ASSUMPTIONS IN ECONOMICS
AN OPERATIONS RESEARCH APPROACH

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ASSUMPTIONS IN ECONOMICS: AN OPERATIONS RESEARCH APPROACH*

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This paper considers the general problem of the assumption set selected for an economic model in terms of the objectives of the model builder and the dimensioning of the operative domain for the model.

Theories or models are not constructed in a vacuum but are purposive behavior. We take the position that a model builder has a set of objectives in mind when constructing his model. It may be the goal of the model builder to examine certain particular features of a physical system by constructing an "unrealistic" model which contains only the features in question (with an implicit or explicit assumption that the exclusion of all other features does not alter the applicability of the results of tests on the model to the physical reality).[1] Except for certain classes of models which we will not consider, the goal for which most models are constructed is prediction of some future set of values for the physical system.[2]

We propose a set of considerations which are fundamental in moving from the operative domain of the model to the (possibly) different operative domain of the physical system. These considerations relate to the assumption set which defines the operative domain of the model and the (possible) difference between this set and the condition set defining the operative domain of reality.

The first section of our paper considers in some detail the general role of assumptions in theoretical systems. In connection with this, we

discuss the reasons why assumptions are specified. In our next section we discuss some measures of the appropriateness of a theoretical system or model. Here we consider various approaches which have been advanced.[3]

In our next section we indicate through the use of several examples drawn from economic literature the application of the measure of appropriateness of an economic model which we outline in the previous sections.

We should utter a *caveat* at this point to the effect that we do not attempt to exhaustively analyze any single economic model; rather we indicate in general terms how our approach to the evaluation of an economic model (in terms of the objectives which are held for it or which caused it to be constructed in the first place) could be applied.

Section I. The Role of Assumptions in Theoretical Systems

In this section we discuss the role of assumptions in theoretical economic systems. We believe that assumptions are constraints which define the environment in which a particular model exists or purports to operate. If this is the case, then assumptions restrict the operating domain of a model to some portion of a real process which is described by the model.

The assumptions defining the domain of a model allow us to neglect certain effects which, on the basis of the objectives we hold for the model, are not expected to produce serious changes in the results we predict from our model.

The assumption set which is selected, since it defines the working or operating domain of a model, restricts the predictions of the model to a certain portion of reality. This implies that the criteria used to select a particular set of assumptions depend (or should depend) upon the

particular portion of reality in which the model builder has an interest. This is equivalent to saying that for the purposes of prediction or explanation, the abstractions obtained by the assumptions allow us to deal more efficiently with the problem; it relieves us of calculations which will not significantly affect the outcome, be it prediction or explanation.

From a formal standpoint, each assumption corresponds to a reduction in the number of variables and/or equations from the extremely large scale system which defines the "global" state of nature at any one point in time. When an assumption eliminates an equation, variable, or restricts the values they may assume, implicitly we state that the remaining solution space of the system, using the reduced set of equations (for the purposes of the problem in which we have an interest), will be equivalent to that which would be obtained utilizing the full set of equations. There are similarities between this line of reasoning and the arguments supporting decomposable linear programming and associated techniques used in mathematical programming in order to reduce the overall complexity and computation time of the problem.

The conditions under which aggregation of variables can be justified are similar to those under which we may justify assumptions. An example is the widespread use of the Hicks-Lange condition to justify aggregation of variables in an econometric model. It states that if two or more variables always move together, they may be aggregated into a single variable which will be an appropriately weighted average of the original. This criterion is a useful approximation since it allows us to aggegate classes of commodities that are perfect substitutes (or are approximately so).

Methods such as this for the reduction of systems must be selected on the basis of whether or not they constitute "sufficiently realistic approximations" judged in terms of the objectives of the model builder. This ultimately must depend on the degree of predictive precision required by the user of the model in question measured against the relative cost of obtaining an increase in precision. Conversation with a number of professional colleagues has made us aware of the startling unwillingness of economists, so dedicated to marginalism in microeconomics, to apply the same criteria to the construction of their models and the estimation of the parameters of these models. In its most extreme form this position is tantamount to asserting that the marginal cost of additional precision is always less than the value of the additional precision.

The opposite viewpoint is taken by Simon and Ando in commenting on the construction of large scale econometric systems:

> The high cost of inverting large matrices has led to a number of experiments to determine whether aggregation can be used to obtain approximate inverses more economically.

They add,

> We concluded that, over a relatively short period, the first group of terms dominates the behavior of the system, and hence each sub-system may be studied (approximately) independently of other sub-systems.

They support their methodological position, not by developing a goal-oriented set of criteria for selection of one reduction method over alternates, but by mentioning that

> Samuelson points out that aggregation of commodities seeks its justification in two kinds of principles. On the Hicks-Lange condition we aggregate parts when these parts are more closely linked with each other than are the rest of the system,....[4]

Dantzig and Wolfe, commenting on practical aspects of decomposable linear programming, say

> Many linear programming problems of practical interest have the property that they may be described, in part, as composed of separate linear programming problems tied together by a number of constraints considerably smaller than the total number imposed on the problem....It would seem that each of the n sets of constraints constitutes a sub-problem of secondary importance to the whole program, and that they should be studied mainly through the restrictions they impose on the activities of the joint constraints.[5]

The operational role of assumptions is to view the problem in terms of the operating constraints and to exclude those variables which may be considered trivial or alternatively will not affect the final solution space.

Section II. The Appropriateness of an Economic Model

We now turn to a discussion of our criteria for defining the appropriateness of an economic model. Here we should point out specifically that the kinds of models that we are concerned with are what we call operative or decision-oriented models. These are models which are constructed for the purpose of predicting what will occur in the future in a real world system when a selection is made from a set of possible alternative courses of action.[6]

Our definition of appropriateness of an economic model incorporates as criteria 1) the objectives of the model builder for his model; 2) the probability that the prediction domain of the model as defined by the assumptions is insensitive to a transfer from the model domain to the real world; and 3) a consideration of the relevance of the set of variables which have been included in the theoretical model.[7]

First, let us consider several ways in which the appropriateness of models have been measured. The first approach might be called the empirical method of which Churchman says,

> ..., think of Leibnitz and Locke not as people trying to find ultimate authorities of the truth or falsity of assertions--but as people interested in the resources that science has at its disposal in the pursuit of its aim.... The effect constitutes the best estimate we can make at the time of the optimal strategy of science.[8]

We can summarize this approach, as measuring the appropriateness of an economic model in terms of the statistical signficiance of the model's predictions. This approach has developed into the modern positivism in economics of Friedman where the appropriateness of the model is based on its ability to perform the predictive task which is assumed to be the goal of its constructors.[9] Critics of the approach have characterized it as the search for those statistical hypotheses which yield the highest coefficients of determination.

A second means by which models may be evaluated is to examine their logical consistency; this method can be considered to be a modern version of the rationalism of Kant where the appropriateness of a set of assumptions and an economic model is judged in terms of its internal logical consistency.[10]

A third method which can be used to evaluate the appropriateness of models may be called an Hegelian approach. Here it is recognized that there exists hierarchical order in society and that the performance of a sub-system or a system of a lower order should be measured in terms of the system's relationship to the higher order system.

A fourth approach, an alternate to the previous three, is to measure the appropriateness of a model in terms of its consistency with reality.[11] This approach has its foundations in the first two approaches with which it finds great fault. First, we combine the positivism of Locke with the rationalism of a Kantian approach. Then a model under study is evaluated

in terms of its assumptions and the degree to which these assumptions can be expected to accord with reality.

We propose as criteria that the appropriateness of a model be judged not merely by its predictive accuracy, but also by the relationship of its (assumption-defined) domain to the domain in which it is expected to operate. Thus, our suggested means of measuring the appropriateness of any given model would be composed of the following points: 1) an evaluation of the goals and purposes of the model--what do we hope to achieve with it; 2) to what extent can we expect to find the assumptions made to produce the model holding in the real world domain we are attempting to predict.

Through the use of this dual criteria we are answering much of the criticism of the predictive criteria as represented by the following dialogue, "Yes, your model does predict on the basis of a limited experience with the future and comparison with past data. But, how can you guarantee that it will continue to predict?" Answer, "I can infer that it will continue to predict not only because I have examined the model's logical consistency, but an examination of the conditions under which the assumptions hold reveals that the assumptions made in my model will transfer to the real world with a high probability of obtaining."

It is this addition to the predictive test which establishes a dual criterion which allows us to answer a major objection raised against the predictive criteria: that we cannot be sure the predictive mechanism will continue to provide adequate results.

Houthakker seems to support this view when he says,

> We need not go all the way with Friedman in holding that a theory should be judged entirely by its conclusions, but few of us would still want to judge it entirely by its assumptions.[12]

We do not suggest that the theory be judged entirely by its assumptions, but we believe that the problem of logical consistency should relate primarily to the mechanism which is used by the mode. That is, given the assumptions of the model and the manipulations necessary to determine the solution space, there should be logical consistency between the assumptions and the manipulations. However, since the assumptions themselves are the base, we may judge them as they relate to the probability that the conditions implied by the assumptions do in fact hold in the real world.

It should be possible to develop a dual approach to the determination of whether a model is appropriate where the only solution which is both feasible and acceptable is one where we reach the same solution (space) through two alternative mechanisms, i.e., the assumption test and the prediction test. Hence, we will be able to obtain similar conclusions using two distinctly different tests.[13] To discriminate between two theories we would unequivocally say that one was better than the other if it predicted better as measured in an *a posteriori* sense and if in an *a priori* sense it appeared that its assumption set were more probable of existence when transferred to the real world than the other. However, if we were to find that one theory predicted better than the other, yet the other had a more probable assumption set, we would have an ambiguity. We would then say that, given our approach, we have an undecidable case.

Section III. Evaluation of Models

Discrimination between alternative economic models is the underlying problem motivating much of the theoretical and empirical work which is

being done today in economics.[14] We wish to illustrate the application of our criteria for evaluating the appropriateness of economic models (in terms of the objectives of the model). For this purpose we have not singled out any particular models or empirical work in economics, but have selected some recent examples from the literature of economics.

Our first example is drawn from an article on measurements of capacity utilization in the American economy by Klein and Preston.[15] After considering a number of measurements of capacity which have been constructed for the American economy, utilizing certain assumptions and certain data, they produce a new series measuring capacity in the American economy. The interesting aspect of their work (for our purposes) is that in the conclusion they outline and stress the assumptions which they have made to derive their particular measures. They point out that some of these assumptions can be relaxed without changing the measurements that they have made. Other of the assumptions, however, if relaxed or changed, would probably, in their opinion, change their results so that their measurements would cease to be useful approximations.

Klein admits a failing in his system when he talks about the more basic use. Generally, when we speak of the basic use of a time series we mean long-term prediction and, hence, extrapolation. They indicate that they are not nearly as definite, that the suggested series are in fact the correct ones, as they are of the usefulness of their capacity measure. Hence, they must rely on what they might call arguments of intuitive plausibility which, translated into our terms, is to state that they are relying on a judgment that the probability that this assumption set which they have selected will represent reality or the real world over an extended period of time.

We feel more economists should detail and carefully define the limitations of the results that they have obtained either in terms of the predictive criteria and/or in terms of an evaluation of the assumptions criteria as have Preston and Klein.

However, most authors have not been quite so circumspect in noting limitations on the applicability of their conclusions, and to a large degree this reduces the generality and usefulness of these conclusions. Perhaps one reason for the large amount of disrepute surrounding certain points in Arrow's *Social Choice and Individual Values* is the lack of evaluation either of some predictive results or of the probability of the assumption set existing in the real world which accompanied this extremely perceptive and seminal work.[16] For example, a considerable amount of the criticism of his condition of the independence of irrelevant alternatives has taken the form of specifying assumptions which are implicit in his presentation, which in fact are either impossible or extremely improbable in the real world.

We have chosen an example which has not received the attention that the irrelevant alternative condition has received, although it, too, is an important condition in the development of the possibility theorem. The example we choose relates to the dictatorship condition in the decisive set. Briefly summarized, we have the decisive set V which Arrow defines in his most recent edition as "a set of individuals V is decisive for x against y if x is socially chosen when every individual in V prefers x to y, and every individual not in V prefers y to x."[17] Further, he states that the decisive set V by construction has the minimum numbers for a set to be decisive.[18] It is interesting to say that if the decisive set consists of one individual, and it is decisive for an

ordered pair x against y while the non-decisive set prefers y against x, 1) this requires that the decisive set be exactly one individual, and 2) have a preference ordering which is different from those not in the decisive set.

When we move to the real world, we ask what is the probability that those not included in the minimal decisive set will have preference orderings different from those in the minimal decisive set. We observe many examples of voting, using a version of Arrow's senate ratification example where the votes may be as high as 96 to 4 or even 100 to 0. This suggests a condition other than that suggested by Arrow, i.e., a decisive set wherein all individuals not in the minimal decisive set may not have opposite preferences to those in the decisive set. One could, conceivably, take the votes cast by members of the Senate of the United States over any given period of time and determine the number of times that the minimal decisive set condition was met.[19] Thus, we suggest that the problem of determination of the validity of this condition is an empirical question. We need to find a probability α that the minimal decisive set as defined by Arrow will exist.

We assert that α will be less than one, $(\alpha < 1)$; hence, the Arrow conditions will not hold in all cases. To demonstrate that the probability will not only be less than one, but be very low, one has only to consider the probability that the members of the non-decisive set will in all cases have a different preference ordering from those members in the minimal decisive set.

When we transfer this analysis to a real domain, we can see that the conditions necessary for the Arrow proof to hold have low probability. Hence, the applicability of the conclusions and predictions based on this

analysis must be seriously questioned.

We would like to demonstrate a method which may generally be used for most equilibrium models to determine the probability of a set of equilibrium conditions holding continually through time. Given any equilibrium system, one would like to determine the probability over time that the system will remain in equilibrium, given that it is presently in equilibrium or that an equilibrium exists towards which the system is tending. In the case of the former, let us presume the probability of a disequilibriating change occurring on any given day t is very low, say .001. Then, the probability that a change will not occur on day t is .999. Now, in the most stable of the possible situations, we would have serial independence between change on day t and change on day t + 1. If we have a 5-year period, the probability of no disequilibriating change on any one day in the 5-year period is $(.999)^{1825} = .1642$. For ten years, the probabiiity would be $(.999)^{3650} = .02695$ and we can see the probability of not having a disequilibriating change in either a 5 or 10 year period is very low.[20]

This analysis allows us to make statements concerning the real world predictive usefulness of comparative static models for long period analysis. Such models typically assume equilibrium for some period of time but do not include a dynamic analysis of the type which seems necessary once the models are evaluated using our probabilitic analysis of the occurrence of a disequilibriating change.

The next example that we consider relates to a phenomenon that arises when use is made of a quadratic preference function in some commodity space (which we can define as an n-space). There appears to be an eventual bliss point as income increases, i.e., where the income plane

touches a point on the top of the cone. This so-called "bliss point" which is an assumption in those models where it is assented that the problem of scarcity does not exist, depends crucially on the assumption that there is no expansion of the choice space for one individual or for all individuals. If there is such an expansion as the choice space expands or changes, the bliss point becomes a moving point in some larger pre-exist-ing, non-individual dependent large choice space. To the extent that there exists over time a very low probability that the choice space will be invariant for any one individual or group of individuals, then there exists no bliss point and no possible problem of this type. The whole analysis depends on the probability of finding this assumption fulfilled in the real world.

Section IV. Summary

We have looked at one class of models in economics, i.e., those which are constructed with prediction of some specified phenomena as a goal. We have asked questions about the criteria which are used (or could be used) to evaluate these models in terms of their goals. We have suggested that, despite the objections which can be raised on the grounds that assumptions of a model become implications of the model, the addition of an examination of the probability of transfer of the assumption set of a model to the real domain to the logical positivism of Friedman will increase the chance of rejection of an "inappropriate" model, hence raising the discriminatory power of the test.

We have illustrated the application of our approach, in principle, by referring to several economic models which appear to us to represent the range of current practice. Further research suggested by our

considerations lies in the area of 1) empirical analysis of the probability of invariance of particular well-defined models to alteration of the model space (through a change in the assumption set), 2) more explicit techniques for assessing the probability of assumptions existing in the real mechanism with which a model is concerned, and 3) further consideration of the importance for our "dual" criteria of the two-way relationship between 1) the role of assumptions of a model as constraints on the operating domain of the model and 2) their role as implications of the model.

FOOTNOTES

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1. For an interesting and perceptive consideration of the difference between the role of models in the physical sciences and the social sciences, see Olaf Helmer and Nicholas Rescher, "The Epistemology of the Inexact Sciences," *Management Science*, Vol. 6, No. 1 (1959), p. 48.

2. We do not deny that descriptive models are of value in most sciences. We are, however, not interested in developing a set of criteria for discrimination between alternate descriptive models or taxonomic systems.

3. Milton Friedman, "The Methodology of Positive Economics" in *Essays in Positive Economics*, University of Chicago Press, 1950. The Friedman approach has generated considerable journal comment. A representative sampling of some of the work on the methodology of economics as a positive science is Sherman R. Krupp (ed.), *The Structure of Economic Science* (Englewood Cliffs: Prentice Hall, 1966; Jack Melitz, "Friedman and Machlup on the Significance of Testing Economic Assumptions," *Journal of Political Economy*, Vol. LXXIII (Feb. 1965), pp. 37-60; T. W. Hutchison, "Testing Economics Assumptions: A Comment," *Journal of Political Economy*, Vol. LXXIV (Feb. 1966), pp. 81-83; Harvey Leibenstein, "What Can We Expect from a Theory of Development," *Kyklos*, Vol. XIX, No. 1 (1966), pp. 1-22; C. Schotta, "Values in Economic Analysis: A Methodological Issue," *Kyklos*, Vol. XIX, No. 2 (1966), pp. 289-298.

4. Herbert A Simon and Albert Ando, "Aggregation of Variables in Dynamic Systems," *Econometrica*, Vol. 29, No. 2, pp. 113-114.

5. G. B. Dantzig and P. Wolfe, "The Decomposition Algorithm for Linear Programs," *Econometrica*, Vol. 29, No. 4 (October 1961).

6. For a view of models in economics where a distinction is drawn between "predictive" and "analytical" models, see W. J. Baumol, "Economic Models and Mathematics," in Sherman R. Krupp, *op. cit.*

7. For a detailed discussion of this third criterion, see C. Schotta, "Values in Economic...," *op. cit.*

8. C. W. Churchman, *Prediction and Optimal Decision* (Englewood Cliffs: Prentice Hall, 1961), p. 89.

9. Milton Friedman, *op. cit.* For a searching examination of the Friedman predictive criterion in terms of the logical theory of proof, see D. V. T. Bear and D. Orr, "Logic and Expediency in Economic Theorizing," *Journal of Political Economy*, Vol. 75 (April, 1967), pp. 188-96. Although they concern themselves with the meaning of Friedman's predictive criterion, they do not attempt to add an additional criterion.

10. This approach characterizes the methodological position of writers such as G. Debreu, *Theory of Value* (New York: John Wiley & Sons, Inc. 1959) and K. J. Arrow, *Social Choice and Individual Values*, Second Edition (New York: John Wiley & Sons, Inc., 1963).

11. Much of the recent work which attempts to directly relate economic theory to organization theory has developed its underlying arguments from variants of this approach. For example see R. Cyert and J. March, *A Behavioral Theory of The Firm*, Englewood Cliffs: Prentice Hall (1963) and J. March and H. Simon, *Organizations*, New York: Wiley (1958).

12. H. Houthakker, "The Present State of Consumption Theory," *Econometrica*, Vol. 29 (October 1961), p. 705.

13. In the social sciences, as in the physical sciences, it should be comforting to see the same result achieved through two different routes, e.g., the National Income and Product accounting framework where we attempt to achieve a measure of GNP.

14. See for example M. Friedman and D. Meiselman, "The Relative Stability of Monetary Velocity and the Investment Multiplier in the United States, 1897-1958," in E. C. Brown (ed.), *Stabilization Policies* (Englewood Cliffs: Prentice Hall, 1963); D. Hester, "Keynes and the Quantity Theory: A Comment on Friedman and Meiselman CMC Paper," *Review of Economics and Statistics*, Vol. 46 (November 1964), pp. 364-68; and the comments by Franco Modigliani and Albert Ando and Thomas Mayer and Michael DePrano and the rejoinder by Friedman and Meiselman, *American Economic Review*, Vol. LV (September 1965), pp. 693-792.

15. L. R. Klein and R. S. Preston, "Some New Results in the Measurement of Capacity Utilization," *American Economic Review*, Vol. LVII (March 1967), pp. 56-57.

16. *Op. cit.*

17. *Ibid.*, p. 98.

18. *Ibid.*, p. 57.

19. *Ibid.*, p. 52. The authors are aware that the votes cast by the mem-

bers of the U.S. Senate are but an approximate measure of the "true" preferences of the members. However, as the "action" assumes greater importance, or if the outcome is in doubt, the indicated preference should more closely approximate the real preferences. This characteristic should reduce the error in the measurement of the probability that a minimally decisive set exists. For explicit discussion of the role of intensity of preferences in the Arrow system see J. F. Coleman "The Possibility of the Social Welfare Function," *American Economic Review*, Vol. LVI (December 1966), pp. 1105-22.

20. R. Sato, "Fiscal Policy in a Neo-Classical Growth Model: An Analysis of the Time Required for Equilibrating Adjustment," *Review of Economic Studies*, Vol. 30 (February, 1963), pp. 16-23, found that by assuming some "reasonable" values for certain parameters in neoclassical growth models, the adjustment time to equilibrium in response to fiscal policy ran somewhere in the neighborhood of 75 years. He assumed in his analysis that no additional disequilibrating changes occurred after the "initial fiscal policy action" which was the subject of his analysis. Our results suggest that the equilibrium state would never have been reached.