

Comments on: “Monetary rules when economic behaviour changes”

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I enjoyed working through this paper, which has packed into it an enormous amount of very interesting material. Certainly, it stimulated far more thoughts than I now have time to comment on. But before proceeding to my more specific comments, let me share with you two broader thoughts which stood out by the time I reached the end of the paper.

The **first** of these was to ask whether, as researchers and policy advisers, we can yet summarise what we have learned to date from stochastic simulation work, relative to what we think we know from deterministic simulations? We continue, of course, to be relatively uncertain about a number of structural equations and transmission mechanisms in our deterministic models.

For what it's worth, my own preliminary guess is that the stochastic work has probably stimulated us to think about important issues in somewhat different ways, rather than having yet led to greater confidence about particular policy rules or reaction functions.

The **second** major question I was left with was: how does one take further the key result of the paper? This result is that increased monetary credibility leading to more stable output and inflation, will in general require the Central Bank to adjust its reaction function. I'll comment briefly on this at the end.

Finally by way of introduction, I can say that like the authors I found the credibility results more important and interesting than the slope of the Phillips curve and fiscal policy material. I'll therefore bypass any comments I could make in those areas, in favour of a number of specific comments focussing on credibility.

More credible monetary policy?

The metric of credibility

It's well known that “credibility” is a slippery concept to quantify, not the least because it is *Central Bank* credibility one is trying to capture. As acknowledged by the authors in their paper, the actual measures used are generally both imperfect and indirect (eg private forecasters' inflationary expectations). Not surprisingly then, I too saw the short sample empirical evidence for Canada in section 2.1 as not particularly convincing, apart perhaps from the evidence on bond yield differentials shown in figure 6.

The authors intrigued me with their comment that “A more compelling test would be to examine what happens to expected inflation following a shock that pushes inflation *above* the ...target range...*this experiment is not available.*” In the New Zealand context, such an experiment *would* seem readily available, as we remain all too aware of underlying inflation having pushed *above* the 0-2% target range during 1996 (and also in one quarter of 1995); moreover, it may well be that other countries could be interested in the context of potential movement *below* the bottom of their range, if the external shocks emanating from Asia are not handled sufficiently well.

Two aspects of CPAM (Canadian Policy Analysis Model) underpinning the work

Firstly, I note that the inflation-forecast-based (IFB) rule [equation (6)], focuses on the slope of the term structure of interest rates, and seems to allow for changes to risk premiums to come exogenously through the shock term. Similarly, the RBNZ's FPS model treats a sovereign risk premium as *exogenous*. It may, however, be of interest to note here that other modelling research work for New Zealand (Hall & Rae (HR), 1998; O'Donovan, Orr and Rae, 1997) has treated the interest rate risk premium (IRRP) as *endogenous*. Of particular relevance is that in the HR paper, following a significant fiscal policy shock, the postulated IRRP was interpreted as a proxy for financial market mechanisms, and was able to contribute at least as much as the monetary policy reaction function to maintaining price stability. I'll come back to the relevance of this matter again, at the end.

Secondly, perhaps the authors should amplify on the extent to which the transmission mechanisms for imported intermediate goods do or do not feature in CPAM, and hence to what extent they think such a simplification might limit the capturing of key external supply-side shocks.

The VAR-based shocks

Given my comment above on exogenous interest rate risk premia, I note this is interpreted in the VAR model as coming through the real exchange rate, z . I also wonder whether consideration was given to an explicit technological change or total factor productivity (TFP) shock, given that fiscal and foreign demand shocks were considered and rejected for this exercise. Or could a TFP shock be seen as coming via Y^* (potential output)?

The “IFB rule” and “more credible MP” runs

Perhaps not surprisingly, the material in tables 3 and 4 on percent of *time outside bands* and *median duration in quarters of departures from bands* attracted my attention. I haven't reached particular conclusions yet, but I do note that for New Zealand these kinds of numbers would need interpretation in the context of existing real business cycle (rbc) empirical evidence. From what I know of that work, the (percent standard deviation) numbers for New Zealand's real GDP volatility which are most comparable with those presented in this paper for Canada's “ y tilda”, would be

somewhat higher at between 3.5 and 4.0. The higher numbers for New Zealand would in large part be due to external shocks and climatic factors. This in turn would seem to raise the additional issue of how far monetary policy credibility can assist when output volatility is substantially supply side rather than demand side based.

Implications of the probable need to “adjust the Central Bank’s reaction function”

Two points, briefly:

First, I suspect that serious consideration of “adjusting the Central Bank’s reaction function” should mean putting even more onus on the importance of trying to gauge in a broad sense, the relative strengths of demand-side versus supply-side components of shocks. This could be seen both in the context of recent external shocks from Asia, and the potentially now different exchange rate and interest rate transmission mechanisms from those understood for pre-stable inflation days. My view is, of course, somewhat in contrast with that emphasised by the authors in their conclusions. There, they suggest that “...it may be preferable to use a rule that performs reasonably well in a range of possible environments, rather than providing the best outcome in one environment.”

A second way of thinking about this may be as I introduced earlier; private sector behaviour in financial markets under floating exchange rates could well achieve such a significant amount of the “reaction” that the reaction function of the central bank may not need to be utilised *either as much or as often*. That certainly would seem the implication of the response dissection analysis done in HR, with its endogenous IRRP being a crude proxy for financial market adjustments.

In summary, then, I found this a very stimulating paper. It has made an important start to the authors’ aim of bringing together two important strands of research.

References

Hall, Viv B and David Rae, (1998). “Fiscal Expansion, Monetary Policy, Interest Rate Risk Premia, and Wage Reactions,” forthcoming in *Economic Modelling*.

O’Donovan, Brendan, Adrian Orr and David Rae, (1997). “A World Interest Rate Model (WIRM),” Financial Research Paper No. 7, National Bank of New Zealand Limited, Wellington.