"What is a good macroeconomic model for a central bank to use?"[1]

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In thinking about a suitable answer to questions like "What is a good macroeconomic model for a central bank to use?" it often pays to study the revealed preferences of the consumers. According to Mervyn King (1999) there are 192 central banks in the world. I have not done an exhaustive survey of their attitudes towards models, and, even if I had done so, the results might turn out to be rather unclear, for a number of reasons. First, even when models exist it is never entirely clear to an outsider how much they get used in the policy process. Second, often one will see a reference to the fact that the central bank in question is not silly enough to put all its eggs into the one basket and so uses a "suite of models". I have to say that I often have the feeling that the "suite of models" stance is more about keeping the research staff happy, and creating the impression that the central bank in question is a "very model of a modern major central bank, than it is about producing models that monetary policy makers might find useful. This is not to deny the fact that we will always have to create some special purpose models that need to be used when considering the impact of rare events such as infrequent changes in tax systems, but it is the core model which accounts for most work at central banks. In what follows I will just assume that the core model that is displayed in the publications is used and I will ignore the plethora of auxiliary models that are sometimes mentioned.

Core Models: A Potted History

So is there any evidence of a core model which commands widespread assent? Table 1 suggests that this is not so and leads us to ask why banks can't seem to agree on the choice.

Table 1: Core Models in Use in Central Banks						
Bank	Model	SS	ECM	Stk	Exp	Size
Fed	FRB/US	ET-W	D	N(?)	DGDPX-D or I	50
B Canada	QPM	ET-S	C	Y	DGDP-I and D	?
B.England	MM (MMT?)	ET-W	D	Y?	DGDP-I/Obs	20
Norges. Bank	RIMINI	GRCO	D	N(?)	DGDP/I	30
MAS	MMS?	ET-W	D	Y	Most DGDP-D	34(?)
RBNZ	FPS	ET-S	C	Y	DGDP-I and D	?
ECB	AWM	ET-W	D	N(?)	Most DGDP-D	15
RBA	EGM	GRCO	D	N	Obs	5

SS: How is a steady state enforced? GRCO: great ratios/co-integration, ET-W, weak theory, ET-S strong theory
ECM: How are the ECM parameters determined?. D: Data, C:Calibrated
Stk: Is stock-flow equilibrium highlighted? Y=yes, N=No.
Exp: How are expectations modeled?. DGDPX-D: Expectations depend on parameters of DGP of exogenous processes, DGDPX-I, independent of these parameters; obs=observable
Size: #of stochastic equations

Part of the diversity in core models reflects the history of macro-economic modeling. I like to think of this as having evolved through five stages. In the first stage the dominant philosophy was "fill 'em in", wherein the national accounts identity was written out and behavioral equations were then supplied for consumption, investment etc. As each of the behavioral relations depended upon other variables, extra equations had to be added to the system. The only thing stopping the spiral in the early days was the lack of computing power. Thus most "induced" variables ended up being treated as determined off-model i.e. as exogenous. Later, as mainframes became powerful, these models blew out to gargantuan proportions and their size generally defeated any attempt to follow the logic of their policy experiments.[2] Outside observers would often come away puzzled by why certain experiments performed on them gave peculiar outcomes. Their builders were often quite brilliant at explaining any perverse outcomes, although the rationales provided led to both admiration and a suspicion that something wasn't quite "right. In particular there seemed to be a lot of "free

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[2] One should be careful to note that size and complexity are really quite different facets of a model. As Allan Powell has pointed out computable general equilibrium models are large, due to their having to account for the decisions of many industries, but not very complex, as the decisions are modeled in an identical fashion in each industry. The complexity of the macro models of the 60s and 70s came from the fact that decisions were modeled in an enormous number of ways.

lunches" for monetary and fiscal policy. So it was no surprise that Stage 2 modelers reacted to this situation by imposing intra-temporal constraints that needed to be obeyed e.g. in portfolio and factor choices. The RDX2 model of the Canadian economy of the late 60s and early 70s is a good example of this development. It was just a short step from that to being concerned about inter-temporal consistency conditions holding for both the private and public sectors in the models.

Once one starts to worry about inter-temporal aspects the question inevitably arises of how expectations are to be formed since they are a key connecting link between the present and future, although it is not necessary that one has an inter-temporal optimizing framework in order to introduce expectations. In third stage models rational expectations became the favoured mode in academia but not so much in operational macroeconomic models. There one saw a number of approaches. If actions depend upon expectations of future outcomes one may say that "forward looking" expectations are in a model. Often a distinction is made between these and "backward looking" expectations, but it is hard to know what this means. Since rational expectations involves forming expectations using the information at the time of decisions, "forward looking" expectations are also "backward looking". From an econometric viewpoint the difference is really one of whether the parameters describing the DGP of the forcing variables of the system (DGPX) enter into the weights assigned to observable variables when forming expectations. In rational expectations they do. In what is often called "backward looking " expectations they do not. Accordingly, models for which there is a dependence of expectations upon the parameters of the forcing processes are designated DGPX-D (D for dependent) while DGPX- I signals that they are independent. When a model was being used in a simulation mode (for policy analysis) combinations of "forward looking" and "backward looking" expectations were mostly used, with the weights to be applied being rationalized as due to liquidity constrained consumers etc. Sometimes only forward-looking elements were allowed as with expectations of financial asset prices. In forecasting mode however expectations were generally replaced by whatever observations could be made directly on these quantities such as from indexed bonds or surveys of wage negotiators. I suspect that this will always be the case. For those countries with a reasonable history of successfully targeting inflation one is likely to see expectations being replaced by the target inflation rate.

By the 1980s one was able to solve all the computational issues that combinations of backward and forward looking expectations raised. Many of the solution methods required the model to converge to some steady state or steady state growth path and this created a suitable milieu for a distinctive characteristic of stage 4 models viz these models had a steady state solution and the responses of the steady state solutions to shocks were consistent with theoretical reasoning. Although some stage 1 and early stage 2 models may not have had a steady state solution, that would have become less likely as computational facilities improved and many dynamic simulations could be performed. So it was not the existence of a steady state that became important but rather its nature.

The description of the development of the Bank of Canada's Quarterly Projection Model (1997) has an excellent discussion of how the experiences of Bank of Canada modelers led to their desire to ensure that a sensible steady state model was embedded in their core model. Those constructing stage 4 models believed that models should be *designed* so as to have a steady state. Two methods have evolved for dealing with the design issue. One is the "great ratios"/co-integration (GRCO) school which either imposes the constancy of many ratios or which uses co-integration methods to estimate steady state relations between variables. The other uses an optimizing framework grounded in some economic theory (ET). However, for a number of reasons, there is not a complete demarcation between the two approaches. First, because in an open economy it is sometimes difficult to find a determinate ET model that appeals and it is easiest to sort out the indeterminacies by imposing the ratio of foreign debt to GDP.

I doubt that anyone thinks that great ratios are bad things to impose but the GRCO approach can't be applied uncritically. If it is some important restrictions may be missed. For example if Y and Y^* are in constant ratio f in steady state then working with the logs, $y=\ln(f)+y^*$ and the use of $y-y^*$ as the measure of departure from equilibrium leaves $\ln(f)$ to be estimated from the data. In a system there will often be some constraints on the $\ln(f)$ from each of the variables, and often such constraints are ignored by the GRCO enthusiasts. For example in a closed economy where GDP is the sum of consumption and investment then $f_C + f_I = 1$ but this restriction is ignored in estimation. Of course one might say it is just a matter of working out what these are and imposing them but that can be an enormous task and it may be simpler to just follow the ET strategy. In practice the distinctions between ET and GRCO methods is also blurred by that some models have what might be called "weak" theory involved e.g. a production function is used to produce marginal products that become the cornerstone of factor demand and equations, and typically these come down to imposing ratio.restrictions. Models with this orientation will be designated as ET-W. Often weak theory implies much the same type of restrictions as in GRCO and, indeed, often unknown parameters are found by using co-integration methods. Strong ET models (ET-S) tend to enforce a single paradigm quite rigorously and often the focus of attention is on basic parameters such as the probability of death of a consumer.

Recognition of the need for a steady state also drew attention to the need to have both stock and flow equilibria. Reconciliation of stocks and flows is hard to avoid with the ET approach to design but can sometimes be left unresolved by the GRCO school.

A second question that stage 4 models had to answer was how one describes the dynamics of adjustment to a steady state position from some initial point. There have been two approaches which are, conceptually at least, distinct. One is to simply estimate the coefficients in an Error Correction Equation (ECM) like

$$\Delta y_t = d + a\Delta y_t^* + b(y_{t-1} - y_{t-1}^* - c) \qquad\qquad (1)$$

$$\Delta y_t = (d - bc) + a\Delta y_t^* + b(y_{t-1} - y_{t-1}^*) \qquad\qquad (2)$$

where $y_t^* = \log(Y_t^*)$ is the steady state value of $y_t = \log(Y_t)$ (of course more lags of Δy_t and Δy_t^* may appear in practice). The ECM assumption is not a very restrictive one as any linear dynamic system can be manipulated into this form. A second solution is to set up some synthetic optimization problem and derive the ECM from that. Of course the latter may produce some restrictions between the parameters but basically there is always an isomorphism between the two.[3]

In practice the distinction has been less about the methodology used to justify an ECM model than it has been about how the parameters in (1) are to be fixed. Here there has been a split between those estimating the parameters from an historical sample (D) and those choosing to "calibrate" (C). Why should one find the latter attractive? The QPM builders had this to say about their motivation for adopting it

" the inability of relatively unstructured, estimated models to predict well for any length of time outside their estimation period seemed to indicate that small-sample econometric problems were perhaps more fundamental than had been appreciated and that too much attention had been paid to capturing the underlying economics….It was concluded that the model…..should be calibrated to reflect staff judgement of appropriate properties rather than estimated by econometric techniques". (p 14)

The actual mechanism is spelled out a bit in that paper under the heading of "matching" where it is shown how one might choose parameters to mimic SVAR impulse responses, the sacrifice ratio as computed from a Phillips curve in a small macro model etc. One can't help wondering about the validity of such matches. If the models estimated from data were so bad, in particular being subject to a lot of structural change (and frequent reference is made in the QPM document to that fact), it seems odd to take quantities from such models as the reference point for a matching exercise. It's also a problem that a model like an SVAR is not an a-theoretic model and one may well be matching up apples and oranges. Of course the proof of the pudding is in the eating and one assumes that we should judge these models by how well they have managed to survive the prediction test that doomed the earlier ones. If the main source of forecast errors is shifts in sample means, as argued by Hendry and Clements (1999), then there doesn't seem to be any higher likelihood that a calibration approach will be successful than a data based one. It is the intercepts in the ECM that are

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[3] One would not expect that an optimal adjustment path would be a univariate scheme like in (1) but ET models tend to enforce such a scheme since $b<0$ then becomes a simple way to enforce convergence to a steady state solution. In this respect the GRCO solution is more general.

shifting and not the other parameters of the model. So far we have seen little in the way of discussion about whether these models have proven to be better. One has a fear that divorcing parameter selection from a specific set of historical data is akin to "seeing no evil, and hearing no evil". It's hard to find out about model failure if you never look for it.

In stage 5 we have seen a questioning of the size of the models. The norm now seems to be around 20-30 structural equations and then many identities. Some of this reduction was achieved by consolidation using arbitrage arguments e.g. many of the stage 3 models aimed at capturing the balance sheets of many financial institutions and so modeled a plethora of interest rates. These were all compressed into a simple financial sector with a few yields, on the grounds that arbitrage would force all the yields to be identical (up to a relatively constant risk premium) and that the extra information found by looking at the detail of the financial sector was rather small.

Why is there Variety in Core Models?

Why do we see such variety in core models? To some extent it is cost. Developing an ET steady state model is not a cheap operation as it requires a team of highly skilled people. In the case of the RBNZ it was contracted out to the Canadians. The BoE has had a small team working on a new model of this type for almost a year. Thus not everyone will have the capability of building such a model. But I also think that perceptions of the need for a particular type of model often come from past experiences and the nature of the monetary policy decision making body. Reading the document which outlines the QPM one is struck by how the need for a steady state was grounded in past experiences of models that failed to have that characteristic; Others may appreciate having some steady state solution but not necessarily one based on an ET scenario e.g. the RBA.

The banks surveyed above also have a wide variety of styles for making monetary policy. Thus the Fed, Bank of England and RBA have external members on their decision making bodies. The background of the external members varies widely and interest in and ability to think in terms of a quantitative model also varies widely. The Bank of England probably has the greatest fraction of committee members who are comfortable with this way of thinking. Minutes of the Fed meetings suggest that most of the members are familiar with the ideas but do not see it as the dominant way of thinking about policy. Traditionally, the RBA board has been largely selected from groups of people who are unlikely to be familiar with model-based policy analysis. When monetary policy is made by internal teams, as in the MAS, RBNZ and the Bank of Canada, it is noticeable that models seem to be much more central to the policy process, presumably because most of the members of the decision making bodies are likely to be professional economists.

A third reason for a diversity of approaches involves the extent to which the monetary policy makers have to publish forecasts on a regular basis, the degree of precision with which they choose to pronounce on the uncertainty surrounding their projections, and the need to produce accounts of their actions to markets and legislators. Thus it is very hard to see how one could produce a fan chart, as pioneered by the Bank of England, without a model. As Heath (2002) indicates in his description of how the fan chart is constructed, the process requires the entry of various scenarios into a model to give monetary policy makers the feel for what the skews of the outcomes are likely to be. Thus if one does not have to publish forecasts or attempt things like fan charts (as is true of the RBA) it may not be surprising that there is a corresponding lack of enthusiasm for a formal use of quantitative models in the monetary policy process. It would be pretty hard to argue that there is no model in people's minds when decisions are made but this is often rather vague and hard to pin down. Of course that may not matter. Building a model of a champion tennis player so that he can understand the science of what he does may not improve his performance very much. He is good at what he does because he manages to rapidly combine together large amounts of information and to issue the right instructions in response. I think there are people who have a feel for the economy that others don't have and there would be little point in these individuals spending a large amount of time thinking in terms of a model, as the latter may be an unproductive constraint on their thought processes. But it is unlikely that all members of a committee (or central bank staff) would have such skills since sometimes these come from a lifetime in the area and it is rarely the case that monetary policy makers have that amount of continuity.

An interesting feature from Table 1 is that there seems to be some convergence in views over the need for a steady state and the way to get dynamic adjustment, even if there are different attitudes to how a steady state should be designed. The real outlier in the table is the RBA. Except that it treats many variables as exogenous, the RBA system is much closer in size to the VAR type models that are rife in some parts of academia. Moreover the RBA model only features a flow rather than stock/flow equilibrium. This raises a number of possibilities and questions. One possibility is that the RBA is a forerunner of a new trend to adopt very small models when looking at policy issues and this seems worth discussion.

In summary there seem about as many varieties of core models as there are breakfast cereals available to a consumer entering a supermarket in a country like the U.S. Like cereals the ingredients are pretty standard but the products are different in the minds of the consumers and, although one does see brand switches e.g.Model 12 of the RBNZ, which emphasised GRCO, was replaced by FPS with its ET orientation, mostly the consumers remain happy with their choices for quite long periods of time. Still, we should take up two of the questions which do seem to come out of the preceding discussion. One concerns

what the minimal number of variables would be in a good macro-economic model for a central bank. The second is whether a VAR can do the job.

A Minimal Size for a Model

How many and what variables should appear in the macroeconomic model? One can't really give an answer to this that is independent of economies. But for economies in which trade and financial flows are important, and it is felt that stock-flow interactions cannot be ignored, observation suggests that the minimum size is 15. Table 2 contains these variables.

Table 2: A Minimal set of Variables in a Macroeconomic Model
GNE
Domestic GDP
Foreign GDP
Domestic price Level (CPI)
Foreign prince level (imports)
Terms of trade
Domestic Short term interest rate
Foreign short term interest rate
Domestic Asset price
Nominal exchange rate
Domestic Potential Output
Unit labour costs
Financial asset stock (money?debt?)
Govt expenditure
Capital Stock (Household Wealth)?

As soon as one sees this list all sorts of objections arise. Import prices are not the same as foreign prices if only due to tariffs. The CPI cannot adequately represent production prices which would be the likely variable to appear in a real exchange rate index, But it is the CPI that is generally the target of policy and so it has to be there for any policy discussion. At least one asset price seems crucial but it is not clear which one to use. Mostly theoretical models have used equity prices but in many countries in which household wealth is primarily held as dwellings, it is has become clear that housing prices are a key element in macro-economic outcomes. Then one faces the dilemma that the variables that are most important to the cyclical responses of housing prices are vacancy rates and this may demand an expansion of the model to capture these. One might also query whether one can just focus upon GDP. Consumption and investment expenditures rarely go in exactly the same way during any cycle and so it is almost inevitable that anyone who just chooses to work with GDP will be queried about which of these components the given change in GDP is due to. It should also be noted that one may need to dis-aggregate by sector as well as by the nature of demand e.g. the MAS model has a number of sectors.

Should money appear in the system?. It has been effectively dropped from most models nowadays simply by treating the interest rate as predetermined and so endogenizing money. One can't help feeling that this is a bit premature. It may be that the way the ECB wants to deal with it is rather unappealing but one has a gut feeling that liquidity problems will arise some day and the degree of money-ness will be important to cyclical outcomes. Having said that I suspect the influence is really a threshold or non-linear one and would be handled in a particular context by adjusting the intercept in the ECM terms, in a similar way

that most modellers handle a wide variety of other factors that they think are only occasionally important to the model outcomes (we say more on this below).

Can we get away with smaller models?

Small is beautiful still appeals as a slogan. The small new-Keynesian macro models that are now very popular in places like the SF Fed do appeal as a way of thinking about the broad issues of policy rather than perhaps the details of a particular decision. This suggests that we want to be able to move between the larger core model and such smaller representations, particularly when the core model has an ET basis. The ability to aggregate and dis-aggregate very rapidly was an important one when using CGE models for tariff analysis in Australia. In the CGE literature this was done by performing linearization in the variable space so that aggregation could be done to any level desired. It's not quite as easy with macro models due to the presence of dynamics but one can still use simulation methods to perform the task. Let there be m variables and shocks in the smaller model and n >>m in the core model. Simply generate synthetic data using the nxn impulse responses for all n variables to the n shocks and then estimate the smaller model with this synthetic data.

Do we get anything from VAR's?

Once one sees a list of variables as in Table 2, and realizes that these are very much the minimal set that any monetary policy discussion would focus upon, I don't think anyone could seriously argue that VAR's should be used as the core model for monetary policy work. Trying to fit a VAR with 12-20 variables and quarterly data is never going to produce much of value. Of course the number of variables could be reduced if some were assumed to be exogenous but that goes against the stance of a lot of VAR proponents. Sims had larger monthly ones with 19 (?) variables at a maximum but I think I have the record of 10 with quarterly data. Even if one could come at the idea that one could think about sensibly estimating a system in which the lags of every variable are present in every equation, the problem with VAR's is that they don't have enough capacity to produce interesting accounts of what is responsible for a projection or a policy action.

I have lots of problems with VAR's. One is that the driving forces in these systems are uncorrelated shocks but it is never clear why one can make such an assumption. Mostly it's treated as if it's innocuous, but that's not so. It is an *identifying* assumption in the same way as deletion of a lagged variable is an identifying assumption. One might be prepared to concede that the shocks are uncorrelated if the system was a very large one but in practice its hard to believe that (say) the only variables agents use when making a guess about monetary policy actions are the six or so variables making up most VAR's. Most economists advising on Fed actions would be out of a job. This often means that the error terms of "structural VAR" equations have common components due to

the omission of some relevant variable from the system and that can produce quite large distortions in policy analysis. Giordani (2001) is a good example. Here the equations of the true structural VAR involve output gaps but an investigator follows Sims et al and uses the level of output so that the errors in the structural equations have a common component – the potential level of output. Imposing the assumption that the errors in the levels system are uncorrelated produces a mis-specification that shows up as a "price puzzle" that was not present in the output gap system. This can be thought of as a case where there is a missing variable in the output levels VAR. It's the principle that an undersize system can invalidate the uncorrelated errors assumption rather than the precise application that is important.

There are other problems with VAR's that also come from their highly aggregated nature. One is that it is unlikely that a VAR can ever fully capture the responses of an actual economy since the number of variables entering into the system are going to be far higher than used in the VAR and, unless there are some fortuitous cancellations, a VAR(1) in n variables will become a VARMA process in m, where m<n (a result pointed out almost three decades ago by Zellner and Palm). The VAR empire's (or maybe it is an Axis) solution to this seems to be to suggest that one can always approximate a VARMA process with a VAR of high enough order. Whilst theoretically true, is it likely that one can ever make this order higher enough with our limited data sets?

How high an order of VAR might we expect to have to choose if it is to represent a given economy? To examine this let's assume that the N.Z. economy can be represented by the FPS model. We will choose six variables to work with in the VAR; these were broadly similar to the six that the RBNZ utilized in one of their VAR studies.

- Short term interest rate
- Real Exchange Rate
- Inflation Rate
- Terms of Trade
- Aggregate Demand
- Foreign Demand

We simulated FPS with six shocks which the RBNZ modelers said that they thought the estimated system was capturing:

- Interest rate
- Real exchange rate
- Inflation
- Terms of trade
- Domestic demand
- Foreign demand

We then fitted a number of VAR models to gain an appreciation of how well the VAR's could capture the true impulse responses There are a number of ways that this might be done. Here we simply make the approximating VAR(K) match the impulses of the FPS model exactly up to K'th order and then study the approximation error from then on. Graphs at the end of the paper show this for the impact of a transitory interest rate rise and a foreign demand shock. For the interest rate shock VAR(2), VAR(6) and VAR(10)'s were chosen and the fit of the latter with the FPS responses is pretty good. The situation is much less encouraging for the foreign demand shock where a VAR(15) was also used. The latter seemed to be far worse than the VAR(10) at approximating the FPS reactions.

It's clear that it is likely to be very difficult to represent an economy with a VAR of an order that is usual with most quarterly data exercises. One could probably make the same point about the ability of small systems like the RBA's to capture an economy, but there one is conditioning upon a lot of variables and I suspect that, if VAR's are to have a role, then they will need to do likewise. They also probably need to start working more with identities. But then we are returning to the traditional structural equation model and we might as well just forget about the idea of refusing to put zero coefficients on some lagged variables.

Does it matter which core model we use?

In practice all the models above are subject to adjustments when used in an actual policy cycle context. Sometimes these are to capture variables that may either be important in particular episodes but not at every point in time or which are hard to endogenize. An example might be business confidence. Conceptually one could add these into ECM equations as deviations from their means with coefficients that need to be set in some way. This formulation means they have no impact in the steady state and mirrors frequent statements by policy makers to the effect that some variable such as confidence is now well above its historical mean and so it has to be accounted for. However, instead of entering the variable directly into the ECM, it is more common to capture its effects by adjusting the value of the intercept in the ECM model.

Intercept adjustment in the ECM has become the standard way of handling a variety of difficulties that can arise in using the model for policy analysis. Thus if one believes that the log of the equilibrium ratio of Y_t to Y_t^* ,which is embodied in the ECM, is more likely to be c_1 than c for a few periods, then this belief can be incorporated by adjusting the intercept accordingly. If there is a permanent shift in the ratio then the intercept would need to be adjusted to its new permanent level. If one ignores this then there will be forecast failure since the ECM is adjusting y_t to the wrong equilibrium position. Even if c remains constant the intercept can vary in the face of non-stationarities. Thus, if we think

of the growth rate of Δy_t and Δy_t^* as being both g in the steady state, so that there is a constant ratio in steady state, then (2) could be written as

$$\Delta y_t = g(1-a)+a\Delta y_t^*+b(y_{t-1}-y_{t-1}^*-c) \qquad (3)$$

so that any shifts in the steady state growth rate are reflected in the intercept. Once again one needs to pay a good deal of attention to the intercept in order to accommodate such shifts. Most models are designed to have a particular g built into them and forecast failures come if any changes in g are not recognized.

The ECM is a highly flexible tool that makes models very adaptable. But this very adaptability raises the issue of whether the particular model we use is going to matter much (except that the utility of some decision-makers may be increased by consuming a particular brand of model). One's impression is that central bank staffs spend a lot of time adjusting these intercepts, and it makes one ask whether it is the intercept adjustments rather than the model which dominate forecasts and policy analysis over the two year or so horizon that most central banks are concerned with. Checking out the relative contributions of the model and the adjustments to a forecast (or a policy action) should be performed on a regular basis, and it would be interesting to know if the users of core models like FPS and QPM perform such adjustments any less often than those who have models more loosely based on economic theory, such as the RBA. . For those models that feature a steady state solution, seeing how well y_t^* tracks y_t seems to be important as a way of ensuring that the intercept adjustments are not just compensating for a flaw in the steady state specification of the model. It has always surprised me that this computation does not seem to be done very much by the model guardians.

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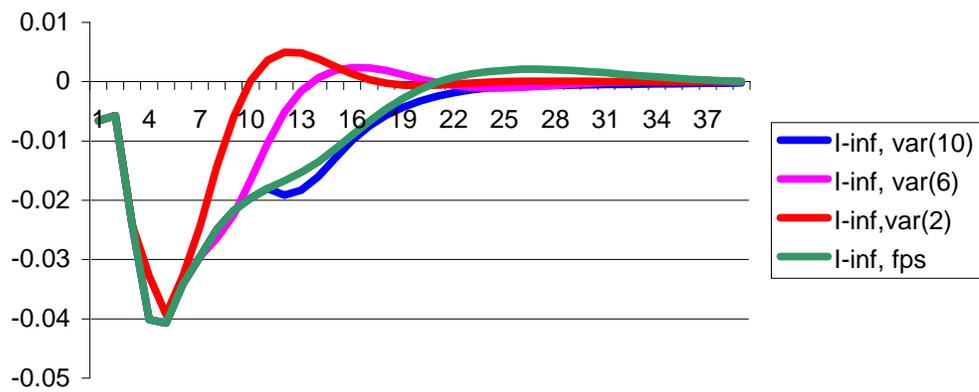Clements, M.P. and D.F. Hendry (1999) Forecasting Non-stationary Economic Times Series (Cambridge University Press)

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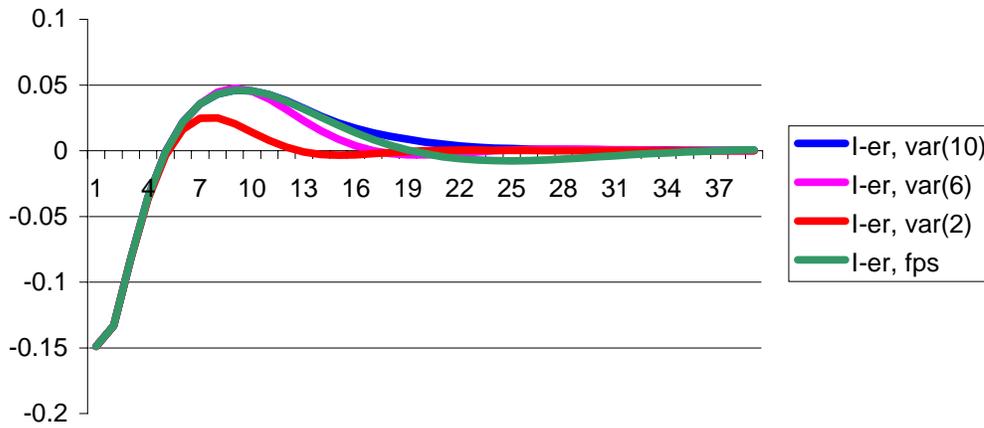
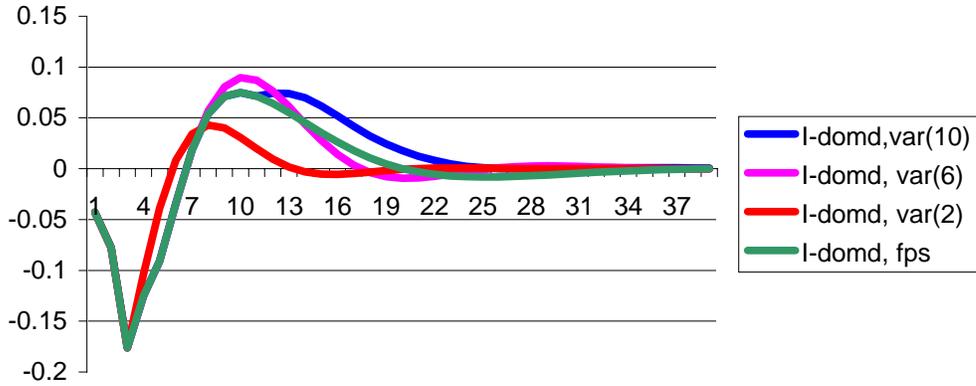**Inflation Response to Interest rate shock, Approximating VAR's**

Dom Demand response to interest rate shock, approximating VAR's

Legend:
- I-domd,var(10)
- I-domd, var(6)
- I-domd, var(2)
- I-domd, fps
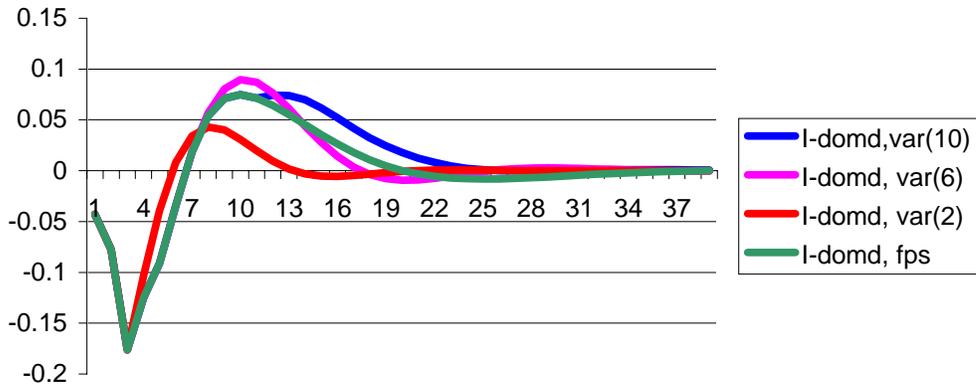
ER response to int rate shocks, var approx to fps

Legend:
- I-er, var(10)
- I-er, var(6)
- I-er, var(2)
- I-er, fps

Dom Demand response to interest rate shock, approximating VAR's

Legend:
- l-domd,var(10)
- l-domd, var(6)
- l-domd, var(2)
- l-domd, fps

Response of Domestic Demand to Foreign Demand Shock, Approximating VAR's

Legend:
- ford-domd, fps
- ford-demd,var(10)
- ford-domd, var(15)