FROM UTOPIAN THEORY TO PRACTICAL APPLICATIONS: THE CASE OF ECONOMETRICS

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INTRODUCTION

In this essay on econometrics in its conception and its use in economic planning for the betterment of man's fate, I will try to cover a very broad field.

When talking about the methodology in the particular fields mentioned - about which I am supposed to have a little more than second hand knowledge - I have always found it *utterly inadequate* to focus attention only on these special fields without seeing them in a much broader perspective.

Therefore it was inevitable that I should have to include in the field of vision of this paper also some branches of science where I can only speak as a layman, hopefully as a somewhat informed layman. For whatever blunders I may have made in these fields I must ask for the reader's forgiveness.

So this paper will include by way of introduction some reflections on human intelligence and wisdom (two very different things), and on the nature of natural laws, including some general reflections of a "Kritik der reinen Vernunft"-sort.

I shall try to present my remarks as far as possible without technicalities and mathematical details, because I want to reach the general reader. This I will do even at the risk of presenting some material which may seem trivial to some of my advanced colleagues.

The subsequent sections will make it more tangible what I mean by the above general formulations. At this stage let me only mention a striking manifestation of the difference between intelligence and wisdom: The case of *Evariste Galois* (1811-1832). He was one of the greatest mathematical geniuses that ever lived. His theory of transformation groups laid for instance completely bare the nature of roots of algebraic equations. This is a striking example of supreme intelligence.

But the case of Galois is also a striking example of lack of wisdom. In a clash with political opponents, where also a girl was involved, in his own words "an infamous prostitute" (1) he accepted a duel with pistols. He was not a good shotsman and knew for certain that he would be killed in the duel. Therfore, he spent the night before the duel in writing down at a desperate speed his mathematical testament. Here we find a brilliant expose of his main mathematical ideas. The next day he was shot, and died the following day at the age of 21.

1. The Lures of Unsolvable Problems

Deep in the human nature there is an almost irresistible tendency to concentrate physical and mental energy on attempts at solving problems that *seem to be unsolvable.* Indeed, for some kinds of active people only the seemingly unsolvable problems can arouse their interest. Other problems, those which can reasonably be expected to yield a solution by applying some time, energy and money, do not seem to interest them. A whole range of examples illustrating this deep trait of human nature can be mentioned.

The mountain climber. The advanced mountain climber is not interested in fairly accessible peaks or fairly accessible routes to peaks. He becomes enthusiastic only in the case of peaks and routes that have up to now not been conquered.

The Alchemists spent all their time and energy on mixing various kinds of matter in special ways in the hope of producing new kinds of matter. To produce gold was their main concern. Actually they were on the right track in principle, but the technology of their time was not advanced far enough to assure a success.

The alluring symmetry problem in particle physics. Around 1900, when the theory of the atom emerged, the situation was to begin with relatively simple. There were two elementary particles in the picture: The heavy and positively charged PROTON and the light and negatively charged ELECTRON. Subsequently one also had the NEUTRON, the uncharged counterpart of the proton. A normal hydrogen atom, for instance, had a nucleus consisting of one proton, around which circulated (at a distance of $0.5. \ 10^{-18}$ cm) one electron. Here the total electric charge will be equal to 0. A heavy hydrogen atom (deuterium) had a nucleus consisting of one proton and one neutron around which circulated one electron. And similarly for the more complicated atoms.

This simple picture gave rise to an alluring and highly absorbing problem. The proton was positive and the electron negative. Did there exist a positively charged *counterpart* of the electron? And a negatively charged *counterpart* of the proton? More generally: Did there exist a *general symmetry* in the sense that to any positively charged particle there corresponds a negatively charged counterpart, and vice versa? Philosophically and mathematically and from the viewpoint of beauty this symmetry would be very satisfactory. But it seemed to be an *unsolvable problem* to know about this for certain. The unsolvability, however, in this case was only due to the inadequacy of the experimental technology of the time. In the end the symmetry was completely established even experimentally. The first step in this direction was made for the light particles (because here the radiation energy needed experimentally to produce the counterpart, although high, was not as high as in the case of the heavy particles). After the theory of Dirac, the positron, i.e. the positively charged counterpart of the electron, was produced in 1932. And subsequently in 1955 (in the big Berkeley accelerator) the antiproton was produced.

The final experimental victory of the symmetry principle is exemplified in the following small summary table

| | **Electric charge** | | |
	$+1$	0	- 1
Heavy Particles (mass around 1)	PROTON (Unit of mass = $1,67.10^{-24}$g)	NEUTRON	
		Antineutron	Antiproton (1955)
Light particles (mass around $\frac{1}{1840}$)	Positron (1932)	Neutrinos	ELECTRON

Note. Incidentally, a layman and statistician may not be quite satisfied with the terminology, because the "anti" concept is not used consistently in connection with the electric charge. Since the antiproton has the opposite charge of the proton, there is nothing to object to the term anti in this connection. The difference between the neutron and the antineutron, however, has nothing to do with the charge. Here it is only a question of a difference in spin (and other properties connected with the spin). Would it be more logical to reserve the terms *anti* and the corresponding *neutr* to differences in the electric charge, and use expressions like, for instance *counter* and the corresponding *equi* when the essence of the difference is a question of spin (and other properties connected with the spin)? One would then, for instance, speak of a counterneutron instead of an antineutron.

The population explosion in the world of elementary particles. As research progressed a great variety of new elementary particles came to be known. They were extremely short-lived (perhaps of the order of a microsecond or shorter), which explains that they had not been seen before. Today one is facing a variety of forms and relations in elementary particles which is seemingly as great as the macroscopic differences one could previously observe in forms and relations of pieces of matter at the time when one started to systematize things by considering the proton, the electron and the neutron. Professor Murray Gell-Mann, Nobel prize winner 1969, has made path-breaking work at this higher level of systematization. When will this drive for systematization result in the discovery of *something still smaller* than the elementary particles?

Matter and antimatter. Theoretically one may very precisely consider the existence of the "anti" form of, for instance, a normal hydrogen atom. This anti form would have a nucleus consisting of one antiproton around which circulated one positron. And similarly for all the more complicated atoms. This leads to the theoretical conception of *a whole world* of antimatter. In theory all this is possible. But to realize this in practice seems again a new and now really *unsolvable* problem. Indeed, wherever and whenever matter and antimatter would come in contact, an explosion would occur which would produce an amount of energy several hundred times that of a hydrogen bomb of the same weight. How could possibly antimatter be produced experimentally? And how could antimatter experimentally be kept apart from the normal matter that surrounds us? And how could one possibly find out if antimatter exists in some distant galaxes or metagalaxes? And what reflections would the

existence of antimatter entail for the conception of the "creation of the world", whatever this phrase may mean. These are indeed alluring problems in physics and cosmology which - at least today - seem to be *unsolvable* problems, and which *precisely for this reason* occupy some of the finest brains of the world today.

Travelling at a speed superior to that of light. It is customary to think that this is impossible. But is it really? It all depends on what we mean by "being in a certain place". A beam of light takes about two million years to reach from us to the Andromeda nebula. But my thought covers this distance in a few seconds. Perhaps some day some intermediate form of body and mind may permit us to say that we actually can travel faster than light.

The astronaut William Anders, one of the three men who around Christmas time 1968 circled the moon in Apollo 8 said in an interview in Oslo (2): "Nothing is impossible . . . it is no use posting Einstein on the wall and say: Speed of light-but not any quicker . . . 30 nay 20, years ago we said: Impossible to fly higher than 50 000 feet, or to fly faster than three times the speed of sound. Today we do both."

The dream of Stanley Jevons. The English mathematician and economist Stanley Jevons (1835-1882) dreamed of the day when we would be able to *quantify* at least some of the laws and regularities of economics. Today - since the break-through of econometrics - this is not a dream anymore but a reality. About this I have much more to say in the sequel.

Struggle, sweat and tears. This slight modification of the words of Winston Churchill is admirably suited to caracterize a certain aspect of the work of the scientists - and particularly of that kind of scientists who are absorbed in the study of "unsolvable" problems. They pass through ups and downs. Sometimes hopeful and optimistic. And sometimes in deep pessimism. Here is where the constant support and consolation of a good wife is of enormous value to the struggling scientist. I understand fully the moving words of the 1968 Nobel prize winner Luis W. Alvarez when he spoke about his wife: "She has provided the warmth and understanding that a scientist needs to tide him over the periods of frustration and despair that seem to be part of our way of life" (3).

2. A PHILOSOPHY OF CHAOS. THE EVOLUTION TOWARDS A MAMMOTH SINGULAR TRANSFORMATION

In the The Concise Oxford Dictionary (4) - a most excellent book - "philosophy" is defined as "love of wisdom or knowledge, especially that which deals with ultimate reality, or with the most general causes and principles of things".

If we take a bird's eye-view of the range of facts and problems that were touched upon in the previous section, reflections on the "ultimate reality" quite naturally come to our mind.

A very general point of view in connection with the "ultimate reality" I developed in lectures at the Institut Henri Poincaré in Paris in 1933. Subsequently the question was discussed in my Norwegian lectures on statistics (5).

The essence of this point of view on "ultimate reality" can be indicated by a very simple example in two variables. The generalization to many variables is obvious. It does not matter whether we consider a given deterministic, empirical distribution or its stochastic equivalence. For simplicity consider an empirical distribution.

Let x_1 and x_2 be the values of two variables that are directly observed in a series of observations. Consider a transformation of x_1 and x_2 into a new set of two variables y_1 and y_2. For simplicity let the transformation be linear i.e.

(2.1)
$$y_1 = b_1 + a_{11} x_1 + a_{12} x_2$$
$$y_2 = b_2 + a_{21} x_1 + a_{22} x_2$$

The b's and a's being constants.

The matrix

(2.2)
$$\begin{pmatrix} a_{11}\ a_{12} \\ a_{21}\ a_{22} \end{pmatrix}$$

is the Jacobian of the transformation, as it appears in this linear case.

It is quite obvious - and well known by statisticians - that   the *correlation coefficient* in the set $(y_1 y_2)$ will be different from-stronger or weaker than-the correlation coefficient in the set $(x_1 x_2)$ ("spurious correlation"). It all depends on the numerical structure of the transformation.

This simple fact I shall now utilize for my reflections on an "ultimate reality" in the sense of a theory of knowledge.

It is clear that if the Jacobian (2.2) is *singular,* something important happens. In this case the distribution of y_1 and y_2 in a $(y_1 y_2)$ diagram is at most one-dimensional, and this happens *regardless* of what the individual observations x_1 and x_2 are - even if the distribution in the $(x_1 x_2)$ diagram is a completely chaotic distribution. If the distribution of x_1 and x_2 does not degenerate to a point but actually shows some spread, and if the transformation determinant is of rank 1, i.e. the determinant value being equal to zero but not all its elements being equal to zero, then all the observations of y_1 and y_2 *will lie on a straight line* in the $(y_1 y_2)$ diagram. This line will be parallel to the y_1 axis if the first row of the determinant consists exclusively of zeroes, and parallel to the y_2 axis if the second row of the determinant consists exclusively of zeroes. If the distribution of x_1 and x_2 degenerates to a point, or the transformation determinant is of rank zero (or both) the distribution of y_1 and y_2 degenerates to a point.

Disregarding these various less interesting limiting cases, the essence of the situation is that even if the observations x_1 and x_2 are spread all over the $(x_1 x_2)$ diagram in any way whatsoever, for instance in a purely chaotic way, the corresponding values of y_1 and y_2 will lie on a straight line in the $(y_1 y_2)$ diagram when the transformation matrix is of rank 1. If the slope of this straight line is finite and different from zero, it is very tempting to interpret y_1 as the "cause" of y_2 or vice versa. This "cause", however, is not a manifestation of something intrinsic in the distribution of x_1 and x_2, but is only a human figment, a human *device*, due to the special form of the transformation used.

What will happen if the transformation is not exactly singular but only

near to being singular? From the practical viewpoint this is the crucial question. Here we have the following proposition:

(2.3)   Suppose that the absolute value of the correlation coefficient r_x in $(x_1 x_2)$ is not exactly 1. Precisely stated, suppose that

(2.3.1) $0 \leqslant |r_x| < 1 - E$ where $0 \leqslant \varepsilon < 1$.

This means that ε may be chosen as small as we desire even exactly 0, but it must not be exactly 1. Hence $|r_x|$ may be as small as we please even exactly 0, but not exactly 1.

Then *it is possible* to indicate a nonsingular transformation from x_1 and x_2 to the new variables y_1 and y_2 with the following property: However small we choose the positive, but not 0, number δ, the correlation coefficient r_y in $(y_1 y_2)$ will satisfy the relation

(2.3.2) $|r_y| (\geqslant 1 - \delta \ 0 < \delta \leqslant 1$

whatever the actual distribution of $(x_1 x_2)$ may be, provided only that it satisfies (2.3.1). The nature of the transformation to be chosen will, of course, depend on the previous choice of ε and δ. But to any such choice it is possible to indicate a nonsingular transformation with the specified properties.

Briefly expressed in words this means the following:

(2.3.3)   Suppose that the distribution of $(x_1 x_2)$ is unknown and arbitrary with the only proviso that it shall not degenerate into a straight line (as expressed by (2.3.1) where we may choose ε as small as we please, even exactly 0). We can then indicate a nonsingular linear transformation of the variables x_1 and x_2 which will produce *as strong a correlation in* $(y_1 y_2)$ *as we please.* (This is expressed in (2.3.2) where we may choose δ as small as we please, however different from 0.)

I have said that it is possible to indicate a nonsingular transformation with the specified properties. This is true, but the smaller we have chosen ε and δ the *nearer to singularity* we must go in order to make the linear transformation such as to have the specified properties.

Now let us *reverse* the viewpoint and assume that y_1 and y_2 are directly observed, perhaps with a strong correlation. It seems that we have no way of excluding the possibility that the observed variables y_1 and y^2 are *in fact* derived from an essentially chaotic distribution of two variables x_1 and x_2. More generally: Perhaps there are many x's and y's, and more x's than y's, and consequently a matrix of transformation from the x's to the y's, whose rank is at most equal to the number of the y's. How could we then exclude the possiblity that the chaotic world of the x's is "the ultimate reality"?

What would the *transformation* mean in this case? For one thing it would express the present status of our *sense organs* as they have emerged after a long development over time.

It is quite clear that the chances of survival of man will be all the greater the more man finds *regularities* in what seems to him to be the "outer world". The survival of the fittest will simply eliminate that kind of man that does not live in a world of regularities. This development over time would work partly unconsciously through the biological evolution of the sense organs, but it would also work consciously through the development of our experimental

techniques. The latter is only an extension of the former. In principle there is no difference between the two. Indeed, science too has a constant *craving for regularities.* Science considers it a triumph whenever it has been able by some partial transformation here or there, to discover new and stronger regularities. If such partial transformations are piled one upon the other, science will help the biological evolution towards the survival of that kind of man that in the course of the millenniums is more successful in *producing* regularities. If "the ultimate reality" is chaotic, the sum total of the evolution over time - biological and scientific - would tend in the direction of producing a mammoth singular transformation which would in the end place man in a world of regularities. How can we possibly on a scientific basis exclude the possibility that this is really what has happened ? This is a crucial question that confronts us when we speak about an "ultimate reality". Have *we created* the laws of nature, instead of *discovering* them? Cf. Lamarck vs. Darwin.

What will be the impact of such a point of view? It will, I believe, help us to think in a *less conventional way.* It will help us to think in a more advanced, more relativistic and less preconceived form. In the long run this may indirectly be helpful in all sciences, also in economics and econometrics.

But as far as the concrete day to day work in the foreseeable future is concerned, the idea of a chaotic "ultimate reality" may not exert any appreciable influence. Indeed, even if we recognize the possibility that it is evolution of man that in the long run has *created* the regularities, a pragmatic view for the foreseeable future would tell us that a continued search for regularities - more or less according to the time honoured methods - would still be "useful" to man.

Understanding is not enough, you must have compassion. This search for regularities may well be thought of as the essence of what we traditionally mean by the word "understanding". This "understanding" is one aspect of man's activity. Another - and equally important - is a vision of the *purpose* of the understanding. Is the purpose just to produce an intellectually entertaining game for those relatively few who have been fortunate enough through intrinsic abilities and an opportunity of top education to be able to follow this game? I, for one, would be definitely opposed to such a view. I *cannot be happy* if I can't believe that in the end the results of our endevaours may be utilized in some way for the betterment of the little man's fate.

I subscribe fully to the words of *Abba Pant,* former ambassador of India to Norway, subsequently ambassador of India to the United Arab Republic, and later High Commissioner of India to Great Britain:
"Understanding is not enough, you must have compassion." (6).

3. A BRIEF SURVEY OF THE DEVELOPMENT OF ECONOMICS IN THE LAST CENTURY

Turning now to the more specifically economic matters, it is inevitable that I should begin by making a brief survey of the development of economics in the last century.

In the middle of the 19th century John Stuart Mill (1806-1873) in his famous work "Principles of Economics" said that so far as general principles are concerned the theory of value and price was now completely elaborated.

There was nothing more to add, he said, neither for himself nor any other author. To us with our relativistic view on knowledge and the development of science, it is difficult to understand that such a statement could be made. But to the generation that lived at that time these words by Mill appeared to be very close to the truth. In Mill's "Principles" the ideas of Adam Smith (1723-1790), David Ricardo (1772-l823) and Thomas Robert Malthus (1766-1834) had been knit together into an organic, logically and seemingly complete whole.

Subsequent developments have thoroughly denounced Stuart Mill's words. Two break-throughs have emerged in economic theory since the time of Stuart Mill.

The classical theory of value - as we find it streamlined in Stuart Mill - was essentially a theory of production costs based on the thinking of the private entrepreneur. The entrepreneur will think about as follows: "If I could only cut my selling price I would be able to draw the customers to me. This, how-ever, is also the way my competitors think. So, there emerges a sort of gravita-tional force that pulls prices down. The cost of production is so to speak the solid base on to which the prices fall down and remain. Hence the cost of production is "the cause" of prices. This general viewpoint the classical economists applied with great sagacity to a whole range of commodities , to the relation between wages and profits and to the theory ofinternational prices etc.

This theory contains, of course, an irrefutable element of truth. But it is too simple to give even a crude presentation of the forces at play. The economic process is an *equilibrium* affair where both technological and subjective forces. are at play. The subjective element was nearly left out by the classicists.

On this point economic theory was completely renewed in the years between 1870 and 1890 when a number of Austrian economists headed by Karl Menger (1840-1921) undertook a systematic study of the human wants and their place in a theory of prices. Similar thoughts were expressed also by the Swiss Léon Walras (1834-1910) and the Englishman Stanley Jevons (1835-l882). This was the first break-through since Stuart Mill.

The Englishman Alfred Marshall (1842-1924) subsequently did much to combine the subjective viewpoint and the cost of production viewpoint. This led to what we now usually speak of as the neo-classical theory.

Neither the classicists nor the neo-classicists did much to *verify* their theo-retical results by *statistical* observations. The reason was partly that the statistics were poor, and partly that neither the classical nor the neo-classical theory was built out with the systematic statistical verification in view. The architec-tural plan of the theory had so to speak not made room for this verification. This fact was criticized by the German historical school under the leadership of Gustav Schmoller (1838-1917) and by the American institutionalists. These schools, however, had an unfortunate and rather naive belief in something like a "theory-free" observation. "Let the facts speak for themselves". The impact of these schools on the development of economic thought was therefore not very great, at least not directly. Facts that speak for themselves, talk in a very naive language.

In the first part of the 20th century the picture changed. Partly under the influence of the criticism of the historical school and the institutionalists the theoreticians themselves took up a systematic work of building up the theory in such a way that the theory could be brought in immediate contact with the observational material. One might say that from now on economics moved into that stage where the natural sciences had been for a long time, namely *the stage where theory derives its concepts from the observational technique, and in turn theory influences the observational technique.*

For the first time in history it now seemed that the work on the theoretical front in economics - now to a large extent mathematically formulated - and the work on the outer descriptive front should converge and support each other, giving us a theory that was elaborate enough to retain the concrete observational material, and at the same time a mass of observations that were planned and executed with a view to be filled into the theoretical structure.

Of course, there had been forerunners for such a combination of economic theory, mathematics and statistics even earlier. It was represented by such men as Johan Heinrich von Thünen (1783-l850), Augustin Cournot (1801-1877), A. J. Dupuit (1804-1866) and Hermann Heinrich Gossen (1810-1858). But from the first part of the 20ieth century the movement came in for full. This was the beginning of the econometric way of thinking. And this is what I would call the second break-through since Stuart Mill.

A crucial point in this connection is the quantification of the economic concepts, i.e. the attempts at making these concepts measurable. There is no need to insist on what quantitative formulation of concepts and relations has meant in the natural sciences. And I would like to state that for more than a generation it has been my deepest conviction that the attempted quantification is equally important in economics.

The quantification is important already at the level of *partial* analysis. Here one has studied the demand for such important commodities as sugar, wheat, coffe, pig iron, American cotton, Egyptian cotton etc.

And the quantification is even more important at the *global* level. Indeed, at the global level the goal of economic theory is to lay bare the way in which the different economic factors act and interact *on each other* in a highly complex system, and to do this in such a way that the results may be used *in practice* to carry out in the most effective way specific desiderata in the steering of the economy.

As long as economic theory still works on a purely qualitative basis without attempting to measure the numerical importance of the various factors, practically any "conclusion" can be drawn and defended. For instance in a depression some may say: A wage reduction is needed because that will increase the profits of the enterprises and thus stimulate the activity. Others will say: A wage increase is needed because that will stimulate the demand of the consumers and thus stimulate activity. Some may say: A reduction of the interest rate is needed because this will stimulate the creation of new enterprises. Others may say: An increase of the interest rate is needed because that

will increase the deposits in the banks and thus give the banks increased capacity of lending money.

Taken *separately* each of these advocated measures contains *some particle* of truth, taken in a very partial sense when we only consider *some* of the obvious direct effects, without bothering about indirect effects and without comparing the relative strengths of the various effects and countereffects. Just as one would say: If I sit down in a rowing boat and start rowing in the ordinary way, the boat will be driven *backwards* because of the pressure exerted by my feet in the bottom of the boat.

In a global analysis that shall be useful for practical applications in economic policy in the nation as a whole, the gist of the matter is to study the relative strengths of all relevant effects and countereffects, hence the need for quantification of the concepts.

This perhaps is the most general and most salient formulation of the *need* for econometrics. How far we would be able to go in this direction was of course another question. But at least the attempt had to be made if economics were to approach the state of an applied science.

It goes without saying that econometrics as thus conceived does not exhaust all the contents of economics. We still need - and shall always need - also broad philosophical discussions, intuitive suggestions of fruitful directions of research, and so on. But this is another story with which I will not be concerned here (7). Let me only say that what econometrics - aided by electronic computers - can do, is only to push forward by leaps and bounds the line of demarcation from where we *have to* rely on our intuition and sense of smell.

4. Some Historical Notes on the Founding of The Econometric Society

In the files of the Oslo University Institute of Economics I have located a folder containing letters and copies of letters dating from the years when the plans for an econometric society took shape. Here are interesting ideas and opinions from outstanding people in different parts of the world. Most of these people have now passed away.

One of them was my good friend professor Francois Divisia. His letter of 1 September 1926 from his home in Issy les Moulineaux (Seine) was handwritten in his fine characters, 8 pages to the brim with every corner of the paper used. Most of the letter contained discussions on specific scientific questions, but there were also some remarks of an organizational sort. He spoke for instance of his correspondence with professor Irving Fisher of Yale. About this he said: *"Je suppose qu'il s'agit d'une liste destinée à établir une liaison entre les économistes mathématiciens du monde entier"*. Whether this was an independent initiative on the part of Fisher in connection with a plan for a society, or it was an outcome of my previous correspondence with Fisher, I have not been able to ascertain, because the files are missing. Divisia continues: *"Dans la politique, je ne suis pas très partisan des organismes internationaux . . . mais dans les domaines desinteresses comme celui de la science, j'en suis au contraire partisan sans restriction"*.

Answering Divisia in a letter of 4 September 1926 I said inter alia: *"Je*

saisis avec enthousiasme l'idee d'une liste ou d'un autre moyen de communication entre les économistes mathematiciens du monde entier. J'ai eu moi-même l'idée de tâcher de réaliser une association avec un périodique consacré à ces questions. Il est vrai que les périodiques ordinaires tels que la Revue d'économie politique ou l'Economic Journal, etc. acceptent occasionnellement des memoires mathematiques, mais toujours est-il que l'auteur d'un tel memoire se trouve duns l'obligation de restreindre autant que possible l'emploi de symboles mathematiques et le raisonnement par demonstration mathematique.

Je connais déjà plusieurs economistes-mathématiciens dans differents pays, et j'ai pensé érire un jour ou l'autre une lettre à chacun d'eux pour avoir leur opinion sur la possiblité d'un périodique, (que dites-vous d'une "Econometrica"?, la soeur du"Biometrika".) Maintenant je serai heureux d'avoir votre opinion d'abord. Si vous pensez que cela vaut la peine on pourra peut-être commencer par former un cercle restreint qui s'adressera plus tard au public. Dans les années à venir j'aurai probablement l'occasion de voyager souvent en Amérique et en Europe, alors j'aurai l'occasion de faire la connaissance des économistes qui pourront s'intéresser à ce projet, et j'aurai l'occasion de faire un peu de propagande. Peut-être pourra-t-on obtenir l'appui d'une des grandes fondations américaines pour la publication du périodique.

Voici une liste de quelque personnes que je connais par correspondance comme étant très intéressées au sujet de l'économie pure: Jaime Algarra, Professeur d'éc. pol. Université Barcelone, L. von Bortkievicz, Professeur de Stat. Univ. Berlin, E. Bouvier, Prof. de Sc. fin. Univ. Lyon, K. Goldziher, Prof. Techn. Hochschule, Budapest, K. G. Hagström, Actuaire, Stockholm, Charles Jordan, Docteur és Sc., Budapest, Edv. Mackeprang, Dr. polit., Copenhague, W. M. Persons, Prof. de Stat. Harvard Univ. Cambridge. Mass. U.S.A., E. Slutsky, Moscou, A. A. Young, Prof. d'éc. polit., Harvard Univ. Cambridge. Mass. U.S.A., P. Rédiadis. Contreamiral, Athènes."

I mentioned also a number of others, among whom were: *Anderson, Prof. Ecole Supérieure de Commerce, Varna, Bulgarie, Graziani, Prof. d'éc. pol. Univ. Napoli, Italie, Huber, Dir. de la Stat.gén. de la France, Paris, Ricci, Prof. Univ. Roma, Gustavo del Vecchio R. Univ. Commerciale, Trieste.*

In a letter of 22 September 1926 Divisia answered inter alia: "*Je suis, vous le savez, tout à fait d'accord avec vous sur l'utilité d'une Association Internationale d'Économie pure et j'aime beaucoup le titre d'"Econometrica" auquel vous avez songé pour un périodique. Toutefois, avant de passer aux realisations, je pense qu'il est indispensable de réunir tout d'abord un certain nombre d'adhésions. . . . je me demande s'il ne serait pas aussi possible et opportun de s'aboucher à une organisation existente comme l'Institut international de statistique. . . . Enfin, d'ores et déjà, tout mon concours vous est acquis.*"

In a letter of 1 November 1926 I wrote to Divisia: "*Mon départ pour l'Amérique a été ajourné de quelques mois. J'en ai profité pour écrire aux personnes suivantes: Bortkievicz, Université de Berlin, A. L. Bowley, London School of Economics, Charles Jordan, Université de Budapest, Eugen Slutsky, Moscou, pour avoir leur opinion sur l'utilité et la possibilité de réaliser d'abord un cercle restreint et plus turd peut-être une association formelle . . .*

J'ai trouvé que je n'ai pas pû expliquer la chose d'une meilleure façon qu'en copiant certains passages de votre dernière lettre . . . C'est peut-être là une petite indiscretion dont je me suis rendu coupable."

The same day 1 November 1926 I wrote to the four persons in question. In

my letter to Bortkievicz I said inter alia : "*Das veranlässt mich Ihnen einen Gedanken vorzubringen, den ich mit Herrn Francois Divisia, Paris, diskutiert habe.*

Es handelt sich urn die Realisierung eines internationalen Zirkels urn den Ideen-Austausch zwischen Mathematiker-Ökonomen zu fördern, und zu einer "mise au point" der Probleme der mathematischen Ökonomie beizutragen.

Ich Weiss dass in Deutschland das Interesse fur die mathematische Methode - sei es in Nationalökonomie, sei es in Statistik - im allegemeinen nicht sehr verbreitet ist. In den letzten Jahren glaube ich aber eine Andeutung zu einem aufwachenden Interesse spüren zu können. In 1924 erschien ja in der Sammlung sozialwissenschaftlicher Meister eine Über-setzung der "Untersuchungen . . ." von Cournot, und in den Jahrbüchern erschien voriges Jahr, wenn ich mich nicht irre, ein Aufsatz über die mathematische Methode, und neu-erdings sehe ich dass im Allg. Stat. Arch. eine ziemlich eingehende Besprechung meiner Arbeit "Sur un, problème d'économie pure", erschienen ist.

Unter diesen Umstanden sollte es vielleicht nicht hoffnungslos sein wenigstens einzelne Perstinlichkeiten - Ob nur noch zwei oder drei - zu finden, die sich fur einen internationalen Zirkel von Mathematiker-Ökonomen interessieren würden."

In my letter to Charles Jordan I said (after having discussed the speed of convergence of certain of his iterative methods): *"J'aborde maintenant une autre question qui serait, je le pense, susceptible de vous intéresser. Il s'agit de la réalisation d'un cercle international pour faciliter les échanges de vues entre les économistes-mathematiciens, et à contribuer à une mise au point des problèmes de l'économie mathématique.*

Par la copie ci-jointe vous verrez l'opinion de M. Francois Divisia."

In my letter to Eugen Slutsky I said inter alia: *"Ich danke Ihnen recht herzlich für die Zusendung Ihrer beiden interessanten Arbeiten "Sulla teoria . . . " und " Uber die zufällige Anordnung . . .",*

Die sehr wertvollen Auseinandersetzungen in Ihrem Briefe haben mich sehr interessiert. Besondersfreut es mich Ihre Anschauungen über die reine Ökonomie kennen zu lernen. Wie Sie bin ich auch davon überzeugt dass der reinen Ökonomie eine grosse Zukunft bevorsteht.

Doch es lässt sich nicht leugnen, dass bis jetzt den verschiedenen Bestrebungen einzelner Persönlichkeiten in den verschiedenen Ländern all zu sehr die Koordination fehlt."

The answers from these four people are interesting.

In a letter of 8 November 1926 professor Bowley writes: "Much as I am interested in your proposal, I should prefer not to take any definite part in its development till a year has elapsed. By that time you will know how far it is capable of realization and I shall hope to adhere to the group that may be formed."

Professor Bortkieviez answered 11 November 1926 in an eight pages hand-written letter, very legibly, and of course with the precision of thought of a mathematician. He said : *" Von Ihrem Brief von 1. d. M. und von dem beigefügten Auszug aus einem an Sie gerichteten Schreiben von Professor Divisia habe ich mit lebhaftem Interesse Kenntnis genommen. Eine Fbrderung der mathematischen Methode in der National-ökonomie erscheint mir an sich als durchaus erwünscht, und nit Rücksicht auf die Schwierig-keiten, die sich auf dem Europäischen Kontinent, von Italien abgesehen, der Unterbring-ung mathematisch gehaltener Artikel in nationalökonomischen Fachzeitschriften entgegen-stellen, würde ich die Gründung eines internationalen Organs für mathematische National-ökonomie begrüssen. . . . Hingegen verspreche ich mir von einem brieflichen Gedanken -*

Austausch zwischen Anhtingern der mathematischen Methode nicht viel. Das wäre etwas unmodern. . . . Die Anregung von Prof. Divisia, eine Organisation für mathematische Nationalökonomie dem Internationalen Statistischen Institut anzugliedern, zeugt davon, dass er über den Charakter dieses Instituts unvollkommen unterrichtet ist."

The following part of Bortkievicz's letter is a long and careful comparative analysis of certain mathematical formulae as they appear in his and my works.

In a letter of 12 November 1926 Charles Jordan writes (after a discussion of Hermite polynomials and similar matters) : *"Je suis de votre avis que la réalisation d'un cercle international des économistes mathematiciens serait extrêmement utile à l'avancement de cette science, que les économistes littéraires refusent de reconnaître. La publication d'un periodique économitrique serait fort desirable. Il n'y a aucun obstacle à la réalisation du cercle; les difficultés financières de la publication d'un périodique de dimension modeste serait, je l'espère, facilement surmontables. . . . Je suis prêt à faire mon possible pour avancer cette question. Pendant plusieurs années j'ai tenu des tours d'économie mathematique à Budapest (Université) et cette science m'a toujours vivement intéréssé.*

En me méttant entièrement à votre disposition vous prie . . ."

In a letter of 16 December 1926 Divisia speaks about the *name*. He said inter alia : *"Etymologiquement, il faut écrire oe, mais alors pourquoi ne pas aussi l'écrire en caractères grecs? Pour respecter l'etymologie, ne faudrait-il pas aussi écrire oeconomometrika ou tout au mois oeconommetrika pour en mutiler aucun des 3 mots $o\iota\eta o\varsigma, \nu o\mu o\varsigma$ $et\mu\varepsilon\tau\varrho o\nu$? Je vous avouerai que votre Econométrica m'avait paru élégant et clair."*

In the Spring of 1927 I went to the United States and discussed the matter with colleagues there. I have located a five page memorandum dated October 1927. It presents more details about the matter. I believe this memorandum was written jointly by Dr. Charles F. Roos and me.

The most important item from the time that follows is an abstract of a conversation between professor Schumpeter, dr. Haberler and me in the Colonial Club at Harvard University on 29 February 1928. The beginning of the abstract reads as follows:

"I. The terms econometric and econometrics are interpreted as including both pure economics and the statistical verification of the laws of pure economics, in essential distinction to the purely empirical manipulation of statistical data on economic phenomena.

II. The possibility of establishing a systematic annotated bibliography of econometric literature was discussed."

It is mentioned that we discussed at great length a list of people who might be interested in supporting the work. The list contained the following number of names : Austria 7, Belgium 1, France 10, Germany 9, Great Britain 7, Greece 1, The Netherlands 3, Hungary 1, Italy 10, Poland 1, Portugal 1, Russia 3, Scandinavian Countries 6, Switzerland 2, Spain 1, United States 14. Enclosed with the abstract of the conversation at the Colonial Club I find an 11 page handwritten memorandum by me in French dated April 1928. It contains in all essentials the same points as the memorandum of October 1927. There also exists a typewritten copy of this French memorandum.

For family reasons (my father died in 1928 and I had to go back to Oslo to assume some heavy responsibilities) my activity in the econometric field was

delayed, as transpires from a letter from Schumpeter written from Chamonix 31 December 1928, where he said: "I have been without your news about our plan of econometrics for quite a time. As I shall have to tell the publishers of the Archiv and others one way or the other, I should be much obliged for a few lines from you about the state of things. Of course, I should not think of going on without you."

I have not been able to locate any other notes from this period, so my work regarding an association and Econometrica seems to have fallen in two distinct periods.

The next piece of information I find is in a book by the French economist, mathematician and philosopher Jacques Rueff. He says (8) : *"C'est en 1929 que Ragnar Frisch, économiste mathématicien norvégien, me demanda de l'aider à promouvoir la creation d'une association qui rénirait les tenants d'une économie dont les principes seraient constamment confrontés avec les faits, quantitativement exprimes et soumis à leur sanction. Le projet fut discuté à un déjeuner où j'avais convié, avec mon interlocuteur norvégien,* **François** *Divisia, qui venait de publier une "Économique Rationelle".* **François** *Divisia représentait avec Rent Roy et moi la descendance intellectuelle de Clement Colson, dont nous avions tous trois été les élèves à l'École Polytechnique.*

Nous donnâmes un accueil enthousiaste au projet de Ragnar Frisch. Il fut convenu que celui-ci partirait pour les États-Unis rallier les concours nécessaires à la constitution d'une 'Société Internationale d'Économétrie'."

About the happenings on my trip to the United States this time I have not been able to locate any notes. But I retain a vivid impression of a very intimate contact and cooperation between professor Irving Fisher, Dr. Charles F. Roos and myself during this period. When Roos and I explained our plan to professor Fisher it was like throwing a burning match into a barrel of powder. Fisher threw himself in the adventure for full. We circulated letters to a great number of persons around the world and had an overwhelming number of positive reactions.

As appears from Econometrica (9) the Econometric Society was formed in Cleveland, Ohio on 29 December 1930. Irving Fisher was elected the first president. 16 people (3 of them Norwegians) were present at the organizational meeting. The first volume of Econometrica appeared in 1933.

The first European meeting of the Econometric Society was held in Lausanne (the place where Walras lived and worked) on 22-24 September 1931. A rather complete record is given in Econometrica (10). Professor Jan Tinbergen was present and spoke at the Lausanne meeting. His important paper "The Notions of Horizon and Expectation in Dynamic Economics" is published in the same volume of Econometrica as the report of the Lausanne meeting. His driving force and great impact on the development of Econometrics from its beginning and up to this day are so well known that I need not dwell upon them here. Alfred Cowles 3rd gave an invaluable financial support to Econometrica.

5. TYPES OF ECONOMETRIC MODELS AS SEEN FROM AN ADVANCED VIEWPOINT TODAY

It is out of the question in this short exposé to give a systematic presentation,

of the problems and methods of econometrics in its modern form. One gets an idea of the broadness of the field by throwing a glance at the program of the Second World Congress of The Econometric Society to be held in Cambridge, England 7-14 September 1970. Here there are a total of 46 precisely defined sessions with a total of 64 organizers of these sessions. The attendance is expected to be between 500 and 1000. (In the first European meeting in 1931 some twenty persons were present.)

What I can do here is to give a survey of types of econometric models at the national level and an example of how one selected problem may be handled.

Some general remarks on types of econometric models at the national level.
The list of variables and the equations and/or bounds that are introduced, constitute the core of the model. It may be linear or non-linear.

In addition to the core one may or may not introduce a preference function, that is a function whose maximization defines the goal of the decisions that might be studied through the model. With a preference function it becomes possible to say that one alternative constellation of the values of the set of variables is *better* than another and it might even be possible to proceed to determining an optimal solution. Otherwise the model is only a *purely descriptive one,* that can be used to produce a sample of alternative constellations, or to answer questions of the type: "What will happen if. . .".

In all cases the model may be either static (i.e. connecting only variables at the same point of time), or dynamic (i.e. connecting variables at different points of time). The *recurrent* model, based on a fixed strategy, is a special case of a dynamic model. A moving (rolling) decisional model based on the concept of already-committed-to variables is better adapted for practical applications, I believe.

In all cases the model may be either deterministic or stochastic.

According to the nature of the core and the nature of the preference function the types of models may be classified in the 2 x 3 divisions of tab. (5.1). Most models in actual use at the national level are at present of the simplest types listed in this table. Some additional explanations of the various cases are given in tab. (5.2)

The preference function
A common misunderstanding regarding the preference function is due to a lack of distinguishing between *targets* (i.e. specific values of some selected variables) which one will try to *realize,* and the use of a *preference function,* and also due to a lack of distinguishing between the free and the reduced form of the preference function. It is said that the decision maker at the national level (the responsible political authority) is not able to understand the meaning of the core. Therefore he cannot formulate targets or define a preference function.

These objections vanish if the expert approaches the decision maker in the appropriate way. I have reached this conclusion not only on theoretical grounds but also because of my practical experiences.

One way to approach the decision maker is through *interview questions.* It is

well known that people will not always behave in an actual situation exactly in the way they *said* in an interview question that they *would* behave in such and such a situation. But still, I think, it remains that valuable information may be obtained through interview questions, provided the questions are wisely formulated in a *conversational manner,* and not simply carried out by some youngster in the opinion poll trade. I have worked out a rather elaborate technique for such conversational interviews to be carried out by the econometric experts. And I have had the good fortune to test this out in conversations with high-ranking politicians both in developing countries and in industrially developed countries. I have found that it is surprising how far one can reach in this field when the conversation is wisely steered.

Essential points in this connection are: (I) To use the free form - the Santa Claus form - of the preference function. (II) To assure that the interviewed person rids his mind completely of any preconceived (and in many cases erroneous) ideas he might have on the nature of the core, and hence rid his mind of whether it is actually possible to *realize* the alternatives involved in the interview questions. (III) To assure that the interviewed person has rid his mind completely of any possibility of trading in the market any of the alternative situations which are hypothetically offered to him in the interview questions.

The interview approach to the preference function is only *a first stage* in an iterative process which in each step proceeds by an optimal solution of the model. For further details regarding these questions see the section on the transition towards economic planning.

Another misunderstanding which we sometimes hear, is this: It is said that there are many *different* systems of preferences. It is impossible to choose between these systems. Therefore the concept of preference function cannot be used in connection with national models. This is one of the biggest pitfalls in the discussion of this matter. Differences of opinion, of course, there are. One social group may have one type of preference and another social group may have other preferences, and different persons may have different preferences and even the same person may have different preferences at different points of time. All this is, of course, true. But the problem of settling differences of opinion *is not a special problem of econometrics.* It is a general problem of human behaviour and opinions. And there exists a machinery for settling such differences. This machinery is simply the political system of the country. This political system - whatever it may be - is just invented in order to settle such differences. What we have to do as econometricians is to apply this very system for the *formalization* of the preferences to go with our models. Thus the preference function as it appears in our models is an expression for the preferences of *the decision making authority,* whatever this authority may be. The preference function in the model must not be confounded with a general "Welfare function" in the sense of welfare theory.

It is not the task for us as econometricians and social engineers to go into a detailed discussion of the political system. Somewhere in the hierarchy of sciences a line of demarcation has to be drawn. And here is where we find the line of demarcation for the econometric planner. As *citizens* we are, of course,

allowed to work for any political system which we think is just and effective. I, for one, would like to work for a system that really deserves the name democracy, but this is another story. Compare the beginning of section 6.

Still another point must be clarified. Sometimes we hear the suggestion that instead of going to the trouble of discussing preferences, we ought to leave it to the experts to put on the table of the politicians a number of *alternatives* for the development path of the nation's economy, and ask the politicians to choose among these alternatives. This may be a defendable procedure if the

Tab. (5.1). Condensed table of types

<table>
<tr><td rowspan="6">The core may be of the following two kinds:</td><td></td><td>No preference function</td></tr>
<tr>
<td>

The Institutionally Stable Core (or shorter: the stable core)

Those equations and/or bounds which we *have to accept* if we confine the analyses to the institutional and political setting which it is out of the question to change within the time horizon of the analyses. For details see tab. (5.2).

    Can also be termed the *obligatory core* or *the obligatory conditions,* or again the conditions that are in-variant under a change in the *facultative* conditions.

</td>
<td>

The purely descriptive stable model
Leads *to* the concept of *dependent* (endogenous) and *free* (exogeneous) variables. The number of the free variables is equal to the number of degrees of freedom of the core. The concept of *targets* cannot be derived from the model. The only legitimate use of this kind of model is for presenting a sample of *alternative* constellations of the economy, or for answering questions of this sort: What will happen if . . .? Even such limited uses of the model may have considerable practical importance.

</td>
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<tr>
<td>

The Institutionally Sensitive Core
(or shorter: the sensitive core)
Equations and/or bounds some of which are sensitive to such institutional and political changes that might be considered within the time horizon of the analyses. When comparing the effects of such institutional or political changes, it is necessary to have a stable core as a fixed point of reference.

</td>
<td>

The purely descriptive sensitive model
Similar to the purely descriptive stable model with the difference that target setting now becomes still more arbitrary. The conclusions drawn from this type of model will as a rule be statistically more uncertain than in the stable case because now many of the data arc difficult to fix numerically.

</td>
</tr>
</table>

of econometric models at the national level

The free form of the preference function The gross form. The Santa Claus form: Which one of some specified few alternatives would you choose, if you had the choice? Not necessary to understand the core in order to answer such questions.	*A reduced form of the preferencefunction* Only understandable in terms of the core: The preference function expressed in terms of a set of variables equal in number to the number of degrees of freedom of the core. Mathematically speaking several reduced forms might (and in general will) exist. The choice of one particular reduced form is a practical question.
The free form of the preference function as applied to a stable core The free form of the preference function is particularly useful when forming interview questions to the responsible politicians. In principle the complete knowledge of the free form is sufficient to formulate an optimalization problem. The expert will then often transform the free form to a reduced form because he finds this convenient.	*A reducedform of the preference function as applied to the stable core* This case is both computationally and for practical uses highly important. It leads to the concept of the *stable core optimal constellation*. It will as a rule not be the final practical optimum, but will be used for comparison purposes. An interesting figure is the amount by which the stable core optimum preference value is *superior* to the value of the preference function which is obtained through any institutional and political system different from that which defines the stable core.
The free form of the preference function as applied to a sensitive core Same as above but now the free form of the preference function may contain a greater number of variables. Statistical difficulties of the same sort as mentioned in the case to the left, and mathematical difficulties as mentioned in the case to the right.	*A reduced form of the preference functio n as applied to a sensitive core* In this case, and in this case only, the concept of targets can be derived from the model provided an optimal solution has been attempted. Mathematical difficulties in explicit search for the optimum constellation of the institutional parameters in a given institutional set-up may be circumvented by arranging institutional games. If so, the games must be defined so as to assure comparability between game results and the stable core optimum. Particularly important in a search for the *optimal institutional set-up*.

Tab. (5.2). Some details regarding the structure of the institutionally stable model.

The institutionally stable core
(The obligatory conditions)

A. Definitonal Equations and bounds.
B. Technological equations and bounds.
C. Such behaviouristic equations and bounds which
we *have to* accept within the time horizon of the
analyses.

The equations and bounds ABC can also be described as relations which are *invariant* under such changes of the institutional and administrative structure which might be possible within the time horizon. A change which only consists in changing (within prescribed bounds) certain *parameters* (tax rates for instance) characterizing the structure, will not be considered as changing the structures itself *provided* these parameters are counted among the authorized variables.

(In distinction to an obligatory core we may speak of an institutionally and administratively facultative core, i.e conditions that are not obligatory, se Note below to the right).

The number of degrees of freedom of the core is the number of authorized variables minus the number of independent *equations* in the core. A *bound cannot influence the number of degrees of freedom of the core except* in the limiting case where an upper bound is equal to the corresponding lower bound. The two bounds in question may be bounds on a single variable or it may be bounds on a more or less complicated function of the variables. This limiting case is equivalent to the introduction of one *more equation, and should be counted as such.* The number of degrees of freedom is further discussed in ref. (11).

Shifting from one set to another set of variables that are to be considered as *Government controlled*, does not change the core but it may need an additional *transmission analysis*. See ref. (11b).

If to this core we add a preference function, we get an institutionally stable *decision model.* The preference function does *not* influence admissibility.

Preferences expressed by *conditions* instead of by a preference *function* might influence admissibility. Compare pp. 113—124 of ref. (11). The terms "selection" and "structure" are there used in the same sense as "institutionally stable" or "obligatory" here.

This core gives rise to the institutionally *admissible region*, i.e. the set of points (in the space of the authorized variables) that are *compatible* with the equations and bounds in the institutionally stable core. The dimensionality of the admissible region is equal to the number of degrees of freedom in the core (depending only on the number of variables and on the number of independent equations) but its size and content and its borders depend on the bounds.

Admissibility depends only on the core, not on the preference function.

Note. The relations among the core, the admissibilitiy region and the preference function in the institutionally sensitive case, are in principle similar to those in tab. (5.2), but now not all the equations and bounds are obligatory, but may be chosen differently according to political inclinations.

number of meaningful alternatives is very small and if we can trust the experts not to smuggle their *own personal preferences into the choice of alternatives.* Ref. (11 c).

Even if we could trust the experts, the listing of alternatives would be impossible in an advanced form of planning. Indeed, in economic political discussions there is a nearly infinite number of specific questions that may be asked. Besides the ones mentioned in section 3 consider for instance these: "Should we build a road between points A and B in the country?", "Should we promote investments that will give employment to many people, or should we on the contrary promote such investment which will save labour?", "Should we aim at a high rate of increase of the gross national product, or should we put more emphasis on a socially justified distribution of it?", "Should we aim, above all, at keeping the price level under control?", "Or should we sacrifice the stability of the price level and put more emphasis on the increase of the gross national product (in real terms) ?", "Should we sacrifice a part of the increase of the total gross national product in order to be able to increase the living standards of one specific social group, say fishermen or industrial workers?", "Should we put more emphasis on things that have up to now not been included in the statistical concept of the gross national product? For instance, should we try to avoid air-pollution and all the kinds of intoxications that may be caused by refuse and waste (a problem that must be studied in its *totality* as a problem of *circulation of matter* in society, much in the same way as we study interindustry relations in an input-output table)?", "Should we assess economic value to an undisturbed nature?" etc.

If we should ask the experts to produce a list of feasible alternatives for the development path of the economy, a list that would be comprehensible enough to cover even very approximately all these various specific questions, the list of possible development path would have to contain millions and millions of alternatives. The number of alternatives would multiply by cross classification.

Such a list is impossible for the simple reason that the experts would be physically unable to analyse and present all these alternatives, and even if all the alternatives could be analysed and put on the table of the politicians, the politicians would be absolutely drowned in information. They would not know where to start and where to end in discussing which alternative to choose. On the electronic computer one speaks of "information death" when one has made the mistake of letting the computer print out too many of the intermediate results. The poor politicians would suffer a similar information death if they found on their table a hypothetical list of the millions and millions of feasible development paths.

In rational economic planning there is no other possibility than to start in patiently on a discussion of the preference function. To begin with the model would have to be heavily aggregated, but as experience is gained, more details would be included.

Finally a warning should be made against one very simple (and therefore very popular) procedure. A large number of references to such simple procedures could be given. Suffice it here by way of example to refer to (12). The unsatisfactory procedure consists in the following: One starts by *guessing* at the

probable growth rate of gross national product in future years. And from this guess one tries to estimate through the use of input-output analyses, national accounts etc. what the development of the various production sectors, consumption etc. will be. This is unsatisfactory for at least three reasons: (I) The growth rate depends essentially on what *decisions* are made regarding the steering of the economy. Guessing at the growth rate therefore implies a guess regarding the economic policy that will be followed in the years to come. (II) Even if the growth rate is given, it does *not* follow what the development of the various sectors or consumption etc. will be. The economy has many more degrees of freedom than just one. (III) How can one assert that the growth rate guessed at is the *optimal* one, i.e. the growth rate that corresponds best to the policy makers' preferences? The growth rate is indeed not a datum but a *consequence* of an optimal solution, with all the intricacies connected with the determination of that optimum.

So much for some general points of principle. In the next section I shall discuss some practical questions connected with the transition towards economic planning at the national level.

6.   THE TRANSITION TOWARDS ECONOMIC PLANNING AT THE NATIONAL LEVEL

Any econometrician who wants to see practical application of his science, will be highly concerned with applications to economic planning at the national level.

Economic planning the basis for efficiency and a living democracy
I will give my personal views in this matter. I stress the aspect of efficiency as well as the aspect of a living democracy. The problem I am driving at is more ambitious than just to increase the long-term average growth rate of gross national product. My purpose is to make economic planning at a high aspiration level one of the pillars of a living democracy. I want a society which is a living democracy, not only a formal one with free elections, socalled freedom of speech, a socalled free press and so on, but a democracy that is living in the sense of actually engaging as many as possible of the citizens to take an active part in the affairs of the small community where they are living, and also to take an active part in the affairs of the nation as a whole. I will give an example. Some years ago I undertook together with Mrs. Frisch, a lecture trip to Iceland visiting also some of the small communities in the north. (It is no secret that I was invited on this trip in order to help Iceland refrain from applying for membership in the EEC.). In the small communities in the north the population depends nearly exclusively on grass farming. This was in the middle of the haying season. In one place there had gathered an audience of 60 people. Think of what this means in the sparsely populated country and in the middle of the haying season. Some of them had travelled 60 km. to come to the meeting. They brought with them long papers to present and discuss after my lecture. This is living democracy.

A high aspiration level. Professor Louis W. Alvarez, Nobel prize winner in

physics 1968 said: "Physics is the simplest of all the sciences . . . When we make a simple change in a system such as adding a little heat, we can easily predict that the whole thing is going to get warmer . . . But in the case of an infinitely more complicated system, such as the population of a developing country like India, no one can yet decide how best to change the existing conditions" (13). I quite agree that such problems as that of India are not yet solved. But to help solving them is precisely the high ambition of the econometric planner. The *difficulty* of such problems is our excuse for not having reached the same level of precision as the physical sciences have. *But we are on our way.* And we are hopeful that we will one day come at least very much closer to the precision of the physicists, than we are today.

It is comforting to know that already politicians in many countries do find our work useful. It warmed my heart when the chairman of the Finance Committee in the Norwegian Storting on 11 November 1969 opened that year's discussion on financial matters by a 170 words speech on behalf of a unanimous committee expressing how much the politicians owe to the efforts of the econometricians (14).

The cooperation between the politicians and the experts
Already today there is, of course, a good deal of cooperation between politicians and experts. But on one point there is need for a new break-through, namely in making it a practice to cooperate on the formalization of the preference function. This will be of basic importance for *clarifying what the political authorities really are aiming at.*

For simplicity let me first describe how to attack the problem for a given political party.

A preparatory phase of the expert's work would simply consist in his making a systematic use of his general knowledge of the political atmosphere in the country, and in particular the political atmosphere in the party in question. The expert will have formed an opinion, *a tentative opinion,* about what the preferences of this party would look like if they were formalized in a way that fits in with the expert's model and is expressed in a language that will be understandable to his electronic computer.

In a subsequent phase the expert - on the basis of his tentative formalization - will work out a system of interview questions through which he will get *closer* to the formalization of the preferences in question. Compare the relevant parts of section 5. As a simple example of an interview question we may take the following: What would you, politician, choose if you had the choice between two packages of economic results, for instance, one package with, say 3% unemployment and an annual inflation rate of 5%, and another package with, say, 10% unemployment and an inflation rate of 1%. By repeating this question, but with different figures involved, it will be relatively easy to reach a situation where the interviewed person would say: It is all the same which one of the two packages I receive. This point of indifference is precisely what the expert is driving at. Similarly for other kinds of comparisons. There will be a whole series of such partial "package questions". From answers to a complete

system of such partial questions the expert will be able to build up a preference function in its free form. If he finds it convenient, the expert may subsequently transform this preference function to a reduced form. But this is only a secondary question.

In the third phase the expert will go back to his electronic computer in which he had already entered the data regarding the core of the economy. To this he will now add the formalization of the preferences in the quantitative form as he now sees it. From this will come out a solution, in the form of an optimal development path for the economy. Optimality being defined through the preferences of this party and in the preference formalization which the expert has now reached.

When the expert comes back to the politicians with his solution, the politicians will perhaps say: "No, this was not really what we wanted. . . We have to change these particular aspects of your solution."

The expert will understand more or less precisely what sort of changes are needed in the formulation of *the preference function* in order to produce a solution that comes closer to what the politicians now say they want. This leads to a discussion back and forth. In this way one will work step by step towards a preference formulation such that the politicians can say about the resulting solution: "All right, this is what we would like to see." Or perhaps the expert will have to end by saying politely: "Your Excellencies, I am sorry but you cannot have at the same time all these things on which you insist." The excellencies, being intelligent persons, will understand the philosophy of the preference questions and the expert's study of the core, and will therefore acquiesce with a solution which is not completely what they like, but at least something better than other alternative shapes of the development path which have emerged from the previous tentative solutions.

Even if we did not go any further with the formalization of the system of preferences than to work out such an analysis *separately for each political party*, an enormous gain would be obtained in elucidating the economic political discussions.

But we should not stop at this point. We should proceed to a discussion of what sort of *political compromise* that might be reached in the formulation of a unified system of preferences. And then having reached this compromise formulation, there would appear a compromise optimal solution. Here too an iteration between politicians and experts would take place.

The top political authority - in a democratic country it would be the elected Parliament - ought to concentrate *most of its time and efforts* on a discussion of this compromise on the formulation of the system of preferences, instead of using practically all of its time on discussing *one by one* the specific economic measures that might have been proposed, and for each of these measures deciding whether to accept it or not. In the way suggested the parliament would concentrate its time and energies on *the most important things,* on the really vital issues. If this were done, many details could safely be left to the experts. *Big* issues would of course finally be checked through one by one Parliament decisions.

A simplified scheme for expressing political preferences regarding a *few* basic questions, is discussed in ref. (15).

7. Investment Starting vs. Investment Sinking

I shall give an example illustrating an advanced approach to a point in the planning technique. It concerns the measurement of *investments* in an advanced planning model. If the planning model has a time horizon of more than, say, a year or two, and if investment *decisions* are an important part of the analyses, the distinction between investment *starting* and investment *sinking* (investment carry-on-activity) is essential.

7.1. *A verbal definition.* Investment *starting* in any given year is the total outlay (measured in volume figures, i.e. in monetary units of a fixed purchasing power) which it is estimated that the projects started that year will have entailed when they are finally completed-perhaps at some future date. Investment *sinking* in any given year is the measure of goods and services that were actually used (that were "sunk") that particular year in order to carry towards completion projects which were started that year or some previous year.

7.2. *The project description* is a collection of all the descriptive details regarding a project, that can be given by the specialists (technical engineers, etc.) who have detailed knowledge about this project, but do *not* have a systematic knowledge of all the broader *social, economic and political* considerations at the national level that one must take account of before one can reach a well founded decision as to whether this project is to be accepted or not.

A rational and coherent treatment of investment criteria can, I think, only be given by considering all the investment projects - defined through the project descriptions - as intergrated parts of a *complete macroeconomic decision model* with all its detailed and (politically) preferential aspects. The project descriptions are building stones in the complete decision model. But nothing more.

We must stress the fundamental distinction between information that is available *before* the optimization of the decision model and information that only emerges *after* this optimization.

This distinction is the crux of the matter in planning at the national level in any country. In this optimization all the geographical, material, cultural and political peculiarities of the country come into the picture. These are parts of the core and of the preference function. This broad perspective can, of course, not be compressed into the format of a project description for a single project.

Therefore, the project descriptions belong definitely to the kind of information that is available *before* optimization. Such information is a *necessary* basis, but very far from being a sufficient basis for reaching well founded investment decisions.

7.3. The sinking year and the starting year. Consider a single investment project No. g

(7.3.1)   t = *the interflow year* (the calendar year) is the year (or quarter or month) to which the complete macroeconomic interflow table with all its balancing and accounting realtions applies. In particular, when we are discussing the difference between investment starting and investment sinking, t will be *the sinking year.*

(7.3.2)   σ = *the starting year* is the year when actual work on the execution of the project may begin. The *decision* year i.e., the year when it was decided whether to accept or reject the project No. g, is the year when the whole plan was adopted. This may be an earlier year than σ. As a rule the decision year (the planning) year) will be denoted as the year 0. Research work in connection with the plan and in particular research work in connection with the project No. g may have taken place even earlier. There may be alternative σ's. The σ's take care of the *phasing problem.*

(7.3.3)   *s* = t-σ   is the *sinking delay.* Roughly formulated the sinking delay is "the number of years that have elapsed since starting". More precisely formulated: s = 0 refers to the sinking that will take place in the *same* year as the starting (if the project is accepted for starting in a given year.) s = 1 refers to the sinking that will take place in the year that follows immediately after the year when the starting took place. And so on for the higher values of s.

7.4. Sinking flows.

In tab. (7.4.1) (g) denotes the number of different years in which sinking inputs for the project No. g will occur, roughly expressed: The *construction* period for project No. g. Also years where descriptionally $J^s = 0$ (s< (g)) are counted.

The symbols given in table (7.4.1) are *general* symbols for the sinking flows and their totals with respect to project No. g. These magnitudes may, for instance, refer to flows that are determined already in the project description,, and if so they are denoted $J^{\bullet s}_{kg}$ (k = h, i, B). This happens for all k and s only in the case where no *sinking substitution possibility* exists. The flows that emerge after the decision model optimization, are denoted $\hat{J}^s_{kg}$ (k = h, *i*, B). These latter flows always exist and are well defined (possibly with some degrees of freedom if there remain degrees of freedom in the optimum). In a more general context the symbols J^s_{kg} (k = h, i, B) in table (7.4.1) may be used simply as indicating *variables* that enter into the decision model before optimization.

7.5. Sinking Coefficients in the Non-Substitution Case for Sinking Inputs. In the special case where no possibility of sinking substitution is assumed to exist, all the flows in table (7.4.1) are *fixed* and *well defined* already in the project description.

Table (7.4.1). Sinking Flows for the Project No. g.			Sinking delay s (after the hypothetical starting year)					
			$s = 0$	$s = 1$	$s = 2$	$\ldots$	$s = (g-1)$	Row Sums
From delivering Domestic Sectors	h	1 2 ·	$J^0{}_{hg}$	$J^1{}_{hg}$	$\ldots$	$\ldots$	$\ldots$	J_{hg}
From Domestic Primary Input Factors	i	101 102 ·	$J^0{}_{ig}$	$J^1{}_{ig}$	$\ldots$	$\ldots$	$\ldots$	J_{ig}
From Complementary (non-competitive) Imports	B		$J^0{}_{Bg}$	$J^1{}_{Bg}$	$\ldots$	$\ldots$	$\ldots$	J_{Bg}
Column sums:			$J^0{}_g$	$J^1{}_g$	$\ldots$	$\ldots$	$\ldots$	$H_g{}^{tot}$

Sinking inputs (left margin label)

J = Gross investment (as distinct from I = net investment after depreciation).
H = "Hardware". The H's are important variables in the model. If the project descriptions are stationary, the J's will be independent of σ.

In this case we may compute the corresponding system of *sinking coefficients*. Denoting *coefficients* by an apostrophe, the sinking coefficients are:

$$(7.5.1) \quad J'^s_{kg} = \frac{J^{*s}_{kg}}{H^{*tot}_g} \quad \left[H^{*tot}_g \begin{array}{l} \text{denotes the total sinking, now} \\ \text{assumed to be determined already} \\ \text{in the project description. Ac-} \\ \text{ceptance is not yet assumed.} \end{array} \right] \begin{array}{l} (k = h, \\ i, B) \end{array}$$

When the coefficients (7.5.1) are computed, any of the sinking flows in the project description can be expressed as

$$(7.5.2) \quad J^{*s}_{kg} = J'^s_{kg} H^{*tot}_g \quad (k = h, i, B)$$

where the coefficients J'^s_{kg} can be read off from the project description.

7.6. *Sinking Coefficients and Equivalence Coefficients in the Case of Substitution Possibilities for Sinking Inputs.* If we have the case where sinking substitution possibilities exist, the total outlay H^{tot}_g—the sum of all items in table (7.4.1)—does not exist as a magnitude that is defined *in the project description*. The sum of all items will then have a definite meaning only *after* optimization.

In this case the *sire* of the project in its full dress must - before optimization - be characterized by some other feature, for instance, by a *capacity addition* that may be associated with the project (if it is accepted in its full dress) or by some other *conventional* measure for the size of the project. Let this conventional

measure of the full dress size of the project be H_g^{*con}, the asterisk* indicating that this is a magnitude that can be read off from the project description, and con indicating that the magnitude is a conventional measure of the full dress size of the project.

Such a conventional measure may, of course, be introduced regardless of whether substitution possibilities exist or not, but in the substitution possibility case it is *necessary* to rely on such a conventional measure.

Even in the substitution possibility case there may be some, and perhaps *many,* but not all, of the interflows in table (7.4.1) that exist as well determined magnitudes already in the project description. And for *these particular flows we* can introduce a project description determined coefficient-concept by expressing each such flow as a fraction of H_g^{*con}, namely

$$(7.6.1) \quad J_{kg}^{'s} = \frac{J_{kg}^{*s}}{H_g^{*con}} \qquad \text{(For the sinking flows No. k - either h or i or B - that are determined already in the project description)}$$

The dimension of (the denomination of) each such coefficient (7.6.1) will depend on the nature of the input flow in question, and on the conventional measure that is chosen for H_g^{*con}.

For the cells of table (7.4.1) for which the flow is *not* determined already in the project description, we assume that we have *instead* information about *equivalence coefficients.* For instance, if the input elements in the three cells formed by the intersection of the three rows α, β, γ, and the column s of table (7.4.1) form a sinking input *substitution ring,* the three flows $J_{\alpha g}^s, J_{\beta g}^s, J_{\gamma g}^s$ are not determined in the project description, but we have instead information about three *equivalence coefficients* $J_{\alpha.rg}^{*eq.s} J_{\beta.rg}^{*eq.s}, J_{\gamma.rg}^{*eq.s}$ such that the three flows $J_{\alpha g}^s, J_{\beta g}^s, J_{\gamma g}^s$ must satisfy the equation

$$(7.6.2) \quad J_{\alpha.rg}^{*eq.s} J_{\alpha g}^s + J_{\beta.rg}^{*eq.s} J_{\beta g}^s + J_{\gamma.rg}^{*eq.s} J_{\gamma g}^s = H_g^{*con} \qquad \begin{array}{l} \text{(r = a substitution ring:} \\ \text{r} = (\alpha\beta\gamma) \text{ for sinking} \\ \text{input into the project g} \\ \text{in delay year s).} \end{array}$$

(eq = "equivalence", s = sinking delay).

Here H_g^{*con} is the conventional measure of the full dress size of the project No. g, and $J_{\alpha.rg}^{*eq.s}, J_{\beta.rg}^{*eq.s}, J_{\gamma.rg}^{*eq.s}$, are equivalence coefficients for each of the three sinking input elements α, β, γ that together form the substitution ring r = (αβγ) for sinking inputs into the project g in the sinking delay year s.

To take a very simplified example : Digging work connected with the project g in the sinking delay year s might be performed alternatively in any of the following three ways :

$(7.6.3)$ α = manual labour unaided by digging machines ("Chinese communes")

   β = use of small and simple digging machines

   γ = use of big and technically advanced digging machines

The meaning of (7.6.2) is that the amounts to be used of the three kinds

(7.6.3), namely $J_{\alpha g}^s$, $J_{\beta g}^s$, and $J_{\gamma g}^s$ are *not determined by the project description* but may be chosen freely, subject to the condition that the left member of (7.6.2) always be equal to the conventional full dress measure of the projects, namely H_g^{*con}. In other words, *before* optimization of the decision model we leave open the possibility that the necessary sinking input from the ring r to the project g in the sinking delay year s may be achieved either through the input element α or through β, or through y or through any *desired combination* of these three elements, which is such as to make the linear form in the left member of (7.6.2) equal to H_g^{*con}. In subsection 7.7 we shall consider a type of *restrictions* which it may be realistic to impose in addition to the equivalence equation, but for the moment we will only discuss the equivalence equation as such.

In the sinking substitution case the individual sinking flows such as $J_{\alpha g}^s$, $J_{\beta g}^s$, $J_{\gamma g}^s$ are not determined by the project description. They are only *variables* to be introduced in the complete decision model before optimization. Therefore, in the sinking substitution case the complete model will have more *degrees of freedom before optimization* than a similar model where no sinking substitution is permitted.

While the existence of input equivalence rings increases the number of degree of freedom in the decision model, it does not introduce any non-linearity. Indeed, the equation (7.6.2) is a *linear* equation. If the *effects* of the investment considered are to *change* the input-output coefficients, or any other coefficients in the model as it existed *before* the introduction of the substitution possibilities, then we will introduce non-linearities. This is the *infra* case.

7.7. The complementarity restrictions that may be associated with an equivalence ring. Consider again the example (7.6.3). In concrete reality even the most automatically advanced digging machine can, of course, not be let loose to perform the work alone without the aid of any manual labour. This fact may be taken account of in a number of more or less elaborate ways when we construct the complete decision model. But the *simplest* way to do it might be *still to use* the concept of equivalence equations as explained in Section 7.6, but now to complete this point of view by adding a certain type of restrictions which we may term *complementary restrictions*.

The meaning of the complementary restrictions can best be explained by changing slightly the definition of the input element α in the example (7.6.3), letting now α simply stand for "manual labour", i.e. dropping the specification "unaided by digging machines". Having changed our example in this way, we may add a restriction expressing the fact that *a part* of the variable $J_{\alpha g}^s$ has to be used as a *complement* to the variable $J_{\beta g}^s$ and *another part* of the variable $J_{\alpha g}^s$ has to be used as a complement to the variable $J_{\gamma g}^s$. If we want to *avoid* the complication which it would be to split the variable $J_{\alpha g}^s$ into *several* variables, we can express the essence of the complementarity situation considered simply by introducing a restriction of the form

(7.7.1) $J_{\alpha g}^s \geqslant$ some coefficient times $J_{\beta g}^s$ plus some coefficient times $J_{\gamma g}^s$.

We can formalize this idea by imposing a restriction of the form

$$(7.7.2) \quad J^{*\mathrm{com.s}}_{\alpha.\rho\mathrm{rg}} \, J^{\mathrm{s}}_{\alpha g} + J^{*\mathrm{com.s}}_{\beta.\rho\mathrm{rg}} \, J^{\mathrm{s}}_{\beta g} + J^{*\mathrm{com.s}}_{\gamma.\rho\mathrm{rg}} \, J^{\mathrm{s}}_{\gamma g} \geqslant 0$$

where

$$(7.7.3) \quad J^{*\mathrm{com.s}}_{\alpha.\rho\mathrm{rg}}, J^{*\mathrm{com.s}}_{\beta.\rho\mathrm{rg}}, J^{*\mathrm{com.s}}_{\gamma.\rho\mathrm{rg}}$$

are three coefficients *that are determined in the project description.* (ρ = "restriction", or more explicitly: ρ = a restriction associated with rgs. The superscript corn indicates "complementarity").

There may be *several* restrictions (ρ = 1, 2, 3 etc.) of the form (7.7.2) expressing, for instance, the fact that if we choose to use some big digging machines - input elements y - we may need also some small and simple digging machines - input elements β - as a complement to γ.

The fact that the *coefficients* J* in (7.7.2) are determined by the project description, does, of course, *not* mean that the actual flows $J^{\mathrm{s}}_{\alpha g}, J^{\mathrm{s}}_{\beta g}$ and $J^{\mathrm{s}}_{\gamma g}$ are also determined by the project description. They are still variables. But *if* we choose to put one of these actual flows equal to a given magnitude, any complementarity restriction of the form (7.7.2) will reduce the *admissibility range* for the other actual flows.

The formal set up (7.7.2) which introduces a set of restrictions ϱ associated with rgs is a very general one. It opens the possibility of expressing *a great variety of complementary* restrictions which we may find it necessary to introduce in order to make the complete decision model *realistic* enough to cover an actual situation. *The equivalence* equations express the fact that substitution possibilities exist, while the *complementary* restrictions express the limitations that exist on these substitution possibilities.

<div align="center">*</div>

In order not to abuse the editor's great generosity in allotting me space, I shall have to stop my little example here. Further considerations along this line are given in ref. (16). These concepts are finally worked into the complete decision model through the concepts of the *hypothetical starting variables* H. This involves many problems of a mixed theoretical and statistical sort. A survey of how the main magnitudes are book-keepingly interconnected is given in ref. (17).

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