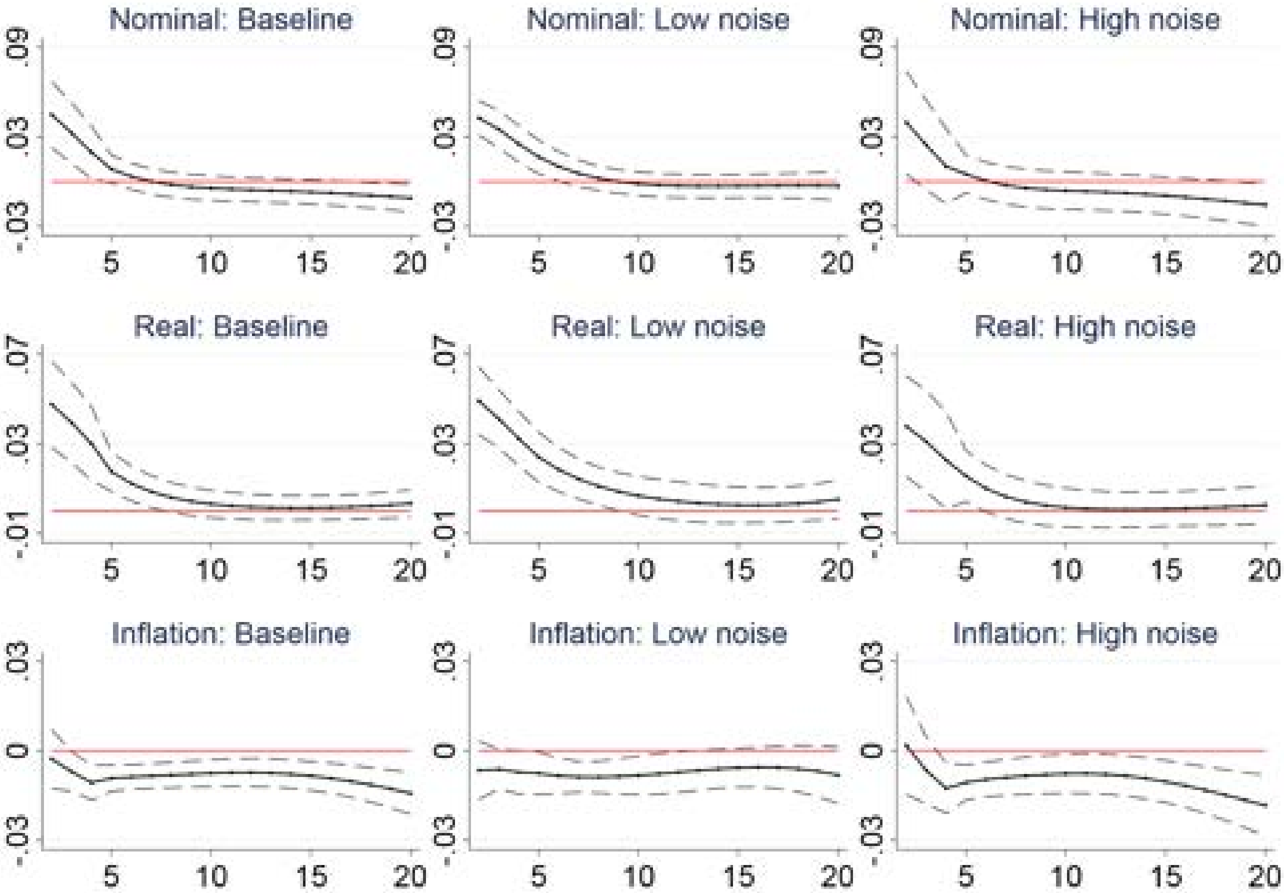
Notes: We report the results from a regression of the yield curve noise measure on a constant and a dummy variable indicating whether there is an FOMC meeting on that week. Each panel corresponds to the results for a given day of the week. The first column presents the results for the original noise measure. The second and third columns present the results for a detrended version of the noise measure, obtained respectively by applying the Hodrick-Prescott filter to the original series and by fitting a quadratic trend to the series. The sample covers the period from 01/01/2000 to 19/03/2014, excluding the observations for the period between July 2008 and June 2009. Standard errors are in parentheses. Asterisks denote significance levels (* p<0.1, ** p<0.05, *** p<0.0).

Table 11: Regression of MP surprise in absolute value on lagged dummy for high US yield curve noise

$1[\text{HighNoise}]_{t-1}$	0.0116**		
	(0.0048)		
$1[\text{HighNoise}^{\text{HP}}]_{t-1}$		0.0073	
		(0.0049)	
$1[\text{HighNoise}^{\text{Quad}}]_{t-1}$			0.0058
			(0.0049)
Constant	0.0175***	0.0196***	0.0204***
	(0.0027)	(0.0030)	(0.0030)
N	106	106	106

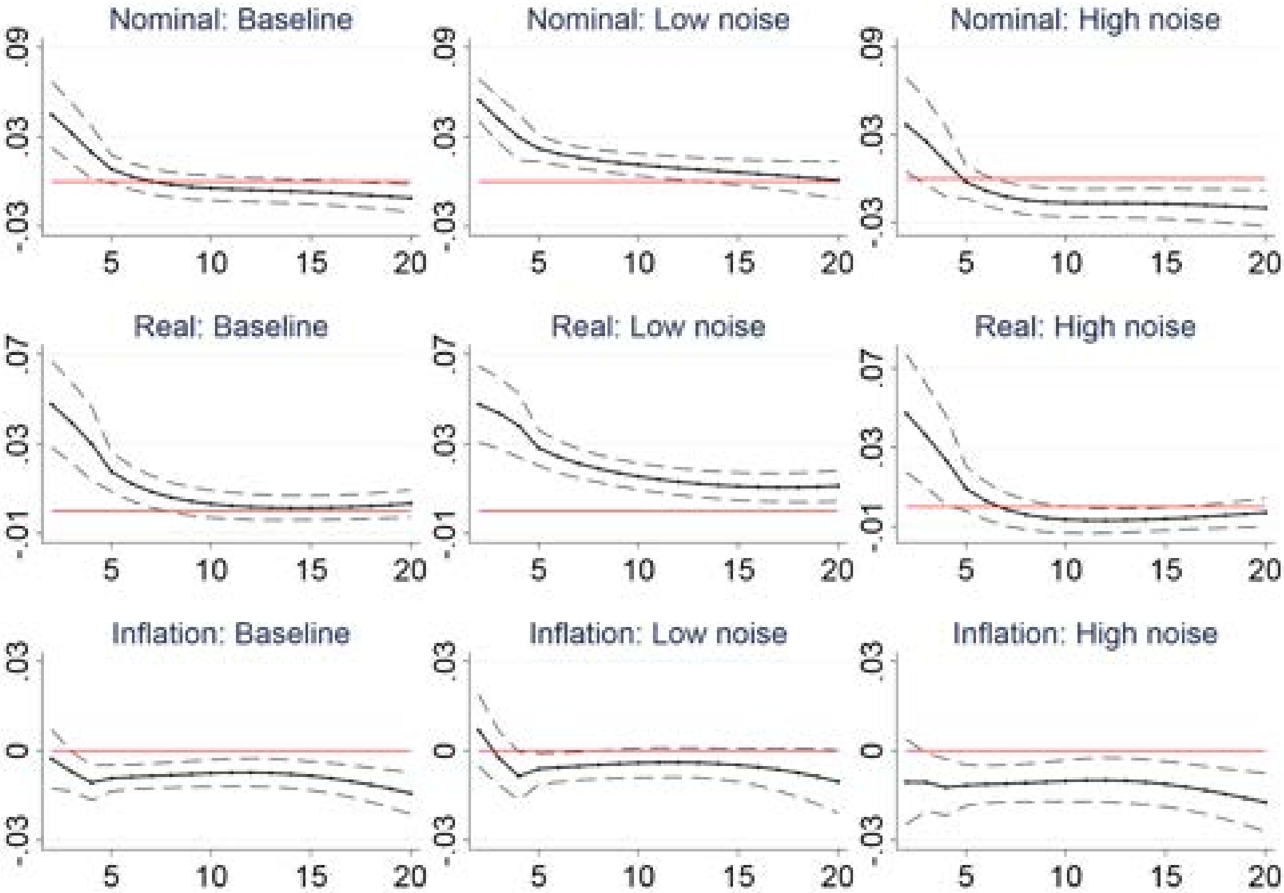
Notes: We report the results from a regression of the absolute value of the monetary policy shock on a constant and a dummy variable indicating whether the corresponding FOMC meeting is in the subset of announcements for which the yield curve noise measure is above its median level. The first column presents the results for the original noise measure. The second and third columns present the results for a detrended version of the noise measure, obtained respectively by applying the Hodrick-Prescott filter to the original series and by fitting a quadratic trend to the series. The sample includes all regularly scheduled FOMC meetings from 01/01/2000 to 19/03/2014, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 106 observations on which the policy news shock is computed and each regression is estimated. Standard errors are in parentheses. Asterisks denote significance levels (* p<0.1, ** p<0.05, *** p<0.0).

Figure 53: Impact of yield curve noise on the transmission of MP shocks to US forward rates: standardized shocks in the extended sample



Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 152 observations on which each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 120 observations (starting in 2004). Note that the shock series in each estimation are standardized such that the coefficient estimates represent the impact of a one-standard-deviation shock respectively for the full sample (first column) and in the low/high noise buckets (resp. second and third columns).

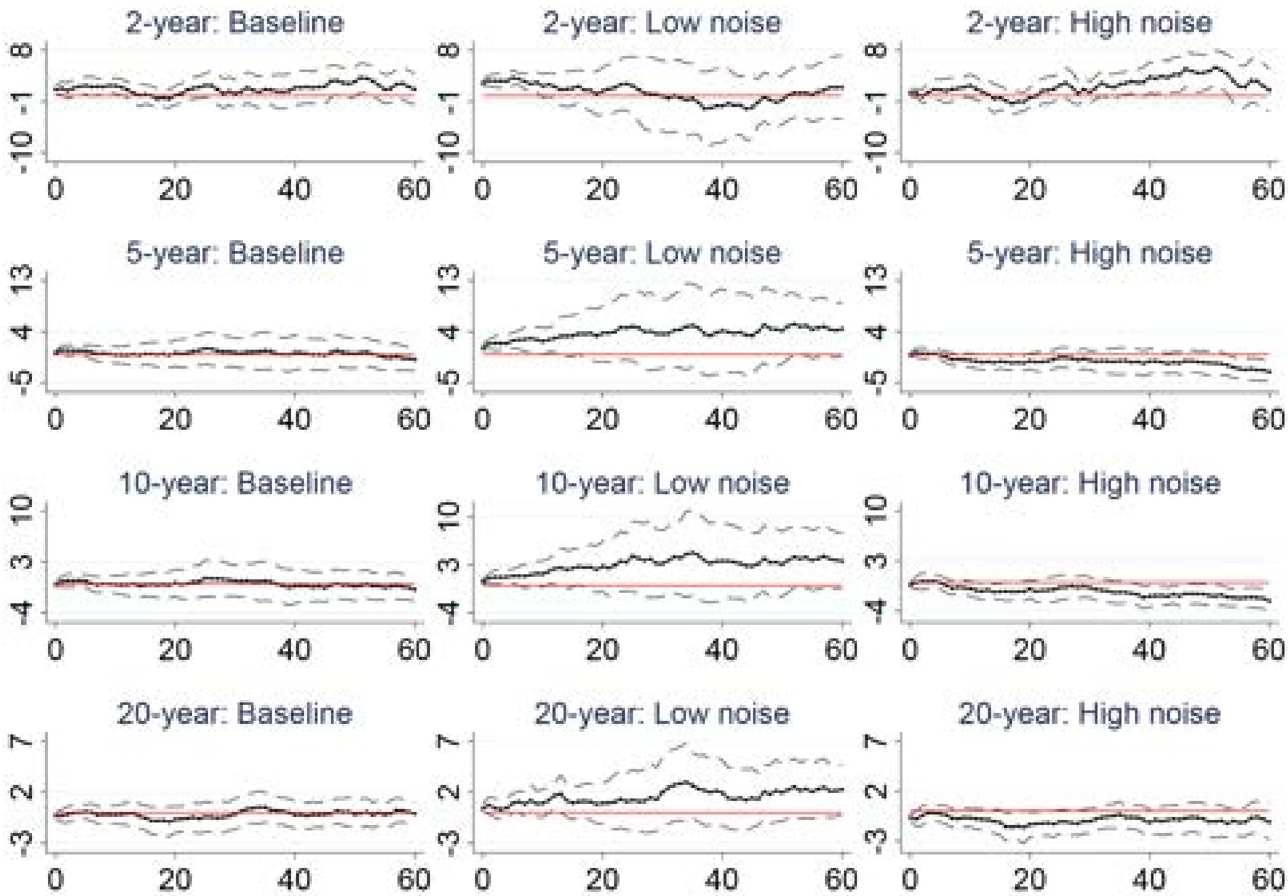
Figure 54: Impact of yield curve noise on the transmission of MP shocks to US forward rates: standardized shocks and detrended noise in the extended sample



Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. We use a detrended version of the noise measure, obtained by fitting a quadratic trend to the series. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 152 observations on which each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 120 observations (starting in 2004). Note that the shock series in each estimation are standardized such that the coefficient estimates represent the impact of a one-standard-deviation shock respectively for the full sample (first column) and in the low/high noise buckets (resp. second and third columns).

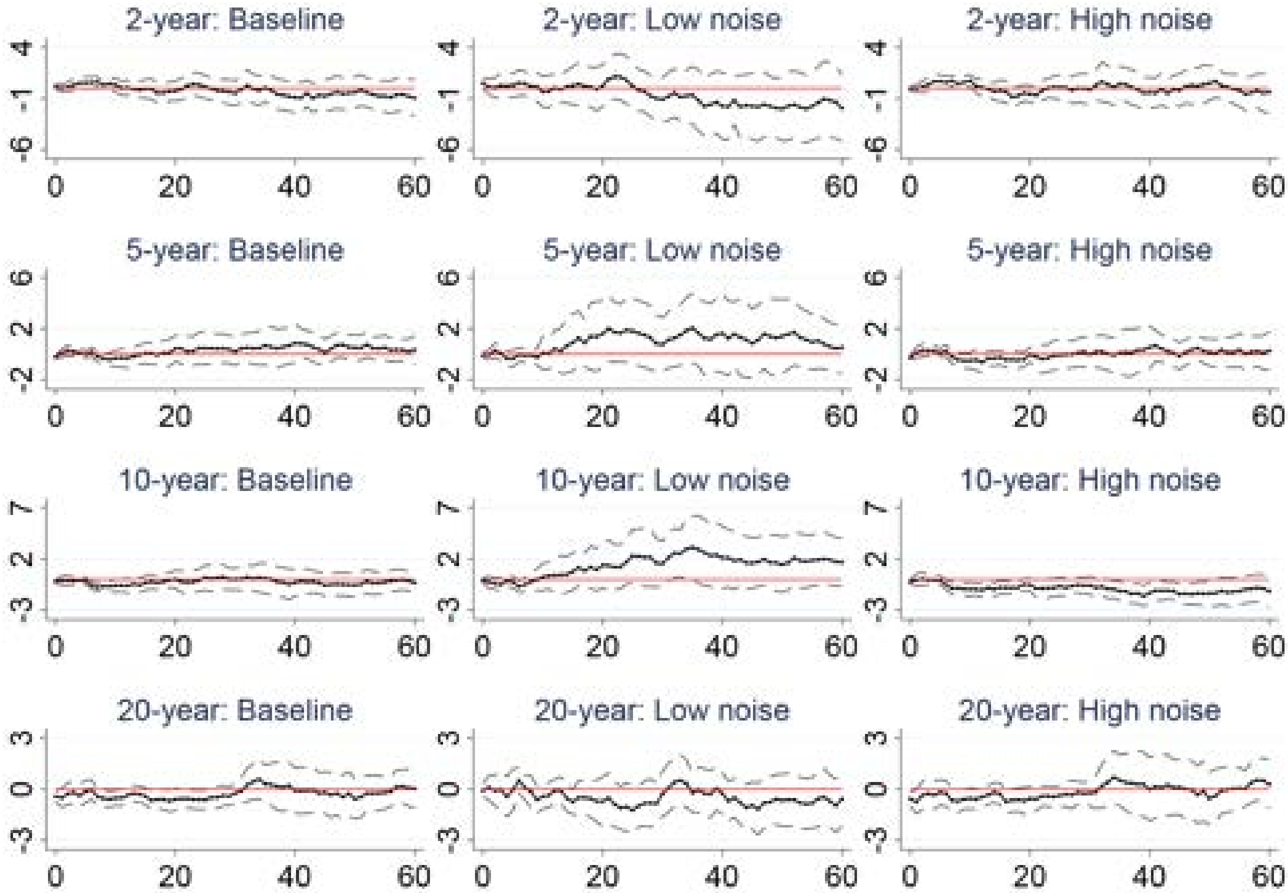
A.4.4 Dynamic Effects over Longer Horizon

Figure 55: Impact of yield curve noise on the transmission of MP shocks to US nominal forward rates at longer horizons



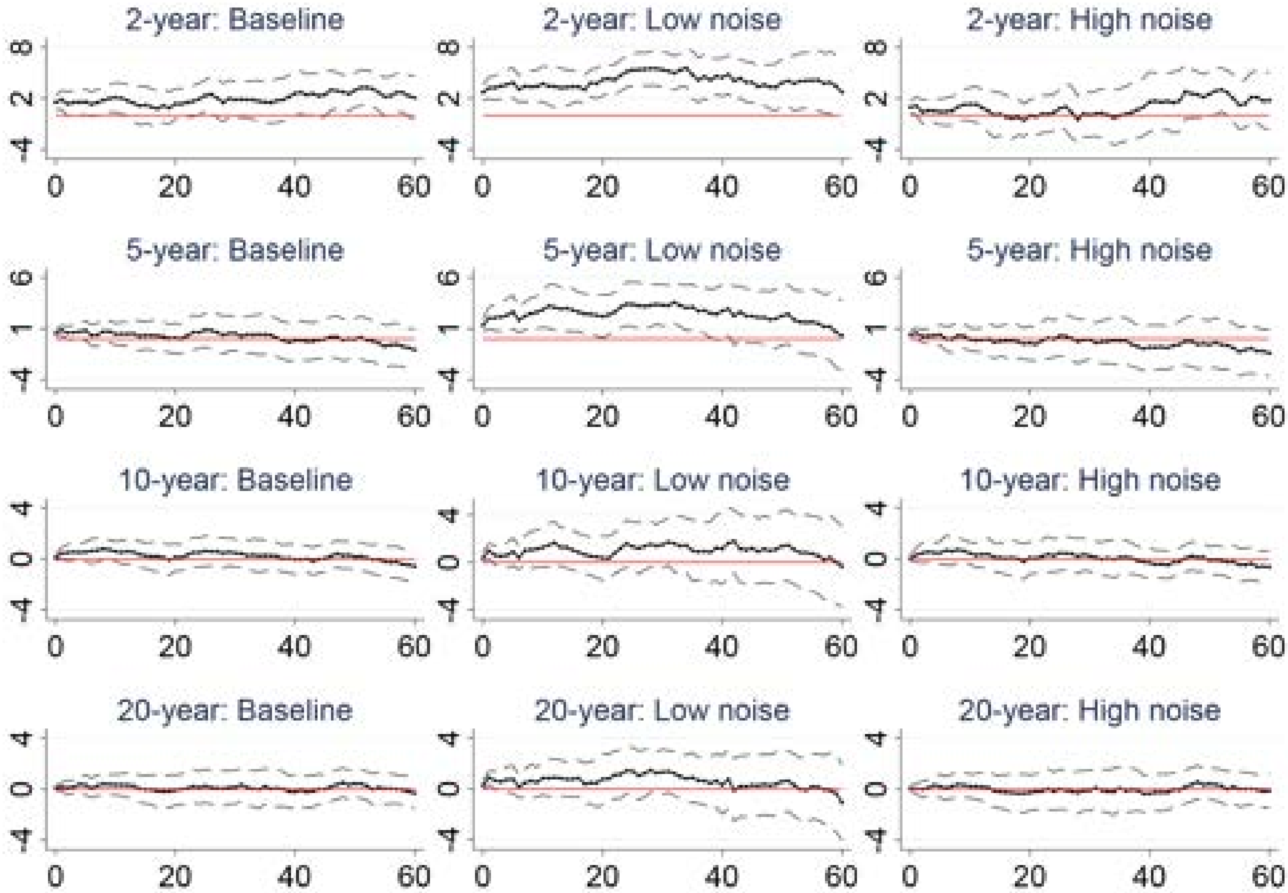
Note: The first column (Baseline) plots estimates of $\gamma^{n,h}_{all,\tau}$ in regression (4) for nominal forward rates with maturities of 2, 5, 10, and 20 years (in each row) and at horizons ranging from 1 to 60 trading days after the announcement, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^{n,h}_{hl,\tau}$ and $\gamma^{n,h}_{ll,\tau}$ in regression (5), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/01/2000 to 19/03/2014, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 106 observations on which the policy news shock is computed and each regression is estimated. Regression results for the 2-year forward rate are based on a sample size of 74 observations (starting in 2004).

Figure 56: Impact of yield curve noise on the transmission of MP shocks to US inflation forward rates at longer horizons



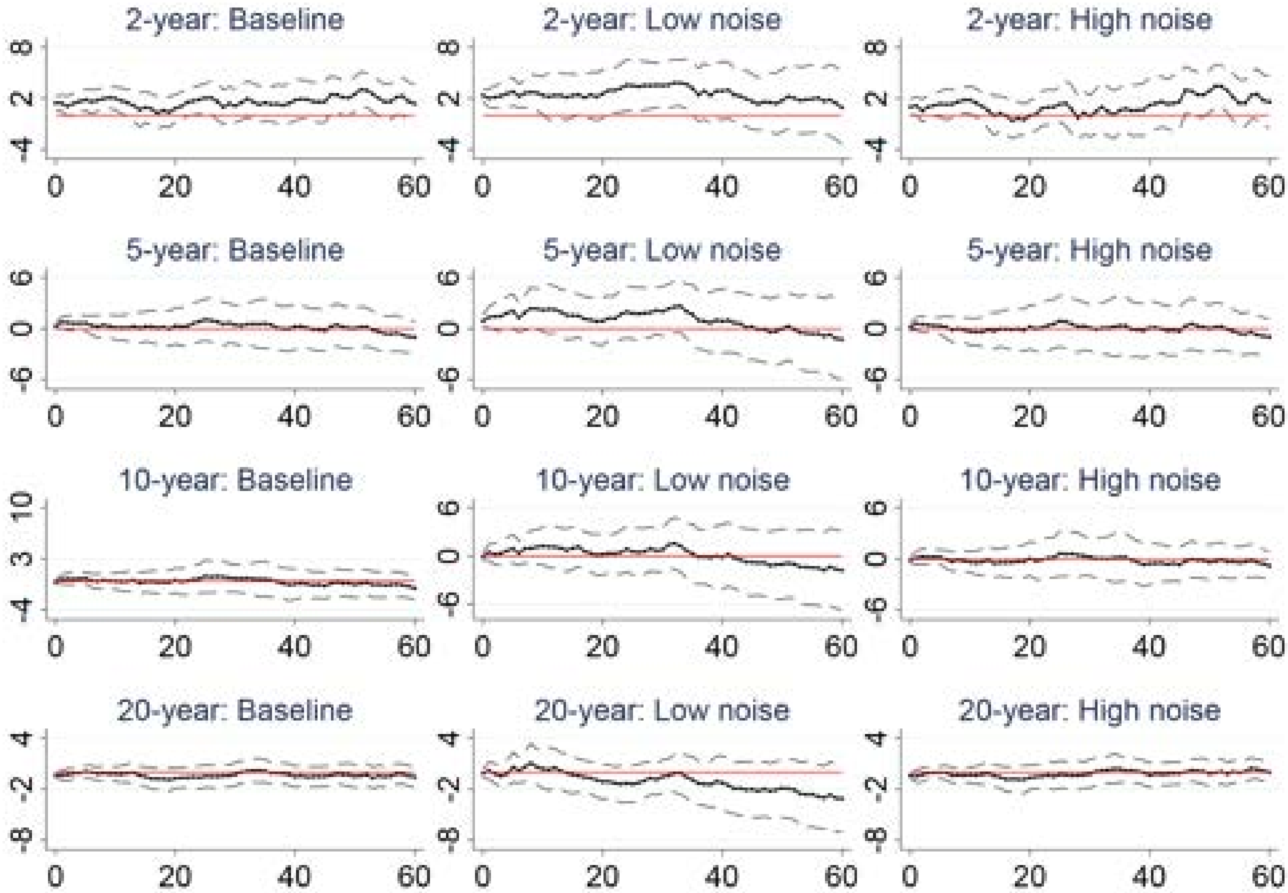
Note: The first column (Baseline) plots estimates of $\gamma_{all,\tau}^{\pi,h}$ in regression (4) for inflation forward rates with maturities of 2, 5, 10, and 20 years (in each row) and at horizons ranging from 1 to 60 trading days after the announcement, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma_{hl,\tau}^{\pi,h}$ and $\gamma_{ll,\tau}^{\pi,h}$ in regression (5), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/01/2000 to 19/03/2014, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 106 observations on which the policy news shock is computed and each regression is estimated. Regression results for the 2-year forward rate are based on a sample size of 74 observations (starting in 2004).

Figure 57: Impact of yield curve noise on the transmission of MP shocks to US real forward rates at longer horizons: extended sample



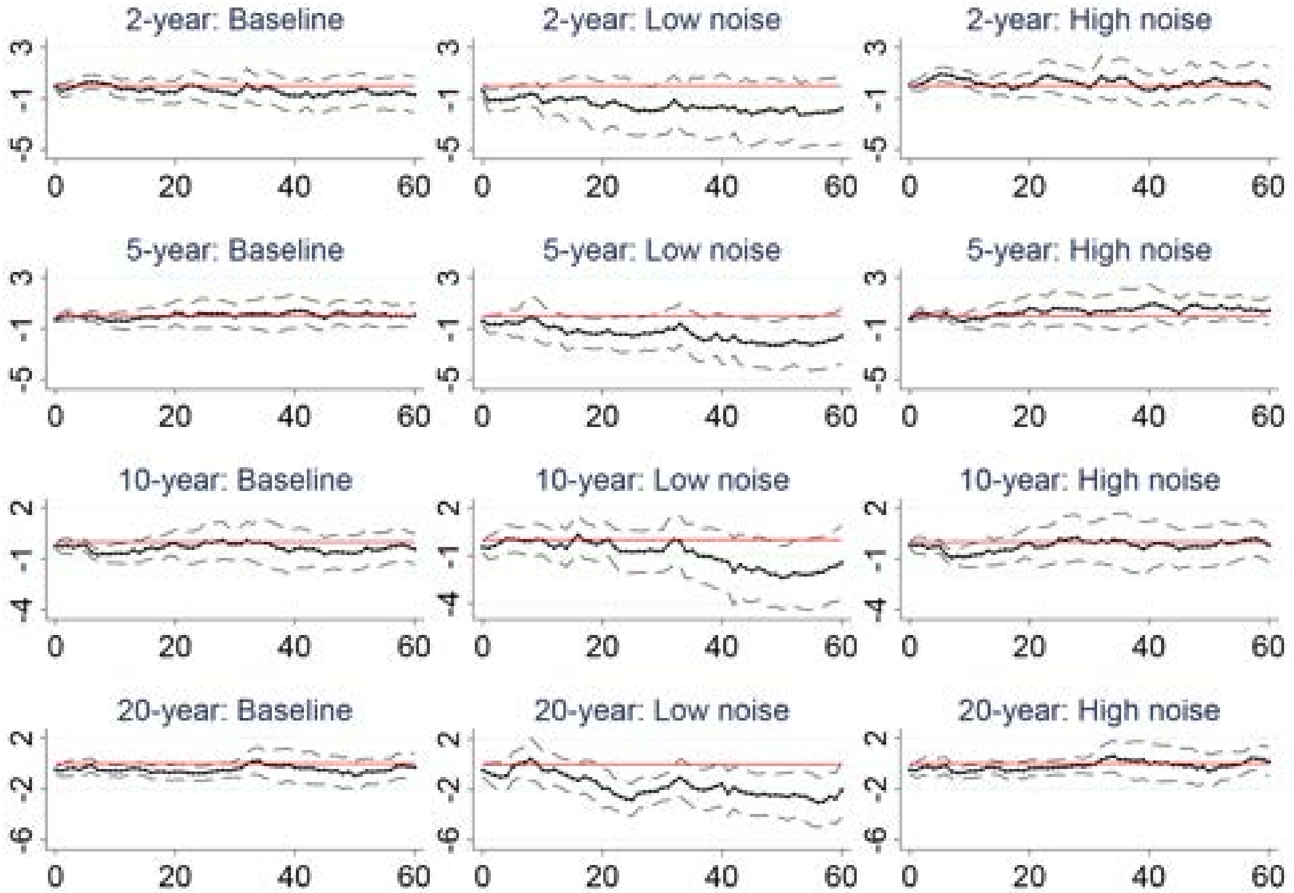
Note: The first column (Baseline) plots estimates of $\gamma^{r,h}_{all,\tau}$ in regression (4) for real forward rates with maturities of 2, 5, 10, and 20 years (in each row) and at horizons ranging from 1 to 60 trading days after the announcement, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^{r,h}_{hl,\tau}$ and $\gamma^{r,h}_{ll,\tau}$ in regression (5), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 152 observations on which each regression is estimated. Regression results for the 2-year forward rate are based on a sample size of 120 observations (starting in 2004).

Figure 58: Impact of yield curve noise on the transmission of MP shocks to US nominal forward rates at longer horizons: extended sample



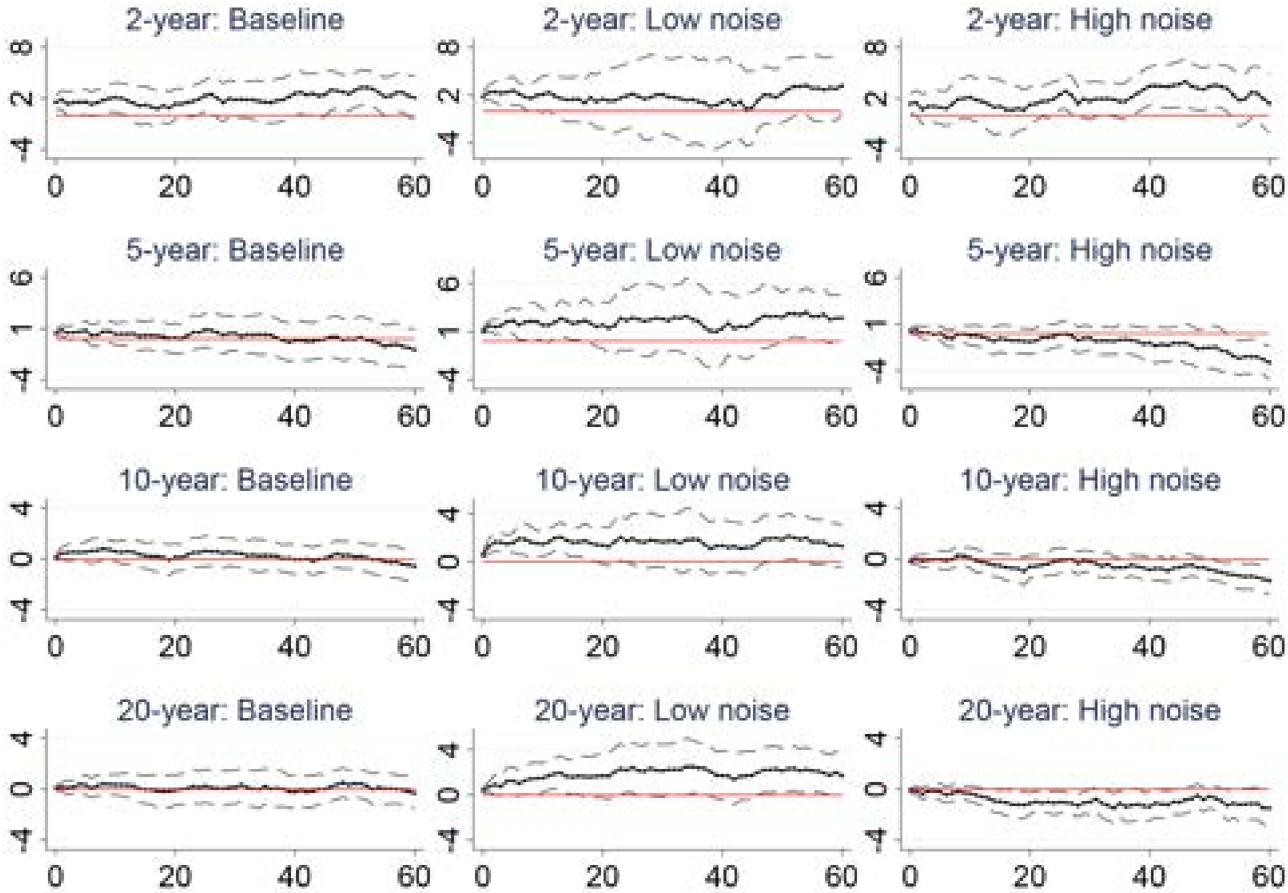
Note: The first column (Baseline) plots estimates of $\gamma_{all,\tau}^{n,h}$ in regression (4) for nominal forward rates with maturities of 2, 5, 10, and 20 years (in each row) and at horizons ranging from 1 to 60 trading days after the announcement, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma_{hl,\tau}^{n,h}$ and $\gamma_{ll,\tau}^{n,h}$ in regression (5), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 152 observations on which each regression is estimated. Regression results for the 2-year forward rate are based on a sample size of 120 observations (starting in 2004).

Figure 59: Impact of yield curve noise on the transmission of MP shocks to US inflation forward rates at longer horizons: extended sample



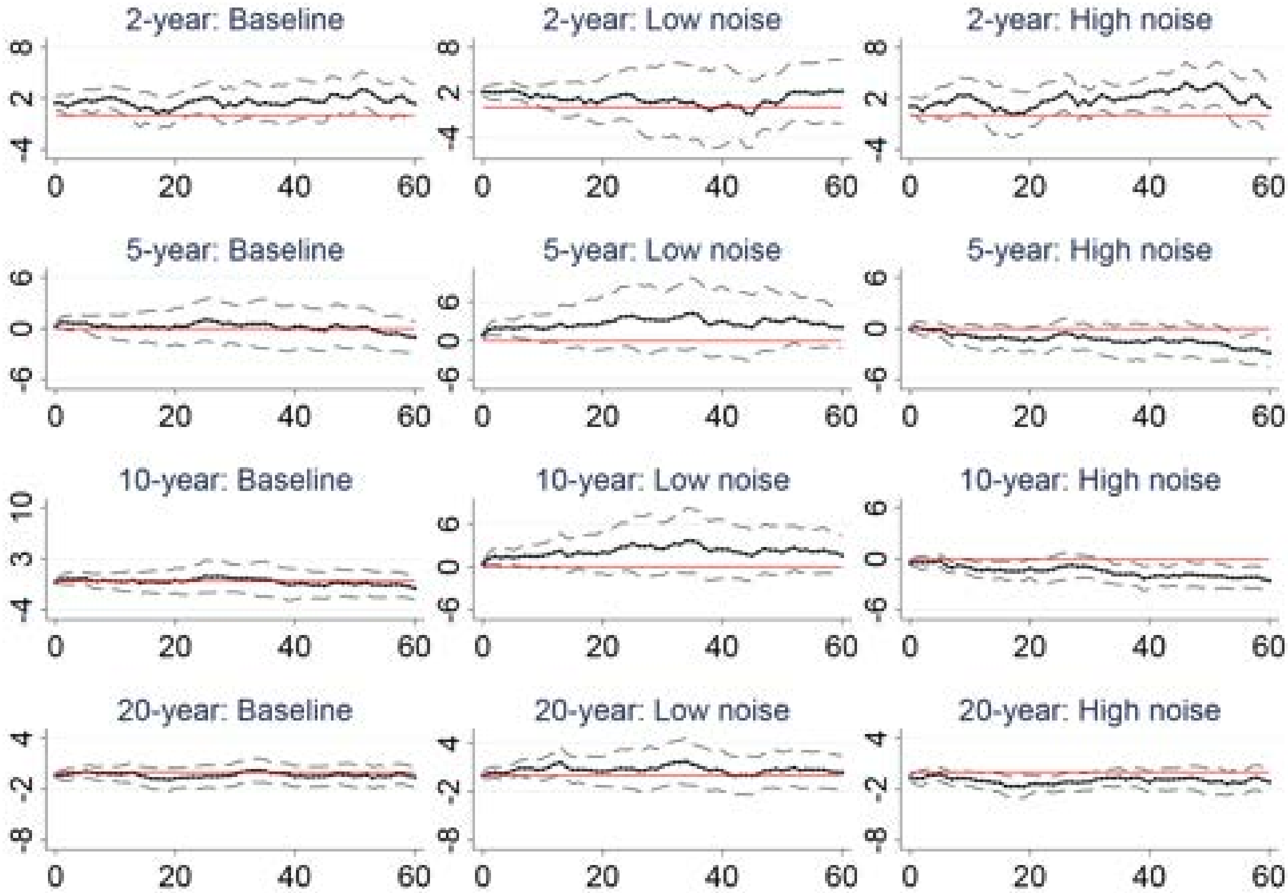
Note: The first column (Baseline) plots estimates of $\gamma^{\pi,h}_{all,\tau}$ in regression (4) for inflation forward rates with maturities of 2, 5, 10, and 20 years (in each row) and at horizons ranging from 1 to 60 trading days after the announcement, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^{\pi,h}_{hl,\tau}$ and $\gamma^{\pi,h}_{ll,\tau}$ in regression (5), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 152 observations on which each regression is estimated. Regression results for the 2-year forward rate are based on a sample size of 120 observations (starting in 2004).

Figure 60: Impact of yield curve noise on the transmission of MP shocks to US real forward rates at longer horizons: detrended noise in the extended sample



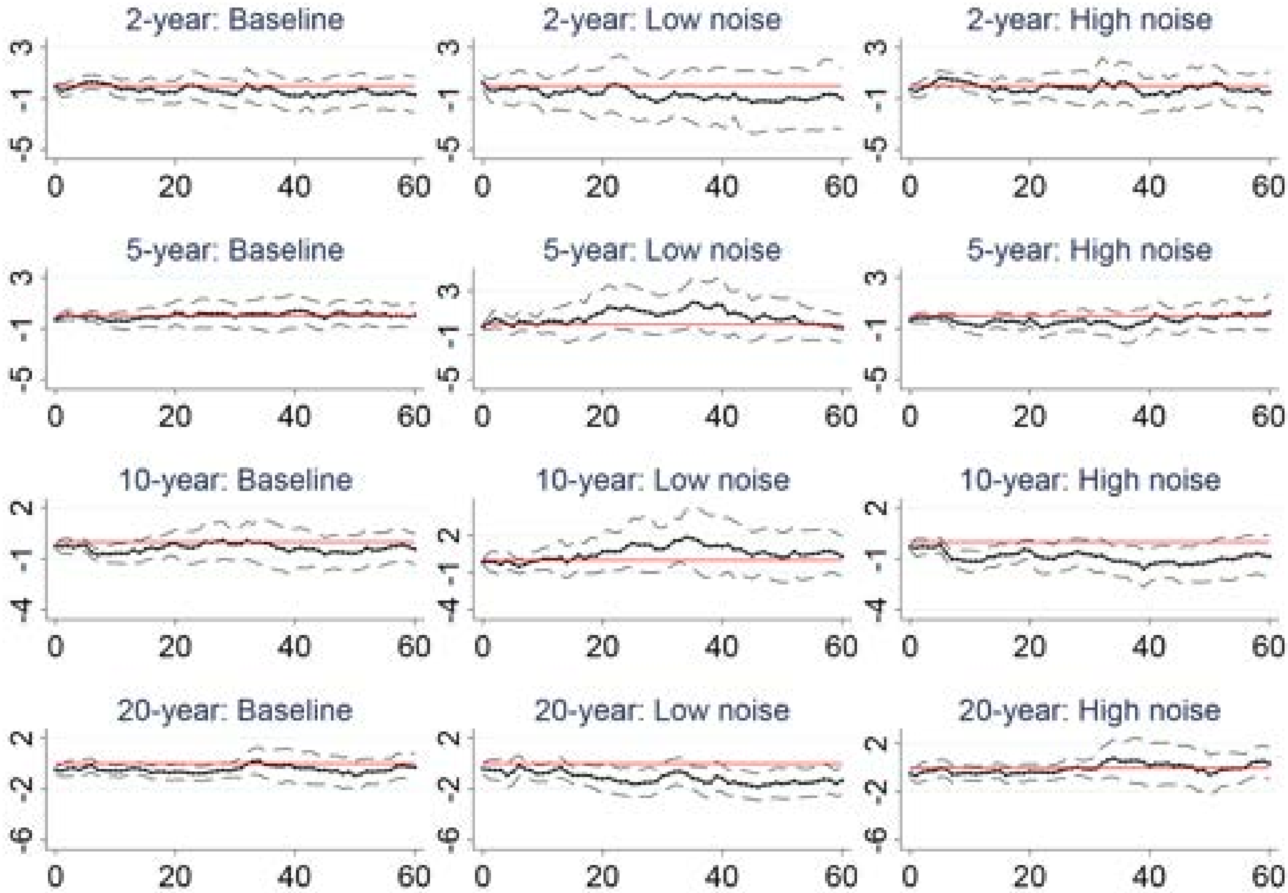
Note: The first column (Baseline) plots estimates of $\gamma^{r,h}_{all,\tau}$ in regression (4) for real forward rates with maturities of 2, 5, 10, and 20 years (in each row) and at horizons ranging from 1 to 60 trading days after the announcement, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^{r,h}_{hl,\tau}$ and $\gamma^{r,h}_{ll,\tau}$ in regression (5), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. We use a detrended version of the noise measure, obtained by fitting a quadratic trend to the series. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 152 observations on which each regression is estimated. Regression results for the 2-year forward rate are based on a sample size of 120 observations (starting in 2004).

Figure 61: Impact of yield curve noise on the transmission of MP shocks to US nominal forward rates at longer horizons: detrended noise in the extended sample



Note: The first column (Baseline) plots estimates of $\gamma^{n,h}_{all,\tau}$ in regression (4) for nominal forward rates with maturities of 2, 5, 10, and 20 years (in each row) and at horizons ranging from 1 to 60 trading days after the announcement, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^{n,h}_{hl,\tau}$ and $\gamma^{n,h}_{ll,\tau}$ in regression (5), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. We use a detrended version of the noise measure, obtained by fitting a quadratic trend to the series. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 152 observations on which each regression is estimated. Regression results for the 2-year forward rate are based on a sample size of 120 observations (starting in 2004).

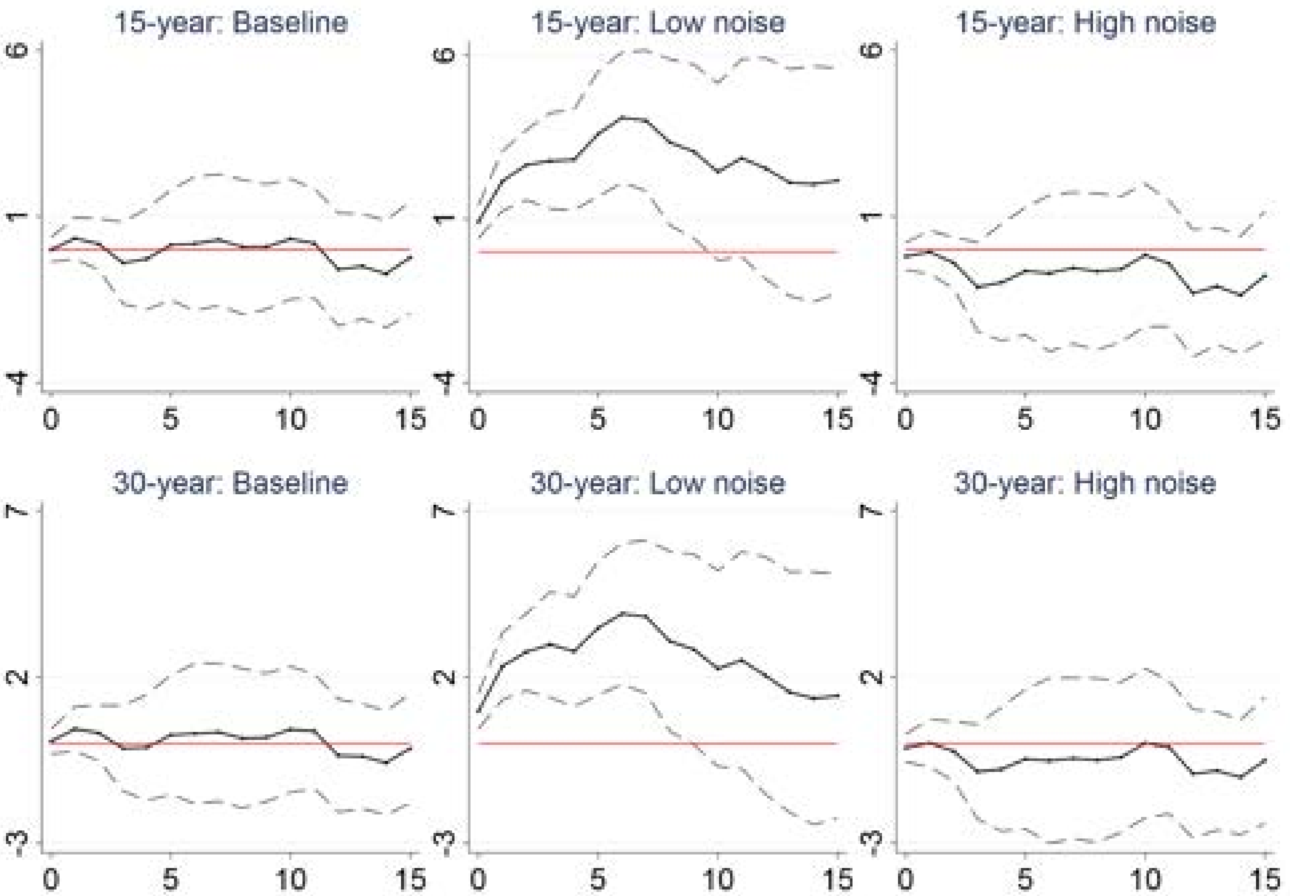
Figure 62: Impact of yield curve noise on the transmission of MP shocks to US inflation forward rates at longer horizons: detrended noise in the extended sample



Note: The first column (Baseline) plots estimates of $\gamma^{\pi,h}_{all,\tau}$ in regression (4) for inflation forward rates with maturities of 2, 5, 10, and 20 years (in each row) and at horizons ranging from 1 to 60 trading days after the announcement, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^{\pi,h}_{hl,\tau}$ and $\gamma^{\pi,h}_{ll,\tau}$ in regression (5), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. We use a detrended version of the noise measure, obtained by fitting a quadratic trend to the series. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 152 observations on which each regression is estimated. Regression results for the 2-year forward rate are based on a sample size of 120 observations (starting in 2004).

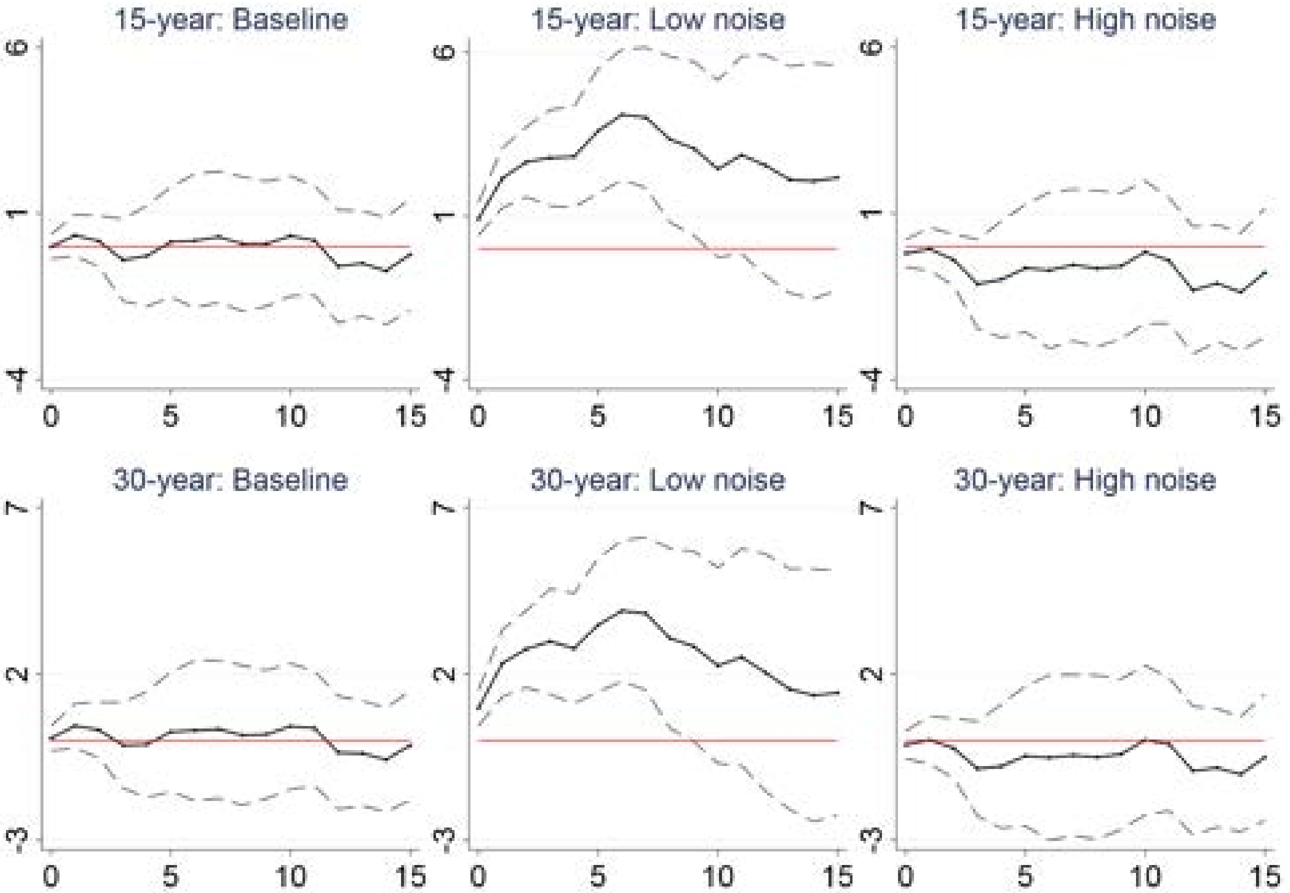
A.4.5 Macroeconomic Implications: The Response of Mortgage Rates

Figure 63: Impact of yield curve noise on the transmission of MP shocks to US fixed mortgage rates: extended sample



Note: The first column (Baseline) plots estimates of $\gamma^{h}_{all,\tau}$ in regression (4) for US fixed mortgage rates with maturities of 15 and 30 years (in each row) and at horizons ranging from 1 to 15 weeks after the announcement, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^{h}_{hl,\tau}$ and $\gamma^{r,h}_{ll,\tau}$ in regression (5), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 152 observations on which each regression is estimated.

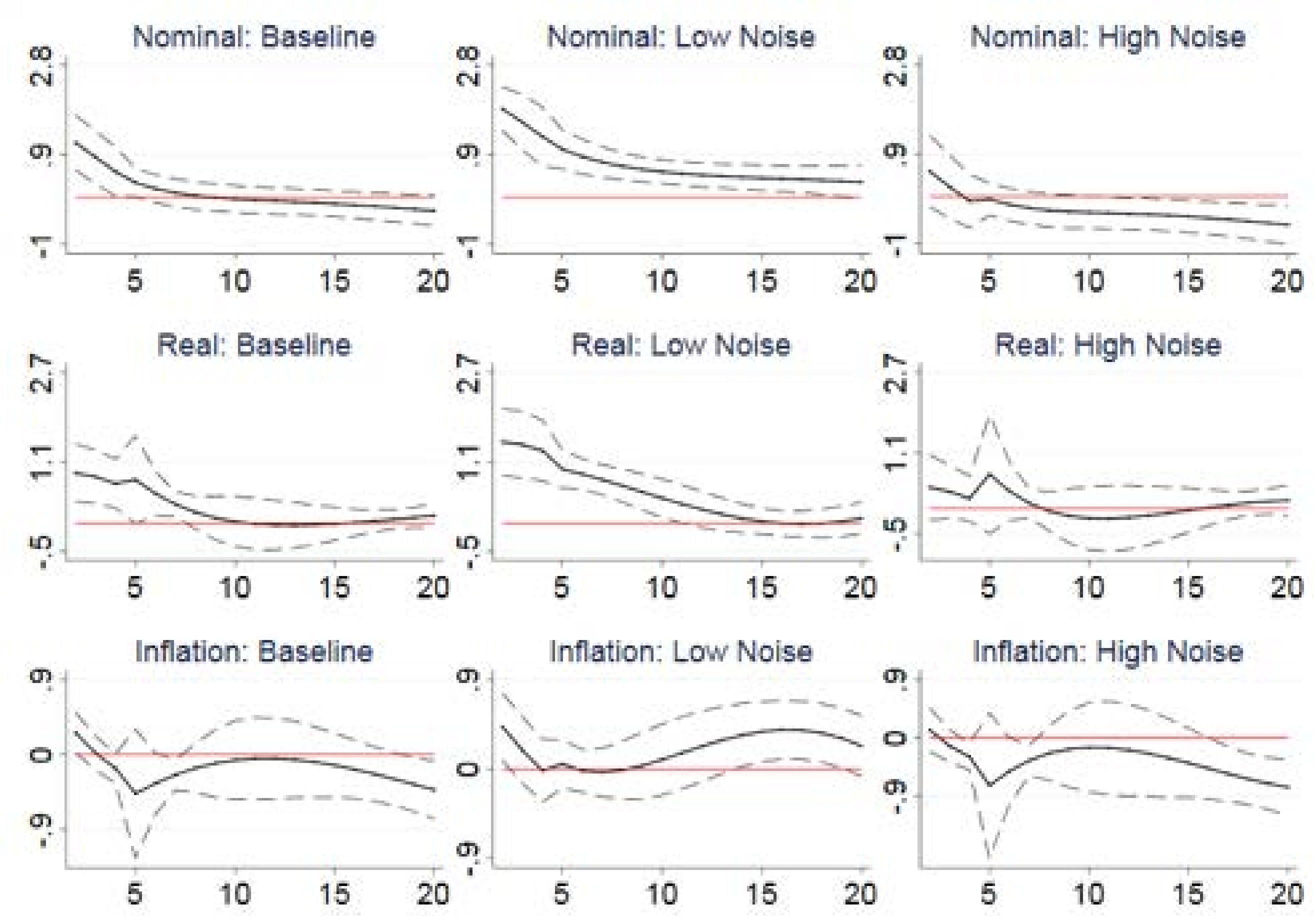
Figure 64: Impact of yield curve noise on the transmission of MP shocks to US fixed mortgage rates: detrended noise in the extended sample



Note: The first column (Baseline) plots estimates of $\gamma^h_{all,\tau}$ in regression (4) for US fixed mortgage rates with maturities of 15 and 30 years (in each row) and at horizons ranging from 1 to 15 weeks after the announcement, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^h_{hl,\tau}$ and $\gamma^{r,h}_{ll,\tau}$ in regression (5), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. We use a detrended version of the noise measure, obtained by fitting a quadratic trend to the series. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 152 observations on which each regression is estimated.

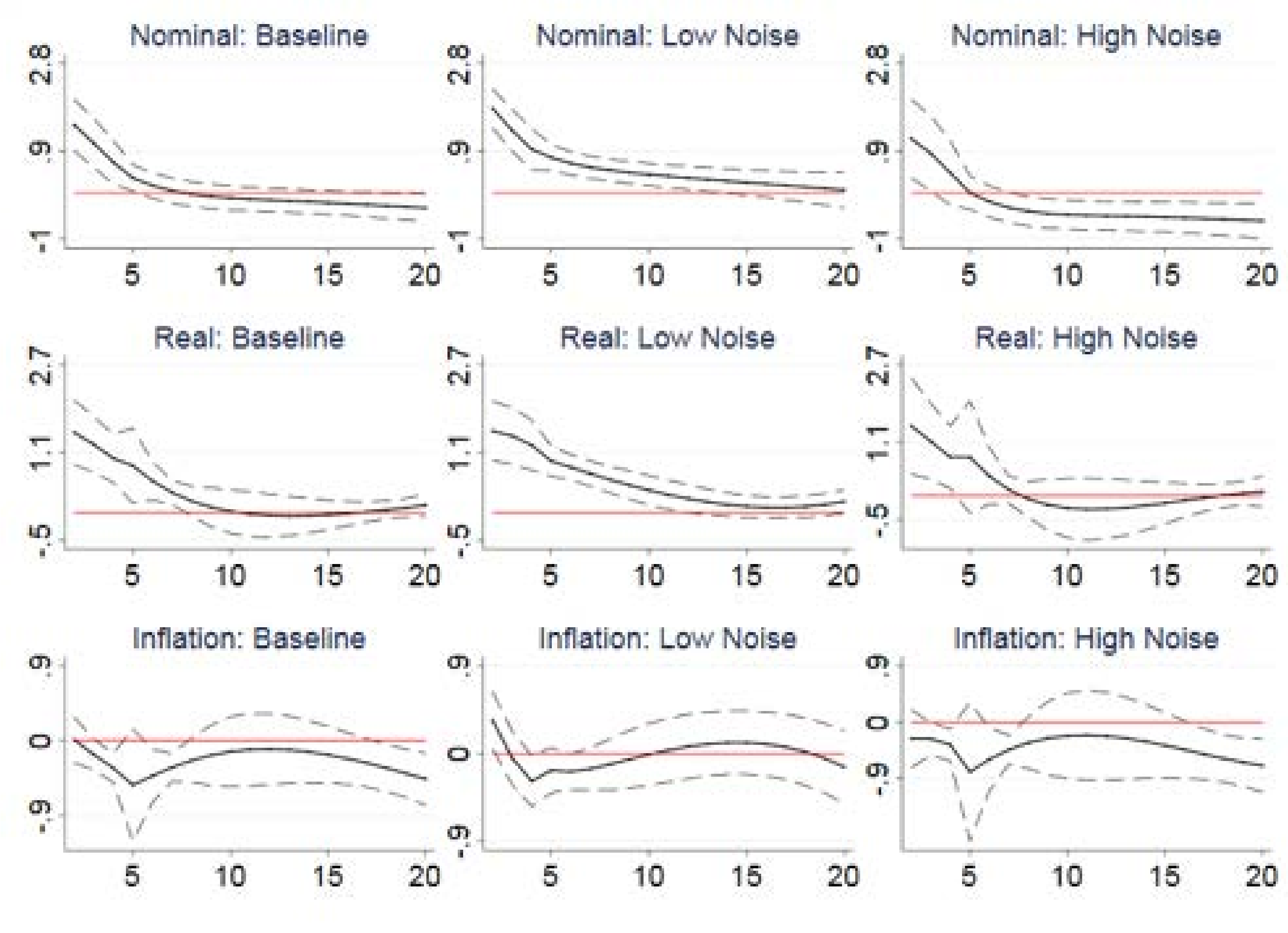
A.4.6 Impact of different data vintages

Figure 65: Impact of yield curve noise on the transmission of MP shocks to US forward rates: using most recent data vintage for real interest rates



Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/01/2000 to 19/03/2014, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 106 observations on which the policy news shock is computed and each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 74 observations (starting in 2004).

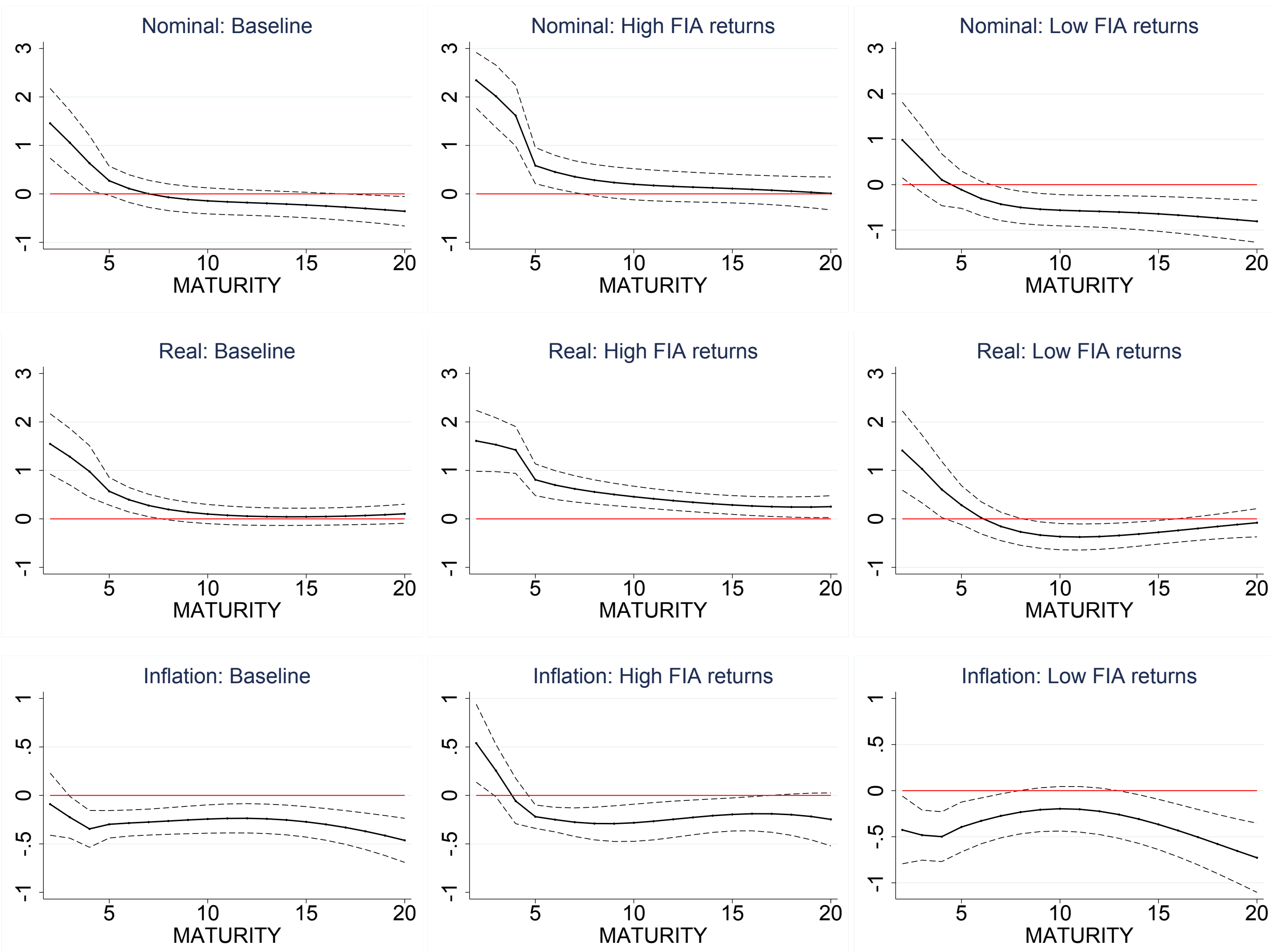
Figure 66: Impact of yield curve noise on the transmission of MP shocks to US forward rates: using most recent data vintage for real interest rates in the extended sample



Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. We use a detrended version of the noise measure, obtained by fitting a quadratic trend to the series. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 152 observations on which each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 120 observations (starting in 2004).

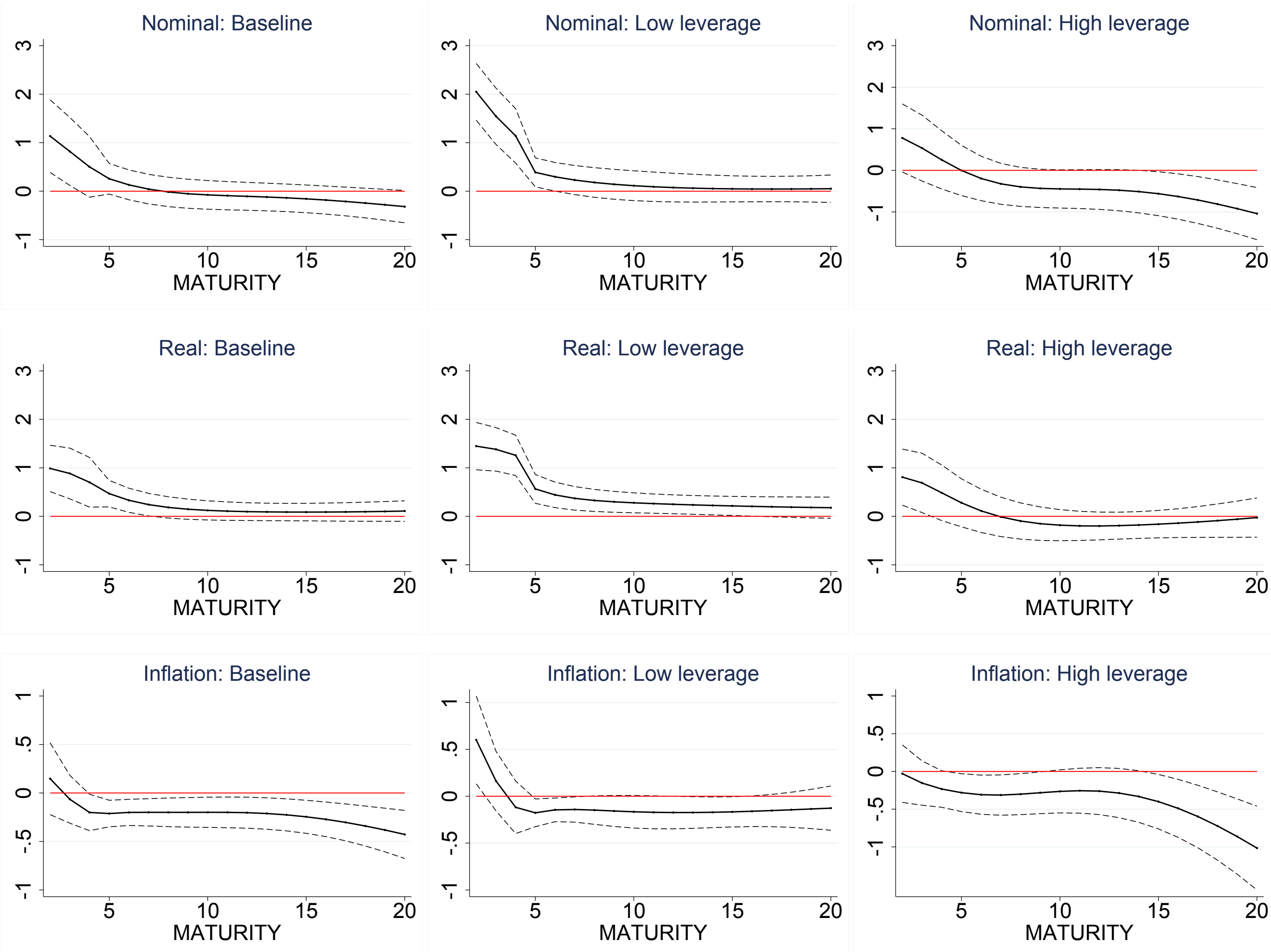
A.4.7 Conditioning on primary dealers and hedge fund proxies

Figure 67: Impact of FIA hedge fund returns on the transmission of MP shocks to US forward rates: extended sample



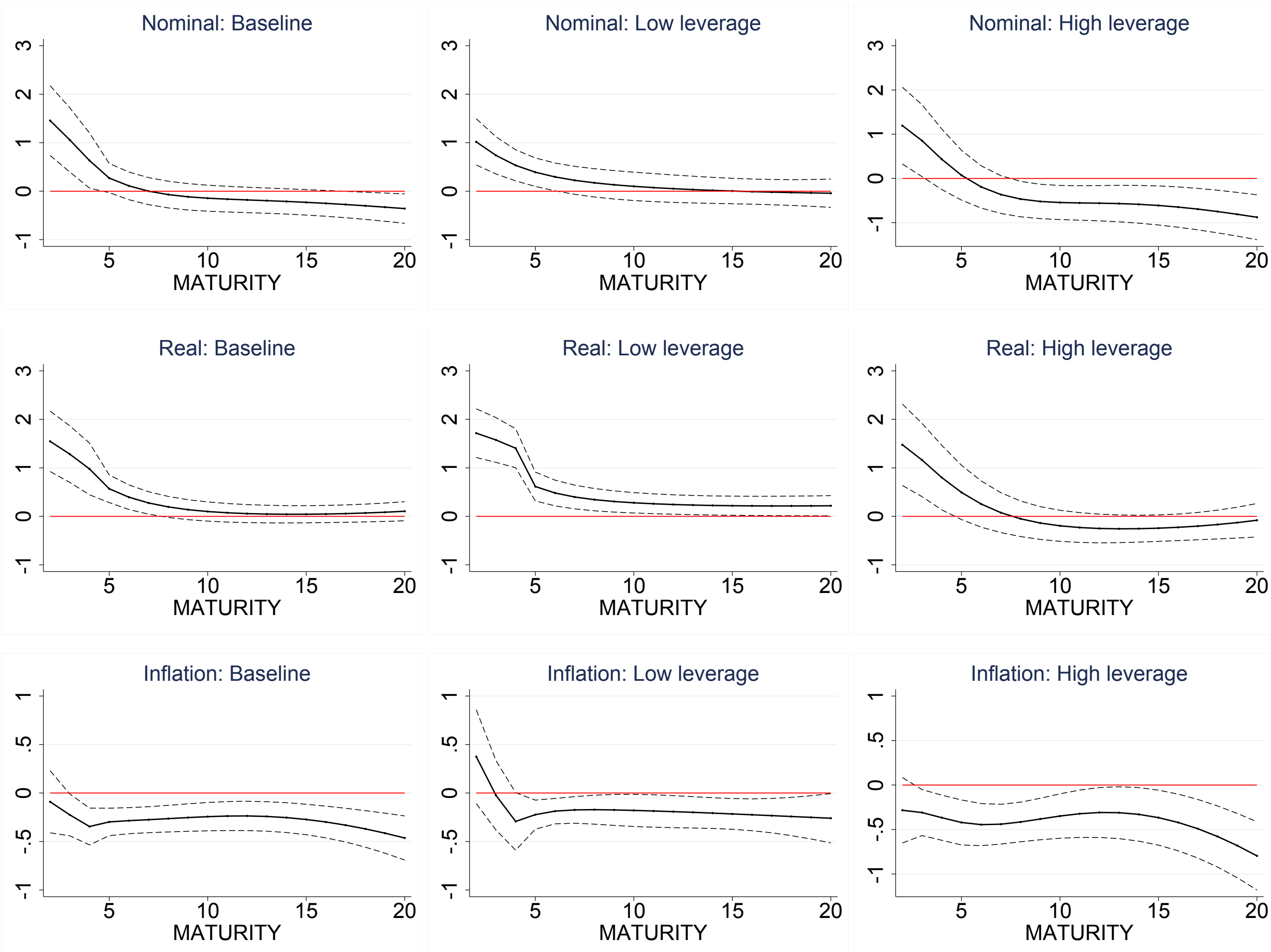
Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (High FIA returns) and third (High FIA returns) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the hedge fund return index is above (resp. below) its median level. The index measures the average return of the fixed-income arbitrage hedge funds in the Barclays database. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 152 observations on which each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 120 observations (starting in 2004).

Figure 68: Impact of leverage on the transmission of MP shocks to US forward rates



Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (Low leverage) and third (High leverage) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the intermediary leverage measure of He, Kelly, and Manela (2017) is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/01/2000 to 19/03/2014, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 106 observations on which the policy news shock is computed and each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 74 observations (starting in 2004).

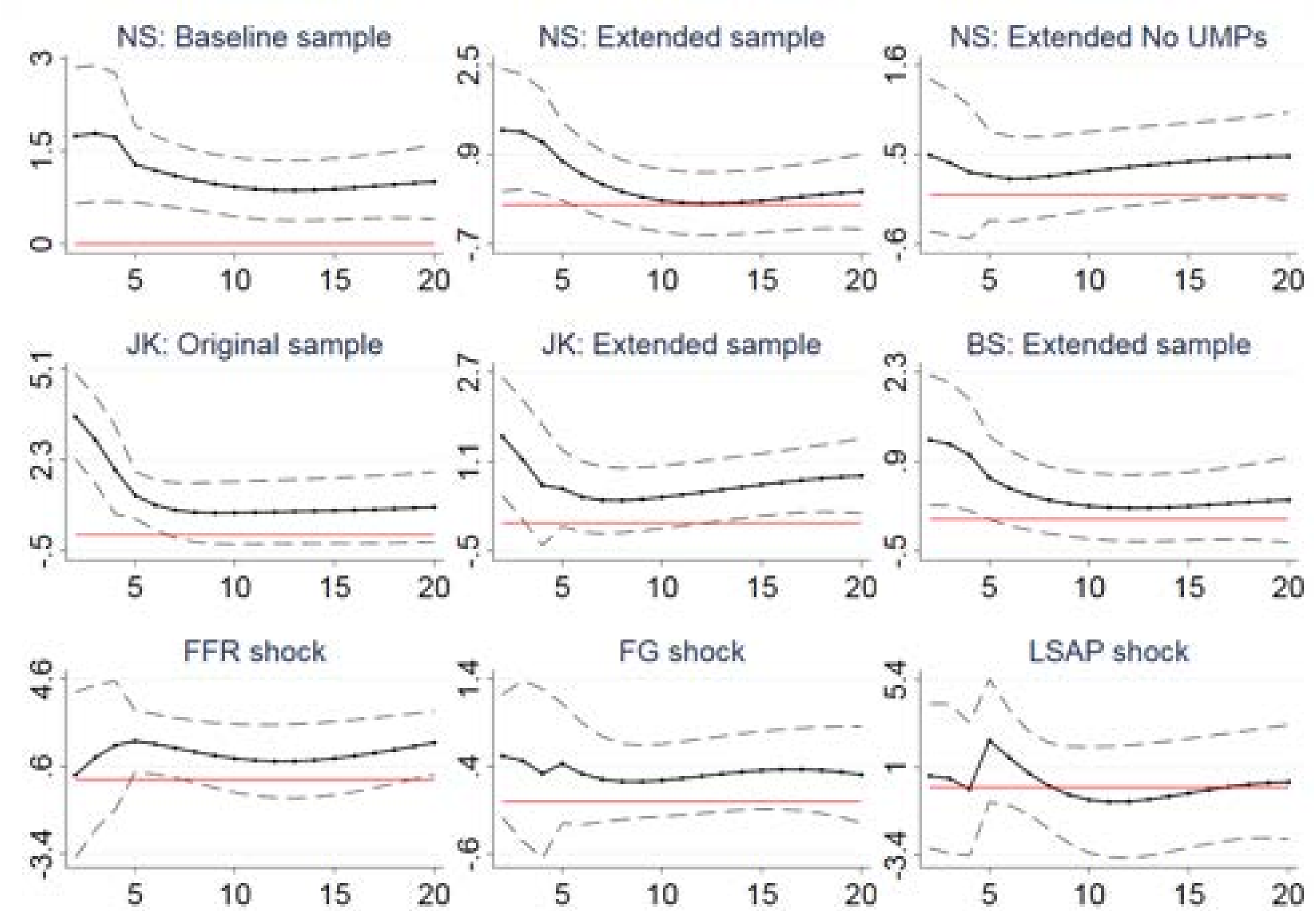
Figure 69: Impact of leverage on the transmission of MP shocks to US forward rates: extended sample



Note: The first column (Baseline) plots estimates of $\gamma^{i}_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (Low leverage) and third (High leverage) columns respectively present parameter estimates for $\gamma^{i}_{hl,\tau}$ and $\gamma^{i}_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the intermediary leverage measure of He, Kelly, and Manela (2017) is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 152 observations on which each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 120 observations (starting in 2004).

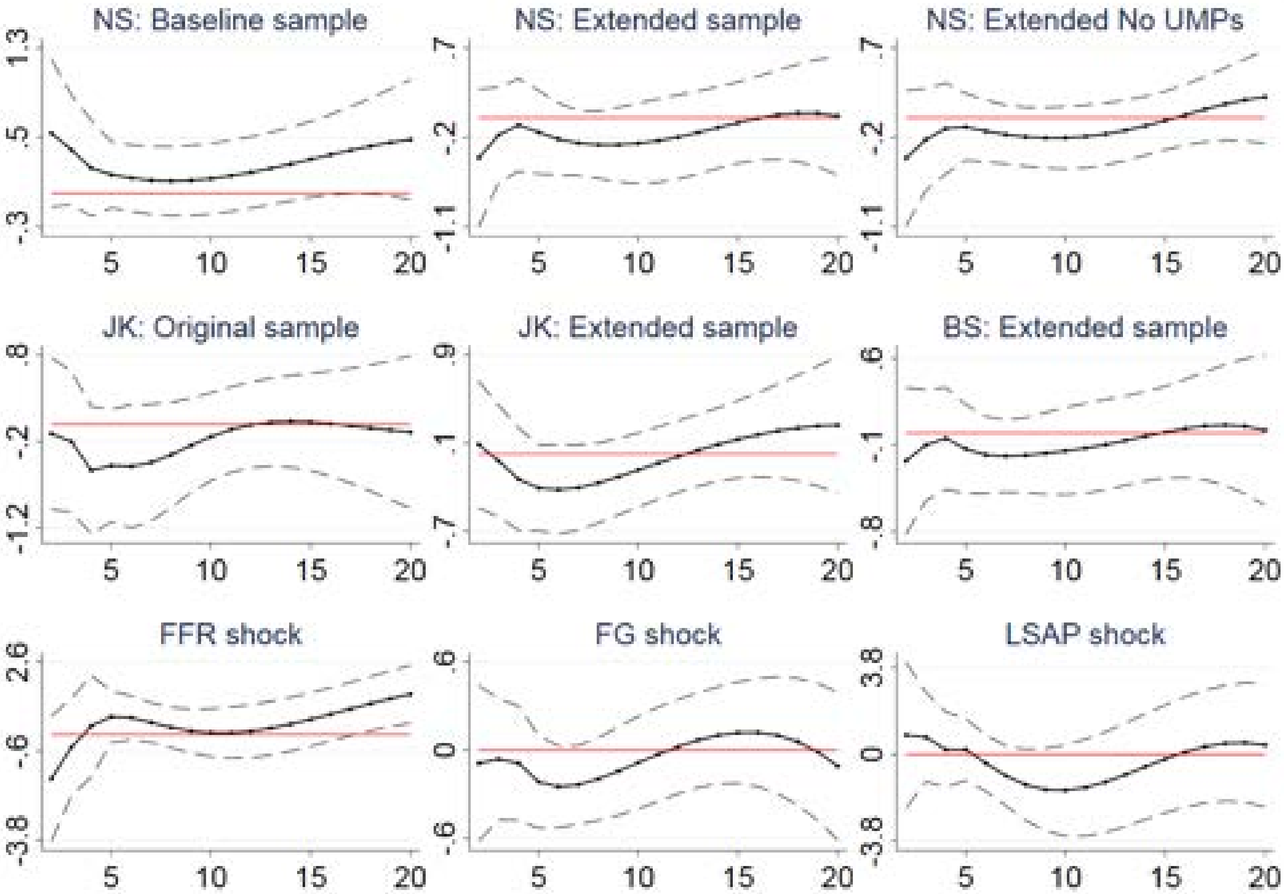
A.4.8 Summary of robustness checks for alternative samples and monetary policy shocks

Figure 70: Robustness of state dependence for nominal forward rates to different samples and monetary policy shocks



Note: The charts plot the estimates of $\gamma^n_{h-l,\tau}$ in regression (3) for each nominal forward rate with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The first row reports the results for the monetary policy shock of Nakamura and Steinsson (2018) respectively using their baseline sample from 2000/01 to 2014/03 (first column); an extended sample from 2000/01 to 2019/12 with the updated data provided in Acosta (2022) (second column); and the extended sample excluding unconventional monetary policy announcements identified in Table 5 of Cieslak and Schrimpf (2019) (third column). The first two columns of the second row report results for the MP shock of Jarocinski and Karadi (2020) using their orginal sample 2000/01-2016/12 and an extended sample from 2000/01 to 2019/06. The third column of the second row reports the results for the MP shock of Bauer and Swanson (2023b) for the sample from 2000/01 to 2019/12. The last row reports the results for the Federal Funds Rate (FFR), Forward Guidance (FG), and Large Scale Asset Purchase shocks of Swanson (2021) using a sample from 2000/01 to 2019/06. We exclude FOMC meetings taking place between July 2008 and June 2009 from the estimation except in the results based on the MP shock of Jarocinski and Karadi (2020) which only considers the subset of policy announcements for which the high-frequency changes in the S&P 500 and interest rates have opposite signs. Regression results for the 2, 3 and 4-year forward rates are based on samples starting in 2004/01.

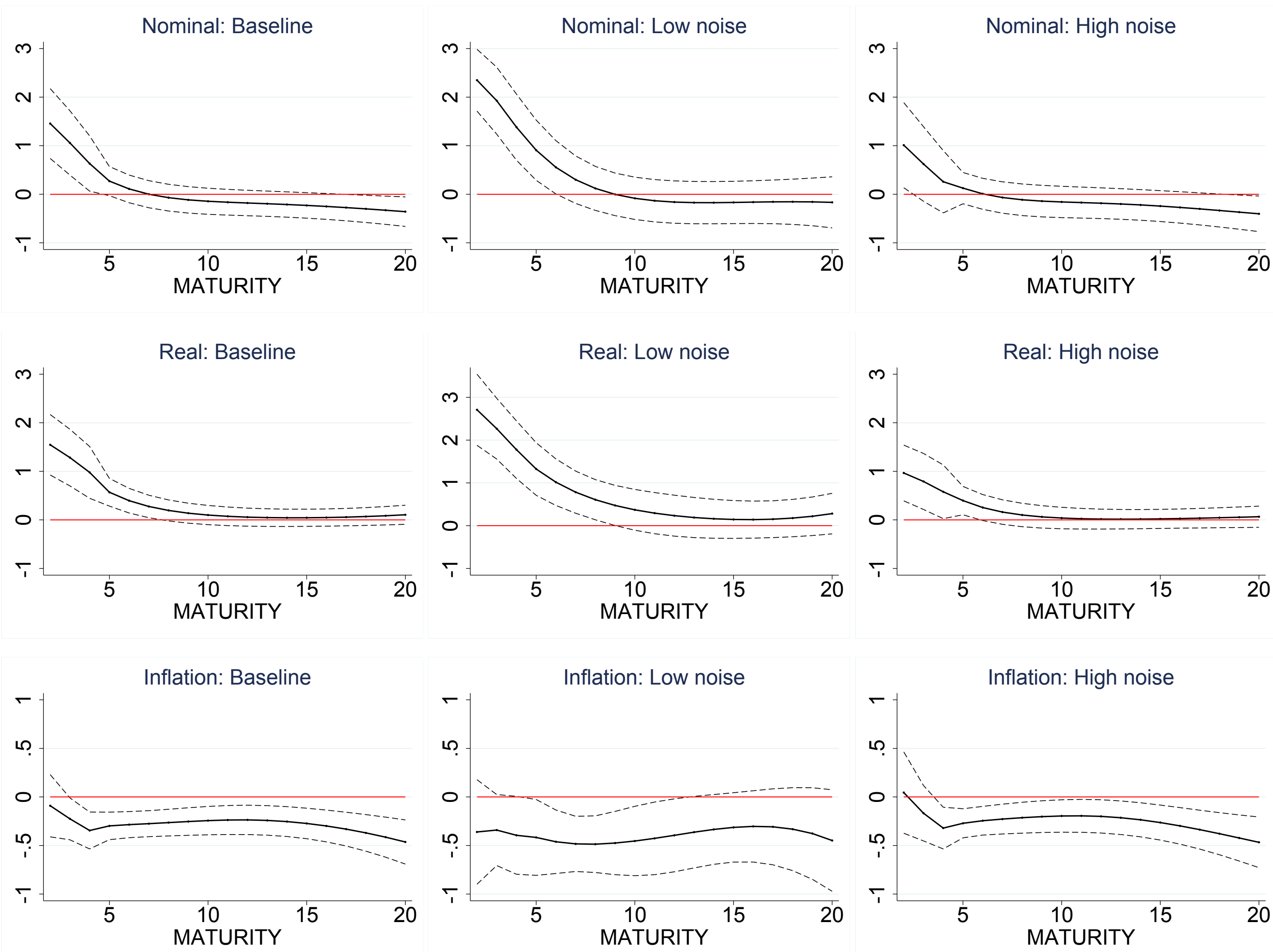
Figure 71: Robustness of state dependence for inflation forward rates to different samples and monetary policy shocks



Note: The charts plot the estimates of $\gamma^{\pi}_{h-l,\tau}$ in regression (3) for each inflation forward rate with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The first row reports the results for the monetary policy shock of Nakamura and Steinsson (2018) respectively using their baseline sample from 2000/01 to 2014/03 (first column); an extended sample from 2000/01 to 2019/12 with the updated data provided in Acosta (2022) (second column); and the extended sample excluding unconventional monetary policy announcements identified in Table 5 of Cieslak and Schrimpf (2019) (third column). The first two columns of the second row report results for the MP shock of Jarocinski and Karadi (2020) using their orginal sample 2000/01-2016/12 and an extended sample from 2000/01 to 2019/06. The third column of the second row reports the results for the MP shock of Bauer and Swanson (2023b) for the sample from 2000/01 to 2019/12. The last row reports the results for the Federal Funds Rate (FFR), Forward Guidance (FG), and Large Scale Asset Purchase shocks of Swanson (2021) using a sample from 2000/01 to 2019/06. We exclude FOMC meetings taking place between July 2008 and June 2009 from the estimation except in the results based on the MP shock of Jarocinski and Karadi (2020) which only considers the subset of policy announcements for which the high-frequency changes in the S&P 500 and interest rates have opposite signs. Regression results for the 2, 3 and 4-year forward rates are based on samples starting in 2004/01.

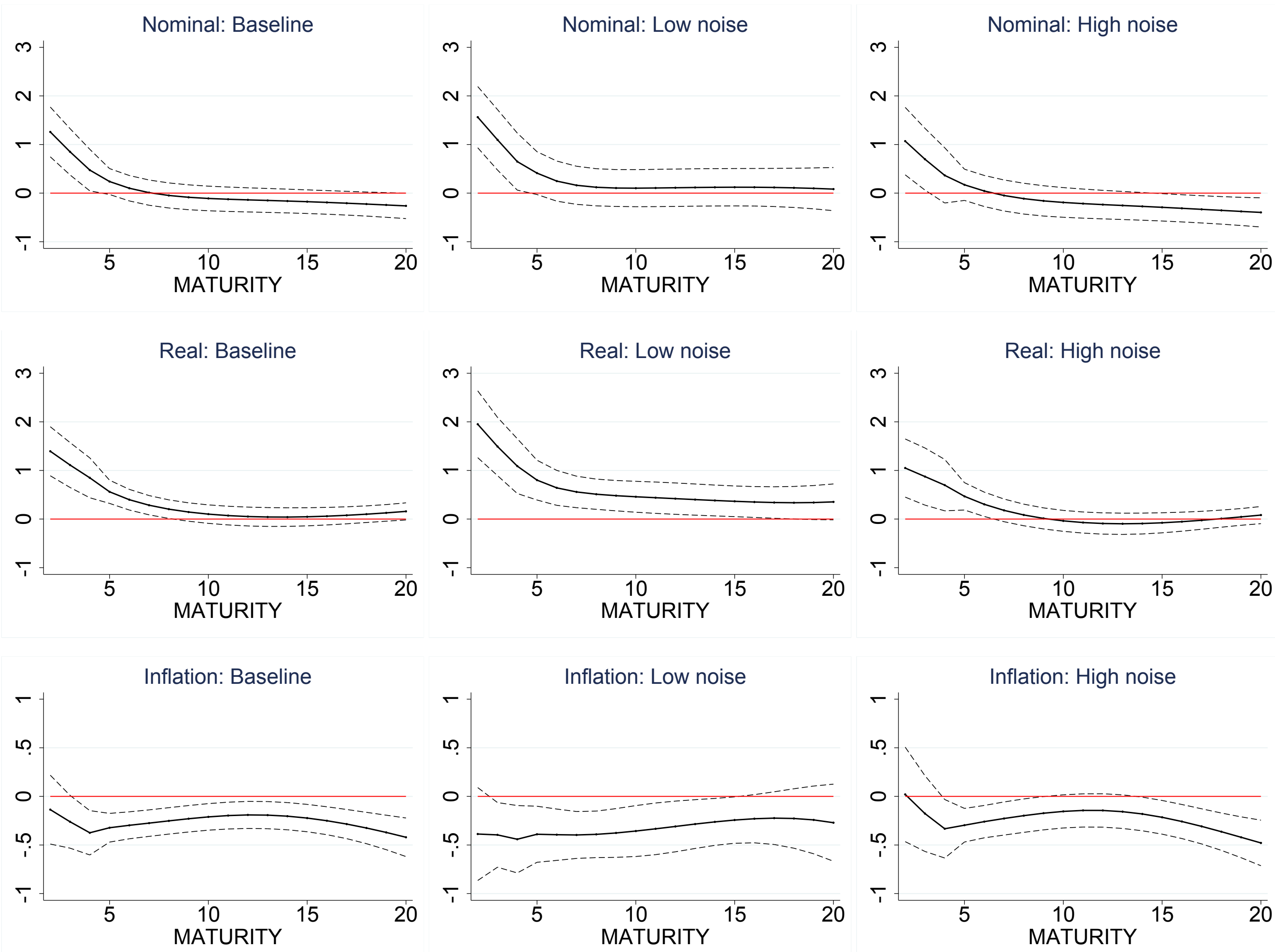
A.4.9 Detailed results for alternative samples and monetary policy shocks

Figure 72: Impact of yield curve noise on the transmission of MP shocks to US forward rates: extended sample



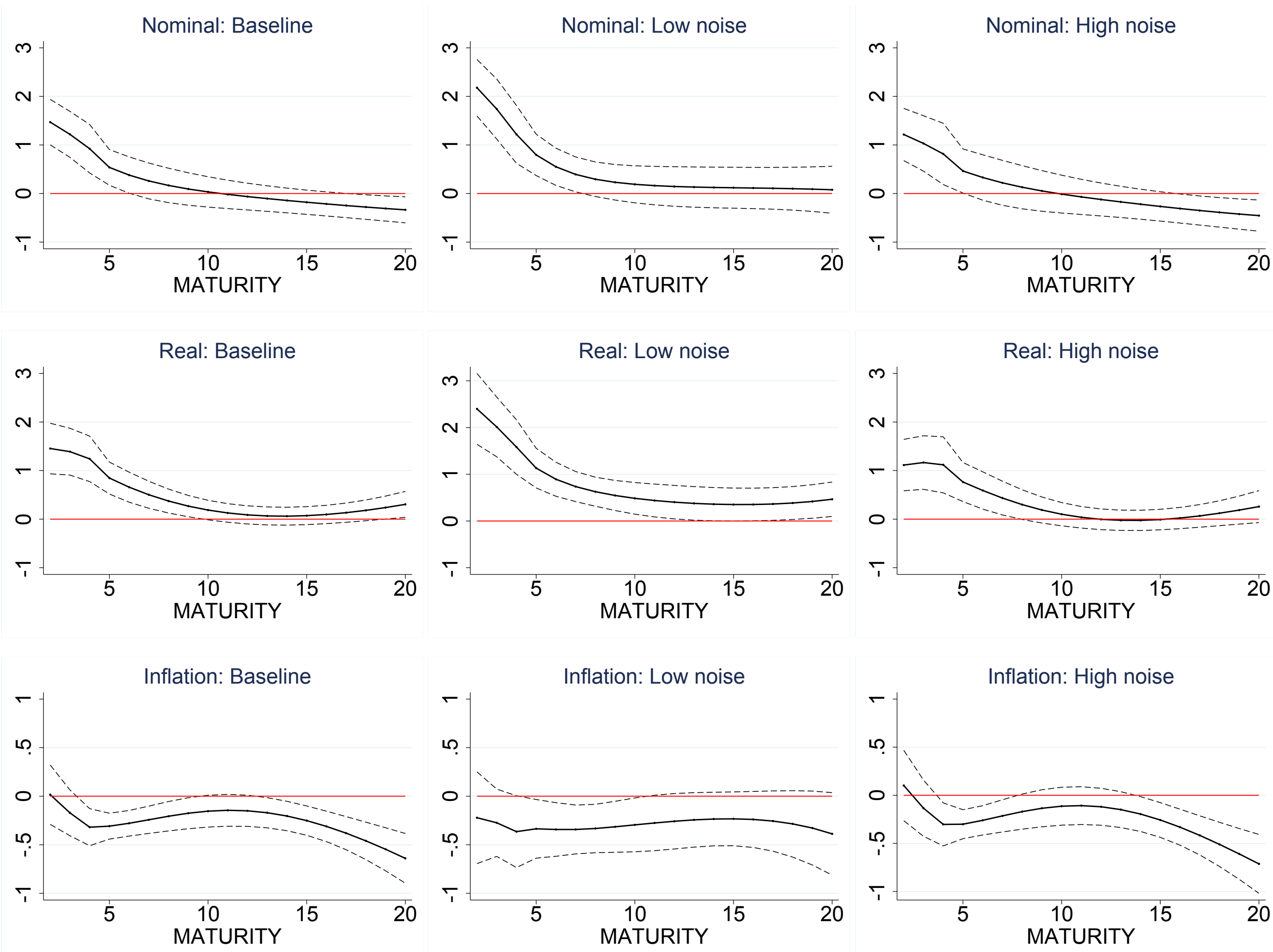
Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 152 observations on which each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 120 observations (starting in 2004).

Figure 73: Impact of yield curve noise on the transmission of MP shocks to US forward rates: extended sample without UMP annoucements



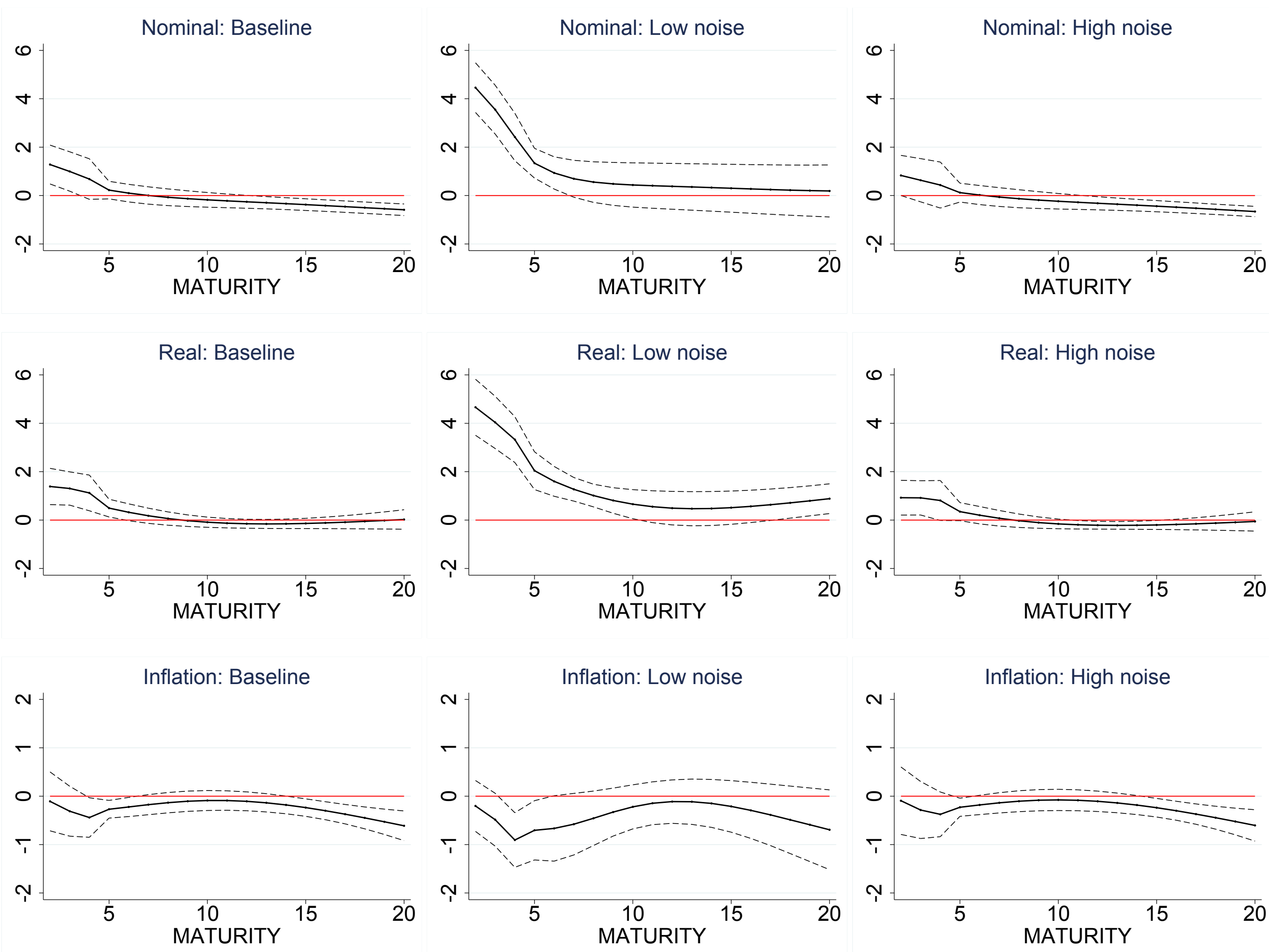
Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those associated with periods of Unconventional Monetary Policy (UMP) announcements as defined in Table 5 of Cieslak and Schrimpf (2019) (except for events with forward guidance). This corresponds to a sample size of 122 observations on which each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 90 observations (starting in 2004).

Figure 74: Impact of yield curve noise on the transmission of MP shocks to US forward rates: all FOMC announcements



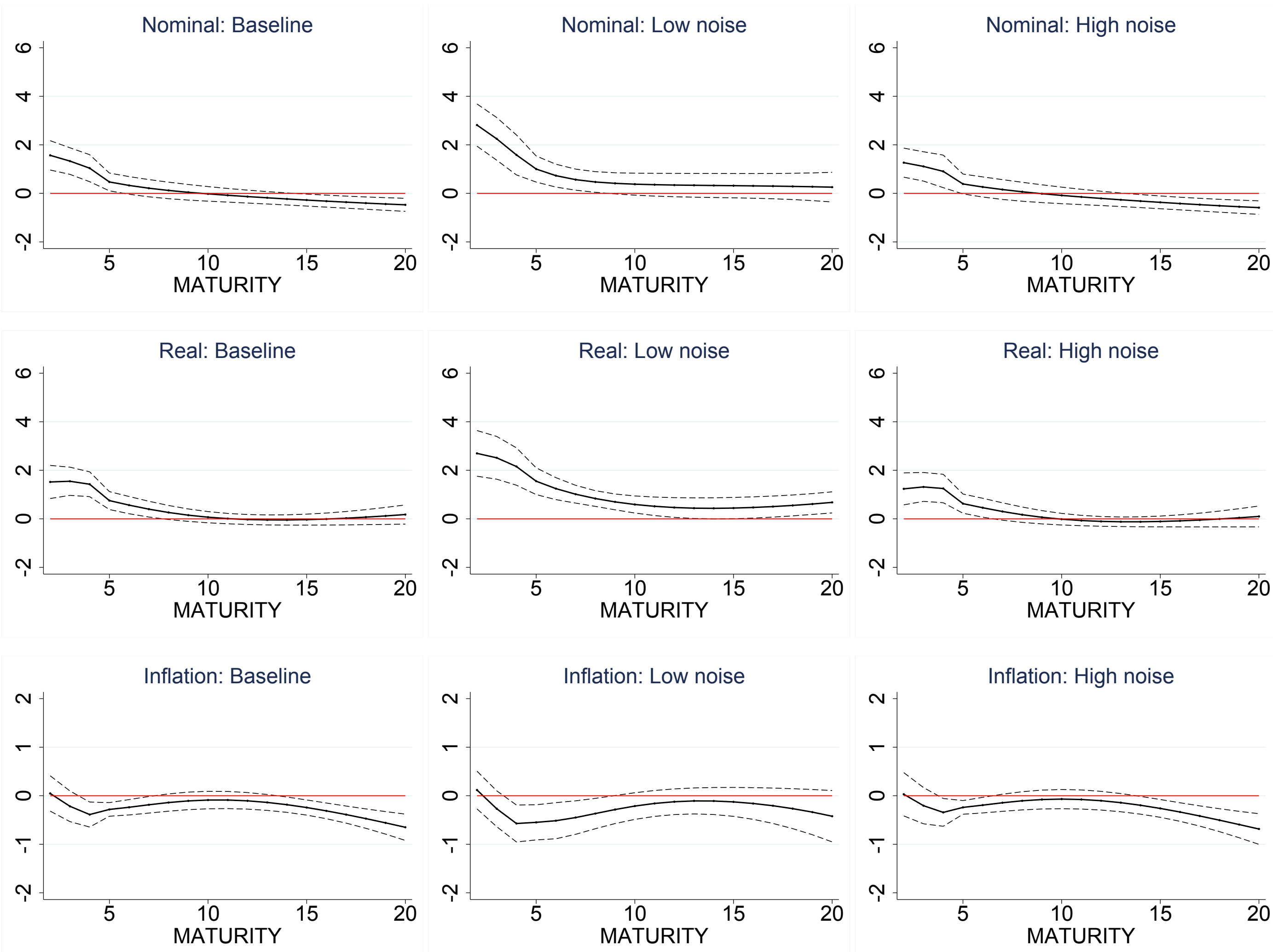
Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, corresponding to a sample size of 160 observations on which each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 128 observations (starting in 2004).

Figure 75: Impact of yield curve noise on the transmission of MP shocks to US forward rates: original sample for Jarocinski and Karadi (2020) shock



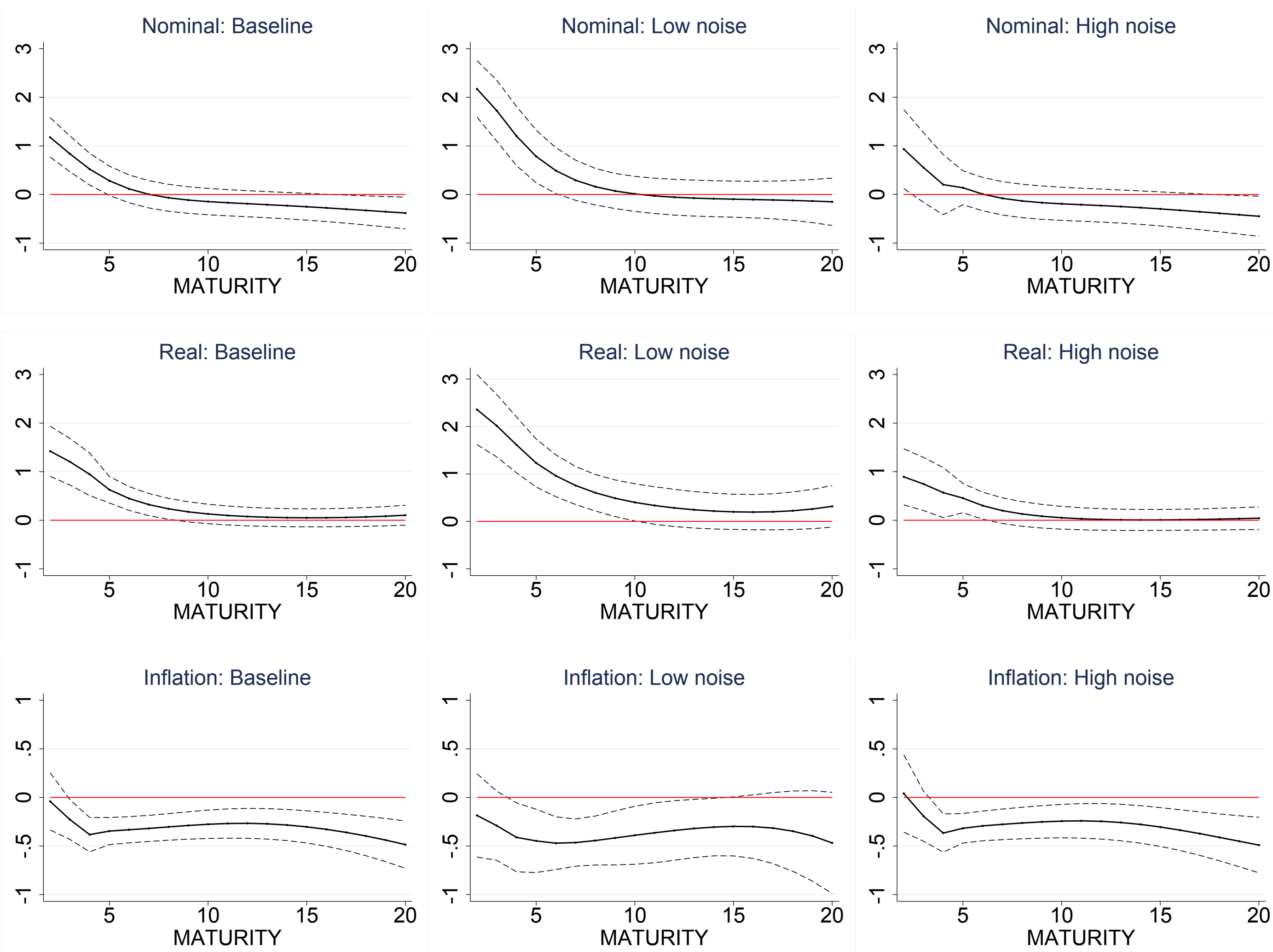
Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. The sample includes all the FOMC announcements between 01/2000 and 12/2016 considered in Jarocinski and Karadi (2020) for which the high-frequency changes in the S&P 500 and interest rates have opposite signs. This corresponds to a sample size of 72 observations on which each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 55 observations (starting in 2004).

Figure 76: Impact of yield curve noise on the transmission of MP shocks to US forward rates: extended sample for Jarocinski and Karadi (2020) shock



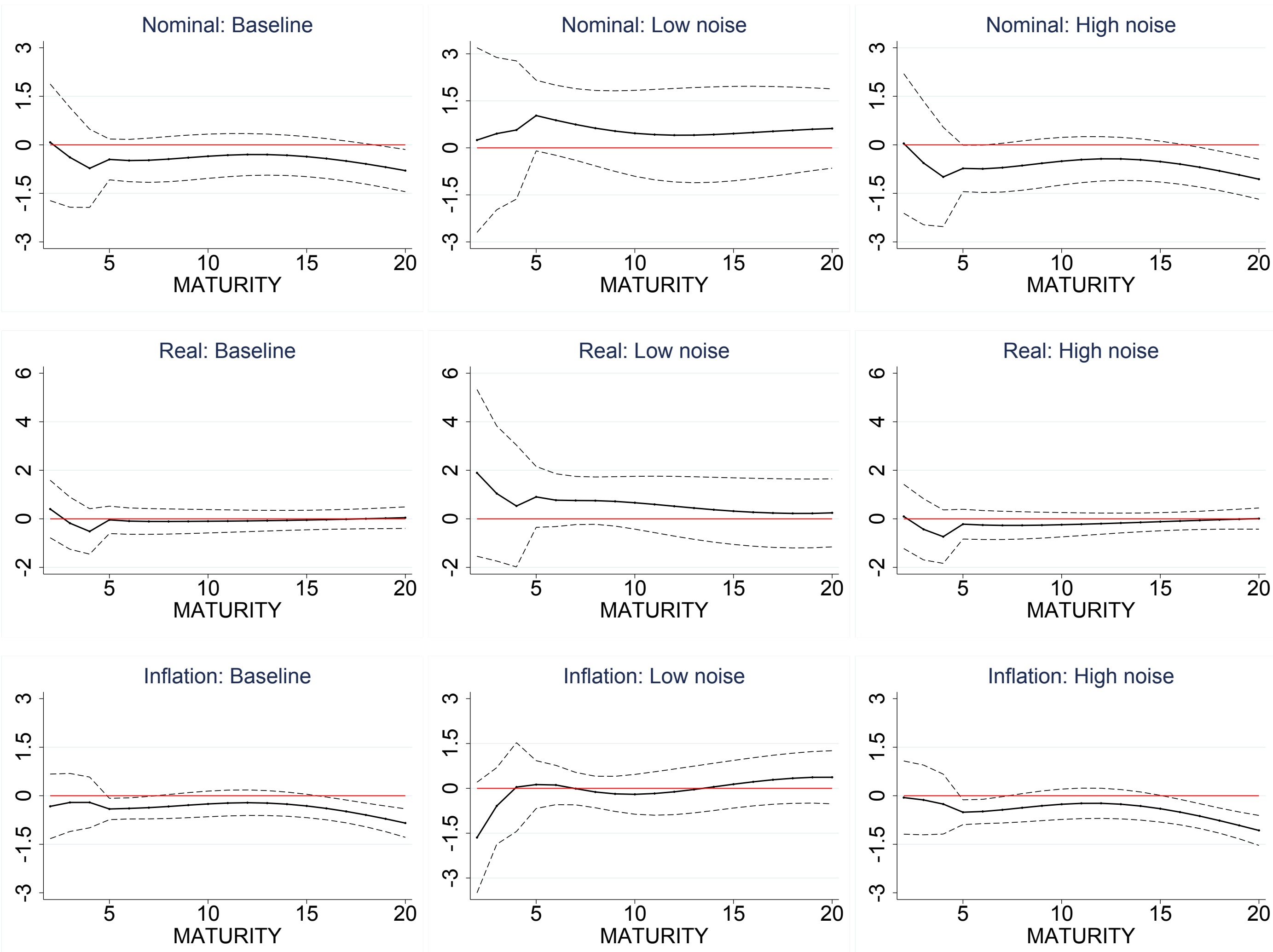
Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. The sample includes all the FOMC announcements between 01/2000 and 06/2019 considered in Jarocinski and Karadi (2020) for which the high-frequency changes in the S&P 500 and interest rates have opposite signs. This corresponds to a sample size of 108 observations on which each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 90 observations (starting in 2004).

Figure 77: Impact of yield curve noise on the transmission of MP shocks to US forward rates: extended sample for Bauer and Swanson (2023b) shock



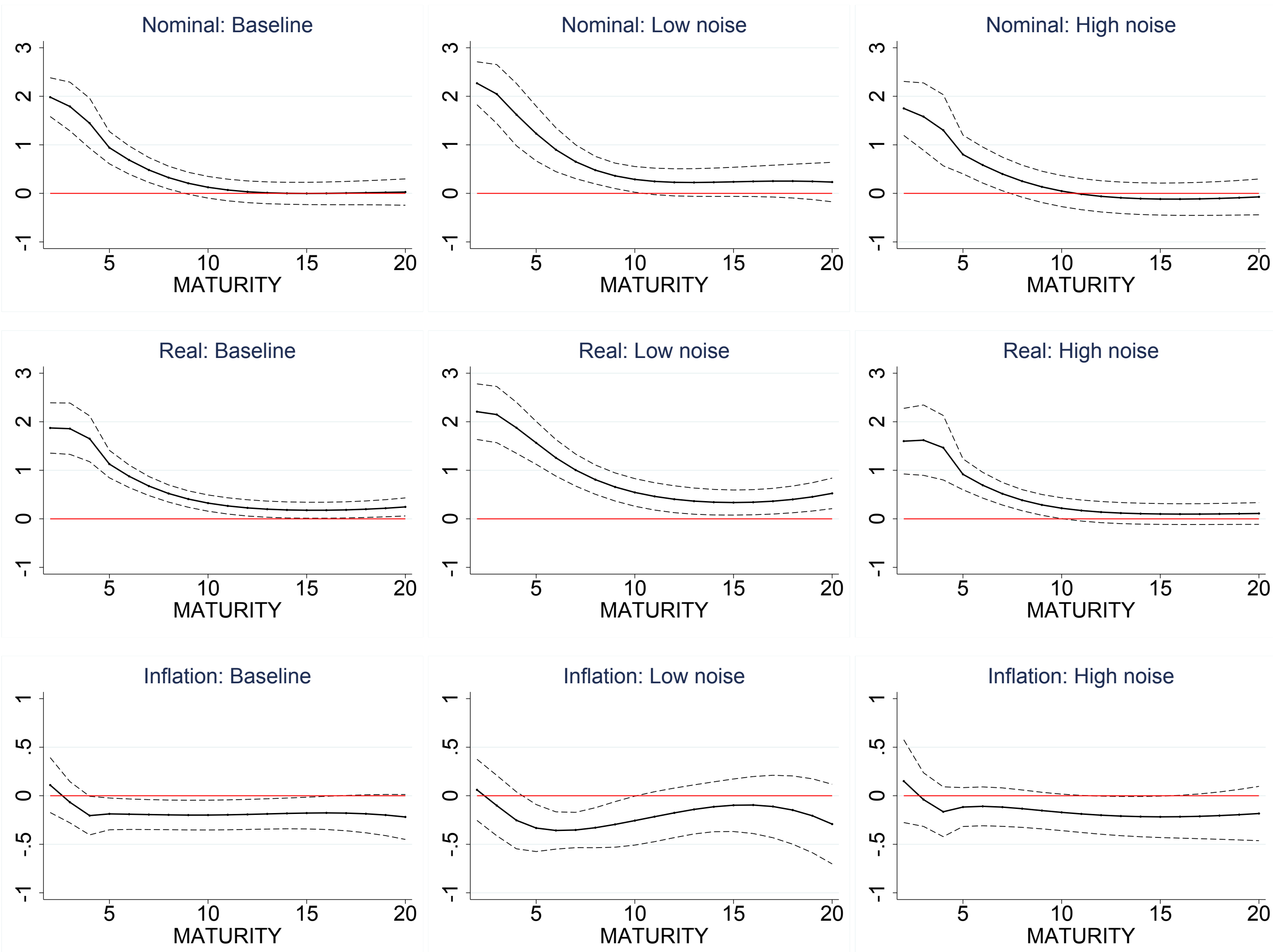
Note: The first column (Baseline) plots estimates of $\gamma^{i}_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^{i}_{hl,\tau}$ and $\gamma^{i}_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 152 observations on which each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 120 observations (starting in 2004).

Figure 78: Impact of yield curve noise on the transmission of MP shocks to US forward rates: extended sample for Swanson (2021) FFR shock



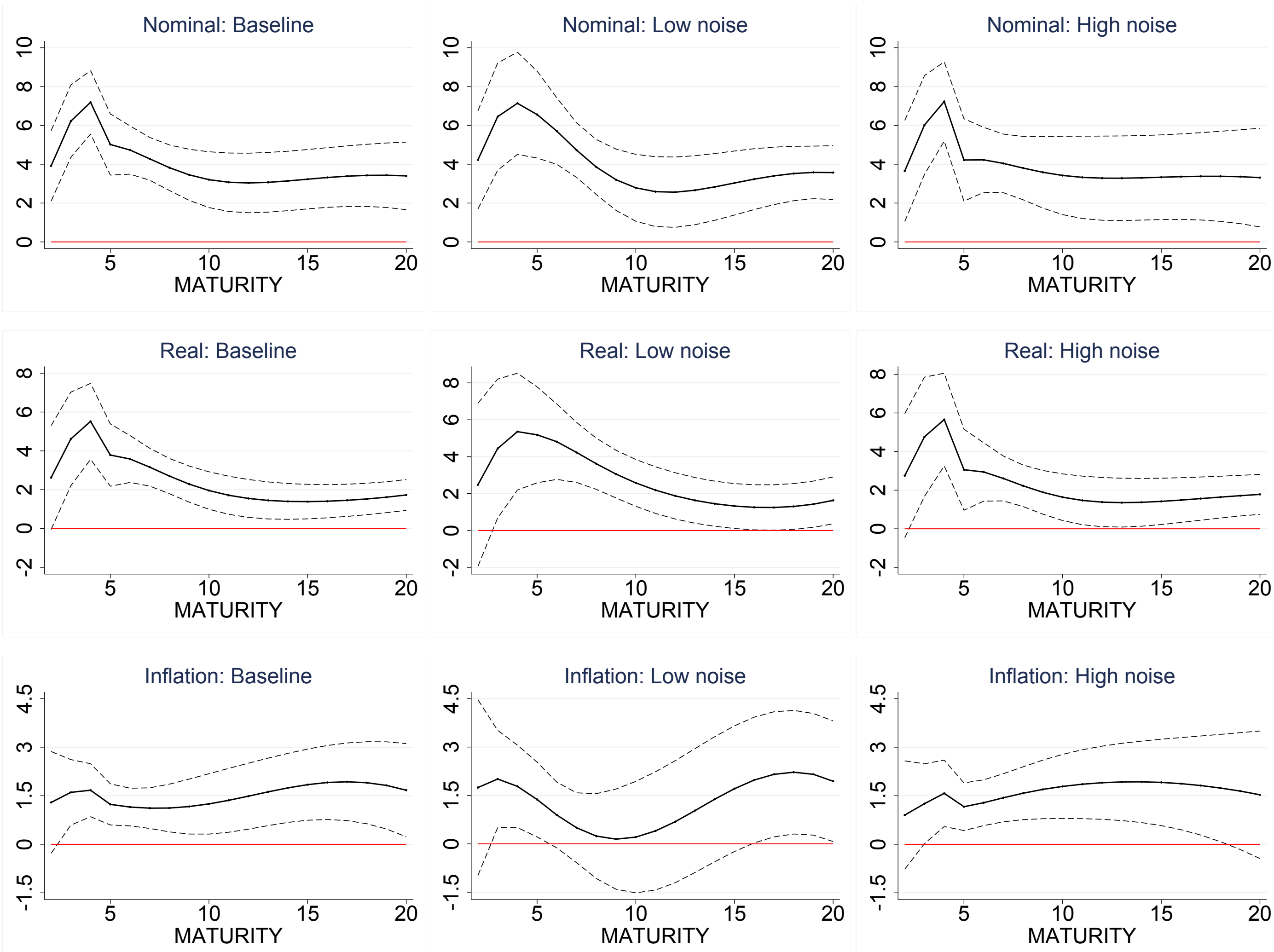
Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 148 observations on which each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 116 observations (starting in 2004).

Figure 79: Impact of yield curve noise on the transmission of MP shocks to US forward rates: extended sample for Swanson (2021) FG shock



Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 148 observations on which each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 116 observations (starting in 2004).

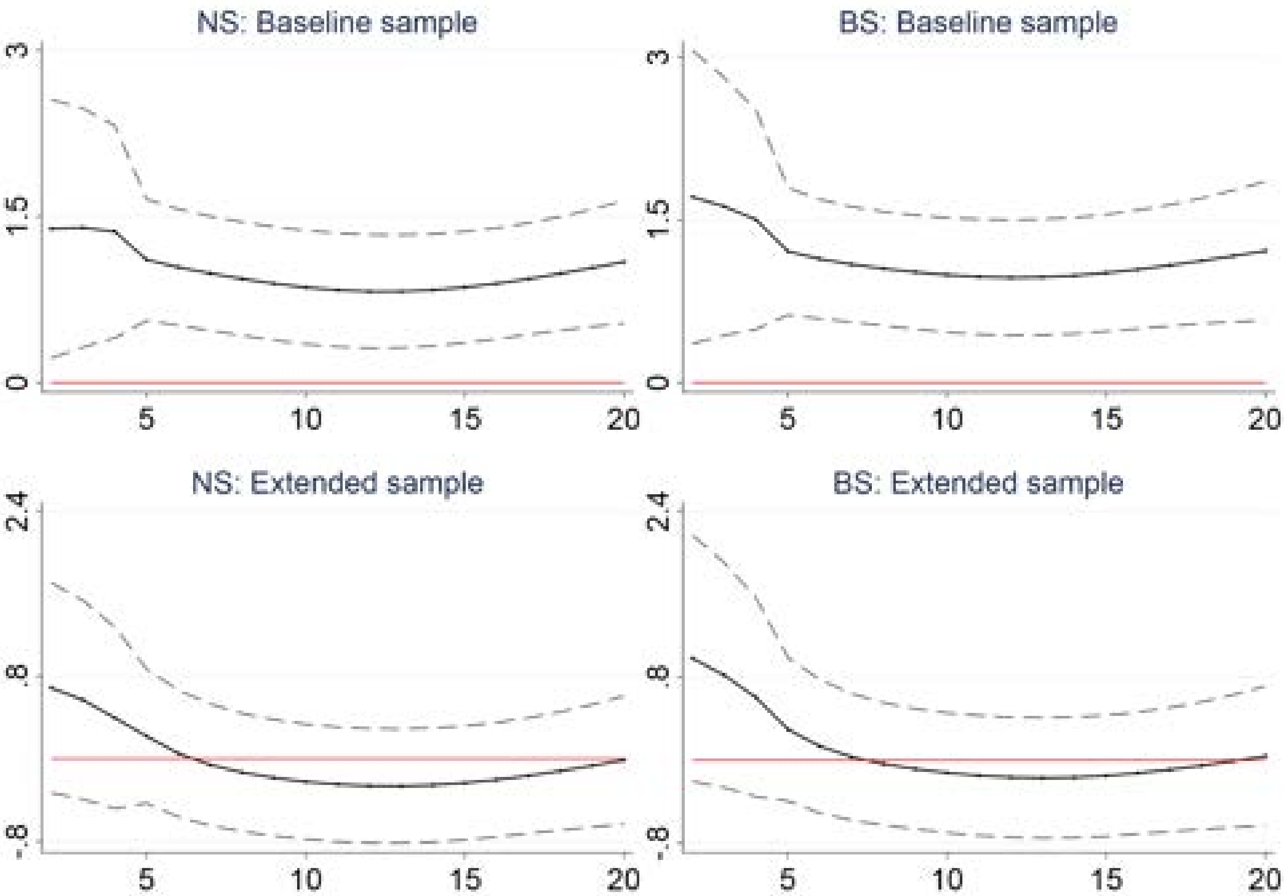
Figure 80: Impact of yield curve noise on the transmission of MP shocks to US forward rates: extended sample for Swanson (2021) LSAP shock



Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 148 observations on which each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 116 observations (starting in 2004).

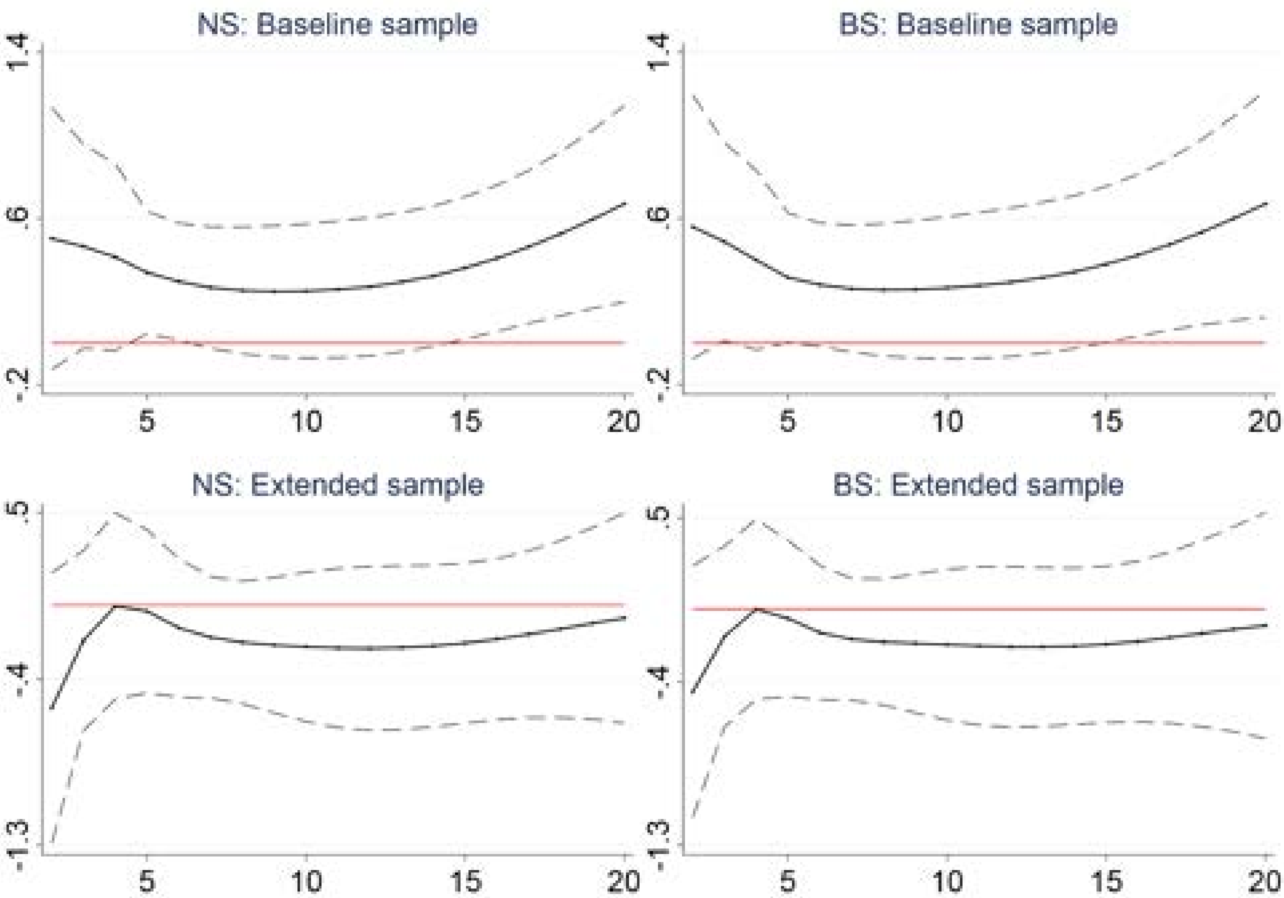
A.4.10 Summary of robustness checks for the information effect and response to news

Figure 81: Robustness of state-dependence for nominal forward rates to orthogonalized monetary policy shocks



Note: The charts plot the estimates of $\gamma^r_{h-l,\tau}$ in regression (3) for each nominal forward rate with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The first row reports the results for the orthogonalized shocks of Nakamura and Steinsson (2018) and Bauer and Swanson (2023b), respectively denoted by NS and BS, in the baseline sample from 2000/01 to 2014/03. The second row reports the same results for the extended sample from 2000/01 to 2019/12. We exclude FOMC meetings taking place between July 2008 and June 2009 from the estimation. Regression results for the 2, 3 and 4-year forward rates are based on samples starting in 2004/01. Details on the approach used to obtain the orthogonalized shock series can be found in Appendix A.2.

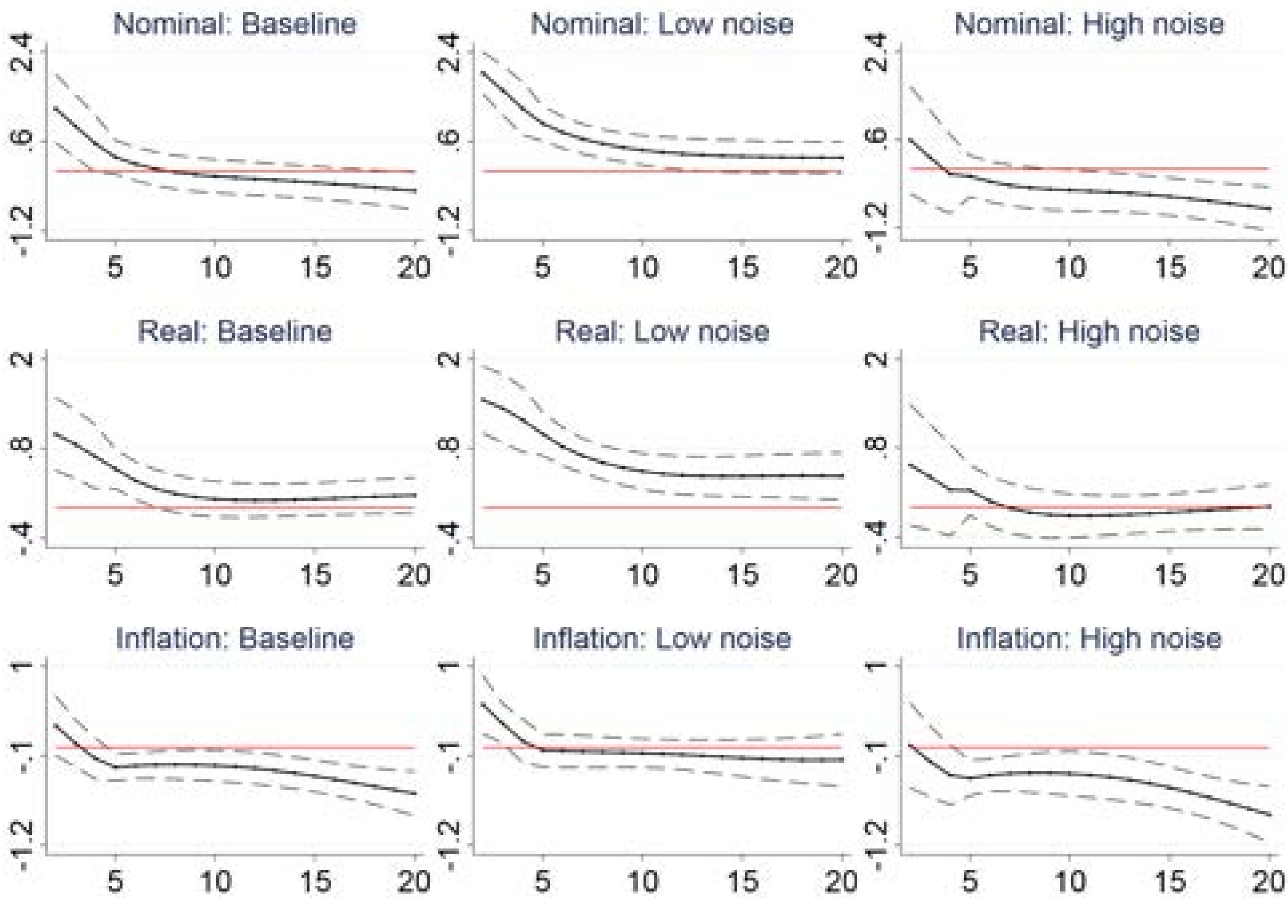
Figure 82: Robustness of state-dependence for inflation forward rates to orthogonalized monetary policy shocks



Note: The charts plot the estimates of $\gamma^{r}_{h-l,\tau}$ in regression (3) for each inflation forward rate with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The first row reports the results for the orthogonalized shocks of Nakamura and Steinsson (2018) and Bauer and Swanson (2023b), respectively denoted by NS and BS, in the baseline sample from 2000/01 to 2014/03. The second row reports the same results for the extended sample from 2000/01 to 2019/12. We exclude FOMC meetings taking place between July 2008 and June 2009 from the estimation. Regression results for the 2, 3 and 4-year forward rates are based on samples starting in 2004/01. Details on the approach used to obtain the orthogonalized shock series can be found in Appendix A.2.

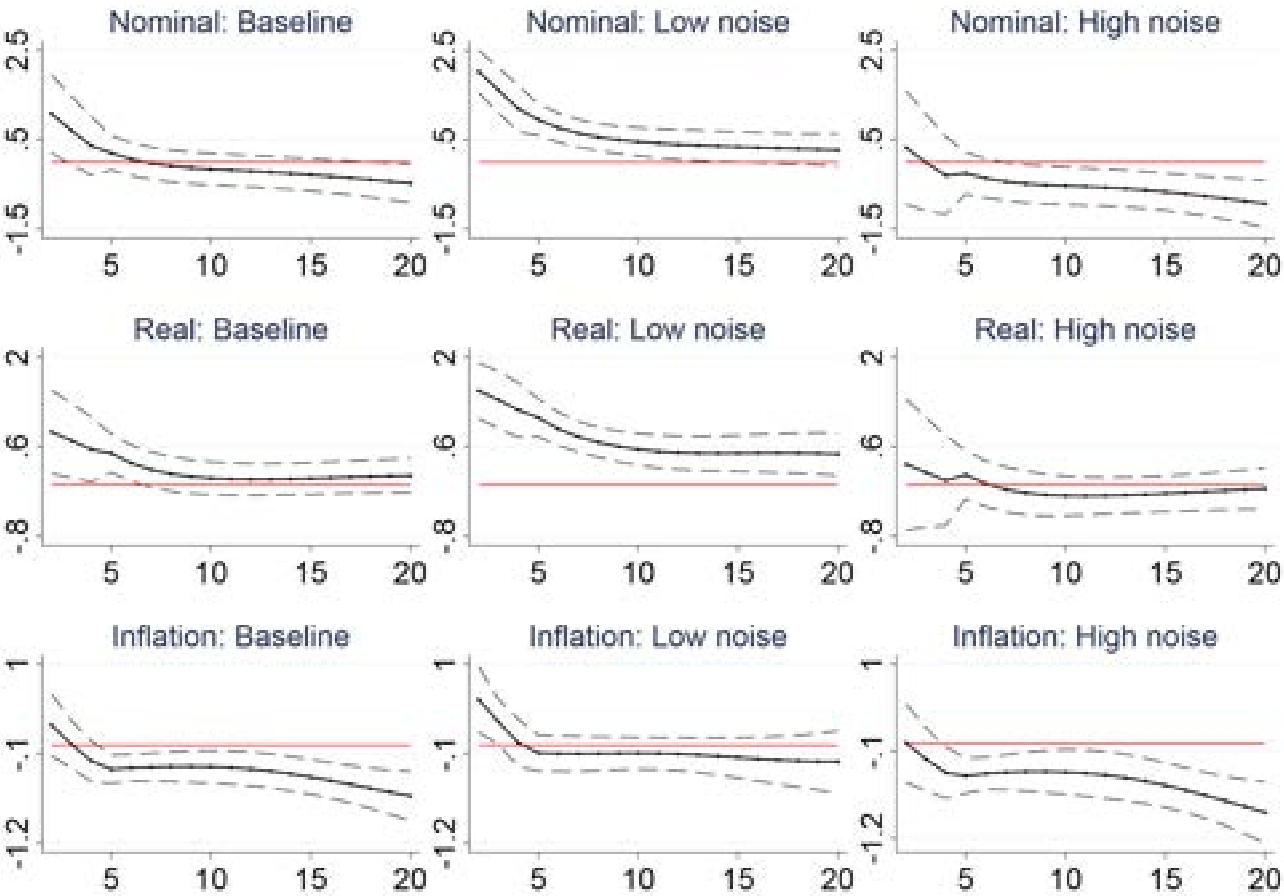
A.4.11 Detailed results for the information effect and response to news

Figure 83: Impact of yield curve noise on the transmission of MP shocks to US forward rates: baseline sample for orthogonalized Nakamura and Steinsson (2018) shock



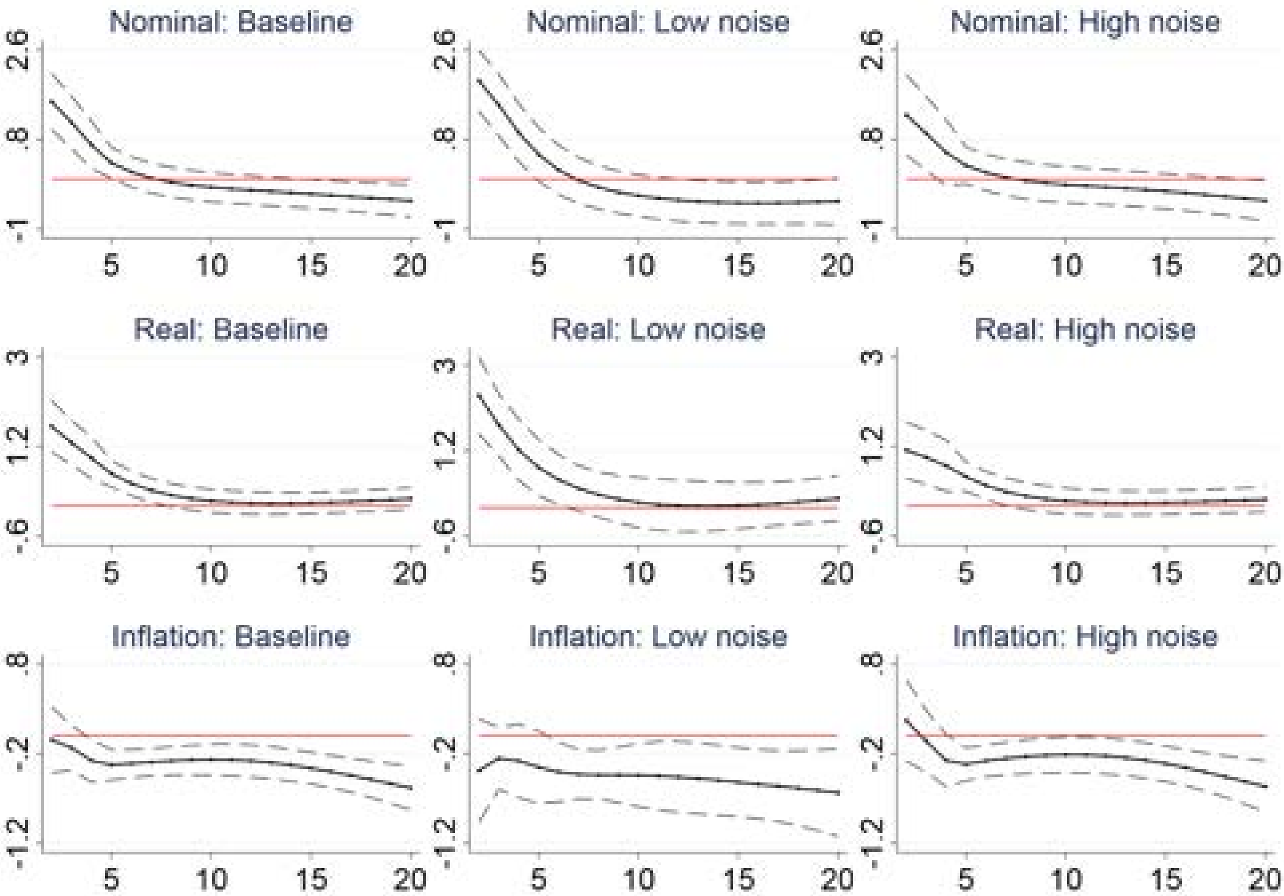
Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/01/2000 to 19/03/2014, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 106 observations on which the policy news shock is computed and each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 74 observations (starting in 2004). The procedure to obtain the orthogonalized shocks follows Bauer and Swanson (2023b) and is outlined in Appendix A.2.

Figure 84: Impact of yield curve noise on the transmission of MP shocks to US forward rates: baseline sample for orthogonalized Bauer and Swanson (2023b) shock



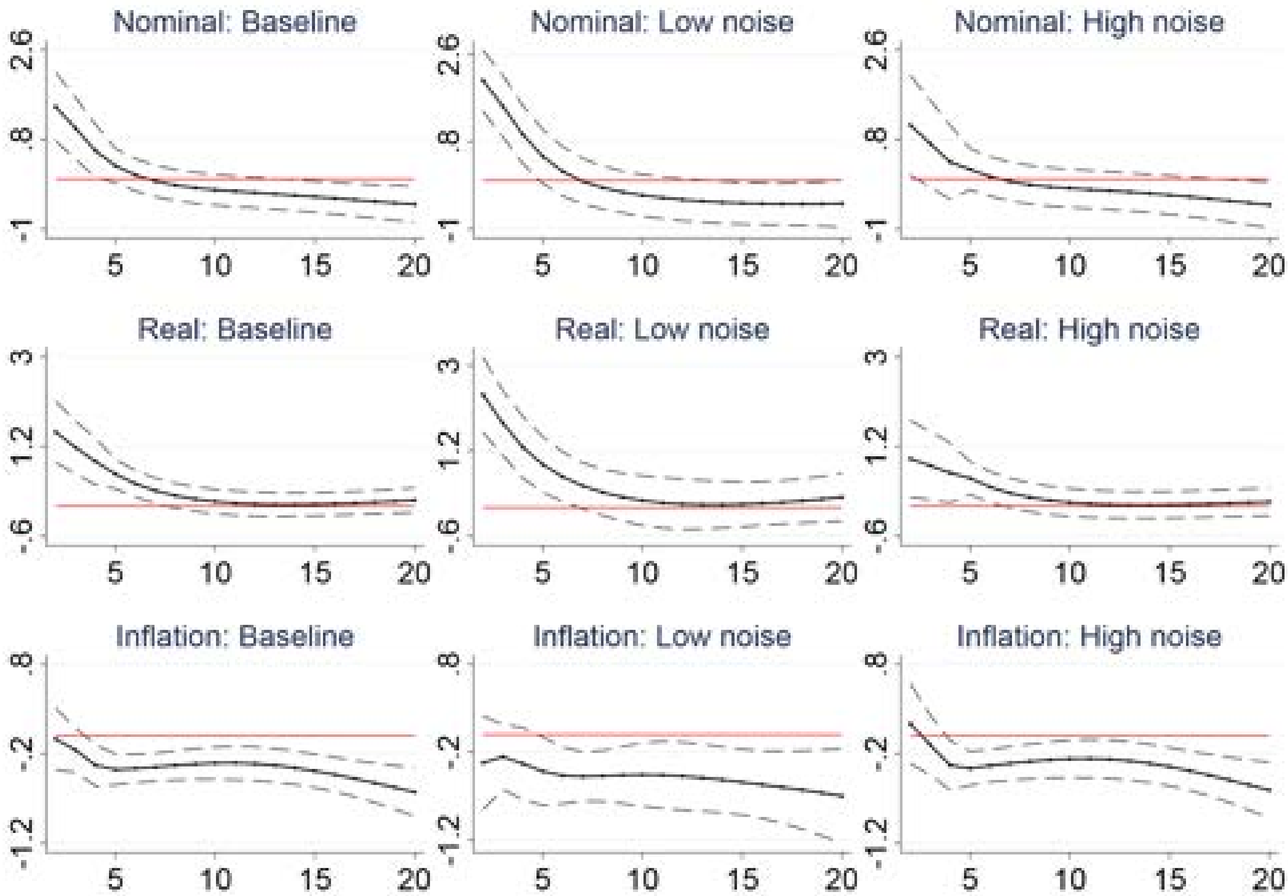
Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/01/2000 to 19/03/2014, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 106 observations on which each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 74 observations (starting in 2004). The procedure to obtain the orthogonalized shocks follows Bauer and Swanson (2023b) and is outlined in Appendix A.2.

Figure 85: Impact of yield curve noise on the transmission of MP shocks to US forward rates: extended sample for orthogonalized Nakamura and Steinsson (2018) shock



Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 152 observations on which each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 120 observations (starting in 2004). The procedure to obtain the orthogonalized shocks follows Bauer and Swanson (2023b) and is outlined in Appendix A.2.

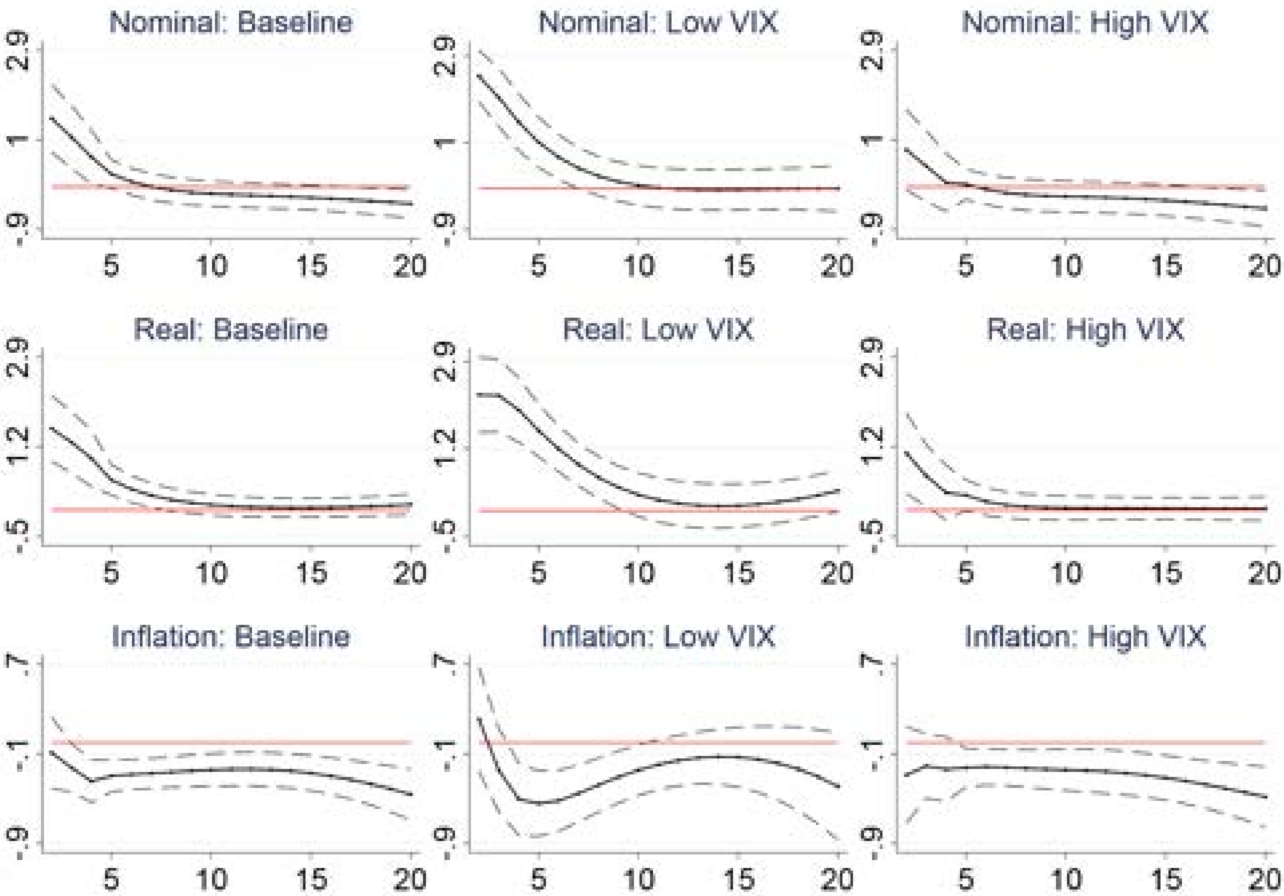
Figure 86: Impact of yield curve noise on the transmission of MP shocks to US forward rates: extended sample for orthogonalized Bauer and Swanson (2023b) shock



Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the yield curve noise measure is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 152 observations on which each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 120 observations (starting in 2004). The procedure to obtain the orthogonalized shocks follows Bauer and Swanson (2023b) and is outlined in Appendix A.2.

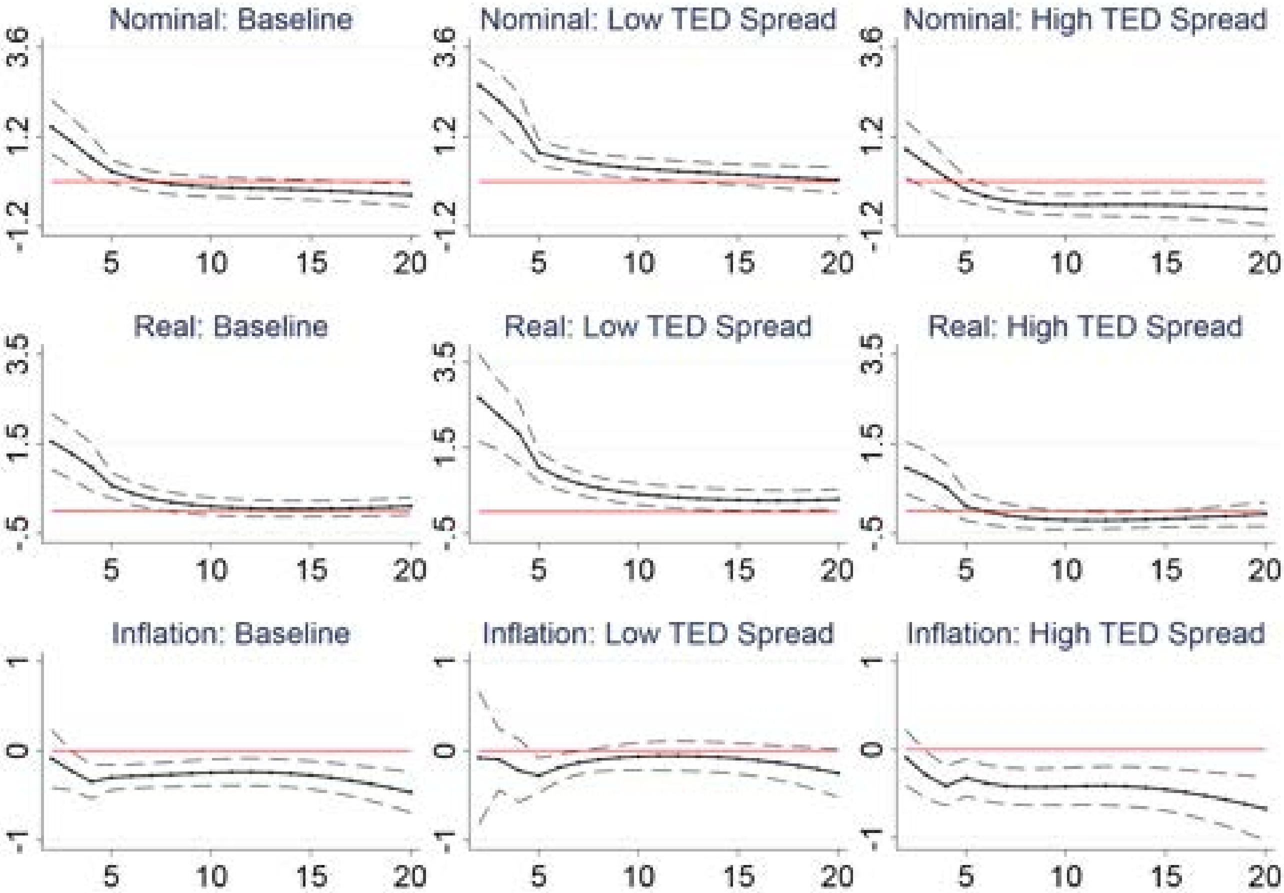
A.4.12 Other measures of market stress

Figure 87: Impact of volatility on the transmission of MP shocks to US forward rates: extended sample



Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (Low VIX) and third (High VIX) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the CBOE VIX index is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 152 observations on which each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 120 observations (starting in 2004).

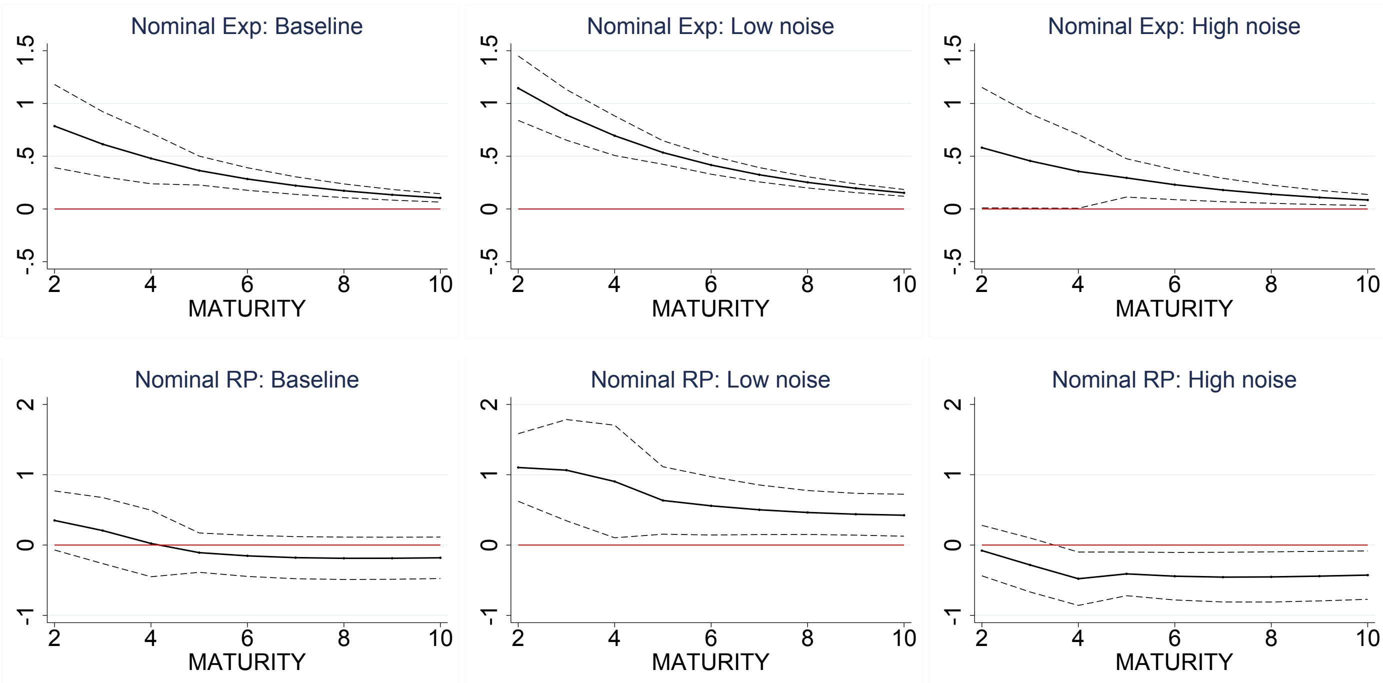
Figure 88: Impact of the TED spread on the transmission of MP shocks to US forward rates: extended sample



Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for each forward rate $i \in n, r, \pi$ (in each row) with maturity $\tau \in (2, 20)$, together with 90% confidence intervals based on robust standard errors. The second (Low TED Spread) and third (High TED Spread) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the T-Bill Eurodollar (TED) spread is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 152 observations on which each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 120 observations (starting in 2004).

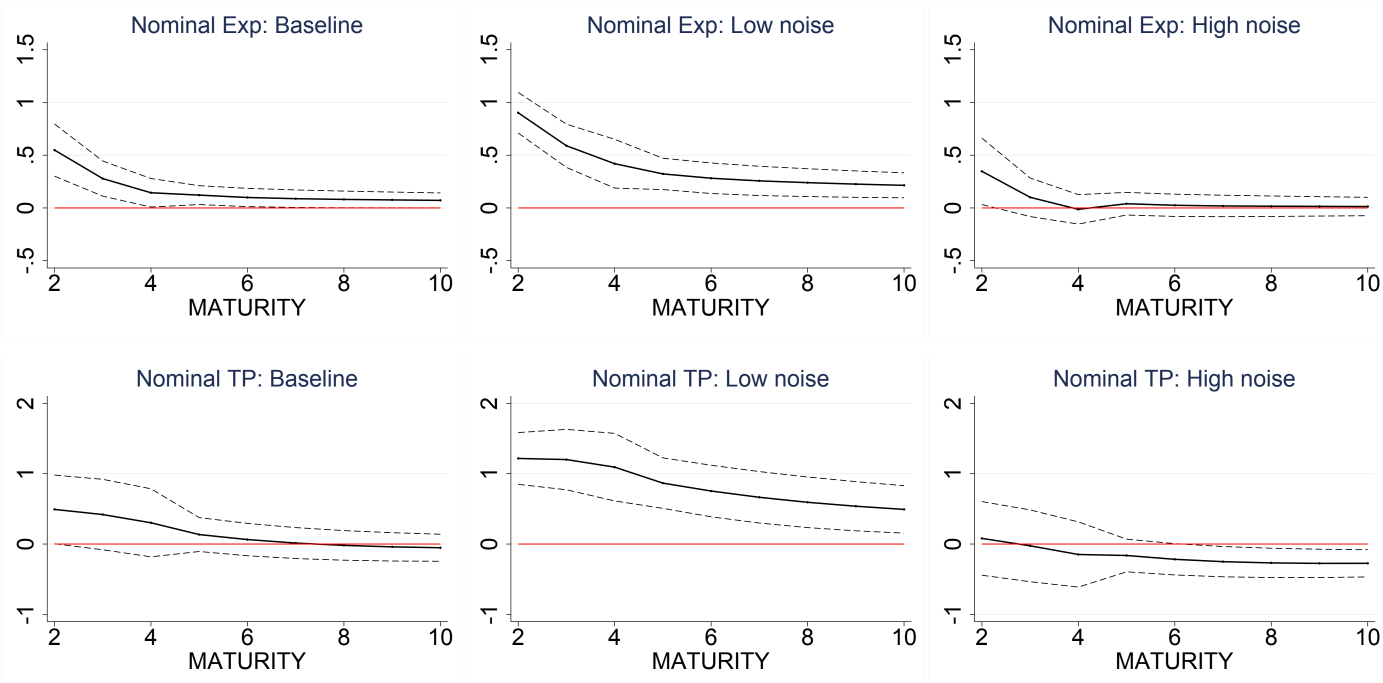
A.4.13 Role of Risk Premium vs Expectations

Figure 89: Decomposition of US nominal forward rates into expected and risk premia components



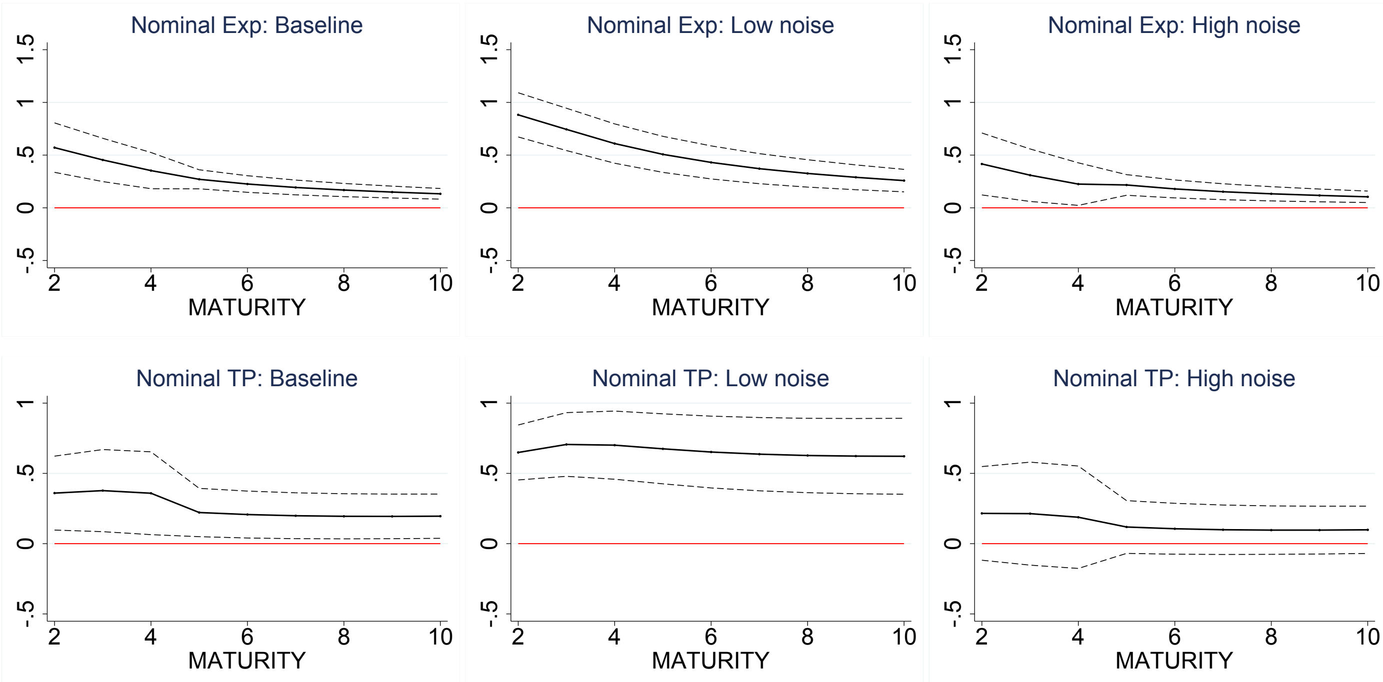
Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for the expected average future nominal forward rate (first row) and nominal risk premium (second row) at maturities $\tau \in (2, 10)$, together with 90% confidence intervals based on robust standard errors. The measures are based on the decomposition introduced in Abrahams, Adrian, Crump, Moench, and Yu (2016) and we follow Nakamura and Steinsson (2018) by grouping the term premium, liquidity premium and model error into a single risk premium component. The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the noise measure is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/01/2000 to 19/03/2014, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 106 observations on which the policy news shock is computed and each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 74 observations (starting in 2004).

Figure 90: Decomposition of US nominal forward rates into expected and term premia components



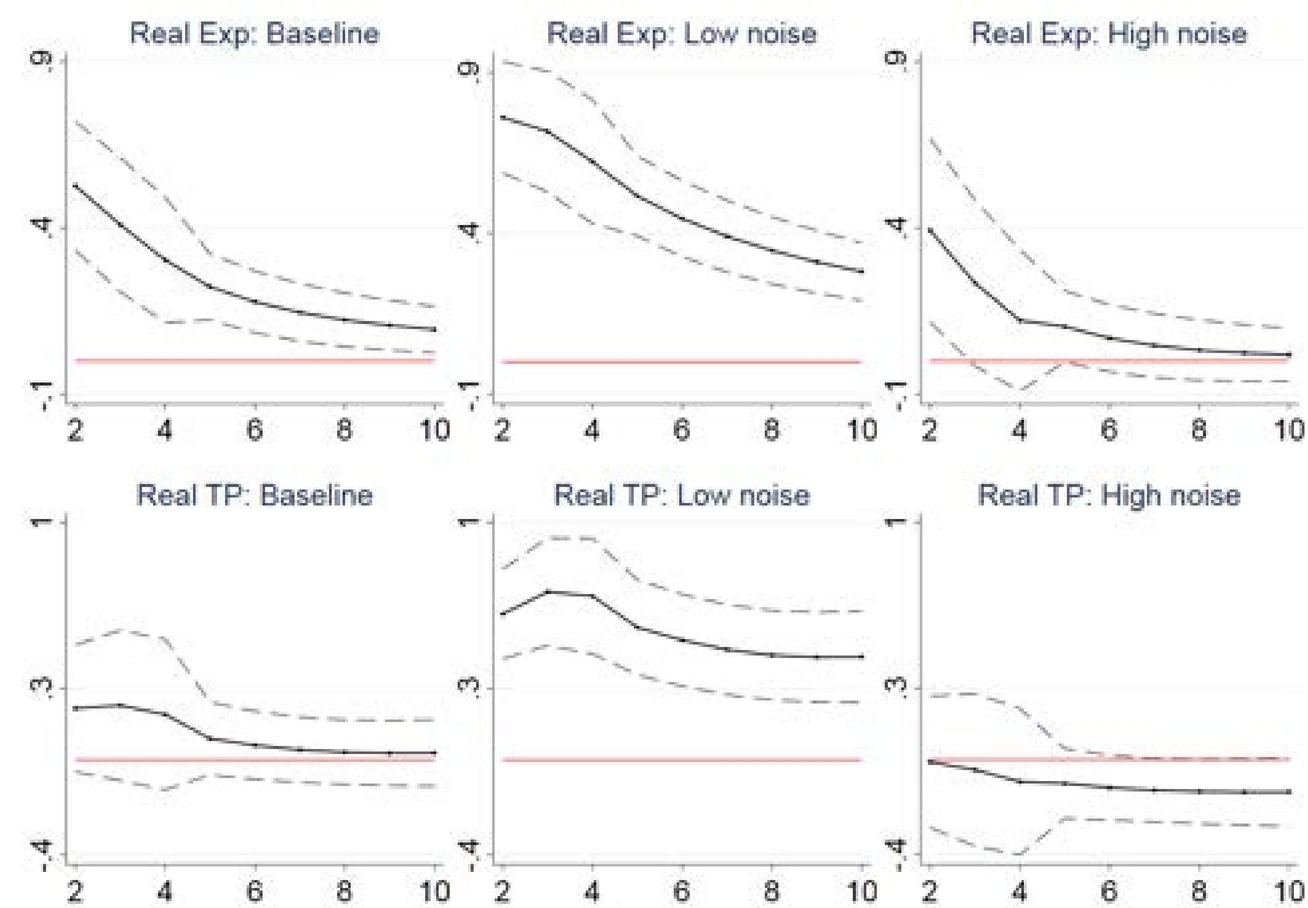
Note: The first column (Baseline) plots estimates of $\gamma^{i}_{all,\tau}$ in regression (1) for the expected average future nominal forward rate (first row) and nominal term premium (second row) at maturities $\tau \in (2, 10)$, together with 90% confidence intervals based on robust standard errors. The measures are based on the decomposition introduced in Kim and Wright (2005). The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^{i}_{hl,\tau}$ and $\gamma^{i}_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the noise measure is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/01/2000 to 19/03/2014, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 106 observations on which the policy news shock is computed and each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 74 observations (starting in 2004).

Figure 91: Decomposition of US nominal forward rates into expected and term premia components: extended sample



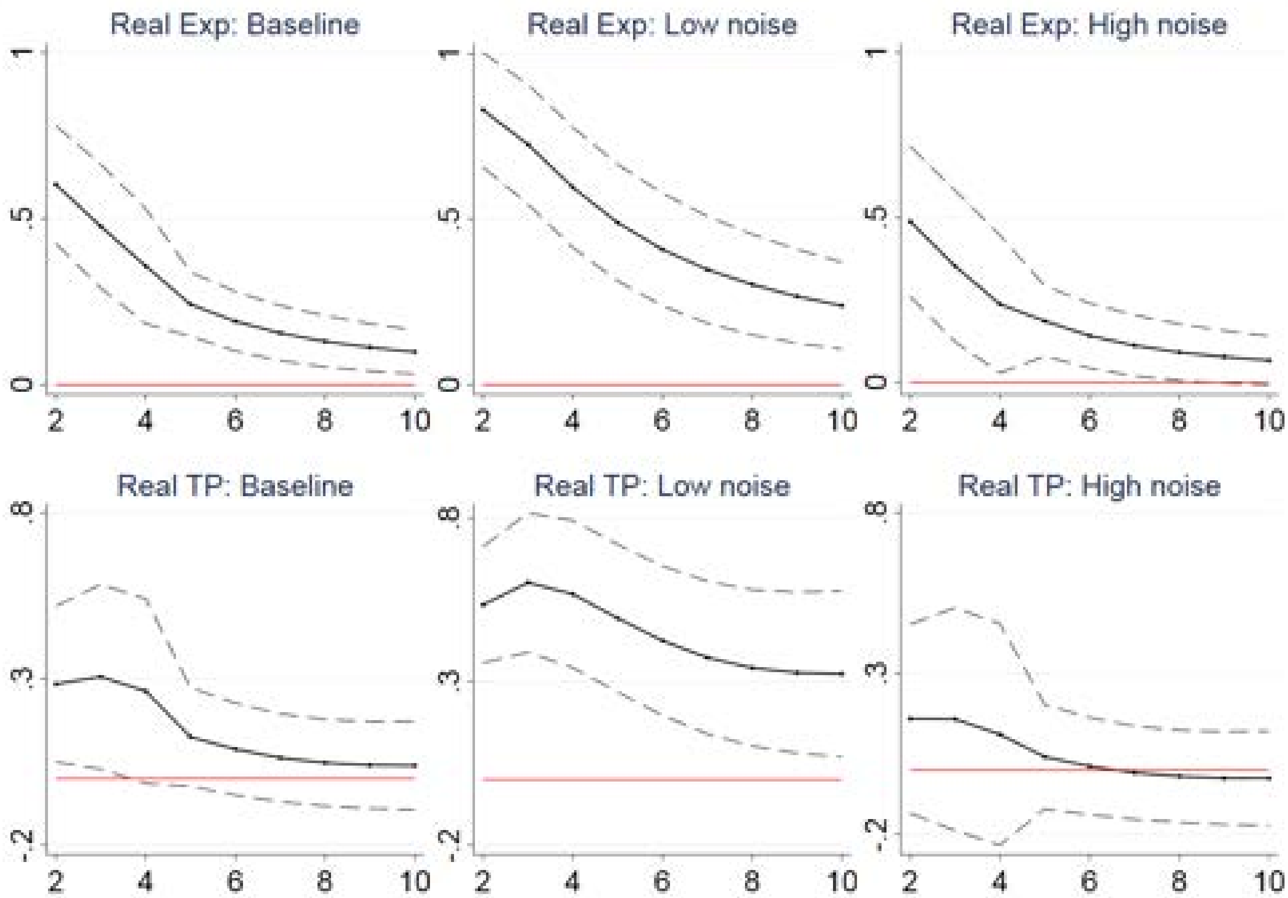
Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for the expected average future nominal forward rate (first row) and nominal term premium (second row) at maturities $\tau \in (2, 10)$, together with 90% confidence intervals based on robust standard errors. The measures are based on the decomposition introduced in Kim and Wright (2005). The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the noise measure is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 152 observations on which each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 120 observations (starting in 2004).

Figure 92: Decomposition of US forward rates into real expected and term premia components



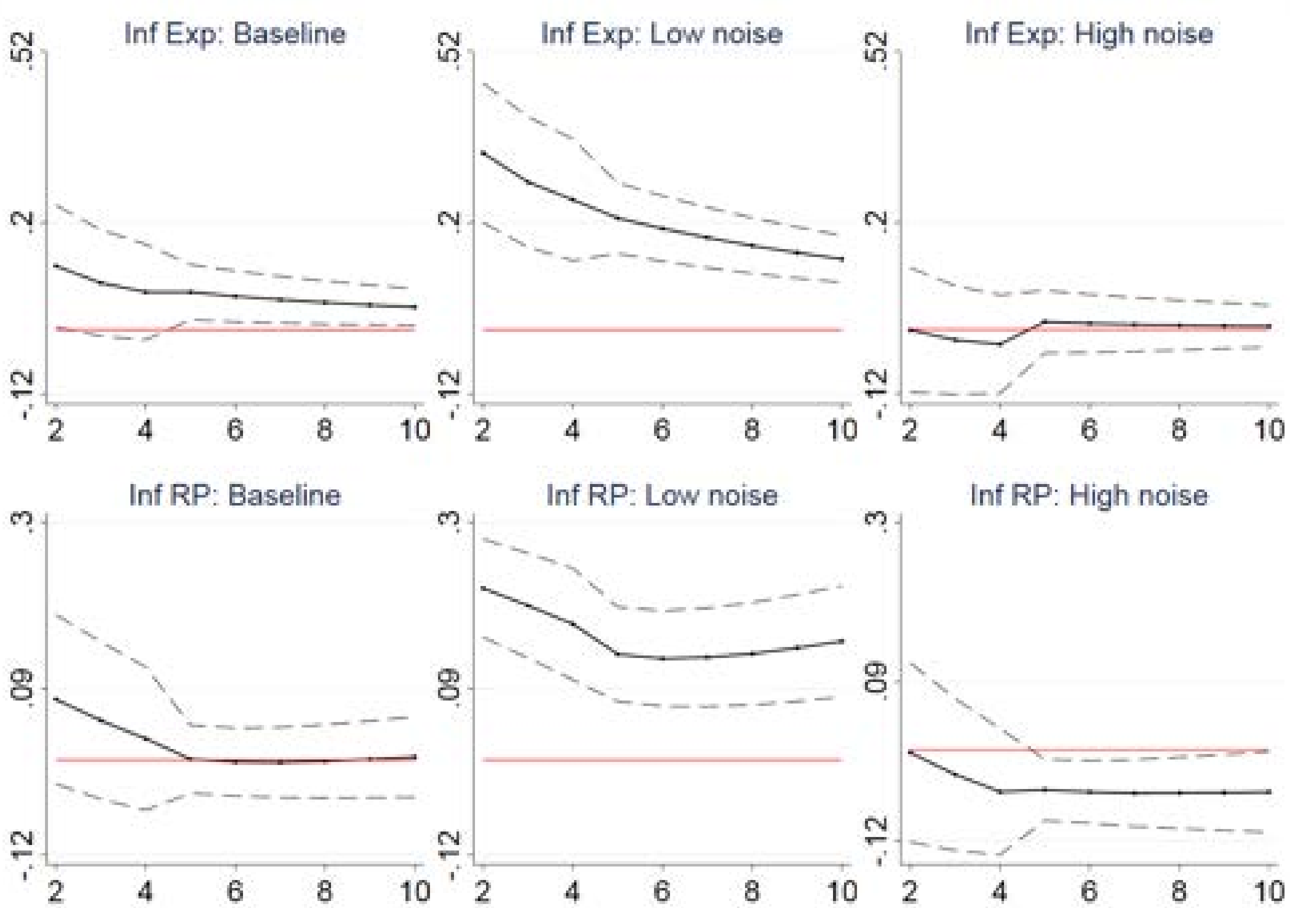
Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for the expected average future real forward rate (first row) and real term premium (second row) at maturities $\tau \in (2, 10)$, together with 90% confidence intervals based on robust standard errors. The measures are based on the decomposition introduced in D'Amico, Kim, and Wei (2018). The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the noise measure is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/01/2000 to 19/03/2014, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 106 observations on which the policy news shock is computed and each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 74 observations (starting in 2004).

Figure 93: Decomposition of US forward rates into real expected and term premia components: extended sample



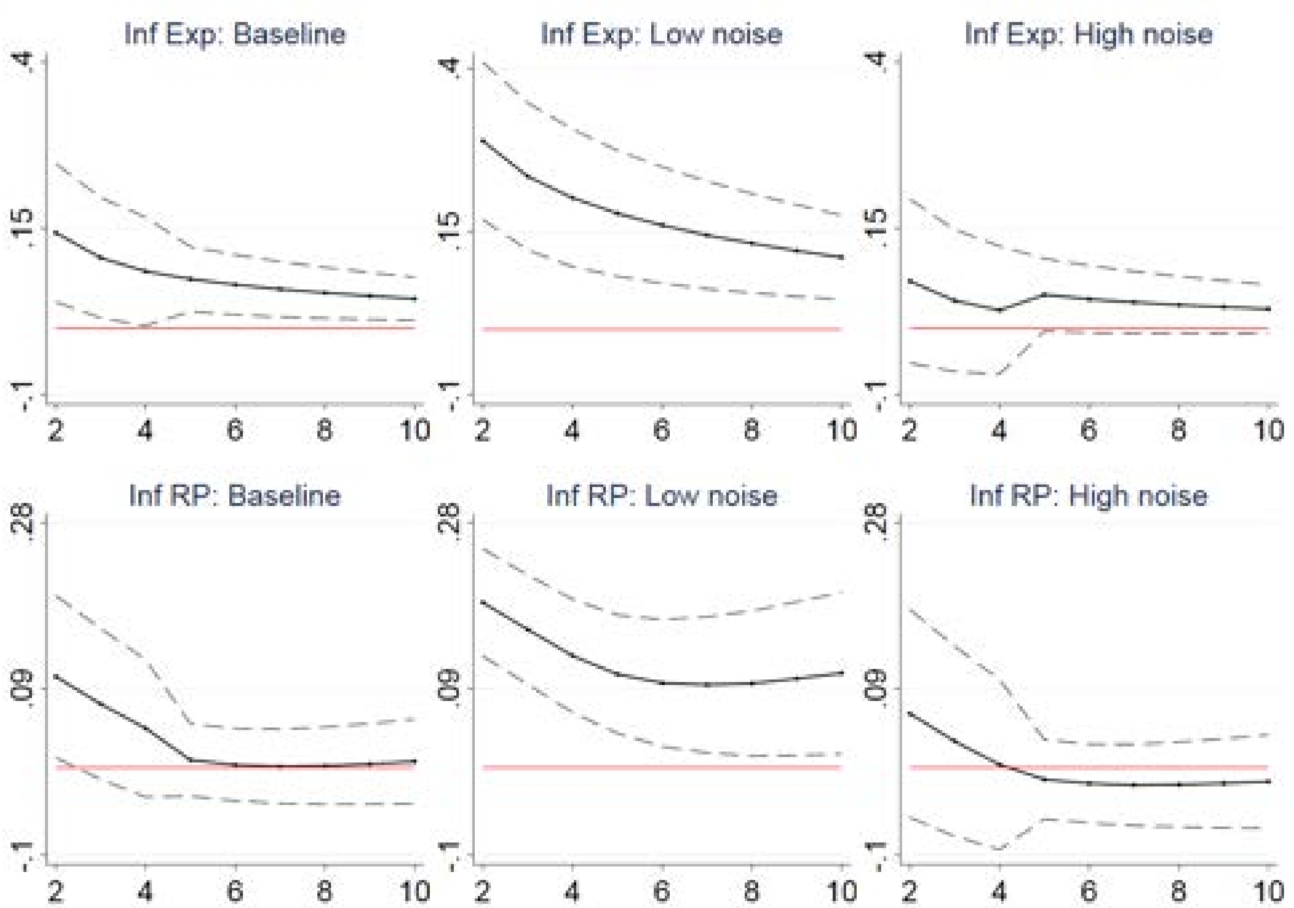
Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for the expected average future real forward rate (first row) and real term premium (second row) at maturities $\tau \in (2, 10)$, together with 90% confidence intervals based on robust standard errors. The measures are based on the decomposition introduced in D'Amico, Kim, and Wei (2018). The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the noise measure is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 152 observations on which each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 120 observations (starting in 2004).

Figure 94: Decomposition of US forward rates into expected inflation and risk premia components



Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for the expected average future forward inflation (first row) and inflation risk premium (second row) at maturities $\tau \in (2, 10)$, together with 90% confidence intervals based on robust standard errors. The measures are based on the decomposition introduced in D'Amico, Kim, and Wei (2018). The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the noise measure is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/01/2000 to 19/03/2014, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 106 observations on which the policy news shock is computed and each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 74 observations (starting in 2004).

Figure 95: Decomposition of US forward rates into expected inflation and risk premia components: extended sample



Note: The first column (Baseline) plots estimates of $\gamma^i_{all,\tau}$ in regression (1) for the expected average future forward inflation (first row) and inflation risk premium (second row) at maturities $\tau \in (2, 10)$, together with 90% confidence intervals based on robust standard errors. The measures are based on the decomposition introduced in D'Amico, Kim, and Wei (2018). The second (Low noise) and third (High noise) columns respectively present parameter estimates for $\gamma^i_{hl,\tau}$ and $\gamma^i_{ll,\tau}$ in regression (2), corresponding to the subset of scheduled FOMC announcements for which the noise measure is below (resp. above) its median level. The sample includes all regularly scheduled FOMC meetings from 01/2000 to 12/2019, excluding those taking place between July 2008 and June 2009. This corresponds to a sample size of 152 observations on which each regression is estimated. Regression results for the 2, 3 and 4-year forward rates are based on a sample size of 120 observations (starting in 2004).