Comments on: "Efficient simple policy rules and the implications of uncertainty about potential output"

Aaron Drew and Ben Hunt

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This paper by Aaron Drew and Ben Hunt (DH), based on the Reserve Bank of New Zealand's new Forecasting and Policy System (FPS) model, is informative and stimulating. It is an important contribution to the analysis of appropriate procedures for conducting monetary policy in New Zealand. It also makes an interesting contribution to a growing international literature that attempts, for a wide range of countries, to compare and evaluate alternative simple rules of thumb for conducting monetary policy.

The details of the FPS model of the New Zealand economy — the analytical "machinery" that underpins the paper's analysis — are largely omitted from the paper. This omission of model details makes the paper easier to read for its policy content, and seems appropriate given the purposes of the paper. But a full appraisal of the paper's conclusions must rest, of course, on judgments about the FPS model itself. At the outset of my comments, therefore, I include brief observations about the FPS model and the research project which gave birth to it.

The FPS research project

The Reserve Bank's Economics Department for the last several years has been engaged in the FPS model project, assisted by several economists currently or formerly associated with the Bank of Canada's model-building activities. I believe the progress made is impressive, not only on the analytical substance of the modelling effort but also in the way that the model is being integrated into the Reserve Bank's ongoing process of forecasting and policy deliberation.

One can raise questions or doubts about particular aspects of the structure of the FPS model. Here are a few examples. Government consumption spending is not modeled as though government consumption matters in the private sector's utility function; in common with the majority of empirical macroeconomic models, government spending in the FPS model is thus in effect a waste of resources, and the model does not adequately capture the substitutability between private consumption and public goods. The model treats risk premia as just exogenous, even though we know that in real life they can vary endogenously, significantly so, in response to other macroeconomic variables determined endogenously in the model. Imports of intermediate goods do not appear as a factor input in the production function, and hence a possibly important channel of exchange-rate

influences is missing from the model. Changes in dependency ratios and labour-force participation rates are not incorporated in the model; hence the model does not include potentially important influences of demographic changes on saving, investment, and the current-account balance. And so on.

Such questions or doubts, however, can (and should) be raised about any macroeconomic modeling effort. I would identify at least as long a list of issues for other macroeconomic models. The FPS model is, in my opinion, close to the frontier of current best practice in empirical macroeconomic modeling. The Reserve Bank can be justifiably proud of having supported this research project and thereby putting itself into the strong analytical position which use of such a model provides.

When judging a research project such as the FPS model, I like to think of the analogy of an expedition whose task is to ascend a high mountain peak. The task is arduous, replete with difficulties. As of the moment, the expedition has only reached an intermediate base camp. Looking up, there are many pitches and ridges to surmount before attaining the summit. But if one looks back and down, it is also clear that the expedition has made very considerable progress. The starting point on a marshy lowland is far below, and the vistas from the current promontory are substantially more revealing than what could be discerned at the outset of the journey.[1]

Other literature on simplified rules for monetary policy

In shaping my comments, I have mainly sought to compare the DH paper with the analysis and results in other recent papers. In particular, I have focused on the degree to which the DH analysis of alternative rules for New Zealand conforms with the conclusions reached in the other work, whether there are notable differences, and whether any surprises are thrown up.

Early empirical work on this topic was summarised in a Brookings-sponsored volume (Bryant, Hooper, and Mann, 1993). John Taylor (1993b, 1995) did further work on the real-GNP-plus-inflation targeting rule used in the Brookings project, now commonly referred to as the "Taylor rule." But substantial research has been done since then, notably at the central banks themselves. I have in mind especially research at the Bank of Canada, the Federal Reserve, and the Bank of England. The Bank of Canada research, using the CPAM/QPM models, is summarized in Black-Macklem-Rose (1997) and the Amano-Coletti-Macklem paper presented at this conference. Numerous papers have been done at the Federal Reserve, but of particular note is Levin-Wieland-Williams (1998), which compares results across four heterogenous models: the FRB/US model (Brayton-Tinsley, 1996), a small macro model at the Federal Reserve known as MSR (Orphanides and Wieland, 1998), a model constructed at the Federal Reserve by Fuhrer-Moore (1995), and John Taylor's multicountry model (1993a). The Bank of England research is summarised in Batini-Haldane (1998). Rude-

[1] The analogy captures the sense of progress made in research projects of this type. But the analogy is misleading in at least one respect. The "summit" is never fully reached in such research, because the economy's structure is constantly changing and improvements are frequently being made in knowledge about the structure.

busch-Svensson (1998) uses a small model to analyse the same alternative simple rules. The Laxton-Isard (1999) paper for this conference is another example. The bulk of this recent work has emphasised stochastic simulation techniques. The forthcoming volume edited by John Taylor (1998) pulls much of this work together in one place.

"Inflation-forecast-based rules" and "Taylor rules"

For inflation-forecast-based (IFB) rules, the FPS model results for New Zealand are discussed in section 3.1 and summarised in figure 1. The qualitative story in the DH paper is much the same as that told in the other research, in particular the evaluations done at the Bank of Canada and the Bank of England. There is a marked trade-off between inflation variability and output variability. The efficient frontier in inflation-variability/output-variability space is sensitive to the forecast horizon and the size of the "feedback coefficient" in the simple policy rule - in the DH equation (2). Notably, the parameters used in the IFB rule taken as the "base case" in the FPS model result in a point in the space well inside the efficient frontier.

For Taylor-type rules, the arguments in the policy reaction function are contemporaneous inflation and the contemporaneous output gap. For these rules, too, the DH results — see figure 2 for a summary — look qualitatively similar to the findings in the other papers for Canada, the UK, and the United States. As in the other research, the location of the efficient frontier depends importantly on the relative magnitudes of the feedback coefficients in the rule (and in DH equation (3)) as well as on the degree of aggressiveness in policy reactions (the absolute magnitudes of the two parameters).

Up to this point in the paper, there are certainly no surprises. The FPS results for New Zealand tend to reinforce the conclusions reached for other countries.

Do rules that use model-based inflation forecasts — rules that are explicitly forward looking — outperform rules which use only contemporaneous information? Specifically, how do IFB rules come out in a horse race with Taylor rules? Here, with this question, we do begin to see differences between DH and some of the other research.

As shown in the paper's figure 4, the DH results with the FPS model say that the Reserve Bank of New Zealand can do significantly better at reducing inflation variability by selecting an IFB rule on the efficient frontier for those rules, than by using a Taylor rule chosen from the efficient frontier for Taylor rules. The Bank of England Batini-Haldane asserts this conclusion strongly for the UK. The published Bank of Canada research tends to come out at this place for Canada. Similarly, the Laxton-Isard paper for this conference argues that an IFB rule can do markedly better once the credibility of policy is endogenously modeled.

But some of the Federal Reserve research for the United States — in particular Levin-Wieland-Williams (1998) — is much more skeptical of the benefits of a forward-looking rule. During an earlier session at this conference, John Taylor observed that information lags may not make all that much difference, and that contemporaneous-variable rules may do about as well as forward-looking rules. So there is a genuine unresolved issue here, on which it is not yet possible to discern an

emerging consensus.[2]

I would like to understand better than I do why the Federal Reserve results seem to give the forward-looking rules little or no advantage. Similarly, I would like to understand more deeply what it is about the properties of the FPS model and about the DH procedures for carrying out the stochastic simulations that causes the forward-looking rules to produce so much more efficient a frontier.

My gut instinct is to believe that, at least in many circumstances, a forward-looking approach for policy will be preferable. When driving down a highway, one does not just look in the rear view mirror, or outside the window on the side. One looks ahead down the road, and adjusts one's actions correspondingly. In some sense, the issue here may be just as straightforward as that driving analogy. For some types of shocks, it surely has to be right that policy ought to be looking ahead at least several periods into the future. Think, for example, of the Asian currency crises and their possible effects on the New Zealand economy!

Nonetheless, there are also substantial conceptual arguments in the other pan of the scales. Forward-looking IFB rules are much more obviously model-specific than the simpler rules which use only contemporaneous information. Given the high degree of uncertainty about which of the available competitor models is the least inappropriate for policy analysis, model specificity for a rule is, other things equal, a disadvantage. Moreover, the contemporaneous-information rules are simpler for the general public to understand. To use a slippery word, such rules are more "transparent."

On both conceptual and empirical grounds, therefore, a general verdict on this important question is still out. For the time being, it would presumably be prudent to adopt an agnostic stance for New Zealand rather than relying heavily on the FPS-model results reported in the DH paper.

Should output variability be an argument in the policy reaction function?

The next question on which I want to focus is whether, and how much, it might add to the performance of an IFB rule if the policymakers were to include the contemporaneous value of the output gap as an additional argument in the rule. Interestingly, the DH/FPS results give a clear cut answer to this question for New Zealand. As is readily seen in figures 4 and 5, the analysis suggests that the Reserve Bank could, for any given degree of intensity of its dislike for inflation, attain a significantly lessened variability for output by including the output gap in its policy reaction function.

Moreover, this conclusion appears to be robust to uncertainty about the output gap. In figures 7 and 8, summarising the analysis which acknowledges and explores the substantial uncertainty

[2] I have the impression that even at the Bank of Canada, which has been a pioneer in the analysis of IFB rules, some research may now be suggesting that contemporaneous-information rules can in some circumstances outperform IFB rules.

about how best to measure the output gap, the efficiency frontier for the IFB-plus-output-gap always lies substantially southwest (mostly south) of the efficiency frontier for the unaugmented IFB rule.

Most of the other research comparing simple rules has not, I believe, focused directly on this question. Loosely speaking, however, it is my impression that some of that research — particularly the Federal Reserve studies of the United States — are qualitatively consistent with the DH finding.

The DH paper takes an important and helpful step forward by focusing (in section 4) on the implications for monetary policy rules of uncertainty about potential output. There appears to be widespread concern among New Zealand economists and policymakers, perhaps even more so than in other countries, about the difficulties in accurately measuring potential output. Even actual output may be measured with a substantial error. The uncertainty stemming from these measurement problems thus definitely warrants attention.

I feel agnostic about the particular procedures used by DH to generate the "typical errors" made by policymakers in measuring potential output. I have not been able to analyse the procedure carefully, understanding it in its details and then asking whether it is adequate for the analytical purpose in hand. The authors themselves identify an alternative procedure that might be conceptually preferable but seems for the moment computationally infeasible. It will be useful in future work to refine and compare alternative procedures, and verify whether conclusions are sensitive or insensitive to the particular procedure chosen.

The general inferences that DH draw from their analysis about the implications of uncertainty regarding potential output seem plausible to me. The Smets paper at this workshop (1999) reaches similar conclusions. And the Laxton-Isard paper tells an analogous story about the closely related question of uncertainty about the NAIRU. Presumably future research on monetary policy rules will devote still more attention to the implications of uncertainty.

I want now to raise a question about the output gap that is not discussed in the DH paper. Why are we not including the forecast path as well as the contemporaneous value of the output gap as an argument in the reaction function, just as we include the forecast path of inflation rather than merely the contemporaneous value of inflation? The easy answer that has been given to this question by the convinced proponents of IFB rules is that the forecast path of the output gap is already included in the inflation forecast; would it not be redundant, they ask, to include the output forecast? But that answer seems to me less than fully convincing. The inflation forecast from the model already includes the contemporaneous output gap as well. Yet the DH paper shows that the efficiency frontier of choices is markedly improved if the contemporaneous output gap gets into the reaction function. I conjecture that there may be good grounds for going further still and including the forecast path of the output gap explicitly.

If I stand back from the results in this paper and think about policy debates in New Zealand about macroeconomic policies, I can't help but recall some of the discussions I had two years ago when I had the pleasure of coming to New Zealand for an extended visit. My suggestion in a public lecture at that time was that it might be helpful for New Zealand monetary policy if output variability were allowed into the RBNZ reaction function with a non-zero weight (Bryant, 1996a). That suggestion,

however, was controversial and appeared to have little support. There were two reasons that prompted my suggestion. First, it seemed likely that for many, if not all, policymakers, output variability was genuinely a source of concern in its own right — in other words, was in fact a component of the underlying loss function regarded as appropriate for guiding overall New Zealand economic policy. Second, even for those who were convinced that the Policy Target Agreement between the government and the Reserve Bank should focus exclusively on inflation, it seemed to me that the inclusion of output variability in the monetary policy reaction function could be beneficial because of its helpful "indicator" properties.

I constructed a schematic model of a "ZZ economy," which was very much in the spirit of today's FPS model. (My model was less carefully and empirically developed than FPS, but it had many of the same conceptual foundations.) And I performed a few simple deterministic simulations with my model, comparing alternative simplified rules. Some of the rules did, and others did not, contain an output variability term. My conclusions were advanced tentatively, but it appeared to me that the work I had done supported the suggestion that New Zealand policy could beneficially pay some attention to smoothing output as part of its overall policy of targeting inflation (Bryant, 1996b).

Two years later, the inferences reached in the DH/FPS paper seem to me pretty much on the same wave length as those I advanced in 1996. As I read the evidence in the DH paper, one can infer that it would probably be beneficial for policy deliberations at the Reserve Bank to include an output-gap term (or several such terms) in the underlying supporting analysis. I would also argue that doing so need not signify in any way a weakening of the resolve to keep inflation in New Zealand under control.

Variability in interest rates and exchange rates

The last subject I want to raise is considered to a small degree in the DH paper, but has gotten substantially more attention in some of the other recent literature. This is the question of how monetary policymakers should regard variability in interest rates and exchange rates.

The DH paper does worry that some of the simulation results carried out in their work could be implausible because the outcomes imply excessive variability in interest rates. In a footnote to section 3.3 [fn. 15 on page 14], for example, the authors observe that as the weight on the output gap term is increased to large values, the variation in the interest-rate instrument eventually becomes "dramatic." Because of their concerns about overly large variations in the interest rate, in particular the possibility that simulations could implausibly require the nominal short-term interest rate to fall below zero, the authors want to focus on only a subset of their results. If I have understood correctly what they have done in constructing figure 5 (to be contrasted with figure 4), they essentially throw away any of the simulation results that would entail so much interest-rate variability that the nominal rate would fall below the floor of zero.

More generally, however, the paper does not address the question of "interest-rate smoothing." Given the New Zealand context, it is an even more notable omission that the issue of variability in

exchange rates (or variability in New Zealand's trade-weighted exchange-rate index) is not alluded to at all.

Note that it is necessary to distinguish in concept between the constraint that the nominal interest rate cannot fall below zero versus the broader question of whether the monetary authority should eschew large changes in its interest rate "instrument" even if the interest rate does not threaten to fall below zero. The research at the Federal Reserve — for example, Williams (1997), Levin-Wieland-Williams (1998), and Orphanides-Wieland (1997) — has addressed these interest-rate issues at length. The same issue comes up in the Laxton-Isard paper. For an open economy in which the monetary authority attempts to use a monetary-conditions index (MCI) as a fulcrum for policy, the analogous issue is not "interest-rate smoothing" alone but rather some combination of smoothing interest rates and "exchange-rate smoothing."

As a reminder of how the issue comes up in the context of simplified rules, recall that a rule permitting both the inflation rate and the output gap to appear in the reaction function as targeted variables can be written as:

$$R_t = \lambda_3 R_{t-1} + (1 - \lambda_3)(r^* + \pi_t) + \lambda_1(\pi_t - \pi^*) + \lambda_2(ygap_t)$$

where R_t, the nominal short-term interest rate, is the policy "instrument" used by the monetary authority, π_t is the inflation rate, π^* is the targeted value of the inflation rate, $ygap_t$ is the output gap, r^* is a "long-run equilibrium" level for the real short interest rate, and the three λ coefficients are "feedback" parameters describing the strength of the policymakers' responses. The relationship can alternatively be rewritten as:

$$R_t = [(1 - \lambda_3)(r^*) - \lambda_1(\pi^*)] + \lambda_3 R_{t-1} + (1 - \lambda_3 + \lambda_1)\pi_t + \lambda_2(ygap_t) .$$

The coefficients in the original 1993 "Taylor rule" were set as $\lambda_1 = 0.5$, $\lambda_2 = 0.5$, and $\lambda_3 = 0$. With the value of λ_3 set to zero, the resulting class of rules has been termed "first-difference" rules. In contrast, if λ_3 is set to unity, the resulting class may be termed "level" rules. More generally still, as discussed in Levin-Wieland-Williams (1998), the parameter λ_3 might have any positive value — even values exceeding unity — and the magnitude of the parameter can be interpreted as calibrating the degree to which the rule engages in "interest-rate smoothing" (the larger the value of λ_3, the greater the degree of smoothing).

The research at the Federal Reserve suggests that the efficiency frontier in inflation-variability/output-variability space is markedly more favorable if the interest-rate smoothing parameter is unity rather than zero. This conclusion held for all four models studied in Levin-Wieland-Williams (1998). My conjecture is that, analogously, the DH/FPS results might look substantially different if, for example, DH were to choose the first difference of the short-long interest-rate spread rather than its level for their left-hand-side instrument variable (equations (2), (3), or (4)).

In the United States, UK, and Sweden, and even in Canada, the issue of "excessive" variability in the monetary authority's policy instrument can be discussed fairly straightforwardly by examining variation in the key short-term nominal interest rate used as the fulcrum in open-market operations. There is no ambiguity what the "instrument" itself is; it is the key short interest rate.

Here in New Zealand, however, it is less clear how to identify the "instrument" of monetary policy. In recent months, policy deliberations have focused on a specific calculation of an MCI index.[3] Public statements from the Reserve Bank sometimes come close to identifying the MCI as the Reserve Bank's policy instrument. At other times, public statements describe the MCI more as an indicator of the Bank's intentions or hopes for monetary conditions, with the implication that announcements and periodic monetary policy statements ("open-mouth policy") are the instrument *per se.* Widespread agreement exists that the Reserve Bank's cash target, changes in which are in any event very infrequent, is *not* the key policy instrument. Correspondingly, it is somewhat less clear in the New Zealand context how to conceptualise "excessive variability" in the instrument of monetary policy. The variability in question probably pertains to the *combined* variability of interest rates and exchange rates. But might "excessive variability" even be defined as "too frequent" or "excessively assertive" Reserve Bank statements?[4]

In any event, the point I wish to stress here is that the issue of variability in interest rates and exchange rates ought to be addressed explicitly as part of any analysis of alternative reaction functions for monetary policy in the New Zealand context. Doing so might lead to significant modifications in the conclusions summarised in the DH paper. Looking into this issue might therefore be one of the next-priority steps to be taken by Reserve Bank staff as they carry forward their important stochastic-simulation research with the FPS model.

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[3] The MCI is a combination of the TWI weighted-average exchange rate and the 90-day bank bill rate, calculated such that a 2 percent increase in the TWI (an appreciation of the New Zealand dollar), representing a tightening of monetary conditions through exchange-rate channels, is deemed equivalent to a 1 percentage point increase in the 90-day bill rate, a tightening through interest-rate channels.

[4] I make this remark with tongue partly in cheek. But the question also serves as a reminder of the unusual, and possibly at times unclear, nature of "policy actions" in the New Zealand context.

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