between different types of manufactured goods, or between different sectors. Thus the usefulness of the RBNZ model is heavily dependent on the exact nature of the policy question under study.

References

Carey, D.A. [1984], "Experiments with a Revised Core Model", *Discussion Paper G84/1*, Wellington: Reserve Bank of New Zealand.

Clements, R.T. [1984], "A Re-estimated Production Sector for the Core Model", *Discussion Paper G84/10*, Wellington: Reserve Bank of New Zealand.

Grimes, A. [1984], "Alternative Exchange Rate Strategies", *Discussion Paper G84/8*, Wellington: Reserve Bank of New Zealand.

Kek, I. [1984], "Overseas Private Capital Flows: An Equation for New Zealand, *Discussion Paper G84/6*, Wellington: Reserve Bank of New Zealand.

Wells, Graeme, Brian Easton and Linda Kay [1983], *Economy-wide Models of New Zealand: Final Report*, Wellington: New Zealand Institute of Economic Research, October.

Discussion on RBNZ Model

Brian Silverstone (University of Waikato) opened the discussion on the Reserve Bank model by thanking Robin Clements for his contribution. He referred to Robin Clements' comment that "the potential value of the RBNZ model for analysing policy questions is that the model provides a consistent macroeconomic structure which forces the analyst to specify explicitly a perception of the simultaneous relationships which exist in the economy". Now while the Bank model might be, in some sense, "consistent", Brian Silverstone thought the potential policy-making value of the Bank model was eroded by "a perception of the simultaneous relationships" which treated significant aspects of government behaviour as exogenous. This comment also applied to most of the other models in the *Report*.

Exogeneity was used in economics in at least two related senses. There was the model-building sense in which an exogenous variable influenced other variables but was not in turn influenced by them. This was the usual meaning of exogeneity, and conversely for the definition of an endogenous variable. On these definitions there was universal agreement that *private* consumption expenditure, for example, was an endogenous variable: consumption influenced income and income influenced consumption. Yet significant components of *public* consumption expenditure in the Reserve Bank model appeared to be treated as exogenous variables in the sense that there was no direct feedback from the "state of the economy" to the components concerned.

Exogeneity was used also in the policy-making sense (by Friedman, for example), to describe a variable which was assumed to be controlled or dominated by the authorities, for

example, the money supply. In this sense, policy-makers were assumed to be able to control the variable or the "instrument" within narrow limits and possibly independently of the state of the economy. Conversely, endogeneity in the policy-making sense was used to describe an instrument, say the monetary base, which was systematically influenced by the joint or "simultaneous relationships in the economy", and especially by the "targets" of policy.

Now in the version of the Reserve Bank model published in the *Report*, the variables annotated by the authors as "instruments" appear to be of the "policy-exogenous" type. The list included the exchange rate, the yield on short-term government securities and a number of income and sales tax rates. As far as he could determine, the exchange rate, for example, was not influenced by any variable which might be thought to influence the rate such as domestic relative to foreign prices, the level of reserves, the state of the balance of payments or interest rate differentials.

Brian Silverstone said he was particularly interested in policy endogeneity (or exogeneity) as used in the literature on policy reaction functions. Policy reaction functions were equations which attempted to find evidence for the systematic setting of "instruments" against "targets". As he had prepared a note on reaction functions for the Seminar [which appear elsewhere in these *Proceedings*], he would not repeat the details in this discussion. In his note he had referred briefly to the work done at the Bank by Grant Spencer and Arthur Grimes on reaction functions. Despite this work, however, he wanted to encourage the Reserve Bank and other modellers to make greater use of the reaction function methodology as a guide to formulating policy. The literature was now developing in important areas including simultaneous modelling, game theory (including rational expectations) and public choice (including political economy).

Richard Smith (Reserve Bank of New Zealand) said that deciding which variables in a model were to be endogenous or exogenous was often a problem. Brian Silverstone was talking about policy reaction functions, but in the Bank model they were talking about the reactions of the authorities. It could be argued that as the Reserve Bank model belonged to the authorities it is they who were reacting to what was going on in the economy. If that was so, why should the authorities try to model themselves? After all, they could see what was going on in the economy and could use the model to run all sorts of simulations.

Robert Buckle (Victoria University of Wellington) thought that the reaction function issue was an important point. He thought it was not entirely a defence to say, in effect, that the question of reaction function specifications need not apply in a model constructed from the policy-makers' perspective. The Reserve Bank was modelling the same world as anyone outside the Bank. He thought the Reserve Bank would have to be concerned about what specification errors resulted from incorrectly modelling government behaviour. It did appear, for example, that some components of government expenditure, such as nominal capital expenditure, were endogenous and therefore should be modelled endogenously.

He wanted to relate the Reserve Bank simulations under alternative exchange rate regimes to the idea of modelling endogenous government behaviour. Presumably the simulations were modelled in the same structure outlined in the paper. In that case, there could be no reaction to nominal exchange rate changes. It had been the experience from a number of studies that quite often it had been the case that nominal exchange rate changes had provoked a policy reaction. In fact, in this situation monetary policy was directed primarily at controlling the exchange rate rather than leaving it free to

move. There was no way he could see this being taken account of in the Reserve Bank model structure. Surely it would make some difference to the results generated in the paper if this feedback from the economy to the exchange rate was considered.

But even if this specification was taken into account in the Reserve Bank model he doubted whether it could be very easily modelled because the Reserve Bank model was a single-good model. A number of difficulties with flexible exchange rates arose because an economy had a number of sectors and exchange rate changes were going to impact on sectors in different ways. This was possibly why one got a policy reaction to it. So until the Reserve Bank had a model with more than one good, they were going to have some difficulties modelling exchange rate changes.

Brian Easton (New Zealand Institute of Economic Research) said there were two implications about reaction functions: modelling effects and estimation effects. The modelling effects had been mentioned already. As regards the estimation effects, if it was believed there were government policy reactions, then some of the instruments being treated exogenously in the Bank model were not valid instruments.

While he was very supportive of Brian Silverstone's point, he wanted to correct a small mistake where he had stated that, with the exception of the PEP models, the *Report* did not discuss models with reaction functions. The Haywood model included a reaction function in that the balance of payments was related to employment and the debt level. The Bailey-Hall-Phillips (BHP) model had a function of sorts as did the Wells-Evans (WE) and Easton's (CORA) models.

He noted that it had been necessary to add reaction functions to his CORA model to obtain a long-run equilibrium

condition. Robin Clements had stated that the next version of the Reserve Bank model would be more neo-classical. Very often now - and this came out of the BHP model - there was a long-run equilibrium setting in models. So if you happen to have a neo-classical specification with a long-run equilibrium, it may be necessary to have government reaction functions built in.

Murray Horn (The Treasury) said one thing that troubled him about models, and therefore reduced the amount of importance he gave to them, was the extent to which they had changed over the years. The Reserve Bank model had changed dramatically. These models were estimated on each occasion in terms of the Bank's criteria for selection as being consistent with past behaviour. Yet they had quite different implications for policy. Models, then, were necessary but not sufficient as far as policy-makers were concerned. There was a problem, then, if you have more than one explanation for past events and they have different policy implications. His general point was that it was possible to have two model structures, - which were to all intents and purposes consistent with historical experience - but which have different policy implications.

Robert Buckle took up the point which was raised about the Reserve Bank model on page 44 of the *Report* where the comment was made that "RBNZ is not well-structured to handle input price shocks in terms of their supply-side effects". This was a criticism which could be levelled at most of the models that were reviewed in the *Report*, perhaps with the exception of the PEP suite of models.

He asked himself how well these models could handle the 1972/73 commodity-price shocks, the 1974 and 1979 oil price

shocks and large terms of trade changes. All of these events would be of relevance to modelling a small open economy like New Zealand. When he looked at the BHP model, for example, it was a single-good model and so he did not think it could serve his interest in small open economy modelling, although it could serve other purposes. Likewise, the Reserve Bank model was a single-good model. These models do take account of external price changes, but in varying ways: some have a foreign exchange constraint, others do not; some have relative price effects, others do not.

This issue occurred to him as he looked at the exchange rate assumptions in the Reserve Bank model and the way the Reserve Bank picked up import price shocks. What would happen if there was an increase in import prices in the Bank model? The immediate impact was on the Consumers Price Index. That was the only direct impact other than through the monetary effects on the balance of payments. Because prices rise there was an increase in government expenditure. So the net effect of an import price shock in the Bank model was rising inflation but maintained output along the path that would otherwise have been taken.

This was a rather different result from a lot of other macro models in which stagflation was the result of this impact. In other words, the supply curve shifts to the left if you are modelling supply. If you are not modelling supply, you will not obtain supply-side effects. Stagflation could be generated in models that take account of supply-side effects. Now this may not be appropriate for New Zealand. But it was certainly a result that had been generated in a number of overseas studies.

More to the point, there had been some empirical work in New Zealand. Wells and Evans (WE) had simulated the effect of external price shocks. The results they obtained for price

and output paths were quite different from those generated by the Bank model. So what we have at the moment in the Reserve Bank model are imports which are not part of the production process. He would have thought that in the New Zealand case import prices would directly affect the price of output. This was not picked up in the Bank model. He thought that if it was, the result would have a dramatic influence on the simulations under alternative exchange rate regimes.

Robin Clements (Reserve Bank of New Zealand) commented that import prices did impact on the user cost of capital in the Bank model, but he thought that this effect was not particularly strong.

Brian Easton noted the point which others had made, that the actual estimation period over which the Reserve Bank model had been constructed was characterised by very substantial structural changes, such as the rise of manufactured exports and resource developments. He was beginning to worry whether a process of estimation over twenty years, and covering an economy which had undergone considerable structural change and which was essentially modelled as a single good, was likely to be a proper replication of the record. This was one of the reasons why the *Report* suggested the case for a Reserve Bank-PEP (JULIANNE) "marriage" in order to get some idea of the structural changes that were occurring inside the production sector.

Departmental Perspectives

MURRAY HORN
The Treasury

I have been asked to talk about the uses that have been made, or could be made, of economy-wide models for policy development and analysis. Treasury has continued to use models in a number of ways and in my experience this has occurred in three areas. First, in the study of "structural" issues, such as the impact of the large projects on the economy and tariff protection, Project on Economic Planning (PEP) models have been used. These models have been helpful as a pedagogical device to illustrate the economy-wide impact of sectoral policies. The large projects can be "tacked on" and, in the case of tariff protection, we can obtain some idea of the impact of protection in one sector for output, employment and policy in another sector.

Secondly, on the macropolicy side, we have mainly used the Reserve Bank model - but sometimes PEP models - to study the consequences of macro policy changes such as budget measures, exchange rate changes, wage-tax deals and the consequences of the wage freeze. Here again there are pedagogical advantages in having a model around you whose assumptions and logic are explicit. This helps to ensure consistency and logical completeness. It forces you, if you disagree with it, to be much more explicit about your own assumptions and logic.

In addition, models of the Reserve Bank type are useful in indicating the relative importance of various transmission mechanisms. You might be considering a policy option and you might have some idea of the way it works. You go to the

model, run it, and it comes up with something that looks different from what you expected. What were the reasons? Was insufficient weight given to certain transmission mechanisms or were certain factors ignored?

The third area where we have used economy-wide models is for forecasting. This has included using the models to help separate the impact of policy changes from exogenous shocks.

How are we likely to use these models in the future? As far as Treasury is concerned, the decision is basically a cost-benefit one. There are a lot of activities competing for Treasury Officers' time, and building or using models is extremely expensive. Even just keeping abreast of the PEP output and changes in the Reserve Bank model takes considerable resources. Since I have been at the Treasury there has been a cooling of interest in using these "general-purpose" economy-wide models to address specific policy problems. This is due to a number of reasons.

First, what New Zealand model is appropriate? We face problems when more than one model is consistent with historical experience. We also face problems trying to illustrate the consequences of policy initiatives which are outside the range of historical experience. This is the so-called "Lucas Critique" of econometric policy evaluation.

The second reason behind a cooling of interest in these general-purpose models relates to a change in our attitude to the way we approach policy problems. On the macro side there has been a growing awareness of the dangers of "fine-tuning". So now there is much less emphasis on the short-term in determining policy choice. On the micro side, our interest in public sector activity has shifted away from setting investment criteria for commercial activity, towards attempting to put state-owned enterprises on a more commercial basis and

letting the managers of those enterprises make their own decisions.

Thirdly, a much greater effort in the Treasury has been put into improving the flexibility of the economy by considering specific interventions by the government and how they impair flexibility. We have put a much greater emphasis on coming up with specific solutions to specific problems. These three reasons have tended to lead us to rely more heavily on developing a framework for thinking about specific issues and research addressed to specific policy problems rather than devoting time to "general-purpose" models and trying to squeeze our problems into them. I guess the biggest change is that rather than trying to make our problems fit a model, we are spending more time trying to make models, or the research, fit the problems.

JOHN YEABSLEY

Department of Trade and Industry

We in Trade and Industry are in are in a different position from the Treasury. They complain about the lack of resources to be able to track models; we have got even fewer resources. Most of our resources are devoted to implementing policy decisions of the past. Nevertheless, it is possible to discern from the kind of work we do, two broad areas where we could have past made more use of economy-wide models. These are basically to do with the macro implications of micro issues.

The first relates to the various major projects. When investments of this kind are examined in the policy context you can usually employ some sort of cost-benefit analysis.

This demands a lot of assumptions which are basically that the most crucial parameters in the economy - especially prices - remain fixed in reaction to the project. If these assumptions cannot be believed (as is clearly the case for a number of the major projects), it becomes a question of moving towards a general model that covers the whole economy.

The other area we are involved in relates to the general implications of microeconomic policy, for example, shifts in government policy towards more flexibility in terms of the functioning of micro markets, and what impact that decision is going to have on the whole economy. Here I think we are not necessarily looking for macroeconomic numbers that we can offer as part of hard policy advice, but rather I think we would be happy if we could get some results that illustrate the sort of outcomes that may eventuate.

Against that brief background, I can speak briefly about some of our practical expérience, which has not been particularly positive. This is mainly because modelling takes considerable time and effort. Even commissioned work takes time and effort as general models have to be tailored to answer the specific questions under examination. Public policy advisors do not just have to advise, they have to be able to show why and on what basis they are advising. This means that the transmission of technology about models to policy-deciders takes time and effort.

The second problem is that existing models seem not, on closer examination, to capture the effects that are of interest, for example, the consequences of increased flexibility. It is very difficult to estimate what is going to happen with even relatively minor shifts in assumptions. Alternatively, some models are invariant to policy changes. Where does this lead us? I am led to repeat the conditions mentioned by Richard Manning in an earlier session at this

Seminar, namely that any model should be consistent with acceptable theory and should be capable of replicating the world. The basic point is the extent to which models capture theory and reality. Let me give a couple of examples.

When the first of the major projects was commencing - and New Zealand Steel was the first to come to our attention - we tried to do some work on their projections. We were worried about the consultants' report and we approached PEP for guidance. It turned out that we would have had to reconstruct large sections of the PEP model. This has now been done, but at the time it was not available and we did not have the time to respecify the model. We were caught in a time-and-analysis bind.

I have another example. Again it relates to PEP, but I am not picking on them; their work has been pioneering. In 1982 work was done for the "Round Table" on export incentives. Again we found that some of the effects we had been building into our thinking could not be captured in the model because of the way the models were set up. This did not, however, stop us in engaging in some useful debate with the PEP modellers.

So the basic point I am trying to make is that policy advice is a highly competitive business. Ministers' ears do not open and close to the words from their permanent heads. Our job, given the tight rein we are on, is to work with timetable and resource constraints and various historical biases and, in particular, those biases about how the economy works. As a result, policy-makers tend to use whatever analytical techniques are readily to hand because they are engaged in a constant struggle to keep their advice in front of ministers or policy-deciders. In particular, in the modelling context, the key trade-off is between the costs (in terms of time and resources) of the "tailor-made" solutions,

which meet all the requirements but take considerable time and effort, and the "off the peg" solutions which are less costly and more timely, but which are not directed specifically at the problem and may miss many of the important issues.

I would like to be able to make a lot more use of explicit models because they have their advantages, especially the pedagogical ones Murray Horn has mentioned. They illustrate what you are doing; you can experiment and show "what would happen if". It is nice to have numbers, to point your finger at the transmission mechanisms, to check out assumptions, to look at options and to check out consistency and logic.

However, the conclusion that I am drawn to is the one Murray Horn has reached. The problems we are looking at these days are almost always probably going to need lower level, more *ad hoc*, less numerical, and definitely less well documented, "one-off" projects.

JOHN CULY

Ministry of Energy

The Ministry of Energy uses macro economic models in a different way to many other Departments represented here today. First, and most successfully, economy-wide models have been used predictively to provide an important input for the planning of energy investments and energy research. Secondly, economy-wide models have been used to investigate the impact of various energy policies such as energy self-sufficiency or alternative energy-pricing policies. I would like to discuss briefly each of these types of use.

Energy investments are typically very large-scale and have

long lead times. It is necessary to make long term (10-15 years) forecasts of future levels of energy demands and prices, for example, in order to make decisions on these investments. A major determinant of energy demands is the level and structure of future economic activity. The Ministry has made extensive use of economic models, of various levels of sophistication, to provide forecasts of the economy. In general the models used have been simple aggregate macro models, such as the Haywood model, developed for other purposes. These have been supplemented, on occasion, by the results of more detailed sectoral models. This is necessary for long-term forecasts since changes in the structure of the economy can be expected and energy intensities vary considerably from sector to sector. There is scope for much more work in this area if energy demand forecasts are to be improved. In addition, it is recognised that interactions between the energy sector and the rest of the economy can sometimes be important. Macro models used in conjunction with energy planning models could provide an estimate of this linkage and therefore the ability to allow for this linkage in energy-investment planning.

In addition to this application relating to specific investment decisions, macro models are being used extensively in a project to develop very long-term scenarios of society, the economy and energy supply and demand. These scenarios are being developed to assess the robustness of energy research and development planning, energy depletion policy and other related policies which are likely to have an impact on the long-term development of New Zealand's energy resources. Two macro models are being used.

The first model is a simple four sector CGE model (LENZ) developed by the Institute of Economic Research specifically for this project. The second model is an input-output model (INNOFLEX) which interpolates the detailed structure of the

economy and the energy sector from the broad features described by LENZ. The two models are used in an iterative manner to trace out possible development paths for the New Zealand economy and the energy sector. These are guided by given scenario prescriptions which include assumptions about the world social, economic and political development and New Zealand's response. The use of these models provides a framework to ensure that the specialists on the "scenario team" are confronted with some implications of their assumptions and that basic linkages are not overlooked. This allows for a detailed, internally consistent, picture of a possible path for the New Zealand economy and the energy sector to be built up.

The second major use of macro economic models has been in the analysis of aspects of energy policy and of the impact of major investments. An example in the analysis of energy policy was a study which aimed to assess the impact of fluctuating oil prices on the New Zealand economy and the possible benefit (or cost) of policies such as stockpiling, self-sufficiency, or pricing, which would partially smooth these fluctuations. Two macro economic models were used to examine this problem. However, they were found to be inadequate because they did not satisfactorily model the adjustment processes and the effects on expectations and investments which are central to this issue.

Another example of the use of economy-wide models was the examination of the wider implications of some major energy investments. The Ministry of Works National Impact Model (NIM) model was used to look at the possible regional and national impacts of the proposed Gas Liquids Extraction developments. In addition, macro models were used (although not explicitly by the Ministry) to assess employment and activity multipliers associated with some of the major energy developments (especially those involving export earnings or

import substitution). Most of these estimates were highly debatable, and implicitly involved assumptions, among other things, concerning the extent to which such projects crowded-out alternative investments, and the profitability of those alternatives.

Although economy-wide models have not proved to be particularly successful in the analysis of the key energy policy issues in the past, there are several areas of energy policy in which economy-wide models might be of use in the future. Of particular interest is an assessment of the economic impact of energy shortages. This has major implications for security levels in electricity supply (and hence for investment requirements), and for policy on stockpiling and self-sufficiency. Because electricity and other energy forms are used so widely, and because they have such few substitutes in the short term, major shortages could have a significant effect on all aspects of the New Zealand economy. An assessment of this impact is essential if energy rationing, and security planning, is to be properly based.

Another area of interest is energy pricing. Macro models have the scope to assess the impact of changes in energy prices on inflation and the relative competitiveness of key sectors and the possible flow-on effects to other industries.

That, very briefly, covers the past and future use of economy wide models by the Ministry of Energy. Although I share many of the reservations expressed by other speakers, (such as the diminishing return for effort in model building, the "black box" problem whereby implicit assumptions and simplifications are not fully recognised by model users), I still see a value from economic modelling as applied to the energy sector.

There will continue to be a need for models to be used

predictively as an input into energy investment decisions. Because of the long lead times, structural economy-wide models are likely to forecast better than simple extrapolative models. There is also some scope for their use in assessing energy policy options. Some of the problems concerning the use of these models may be overcome by allowing a greater degree of user intervention. This can be done by dividing models into submodels which can be maintained by specialists and can be varied in detail and assumption depending on the application. This also allows the user to be aware of, and to modify if necessary, the implicit assumptions within, and linkages between, the submodels to ensure that the key relationships are understood and appropriately modelled for the particular application.

This is the approach being attempted in the scenario project, and is the approach now being pursued by the Ministry of Energy in its modelling of the technological relationships between the various energy forms. This is an alternative to the grandiose total energy models attempted in the 1970s.

Discussion on Departmental Perspectives

Bryan Philpott (Victoria University of Wellington) opened the discussion by thanking the contributors for their comments. He was particularly interested in the comment made by all three contributors that apparently the "new approach" to policy problems was to use a partial approach rather than a general approach. While he agreed that many problems could be handled by partial equilibrium techniques, many problems could not be handled in this way. He agreed that assessing the New Zealand Steel proposals in the context of an economy-wide model would have taken too long to consider at the time the project was being evaluated. Nevertheless, when such major projects are being considered there still ought to be an interface with economy-wide models, preferably of the JULIANNE type, so that the implications, for example, of import controls and tariffs, could be considered.

Bill Smith (Ministry of Works and Development) supported Bryan Philpott's remark by saying that what made a major project like New Zealand Steel "major" was the fact that it affected the economy as a whole. He was concerned that such proposals might be considered outside an explicit overall framework. Policy advisers do have models and often these models are in their heads. A model that was at least written down was many times better than a model in someone's head, and a model that was well documented was even better [laughter]. This was one of the reasons he became interested in explicit economy-wide modelling.

Murray Horn (The Treasury) countered by saying that there was another way to approach the study of policy problems besides

using general-purpose models. You could say, for example, "We don't know what the growth rate will be over the next fifteen years: it could be zero, negative five or plus ten percent". We might be that uncertain about the range of outcomes. So rather than pouring considerable resources into becoming a little more certain about whether it is minus four or plus nine, might it not be better to put those resources into attempting to find a system that minimises the risk of getting it wrong? It was not a "yes/no" answer for or against models problem. It was a marginal problem about where to put resources.

Brian Easton (New Zealand Institute of Economic Research) said that this view simply amounted to a plea for another model; it might be a model "done in the head" or it might be a model which was not currently available. He said that Murray Horn had actually defined a new problem. While this was another way of thinking about it, it actually involved another formal model. Murray Horn, then, had not escaped from a model. He had simply said "Look: PEP, RBNZ and NIM are not doing what we want, let us think about it again". This was fine, but you have still got to have a model to think about it.

Brian Easton followed up this point by saying that he was not sure that final users actually should be running these models anyway. He suspected that the models were really research programmes to enable economists to think a little better about the economy. The Institute of Economic Research, for example, was looking at a particular problem in energy. One of the important issues was asymmetrical loss functions. Most theories about estimation and forecasting depend upon symmetrical ones. Fortunately, some New Zealand work had already been done on the problem of asymmetrical functions, so the Institute was able to think about its research on energy in the context of this work.

It may well be that it is not a question of the user coming and asking "Have you got this?", but asking "Is there actually a research programme that helps me to think better about it?" A good example of this approach occurred in an earlier session in this Seminar. Bob Buckle criticised the Reserve Bank model. But the truth was that the Reserve Bank model had stimulated Bob to think about some problems in a way he may not have thought about them if the Reserve Bank model had not existed. That was how policy-makers should be using models, namely, as a basis for a research programme which enables advisors to think better about general problems in the way they want to think about them.

Allan Catt (University of Auckland) made two points. First, it seemed to him that policy-makers should consider farming out much more work than they do at the moment. He thought there was probably some comparative advantage in having forecasting done by those who are best able to do it. Secondly, as regards the use of models themselves, he thought that policy-makers should not discard using general models. The question was not whether models should be used, but which class of models to use.

John Yeabsley (Department of Trade and Industry), in reply to Allan Catt, said that nobody was advocating that models should be discarded. What he was discussing was the use that should be made of models and he had given some indication of the use that his Department had made of models. The issue was the availability of resources given Departmental work priorities.

On the problem of farming out more work to outsiders, he said it was unfortunate that the marginal rate of substitution between a consultant and an advisor was often quite low. Somebody working in the system was required to appreciate the

strengths and weaknesses of a problem and be able to stand up for it. Under our system of government it was not possible to make an outsider stand up and represent a policy-advisor. Under our system, "outside" forecasts, for example, tend to get thrown out, because somebody will say "where do those figures come from, what assumptions have been made, how does the model work and does it take account of X or Y?".

John Culy (Ministry of Energy) said that in his experience it was possible, under some circumstances, to use figures just as figures. In forecasting exercises, for example, it had been possible to accept forecasts from a variety of outside groups with only a minimal understanding of them as forecasts. Once again it was a cost-benefit issue. If some four or five people, say, have produced a range of forecasts was it really worth the effort to try to understand exactly how the forecasts were put together? For some forecasting questions it might be possible to adopt this approach. However, it is unlikely that this approach could occur with policy questions.

How Much Modelling

DENNIS ROSE
New Zealand Planning Council

Introduction

When I was invited to speak at this Seminar, it was suggested that I talk either about current developments in the Planning Council's National Sectoral Programme or develop some thoughts on how our profession might go about determining an appropriate level of modelling activity. I have decided to focus on the latter question.

Formally, I suppose, the New Zealand profession is interested in seeing modelling activity pitched at a level where marginal returns equal marginal cost, but clearly some considerable difficulties arise in making that test operational. The industry is heavily concentrated and largely publicly funded. Its output, in the form of intellectual property, is in some cases freely available to all who are interested in it, but in other cases it is closely held. Where it is available, familiarisation requires a heavy investment of time on the part of those interested. Also, given the inherently uncertain and qualified nature of the information output from modelling, it is clearly difficult for the interested professional, and even more so for the interested person, to form an accurate feel for the credibility which he or she should accord any particular piece of that output.

There are three basic inputs to any modelling exercise: materials in the form of the data base, qualified labour for

model design, estimation and interpretation and capital in the form of appropriate computing facilities. A few comments are in order on each of these inputs.

The Data Base

Limitations in official statistics has meant that all major models have depended on the time-consuming development of supplementary data as evidenced, for example, by the work over many years of Colin Gillion, the Reserve Bank, and the Project on Economic Planning at Victoria University. This pattern of substantial supplementation of official statistics clearly raises questions about priorities both within the official statistical systems and within the profession at large. One also has to raise a question about the problem of measurement errors in unofficial statistical series. The inevitably *ad hoc* nature of many derived series inevitably involves some degradation of quality - a problem which is all too easily assumed away once the data are printed. Needless to say, this problem does not apply only to unofficial statistics.

Model Specification and Estimation

In almost every case, model specification and estimation has involved many years of skilled professional input developing a model. Typically this has been a downstream adaption of an overseas modelling system to New Zealand circumstances. Given our small size, the general theory of our models "swims up over the horizon". The major gain from downstream adaptive innovation is the opportunity to free-ride our basic development costs. Against this we have to weigh the disadvantages of possibly adopting yesterday's models.

Computing Facilities

The steady development and diffusion of computing facilities

is producing a significant transformation. The original models were built on expensive mainframe computers and were concentrated in a few institutions. The introduction of personal computers and improved conditions of access to larger computing facilities is making it possible for many more economists to contemplate modelling activity.

Our developing data base, the steady progression of the profession along the learning curve of model design, operation and use, and the falling cost of computing, all point to a steady decline in the unit cost of producing whatever it is that modellers produce.

Modelling Output

At this point we need to consider our output. Probably the central point here is that the output of the models cannot be immediately assimilated or digested by the uninitiated. The model operator has to communicate the results of his or her work to the rest of the profession and, if it seems to carry messages which may be of interest to policy-makers, they, and the rest of the profession, have to provide some basis for judging the weight which should be attached to particular results.

In the end, the main tests of the value of modelling output lie in the use which the profession as a whole is prepared to make of them. At this point it is useful to draw on the distinction in the Wells-Easton-Kay *Report* between the forecasting, research and policy analysis functions of modelling systems.

Forecasting, Research and Policy Analysis

On forecasting, it will be unnecessary to remind anybody who has been in the business that there is a steady demand for

predictions about the future. A large part of this demand is, in effect, a demand for a default forecast. The private or public decision-maker, knowing that what he or she is interested in is in some way related to developments in the economy at large, wants somebody to give him or her some idea of how the economy or some part of it may look in future. They know that any such forecast will almost certainly be wrong, but they rely on the forecaster to give them some idea of probable central values and may or may not be interested in error margins and sensitivity to changes in exogenous assumptions and model specification. Clearly there are very severe limits on how far the profession can or should claim to be able to go in this area. Equally, there is a substantial demand, and experience to date is sufficiently encouraging to suggest that we should keep trying.

Research demand comes essentially from within the profession and falls into two main classes. First, there is the specification, estimation and testing of particular functional relationships. The Reserve Bank's real wage-employment equation, and the subsequent controversy, is a classic example. Secondly, there is the study of systems behaviour.

As regards the estimation and testing of particular functional relationships we are, I suggest, in the midst of a significant sifting of evidence and it is likely that in a few years we will have a fairly well developed body of evidence on parameters such as the short and long-run elasticities linking key pairs of economic variables. This is a valuable output and a working knowledge of such parameter values and their stability through time will become part of the stock-in-trade of most professional economists.

Systems analysis present much more complex problems. In particular, there is a difficult choice to be made between

keeping a system sufficiently compact so that it remains reasonably comprehensible and transparent and, on the other hand, extending it so that it more closely mirrors the economy at large. In the latter case we frequently face a problem in distilling the relative importance of particular influences at work within a model. A particularly instructive example of this type of problem was provided by the work undertaken at the Reserve Bank by Grant Spencer and Kevin Duggan on developing alternative neo-classical and Keynesian versions of the core model. The significant variation in results generated by the alternative specifications provided a timely reminder that our models are to a significant extent creatures of the theoretical assumptions which we build in to them.

It follows that the economist who wishes to draw inferences from a particular model has to persuade him or herself that the structure of the model is appropriate to the task in hand. Also, in considering the development of our modelling systems, we have to ask ourselves whether as a group we are spending sufficient time absorbing each other's results.

And finally, a brief comment on the policy question. Typically the policy-maker is interested in securing some feel for the probable impact of discrete changes in particular policy variables upon a range of economic objectives. It is clear that our modelling efforts mark a significant advance from purely algebraic analysis in which inference was frequently limited to judgments about the probable sign of partial derivatives in the system. It is also very clear that we cannot place a great deal of confidence in many of our numerical results. That said, we can reasonably claim that modelling work has helped advance our understanding of a wide range of economic relationships and that, as a result, much policy analysis is more soundly based.

Conclusion

What does all this imply in terms of the central question of the scale and direction of New Zealand's modelling effort? I suggest that it is clear that in large part the decision on how much effort should be devoted to modelling has to be determined within the economics profession itself. It is going to be made by professional economists within their day-to-day working environment making judgments about the relative utility of modelling and other forms of analysis in addressing the problems which confront them.

In this regard we have to place a high priority on openness of communication and promoting interaction between modellers and other economists. The authors of the *Report* comment that "the strength of economy-wide modelling in New Zealand probably arises because a number of different modelling centres have pursued their own visions" (p.292). In promoting interaction there is a need, they suggest, to ensure that models are reasonably documented, allow reasonable access to outside researchers and have a reasonable proportion of research on them made by groups outside the model team.

They suggest "this process of professional scrutiny is a vital part of the nation's modelling evidence. Despite the need to provide additional resources and develop models in certain directions, probably the most important requirement is that all models are subject to continuing pressure to improve their theoretical analysis and econometric methods" (pp.292-93).

Postscript

The period since the Seminar has been characterised by a profound revolution in economic policy-making. The floating of the dollar, reductions in protection and industry assistance, the policy of fully funding the fiscal deficit, and a

range of other initiatives, fall within a pattern. The policy-makers' concern is to get the institutional framework right and to rely on market mechanisms to deliver the appropriate signals.

This revolution profoundly affects the environment within which economic modellers and analysts work. Do we need anything more than a one-equation model where the derivatives of public happiness with respect to regulation are negative, and nothing else is significant over all feasible political ranges? Why persist with more complex econometric measurement and modelling? There are I, suggest, two reasons.

First, it is clear that the hands will, at some point, reach back to some of the policy levers. Then the operators will want as high a level of information as they can get on the relationship between policy instruments and target variables.

In the field of monetary policy, for example, a zero injection of primary liquidity cannot remain permanently appropriate. It is essentially an interim setting within the period of transition from one system of monetary control to another. As the vessel swings over, there is a period during which the control system becomes inert, but the time will come when primary liquidity needs to be adjusted in pursuit of monetary or other targets. The econometric relationships between variables will again become a question of interest.

Similarly the international pressures which are leading the major industrial nations to co-ordinate their macroeconomic forecasts and policy settings are unlikely to pass New Zealand by. Commenting on the Tokyo Economic Summit, the May 1986 issue of the *OECD Economic Outlook* included the comment that inter-country analysis "needs to pay considerable attention not only to the evolution and determinants of exchange rates,

but also to a range of other indications of compatibility or of strain between different countries' policy settings" (p.xviii). The need for a good quantitative feel for the underlying relationships is evident. Moreover, if these initiatives by the major industrial countries are successful, the smaller OECD members are likely to welcome the opportunity provided by a more stable international environ-ment, to return to more active macro policies.

The second reason for persisting in econometric measurement and modelling stems from the fact that the range of people interested in gaining a quantitative feel for relationships between economic variables is very much wider than public policy-makers. A multitude of private goals are promoted by a clearer understanding of a range of economic relationships such as what drives the exchange rate, and the prospects for growth in the output in one sector or another.

At this point we do come up against a major institutional problem. The major developments in modelling have been undertaken within the universities and public institutions such as the Reserve Bank. The product has been developed as a public good. Although the modellers can capture some of the private benefits through contract, use of the models' other benefits are more widely diffused and are properly dependent upon public funding through the tax base.

Indeed, the issue is wider than that. Effective model operation depends upon a substantial private input both in terms of basic statistical data and in helping form perceptions on the appropriate specification of relationships and settings for exogenous data. Moreover, the intellectual worth of any model depends upon its being open and accessible to professional scrutiny and testing. Openness of communication and co-operation are essential elements in economy-wide modelling. Model results only have meaning if one has the

opportunity to "kick out some of the props" which support them.

The advantages of openness can be illustrated with reference to the latest round of the New Zealand Planning Council's National Sectoral Programme. The programme uses two linked models to explore the outcome of a round of consultations with private and public sector organizations. In preparing the runs for the latest publication in the programme (*Towards 1995*) we encountered significant differences between our respondents and our models on major questions such as the likely rate of export growth and likely changes in import penetration consequent on industry assistance reform.

The public exploration of differences such as these is important. To what extent do they stem from differences in access to relevant information, from different theoretical perceptions or simply from errors in logic? Issues such as these need to be worked through in open discussion and debate. The contribution which modelling can make depends on a collective process of information exchange and development. Where identifiable and exclusive benefits accrue, efficiency criteria suggests that the beneficiaries should pay, but it would clearly be damaging were we to misread this principle as implying that the optimal level of model-building should be determined as the sum of privately-appropriated benefits.

The Role of Models in Economic Policy Evaluation

R.G. SMITH
Reserve Bank of New Zealand

Introduction

Over the past decade or so econometric models have become an increasingly popular tool in assessing the merits of alternative macroeconomic policies. The results from policy simulations have gained greater acceptance among a wide range of groups, including policy-makers. However, over the same period that models have gained greater acceptance, the validity of using econometric models for policy evaluation has become increasingly questioned. Much of the more damaging criticism has come not from causal, sometimes ill-formed, observers - who often condemn models as being too simplistic (or too complicated), or too big (or too small) - but from economists well qualified in the area of econometrics and quantitative analysis.

Some of these criticisms have been made in passing such as that of Deane [1981, p.16] who noted that:

> "Hypothetical simulation experiments can reveal as much about one's talent to fine-tune a model as they do about one's ability to replicate economic adjustments which correspond to real life possibilities".

Other criticisms, such as that of Lucas [1976, p.20], have been more direct and damning:

"simulations ... can, in principle, provide *no* useful information as to the actual consequences of alternative economic policies...".

The object of this paper is to outline some of the arguments against placing too much emphasis on econometric models for evaluating alternative economic policies. In particular, the paper discusses the Lucas criticism and presents a simple illustrative example. Following this, approaches designed to overcome the Lucas problem are discussed, along with some of the arguments which have been put forward in defence of policy simulations.

The Lucas Critique

Under current practice, policy evaluation usually proceeds as follows. First, a model of the economy is constructed from available historical data. This model is then simulated under current policies to produce a "control" run. An alternative set of policies is formulated and "fed" into the model by changing the values of the relevant exogenous variables. The model is simulated again using these revised data. The difference in simulation paths between the two runs is taken as the effect of the alternative policies from existing policies.

More formally, an econometric model can be viewed as specifying the state of the economy in the following form:

$$y_{t+1} = f(y_t, x_t, u_t)$$

where y_t is a vector of endogenous variables, x_t is a vector of exogenous variables and u_t is a vector of serially independent, identically distributed random shocks. The function f is assumed to be fixed but not directly observed, hence it is the task of econometricians to determine it. In practice, it is common to fix f in advance, as F say, and

estimate the values of a fixed parameter vector α where:

$$f(y_t, x_t, u_t) = F(y_t, x_t, \alpha, u_t)$$

Having fixed F and estimated α, policy evaluation proceeds as described above.

The crux of the Lucas criticism is quite straightforward. It questions the important assumption, which is implicit in econometric policy evaluation, that f, or alternatively (F,α), remains fixed under alternative policy scenarios. If it does not, as Lucas suggests, then it may be inappropriate to use models for evaluating alternative policies.

According to Lucas, the function F and the parameter vector α are functions of decision rules (that is, aggregate supply and demand functions) of economic agents which, at least theoretically, are optimal given each agent's environment. When policies change, agents' environments change also so we can expect them to modify their decision rules accordingly. As these decision rules change then so does (F,α). To assume the stability of (F,α) under alternative policy regimes is to assume either that agents fail to understand the consequences of policy changes or that agents fail to modify their behaviour in the light of these changes. The available evidence would suggest that such assumptions are not justified.

Because agents' decisions usually will have implications lasting well into the future, the sort of arguments put forward by Lucas are inextricably tied to expectations. Indeed, in the example set out below, it is shown that a failure to account for agents' price expectations seriously biases a simulation experiment aimed as assessing a contemplated change in monetary policy.

Example

The following example is based on the so-called hyper-inflation model of Cagan [1956], and is taken from Begg [1982, p.82ff]. (For further examples see Lucas [1976] or Sheffrin [1983]). The model consists of three equations, with all variables expressed in logs:

$$m_t = m_{t-1} + \sigma \qquad (1)$$

$$m_t = p_t + \alpha_1 - \alpha_2\sigma + u_t \qquad \alpha_1>0,\ \alpha_2>0 \qquad (2)$$

$$e_t = p_t^* - p_t \qquad (3)$$

where: m = nominal money stock
p = domestic price level
p^* = foreign price level
e = exchange rate.

Equation (1) describes a simple money growth rule in which money grows at a constant rate σ. Equation (2) is a money demand function under rational expectations. Inflationary expectations, which influence the demand for money negatively, are proportional to the rate of money growth (hence the term $\alpha_2\sigma$ in equation (2)). The third equation represents the exchange rate and assumes purchasing power parity.

For simplicity, we assume there is no inflation in the foreign country so that $p_t^* = p^*$. Thus, equation (3) becomes:

$$e_t = p^* - p_t \qquad (3)$$

Combining equations (2) and (3) to eliminate p_t the model reduces to:

$$m_t = m_{t-1} + \sigma \qquad (1)$$

$$e_t = (p^* + \alpha_1 - \alpha_2\sigma) - m_t + u_t \qquad (4)$$

Given data from a sample period over which the rate of money growth had been maintained constant at σ (and over which foreign prices p^* had remained constant) the model would be estimated in the form:

$$m_t = m_{t-1} + \sigma \qquad (1)$$

$$e_t = \alpha_3 - m_t + u_t \qquad (5)$$

Now suppose that the authorities wish to know what would happen to the exchange rate should they change their monetary policy. We will consider an extreme case where the authorities are contemplating a policy to keep the money stock constant forever more (that is, $\sigma=0$). Traditional policy evaluation would involve obtaining a set of control forecasts for e_t, with σ set at its historical level, and then obtaining a set of policy forecasts with σ set at zero. By comparing the two sets of forecasts, the effects of the policy change could be determined.

Such a policy evaluation based on equation (5) would conclude that, under a policy of zero money growth, the exchange rate would gradually appreciate relative to the path it would have followed under no-policy change, as the new policy holds down the nominal money stock by ever-increasing amounts, relative to the previous policy. However, by reverting to equation (4) we can see the error in this line of reasoning. Simulating the new policy using the estimated value of α_3 in equation (5) leads to a spurious result, as α_3 is a function of σ and hence changes as σ changes. In other words, α_3 in equation (5) is *not* invariant to policy changes.

Discussion

Although the example might appear rather contrived, it has an interesting parallel with what happens in the applied world of

econometric forecasting. A common practice at the start of a forecast round is to examine the patterns in recent residuals and to use these to revise the estimate of the intercept. For example, if a run of positive residuals arises in an equation in recent periods (that is, the equation is under-predicting) then one usually revises the constant upward by their average amount. In doing this, the forecasters are accepting that the intercept term is liable to change over time. In the example above, it was the intercept term which changed with the new monetary policy.

Another parallel in applied econometrics is the regular refitting of econometric relationships. Econometric models are normally re-estimated for two reasons; first, to try and improve the theoretical structure of the model, and, secondly, to utilise up-to-date data. Even if the theoretical structure (that is, F) of the model remains unchanged, it is usually considered desirable to revise the estimates of α on the basis of an extended data set. Given the sometimes significant movements in coefficients which occur when models are re-estimated, and which model builders usually accept as being quite normal, there is again an implicit acceptance that model coefficients are likely to change over time.

It is clear from the example above that had equation (4) been used to evaluate the new monetary policy, instead of equation (5), then the policy evaluation problem would not have arisen. Thus, there is a need to consider whether the types of problems raised by Lucas are just a matter of model mis-specification, as this example might suggest, or whether the problem goes deeper than that.

Again, the example is able to suggest an answer. Although equation (4) is the "true" structural equation for the exchange rate, it was not possible to estimate this equation because the parameters α_1 and α_2 were not identified. This is

because σ had remained constant over the sample period. The inability to identify α_1 and α_2, in the example, illustrates the proposition that if contemplated policies are of a completely different character to past policies (rather than an extension of past policies) then it is unlikely that the effect from changing to these policies can be captured adequately by even the most sophisticated models. In our example, it was impossible to model agents' price expectations (with respect to changes in the money supply) as the money supply rule had not changed over the course of the sample period.

The converse of this argument is that models are likely to be useful for policy evaluation only when contemplated policy changes are of a similar nature to past policy changes. Indeed, it is on this premise that the defence for using traditional econometric models for policy evaluation is based. For example, Mishkin [1979] argues that most econometric relationships embody the stochastic relationships of variables as they prevailed during the estimation period. If simulations can be designed so that these relationships are preserved, then models can be of some use. Similarly, Sims [1980] argues that many policy actions are exercises within a stable framework so that equations of econometric models may actually be invariant to some types of policy actions.

Nevertheless, it is easy to find examples where models are likely to be of little guidance even when contemplated policy changes are similar in character to past policy changes. For example, if a given policy in the past had induced agents to react in a particular way, but these agents had subsequently regretted their actions, it is unlikely that they would act in a similar fashion if this policy were repeated in the future (that is, they learn from past experience). Moreover, agents are likely to react differently to a given policy depending, in part, on other factors such as the state of the economy, or

their expectations about future states of the economy.

Another aspect of the problem of policy evaluation, also discussed by Lucas, is the likely effect of highly volatile policies. Volatile policies reduce the ability of agents to form expectations and reduce the degree of confidence with which they are held. In other words, rapidly changing and unpredictable policies make the future uncertain. This has the effect of making the structural parameters in models much less predictable so that the task of policy evaluation becomes considerably more difficult.

The type of criticisms raised in the Lucas critique are easily side-stepped. They are often either ignored by model simulators or deemed to be of little consequence (see, for example, Ando [1981. p.353]). Opinions as to the extent to which simulation results are affected by the sort of problems raised by Lucas will differ between individuals depending on their perception and understanding of economics and economic models. Nevertheless, there is considerable support for the Lucas argument. As Lucas notes, his critique had been anticipated much earlier in works by Marschak [1953] and Tinbergen [1952] and is implicit in works by a number of economists including Friedman [1957] and Muth [1960].

On the face of it, it would appear that the sort of criticisms raised by Lucas are likely to be particularly relevant to the New Zealand situation. For example, a key variable which concerns New Zealand policy-makers is private capital formation. However, expectations are likely to play a crucial role in investment decisions as these decisions have important implications lasting well into the future. Thus, models with traditional transmission mechanisms where, for example, increased government spending leads to an increase in GDP which in turn increases investment, are unlikely to produce realistic simulation results for. say, the effects of

alternative fiscal strategies on economic growth.

A second reason why the New Zealand economy might not be particularly well suited to econometric policy evaluation relates to the degree of volatility of past policies. As noted above, volatile policies, which in the past have characterised the New Zealand economy, increase the variability of expectations and reduce the confidence with which they are held. According to Lucas, the movement in the structural parameters of models will be unsystematic and econometrically unpredictable, so that policy simulations under these conditions are likely to be particularly dubious.

Concluding Remarks

This paper has set out some of the arguments against the use of econometric models for economic policy evaluation. While most economists are aware of the type of criticisms put forward by Lucas, some involved in policy simulations choose either to ignore them or to shrug them off as being of little consequence. The extent to which these sorts of criticisms are relevant to a particular model's ability to perform policy simulations, in a given economic environment, is largely an empirical question (and one which is unlikely to be resolved). However, in the New Zealand context, it would appear that such criticisms are likely to be particularly relevant. Thus, the criticisms raised by Lucas cast at least some doubts on the validity of using the currently available models for policy simulations.

References

Ando, A. [1981], "On a Theoretical and Empirical Basis of Macroeconomic Models", in J. Kmenta and J.B. Ramsey (eds.), *Large Scale Macroeconomic Models*, Amsterdam: North-Holland.