

## THE ET INTERVIEW: PROFESSOR EDMOND MALINVAUD

*Interviewed by Alberto Holly and  
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Econometrics owes its essence and much of its ongoing vitality to the infusion of ideas from economic theory, the development of appropriate statistical methods, and the quantitative recording of economic phenomena. Each of these elements is an important source of energy for further scientific change. As professionals, we contribute to this evolution through research and teaching, most commonly in individual specialisms and all too frequently with little concern for holistic issues. Few economists indeed have the knowledge, the scientific expertise, and the professional experience to speak out with authority on the subject in its entirety and its scientific achievements. Even fewer command the respect of colleagues and authorities in neighboring disciplines, like mathematical statistics. For some time now, Edmond Malinvaud has stood out from the rest of our profession as a scholar who is uniquely qualified in this regard. His writings influence every field of economics. His advanced textbooks are cornerstones of graduate teaching programs. The scientific standards of his work set an example

to the entire profession. And his recent evaluations of scientific accomplishments in quantitative economics have brought unity and direction to what was earlier just uncoordinated, individual research.

Edmond Malinvaud's career has been distinctive in research, in teaching, and in administration. His research papers and textbooks are well-known to the international community and have earned him many distinctions and honours. He is a Gold Medalist of the Statistical Society of Paris, a Silver Medalist of the Centre National de la Recherche Scientifique, an honorary foreign member of the American Academy of Arts and Sciences, a corresponding fellow of the British Academy, a foreign member of the Finnish Academy of Sciences and Arts, and a foreign associate of the National Academy of Sciences of the United States. He has honorary doctorates from the Universities of Louvain, Basle, Helsinki, Geneva, and Lausanne.

Malinvaud's achievements in administration are equally impressive but they are generally less well-known outside of France. In 1948 he joined the Institut National de la Statistique et des Etudes Economiques (INSEE) and since 1974 he has been the Director of that Institute. INSEE's primary responsibility is the collection and publication of national economic statistics, an immense activity involving thousands of people. Within INSEE, Malinvaud established a special research unit and a regular research seminar, now known throughout Europe as the Malinvaud Seminar. During 1970-1972, Malinvaud was Director of the companion institute ENSAE (the Ecole Nationale de la Statistique et de l'Administration Economique) which trains scientists and civil servants for the French Administration. And in 1972-1974, Malinvaud was Directeur de la Prévision at the Ministry of Finance. In addition, he has served on the Boards of several other economic institutions in France, most notably the Bank of France. All of these positions carry major administrative responsibilities. And Malinvaud has received some of the highest administrative distinctions in France for his service, including Officier de l'Orde des Palmes Académiques, Commandeur de l'Orde National du Mérite, and Commandeur de l'Ordre National de la Légion d'Honneur.

The interview that follows took place on March 5, 1986, in Edmond Malinvaud's office at INSEE. We are privileged to report this conversation to you and we hope that it will be of interest to economists as well as to econometricians.

Would you like to tell us about your early schooling?

My early schooling was quite typical in France. I took classical training with some mathematics and graduated from my school. Then I had to decide what to do after high school and, since I was rather good in mathematics, I prepared for the "grandes écoles" and entered into the Ecole Polytechnique. But at the same time I was interested in the study of law, because my father was a lawyer and he would have liked me to join his office. I did not object to the idea; and I was quite open minded at the time. Although my main

training was in mathematics, before and at the Ecole Polytechnique, I was studying law on the side, that is, when I had some spare time. This is how I discovered economics; I was interested in the subject right away.

You studied at the faculty of law?

Yes, I was a student at the faculty of law, where one-quarter of the program was in economics in the first years. Since I had been living as a boy in an area which was rather hard hit by the depression of the 1930s, I was already interested in economic matters by the age of seventeen. I entered Ecole Polytechnique in 1942, which meant that it was during the war. There was so much upheaval at the time that my studies were cut short by various activities, in particular, by staying in the army and the like. So, during all these years I read economics in a very disorderly fashion, much as a self-taught man reads books.

To which subjects were you attracted in those days?

Well, of course, to the main macroeconomic issues, to the problems of unemployment, economic growth, inflation and the like. You must realize that at this time I did not study economics with a view to doing research in the field. My motive was to learn what people already knew about economic phenomena.

Who influenced you in your studies in economics and statistics?

Well, that came later on, after I had been at the Ecole Polytechnique and after the end of the war, when I studied economics and statistics thoroughly, just before getting my job as an official statistician. I met two people, among others who were very influential upon me. In the immediate post-war period in France, there was a real spirit of research. People really wanted to understand the phenomena that had occurred. Many of us had been deeply affected by the war and had time to reflect, sometimes in concentration camps or in prisoner of war camps. People had various experiences. It was therefore quite an open society of people with candid discussion. As I said, two people in particular played an important role in my development. The first was a professor of probability and statistics, Professor Georges Dar-mois. He is very little known outside of France because he did not publish very much. But he was an excellent teacher and he was also a very good research worker. The second one was Maurice Allais, whom I met twice. I first saw his major work in 1945 or so when I was still at Ecole Polytechnique and started reading it. But I stopped, because I thought that access to it was difficult and not very promising; all through the first 80 pages I had the impression of reading only definitions and not results. Later, in 1947–1948, Maurice Allais was my teacher. As I said before, I had read a lot before that, but I was lacking a well-structured framework for the analysis of economic phenomena; and Allais came with a very organized mind. He

had been working all alone for many years and he was very influential upon me, providing me with what was lacking. At the same time he was very stimulating to his research students, Gérard Debreu and Marcel Boiteux, who were around him. So he played a very important role in my further development. He also encouraged me to apply for a Rockefeller Fellowship, which I did and was awarded in 1950.

During the early 1950s I believe that you visited the United States. Would you like to tell us about this visit? Did it turn out to be important in shaping the direction and nature of your research?

Yes. In all respects. I had met Allais in 1947–1948, and then I started working at INSEE in the fall of 1948. But at the same time I was attending a kind of permanent ongoing seminar, often several times a week: sometimes at lunch time, some other days in the late evening, even after dinner since in those days it was quite common to have after dinner activities. We were meeting with Allais and a number of other people and I was reading a lot, by now, in a better organized way. So, when I got a Rockefeller Fellowship and was accepted as a guest at the Cowles Commission in June 1950, I was well prepared. I realized that, at the time, I was put right into the center of economic and econometric research. Of course, this was very important to me. It gave me an understanding of how research was actually performed and placed me at the forefront of ongoing economic research.

Did the intellectual tradition in the United States in economics seem very different to you, from that to which you were accustomed in France?

No. For two reasons. First, my training in economics had been rather atypical from the French tradition, due to the war years and, later, to my association with the intellectual environment created by mathematical statisticians like Darmois. In the same way, the Cowles Commission was not at all typical of the American intellectual tradition. The Cowles Commission was itself something of an international group with its two senior people—Jacob Marschak, who was born in Russia and who had been living in England for some time and Tjalling Koopmans, a Dutchman. We were a number of people from various countries. So really it was not a center of American intellectual tradition, but more a research group of a very cosmopolitan nature.

After your visit to the Cowles Commission, you worked for almost five years on French National Accounts at INSEE. Could you describe your work at that time?

Yes. It's very simple. We started from a situation in which the statistical system in France concerning economic matters was very underdeveloped. And we had both to develop the statistical system and to build the national accounts. So I worked on various things which were necessary to achieve this

aim. Of course, most of the time I was working on the overall synthesis, but I also suggested work on particular aspects of the project. For instance, I remember I launched a household survey on particular aspects of consumption, like clothing, and the like. I also initiated some statistical exploration of business data collected by tax authorities in order to derive estimates of various items. At the same time, I worked on the mainframe of national accounts.

When your book *Statistical Methods of Econometrics* appeared, it received unparalleled acclaim from the econometrics profession. One might almost say that it has done for econometrics what Harold Cramér's *Mathematical Methods of Statistics* did earlier for statistics. Would you like to tell us more about the origins of the book? Did you set out, in particular, to write such an encyclopaedic work? Now that the book has helped to educate many generations of econometricians, has its longevity surprised you?

The book was the outcome of ten years of teaching. I started teaching econometric methods in 1954 and the French edition was published in 1964. What started on a rather small scale in the beginning developed into a course that covered the whole field of econometrics. Finally, the book was more advanced than the course I gave. The encyclopaedic nature of the work may be a reflection of the fact that I tried to be up to date on the development of the subject at the time. And, certainly, the longevity of the book has surprised me a bit. When I was asked by a publisher to write a book, I told him that it would be a very specialized book and that there would be very few readers for it. But it turned out to be not a bad bargain for the publisher.

The distinctive features of *Statistical Methods of Econometrics* are its generality, its mathematical rigor, and its originality. The latter is particularly evident in your theory of linear estimation and your chapter on nonlinear regressions. What was it about the state of the subject of econometrics that made you single out these areas for new theoretical treatment? Why was it that you chose a less theoretical treatment for the time series part of your book?

I did not really intend to make theoretical advances in the methodology of econometrics. As I said before, my purpose was to organize the subject in such a way that I would be able to teach it properly. So I was looking for a unified treatment of the subject, as it was around 1960. I was looking particularly for a unified treatment of linear estimation. It was already available in some form. Essentially, it was the Gauss–Markov theory. It so happened that Georges Darmois was really mastering this whole theory and teaching it very elegantly, so I had there a unified treatment for one part of the subject. It was clear to me that a large part of the rest of the subject was really a kind of extension of linear theory to structures that had some nonlineari-

ties. I, therefore, looked to organize the rest of the subject starting from the linear theory. I did not set out to make a less theoretical treatment of time series. It just happened that I was not able to organize or to unify the time series parts with the rest of the book. It was not the outcome of a particular choice. You must, of course, realize that time series aspects of the subject were a bit neglected in general around 1960. They had been the subject of much interest in the 1930's, during the early years of the Cowles Commission. When you read Monograph 10, which was written really around 1944–1946, time series aspects are still important. But in the new developments that took place, people became more interested in the simultaneous equation aspect, neglecting a bit the time series aspect. That may explain why I didn't invest as much effort in the time series part of the book.

In several places in your book, *Statistical Methods of Econometrics*, you seem to give preference to geometric thinking over analytical reasoning. Who influenced you in this choice and what advantages do you see in geometric thinking? Do you just see it simply as a means of visualizing and improving the understanding of the econometric methods?

There are various kinds of minds and some people like geometry, others don't. I am one of those who like geometry. This may come from far in the past when I was first taught mathematics, when in France the geometrical aspect was very much stressed. It may also be a reflection of the fact that I feel I may not be good at calculus and tend to make many mistakes in calculus. Therefore, I am not so confident in my analytical reasoning. But as I said before, I met Professor Georges Darmois, who had a very well-organized theory of linear estimation, in particular, and mathematical statistics, in general. He was also using the geometrical approach. I think I can say that geometric thinking provides you not only with a good visualization of a method, but perhaps also with an intuitive understanding of the origin of results. Now some people may get these intuitive understandings from the analytical side. But I myself got more of it from the geometrical side, a bit like Georges Darmois and even Sir Ronald Fisher who very often presented his arguments in geometrical terms.

In your book *Statistical Methods of Econometrics*, you introduced the concept of the minimum distance estimation procedure for nonlinear models, and you compared it with the maximum likelihood estimation procedure. Could you tell us more about the origin of this concept?

It was really something natural since I started from a Gauss–Markov theory of linear estimation and I tried to see what was different in the case when you have a nonlinear regression structure. It seemed rather natural to think in terms of minimizing a kind of distance. The idea was already established in mathematical statistics, of course, and had been used by others. The

Gauss–Markov theory was understood to have a great advantage in linear estimation, namely, that it did not assume that the disturbances were Gaussian. Therefore, I also tried to find a presentation that would rely as little as possible on the hypothesis of normality. Now, usually, when you introduce maximum likelihood you also introduce at the same time the normality hypothesis, which I prefer to avoid.

Showing that the minimum distance estimation procedure leads to the maximum likelihood by an iterative procedure was also a new idea, wasn't it?

Yes. In the same way that the linear estimator in Gauss–Markov theory is shown to be maximum likelihood under normality, I tried to see whether something similar might hold in the case of nonlinear regression. In particular, when you estimate the covariance matrix of the errors either by maximum likelihood under normality or by some kind of iterative method you select at the same time an appropriate measure of distance with respect to these covariance matrices. Again, this was a natural idea for someone using the geometric approach.

Yes, we see the importance of that. The paper you published in 1970 on nonlinear regressions has been very influential in econometrics, notably in research on asymptotic theory. What were the origins of this paper? Were you familiar with Jennrich's work at that stage?

The origin of my study of this problem was really the fact that I was not very satisfied with what I wrote on the subject in the first edition of my *Statistical Methods of Econometrics*. In the chapter on nonlinear regression, I originally had a consistency result that was dependent on a rather complex assumption. I felt it would be desirable to be able to deduce it from simpler properties assumed on the structure of the model. I then tried to prove consistency of nonlinear regression from clearer and more basic assumptions. At the same time, I was not at all satisfied with the literature. I am not only speaking of the literature in econometrics; the whole mathematical statistics literature on nonlinear regression was asserting results concerning asymptotic distributions by just assuming consistency and not proving it. It was at Berkeley in 1967 that I found a way of proving consistency from more basic hypotheses. Then I wrote a paper on the subject that was submitted to the *Annals of Mathematical Statistics*. It was read by a referee who made some useful suggestions. When I sent the revised and final text to the *Annals of Mathematical Statistics*, I came across a recent issue of the *Annals of Mathematical Statistics*, and there was the paper by Jennrich. That's how I discovered it. Jennrich had apparently been motivated by the same dissatisfaction and had therefore worked on the same subject. He actually worked with more powerful mathematical tools than I did. But I didn't know of his work when I worked on the problem. I must say, however, that I think I did

a bit more than Jennrich. Actually, there are two parts to my own paper in the *Annals of Mathematical Statistics*. The first part gives a result that is similar to the one of Jennrich with similar hypotheses, although Jennrich proves strong consistency where I prove only weak consistency. In the second part of the paper, I tried to introduce hypotheses that would be easier for an econometrician to check from the basic elements of these models. So to summarize how I see the progress in this field, there is the first edition of my book with a very obscure hypothesis, then there is the main result of Jennrich, and the result in the first part of my own paper which provides less obscure hypotheses. However, I found that people didn't pay enough attention to the second part of my paper in which still clearer hypotheses were introduced. I think these hypotheses are easier to check than those of Jennrich and those I gave in the first part of my own paper. But, probably, people doing scientific work just want to make a reference saying that consistency has been proved and do not bother about the hypotheses from which it has been proved.

Staying on this topic, it is interesting that both of you used convergence of measures in your papers. Is this an idea that was floating around in the nonlinear theory or was it an accident that both of you did try to approach it this way?

It was certainly not an idea that was floating around for these particular problems. But it was an idea that was appearing in various fields. For instance, certainly it was around the time the book by Billingsley was published. And there were a number of problems in mathematical statistics that were clarified by using this approach. I may even say that in economic theory this was a time when Hildenbrand introduced this kind of reasoning.

There is a strong emphasis in your book *Statistical Methods of Econometrics* on classical statistical inference and you give only a brief description of Bayesian statistical inference. I have the feeling, however, that you are rather sympathetic to Bayesian methodology. Am I correct?

Well it depends on what you mean by Bayesian methodology. If by Bayesian methodology you mean the development of an analytical apparatus with conjugate priors, and methods of computation of posterior distributions, then I am not so sympathetic. Because the method seems to me too cumbersome for what it achieves; in particular, it relies too much on assumptions about the prior distribution. But if you mean by the Bayesian methodology an approach to the understanding of logical foundations of inference, then I am quite sympathetic. I think the classical methodology was introduced because the Bayesian methodology looked too cumbersome to apply directly and relied too much on hypotheses about prior distribution, whereas relying on such hypotheses did not fit with the idea that statistical procedures



have to be objective if they are to be used in scientific research. So, I am fundamentally more sympathetic to classical methodology when it comes to actual procedures, but sympathetic to the Bayesian foundations of inference.

Although you emphasize the importance of theoretical models and statistical inference for good empirical work, you have shown interest in some of your publications in methods of data analysis. What advantages do you see in these methods for exploring the data in econometrics?

Yes, you're right to say "exploring the data in econometrics"; it is at the exploratory stage that the method of data analysis can be used because they are essentially of a descriptive nature. I would be inclined to distinguish between econometrics and socioeconomic research. In econometrics most of the time we already have some definite ideas as to the specification of the data generating process, and therefore classical inference methods are appropriate. But very often in socioeconomic research, we face questions that are still of a purely descriptive nature, with little concern for generality. Then, in that case, data analytical methods are often preferable to the assumption of a model that has no particular foundation except that it is convenient and that you have software for dealing with data in the context of this model. Now it is true that it is also good to look critically, even in econometrics, at the results that have been obtained by traditional econometric methods, to see whether they will stand up to the test of rougher data analysis methods. Because if they don't, then there is cause for concern and there may be something wrong with the specification. Also, there are cases in which we are unable to come up with a precise specification, in particular, in time series analysis when we want to study the pattern of lags. It is certainly not very good to have to introduce assumptions that are too strict on the patterns of lags and again some data analytical methods may be preferable for exploring the pattern of lags. So I am more sympathetic to these methods than a number of econometricians, but I would also object to the complete rejection of econometric methods that do start from a precise specification.

In your book there is, I think, a whole chapter on econometrics without models.

Yes, that is Chapter 1.

In the conclusion of your *Statistical Methods of Econometrics*, you give equal weight to three ingredients of good econometric work: the choice of statistical procedure, the subjective process of model construction, and the use of good statistical data. The accumulated experience of many econometricians suggests that your book provides an exemplary training in the first activity. What do you believe is the best way of training econometricians in the second and third activities?

Well, let me first object to one phrase in what you have said. You say that in the conclusion of my book, I give equal weight to three ingredients of econometric work and you mention as the second, "the subjective process of model construction." These conclusions, which come from one page of a long book, do not use the term "subjective process of model construction." They discuss the choice of the model and say that the model condenses all the available information that we have on the phenomenon and that this information is generally vague, badly formulated, and that it often appears fairly subjective, which does not mean that it is subjective. It appears fairly subjective, but the process of model construction is not a subjective process. It is a process which we should aim at making as objective as we can. Now I must recognize that, as in many other cases of human activity, ideals are never completely reached, but one shouldn't overstress the subjective aspects of specification. Now to come to your question, what should I recommend for the training of econometricians in the second and third activity. I think my recommendation would be to require young econometricians to do some applied work on relevant topics so that, starting from a given data base, they would have to consider, for instance, the determinants of consumption or investment and the like. If they are not blind, they have to look at the quality of the data and at their limitations. They also have to realize that they have to choose a specification. I think that this process of working on application is probably the best training for econometricians for the second and third activity.

In 1971 you wrote to the Editor of the *International Economic Review* about standards of rigor in the field of asymptotic theory in econometrics. Do you feel that the situation has improved in this regard since then? What would you consider to be the main single weakness in the econometrics profession now?

With respect to your first question, I am sure the standards of rigor of asymptotic theory in econometrics have much improved since 1971. As to your second question, what would I consider to be the main single weakness in the econometrics profession, I must simply be frank. I am not working enough now on econometric methodology to be able to say. I think that it is for people who are more involved in the research today to comment on this; and I would prefer to refrain from any sweeping statement that probably would not be valuable.

Much of your research in the last decade has been in the field of temporary equilibrium and the economics of disequilibrium. Do you see the work in this field leading ultimately to empirical research or perhaps to a new class of econometric models?

Yes, of course. One should, however, first say that temporary equilibrium and disequilibrium were already there at the root of many models that were

used before. For instance, any model of a Keynesian type is a model of temporary equilibrium, of general temporary equilibrium with disequilibrium features. So these features were already present. Now it is true that the recent theoretical exploration of various kinds of disequilibria in the economic system has suggested a new class of models to be explored, these models having stronger nonlinearities than the ones formerly in use. I am sure that these models will be the subject of empirical research when they are properly adapted to empirical work. In fact, there are already a number of such attempts to use these models for macroeconomic research. In particular, there is a prominent group of econometricians working in Belgium who are at the forefront of this field and there is also work being done by some French researchers and others.

Have you done some empirical work yourself?

No. Any empirical work that I have done recently on disequilibrium is not econometric. It's more of the data analysis type so far.

In your book *Profitability and Unemployment*, you analyze the relevance of the assumption of rational expectations and you express some critical views on this approach. I would like to ask your opinion on some of the conclusions of the rational-expectations theory. Let me start with Lucas' critique which may be formulated as follows: conventional econometric tests of the effects of policy stimulations ignore the effect of changes of some of the parameters of the model under consideration to the change in policy rules. For this reason these tests give misleading results. What is your own reaction to Lucas' critique?

I think the critique is worth considering. Misspecification may indeed distort the result of any statistical procedure. And there are many reasons for misspecifications in econometrics, unfortunately. Since the formulation of Lucas' critique, however, little proof has been provided that the misspecification that he pointed out played an important role. In principle, this critique is quite valuable and one should look at its importance in applied work. But I have the feeling that the critique has been overemphasized. There are many other reasons for misspecification that may be more serious than Lucas' critique. But that is an open question. It must be further studied.

According to rational-expectations economists, one should use methods of econometrics which are consistent with the way that both the generation of data and the error terms are modeled. In addition, to test the propositions, rational-expectations economists were compelled to deal with the issues of causality and exogeneity. You wrote a paper in the mid-1960s on causality and I believe you have retained interest in that question. What is your own opinion on the importance of exogeneity and causality concepts for econometric practice in view of

the insight that rational-expectations economists have given and the recent developments in econometric time series analysis such as the causality tests of Granger and Sims?

Questions of causality and exogeneity do not have to be related to the rational-expectations fashion. They are important questions and they are even fundamental questions for any econometric practice. One may view them at two levels. There is a fundamental logical level, which is what we mean when we say that one variable is the cause of another and what proof we give to back up such a statement. These logical questions have been bothering scientists for many years, and I should just confess that I don't feel at ease with it. I think that it is an important question and it cannot be brushed aside as some logicians sometimes have done. This is a fundamental question in any scientific research because, really, we are interested in what causes what and, therefore, we can not neglect the question. But I am not such an expert logician. I can simply say that what I have read on the subject has never inspired full confidence. There is also another level at which the question may be raised: should we try in econometric practice to test exogeneity and causality, as we usually understand them, for instance in the sense of Granger causality? I certainly think we should. This is a very important aspect of the development of good econometric understanding and, for some time, testing has been too neglected. One used to be concerned mainly with estimation, as if the hypothesis of a model were inviolate. Now testing exogeneity and Granger causality certainly are useful objectives.

According to some of the rational-expectations economists, econometric models must be firmly grounded in optimizing behavior. The common practice in building large macroeconomic models is to proceed as if you could use several optimization functions (for consumption, investment, etc.) in isolation from one another. These rational-expectations economists view this practice as partial equilibrium exercises and suggest that the force of rational expectation is that it imposes a general equilibrium discipline. This opinion has generated a debate on the usefulness of large scale macroeconomic models. What is your view on this issue?

I have, first, some minor comments to make and then I will answer the main question. The minor comments are that the optimizing behavior ought to be tested. It's not a hypothesis that should never be tested. It ought to be tested and perhaps it will be found that it's not always a perfect hypothesis. That's a side comment. Indeed, I think that assuming optimizing behavior is very powerful at the stage of model specification; there are good prior reasons to believe that optimizing behavior is a useful first approximation. Also, I agree with the view that a general equilibrium discipline is indispensable and is a good thing. What conclusion do you draw from that? You should certainly

not draw the conclusion that a general equilibrium discipline should be applied to the wrong model—for instance, to a model which is so simplified that it has no relationship to the phenomenon you are trying to analyze. And what I object to in some of the rational-expectations literature is not the general equilibrium aspect of it, but simply the fact that it is applied to such simple-minded models that they bear little relationship to the facts—for instance, models in which it is assumed that the general price level is directly dependent, quarter after quarter, on the condition of equilibrium between the demand and the supply of money. I am sure that the phenomenon of determination of the general price level is much more complex and, therefore, we should have a good model of reality with its complexities before we fully apply the general equilibrium discipline. Of course, sometimes we are not able to and, for a lack of a better alternative, we therefore use some partial equilibrium discipline. This, I believe, is a whole lot better than pretending you have a general equilibrium discipline, and assuming something which is not substantiated and is, in all likelihood, wrong. The question of the usefulness of large scale macroeconomic models has a different nature. If we can get away without a large scale macroeconomic model, good! But if in order to take into account the important complexities of the phenomenon that you are analyzing you need a large scale macroeconomic model, then you should use it.

Aggregation over space, time, and economic agents underlies the composition of much macroeconomic data. This is a topic on which you have done original research. You have also written on the subject recently in your book *Théorie Macroéconomique*, where you report some of the results of both exact aggregation theory and statistical aggregation over the distribution of agent attributes. What is your view of the widespread practice in macroeconomic theory and in macroeconomics of appealing to the concept of representative agent behavior in the construction of models? Do you consider it to be meaningful to try to estimate empirically the “deep structural parameters” that arise in this approach, particularly in certain rational-expectations models?

I certainly think that the specification of a model should give more attention to aggregation issues. Moreover, I think that these aggregation issues are of many different types and the important issues of aggregation vary from one case to another. I do think that people are not spending enough time in dealing with these aggregation issues. There is indeed a risk that the use of representative agent behavior in the construction of models is a source of some misspecification. I say there is a risk. It is not always the case that it will be the source of misspecification. But it can be and, therefore, it should be more seriously looked at. Now, as to the estimation of the “deep structural parameters,” of course, one should try to estimate the deep structural parameters and not the surface structural parameters. It is always better to go

deeper. But my own experience as an economist is that very often we cannot pretend to go very deeply into such matters. Again, what I object to in the expression “deep structural parameters” in certain rational-expectations models is that these rational-expectations models do not contain deep structural parameters. They contain parameters that the author of the model has classified as deep structural, but really they are not. They are not even good parameters of a model, whether at the surface or deep into it. Now I should not give the impression that I am against the study of rational-expectations models. In particular, a number of theoretical issues are worth looking at with rational-expectations hypotheses, or more simply with perfect foresight hypotheses. Moreover, the rational-expectation hypothesis is important in the study of a number of problems, such as problems of information in economics and many other issues.

In recent years, there has been an ongoing reappraisal of econometrics with particular, but not exclusive, attention to methodological issues that arise in macroeconometrics. This is a debate to which you have contributed in your published work and in your conference lectures. Do you see the econometrics profession emerging from this reappraisal, perhaps with renewed confidence in certain directions of research? Or do you see the debate and disquiet continuing with little agreement on methodological issues?

I am unsure how to answer these questions. It is clear that economists in general should not have undue confidence in what they are doing. And it was true that in the 1960s the economists were overconfident of their work and the media flattered the economists themselves into believing that they were mastering the whole range of questions that they were approaching. Now we are living in the opposite situation in which there is a lack of confidence in economics. Macroeconometrics and econometrics have been regarded in much the same light and I believe that there has been both an excess of confidence at some point and that there is perhaps now too low a level of confidence in what can be achieved. I don't know whether there has been much actual disagreement over methodological issues. Of course, there has been some debate on methodological issues, but this occurs in all sciences. I don't think that in econometrics this has been more acute than it has been in other sciences. So I don't feel worried really by a lack of agreement on fundamental methodological issues. In econometrics proper, the profession seems to me to be rather unified in its appraisal of fundamental issues. In macroeconometrics it's a bit different, of course, because of the choice of a specification. Some people don't always agree on the best way of defining the specification. But even taking that into account, I don't accept the idea, from the scientific point of view, that we have gone through a period of deep crisis. I think that the crisis of confidence was a natural reaction to overconfidence in the period before.

I would like to ask you another general question on methodology in economics and the role of econometrics. Economists seek to analyze important economic phenomena, such as growth, capital accumulation, and unemployment for more than 100 years. You even mentioned these phenomena at the beginning of the interview. You yourself have made important contributions on these questions. There seems to be a strong and persistent disagreement between neo-Keynesian, monetarists, Marxian, new-classical, and other economists about the answers to these economic questions. Some economists believe that econometric methodology will enable us to test economic theories and determine who is right. Other economists are rather sceptical about the value of empirical testing to arbitrate in these disputes; they say that econometrics has never been able to prove or disprove any economic theory. What is your opinion about the role of econometrics in the acquisition of economic knowledge?

It depends on what your level of aspiration is and what you mean by economic theory. When we make reference to Keynesian theory, monetarist theory, Marxian theory, and new-classical theory, we make reference explicitly to very ambitious theories that pretend to explain the development of economic activities in all its aspects, all at once. And I think that economists, when they are fooling themselves into believing that they have such theories, are simply too ambitious. They want to have quick and easy answers to big and difficult questions. Now it is true that econometrics is not able to discriminate among all these ambitious theories which, at the present time, should be seen as rather premature. It doesn't mean that someone who is doing economic research cannot benefit from knowing that there are big overviews of the functioning of economic systems that are expressed, for instance, like Keynesian theory, monetarist theory, Marxian theory or new-classical theory. But these are a bit like paradigms, almost ideologies, that any human mind needs to be aware of in order to select this or that hypothesis in a more objective construction of knowledge. If we look at these more objective constructions of knowledge that are feasible for economic research, I may say that certainly econometrics is able to test a number of things at these more modest levels. I personally think that I understand a number of phenomena much better now than I did when I was younger. Not only because I have had time to reflect, but also an accumulation of econometric work has occurred in the meantime and now we are less in the dark when we have to say something about price elasticities in international trade, about the life cycle behavior of consumers, about the determinants of business investment, and the like. So, progressively, we are acquiring some economic knowledge through econometric methods.

In your book *Théorie Macroéconomique*, you often report empirical results. This is not the case for your book *Leçons de Théorie Microéco-*

*nomique*, which was written much earlier. Since the publication of this book, the existence of surveys and panel data has had a profound inference on empirical work in microeconomics. In recent years we have seen the development and the testing of elaborate models of individual agents. In the light of this evolution, if you were now to write a book on microeconomics, would it be very different from your *Leçons de Théorie Microéconomique*?

No. Actually, a recent edition of the *Lectures on Microeconomic Theory* appeared which is not very different from the edition published in 1972. You will see some differences. For instance, there is some development in the part on imperfect competition, and the role of the theory of games. There is also some reference to laboratory experiments that were made recently in order to better understand the working of competition in very simplified cases. But I wouldn't go into the econometrics of microeconomic behavior, because there the word "microeconomic theory" is really misleading. I use the word microeconomic theory as being equivalent to the theory of allocation of resources and the theory of determination of relative prices. And those are theories of the economic system and not theories of individual agents. One needs hypotheses concerning the behavior of individual agents. But looking in detail at the behavior of economic agents in general theories of the economic system doesn't seem to me to be required at this stage. Of course it's better to have a proper hypothesis about the behavior of economic agents. But for the time being, I don't think it's necessary to abandon the hypothesis of optimizing behavior, which is good enough, so far as we know, in the building of these theories of an economic system.

You are, I believe, the only person in the profession ever to have written an advanced text on microeconomic theory, macroeconomic theory, and econometrics. Each of these works has been extremely well received. Is exposition at such a high level a strong French tradition? Has the writing of these major works left you less time than you would like for your personal research?

First, I should say that I am not so sure that my book on macroeconomic theory has been so well received. Let us wait and see. Its publication in French is still fairly recent, less than five years, and it does not yet appear in English. It's true that we like to think that there is a strong French tradition in good exposition. So I am pleased to hear your question. Has the writing of these works left me less time for my personal research? Undoubtedly yes. But less time than I should have liked, I don't think so. Because I don't think that I regret the time I spent on these works. It is just a question of what I feel to be my vocation, my professional function in this world. Certainly education of people, first, good students and, beyond that, the general public, is one aim that I place rather high.



Perhaps we could move now to your work in the French Administration. From February 1972 to October 1974, you were the head of the "Direction de la Prévision" at the Ministry of Economy and Finance. Could you explain to us the nature of the work you did there?

This involved a rather large group of people, something like more than a hundred professionals, working at the Ministry of Economy and Finance on questions of economic analysis, either microeconomic analysis or project analysis, on questions which faced the finance ministry. Therefore, it was a work of economic advising on a number of issues. We had to be aware of the problems, to study them, and to write reports. For instance, in the period of 1972 to the end of 1974, we had first, and speaking only of macroeconomic affairs, to understand the mounting inflation and to make some proposal in order to control it. Then there was the first oil shock and we had to study how to adapt to the new situation. I don't pretend we did such a good job but, in any case, that was ours.

At the end of 1974, you took up your current position as head of INSEE. Could you briefly explain to us the nature of your work?

My function is now to be the head of a large organization, first, over everything else. With 7,000 people, INSEE is in charge of the coordination of the French official statistical system, and of the production of the main part of French statistical data. Being the head of the official statistical office implies that you have to speak for the organization. I don't think I need to say more about that. One should say also that INSEE is a little special, in the sense that while a large number of people are working on producing official statistics, there is a small group of people working on economic modeling, economic analysis, and economic projections. Therefore, there is more macroeconomic work in INSEE than you would typically find in other statistical offices. So that is also a part of the work.

Has there been any interaction between the work which is done at INSEE and your own research or teaching activity?

Yes. There has been some interaction with the work at the "Direction de la Prévision" and with the macroeconomic work at INSEE. A good deal of my research during the past fifteen years has really concerned issues on which I thought that there was a gap, a gap between the development of practice and the state of the theory. When thinking over the way in which we would discuss macroeconomic issues when trying to advise a minister or the government or when reflecting over our way of analyzing these issues, I was conscious of a discrepancy between practice and what I was teaching. I couldn't blame the economic advisors or the applied economists, because they were dealing in their own way with the real problems. My blame went, part of the time, to economic theory, which was not developed well enough to provide

the background for analytical work. So, indeed, there has been more than what most people would expect in the way of interaction between my applied work at INSEE and my own direction of research.

May I ask you a personal question? The amount of books and papers you have been able to publish in parallel with your work in the administration is amazing. Like many other people, I wonder how you do it?

I don't know how to answer that. Life is long. Perhaps I am publishing too much, or perhaps I am not performing my job as I should as the head of INSEE. But what else can I say.

Can you more or less describe what is a normal day of work for you?

Yes. First, one should say that not all days of work are normal. For instance, there are the weekends in which I have the habit of working almost all of Saturday on my research and also doing some work on Sunday. There are also the occasions of seminars, and I hold a seminar about once a week during the active part of the year. There are meetings and conferences that I attend, not many of them, but a number of them. But if you take a normal weekday, I arrive around 8:00 A.M., which is very early for Parisian administration. Typically, the hired staff of the Parisian administration reaches the office a little after 9:30 A.M. From 8:00A.M. to this time, I am able to work on my research. But, of course, if I am called on the phone I have to answer it and some people know that I come in early. But, on the other hand, I can sometimes work without interruption a little longer. So, if I am working on a paper and I am undisturbed, I may be able to work up to 10:00 A.M.-10:30 A.M. or so. I work, of course, at night at home. I may read some papers and the like.

To change the subject, you are one of the few persons in the profession who has done important research work and, at the same time, has had the opportunity to participate, over a relatively long period, in discussions at the government level. How do you view, in general, the role of economists in the decision-making process of politicians?

I think our role is to impress upon the politicians a more objective assessment of what is feasible. In government circles and in the public at large, people tend to believe in dreams. Some tend to believe, rather simplistically, that they are able to obtain better results because they are smarter than others. Others tend to neglect all the conditions of success, all the constraints of economic activities. Therefore, as advisors, we have to keep reminding that a particular measure that is intended may have perverse indirect effects of a complex nature or may have a weaker direct effect than was thought.

Do you believe that you have personally had some influence on the economic policy of the past or present French governments?

Of course, many people have had some influence on the economic policy of the past and present French governments. But among them, I do think that I have had some small influence and I would say this in two ways. Perhaps the first way will surprise some people but it's more important than the second. The first way is that I have been teaching for many years. I wrote a book that is not so well-known, about national accounting in the 1950s. I wrote my book on econometric methods. I have discussed the building of macroeconomic models, and I have taught many people. These people that I taught, either through my books or through courses, grew up and were influenced by what they learned. And so I see the largest part of my influence as that of a professor and not that of an economic advisor. It so happens that as an economic advisor I also have had some minor influence. Although in order to have any influence as an economic advisor, the ministers and politicians or the people working directly for them should already be very well prepared to understand you.

Do you think it's very difficult for them?

Yes.

Taking a larger view of the future of economics towards the end of this century, what developments do you think will bring the most important gains in improving our understanding and management of national economic policy? Do you, for instance, attach much importance to the process of ongoing computerization of banking and financial activity which seems likely to eventually lead to accurate, comprehensive, and instantaneous monitoring of economic activity?

It's a very broad question and a very difficult one. Of course, if we look at the past development of economics and macroeconomics, we know what has been achieved and we also know where we have failed. What has been achieved is an accumulation of data, the development of tools for the analysis of short-term evolutions, even some tools that are more reliable than one would think for medium-term projections and the like. The failures are of a different nature. For instance, we economists were not able to avoid the disorder of our economies that developed in the late sixties, seventies, and eighties. Neither were we able to help our economies to adjust to these new situations. This has been particularly true of Western Europe. As to the future, you are for instance thinking of improving the understanding and management of national economic policies. First, national is a bit too narrow. We have to improve our understanding and management of the world economy and what we can do to achieve this. This is definitely very difficult to answer. My own feeling is that the main challenge for a better understanding is better theoretical analysis of disequilibria in the economic system, better economic analysis, and that may be still more difficult, of the exact role of mobility, flexibility, and structural rigidities. These are problems at the

interface between microeconomics and macroeconomics, and we don't even know well how we should approach them. It's a real challenge to our understanding. On the data front, I certainly believe that we should and shall make increased use of computerized data, not only of the banking and financial origin, but of other origins too. However, one should not expect that these computerized data will always meet the standard of good data. For instance, the computerization scheme is revised often and in order to have good data one must correct for all the revisions, which is sometimes very difficult. There are also many problems of matching the data sets, and problems of various kinds that I cannot list completely. Certainly, I see on the data front a possible gain to be obtained from the present evolution of economic and administrative activities. But the development of these computerized data is not likely to eventually lead of itself to accurate monitoring of economic activity. Given the weakness in our current theoretical understanding, the existence of computerized data isn't of itself going to solve the main issues.

Considering the developments in econometrics over time, I wonder if you might point out what you regard as the most significant steps in the evolution of this discipline?

Again, this is a difficult question. Let me just survey broadly how I see it. First, we need a good understanding of how to analyze nonexperimental data on economic phenomena. We may say, roughly speaking, that the work of the Cowles Commission and, of course, the work of a number of people at the same time, achieved that end. That was in the early forties. Then came a phase of consolidation: a consolidation in which one developed what appeared, at the time, to be necessary methods in order to apply the methodology of inference from nonexperimental data. And my book on the statistical methods of econometrics is a kind of witness to this consolidation phase. Included, of course, in this phase is the development of time series analysis, which is not so well covered in my book. After that came three developments that were a sequel to that phase of consolidation: the finite sample distributions that were badly needed and that have been worked out and developed; robust estimation, which was the second line of development; and finally the new emphasis on testing that had been neglected during the previous period. This being said, I think that there is now a new phase that is very important: this involves working on discrete data, on limited dependent data, and on a number of fundamentally nonlinear structures that are required in order to analyze microeconomic data or sometimes panel data and the like. This new phase is quite important and active at present. I am sure that this is too simplified a view of the development of the discipline. But since you wanted such a large overview, this is the one I would give you.

As far as research students are concerned, what do you see as important problems or promising areas to work on?

I am not a good judge, because I am not actively working on questions of econometric methodology now. But in the new phase that I just mentioned there are many problems to be solved which will be approached one after the other; a good many of them will be solved, at least to some degree. So there are important problems and I am sure a number of them are promising. If I may add a wish, coming from someone not directly involved in this theoretical work, it is that an econometrician or maybe a group of them should succeed in a unification of these new theoretical developments. For those not deeply involved in these developments, they appear to be quite complex to master. If possible, a unification would be quite useful.

#### SELECTED PUBLICATIONS OF EDMOND MALINVAUD

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