Paradoxes of Perfect Foresight in General Equilibrium Theory

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Introduction

In Debreu's axiomatic analysis of intertemporal general equilibrium theory, there are many assumptions. Some of these are traditional within the subject of the free market and general equilibrium. But special are two: the auctioneer, and the use of dated commodities to avoid dynamic questions or, rather, reduce the dynamic model to the static model. This reduction of the dynamic to the static is typically axiomatic: the formal system is one and the same (there is not one static system to analyse static economies and a different dynamic set of axioms to analyse dynamic economies). But there are two interpretations possible. In the static interpretation, the commodities are interpreted as commodities in the usual sense. But the formal object 'commodity' can also be interpreted as 'dated commodity' and this gives a dynamic interpretation.

There is a peculiar interaction between these two assumptions when capital goods are present which has not been heretofore noticed. It robs the analysis of all that value towards elucidating the Capital Controversy which has sometimes been claimed for it.

It has always been hoped that the presence of the auctioneer in our models of general equilibrium theory (whether static or dynamic) was merely an expedient and did not lead to models with a significantly different behaviour from either reality or some putative general equilibrium theory to be developed eventually, without an auctioneer. (But no one has actually known how to remove the auctioneer.) The purpose of this paper is to show how this may be done in the static case, and in the dynamic case *without* capital goods,

under a certain special assumption, the epsilon assumption, which assumes, roughly, that individuals cannot by their market actions appreciably affect prices or quantities. Rather, the purpose of this paper is to show that a method which works in those two cases does not work in the case of the dynamic economy with capital goods. Thus there is a fundamental problem in general equilibrium theory related to the interaction between capital goods and so-called perfect foresight. A market needs social constraints, such as expectations or consensus or auctioneers, in order to stably valuate capital goods.

The Capital Controversy began with the study of simple aggregative models of the marginal productivity of Adam Smith's three factors, Land, Labour, and Capital. The point of this paper is to show that some of the concerns about the logical consistency of such models remains even in a fully disaggregated model. Sraffa, and then Robinson, pointed to a difference in the competitive market valuations of Capital from Labour. This difference was, they claimed, a fundamentally logical difference, due to the fact that the productivity of Capital depended on the time-pattern of its use in the productive process. Hence the rate of interest entered into the cost accounting of the process. But the rate of interest was supposed to be explained by the fundamentals entering into the cost. Is this a logical circle? We won't enter into all the twists and turns this controversy took. Both sides claimed a victory, of sorts. Samuelson and Solow claimed that Debreu's intertemporal, fully disaggregated model avoided all problems for each concrete individual capital good. But they conceded that aggregation would be, at least sometimes, invalid, and hence one could not speak of Capital as a whole. But Robinson and her co-thinkers had to fall back on criticizing the realism of Morishima's assumption of perfect foresight, and Debreu's model

too, and criticizing the methodological procedure of comparing different static equilibria with a process of motion in a dynamic economy. Now, both of these criticisms are a kind of retreat from her original criticism, that it was a logical problem, not an aggregative or index-number difficulty. Also, neither of these latter criticisms are peculiar to Capital. So, the point of this paper is to come up with a problem with Debreu's model that only affects capital goods, but does not affect the static model or the dynamic model without capital goods.

In retrospect, it will turn out that Robinson's point can be re-stated as follows. In general equilibrium theory as usual, the price of a commodity serves the purpose of rationing its use between competing uses. But a capital good has not only to be rationed between the different materialistic, individualistically utilitarian uses to which it may be put. (A concrete use, defined in individualistically utilitarian fashion, is independent of the rate of interest, of the degree of inequality in society, etc.) A capital good has two very different kinds of uses. The one is, as with any commodity, the production of utilities. The true opportunity cost of a capital good is the utilities foregone by the dedication of the resources it took to produce it to its production instead of to the production of other possible utilities. But the other is different than non-durable commodities. It is as a store of value, a speculative investment. Robinson's point is that because the future course of the (capitalised) value on the re-sale market of any durable good, including a capital good, influences this latter, one has the following characteristically deviant phenomenon: changes in the rate of interest affect the cost of a productive process, because the capitalised value of the capital goods has been tied up for that length of time and incur a purely financial opportunity cost which varies with the rate of interest. So the price is overburdened with

too many functions, compared to an ordinary commodity. This logical circularity, if so, arises even if there is only one capital good, as long as the time-pattern of production can vary. So it is not a problem of aggregation, if it is a problem. We could even assume that there is only one Capital Good!

Now the true justification of this insight depends on an analysis of some concrete mathematial model. We are going to define a model which ought to be equivalent to a special case of Debreu's model if his special assumptions were economically valid. We will show that these models do not behave well. It follows, then, that there is a hidden problem with his reduction of the dynamic to the static, a problem that does not arise except for capital or other durable goods. The possibility of speculative, strategic behaviour, is artificially ruled out by the presence of an auctioneer in a manner which is economically unjustified. But if there were no durable goods, the auctioneer and the reduction device of dated commodities is harmless and a justifiable simplification, at least in our special case.

Now, one must distinguish between foresight, which should be objective, and expectations (possibly based on a rule of thumb) which turn out to be self-fulfilling.[1] The latter are Keynesian, and the former are neo-neoclassical. It was not part of Robinson's critique to deny that capitalism could hold itself up by its bootstraps, in theory. It was simply to deny that any objective mechanism existed as to grounding these expectations.[2] More importantly, the latter cannot be based on rules which would also work out of equilibrium. We wish to investigate the possibility of a typical neoclassical justification (for a typically neo-neoclassical construction) for a foresight which could in principle be extended to be defined over an open set of out-of-equilibrium states of the economy, even though it would

reduce to a manageable, analysable expression only on the equilibrium points.[3]

The original Sraffa–Robinson critique was not about a fully Walrasian model, but a model in which the motivation for the ownership (and production) of capital goods was a profit maximisation (instead of a utility maximisation). And the way in which profit was calculated was based on conventional cost-accounting rules of some sort. So it becomes a question whether a deeper investigation of the motivation for owning capital could, by avoiding the notion of profit altogether, circumvent this logical circle with a better logical grounding.

Walras's original model attempted something like this, but had a serious conceptual flaw. Walras wished to ground the motivation for holding capital in the utility which would be enjoyed by the future flow of services (*via* profits but not by giving profits any special status). But in order to have a determinate model, he had to stipulate that only newly produced capital goods could be sold or bought. What he accomplished by this amazing stipulation is that the 'use' of capital goods as a speculative store of value (a use which has been highlighted by recent studies of overlapping generations models) was ruled out, so that it could not interfere with the use as a title to a flow of utilities. This reduced the 'load' on the price of capital (the number of different functions which it had to serve) and made the model determinate. (He also eliminated the role of expectations from his model, but Debreu's work showed that this is a less important a shortcut, if we can accept it.) Thus Walras eliminated mere accounting conventions from a role in the valuation of capital, but could not incorporate the re-sale value into his model and into the motivation for holding capital goods. Debreu, later, incorporates this re-sale value by means of his

device of using dated commodities (instead of succession through time of spot markets), but still has to use a fascist auctioneer to preclude attempts to influence or de-stabilise prices by means of bear raids large enough to influence prices.

We wish to also demonstrate that the most plausible avenue of revision of Debreu's theory suggested by the desire to remove these inconsistencies, is also an impasse, although for different reasons (of course). Hence, the third section of this paper deals with an infinite-dimensional revision of Debreu's theory for which, unfortunately, perfect foresight cannot exist due to the well-known generic failure of fixed point theorems in infinite-dimensional spaces (and hence, failure of equilibria points to exist).

Many writers have criticised the very notion of perfect foresight. Morgenstern's criticism[4] is highly logical and abstract in nature, and could be circumvented well enough for many theoretical purposes by the constructions of this paper if capital goods were not present. Davidson, too, has criticised for lack of realism the abolishment of genuine uncertainty and genuine dynamism by the replacing of spot markets by forward markets. However, none of these criticisms are accompanied by a perfectly precise model which exhibits reasonable perfect foresight behaviour in the absence of durable goods, but fails to work successfully in the presence of durable goods. This means that their critique has missed the connection with the Capital Controversy. It is the presence of capital (or other durable goods) which makes the future incalculable and unforeseeable, not the incalculability of the future which makes capital goods un-valuatable. It is because a) the presence of capital goods makes speculative strategies possible, and b) speculative strategies are not necessarily stabilising, that reasonably-perfect foresight is theoretically impossible.

That said, economists should then abandon general equilibrium theory *qua* foundation for monetary or macro-economic theory, as Davidson has argued, in favor of the study of the actual sociological institutions that provide stability to valuative processes. Capital is really a sociological institution, and so is Money.

I General Equilibrium Theory in general

The economic model traditionally and implicitly underlying Debreu's system has various assumptions, among which are those substantially due to Adam Smith: independent (atomistic) economic agents; separation of the roles of consumer and producer; production decisions by entrepreneurs alone; etc. on this level of theory. Especially I wish to call attention to two: individuals are rational and have as their goal the maximisation of their utility; and no individual has enough resources at their command to be able to affect prices, by their own individual actions, to any appreciable extent. Of course prices will be affected to some extent, so this assumption has to be structured a little more carefully.

Many careful writers on general equilibrium theory postulate that individuals are price-takers,[5] but this is, strictly speaking, backwards. It should be a theorem of general equilibrium theory that the individuals do indeed, at least approximately, wind up acting as price-takers at equilibrium. All we will assume is that the degree of imperfection of competition is very small. We must then show by analysis, and not by virtue of a separate additional assumption, that the effects of an individual's actions are of a certain smallness, say ϵ.

We next make the further assumption that each individual has a 'calculation threshold,' so to speak —a level below which effects or causes are left out of that rational

individual's calculations. If ϵ can be made arbitrarily small (for example, if this can be accomplished by increasing the size of the economy to a sufficiently large size and decreasing the largest fraction of total resources, by some conventional measure, held by any individual to a sufficiently small fraction) then we may assume that ϵ is less than this calculation threshold. For the remainder of this paper, we refer to this complex of assumptions and analysis as 'the ϵ-assumption.'[6]

So we have an assumption, a theorem, and a further assumption.

Equilibrium meant that each agent had done the best for themselves that they could have, and hence, if they had to do it all over again, would repeat their actions exactly. (This is not precise enough either...but to restructure it requires a later chapter.)

Next on the historical agenda was to extend general equilibrium theory to intertemporal processes. Smith did not attempt this. But as Capital accumulates, the rate of profit might go down. Was this foreseen? This must affect the notion of equilibrium, for investors in the production of Capital goods would be kicking themselves for having made a mistake if an unforeseen fall in the rate of return to Capital eventuates as a result of their collective actions.

This has been done by Walras, Hicks, Morishima, etc. on the basis of moving equilibrium and various forms of foresight. In the case of Walras, no foresight at all (together with an unmotivated demand for bonds and an artificial restriction that bonds are non-negotiable and that durable capital goods can never be resold). Hicks used conventional expectation rules. Hicks and those who came after him defined pefect foresight in the sense of a label only, a usage we wish to criticise severely. You don't get to make the evolution

of the system foreseen, simply by calling it 'foreseen.' These are merely what Robinson referred to as 'bootstrap expectations.'

Debreu and others tried to reduce the dynamic, future, intertemporal aspects to the existing static framework by introducing the formal mathematical device of a dated commodity. Actors in the market trade *now* in futures contracts for a commodity at a definite date. By this device, it was hoped, perfect foresight could be automatically achieved without a new, dynamic analysis. But this device interacts unexpectedly with the auctioneer if there are capital goods.

The point of this paper is to show that there is indeed a logical flaw in Debreu's formal device of dated commodities, his attempt to come up with a mathematical, formal trick to reduce the intertemporal case to the static case. We will show that the auctioneer plays a much more decisive role in the dynamic interpretation of his model than in the static interpretation, and that this is due not to friction or uncertainties or dynamism, but to the possibility of durable goods performing monetary functions and making destabilising speculative strategies possible. We show this by showing that there is a method of removing the auctioneer that works for Debreu's model if no durable goods are present, but does not work if capital goods are durable. This shows that the auctioneer is playing a very different role in the two interpretations. It shows that the two interpretations are not equivalent. It shows that the reduction 'trick' of using dated commodities to reduce a dynamic economy to a static one, is invalid.

The point is that things equal to the same thing are equal to each other, and if equal operations are performed on two models, putatively equivalent, the results should

be equivalent as well. The same method of removing the auctioneer, when performed on Debreu's static model, leads to one result, and when performed on the dynamic interpretation, leads to a different result. (It is irrelevant whether there is some *other* way, as yet undiscovered, which would work.) Therefore, the device of dated commodities was not some harmless formal equivalence, but concealed significant economic assumptions, and the economy with durable goods is not in fact equivalent to either the static one or the dynamic one without capital goods. (Debreu never claimed that his work was relevant to the Capital Controversy, this claim on its behalf was made by some of the other participants.) The auctioneer in either a static or dynamic economy without durable goods is relatively harmless, but Debreu's auctioneer in the case of Capital Goods is equivalent to a legal regulation forbidding speculation, and is not an economic argument which shows that in the absence of market imperfections, speculation would be stabilising or efficient.

In private, many experts[7] in general equilibrium theory are aware that such a problem probably exists (it is merely the Kakutani counter-example in a different guise), but it has not been embodied in a concrete precise mathematical model as of yet. Many recent papers in effect have a hidden auctioneer present, by building in a restrictive structure to the permissible actions of the economic actors. Our model shows why some restrictive structure is in fact necessary for equilibrium to exist. In the real world, these are sociological and subject to sudden short spasms, not mathematical or game-based.

Many models of dynamic economies have incorporated rules for forming expectations and investigated their consequences. This represents a departure from the free-market economic project, which is to show that maximising hypotheses alone suffice.[8] The realistic

study of markets and expectations is important, but not relevant to this paper or to the Capital Controversy, which is about whether *if* all imperfections or arbitrary subjective irrational modes of acting or forming expectations were removed, would stability and efficiency be achieved?

Many 'market game' structures have been studied. This concept will be discussed in detail at the appropriate place. Here we forewarn the reader that in all of these studies, an equivalent to the auctioneer is hiding in the structure of the strategies allowed. If a play of the game or strategy is defined in terms of uniform market prices, this begs the question we wish to investigate because it rules out many out-of-equilibrium strategies which would operate precisely to prevent the market from forming prices, just as the device of the auctioneer prevents collusive behaviour.

II Debreu's model and the ϵ-assumption

We now recall the relevant features of Debreu's model. More precisely, he offers two interpretations of the axioms: the static and dynamic.

In the dynamic general equilibrium model of his axioms, *op. cit.*, pp. 32ff, all commodities (or factors of production) are dated commodities. There are a finite number of points of time considered, a finite number of actors and of types of commodities, so there are a finite number of dated commodities and dated labour-powers (of every individual type) etc.; at time $t = 0$ everyone starts out with some initial endowment of commodities and factors or other and there are then a finite number of other values of t to be considered. We may as well take them to be all the positive integers up to N. As in Walras, there is no money, but one of the commodities may be selected as the numeraire as usual.

Each actor has an individual utility function. Every dated commodity enters into it separately as a separate variable. Thus any conceivable inter-temporal preference can be embodied in some utility function: including those characteristic of the life-cycle hypothesis, for example a non-monotonic (hence, especially, non-constant) time discount rate varying over time. (That is, when very young one may discount the future, later one may discount it less or not at all, and finally one may choose to discount it 100%.)

Before time $t = 0$ begins, bidding starts, just as in Walras. But since dated commodities of different dates are treated equally as commodities, but as different ones, the bidding is about each time period equally, *i.e.* all time periods are taken care of simultaneously, in advance, before $t = 0$. All contracting for processes of production, all choices of productive processes and so on, are likewise to be settled in advance by this auction, this tatonnement in the usual sense. That is, we are trading in futures contracts of every sort.

There is no uncertainty. Since all technological and market processes are 'open,' anyone can calculate, on the basis of a complete set of accepted bids, the entire future course of the economy and can hence, in particular, tell whether or not any of the bids are unfeasible. I will refer to these assumptions as those of perfect foresight (or lack of uncertainty plus calculability —thus calculability is part of perfect foresight).[9] By complete set of bids we simply mean that no other transactions will take place: thus the empty set of bids is complete, and if accepted and then finalized would lead to the trivial economy.

Not every feasible complete set of accepted bids will be ratified, however, since upon examination an actor might conclude that the resulting price system would allow them to increase their utility by adopting a different set of actions. To implement these actions of course would require a different set of contracts involving that individual. Hence they will

attempt to recontract in the usual Walrasian manner and so bidding starts up again. The conventional definition of equilibrium is that of[10] an equilibrium solution of the system as one following from a complete set of accepted bids which, if ratified, would yield an economy where no one is kicking themselves. That is, no actor could increase their utility by any amount of recontracting, at any point, at the market prices ruling[11] from the continued enforcement of everyone else's contracts with each other. The question for Debreu is, does an equilibrium solution exist?

He shows, under some convexity assumptions regarding the functions involved, an equilibrium solution exists. If the corresponding complete system of bids were made and accepted, they would then be ratified, the economy would start going, and every actor would be satisfied, at each point of time, that they were as well off as they could possibly be under the circumstances.

The premise of an auctioneer is a problem. Is it a serious one? If the epsilon-assumption holds, it is not a serious one, it could be removed easily (for example, by modelling the decentralised economy by a cellular automaton instead of a game).

The epsilon-assumption can hold for many static economies, and for many dynamic economies provided there are no durable goods. But if there are durable goods, then, as Jay Gould and Bunker Hunt showed, it is possibly profitable to obtain a corner on a market or a partial corner, up to the point where one can manipulate prices. Thus the epsilon assumption does not hold and price-taking behaviour cannot be assumed.

An admittedly less typical but still common enough speculation is the bear raid. One borrows a significant proportion of the total outstanding stock in a corporation and sells

it short, hoping to drive the price down. When the price is down, one covers the short by buying back the stock and repays the loan. Doubtless in real life one relies, for the success of such a raid, on imperfections of the market. However, the generalised life cycle hypothesis once again allows for the possibility of success in the presence of perfect foresight. There may be enough people at that point in their life cycle where they have to sell, or can't sell, or whatever, and are thus blocked from taking evasive action.

This last example shows where the ϵ-assumption comes in. An individual can borrow enough resources to try to corner an individual market, positively or negatively. Therefore analysis cannot succeed in showing that an individual's actions can *never* affect prices more than ϵ. Heretofore, economists have attempted to argue that at least no actions which take place at equilibrium can — that those actions which clearly can, are irrational at equilibrium: clearly dominated by some better strategy. But this argument is not available until an equilibrium point has been proved to exist. And the equilibrium point cannot be proved to exist unless we argue that strategies don't matter, only plans do. And this argument cannot be made until at least the ϵ-assumption version of 'price-taker' is established. General equilibrium theory founders on this vicious circle.

Intuitively, this makes sense. Your bearishness on stocks is primarily a function of your perception of the bearishness of all other investors (and the spot in your life cycle).

In what follows, we formulate an economic dynamical model without an auctioneer which, if Debreu's reduction device were valid, would be equivalent (to within epsilon) to his model with an auctioneer. Then we derive an inconsistency.

It suffices to derive this inconsistency under more special assumptions, so we assume

not only cardinal utility functions but even something more in our genuinely dynamic model, as follows.

In addition to all of Debreu's assumptions, we assume further that each individual has a utility function, it need not be fixed over time, $f(x_1, \ldots, x_n)$ where the x_i are the quantities of commodities (we are abandoning the notion of dated commodities so these are simply commodities in the usual Walrasian sense) consumed at that time (with the usual convention for factors of production, services, etc.). By plan, we mean a determination of all x_i as functions of t for all $0 \leq t \leq N$.

We assume that if the individual for some reason has decided on their budget of numéraire spent on consumption, $C(t), 0 \leq t \leq N$, then at every instant $C(t)$ is spent in such a way as to maximise utility at that instant at the prices ruling at that time. In this way, $C(t)$ determines a flow of utilities, $u(t)$. How to compare, intertemporally, utilities? We have that $u(t) \varepsilon L^2(\mathbf{R})$ (or some other infinite dimensional space, the exact one does not matter) (Wiener and von Neumann argued that every physiological process, including utility, was essentially L^2 so we go along with them for definiteness sake) so we assume that $F : L^2(\mathbf{R}) \longrightarrow \mathbf{R}$ is a function on the Hilbert space $L^2(\mathbf{R})$ such that if $u(t)$ is a flow of utilities, then $F(u)$ measures how much that flow is really worth to the individual (we use an atomic measure, of course, concentrated on a subset of the integers). We write $U = F(u)$ for the super-utility of that flow of consumption which follows the plan of that individual.

Next we extend the meaning of the word plan to include all sales, purchases, and production decisions of an actor, including investment purchases (and sales). Given a plan

for each other actor, we assume that one can tell whether a contemplated plan for one's self is feasible or not. (By calculability and perfect foresight.) We assume that the individual chooses a plan (practically this means choosing $C(t)$) which maximises U subject to the feasibility constraint (which replaces the budget constraint, including it).

Preferences cannot be revealed by behaviour at equilibrium, by a plan. (They always include information about what to do in case of disequilibrium.)

Unlike Walras, we do not assume that an investment good or a quantity of un-spent numéraire possesses utility, since this begs the question. Such holdings enter into maximisation only *via* their foreseen effect on U through future flows of $C(t)$ attributable to their sale or earnings.

This framework ought to be equivalent to Debreu's pseudo-dynamic model. For if equilibrium exists in the dynamic model, clearly everyone would be happy to make at time $t = 0$ the exact same decisions, and so would have no objection to committing themselves to all of their transactions, future and spot altogether, by futures contracts alone. Then, being perfectly happy, if they did this there would never be any recourse to the spot market. That is, for example, if at time $t = t_0 + m$ one did decide to sell off a Walrasian bond which one bought at time $t = t_0$, Debreu's device allows one to do this in advance, because of the extra flexibility of dated numéraire compared to annuities. This is doubtless what persuaded Debreu that he had not repeated, at a higher level, Walras's old evasion. If equilibrium exists in the dynamic model, there is never any harm in outlawing the spot market. Obviously, the conclusions of the model were intended to be robust under changes such as this one, provided perfect foresight is present.

This inadequate definition of equilibrium assumes that individuals are price-takers, whereas it should be a result (a theorem) that follows from the analysis of equilibrium (after it is proved to exist) that individuals are price-takers, *i.e.* that all our reasoning about the ϵ-assumption holds. Otherwise, price-taking contradicts rationality and perfect foresight.

But what is the definition of equilibrium?

III The notion of equilibrium in the absence of the ϵ-assumption

The definition of equilibrium had always been that one first of all has *a priori* defined the totality of states of the economy. One cannot use equilibrium conditions to rule out possible states prematurely.[12]

Secondly one must have some notion of force or movement. That is, each state of the economy has associated with it a (set of) mechanisms which would tend to move the economy into another state.

Then the definition of equilibrium is that at such a point, these forces are zero. (Accumulation is not regarded as a force.)

That all markets "clear" is not quite an acceptable definition of equilibrium, because at first it seems as though of necessity all markets clear: every good bought is also sold. Aside from this, what more can "clear" really mean? A superficial discussion of the demand curve for capital will run into the usual capital paradoxes. Furthermore, in some infinite-dimensional settings, the notions of supply or demand curves are not well-defined.[13]

There does not have to be a full-fledged theory of dynamics, in spite of the impression which may have been produced by the above discussion. Since tatonnement occurs outside

of time and since stability is not an issue, we only need to know whether the force is zero or not. We do not have to have a theory as to either its direction or its magnitude.

The force is always a market force of some sort: it always has to do with tatonnement. Therefore it always arises from a maximisation of some sort.

The universal definition of equilibrium is now posed at a more general level of theory. It is the game-theoretic one of Nash equilibrium. At equilibrium, if it exists, each individual's chosen strategy yields a result, given the other actors' choices of strategies, which is not worse than what would have resulted from the choice by that individual of any other strategy, in the face of the exact same choices unvaried by all other actors. This can be expressed for short as, 'no one is kicking themselves for having made that choice.' This formulation is not equivalent to the previous definition. This formulation allows for the possible effects, of one individual's change of strategy, on prices and quantities being arbitrarily large, and allows for these effects to be calculated and taken into account by all actors simultaneously.

To see this, recall the crucial distinction between plan and strategy. (They are sharply distinguished because we have in fact already given an unorthodox meaning to the word plan, but the word strategy we take in its orthodox game-theoretic significance, which we recall immediately.) In *e.g.* the game of chess, the actions taken by the players are the moves, one after the other. The result is (the moves plus) the end of the game—a draw, or a win for one or the other. But the strategy is a plan for every contingency, laid out in advance, as 'if White plays there, I'll go here, but if White goes elsewhere, I'll resign, ...' *etc.*, constituting a tree of contingent decisions. Consequently, given the choices of

strategies of White and of Black, the game is completely determined, there are no further decisions to make: calculability means each player can formulate and carry out their own strategy without any more knowledge of the strategy of the other opponent than is entailed in the knowledge of the past and the present. but perfect foresight is not a prerequisite. Perfect foresight is guaranteed if, for simplicity, we further assume both players announce their strategy during tâtonnement. We may as well assume discrete finite time, from $t = 0$ to $t = N$, as before, t takes on integral values only. We agreed, before, that a plan was given by the complete future course of transactions of all sorts of an actor. Since there are only finitely many points of time and finitely many sorts of transactions there are only finitely many pieces of information needed (in one sense, *i.e.* in the context of analysis—not in the sense of information theory). For simplicity, let us assume in fact that the knowledge of $C(t)$ suffices. Then the space of plans is a finite dimensional space.

It is simply a mistake to define equilibrium in terms of what we have called plans (or allocations). Properly speaking, equilibrium can only be defined in terms of strategies, when calculability and perfect foresight are present. This observation would also be a non-issue except for the, heretofore unappreciated, fact that the ϵ-assumption is violated in Debreu's dynamic model. Therefore we must adopt a definition of equilibrium which leaves open, *a priori*, the possibility of an individual's actions having a calculable and foreseen effect on the future course of prices and quantities. This is done by considering strategies.[14]

As remarked above, if there were an equilibrium point, then less would do in fact. (But the existence of equilibrium must be proved first.) For precisely this reason, the usual

sloppy neo-neo-classical paradigm adopts the overly specialised definition of equilibrium rejected above, in spite of its manifest unsuitability for dynamic optimisation when the generalised life cycle hypothesis (general non-homogeneous F) is allowed.

Now what is a strategy? It is at the very least a function of all other actors' plans. It is therefore a function of a continuous variable (since plans admit of continuous variability in their parameters: even when time is discrete, prices are not, and especially the rate of interest is not).

Therefore the set of strategies is a domain in some Hilbert space.

But, as is usual in game theory,[15] from the game we construct, by a standard construction, an operator on the cartesian product over all players of the strategy space of each player (it will be multivalued, i.e. a "correspondence" in Debreu's terminology, except under suitable convexity assumptions, which we may as well assume for simplicity). This cartesian product is *a fortiori* infinite-dimensional and hence not locally compact, since each space of strategies is so. But there are not usually fixed points for such an operator: certainly all the known fixed-point theorems in mathematics are inapplicable to the non locally-compact case. Furthermore it is elementary to construct operators on a Hilbert space which preserve the unit ball but have no fixed points, although one cannot easily tell when they arise from a game by the standard construction. And not every game arises from an economy in our sense.[16]

Let us look intuitively at this situation, in order to be convinced that this inconsistency is significant and irremediable. How would it crop up in practice?

Our genuinely dynamic model is, recall, as follows: all transactions are bargained for

immediately preceding the unit of time, the 'period', in which they are to occur, as in the usual Walrasian picture of moving equilibrium. (But perfect foresight is present, unlike the *locus classicus* of Walrasian growth theory, and unlike Hicks as well.) By calculability, any individual can, during the process of tatonnement, predict the future course of prices and quantities of production and distribution which will result over all future time if their investment strategy $C(t)$ and any given set of all other actors' investment strategies are implemented. (Thus one can tell whether a plan is feasible or not. But it is more convenient to imagine the strategies posed in some sort of terms that makes them always feasible, which is why we shifted to the game theoretic perspective in the first place.)

But what sort of information, what sort of calculation does an actor need to make in order to decide on the profitability of a typical financial speculation? Heretofore, neo-neo-classical economists have thought that they had proven that speculative behaviour no longer exists at equilibrium. However, in order to prove this, one must not assume that speculative behaviour does not exist or that speculative motives would not shape strategies meant to function at every point in economic space (or at least in an open domain around the putative equilibrium point). It makes no sense to say $f = 0$ at equilibrium unless f is defined on a domain in the entire space to begin with.[17]

A typical financial speculation would be to sell off a bond now, with the expectation of buying it back at a later time when its price has dropped. It is simply a mistake to argue that in a world of perfect foresight, no buyer could be found: what if bonds were the only durable goods? If the only way to save (or hoard) is to pay a premium for so doing, the life cycle hypothesis allows of the possibility of positive saving nevertheless.[18,19]

To correctly calculate the future course of the bond market, therefore, it would seem on the face of it necessary to know the inter-temporal preferences of the whole population of potential bondholders. This seems to suggest even more than we proved. It seems to suggest that one's strategy requires even more information than everyone else's plans, but requires even a knowledge of their preferences.

Probably the point that has caused the confusion is precisely this *petitio principii*—that is, whether only prices and quantities enter into all functions considered (including strategies and plans). If this is assumed, as clearly Debreu does, then there would be only a finite-dimensional space to operate in, on which Debreu's use of fixed point theorems depends very strongly.[20]

If no bear raid, no matter how large, undertaken by an individual, could have repercussions on prices and quantities, this might be a valid assumption. And if borrowing or naked shorting were banned by institutional factors from taking on magnitudes large enough to have a significant repercussion on prices and quantities, then one could argue classically, as Walras did (and justifiably so) that no such repercussions would occur. But the completely free market precisely abandons all such institutional constraints; as is clear in Debreu the only constraint is feasibility in the judgement (assumed, by calculability, to be always correct) of the two contracting parties to the futures contract (or loan).[21]

Then one individual's contemplated action will, possibly, affect prices and quantities. Thus, in order for that individual to judge the consequences of that action, they must know how each other actor plans to respond to any feasible contingency. It is, then, not even enough to know the individual demand and supply curves for each other actor. One must

know all strategies. That is, in order for an individual to maximise their super-utility subject to the constraint of others' given strategy choices, they must know how those other strategies will yield different actions in response to any changed contingencies due to repercussions in prices (or, indeed, anything at all) following a (virtual or notional) change in that first individual's own actions. Granted, if one's own actions never concuss prices, there might be no apparent need for taking the strategic dimension of others' behaviour into account. Debreu's analysis would be valid. But precisely this is what cannot be granted in the absence of institutional constraints—or the Walrasian absence of borrowing altogether (which would rob Debreu's analysis of its capability of dealing with growth and interest rates as well as rob its axiomatisation of its only real point, which was to englobe both static and dynamic analysis in exactly the same formal framework). So Parsons[22] is distinctly seen to have made a very prescient point: so far from its being the case that the notorious instability of financial markets is due to friction, uncertainty, and institutional constraints, it is only the institutional structure that allows market clearing at all and provides what little observable stability there is. And it is only the lack of foresight which allows the reign of what Keynes calls the conventional public state of expectations.

IV No examples

No example *tout court* can be given since the whole point is that perfect foresight is paradoxical.

Given, however, simply the set of all actors, their utilities and endowments, we can calculate at each point the temporary equilibrium if the savings plan is given also. Therefore, given the savings plans of each actor, we can calculate the succession of temporary

equilibria and assign a determinate super-utility to each actor. It may seem as if this succeeds in defining the game. And certainly this is not an infinite-dimensional situation.

Perhaps in extensive form, but not in strategic form. The strategy space of this game is still infinite-dimensional, even if the space of possible *plays* of the game[23] were a finite-dimensional space. There are, due to the failure of the ϵ-assumption, important strategies which can not even be approximately represented by plans. And Nash equilibrium cannot, as has been remarked, be defined in terms of plays of the game, but must be defined with reference to the space of all (non-dominated) strategies.

But the game if thus defined in extensive form is inadequate for a discussion of expectations, and especially the special case of expectations known as perfect foresight. Because no reference to, no incorporation into the model of, perfect foresight has been made by the utility functions, inter-temporal preference functions, or the savings plans. One could attempt to split up perfect foresight into two components: one, the knowledge necessary to calculate each temporary equilibrium which will result from the choice of a savings plan by every agent and hence the future evolution of the economy; two, the ability to predict the reaction of all other agents to one's choice of actions dictated by some other strategy, not describable by a savings plan. It would seem as though, heretofore, perfect foresight has been thought of as necessitating at worst only the former. If the ϵ-assumption held, this would be tenable. If price-taking behaviour were to be posited by *fiat* for no reason other than the wish to avoid the analysis of this paper, this could also be done, with logical consistency at the level of the mathematical model (but not at the level of the economic theory, where price-taking can be irrational and so contradict the postulate of rational

utility-maximising behaviour: price-taking becomes a sociological convention). But an equilibrium point has to be optimal with respect to all strategies, not just price-taking plans, so perfect foresight has to have the second component as well.

Now when this is done and one attempts to make use of it in the formulation of the game, we see that the game does not even exist. For simplicity, consider the inadequate version of the game, defined as above in extensive form and with one rule change: non price-taking behaviour is, somehow, not allowed by the rules. (I hardly know how this could possibly be implemented, but suppose, for the sake of simplicity of exposition, that it could be done.) For this derived game, whose rules are different, the strategic form will be the same as the extensive form of our original game, as described above, *i. e.* the strategies are the plans. Perfect foresight in the former sense is then needed for the players to know the pay-offs of the game. So, by analogy with the actual (but, as I claim, non-existent) game, one would expect perfect foresight (this time in the full sense) to, again, enter into the very knowledge of the pay-off matrix.[24] It would have to enter in in the following way: at time $t = 0$, each player announces their bids, and, since some of these are for capital goods, at least part of the savings plans are being proposed for implementation. A temporary equilibrium is attained by tatonnement.[25] But now we can see that perfect foresight is impossible, unless a Nash equilibrium existed. There is no way to predict the future course of relative prices without having the players announce their strategies to each other. But such an announcement is not required by the game.

Thus, perfect foresight in the ultimate sense cannot be built into the rules of the game. It belongs to an extra-game convention. It is part of the economic model, of course, and

this demonstrates that the economic model which we have constructed in order to revise Debreu's model in taking account of the failure of the ϵ-assumption, cannot be modelled by a game: it has to be modelled by the game whose extensive form is as above, plus an extra-game convention, that each player is committed to respecting a Nash equilibrium point. But now comes the contradiction. The extensive form of the game, as described above, assumes that temporary equilibrium is observed at each point in time (so, in particular, prices exist). How, then, are the players going to (how, even, are *we*, the analysts, going to) calculate the pay-offs resulting from the simultaneous choices of strategies which do not happen to be a Nash equilibrium? These strategies require that disequilibrium points be studied, and in particular, prices may not exist. But one cannot define the Nash equilibrium point without comparing it to these other pay-offs. So one cannot even define the game—it turns out that the extensive form described above is not correct.

In less abstract language, the proposed extensive form above didn't take into account predatory plays such as overbidding that never settles down, even after tatonnement, to a temporary equilibrium. The only correct model is thus a cellular automaton, which would allow people to 'get on with it' and make transactions even out of equilibrium, if it looked like things were never settling down (due to the absence of any Nash equilibrium point).

Thus, although one could make a game model out of such a cellular automaton, it would require very specific disequilibrium dynamics, analogous to the sort of expectations that, in Grandmont's work, prevent a sequence of temporary equilibria from being an intertemporal equilibrium. But then this would just open the whole Pandora's box of learning from past mistakes, and would make the use of game theory rather pointless.

And, as shown above, perfect foresight would be impossible. So one or another sociological convention would have just as much claim on our attention for a model as the more neo-neoclassically flavoured versions of rational expectations, whose only real advantage over the former was the alleged, putative optimality properties arising from equilibrium.

V Some objections that miss the point

It is simply impermissible to attempt to argue from the optimality of an allocation compared to the other allocations in the space of all allocations.[26] This fails to be an analysis of the market mechanism, tatonnement, *etc.* under discussion. It assumes the question away. The question is precisely whether individual, rational, but atomistic consumers can attain such an allocation *via* a Nash[27] equilibrium.[28] But the question of Nash equilibrium[29] pertains only to strategies[30] whereas the allocation is rather a *play* of the game. Everyone knows that two games can have the same plays, but different strategies and hence different Nash equilibria (and different values, because the rules are different).[31] An investigation of the space of allocations alone is like looking only at the set of plays of a game: it leaves the question of equilibrium untouched.

For these reasons, results[32] purporting to show (and in terms of the inadequate mathematical concepts chosen to model the economic ideas, they do so show) that as the number of actors in the economy grows, the core allocations approximate to the Walrasian equilibria of a perfectly competitive, price-taking economy, are nothing to the point. Because they do not imply that any of the finite economies have even an approximate equilibrium in the correct, adequate definition of equilibrium discussed here. But even besides the question of convergence, is the core really relevant to Walras's or Smith's concerns? The

answer of course is, no. The market participants cannot directly attain a given allocation, even if it is Pareto optimal and feasible, *etc.* Now in certain infinite-dimensional settings (although not very similar to the one we study in section three, so it is not in fact the case that they apply), there are mathematical results on the existence of optimal, feasible allocations (at least when preferences are subjected to a serious loss of generality, which seems incompatible with the generalised life-cycle hypothesis[33]). No, the market actors can only, at best, go through the maximisation of super-utility and then tatonnement.

Therefore, in defining equilibrium, we cannot simply optimise over allocations. We have to optimise over the reactions of the market to all feasible investment strategies. The actors have no direct access to allocations. They only control their strategies in the market game, their bidding strategies.

It is useless to point to the existence of a price system which is optimal when compared only to other price systems. That does not constitute a social equilibrium. By definition a social equilibrium is a choice of strategies which have certain optimality properties relative to all other choices of strategies.

Much work on 'imperfect' market structures has been done, and attempts to give Cournotian foundations to Walrasian general equilibrium theory have been published, for example by Mas–Colell[34]

As always, Cournotian approaches take prices as strategies.

Burgstaller has written a specialised tome in which he formulates a quasi-Hamiltonian sequence dynamic economic model which is equivalent to the intertemporal Debreu model. However, since his concerns are different than ours, he neglects the aspect of pure rational-

ity, utility maximisation, and perfectly rational foresight. Although he claims to remove the Walrasian auctioneer, he has incorporated even stricter unfreedoms into his model in a hidden way. His agents are divided into three classes, each with a well-defined role. Entrepreneurs have myopic preferences, as do households. Arbitrageurs have enough foresight to take advantage of the entrepreneurs by short-selling and other arbitrage techniques, but not enough foresight to try to outsmart each other. To make an analogy, the Walrasian auctioneer is like a fascist preventing the individual agents from freely colluding with each other against each other. But Burgstaller's castes are like a traditional caste system where no one can even imagine acting outside their restricted role. This accomplishes the same purposes as the Walrasian auctioneer, by similarly preventing truly strategic, anomic, behaviour from even being considered by the agents. (On the other hand, it is a welcome dose of realistic structure being imposed on the abstract Debreu model. Even more realism would be obtained by letting the castes generate deviant individuals who write their own rules... but eventually this extra realism could replicate the anomic lack of equilibrium we have demonstrated to lurk behind perfectly unfettered rationality.)

For example, his arbitrage equations assume that the whole future course of prices of capital goods enter into the arbitrageurs or other portfolio holders preferences *only through the rate of return*. In this paper, by contrast, we are investigating much more chaotic but foreseen time series of prices of a capital good, which cannot possibly be described by a homogeneous rate of return. His arbitrageurs act according to portfolio evaluation rules of thumb, our investors try to get the jump on each other...

Work summarised, for example, in Radner, R., 'Role of private information in markets

and other organisations',[35] falls into this category. Even when 'called' rational expectations equilibria, it is, clearly, only what Robinson and Hahn[36] called bootstrap equilibria. As with all theories of 'rational expectations' known to this authour, the variables taken are prices, and not strategies or preferences. Furthermore, the main interest in the research is in exploring the consequences of imperfections in the markets, for example in the failure of the existence of fully competitive future markets in all commodities at all dates. It could, however, be argued that the main significant deviation from perfection, in the real world, is the concentration of ownership of Capital and the resulting possibility of collusive behaviour that creates 'power', so although interesting, the reviewed research is probably less important than the sociological reconstruction of economic theory.

The Capital Controversy began with the claim by Robinson and Sraffa that there was a problem, which was *not* merely an index number or aggregation problem, with determining an abstract unit of 'capital' that would be independent of the rate of profit. The neo-neo-classicals, Samuelson and Solow, eventually gave up the point in part, falling back on a fully disaggregated model of general equilibrium with perfect foresight, such as Debreu's. The Cambridge side then could not formulate a criticism consisting of equations, as they had originally done, but fell back in turn on a) criticising perfect foresight as unrealistic and hence unscientific, b) criticising comparing equilibria points as if it could describe motion in time, and c) criticising Debreu's model as not 'authentically' dynamic, but only 'formalistically' dynamic. But this is weaker than the criticisms which were originally levelled against Clark and textbooks. For example, what is unrealistic in one historic period can become more so in another. Again, c) is more rhetorical than precise. The point of this paper could be seen as to make c) more precise and give it a mathematical

turn. We showed, quite specifically, that Debreu's model, which relies on the mathematical or formal device of dated commodities to reduce a dynamic economy to the same formalism as a static economy, gives the auctioneer a quite unsuspected role in the former. We showed that modifications of Debreu's model designed to remove the auctioneer work in the case where no durable goods are present, but fail in case capital goods are allowed to be re-sold. Hence there is something precise that can be said about the defects of his formal reduction of the dynamic to the static: it unintentionally and unsuspectingly strengthens the role of the auctioneer, a role that was already undesirable even in the traditional static case.

More importantly, Debreu was wrong to think that the issues of calculability were evaded by this device. Although the issues of perfect foresight and more particularly calculability, which is a part of it, are easier to see intuitively in the genuinely dynamic model, there leading to an inconsistency with the ϵ-assumption, they are still present in a disguised form in the pseudo-dynamic model. That is, perfect knowledge, by each actor, of their own feasible[37] set of strategies ϕ_j, is equivalent to our assumption of dynamic calculability. It is, then, a blemish to outlaw the spot market. It turns out that this is the exact analogue of Walras's evasion of assuming that bonds were non-negotiable and that the stock market sold only new issues.[38]

Notes

The author wishes to thank the Institute for Advanced Study for its hospitality while writing the first draft of this paper.

1. Hahn, "Expectations and Equilibrium," pp. 25f.
2. And to suggest that they would not survive exogenous shocks, since such expectation formation rules were unlikely to have the property of self-fulfillment generically.
3. Indeed, this is part of Von Neumann's definition of rationality and hence is a logical requirement for the application of game theory (Von Neumann–Morgenstern, p. 147f.). Since a player is rational, they act as though their strategy was "found out" by their opponents. It follows, then, that if a game has a unique solution each player's strategy is calculable by their opponents from their preferences and endowments. (And locally so in the case of finitely many discrete solutions.) Therefore I reserve the term "rational expectations" or,

more simply, perfect foresight, in microtheory, for this concept.

4. Schotter, p. 173ff.
5. Typical is Radner, 3.7.2, p. 442.
6. One cannot pass to an atomless measure space of agents, as a replacement for the ϵ-assumption, as if it were a limit of a larger and larger number of agents; unless the ϵ-assumption holds anyway, there won't be convergence, *pace* Aumann.
7. 'However, if agents are uncertain about other agents' behaviour, *so that strategic reasoning intrudes*, no equilibrium may exist even *with* complete futures markets. This problem is severe.' Weintraub, p. 92. And Morishima, 1996, p. 285f 'The mechanism of self-regulation of the market does not work... It is deeply disappointing particularly for the authour that he has to complete this volume with the final section of the Addendum which establishes a theorem that no general equilibrium of full employment is possible unless the equalisation of rates of profits between capital goods is ruled out. I call this thesis the "dilemma of durable goods"... [because of this] Walras's weak point [is] that the true demand functions of new capital goods are absent [from the model]. And Radner, personal communication.
8. The concept of a Nash equilibrium is a profound and valid generalisation of the historically previous, more naïve, notions of maximisation.
9. The way we have set up our model is different from Debreu's in several respects. Firstly, we assume rolling budget constraints, but this should make no difference in the presence of perfect foresight. Secondly, we assume that each firm is owned by precisely one entrepreneur who is also regarded as a consumer. Thus the entrepreneur maximises utility (or, rather, super-utility, see *infra*) in exactly their role as consumer. It is necessary to make this change because of the generalised life-cycle hypothesis. This should make no difference either, since even if we didn't assume it, the phenomenon of consumers' buying up a durable consumption good such as wheat for speculative purposes would crop up in the dynamic interpretation. It would be hard to justify, in a perfectly free market, banning this sort of behaviour to consumers and reserving it for producers. The third and only important difference is the definition of equilibrium.
10. This is a preliminary definition, which proves to be inadequate, see *infra*.
11. This is the point that proves inadequate in this preliminary definition.
12. Thus one cannot, by *arbitrage*, assume that all transactions take place at the same price—this begs the question.
13. Hildenbrand, W., "Comment on Chapter Four," in McKenzie, L., and Zamagni, S., eds., *op. cit.*, p. 111, and Aliprantis, C., Brown, D., and Burkinshaw, 0., *op. cit.*, pp. 168,176.
14. In general this invalidates models whose object are defined on a space of functions where the variables are prices of allocations.
15. Debreu, G., *loc. cit.* The operator is ϕ.
16. One cannot ask for an example of a dynamic economy with perfect foresight, for its operator, and for a proof that that operator does not have a fixed point. Because unless one knows in advance how to calculate the sequence of temporary equilibria, one cannot model the foresight, and thus cannot define the strategies of the game, and *a fortiori* the operator. I. e., *because equilibrium need not exist, perfect foresight cannot either: there is a contradiction in the model.*
17. Any variable occurring in f has to make economic sense, and be defined, in its economic meaning, operationalisable, independently of whether we are at an equilibrium point or not. It is for a similar reason that, as we argued before, perfect foresight is not the same as self-fulfilling expectations: firstly, Debreu's model is supposed to be an attempt to do without expectations. But more importantly, the methods of foresight, of calculability, should, just as any variable, make sense out of equilibrium also, since they should in principle at least be capable of being developed into a full-fledged economic dynamics, as would be required in order to be implementable on the cellular automaton which the general equilibrium theory is a putative approximation to.
18. Keynes, p. 357.
19. Robinson, 'Saving Without Investment,' p.191.
20. Recent work such as in Khan and Yannelis, eds., *Equilibrium Theory in Infinite Dimensional Spaces*, Berlin, 1991, or Aliprantis, Brown, and Burkinshaw, deals with a completely different set-up. Since they allow, *e.g.*, a continuum of commodities, there is no such thing as an excess demand function (*op. cit.*, p.168, p.176) so there is no operator at all. But the role of prices is, if anything, increased. This paper deals with the opposite situation: disequilibrium evolution

means that prices no longer exist: only strategies. But the operator still makes sense, as is usual in game theory. Even if convexity were guaranteed, the domain is noncompact, so the result of Ichiishi–Schaeffer doesn't apply.

21. In the literature, this issue is usually posed differently, though in a way that would be equivalent if not for the issues raised by this paper. A budget constraint in terms of discounted future values would, because of perfect foresight, come to the same thing as requiring that payments clear only as time rolls on, if equilibrium existed and the ϵ-assumption held. But if they are not equivalent, clearly the latter is more in keeping with the notion of dynamic.
22. Parsons and Smelser, p. 69f.
23. The plans are more analogous to plays than they are to strategies, or, rather, a savings plan, in the sense in which we have defined it, is a sort of strategy, but one in which the player resolutely acts as a price-taker. Thus it is very closely related to an allocation.
24. And not, note well, into any of the utility functions or preferences.
25. When we, earlier, assumed further that each player announces their own strategy, this was not because this was made part of the rules of the game, it was simply in order to facilitate a discussion of Nash equilibrium, which requires that this thought experiment be made.
26. Aliprantis, C., Brown, D., and Burkinshaw, 0., *op. cit.*, pp. 126ff., 3.3.9, p.131.
27. Nash, J., "Non-Cooperative Games."
28. Hahn, *loc. cit.*
29. Contrary to a superficial impression, Hurwicz, L., "On Allocations Attainable Through Nash Equilibria," in Laffont, J., ed., *Aggregation and Revelation of Preferences*, Amsterdam, 1979, fails to address this question, for the same reasons as previously given.
30. Of the game, and note well that this is not at all a "market game" in the sense of Shapley–Shubik.
31. Von Neumann, J., and Morgenstern, 0., *loc. cit.*
32. Aliprantis, C., Brown, D., and Burkinshaw, 0., *op. cit.*, p.43.
33. *loc. cit.*, p. 112.
34. 'The starting point of the research is the (informal) hypothesis that economic agents interact noncooperatively through given institutions. Those being essential, it cannot be expected that the same level of institutional parsimoniousness as in Walrasian theory can be reached.' Hildenbrand, W., ed., Advances in Economic Theory, Cambridge, 1982, p. 183.
35. Hildenbrand, W., ed., *op. cit.*, p. 96.
36. Hahn, F., 'Hahn Problem,' in, The New Palgrave: A Dictionary of Economics, London, 1987, 'the economy evolves the way in which it does because expectations are what they are and not for any 'real' reason.'
37. Debreu, p. 888.
38. The full generality of *op. cit.*, in an infinite-dimensional setting, must be called upon in the pseudo-dynamic model. That is, A depends on more than just the price system underlying the allocation x. It depends on the strategies of the other actors, as indeed the formal framework allows. Unfortunately the space of strategies is not locally compact, as we saw.

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