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Some Thoughts on the Relevance of Macroeconomic Theories, and on the Role of Real Rigidities

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I will give two parts to my paper. The first one will concern the relevance of macroeconomic theories, particularly of those pretending to capture imperfect competition. The second shorter one will concern the role of real rigidities, about which I believe I disagree with many economists, even among those using very similar approaches to mine.

1 About the Relevance of Macroeconomic Theories

At this time in Paris, and more generally in Europe, I am tempted to claim right away that the macroeconomics of perfect competition has little relevance. Almost all real macroeconomic problems seem to be as many challenges to the macroeconomics of imperfect competition. But I must dominate this first reaction, for reasons that will, I hope, clearly appear: before rejecting an approach, one has to think about what alternative approaches can offer.

A warning is required from the start: I sadly deplore the trend in some quarters where nowadays just one recommendation is given for macroeconomic policies, namely to remove rigidities so as to make competition supposedly more perfect; this program is alleged by some to be both sufficient and necessary for solving present problems. I strongly disagree with such a view; but it is not the place to argue why. Indeed, my discussion of the relevance of theories would be much too long if it had to cover all the ground up to policy precepts.

After recognizing a role for the macroeconomics of perfect competition, I shall argue that, as we move to imperfect competition, the distance

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unavoidably much increases between the ideal theoretical constructs and the actual macroeconomic problems they are intended to eventually tackle. Such being the case, I shall claim that complete models, which formalize our present understanding of macroeconomic phenomena and serve for applications, cannot be much different from what they now are, although progressively improving on them must remain on the research agenda. But I shall admit that relevance of a theory may also lie in its analysis of some particular relationship that plays a crucial role in important aspects of our world. Let me elaborate these various points in turn.

1.1. Role of the Macroeconomics of Perfect Competition

With respect to the actual phenomena it aims at explaining, a theory always is an approximation. A good theory must provide a useful approximation. But judging whether an approximation is useful or not must be recognized as delicate and subject to discussion. Well aware of this difficulty, I have to be explicit according to me the inherent simplicity and generality of models of perfect competition give them a premium over models taking some of the imperfect features of actual competition into account. As I shall recall in a moment, most models of general equilibrium with imperfect competition cannot even provide foundations for positive comparative statics arguments [1].

Starting from this fact, one may assign two kinds of role in macroeconomics to theories of perfect competition. They may either directly provide a useful approximation, or they may serve as benchmark from which such an approximation is obtained after some corrections.

Direct applications of the macroeconomics of perfect competition to long term phenomena often ought to be appropriate. For instance we can thus study the nature of technical progress and its effect on trends in relative remuneration rates; we can similarly discuss the value of the discount rate to be used in the evaluation of public projects; we can consider global environmental policies and elucidate the conditions under which development would be sustainable, and so on.

On the other hand, the mathematical tractability of competitive growth models exposed to real shocks does not suffice to make such models directly appropriate for the study of business cycles. Since most of us take as convincing the empirical evidence against the resulting business cycle theory, other models have to be used. Perhaps, however, real business cycle models can still provide a benchmark for more appropriate models.

Two conditions are required in order to make perfect competition useful as a benchmark. In the first place, direct treatment of the actual forms of competition must not be feasible within a framework having a sufficient scope for the intended applications. In the second place, these actual forms of competition and the actual structures within which they operate must not

1. This last sentence may appear somewhat unfair and be disputed by those who will closely study the articles published here. An opening address has to be moderately provocative.

deviate so much from those assumed in perfect competition models as to make any attempt at simply correcting the results of these models irrelevant.

I tend to look at some of the modelling practices to which we are used as providing the means to derive practically useful corrections. For instance a price may be assumed to be rigid, or the demand addressed to firms may be assumed to be finitely elastic, or adjustment costs may be introduced, or expectations may be supposed to adapt sluggishly; but all this is done within a framework that does not incorporate all consequences of the corresponding deviations from the perfect competition model serving as a reference.

1.2. Distance Between Actual Problems and Ideal Theories

I started from the idea for some actual problems, concerning for instance long term trends, perfect competition was an admissible hypothesis. I then implicitly accepted the notion that a well founded theory of growth with perfect competition was available and fully appropriate for dealing with the actual problems in question. This last requirement is of course not innocuous, but in favorable cases it can be met to some acceptable degree.

I should now like to draw your attention to the rather exceptional character of such favorable cases. As soon as we consider problems for which imperfections in competition are essential, as soon as we look at theoretical models in which such imperfections have been seriously taken into account, we are impressed by the tremendous increase in the distance between the two sides: the concrete side with the problems to be faced by policy makers, the theoretical side to which so much of academic research is devoted.

Think of any policy relevant analysis of the well known unemployment-inflation trade-off. Think of the skill mismatch with its consequences that social policies attempt at mitigating, more or less imperfectly because of interference with economic incentives. Think of debt deflation, which may be exceptionally important in some recessions and then recommend an exceptional stance of monetary policy and banking regulation. Think of the spontaneous volatility of exchange rates, even between countries intensely trading with each other, and the resulting challenge to the move toward a single market. In each one of the preceding cases, analysis has to consider deviations from perfect competition, but deviations that go deeper and are more complex than those I mentioned a moment ago.

On the other hand, think of modern trends in the theory of imperfect competition. Full general equilibrium models of the Blanchard-Kiyotaki [1987] type are limited to monopolistic competition and accept to consider very particular specifications as to economic structures, technology and individual behavior; nevertheless their ability to deal with some of the present pressing problems is hardly superior to that of perfect competition models [2]. Nash solution to the bargaining problem has been used extensively

2. Their main advantage is to show that, under severe restrictions, one can easily put in a general equilibrium framework the partial equilibrium arguments for monopolistic competition in case of increasing returns to scale.

in macroeconomic models of the labor market; but the relevance of its axioms for this purpose is hardly ever discussed. After so many developments, the theory of oligopolistic competition did not lead to much that could be useful for macroeconomics: one still belives, as Sweezy did in the 30's, that price rigidity has something to do with an oligopolistic behavior that may be approximately captured by monopolistic competition with kinked demand curves; but the argument was hardly made more rigorous after sixty years. Cournot equilibria are sometimes proposed and they may well turn out to have many more applications than has been realized thus far; but this has still to be seen. The role of asymmetric information is now much better analyzed; it is understood to explain current forms of contracts or features of industrial relations; but I have difficulty in recognizing the impact of this progress on macroeconomic theory; except as providing a remote justification for assuming some form of rigidity.

The preceding comments should not be misunderstood. They are meant to describe a stade of affairs and to point to a challenge for research in macroeconomic theory. My own conclusion is not to dispair and give up, but rather to be realistic, hence not overly ambitious. This recommendation has different implications for theories with a general equilibrium scope and for those dealing only with a well identified in some important phenomena.

1.3. **Prospects for Complete Macroeconomic Models**

According to me, economists paid too much attention during the past twenty years to some models claiming to represent temporary general equilibria with macroeconomic implications. The best expert in creating models of this kind probably is R. Lucas, who was able to imagine a number of prototype societies for which nice closed models could be built and solved, starting from supposedly deep parameters or fundamental behavior. The analytical achievement was beautiful enough to stimulate imitation by others. But if these fictitious societies are just "pseudo-worlds" and not useful approximations to our real world, the models are no more than curiosities, or exercises for training students. As for me, they are indeed almost that. I do think that few of them readly changed our vision of the world or our analysis of actual facts and problems.

Twenty years ago the macroeconomic models of the neoclassical synthesis and the macroeconometric models promoted for instance by L. Klein were giving a consistent set of tools. A little later research stimulated by the specification of fixed price general equilibria gave a new perspective and somewhat enriched the set of tools. Unfortunately ideological alternance and discredit of Keynesian policy precepts turned attention away from that type of macroeconomic theory. Some then claimed that imperfect competition models were opening a new and promising line of research. As I had occasion to argue in Malinvaud [1991*b*], this claim looks somewhat misleading to me because, in order to deal with the real problems, the research in question has to remain pretty much in line with the previous fixed price theory. I read the various writings of J.-P. Benassy about imperfect competition as implying the same conclusion (see in particular his survey, Benassy, 1993).

I realize, of course, that I am here referring precisely to the subject of this conference. I do not want to play down this subject. On the contrary I hope to learn much from the various papers. I am just reporting what is my present state of mind. What I am expecting is not new theories, but rather useful developments of previous ones. At my age and at this depression time, I shall find more satisfaction in progress within the previous appropriate framework than in any, however, brilliant, treatment of still another “pseudo-world”.

This statement makes it clear that I view market failures as important aspects of macroeconomic phenomena. But perhaps I should not hide reservations I have with respect to the relevance for our purpose of some recent developments of the fundamental microeconomic theory of prices and resource allocation, concerning for instance sequences of rational expectation temporary equilibria. I feel uneasy about them because I am not convinced of the macroeconomic implications they are claimed to bear by some of the economists working in this field. The same kind of reservation applies to the macroeconomic relevance of formal results about the multiplicity of equilibria. I tried to explain my position in this respect in MALINVAUD [1992].

1.4. Clarification of Some Privileged Links

There is another approach to the study of actual forms of competition and of their macroeconomic implications, namely the close of particular and crucial relationships. I find this alternative approach at least as needed as is research concerning the general equilibrium features.

Let me give three interesting cases. The level and evolution of wage rates are determined as a result of notoriously complex bargaining, operating within many institutional constraints. Labor economists are able to report on a lot of specific studies, dealing with particular industries at particular times. How can their results be aggregated into a global relationship, on which macroeconomists could rely? A large number of econometric fits of Phillips-type equations is also available. Is there nothing better in order to represent the particular form of imperfect competition that prevails on the labor market? Or do these various empirical studies bring a general support to the kind of bargaining models that theoreticians are more and more often using?

Because of the risk involved and of the difficulty for lenders of knowing it precisely, many credits are subject to quantitative constraints. Those are more or less binding, more or less tight, depending on overall business conditions, on the financial situation of the banking sector and on the degree of competition between banks. How can we characterize the forms and intensity of credit rationing? How can we explain it? How can we trace its effects?

Macroeconomic performance is related to the dynamism of enterprises when they decide their development. This is reflected in their investment, which has rightly been the subject of much attention by macroeconomists, with however less success than was wished. It is clear today that business investment depends on many factors. But I wonder whether more role ought not to be given to the forms of competition and their changes, from one country to another, from one period to another. For the time being, this role appears only indirectly in the macroeconomics of investment, through profitability and internal funds.

For making progress on anyone of these three cases, as well as on similar other ones, we need to collect data and to repeatedly look at them, within the framework of alternative models. We know that work of this type is unfortunately less praised in our profession than purely theoretical research, in which one can exhibit one's mathematical expertise. Too little reference according to me will be made during this conference to empirically oriented investigations. I believe I must point to this fact, so that some of us at least feel uncomfortable about it.

2 About Real Rigidities

R. Solow [1986] was addressing those taking part in the first round of the European unemployment program, in which parallel national studies were performed in order to find and test explanations of the rise in unemployment in our various countries. R. Solow thought it necessary to "get the questions right" on three tissues, particularly on the supposed role of real wages. He argued that the explanation should start from exogenous variables, such as the nominal wage rate and that the real wage rate could not be such an exogenous variable.

R. Gordon [1990] wrote in the *Journal of Economic Literature* a long article on the ideas that were characterizing "New Keynesian Economics" and on the econometric and other justifications for these ideas. He put a great emphasis on "the division of a change in nominal GNP growth between changes in prices and output". I then asked myself the following question: in which sense is nominal GNP growth causally antecedent to real GNP growth and to price changes? I did not find the answer, but I then realized that, for quite a few economists, nominal GNP was indeed considered as more exogenous than real GNP. Again saying something about a nominal magnitude was seen as giving a more fundamental explanation than saying something about the corresponding real magnitude.

2.1. Nominal Rigidities in the Neoclassical Synthesis and in New Keynesian Macroeconomics

It seems to me that this vision finds its origin in neoclassical theory, where the general equilibrium of markets and behaviors is seen as giving a determination of real magnitudes, nominal magnitudes being then proportional to the quantity of money, except for exogenous changes in the velocity of money. Starting from there, the neoclassical synthesis argued there could be real feedbacks from the monetary equilibrium. Quite explicit in this respect was D. Patinkin [1956] who distinguished two cases depending on whether the neoclassical equilibrium would imply or not a decrease in the general price level with respect to the preceding period: in the second case neoclassical theory would apply, but in the first case downward rigidity of the price level would lead instead to a Keynesian equilibrium.

SOLOW [1986] discusses such an underemployment equilibrium in which firms have monopoly power and are price makers, so that the general level of commodity prices is endogenous and flexible. But the nominal wage rate is rigid and exogenous, simultaneously with the quantity of money. It is then indeed clear that the real wage rate is endogenous, simultaneously with the level of employment.

The assumptions are different in the BLANCHARD-KIYOTAKI [1987] model of monopolistic competition because workers have monopoly power in the supply of their labor and so are wage makers. Hence, nominal wages are flexible. Firms are also price makers, but with the proviso that they have to bear menu costs when they change their prices. So, these prices are imperfectly flexible: small changes in the quantity of money react on employment, not on prices.

2.2. **Why Nominal Rather than Real Rigidities?**

The argument in favor of nominal rigidities appears to be that they would correspond to facts whereas real rigidities would not. Solow is quite explicit when he writes: « groups of workers and employers cannot bargain over the real wage », and when, assuming an exogenous nominal wage, he means that bargaining is over the nominal wage and can be taken as exogenous in macroeconomic analysis.

The argument looks quite weak to me, when it is placed in the context of the many simplifications otherwise accepted in macroeconomics and in the context of many econometric results showing that changes in the price level are quickly transmitted into equal relative changes in the nominal wage rate. Why should the macroeconomic equilibrium be so dependent on a feature that is just a transitory short term one? Are not the exogenous factors affecting the respective positions of workers and employers in wage bargaining more real than nominal?

From a formal point a view, real rigidities can have rationing effects in the same way as nominal rigidities do. This was well shown within the fixed price equilibrium research program. So, looking for real rigidities as an explanation for unemployment is logically just as well founded as looking for nominal rigidities.

In other words, I tend to see the preference given to nominal rigidities as an unconscious and unfortunate consequence of the development of the theory through the past four decades. When trying to explain the rise of Western European unemployment, economists most often refer to shocks that are more easily classified as real than as nominal. With just a bit of provocation, I may even ask whether a good framework for analysis of the consequences of these shocks cannot be given a model in which money plays no role, prices and remuneration rates being determined up to a multiplicative positive constant.

2.3. **An Explanation based on Real Rigidities**

In order to show that such a framework is feasible, I may be permitted to refer to a model I proposed in MALINVAUD [1991 *a*]. It is meant to characterize medium-term effects of exogenous shocks in our open economies. It is specified purely in real terms. The real wage rate is endogenous but rigid, to the extent that it does not clear the labor market; it is the result of wage bargaining. The world real interest rate is exogenous; but the domestic interest rate depends sluggishly on the balance of payment surplus.

When presenting the model and its comparative statics implications, I claimed it could support the following explanations of actual medium-term changes in European unemployment, these explanations being in terms of known changes in exogenous variables:

– the exogenous changes from 1970 to 1977 can be characterized mainly by (i) the social unrest and malaise that weakened the position of employers in the wage-bargaining process, (ii) the new uncertainties of the world economy, (iii) the deflationary impact of the first oil shock; the first change had effects going in a reverse direction from those of the two last changes; but intensities differed depending on whether employment or the wage rate was concerned; the overall effect was weak on the wage rate, whereas it meant a clear rise in unemployment.

– The exogenous changes from 1977 to 1983 combined a strong deflation, due to the second oil shock and to the reversal of demand management policies, with a sharp rise in world real interest rates; as a result, there were negative effects on both employment and the real wage rate.

– The exogenous changes from 1983 to 1989 were (i) an increase in world demand, (ii) a change in attitude of the European people, which gave more power to firms in wage-bargaining; starting from a situation of low profitability, these two changes improved the demand for labor; on the other hand, the global effect on the real rate was small.

Of course in this paper, it would not be proper for me to argue more for the value of the story I just gave. Its point is simply to stress that relevant macroeconomic explanations can be given directly in terms of real rigidities and "real" shocks, as long as the object to be explained is not a nominal magnitude such as the rate of inflation, but a real magnitude such as the rate of unemployment. Even when a nominal magnitude is to be explained, it may very well be that real rigidities and shocks are more important than nominal ones.

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