THE QUEST FOR INFORMATION EFFICIENCY:
SOME MACRO CONCEPTS

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INTRODUCTION

AND

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The past several decades have shown a tremendous increase in the flow of information originating in the private and the public sector. Enabled by the development of computers and data-storage devices, the gathering, processing, tabulation, analysis, and (often) publication of all kinds of data is still growing at an undiminishing rate.

Within the macro-economic sphere, the increased participation by governments in various capacities has resulted in a greater need for information in order to plan, guide, observe, and evaluate policies. As of now, however, there appears to be only a weak link between the goal of data gathering and the intended use. When the Economic Council of Canada (1971) quotes Rivlin (1971):

> "In the United States, for example, it has been stated: One can now literally punch a button and find out how many of the poor, by any appropriate definition, are black, or aged, or have six children or work full-time or live in cities. This does not solve the problem of poverty, but it certainly helps in establishing what and where the problem is.",

such button-punching is justifiable only if there is an intention to do something about the problem of poverty. The development of Goal Output Indicators and of Goal Distribution Indicators, as suggested in the same E.C.C. publication, is a great step in the right direction towards a more adequate description of the performance of our social-economic system. But it seems that such a list of statistics should be drawn up in the light of its future application. The efficiency of information should be considered both from the cost and the benefit sides.

Although the last decade has seen ample work on the role of information as an input into decision-making processes, there is much left for further discussion, particularly at the macro-economic level. The emphasis in the literature is still on the merely technical aspects of information processes whereby the choice and design of the optimal system is being considered.

As Gaffney (1965) writes, however,:

> "Not only is more information needed, but more information is needed concerning what information is needed most. Shotgun support for research is not enough. Interaction is needed between research in social policy and in technology. 'Get the facts' is a misleading and artless slogan. Research needs to make sense of known facts, and direct the search for a few key facts needed to guide policy and to serve it."

In other words, there is a need for a procedure to select the types of information necessary to design and carry out the intended programs.

The Dominion Bureau of Statistics, in its brief submitted to the Senate Special Committee on Science Policy, wrote:

> "The process is not necessarily rendered easier by the fact that, in almost all cases brought to DBS attention, benefits appear to exceed costs by a comfortable margin. The difficulty is, of course, that there is not enough resources to do all the good things, and the problem is one of ranking, in order to make the best choices out of many good choices."

The goal of the present study is to develop some concepts which may be used in the measurement of information efficiency. Its scope is exploratory rather than problem solving. The material presented is directed at researchers in the fields of natural resource management, macro-economic policy-making, and statistical-projects design. Nontechnicality has been considered a virtue; academic rigour had sometimes to be sacrificed in favour of readability. Some technical comments have been placed in footnotes (indicated with square brackets) and printed collectively at the end of the study on pp. 93-105.

In Chapter 1, we will indicate the magnitude of data use in the modern society in general, and governmental policy agencies in particular. Subsequently, some problems of measurement and application are mentioned. The notion of information efficiency is developed further on the basis of its cost and benefit aspects. It is being argued that evaluation of information efficiency, however uncertain and complicated, best be performed within the context of a formal decision model for the policy problem, the solution of which requires the gathering of some specific data. Chapter 2 is devoted to some statistical concepts related implicitly to the problem under study. The remainder of the paper is concerned with various models of decision-making and their needs for information. In particular, we will attempt to show the strengths and weaknesses of some of these models from an information-efficiency viewpoint.

We have worked our way through a great deal of literature, although little appeared relevant for our purpose. Some of the latter is reflected in occasionally lengthy quotations in the paper. It is our sincere belief that quoting the masters is preferable to paraphrasing them. References are indicated with the author's name and the year of publication; an alphabetical bibliography can be found on pp. 106-120.

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CHAPTER 1

THE PROBLEM AND ITS DIMENSIONS

1.1 Introduction

The information revolution is here. Large sums of money are nowadays spent by governments and business for the sake of gathering, processing, tabulating, analyzing, and conveying huge bodies of data of various natures. Numerous articles have been written on the technical problems involved. Similarly, the use of certain data to solve specific questions is adequately documented, with due consideration being given to the proper search method to obtain this information.

Little attention has been given to the notion of 'information efficiency' from an economic (rather than technical) viewpoint. The questions 'how to make' and 'where to apply' may be dealt with separately. They are, however, closely connected because of the very fact that 'making' is done for the sake of 'applying'. In this paper, we will attempt to show some of the concepts involved in this connection. They are the tools required to attack the rather complicated problem of 'how much information of what quality is needed to solve a specific problem or a whole set of problems?'

"The preliminary analysis has revealed lack of literature on the subject", wrote Simari in 1967. He reported on discussions by an O.C.S.E. Work Group which was set up for the following purposes:

"a) to identify and establish within the systems of information elements and parameters which will permit the estimation of costs and profits (information efficiency). Such estimates are necessary for making valid decisions in the field of scientific and technical information and the organization thereof;

b) to develop uniform standards and rules, to allow significant confrontations and comparisons between the national information systems of different countries;

c) finally to evaluate costs and profits, as well as the efficiency of the various procedures in process, in consideration of their functional elements and of the usefulness achieved."

During the period elapsed since Simari reported on the absence of literature, not too many developments have taken place. Most publications are concerned with the technical aspects and the immediate applications of information. We will report on some interesting papers in the field of 'information efficiency' from both before and after 1967. The scope of this study does not allow us to go beyond some conceptual comments of an economic character.

1.2 The Growth of Information

Nobody will question the statement that information and knowledge are growing rapidly in our society. This holds not only for the stock accumulated over a number of years [1], but also for the annual growth itself. Machlup (1962) has calculated that in 1958, the measurable United States knowledge business amounted to nearly 30% of the U.S. Gross National Product, and Burck (1964) mentions a percentage of 43 for 1963. Although Machlup's definition of 'knowledge industry' may well be questioned as far as some components are concerned, the figure is staggering, anyway.

Apart from its productive and income-generating aspects, the 'knowledge industry' contributes heavily to our economic growth. Denison (1962) estimates that

> "between 1929 and 1957, the rising education of the labour force was responsible for 21% of the growth in Real National Income, and that the general 'advance in knowledge', which includes many factors but notably research and development work and better management, was responsible for another 19%." (Burck 1964)

Tyas (1970) mentions the following items as major growth components in rapidly expanding economies like Italy, Germany, and Japan:

1. entrepreneurship;

2. hard work and good organization;

3. technology and experience transfer;

4. closer links between academic and industrial research; and

5. risk capital.

He proceeds:

> "Although new knowledge is not the only ingredient of economic growth by any means, it is an essential ingredient. The transfer of technology, experience, information and data are very important links in the chain. These links today are in need of new methods and ideas to give them added strength, new technology, new equipment and young and agile minds to produce the systems and services required."

Later in his paper:

> "Knowledge is basic to all of this. It is our greatest resource -- the accumulated human experience in social sciences, economics, technology, and science. Results of basic research must be available:
>
> 1. to aid in teaching;
> 2. to further mission-oriented research;
> 3. to be interpreted into new technology; and
> 4. to aid our society economically, culturally, and socially." [2]

In 1969, the Organization for Economic Co-operation and Development (OECD) set up an Ad Hoc Group on Scientific and Technical Information. In its report (1971), the terms of reference are given as:

> "a) (to) explore the nature, magnitude and implications of the need for scientific and technical information and data in science, the economy, and society, and how these needs may be met through changes in the structures, technologies and policies and management concepts in relation to information;
>
> b) (to) present their conclusions and recommendations for the developing role of scientific and technical information and data systems and the policy and program resources required for them to fill this role effectively in Member countries."

We quote the summarizing 'margin comments':

"Information the key to man's future ... society must learn to use it effectively. Scientific and technical information (STI) is only part of the information we need, a means to an end. But what end? Society must decide. The ability of society to use technology wisely is at stake. Information policy is more than technology, it is needed also for policy. OECD can give important leadership. The information revolution has just begun. These are policy for STI, not propaganda! Strategies for policy ... the challenge for OECD."

"An STI policy is a part of science and other national policies. Such STI policies are now starting to evolve. Much coordination is needed. Ministers recommended a national STI policy (focus) with OECD as the international focus. Other international bodies also promote cooperation. User groups include scientists, engineers, policy makers, politicians, and the public. The scientist creates and uses information. But 'information' does not always add to 'knowledge'; evaluation review and analysis are needed, often in specialized 'analysis centres'. Information services are lagging in the life sciences. Industry needs information too, not only for R and D, but also for design, production, and marketing. Decision makers also need a variety of information which may be difficult to identify and impossible to find. 'Management information systems' already help, but need further development. STI for the 'man-in-the-street' ... to understand the consequences of change."

"STI systems have arisen largely unplanned, but policy guidelines are now needed. To improve the means, we must define the ends. There are four primary goals:

1. To help use science wisely.
2. To strengthen science and technology.
3. To enable wise choices by management and government.
4. A better understanding of change brings problems of 'priority'. Informed public debate on policy issues - an essential factor in democracy."

"What must government <u>do</u> here? It should ensure information availability, <u>accessibility</u> ... in a social context ... evaluated and analyzed, funds to study user needs and to satisfy them, effective management, trained information staff. A policy focus is needed in government and the network must be <u>national</u> in scope. Knowledge is global ... <u>the</u> nations must intercommunicate. Specialists must know subject matter, as well as systems. New skills must be encouraged, together with a sound career structure. Systems should respond actively to their users. R and D is needed on how information is used, on how to handle it more effectively, and on how to use R and D information better! Probably networks ... for economy, responsiveness and innovation. Balance between national <u>in</u>- and <u>inter</u>- dependence must be found in relation to special national needs."

There certainly is a need to indicate the conceptual framework of STI policies, given the lack (or lag) of implementation/application of such ideas in the formulation of the proper policies. Indeed, the then chairman of the Economic Council of Canada, Dr. Arthur Smith, told the Senate Special Committee on Science Policy:

"We do not really have any good studies that I know of in Canada dealing with the use of scientific and technical knowledge as it affects economic activities."

This study confines itself to numerical information. It seems that the preceding sobering thoughts may be applied to this field as well.

1.3 What is Information?

It may strike the reader that, until now, we have refrained from defining notions such as information, data, etc. Handovsky (1969) considers defining the concepts of information and documentation as dangerous; it may "lead to a Byzantine discussion about the sex of the angels." He then proceeds with the following definitions:

> "Documentation is ... stored knowledge with a dynamic input and a static output.
>
> Information (is) a flow of thoughts based on raw material that must be treated, upgraded, or, in one word, managed."

One of the foremost experts in the field of economic measurement, Oskar Morgenstern (1963), describes observations as deliberately designed and selected information, whereas other data are merely obtained unplanned. He proceeds to point out that terms like observation, data, statistics, evidence, etc. are to be described in a systematic relationship with each other. (From his book, "On the Accuracy of Economic Observations", it becomes clear, however, that definitions are not always indispensable in order to have an intelligent discussion of the issue involved.) For general interest, we reproduce in a footnote a relevant paragraph on this relationship. [3]

Likert (1960) talks about the necessity to distinguish between information as to the nature of the system described as opposed to the state of the system. The former refers to both the conceptualization and the quantification of the system, whereas the latter reveals the quantitative aspects of their system only. Likert suggests that, in a fast changing society like ours, more emphasis should be placed on the gathering of information regarding the nature of the system. The fact of the matter is that this society seems to spend more effort on information about the state of the system. Often, it seems, the industrious collection of state-data stands in the way of studying the nature in an integral manner. [4]

Even though the goal of information gathering may not always be very clear, it should not be forgotten that some goal does exist. Information implies a message to a decision maker and is, therefore, more than just a set of data. (Dickson 1968)

Gathering and tabulation of data must, therefore, be followed by analysis and conclusions for the sake of decisions (compare Stone and Beyer 1970).

1.4 The Quality of Information

Quite a few economists and statisticians have drawn attention to the problem of quality of social-economic data. Wallis (1966) suggests

> "that (the American) economic policies in the 1950's were misguided by statistics which were properly enough interpreted, had they been accurate, but were not sufficiently accurate; and that ... policies in the 1960's are being misguided by statistics that are accurate enough, but that are being misinterpreted."

Quite properly, Wallis indicates the importance, for the sake of policy application, of these errors of measurement or interpretation. The loss of economic welfare because of these errors may indeed be substantial.

U.S. Senator Proxmire transmitted a report on government price statistics with the following comments:

> "... there is widespread recognition that our price information is not adequate for the burden of public and private policy formulation that it must carry. Better price statistics are urgently called for, since they are necessary in making decisions affecting billions of dollars in output, but more importantly -- the well being of the American people ..." (see Stephen 1967).

And, at the 1966 meetings of the American Economic Association, Leijonhufvud stated that "Keynesian unemployment may be mostly due to lack of information" (Marschak 1968).

Morgenstern (1963) attempts to provide us with some consolation, recalling that, in nuclear physics, some measurements are (or were?) made with only a 50% accuracy. If economists can work with data that are subject to a 10% error margin, they may not be in all that bad a position, he suggests. With all respect for Morgenstern, we must say that such a comparison is not quite valid. The yardstick of observational quality in economics is related to the welfare (or lack of such feature) of real individuals, rather than of some material substance. If ever, fine-tuning is required here. [5]

Morgenstern distinguishes between three principal types of error in the statistics of National Income, to give just one example of economic data. Some might be "introduced in the basic data of production or expenditure for the separate industries and other economic activities". Others "result from the effort to fit the available statistics to the conceptual framework of the aggregate". "In some cases, the existing data are not collected in a form directly suitable for use in estimating gross national product or one of its component items."

Juster (1970) says in this context:

> "Since an information system is general purpose by definition, the data are unlikely to be optimal for any specific analytical use and will generally have to be modified or adjusted to fit requirements."

Morgenstern also mentions the case where data "become available from government agencies as a by-product of their administrative functions", which suggests "a very strong reason to suspect the quality of data obtained in this manner". Finally, wherever data are not available, another type of error is introduced when an attempt is made to fill the data gaps for industries and years where estimates are not known , by means of interpolation, extrapolation, use of imputed weights, inserted trends, blowing up methods, etc.

To give us some indication of the empirical importance of error margins, Morgenstern points to the outstanding work done by Kuznets (1941). The latter distinguishes three groups of industries; classification criterion is the percentage error margin probably inherent in the estimate of their output value. Well below 15 per cent appeared to be the basic manufacturing industry error judged to be present in their estimate. Well below 15 per cent appeared to be the basic manufacturing industry and public utilities; error margins of about 15 per cent, but well below 30 per cent, were found in agriculture, mining, manufactured gas, pipelines, trade, banking, insurance, and government; and, finally, some industries had error margin of 30 per cent or higher, such as construction, water transportation, real estate, and direct service industries. Making some more adjustments in these data, Kutznets suggests that an average margin of error for national income estimates of about 10 per cent would be reasonable.

The implication of this statement is that, depending on the particular nature of the error, the results of many economic analyses could become irrelevant, wrong, and misleading rather than aiding. The costs for society could then be impressive indeed.

There is another aspect which we should not neglect to mention. Data to be used for policy decisions should be available at an appropriate point of time. Most modern economies are characterized by extensive government influence. This may consist of voluntary or compulsory guidelines for the private sector and/or government-sponsored predictions of economic conditions; these forecasts will have some bearing on subsequent decisions to be made by business, households, and the various levels of government (see also Section 1.6).

It seems reasonable to expect that the necessary data will become available as quickly as possible. A former Prime Minister of Great Britain, Mr. McMillan, said in this context:

> "... too many of our tables are too late to be as useful as they might be. We are always, as it were, looking a train in last year's Bradshaw." (Fessey 1969).

The speed at which new data become available appears to be a major problem. [6] Moreover, already published data need to be updated, particularly in dynamic models where the variables exercise their influence over a number of periods. Repeated observation of historical figures plays an important role in environmental models.

There is, of course, a trade-off between speed and quality. An early estimate of some macro-economic yearly figure can be expected to be farther from the true value than a later measurement. Stekler (1970) concludes from his investigations in this area that

> "the accuracy of the statistics is inversely related to the speed with which the data are reported. If the errors are measured by the difference between the early and final figures, there is little trade-off between the reporting speed and the turning point errors that are indicated. The size of the error, however, was inversely related to the reporting promptness."

For the interested reader, we reproduce in a footnote some results in the field of data revision from Zellner (1958; 16 U.S. quarterly series, 1947-1955); Stekler (1967; 12 U.S. quarterly series, 1947-1964); Stekler (1970; 5 U.S. monthly series, 1957-1965); Glejser and Dramais (n.y., G.N.P. figures for 42 countries, 1956-1964); and Denton & Oksanen (1972; 6 series for 21 countries, 1955-1964; the 2 sets of data are used to estimate national econometric models -- see also Chapter 6 of this paper). Theil (1965) analyzes a data set consisting of two successive forecasts, two interim estimates, and three measurements afterwards of 20 variables in the Netherlands' economy, 1953-1962. [7]

1.5 Confidentiality and Aggregation

A very important aspect of data collection and publication is the necessity to treat certain materials as confidential. Indeed, many economists and other social scientists have been frustrated in their research attempts because of non-availability of gathered data for reasons of security and privacy. In the United States, the Ruggles (1966) and the Kaysen (1967) Committees have dealt with this problem. Rules exist in some countries to aid researchers having run into the difficulty of unavailability of gathered data. They may carry on with their project through governmental statistical agencies which will perform the desired calculations without revealing the underlying raw data. On the Canadian scene, some aggregation levels of the (product of 2 rectangular matrices, forming the square) Canadian input-output table can reveal information about market shares of certain corporations, which is in conflict with the protection given by the Statistics Act. [8]

In such a case, the research might be carried out with the help of the computer services of Statistics Canada. Another example we suggest is investigation of consumer behaviour with the help of micro data collected from individual families. [9] Generally, there is an economic advantage in having statistical analyses performed at the researcher's request by the statistical agency which has gathered and stored the relevant data; much unnecessary work may thus be avoided.

When micro data are not available to the investigator, he is forced to use various types of aggregations. The problem of admissability of aggregation of micro-economic relationships into macro-economic ones is dealt with extensively in the literature. For the moment, we should like to mention the empirical work done by Orcutt, et. al. (1968), who showed that the degree of aggregation influences the efficiency of conventional estimation techniques. The main conclusion of his paper is that "use of less aggregated data than the national accounts data presently relied on would make possible:

1. the virtual elimination of small sample biases,

2. enormous improvements in the precision of estimates of parameters in macro-economic models, and

3. greatly improved possibilities of detecting misspecifications and of correctly choosing between alternative formulations."

If aggregation of micro data leads indeed to inferior results in applications, then economists should attempt to avoid this procedure as much as possible. Note that large segments of macro-economic data do require measurement at the micro level. Additional cost would probably be minimal in such cases. Juster (1970), despite his comment about inappropriateness of some measurements in relation to the various applications to which these data might be put, suggests that far more coordination in the measurement of social-economic features is quite possible, resulting in important economies of scale:

"Using households to illustrate the possibilities, there are few important problems in economic research that do not require precise measurement of a common set of variables over an extended period of time. Investigation of the demand for consumption, the demand for money, the response to price changes, the returns to education, the allocation of time in nonmarket activities, the formation of expectations, the demand for household capital goods, and the determinants of marriage and birth rates -- all require collection of a large common set of variables plus a smaller collection of variables specifically oriented to each problem. Thus, a single sample could be used to examine a wide range of research problems by adding a moderate collection of additional information."

There is, however, the great danger of decreasing drastically the already battered remnants of individual privacy. The question is whether this price is not too high compared with the gain of better quantification of economic insight (see also the next section).

1.6 On Economic Forecasting

There are various reasons why governments indulge in economic forecasting. The simplest argument might be to inform the public of what may happen in the near or distant future. Only a well informed public is able to make intelligent decisions regarding their private lives and the community's needs.

A second reason is related to the sheer size of the government sector within the economy. Unless a government is completely irresponsible, it will take into account the impact of its own actions on the economy at large; this requires some forecast of what the private sector is expected to do within the policy concerned. Eckler (1970) refers to the fear some people have for this kind of government involvement:

> " ... there are some people, including legislators, who ... want less intervention by government ... and fear that statistical data may provide the basis for more social and economic legislation."

We have here an excellent example of the potential feedback of information into the decision process.

A third argument in favour of forecasting of economic data exists whenever the government plans its policies so as to achieve certain desirable goals; e.g., full employment, maximum economic growth, equilibrium in the balance of payments, etc. A prerequisite for an adequate estimate of magnitudes of policy measures is some 'foreknowledge' of what will happen under the various possibilities of government programs.

Tintner (1968) recalls the opinion that

> "predictions in economics are impossible by principle since these very predictions, if they are believed by the acting economic subjects, will falsify the original predictions".

He refutes this argument by quoting from Grunberg and Modigliani (1954):

> "It has been shown that, provided that correct private prediction is possible, correct public prediction is also conceptually possible. Two possibilities may be distinguished

1. The public prediction does not affect the course of events because the agents are indifferent to or incapable of reacting to the public prediction. In this case, correct public prediction coincides with correct private prediction.

2. Agents react to public prediction, and their reaction alters the course of events. The reaction can conceptually be known and taken into account. It has been shown that boundedness of the variables of the predictive system and the continuity over the relevant interval of the functions relating the variables to each other are sufficient, though not necessary conditions for the existence of correct public predictions. These conditions were found to be normally fulfilled in the world about which predictive statements are to be made. The argument of this paper establishes the falsity of the proposition that the agents' reaction to public prediction necessarily falsifies all such predictions and that, therefore, social scientists may never hope to predict both publicly and correctly. But it demonstrates no more than that correct public prediction is possible if the possibility of correct private prediction is accepted. About the possibility of private prediction, it has nothing to say. So, in the end, the major difficulties of prediction in the domain of social prediction turn out to be those of private prediction."

Government use of predictions to set its own policies may now have a dual effect:-- direct influence through the program itself, and an indirect one because of the information content of such policies. To give a simple example:-- If the government predicts the development of the national economy under 'normal' fiscal conditions, it may find an unacceptable level of unemployment. It will probably decide on a shift in economic policies in order to alleviate the problem; e.g., through a decrease in taxation and an increase in government expenditures. These measures will have a direct effect through the national-income multiplier. The prediction of a (minor) depression, will have an unfavourable information effect on the economy, as we saw in the previous paragraph. There will be a positive information effect of the very announcement of the government's counteracting policies. In extreme cases, the direct effect of the policy can be minimal; the message relayed may yet be very informative. An example is the 'bank rate' or 'discount rate' policy of the central bank.

Strictly spoken, there is no strict need for chartered banks to raise their lending rates with an increase in the bank rate. The desired result will take place simply because the banks and the public know that such action is what the government (or Bank of Canada) wants.

There is no reason to believe that the prediction method could not take into account the information effects of forecast and associated policies, similar to the incorporation of the impact of the substance of the policy measure (like the multiplier effect).

Finally, a fourth argument for economic forecasting is the need for very detailed information in planned economies. We should distinguish between the cases of compulsory and indicative planning. In the former case, the (major) producers are prescribed what and how much to make and to deliver to other companies and stores. It is obvious that this process requires large numbers of calculations, predictions, and feasability checks. Input-output tables with millions of commodities would be necessary, requiring vast amounts of information and almost insurmountable computing problems. The cost of these activities is enormous; witness the fact that no country in the world actually runs its economy this way. Output planning in Communist countries is done in much detail for major industries only. Guidelines become weaker in substance and sophistication for less and less important producers. It would be interesting to perform an alalysis of information efficiency within this context.

Indicative planning takes place whenever the government publishes a medium or long-term forecast with the explicit goal that it serve as information tool for decisions made within the private sector. This type of planning-through-prediction has become increasingly important in most Western countries during the past few decades, with varying emphasis on either element. As Leser (1969) said in his Inaugural Lecture at Leeds:

> "The distinction between a plan and a forecast is not, however, as strict as might be imagined. A national plan may stipulate an over-all growth rate and estimate the expansion rates in various industrial sectors which are consistent with the rate of progress in the economy as a whole. If the general picture, together with the demand conditions which result from it, is accepted as reasonable by the leading industrialists, then they may base their decision on the plan, thus helping actively with its realization. It is said that the French

National Plan, in this manner, acted as a forecast and tended to bring about and maintain a stable growth rate for a substantial length of time. Of course, if the plan is not taken seriously, then, conversely, there is little chance of its realization."

As we saw with Tintner before, the feedback might be incorporated into the forecasting/planning method.

This is not the proper place to discuss the quality of economic predictions actually made over the past several decades. The reader may be referred to Theil (1961, 1966, 1967) for methodological considerations in the analysis of forecast accuracy, and of numerous applications. The findings of the Central Planning Bureau of the Netherlands (established shortly after the Second World War and one of the first forecasting agencies using sophisticated econometric models) for the period 1953-1963 seem to be quite general (see C.P.B. 1965 and also Verdoorn 1967):

- changes are generally underestimated;
- this property is largely caused by underestimation of changes in exogenous variables;
- the signs of changes are generally predicted properly;
- the predictive quality is different for the various variables involved;
- the predictive quality of the final version of the Central Economic Plans is better than that of earlier versions;
- the revisions made in successive versions of these Plans are generally small.

We will close this section with a methodological observation. Forecasts serve a purpose and their quality should be judged in the light of applications. And, even then, conclusions are not easy to reach. As the OECD report (1965) states:

"There is no doubt that it is difficult to forecast accurately. In all the countries, one could point to forecasts which proved to be badly wrong. A simple comparison of forecast with outcome is by no means a fully satisfactory test of a forecast's adequacy. But, even on the more pragmatic test of 'did the forecast help the policy maker?', it would be easy to find

> examples of forecasts which have positively misled policy. However, the important question is not whether forecasts are always right, or even whether they are right more often than they are wrong, but whether there is a better alternative to framing economic policy on the basis of quantitative national income forecasts."

There will always be a need to improve predictions because perfection cannot be reached. Such improvements will take place in the fields of data collection and revision, estimation of the present structure of the economy, and prediction of (changes in) parameters and exogenous variables. We should repeat, however, that there is no rule of thumb to decide where our efforts should be focussed first and most strongly. Such a decision requires an evaluation of the 'welfare gains' of better forecasts and subsequent policy actions.

1.7 Information Efficiency

Information is a means rather than an end in itself. Whether by a consumer or by a producer, knowledge is being used to achieve a certain goal. The justification for data gathering activities must be derived from the purpose.

The 'purest' use exists within the consumers' market when knowledge and understanding are acquired for the sake of simple pleasure; information is then a consumption good like any other commodity or service. Its demand will be determined by means of maximization of the consumer's preference function, subject to certain constraints.

In most cases, however, the satisfaction or usefulness derived from information is generated via a round-about procedure. Gathering data enables the decision-maker to acquire increased insight into the phenomenon under study, its potential and limitations. Information, therefore, takes on the role of an input into a decision-making process. Its efficiency must be measured by its contribution to the evaluated outcome (financial, moral, social) of the choice process under investigation. As Morgenstern (1963) writes:

> "The usefulness of economic data, therefore, cannot be gauged without relating them to the uses (theorems) to which they are to be fitted."

Pathbreaking work in this field was done by Stigler (1961) who investigates the role of information in the market behaviour of the individual. "Price dispersion is a manifestation - and, indeed, is the measure - of ignorance in the market" he writes, and proceeds to give some insight as to how the consumer and producer might attempt to reduce this ignorance. A buyer or seller will search for a lower offer or higher bid, respectively, up to the point where the expected gain of more search will fall below the additional cost (including the potential loss because of the disappearance of previous quotations). The frequency distribution of prices may take different forms and Stigler derives optimal search rules for each case. Some conclusions are:

"1. The larger the fraction of the buyer's expenditures on the commodity, the greater the savings from search, and hence, the greater the amount of search.

2. The larger the fraction of repetitive (experienced) buyers in the market, the greater the effective amount of search (with positive correlation of successive prices).

3. The larger the fraction of repetitive sellers, the higher the correlation between successive prices, and hence, by condition (2), the larger the amount of accumulated search.

4. The cost of search will be larger, the larger the geographical size of the market."

The above mentioned ideas expressed by Stigler should provide us with some framework in which the problem of information acquisition can be developed. There is no reason to restrict this approach to the market for commodities and services. [10]

Like all inputs into the production or consumption process, information has the aspects of acquisition, processing, and use/sale. The former two items are concerned mainly with matters of costs and techniques. The latter (dual) item is related to the desire/need for information. Evaluation of all these features is necessary in order to come to a meaningful decision rule (like the ones derived by Stigler -- see above). We suggest that the economic aspects of cost of and demand for information have been grossly neglected, whereas the technical aspects have been given prime treatment in most of the literature. An excuse for this default may not only be found in peculiar measurement problems but also in the special nature of information as a commodity. Unlike other inputs into productive and consumptive activities, data are not being destroyed, partly or wholly, during their use.

We should like to make two observations with respect to the cost side. Information systems are heavily capital-intensive. Once this basic investment has been made, the system can be used in various ways to carry out different types of operations in a wide field of analysis and reporting. It appears that the capital costs are well estimable. The variable costs related to specific projects may vary from near zero to rather large sums. We have not discovered generally applicable methods to develop a cost measurement base for specific input requirements. But ad-hoc procedures may well be more appropriate in this instance. The delivery side of the production system can operate fairly cheaply. This implies that marginal cost pricing may not cover total costs (Holt 1970); this suggests that public ownership may well be desirable. Such a system would also overcome the problem re property rights which are difficult to protect when it comes to information (which is easily revealed by its very use; see Arrow 1962, Demsetz 1969).

A second comment has to do with the type of costs, rather than the cost measurement itself. As Juster (1970) writes:

> "In a similar vein, sponsors of micro data collections are seldom prepared to compensate behavioural units for the time and annoyance involved in the observation. In the behavioural sciences, after all, it is not possible to measure relevant behaviour without the cooperation and assistance of the unit being observed. But the measurement process inevitably involves at least the cost of time and, for some units, annoyance as well. A straightforward agreement with sample respondents to trade accuracy and consistency in the set of variables being measured against compensation for the time and inconvenience involved, might well be a more efficient procedure than the current practice of appealing to the social conscience of respondents to obtain observations whose accuracy is undermined by an unwillingness to examine internal consistency because of its possible effect on the refusal rate."

Similar problems are well known to executives and people in leading positions in fairly large organizations, who have to decide on the optimal level of administration and feedback via questionnaires, timetables, etc.

The demand side presents many problems, too. In his outstanding article, "Statisticians and Shoemakers" ("who is worse shod than the shoemaker's wife?"), Eckler (1970) says:

> "There are a number of reasons for failure thus far to quantify the value of uses or the extent of need for statistical data. First of all, the identification and listing of users are not easy tasks. The difficulty of tracing end use is accentuated by the fact that many elements of economic and social statistics are taken into the public domain through re-publication, so that it is difficult to trace all the uses that are made of a particular body of statistical information. Even when the use can be traced and clearly identified, it is difficult to determine just how much bearing a particular item of information had upon an individual decision. A further complication arises because of the possibility that one figure may serve as a proxy for another so that the importance of either one is difficult to assess."

In other words, not only may we be unable to measure marginal benefit of an additionally made observation, alternative uses may well imply that these benefits are different if considered over a range of possible applications, possibly spread out over a large number of years. Indeed, once-accumulated information can be stored at very low cost and made available for re-use without limits over an indefinite period. File-merging is but one reflection of this possibility, apart from its usefulness as a cross-checking device. This continuous availability of data with many potential applications unknown in number, use, and time-distance, makes marginal cost pricing impossible. Again, public ownership seems a logical implication.

Rees (1963) points to another problem:

> "... it is evident, in many surveys of the information needs ..., that a confusion exists between wants of users and their real needs. An expressed want is often a very imperfect characterization of what is really needed -- if indeed this is known. The needs of users have been conditioned by what is available in terms of present systems and equipment and represent a compromise between what they think they would like and that which can be obtained." (As quoted by Voos 1969.)

Orcutt (1970) mentions in this context:

> "If, as professors, economists finally secured some experience in posing hypotheses or questions, and also in specifying and actually securing data needed in testing the hypotheses and in resolving the questions, matters would not be so serious. As things stand, however, even at the professional level, few individuals ever play a significant role in figuring out what new data are needed for effective testing of hypotheses, how the needed measurements may be made, and in actually making them."

One such example of specific suggestions made by economists with respect to the collection of economic data is made by Tsurumi (1971), who writes:

> "However, from the point of view of a user of statistical data who is interested in building a disaggregrated macro or industry model, the following types of data would be quite useful if they were published along with national income accounts.
>
> '1. Domestic Product (or National Income) data by industry (quarterly data in value terms, not in indexes).
>
> 2. Business gross fixed capital formation estimates by industry (quarterly data).
>
> 3. More detailed industrial classification of the Labour Force survey data than currently available.
>
> 4. Merchandise exports and imports classified by industry of origin in the reconciliation statement of exports and imports of goods and services.'
>
> If these data are readily available in such publications as the national income accounts or the Canadian Statistical Review as an integral part of reporting national income accounts, econometric model builders may be greatly relieved in their effort to 'dig out' and 'construct' necessary data."

Note that Tsurumi deals with two problems; i.e., the type of data to be collected and their publication format.

Once the purpose of a data gathering process has been defined, we are still left with the task of evaluating the contribution of the information body to the optimal conclusion or decision made. This cannot be done unless some quantitative measurement of the end-goal itself is available. For example, Stigler (1961) compares the cost of price search with the benefit for the buyer who manages to decrease the price for a desired object by means of the search procedure.

In a macro-economic context, such evaluation is more difficult. One might suggest (see earlier in this chapter) that Keynesian unemployment is mainly due to the lack of information, and subsequently recommend that measures be taken to reduce this uncertainty. Even if we are able to design a workable system and estimate its costs, no proper decision as to its implementation can be made if no measurement can be obtained of the social value of reduced unemployment. This is why we suggest that the evaluation of (increased and improved) information must take place within a quantitative model of economic policy formulation. [11]

This requirement is drastic, indeed, but not impossible. In fact, we will show later that analytical techniques for policy purposes are available. The need for information evaluation can only help to speed up the development and further application of such models.

For the sake of optimal policy (and intellectual honesty), any type of model should be accompanied by or incorporate the following:

1. the investigator's or policy maker's own preferences.

2. an optimal set of available policy options, whereby the optimality is influenced by objective circumstances.

3. a model describing the relationship between 1. and 2., resulting in optimal decisions subject to the characteristics embodied in 1. and 2.

It is not always easy to distinguish between objectives and constraints. Let us illustrate this with the following quotation from Dorfman (1967):

"... a practicable distinction between constraints and objectives might go as follows:

A requirement is a constraint if

(a) it must not be violated at any cost, however high, or with any probability, however low, and

(b) there is no gain or advantage in overfulfilling it.

On the other hand, a requirement is one of the objectives of the firm if it can be violated, though at a cost or penalty or if there is an advantage in overfilling it. Mixed cases can occur. Thus, if deliveries must be made within two weeks at all cost, and if there is an advantage in quicker deliveries, the maximum delivery time is a constraint while, say, the average delivery time enters into the objective function. If this distinction be accepted, it will be seen that only technological requirements will qualify as constraints (e.g., the output of a refinery cannot exceed the volume of crude oil inputs); the attainment of all other requirements is part of the objective. In other words, every practicable design of an operation is subject to failure in sufficiently adverse circumstances; one designs so as to balance the cost of failure against the cost of decreasing that risk. Thus, the use of policy constraints, though prevalent and perhaps inevitable, must entail some loss in attainment of the 'real' ... purpose of the enterprise."

The explicit formulation of goals and limitations has the advantage that the discussion of subsequent policy actions can become more objective than if these features remained unknown. On the other hand, one should see to it that the policy maker does not control the analytical process and information input as well. This remains a touchy issue in many decision processes. Apart from its influence on the economy proper, "President Johnson's unwillingness to disclose the mounting cost of the Vietnam war" (Silk 1972) may well have contributed to the support for his military policies among members of Congress. As C. West Churchman writes, suggesting the absence of accurate or objective information:

"Instead, so-called 'information' is simply one kind of incentive, which can be used by one person or group to influence the behaviour of another person or group. It is, in fact, a commodity with its own price, a commodity that serves the purpose of shaping social action." (C. West Churchman 1968, as cited by Hoos 1971.)

This manoevering with information is a well-known feature in large business or government organizations. It plays an important role in the difficulties of central planning of a national economy. Kornai (1967) pays considerable attention to the problem of data distortion in the Hungarian economy. For example, after having estimated that "the 22.2 per cent mean of the overspending reflects the tendentious distortions, biases in the estimates", he concludes:

> "... it may be stated that there is a marked tendency to 'under-estimate' costs, especially construction costs and those coming under the 'other' heading, as well as the period of realization. It is the 'chauvinism' of the investing firms or sectors that comes here into play. The investing party -- in order to secure the granting of a permit -- will endeavour to make investment costs appear lower in the preliminary calculations than the actual requirements. It will be assumed that the permit once granted, overspending and the protraction of the realization period would not matter too much; the investment project once launched would hardly be stopped later on; and the amount of overspending would ultimately be paid out. Nor does the system of material incentives connected with the investments counteract these tendencies, as the fact of overspending does not involve any sanctions."

We stumble here on an inalienable aspect of any planning process:

> Although an overall policy goal is being specified and -- possibly -- accepted at large, the interests of the various executors will contaminate the decision's implementation.

We shall return to this problem at a later point.

Our conclusion from the above discussion is that the evaluation of information should take place within the context of a model describing the decision maker's objectives and the constraints in a pure and undistorted form. It will appear later that a general class of optimization problems in the fields of macro-economic policy and resource management may be solved with similar analytical models.

Information can now be embodied into various components of the decision-making procedure. The possibilities and costs of information gathering are incorporated into the selection process of the 'optimal' model, the estimation of the model is carried out with help of such data, and the solution is, finally, tested for its sensitivity for changes in the input structure (both quantitatively and qualitatively).

The solution of any decision-making problem is subject to the design and information used in the formulation of the model. Changes and errors in these features may result in alterations in the optimal decision. As Meier, Weiss, et al write:

> "In fact, it is often more important for a planner to know how the system will perform under varied operational conditions than it is for him to know the 'optimum plan' which may exist under a single set of postulated economic and physical conditions. Moreover, it is highly desirable for him to be able to quantify the effects that changes in the more important variables have on the performance of various 'good plans'."

In effect, sensitivity analysis is a method of seeing how much the various parameters that have been estimated are able to change before the optimal plan becomes sub-optimal. It also enables us to investigate the trade-offs which may occur for variations in some set of parameters. A well-known example of explicit sensitivity information is the Lagrange multiplier in optimization and programming, measuring the marginal impact of the corresponding constraint.

By studying the sensitivity of the answers our model has given us to variations in the data input estimated, we are able to derive the relative importance of the various input streams, and decide (explicitly) on where and how to spend additional funds for information-gathering purposes.

Moreover, incorporation of these qualitative aspects of available information is important in the expost evaluation of decisions 'prescribed' by the model. A good example of how such insight might be used (but wasn't) is described by Bakker (1962). In 1960, the Dutch cabinet resigned because of a Lower Chamber motion asking for more subsidized-housing allocations for 1961 than the government was willing to grant. Although nobody faults politicians for standing up for their principles, the reasonableness of the crisis becomes less clear if one realizes that the total discrepancy involved in the conflict amounted to 1/4 or 1/3 of the average prediction error in the housing program over the preceding years. Had the Dutch politicians been aware of this statistical feature, they might well have decided differently regarding the cabinet breakdown.

We conclude this chapter with the observation that the problem of information efficiency is a relative one. We suspect that, for the time being, the choice is not a question of 'more data: yes or no?', but rather of 'more data: where and with what level of expenditures?', as indicated by the Dominion Bureau of Statistics' statement quoted in Chapter 0. This implies that the decision may well be based on a cost-benefit approach in which the outcomes of various sensitivity analyses of additional observations are being compared and ranked by the policy maker according to his judgement.

CHAPTER 2

SOME THEORETICAL-STATISTICAL CONCEPTS

2.1 Estimation and Testing

Ignorance about social and economic phenomena takes often the form of uncertainty as regards numerical relations. The need for quantitative insight makes the application of statistical principles of estimation and testing necessary.

Estimation is concerned with measurement under uncertainty, whereby two cases may be distinguished:

1. The characteristic to be determined does exist uniquely; our instruments, however, are imprecise and introduce an element of variation of outcomes around the true value.

   As Pritchard (1964) writes:

   > "The advent of the word 'true', together with the word 'real', in scientific discussion has done much to cloud the nature of scientific activity. The request for a 'true' story instead of a 'likely' story tacitly postulates the existence of a 'real' world underlying and giving rise to appearances and asks for information about that 'real' world. The scientist qua scientist cannot answer such a question since the material on which he operates consists entirely of appearances."

   And O. Anderson (1953) writes:

   > "Statistical numbers are really no more than 'statistical shadow images' of the mass phenomenon they refer to." (my translation: J.I.V.)

   The data with which we work is statistical and measured by the devices of man. We are not working with facts in the sense of known values.

   The degree of uncertainty is obviously related to the quality of the measurement instrument. This is to a large extent a technical problem which should be solved within an economic context, considering the cost of more refined observations and the associated benefits. Thus, three elements are involved:

- the technical measurement systems and the introduction of uncertainty connected therewith;
- the costs associated with various measurement systems; and
- the benefits accruing from improved observations.

2. The characteristic to be determined is (possibly) unique, but appears merely as an average feature of a group of nonhomogeneous items (the 'elements' of a 'population'). Uncertainty surrounding the outcome of the measurement process is caused by the intrinsic variability of the underlying material. This is a datum we cannot affect in many instances (particularly in economics), as the population from which a sample can be drawn may well be beyond the investigator's control. In addition to the variability within the population, a measurement problem may exist. For example, if we attempt to measure the Marginal Propensity to Consume (the increase in national consumption divided by the associated increase in Gross National Income), we have intrinsic variability, as the M.P.C. is different for the respective elements (families) of the population. Moreover, the technical features of the measurement system are imperfect and introduce an additional amount of uncertainty in the sample outcomes.

The classical solution to both problems works with the assumption that measurement (now to be called 'estimation' because of the uncertainty feature) takes place with help of a sampling procedure from a distribution characterized by the true value and the variation of both origins as described above.

It appears that ordinarily the uncertainty can be reduced by increasing the sample size. A simple example may illustrate this.

We sample from a distribution with mean M:

$$E[X] = M$$

and variance V^2:

$$E[(X - M)^2] = V^2$$

where 'E' stands for the Expectation operator (= taking the average value of all possible outcomes weighted with their respective probabilities; compare this with an ordinary weighted arithmetic average).

We calculate the average outcome $\overline{X}_N$ of N sample observations:

$$\overline{X}_N = (X_1 + X_2 + \ldots + X_N) : N = \frac{1}{N} \sum_{i=1}^{N} X_i$$

and observe that its expectation equals the true population mean, M:

$$E(\overline{X}_N) = E\left[\frac{1}{N} \sum_{i=1}^{N} X_i\right] = \frac{1}{N} \sum_{i=1}^{N} E\left[X_i\right] = \frac{1}{N} \sum_{i=1}^{N} M = \frac{1}{N} N.M = M$$

The sample mean $\overline{X}_N$ is thus called an 'unbiased estimator' of the true, but unknown population mean M. This does not imply that $\overline{X}_N$ equals M in each and every sample, but only when averaged out over all possible sample outcomes. In order to find out how close we can expect $\overline{X}_N$ to lie near M, we calculate its average quadratic deviation from M (the 'sample-mean variance'):

$$E\left[(\bar{X}_N - M)^2\right] = E\left[(\frac{1}{N}\sum_{i=1}^{N} X_i - M)^2\right] =$$

$$= \frac{1}{N^2} E\left[(\sum_{i=1}^{N} X_i - NM)^2\right] = \frac{1}{N^2} E\left[\left\{(X_i - M) + (X_2 - M) + \dots (X_N - M)\right\}^2\right]$$

$$= \frac{1}{N^2} E\left[(X_1 - M)^2\right] + \frac{1}{N^2} E\left[(X_1 - M)(X_2 - M)\right] + \dots + \frac{1}{N^2} E\left[(X_1 - M)(X_N - M)\right]$$

$$+ \frac{1}{N^2} E\left[(X_2 - M)^2\right] + \frac{1}{N^2} E\left[(X_2 - M)(X_1 - M)\right] + \dots + \frac{1}{N^2} E\left[(X_2 - M)(X_N - M)\right]$$

$$\vdots$$

$$+ \frac{1}{N^2} E\left[(X_N - M)^2\right] + \frac{1}{N^2} E\left[(X_N - M)(X_1 - M)\right] + \frac{1}{N^2} E\left[(X_N - M)(X_2 - M)\right] + \dots$$

$$= N.\frac{1}{N^2} V^2 = \frac{1}{N} V^2$$

because

1) all observations $X_1, X_2, \dots, X_n$ are drawn from the same population and have thus the same mean and variance; and

2) all observations were made independently from each other ('random sampling').

The latter feature implies that knowledge of the outcome of, say, X_1 does not tell us anything about X_2 (in addition to what we knew already). Therefore, knowing that X_1 deviates 'so much' $(X_1 - M)$ from its expectation M, is irrelevant for the determination of X_2's deviation from its expectation (in this case the same as X_1's, i.e., M). Formally, we write this as

$$E \left[(X_1 - M) \quad (X_2 - M) \right] = 0$$

As we picked X_1 and X_2 arbitrarily, and all X's are really interchangeable, we conclude that all expectations of similar terms (involving two different observations) are zero, too.

We have thus proven that the sample mean $\overline{X}_N$ has expectation M (the true mean) and variance V^2/N. Regardless of the sample size, $\overline{X}_N$ is right on the target on the average, and the degree of precision (measured by the variance of the sample mean) improves proportionally with the sample size. [12]

There is no strict reason why we should use the sample mean as an estimator of the population mean, however logical such method may sound. We could, for example, use the sample median (the observation which lies in the middle, of the series of sample outcomes ranked in ascending or descending order) which can be proven to be unbiased with respect to M, too. We thus need a criterion for the selection of an estimator. The argument used is 'efficiency' in terms of the smallest variance around M. [13]

The sample median has a variance, for large n, equalling $\frac{1}{2}\pi \ V^2/N$ which is $\frac{1}{2}\pi$ or approximately 1.5 as large as the variance of the sample mean. We thus make better use of our sample information when we use the mean as an estimator rather than the median, in the sense that the average squared distance between estimate and target (the true value) is smaller. Conversely, the sample mean is more efficient because it requires only 2/3 the sample size that the median needs in order to obtain the same reliability.

It can be shown that the sample mean is the most efficient estimator of the population mean available. For a given number of observations, no other formula will approximate the true value better (with average squared estimation error as the criterion). Such a clear situation does not always exist; estimation problems might be conceived where for a range of low values of N, a certain estimator is best, while for a range of higher values, another one will be more precise.

While the sample mean is unbiased, attempts could be made to improve its reliability by reducing its variance. As we cannot be sure that $\bar{X}_n$ really equals M, we want to be quite confident that $\bar{X}_N$ lies fairly closely to M. In other words, given $\bar{X}_N$, we write

$$M = \bar{X}_n \pm \text{error allowance}$$

and hope that we may be confident that this 'confidence interval' does contain the true value M indeed. As the 'error allowance' is related to the variance (of the sample mean), the interval can be shortened by increasing the sample size.

In case of the 'normal distribution' (the bell-curve which is characterized by just two numbers, i.e., the mean M and the variance V^2), we know that 95% of all observations can be expected to lie within an interval of $M \pm 1.96V$. For the sample mean $\bar{X}_N$ we thus have

$$\text{Probability} \left[M - 1.96 \frac{V}{\sqrt{N}} < \bar{X}_N < M + 1.96 \frac{V}{\sqrt{N}} \right] = 95\%$$

or

$$\text{Probability} \left[\bar{X}_N - 1.96 \frac{V}{\sqrt{N}} < M < \bar{X}_N + 1.96 \frac{V}{\sqrt{N}} \right] = 95\%$$

We should make two observations here. By definition, we will never know whether $\bar{X}_N$ is really close (or even equal) to the true value M. All that can be said is that, with a larger sample, $\bar{X}_N$ is more likely to lie close to M than with a smaller one. Moreover, as the reliability of the estimate depends on the sample size, and sampling involves some cost factor, we must have a mechanism by which we can evaluate whether increased accuracy offsets the extra cost of a larger sample. We then require a monetary evaluation of the benefit of higher reliability. This is usually done by means of a 'loss function' specifying the 'fines' associated with the various theoretical estimation errors. We will come back to this approach in S. 2.5.

Estimation results in one selection of a 'likely' value of the true but unknown parameter. In reality, however, this value must be compared with previously made measurements. In other words, statistical analysis is not just a matter of picking a good approximation but involves also a comparison of various choice alternatives available.

This brings us to the problem of testing of statistical hypotheses. We have, for example, two possible values for M, based on two different estimation processes (sampling, prior knowledge, etc.) and we try to make the 'best' selection. Several questions arise:

1. We always run the risk of accepting the wrong value. Even if one estimate lies closely to the true value (the unknown parameter), a more remote second (for some reason more reliable looking) value may well pop out of the sample because of the variability of the sampling procedure. Whatever choice we make, we can never be sure to be right. We thus have to balance the risks of the wrong decisions.

2. The choice should not just be made in the light of likelihoods of erroneous decisions, but also should incorporate the 'fine' associated with the acceptance of a wrong alternative. Just like a large sample is more probable to pin down the true value closely, the choice between two alternatives becomes more accurate when we increase the sample size. We thus have, again, a trade-off between costs and benefits of the decision procedure.

3. It could be (and is in fact very likely) that the investigator has good reasons to consider one hypothesis more likely than the other. He should then be enabled to incorporate these beliefs into the choice procedure.

The first problem is usually solved within the context of classical Neyman-Pearson statistics. To incorporate the other two, it is desirable to use a different branch; i.e., Bayesian analysis (see S. 2.5).

2.2 Estimation of Economic Relationships

We will now present some comments on the estimation of economic relationships. We thus are interested in finding the value of a parameter describing the connection between two (or more) variables. Suppose, the following relationship is postulated:

$$Y_i = A + BX_i + U_i \qquad (i=1,\ldots,N)$$

where Y is the dependent variable and X the independent or explanatory variable, A and B are the parameters describing the quantitative relationship between Y and X. U is a disturbance term (with theoretical mean 0) measuring the effect(s) of

(1) intrinsic variability of Y around A + BX;

(2) explanatory variables which were (improperly) deleted; or

(3) measurement errors in Y resulting in a proxi value used instead of the true Y (for which the relationship holds perfectly).

The rank of the observation is indicated by i.

It is now customary to estimate A and B with the least-squares formulae:

$$\hat{B} = \frac{\sum_{i=1}^{N} (X_i - \bar{X}) \quad (Y_i - \bar{Y})}{\sum_{i=1}^{N} (X_i - \bar{X})^2}$$

$$\hat{A} = \bar{Y} - \hat{B}\,\bar{X}$$

These estimators have the properties of linearity (in the dependent variable) and unbiasedness ($E[\hat{A}] = A$; $E[\hat{B}] = B$). It can be shown that within this class, the least-squares estimators are 'most efficient': no other unbiased estimators approach their respective target values more accurately. Note that this holds true for any given number of observations (the sample size).

Just as in the case of the sample mean, the precision of the least-squares estimation procedure improves with the number of observations. The variances of A and B are, respectively,

$$\text{var}(A) = \frac{V^2}{N} \qquad\qquad \text{var}(B) = \frac{V^2}{\sum_{i=1}^{N} (X_i - \bar{X})^2}$$

where V^2 is the variance of the disturbance term U.

The property holds clearly for var(A) as the numerator is constant and the denominator increases with the sample size. Using the fact that for any set of numbers, the arithmetic mean minimizes the sum of squares around itself better than any other value, we can write:

$$\sum_{i=1}^{N}(X_i - \bar{X}_N)^2 \leqslant \sum_{i=1}^{N}(X_i - \bar{X}_{N+1})^2 \leqslant \sum_{i=1}^{N}(X_i - \bar{X}_{N+1})^2 + (X_{N+1} - \bar{X}_{N+1})^2 = \sum_{i=1}^{N+1}(X_i - \bar{X}_{N+1})^2$$

where $\bar{X}_N$ and $\bar{X}_{N+1}$ stand for the mean of the first N, and of the first N+1 observations of the explanatory variable. Note that the inequality signs become equalities if and only if $X_{N+1} = \bar{X}_N$ implying $\bar{X}_{N+1} = \bar{X}_N$. We conclude that adding an observation is almost always useful as far as improved average precision is concerned. Note that this does not mean that $\hat{B}_{N+1}$ lies closer to B than $\hat{B}_N$ (the value for $\hat{B}$ based on N observations). Some outlying value of Y_{N+1} may well deteriorate the estimation quality, implying, in fact, that U_{N+1} is abnormally large or small. The formula Var $(\hat{B})$ has already averaged out all possible values of U, though.

The decreasing variances of the least-squares estimators (with increasing sample sizes) leads to a better concentration of possible $\hat{A}$ and $\hat{B}$ values around the true A and B, respectively. In the limiting case with the number of observations going to infinity, the distribution of the estimator is completely localized on the true parameter and the estimate will be accurate with probability approaching 100%. This feature is called consistency. Note that the speed of convergence may well differ for different estimators (compare the sample mean and the sample median, used as estimators of the population mean).

2.3 Some Extensions

We now turn to some assorted comments on extension of the preceeding analysis. (See for more details any standard textbook in econometrics; e.g., Theil 1971.) This section may be omitted without loss of continuity.

1. The assumption that var (U) is constant may be dropped. For example, when estimating consumption patterns with income as an explanatory variable, the variance of the disturbance term is expected to be higher for rich people than for poor, because of the sheer existence of a greater freedom of choice (see, e.g., Prais and Houthakker 1955). As ordinary Least Squares is not most efficient any more, application of Generalized Least Squared becomes necessary, by which the model is transformed so as to restore the constant-variance feature.

2. If the model contains more than one explanatory variable, least-squares can still be applied, although the calculations become more complicated. The method is then equivalent to the minimization of the sum of average (expected value of) squared estimation errors of the various parameters (constant term, coefficients). It is quite conceivable, however, that we want a certain parameter estimated 'better' than the other ones. In this case, too, application of Generalizes Least Squares gives the desired result.

3. Suppose prior knowledge exists with respect to some of the coefficients of the model. (e.g., the value of one parameter, or the sum of some of them). If this information is assumed to be perfectly true, Least-Squares Under Constraints can be applied. Less-than-certain prior knowledge should be specified stochastically (mean, disturbance term), after which Mixed Estimation follows, incorporating the empirical evidence and the prior knowledge. Note that this comes quite close to the Bayesian approach, referred to earlier and to be discussed further in Section 2.5.

4. If the explanatory variable X is subject to a probability distribution as well, the model is called a regression. Its interpretation is quite different from the model described above. Moreover, it leads in economics to many conceptual problems and should thus be used with care.

5. Whenever a large set of relations is being investigated (e.g., an equation system as used in econometric models -- see Chapter 6), the occurrence of stochastic variables as explanatory items can hardly be avoided. This may call for different estimation procedures. Similarly to the previous points (1-4), this problem is not (or only in a very complicated manner via optimal selection of estimation techniques) related to ours, and it will not be pursued here. [14]

6. Although we consider the X-variable(s) nonstochastic, there is no reason to believe we have observed the true value (see also our discussion of measurement in economics and the last footnote). We could now have to decide how to allocate funds for programs to improve the measurement of the various independent variables. In principle, sensitivity analysis could be applied in such a case. By differentiating the objective function with respect to the observations on explanatory variable(s), after the optimal expressions for the estimators (e.g., the least-squares formulas $\hat{A}$ and $\hat{B}$) have been plugged in, we obtain the potential gains of measurements improvement in terms of higher values for the objective function. Note that the postulation of an objective function (or 'welfare function') is indispensible for this purpose. [15]

7. The selection of the functional form and independent variables therein for the explanation of Y usually takes the form of seeking the specification with the highest correlation coefficient. However mechanistic this approach, there is a theoretical foundation for it. Theil (1958) proves that

> "if the specification is incorrect ..., the sum of squares of the residuals (corrected for loss of degrees of freedom) is on the average larger than the sum of squares of the residuals implied by the use of the correct specification; in other words, the residual variance shows then an upward (at least not downward) specification bias. On the average, therefore, the criterion of minimum residual variance leads to the correct choice of the specification; and the same is of course true for the equivalent criterion of maximum multiple correlation (coefficient) ..." [16]

Koerts and Abrahamse (1969) warn against the use of the correlation coefficient as a criterion for discrimination between different models, unless R^2 is interpreted in the light of its own distribution which depends on (and is very sensitive to changes in) the values of the explanatory variable(s). Theil (1971) points to the problem of interpreting residual variances and correlation coefficients when the dependent variable is taken in different mathematical forms (e.g., the integral value versus its logarithm).

2.4 Some Comments on Aggregation

A very important issue is the degree of aggregation of data used in a model. We saw already that Orcutt et. al. recommended strongly that micro data be used. They came to this conclusion following the construction of a microanalytic model of the U.S. economy, the use of these models to produce large numbers of histories, and estimation of the underlying structure of the generating models at three different levels of aggregation.

In a classical article in the field of aggregation, Grunfeld and Griliches (1960) came to the following conclusions:

"1. It is quite likely that a macro equation will have a higher R^2 than a micro equation, but this is not very relevant in judging the performance of either equation;

2. considering the more relevant comparison, the aggregate equation may explain the aggregate data better than all micro equations combined if our micro equations are not 'perfect'. Since perfection is unlikely, aggregation may result in a 'net gain'."

They do mention some limitations of their study, such as:

"We have not investigated, except by implication, the results of aggregation procedures in the presence of measurement errors in the independent variables. In particular, the poor quality of micro data may be another source of aggregation gain."

"... we do not claim that a perfectly specified micro system would not out-perform a macro equation, only that we do not live in a world of perfect micro systems. In particular, most of our economic theory, though couched in micro language, has really been derived with aggregates in mind. It is a theory that explains 'average' behaviour, never claiming to be able to explain the behaviour of a particular individual. To give an 'adequate' explanation of individual behaviour would require a much more detailed theory; such a theory, among other things, would have to account for the interdependence in the behaviour of individuals. Most of the theoretical work has concentrated, however, on improving and making more rigorous the existing theory, a theory essentially designed for the explanation of aggregates."

"It is undoubtedly true that disaggregation has certain advantages. In particular, it may suggest to us how to improve our theory. It may be futile, however, to expect that disaggregation will result in a better explanation of the aggregates without an appropriate change in the model. Different levels of aggregation require theories with different levels of abstraction."

"In the light of this discussion, an econometrician could reach two alternative conclusions. One would be to try to improve our theory so that it would be more applicable to micro data. Alternatively, one could concentrate on improving the macro theories and estimation techniques. The authors are somewhat prejudiced in favour of the second direction, as they doubt that economists have a comparative advantage in deriving theories that would explain adequately individual behaviour. But surely both directions are worth pursuing. It is worth remembering, however, that aggregation is not necessarily bad if one is interested in the aggregates."

Although many articles have been written on the topic of aggregation since 1960, no progress seems to have been made from a philosophical viewpoint. If we know the micro structure truly and measure the micro data correctly, aggregation seems to be disadvantageous. In the other cases, it appears that working with macro relations and the corresponding data is often preferable.

We would suggest that a researcher should investigate the state of affairs with respect to theoretical model and available empirical information, before deciding on the 'best' estimation procedure. Similarly, model construction for the sake of policy making should incorporate the desired level of disaggregation in the policy goals and the associated measures. Not only will such procedure help the policy maker in arriving at the proper result, it will give him (and the investigator) the proper picture of information costs associated with the policy proposal. Many economic policies, although formulated in the aggregate, are designed to have an impact at the micro level. In cases where macro observations are easier to obtain than their micro counterparts, a trade-off exists between program effectiveness and (information) costs. For example, inflation correction of income taxes should allow for differences in Consumer Price Indices for the various income groups and family compositions, and regional price movement disparities. Applying an 'average' C.P.I. will possibly benefit those groups who need it the least and vice versa. Similarly, policies designed to reduce frictional unemployment should incorporate the characteristics of the potential labour force in the fullest detail. Quite obviously, both cases present a trade-off between program effectiveness and information costs. Without an explicit evaluation of both factors, no decision on their optimal levels can be made.

On the other hand, the policy-maker may well be satisfied with aggregate figures as his actions are designed to influence the collective rather than individuals within the collective. Keynesian policies to stimulate demand are usually developed within that context. Aggregate policies may be dealt with via aggregate models based on aggregate data. And these are usually available in more accurate form than the micro data. As Juster (1970) writes:

> "Despite the fact that there are no reasonably accurate micro data, aggregate savings can be estimated with fair accuracy as the difference between income and consumption. And one of our most famous empirical generalizations about savings -- that the savings/income ratio is secularly independent of the level of income -- is based solely on the measurement of net investment in capital assets, which conveniently enough happens to be equal to observed saving."

2.5 Bayesian Analysis

The major differences between classical statistical methods and their Bayesian counterparts are the explicit use of prior knowledge and the incorporation into the various procedures of the purposes of statistical decision making.

It is hardly conceivable that a statistician, when attempting to estimate parameters, has no idea whatsoever of their possible values. Similarly, when testing hypothesis, he may well have some other information as to which hypothesis is most likely to be true.

The Bayesian researcher puts this prior knowledge in the form of a 'prior density function' of the parameter(s) involved. [17]

The statistician subsequently specifies the expected distribution of the sample outcome given the unknown parameter: $P(X | A)$ where 'A' is the unknown parameter and 'X' stands for the sample outcome: 'P' indicates 'probability', and means 'given' or 'under the condition of'. He then performs the experiment and obtains a sample. The next step is most crucial. Just like he was able to specify the distribution of the (undrawn) sample given the parameter, he can work backward and construct the distribution of the parameter given the sample outcome. This application of the Rule of Bayes' or Law of Inverse Probabilities can be written as

$$P(A | X) = \frac{P(A)\ P(X | A)}{P(X)}$$

The function $p(A | X)$ is called the 'posterior density function' of A. It differs from the prior d.f. in that it incorporates the sample information along with the prior knowledge or beliefs.

The following step is the estimation of the parameter from the available information embodied in the posterior d.f. This is done with a so-called loss-function which specifies the 'fine' incurred for wrong decisions. We select now the estimator which minimizes the (expected) loss.

We will illustrate the importance of proper selection of the loss function -- based on the eventual use of the estimates

-- with the help of an example from Wonnacott and Wonnacott (1969):

"Suppose the judge at a beauty contest is asked to guess the height A of the first contestant, whom he has never seen. Yet he is not in complete ignorance; suppose he knows that the heights of contestants follow this probability distribution

p(A):

A (inches)	p(A)
64	.1
65	.1
66	.2
67	.2
68	.3
69	.1

i. Suppose, in order to encourage an intelligent guess, the judge is to be fined $1 if he makes a mistake (no matter how large or small); 'a miss is as good as a mile'. What should the rational judge guess?

ii. Suppose the rules become more severe, by fining the judge $y for an error of y inches; the greater his error, the greater his loss. What is his rational guess?

iii. Suppose the rules are made even more severe, by fining the judge $$y^2$ for an error of y inches; this is the same as (b), except that the loss becomes more severe as his error increases. What is his rational guess now?"

The solutions to the three cases are as follows:

i. The judge should select the height with the highest probability, i.e., the mode (= 68).

ii. The judge must pick that value of $\hat{A}$ for which

$\Sigma \left| y_i - \hat{A} \right|$ (the sum of the absolute

deviations) is minimal. The result is the median value 67. (write

$$\sum \left| y_i - \hat{A} \right| \quad \text{as} \sum_{i=1}^{n} (\hat{A} - y_i) + \sum_{j=n+1}^{n+m} (y_j - \hat{A})$$

where n and m are the number of cases to the left and to the right of $\hat{A}$, respectively, where the y-values have been ranked in increasing order; differentiate with respect to $\hat{A}$, obtain $n - m = o$, or $n = m$; and conclude that $\hat{A}$ must lie in the middle of all observations ranked in ascending order).

iii. The best value is now the mean value 66.8 as this minimizes the sum of squared deviations $\Sigma\ (y_i - \hat{A})^2$ (differentiate with respect to $\hat{A}$, obtain $-2\ \Sigma (y_i - \hat{A}) = o$, and conclude $\hat{A} = \bar{\bar{y}}$. Note that we used this result in Section 2.2 to prove that the least-squares estimator $\hat{B}$ has a decreasing variance with increasing sample size.)

We see that three criteria lead to three different results. As any statistical measurement is performed with a certain goal in mind, and the improper decisions will have some kind of consequences, it is quite logical to take these consequences into account. [18]

A similar approach is followed with respect to hypotheses testing. Again, prior information about the parameter, together with the sample information, leads to posterior density functions, now for each of the hypothesized values. Combining these posterior d.f.'s with the losses to be incurred (nil or positive) because of the various decisions (correct and incorrect) implied, we can decide for the hypothesis which will minimize the expected loss. We thus integrate the likelihood of various losses with their respective levels of seriousness.

Comparing the Bayesian approach with the classical one, we suggest that it is quite superior in that all information is being used; moreover, the selection process is based on the eventual application of the results. [19]

In the extreme case that no prior information can be thought of, we may use 'diffuse', 'vague', or 'uninformative' prior density functions for the parameter(s), the most 'uncertain' [20] of which is the 'normal' distribution (for infinite ranges and a given variance) or the 'uniform' or 'rectangular' distribution (for finite ranges). As soon as some more advance knowledge is available, this can be incorporated in more informative 'priors'. [21]

The result of Bayesian analysis is usually more accurate than that of the classical method for this very reason.

With respect to the loss-function, we might remark that the classical model uses a loss-function implicitly; e.g., least squares minimizes the sum of squared deviations from the optimum parameter value; as we saw above, this is a special kind of loss-function. Also, it can be shown [22] that sound classical methods like Maximum Likelihood Estimation lead to the same results as Bayesian analysis with quite unlikely prior density functions like, for example, a U-shaped curve.

Most Bayesian - just like classical - procedures become more accurate with increasing numbers of observations. Moreover, their estimation results approach the classical values for larger and larger sample sizes, and quite rightly so. Such Bayesian estimates are 'weighted averages' of the prior and the sample information. The larger the sample, the less the weight of the prior knowledge, hence, the closer the Bayesian results should lie to the classical ones.

The Bayesian statistician may evaluate his results on their sensitivity for the assumptions made. A very useful implication is the consideration whether improved prior information can sway the results and, through the mechanism of the loss function, lead to a smaller expected loss for the (new) optimal decision. Thus, we have acquired a powerful measure of information-gathering effectiveness as the (monetary) value of additional information can be established. Of course, this does not guarantee that we do make the 'proper' decision. Averaged out over all possible possibilities, we obtain the (improved) optimum result. This information aspect can be used to dynamize the model, too, leading to information-gathering strategies and optimal stopping rules (see, for example, McCall 1965). [23]

Summarizing, we have the following structure of the Bayesian decision theory (quoted from Green):

"1. A decision maker is faced with a choice among alternative options, the consequences of which are not known with certainty.

2. The decision maker incurs costs associated with wrong decisions. These costs of wrong decisions would be monetary in nature, or more generally, may be expressed in terms of utilities.

3. The decision maker has the option to acquire more information about the consequences associated with the courses of action under evaluation. This information will be cost incurring in itself, will delay the decision and, in addition, will be usually less than perfectly reliable.

4. The model provides a way to determine how much information should be acquired and what choice to make after its receipt. Moreover, the model can be extended to deal with a sequence of actions over some planning horizon."

Apart from their extensive application in statistics and econometrics, Bayesian principles have found a place within the theories of economic decision-making. We shall mention a few cases.

Turnovsky (1969) applies Bayesian techniques to the dynamic theory of the firm under uncertainty, assuming prices that are normally and independently distributed over time. He concludes:

"If a decision maker accumulates information by Bayesian sampling, then his subjective expectations are found to change in an adaptive manner, where the rate of adaption varies at a rate which depends upon the relative precisions of the prior and sample means. By taking special cases, we are thus able to justify several existing expectation hypotheses as arising within this Bayesian context. For illustrative purposes, it is then shown that, if a firm forms its expectations in this way, then, a change in the prices or uncertainty of one period will indirectly affect all future decisions. Necessary conditions for these long run effects to imply finite long run supply elasticities are found to require that the firm's prior subjective distribution of the expected price must ultimately collapse to the true expected price. In general, this condition will be satisfied, but in any event, any increase in price will always have a positive effect on the output of subsequent periods, while the effect of an increase in uncertainty can be of either sign, depending upon what actual prices turn out to be."

Hartigan (1969) suggests a 'Bayesian' method of linear prediction, using only the first two moments (mean and variance) of the distribution of parameters and observations, rather than the complete probability model. This appears a quite 'economical' approach as satisfactory results can be obtained with much less specification.

An interesting macro-economic application of Bayesian methods is given by Fisher (1962). Working with a quadratic target function (see Ch. 6) and a linear model of the economy, he derives that - under certain assumptions - least-squares estimation of the (reduced form of the) model does lead to the best predictions, but not to the best decisions if the coefficients of the target function are unknown. Only when a large sample is available and the variance of the disturbances is small, the differences between the classical results and their Bayesian counterparts become small. But, these conditions are hardly ever met in economics.

The literature on Bayesian analysis is growing rapidly. Lucid introductions of a general nature can be found in Raiffa (1968), Hamburg (1970), and Lindley (1971). The economist might consult Wonnacott and Wonnacott (1969) for an elementary expose and Theil (1971) for a brief summary and some comparisons with classical econometric techniques. Zellner (1971) presents the most complete and advanced treatment to date.

Some applications of Bayesian techniques in water resources planning can be found in Cox and Siskin (1971) and Conover (1971).

CHAPTER 3

OPTIMIZATION IN ECONOMICS

Our main conclusion in Section 1.7 was that the question of information efficiency in economics is best dealt with via the use of formal decision models. Most of these structures are of an optimization nature. Conceptualizing information efficiency requires an evaluation of the sensitivity of the optimum for expanded and improved data input of various kinds. We thus have a dual optimization problem in our hands. Ordinarily, the benefits of more and better information are derived explicitly in terms of the optimum of the policy solution, and subsequently compared with the additional information cost involved. Incorporation of both costs and benefits of information into the policy model appears to be difficult, if not impossible.

In this chapter, we will be concerned with optimization in a macro-economic framework. Assuming that the statement of the problem itself can be achieved accurately and uniquely, it remains necessary to specify a criterion (or set of criteria) for optimality.

Unfortunately, there is no agreement on this issue. In micro-economic analysis, the theory of the firm has traditionally been developed under the assumption that the entrepreneur seeks to maximize his profits. Although economists accepted such a premise for decades, new developments point to other goals of managerial policies. Baumol (1958) states that businessmen maximize sales volume, given a constraint on the profit rate, while Lanzillotti (1958) observed that they simply seek to reach a certain target rate of profit. Whereas these criteria are at least explicit and measurable, things become complicated with Simon's theory that entrepreneurs try to achieve some aspiration level without maximizing anything in particular: they are 'satisficers' rather than maximizers (see Simon 1959).

The problem of criterion selection is even more difficult in macro-economic analysis.

The measurement of 'social utility' has always presented the economist with insurmountable difficulties. According to Joan Robinson, economists in the mainstream of thought have circumvented this problem, at least in their own minds, by means of identifying social utility with total production. The context within which Mrs. Robinson (1966) makes her charge concerns the egalitarian marginal utility of income. In her words,

> "The method by which the egalitarian element in the doctrine was sterilized was mainly by slipping from utility to physical output as the object to be maximized. A smaller total of physical goods, equally distributed, admittedly may yield more utility than a much larger total unequally distributed, but if we keep our eye on the total of goods, it is easy to forget about the utility."

Joan Robinson continues her critique of those who employ income as a proxy for that elusive concept, utility:

> "The utility concept purports to look behind the 'veil of money', but utility cannot be measured, while money values can, and economists have a bias in favour of the measurable, like the tanner's bias in favour of leather.
>
> The very fallacies that economics is supposed to guard against, economists are the first to fall into. Their central concept, National Income, is a mass of contradictions. Consumption, for instance, is customarily identified with sale of consumers' goods, and a high rate of 'consumption' is identified with a high standard of life."

Almost as if his remarks were designed to confirm the truth of Mrs. Robinson's charges, James Tobin (1966) stated the following:

> "Some of the noneconomic reasons for favoring faster growth also suggest that GNP is the relevant measure. But, as economists, we would make welfare or utility depend on consumption. We would require the investment part of GNP to derive its value from the future consumption it supports."

It seems necessary and desirable at this stage, to explore in greater depth the relationship between consumption and social welfare before making any substantive judgements.

> "Consumption is the sole end and purpose of all production."

So wrote Adam Smith in 1776.

In 1962, Kenneth Boulding protested:

> "Economists have frequently written as if consumption was the desideratum, the end product of all economic activity. Such, however, is not the case. It is true that there are some commodities which must be consumed in the utilization, such as food and fuel. This, however, is a technical accident. For most commodities, consumption is merely incidental to their use, and far from being a desideratum, is to be avoided as much as possible."

A pertinent question to pose is whether these two eminent gentlemen are really in fundamental disagreement with each other. A superficial consideration of the matter might well lead one to answer in the affirmative. However, by examining their respective comments within the context of the social milieu in which they were penned, one might be convinced that the comments of both men reflect their common concern for the welfare of consumers. The seemingly contradictory nature of their remarks is a reflection of the transformation in the import of the term 'consumption' which has transpired between 1776 and our time.

Adam Smith's assertion may well be motivated by his concern for the masses of people who barely managed to subsist. His plea seems to be for a more humanitarian economics wherein sight is not lost of the ultimate objectives of this discipline: namely, as Lionel Robbins has phrased it, to study "human behaviour as a relationship between ends and scarce means which have alternative uses." It must be recalled that the Mercantilist preoccupation was more with productive capacity as a means to national power than as a means to the economic well-being of the populace; Smith was probably attempting to redress the imbalance between consumption and investment goods in order to ameliorate the strife of the starving masses.

Boulding's remarks appear to have their origin in his general dissatisfaction with traditional economic theory which treats purchases and consumption as identical phenomena with respect to consumer commodities; according to him, these two phenomena are distinguishable, and are separated by a stock of consumer capital. Joan Robinson seems to capture the essence of Boulding's critique when she asserts that:

> "... consumption, in the plain meaning of the term, in the sense that it is connected with the satisfaction of natural wants, does not take place at the moment when

> goods are handed over the counter, but during longer or shorter periods after that event. This time-dimension is completely left out of the figures. It is left out not because anyone denies its importance, but because of the mere difficulty of catching it in a statistical net."

What Boulding does in his analysis, in effect, is to select as his standard of quality measurement 'durability', subject to the condition that the commodity fulfil its functional role.

Boulding equates the consumption of an asset with the depreciation of that asset. Boulding proposes a "principle of minimum consumption" as a yardstick for measuring how economically society rations its scarce resources. His principle essentially claims that, given an allotment of scarce (finite) resources, the lower the consumption (or depletion) of resources in the attainment of a given end, the higher will be society's material welfare. Boulding formulates the principle of minimum consumption in the following manner:

> 'of the various types of assets which satisfy a given want, we will prefer that which is consumed (or depreciated) at the slowest rate in terms of the value unit".

Boulding's principle of minimum consumption appears to be predicated upon his own assessment of the source of social welfare: namely, society's stock of real assets at a given time. His conception of consumption is of a necessary, albeit regrettable, means to the attainment of utility-yielding service (or good). The following passages from Boulding's book illustrate this view:

> "We want to live in houses, and we derive our satisfactions from their existence, not from their wearing out or depreciation. If our houses did not depreciate: if roofs never leaked, if paint never weathered, if beams never sagged and floors never rotted, we should have less consumption of houses, but we would clearly be much better housed. Similarly we want to wear clothes rather than to wear them out, to use furniture rather than to use it up, and so on down the whole list of commodities. If we had unadradable fabrics, unbreakable china, and indestructible furniture, we should again have less consumption, but we would be all the wealthier for this fact..."

> "We do not want to consume fuel except as a means to getting heat, light, or power: the more of these things we can get from a unit of fuel, the better off we are ... We burn coal in a house furnace only because the temperature of the house is a depreciating asset in winter, which has to be replaced by the consumption of fuel ... Even with food, ... an invention which enables us to satisfy our hunger and to provide fuel and maintenance for the body with a smaller consumption of food would leave us better off than before."

In reply to those who would claim that satisfaction is derived from the very act of consumption, Boulding argues as follows:

> "The principle of the minimization of consumption is not invalidated merely because there are technical cases in which consumption cannot be reduced without reducing satisfaction. One cannot escape the feeling also that satisfaction in consumption itself verges on the sadistic and morbid. The sadist, the pyromaniac, and the glutton are alike in that they obtain satisfaction from the destruction or disintegration of those orderly arrangements of matter which constitute capital and which are the result of productive activity. Such satisfactions, however, can hardly be regarded as healthy."

Perhaps the kindest thing one can say about Boulding's value judgements is that they are explicit. Nonetheless, when one applies his standards for measuring society's material welfare to a mature capitalistic economy such as the U.S. today, the evidence seems to show an age of unprecedented wastefulness. Boulding's estimation of a commodity's durability is very closely akin to that of the engineer (embracing only the objective physical characteristics of the commodity). However, in a mature capitalistic economy, there is an additional dimension which influences the 'lifetime' of a commodity, and that is the rate of its psychological obsolescence.

Vance Packard (1963) delineates the following three types of obsolescence:

1. Obsolescence of function, wherein "an existing product becomes outmoded when a product is introduced that performs the function better."

2. Obsolescence of quality, wherein according to plan, "a product breaks down or wears out at a given time, usually not too distant."

3. Obsolescence of desirability, wherein "a product that is still sound in terms of quality or performance becomes 'worn out' in our minds because a styling or other change makes it seem less desirable."

The U.S. economy is characterized by the existence of all three varieties of obsolescence to a greater or lesser extent. However, only the first type -- obsolescence of function -- ought to be considered to be a social gain per se. The latter two varieties of obsolescence -- those of quality and of desirability -- may be deemed to be social benefits only insofar as they facilitate other social objectives such as the fuller utilization of the economy's resources. Certainly, from an engineering viewpoint, obsolescence of quality and of desirability represent wastefulness of resources in that the lifetime of commodities is unnecessarily diminished. In the U.S. context, the logical explanation for the emergence of the latter types of obsolescence is that "the spectre of satiation" has manifested itself; that is, "America's capacity to produce may have outstripped its capacity to consume." To the extent that obsolescence of quality and of desirability are effected by means of consumer manipulation through large-scale advertising, the magnitude of society's aggregate consumption as a barometer of social welfare is of highly dubious value. Moreover, if durability of goods is taken as an index of their quality, the measure of durability must embody all three obsolescence components, since the actual use-lifetime of the commodity is diminished accordingly.

The prescription for buoyant economic conditions proposed by Victor Lebow, that of 'forced consumption', has rather frightening overtones, somewhat reminiscent of Brave New World:

> "Our enormously productive economy ... demands that we make consumption our way of life, that we convert the buying and use of goods into rituals, that we seek our spiritual satisfactions, our ego satisfactions, in consumption ... We need things consumed, burned up, worn out, replaced, and discarded at an ever increasing rate." (as cited in Packard 1963)

Can one really discern genuine social progress if man must be transformed into a self-indulgent glutton who gorges himself upon the fruits of the industrial system in order to forestall its demise? Can one justifiably claim that man is being served by the industrial system where the role of man has degenerated to that of being a mere cog, albeit a not insignificant cog, in the industrial system? One's reply must, of course, ultimately depend upon one's conception of 'progress'. According to Harry G. Johnson (1964):

> "Progress takes the form both of satisfying wants more fully, and of raising the standard of wants. In technical jargon, it is expressed both in improvements in the production function, and in improvements in the consumption function ... it pays the entrepreneur ... to invest in improving the technique of consumption in order to adjust the want more profitably to the product; in short, it pays to advertise."

And when he ventures the opinion "that the creation and satisfaction of wants by advertised production does result in social gain", he does so "on the grounds that there are generally accepted standards for distinguishing meretriciously from genuinely superior products ..."

The evidence that the standard of wants has indeed been raised, or that improvements in the consumption function or in 'the technique of consumption' appears to be conspicuous by its absence. For one thing, these phrases have a very high metaphysical content, and it is most difficult to devise a scientific test which could satisfactorily gauge their veridicality. Moreover, such evidence as does exist seems to undermine the strength of claims that progress of this variety has occurred. In their study of "The Cost of Automobile Model Changes Since 1949", F. M. Fisher, Z. Grilliches, and C. Kaysen

> "concentrate on the cost of the resources that would have been saved had cars with the 1949 model lengths, weights, horsepowers, transmissions, etc. been produced in every year. As there was technological change in the industry, [they] were thus assessing not the resource expenditures that would have been saved had the 1949 models themselves been continued, but rather the resource expenditures that would have been saved had cars with the 1949 specifications been continued but been built with the developing technology as estimated from actual car construction cost and performance data." (F. M. Fisher, Z. Grilliches, and C. Kaysen (1962))

Their results indicated that the cost of model changes amounted to $700 per car (i.e., in excess of 25% of the purchase price) or a total of 3.9 billion dollars per year from 1956 to 1960. However, after due allowance for other costs associated with model changes, such as those resulting from accelerated obsolescence of repair parts, increased repair costs due to changes in car design and construction, and additional gasoline consumption, the estimate for the average increment in gasoline consumption expenditures per year was $968 million for the 1956-60 period. The final conclusion of the authors was as follows:

> "We thus estimated costs of model changes since 1949 to run about $5 billion per year over the 1956-1960 period with a present value of future gasoline costs of $7.1 billion. If anything, these figures are underestimates because of items not included."

To the extent that the automobile industry typifies other large-scale industry in the U.S., many of the vociferously-acclaimed products are, at best, meretriciously superior to the models which they displace, the contrary claims of Prof. Johnson notwithstanding. On grounds of physical durability (Boulding's criterion), there is serious doubt that significant 'progress' has been made. The tendency appears, in fact, to be in the opposite direction, with still further diminution in average product lifetimes.

Crucial for the above-mentioned problem is that the definition of 'good' has resulted in a feedback of contamination of that very goal. This seems to be an almost unavoidable feature of optimization processes where the 'target' or 'preference function' cannot be formulated except through the use of proxies. Without some proper method of evaluation, quantity (being measurable accurately) drives out quality (hardly estimable, except by complete destruction). We could call this Gresham's Law of Mass Production. In this case, the (over?) emphasis on measurable goals has steered our attention away from other objectives of economic activities.

Within the set of measurable descriptions of society's welfare, there is ample room for improvement. As the Economic Council of Canada (1971) writes:

> "Net product, as opposed to gross, excludes assets used, destroyed, or damaged in the production process. It is therefore widely, if not universally, recognized as a more appropriate basis for measuring economic growth. It is not a simple or easily measured concept. The

estimates currently in use provide for the depreciation of traditional assets such as buildings, and machinery and equipment, but they do not take account of deterioration in the natural environment."

"This point leads to the second suggested change. Environmental assets such as air and water are neither so free nor unlimited as theory and practice once assumed. They are now viewed as assets that change in quality or quantity and can be improved or destroyed. The next few years will, undoubtedly, see a significant increase in expenditure by the public and private sectors to undo past ecological harm and to limit its increase. There are several ways in which this use of resources could be reflected in the national accounts. Under existing procedures, investment expenditures by governments and business to maintain or restore the environment would result in an increase in the real rate of growth of Gross National Product. A clarification of this contradiction as soon as possible is highly desirable."

Heller (1967) suggests in this regard:

"If, as by-products in our quest for growth, we destroy the purity of our air and water, generate ugliness and social disorder, displace workers and their skills, gobble up our natural resources, and chew up the amenities in and around our cities, the repair of that damage should have first call on the proceeds of growth ... If we could isolate that part of it which is a direct cost or by-product of growth from that which is a natural concomitant of population growth and urbanization and so forth, we should probably make a subtraction each year from our total output, an adjustment of our GNP figures, to take account of it."

An empirical application of these ideas is presented by Leontief (1970) who tries to incorporate pollution into the input-output approach to economic modelling. Input-output analysis describes the interrelationships in the economy based on the input structures of the various sectors; i.e., any sector produces according to the input needs of all sectors, in addition to the output used to satisfy final demand (from outside the 'productive' sectors). As some production processes result in so-called 'by-products', i.e., not belonging to the proper output package of the industry concerned itself, economists have amended input-output models as to incorporate these aberrations. Leontief now defines pollution as a "by-product of regular economic activities" which "is related in a measurable way to some particular consumption or production process". This by-product can now be used as an input by a "pollutant eliminating" industry which, on one hand, uses up labour and other resources as a true cost of production, and, on the other hand, may not be able to use the total amount of pollutants. The latter quantities are then transferred to the final-demand sector as non-demanded but yet tolerated undesirable outputs. Leontief's approach also allows us to calculate the pollution-related components of the prices at which output is being traded.

The beauty of the above model is the opportunity to trace the effects of various rates of pollution and of anti-pollution activities through the economy.

It can also handle cases of completely inadmissible polluting acts of production and consumption. [24]

Uzawa (1970) goes further than just correcting GNP for undesirable effects of production and consumption and, in fact, adapts the Boulding philosophy:

> "When we compute the GNP, we deduct depreciation -- whatever it costs us to keep our capital intact. What I propose is also to deduct from the standard concept of national income the amount needed to maintain the present stock of natural and social capital." (as quoted in Lekachman 1972)

This change in measurement is based on a change in economic philosophy as ably described by Boulding (see earlier in this chapter). Further elaborating on the concept of welfare versus output, Boulding (1966) argues that the fallacy of using Gross National Product as the yardstick of economic performance is caused by society's failure to realize that it is moving rapidly from a "cowboy economy" towards a "spaceman economy". In the former, "... there are infinite reservoirs from which material can be obtained and into which effluvia can be deposited, ... the throughput is at least a plausible measure of the success of the economy. The gross national product is a rough measure of this total throughput." The "spaceman economy" is a closed system "in which the earth has become a single spaceship, without unlimited reservoirs of anything, either for extraction or for pollution, and in which, therefore, man must find his place in a cyclical ecological system which is capable of continuous reproduction of material form even though it cannot escape having inputs of energy." It seems to be most crucial "to distinguish that part of the GNP which is derived from exhaustible and that which is derived from reproducible resources, as well as that part of consumption which represents effluvia and that which represents input into the productive system again." Boulding insists on a complete change in philosophy:

> "The essential measure of the success of the economy is not production and consumption at all, but the nature, extent, quality, and complexity of the total capital stock, including in this, the state of the human bodies and minds included in the system. In the spaceman economy, what we are primarily concerned with is stock maintenance, and any technological change which results in the maintenance of a given total stock with a lessened throughput (that is, less production and consumption) is clearly a gain. This idea that both production and consumption are bad things rather than good things is very strange to economists, who have been obsessed with the income-flow concepts to the exclusion, almost, of capital-stock concepts."

Changes in philosophy and measurement must now be followed up with appropriate policies. We should attempt to reverse the 'contamination' process by re-evaluating the trade-off between quantity and quality, particularly where it reflects on the balance between present and future needs and their respective satisfaction. The "Blueprint for Survival" suggests an "amortization tax" proportionate to the estimated lifetime of the product;

> "e.g., it would be 100 per cent for products designed to last no more than a year, and would then be progressively reduced to zero per cent for those designed to last 100 + years. Obviously, this would penalize short-lived products, especially disposable ones, thereby reducing resource utilization and pollution, particularly the solid waste problem. Plastics, for example, which are so remarkable for their durability, would be used only in products where this quality is valued, and not for single trip purposes. This tax would also encourage craftsmanship and employment-intensive industry."

To improve the allocation of our resources over time, the Blueprint proposes a "raw materials tax".

> "This would be proportionate to the availability of the raw material in question, and would be designed to enable our reserves to last over an arbitrary period of time, the longer the better, on the principle that,

> during this time, our dependence on this raw material would be reduced. This tax would penalize resource-intensive industries and favour employment-intensive ones ... it would also penalize short-lived products."

It might be even more appropriate to depart completely from the use of GNP as a yardstick of economic performance. The logical conclusion of Boulding's philosophy is the design of different criteria of social-economic welfare. The Economic Council of Canada (1971) suggests the use of Goal Output Indicators, measuring the impacts, rather than the means, of government programs. For example, in the field of health care, we might use the following criteria:

Longevity	Infant mortality rate Live birth rate Death rate from: Accidents Degenerate diseases of old age Acute/chronic diseases
Days free of physical illness or disability	Days hospitalized or abed Days off work/school Level of nutrition Level of fitness
Days free of mental/ emotional illness or disability	Days in psychiatric institutions Number of defective/retarded children Suicide rate

This approach is being used within the context of Cost-Benefit Analysis, to which we devote the next chapter. It is a good example of the integration of measurement and policy-making, and, unavoidably, of the errors involved in both of these procedures.

CHAPTER 4

COST-BENEFIT ANALYSIS

The general idea that the benefits of government projects be measured and related to their costs has been systematically investigated since World War 2, along with the attempt to develop evaluation procedures to apply to government investments in water resources. The two, in fact, appear to have been worked out together, with a large amount of the best work in either field (at least as regards economic content) being found under subject matters spanning both.

Regardless, the cost benefit approach to the analysis of such problems is not restricted to this area of study but is rather general, and may be applied to a large class of projects including, of course, the private sphere. Our concern here will lie with the public activities.

The main objective of such public undertakings is, or is purported to be, the increase in the welfare of the society. There is a need for some technique which will enable us to evaluate the potential projects and rank them according to their respective value. This is not always possible, due to the problem of comparing and measuring the values of individuals. Nonetheless, there appears to be a large class of problems which can be evaluated on the grounds of economic efficiency. What this merely means is that the policy maker, rather than being stymied by these problems, must turn pragmatic and use a subjective ordering in lieu of available measures.

Having stripped the problem of these aspects and thus reduced it to one of allocation of resources, the policy maker is in the position where he can make use of cost-benefit analysis. The criterion that this method attempts to use is an evaluation of alternatives on efficiency grounds.

An investment means an allocation of current resources to an activity that will (hopefully) generate in the future some goods and/or services. These future goods and resources are referred to as the benefits received from such an investment. The future goods and services that could have been generated from the use of the next best investment (assuming the first to be optimal) are referred to as the costs of such an investment. The problem then is to determine the various benefits and cost from the competing alternatives and, from these calculations, find that investment which would yield the highest net benefits (i.e., net benefits equals benefits minus costs; realizing that doing nothing is a competing activity and, thereby, restricts the solution to the non-negative sphere).

We can measure the benefits as being the maximum amount of money that the recipients of the benefits would willingly pay for their receipt. We evaluate costs by equating them to the minimum amount of money that would be required to compensate the owners of the current resources that are to be allocated for investment in this activity. The cost-benefit approach is seen to be a criterion of compensation whose roots lie in economic theorization.

It can and has been shown that if the economy approximates the perfectly competitive model, then these benefits and costs may be dealt with in terms of prices. Unfortunately, perfect competition is non-existent. Therefore, prices do not reflect 'true' costs. Moreover, future prices will still need estimation under conditions of uncertainty.

There is also the problem of measuring the impact as well as the value of public benefits and public costs (i.e., those that cannot be attributed to specific individuals). This idea of public goods is a special case of what is known as external effects. These can be said to exist wherever we find a divergence between private and social benefits and costs. External costs are generated by industries polluting waters or air as these impose a cost on others. A private firm operating under no obligation to make restitution for these costs may be maximizing private net benefits; yet, there is divergence from social net benefits. [25]

Whereas some of these divergences may be rectified through taxation, subsidies, or legal means, and ignoring the cases where such divergences are judged to be insignificant, we are still left with a significant area for public involvement in the form of investment projects. This means that there are some situations where it is more efficient for the government to undertake a development, rather than to leave it up to the private sector or to develop those market mechanisms enabling the optimum solution to be achieved.

A major limitation of the cost-benefit method lies in the fact that it is amenable to those projects having relatively small effects on the major parts of the economy. If the project is to have significant ramifications on the rest of the economy, or with regard to the physically immediate economy, it is desirable to use macro or intermediate economic models, possibly linked with programming techniques.

The past effort in cost-benefit analysis is a source of valuable information regarding the measurement, definition, and use of both benefits and costs involved in such investments. These benefits and costs have been found to run into several conceptual problems when we attempt to evaluate them. W. Whipple Jr. (1970) gives an extremely good analysis of water quality benefits and additional information can be gleaned from Scherer (1963). Some problems on the cost side are to be found in Loughlin (1970).

It should be noted that the major problems in the area of benefits are multiple goals and non-monetary objectives.

Schramm and Burt (1970) discuss the former problem extensively, in particular with respect to the choice between the maximization of national income and environmental quality.

Howe (1972) indicates three major ways of approaching the problem of project design and ranking in the case of non-monetary (and not expressible in money, either) objectives or impacts:

1. the maximization of national economic net benefits, subject to constraints on the other objectives;

2. using a system of explicit weights to make the several objectives commensurable;

3. the generation of several alternative designs for each project, each emphasizing a different objective, permitting the legislature or decision-making body to choose according to their interpretation of society's needs.

Toebes (1970) presents the following table:

SUMMARY OF FLOOD CONTROL OBJECTIVES

Objectives	Measurement Unit	Desired Effect
Economic Efficiency	Dollars	Maximize: Benefit-Cost
Environmental Quality	Open Space Acres	Maximize: Acreage-Surroundings Urbanization Product
Regional Development	Jobs	Maximize (except in areas of excessive congestion)
Income Redistribution	Dollars by Income Group	Shift Net Benefits to those with Lower Incomes
Financial Stability	Standard Deviation of Annual Damages	Minimize

See also McKean (1958), Crutchfield (1968), Lind (1968), Freeman III and Haveman (1970), S. L. Smith and Hogg (1971) and Dewess (1972).

These cover the majority of the conceptual problems. However, an overview of the literature suggests that the resolution of these problems lies very much with the individual nature of specific projects; a look at past efforts in projects very similar to the one on hand is likely to yield more valuable results than a general knowledge of these conceptual problems.

The multiple objectives problem is obviously related to the size of the decision problem.

Any decision-making process should have a limited character. Too large a number of alternate solutions to one and the same problem make the selection process extremely complicated and, sometimes, impossible. On the other hand, one should realize that such limitation implies that no priority can be given to the optimal solution compared with lines of action not included in the set considered, whether specifically designed to solve this or some other problem.

We may refer to Baxter (1965) who posed the question whether the money required to implement a water-quality-improvement program could not better be spent on education. Cost-benefit analysis does not attempt to make such a decision.

However technical the problem considered might be, one should not disregard the social surrounding in which the discussion takes place (Smith & Hogg 1971). Disregard for this principle often results in failures to implement possibly desirable policies in the developing countries and in 'disadvantaged' areas of the Western World. Changes in the existing social structure or philosophy, as a result of the project - if not a prerequisite for its 'success' - are often met with great resistance and cause a considerable amount of social dissatisfaction. On the other hand, there is every indication that the design of an optimal program for its own social and economic survival may well require the Western (and, in particular, the North-American) world to change its set of social and moral values (see "Blueprint for Survival").

The problem of immediate versus long-term implications of economic programs is obviosly related to the question how costs and benefits are weighed over time. The outcome of a cost-benefit analysis depends heavily on the chronological weights distribution. Moreover, individuals have their own time preference; they may thus assess a project differently, even though all (other) factors are evaluated equivalently. Finally, people tend to give more value to future consumption collectively than they do individually. This is an important argument in favour of government by delegation, rather than via direct participation.

The concept of present value is used to make time-dispersed benefits and costs commensurate. This method discounts the values of future benefits and costs by the marginal rate of time preference and, thus, for total benefits and costs, we add up these discounted values, and proceed as usual.

It should be emphasized that cost-benefit analysis, by limiting its time span to a certain period, implicitly assumes that benefits and costs, as applying within that period, are the full and only determinants of whether the intended investment should be carried out. At the cut-off date, the state of affairs is identical to the initial situation, or the expected impact of spill-overs is positive. In reality, the latter effects may well be negative; in such a case, a decision to go ahead with the plan merely disregards the long-term implications or sacrifices them in favour of immediate interests. This is particularly true when the intended investment has a lasting effect on society which is irreversible, such as resource depletion and permanent change in the environment. Similar problems may arise if the choice is between a project involving a heavy (research) investment to solve basic problems, and a plan of simpler design that merely cures the symptoms without getting to their roots. It does not seem possible to incorporate these aspects into a meaningful and operational analytical framework. The decision-maker may have to revert to a mere ad-hoc procedure of program selection.

It is obvious that cost-benefit analysis requires a great deal of information re the various programs considered. Similarly, the sensitivity for information improvement can be traced, and available funds may be allotted accordingly. The timing element plays a role, here, too. Additional statistical work requires time but any postponement of the action decision results in some social loss. Apart from the optimal allocation of research funds, we have to investigate whether and to what extent waiting is worthwhile in terms of improved outcomes of the analysis.

CHAPTER 5

PROGRAMMING

5.1 Introduction

Programming, in general, offers techniques for the resolution of decision processes, evaluating the alternatives within one analysis (rather than separated ones like the cost-benefit approach). We may characterize the decision processes into types; each type being solvable by an application of one of the programming techniques available.

"The plan, or arrangement, of a water resources project may be called a system. Modern water resources projects often constitute very complex systems which may be created through different combinations of systems units (reservoirs, canals, etc.), levels of output, and allocation of capacity of the units to various purposes (water supply, flood control, hydro-electric power, etc.), at different times. The objective of the system design is to select the combination of these variables that maximizes benefits in accordance with the requirements (constraints) of the design criteria. The constraints can be technical, economical, social, or political, and the benefits can be real or implied." (Chow and Meredith 1969)

Programming techniques are able to maximize benefits within the requirements of the design criteria. It is the transition from actuality to the model with which our concern lies.

Applications of the programming principle in macro-economics will be discussed in Chapter 6. Most attempts have been made within academic circles without due policy applications. Similarly,

"... the use of mathematical programming techniques in water resources system design is just beginning and no extensive applications or verification have yet been made in practical problems ..." (Chow and Meredith 1969)

Although several models and techniques have been developed and are accessible to workers in the area, the change to the new techniques is slow. There are indications that the lack of trained people and of efficient data are the main problems in this trend. (Viessman (1970); contains also a good analysis of the extent to which programming techniques are being used by the various state agencies in the U.S.)

As indicated earlier, we attempt to maximize something (usually benefits), subject to the requirements of the design criteria. The 'something to be maximized' will be called the objective function or the welfare function of the policy maker. Mathematically, these involve certain endogenous variables that the policy maker is able to change, quantitatively, and hence, these have come to be known as the decision variables.

The requirements of the design criteria are called, in their mathematical form, the constraints of the system. These serve to confine the objective function. It is to be understood that the relationships postulated to exist between all variables at one point of time remain quantifiably the same throughout the period under consideration. Whereas this may seem to be a trivial assumption with regard to physical processes, at times, it becomes a crucial assumption when we are dealing with social and political processes.

5.2 Some Classifications

Programming methods can be classified in several manners. The distinction between 'linear' and 'quadratic' programming lies in the power of the decision variables in the objective function; in both cases, the constraints are of a linear character. The term 'geometric programming' is reserved for cases where the objective function is of more than the second degree or the constraints are non-linear. Linear programming is the simplest method, although the use of high speed computers has eliminated many of the earlier technical problems of other models. Still, the linear case is widely used, either because its assumptions are truly correct [26], or a linear approximation (via a Taylor expansion) is acceptable. Similarly, quadratic programming (which assumes a certain symmetry around the optimum) may be used as simplification of a higher-order model. Note that a (large) portion of the objective function can still be taken as linear. Some examples of L.P. applications in the field of water resources may be found in Thomas & Watermeyer (1962), and in Revelle et. al. (1968). O.P., in this domain, is described; i.e., by Lynn (1966), Kerri (1966), and Burt (1967).

Another distinction among programming models has to do with uncertainty. In the above section, we had implicitly assumed that the parameters of the problem with which we were dealing were known quantitatively. In most problems, however, we would not be dealing with known quantities, but, rather, with approximations in the form of estimates. The question now becomes whether or not we may reach an optimal decision, given that we explicitly recognize the uncertainty associated with our data.

If the individual elements of the coefficient vector (described above as being associated with the vector of decision variables) were subject to a probability distribution, then we would wish to work with those values of the parameters that we expect to occur. Thus, if a stochastic process is thought to govern these parameters, then the technique of finding expected values will readily convert the stochastic programming problem into one that is deterministic. Another option consists of taking, e.g., a pessimistic estimate (Loucks 1968). Following the parameter estimation, the problem is solvable by programming techniques.

Similarly, if this occurs with regard to the system of constraints, we may use the technique of taking expectations. Here, we also have two other alternatives available to us. One of these is to make the constraints hold, in the solution, with a probability of one. Formally, this technique is the one labelled stochastic programming (Madansky 1963). We may also incorporate the concept of risk, and in so doing, require that the constraints hold X per cent (5%, for example) of the time (Charnes and Cooper 1959). This is called chance-constrained programming.

Models, in general, and hence programming models too, can be static or dynamic. In the former case, one (set off) decision(s) is selected for only one period of time which happens to be the only decision period involved.

Dynamic programming techniques are those which involve a sequence of decisions. Thus, control of water flows, inventory ordering and the like involve decisions that will reoccur over time and may be included in those problems that are to be dealt with through the application of dynamic programming. These processes and similar ones have come to be known as multiple stage decision processes.

The dynamic programming technique differs then from the static (linear or non-linear, stochastic or non-stochastic) in that it is defined with respect to time. At each of the decision points, however, one of the programming techniques will be applicable. Which of these depends on the current problem, as was indicated in other sections.

We are able to set out some characteristics of the situations which require this type of programming. First of all, the problem must be capable of division into stages that occur at different points in time and each of these stages must be characterized by the requirement that a decision is to be made. Secondly, each of the individual decisions must be amenable to one of the static techniques. A third requirement is that each of the decision stages must be independent of decisions made in past stages.

The literature in water resources management contains a significant number of models utilizing this technique. A good discussion of interdisciplinary methodology may be found in Buras (1972). This is fairly detailed. Some of the problems to be encountered and the resolutions may be found in Meir and Shih (1970), Young (1967), Millham and Russel (1970). For an exceptionally good article in this area, see Lee (1968).

A particularly interesting situation exists when decisions have to be made under uncertainty, maximizing some objective function over a series of periods. One could conceivably solve these problems by applying standard programming techniques after some allowance has been made for the uncertainty involved. However, doing so, one would forego a most important feature, viz., that between subsequent decisions, more information becomes available, reducing our ignorance. We need to develop a strategy incorporating the expected acquisition of information in some optimal way. Such a procedure can be found in Chapter 6.

CHAPTER 6

MODELS

6.1 Introduction

This chapter is concerned with the philosophy and technique underlying model building in economics. It is not, exclusively, geared towards resource management problems. The interested reader is referred to Biswas (1972) for numerous excellent discussions on applications in the latter field.

We are concerned with empirical models describing the state and possible future states of an economy. Some of these models are deterministic in the sense of a simple algebraic procedure by which one set of variables is linked with another set. Other models are so-called optimization models as they attempt to reach a certain optimum, subject to constraints imposed because of the actual or likely state of the economy.

There is no need nowadays to defend the construction and use of formal models in economics for policy purposes. Just as in the case of forecasting, we suggest that, if the government has a political responsibility for the welfare of the nation, it should evaluate the alternative actions intelligently.

Such investigation is best performed with help of formal models rather than ad-hoc reasoning.

As Ball (1963) writes:

> "The simultaneous model approach has the virtue of making its assumptions more explicit. This makes it, at once, easier to criticize, but also to locate the causes of trouble and bad forecasts. Often those who decry the econometric method and produce forecasts themselves are, in effect, operating within a framework that could, in fact, be formalized. They make assumptions and then draw inferences from the assumptions as to the probable forecast values of variables. An unwillingness to make the approach more formal is a rather unfortunate way of suppressing assumptions that might not stand up in the light of day. The econometrician is open and sticks his

> neck out and, not unexpectedly, finds many happy to chop his head off. But, at least, it is possible. The explicit character of the model approach permits the weak spots to be examined as more experience becomes available and suggests lines of improvement. The construction of a model is only a starting point for development. One of the difficulties of the past is that few models have been developed after they have been built. It has been an achievement to build them, after which they merely survive as relics in academic literature. In the few cases where development has succeeded initial construction, the evidence suggests that the pay-off may be substantial (for an example cf. Suits (1962))."

We should recognize that the construction of a model works on the principle that we seek simplicity that captures the essence of reality rather than a perfect description. This latter is impossible in a stochastic world, and attempts to approximate it are associated with exponentially rising costs. Coupled with this is the real danger of implying too much about the 'real' world, as it may frequently be seen that some modellers, having chosen a simple model on the principle of Occams' razor, may turn around and impute simplicity on nature. This is seen as somewhat paradoxical if one returns to the initial stages of the construction of the model and observes again the conceptual difficulties involved.

As quite customary in the social sciences, empirical models are difficult to build because of the vagueness of their theoretical counterparts.

Ball (1963) states:

> "Constructing a macro-statistical model is not simply a matter of applying existing economic theories to available data, principally because a great many economic theories and hypotheses are formulated in such a way that one hardly knows how to go about applying them. In this respect, the methodology of economics has been grossly at fault. A large proportion of economic theorizing is conducted not in the context of challenging facts and figures, but in a vacuum. The result is that theories emerge in search of phenomena, rather than phenomena appearing seeking to be explained. Theory and applied work lack the unity that is essential to make them both important, which leads to the proliferation of economic boxes -- not quite empty, but in which the facts rattle rather uncomfortably. In

practice, therefore, the model builder finds himself forced into a great deal of ad hoc theorizing to deal with specific problems as they arise. The inefficiency of this procedure need hardly be stressed."

No economist will consider any model to be faultless. In fact, the proof of the pudding is in the eating, and quite often the gastronome (or patient) will end up suffering from a severe case of indigestion. But, as the alternatives are not eating at all or at random (possibly aided by a well-outdated book on nutrition), we might consider the risk of falling ill worthwhile.

Even if models are adequate descriptions of average events and movements as have actually taken place during the observation periods, performance deteriorates as soon as they are used for forecasting purposes. Perfect advance information re exogenous variables and model parameters is impossible. Every prediction takes, thus, the form of a conditional statement. The more explicit these conditions are, the better and quicker the model can be adjusted. Extrapolation methods (indicators, Markov-chains) fail to incorporate (and certainly to reveal) the required conditional character. Klein (1971) gives some examples of the inability of such methods which not only predict poorly, but also fail to recognize when the pattern of the economy has actually changed direction. Measures of economic policies, to offset undesirable developments (as possibly predicted by these very same models), cannot be incorporated unless

a) on a very subjective basis; or

b) if embodied into the framework in the form of automatic stabilizers, for a sufficiently long time as to be recognized by the prediction formula.

In both cases, we consider the use of more sophisticated models (Tinbergen, Theil) desirable. In fact, they will enable us to perform 'comparative statics' on the economy by comparing the various outcomes, given different policy options (see also Samuelson 1965).

As we stated earlier (Section 1.7), the choice of model sophistication may be done via an evaluation of the costs and benefits of various alternatives. It may well be true that simultaneous-equation systems tend to predict better than simple extrapolation methods. As the latter are much cheaper than the former ones, the choice is a matter of trade-off between cost of input and benefit of results. The benefits then include the positive effects of subsequent policy decisions. Given the limited familiarity with analytical techniques among policy makers, there is a strong argument to keep models relatively simple lest the results be misinterpreted or even ignored (see also O.E.C.D. 1972).

The use of rather simple models is preferred -- even for tentative policy purposes -- if the demonstration of certain overall effects is more important than the exact quantification of all implications of the model. As the Economic Council of Canada (1971) writes:

"Knowledge about interrelationships and spillovers provides a better basis for designing a program network in which these effects reinforce, not impede, progress towards objectives. There is, of course, an important element of judgment here too. At some depth of analysis, almost everything can be seen to relate to everything else; at that point the analysis sinks under its own weight. Long before then, however, many simple but important interrelationships can be identified, anticipated in the design of programs, and accounted for in their evaluation."

An example of this approach is the so-called Club-of-Rome Model (Meadows 1972) which investigates the earth's economic and social future development under conditions of continued growth in output and resource use. In fact, the justification for this method can be found in the increased awareness for the problems of congestion, pollution, and exhaustion of the environment in all its facets. On the other hand, the sensitivity of the model's results to changes in the input structure has been demonstrated amply. Similarly, the 'information effect' we encountered during our discussion of economic forecasting could well change the underlying relationships. Incorporation of these two aspects into the main model would enable the development of a set of forecasts, depending on various levels of data quality and feedback mechanisms. [27]

A very interesting experiment is performed by Baumol and Quandt (1964) who accept the fact that true and complete models are expensive even if they are possible to come by. They investigate how efficient the use of rules-of-thumb might be, using the concept of 'optimally imperfect decisions'. Such a procedure is defined as

"a set of rules describing a decision procedure with the following characteristics:

a) The variables which are employed in the decision criteria are objectively measurable.

b) The decision criteria are objectively communicable, and decisions do not depend on the judgement of individual decision-makers.

c) As a corollary to (b), every logically possible configuration of variables corresponds to a (usually unique) determinate decision.

d) The calculation of the appropriate decision is simple, inexpensive, and well suited for frequent repetition and for spot checking by management in higher echelons.

A decision process with these characteristics seems to be designed as the instrument par excellence of optimally imperfect decision-making for routine and recurrent problems."

The authors then proceed with a simulation of the profit-maximizing behaviour of a single-product monopolistic firm under various assumptions re the mathematical form of the demand and the cost function. Instead of expensive market surveys and cost analyses, the firm takes just two points on each curve (demand and cost) as historically observed, and estimates the complete functions. The various rules (based on the mathematical formulation, except for two naive cases of fixed price and random price) of profit-maximization through price selection, are tested for each feasible case of certain discrete demand and cost functions. Finally, comparing the results of the rules-of-thumb with the true profit-maximizing behaviour, it appears that several non-optimal decisions are quite good. Although Baumol and Quandt do not investigate the cost side of the problem, it does not take too much imagination to suggest that the degree of non-optimality might be less important (in terms of profits foregone) than the reduced spending on the decision process itself.

6.2 The Tinbergen Model

Tinbergen (1952, 1956) centers his methodology of economic policy around an econometric model. Such a model usually consists of

1) behavioural equations (e.g., consumption function);

2) technical relations (e.g., production function);

3) institutional relations (e.g., tax rates); and

4) definitional and balance equations (e.g., the formation of Gross National Product as the sum of all aggregate demand items for domestic production; the level of this year's capital stock equalling last year's stock plus this year's net investment).

We distinguish between the following variables:

x^i = economic variables that are not targets (irrelevant variables); total: I;

y^j = target variables; total J;

z^k = instrument variables; i.e., data controlled by the policy maker; total K; and

u^l = data, not controlled by the policy maker; total L.

Tinbergen argues now as follows:

Given the z^k and u^l, we should be able to find the corresponding levels for the x^i and y^j. Indeed, when the number of equations equals I and J (and assuming no inconsistency nor dependence within the equation system for the selected z^k and u^l) the 'endogenous' variables can be derived from the model by simply inserting the values of the 'exogenous' variables. On the other hand, we might be interested in determining the necessary levels for the z^k variables in order to achieve certain desired values for the y^j. This 'inverted reduced form' (Theil 1956) can be obtained when J = K, or when the number of target variables equals the number of instruments (note that there are still N equations, and that there are always I unknowns because of the x^i). [28] [29]

It is obvious that the Tinbergen model may result in a set of alternate policy rules via policy simulation, each subject to a corresponding set of target values. (Dutch planners refer to this list as the 'train directory'.)

Such a program of alternatives may then be presented to the politicians who will make the most appropriate choice, given their (and the electorate's) preferences.

The Tinbergen approach to quantitative economic policy is now being used in many countries. Its applicability appears to be enormous for both planned and unplanned economies.

Yet, there are some weaknesses as acknowledged by Tinbergen himself (1956). One is the exclusion of uncertainty from this approach to economic decision making. A second (with ample liberty with respect to variables and levels) and, subsequently, to derive the corresponding levels for the instruments. Preferences become thus absolute and the only trade-offs allowed are those embodied in the model, and not in the preference function of the decision maker (compare the quote from Dorfman in Section 1.7). The method does not allow optimization in the normal sense.

Another deficiency is the absence of true dynamics. Predictions and decisions are made on any one occasion without reference to possible revisions at future dates. The Tinbergen model is basically a static approach to economic policy, even though dynamics (in the form of interrelationships between time-separated variables) may be embodied.

We will see later that Theil expands the Tinbergen model to alleviate these problems.

As do all empirical policy models, working with the Tinbergen version requires a great deal of numerical information. First, the model has to be estimated for which we can use only imperfectly measured data. Secondly, we will derive stochastic (hence, uncertain) estimates of the true, but unknown parameters. Thirdly, whatever variables are taken as exogenous, perfect forecast of their future behaviour is impossible. And fourthly, we cannot predict the value of random disturbances occuring in the structural equations during the prediction period.

By the same token, however, the model provides us with some indications as to where to focus our information-research. Solving the equation system analytically, we obtain the reduced form with the endogenous variables expressed in terms of the exogenous ones. By varying the values of the exogenous forecasts, we can test the outcome for their sensitivity for measurement errors in the independent variables. Similarly, we may vary the coefficients in the structural equations and investigate the impact on the coefficients in the reduced form and, hence, on the predicted values of the dependent variables, given exogenous predictions.

Morgenstern (1963) applied this method to the "Klein I Model" (Klein 1950). The results are quite startling. Frequently, 'error elasticities' (% change in the predicted value of a dependent variable divided by the associated % change in a coefficient) are substantially above I for the cases investigated. For example, if the Marginal Propensity to Consume (for wage earners) is increased by 5% (from .80 to .84), predicted Consumption, Profit Income, and Private Sector Wage Sum increase by 4.4%, 10.9% and 3.1% respectively. Having carried out these experiments for changes by 5% in all coefficients (one at a time), Morgenstern reports the following maxima and minima of percentage changes in the predicted dependent variables:

Consumption:	8.6% and - 7.0%;
Profit Income:	19.5% and -18.8%;
Private Sector Wage Sum:	11.3% and - 9.1%.

We referred earlier (Ch. 1) to the paper by Denton and Oksanen (1972) in the field of data revision. They used both sets of data to estimate a simple econometric model for the countries involved, and compared the results. We quote from their conclusions:

"There was no evident tendency for the effects of data revisions on slope-coefficient estimates to be in one direction or the other; increases and decreases were observed in about equal proportion, whether one considers the actual or absolute values of the coefficients. However, in the case of intercept terms, there was an unexplained tendency toward increases in absolute size."

"There was no evident tendency for the effects of data revisions on slope-coefficient estimates to be in one direction or the other; increases and decreases were observed in about equal proportion, whether one considers the actual or absolute values of the coefficients. However, in the case of intercept terms, there was an unexplained tendency toward increases in absolute size."

"There was a strong tendency for data revisions to alter the slope-coefficient estimates within a given equation in different directions."

"Coefficients changed sign in only a very small proportion of cases as a result of data revisions."

"There was a rather modest tendency for data revisions to bring about improvements in the statistical significance of coefficient estimates, as represented by increases in 't-ratios'."

"There was no evident tendency for data revisions to bring about improvements in the overall closeness of fit of equations; in fact, there appeared to be a tendency in the opposite direction."

It appears on first sight that the results of data revisions - a rather extensive and expensive program - have been minimal. We cannot judge their usefulness, though, if there is no measure for the quality of policies based on the various models. Similarly, we are unable to indicate which revisions have been most successful in terms of 'social welfare'.

Within the Tinbergen model, there is no systematic approach to solve the problem of which coefficient or exogenous forecast should be improved first, second, etc., or simultaneously, by what relative amounts of money. In fact, the absence of an explicit social welfare function in the Tinbergen model is reflected in the inability of the model to indicate the marginal usefulness of additional data. If it is impossible to formulate an economic-policy optimizing function, we cannot expect this to be feasible for an information-efficiency function. A second-best approach would be the incorporation of data-gathering activities within the model, although that would probably require the specification of confidence and prediction intervals, the width of which would vary negatively with the amount of research money. Also to be considered is the use of multiplicative reliability coefficients (with ideal values equalling 1). Based on results of computer-run simulations for a number of data-quality measures, ad hoc comparison of cost and benefits (in terms of changes in the values of certain variables) could take place. Such an intuitive line of action is quite possible without any formalization of decision or information production processes.

6.3 Optimization and 'True' Dynamics

Up to now, the formulation of an optimum decision either took place within a 'static' model (simultaneous equations; linear, nonlinear and stochastic programming techniques) or within a dynamic model composed of a sequence of such techniques. In either case, the optimal decision was solved for one period of time and no account of the effects pertaining to a period of time was necessarily taken. For the 'static' model, this is permissible, as the problems incorporating these types were simultaneous in nature, as well as independent of past decision. With regard to dynamic programming models, we find that there must be an examination of the effects of decisions as we travel the line of time.

In the past, efforts at controlling economic forces have been manifest as pushes in the right directions. Recent improvements in the sophistication and accuracy of economic models have allowed more advanced efforts to 'fine-tune' the economy. Cumulative effects of past policy decisions appear as major factors determining the developments taking place in the future. That is, as we go into greater details, the independence assumption regarding sequential decisions begins to falter, and we must account for these. Such is particularly important in cases where apparently decisions required to offset the impact of past decisions may require greater and greater changes in our 'optimal' plan, until the model explodes. A situation characterized by this is said to have 'instrument instability'. The word 'instrument' refers to those decision variables which the policy maker has under control. In sum, the policies that were optimal in the short run of time may turn out to be unstable in the longer run.

Analysis of dynamic models reveals that, through the use of the same techniques incorporated within the modelling techniques (specifically the theory of expected value), we can find a relation and evaluation procedure for the choice between a policy that is flexible (short run optimization) and one that is inflexible (a long run optimization procedure). The decision rests on the relative values of the variance of the estimated policy or decision variable and the covariance between this estimate of the variable and the variable itself (compare Tisdell 1971 and Holbrook 1973). There is some suggestion in the literature that the longer run policies are generally optimal; the flexible or shorter run or fine-tuning models would then be suboptimal.

Even if we are working with models that are not so finely detailed (so that this problem may not be relevant), we should realize that, as we travel from decision state to decision state in a dynamic model, we accumulate the actualization of values for variables that were previously estimated. This is a source of knowledge that has been left out and should be incorporated. This is important to us if we are working with a relatively short period of time (so that the statistical averaging procedure of expectations is not accurate), and if the structures that we assume to be rigid over time (possibly because of lack of mathematical techniques for handling variable structures) are, in fact, changing.

6.4 The Theil Model

As stated above, the Theil model (1964; see bibliography there for numerous other publications by Theil and his research associates) attempts to improve Tinbergen's approach by adding several features. We will review the model briefly. [30]

1. The model is made dynamic in the sense that forecasts and policy implications for several years are incorporated.

2. A preference function is postulated, consisting of the sum of weighted squared deviations of actual values from desired values for relevant variables. Preferences are thus introduced twice, viz., via the selection of desired values and via the choice of weights (measuring trade-offs between squared deviations from optimal patterns).

3. The preference function is minimized (or maximized after the introduction of a minus-sign) subject to the constraints of the model (a la Tinbergen). This optimization takes the form of a strategy; i.e., a decision rule specifying the best decision at the beginning of each year, given the information available at that moment, and incorporating the fact that, at future decision dates, more (and more exact) information will be available.

There is a very important aspect to the dynamic feature of the model. The government will calculate tentative forecasts and decisions for a number of years, given its preference function and available exogenous information. But every successive year, more information becomes available and the policy maker can revise his forecasts and his decisions. Apparently, this approach combines the simplicity of one-year-ahead forecasting with the need for long-term planning. As such, it is a very appropriate model for indicative planning with various feedbacks incorporated. Moreover, it enables the planner to be more 'risky'. Possible errors in policy judgement are reversible in the future.

Theil goes into many details regarding the various levels of uncertainty in the model:

> the constant terms of the model (which include the stochastic disturbances for the prediction periods), the multiplicative coefficients of the model, the coefficients of the linear and the quadratic parts of the preference function. In fact, for each and every element of uncertainty, a 'loss function' can be derived, expressing the decrease in the value of the preference function from its maximum because of faulty information.

From the viewpoint of information policies, the Theil model is very useful. Since 'welfare losses' due to imperfect specification and information can be calculated, these values indicate to an information-gathering institution where and how much services should be improved. Obviously, the introduction of an explicit objective function not only allows the economist to find the strategy maximizing the policy-maker's preferences, but also to gain insight as to where the most beneficial measurement improvements may be applied.

Reflecting briefly on the applicability of this model for resource management policies, we must say that its application could be beneficial. Too many debates re the development of national energy sources, etc., take the form of now, in 20 years, or never. Careful manipulation with Theil's model might enable the decision maker to indulge in a piecemeal approach. Of course, such a method becomes less attractive whenever irreversible actions are involved.

6.5 Some Reflections on Theil's Model

This section will be devoted to some criticisms and extensions of Theil's model. We do not claim to be complete, but merely to make some interesting observations.

As was to be expected, a major point of debate has been the quadratic form of the social welfare function. Indeed, the symmetry implies that a deviation from the desired target in downward direction weighs as heavily as a deviation upward. We may question this procedure in cases like (un)employment and rate of inflation. Theil anticipates this criticism in his major work on the topic (1964):

> "These questions as such should, of course, receive an affirmative answer; but the point is that they are not relevant. A quadratic social preference function can, in general, claim no validity for arbitrarily large variations of its arguments and the specifications have been made, partly at least, on the basis of an idea of the actual range of variation that may reasonably be expected. This implies that our preference function does not measure 'pure preferences': it approximates these in the relevant range, and what is the relevant range is determined by the decision made. Now the decision made is the one which maximizes the approximating preference function or its expectation, subject to the constraints. Hence, our quadratic preference function is indeed 'impure' because it contains elements of the constraints. Also, the fact that the decision made and, hence, the relevant range of variation of our variables depends on the preference function which implies a certain amount of circularity in the argument; the preference function chosen determines the relevant range, and this range determines the way in which this function is approximated. As long as the quadratic approximation is not very sensitive to changes in this range, this is not serious; but it shows that the procedure followed is partly an art, partly a science."

This statement meets the subsequent critique by Fromm and Taubman (1968) that targets should be selected so as to lie on the 'production possibilities frontier'. They also suggest that interaction terms (the product of deviations from their respective targets of two variables) should be included. This is anticipated by Theil, indeed; the desire and (possibly) need for simplicity induce him to delete these terms, though.

The quality of the constraints plays an important role in the validity of the optimal decision. But, as Theil writes:

> "The statistical theory which underlies the estimation of the coefficients of a model proceeds under the assumption that the form of the equations is specified correctly; but it is, in general, unknown whether a specification is correct or not, and the usual procedure is to experiment with many alternative specifications and to choose the one which seems most satisfactory on the basis of some more or less well-defined criterion. It will be clear that it makes no sense to devote considerable resources to a refined specification of the preference function as long as the specification of the constraints is in bad shape."

The latter argument cannot be valued too highly. Theil's model does not just indicate the optimal policy to be followed. It also indicates to the researcher, whose task it is to provide the decision maker with the proper information, where improvements in the system can be applied optimally (in the sense of a higher -- or less subject to a 'welfare loss' -- preference function).

We have emphasized that sophisticated models are, ideally, to be preferred to rules-of-thumb, and as Ball (1963) states it, "armchair attitudes". The crucial question is whether better decisions can be reached along alternative routes. As Theil writes in the same context as above:

> "By now, perhaps, the reader's difficulties will have increased rather than decreased. What to say about a method of making decisions whose two cornerstones -- preference function and constraints -- compete in the poorness of their quality? Should we have any illusions about the quality of such a decision? Again, these questions are sound as such, but not completely relevant for two reasons. For one thing, it is not unreasonable to compare the quality of this decision with the way in which similar decisions are made in practice; and it is not unreasonable either to expect that it should be

possible to obtain better results. For another, the theory developed here enables us to compute the losses associated with erroneous specifications of the preference function and the constraints, and such losses form a direct indication of the way in which research efforts can be spent usefully. Particularly also because of the first-order certainty equivalence result ... which suggests that errors in the multiplicative structure of the constraints should be considered more serious than errors in the coefficients of the preference function."

Several authors have expanded the Theil model. Van de Panne (1965) makes the following observation:

"This (i.e., Theil's: JIV) method is computationally efficient in one particular case. If the coefficient vectors and matrices of the constraints have a regular structure, which means that corresponding coefficients for different periods are the same, apart from initial and final effects, and if the number of periods taken into account is infinite or very large, the optimal decisions for the period immediately ahead are always the same linear functions of the mathematical expectations of unknown variables for the periods ahead and of variables of past periods. The paint factory is a good example of this. Optimal work force and production decisions for period t are linear functions, independent of t, of unbiased sales forecasts for the periods t + 1, t + 2, ..., and of inventory at the end of period t = 1. In the general case, however, this is not valid; if the number of periods taken into account does not approach infinity, or if the structure of coefficient vectors and matrices is not regular, or if both, the whole problem must be reformulated in order to determine optimal decisions for a later period."

He then attempts to adjust Theil's model for this deficiency. We quote his summary:

"In this paper, an expression is developed which gives explicitly the decisions for each period of the optimal strategy. This expression is useful in all cases when the problem does not have a regular structure and an infinite horizon. The explicit formulation of the decisions of the optimal strategy makes it possible to derive another formulation which relates the optimal decisions for a certain period to decision errors made in previous periods. More precisely, an expression is derived which states that the optimal decisions for

> period t, given the information available at the start of that period and given the decisions for previous periods, are equal to the optimal decisions for period t if all previous decisions had been optimal plus a linear combination of decision errors made in previous periods as perceived at the start of period t. A decision error for a period t', t' < t, is defined as the difference between the decisions for period t' based on the information available at the start of period t', given the decisions for periods prior to t', and the decisions for period t' which would have been optimal given the information available at the start of period t and given the decisions for periods prior to t'. The resulting formulation may be called a feedback rule, since it relates differences of decisions from the vector of over-all optimal decisions to past decision errors. This relationship is a linear one; the matrices of feedback coefficients are built up from submatrices of the so-called substitution matrix of the instruments, and are, therefore, easy to derive."

In an attempt to cut down on data requirements of Theil's model, Kunstman (1971) analyzes the possibilities of truncation of long-term decision models, using an objective function which is linear in the target variables only, and quadratic in the instruments only. Moreover, the approach is deterministic rather than probabilistic. Truncation is performed on the objective function through the replacement of part of the objective function by linear terms, linking the omitted terms with the remaining part of the model. He shows that such a procedure, which makes the model more manageable and more understandable for the non-economist policy maker, leads approximately to the same first-period decision as the nontruncated model. Moreover, the results appeared to be quite insensitive to changes in the coefficients in the objective function and in the applied data. Kunstman's results would indicate that complicated models may well be turned into 'rules-of-thumb' by means of this method of truncation.

Wennekers and Harkema [31] are reported to have tried to deal with the additional information about the parameters in the constraint model, as it became available during each decision period. If the acquisition and incorporation of such information could conceptually be embodied, another strategy element might be added to the Theilian decision rules. Unfortunately, they ran into insoluble computational problems, except for the case of two-period decision problem with a loss function linear in the target variables.

The Theil - Van de Panne model comes close to the process of 'adaptive decision making', whereby choices are being made using (and anticipating) cumulative experience of 'doing while learning' (see Murphy 1965). This is not the place to describe these models in detail. We will restrict ourselves to the following listing from Murphy who concludes from his comparison of social and biological processes:

"First, in taking an action, we see that, at any point in time, an adaptive system can move off in any one of many directions.

Second, we find that a move in any of these directions will cause a change to the system and perhaps its environment. This change becomes part of a record, the historical record of the system which is unchangeable.

Third, there is, in each adaptive process, a choice or decision-making function. With knowledge of the historical record and an evaluation of the effect of each action on the present and future states of the system, the decision-making function chooses one of the alternative actions.

Fourth, following the taking of an action, the system may or may not achieve the anticipated result. Uncertainties in the environment or the effect of the action may cause the system to move in a direction which was unintended.

Fifth, if the result was unfortunate, there is no recourse. The only corrective measure is to re-establish the above sequence of events all over again.

Certain fundamental notions will be required to formulate adaptive processes.

1) A model of an adaptive process must be a sequential time process since uncertainties on the environment make success difficult to obtain and hold.

2) We must find a way of treating the information contained in the historical record. Thus, a concept of information will be an important notion.

3) We must have a stochastic representation of the effects of the system's environment which, as we see above, plays a vital role.

4) The role of the decision-making function must be made explicit. [32]

5) We note that only one action is to be selected out of many possible alternatives. How this one action is selected is a fundamental notion with which we must deal.

6) We must specify the ways in which the structure of the system is changed by each of the possible actions and the possible effects of the environment.

A mathematical model is the best way to provide answers to these fundamental requirements for the study of adaptive processes."

It is obvious that these types of models can be used quite effectively to reduce -- through the concept of 'doing while learning' and vice versa -- the amount of information needed in the early stage of model development and decision making.

CHAPTER 7

CONCLUDING REMARKS

We suggest that the large expenditures connected with model building make reflection on their social usefulness necessary. Economies can obviously be reached through careful selection of a model, incorporating its implicit need for information, and comparing costs with benefits.

On the other hand, some fundamental problems remain unresolved pertaining to the multiple - indeed limitless - applicability of gathered data. The best we can suggest is the estimation of truncated and/or discounted future benefits which can potentially be derived from information, initially gathered for some specific purpose. Factors to be considered in the process are concerned with generality of the observations with respect to, e.g., time and location. And to make our conceptual problems even more complex: the efficiency argument should be applied to this estimation of future applicability as well.

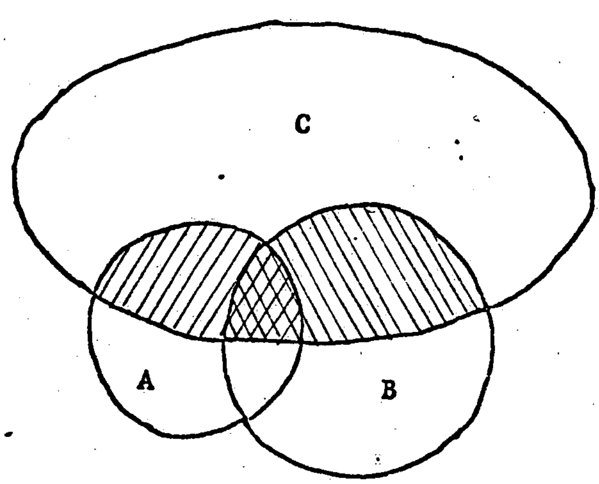
Yet, it is better to conclude that the gained insight is incomplete, rather than overlooking the problem of information efficiency completely - as is often done.

FOOTNOTES

[1] There are estimates that this stock doubles every 7 - 10 years.

[2] See also Griliches (1970).

[3] "It is desirable to set forth systematically the relationship of such terms as 'observation', 'statistics', 'evidence', etc. Our use may not find general acceptance, but, in order to achieve precision, clarity of the terminology is essential. The ordering of the concepts is illustrated in Fig. 3. A is the body of data consisting of gathered (numerical) statistics; B represents other data, such as historical events or (now) non-measurable data; e.g., 'expectations'; C is the theory based partly on A and B, as well as on deductively obtained facts (perhaps not accessible to direct observations; this class may, as yet, be empty in economics). The intersection of A and C, and B and C, and, if it exists, of A and B and C, is called scientific information which is thus made up of quantitative observation (i.e., the intersection of A and C) and description (i.e., the intersection of B and C). Data are thus a much narrower concept than scientific information; the latter alone is related to theory. Data became scientific information only through this connection. Otherwise, they are nothing more than possible building stones for theory. Should they ever be used, they assume a state characterized by the relation into which they enter with other data and their explanation by the theory. Most economic quantitative (statistical) data are of the class A minus C type; i.e., they are represented by the non-shaded area of A in Fig. 3. While they may illuminate something, they do not do this automatically. These data, as such, tell no story, or they tell many different and conflicting stories simultaneously; either condition is equivalent to the lack of a theory.



(Figure 3. Data and Scientific Information)

[4] "For purposes of easy reference, let us call these two kinds of information, respectively, information as to the nature of the system and information as to the state of the system. By information as to the state of the system, let us mean the statistical measurements which reveal the current situation of the nation or economy, such as population data, price indices, and measures of the level of business activity. By information as to the nature of the system, let us mean the basic conceptual model or understanding which serves as a guide to tell what dimensions of the nation, or society, or economy should be measured and how these measurements should be interpreted in making decisions. This information as to the nature of the system includes, of course, both the conceptualizations themselves and the extensive, quantitative measurements which are required for valid conceptualizations."

"It is the job of statisticians, in cooperation with other scientists, to provide the two kinds of information needed for effective self-guidance. In the United States, and I suspect also in Canada, our attempts to provide both kinds of information have not been equally successful. We are doing a far better job of collecting information about the state of our nation than we are of obtaining data dealing with the nature of our nation and developing valid generalizations and theories based on these data."

"This deficiency would be serious enough if the nature of our nation were static or changing slowly, but the evidence suggests that this is not the case. Our society is growing more complex at an accelerating rate. This makes it even more important that we take the necessary steps to achieve and maintain fairly continuous understanding of the basic nature of the nation. As statisticians, I believe we have a major responsibility, along with our scientific colleagues, to learn much more about our society's fundamental nature. Without valid understanding and correct conceptualization of the nature of our society, we cannot know what measurements to obtain nor how to interpret these statistics correctly."

We will meet the distinction between state and nature later in this paper when we are dealing with the degree of sophistication of economic models.

[5] "Thus, since there exists no integrated system of theory in any one area, be it physics or economics, there are simultaneously several degrees of acceptable levels of precision of measurement. In physics, it is possible, for example, on the one hand, to have a measurement of the 'Ritchie constant' with an accuracy of 10^{-14}, but on the other, to know the age of the earth and of the universe only approximately, with a deviation of several billion years, and to make acceptable cross-section measurements in nuclear physics with only a 50% (!) accuracy. In the overall, a measurement in physics with 10% accuracy is a very good measurement. The reader should compare this with the alleged ability of the economist to measure changes in national income, consumers' spendable income, price levels, imports, etc., to an overall accuracy of ten to one hundred times better -- even in the average."

"Newton established the law of gravity and verified it with an accuracy of about 4 percent (it was later proved to be accurate to about 1/10,000 of one percent).

[E. P. Wigner, 'The Unreasonable Effectiveness of Mathematics in the Natural Sciences', Communications on Pure and Applied Mathematics, Vol. XIII (1960), p. 8.

This brilliant paper might well be studied by anyone concerned with clarifying in his mind the role of theory formation, and to see in particular how 'false theories ... give, in view of their falseness, alarmingly accurate descriptions of groups of phenomena'.]

We conclude from such shining examples in the history of physics that the economist need not despair that good, workable theory is impossible unless the data are of the presently alleged high accuracy which even the physicists now do not enjoy in general. But the theory will look different from what is now being advanced."

"Theories can be classified according to the requirements they make upon the data with which they are concerned; some are demanding, some are not. But, in each case, a clear evaluation of the situation must be made since, otherwise, meaningless associations are established."

"For example, while it might be desirable to have simple models of the economic world, there is an anachronism of proposing naive Keynesian models of only a few variables, yet to insist that the data to which they are to be applied -- aggregations of vast scope -- be known to hundreds of one percent accuracy when, in fact, the second digit is already in doubt. This will change, of course. In less time than a generation, one will wonder how such proposals could ever have been taken at face value."

[6] Canadian economists and policy makers still have to work with the 1961 Input-Output Table. Given the structural changes in the Canadian economy, decisions based on these outdated figures may well be off-target.

[7] Zellner (1958):

"Provisional quarterly estimates of 16 items in the national accounts have been compared with the most thoroughly revised estimates currently available. The comparisons made in this study reveal that provisional estimates of items estimated by the residual method and of items whose estimation rests heavily on infrequently available benchmark data tend to be characterized by larger percentage errors in the level and amount of change than are the provisional estimates of other items considered. Perhaps in the future the situation with respect to these items will be improved since G. Jaszi has written:

'Impressive progress has been made with the sampling surveys which are used in the annual, quarterly, and monthly extrapolation of the benchmark figures. Larger areas of the economy are covered by such surveys than ever before and they make it possible for us to prepare estimates on an up-to-date basis.'

Also, it is possible that the improved methods to which Jaszi refers may correct the consistent biases which were discovered in the provisional estimates of personal consumption expenditures, personal consumption expenditures on services and nondurables, and new construction expenditures for the period 1947 II - 1955 IV.

While errors in showing direction of quarterly change occurred frequently for some items (see Table 62), 8 of the 16 items considered showed 5 or fewer errors in 34 quarterly changes. This good showing, however, must be tempered by the fact that a sizable proportion of the errors in direction of change occurred in the neighborhood of cyclical turning points. For example, whereas provisional estimates of GNP disagreed with revised estimates on direction of change in only 5 cases, these occurred at the lower turning points of the 1948-1949 and 1953-1954 recessions."

Stekler (1967):

"This paper has examined an issue which is important to all users of the data in the National Income accounts: Do the earliest published indications of changes in GNP and its components provide information which is useful in the interpretation of economic trends? Or are those figures so inaccurate, as Morgenstern has suggested, that subsequent revisions reveal that the true movements were radically different from those which were initially reported?

Our analysis has revealed that:

(1) While the estimated quarterly changes in GNP (and components) as measured by the provisional data differ from the final figures, the estimates have improved over time. Moreover, the small number of turning point errors in the overall GNP provisional data and the relatively small quantitative errors indicate that the early data present an approximation of the true pattern of economic movements.

(2) The advance figures, which are released one month earlier than the provisional data, have somewhat larger errors than the later figures,

but they do provide useful predictive information. Moreover, a large percentage of the revisions which occur between the release of the advance and provisional data are unsuccessful, thus indicating that little advantage is to be gained in interpreting economic changes by waiting for the later data.

(3) An analysis of the temporal discrepancies in the year to year changes in the National Income Series indicates that the pattern of data revisions is such that the data provide meaningful information (and not a wide degree of uncertainty, as Morgenstern has suggested) about the movements which actually occurred. There was little difference between the maximum and minimum reported changes and the actual movements.

(4) This combined evidence indicates that the earliest published indications of changes in GNP, while likely to be revised, are sufficiently accurate approximations of the actual movements to be useful for economic analysis."

Stekler (1970):

"(1) When the earliest data are compared, the second month's figures generally correspond more closely than the first month's to the third month's data; here the trade-off is significant.

(2) In the best series, however, the errors associated with the early data are 50 per cent of the changes in the final data. This error may be as high at 100 per cent for some of the poorer statistics. Moreover, the direction of movement indicated by the early and final sets of data differ in 20-50 per cent of the months.

(3) Among the early data, the errors in the best series were 30 per cent of the change, and turning point errors occurred in 7-15 per cent of the months. Other series had substantially larger errors. The errors in these underlying

data are translated into the contemporaneous GNP accounts."

H. Glejser and A. Dramais:

"a) Whereas clear differences exist in the distribution of revisions, as between developed and underdeveloped countries, high or low rates of growth seem to be equally well (or badly) estimated at all stages.

b) It could not be detected that the availability of census data brought about more radical revisions than those of routine and plain crash-attack on previous estimates.

c) The first estimate of the GNP rate of change contains a systematic negative bias in a great number of countries. This may be due to various reasons, ranging from conservatism in the absence of definitive information about some GNP components to the fact that an upward revision is politically more comfortable than a downward one.

d) As far as the test used is trustworthy, an improvement in quality over successive estimates (reckoning from the third) seems to be present, but to a relatively small extent.

e) The magnitude of the revisions did not vary linearly over the period considered.

f) He who wants to forecast the 'last' (i.e., fifth) revision of the GNP figure for one given year t has to wait - whatever the predictor chosen - until year t + 3 to obtain a reduction in the mean square prediction error higher than 60% with respect to the naive assumption of a zero-revision."

Denton & Oksanen (1972):

"1. National accounts data revisions in the 21 countries studied were highly variable. In the case of GNP and some of its major components, including consumption and investment, the revisions were predominantly in an upward direction. The tendency toward upward revision accords with the findings of other studies.

2. Revisions were found to be highly correlated between variables, in some important cases, thus casting doubt on the advisability of assuming measurement errors in economic time series to be independently distributed.

Theil (1965):

"The analysis deals with (largely) the same variables and the same period as those of the two preceding chapters, but it focuses on series of seven successive forecasts and estimates of the values taken by each variable in each year. Two of these seven are made before the beginning of the year ('true forecasts'), two during the year, and three after the end of the year ('true estimates' of past realizations). The last of these estimates is taken as the correct or 'true' value. The main conclusions are the following:

(1) The frequency of turning point errors of the first forecasts ('stage 1'), which are made in September of the preceding year, is about 20 per cent. This is reduced by about one third in stage 2, which is the set of forecasts made in December of the preceding year. It is further reduced in stage 3 (September of the year itself) and also in stage 4 (December), after which the frequency of turning point errors remains roughly constant at a level of about 3 per cent in stage 5 (September of the next year) and stage 6 (September of the year thereafter).

(2) The bias towards underestimation of changes is considerable for stage 1, the number of underestimation cases exceeding that of the overestimation cases by a factor 3. This bias decreases gradually in the later stages and can be regarded as no longer existing in the last two.

(3) The RMS (Root Mean Square; JIV) errors decrease gradually from stage 1 onward until their level in stage 6 is of the order of one third of their stage 1 level. However, most of this decline of uncertainty takes place in

the first four stages (which are all forecasts and estimates made before the end of the year).

(4) A revision is defined as the predicted or estimated log-change of some stage minus the corresponding value of the preceding stage. It turns out that the majority of these revisions were successful in the sense that they reduced the error in absolute value. Nevertheless, they could be improved by applying simple linear corrections. The forecasts of stages 2 and 3 would generally have a smaller error if about 1 per cent is added. The estimates of stages 5 and 6 would be improved on the average if only half of their implied revision is accepted. The former result is primarily due to the bias towards underestimation of changes, the latter to the fact that a majority of the revisions is either in the wrong direction or in the right direction but too large."

[8] It appears that a smart investigator will be able to deduce market shares from the product of the two matrices involved; i.e., the market share matrix and the technology matrix.

[9] Of course, whenever a relatively large area is involved, nothing should prohibit Statistics Canada from making micro data directly available to the researcher. Crucial is whether or not the original supplier of information can be identified. See also Holt (1971).

[10] Stigler's 1961 article has been followed up by a number of interesting studies. See, e.g., Stigler (1962), Rees (1966), Nelson (1970), Alchian (1970), Alcaly (1971), and Hirshleifer (1971).

Of a more abstract nature are a series of papers dealing with the role of information in general equilibrium problems in economics. See, e.g., Martin (1964), Davis & Whinston (1966), Jenner (1966), Neuberger (1966), Hurwicz (1969), and Albin (1971). Interesting (and of increasing degree of difficulty) are articles by Marschak (1954, 1966, 1968, 1969, 1971, 1972.)

Another context in which the expression 'information' is currently being used is that of 'information theory'. The main publication re economics is Theil (1967). Unfortunately, this branch deals primarily with the reduction in uncertainty by means of messages and not with the content and usefulness of the messages themselves. Although we cannot exclude the possibility of successful integration of information theory into economic decision processes, we have not been able to discover or develop such procedures.

[11] Theil (1963) developed a similar model for the optimal amount of research. In fact, he uses the same model for macro-economic decision-making; see chapter 6.

[12] It is interesting to note that these results were based on only two assumptions, i.e., the existence of the population mean M and of the population variance V^2.

Mandelbrot (1963, 1967) has shown that some economic phenomena can be characterized by distributions without a finite variance. This implies that classical statistical methods are not applicable. Granger and Orr (1972) give the following suggestions for handling such cases:

"1. Transform the data to remove the problem of long tails;

2. Discard current techniques based on least-squares criteria, which include nearly all current methods, and attempt to derive new nonparametric techniques; or

3. Continue to use classical methods and either hope, or show, that those methods are sufficiently robust (insensitive to the form of the distribution; JIV) so that they give meaningful results even for data for which the possibility of infinite variance must be seriously acknowledged."

[13] We assume that only unbiased estimators are considered. This is not strictly necessary. We might well prefer a slightly biased estimator, the distribution of which is very closely concentrated

around its mean, to an unbiased one with thick tails, making any outcome highly unreliable.

[14] From a study by Denton & Kuiper (1965), it may be concluded that the choice of estimation procedure has less influence on the parameter estimates than revision of data. Although it does not concern here a problem of either/or, such a statement may help the researcher determine his line of action.

[15] There is a conceptual problem in this approach, which may be illustrated as follows:

For the least-squares case, the objective function takes the value $1/\Sigma(X_i - \bar{X})^2$ in the optimum, i.e., this is the value of the objective function after the optimizing expressions of the argument variables -- here the coefficients A and B -- have been plugged in. The X_i values which lie farthest away from X have the greatest marginal influence on this optimum. Once the sample has been observed, however, these X_i values are considered fixed and measured without error. If these assumptions are correct, no improved measurement of X_i will result in different values for the optimum. If the latter does happen (which is quite likely), the nonstochastic assumption about the X_i - s is obviously inaccurate; they are subject to measurement errors after all.

[16] See Wonnacott and Wonnacott (1970) for the possibility of testing for optimal specification within the framework of the k-class estimators.

[17] This is often interpreted as a decision to consider the parameter as a stochastic variable rather than as an (unknown) constant. Such an approach is not strictly necessary; the true value may well be accepted as being fixed. The prior density just specifies the environment in which we think the parameter truly lies.

[18] In cases where the posterior d.f. is "normal", the mode, median, and mean coincide.

[19] For some reflections on Bayesian versus non-Bayesian methods in statistics, see Zellner, Rothenberg, and various discussants in Intriligator (1971).

[20] In terms of entropy -- see Goldman (1953).

[21] See Winkler (1967) for problems of assessment of prior distributions in Bayesian analysis.

[22] See Wonnacott and Wonnacott (1969).

[23] Sometimes stopping rules are "non-informative"; i.e., the Bayesian inferences do not depend on whether or not the stopping probability is introduced explicitly into the statistical analysis. See Roberts (1967) for an investigation of some examples.

[24] Müller and Pelupessy (1971) have applied Leontief's approach to air pollution problems along the final 30 mile stretch of the Rhine, west from Rotterdam. See Netherlands Central Planning Bureau (1972) for an extremely refined and useful analysis of water pollution.

[25] See, e.g., Tybout (1970).

[26] See Chow and Meredeth (1969):

"The major underlying assumptions of linear programming that limit its applicability are:

(1) that all mathematical functions be linear functions and this, in turn, assumes that the

measure of effectiveness and resource usage must be proportional to the level of each activity conducted individually;

(2) that the total measure of effectiveness and each total resource usage resulting from the joint performance of the activities must equal the respective sums of these quantities resulting from each activity being conducted individually;

(3) that it is permissible for an optimal solution to contain fractional levels of resource usage; and

(4) that all of the coefficients in the linear programming model are known constants.

[27] See, e.g., Oerlemans, 1972, et. al. , for some further experiments.

[28] Aberg (1967) makes the interesting observation that, by this procedure, Tinbergen makes the target values exogenous and the policy variables endogenous. This has important consequences for the statistical estimation procedure of such a model if historical observations are being used generated by this procedure.

He then suggests to use the policy goals as exogenous variables in the estimation procedure. Unfortunately, this can be done only if these goals are explicitly given. In reality, the actual outcome of the target variable is measured, rather than the intended value. We suggest that this problem can be solved (within the context of the Tinbergen approach) by expanding the model with a set of equations describing the planning portion of the economic policy formulation.

[29] We will disregard the problem of upper and lower limits on certain variables (e.g., employment cannot exceed the available labour force; prices cannot be negative).

[30] See for a very lucid description Theil (1965).

[31] Econometric Institute, Netherlands School of Economics. No final paper available as yet.

[32] Murphy's text reads 'explicitly'.

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