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## The overstated effects of conventional monetary policy on output and prices

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**September 2025**

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# The overstated effects of conventional monetary policy on output and prices\*

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**Abstract.** We build a dataset of output and price effects of conventional monetary policy containing 146,463 point estimates and confidence bands from 4,871 impulse-response functions in 409 primary studies. Simple average responses suggest that interest rate hikes substantially dampen output and prices. However, we find robust evidence for publication bias. Bias corrections reduce effect sizes by half or more: in response to a 100 basis points rate hike, output and prices are unlikely to fall by more than 0.5 and 0.25 percent, respectively. Shock identification choices and publication characteristics correlate with effect sizes but are quantitatively less important than publication bias.

**Keywords:** Meta-analysis; conventional monetary policy; interest rate; output; inflation; price level; publication bias.

**JEL classification:** C83, E32, E43, E52.

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## 1. Introduction

After a long period of close to zero central bank policy rates in many advanced economies, the recent resurgence of inflation has put conventional monetary policy back on the agenda. How output and prices respond to changes in central bank policy rates is one of the most studied questions in applied macroeconomics. Estimates of the macroeconomic effects of monetary policy shocks are important when it comes to informing monetary policy decisions and evaluating macroeconomic theories and models. A cautious reading of existing studies suggests that the reported results vary considerably. Although most empirical results are consistent with the qualitative theoretical prediction that contractionary (expansionary) monetary policy leads to a hump-shaped decline (increase) in economic activity and inflation (Eichenbaum 1992; Christiano et al. 1999; Nakamura and Steinsson 2018a; Arias et al. 2019; Wolf 2020; Miranda-Agrippino and Ricco 2021), there is disagreement with regard to the strength, the transmission time, and even the initial sign of the effects (Bernanke and Mihov 1998; Cochrane 1998; Uhlig 2005; Olivei and Tenreyro 2007; Cloyne and Hürtgen 2016; Bu et al. 2021). A central concern in the literature refers to the identification of truly exogenous monetary policy shocks (Ramey 2016; Nakamura and Steinsson 2018b; Plagborg-Møller and Wolf 2021). Existing surveys highlight different data and modeling choices, in particular when it comes to shock identification strategies and estimation methods (Coibion 2012; Sims 2012; Ramey 2016).

This paper presents the first comprehensive meta-analysis of the responses of output and prices to conventional monetary policy shocks.<sup>1</sup> To our knowledge, our study is the first pre-registered meta-analysis in economics that is based on an extensive pre-analysis plan (PAP).<sup>2</sup> We build and analyze a novel dataset comprising 409 relevant primary studies that report 4,871 impulse response functions (IRF) from which we extract 146,463 point

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<sup>1</sup>De Grauwe and Costa Storti (2004) provide an early quantitative synthesis of 43 primary studies of output and price effects of conventional monetary policy, but the paper remained in draft state and did not apply rigorous meta-analysis techniques. Nguyen et al. (2021) focus on a small sample of 45 emerging and developing market studies and focus on output effects.

<sup>2</sup>The PAP was published in April 2023 and is available via: <https://osf.io/cduq4>. It lays out our search strategy, data collection, data processing, hypotheses, and regression techniques. PAPs contribute to reducing the selective reporting of research findings (Brodeur et al. 2024a; Christensen and Miguel 2018).

estimates and confidence bands for a wide range of countries, time periods, estimation and identification methods, as well as additional study characteristics.

Our research questions are: (1) What does the synthesized empirical evidence suggest about the sign, strength, and transmission time of the effect of conventional monetary policy on output and prices? (2) Do *p*-hacking and publication bias affect the distribution of reported effects, thus distorting statistical inferences drawn from conventional literature syntheses? (3) How do data and modeling choices affect the findings, in particular when it comes to using different shock identification approaches?

*p*-hacking, the preference for statistically significant results, is widely recognized as an important issue in economics (Brodeur et al. 2016; Brodeur et al. 2023; Chopra et al. 2024; Elliott et al. 2022; McCloskey and Michailat 2024). More generally, publication bias, which includes *p*-hacking but also a tendency to prefer large effect sizes or to conform to theoretical expectations and seminal publications, may distort the conclusions of qualitative or simple quantitative reviews of the empirical evidence (Rosenthal 1979; Stanley 2001; Andrews and Kasy 2019).

Using the meta-analysis toolbox, we provide a quantitative synthesis of the literature, including the identification of and correction for *p*-hacking and publication bias. We use a set of established tests and correction methods for publication bias (Andrews and Kasy 2019; Ioannidis et al. 2017; Stanley 2008; Brodeur et al. 2020), account for sampling uncertainty, and propose a range of bias-corrected impulse-response functions for output and prices.

Figure 1 summarizes our main findings. It shows the synthesized responses of output, price level, and interest rate to a 100 basis point (bp) contractionary shock to the central bank policy rate. The simple average output response (left panel, blue line) is negative, initially small, and peaks at around  $-1$  percent after about 2 years. The average response of the price level (right panel, blue line) builds up more slowly and peaks at about  $-0.75$  percent after 4 to 5 years. The average interest rate response (lower panel, blue line) starts out at 1 percentage point and tapers out to zero after about 2 years. These average effect

Figure 1: Average responses to contractionary 100bp monetary policy shock and range of bias-corrected responses based on 4,871 IRFs



*Notes:* The figures show average responses for output and the price level (in percent), and the percentage point response for the interest rate (blue lines) after a contractionary 100bp shock to the monetary policy rate. These average responses, along with the blue-shaded average upper and lower 68% and 95% confidence intervals are derived from our meta sample of 4,871 impulse-response functions found in 409 primary studies. Additionally, the figures contain a range of publication bias-corrected output, price level and interest rate responses (green area). This range displays the 16th-84th and the 2.5th-97.5th percentile range of bias corrections from up to 42 meta-regression estimates for each response horizon, based on a combination of (i) seven variants of established methods to detect and correct for publication bias (Andrews and Kasy 2019; Ioannidis et al. 2017; Stanley and Doucouliagos 2014; Stanley 2008) and (ii) 6 levels of winsorization of outliers in our sample (from 0 to 5 percent). The mean of this range is shown as the dark green dashed line. Note that the entire green areas should be considered in relation to the simple average responses (blue lines). They are not directly comparable to the simple average confidence intervals (blue-shaded areas) which reflect sampling uncertainty both between and within primary study estimates. See [Section 3.2](#) and [Section 4.5](#) for more details on the derivation of the simple average and the bias-corrected estimates.

sizes are consistent with an assessment requested from a leading artificial intelligence (AI, ChatGPT).

The average confidence intervals for output and prices (light blue-shaded areas) are wide and comprise a zero effect for the 95 percent level throughout the response horizons. However, note that these average confidence intervals are rather conservative as they reflect uncertainty within and between all primary study estimates. The 68 percent confidence intervals do not contain zero for the peak responses of output and prices.

However, we show that these average responses of output and prices are likely plagued by *p*-hacking and publication bias, leading to inflated effect sizes. We document a robust pattern of selective reporting of statistically significant dampening effects of contractionary monetary policy shocks on output and prices, in particular at the most relevant response horizons. The corrected estimates are smaller and in a range of +0.05 to −0.75 percent for output and between +0.1 to −0.5 percent for the price level (2.5th-97.5th percentile range, light green-shaded areas). The corrected mean IRF for output peaks at −0.25 percent after 1 to 2 years and for the price level at −0.15 percent after 4 years (green dashed lines). Output and prices are unlikely to fall by more than 0.5 percent and 0.25 percent, respectively (minimum peak effect of the 16th-84th percentile range of bias corrections).

Notably, applying the same methods of detecting and correcting for publication bias to interest rate responses results in almost no correction. We consider this null finding a useful placebo test in applying our methods. The interest rate response is typically reported in primary studies without much notice, and we would not expect publication selection on this variable.

In a heterogeneity analysis, we investigate how study and estimation characteristics such as identification strategies, samples, author affiliations, and journal ranking are related to the variation of reported effect sizes. With respect to the countries in our sample, the differences between advanced economies are small, while the average output and price effects are clearly weaker in emerging market economies. Authors affiliated with central banks report effect sizes that do not differ much from those without a central bank affiliation. Estimates published in top-tier journals clearly show larger output and price effects on average, but publication bias also seems to be stronger there. The choice of shock identification approaches matters: using high-frequency identification results in stronger output effects on average; employing the traditional Cholesky/SVAR identification tends to produce a “price puzzle” over the first year and small negative responses afterwards, while sign restrictions and high-frequency approaches tend to deliver stronger dampening price-level effects at short and long response horizons.



However, we find that shock identification choices are quantitatively less important than publication bias. For any of the identification approaches, the corrected IRFs are closer to zero than the simple uncorrected average IRFs. When using a selected sample of only top-tier publications and excluding Cholesky-identified models, the mean of bias-corrected estimates peaks at  $-0.45$  for output and  $-0.3$  percent for the price-level response, well within the range of the full sample. The average corrected effect sizes for high-frequency identifications (the most sophisticated and recent approach) are relatively strong for the output response with a peak at around  $-1$  percent, but are in line with the full-sample corrected mean effect of  $-0.15$  percent for the price level. There is robust evidence for selective reporting of research findings, regardless of subsampling and including or excluding a long list of possible confounders, both in frequentist and Bayesian model selection approaches.

In sum, our findings suggest that the power of conventional monetary policy to steer prices and the business cycle may have been overstated in the past, based on a simple average assessment of the empirical literature, seminal empirical studies in leading journals, the predictions of standard New Keynesian models and a summary given by a leading AI. We provide comprehensive new evidence for the skeptical conclusion in Ramey (2016) that the macroeconomic effects of conventional monetary policy shocks are uncertain and likely small. In addition, the sacrifice ratio of output losses to price-level reductions according to the respective peak effects is 1.3 for uncorrected mean effects but increases to about 1.7 after correcting for publication bias.

*Related literature.* Our paper contributes to the literature on  $p$ -hacking and publication bias in economics (Andrews and Kasy 2019; Brodeur et al. 2016; Brodeur et al. 2023; Campbell et al. 2024; Elliott et al. 2022; Gechert et al. 2024; Stanley et al. 2021). Inflated effect sizes, excess statistical significance, and insufficient reproducibility are recurring themes in this literature. Other examples of recent applied meta-analyses documenting publication bias include Brown et al. (2024), Havránek et al. (2024), Gechert and Heimberger (2022), and Imai et al. (2021). Our study follows standard guidelines for meta-analysis in economics (Havránek et al. 2020; Irsova et al. 2023b) and extends these standards through

a pre-registration combined with a comprehensive pre-analysis plan and documentation of data collection and analysis. By providing comprehensive documentation of how we constructed the data set and how we coded the various steps of the analysis, we ensure that our results can be reproduced in full, following the literature on the importance of reproducibility for the credibility of empirical research (e.g. Dreber and Johannesson 2025; Brodeur et al. 2024b; Christensen and Miguel 2018).<sup>3</sup>

In addition, we address the literature that synthesizes the macroeconomic effects of monetary policy. Ramey (2016) provides a qualitative review with a focus on shock identification. Rusnak et al. (2013) meta-analyze the price puzzle of monetary policy while Havránek and Rusnak (2013) consider transmission lags on prices with similar techniques. Ehrenbergerova et al. (2023) study the effects of monetary policy on house prices using a meta-analysis. Bhattarai and Neely (2022) analyze the international literature on unconventional monetary policy, and Papadamou et al. (2019) provide a meta-analysis of this strand. Fabo et al. (2021) focus on the macroeconomic effects of Quantitative Easing and the possible influence of author affiliation on reported results.

Our meta-analysis results may serve as a reference for developers of applied monetary policy models who want to match and compare their results to a comprehensive basis of empirical estimates. For the Euro Area, Lane (2023) compares the outcomes of how rate hikes affect output and inflation for several structural and semi-structural models. Del Negro et al. (2013) develop and test an applied DSGE model for the United States, while Acharya et al. (2023) compare the empirical fit of Heterogeneous Agent New Keynesian (HANK) models to standard DSGEs. Auclert et al. (2020) also develop an applied HANK model that is meant to match micro and macro stylized facts of monetary policy shocks. Our peak effects are smaller and imply longer transmission lags than predicted by standard representative agent New Keynesian models. They fit better to the muted output and price responses of semi-structural models and applied HANK models.

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<sup>3</sup>The documentation and replication files are available via: <https://github.com/META-CMP/data>.

The remainder of the paper is structured as follows. [Section 2](#) reviews the most important theoretical transmission channels and empirical issues in the literature. [Section 3](#) explains the construction of our dataset. [Section 4](#) presents the analysis on  $p$ -hacking and publication bias. [Section 5](#) entails the heterogeneity analysis with a focus on the role of shock identification strategies, publication outlets, author affiliation, and country specifics. The final section concludes.

## 2. Main lessons from the literature

### 2.1. Theoretical transmission channels

Standard New Keynesian theory posits that output and prices respond strongly negatively to contractionary conventional monetary policy shocks with relatively short lags. The output then quickly returns to its baseline, while the price level is permanently reduced (Galí [2015](#)). The main transmission mechanism runs through intertemporal substitution and changes in aggregate demand that affect both output and prices in the same direction (Bank of England [1999](#); European Central Bank [2022](#)). A rise in the policy rate tightens both short and long-term market interest rates (through the yield curve) and thus encourages saving over current domestic consumption, which hampers credit-financed investment (Gertler and Karadi [2015](#)). Higher rates also reduce asset prices, lowering consumption spending through wealth effects and investment spending through Tobin's  $q$ . Reduced aggregate demand ultimately reduces inflationary pressures on goods and labor markets.

Monetary policy may also be transmitted through the exchange rate channel. A contractionary monetary policy shock should lead to an appreciation of the domestic currency, resulting in lower import prices. These should reduce the demand for domestic goods, increase imports, and lower cost-push inflation (Walsh [2017](#); Mishkin and Serletis [2022](#)).

The power of these mechanisms can be enhanced by anchoring expectations via credible central bank actions (Rudd and Whelan [2007](#)). However, the effect of a rate hike on markets may not be straightforward as it can signal both news about the macroeconomic outlook

of the central bank and a change in its future policy stance (Andrade and Ferroni 2021; Miranda-Agrippino and Ricco 2021).

HANK models have recently questioned the standard transmission mechanisms of monetary policy, highlighting the reduced role of intertemporal substitution effects on consumption when some agents do not own liquid wealth, face credit constraints, or follow a precautionary saving motive (Kaplan et al. 2018). In general, expectations play a subdued role in transmission (McKay et al. 2016). However, stronger current income effects of monetary policy on constrained consumers may compensate for this effect such that the reduced-form macroeconomic effects can be quite similar to a standard New Keynesian model (Auclert et al. 2024; McKay and Wolf 2023). The strength and transmission time may vary depending on the economic conditions and the effectiveness of the monetary policy tools.

## 2.2. Empirical findings

*Effect sizes and dynamics.* What does a first look at seminal empirical papers reveal? First, output and inflation responses are rather hump-shaped: they take longer to build up and peter out than in stylized calibrated models. For example, in Christiano et al. (1999) output falls by around 0.5 percent after 6 quarters and then returns to the baseline while the aggregate price level reaches its peak at approximately  $-0.4$  percent only after 15 quarters. In Romer and Romer (2004) the negative output response to a 100bp contractionary monetary policy hike peaks at around  $-4$  percent after about two years and then recovers slowly. Prices remain roughly unchanged for 22 months, but decrease substantially afterward, with a peak dampening effect on the CPI level of about  $-6$  percent after 4 years. Gertler and Karadi (2015) report industrial production to fall by  $-0.4$  percent after 2 years following a 25bp rate hike and that the CPI declines by  $-0.1$  percent after 40 months. Extended models with inertia and investment adjustment costs can partly account for a slower transmission, but in general the empirical transmission time is still much longer than standard models would project.

Moreover, many early influential studies (Sims 1992; Christiano et al. 1999; Romer and Romer 2004) feature a “price puzzle” where prices initially increase in response to a contractionary monetary policy shock. Some studies refer to a similar “activity puzzle” for output. Several studies suggest that the price puzzle is due to omitted variables. For example, Sims (1992) does not observe a price puzzle when including a commodity price index, arguing that commodity prices contain important information on future inflation. Bernanke et al. (2005) includes several factor variables to avoid omitted variable biases, thus solving the price puzzle. Jarocinski and Karadi (2020) use a high-frequency monetary policy shock purged of information content from monetary policy announcements and show that the price puzzle can be eliminated in some cases. Rusnak et al. (2013) present a meta-analysis on the price puzzle and find that it can be explained away by including commodity prices and the output gap, and by avoiding recursive identification.

*Explanatory factors.* After all, empirical studies differ strongly in their quantitative or even qualitative assessment of the output and price effects of conventional monetary policy. Why might this be the case? In addition to obvious white noise in estimation, studies systematically differ in terms of data and methods (Coibion 2012; Sims 2012; Ramey 2016). The definition of the main variables (the interest rate, the output, and the price level measure) may also be a part of the explanation. With respect to sample choices, the effects likely depend on where (currency area, region) and when (time period) the monetary policy changes take place; for example, a stronger institutional setting and larger and well-developed domestic financial and goods markets may lead to a more effective transmission mechanism. While most researchers study US monetary policy and the Euro Area, there is also a considerable literature focused on other advanced economies and on emerging market economies, sometimes employing multi-country panel data. However, even US studies featuring the same period partly report divergent results.

*Estimation techniques.* Most empirical studies use impulse response functions (IRFs) to estimate the dynamic effects of monetary policy shocks, allowing researchers to analyze the timing, persistence, and magnitude of the effects. The estimated parameters of vector

autoregression (VAR) models are the most popular basis for calculating IRFs (Sims 1980; Baumeister and Hamilton 2018; Jarocinski and Karadi 2020). IRFs can also be directly derived from local projections (LP) (Jordà 2005). This method has recently been used more widely in the monetary policy literature (Nakamura and Steinsson 2018b; Plagborg-Møller and Wolf 2021). Ramey (2016) shows that LP and VAR models can lead to substantially different outcomes even with the same underlying shock series. VAR models are sometimes extended to factor-augmented VARs (FAVAR), including multiple potentially relevant factor variables to reduce the risk of omitted variable bias (Potjagailo 2017). Longer time series have also facilitated the use of time-varying parameters (Castelnuovo and Surico 2010). Global VARs capture the interconnectedness of multiple economies (Georgiadis 2015). Data-driven dynamic stochastic general equilibrium (DSGE) models put additional structure on the behavior of the variables, but can also be used to estimate the effects of monetary policy for a certain economy (Smets and Wouters 2007).

*Identification.* The identification of truly exogenous shocks of monetary policy is crucial in distinguishing the effects of monetary policy on the economy from other shocks. In addition, given the significant role of market expectations about future policy, many studies account for the anticipation of monetary policy actions when identifying these shocks. As a result, the empirical literature has used a variety of identification assumptions. The recursiveness assumption (Cholesky decomposition) and other contemporaneous restrictions in structural VARs (SVAR) were the most widely used in the earlier years of the empirical literature. They are based on the idea that monetary policy reacts relatively quickly to changes in output and inflation but that these variables will only respond to policy rate changes with a delay (Christiano et al. 1999; Auer 2019). In a different approach, theory-guided sign restrictions pre-define the sign of contemporaneous and lagged responses of selected variables to trace out what is a monetary policy shock (Uhlig 2005; Rafiq and Mallick 2008).

The narrative identification approach uses documents and records from central bank meetings and press conferences to separate discretionary policy rate changes from reactive and anticipated movements (Romer and Romer 2004; Romer and Romer 2023). The high-

frequency approach takes advantage of short-term movements in financial market data in a narrow time window around monetary policy announcements (Nakamura and Steinsson 2018a; Cesa-Bianchi et al. 2020). These are typically accompanied by forecast error approaches, which measure the discrepancy between what policy makers actually do and what forecasters expected them to do (Andrade and Ferroni 2021).

There are further approaches that are less widely used, such as imposing restrictions on the long-term relationships of key macroeconomic variables (Shapiro and Watson 1988; Blanchard and Quah 1989), or exploiting patterns of heteroskedasticity in the VAR residuals to infer the timing and magnitude of monetary policy shocks (Normandin and Phaneuf 2004; Brunnermeier et al. 2021). Recent studies tend to combine several identification assumptions, for example, using external narrative instruments with forecast errors and high-frequency data (Miranda-Agrippino and Ricco 2021) or identifying monetary policy shocks from high-frequency data and sign restrictions (Cieslak and Schrimpf 2019; Jarocinski and Karadi 2020).

In summary, the dominant prediction about conventional monetary policy shocks is that a rate hike will significantly reduce output and prices with considerable lags. However, a cautious review of the empirical evidence suggests that the results vary considerably and are partly contradictory. The choice of identification assumptions, estimation methods, samples, and variables can all affect the results. We provide the first comprehensive quantitative synthesis of this extensive literature using the meta-analysis toolbox (Stanley and Doucouliagos 2012; Andrews and Kasy 2019).

### 3. Dataset

#### 3.1. Search, collection and extraction

For data collection and analysis, we follow standard guidelines for meta-analyses in economics (Havránek et al. 2020). Collecting our final dataset involved the following steps: (i) selecting literature databases and defining search queries; (ii) determining inclusion/exclusion criteria for studies; (iii) choosing relevant study and estimation characteristics derived from

our hypotheses; (iv) screening titles and abstracts of the entries found; (v) conducting full-text screening to code all study and estimation characteristics; (vi) digitizing the IRFs and standardizing coefficients to output and price responses (in percent) after a 100bp conventional monetary policy tightening shock; (vii) performing quality and plausibility checks. A detailed outline of the search strategy, including search queries, inclusion / exclusion criteria, rules for data screening, the number of results, and the data management tools used, is available in the [PAP](#). [Appendix A.1](#) includes further information on the actual data collection process, including [Figure A.1](#), a flow chart of the steps involved, and the number of excluded and potentially relevant studies in each step. Our focus is on the effects of conventional monetary policy shocks (via interest rates), so that we do not consider the literature that investigates the effects of quantitative instruments (e.g. non-borrowed reserves, credit controls) more frequently used in the past (e.g. Monnet [2014](#)). The full documentation of the selection, coding and transformation of primary studies as well as replication files are available via <https://github.com/META-CMP/data>.

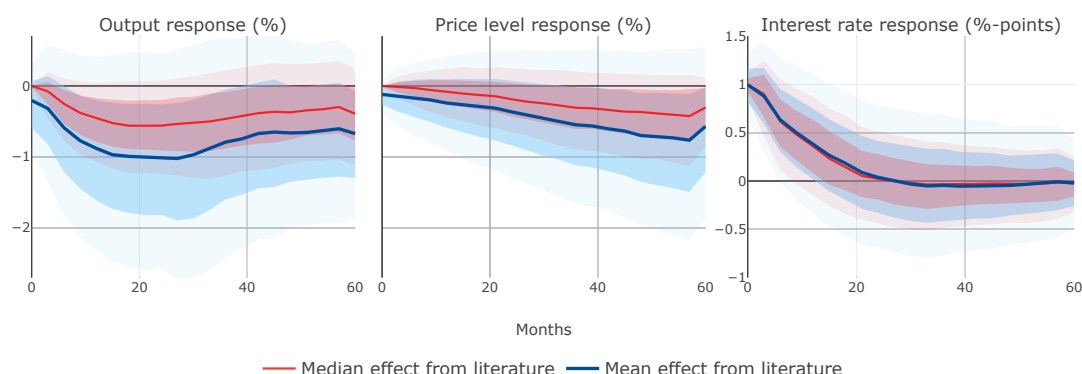
In total, full-text screening led to the inclusion of 409 primary studies that reported 4,871 graphical IRFs from which we extracted 146,463 point estimates and confidence bands. Averages and medians are provided in the next sub-section. Moreover, we collected information on a set of study, data and model characteristics, which are further explained below. [Table A.1](#) in [Appendix A.2](#) presents details on the number of observations.

### 3.2. Quantitative summary

[Figure 2](#) shows the average IRFs of output, prices, and interest rates after a 100bp conventional monetary policy shock based on our full sample. The point estimates (solid lines) are calculated at the mean and median based on all primary study point estimates per quarter. The confidence bands (shaded areas) are the standardized mean and median values of the respective upper and lower confidence intervals from the primary studies. To reduce the impact of potentially spurious outliers, we winsorize the collected estimates and their stan-



Figure 2: Mean and median output, price level and interest rate response to a conventional monetary policy shock



*Notes:* The figures show average (blue line) and median (red line) responses for output, the price level and the interest rate along with 68% and 95% confidence intervals (shaded areas) to a contractionary 100 basis points (bp) conventional monetary policy shock, based on the estimates collected from the primary literature. The point estimates are calculated as averages (mean and median) of all primary study point estimates per quarter. The confidence bands are the standardized averages (mean and median) of the respective upper and lower confidence intervals from the primary studies. Collected estimates and their standard errors are winsorized at the 2nd and 98th percentile.

dard errors at the 2nd and 98th percentile, which is standard in the literature (e.g. Zigràiova et al. 2021; Gechert and Heimberger 2022).<sup>4</sup>

The mean output response (left panel, blue solid line) is negative on impact but initially small, suggesting that there is no “activity puzzle”. The average response builds up and peaks around  $-1$  percent about 2 years after the shock. However, confidence bands (blue shaded areas) are wide. For the 95 percent confidence interval, we cannot reject a zero effect at all response horizons. The 68 percent confidence interval does not include zero for the response horizons between 6 months and 40 months.

The average price-level response (right panel, blue solid line) to a contractionary monetary policy shock is also negative on impact, that is, there is no “price puzzle”. However, the response builds up more slowly than the output response. The peak price-level response is  $-0.75$  percent after about five years. The kink that appears afterwards is partly due to a fall in the number of observations after a horizon of four years. Note that the price-level

<sup>4</sup>This symmetric 2 percent winsorization is our standard choice. In some cases, we use a broader range of winsorization levels from 0 to 5 percent to show the robustness of this choice.

response is not expected to return to zero according to standard theory. The 68 percent confidence interval does not contain a zero price response only after around 36 months.

As we show in [Appendix A.4](#), these average responses coincide with answers given by ChatGPT after requesting a summary of findings from the empirical literature on conventional monetary policy effects on output and prices. The AI claims that the point estimate for output should peak at around -1 percent after one to two years with an uncertainty range of -0.5 to -2 percent. The response of the price level should be around -0.3 to -0.6 percent after 12 to 24 months and fall more slowly afterwards.

The median responses of output and prices (red solid line and shaded areas) point to weaker dampening effects of contractionary monetary policy shocks at all response horizons. The median responses of the output and the price level are zero on impact and peak at horizons similar to the mean responses. However, the median effect sizes are only about 60 percent of the mean. This indicates that the distribution of output and price-level effect sizes is substantially left-skewed.

The average IRF for the interest rate starts at 1 percentage point and then returns to a value close to zero in the medium run. Notably, the mean and median responses of the interest rate variable are virtually the same, which points to a symmetrical distribution.

Simple averages of all reported estimates are useful when it comes to eliminating sampling error and random misspecification bias. The central limit theorem ensures that the average of a large number of estimates provides adequate estimates of ‘true’ effects, as long as sampling error and misspecification bias are random (Ioannidis et al. 2017). However, the desirable statistical properties of such meta-averages no longer exist if there is systematic publication selection bias (Havránek 2015; Andrews and Kasy 2019; Brodeur et al. 2020), an issue that we will focus on in [Section 4](#). The left skewness of the distribution of the output and price level responses may be a first hint of such selective reporting.

The primary studies in our sample may also yield varying effect sizes due to different modeling choices. [Section 5](#) explores variables that may explain differences between estimates. The next subsection introduces the relevant moderator variables.

### 3.3. Moderator Variables

The included studies and their estimates differ in various dimensions. We classified estimates using a pre-registered list of moderator variables. For the scope of this paper, these include details on impulse and response variables, identification strategies, estimation methods, sample characteristics, and publication characteristics related to author affiliations and publication outlets. The main moderator variables, along with some basic descriptive statistics, are summarized in [Table 1](#). Further characteristics are described in [Appendix A.3](#) and shown in [Table A.2](#).

Table 1: Description and summary statistics for baseline moderator variables

Variable	Explanation	Share in %/ Mean
<b>Identification approach</b>		
Cholesky/SVAR	Cholesky decomposition or SVAR restrictions	48.29
Sign restrictions	Sign restrictions of responses	18.51
High-frequency	High frequency information	12.79
Narrative	Narrative approach	12.31
Other identification	Other strategy (e.g. long-run restrictions, heteroskedasticity, DSGE)	8.10
<b>Publication characteristics</b>		
Top publication	Paper published in 12 top economics journals	13.63
Central bank	Central bank outlet or authors affiliated with central bank	54.35
<b>Sample characteristics: country groups</b>		
US	Estimates based on US data	47.51
Euro Area	Estimates based on Euro Area country data	16.20
Other advanced	Other advanced countries (e.g. UK, Japan, Canada, Australia)	25.44
Emerging	Emerging market countries	10.85

*Notes:* the column share / mean is in percent of the total. The total number of estimates for output, prices or the interest rate without quality concerns is 146,463 (see [Table A.1](#)). The shares above are based on quarterly values from IRF estimates within response horizons up to 5 years (81,697 estimates). The 12 top economics journals are selected based on the Scientific Journal Rankings (SJR) indicator. Further characteristics are shown in [Table A.2](#).

*Identification approaches* form an important dimension of clustering studies in [Table 1](#). The largest group, which represents about half of the effect sizes, features the traditional Cholesky and SVAR models that impose restrictions on the contemporaneous relations of the variables (Christiano et al. 1999). Sign restrictions (Uhlig 2005) make up about one-fifth of our observations. Shock series constructed from narrative identification methods (Romer and Romer 2004) and high-frequency approaches (Gürkaynak et al. 2005) have

become more popular in recent years; they represent approximately one-eighth of all observations each. Finally, around 8 percent of the observations belong to a mixed group of other identification approaches, including, for example, estimated DSGE models and VARs exploiting heteroskedasticity in the data. Note that the categories are designed to be mutually exclusive and jointly exhaustive such that each single observation belongs to exactly one identification scheme. In some studies, identification approaches are combined (e.g. sign restrictions + high frequency as in Jarociński and Karadi (2020)) and in such cases we assigned them to the more recently developed and more elaborate approach (in the above example this would be high frequency).

*Publication characteristics* represent another group of moderator variables. There is a dummy variable for studies published in a leading economics journal. We include all publications that belong to a potential list of top-50 economics journals according to the SCImago Journal Rank (SJR) indicator. In fact, 13 such journals are represented and about 13 percent of our observations belong to this group.<sup>5</sup> Studies in top journals might be more influential than others and the results may show stronger selective reporting patterns (Askarov et al. 2024). Furthermore, we code whether a paper was published in a series of major central bank working papers or whether one of the authors was affiliated with a central bank, which is the case for about half of our sample. Fabo et al. (2021) show that a central bank affiliation is associated with the reporting of larger expansionary effect sizes of quantitative easing.

*Sample characteristics* represent differences in institutions. As our main characteristic of interest, we collect the groups of countries and monetary areas (US, Euro Area, other advanced countries, and emerging market countries) on which the estimation is based. Close to 50 percent of our sample are estimates for the US, 16 percent are estimates for (members

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<sup>5</sup>We use the SJR from the year 2022. The 13 relevant journals for our sample are (in alphabetic order): American Economic Journal: Macroeconomics, American Economic Review, Brookings Papers on Economic Activity, Economic Journal, Journal of Business & Economic Statistics, Journal of Finance, Journal of Financial Economics, Journal of International Economics, Journal of Monetary Economics, Journal of the European Economic Association, Review of Economics and Statistics, Quantitative Economics, Review of Economic Studies.

of) the Euro Area, 25 percent for other advanced economies (e.g., UK, Japan) and the rest for emerging market economies.

## 4. *p*-hacking and publication bias

### 4.1. Theory and application to monetary policy effects

Publication bias may arise for two reasons: (i) a preference of researchers, referees and editors for statistically significant findings (*p*-hacking) (Brodeur et al. 2016; Brodeur et al. 2023; Chopra et al. 2024); (ii) a selection of results that conform to conventional wisdom, dominant economic theories, or findings of influential publications (Stanley 2005; Christensen and Miguel 2018; Andrews and Kasy 2019). In the case of output and price effects of monetary policy, standard theory predicts a negative, hump-shaped temporary response of output and a negative permanent effect on the price level. Positive responses have been termed “price puzzles” and “activity puzzles”, leading to the development of identification approaches and model setups aimed at “solving” these puzzles (Rusnak et al. 2013).

Even if the standard theory were universally true and powerful, random sampling error and model misspecifications should frequently lead to positive or statistically insignificant findings. If such findings are deemed implausible or less publishable, while large negative effects pass the plausibility filter, the consequence can be an overrepresentation of statistically significant and large negative output and price responses, while statistically insignificant or positive responses are missing. In particular, this selection bias does not necessarily involve outright fraud or bad intentions by authors, editors, or referees, but can be a rational individual decision for a plausible specification. However, it can lead to a fallacy of composition, biasing the average effects and conclusions of conventional literature syntheses.

*p*-hacking can typically be detected by discontinuities in the distributions of *p*-values or related *z*-statistics around critical significance thresholds. In many fields, estimated coefficients are missing just above the typical 5 percent critical level of significance, while there is a clustering of coefficients just below this level (Brodeur et al. 2016). For a given

standard error, a statistically significant coefficient can be achieved with a larger (negative) point estimate. Vice versa, a given point estimate can be made significant by searching for a specification with a smaller standard error. In our sample, such  $p$ -hacking would produce a negative correlation between point estimates and standard errors.

The same negative correlation applies in the case of selecting negative output and price responses. In the absence of selection bias, the estimates should scatter symmetrically around the true effect. Those with high power and precision should be closer to the true effect. Thus, if the true effect were small and negative, so would the precise estimates, while low-precision estimates scatter more widely around the true effect. Therefore, especially estimates with positive signs and large standard errors would be discarded, while large but imprecisely estimated negative coefficients would be overrepresented. Thus, a regression of point estimates on standard errors would have a negative slope coefficient if there were publication selection biases.

In the case of monetary policy IRFs, publication bias may appear in idiosyncratic forms. First, VAR models typically have low degrees of freedom; 68 percent (or 1-SD) confidence bands are often standard measures of uncertainty that are accepted as signals for statistical significance in the monetary policy literature. Second, the output response is expected to be hump-shaped, and it typically suffices if the upper confidence interval turns negative for the middle range of the IRF to declare a statistically significant effect. Thus, there may be less selection for statistical significance in the first and final horizons of the output IRF. In contrast, for the price level, a permanently negative response is expected, and such IRFs might be selected on theoretical grounds. In the very short-run, the specter of the activity and price puzzle might lead researchers to discriminate against positive responses, while insignificant negative or zero effects might be considered consistent with theoretical predictions. A Cholesky identification even imposes a zero response of output and prices on impact.

The correlation between standard errors and point estimates can be local in the proximity of critical  $p$ -values or global when it comes to small study effects or selection on the sign of

coefficients. The meta-analysis literature has therefore developed different tests for detecting  $p$ -hacking and publication bias that we apply in the following.

## 4.2. Signs of $p$ -hacking

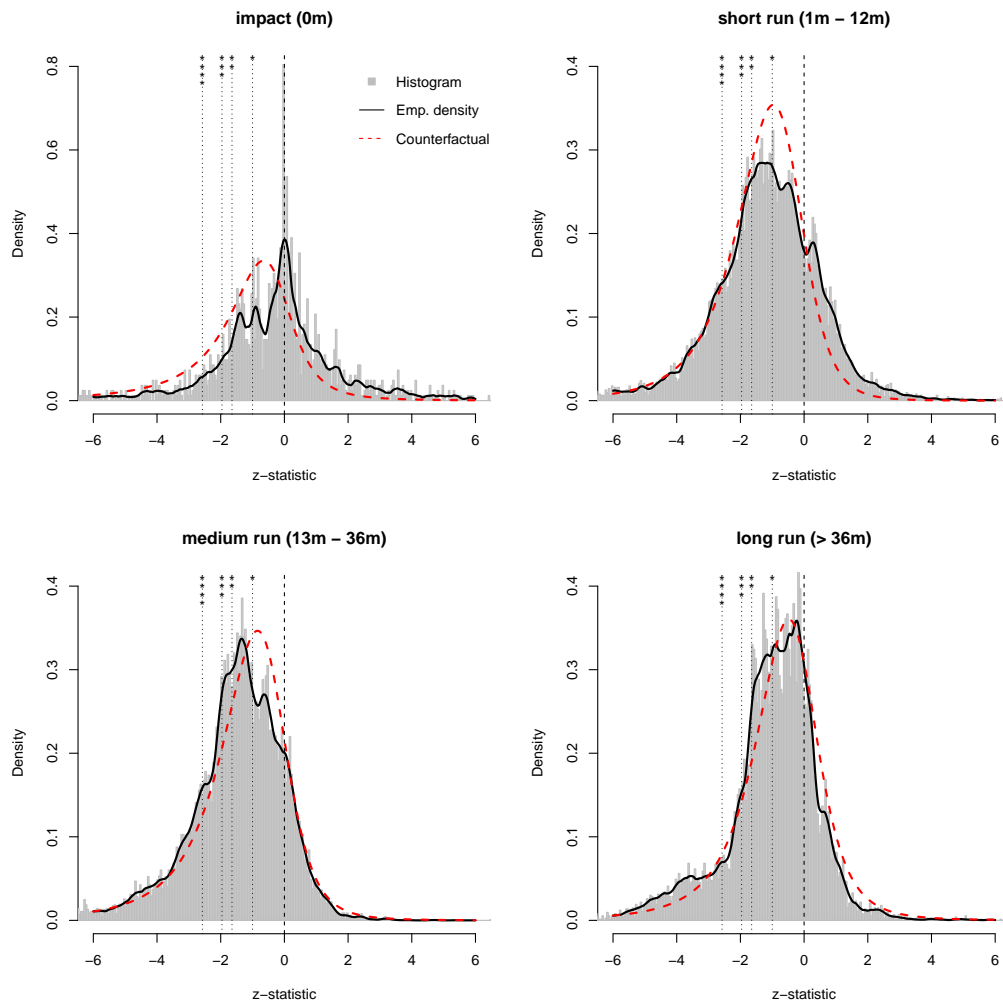
First, we investigate  $p$ -hacking by analyzing the distribution of  $z$ -statistics at critical thresholds of statistical significance (Brodeur et al. 2016; Brodeur et al. 2020; Brodeur et al. 2023). This approach compares the mass of test statistics just below and above critical significance thresholds, assuming that the probability mass should be smooth in the absence of  $p$ -hacking.

Figure 3 and 4 show the distribution of  $z$ -statistics of the output and price responses for different horizons, respectively. Note that we extend the approach of Brodeur et al. (2016) and display both the positive and negative range of  $z$ -statistics to investigate a possible discrimination against positive significant output and price responses. For calculating the  $z$ -values, we use the point estimates and the upper bounds of the confidence intervals in the primary studies that signal statistical (in)significance of negative responses.

The gray bars show densities according to bins whose width we chose to meet standard significance thresholds at the 68, 90, 95 and 99 percent confidence level, indicated by the vertical dotted lines. The solid black line represents a smoothed kernel density of the  $z$ -statistics. The red dashed lines show counterfactual kernel densities that would be expected from a regular distribution of  $z$ -statistics. The counterfactual is calibrated by matching the empirical mass for observations with  $z < -5$ , since  $p$ -hacking is unlikely in this range (Brodeur et al. 2016). However, note that this can only serve as a rough orientation, as the number of observations in this range is small.

Let us first focus on the response on impact, which is a special case, since it is strongly influenced by identifying restrictions: Cholesky/SVAR estimates typically assume a zero response of output and prices on impact, which explains the strong concentration of estimates around zero. Moreover, there is a second heap with a  $z$ -value of around  $-1.5$ , both for output and the price level, which arises from the sign-restricted estimates (see Figure C.2 in Appendix C).

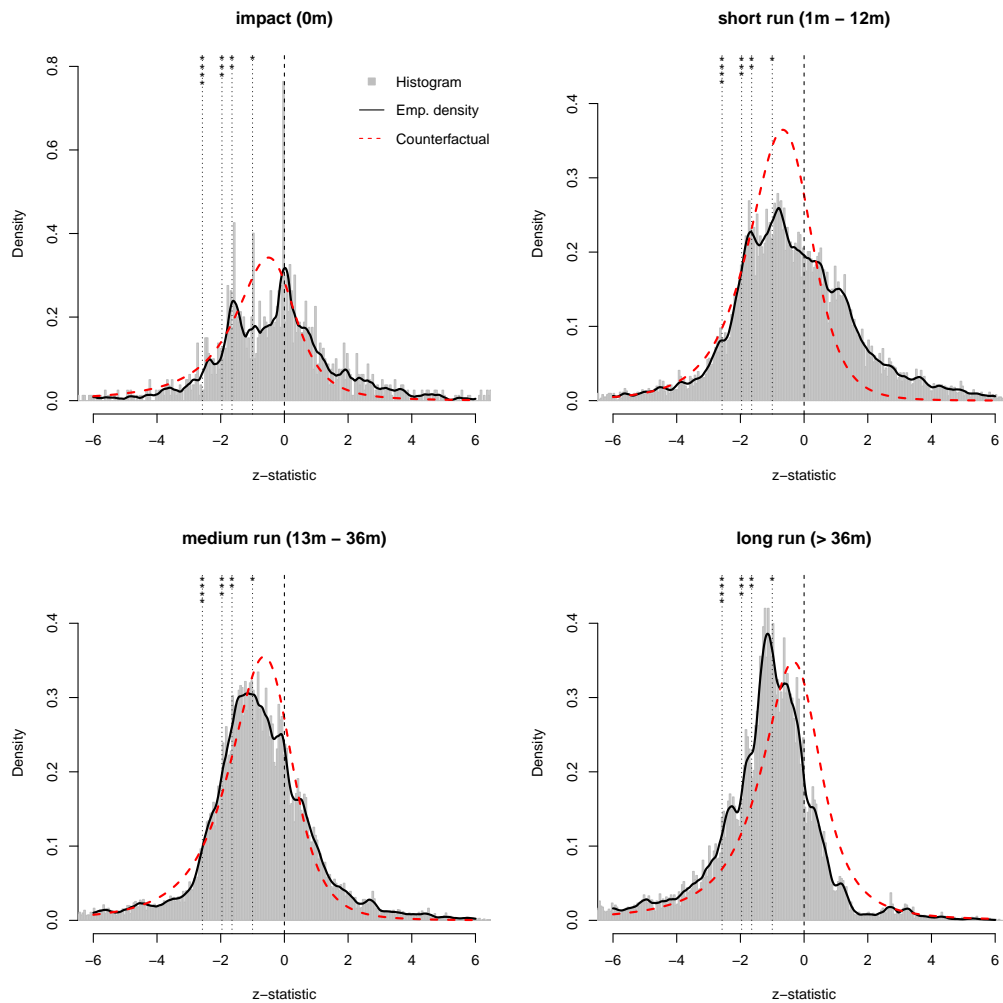
Figure 3: Histograms and kernel density of z-statistics for output



*Notes:* The graph shows the distribution of z-statistics of the output responses to a contractionary monetary policy shock for different horizons. The z-statistics are calculated from the point estimates, divided by their standard errors according to the upper bounds of the reported confidence intervals. The gray bars show the densities of z-statistics in bins. The vertical dotted lines indicate standard significance thresholds at the 68, 90, 95 and 99 percent confidence level (from left to right). The solid black line represents a smoothed kernel density of the z-statistics. The red dashed lines show counterfactual kernel densities, derived from a regular distribution of z-values. The counterfactual is calibrated by matching the empirical mass for observations with  $z < -5$ , as *p*-hacking is unlikely in this range (Brodeur et al. 2016).



Figure 4: Histograms and kernel density of z-statistics for the price level



*Notes:* The graph shows the distribution of z-statistics of the price level responses to a contractionary monetary policy shock for different horizons. The z-statistics are calculated from the point estimates, divided by their standard errors according to the upper bounds of the reported confidence intervals. The gray bars show the densities of z-statistics in bins. The vertical dotted lines indicate standard significance thresholds at the 68, 90, 95 and 99 percent confidence level (from left to right). The solid black line represents a smoothed kernel density of the z-statistics. The red dashed lines show counterfactual kernel densities, derived from a regular distribution of z-values. The counterfactual is calibrated by matching the empirical mass for observations with  $z < -5$ , as *p*-hacking is unlikely in this range (Brodeur et al. 2016).

More interesting in terms of possible  $p$ -hacking are the responses after the impact horizon, which are not strictly determined by most identifying restrictions. The histogram bars for output over the short and medium run show signs of heaping just above the 68 percent confidence level (one \*), which is a standard threshold for statistical significance in monetary policy IRFs. At the same time, observations seem to be missing just below this threshold. Such discontinuities at standard significance thresholds point to  $p$ -hacking.  $p$ -hacking is most plausible for the short and medium-run horizons, where one would indeed expect a significant output response. The pattern is most pronounced at the medium-run horizon. The long-run horizon is rather characterized by heaps just below zero, which might signal a preference for a still negative (even if not statistically significant) long-run output response.

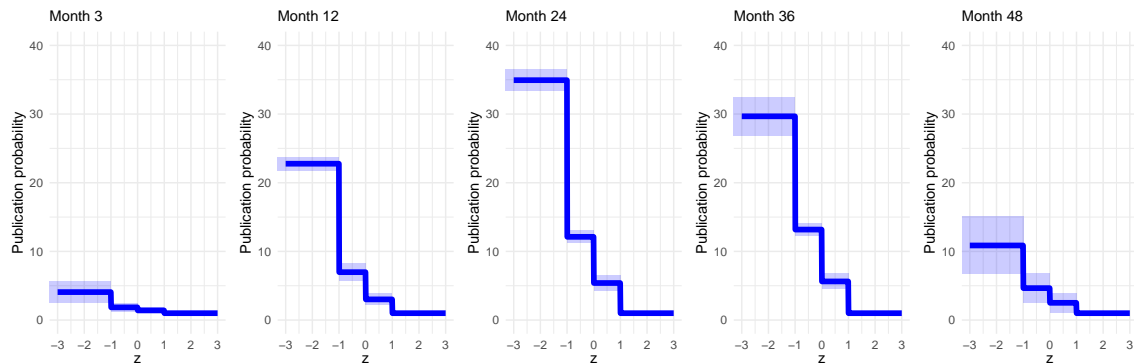
Considering the distribution of  $z$ -statistics for the price level in [Figure 4](#), there appears to be a heaping above the threshold corresponding to the 68 percent confidence bound for the medium and long-term horizons. The shift of the probability mass across the significance threshold is visible for the medium run, but the heaping is most pronounced for the long-run horizon, where conventional wisdom would predict a significant negative response.

We test the validity of these visual inspections more formally in [Figure B.1](#) and [B.2](#) in [Appendix B.1](#). The null hypothesis of the tests is that in symmetric windows around standard significance thresholds, the number of observations just above and below the threshold should be equal to 50 percent each. In the absence of  $p$ -hacking, one would expect a random 50 percent share of significant effect sizes at all intervals and time horizons. The size of the window can be altered to zoom in on the region just around the threshold. We focus on the 68 percent confidence threshold here. In line with the visual inspection above, for output we find evidence for a higher share of just significant estimates over the medium run, and for the price level over the medium and long run.<sup>6</sup>

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<sup>6</sup>For the price level, the effects vanish at narrower windows, which may be due to the small number of observations for narrow windows in our case in comparison to Brodeur et al. (2016). Moreover, since we collect the point estimates and confidence intervals from graphs in the primary studies of varying quality, measurement error in the  $z$ -statistics could play a greater role in our sample, which could particularly affect the findings for narrow windows.

Figure 5: Relative conditional publication probabilities for the output response

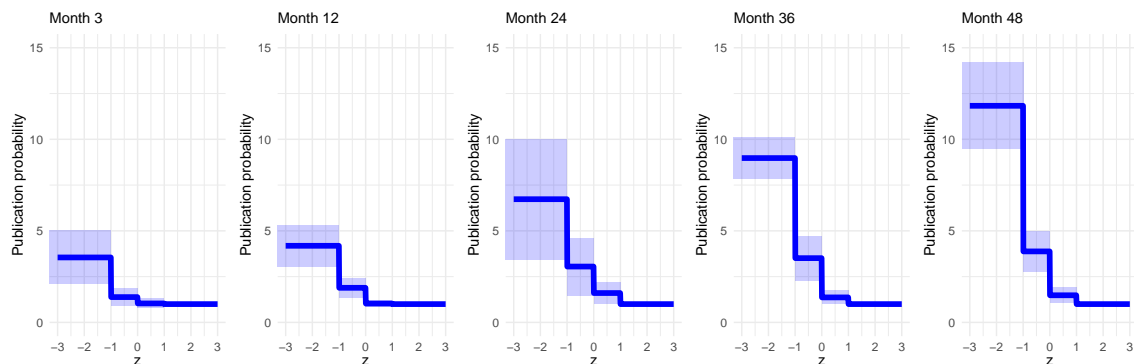


Notes: The figures show relative conditional publication probabilities of output responses on the vertical axis for typical ranges of the  $z$ -statistics (displayed on the horizontal axis) at different time horizons. Shaded areas show 89 % confidence bands. Cutoffs are specified at  $z = 0$  and  $z = \pm 1$ . The publication probability of observations with  $z > 1$  is normalized to 1. Figure B.3 in Appendix B shows the same statistics for the output and price responses separately by quarter.

### 4.3. Conditional publication probabilities

Andrews and Kasy (2019) provide an alternative assessment of publication selection, considering selection both on statistical significance and on the sign of the estimates. Their test calculates stepwise conditional publication probabilities in relation to a reference range of  $z$ -statistics. That is, compared to the randomization tests above, the focus is less on marginal differences in publication probabilities in the proximity of cutoff values, but on differences of average publication probabilities for typical significance levels.

Figure 6: Relative conditional publication probabilities for the price level response



Notes: The figures show relative conditional publication probabilities of price responses on the vertical axis for typical ranges of the  $z$ -statistics (displayed on the horizontal axis) at different time horizons. Shaded areas show 89 % confidence bands. Cutoffs are specified at  $z = 0$  and  $z = \pm 1$ . The publication probability of observations with  $z > 1$  is normalized to 1. Figure B.3 in Appendix B shows the same statistics for the output and price responses separately by quarter.

Figure 5 and 6 show the results of this test for the responses of the output and price level on different horizons, respectively. The reference range is a positive estimate that is statistically significant at least according to the 68 percent confidence interval ( $z > 1$ ), to which we assign a normalized publication probability of 1. For the other ranges ( $0 < z \leq 1$ ;  $-1 < z \leq 0$ ;  $z \leq -1$ ), the publication probability could then be larger, smaller, or similar. Vastly different publication probabilities may point to selection of results based on the sign or statistical significance of the estimate.

As a general pattern, the publication probabilities remain roughly equal for the responses on impact and in the short run, while the differences are remarkable for the medium-run response of output and for the long-run response of prices. For output, it is about five times more likely to observe an insignificant positive effect (at the 68 percent confidence interval) than a significant positive effect (the reference category) after 24 months. It is around 12 times more likely to observe a negative insignificant result and about 35 times more likely to observe a negative significant response. For the price level, the differences are quantitatively smaller, but follow a similar pattern. The strongest differences can be observed for the long-term horizon, where selection for negative and statistically significant findings seems to be more prevalent.<sup>7</sup>

Both the tests for marginal as well as average differences in the publication probabilities at typical thresholds point towards publication selection bias leading to inflated output and price responses at relevant horizons of the IRFs. However, selection on the sign or statistical significance of estimates can be more granular, which might be better detected by funnel asymmetry.

#### 4.4. Funnel asymmetry

As mentioned above, selection of statistically significant and negative output and price responses leads to an asymmetric distribution of effect sizes around the ‘true’ value and a

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<sup>7</sup>Figure B.3 in Appendix B.2 provides a more granular picture of the relative publication probabilities per quarter of the IRFs. The patterns are rather similar. Figure B.3 also shows the test results for the interest rate response. As expected, the differences in the publication probabilities for different levels of z-statistics are much smaller for the interest rate.

correlation between the point estimates and their standard errors (Stanley 2008). Such an asymmetry can be visualized by funnel plots – scatter plots of standardized effect sizes on the horizontal axis and their precision (the inverse of the standard error) on the vertical axis.

With standard econometric assumptions, the distribution should have the shape of an inverted funnel with a narrow upper end and a wide lower end: the most precise estimates are close to the ‘true’ effect, while the least precise estimates should scatter widely on both sides. If there is only random sampling error and random misspecification bias, the least precise estimates should be equally likely to overestimate or underestimate the ‘true’ effect size. Hence, in the absence of publication bias, the funnel plot should be symmetric, and an asymmetric distribution may be due to publication bias (Egger et al. 1997).

The funnel plots shown in Figure B.4 and Figure B.5 in Appendix B.3 consist of all estimates of the responses of output and the price level to a 100 bp contractionary monetary policy shock at the respective horizons. The percent response of output or the price level is shown on the horizontal axis and the precision of these estimates is displayed on the vertical axis.

The figures provide further evidence of a left-skewed distribution, consistent with selective reporting of research findings. The funnel plots become more asymmetrical at the response horizons that are of particular interest in the relevant literature: researchers report fewer imprecisely estimated positive coefficients for output responses over the medium run and for price-level responses over the long run.

Figure B.6 in Appendix B.3 shows funnel plots for the IRF of the interest rate variable. The funnel center moves from an effect size around one percentage point on impact towards zero in the medium to long run as the shock peters out. Interestingly, the funnels for the interest rate remain symmetric throughout. This provides a placebo test for our analysis of publication bias. Typically, there are no strong priors about the dynamics, size, and statistical significance of the interest rate IRF. The interest rate response is usually reported

for the sake of completeness, although without much notice. Consequently, we would not expect publication selection and  $p$ -hacking on the interest rate response.

Funnel asymmetry can also be tested formally. Funnel asymmetry tests (FAT) are variants of the following generic regression model that we estimate separately for each horizon:

$$R_{i,j,h} = \beta_{0,h} + \beta_{1,h}SE_{i,j,h} + v_{i,j,h} \quad (1)$$

where  $R_{i,j,h}$  is the standardized response coefficient (which measures either the output, price level or interest rate response to a contractionary 100bp monetary policy shock) from estimate  $i$  of study  $j$  at horizon  $h$ . Since publication bias creates a correlation between the point estimate and its standard error  $SE$ , we include  $SE$  as a regressor and add random sampling error  $v$ .  $\beta_{1,h}$  measures funnel asymmetry. In line with the funnel plots, we expect  $\beta_{1,h}$  to be clearly negative and show meaningful magnitude for output over the short to medium run and for the price level over the medium to long run. Note that this specification could suffer from omitted variable bias due to influential study characteristics, which we address in [Section 5.2](#).

The conventional approach is to estimate [Eq. \(1\)](#) with weighted least squares (WLS), using the inverse of the variances ( $1/SE^2$ ) as weights to account for heteroskedasticity (Stanley and Doucouliagos 2012). WLS assigns more weight to more precise studies, whose estimates should generally be closer to the true effect.<sup>8</sup> However, this assumption might be too strong in light of more elaborate approaches to estimate standard errors (e.g. using clustering, bootstrapping, instrumentation) that typically produce larger standard errors. In this case, inverse variance weights would systematically downgrade more elaborate estimates.

Given these concerns, we estimate both weighted and unweighted versions of the various tests to derive a range of corrections that is agnostic to weighting. Estimating unweighted

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<sup>8</sup>For some estimation methods, standard errors are forced to be zero in the initial period 0. This implies infinite precision, which causes WLS regressions with precision weights to fail. To address this issue we implement a capping procedure. For each outcome variable, we limit the precision values for period 0 to the maximum precision observed in period 1. Analogously, we set the standard errors for period 0 to be no less than the minimum standard error observed in period 1. This approach allows our estimation methods to function properly while still maintaining appropriate precision.

versions is also common in the meta-regression literature (e.g. Havránek et al. 2024). In addition, instead of the linear  $SE$ , Stanley and Doucouliagos (2014) show in Monte Carlo simulations that the quadratic form  $SE^2$  can often better represent the relation between the standard error and the point estimate in the presence of a genuine effect. We cluster the standard errors of the meta-regression at the study level as observations from the same study are likely not independent (Irsova et al. 2023b).

Figure B.7 in Appendix B.4 shows 89% confidence bands for the publication bias coefficients from different funnel asymmetry tests for the output, price level, and interest rate response. Notably, the confidence bands for output and price level indicate sizeable and negative coefficients and do not cross the zero line for all horizons, while for the interest rate, the bands show that the FAT coefficients are narrowly distributed around the zero line.

#### 4.5. Bias-corrected estimates

In the previous sections, we discussed evidence of publication bias. Most of the methods also provide estimates of average effect sizes corrected for publication bias. Consider again Eq. (1) above. The coefficient  $\beta_{0,h}$  gives the underlying effect size after controlling for publication bias and random sampling error. The test of  $\beta_{0,h}$  against zero is also known as the precision effect test (PET), which can be calculated for WLS and OLS estimators. The version with  $SE^2$  is called the precision effect estimator with standard error (PEESE).

Besides these approaches that effectively filter out the asymmetry in the funnel plot and assume a linear (or quadratic) relation between standard errors and effect sizes, the meta-analysis literature provides further non-linear methods of estimating effect sizes corrected for publication bias. The test for conditional publication probabilities by Andrews and Kasy (2019) estimates an underlying mean effect, weighting the response estimates according to their inverse publication probabilities (AK).

The weighted average of adequately powered estimates (WAAP) focuses only on estimates with sufficient power, where  $p$ -hacking is less likely (Ioannidis et al. 2017). In the baseline

specification, we adopt their standard threshold for adequate power at 80% in the case of an underlying 5% significance level, which boils down to estimates whose standard error is smaller than the weighted average of point estimates divided by a t-statistic of 2.8.<sup>9</sup> We measure this adequate power of an estimate at relevant horizons (12 months for output and interest rate and 48 months for the price level).<sup>10</sup>

Figure 7 summarizes the various test results by showing the corrected IRFs for output, prices, and interest rates based on the mean beyond bias estimates per quarter. This includes the AK test and both weighted and unweighted corrections of WAAP, FAT-PET, and PEESE. All tests correct the average output and price responses towards zero, while there are no relevant corrections for the interest rate. The absolute correction is strongest for the short-to-medium-run response of output and for the medium-to-long-run response of the price level. The AK test produces the strongest corrections for output and prices, where the resulting IRFs are close to zero over the whole horizon. The corrections using WAAP and FAT-PET are similar in size, while the PEESE results point to more moderate corrections. The unweighted OLS and OLS with  $SE^2$  give the smallest bias corrections.

It is a priori unclear which of the bias corrections is to be preferred. We account for this uncertainty by providing a range of bias-corrected IRFs. The range is based on the seven

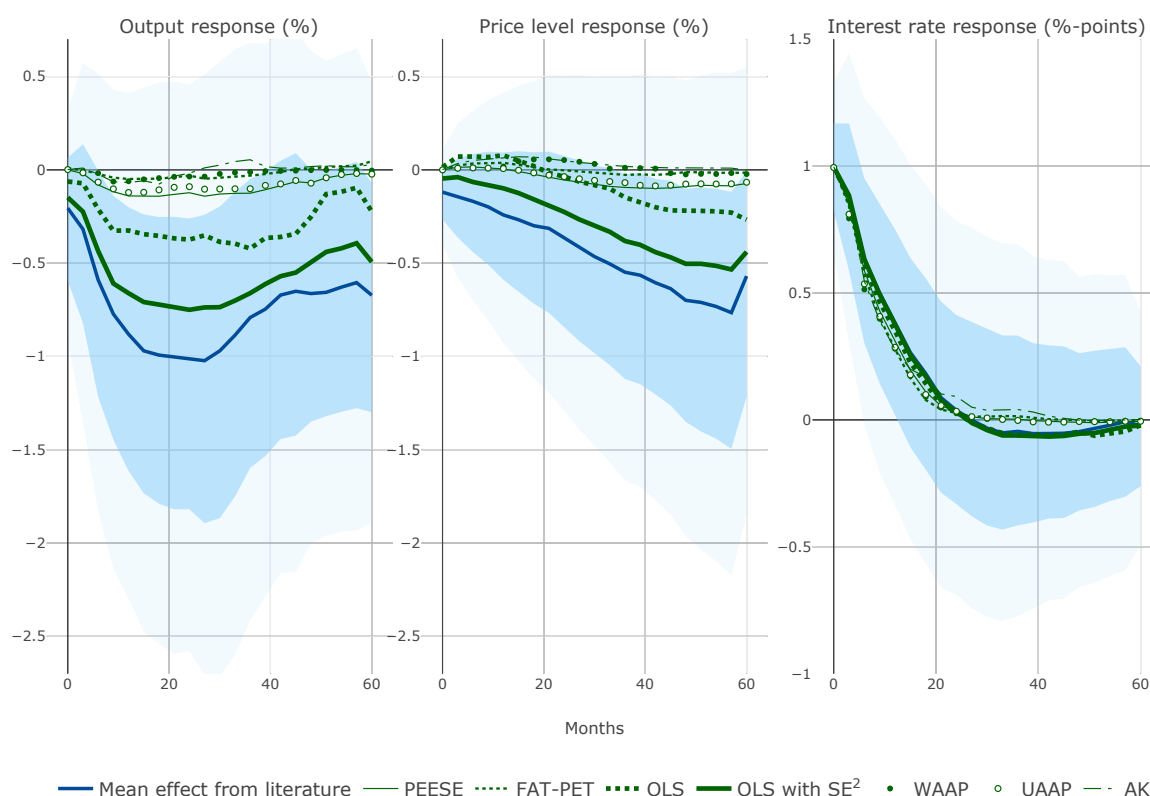
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<sup>9</sup> $t = 1.96$  for the 95 percent significance threshold + 0.84 for the standard normal value referring to a 20/80 split in the cumulative distribution, consistent with 80 percent power level.

<sup>10</sup>Note that we planned three additional tests according to the PAP: (i) The Bom and Rachinger (2019) test extends Eq. (1) in that the correlation between point estimates and standard errors may disappear for high-precision estimates, for which selection is less likely. However, the test collapses to the simple FAT-PET test of Eq. (1) if the non-linearity is weak. This is the case in our sample, thus we do not report the results. This finding is also consistent with the comparably weaker evidence of  $p$ -hacking at the cut-off values in Section 4.2. (ii) The test by Furukawa (2019) calculates a corrected average based on the most precise estimates, the ‘stem’ of the funnel. This sub-sample is determined by minimizing the trade-off between the variance per observation (which shrinks with an increasing sample) and publication bias (which rises with a larger sample). The Furukawa test typically selects a very small number of estimates, and thus, in our case, the corrected effect varies substantially across response horizons. We therefore abstain from reporting the effect sizes. (iii) A 2SLS approach in which the number of observations used in the primary estimates instruments the standard error, the MAIVE test (Irsova et al. 2023a). The inverse of the number of observations could be a useful exogenous proxy for the squared standard error, which might be endogenous to the effect size in Eq. (1), which would then suffer from reverse causality. However, we fail to establish the inverse of the number of observations as a relevant instrument in our case, as the 1st-stage F-statistics are clearly below conventional thresholds. VAR estimates with low degrees of freedom and standard errors calculated from complex iterations may not be well suited for the underlying MAIVE assumptions. We report the details of the 1st-stage tests in Appendix B.5 but do not report the invalid 2nd-stage results.



Figure 7: IRFs of bias-corrected estimates from different publication bias methods



*Notes:* The figures show the IRFs of output, prices and interest rates after a 100bp contractionary shock to the central bank policy rate after correction for publication bias via different methods: the FAT-PET (using the standard error) and PEESE (using standard error squared), which build on testing and correcting for asymmetry in the respective funnel plot, estimated by using weighted least squares (Stanley and Doucouliagos 2012), and the unweighted OLS versions; the WAAP, i.e. the weighted average of the adequately powered, which focuses on estimates with sufficient power (Ioannidis et al. 2017), and the unweighted version UAAP; and the AK test, which weights the response estimates according to their inverse publication probabilities (Andrews and Kasy 2019). Effect sizes and standard errors are winsorized at the 2% level.

tests shown in [Figure 7](#). Moreover, we account for six different winsorization levels: 0, 1, 2, 3, 4 and 5 percent.<sup>11</sup> Overall, this provides up to 42 combinations that form the range of bias-corrected estimates at each response horizon, as shown in [Figure 1](#) in the Introduction. We show the mean and the 16th-84th and 2.5th-97.5th percentiles of estimates.

The range of corrected estimates in [Figure 1](#) is clearly closer to zero and does not overlap with the simple average IRF for both the output and the price-level responses. For output, the mean of the corrected range starts at zero and reaches a peak of  $-0.25$  percent after around 18 months. The IRF then slowly returns to zero in the long run. When considering the 2.5th-97.5th percentile range of the corrections, the output response is still negative for the first two years after the shock. The range widens and contains zero effects for longer horizons. Regarding the price level, the mean corrected response remains close to zero for the first two years and reaches a peak of  $-0.15$  percent after around five years. The 2.5th-97.5th percentile range includes zero effects at all horizons, while the 16th-84th percentile range is narrowly in the negative territory. Output and prices are unlikely to fall by more than 0.5 and 0.25 percent, respectively, according to the 84th percentile.

The IRFs for the simple mean and the corrected mean of the interest rate are almost identical throughout the observed horizon. There is no strong incentive to select results based on the interest rate response. Overall, we do not find publication bias for this variable according to our tests. This placebo test strengthens the evidence that publication bias is present in the effects of the output and price level, for which the correction of the effects is strong and robust.

[Figure B.8](#) in [Appendix B.6](#) provides sub-sample robustness checks for the range displayed in [Figure 1](#) looking at specific definitions of output and prices changes (e.g. GDP and CPI data only, log-level estimations only) and monthly vs. quarterly frequency of the underlying data. One test considers a selection of estimates published only in top journals and excluding Choleski/SVAR estimates that might be overrepresented in our sample. For this selection, the unweighted averages of output and price responses are about twice as large as for the full

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<sup>11</sup>For the interest rate responses we needed to restrict the winsorization to 0-4 percent due to singularity issues.

sample. However, the correction for publication bias is even stronger, so that the corrected average responses do not exceed  $-0.45$  for output and  $-0.3$  for prices, well within the range of the full sample.

Figure B.9 presents several alternative versions of the range estimation, including a different threshold for the WAAP estimate; focusing on 2% winsorization; random sampling of models, choosing only one model per study, and repeating this selection ten times; and separating corrected effect sizes from weighted and unweighted tests for publication bias. These variations show that the range of bias-corrected estimates is robust to further perturbations of the sample and methodological choices. Moreover, weighted tests lead to stronger downward-corrections of the effect sizes.

## 5. Heterogeneity

This section is concerned with sub-sample stability and investigates the factors that contribute to explaining the variation in the reported results on the macroeconomic effects of monetary policy shocks. As shown in Table 1, primary estimates can be categorized by a number of study and estimation characteristics. We cover important dimensions of heterogeneity with a focus on different shock identification strategies, data and estimation choices, as well as publication outlets. Section 5.1 provides a sub-sample analysis while Section 5.2 presents a multivariate meta-regression model.

### 5.1. Sub-sample analysis

*Estimates published in leading journals.* Do estimates of the macroeconomic effects of monetary policy published in leading journals differ from estimates published elsewhere? Although the review process in top-tier journals may hold submissions to particularly high quality standards, the existing literature also highlights that publishing in top-ranked journals may set incentives for authors to show that their results are particularly strong and statistically significant to increase the chances of publication (Chopra et al. 2024). Askarov et al. (2024) analyze more than 20,000 empirical results published in 31 leading economic

journals. They find that more than half of all significant results were selected for significance. In the top five journals, this selection bias seems to be even stronger, as two-thirds of all estimates published in these journals were selected for significance. Other studies also find that publication bias in leading journals is substantial and at least not less than in lower ranked publications (e.g. Brodeur et al. 2020; Heimberger 2022).

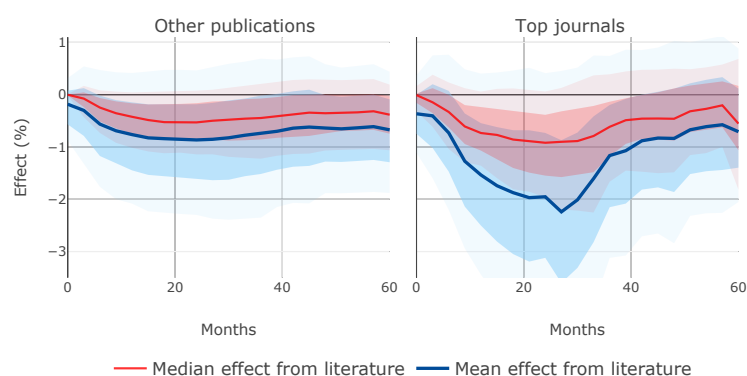
We define estimates in leading journals as those published in a top-50 journal of the economics profession as measured by the SJR indicator, which accounts for around 14 percent of the estimates in our sample. The results in Figure 8 indeed suggest that the output and price-level responses published in leading journals point to stronger dampening effects of contractionary monetary policy than the estimates published elsewhere. However, the larger difference between the median and mean responses – in particular at the relevant response horizons – indicates that the distribution is more left-skewed. In fact, the median responses are closer to each other.

Figure C.3 and Figure C.4 in Appendix C show that there is stronger bunching of z-statistics at critical thresholds of significance for estimates published in leading journals, consistent with a stronger selection of research findings.

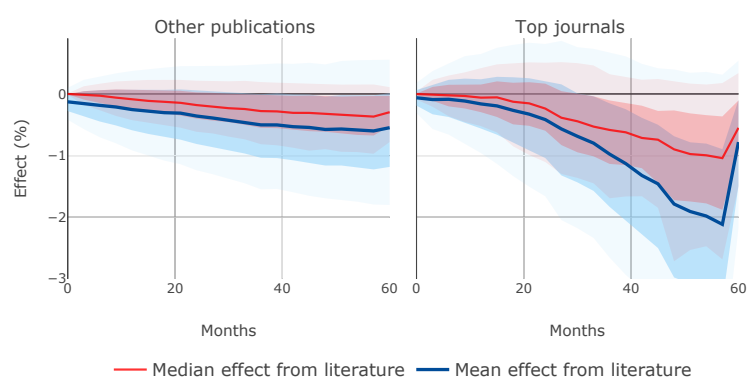
*Central bank affiliations.* Fabo et al. (2021) find that the reported results of the macroeconomic effects of Quantitative Easing differ significantly between central bank researchers and academic economists. For our sample, Figure C.1 in Appendix C shows that there is no marked difference between estimates produced by central-bank-affiliated researchers and those without central bank affiliation when it comes to the average and median responses of output and prices to conventional monetary policy shocks. Central bank researchers, on average, seem to report a slightly more negative price level response at longer horizons, and the distribution is more left-skewed, but the differences are small in comparison to other characteristics.

Figure C.5 and Figure C.6 in Appendix C confirm that bunching of just statistical significant results (at the 68 percent level) is prevalent for the price level at the long-run horizon

Figure 8: Average, median and corrected responses to a conventional monetary policy shock  
- Top journals vs other publications



(a) Output response



(b) Price level response

*Notes:* The figures show average responses (blue line) and median responses (red line) of output and the price level in percent with confidence intervals (shaded areas) after a contractionary 100bp conventional monetary policy shock. The sample is split into those estimates from leading journals (around 14 percent of all observations) and other publications.

for both central-bank related authors and others, but the effects are more clear-cut for the former.

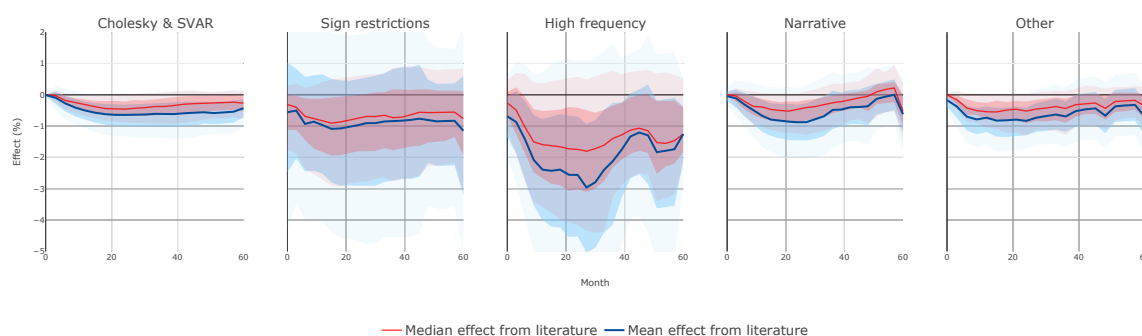
*Identification.* As identification of monetary policy shocks may systematically affect the results (e.g. Coibion 2012; Ramey 2016; Nakamura and Steinsson 2018b), we study the median and average macroeconomic effects of monetary policy shocks for the various shock identification methods. Figure 9 indeed points to different sub-sample averages and medians. Employing a high-frequency identification is related to particularly large dampening effects of contractionary monetary policy shocks on output, but the uncertainty is also large. Sign restrictions, which typically constrain the output and price response to be negative within the first months, also exhibit larger and more persistent output effects than the rest of the approaches.

Concerning the price-level response, sign restrictions produce relatively large negative effects, but the error bands are again wide. The narrative identification is associated with close to zero price effects in the short run; however, the price-level response turns strongly negative at longer response horizons. Note that the number of observations becomes smaller at longer horizons, since some studies only report shorter IRFs. There is a significant drop after 48 months. In some cases, this may lead to small-sample issues such that the IRFs become more volatile.

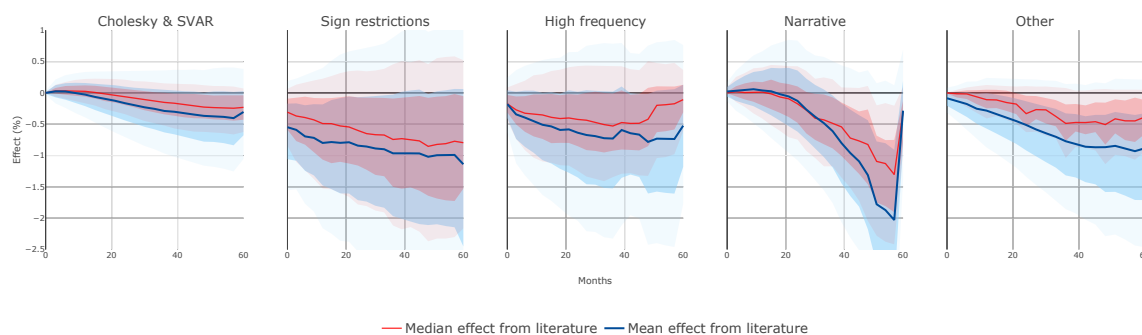
*Country-(group)-specific effects.* An other important dimension of the heterogeneity prominently discussed in the existing literature is whether the effects of monetary policy shocks in the United States differ markedly from those in other advanced economies or other parts of the world.

The results reported in Figure 10 point to two key insights. First, the effects for the US do not differ markedly from the Euro Area and other advanced economies. In particular, the median estimates are of similar size. For the Euro Area and other advanced economies, output effects are more left-skewed than for the US. However, the peak of the mean price-level effect is more of an outlier for US data.

Figure 9: Average, median and corrected responses to a conventional monetary policy shock by identification method



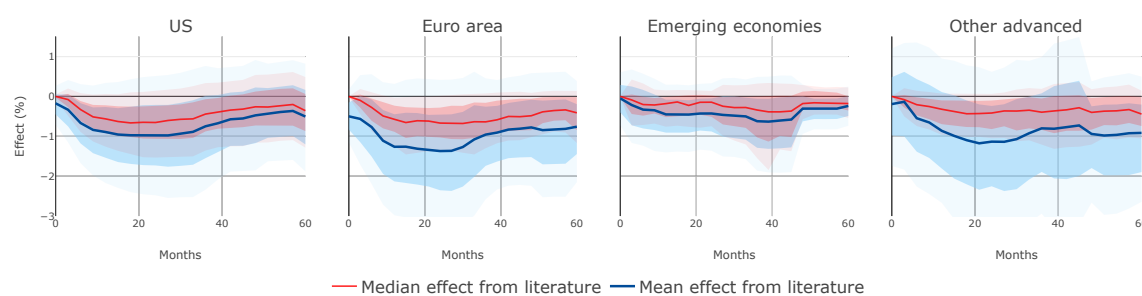
(a) Output response



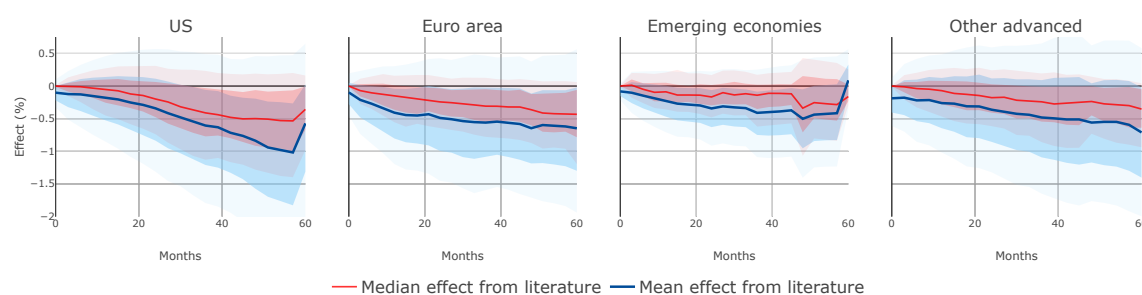
(b) Price level response

*Notes:* The figures show average responses (blue line) and median responses (red line) of output and the price level in percent with confidence intervals (shaded areas) after a contractionary 100bp conventional monetary policy shock by identification method.

Figure 10: Average, median and corrected responses to a conventional monetary policy shock by country/group



(a) Output response



(b) Price level response

*Notes:* The figures show average responses (blue line) and median responses (red line) of output and the price level in percent with confidence intervals (shaded areas) after a contractionary 100bp conventional monetary policy shock by country or country group.



Second, the dampening effects of contractionary monetary policy on output and prices are markedly smaller in emerging market economies than in advanced economies. This is consistent with Unsal et al. (2022), who show that monetary policy frameworks concerning independence, accountability, strategy, and communication in emerging market economies are catching up, but still lack behind those of advanced economies. We plan to analyze institutional differences across countries and time in more detail in a follow-up paper.

Our sub-sample analysis reveals some interesting patterns in the data, but this could mask correlations among multiple study characteristics. The next sub-section develops a systematic heterogeneity analysis based on multivariate regressions, where the standardized effect sizes are regressed on the moderator variables.

## 5.2. Multivariate meta-regression

In what follows, we test for the impact of modeling and sample choices as well as publication characteristics in a multivariate meta-regression (MMR) framework, while also controlling for publication bias. As our baseline correction method, we choose a simple OLS specification, as it results in a medium-sized correction that is also closest to the corrected mean effect in Figure 1.

That is, we modify Eq. (1) to include a vector  $X_{i,j,h}$  of moderator variables that captures the estimation and study characteristics introduced in Table 1. We can thus write the MMR model as follows (with clustering standard errors at the study level):

$$R_{i,j,h} = \beta_{0,h} + \beta_{1,h}SE_{i,j,h} + \beta_{2,h}X_{i,j,h} + v_{i,j,h} \quad (2)$$

$X_{i,j,h}$  primarily contains dummy variables or categorical variables of which one category is omitted from the regression. Thus, in the MMR,  $\beta_{0,h}$  reflects the corrected effect sizes of the omitted reference categories, which are to be defined. In our parsimonious baseline specification, we include identification methods and dummies for a top-tier publication, as well as a central bank affiliation in  $X_{i,j,h}$ . We select Cholesky/SVAR estimates published in a non-top-tier publication and with no central bank affiliation as the reference categories.

The coefficients for the other identification strategies, top-tier publication and central-bank affiliation in  $\beta_{2,h}$  reflect the differences to this reference group. Due to the linear regression form, the choice of one omitted category does not affect the coefficients of other variables in  $\beta_{2,h}$ . However, it shifts the intercept  $\beta_{0,h}$ , which cannot be interpreted as the general mean beyond bias anymore.  $\beta_{1,h}$  is the estimate of the strength of publication bias when controlling for study characteristics.

Figure C.7 and C.8 in Appendix C.3 illustrate the coefficients along the estimation horizons. Regarding our measure of publication bias, the coefficients  $\beta_{1,h}$  for the output level and the price level are large and negative throughout the observed horizon, and are very similar to the simple univariate estimates in Section 4.4.

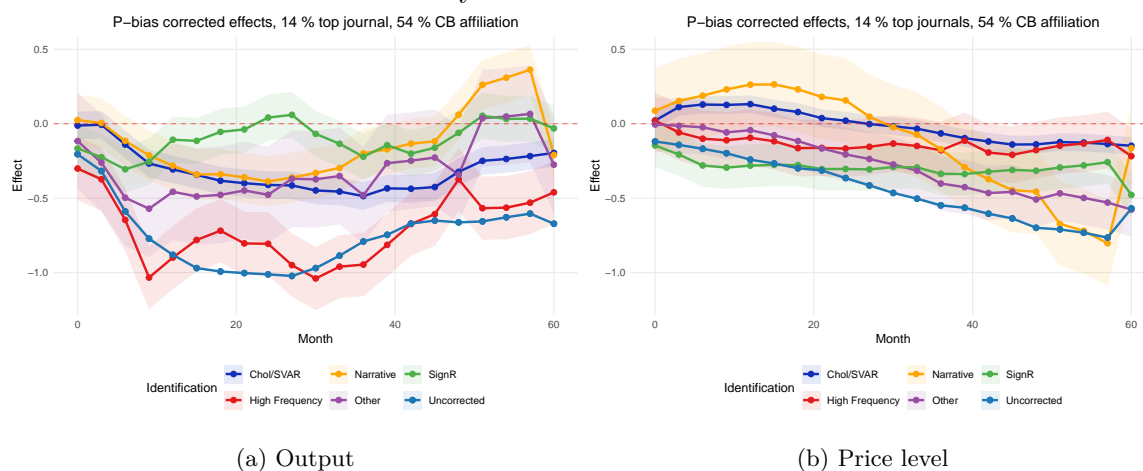
Looking at the moderator variables for output, identification approaches partly show differences in comparison to the Cholesky/SVAR specification, but the coefficient is sizeable only for the high-frequency and the sign restrictions approach at the most relevant horizons. Top-tier journal publications appear to exhibit stronger negative output effects than other publications in the short to medium run.

Turning to the price-level regression, the high-frequency and sign-restriction approaches exhibit somewhat more contractionary price-level responses than the Cholesky/SVAR models in the short to medium run, while the coefficients for the other identification approaches rather turn negative in the long run. Publications in top journals show slightly less contractionary price responses in the short run and the coefficient turns slightly negative over time. Central bank affiliations do not seem to matter for both output and price level estimates.<sup>12</sup>

As the discussion about different identification methods is central in the empirical monetary policy literature, we take a closer look at its relevance in our meta-dataset. Figure 11 shows the predicted values from Eq. (2) for the effects corrected for publication bias us-

<sup>12</sup>In Figure C.9 and Figure C.10 of Appendix C.4, we test the robustness of our MMR results by including additional control variables like measurement of output, prices and interest rates, estimation methods, sample characteristics and further publication characteristics. Moreover, we compare our MMR estimates to the outcome of a Bayesian model averaging (BMA) exercise to account for model uncertainty regarding the choice of moderator variables. These robustness tests lead to more moderate coefficients for some control variables, making them appear less relevant. However, this is not the case for the coefficient indicating publication bias, which is very little altered both for output and the price level over the entire reported horizon.

Figure 11: Output and price level effects of monetary policy shocks: Uncorrected mean vs. MMR-corrected effects by identification method



*Notes:* The figure summarizes the results from the multiple meta regression (MMR) based on Eq. (2). It shows the predicted values (colored lines with dots) and 67% confidence intervals (colored shaded areas) of output and price responses for the different identification strategies, corrected for publication bias (p bias) and compares them to the uncorrected mean responses (blue dotted line). The predicted values assume sample means for the other control variables, the top-journal dummy (14 percent) and the central-bank (CB) affiliation dummy (54 percent). Figure C.13 in Appendix C.4 provides the comparison with uncorrected sub-sample averages and a robustness check using a WLS-PEESE specification.

ing different identification methods, assuming sample means for the dummy for top-tier publication and the dummy for central bank affiliation.

The predicted values illustrated in Figure 11 are compared to the uncorrected average estimate for the output and price-level responses, respectively. In general, the results confirm those of Figure 9 above. Thus, correlations between moderators do not seem to drive the main results. Consistently, high-frequency identification stands out with the strongest and most persistent output effects. The average output effect in an estimation with sign-restrictions is less pronounced than in Figure 9 and rather exhibits the smallest output effects after bias correction. Among the other identification approaches, a hump shape with a peak effect after about two years is a consistent pattern. Concerning prices, sign restrictions in the short to medium run and the narrative approach in the medium to long

run tend to yield the strongest contractionary price-level responses. After correcting for the sizeable publication bias, the price puzzle returns for some of the identification approaches.<sup>13</sup>

In summary, our results suggest that identification matters but is quantitatively less important than publication bias: for none of the identification approaches is the corrected IRF consistently more negative than the simple uncorrected average IRF of the full sample.

Multivariate meta-regressions often conclude with a best-practice estimate, defined by superior estimation techniques, influential variables, and/or common-sense approaches. We refrain from deriving a best-practice estimate because our results suggest that focusing on particular identification approaches or other data or estimation choices may come with biased or extreme results. Moreover, there is no consensus on best-practice modeling choices in the literature. Instead of relying on contestable best-practice estimates, we rather emphasize the range of publication bias corrected estimates in [Figure 1](#).

## 6. Conclusion

This paper analyzes the output and price effects of conventional monetary policy shocks based on a novel and large meta-dataset containing nearly 150,000 point estimates and confidence bands from around 5,000 impulse-response functions (IRF) that we collect from more than 400 empirical primary studies. The studies cover many countries and time periods, different identification and estimation approaches, and are published by a diverse set of authors in different publication outlets.

We have shown that a synthesis based on the naïve average of all IRFs would come to the conclusion of substantial contractionary effects of a 100 basis points interest rate hike on output (with a peak effect of  $-1$  percent after around 2 years) and the price level (with a peak effect of  $-0.75$  percent after 4 to 5 years). However, such a conclusion would be misleading since the literature suffers from substantial publication bias according to a series

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<sup>13</sup>As a further robustness test for the corrected effects in [Figure 11](#) we estimate a PEESE specification with  $SE^2$  and WLS with inverse-variance weights instead of the OLS estimate with  $SE$  in [Eq. \(2\)](#). For each identification approach, the corrected effects for OLS and PEESE are compared with the respective uncorrected sub-sample averages in [Figure C.13](#) in [Appendix C.4](#). The PEESE specification results in substantially smaller corrected effects due to the weighting assumption.

of established tests. When we correct for this bias, the resulting range of corrected IRFs points to substantially smaller dampening effects of contractionary monetary policy on the economy. The strongest corrections would be consistent with zero effects and the mildest corrections would imply a peak effect of  $-0.7$  percent for output and  $-0.5$  percent for the price level. The mean IRF beyond publication bias for output peaks at  $-0.25$  percent after 1 to 2 years and for the price level at  $-0.15$  percent after 4 years.

Turning to factors that explain the variation in effect sizes, the differences between advanced economies are comparably small, while the average output and price effects are clearly weaker in emerging market economies. Studies published in leading journals tend to show somewhat larger effects, and publication bias appears to be more prevalent there, consistent with the findings in Askarov et al. (2024).

There are weak signs of stronger *p*-hacking towards statistically significantly negative price responses after a contractionary conventional monetary policy shock in studies with authors affiliated with central banks. However, the resulting differences in the mean effects are small such that for our sample of conventional monetary policy, we cannot confirm the results found by Fabo et al. (2021) for Quantitative Easing.

Identification methods matter for the reported effect sizes. In particular, modern high-frequency identification approaches stand out with relatively strong output effects, while sign restrictions tend to produce more negative price-level responses to contractionary monetary policy shocks. However, when taking into account both identification approaches and publication bias, the latter dominates: none of the corrected IRFs of the different identification approaches shows a consistently more negative output or price-level response than the simple uncorrected average IRF of the full sample.

What are the implications of our findings? First, the power of conventional monetary policy to steer inflation and the business cycle may have been overstated in the past, based on conventional literature reviews or seminal papers in leading journals. Our results provide comprehensive evidence in favor of the skeptical conclusion in Ramey (2016) that the macroeconomic effects of conventional monetary policy are uncertain and likely small. Since

the correction is stronger for prices than for output, the sacrifice ratio of lost output in relation to the reduction of the price level is worse after correcting for publication bias than for the simple average.

Second, it is interesting to compare our effect sizes to the predictions of different applied (semi-)structural models. Lane (2023) discusses model outcomes for the Euro Area economy. It turns out that our simple average effects, which are likely plagued by publication bias, are closer to those from forward-looking representative agent DSGE models. An example is the ECB's New Area Wide Model (NAWM), which predicts a peak output effect of about  $-1$  percent and also finds substantial negative effects on inflation. However, the effects corrected for publication bias are closer to those of semi-structural models, such as the Euro Area BASE and BME models discussed in Lane (2023) where the output effects peak at  $-0.5$  and  $-0.25$  percent, respectively, and the inflation effects are also weaker and build up more slowly. A similar picture emerges for models tailored to the US economy, like the FRBNY representative agent DSGE model that predicts a peak output decline of  $-1.5$  percent and a strong reduction in inflation (Del Negro et al. 2013) vs. the semi-structural FRB-US model that exhibits smaller output and price responses, consistent with our bias-corrected range (Tetlow 2008; Angelini et al. 2019). This might partly explain publication bias as being driven by a preference to confirm the results of conventional DSGE models. Most of the more recent Heterogeneous Agent New Keynesian (HANK) models focus on establishing new transmission channels of monetary policy (Kaplan et al. 2018). Applied models designed to make quantitative predictions for specific economies are still in the making. A canonical HANK model can exhibit output and price effects after a rate shock, which are broadly in line with conventional DSGE models (Auclert et al. 2024; McKay and Wolf 2023). However, Auclert et al. (2020) develop a model with sticky expectations that matches the slow and hump-shaped responses of our bias-corrected estimates well. The dampening effect of contractionary monetary policy on output in Auclert et al. (2020) peaks at  $-0.4$  percent after 1.5 years and the price level falls by  $-0.5$  percent after 4 years. These point estimates are stronger than our bias-corrected mean estimates, and the sacrifice ratio

is clearly more favorable. However, the responses in Auclert et al. (2020) fall into our range of bias-corrected estimates.

Third, we find that the choice of how to identify monetary policy shocks affects the results. However, identification may have received disproportionate attention in comparison to *p*-hacking and publication bias, which are more influential according to our findings. Future research on monetary policy may put more weight on questions of open science, replicability, and acceptance of null results, which have received increasing attention in the wider economics profession more recently (Brodeur et al. 2023; Bartoš et al. 2024; Chopra et al. 2024; Christensen and Miguel 2018).

Fourth, there is some heterogeneity across country groups and there may be further differences in the time dimension or according to specific regimes (business cycle, supply constraints, exchange rate, central bank target) that we could not cover in sufficient detail in this paper. These questions are also interlinked with the transmission mechanism of conventional monetary policy on output and prices, for example via labor market responses. Our dataset could be extended and used to cover such topics, which provides ample scope for future research.

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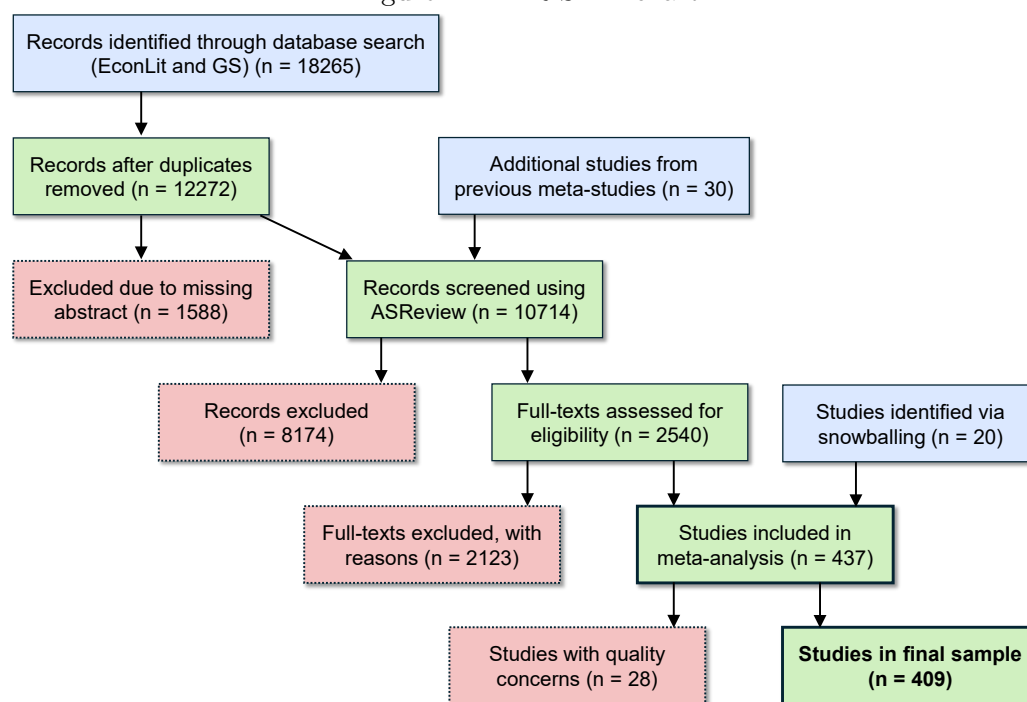
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## Online Appendix A Further information on search and data collection

### A.1 Search process

Figure A.1 shows the flow of information during the different phases of our systematic literature review as a PRISMA chart (Moher et al. 2015). Our strategy for finding relevant papers started with search queries using keywords related to monetary policy shocks and responses of output and inflation. We collected 7455 EconLit entries and 10810 Google Scholar entries, providing a total of 18265 potentially relevant studies. Furthermore, we added 30 studies from previous surveys that were not yet covered by our search. We removed 5993 duplicates. 1558 entries had to be excluded due to missing abstracts.

Figure A.1: PRISMA chart



Notes: The chart shows how many studies have been found and included or excluded at different stages of the search and collection process.

Two economists from the research team then independently screened titles and abstracts of the remaining 10714 records, AI-assisted by the active learning software ASReview. The AI predicts the relevance of entries based on the researcher's previous eligibility decisions and iteratively sorts studies by relevance. Based on predetermined stopping criteria (van de

Schoot et al. 2021), we ended abstract screening after around 5000 entries for each researcher, as the number of relevant papers in the previous 100 screened entries decreased below 5. This approach greatly improved the efficiency of excluding irrelevant entries. Studies were excluded from full-text screening if both economists agreed on ineligibility. Overall, abstract screening led to the exclusion of 8174 entries.

We then screened the full texts (and online appendices) of the remaining 2540 potentially relevant studies in random order. The findings and characteristics of a study were coded by one of the researchers if the study met our pre-registered inclusion criteria.<sup>14</sup> Version control in study coding, complete documentation on GitHub, and frequent meetings during the coding phase ensured consistent coding decisions. Doubtful cases were discussed and decided in the group of authors. After the initial coding was completed, 10 percent of all data entries were randomly assigned to other team members for double checks (as planned in the PAP) to enhance consistency and reliability. Since double checks revealed some systematic differences in coding, we double-checked further estimates. Moreover, we systematically checked outliers with very large or small point estimates and standard errors to rule out coding errors. After all, about 50 percent of the models were double-checked.

Almost all studies report results as impulse-response graphs, which trace the dynamic response of output or inflation to a monetary policy shock across time horizons. We digitized the point estimates, error bands and their scaling for each given time horizon from graphical IRFs by using the software WebPlotDigitizer. Digitizing thousands of non-standardized impulse responses was a laborious task that could not be delegated to an AI tool and thus partly relied on human research assistants.

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<sup>14</sup>We included a study if: it was written in English; it was not retracted; it not only reported replicated results with the same data and method as in the original study; it reported (graphical) point estimates, together with either confidence intervals, error bands, or standard errors; it was not only based on descriptive, theoretical, or calibrated model analysis; it made a clear distinction between conventional and unconventional monetary policy effects; it showed responses to a 100-basis points interest rate shock or provided sufficient information to normalize to such a shock size (e.g., the shock size is scaled to one standard deviation and the size of one standard deviation is given); it used output or price responses in percent changes or provided sufficient information to transform responses accordingly. We included studies from a variety of publication types, such as peer-reviewed journal articles, working papers, hand-book chapters, edited volumes, and PhD theses. However, we excluded books, as well as Master's and Bachelor's theses.

In summary, we extracted all eligible empirical estimates that provided sufficient information to directly derive or calculate the effects of a conventional monetary policy shock (of 100bp) through interest rates on output and prices (in percent). The raw IRFs are given either in levels, log levels, log first differences, or percent changes. The output response is measured by (real) GDP, industrial production, or the output gap. The price response is given as a price index (CPI, core CPI, WPI, GDP deflator) or an inflation rate thereof. The interest rate response is typically given by, e.g. the Fed Funds Rate or some other short-to-medium term money market rate, overnight rate, or government bond rate. We closely followed Fabo et al. (2021) and Ehrenbergerova et al. (2023) for the normalization of effect sizes and confidence bands.

## A.2 Final sample

In total, the full-text screening led to the inclusion of 409 primary studies that reported 4,871 graphical IRFs from which we extracted 146,463 point estimates and confidence bands for the present study. The full dataset also includes further estimates on employment and unemployment effects that we plan to analyze in a follow-up paper. Moreover, we excluded some estimates due to quality concerns. Table A.1 summarizes the number of studies, models, IRFs and observations for the different response variables. The final sample for the present study refers to the entry in the final row. Table A.2 shows the summary statistic from other moderator variables used in the robustness checks of the multivariate meta-regression, presented in Appendix C.4

## A.3 Additional moderator variables

Section 3.3 describes the main moderator variables for our analysis. Here we summarize the additional study characteristics that serve as control variables in different robustness tests. Table A.2 provides descriptive statistics for these variables.

*Estimation methods.* By far the most important group are standard parsimonious VAR models (Sims 1980), making up 70 percent of all estimates (including small 1 percent share

Table A.1: Sample sizes with and without quality concerns by outcome variable

Dataset	Response variable	Studies	Models	IRFs	Obs.
Full dataset	Overall	437	2,136	5,531	169,737
Full dataset	Output	399	1,857	1,857	56,175
Full dataset	Prices	414	1,760	1,760	53,781
Full dataset	Employment	43	105	105	3,475
Full dataset	Unemployment	64	251	251	8,440
Full dataset	Interest rate	374	1,558	1,558	47,866
No quality concerns	Overall	415	2,009	5,169	155,676
No quality concerns	Output	377	1,735	1,735	51,405
No quality concerns	Prices	392	1,670	1,670	50,519
No quality concerns	Employment	39	99	99	3,288
No quality concerns	Unemployment	62	199	199	5,925
No quality concerns	Interest rate	354	1,466	1,466	44,539
No quality concerns	Output and/or prices and/or interest rate	409	1,933	4,871	146,463

*Notes:* The tables shows the numbers of primary studies, estimated models, impulse-response functions and observations (per horizon) of point estimates and confidence intervals for different response variables and including or excluding studies for which we flagged a quality concern. The final sample for the present study refers to the entry in the final row.

of VECM models). Local projections (Jordà 2005) have become a popular alternative in recent years and represent 13 percent of the estimates in our sample (including a small group of ARDL/dynamic OLS estimates). Another strand of the literature emphasizes the need to include more variables and thus employs factor-augmented VAR (FAVAR) models (Bernanke et al. 2005). About 9 percent of the studies account for several non-linearities or other restrictions, e.g. time-varying coefficients, global VAR (GVAR) models, etc. that we subsume under the heading of ‘other VAR’. Finally, our sample includes a small fraction of empirical DSGE models that predefine a theoretical structure on the estimated IRFs.

*Publication characteristics.* In addition to journal rank and author affiliation, we collect the publication year and the number of citations to control for publication trends. Another variable signals whether the estimate was the main research question of the study or merely a by-product. If estimates are selected for publication, this should more likely be the case for central variables of interest, and less so for further control variables. For the same reason, we code if the authors signal an estimate to be their preferred one. We deviate from the

Table A.2: Description and summary statistics for other moderator variables

Variable	Explanation	Share in %/ Mean
<b>Estimation method</b>		
VAR	Vector autoregression (VAR) + vector error correction models (VECM)	70.09
LP/ARDL	Local projection (LP) and autoregressive distributed lag (ARDL) models	12.81
FAVAR	Factor Augmented VAR (FAVAR) estimation	4.75
Other VAR	Other VAR estimation method (e.g. TVP-VAR, GVAR)	9.59
DSGE	Estimated DSGE model	2.75
<b>Publication characteristics</b>		
Publication year	Publication year of the study	2015.28
Citations	Number of citations in Google Scholar	155.95
By-product	Indicator whether the estimate was not the main research question	58.35
Preferred estimate	The authors signal an estimate to be their preferred one	38.52
<b>Measurement and sample characteristics</b>		
GDP	GDP is the output variable	34.22
IP	Industrial production is the output variable	13.09
Output gap	Output gap is the output variable	3.19
CPI	Consumer Price Index is the price level variable	29.78
GDP deflator	GDP deflator is the price level variable	16.55
Core	Core price index is the price level variable	2.30
WPI	Wholesale Price Index is the price level variable	0.86
Overnight rate	Short-term rates (including money market and policy rates)	47.26
Lending rate	Weekly to monthly lending and bond rates	34.01
Year rate	Yearly to longer-term rates	18.73
Annual frequency	Estimates based on annual frequency data	0.81
Quarterly frequency	Estimates based on quarterly frequency data	63.90
Monthly frequency	Estimates based on monthly frequency data	35.29
Panel data	Panel data used	11.63
Time series	Time series data used	88.37

*Notes:* the column share / mean is in percent of the total, except for publication year and number of citations, where the respective value represents the mean value. The standard deviation of the publication year and number of citations is 6.47 and 482.93, respectively. The total number of estimates is 146,463.

PAP here as we also planned to code a category of estimates that were deemed inferior by the authors of the primary study. However, such cases were rare and often not clear-cut, such that we left out this classification.

*Measurement and sample characteristics.* We collect information on whether the output measure is GDP (Alpanda and Zubairy 2019), industrial production (Bauer and Swanson 2023) or the output gap (Carillo 2012). For the price variable, we code whether the primary study uses a Consumer Price Index (CPI) (Bernanke et al. 2005), the GDP deflator (Christiano et al. 1999), WPI or core inflation. Concerning the interest rate variable used in primary studies, we classify them as short-term overnight rates (including most policy rates such as the Fed funds rate) (Corsetti et al. 2022), weekly to monthly lending and bond rates, and yearly to longer-term rates (Kerssenfischer 2019). Moreover, the frequency of the data and the dimension (panel vs time series) is collected.

Note that the PAP discusses further moderator variables, like control variables employed in the primary study, sample years, monetary policy regimes and economic circumstances, which we collected in our dataset, but which we plan to analyze in a follow-up paper focused on regime dependence.

#### A.4 AI summary

##### *Output response*

**Prompt.** We used the following prompt with GPT-4o mini via chatgpt.com on June 12, 2025: “Please provide an estimate of the response of output to a 100bp conventional monetary policy shock. The estimate should take into account all available scientific studies, not just one prominent study. At the same time, the estimate should rigorously summarize the conventional wisdom in the literature. Answer like an economist and expert in this field. Provide the best possible point estimate of the effect together with the corresponding 95 percent confidence intervals.”

**Answer.** “The response of output to a conventional monetary policy shock—specifically, a 100 basis point (bp) unexpected increase in the policy rate—has been studied extensively

using a variety of empirical methods, including Vector Autoregressions (VARs), local projections, high-frequency identification, and narrative approaches. While estimates vary due to identification strategies, sample periods, and model specifications, the literature has converged on a fairly robust range for the short- to medium-run effects on real GDP.

**Baseline Point Estimate:** A 100 basis point (bp) monetary policy tightening shock—defined as an exogenous, unexpected increase in the short-term nominal interest rate—typically reduces real GDP by approximately: 1.0 percent at the peak effect, usually occurring within 1 to 2 years.

Based on meta-analyses and the distribution of estimates in the literature: 95% CI:  $-0.5\%$  to  $-2.0\%$ , depending on identification strategy and economic environment.

This interval reflects heterogeneity in the estimated impulse response functions across studies, with tighter estimates clustered around the  $-1\%$  effect and larger effects in studies using high-frequency or narrative identification methods that better address endogeneity concerns.

**Conclusion:** The best point estimate of the output response to a 100bp contractionary monetary policy shock is: Real GDP falls by 1.0%, with a 95% confidence interval ranging from  $-0.5\%$  to  $-2.0\%$ , peaking around 4 to 8 quarters after the shock. This estimate is robust across multiple methodologies and is consistent with the prevailing consensus in the empirical macroeconomics literature.”

#### *Price-level response*

**Prompt.** We used the following prompt with GPT-4o mini via chatgpt.com on June 12, 2025: “Please provide an estimate of the response of the price level to a 100bp conventional monetary policy shock. The estimate should take into account all available scientific studies, not just one prominent study. At the same time, the estimate should rigorously summarize the conventional wisdom in the literature. Answer like an economist and expert in this field. Provide the best possible point estimate of the effect together with the corresponding 95 percent confidence intervals.”



**Answer.** “The price level typically responds to a 100 bp increase in the policy interest rate in a gradual manner, with the cumulative price effect being larger than the immediate inflation response because the price level reflects accumulated inflation. Based on the typical response of inflation, the following can be stated for the price level:

Point Estimate: A 100 bp increase in the policy rate would lead to a reduction in the price level of approximately 0.3% to 0.6% over the medium term (12–24 months). This estimate reflects the cumulative effect of lower inflation over time. As inflation reduces, the price level gradually falls, and the total effect after 1–2 years would generally be in the 0.3% to 0.6% range. Persistence: While the immediate effect on inflation\* might be more transitory (peaking within 12–24 months), the price level adjustment will persist. The price level may continue to adjust at a slower rate after the initial response, but the bulk of the adjustment occurs in the first two years.

The confidence interval for the price level response can be derived from the variability in inflation effects across different countries and economic conditions. As a result, the 95% confidence interval for the cumulative price level response over the 12–24 month horizon is likely to be: Lower bound: A 0.1% reduction in the price level, in cases where nominal rigidities are strong, or the economy’s response to the monetary shock is muted due to low central bank credibility or other countervailing factors. Upper bound: A 0.7% reduction in the price level, in cases where inflation expectations adjust rapidly, and the monetary policy shock is effective at curbing inflation. Thus, the full confidence interval for the price level response would be between 0.1% and 0.7% over a 12–24 month horizon.

Conclusion: To summarize, the price level response to a 100 basis point conventional monetary policy shock is expected to be: Point Estimate: A reduction of approximately 0.3% to 0.6% in the price level over the medium term (12–24 months). 95% Confidence Interval: Between 0.1% and 0.7%, accounting for different economic contexts and central bank effectiveness. This estimate reflects the gradual but persistent effect of a monetary policy shock on the price level, with the impact being more long-lasting than the immediate inflation effects.”

## Online Appendix B Further information on publication bias

### B.1 Details of $p$ -hacking and distribution of $z$ -statistics

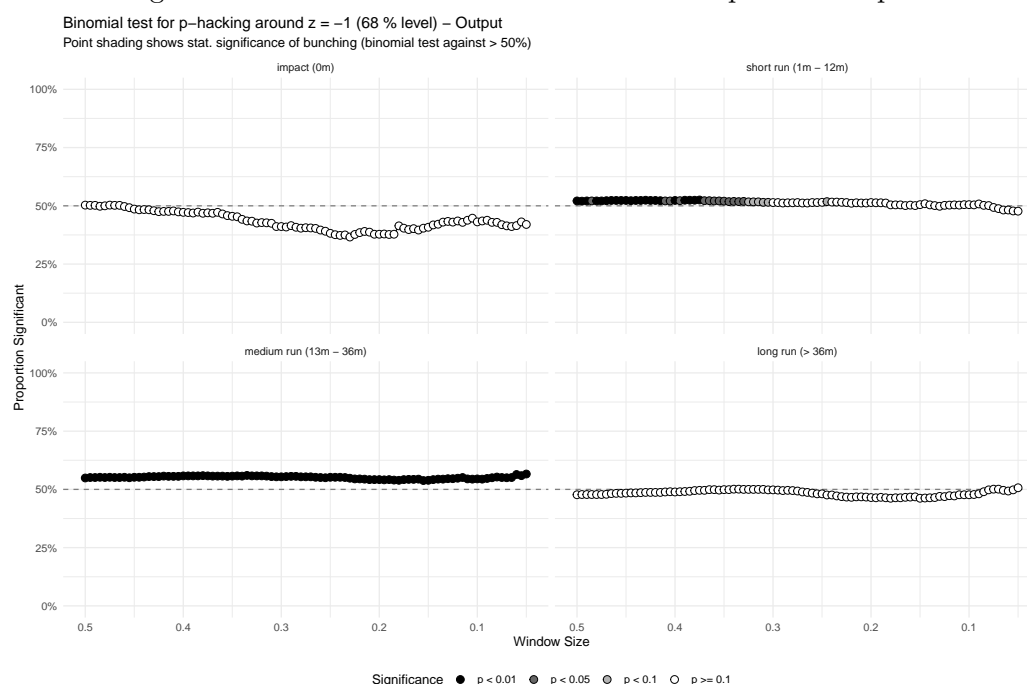
Figure B.1 and B.2 show the results of formal randomization tests of the distribution of  $z$ -statistics in symmetric (and narrowing) windows around the significance threshold. The four panels again make a distinction for the impact, short, medium, and long-term horizons for the output and price level response, respectively. On the vertical axes, they show the proportion of statistically significant estimates (at the 68 percent confidence level) in symmetric windows around a  $z$ -statistic equal to one. On the horizontal axes, the window of included estimates narrows down from left to right. In the absence of  $p$ -hacking, one would expect a random 50 percent share of significant effect sizes at all intervals and time horizons.

The test results confirm the visual inspection according to Figure 3 and Figure 4 in Section 4.2. The discontinuity is strong for the output response at the short and medium-term horizons and for the price level at the medium and long-term horizons where the share of just statistically significant estimates is consistently larger than the expected 50 percent. The effects become less clear cut at narrower windows in our case in comparison to the large sample in Brodeur et al. (2016). This may be due to the small number of observations for narrow windows in our case. Moreover, since we collect the point estimates and confidence intervals from graphs in the primary studies with varying quality, measurement error in the  $z$ -statistics could play a bigger role in our sample, which particularly affects the findings for narrow windows.

### B.2 Conditional publication probabilities: details

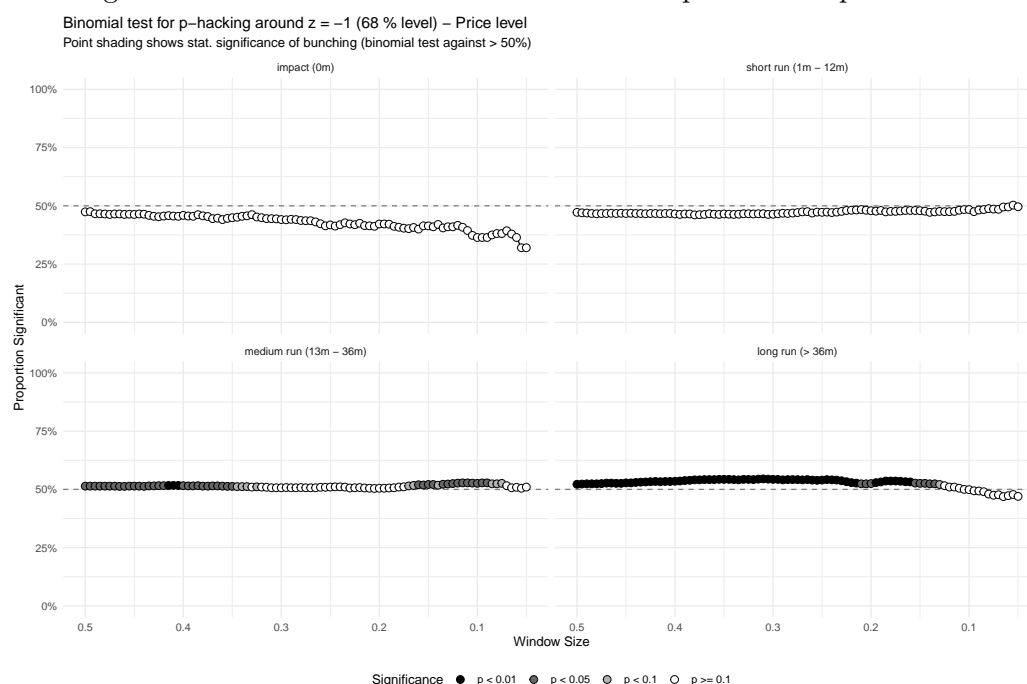
Figure B.3 provides a more granular picture of the relative publication probabilities per quarter of the IRFs. The patterns are rather similar to Figure 5 and 6 in Section 4.3. Figure B.3 also shows the test results for the interest rate response. In general, the differences in publication probabilities for different levels of  $z$ -statistics are much smaller. However, in

Figure B.1: Randomization test at different calipers for output



*Notes:* The graphs show the results of randomization tests according to Brodeur et al. (2020) for the impact, short, medium and long-term horizon of the output response, respectively. The vertical axes display the proportion of just statistically significant estimates in symmetric intervals around a  $z$ -statistic equal to one (68 percent confidence level). The horizontal axis reflects the size of the  $z$ -value intervals from 0.5 to 0, with each dot representing one specific window size. In the absence of  $p$ -hacking, one would expect a random 50 percent share of significant effect sizes at all windows and time horizons. Darker shades of the dots signal statistically significant deviations from the expected 50 percent share. Note that the number of observations shrinks with a smaller window size, which lowers the power of the test and increases the likelihood of type-II-errors.

Figure B.2: Randomization test at different calipers for the price level



*Notes:* The graphs show the results of randomization tests according to Brodeur et al. (2020) for the impact, short, medium and long-term horizon of the price level response, respectively. The vertical axes display the proportion of just statistically significant estimates in symmetric intervals around a  $z$ -statistic equal to one (68 percent confidence level). The horizontal axis reflects the size of the  $z$ -value intervals from 0.5 to 0, with each dot representing one specific window size. In the absence of  $p$ -hacking, one would expect a random 50 percent share of significant effect sizes at all windows and time horizons. Darker shades of the dots signal statistically significant deviations from the expected 50 percent share. Note that the number of observations shrinks with a smaller window size, which lowers the power of the test and increases the likelihood of type-II-errors.

the first quarter, due to the autoregressive structure of the estimated models, it is much less likely to observe a negative significant response than a positive significant one.

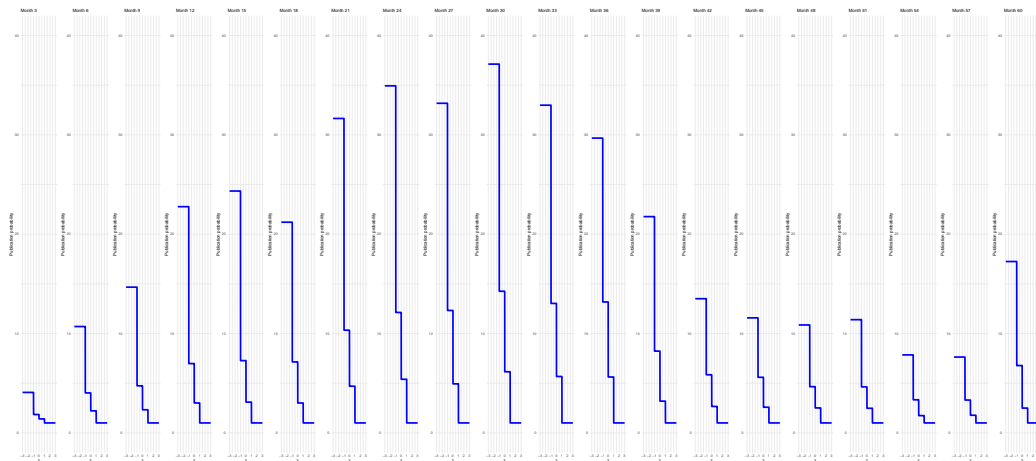
### B.3 Funnel plots

The funnel plots shown in [Figure B.4](#) and [Figure B.5](#) consist of all estimates of the output and price level responses to a 100bp contractionary monetary policy shock at the respective response horizons. The percent response of output or the price level is shown on the horizontal axis and the precision of these estimates is displayed on the vertical axis.

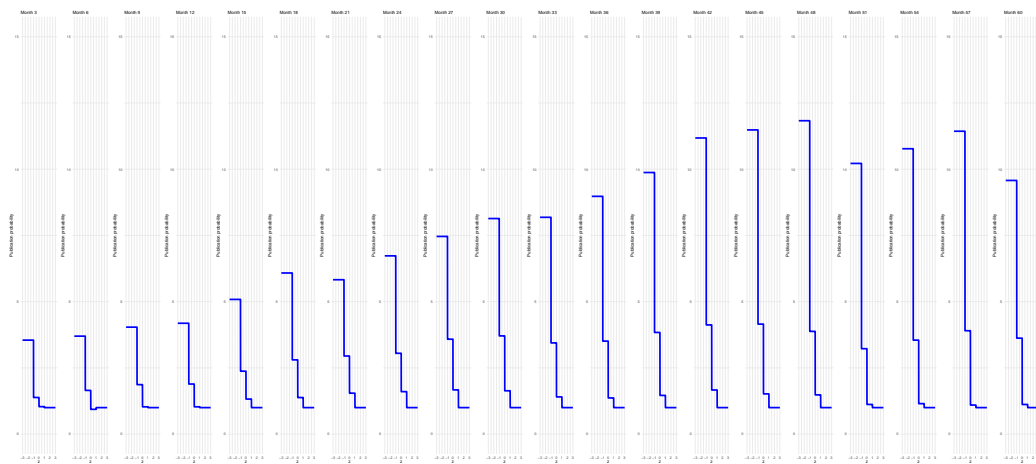
With standard econometric assumptions, the distribution should have the shape of an inverted symmetric funnel with a narrow upper end and a wide lower end: the most precise estimates are close to the “true” effect, while the least precise estimates should scatter widely on both sides. If there is only random sampling error and random misspecification bias, the least precise estimates should be equally likely to overestimate or underestimate the “true” effect size. Hence, in the absence of publication bias, the funnel plots should be symmetrical, and an asymmetrical distribution may be due to publication bias (Egger et al. 1997).

[Figure B.4](#) and [Figure B.5](#) provide further evidence of a left-skewed distribution, consistent with selective reporting of research findings at the most important response horizons. The funnel plots at short-run horizons already indicate that the least precise estimates are concentrated on the left side, pointing to an over-representation of imprecisely estimated and large dampening effects of contractionary monetary policy on output and prices. The more precise estimates tend to be closer to the vertical zero effect line. The funnel plots become more asymmetrical at the response horizons that are of particular interest in the relevant literature: researchers report fewer imprecisely estimated positive coefficients for output responses over the medium run and for price-level responses over the long run. These findings are consistent with our previous evidence of selective reporting of statistically significant and conformist results at important response horizons.

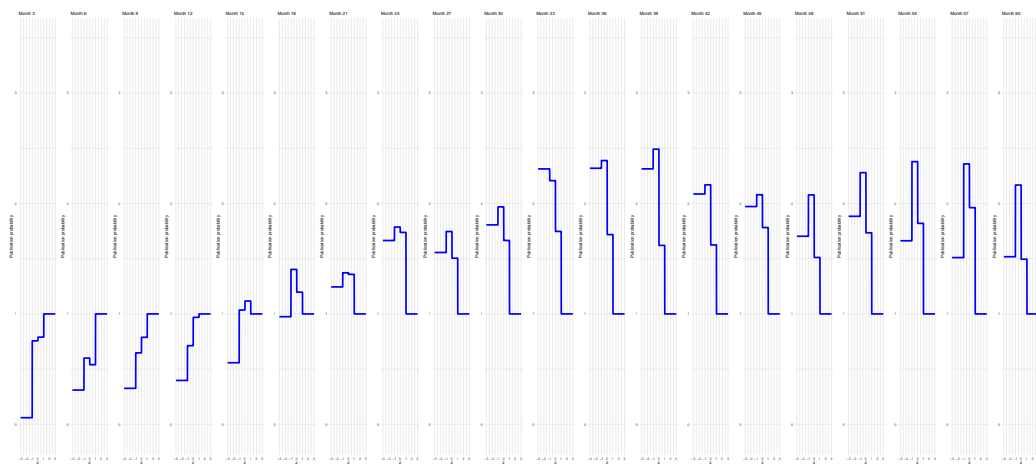
Figure B.3: Publication probability for output and price level by quarter



(a) Output



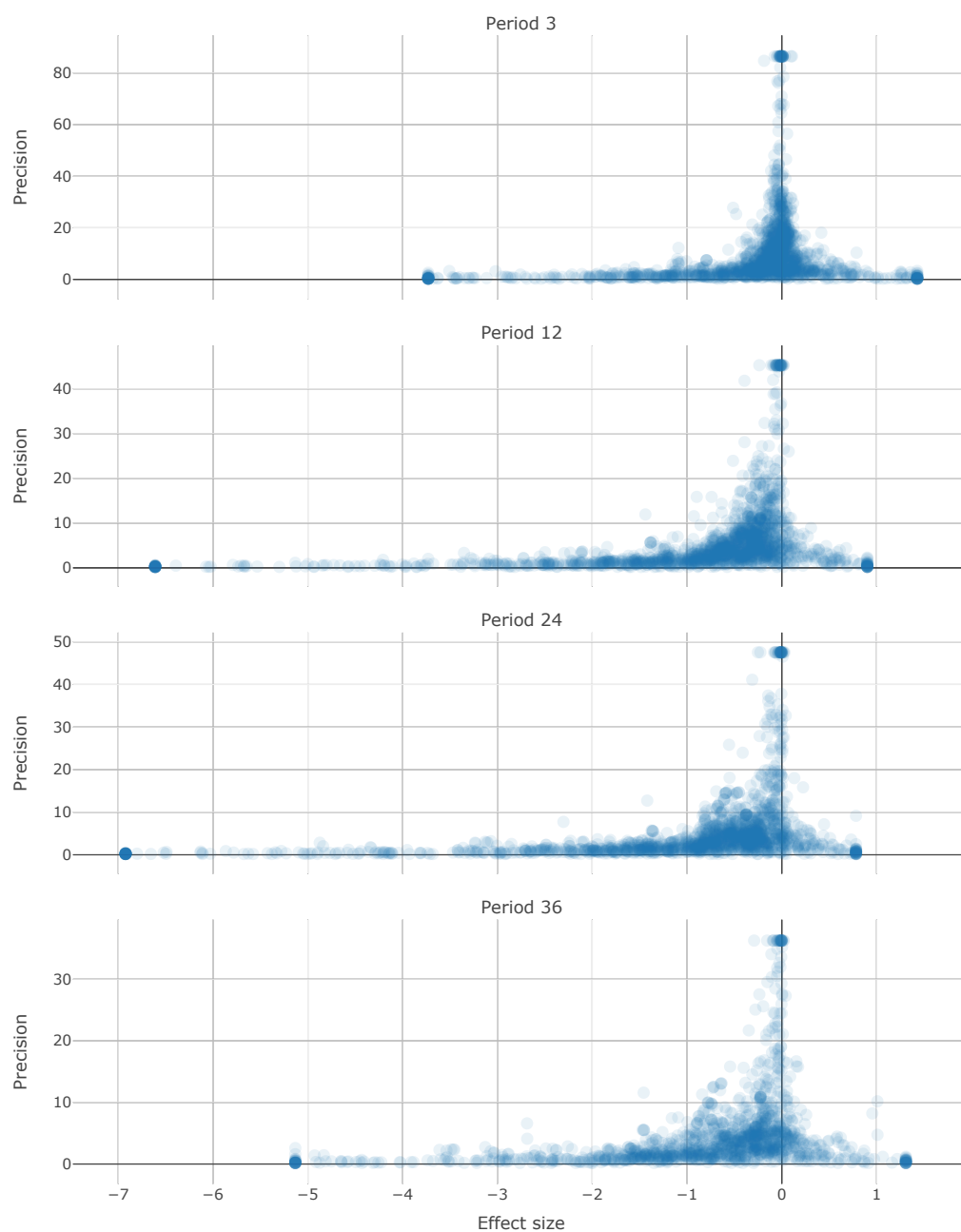
(b) Price level



(c) Interest rate

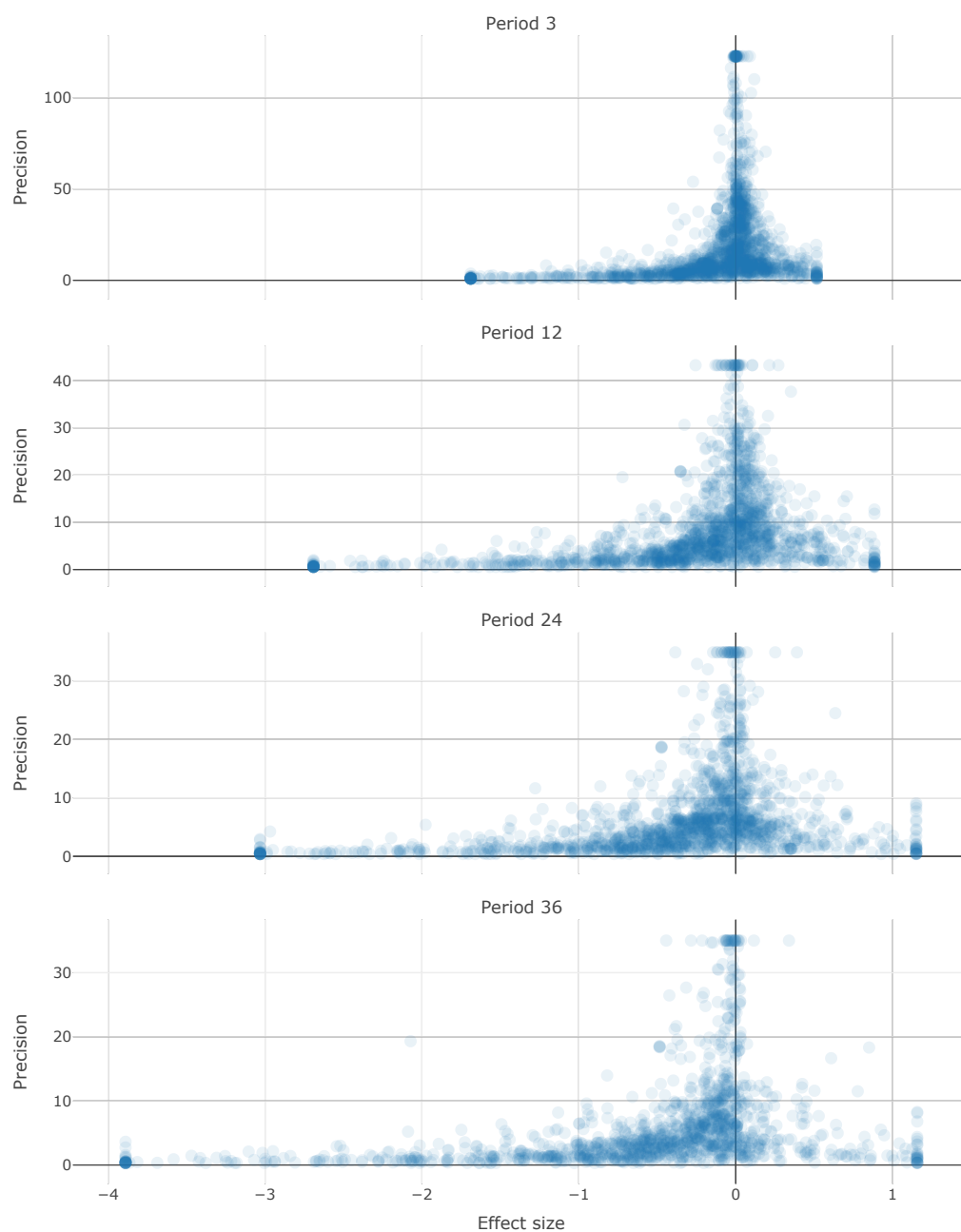
*Notes:* The figures show relative conditional publication probabilities of responses of output, the price level and the interest rate (on the vertical axis) for typical ranges of the  $z$ -statistics (displayed on the horizontal axis) at different time horizons, separately by quarter. Cutoffs are specified at  $z = 0$  and  $z = \pm 1$ . The publication probability of observations with  $z > 1$  is normalized to 1.

Figure B.4: Funnel plot for output



*Notes:* The figures show scatter plots of point estimates of output responses to a 100bp contractionary monetary policy shock on the horizontal axis and their precision (the inverse of the standard error) on the vertical axis for different horizons, respectively. In the absence of publication bias, the scatter should look symmetric.

Figure B.5: Funnel plots for the price level



*Notes:* The figures show scatter plots of point estimates of the price responses to a 100bp contractionary monetary policy shock on the horizontal axis and their precision (the inverse of the standard error) on the vertical axis for different horizons, respectively. In the absence of publication bias, the scatter should look symmetric.

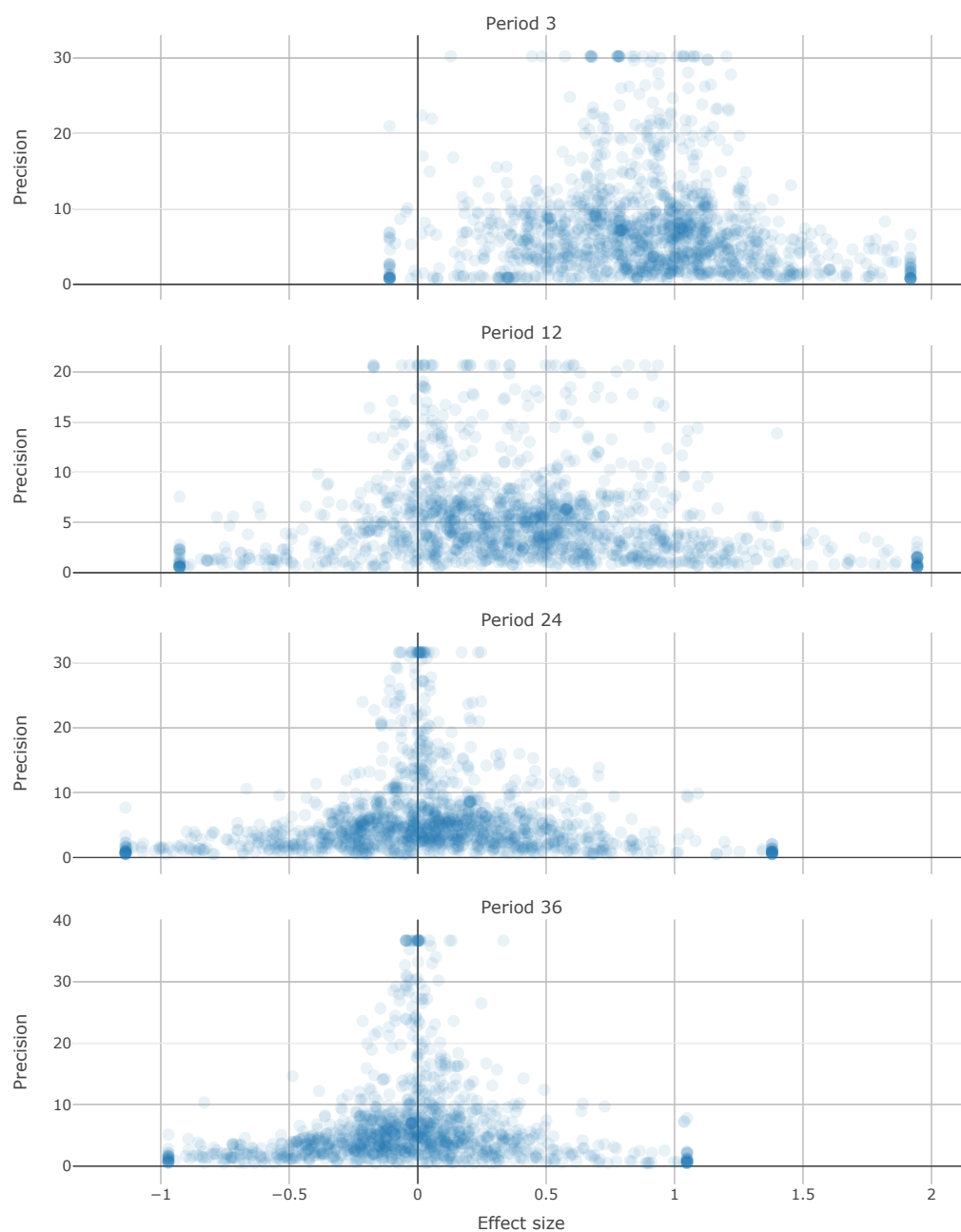


Figure B.6 shows funnel plots for the IRF of the interest rate variable. The funnel center moves from an effect size around one percentage point on impact towards zero in the medium to long run as the shock peters out. Interestingly, the funnels for the interest rate remain symmetric throughout. This provides a placebo test for our analysis of publication bias. There is no clear theory about the dynamics, size and statistical significance of the interest rate IRF. The interest rate response is usually reported for the sake of completeness, though without much notice. Consequently, we would not expect publication selection and  $p$ -hacking on the interest rate response.

#### **B.4 Details of funnel asymmetry tests and corrected effect sizes**

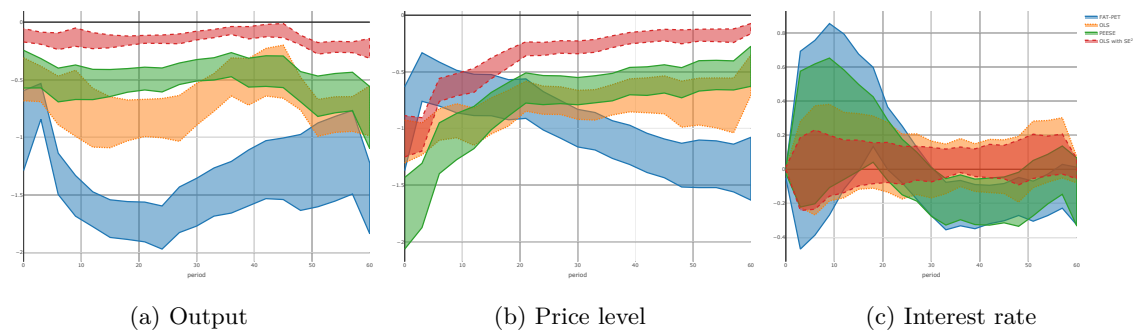
Figure B.7 shows 89% confidence bands for the publication bias coefficients from different funnel asymmetry tests (FAT) as discussed in Section 4.5 of the main text. The respective means beyond bias are displayed in Figure 7.

Figure B.6: Interest rate



*Notes:* The figure shows scatter plots of point estimates of interest rate responses to a 100bp contractionary monetary policy shock on the horizontal axis and their precision (the inverse of the standard error) on the vertical axis for different horizons. In the absence of publication bias, the scatter should look symmetric.

Figure B.7: Confidence bands of publication bias coefficients from different funnel asymmetry tests



*Notes:* The figures show the 89% confidence bands for the publication bias coefficient for output, price level and interest rate across IRF horizons.

## B.5 MAIVE

The Meta-Analysis Instrumental Variable Estimator (MAIVE) developed by Irsova et al. (2023a) is a method designed to correct for spurious precision in meta-analyses by instrumenting the reported standard error with the inverse of the sample size in a 2SLS regression. Unlike other methods, MAIVE accounts for potential selection of precision instead of selection on point estimates. The intuition is that  $p$ -hacking might affect not only the point estimate (for a given standard error), but also the standard error (for a given point estimate). In the second case, the standard error cannot be considered an exogenous variable in standard tests such as Eq. (1). Sample sizes by definition affect the size of the standard error of an estimated coefficient (relevance condition). Moreover, they are typically given to researchers (exogeneity condition). Thus, the inverse sample size qualifies as a potentially valid instrument for the squared standard error, as employed in the PEESE test.

In the first stage, we regress the squared standard error on the inverse sample size across all horizons for output and the price level. The results are shown in Table B.1.

A key concern in instrumental variable estimation is the relevance condition, typically assessed through the first-stage  $F$ -statistic. Typically, an  $F$ -statistic below 10 suggests a weak instrument. As Table B.1 shows, the  $F$ -statistics remain below 10 for all horizons, indicating that the relevance condition is not met. This weak instrument raises concerns about the validity of the second-stage regression, which we therefore do not report.

Why might the number of observations be a weak instrument for precision in our case? Importantly, the standard errors from VARs are calculated using complex methods (such as the delta method or bootstrapping), where the resulting standard errors are only weakly related to the sample size. Moreover, sample sizes and degrees of freedom are typically small and quite similar across studies, providing little variation in the explanatory variable.

## B.6 Robustness of bias-corrected effects

Figure B.9 presents several alternative versions of the bias-corrected range estimation, including random sampling of models. The first row of Figure B.9 considers a lower threshold

Table B.1: MAIVE first-stage estimation at selected horizons

Output	3	6	12	18	24	30	36	42	48	54	60
1st stage coefficient	26.05 (13.08)	47.83 (26.32)	27.93 (18.76)	33.31 (25.08)	48.31 (26.34)	37.99 (32.11)	42.00 (34.62)	32.60 (30.01)	46.42 (35.08)	47.40 (37.56)	120.80 (63.89)
F-statistic	3.96	3.30	2.22	1.76	3.36	1.40	1.47	1.18	1.75	1.59	3.57
Observations	1685	1684	1725	1648	1657	1506	1409	1239	1025	768	503
Price level	3	6	12	18	24	30	36	42	48	54	60
1st stage coefficient	3.29 (4.51)	5.40 (5.75)	4.01 (7.33)	7.47 (11.58)	13.57 (13.27)	17.37 (18.91)	24.21 (22.51)	24.51 (26.15)	4.43 (16.23)	-13.51 (14.00)	39.74 (44.14)
F-statistic	0.53	0.88	0.30	0.42	1.05	0.84	1.16	0.88	0.07	0.93	0.81
Observations	1640	1639	1662	1601	1586	1454	1355	1236	1013	810	510

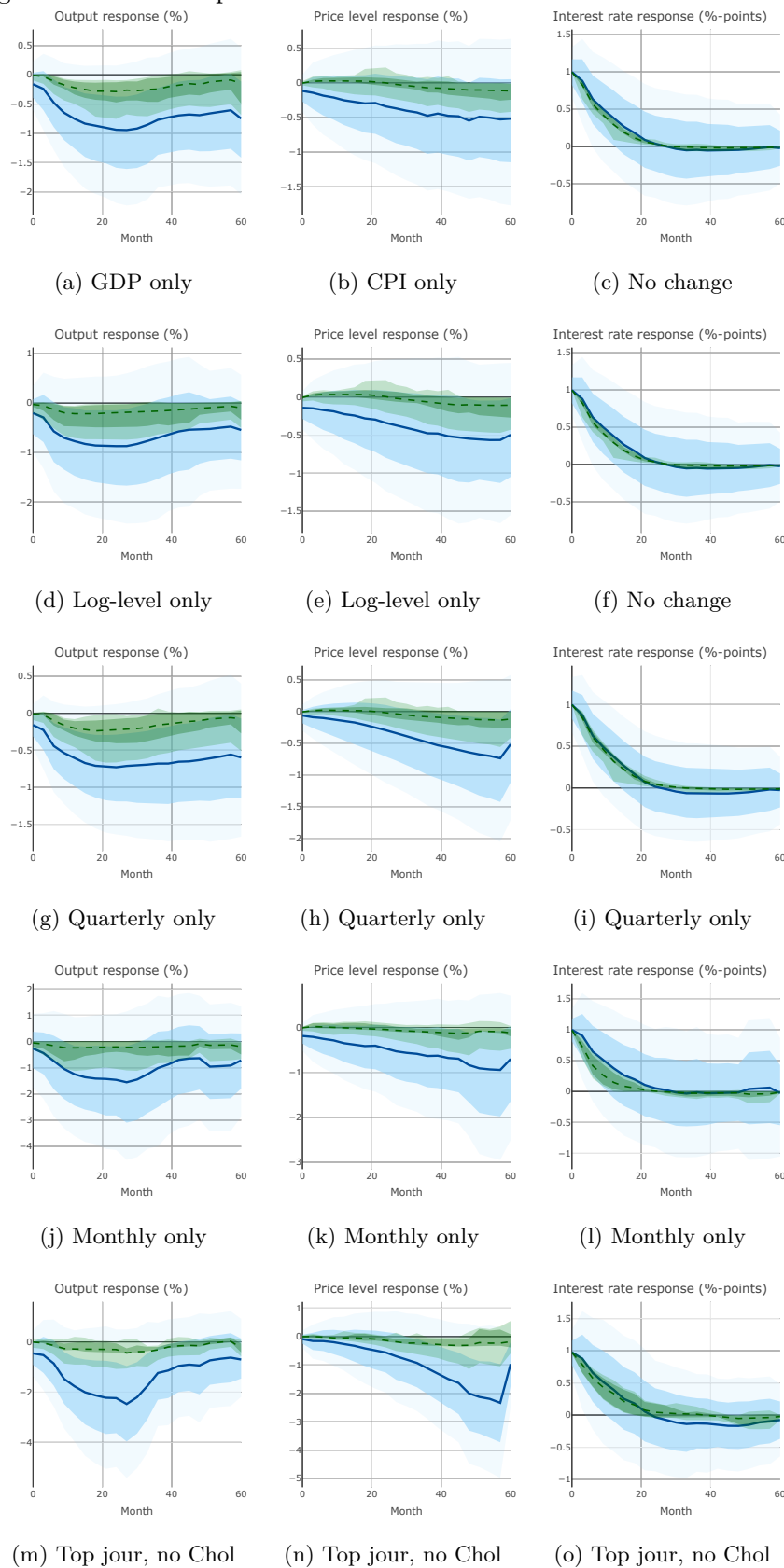
*Notes:* heteroskedasticity-robust standard errors in parentheses (clustered at study-level). The F-test for the first step is the robust Kleibergen-Paap F-statistic. MAIVE version of PEESE without weights and including study fixed effects as preferred by Irsova et al. (2023a).

for adequate power for the WAAP test. In its original form, the WAAP uses inverse variance weights and sets the threshold for adequate power at 80% in the case of an underlying 5% significance level. However, in monetary policy VARs, degrees of freedom are typically low and hypothesis tests often have low power. One standard deviation (or 68%) confidence bands are conventionally adopted to illustrate the statistical significance of estimated IRFs. We therefore modify the WAAP threshold of adequate power in line with this convention. We use a threshold for the t-statistic of 1.84 ( $t = 1$  for the 68 percent significance threshold + 0.84 for the standard normal value referring to a 20/80 split in the cumulative distribution, consistent with 80 percent power level), instead of 2.8 ( $t = 1.96$  for the 95 percent significance threshold + 0.84) suggested by Ioannidis et al. (2017).

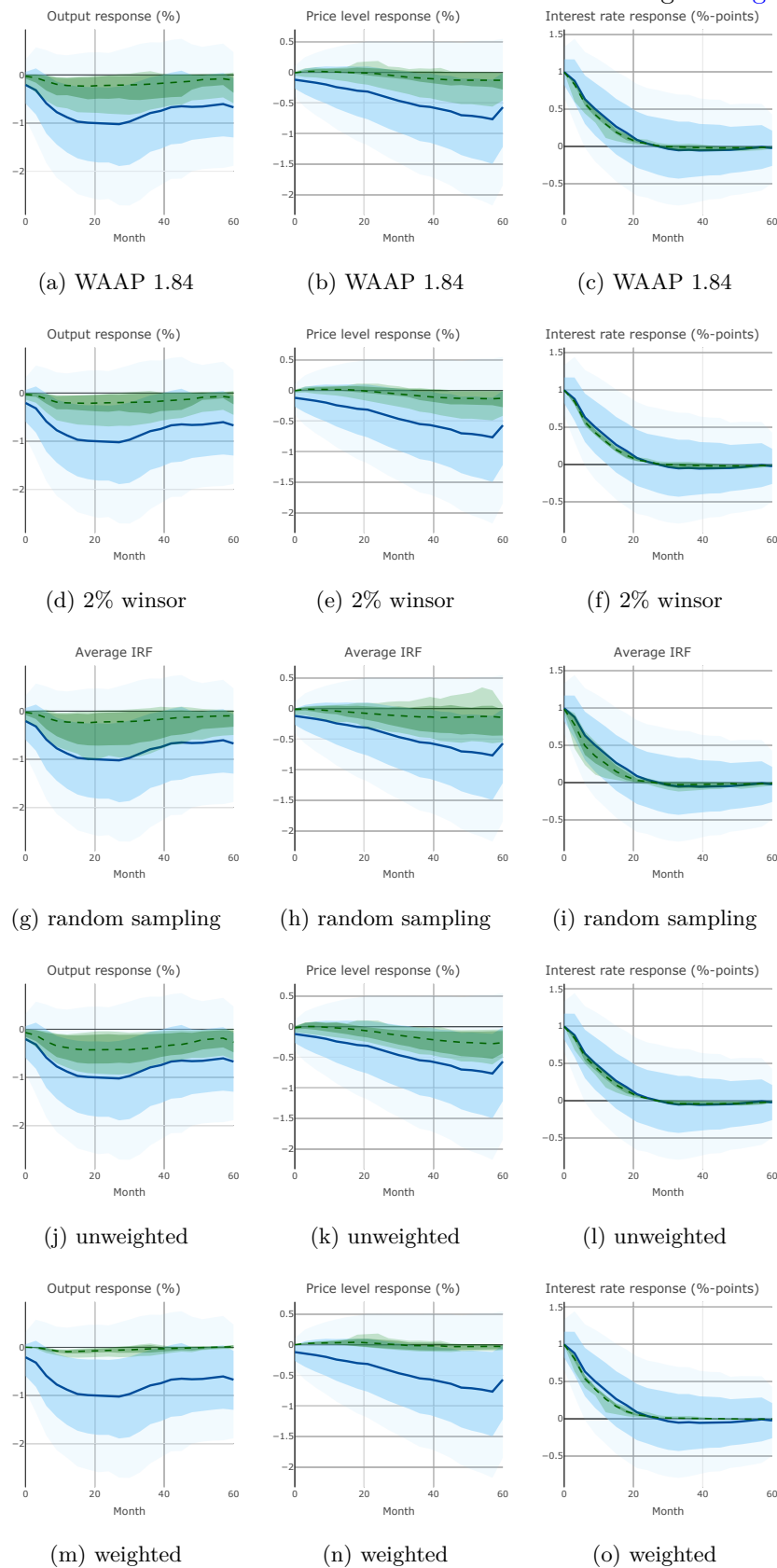
Figure B.8 provides sub-sample robustness checks for the bias-corrected range displayed in Figure 1. For some sub-samples,<sup>15</sup> the threshold for adequately powered estimates needed to be lowered to a value in line with 68% (or 1 standard error) confidence intervals, which are commonly used in the literature on monetary policy shocks.

<sup>15</sup>Specifically, the CPI-only estimation of the price level and the monthly-only estimations for output and the interest rate.

Figure B.8: Sub-sample robustness checks for bias-corrected IRF range



*Notes:* For interpretation, please refer to notes and legend of [Figure 1](#). **Row 1:** Only GDP and CPI as measures for output and price level in original models. **Row 2:** Only log-level specifications in original model for output and price level. **Row 3:** Only models using quarterly data. **Row 4:** Only models using monthly data. **Row 5:** Only models published in top journals and excluding Choleski/SVAR models. Otherwise equal to specifications in [Figure 1](#).

Figure B.9: Alternative estimation of bias-corrected IRF range in [Figure 1](#)

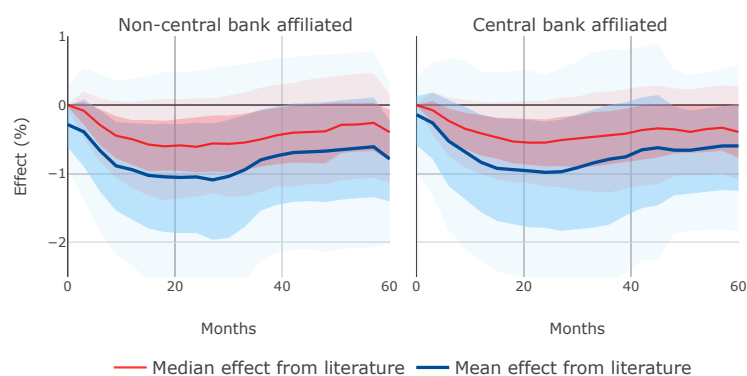
*Notes:* For interpretation, please refer to notes and legend of [Figure 1](#). **Row 1:** Alternative selection of adequately powered for (W)AAP in line with 68% (or 1 standard error) confidence bands, resulting in a threshold t-statistic of 1.84 (Ioannidis et al. 2017). **Row 2:** Only 2% winsorization. **Row 3:** Sampling: 1 model per study, repeated 10 times. **Row 4:** Unweighted tests only. **Row 5:** Weighted tests only. Otherwise equal to specifications in [Figure 1](#).

## Online Appendix C Further information on heterogeneity

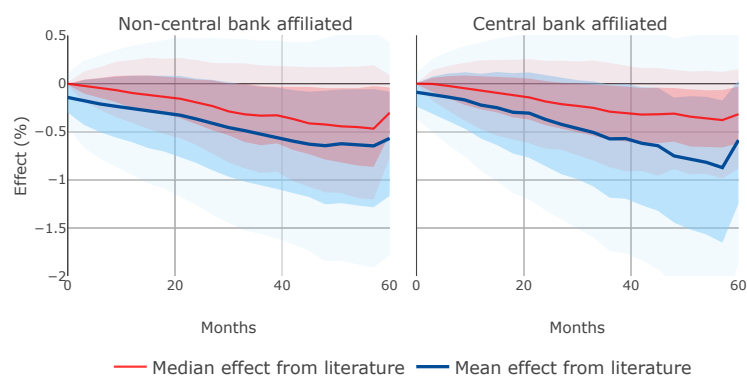
### C.1 Reported effects – central bank affiliation vs no central bank affiliation

Figure C.1 shows the reported effects for central bank affiliation versus no central bank affiliation.

Figure C.1: Average, median and corrected responses to a conventional monetary policy shock - central bank affiliation vs no central bank affiliation



(a) Output response



(b) Price level response

*Notes:* The figures show average responses (blue line) and median responses (red line) of output and the price level in percent with confidence intervals (shaded areas) after a contractionary 100bp conventional monetary policy shock by central bank affiliation.

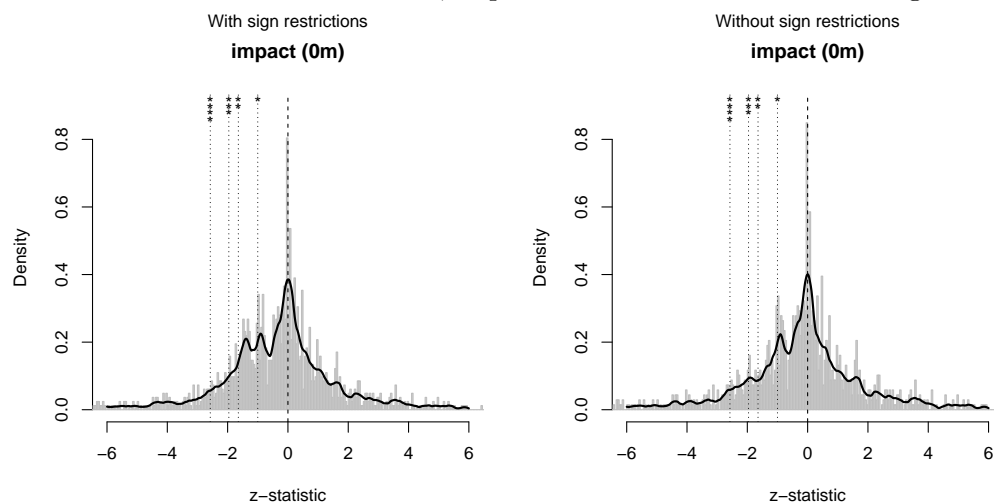


## C.2 Sub-sample density of z-statistics

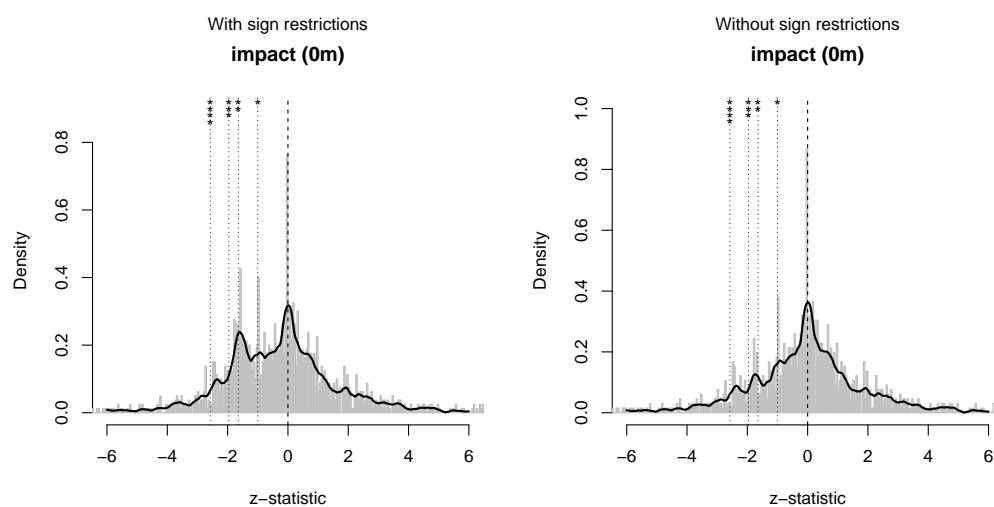
Figure C.2 shows the distribution of z-statistics for the response on impact, reproducing Figure 3 and 4 with and without the sign-restrictions estimates. The humps at about  $z = -1.5$  largely disappear when sign-restriction estimates are excluded, although for output, a second hump at about 0.9 remains.

Figure C.3 and Figure C.4 show the distribution of z-statistics for top journals versus other publications. Figure C.5 and Figure C.6 show the distribution of z-statistics for studies with a central bank affiliation vs. non-central bank affiliation.

Figure C.2: Distribution of z-statistics, impact horizon with and without sign restrictions



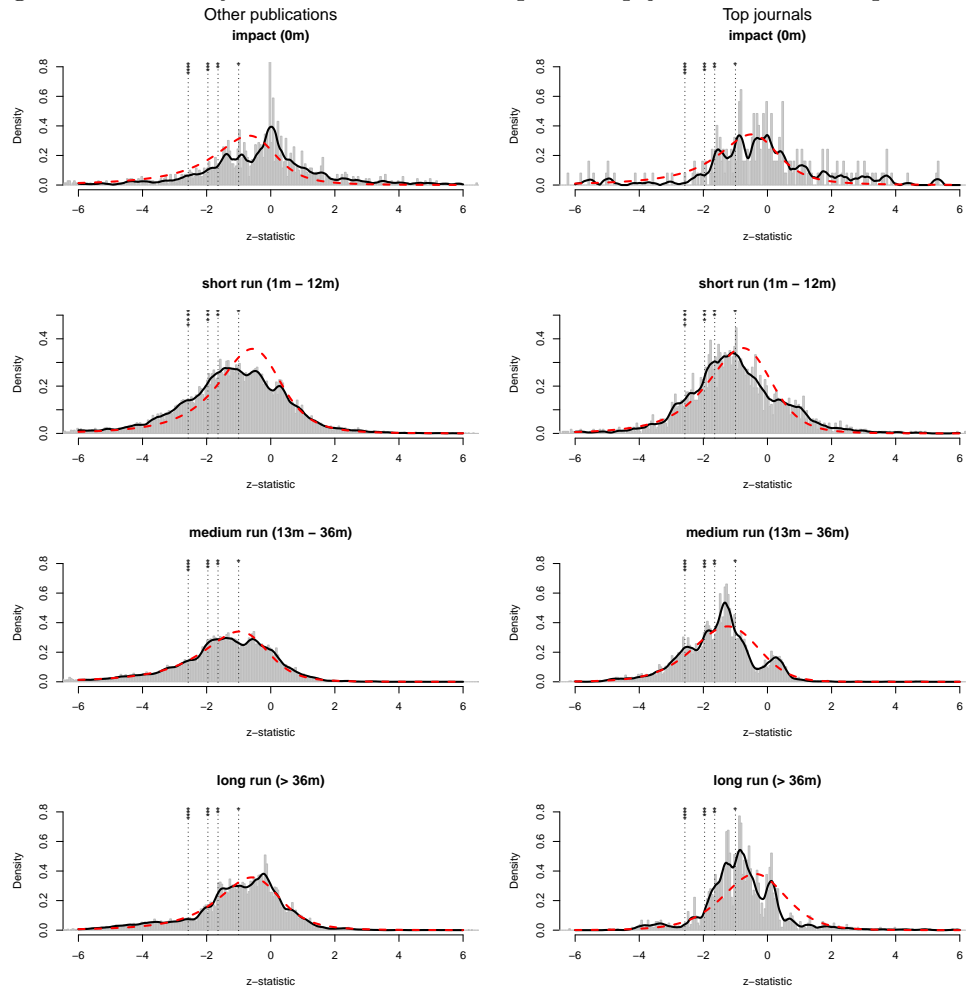
(a) Output



(b) Price level

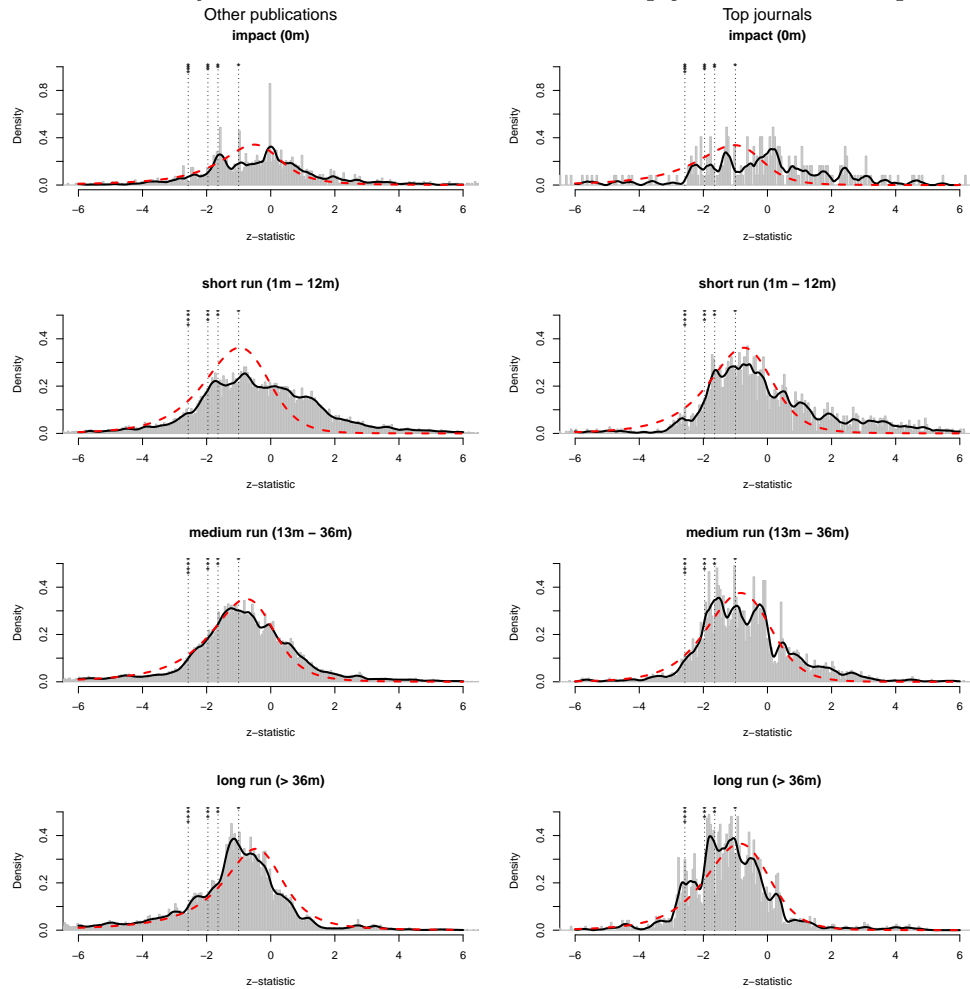
Notes: The figure reproduces [Figure 3](#) and [4](#) for the response of output and prices on impact, with and without the sign-restrictions estimates.

Figure C.3: Density of z-statistics for Output - Top journals vs other publications



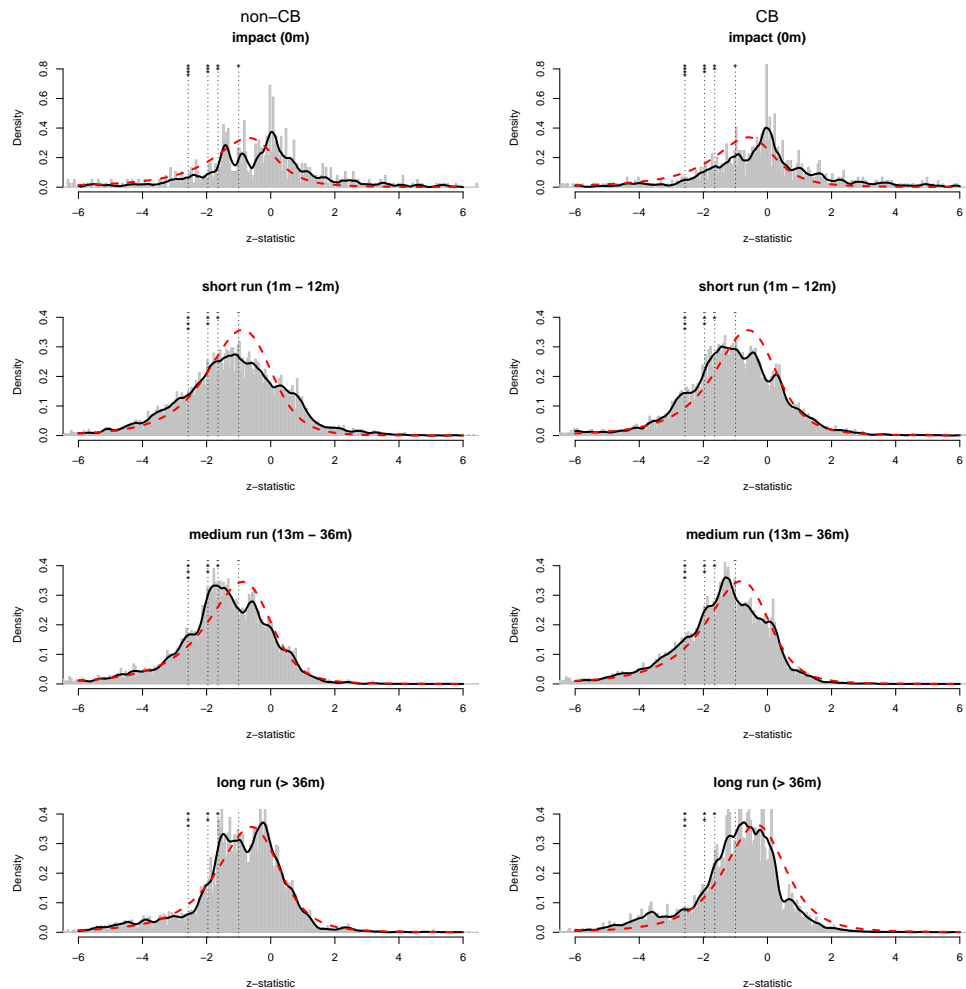
*Notes:* The z-statistics are calculated from the point estimates, divided by their standard errors according to the upper bounds of the reported confidence intervals. The gray bars show the densities of z-statistics in bins. The vertical dotted lines indicate standard significance thresholds at the 68, 90, 95 and 99 percent confidence level (from left to right). The solid black line represents a smoothed kernel density of the z-statistics. The red dashed lines show counterfactual kernel densities, derived from a regular distribution of z-values. The counterfactual is calibrated by matching the empirical mass for observations with  $z < -5$ , as *p*-hacking is unlikely in this range (Brodeur et al. 2016). The figure shows the distribution of z-statistics for output estimates in leading journals versus other publications.

Figure C.4: Density of z-statistics for Price Level - Top journals vs other publications



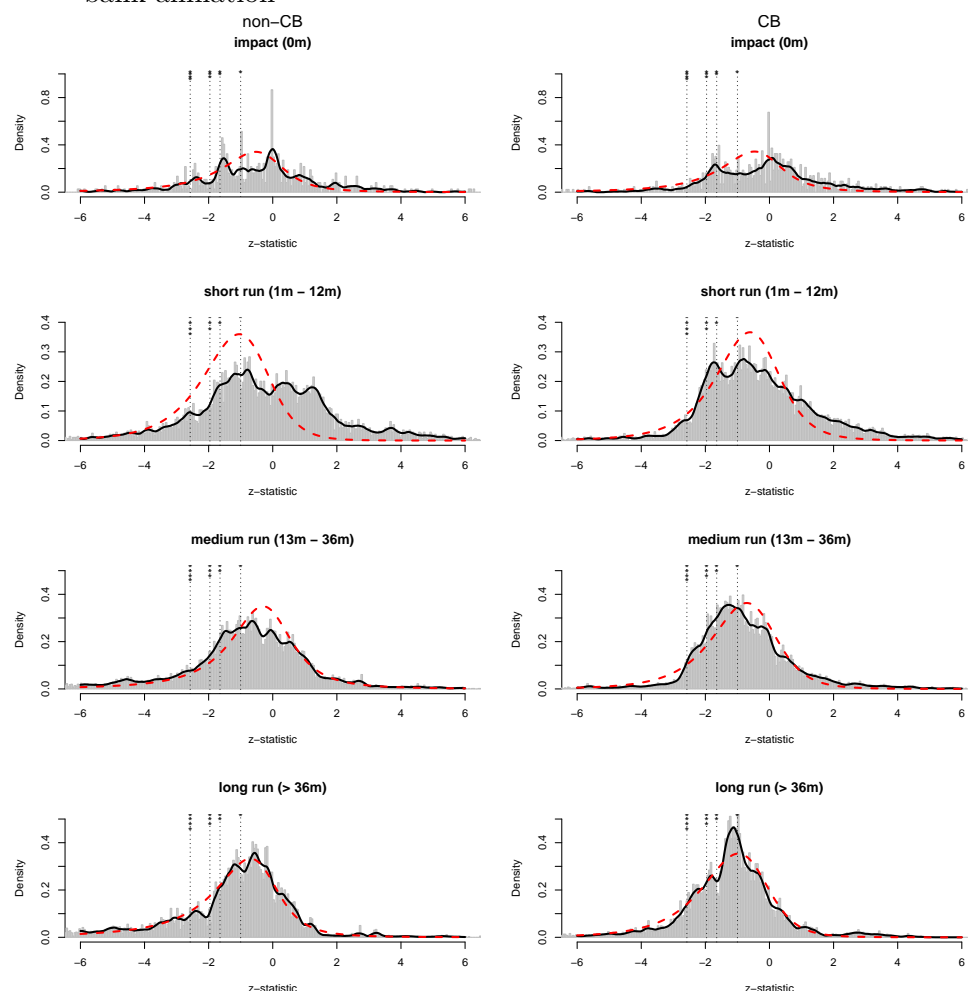
*Notes:* The z-statistics are calculated from the point estimates, divided by their standard errors according to the upper bounds of the reported confidence intervals. The gray bars show the densities of z-statistics in bins. The vertical dotted lines indicate standard significance thresholds at the 68, 90, 95 and 99 percent confidence level (from left to right). The solid black line represents a smoothed kernel density of the z-statistics. The red dashed lines show counterfactual kernel densities, derived from a regular distribution of z-values. The counterfactual is calibrated by matching the empirical mass for observations with  $z < -5$ , as *p*-hacking is unlikely in this range (Brodeur et al. 2016). The figure shows the distribution of z-statistics for price level estimates in leading journals versus other publications.

Figure C.5: Density of z-statistics for Output - Central bank affiliation vs no central bank affiliation



*Notes:* The z-statistics are calculated from the point estimates, divided by their standard errors according to the upper bounds of the reported confidence intervals. The gray bars show the densities of z-statistics in bins. The vertical dotted lines indicate standard significance thresholds at the 68, 90, 95 and 99 percent confidence level (from left to right). The solid black line represents a smoothed kernel density of the z-statistics. The red dashed lines show counterfactual kernel densities, derived from a regular distribution of z-values. The counterfactual is calibrated by matching the empirical mass for observations with  $z < -5$ , as  $p$ -hacking is unlikely in this range (Brodeur et al. 2016). The figure shows the distribution of z-statistics for output estimates comparing authors with central bank affiliation versus those with no central bank affiliation.

Figure C.6: Density of z-statistics for Price Level - Central bank affiliation vs no central bank affiliation

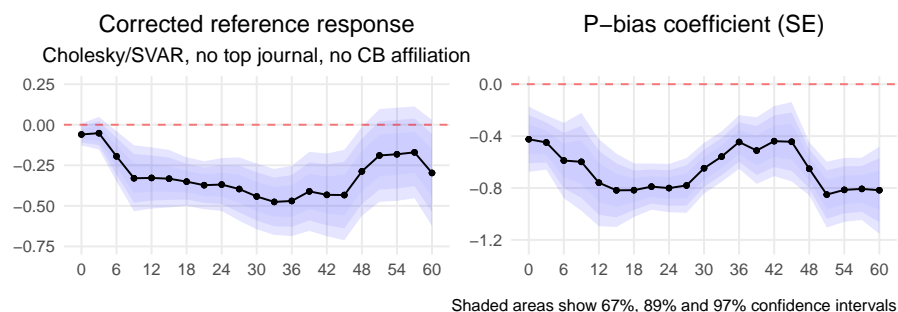


*Notes:* The z-statistics are calculated from the point estimates, divided by their standard errors according to the upper bounds of the reported confidence intervals. The gray bars show the densities of z-statistics in bins. The vertical dotted lines indicate standard significance thresholds at the 68, 90, 95 and 99 percent confidence level (from left to right). The solid black line represents a smoothed kernel density of the z-statistics. The red dashed lines show counterfactual kernel densities, derived from a regular distribution of z-values. The counterfactual is calibrated by matching the empirical mass for observations with  $z < -5$ , as *p*-hacking is unlikely in this range (Brodeur et al. 2016). The figure shows the distribution of z-statistics for price level estimates comparing authors with central bank affiliation versus those with no central bank affiliation.

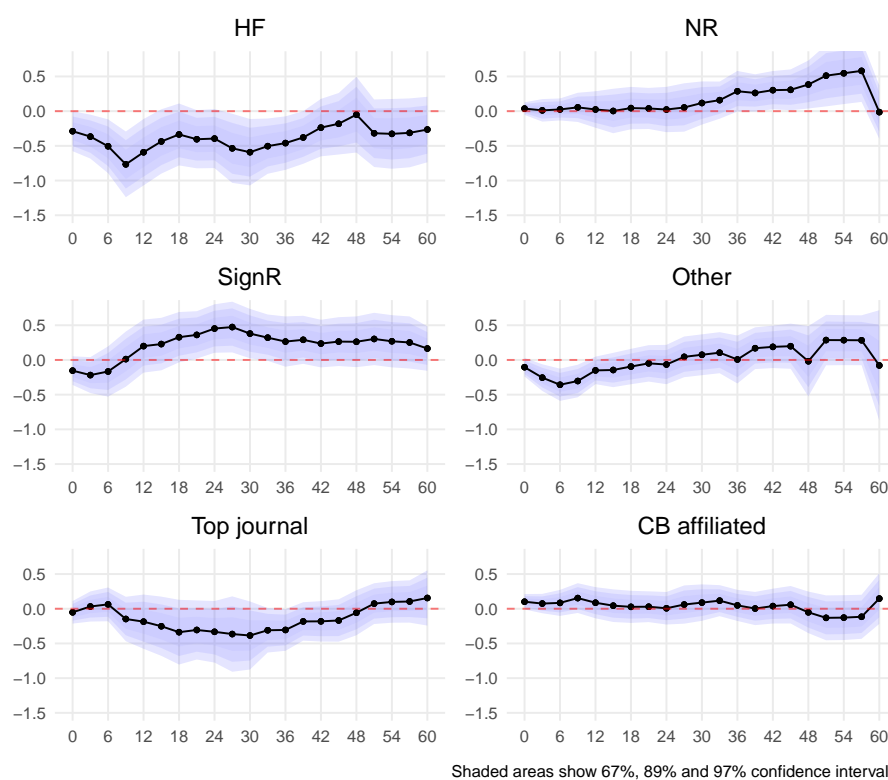
### C.3 Baseline results of multivariate meta regressions (MMR)

Figure C.7 and C.8 illustrate the coefficients of our baseline OLS specification from Eq. (2) along the estimation horizons. In both figures, panel (a) displays the development of the intercept  $\beta_{0,h}$  of Eq. (2), which is the corrected effect for the reference specification (i.e. Cholesky/SVAR, no top journal, no CB affiliation), and for  $\beta_{1,h}$ , which should signal publication bias. Panel (b) shows the respective differentials for the other identification methods and publication characteristics.

Figure C.7: Visualization of baseline MMR coefficients for output per horizon



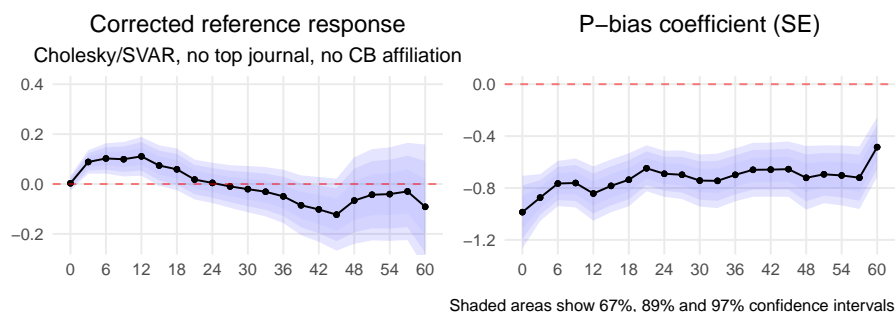
(a) MMR intercept and publication bias coefficient



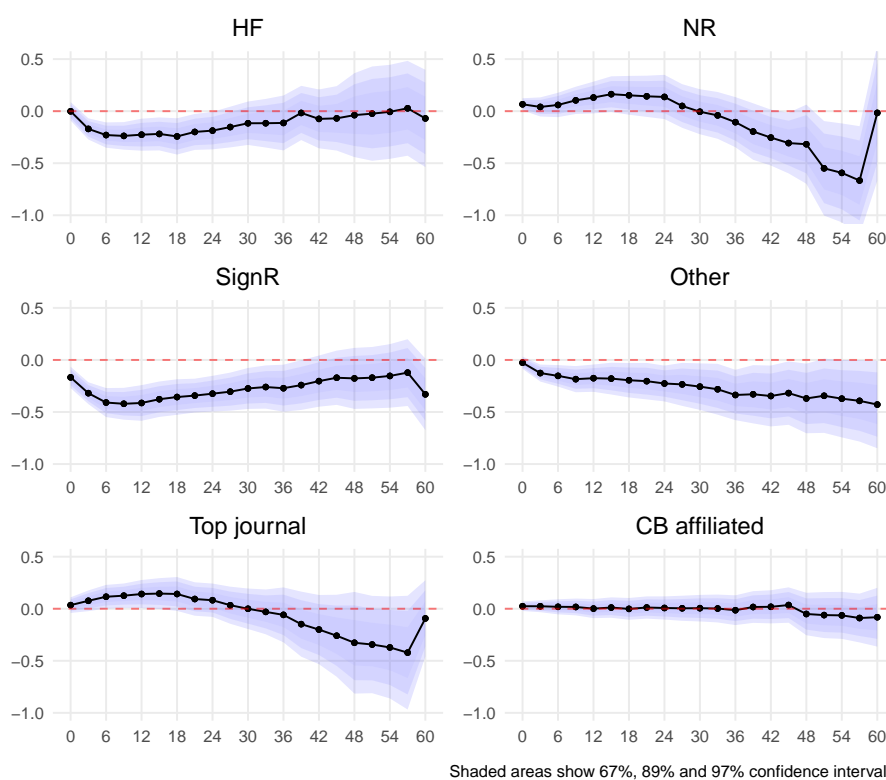
(b) MMR moderator coefficients. Differential to publication bias corrected effect for Cholesky/SVAR, no top journal, no central bank affiliation.



Figure C.8: Visualization of baseline MMR coefficients for the price level per horizon



(a) MMR intercept and publication bias coefficient



(b) MMR moderator coefficients. Differential to publication bias corrected effect for Cholesky/SVAR, no top journal, no central bank affiliation.

## C.4 Robustness of MMR

We include additional control variables like measurement of output, prices and interest rates, estimation methods, sample characteristics and further publication characteristics to check the robustness of our baseline specification of Eq. (2). We include as categorical dummies the main categories of estimation methods (VAR vs. local projections and ARDL models, FAVAR, other VARs and DSGE models), the measurement of output (GDP vs. industrial production, output gap), the price level (CPI vs. GDP deflator, WPI, core inflation), the measurement of the interest rate (overnight vs. weekly/monthly rate, yearly rate), preferred estimate dummy, by-product dummy, data frequency categories (monthly, quarterly, annualy), and panel data dummy. Continuous control variables are publication year and the number of citations, both demeaned.

In addition to the inclusion of these additional moderator in our baseline MMR regression, we also conduct a Bayesian model averaging exercise to address the model uncertainty due to the large number of possible control variables. For each IRF horizon, the BMA runs a large set of regression models, each using a different combination of our potential moderator variables from Table 1 and Table A.2. To make this practically feasible given  $2^n$  possible subsets for  $n$  moderator variables, we apply an MCMC sampler using the Metropolis-Hastings algorithm to focus on the most important part of the posterior model distribution (Zeugner and Feldkircher 2015). As a result of the BMA we obtain a weighted average of these models, where the weights correspond to the posterior model probability. We also obtain the posterior inclusion probability (PIP) for each moderator. We use a unit information g-prior (Eicher et al. 2011) and a dilution model prior (George 2010) to address collinearity.

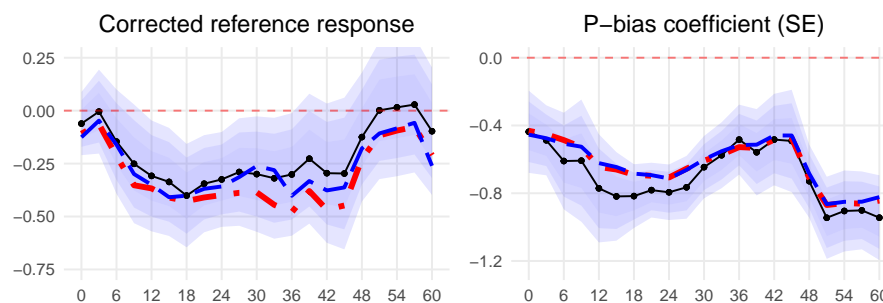
We perform the BMA exercise using two different approaches. The first version is a robustness check to determine whether our baseline coefficients, which are included on theoretical grounds, are robust to model uncertainty with respect to other control variables. In this version, we keep the baseline regressors fixed across models for theoretical reasons and let the other moderator variables be selected by the algorithm. For *SE*, representing the publication bias test, we also let the algorithm decide upon its inclusion. In the second

BMA approach, we completely let the data speak by letting all variables be selected by the algorithm. This provides a further robustness check on the importance of our moderator variables in explaining the variance in our data under model uncertainty.

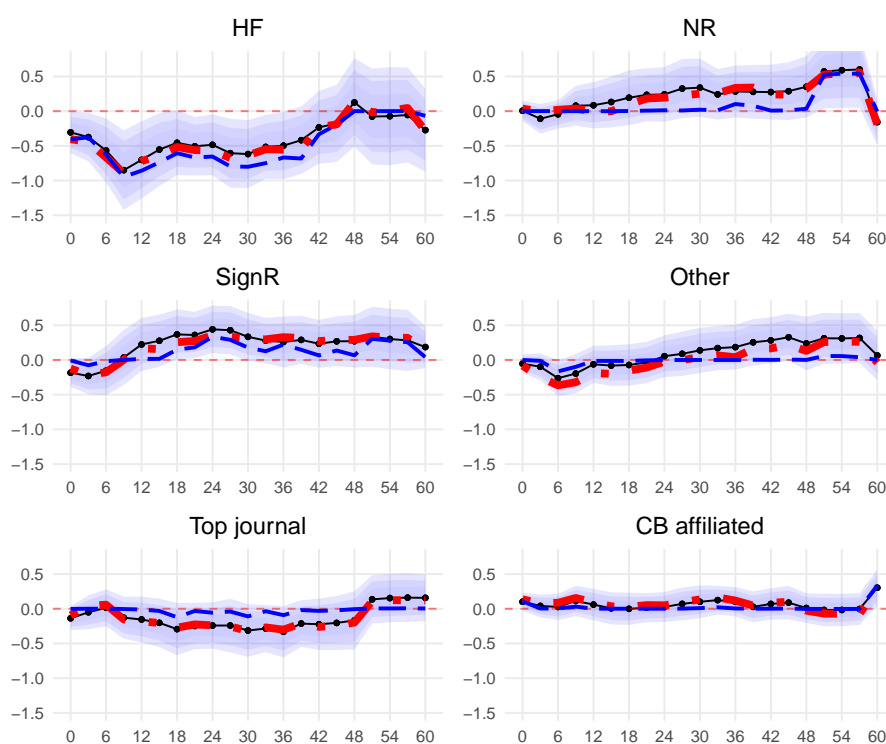
Figure C.9 and Figure C.10 compare the coefficients of our baseline variables from the extended MMR, estimated with all moderator variables, with the BMA-weighted coefficients for both BMA approaches (red line for the version with fixed baseline variables; blue line for the agnostic version). Figure C.11 and Figure C.12 show the corresponding posterior inclusion probabilities of our baseline variables. The red line shows the first BMA version, where the PIP of the baseline moderators (except for  $SE$ ) is 1 by assumption. The blue line again shows the agnostic PIPs. Most importantly, the results from our BMA exercise show that the publication bias term ( $SE$ ) is included in all models and across horizons for both BMA versions. For identification methods, the importance of explaining the variance in the data in the empirical BMA varies across horizons, with some exceptions. These BMA results therefore confirm and highlight our earlier finding that publication bias seems to play an important role, also in comparison to identification. In addition, the MMR coefficients appear to be broadly robust to BMA.

Finally, Figure C.13 compares the publication bias-corrected predicted values from Figure 11 with the respective uncorrected sub-sample averages by identification method. It also provides a WLS-PEESE robustness check of our baseline OLS MMR specification. In line with the results from Figure 7, our baseline correction for publication bias is somewhat weaker than the WLS-PEESE version. For both versions publication bias remains important compared to the differences between identification methods as the corrected effects diverge less than the uncorrected sub-sample averages.

Figure C.9: Robustness: visualization of MMR coefficients for output per horizon - OLS version with all moderators and BMA comparison



(a) MMR intercept and publication bias coefficient

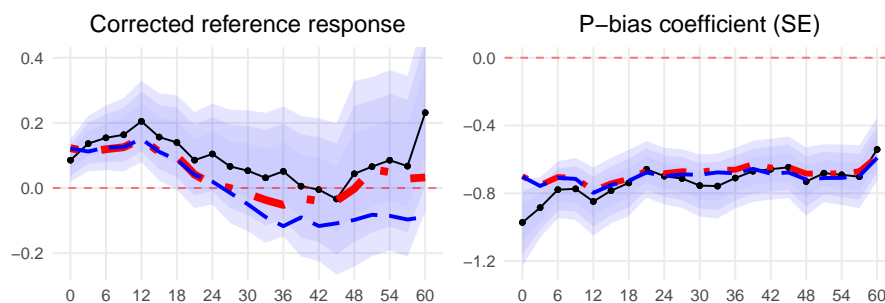


Shaded areas show 67%, 89% and 97% confidence intervals

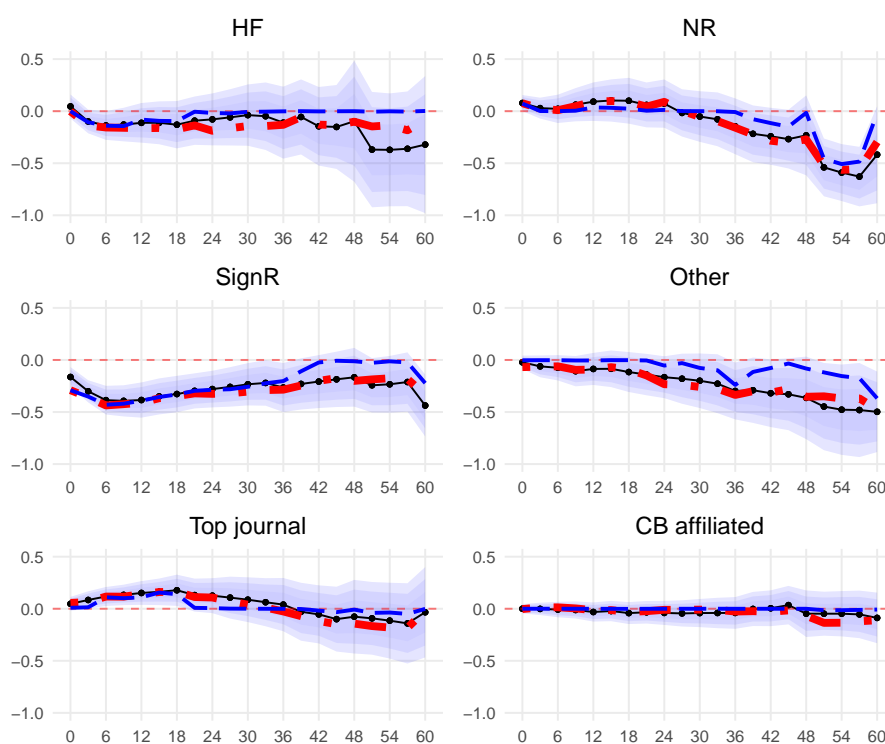
(b) MMR moderator coefficients. Differential to publication bias corrected effect for Cholesky/SVAR, no top journal, no central bank affiliation.

Notes: Re-estimation of Figure C.7 with additional moderators (black line and intervals) and using BMA (red: with fixed baseline variables except for the variance; blue: no fixed variables).

Figure C.10: Robustness: visualization of MMR coefficients for the price level per horizon - OLS version with all moderators and BMA comparison



(a) MMR intercept and publication bias coefficient

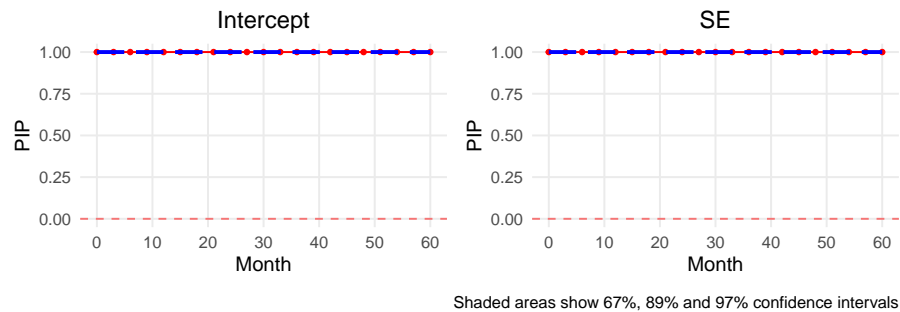


Shaded areas show 67%, 89% and 97% confidence intervals

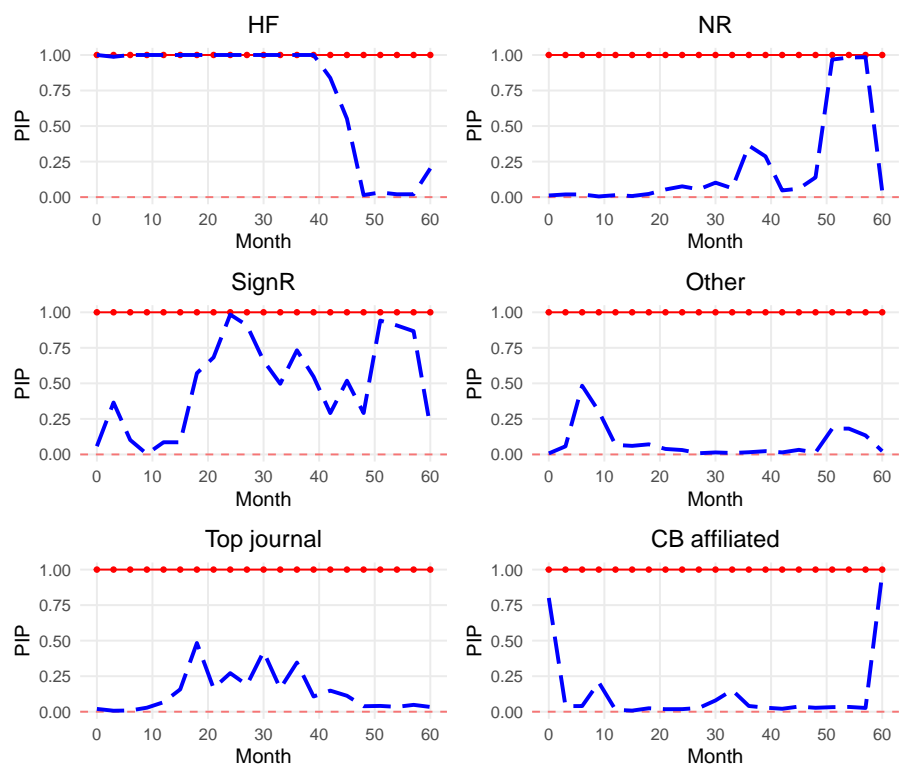
(b) MMR moderator coefficients. Differential to publication bias corrected effect for Cholesky/SVAR, no top journal, no central bank affiliation.

Notes: Re-estimation of Figure C.8 with additional moderators (black line and intervals) and using BMA (red: with fixed baseline variables except for the variance; blue: no fixed variables).

Figure C.11: Posterior Inclusion Probabilities (PIP) from BMA - output



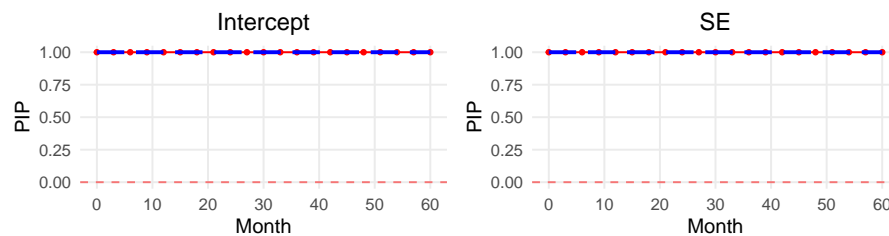
(a) PIP of intercept and publication bias coefficient



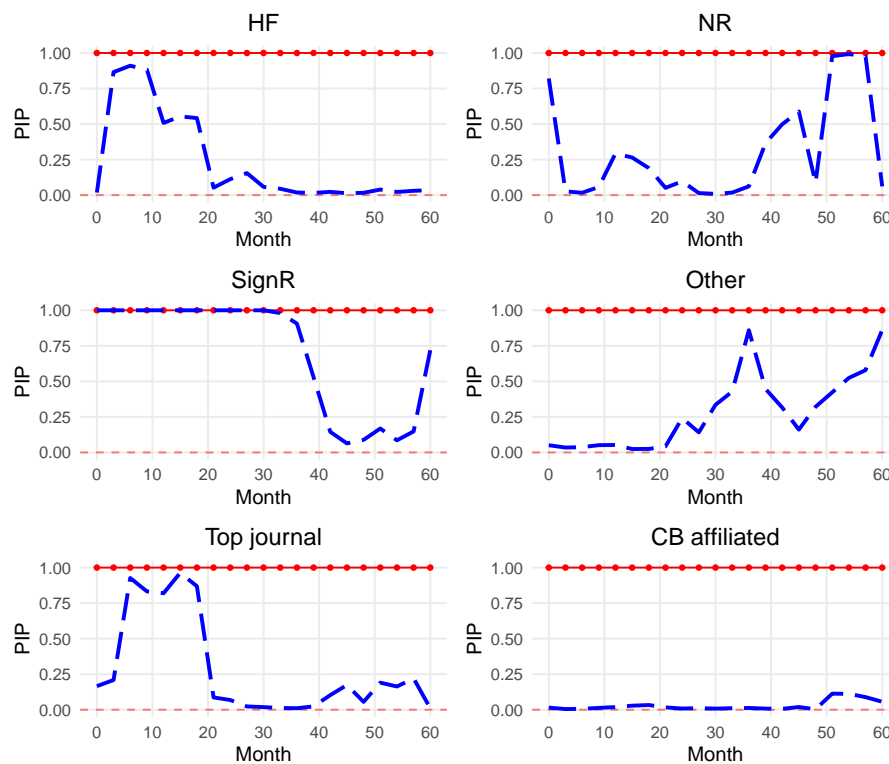
(b) PIP of identification methods

Notes: BMA Posterior Inclusion Probabilities (red: with fixed baseline variables, i.e.  $PIP = 1$  by assumption, except for the variance; blue: no fixed variables).

Figure C.12: Posterior Inclusion Probabilities (PIP) from BMA - price level



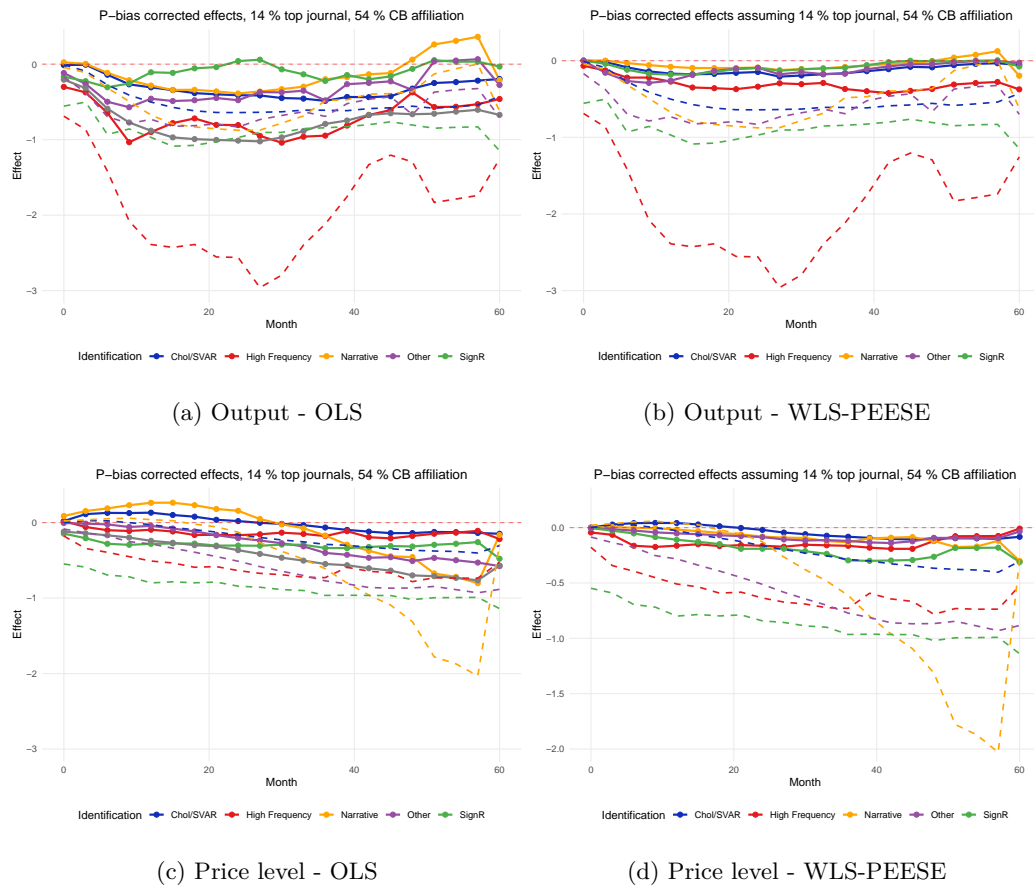
(a) PIP of intercept and publication bias coefficient



(b) PIP of identification methods

Notes: BMA Posterior Inclusion Probabilities (red: with fixed baseline variables, i.e.  $PIP = 1$  by assumption, except for the variance; blue: no fixed variables).

Figure C.13: MMR p-bias corrected effects compared to uncorrected sub-sample averages:  
OLS vs. WLS-PEESE estimates.



*Notes:* The figure compares p-bias corrected effects (colored lines with dots) to uncorrected sub-sample averages (dashed) by identification method. It also provides a robustness check of the OLS estimation from the baseline MMR based on Eq. (2) by comparing it to a WLS-PEESE estimation. As in Figure 11, the corrected effects assume sample means for the other baseline moderator variables, the top-journal dummy (14 percent) and the central-bank affiliation dummy (54 percent).



## Online Appendix D List of studies included in the meta-analysis dataset (after excluding quality concerns)

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