# Equilibrium and Macroeconomics

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# Introduction

This, with one or two exceptions, is a selection from my less technical papers.[1] A number of them were written for delivery as public lectures and a number of them are polemical. Nevertheless, all the papers are concerned with economic theory. This has sometimes required me to translate mathematics into English. Marshall advised economists to follow this course on all occasions. I am not convinced that this is sound advice. But sound or not as it may be, I find on rereading that I am not outstandingly good at following it. I could have attempted to make changes for this occasion but soon convinced myself that I was at least as likely to make matters worse as I was to improve them. Accordingly some difficulties of certain passages remain as they were but they should all yield up whatever mystery there may be on second reading.

I shall use this introduction to comment on some of the papers with hindsight and I shall allow myself the indulgence of some remarks on my theorising in economics.

I have frequently, and especially in my university, been classified as a neo-classical economist. Since I myself label others (e.g. as 'monetarists') I must not complain, but it is perhaps useful to say in which sense I accept the label. There are three elements in my thinking which may justify it:

(1) I am a reductionist in that I attempt to locate explanations in the actions of individual agents.
(2) In theorising about the agent I look for some axioms of rationality.

[1] In writing this introduction I have had valuable comments from R. Solow, M. Hollis, T. Lawson and J. Thomas. I am also much indebted to T. O'Shaughnessy for comments on the papers, proofreading and preparing the index.

(3) I hold that some notion of equilibrium is required and that the study of equilibrium states is useful.

If a historian of thought considers these to be sufficient elements in the making of a neo-classical economist then that is what I am. But I am not sure that this qualifies me on Lord Kaldor's characterisation or that of Marxists and neo-Ricardians.

I am not equally comfortable in my commitment to these three elements. My conviction that (1) is the right approach is pretty strong. For instance, although I have no difficulty with the idea of class I have not been able to give meaning to 'class interest' or the actions of a class until these interests and actions have been located in the individual member. Again I am quite prepared to accept that 'the whole may differ from the sum' but it seems only comprehensible when one starts at the level of the individual. Then, for instance, the theory of externalities can make for comprehension. I know too little of the philosophical literature on 'holistic' explanation to discuss them conclusively. But what I have read and what I have heard argued leaves me faithful to (1).

Element (2) is rather harder. A part of my acceptance of it is its theoretical fruitfulness. Another is the ease in which regular behaviour can be viewed in its light; for instance, by considering the time costs of computation and information gathering, Simon's satisficer does not contradict rationality. By far the largest part is that I know of no satisfactory alternative.

But I am aware of its weakness and of its dangers. In practice an axiom of rationality postulates a complete preordering of alternatives and a choice which is not dominated (in preference) by another available one. but the space of alternatives could be very general indeed and the perceived set of alternatives may not coincide with the actual one. The result is that the theoretical fruitfulness I spoke of requires considerable narrowing of the meaning. For instance, in a great deal of the literature, preferences are only between bundles of goods consumed by the agent and the set of available choices between them is also the perceived one. The danger of the latter is easily seen when one distinguishes goods by date as well as other characteristics and there are not enough futures markets. The danger of the former is that it may leave many actions unexplained; for instance, benevolent actions or envious ones. It also seems as well established as any empirical proposition in economics that the valuation a person places on some goods depends on the actions (and valuations) of others. But the real danger is this: one is tempted to confuse the narrowed formulation with the axiom of rationality itself. For instance, Keynes argued that workers care about relative wages. In the present context this amounts to the claim that the wages of others are arguments of one particular worker's utility function. This however has been condemned as an *ad hoc* procedure. Certainly it is not often considered in the current literature. But it is no more (or less) *ad hoc* than any other postulate one employs to make (2) usable, e.g. that the workers are only interested in their own wage.

So when I accept (2) I mean this: I want the superior 'advantage' of an action to serve as its explanation. As I have argued this cannot be accomplished without an empirically motivated specification of the domain of preferences and of the agent's perception of possibilities. This, of course, is hard because firm empirical knowledge on these matters is lacking. But in many applications it certainly seems straightforward. For instance, whatever the domain it seems safe to say that people will want to buy the cheaper of two identical goods which they regard as identical and that then leads to a theory of the equalisation of their prices. But even here information has to be specified from 'outside'. It is the concrete specification we give to preference theory which may also make it empirically interesting. To revert to the example: if two identical goods continue to sell at a different price then I for one consider that there is something for the theorist to explain. That is because I consider it plausible that people prefer more goods to fewer. In this I can be wrong. I can also be wrong in the acceptance of (2) and as I have already said I hold to it because I can see no other alternative of comparable power and appeal.

On element (3) I shall be brief because two of the papers which follow are concerned with equilibrium. I once again, as in the case of (1), feel rather secure with (3).

The notion of equilibrium is often misused and misunderstood. For instance, in parts of America it is restricted to denote market clearing everywhere under competitive conditions. Thus narrowly used it loses much of its usefulness. The latter, as I say in paper 2, seems to me to be its character as a critical point of an implicit or explicit dynamics. For instance, the competitive equilibrium gains its interest from the postulate that prices must change in all other states of the economy. But there are many other plausible dynamics and the competitive one suffers from having no theory of agents who change prices. But in any event if economic theory has anything to offer on the interaction of market signals and agents' actions then it will need to formulate an equilibrium concept.

But there are also dangers. One of these is that one considers *nothing* but equilibrium. Professor Lucas (in conversation with Oliver Hart) has in fact argued in favour of proceeding in just this manner but I have not been able to make any kind of sense of his argument. What is plain is that by narrowing our viewpoint in this manner we shall remove a great deal of interest and importance from scrutiny. For instance, imposing the axiom that the economy is at every instant in competitive equilibrium simply removes the actual operation of the invisible hand from the analysis. By postulating that all perceived Pareto-improving moves are instantly carried out all problems of co-ordination between agents are ruled out. Economic theory thus narrowly constructed makes many important discussions impossible.

However, there are also purely theoretical objections. It is only very rarely the case that one has any reason to claim that equilibrium is unique. This robs the axiom of instantaneous market clearing of its power either in comparisons or in the tracing of the evolution of an economy. The multiplicity of equilibria also means that determinateness requires a theory of the economy out of equilibrium. Many of these matters are more fully discussed in some of the papers which follow and in particular in paper 2. In any case my acceptance of (3) does not entail anything as foolish as the claim that all theory should be equilibrium theory.

Besides sketching the three principles that underlie all of my work, I also ought to say briefly how I view theorising in economics.

The short answer is that I view it as an ongoing attempt to bring some order into our thinking about economic phenomena and as the creation of a language in which these attempts can be discussed. I do not expect this activity to reach very many definitive conclusions. I shall call the attempt at orderly thinking the attempt to understand.

It is plain that we can claim understanding of an event without claiming that we can predict it. Geophysicists, for instance, believe that they understand earthquakes but cannot predict them; biologists claim to understand the process of speciation but in general cannot predict the next occurrence. Economists probably agree in their explanation of the recent rise of the dollar but it is doubtful that it could have been predicted with confidence. In all these cases there are very many elements which enter into the explanation of an event. This in turn hinders prediction and so also falsification. In economics it is certainly hard to think of any theory which has been conclusively falsified.

It would, of course, be nice if matters were as Professor Friedman (1953) once thought; I am referring to his 'as if' positivist methodology. But it does not correspond to what economists do or could do. For instance, econometric investigations have been much more useful in providing descriptions of the world which we seek to understand than they have been in confirming or falsifying theories. A striking example of the difficulty of refutation is this. It is now known (Debreu 1974) that any set of continuous homogeneous aggregate demand functions which satisfies Walras' Law can be generated by the behaviour of some rational consumers. Hence consumer theory cannot be falsified by studying such functions. One would have to study the individual consumer (to whom the theory is in any case not meant to apply in practice) or to find evidence of the characteristics of consumers which can then be shown to be not capable of rationalising the excess demand functions. Or one could resort to experiment. It will be agreed that these are tall orders and it is neither surprising nor scandalous that a very old theory has no very firm empirical basis. Yet it would be odd to claim that it does not aid understanding.

This, or course, is at a high level of generality and there are indeed various levels of theorising. Thus, there are theories in which important elements are given quite particular forms. Their justification rests on much more specific empirical hypotheses than that of rationality and it is thus easier to make empirical tests. These special theories are of obvious importance to practical economics and they also aid understanding. But I do not accept that all theory should be special nor do I accept that more general theories are bound to be vacuous tautologies.

If we have only special theories then we do not know where to look next if they are not confirmed. For instance, there is nothing in our understanding of the behaviour of agents which leads us to expect log-linearity in the equations which describe it. Or take a more purely theoretical specialisation. It has been suggested (Kaldor and Mirrlees 1962) that firms are guided in their investment by specifying a 'pay-back period' which any project must meet. This can be consistent with a quite general theory of maximising behaviour. But where do you go next when this special hypothesis is not confirmed, or when it contradicts some other specialisation? Moreover, the more general theory helps in establishing what else would have to be true if the specialisation were true.

One cannot object to bold hypotheses or to empirical hypotheses in economics; indeed, one welcomes them. But the regularities in human behaviour, if such there are, will almost certainly be found at a deeper level than, say, that of the pay-back theory of investment. Such behaviour itself needs to be further understood.

Now the objection to more general theorising is that 'anything can happen' and so the 'axiomatic deductive method' cannot yield empirical insights. This objection is false on two counts. It is true that often many things can be the case in a general theory but not that everything can be. Everyone who knows the textbooks can confirm that; for instance, you cannot get a Pareto improvement in an Arrow–Debreu equilbrium, nor can you observe firms producing under increasing returns. The point is, of course, that although theories like those of Arrow–Debreu are far more general in application than, say, recent three equation models of rational expectations equilibrium, they too are a long way from vacuous generality. For instance, there is perfect competition, a law of property and of contract etc. etc.

The second reason why the objection is false is that it does not understand either axioms or the axiomatic method. Axioms, like special hypotheses, are there to specialise. It is not that they are divorced from experience or observation but rather that they mark the stage beyond which one does not seek to explain. The axiom that firms maximise some function of profits is stated as such because the theorist is not proposing to answer the question why firms should so so. But it is not plucked out of the air or from dreams. It encapsulates an empirical phenomenon which many practical people and economists believe to be the nature of the capitalist. It does so at a more general level than, say, the pay-back theory but it is every bit as empirically motivated.

One of the more astonishing objections sometimes heard against the axiomatic method is that since it proceeds by logical steps from axioms to outcomes it cannot reach empirically relevant conclusions. This is like arguing that the manner in which calculus is derived (from axioms or number systems etc.) makes it impossible to apply to real problems. It seems of things which are logically true that they are also true. Of course, in economics there are contingent truths – contingent on the truth of axioms. But that is precisely why good theorists devote much care and attention to the formulation of these axioms.

Lastly, I want to argue that it is one of the virtues of theories derived from axioms more 'fundamental' than those used in special theory, that they usually do not yield single valued restrictions on the world. Although I have already maintained that it is false that 'anything can be true' it is the case that a number of different things could usually be true. This is a virtue because the economist is thereby restricted from claiming more than he has reasons for claiming. The axioms have summed up what one regards as pretty secure empirical knowledge. The set of outcomes which are possible is simply the reflection of our lack of knowledge. A special theory can usefully narrow them down. But our confidence in the special hypothesis is smaller than in the axioms. A claim of only one outcome should always include the proviso that given our state of knowledge there are also other possibilities.

The most strongly held of my views I have left to the last of these general reflections. It is that neither is there a single best way for understanding in economics nor is it possible to hold any conclusions, other than purely logical deductions, with certainty. I have since my earliest days in the subject been astonished that this view is not widely shared. Indeed, we are encompassed by passionately held beliefs. There are those with burning convictions in the virtues of 'small' models and in the absolute need for 'full' models; in the uselessness of mathematics in economics and in its absolute necessity; in the need to postulate 'market clearing' and in the meaninglessness of this postulate; in rational expectations models and in the madness of such models; in the absolute need for historical and institutional elements and in a purely analytical approach; in short run analysis and in long run analysis; in the uselessness of all theorising and in the uselessness of econometrics and fact collection; in short, in almost anything that has ever been tried. In fact all these 'certainties' and all the 'schools' which they spawn are a sure sign of our ignorance. Perhaps something like this is needed to spur us on but I regard it simply as *trahison des clercs*. For it is obvious to me that we do not possess much certain knowledge about the economic world and that our best chance of gaining more is to try in all sorts of directions and by all sorts of means. This will not be furthered by strident commitments of faith.

Of course, it is not difficult to propose a theory for this state of affairs. But I shall not do so except to note one of its possible elements. Economics like dentistry is expected to be 'useful' (although I have never seen why understanding is not its own reward). In particular it should be a source of advice to those with power to act. Such advice, it is held, must be given with conviction if it is to be sought, leave alone taken. Economists who seek to influence people in power soon come to resemble their patrons. Moreover, they come to feel an urgent need to defend what they proposed through thick and thin. Add this to political beliefs and one is well on the way to explaining some of the zealotry. My own position is that economists are at their most useful when they give an account of the alternative scenarios which the present state of our knowledge allows. (More on all of this will be found in paper 16).

I now turn more directly to the papers in this volume. It can be said of all of them that they exemplify a general equilibrium approach. By this I mean that they do not much utilise Marshallian partial equilibrium theory, and not that I am only concerned with equilibrium leave alone that I always postulate an economy in perfectly competitive equilibrium. No doubt partial analysis can also be very fruitful; I just do not happen to have employed it much.

The first two papers are concerned with a usable and interesting notion of equilibrium. One of these was written long ago (paper 1) whereas the other is more recent. When I was writing the latter I had not reread the former and I am now somewhat gratified to find that they do not contradict each other; indeed, the reader will I hope excuse a certain amount of repetition. I am also more convinced now than I have ever been that it is of high importance that economists should get this matter straight. For not only is there increasing evidence of sloppy thinking brought about by a sloppy equilibrium concept but this failing seems also to have become of some practical importance.

There are, of course, those who believe that definitions and language do not much matter as long as they are consistently employed. This seems to me quite false. Definitions used have an immediate and potent influence on the analysis which follows, and language has enormous potential for good or ill. One need only think of the use of and definition of 'exploitation' to see this. More pertinently the recent meaning given to equilibrium (and disequilibrium) has had quite disastrous effects. Equilibrium is defined as Walrasian competitive equilibrium or a rational expectation Walrasian competitive equilibrium. All other states are said to be in disequilibrium. But, as I have already noted (and argue at length in paper 2), the motivation of the definition is largely that disequilibrium states cannot last (the implicit dynamics of the definition). Hence it is concluded that only Walrasian rational expectations equilibria can have any permanence which I hope will be recognised as a substantial claim coming purely from the definition. The further step that is then taken is to claim that the equilibria are stable. But that in the literature is pure assertion and I am at a loss to understand why it should have been so widely adopted as an axiom.

A consequence of all of this has been, for instance, to designate all economic states with Keynesian features (e.g. involuntary unemployment) as disequilibria with the further implication that they will, if they can exist at all, also soon disappear. Those who have been somewhat more sympathetic to Keynes and who have been attempting to give his theory more modern expression have none the less quite supinely agreed to having their endeavours called 'disequilibrium economics'. They have also much to their cost gone along with the vacuous proposition that there could be no Keynesian problems if prices and wages were 'flexible' when this in turn is translated to mean 'if prices and wages at all times cleared all Walrasian markets'.Tautologies are here given instrumental interpretations. These are all examples which show that definitions and language matter profoundly. They are rather fully discussed in papers 14 and 15.

My own approach discussed in the two equilibrium papers is to take the use which we shall want to make of the equilibrium concept into account when formulating it. That use is to make a distinction between economic states which cannot last and those for which there is no theoretical reason to expect a change. I therefore think of equilibrium states as those in which agents learn nothing new. They therefore have a lasting policy which gives their actions as a map from relevant variables. Sometimes I refer to this policy as routine behaviour, an idea which I must have got from Schumpeter. The claim I make for this conceptualisation is based not on its generality but rather on the fact that it accurately captures the use we all want to make of equilibrium and so avoids some of the nonsense I have already described. For instance, there could be an equilibrium with rigid money wages and unemployment if none of the designated agents find it, in their perceived circumstances, advantageous to make any change. To clinch this should have nothing to do with any Walrasian axioms and everything to do with a theory of rational agents, their information and their learning.

There is one matter which the two equilibrium papers do not treat in any detail. That is the axiom of continuous 'market clearing'. No one had seriously proposed it at the time. I am not sure that it is worth much discussion even now especially in its perfectly competitive form. It has been much confused with another axiom to the effect that at any moment agents do what they prefer to do. To use that axiom for present purposes one needs to specify also what agents can do. For instance, if an unemployed worker cannot accept a lower wage without union agreement or without social action, and if an employer cannot lower the offered wage without courting a costly strike then everybody may be doing what they prefer and yet the offers to work at the current wage can exceed the demand for such work. There are many far less drastic examples of the same phenomenon. The most superficial acquaintance with game theory is enough to convince one that competitive instantaneous market clearing is not an axiom one wants to adopt. That, of course, does *not* mean that it may not be interesting to study the consequences of imposing such clearing as an assumption. What one must, however, not do is to claim that it comes from a deep 'universals' of economics or that there are profound philosophical reasons for its employment.

Indeed, the next two papers (3 and 4) are concerned with a discussion of theory when that assumption is made. Of course, I consider its realism and relevance but on the whole I am largely concerned with difficulties internal to the theory. Some of these are technical: for instance, those arising from multiple equilibria, or from the idea of a firm when assumptions exclude set-up costs. Others are concerned with the fact that certain important phenomena seem to escape the theory. On the whole my attitude is this: if we did not have the Arrow–Debreu machinery there would be an urgent need to invent it because it gives us the best base camp for sallies into new territory. On the other hand it *is* only a base camp. The rational expectations perfectly competitive economy is indeed a camp higher up but not much. It is, I argue, difficult to take it seriously, for instance as a basis for policy conclusions. (I return to all of this in paper 5.)

In paper 4 I consider some of the difficulties with the implicit dynamics which underlies the Walrasian equilibrium notion. It is somewhat technical in parts. The main conclusion is rather pessimistic: we have no good reason to suppose that there are forces which lead the economy to equilibrium. By that I mean that we have no good theory. I examine two examples in some detail. In retrospect I think I should have paid more attention to the 'practical' argument that we seem to live in an economy which on the whole is orderly. It will be recalled that Keynes argued that violently unstable models are for that reason bad models. No doubt there is some force in this view although it is so imprecise as to make it unclear what exactly is supposed to be the case. I should now want to say that I am agnostic on the general tenor of the practical claim although I lean in the direction of accepting it in some formulation. However, this does not at all affect the position I took; if indeed there is order we do not now understand how it is brought about. What we know is that in some circumstances orderly states (equilibria) are possible. But is it a mistake to believe that this provides an answer to the question of how order is imposed.

The last paper on this general topic (paper 6) is a review of Kornai's *Anti-Equilibrium*. I am not sure that I adequately conveyed my view that this is an interesting and stimulating book and I may well have been too eager to defend what Kornai attacked. I am now also somewhat more ready to grant that equilibrium may be the wrong, or at least a dangerous, benchmark. Certainly Kornai's 'systems' approach cannot be dismissed. But I confess that it still strikes me as both too difficult and too ineffable. Of course, it is descriptively superior to equilibrium but that I still hold need not be a decisive consideration when building theory. Moreover, I also continue to believe that Kornai greatly underestimated the theoretical richness of the orthodox approach and that he was wrong in his strictures on the axiomatic method. But there is something here to think and argue about and I hope that the paper will not give the impression that I regard matters as settled.

The next four papers are concerned with monetary theory and they are somewhat more academic than the preceding ones. A great deal of work continues in this field (for instance, recently there were published three important books by Grandmont 1983 and Gale 1982, 1983), and some of my puzzles have been resolved and others have arisen. My own starting point was deeply