Nonlinearities in Monetary Policy Transmission

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August 8, 2024

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Abstract

To find if the interaction between the size and sign of interest rate movements affects their effects, I estimate impulse responses to monetary policy shocks using a Local Projection Instrumental Variable (LP-IV) framework and look for evidence of nonlinearities. Specifically, I extend the monetary surprise series of Aruoba and Drechsel (2024) for an instrument and decompose the measured shock into regimes based on how the Fed Funds Rate changed in a given month. Focusing on output and inflation, I find extensive evidence of size effects and asymmetries that work in opposite directions. I show that a New Keynesian model with downward rigidities in price and wage adjustments does not produce similar IRFs.

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Introduction

Much of the evidence on the effects of monetary policy has been produced from model-based and empirical approaches that impose a linear relationship between shocks and real variables. Namely, while standard impulse response functions (IRFs) can be non-linear functions of time, it's typically the case that if the impulse response to a shock s_t is $\{\tilde{y}_{t+h}\}$, the impulse response to αs_t is simply $\{\alpha \tilde{y}_{t+h}\}$. Possible nonlinearity from *size effects* (disproportionate impact of big and small shocks) and *sign effects* (asymmetric impact of positive and negative shocks) is thus ruled out by construction.

Little attention has been paid to the potential for size effects, but economists dating back to the Great Depression have explored the idea that expansionary and contractionary monetary policy have asymmetric properties. One popular narrative is that during a downturn there is little central banks can do to create an appetite for lending and spur broader economic activity, commonly paired with the analogy that "pushing on a string" is futile. Grigoli and Sandri (2023) summarize the body of empirical evidence on this thesis as being generally mixed but slightly supportive, albeit with an abundance of statistical insignificance. Still, there is currently no knowledge on the intersection of size and sign. If such nonlinearity is a defining feature of the data, monetary policy transmission depends on a constraint or channel that would be neutered by solving models to first order, meaning many foundational notions about optimal monetary policy would then need to be reexamined.

I first estimate impulse response functions using LP-IV, the Stock and Watson (2018) instrumental variable extension of Jordà (2005) local projections (LP), and allow for the possibility for size and sign effects by introducing 4 interest rate change regimes (combinations of big vs. small, positive vs. negative) and instrumenting each with an extended version of the Aruoba and Drechsel (2024) monetary surprise series. There is a vast literature assessing the effects of monetary policy via LP or VAR (see Ramey (2016) for a survey), but few of these efforts have tested for sign effects (e.g., Tenreyro and Thwaites, 2016; Barnichona and Matthes, 2018) and to my knowledge only Ascari and Haber (2022) has allowed for size effects, with no work on the joint impact of size and sign. Using outcome variables of consumption, industrial production, and the Consumer Price Index (CPI), I find extensive size effects exist with both interest rate cut and hikes and sign effects with big changes for all variables. In general, big cuts are less expansionary than small cuts and big hikes are more contractionary than small hikes with the exception of CPI: the expected inflation rate is proportionally 0.6% higher a year after a big hike. Most striking, all size effects are largest in the medium run (10-15 months out) and essentially no nonlinearity exists on impact. Returning to the "string" narrative, the output proxies (consumption and industrial production) do respond more to hikes for large changes but for the opposite is true for inflation with both big and small changes.

I then show that a workhorse New Keynesian model cannot match these findings. To allow for consequential nonlinearities, the model is augmented with asymmetric adjustment costs in price and wage setting and is estimated to second order via a Metropolis-Hastings routine and particle filter from Fernandez-Villaverde and Rubio-Ramirez (2007), producing a posterior distribution implying downward-rigid prices and wages found in micro-evidence and past quantitative work (e.g., Kim and Ruge-Murcia, 2009; Aruoba et al., 2017). Barnichona and Matthes (2018), who find similar sign effects using unemployment and inflation, conjecture that such a model can rationalize their results. A comparable LP-IV specification combined with model-simulated data is used to produce Bayesian IRFs and also search for parameter combinations that minimize deviation from the results from US data. These illustrations show the model does produce non-linear effects of monetary policy, but they appear on impact and quickly vanish. The implication is that there is not enough internal propagation in this model to generate the sustained nonlinearities observed empirically. Namely, a Taylor Rule, even with a lagged interest rate, does not create a sufficient amount of persistence on its own. Much of the recent literature on adding meaningful nonliterary to standard models has focused on the zero lower bound (ZLB) episode (e.g., Eggertsson, 2011; Christiano et al., 2011; Wieland, 2019), but such extensions are insufficient for this application. Future work will build on less restrictive structures of price stickiness that nest popular approaches (Reiter and Wende, 2024).

A prerequisite to taking this research direction seriously is confidence in the methodology of the motivating results. To these ends, this paper is the first in this setting, to my knowledge, to leverage the wealth of research that has come out the past few years about local projections. Namely, the differences between LPs and vector autoregressive estimation (VAR) have been made clear.[1] Plagborg-Møller and Wolf (2021) shows the methods yield the same population estimates of impulse responses and in finite samples the trade-off boils down to flexibility (LP) vs. efficiency (VAR). Even more recent work by Montiel Olea et al. (2024) reveals the cost of the efficiency gains from VARs can be prohibitive: they are comfortably insensitive to misspecification if and only if the coefficient of interest has similar variance to its LP analogue. I use a penalized approach (Barnichon and Brownlees, 2019) to retain the appealing properties of LPs with some added efficiency. In addition to having less bias within broad classes of data generating processes, LPs can more easily accommodate an inclusion of nonlinearity. Debortoli et al. (2023) provides a more general VAR-based approach allowing for similar inclusions, but the monetary policy shocks they identify reveal being explicitly tied to a Taylor Rule that does not account for the ZLB is an insurmountable hurdle to inference, especially given the limited sample size. Finally, the LP-IV framework, which has been underutilized, provides more interpretability while retaining these advantages of LP.[2] Standard LP IRFs are plots of the coefficient on a monetary surprise series (e.g., Romer and Romer, 2004), but the exact meaning of a unit change in these surprise measures is unclear, and whatever underlying meaning does exist inherently varies by shock series (Brennan et al., 2024). However, the uniformity across all approaches is they are developed

[1]Other important considerations include bias in estimates (Gonçalves et al., 2024; Herbst and Johannsen, 2024), bias in standard errors (Plagborg-Møller and Montiel Olea, 2021; Herbst and Johannsen, 2024), and general identification (Stock and Watson, 2018; Jordà, 2023)

[2]Also superiority in dealing with non-stationarity (Plagborg-Møller and Montiel Olea, 2021; Plagborg-Møller et al., 2024) and relaxing VAR's structural assumptions (e.g., recursiveness and inevitability). Ramey (2016) and Mavroeidis (2021) find recursiveness is not palatable.

in order to capture relevant changes in monetary policy while maintaining exogenous variation. Thus, it's more appropriate to treat them as instruments.

In addition to building on the methodology informing the literature, this paper expands the overall scope of efforts to leverage externally identified shock series to document monetary policy's effects. Ramey (2016) provides a survey of various method-shock series combinations and documents prevalent "puzzles", implications of impulse responses that conflict with standard intuition. In general, this literature is noisy – one can find a well-cited paper suggesting that macro variable x responds in y direction for any combination of x and y. I sidestep the puzzle rabbit hole altogether by focusing on *relative* effects, which haven't received much theoretical or applied attention. I also provide a novel depiction of nonlinearities using standard deviations of effect sizes, making them more readily comparable to estimations of DSGE models by removing any meaningless distortion from scaling differences induced by finite sample properties of time series and model-simulated data.

The primary contribution of this paper is presenting new facts about the historical effects of monetary policy and our default model's ability to account for them. First, we observe that non-linear behavior is most pronounced in the medium run. Earlier effort has been made to modify New Keynesian models to yield "hump shaped" impulse responses (e.g., Tsuruga, 2007), but whether it's possible to produce the same for non-linear differences based on the type of shock is unclear. We also see that for small shocks, there is no evidence for the "pushing on a string" dynamic between policy and real variables. This possibly suggests policymakers do not face asymmetric constraints to stimulate the economy until the goal for expansion becomes sufficiently large. For big changes in policy, such asymmetric constraints are observed for output, but the opposite relationship exists for inflation. A model of downward nominal rigidities can only reproduce this fact in the period a shock occurs. Friedman (1960) posited that the peak effects of monetary policy take time to materialize and the length of this delay can change ("long and variable lags"), but especially in the era when the central bank does not exert control over the level of monetary aggregates (Cochrane, 2024), it's not well understood what mechanism would yield such a transmission path. Optimal monetary policy in a model that is able to capture nonlinearities that are minimal on impact but cumulate into a strong driving force will assuredly not be more of the same.

Empirical Methodology and Results

Framework and Data

Impulse responses generated by local projections (LP), pioneered by Jordà (2005), are a collection of coefficients $\{\hat{\alpha}_h\}$, where each $\hat{\alpha}_h$ comes from a regression of y_{t+h} on time t control variables X_t and shocks s_t

$$y_{t+h} = \alpha_h s_t + \beta_h X_t + \epsilon_{t,h}$$

Because these regressions are independent of one another, this is a non-linear impulse response function (IRF). However, this is only non-linear with respect to time, not the shock itself. We want to relax this imposed linearity and allow responses to vary by the type of shock that occurred at $t = 0$. Linearity in this context means big shocks and small shocks have nearly proportional effects and positive and negative shocks (of the same size) produce symmetric effects. The usual LP framework can be modified to accommodate potential nonlinearities by decomposing our shock into several shock series based on what "regime" of monetary policy we are in. We specify 4 regimes by creating a single threshold within our two dimensions of size and sign:

1.BH (Big Hike) 2.BC (Big Cut) 3.SH (Small Hike) 4.SC (Small Cut)

Formally, regimes can be represented as indicator variables and the vanilla LP decomposition becomes

$$y_{t+h} = \underbrace{\alpha_h^R R_t s_t}_{(\alpha_h^{BH} r_{1,t} + \ldots + \alpha_h^{SC} r_{R,t}) s_t} + \beta_h X_t + \epsilon_{t,h}$$

where α_h^R is a 1×4 matrix of coefficients and R_t is 4×1 matrix of the corresponding regime indicator variables, which can be generalized to more regimes as needed. One interpretation of this setup is that there are 4 shocks, one of which will be "activated" in each period.[3]

The first step towards implementation is finding a suitable shock series s_t to represent exogenous variation in interest rates. We are in essence seeking to recover the "treatment effect" of monetary policy and therefore must overcome the classic identification concerns presented by simultaneity (outcome variables influencing the probability of treatment) and anticipation (the model will be misspecified if we're capturing responses to something other than our s_t). There have been a litany attempts to construct monetary surprise measures that reflect unanticipated change relative to what people expect, with many using a high-frequency window around monetary policy announcements to plausibly argue the data is solely capturing responses to central bank action. Because these measures can be normalized in different ways (Acosta, 2023) or have interpretation that is sensitive to units or specification (Brennan et al., 2024), it's less appropriate to include them directly as regressors. We can instead leverage the fact that they are constructed to satisfy the usual IV assumptions of relevance and exogeneity (a sufficient condition for the exclusion restriction). This makes the relevant coefficient correspond to "cleaned" interest rates, rather than units of a given shock series. Thus, we proceed with the LP-IV approach put forward by Jordà et al. (2015) and Stock and Watson (2018).

We also need to explicitly define our regimes, namely take a stance on what constitutes a "big" change. I use a cutoff of a 10% jump from the previous fed funds target, which balances sample sizes between regimes nicely. The cost is no changes near the zero lower bound (ZLB) are classified as small. However, percent change is more in

[3] In a pure LP setup, there would also be a fifth regime for when there is no change in the LP target. However, because we are using LP-IV and thus instrumenting changes in the target, there is no such regime in LP-IV and thus this is omitted from the initial setup for clarity

line with the default treatment of shocks in structural setups (magnitude relative to standard deviations away from steady state). There is also a problem finding valid instruments when using magnitudes to define regimes, partially due to a scarcity of hikes greater than 25 basis points. Conversely, under our chosen specification I find that the Aruoba and Drechsel (2024) monetary surprise series, which orthogonalizes Romer and Romer (2004) to information in Fed meeting transcripts, is a strong instrument for all regimes.[4] As detailed in the Appendix, I extend their series to 2016 and in future work will modify their approach to construct a new series.

As previously mentioned, LP faces efficiency concerns and there can also be large variability in the coefficients from one horizon to the next. To address both concerns, the main visualizations are generated by 2SLS combined with the penalized LP approach of Barnichon and Brownlees (2019), who approximate the shock coefficients using B-spline basis functions and minimize a ridge loss function with a particular penalty matrix that shrinks the IRFs towards a polynomial of a given order as the regularization parameter λ grows. Following their guidance on the sensitivity of inference in light of penalization and cross-validation induced complications, I fix λ at a mild level and use standard errors from an even more "under-smoothed" estimation (with $.1^*\lambda$). Most applications of local projections estimate standard errors using a Newey-West adjustment, which theoretically accounts for heteroskedasticity and autocorrelation (HAC), but Herbst and Johannsen (2024) find this procedure yields biased estimates. Rather than introducing unneeded complexity by using a more sophisticated HAC-robust method, Plagborg-Møller and Montiel Olea (2021) shows that using the usual Huber-White heteroskedasticity-robust standard errors paired with including a sufficient number of lagged control variables is sufficient.

Following Ramey (2016), we consider outcome variables at a monthly frequency of the Consumer Price Index (CPI), industrial production, 1 year treasury yields, excess bond premium (Favara et al., 2016), and add real consumption expenditures.[5] Control variables also include lagged interest rates, monetary policy uncertainty (Husted et al., 2020), an indicator for the ZLB, and a healthy number lags (12) of both outcome and controls following our discussion of standard errors. Data is sourced from FRED unless noted otherwise and the maximum sample periods are retained. The pre-1983 target funds rate data is discarded to reflect incongruities in Fed policy norms (Thornton, 2006) and a few earlier instances of "intermediate" changes (e.g., adjust 12 basis points immediately and 25 more in a few weeks) are cumulated. I focus on CPI and the joint picture of output painted by industrial production and consumption in order to take the findings directly to models. Outcome variables are cumulative log differences, yielding an approximate percent change interpretation: $\widehat{\alpha}_h$ is represents the percent change in levels h periods after a shock at t. At $h = 12$, this takes a nice form of year over year growth.

[4] In principle, an advantage of LP-IV is using combinations of shock series, simply selecting the strongest instrument for a given regime (like one might estimate VAR components over a subsample for efficiency gains). Unfortunately, other series didn't provide these potential gains

[5] Some of these variables are highly non-stationary (McCracken and Ng, 2016). Plagborg-Møller and Montiel Olea (2021) show LP is remarkably robust to the presence of unit roots and non-stationary variables. I find some anecdotal support for this: estimating in differences and summing for the cumulative effect in levels produces very similar IRFs to estimating in levels directly

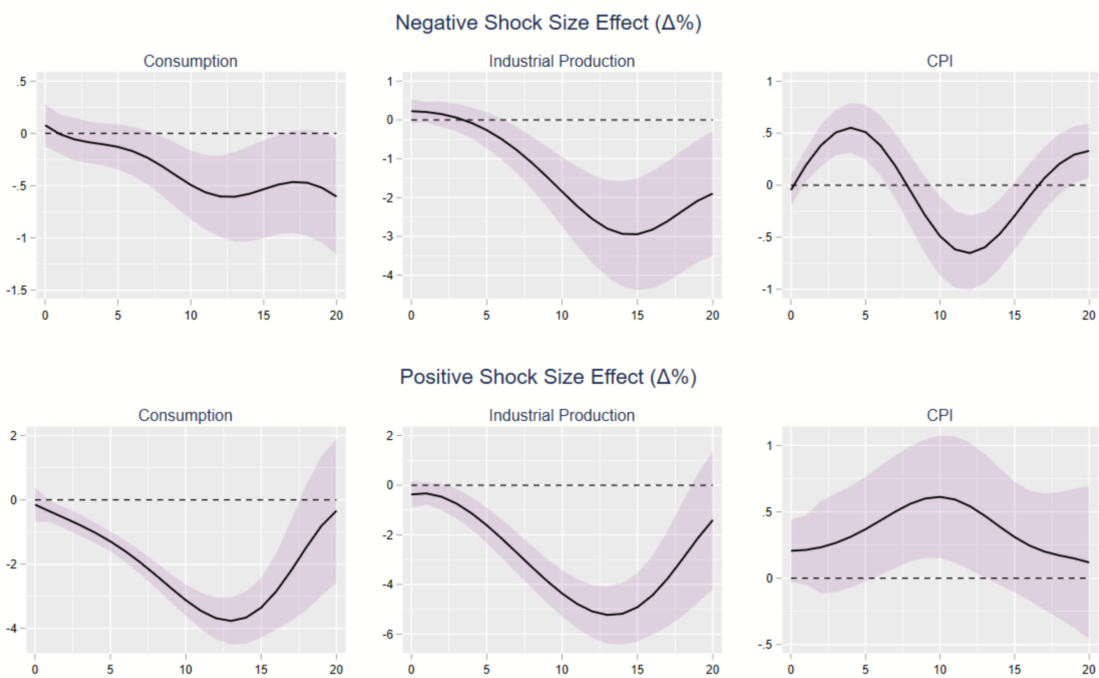
Results

The described LP-IV framework can be used to illustrate possible nonlinearities, which I refer to as size and size effects. The most straightforward way to think of these effects is as functions of parameters. For the simple case of plotting in levels, the objects of interest are

$$\text{Size Effect}_h : \hat{\alpha}_h^B - \hat{\alpha}_h^S \qquad \text{Sign Effect}_h : \hat{\alpha}_h^P + \hat{\alpha}_h^N$$

A size effect exists if we can conclude the difference in the big and small (regime) coefficients are distinct from 0 and a sign effect exists if positive and negative coefficients have different magnitudes. This is complicated slightly by wanting to allow for both types of non-linearities simultaneously: we want to see if a size effect exists for both cuts and hikes and a sign effect exists for both big and small changes, in other words 4 graphs per outcome variable. Grouping is thus by type of nonlinearity, rather than outcome.

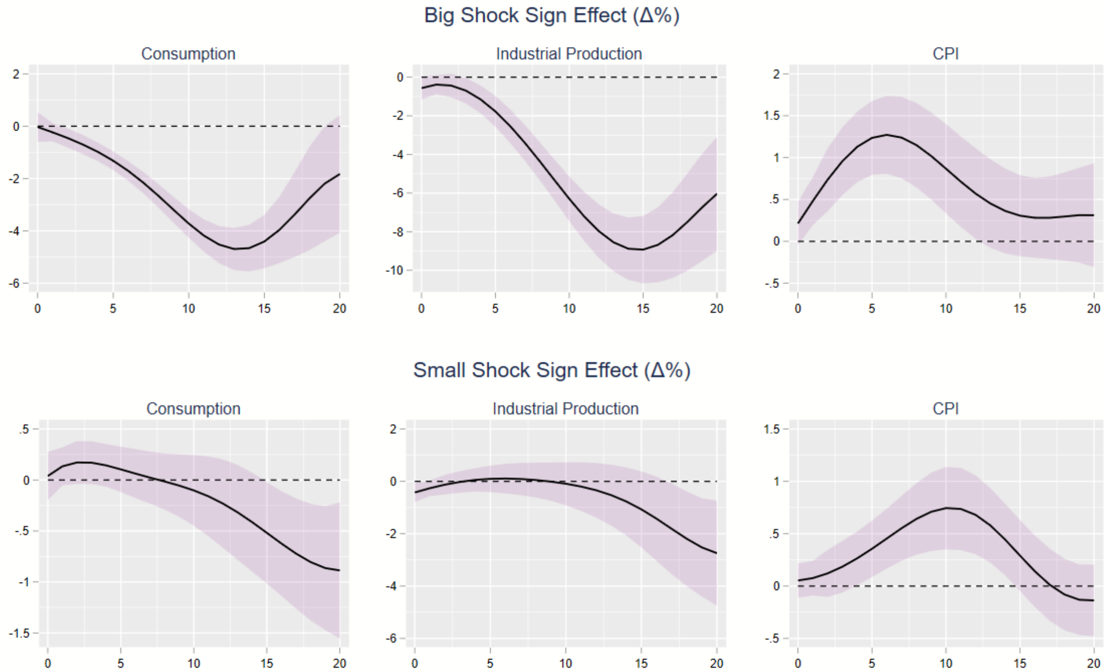
Figure 1 shows that significant size size effects exist for all variables and both positive and negative monetary policy changes at some horizon.[6] Big cuts yield proportionally smaller values of all of our variables of interest in the medium run. The same is true for big hikes relative to small hikes with the exception of inflation, where CPI growth is higher for big hikes. For sign effects, Figure 2 shows that while not much can be said for small changes, there is asymmetry for large changes.



Confidence bands are 95%; standard errors obtained from the variance-covariance matrix.

Figure 1

[6]IRF plots are often scaled to represent responses to a 25 basis point change in i_t. I scale the coefficients to correspond to a 50bp change. Intuitively, these plots then show the difference in effects between a 50 bp change and 2× the effect of a 25bp change while sitting at $i_t \in [2.5, 5]$. The findings do not materially differ but this is in principle a necessary step because our instruments have differing abilities as a Δi_t proxy



Confidence bands are 95%; standard errors obtained from the variance-covariance matrix.

Figure 2

For sign effects, we can interpret positive statistically significant results as evidence for a "pushing on a string" narrative, the idea that it's more difficult (especially in recessions) for expansionary monetary policy to stimulate the economy than it is for contractionary policy to suppress it. Even if the individual point estimates go against what standard theory might suggest, we abstract from the notion of puzzles by simply focusing on one estimate relative to the other. For instance, if the coefficient of a big cut on CPI is -3 and the coefficient for a big hike is 2, these estimates are consistent with the string story because the the big hike's contractionary power, albeit a lack of one, is still greater than the expansionary effect of a big cut.

The Appendix contains a table summarizing these results at selected horizons using standard deviations of effect sizes. This representation complements the main figures in several ways. For example, if the coefficient on a small cut is 10%, one could argue a size effect of .2%, even if statistically significant, implies there is not a large cost to restricting ourselves to linear estimation and solution methods. Consulting the table reveals the most potent effects in this section's illustrations amount to a more than 2 standard deviation away realization of the baseline coefficient, with the exception of the size effect for negative shocks on consumption. That level of magnitude leaves no doubt that the difference in effects should be taken seriously and living in a linear world can be ill-advised for some questions. In addition, this formulation has consistent interpretation across variables and data generating processes (DGPs). A main motivation for these exercises is to see how models hold up to the task of replication. In principle, percent change is a "unitless" point of comparison. But in a small sample setting, having parameters in a DSGE model that dictate growth rates can induce distortions in scaling and correlations relative to time series data that add meaningless noise to analogous estimations of our objects of interest. By using standard deviations, everything is normalized by whatever scale persists in the DGP, meaning the finite sample idiosyncrasies are softened. Ultimately, percent change in levels carries more real world weight, but standard deviations add indispensable context. The Appendix also contains further discussion on the mechanics of each.

Sensitivity

Before gauging how models holds up to these data-based findings, it's important to have a sense of what, if anything, can make these results weaken when pushed. Changing the lag order, adding and removing controls, estimating in differences vs. levels, different measures for inflation, bias-correcting point estimates (Herbst and Johannsen, 2024) and using LP instead of LP-IV in general do not yield IRFs with different interpretations, even under various combinations of these factors. One dimension that revealed some delicacy is sample size, in line with Ramey (2016).[7] This is unsurprising given the relatively few months there is data ($n/k \approx 2$) and the even fewer periods with consequential central bank intervention. Future work will extend these procedures to VAR and Bayesian methods. Alternative measures of interest rates to address the zero lower bound can also be considered. One option would be using a non-linear filtering procedure (e.g., Farmer, 2021) to construct a shadow interest rate, or a measure of how interest rates "would have moved" if the ZLB didn't bind.

A more involved critique of model-free IRF estimation is an inability to account for state-dependence. For example, many past efforts try to allow for responses in boom and bust cycles to be asymmetric. With respect to interest rate shocks of a given size, another worry could be that beliefs about the future path of policy may not be updated in the same for different histories of action. The econometric concern is that these local projection coefficients amount to weighted averages and these weights could be biased if the joint distribution of the shock and state space has disparate behavior from a product of their marginals. In a regression context, this essentially amounts to the difference between including a variable as a control and additionally interacting it with the shock. Jordà (2023), following the revelation of Gonçalves et al. (2024) that the previous default methodology can severely distort IRFs, provides a framework incorporating interaction terms to estimate state-dependent effects. While this is not feasible for LP-IV because of the curse of dimensionality, applying this approach in the LP analogue does not affect the conclusions. Gonçalves et al. (2024) themselves suggest non-parametric estimation, which has an over-parameterization problem with or without instrumenting (i.e., in either case, control variables must be shed). The non-parametric estimation performed thusfar focusing on boom and bust dependence has yielded similar size effect results. Future work will extend to IV by using a parametric first stage and also include a

[7] For instance, increasing the size threshold to 12.5% for our regimes yields much different sample distributions. The results generally hold under this definition but are less pronounced for inflation

focus on the ZLB, since lifting off from the bound is classified as a large change under our definitions but may be dissimilar to changes in unconstrained states. It's also worth noting that Rambachan and Shephard (2021) show that under fairly mild assumptions, LP estimands do recover a weighted average of the true marginal effects under general state-dependence.[8] In light of earlier discussion, this implies if state-dependence is of great concern the bias-reducing properties of LP should be even more appealing relative to a baseline VAR.

A separate issue that interacts with the LP-IV specification is the validity of the instruments themselves. When using the various shock series in a local projection, the results remain generally unchanged, especially between series with similar sample sizes. However, this is not true for LP-IV. Only Aruoba and Drechsel (2024) and Romer and Romer (2004) are strong instruments for all regimes, with the others a strong instrument for a maximum of 2, resulting in first stage predicted values that are relatively dissimilar from the baseline specification. Part of the reason for this difference can be attributed to their construction – these other series are high frequency measures of market instruments in a narrow window around Fed announcements. Aruoba and Drechsel (2024) and Romer and Romer (2004) can be thought of as "Greenbook identification" – they take the change in the interest rate and try to orthogonalize it to the Fed's information set, which presumably is contained within their meeting notes. If we can assume that the Fed staff know the form of the policy function, then we can interpret the these measures as the shock term in a Taylor rule. This is different from a high-frequency series: because of the uncertainty, nearly all policy announcements will cause a change in the price of various interest rate-sensitive futures, even if market participants have a perfect approximation of the Fed's policy rule. If the distribution of prior beliefs about interest rate changes is not symmetric, then high-frequency movements will often not be capturing a "surprise" or any information about whether the Fed tightened or loosened relative to expectations. As a simple illustration: if beliefs are a 0% chance of a cut and an ϵ% chance of any magnitude of hike, the Fed choosing to not move interest rates will register as a loosening surprise in Fed Funds futures. This dynamic exists for any prior distribution of beliefs that is asymmetric, which is almost always the case (Lobão, 2023), rationalizing the high frequency measures being much worse instruments. More discussion on the use of high frequency measures as instruments and additional considerations for Greenbook identification can be found in the Appendix.

Quantitative Benchmarking

To compare to the IRFs estimated directly on US data, a basic point of reference would be using a model that features meaningful nonlinearities to generate data and then run the same regressions. For narrowing the choice set, relaxing the structure of pricing frictions is a natural starting point because monetary policy in the basic New Keynesian model is understood to primarily operate through the firm's problem (Rupert and Šustek, 2019). Further,

[8] However, one needs to be careful about the implied weighting scheme. See Appendix

Barnichona and Matthes (2018) conjecture that sign effects which work in opposite directions for output and inflation, which is what we observed in the last section, can be rationalized in a model with downward-rigid prices and wages. To these ends, I estimate the model of Kim and Ruge-Murcia (2009), who add on asymmetric price and wage setting frictions to a standard New Keynesian model. Specifically, a firm seeking to change its price at a rate different than steady-state inflation face a linear-exponential (Linex) adjustment cost that takes the form of

$$\Phi_t^p(\pi_t) = \frac{\phi_p}{\psi_p^2}\left(e^{-\psi_p(\pi_t-\pi^*)} + \psi_p(\pi_t - \pi^*) - 1\right)$$

For $\psi_p > 0$, it's more costly to decrease prices than raise them (downward-rigid), for $\psi_p < 0$ prices are upward-rigid, and the function limits to symmetric adjustment costs as $\psi_p \to 0$. Nominal wage adjustment costs take on the same structure. Past estimation of this model have found evidence of downward rigidity in prices and wages, consistent with empirical evidence dating back to Keynes (1936) and Tobin (1972).

I first outline the model and estimation then provide post-estimation attempts to match the empirical results.[9] These exercises show that this while the model can generate nonlinearities, in general the observed asymmetric effects for both size and sign occur on impact and then quickly dissipate, unlike what we saw with US data.

Full Model Specification

I largely follow the discussion and modifications of Aruoba et al. (2017) to the canonical downward-rigid price model of Kim and Ruge-Murcia (2009). Originally, there were only two stochastic processes in the model, meaning Bayesian estimation can only be used to match two observed variables. Aruoba et al. (2017) add stochastic processes for government spending and (crucially) the monetary policy rule with observed variables of real GDP per capita, inflation, nominal wage growth, and interest rates.[10]

Stochastic Processes

The (non-stationary) aggregate productivity process is

$$\ln A_t = \ln\gamma + \ln A_{t-1} + \ln z_t, \quad \text{where} \ \ \ln z_t = \rho_z \ln z_{t-1} + \epsilon_{z,t}$$

The government consumers a fraction $\frac{1-g_t}{g_t}$ of output to finance wasteful spending. g_t follows

$$\ln g_t = (1-\rho_g)\ln g + \rho_g \ln g_{t-1} + \epsilon_{g,t}$$

The inverse-demand elasticity for intermediate goods is given by

$$\ln\lambda_{p,t} = (1-\rho_p)\ln\lambda_p + \rho_p \ln\lambda_{p,t-1} + \epsilon_{p,t}$$

[9]A consolidated list of model equations, including equilibrium conditions, and more detail on the estimation can be found in the Appendix.
[10]Inflation is annualized changes in the GDP deflator and wage growth is changes in non-farm weekly wages from the BLS

And finally the (nominal) interest rate setting rule is

$$R_t = R_t^{1-\rho_R} R_{t-1}^{\rho_R} e^{\epsilon_{R,t}}, \quad \text{where } R_t^* = r\pi^* \left(\frac{\pi_t}{\pi^*}\right)^{\psi_1} \left(\frac{Y_t}{\gamma Y_{t-1}}\right)^{\psi_2}$$

The target interest rate R_t^* is relative to its (real) steady state r, how much weight the monetary authority places on deviations of inflation π_t from their time-invariant target π^* (which in equilibrium must be steady state inflation), and deviations in output Y_t from its expected growth rate γ. The means of the $\lambda_{p,t}$ and g_t processes are fixed but otherwise all other parameters are estimated. Each ε is assumed to be a white noise process and follow an normal distribution mean zero and a standard deviation that is also an estimated parameter.

Households

There are a continuum of households indexed by k that pick consumption $C_{t+s}(k)$ and labor hours $H_{t+s}(k)$ to solve

$$\mathbb{E}_t \sum_{s=0}^{\infty} \beta^s \left(\frac{\left(\frac{C_{t+s}(k)}{A_{t+s}}\right)^{1-\tau} - 1}{1-\tau} - \chi_H \frac{H_{t+s}^{1+\frac{1}{\nu}}(k)}{1+\frac{1}{\nu}} \right)$$

where $1/\tau$ is the intertemporal elasticity of substitution (IES) and ν is the Frisch labor supply elasticity. To ensure this models admits a balanced growth path, consumption is treated as being relative to a habit stock. As mentioned earlier, there is an asymmetric adjustment cost for changes in wages relative to steady state growth

$$\Phi_t^w(x) = \frac{\phi_w}{\psi_w^2} \left(e^{-\psi_w(x-\gamma\pi^*)} + \psi_w(x-\gamma\pi^*) - 1 \right)$$

where the argument x is nominal wage growth. The adjustment cost is paid by households: for a given nominal wage $W_t(k)$, their effective wage is $\widetilde{W_t}(k) = \left[1 - \Phi_t^w\left(\frac{W_t(k)}{W_{t-1}(k)}\right)\right] W_t(k)$. Households can borrow and save through nominal government bonds $B_t(k)$ earning gross interest R_t, pay taxes net of transfers T_t, and receive dividends $D_t(k)$, resulting in a budget constraint based on an aggregate price level P_t they take as given

$$P_t C_t(k) + B_t(k) + T_t = \widetilde{W_t}(k) H_t(k) + R_{t-1} B_{t-1}(k) + P_t D_t(k)$$

Supply Side

There is a perfectly competitive packer of goods and of labor. Using CES aggregators, the final good packer combines a continuum of intermediate goods and the labor packer combines labor from the continuum of households

$$Y_t = \left(\int_0^1 Y_t(j)^{1-\lambda_{p,t}} dj \right)^{\frac{1}{1-\lambda_{p,t}}} \quad \underline{\textbf{and}} \quad H_t = \left(\int_0^1 H_t(k)^{1-\lambda_w} dk \right)^{\frac{1}{1-\lambda_w}}$$

where λ_w is fixed at the steady state level of $\lambda_{p,t}$. This results in aggregate price levels

$$P_t = \left(\int_0^1 P_t(j)^{\frac{\lambda_{p,t}-1}{\lambda_{p,t}}} dj \right)^{\frac{\lambda_{p,t}}{\lambda_{p,t}-1}} \quad \underline{\textbf{and}} \quad W_t = \left(\int_0^1 W_t(k)^{\frac{\lambda_w-1}{\lambda_w}} dk \right)^{\frac{\lambda_w}{\lambda_w-1}}$$

Each intermediate good is indexed by j is produced by a monopolist with production technology $Y_t(j) = A_t H_t(j)$. Each firm buys labor at the aggregate factor price W_t and face the aforementioned asymmetric adjustment costs for changes for price changes that deviate from steady state inflation. Firms then solve

$$\mathbb{E}_t \sum_{s=0}^{\infty} Q_{t+s|t} \left(\frac{P_{t+s}(j)}{P_{t+s}} \left(1 - \Phi_p \left(\frac{P_{t+s}(j)}{P_{t+s-1}(j)} \right) \right) Y_{t+s}(j) - \frac{1}{P_{t+s}} W_{t+s} H_{t+s}(j) \right)$$

which represents the discounted present value of future profits subject to $Q_{t+s|s}$, the value of a $t+s$ consumption good in terms of time t goods. The model is solved at its symmetric equilibrium.

Estimation and Extensions

Using the same sample period of US data as the empirical IRFs, I performed a second-order, Bayesian estimation of the model via a standard random walk Metropolis-Hastings algorithm and a particle filter outlined in Fernandez-Villaverde and Rubio-Ramirez (2007). I use the distribution of parameters generated by this exercise to simulate data and run the same Local Projection Instrumental Variable (LP-IV) procedure to create various IRFs, resulting in two main illustrations of how this model generates nonlinearity. The first are relatively standard "Bayesian IRFs": data is simulated for each set of parameters, LP-IV is used to estimate the IRF for each, and then I plot the median outcome variable response at each horizon with bands given by the 10th and 90th percentile of responses, called a credible set. For the second illustration, I take the posterior mode of all parameters and then vary both asymmetry parameters (one at a time, in both directions, and then both at once in the same direction) while keeping everything else fixed, then simulate data and plot IRFs for each combination. This is meant to shed some light on what the relevant comparative statics for the parameters we most care about might resemble. I also attempt to directly match the empirical results by using Metropolis-Hastings to search over the parameter space and find a vector that generates IRFs which minimize loss with respect to the original estimation.

Bayesian IRFs

For the set of draws that came out of our Metropolis- Hastings routine, I simulated data of 400 observations for each group of parameters to align with the US data sample size. I then created IRFs using LP-IV estimation, where the instrument in this case is $z_t = i_t - \mathbb{E}_{t-1}[i_t]$ to match the construction of monetary surprise measures in the literature.[11] Analogous control variables are included (lagged interest rates, zero lower bound, unemployment, output and interest rate variance) and plots are in terms of standard deviations to abstract away from any differences between model-simulated and US data. One caveat is I do not use the aforementioned penalized

[11] I use i to distinguish the model object R from its observation equation analogue.

2SLS to save computation time. However, Bayesian LPs can be thought of as another type of "smooth" LP, so this procedure is still capture the spirit of previous the previous illustrations.

For size effects (Figure 3), we largely see extremely precise estimates around 0, suggesting in this model a big interest rate change change does in fact amount to a scaled small change. The exception is the immediate impact on output: interest rate cuts are proportionally less stimulative and hikes are proportionally less contractionary on impact. This effect dissipates quickly. This is an indication is that there is a lack of strong shock propagation, intuition that will be confirmed later when looking directly at model output without the filter of the LP-IV.

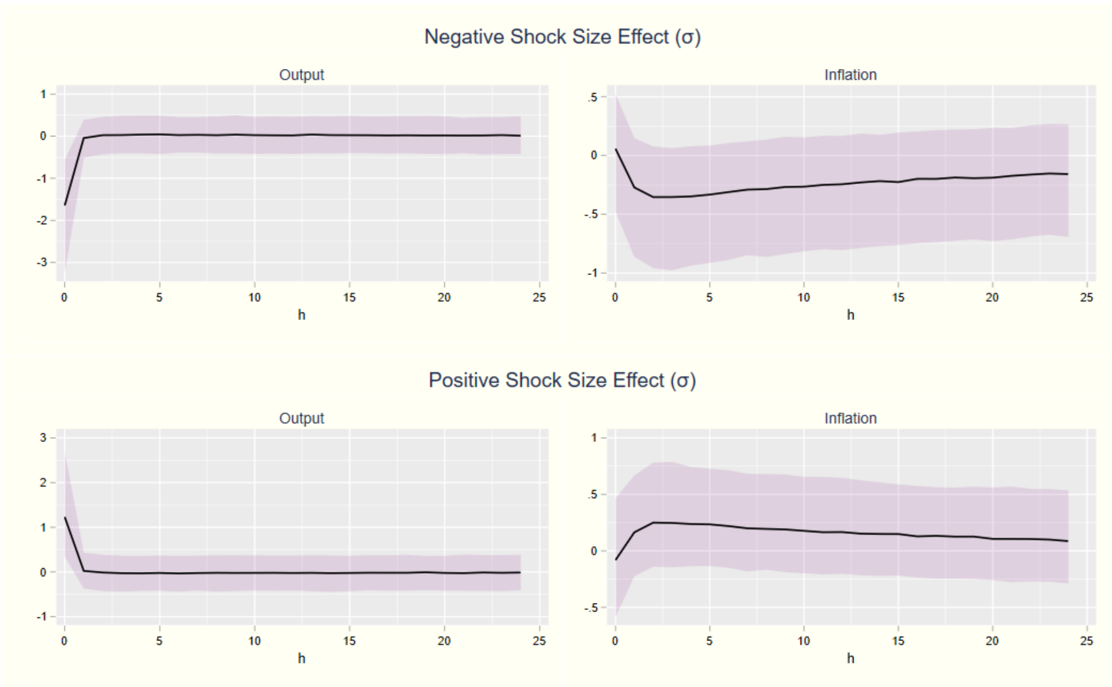


Figure 3

For sign effects (Figure 4), the bands are much larger. So what we can say is that the evidence is suggestive against the presence of sign effects, but we cannot be as confident as before. Some of the estimation performed for the illustrations in the next section reveals a likely reason for the wide credible sets. The estimates for sign effects for a given set of parameters tends to fluctuate around 0. This leads to lots of variance when pooling the results from thousands of sets of simulated data. In light of this context, we can be more confident that this model is not generating meaningful nonlinearity.

Looking at the IRFs in levels directly from the model (rather than running a local projection) corroborates the above interpretations. Figure 5 shows impulse responses to the observation equations for output growth and inflation for both negative and positive shocks of different sizes. At $t = 0$, size and sign effects consistent with the LP-IV Bayesian IRFs are observed for each variable. However, by a horizon of 5 periods after the shock,

Figure 4

the magnitude of responses are near or below machine zero in all cases. This further confirms the model is not generating sufficient internal propagation of shocks.

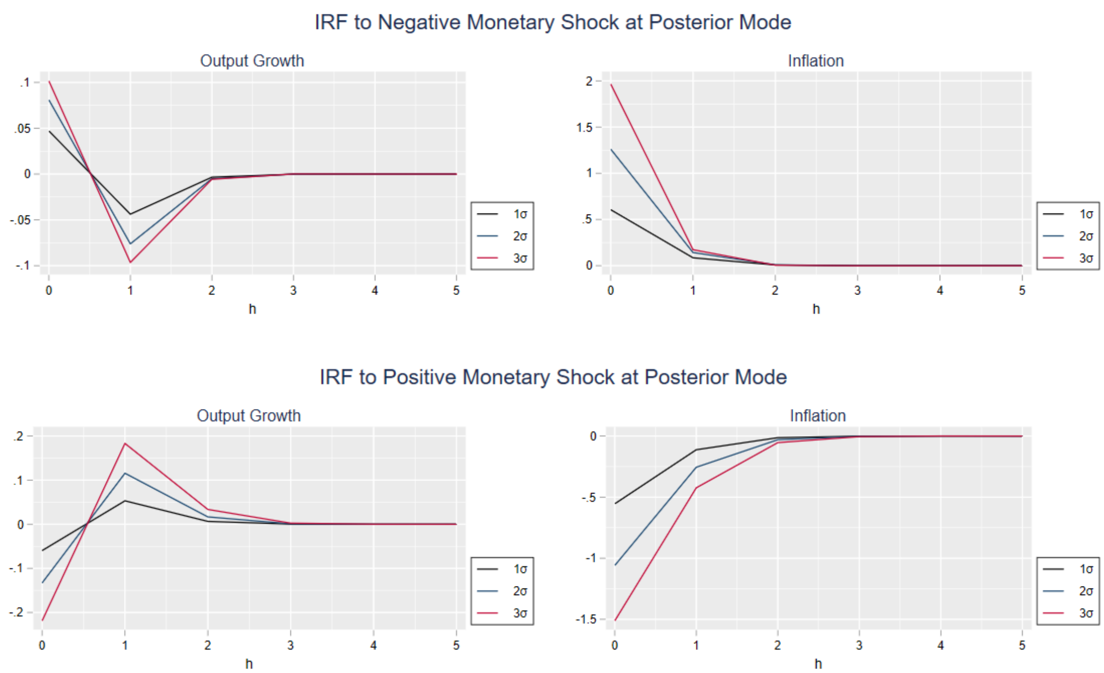


Figure 5

One reason why the effects of monetary shocks may not have a lasting effect is because of the lack of inertia in interest rate setting. Even though the Metropolis-Hastings produced draws with moderately high persistence in the Taylor Rule (posterior mode of $\rho_r \approx .67$), an inspection of model simulated data reveals that whenever a large monetary shock takes the central bank away from its (nominal) target i^*, it generally doesn't take long to get back.[12] Table 1 shows the results of 10,000 simulations at the posterior mode. For each simulation, I take the median distance between the target interest rate and the current interest rate h periods after a big change in interest rates (magnitude greater than 10%) and then average across simulations. In periods in which the central bank heavily adjusts the interest rate, the target is relatively far away, but this is almost completely undone 2 periods later. There is also a large asymmetry on impact that quickly becomes less dramatic.

	h		
	0	1	2
Big Cut	-18.8%	-5.5%	-1.7%
Big Hike	36.6%	5.5%	0.4%

Table 1: Average % Deviation from i^*, h periods after large change in i_t

A salient revelation of this exercise is baseline Bayesian estimation can paint a misleading picture of how well the data is being matched. When fitting the model to its observation equations at a given parameter vector, the implied shocks are restricted to abide by fixed first and second moments, but they need not remotely satisfy the no autocorrelation and independence properties. In contrast, when when simulating the model with the standard deviation draws from the Metropolis-Hastings, the data are generated from genuine white noise processes. In this case, the result is sets of model-simulated data which have counterfactually low inertia in interest rates.

Sketch of Comparative Statics

We also have an illustration of how size and sign effects vary as we make price and wage-setting frictions more and less asymmetric (both individually and jointly). This results in 12 graphs which were collectively informative but ultimate not unique visualizations as standalone objects, so the illustrations are left in the Appendix, along with a table of concise descriptions and links to each. Each plot contains a solid black line at the posterior mode and then dotted, colorful lines to represent the IRFs for different values of the asymmetry parameters. For each graph, the way in which the parameters was changed was consistent: the dotted lines exclusively represent values that get increasingly far away from the mode in the same direction (e.g., the first figure shows how the IRFs change as price-setting frictions are made progressively more asymmetric).

For size effects, the partial derivative evaluated at the mode (for both asymmetry parameters) appears to be relatively monotonic: if any one of the dotted lines was far from the black line, it typically implied that the other lines were in the space in between. However, the deviation from the mode line was is hardly ever consequential.

[12] Regardless of model, consecutive, big realizations of white noise innovations are unlikely, but the staying power of shocks can vary.

Almost all graphs contained at least one deviation at $h = 0$, but otherwise tracked the mode line well. For sign effects, as alluded to in the previous subsection, the mode lines here are a lot more volatile and tend to bounce around 0. This also meant that the behavior of the dotted lines (as we varied the asymmetry parameters) was more pronounced, but ultimately these are seemingly random, one-time deviations. There was still general monotonicity (dotted lines all moving away from mode line in same direction) but it was definitively less consistent.

Therefore, the same conclusion can be drawn as from the Bayesian IRFs: the asymmetry parameters can be tweaked to get any desired type of non-linear behavior on impact, but sustained sign and size effects cannot be produced. The most serious long-term nonlinearity is for big shocks and inflation, with an sign effect that fluctuates around 0 but is still persistently negative. Even if that volatile point estimate is taken seriously, this is the opposite direction of asymmetry we found in US data.

Fitting Empirical Results Directly

One thing lost in the credible sets of the Bayesian IRFs is keeping track of individual parameter estimates. For example, a given parameter vector from our Metropolis-Hastings could be in the 99th percentile for h=0 and the 1st percentile for h=1. We can instead try to find $\boldsymbol{\theta}^*$ that minimizes loss with respect to the empirical IRF coefficients. This is unfortunately not as simple as minimizing a traditional objective function. We must simulate multiple sets of data, which means thousands of regressions (51*the number of sets) at each step of the minimization algorithm. Given the length of the parameter vector, this would be overly time consuming and almost certainly get stuck at a local minimum. Instead, we do a loss-minimization procedure via a quasi Metropolis-Hastings algorithm. For each iteration, we take a draw as we would for a traditional Metropolis-Hastings and compute the loss. The difference is the loss is completely deterministic (the same sets of shocks are used to simulate data) so we accept or reject purely based on having a lower or higher objective function value.

The point of this exercise is to strip out the features of the data that are not of great importance and just try to generate these nonlinearities. We strip out even more by just focusing on the medium run (a year out), rather than trying to match the entire IRF. The barometer is not matching these point estimates exactly but simply to see if the effects can be non-trivially pulled in the proper directions.[13] It does not appear this is possible in a meaningful sense. This procedure was able to produce a parameter combination that cut the loss in half relative to the posterior mode, but this combination is extremely fragile (the loss would change dramatically for minor perturbations to the parameters). This fragility even showed when adjusting the steady state levels of output and inflation growth in either direction, suggesting that at a certain point additional relative improvements are coincidental consequences of arbitrary combinations of parameter values. Looking at the entire IRF confirms this

[13] In any event, industrial production and consumption have to be combined to make a comparison

intuition: the values at a year out were only matched thanks to wild swings from one horizon to the next. It's thus unsurprising that introducing smoothness penalties resulted in even worse performance.

Other additional approaches that were attempted include trying to match the entire IRF, adding habit persistence, and initializing the minimization from areas that heavily influence the amount of big interest rate changes (government spending persistence, IES, and shock standard deviations). These attempts did not result in any noteworthy improvements. Future work will try other mechanisms besides habit persistence to generate more internal propagation, including introducing additional persistence within the monetary policy shocks and adjustment costs. Because this procedure will only span the parameter space asymptotically, these results will never be complete in some sense. This algorithm will continue to be ran with different starting points.

Conclusion

I find significant nonlinearities in the transmission of monetary policy to real variables that a workhorse New Keynesian model was unable to match. Specifically, big changes in interest rates depress the effects on inflation but for output the results match a "pushing on a string" story: big cuts have proportionally less stimulative effects but big hikes have proportionally more contractionary effects. but depresses the effects on inflation. For small shocks, the presence of asymmetry cannot be identified from the data. Adding downward rigidities in price and wage setting to a representative agent New Keynesian model comes nowhere close to replicating these findings. There, the nonlinearities appear on impact and quickly dissipate, whereas we observe empirically the most significant effects occur roughly a year after a given interest rate change. These results are novel along several fronts, including approach, visualization, and incorporation of recent literature about impulse response methods.

The clear next step is to determine what can successfully account for what we found. Some immediate modifications to the model to generate more internal propagation could include habit formation and price indexation with respect to lagged prices, rather than steady state. On a deeper level, any direction needs to carefully consider the equilibrium effects of policy adjustment. For example, Lee (2023) estimates a similar model with comparable findings and suggests that perhaps the Fed should refrain from making large changes. But this prescription does not account for anticipation effects, and Stein and Sunderam (2018) presents compelling theoretical evidence that gradualism is not possible without forward guidance. This leads to a natural potential avenue of directly measuring how forward guidance affects transmission. There has been some work done on this front, but given the differences in nonlinearities between output in inflation, more attention needs to be paid to the response of firms, which have well documented inattentiveness to policy change (Coibion et al., 2018). Another direction to account for the unique traits of inflation responses could be recognizing the non-uniform coordination of policy because of the global marketplace, which our models say would lead to disparate incentives for domestic and foreign firms. In any event, an overarching goal for the future will be quantifying welfare implications, as the optimal policy under a model that is able to match these observed trends may be very different.

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Appendix

Extension and Discussion of Aruoba and Drechsel (2024)

Aruoba and Drechsel (2024) makes an important contribution to the literature by reviving the previously most popular shock series, Romer and Romer (2004). Cochrane (2004) crystallized the potentially enormous potential of this framework by abstracting from some of the finer details and pointing out that all it takes for changes in interest rates not to have a simultaneity problem with a given fundamental is to orthogonalize it to the forecasts of that variable, arguing Romer and Romer (2004) actually included *more* variables than it needed to meet its goals. However, this reasoning is predicated on the forecasts provided in Fed meeting notes representing expected values, whereas Aruoba and Drechsel (2024) provides and cites compelling evidence that these forecasts are instead modal. To overcome this obstacle, they obtain sentiment indicators for hundreds of commonly used terms in Fed staff reports to more concretely represent the Fed's information set.

Aruoba and Drechsel (2024) stop their estimation at the zero lower bound (ZLB) episode. I extend it to 2016 using structural breaks. In line with Acosta (2023), I also try to see what aspects of their specification are important for their results by perturbing certain aspects and comparing the resulting R^2. I find their inclusion of lags and squared terms boosts performance, but the number of sentiment indicators is excessive: after dropping roughly 100 indicators that weren't used often or largely tangential to policy decisions (e.g., Thailand), there is no discernable affect on performance or IRFs. Aruoba and Drechsel (2024) provide high pseudo-R^2 values in their paper and argue the fact their specification "accounts for more of the variance" in interest rate changes, as we would expect from a central bank crafting systematic policy (Leeper et al., 1996), this is evidence of their methodology compared to Romer and Romer (2004). However, given they are using a Ridge regression with $k >> n$, their use of an ad hoc liklihood function to derive an R^2 function seems to be accounting for much of this advantage. Using a "out of sample" R^2 from the cross-validation, the gains are dramatically reduced, though still present.

Future work will extend the spirit of their series. At a base level, the task they are trying to accomplish is not overly difficult: create a model that does will at predicting 57 non-zeros out of 150 observations (pre-ZLB) using roughly 6,500 variables. Post-ZLB, this is an even simpler task. The precipitously diminished performance when using a different measure of a pseudo-R^2 suggests the current approach is indeed an overfitting exercise, whether penalization is used or not. Thus, a much more parsimonious approach is appropriate. In addition, their method counts positive words in a 10 word window before and after certain terms are used. A single sentence in Fed reports will often mention several of these terms, yet contain very different descriptions for each. Given this, a more sophisticated natural language processing approach should be used, in particular leveraging the arrival of large language models. In addition, capturing the end of the 2010s decade will be critical for increasing the low sample size, in particular for tightening cycles. Finally, a central thesis of this work is about capturing non-linear effects. Machine Learning can also be used to classify shocks by the extent and direction of surprise, which can avoid some of the aforementioned problems of high frequency measures as well as account for behavior at the ZLB.

Empirical IRFs using Standard Deviations

In a linear regression, coefficients are the estimated effect of a marginal (size), positive (sign) change. If we normalize our previous definitions by the standard deviation of the coefficient corresponding to this linear "default", we have an alternative formulation of size and sign effects in terms of standard deviations instead of percent change in levels at a given horizon. For example, if $\frac{\hat{\alpha}_{BC}-\hat{\alpha}_{SC}}{\sigma_{SC}} = 3$, the interpretation is that the big cut coefficient amounts to a 3 standard deviations away realization of the small cut coefficient. Additional intuition can be gleaned by noticing that if we instead normalized by the standard deviation of the entire (original) definition, we would simply have a t-statistic. This approach has the advantage of the y-axis having a uniform representation across all outcomes of interest and arguably removes some of the subjectivity implicit in deciding what % constitutes a meaningful effect for a given variable-horizon combination. Put differently, this representation sends a similar signal to the results of a hypothesis test (is there enough evidence from data to infer these parameters are drawn from distinct distributions), but unlike a t-statistic the units lend themselves more to economic meaning (moment of the distribution for our baseline coefficient, rather than a general normal distribution).

Table 2 shows the largest size effect from Figure 1 terms of standard deviation. Without exception, these peak effects occur in the medium run (10-15 months out) and are statistically significant. To guard against the possibility of upward bias in magnitude from using a penalized approach, these results are from an unpenalized estimation. The trends in sign effects are different. For small shocks, there is only detectable asymmetry within a year after a shock for inflation (5.1 standard deviations). Moreover, for big shocks and inflation the asymmetry declines over time (from 2.2 standard deviations), the only nonlinearity where that is the case. The behavior for big shock size effect is similar between the two output proxies, with peaks over 5 standard deviations in magnitude and occurring around 12 months out.

Variable	Cut Size Effect	Hike Size Effect
Consumption	-1.7	-15.4
Industrial Production	-3.1	-8.6
CPI	2.0	3.6

Table 2: Largest Size Effects (standard deviations away)

Full Model Equations

Description	Equation	#
Consumption Euler Equation	$1=\beta\left(\frac{C_{t+1}}{C_t}\right)^{-\tau}\frac{R_t}{\Pi_{t+1}\tilde{A}_{t+1}}$	(1)
Definition for Real Wages	$\Delta_t^w=\frac{W_t}{W_{t-1}}\cdot\tilde{A}_t$	(2)
Resource Constraint	$\frac{G_t-1}{G_t}\cdot Y_t+C_t=Y_t(1-\Phi_t^p)+W_tY_t\cdot\Phi_t^w$	(3)
Wage Equation, Household's problem	$\frac{\chi_h}{\lambda_w}\cdot W_t^{-\tau}C_t^{\tau}Y_t^{\frac{1}{\nu}}+(1-\Phi_t^w)\left(1-\frac{1}{\lambda_w}\right)=$	(4)
	$\Delta_t^{w_{nom}}\cdot\Phi_t^{\prime w}-\beta\left(\frac{C_{t+1}}{C_t}\right)^{-\tau}\frac{\Pi_{t+1}R_t}{\tilde{A}_{t+1}}W_{t+1}^2Y_{t+1}\cdot\Phi_{t+1}^{\prime w}$	
Price Equation, Intermediate Firms problem	$(1-\Phi_t^p)+\beta\left(\frac{C_{t+1}}{C_t}\right)^{-\tau}\Phi_{t+1}^{\prime p}\frac{\Pi_{t+1}}{\tilde{A}_{t+1}}\cdot Y_{t+1}\tilde{A}_{t+1}=\frac{\mu_t}{\Lambda_t}+\Phi_t^{\prime p}\Pi_t$	(5)
Hours Equation	$W_t=(1-\Phi_t^p)+\mu_t$	(6)
Adjustment Costs, Nominal Wages	$\Phi_t^w=\frac{\phi_w}{\psi_w^2}\left(e^{-\psi_w(\Delta_t^{w_{nom}}-\gamma\pi^*)}+\psi_w(\Delta_t^{w_{nom}}-\gamma\pi^*)-1\right)$	(7)
Adjustment Costs, Prices	$\Phi_t^p=\frac{\phi_p}{\psi_p^2}\left(e^{-\psi_p(\Pi_t-\pi^*)}+\psi_p(\Pi_t-\pi^*)-1\right)$	(8)
Derivative, Adjustment Costs Nominal Wages	$\Phi_t^{\prime w}=\frac{\phi_w}{\psi_w}\left(1-e^{-\psi_p\left(\Delta_t^{w_{nom}}-\gamma\pi^*\right)}\right)$	(9)
Derivative, Adjustment Costs to Prices	$\Phi_t^{\prime p}=\frac{\phi_p}{\psi_p}\left(1-e^{-\psi_p\left(\Pi_t-\pi^*\right)}\right)$	(10)
Taylor Rule	$R_t=\exp(r_t);\quad r_t=\rho_r r_{t-1}+(1-\rho_r)r_t^*+\sigma_r\varepsilon_r$	(11)
TFP Growth	$\tilde{A}_t=\exp(a_t);\quad a_t=(1-\rho_a)\log\gamma+\rho_a a_{t-1}+\sigma_a\varepsilon_a$	(12)
Government Spending Shocks	$G_t=\exp(g_t);\quad g_t=(1-\rho_g)\log g^*+\rho_g g_{t-1}+\sigma_a\varepsilon_g$	(13)
Price Markup Shock	$\Lambda_t=\exp(\lambda_t);\quad \lambda_t=(1-\rho_p)\log\lambda_{p_{ss}}+\rho_p\lambda_{t-1}+\sigma_p\varepsilon_p$	(14)
Output change	$\Delta_t^y=Y_t/Y_{t-1}$	(15)
Nominal wage change	$\Delta_t^{w_{nom}}=\Delta_t^w\Pi_t$	(16)
Interest rate target	$r_t^*=\log\left(\frac{\gamma}{\beta}\cdot\pi^*\right)+\psi_1\left(\pi_t-\log\pi^*\right)+\psi_2\left(\Delta_t^y+a_t-\log\gamma\right)$	(17)

Technical Details of Estimation

- The priors are largely from Aruoba et al. (2017). Because of the difference in sample period, I scaled down the priors for annualized output growth (μ_y) and inflation (μ_π), as well as β. In fact, it's actually not possible for this model to generate a steady state that matches the data. Steady state interest rates are $\mu_y/4+\mu_\pi+400(\beta^{-1}=1)$. If μ_y and μ_π are picked to match inflation data, you must pick $\beta>1$ to match interest rate data.

- Related^, the authors start the M-H algorithm at the mode of the linear model and then manually append starting values for the asymmetry parameters and fix the diagonals of the inverse hessian at 4. Asymptotically, there's nothing wrong with this, but I'm going to play around with this to see how sensitive the algorithm is to initial values and priors. From what I've done so far, it seems like there's sensitivity on both fronts. One key point is that when estimating the linear mode, they include ψ_w, which is not identified in a first order estimation. I'm also digging further into the particle filtering procedure.

- A running "diary" of some things I've discovered working with this model/codes can be found here
- For consistency in the comparative static illustrations, it was necessary to make sure this mode line was the same across plots, but that meant the same series of shocks would need to be used for all sets of simulated data, which could paint a misrepresentative picture for a short sample size. So I plotted the median estimate for 100 samples for each parameter group (for simulation i, seed was set to $i = \{1, \ldots, 100\}$).
  - It'd be nice to do this for the Bayesian IRFs, but that would take a month to run. For these, I even had to be parsimonious with what I did – because each loop of the local projections file performs 25*number of outcome variables calls to regress, I randomly selected 10,000 draws of the post burn-in M-H output.

Additional Sensitivity Concerns

A prerequisite to ensure the same shock series can instrument multiple regimes is including the regimes in instrument construction. This ensures that the same shock series can instrument multiple regimes. In other words, this extension results in 4 instruments which each contain mostly 0 values. Recent work by Barnichon and Mesters (2024) shows this is potentially problematic because of the short sample size. Specifically, it makes our approach more sensitive to the possibility that our instruments are correlated with other structural shocks. This is not a problem if there is no "central bank information effect", in other words if people don't act as if the central bank has superior information about the underlying economy. The debate on this issue is ongoing (see, e.g., Acosta, 2023), but the takeaway is whatever validity exists for this criticism is compounded by our chosen approach. Other than using an aforementioned non-parametric approach, another option could be committing to using different instruments for each regime. This alternative is plagued by the fact that (i) even if these shock series are less correlated than they probably should be (Brennan et al., 2024), the remaining collinearity could still be an issue and (ii) more practically, we don't have a unique strong instrument for all 4 regimes. With these drawbacks in mind, this approach still shows the core results hold, albeit weakened in essentially all cases. Another alternative is using one monetary policy surprise series to instrument changes in the fed funds rate in general and then attaching the regime indicators afterwards (see, e.g., Wooldridge, 2002). The drawback of this approach is it somewhat violates the spirit of treating the regimes as fundamentally different and relaxing linearity when possible. Here, the results are largely hold across various surprise series, with the exception that there is more evidence that big cuts have more expansionary effects on output on impact.

Jarocinski and Karadi (2020) and Acosta (2023) present evidence that when using high frequency monetary policy surprise measures, it may be important to not assume that Fed announcements only affect people's beliefs about interest rates, i.e., they argue it's important to account for a "Fed information effect". Koo et al. (2024) are the first to document formally how this affects inference when using impulse responses generated by LP-IV. The monetary policy surprise literature has yet to reach a consensus on several key issues (Acosta, 2023; Brennan et al., 2024), crucially including how justifiable different identification strategies are across the array of shock series. Future work will seek to address these concerns, including a procedure that bootstraps the difference between a purely linear specification and ours. Otherwise, this adds to the list of reasons high-frequency shocks are not as reliable as instruments. Jacobson et al. (2024) presents important work on the importance of temporal aggregation bias – the idea that IRFs can be skewed based on differences in measurement between a high frequency shock and a low frequency variable. Adding a control for the day of the month when a Fed announcement occurred did not affect the results, but because this is a relatively new critique, it's still unclear the best way to guard against its presence. One possibility could be adding high frequency asset prices as an outcome variable to have a better sense of the extent of information and related fast-moving transmission effects.

Comparative Static Description

To make efficient use of space, the exact figures are relegated to the very end of the Appendix, but there are hyperlinks to each. Again, what we learned from this exercise is that any sort of nonlinearity desired can be generated on impact using the right combination of asymmetry parameters, but it does not last for even one period longer in most cases.

Size Effects

	Description	**Anything Interesting? (all at $h = 0$)**	**Link**
1	$\psi_p \uparrow$	(slightly) amplifies negative size effect for hikes on π at $h = 0$	Figure 6
2	$\psi_p \downarrow$	(slightly) amplifies all $h = 0$ size effects except for hike on π	Figure 7
3	$\psi_w \uparrow$	(slightly) increases the positive size effect for cuts on Y at $h = 0$	Figure 8
4	$\psi_w \downarrow$	No.	Figure 9
5	$\psi_p \uparrow$, $\psi_w \uparrow$	amplifies size effect (-) of cuts on Y, depresses size effect (+) of hikes on Y	Figure 10
6	$\psi_p \downarrow$,$\psi_w \downarrow$	(slightly) amplifies negative size effect for hikes on π at $h = 0$	Figure 11

Sign Effects

	Description	Anything Interesting? (all at $h = 0$)	Link
1	$\psi_p \uparrow$	depressed all $h = 0$ sign effects except for small changes on π	Figure 12
2	$\psi_p \downarrow$	low values reversed the direction of the sign effect for big changes on π.	Figure 13
3	$\psi_w \uparrow$	Depresses small change on Y size effect and amplifies everything else	Figure 14
4	$\psi_w \downarrow$	(slightly) amplified sign effect of big changes on π and small changes on Y	Figure 15
5	$\psi_p \uparrow$, $\psi_w \uparrow$	depressed sign effect of small changes on Y and amplified everything else	Figure 16
6	$\psi_p \downarrow$,$\psi_w \downarrow$	reversed the direction of sign effect for big changes on π	Figure 17

Specification Choice and Implicit Weighting schemes

Consider the local projection setup (LP, not LP-IV) with the specified regimes. An equivolent specification is

$$y_{t+h} = \alpha_{1,h} s_t + \alpha_{2,h} |s_t| + \left(\alpha_{BC,h} R_{BC} + \alpha_{BH,h} R_{BH}\right) s_t + \beta_h X_t + \epsilon_{t,h}$$

In other words, this setup of using $x, |x|$ and indicators for the magnitude of x will deliver the same impulse response function as specifying four regimes (e.g., small hike coefficient is $\alpha_{1,h} + \alpha_{2,h}$). However, this is not the case for LP-IV because the instrumentation structure must be completely different. If we consider a first stage analogous to the above structure (replacing s_t with an instrument z_t), the regimes are still defined with respect to s_t. For our application, this seemingly subtle change makes for massively worse instrumenting to the point implementation is not an option[14], but as discussed in the next subsection, there may still be an appeal to this method in similar settings.

All this detail is only relevant under the umbrella of a larger question: what are we estimating and how does it change with specification? The representation of regressions as a weighted average of (average) marginal effects has been well established, but recently a lot of attention has been paid to the weights themselves and their potential to distort parameter estimates. In particular, Caravello and Martínez-Bruera (2024) makes the point that even when indicator functions are used, non-trivial weight can still be placed where the indicator is 0. For instance, consider an extension of a DGP they use as an illustration

$$y_t = \varepsilon_t^d, \ \pi_t = c(y_t) + \beta \mathbb{E}_t[\pi_{t+1}] + \varepsilon_t^s \quad \text{where } \varepsilon_t^d \sim \mathcal{U}[-b, b], \varepsilon_t^s \sim \mathcal{N}(0, \sigma^2), c(y) = \begin{cases} \kappa y^2 & \text{if y} > 0 \\ 0 & \text{o.w} \end{cases}$$

A regression of π on $y \mathbb{1}_{y>0}$ and $y \mathbb{1}_{y>0}$ will not produce coefficients that represent the average marginal effects

[14] One of the reasons: in a regression on the funds rate, the coefficients on the predicted values for R_{BC} and R_{SC} have the wrong sign.

conditional on the indicators (κb and 0, respectively). We can repoduce and extend their Lemma 1 to formalize this. Without loss of generality, assume the relevant shock process ε has bounded support $[\underline{\varepsilon}, \overline{\varepsilon}]$. Then if x_i is the ith element of covariate matrix X and is some function of ε, its corresponding local projection estimand is given by

$$\beta_i = \int_{\underline{\varepsilon}}^{\overline{\varepsilon}} \omega_i(a)\mathbb{E}[g_\varepsilon(a, \bullet)]da \quad \text{where} \;\; \omega_i(a) = \frac{\text{Cov}(\mathbf{1}_{\{a \leq \varepsilon_t\}}, x_i^\perp)}{\text{Var}(x_i^\perp)}, \; x_i^\perp \equiv x_i - \hat{x}_i = x_i - (X'_{-i}X_{-i})^{-1}X'_{-i}x_i$$

under some mild regularity conditions. The connection to impulse response functions can get muddied by this weighting function. We first assume our outcome variable follows are arbitrary data generating process $g(\varepsilon, \bullet)$. Let $g^h(\cdot)$ represent the (potential) outcome variable h periods after t. Then the impulse response to a shock of size δ is $\mathbb{E}[g^h(\varepsilon + \delta, \bullet) - g^h(\varepsilon, \bullet)]$. By the fundamental theorem of calculus, the LP estimand captures a clearly relevant object for impulse responses but features two important departures: the weighting function and integrating over the entire support (instead of ε to $\varepsilon + \delta$). In other words, the LP estimand is a weighted average of marginal effects. Returning to the example DGP, the weights don't look like what we would "want them to" ex-ante: ideally, if our function involves an indicator, the weight will be 0 whenever the indicator is not active. Unfortunately, that's not the case and even more problematic is that weights on non-linear functions must place *negative* weight on parts of the support. The best we can do is compute the weights and choose our specification to have most of the weight on the part of the function we care about. However, this can potentially open a path for an advantage of IV. While negative weights in IV are also a potential issue, it's *possible* to avoid them. Whereas for a regular LP setup with nonlinear functions, the nonlinear functions must have some negative weights by construction.

It should also be noted that we are asking the first stage to essentially predict treatment. This is why Angrist et al. (2016) opts for an inverse probability weighting approach, but as mentioned this is not feasible given the overlap assumption. In some settings it's possible to estimate treatment effects using a sub-sample where overlap holds. In our case, this would at a minimum eliminate 2008-2016, completely insurmountable since that would wipe out more than a quarter of our already short sample.

Comparative Static

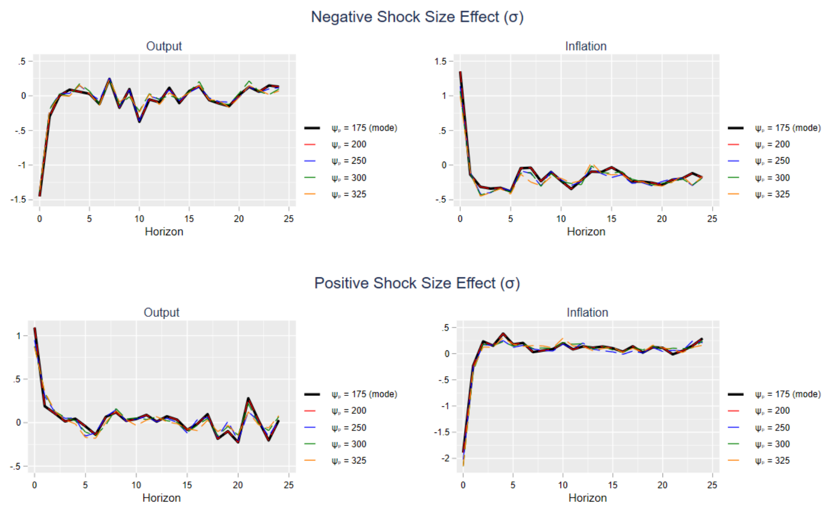
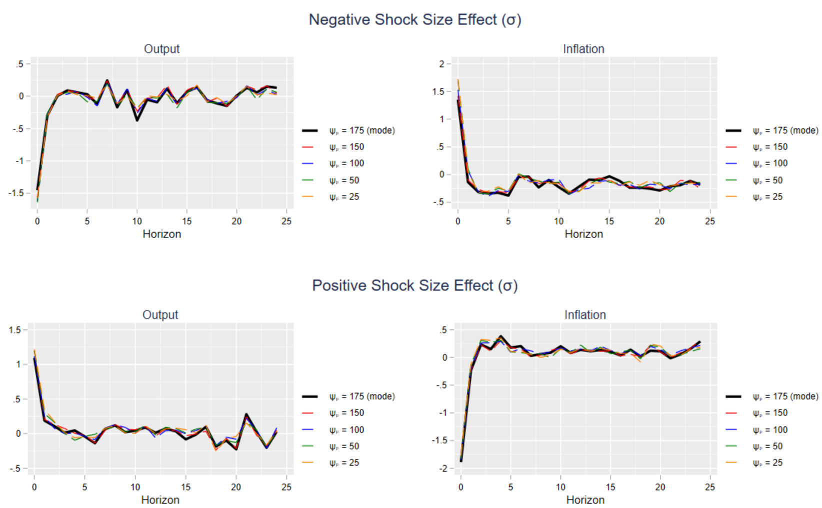


Figure 6: (click to go back to tables)



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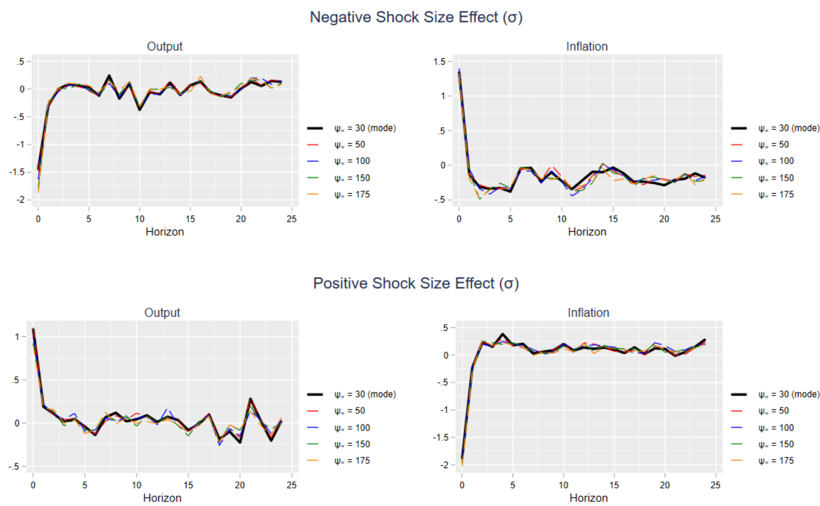

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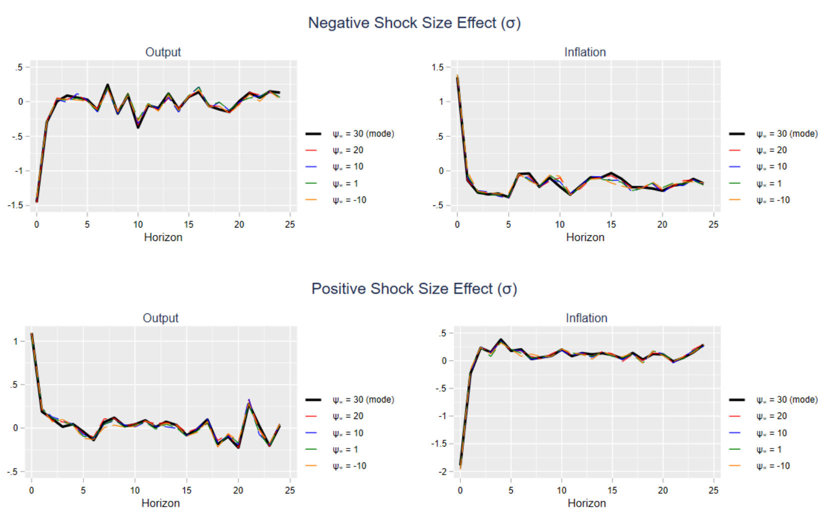

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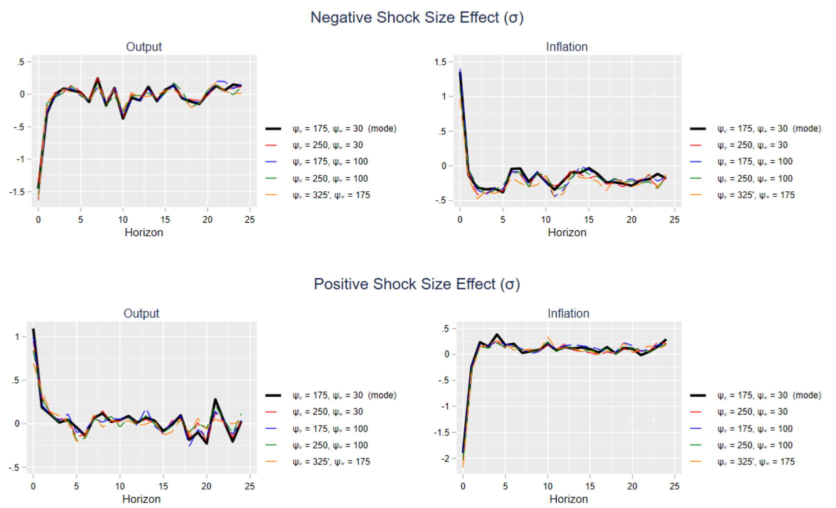

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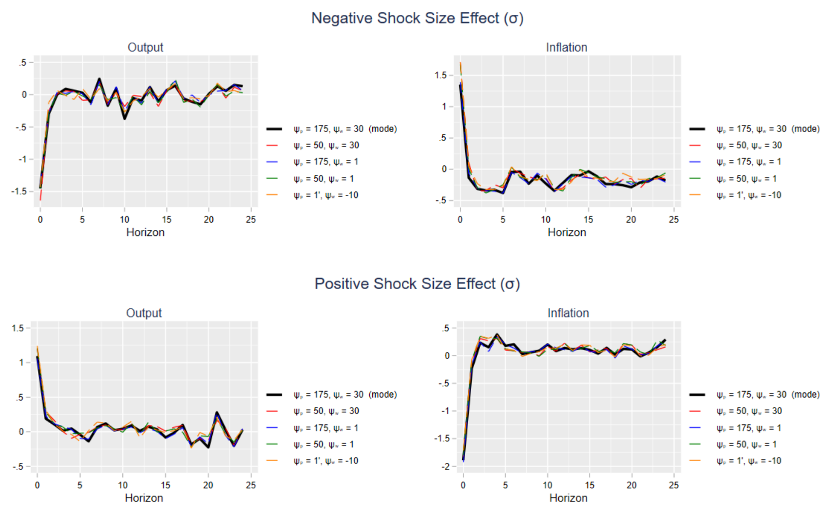

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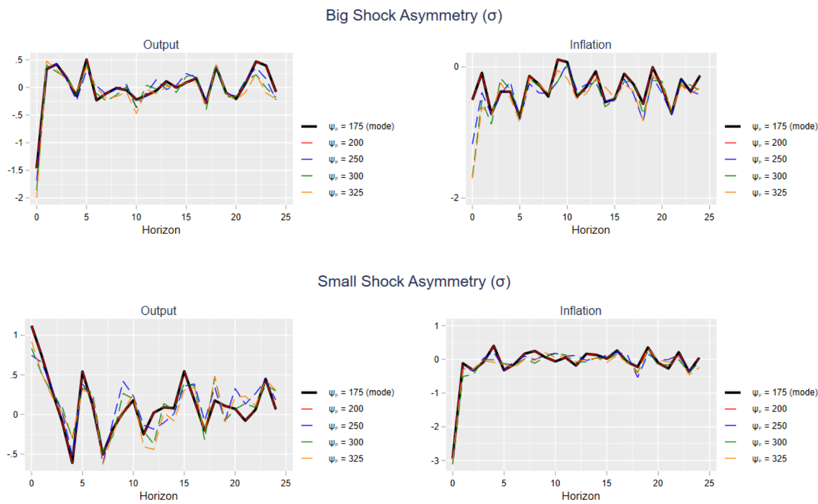

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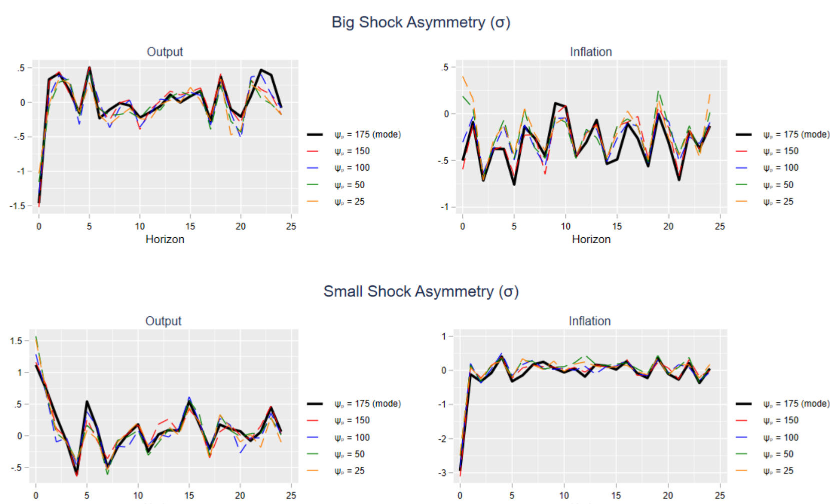

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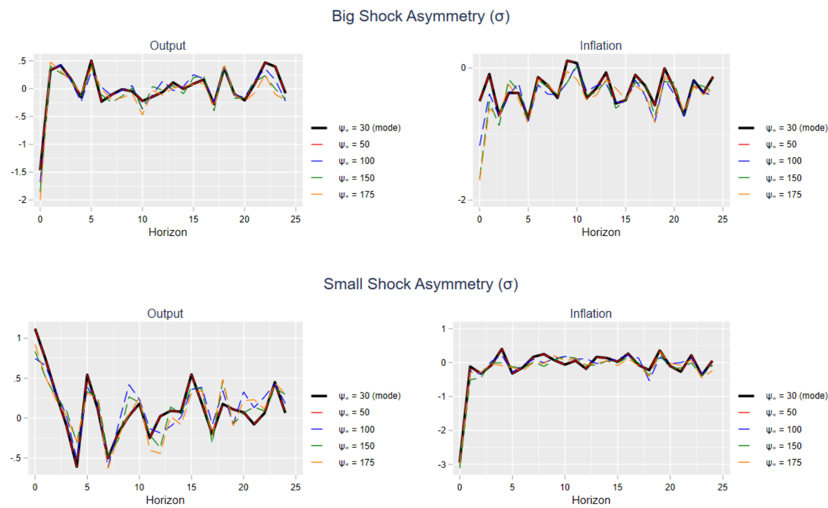

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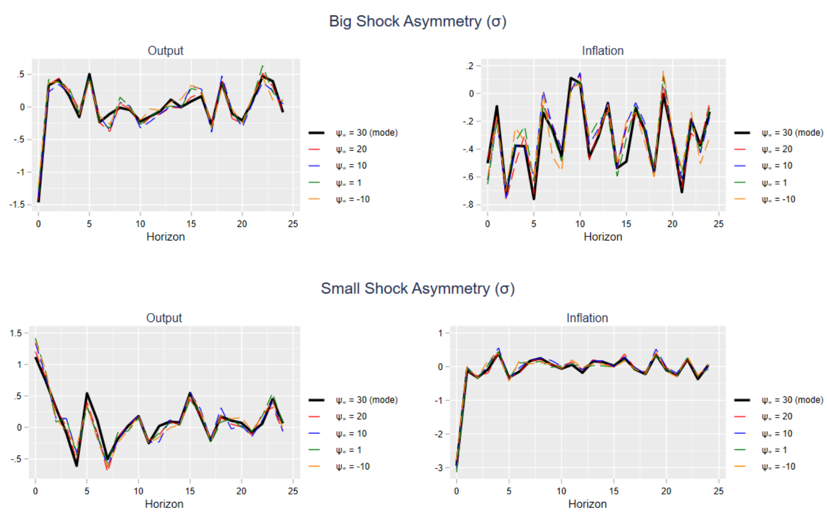

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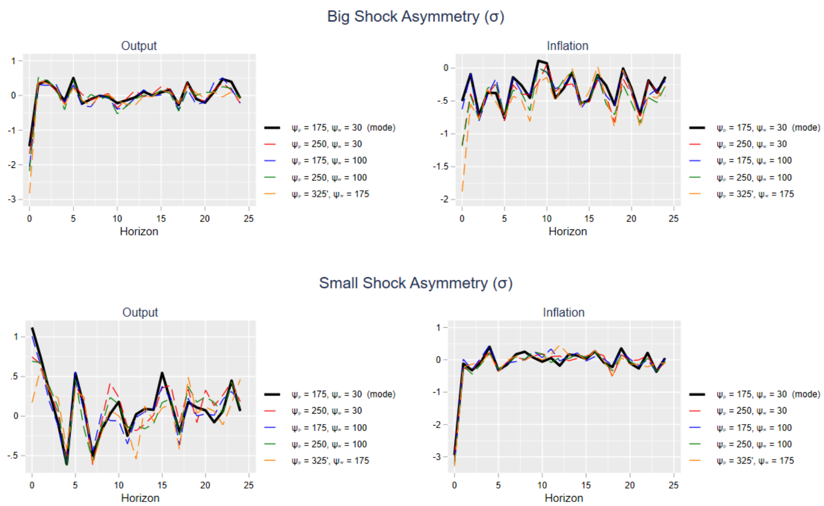

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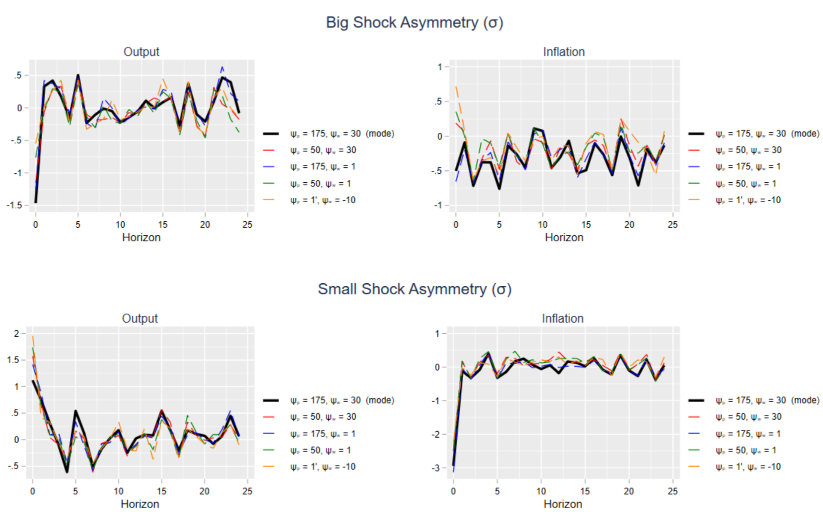

Figure 17: (click to go back to tables)