

# The New-Classical Contribution to Macroeconomics \*

## I. - Introduction

Macroeconomics is prone to "Revolutions" — intellectual upheaval in which some new idea or ideas claiming to establish fresh and valid insights into the workings of the economic system sweep away a prevailing orthodoxy. The last fifty years have seen the "Keynesian Revolution" overwhelm "Classical Economics" so-called, to be succeeded in turn by a "Monetarist Revolution" which seemed to overthrow "Keynesian" economics. In the last fifteen years or so "Monetarism" has in turn yielded to a "New-Classical Revolution" which self-consciously, and much more thoroughly than Monetarism, has sought to re-establish macroeconomics on foundations that bear a close resemblance to those of certain strands in pre-Keynesian economics.<sup>1</sup> In every case, the superiority of the "new" approach has undoubtedly been oversold by its adherents, but, at the same time, insights and tools of lasting value have also been added to the corpus of economic knowledge.

This paper is devoted to assessing New-Classical ideas, and to asking what of lasting importance this school of macroeconomics has contributed since the early 1970s. It deals in turn with the relationship between New-Classical Economics and Monetarism, the relative ex-

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<sup>1</sup> For an account of the nature of "revolutions" in economics, illustrated with reference to the Keynesianism and Monetarism, see JOHNSON (1971). A number of commentators (*e.g.* TOBIN (1981), HOWITT (1986)) treat New-Classical Economics as a "Mark 2" Version of Monetarism. For a contrary view, see LAIDLER (1982, Ch. 1) who argues that whereas, from the point of view of the analytic structure of the models it utilised, Monetarism was a development of Keynesian Theory, New-Classical Economics in important respects is a throwback to the Austrian economics of the 1920s and early 1930s. This theme also runs through much of this paper but, because the adjective "neo-Austrian" seems to upset some people, I have not used it here.

planatory power of these two bodies of doctrine over empirical evidence, and the claims of New-Classical Economics to embody a superior analytic method. It argues that, although the particular ways in which New-Classical Macroeconomics has applied its basic ideas, notably in its insistence that the interaction of the maximizing behaviour of individuals be analysed in the context of continuously clearing markets, and that agents' expectations be represented by the predictions of the true model of the economy in which they operate, are unnecessarily restrictive, its stress on equilibrium behaviour conditioned by the state of individual agents' expectations as a basis for macro modelling is nevertheless valuable, and has been salutary for the discipline.

## II. - Monetarism and New-Classical Macroeconomics

New-Classical Macroeconomics was initially a response to the inflation of the 1960s and '70s, and to Monetarist analysis of that inflation. Indeed, in its earliest manifestations, it appeared to be nothing more than an attempt to restate Monetarist analysis with greater rigour than its pioneers — notably Milton Friedman — had achieved.<sup>2</sup> In order to put matters in perspective it will be helpful to recall the nature of the intellectual problem which that inflation created for most macroeconomists. Quite simply the empirical evidence it generated proved to be utterly inconsistent with then prevailing Keynesian views about how the economy worked, and about how policy could be used to improve its performance. Expansionary demand side policies, predominantly fiscal, could, according to that orthodoxy, generate lasting reductions in unemployment at the cost of somewhat higher, but nevertheless stable, inflation. When the Keynesian experiment occurred, it failed.<sup>3</sup> Gains in output and employment, where they material-

<sup>2</sup> LUCAS has made this point on a number of occasions, see *e.g.* (1980).

<sup>3</sup> The choice of the word "occurred" is not accidental. Though in some places (*e.g.* Britain) fiscal expansion in the mid-1960s and again in the early 1970s was deliberately used in an attempt to generate real "growth", the experiment in the United States was less wholeheartedly and self-consciously "Keynesian", but had a great deal to do with the politics of financing the Vietnam War. Note that, in this essay, I am using the adjective "Keynesian" to refer to the economics of what LUCAS *e.g.* (1980) referred to as the "Neo-Classical Synthesis". I am not talking about the "Economics of Keynes", to borrow LEIJONHUFVUD's (1968) phrase.

ized at all, proved to be temporary, and inflation, instead of shifting once to a new higher level, rose continuously.

Monetarist macroeconomics (whose components were available before the event, be it noted) explained these facts by arguing: first that Keynesian orthodoxy had underestimated the role of the quantity of money as an influence on aggregate demand in general and the behaviour of prices in particular; and second that the idea of a stable inflation-unemployment trade-off — the Phillips curve — was based on an implicit assumption that the private sector of the economy suffered from perpetual money illusion. To the pressure of aggregate demand as a proximate influence on the inflation rate, Friedman (1968) (not to mention Phelps, 1967) added the expected rate of inflation. Furthermore, because Friedman viewed inflation expectations as deriving from past experience, and as being formed in such a way that expectations would in fact come to catch up with experience eventually, he argued that any attempt to reduce the unemployment rate below that determined by the normal frictions inherent in the labour market would lead, in the long run, not to higher, but to rising, inflation.

From the point of view of policy prescriptions and empirical judgements about the reliability of particular functional relationships in the economy, Monetarism presented a clear alternative to Keynesian orthodoxy, but constituted no radical theoretical challenge to it. Keynesian models already contained a demand for money function, and if Monetarism was correct in arguing that this relationship was more stable than had in the past been believed, such a modification could easily enough be accommodated.<sup>4</sup> If expected inflation belonged as an extra variable in the Phillips curve, and depended upon the past behaviour of inflation, that would alter one's view of what demand management policy could accomplish, but it did not require any fundamental change in economists' vision of how the economy worked. There is no stronger evidence in favour of the latter judgement than the fact that the first explicit Monetarist analytic models were recognizable extensions of the IS-LM model.<sup>5</sup> Moreover large scale Keynesian econometric systems proved easily able to absorb Monetarist ideas as well.

<sup>4</sup> HARRY JOHNSON as long ago as (1970) noted the effects of "conditioned Keynesian reflexes" in preventing the idea of a stable demand for money function being incorporated into British Keynesian thought. American Keynesians, such as JAMES TOBIN (*e.g.* 1981) and FRANCO MODIGLIANI (*e.g.* 1977) were much more open minded in this respect.

<sup>5</sup> Thus MILTON FRIEDMAN's early 1970s "Monetary Framework" (1974) was explicitly cast in IS-LM terms, while BRUNNER and MELTZER (*e.g.* 1976a) used an extended IS-LM model to

The difficulty here was that the new version of the Phillips curve was hardly more satisfactory than the old one from an analytic point of view. Though the proposition that money wages and prices tend, given expectations, to rise faster the higher the level of aggregate demand in the economy, might be a plausible enough empirical generalization, it does not constitute an explanation of the phenomenon which relates it to the purposeful maximizing behaviour of individual economic agents. The Monetarist "expectations augmented Phillips curve" was an empirical observation in need of an explanation, not a well grounded structural relationship in its own right. In attempting to provide an explanation of it, New-Classical economists, and in particular Robert E. Lucas Jr. (1972), set in motion the "New-Classical Revolution", based upon two analytic devices, namely the aggregate supply curve and the rational expectations hypothesis.<sup>6</sup> Though the rational expectations idea has probably attracted more attention, it is its use of a particular version of the aggregate supply curve which constitutes the most fundamental innovation of New-Classical Economics. Keynesian Macroeconomics (including its Monetarist variation) can accommodate rational expectations, but it cannot be reconciled with the universal existence of the continuously clearing flexible price competitive markets which are a *sine qua non* of the "aggregate supply curve" explanation of the Phillips curve.

Sticky prices lie at the very heart of Keynesian Macroeconomics, and it explains quantity fluctuations in goods and labour markets as equilibrating movements arising because prices do not immediately change when aggregate demand shifts. The postulate of price flexibility

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expound their important insights about the role of credit markets in the process of money creation. So strong were the IS-LM roots of Brunner and Meltzer's work at that time that at least one commentator, DORNBUSCH (1976), was misled into believing that their essential contribution could be grasped without any extension at all to the IS-LM framework. Dornbusch's misconception did at least have the productive consequence of provoking an exceptionally clear statement from BRUNNER and MELTZER (1976b) of where they saw their contribution as lying. This author too, in analysing inflation unemployment dynamics in (1973), used a vertical LM curve IS-LM model as a starting point.

<sup>6</sup> The aggregate supply curve interpretation of the Phillips curve was not a component of early Monetarism though Friedman did accept it on at least one later occasion. In 1968 he said "Phillips' analysis... contains a basic defect — the failure to distinguish between *nominal* wages and *real* wages..." (Friedman's italics). In 1975, (pp. 12-14), in a pamphlet explicitly dealing with role of rational expectations and such in Monetarist analysis, while continuing to point up this nominal-real confusion, he characterized taking "the *rate of change of prices* as the independent variable" as "the truth" and taking "the level of *employment* to be the independent variable" as "error" (Friedman's italics). Friedman's acceptance of the aggregate supply curve interpretation of the Phillips curve, quite clearcut in this 1975 pamphlet, was never thoroughgoing, however. Thus, the "framework" of 1974 is used, without apology, as the theoretical starting point for the analysis contained in FRIEDMAN and SCHWARTZ (1983) and is quite incompatible with New-Classical style equilibrium Macroeconomics.

lies at the centre of New-Classical Economics. It has it that prices always move to equilibrate markets when demand shifts, but that individual agents, who are not fully informed about the behaviour of all money prices in the economy, mistake money price changes in the markets for the goods they sell for relative price changes. Hence they respond by changing the quantities of goods they supply. In the aggregate, an unperceived demand increase which raises the general price level therefore causes an expansion of output along an aggregate supply curve, and a fall of demand causes a contraction. Output and employment fluctuation such as we observe in the real world are, according to New-Classical Economics, voluntary responses to misperceived price signals. They occur because prices change. Keynesian economics (including its Monetarist variant) explains quantity changes as occurring because prices do not change fast enough to keep markets cleared. In this vital matter the contrast between the two approaches could not be more stark.<sup>7</sup>

Now the clearing markets hypothesis of New-Classical Economics is logically compatible with the idea that expectations are naively extrapolated from past experience, but the use of the two ideas in conjunction certainly strains credulity. If agents are in no way tied down by sticky prices, and make costly errors in quantity decisions because of faulty expectations about the behaviour of prices in markets other than those in which they are currently active as sellers, they have every incentive to make their expectations as accurate as possible, and to use all available information in order to do so. Maximizing agents should be presumed to form expectations, as Sargent and Wallace (1973, p. 328) put it, so that they "depend, in a proper way, on the same things that economic theory says actually determine that variable". Hence, though the literature of the 1960s-early 1970s does contain examples of models which combine clearing markets with adaptive expectations, such hybrids soon vanished to be replaced by a substantial body of New-Classical theory, based upon the twin hypotheses of clearing markets and rational expectations.<sup>8</sup>

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<sup>7</sup> I have discussed these issues in some detail in LAIDLER (1982) particularly Chs. 1, 3 and 4. It is this fundamental theoretical difference which leads me to treat New-Classical Macroeconomics as a distinct body of analysis, rather than as a simple extension of "Monetarism".

<sup>8</sup> Thus the paper by Lucas and Rapping, among others contained in the famous PHELPS volume of (1970), was based on just such a hybrid.

### III. - The Case for New-Classical Macroeconomics

Economists have no clearly agreed criteria for deciding among competing bodies of theory, but certain factors are widely accepted as being relevant. The ability to explain past events, or (even better) to forecast future ones, is highly valued, as is the closely related capacity to yield insights into the nature of policy options available, and into the likely outcome of whichever option is chosen. Also important are matters of logical coherence, and intellectual compatibility with other available and accepted doctrines. Proponents of New-Classical Macroeconomics have, at various times, claimed it to be superior to Keynesian and Monetarist alternatives on all three criteria.<sup>9</sup>

As will already be apparent, I quite agree that the Western World's experience with inflation and unemployment of the 1970s constitutes a massive refutation of "Keynesian Economics" as the term was understood in the mid-1960s. Nor would I deny that the New-Classical Macroeconomics of the late 1970s, emphasizing as it did the role of the quantity of money in generating inflation, and the crucial role played by expectations in the inflationary process, provided a superior explanation of that experience. If we were forced to make a choice between these two alternatives alone, we would have to accept the claims of Lucas and Sargent (1978) that their brand of Macroeconomics is the only respectable one available. However, we are not forced to make this choice.<sup>10</sup>

Before the inflation of the 1970s was dreamed of, Monetarists, such as Friedman (*e.g.* 1959) and Brunner and Meltzer (*e.g.* 1963) had been attacking Keynesian orthodoxy for underestimating the importance of the quantity of money. Furthermore, Friedman and Phelps (surely no Monetarist) had, as we have seen, criticized the idea of a permanent inflation-unemployment trade off in the mid-1960s on the grounds that the behaviour of inflation expectations, themselves endogenous to the structure of the economy, would render any such trade-off temporary.

<sup>9</sup> There has been a considerable change of emphasis over time here. The claim to have a superior method of analysis looms much larger in more recent defenses of their work by New-Classicals, than in earlier ones. Compare for example LUCAS and SARGENT (1978) to their recorded comments in KLAMER (1984) which led Kramer himself to argue that the New-Classical Revolution was a matter of method and rhetoric, rather than substance. HOWITT (1986) rightly criticizes Klammer for this judgement.

<sup>10</sup> As LUCAS himself acknowledged in (1980). Not so Sargent. See KLAMER (1984), pp. 66-67.

As we have also noted, however, these ideas could be, and eventually were, easily incorporated into otherwise orthodox Keynesian models, but Keynesian models *so modified* do very well indeed in explaining the 1970s.<sup>11</sup> A system in which prices are sticky (though not rigid), in which quantities change to absorb demand side shocks in the short run, and in which inflation expectations though mainly backward looking, are endogenous, can account for the 1970s experience at least as well as any New-Classical system based on price flexibility, clearing markets and rational expectations. To put it in terms of labels, the empirical experience of the 1970s does not force one to reject the "Monetarist" variation on the "Keynesian" model and embrace "New-Classical Macroeconomics".

The methodological criteria proposed by New-Classical economists in defense of their work have much in common with those sketched above, and implicitly or explicitly adopted by economists in general. If they did not, it would be hard to explain why their arguments have proved so widely persuasive. However, though claims to superior predictive power, and to deeper insights into the nature of economic policy processes, have certainly been made from time to time on behalf of New-Classical Macroeconomics, it has also, from the very outset, been presented as the product of a major advance in the application of analytical methods; and, with the passage of time, its proponents have come to place increasing emphasis on this last factor, claiming that their macroeconomics is more logically coherent and more closely related to micro theory than anything which went before it. It is certainly true, as we shall now see, that these are the strongest arguments in favour of New-Classical Economics.

To begin with, and uncontroversially, New-Classical economists tell us that an important purpose of macroeconomic models is to deduce predictions about the behaviour of an economy when subjected to various shocks. Equally uncontroversially, they argue that key components of such a model should be logically coherent and well tested propositions about the behaviour of individual agents. That these

<sup>11</sup> Which is not to say that no differences remained between Keynesians and Monetarists. For example, though there is little difference between this author's views on the way in which the economy works, and those expressed by say MODIGLIANI (1977) or LIPSEY (1981), I am much less optimistic than are they about the scope for stabilization policy. In general, there is considerable continuity between the policy views of Monetarists and New-Classicals, and it is this continuity that persuades TOBIN (1981) and HOWITT (1986) to take the work of the latter as an extension of that of the former. I regard theoretical differences as decisive in this matter of classification. See LAIDLER (1982) particularly Chs. 1 and 3.

propositions about individual agents should in turn be derived from analysis of rationally purposeful utility maximizing behaviour might be less universally accepted, but I do not wish to quarrel about this particular principle.<sup>12</sup> Reasons for controversy only begin to arise when we seek an institutional framework in terms of which it is possible to derive coherent predictions about the behaviour of the economy as a whole from knowledge of individual behaviour, and I shall argue in due course that the particular choice made at this point by the New-Classicals is not the only respectable one available to us.

Be that as it may, New-Classical economists propose that we model agents as operating in an environment of perfect competition, in which markets costlessly adjust to maintain the supply and demand for every good and service, not least labour, in constant equilibrium. Their competitive model differs from traditional treatments of perfectly competitive economies inasmuch as agents in it do not have full information about the structure of relative prices when they engage in trade. The demand and supply schedules which determine the equilibrium structure of market prices in a New-Classical model are conditional, not upon full and accurate information about that same structure of market prices, but upon agents' perceptions (expectations is the more commonly used word) of that structure. Because agents are supposed to be purposeful rational maximizers, they form their expectations so that they differ from the actual values of the variables in question only to the extent of a serially uncorrelated random error. For agents to operate on the basis of any other kind of expectations would result in them encountering unnecessary losses, and hence in violating the purposeful utility maximization assumption.

The "rational" approach to modelling expectations formation has been translated by New-Classical economists into the postulate that agents form expectations "as if" they were fully informed about the structure of the economy in which they operate, and make mistakes only to the extent that the economy is subjected to random exogenous shocks, either in the form of "policy surprise" — any systematic

<sup>12</sup> The reader's attention is drawn to the use of the word "purposeful" here. To adherents of revealed preference analysis, for whom consistent behaviour is logically *equivalent* to utility maximization, as opposed to being a *consequence* of it, the methodological case for New-Classical Macroeconomics, particularly as it relates to the rational expectations idea will not perhaps be as strong as I here present it. It might be noted, that in stressing the individualistic maximizing foundations of their model, as opposed to its empirical content, New-Classicals are reverting to a weighting of methodological criteria used by Austrians in the 1920s and '30s. See especially ROBBINS (1935).

component of policy behaviour being, and being perceived to be, part of the economy's structure, — or in more recent literature, random fluctuations in technology, "real shocks" as they are called. In such a framework, given currently (and only rather recently) available analytic techniques, it is possible to derive predictions about the aggregate behaviour of the economy directly from premises concerning individual behaviour. More to the point, these predictions in certain important ways mimic the behaviour of real world economies, specifically in the matter of co-movements of money wages and prices and quantities of employment and output over the course of the business cycle, and indeed the very fact that New-Classical Macroeconomics involves the exploitation of these new analytic techniques is sometimes advanced as an argument in its favour.<sup>13</sup>

The really critical point, however, as far as the proponents of New-Classical Economics are concerned, is that the above-mentioned analytic techniques, in their current state of development, can be used to derive macro-predictions with empirical content from nothing but well specified micro-premises only on the assumptions of representative agents operating in competitive markets cleared by flexible prices. A model which postulates some form of wage or price stickiness inevitably involves the use of some (allegedly) *ad hoc* element in forming the link between micro-postulates and macro-predictions. This is not because there do not exist models of individual maximizing behaviour that explain price stickiness, because there obviously do, but because our current analytic capacity does not permit us except in exceptionally simple examples (*e.g.* Howitt, 1981) to embed such behaviour in a model of the economy as a whole, to allow for the way in which such behaviour might influence expectations, and then explicitly to derive macro predictions.

As a result, those who wish both to postulate phenomena such as price stickiness and to build models with empirical content, are led to introduce qualitative empirical "laws" into them and to permit the data to find quantitative values for the parameters which characterize these "laws". One way of looking at the issues at stake here is in terms of alternative strategies for evading that perennial barrier to truly rigorous Macroeconomics, the aggregation problem. The New-Classical assumptions of representative agents plus perfect competition certainly

<sup>13</sup> LUCAS (1980) comes close to arguing along such lines. I do not find this style of argument persuasive.

permit clearly defined links to be established between individual and market experiments without recourse to empirical laws, but those links are only as defensible as the assumptions that permit them to be forged.

Even so, if we regard the presence of "free parameters", as Lucas (1980) calls them, in a model to be a fatal drawback, then New-Classical Macroeconomics, with its assumptions of universal competition among representative agents, perfect price flexibility, and rational expectations, has no rivals. If it is objected that perhaps empirical evidence might nevertheless have a role to play in such a judgement, the answer offered by the proponents of New-Classical Economics, notably Lucas (1980), is that, since their basic model uses no "free parameters", a model which fits the facts better, or at least as well, can always be constructed by adding one (or more) such parameter to a basic New-Classical system. Economic models are not supposed to be descriptions of all elements of reality (whatever that might be); and to show that greater descriptive accuracy may be achieved by the addition of free parameters is said to be neither surprising nor compelling as an argument against New-Classical Economics. I shall now turn to an examination of this argument.

#### IV. - Empirical Evidence and "Free Parameters"

I remarked earlier that there is no completely agreed set of methodological criteria for judging economic models. As a matter of simple logic, it cannot be denied that, if rigorous connections between maximizing premises and ultimate conclusions is regarded as the be all and end all of economic analysis, then New-Classical Macroeconomics is indeed the only game worth playing. The most that anyone who denies this viewpoint can do is explain why he thinks other criteria are relevant, show how they support his position, and hope that his reasoning will be taken seriously. Such is my purpose here.

My starting point is that the ultimate aim of economic theory is to explain observations, in the sense of deducing statements which describe such observations from more general premises. Moreover, and quite crucially, such premises should also yield other statements whose truth is not contradicted by the facts. The more general the predictive

power of a set of premises (and the more propositions about purposeful maximizing behaviour, and the fewer theoretically unsupported generalizations relying upon "free parameters" there are among them) the better. An economics which can deduce true predictions about all the phenomena that might interest us from nothing but premises about maximizing behaviour is presumably the ideal towards which we are all striving. That we are unlikely to achieve this ideal is not the point, though. Rather it is that, even if we did stumble upon it, we could never know this. The most we can ever be sure of about our models is that they have not been contradicted by evidence gathered to date. In the very nature of things we can never know that they are true in the sense that they never will be contradicted.

As a practical matter we must always be more concerned with criteria for choosing among less than ideal theories than with laying down unattainable and non-operational standards of theoretical perfection. For this rather humdrum task, primacy must be accorded to empirical evidence, because it is surely uncontroversial that a theory which makes systematically false predictions about some phenomenon is itself false, and in need of modification, no matter how closely it satisfies other criteria.<sup>14</sup> Even so we must be careful when we advance this last proposition not also to demand that a theory's predictions be "descriptively accurate". A theory may abstract from all manner of phenomena, have nothing to say about them, and hence be "descriptively inaccurate" (or incomplete), but that does not make it false. The question of falsity only arises when a theory yields definite predictions about some phenomenon which turn out to be untrue. Descriptive inaccuracy is an inherent quality of any abstract model; but falsity is not. To use a standard platitude of the elementary logic class as an illustration, the reason why the proposition "all swans are white" is false is not that this statement fails to mention feathers, and into the bargain has nothing to say about ducks; rather it is that some black swans do exist.

<sup>14</sup> In his contributions to KLAMER (1984) Sargent at one point appears to accept this view of the ultimate primacy of empirical evidence. See KLAMER p. 68. However, the general thrust of his work, and that of other New-Classicals seems to be to stress the importance of deriving results from what they take to be "first principles". For this indulgence in the "Cartesian fallacy" they are, rightly in my view, taken to task by Brunner in his contribution to KLAMER (see pp. 191-95). The reader who is familiar with Brunner's methodological views on these issues will recognize the common debt that we both owe to Karl Popper.

My reason for denying the inherent superiority of New-Classical Macroeconomics is not, therefore, that there might be interesting facts from which it abstracts and about which it has nothing to say; rather it is that it makes false predictions about the very phenomena with which it purports to deal, and that if it is to be rescued, parameters every bit as "free" as those utilized in the Keynesian (or Monetarist) alternative seem to be required. The original task which New-Classical Economics set itself was to provide a foundation in qualitative microeconomic reasoning for Friedman's propositions about the temporary nature of the inflation-unemployment tradeoff. The fact that it succeeded in doing so is, however, not an empirical argument in its favour. That statements describing a set of already known facts may be deduced from a model is evidence, not of its truth, but of the logical skills of the person who constructed it. An empirical test arises only when conclusions yielded by the same model about facts not used to discipline its construction, and better still, initially unsuspected, are compared with those facts.<sup>15</sup> Here, New-Classical Economics finds itself in trouble.

To begin with, it gets rid of the free parameter linking money wage and price changes to "excess demand" by postulating that the Phillips trade off reflects, among other parameters of the system, the elasticity of the supply of labour with respect to real wages. In doing so it yields a testable prediction about the quantitative relationship between inflation and employment fluctuations. Empirical evidence shows that the relative amplitudes of those fluctuations do not square up with what we think we know from micro-studies about this supply elasticity. Aggregate employment fluctuations seem to be systematically much too large relative to inflation fluctuations to be treated as movements along a supply curve of labour when the labour force misperceives nominal wage changes as reflecting real wage changes, and hence to be accounted for along New-Classical lines. Closely related, the nature of the interaction of employment and real wages over the business cycle is

<sup>15</sup> The danger here is one that Sargent is aware of. See KLAMER (1984) pp. 75-76. We may illustrate it from an earlier episode in the development of Macroeconomics. Their ability to deal with the conflict between time series and cross section evidence on the consumption income relationship did not constitute an empirical argument in favour of the FRIEDMAN (1957) and MODIGLIANI and BRUMBERG (1954) theories of the consumption function, but only confirmed the logical powers of their authors. They knew about the evidence in question before they constructed their models and calibrated them to it. The important empirical content of their theories which rendered them testable lay in their ability to tell us about other empirical regularities, which either had not been observed, or were not regarded as being related to the theoretical foundations of consumer theory, until new theoretical insights were put to work.

hard to reconcile with the New-Classical postulate that the real wage is always equal to the marginal product of labour and that employment fluctuations involve movements along a downward sloping marginal product schedule.<sup>16</sup>

In a New-Classical world, quantities change because prices fluctuate. Output and employment should therefore vary at least simultaneously with (or perhaps lag behind) the price level; but it is a stylized fact of real world business cycles that quantity changes seem to precede associated price level changes. Moreover, if the price level is free to move to keep the supply and demand for money in equilibrium, the economy should always be on its long-run demand for money function; but empirical observations suggest that the economy is often and systematically "off" this relationship for extensive periods of time.<sup>17</sup> In the early 1980s, predictions about all of these phenomena were put to the test in one real world experiment which was surely just as damaging to the New-Classical Economics of the 1970s as the experience of the 1970s was to the Keynesian Orthodoxy of the 1960s. Then, in a number of countries, sudden, but nevertheless well publicized, monetary contractions were followed by unusually low real balances (relative to the values of the variables determining their demand), rapid and severe output and employment contractions, and only later by price and money wage responses; according to New-Classical Economics they should have generated price changes on the spot, and, being well publicized, only a rather mild quantity response.<sup>18</sup>

The New-Classical economist does of course have answers to all of these questions. To begin with, monetary contraction will only have its major effect on prices if it is expected that the authorities will persist

<sup>16</sup> On the matter of real wages and employment, see GEARY and KENNAN (1982).

<sup>17</sup> See LAIDLER (1982) Ch. 2 and LANE (1983) for discussion of this matter.

<sup>18</sup> This is not to say that the 1980s experience was any more the outcome of a conscious attempt to implement New-Classical policies than was the 1970s the result of a conscious Keynesian experiment. Nevertheless, before the event, New-Classical economists did make confident predictions about the outcome of preannounced monetary contraction. Thus, Lucas is quoted by *Time* magazine, Aug. 27, 1979 p. 29 as having said "Ideally we should announce a monetary expansion policy of 4% annually for the next seven years and then stick to it. People would respond, and inflation would be cured with a minimal risk of a deep recession". The basis of such prediction as this was the SARGENT and WALLACE (1976) analysis of the effects of rational expectations on the ability of the monetary authorities to influence real variables. Nowadays it is claimed that this paper was taken more seriously and literally by its readers than by its authors. (See Sargent in KLAMER 1984, pp. 70-71.) Certainly, the opening of the paper in question suggests that the analysis which it contains is to be treated as a counterexample to a prevailing Keynesian view of policy, rather than as serious alternative, but its last two or three pages mount a strong case for treating it as just such an alternative.

with such a policy. The policy must, that is to say, be credible if it is to influence behaviour by way of its effects on expectations. In a New-Classical model the less credible is a policy, the more will the price level changes it generates be misread for relative price changes, and the larger will be the quantity responses. Perhaps policy was not, despite the publicity, credible in the early 1980s. As to the arrival of quantity changes before price level responses this could have been the result either of our observations of the price level being unreliable, because they are based upon posted prices rather than those at which trade "really" took place, or because the downturn in question did not stem from monetary contraction after all, but from some exogenous contractionary shift on the supply side of the economy. Why were economies apparently "off" their demand for money functions? Perhaps these functions were estimated using data that only imperfectly measure the true variables upon which the demand for money depends. In this case, an apparent departure of the economy from its demand for money function might be an illusion created by measurement error.<sup>19</sup>

It may, of course, be that all of these propositions have some truth to them, but it is also the case that they offer to the New-Classical economist a rich array of free parameters with which to rescue his model from empirical evidence. How fast, and by what mechanisms does any policy become credible? How can we test propositions about measurement error when they result from our inability to observe the true variables? How are we to allocate responsibility for a particular cyclical turning point between demand side and unobservable supply side factors without referring to the timing and amplitudes of price and quantity fluctuations? The point of all this is not to suggest that New-Classical Macroeconomics is unique in relying upon *ex post ad hoc* postulates about the values of free parameters to reconcile it with empirical evidence. The criticisms which its adherents advanced of alternative approaches for using free parameter were not without merit. The point is rather that New-Classical Economics appears to be in the same trouble as these alternative approaches, because it can avoid recourse to free parameters for just so long as it avoids confrontation with empirical evidence, and no longer. That can hardly be comfortable for proponents of an approach whose major claim to superiority lies in a claim that it avoids such problems.

<sup>19</sup> A New-Classical economist should treat the "credibility" alibi with care. Before the event, SARGENT and WALLACE (1976) p. 181, developed what they characterized as a "telling argument" against its empirical relevance. On the matter of the demand for money, see GOODFRIEND (1985) and KOHN and MANCHESTER (1985).

Perhaps the New-Classical economist would answer the foregoing argument with a "so what?". After all, Lucas did tell us in (1980) that the addition of free parameters to a New-Classical model would indeed improve its predictive performance. This answer will not quite do however. A Keynesian (or Monetarist) model, to the extent that it relies on expectations, must also face up to problems concerning the credibility of policy and hence is no improvement upon a New-Classical system in this respect. However, it can dispense with conjectures about unobservable supply side shocks, measurement error, and such, when confronted with the data. If we add the postulate of price stickiness to an ordinary full information Walrasian general equilibrium framework, we may model the occurrence of quantity movements in advance of price changes in the face of demand side shocks to the economy as an equilibrating mechanism, and we have no difficulties in generating persistence over time in fluctuations in real variables, including real balances. Nor do we have to puzzle over the relative magnitudes of price-quantity fluctuations. The empirical puzzles which require New-Classical Economics to add free parameters do not, that is to say, arise in the Keynesian framework it seeks to supplant, once a free parameter characterising price stickiness is allowed to do its work.<sup>20</sup>

The choice here is between two models, one of which (the New-Classical model) happens to yield predictions about output fluctuations without resort to free parameters, and one of which (the Monetarist version of the Keynesian alternative) does not; and it would be an easy one to make if other predictions yielded by the New-Classical model were empirically supported, but, as we have seen, they are not. The choice between New-Classical and Keynesian Economics is thus a choice about which free parameters to use and at what stage in the analysis to deploy them when modifying a standard full information Walrasian model. It is not about whether to do without them or not.

<sup>20</sup> LAIDLER (1985) demonstrates that price stickiness is an *alternative* to New-Classical assumptions in generating such results rather than a supplement to them, and does so in terms of a model in which agents are all "in equilibrium" in the sense of being able to execute their *ex ante* plans, albeit not with the expected results *ex post*. The model in question is not of course an equilibrium framework in the sense that markets are cleared by flexible prices. Instead quantity fluctuations play an equilibrating role in some markets. Note that some New-Classical Economists (e.g. BARRO (1977b)) have argued that even with sticky wage and price contracts it is possible to model quantity fluctuations as taking place "as if" they reflected appropriate market clearing responses to variations in agents' perceptions of the marginal product and marginal disutility of labour. This is true enough, but hard to take seriously as an empirical proposition, because it implies that agents have enough information to take market clearing decisions in the absence of price signals. For a discussion of this, see LAIDLER (1982) Ch. 3, pp. 90-92.



## V. - The Price Stickiness Postulate

In the light of the preceding discussion, the Monetarist variant of the traditional Keynesian model begins to look attractive. Moreover as I shall now argue, its attractiveness is further enhanced by the fact that the free parameters it utilizes are rather harmless, linking as they do rates of change of money wages and prices to the levels of "excess" demand and supply in particular markets.<sup>21</sup> To begin with, though Keynesian theory does not tie down the parameters in question to any particular quantitative value, they are nevertheless *not* left to take on whatever value might be needed to reconcile a model with any data it might encounter. These parameters are at least required to take a non-negative sign, thus ruling out a rather wide variety of logically possible observations whose real world occurrence would therefore refute the Keynesian model.

More important, the price stickiness postulate amounts to a good deal more than an unfounded *ex post* and *ad hoc* rationalization of otherwise inexplicable observations about the interaction of quantities and prices over time. It is, at the very least, a descriptively accurate empirical generalization whose truth is quite independent of any macroeconomic observations. In the real world, pricing in many branches of the labour market *is* characterized by contracts which set terms for money wages and endure for rather long time periods; similar long term contracts, also negotiated in terms of money, *do* characterize many final output markets as well; the contracts in question *are not* all negotiated at the same time, and they *do* overlap; it *does* follow from these facts that, in the aggregate, money wage and price levels *will* display just the kind of stickiness with respect to demand changes that Keynesian Macroeconomics postulates; and it also follows that quantities *will* indeed fluctuate, as Keynesian economics says they will,

<sup>21</sup> One must be careful here, because the phrase "excess demand" has strong overtones of "disequilibrium" about it and there is much semantic confusion in the Macroeconomics literature caused by contributors referring to any non-flexible-price-Walrasian system as a "disequilibrium" one. Excess demand is here used to refer to the difference between the level of output at which markets currently clear, and that at which they would clear if prices were perfectly flexible and all expectations were completely fulfilled. The literature with which I am dealing here treats the latter as a unique level of output, determined by tastes, technology, and market institutions, but recent work by DIAMOND (1984) and HOWITT (1985) on search equilibria suggests that we ought not to take such uniqueness for granted once we get away from an economy presided over by a Walrasian auctioneer. The work to which I refer here provides a complementary analysis of potentially great importance to short run sticky price macroeconomic models.

instead of prices. That *is* what the work of Fischer (1977), Phelps and Taylor (1977) and Okun (1981), among others is all about.

Moreover, the Micro-economics literature *does* enable us to explain wage and price stickiness in terms of maximizing behaviour. Barro (1972) and Kawasaki *et al.* (1983), among others, invoke costs of changing prices as a reason for the phenomenon. There exists a literature, surveyed by Hall (1980), which explains wage stickiness as the outcome of contracts designed to share the risks inherent in demand fluctuations between firms and their employees. Mancur Olson (1984) has recently argued that the existence of rent-seeking coalitions in the market sector of the economy is likely to be associated with wage and price stickiness, for the simple reason that such coalitions find it easier to monitor the pricing behaviour of their members than to enforce agreements about quantities.

What then is the difficulty about accepting wage and price stickiness? The problem is that, though it is easy enough to explain the existence of sticky wages and prices at the level of the individual experiment, it has not, thus far, proved possible to explain why the stickiness in question should characterise *money* wages and prices as opposed to *relative* wages and prices. Thus Barro (1977b) purported to show that optimal contracts should be concerned with relative prices, and argued that models dealing with them cannot therefore be used to explain money wage and money price stickiness. Since contracts set in money terms do exist in the real world, the correct inference to draw here is that there must be something missing from the particular maximizing models that deny their occurrence. Incredibly, New-Classical economists seem to have concluded that the maximizing models must be correct, that the facts about contracts cannot be what they patently are, and that they therefore must not be used as a basis for an empirical generalization which, when inserted into macro-economic model, helps it to yield useful predictions about the world.<sup>22</sup>

Now, if the claim of New-Classical Economics to be able to deduce everything with which it deals from nothing other than funda-

<sup>22</sup> Recently MONTGOMERY and SHAW (1985) have investigated the role of money wage stickiness in an otherwise New-Classical Framework, have conceded it to be a pervasive phenomenon, but have argued that it has little explanatory power over quantity fluctuations. The basis for this last conclusion appears to be the assumption that, wage contracts notwithstanding, money prices are perfectly flexible, and hence it misses the point of Keynesian analysis which models quantity fluctuations as an alternative equilibrating mechanism to price fluctuations, and not as a response to them.

mental premises about tastes and technology were true, the reluctance of its proponents to use an unexplained empirical generalization about contracts being set in terms of money would be understandable. However, quite apart from its need for "free" parameters already discussed, New-Classical Economics also requires us to accept important unsupported assertions about institutional arrangements. Consider: in every New-Classical model agents trade, but the existence of trade presupposes a system of property rights and legal arrangements permitting their exchange; and New-Classical models are frequently used to analyse policy problems of one sort or another, but the existence of policy presupposes both that a government of some description exists, and that this institution has a capacity for purposeful behaviour.

We might prefer it if we could explain the existence of these social institutions as the outcome of the maximizing behaviour of the individuals who inhabit the economy. However, we do have to start somewhere, and our inability to explain social institutions as the consequences of individual tastes and technology should not prevent us from getting on with our economics.<sup>23</sup> Precisely: but what is monetary exchange, including the practice of contracting in money terms, if not a social institution on the same level as property rights, markets, and government? And why should our inability to explain it prevent us assuming it as a starting point for certain pieces of economic analysis? However, if we do treat monetary exchange as such a starting point, we can of course explain money wage and price stickiness in terms of the analysis invoked above.

To sum up, the assumption of price stickiness used in conventional Keynesian Macroeconomics does permit a degree of freedom in the determination of certain parameter values that is larger than ideal. Moreover, we do not, in the current state of knowledge, have a full understanding of the phenomenon. However, given the institution of monetary exchange, money wage and price stickiness can be explained as the result of maximizing behaviour; they do exist at the micro level, and they do have certain implications for macroeconomic phenomena that appear to conform to the facts. Given the choice, therefore, between a Macroeconomics which recognizes the existence of price stickiness and one which refuses to do so, there does not seem to be very much harm done if we opt for the former, particularly since the

<sup>23</sup> However, see ROWE (1985) for a pioneering attempt to come to grips with problems of this sort.

alternative approach also seems to rely on a good share of free parameters and unexplained institutional assumptions to get results with non-falsified predictive content.

## VI. - Rational Expectations

The notion that the world may, and indeed ought, to be modelled as if the activities of individual agents were co-ordinated in continuously clearing flexible price competitive markets is one foundation of New-Classical Economics. The other is the rational expectations hypothesis. The idea that expectations about the future behaviour of prices must be important determinants of current market behaviour is an old one, as is the closely related proposition that, only if such expectations are fulfilled, can the economy be said to be in full equilibrium.<sup>24</sup> In extending these notions by arguing, first, that we should think of expectations as being the output of an economic model, knowledge of whose structure is attributed to agents, and second, that for full equilibrium to rule, the model in question must be the "true" one of the economy under analysis, New-Classical Economics has made a contribution of immense importance to our understanding of these matters. Economic theory has been permanently changed by these insights, and for the better.<sup>25</sup> That being said, I am not enthusiastic about the way in which New-Classical economists have *applied* these insights. Two issues in particular are worth considering, the first having to do with the choice of the "model" of the economy which one attributes to agents in analyzing their behaviour, and the second having to do with interaction between policy authorities and the private sector, and specifically the way in which the question of "credibility" is handled.

For analytic exercises designed to reveal the long run equilibrium properties of economic models, it is of course quite appropriate to attribute to agents within the model knowledge of that same model. Any

<sup>24</sup> The argument was well developed by HAYEK (1928), and according to HANSSON (1983) a slightly later version of it, developed initially by Gunnar Myrdal, was seminal to much Swedish dynamic economics in the 1930s. See also McCLOUGHRY (1984).

<sup>25</sup> I have developed this argument at greater length in LAIDLER (1984). The interaction of expectations and the structure of the economy is most fully developed by LUCAS (1976) in what I suspect will turn out to be the most durably important paper of the New-Classical Revolution.

other basis for expectations formation would, under some condition or other, lead agents into systematic error, causing them to revise their method of forming expectations. Hence, it could not be a component of a full equilibrium structure. To say this, however, is not to say that this same procedure is appropriate as a foundation for applied work on any particular historical episode.<sup>26</sup> If it is true that expectations should “depend, in a proper way, on the same things that economic theory says actually determine that variable”, then surely, when trying to understand the behaviour of a particular economy at a particular time in its history, we should attribute to agents expectations based, not on what we now believe is the proper model of that economy, but rather on what the economic theory available and believed at that time and place said was a proper model.

We may illustrate this proposition with a concrete example. Among the seminal papers of New-Classical Economics are empirical studies, by Robert J. Barro (1977a) (1978), of the influence of money on unemployment, output and prices in the United States since the Second World War. It is the essential claim of these papers that only “unanticipated” changes in the quantity of money affected employment and output (relative to trend) over this period; agents inhabiting the economy at that time are treated by Barro as believing in the equilibrium competitive model of New-Classical Economics, supplemented by a primitive version of the Quantity Theory of Money, and as using this model for forming their expectations.<sup>27</sup> However, if, in the 1945-76 period agents really had held New-Classical beliefs, there would have been no need for a New-Classical revolution. As it is, we know very well that until the mid-1970s, firm beliefs in a certain kind of Keynesian economics, whose centerpiece was a permanent inflation unemployment trade-off, were the common property of American policy-makers and key private sector agents alike. Indeed the primary claim made by Lucas and Sargent (1978) to support the scientific importance of their work was that it had undermined just this Keynesian consensus. That being the case, logical consistency requires New-Classical Economics to

<sup>26</sup> Thus, I stand by the judgement offered by LAIDLER and PARKIN (1975) p. 771 that “The Rational Expectations hypothesis... is probably better suited to a characterization of expectations formation in the very long run.” I do not wish to imply that my co-author would still subscribe to this view, though.

<sup>27</sup> The argument here abstracts from more down to earth issues such as whether the proposition that only “unanticipated money” affects output is uniquely a prediction of New-Classical Macroeconomics, (it isn’t — see LAIDLER, 1985), or whether the data actually do support Barro’s analysis (they don’t appear to — see MISHKIN, 1982).

model the economic history of the period in question by postulating that agents operating within the U.S. economy used an erroneous Keynesian model to form their expectations. To do otherwise would be to wind up in a hopeless logical tangle.

The point illustrated here is of course quite general. New-Classical Economics argues, with great persuasiveness, that the nature of agents’ information about the structure of the economy is itself an important component of that structure. If that information changes, then so does the economy’s behaviour. If it is right so to argue, then the state of economic knowledge itself becomes a key ingredient of any economic model, and Economic History cannot be studied without recourse to the History of Economic Thought. This latter insight is not new, of course. It is central to the kind of Austrian economics associated particularly with the later work of von Hayek, but he was led to this position from a starting point very similar to the stance of contemporary New-Classical Economists.<sup>28</sup> The fact that the latter insist that agents, living at any time or place, should be thought of as believing that the economy which they inhabit behaves “as if” it was driven by the mechanisms highlighted by a theory first advocated by a particular group of American economists in the 1970’s, certainly sets them apart from the later Austrians. The comparison here is hardly in favour of New-Classical economists, however.

A similar type of unhistoric naïveté is to be found in the way in which New-Classical Economics approaches the problem of “policy credibility”. It is undoubtedly true that, in a New-Classical world, a well publicized change in, say, monetary policy, will only have its effects concentrated on prices if the publicity is believed. Just as the traditional Keynesian — though his ancestors here are Meade and Tinbergen, not Keynes — viewed the policy maker’s task as the maximization of a social utility function subject to a constraint given by the structure of the economy, so a New-Classical economist regards the typical private sector agent as maximizing a private utility function subject to a structure determined both the activities of other private sector agents, and by the activities of policy makers. Suppose that both policy makers and private sector agents are aware of this: how do they interact? The

<sup>28</sup> HAYEK paid increasing attention to problems of knowledge as a determinant of economic behaviour and became less and less inclined to ascribe empirical content to what we would now call a full rational expectations equilibrium, such as he described in (1928), from the mid 1930s onwards. The turning point in his thought is perhaps to be found in HAYEK (1937). On this matter see also MCCLOUGHRY (1984).

answer, we are told, will be found by the application of "differential game theory" in which policy makers and private sector agents communicate and establish credibility with one another solely through observable behaviour.<sup>29</sup> Ultimately in such games an "equilibrium" emerges in which each agent's maximizing behaviour imposes a constraint on the other which leads to that behaviour being sustained. Analysis of this type is intellectually challenging, but a little scepticism about its empirical relevance is surely in order.

"Policy makers" in the real world are not entities who exist outside of their society and economy. They are endogenous self-interested maximizing agents. Moreover, they interact with the private sector in many more ways than by giving and receiving market signals to establish their credibility. In particular, they achieve the positions that they do, and maintain them, as the result of political processes in which private sector agents participate. A whole literature in the area of "public choice" analysis is devoted to all of this, and I am not saying anything novel in drawing attention to these matters.<sup>30</sup> I am however suggesting that to rest one's analysis of macroeconomic policy making on "differential game theory" is simply to ignore this critical dimension of the policy making process. Perhaps political institutions have nothing to do with the way in which policy is made and changed; perhaps ideology has no influence here either; but I doubt it. Rather, I suspect that the New-Classical approach to the analysis of policy making, in ignoring these factors, threatens to lead us down a blind alley.

<sup>29</sup> Both Lucas and Sargent recommend differential game theory in their contributions to KLAMER (1984). (See pp. 55, 73). It is instructive to compare their discussion of this issue with Karl Brunner's contribution to the same volume, pp. 185-186. Brunner has, of course, long been acutely aware of the role of political processes in forming policies and conditioning the private sector's responses to them.

<sup>30</sup> The contribution of MANCUR OLSEN (1982, 1984), and of JAMES BUCHANAN and his associates (see, e.g., BUCHANAN, TOLLISON and TULLOCH, 1980) to this literature are well known. It is surely no accident that two prominent Monetarists who have refused to join the "New-Classical Revolution", Karl Brunner and Allan Meltzer, have also worked in the public choice area. It should also be noted that HARRY JOHNSON drew similar conclusions to those developed here about the interaction of expectations, policy, and political processes as long ago as (1972).

## VII. - Concluding Comments

The bulk of this paper has been critical of New-Classical Macroeconomics. This does not mean that such analysis has nothing of importance to say to us; quite the contrary. Though the "New-Classical Revolution" has had exaggerated claims made on its behalf, and it is these exaggerated claims which I have been concerned to criticise in this paper, it is also the case, as noted at the very outset of this paper, that "revolutions" in macroeconomics usually leave behind them contributions of lasting importance to be absorbed into the mainstream of the discipline. The New-Classical revolution has certainly done this, as I shall now argue.

Consider first the New-Classical insistence on equilibrium modelling. If it is desired to construct an economics with predictive content, then the postulate that agents formulate purposeful and consistent plans and that they are able to execute those plans is surely a useful starting point; but at the level of the individual, the execution of such plans is precisely what we mean when we speak of equilibrium behaviour. If assumptions about the nature of plans do not permit us to say anything about actions, as they can not if we entertain the possibility of "disequilibrium" at the level of the individual agent, then an economics based on the analysis of the individual can have no predictive content. This idea is an old one, to be sure, having been a constant theme in Austrian economics from Menger (1871) onwards, but a glance at the Macroeconomics literature of the 1960s will soon confirm that we had lost sight of it, and needed to be reminded of its importance. New-Classical Economics did just that.

The difficulty with New-Classical Economics lies not in the equilibrium postulate *per se*, but in its insistence that we model the economy as a whole as if the equilibrium strategies of individuals were formulated and executed in an institutional framework characterized by continuously clearing competitive markets. The fact that such a framework is the only one which, in the current state of analytic techniques, permits a seamless connection between the analysis of the microeconomic equilibrium of the individual and macro behaviour, is no reason for insisting that macro-predictions obtained by other less pristine methods are unworthy of consideration. That, though, is what New-Classical macroeconomists have, quite unjustifiably, been doing. However, we ought not to let dissatisfaction with a particular applica-

tion of a methodological precept lead us to underestimate its general importance.

Equilibrium modelling of individuals surely ought to be the basis of macroeconomic reasoning, and the fewer empirical generalizations about behaviour we need to make to get from such a basis to empirically robust predictions about the economy as a whole, the better.

Exactly parallel arguments to these may be advanced about the rational expectations idea. This is hardly surprising, since there is a real sense in which this hypothesis is simply a particular consequence of the purposeful maximizing postulate. The idea that the state of agents' knowledge, and the nature of their expectations about future events, form a key part of the economy's current structure, and help to determine the outcome of current maximizing behaviour, is hardly new. It was, as has been pointed out, a prominent ingredient of Austrian economics, but once more, a glance at the macroeconomic literature of the 1960s, (replete as it is with exercises in which the consequences of alternative policy measures are derived from the same, allegedly structural, representation of the private sector of the economy) will show how badly we needed to be reminded of this insight.

As with the equilibrium idea, criticisms of the rational expectations notion advanced above have been of the particular and very special ways in which it has been applied, rather than of the basic idea itself. It is at best logically dubious to analyse historical episodes "as if" agents involved in them possessed a vision of the economy which has only been created in the last decade. When the very purpose of the analysis in question is to expose flaws in the economics which was commonly believed during the episode under analysis, perhaps stronger epithets are called for. Nevertheless it is important to formulate hypotheses about the way in which the state of knowledge influences the structure of the economy at particular times and places, and to investigate the way in which that structure changes in the light of actual experience and of changes in economic doctrines. That is the key implication of the rational expectations idea for empirical work.

Problems posed by the credibility of policy for the predictive content of Macroeconomics are also real. To argue, as I have, that New-Classical economists do not seem to be following the most fruitful path in investigating such matters (which probably lie in an analysis of the way in which private and public sector agents interact through political processes) does not alter the fact that it has been the New-Classicals' initial insights which have compelled macroeconomists

in general to recognize the importance of these questions. They have stressed that a positive theory of government behaviour must be an important factor conditioning private sector behaviour, and I have criticised them, not for advancing this view, but for failing even to attempt to incorporate currently available positive theories of government into their work.

It is worth pointing out explicitly that the problems with New-Classical Economics discussed in this paper are, in a fundamental sense, different aspects of a single issue. At least since the first publication of Smith's *Wealth of Nations* (1776) economists have been arguing about the extent to which a society that organises its economic activity on the basis of voluntary exchange of private property rights can be expected to achieve a coherent solution to problems of resource utilization and allocation (not to mention distribution). From their arguments has emerged an increasingly clear understanding that analysis of the institutional framework within which, and the processes whereby, the decisions of agents are co-ordinated, and the information upon which those decisions are based is disseminated, must lie at the heart of any attempt to come to grips with these issues.

New-Classical economists insist that we assume agents to possess, as common knowledge, almost all systematic information about the structure of the economy relevant to their welfare before we model their decision making. They also insist that, in analysing the interaction of agents, we must assume that their behaviour is co-ordinated by a price mechanism that never permits their plans to be incompatible for long enough to have observable consequences. In short, New-Classical Economics requires that we treat certain (and extreme) proposition about a market economy's capacity for solving problems of disseminating information and co-ordinating decisions, not as hypotheses to be questioned and investigated, but as axiomatic assumptions. To adhere to the "first principles" of analysis upon which New-Classical Economics is based requires that we give up questioning the coherence of economic activity co-ordinated by markets and confine our activities to describing the nature of a coherence that is presumed to exist. If the popularity of Keynesian economics in the years following the Depression was, as Lucas is said to have told *Newsweek* (Feb. 4th 1985, p. 60), "based on political needs, not economic truth" then, so, surely, as Howitt (1986) has remarked, does the current popularity of New-Classical Economics reflect its compatibility with the ideology of the New Right.

And yet the pioneers of New-Classical Economics are no more ideologues than was Keynes. Disinterested seriousness about following the logic of an argument wherever it might lead is surely the hallmark of the writings of Lucas and Sargent, and let it be said explicitly, that, in this paper I intend to accuse them of no worse an offence than permitting this very seriousness of purpose to lead them into carrying good ideas too far and sometimes in the wrong direction. If this characterisation of the "New-Classical Revolution" is accepted, it has not, of course, in this respect been different from other periods of advance in economic knowledge. The Keynesian Revolution and the Monetarist Revolution were both in their own ways equally open to criticism on such grounds in their respective days. More to the point, in rejecting the extremes to which New-Classical Economics has taken them, we should not lose sight of the fact that the ideas in question are, after all, good ones. When, as I hope it will, the main thrust of macroeconomics research returns to addressing problems of *Information and Co-ordination*, to borrow yet another phrase from Leijonhufvud (1982) it surely will do so with a much clearer understanding of the role of purposeful maximizing individual behaviour in the solution of these problems than could have been possible had the New-Classical Revolution never occurred.

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