

The Challenge of Macroeconomic Understanding *

A permanent and multifarious quest

There was more unity in my professional activities than my academic colleagues seem to realise. No schizophrenia is revealed by the fact that I accepted work as an official statistician, and even to become head of a large government organization, while at the same time planning to devote effort to research and teaching. My hope to improve understanding of macroeconomics provides the unifying concern behind these activities.

Improvement of knowledge of macroeconomic phenomena not only requires the success of many research projects, some quite fundamental, others dealing with the measure of specific effects, it also requires sensitiveness to problems that policy makers are trying to solve. It further requires appropriate teaching, *i.e.* transmission of proven methods of analysis and of the accompanying scientific corpus. It finally requires progress in the collection and analysis of data, as well as in the diffusion of the pertinent results to all users, including the general public.

None of these tasks is trivial. In their most delicate aspects, they all involve judgement as to what should be stressed and what the real needs are. This has two consequences, the one general, the other particular to this article. On the one hand, significant progress is not identified as easily in macroeconomic understanding as in the hard sciences or in microeconomic theory. On the other hand, I have long been reluctant to express myself in scientific journals about fundamental macroeconomic questions because I did not feel sure enough about my own views; this

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uncertainty may have influenced the orientation of my own research for the two decades of the 50s and 60s when my main publications dealt with microeconomic theory or econometric methods.

This article will, I hope, show how the concern for macroeconomic understanding came early in my life, and also how it incidentally led me to face some well-defined unsolved microeconomic or econometric analytical questions that I thought I could solve. As I got older, and microeconomics and econometrics progressed toward the consideration of what appeared to me as more and more special issues, my thoughts and writings progressively concentrated on macroeconomics. This does not mean that I now think the subject to be simpler, or my views about it to be necessarily right. But macroeconomic problems are important; my judgement was formed through years of thinking and experience; I no longer believe that other macroeconomic writers master the subject any better than I do.

Macroeconomics is indeed particularly difficult. Its theoretical construction must be firmly based on observation of complex phenomena. But these phenomena are changing as the technological, sociological and institutional context evolves, so that observation often is inconclusive. Interdependencies are as important as in microeconomic equilibrium theory but they occur within a less pure framework: industrial structures are partly oligopolistic and competition is partly monopolistic, adjustments are incomplete, markets do not always clear, anticipations matter and their formation obeys rules that are not easily detected. By necessity theoretical representations must be drastic simplifications. It is then a delicate question which representations will retain the most pertinent features of the phenomena and so be most useful.

While keenly aware of this fundamental difficulty of macroeconomic understanding throughout my life, it is noteworthy that I felt secure not only about the general structure within which macroeconomic analysis ought to take its place (national accounts, behavioral relations, market adjustment laws,...) but also about the scientific way of proceeding in this field of knowledge. Since I am not really worried about the philosophy of science as applied to macroeconomics, I did not invest much time in it. But I feel at ease with what I understand to be Karl Popper's views on science in general and I do not think economics to be fundamentally special, even though its scientific achievements may be found meager as a whole with respect to the questions to be solved.

Initiation

During my school years I was somewhat exposed to economic facts but not at all to even the most rudimentary economic analysis. The classical French teaching did not then deal with socio-economic realities, except indirectly by our study of French, Latin and Greek literatures and by our less stressed study of history or geography. My father, who was a lawyer in the provincial city of Limoges, had socialist ideas, which mainly meant a concern for the social situation in France; at the family table or beside the fireplace particular aspects of this situation were occasionally stated and commented on. Moreover, I could see in Limoges the impact of the depression on the traditional porcelain or shoe industries and on their workers.

I discovered economics at the age of 18 when studying law as a complement to my main studies in mathematics. I was attracted by the subject, which was probably aided by my increasing awareness of economic problems. But for some time I had no real teacher in the field. I first read manuals, then books, but without direction, as an autodidact discovering what was available in French in the early 40s. It is hard for me to know what I really learned then: certainly the main basic concepts, the main historical facts and a sense of what writers were up to, probably a good knowledge of partial equilibrium price theory together with an understanding of the respective roles of observation and reflection in economics (I remember I was particularly interested in books where some data were discussed and that I found too few of them). But I did not gain then a proper and coherent analytical apparatus for the discussion of economic problems.

This came from 1946 on, when I entered the *Ecole Nationale de la Statistique et de l'Administration Economique* (ENSAE) of the *Institut National de la Statistique et des Etudes Economiques* (INSEE), thinking that it was the best place in which I might later work as an economist. I was then 23. The curriculum in economics was not so extensive but it brought to me what I was lacking, namely a structure for the organisation of knowledge and thought. The most significant event was my meeting Maurice Allais who became one of my teachers in 1947. After the initial surprise to be confronted with such an unusual man, I quickly understood I had much to learn from him. Not only was I a diligent student, but also, in 1948, at the same time I was beginning my career as an official statistician, I joined the informal group of young economists

that was meeting around Allais at lunch time or in the late evening. The group included Marcel Boiteux, Gérard Debreu and others gifted with enthusiasm and imagination.

Besides an introduction to the Rockefeller Foundation, which gave me a fellowship to work in Chicago in 1950-51, I owe to Allais three important debts: my understanding of general equilibrium and capital theories, my access to the then modern literature available in English, and above all my association with someone who was doing real research (it is indeed a disgrace for the French educational system that I had had up to then no real opportunity to be exposed to active scientific research).

Arriving at the University of Chicago in June 1950, as a guest of the Cowles Commission for Research in Economics, I no longer had much to learn from formal teaching in economics. I was a bit annoyed because my fellowship stipulated that I should attend classes, but looking at the calendar I did not see any course in economics that I could usefully take; my problem was resolved when I discovered the mathematics section of the curriculum. My prior training in mathematics in school and at the *Ecole Polytechnique* had been quite good, but traditional; I still had to learn what was then called modern mathematics, which was precisely the backbone of the mathematics curriculum at Chicago.

The Cowles Commission was an ideal place for a 27 year old mathematical economist and statistician, eager to learn. Jacob Marschak, Tjalling Koopmans, Gérard Debreu and others were deeply involved in fundamental research and always available for examination of a scientific point. The main work on the econometrics of simultaneous equations had recently been completed; but extensions, applications or better presentations were still looked for. Research on activity analysis and general equilibrium theory was in its most active phase. We could often see visitors involved in this joint effort, such as Kenneth Arrow or Leonid Hurwicz. Leonard Savage was at the statistics department and came often. From time to time Milton Friedman granted us the benefits of his criticism.

Microeconomic theory

My list of publications begins in 1950 with three articles, two dealing with price indices and the estimation of price elasticities of

imports and exports. But my entry into the circle of academic research workers was due to my work on theoretical questions of microeconomics.

The microeconomic theory of resource allocation is neat and clear. It poses well-defined problems of a purely logical nature. A young mathematical economist approaching them directly knows what the issues are. He has none of those doubts that macroeconomics inspires in a critical mind. In the early 50s moreover, some of the main questions had not yet been solved and the recent development of mathematics was providing new and efficient tools to deal with them.

The preceding ten years I had spent studying economics had given me a better understanding of the existing theory than I realised. This is why I was able to point out to L. Metzler, then quite influential, a confusion in one of his articles on capital theory (see L. Metzler, 1951, p. 67). I was also able to give P. Samuelson the key to a problem in the axiomatics of the von Neumann-Morgenstern hypothesis that had been puzzling him (E. Malinvaud, 1952).

For all those reasons, including my earlier association with Maurice Allais and my presence within the Cowles Commission group working on activity analysis, it was natural that I should work in microeconomic theory. My article on capital theory (E. Malinvaud, 1953) was the outcome of an effort to unify two approaches to resource allocation over time, the one extending the static neoclassical model, the other directly considering stationary states; the opposition between these approaches had been at the heart of a debate on capital theory in the 30s, notably between F. Hayek and F. Knight (see in particular their papers in the *A.E.A. Readings in the Theory of Income Distribution*, 1950). The unification raised two mathematical problems: since it required the consideration of infinite time, generalisation of the results proved for the static model to the extended model was not trivial; on the other hand, one had to prove the stationarity of the price system supporting an efficient stationary state. This work attracted attention and gave me the opportunity of discussions with many older economists. In particular I benefited in 1959 from a visit to Oxford at the invitation of Sir John Hicks.

If I later worked on other problems of microeconomic theory, it is because I had to teach a course on the subject and more importantly because of my association with French planning. As is well known, a few French engineers working on public utilities have contributed over several decades to the theory of resource allocation. In the 60s some of them were particularly concerned with the logical problems raised by

the determination of the interplay between their work, dealing with the choice of projects, and macroeconomic planning intended to give future growth prospects, a planning process to which INSEE contributed. P. Massé in particular, then "Commissaire au Plan", was posing challenging questions. I thought that one way of enlightening these problems was to view them within the framework of the decentralized determination of an optimum resource allocation program (E. Malinvaud, 1967). Such a vision is, of course, bold for one who is aware of the actual planning process; I believe, however, it helps to put ideas in order.

Its usefulness is not limited to providing a background for the choice of public projects. It provides microeconomic theory with a chapter that is too often forgotten after the three following ones: 1) existence and properties of a competitive equilibrium; 2) existence and properties of an optimal state; 3) definition and convergence of a process leading to a competitive equilibrium. The third of these chapters is notoriously less developed than the preceding two and less satisfactory than one should wish for a good understanding of the stability issue; but it exists in most books on microeconomic theory. On the contrary no chapter is usually found on the definition and convergence of a process leading to an optimal state. The subject has a definite interest; it was at the core of the debate about "the economic theory of socialism" during the interwar period; it becomes almost inescapable when one claims to deal with the optimal provision of public goods; what can be said about it today is at least as relevant as what can be said about the stability of the very idealised Walrasian *tâtonnement* process. In the 60s my confidence in the relevance of the issue was strengthened by the discussions I had with L. Hurwicz and J. Kornai who were independently working on related issues.

Another question raised by the choice of public projects concerns the proper rules for risk-taking. Beyond my general interest about problems of resource allocation under uncertainty, I then had a particular motivation to consider the domain of validity of a property that had long been taken as intuitive: risk premia should not occur in economic calculations on public projects because, at the level of a whole nation, risks cancel due to the effect of the law of large numbers. Stating the property suffices to show that it abstracts from major common risks such as wars, but it deserves a theoretical investigation (E. Malinvaud, 1972 and 1973).

Econometric Theory

A training in mathematical statistics, participation in seminars at the Cowles Commission and work at INSEE, in particular for the setting up of French national accounts from 1951 to 1956, provided a background for my interest in econometric theory. But I was more deeply involved. I was regularly going to the European meetings of the Econometric Society and of the International Association for Research in Income and Wealth, which were then attended by fifty to hundred people and were quite convivial, with such figures as R. Frisch, R. Stone, H. Wold and younger econometricians like H. Theil, J. Sargan, occasionally J. Durbin. From 1954 to 1964 I acted as Co-editor of the journal *Econometrica* and in this capacity I had to read a large number of manuscripts in econometrics.

Above all, I was developing some teaching materials in econometric methods. I had initially been stimulated to do so by G. Darmon, the French mathematical statistician whose shrewdness and charm strongly influenced his students in my generation. I thought it was necessary to organise the subject on the basis of some fundamental theories; for so doing, I had to clear up some then unsolved problems. This led to my 1964 textbook, which was updated for later editions and widely used during twenty years.

Macroeconomic observation

Turning back now to the main subject, macroeconomics, I shall consider it under its various aspects: (i) observation of the phenomena, (ii) theory, (iii) diagnosis and policy. Finally I should not forget: (iv) teaching.

It is particularly noteworthy that nowadays problems raised by the observation of macroeconomic phenomena do not attract much academic attention, as if they were all satisfactorily solved. This situation is not very sound, since obviously some problems remain. It may reflect in part the complexity of these problems, which are not likely to have "nice" solutions, but in part also a tendency to oversimplify the features of macroeconomic evolution when confronting them to theoretical constructs.

The situation was different during the first half of the century and even in the 50s. The definitions of the basic concepts such as capital or income were seriously discussed; the aggregation problems, probably the main ones for macroeconomic observation, were considered within the theory of price and volume indices. Some of the best economists took an important part in the elaboration of these subjects, such as I. Fisher, J. Hicks and P. Samuelson.

During the 50s the focus of the discussions often was the structure to be given to the system of national accounts. A group of French experts, with whom I collaborated, was advocating a structure that would be systematically built from the two basic notions of agents and operations, while other people tried to maintain the solution that had been chosen for systems previously introduced, *i.e.* an outgrowth of the balance between production of goods and its main uses: consumption, investment, net exports. Finally, reflection on the uses of national accounts, as well as the need of a common framework for the central system, for input-output tables and for financial flows tables, led to the acceptance of what had been the French position, R. Stone playing the main role in the evolution of ideas. (For French readers my 1957 book on national accounting may have helped to a softening of what had been an excessive dogmatism.)

I think that the most important problem that ought currently to be more widely discussed concerns the proper definitions that capital, income and profit rates should receive while prices and wage rates evolve. Clearly the figures given by business and national accounts are not adequate from this point of view. Macroeconomists need to make important corrections, for instance when they want to make inter-temporal or international comparisons. If the methodology for such corrections does not attract more attention beyond a narrow circle of specialists, it is probably because national capital accounting is everywhere underdeveloped and hence not often used.

Still close to observation stands the descriptive study of economic evolution, often seen in a historical perspective. This activity always involves some analysis and therefore has theoretical foundations; but the latter may often be taken for granted, at least by lack of better alternatives. My work within the French public administrations dealing with economic problems, at INSEE, and from 1972 to 1974, as the head of the "Direction de la Prévision", advising the minister of economics and finance, often required such descriptive study. Beyond what was required by this work, I have often spent time in looking more precisely

into the quantitative assessment of a situation or of an aspect of observed evolution.

Considering how narrow our macroeconomic understanding is, I do believe that all macroeconomists, even those working on the most theoretical parts of the subject, should also spend some of their time on descriptive studies. In the absence of an intimate contact with the facts, one is all too easily tempted to make too much of a specific point on which one is working. This is why I never considered I was wasting my time by spending it on what some of my academic colleagues see as a trivial activity.

This may explain why I reacted positively in 1962 when I was asked by M. Abramovitz to take part in a joint project of parallel studies on modern economic growth in each one of the main developed countries. The project assumed acceptance of the quantitative and step-by-step methodology promoted by Kuznets, a part of which became known as growth accounting. Since it involved both judgement about some substantial issues and an important effort in data collection and processing, it seemed to require more than one person for each study. As for France I was fortunate to have the collaboration of three friends, one of which was not able to lead the project to completion and died just when our book was being published: P. Berthet, J.-C. Carré and P. Dubois. Work on this project was a side activity for all of us, which in part explains why ten years passed before the book was published, in advance however of the studies dealing with the other countries. In our conclusion, speculating about the future, we did not exhibit perspicacity: writing in 1972, we did not forecast the turndown of world and French economic growth. We were not alone in making that mistake.

Much more recently, when approached in 1984 by R. Layard, I accepted responsibility for a study of France. This time the subject was for parallel national studies assessing the factors leading to the rise of unemployment during the last fifteen years. The outcome was an article in the special 1986 *Economica* supplement volume. Compared with others in the same volume, my contribution stays closer to observed facts and tries to avoid reliance on a specific model. This is revealing of my views about the respective roles of data analysis, judgement and econometrics when the purpose is explanation of a real complex phenomenon.

Macroeconomic Theory

I am today considered as a proponent of disequilibrium theories and indeed I am. More precisely, for the correct elaboration of a theory the notion of disequilibrium must be clearly brought out wherever it is present; this is simply good axiomatics. Once this principle is accepted, one must recognize that disequilibria are involved in many more or less traditional ways of thinking about some important economic phenomena. Since these ways of thinking make sense, disequilibrium theories have a role to play in order to tighten up the rationale and help to the progress of our scientific understanding.

Such a position means neither that equilibrium theories have no role to play in macroeconomics, nor that I am satisfied with the existing disequilibrium theories. These two points will be considered here.

For the study of economic growth, a good part of the analysis relies on equilibrium theories; this is so for instance with growth accounting. Analysis will then benefit from improvement in the theories, which ought to be made more rigorous and more appropriate in their basic hypotheses.

In particular one must welcome the theoretical scrutiny of the validity of aggregate production functions and of the validity of the associated comparative statics properties linking changes in capital intensity with changes in real remuneration rates. This major example well shows the more general relevance of a systematic study of the aggregation leading from microeconomic relations to corresponding macroeconomic relations. The study is difficult since a purely theoretical search for perfect aggregation fails to lead to really positive results; one then has to take simultaneous account of specific features of the micro-relations and of the nature of the statistical distribution of the micro-units; the approach has been followed occasionally and is again the subject of attention; my 1956 methodological study of aggregation had already presented it as essential.

The purely theoretical scrutiny of the particular problem raised by macro-capital theory was the core of the well known dispute between the two Cambridges. This dispute resulted in a better understanding of the foundations of important macroeconomic issues. While I felt concerned by it because of my interest in macroeconomics and of my work in microeconomic capital theory, I avoided taking part because the dispute looked to me as misplaced: too much passion was involved

in the exchange of arguments about purely abstract properties. The discussion too often seemed to imply that answers to the theoretical problems had far reaching implications about capitalism, a claim that was absolutely unwarranted. I tried on various occasions, particularly in my Hicks Lecture (1986), to explain what I think has been learned from the dispute.

For the knowledge of the global operation of market economies, equilibrium theories have a different and more important role to play, namely exploring the stability or instability of sequences of temporary equilibria. The point was often made by F. Hahn with whom I had many stimulating discussions throughout the years. Stability depends on what are technological constraints, saving behaviour, modes of expectations formation and market structures. The issue then is so complex that one cannot hope to much improve insight about it by a microeconomic approach. Progress was usually made by the study of macroeconomic specifications chosen so as to exhibit what are thought to be the most relevant features of the problem.

But permanent market clearing, together with the required continuous full adaptation of the price system, rules out a number of phenomena that have long been considered as important in economic thinking. The most obvious one is provided by variations in involuntary unemployment. But positive or negative stimuli given by disparities in remuneration rates, in particular by the more or less strong leverage between profit rates in production and interest rates have long been considered major factors of economic evolution. Here, while discussing disequilibrium theories, I shall limit attention to those intended to deal with unemployment. I do think that disequilibrium growth theories also are relevant, as I tried to explain in various places, for instance in my Hicks Lecture (1986).

Already in the 50s, while teaching or applying postwar Keynesian theory, I was well aware that the major discrepancy with microeconomic price theory lay in the notion that agents were facing quantitative market constraints: involuntarily unemployed workers had an unfilled labor supply while some firms could not sell as much as would have been profitable for them at prevailing prices. This is why I considered that building a general equilibrium fixed-price theory was a relevant fundamental achievement, on which young mathematical economists were fruitfully working in the early 70s, notably in France. Stimulated by Y. Younes, and in collaboration with him, I tried to contribute to this theory. This was also one of the many subjects that gave me the opportunity of scientific discussions with J. Drèze.

About at the same time, when my main job was economic advising, I noted a new widespread concern among European government officials: it was no longer a problem with a too low or too high aggregate demand, but a disturbing trend in real wages, which were found to be increasing too rapidly, and in profit rates, which were deteriorating. The common diagnosis attributed to this trend part of the responsibility for the mounting unemployment. It became clear to me that the analysis then made by practitioners probably had some value but assumed a different hypothesis from the Keynesian one: a too low labor demand was related to an insufficient demand for goods. This could indeed occur, even within a very aggregated model, if the situation was considered as an example of a case that had previously been neglected in macroeconomic theory, namely classical unemployment.

Since I had been asked to deliver the Yrjö Jahnsson conferences in January 1976, I decided to devote them to presenting the newly elaborated theory of fixed price general equilibrium, to explain why it provided an adequate foundation for the macroeconomic theory of unemployment and to arguing that classical unemployment could have some relevance. The book, published a little later (1977), was my first production dealing with the heart of macroeconomic theory.

The book used a variant of the simple macroeconomic Barro-Grossman model. Ever since it was published, I have stressed that the study of this simple static model could only be a first step and I have tried to devote my research to what I thought to be important further steps. Those were first the dynamics of the static equilibrium, viewed as applying in the short run but as spontaneously evolving; second the specification of a model admitting existence of a full spectrum of cases from Keynesian to classical unemployment or to repressed inflation, the reason being the multiplicity of markets for the various goods and types of labor. Clearly, many others worked on these two important extensions of the theory, as well as on a third one concerning the macroeconomic equilibrium of an open economy.

After ten years some progress has been also made in understanding better what should be thought of the practitioners' view that real wages were too high in the 70s and early 80s. In particular macroeconomic modelling of the short term equilibrium provided a test of the frequency of classical unemployment. Although this modelling work cannot be considered as complete, one of its conclusions is robust enough to be established, namely that classical unemployment, with its excess demand for goods, never proves to be the dominant feature of the short

term equilibrium, except at times of fast recovery from a depression when unemployment still exists and adjustment costs prevent production to keep pace with demand. Since the 1974 downturn, excess supply of goods seem to have prevailed almost permanently in Europe. This, however, occurred together with a growing discrepancy between full employment output and productive capacity, which increased much more slowly. I thus see the proper diagnosis as being that low profitability leads to an insufficient progress of domestic productive capacity, hence to a tendency to foreign trade deficit, which forces governments to adopt depressive macroeconomic policies. Seen as a medium run phenomenon, this situation has the same characteristics as classical unemployment since firms do not find it profitable to employ the full labor supply, which they would have employed under more favorable price conditions. Loosely speaking, one can then characterize the situation as medium term classical unemployment, but short term Keynesian unemployment.

A proper theory of this phenomenon ought in principle to be based on a dynamic model in which the evolving temporary equilibrium would present the two preceding features. This model ought in particular to correctly represent the productive sector and its decisions about expansion or contraction of productive capacities. It may be too complex for discussions in which the basic issues concern medium term development. This is why I came to think that it would be useful to have a theory intended to directly deal with medium term consequences: the comparative statics of the equilibrium defined by such a theory would provide a test of the logical validity of some properties to which practitioners believe but which are mainly based on their intuition. I intend to pursue work on the definition of such a theory, whose main building blocks seem to be now available.

While my reflections during these past fifteen years were concentrating on the research program in disequilibrium macroeconomics, I witnessed with a good deal of dissatisfaction the main trends in macroeconomic theory that were occurring in American universities. My dissatisfaction was less concerned with research, which was exploring a number of often relevant questions, than what were said to be its implications about macroeconomic phenomena and what I imagined was then taught to good students coming from all over the world. Clearly, considering what I have written here, I have no *a priori* objection to the study of the supply side; but claims once made by the so called 'supply-siders' were distressing for anyone who consulted avail-

able econometric evidence; indeed they by now commonly appear as ridiculous. Similarly, the study of the rational expectations hypothesis is relevant as soon as one recognises, as I easily do, that dealing with expectations as exogenous, or even as adaptive, is not always realistic; but one should be aware of the extreme character of the hypothesis and of what is usually associated with it concerning the information of economic agents (one should even speak of credulity of economic agents, when they are assumed to hold as exact the theory one particular author is elaborating); in fact the claims of the rational expectations macroeconomists are usually not better founded on econometric evidence than the claims of supply siders. One sentence cannot do justice to monetarism, its many valuable contributions and its benign neglect of a number of considerations; I shall, however, note that it too easily tends to transpose to the short run, even to the medium run, properties that have good reasons to hold in the long run.

Macroeconomists, politicians and the public

In February 1972, Mr. Giscard d'Estaing, then minister of economics and finance, was looking for someone who would not be a conventional civil servant. He asked me to assume the responsibility of heading his "Direction de la Prévision", the group in charge of advising him on economic policy. This assignment lasted up to the fall of 1974, when I became director of INSEE, which is best described as the central statistical office of France, but is also a *de facto* independent institute of economic and social analysis. After these three years, mainly devoted to economic advising, I am still occasionally called to take part in policy advising at the national or European level.

I shall not dwell at length on my experience in this capacity, since it is not very different from that of others. But a few comments may be in order. As an economic adviser who witnessed the downturn in European economic growth, and more particularly in French growth, I cannot help asking myself whether my advices were adequate. In retrospect I think they were not. My colleagues and I underestimated the difficulty of the change and gave too much weight to short term improvements that did not help, or even made matters more difficult, from a longer term viewpoint. We were, however, more far sighted than

average informed public opinion, to which politicians always are very sensitive. This was obvious in particular in 1975-76 when at the *Commissariat du Plan*, a medium term programme up to 1980 was being discussed, almost all partners agreed in thinking in terms of a quick return to past growth rates, against the advice of the technicians. Similarly the gloomy medium term projections issued by INSEE in 1975, 1978 and 1979 were considered as almost scandalous. Governments repeatedly said these projections, which turned out to be too optimistic, would not materialise because proper action would be taken. The whole society, except for some rare individuals, did not want to face the challenge. This was the second time in my life when such type of reaction could be observed, the first one having been the period immediately preceding the Second World War.

When they address others, macroeconomists should, it seems, limit their statements to what is sufficiently well established to be the object of a kind of consensus in the profession. Indeed, their statements are supposed to be objective, so that they can be accepted by people who are not able to judge of their validity by themselves. Acting in a country where the marxist influence on intellectuals has been traditionally strong, I have been more constrained by this rule than my colleagues acting in some other countries. The rule, however, is imperative for anyone who considers economics as a science. It also implies that we should always resist the temptation of gaining easy success in the media by presenting as truth some of our personal views that are not yet shared by our professional colleagues.

Unfortunately what can objectively be said in macroeconomics is limited, while acting against macroeconomic difficulties may appear as urgent. The proper answer to this problem is, of course, that advisers should convey to decision makers some feeling about the likelihood of effects that are still imperfectly known. Politicians, however, typically interpret any probability assessment as leaving them the option to forget about the most unpleasant events. This difficulty of the division of roles between advisers and politicians seems to have two consequences. The first one is to bias economic policy toward the short term: politicians are naturally concerned by it and advisers can speak more confidently about short term effects, most of which are by now well established, than about medium term effects, many of which cannot be objectively assessed at present. The second consequence, which I noted in the behaviour of some high civil servants, is their inclination to take decisions themselves and then to find any kind of argument in order to

talk politicians into endorsing the decisions; but this does not contribute in the long run to confidence in the civil service on the part of the political class.

An official statistician has to convey to the general public the result of his most important observations. This also requires confidence: the statistical tools must be commonly recognized as reliable. In my country in which most of the statistical system is of recent origin, in which economic and social education is insufficiently developed and in which the degree of political consensus is low, I have had to spend part of my time on this activity, notably when the matter measured by some statistics was the object of a public debate. I did it without reluctance and considered it as one aspect of my role as a teacher.

Teaching macroeconomics

Most of my readers share with me the vocation for teaching. This is why I shall not explain here how I always thought my time to be usefully spent when I was preparing a course or working on a textbook. By far the largest part of this teaching took place at the INSEE school, ENSAE (*Ecole Nationale de la Statistique et de l'Administration Economique*). Considering the length of their previous studies at university level, students are there at the graduate level. They typically have a good background in mathematics, but a weak one in economics. Hence the teaching of economics can rely on mathematical formalization, but must devote time to place each question in its proper context.

My teaching materialized into three textbooks, respectively on econometric methods (1964), on the microeconomic theory of prices and resource allocation (1969) and on macroeconomic theory (1980-81). The first one took almost ten years of preparation, as my teaching on econometrics was progressively taking a more satisfactory shape. The second one was very quickly produced: general competitive equilibrium theory was a well organised subject; I needed only to supplement it by a number of chapters, dealing for instance with imperfect competition or with public goods, and to find the appropriate level of rigor and generality for such a textbook.

The real challenge was teaching macroeconomics. I began in 1957 and went on giving a course in the field every year up to about the time

when my textbook was sent to the editor. I long hesitated on the organisation of the subject and often changed the ordering of the main chapters and their contents. Above all, I was permanently concerned by how to choose the best way to show to the students the relevance of each piece of theory with respect to actual macroeconomic problems; I never found that easy and I do think that this difficulty reveals the weaknesses of present macroeconomic theory. This is why I shall comment here on the main aspects of the macroeconomic approach, as I think they fundamentally are and therefore ought to be taught.

The scope of macroeconomics covers both growth and fluctuations. The two dimensions, the long and the short run, must often be kept in mind simultaneously. An ideal theory would realistically deal with both; but it is out of reach. At least one must be aware of the assumption that short term and long term methods of analysis can deal consistently with the two dimensions that many macroeconomic problems actually have. This is why growth theory should be discussed as well as employment and inflation theories. On the contrary I do not insist on dealing with open economies in a general teaching of macroeconomics; depending on how small and open an economy is, its reactions to exogenous shocks will of course be different; but I think this can easily be understood as long as one has a good grasp of one theory of the closed economy. I grant, however, that one may disagree with this view and then be obliged to go into more elaborate theoretical constructions in order to avoid the closed economy limitation.

National accounting has contributed to making students more familiar with the basic structure to be used in macroeconomic analysis. This structure, which distinguishes the various types of agents and operations is fully appropriate for theory, at least if it is understood to cover not only flows (incomes, output,...) but also stocks (wealth, productive capital,...). There are great advantages to introduce it systematically at the beginning of macroeconomic teaching. It deals with enterprises and households as independent units; the hypothesis looks to me as appropriate for almost all macroeconomic phenomena, even though enterprises are owned by the people, either directly or indirectly through public ownership; certainly the hypothesis is much preferable to the one that would lead to models in which enterprises would not appear, the idea then being that they would have no autonomy for their decisions. Macroeconomic phenomena depend so much on the behaviour of the production sector that it should always be recognised as playing the major role.

This raises the issue of the representation of market structures in theories dealing with the whole economy. This representation can only be very rough and neglect the great heterogeneity that exists in this respect. Many growth phenomena are properly approached with the perfect competition hypothesis, notwithstanding its extreme character. When the hypothesis cannot be fully satisfactory, for instance because full market clearing cannot be admitted, dealing with firms as pure price takers may still be sufficient. A better and still manageable hypothesis assumes imperfect competition, firms having market power for their output but acting as price takers for their inputs. The kinked demand curve hypothesis may be appropriate for the study of some phenomena; it is then almost tantamount to the assumption that firms face a quantitative constraint on their sales and that the price of their output is given.

Proper representation of behaviour plays a crucial role, the time dimensions being almost always essential. Since information is incomplete, since adjustment costs and even irreversibilities exist, modelling of behaviour raises many problems that are rightly considered in the literature. But aggregation does not usually receive the attention that it requires. Depending on what is assumed, for instance about market structures, it does not operate in the same way. It should be considered much more systematically and have its proper place in macroeconomic textbooks.

It is traditional to state that a macroeconomic model contains two types of equations: behavioural laws and accounting identities, the latter containing in particular equalities between supply and demand for markets that are assumed to be cleared. I prefer to speak of three types of equations and to qualify some of them as "adjustment laws": they mostly describe the evolution of prices and wages when market clearing is incomplete; for instance a Phillips curve is an adjustment law. Using this denomination stresses the point that one has not fully clarified, in terms of behaviour, the rationale for the corresponding equation. There is no shame in recognising this situation and in stating that the justification lies in observed regularities. The wrong thing to do, when one cannot fully explain a complex phenomenon, is to pretend the phenomenon is different so as to be able to easily explain it by maximising behaviour.

Indeed, macroeconomics has two roots: theory and observation. Trying to forget about one of them is bound to fail, because one is then unable to prove specific conclusions of interest. This means in particular that our present macroeconomic knowledge embodies a lot that has

been learned from statistics combined with reasoning. Hence, macroeconomic theory cannot be properly presented without its empirical base. I have long believed that some economists will never become macroeconomists precisely because they dislike too much the uncertainty attached to econometric results; this uncertainty exists but becomes bearable when enough replication of similar results has occurred. Combination of deductive theory and inductive econometrics within a textbook implies many imperfections that I shall not list; but elegance must not have precedence on transmission of a queerly shaped body of knowledge.

Any teaching of macroeconomics must contain a discussion of policy formation. This implies a part taken from decision theory, the time and uncertainty dimensions both playing a role. But the teacher should have the sense of avoiding developments whose technical difficulty outweighs their relevance. To take just one example in order to make my point clear, I believe that any serious presentation of the time consistency issue in a macroeconomic textbook would today be premature; the distinction between fixed and discretionary rules can well be heuristically discussed without the recently developed formal apparatus. Actually the main difficulty in the parts discussing questions of economic policy lies in the choice of the underlying macroeconomic models. These must be very simple in order to permit an analytical treatment, but they should also be realistic from the point of view of the policy problem under examination. Simple Keynesian models can well illustrate short term economic regulation; but simple monetarist models, which are meaningful only in the long run, cannot do. In any case the reader must be warned of the bias that reliance on a simple model necessarily implies. Presentation of a full macroeconomic model, actually used for the analysis of business trends, may help to make the point clear.

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Such are the various challenges that I perceived in trying to understand macroeconomics. I have no doubt that our profession will eventually meet them better and better, notwithstanding their difficulty. Disputes of yesterday, today and to-morrow should not make us blind: slowly we are progressing in the right direction.

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