On the Notion of Equilibrium in Economics*

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(I)

Wherever economics is used or thought about, equilibrium is a central organising idea. Chancellors devise budgets to establish some desirable equilibrium and later exchange rates to correct 'fundamental disequilibria'. Sometimes they allow rates to 'find their equilibrium level'. For theorists the pervasiveness of the equilibrium notion hardly needs documenting. Here in Cambridge the predominant recent preoccupation and controversy concerned the question of which techniques would be observed in different economies in the long-run equilibrium at different profit rates. The Marxian analysis of value and prices insofar as it is comprehensible to me, seems to be describing an economy in equilibrium. The 'crises' which Marx predicts and studies gain their precise significance in comparison wit the equilibrium which they disrupt In what is, alas, called 'Neo-Classical' economics the last twenty years have seen the definitive investigation of the logical coherence of an equilibrium by Arrow and Debreu, the beautiful bringing together of the Core - an equilibrium concept of game theory - with the traditional competitive equilibrium, and also numerous studies of mechanisms which might be the causal chain by which equilibrium is attained. The 'Golden', 'Silver' and 'Leaden' ages

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of growth are too familiar to require comment. In Keynesian economics recent discussions, have centred on the question whether Keynes' main insights are misunderstood when they are translated into the equilibrium framework of Hicks and of what Professor Joan Robinson calls the 'bastard Keynesians'. All this is familiar and some of it I shall return to. What is abundantly clear is that the claim which I started with, namely that an equilibrium notion is an all-pervasive one in economic, is easily substantiated.

It is of course not the case that there is a unique specification of the states of an economy which we want to describe as an equilibrium. The distinction between 'short run' and 'long run', for instance, is an ancient one and there are others less well discussed to which I shall return. But a central theme runs through many of these usages, namely the singling out of those states in which the intended actions of rational economic agents are mutually consistent and can therefore be implemented. This is as true of, for instance, neo-Ricardian economics as it is of Walrasian. In the former we search simultaneously for a set of relative prices and a rate of profit which if they ruled would cause rational producers to choose just those techniques which would allow them to earn that rate of profit at which the intended savings also equal intended investment. The Walrasian story, although of course much more general, is similar.

It is precisely this exacting correspondence of rational plans and feasible actions which has been causing concern to practical men and some unease to theorists. To the first of these, the world which he sees does not seem to be the world described by an equilibrium and so he is inclined to think that the notion is not of much use to him although typically he continues to use it in a loose way. For the theorist the difficulty is that for important cases the notion is ill-defined or not defined at all. This point will presently become clear.

For it is my intention in what follows to examine the theoretical and conceptual difficulties which arise with the Arrow-Debreu paradigm when it is modified to serve descriptive purposes. I shall also sketch a tentative proposal for dealing with these difficulties.

I have chosen to start with Arrow-Debreu for the following reasons:

(1) It is precise, complete and unambiguous.

(2) It has been much maltreated by both friend and foe who know it only from hearsay.

(3) In the paradigm it is possible to pinpoint with great accuracy where a change is required if a change is made in the economic circumstances it is asked to illuminate.

(4) Because it so happens that all serious work which is now proceeding to recast the equilibrium notion is being undertaken by those who have been most active in building the paradigm in the first place and who consequently understand it.

I fear that some of what I have to say may turn out to be a little hard to understand at first hearing. I apologise for this but the difficulty seems to be inherent in the topic. I do not however apologise for the fact that an abstract line of thought is being pursued, although I understand the risks. Here is what Russell (1931) has to say:

> Many people have a passionate hatred of abstraction, chiefly, I think, because of its intellectual difficulty; but as they do not wish to give this reason they invent all sorts of others that sound grand. They say that all reality is concrete, and that in making abstractions we are leaving out the essential. They say that all abstraction is falsification, and that as soon as you have left out any aspect of something actual you have exposed yourself to the risk of fallacy in arguing from its remaining aspects alone. Those who argue in this way are in fact concerned with matters quite other than those that concern science.

And he maintains that 'it is the characteristic of the advance of science that less and less is found to be datum and more and more is found to be inference'. I quote Russell here not because I want to maintain that economics is a science - whatever that claim would mean - but because I happen to believe that what he is here saying applies to our subject.

Before I start there is a tiresome matter to get out of the way. It is well known that on certain assumptions an Arrow-Debreu equilibrium of an economy can be shown to be Pareto-efficient. Everyone who has understood this latter concept and the assumptions required to prove the result also understands that to claim this efficiency for any actual economy would be a singularly weak claim in an argument designed to persuade us that the economy is also in some sense morally to be approved.

The evidence that the theory has not been understood is readily available. Only the other day Mr Dobb[1] claimed that Samuelson, who has done more than most to combat this mistake, had maintained that a competitive equilibrium 'gives the unique social optimum'. Again, the great fervour that is put into special models of the economy where equilibrium prices are independent of demand has its origin in the quite absurdly mistaken belief that in the more general case one would be led to conclude that the equilibrium was good, or just or even optimal. Only rudimentary scholarly case is required to show what nonsense all this is. I do no see how a similar misreading, by, say, Chicago excuses such lapses. Nor do I find the cause of reason and honesty served when Ellman (1972) declares that the theory is so vicious that it should be banned from University syllabuses.

But this unattractively illiberal view and others like it are connected with a rather more serious and all-embracing claim, viz. that social science in general and economics in particular must be 'political'. By this it is claimed that the practitioners in these fields are bound consciously or otherwise to seek propositions which support or damage some section of society or support or damage existing social arrangements. One can, of course, ask straight away whether this proposition is not itself politically motivated. But in any case, at this level, the whole thing amounts to no more than the observation that a person's actions, intellectual or otherwise, will hardly be independent of his biography or of the society he lives in and that we are often engaged in the activity of persuading and of influencing others. Certainly also it is of interest to the historian of ideas to locate theories in these contexts in a precise way. I am also willing to concede that in certain instances the meaning of even non-normative sentences may not be understood

[1] In his second Marshall lecture for 1973.

without knowing the intention of the speaker. Yet when all this has been agreed to it is surely can't to maintain that we have no criteria of true and false in these fields. For instance there may be an adequate social and biographical account of why Marx wrote what he did write. But this does not help me to decide whether or not to accept any or all of his propositions. In particular this account is irrelevant to my observation that his version of the 'transformation problem' is logically at fault or that his prediction of falling real wages has so far been falsified.

'The faintest of human passions', A. E. Housman remarks somewhere, 'is the love of truth'. The observation seems a just one. But it seems to me sad that to this general handicap under which economists, like others, labour, some should wish to add the further one of a belief that nothing in economics is either true or false, or if you like, empirically or logically falsifiable. I do not know what activity those who hold this view believe themselves engaged in when they earn their living as economists. Nor do I want to enter into further epistemological speculation, at which economists have shown themselves to be conspicuously bad. I happen to believe that the view lacks all merit and accordingly in what follows will not be inquiring into the motives or biographies of those I agree or disagree with.

One final preliminary remark remains to be made. Professor Kaldor on hearing what I proposed to discuss on this occasion urged me to take notice of his latest paper in the *Economic Journal* (Kaldor 1972). This I have done and it accounts for the fact that in certain sections, references to this paper are so numerous.

(II)

I begin with reminding you of the main features of the Arrow-Debreu equilibrium. Goods are distinguished one from the other by their physical property, by their location in space and in time by the state of the world. A price is defined for each good. There are two kinds of agents - households and firms. Given any non-negative price vector each household chooses an action which defines a point in the space of all goods. It has the property that there is no other action available to the household under its budget constraint which it prefers. Again, given any non-negative price vector, firms choose an action represented by a point in the space of all goods such that there is no other action which is both technologically feasible and more profitable. An equilibrium is then a triple; a non-negative price vector, a vector of demand and a vector of supply, such that (a) the demand vector is the vector sum of household action at these prices, (b) the supply vector is the vector sum of firms' actions at these prices and (c) for no good does demand exceed supply.

The first important point to understand about this construction is that it makes no formal or explicit causal claims at all. For instance it contains no presumption that a sequence of actual economic states will terminate in an equilibrium states. However, it is motivated by a very weak causal proposition. This is that no plausible sequence of economic states will terminate, if does so at all, in a state which is not an equilibrium. The argument is straightforward; agents will not continue in actions in states in which preferred or more profitable ones are available to them nor will mutually inconsistent actions allow given prices to persist. It will be seen that this is not a strong proposition in that no description of any particular process is involved. It is also clear that weak as this claim is, it may be false.

Professor Kaldor's theory of what it is that Debrue's (1959) book might be about is thus incorrect, as a perusal of its ninety-odd pages will quickly show. I do not here refer to his remarkable belief (Kaldor 1972) that Debreu or for that matter any of the general equilibrium theorists postulate 'linear-homogenous and continuously differentiable production functions', nor to the even more surprising claim that the inventors of the beautiful theory of contingent markets postulate 'perfect foresight'. Nor again do I want to blame him for not reading the large literature on the 'removal of scaffolding', not even for not knowing that Arrow (Arrow and Hahn 1972) has provided a rigorous general equilibrium model with increasing returns and imperfect competition. What I want to note here is the incorrectness of the claim that Debreu was looking for the 'minimum basic assumptions for establishing the existence of an equilibrium set of prices which is (a) unique (b) stable'. Here is one of those perennial misunderstandings which I have mentioned and I believe that, odd though it is that so clear a writer as Debreu should be misread, it can be explained by a genuine problem. For Professor Kaldor and others find it so natural to regard an equilibrium outcome of some particular process that they find it difficult to believe that any one should wish to use an equilibrium notion in a different way. And indeed it is a fair question whether it can ever be useful to have an equilibrium notion which does not describe the termination of actual processes.

For the purposes of this question uniqueness of an equilibrium is not an issue, for plainly what is to be discussed is the view that an equilibrium notion is only useful to economists insofar as it involves the falsifiable claim that all actual economic processes converge to *an* equilibrium state. Certainly this is the way, for instance, Marshall justified his interest in equilibrium. I want however to maintain that this view is not correct. I do so on two grounds: first I shall much later argue that our need for equilibrium concepts is largely connected with ignorance of precisely those features of an actual economy which the view under discussion wishes us to be precise about. Secondly I want to maintain now the related but weaker claim that even when equilibrium states cannot be shown to be asymptotic outcomes of processes it is useful to have a concept of equilibrium states. As will however presently become clear, I do not believe that Arrow-Debreu notion to be the appropriate one.

It is the weak causal claim which I have already noticed that gives the clue. For, it involves a perfectly good empirical statement which can be made of any given state, viz. that it will not persist. Indeed with the aid of only the most general features of actual processes it is often possible to say something about the direction in which some variables will move next, without however being able to say what their final resting place, if they have one, will be. In an economy with unemployed resources an excess of intended investment over intended savings is used to predict that incomes will not persist at their present level and indeed they are very likely to rise. This we can do and usefully do, even if we have no means of knowing whether our observations are taken from a process which is oscillatory or from one which converges to some equilibrium. One could quote an endless number of examples with the same force. It is true that we should like to be able to describe and predict the course of economic processes in great detail, but it is not true that in our present stage of knowledge the notion of an equilibrium which may never be attained is not of very great help in doing the best we can.

But of course for all this to be possible we must be able to say of any given state whether it is an equilibrium or not and one must be satisfied that the weak causal claim is in fact correct. Here one encounters the first serious objection, for it is due to the work of general equilibrium theorists themselves that one is persuaded that the weak causal claim is false.

Let me recall that this claim is that any process purporting to describe an actual economy could terminate, if it terminates at all, only in an equilibrium. But returning to a line of study first pursued by Edgeworth, it was noticed that a feasible state of an economy in which no coalition of agents could improve themselves would certainly be one for which we would be prepared to say that it could be a resting place of an actual economic process. The same arguments as before would apply. This set of states is called the Core of an economy. It is easy to show that every Arrow-Debreu equilibrium is in the Core. But the converse is only true under an extremely restrictive postulate.

What this means is that one can describe states of the economy which if they obtained would leave us no plausible reason for supposing that these states would change; yet these states are defined without for instance prices entering into the description at all. Thus, except for the special case when the Core and an Arrow-Debreu equilibrium coincide, there are already two differing equilibrium concepts of which the Core is plainly the more general. I hope that this is sufficiently clear and that the plurality of equilibrium concepts is not confused with the possibility of there being many states which are Arrow-Debreu equilibria. In particular the method by which agents form coalitions and sustain a state in the Core need have nothing to do with the parameters of an Arrow-Debreu economy but may rest on rules of thumb, law, etc. The Core indeed has some claim to be regarded as a concept of a social equilibrium in the sense that for a Core-state we can think of no reason why self-seeking agents should wish to combine to upset the status-quo.

The special circumstances for which the Core and an Arrow-Debreu equilibrium coincide occur when the number of agents is very large - strictly when there is a continuum of agents. This is simply a formalisation of the notion that each agent is without power, which in turn is what Joan Robinson and others have always noted to be a requirement if prices are to be treated parametrically by economic agents. One the other hand when costs of coalition formation are zero it can be shown that when the number of agents is large enough (but less than infinite and of course not a continuum), any core-state is 'near' an Arrow-Debreu equilibrium. Of course 'near' etc. must be properly defined. In any event, unless we are satisfied that the approximation is in practice a good one we must abandon the claim that all states which are not Arrow-Debreu equilibria cannot persist.

Now as far as households are concerned I have no great difficulties in accepting that a continuum or very large number of them is a satisfactory idealisation. But the same is not true of firms and indeed a consideration of these agents leads to great difficulties with the Arrow-Debreu equilibrium which are additional to those which arise from the Core. For it now seems to me clear that there are logical difficulties in accounting for the existence of agents called firms at all unless we allow there to be increasing returns of some sort. But when there are increasing returns it may not be possible to show that there are any logically possible economic states which qualify as either Arrow-Debreu equilibrium or as members of the Core. It may also be wrong to think of a very large number of firms.

It is one of the great virtues of the way good economic theorising proceeds that it allows us to pinpoint difficulties precisely and to be precise about the difficulties. Thus while it is the case that I agree with Professor Kaldor that increasing returns are a telling objection to the perfect competition equilibrium notions I have so far discussed, it seems to me important not to let this observation be an occasion for the slackening of our intellectual muscles. So I shall first briefly explain what is known in the present context. Later, when I have made certain proposals I shall return to the problem.

The first point to emphasise is that an Arrow-Debreu equilibrium may exist when there are increasing returns. Not only is this so when these increasing returns are not internal to firms, but even if they are, provided they are not too large. I want to emphasise here the paradoxical position of some of the critics. They complain of the excessive generality of the construction but at the same time believe that the whole edifice must tumble if it ceases to be completely general. But if we have particular information about the relationships characterising the economy then it is perfectly possible for an Arrow-Debreu equilibrium to exist even though the axioms of the theory are violated.

But if it does exist then of course any particular equilibrium returns will not be increasing. This may be unacceptable to us on empirical grounds and we consider an alternative which turns on a second kind of approximation. This route was first explored by Lerner (1944), and much clearer in a splendid paper by Farrell (1959), and finally made general and rigorous by Starr (1969). Here is the result. If in a precise sense increasing returns to scale are small relatively to the scale of the economy then there is an Arrow-Debreu equilibrium which is an approximate equilibrium for the increasing returns economy. This approximation improves with the scale of the economy.

If increasing returns are not to be important evidence against the equilibrium notions which I have been discussing then both approximations must be close enough. That is, it must both be the case that increasing returns are small relatively to the scale of the economy and that there are sufficiently many firms to allow us to deduce a close correspondence between the set of Arrow-Debreu equilibria and the Core. I now want to say that while I think Professor Kaldor's belief in unbounded increasing returns to be false I do agree that we may not be able to maintain that they are small enough to allow the approximations here spoken of to be judged good enough.

I shall return to the whole matter later. Here I want to note that the rather uncontroversial view that increasing returns cause difficulties to perfect competition seems to me to bear no logical relationship to the claim that therefore equilibrium notions are not required or that they are sterile.

(III)

But let me now turn to other difficulties. The Arrow-Debreu equilibrium is very useful when for instance one comes to argue with someone who maintains that we need not worry about exhaustible resources because they will always have prices which ensure their 'proper' use. Of course there are many things wrong with this contention but a quick way of disposing of the claim is to note that an Arrow-Debreu equilibrium must be an assumption he is making for the economy and then to show why the economy cannot be in this state. The argument will here turn on the absence of futures markets and contingent futures markets and on the inadequate treatment of time and uncertainty by the construction. This negative role of Arrow-Debreu equilibrium, I consider almost to be sufficient justification for it, since practical men and ill-trained theorists everywhere in the world do not understand what they are claiming to be the case when they claim a beneficent and coherent role for the invisible hand. But for descriptive purposes of course this negative role is hardly a recommendation.

Once again I believe it important not to relax precision just when it is most required. It is difficult to think of words other than perhaps ‘struggle’ which are more of an incitement to idle chatter than is the word ‘dynamic’. Samuelson (1967) noted this years ago, but it is still true that to claim your theory to be dynamic often allows you to get away with murder. So let me develop what I have to say with as much care as I can.

An Arrow-Debreu equilibrium can be interpreted as a state of affairs where (a) all actions are decided upon at only one instant of time and (b) actions always contain contingent elements. The latter follows from including the state of nature in a definition of goods and representing actions in the space of all goods. However this interpretation if is to make sense requires there to be markets in all goods and so a large umber of contingent futures markets. We have an empirical confrontation since we know that these are in fact very scarce. We also have a theoretical confrontation. Elsewhere (Hahn 1971) I have shown this by an appeal to transaction costs and Roy Radner (1968) has shown that the state of the world formulation of contingencies is too narrow and that when it is supplemented by states which depend on the actions of agents, some contingent markets could logically not exist. So we are in the following position. We can use the Arrow-Debreu equilibrium in a very effective and empirical fashion. We can easily refute propositions such as those on exhaustible resources which I have already referred to. Moreover, one can locate precisely where the argument goes wrong. On the other hand we have now yet another reason why this equilibrium cannot be claimed to describe properties of all potential terminating points of any actual process.

We thus find it reasonable to require of our equilibrium notion that it should reflect the sequential character of actual economies. But I believe that we require more than that: we want it to be sequential in an *essential* way. By this I mean that it should not be possible without change in content to reformulate the notion non-sequentially. This in turn requires that information processes and costs, transactions and transaction costs and also expectations and uncertainty be explicitly and essentially included in the equilibrium notion. That is what the Arrow-Debreu construction does not do. I do not at all believe that therefore it is quite useless. But certainly it is the case that it must relinquish the claim of providing necessary description of terminal states of economic processes.

We have reached the point where a great deal of research and discussion is going on just now. We have also reached the point where the rather grandiose Arrow-Debreu notion gives way to the more 'feet on the ground' Keynesian one. I shall only be able to make passing reference to the many new ideas now being actively studied.

Certainly it is now widely agreed that is in undesirable to have an equilibrium notion in which information is as perfect and as costless as it is in Arrow-Debreu. This is so for the reasons which I have already discussed. Loosely speaking the information an agent gets can be thought of as a message from the environment to himself and the information he has can be thought of as a partitioning of the environment. The finer the partition the greater the information which one has. Radner (1968) has taken the first step in studying an equilibrium relative to the information available to agents. Hurwicz14 and others have been examining in a formal way the extent to which prices are adequate and, in a precise sense, efficient, informational signals. I and others have been studying equilibria relative to transaction possibilities which are costly and the resulting sequential character of the economy. Most importantly Radner (1972) has pioneered the study of stochastic equilibria in relation to the von Neumann growth and to Arrow-Debreu. In the latter he requires that agents do not differ in their expectations as to which price vector will be observed in each state of nature but that they assign different probabilities to the occurrence of each state. The economy is sequential and the stationary distribution of prices at which in no state is any good in excess demand and which are the prices agents expect for each state, is the equilibrium. Green

(1971) and Green and Majumdar (1975) as well as Hildenbrand (1971) have studied equilibrium notions for an economy in which preferences and endowments held by agents are random. There are theories for instance which give the precise circumstances in which the expected excess demands everywhere will be small and the market disappointments small on average with very high probability. Grandmont8 and others have studied short period equilibria with multi-valued probabilistic expectations.

All of this work is in its infancy and one would be a very dull sort of chap if one could not think up objections. But the whole subject is plainly on a promising track. What I want to do now is to make a suggestion of how we may want to proceed.

(IV)

It will be useful to go back to the beginning. In particular one wants to re-examine the idea of the equilibrium actions of agents where the latter are taken as acting sequentially in real time. In what follows I do not assume perfect competition. I ask for your indulgence for a brief lapse into an abstract more of proceeding.

At any date t there is a history of messages received by the agent. We divide these into those which the agent considers independent of his own actions, the *exogenous messages*, and the remainder. For instance observations on the weather, Government policy and some prices will be exogenous. His own actions, such as the amount invested etc., are messages by the agent to himself and fall into the other category. But so does the amount of his output demanded at prices which he has set when competition is not perfect. Just as the brain must process the many complex messages received by the eye, so the agent must process the messages from the economy and nature. This processing at t I want to call the agent's theory at t.

To make this notion precise requires a careful description of the message space and I do not attempt this here. But by an agent's theory at t I want to mean the following:

(a) the agent has divided the messages into the two categories mentioned;

(b) for any sequence of exogenous messages from date t the agent has a probability distribution of the outcome of any proposed sequence of acts from t onwards;

(c) the agent has at t a probability which he will assign to receiving any exogenous message at any date in the future conditional on the messages received since the date t and that future.

Thus for instance if the agent is thought of as a Bayesian econometrician constructing a model of the economy in which he is an actor he would be said to have a theory.

I shall want to say that an agent is *learning* if his theory is not independent of the date t. It will be a condition of the agent being in equilibrium that he is not learning. There are at least two ways in which this requirement can be misunderstood, which I deal with now.

Suppose that at t the agent assigned probabilities to the two events that it will and will not rain in Cambridge at $t+1$. At $t+1$ he will know which has been true. This increase in his knowledge is *not* what I mean by learning. An example of learning in my sense would occur if at $t+1$, having observed rain, the probability he attaches to rain in Cambridge at $t+2$ differs from that which he attached to that event at t conditional on rain at $t+1$.

Secondly, the requirement that the agent should not learn does not imply that in the more customary sense his expectations must remain the same. For instance at t the agent may assign probability one to the price of some good at any subsequent date being equal to the exponentially weighted average of the prices for that good observed at all times up to that date. He may of course be wrong but as long as, roughly speaking, the method by which he makes his forecasts is the same at all dates he will not be learning in my sense.

I now return to the argument. This is best conducted at the moment by thinking of the agent as a dynamic programmer. Given the agent's theory at t, the programme is solved if with every message array at any date from t onwards the agent can associate an act for that date. This mapping from messages to acts is called a policy. In general this policy will be independent of t only if (a) the agent does not learn in the sense I have used this term, and (b) his objectives do not change. So the reason why I want the absence of learning to characterise the equilibrium of the agent is that I want his policy to be independent of t.

Again there is a possible misunderstanding best dealt with now. The agent's policy being independent of t does not imply that it is independent of calendar time. For instance the agent's age can be an argument of his policy without that policy ceasing to be an equilibrium policy.

The concept of the equilibrium action of an agent here proposed is such that if it is in fact the action pursued by the agent an outside observer, say the econometrician, could describe it by structurally stable equations. When the agent is learning, however, then there is a change in regime so that one would require a 'higher level' theory of the learning process. Such a theory is not available at present. If it were then I still agree with what I wrote twenty years ago (Hahn 1952): 'if a definite behaviour pattern can be established for all situations then nothing is gained by labelling any particular behaviour as equilibrium behaviour'. In our present state of knowledge however it is routine behaviour and not behaviour which we can hope to describe. Indeed one of the reasons why an equilibrium notion is useful is that it serves to make precise the limits of economic analysis.

I have of course in my description made excessive demands on the rationality and computational ability of the agent. There are a number of ways in which one can depart from this. One way is via a route called 'bounded rationality' by Radner. As an example of this one can suppose that the agent peers only a short distance into the future, or that he has to ignore a class of messages which we can recognise as relevant to his objectives or again the objectives themselves can be drastically simplified, as say in the Robin Marris theory of managerial behaviour. But it will be clear that the particular description which I have used was chosen in order to lend precision to concepts and that these for a wide class of alternative routes will continue to serve. N particular the notion of the agent's theory, and of his actions conditional on that theory, as well as the rather general description of what one wants to mean by learning should continue to be appropriate in much less abstract formulation.

On the other hand this is the point to pause to take note of objections of practical men, of psychologists and of some economists, to the idea of the calculating rational agent. This notion is not peculiar to any school: it occurs in Marx and Ricardo as centrally as it does in the work of say Professor Hicks and it is used by Professor Robinson in her study of the choice of technique as much as by Professor Solow in his. Indeed, even hard-faced Treasury men are accustomed to assume such agents when they scrutinise new tax proposals both for their effects an for avoidance possibilities.

Now the objectors are by no means agreed in their objections. For instance Professor Kaldor (1972) believes that the received theory is vacuous by virtue of being unfalsifiable, while Professor Kornai (1971) believed that the theory is false. Other critics point to the prevalence of habitual and conventional behaviour, while others emphasise the spontaneous and perhaps erratic element. Many simply dislike the formal apparatus which the doctrine has evolved and hold the views which Walras (1954) reports: ‘such as “that human liberty will never allow itself to be cast into equations” or that “mathematics ignores frictions which is everything in social science” and other equally forceful and flowery phrases’. So the objectors are a mixed bunch and it is not at all clear what each of them proposes to do about the problem they raise. One proposal - to do without micro-theory altogether - which is occasionally made I shall take up briefly later.

Some of the objections are of course easily met while others have more force. For instance it can be agreed that profit maximisation is not a falsifiable hypothesis until we have decided what the definition of profit in that proposition is to be. But of course we do decide on this when the theory is used. In my approach we would have to specify the theory which the agent holds as well. When this is done this falsifiability is obvious. What is more important in my present context is that it is precisely the empirical claim for the usefulness of the equilibrium notion that the theories and motives of agents are sufficiently stable and we are not allowed to invoke changing theories or motives to help us out of falsified predictions. That is the whole point of the distinction between the two kinds of actions which I have been making. It is worth noting here that even so abstract a hypothesis as the maximisation of Bernoulli utitlities has been falsified by experiments carried out by Professor Raiffa at Harvard.

Another of the misconceptions arises simply from the difficulty the practical sound commonsense man has in understanding what the theorist is doing. I have already noted before that he has difficulty in understanding that for empirical purposes it is not only possible but desirable to be far more particular and that this need not at all be damaging. For instance the assumption that individuals have a preference ordering over the whole commodity space is rather dotty - the postulate that they can order it in the vicinity of where they are is not. Moreover it has been known for a long time that theorising survives a certain randomness of preferences. The observation that preferences themselves are the result of complex social and biographical processes has of course nothing to do with the issue.

The real objections are really quite different: they are that we know too little about motives and theories which are held by agents and not that if we knew them it would be a bad hypothesis to suppose agents do as well, in the light of motives and theories, for themselves as they can. For instance I have laid great stress on the difference between the perceived environment and the environment but very little seems to be known of how the two are related. There is also the very serious difficulty connected with the plausible requirement that the theory held by the agent must in some sense be simple enough to be intellectually and computationally feasible for him. Indeed I have no doubt that the simple textbook treatment of these matters is false, as are for instance many elaborate models of portfolio choice. But all these are indications that we have a lot to do in economics and they do not seem to bear on the basic methodological stance.

There is, however, an important link with my main argument which I wish to make. The reason why economists have for so long been interested in rational actions is because they claim that these have survival value. Schumpeter (1955), writing of what happened to people in the transition to capitalism, notes that 'they were rationalised, because the instability of economic position made their survival hinge on continual deliberately rationalistic decisions - a dependence which emerged with great sharpness'. One is here back to the weak causal claim which is to be made for an equilibrium and equilibrium actions. But it is not only the way actions are determined but also the motives which determine them which may be selected. The low aspiration haberdasher may not survive Sir Isaac Wolfson's scouts. So it is one of the claims of this kind of theory that certain institutional environments only permit certain kinds of behaviour to qualify for equilibrium behaviour. In the institutional set-up of capitalism, as Marx noted, the biographical peculiarities of agents may be of little significance in describing equilibrium states. Objector who focus on the failure of the theory to describe any given individual are thus wide off the mark.

Lastly, it is of course precisely my contention that equilibrium actions of agents will reveal themselves in habitual behaviour so that objections from that source I can ignore. But notice the difference between the man who says people choose goods out of habit and the one who says people have a habitual way of translating prices and incomes into choices. The former is not very helpful.

I now turn to the most difficult of the remaining questions of how to characterise the equilibrium of the economy as a whole. The proposal which follows is not quite the proposal which I actually want to make. The latter would include a postulate on the distribution of agents by type. But I have found that in the present formative stage of my ideas the putting across of the full story would have been very complicated and so I shall concentrate on a kind of reduced form version.

The definition I want to adopt is the following: an economy is in equilibrium when it generates messages which do not cause agents to change the theories which they hold or the policies which they pursue. This is not the usual definition and I return to the difference. The difficulty which arises is in specifying precisely the conditions which will cause an agent to abandon a given theory and change his policy. For the rather abstract formulation of the agent's theory and policy which I have adopted even simple examples involve the language of statistical decision theory. I am at this stage not at all clear of what the precise formulation should be. So I content myself with the ill-specified hypothesis that an agent abandons his theory when it is sufficiently and systematically falsified.

Here is an example. In a given economy an element of each agent's theory is that prices can be treated parametrically. In particular, firms believe thy can sell what they wish at prevailing prices and households that they can buy what they wish. The theory of agents therefore predicts prices but not quantities. This theory would be falsified if sufficiently frequently firms found that they could not sell what hey wished at prevailing prices or households that they could not buy what they wished. The amounts actually sold at any hypothetical price may have to become an element of the theory held by firms. So one of the conditions for the economy to be in perfectly competitive equilibrium is that agents almost always can sell and buy almost all they wish to at ruling prices. In this case the definition of equilibrium which I have suggested implies almost the missing traditional complement that markets are cleared.

It does not quite imply it for two reasons. Firstly, when one makes the present ideas more precise the exact clearing of markets is not a reasonable necessary condition for the theory I spoke of to be persisted in. Secondly, short enough and rare episodes of uncleared markets would on my definition be consistent with equilibrium. These are to me agreeable implications. For instance I am not forced, as is tradition, to say that the economy is out of equilibrium if a housewife finds on a rare instance that the shop has sold out of butter or indeed if there is always some housewife who fins this to b the case in some shop.

It is of course not an implication of this formulation that in equilibrium any quantities and prices or rates of change of these are constant. What is required is a frequency distribution of prices conditional on exogenous events which in some precise sense corresponds closely enough with the prior conditional distributions held by agents. Here much depends on the precise description of the agent which we adopt and if we are much less abstract and demanding in this we shall also have simpler descriptions of equilibrium states. If we restrict ourselves to short intervals of time only the errors which we can permit in the theories held by agents became larger.

In a purely verbal exposition and at this level of generality I cannot really go very much further in describing the equilibrium which I intend. But I must note an important and interesting open question of a technical kind before I justify the approach. In order that any kind of equilibrium, even in simple cases, can be shown to exist I must show that there are theories which, if agents held them, would in that economy not be falsified. This is really what Radner (1972) did in the extremely simple case where the theory consisted in associating a given price vector with every state and having a probability distribution over states. In my case of course the complexity is far greater and it certainly will be a hard job to specify properly even a class of theories agents can hold which may be candidates for equilibrium theories. But it is not just a mathematical but a real problem. For what one is asking in the last resort is whether it is possible to have a decentralised economy in which agents have adapted themselves to their economic environment and where their expectations in the widest sense are in the proper meaning not falsified.

The traditional notion of an equilibrium which I described at the outset requires the equilibrium actions of agents to be consistent, whereas I have the weaker requirement that they not be systematically and persistently inconsistent. Again in the sequential formulation of the traditional notion, single valued expectations are exactly met while I very roughly require the convergence of prior probabilities to frequencies. In the traditional notion the environment to which agents are supposed be adapted bears only a pale resemblance to a capitalist economy. In the notion which I am proposing adaptation is to fluctuating prices and to noise. If one lives in a capitalist society that is what one is likely to regard as 'normal' and that is what one will have adapted to.

(V)

It will not have escaped the notice of the professional that it will be a consequence of my approach that one can only discuss the stability in the small of an equilibrium. Disturbances which in a proper sense are small and short enough will allow us to suppose that agents continue in equilibrium actions. Stability will mean that for short enough periods and small enough disturbances the set of equilibria is large but that it shrinks. Some quite interesting arguments are possible here but cannot be pursued. What is to be emphasised is that the position which I have adopted makes it impossible to make any global stability claims.

Indeed it is part of the case that when 'regularity of behaviour' has been translated into the rather broad definition of 'equilibrium behaviour' which is here proposed, we have gone as far as an economist can in the present state of knowledge go. That is why the notion of equilibrium behaviour is of interest and importance. I take this up again in my concluding remarks.

I now return to increasing returns. It has long been a commonplace that in a sequential setting with uncertainty, internal economies to scale and perfect competition need not be incompatible. Moreover in the fuller version which I have referred to we are permitted the waxing and waning of firms which Marshall had in mind. So there are good grounds for believing that in the formalisation of the ideas here put forward, and it is perhaps useful to stress that this has not yet been accomplished, increasing returns will not prove a great embarrassment.

But it would not be satisfactory to leave it at that when I have undertaken to take special note of Professor Kaldor. The first thing to notice is that Professor Kaldor is describing an equilibrium process where market coherence is ensured by the actions of merchants. These merchants endowed with suitable expectations are quite traditional maximising agents. The increasing returns which are being discussed are, except for the volume/circumference case, of the 'learning by doing' kind. We are not told whether firms in the process grow very large relatively to the scale of the economy or not. Curiously enough the process continuing smoothly depends on expectations being more or less correct. Great stress is laid on the mutual interaction of economic forces, in particular that between the extent of the market and the division of labour.

At first sight there is nothing here to cause distress to a champion of the equilibrium notion in economics. Indeed even if learning by doing is internalised by firms and we allow for uncertainty everything is ship-shape for traditional tools. Amongst these of course I include those provided by Professor Kaldor (1939) in a splendid paper on speculation which he wrote many years ago. It is also worth stressing that 'learning by doing' is perfectly consistent with the absence of learning in my sense. So even given the quite natural propensity for all of us to differentiate our product and given also that there is intrinsic interest in the role assigned to merchants, one is puzzled by the extraordinarily revolutionary implications Professor Kaldor detects in his ideas.

The answer I think is partly provided by the textbook picture Professor Kaldor has of what his colleagues are saying. Roughly speaking the well-behaved transformation and indifference curves in two dimensions fill his imagination. For instance the old neo-classical theorem that we let time progress into the future the transformation surface gets flatter and flatter even without increasing returns is not a result he mentions. Nor I think is he familiar with the rather sophisticated intertemporal version of opportunity cost. The other part of the answer is that he is simply wrong.

In saying this I want to grant everything that is being claimed for the division of labour and its interaction with the extent of the market. I also want to accept the merchants and the importance given to financial matters. One then asks whether Professor Kaldor has any foundation for his claim that one can no longer speak of the efficient allocation of resources and of production or of an equilibrium.

Now we say that a given path taken by the economy is production inefficient if there is an alternative one which gives us more of some good at some time and not less of any good at any time. There is nothing in the economy here discussed which makes such an ordering impossible. If we take finite time horizons, as long as we like, and suppose the set of alternatives closed, then an efficient path also exists. It is simply a muddle to go from the difficulties increasing returns pose to perfect competitive decentralisation to the view that allocation does not matter. Indeed the truth is orthogonal to this view. For the more important increasing returns are, especially the dynamic variety, the greater the potential losses from misallocation. I recommend here Professor Landes (1970) splendid analysis of why inventions in the textile industry became innovations in England and not for along time on the continent. Also Professor Kornai (1971) on the consequences of misallocation for the Hungarian economy is very instructive.

I have already dealt with the second part of the question and do not repeat the argument. But I now want to say that not only do Professor Kaldor's critical thrusts go astray, but they are also far too mild. For at no stage does he notice the important increasing returns, not only in production but also in information in the widest sense, will in due course have profound consequences for the institutional arrangements of an economy. Indeed one answer is that it is precisely the difficulty of efficient decentralised acts, if you like the growing realisation of their potential wastefulness and irrationality, which will generate just these forces which may bring the whole system down. In addition of course there are the classical Marxian forces of increased concentration and of formation of coalitions. It is at this point, as I have already remarked before, when a large historical vision is at issue, that equilibrium economies, whether my kind or Professor Kaldor's, is inadequate to the task. I fear that in tilting at the windmill of some old-fashioned textbook Professor Kaldor has missed the dragon.

(VI)

I have already noted that there are economists who wish to do without micro-theory altogether. I have only time to treat the matter slightly.

There are first of all those who believe that an analysis of the kind which I have been developing and which of course has firmly traditional roots, implies that the explanatory emphasis is put on the individual agent when it should be put on social institutions, such as property rights and the social relations which flow from them. I can deal with this very briefly because the view is simply based on a misunderstanding. As I noted right at the outset, traditional equilibrium theory does best when the individual has no importance - he is of measure zero. My theory also does best when all given theoretical problems arising from the individual's mattering do not have to be taken into account. The social institutions of property and markets have the dominant role. Indeed as Arrow and I wrote in our book (1972) 'the notion that a social system moved by independent actions in pursuit of different values is consistent with a final coherent state of balance and one in which the outcomes may be quite different from that intended by the agents is surely the most important intellectual contribution that economic thought has made to the general understanding of social processes'. So the point is quite simple: to argue that one requires a theory of the action of agents is not at all to maintain that the economy is to be understood by what any one agent wants. For my money, general equilibrium theorists are much closer to Marx than many a Marxist!

It is of course true that it is part of my case that I do not believe there to be an adequate theory of learning n my sense of routine formation. Certainly here I am at variance with the Marxists. But this fact does not bear on the issue of whether indeed these are important problems. I say that they are and I hope that to this all good Marxists will say amen.

Let me now turn to macro-economics in its relation to the present issue.

About two thirds of the *General Theory* deals with the theory of the action of agents, their motives for saving and for holding money, their investment and speculative behaviour etc. It is a consequence of intellectual coarseness and not of Keynes that University syllabi are so frequently divided into watertight macro and micro courses. Even if it is granted that in the manipulative, one might also say arithmetical stages of Keynesian economics, relative prices play a subordinate role, it is after all the case that Keynes argues that the actions of agents in markets would not result in the equilibrium posited by his predecessors. It is hard to see how this very important proposition is to be understood without micro-theory. Moreover the fundamental postulate that agents will not persist in actions when more advantageous ones are open to them plays a central role in the Keynesian scheme.

But of course it is absolutely correct to maintain that every feature of an actual economy which Keynes regarded as important is missing in Debreu. Indeed a great deal of what I have said already was in the direction of remedying that deficiency. But it is also true that Debreu and other have made a significant contribution to the understanding of Keynesian economies just by describing so precisely what would have to be the case if there were to be no Keynesian problems.

In the context of the suggestion which I have been making it is for instance plain that for Keynesian reasons the theories held by the sellers of labour must include forecasts about the amount they will be able to sell and that in the description of an equilibrium the theory that there is a ruling wage, at which one can or cannot work, will not be an equilibrium theory. I have constructed a miniature and rather crude model of a simple economy in my kind of equilibrium and it has a satisfactory number of Keynesian characteristics. There seems no good reason to suppose that the careful study of the interaction of agents is an activity hostile to Keynes.

But there is another and more difficult point. Keynes deals essentially with a Marshallian 'representative or average' agent that is reflected in the work of practical men when they speak of say 'the investment of manufacturing industry' or of 'the savings of the private sector'. This of course is a drastic short-cut and it lends to macro-economics that enviable air of commonsense. But certainly one must ask when such a short-cut is justified and in particular whether it will lead to significant errors. It is one of the oddities of the present scene that the very people who are most convinced that aggregation errors are decisive in rejecting a short-cut in the theory of capital are also the most disinclined to enquire what it is claimed to be the case when say investment equals savings. The latter equality is certainly consistent with disequilibrium in every market. I cannot pursue this in any detail now but I should like to make the following point. A macro-procedure is likely to be most reliable and approximately valid when the economy is in the kind of equilibrium which I have described. The argument here is pretty obvious. In any event it suggests that macro-theorists should not be disinterested in the study of such equilibria.

The view that macro-economics is in some sense essentially different from other kinds of economics in dealing with relations which are not deducible from the actions of agents I do not deal with, since it is rather obviously false. Also I have already touched on a related view at the beginning of this section.

I have left to the last a quite different matter, which I believe to be best exemplified by Professor Champernowne's important study (1953) of the distribution of incomes between persons. As you know he described a stochastic process, the stationary distribution of which has the Pareto-property over the relevant range of a observed distributions. This stationary distribution has a claim to be called an equilibrium. Champernowne's work is to be distinguished from that of Green and Majumdar, which I have already referred to, in that the whole theory treats the agent, not as making choices or taking decisions but as a passive receptacle of the random forces to which his income is subject. Another example of this kind of approach is Maurice Kendall's well known work (Kendall and Hill 1953) on the behaviour of share prices, which showed that it could be understood as a

Brownian motion.

From the present point of view the importance of these examples and of others like them is that it appears that a successful equilibrium interpretation has been put on observations without the notion of equilibrium actions of agents. Moreover it is usually an equilibrium in a strong sense in that not only will it persist if once attained but that all paths converge on it. But important and interesting as this work is, I do not believe that it contributes evidence against the approach which I have adopted. For it seems to me the case that the argument which these models have in common can only be clinched by an appeal to considerations which are outside the model, amongst which the equilibrium action of agents is one.

For it is of course not only an open question whether having exhibited one process with satisfactory asymptotic state there are not others which will do equally well. More importantly the theory must be made congruent with other things which we know, for instance the income chances of different social classes, the genetic distribution of the population and its economic relevance, choices and so on. In other words we shall want to distinguish the randomness generated over states of nature from the random prices generated by actions and choices. To take an extreme example, we may take the distribution of bulls and bears at any price as being random simply because the past has provided no evidence for agent's theories to converge. The quality of bullishness or bearishness acts like a state of nature which is assigned with certain probabilities over agents. But we must still know what it is that bulls will wish to do and what it is that bears will wish to do. Of course this is not a criticism of the very important work which I have referred to - it is an argument designed to show that this work does not lead one to want to do without a theory of equilibrium behaviour of agents. Indeed, as the literature testifies, the processes cannot be understood without such a theory.

(VII)

I have come to the end of what I can say in the allotted time and also to the end of what at the present stage of my thinking I can usefully say. It will be quite clear that this leaves me very much at the beginning of what could be called a theory. For instance I must draw your attention to the fact that while I used game theoretic considerations to criticise the Arrow-Debreu equilibrium I have hardly mentioned them again since. It is true that the notion here proposed is sufficiently general to accommodate such considerations but then also by the same token the level of generality is too high. Mathematical economists will have noted that the relevant spaces of action and of the environment for instance have been left rather undefined, and it is clear that in any concrete instance of the ideas here discussed they will have to be attended to and may eventually prove very hard. In particular will this be true of acts concerned with coalition formation and preservations. But my purpose has not been to construct a general model of the economy but to outline some of the conceptual operations which I believe now to be required. I have, as I have already reported, tried some extremely simple examples. But very many more will have to be tried before even a tentative judgement on what has been proposed is possible.

But it must be confessed that I have some confidence in some of the main features of the story.

Thus the view that we must require an equilibrium notion to make precise the limits of economics and think accordingly, seems to me to be sound. The fact that our evidence is always from the past makes it important to be able to say in what sense and in what circumstances we expect the past to shed light on the future. Our task is both more analytic and far less profound and universal than that of a historian. Certainly we want to study quite specific relationships - say between wage changes and unemployment - which take history for granted and to make generalising claims for regularity. But this regularity which we are interested in is surely associated with the kind of adaptation to an economic environment which I have been discussing.

For instance when one looks at the recent interesting work by Nordhaus and Godley (1972) on the pricing behaviour of firms one notices that one is asked to accept evidence for a particular form of routine behaviour. Certainly it sheds light on this work. But in any case Godley and Nordhaus have made claims not for how firms' behaviour in general is to be understood, but for how equilibrium behaviour should be understood. And that is as it should be.

This view of the rather limited possibilities of economic analysis is not one which will recommend itself to those who want economics to be a study of the 'laws of motion of a capitalist society'. I am not sure what sort of propositions would qualify as such laws nor what their status would be. But I am certain that in such an ambitious intellectual programme the expertise of the economist will only be a very small part of what is required. In the meantime there are many important problems in all societies which if they are not understood by economists will not be understood by anyone and it is here that our main obligation must lie.

I have also considerable confidence in my view that the main progress to be made now is to recognise quite explicitly the essentially sequential structure of the economies which we study and to wrestle with some of the very serious conceptual problems which this raises. In particular the distinction between the perceived environment and the environment and the consequential importance of the theories which are held by agents seems to me bound to become increasingly important in analysis, although it may come to be tackled rather differently that I have suggested here. Lastly I am rather convinced that the rational greedy economic agent will continue in a central role.

The kind of issues which I have been discussing have been concerned with the conceptual apparatus of economic theory. As such the analysis is almost bound to lack concreteness especially when some of the terrain is so speculative, and this fault causes me no feelings of guilt. But since some of what I have had to say turned on the inadequacy of our present paradigms I fear that the impression may have been gained that I think the latter to be 'sterile' and 'useless'. Nothing could be further from the truth. Not only does the Arrow-Debreu equilibrium continue to be a special ideal type of the notion here proposed, but it is also of great use for many purposes, some of which have already been noted. But the paradigm itself is of course of ambitious generality and for very many important purposes a much more modest Marshallian apparatus will do very well. For instance no economist required the recent investigation into beef prices. Most economists can go a long way in analysing the consequences of successfully controlling both wages and prices with perfectly traditional tools. In particular they can successfully use traditional notions of equilibrium and disequilibrium.

Indeed given that any actual economy is at least as complex as say the human brain it is surprising of how many propositions concerning it we can say that they are false. One need only think of the amount of misery which has been averted by the demonstration that the arguments for balanced budgets are false to agree that economics can do good even when it does not predict. The many false and harmful views on the role of prices which Arrow-Debreu confine to oblivion are also a feather in our cap.

I have therefore been concerned with the task of extending the range of phenomena the theory can deal with and not at all with a demonstration that the theory at present cannot deal with anything at all. Indeed I attach the greatest importance to the continuity of intellectual enterprises and would consider it a sure signal of bad scholarship and reasoning if I had kicked away all the ladders which we have. Professor Kaldor (1972) has quoted Einstein evidently engaged in answering a Kaldorian critique of abstract and difficult theory. Einstein in effect says that of course the final arbiter of any theory will have to be the evidence and this Einsteinian aversion to sin I share. It is not at all clear that the views of a physicist of genius on matters of epistemology of economics should have a special claim on our attention. But it so happens that Einstein has also delivered a pronouncement on the matters[2] which I have been discussing in the last few minutes. Since I so much agree with it I conclude by giving it here:

> Creating a new theory is not like destroying an old barn and erecting a sky scraper in its place. It is rather like climbing a mountain, gaining new and wider views, discovering new connections between our starting point and its rich environments. But the point from which we started still exists and can be seen, although it appears smaller and forms a tiny part of our broad view gained by mastery of the obstacles on our adventurous way up.

[2] Quoted in Sacks (1972).

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