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THE INTRINSIC LIMITS OF MODERN ECONOMIC THEORY. The emperor has no clothes.

by

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"Therefore send not to find for whom the
bell tolls,... it tolls for thee".
John Donne.

General Equilibrium Theory regarded by many as the summum of the "grand neo-classical synthesis" has throughout its development been systematically attacked by a wide variety of critics from many different angles. Yet, curiously, these criticisms have been largely ineffective and it would not be unfair to say that this theory underpins what many are pleased to call "mainstream economics". Indeed such theory as is used by practical men to justify their economic recommendations is derived from this underlying framework albeit with unwarranted appendages. There seems to be a quiet confidence in the profession that we are moving, if only slowly, towards a more scientific basis for economics. Indeed, many economists seem persuaded that we are arriving at the point where the simplest criterion for a scientific theory, that it generate empirically testable and falsifiable propositions is met. This confidence that by enlarging the scope of the existing model without changing it fundamentally, we will be able to explain more and more satisfactorily observed economic phenomena, is not new.

Alchian (1965) said "Attacking any theory is easy enough, since none is perfect. But the wide class of empirical observations that <u>is</u> explained by economic theory should caution one against sweeping that theory aside...

I would like to thank Werner Hildenbrand for many helpful discussions and for specific comments, Walter Trockel for particularly enlightening comments and Pierre Dehez and Lucrezia Reichlin for their comments and insights. They are however absolved from all responsibility for the views expressed and the errors that remain.

What is wanted is a generalization of economic theory to obtain an expanded scope of validity without eliminating any (or "too much") of the class of events for which it is already valid...". Others have gone further and suggest that the neo-classical road is the unique path to scientific economics. North (1978) said directly, "To abandon neo-classical theory is to abandon economics as a science".

Yet paradoxically those who have developed or are developing general equilibrium theory are less complacent. This lack of complacency is epitomised by Hahn (1981) and is due to his awareness of the strength of the assumptions necessary to yield the propositions which are then applied by practitioners to real phenomena. The "first theorem of welfare economics" that a competitive equilibrium is a Pareto optimum is frequently used by certain economists to justify the "liberal" position in economic policy. Yet as Hahn says, "if these assumptions were stated and discussed they might be less inclined to declare free trade 'optimal'".

Paradoxically, there are many economists, some of distinction, who while staunch defenders of the basic tenets of general equilibrium theory have little but contempt for the abstract structure which gives the only rigorous justification for these tenets.

Hahn (1981) goes further however and observes "The ease with which so much current critique of General Equilibrium analysis can be countered is potentially dangerous. For as I said at the outset, the citadel is not at all secure and the fact that it is safe from a bombardment of soap bubbles does not mean that it is safe. Fortunately those "inside" have begun to build new walls and to lay new foundations".

Thus Hahn's position is that whilst we are heading in more or less the right direction the cause of economic theory is disserved by those who are ready to apply it naively in practice, a view incidentally that Pareto (1909) shared.

My purpose in this paper is to make a more radical point and to suggest that recent results have shown that, in a sense, the citadel which we are vigorously rebuilding is empty. Empty if it is to be considered as housing the elements of a scientific theory in the simple sense that I have described, one that generates empirically testable propositions. I would like to go further and to argue that this emptiness is inherent in the fundamentally individualistic approach which is at the heart of general equilibrium theory as it stands.

These remarks should be interpreted with caution. What I am suggesting is that those who are generalising standard results by weakening assumptions, or by adding small imperfections or rigidities to the model, no doubt strengthen the walls in Hahn's sense but the important task for the theorist is to people the citadel and, of course, many theorists are trying to address this task.

Criticism of the model

Before proceeding to explain the results in question I shall briefly look at some of those criticisms which as Hahn says, have proved remarkably ineffectual in stopping what many consider to be the inevitable progress of general equilibrium. This criticism will be relevant to what follows in that the results to which I have alluded lend force to what were sometimes misdirected arguments.

Much of this criticism is from without and argues that the very specification of the general equilibrium model is either tautological or misconceived. Whilst few question the logical coherence of this model many doubt its relevance to what they consider to be the essential problems of economics. This is not the place to review the wealth of critical literature that exists on this subject whether it be from schools or counter schools such as the Marxists, neo Ricardians, Keynesians, revisionist Keynesians, neo-Austrians etc. Collections of articles serving this purpose can be found in Bell and Kristol's (1981) "The Crisis in Economic Theory" in "Economics in Disarray" (1984) and in many books on economic methodology.

Without categorising the different criticisms we can say that there is a group who argue that economic evolution is essentially situated in time and cannot be reduced to the general atemporal "scientific model" which is that proposed by general equilibrium theory. Hicks (1979) remarked that "economics is on the edge of science and of history". He did not go quite so far as those who argue that it falls directly and unavoidably into the domain of history, but there is a long tradition of those who would maintain just that. In fact, Hicks argued that there may be sufficient constancy over time of certain economic relationships to maintain them as rules or laws, at least in the short run. Such a view is, of course, in a formal sense, incompatible with the full-blown Arrow-Debreu general equilibrium model since in that model everything is decided once and for all, and there are no repetitions. The economist frequently seeks to overcome this paradox by assuming some "separability" of utility over time, or those who consider a model with stochastic elements look for some sort of stationarity. In this way the economy can be reduced to something like a repeated experiment and thus one can reasonably hope to look for irregularities in reality which would be incompatible with this picture. I will return briefly to this and its relation to the evolution of economic theory in a moment.

Whilst the most extreme version of this view maintains that economics is intrinsically nonscientific other criticisms are addressed more directly at the specific model which is epitomised by general equilibrium theory. Here some of the schools, referred to earlier, question essential features of the model. However rather than detail these criticisms which would amount to summarising a century of heated debate, I will just mention some that will be particularly relevant to what follows.

There are those who argue that the underlying hypothesis of individual maximisation is inappropriate and that such an assumption is unnecessarily restrictive. Simon's work, with others, is often thought of as being a welcome and healthy alternative approach to the problem of individual motivation. Yet it should be noted that provided that the basic model

is one in which individuals react in some continuous way to signals (prices) it is formally equivalent to the Arrow-Debreu model.

A second objection is to the notion of equilibrium. In this view those signals (prices) which equilibrate demand and supply and thus make individuals' actions consistent although proved to exist in an Arrow-Debreu world, may not be attained. There are two problems here; in the first place if we do not allow all prices to be possible then we must redefine "equilibrium" and allow a different sort of compatibility between supply and demand, by imposing quantity restrictions, for example, than that envisaged in the standard model. This is, of course, the position maintained in the extreme case by the Barro-Grossman, Drèze, Benassy, Malinvaud literature. The question asked here is, if the domain of prices is restricted can we redefine an equilibrium notion and show that it exists. A second point is that when discussing whether or not equilibrium "will be attained" many authors are, at least implicitly, introducing the idea of an adjustment mechanism. They are asking much more than what is specifically proved in the Arrow-Debreu model. They wish to know whether with respect to some adjustment process, most commonly, the Walrasian tatonnement process, there is an equilibrium which is stable. Indeed, one formal interpretation of Keynes' major contribution is that he allowed for the possibility that in a general framework the standard adjustment process might not push the economy towards equilibrium. Without the stability of equilibrium changing one price (wages in this case) in the "right" direction might move the economy further from equilibrium. It is worth emphasising at this point that there are no "individualistic" assumptions of the sort used in the Arrow-Debreu model which will guarantee stability nor are there such assumptions which will ensure uniqueness of equilibrium. Such assumptions as are known are made at the aggregate level and are of an intrinsically different nature than those made on the endowments, production possibilities and preferences of individuals.

A slightly different point but one which goes in the same direction is made by Morishima (1984) who can hardly be accused of being unaware of

the precise content of general equilibrium theory. He says: "If economists successfully devise a correct general equilibrium model, even if it can be proved to possess an equilibrium solution, should it lack the institutional backing to realize an equilibrium solution, then that equilibrium solution will amount to no more than a utopian state of affairs which bears no relation whatsoever to the real economy".

Without any strong stability or convergence result it is true that the corpus of economic theory seems to be wanting. In particular if one thinks of the way in which economics is taught and justified, it involves, in an essential way, stories about adjustment. The "invisible hand" is, after all, the adjuster par excellence. Yet despite the lack of any such formal result to underpin the view that the economy can be treated as a process evolving towards an orderly situation there are those who strongly urge such a view.

For example Buchanan (1982) says "The 'order' of the market emerges only from the process of voluntary exchange among participating individuals. The 'order' is, itself, defined as the outcome of the process that generates it. The 'it', the allocation-distribution result does not, and cannot, exist independently of the trading process. Absent this process, there is and can be no 'order'".

A last criticism and one which has its relevance to what follows is that economics has been developed within a mathematical paradigm which is not really appropriate to the reality it wishes to portray. The view that economists have been trapped in a system derived essentially from 19th century mechanics is one which has been forcefully put by Ingrao and Israel (1987) in their recent book "La mano invisibile". This is far from novel as an observation but, argued carefully, is more convincing than the usual remark that the general equilibrium model is static and ought to be dynamic. For as Samuelson (1947) remarked, "Often in the writings of economists the words 'dynamic' and 'static' are used as nothing more than synonyms for good and bad, realistic and unrealistic, simple and complex. We damn another man's theory by terming it static and advertise our own by calling it dynamic...".

If the mathematical paradigm which we have adopted is indeed essentially static in nature then we will inevitably come face to face with the contradiction inherent in the dynamic nature of economics.

Before reaching this crisis however it seems that we have arrived at the point where the current model is shown to be intrinsically incapable of generating verifiable propositions. In other words instead of occupying themselves with defense against attacks from without general equilibrium theorists should be and indeed many are reflecting on the problem within. Thus what now follows is an account of how we have arrived at an impasse unaided by exterior criticism. A last word is in order before proceeding to detail the results in question. Lest the ordinary economist should regard this as merely the whimsical fantasy of the mathematical economist he should reflect on the fact that what is explained here undermines the major part of the justification of "everyday" economic analysis. Far from being a purely abstract theoretical problem it is one of real significance for practising economists. Some of the latter are well aware of this and I will try to explain precisely why this is the case in my conclusion.

Fundamental but negative results

For the moment let me consider the simplest basic general equilibrium model, that of pure exchange, and let me come back later as to whether that restriction is one which favours the conclusions that I draw.

In the standard exchange model in which there are a finite number ℓ of goods and n of consumers, with remarkably few assumptions on individuals' characteristics, we can prove the existence of an equilibrium. That is we can find, for a given exchange economy $\mathcal{E}$, a price vector p* and an allocation f* to each of the individuals a in the set of agents A such that the excess demand Z(p) function of the economy is zero for every good

i.e. $\quad Z(p^*) = 0.$

Without giving all the standard notation and assumptions let us first note that if an individual a's demand function is expressed by $\phi(a,p)$ the aggregate excess demand Z is given by

$$Z(p) = \sum_{a \in A} \phi \quad (a,p) - \sum_{a \in A} e(a)$$

where e(a) is the initial endowment of individual a.

The student's first reaction to the standard assumptions made on preferences, that they should be given by a continuous, convex, monotone preorder, is that they are too strong to be realistic. He then derives considerable comfort from the fact that these assumptions may be weakened whilst the existence of an equilibrium is preserved. Indeed, in passing, it should be observed that if we are simply interested in existence of equilibria then remarkable results have been obtained for "large economies". Since such large economies are precisely those where perfectly competitive bahaviour makes sense this is reassuring. As an example, the assumption of the convexity of preferences which is vital in small economies, becomes "less important" in large finite economies and is unnecessary in the perfect analogue of pure competition, the continuum economy. Thus for the problem of existence of equilibrium aggregation over individuals helped considerably. Yet paradoxically as the reader will see the problem of aggregation will be the source of woes in what follows. Given the nature of the general equilibrium model it is clear that the existence of such an equilibrium is a first and crucial test of its consistency. Yet once this first test is passed we obviously wish to know more. What restrictions are imposed by the basic "individualistic" assumptions that I have just mentioned? If we are interested in questions of stability with respect to some adjustment process, in the uniqueness of equilibrium, or in comparative statics then one would hope that the assumptions made would restrict the admissible class of economies to those that would have desirable properties of this type.

Put another way, we know that uniqueness and stability results can be obtained by making assumptions on the aggregate excess demand function. The question then is do the individualistic assumptions we make restrict possible aggregate excess demand functions to ones satisfying this sort of assumption?

Now before proceeding it should be said that, in a certain sense, as I have remarked, theory has made considerable progress in adding structure to individual demand functions when they are aggregated. Thus while individuals may have neither continuous demand functions nor correspondences yet if they have sufficiently dispersed preferences, the aggregate demand will be, in general, a smooth[1] function. This confirms an old intuition which was already expressed by Cournot (1838) when he said,

"We will assume that the function F(p), which expressed the law of demand or of the market, is a continuous function ... It might be otherwise if the number of consumers were very limited ... But the wider the market extends, and the more the combinations of needs, of fortunes or even of caprices, are varied among consumers, the closer the function F(p) will come to varying with p in a continuous manner. However little may be the variation of p, there will be some consumers so placed that the slight rise or fall of the article will affect their consumptions..."

This result has been since formalised and a full account may be found in Trockel (1984).

Thus starting from "badly behaved" individuals, we arrive at a situation in which not only is aggregate demand a nice function but by a result of Debreu equilibrium will be "locally unique". Whilst this means that at least there is some hope for real stability, the real question is, can we hope to proceed and obtain global uniqueness and stability?

1. To avoid confusion, I should say here that I mean a C_1 function.

The unfortunate answer is a categorical no! Sonnenschein (1972) gave a fundamental result which, together with his (1973) and Debreu's (1974) results, yield the conclusion that even with strong individualistic assumptions no restrictions other than the following three are imposed on aggregate excess demand functions. Z.

1) $Z(p)$ is continuous for all strictly positive prices, i.e. p in P
2) $Z(p)$ satisfies Walras' law i.e. $p.Z(p) = 0$
3) $Z(p)$ is homogeneous of degree 0 i.e. $Z(\lambda p) = Z(p)$ for $\lambda > 0$.

Thus perversely, even by imposing the most exacting requirements on individuals, we get even less structure than that obtained by aggregating much less well behaved agents behaviour, if their characteristics are dispersed.

When Sonnenschein's first result appeared it was dismissed by some since it said that one could closely approximate arbitrary functions satisfying the above properties by aggregate excess demand functions of economies with "well-behaved" individuals. This was clarified by the later result of Debreu (1974) which can be stated as follows.

Theorem: Given a continuous function $f : p \to R$ satisfying Walras' Law, i.e. $p.f(p) = 0$ for all p in P then for any positive epsilon ε there is an economy $\mathcal{E}$ with consumers with strictly convex monotone preferences such that

$$f(p) = Z_\varepsilon(p) \text{ for all } p \text{ in } \Delta_\varepsilon$$

Here Z_ε is the excess demand of the economy $\mathcal{E}$ and Δ_ε is the price simplex with prices above ε i.e. $\left\{p \mid \sum_1^\ell p_i = 1 \text{ and } P_i \geq \varepsilon \text{ for all } i\right\}$

This result says that any arbitrary continuous function satisfying Walras' Law $f: \Delta_\varepsilon \to R$ concides, for those prices, with the excess demand function of any economy with ℓ well behaved consumers.

Stated as it is here it might seem that any continuous function from positive prices into R^{ℓ} satisfying Walras' law is the aggregate excess demand for some economy $\mathcal{E}$.

This is, as Balasko (1986) says, not true but it is not obvious that the misinterpretation is harmful in some way. In fact aggregate excess demand functions of exchange economies also have "boundary properties", that is, when the price of one good goes to zero average excess demand goes to infinity. To see the problem consider the case of two goods and then normalise prices so that $p_1 + p_2 = 1$ and by Walras' law that $p_1 Z_1(p) + p_2 Z_2(p) = 0$ it is sufficient to consider the graph of $Z_1(p)$. The conditions given earlier together with boundary behaviour imply that the aggregate excess demand for the first good must look something like the one illustrated in figure 1.

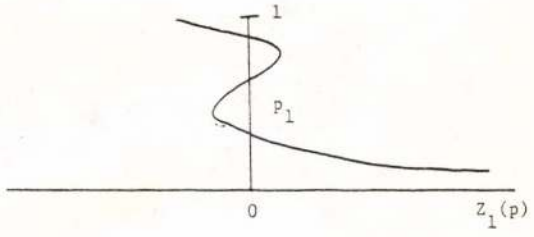

Figure 1

Two things should be noted. Firstly $Z_1(p)$ becomes negative for high p. Secondly, $Z_1(p)$ goes to infinity as p_1 approaches zero. Since $Z_1(p)$ is continuous in this simple case there must be at least one equilibrium i.e. at least one price at which $Z_1(p)$ and hence $Z_2(p)$ are zero. Thus excess demand functions have equilibria and this already means that not "any" continuous function can be an aggregate excess demand function for all prices. Nevertheless if we are prepared to restrict our attention to prices not too close to zero, however small, then any continuous function can indeed be, for that range of prices, an aggregate excess demand function. Think of the function $f_1(p) = 1$ and hence $f_2(p) = -\frac{p_1}{(1-p_1)}$ for $0 \leq p_1 \leq 1$. This is illustrated in figure 2.

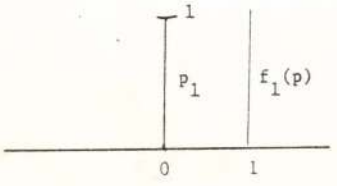

Figure 2

The function f clearly cannot be obtained as an aggregate excess demand function for any economy satisfying the standard assumptions. Nevertheless if we restrict our attention to prices, for example, to ones such that then we can construct an economy having an aggregate excess demand function looking like that in figure 3.

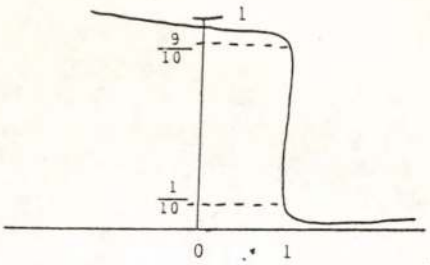

Figure 3

For the prices in question this coincides precisely with that specified above. This can be done for $p_i > \varepsilon$ for however small the ε provided it is positive.

I have belaboured this point since Balasko says "It turns out that many non-mathematically oriented economists have understood the above results as saying that the aggregate excess demand function can be arbitrary besides being continuous and satisfying Walras' Law... this confusion is harmful and that the so-called intuition associated with the above results is incorrect".

Although from a mathematical point of view he is right, the fact that certain functions satisfying our conditions cannot be excess demand functions for all prices is of little interest for the economist. I do not agree therefore that the intuition is incorrect since by adding the boundary condition

we can make the correct statement. Since even with boundary conditions, we have no restrictions which will help to obtain global uniqueness or stability I doubt that there are many economists who would regard this as a buoy of hope cast to them as they struggled in a sea of doubt. In fact the point of this paper is, of course, that most economists are not even concerned over the sea-worthiness of the vessel which they are sailing.

To return now to the basic argument. Mantel (1976) reinforced the negative Debreu and Sonnenschein results by showing that one could in fact make even stronger assumptions on individual characteristics and still not remove the arbitrary nature of the aggregate excess demand function. He showed that one can generate arbitrary functions (with the caveats mentioned above) even if one assumes that all individuals have homothetic preferences, i.e. always spend a fixed proportion of their incomes on each good. This is, of course, a much stronger requirement than those needed for the existence of equilibrium. Thus three standard initial objections to the relevance of these results were removed. The excess demand function could be fitted exactly, and not just approximated over the range of prices considered. The number of consumers needed in the economy was no greater than the number of goods (in these days when the passion for infinite dimensional commodity spaces prevails, the value of this restriction might be questioned) and preferences could be extremely well behaved.

Two further objections remained. Firstly the distribution of endowments might have to be very "odd" and secondly preferences might have to be dispersed in a rather exaggerated way in order to obtain some particular function as an aggregate excess demand function. Indeed Deaton (1975) showed that thoughtful empirical economists do take serious account of theoretical developments by noting the impact of the Debreu, Sonnenschein results on work in applied demand analysis. However he argued, ".... this does not mean that the theory is incapable of generating empirically useful restrictions on patterns of behaviour. Such restrictions we shall always heed from somewhere. Admittedly, the results

of Sonnenschein and of Debreu ... remove the basis for an unqualified belief in such a position, yet the construction of arbitrary demand (sic) functions requires arbitrary manipulation of the income distribution and of preferences, and it is unlikely that the fates manipulated real income with the sole object of frustrating demand analysis".

Unfortunately this is placing too much confidence in the fates, for the income distribution does not have to be manipulated, it can be chosen arbitrarily and furthermore one can generate arbitrary excess demand functions by using individuals all of whom have identical preferences. In other words, taking these two things separately, the income distribution has no impact on the sort of aggregate excess demand function that can be obtained in a finite economy. In fact the income distribution can be chosen at will. The simplest way to see this is to construct for the given function an exchange economy à la Debreu in which each individual i possesses some fraction α_i of the total resources. The (price independent) income distribution is then given by the ℓ numbers α_i each of which gives the proportion of total income owned by individual i.

Now, Debreu's construction only requires a certain minimum amount of goods for each individual, so, provided total endowments are big enough this requirement will be satisfied. Notice further that if the income distribution required is described by more than ℓ numbers α_i no problem is posed since it is always possible to construct an economy with more than the ℓ individuals required by Debreu.

Thus restricting income distribution alone will not help, contrary to the hope expressed by Deaton. However, if we now restrict the dispersion of preferences will this help us to get out of the impasse? That such a restriction may help is clearly what Grandmont (1987) has in mind when he writes "Economic Theory is plagued by quite a few embarrassing results. An obvious example is social choice theory with Arrow's famous impossibility theorem.... No less important is the Debreu-Sonnenschein claim that summation over consumers does not place any other restrictions on competitive aggregate excess demand than Walras' law on arbitrary compact sets of

prices. The principle of a possible solution to the problem has been known for some time but has not yet been implemented much successfully. It is to put restrictions not so much on the support of the distribution of the agents' characteristics but on its shape".

The idea is clear, by restricting the dispersion of preferences i.e. the "size of the support" but also by restricting the way in which individuals are distributed one could hope to obtain restrictions on the aggregate excess demand functions that could be generated.

It is obvious, in fact, that by making this observation Grandmont is leading up to a result. However this result is for an economy with an infinite number of consumers. Indeed the first result linking the distribution of characteristics to restrictions on excess demand functions was given by Hildenbrand (1983a) for an infinite economy. I will come back to the relation between this and the negative results for finite economies and the relation between Hildenbrand's and Grandmont's results a little later but for the moment the following should be clear. For finite economies an arbitrary excess demand function can be generated for "compact price sets", (in terms of the previous discussion those with prices above some positive) by economies with any income distribution and in which all individuals have identical preferences.(1) Thus there is no hope that making the distribution of preferences or income "not too dispersed" or "single peaked" will help us to avoid the fundamental problem.

This easy extension of the Debreu-Sonnenschein result which is the last nail in the coffin of hopes for theoretical restrictions on finite economies can be expressed as follows.

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(1) One word of caution, obviously individuals' incomes cannot be identical, since otherwise the economy would behave as one individual and its excess demand function would satisfy the Weak Axiom of Revealed Preference for example.

Theorem (Kirman-Koch (1986))

Let n be an integer greater or equal to ℓ and v_1 and v_n be n different positive real numbers with $v_1 + \ldots + v_n = 1$.

Let $f:\Delta \to R^\ell$ be continuous and satisfy Walras' Law then for every $\varepsilon > 0$ there exists a continuous monotonic strictly convex preference relation $\succsim$ on R^ℓ and an endowment vector e in R^ℓ such that the aggregate excess demand function of the individuals i = 1 ... n having the preference relation $\succsim$ and the endowment $v_i e$ sum up to f on Δ_ε.

I have spelled out this result in detail so that there can be no ambiguity. Repeating yet again, except for prices smaller than a positive ε, which can be as small as one wants, any continuous function satisfying Walras's Law can be considered as the excess demand of an economy with any large but finite number of individuals having identical preferences. Moreover the individual endowments can be chosen proportional to each other so that any given $v_1 \ldots v_n$ represent the price-independent distribution of relative income.

Escape routes

The question now arises as to how can we still obtain in the context of the general equilibrium model Deaton's "empirically useful restrictions" on excess demand functions? As I have observed, a possible route has been suggested by Hildenbrand (1983a) which is to put restrictions on the shape of the distribution of agents' characteristics, in particular on the income

distribution. What he shows is that the mean demand of a continuum of consumers with identical preferences is strictly monotonic (i.e. obeys the "Law of Demand") if the price independent distribution of wealth has a decreasing density (i.e. if the proportion of individuals in each successive income class is smaller and smaller). Now it would seem that this is in contradiction with the result given above. However, this is not the case since Hildenbrand requires that mean income be finite. In order to generate a sequence of economies having as its limit a continuous economy with the appropriate income distribution per capita income must necessarily be unbounded.

The question that is then posed, is whether the fact that the mean income of individuals becomes arbitrarily large as one tries to approximate an arbitrary income distribution with a large number of individuals is intrinsic. In other words if one imposes some bounds on the income of individuals is there the hope of getting back meaningful restrictions on excess demand functions.(1) Although this is an open question it is a rather small hope on which to reconstruct the scientific basis of general equilibrium theory. It should be noted in passing that restrictions on the distribution of preferences such as those proposed by Grandmont (1987) do not advance our problem for the moment, since they can be shown to be obtained directly from the restrictions on the income distributions imposed by Hildenbrand (1983a), by a simple change of variable, as Grandmont himself points out. This does not necessarily mean however that there are no restrictions on the distribution of preferences which will improve the picture. There is still hope that with rather general conditions on the distribution of preferences we may get back the sort of structure obtained by Hildenbrand but for a less restrictive class of economies. This is because we have more freedom in specifying a distribution of preferences than an income distribution.

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(1) Clearly such bounds can only be given once the function and the set of prices i.e. the ε such that $p_i > \epsilon$ for all commodities are known. Nevertheless even Debreu's construction uses larger quantities of commodities than those strictly necessary.

One possible solution is then through restrictions on the distribution of agents' characteristics, which I have to emphasize again is a clear break with the strictly individualistic tradition.

Another suggestion is that made by Hildenbrand (1983b) which is that the problem of restriction is possibly due to the omission of production in the model considered. He suggests that the pure exchange model is in fact not a suitable basic model for economics even though it has so long been used as one.

Whilst no-one could quarrel with the general truth of this statement there are two objections to the view that this is the root of the problem that we are discussing. Firstly production as it is typically treated in the general equilibrium model can be argued to yield little more than a glorified exchange economy. Indeed old results of Rader (1972) point in this direction. Thus it would be surprising if the introduction or rather consideration of production per se would significantly alter those results obtained in the exchange model. A second and more disconcerting argument is that provided by Kehoe (1985). He suggests that the introduction of production may actually worsen matters in the following sense. Suppose that one is looking for restrictions on economies that yield unique equilibria, then some of those conditions which guarantee uniqueness for pure exchange economies do not do so for production economies. Thus when production is added to an economy, in which there is gross substitutability in demand, the equilibrium is no longer necessarily unique. Thus already in the pure exchange case we cannot restrict the form of the excess demand function through individualistic assumptions, adding production except of a totally unnatural sort (complete reversibility) destroys the power of some of the conditions for uniqueness and stability at the aggregate level. This would seem to imply that introducing production seems to give us more and not fewer degrees of freedom in constructing arbitrary excess demand functions. Worse still, Hildenbrand (1987) has shown recently that if we consider economies in which individuals own pure factors of production from which they derive their income and consume the other goods then almost no economy has a market demand which satisfies the Weak Axiom of Revealed Preference.

All of this might seem unequivocal but, in fact, the addition of production can help. The intuition behind this is that in an exchange economy income is very directly linked to the prices of consumer goods. By introducing production and ownership of factors we can reduce this direct dependence and this, in turn, can push the economy towards one of the conditions guaranteeing uniqueness or stability. In Scarf's famous example instability is crucially dependent on the distribution of initial resources through the income effect of price changes. Introducing production can reduce such dependence.

Thus the question as to whether the introduction of production as normally specified helps is still an open one. Of course it may well be true that a better model of production might make a radical change but this is a different question.

Demand rather than excess demand functions.

The reader will have noticed that all of the remarks so far have been addressed to the properties of excess demand functions rather than to those of demand functions. That the situation is not quite the same in the two cases is easily seen from the following example.

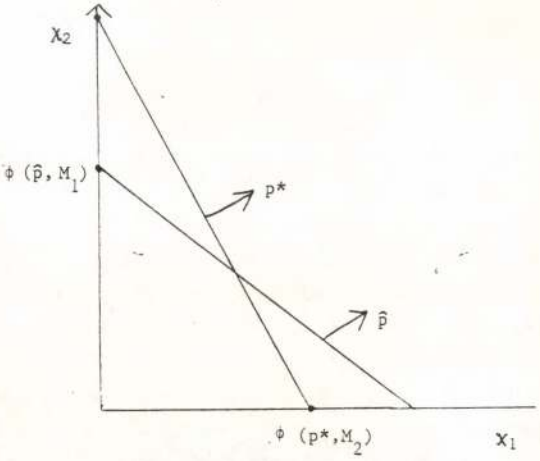

Figure 4

Suppose that we consider an aggregate demand function which takes on the two values illustrated in figure 4. This clearly violates the Weak Axiom of Revealed Preference (W.A.R.P.). Could it be generated by individuals with classic preferences? Obviously not since all consumers at prices p^* consume only good 1 whilst at prices $\hat{p}$ all consumers consume only good 2 thus there must be a violation of the W.A.R.P. at the individual level, which is a contradiction. Thus at least some additional restrictions are imposed on market demand functions. Whether such restrictions are of any real importance is open to question and for a discussion the reader can see for example Shafer and Sonnenschein (1982) but it does not look very likely that the distinction between demand and excess demand will be our saving grace.

The Realism of Assumptions

Without entering into the long methodological debate on this topic it might be argued that the obvious way out of the difficulties presented here would be to examine the realism of the assumptions made in building the general equilibrium model. One view is that it is only the results that can be deduced from assumptions that should be tested and that any assumptions that lead to consequences compatible with reality are acceptable. A less extreme view is that advanced by Pareto that one should look at "man as he is" and should not be afraid of asking whether assumptions are realistic. The problem here is that within the strict context of the Arrow-Debreu model the basic assumptions are not really testable unless additional hypotheses as to separability in time or stationarity in a stochastic context are added. If an individual makes choices once and for all and never has to repeat them in the same circumstances then no contradiction can be observed. This is the same as observing one realisation of a stochastic process, nothing can be deduced. Paradoxically as economic theory has advanced assumptions have been weakened,

the context generalised and hence the possibility of observable contradiction reduced. If individuals are choosing between infinite streams of consumption, their choice can never be observed to be contradictory. Thus the great generality of the structures within which we can prove the existence of equilibrium is, of itself, making the verification of the underlying hypotheses further from reality. If we remember Solow's (1979) remark: "All theory depends on assumptions which are not quite true. That is what makes it theory"; the "not quite" has become less and less meaningful.

Again, Koopmans (1979) said: "In all formal procedures involving statistical testing or estimation, there are explicitly stated but untested hypotheses ... In ... econometric studies ... the "premises" (e.g., profit maximization, maximization of satisfaction) ... play that role. More in general, any statement resulting from such studies retains the form of an "if ... then ... " statement...

The "if ... then ..." statements are similar to those in the formal sciences. They read like logical or mathematical reasoning in the case of economic theory, and like applications of statistical methods in the case of econometric estimations or testing. The heart of substantive economics is what can be learned about the validity of the 'ifs' themselves, including the 'premises' discussed above. 'Thens' contradicted by observation call, as time goes on, for modification of the list of 'ifs' used. Absence of the contradiction gradually conveys survivor status to the 'ifs' in question. So, I do think a certain record of noncontradiction gradually becomes one of tentative confirmation. But the process of confirmation is slow and diffuse."

It seems that as time goes on the "thens" have become impossible to contradict and the "ifs" can no longer be tested against reality.

Relevance

The basic premise of this paper is that the fundamental underpinnings of most modern economic work and indeed of quantitative work is the general equilibrium model. Now there will be many economists who view the latter as a special branch of economics and will be perfectly happy that this branch should have painted itself into a corner. However if one examines carefully the terminology employed in the less theoretical literature one constantly finds reference to "the equilibrium" or "the natural rate" and moreover a discussion as to how long the economy will take to return to the equilibrium. The underlying assumptions of uniqueness and stability are clear, yet as should be clear by now such assumptions have no theoretical justification.

A simple view of the world would be one in which theorists regard their work as self contained and not, at least at present, relevant for empirical work whilst applied economists regard theory as only being, in some loose way, useful as a justification for their work.

Such a simple view is however belied by the protagonists' statements. Theorists are not so detached from reality. Why does Grandmont (1987) describe the results I have mentioned as "embarrassing". Why does Drèze (1987) when talking of the way to incorporate uncertainty into the general equilibrium model say "In that way, general equilibrium theory takes life and acquires substance" if he has not in mind a move in the direction of something more useful for understanding reality?

Perhaps the strongest statement was that of Samuelson (1947) whose "Foundations of Economic Analysis" was subtitled "The Operational Significance of Economic Theory" and who said that one of his purposes in writing the book was to derive "operationally meaningful theorems" from economic theory i.e. theorems which said something "about empirical data which could conceivably be refuted if only under ideal conditions".

On the other side the role of theory in the work of applied economists and econometricians is clearly illustrated by Pagan (1987) who says when describing Hendry's econometric methodology:

"Theory and data continually interplay in this methodology. Unless there are good reasons for believing otherwise, it is normally assumed that theory accepts which variables should enter a relationship and the data is left to determine whether the relationship is static or dynamic (in the sense that once disturbed from equilibrium it takes time to re-establish it".

Thus the conceptual framework within which such econometric models are formulated is precisely that which does not seem to be justified by theory as it stands.

Conclusion

Having come so far in what is clearly a rather negative and provocative exercise it seems only fair to try to identify the source of the problem. I personally do not find the argument that the root of the problem lies in the assumptions made as to the optimising behaviour of agents, nor that we are confined by a mathematical strait jacket which allows us no escape. That the mathematical frameworks that we have used have made the task of changing or at least modifying our paradigm hard, is undeniable but it is difficult to believe that had a clear well-formulated new approach been suggested then we would not have adopted the appropriate mathematical tools.

The problem seems to me to be embodied in what is an essential feature of a centuries-long tradition in economics, that of "methodological individualism". To use Boland's (1982) definition: "Methodological individualism is the view that allows only individuals to be the decision-makers in any explanation of social phenomena".

To base economic theory on such an approach is not to deny any interaction between individuals for as Samuelson (1963) says:

"... individualistic atoms of the rare gas in my balloon are not isolated from the other atoms. Adam Smith, who is almost as well known for his discussion of the division of labor and the resulting efficiency purchased at the price of interdependence, was well aware of that. What he would have stressed was that the contacts between the atoms were organized by the use of markets and prices".

To argue in this way, however, suggests that once the appropriate signals are given, individuals behave in isolation and the result of their behaviour may simply be added together. Then the equilibrium signals can be determined.

It is precisely this denial of any organic content of society as such that seems to me to lead to the lack of conclusions for general equilibrium theory. It is not mere chance that one assumption that leads to strong results on uniqueness and stability is that society should behave as an individual is supposed to in our existing theory. If we are to progress further we may well be forced to theorise in terms of groups who have collectively coherent behaviour. Thus demand and expenditure functions if they are to be set against reality must be defined at some reasonably high level of aggregation. The idea that we should start at the level of the individual is one which we may well have to abandon. There is no more misleading description in modern economics than the so-called micro-foundations of macro-economics which in fact describe the behaviour of the consumption or production sector by the behaviour of one individual or firm. If we aggregate individuals, as I have already explained at length, such a model is unjustified. On the other hand if we do not then we should be honest from the outset and assert simply that by assumption we postulate that each sector of the economy behaves as one individual and not claim any spurious micro-justification. Again,

this attempt to obscure or avoid the aggregation problem is an old one. We only have to think back to the long and heated debate over the significance of the aggregate production function. The question that then might be posed is to explain how and why a sector of society or society itself organizes itself in such a way as to behave like an individual, if indeed it does. Whatever the answer, this seems to be a question which economists have singularly failed to address.

At the risk of being repetitive I would emphasize here that making assumptions on the distribution of agents' characteristics amounts, in some sense, to making assumptions about the organization of society. Thus if we obtain more structure by such assumptions we have to justify them. Thus anyone who makes significant progress in this direction either by examining and explaining the nature of interaction and communication between individuals yielding regularity at the aggregate level or by explaining how interaction may yield restrictions on the evolution of the distribution of agents'characteristics, will have made a radical step forward.

I would like to conclude by emphasizing yet again that the significance of everything that I have said is greatest for the applied economist. Many theoreticians such as Hahn, Hildenbrand and Sonnenschein have already sounded warning notes. These are happy days for theorists since they have shown that by explicitly formalising a model and thus shedding harsh light on assumptions and conclusions the limited structure of that model become clear. Now remains the challenging task of either building a new structure perhaps by moving in the sort of direction I have mentioned. The practising economist is unfortunately less well placed. Probably he will behave just like the bumble bee when somebody proved that it could not fly, it kept on flying since nobody had informed it of this important result. Economists even when warned by theoreticians that current theory has very little to offer in the way of testable propositions will carry on just as if it does. The difference is that the bumble bee does indeed fly whereas the economist may simply be labouring under the illusion that he has something meaningful to say.

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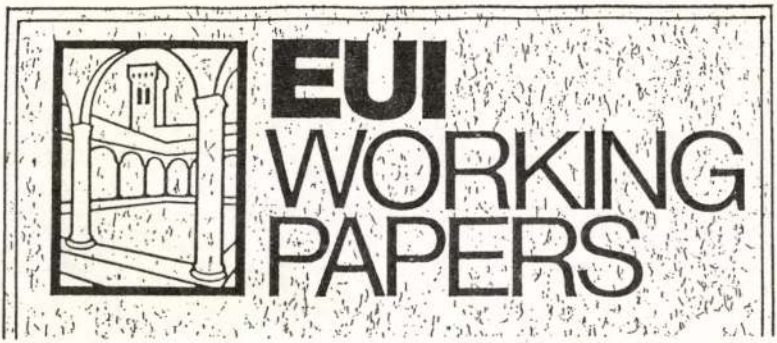

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