The passion for research*

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At first sight Michael Szenberg's invitation, in the name of the *American Economist,* to write a twenty-page essay on "my life philosophy" seemed very tempting. To give an overview of my economic, social, and political ideas, together with my career and scientific works, and to finish with my views on the nature of life and the physical world, appeared to be a fascinating task. So I accepted, probably very rashly.

In a first draft, in fact, I strove to respond to this idea, and to connect my life philosophy to the influences that determined it and which are inseparable from it: my social origin, the education I received, my professional experience, the para-political activities I was involved in, my works in fundamental and applied economics, my parallel interests, my conceptions of the physical world - all closely interdependent.

But just as this general conception was simple to define, just as it appeared easy to carry out, provided an entire book were to be devoted to it, so it became apparent that even in thirty pages it would be impossible to respond to this general conception, so vast was the subject. I would have been forced into difficult and certainly very arbitrary choices and over-simplifications, which truthfully are incompatible with the many and relatively complex aspects of my life philosophy, and therefore completely unacceptable.

I have limited myself therefore, to a brief analysis of the factors determining my scientific vocation, the main philosophical lines of the original contributions in my works, and the philosophy of my conception of science and economics.

I have deliberately avoided presenting and analysing my views on political fundamentals of life in society, on the different aspects of national and international political economics,

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and finally on the physical universe, all fascinating subjects which I have analyzed and worked on for over forty-five years, and which have resulted in many publications.

I do realize the regrets that these limitations may cause for any reader interested in knowing all aspects of my personality, but essentially, my life has been identified with my scientific work, and it is this which seems most important to me from the viewpoint of my life philosophy.

The determining factors in my scientific vocation

My youth

I was born May 31, 1911, in Paris. My parents owned a small cheese shop, and my maternal grandfather was a carpentry worker. I thus came from what is commonly known as the working class.

In August 1914 my father was called to war, and then taken prisoner. He died in captivity in Germany, on March 27, 1915. My youth, indeed my entire life, was deeply marked by this, directly and indirectly.

Albeit in often difficult conditions, I was nevertheless able to pursue my secondary studies. I received my high school baccalaureate diploma in Latin and Science in 1928, then my two baccalaureate diplomas in mathematics and philosophy in 1929. Throughout my college career I was generally first in my year in almost all subjects, including French and Latin as well as mathematics.

Fascinated by history, I wanted to apply to the Ecole des Chartes, but on the insistence of my mathematics teacher I entered the special mathematics class in order to prepare for the Ecole Polytechnique, which I entered in 1931. I graduated first in my class in 1933, which is commonly considered to be as a "*summum*" in France. Indeed the Ecole Polytechnique, together with the Ecole Normale Supérieure, are the top of French education in the sciences.

My choice of administration agencies upon graduation was the Corps National des Mines, not because of any particular vocation, but simply because each year the top graduates of the Ecole Polytechnique (three in my class) always chose this government service because of the career possibilities it opened up in the country's large industrial enterprises.

After a year of military service in the Artillery School first at Fontainebleau, and then in the Alpine Army, and two years at the Ecole Nationale Supérieure des Mines in Paris, I started as an engineer in the Mines public service in October 1936.

My professional career

In 1937, at the age of twenty-six, I found myself in charge of the Nantes mines and quarries service, which included five of the eighty-nine French "departments,"[1] and also put in charge of a number of controls, in particular that of the general and local railway system.

In 1939 I was called back to the Alpine Army on the Italian front, and was given command of a heavy artillery battery in the area of Briançon. But the real war only lasted two weeks, from June 10, 1940, when Italy declared war on France, until June 25, 1940, the date of the armistice.

Released from service, I took up my old position in Nantes in July 1940 in the German occupation zone. From October 1943 to April 1948 I was director of the Bureau of Mines Documentation and Statistics in Paris.

From January 1941 to April 1948 I simultaneously carried out my administrative functions and published my first works: two fundamental works, *A la Recherche d'une Discipline Economique* (In Quest of an Economic Discipline) (1943) and *Economie et Intérêt* (Economy and Interest) (1947); and three minor works, *Economie Pure et Rendement Social* (Pure Economics and Social Efficiency) (1945), *Prolégomènes à la Reconstruction Economique du Monde* (Prolegomena for the World Economic Reconstruction) (1945), and *Abondance ou Misère* (Abundance or Misery) (1946), as well as various topical articles. Throughout this period I worked very hard, at least eighty hours per week.

From April 1948 on, I was relieved of all administrative duties and was able to devote all my time to teaching, research, and writing for publication. I was professor of economic analysis at the Ecole Nationate *Supérieure* des Mines in Paris from 1944 on, and director of a research unit at the Centre National de la Recherche Scientifique (C.N.R.S.) from 1946 on. At various times I have also held teaching positions at other institutions, such as the

1 A department is a French administrative district.

Institute of Statistics at the University of Paris (1947-1968), the university of Paris-X (1970-1985), the Institute of International Studies in Geneva (1967-1970), and the Thomas Jefferson Center of the University of Virginia as a Distinguished Visiting Scholar (1958-1959).

I retired from the civil service on May 31, 1980, but have been able to continue to work very actively in teaching, research, and writing.

I have received many awards for my works (fourteen scientific prizes from 1933 till 1987). The most important was the Gold Medal of the National Center for Scientific Research (C.N.R.S.), the most distinguished honor in French Science. It was awarded to me in 1978 for my lifetime work, the first and so far the only time an economist has ever received this honour.

My involvement in applied economics and politics

In addition to the above activities 1 have undertaken economic studies for both private and nationalized firms, and for the European Economic Community.

Throughout the years following World War II and until the formation of the European Economic Community in 1958, I was very active as a national or international rapporteur at many of the international conferences aiming to establish a European Community. I worked also for the foundation of an Atlantic Community and 1 wàs a rapporteur at the "NATO in Quest of Cohesion" international conference organized in 1964 in Washington by the Center of Strategic Studies at Georgetown University.

Finally, from 1959 to 1962 1 was also founder and general delegate of the Movement for a Free Society, a liberal (in the European sense of the term) para-political organization.

My two parallel interests

During my entire career, since 1936, I have been actively involved in two fields: physics and history.

In physics, on the theoretical level, I have worked at different times on trying to develop a unified theory of gravity, electromagnetics, and quanta. On the experimental level, and as a by-product of this theoretical research, I conducted research from 1952 to 1960, at first in a small private laboratory, then in a laboratory which was specifically assigned to me at the Steel Industry Research Institute, on the anomalies of the paraconical pendulum whose existence I proved. The workload of this research, conducted in addition to my work as an economist, was as heavy at it was fascinating. For these experiments I received the Galabert Prize in 1959 from the French Astronautical Society, and I was laureate in 1959 of the Gravity Research Foundation in the United States (on these experiments see the Appendix).

In history, I wrote from 1961 to 1966 the first version of a book: *Essor et Déclin des Civilisations - Facteurs Economiques* (Rise and Fall of Civilizations - Economic Factors), which I have continued to improve and develop at different times over the past twenty-five years. This work, as ambitious as it is daring, tries to draw out permanent regularities, particularly quantitative, from the history of civilizations, dealing with economic systems, standards of living, technology, monetary phenomena, demographic factors, inequality and social classes, the respective influences of heredity and environment, international relations, exogenous physical influences on human societies, and political systems.

My scientific vocation

My vocation as an economist was not determined by school; it was the economic reality I faced in the first years of my working life that decided it: my career as a state engineer, the intellectually shocking and socially dramatic Great Depression. the stress of social problem, and the conviction that objective economic analysis could contribute to their solution.

However, during these past forty-eight years, the evolution of my thinking has been very similar to Pareto's. I am now more concerned with understanding what men do than with convincing them. I still believe that certain policies are better than others, but more and more I think that men are motivated by their interests, their prejudices, their passions, and that logic, even scientific, really has no hold over what they do.

The inspiration for my scientific works derived simultaneously from: meditation upon the works of Leon Walras, Irving Fisher, and especially Vilfredo Pareto, three great masters who deeply influenced me; reflection on the economic and social history of recent centuries; thorough study of the applied economic questions I had been asked to investigate; and forty-five years of courses and seminars, an incomparable source of fruitful discussions.

I was involved in my two parallel interests much earlier than in my activities as an economist. During my secondary school studies, 1 was fascinated by history This fascination has never left me. My involvement in physics dates from my reflections on physics, mechanics, and astronomy courses at the Ecole Polytechnique. Had the National Center for Scientific Research existed in 1938, I would have devoted myself to the study of physics and would not have become an economist. And in fact, over the past fifty years, I have never stopped reflecting and working on the problems involved in the elaboration of a unified theory of physics.

My scientific career was certainly determined by my basic nature, but, in fact, outside factors determined the directions it took, and what is commonly known as chance always played a large part: the element of chance in life's circumstances, in those we meet, in the authors we discover through reading and on whose work we reflect. It is this very complex chain of inter-connected multiple causes which really led to my researches and publications.

The philosophy underlying my works on the essence of economic phenomena

What I have tried to do

All my researches, all my works on fundamental and applied economics, have been dominated by the concern to understand concrete reality and elaborate a theoretical synthesized analysis capable of helping, if not bringing about, this understanding. In the beginning, my desire to understand was associated with a profound desire to act, with the concern of influencing opinion and politics; progressively, however, this motivation became of secondary importance, far behind my desire to understand.

In the final analysis, everything I have done has been motivated by the need I felt to answer questions suggested to me by the obscurities, contradictions, and gaps in existing literature as regards observed reality. It has thus been a long and often laborious effort on my part to detach myself from the established tracks and dominant ideas of my time.

My approach has never been to start from theories to arrive at facts, but on the contrary, to try to bring out from facts an explanatory thread without which they appear incomprehensible and elude effective action. I have always established a close interdependence between theory and application; for me the only purpose of economic theory is the explanation of concrete phenomena and the analysis of conditions likely to assure maximum utilization of limited resources to satisfy practically unlimited needs.

Whether dealing with theoretical economic analysis or applied economics, consideration of human psychology has always appeared fundamental to me. What are the factors which determine it, and to what degree does it determine the evolution of the real world? These are the two essential questions that 1 have always endeavored to analyze, searching for the invariant relations that characterize them.

The fundamental structure of economy

As regards the functioning of the economy, I have tried to bring our the fundamental factors of any economic system, and my contributions have essentially been aimed at *five highly interdependent* fields in which I have worked continuously since 1941. the theory of maximum economic efficiency, the theory of capitalistic intertemporal processes, the economic theory of uncertainty, the theory of monetary dynamics, and the theory of chance and exogenous physical influences. In each of these fields I think I have freed myself from currently accepted ideas, introducing new concepts and formalizing new theories likely to give a better representation and understanding of reality.

The achievement of an economic situation of maximum efficiency. First of all, in my first book in 1943, A *la Recherche d'une Disipline Economique,* I demonstrated with great generality the equivalence of situations of general economic equilibrium and situations of maximum efficiency. This analysis is based on the consideration of three new concepts: the concept of *surface of maximum possibilities* in the hyperspace of preference indexes; the concept of *distributable surplus for the whole economy,* a concept fundamentally different from the concept of surplus usually considered in the literature; and the concept *of economic loss* defined as the maximum value of the distributable surplus for all the modifications of the economy which leave the preference indexes of the operators unchanged.

From 1966 on, breaking away totally from the generally accepted theories and the Walrasian model on which they are based, I developed a new theory which bases the whole of economic dynamics in realterms *on the search for, achievement of, and distribution of surpluses.* The associated model is the model of the economy of markets.[2] This theory is free of any unrealistic hypothesis of continuity, derivability, and convexity. As regards the quest for situations of maximum efficiency, this approach replaces the search for a certain price system - the same for all operators by the search for a situation in which no surplus can be achieved. The concept of price becomes secondary in the analysis and no longer plays but a subsidiary role. *It is the concept of surplus which plays the primary role in the new formulation.*

2 "Markets": in the plural.

Not only does this theory, which generalizes the classical marginal approach to the most complex cases while simplifying its principles, give a realistic representation of economic dynamics in real terms, free from any artificial and useless hypothesis, but it allows a better understanding of both the profound nature of economic calculus and 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 as well to analysis of international exchanges as it does to analysis of national economics, as well to the analysis of Eastern bloc countries and the Third World, as it does to the analysis of Western economics, and as well to the economics of the past as it does to the economics of today.

I think that this formulation constitutes considerable progress by comparison with all preceding theories. In any case, it allowed me to overcome the major difficulties which faced me from 1940 to 1966 in my works on applied economics.

Capitalistic intertemporal processes. As regards the *theory of capital* I developed in 1947. and later generalized, a theory of capitalistic processes and maximum capitalistic efficiency which is based on three new concepts: *the primary income, the characteristic function, and maximum capitalistic efficiency.*

In Economie et Intérêt published in 1947 I presented, to my knowledge for the first time in the literature, a rigorous demonstration of the existence of a situation of *maximum maximorum* (i.e., assuring a maximum real consumed income *per capita*) for a stationary process. Such a situation corresponds to a zero rate of interest.

I later generalized the theory of maximum capitalistic efficiency to the case of a dynamic process, and showed in 1960 that in this case the *maximum maximorum* is reached when the interest rate and the rate of growth of primary income are equal (*the golden rule of accumulation*).

I think I have given the first general and rigorous demonstration 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. And it is *entirely confirmed by observed data.* An application of this analysis is given in my study of the influence of capital on the difference in average productivity between the United States and France in the 1950.

The economics of uncertainty. The analysis of the fundamental factors underlying uncertainty of the future led me to: a critical analysis of the neo-Bernoullian theories of choice under uncertainty generally accepted after von Neumann-Morgenstern, Marschak, Samuelson, and Savage, and in fact contradicted by the behaviour of people in the neighborhood of certainty; the elaboration of a positive theory of choice under uncertainty in conformity with observed data; the definition and application of a direct method allowing the existence of cardinal utility to be demonstrated and its measurement; and finally, the generalization of the theories of general equilibrium and of maximum efficiency to the case of risk.

One of the counter-examples to the neo-Bernoullian theories that I presented in 1952 became famous under the denomination of the *Allais Paradox. In* fact, this Paradox is only paradoxical in appearance, and it simply corresponds to a very profound reality, the preference for security in the neighborhood of certainty.

In my 1955 *memoir Method of Appraising Economic Prospects of Mining Exploration over Large Territories Algerian Sahara Case Study,* I applied my theory of choice under uncertainty. The guiding principle was to offer the Mining Research Office of Algeria a reasonable compromise between the mathematical expectation of gains that might be expected and the probability of ruin. For this memoir I received the Lanchester Prize 1958 of the Johns Hopkins University and the Operations Research Society of America.

Monetary dynamics. My analysis of the fundamental factors underlying monetary dynamics has essentially dealt with: the general theory of monetary dynamics; the theory of economic fluctuations; the hereditary and relativistic theory of demand and supply for money, and of the psychological rate of interest; the analysis of the structural links between growth and inflation, especially as regards the capitalistic optimum; and finally, the implications of the creation of money and purchasing power through the credit mechanism.

My major contribution has been the development of the *hereditary and relativistic theory of monetary dynamics.* This theory is based on four pillars: the fundamental equation of monetary dynamics and the three hereditary and relativistic formulations of demand and supply of money and the psychological rate of interest. It is essentially founded on new guiding ideas applicable in numerous fields - economics, psychology, sociology, and political science: the hereditary process of forgetfulness, the fundamental analogy between forgetfulness and interest, the consideration of psychological time, the hereditary conditioning of men by past events, the hereditary propagation of monetary phenomena with a progressive weakening through time, and the existence of a lagged regulation generating limit cycles.

This theory is based on the introduction of new concepts which have no equivalent in the prior literature: the concepts of the *rate of forgetfulness and the time of reaction,* for which the values vary according to the economic situation; the concept of the **coefficient** *of psychological expansion* representing the average appraisal of the economic conjuncture by all economic agents; the concept of *psychological time,* the frame of reference of psychological time being such that the laws of monetary dynamics remain invariant therein.

The empirical verifications of the new theory of demand for money are remarkable; indeed they are the most extraordinary that have ever been found in the social sciences, and this in a field essential to the life of societies. In fact, 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 of December 1919 to October 1923 during which period the price index reached a value comparable to the speed of light measured in centimeters per second, or Soviet Russia from January 1922 to February 1924. *These results demonstrate the underlying existence of structural regularities in social phenomena* which are as striking as those we observe in the physical sciences.

I believe this is the only case in the whole history of econometric research where a model which utilizes only *one explanatory variable,* and which generally includes only *two arbitrary parameters, or even only one according to the approach considered,* has been able to provide such results in so numerous and so different cases.

By revealing the existence of *invariant effect of a hereditary and relativistic type* in social phenomena, the new approach opens up wide perspectives, almost unsuspected up until now. The results obtained show that *everything happens as if,* irrespective of the institutional framework, contingent historical situations, and their particular aspirations, people react in the same way, as it were mechanically, to identical complex sequences. They *show that we are conditioned by our past,* and they open up new perspectives in the general debate over determinism and free will.

Chance and exogenous physical influences. The reflection on the theory of random choices and the search for the fundamental factors underlying the fluctuations in time series led me at the same time to a critical analysis of the concepts of chance and the theories of probability, to the demonstration of a new theorem, the *T Theorem,* and to the consideration of a new concept, the *X factor,* representative of the exogenous physical influences on time series'

Actually, the mathematical theories generally designated as *mathematical theories of chance* do ignore chance, uncertainty, and probability. The models they consider are *purely deterministic* models, 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. The axiomatic theory of *probability* makes absolutely no use of the concept of chance, and it is inconceivable that it could do so. The concepts of probability and chance *are only creations of the human mind.* and are unknown to both nature and mathematics. They are totally absent from the currently accepted theories of probability, as can be clearly seen as soon as we examine *the sub-stance, and not the semantics* that they use in an absolutely unjustified manner.

According to the hypothesis of the X factor the fluctuations in time series that we observe in phenomena occurring in the physical, biological, and human sciences result in some part from the influence, through effects of resonance, of countless vibrations which are scattered through the space in which we live, and whose existence is by now a certainty. Thus we can explain, at least partially, the structure of the fluctuations, at first sight so incomprehensible, that we observe in a large number of time series, such as, for example, those in sunspots or in stock exchange quotations. In fact, these fluctuations present all the aspects of an almost periodic structure.

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

With regard to this analysis, the interest in the search for hidden periodicities which at one time dominated a large part of the literature now reappears under a new light. For this research I developed a test generalizing in the case of auto-correlated series, the *general case in observed reality,* the classic Schuster test, whose application is limited to the case of time series with independent terms.

The confrontation of theories with empirical evidence and the quest for invariances

Taken as a whole, all my research into the structural regularities underlying observed reality has been marked by a growing concern with numerical applications using data provided through observation. I believe that the only source of knowledge is, and in fact has to be, experience. A theory which applies to a quantitative area can only be valid if it can be verified quantitatively.

Numerical applications, more and more numerous, crowned with success, especially in the theories of surplus, capital, choice under uncertainty, monetary dynamics, and time series, have progressively convinced me that there is a very great internal coherence and an underlying in-variant structure in observed data, and that this offers extremely valuable guidance to the economist.

Implications of economic analysis for economic policy

During all my career, I have always been motivated by the conviction that a scientist cannot remain disinterested in the fundamental problems of his time. Indeed, I have never ceased to believe that whether he advises or teaches, the economist *as such* should not take a position on individual aims which for the most part are not compatible. The aims to be pursued belong to the political field, and in fact, it is the essential task of political systems to define them through general compromises. But precisely, in economics, the role of the economist is to examine whether the aims defined through these compromises are in fact compatible with each other, and whether the means used to achieve them are actually the most appropriate.

On the whole, in theoretical analysis as in applied economics, I have endeavored in my work to rethink the role of economic freedom and of the market economy as regards the attainment of economic efficiency and the achievement of the ethical objectives of our time, and to contribute to a thorough study of the questions arising from the economic organization of societies.

How can economic efficiency, which conditions the success of any social policy, be achieved? Can it be achieved without compromising an equitable distribution of production? What is the connection between inflation and the creation of money? Can inflation be considered as a condition for growth, or not? What are the causes of unemployment? For any given country, which conditions would be most likely to bring about maximum per capita real income? Is it, or not, in the interest of a given country to protect its economy from foreign countries? Can deflationary or inflationary economic fluctuations, which tend to destabilize the very foundations of our society, really be avoided? What are the monetary conditions of an economy of markets?

Can the changes implied by technical and economical progress - which conditions all improvements in our life - be made socially and humanly acceptable? What are the factors which determine inequality and social classes? What is the respective influence of heredity and environment? What kinds of income transfers are desirable, and under what form? What should taxation be? What role should the State play in the working of the economy?

From the point of view of economic analysis, historical lessons, and the observation of contemporary facts, how can one judge economic systems which are founded on liberty, decentralization of decision making, and private property, as against centralized economic planning and collectivise property? Which institutional economic framework appears to be the most favorable to social progress and human advancement? Can an international economic order be achieved which would contribute to the development of all countries and to the creation of international peace?

In the light of these different analyses, what measures or reforms appear most suited to each case? These are the questions to which I have endeavored to give *precise reasoned-out answers* in the field of applied economics, through theoretical analysis and the study of observed data.

For any question in applied economics, the analysis of a given situation *is always very complex,* because of the interdependence of numerous factors, multiple chains of cause and effect, specific historical conditions, and social and political implications. The inevitable result is that any applied economic analysis contains, *explicitly or implicitly,* a share of value judgements, a characteristic which is even more marked when the analysis leads to normative recommendations.

From this viewpoint my thinking has unquestionably been greatly influenced by a philosophy of liberal (in the European sense of the word) inspiration, along the lines of Alexis de Tocqueville, Leon Walras, Vilfredo Pareto, and John Maynard Keynes, to name but a few. But whatever this influence was, I always endeavored to keep my applied economic analyses as scientific as possible, following two principles: first, to constantly found them on in-depth theoretical investigation; second, always to provide accompanying quantitative estimates.

On a national level, my work in applied economics has dealt with economic management, income distribution and taxation, monetary policy. and, in energy, transportation and mining research policies.

At the international level, my work has examined comparative standards of living and productivity· development factors; the monetary conditions for the efficiency of international trade; and also, economic unions and liberalization of economic relations.

Finally, together with my works in applied economics I have tried to study the meaning and the implications of the different political systems, and published various studies on the sociological and political aspects of life in society, on liberalism and socialism, on democracy and totalitarianism, on inequality and social classes, especially with regard to the respective influences of heredity and environment. These works analyze the general framework and the structural conditions in which the economy functions, and strive to clarify the interaction between political and economic systems.

The contributions of my parallel interests to my economic philosophy

My research on the history of civilizations. My research into the economic and social factors of the history of civilizations was for me extremely illuminating. Nothing is more instructive than the history of facts, doctrines, and economic thought. Whether it be economic systems, the evolution of real incomes, monetary phenomena, demography, international relations, ideology, or the interaction between these factors and their chains of cause and effect, nothing is more significant.

I cannot but side with Schumpeter here. If, *in order to understand economics,* one had to choose between mastering economic history or mastering mathematics and statistics, there is no doubt that one must choose the former. But the best for an economist, of course, would be this dual mastery. This was the case for Vilfredo Pareto, the greatest economist the world has produced to date, who was able to bring together with penetrating intelligence a wide command of the mathematics of his time and a truly extraordinary command of the history of societies since Greco-Roman antiquity.

My work in theoretical and experimental physics. My studies in theoretical and experimental physics, which at first seem so far removed from my main activity as an economist, in reality enriched me with extremely valuable experience.

These researches, which constantly presented all kinds of very great difficulties, led me to reflect on the nature of our knowledge, the nature of experience and theory, the difficulties of experimentation and interpretation of results, and the scientific method in general. More than ever, I am convinced that the only source of knowledge is experience, and that any theory is worthwhile only insofar as it forms a usable condensed synthesis of experience.

I was particularly struck by the *identity* of problems relating to model building, and the meaning of experimental data in economics and physics. One of the major problems that I had to solve in my experimental research in physics was to test the reality of given periodicities in the anomalies of the movement of the paraconical pendulum, the structure of which seems to be almost periodic. This problem is, in fact, identical to the one dealt with by economists in their works on economic cycles and their research into "hidden periodicities." This example has general validity: all econometric studies present methods of analysing time series which apply equally to geophysics. Likewise geophysicists have studied analogous problems, and the methods they have developed can only be of the greatest benefit to economists.

Nothing was more instructive for me than this confrontation between the two sciences, apparently so dissimilar. I think that the very deep, and indeed invaluable, influence that my experimental and theoretical physics, research had on me considerably improved my work and my teaching in economics, by helping me better to understand the nature of economic science, and contributing to an improved presentation of all my work.

My preoccupation with synthesis

Taken as a whole my work has covered very different fields, but has always been inspired by the *same conception.* During the course of my whole career, my dominant concern was with synthesis: to bring together into one comprehensive view the study of real and monetary phenomena; to closely associate theoretical analysis and applied economics, to link economics to the other social sciences, psychology, sociology, political science, and history. Just as physics requires a unified theory of universal gravitation, electromagnetics, and quanta, so do the social sciences need a unified theory of human behavior.

First of all, on the purely economic level, I have always endeavored to develop a comprehensive theory of economic phenomena which presents their different aspects coherently, connects deductions to principles logically and rigorously, and allows a constant linkage between theory and application. All my works are closely interdependent and complementary. Theoretical analysis led me naturally into applications, and the study of concrete economic questions led me to consider the theoretical foundations which bring satisfactory answers within reach.

Moreover, I have been guided always by the principle that Economics is but a part of a whole, and that any concrete economic decision not only has a quantitative character, but also a human aspect, and is part of a historical context. In numerous studies, I emphasized that no valid solution to economic problems can be found solely using economic theory and quantitative aspects of social life. Analysis of societies obviously requires a synthesis of all the social sciences: political economics, law, sociology, history, geography, and political science, and I specifically tried to bring out certain essential aspects of this synthesis in several studies on the working of democracy, the balance of the different powers and the decentralization of economic power, and the competition for power, and the essential role of elites and of social mobility.

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

My philosophy of scientific method and economic science

The fundamental criterion of experience

The essential condition of any science is the existence of regularities which can be analyzed and forecast. This is the case in celestial mechanics. But it is also the case, for a great part, of social phenomena, particularly economic phenomena, which, when analyzed thoroughly, display the existence of regularities which are just as striking as those we find in the physical sciences. This is why economics is a science, and why this science rests on the same general principles and methods as physics.

All science is based on models, whether descriptive, explanatory, forecasting, or decision making, and every scientific model entails three distinct stages: start with well-specified hypotheses, deduce from these hypotheses all their implications and nothing but these implications, and confront the implications 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* of mathematical interest only.

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

Submission to experimental data is the golden rule which dominates any scientific discipline. It explains the extraordinary success of Western thought in the last three centuries. This rule is the same in economics as in physics. No theory whatever can be accepted unless it is verified by empirical evidence.

Abstraction plays an essential role in the construction of theories and their models. The role of science, in fact, is to simplify and to choose: to reduce facts to significant data and to seek their fundamental dependences. A mass of facts does not constitute a science. However, if abstraction is necessary, how we set about achieving it is not a matter of indifference. We can simplify reality without danger and with advantage, if this is not likely to change the actual nature of phenomena. But under no circumstances should the concern for simplification change the essence of reality.

The legitimacy of abstraction can only be justified *a posteriors. A priori* all abstraction can legitimately be considered inadmissible. Reducing planets to points in order to study their movements is a shocking abstraction, but it works successfully, and this very success makes it legitimate. It is thus with all abstraction. This principle is valuable in economics as in any other science.

All science is a compromise between the concern for simplification and the concern for resemblance. Great simplicity is convenient, but carries the risk that the picture which emerges does not sufficiently resemble the facts; a more exact resemblance makes the model too complex and unusable in practice. What can be said is that for any given level of approximation, the best scientific model is the one which is most convenient to use.

The claim that theory and practice arc opposed is completely unjustified, because a theory is valid only insofar as it constitutes a condensed synthesis of reality. If it does not, it is purely a creation of the mind, totally artificial, and of no value from the scientific viewpoint. If, however, a theory is actually a condensed synthesis of reality, it is extremely useful, because it represents in succinct and easily usable form a vast amount of information of all kinds on the real phenomena of which it treats.

In science the notion of *truth* is, in fact, relative. No theory, no model, can claim to represent *absolute truth* and, if there is such truth, it will certainly always remain inaccessible to us. There are only models which are more or less well verified by observed data, and of two models, the "best" will always be the one, which, for a *given degree of approximation,* will provide the simplest representation of observed data. Whatever its empirical verification, the most we can ever say about a theory is that *everything happens as if* its hypotheses actually correspond to the real nature of phenomena concerned.

These are the general principles of the method that long ago Henri Poincaré commented on so pertinently with respect to physical sciences, that Vilfredo Pareto so appositely extended to the social sciences, and which I have continuously observed in all my work.

Pseudo-theories

The criterion of confronting theory with experimental data is merciless. Easy as it is, with only a pen, to work out a purely literary analysis or an abstract mathematical theory as long as no empirical application is made, it is equally difficult to elaborate an analysis that is effectively verified by observed data. This doubtless explains the propensity of so many authors to avoid numerical confrontation, except in vague and general terms.

To test the logical coherence of a theory and to bring out its real content when it deals with magnitudes linked to each other in a somewhat complex manner, mathematics is certainly an instrument without equal, *indeed irreplaceable.* But in examining certain contemporary theories in terms of the two requirements of scientific method - logical coherence and conformity with observed data - we find two kinds of deviation: logical inconsistency and neglect of real phenomena.

Literary theories. The defects common to many literary theories are the continual use of non-operational concepts, vague and undefined words, whose meaning changes constantly within the analysis and varies from author to author; the absence of rigor in the analysis; the abundant use of more or less metaphysical expressions, which, having no precise meaning, can mean anything one wants, and are thus sheltered from objections; the use of expressions charged with emotional content which, while they may ensure the popularity of their authors, cannot lend themselves to rigorous reasoning.

"Mathematical charlatanry" While many literary theories cannot be considered scientific, the same can be said of a great number of theories, purely logical, with no real link to facts. While mathematics is an at instrument whose mastery is extremely precious, it is, and can only be, an instrument. One cannot be a good physicist or economist simply because one has some ability and skill in mathematics.

For almost forty-five years contemporary economic literature had developed too often in a totally erroneous direction with the construction of completely artificial mathematical models detached from reality; and too often it is dominated more and more by a mathematical formalism which fundamentally represents an immense regression.

Certainly, it is no longer necessary today to justify the necessity and the utility of rigorous building of models on the basis of perfectly specified axioms. However, one must be very careful not to consider that it is enough to base a theory on a rigorous axiomatization for it to be scientifically valid. Axiomatization may be necessary, but it is secondary to the confrontation of its implications in relation to experimental data. Paradoxically, from the scientific viewpoint, incomparably more care is brought today to the mathematical elaboration of models than to the discussion of their structure, their hypotheses, and their results from the viewpoint of analysis of facts.

The contemporary literature offers us countless examples of aberrations which flow from neglect of the essential principle that a theory is valid only if it is in agreement with observed facts, and that the only source of truth is experience. Indeed a large part of the contemporary theoretical literature has progressively come under the control of pure mathematicians who are more concerned with mathematical theorems than with analysis of the real world. A new scholastic totalitarianism has arisen based on abstract and apriorist conceptions, detached from reality; this kind of "mathematical charlatanry" had already been denounced by Keynes in his *Treatise on Probability.*

It cannot be repeated too often: for the economist, as for the physicist the essential objective is not to use mathematics for its own sake, but as a means of exploring and analysing concrete reality, and consequently never to dissociate a theory from its applications.

Unsound econometrics. But the abusive use of mathematics is unfortunately not the only failing in contemporary literature, which too often has generated a crop of pseudo-theories based on the mechanical application, devoid of any real intelligence, of econometrics and statistical techniques. All these theories have the same characteristics: the elaboration of models of linear correlation which are in reality only *pseudo-models,* accompanied by a mathematical - statistical panoply of *untamed,* totally unjustified econometrics which seem to the naive to be scientific theories whereas they are generally just empty shells; *the blind and brutal* application of linear correlation programs and the tests associated with them, although these *tests generally are not applicable to the cases studied;* and the use of models, too often applied to a single country and for a short period, where the number of explanatory variables and the number of arbitrary parameters *are such that the fittings can have no real meaning.*

Excessive specialization. Finally, a very regrettable tendency continues to develop in the world of economics: *excessive specialization.* It is too often forgotten that only through a vast effort of synthesis can social sciences achieve significant progress. What is needed are economists with a broad perspective of history, sociology, and political science, historians skilled in economic analysis and the study of sociology, and sociologists who also have training as economists and historians.

One cannot help but be reminded here of the portrait of the master economist painted long ago by J. M. Keynes:

> *Yet good, or even competent, economists are the rarest of birds. An easy subject, at which very few excel! The paradox finds its explanation, perhaps, in that the master-economist must possess a rare combination of gifts. He must reach a high standard in several different directions and must combine talents not often found together. He must be mathematician, historian statesman, philosopher in some degree. He must understand symbols and speak in words. He must contemplate the particular in terms of the general, and touch abstract and concrete in the same flight of thought. He must study the present in the light of the past for the purposes of the future. No part of man's nature or his institutions must lie entirely outside his regard. He must be purposeful and disinterested in a simultaneous mood; as aloof and incorruptible as an artist, yet sometimes as near the earth as a politician.*

New ideas and the tyranny of dominant doctrines

In the development of science, that is, in building theories and their models, *creative intuition always plays the determining role.* It is thanks to intuition that, from knowledge already acquired, the selection occurs of concepts and the relationships between these concepts that allow the essential structure of reality to be represented, that is, the choice of hypotheses. Deductive reasoning draws out all the consequences of these hypotheses, which are then confronted with the facts. Thus, creative intuition, logical deduction, and confrontation of the consequences of hypotheses with observed data are the three basic stages in any scientific endeavor. The history of science can be characterized by the indefinite repetition of these three stages in a process which leads towards increasingly comprehensive and well-verified models.

Indeed, only through the blossoming of new ideas suggested by creative intuition and empirical evidence can science truly progress. But all real 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 of a new conception, no matter how fertile it may later turn out to be. The 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, Pasteur, Wegener, and so many others demonstrate the obstacles encountered by discoverers of genius. It is this resistance to new ideas that explains why in economics we had to wait so long to discover the major contributions of Dupuit, Walras Edgeworth, Pareto, and many others. The successful scholar is always the one who adds some marginal improvement to the dominant theories everyone is already accustomed to. If, however, a new theory falls outside of customary channels, it is certain to face general opposition whatever its empirical justification.

In science, the action of the "establishment" and pressure groups is often exercised insidiously, sometimes moreover for reasons entirely foreign to science, and in recent years a dangerous tendency to politicization has developed in science and scientific research, based on ideological ideas of all kinds.

For all of these reasons, today just as yesterday, it is essential to constantly subject admitted "truths" to critical analysis without complaisance, always keeping in mind Pareto's statement: "*The history of science boils down to the history of errors of competent men.*"

The major principle of scientific discipline is always to doubt what is considered true, always to be open to examine opposite opinions favorably, and to foster research which might disprove propositions one believes in. Doubt of one's own opinions, and respect of those of others, are the first condition of any real progress in science. Universal consent, or even majority consent, cannot be considered a valid criterion for truth. In the final analysis, the essential condition for progress in science is a complete surrender to the teaching of experience, the only real source of knowledge. There is not, and there cannot be, any other test of the truth of a theory than its conformity, more or less perfect, with concrete phenomena.

My passion for research

Whatever the field of application, my whole life has been dominated by the thirst to know more, by the passion for research. I have felt this passion since my early youth; it has since formed the very foundation of my entire existence, and without any doubt, will remain so until the end.

Of all my experience, it is basically research, considered as an exploration of the unknown and totally detached from any concern with success, which appeared to me in itself as the most enlivening, the most exciting, because in this field any thorough investigation can only widen perspectives. It is this passion for research which, throughout my life, formed my major motivation.

Research is a sort of adventure full of risks, but a fascinating adventure. When a researcher undertakes some research, he is never sure of success. Very often he fails: reality is contrary to his expectations; and if he carries out an analysis and discovers some new regularity, what he finds is generally not exactly what he was looking for. Sometimes the results may even be more or less disappointing, but also, sometimes, at the end of an often painstaking effort, he discovers under the extreme complexity of the facts new regularities whose reality cannot be doubted. Sometimes, too, his findings can surpass his expectations. Such moments are rare, but they exist, and they compensate for the rest.

As the geologist Pierre Termier, a figurehead of scientific thought, put it long ago in an incomparable style on the borderline of science and poetry:

> *The Joy of knowledge! Many scholars have experienced it. Some several times during their lives; some even in a lasting and persistent manner.... The joy of knowledge has marvelously consoled them from mediocrity, from incomprehension, from contradiction, from hostile silliness.... Knowledge is one of the reasons of our life and there is no satisfaction comparable to the one scientific research gives....The researcher knows immense joys that others do ignore.... The joy of the scholar or philosopher, the joy of the artist or poet. It is somewhat impossible to speak about. It is indescribable.*

In fact, there is hardly any greater satisfaction for the researcher than that which follows from the achievement of a vast synthesis between elements which at first seemed disparate or contradictory, or the display of new relationships between facts which seemed to have no connection, of regularities previously unrecognized, of invariant relationships in space and time.

However, such a synthesis can only result from patient and often unrewarding effort Research in any subject must begin by first exploring its particularities, its differences, its varieties. Only by starting with details can an overview of a subject be reached. Only slowly, after lengthy effort, does the intimate interdependence between parts begin to reveal itself. Slowly, the difficulties subside, the whole becomes dear and limpid, like the countryside viewed from the top of a high mountain.

The ultimate goal of such an approach is the attainment of a reciprocal and coherent symbiosis between theory and observed data. This process is based on a twofold conviction: the conviction that without theory knowledge remains unavoidably obscure, and that an accumulation of facts only constitutes a chaotic and inevitably incomprehensible aggregate; and the even stronger conviction that a theory which cannot be confronted with fact or which has not been verified quantitatively by observed data is of no scientific value.

Certainly, nothing is comparable to the inextinguishable passion for research; to the unquenchable desire to know, understand, clarify, explain; to the constant will to persist in overcoming every difficulty wherever it is encountered, never to be content with approximation; to the permanent concern to never lose sight of the whole; to constantly think about the synthesis. In reality, nothing comes close to the satisfaction of this construction, the ineffable euphoria of innovation and discovery.[3]

Appendix: On my experiments in physics, 1952-1960[4]

1. I believe it is very desirable to state explicitly what was the origin of all my experiments in physics.

   I have always held the conviction that the propagation of the gravitational and electromagnetic actions implies the existence of an intermediate medium, the "ether" of Fresnel and the 19th century physicists, but without there being grounds to believe, as was generally considered in the 19th century, that all the parts of that medium are perfectly motionless in relation to fixed stars.

   This conviction led me to consider that a magnetic field corresponds to a local rotation of this intermediate medium.

3 Tea written in France in 1986, before 1988 Nobel prize.

4 Appendix added on December 16, 1998.

From this, I inferred that a link could be established between magnetism and gravitation by observing the effect of a magnetic field on the movement of a pendulum consisting of a glass ball suspended on a thread of a length of approximately two meters.

2. In order to detect such an effect. I began by observing the movement of such a pendulum in the absence of any magnetic field other than that of the earth. *To my great surprise*, I found out that this movement did not reduce itself to the Foucault effect, but displayed *significant anomalies* in relation to this effect. It was these *totally unexpected anomalies* which made up the essential object of my experiments from 1953 to 1960.

3. Of all the *very limited number* of observations made in 1952 and *1953* of the movement of a glass ball oscillating in a magnetic field of the order of a few hundred Gauss, *I was not able to draw any definitive conclusion.* With certain experimental devices, 1 found positive effects, while with others, I obtained no effect whatsoever.

   However great, *indeed very crucial,* the importance of these experiments, I was led, given the difficulties to realize a much stronger magnetic field, to interrupt them in order to devote all the resources it my disposal to the study of the anomalies in the movement of a short pendulum the existence of which had been demonstrated *indisputably* in 1952 and 1953.

4. In order to study the anomalies detected in the movement of a short pendulum, I made use mainly of a paraconical pendulum, approximately one meter in length, consisting of a vertical bronze disc attached to a bronze rod suspended from a stirrup resting on a steel ball.

   Indeed, outside any magnetic field other than the earth's magnetic field, I observed, on the basis of *uninterrupted* observations realized over periods of a month between 1954 and 1960, *very remarkable anomalies* in the movement of the paraconical pendulum. A key finding was the existence of a significant periodicity of the order of 24h 50mn.

   Identical results were found in June and July 1958 in two laboratories, some 6 km away from each other, one in a basement at Saint-Germain, the other in an underground quarry at Bougival 57 meters below ground.

   Indeed, such a periodic *lunisolar* effect is quite *inexplicable within the framework of the currently accepted theories.*

5. The existence of the anomalies observed in the precision levelling and triangulation operations, compared with the anomalies observed in the movement of the paraconical pendulum, led me to realize, in parallel with my pendulum experiments at Saint-Germain and Bougival in 1958, a series of North-South and South-North optical sightings on fixed sighting-marks. As a result of technical difficulties, it was not possible to realize these optical sightings satisfactorily until the second half of July *1958.*

   Indeed I found, in the second half of July 1958, *a remarkable correspondence* between the anomalies of the paraconical pendulum and the anomalies corresponding to reciprocal optical sightings of two theodolites on two sighting-marks borne on the same supports as the theodolites. In any *case,* these optical anomalies, considered in themselves, are *inexplicable within the framework of the currently accepted theories.*

6. Finally, during the total eclipse of the sun on June 30, 1954, a remarkable deviation of the plane of oscillation of the paraconical pendulum was observed. This deviation is *quite inexplicable within the framework of the currently accepted theories. An entirely similar deviation* was observed once again during the total eclipse of the sun on October 2, *1959.*

7 These various anomalies appeared to me to be closely connected to the very many anomalies observed during the 19th and 20th centuries in mechanical, optical, and electromagnetic experiments, which have remained *unexplained,* and of which I presented an overall analysis in a paper in 1958 (published in English in 1959) (see References).

8. To conclude this very brief survey of my experiments, I believe I can make a prediction. *If, without interruption, for at least a month, at the same place and simultaneously,* observations were made of the movement of the paraconical pendulum, together with optical sightings such as those I made in 1958, and a repetition of the experiments of A Michelson and A. Morley (1887) and E. W Miller (1925), the purpose of which was to display the movement of the earth relatively to the ether, it would be found that the effects observed by Miller in 1925 correspond to the anomalies of the movement of the paraconical pendulum and the anomalies of the optical sightings observed in July 1958.

References

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An English version of this book will be published soon by Kluwer Academic Publishers, Dordrecht, Netherlands, under the title *Markets and Risk: Essays in Honour of Maurice Allais,* edited by Bertrand Munier.

The main references to my experiments in physics are given in Boiteux, Munier, and Montbrial, *Marchés* (pp. 253-254). See especially Allais, 1959, "Doiton reconsidérer les lois de la gravitation"; and Allais, 1959, "Should the laws of gravitation be reconsidered" *(Aero-Space Engineering, Sept.* 1959, no. 9, pp. 46-52; Oct. *1959,* no. 10. pp. 51-55, and Nov. 1959, no. 11, p. 55).

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