# THE USE OF MODELS: EXPERIENCE AND PROSPECTS

**JAN TINBERGEN***

## 1. ESSENCE OF MODELS

In this lecture I propose to discuss the experience we have had with the method of model building as a contribution to economic science and the prospects for its further application. First of all I want to remind you of the *essential features of models.* In my opinion they are: (i) drawing up a list of the variables to be considered; (ii) drawing up a list of the equations or relations the variables have to obey and (iii) testing the validity of the equations, which implies the estimation of their coefficients, if any. As a consequence of especially (iii) we may have to revise (i) and (ii) so as to arrive at a satisfactory degree of realism of the theory embodied in the model. Then, the model may be used for various purposes, that is, for the solution of various problems. The advantages of models are, on one hand, that they force us to present a "complete" theory by which I mean a theory taking into account all relevant phenomena and relations and, on the other hand, the confrontation with observation, that is, reality. Of course these remarks are far from new.

While building models econometricians were often forced to *supplement "literary" theories,* since these often did not specify all relationships they were implicitly using.

Models have been built for a number of different purposes; first of all, for purposes of explaining actual developments and next, for finding ways of influencing actual development in some desired direction. Another aspect is whether short-term or long-term movements were the objective of either explanation or policies. There are large numbers of further alternatives of focussing. We will discuss some of them in this lecture.

## 2. SOME EXPERIENCES

First, I am going to discuss a number of experiences econometricians had with the activity of model building. Some of us were masters in hunting after high correlations, that is, *good fits* with observed values. In fact this was part of the art. Some of our critics thought this was easy enough and indicative of the futility of the activity. It wasn't always so easy, however. Some of the fits in our models never became very good, or, if finally they had been forced into a high correlation, broke down a few years later. I am afraid that the first subject I tackled in my work for the League of Nations, namely to explain the fluctuations in *investment* activity, never has become a great success. In the Netherlands Central Planning Bureau we found it safer, after some years, to ask industrialists for their investment programs rather than rely on an econometric explanation. Also government expenditures were among the variables difficult to explain. In both cases we may account for the lack of success by the fact that a small number of decision makers determine the picture and that hence random deviations will be important.

In a more general way many of us know that quite a few business cycle models were "forecasting" the turning points only *after* they had occurred. Ragnar Frisch (6) was quite right when, at an early stage of model building, he introduced *random shocks* as an essential element of the business cycle, leaving the cumulative process between turning points rather than the latter themselves as a thing that really could be explained by the models. Even so some turning points can be explained by the inner dynamics of economic systems.

In a number of cases models were hardly necessary to clarify some features of reality. I could not help thinking of this class of cases when recently I saw as a conclusion of one recent model that "Japan was a success in development". I thought we knew that already. Let me add, however, that the same model did explain something more.

During our hunting for good fits we did sometimes *learn,* as it should be. Thus, annual price fluctuations in beef prices could only be explained satisfactorily by the introduction, with a negative sign, of fodder prices some time before. High fodder prices force peasants to slaughter part of their livestock and hence depress beef prices. This was an aspect my collaborators and I had not been aware of and which

---

*Erasmus University. Lecture J. Tinbergen delivered in Stockholm, Sweden, December, 1969, when he received the Nobel Prize in Economic Science. 

text books on agricultural economics did not explicitly state. Another example was the explanation of the fluctuations in the general wage index in Britain before 1900 (20). No good fit could be obtained unless as one of the explanatory variables an index of mineral prices were included. After an intensive search I found that among the wages in various industries those for miners fluctuated by far the most and that for quite some time there prevailed a sliding-scale arrangement linking miners' wage rates directly with wholesale coal prices, something inconceivable to-day.

In several parts of our science, and I presume in other sciences as well, we must beware of following *vogues* too easily. Model building has become a vogue, just as, after that, linear programming or matrix algebra have become. Of course warnings against vogues are first of all coming from those who don't command the techniques implied. This is why I am myself inclined to hesitate to apply one of the two latter methods mentioned. But a critical examination of the structure of the problem before we try to solve it remains useful. And let me add immediately that linear programming does constitute a very useful technique in many cases indeed.

Returning to models, I am sometimes wondering whether, upon looking at some recent work by planners, I should not repeat the famous words by Goethe's Zauberlehrling "Die ich rief die Geister werd' ich nun nicht los" ("The ghosts I called I can't get rid of now"). Sometimes indeed some of our followers *overdo* model building.

In an attempt to evaluate what model building has contributed to the theory and practice of economic science I feel that at least we can say that models have had a *didactic* value. Often in our text books we bring simplified, not to say over-simplified, pictures of reality which nonetheless contribute to making understood some essential features of that reality. This is true already of some models inspired by Lord Keynes' fundamental work (10). It is true also of Leontief's input-output models (13). If I am allowed to quote a recent example I am guilty of myself, the same can be said of models introducing the difference between tradables and non-tradables (14). This model shows that if a country wants to eliminate a balance of payments deficit by living within its means, that is, by reducing its expenditure to its income, income itself is bound to fall and not so little.

What I called "didactic value" also stands for *communication* value. The ability of a planning expert to communicate with politicians and with citizens constitutes an important element in any type of democratic or semi-democratic planning and such communication can be enhanced by relatively simple models. In order not to misrepresent reality, however, there will be a need for a succession of models, as used in planning in *stages* or, as we now say, multi-level planning (12).

I do think, however, that the utility of models goes beyond their didactic value. They are a real and essential element in the preparation of well-coordinated policies. But they cannot do this job all by themselves. Models constitute a *framework* or a *skeleton* and the flesh and blood will have to be added by a lot of common sense and knowledge of details. Again, as framework they can be of vital importance. Some of the simplest models were sufficient to show that the investment programmes recommended by the World Bank in its early days were not of the necessary *order of magnitude.* During the Great Depression already the same could be said of some of the programmes of an anti-cyclical character.

The framework I am referring to supplies the main ingredients for coordinating government policies at the level of a central government, that is coordinating the policies of the various ministries. Already many of the details concerning one ministry only would require the introduction of partial models, or could at least be left to them.

For short-term models sufficient time has elapsed already since their construction started, in order to *test* their *forecasting performance.* Several publications of the last ten years or so dealt with that subject, comparing, among other things, forecasts made in Scandinavian countries, Britain and the Netherlands. One score of success has been the number of *turning points* correctly predicted; this score is interesting since so-called primitive forecasts, that is extrapolations of past movements, are unable to produce turning points. Some models have been able to correctly forecast two-thirds of the turning points.

A need generally felt by model builders and their critics is the need for *refinement,* that is, for the introduction of many more variables. In a way this experience again was a lesson also to economists in general, since often their arguments run in terms not showing this degree of detail. One example we in Benelux experienced: real development showed that the grossly increased volume of trade between the three countries did not imply that whole two-digit industries were wiped out in one or the other country, but only much smaller *subsectors.* Here one has to introduce hundreds if not thousands of different products in order to do justice to reality. The same applies to the problem of the *optimal division of labour* (7) among all countries of the world, although the establishment of such an optimal division of labour may wipe out more important parts of two-digit industries. Here we tried to apply the Heckscher—Ohlin principle in a very concrete way. This also implies that for the choice of the best investment projects of some developing country much preciser information is needed than ordinary statistics can give us. It is a well-known experience that even so-called project data are far from sufficient to design the optimal development policy.

Two other examples of the need for refinement of models may illustrate the case. One has been often mentioned by Erik Lundberg (15) in his analyses of *anti-cyclical policies,* mainly financial policies. Partly we have a need here for much smaller time units and corresponding information, for these time units, on a number of relevant variables. Among the variables are a number of expectations not usually collected by statistical bureaus or even central bank statistical departments.

The other additional example of the need for more refined models and information can be taken from the experiences of the United Nations Research Institute for Social Development (UNRISD). The essential feature of this Institute's work is to include a number of so-called *social* variables. Leaving apart the question of how to define these variables—at present we have three different definitions competing—in two ways refinement is needed. On one hand, information about many more aspects of social phenomena is needed. Taking education as an example, the usual data available, such as enrollment, are too crude and should be specified, say, with regard to the type of education. On the other hand, data for smaller geographical units are needed, closer to what in other contexts is known as the "grass roots". The intuitive judgment of several sociologists that research and inquiries at this lowest level are by far more productive than the macrosocial research undertaken by UNRISD should be so interpreted (17). This implies that the question is not whether quantitative models are or are not productive. Precise knowledge about interrelationships can be obtained only by the technique of quantitative models; but the lack of homogeneity of crude information is the reason for the lack of success in the social area and hence refinement of the base material is the real need here.

All these refinements will make for ever more complicated models and therefore threaten to make models unmanageable. This once again underlines the need for several stages of decision-making and hence of planning. As already said, the need for communication with the people and groups involved also points in the direction of this step-wise use of models. So also does the *organizational* aspect of decision making; a correspondence between the organizational setup of an optimum socio-economic order and the levels or stages of planning and the use of models for it is desirable. One of the future features of such a setup will also be the more precise location of the flows of *information* and, more particularly, the type of information needed.

## 3. PROSPECTS OF MODEL BUILDING

Let us now turn to the future of model building. The primitive state of the art has become clear from our preceding remarks. We have already indicated some of the directions in which models will have to be developed. Our present subject, the prospects of model building, of course overlaps with the subject discussed so far. As a consequence, some repetition cannot be avoided; but we will approach the subject from a somewhat different angle. Our central question will now be what extension should be given to the scope of model building. Some of the developments to be discussed have already been started.

A first subject to be dealt with refers to the necessity to introduce the element of *space* into socio-economic models. For a long time this aspect of economic science has been neglected. Relatively few authors have dealt with it. As a consequence there is a clear gap between economic models on one hand and the practice of town and country planning or transportation planning on the other hand. Town and country planning is carried out more often by engineers, architects, geographers, and sociologists than by economists. Economic science may and has to contribute, here too, to some more co-ordination between the contributions made by the other sciences or arts just mentioned. In his dissertation Bos (3) has offered some interesting new approaches to the problem of the optimal spatial dispersion of economic activity, using some results of Serck Hanssen's. The main new entities entering the picture after the work done by Losch are *"centres"* which is a concept covering villages, towns and cities of all sizes. Centres are clusters of production units and the complementary dwelling units. The main problem is how to group the production units into centres of different size and composition so as to maximize welfare of, say, a country, under a number of constraints. Some of these constraints are production functions in the usual sense, others are the specification of transportation costs arising from differences in location. In a recent publication Mennes and Waardenburg (16) have elaborated on the subject especially for larger spaces, usually called regions, countries and continents. In some cases they arrive at satisfactory approximations by what might be called *"two-stage Hitchcock solutions",* that is by the consecutive application of two Hitchcock or transportation problem solutions, a simpler brand of linear programming.

Together with Herman (8) the same authors have elaborated elsewhere an example of what could be called a *multi-level semi-input-output model* (4). By this model they are able to take into account the existence of more than one level of "tradability".

Some products may be tradable between regions of one country but not between countries. Other goods may be tradable between the countries of one continent, but not between continents. As a consequence the expansion, in a given region, of an international industry may entail necessary investments bound to the region, others bound to the country and still others bound to the continent. The impact of such an expansion on the economies of the country and the continent concerned are similar to, but more complicated than, the impacts studied by the semi-input-output method with one level of non-tradability.

While these various examples show the picture of lively activity in the field of the economics of space, it is clear that some fundamental features characteristic of the subject can hardly be included already into the methods used. On one hand, empirical data are lacking on such features as *external effects* and on the other hand mathematical techniques are lacking—or at least not known to my collaborators, let alone to myself—needed to generalize sufficiently the models developed as examples.

A second subject to deal with, in discussing the widening of the scope of economic models, is the inclusion of so-called *social* and *political* variables. As already stated I do not intend to discuss to-day which of the three alternative definitions of social variables should be preferred. In all three most of *education* will be considered a social activity. Some models of education have been developed of late (19). Because of the long lags involved, the education process is one of the best examples of the use of difference equations, even if Balogh's warning (2) is taken into account that quality aspects are particularly important in this field and sometimes are the aspects in immediate need of change. With the increased awareness of the role of education in economic and social development and with the budget for education being among the largest budgets of the various ministries, the need for further work in this type of model can easily be understood.

Another social subject is *income distribution.* Models for its explanation as well as for its manipulation have been developed, some of them long ago already. As in other cases the need for a large number of variables is quite clear also here. Correspondingly, there is a need for a large volume of information especially with regard to the description of jobs offered by the production process and of skills available with the population. For the last thirty years a tremendous material on job evaluation has become possible, mostly for manual workers and adminstrative personnel, using an about twenty-dimensional vector to describe jobs. It should be possible —although some psychologists deny this—to arrive at a corresponding vector describing the available skills. Elsewhere (21) I described a model which may serve as a framework to find the resulting distribution of labour income. To be sure I used two dimensions only in the concrete elaboration; but there are indications that the twenty dimensions used in practice are intercorrelated to a considerable extent. Some earlier authors have worked with one dimension only; sometimes the IQ is used and another proposal has been (22) to take the degree of leadership as the one variable describing a man's ability to produce. These must be oversimplifications, of course, but with far less than twenty criteria one could probably attain a satisfactory first approximation. As a quasi-capability component a person's wealth may be introduced. Models of this kind may soon be used to study in a more precise way also the possibilities to change income distribution and, for example, to find out what degree of inequality is inevitable. In the light of the renewed interest for these questions—I think of Jamlikhetsrapporten of SAP and LO—models of this kind may well be elaborated further. The main policy instruments to be used in order to reduce inequalities, tax and education policies, can be built in into the model without difficulty.

A third example of the simultaneous introduction of many social and political variables into models, especially for developing countries, is the impressive attempt made by Irma Adelman and her collaborators (11) using factor analysis and discriminant functions in order to discover, which of some thirty odd factors, measured in a heroic way, seem to play a preponderant part in the process of development. One may wonder, with Tjalling Koopmans, whether such "measurement without theory" is meaningful; as an exploration of a new territory of science I think it is. But prolonged discussions of the type already started during the Christmas meetings of the American Economic Association 1968 will be needed to disentangle the complicated and daring combination of theory and testing produced by three women, for which they deserve our admiration (5).

The third subject to be discussed under the heading of widening the scope of models is the one of *specifying optimum socio-economic orders.* Optimization is not a new subject, of course. Mathematical programming models are now widely used, both on the level of the production unit and on higher levels. Scientific development took place along the various tracks. Among the most sophisticated we have had, in recent years, dynamic models for long, even infinite time periods, such as developed by Phelps (18), Tj. Koopmans (11) and M. Inagaki (9). Among the results attained by these authors are the limits set to our freedom to choose some parameters which we might have thought we are free to choose, such as the time discount occurring in a preference function. For some value intervals of this parameter Koopmans has shown that no preferential ordering of the various conceivable development paths is possible. Results of such as these belong to the really fundamental features of economic science. Inagaki has introduced the other fundamental idea that some older models only apply to a "society of immortals", though not meant for the French Academy. He then introduced the concepts of the *generation at time t*, representing an "ensemble renouvele" or a self-innovating set, and *"instantaneous government"*. I cannot discuss this subject any further, since I did not study it in sufficient detail, for lack of mathematical knowledge.

There are two other tracks, however, which I want to sketch out. One is the discussion of the social welfare function, or the objective function. I share the opinion of those, like Frisch and Bergson, who think that the scientific strategy of official welfare economics has not been optimal. By this I mean that, in our opinion, it is better first to specify the social welfare function as precisely as possible and then to use it for finding the socio-economic optimum. In a discussion with Kornai about his outstanding book I proposed that both East and West try to specify their social welfare function so as to see whether the ultimate aims are very different or not. Hopefully some thorough work will be done on this subject in the coming years.

The second other track I want to sketch out is to *reformulate* the problem of the socio-economic optimum. The true unknowns of the problem are not so much the quantities of consumption and productive effort to be made and a few more traditional unknowns, but rather the *set of institutions* which taken as a set are able to approach the welfare economic optimum as well as possible. So far the method followed by some of us, interested in this version, has been to formulate the conditions the optimum has to fulfil and then to indicate a set of institutions which, by their behaviour equations, would produce the same conditions. Thus, in olden days, when too simple production functions were assumed to represent the available production processes, men like Adam Smith or Vilfredo Pareto suggested that private enterprises and competitive markets would do the job. Today we look at these things somewhat differently and arrive at other suggestions. One particular subproblem worth being mentioned is the problem arising from the existence of *costs of institutions*. Some types of taxes, for instance, show quite considerable costs to collect them. How do we have to deal with these costs if the institutions to be chosen are unknown beforehand? The phenomenon of costs of institutions requires a reformulation of the optimum problem so as to take into account these costs if and only if the institution causing them is chosen as an element of the set of institutions constituting a solution to the optimum problem. Some first attempts by my collaborator Waardenburg make us hope that a way out will be found.

It is also our hope that the interpretation of the socio-economic optimum as a set of institutions may help to get under way a discussion of a more scientific character than was usual so far about the relative merits of various existing socio-economic orders, especially those of Eastern and Western Europe, including such interesting cases as Sweden, Switzerland and Yugoslavia. A considerable amount of better information about various types of external effects will be one of the necessary ingredients if we want to give concrete content to such merit rating of various systems. It is my hope that in such a way we may again, as Marx claimed, find scientific arguments in the competition between various systems, but up-to-date scientific arguments rather than obsolete ones. This more fundamental research in economics deserves relatively more attention and resources than the more superficial versions of economic research directed at forecasting or analysing very short-term fluctuations in market prices, on which quite some money is being spent to-day.

## REFERENCES


1. Irma Adelman and Cynthia Taft Morris, Society, Politics and Economic Development. Baltimore 1967.
2. Thomas Balogh, in a discussion organized by OECD around 1964.
3. H. C. Bos, Spatial Dispersion of Economic Activity. Rotterdam 1964.
4. Peter A Cornelisse and Jan Versluis, "The Semi-Input-Output Method under Upper Bounds", in: H. C. Bos, ed., Towards Balanced International Growth. Amsterdam—London 1969.
5. Peter Eckstein, Quantitative Measurement of Development Performance: A Critique of the Adelman—Morris Model. Ann Arbor, 1969.
6. Ragnar Frisch, "Propagation Problems and Impulse Problems in Dynamic Economics", in: Economic Essays in Honour of Gustav Cassel. London 1933.
7. B. Herman and J. Tinbergen, The International Division of Labour—A Quantitative Illustration, Netherlands Economic Institute. Rotterdam, 1969.
8. B. Herman, L. B. M. Mennes and J. G. Waardenburg, "Some Exercises with a Simple Model for World Development Planning", in: H. C. Bos, ed., Towards Balanced International Growth. Amsterdam—London 1969.
9. M. Inagaki, Dissertation Rotterdam (to be published); cf. also some contributions mentioned in (11).
10. J. M. Keynes, The General Theory of Employment, Interest and Money. London 1936.
11. Tjalling, C. Koopmans, "Intertemporal Distribution and "Optimal" Aggregate Economic Growth", Cowles Foundation Paper No. 269 (Reprint from Ten Economic Studies in the Tradition of Irving Fisher, 1967), where other works are quoted.
12. Janos Kornai, Mathematical Planning of Structural Decisions. Amsterdam 1967.
13. Wassily Leontief, Essays in Economics. New York, London, Toronto, 1966, where previous work has been quoted.
14. Ian M. D. Little, unpublished report; the concepts have been used also by Iversen and Leontief.
15. Erik Lundberg, Konjunkturer och ekonomisk politik. Stockholm 1953 (also available in English).

16. L. B. M. Mennes, Jan Tinbergen and J. George Waardenburg, The Element of Space in Development Planning. Amsterdam 1969.
17. Gunnar Myrdal, Asian Drama. New York 1968.
18. E. S. Phelps. Golden Rules of Economic Growth. Norton 1966.
19. Tore Thonstad, Education and Manpower, Edinburgh and London 1969, where many other authors are quoted.
20. Jan Tinbergen, Business Cycles in the United Kingdom, 1870–1914. Amsterdam 1951.
21. Jan Tinbergen, "On the Theory of Income Distribution", Weltwirtschaftliches Archiv 77 (1956), p. 155; also in: Selected Papers. Amsterdam 1959.
22. R. H. Tuck, An Essay on the Economic Theory of Rank. Oxford 1954.