AN OUTLINE OF MY MAIN CONTRIBUTIONS TO ECONOMIC SCIENCE

Nobel Lecture, December 9, 1988

by

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The Nobel Prize which has been awarded to me by the Royal Academy of Sciences of Sweden is a very great honour by which I am deeply moved.

I feel all the more honoured as for the first time this year the Prize-winner in Economic Sciences delivers his Nobel lecture before the Royal Academy of Sciences.

It has become a tradition that the Prize-winner should present the main contributions of his work which are directly related to the motivation of the Prize which, in my case, is my *"pioneering contributions to the theory of markets and efficient utilization of resources".* I should like to interpret this motivation in its broadest sense, that is to say, as relating to all those conditions which may ensure that the economy satisfies with maximum efficiency the needs of men given the limited resources they have at their disposal.

I

THE MOTIVATION OF MY CAREER AS AN ECONOMIST, MY 1943 BOOK, AND ITS SUBSEQUENT DEVELOPMENTS

The motivation of my career as an economist

The contributions I have made to Economic Science make up a whole; and they can only be understood in the light of the motivations which prompted my career as an economist.

Fascinated by History during my secondary education, then by Physics and Mechanics at the Ecole Polytechnique, I finally entered the national administration of mines in 1936.

My true inclinations, however, lay elsewhere, and isolated as I was in a provincial service, I devoted my leisure time to reading in the fields of physics and probability theory. I even undertook, in 1939, to write a general work on probability.

Then came the war and the defeat of France. In July 1940, after my

demobilization, I resumed again my duties as an engineer in the State mining administration at Nantes, in the zone occupied by the German army. But my pre-war concerns had completely changed. For me, with all the illusions of youth (I was only 29 at the time), it was clear that the best I could do was to contribute to prepare for the post-war period.

In the summer of 1933 I had visited the United States, then in the grip of the Great Depression, a very astonishing phenomenon for which no generally acceptable explanation had been found. I had also been very close to the social unrest which had broken out in France following the elections of 1936.

What could be a better way of preparing for the aftermath of the war than to try to find a solution to the fundamental problem of any economy, namely *how to promote the greatest feasible economic efficiency while ensuring a distribution of income that would be generally acceptable.*

Thus, my vocation as an economist was not determined by my education, but by circumstances. Its purpose was to endeavour to lay the foundations on which an economic and social policy could be validly built.

My 1943 book - "A la Recherche d'une Discipline Economique. L'Economie Pure "

Then, I bought, somewhat haphazardly, all the works on economics written by French authors, or by foreign authors translated into French, which I could find.

It was in those days that a period in my life began which today seems to me almost incomprehensible. How did I succeed in writing a book very dense and very structured, of about nine hundred pages, *"A la Recherche d'une Discipline Economique. L'Economie Pure"* (In Quest of an Economic Discipline, Part I, Pure Economics), in only thirty months, from January 1941 to July 1943, a work which your Academy expressly referred to in the statement of the reasons for its decision to award me the 1988 Nobel Prize in Economic Science?

Admittedly, the activity of the administrative service for which I was responsible and which covered five French departments had slowed down somewhat under the German occupation, but my duties required at least twenty-five hours each week. I had also to visit mines and quarries and to make frequent trips to Paris, some five hundred kilometers away.

At the time, I was no more than self-taught. In fact, my readings in economics had really begun only in July 1940. From the handwritten notes and, more particularly, from the dates which I used to inscribe on the books that I read and annotated, it follows, for example, that it was only between July 1940 and May 1941 that I read the fundamental works of Leon Walras, Vilfredo Pareto and Irving Fisher, the three great economists who have had the deepest influence on my thought.

How did I succeed in writing such a book in so short a time and in the particularly difficult conditions of the darkest years of the war and the German occupation of France? How was I able to complete the *"Introduc-*

tion" to this book by July lst, 1941, only one year after being demobilized? How was I able, within that same period of thirty months, to prepare efficiently the material for the five volumes which were to follow?

How did I succeed, at the same time, in finding a printer for the book, paper (very scarce at the time) for its printing, and in organizing a subscription in view of the refusal of all the publishers I had contacted to publish this book?

All this may appear totally unbelievable today, and yet all that work was effectively carried out and entirely completed within only thirty months.

No words, perhaps, can describe more aptly the sort of exaltation I experienced at the time than these extracts from a letter written by Leibniz: *"I wished to swim by myself, without any master. . . Frequently, in the light of a few lines encountered in my reading, I drew the substance of countless meditations . . . "* I should add that I was guided, although unwittingly, by the fundamental principle set forth by Abel, *"To read only the great masters in their original works".*

Thus, my 1943 book was the work of a mere amateur, but a passionate one, and were anyone to be somewhat surprised to learn that the Royal Academy of Sciences of Sweden had expressly referred to the work of an author who called himself an amateur, he would only betray an ignorance of all that amateurs have been able to contribute to science over the centuries. Indeed, within their ranks are to be found such great figures as Fermat, Leibniz, Lavoisier, Mendel, Pasteur, Louis de Broglie, and so many others, who, in their early careers, or even, in some cases, throughout their whole life, were only amateurs. Walras and Pareto were, themselves, only self-taught, only amateurs.

Amateurs are usually detested by professionals and members of any kind of *"establishment";* but they do possess one *very exceptional* advantage, that of never having been conditioned by university training and the constant repetition of *"established truths",* and, therefore, of being able to examine every question with a fresh eye, without any preconception and prejudice.

The developments following from my *1943 book*

As a matter of fact, my 1943 book was the starting point of all my subsequent publications, which have merely extended and complemented it in the various domains of the economic science at which I have worked, always pursuing the same aim: the rigorous foundation of a general theory in full agreement with empirical data.

Initially, as stated in its opening pages, my book was to be followed by five other volumes, the first three dealing respectively with the theory of interest, monetary theory, and the international economy; the fourth devoted to an analysis of the disequilibria of the real economy; and the fifth volume, of a normative nature, *'L'Economie de l'Avenir"* (The Economy of the Future).

This very ambitious project proved to be somewhat unrealistic and I thought it was preferable to abandon it. Much of the content of the second, third and fifth volumes was published in 1947 in a book entitled "Economic

et Intérêt" (Economy and Interest), which your Academy also expressly referred to. Once again, this publication was made possible only through a subscription.

Indeed, my entire work has been but the gradual realization of the program I set myself in 1943. This task is not yet completed, and it is still being carried on.

While my initial motivation was normative, while it corresponded, to a large extent, to the preoccupations expressed by Walras in his "*Etudes d'Economie Sociale*" (Studies in Social Economy), I have nonetheless constantly endeavoured to establish a clear distinction in my analyses between my work on the fundamental Economic Science and that on applied economics and political economy.

Throughout my work, my dominant concern has been with synthesis: to bring together into one comprehensive view the study of real and monetary phenomena; to associate the analysis of the conditions for efficiency and that of income distribution; to link closely theoretical analysis and applied economics; to relate economics to the other social sciences, Psychology, Sociology, Political Science, and History. Such have been my constant aims.

I believe that this concern with a synthesized conception of all economic and social phenomena constitutes the basis for all my thinking, and for the close connection between my work in theoretical economics and my work in applied economics. This concern explains what, it seems to me, constitutes the deep underlying unity in all my work.

My approach has never been to start from theories to arrive at facts, but on the contrary, to try to bring out from the facts the explanatory thread without which they appear incomprehensible and elude effective action.

On the whole, my work has been a response to the need I have felt to understand concrete reality and to provide satisfactory answers to the questions suggested to me by the obscurities, contradictions, and gaps in the existing literature. My work has thus been a long, and often painful endeavour to steer away from the beaten paths and dominant ideas of my time.

At the beginning of my career, my desire to understand was associated with a profound desire to act, with the wish to influence opinion and policy; but, over the years, this motivation has come to be of secondary importance, far behind my desire to understand.

II

MAIN CONTRIBUTIONS TO ECONOMIC SCIENCE

My contributions to fundamental economic analysis have dealt essentially with five fields, *all* related to the search for the conditions of the greatest possible economic efficiency and to the analysis of the income distribution it implies, fields in which I have been working continuously since 1941; the theory of economic evolution and general equilibrium, of maximum effi-

ciency, and of the foundations of "economic calculus"[1]; the theory of intertemporal processes and maximum capitalistic efficiency; the theory of choices under uncertainty and the criteria to be considered for rational economic decisions; the theory of money, credit, and monetary dynamics; and probability theory, as well as the analysis of time series and their exogenous components.

In each of these fields, I believe, I have freed myself from current conceptions, opened up original paths, and brought out new perspectives.

In the following I will limit myself to commenting *very brief* on those of my contributions which may be considered, in the sense of Alfred Nobel's will, to be either major contributions to fundamental Economic Science, or even, if I may say so, genuine discoveries.

Theory of Economic Evolution and General Equilibrium, of Maximum Efficiency, and of the Foundations of Economic Calculus

My work on economic evolution and general equilibrium, maximum efficiency, and the foundations of economic calculus has developed in two successive phases, from 1941 to 1966, and from 1967 to the present day.

To a large extent, my 1943 book, *"A la Recherche d'une Discipline Economique. L 'Economic Pure",* focused on the proof of*two fundamental propositions:* any state of equilibrium of a market economy is a state of maximum efficiency, and, vice versa, any state of maximum efficiency is a state of equilibrium of a market economy (equivalence theorems).

In the framework of Walras' model of the market economy, a rigorous proof of these remarkably simple propositions, which had already been foreshadowed by the classical economists, raises many difficulties. I believe that I gave, for the first time, a proof of great generality, for an economy considered at a given time but taking the future into account.

This proof takes into account the second order conditions and is free of any unrealistic hypothesis of general convexity regarding the production sets. It shows up the arbitrary character of the distribution of income. The formulation of discounted values appears as a condition of maximum efficiency.

In this work, I defined *four quite fundamental new concepts:* the concept of *the surface of maximum possibilities in the hyperspace of preference indices of the consumption units;* the concept of *distributable surplus* corresponding to a feasible modification of the economy from a given situation; the concept of *loss,* defined as the maximum distributable surplus for all feasible modifications of the economy which leave the preference indices unchanged; and the related concept of *surfaces of equal loss* in the hyperspace of preference indices.

During the winter of 1966-1967, I was led to discard the Walrasian general model of the market economy, characterized at any time, *whether there be equilibrium or not,* by a single price system, the same for all the

[1] In French: "calcul économique".

operators, - *a completely unrealistic hypothesis,* - and to establish the theory of economic evolution and general equilibrium, of maximum efficiency, and of the foundations of economic calculus, on entirely new bases resting *on the concept of distributable surplus* which I had elaborated and used in my 1943 book, and on a new model, *the model of the economy of markets (in the plural).*

According to this new approach, which had already been outlined in numerous developments of my 1943 book, the whole of economic dynamics in real terms is founded on *the search for, the realization and the distribution of surpluses.* This new approach allows remarkably simple proofs of the two equivalence theorems, without any restrictive hypothesis, whether it be of continuity, differentiability, or general convexity. It also allows the definition of a system of rules of the game whose implementation is likely to lead to states of maximum efficiency. When no surplus can be achieved, then there is general economic equilibrium, and maximum efficiency.

This theory is based on the *fundamental concepts of distributable surplus, loss, and surfaces of maximum efficiency and equal loss presented and developed in my* 1943 *book,* and it permits *the integration of* money into the theory of general economic equilibrium and maximum efficiency.

In its general principles, *my theory of surpluses generalizes the marginalist analyses of the nineteenth century* by considering not merely differential variations but also discrete variations, and by taking into account the complex interactions of the variations of *all* the quantities within the economy as a whole. It constitutes in fact a synthesis of the marginalistic approach of causality and the Walrasian approach of functional interdependence, two complementary approaches, the analysis of which I had already presented in a chapter of my 1943 book.

Not only does this theory give a realistic representation of economic dynamics, free from any useless hypothesis, but it also affords a better understanding of the true significance of the functioning of the economy under its twofold aspect of management and distribution, which it presents in a completely new light.

This theory lends itself just as well to the analysis of international trade as to the analysis of national economies, and just as well to the analysis of Eastern and Third World economies as to the analysis of Western economies.

I think that *my general theory of surpluses* constitutes *very considerable progress, and is indeed quite revolutionary, by comparison not only with all previous theories but also with all contemporary theories.*

Theory of inter-temporal processes and of optimal capitalistic structure

My book, *Economie et Intérêt* (Economy and Interest), published in 1947, presented a general theory of the efficiency of intertemporal processes with two particularly significant original contributions: the extension of the theory of maximum efficiency to the case where one considers the different generation, and the theory of productivity of capital.

Firstly, when future generations are taken into account, fundamental circumstances come to light which, *even today,* are very largely overlooked in the literature. Essentially, this analysis shows that, while the uniqueness of interest rates is a condition of efficiency in the production sector, it is not so in the economy as a whole.

It follows that the classical theory, according to which an equilibrium of the supply of and the demand for capital leads to an intrinsic optimum, is unacceptable, and that in particular a policy of compulsory saving for old age is perfectly compatible with conditions of intertemporal maximum efficiency.

Secondly, *my theory of the maximum productivity of capital* analyzes the influence of the more or less indirect character of production processes on the level of real national income, itself related to the wage rate of interest (i.e. the average value of the wages being taken as a unit). This theory has provided, *to my knowledge for the first time in the literature, a rigorous proof of the existence of a state of "maximum maximorum" for a stationary process. Such a state corresponds to a zero wage rate of interest.*

The theory rests on two new concepts: that of primary *income* (the overall value of wages and land rents), and that of the *characteristic function,* representing the intertemporal production processes, concepts I have elaborated by generalizing the penetrating analyses of Georges Bousquet, themselves inspired by those of Stanley Jevons.

This theory constitutes the basis on which all my subsequent works are founded. In the case of a dynamic process, I showed in 1961 that the *situation of "maximum maximorum" corresponds to the equality of the wage rate of interest and the rate of growth of the primary income* (golden rule of accumulation). I think I have given the *first general and rigorous proof of this theorem.*

To my knowledge, *of all the theories of dynamic capitalistic processes, the one I have presented is the only one which lends itself to numerical applications. It is fully confirmed by empirical data.*

Theory of choice under uncertainty and the criteria for rational economic decisions

My theory of choice under uncertainty stems from a twofold motivation: - the desire to extend the theories of general economic equilibrium and maximum efficiency to an economy with uncertainty; a critical analysis of the 1947 *Theory of Games* of von Neumann and Morgenstern, and the criteria for rational economic decisions.

In order to *generalize* the theories of general economic equilibrium and maximum efficiency to the case of risk, I showed in a 1952 paper how it was possible to take into account uncertainty with regard to the future, fields of choices under uncertainty, and operations relating to the composition of risks (a transposition in the case of uncertainty of production functions concerning the transformation of physical goods).

In the *Theory of Games,* von Neumann and Morgenstern presented both a method for determining cardinal utility and a rational rule of behaviour. Both are based on the consideration of an index which may be called the

neo-Bernouillian utility index. The theory devised by von Neumann and Morgenstern demonstrates the existence of this index from a system of postulates, and they identified it with cardinal utility in Jevons' sense. According to them, in order to be rational, any operator must maximize the mathematical expectation of this index.

This stance struck me as being unacceptable because *it amounts to neglecting the probability distribution of psychological values around their mean,* which precisely represents the fundamental psychological element of the theory of risk.

I illustrated my argumentation through counter-examples; one of them became famous as the *"Allais Paradox".* In fact, the *'Allais Paradox"* is paradoxical in appearance only, and it merely corresponds to a very profound psychological reality, *the preference for security in the neighbourhood of certainty.*

In order to test empirically the conflicting doctrines regarding rational behaviour in the face of uncertainty, *I carried out a survey in 1952* involving about a hundred subjects with good training in and knowledge of the theory of probability so that they could be considered to behave rationally.

It was not, however, until 1974 - 1976 that I was able to carry out a thorough analysis of the answers provided by the 1952 survey. This analysis fully confirmed the conclusions of my 1952 paper by showing that, *for every subject, there was no index, the maximization of whose mathematical expectation could explain the observed behaviour.*

Furthermore, this survey allowed me to show that, *for all the subjects analyzed, there exists an index of psychological value,* or cardinal utility, which can be determined independently of the consideration of any choice under uncertainty.

The corresponding cardinal utility function is *invariant* from one individual to another when one considers the *relative* variations of capital, and the knowledge of this function enables quantitative analysis of questions *which, up to now, have remained beyond the scope of any accurate calculation,* such as the psychological incidence of the transfer of wealth from the richest to the poorest, or the assessment of the psychological incidence of tax burdens.

Theory of money, credit, and monetary dynamics

Experience has shown that *there can be neither economic efficiency nor equitable distribution of income in an economy with monetary instability,* which is subject to major fluctuations, such as those corresponding to the Great Depression of 1929 - 1934. It was this fact which led me from 1941 onward to think over monetary phenomena, money, credit, and economic fluctuations.

In two papers published in 1954 and 1955, I presented a *non-linear* explanatory model of the fluctuations of global expenditure. The model is based on an equation that I have called the *fundamental equation of monetary dynamics,* and on a *hereditary formulation* of the demand for and the supply of money. Through this model, it is possible to express variations in global expenditure as a function of the difference between the supply of and the

demand for money, the latter being functionals of past variations in global expenditure.

It can be seen that, for values of parameters close to those provided by experience, the model presents limit cycles whose duration and amplitude are little different from those observed.

These two papers constitute the foundations on which I based my general theory of monetary dynamics in all its later developments.

In various papers presented between 1965 and 1987, I extended the previous results by specifying the functions of the demand for and supply of money on the basis of an entirely new formulation. This formulation is *hereditary* in so far as it determines the present as a function of the past, and it is *relativistic* in the sense that such dependence is *invariant* when physical time is replaced by *psychological time.* It postulates that the past is forgotten in the same way as the future is discounted.

This means that at any given moment in time the common value of the rate of forgetfulness and the rate of interest is itself an invariant functional of previous variations of global expenditure.

Today, my theory of monetary dynamics is based on four main pillars: the fundamental equation of monetary dynamics, and the three hereditary and relativistic formulations of the demand for money, the supply of money, and the rates of forgetfulness and psychological interest.

This theory essentially rests on *original guiding ideas* which are applicable to many fields, such as Economics, Psychology, Sociology and Political Science. They are: the fundamental analogy between forgetting the past and discounting the future; the herediatary psychological process of forgetfulness; the consideration of psychological time; the hereditary conditioning of men by past events; the hereditary propagation of monetary phenomena with a gradual weakening through time; the concept of lagged regulation implying the existence of limit cycles.

My theory of monetary dynamics is based on the introduction of new *concepts which have no equivalent in the earlier literature;* the concepts of the *psychological rate of interest,* the *rate of forgetfulness,* and the *reaction time,* whose values vary according to the economic situation; the concept of the *coefficient of psychological expansion* which represents the average appraisal of the economic situation by all economic agents; the concept of *psychological time,* the referential of psychological time being such that the laws of monetary dynamics remain invariant therein.

Once an appropriate measurement of the quantity of money is available, the empirical verifications of the new theory are *very remarkable. This is the only case I know of in the entire history of econometric research where a model which uses only one single explanatory variable, and includes only two arbitrary parameters, or even only one, according to the approach considered, has been able to provide, in cases so numerous and so different, such good results, far better indeed than those of all the other theories proposed before or after its publication.*

By revealing the existence of invariant effects of a hereditary and relativistic type in social phenomena, the new approach opens up wide perspectives, hitherto

unsuspected. The results obtained show that everything happens as if, irrespective of the institutional framework, contingent historical situations, and individual aspirations, people react in the same way, somewhat mechanically, to similar complex sequences of events. *They show that we are conditioned by our past, and they open up new perspectives in the general debate on determinism and free choice.*

Probability theory, and the analysis of time series and their exogenous components
Reflection on the theory of choice under uncertainty and the search for the fundamental factors underlying the fluctuations of time series, and particularly the fluctuations of the residuals of the best empirically verified models, led me to a critical analysis of the concept of chance and of probability theories, to the proof of a new theorem, the *"T Theorem"*, and to the introduction of a new concept, the *"X Factor"*, which represents the exogenous physical influence on time series.

The mathematical theories generally called *"mathematical theories of chance"* actually ignore chance, uncertainty, and probability. The models **they consider are** *purely deterministic,* and the quantities they study are, in the final analysis, no more than the mathematical frequencies of particular configurations, among all equally possible configurations, the calculation of which is based on combinatorial analysis. In reality, no axiomatic definition of chance is conceivable.

According to the *"X Factor"* hypothesis, the fluctuations in time series that we observe in physical, biological and psychological phenomena result, to a large extent, from the influence, through resonance effects, of countless vibrations which fill the space in which we live, and whose existence is by now a *certainty*. Thus, we can explain, to a large extent, the structure of the fluctuations, at first sight so incomprehensible, that we observe in very many time series, such as, for example, those in sunspots or in stock exchange quotations. In fact, these fluctuations present all the features of an almost periodic structure.

To such a structure there corresponds an *almost periodic function,* defined as the sum of sinusoïdal components of which certain have incommensurable periods. It follows from the *"T Theorem"* that under very general conditions, the successive values of an almost periodic function are normally distributed. *It is thus established that the deterministic vibratory structure of the universe can bring about seemingly random effects, and that determinism can generate what is commonly referred to as chance.*

III

MY CONCEPTION OF ECONOMIC SCIENCE

The above are the main original and innovative contributions which I believe I have made to fundamental Economic Science. Now I should like to sum up in a few words the principles which I have constantly followed in my work ever since my first book in 1943.

The fundamental criterion of experience

Firstly, *the prerequisite of any science is the existence of regularities which can be analysed and forecast.* This is for example the case in celestial mechanics. But it is also true of many economic phenomena. Indeed, their thorough analysis displays the existence of regularities which are just as striking as those found in the physical sciences. This is why Economics is a science and why this science rests on the same general principles and methods as the physical sciences.

All science is based on models and every scientific model comprises three distinct stages: statement of well-defined hypotheses; deduction of all the consequences of these hypotheses, and nothing but these consequences; confrontation of these consequences with observed data. Of these three stages, *only the first and the third* - establishing hypotheses, and confronting results with reality - are of interest to the economist. The second stage is purely logical and mathematical, that is tautological, and is only of mathematical interest.

The model and the theory it represents must be accepted, at least temporarily, or rejected, depending on the agreement or disagreement between observed data and the hypotheses and implications of the model. *When neither the hypotheses nor the implications of a theory can be confronted with the real world, that theory is devoid of any scientific interest.* Mere logical, even mathematical, deduction remains worthless in terms of the understanding of reality if it is not closely linked to that reality.

Submission to observed or experimental data is the golden rule which dominates any scientific discipline. Any theory whatever, if it is not verified by empirical evidence, has no scientific value and should be rejected.

This is true, for example, of contemporary theories of general economic equilibrium which are based on the hypothesis of general convexity of the fields of production, a hypothesis which is disproved by all the empirical data and leads to absurd consequences. That is also the case of the neo-Bernoullian theories of expected utility which are founded on postulates whose consequences are incompatible with observed data.

My approach has always been based on a twofold conviction: the conviction that, without theory, knowledge inevitably remains confused and that an accumulation of facts only constitutes a chaotic and unavoidably incomprehensible aggregate; and the even stronger conviction that a theory which cannot be confronted with the facts or which has not been verified quantitatively by observed data, is, in fact, devoid of any scientific value.

The illustration of theories by models

Secondly, I have always illustrated the general theories I have presented through particular models for which all calculations can be carried out explicitly.

My conviction is indeed that no general theory can be really understood if it is not illustrated by the consideration of particular models, judiciously chosen in order to make possible the analysis of all relevant circumstances,

and devised in such a way as to show up very clearly the implications of the hypotheses.

Just as there can be no scientifically valid theory which is not general and not applicable to all particular cases, so there is no general theory which can be fully understood if it is not illustrated by its application to particular cases. The more general a theory, the more its illustration by appropriate models ensures a full understanding of its significance and scope.

The confrontation of theories with empirical evidence and the search for invariants

Thirdly, my work has been marked by a growing concern for numerical applications based on numerical data provided by observation.

The empirical verifications of my hereditary and relativistic theory of monetary dynamics are quite remarkable; indeed they are the most extraordinary ones that have ever been found in the Social Sciences, and this is in a field essential to the life of society. In fact, the observed reality is represented in an almost perfect manner by the formulation to which this theory leads, whether it is applied to, for example, the United States during the Great Depression, the German hyperinflation from December 1919 to October 1923 (during which period the price index, on the basis of 1913 = 100, reached the value of 1012), or Soviet Russia's hyperinflation from January 1922 to February 1924. These results demonstrate the underlying existence of structural regularities in social phenomena which are *as striking* as those observed in the physical sciences.

I have been gradually led to a twofold conviction: human psychology remains fundamentally the same at all times and in all places; and the present is determined by the past according to invariant laws. It seems to me that, to a very large extent, the social sciences must, like the physical sciences, be based on the search for relationships and quantities *invariant in time and in space.*

Thus, whatever the economics considered, whether in the past or in the present, the whole human economic activity comes down to the search for, and the realization and distribution of surpluses according to fundamentally *invariant* processes.

My theory of intertemporal processes brings out an *invariant* structure as regards the relationship between production at any given time and the factors of production supplied in the past which may be considered to be the source of this production.

The analysis of the answers to the survey I undertook in 1952 led me to the conclusion that cardinal utility does exist, and that for all subjects this cardinal utility can be represented by an invariant function of the relative variations of their capital.

The theory of monetary dynamics which I have elaborated rests on the consideration of a hereditary link, *invariant in time and space,* between the present and past evolution. The results show that human societies, within very different contexts - whether they correspond to current situations, either inflationary or deflationary, or to hyperinflations, to capitalist or

communist countries, either today or a century ago - behave in a similar way. Thus, the general study of our conditioning by the past may be founded on this basis, and the hereditary and relativistic formulation which I arrived at may be used in numerous applications in all fields of the human sciences.

The use of mathematics

Fourthly, I have been constantly led to use mathematics in all the cases where ordinary logic was manifestly insufficient for the analysis of economic phenomena, which are essentially quantitative, and often very complex. This use enabled me to provide rigorous solutions to problems which would otherwise have been intractable on account of their complexity.

However, mathematics is not and cannot be anything more than a tool, and all my work rests on the conviction that, in its use, *the only two really fruitful stages in the scientific approach are, firstly, a thorough examination of the initial hypotheses; and, secondly, a discussion of the meaning and empirical relevance of the results obtained.* What remains is but tautological calculation which is of interest only to the mathematician, and the mathematical rigour of the reasoning can never justify a theory based on postulates if these postulates do not correspond to the true nature of the observed phenomena.

The use of even the most sophisticated forms of mathematics can never be considered as a guarantee of quality. Mathematics is, and can only be, a means of expression and reasoning. The real substance on which the economist works remains economic and social. Indeed, one must avoid the development of a complex mathematical apparatus whenever it is not strictly indispensable. Genuine progress never consists in a purely formal exposition, but always in the discovery of the guiding ideas which underlie any proof. It is these basic ideas which must be explicitly stated and discussed.

Mathematics cannot be an end in itself. It can be and should only be a means.

New ideas and the tyranny of dominant doctrines

Finally, I have never hesitated to question commonly accepted theories when they appeared to me to be founded on hypotheses which implied consequences *incompatible* with observed data.

Indeed, it is only through the constant questioning of *"established truths"* and the blossoming of new ideas suggested both by empirical evidence and by creative intuition, that science can truly progress. But all genuine scientific progress comes up against the tyranny of the dominant ideas generated by the *"establishment "*. The more such dominant ideas are taken for granted, the more they become rooted in the psychology of men, and the more difficult it becomes to gain acceptance for a new conception, no matter how fruitful it may later turn out to be.

Dominant ideas, however erroneous they may be, end up, simply through

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