Staff memo

Effects of monetary policy in small open economies during the inflation targeting period

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Summary

We employ a standardised structural VAR framework to estimate the effects of monetary policy in ten small open economies (SOEs) with inflation targeting and flexible exchange rates: Australia, Canada, Chile, the Czech Republic, Iceland, New Zealand, Norway, Poland, Sweden and the United Kingdom. While there is a large literature on monetary policy transmission, there remains limited consensus on whether these effects differ across types of economies. Existing evidence on monetary policy transmission in SOEs is often country-specific, while cross-country comparisons based on a unified empirical framework are less common. Our comparative analysis contributes by providing directly comparable estimates of the policy effects on inflation, GDP, unemployment, and the exchange rate.

In the cross-country aggregate, the estimated effects of a policy-rate increase for the group of SOEs align with macroeconomic theory and are consistent with the broader VAR evidence summarised by Enzinger et al. (2025). The estimated magnitudes are also similar to those typically reported for larger economies such as the United States and the euro area in the same study. While there is cross-country variation in the estimated effects, the data do not provide strong statistical support for many of the differences relative to the typical cross-country response.

If transmission relationships were similar across countries, one might expect countries with a large GDP response to a monetary policy shock to also exhibit large unemployment and inflation responses – that is, for variable responses to co-move across countries. We find little evidence of such co-movement. Instead, cross-country differences are more closely associated with monetary policy transmission elasticities, such as the Okun coefficient, the Phillips slope and exchange rate pass-through. For example, larger inflation responses tend to be associated more with a steeper Phillips slope than with a larger GDP response. Furthermore, the policy-conditional Okun and Phillips elasticities appear to be rather well approximated by simpler regression-based estimates.

When examining whether the effects have changed over time, two broad conclusions emerge. First, across the ten SOEs, the estimated effects of monetary policy on inflation and GDP appear broadly stable as the estimation sample is extended and, in some cases, increase modestly. Second, even where the point estimates suggest an increase, the associated

probability intervals generally imply that the changes are not statistically significant in most cases.

Lastly, focusing on inflation, we find that estimated responses are particularly sensitive to both the inflation measure and the identification strategy. In roughly half of the countries, identification based on short-run restrictions produces a price puzzle – that is, inflation rises in response to a contractionary policy shock. This issue is mitigated when using core inflation measures that exclude volatile CPI components, or when employing sign restrictions. Decomposing the inflation response further indicates that countries with stronger inflation responses tend to exhibit a larger exchange-rate-related contribution, reflecting both the response of the exchange rate to the policy shock and the pass-through from exchange rates to prices.

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1 Introduction

This paper examines the effects of monetary policy in ten small open economies (SOEs) with inflation targeting regimes and flexible exchange rates: Australia, Canada, Chile, the Czech Republic, Iceland, New Zealand, Norway, Poland, Sweden, and the United Kingdom. Existing evidence on monetary policy transmission in SOEs is often country-specific, and cross-country comparisons based on a unified empirical framework are less common. By estimating comparable structural VAR models across a broad set of SOEs, we provide systematic evidence on how monetary policy affects inflation, GDP, unemployment and the exchange rate during the inflation targeting period.

Since its adoption by the Reserve Bank of New Zealand in 1990, inflation targeting has become the dominant monetary policy framework used by central banks worldwide. It is characterised by (i) an announced numerical inflation target[2], (ii) an implementation of monetary policy that gives a major role to an inflation forecast, and (iii) a high degree of transparency and accountability.[3] The transition to inflation targeting involved abandoning exchange-rate pegs in favour of explicit inflation objectives and a floating exchange rate, which has helped these economies absorb external shocks. Overall, the experience has been favourable: inflation has been lower and more firmly anchored, monetary policy has become more transparent and accountable, and the framework has demonstrated flexibility in responding to large external shocks without eroding credibility.[4]

Despite its widespread success and a large body of research on monetary policy transmission, there remains limited consensus on whether monetary policy effects differ across types of economies. In particular, relatively few studies analyse multiple non-euro small open economies in a common standardised framework. Small open economies differ from large economies primarily in their degree of openness and exposure to external conditions. They rely more heavily on international trade and capital flows, making them more sensitive to global shocks. As a result, exchange rates play a larger role in the transmission of monetary policy, influencing inflation and output more directly than in larger, more closed economies.[5]

The central contribution of this staff memo is the analysis of monetary policy transmission elasticities. We examine the joint responses of key macroeconomic variables to a monetary policy shock through an Okun coefficient, a Phillips slope and sacrifice ratio, and exchange rate pass-through. This allows us to assess whether cross-country differences in inflation and unemployment responses mainly reflect differences in GDP and exchange-rate responses, or differences in the elasticities linking these

[2] Most central banks conduct what is known as ‘flexible’ inflation targeting, which means that in addition to stabilising inflation, some weight is assigned to stabilising output and employment, see Rogoff (1985).

[3] Svensson (2007).

[4] Inflation targeting has been successful both in anchoring inflation and managing the large shocks associated with the Global Financial Crisis and Covid-19 pandemic, see Kiley and Mishkin (2025) and Svensson (2007). For a discussion on how monetary policy has become more transparent with inflation targeting, see Jonsson and Vredin (2025).

[5] In contrast, large economies are more driven by domestic demand, less exposed to external developments, and face weaker exchange rate effects.

variables. In addition, the memo contributes by applying a standardised SVAR framework to a broad set of inflation targeting SOEs, yielding directly comparable estimates of the effects of monetary policy on GDP, inflation, unemployment and the exchange rate. Finally, we examine the robustness of the inflation effects across inflation measures and identification strategies, given the well-known sensitivity of inflation responses in SVAR models.

Table 1 documents the adoption of inflation targeting in these countries over the past three decades. The table also shows that these ten economies are deeply integrated into global trade and financial markets, with trade-to-GDP ratios typically ranging between 50 and 120 percent, and their business cycles are closely linked to developments abroad.[6] With roughly three decades of inflation targeting experience, this is an appropriate moment to take stock. We implement a standardised structural VAR framework across countries and comparable samples, identifying monetary policy shocks using both short-run zero restrictions and sign restrictions. This approach yields directly comparable quantitative estimates of the effects of monetary policy on inflation, GDP, unemployment, and the exchange rate.

Table 1. Ten small open economies with inflation targeting and flexible exchange rates

Country	Inflation target	Flexible exchange rate	Trade-to-GDP*, Per cent	GDP correlation**
Australia	1993	1983	42	0.74
Canada	1991	1970	68	0.89
Czech Republic	1997	1997	122	0.76
Chile	1999	1999	64	0.70
Iceland	2001	2001	81	0.64
Norway	2001	1992	71	0.75
New Zealand	1990	1985	57	0.60
Poland	1998	1999	81	0.72
Sweden	1993[7]	1992	85	0.83
United Kingdom	1992	1992	57	0.93

Note. *Historical average, 1995-2024. World Bank. An average for the world equals 54%. **Correlation between year-on-year domestic and foreign GDP growth, using the trade-weighted average of the US and Euro Area. 1996Q1-2024Q4. The first two columns show the year inflation targeting and a flexible exchange rate, respectively, were introduced.

The remainder of this staff memo is organised as follows. In Section 2, we present our main results. At the cross-country level, the estimated effects of monetary policy for the group of SOEs align with standard macroeconomic theory, with impulse responses displaying the expected signs. The magnitudes and dynamics of these responses are also in line with the existing literature and do not provide clear evidence that the effects in small open economies differ systematically from those in larger economies.

[6] For example, the correlation between GDP growth in these countries and weighted measures of GDP growth for the euro area and the US range between 0.6 and 0.9 in the period 1996–2024.

[7] In Sweden, the inflation target was introduced in 1993 and came into effect in 1995.

Although the estimated responses vary across countries, only a few country–variable responses are statistically different from the cross-country median.

In Section 3, we examine the joint responses of variables to a monetary policy shock. Across the ten SOEs, we find that cross-country heterogeneity in the effects of monetary policy is better characterised by differences in monetary policy transmission elasticities — the Okun coefficient, the Phillips slope and exchange rate pass-through — than by co-movement among the raw variable responses. If transmission relationships were similar across countries, one might expect countries with a large GDP response to also exhibit large unemployment and inflation responses. We find limited evidence of this. Instead, cross-country differences become clearer when responses are expressed relative to one another, through transmission elasticities. For example, larger inflation responses tend to be associated more with a steeper monetary policy–conditional Phillips slope than with a larger GDP response. Furthermore, the Okun and Phillips elasticities appear to be well approximated by simpler regression-based estimates.

In Section 4, we analyse whether the effects have changed over the inflation targeting period. Two broad conclusions emerge from the expanding-sample analysis. First, across the ten SOEs, the estimated effects of monetary policy on inflation and GDP are broadly stable over time and, in some cases, increase modestly. Second, even where the point estimates suggest an increase, the associated probability intervals generally indicate that these changes are not statistically significant for most country–variable responses.

Finally, Section 5 examines inflation responses. We show that the estimated inflation response is sensitive to both the inflation measure and the identification strategy: under short-run zero restrictions, a price puzzle arises in roughly half of the countries. This puzzle largely disappears when using core inflation measures that exclude volatile CPI components or when imposing sign restrictions. A decomposition further indicates that countries with stronger inflation responses tend to exhibit a more pronounced exchange-rate channel.

2 Effects of monetary policy in a SVAR model

In this section, we present the model, data, and identification approach used to estimate the effects of monetary policy in the ten small open economies (SOEs). We then report the main results, both at the aggregate level and for individual countries. We also compare our findings with the broader SVAR literature and with the available country-specific evidence.

At the aggregate level, the estimated effects of a policy-rate increase for the group of SOEs align with macroeconomic theory and are consistent with those reported in the existing literature. Moreover, they do not provide clear evidence that monetary policy has different effects in SOEs than in larger economies. While there is cross-country variation in the estimated effects, the data do not provide strong statistical support for many of the differences relative to the typical (median) response.

2.1 Model, data and identification

Model

We study the effects of monetary policy in a Bayesian vector autoregressive (VAR) model with a small set of key macroeconomic variables. We estimate the model for ten SOEs, keeping the elements that influence estimation, most importantly the sample period, model specification (i.e. the variables), prior distribution for the parameters and identification method, as uniform as possible across countries.

The model uses quarterly data on eight variables, three foreign and five domestic.[8] The foreign variables are trade-weighted measures of log real gross domestic product (GDP), the quarterly change in the consumer price index (CPI), and the policy rate for the United States and the euro area. The domestic variables include log real GDP, the unemployment rate, quarter-to-quarter change in the CPI, a measure of the policy rate, and the log real exchange rate. Thus, all variables enter the model in levels except for the foreign and domestic price indices which are included as inflation rates (i.e. first differences). The specification is intentionally simple: we exclude deterministic trends (e.g., linear trends) and dummy variables. We estimate the model over the period 1995Q1–2024Q4 with K = 4 lags and a Minnesota prior on the coefficients.

[8] The model specification follows Berggren et al. (2024) closely.

Data

The data underlying the model are constructed as consistently as possible across the ten small open economies. Harmonised variable definitions enable meaningful cross-country comparisons of the effects of monetary policy on key macroeconomic variables. The real exchange rate is constructed in an identical manner for all countries, and the national statistical series for GDP, CPI, and unemployment follow broadly comparable definitions. Consumer price indices are used throughout, with the exception of Sweden, where the CPI with fixed interest rate (CPIF) is employed. In our notation, a decline in the real exchange rate corresponds to an appreciation of the domestic currency. The trade-weighted foreign variables are also compiled using a common procedure across countries. Further details on the data sources and the transformations applied are provided in Table B1 in Appendix B.

Identification

The monetary policy shock is identified using two standard approaches: short-run zero restrictions and sign restrictions. Under the short-run zero restriction approach, we assume that the shock can affect the nominal, and therefore also the real, exchange rate within the same quarter as the change in the policy rate but cannot contemporaneously influence the other variables. These assumptions are commonly used in the literature.[9] Domestic monetary policy is further assumed not to affect the foreign variables, which is a standard small-open-economy assumption.[10] Equivalently, under this identification the central bank's reaction function in the model allows the policy rate to respond to contemporaneous and lagged values of the foreign variables, to contemporaneous and lagged domestic GDP, unemployment, and inflation, and to lagged values of the policy rate and the real exchange rate.

The sign-restriction approach imposes that a contractionary monetary policy shock, which raises the policy rate, leads to an immediate appreciation of the exchange rate and, on average over the subsequent two years, lower GDP and lower inflation. Likewise, the average unemployment response is restricted to be positive. In this case we retain the short-run zero restrictions described above and hence the sign restrictions become additional identifying restrictions.[11]

2.2 Aggregate effects of monetary policy in the ten SOEs

Figure 1 reports the median responses across the ten SOEs to an unanticipated tightening that raises the policy rate by 1 percentage point on impact. This normalization is maintained throughout the paper to facilitate comparability. For each country and variable, we first obtain the pointwise posterior median response and then we take

[9] Christopher A. Sims first introduced vector autoregressive (VAR) models as an empirical approach to macroeconomic analysis and proposed an identification strategy based on minimal theoretical restrictions. See, for instance, Sims (1980) and Sims (1986).

[10] This is achieved by assuming a block exogenous VAR in combination with the short-run zero restrictions.

[11] In the sign restriction case, we retain the contemporaneous zero restrictions for all variables except the real exchange rate. A similar hybrid approach is used by e.g. Bjørnland and Halvorsen (2014) who combine zero impact restrictions for GDP and inflation with a sign restriction for the exchange rate. As they discuss, a problem with relying on pure sign restrictions is that the identification scheme will be non-unique.

the median across countries to obtain the response shown in the figure.[12] Following the shock, the policy rate gradually returns to its baseline over approximately two to three years. Under short-run zero restrictions, the responses broadly align with standard theory: output falls, unemployment rises, and the real exchange rate appreciates, while the median inflation response is negative but relatively small. Under sign restrictions, the qualitative responses are consistent with the imposed restrictions by construction, with output and inflation falling, unemployment rising, and the real exchange rate appreciating. The median estimates also imply long-run neutrality of monetary policy, i.e. there are no permanent effects on GDP, unemployment, or the real exchange rate.

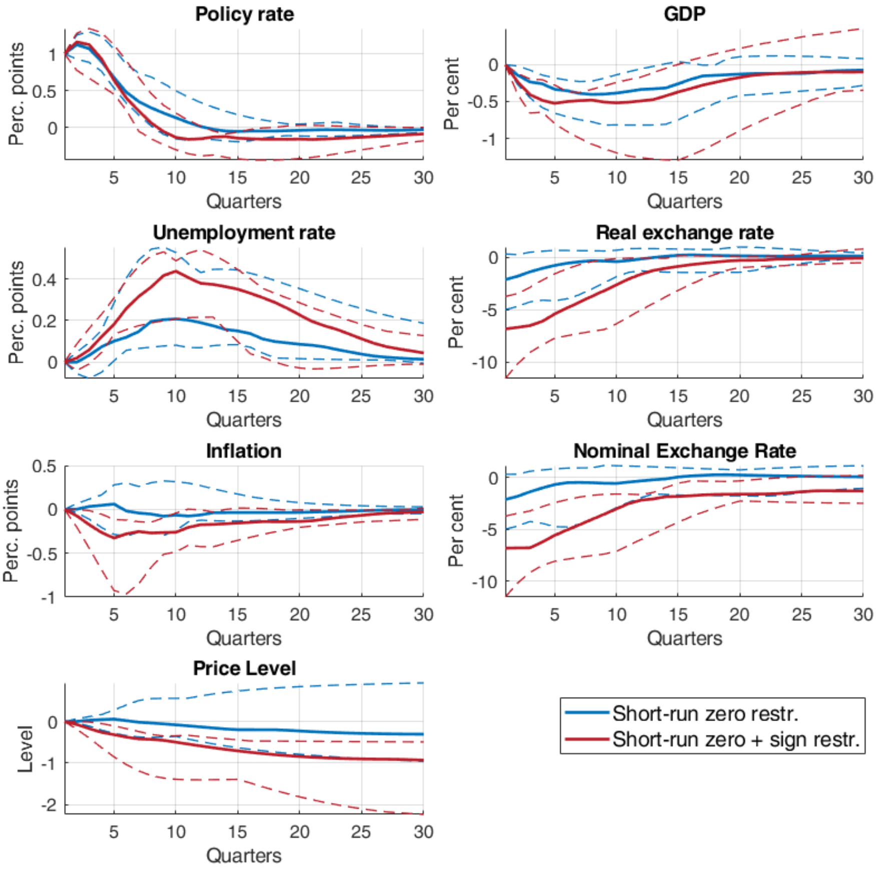
Under sign-restriction identification, real activity declines gradually and with a delay: real GDP falls by around 0.5 percent after one to two years (median response), while the effect on unemployment peaks somewhat later at about 0.4 percentage points. Hence, real-economy effects are hump-shaped and lagged, and rather persistent. For GDP, the results obtained with (zero) restrictions are broadly similar to those under sign restrictions.

Under sign-restriction identification, the nominal, and by implication the real, exchange rate responds swiftly, appreciating by roughly 7 percent on impact. This appreciation exceeds what a simple real interest-parity mechanism would predict based solely on the rise in the real policy rate. Annual inflation falls by about 0.3 percentage points, with the maximum effect materialising after one to two years. For both inflation and the exchange rate, the estimated amplitudes under sign restrictions are substantially larger than with identification based on short-run zero restrictions. In a subset of countries, the short-run restrictions case exhibits a price puzzle, which helps explain why the median effects are larger (i.e. more negative) under sign restrictions.[13] This is discussed further in Section 5.

[12] Impulse responses are computed from the posterior distribution of model parameters obtained via the Gibbs sampler. For each country, each posterior draw of the impulse response function (IRF) is first normalized such that the impact response of the policy rate equals a 1 percentage point increase. The median IRF is then computed pointwise across the normalised posterior draws for each country. Finally, the reported response is the pointwise median across these country-specific median IRFs. The aggregate response should therefore be interpreted as a descriptive summary of the typical cross-country response, rather than as the impulse response of any individual country or of an estimated pooled model. See Appendix C for more details.

[13] A price puzzle refers to inflation rising (falling) in response to a contractionary (expansionary) policy shock.

Figure 1. Median effects of monetary policy in ten small open economies

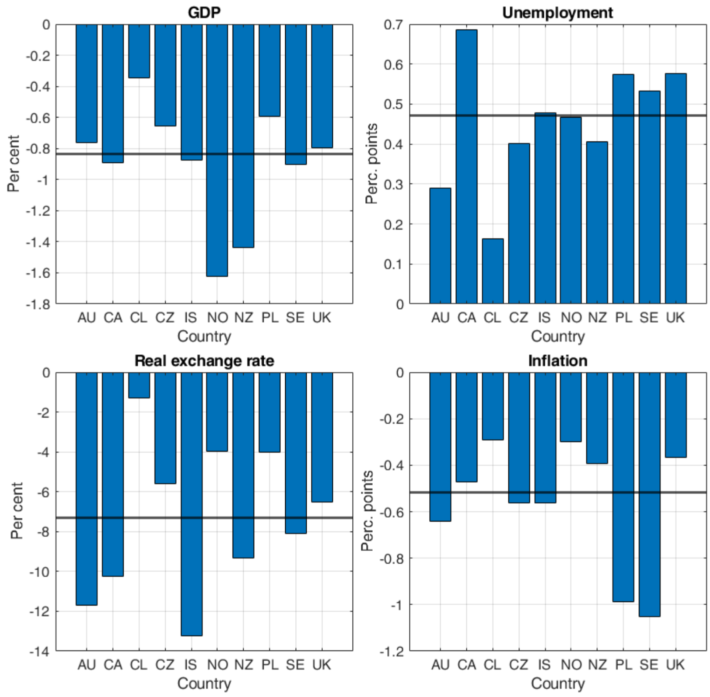


Note. The figure reports the median impulse responses of a 1-percentage-point increase in the policy rate across ten small open economies. The country models are estimated over the period 1995Q1–2024Q4, except for Chile, whose sample begins in 1995Q3. The bands represent second smallest and largest responses across countries and for each quarter. The policy rate, GDP, the unemployment rate and the real exchange rate are measured in levels while inflation is in annual percentage changes.

2.3 Country-specific effects of monetary policy

Figure 2 shows the posterior median peak effects of a normalised monetary policy shock in the individual countries, with the horizontal line indicating the median of the country-specific peak effects. In what follows, we emphasise the estimates from the combined identification that includes sign restrictions. These restrictions help ensure that the identified shock aligns with standard monetary transmission mechanisms, while keeping the underlying short-run structure unchanged. However, we note that the relative effects of the different countries are similar with identification based merely on short-run zero restrictions.

Figure 2. Peak effects of monetary policy in ten small open economies using sign restriction identification



Note. The figure shows the country median peak impulse responses for four different variables identified using sign restrictions. GDP, the unemployment rate and the real exchange rate are in levels, whereas inflation is reported in annual percentage changes. The horizontal line refers to the median of the country-specific median peak impulse responses. See Appendix C for more details.

We note that the aggregate peak effects implied by Figures 1 and 2 differ somewhat. Figure 2 does not account for uncertainty in the timing of the peak response. As a result, the peak effects reported there are larger than those inferred from Figure 1. These differences are discussed in detail in Appendix C.

For most countries, the peak effects on inflation range from −0.2 to −0.6 percentage points, while Poland and Sweden show significantly larger declines, around −1 percentage point. The peak effects on GDP lie in the range of −0.6 to −0.9 per cent, with Norway and New Zealand exhibiting larger declines and Chile showing a relatively small response. The peak effects on unemployment generally fall between 0.3 and 0.6 percentage points, with Chile again showing a comparatively smaller effect. The peak responses of the real exchange rate show a wide dispersion. Chile's response is once more relatively small, whereas Australia, Canada and New Zealand exhibit comparatively larger appreciations.[14]

Across all variables, the effects of monetary policy in Chile are relatively small, below the median for every variable, while the effects are comparatively large in Canada and Sweden. The Czech Republic and the UK display effects that are close to the overall median across variables. New Zealand and Norway, in turn, are characterised by

[14] Our estimated effects on the exchange rate are well in line with those reported by Bjørnland and Halvorsen (2014) based on SVAR models estimated for a subset of the countries studied here in the period 1990-2009. They find relatively large effects for Australia and New Zealand, average effects for Sweden and UK, and relatively small effects for Norway.

relatively large effects on GDP but more moderate responses in unemployment and inflation.

Finally, Table 2 evaluates the statistical significance of the cross-country differences highlighted in Figure 2. Specifically, for each country and variable we report the posterior probability that the country-specific peak effect exceeds the cross-country median peak effect (depicted by the black line in Figure 2). A probability close to one indicates that the country-specific peak effect is likely to be above the cross-country median, while a probability close to zero indicates that it is likely to be below the median. We interpret probabilities above 90 per cent, or below 10 per cent, as indicating a statistically meaningful deviation from the median; these entries are highlighted in the table. While the estimated peak effects vary across countries in ways that may be economically relevant, the probabilities suggest that a substantial share of these differences cannot be distinguished from the cross-country median with high statistical confidence under this criterion. Notably, Chile exhibits statistically significantly smaller effects than the cross-country median across all variables. In addition, the GDP response in Norway is significantly larger than the median, and the inflation response in Sweden is significantly larger than the median.

Table 2. Probability that the peak effect is larger than the cross-country median peak effect

	GDP	Unemployment	Real exchange rate	Inflation
Australia	0.44	0.26	0.73	0.65
Canada	0.55	0.73	0.65	0.45
Chile	**0.05**	**0.01**	**0.01**	**0.10**
Czech Republic	0.35	0.41	0.40	0.53
Iceland	0.52	0.50	0.70	0.53
Norway	**0.94**	0.49	0.33	0.24
New Zealand	0.80	0.42	0.61	0.35
Poland	0.34	0.61	0.32	0.82
Sweden	0.55	0.59	0.56	**0.90**
United Kingdom	0.47	0.66	0.45	0.30

Note. The table shows the probability that the peak effect on the variable for a given country is larger than the cross-country median peak effect which is shown in Figure 2.

2.4 Comparison with other evidence

Table 3 compares our cross-country aggregate (median) results for the ten SOEs with three external benchmarks: the Macroeconomic Model Database (MMD), the meta-study by Enzinger et al. (2025) and a selection of country-specific studies for our group of SOEs. This comparison yields two main conclusions. First, our estimated effects are broadly consistent with those reported in the wider research literature. Second, the estimated effects of monetary policy in SOEs are of a similar order of magnitude to those reported for larger economies such as the United States or the euro area.

Table 3. Effects of monetary policy in a large number of models and studies

Models and studies	Peak effects on GDP	Peak effects on inflation
Small open economies, 10 countries (This paper)		
Short-run zero restrictions (Figure D3)	-0.5	-0.2
Sign restrictions (Figure 2)	-0.8	-0.5
Macroeconomic Model Database		
United States, 48 models	-0.5	-0.2
Euro area, 16 models	-0.5	-0.3
Meta analysis by Enzinger et al. (2025)		
All (bias-corrected), 409 studies	-0.3	-0.2
Short-run zero restrictions	-0.5	-0.2
Sign restrictions	-0.9	-0.5
High-frequency	-1.8	-0.4
Narrative	-0.5	-0.5
United States	-0.6	-0.3
Euro area	-0.6	-0.2
Other advanced economies	-0.4	-0.2
Emerging economies	-0.4	-0.2
Selected country-specific studies, Median of peak effects		
Australia	-0.8	-0.4
Canada	-1	-0.5
Chile	-2	-0.8
Czech Republic	-	-0.4
Iceland	-0.9	-0.6
Norway	-1	-0.3
New Zealand	-0.2	-0.8
Poland	-0.7	-0.4
Sweden	-0.8	-0.4
United Kingdom	-1	-0.6
Median of country-medians	**-0.9**	**-0.5**

Note. The effects reported in the table refer to GDP in levels and inflation measured as the annual percentage change. The policy rate effect is normalised to 1 percentage point. For the selected country-specific studies the median for each country is calculated based on studies employing VAR models with short-run zero or sign restriction identification methods. See Table 6 in Section 5.3 for study-specific results underlying the median for each country.

Using the MMD, we extract impulse responses from a broad set of Dynamic Stochastic General Equilibrium (DSGE) models estimated on U.S. and euro-area data. In terms of peak effects, the benchmarks from these models are of similar magnitude to our SOE estimates for both GDP and inflation.

Turning to the meta-study, Enzinger et al. (2025) assemble a large set of published impulse responses and allow comparisons by identification strategy and country grouping. Our SOE sample aligns most closely with their “other advanced” economies category.[15] Both under short-run zero restrictions and sign restrictions, our estimated effects are broadly in line with the corresponding estimates in the meta-study.

The comparison with high-frequency identification is also informative. Our GDP effect is smaller than that obtained in high-frequency studies, while the inflation effect is similar. This mirrors the broader pattern documented by Enzinger et al. (2025), where high-frequency studies imply substantially larger GDP effects than other identification approaches, while inflation effects differ less.[16]

Our results are broadly in line with the country and regional benchmarks summarized in Table 3, and in particular with the “other advanced” economies that most closely match our set of SOEs. To further compare our results with studies focusing specifically on the small open economies included in our sample, we also benchmark our aggregate estimates against a range of country-specific studies.[17] Overall, our results appear broadly consistent with the existing literature. We obtain similar effects on GDP: around –0.8 compared with a median of –0.9 in the selected country studies, as well as comparable inflation responses (–0.5 compared with a median of –0.5). The country-level medians are drawn from VAR models using either short-run zero or sign restrictions; however, we only have a few relevant studies for each country and both model specifications and empirical results differ substantially across studies, and, furthermore, the results are often sensitive to the choice of inflation measure. For these reasons, the comparisons should be interpreted with caution and seen primarily as indicative of the general magnitude of monetary policy effects rather than precise quantitative differences. Table 6 in Section 5.3 summarises the underlying country-specific studies, and Section 5 discusses the robustness of our estimated inflation responses when alternative inflation measures are used.

[15] In Enzinger et al. (2025), the “other advanced countries” category includes, for example, Australia, Canada, Japan and the UK.

[16] For example, in the case of Sweden, Almerud et al. (2024) use a high-frequency approach and obtain relatively large effects of a policy shock on GDP.

[17] The country-specific studies represent a selected sample, and the number of available studies varies across countries. Our focus is primarily on studies employing VAR models with short-run zero or sign restriction identification and sometimes conducted in close association with the respective national central banks. These studies are used to compute the country-specific median estimates in Table 3, since they are closest to our empirical specification. However, we also report evidence from studies using high-frequency, external-instrument, narrative and FAVAR approaches when available, as these provide important benchmarks from the recent empirical literature. These estimates are not included in the medians because their coverage across countries is limited and because the identifying variation differs from that in our baseline SVAR framework. In some cases, results from studies based on DSGE (dynamic stochastic general equilibrium) models are also reported, although not included in the median values reported for each country. Further details on the selected country-specific studies are provided in Table 6 in Section 5.3.

3 Monetary policy transmission elasticities

In this section, we examine whether there are systematic relationships in how key macroeconomic variables respond to a monetary policy tightening across the ten SOEs. For example, do countries with relatively large real-economy responses also tend to exhibit larger inflation responses? To address this question, we compare monetary policy transmission elasticities, defined as summary measures that capture the joint responses of GDP, unemployment, inflation, and the exchange rate to a monetary policy shock. We also assess whether simple regression-based estimates of these elasticities, which are often used as benchmark relationships in informal policy discussions, are consistent with the monetary-policy-conditional estimates implied by the VAR models.

If transmission relationships were similar across countries, one might expect countries with a large GDP response to a monetary policy shock to also exhibit large unemployment and inflation responses. Across the ten SOEs, we find little evidence of such systematic co-movement among the raw variable responses. Instead, cross-country heterogeneity in the effects of monetary policy is better characterised by differences in monetary policy transmission elasticities, such as the Okun coefficient, the Phillips slope and exchange rate pass-through. For example, larger inflation responses tend to be associated more with a steeper Phillips slope than with a larger GDP response. Furthermore, the policy-conditional Okun and Phillips elasticities appear to be rather well approximated by simpler regression-based estimates.

To characterise cross-country differences in the relative magnitude of the responses to a monetary policy shock, we compute transmission elasticities defined over pairs of variables. We focus on three sets of elasticities: an Okun-type elasticity linking GDP and unemployment; a Phillips-type elasticity and the associated sacrifice ratio linking GDP and inflation; and exchange rate pass-through (ERPT) linking the exchange rate and inflation.[18]

We also compare these monetary-policy-conditional (MP-conditional) elasticities with conventional regression-based (unconditional) estimates of the same elasticities. Simple OLS regressions are frequently used in practical policy work to form benchmark

[18] We use the term transmission elasticities to distinguish these objects from the monetary policy multipliers studied by, for example, Barnichon and Mesters (2021) and Alessandri, Jordà and Venditti (2025). Those papers use multiplier concepts to characterise the effectiveness or trade-offs of monetary policy through cumulative response ratios. Our measures are instead MP-conditional versions of standard structural relationships — Okun's law, the Phillips curve and exchange rate pass-through — computed from the impulse responses to an identified monetary policy shock.

views on these relationships. It is therefore useful to assess whether such approximations are potentially misleading in this context, or whether they provide a reasonable guide despite not conditioning on a monetary policy shock. The regression specifications are described in detail in Appendix E.

Under Okun's law, assuming a common Okun coefficient, a monetary policy tightening that induces a larger decline in GDP in one country than in another should also be accompanied by a larger rise in unemployment, implying broadly similar transmission from output to labour market conditions across countries. Likewise, consistent with the Phillips-curve relationship and standard exchange rate pass-through theory, larger GDP contractions and stronger exchange rate appreciations should be associated with larger declines in inflation. If the responses of GDP, unemployment, the exchange rate, and inflation line up in this manner across countries, this would suggest that the relationships between these variables are broadly similar, even though the responses of GDP and the exchange rate to the monetary policy shock itself may differ across countries. Conversely, sizeable differences in inflation responses despite comparable GDP or exchange rate movements would point to cross-country heterogeneity in the Phillips slope and/or exchange rate pass-through.

There are several ways to construct MP-conditional elasticities. A natural approach is to base them on peak responses, which highlight the largest effects of a monetary policy shock and, importantly, deliver a single scalar elasticity per country that is straightforward to compare across countries. Peak-based measures also avoid placing weight on long horizons where estimated impulse responses may be noisy and imprecise. The drawback is that peaks may occur at different horizons across variables, creating a timing mismatch, and ratios of peaks need not coincide with ratios computed at a common horizon. An alternative is to use average or cumulative responses over a given horizon window, which aligns timing more explicitly and smooths out small irregularities in the impulse responses, but at the cost of introducing a horizon choice and potentially generating multiple elasticity measures per country.

In this paper, we compute the MP-conditional Okun coefficient and Phillips slope using peak responses of unemployment, GDP, and inflation, as this captures the largest effects of a monetary policy shock while facilitating cross-country comparisons across our ten SOEs. As a robustness check, we also consider cumulative (or window-average) versions of these elasticities and find that the qualitative patterns and cross-country comparisons are generally similar. That is, our main conclusions regarding the Okun and Phillips relationships would not be materially affected if cumulative or average responses were used instead of peaks.

Figure 3 reports the estimated MP-conditional elasticities and Figure 4 relates the MP-conditional elasticities to corresponding regression-based (unconditional) estimates of the elasticities. Figure 5 presents the relationships between variable responses and MP-conditional elasticities.

Throughout this section, the MP-conditional elasticities are computed from the baseline impulse responses identified using the combined short-run zero and sign restrictions. They should therefore be interpreted as conditional on this identification scheme. The sign restrictions impose the qualitative direction of the responses of GDP, inflation, unemployment and the exchange rate, but they do not mechanically determine the relative magnitudes of these responses or the cross-country ranking of the elasticities. Still, the estimated heterogeneity in the elasticities should be viewed as reflecting the data together with the identifying assumptions.

3.1 Okun coefficient

The Okun coefficient measures the elasticity that relates changes in the unemployment rate to changes in real activity. We estimate the MP-conditional Okun coefficient as the ratio of the peak responses of the unemployment rate and real GDP to a monetary policy shock. We compare this with an unconditional estimate derived from a simple dynamic specification of the Okun relationship.[19] The estimates are displayed in Figure 3. Across countries, the median Okun coefficient conditional on a monetary policy shock is 0.6, shown by the black solid line in Figure 3, whereas the median unconditional estimate is somewhat lower at 0.4.[20] Both figures are broadly consistent with the ≈0.5 typically reported for unconditional Okun's law estimates in advanced economies.[21]

There is meaningful cross-country heterogeneity in the MP-conditional estimates: Norway and New Zealand display relatively low values at around 0.3, whereas the estimates of Canada, Poland and the UK are relatively large, indicating a comparatively strong unemployment response relative to output. For the remaining countries the estimates are typically rather close to the median across countries.

Figure 4 further displays a positive relationship between the country-level unconditional and MP-conditional coefficients: economies in which unemployment reacts strongly to output changes in general (i.e. for different types of shocks) also exhibit a strong unemployment response to MP-induced output changes. The MP-conditional estimates are larger for slightly more than half of the countries, while for the remainder they are roughly similar to the unconditional estimates. In summary, this result suggests that the MP-conditional estimate of the Okun coefficient may be reasonably well approximated by a simple unconditional estimate.

3.2 Phillips slope and the sacrifice ratio

The Phillips slope links inflation to economic slack, while the sacrifice ratio measures the cumulative real-activity cost of disinflation; the two quantities are naturally inversely related. We calculate the MP-conditional Phillips slope as the ratio of the peak

[19] See Equation (1) in Appendix E.

[20] Our results remain robust when the coefficient is instead calculated as the ratio of cumulative, or, equivalently, average effects on unemployment and GDP over different horizons; see, for example, Ziegenbein (2021) for this approach. The median Okun coefficient across countries computed in this way is 0.4, 0.5 and 0.5 at the horizons 8, 12 and 20 quarters, see Table E1 in Appendix E.

[21] See, e.g., Ball, Leigh, and Loungani (2013). Our median estimate is larger than the MP-specific estimates for the United States reported by Daly et al. (2014) and Ziegenbein (2021), and larger than the demand-specific estimate in Foroni and Furlanetto (2022) for the United States and the euro area; those papers place the shock-specific Okun elasticity around 0.3.

responses of inflation and real GDP to a positive policy-rate shock. The cross-country median slope is 0.7 (Figure 3).[22] For comparison, the median MP-conditional Phillips slope in the macroeconomic model database is 0.4 for models estimated using U.S. data and 0.6 for those estimated using euro-area data.[23]

There is significant country heterogeneity: the slope is larger for Poland and Sweden, while it is comparatively small for Norway, New Zealand and the UK. For the remaining countries the estimated slope is reasonably close to the cross-country median estimate.

We calculate the sacrifice ratio as the ratio of cumulative GDP losses to the price-level effect over three years.[24] The pattern of the sacrifice ratio is broadly consistent with that of the Phillips slope discussed above: it is lower in Poland and Sweden, where the Phillips slope is larger and policy-induced disinflation is relatively strong, and higher in Norway, New Zealand and the UK, where the Phillips slope is smaller. The median sacrifice ratio is approximately 1.7, which broadly aligns with unconditional episode-based estimates for advanced economies.[25] Our estimates are closely aligned with the median MP-conditional sacrifice ratios in the macroeconomic model database: 2.1 for models estimated using U.S. data and 1.6 for those estimated using euro-area data. This implies that there is no clear evidence that the MP-conditional Phillips curve slope or sacrifice ratio differs between small open economies and large economies.

When comparing our MP-conditional and unconditional estimates of the Phillips slope, we find a positive relationship between the two, see Figure 4.[26] For most economies, unconditional estimates are either as large as or larger than the MP-conditional ones. Only Iceland and the UK exhibit a stronger MP-conditional Phillips slope compared to their unconditional counterpart. Similar to the case of the Okun coefficient, this comparison exercise suggests that the MP-conditional Phillips slope may be rather well approximated by a simple regression-based estimate of the slope.

3.3 Exchange rate pass-through

The exchange rate pass-through (ERPT) is calculated as the ratio of the effect on the price level after three years to the peak effect on the nominal exchange rate.[27] The

[22] If the Phillips slope is instead computed as the ratio of the average effects on inflation and GDP, the median across countries equals 0.6, 0.6, and 0.5 at horizons of 8, 12, and 20 quarters, respectively, see Table E1 in Appendix E.

[23] We compute the Phillips slope and the sacrifice ratio for the models in the database ourselves which guarantees that an identical definition is applied.

[24] The median sacrifice ratio, as well as cross-country differences in magnitude, remains robust when computed using two- or five-year horizons. When defined this way, the ratio corresponds to the cumulative GDP loss required to reduce the price level by one percent, expressed in percentage points of one year of annualised GDP.

[25] Mazumder (2014) reports a median sacrifice ratio of 2.0 for 78 disinflation episodes in OECD countries in the period 1974 to 2003. For a subset of 16 "intentional disinflations", i.e. disinflations caused by "pure demand shocks", the median sacrifice ratio is 2.8, see Acevedo and Hofstetter (2023). Tetlow (2022) reports sacrifice-ratio estimates from historical episodes ranging from 0 to 18, based on a selection of studies conducted on the U.S.

[26] See Equation (2) in Appendix E for the regression specification for the unconditional Phillips slope.

[27] For most countries the peak effect on the nominal exchange rate occurs instantaneously, i.e. in the quarter when the policy rate is changed.

cross-country median MP-conditional ERPT is approximately 0.1, meaning that a monetary policy shock which appreciates the nominal exchange rate by 1 percent leads to a 0.1 percent decrease in the price level after three years. The corresponding effects on the price level at the 2- and 5-year horizons are 0.1 and 0.2 percent, respectively.[28]

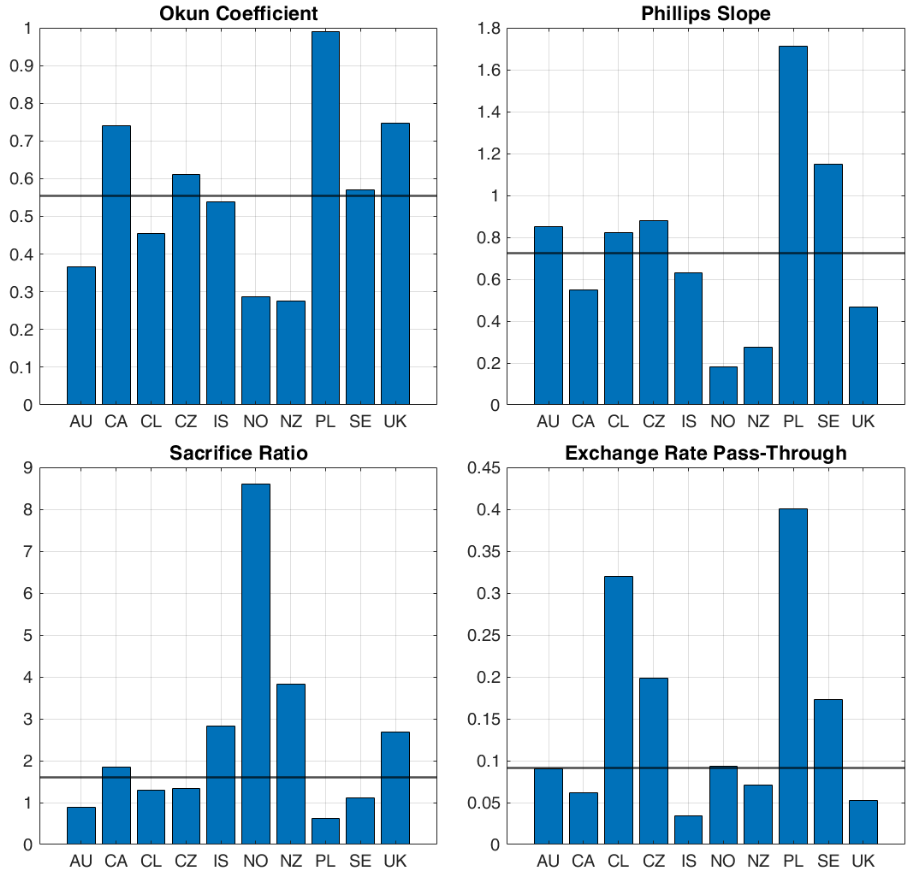
There is considerable heterogeneity in the ERPT, with estimates ranging from 0.05 to 0.4 at the three-year horizon across countries. The ERPT appears relatively high in Chile and Poland, while it is relatively small in Iceland and the UK.[29] Figure 4 shows that there is no clear relationship between the unconditional and MP-conditional ERPT.[30] Therefore, unlike for the Okun coefficient and the Phillips slope, the unconditional estimates do not provide a useful approximation to the MP-conditional ERPT. This may reflect that exchange-rate movements in the data are driven by several different shocks, and that pass-through to prices can differ depending on the source of the exchange-rate movement.

[28] See Table E1 in Appendix E. Our MP-conditional estimate of long-run ERPT for Sweden is consistent with that reported by Corbo and Di Casola (2022), who report a long-run ERPT in response to a monetary policy rate shock to be 0.2 using their benchmark model.

[29] The relative discrepancy in ERPT between advanced and emerging economies is supported by Jašová et al. (2016), who found that different pass-through measures remain relatively stable at lower levels in advanced economies compared to emerging economies, such as Chile and Poland.

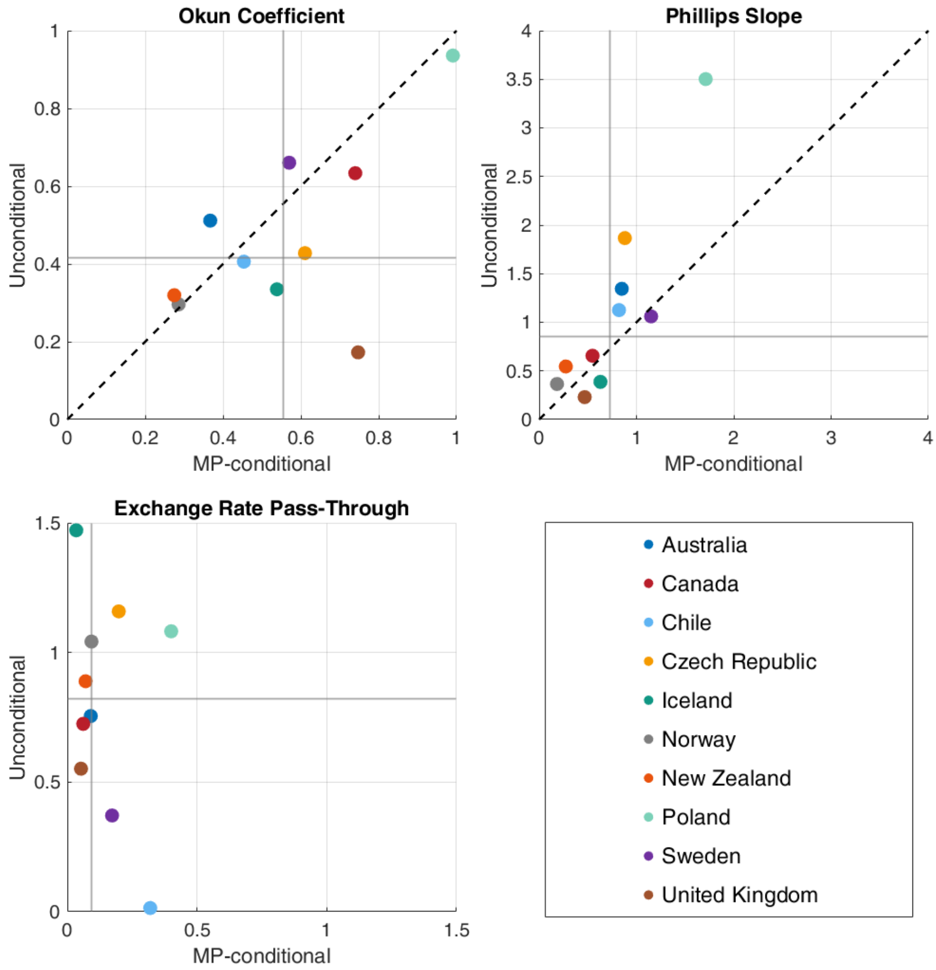
[30] Our unconditional estimates of long-run ERPT for Australia, Canada and the UK are consistent with those reported by Anderl and Caporale using the same linear specification (2023), although there are some differences for New Zealand and Sweden. Anderl and Caporale (2023) found that the unconditional long-run ERPT is 0.49 for the U.S. and 0.39 for the euro area.

Figure 3. Monetary policy transmission elasticities for ten SOEs



Note. Okun coefficient is calculated as the ratio of the peak effect on the unemployment rate to the peak effect on GDP. Phillips slope is calculated as the ratio of peak inflation response to the peak effect on GDP. Sacrifice ratio is calculated as the cumulative GDP loss divided by the cumulative inflation response, i.e. price level, over a 3-year horizon. Exchange rate pass-through is calculated as the cumulative inflation response divided by the peak nominal exchange rate over a 3-year horizon. The horizontal line refers to the median of the country-specific elasticities.

Figure 4. Monetary policy transmission elasticities and regression estimates for ten SOEs



Note: This figure reports the MP-conditional and unconditional Okun coefficient, Phillips slope and exchange rate pass-through. The unconditional elasticities are estimated with regressions and data outlined in Appendix E. The country-specific MP-conditional elasticities shown are identical to those reported in Figure 3. The black dashed lines represent the 45-degree line.

3.4 Statistical significance

Table 4 evaluates the statistical significance of the cross-country differences highlighted in Figure 3. Specifically, for each country and monetary policy elasticity, we report the posterior probability that the country-specific ratio exceeds the cross-country median ratio (depicted by the black line in Figure 3). A probability close to one indicates that the country-specific elasticity is likely to be above the cross-country median, while a probability close to zero indicates that it is likely to be below the median. We interpret probabilities above 90 per cent, or below 10 per cent, as indicating a statistically meaningful deviation from the median; these entries are highlighted in the table. While the estimated ratios vary across countries in ways that may be economically relevant, the probabilities suggest that a substantial share of these differences cannot be distinguished from the median with high statistical confidence under this criterion. Notably, Poland displays a significantly larger Phillips elasticity and exchange rate pass-through than the cross-country median, while its Okun coefficient is close to but below the significance threshold. In addition, the Phillips elasticity in Norway and New Zealand is significantly smaller than the median, and exchange rate pass-through in the Czech Republic is significantly larger than the median.

Table 4. Probability that the monetary policy transmission elasticity exceeds the cross-country median

	Okun	Phillips	Sacrifice Ratio	Exchange Rate Pass-Through
Australia	0.29	0.59	0.31	0.49
Canada	0.67	0.34	0.55	0.28
Chile	0.36	0.56	0.42	0.89
Czech Republic	0.60	0.61	0.43	**0.92**
Iceland	0.48	0.42	0.66	0.17
Norway	**0.03**	**0.01**	**0.99**	0.50
New Zealand	0.12	**0.09**	0.83	0.34
Poland	0.89	**0.92**	**0.10**	**0.98**
Sweden	0.51	0.77	0.33	0.86
United Kingdom	0.65	0.32	0.63	0.33

3.5 What helps explain the heterogeneity in country-variable responses?

In Section 2.3 we compared the effects of monetary policy across the ten SOEs. Although the estimated impulse responses display some cross-country heterogeneity, relatively few country–variable responses are statistically distinguishable from the cross-country median. In this section, we instead focus on monetary policy elasticities, that is, ratios of responses that summarize key trade-offs in the transmission mechanism. These elasticities also exhibit heterogeneity, but once again only a minority are estimated with sufficient precision to be statistically significant.

Figure 5 sheds light on the sources of cross-country heterogeneity by relating each country's estimated variable responses to the corresponding MP-conditional elasticities. Specifically, for each pair of variables (e.g., GDP and inflation), the figure reports two scatter plots: one plotting the response of each variable against the response of the other, and another plotting the response of a given variable against the associated elasticity (e.g., the Phillips slope), computed as the relevant ratio of responses. This comparison helps separate heterogeneity arising from differences in the magnitude of underlying responses from heterogeneity reflecting differences in the transmission trade-offs summarized by the elasticities.

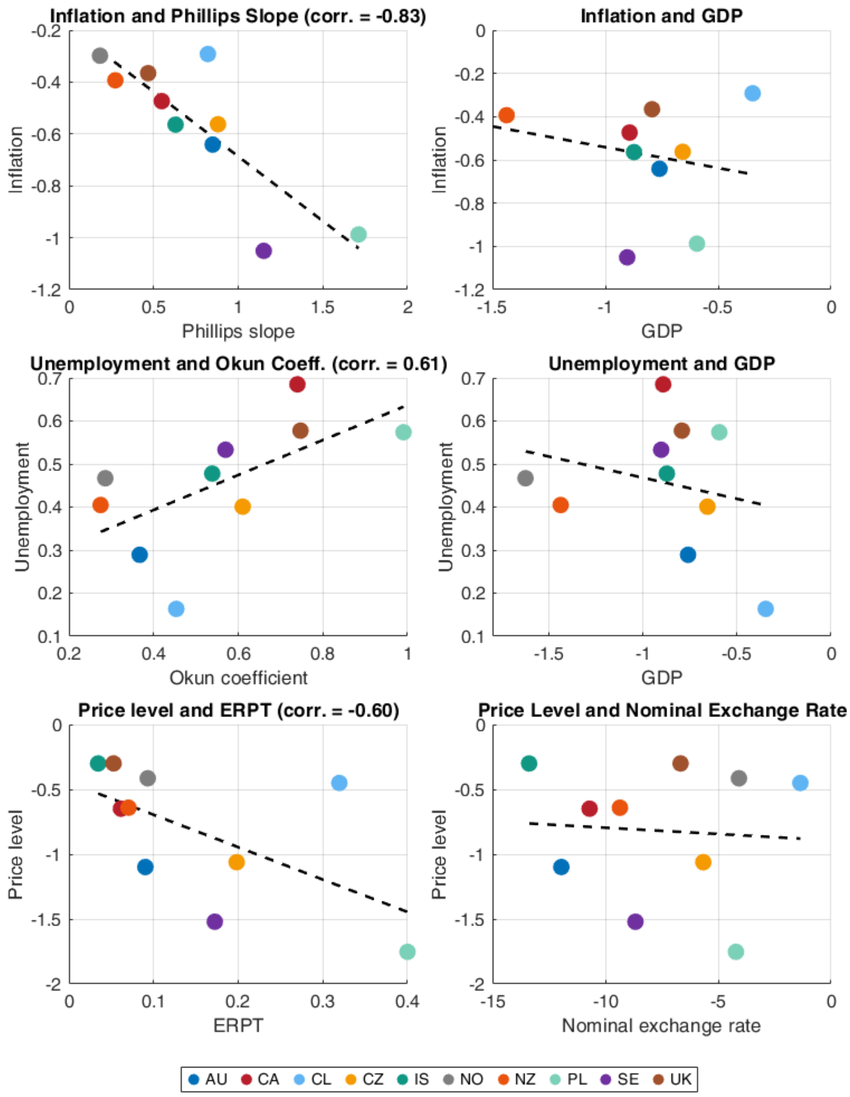
Given that the comparison is based on only ten countries, Figure 5 should be interpreted as descriptive evidence rather than as a formal statistical test of the cross-country relationships. Individual countries can have a visible influence on the fitted relationships, and Chile in particular is an outlier in several dimensions. We therefore view the figure as illustrating broad patterns in the data. The main qualitative message is that inflation and unemployment responses appear more closely related to the corresponding transmission elasticities than to the underlying GDP or exchange-rate responses themselves.

Consider first the relationship between output and inflation. The upper-right panel shows a weak association between countries' GDP and inflation responses, suggesting that cross-country differences in the inflation response cannot be explained simply by differences in the output response. By contrast, the upper-left panel reveals a noticeably tighter relationship between the inflation response and the Phillips slope. This pattern indicates that heterogeneity in the MP-conditional Phillips trade-off accounts for an important share of the cross-country variation in inflation responses.

A similar conclusion emerges for unemployment. The scatter plot relating GDP and unemployment responses displays little systematic relationship across countries. Instead, cross-country differences in unemployment responses appear more closely aligned with variation in the Okun elasticity, suggesting that heterogeneity in the relationship between output and unemployment plays a central role in shaping labour market outcomes following a monetary policy shock. Finally, the association between exchange rate responses and price-level responses is also weak, whereas the link between inflation (or the price level) and the exchange rate pass-through is comparatively stronger. This again points to heterogeneity in exchange rate pass-through, rather than differences in the size of the exchange-rate response itself, as the more important source of cross-country differences in inflation effects.

Taken together, the patterns in Figure 5 should be interpreted cautiously. The correlations are not uniformly strong, and with only ten countries the fitted relationships are sensitive to individual observations. Nevertheless, the figure suggests that cross-country variation in transmission elasticities helps account for differences in variable responses, whereas relationships between pairs of raw variable responses are generally weaker. Put differently, we find limited evidence that countries with larger real responses necessarily exhibit larger inflation responses; instead, countries with larger MP-conditional Phillips slopes tend to display larger inflation responses.

Figure 5. Peak impulse responses in key variables and monetary policy transmission elasticities for ten SOEs



Note. The left column shows subplots relating peak impulse responses of the variables to MP-conditional elasticities. The right column shows subplots relating the components of the MP-conditional elasticity to one another, i.e., the peak impulse responses from which the elasticity is constructed. The units of the responses for inflation, GDP, unemployment, price level and nominal exchange rate are the same as those in Figure 1. The black dashed line denotes the fitted trend line.

4 Have the effects of monetary policy changed over time?

In this section, we estimate the SVAR model over alternative sample periods to assess whether the effects of monetary policy on inflation and GDP have changed over the inflation targeting era. This also allows us to examine whether extreme episodes, most notably the COVID-19 pandemic, have influenced the peak effects estimated in the full sample.

Two conclusions emerge from the results. The estimated effects of monetary policy on inflation and GDP across the ten SOEs are broadly stable across expanding samples and, in some cases, have increased modestly. Moreover, where the estimates do suggest an increase, the associated probability intervals generally indicate that these changes are not statistically significant in most cases.

To examine whether and how the effects of monetary policy have changed as the sample is extended, we estimate the model under sign-restriction identification for different sub-periods within the sample spanning 1995Q1 to 2024Q4.[31] The purpose of this approach is to obtain estimates that rely on varying degrees on older versus more recent data within the inflation targeting regime. This also makes it possible to assess to what extent periods characterised by extreme events, such as the pandemic years, influence the estimated effects in the full sample. We estimate the SVAR model using short-run zero and sign restrictions on an expanding sample with a fixed starting quarter and a varying end quarter. The initial sample covers the period from 1995Q1 to 2009Q4, while the final sample extends from 1995Q1 to 2024Q4. By compiling the estimates from all expanding samples, we obtain peak effect estimates that are based on different sample periods, allowing us to assess how the estimated effects of monetary policy on GDP and inflation have changed as the sample has been extended. The analysis focuses on the magnitude of the peak effects and does not examine whether the timing of monetary policy transmission has changed over time.

Figure 6 illustrates, for each SOE, how the estimated peak effect of a monetary policy shock on inflation changes over time, where the point on the x-axis denotes the sample endpoint.[32] Figure 7 shows the corresponding peak effect on GDP. In all cases, the responses are normalised to an unexpected one-percentage-point increase in the

[31] The estimation follows the model and identification outlined in Section 2. The expanding-sample approach should be interpreted as a sequential updating exercise: it shows how the estimated effects change as additional observations become available, while keeping the early part of the inflation targeting period in the estimation sample. This provides a natural Bayesian interpretation, as the posterior estimates are updated when the information set is expanded. However, it is not designed to identify structural breaks at specific dates. Rolling-window or time-varying-parameter approaches could be more sensitive to such changes but would also rely on shorter effective samples and typically imply less precise estimates.

[32] We were not able to estimate the effects for Iceland using the expanding sample due to computational constraints. As a result, Iceland is excluded from the time-varying analysis based on expanding samples.

policy rate in the initial period.[33] The figures also display probability intervals for the estimated peak effects and their evolution over time; for example, the outer bands represent a 90 per cent probability interval. These intervals illustrate the uncertainty surrounding the estimates and provide an informal basis for assessing whether the estimated effects have changed significantly over time.

We draw two overall conclusions from the results. First, the estimated effects of monetary policy on GDP and inflation in the ten countries are broadly stable over time and, in some cases, have increased somewhat. Second, where the estimates do indicate an increase, the associated probability intervals generally imply that the change is not statistically significant in most cases.

To assess more formally whether the estimated effects have changed over time, we compare results for the samples 1995Q1–2019Q4 and 1995Q1–2024Q4. The additional period, 2020–2024, was unusually turbulent, encompassing the Covid-19 pandemic in 2020–2021 and the subsequent high-inflation episode beginning in 2021. Using a posterior probability threshold above 90 per cent (or below 10 per cent) to define statistical significance, we do not find any statistically significant differences in the estimated effects across the two sample periods (see Table 5). One interpretation is that, within the baseline model, including the years 2020–2024 does not materially alter the estimated effects of monetary policy.

Table 5. Probability that the effect of monetary policy on GDP and inflation has increased when estimated to 2024 compared to 2019

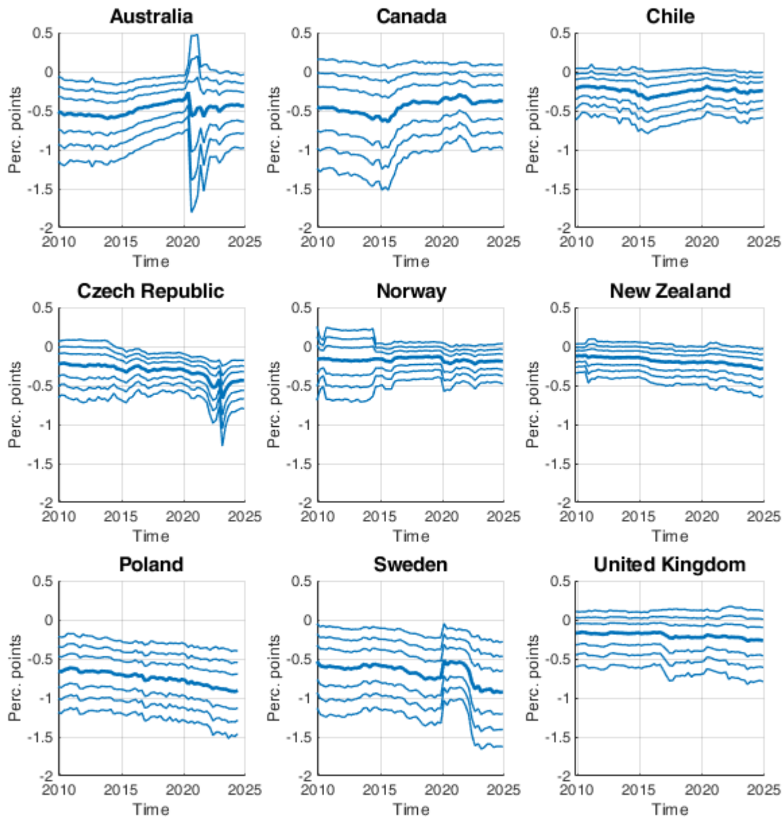
Country	GDP	Inflation
Australia	0.88	0.69
Canada	0.67	0.50
Chile	0.33	0.45
Czech Republic	0.54	0.61
Iceland	0.52	0.47
Norway	0.69	0.59
New Zealand	0.69	0.61
Poland	0.47	0.54
Sweden	0.44	0.58
United Kingdom	0.55	0.58

While the changes in estimated effects are not statistically significant in most cases, they may nevertheless be relevant from a monetary policy perspective. Central banks must base decisions on the best available assessment of the current transmission mechanism. From this viewpoint, the time variation in Figures 6 and 7 may be economically meaningful even when it cannot be distinguished from earlier estimates

[33] This normalisation does not capture potential changes over time in the persistence of the policy rate response, which could affect the estimated effects, see, for example Ramey (2016).

with high statistical confidence. We therefore briefly comment on the patterns in the data.

Figure 6. Peak effects on inflation of an increase in the policy rate estimated using sign restrictions for different samples

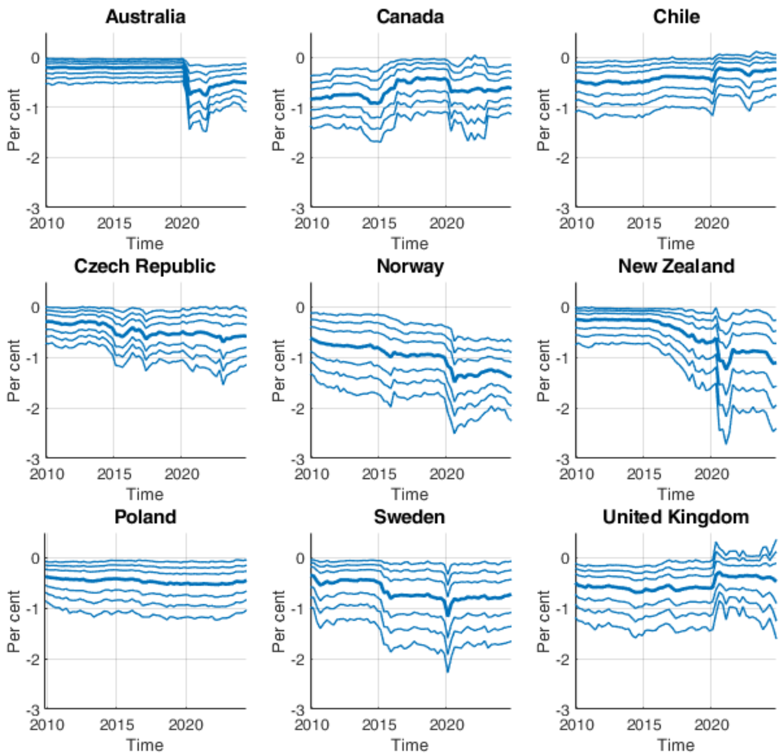


Note. The BVAR model is estimated for different samples, and the figure shows how the maximum effect on inflation has changed over time. The initial effect on the policy rate is normalised to one percentage point. The thin lines represent the 5th, 15th, 25th, 75th, 85th and 95th percentiles of the probability distribution of the responses. The x-axis shows the endpoint of the sample period. Iceland has been excluded due to difficulties in estimation with expanding samples.

Overall, the estimated inflation responses are relatively stable over time. For all countries, the peak inflation response to a monetary policy tightening is negative across the alternative sample endpoints. At the same time, the estimates suggest that the magnitude of the inflation response has increased somewhat over time in a subset of countries, including New Zealand, the Czech Republic, Poland, and Sweden.

The evidence for GDP is less clear-cut. In some countries, such as Australia, Norway, and New Zealand, the peak GDP effect appears to have become larger over time, although the associated uncertainty bands imply that these changes should be interpreted cautiously. In both Figures 6 and 7, the pandemic years 2020–2021 introduce a visible kink in the estimated peak effects for several countries. However, extending the sample to include the post-2019 period does not appear to materially change the overall magnitude of the estimated effects. Nevertheless, given the exceptional nature of the pandemic period, reporting estimates both including and excluding these observations helps assess the robustness of the results to recent macroeconomic disruptions.

Figure 7. Peak effects on GDP of an increase in the policy rate estimated using sign restrictions for different samples



Note. The BVAR model is estimated for different samples, and the figure shows how the maximum effect on GDP has changed over time. The initial effect on the policy rate is normalised to one percentage point. The thin lines represent the 5th, 15th, 25th, 75th, 85th and 95th percentiles of the probability distribution of the responses. The x-axis shows the endpoint of the sample period. Iceland has been excluded due to difficulties in estimation with expanding samples.

5 A closer examination of the inflation effects

In this section, we focus on the estimated effects of monetary policy on inflation, as these responses appear relatively uncertain compared with other macroeconomic variables.[34] Across the ten SOEs, inflation responses under short-run zero identification frequently exhibit a price puzzle. This raises the question of whether the pattern is driven by the choice of inflation measure or instead reflects underlying identification problems. Lastly, we decompose the inflation responses into a policy rate and an exchange rate channel.

We find that the inflation responses are particularly sensitive to both the inflation measure and the identification method, with a price puzzle emerging in roughly half of the countries under short-run zero restrictions. Using core inflation measures that exclude volatile CPI components or sign restrictions resolves this issue. Decomposing inflation responses shows that countries with stronger inflation responses also exhibit a more pronounced exchange-rate channel.

5.1 Difficulties in estimating inflation effects using short-run zero restrictions

Difficulties in identifying and estimating the inflation response in small open economies have long been recognised in the monetary policy VAR literature, and many studies have therefore employed alternative inflation measures to mitigate counter-intuitive results. For example, in the case of Norway, most SVAR analyses rely on CPI excluding energy and tax effects (CPI-ATE) rather than headline CPI to address identification challenges and to reduce the influence of volatile components.[35] Australia provides another example, where the CPI Trimmed Mean has been used as an alternative inflation indicator in several studies.[36] Similarly, for the United Kingdom, the CPI Core

[34] Other variables may also be affected by measurement and sample issues. For GDP, the expanding-sample analysis in Section 4 suggests that including the pandemic and post-pandemic observations does not materially change the overall magnitude of the estimated effects, although the pandemic years introduce visible movements in some country estimates. For unemployment, cross-country comparisons should be interpreted with some caution because the underlying labour-market series are not fully harmonised: as shown in Table B1 in Appendix B, some countries use survey-based measures while others use registered unemployment. This may affect the estimated Okun coefficients and reinforces our interpretation of the cross-country elasticity comparisons as suggestive rather than exact structural rankings.

[35] The CPI-ATE is published by Statistics Norway and refers to CPI adjusted for tax changes and excluding energy products.

[36] The CPI Trimmed Mean is published by Australian Bureau of Statistics and refers to the CPI adjusted to exclude large price movements in either direction. For information about the calculation methodology, see Australian Bureau of Statistics (2025).

measure is commonly used. More broadly, the literature on SOEs reports considerable dispersion in estimated inflation responses.[37] Based on our survey of research articles, this uncertainty appears particularly pronounced in studies for Australia, Chile, New Zealand, and the UK. The variation likely reflects not only the choice of inflation measure but also differences in identification strategies, including the use of recursive orderings versus sign restrictions.[38]

Our own estimates reflect these challenges. The estimated effect of a monetary policy shock on inflation is highly sensitive to both the choice of inflation measure and the identification strategy employed. In this section, we therefore document how our results are affected by alternative identification assumptions and inflation measures. We then relate these findings to the broader literature on price puzzles and discuss potential mechanisms that may generate such puzzles within structural VAR frameworks.

5.2 Inflation effects with various measures and identification strategies

Figure 8 presents the country-specific inflation responses to a monetary policy shock. For some countries, inflation responses are estimated using three alternative inflation measures: headline inflation (CPIF for Sweden and CPI for the remaining small open economies), a core inflation measure excluding volatile components, and, when available, an additional measure commonly used in earlier country-specific studies (such as the Trimmed CPI in Australia). The figure shows results obtained under both identification using short-run zero restrictions and sign restrictions and also includes estimates from specifications in which oil prices are included in the model.[39]

Broadly, our findings are consistent with existing studies that rely on similar inflation measures and identification schemes, see Table 3 in Section 2.4.[40] Several patterns emerge from the country-level evidence. First, price puzzles are common under short-run zero restrictions and when CPI is used as the measure of inflation. In the SVAR literature, researchers distinguish between *short-run price puzzles*, where prices rise only temporarily following a monetary policy rate increase, and *persistent or long-run price puzzles*, where the price level increases more durably over several years.[41] Our results reflect both types: we find short-run puzzles for countries such as Australia, Norway, and New Zealand, and more persistent puzzles for the Czech Republic,

[37] See Table 6 in Section 5.3 below.

[38] Previous studies have also employed other identification methods, such as FAVAR and narrative approaches.

[39] In Section 5.3, we outline the rationale for including commodity prices, such as oil prices, in VAR models.

[40] Table 3 in Section 2.4 compares our results with previous findings from SVAR studies in all ten small economies. For some countries, we also include estimated effects from DSGE studies, however, these are less comparable with our estimates.

[41] For estimated short-run price puzzles, see, for instance, Christiano et al. (2005); for documented persistent price puzzles, see, for instance, Baumeister et al. (2010) and Chen and Valcarcel (2021). Previously published results often exhibit the price puzzle in the short run, possibly because findings showing persistence in the long run are more difficult to publish, given that macroeconomic theory suggests prices should eventually decline following a monetary contraction. For example, the meta-analysis by Rúsnak et al. (2013) finds evidence of publication selection against results documenting the price puzzle.

Iceland, and the UK.[42] Importantly, for Australia, New Zealand, Norway and the UK, using a core inflation measure eliminates the puzzle, illustrating how inflation measurement can materially alter the inferred effects of monetary policy. A similar pattern appears in the Czech Republic, where previous work using CPI excluding energy finds no puzzle.[43] In countries where the literature has converged on a preferred price index precisely to address this issue, such as CPI-ATE in Norway or core CPI in the UK, our results closely match earlier studies. This suggests that tailored inflation measures can improve identification and avoid price puzzles in recursive SVAR models.

Sweden stands out as a notable exception: none of the inflation measures we use generate a price puzzle. Moreover, the relative sizes of the impulse responses (largest for CPI, smaller for CPIF, and smallest for CPIF excluding energy) reinforce the notion that measurement choices shape the estimated inflation response.[44]

Second, sign-restricted identification consistently produces larger negative inflation responses. Since the negative inflation response to a policy tightening is imposed by assumption, these results are not informative about the existence of price puzzles. Nonetheless, the fact that sign-restricted inflation effects exceed those obtained with short-run zero restrictions underscores the extent to which identification assumptions influence the estimated inflation effects.

Finally, we explore whether augmenting the model with commodity prices, often proposed as a remedy for price puzzles, improves identification.[45] Including oil prices, which track broader commodity price movements well, does not change our overall findings.[46] However, it somewhat mitigates the price puzzle in Iceland (see Figure 8). This suggests that the instability of inflation responses likely stems from deeper issues related to measurement and identification rather than omitted global price dynamics. As the analysis using two-channel transmission decomposition shows, the countries for which we obtain price puzzles tend to have a weaker exchange rate channel, potentially resulting in a smaller inflation response (see Section 5.4 below).

[42] Bjørnland and Halvorsen (2014) estimate SVAR models for six of the countries studied here over the period 1990-2009, using a similar identification method based on short-run zero restrictions and a sign restriction only for the exchange rate. They find short-run price puzzles for Australia, Canada, New Zealand, and Norway, a persistent price puzzle for the UK and no puzzle for Sweden. While we use a more recent and extended sample, our results are very similar to theirs.

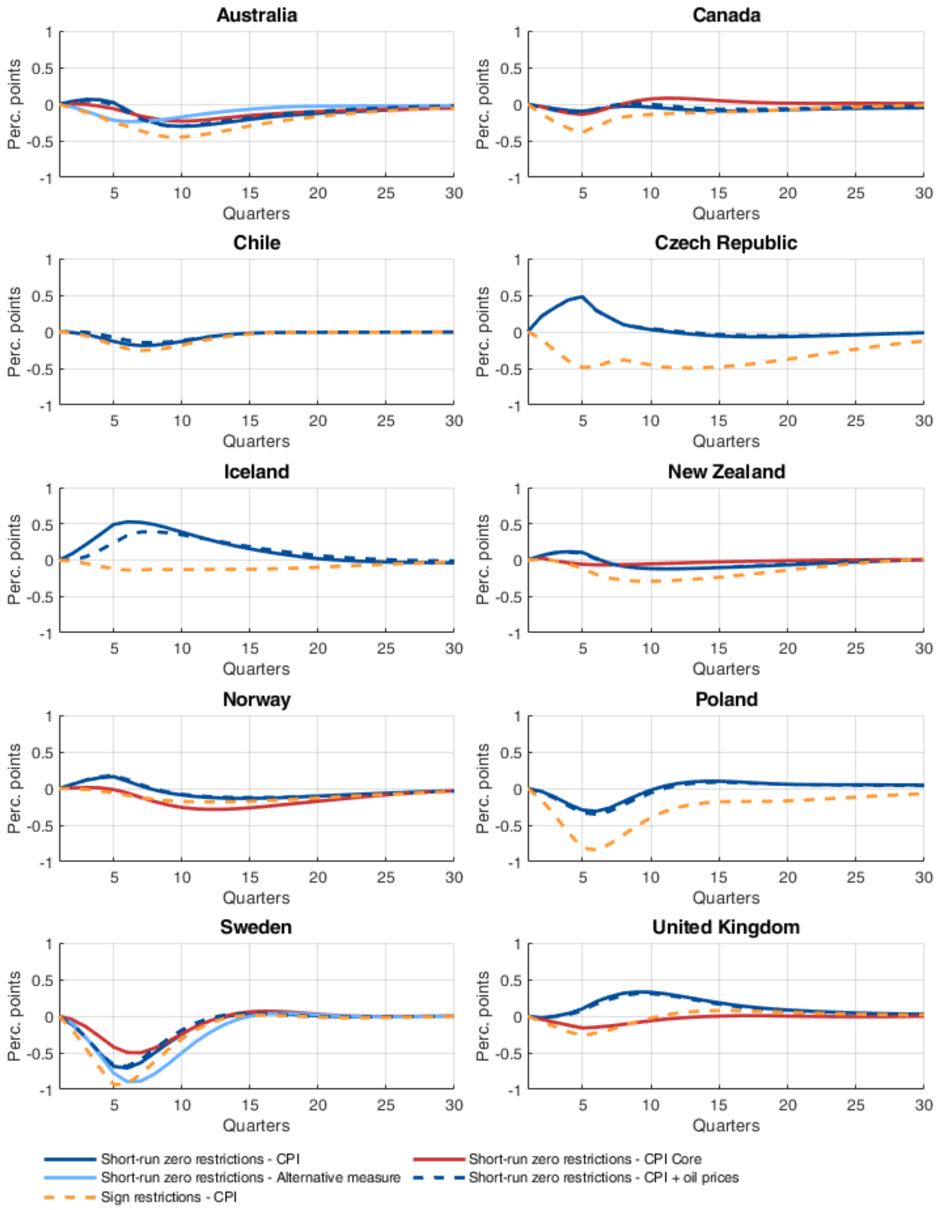
[43] See Borys and Horvath (2008).

[44] One caveat is that, under our short-run zero restrictions, all inflation measures are restricted not to respond contemporaneously to the monetary policy shock. This may understate the prevalence of price puzzles if some CPI components respond very quickly to policy-rate changes. Sweden is a useful example. In our specification, the CPI inflation response is rather similar to the CPIF inflation response, and no price puzzle arises. By contrast, Berggren et al. (2024) find a short-run price puzzle for Swedish CPI when allowing inflation to respond contemporaneously. This difference is consistent with the interpretation that interest-rate-related housing costs can affect CPI inflation within the quarter of the policy-rate change. The price-puzzle problem may therefore be more severe than suggested by our baseline short-run zero-restriction estimates.

[45] See the next section, "Potential explanations from the price puzzle literature" for a discussion of why short-run zero identification can generate price puzzles.

[46] The correlation between global Brent oil price series and the IMF's World Commodity Price Index is 0.88.

Figure 8. Country-specific impulse response functions with headline and core inflation measures and different identification methods



Note. The number of inflation measures differ across countries and are chosen based on the previous literature (see Table B2 in Appendix B for more details). Refers to inflation measured as the annual percentage change. For Sweden, the dark blue solid line represents the CPIF, the red line the CPIF excluding energy and the light blue line the CPI. For Norway, "CPI Core" refers to CPI-ATE. For Australia, the alternative inflation measure refers to the CPI Trimmed Mean.

5.3 Potential explanations from the price puzzle literature

Price puzzles appear frequently in our sample of small open economies when using short-run zero restrictions, making it essential to consider why such puzzles arise. Price puzzles have long been recognised as a central challenge in the SVAR literature, and the prevailing view is that they typically reflect modelling or identification shortcomings rather than genuine features of the monetary transmission mechanism.

A first explanation concerns **inflation measurement**, particularly the reliance on headline CPI. In our sample, using headline CPI consistently generates price puzzles. A general feature of the CPI is that it includes household housing costs.[47] When the policy rate increases, housing-related expenses such as mortgage interest payments or rents also rise, which in turn pushes up CPI inflation.[48] Since these housing costs are included in the CPI for all ten SOEs, this mechanism may help explain the price puzzles we observe across several countries. Evidence based on US data further suggests that this channel can account for a substantial share of the price puzzle, i.e., the counterintuitive response of inflation moving in the "wrong" direction.[49]

Another feature of headline CPI is that it includes volatile components, most notably energy and food, so its behaviour is heavily influenced by global supply shocks. Previous studies have argued that, as a result, the CPI can distort estimated inflation responses in SVAR frameworks (Sims, 1992; Hanson, 2004). When a central bank raises interest rates in response to an oil-price-driven inflation surge, oil prices may continue rising due to global conditions, causing headline CPI to rise as well. In a VAR that does not adequately capture these contemporaneous information channels, the resulting dynamics can be misinterpreted as a price puzzle. Consistent with this interpretation, switching from headline to core inflation measures removes the puzzle in several countries, including Australia, Norway, New Zealand, and the United Kingdom.

A related strand of the literature has focused on **omitted variable bias**, particularly the exclusion of information on commodity prices. Early contributions, such as Sims (1992), showed that price puzzles largely disappear when commodity prices are included in the VAR. The underlying argument is that central banks may respond to signals about future inflation that are not captured by the model, causing a contractionary policy shock to coincide with news of rising future prices. However, in our setting, augmenting the SVAR model with oil prices has virtually no effect on the inflation response, and the puzzle remains. This finding aligns with earlier evidence from Hanson (2004), who finds that including commodity prices does not consistently eliminate the puzzle, and with the meta-analysis of Rusnák et al. (2013), which identifies additional sources of bias, including the failure to control for potential output and financial conditions. Similarly, the seminal contribution by Giordani (2004) argues that the price puzzle often arises from omitting GDP or financial variables, and that addressing these omissions can eliminate the puzzle without relying on commodity prices. In his analysis, adding oil prices has little effect, whereas controlling for GDP resolves the puzzle.

[47] In fact, the Riksbank changed the inflation target variable from the CPI to the CPIF in 2017, based on the argument that changes in the policy rate have direct effects on inflation through mortgage interest rates. For a discussion of the drawbacks of the CPI and the reasons for switching to the CPIF, see Sveriges Riksbank (2016).

[48] Again, the reason we do not obtain a price puzzle with CPI inflation in the case of Sweden is because we impose a contemporaneous zero restriction for this variable. In the absence of this restriction CPI inflation increases on impact in response to a positive policy shock.

[49] Dias and Duarte (2019) estimate the effects of monetary policy on house prices and rents using a proxy-SVAR model and a small set of U.S. macroeconomic variables for the period 1983–2017.

Identification assumptions themselves are also central to the prevalence of price puzzles. A substantial literature highlights the sensitivity of estimated inflation effects to short-run zero restrictions and lag structures. Estrella (2015) surveys a wide set of VAR specifications and shows that puzzles are strongly associated with identification based on short-run zero restrictions. Non-recursive identification schemes or models with richer information sets substantially reduce the incidence of such puzzles. Rusnák et al. (2013) similarly find that identification based on short-run restrictions increases the likelihood of observing a price puzzle. As summarised in Table 6, most previous studies on the small open economies in our sample rely on sign restrictions precisely to avoid these problems. Complementing this approach, factor-augmented VAR (FAVAR) models expand the information set. Bernanke et al. (2005) demonstrate that including a broad array of macroeconomic and financial indicators, thereby approximating the central bank's information set, yields more standard inflation responses, suggesting that limited-information VAR models may suffer from systematic misidentification of monetary policy shocks.

A further explanation relates to **the monetary policy rule** itself. Castelnuovo and Surico (2010) argue that price puzzles are more likely when the central bank responds only weakly to inflation. Using U.S. data from 1960–2006, before the adoption of inflation targeting, they show that contractionary policy shocks tend to generate positive price responses in periods when the policy rule's inflation coefficient is low, reflecting uncertainty about the central bank's commitment to inflation stabilisation. In our sample period, which occurs after the introduction of inflation targeting regimes, such weak responses should be less relevant in principle. Nonetheless, price puzzles persist for several countries, suggesting that the VAR may fail to capture agents' expectations of how the central bank reacts to inflation. Thus, weak perceived inflation responsiveness cannot be ruled out as a contributing factor. Incorporating explicit measures of inflation expectations into our identification scheme may therefore offer a promising direction for future research.

Table 6. Peak effects of monetary policy on inflation across selected studies using different inflation measures

Country	Article	Model and identification Method	Sample period	Inflation measure	Peak effect on inflation, value (quarter)
Australia	Kim and Lim (2018)	SVAR, sign restrictions	1993–2014	CPI	-0.1 (3)
	Gibbs et al. (2018)	DSGE	1991–2016	CPI	-0.3 (3)
	Read (2022)	SVAR, sign restrictions	1984–2019	Trimmed Mean CPI	-1 (14)
	Fisher and Huh (2023)	SVAR, sign restrictions	1992–2016	CPI	-0.4 (1)
	Median VAR models	-	-	-	**-0.4**
	This paper	SVAR, sign restrictions	1995–2024	CPI	**-0.3**

Country	Article	Model and identification Method	Sample period	Inflation measure	Peak effect on inflation, value (quarter)
Canada	Champagne and Sekkel (2017)	VAR, Narrative	1974–2015	CPIX	-0.5 (10)
	Kim and Lim (2018)	SVAR, sign restrictions	1992–2014	CPI	-0.4 (5)
	Alexander and Reza (2022)	SVAR, sign restrictions	1981–2018	CPI	-0.5 (5)
	Ha and So (2023)	SVAR, Cholesky, IV	2000–2017	CPI	-1 (4)
	Median VAR models	-	-	-	**-0.4**
	This paper	SVAR, sign restrictions	1995–2024	CPI	**-0.3**
Chile	Madeira and Salazar (2023)	FAVAR	1996–2012	CPI	**-0.8 (1)**
	Aruoba et al. (2021)	BVAR, Cholesky	2001–2020	CPI	**-0.8 (24)**
	Median VAR models	-	-	-	**-0.8**
	This paper	SVAR, sign restrictions	1995–2024	CPI	**-0.3**
Czech Republic	Borys and Horvath (2008)	SVAR, Cholesky	1998–2006	CPI excl. energy	-0.3 (8)
	Kucharcukova et al. (2013)	SVAR, sign restrictions	1998–2010	CPI	-0.8 (6)
	Czech National Bank (2021)	SVAR	2000–2021	CPI	-0.4 (8)
	Median VAR models	-	-	-	**-0.4**
	This paper	SVAR, sign restrictions	1995–2024	CPI	**-0.5**
Iceland	Pétursson (2023)	SVAR, Cholesky	2009–2022	CPI	-0.6 (10)
	This paper	SVAR, sign restrictions	1995–2024	CPI	**-0.1**
Norway	Bjørnland (2008)	SVAR, Cholesky	1993–2004	CPI-ATE	-0.1 (12)
	Gerdrup et al. (2017)	DSGE	-	CPI-ATE	-0.1 (7)
	Robstad (2014)	BVAR, sign restrictions	1994–2013	CPI-ATE	-0.3 (6)
	Median VAR models	-	-	-	**-0.3**
	This paper	SVAR, sign restrictions	1995–2024	CPI	**-0.4**
New Zealand	Bloor and Matheson (2008)	BVAR, Cholesky	1990–2007	Tradable and non-tradable CPI	-0.1 (8)
	Culling et al. (2019)	DSGE	1993–2018	CPI	-0.8

Country	Article	Model and identification Method	Sample period	Inflation measure	Peak effect on inflation, value (quarter)
	Culling et al. (2019)	SVAR, sign restrictions	1993–2018	CPI	-1.2
	Buckle et al. (2003)	SVAR, Cholesky	1983–2002	CPI	-0.8 (9)
	Median VAR models	-	-	-	**-0.8**
	This paper	SVAR, sign restrictions	1995–2024	CPI	**-0.4**
Poland	Demchuk et al. (2012)	SVAR, Cholesky	1997–2006	CPI	-0.3 (6)
	Chmielewski et al. (2019)	SVAR	1999–2019	HICP	-0.4 (9)
	Greszta et al. (2023)	SVAR, sign restrictions	2003–2022	CPI	-0.5 (8)
	Median VAR models	-	-	-	**-0.4**
	This paper	SVAR, sign restrictions	1995–2024	CPI	**-0.8**
Sweden	Laséen and Strid (2013)	BVAR, Cholesky	1995–2013	CPIF	-0.4 (6)
	Corbo and Strid (2020)	MAJA (DSGE)	1995–2018	CPIF	-0.2 (4)
	Di Casola and Iversen (2019)	BVAR, Cholesky	2006–2015	CPIF	-0.3 (5)
	Lyhagen and Shahnazarian (2023)	BVAR, Cholesky	2001–2022	CPIF	-0.2 (10)
	Berggren et al. (2024)	BVAR, Cholesky	1995–2024	CPIF	-0.5 (6)
	Almerud et al. (2024)	High-frequency	2004–2024	CPIF	-1
	Median VAR models	-	-	-	**-0.4**
	This paper	SVAR, sign restrictions	1995–2024	CPIF	**-0.9**
United Kingdom	Ellis et al. (2014)	FAVAR, Cholesky	1992–2005	CPI	-2 (7)
	Cloyne and Hurtgen (2016)	VAR, Narrative	1975–2007	RPIXm inflation	-1 (11)
	Cesa-Bianchi et al. (2020)	Proxy-SVAR, High-frequency	1997–2015	CPI	-0.4 (4)
	Brignone and Piffer (2025)	DSGE	-	CPI	-0.4 (12)
	Brignone and Piffer (2025)	SVAR, sign restrictions	-	CPI	-0.6 (4)
	Median VAR models	-	-	-	**-0.6**
	This paper	SVAR, sign restrictions	1995–2024	CPI	**-0.3**

Note. The effects reported in the table refer to inflation measures as the annual percentage change. The policy rate effect is normalised to a 1 percentage point increase for all studies. In cases where a study reports results under multiple assumptions, we have selected the one deemed most comparable to our analysis. In most cases, the effects have been manually and visually extracted from figures, which entails a potential risk of reading errors. The effects from DSGE models refer to the national central bank's dynamic stochastic general equilibrium model. The calculation of the median for VAR models includes only BVAR and SVAR estimated employing Cholesky or sign restriction identification methods.

Table 6 also reports estimates from studies using alternative identification approaches, including high-frequency, narrative and FAVAR methods, when available. These studies are not included in the country medians, which are based on VAR models using Cholesky or sign-restriction identification, but they provide useful benchmarks for assessing whether our inflation effects are quantitatively similar to estimates based on other identifying assumptions. Overall, the comparison reinforces the main message of this section: inflation responses are sensitive to both identification and measurement choices, and the range of estimates across studies is wide.[50]

Taken together, the evidence shows that inflation responses to monetary policy shocks in the ten SOEs are considerably less stable, and more sensitive to modelling choices, than responses of other macroeconomic variables. Price puzzles arise frequently with identification based on short-run zero restrictions, particularly when headline inflation is used, and they are not resolved by adding commodity prices. While sign-restricted approaches and carefully chosen inflation measures, such as country-specific core indices, help mitigate these issues, they do not eliminate the underlying identification challenges. Overall, the results highlight that our inflation estimates based on short-run zero restrictions in small open economies must be interpreted with caution, as even the sign of the inflation response often depends more on the modelling specification than on a robust empirical regularity.

5.4 Decomposing inflation effects into channels

Above we noted that there is considerable heterogeneity in the inflation effects of monetary policy across the SOEs. In this section, we decompose the inflation effect into two distinct channels: an exchange rate channel and a policy rate (or residual) channel. The analysis is based on the impulse responses to a monetary policy shock identified using both short-run zero and sign restrictions. We apply a two-channel decomposition, which provides a convenient method for attributing impulse responses to different transmission channels. In our setting, the response of each variable to a monetary policy shock is decomposed into the dynamic effects that originate from the impact responses of the policy rate and the real exchange rate, respectively. A formal description of the decomposition is provided in Appendix A.[51]

[50] See, for instance, Ellis et al. (2014) and Cloyne and Hurtgen (2016) for the United Kingdom; Madeira and Salazar (2023) for Chile; and Champagne and Sekkel (2017) for Canada.

[51] For a recent comprehensive treatment of Transmission Channel Analysis (TCA) in dynamic linear models, see Wegner et al (2024). Our decomposition may be viewed as a particular case of the type of decompositions discussed by these authors.

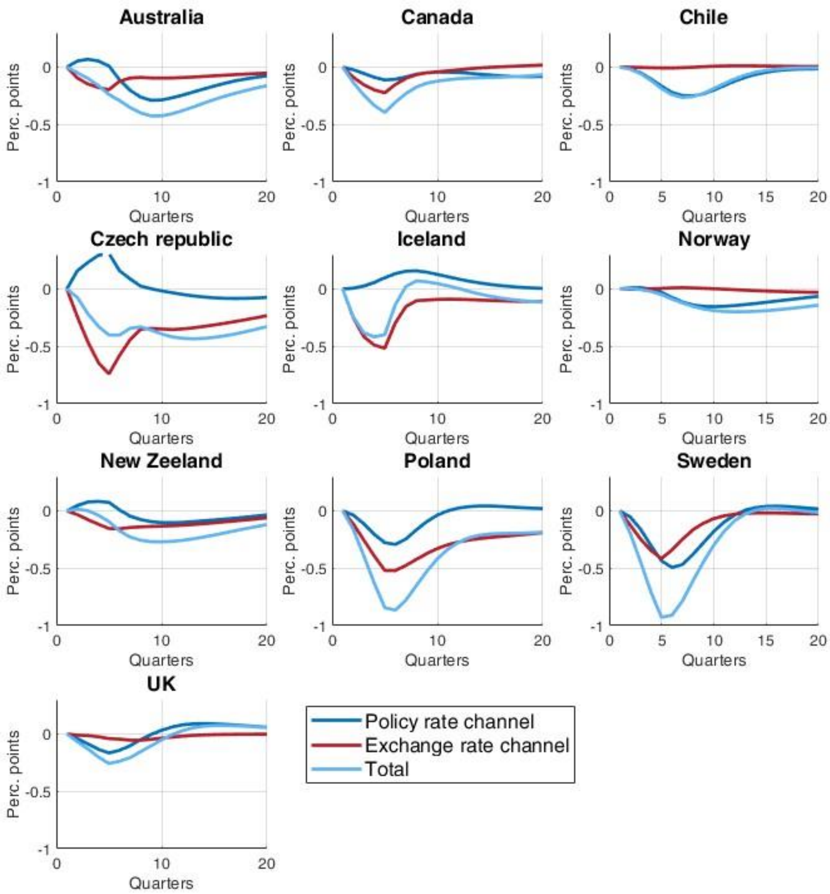
A first observation is that the response of the real exchange rate is almost entirely accounted for by the exchange rate channel.[52] This finding supports the interpretation of this component as a genuine exchange rate channel, as it captures the part of the monetary policy transmission mechanism that operates through movements in the real exchange rate.

Figure 9 presents the decomposition of the CPI inflation responses (and, in the case of Sweden, CPIF). Several patterns emerge. First, the relative importance of the policy rate and exchange rate channels differs across countries. Second, the exchange rate channel typically exerts its influence on inflation more rapidly, with effects that appear earlier and peak sooner than those associated with the policy rate channel. Third, there is a clear relationship between the size of the overall inflation response and the importance of the exchange rate channel. The Czech Republic, Iceland, Poland, and Sweden exhibit relatively large inflation effects, and in these countries the exchange rate channel is also comparatively strong. These are likewise the countries in which the monetary-policy-conditional exchange rate pass-through was estimated to be relatively high. By contrast, Chile and Norway display smaller inflation responses, and in both cases the exchange rate channel is considerably weaker.

Finally, in the two countries with the largest inflation effects – Poland and Sweden – the policy rate channel is also relatively strong, while it is rather weak in most countries. These are likewise the countries in which the monetary-policy-conditional Phillips elasticities were estimated to be relatively high.

[52] The exchange rate channel accounts for nearly all of the real exchange rate response in all countries except Australia and Sweden, where it instead explains roughly three-quarters of the total effect.

Figure 9. The inflation effect of monetary policy decomposed into policy rate and exchange rate channels



Note. The figure shows the inflation response to a normalised policy shock decomposed into policy rate and exchange rate channel contributions, respectively. The decomposition is described in Appendix A.

Concluding comments

This staff memo has estimated the effects of monetary policy in ten inflation targeting small open economies using a standardised SVAR framework. The cross-country aggregate responses are broadly consistent with standard monetary transmission: a contractionary policy shock lowers GDP and inflation, raises unemployment, and appreciates the real exchange rate. The estimated magnitudes are also broadly in line with the wider VAR literature and of a similar order of magnitude to those typically reported for larger economies. At the same time, the estimated effects differ across countries, although only some of these differences are statistically distinguishable from the cross-country median. The expanding-sample analysis suggests that the estimated effects on GDP and inflation are broadly stable as the sample is extended, even though point estimates increase somewhat in some countries. Finally, the inflation responses are particularly sensitive to the inflation measure and the identification strategy. Price puzzles arise frequently under short-run zero restrictions but are mitigated when using core inflation measures or sign restrictions. The decomposition of the inflation response further suggests that countries with larger inflation effects tend to have a larger exchange-rate-related contribution to inflation.

A central contribution of the memo is the analysis of monetary policy transmission elasticities. Rather than only comparing how strongly GDP, unemployment, inflation and the exchange rate respond to the policy shock, we examine the responses relative to one another through an Okun coefficient, a Phillips slope, a sacrifice ratio and exchange rate pass-through. This helps distinguish between countries where inflation or unemployment responses are large because GDP or the exchange rate responds strongly, and countries where they are large because the relationships between these variables are stronger. The cross-country patterns suggest that unemployment and inflation responses are more closely related to the corresponding transmission elasticities than to the raw GDP and exchange-rate responses alone. In this sense, the elasticities provide a useful summary of how policy-induced movements in GDP and the exchange rate are associated with labour-market and price outcomes across countries.

An important topic for future research is to explain more systematically why the effects of monetary policy differ across countries. The analysis in this memo documents cross-country heterogeneity in responses and transmission elasticities, but it does not attempt to relate these differences to underlying structural characteristics. One avenue would be to relate country-level responses to features such as household indebtedness, mortgage-market structure, labour-market flexibility and financial depth, in the spirit of cross-country studies of monetary policy transmission heterogeneity such as Deb et al. (2023), or more specific applications such as Di Casola and Grothe (2026). For small open economies, it would be particularly useful to study the determinants of the exchange-rate-related part of transmission. Import intensity, commodity exposure, exchange rate pass-through and the currency in which trade is invoiced may all affect how exchange-rate movements are transmitted to consumer prices. This would connect the cross-country evidence in this memo to the broader exchange rate pass-through literature, including work that emphasises heterogeneity across countries, industries and time periods, such as Ortega and Osbat (2020), and work on the role of currency invoicing and pricing behaviour, such as Gopinath, Itskhoki and Rigobon (2010).

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APPENDIX

APPENDIX A describes the block exogenous VAR model and short-run zero identification approach. A description of the method for decomposing the inflation effect of a monetary policy shock into two channels – a policy rate channel and an exchange rate channel – is also provided.

APPENDIX B describes the data and transformations used to construct the macroeconomic variables used in the VAR model, as summarised in Table B1. Table B2 presents an overview of previous related country-specific studies, and the different inflation and policy-rate measures they employ. Table B3 presents the trade weights used to construct the country-specific foreign variables.

APPENDIX C provides an exact description of the impulse response statistics (e.g. median peak effects for individual countries and aggregated across countries) reported in the paper.

APPENDIX D presents additional figures from the analysis. Figure D1 displays country-specific impulse response functions under sign restrictions, while Figure D2 shows the corresponding responses under short-run zero restrictions. Figure D3 reports the peak effects of monetary policy for the ten SOEs using short-run zero restrictions, and Figure D4 illustrates the cumulative exchange rate pass-through in the ten SOEs.

APPENDIX E describes the regression estimates of the Okun coefficient, Phillips slope, and exchange rate pass-through that are used to derive the monetary policy transmission elasticities in Section 3. Table E1 presents comparisons of MP-conditional elasticities using different calculation methods.

APPENDIX A – VAR, short-run zero identification, moving average representation and two-channel decomposition

The reduced form VAR(K) model is

$$y_t = c + A_1 y_{t-1} + A_2 y_{t-2} + \cdots + A_K y_{t-K} + u_t$$

with

$$E(u_t) = 0, \qquad E(u_t u_t') = \Sigma_u, \qquad E(u_t u_{t-s}') = 0, s \neq 0$$

where y_t is a $P \times 1$ vector of variables, A_k $k = 1, .., K$, are $P \times P$ coefficient matrices and u_t is the $P \times 1$ vector of reduced form innovations.

Short-run zero identification introduces orthonormal structural shocks ε_t such that

$$u_t = B\varepsilon_t, E(\varepsilon_t \varepsilon_t') = I,$$

which implies

$$\Sigma_u = E(u_t u_t') = BB'.$$

Under short-run zero identification, B is taken to be lower triangular, with the ordering of variables in y_t determining the implied contemporaneous restrictions.

We assume that the foreign variables are block exogenous. With three foreign variables placed first in y_t the upper-right block of A_k of dimension $3 \times (P-3)$ is zero (foreign variables do not depend on lagged domestic variables).

We further assume that the real exchange rate may respond contemporaneously to the monetary policy shock while the other variables respond with a lag. Accordingly, we order the variables so that the real exchange rate is placed last in the vector y_t, (position P), and the policy rate is placed in position $P-1$. In our baseline model we have $P = 8$ variables.

Let the monetary policy shock be $\varepsilon_{P-1,t}$ (shock index $P-1$). Under the recursive structure, the impact (horizon $h = 0$) responses are read from $u_t = B\varepsilon_t$. Holding all other shocks at zero, a one-unit policy shock implies

$$\Delta y_{P-1,t} = u_{P-1,t} = b_{P-1,P-1}\varepsilon_{P-1,t}$$

and

$$\Delta y_{P,t} = u_{P,t} = b_{P,P-1}\varepsilon_{P-1,t}$$

while $u_{j,t} = 0$ for $j < P-1$. Hence, on impact the monetary policy shock enters the system only through the reduced-form innovations of the policy-rate equation ($u_{P-1,t}$) and the exchange-rate equation ($u_{P,t}$). Other domestic variables (e.g. GDP, unemployment, inflation) do not respond contemporaneously to $\varepsilon_{P-1,t}$.

Because the foreign block is block-exogenous, the foreign variables are not affected by domestic shocks in the dynamic system.

Now, the responses to the monetary policy shock are fully determined by its contemporaneous impact on the reduced-form innovations in the policy-rate and exchange-rate equations. Under the recursive identification with the policy rate ordered in position $P-1$ and the real exchange rate in position P, the monetary policy shock loads contemporaneously only on $u_{P-1,t}$ and $u_{P,t}$. This motivates a decomposition of the impulse responses to the monetary policy rate shock into two channels; an exchange rate channel (the component transmitted through $u_{P,t}$) and a policy rate (or residual) channel (the component transmitted through $u_{P-1,t}$). We may note that for most countries in our study the movement in the real exchange rate following a monetary policy shock is almost entirely accounted for by the exchange rate channel. This, we believe, validates the decomposition.

Next, we show how the decomposition is obtained using the moving average (MA) representation which is provided by

$$y_t - \mu = \sum_{h=0}^{\infty} \Gamma_h \, u_{t-h} = \sum_{h=0}^{\infty} \Gamma_h \, B\varepsilon_{t-h} = \sum_{h=0}^{\infty} \Theta_h \, \varepsilon_{t-h}$$

where

$$\Gamma_h = \sum_{j=1}^{min(K,h)} A_j \Gamma_{h-j} \, , h \geq 1$$

and $\Gamma_0 = I$ and $\Gamma_h = 0$ for $h < 0$. In our case the impulse response to the policy shock (in position $P-1$) for domestic variable i at horizon h may be written as the sum of the two channels

$$\Theta_{i,P-1,h} = \Gamma_{i,P-1,h} \mathrm{b}_{P-1,P-1} + \Gamma_{i,P,h} \mathrm{b}_{P,P-1}$$

The first term captures the part of the response that originates from the contemporaneous impact of the policy shock on the policy-rate innovation $u_{P-1,t}$ propagated through the VAR dynamics. The second term captures the part that originates from the contemporaneous impact of the same policy shock on the exchange-rate innovation $u_{P-1,t}$, also propagated through the VAR dynamics. We refer to these as the policy-rate (residual) channel and the exchange-rate channel, respectively.

APPENDIX B – Data and transformations

The variables used in the model, together with their respective sources and transformations, are presented in Table B1 below. The measures used for GDP, inflation, the real effective exchange rate, the unemployment rate, and the foreign variables are largely consistent across all ten SOEs. Using the same measures for these variables ensures internal consistency and facilitates cross-country comparisons of the effects of monetary policy on key macroeconomic indicators.

In our baseline VAR model, inflation is measured using the CPI for all countries except Sweden, where the CPIF is applied. In the in-depth analysis of inflation responses, we additionally employ core inflation measures and other indicators frequently used in the literature to better understand the underlying drivers of price dynamics. In some specifications, we include oil prices.[53] Table B2 provides an overview of the inflation measures used in the previous literature.

[53] See Section 5.3 for a discussion on the rationale for including oil prices in structural VAR models.

However, the choice of monetary policy interest rates differs across countries, reflecting conventions in the SVAR literature.[54] Previous research shows that while some countries rely on the central bank's official policy rate, others use short-term market rates as proxies.[55] In the Czech Republic, Norway, New Zealand, and Poland, the short-term domestic effective nominal interest rate is employed, whereas in the remaining countries the central bank's policy rate remains the standard measure. This choice is due to the lack of policy rate data covering the entire sample period for the Czech Republic, New Zealand and Poland, while for Norway, it follows established literature conventions.[56]

To ensure comparability, our model follows a standardised approach. The foreign variables (GDP, CPI, and the policy rate) are largely constructed in the same way for all countries. For most countries, they are calculated as trade-weighted aggregates of the United States and the euro area, using time-varying, country-specific trade weight data from the national central banks. The historical average country trade weights are reported in Table B3, offering an indication of the relative trade shares between the US and the euro area for each country. For the remaining countries, we use either the United States or the euro area as the foreign aggregate. For Australia and Chile, the United States is used, while for Poland the euro area is chosen. These selections reflect the predominant trading partners of each country. As shown in Table B2, the use of foreign variables in previous VAR models varies across countries.

[54] These studies were selected based on their relevance to our VAR framework, identification strategy, and connection to central bank research.

[55] See Table B2 below for an overview of previous studies and the various policy rate measures they employ.

[56] The correlation between policy rate and short-term domestic effective nominal interest rate is above 0.98 for the Czech Republic, Norway, New Zealand and Poland.

Table B1. Data and transformations		
Key macroeconomic variables	**Sources**	**Transformations**
Domestic variables		
Inflation measured by the Consumer Price Index (CPI). For Sweden, the CPIF was applied.	Australian Bureau of Statistics, Chilean National Statistics Institute, Czech Statistical Office, Latin Macro Watch, Polish Central Statistical Office, Statistics Canada, Statistics Iceland, Statistics New Zealand, Statistics Norway, Statistics Sweden and U.K. Office for National Statistics.	First difference of the natural logarithm. Inflation measures for each country were seasonally adjusted by the Riksbank using the X-11 ARIMA method developed by the U.S. Census Bureau. Inflation data were converted into quarterly values. For Chile, the series from the Chilean National Statistics Institute were extended backwards from 2009 to 1995, using data from Latin Macro Watch.
Gross Domestic Product in constant prices, quarterly data. Seasonally adjusted data for all countries, and for Sweden they are additionally calendar-adjusted. For Norway, the measure used is mainland GDP, which excludes the oil-based offshore sector. This measure is used in comparable studies, as it is considered to better reflect the country's export-oriented economy (see, for example, Robstad, 2014; Bjørnland, 2008; Bjørnland et al., 2024).	Australian Bureau of Statistics, Central Bank of Chile, Eurostat, OECD, Statistics Canada, Statistics Iceland, Statistics New Zealand, Statistics Norway, Statistics Sweden, Sveriges Riksbank and U.K. Office for National Statistics.	Natural logarithm. For Chile, the series from the Central Bank of Chile were extended backwards from 1996 to 1995 using data from the OECD. In this process, the GDP growth rate was calculated based on the OECD series, which was then employed to reconstruct historical values back to the first quarter of 1995.
Unemployment rate in per cent at the quarterly level. Seasonally adjusted data. While the age groups are broadly similar across countries, some differences exist; in Canada and Chile, 15 years and over; in the United Kingdom, 16 years and over; and in Australia 18-74 years. In Norway, New Zealand, the Czech Republic, Poland, and Iceland, the unemployment rate is instead based on the total number of registered unemployed citizens.	Australian Bureau of Statistics, Czech Ministry of Labor and Social Affairs, Icelandic Directorate of Labor, Norwegian Labour and Welfare Administration, OECD, Polish Ministry of Family, Labour and Social Policy, Statistics Canada, Statistics New Zealand, Statistics Sweden, Sveriges Riksbank and U.K. Office for National Statistics.	Level. For the Czech Republic, Norway, and Poland, the unemployment data were seasonally adjusted by the Riksbank using the X-11 ARIMA method developed by the U.S. Census Bureau. For the remaining countries, seasonally adjusted data were obtained from national statistical agencies. Quarterly data were available for New Zealand and Sweden, while for the remaining countries (Australia, Canada, Chile, the Czech Republic, Iceland, Norway, Poland, and the United Kingdom), monthly data were converted into quarterly series.
Real effective exchange rate For each country, we use the broad index for the real effective exchange rate developed by the Bank for International Settlements (BIS). The BIS effective exchange rate indices cover 64 economies and are calculated as geometric trade-weighted averages of bilateral	Bank for International Settlements.	Natural logarithm. The original series are at the monthly frequency and were converted by the Riksbank into quarterly values.

Table B1. Data and transformations		
exchange rates adjusted for relative consumer prices.		
Policy rate in per cent and quarterly averages. AU: Cash Rate CA: Overnight Target Rate CL: Policy rate CZ: 3-month PRIBOR IC: Key Interest Rate NO: 3-month NIBOR NZ: 90-day Bank Bill Rate PL: 1-month PRIBOR SE: Policy rate UK: Bank Rate	Bank of England, Central Bank of Iceland, National Bank of the Czech Republic, Norske Finansiella Referanser AS, Polish GPW Benchmark S.A., Reserve Bank of Australia, Reserve Bank of Chile, Reserve Bank of New Zealand, Statistics Canada, and Sveriges Riksbank.	Level. For Sweden, the Riksbank publishes policy rate data at the quarterly level. For all other countries, daily rates were converted into quarterly averages by the Riksbank.
Foreign variables		
United States Gross Domestic Product in constant prices (USD) at the quarterly level. Seasonally adjusted data.	U.S. Bureau of Economic Analysis.	Natural logarithm.
United States Consumer Price Index (CPI) at the quarterly level. Seasonally adjusted data.	U.S. Bureau of Labour Statistics.	First difference of the natural logarithm. Monthly inflation data were converted into quarterly values.
United States Policy Rate in per cent and quarterly averages.	Federal Reserve.	Level. Daily rates were converted into quarterly averages.
Euro Area Gross Domestic Product in constant prices (EUR) at the quarterly level.	Eurostat.	Natural logarithm.
Euro Area Harmonised Indices of Consumer Prices (HICP) at the quarterly level. Seasonally adjusted data.	ECB and Eurostat.	First difference of the natural logarithm. Since the seasonally adjusted HICP series from the ECB is only available from January 1997 onwards, the series is extended back to January 1995, using seasonally adjusted historical HICP data from Eurostat. The two series are linked based on quarterly percentage changes. The historical HICP data from Eurostat are seasonally adjusted using the X-11 ARIMA method developed by the U.S. Census Bureau.
Euro Overnight Index Average (EONIA) in per cent and quarterly averages. EONIA is the average overnight reference rate for which European banks lend to one another in euros.	The Riksbank.	Level.
Oil prices measured as spot price of Brent crude oil in USD.	Intercontinental Exchange and the Riksbank.	First difference of the natural logarithm. Daily rates converted into quarterly values.

Table B2. Variables used in the previous VAR literature

Country	Domestic inflation	Domestic policy rate	Foreign variables
Australia	CPI (Gibbs et al., 2018; Brischetto and Voss, 1999; Berkelmans, 2005; Fisher and Huh, 2023) Underlying CPI (Beckers, 2020) Trimmed Mean CPI (Read, 2022, Jääskelä and Jennings, 2010)	RBA Cash rate (Gibbs et al., 2018; Berkelmans, 2005; Jääskelä and Jennings, 2010; Fisher and Huh, 2023; Read, 2022; Beckers, 2020).	<u>Foreign GDP:</u> GDP of Australia's largest major trading partners weighted by export share (Gibbs et al., 2018). US GDP (Jääskelä and Jennings, 2010; Read, 2022). <u>Foreign inflation:</u> Inflation rate of Australia's major trading partners weighted by export share (Gibbs et al., 2018), US inflation (Jääskelä and Jennings, 2010; Fisher and Huh, 2023) <u>Foreign policy rate:</u> Policy rate of the US, Japan and euro area (Gibbs et al., 2018), US policy rate (Jääskelä and Jennings, 2010; Fisher and Huh, 2023; Read, 2022; Beckers, 2020).
Canada	CPI (Bhuiyan, 2012, Raghavan et al., 2016; Alexander and Reza, 2022; Ha and So, 2023; Kim and Lim, 2018) CPIX (Champagne and Sekkel, 2017)	Overnight Target Rate (Bhuiyan, 2012; Raghavan et al., 2016; Alexander and Reza, 2022; Ha and So, 2023)	<u>Foreign GDP:</u> US Industrial production (Bhuiyan, 2012; Ha and So, 2023; Kim and Lim, 2018), US GDP (Alexander and Reza, 2022) <u>Foreign inflation:</u> US inflation (Bhuiyan, 2012; Bjørnland et al., 2024; Ha and So, 2023; Kim and Lim, 2018) <u>Foreign policy rate:</u> US policy rate (Bhuiyan, 2012; Raghavan et al., 2016; Ha and So, 2023; Kim and Lim, 2018)
Chile	CPI (Madeira and Salazar, 2023, Auroba et al., 2021)	1-year real interest rate (Madeira and Salazar, 2023) Policy rate (Aruoba et al., 2021)	Previous models do not include foreign variables.
Czech Republic	CPI (Kucharcukova et al., 2013; Czech National Bank, 2021) CPI excluding regulated prices (Borys and Horvath, 2008)	3-month PRIBOR (Borys and Horvath, 2008; Kucharcukova et al., 2013, Czech National Bank, 2021)	Foreign policy rate: 1y EURIBOR (Borys and Horvath, 2008)
Iceland	CPI (Pétursson, 2023)	Key Interest Rate (Pétursson, 2023)	<u>Foreign policy rate:</u> Trade-weighted average foreign monetary policy rate (Pétursson, 2023)
Norway	CPI-ATE (Bjørnland, 2008; Gerdrup, 2017; Robstad, 2014) CPI (Bjørnland et al., 2024)	3-month NIBOR (Bjørnland, 2008; Bjørnland et al., 2024; Bjørnland et al. 2024; Robstad, 2014) Nominal sight deposit rate (Gerdrup, 2017)	<u>Foreign GDP</u>: GDP in 25 of Norway's most important trading partners (Gerdrup, 2017) <u>Foreign inflation</u>: Trade-weighted CPI for 25 of Norway's trading partners (Gerdrup, 2017; Robstad, 2014) <u>Foreign policy rate:</u> Trade-weighted foreign interest rate based on Norway's four largest trading partners

Table B2. Variables used in the previous VAR literature

			(Bjørnland, 2008; Bjørnland et al., 2024), 3-month nominal money market interest rates for trading partners (Gerdrup, 2017)
New Zealand	CPI (Albertini et al., 2011; Buckle et al., 2003; Culling et al., 2019) Tradable and non-tradable CPI (Bloor and Matheson, 2008)	90-day Bank Bill Rate (Albertini et al., 2011; Bloor and Matheson, 2008; Culling et al., 2019) Nominal interest rate (Buckle et al., 2003)	Foreign GDP: Trade-weighted measure of the GDP of New Zealand's 16 largest trading partners (Albertini et al., 2011) World GDP (Bloor and Matheson, 2008), Trade-weighted world industrial production (Buckle et al., 2003) Foreign inflation: Trade-weighted measure of the CPI inflation of New Zealand's 16 largest trading-partners (Albertini et al., 2011), World CPI (Bloor and Matheson, 2008) Foreign policy rate: 80-20 weighted measure of the US 90-day bank bill yield and the Australian 90-day bank bill yield (Albertini et al., 2011), World 90-day rates (Bloor and Matheson, 2008), weighted average of Australian, US, UK, Japan and German 90-day rates (Buckle et al., 2003).
Poland	CPI (Greszta et al., 2023; Haug et al., 2019) HICP (Demchuk et al., 2011)	3-month WIBOR (Greszta et al., 2023; Haug et al., 2019) Inter-bank short-term interest rate (Demchuk et al., 2011)	Foreign inflation: Euro-area HICP (Haug et al., 2019) Foreign policy rate: 1-month EURIBOR (Demchuk et al., 2011)
Sweden	CPIF (Lyhagen and Shahnazarian, 2023; Di Casola and Iversen, 2019; Laséen and Strid, 2013; Hopkins et al., 2009; Villani and Warne, 2003)	Policy rate (Lyhagen and Shahnazarian, 2023; Di Casola and Iversen, 2019; Laséen and Strid, 2013; Hopkins et al., 2009; Villani and Warne, 2003)	Foreign GDP: Trade-weighted measure of foreign GDP (Laséen and Strid, 2013; Di Casola and Iversen, 2019; Hopkins et al., 2009; Villani and Warne, 2003) Foreign inflation: Trade-weighted measure of foreign inflation (Laséen and Strid, 2013; Di Casola and Iversen, 2019; Hopkins et al., 2009; Villani and Warne, 2003) Foreign policy rate: Foreign short-term interest rate (Laséen and Strid, 2013; Di Casola and Iversen, 2019; Hopkins et al., 2009; Villani and Warne, 2003)
United Kingdom	CPI (Cesa-Bianchi et al., 2020; Ellis et al., 2014; Brignone and Piffer, 2025) RPIX12m inflation (Cloyne and Hurtgen, 2016)	Bank Rate (Cloyne and Hurtgen, 2016; Elis et al., 2014; Brignone and Piffer, 2025) 1-year nominal gilt yield (Cesa-Bianchi et al., 2020)	Foreign GDP: Trade-weighted World GDP (Brignone and Piffer, 2025) Foreign inflation: Trade-weighted World CPI (Brignone and Piffer, 2025)

Table B3. Relative historical average trade weights of the US and euro area for a selection of small open economies (per cent)

Country	United States	Euro area
Canada	82	18
Czech Republic	4	96
Iceland	20	80
Norway	14	86
New Zealand	57	43
Sweden	16	84
United Kingdom	27	73

Note. Trade weights refer to the average for the period 1995-2024. Data on trade weights are collected from national central banks. For some countries, trade weight data are not available for the full sample and therefore represent an average based on a subset of years. Time-varying trade weights were used for all countries except Poland and the Czech Republic. Sources: National central banks and the Riksbank.

APPENDIX C – Reporting of impulse response functions

Here we provide an exact description of the impulse response statistics reported in the paper. These statistics are computed based on the posterior distribution of model parameters, which is represented by a sample of parameter draws from the posterior distribution obtained using the Gibbs sampler.

In Figure 1, we report the posterior median impulse responses across the 10 countries. For each country, we first normalise each IRF posterior draw such that the impact response of the policy rate increases by 1 percentage point on impact. Henceforth, it is implicitly understood that all IRFs are normalised. We then take the median across these normalised IRF posterior draws to obtain the median IRFs for each variable and country (i.e. the median IRF based on the posterior draws). Finally, we compute the median response for each variable across countries (i.e. median of country median IRFs).

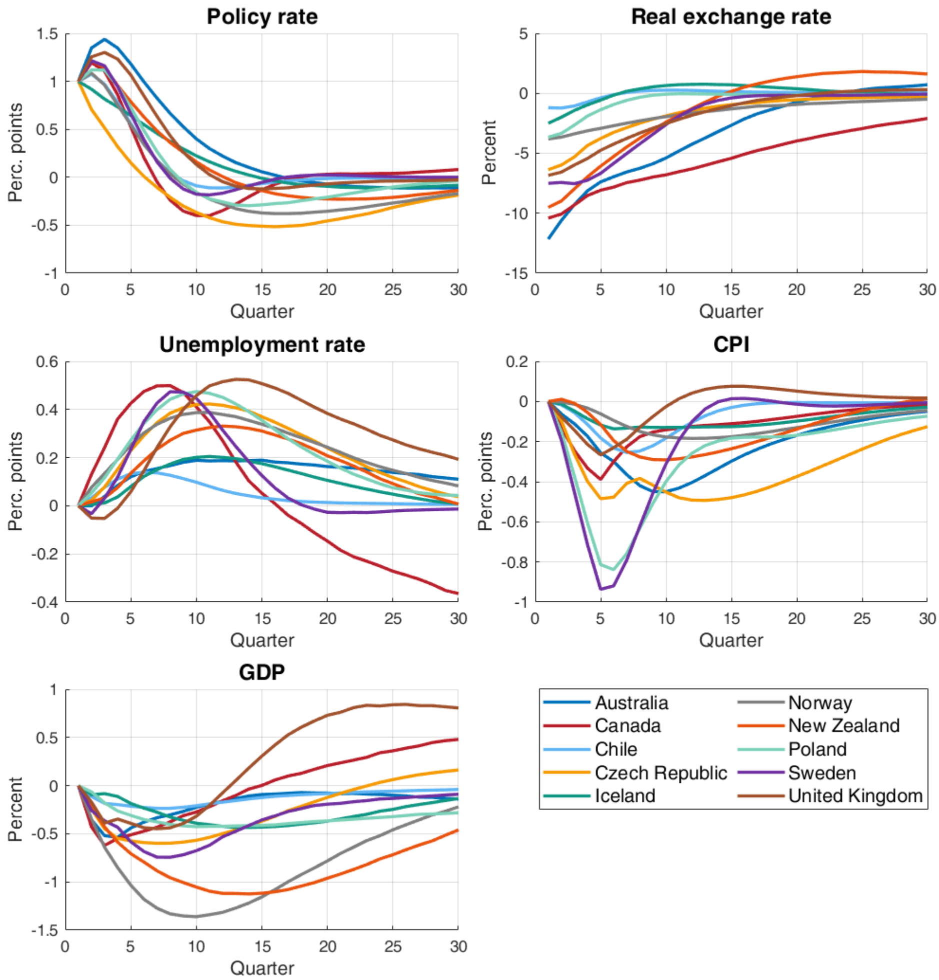
In Figure 2, our focus is on the median peak effects for a set of variables in each country. For a given variable and country, we compute the peak value for each normalised posterior IRF draw. We then report the median peak effect (the median across posterior draws) and finally compute the cross-country median peak effects.

Comparing Figure 1 and 2, it may appear that the aggregate peak effects reported in the two figures are inconsistent. For example, the aggregate peak effect for GDP is −0.8 in Figure 2 but appears to be around −0.5 based on Figure 1. What explains this stark difference? We note that the posterior distribution of the IRFs includes uncertainty about the timing of the peak effect. In Figure 2, we compute the distribution of the peak effect while disregarding its timing, which naturally yields a larger estimate of the peak effect.

Similar discrepancies arise when comparing the peak effects of variables in Figure 2 with those in Figure D1, or in Figure D2 with Figure D3. Figure 2 and D3 present the distribution of the peak effect while disregarding its timing, whereas Figure D1 and D2 report the peak effect at each time period.

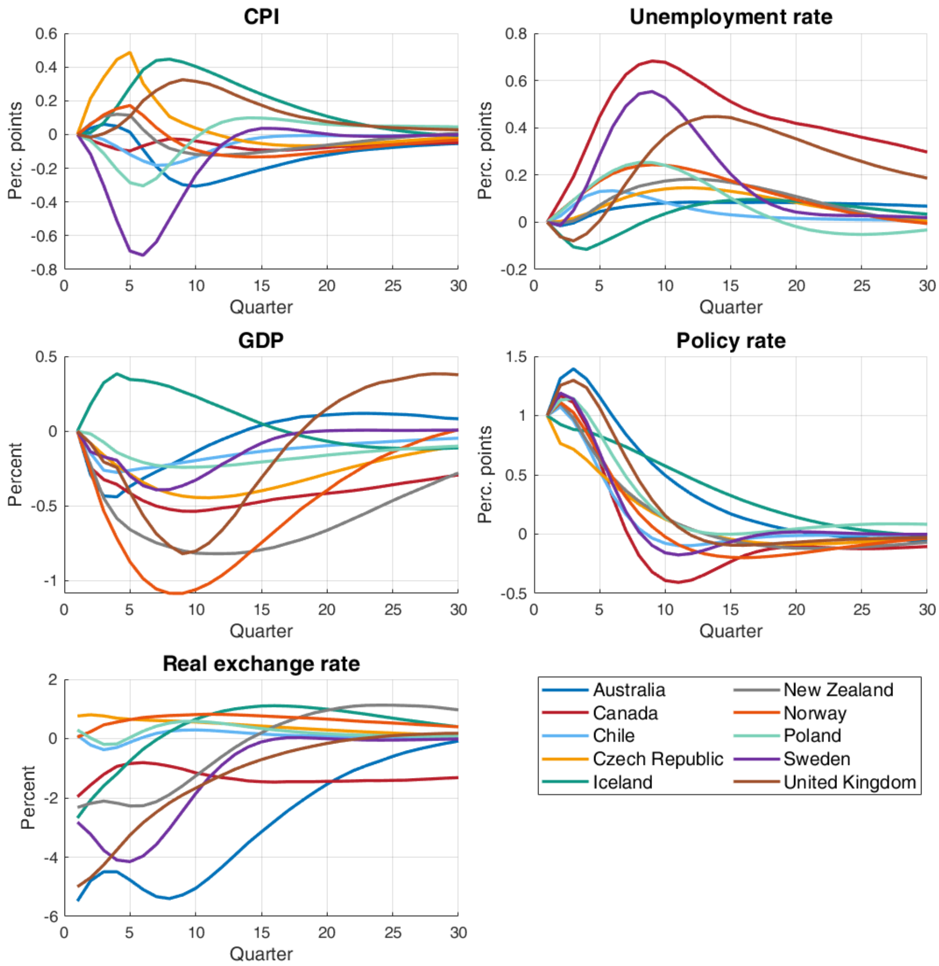
APPENDIX D – Additional results

Figure D1. Impulse responses to a monetary policy shock in ten SOEs identified using short-run zero and sign restrictions



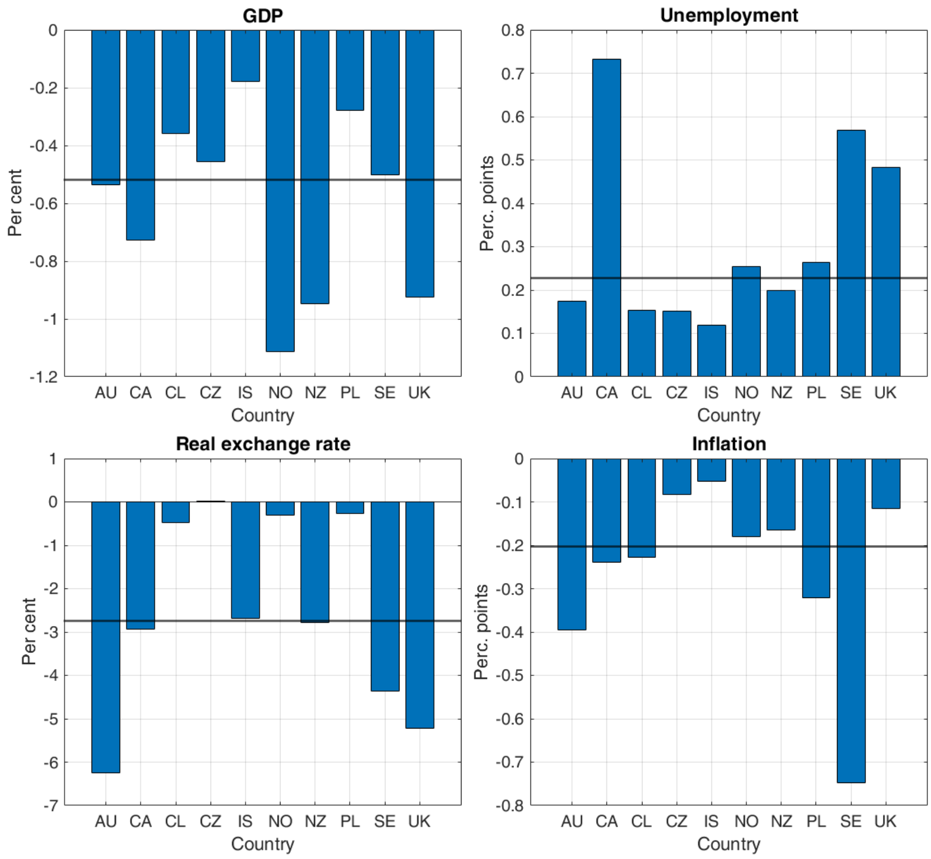
Note. The figure reports the impulse responses of a 1-percentage-point increase in the policy rate in ten small open economies identified using sign restrictions. The country models are estimated over the period 1995Q1-2024Q4, except for Chile, whose sample begins in 1995Q3. The policy rate, GDP, the unemployment rate and the real exchange rate are measured in levels while inflation is in annual percentage changes.

Figure D2. Impulse responses to a monetary policy shock in 10 SOEs identified using short-run zero restrictions



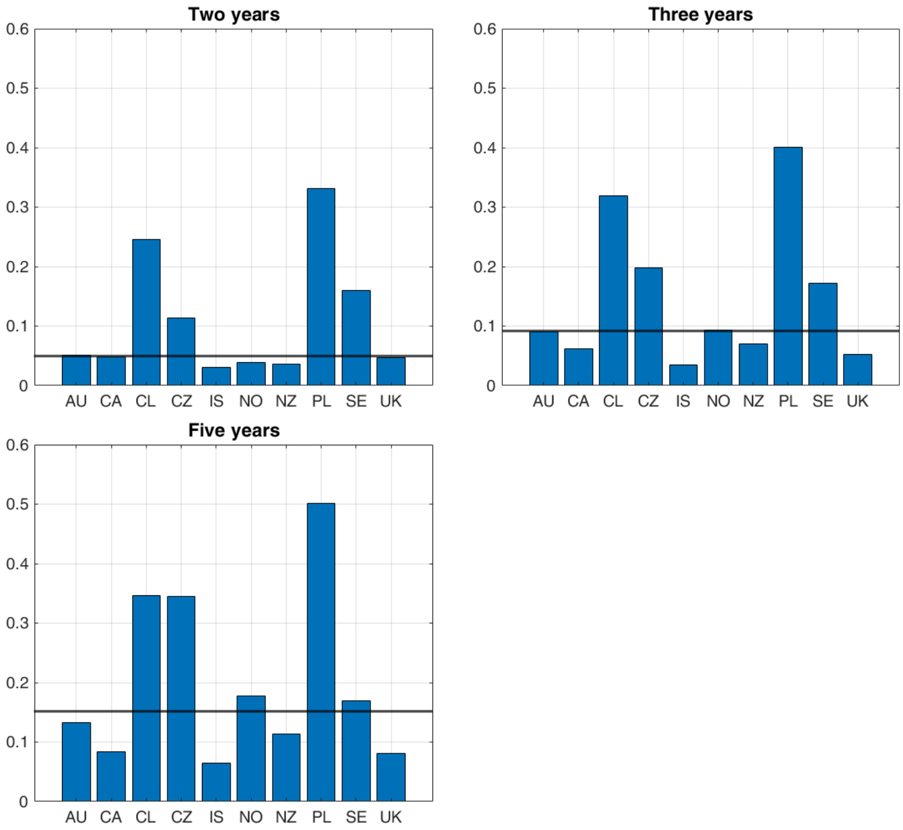
Note. The figure reports the impulse responses of a 1-percentage-point increase in the policy rate in ten small open economies identified using short-run zero identification. The country models are estimated over the period 1995Q1-2024Q4, except for Chile, whose sample begins in 1995Q3. The policy rate, GDP, the unemployment rate and the real exchange rate are measured in levels while inflation is in annual percentage changes.

Figure D3. Peak effects of monetary policy in ten SOEs using short-run zero restrictions



Note. The figure shows the country peak impulse responses for four different variables identified using short-run zero restrictions. GDP, the unemployment rate and the real exchange rate are in levels, whereas inflation is reported in annual percentage changes. Here, the peak responses are defined as the lowest values along the impulse response paths for GDP, the real exchange rate and inflation, and as the highest values for the unemployment rate. The horizontal line refers to the median of the country-specific median peak impulse responses.

Figure D4. Cumulative exchange rate pass-through in ten SOEs



Note. The figure reports the cumulative exchange rate pass-through, calculated as the ratio of cumulative inflation responses and the peak nominal exchange rate, over horizons of two, three and five years across ten small open economies. The results at the three-year horizon are shown in Figures 3 and 4. The horizontal line refers to the median of the country-specific median peak impulse responses.

APPENDIX E – Regression estimates of monetary policy transmission elasticities

To calculate the unconditional monetary policy transmission elasticities in our sample of SOEs, we use standard model specifications that are deliberately straightforward, mirroring the simplicity of our VAR specification. The simplicity is reflected in the fact that these models are commonly used as benchmark models in the existing literature, linear and estimated using Ordinary Least Squares (OLS).

To ensure comparability, we estimate the regressions over the same sample period used in our main VAR analysis. The data employed in these regressions are identical to that used in the VAR, with the addition of a new variable, the nominal exchange rate, in the estimation of exchange rate pass-through.

The estimated unconditional elasticity, μ, is presented in Figure 4 and can be read from the y-axis of each subpanel.

Okun coefficient

We estimate the Okun coefficient in each country using the following specification:

$$(u-\bar{u})_t = \alpha + \sum_{k=0}^{2} \beta_k (y-\bar{y})_{t-k} + \gamma(u-\bar{u})_{t-1} + \varepsilon_t \tag{1}$$

where $(u-\bar{u})_t$ and $(y-\bar{y})_t$ are the quarterly unemployment and GDP gaps, respectively, both measured using the Hodrick-Prescott filter. The Okun coefficient is given by $\rho = \sum_{k=0}^{2} \beta_k$, while the long-run coefficient is calculated as $\mu = \frac{\rho}{1-\gamma}$.

Phillips slope

Our estimation of the Phillips slope for each country is guided by the model formulated by Ball and Mazumder (2020), which we implement empirically as:

$$\Delta p_t = \alpha + \beta \Delta p_{t-1} + \gamma(y-\bar{y})_t + \varepsilon_t \tag{2}$$

where p_t is domestic consumer prices, $(y-\bar{y})_t$ is the quarterly GDP gap measured by the Hodrick-Prescott Filter and Δ is the difference operator. The Phillips slope coefficient is given by γ and the long-run coefficient is calculated as $\mu = \frac{\gamma}{1-\beta}$. Changes in domestic prices are expressed as annual quarterly percentage changes.

Exchange rate pass-through

For exchange rate pass-through (ERPT) in each country, we estimate the model specified by Anderl and Caporale (2023) which takes the following form:

$$\Delta p_t = \alpha + \beta \Delta p_{t-1} + \gamma \Delta NER_t + \delta \Delta RER_t + \theta \Delta p_t^* + \lambda(y-\bar{y})_t + \varepsilon_t \tag{3}$$

where p_t is domestic consumer prices, NER_t is the nominal effective exchange rate, RER_t is the real effective exchange rate, p_t^* is the trade-weighted foreign consumer prices, $(y-\bar{y})_t$ is the GDP gap measured by the Hodrick-Prescott Filter and Δ is the difference operator. The ERPT coefficient is given by γ and the long-run coefficient is calculated as $\mu = \gamma/(1-\beta)$.

The nominal exchange rate is sourced from Bank of International Settlements (BIS) and represents the broad index, similar to the real exchange rate used in our main analysis. For all countries except Poland, we use the trade-weighted consumer prices as the measure of foreign prices.[57] Changes in prices and exchange rates are expressed as annual percentage changes.

[57] See Table B3 in Appendix B for more details.

Table E1. Comparisons of conditional monetary policy transmission elasticities calculated using different methods

	Okun coefficient	Phillips slope	Sacrifice ratio	Exchange rate pass-through
Peak	0.6	0.7	-	-
Cumulative, horizon				
8	0.4	0.6	1.4	0.1
12	0.5	0.6	1.6	0.1
20	0.5	0.5	2.1	0.2

Note: Cross-country medians of cumulative monetary policy–conditional elasticities computed at 8, 12, and 20 quarters, corresponding to two-, three-, and five-year horizons.

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