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VAR meets DSGE:  
Uncovering the Monetary Transmission Mechanism  
in Low-Income Countries

by Bin Grace Li, Stephen O'Connell, Christopher Adam, Andrew Berg, and  
Peter Montiel

I N T E R N A T I O N A L M O N E T A R Y F U N D

**IMF Working Paper**

Research Department and Institute for Capacity Development

**VAR meets DSGE: Uncovering the Monetary Transmission Mechanism  
in Low-Income Countries\***

**Prepared by Bin Grace Li, Stephen O’Connell, Christopher Adam, Andrew Berg, and  
Peter Montiel**

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**Abstract**

VAR methods suggest that the monetary transmission mechanism may be weak and unreliable in low-income countries (LICs). But are structural VARs identified via short-run restrictions capable of detecting a transmission mechanism when one exists, under research conditions typical of these countries? Using small DSGEs as data-generating processes, we assess the impact on VAR-based inference of short data samples, measurement error, high-frequency supply shocks, and other features of the LIC environment. The impact of these features on finite-sample bias appears to be relatively modest when identification is valid—a strong caveat, especially in LICs. However, many of these features undermine the precision of estimated impulse responses to monetary policy shocks, and cumulatively they suggest that “insignificant” results can be expected even when the underlying transmission mechanism is strong.

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## 1. Introduction

Mishra, Montiel and Spilimbergo (2012) and Mishra and Montiel (2013) survey a large literature on the effectiveness of the monetary transmission mechanism (MTM) in low-income countries. They find that standard empirical methods, in the form of vector autoregressions (VARs) applied to macroeconomic data, are consistent with *weaker and less reliable* MTMs in low-income countries than in high-income and emerging economies. By *weaker* they mean that monetary policy instruments tend to have small estimated effects on aggregate demand. By *less reliable* they mean that these estimated impacts are not precisely estimated, leaving considerable statistical uncertainty about the true MTM. Mishra *et al.* suggest two broad possible explanations for these findings:

- “Facts on the ground”: Formal financial markets are small and poorly arbitrated in these countries, and many low-income countries (LICs) maintain fixed or heavily managed exchange rates; as a consequence, the link between the short-term interest rates that central banks can control and the variables that matter for aggregate demand (e.g., longer-term interest rates, the exchange rate) may be weak or absent. Even the bank lending channel may tend to be weak when the formal financial sector is small, financial frictions are severe, and the banking industry is characterized by imperfect competition.
- “Limitations of the method”: The MTM is not in fact weak, but the data-intensive, atheoretic methods typically used to evaluate the MTM empirically are not capable of measuring its strength accurately in the research environment characteristic of LICs. If this explanation is correct, then it is the VAR evidence in LICs that is weak and unreliable, not the MTM itself.

The stakes here seem high. If the ‘facts on the ground’ explanation is correct, the empirical literature suggests that managing monetary policy successfully may be particularly difficult in low-income countries. Along with other features of the LIC environment such as frequent large supply shocks, weak and uncertain transmission may make it more difficult for policymakers to keep inflation within narrow bounds and to stabilize activity in the face of demand shocks. On the other hand, if the missing MTM mainly reflects methodological limitations, then the results of the VAR-based literature should be discounted by policymakers and researchers evaluating the strength and reliability of the MTM should seek empirical approaches that are more robust to the peculiar weaknesses of these methods in LIC-like

environments. For example, the use of bank-level or even loan-level data to investigate the strength of the bank lending channel is an obvious candidate (e.g., Mbowe 2012, Walker 2011, Abuka *et al* 2015).

In this paper we attempt to discriminate between the “facts on the ground” and “limitations of the method” interpretations of the missing MTM, focusing on whether the characteristics of a LIC research environment are particularly hostile to VAR-based approaches to identification. Specifically, if a strong MTM is present, can standard structural VAR (SVAR) methods uncover it in a LIC-like research environment? We address this question by applying SVAR methods to a world in which a strong MTM exists but the research environment has some of the features that are characteristic of LICs. A list of these features might include, for example, poorly-understood economic structure and non-transparent central banks; short data samples due to missing data or recent major structural changes or policy reforms; large measurement errors; and a high volatility of macroeconomic shocks (especially the prevalence of large temporary supply shocks).

To implement this program we need to be specific enough about the data-generating process to generate Monte Carlo evidence on the sampling properties of VAR estimators, while at the same time being specific enough about the underlying economic structure to fully control the nature of the “true” MTM. In both respects, small dynamic, stochastic general-equilibrium models (DSGEs) provide an ideal platform for our analysis. We therefore flip the familiar dialogue between VAR evidence and DSGEs on its head. Instead of assessing the properties of DSGEs against the VAR evidence, as in Christiano *et al.* (2005), we assess VAR methodologies against a DSGE-based data-generating process, along the lines of Li (2008).<sup>3</sup> We develop a simple DSGE that embodies an MTM with an interest-rate channel and an exchange-rate channel, use the solution to this DSGE to generate multiple independent runs of data, and then within each of these runs, mimic the process of an empirical researcher using VAR-based methods to infer the nature of the MTM. In particular, we examine the properties of the impulse response functions (IRFs) that she would produce. We compare her median estimated IRF to the true one, study the spread of estimated IRFs across simulations, and examine the power of conventional significance tests against the hypothesis of a zero response.<sup>4</sup>

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<sup>3</sup> Li (2008) uses Monte Carlo simulations to evaluate alternative identification strategies in VAR estimation of monetary models, and to assess the accuracy of measuring money instability as a cause of output fluctuations, and the result supports the claim that monetary shocks contribute no more than one third of the cyclical variance of post-war U.S. output (Lucas, 2003).

<sup>4</sup> We could examine instead the properties of the coefficient estimates underlying the IRFs, but our interest here is in mimicking the approach of applied empirical researchers, who are likely to focus on producing, analyzing, and using IRFs for policy analysis and forecasting.

Sections 2 and 3 of the paper introduce a stripped-down and linearized stationary DSGE in four macroeconomic variables: the GDP gap, the inflation rate, the real exchange rate, and the nominal interest rate. We discuss the relationship between DSGEs and structural VARs that can be identified using restrictions on the contemporaneous interactions between the variables. Drawing on Christiano, Eichenbaum and Evans (1999), we motivate the imposition of information restrictions that retain a high degree of simultaneity in the DSGE but allow the successful identification of shocks to monetary policy. Section 4 begins by documenting the empirical success of the VAR-based approach given a valid identification scheme and adequate data, under both strong and weak structural transmission. Section 5 quantifies the effects on inference of various sources of weak transmission, some of which are plausibly related to the structure of LIC economies and others to the characteristics of monetary policy regimes themselves.

The core of the paper then lies in sections 6-9, where we develop Monte Carlo evidence on the performance of VAR estimates, under research conditions that are arguably characteristic of LICs, conditional on a specific lag and information structure that is appropriately identified by the researcher. We address in turn the implications of short data samples (Section 6), volatility and measurement error (section 7), and high-frequency supply shocks (Section 8). In Section 9, we consider the combined effects of some of these conditions.

Section 10 offers a cautionary detour. It is well-understood that correct identification is critical to reliable inference about the monetary transmission mechanism. When the environment in which the central bank operates and its mode of operation are poorly understood—as is perhaps particularly the case in most LICs, both because of the opacity of the regimes and the scarcity of research—identification is especially challenging, and estimates of the MTM can go badly wrong.

Section 11 concludes the paper with a summary of findings and a discussion of possible extensions and policy implications.

## 2. DSGEs as a data-generating process

The MTM is about the ability of monetary policy to exert a temporary effect on aggregate demand.<sup>5</sup> To focus on these effects we begin by ignoring stochastic trends in the data, implicitly assuming that these can be estimated with reasonable statistical confidence so that the stationary part of the data is cleanly isolated. Our DSGE models will therefore generate a stationary vector  $x_t = [\tilde{y}_t, \pi_t, \tilde{e}_t, i_t]'$  of quarterly

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<sup>5</sup> Weak and unreliable transmission in the short run is, of course, perfectly consistent with monetary policy providing an effective long-run anchor for inflation.

values for the GDP gap ( $\tilde{y}_t$ , defined as the gap between actual GDP and unobservable potential GDP), the inflation rate ( $\pi_t$ ), the real exchange rate ( $\tilde{e}_t$ , with an increase being a real appreciation), and the annualized nominal interest rate ( $i_t$ ). We introduce an underlying trend in section 8, where we argue that LIC applications confront particular difficulties in inferring the GDP gap from observed measures of output. But for the bulk of the paper we treat the model-generated GDP gap as observable.

The four endogenous variables in the model will in turn be functions of a vector  $\varepsilon_t = [\varepsilon_t^y, \varepsilon_t^\pi, \varepsilon_t^e, \varepsilon_t^i]'$  of structural shocks that are not directly observable by the researcher. The objects of interest to the researcher are the responses of  $x_{t+j}$  to a one-time unit-value shock to monetary policy ( $\Delta\varepsilon_t^i = 1$ ). To estimate these, the researcher starts by estimating a reduced-form VAR of the form

$$x_t = A(L)x_{t-1} + u_t, \quad (1)$$

where  $A(L)$  contains enough lags to render the reduced-form innovations  $u_t$  approximately white noise. In the absence of measurement error or inappropriate truncation, this produces consistent estimates of the lag parameters in  $A(L)$  and the covariance matrix  $\Omega$  of the reduced-form innovations.<sup>6</sup> The researcher then imposes enough restrictions on the reduced form to identify the structural shocks to monetary policy. In our case, these take the form of zero restrictions on elements of the square and invertible matrix  $B$  in<sup>7</sup>

$$u_t = B\varepsilon_t. \quad (2)$$

Conditional on identification, the impulse responses (IRs) can then be calculated as nonlinear functions of the estimated lag parameters and reduced-form shock covariances. The researcher computes these estimated IRs and, in a final step, bootstraps their standard errors and calculates  $t$  ratios for each impulse-response step. When a ‘true’ MTM is present in the data-generating process, the researcher should see impulse responses that are appropriately signed and shaped, of roughly correct magnitude, and of reasonable statistical significance. We loop over multiple simulated datasets in order to study the population distribution of estimated impulse responses, the associated  $t$ -ratios, and the power of the  $t$ -ratio test in a wide variety of specific environments.

<sup>6</sup> The VAR representation of the DSGE solution may be infinite-order; see below.

<sup>7</sup> In practice, we estimate the elements of  $A(L)$  and  $B$  simultaneously using Bayesian methods. In the just-identified case the results are extremely close to what we obtain by estimating  $A(L)$  by OLS in (1) and then solving for  $B$  using  $\hat{\Omega} = B(B^{-1})'$ , where  $\hat{\Omega}$  is the estimated covariance matrix of the OLS residuals.

The model we employ for our experiments is a canonical New Keynesian open-economy model that combines an IS curve, a New Keynesian Phillips curve, an interest-parity condition, and a Taylor Rule for monetary policy (e.g., Berg, Karam and Laxton 2006). There is no empirical consensus on the appropriate parameterization of such a model for LICs, but in choosing parameters we can draw on recent research that develops partly-calibrated and partly-estimated DSGEs for low-income countries in Africa. We rely particularly on Berg, Portillo and Unsal (2010), who develop DSGEs with similar 4-equation structure for Kenya, Tanzania and Uganda, and Andrieu *et al.* (2013) who estimate a somewhat more disaggregated DSGE for Kenya. Where the relevant parameters differ sharply across these two sources, we choose midpoint or close-to-midpoint values. Our basic model, complete with parameters, is:

**IS equation:**

$$\tilde{y}_t = 0.5 \cdot E_t[\tilde{y}_{t+1}] + 0.5 \cdot \tilde{y}_{t-1} - 0.2 \cdot [0.5 \cdot (i_t - E_t[\pi_{t+1}] - \bar{r}) + 0.5 \cdot \tilde{\epsilon}_t] + \varepsilon_t^y, \quad (3)$$

**New Keynesian Phillips curve:**

$$\pi_t = 0.5 \cdot E_t[\pi_{t+1}] + 0.5 \cdot \pi_{t-1} + 0.15 \cdot \tilde{y}_t - 0.15 \cdot \tilde{\epsilon}_t + \varepsilon_t^\pi, \quad (4)$$

**Uncovered interest parity equation:**

$$\tilde{\epsilon}_t = 0.5 \cdot E_t[\tilde{\epsilon}_{t+1}] + 0.5 \cdot \tilde{\epsilon}_{t-1} + (1/4) \cdot [i_t - E_t[\pi_{t+1}] - \bar{r}^*] + \varepsilon_t^e, \quad (5)$$

**Taylor-type rule for monetary policy:**

$$i_t = 0.5 \cdot (\bar{r} + 1.4 \cdot E_t[\pi_{t+1}] + 0.5 \cdot E_t[\tilde{y}_{t+1}]) + 0.5 \cdot i_{t-1} + \varepsilon_t^i, \quad (6)$$

**Structural shocks:**

$$\varepsilon_t \sim i.i.d. N(0, I_4) \quad (7)$$

Here  $E_t$  denotes an expectation conditional on information available at time  $t$ . As explained below, we allow information sets to vary across equations, reflecting differences in the information available to agents. Note also that equation (7) departs from the bulk of the DSGE literature by assuming *i.i.d.* shocks, in preference to the standard AR(1) structure: our version allows for distributed lag responses similar to those in the literature, but these are governed completely by the lags within the behavioral equations. By eliminating purely exogenous dynamics, we substantially simplify the task of solving the DSGE and representing its solution as a structural VAR, although in principle the VAR could be conditioned on strongly exogenous variables such as world oil prices.



The model we are employing was of course not developed for LICs, and in characterizing the MTM it makes no effort to capture the financial architecture or other ‘facts on the ground’ that may differentiate LICs from the advanced countries for which these models were developed. For most of the analysis, monetary policy follows a Taylor-style rule, even though many LICs use the monetary base and other measures rather than a policy interest rate as the main operational instrument.<sup>8</sup> In Section 10 we briefly consider the case where the authorities’ true reaction function gives weight to deviations in money aggregates from target, but where this reaction function may not be correctly identified by the econometrician. We also omit a banking sector from the model, even though the nature of the credit channel may differ in LICs as compared to more advanced countries. Finally, we simplify by assuming that the structural shocks are mutually uncorrelated. This is standard when building a DSGE up from micro-foundations, but cross-correlations can emerge when the behavioral relationships are ‘solved down’ to the four we feature here, and we have omitted these.<sup>9</sup> These simplifications reflect our focus on aspects of the research environment that are largely model-independent. Our Monte Carlo approach can of course be applied to *any* structural model, a topic to which we return in the concluding section.<sup>10</sup>

Our data-generating process will not be the DSGE model itself, but rather its solution in terms of the endogenous state variables and the shock vector  $\varepsilon_t$ . This introduces a set of technical issues that are well understood in the DSGE and VAR literatures but that appear here in combination. First, for VAR-based methods to have a chance of uncovering the features of the MTM, the solution to the DSGE must be representable, at least approximately, as a finite-order VAR in observable variables. The relevant conditions are developed in Fernandez-Villanueva *et al.* (2005). As we show below, our model solutions have *exact* representations as finite-order VARs (a side effect of which is to eliminate truncation bias from our results). Second, the monetary policy shocks must be identifiable through the imposition of conventional structural-VAR restrictions on this representation. We focus on short-run

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<sup>8</sup> Whether de jure money-targeting central banks can be approximated by a Taylor rule is an open question. In practice, money-targeting LICs frequently miss their operating base money targets by substantial margins; see IMF (2015, appendix 2). Other operating regimes can be readily introduced. For example, Benes *et al.* (2008) introduce foreign exchange intervention in a similar simple set-up, while Andrle *et al.* (2013) introduces money targeting. Other LIC-critical elements such as food prices are introduced in Andrle *et al.* (2015).

<sup>9</sup> These last two points are of course potentially related. Whether differences in the transmission mechanism would be manifest in different coefficient values in the same simple model, correlated “structural” shocks in this simple model, or other differences in an open question. Baldini *et al.* (2015) incorporate a banking sector into a somewhat more complex version of this model and apply to LICs. In that framework, shocks to the banking system and a “sudden stop” in the capital account would imply correlated structural shocks in the model presented here.

<sup>10</sup> Although our calibration is based on the quarterly frequency, which is mostly common in the developing country VAR literature, our approach could also be applied to monthly frequencies with a different calibration. We choose a quarterly frequency because there is little output data available for most low-income countries at monthly frequencies; even quarterly series are typically short or unavailable.

restrictions, because these remain the dominant approach to identification in the applied literature reviewed by Mishra *et al.* (2012, 2013). As discussed below, such restrictions work by limiting the contemporaneous interactions between the variables in the VAR.

### 3. Motivating CEE-recursive structure

The restrictions we impose at the estimation stage are typically motivated in the structural VAR literature by appealing to a structural simultaneous equations model of the form

$$B_0 x_t = B(L)x_{t-1} + \varepsilon_t. \quad (8)$$

The shocks  $\varepsilon_t$  are *i.i.d.* and mutually uncorrelated variables that can be normalized without loss of generality to have unit variances ( $E[\varepsilon_t \varepsilon_t'] = I$ ).<sup>11</sup> As long as  $B_0$  is invertible, equation (8) implies the reduced-form VAR representation in equation (1), with  $A(L) = B_0^{-1}B(L)$ . The relationship between the structural and reduced-form innovations is then given by equation (2), with  $B = B_0^{-1}$ . When we refer to ‘short-run restrictions’ in a structural VAR context, we mean restrictions on the elements of  $B_0$ .

Within the class of short-run restrictions, the most common are those that impose a recursive structure on  $B_0$ . Cholesky decompositions assume that the model is fully recursive, so that  $B_0$  is lower triangular. As Christiano, Eichenbaum and Evans (1999) have shown, however, if the focus is on the impulse responses just to monetary-policy shocks, these can be recovered from the reduced form VAR under the considerably weaker condition that the system be *contemporaneously block-lower-triangular, with the interest rate occupying its own diagonal block*. We will therefore refer to any system that can be ordered into two or more block-recursive segments, with the interest rate occupying its own diagonal block, as ‘*CEE-recursive*’. Note that in this case it is immaterial whether the zero restrictions are motivated with reference to  $B_0$  or  $B$ , because matrix inversion preserves the block-triangular structure.

Equation (9) illustrates the concept of CEE recursiveness in a seven-variable case, using ‘ $X$ ’ as a placeholder for any element of  $B_0$  that is not restricted to be zero.

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<sup>11</sup> If the structural shocks have covariance matrix  $\Lambda$ , then the model can be written  $\Lambda^{-1/2}C x_t = \Lambda^{-1/2}D(L)x_{t-1} + \varepsilon_t$ , where  $E[\varepsilon_t \varepsilon_t'] = I$ , and  $B = C^{-1}\Lambda^{1/2}$ . A one-unit shock to  $\varepsilon_t^i$  is then equivalent to one standard deviation of the structural shock to monetary policy

$$B_0 x_t = \begin{bmatrix} X & X & X & 0 & 0 & 0 & 0 \\ X & X & X & 0 & 0 & 0 & 0 \\ X & X & X & 0 & 0 & 0 & 0 \\ X & X & X & X & 0 & 0 & 0 \\ X & X & X & X & X & X & X \\ X & X & X & X & X & X & X \\ X & X & X & X & X & X & X \end{bmatrix} \begin{pmatrix} x_{1t} \\ x_{2t} \\ x_{3t} \\ i_t \\ x_{5t} \\ x_{6t} \\ x_{7t} \end{pmatrix}. \quad (9)$$

When a structural VAR model is CEE-recursive, the impulse responses to monetary policy shocks can be recovered from the reduced-form VAR even if the remaining impulse responses cannot.<sup>12</sup> For the same reason, the ordering of variables within each of the recursively prior and posterior blocks is irrelevant to obtaining the responses to interest-rate shocks (Christiano, Eichenbaum and Evans 1999).

The restrictions in (9) are typically motivated by behavioral lags and information assumptions. The upper block in (9) is occupied by a three-variable system that is recursively prior to the interest rate and also to the three-variable system in the third block. The monetary authority is assumed to observe these variables contemporaneously and respond to them (the information assumption), but this set of variables responds to the interest rate and the remaining variables in the model only with a lag (the behavioral lag assumption). The third block in (9), in contrast, is occupied by variables that are recursively posterior to the interest rate and its determinants. Monetary policy and its determinants affect these variables immediately, but either they are not observed by the monetary authority except with a lag, or their contemporaneous values are excluded from the authorities' policy reaction function for other reasons.<sup>13</sup>

A glance at equations (3) – (7) confirms that the solution to our DSGE will not exhibit the block-recursive property under full information. Instead, it will tend to be highly simultaneous. This is in part because monetary policy is assumed to affect all endogenous variables contemporaneously, so the interest rate does not occupy its own diagonal block. However, it also reflects the role of expectation variables, since any endogenous variable that is in the information set of a particular class of agents will contemporaneously affect all of the endogenous variables that are influenced by the forecasts or 'nowcasts' formulated by those agents. Since all of the equations in our model contain such expectation

<sup>12</sup> Note that the  $B_0$  matrix in (9) is six restrictions short of being lower triangular. The impulse responses to the monetary policy shock can nonetheless be recovered through any Cholesky decomposition that places the interest rate in its proper position in the ordering.

<sup>13</sup> The example in (9) happens to be symmetric in the sense that the upper and lower blocks are of identical size and configuration. This is useful for expositional purposes below, but the only conditions that matter are that the interest rate occupy its own diagonal block and that the overall structure be block recursive.

variables, under full information *all* of the model's endogenous variables would tend to appear in every equation.

We therefore have to impose additional restrictions on our DSGE in order to produce a data-generating process that is identifiable via short-run restrictions. For simplicity, we rely only on the types of informational asymmetries that have played a key role in the applied VAR literature, rather than on behavioral lags. That is, we retain the simultaneity of the structural model and obtain exclusion restrictions through assumptions about the information sets available to the private sector and the central bank.

Partial information, and particularly *mixed information*, where different agents have different information sets, complicates the solution of DSGE models. As explained in the Appendix, we solve these models using a version of the undetermined coefficients approach developed by Christiano (2002). The solutions have exact VAR(1) representations with CEE-recursive  $B$  matrices. For most of the paper, we focus on a model in which the interest rate is first in the recursive ordering so that it affects all other variables contemporaneously: this model then corresponds to the lower sub-system defined by  $[i_t, x_{5t}, x_{6t}, x_{7t}]'$  in equation (9). In this case, the private sector has full information, but the central bank can only observe the endogenous variables with a lag. This structure can be rationalized as follows: the central bank (strictly its monetary policy committee) sets the systematic part of the policy interest rate at the beginning of the period, before any shocks arrive. Shocks then hit the system and are observed by the private sector and the monetary policy committee. The private sector can react immediately but the central bank cannot do so until the beginning of the next period (i.e. the next MPC meeting). The central bank is, therefore, setting the systematic part of its policy on the basis of  $t-1$  information, while the private sector is behaving on the basis of time- $t$  information. We suggest this structure may have greater plausibility in a LIC context, where the central bank has less access to timely information on the state of the economy, than in higher-income countries.<sup>14</sup> To denote the informational advantage of the private sector, we refer to this block-recursive identification strategy as 'CEE-PS'.

For robustness, we also examine an alternative strategy in which, the informational advantage accrues to the central bank. It sets the interest rate with full information, but the interest rate does not affect the model's other endogenous variables contemporaneously, not because of a behavioral lag, but because the private sector does not observe the shock to monetary policy contemporaneously, so it

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<sup>14</sup> This assumption makes substantial sense for output and to a lesser extent for inflation, but much less so for the exchange rate, as discussed below.

reacts to its *forecast* of the time- $t$  interest rate based on information dated at time  $t-1$ . This structure, which corresponds to the sub-system defined by  $[x_{1t}, x_{2t}, x_{3t}, i_t]'$  in equation (9) follows Christiano, Eichenbaum and Evans (2005), and reflects the common practice in the advanced-country VAR literature of attributing an information advantage to the central bank. We refer to this identification strategy as 'CEE-CB'.

These identification strategies still fall foul of the serious challenge to recursively-identified VARs in an open-economy context posed by Kim and Roubini (2000). As they point out, the interest rate cannot occupy its own diagonal block unless the central bank does not respond contemporaneously to the current exchange rate (in our preferred CEE-PS formulation) or if the exchange rate does not respond to the current interest rate (in the CEE-CB formulation). Absent either of these two conditions, the interest rate and exchange rate are simultaneously determined regardless of the recursive structure of the remainder of the model. This is handled within the structural VAR literature by appealing to non-recursive short-run restrictions, sometimes in combination with theoretically motivated long-run restrictions (e.g., that monetary policy has no long-run impact on real variables). Non-recursive identification is a possible area for extensions of our analysis, and we return to it in the concluding section.

#### 4. Strong VAR performance under baseline conditions

Figure 1 reports the performance of a validly identified CEE- recursive VAR using 40 years of quarterly data. The experiment assumes equal variances for the four structural shocks, and the information structure is CEE-PS. The researcher estimates the VAR with four lags.<sup>15</sup> To focus on parameters of interest we report only the impulse response functions for the monetary policy shock. Since the components of Figures like 1a and 1b will appear throughout the paper, we begin by describing their content.

The researcher is trying to uncover the true, model-based impulse responses, which appear as the bold lines identified by dots in Figure 1a. As shown in the figure, these IRFs display the conventional hump-shaped responses of the real exchange rate, inflation and output to a monetary contraction. On impact, a 100 basis point increase in the interest rate leads to a one percent contraction in inflation and a reduction in the output gap by around 0.7% of GDP, values that are broadly in line with Christiano *et al*

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<sup>15</sup> Given the true model's VAR(1) structure, assuming the researcher estimates a VAR(4) model leads to some loss of efficiency. In principle it would be straightforward to embed a data-driven choice of lag length, but we leave this for future work. We can show, however, that the loss of efficiency is not large (results available on request).

(2005). To examine whether VAR methods can uncover these responses, we generate 1,000 data samples from our model, based on independent simulations of the DSGE solution, each generated by 40 quarters of independent draws on the shock vector  $\varepsilon_t$ . For each data sample, a researcher estimates a VAR and constructs IRs by imposing the CEE-PS identifying restrictions. The empirical performance of these IRs is summarized by the three lighter lines in Figure 1a. These lines show the 5<sup>th</sup>, 50<sup>th</sup>, and 95<sup>th</sup> percentiles of the population distribution of simulated point estimates for the impulse responses (with percentiles computed separately for each impulse-response step).<sup>16</sup>

Figure 1b describes the econometrician's inference environment. For each of the 1,000 simulations, the researcher computes the VAR coefficients and standard errors using conventional Bayesian estimation methods, with the standard errors computed by a Gibbs Sampler algorithm with 1,000 draws (see Waggoner and Zha, 2003). The figure shows the probability of rejecting the (incorrect) null hypothesis of a zero impulse-response coefficient at each step. We assume that the researcher treats the  $t$  ratios as asymptotically normal and applies the relatively undemanding hurdle of 10 percent significance.

Figures 1a and 1b establish that with appropriate identification and in the presence of ample and high-quality data, the VAR methodology does very well at uncovering strong monetary transmission when it is present. The estimated impulse responses for output and inflation show only a trivial degree of small-sample attenuation at the median, and for the first few quarters fully 90 percent or more of the point estimates lie on the correct side of zero.<sup>17</sup>

The researcher's own inference will of course frequently be less confident than suggested by Figure 1b, because the researcher has only one data sample. Figure 1c reflects this by showing the full distribution of  $t$  ratios across the 1,000 runs. The structure of the exercise suggests that the width of bootstrapped confidence intervals for the IR coefficients will not be far from that implied by the population distribution of impulse responses, and the comparison of Figures 1a and 1c bears this out. When one end of the population distribution of IRs is close to zero in Figure 1a, roughly half of the  $t$  statistics reported in Figure 1c (corresponding loosely to the point estimates that lie closer to zero than

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<sup>16</sup> The line corresponding to the median response is virtually invisible in the figure because it overlaps almost exactly with the true response in each case.

<sup>17</sup> The VAR residuals  $B\varepsilon_t$  are correlated with later-dated values of  $x_{t+j}$ , violating the assumption required for unbiasedness of OLS. In simple autoregressive models, this produces attenuation of OLS lag coefficients towards zero, to a greater degree the more persistent the dynamics. See Favero (2001).

the median) fail to reject the null.<sup>18</sup> Thus the researcher will tend to reject the null only about half the time.<sup>19</sup>

Figure 2 shows the results of the alternative CEE-CB experiment, where the central bank has the information advantage and the model solution places the interest rate last. To keep the presentation of results manageable we reproduce, on the top row, the impulse response plots for output and inflation only and the corresponding power plots on the bottom row of the figure. The model parameters are identical in the two cases, but there is a substantial difference in the true impulse responses, with the MTM being much weaker in this case (compare the top row of Figure 2 with Figure 1a, noting the difference in the vertical scales). The difference is driven by the effect of lags in diluting the impact of a monetary policy change in the CEE-CB case. The VAR results are somewhat weaker in this case: though the median estimated IRF continues to track the true IRs generated by the model very closely, the 5<sup>th</sup> and 95<sup>th</sup> percentiles of the estimated IRFs are now more widely dispersed relative to the median, and there is a correspondingly substantial loss of power. The deterioration in the inference environment relative to Figures 1a and 1b reflects the unfavorable effect on inference of smaller true effect sizes.

## 5. Low power to detect weak transmission

The weaker MTM just described arose from an alternative information structure, with unchanged model parameters. To explore further the implications for inference of a weak MTM, we now consider the impact on VAR-based inference of small true effects driven by model parameters, rather than by the information environment. To do so, we return to the CEE-PS information structure as the baseline. Even within a tightly-parameterized DSGE, there are many parameters that may differ substantially between LIC and higher-income applications. The private-sector block incorporates both an interest-rate channel that operates through the IS curve and an exchange-rate channel that branches off from the interest parity condition to the IS and Phillips curves, while the monetary policy rule incorporates feedback from both inflation and the GDP gap along with a parameter that governs the degree of interest-rate smoothing. Based on Mishra *et al.* (2012, 2013), we focus here on two simple experiments. In Figure 3, we scale down the transmission elasticities in the IS and Phillips curves by a

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<sup>18</sup> The inference plots are very consistent with this effect. This suggests that the bootstrapped standard errors calculated by the researcher on each run of data tend to closely approximate the spread of the population distribution.

<sup>19</sup> To see this concretely, consider the estimate for the effect of  $i$  on inflation at  $t=5$ . The 10<sup>th</sup> percentile for the point estimate of this effect is about 0; i.e. the upper red line crosses the zero axis at about  $t=5$  in Figure 1a. The  $t$ -statistics are centered at around 1.96 (Figure 1c). Meanwhile, the power to reject the null is about 30 percent (Figure 1b).

uniform 75 percent relative to the baseline model, and in Figure 4 we leave the transmission elasticities untouched but reduce the lag parameter in the monetary-policy rule by 75 percent.

Figure 3 shows the impact of uniformly low interest-rate and exchange-rate elasticities in the private-sector block. The (new) true IRs in this case are shown by the heavy solid line. As before, the lighter lines show the 5<sup>th</sup>, 50<sup>th</sup>, and 95<sup>th</sup> percentiles of the population distribution of simulated point estimates. For reference, the true IRs with the original model parameters are retained in the figure in the form of the heavy line with dots. As expected, the change in the parameters weakens the effect of monetary policy on aggregate demand, which shows up in the form of smaller impacts on the GDP gap and the inflation rate. The true MTM is particularly weakened with respect to its effects on real activity. Notably, however, the estimated impulse responses continue to show very strong fidelity at the median, with the median IRs corresponding very closely to the true ones. Comparing Figure 3 and 1a, there is also no discernible impact on the spread of estimated impulse responses. To a first approximation, therefore, the impact of weak transmission elasticities operates exclusively through the impact of small true effect sizes on the power of *t*-ratio tests against the null hypothesis of no effect. That impact is substantial, however, with the scope for confident inference cut roughly in half (bottom row of Figure 3).<sup>20</sup>

In New Keynesian models such as (3)-(6), the strength of transmission depends not only on spending elasticities, but also on a transmission channel that may differ sharply between LICs and higher-income countries. Mishra, Montiel, and Spilimbergo (2012) find that the correlation between short-term interest rates and lending rates tends to become progressively weaker at lower levels of development. While equations (3) – (6) do not directly incorporate a lending channel, the IS curve and interest-parity condition can be solved forward to express the levels of the current GDP gap and real exchange rate gap as functions of current and expected future short-term interest rates. As emphasized by Woodford (2001), monetary policy shocks affect the ‘tilt’ of the spending and real exchange rate gaps via the short-term interest rate, but they alter the equilibrium *level* of these variables only to the degree that they change current long-term rates. The pass-through of short rates to long rates is in turn governed both by the parameters of the private sector block and, very importantly, by the degree of

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<sup>20</sup> The exchange-rate channel is quantitatively important in our model. In simulations not reported here, we show that if it is only the interest-rate elasticity that differs between LIC and non-LIC applications, and not the exchange rate elasticity, the deterioration in the inference about the MTM is mild.



interest-rate smoothing implemented by the central bank.<sup>21</sup> To investigate the role of the latter, in Figure 4, we leave the transmission elasticities unchanged and reduce the smoothing parameter in the monetary policy rule by 75 percent. Monetary policy shocks now pass through much more weakly into long rates and spending.

These results underscore the leverage of interest-rate smoothing in the New Keynesian model. The true impulse responses (shown by the solid line in bold) decline slightly more sharply than when the transmission elasticities are reduced by the same proportion, but the overall shapes are virtually identical. Consistent with our previous results, there is minimal evidence of bias in the impulse responses: the very weak true IRs are faithfully reproduced by the median estimated IRs. Not surprisingly, there is now virtually no scope to reject the null hypothesis of zero monetary policy effects.

The overall impression from Figures 1 to 4, then, gives weight to a pure ‘facts on the ground’ interpretation of the ‘missing MTM’ puzzle. Given a valid block-recursive identification scheme and sufficient data, structural VARs identified through short-run restrictions do well at uncovering the true MTM—whether it is strong or weak—in the strict sense that the median estimated IRs track those generated by the true MTM very closely. At the same time, the population dispersion in estimated IRs is not substantially affected by the strength of the underlying “true” MTM. Therefore, the weaker the true MTM, the harder it is for the data to reject the null of no response—i.e., the weaker the power of tests of the null hypothesis that the MTM is entirely missing. Even where a (plausibly weak) MTM exists, the researcher armed with only one data set, even a pristine one that is 40 years long, may well conclude that it is missing.

## **6. Small sample sizes generate low precision**

We now turn to the challenges posed for the VAR researcher by the specific research environment that characterizes LICs. We begin with short data samples, which are surely among the most daunting if perhaps mundane constraints in the LIC research environment. Throughout, we focus on the CEE-PS case where the research imposes the correct identification structure. Except as noted, we assume the underlying strong transmission mechanism of the baseline, presented in section 4.

Structural economic reforms (e.g., financial liberalization) and changes in the monetary policy regime (e.g., a move to a flexible exchange rate, a very different smoothing parameter for interest rates, or a much different role for monetary aggregates) are often of sufficiently recent vintage that the

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<sup>21</sup> Our simple model does not incorporate a separate bank lending channel, which would introduce an additional potential source of weak transmission related to imperfect competition and/or high intermediation costs in the banking sector of low-income economies (Mishra *et al.* 2012, 2013)

researcher cannot expect that the current data-generating process has been in place for very long.<sup>22</sup> Data-collection limitations also undermine the availability of long data samples in LICs; quarterly data on the real economy, for example, may be unavailable well through the 1990s or even more recently.<sup>23</sup>

Figure 5 shows an experiment that is identical to the CEE-PS baseline with the exception that the researcher has 10 years rather than 40 years of quarterly data. As in the case of more abundant data, the basic shape of the true impulse responses is strongly reproduced, although the median estimated impulse response is discernibly attenuated over the first half-dozen periods relative to Figure 1.<sup>24</sup> In addition, the reduction in sample size produces a substantial widening in the population distribution of IR point estimates and, along with the attenuation, a sharp deterioration in the scope for confident inference about the MTM. As shown in lower half of Figure 5, the power of statistical tests for the IRs to reject the null of no monetary policy effect based on bootstrapped standard errors is 30 to 50 percent smaller with 10 years of data (solid line) than with 40 years of data (bold line with dots, which reproduces the lower half of Figure 1).

The upshot is that estimation of the strength of the MTM in LICs with VAR methods is likely to produce wide confidence bands around estimated IRs when the available span of data is limited. In combination with the somewhat attenuated point estimates of the strength of policy transmission, judgments about unreliability based on the width of confidence bands around estimated IRs are unwarranted when data samples are as short as they often tend to be in LIC applications. In this context, wide confidence bands around estimated IRs should be interpreted as uncertainty about the estimates rather than as confirmation of on-the-ground instability in policy effects.

## **7. Volatility does not undermine VAR-based inference, but measurement error does**

Macroeconomic variables display greater volatility at business-cycle frequencies in LICs than in higher-income countries. One dimension of variability is of course crucial to uncovering the MTM in a VAR context: even a perfectly-specified VAR will fail if the variance of monetary policy shocks is small enough relative to that of other shocks. We abstract from differences in the relative variability of

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<sup>22</sup> See Berg et al. (2015) and IMF (2015).

<sup>23</sup> Only fourteen of the seventy low-income countries in IMF databases have any quarterly GDP data: Burundi (9 years long through 2014), Cambodia (6 years), Ghana (9), Honduras (15), Kenya (14), Kyrgyz Republic (14), Malawi (13), Maldives (3), Moldova (24), Nicaragua (9), Rwanda (7), Solomon Islands (1), Tanzania (13), and Uganda (9). And these numbers may overstate the relevant lengths given major regime changes, such as Uganda, Ghana, and Kenya's switch from money targeting to some form of IT and Malawi's interment exchange rate pegs.

<sup>24</sup> As mentioned in footnote 17, this attenuation is consistent with the small-sample downward bias characteristic of VARs.

interest rates. Rather, we focus on two drivers of data variability that clearly differ systematically between developing and advanced countries. We show that *true economic volatility* and *measurement error* have sharply different impacts on VAR-based inference about the MTM. To a first approximation, economic volatility leaves VAR-based inference unchanged, while measurement error rapidly undermines it.

The intuition for the neutral effect of volatility can be illustrated by considering the stochastic-regressor model  $y_t = \beta x_t + \varepsilon_t$ , where  $x_t$  and  $\varepsilon_t$  are mutually uncorrelated with mean zero. Let  $b_T$  be the OLS estimator of  $\beta$  in a sample of size  $T$ . This estimator is consistent, and converges in distribution to a normal random variable:

$$\sqrt{T}(b_T - \beta) \xrightarrow{L} N(0, \sigma_\varepsilon^2 / \text{Var}[x_t]). \quad (10)$$

The precision of the OLS estimator in any finite sample therefore depends approximately on the two variances on the right-hand side. These variances have the familiar effects: volatility in the disturbance term undermines inference about  $\beta$ , while volatility in the independent variable enhances inference.<sup>25</sup>

A similar expression characterizes the OLS estimator in the AR(1) model  $x_t = \alpha x_{t-1} + \varepsilon_t$ , viewed here as the simplest possible stationary VAR ( $\varepsilon_t$  is white noise with variance  $\sigma_\varepsilon^2$  and finite higher-order moments, and  $|\alpha| < 1$ ):

$$\sqrt{T}(a_T - \alpha) \xrightarrow{L} N(0, \sigma_\varepsilon^2 / \text{Var}[x_{t-1}]) = N(0, 1 - \alpha^2). \quad (11)$$

In contrast to the conventional stochastic-regressor case, therefore, the variance of  $x_t$  in a VAR model is a function of the variance of the shocks. Thus there is only one source of underlying volatility in a VAR model, which is the volatility of the shocks. After solving for  $\text{Var}[x_{t-1}]$  from the moving average representation, the limiting distribution in (11) is completely independent of  $\sigma_\varepsilon^2$  (Hamilton, 1994).

Any factor that uniformly scales up the variances of the shocks in a VAR therefore has little effect on inference about the VAR coefficients, because the sampling variances of the estimated coefficients are simultaneously pushed upwards by the variances of the shocks and downwards by the

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<sup>25</sup> These statements about inference can be re-cast more properly in terms of the power of  $t$  ratios against the null hypothesis  $\alpha = 0$ . The relevant  $t$  ratios take the form  $t_T^0 = a_T / \sqrt{s_T^2 / \sum x_{t-1}^2}$ , where  $s_T$  is the OLS estimator of  $\sigma_\varepsilon^2$ . The  $t$  ratios are not asymptotically normal except under the null, but when the null is false the ratio of variances that appear on the right-hand side of (10) or (11) serve as a shift factor that reduces the power of the test for any finite  $T$ .

variances of the lagged variables in the VAR. These effects cancel, leaving the finite-sample variances of VAR coefficients approximately invariant to the variances of the structural shocks. This property carries over to the variances of the IR coefficients, because the latter are continuous (though nonlinear) functions of the VAR coefficients. A straightforward Monte Carlo experiment confirms this effect: doubling the variances of all four shocks in our DSGE model has no discernible effect on the population distributions of either the impulse responses or the  $t$  ratios (results not shown).

Measurement error, in contrast, unambiguously undermines the accuracy of VAR estimates. Figure 6 quantifies this effect for the simple case of classical measurement error that affects the researcher but not the agents in the model.<sup>26</sup> For this and subsequent experiments, we return to the section 4 baseline with a full sample of 40 years of quarterly data, again with CEE-PS identification. We induce classical measurement error in the GDP gap and inflation (but not the real exchange rate), by adding white noise to the model-generated values of these variables, with a variance that is equal to 20 percent of the variance of the structural shocks.<sup>27</sup> As shown in the top row, the point estimates show attenuation towards zero in the first two quarters (the estimated effects are slightly more than half of the true effects) for the GDP gap, but little impact in later quarters. Attenuation effects are larger and more persistent on inflation, with such effects persisting through the first two quarters. In both cases, there is little effect on the dispersion of estimated IRs but the power of  $t$ -ratio tests deteriorates badly during the quarters when attenuation bias is most pronounced.

## **8. High-frequency supply shocks obscure transmission to output, but not to other variables**

We have been assuming that the GDP gap is observable. In reality, the gap must be inferred—even when actual output is measured without error—by developing an empirical proxy for potential

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<sup>26</sup> To contemplate measurement error is to open a can of worms when considering not just the VAR but the underlying economy, as captured by the DSGE model. To assess the impact of measurement error more fully, a natural approach might be to confront the agents in the model with the same measurement error faced by the econometrician. This seems likely both to weaken the true MTM as well as to undermine the ability of VAR methods to discover it, including by inducing infinite lags in the DSGE solution and therefore exposing the VAR results to truncation bias. It remains to be established whether such bias would tend to overstate or understate the strength of the MTM.

<sup>27</sup> Reliable estimates on the scale of measurement error in low-income countries are rare but there is reason to believe that our value of 20 percent is on the conservative end of the spectrum (see, for example, Jerven, 2013, and Ley and Misch, 2014). If for example we assume that measurement errors are 100 percent of the variance of the structural shocks, the power of  $t$ -ratio tests deteriorates to only about 20 to 40 percent for output gap and inflation (results not shown). Ley and Misch examine the deviation between final estimates of annual real GDP in year  $t$  (made at year  $[t+5]$ ) and those made by IMF staff in the spring of year  $t+1$ , as a proxy for measurement error. They find that the variance of this deviation is about double in LICs relative to OECD countries (in both cases excluding resource-rich countries). Of course, this is an estimate of measurement error in the level of output, not the output gap, but it seems unlikely that this would reduce the relative size of LIC errors.

GDP. In this section we show that the high-frequency real-side shocks that are characteristic of LICs exacerbate measurement error in the GDP gap. When we follow typical practice and use a one-sided filter to measure the gap, this effect substantially weakens inference about the real side of the monetary transmission mechanism. The transmission to inflation remains surprisingly robust, however.

The GDP gap is typically measured in empirical applications by assuming that potential GDP follows a slow-moving trend. This trend is then either extracted from the actual series by differencing or filtering (leaving a stationary gap variable that can be used in the VAR) or controlled for within the VAR by expanding the set of variables to include slow-moving proxies for aggregate supply (e.g., a deterministic trend or the economy-wide capital stock and labor force). As emphasized in the literature on the New Keynesian Phillips curve, however, the concept of potential GDP that matters for inflation dynamics is the natural or ‘flex-price equilibrium’ level of GDP. Natural GDP is a function of slow-moving processes like factor accumulation and technological change, but it also depends on transitory supply-side shocks that can affect output in the absence of sticky prices. Such shocks may play a greater role in determining natural GDP in LICs than in high-income countries. Droughts, for example, are likely to have larger effects on natural GDP in countries with larger agricultural sectors. Supply-side shocks may also be more important in countries with less diversified non-agricultural sectors. If this is indeed the case, then the practice of proxying potential GDP with a slow-moving trend will induce more serious measurement errors in LICs than elsewhere.

To formalize this idea and assess its impact on VAR-based inference, we rely on the fact that the DSGE specifies the stationary interactions of macroeconomic variables once stochastic trends have been removed. This leaves us free to construct the path of actual output as the sum of our model-generated GDP gap (now denoted  $\tilde{y}_t^M$ ) and a new, exogenous stochastic process for natural GDP,  $y_t^n$ :

$$y_t = y_t^n + \tilde{y}_t^M. \quad (12)$$

In each of our 1,000 simulation runs, therefore, we construct output from the two components on the right-hand side of equation (12). The researcher receives the vector  $[i_t, \tilde{e}_t, y_t, \pi_t]'$ , which includes observable output  $y_t$  rather than the unobservable gap  $\tilde{y}_t^M$ , and then approximates the gap by applying a one-sided Hodrick-Prescott (HP) filter to actual GDP, with the standard quarterly smoothing parameter

of 1600. The VAR is then estimated on  $[i_t, \tilde{e}_t, \tilde{y}_t^R, \pi_t]'$ , where the researcher's GDP gap,  $\tilde{y}_t^R$ , is the HP-cycle in actual GDP.<sup>28</sup>

To implement this approach, we require a measure of natural GDP. We assume that natural GDP consists of two components: a stochastic trend  $y_t^{nT}$  that follows an integrated random walk with deterministic drift, and a stationary component  $y_t^{nS}$  that is present only in a LIC environment. Thus

$$y_t^n = y_t^{nT} + y_t^{nS}, \quad \Delta y_t^{nT} = \bar{g} + \varepsilon_t^{nT}, \quad y_t^{nS} = \varepsilon_t^{nS}, \quad (13)$$

where  $\bar{g} = 0.02$ ,  $\varepsilon_t^{nT}$  is a white noise stochastic shock and  $\varepsilon_t^{nS}$  a mutually uncorrelated stationary AR(1) process, and where the variance of  $\varepsilon_t^{nS}$  is zero outside of a LIC environment. To justify the use of the smoothing parameter of 1600, we calibrate the variance of  $\varepsilon_t^{nT}$  to be  $1/1600^{\text{th}}$  of that of the model-based gap and in the LIC environment the variance of the supply shocks is defined as

$$\text{var}(\varepsilon_t^{nS}) = \text{var}(\varepsilon_t^{nT})/(1 - \rho^2)$$

where  $\rho$  is the autoregressive parameter of the AR(1) process.<sup>29</sup>

The researcher in these experiments estimates  $\tilde{y}_t^M$  as the HP cycle in actual GDP,  $y_t$ . This induces some degree of measurement error in the GDP gap even when there is no high-frequency component of natural GDP.<sup>30</sup> To benchmark this 'unavoidable' deterioration, we first show results for a hypothetical case where the GDP gap is unobservable but estimated—under optimal conditions for a one-sided filter—by the researcher but where we assume that the true variance of  $\varepsilon_t^{nS}$  is zero. This is shown in Figure 7 which suggests that the impact on inference is confined to the GDP responses which

<sup>28</sup> Short-term supply shocks of the sort considered here also may manifest themselves in temporary shocks to the price level. Thus, an alternate modeling strategy would be to include an additional shock with a negative moving-average component in the Phillips curve. We do not pursue this here. However, it seems likely that this would reduce the ability of a finite-order VAR to correctly capture the DGP.

<sup>29</sup> The optimal smoothing parameter for the HP filter in the case of an integrated stochastic trend and a white noise cycle is given by the ratio of the two variances. This property is of course not precisely relevant in either of our cases: in Figure 7, the cyclical component of actual output is the model-based gap, which is not white noise, while in Figure 8, in addition, the transitory supply shock represents a deviation from the standard HP case. The researcher, in our environments, is not aware of these details of the data-generating process and follows the standard practice of applying the HP filter to remove the trend in measured GDP.

<sup>30</sup> The HP filter yields  $y_t = y_t^{HPT} + \tilde{y}_t^R$  where HPT denotes the Hodrick-Prescott trend. Combining this with the equation used to construct  $y_t$ , we can see that  $\tilde{y}_t^R = \tilde{y}_t^M + [y_t^n - y_t^{HPT}]$ . The measurement error in  $\tilde{y}_t^R$  is therefore the difference between two highly persistent series. Estimating the gap in this way tends to produce a measurement error that is persistent, even in the 'best' of circumstances, in which actual GDP is measured without error.

show modest initial attenuation towards zero. The  $t$  ratios are not very strongly affected. These results suggest that despite unavoidable measurement error, considerable scope remains for confident and qualitatively accurate inference about the transmission of monetary policy to output, at least for the impact effect and the early steps of the process. Inference about inflation is even less affected.

This conclusion, however, depends strongly on the absence of transitory shocks to potential GDP. Figure 8 illustrates the case where there are transitory supply shocks with variance equal to that of the true model-based GDP gap.<sup>31</sup> The researcher proceeds as before, but since natural GDP now includes a high-frequency component, the HP filter over-smooths the GDP series and thereby exacerbates the measurement error in the GDP gap. Inference deteriorates correspondingly, so that Figure 8 portrays a transmission mechanism that is both weak and unreliable with respect to the (true) GDP gap, slightly more so if there is persistence in the measurement error.

## 9. Assembling the pieces

We have so far analyzed factors one at a time: some salient features of the transmission mechanism itself ('facts on the ground'), and a set of LIC-specific features of the data and environment that may weaken the power of VARs to detect transmission that exists, taken one at a time. Of course these problems do not come in isolation. In this section, we get a feel of how these various factors work together. First, we combine several of the most plausible data/environmental factors to see what happens when the transmission mechanism is strong (as in Figure 1) but data series are only 10 years long, there is 20 percent measurement error, and there are i.i.d shocks to potential output that complicate estimation of the output gap. As is shown in Figure 9, in this case the VAR estimates are substantially biased downwards, with the median estimated impulse response of output to an interest rate shock at about half of its true value. Indeed for inflation the median estimated impact effect of a monetary policy contraction is zero. And uncertainty is high; the VAR has very little power (only about one third of what we had in the baseline) to extract the MTM. Finally, Figure 10 in addition reduces the smoothing parameter in the reaction function by half (rather than the 75 percent in section 5). Now, the true MTM is even more strongly attenuated, along the lines of earlier results, and the VAR has only about 20 percent power to reject the null.

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<sup>31</sup> In Figure 8 we assume white noise shocks. Results are similar where they follow an AR(1) process with for example  $\rho = 0.9$ .

## 10. Identification may be especially tricky in LICs

We have maintained the assumption of correct identification. That incorrect identification dooms a VAR is not LIC-specific or surprising, of course. However, there tends to be no general consensus on the nature of behavioral lags and information sets in the LIC environment, and central banks in LICs tend in general to be far less transparent than those in high-income countries (making the nature of the monetary policy rule less evident).<sup>32</sup> Thus, identification of monetary policy shocks is likely to prove far more difficult in the LIC environment than in the more familiar and transparent environment of high-income countries.

In this section, we consider two types of relatively straightforward – and not unlikely – identification errors. First, we examine the case in which the researcher places the interest rate too late in the recursive structure of the model. If our CEE-PS ordering has any special plausibility in low-income applications, this error might be a natural one for a researcher trained in the advanced-country literature. The researcher in Figure 11 assumes a CEE-CB information structure when the true structure is in fact CEE-PS. The solid line with dots in bold represents the true CEE-PS impulse responses (reproduced directly from Figure 1) while the light lines once again represent the 5<sup>th</sup>, 50<sup>th</sup>, and 95<sup>th</sup> percentiles of the estimated IRFs when the data are generated by CEE-PS but the researcher mistakenly imposes CEE-CB identification.

The result of this error is sufficient to produce impulse responses that are ‘weak and unreliable’ in the extreme: they are essentially zero, both economically and statistically. At the median, they closely approximate the relatively weak shapes of the impulse responses that would have been generated by a CEE-CB structure, even though the true responses are the much stronger ones generated by CEE-PS. However, the dispersion of the estimated IRFs is dramatically wider than was observed when CEE-CB was in fact the correct identification (compare Figure 11 with Figure 2). Not surprisingly, statistical tests based on bootstrapped standard errors for the estimated IRs will have essentially no power to reject the null of zero monetary policy effects in this case (Figure 11), even though the true effects are in fact extremely powerful.

The second case we consider is one where the authorities optimally update their policy interest rate on the basis of the growth of money aggregates relative to target *but where this additional information on money growth is not exploited by the researcher*. This setting is described in Berg *et al* (2010) where money aggregates, which are essentially observed in real time, are systematically related to expected output and inflation through the private sector’s demand for money. *Ex ante*, there is

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<sup>32</sup> See IMF (2015) for a discussion.



therefore an exact equivalence between any given interest rate rule and a corresponding money target. This equivalence does not hold when the economy is subject to shocks, including to money demand, so that the authorities' optimal policy rule entails gives weight to both deviations of the interest rate from target and money from its target. How much weight is placed on money in the policy rule will depend on the volatility of money demand shocks relative to real shocks and on the interest elasticity of the demand money. When money demand is highly volatile and the interest elasticity is high target the optimal weight placed on money should be low, and vice versa.

To operationalize this idea, we augment our baseline model by introducing the nominal money target into the Taylor rule defined in equation (6), recalling that the inflation and nominal money growth targets are both zero. First, consider a pure money targeting rule, in which the authorities allow interest rates to move so as to achieve the desired growth rate of money, assumed 0 for simplicity. We can start with a conventional money demand equation for the change in money,  $\Delta m_t$  :

$$\Delta m_t - \pi_t = \Delta y_t - \theta \Delta i_t + \Delta \varepsilon_t^d. \quad (14)$$

The associated interest rate that sets money growth to the target,  $i_t^M$ , is then

$$i_t^M = \frac{1}{\theta} (\pi_t + \Delta y_t + \Delta \varepsilon_t^d) - i_{t-1}. \quad (15)$$

Following Andrieu et al. (2013), we can define a hybrid rule as follows:

$$\lambda(i_t - i_t^T) + (1 - \lambda)(i_t - i_t^M) = \varepsilon_t^i, \quad (16)$$

where  $i_t^T$  is the interest rate target defined in equation (6) (excluding the monetary policy shock in that equation, which is now explicit in equation (16)). The parameter  $\lambda$  defines the weight placed on the interest rate rule. When  $\lambda = 1$ , the result is a standard Taylor-type rule as we discuss before; when  $\lambda = 0$ , policy is defined in terms of a target (0 in this case) for the growth rate of money, and the interest rate is a residual in that it follows from the authorities' efforts to hit their money target. In many "money targeting" countries, it is plausible to think that  $\lambda$  lies somewhere in the middle.<sup>33</sup>

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<sup>33</sup> See IMF (2008 and 2014) on money targeting in practice. Many LICs have de jure money targeting frameworks, in which monetary aggregates play a role as operating and/or intermediate targets. In these frameworks, the

Solving equation (16) for the interest rate yields a hybrid rule:

$$i_t = \lambda i_t^T + (1 - \lambda) i_t^M + \varepsilon_t^i \quad (17)$$

Replacing  $i_t^T$  with equation (6) and  $i_t^M$  with equation (15), we recover an implicit interest rate rule of the form:

$$i_t = \psi_0 + \psi_i i_{t-1} + \psi_{y,t-1} y_{t-1} + \psi_{y,t} y_t + \psi_{\pi,t} \pi_t + \psi_{\pi} E_t[\pi_{t+1}] + \psi_y E_t[\tilde{y}_{t+1}] + \varepsilon_t^m, \quad (18)$$

where, critically,  $\varepsilon_t^m$  is a composite of the money supply and money demand shocks, and the appearance of  $y_t$  and  $\pi_t$  implies that all shocks that matter for these variables, notably contemporaneous aggregate demand and supply shocks, also affect the interest rate contemporaneously.

With the true model now defined by equations (3)- (5) and (7) and (14) plus (6), (15), and (17) (or (18)), the researcher armed with the four-variable vector of data  $x_t = [i_t, \tilde{e}_t, y_t, \pi_t]'$ , will only correctly identify the monetary policy shock if  $\lambda = 1$ . For any  $\lambda < 1$ , however, where money growth provides the central bank with information on the evolution of inflation and the output gap, the researcher is unable to decompose the composite error term in (18) so as to cleanly identify the monetary policy shock.

Figure 12 shows the outcome of applying our standard four-variable VAR with the same recursive identification to data produced by a model with hybrid monetary policy, where  $\lambda = 0.95$ , i.e. where in the model the authorities place only a small weight on money deviations from target. As the figure shows, even this seemingly minor deviation strongly attenuates the median estimated impulse responses and greatly reduces power relative to the baseline.<sup>34</sup>

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money aggregate targets are typically set three to six months in advance, and deviations from these targets are frequent. One interpretation is that the central banks have the typical inflation and output objectives of inflation targeting (IT)-type countries but for various reasons (imperfect credibility, threat of fiscal dominance, traditional IMF programming practices) maintains intermediate money targets. It has been useful to model them as hybrid rules according to which the central bank places some weight on the achievement of the money target per se and some on its broader inflation and output objectives (Berg, Portillo and Unsal 2010; Andrieu et al 2013).

<sup>34</sup> It need not be the case that this misspecification strongly attenuates the impulse responses towards zero. Depending on the relative frequency of the different structural shocks and the value of  $\lambda$  (and other parameter values), different outcomes are possible. For example with  $\lambda = 0.95$ , and all structural shocks having equal variances and we have been assuming, the estimated impulse responses are generally the opposite of the true ones.

In both the case of the alternative information structure (implying a different recursive ordering) and the case of hybrid policy rules involving money, the nature of the impulse response functions might lead the researcher to re-think their estimation strategy (rather than conclude that transmission is weak, for example). In the first case, experimenting with alternative recursive orderings would help. In the second case, however, it would not. Even with the full five-variable vector, neither recursive identification strategy can isolate the monetary policy shock in its own block.

Imposing alternative identification strategies might help in the hybrid rule case.<sup>35</sup> However, this solution is far from trivial. First, our conventional specification of money demand is very limited. In the DSGE tradition, money demand may also depend on expected inflation and other variables.<sup>36</sup> In this situation, no contemporaneous zero restrictions in the money demand equation are available to identify the monetary shock. More generally, getting identification restrictions right would require being precise about the details of a quite complex policy framework. If our money-targeting example is indicative, there is not likely to be a one-size-fits-all solution to properly identify monetary shocks in more complex settings.

Taken together, the results in this section suggest that incorrect identification of monetary policy shocks is itself a prime suspect in the case of the missing MTM.

## 11. Conclusions

In an effort to come to grips with the “missing MTM” in the empirical literature on LICs, we have reversed the standard dialogue between DSGEs and VARs. In the standard dialogue, VAR-based impulse responses provide an empirical standard against which DSGEs or other theory-based models can be evaluated. We have instead used DSGEs as a data-generating process, in order to ask a question about the validity of VAR-based impulse responses. If a strong MTM is present in the data, can standard VAR methods uncover it?

No parametric method will do very well if it mis-specifies the data-generating process. This is the basis of Sims’s critique of structural econometric modeling, and as long as the data are generated by a stable but unknown data-generating process, this critique favors the use of as few structural restrictions as possible to identify the MTM. VARs identified via short-run restrictions are very widely used in the literature on LICs, and perhaps even more intensively there than elsewhere given the

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<sup>35</sup> For example Boughton and Tavlas (1991) used instrumental variables to estimate money demand and money supply shocks via a two-step least squares method. See also Sims and Zha (2006).

<sup>36</sup> Nelson (2002) argues forcefully that money demand depends on the long interest rate, which would bring all shocks that matter for future interest rates into the money demand equation.

relative dearth of structural modeling in these countries. Within this class, we have focused on CEE-recursive VARs, which impose just enough recursive structure to identify the monetary policy impulse responses, while leaving the other responses potentially unidentified. Our off-the-shelf DSGE will not generate a solution with this property, but we have demonstrated that an otherwise-canonical DSGE is capable of doing so under mixed-information assumptions of the type often seen in the structural VAR literature.

When the VAR researcher imposes a valid identification scheme and has access to ample and high-quality data, the virtues of the VAR approach come through strongly. The LIC environment nonetheless poses a set of well-defined challenges to a strategy that ‘lets the data speak’: we investigate inappropriate identification of monetary policy shocks, short samples, volatile data, measurement error, and high-frequency shocks to natural GDP.

We find a surfeit of plausible explanations for the ‘missing MTM’. First, a weaker—but otherwise standard—MTM would be hard for a typical VAR to detect, even with 40 years of pristine data and proper identification. Point estimates of the impulse responses to monetary shocks are at most only mildly attenuated when the MTM is unusually weak. However, the power of the VARs to reject the null that the MTM is ‘missing’ is very low. Among the potential sources of weak transmission, we have emphasized two: small interest rate and exchange rate elasticities in the goods markets, possibly caused by the small size of the formal financial system and the structure of external trade in LICs, and a limited degree of interest-rate smoothing that reduces the signaling power of a given interest rate change. Some of these features would presumably be highly persistent features of the economies. Those that depend on the policy regime, notably low smoothing, could change quickly.

Second—and this is the core result of the paper—likely LIC-specific features of the estimation environment such as short time series, measurement error, and the need to estimate the output gap greatly weaken the power of the VAR methodology to reliably uncover the MTM. The small samples moderately attenuate the median point estimates. In addition, the resulting uncertainty is high. Each of these features reduces sharply the power of standard  $t$ -ratio tests against a null hypothesis of zero response, even in properly-identified structural VARs with a strong MTM. Together, they are devastating. A combination of plausibly short time series, a modicum of measurement error, and an estimated output gap with some high-frequency supply shocks almost entirely eliminates the power of a VAR, even when the underlying MTM is strong. A reduced degree of interest rate smoothing puts the nail in the coffin.

These results and conclusions emerge from a set of experiments based on our CEE-PS identification which, as we noted in Section 4, generated much larger true effect sizes than in the alternative CEE-CB model. Although not reported here, we find that controlling for the much weaker baseline inference in the CEE-CB case, the *qualitative* effects of each dimension of the LIC environment are similar in the CEE-CB case to what we have observed under the CEE-PS baseline. By implication, of course, since the strength of the MTM is quantitatively much weaker under CEE-CB, much weaker pathologies are required in the CEE-CB case to generate a ‘missing MTM’.<sup>37</sup>

Finally, and unsurprisingly, improper identification can easily produce estimates suggesting a weak MTM, even when a strong MTM is present. Because the identification challenge is likely more severe in the LIC context than in the better-understood context of high-income countries, deficiencies in identification strategy are another prime suspect in the “case of the missing MTM.”

Different approaches to identification may be worth pursuing in some cases. There is a paucity of information on which to base standard block-recursive identification of monetary policy shocks in the LIC context, and the role of exchange rates and perhaps money aggregates as well as interest rates complicates the identification challenge and renders exclusion restrictions particularly difficult to justify. A natural extension of the approach of this paper would be to acknowledge the reality that agents in the economy (central bank and private sector alike) can observe both the exchange rate and the interest rate in real time. When these two variables form a simultaneous block, the monetary policy shock will not be identifiable by imposing a block-recursive structure.<sup>38</sup> Another approach to identification of monetary policy shocks makes use of long-run restrictions, as in Mishra, Montiel, Pedroni and Spilimbergo (2014).

However, this paper has demonstrated that even when identification is proper and the MTM strong, features of the environment such as short data series and measurement error will likely make it hard to reliably hear the data speaking with a VAR approach. A potential extension to this paper would address one of the simplest questions posed by this analysis: why not use monthly data? There are two powerful reasons to do so in a LIC environment, and one potentially daunting constraint. The first

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<sup>37</sup> At the risk of further complexity, we could change the calibration of the MTM when we change the identification assumption. For example, if we want to match the main qualitative features of Christiano *et al.* (2005) for the CEE-CB case, we would want to increase the size of some of the parameters in the IS and Phillips curves. The implications of the LIC data environment would be very similar to the CEE-PS case.

<sup>38</sup> Kim and Roubini (2000) identify the monetary policy shock by imposing a non-recursive set of behavioral and information restrictions on the  $B_0$  matrix. From a DSGE perspective, however, their key exclusion restriction (that countries do not react directly to foreign interest rates) may be hard to justify. See the discussion in Faust and Rogers (2003). Sims and Zha (2006) motivate similar restrictions in a DSGE context, though in a closed-economy context.

reason for proceeding with monthly data is that ten years of monthly data yield 120 observations rather than 40. The sample information will of course increase much less than three-fold, because the data cover the same time period and the monthly model will display greater short-run persistence. But other things equal, we would expect sharper inference from the increase in sample size. In a structural VAR context, however, the more important reason for going to monthly data is that contemporaneous timing restrictions become more plausible. CEE-recursiveness is the least demanding and therefore most *a priori* plausible approach for a researcher seeking to identify the MTM via recursive structure, but it strains credulity when applied to quarterly data. By underscoring the leverage of valid identification, our results suggest that the gains in small-sample bias from better identification in monthly data may be very substantial. A major constraint is that measurement error is likely to be greater in monthly data, especially for measures of real activity. Our Monte Carlo approach is well suited to running this horse race, as a natural extension of the current paper.

Altogether, all these VAR based methods seem worth pursuing. Further country-specific analyses that carefully tailor the approach to the particular country-specific institutional framework for monetary policy may permit progress. Even in the U.S., with its unusually long, stable data series and policy regimes, economists experimented for many years before reliably generating acceptable results, only eventually solving the “liquidity puzzle” that interest rates tended to rise in response to an increase in the money supply and the “price puzzle” that inflation seemed to rise after a shock that tightened monetary policy.<sup>39</sup>

But this paper does suggest that the challenges to achieving similar success in LICs will be severe. Other empirical methods may thus be helpful. The case study approach adopted by Berg *et al* (2013) examines the implications of a large monetary policy shock identified through a narrative approach.<sup>40</sup> Abouka et al. (2015) use loan-level data to assess the bank-lending channel in Uganda. Perhaps more radically, the imposition of more economic structure and use of Bayesian techniques is a natural way to make use of available data while accepting its scarcity.<sup>41</sup>

It seems clear, despite the possible ways forward, that uncertainty about the MTM is likely to continue to face LIC central banks. Moreover, while we have shown that in appropriate identification can lead to downward biased coefficients, while various features of the data environment can produce

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<sup>39</sup> On these points, see Sims (1992), and Leeper and Gordon (1992).

<sup>40</sup> Arguably, the strongest evidence that monetary policy works in advanced countries in developed countries comes not from VARs but from the history of the Volcker disinflation and the great depression, as argued in Summers (1991).

<sup>41</sup> See for example Berg et al. (2010) and Peiris and Saxegaard (2007).

low t-statistics and substantial variation in estimates of the MTM, we have also shown that weak transmission is also a possible reason for poor VAR results.

What broader implications follow from these conclusions? One view is that, in the face of this uncertainty, monetary policy should be passive. In particular, frameworks that do not require confident knowledge of the MTM may be more appropriate, notably fixed exchange rate regimes or perhaps textbook money targeting regimes that aim to keep money growth at a constant predetermined rate.

Another perspective, however, recognizes that this uncertainty is hardly unique to LICs but is a general characteristic, particularly of countries implementing new policy frameworks, often in the face of rapid structural change or financial crises. The endogeneity of the MTM to the regime suggests that learning-by-doing is a necessary counterpart to the reform of monetary policy regimes.<sup>42</sup> Moreover, this uncertainty may justify caution, but it is not clear that it justifies inaction.<sup>43</sup> Exchange rate flexibility, for example, may play a stabilizing role even absent a clear quantitative assessment of the transmission mechanism. And textbook money targeting would generate essentially arbitrary monetary policy shocks in the face of money demand shocks. That the effect of these shocks is uncertain does not make them more desirable.<sup>44</sup> Moreover, the idea that policy action requires a precise and reliable quantitative understanding of transmission represents an excessively idealized view of the monetary policy-making process. There is a critical element of ‘tatonnement’ for all countries, including LICs: assess the state of the economy and the outlook; adjust policy if it seems too tight or too loose; then wait for new information and repeat. For this process only a view about the sign of the effects of monetary policy is required. However, clearly further empirical work is a critical component of this learning process. We hope that the results of this paper will help guide the agenda.

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<sup>42</sup> See Batini and Laxton (2007) for the case of inflation targeting in emerging markets. IMF (2015) discusses some of these issues for the case of low and lower-middle-income countries.

<sup>43</sup> The recent experience with quantitative easing in advanced countries is a (distant) case in point. Furthermore, uncertainty about the transmission mechanism is one of the main reasons that central banks in advanced countries use simple policy rules (like the Taylor rule) as benchmarks in determining the course of monetary policy (see Levin, 2014).

<sup>44</sup> Fixed exchange rate regimes of course have their benefits, and a discussion of their relative merits is outside the scope of this paper. Textbook money targeting—in which the money supply grows at a predetermined constant rate—is not a practical alternative even in LICs. In practice, LICs that follow de jure money targeting frequently miss targets by economically meaningful amounts, with positive misses associated if anything with positive deviations of interest rates from trend. Moreover, these misses are generally accommodated by adjustment of subsequent targets, rather than a policy that brings money back to the previous trend (IMF 2015). This is consistent with the view that the monetary policy volatility associated with textbook money targeting is unacceptably costly in practice.

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Figure 1(a) Impulse Responses to Monetary Policy Shock: CEE-PS Baseline

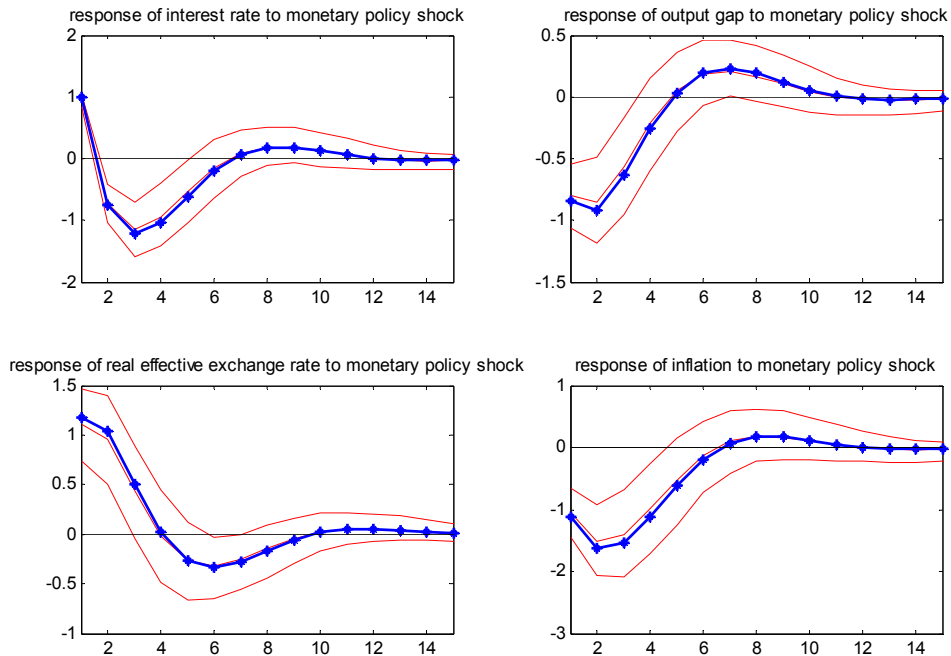


Figure 1(b) Power Functions for Monetary Policy Shock: CEE-PS

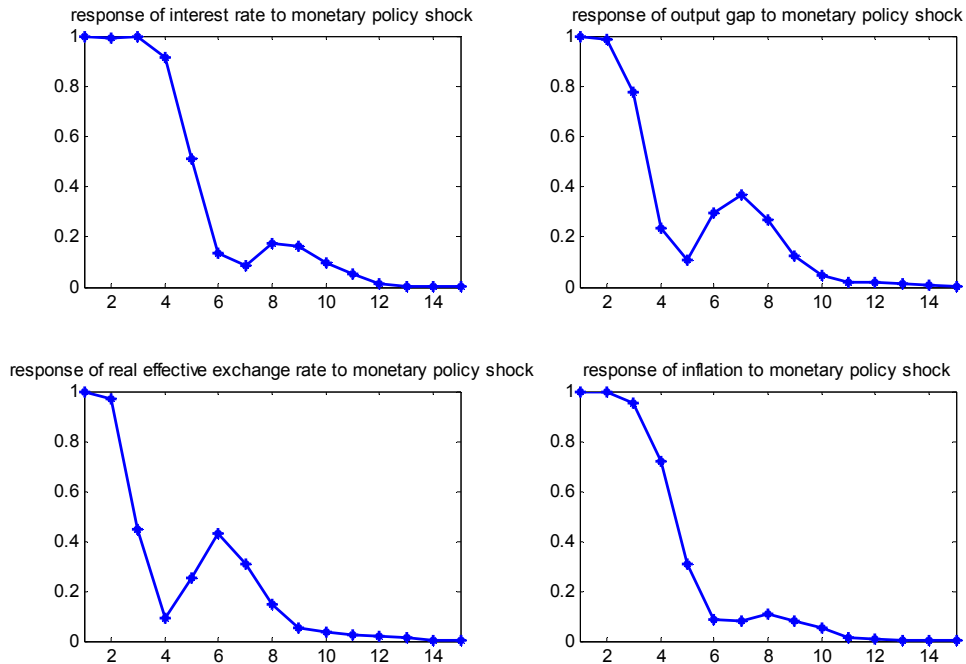


Figure 1(c) T-stat for Monetary Policy Shocks: CEE-PS

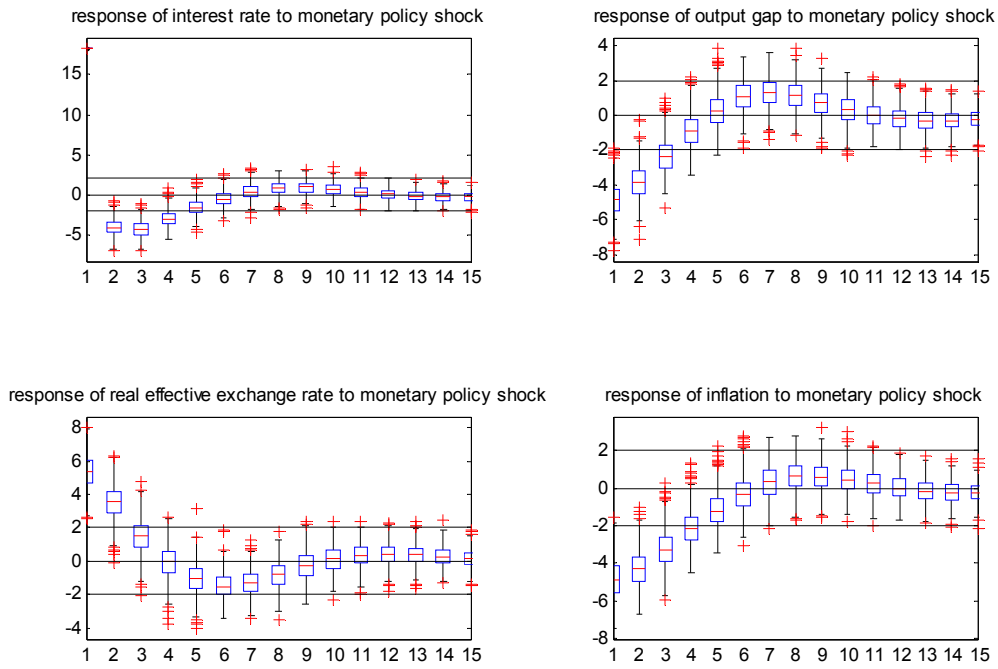


Figure 2 Impulse Responses and Power Functions: CEE-CB Baseline

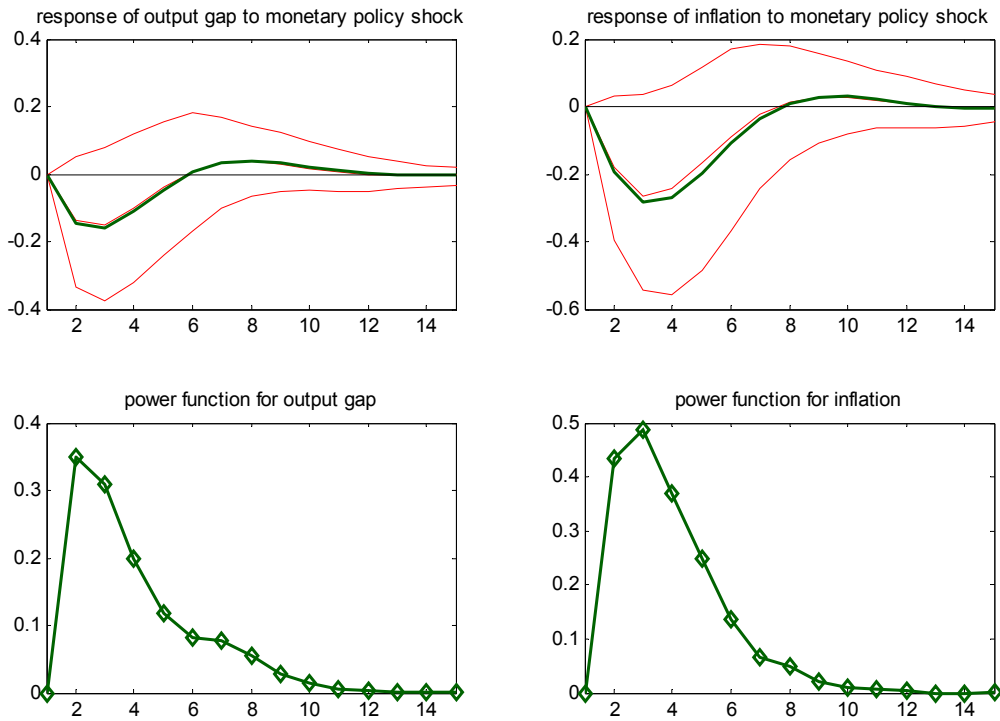


Figure 3: CEE-PS Weak Transmission (elasticities in IS and PC scaled down by 75%)

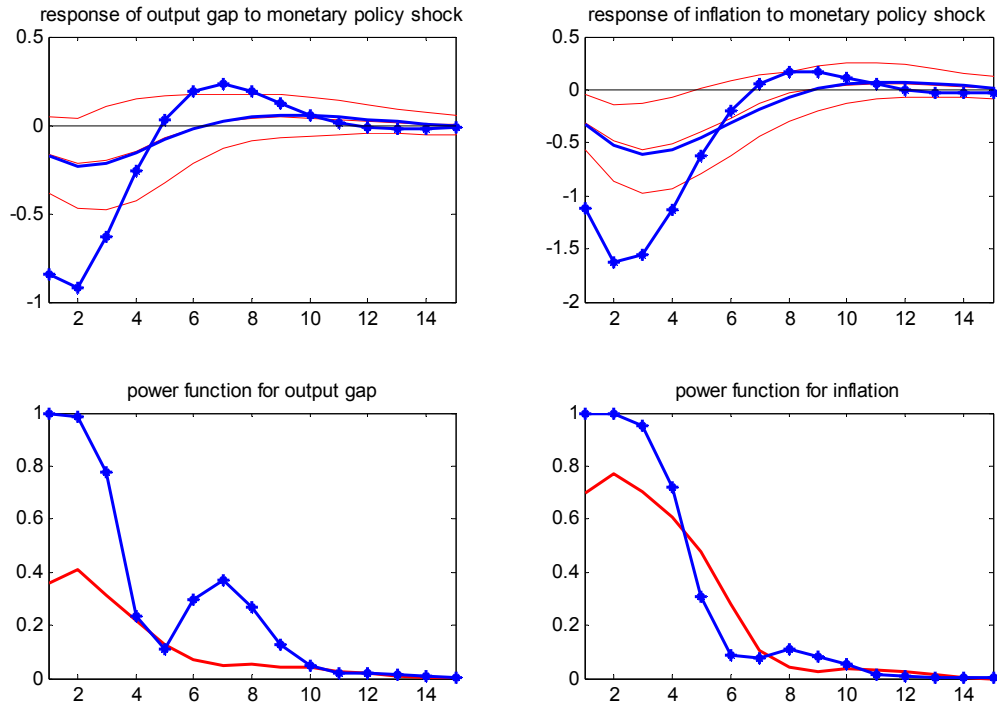


Figure 4: CEE-PS Smoothing Parameter (scaled down by 75%)

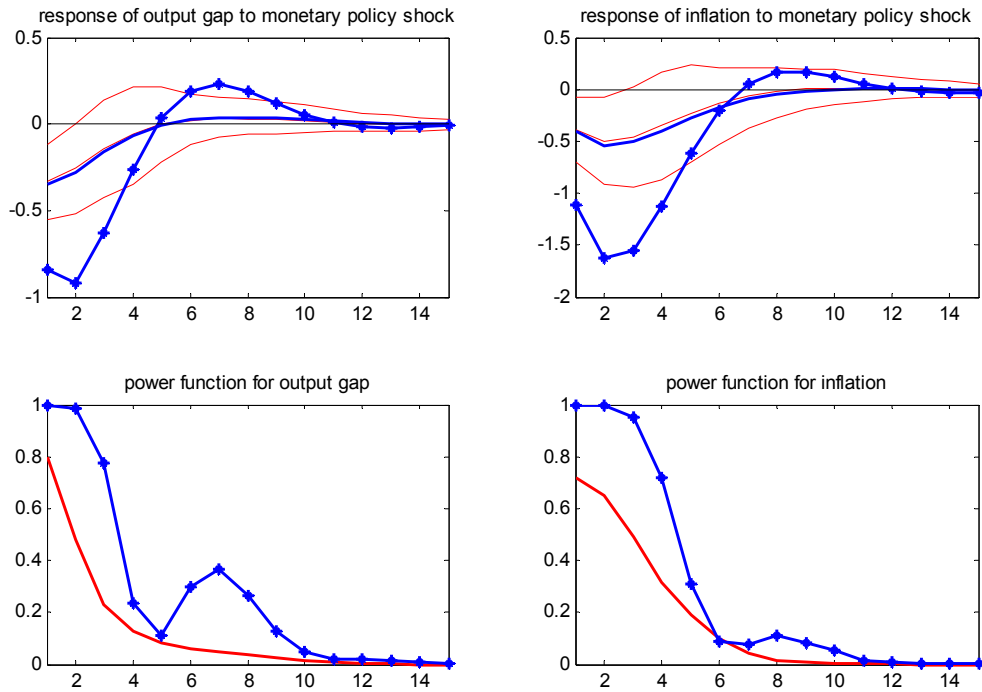


Figure 5: CEE-PS Small Sample

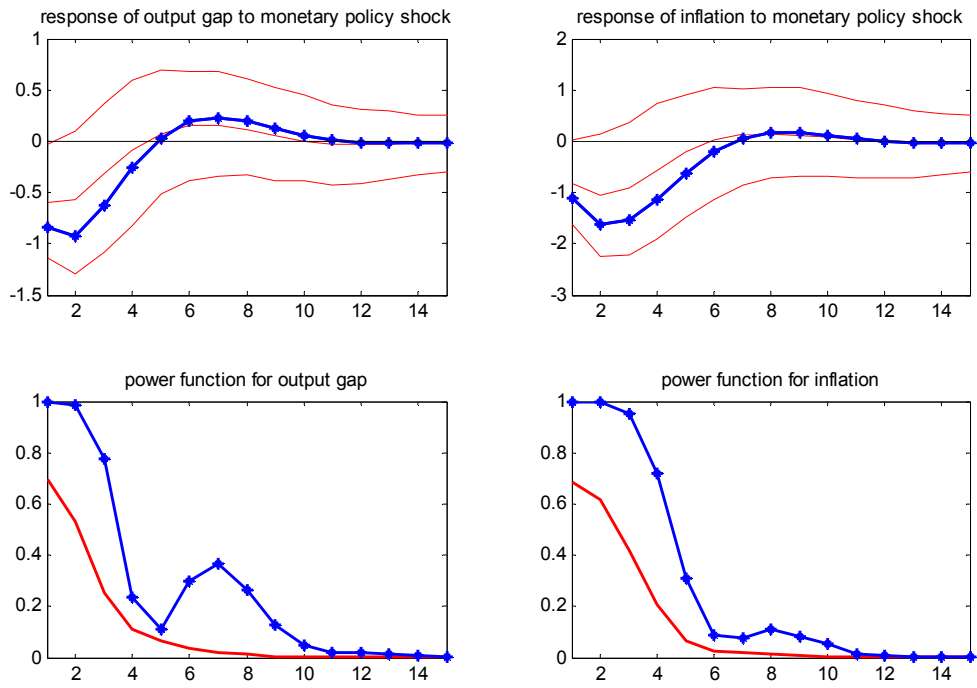


Figure 6: CEE-PS Measurement Errors  
(additional 20% variance on output gap and inflation)

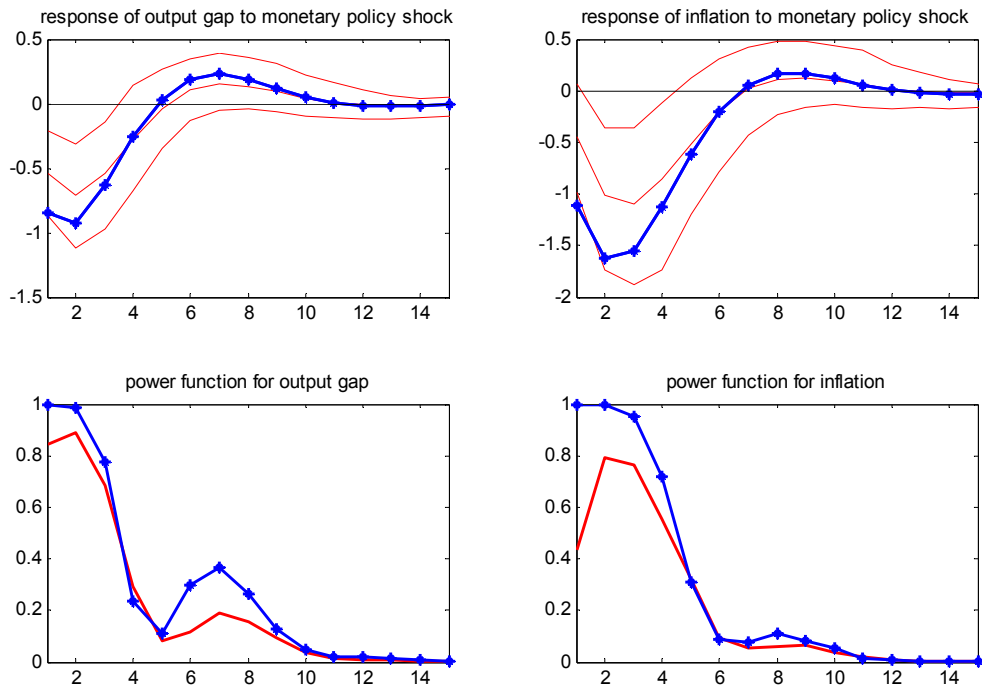


Figure 7: CEE-PS Output Gap Estimated with One-sided Filter (no supply side shocks)

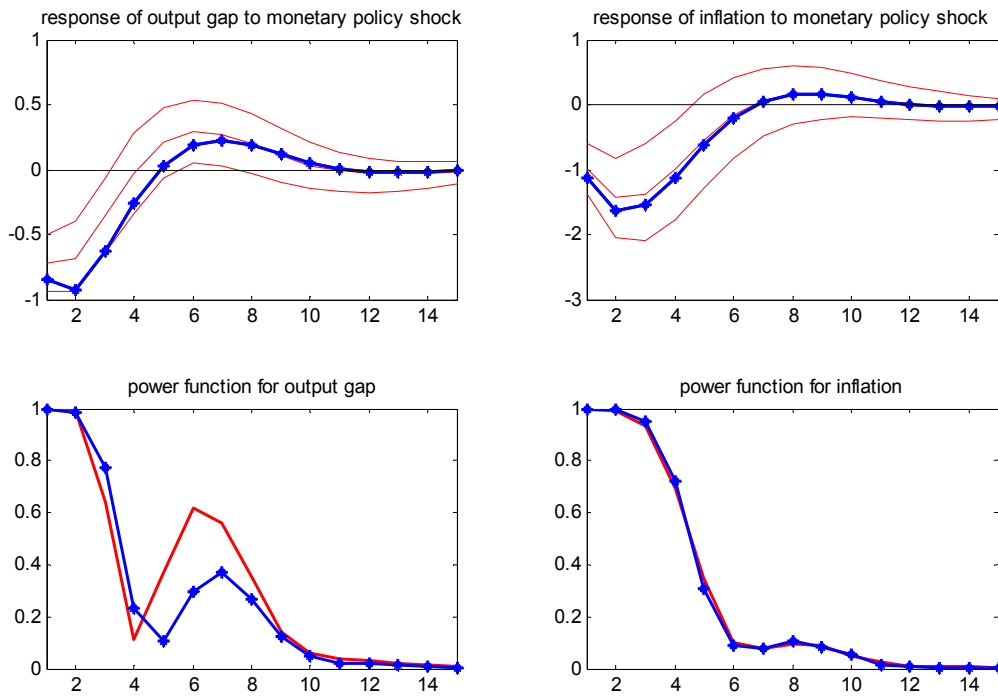


Figure 8: CEE-PS Output Gap Estimated with One-sided Filter (iid supply side shocks)

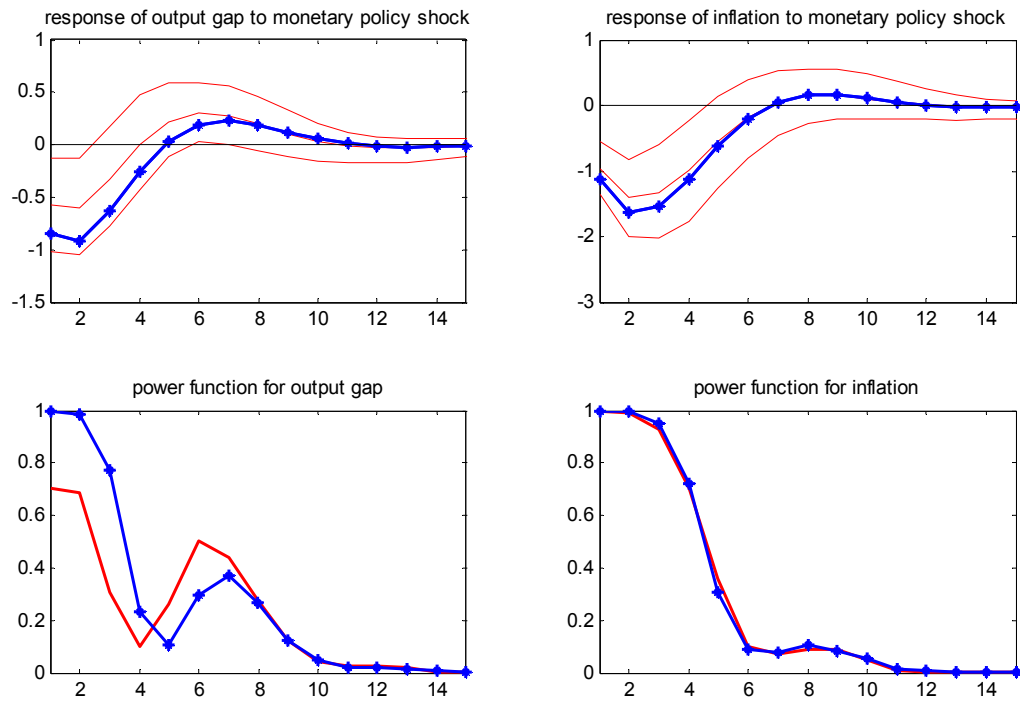


Figure 9: CEE-PS Combined Scenario 1  
 (small sample, measurement errors, and iid supply shocks)

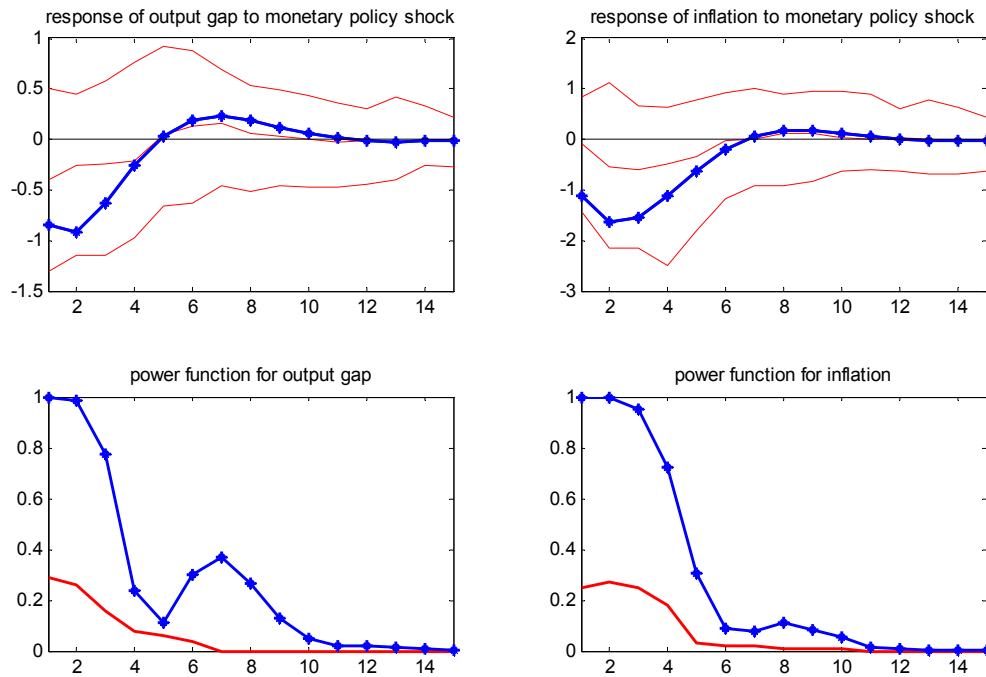


Figure 10: CEE-PS Combined Scenario 2  
 (small smoothing parameter, small sample, measurement errors, and iid supply shocks)

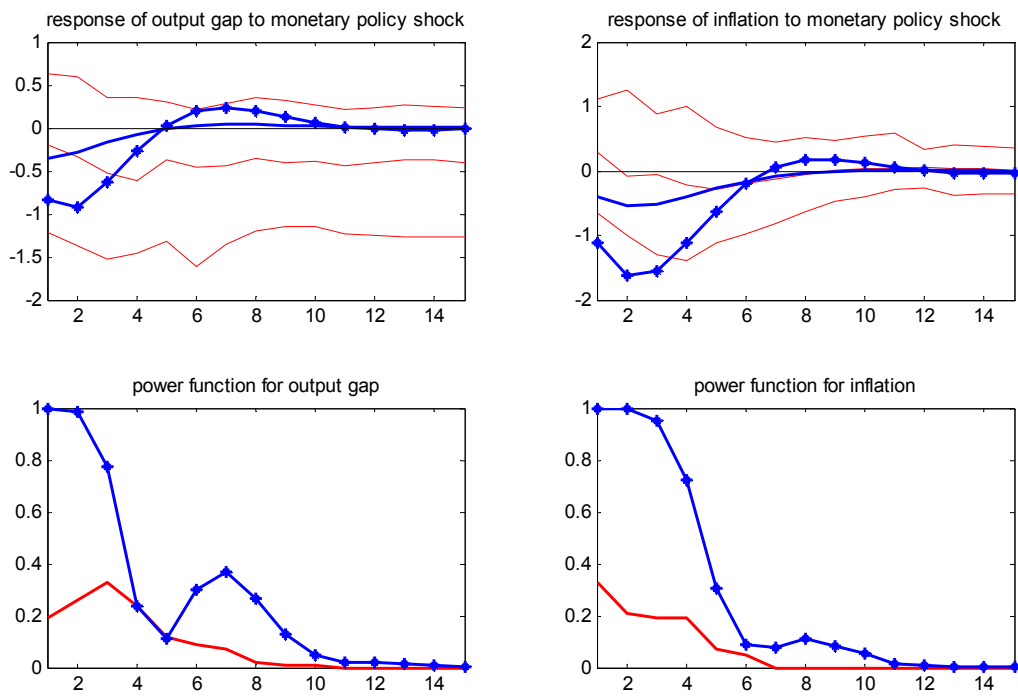




Figure 11: CEE-PS Wrong Identification

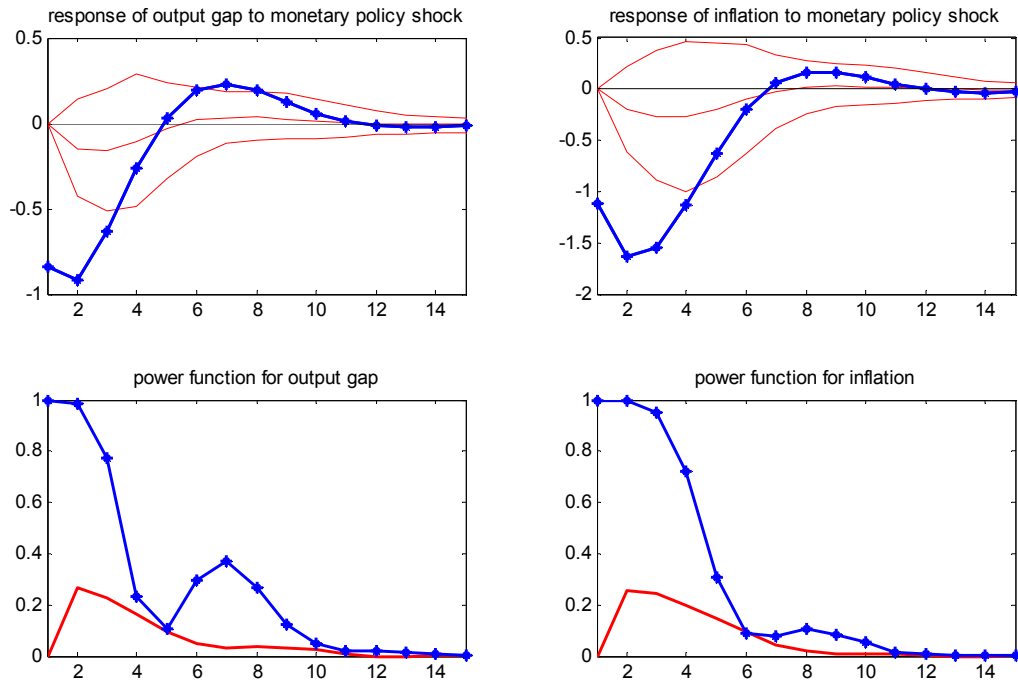
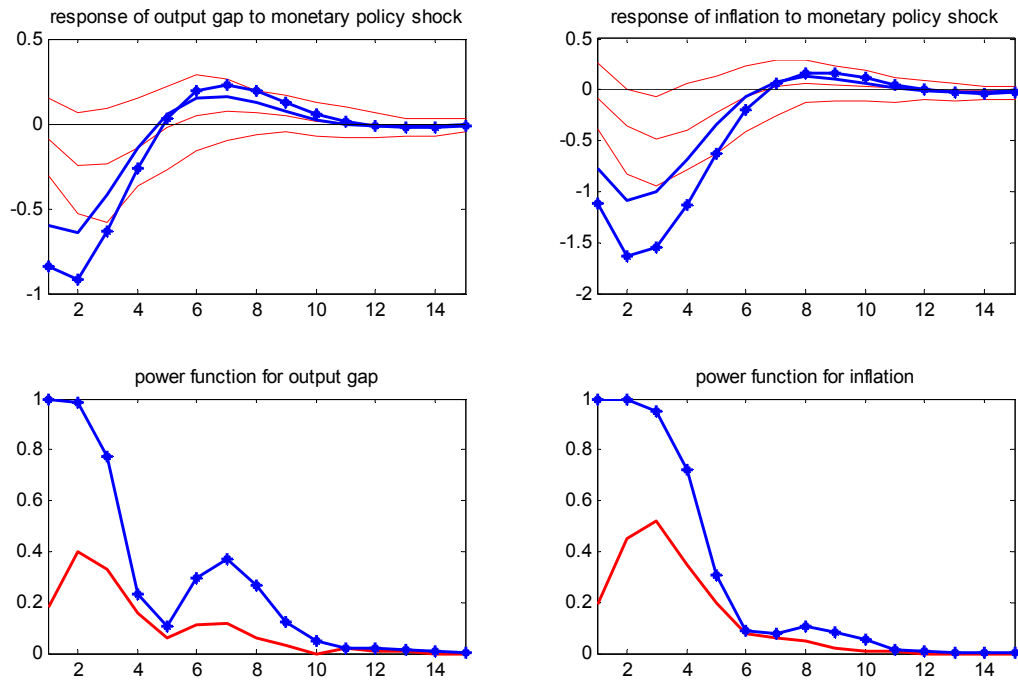


Figure 12: CEE-PS Money Target Model ( $\lambda = 0.95$ )



## APPENDIX

The solution to a DSGE does not always imply reduced-form VAR representations for vectors of observable endogenous variables, and when such representations exist they may be infinite-order (Fernandez-Villanueva *et al.* 2005). When a VAR representation does exist, moreover, there is no guarantee that the structural shocks in the DSGE will be identifiable via short-run restrictions on the VAR. This appendix provides details on the DSGE models we use in the paper and the representation of their solutions as finite-order and CEE-recursive structural VARs.

### Model Specification

As described in the text, our base model is New Keynesian open-economy model that combines an IS curve, a New Keynesian Phillips curve, an interest-parity condition, and a Taylor Rule for monetary policy (e.g., Berg, Karam and Laxton 2006). Using  $\Omega_t^{CB}$  and  $\Omega_t^{PS}$  to denote the information sets held by the central bank and the private sector at time  $t$ , the linearized and stationary form of this model can be written as

#### IS equation:

$$E[\tilde{y}_t - a_1\tilde{y}_{t+1} - (1 - a_1)\tilde{y}_{t-1} + a_2[a_3(i_t - \pi_{t+1} - \bar{r}) + a_4\tilde{e}_t] - \varepsilon_t^y | \Omega_t^{PS}] = 0 \quad (A1)$$

#### New Keynesian Phillips curve equation:

$$E[\pi_t - b_1\pi_{t+1} - (1 - b_1)\pi_{t-1} - b_2\tilde{y}_t + b_3\tilde{e}_t - \varepsilon_t^\pi | \Omega_t^{PS}] = 0 \quad (A2)$$

#### Uncovered interest parity equation:

$$E[\tilde{e}_t - c_1\tilde{e}_{t+1} - (1 - c_1)\tilde{e}_{t-1} - (1/4) \cdot [i_t - \pi_{t+1} - \bar{r}] - \varepsilon_t^e | \Omega_t^{PS}] = 0 \quad (A3)$$

#### Monetary policy equation (Taylor rule):

$$E[i_t - d_1(\bar{r} + d_2\pi_{t+1} + d_3\tilde{y}_{t+1}) - (1 - d_1)i_{t-1} - \varepsilon_t^i | \Omega_t^{CB}] = 0 \quad (A4)$$

#### Structural shocks:

$$\varepsilon_t \sim i. i. d \ N(0, \Sigma), \quad \Sigma \text{ is diagonal} \quad (A5)$$

### Mixed information 1: CEE-PS

Model solutions will depend sensitively on the assumed structure of information. Our first model assumes that the private sector has full information while the central bank has only lagged information up to the period  $t-1$ . Thus  $\Omega_t^{PS} = \{\Omega_{t-1}, \varepsilon_t^y, \varepsilon_t^\pi, \varepsilon_t^e, \varepsilon_t^i\}$  and  $\Omega_t^{CB} = \{\Omega_{t-1}\} = \{\Omega_{t-1}^{PS}\}$ , where  $\Omega_t$  (no

superscript) denotes the full information set containing all shocks and variables up to and including period  $t$ .

Our solution procedure uses the Dynare toolkit to adapt the undetermined coefficients approach to a situation of mixed information. The general form of a solution makes all endogenous variables functions of the state variables of the model, including the exogenous shocks. In our case, all four endogenous variables are state variables, given the structural lags in each equation. Our ‘guess’ solution for the model therefore takes the form

$$x_t = Ax_{t-1} + B\varepsilon_t, \quad (\text{A6})$$

for undetermined matrices  $A$  and  $B$ , where  $x_t = [\tilde{y}_t, \pi_t, \tilde{e}_t, i_t]$ .

Following our assumptions, the structural equations (A1) – (A3) are defined under full information. As the central bank only observes the lagged information available to the private sector, the structural equation (A4) is defined using only lagged variables and the expectational variables based on lagged information. To solve the model with Dynare, we create the two auxiliary variables  $v_t = E[\tilde{y}_{t+2}|\Omega_t]$  and  $z_t = E[\pi_{t+2}|\Omega_t]$ . In the monetary policy equation (A4), the central bank forecasts the output gap and inflation using lagged information up to time  $t-1$ . The auxiliary variables  $v_{t-1}$  and  $z_{t-1}$  are used in deriving the solution. The solution then proceeds by plugging (A6) into the structural equations (A1) – (A4) and solving out for  $x_t$ , using the informational assumptions to express expected values as the appropriate linear functions of past variables and currently observed shocks. Matrices  $A$  and  $B$  can then be solved by Dynare, by equating coefficients between (A6) and the derived expression for  $x_t$ .

Because the vector of interest to the VAR researcher is  $x_t$  itself, the structural VAR representation we are seeking is exactly the SVAR(1) in equation (A6). In our case, therefore, the VAR representation corresponds to the observable component of the state space representation of the solution to the DSGE.

The structure of  $B$  in the SVAR(1) representation of the solution will reflect our assumptions about the observability of contemporaneous shocks. In the CEE-PS case, our assumptions allow the GDP gap, inflation rate, and real exchange rate gap to respond contemporaneously to all four shocks, while the interest rate is capable of responding to all lagged information and to the contemporaneous monetary policy shock. Although the central bank responds to *forecasts* of inflation and the GDP gap, the structural shocks to the GDP gap, inflation and the real exchange rate gap enter the central bank’s reaction function with a lag. These assumptions give rise to the following relationship between the structural shocks and the SVAR residuals:

$$\begin{pmatrix} u_t^y \\ u_t^\pi \\ u_t^e \\ u_t^i \end{pmatrix} = \begin{bmatrix} X & X & X & X \\ X & X & X & X \\ X & X & X & X \\ 0 & 0 & 0 & X \end{bmatrix} \begin{pmatrix} \varepsilon_t^y \\ \varepsilon_t^\pi \\ \varepsilon_t^e \\ \varepsilon_t^i \end{pmatrix}. \quad (\text{A7})$$

For the parameterization reported in the text, we proceed as described above. The solution to the DSGE model augmented by the two auxiliary variables  $v_{t-1} = E[\tilde{y}_{t+1}|\Omega_{t-1}]$  and  $z_{t-1} = E[\pi_{t+1}|\Omega_{t-1}]$ , used to define the lagged information set of the central bank is solved by Dynare as

$$\begin{pmatrix} \tilde{y}_t \\ \pi_t \\ \tilde{\epsilon}_t \\ i_t \end{pmatrix} = \begin{bmatrix} 0.8 & 0.2 & -0.5 & -0.4 & -0.2 & -0.6 \\ 0.3 & 1.1 & -0.9 & -0.6 & -0.3 & -0.8 \\ -0.1 & -0.4 & 1.2 & 0.6 & 0.3 & 0.8 \\ 0 & 0 & 0 & 0.5 & 0.3 & 0.7 \end{bmatrix} \begin{pmatrix} \tilde{y}_{t-1} \\ \pi_{t-1} \\ \tilde{\epsilon}_{t-1} \\ i_{t-1} \\ v_{t-1} \\ z_{t-1} \end{pmatrix} + \begin{bmatrix} 1.6 & 0.5 & -1.0 & -0.8 \\ 0.6 & 2.2 & -1.7 & -1.1 \\ -0.1 & -0.8 & 2.5 & 1.2 \\ 0 & 0 & 0 & 1 \end{bmatrix} \begin{pmatrix} \varepsilon_t^y \\ \varepsilon_t^\pi \\ \varepsilon_t^e \\ \varepsilon_t^i \end{pmatrix} \quad (\text{A8})$$

Critically, because the structure of the lagged information does not impinge on the structure of the contemporaneous shocks, the  $B$  matrix in (A8) correctly reflects the identifying restrictions defined in equation (A7).

### Mixed information 2: CEE-CB

Our second model, CEE-CB, follows Christiano, Eichenbaum and Evans (2005) in assuming that the central bank has full information while the private sector does not observe the contemporaneous shock to monetary policy.

Here  $\Omega_t^{PS} = \{\Omega_{t-1}, \varepsilon_t^y, \varepsilon_t^\pi, \varepsilon_t^e\}$  and  $\Omega_t^{CB} = \{\Omega_t\} = \{\Omega_t^{PS}, \varepsilon_t^i\}$ , where  $\Omega_t$  (no superscript) denotes the full information set containing all shocks and variables up to and including period  $t$ .

In this case, our solution procedure applies Christiano's (2002), undetermined coefficients approach to a situation of mixed information. As before, our 'guess' solution for the model takes the form

$$x_t = Ax_{t-1} + B\varepsilon_t, \quad (\text{A9})$$

for undetermined matrices  $A$  and  $B$ , where  $x_t = [\tilde{y}_t, \pi_t, \tilde{\epsilon}_t, i_t]$ . The solution again proceeds by plugging (A9) into the structural equations (A1) – (A4) and solving out for  $x_t$ , and using the information assumptions to express expected values as the appropriate linear functions of past variables and current observed shocks. As described by Christiano (2002),  $A$  and  $B$  can then be solved sequentially, by equating coefficients between (A9) and the derived expression for  $x_t$ .

The structure of  $B$  in the SVAR(1) representation of the solution will reflect our assumptions about the observability of contemporaneous shocks. In the CEE-CB case, our assumptions prevent the GDP gap, inflation rate, and real exchange rate gap from responding contemporaneously to the monetary policy shock, while the interest rate is capable of responding to all four structural shocks. Because the central bank responds to *forecasts* of inflation and the GDP gap, the structural shocks to the GDP gap, inflation

and the real exchange rate gap enter the central bank's reaction function because they are useful in predicting the future state of the economy.

In contrast with the CEE-PS model, the mapping from model residuals to the structural shocks in the CEE-CB model takes the following form, in which only the private sector does not respond to the monetary policy shock.

$$\begin{pmatrix} u_t^y \\ u_t^\pi \\ u_t^e \\ u_t^i \end{pmatrix} = \begin{bmatrix} X & X & X & 0 \\ X & X & X & 0 \\ X & X & X & 0 \\ X & X & X & X \end{bmatrix} \begin{pmatrix} \varepsilon_t^y \\ \varepsilon_t^\pi \\ \varepsilon_t^e \\ \varepsilon_t^i \end{pmatrix}. \quad (\text{A10})$$

For the parameterization reported in the text, the full model solution in the CEE-CB case is

$$\begin{pmatrix} \tilde{y}_t \\ \pi_t \\ \tilde{e}_t \\ i_t \end{pmatrix} = \begin{bmatrix} 0.63 & -0.01 & -0.30 & -0.30 \\ 0.26 & 0.71 & -0.50 & -0.34 \\ 0.08 & -0.02 & 0.74 & 0.23 \\ 0.23 & 0.27 & -0.50 & 0.10 \end{bmatrix} \begin{pmatrix} \tilde{y}_{t-1} \\ \pi_{t-1} \\ \tilde{e}_{t-1} \\ i_{t-1} \end{pmatrix} + \begin{bmatrix} 1.27 & -0.02 & -0.60 & 0 \\ 0.52 & 1.42 & -1.00 & 0 \\ 0.17 & -0.05 & 1.48 & 0 \\ 0.46 & 0.54 & -1.01 & 0.76 \end{bmatrix} \begin{pmatrix} \varepsilon_t^y \\ \varepsilon_t^\pi \\ \varepsilon_t^e \\ \varepsilon_t^i \end{pmatrix} \quad (\text{A11})$$