AN OUTLINE OF MY MAIN CONTRIBUTIONS TO ECONOMIC SCIENCE

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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.

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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-*

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 10^{12}), 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 continual repetition, by acquiring the quality of established truths which cannot be questioned without confronting the active ostracism of the *"establishment "*. The examples of Copernicus, Galileo, Abel, Galois, Pasteur, Arrhenius, Wegener, and many others, demonstrate the obstacles encountered by discoverers of genius.

It is this resistance to new ideas that explains why in Economics it took so long before the major contributions of Dupuit, Walras, Edgeworth, Pareto, and many others were acknowledged. Walras' *Elements d'Economic Pure* were not translated into English until seventy-five years after its publication. Some sixty years were to elapse before Pareto's *Manuel d'Economie Politique* was to be published in English.

The successful scholar is always the one who adds some marginal improvement to the dominant theories to which everyone is accustomed. If, however, a new theory falls outside established paths, it is certain to face general opposition whatever its justifications.

For all of these reasons, it is essential to subject *"established truths"* constantly to a critical analysis without indulgence, while always bearing in mind Pareto's statement: *"The history of science boils down to the history of errors of competent men".*

Whatever the price he might pay for it in his career, the scientist should never steer his course according to the fashions of the day, or of the approval or disapproval of his contemporaries. His sole concern must be with the quest for truth. This is a principle from which I have never departed.

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Finally, I should like to express once more my deep gratitude for the very great distinction which has been awarded to me.

As Walras once wrote, the true scholar undoubtedly seeks truth for its own sake, but he cannot be insensitive to the recognition of the value of his work. Whatever they may have said, the most eminent scientists have never remained completely indifferent to the opinions of others.

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