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DECISION-MAKING AND AGRICULTURE

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The Role of Models in the Formulation of Agricultural Policy

1. INTRODUCTION

A well-known definition states that a model is a simplified description of reality. It is then safe to assume that agricultural policy, as well as economic policy in general, is always based on models. It is obvious, however, that the theme of my paper contains many more aspects than this particular one. To display at least some of these aspects, I have found it necessary to set certain limits to my presentation.

First, I will discuss quantitative models, based on so-called modern mathematical and econometric methods. The building of such models has now for rather a long time been a subject at the research frontier of our field of science.

Second, within the field mentioned I will especially discuss macroeconomic models. This does not mean that in my view, microeconomic models have no role in policy formulation. The need for knowledge of a microeconomic nature, when constructing macroeconomic models, is also fully recognized.

My third limitation is that I will keep the economic and policy-making systems of the developed market-oriented economies particularly in mind. I am aware that there is a very interesting development of quantitative macroeconomic models in several countries with centrally planned economies and that this may also be true for some developing countries but my experiences do not permit me to speak on these issues. I can therefore only hope that those who have experiences of model applications from centrally planned or developing economies will share these with the rest of us in the discussion which follows.

The particular question identified is thus the role of macroeconomic models in the formulation of agricultural policy within developed market economies. This question is controversial. It is controversial not only at the policy-making level, but also among us agricultural economists ourselves. To support this statement with a number of quotations would be easy. The only generally accepted viewpoint seems to be that, at least so far, in the vast majority of cases agricultural policy is formed without reference at all to the models we

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are interested in. It is therefore a matter of great importance to develop a better common understanding of the models, among policy-makers as well as among ourselves. In my opinion, this task is even more important than the development of more sophisticated and complex models.

If so, how can we identify the specific role that the models could and should play? How can we judge whether or not the models we develop fulfil this role? And, above all, how much of our limited time should we allocate to the building of quantitative macroeconomic models?

These and related questions are, as far as I know, not discussed very much in our literature or scientific journals. Neither does this paper claim to present the answers. My hope is, however, that it will point to at least some of the issues involved.

2. FORMULATION OF AGRICULTURAL POLICY

From our literature and scientific journals it is obvious that the making of policy recommendations is one of the major objects of our science. At the same time, we do not seem to spend very much time on analysis of the policy formulation process itself.¹ What did initiate a particular political decision? How much time was available before a decision had to be made? What information material was collected? How was the material analysed? What influence did the political and administrative framework have on the decision? How was it followed up?

I want to stress the lack of such analyses. Some of you may feel that questions of this type should be left to the political economists or to the political scientists. But is it realistic to expect those professions to take an active interest in agricultural policy? Others of you may feel that you already know the answers to the questions raised, due to active personal participation in the policy formulation. My question to you is then: Why don't you make more efforts to document your experiences, so that they also can be made available to those of us who want to promote the use of models?

Behind my argument is a firm conviction that knowledge about the policy formulation process itself is of vital importance, if the aim is to develop models which are intended to play a role in this connection. For how can we, in the absence of such knowledge, define the role of models or judge whether or not the models developed fulfil this role? This can hardly be done, if we have no clear picture of the alternative or complement to model use, or if our only assumptions are that the existing policy formulation is "highly informal", that it is "unsystematic" and that it is based on "hunch and intuition".

Let me therefore, make a few remarks about the policy formulation process. My starting point is then the description of it in the theory of economic planning. This may or may not be a good picture of how it actually works under real-world conditions.

I am aware that the very term economic planning is controversial, in particular when, as here, it refers to macroeconomic or central planning. So it is necessary for me to be rather precise on this point. There are several definitions of economic planning to be found in the literature. The one I have

chosen has been provided by Professor Leif Johansen of the Oslo University (Johansen, 1974). I quote:

Macroeconomic planning is an institutionalized activity by or on behalf of a central authority for:

- (a) the preparation of decisions and actions by the central authority and
- (b) the coordination of decisions and actions by lower units of the economy

for the purpose of governing the development of the whole economy and its constituent parts so as to achieve certain goals for the economy and to harmonize the development with broader non-economic goals.

This definition claims to be valid for centrally planned as well as market economies. The important thing is, as is pointed out by Professor Johansen, the weight that is put on “decisions and actions of the central authority” relative to “coordination of decisions and actions taken by the lower units of the economy”.

By lower units of the economy is meant, e.g., households, farms and industrial enterprises. It is then obvious that in market economies, the policy formulation aims much more to the “coordination of decisions and actions taken by the lower units of the economy” than to “decisions and actions of the central authority itself”. All the important elements of the agricultural policy underline this statement.

Even so, if we accept Professor Johansen’s definition, it is quite relevant to speak about the existence of central economic planning in the market economies.² It is then also relevant to speak about a corresponding planning model. This model, i.e., the planning model, must not be mixed up with the quantitative macroeconomic models of interest here. I will come back to this very important point later.

Another aspect of the policy formulation process is, again according to the theory of economic planning, its sequential or stepwise character. This specific feature is described by different authors in slightly different ways (see, e.g., Kirschen *et al.*, 1964, and Tinbergen, 1956). The following steps are, however, usually identified:

- (1) recognition of a policy problem, initiation of the policy formulation process,
- (2) analyses and investigations,
- (3) consultations with, e.g., political parties and different interest groups,
- (4) governmental proposal to parliament, discussion and decision, and finally
- (5) execution of the policy measures agreed on.

This description is obviously especially valid in case of a major policy issue. It is then normal, at least in my own country, that the initiation takes the form of a governmental directive to a specially appointed *ad hoc* committee. Members of this committee may, again at least in my own country, be members of parliament and/or governmental administrators as well as experts from different non-governmental organizations and “independent” experts. The

committee is, moreover, charged with the responsibility to report its findings to the government.

Needless to say, the policy-making process may take several different forms under real-world conditions. This may not only depend upon its being a major policy issue or not. Other factors such as if the policy issue at hand is of a repetitive nature or not, the time available for the decision and the degree of political agreement of different levels, may also be of importance in the connection. These features are fully recognized in the literature on economic planning.

If we look at the content of the agricultural policies of the developed market economies, we find a number of common features. As we all know, there is also a number of specific features for each country as to the formulation of goals and as to the choice of means.³ The specific features are, among other things, related to the foreign and trade policy, to the relative size of the agricultural sector and to the prevailing structural conditions within agriculture as well as within other sectors of the economy.

There is one common feature that I want to stress in particular. It is that the goals of policy are in most cases expressed in quite general terms. I mention this because I now and then get the impression that many model-builders argue for more precise goal formulations. They are of course completely free to do so. They should then, however, be aware of the fact that there may be very rational arguments behind the choice of quite general goal formulations. One such argument, trivial but nevertheless important, may be that it is not possible to obtain a needed political agreement about more precise goal formulations.

There may also be even more important arguments behind the goal formulation strategy referred to. These arguments are directly related to the basic concept of central economic planning within a market-oriented economy.

The major role of this planning is, as I have already pointed out, to coordinate the decisions and actions by the lower units of the economy. It is then quite obvious that the more precisely the overall policy goals are expressed, the stronger will be their influence on the decisions and actions by the lower units. This effect may sometimes be in agreement with the prevailing political and planning principles. In other cases, it may very well be in conflict with these principles. This is the case when it is felt that more precise goal formulations would limit too much the role of the decision-making at the lower level.

Another argument for general goal formulations, also related to the basic concept of central economic planning within a market-oriented economy, can be derived from the fact that there is always a considerable uncertainty about the future development. I have only to refer to the international market development, or to the supply response, in order to support this statement.

Let us, with the uncertainties in mind, assume that the policy-making authorities issued very specific goal formulations. Let us, moreover, assume that they really were prepared to take actions to secure the achievement of the goals. Depending upon what the future developments actually turned out to be, the achievement of the goals might require the use of, e.g. far-reaching

quantitative regulations of the foreign trade and/or of the supply. The use of such means may again be felt to limit too much the role of decision-making at the lower level. If so, it is again quite rational to stick to the general goal formulations.

3. QUANTITATIVE MACROECONOMIC MODELS

I will now add a few words about the macroeconomic models. It is outside the scope of this paper to present a full catalogue of all such models, even within the field of agricultural economics (see, e.g., Heady, 1971). More important is to identify the common features of the models and to make some general observations in this connection.

The models of interest describe the whole or at least the major part of the agricultural sector. This description is, in the typical case, quite disaggregated in terms of the number of outputs, production opportunities and inputs. A further level of disaggregation, with respect to regions and/or types of farms, is also found in many cases.

Another feature in common is that the models are composed of a number, often quite large, of mathematical relations or equations. Each equation is then assumed to represent a particular aspect of the underlying economic system. Examples of such aspects are different balances of a physical or of a monetary nature, different technological relationships, different assumptions about the behaviour of the lower level economic units and different institutional relationships such as, e.g., the existence of market regulations.

A third feature in common is that the construction of the models and the estimations of the data needed are in most cases major undertakings in terms of time required and costs involved. At the same time, the calculations are in many cases easily carried out by the use of a computer. Under such circumstances, the models often lend themselves very well to the generation of a number of alternative solutions from alternative specifications of different model parameters. This feature is of considerable importance for our discussion.

There is quite a large number of different models that fit the description given so far, with respect, e.g., to how the time factor is dealt with, to the types of data used, as well as to the mathematical techniques employed to generate the model solutions. I will not, as I have already said, go very deeply into these questions. There are, however, two observations that I want to make.

My first observation is related to the purpose of the model calculations. As is well known, we may distinguish between models for forecasting the development of the economic system being studied, on one hand, for determining a preferable state or a development path of the system, on the other.

In the first case, when the purpose is to make forecasts, the development of the system studied is viewed as a function of factors which cannot be controlled by the policy-makers as well as of the application of different policy instruments. Moreover, it is implicit in this approach that the evaluation of the alternative forecasts generated and, consequently, also the choice of values of the policy instruments, has to be made outside the models.

In the second case, when the purpose is to determine a preferable or optimal state of the system studied, we have to introduce an explicit representation of the relevant policy goals. This is done via the introduction of an objective function, which is to be maximized or minimized, and sometimes also via the introduction of specific restrictions into the equation system of the model.⁴

In this case, as well as when the purpose is to make forecasts, we also have to make assumptions about the factors which cannot be controlled by the policy-makers. A difference is, however, that as a rule we cannot determine the values of the policy instruments, if our purpose is to determine the state of the system which is optimal in relation to the policy goals specified.⁵ As a consequence, the determination of the policy instruments that lead up to the calculated optimal state usually has to be made outside the models.

Although there are important differences of principle between the two approaches mentioned, there are also a number of similarities. The choice of approach is therefore a practical question that has to be determined in view of the policy problem at hand.

Against this background it is a little surprising how we model-builders often debate quite intensely the pros and cons of, e.g., econometric models in relation to programming models. This discussion is perhaps even more surprising since, as a rule, it does not give many references to the underlying policy problems.

Behind the debate I think one can trace the fact that the whole field of quantitative macroeconomic models is a large one. We, who are interested in the development and application of such models, therefore tend to specialize within one particular sector of the whole field. As a result, we develop vested interests in the promotion of the models that belong to our own particular sector.⁶ Our discussions of the different models are then not seldom a reflection of the power struggle within our field of science and it is this reason that I argue for a better common understanding among ourselves about our models. It is hard to see how the models can play a proper role unless such an understanding can be developed. Perhaps this conference could play a role to this end.

My second observation is related to a question of terminology. We often present our programming models, when they include explicit assumptions about the policy goals, as so-called planning models. But in my experience this frequently creates a lot of misunderstandings when the models and their results are discussed with administrators and policy-makers.⁷

An example may illustrate the possible reasons for the misunderstandings. Let us assume that the policy-making authorities are considering the introduction of active measures to promote a certain regional development within agriculture. Let us assume, further, that they ask an agricultural economist to study the problem and that he does so by means of an inter-regional programming model of the standard type. His results would then consist of one or several location patterns, together with the corresponding cost estimates.

If the economist then claimed that he had prepared a "plan" for the location of agriculture, there would be good reasons for misunderstandings.

First of all the policy-makers could feel that it would be completely outside the prevailing policy-making framework to prepare such a plan. Secondly, the policy-makers would probably remind the economist that if such a plan is to be prepared at all, several other types of information in addition to those represented in an inter-regional programming model would have to be made use of. Thirdly, the plan that the policy-makers themselves had in mind would probably be quite specific about the actions to be taken, rather than about the expected "end" results. They would therefore not understand the economist who claimed that he had prepared a "plan", if he did not show at the same time how it could be achieved.

So, in order to avoid misunderstandings, I think it would be very wise not to use the word "planning" in connection with our macroeconomic models. Though I am not certain which terminology we should use instead and am open to different suggestions. I am, however, quite sure that we should reserve the term "the planning model" for the policy-formulation process itself.

4. ROLE OF THE MODELS

Let me now return to the questions I raised in my introductory remarks. The first one was: How can we identify the specific role that the models could and should play in policy formulation?

In an interesting paper (Johansen, 1971) on planning theory Professor Johansen, whom I have already quoted, has defined what he calls "the maximum function" of a model. A model is said to satisfy this function if it is used "to calculate which is the best possible decision and if the decision then is implemented".

Is this "maximum function" what we have in mind, when we develop models about different aspects of agricultural policy? Or, to carry the argument one step further, do we like Simon in his book *The Shape of Automation: For Men and Management* foresee a situation when we will be able to gather all relevant data in a gigantic information system? And be able to develop the models needed to analyse all the data? A situation when the information and model systems are supervised by a group of "managers", sitting in front of computer display units, and whose closest collaborators are model and programming specialists?

This vision of "the maximum function" is certainly not that of Professor Johansen. Nor, for that matter, is it my own vision – not even if we limit our discussion to policy issues of a "pure" quantitative nature. I base my view on several arguments, some of which I have already indicated. One of these is that what can be expressed by numbers and equations is only part of the information needed for policy decisions. Moreover, the character of the decisions that have to be made is changing all the time, due to the dynamics of the technological, economic, social and political developments. On the other hand, the construction and introduction of new information and model systems take a considerable time. When a policy-decision has to be made, would it not then be very irrational to rely only on the particular results coming from a formal information and model system?

So it is safe to assume that our models could hardly satisfy "the maximum

function” mentioned. As a personal observation I would like to add that even if they could, they should not – not be allowed to, if you wish – play such a role. For this would mean that we would have to go over to a policy-making system that certainly would be much more authoritarian than the present, unless every citizen obtained a PhD in econometrics and computer programming, which seems to be highly unlikely.

Which are then the arguments that we model-builders usually use, when we want to promote the role of our macroeconomic models in policy formulation? These arguments are, as far as I have been able to find out, of the following types:

- (1) that the models permit a large number of relationships to be analysed simultaneously,
- (2) that the models permit even large volumes of data to be analysed systematically and
- (3) that the models permit the generation of alternative model solutions within a relatively short time and at low costs,

and moreover, as a consequence of the properties mentioned:

- (4) that the models can increase knowledge of the planning situation and also that the models can improve decision making on the basis of existing knowledge.

To be able to evaluate these claims, I have looked for comments from people holding responsible positions in policy formulation at the national and international levels. I have found several remarks about the role of our science in policy formulation. Some of those remarks, e.g., that agricultural economists should take more interest in real-world problems and that we should use a more understandable language, may have a connection with our model-building activities. But I have certainly not found any comments that take up directly the claims that we use to justify our work. How should this silence be interpreted? It seems reasonable to assume that the responsible policy-makers are not unaware of the fact that model-building is an important activity of our science.

Perhaps the policy-makers, as well as some of ourselves, are still uncertain if our claims are justified? This assumption does not seem unreasonable in view of the fact that model-building is after all a relatively new activity.

Or is it that the preparation of quantitative forecasts, and quantifying the consequences of alternative decisions, are tasks that are in practice carried out at lower administrative levels? And that we model-builders are wrong in claiming, as we sometimes do, that the results from our calculations are of specific interest at the highest policy-making level?

One feature common to our claims seems to be that they particularly stress quantitative aspects such as the many relationships included, the large volumes of data handled and several alternative solutions possible. Nobody can deny that many of our model studies are quite large in these dimensions. But how strong is the correlation between, for example, the number of relationships and data on one hand, and the quality of the results on the other? Not very strong, I think, if it exists at all.

And moreover, do not the very properties mentioned sometimes tempt us to build large models, very detailed within areas where data are easy to obtain, but with a very limited scope? Are we not tempted to present very detailed results, which may give a wrong impression as to their exactness? Do we not sometimes calculate so many alternatives, that it becomes impossible even for ourselves to identify the main findings?

Moreover, could not claims also be made for the real-world planning model? I assume that it is designed so that the insight and experiences accumulated within different administrative agencies are made use of in a proper manner. And that persons with knowledge about different spheres of economic and social life are actively engaged in the different steps of the policy formulation.

By now I may have given some of you the impression that my own view is that the quantitative macroeconomic models have no role to play at all in policy formulation. As I have indicated earlier, this is not the case. I have only tried to argue in the spirit of the Swedish author Strindberg, who told us that we must demolish the old structures to let in air and light.

My own view is that quantitative macroeconomic models certainly could have a role of their own to play in policy formulation, as one source of information among several others. They could be a tool to identify which of the assumptions made – about policy goals, instruments and non-controllable factors – are especially important for the outcome of the analysis. Such sensitivity analyses could in my view very well lead to a more efficient use of the total resources available for data collection, analyses and investigations.

The models could moreover form a basis for bringing together information from several different sources and thereby maybe lead to new formulations of real-world problems. However, this role is certainly more modest than the one some of us seem to have in mind when the claim “to improve decision-making” is made. After all, what is a wise decision?

If you accept my own view on the role of the models, channels are essential for communications between various agencies, especially those whose task it is to prepare material for policy decisions and the model-builders. This has been pointed out by several authors. Some authors have suggested that there should be certain persons entrusted with the specific task of acting as communication links. The motive for this suggestion is that a good ability to develop models, and a good ability to communicate with other people, are not always found together.

I must confess that I find the idea of such intermediaries rather peculiar. The reason may be that I come from a small country where we cannot afford too many types of specialists. At the same time I feel that having such intermediaries would only add to the present confusion. How can a model-builder who is not prepared or able to make personal efforts to obtain insights into policy issues develop models of an applied nature? And how can he do this, if he is not prepared or able to explain in an understandable way how the models are constructed and what the findings are?

My second introductory question was: How can we judge if our models fulfil their role or not? In the literature one can find quite a lot of suggestions concerning the evaluation of forecasting models but very little about the evaluation of programming models, designed to illustrate the choice between

policy alternatives. Professor Schultz made a rather grim remark on this issue when he said: "I am regretful we have not even tried to resolve this problem". Personally I am not sure that the situation is much better when it comes to the forecasting models. I have in mind the tests that could show if our models do play a role in the policy formulation or not. I, moreover, assume that we are satisfied if our models serve as one source of information, among several others, for policy formulation.

One idea could then be to test the models in relation to "the maximum function" earlier mentioned. But such a test would most likely be negative in almost all cases. If the test on the other hand turned out to be positive, this would, as I see it, rather be the result of pure coincidence than of anything else. So this type of test would not be helpful. The tests that I would like to propose would rather be to ask questions of the following types: Was it possible to explain the construction of the model to the administrators and the policy-makers? Because if it weren't, they would be unlikely to pay attention to the findings. Did they ask for modifications of the model and for more alternative calculations? Did they spend a considerable time discussing the findings? Did they explicitly refer to the model study in their own report? If they did, my own conclusion would be that the model has played its proper role.

Maybe the ultimate test of this type would be the following: Did the administrative agencies want to take over the model and use it themselves for further studies? Or did they claim that after all, the model was their "own baby"? They would certainly do so if they thought that the model was a very useful one because they would feel that access to the model could strengthen their own role in the policy formulation.

The type of tests now proposed should, however, by no means be a substitute for our usual professional tests. I have in mind, for instance, if the assumptions and the models were clearly described, if the study could be repeated by someone else, if its validity were described and if the results were presented independent of prejudice. I rather think that these tests should be applied more vigorously than they sometimes are.

The two types of tests argued for could occasionally give different results — for instance, where a heavily promoted model study, which was quite poor from the professional point of view, reached the attention of agencies with little earlier experiences of such studies. But this would probably only happen once. So I am convinced that the two types of tests would need to yield the same result, if our models are to play a role in a longer perspective.

My last introductory question was: How much of our limited time should we agricultural economists allocate to model-building activities? The answer is related to our personal aims, so the question is a very general one. Let me assume, however, that our aim is to put ourselves in a position that enables us to make policy recommendations. What are then the alternatives open to us, as agricultural economists in particular?

There are then several alternatives open to us. Among them are the identification of new sources of statistical data, the description of the current development in a consistent way, the forming of new theories about the factors behind the development, the discovery of new policy instruments and, of

course, also the building of quantitative models that could summarize existing knowledge. Which alternative should we then choose? That is a decision that I think each one of us has to take on a personal judgement of his or her comparative advantages, after making a careful analysis of pros and cons of each alternative in relation to the current state of knowledge.

5. CONCLUDING REMARKS

I will not try to summarize my discussion, so my concluding remarks will be very brief.

I am aware of the fact that I have not been able to give very precise answers to the questions I raised in the beginning of my paper. Nevertheless, I feel that my approach has been justified. For before one can give the answers, one has to formulate the questions.

When thinking about my concluding remarks I have looked to our literature for a good way to characterize the particular activity of developing quantitative macroeconomic models. The best I have found has been formulated by Jan de Veer. I can only agree with him when he has said (de Veer, 1971) that "the building and operation of these models is itself an act of policy-making".

NOTES

¹One of the few works that I know of, where not only the models but also their policy-making framework are dealt with, is Miller 1973. Another pioneering work, which gives a description of the Swedish approach to the formulation of agricultural policy, is Anderson 1972.

²The term "indicative planning" is sometimes used in this connection.

³See, e.g., OECD, 1975.

⁴I have in mind the case when the goal structure includes at least one goal dimension which has the character of a flexible target in Tinbergen's terminology. This is, as far as I have been able to find out, the typical case for the agricultural policies of most of the market-oriented economies.

⁵There may, for example, be a number of alternative values of the policy instruments that can be expected to lead to the same state of the system studied.

⁶In this respect, I look at myself as a rather typical "programming man".

⁷The same observation has been made by a Norwegian colleague of mine. See, Reisseg, 1971.

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DISCUSSION OPENING – Q. B. O. Anthonio, *Nigeria**

Dr. Petit started by explaining that the general model of adaptive behaviour was designed “to establish close interrelationship between reflection and action”. After defining agents, environments, goals, feasible domains and the agent’s perception, the author lucidly analysed the close relationship between perception, learning and acting. This was followed by a full discussion of adaptive behaviour and the role of models which opened with the – albeit controversial statement – that “at any point in time the agent has a perception of his situation” (not necessarily) . . . “of his goals” (not always) . . . of his possible actions, and some of the values he holds . . .”.

In the discussion of classification of models the author seems to reject the notion of the normative versus positive models on the basis of Glenn Johnson’s (1976) dictum and evolved the “better” phraseology of “optimization model” instead of normative model and preferred the word “positive” to be in quotation marks. I feel that this is rather hair splitting. The crucial issue really is that “optimization” is, after all, a normative concept no matter how one wants to redefine it and inclusive of the fact that other objectives (e.g. income or consumption needs) impose constraints on any one objective.

At one stage of the discussion on model classification, the paper becomes rather muddled. To be specific, the author asserts that “Thus, if one speaks in terms of norms, the main purpose of an optimizing model is not to produce goals (“norms”) but rather to question the “norms” implicitly, or explicitly, accepted by the decision-maker”. (p.) I am totally at a loss in reconciling “norms” and optimizing. I would have assumed from the author’s own definition of goal, (“Agents act in order to modify their situation with respect to their environment. Their action is thus postulated to have a purpose. They have *goals*”) that optimizing is, of course, a goal and thus consistent with a type of “norm”. Whether or not linear programming correctly and adequately represents this “norm”, is the key question. And if it does, whether farmers behave in such a way as to optimize a particular constraint or objective is, to my understanding, again, a different question.

The argument that “the usefulness of models depends more on the role given to them by the analyst rather than on the type of model used”, is not

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tenable, since the capacity to operate the model itself is highly dependent on the type of model, its versatility, and relevance.

In most developing countries, and particularly in areas where computer facilities can be a constraint, over-complicated models make a mockery of the concept of models as an aid to the decision-making process. They may become too cumbersome, too costly and time consuming.

I am frustrated by the assertion that “failure of a model to capture essential aspects of reality does not necessarily invalidate the theory”. (p. 74n). The question then is, what help is the model giving to decision-makers in the light of this type of uncertainty, especially at macro level analysis which often builds within it a whole series of aggregative malfunctioning (or errors)?

Professing that “all economic models are not decision models” is really begging the question. To my understanding of this exercise, the choices to be made have to be obvious and in fact to precede the hypothesis to be tested, the thesis to be confirmed; the models give the decision-maker the opportunity of choice by illustrating expected consequences of alternative decisions.

The author took us through a kind of structural classification of models. Discussing the advantages and disadvantages of these models (econometric) (1920), programming (1940's), and simulation models (1960's), the author believes that the best of the lot is the simulation model since it is a form of system analysis and permits of a very wide range of uses. I agree with this, but mention should be made of the fact that because of the lack of scientific rigour in this model, it permits a whole lot of errors which in most cases cannot be crosschecked and the unwary believer can easily be led astray. For policy-decisions based on such a model, these errors can give rise to gross misallocation of resources and an outright unsavoury position. This situation should not be overlooked.

Our profession is constantly being criticised by decision-makers on the basis that we allow slipshod work to pass, using data of doubtful authenticity to build up magnificent designs that are not only expensive but of limited practical use. As scientists, even social scientists, we have to search more and more for useful models with built-in verification. The underlying “philosophy” is important, but so are the data and model that correctly explain this “philosophy”. Personally, I consider these three aspects, philosophy (or hypothesis), model used, and the data incorporated, constitute an eternal triangle of interrelated “variables” essential in any meaningful analytical economic model.

Perhaps the frightening portion of the paper is in the conclusion that “scientific objectivity is an illusion”. If that is correct, and I doubt if it is, what is the whole exercise about models for? Is it not to be more rational and to use more scientific concepts to improve decision-making? What are optimization, maximization, cost minimization, all about – scientific or mathematical concepts and functions are surely intended to illustrate rational optimum positions.

There is no doubt whatsoever that models are very useful in decision-making. Unfortunately, the types of models which our professionals have produced, to the best of my knowledge, have in no way engendered confidence

in the minds of decision-makers, or even solicited it. Far too often, unfortunately, it is the mechanics of such models that are given the greater attention. In this respect, I agree with the author and state finally that “model formalization can be a source of progress . . . provided that it produces reliable and sufficiently flexible models at a reasonable cost”.

DISCUSSION OPENING – P. Reider, *Switzerland*

Dr. Folkesson raised all the important questions relating to interregional models but they are too many to answer fully. Regarding the relationship between model-builders and decision-makers, the former is too narrow a term. The builder must be deeply knowledgeable about agricultural policy and politics. With such experience good, problem-orientated, country or region specific, models can be built.

Decision-makers are not interested in methods so it is not a question of “selling” models but of using suitable methods for solving problems. A model-builder who is an agricultural economist with a thorough understanding of agricultural and economic problems can give useful and effective advice to the decision-makers out of his experience of applying models. He knows the problems of government and what government decision-makers must know to be in a position to make the best decisions.

A search for features common to a number of models may not be very useful. How to build a model is often a matter of what is practicable. The question of objective functions cannot be reduced to simple terms either. Since each policy may have several goals, different objective functions must be considered. But it is not possible to treat the whole complex of agricultural goals with mathematical models. Because of this complexity, I think the best approach is usually to apply the model to calculating the outcome of different alternatives – for instance, the effect on agricultural income distribution of different support measure.

Finally, I believe that only the builder understands his model well enough to evaluate it. Consequently it is only to be expected that evaluations in the literature will be scarce.

Report of the general discussion

The importance of models in promoting rigour in policy analysis, the specifically LDC problems in using models, and the pros and cons of simple models were aspects common to several contributions.

On the rigour aspect the importance of developing a framework of thought for the policy-maker, the value of simple models, the contribution of models to greater discipline in analysis, seemed to meet general agreement in principle. Where a simple model, could be developed involving a small number of variables, and showing clearly the economic mechanism and the behaviour of economic institutions, the alternatives were easier for the policy-maker to grasp.

Though simple models might have such attractions it was felt that, in fact, decision-making in agriculture was by its nature complex and, since models contributed only in so far as they contributed to a knowledge of reality, it was unlikely that simple models would be realistic. Departures from realism were seen to take several forms. Assumptions about economic relationships and the decision-making framework – often that of North America or Western Europe – with the resultant models, were liable to be applied in a much too imitative way in developing countries. Application of sophisticated models is pushed ahead before theory has been modified to reflect local conditions. Into what model, for instance, can the decision process be fitted which placed a Nigerian cocoa farmer in a position now of great socio-economic influence because, as a young man, he spent his first income on buying a chieftaincy instead of buying land?

Distortion and irrelevancy were traced to deficiencies in data – sometimes directly from error, sometimes because absence of the necessary data forced modification and simplification of models beyond a reasonable relation to reality.

The less specifically economic features needed more attention in model-building than, so far, they had received – for example, political and administrative considerations. The significance of the increasing impact of agricultural producers and workers on decision-making at all levels was also stressed; so too was its importance.

Apart from this general core of discussion, various specific questions were posed. For example, since agricultural economists could be presumed to be scientists and concerned with facts, was not the positive their natural approach; the normative seemed more suited to priests or politicians. And if so, any contribution in a normative vein invited frustration. Or again, what meaning was to be attached to “simple” in relation to models? Who was in mind? – Presumably non-economists. But in that case it was “operational” that should be stressed – an operational model could, through its results, be understood by administrators and politicians.

Participants in the discussion included: J. A. Akinwumi, *Nigeria*; A. T. Birowo, *Indonesia*; P. C. van den Noort, *Netherlands*; L. A. Odero-Ogwel, *Tanzania*; K. Prasad, *India*; D. Tomic, *Yugoslavia*.

M. Petit (in reply)

It has been questioned whether it was reasonable to assume that at any point in time the agent has a perception of his situation, of his goals and of his possible actions. The question is put whether this does not assume too rationalistic a pattern of behaviour? A similar point is raised by the question whether a model could take account of the decision by a cocoa farmer to buy a chieftaincy rather than land. My claim is precisely that the general adaptive behaviour model provides us with an approach to building more specified models capable of taking account of such behaviour. Saying that our agent has a per-

ception of his situation, his goals and his possible actions does not imply that these perceptions are clear or even free of inconsistencies. Understood in this manner, I do not feel that these assumptions are unrealistic.

About the distinction between normative and positive models, it is precisely because I feel that the distinction is misleading that I have used the expression "optimization models". For reasons already indicated, I do not agree with the claim that optimization is normative.

The failure of models to capture essential aspects of reality does not invalidate the theory on which these models are based. I am surprised that this should surprise anyone. For instance, there are many good modern agricultural economists in Western Europe using Marx's theory in spite of Marx being totally mistaken about the evolution of agriculture in Western Europe. Similarly the numerous failures of market mechanisms do not prevent us from using neo-classical market theories.

I would concede that I may have overstated my case in emphasizing that the usefulness of a model depends more on the role given to it by the analyst than on the type of model used. This might imply that the type of model has no importance. Of course such is not my position. I have, in fact, written that some models will be adequate in some circumstances and other models in other circumstances.

The point that it may serve African colleagues to have a paper incorporating models of North American, or West European origin published in international journals – even though they are of little practical use to Africa – is very well taken. I submit that the general adaptive behaviour model is general enough to raise good questions about the behaviour of farmers everywhere, permitting economists to conduct "concrete analyses of concrete situations" and thus follow Lenin's excellent prescription.

Finally I must record two further points of disagreement. I do not believe that we should confuse philosophy and hypotheses. The former deals here with the scientist's relationship with society; the latter with the way society, or more precisely in our case the economy, is working.

I maintain that pure scientific objectivity is our illusion. Our main task as social scientists is, in my view, to increase everybody's understanding – including our own – of social problems – in other words to make knowledge more objective, knowing full well that it will never be fully objective.

Dong Hi Kim (in reply)

Agricultural economists should pay more attention to what we do not know about how to sell the products of our research to potential customers in their diverse situations. It seems to be a necessity for successful co-ordination in modelling for agricultural development to maintain a minimum balance in basic knowledge and attitudes toward the problems and analytical approaches between the analysts and the decision-makers.

On adaptability, simplicity and operational feasibility of a decision model, the concept of adaptability of a planning or a decision model can be expanded

so that a practical decision model in agriculture includes the behavioural variables of the micro decision-making units. Secondly, the theoretical frameworks and analytical models which have been developed mainly in the western industrialized countries should be made adaptable to meeting the current problem-solving needs of the underdeveloped countries.

The operability, and the simplicity, of a decision model is important so far as it fulfils the specific needs of an analysis or a planning situation. However, a simplified model, in terms of structure and variables covered, can hardly be meaningful for the explanation of a complex situation or for the planning of a bio-sociological system such as agriculture. By contrast, a rather sophisticated model can better serve for comprehensive analysis or for workable planning in agriculture.

L. Folkesson (in reply)

I would agree with Dr. Rieder that my paper raises more questions than it answers. I mentioned this in the paper. But my paper has served its purpose if the questions taken up are the relevant ones.

My definition of a "model-builder" has been held to be too vague. When I began to write the paper, I intended to be more specific but it turned out to be difficult to draw a clear picture of a "model-builder" without, at the same time, drawing a picture of an agricultural economist — and an economic advisor in general, for that matter.

As regards the role of a model as a tool to obtain a better understanding of practical policy issues, I fully agree with this statement if it is read as better understanding among policy-makers. I, myself, also think that the sociological aspect is a very important one and had it in mind when I stressed the role of our model as a link between different experts as well as between experts and decision-makers.

We have confined our discussion to the role of models in relation to policy-making, but a valuable alternative would be to look at them as educational tools. This question is very well worth a seminar of its own.