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## On methodology in macroeconomics-with application to the demand for unskilled labour

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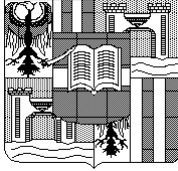
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**On methodology in macroeconomics –  
with application to the demand for unskilled labour**

by

Edmond Malinvaud

Working Paper No. 0113  
November 2001

# On methodology in macroeconomics- with application to the demand for unskilled labour<sup>1</sup>

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E. Malinvaud

## 1. Introduction

This is a pleasure for me to be here today in order to honour an economist who entered the profession almost a decade before me but with motivations quite similar to those which determined my own choice. If I had time I could be long about the comparison. Both of us discovered economics in our study of law, and economics soon became the only subject in which we as students invested more time and efforts than was absolutely necessary. Our experience of the social problems of the Great Depression was an important determinant of this behaviour. Another such determinant was to meet in economics an approach which had ample room for analytical reasoning and theorizing. Both of us could not make sense of our first reading of Keynes' *General Theory*, a book which would later mark so much our macroeconomic conceptions<sup>2</sup>. Yes, Professor Rothschild, it is indeed a pleasure to see face to face, and for this occasion, one of my older brothers.

I shall speak today on methodology in macroeconomics, a subject about which I forged a few strong convictions, progressively throughout the years by my experience of applied macroeconomics and of research in macroeconomic theory. Also by my exposure to, or even participation in, so many controversies that I now have a feeling of fatigue when remembering them. Let me just list a few of the questions behind these controversies. Is public planning feasible and useful, at the microeconomic, sectoral or macroeconomic level? Would economics benefit from being merged into an overall embracing social science? Do we need mathematical modelling? Which of the British or American Cambridge schools convey the right messages about the theory of capital? What should be the role of exploratory data analysis? Are structural macroeconometric models relevant? Why should we bother about a theory of general equilibrium with sticky prices? Does macroeconomics need microeconomic foundations? And if so, which? On his side Professor Rothschild took also part occasionally in such controversies<sup>3</sup>.

Speaking about methodology runs the risk of being tedious, even when the speaker has strong convictions. All the more so if these convictions stand somewhere in between the extreme answers that some others have given to the questions I just listed, and to other questions I could have listed.

In order to avoid this risk as much as possible, I decided to appear from time to time today as an applied macroeconomist, as a practitioner who is being asked whether it is right to subsidize the demand for unskilled labour, but a practitioner who wants to exhibit the methodology he is using for answering such a difficult question. It is relatively easy for me to take this stand because, as some of you may know, I have been for almost a decade, and still am, a supporter of such a subsidy in Western European countries.

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<sup>1</sup> Kurt W. Rothschild Lecture, Johannes Kepler University, Linz, 22 November 2001.

<sup>2</sup> See K. Rothschild (1991).

<sup>3</sup> I may mention here as an example K. Rothschild (1988).

Justification of this position implies a diagnosis (our countries will still long experience an excess supply of unskilled labour), a model (exhibiting how the subsidy would change the demand for unskilled labour), an econometric investigation (measuring by how much this demand would increase) and a recommendation (specifying rates and conditions for attribution of the subsidy, as well as resources for its financing). I shall speak about each one of these four parts in succession, in general or referring to this particular case. But clearly the second, model building, and the third, econometric investigations, will be particularly important, all the more so as modelling and econometric inference will be present already at the diagnosis stage, as well as in the last stage when recommendations will be worked out.

But taking a particular problem as an example I am running an opposite risk to the one of being tedious, namely to be too special for a methodologist. Interesting ideas in methodology have to be relevant for a wide spectrum of applications. This is why I shall not feel bound to refer systematically to the study of the demand for unskilled labour. I hope to make clear at each moment whether I am speaking of methodology in general or of the particular application I selected.

## 2. Diagnosis

(1) Methodological writings about macroeconomics usually ignore the fact that many economists spend their time at diagnosing. Such economists work in large firms or in government because macroeconomic trends are important for an appropriate programming of the operations of their employers. They work for public information because knowing how these trends will evolve may also be valuable for small firms, households and citizens. Diagnosing is moreover a crucial part in most research projects of applied macroeconomics. It is common to think of this diagnosing activity as concerning the current situation or short-run trends. But as we shall see in particular with the demand for unskilled labour, diagnosing long-run trends may matter still more.

Methodological issues appear in full force in diagnosing. They concern the conceptual apparatus, the collection of data, the processing of data in the elaboration of relevant synthetic indicators, the arguments leading to extrapolation of present trends and to the identification of future problems. Of course, these same issues appear in serious discussions about the methodology of economic theory. But their implications are then abstract and less vivid than when methodological weakness appears in public diagnoses. For instance, when we read critically the estimates which were spread from FAO during the two first postwar decades about the extent of hunger in the world, or the assessment given now by UNDP about the effect of globalisation on poverty in the Third World, we see that even important international institutions can lack methodological rigour on issues which receive large public attention.

(2) Without going deeply into the methodology of economic diagnosis, I should like to suggest its importance for epistemology by making four brief comments, one for each of the four parts of this methodology: conceptual apparatus, data collection, synthetic indicators, arguments.

That current problems stimulate the development of the conceptual apparatus appears in many cases. For instance it was not mainly for the needs of economic theory that the definition of income attracted interest in the first decades of the twentieth century but because of the introduction in some countries of an income tax, Irving Fisher then contributing to the debate. The definition was investigated further in the middle of the century by theorists such

as John Hicks and by national accountants. The purpose was then not only to apply Keynes' theory to macroeconomic policy but also to build an information system with a much wider range of uses.

(3) Diagnosis requires data. But collection of statistical data is costly and subject to a number of constraints. Statisticians have to make shift with these costs and constraints, while reaching, however, a fair degree of accuracy. This means that no data can be obtained for some concepts that the theorist would like to use. It would then be wishful thinking to neglect the fact. Indeed, applied macroeconomists know the ensuing limitations. We may wonder whether methodological reflections should not also pay more attention to them.

(4) Synthetic indicators are needed because macroeconomic knowledge has often to be comparative : we need to know for instance what would be the consequences of lower or higher interest rates. Such comparative assessments are subject to pitfalls, of which quite a few examples can be found in the history of economic theory. These came in particular from overlooking the distinction between quantities or real values and nominal values. Or they came from dealing incorrectly with the distinction. For instance, Professor Rothschild identified such a confusion in 1957 when discussing an article about the question whether a general rise in wages will lead to an increase in demand, hence employment. The author had focused on changes in money incomes, whereas it was more relevant for a European to consider real incomes, which led to different conclusions, as Professor Rothschild showed.

Synthetic indicators, such as the real wage rate or the aggregate volume of productive capital, are often needed for comparative assessments, but they are delicate to define, up to the point where conventions have to be chosen. Once this has been done, the indicators must be interpreted and used for diagnoses in conformity with the conventions on which they are based. We had many occasions to see the importance of this consideration, for instance when the sources of changes in productivity trends during the last few decades were being discussed.

(5) Finally, looking precisely at the arguments supporting macroeconomic diagnoses we cannot but notice that they are eclectic, and rightly so. This does not come only from the need to consider non-economic determinants, such as some of those explaining trends in oil prices or in the skill composition of the labour supply. It also reflects the state of our macroeconomic knowledge as it is embodied in macroeconomic theories.

Aiming first and foremost at the positive explanation of phenomena, and so forced to accept existing behaviours, structures and institutions as they are, macroeconomics as a scientific discipline has a very complex domain to study: a large variety of agents, many operations classified in multiple categories, operations also involving time and uncertainty in an essential way, legal and regulatory systems that vary from one country to another and change through time.

Under such conditions we should not be surprised to hear that theory has difficulty in getting a tight grasp of phenomena ; theory is indeed on the whole hesitant and heterogeneous. It offers incomplete models, which may supplement one another but are not fully incorporated in a consistent grand system. The accuracy of the results leaves much to be desired. It is almost always poorer than would be required for a persuasive explanation and for secure applications.

It would be not only dishonest but also counterproductive to hide and ignore this actual state of our macroeconomic knowledge. Being familiar with it will, indeed, suggest which approaches are likely to be the most appropriate in each particular case, and how to combine different approaches for a balanced and fruitful assessment.

(6) These considerations led us to already enter the domain of the next section of this talk, which will be devoted mainly to discuss a few important issues about the modelling of macroeconomic theories. But I should not leave the issues raised by diagnosis without being a little more explicit concerning the case of the demand of unskilled labour.

I stated the diagnosis as being that our Western European countries will still long experience an excess supply of unskilled labour. This had to be a long-term diagnosis for the simple reason that subsidising the employment of this labour makes sense only when a rather long horizon is entertained: a decade or more. Indeed, the intended effect of the subsidy in question on employment of the unskilled can hardly be felt in the short run. The aim is to provide appropriate incentives to employers, so that they decide to change their output-mix and their methods of operating, and to rely more on unskilled labour. For being beneficial such decisions have to be fairly irreversible. Their implementation requires time and confidence in the prospect that the subsidy was instituted for being long maintained.

The diagnosis, as I gave it, was discussed and disputed. It relies on the assessment of past trends and future prospects in the demand for, and supply of, unskilled labour. To make it short, I shall just say that, first, during the past five decades, upgrading of the skill composition concerned both the labour demand and the labour supply; but, second, after having been a bit faster for the supply than the demand, the shifts occurred according to a reverse ranking during the last three decades; third, the main cause for this, fairly slight, reversal were increasing competition from the newly industrialised countries, in manufactured goods with a high unskilled labour content, and increasing prices of services with a similar content, leading to a decrease in the home demand for these services; fourth, competition from the NIC is expected to keep increasing during the coming decade; fifth, there is little prospect to even maintain the speed of the quality upgrading of the supply of unskilled labour, considering the difficulties now experienced by teachers with children and teenagers, an increasing proportion of whom are growing up in unfavourable family and urban environments. I have no time to argue in detail with these five points and to look precisely at the available data. But listing these points permits us to reflect on the concrete nature of the terms in which the diagnosis often operates. No ideology is involved, but rather facts and their lucid interpretation.

### 3. Modelling

Let us leave for the moment the particular case of the demand for unskilled labour and turn our attention to some of the main questions posed by macroeconomic theories and their modelling.

(1) First, let us face the famous issue of the microeconomic foundations of macroeconomics<sup>4</sup>. To begin with, I protest against the way in which the issue was presented in some of the most heralded writings of the 1970s and 1980s. Since I am protesting I am not entering into a gentle balanced assessment of these writings. I am proclaiming instead that an archetype of methodological perversion emerged when proponents of the real business cycle

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<sup>4</sup> On this subject see K. Rothschild (1988).

theory claimed that their theory had sound microeconomic foundations in the now well-known aggregate, intertemporal, market-clearing model of a unique representative agent. This may have been a respectable academic reference, but certainly not a valid microeconomic foundation when the microeconomic business-cycle facts experienced by many firms and workers were market disequilibria and when the model was so cavalier in its ignorance of the difficulty of aggregating heterogeneous microeconomic behaviours.

After this declaration and in order to take the subject more positively, I find it convenient to distinguish three groups in the research projects that are necessary for consolidating the microeconomic foundations of macroeconomics. The first group concerns characterisation of the structures within which agents actually operate and of the kind of interdependence that results. Such research naturally focuses on deviations from the general competitive equilibrium model. Since there are in fact many deviations, we cannot expect to build a model incorporating all of them simultaneously. But looking at a variety of models and at their respective properties, we shall progressively better understand the foundations of our macroeconomic assessments. The literature contains a number of such models, notably all those which have been imagined in order to explain variations in involuntary unemployment between periods or countries.

More precisely, considering now the main theoretical tools structuring our understanding of the short and long-run macroeconomic phenomena, I may insist on the role of the temporary general equilibrium, the concept introduced by John Hicks in *Value and Capital* (1939), and on the so-called « neoclassical synthesis », which asserts that perfect competition provides the main reference for understanding growth and long-run phenomena in market economies, whereas the Keynesian equilibrium provides the appropriate reference for the short run.

As you may know, I worked in the late 1970s and early 1980s on macroeconomic versions of the fixed-price general equilibrium and on the role of the concept of classical unemployment, complementing that of Keynesian unemployment. I do not want to repeat today what I then tried to explain. I shall just make three comments about the work on fixed-price equilibria.

A number of theorists prefer models of temporary equilibria under imperfect competition to the extreme fixed-price equilibrium, as providing foundations to the macroeconomic theory of unemployment and I suppose Professor Rothschild belongs to this group of theorists<sup>5</sup>. I have no objection in principle to this position. The difficulties with it come from the tremendous complexity of actual forms of competition in modern market economies, from the logical uncertainties surrounding the definition of an equilibrium for such a context, and from the need in macroeconomics to solve the models up to the derivation of comparative statics properties.

Also, I am fully aware that the extreme fixed-price equilibrium provides just a starting point and that it needs to be supplemented by results of a dynamic study of the ensuing changes in market disequilibria. These results are precisely the purpose of the theory dealing with the dynamics of inflation and employment. This was well understood since the beginning in Keynesian economics. It is also well understood that this dynamics is complex and that theory about it needs crucial inputs from the econometric analysis of time series data.

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<sup>5</sup> See Rothschild (1947).

Finally, we must acknowledge the work done in the 1980s and early 1990s on the econometric investigations of macroeconomic models with explicit short-run fixed-price equilibria (for a survey see G. Laroque and B. Salanié, 1995). This work was appropriate for identifying the alternation between different combinations of market disequilibria, for instance from classical to Keynesian unemployment. But it had to face a number of rather thorny econometric problems, linked to the inherent non-linearities of the models. Because of these difficulties and of the simplifications they otherwise imposed at the structure of the models, this work did not generate a new family of macroeconometric models which would be currently used by teams in charge of policy analysis. The results reached are, however, worth remembering.

(2) The second group of research projects required for consolidating the microeconomic foundations of macroeconomics concerns individual behaviour of agents with respect to the kinds of choice which play a major part in the determination of the macroeconomic equilibrium and of its change through time: saving, investment, wages and markups, and so on. Since I became an economist more than fifty years ago, I have seen important progress in the knowledge of individual behaviour and of the laws that characterize it. This was achieved by a sound combination of improvements in models representing this behaviour and of econometric investigations from increasingly rich bodies of data. This progress continues, and there is indeed much to gain still, with respect to accuracy in particular.

The third group of relevant research concerns aggregation of microeconomic relations. It is divided into as many projects as there are types of such relations. For lack of time I shall just refer to one example today, at the end of the next section, concerning precisely the demand for unskilled labour.

But it is appropriate to remain now a little longer on one aspect of the study of microeconomic behaviour. Indeed, the present assessment of its use of the rationality hypothesis makes it timely to refer to an article published by Professor Rothschild in 1946. In this early article he pointed out that the rationality of *homo economicus*, as normally used in economic theory, had a restrictive meaning. Rationality there went beyond the notion that economic units act in conformity with some rational pattern. *Homo economicus* was meant to pursue self-interest and in the way fostered by a capitalist environment. Professor Rothshild argued that different types of rationality existed, fostered for instance by religious customs or by moral norms such as « from each according to his ability, to each according to his need ».

During a long subsequent period such an argument made little impact on the development of economic theory, in particular when this theory built foundations for macroeconomics. But more recently conceptions evolved, including in mainstream research. During decades we fruitfully investigated the many forms and consequences of narrow economic rationality, consequences with respect to time, uncertainty, information and its asymmetries, also with respect to the difficulties of interactive behaviour. After these decades we now feel the need to better account for bounded rationality, for the role of reciprocity and trust in mutual relations, and for the importance of social norms and behavioural routines<sup>6</sup>. We realize that psychology, cognitive sciences, sociology, and history may help us to achieve a better command of these features which look somewhat unfamiliar in the main core of our teaching.

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<sup>6</sup> The role of social norms and conventions in individual behaviour is discussed in Chapter 4, «Homo Oeconomicus-Homo Sociologicus » of K. Rothschild (1993).



Certainly, we should not expect this change in attitudes to overturn our theories. As Amartya Sen wrote : «It will not be an easy task to find replacements for the standard assumptions of rational behaviour – and related to it of actual behaviour – that can be found in the traditional economic literature, both because the identified deficiencies have been seen as calling for rather divergent remedies, and also because there is little hope of finding an alternative assumption structure that will be as simple and usable as the traditional assumptions of self-interest maximization, or of consistency of choice » (A. Sen, 1987). But we are likely to better identify systematic deviations from the conclusions reached by models of the rational household or the rational employer. Taking these systematic deviations into account will lead us to improved representations of actual behaviours, even if we do it piecemeal.

(3) After this opportunity of mentioning contributions received from other social sciences, I may just say briefly how I see more generally the division of the scientific work between economics and other social disciplines. In the first place, fruitful collaborations exist on well-specified issues. For instance, the field of labour economics overlaps at various points with that of sociology : in particular for the study of industrial relations, of people's work experience, and of poverty. On many relevant issues concerning either pure knowledge or policies, combining the two viewpoints of sociologists and economists is fruitful. Similarly, the empirical study of economic growth by economists was often found to be complementary to the study of history. Experimental economics has interesting joint projects with psychology or social psychology. In all such cases collaborations are welcome. They show in particular that the boundaries of our discipline are a bit fuzzy and flexible.

More delicate is the question of knowing what to think of the various cases in which, during the past decades, some economists extended their domain by deep entries into territories that were formerly considered to belong to others. The common characteristic in these cases was to see economists study how economic incentives would play in domains that were not viewed as specifically economic. For instance, beyond the economics of the household, the latter being working, earning, consuming and saving, the economics of the family discussed how you marry, have children, care for them, educate them, divorce... Who ought to judge the scientific value of such developments ? I submit that economists are not well placed for that. Rather, it belongs to demographers, psychologists, sociologists, lawyers and political scientists.

In contrast, I have absolutely no doubt or reservation to add when I assert the futility of the quest for a grand interdisciplinary synthesis, which would wholly renew economics.

#### 4. Inference from data

(1) Let me now turn to the role of inference from data in macroeconomics. Before anything else I shall assert the importance of the issue. I must do it because I still happen to read articles or books dealing with methodology and stressing macroeconomics, but ignoring wholly inference from data, this major part of our subject. Authors of such writings often stand as critics of what they loosely call neoclassical economics, an expression which should be avoided now because its original meaning is completely lost, in particular when any econometric work is implicitly or explicitly covered under the designation.

There are important reasons for a methodological study of inference from data in macroeconomics. First, since its beginning long ago, reflection about macroeconomic facts

and phenomena paid large attention to what was more or less systematically observed at the aggregate level. As time went on, this observation was more and more fostered by the development of statistics, of sample surveys and of statistical methods. Second, at present progress of macroeconomic knowledge owes much to the empirical research projects conducted by many economists. The outcome of this research, whether it is positive or negative for the hypotheses which are confronted with data, is often more relevant than articles that keep going over old ideas, formally or informally. Third, as we saw in the foregoing section, some of the most interesting developments in the models which progressively improve macroeconomic theories now come from econometric inductions about the behaviour of agents. Fourth, a serious study of macroeconomic policy issues often calls for extra examination of data, indeed in many cases it calls for fully-fledged new econometric investigations. Fifth, it would be wrong to think that this essential activity in its various forms raises no methodological problem. Quite the contrary: since except in some of its corners our science is not experimental, the methodology of induction is more complex and more delicate in economics than in the natural sciences. So, let me take it seriously and present my own ideas about it.

(2) I acknowledge right away to be holding an intermediate position in the debate about this methodology. I fully accept the point made by Lionel Robbins (1935) insisting on a major difference between economics and the natural sciences. Experimentation in economics can only have a limited scope, but economic phenomena result from the activity of human beings and from human institutions. Hence, much of what we know comes from our direct acquaintance with the rule chosen for the functions and operations of various institutions and from our direct understanding of the constraints and motivations shaping individual economic behaviours. Modelling this direct knowledge takes an important part in the construction of our science. But in many cases, particularly in macroeconomics, the results would remain too qualitative and limited if we were not taking advantage also of the existence of statistical data and other observations on the manifestation of economic phenomena, data and observations thanks to which we can make our models progressively more precise.

For this inductive process, the best guide and rationalization is what was called «the probability approach» at the Cowles Commission in the 1940s and is since then normally taught in econometrics, as well as more generally in mathematical statistics. Each inference on a sample of data takes place within a stochastic model, which is believed to provide a good representation of what is known *a priori* about the generation of these data. I do not doubt the value of the methodological principle that is so defined. But discussion may concern the question of knowing how to apply it in the practice of research, or even whether in all circumstances the principle is appropriate.

With respect to this last question I never shared the dogmatism expressed by some. I recognize the value of exploratory data analysis, which for instance searches for regularities in time series, and looks at how to contribute to theories from such facts, rather than rushing to tests and estimations within pre-selected models. At the exploratory stage, eclecticism is particularly wise.

This being said, I am also aware of the strict limits of data analysis, as sometimes practised by critics of methodologies developed within the probability approach paradigm. For instance, I was not really impressed when, in an influential article published in 1980, Christopher Sims attacked macroeconometric structural models while introducing his own approach (the vector autoregressions, now known as VARs). The objection to structural models was the existence, in their specification, of identifying restrictions stating that such

and such variables of the model were not appearing in the structural equation meant to represent at the aggregate level the behaviour of some agents with respect to a particular type of operations. Sims claimed that such restrictions were « incredible » because, for practically any variable, we could find an argument explaining its presence in any structural equation. But the weight he gave to this objection was clearly misleading.

In the first place, the specifications of our models do not have to be strictly exact, ignoring a restriction which we know to be approximately valid would be neglecting a useful piece of information. Moreover, it is by now well recognized that no firm conclusion could be drawn from purely empirical VARs, since identifying restrictions are also required for interpreting the statistical fits in a meaningful way. Indeed, the later developments of the VAR methodology recognised the value of the hybrid « structural VARs ».

(3) These statements of my positions in the methodological debate about inference from data in macroeconomics may look very abstract to a number of you. So, it is time for me to go back to the example of the demand for unskilled labour and of the stimulus this demand would receive from a subsidy lowering the cost of this labour. As I said in my introduction, we need econometric investigations in order to estimate the effect of such a stimulus. Estimation has to take place within a model which explains changes in the aggregate demand in question. The model has to contain other explanatory variables than the cost of labour. Indeed other changes occurred simultaneously. Precisely econometric investigations aim at sorting out the respective effects of changes in all explanatory variables.

A direct estimation of what we are looking for normally comes from quarterly or annual time series data of all the variables appearing in the model explaining the aggregate demand for unskilled labour. These time series have to bear not only on employment of unskilled workers and on an indicator of the cost of their labour for employers, but also on determinants such as the aggregate demand for goods and services, more or less stimulating the production of employers, the cost of capital, which may be more or less substituted for unskilled labour, the cost of skilled labour, and so on. Moreover, the model has to take account of the lags occurring before the full effects of changes in the explanatory variables are felt, all the more so as we are interested in long-term effects. Even without mentioning other problems concerning the conditions required for observed correlations to reveal causations, we understand that a correct identification of the size and timing of the effect concerning us now is complex. Indeed, the results of such direct estimations from aggregate time series leave very large margins of uncertainty as to the value of the long-run elasticity of the demand for unskilled labour with respect to its costs, so large that these results are hardly useful for policy decisions.

During the last two decades econometricians have been able to take advantage of the existence of large bases of microeconomic data, concerning results obtained from a given survey or from administrative records for a given year. Still better, they could benefit from panels of microeconomic data, many units (households or firms) being observed for a sequence of periods. Econometric processing of such microeconomic data also raises problems. In particular the units observed differ in many respects, which have to be modelled simultaneously for simultaneous estimation of the effects coming from various features explaining the difference. Again the accuracy leaves to be desired. Nevertheless the best estimates of the elasticity of the demand for unskilled labour with respect to the cost of this labour are now given by the econometric processing of microeconomic data collected on the activity of the employing firms or establishments.

(4) Using such estimates of effects observed at the microeconomic level is legitimate for our problem. Indeed, we need to assess the importance of substitutions between factors of production in favour of unskilled labour, and these substitutions take place first and foremost in the employing units. However, an aggregation problem may disturb the macroeconomic transposition of results reached at the microeconomic level. Let us briefly look at this problem.

It comes from the idea that relying only on microeconomic estimates is overlooking the fact that some substitutions between inputs may come indirectly. The relative prices of the goods and services whose production is unskilled labour intensive are likely to decrease when the cost of this labour decreases. Hence the consumption of those goods and services will increase. It is natural to conjecture that neglect of this indirect effect through relative prices leads to an underestimation of the increase in the demand for unskilled labour. Validation of the conjecture requires a model in which relative prices are endogenous, substitutions occur both in production and in consumption, and finally production functions of the various goods and services are heterogeneous with respect to the use of unskilled labour, so that relative prices change.

I built and formally studied such a model. It turns out that the conjecture as stated is not valid in full generality. The main implication of my abstract research should then be to show how to combine, for the problem at hand, estimates of elasticities of substitution between primary inputs in production units with estimates of other elasticities of substitution, between consumptions and between intermediate inputs broadly understood. It is, however, not an easy econometric task.

It is not surprising to find, in this case like in many others, that a serious study of aggregation leads to complex results. Arguing in macroeconomic terms is bypassing many of the complexities and heterogeneities of the real world. But precisely we should strive for better knowing under which sets of side conditions, about heterogeneity and the like, various estimates of important parameters are respectively valid.

## 5. Policy studies

I am now turning to the last part of my talk, namely to the methodology of studies which are directly aiming at policy recommendations. A complete account of this methodology would require, first, a survey of evaluations of concrete policies applied in various countries at various times. I am thinking, as an example, of the article in which Professor Rothschild drew the lessons to be learned from the rather successful Austro-Keynesian episode of the 1970s and early 1980s<sup>7</sup>. Besides such a background the complete methodological account would require, second, a description of the institutional environment within which the results of policy studies are used.

Let me say just a few words about this environment and the role of policy advisers<sup>8</sup>. Economic advisers have frequent and direct access to the political authorities for whom they are working and who will eventually decide. They are also supposed to be familiar with the studies produced by economic analysts. In other words, an economic adviser is an intermediary who, like other intermediaries, must understand both sides: what scientists can

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<sup>7</sup> See « Austro-Keynesianism reconsidered », chapter 26 in K. Rothschild (1995).

<sup>8</sup> For a longer presentation and references see « Economic policy making in practice », pages 1632-36 in E. Malinvaud (1998-2000).

deliver and what deciders really want. This function may at times experience tensions, but usually not to the point of making fuss about them. So today I must focus on the work of economic analysts who, studying policies, have to be aware of the aims pursued, to be conscious of the practical conditions of implementation and to be wary of the possibility of perverse unintended effects. After a brief reference to the policy of subsidizing the demand for unskilled labour, I shall turn to the use of macroeconomic tools in the study of monetary and fiscal policies.

(1) There are various ways of subsidizing. But in practice, since most Western European countries are financing a large part of their welfare transfers by a payroll tax, or something equivalent, the idea was to reduce the rate of the tax levied on low wages and to find alternative ways for replacing the so lost public funds. The minimum wage, and more generally the wages received by low-wage earners, could so be maintained unchanged while the cost of labour borne by their employers would be lowered and the demand for low-skill labour would be stimulated. After the studies devoted to this strategy and its implementation in various countries we can draw now some lessons.

The first one is the difficulty of estimation of the long-term employment effect of a permanent cut in payroll taxes. From what I already reported, you realise that this estimation cannot be precise. This so much so that it hardly matters for the econometrician to know how the loss in public revenues resulting from the cut will be compensated, as long as the compensation will not discriminate against employment of the unskilled. However, rough orders of magnitude can be given such as the following : if the decrease in the labour cost concerns the 20 per cent of wage-earners that are least paid and if the decrease amounts on average to 10 per cent for these workers, the long-run increase in unskilled employment will be something like 10 percent.

This is non-negligible, considering the importance of employment for those who are unemployed. But that will require time ; it cannot be expected to bring significant results at the short-run horizon that our politicians usually have in mind. Indeed, policy advisers in France had to repeatedly argue that it made no sense to introduce the policy as being an experiment. Such an announcement would foster the unwarranted expectation of quick effects. It would leave employers very uncertain as to whether to increase their staff of unskilled workers, a move which would in most cases imply rather irreversible decisions about changes in methods of producing and operating.

Another important lesson was to realize that perverse incentive effects could be generated if, after the cut, the payroll tax rates would increase too quickly with the wage, at the lower end of the scale. With the pattern initially given to the cut in France for instance, granting a net wage rise to an unskilled employee, either for seniority or promotion, was very costly for the employer. Maintaining such a situation was likely to lead to unhealthy practices in the personnel management concerning low-wage earners. More of those people than was earlier the case might have been kept in a kind of «poverty trap». A reconsideration of the schedule of the effective tax rate was decided and had to be studied in its financial and social implications.

(2) During the 1950s, 1960s and 1970s studies of monetary and fiscal policies were more and more systematically organised along Keynesian lines around simulations provided by structural macroeconomic models, which had been actively built in many countries after the initial lead of Jan Tinbergen and Lawrence Klein. But critics against the Keynesian approach and the macroeconomic models were progressively gaining momentum, up to a

point where they appeared diriment to many teachers and students. Eventually, however, the family of macroeconomic models has survived the storm, in the sense that institutions responsible for the study of macroeconomic policies keep using some such models and giving them a central role among the tools at their disposal. Simultaneously the value of the Keynesian approach, and how it ought to be used in conjunction with other approaches, was better assessed. This did not mean a return to the knowledge and practice of 1970. There were indeed profound changes, but not those expected by the critics of the time.

An obvious change is the present multiplicity of macroeconomic tools used by those in charge of policy studies. We no longer see in operation the very large models which were built thirty years ago. We rather see, still at the center, a medium-size structural model, but around it a number of smaller instruments: alternative structural models of the whole economy or of only one part of it, such as the simultaneous determination of prices and wages, or of the main aggregates characterizing the labour market. These secondary tools often differ by the force of the constraints that their structure imposes: from vector autoregressions (VARs) with their minimal identifying restrictions to small theoretical models with a minimal need for estimation or calibration. The central model itself is often modular, by which I mean that several versions are ready to be used, differing by what is assumed for some behavioural or adjustment laws.

This new image brings to the fore two related characteristics of macroeconomic forecasting and policy making: the role of judgement in the use of instruments, and the advisability of eclecticism in the reference to scientific foundations. These two characteristics were already present twenty years ago, but less visible for the layman. In particular, eclecticism was embodied in the structure of the unique big model instead of appearing in the multiplicity of instruments and the modularity of the central model.

Another change concerns the representation of phenomena that is given by structural models. It was in fact a continuous trend from the early models focusing mainly on quantity adjustments between supplies and demands expressed by rather simple-minded behavioural laws. Decisions of firms had to be seriously formalised, so as to give a proper and consistent representation of the joint determination of output, prices, employment and business investments. A closer examination of the labour market was required, both for the determination of labour supply and unemployment, and for integration of wage bargaining. The wage-price dynamics had to be better founded, with microeconomic theory being the main source at the specification stage, particularly for the presence of error-correction terms embodying ideas of the long run, and with time series regressions being the main source at the estimation stage. The list of such substantial improvements could be long extended.

Another important development, which I cannot describe here by lack of time, was emergence of a practice and a methodology for analysing the properties of macroeconomic models (see p. 1492-1510 in E. Malinvaud 1998-2000). Two conclusions emerged. First, the forecasting performances are poor, even relatively because they are not much superior with macroeconomic models to those reached with more elementary procedures. Second, few predictions of the models are robust with respect to small changes in the specification or the economic environment; hence, few can be delivered to users without a large number of caveats.

But what should we do, once these sad facts have been realised? Clearly, the right response is to accept them and keep working. Macroeconomic forecasts are useful, as well as are weather forecasts. Macroeconomic policies are often useful, if their effects are

competently predicted after a competent diagnosis about spontaneous trends, and if the uncertainty of the diagnosis and predictions is taken into account, among other considerations (lags, difficulties in implementation, and so on). For these useful purposes we have no better kinds of instrument to propose than those now used by the best teams in charge. As an expert wrote : « The experience of policy analysts-advisers is that macroeconomic models provide a formal and quantified framework that is an irreplaceable adjunct to the processes of policy thought and that there is no real alternative ».

(3) It is relevant to spend now some time to look into an issue which was much debated and about which a number of economists expressed strongly their positions. The issue is to know which hypotheses should be made in macroeconomic policy analyses about the expectations entertained by agents.

Everybody agrees that people's expectations matter because they influence behaviours. We have fairly reliable ideas on what this influence is likely to be. Data on expectations are thus useful in macroeconomic forecasting. More data and better knowledge of the influence of changes in expectations would contribute to improving the accuracy of forecasts. But for policy analysis we need more : we need to know also how expectations directly and indirectly change after a change in policy. In other words we need a theory about the formation of expectations. The point is important and was probably too often ignored in the 1960s.

There is no problem either with the idea that people have in their mind a little model of the future economic world and that people draw their expectations from their model. Adaptive expectations can be so rationalised if the model held by the agents applied to the past as well as it will apply to the future, if this model is a simple juxtaposition of as many autoregressive processes as there are variables on which expectations must be made, and if policy announcements are ignored by agents as long as the effects of policies are not seen.

For an analyst choosing to introduce into his or her structural model rational expectations, as usually defined, rather than adaptive expectations, which was the traditional practice, is tantamount to guessing that he or she will so have a better approximation of the agents' little models than would be provided by the autoregressive model I just sketched. This is a bold guess, but its consequences are worth considering, because adaptive expectations are not fully credible either.

But I must draw your attention to the fact that present structural models with rational expectations are very different from the theoretical models which were worked out under the banner of the rational expectations revolution. They allow for market disequilibria and imperfectly flexible prices, which have only recently and partially made their appearance in the models built by some unconventional participants in the Real Business Cycle movement. Structural models with rational expectations so avoid paradoxical implications of some classical abstract models, according to which, for instance, disinflation could be obtained at zero cost.

In most cases rational expectations appear in structural models for only few selected variables, but then often for those variables which may play a particularly strategic part in the behaviour of the solution. It is thus not surprising to realise that the choice of variables about which rational expectations will be assumed may much matter. There are moreover technical problems for finding the relevant solution in such forward-looking models.

Given these various facts it is advisable, at least until more experience will be available, for owners of macroeconomic models to operate with two specifications about the formation of expectations, one assuming adaptive expectations, the other rational expectations. It is the practice taken up at the US Federal Reserve Board. Systematic comparisons between what is obtained with the two specifications is interesting, not only for the work of the Board but more generally for macroeconomists. For instance, it was found that the model under-predicted the amplitude of GDP fluctuations when adaptive expectations were assumed but over-predicted it when rational expectations were chosen.

Before we leave this subject, let me still note that teams working with structural models containing rational expectations of some variables find these models convenient for investigation of relevant issues which cannot be well tackled with models assuming only adaptive expectations. Here are such issues: what difference does it make depending on whether a policy move was expected or not in advance, or still depending on whether the move is believed to be temporary or to be maintained permanently in the future? These are clearly relevant issues in policy analysis. The answers given by models with adaptive expectations, namely that it makes no difference, cannot be always right.

Phew! We covered a wide spectrum of issues concerning the methodology of macroeconomics.



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