Comments on: "Output gap uncertainty: Does it matter for the Taylor rule?"

Frank Smets

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Whenever I read a paper by Frank Smets, it's clear and rigorous and I always learn something. This paper is no exception to that pattern. He has presented a careful discussion of the implications for policy when theory ascribes an important role to a variable that is inherently unobservable, and potentially stochastic.

Smets' results confirm the intuition that uncertainty is detrimental, but an optimising policymaker has no reason to alter its behaviour if the uncertainty is additive in form and the model is linear-quadratic. If the policy is restricted to simple rules, however, the efficient weight on the output gap is lower if the gap is uncertain than if it is observed with certainty. In addition, in simple rules containing both inflation and the output gap, the weight on inflation also falls. This appears to be because the policymaker's preferences tend to determine the *relative* weights on the target variables, and not just their absolute magnitudes.

Even allowing for uncertainty about the output gap and a moderate degree of interest-rate smoothing, the estimated efficient weights in the simple rules are much higher than those estimated in empirical studies. Smets attributes this to greater uncertainty about the gap than might be expected from his estimation results, or a stronger tendency to smooth interest rates than we normally think of as plausible. In the context of this model, this is reasonable. Even if uncertainty really arises from a wider variety of sources, in this model it would still be captured in the residuals of the estimated model. Perhaps the answer lies in uncertainty about the correct model of the economy, rather than uncertainty about particular parameters or stochastic variables.

The paper presents estimates of the (unobservable) output gap using an unobserved components methodology and the Kalman filter. Potential output is modelled in log form as a random walk with positive drift. This specification falls neatly out of standard forms for production functions such as the Cobb-Douglas. Although Smets cannot reject the null of the drift term (trend output growth) being constant, we should not assume that it will never change. We can think of potential output as being determined by a Cobb-Douglas production function:

$$Y_t^P = e^{\alpha t} K_t^{\gamma} L_t^{1-\gamma} ,$$

where γ represents the relative capital share and α the rate of technical progress.

Then by taking logs and rolling forward one period, we obtain:

$$y_{t+1}^P = \alpha + \alpha t + \gamma \ln\left(K_t + \Delta K_{t+1}\right) + \left(1-\gamma\right)\ln\left(L_t + \Delta L_{t+1}\right).$$

If we define $\dot{k}_t = \left(\frac{\Delta K_t}{K_t}\right), \dot{l}_t = \left(\frac{\Delta L_t}{L_t}\right)$ and $\bar{k} = \left(\frac{\Delta K}{K}\right), \bar{l} = \left(\frac{\Delta L}{L}\right)$, where the absence of a time subscript indicates a long-run average, then after some rearrangement:

$$y_{t+1}^P = \alpha + \gamma \ln\left(1 + \dot{k}_{t+1}\right) + \left(1-\gamma\right)\ln\left(1 + \dot{l}_{t+1}\right) + y_t^P$$

so in Smets' equation (3):

$$\mu = \overline{\alpha} + \gamma \ln\left(1 + \bar{k}\right) + \left(1+\gamma\right)\ln\left(1 + \bar{l}\right), \text{ and}$$

$$\varepsilon_{t+1}^y = \left(\alpha_t - \overline{\alpha}\right) + \gamma\left(\ln\left(1 + \dot{k}_{t+1}\right) - \ln\left(1 + \bar{k}\right)\right) + \left(1-\gamma\right)\left(\ln\left(1 + \dot{l}_{t+1}\right) - \ln\left(1 + \bar{l}\right)\right)$$

Clearly μ is only going to remain constant so long as there are no mean shifts in the rates of technical progress, capital accumulation or labour force growth.

The thesis "additive uncertainty does not change the *optimal* path of the policy instrument, but it might matter when policy is constrained to follow a simple rule" would seem uncontroversial. The first part is simply an application of the certainly equivalence result. Sometimes, a counter-intuitive result might seem more exciting. However, we have no cause to be disappointed that our intuition is confirmed, when the analysis is as rigorous and technically sophisticated as in this paper.

There is a lot to like about this paper, and any negative comments I might have are quibbles rather than serious challenges to the paper's conclusions. Indeed, my only real quibble is that the simple rules conditioned on forecasts of inflation (F, FT, FS and FTS) ought to use policy-consistent forecasts rather than forecasts based on constant nominal interest rates. It seems strange to respond to a variable that you know will not be the outcome even in expectation. Using forecasts in the rule allows the policymaker to incorporate more information. However, in many models, assuming constant nominal interest rates to obtain the inflation forecast is inherently destabilising and can generate unnecessary cycles.[1] This would be particularly important with a relatively long forecast

[1] See Appendix B in de Brouwer, G and L Ellis (1998) for an example of this.

horizon such as the eight-quarter horizon used here. It might partially explain why the unconditional loss function increases when moving from actual-inflation feedback rules to rules using forecasts of inflation.

In this particular study, simple rules do quite well relative to the fully optimal policy. I remain unconvinced that this would be robust to more complex, open-economy models with more complicated lag structures, significant non-linearities, or multiple exogenous variables that are not included in the feedback rules.

An extension to the analysis of the simple feedback rules that I would like to see would be an examination of the robustness of the results to changes in the forecast horizon. The model used in this paper has a control lag from interest rates to inflation of two periods - which is fairly standard in the literature - but the forecast horizon is much longer. Haldane (1997) suggested that the forecast horizon needs to be *at least* as long as the control lag, so the horizon could safely be shorter. It would be interesting to see an exploration of the trade-off of reduced forecast error against the reduced information set arising from shorter forecast horizons (greater than two quarters).

Although the analysis presented in this paper is useful and the conclusions pertinent for policy makers, we should bear in mind that this paper deals with only one type of uncertainty, stochastic noise. It is presumed that the model for the evolution of potential output is correct, even though the variable itself is unobservable. However, there are a number of other possible models for potential output, for example the class of models that pop out of various smoothers and filters. It seems to me that the next stage in this line of research is to examine the implications of uncertainty about the *model* for potential output, and the cost of using an incorrect model.

References

de Brouwer, G, and L Ellis (1998). "Forward-looking behaviour and credibility: some evidence and implications for policy" Reserve Bank of Australia Research Discussion Paper No. 9803.

Haldane, A G, (1997). "Designing Inflation Targets" in P Lowe (ed.), *Monetary Policy and Inflation Targeting*, Proceedings of a Conference, Reserve Bank of Australia, Sydney, pp. 74-112.

Smets, F, (1998). "Output gap uncertainty: Does it matter for the Taylor rule?" Paper presented at the Reserve Bank of New Zealand conference, "Monetary policy under uncertainty," 29-30 June 1998.