ECONOMIC MODELLING IN NEW ZEALAND

Proceedings of a Seminar

Edited by
Brian Silverstone
and
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Foreword

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As far back as 1982, the newly constituted Economic Monitoring Group (EMG) of the New Zealand Planning Council decided it needed more information than was available to it about the capacity of existing empirical models to assist the EMG's analysis of longer term objectives and paths towards those objectives. Consequently the EMG commissioned the New Zealand Institute of Economic Research (NZIER) to review the usefulness of existing models for the EMG's purposes. The result was the 1983 Wells-Easton-Kay *Report on Economy-wide Models of New Zealand* which was subsequently published as NZIER *Research Paper No.33*.

Basically what the authors did was to look at most of the accessible economy-wide models in New Zealand, although a few were excluded, including some of the older models. They tried to do three things: first, to report on each model in its own right, secondly, to consider each model against a specific checklist and thirdly, to illustrate some general modelling problems. They concluded with the view that it was not possible to meet all the EMG's requirements using any single existing model. They did, however, believe that it was feasible to modify existing models so that, in combination, they could be used to meet the EMG's requirements. In particular, they thought a worthwhile development would be some interface between the Reserve Bank model and the JULIANNE model from Victoria University of Wellington's Project on Economic Planning.

The *Report* has already had a fairly wide circulation amongst practitioners. A summary of it was presented at the August 1983 Economists' Conference and subsequently published in an abridged version in the 1983 issue of the *New Zealand Economic Papers*. To encourage even further discussion on the usefulness of the *Report* and the usefulness of models in general, the EMG planned a one-day seminar as part of the overall project.

These *Proceedings*, then, are the outcome of a Seminar held in Wellington in December 1984. The purpose of the Seminar was to address the broad issues of economic behaviour and policy effectiveness and the overall place of models in assessing such matters, rather than detailed questions of specification and policy design. In particular, two policy issues were formulated as a theme for the meeting in order to direct the discussion away from excessive or narrow technical detail. The two issues concerned the impact of real wages and the "major projects" on the economy. As they formed an important theme for the Seminar, it may be appropriate to give some background to these issues.

There is general agreement, as a result of international and domestic conditions since at least the mid-1970s, that the New Zealand economy faces an adjustment problem of some magnitude. The problem could be expressed in terms of a productive structure being allowed to support real incomes which are not aligned with the real international value of output. Adjustment, therefore, was likely to entail either a reduction in real incomes or higher productivity.

It is assumed that the preferred policy objective would be to achieve higher productivity through resource reallocation (or structural change) so that real incomes could be maintained, or even increased. The central issue hinges on the determinants of economic growth. There is, however, room

for debate regarding the mix of policies which could achieve growth and the feasible time path of output and incomes. Many would say that in the short term, a downward adjustment of real incomes is a necessary prerequisite to resuming a more acceptable long-term growth path. At least two issues may be relevant to this adjustment process.

First, in any adjustment process, the time path traced by real and money wages may be considered to have an important impact on the level of production. There may also be important associated equity judgments which could influence political choices of the economic options available. There is, of course, considerable economic debate regarding which policy instruments can influence either real or money wages. Nevertheless, the consequences of different money wage paths are likely to be important information for policy analysis and decision-making. This issue is clearly relevant, for example, in the context of tripartite wage talks.

Secondly, it is widely perceived that due to sustained shifts in the international relative price of energy, New Zealand had experienced a marked increase in comparative advantage in energy supply, mainly in the form of hydro-electric development opportunities and natural gas deposits. Because of this, a number of large-scale investment projects (the "major projects") were conceived and promoted, often with government support. The rationale for these projects was that they would contribute positively to the adjustment process. The consequence of these projects on the economy is, therefore, extremely relevant policy information.

The EMG, therefore, asked the operators of three major models (Reserve Bank, Victoria University and Ministry of Works) to respond to the issues arising from the *Report* and to indicate the potential value of their models in the context of the two issues we have just outlined. In particular, the

modellers were asked to indicate how they would model the issues, describe the economic mechanisms which their model would employ, and, where appropriate, to report on some past research. The modellers were also requested to indicate what relevant issues their model could not handle.

The EMG also asked the representatives of three Government Departments to briefly give their perspective on policy issues in relation to model building. This was to be followed by a number of papers on broader issues relating to modelling and policy.

This ambitious day's programme was largely achieved and resulted in a useful exchange of views. Of course the usual disclaimer applies; namely, that the views expressed by the participants are their own views and not necessarily the views of their employing organisations. Although the *Proceedings* were taped, unforeseen technical problems subsequently revealed that much of the taped discussion was either lost or was inaudible. The editors, however, have done their best to reflect the spirit of the Seminar in these *Proceedings*.

The slight publishing delay has given the contributors the opportunity to revise their papers since the Seminar and most have done so. Unfortunately, the Ministry of Works National Impact Model (NIM), has not been included in these *Proceedings* because a suitable version of the model was unavailable at the time of this publication.

Finally, it is our pleasure to thank Rory O'Malley for handling the administrative arrangements for the Seminar, the participants for their contribution, the Planning Council, Reserve Bank and Department of Trade and Industry for funding the original study and Brian Silverstone and Graeme Wells for the work they have put into editing these *Proceedings*.

Project on Economic Planning Models*

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Introduction

I should like to congratulate Graeme Wells, Brian Easton and Linda Kay for their *Report on Economy-wide Models of New Zealand*. It is an impressive achievement and the economics profession should be deeply grateful for such an important reference work. By and large I have no quarrel with any of the comments made on the Project on Economic Planning (PEP) models and I note that the authors recommend the use of our JULIANNE model for Planning Council purposes especially if it interfaced with a macro model.

The overall brief for this Seminar is the role of models in policy evaluation. In view of the current disposition to regard models with disfavour, I offer the following series of points, not by way of defence, but in the spirit of attack. They are put down for the record as one economist's credo.

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* *A number of other Project on Economic Planning (PEP) papers were presented at the Seminar. These papers described alternative approaches to the "major projects" question (Philpott and Montrivat [1984]) and the "money wage path" question (Nana and Philpott [1984]). A synopsis of other PEP research relevant to these issues is provided in Philpott [1984a]. The present paper is based on Philpott [1984a] (eds.)*

First, everyone has a model in mind even when this is disavowed. Currently it is the neoclassical "invisible hand" framework which, as Hahn [1982] has pointed out, is based on the general equilibrium model, but which suffers from a dearth of unsolved theoretical problems, particularly in demonstrating just how systems get into equilibrium.

Secondly, the "invisible hand" approach assumes much more about universal Walrasian price flexibility in product and factor markets than the actual Marshallian or Keynesian situation of output flexibility. This has led a number of people to question, for example, the free market virtues of an exchange rate float. Compared with the Walrasian timeless price-flexible model of the textbook we should be dealing with the *actual* New Zealand in economy in *actual* time with *actual* resources already committed to various forms of *actual* production.

Thirdly, restructuring takes time; it is dependent on the process of new investment rather than the reallocation of existing capital. To rely on unaided market forces to reallocate may bring the danger of a collapse of new investment and so nothing is achieved. This is just part of the Hahn *tâtonnement* problem just mentioned.

Fourthly, real wage questions, which continue to bedevil us, cannot properly be examined in a macro context. It is at heart a general equilibrium matter, since what we are really concerned about is wage cost compared with capital cost and the latter depends also on wage rates. This is, of course, over and above the general real wage question which should more properly be called the real *income* question which relates to the comparison of the local compared with the overseas price level.

Fifthly, the evaluation of critical parameters in policy

formation, such as the Marshall-Lerner conditions, cannot be carried out except in the context of a general equilibrium model reflecting the actual economy. This also applies to questions relating to equity, as compared with allocation, in trade policy questions, and also to the evaluation of tax incidence.

Sixthly, partial equilibrium cost-benefit type analyses of such major structural changes in the economy as steel industries, energy projects, as well as the evaluation of effective rates of protection, are inadequate unless carried out in a general equilibrium modelling framework.

Finally, we have recourse to the possible authority of the Treasury's [1984, p.131] recommendation for "the establishment of a process by which the costs and benefits of current measures and major policy changes can be quantified...". Such a process inevitably depends on the use of an empirical general equilibrium model and is thoroughly supportable if only because it may ensure confrontation of some of the conventional wisdom with the economic cliché's which often substitute for economic policy.

The PEP and Policy Questions

The Economic Monitoring Group have posed two policy questions for this Seminar, relating to the major projects and to alternative wage paths. We have considered both questions, but space restricts the present discussion to the feasibility of using an economy-wide model for the evaluation of the major projects programme initiated in 1980.

Our results, then, are indicative, rather than definitive, and are concerned with an exploration and exposition of the methodology of such an evaluation in a general equilibrium model. We wish to assure ourselves that the economic mech-

anisms embedded in the model continue to work satisfactorily in this particular context.

Before discussing the present policy question, there are two ways in which our results can be put into context. The first concerns the general policy conclusions which have emerged from a number of studies using models in the PEP suite. The second relates the JULIANNE model to other PEP models which are available.

General Conclusions on Structural Issues

PEP research using Computable General Equilibrium (CGE) models has covered a wide field including the policy questions posed for this Seminar. The following two generalisations are among the major conclusions we have reached.

First, quite a number of our studies have dealt with various aspects of protection and growth. In general, these studies highlight the importance of efficient import substitution as well as export expansion, and reveal some strong arguments for an optimal tariff. Some of our early investigations showed that the benefits of the removal of protection are modest, but these results are strongly influenced by the extent to which protection applies to intermediate goods rather than to final goods.

This conclusion led to a more detailed study, during 1984, of the incidence of protection. This involved the compilation of a very detailed commodity/sector protection matrix which showed that the average level of protection was in fact much lower on intermediate than on final goods. When these new estimates of protection were adopted in the models, even smaller benefits from the removal of protection became apparent. Work is still proceeding on this question, but some results are to hand which cast considerable doubt on the

proposition that the failure to remove protection over the period 1977-84 was a basic cause of the failure to grow. At most, complete removal of protection would only have lowered exporters' costs by about 5%. As described in Shoven and Whalley [1984], the small quantitative benefits from the removal or lowering of protection is a conclusion confirmed by other general equilibrium modellers overseas.

By contrast, efficiency growth appears to be a much more powerful influence on GDP levels and growth than even dramatic changes in assistance rates. Even without a reversal of the last decade's negative rates of efficiency growth, the prospects for 1990, other policy changes notwithstanding, are bleak. Thus, important as improved resource allocation is, it is of secondary importance to efficiency growth in quantitative terms.

Of greater importance, and contrary to conventional wisdom, is the evidence which shows that protection does not appear to be the root cause of our slow growth problem over the last decade, unless protection *itself* has been a cause of declining efficiency. Although this is often asserted, the only empirical evidence relating to this proposition in fact refutes it (see, for example, Campbell [1984] and Philpott [1984b]). Much of the decline in New Zealand efficiency growth has in fact been in the traditional agricultural industry, and this can hardly be attributed to protection.

Secondly, another aspect of our research, relevant to contemporary discussion, concerns the relation between the real and nominal exchange rate, and money wages. Early work with JULIANNE showed that a continuation of trends prior to the 1984 devaluation would lead to a 13% appreciation in the real exchange rate by 1990. Bearing in mind the dangers of wage indexation, the optimal level of nominal devaluation would appear to have been less than the 25% introduced in 1984.

Most of the benefits of the 25% devaluation, it would appear, will have been lost over the next few years, and one must raise the question as to whether nominal exchange rate changes are the most efficient means of achieving our goals as compared with other policy instruments.

Some as yet unpublished work by Adolf Stroombergen and myself shows that much the same level of GDP and consumption (at full employment and with a sustainable balance of payments deficit) might be achievable by 1990, either with a continuation of protection and subsidies on the one hand, or with abolition of assistance and massive real devaluation on the other. If we bear in mind the big problems just alluded to in securing in the future the *real* devaluation (and that the wage and price policies associated with the pre-devaluation era were in fact securing a small real devaluation), then we should be careful in unequivocally supporting the exchange rate route.

The PEP Model Suite

For the evaluation of the "major projects" policy question, we use JULIANNE, which is one of a number of models developed by the PEP. The earliest, VICTORIA, is a linear programming model and involves the optimisation of, say, real consumption expenditures, subject to sectoral and intertemporal inequality constraints on resource allocation. JOANNA is a multisectoral CGE model which has its intellectual origins in the general structure described by Johansen [1960], and has a number of features in common with the Australian ORANI model described in Dixon *et al.* [1982]. It is linear in percentage changes, and is very easy to solve, but it does not have a precise calendar-time interpretation.

JULIANNE is a CGE model which is solved in terms of the levels of the variables. It shares almost the same data base

as JOANNA, but incorporates a number of differences in the way economic behaviour is modelled. These differences are described in Wells *et al.* [1983], as well as in PEP *Discussion Papers*. Finally, JUDY is a time-staged version of JULIANNE solving for general equilibrium within each year, with capital stocks updated in the light of previous investments, which themselves are determined by relative sectoral profit rates. The relationship between models of the JULIANNE and JUDY type is described in Dervis *et al.* [1982], as well as in Philpott [1984a].

Turning to our present application of JULIANNE, the data and general assumptions are described in the next section. This is followed by the particular amendments to the data base which are needed to take account of the major projects. Having incorporated these amendments, we use the model to ask the question:

> "With given stock of capital and labour, both fully employed, what will the macro and micro economy look like in 1990, with and without the projects? In particular, what changes in resource allocation, export and import types etc. are implied?"

The JULIANNE General Equilibrium Model

The general theory of the JULIANNE model is given in Stroombergen and Philpott [1982]. The particular version used here contains 40 sectors and is calibrated to provide alternative multisectoral snapshots of 1990 reflecting specified changes in parameters or data. In one version of the model the major projects are included and in another they are not. For this indicative evaluation, all other variables are held constant. The general parameter and data set used follows closely that given in Philpott and Stroombergen [1984a and b]. The key

items are as follows:

The overseas balance of trade is set at a level which, after deducting overseas factor payments, yields a balance of payments deficit of 2% of GDP.

Sectoral-specific rates of technical change are set such as to give an economy-wide average of 1% p.a. from 1985 to 1990.

Commodity-specific export demand curves are set at levels for 1990 which reflect the export projections of the National Sectoral Programme. In the aggregate, the shift factors average at 4% p.a. from 1980 to 1990.

Export price elasticities are set as follows:

Traditional Agriculture	-1.0
Horticultural Products and Energy	-2.0
Manufacturing and Other Exports	-5.0

Domestic import-substitution elasticities are set at a value of 2.0 in all cases, except for clothing (4.0), and transport equipment (3.0).

Labour force in 1990 is set at 1,595,000 persons.

Investment/GDP ratio has been set at 22% with government social investment assumed to grow by 0.9% p.a. and (exogenous) housing investment in 1990 set at 21,000 new houses. Capital stock, with these investment settings, equals $72,870m in 1990 in 1976/77 prices: a long run growth rate of 2.5% p.a.

Tariffs and quantitative restrictions are assumed removed by 1990 as are also subsidies in the form of SMP's

and EPTI's. The incidence of the removal of tariffs and quantitative restrictions reflects estimates of the import protection matrix given in Stroombergen [1984].

Exchange rates. The nominal exchange rate is used as a *numéraire* in this version of the model and therefore changes in it have no effect on relative prices. The real exchange rate, which is of far greater importance, is given by the relativity between the world price levels measured by import prices, and the GDP deflator relative to 1976/77 = 1.0. A real devaluation is represented by a fall in this price relativity which, in a sense, measures our degree of competitiveness.

The world real price of oil (real, that is, relative to the general non-oil import price level), is set at 2.125 with 1976/77 = 1.0. This is about its present level.

With these general assumptions, we now proceed to examine the general equilibrium structure of the economy with "projects on" or "projects off". But first, we must briefly set down some detail on the projects themselves.

The Projects, Assumptions and Data

The major projects in this analysis include the following:

Synthetic petrol plant
Methanol plant
Marsden refinery expansion
Ammonia urea plant
New Zealand steel expansion
Comalco expansion
LPG/CNG conversion
Main trunk electrification.

The details of the structure of capital and current inputs and export and import substitution, are described in Stroombergen and Philpott [1983] and in Burnell [1982a,b,c]. It should be noted that this data, much of it official, related to project plans as published in 1980, rather than to the actual outcome. As we know, in some projects, the outcome, especially in terms of capital cost, will be vastly different from the proposals. However, on the basis of the *proposals*, the total cumulative investment in the projects, that is, that capital stock embodied in them by 1990, amounts to $2100m (in 1976/77 prices) of which $1259m is related to synthetic petrol, methanol, and steel expansion.

The amendments to the JULIANNE model structure to take account of the projects involve not only the exogenous allocation of part of the 1990 capital stock to the projects, but also allowance for new export and import substitution potentials. Briefly, the major projects generate exports of ammonia-urea, methanol, steel and aluminium. The quantities projected (in 1976/77 prices) are modelled by introducing the appropriate shifts in the model's export demand curves for these products.

The import-substitution potentials are modelled by appropriate coefficient changes in the following important areas:

A reduction in the proportion of refinery inputs accounted for by crude and naptha and a corresponding rise in domestic feedstock from the natural gas sector.

The routing of synthetic petrol and methanol as substitutes for normal petrol.

A reduction in liquid fuel use by railways and an increase in electricity usage.

A reduction in liquid fuel use by road transport and an increase in CNG/LPG use.

Domestically-produced ammonia urea replacing some imported fertiliser.

Domestically-produced steel replacing some of the projected increase in imported steel.

Model Runs

Four runs are presented to show the effects of the major projects (taken as a whole) under alternative assumptions about the real price of oil in 1990, (that is, relative to 1976/77). Specifically, the runs correspond to the following schedule:

	Real Oil Price	
	1.000	2.125
Projects Off	Run 637	635
Projects On	Run 636	633

At 2.125, the real price of oil in 1990 would be at approximately its current level. In each run, employment of labour and capital is held constant, as is the balance of trade measured in world prices. Thus the implicit assumption is that, had the projects not gone ahead, all the resources would have been utilised elsewhere, as chosen by the model. This provides a tougher test of the projects than assuming that the resources would otherwise have been idle.

Fixing the employment of factors also fixes approximately the size of the gross domestic product since no changes in production efficiency are permitted. However, the composition of GDP, both in terms of sectors and in terms of its final demand components is free to vary, as is the effective GDP.

Model Results

The following table shows the macro results of the model for 1990.

Macro Results for 1990

1976/77 Prices

	Oil Price = 1.0		Oil Price = 2.125	
	Off Run 637	On Run 636	Off Run 635	On Run 633
Private Consumption ($m)	10389	10601	9872	10251
Gross Investment ($m)	3959	4025	3799	3917
Exports ($m)	5654	5110	6251	5516
Imports ($m)	5779	5482	5550	5333
Gross Domestic Product ($m)	17880	17934	17981	17999
Effective GDP ($m)	18093	18393	17359	17896
Employment ('000)	1595	1595	1595	1595
Price Relativity (N.Z./World, 1976/77 = 1.0)	1.034	1.091	0.859	0.958
Terms of Trade (1976/77 = 1.0)	1.038	1.090	0.901	0.981
Real Wage Rate Change (% p.a. on 1985)	1.25	1.35	0.39	0.77
Imports/GDP (%)	32.30	30.60	30.60	29.60
Exports/GDP (%)	31.60	28.50	34.50	30.60
Investments/Effective GDP (%)	21.90	21.90	21.90	21.90

It can be seen that the projects are beneficial under either oil price assumption but their contribution to welfare increases when the real oil price increases. This is not surprising. The projects taken as a whole always provide a positive return in terms of private consumption. However, particular projects may not always be beneficial, and at a low real oil price it may take many years before the discounted present value of consumption gains is sufficient to negate the consumption losses during the construction phase of the projects.

Concentrating on Runs 635 and 633 (that is, with the oil price at 2.125) one can see that the introduction of the projects reduces the demand for imports (as is the intention) which, for a given balance of trade, allows exports to fall. (The boost to exports provided by the major projects is more than offset by the fall in other exports). The higher real factor prices, made possible by the programme of efficient import substitution, causes export prices to rise, which reinforces the decline in export volumes (for given world demand curves). This rise in export prices constitutes 5.9% of the 8.9% increase in the terms of trade (from 0.901 to 0.981), with the other 2.8% being accounted for by the drop in the mean import price due to the now smaller weighting on oil imports. The substantial increase in the terms of trade generates a significant lift in effective GDP of $535m (in 1976/77 prices), most which (about $380m) is channelled into private consumption. The rest is split into investment ($118m) and stock changes ($39m).

Another way of interpreting the results is to say that in "projects off", the $735m increased exports required to import oil etc., in place of domestic supply, could only be sold, given the position and slope of world demand curves, at much lower prices (that is, lower terms of trade). And, as a consequence, only $217m of extra imports (of oil etc.) could be

afforded.

The interpretation of results from Runs 637 and 636 is that a lower real oil price is better than a higher one, irrespective of whether or not the projects exist. That is, the benefits of a lower oil price, which raises the terms of trade, are reinforced by the improvement in export prices which results from not having to sell as many exports to pay for the (now) cheaper oil. And the direction of this terms of trade change is not dependent on the presence of the projects.

A vast amount of sectoral and other microeconomic detail is available from the model runs - too much to be presented here. Nevertheless, it is interesting to see which sectors gain or lose from the presence of the projects. The table below shows the changes in the allocation of capital between "projects off" and "projects on", (Runs 635 and 633), for nine sectoral groupings. The total capital stock in the projects amounts to $2100m and the table shows $1259m is required in synthetic petrol, methanol and steel. The balance ($841m) is included in the figures for other sectors.

Sectoral Capital Allocation

1976/77 Prices

	Projects Off (Run 635)	Projects On (Run 633)	Percentage Difference
Agriculture, Fishing, Forestry	$ 14963	$ 13695	-8.5
Coal, Natural Gas, Mining	1210	1328	9.8
Petrol	519	476	-8.3
Base Metals	392	413	5.4
Manufacturing	7922	7507	-5.2
Electricity, Gas, Water	6494	6830	5.2
Ownership of Dwellings	19309	20165	4.4
Services	22061	21196	-3.9
New Zealand Steel Expansion, Synthetic Petrol and Methanol	-	1259	-
Total Capital Stock	$ 72870	$ 72870	

As one would expect, the energy-orientated sectors such as electricity, gas and coal and natural gas, expand due to the input requirements of the major projects. Ownership of Dwellings expands because of the rise in private consumption; and the rise in the Base Metals sector is attributable to the Comalco expansion. The output of the Petrol sector declines indicating that the combination of the synthetic petrol plant and the refinery expansion, plus the increased use of methanol, CNG and LEP, could result in excess capacity (somewhere in that group) by 1990.

The more traditional sectors - agriculture, manufacturing and even services - show a relative loss, (that is, a loss relative to the project case, not necessarily relative to 1976/77). This is attributable mostly to a lesser need for exports when the projects exist. In fact, in these sectors the reduction in capital employed is greater than the reduction in output, essentially because the projects are capital intensive. This yields a higher rate of return which is reinforced by the better export prices mentioned before.

This observation has important implications for cost-benefit type methods of project appraisal, since the "standard" rate of return against which the viability of a project is measured (which is too large to be classed as a marginal increment to production), is not independent of that project. Specifying an absolute rate of return as a benchmark is even worse. For example, in Run 635 the mean economy-wide gross rate of return on capital is 10.4%. The rates for the two special project sectors, New Zealand Steel Expansion and Synthetic Petrol and Methanol, are 10.6% and 11.1%, respectively, in Run 633, but the mean rate rises to 11.5% in that run.

Finally a word on sensitivity. The above results were tested with doubled export price elasticities of demand and

with an increase in the balance of trade of $160m (1976/77 prices), in the "projects on" runs, to simulate the extra overseas factor payments generated by the borrowing during the projects' construction period. The former change caused a small deterioration under "projects on", since higher absolute elasticities imply a reduction in demand when New Zealand export prices are high relative to world prices, and cause a very small improvement under "projects off" when export prices were lower. The latter change naturally reduces the benefits of the projects somewhat but neither change affects the ordering between "projects on" and "off", nor between the real price of oil of 1.0 versus 2.125.

Anatomy of the Results

One further and final way of interpreting the results (using the 2.125 oil price case) is to set down the gross rate of return on capital in the "projects off" and "on" cases. The following table gives the structure of exports and imports in both cases and the consequent net foreign exchange implications.

Results: Exports and Imports

$m 76/77 Prices

		Projects Off Run 635	Projects On Run 633
Exports:	Dairy, Meat & Wool	2508	2339
	Projects	0	178
	The rest	3743	2999
	Total Exports	6251	5516
Imports:	Petrol	545	526
	Basic Metals	586	588
	The rest	4419	4219
	Total Imports	5550	5333

The apparent import savings in oil and metals of about $17m are very small, but Run 633 has a higher GDP than Run 635, and to the $17m we must add the output of steel and oil used domestically and which otherwise would have had to be imported. This amounts to:

Steel output of $171m of which $74m exported	=	$ 97m
Synthetic petrol and methanol of $139m of which $24m exported	=	$115m
Per unit oil imports to refinery saving	=	$136m
		$348m

Thus the foreign exchange contribution of the projects equals:

Extra exports	$178m
Import savings	$348m
	$526m

This requires a use of capital of $2100m or a gross return of 25%.

The foreign exchange earnings of the "projects off" case involves an expansion of exports of $735m, less the loss due to lower terms of trade of $518m (analysed earlier); that is, net earnings of $217m for a capital use of $2100m; a gross return of 10%.

The loss due to lower terms of trade reflects two things: first, the lower price received for expanded exports, given the position and slope of world demand curves, (about two-thirds of the change), and, secondly, the greater incidence of imported oil at price 2.125 in the country's overall import price index and therefore terms of trade (about one-third of the change). This difference in the gross foreign exchange rate of return on capital is reflected in the economy-wide true rate of return on capital which in "projects off" amounts to 10.4% and which rises to 11.5% in the "projects on" case.

Conclusions

We repeat the injunction given at the beginning of this paper, that the model approach we have adopted in analysing the projects is indicative only. Many additions and amendments would be required for a thorough going-appraisal. These additions, which are all feasible, would include:

Updating the capital and other input costs of the projects.

Introducing more realistic pricing and tariff routines for the output of the projects and including their output in the composite commodity approach used elsewhere in JULIANNE.

Recognising that the results are a snapshot of 1990 and say nothing about the intervening period and the discount rate which should be applied over the project gestation periods. For this purpose we would use the JUDY dynamic model.

Finally the analysis has been conducted for the projects as a package and while the results suggest they are beneficial, nothing should be inferred about individual projects within the mix. Such individual project analysis would follow the same lines as those we have adopted or suggested above for the package. For small projects representing marginal changes in the economy, the general equilibrium modelling approach may not carry many advantages over a traditional partial equilibrium investment appraisal analysis using border prices. For large projects, representing substantial structural changes in the economy, analysis in a general equilibrium context is essential.

Postscript on the Major Projects

The foregoing analysis of the major projects was carried out in 1983 when current expectations as to oil prices were that they would continue upwards until the end of the century. In fact, the oil cartel and oil prices have now (June 1986) collapsed to a point where they are running at around half their level of three years ago.

Does this mean that the major project strategy was ill-founded, and with it the analysis presented above? To answer this question we need to have recourse to some notions from game theory and especially the concept of a regret function. In particular we must ask the question:

> In the oil price situation in which we now find ourselves, do we regret more or less having built the energy import substitution projects than we would have if we had not embarked on this strategy and oil prices had stayed high or had even risen further?

From the results given above we note, first, that if we had not embarked on the projects and oil prices had stayed high we would have been $517m (that is, $10601m-10250m) worse off in terms of aggregate consumption. Secondly, if we had built the projects - as we did - and oil prices fell we would have been $350m (that is, $10601m-$10250m) worse off.

From this we can say that, regretful as we might be that we embarked on the project strategy, our degree of regret is not as great, to the extent of $167m (that is, $517m-$350), as it would be if the opposite series of events had occurred. Given that in 1981 when the projects were designed there was no way of telling which way oil prices would move - and indeed everyone expected them, if anything, to harden - then on the grounds of minimum regret, we were correct to embark on the strategy.

At the present time the projects have been roundly condemned by Treasury reports written after the event. Substantial costs, it is said, will be incurred by the taxpayer largely on account of the assumption by government of the financing arrangements and because of agreements unwisely entered into, which meant that government shouldered all the risks.

But the fact remains that the projects, even with lower oil prices and substantial cost over-runs, will earn a real return of around 5% - a respectable return even if much lower than the planned return of 10%. In 1987, in spite of reductions in world oil prices, the New Zealand overseas balance of payments will benefit from the energy import substitution resulting from the projects by around $500m. It is contributions to the economy of this nature which are not captured by the critical analysis of the projects carried out after the event, with the benefit of hindsight, and in a partial equilibrium framework.

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Comments on PEP Models

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General Systems

I spent 1983 on sabbatical leave in Canada and there I came across the writings of an extremely good Canadian economist. I would highly recommend him to you. His name is Stephen Leacock - one of the greatest humorists of all time - and a professor of economics at McGill. One of the things he said, and I have got to agree with it, is that theorists only explain events after they have happened.

What I want to do is play the theorist and give an explanation after the event of what I see general equilibrium modelling being about. One of the tasks of the theorist is to provide a vocabulary with which to discuss models. So let me try to do that. I will try to give a general overview of what I think general equilibrium modelling is all about as simply as I possibly can. Hopefully the discussion can take off from there.

The first thing that I would like to note is the nature of any deterministic theory. Any deterministic theory, whether it is in economics or in any other subject, seems to me to have the following form. On the one hand there are some things you want to explain. Let us suppose there is a vector of these; call them **x**. **x** is a vector of things you want to try to explain (or endogenous variables). On the other hand there are some variables you want to use to explain things. Let us call these exogenous variables **y**.

Any theory says that there is some relationship between the **x**s and the **y**s. Let me summarise that as succinctly as I can by saying that there is a vector-valued function, $f(\mathbf{x},\mathbf{y})$, and theory says that the **x**s and **y**s are related by having this function equal zero. I am proposing that any deterministic theory has this general form: there is some equation $f(\mathbf{x},\mathbf{y})=0$ relating the endogenous and exogenous variables. This could be a theory in economics or in anything, and what we believe is that you put in a value for **y** and a figure for **x** comes out.

Thinking in these terms helps to see the famous trilogy of questions that theorists ask about existence, uniqueness and stability. The existence question is "For every **y** is there an **x** that satisfies the system?". The uniqueness question is "Is there a unique solution to this system?". The stability question asks "Is there any reason to believe that for a given **y**, the **x** comes about?".

What would be nice would be to convert this general theoretical system into a reduced form, that is, have $\mathbf{x}=g(\mathbf{y})$. There is a spot of bother in doing this in general. The equations $f(\mathbf{x},\mathbf{y})=0$ are (highly) non-linear, so it might not be a straightforward matter to solve for a reduced form. There are two ways of handling this as far as I can see. The first way really builds on the possibility of being able to find a reduced form. The possibility depends on whether or not the conditions of the implicit function theorem are satisfied.

The implicit function theorem, if its conditions are satisfied, says that there is such a function g and, most importantly, it tells what the derivatives of that function are. If you are interested in this sort of relationship then you can write it in the derivative form $d\mathbf{x} = f_x^{-1} f_y d\mathbf{y}$, where f_x and f_y are matrices of partial derivatives of f with respect to the elements of x and y. The alternative procedure would be to solve the system numerically; that is, plug in the

y values that happen to interest us and use some numerical technique to solve the resulting system of equations.

Now I said that one role the theorist can play is to give a framework for discussion. Let me now go back and suggest that if this system of equations is an economic model, we have got some endogenous variables we are interested in: prices, quantities, inputs of capital and labour and intermediate goods in various sectors of the economy. Exogenous variables are also important: the labour force, terms of trade and, most importantly, policy variables - tax rates, subsidies, import quotas and the like. So this system could be a structural model of the economy (it could be a general equilibrium model or it could be a general disequilibrium model).

Let us suppose it is a general equilibrium model relating the endogenous and exogenous variables. Having got it, there are two ways to proceed. You want to know the effect of changing the policy variables - tax rates, subsidies and the like. Way number one is the form that is followed in the ORANI-JOANNA class of models. Essentially they take a linear form in terms of the derivatives.

The alternative is to solve numerically for each **y** of interest. This is the approach taken in JULIANNE and also in a very large number of overseas models. Now we see how the JOANNA-JULIANNE models fit into the overall scheme of things. The point I want to make here is that the same type of structural model could be handled in either of these ways. In the Wells-Easton-Kay *Report* this point was made.

Modelling Criteria

This is background. The next step is to ask by what criteria would we want to judge a particular modelling exercise. It

seems to me that there are two things that we have got to bear in mind. The first is that the general structural form we set up is consistent with the accepted body of opinion or knowledge in economics. So we want the structure of the model to be one that at least most economists are going to agree on.

The second thing is that the model we are using here is theoretical but when we come to implement it in a particular setting, we require that this general structural model be capable of replicating the facts. What I want to do now is to briefly make some comments about PEP from both of these points of view.

For the most part one would accept the theoretical structure of the PEP models: profit maximisation, real production functions and so on; but I have some reservations. These reservations are technical rather than fundamental. For example, with respect to the short-run JOANNA model, I find myself rather uneasy about the dynamics of investment, and the determination of rates of return in individual sectors. In short-run JOANNA, the rates of return to sector-specific capital are determined on expectations about which rates of return will arise on that sector-specific capital in the next period. In the modern jargon, it would seem that the expected rates of return on sector-specific capital are not rational expectations. It is not clear to me that the expected rates are the rates of return that would be thrown up by the model. In this sense, the model is internally inconsistent.

I said that there was another criterion that ought to be considered. Let us assume that the model is correctly specified and the assumptions being made are consistent with economic theory. None the less, when it comes to implementing the model what one would want is that the model which is used actually be capable of reproducing the data in the world. In particular, when you set the policy variables in the model at

the values which prevail in the world, then the **x**s which come out of the model - the endogenous variables - are actually those which prevail in the world.

The point is that this structural model is deterministic; it is the maintained hypothesis about the world. It says that there is a definite deterministic or exact relationship between the variables, not a stochastic one. So it seems to me, as it seems to most of the overseas writers on applied general equilibrium modelling, that actually implementing the model, putting in the numerical values of the parameters, is an extremely difficult task.

Implementing Models

There are at least two aspects to implementing models. The first is that the data from which the parameters of the model are to be determined will seldom be consistent with the theoretical model. The data will have been collected on some, maybe implicit, theoretical framework, and it has to be put into the right form for the model. There will be items in the data which are not present in the structural model. For example, you may have a model which does not treat second-hand assets, yet these figure in the national accounts.

Alternatively, the model may require data from more than one source. A model which disaggregates over households gives this sort of problem. National account and household expenditure data must be combined to give a consistent data set. What is difficult to guarantee is that the data set on which the model is calibrated is such that all restrictions of the model are satisfied.

Whatever data you feed into a model will allow the parameters to be calculated. However, the maintained hypothesis is that the model exactly describes the world. This means

that for these parameter values, and the real-world values of the exogenous variables, the model should predict the real-world values of the endogenous variables. That is, it should replicate the data on which it is based. This check should always be made. If the calibrated model does not reproduce the data on which it is based, it signals that you have the wrong parameter values. Policy analysis with the model must then be in error, and vou have no real idea of the size or sign of the error.

There may be lots of errors in an general equilibrium model of the economy. Overseas writers are clear on the importance of correct calibration based on a replication test, though. I suppose it is because if there are going to be lots of errors it is a good idea to eliminate as many as possible. By doing this a more informed judgement can be made on the validity of the results.

I said before that there are two criteria to judge a modelling exercise by. One is that the theoretical model is acceptable in terms of economics. The other is that the implemented model should be able to replicate the facts. On the former count, I acknowledged some reservations about the PEP models, although these are not perhaps severe. I have not seen any discussion of the problems of data preparation in the many papers produced by the PEP workers; indeed, there does not seem to be any recognition that there is an issue. Without knowing that the models satisfy the replication test we cannot really judge the conclusions based on them concerning policy.

Discussion on PEP Models

Bryan Philpott (Victoria University of Wellington) opened the general discussion on the PEP models by responding to Richard Manning's comment on validation. Professor Philpott said the validation of a General Equilibrium (GE) model in any one year was *simpliste* because it took the data of that year, set it up in a GE form and you got out what you put in. Whether or not a model revealed a picture of the "real" world could only be assessed in a dynamic version of a model. PEP had performed some dynamic runs and these disclosed a divergence between the real world and the model simulations. This divergence was not easy to embrace in a test statistic. Although the differences could not be ignored, the results were, nevertheless, surprisingly close to the actual outcomes.

Adolph Stroombergen (Victoria University of Wellington) said that the calibration of dynamic models was a matter of degree. Over time events occur in an economy for which the models have no corresponding variable, such as demographic changes or a speculation about devaluation. Shifts in the structure of the population from older to younger people, for example, could lead to a change in consumer spending. Some attempt could be made to model these events. Currency speculation, for example, could perhaps be measured by an increase in the elasticity of substitution between export and import goods to some arbitrary high amount. But that would be a spurious measure as the value would alter year to year. So calibration had to be a matter of degree.

Alan Woodfield (University of Canterbury) said that as he understood it, a GE model of the Walrasian type could not

determine absolute prices. He asked what was meant by the money wage rate in these circumstances.

Bryan Philpott said the short answer was that one could have absolute price levels in which case everything was being expressed in terms of a *numéraire*, which was the usual case. Alternatively, current price levels could be inserted provided one was changing the things that affected current price levels such as actual import prices and actual money wage rates.

Alan Woodfield raised some further issues. First, when a model like JULIANNE was being calibrated there was a problem when prices were normalised on a *numéraire* because there were many different prices, tariffs and indirect taxes. Take the most simple version of the JULIANNE model. In order to be able to estimate some of the parameters, like some of the scale parameters, prices have to be normalised to obtain a measure of output. So on which prices do PEP normalise - consumer prices or producer prices?

Another issue that worried him was a specification problem relating to the short-run versus the long-run interpretation of, say, the JOANNA model. It seemed that the structural equations which described the long-run equilibrium were always being satisfied; profit maximising conditions, for example, were always fulfilled. But when the model switches from a long-run model to a short-run model, certain assumptions are made. It seemed fine to him that when PEP say that the JOANNA model is not in full equilibrium they mean that not all markets clear; there is, say, some slack in the labour market which is compensated by a balance of payments disequilibrium. The problem is, however, that in the initial run, sectoral capital stocks are fixed. If that was true, and assuming profit maximisation where producers set prices equal to

marginal cost, then marginal cost and average cost are going to differ. This outcome did not appear to show up in the specification of JOANNA's pricing equations. So how are short-run prices determined in JOANNA when there were fixed sectoral capital stocks?

A calibration issue that worried him concerned the treatment of rates of return. In JOANNA there was a uniform rate of return when the model was in long-run mode, but with the basic models of JULIANNE there were wage rates and rates of return differentials across sectors. He could not quite understand where, in an equilibrium model, these differentials came from. In a pilot model where this was examined, he could find no wage rate differentials in the data set used to calibrate the model. Yet when the model was solved, these rates of return differentials emerge. This did not appear to be consistent with the concept of equilibrium and may suggest the possibility of a calibration error.

On a more general issue, Alan Woodfield said that he was a little concerned that JOANNA and JULIANNE were models of the same economy. There should really only be one structure if you really believed in only one structure. PEP do not have one structure. There are several versions of JOANNA and JULIANNE and different ways of modelling, say, investment, consumption and intermediate goods between these models. If the same type of experiment was conducted from an initial equilibrium, presumably different values of the endogenous variables were going to be generated as a result.

Adolph Stroombergen and Bryan Philpott replied that the different rates of return were inserted as a proxy for skills. As far as the issue regarding the use of different models to explain the same economy was concerned, PEP had found that they produced much the same sort of results. The choice of

which model to use from the PEP suite was partly influenced by the amount of detail required for the problem under consideration and the complexity of the solution routines.

The Reserve Bank Model

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Reserve Bank of New Zealand

Overview

The Reserve Bank of New Zealand (RBNZ) quarterly econometric model comprises 35 behavioural equations and 23 identities. The model used in this paper is that described in Carey [1984], modified by equations reported in Kek [1984], Grimes [1984] and Clements [1984]. In contrast to earlier versions, the current version is more compact, sounder theoretically, and based on a more satisfactory system of national accounts (SNA) data. More emphasis is placed on the model's long-run properties and on the transmission channels from the monetary sector to the real and price sectors. The model can be described as a Keynesian-neo-classical synthesis, which draws on various strands of theory with the intention of producing an integrated model which approximates real world aggregate economic behaviour.

In broad terms the model can be divided into five sectors: the real sector; the external sector; the financial sector; the wage-price sector; and the public sector. The real sector provides a description of the production technology, the factor inputs and consumption. Private sector output is modelled in the long-run by a constant elasticity of substitution (CES) production function with capital and labour as inputs. Adjustment towards this position is altered by a disequilibrium inventory variable (that is, by the difference between desired and actual inventory). Decisions regarding non-residential real private sector capital formation and

private sector employment are assumed to be determined by the same CES production technology, with output and relative price terms as the major explanatory variables. The main determinant of consumption is real disposable income, while a monetary disequilibrium effect is included to capture the role of money as a buffer stock.

The primary influences on the external current account balance are domestic demand conditions and domestic prices relative to external prices. In addition, the current balance includes net property and entrepreneurial income from abroad which changes in relation to the level of net foreign claims on New Zealand. Changes in net foreign claims are determined by movements in official overseas reserves and the current account. The private capital account is modelled as a function of wealth proxies, differentials between domestic and foreign interest rates and exchange rate expectations.

The financial sector has M3 as its main monetary aggregate which, in the long-run, is demand determined. Supply influences dominate its short-run behaviour. Hence, money is generated within the model by a supply identity derived from the balance sheet identities of the various sectors of the economy. The behaviour of trading bank and non-trading bank M3 institutions is explained separately within the model as are a variety of interest rates.

The wage-price sector outlines the mechanisms which link real and nominal shocks in the model to changes in the wage rate and price level. Producers determine the price of output according to a mark-up on costs, taking into account short-run excess demand pressures in both the goods and money markets. Private sector wages are a function of normal labour productivity and include tax and excess labour supply terms as short-run influences. Consumers' prices are determined by the market price of domestic goods, the price of imported goods

and the price of existing dwellings.

Net national product at factor cost is distributed among the four main income-earning groups: wage and salary earners, other persons, farmers and companies. All major forms of taxation are explained endogenously by relationships which include the policy-determined tax rates and the various classes of income. The real components of government expenditure are treated as exogenously determined policy variables. These are reflated to give nominal government spending.

Concepts of Equilibrium

The partial-adjustment framework adopted in the model is a standard one commonly used where the main aim is to explain short-run movements in economic variables. The RBNZ model is a model where the principal objective is to explain short-run movements in the main economic aggregates in New Zealand as well as taking into account long-run properties which conform with economic theory. As such, the partial-adjustment framework represents agents' actions in endeavouring to achieve a desired path while taking into account adjustment costs and time lags. By accepting this framework, it should come as no surprise that if the desired position continuously shifts, perhaps quite randomly as might describe the real world, then the desired path will never be reached. Indeed, should the steady-state growth world ever be attained, then the desired position continues to shift but in a known and predictable manner.

Agents in this environment would face no uncertainties, thus they could plan with perfect foresight and would adjust instantaneously. In this case it would be expected that the adjustment coefficient would take a value of one and the adjustment framework would consist solely of the long-run

specification. Similarly, in a perfect steady-state growth world the adaptive expectations model (which underpredicts in the stationary steady-state) would no longer be appropriate as agents could form their expectations with certainty and hence would change their model.

At a more practical level, there is the question of whether this particular property, of not achieving the long-run path, influences the usefulness of the model for policy assessment. In order to assess whether one policy configuration is preferable (in some sense) to another, a benchmark or reference point must be established as a point for comparison. Because the alternative policy will be assessed on the basis of the simulation's divergence from this "control" level, the fact that both the simulation and control differ from their steady-state growth path by a *constant* will be of no consequence. Policy assessment will be the same as in the stationary steady-state.

Structure of the Model

The current model structure is presently being re-estimated to incorporate data up to 1984(1). It is intended that this project will involve only the "neo-classical" variant described in the Wells-Easton-Kay [1983] *Report*. However, several areas of the model have been modified since the 1983 version on which the *Report* is based.

As mentioned earlier, the production sector has been revised so that normal output is determined from a constant elasticity of substitution (CES) production function and both labour and capital investment equations are consistent with this underlying technology. Other changes include endogenising private capital flows and property and entrepreneurial income payments. Work is also continuing on developing a model of monetary influences in order to improve

the linkage between the monetary sector of the model and the real and price variables.

Policy Questions

The potential value of the RBNZ model for analysing policy questions is that the model provides a consistent macro-economic structure which forces the analyst to specify explicitly a perception of the simultaneous relationships which exist in the economy. The alternative - qualitative assessment - may take similar relationships into consideration but is unable to provide a rigorous quantitative indication of the net impacts of various policies, whereas a model does.

The best way to highlight the benefits which may be gained from using the RBNZ model as a policy analysis tool is to consider the two policy questions posed for this Seminar, namely, the questions relating to alternative wage paths and to the large-scale projects.

Alternative Wage Paths

To keep the discussion manageable, and because analysis of this particular question usually involves consideration of an upward adjustment in money wages, simulations of the RBNZ model were carried out over 1976(1)-1983(4) with a five per cent increase in the private sector wage rate over the historical path. Essentially this involves the proposition that money wages were five per cent above what they had been at the beginning of the simulation and that subsequent actual rates of change in wages were applied to that new level.

Several alternative money wage paths could have been examined, such as those implied from maintaining real after-tax wages, a wage-tax trade off, or holding real wages unless productivity gains warrant change. However, while these

experiments would have been interesting in their own right, a simple example, such as that described, can more easily display the linkages contained in the model.

Because we are interested in the consequences of different money wage paths in the context of policy analysis and decision-making, we also need to outline the accompanying assumptions with regard to government policy. In this context we are concerned with fiscal, monetary and exchange rate policy assumptions.

Fiscal: Government fiscal policy is assumed to be accommodating in that changes in revenues and outlays are permitted to translate into changes in the nominal government deficit before borrowing. No allowance is made for government reaction to the changes which might have had a secondary impact on the deficit.

Monetary: Two options are used with respect to monetary policy. First, M3 can be freely determined by the short-run supply and long-run demand influences as described in the model. Secondly, M3 can be preserved at its control (that is, historical) level via compensatory changes in the sales of government securities to the non-M3 private sector. This is achieved by varying the own rate of interest on the security.

Exchange Rate: Two exchange rate systems are considered. In the first, the nominal exchange rate is fixed at control values and in the second, a flexible system is adopted where the exchange rate adjusts in order to maintain the balance of payments (the current account balance plus private capital flows) at its control level.

Three cases are sufficient to trace the implications of the alternative wage path hypothesised. The results are discussed in the text and illustrated in the charts.

Case (a): M3 Free/Exchange Rate Fixed

The increase in private sector wages boosts disposable incomes and hence generates an increase in consumption above control by a maximum of $20 million real or 1.04 per cent (Fig.1). Real labour cost is increased which induces a reduction in employment levels below control of about 10,000 persons after one year (Fig.2). The drop in employment reduces output capacity and hence output (Fig.3).

As a result of the initial boost in consumption, domestic inventories are run down (which stimulates an offsetting influence on output) while an increase in imports is observed. Exports fall as a greater proportion of goods are diverted to satisfy domestic demand while the current balance deteriorates in line with the balance of trade deterioration (Fig 4). The fixed nominal exchange rate implies that the current balance position accumulates to increase total net foreign claims on New Zealand (Fig.5).

Consumer prices also rise as increased unit labour costs feed through to the domestic portion of the index (Fig.6). This price rise, which reduces real incomes, combined with the drain on M3 caused by the current balance position, serves to finally force consumption below control. The long run position indicated by the model after nearly 8 years has consumption 0.53 per cent below control, employment 4.32 per cent down, output 2.15 per cent below, the current balance about $80 million lower, net foreign claims 11.38 per cent above control and consumers' prices 1.63 per cent higher.

If the government could not tolerate the decline in the current balance and the accompanying accumulation of foreign debt then it might consider allowing the exchange rate to float in order to neutralise the balance of payments position. This leads to the second case.

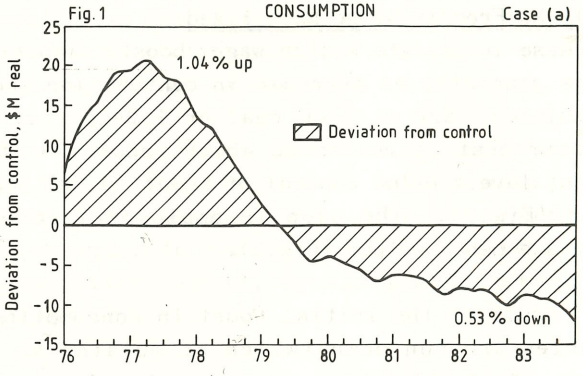


Fig. 1
CONSUMPTION
Case (a)
Deviation from control, $M real
25
20
15
10
5
0
-5
-10
-15
1.04 % up
Deviation from control
0.53 % down
76
77
78
79
80
81
82
83

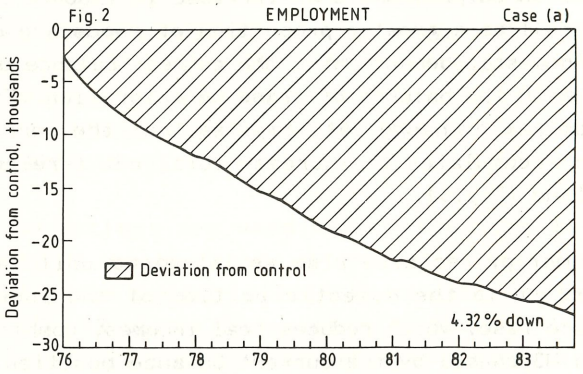


Fig. 2
EMPLOYMENT
Case (a)
Deviation from control, thousands
0
-5
-10
-15
-20
-25
-30
Deviation from control
4.32 % down
76
77
78
79
80
81
82
83

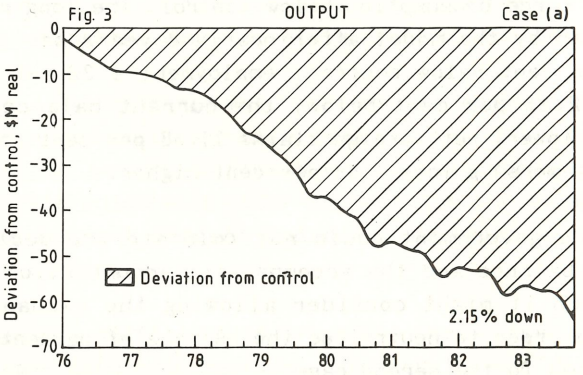


Fig. 3
OUTPUT
Case (a)
Deviation from control, $M real
0
-10
-20
-30
-40
-50
-60
-70
Deviation from control
2.15 % down
76
77
78
79
80
81
82
83

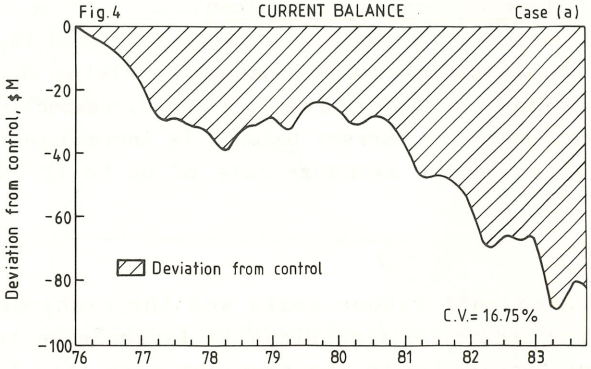


Fig. 4
CURRENT BALANCE
Case (a)
Deviation from control, $M
0
-20
-40
-60
-80
-100
Deviation from control
C.V.= 16.75%
76
77
78
79
80
81
82
83

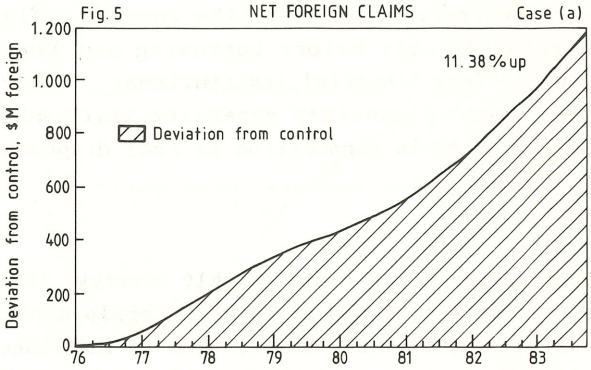


Fig. 5
NET FOREIGN CLAIMS
Case (a)
Deviation from control, $M foreign
1.200
1.000
800
600
400
200
0
11.38% up
Deviation from control
76
77
78
79
80
81
82
83

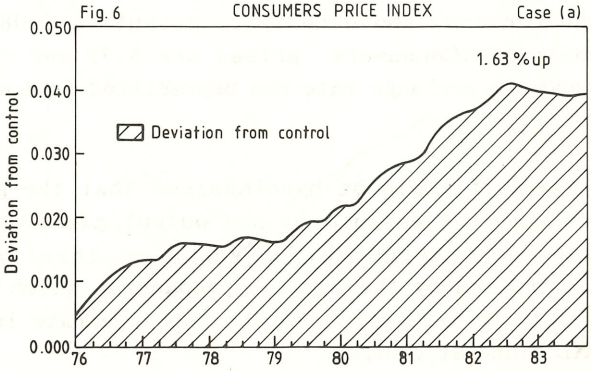


Fig. 6
CONSUMERS PRICE INDEX
Case (a)
Deviation from control
0.050
0.040
0.030
0.020
0.010
0.000
1.63% up
Deviation from control
76
77
78
79
80
81
82
83

Case (b): M3 Free/Exchange Rate Free

The initial impact of the wage rise in this case is similar to that in the first case with consumption rising while employment and output falls (Figures 7, 8 and 9, respectively). The deterioration in the current balance is instead reflected in a depreciation of the exchange rate of up to 12.54 per cent (Fig.11).

Increased unit labour costs and the exchange rate depreciation produce a significantly larger rise in consumer prices as compared with the previous case (Fig.10). These price increases serve to inflate the current dollar value of the government deficit before borrowing and lending to the private sector from financial institutions. These latter two influences induce a monetary expansion which adds to price pressures and supports consumption as real disposable incomes fall.

The much larger price rises quickly overtake the wage rise, reducing real wage cost and stimulating employment and output growth. After eight years the position is that consumption is more or less back at control while employment is 1.33 per cent (or 8,000 persons) above control, output is 1.08 per cent above control. Consumers' prices are 5.79 per cent above control and the exchange rate has depreciated by 6.6 per cent.

Once again it could be hypothesised that the government considers that the employment and output gains were at the expense of an unacceptably high price level which was primarily induced by the monetary expansion which occurred. This poses the final case where the exchange rate is free but M3 is maintained at control.

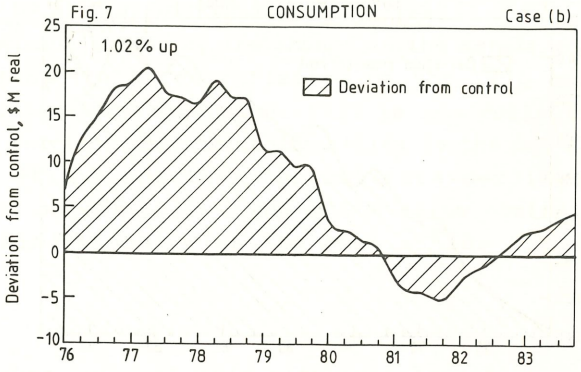


Fig. 7
CONSUMPTION
Case (b)
1.02% up
Deviation from control
Deviation from control, $M real
25
20
15
10
5
0
-5
-10
76
77
78
79
80
81
82
83

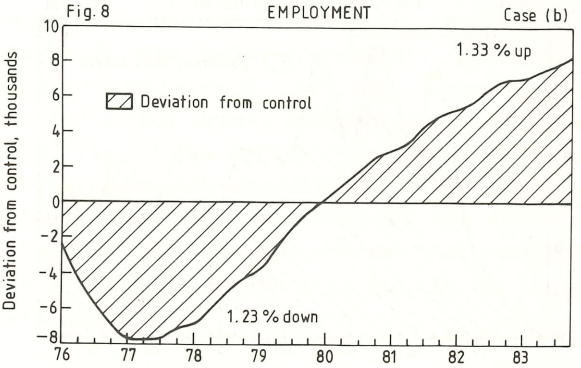


Fig. 8
EMPLOYMENT
Case (b)
1.33 % up
Deviation from control
1.23 % down
Deviation from control, thousands
10
8
6
4
2
0
-2
-4
-6
-8
76
77
78
79
80
81
82
83

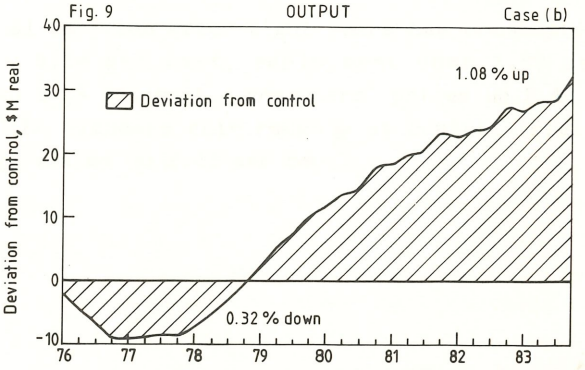


Fig. 9
OUTPUT
Case (b)
1.08 % up
Deviation from control
0.32 % down
Deviation from control, $M real
40
30
20
10
0
-10
76
77
78
79
80
81
82
83

Fig. 10 CONSUMERS PRICE INDEX Case (b)

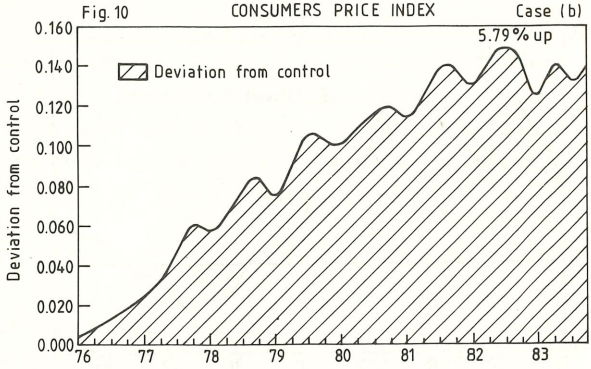
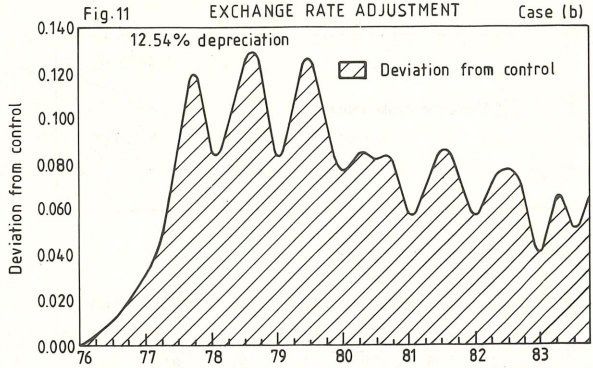


Fig. 11 EXCHANGE RATE ADJUSTMENT Case (b)



Case (c): M3 at Control/Exchange Rate Free

The predominant impact of the change in the monetary stance is that whereas the price inflation of the last experiment induced a monetary expansion, this is now fully offset by increased sales of government securities to the non-M3 private sector. This means that the ultimate decline in real disposable incomes pulls consumption below control up to a maximum almost equivalent to that of the initial rise (Fig.12).

The higher interest rate structure brought about by the restrictive monetary policy increases the user cost of capital and discourages private sector investment. This combined with the result that employment is lower, because real wages decline less and output drops, produces a reduction in normal output levels (Fig.13 and Fig.14). Higher domestic interest rates attract inward private capital flows which offset a current balance decline. As a result net foreign claims increase, (Fig.17), but with the proviso that ownership of the debt is now private as opposed to public as in the fixed exchange rate case.

The final position after eight years has consumption below control by 0.84 per cent, employment down 3.96 per cent, output down 2.59 per cent, consumers' prices up 3.44 per cent (Fig.15), the exchange rate roughly at control (Fig.16), and net foreign claims up 2.43 per cent.

Fig. 12 CONSUMPTION Case (c)

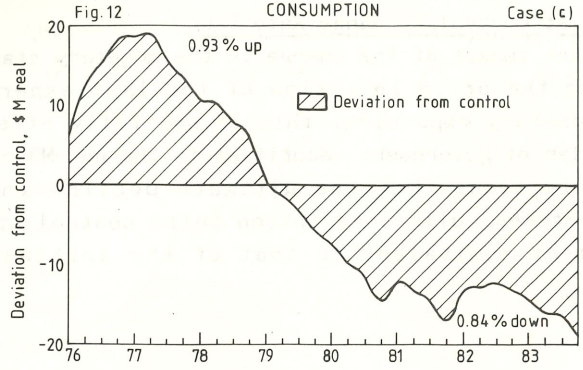


Fig. 13 EMPLOYMENT Case (c)

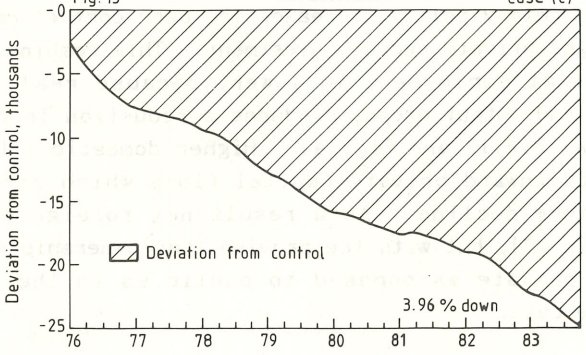


Fig. 14 OUTPUT Case (c)

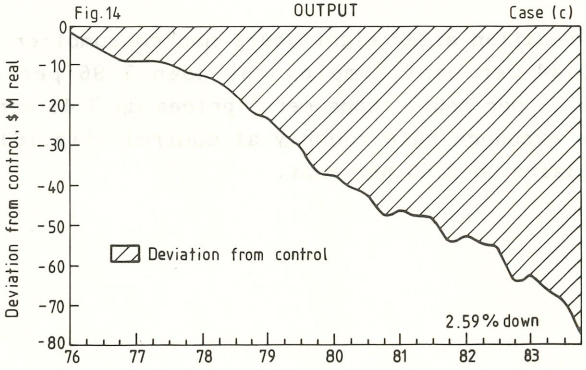


Fig. 15 CONSUMERS PRICE INDEX Case (c)

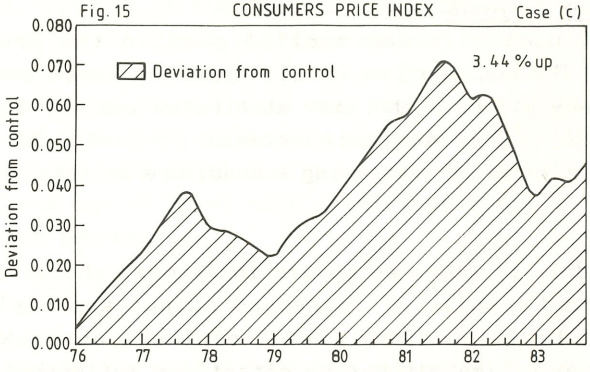


Fig. 16 EXCHANGE RATE ADJUSTMENT Case (c)

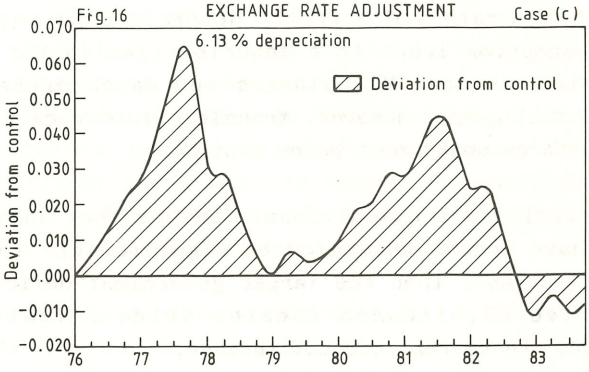
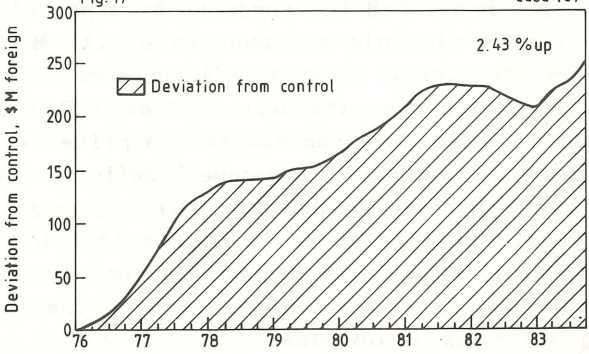


Fig. 17 NET FOREIGN CLAIMS Case (c)



Large-Scale Projects

The model uses only one capital good in the production process. The aggregation of all types of investment goods assumes they all have the same attributes and are equally as productive. Large-scale investment projects can only be analysed in terms of increasing expenditure on this investment item.

The model, if given an upward investment shock, provides the expected results of increased output and employment, although the stimulatory impact of these on consumption expenditures ultimately has an offsetting influence. With a fixed exchange rate and a freely determined M3 aggregate, higher consumption leads to a deterioration in the current account which reduces M3 balances and eases pressures on prices. Output gains, however, translate into wage rises and eventually force employment below control.

Had we included the additional feature that large-scale projects have been accompanied by significantly increased government outlays, then the larger government deficit would have boosted M3, induced greater price inflation and deteriorated the current account further.

On the other hand, had the projects been promoted in an environment of a flexible exchange rate with M3 held at control, then the current account influences would have been reflected in an exchange rate depreciation. This and wage rate increases would have contributed to price inflation. Higher prices and the expanded government deficit, due to the projects, would increase M3 and result in higher government stock interest rates in order to absorb it. Ultimately consumption would fall below control as real incomes drop while output remains above control despite the impact of higher interest rates on investment.

Several features of the large-scale projects have not been allowed for here. These include, amongst other things, the higher import content of these projects and the import substitution that it is envisaged would ultimately result. The inclusion of these factors would require suitable data and/or further assumptions which would be crucial to the results. Indeed, these could be of sufficient importance to make the model's role secondary.

Assessment

The RBNZ model is a useful tool for assessing the major macro economic consequences of alternative policy strategies. However, the ability of the model to perform certain tasks will vary depending on the nature of these tasks. In particular, although care has been taken to ensure the model has sensible long-run properties, it is essentially designed for short-term (that is, about two years) forecasting and simulation work. For example, the model places considerable emphasis on short-term adjustment processes.

The two policy questions addressed in the previous section illustrate how the model is better suited to some types of simulation work than other types. Questions such as the short-term effects of alternative tax rates or wage paths are easily assessed with the model whereas questions concerning the long-term effects of different investment decisions are not so easily answered. A number of important assumptions, not already captured by the model, would be required to assess adequately the effects of such policies. As noted earlier, the importance of these assumptions could be such as to make the model's role, secondary. In addition, the model is very aggregated - essentially assuming a single-good economy. It is, therefore, of little use in assessing the effects of a change in, or the impact on, the composition of output; for example, it can give no indication of a shift in output

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.

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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 experience, 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

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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 per-ceptions 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

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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.

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Policy Evaluation

Using Reaction Functions

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Introduction

It is clear from the final *Report on Economy-wide Models of New Zealand*, which forms the basis of this Seminar, that there has been a quiet revolution in economy-wide modelling in New Zealand. In addition to the well-known Reserve Bank and Victoria University Project on Economic Planning (PEP) models, Graeme Wells, Brian Easton and Linda Kay [1983] have made us aware of at least ten other significant contributions which attempt to model aspects of the New Zealand economy. These modelling efforts, invariably by small teams or even by individuals, are most impressive. The authors of this *Report* have provided a very useful service for New Zealand economists and deserve our thanks.

Within the *Report* it is pleasing to see the contribution by Professor Bryan Philpott on the PEP models. His helpful guide through his suite of models will be appreciated by many people who, like myself, have been disappointed that the Project on Economic Planning have not published final-form research papers equivalent to the Reserve Bank research papers. It is easy to miss working papers and, as in the PEP case, to often miss very interesting analyses of contemporary policy issues. The *Report* itself recommends an interface between a PEP model and the Reserve Bank model.

This "quiet revolution" is also impressive because it confirms what we know from formal economic analysis, including empirical analysis, namely, that there are different explanations of "how the world works" and how it might work better. We have tended in New Zealand, I believe, to have a dominant "received wisdom" of how our economy works and how it might work better (whether it has come from dominant politicians or from dominant economists). The recent economic management and real wage-employment debates, however, have highlighted some major underlying differences or "schools of thought" amongst New Zealand economists and this *Report* does the same. Overall, this seems to be a desirable trend.

Participants at this Seminar have been requested to comment on the *Report* in terms of "the broad issue of economic behaviour and the overall place of models in assessing such matters" rather than on technical aspects of model building". Within the spirit of this request, my comments relate to the broad issue of government behaviour as discussed in the literature on reaction functions or government equations. Although this literature is becoming increasingly technical, as one would expect in such a complex area of economic behaviour, my comments will be non-technical.

Reaction Functions

It is interesting to note that despite the diversity of the models in this *Report*, at least one common feature emerges in the majority of the models surveyed. This feature is the assumption that government behaviour (unlike, say, consumption behaviour or investment behaviour) is exogenous in the model-building sense that there is very often no feedback from the state of the economy to many components of government behaviour, for example, changes in government expenditure, reserve ratios, interest rates and taxes. But if government expenditure, for example, is systematically related to the

state of the economy, as expressed in an econometric model, then, in principle, a specification error has occurred if this expenditure is treated as exogenous. This was demonstrated over a decade ago an the important paper by Goldfeld and Blinder [1972] (as the authors of the *Report* also mention on page 71).

Why, in view of the generally accepted notion that government behaviour is usually endogenous, do most of the models discussed in this *Report* treat so many aspects of government behaviour as exogenous? One major explanation, I suspect, apart from the difficulty of the task, is the con-ditioning process established by the relatively unchanging treatment of government behaviour in a wide range of classroom macroeconomic models. In these models government is invariably treated as exogenous in the policy-making sense of government supposedly being able to make its expenditure or money supply whatever amount it desires so as to achieve its target(s), say, full employment income. If governments do not behave entirely in this manner, then models which assume they do may produce incorrect time paths for variables, equilibrium values and multipliers.

Suppose we are given the following basic macroeconomic model:

$$Y = C + \bar{I} + \bar{G} \qquad (1)$$

$$C = \bar{C} + bY \qquad b>0 \qquad (2)$$

where Y is income, C is consumption, $\bar{I}$ is investment and $\bar{G}$ is government. Now suppose we amend government such that in addition to the exogenous component, $\bar{G}$, government spending is proportional to the error between actual income and desired income, Y^*, to give a negative feedback. Then

$$G = \bar{G} - g(Y-Y^*) \qquad g>0 \qquad (3)$$

In this amendment to the most basic macroeconomic model, the "true" income multiplier is now

$$\frac{dY}{dG} = \frac{1}{1-b+g} \qquad (4)$$

which is smaller than the basic goods market equilibrium multiplier of 1/(1-b).

Equation (3) is a traditional reaction function in which government behaviour responds to the "state of the economy": government is now partly endogenous. Although this equation appears to be casual or *ad hoc*, it can, in principle, be derived from an optimising framework which incorporates a policymaker's preference or loss function which is to be minimised subject to a model of the economy.

Aspects of this framework seem to be implied on pages v and vi of the *Report* (amongst other places) where the authors mention preference functions, assignment rules and optimal control and remark that "apart from the work of Spencer and Grimes [1980] on assignment rules, there has been no recent work published in New Zealand on [preference functions and optimal control]". In a strict sense, this is probably correct, but it does overlook some work, mainly unpublished, related to New Zealand. Elsewhere, for example, I have made an attempt to estimate some single-equation monetary policy reaction functions for New Zealand (Silverstone [1978]) and a former student modelled reaction functions simultaneously in the Reserve Bank Core Model (Pigou [1983]).

In my experiments with reaction functions I broadly followed the traditional format and regressed an "intermediate target", domestic credit, against stabilisation goals (employment, inflation and the balance of payments), financial or "defensive" goals (public debt and the mortgage interest rate), a transactions goal (retail trade) and seasonals. The

most satisfactory results were achieved using first differences of domestic credit and several independent variables. The overall results were mixed, but the strength of the offsetting impact of overseas reserves on domestic credit was interesting while the strength of retail trade and seasonals on domestic credit was not unexpected.

Pigou estimated reaction functions for four exogenously-treated "policy instruments" in the Reserve Bank model: the effective income tax rate on wage and salary earners, nominal government expenditure on monetary benefits, the yield on short-term government securities and the nominal exchange rate. He compared the performance of the Reserve Bank model with and without reaction functions and when subjected to shocks from export and import price changes. He concluded, with reservations, that the results of his study "indicate that endogenous policy actions may exist in the New Zealand context, and that their introduction into a structural macroeconomic model significantly alters the responses (multipliers) for selected exogenous shocks" (Pigou [1983, p.118]).

There is also the work of Gillion [1978] and Ursprung [1983]. Gillion included four reaction functions in his 21-equation "experimental small model of the New Zealand economy": for real government current expenditure, tax receipts, financial advances to the private sector and for the exchange rate. Ursprung estimated a political-economic reaction function which he claimed showed that the inflation and unemployment rates and probably also the balance of payments "exerted a significant influence on the popularity of the New Zealand government during the period 1970-1981" (Ursprung [1983, p.13]).

All these studies represent attempts to discover historical or *ex post* reactions to the state of the economy rather than

optimal control or *ex ante* reactions which I think the authors of this *Report* would like to see conducted. *Ex post* functions seek evidence for "what happened" (they are backward-looking) and do not usually include explicit loss functions for the policymaker, whereas *ex ante* optimal control exercises usually seek evidence for "what could happen" if certain policies were followed. The later are forward-looking, usually include explicit loss functions and are commonly found in optimal control forecasting experiments.

A Further Illustration

As a further illustration of some of these ideas, I want to refer very briefly to the interesting paper by Mosley and Cracknell [1984] who use a "satisficing" reaction function framework in an attempt to answer the question: "if macro-economic policy is thought of as endogenous, and 'reaction functions' reflecting this idea are inserted into a model of the UK economy, what difference does this make to that model's ability to explain the state of the economy?" In a "satisficing" framework, the authorities are assumed to react at irregular intervals to unsatisfactory deviations of targets from desired values. As Figure 1 indicates, the authors endogenised three "instruments" (minimum lending rate, budget tax changes and public sector capital formation) into the teaching version of the Southampton econometric model of 25 behavioural equations and 15 identity equations. The instruments are functions of the usual short-run targets as well as a money growth target and an "election dummy" (which equals one during the three quarters before an election and zero at other times).

The authors, like Pigou [1983], conducted two main experiments. First, they compared the performance of their model with and without reaction functions and found that for six out of eight key target variables, the modified model

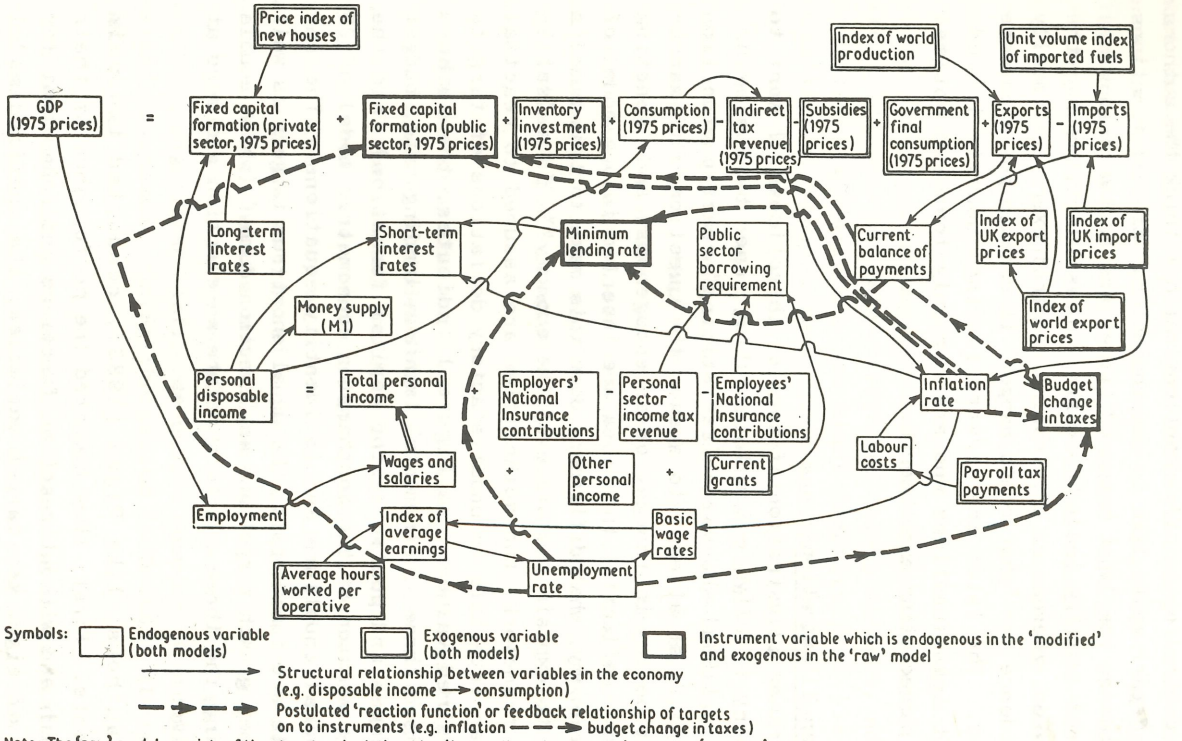

Fig. 1. Flow chart of 'Southampton model'.

Source: Mosley and Cracknell [1984, p.635]

predicted better than the raw model. Their second experiment compared the respective multipliers for a series of targets for a sustained five percent increase in the exchange rate, public expenditures and world prices. They found, consistent with my earlier textbook illustration, that when feedbacks were introduced, the multipliers were generally smaller in the modified model than in the raw model. They concluded, in effect, by calling for more of the work on government behaviour to have an influence on practical policy-making: "the question of how the government *actually will* behave [in major UK forecasting models], even if somewhat predictable, is left on one side when predictions of the economy's future path are made" (Mosley and Cracknell [1984, p.633]).

Recent Developments

Simultaneous modelling of this type has been a relatively recent development. The initial enthusiasm for empirical reaction functions, which began in the mid-1960s and which produced numerous single-equation estimates, had begun to wane a decade later largely as a result of seemingly overwhelming specification and estimation problems (see Silverstone [1985]). Several developments have created the renewed interest in reaction function studies currently being observed (see, for example, Hodgman [1983]). These include the estimation of political-economic reaction functions, the increased use of simultaneous modelling, the implications of the rational expectations "revolution" and experiments with different estimation techniques.

The proponents of political-economic reaction functions feel strongly that there are political as well as economic cycles and these should be taken into account when modelling government behaviour. A typical political-economic model, as Figure 2 indicates, includes links for government popularity and ideology in addition to the traditional links between

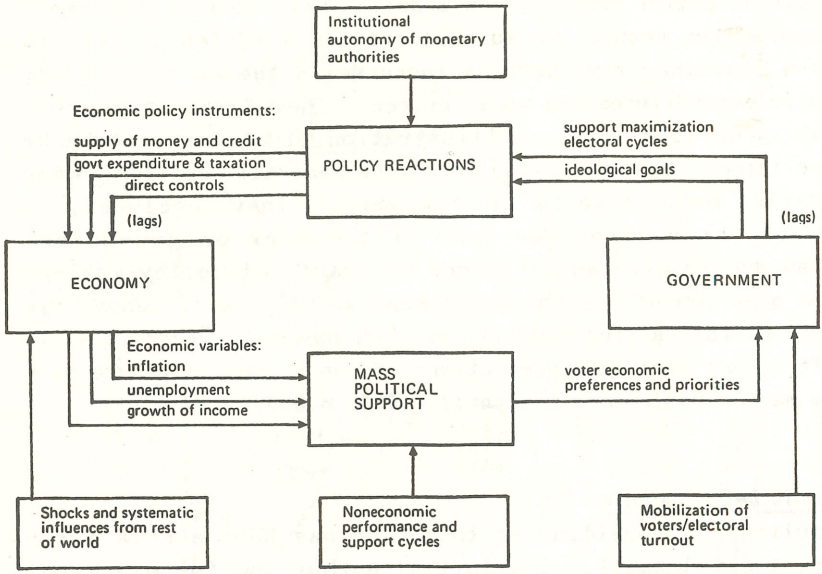

Figure 2. A simplified political–economic system.

Source: Hibbs and Fassbender (eds.) [1981, p.4]

policy and the economy. The development of these models has become a "growth industry" within the reaction function literature. It is a development which is yet to have widespread appeal among economists. Nevertheless, it has generated an important debate on the real or imaginary impact of political influences on government economic behaviour and how such influences, if they exist, might be modelled (see Alt and Chrystal [1983]).

Simultaneous modelling of government behaviour was an

inevitable development given the generally accepted interdependence between policy instruments, say between monetary and fiscal policy, and the economy. A major problem with single equations, or a series of single equations, is that implicitly such equations assume that the goals of policy are being made the responsibility of a single authority which apparently looks at one "instrument" as a measure of success in achieving its multiple goals. "Other things being equal" takes considerable punishment in these single-equation studies and while single-equation work will continue, simultaneous modelling will becoming increasingly important.

The most profound challenge to the traditional reaction function approach, as a theory of behaviour, comes from the rational expectations theory and in particular from the conclusion that systematic monetary policy is ineffective unless it does the unexpected. If expectations are formed rationally, it is claimed, one should not be able to detect either systematic policy reaction cycles or, I would think, certain types of political business cycles. Is this a substantive claim or merely what Sims [1982] has called a "cautionary footnote"? There are persuasive aspects of the rational expectations challenge which reaction function model-builders now have to be aware of (see, for example, Minford and Peel [1983]).

Conclusions

The ultimate test of reaction functions or government behavioural equations, in my opinion, is whether or not they are a useful device for the evaluation, and perhaps formulation, of policy. In my review of mainly single-equation functions, I concluded somewhat pessimistically about their usefulness because of numerous specification and estimation problems which existed in the "first generation" studies. I have now become a little more confident about the usefulness

of reaction functions as a result of recent developments (see Silverstone [1985]).

All empirical research in macroeconomics is confronted with the fundamental problem of discriminating among alternative explanations for actual outcomes, for example, investment behaviour. This problem is particularly acute with reaction functions. In explaining British bank rate, for example, what are the major variables from employment, inflation, the balance of payments, domestic and foreign interest rates, political considerations, the budget, the money supply and OPEC? All these variables have appeared in recent studies and usually with different specifications. The policy-maker is therefore confronted with a bewildering range of explanations for his or her behaviour. Attempting to model this behaviour is the continuing challenge for those who are interested in the study of policy reaction functions.

In comparison to the energies devoted to the study of consumption, investment, money demand and wage and price behaviour, the study of government behaviour has been relatively neglected until fairly recently. Exogenously-given policy specifications remain a persistent feature of basic macroeconomic theory and monetary theory and of many econometric models. I hope these comments will encourage more New Zealand model-builders to consider including a greater degree of policy endogeneity in their models. The information gained may strengthen the answers to contemporary policy questions.

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A Review of Psychological Research of Relevance to Economic Planning

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Introduction

This paper offers a different perspective on economic model building activities than the usual presentation or critique of a model, its assumptions and properties. I considered, first, my perceptions - reinforced while a Reserve Bank visiting scholar in the summer of 1983/84 - of the limited success of economists, with or without modelling assistance, to impact on government policies. Secondly, I considered the heated controversies that can surround economic modelling as well as the personalities promoting their modelling creations. Consequently, my attention was directed to the justification of formalised model building from the perspective of psychological research findings of relevance to economic forecasting and planning, particularly concerning the limitations of human judgmental abilities.

These limitations can be very serious, with fundamental consequences for economic planning. The nature of these limitations is analysed to increase awareness of the psychological weak spots and some practical suggestions are made for enhancing planning methodology to inhibit the inherent judgmental biases. Although the psychological studies cited draw their findings mainly from "laboratory" environments with "typical" cross-section participants, the findings appear to be relevant to economic decision processes and I regret to

report some casual trials of these tests on fellow economists have failed to reveal psychological superiority to those reported in these publications.

The starting point in assessing the relevant psychological research is the fundamental finding that "people are strongly motivated to understand and thus control the environment in which they live" (White [1959]). This motivation can be so strong that people seek to control beyond what is feasible (Langer [1975], Langer and Roth [1975]). It is against this background that we analyse the nature of judgmental weakness. The sources of bias can be categorised as arising from: acquisition of information, processing of information and the output/action/feedback process. For professional psychologists the acquisition of information is a minefield with biases arising from data selection particularly from recall, interviewing and selective perception (Hogarth and Makridakis [1981]). For macroeconomic data these biases are usually irrelevant, although with increasing use of surveys (for example, NZIER Investment Intentions) this could be more serious in the future.

Information Processing

The processing of information is a much more dangerous ground for judgmental biases in macroeconomic planning and is worthy of a more detailed analysis:

(a) **Excessive conservatism:** people fail to revise opinion on receipt of new information, that is, they have less than Bayesian adjustment (Armstrong [1978a], Edwards [1968]).

(b) **Poor extrapolation:** people tend to under-estimate from exponentially increasing processes (Tversky [1979]).

(c) **Joint probabilities:** people tend to over-estimate the probabilities of joint occurrences of several events (Bar-Hillel [1973], Cohen *et al.* [1972]).

(d) **Reflection of preferences:** people exhibit risk-aversion when facing choices between probabilistic beneficial prospects, but are risk-seeking when choosing between adverse prospects (Kahneman and Tversky [1979]).

(e) **Heuristics:** excessive faith in "rules of thumb" (Langer [1975], Tversky [1974]).

(f) **Small sample:** too much reliance is placed on small samples which are likely to be atypical (Tversky and Kahneman [1971]).

(g) **Regression bias:** failure to allow for "regression to the mean" with extreme values used to predict the next observation (Campbell [1969], Kahneman and Tversky [1973]).

(h) **Decision environment:** considerable evidence demonstrates that time pressure, information overload, distraction, stress and group pressures adversely affect judgment (Asch [1951], Makridakis and Hibdon [1979], Pollay [1970], Wright [1974]).

The Output/Action/Feedback Process

The output/action/feedback process, which is crucial to successful economic planning and control, has been shown to

possess a number of psychological traps for planners:

(a) **Wishful thinking:** people's preferred outcomes are given higher probabilities than evidence justifies (Armstrong [1978b], Morlock [1967]).

(b) **Illusion of control:** the forecasting and planning process can induce a confidence in control over uncertain events (Langer [1975], Langer and Roth [1975]).

(c) **Success/failure attributions:** the tendency to attribute failure to bad luck but success to one's skill; this can reinforce the illusion of control (Ross [1977]).

(d) **Selective learning structures:** observed outcomes yield incomplete information about relationships which can lead to excessive confidence in ones own's judgment. For example, you can assess the worth of previously implemented decisions but not those rejected (Einhorn [1980], Einhorn and Hogarth [1978]).

(e) **Gambler's fallacy:** occurrence of similar chance outcomes leads to expectation of higher probability of an event not recently seen (Jarvik [1951]).

(f) **Hindsight bias:** in retrospect people are not surprised by past outcomes: they find plausible explanations (Fischhoff [1975, 1977]).

When the information/action/feedback link is short and

frequent, people's adaptive skills quickly enhance improved decisions and corrective actions. The skills associated with "motor tasks", such as in driving a car or in skiing, which exhibit fast feedback loops, are readily amenable to human judgmental skills. But where short feedback loops are lacking, the psychological deficiences catalogued above seriously impair judgment, and in economic planning the feedbacks are neither short nor clear.

Long run (in excess of two years) planning poses the greatest difficulties. Forecasting is inaccurate (Ascher [1978], Gold [1976]): comparisons of "expert" forecasts with even simple quantitative models have tended to show the superiority of models (Armstrong [1978b], Fair [1979], Goldberg [1976], Ledingham [1976] and Sawyer [1966]). The most recent evidence supports the more sophisticated econometric models (Kahneman and Tversky [1979]). This forecasting superiority of simple models over "experts" has also been found for the medium term (one quarter to two years) particularly for predicting turning points (Makridakis and Hibon [1979], McNees [1979]).

In the short term (less than 3 months), forecasting is relatively easy because most economic systems exhibit considerable inertia where even simple mechanistic time series models yield accurate predictions and can out-perform theoretically more elegant econometric models (Armstrong [1978a], Makridakis and Hibon [1979]).

Implications

There are a number of similarities between the findings of the psychology of judgment and evidence on economic forecasting and planning. The "illusion of control" is comparable to the success of economic planners in the sixties which was subsequently undermined by the oil crises of the seventies. This

tendency to underestimate the uncertainty results in planners being surprised too frequently by the unforeseen. In the words of Paul Samuelson [1974, p.51] "the greatest error in forecasting is not realising how important are the probabilities of the events other than those everyone is agreeing upon". Greater use should be made of sensitivity analysis to assess the relative robustness of alternative planning strategies to forecast errors. Furthermore, because people are confronted by "too many surprises" the models should be subject to examination with a greater variety of inputs and structural modifications.

Subscribing to many forecasting services can be a danger since the additional information will increase confidence in judgment without necessarily increasing predictive performance and offers scope for selecting the forecast which supports a prejudice.

Forecasts are inherently uncertain but Bertrand Russell proposed that people should learn "how to live without certainty, and yet without being paralysed by hesitation" (Russell [1961]). The planning process should recognise the uncertainties and not assume forecast accuracy. Indeed the specifying of and adherence to precise goals is a questionable practice in the longer term as it can hamper adaption, since experience and environmental changes modify values. Bray [1975] suggests selecting a trajectory over time which can be adjusted *en route* in contrast to the traditional "stop-go" policies which have characterised United Kingdom macroeconomic management.

Recommendations for Economic Planners

The thrust of the findings from psychological research is that human judgment has severe limitations in economic planning. The biases of subjective judgement can be reduced if

structural models for forecasting and planning are adopted, particularly when approached with the attitude that such models are "to be used and not to be believed", to use the famous quote of Theil [1971].

In the context of the New Zealand Planning Council's review of economy-wide models, on-going modelling would greatly benefit from greater access by other researchers than has traditionally occurred, to examine a model's sensitivity to different inputs, structures and assumptions. These outside researchers are likely to be less blinded to a model's deficiencies than the model's parents who have nurtured it with such tender loving care.

In this age of more open government, when models are financed from public funds, there should be an obligation to disseminate information more effectively. Printed discussion papers are less than adequate and modellers should be prepared to supply computer-readable detail on the data and structures to facilitate simulation by outsiders. Interrogating the reliability of a macro model can make an excellent project for a post-graduate researcher with potential benefits to all parties. The Reserve Bank, in particular, deserves praise for its enlightened attitude in encouraging these developments (Devine [1984], Lye [1985]).

The concluding recommendation is also on the theme of improved communication. At the University of Canterbury, economists share their Department with the operations researchers. It has become apparent that the OR approach to modelling has been increasingly and consciously adopting a strategy of more involvement of decision-makers at the model construction stage. They do not go off on their own to do their model-building and then present their results, which is the way the econometricians usually operate. Rather, they integrate management into modelling at the earliest stages.

The operations researchers claim that this involvement can enhance model-building and facilitates acceptance of recommendations derived from the modelling.

Economic model-builders could learn from this experience and strive for greater involvement by senior advisors and ministers in the model forecasting/planning exercise. In order for advice to be more readily understood, accepted and adopted, there is a need to consult the decision-takers early and draw them into model usage.

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General Discussion

Graeme Wells (Victoria University of Wellington) opened the general discussion. He said that in reflecting on the day's interesting activities, he had in mind the purpose of the Seminar, which was to address the place of quantitative economic models in policy formation and in the understanding of economic behaviour. He wished to make some brief comments in two broad areas: first best versus second best and model robustness.

First best versus second best. In the light of the discussion in the Treasury [1984] volume on *Economic Management*, it was interesting, but not unexpected, to have the views expressed on the cooling of interest by policy-makers in quantitative modelling. This was most clearly expressed by Murray Horn but it was also expressed by others during discussions. As he understood it, the loss of enthusiasm for the use of models as a guide to policy formation stems from a perception that the major current policy issues are those which involve changes in the regulatory environment, rather than in the traditional instruments of policy such as taxation structures.

The argument seemed to be based on the proposition that it was self-evident that competitive markets will provide a "better" outcome than government intervention, such as the major projects investment. On this view, the focus of attention had moved away from Computable General Equilibrium (CGE) models, which might be used to find an optimal allocation of investment, towards consideration of changes in the regulatory environment which would make markets more competitive and efficient signallers of relative scarcities. This argument leads to the conclusion that a better resource

allocation could be obtained by this route without the need for, or modelling of, any specific changes in traditional instruments.

One problem with this view was that the characterisation of a "better" outcome was rather fuzzy. What we all probably mean is an outcome in which there was a Pareto improvement. From general equilibrium theory we know sets of sufficient conditions which guarantee that a unique Pareto-efficient resource allocation exists. But that theory was not a terribly useful guide to policy in a world in which the sufficient conditions do not hold. In that case, the theory of second best was possibly a better framework for organising ideas about the formation of policy. The major relevant characteristic of this theory was that in evaluating the effect of moving towards a competitive outcome in one market, one needs to know quite a lot about the departures from marginal conditions which actually apply elsewhere in the economy. It was precisely this sort of information which could be incorporated in a CGE model. In that context it seems that there was still an important place for such models in the policy-makers' tool kit, even if the current focus of attention was on incentive structures and market competitiveness.

To tackle the matter from a slightly different perspective, one of the valuable insights from the CGE trade and protection models has been that the allocative gains from the removal of protection are typically quite small, unless one believes that there will be large X-efficiency gains from an increasingly competitive environment. It would be interesting to know just how important the general appreciation of that quantitative CGE result has been in leading to increased interest in the area of regulatory reform. If, as he suspected, it had been important, then he would draw a different conclusion from that reached by Murray Horn.

Perhaps there are equally important, but as yet unknown, lessons to be drawn with respect to intervention with regard to social welfare policies and income distribution. Certainly it was true that very little was known about the effect of government interventions on income distribution in New Zealand. Given that reform in these areas was likely to be high on the policy-makers' agenda in the next few years, it seemed shortsighted to withdraw support for CGE modelling because most of what could be learned, has in fact been learned from the current crop of trade-oriented models.

Model robustness. Graeme Wells' second comment related to dynamic macroeconometric models. He began by considering Richard Smith's paper on the "Lucas Critique" of policy evaluation by means of model simulations. Given the underlying assumptions, Lucas-type criticisms were unassailable, but, as Richard Smith correctly pointed out, the relevance of the critique to any particular policy problem was ultimately an empirical matter.

It was too early to be precise about the appropriate modelling strategy to follow (although see, for example, Buiter [1981] for a discussion of modelling strategies in pursuit of policy-invariant structures). But if one accepted Richard Smith's doubts about the validity of using the currently available models for policy simulations, it was clear that a good deal more could be done in exploring the robustness of macroeconometric models. If the comprehensive analysis of the consumption function by Davidson *et al.* [1978] was taken as a standard of comparison, then present structural macroeconometric modelling in New Zealand was a good way behind best-practice technique.

One must be careful about drawing the incorrect inference from parameter variation in successive estimated versions of econometric models. It may be the case that neither the old

nor the new structure has been explored in a particularly comprehensive way. In fact, the only recent case where a particular structural relationship has been at all "done over" in New Zealand was the recent flurry of interest in the real wage-employment relationship. But even that illustration did not come up to the standards of specification and data analysis set in the consumption function example just cited. However, if a way could be found to encourage further interchanges of that kind, then we would be in a better position to assess the charge that structural relationships are unstable across policy regimes.

One small step which would be particularly fruitful in this respect would be to give much more attention to providing easier access to data bases in use by, say, the Reserve Bank and Project on Economic Planning modellers. The problem was not so much in getting access to the numerical data, but for the data-generators to provide a systematic and comprehensive account of the way in which the data were derived.

Graeme Wells' final remark concerned the future of quantitative modelling work in New Zealand. In the *Report* on modelling which stimulated this Seminar, he and his co-authors were surprised by the innovation and breadth of economy-wide modelling in New Zealand. However, they did not claim more for it than an impressive beginning. While he thought there was more scope for private sponsorship of modelling work, it would be the policy-makers who would ultimately be the agencies responsible for funding the "public good" component of this work. It would be a great pity if that component were eliminated because of the present preoccupations of policy.

Brian Easton (New Zealand Institute of Economic Research) said an issue that worried him across all models was the presentation of simulation results in which point estimates were

given rather than supplementing this information with a range of possibilities. A PEP study, for example, showed that on average a one percent cut in real wages would create 9000 jobs on average each year. But was the number 9000 or statistically a number between -53000 and +71000? We cannot tell from the PEP study. We have very little feel just how accurate these sort of numbers are and this problem went through an enormous amount of our work and had important implications for policy. If he was at the Seminar representing the Federation of Labour, for example, what weight would he give to taking a one percent real wage cut for an extra 9000 jobs given this "number problem"? The *Report* specifically mentioned simulation of model error structures to give some indication or feel about the accuracy of that issue. So error structures are one of the problems we ought to think more about in our modelling.

Richard Manning (University of Canterbury) said that this problem had been addressed elsewhere, at least as it applied to GE modelling. At a recent Econometric Conference there was an experiment where elasticities were used in the structure of a model and the modellers used the standard errors of the parameters to simply crank out the distributions of the endogenous variables. More recently, Adrian Pagan had attempted this exercise analytically.

Brian Easton replied that this situation was satisfactory when something was known about the sampling distribution of the estimates. But some crucial numerical data, such as input-output data, had unknown sampling distributions.

Alan Woodfield (University of Canterbury) said that prior to the *Report* there had been relatively few surveys of economy-

wide New Zealand models. Certainly there had been little public work on the models in terms of international journals. So in many respects economy-wide modelling had been a kind of "in-house", or New Zealand, effort rather than an outside effort. He thought that critical peer reviews from outside New Zealand were a good thing in their own right.

Alan Woodfield said he wished to discuss the evolution of the PEP models, raise one or two other matters where the work of the PEP models appeared to be a little different from what one might see in other received work and make a couple of points about the issue of where PEP might go in terms of modifying their models.

When he first read the basic papers on JOANNA and JUILANNE put out by PEP, it seemed to him that they were consistent with the belief that general equilibrium (GE) means "market clearing everywhere". These PEP models commenced with market-clearing everywhere and with fixed primary factor supplies. They were initially static models used to produce counter-factual exercises of the type: if there was a different policy vector, how would the world have looked in comparison with how it did look?

He thought the PEP extensions had been meritorious in terms of the detail in them: the sectoral disaggregation of production, the addition of further primary factors instead of one homogenous class of labour, the addition of more general functional forms (from simple Cobb-Douglas to CES and CRESH production functions and linear expenditure systems on the demand side) and the dynamic versions of JULIANNE. He thought these were all worthy developments. Yet he found it a little curious that the policy emphasis in many cases had tended to be on short-run macroeconomic issues.

Many PEP papers had considered structural issues relating

to the disaggregated, sectoral composition of output and employment, but in many cases, the policy emphasis seemed to be on macro variables: changes in aggregate employment and output, the price level and the balance of payments. Given that the initial models were designed to be market-clearing models, this concern was not typical of what the overseas literature would have addressed. That literature would have addressed more the sectoral composition of output and employment. In particular it would tend to disaggregate on the demand side, if possible, and to differentiate between different categories of consumers and consequently examine tax and tariff policies especially from the point of view of changes in the welfare effects on different classes.

This is a side of the GE models he would like to see developed. On the other hand, the policy impacts that have been on the short-run macro side, say in short-run JOANNA, have gone away from the fixed-factor supply market-clearing type of assumption. The introduction of non market-clearing sectorally-fixed capital stocks and tax or trade policies have been geared to what seemed to be macro issues rather than micro issues. In addition, no monetary policies were addressed in the PEP policy emphasis on macro issues.

If this was so, if the emphasis was going to continue on the short-run macro side, he would like to see a rather different specification of the way in which the labour supply, for example, was modelled. The JOANNA model, say, moved from a totally-fixed factor stock (in terms of persons) to worker hours. That seemed to be quite a jump and he was not quite sure what was happening to labour supply or labour hours.

The other thing he would like to see, if PEP was going to have a macro emphasis, would be the introduction of money. At the moment it appeared that when you introduced money into a GE model, which was a longer-run market-clearing everywhere

model, money was just another commodity which you introduce as the *numéraire* commodity.

Bryan Philpott (Victoria University of Wellington) thanked Alan Woodfield for his helpful comments. In reply he said that the emphasis and origins of the PEP programme had always been on the longer-run structural resource allocation questions of the sort Alan Woodfield mentioned. However, one responded to the client's demands and if these were short-run questions, and your research grant was limited and you wanted to make sure it was going to be renewed, then you responded to the client by doing what he or she wanted.

A number of issues that were structural in nature were also related to what we choose to think of sometimes as being short-run in nature. For instance, the introduction of GST in 1986 was not going to be looked at in a GE context as far as he was aware, yet it ought to be. It had been looked at by PEP before and this work disclosed some very profound sectoral influences. But it was really a short-run sort of question. It could not be looked at in a GE model without introducing the sort of routines that one would not normally be too worried about in such models like the budget, tax rates and the flows of finance or indeed, as Alan Woodfield suggested, bringing in money itself. As regards money, he had hoped to introduce it into JOANNA, but this had not proved popular with the Project's sponsors.

Professor Philpott wanted to stress that there was a major critical aspect to the long-term modelling structure. This aspect was that the long-term modelling structure could not be done until we knew what the sectoral levels of protection were. This included not just tariffs, which were "a piece of cake", but import licencing and controls. He had hoped the money spent by the Treasury on overseas consultants to measure

the level of the effective rate of protection would have provided this information, but it did not. What it provided was a very interesting essay on how to do what all of us have known for years what to do, but have never had the funding to do it.

PEP have, however, done some work in this area and the first results from the JULIANNE model now give us greater confidence that when average levels of tariffs or specific tariffs were changed, the correct incidence on the different sectors was obtained.

Alan Woodfield hoped he had not misrepresented PEP on the level of aggregation. He thought he saw an emphasis on macro rather than structural issues to a greater extent than one might find in overseas studies and he was interested in seeking an explanation. Since we all seemed to agree that there was a substantial macro emphasis in PEP output on the basis of giving the client what he or she wanted, was all the disaggregation necessary?

Bryan Philpott agreed that disaggregation was not necessary to get the macro results. Nevertheless for a macro-type question, like the incidence of GST, he thought the more sectors one had the better because PEP models were used for consultation purposes and entrepreneurs were interested in the results of disaggregation.

Conrad Blyth (University of Auckland) concluded the Seminar with two comments. First, most of us, he thought, would agree that we must always rely on theory as a starting point in our work. Secondly, there was a continuous research challenge to discover the more permanent structures in the economy. The

challenge was to try and separate what appear for the time being to be the permanent structures from the more variable parameters and transient features. This was a continuing research exercise in economics given the learning process and the range of curious things that governments can do.

As one who had started the Economic Monitoring Group (EMG) off on the economy-wide survey, the question arose of which model to choose from. How many of them were any good at all? He hesitated to say that the Group had come to any final conclusion on that. But he agreed with one of the *Report's* major conclusions, namely, that for the purposes which the EMG had in mind (which he took to mean the identification of medium-term issues, the identification of objectives, the trade-offs and the paths to those objectives), then no one existing model of the ones reviewed really served the EMG's purposes.

In the light of the *Report* and the discussion, it was his view that new models needed to be developed. This raised difficult questions of who was going to pay, whether the work was best done inside the government, a university or research institute. These sort of issues had not really been addressed. But on the general issue as to whether the present range of models needed to be developed and modified or new models added, he was clear in his mind that we have to continue model-building. To stop would be to put too much faith in theory. Although he began his comments with the remark that we must always start with theory, to just stop with theory would lead inevitably to a sort of *ad hocery* which was unsatisfactory.

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