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Abstract

What I argue in this paper is that the direction economics ,and particularly theoretical economics, took in the 20th century was to a great extent due to Walras' influence. This was not so much the result of his own results but rather a reflection of his vision. He was convinced that economics should have "sound mathematical foundations" and his concern for this is reflected in his correspondence with his contemporaries such as Poincaré. However, his specific vision of the nature of equilibrium became the benchmark for modern economic theory and led us to the Arrow-Debreu model which is characterised by its lack of institutional features, and the lack of any proof of stability under adjustment, as later to be shown by Sonnenschein, Mantel and Debreu. Above all there is no place in this framework for out of equilibrium dynamics. Whilst Walras is to be lauded for his insistence on the interdepence of markets, we should also be aware that he set us on a path towards economic models which, while admirably internally consistent, seem to be unable to match the empirical evidence. I fear that Walras would not have been unhappy with this outcome.

« So far as pure theory is concerned, Walras is in my opinion the greatest of all economists. This system of economic equilibrium, uniting, as it does, the quality of « revolutionary » creativeness with the quality of classic synthesis, is the only work by an economist that will stand comparison with the achievements of theoretical physics. Compared with it, most of the theoretical writings of that period – and beyond – however valuable in themselves and however original subjectively, look like boats beside a liner, like inadequate attempts to catch some particular aspect of Walrasian truth. It is the outstanding landmark on the road that economics travels towards the status of a rigorous or exact science and, though outmoded by now, still stands at the back of much of the best theoretical work of our time » *Schumpeter (1954), p.827*

This testimony could not be clearer, but, the purpose of the paper is to suggest that the road which, as Schumpeter explains, Walras set us on, has led us into an impasse. To what extent is Walras responsible for that and to what extent does the responsibility lie with those who have interpreted him ? These are the questions I will examine in this short contribution. Many would argue that the Walrasian route culminated in the work by Gerard Debreu (1959) and the Arrow-Debreu model of General Equilibrium. Indeed, Werner Hildenbrand wrote in his introduction to Debreu's collected papers (Debreu (1981), that while Walras was the architect of the General Equilibrium edifice, Debreu was its Master Builder. Master Builders follow the architect's plans for a building or, at least, their perception of those plans, so it is not unreasonable to see the Arrow-Debreu model as the direct inheritor of the Walrasian vision. But the obvious question here is as to whether that model coincides with Walras' own aims. I suggest that there are two parallel interpretations of what Walras had in mind, which are discussed by many economists but that neither of these provides us with a way forward in economic theory. The main thrust of my argument is that neither of the possible interpretations is logically consistent and that Walras himself did not have a clear vision as to which to choose and for various reasons switched between the two. In addition, he was enamoured of the idea of mathematical economics and this exarcebated the contradictions in his work and led us to the unsatisfactory state of economic theory today.

To develop my argument, the following questions have to be answered. What are the ingredients that characterise the « Walrasian » General Equilibrium model and what was Walras' own position with respect to them ? Backhouse and Maks (2006) suggest the following caricature of what they describe as « the common university trained scholar's » understanding of the Walrasian model,

« Walras developed the general economic equilibrium model, but did not care about uniqueness and stability of an equilibrium. It is a model with exchange and production only and it assumes

an auctioneer who announces price vectors to establish the equilibrium. » *Backhouse and Maks (2006) p.1*

To this highly restricted vision should be added the underlying characteristics of the model, perfect competition, the nature of equilibrium as a market clearing set of prices, and some notion of perfect information. In addition, one cannot actually leave to one side the problem of stability or how one actually gets to equilibrium and Walras devoted a great deal of time to the discussion of this issue and the nature of Walras' ideas on the subject has been the subject of an intense debate, (see Walker (1996, 1999) and De Vroey (1999) for example). This is important since what is at stake is whether Walras had a vision of the economy as a genuinely dynamic process or one which somehow, almost by assumption, arrived at, what he had in mind as, a static equilibrium. The vision of equilibrium as a state rather than as a manifestation of a dynamic process is certainly that which is characteristic of the Arrow-Debreu model. It is also the one which despite, the misleading name used for modern macroeconomic models, Dynamic Stochastic General Equilibrium, underlies them. Before proceeding to my main argument it is worth having a look at the basic features of the « Walrasian model ».

Perfect or free competition and perfect information.

If there is one thing that we owe to Donald Walker (1996), it is to insist on the fact that Walras never saw the market or economy as one in which there was free competition, in the sense that all the participants were price takers. He argues, and even his most hostile opponents agree that the agents in Walras' models could not have been viewed as « price takers » since nothing was specified about whom they were supposed to be taking prices from. Rather he argues, Walras had in mind a notion of free, or perfect competition as corresponding to a situation in which large numbers of individuals interact with each other freely but they are not passive price takers since they themselves quote the prices at which they are prepared to buy or sell. Walras, also saw pure or perfect competition as an ideal rather than a reality. Walker quotes Walras, in a letter to Von Bortkiewitz, as saying,

« Free competition is the principle mode of exchange in the real economy, practiced on all markets with more or less precision and therefore with less or more efficiency »,

Letter from Walras to Von Bortkiewitz, 1891 letter 999 in Jaffé (1965).

However, as is clear from this citation, Walras did believe in perfect competition as the bench mark. Thus his position was directly at odds with Marshall who said,

« It may be well to insist again that we do not assume that competition is perfect. Perfect competition requires a perfect knowledge of the state of the market; and though no great departure from the actual facts of life is involved in assuming this knowledge on the part of dealers when we are considering the course of business in Lombard Street, the Stock Exchange, or in a wholesale Product Market: it would be an altogether unreasonable assumption to make when we are examining the causes that govern the supply of labour in any of the lower grades of industry ».
Marshall (1920): pp. 540–1

In the case of Walras, Walker argues that it is free competition that allows equilibrium to be approached, if not reached, by a process of tatonnement or groping by the individuals in the market. However, he also argues that perfect competition was not consistently assumed by Walras and should, if anything, be attributed to Cournot who did make such an assumption explicitly. If we take this position then, when economists refer to the necessity of some such notion as perfect competition to ensure the existence of Walrasian equilibrium, they are referring to the logical and analytical conditions for existence and not to what Walras himself explicitly stated.

Thus, there is in this vision no central price mechanism, no auctioneer and markets self organise through the actions of the individuals and this view is, in a sense, shared by Marshall. This is in stark contrast to Michel De Vroey's assertion for, while admitting that the auctioneer does not explicitly figure in Walras' work, he says,

« … the auctioneer hypothesis is part and parcel of Walrasian theory. Without it, the latter would lack any scenario about the institutional dimension of its price formation mechanism. » De Vroey (2003) p. 466

Thus, unlike the « Walrasian » tradition that grew up later, and I will come back to this, in Walras' own work, there was no figure or mechanism which centrally generated prices which were then announced to market participants. As I have said, price taking behaviour would not make sense in this context. What Walras suggests is that the market participants made and changed their offers of prices at which to buy or sell and the corresponding quantities, and that none of them saw themselves as having an impact on the overall prices that prevailed. It was this

last feature that corresponds to his idea of free competition, for nobody was able to influence the level of prices nor would they behave as if they could. Thus free competition was assimilated to the idea that there were many individuals each of whom had a negligible weight in determining the market outcome. The conditions under which this was logically consistent were only to be made precise by Aumann (1964) many years later.

Yet, this already begins to pose problems. What Walras argues is that somehow after the open outcry, prices will settle to an equilibrium in which the price or prices that prevail in the market or markets[1], are just such that the total quantities demanded are just equal to the total quantities supplied. This raises two questions. Firstly how do agents know when to stop calling out prices and to trade ? Walras seems to suggest, that, by looking around, they would be able to see or hear the opportunities available and to trade accordingly.[2] Yet, if there were enough traders to make the assumption of free competition plausible, then checking who had an excess demand or supply and how much was involved, would be a daunting task. In particular this is not a simple binary matching problem. Some traders may need to meet several other traders to satisfy their needs. The information required to know that total demand is equal to total supply for each good, is also more than is normally assumed in the Walrasian context, (see De Vroey (2003). How then can one explain the differences between what one finds in Walras' own writings and the position taken by scholars such as De Vroey ?. My own view, is that De Vroey is right to argue, for the reasons that I have just given, that some sort of auctioneer or central authority is logically necessary to remove the inherent contradictions in Walras. As he says,

« To conclude, strictly speaking, 'perfect information *à la* Walras' is not to be found in Walras' *Elements* – first, because he was unaware of the importance of the information dimension and, second, because his treatment of expectations was too rudimentary. However, it becomes part of Walrasian theory as soon as it is accepted that the auctioneer hypothesis is its proper trade technology assumption. Still, its full presence had to await the emergence of the Arrow–Debreu model and its 'Arrow–Debreu commodity' concept, i.e., the claim that a commodity ought to be defined not only in terms of its physical characteristics but also of its delivery date and of the state of nature contingent on which the exchange is to occur. Therefore, the model where perfect information *à la* Walras comes out most clearly is the Arrow–Debreu model ».

[1] There is a debate between Walker (1999) and De Vroey (1999) as to whether Walras had in mind one market on which all goods are exchanged, which is the view of De Vroey or that markets for each good were considered as separate, the view of Walker. Both authors point out passages which are not consistent with their view but regard the exceptions as minor. At the risk of attracting the ire of the unconditional admirers of Walras, I have the impression that Walras was neither clear nor consistent on this point.

[2] Walras discussed out of equilibrium trades but, in the end, settled for the idea that no trade took place before equilibrium was reached.

Michel De Vroey, (2003) p.469

Therefore De Vroey goes on to argue that we can safely assume that there is such a figure and that it is consistent with what Walras had in mind. Walker's (1996) position is simpler. He argues that whether or not one needs an auctioneer to make the theory coherent, this is not what Walras said. The answer, it seems to me, although not one that will sit comfortably in a book to celebrate Walras, is that Walras' position was not logically coherent. In this I am not alone, since Bridel (1997) argues that Walras found himself in the uncomfortable position of having two inconsistent views, but when pushed he inevitably chose mathematical consistencey over realism. Thus while on the one hand by his references to the Bourse etc, he wished to attach his work to the real world, on the other hand, he wished to construct a mathematically coherent framework. Walker (1996) is convinced that he favoured realism over mathematical consistency, whilst De Vroey makes the opposite argument. I think it is easier to accept the lack of consistency in Walras' work rather than to make the necessary modifications to make it consistent. Thus, the fact that De Vroey shows us that the auctioneer is somehow implied by Walras' analysis is not reason enough to argue that Walras had such a figure in mind. He may well simply not have understood completely the logical difficulties in his exposition of his theory.

Nonetheless, whilst Walras' own position was ambiguous, what we have inherited as the Walrasian legacy, is the idea of a market in which the agents are passive and accept prices as given. Now, suppose that we accept the position that many attribute to Walras, that which De Vroey supports, and involves, at least implicitly the auctioneer who completes the model by calling out the prices. In this context, the first question becomes, are there prices which, if announced by the auctioneer would clear the market or markets ?

This brings me to my second theme which is the role of equilibrium in Walras' work.

Equilibrium

As I have observed, Walrasian equilibrium as understood in Walras' own work, could either be thought of as a rest point of a dynamic process, or as a static solution to the problem of finding a price vector at which all markets would clear. However, by the time we get to Arrow and Debreu, the choice has been made and equilibrium has become a solution of a set of simultaneous equations. This is not the fault of those authors. For, as Hicks already remarked, disparagingly,

"To some people (including no doubt Walras himself) the system of simultaneous equations determining a whole price-system seems to have vast significance. They derive intense satisfaction from the contemplation of such a system of subtly interrelated prices; and the further the analysis can be carried (in fact it can be carried a good way)...the better they are pleased, and the profounder the insight into the working of a competitive economic system they feel they get."

John Hicks, Value and Capital, 1939: p.60

But the drive towards finding a solution to the set of equations which was, in large part solved by Wald (1932), was in the spirit of the Bourbaki tradition and it is not surprising that it should have been Debreu who pushed Walrasian theory in this direction. He was following on from Maurice Allais, his mentor who was clearly oriented in the direction of mathematical purity rather than realism. He said,

"The fundamental Anglo-Saxon quality is satisfaction with the accumulation of facts. The need for clarity, for logical coherence and for synthesis is, for an Anglo-Saxon, only a minor need, if it is a need at all. For a Latin, and particularly a Frenchman, it is exactly the opposite."

Maurice Allais, Traité d'Economie Pure, 1952: p.58

But, if it was the static solution that many authors claim that Walras had in mind, he was not always consistent in that view. For example, as I have mentioned, many authors have pointed out that he often speaks about how prices adjust to equilibrium through the actions of individuals and, furthermore, he argues that prices will never converge since there will always be changes in and shocks to the system which will mean that it is continually adjusting. For example, he made the well known remark, in the Elements that the market is:

"like a lake stirred by the wind, in which the water continually seeks its equilibrium without ever achieving it".

Walras (1877) P.310

This is consistent with the view of Dillmann and Frambach (2006) who describe the stages of development of Walras's pure theory of free competition, but emphasise his opinion that

equilibrium is an ideal and not a real state, a state towards which things tend under a régime of free competition. A state that will never be reached, because, everything that is assumed constant in the beginning of the equilibrating process will change and the process will start all over again.

Yet, elsewhere we find distinguished economists who give explanations which seem to suggest that Walras was preoccupied by the solution of the excess demand equations in the economy and that he thought of the tatonnement process merely as a mechanistic way of finding such a solution. This is the view of Walras' work which has been adopted by Hahn, who suggests that Walras was just giving a verbal account of the Gauss-Seidel method for solving equations. In fact it is easy to understand why one should be led to Hahn's (1982) point of view. For example, as Walker himself points out, Walras in a letter to Von Bortkiewicz in 1889 says that :

"operations of the raising and lowering of prices, of the increases and decreases of the quantities of products produced, etc., on the markets are nothing other than the solution by tatonnement of the equations of exchange, of production, and of capital formation. The object and goal of economic theory consists above all and before all in the demonstration to which I am referring".

L. Walras, letter to Von Bortkiewitz, letter 868 in Jaffé (1965)

We seem then to be faced with two different and inconsistent views of equilibrium, one which is simply the solution of a system of static equations and the other which is the limit point of a dynamic process. The role of time is important here also. For, if the process of adjustment to equilibrium is one which happens in the real economy, it must take time, in particular when the production of goods is involved. Alternatively, one could think of the process as a virtual one which happens instantaneously and does not take any time at all. This is discussed by Donzelli (2009) who says,

« Among the epistemological inconsistencies, the most evident is revealed by the dualistic character of the Walrasian theoretical system: in fact, in the versions of this system developed in the second (and third) edition of the *Eléments*, one can find, side by side, two alternative interpretations of the *tâtonnement*, the virtual and the effective; these two kinds of processes evolve in two distinct time sets, the "logical" and the "real", lead to alternative notions of equilibrium, the "instantaneous" and the "stationary", and finally pertain to different models, the pure-exchange model, on the one hand, and the models with production, on the other. »

Donzelli F (2009) p.15

As Donzelli rightly points out, it is production which makes the Walrasian concept of adjustment in real time implausible. The conflict over the interpretation of the Walrasian price adjustment process is apparent in the acrimonious debate that Walras had with Edgeworth, often through Von Bortkiewitz, as intermediary. Edgeworth was convinced that Walras had described a system which was not functional and he concluded that what he was describing was a virtual adjustment and he said

« Eh bien, le mode de résolution des équations d'équilibre, étudié par M. Walras, est absolument conforme à l'idée que Jevons c'est faite de la nature de ces équations. Quant au problème de l'échange, M. Walras l'envisage au point de vue purement statique, en ce sens qu'il suppose les quantités possédées de produits comme étant des quantités constantes, et les courbes de rareté comme ne variant pas; ces suppositions, il les maintient en traitant la question de la résolution des équations de l'échange par la hausse et la baisse des prix. »
Von Bortkiewicz, (1890), p. 359

Edgeworth was evidently convinced that the process described by Walras was virtual since there was none of the « higgling » that he described in his recontracting process and also the quantites to be traded and the preferences were not modified while the process was taking place. In fact it is also true that in *Mathematical Psychics,* the nature and timing of the bargaining process are not discussed in detail. Indeed Edgeworth himself admitted as much ten years later in his reply to Von Bortkiewicz, at least as far as the "time" factor is concerned (Jaffé (1965). However, Edgeworth came to be convinced that the stability problem did not have a general solution and that, at best, a solution could be found for particular institutional arrangements. He discussed various forms of auctions for example. When he expressed these views Walras reacted angrily, for Edgeworth he felt, was convinced that,

« I am engaged in absolutely useless exercises in my efforts to demonstrate that the operations of the raising and lowering of prices, of the increases and decreases of the quantities of products produced, etc on the markets are nothing other than the solution by tatonnement of the equations of exchange, of production and of capital formation », *Letter no. 927 to Von Bortkiewicz, in Jaffe (1965).*

He objected particularly strongly to the idea that adjustment processes could only be applicable in specific institutions. He seemed to assimilate the notion of « free competition » to the tatonnement process and once again he complained to Von Bortkewicz,

« I take the almost universal regime of free competition in regard to exchange, that which was described by John Stuart Mill, and which consists in raising the price in the case of the quantity demanded exceeding the quantity supplied and lowering it in the case of the quantity supplied exceeding the quantity demanded, and I demonstrate that the process leads to equilibrium by establishing the equality fo the quantities supplied and demanded. Whereupon there is thrown at my head the market for English public debt, the system of English auctions, the system of Dutch auctions etc., etc, »
Letter no. 999 to Von Bortkiewicz, in Jaffe (1965).[3]

The interesting thing about the disagreement between Walras and Edgeworth is that the latter's opinion has been redeemed by the much later results of Sonnenschein (1974), Mantel (1974) and Debreu (1976),(which I will refer to as SMD), to which I will come back. SMD showed that it is not possible to show the convergence of an adjustment process to an equilibrium and thus, in a sense, Walras' efforts were in vain. Walras, could not have known this himself, and became more assertive in his claims. For example he wrote to Hermann Laurent,

« I have thus completely finished static economics, that is to say I have completely solved the problem that consists in using as a point of departure given utilities and given quantities possessed of all the species of wealth by a certain number of exchangers, to establish rationally a complete equilibrium of economc society at a given moment », *Letter no.1396. In Jaffe (1965)*

It is clear that whenever Walras found his ideas under attack or even just criticised, he turned to his mathematical contacts and tried to recruit their support, frequently brushing aside their reservations.

We are thus, still left with considerable ambiguity as to Walras' vision of equilibrium. The modern literature has tended to accept equilibrium as a state which is a solution of a set of equations. The result has been to consider the adjustment process as separate from the existence problem. Here the confusion is heightened by something which I have already mentioned, the almost universal attribution to Walras, of the auctioneer who determines the prices at each stage. I think, in particular, that the modern tendency to refer to the Walrasian auctioneer is a convenient fiction to explain convergence to market prices, rather than a description of what Walras himself had in mind when he described the tatonnement process. At the risk of being repetitive, I think that the auctioneer has appeared as an ex post way of making Walras' position

[3] I have drawn on Walker's (1996) very complete discussion of the debate between Edgeworth and Walras for this section.

more consistent and this would be consistent with De Vroey's position. But, Walras did, as Walker (1996) is at pains to point out, take the dynamic process seriously and as everybody now admits did not explicitly envisage an auctioneer. Furthermore, he clearly talked about « open outcry » or prices being « cried out » and it is undoubtedly this that has led subsequent authors into confusion. For, the only modern equivalent to what Walras describes, would be the double auction in which an auctioneer is not present. Ingrao and Israel (1990) who are usually very meticulous in their analysis and attributions state that equilibrium is achieved on a general auction market and actually talk of the auctioneer adjusting the gaps between supply and demand. However they do assert that Walras' tatonnement was an attempt to describe the working of a market and not simply a fiction to describe an algorithm for obtaining equilibrium.

In the end, I have to come down on the side of Bridel (1998) when he says,

« Torn from the beginning between the internal coherence of a static mathematical model based on a set of equilibrium equations and the paucity of its interpretative results, Walras became the victim of his relentless search for the former from which any claim of describing a price formation process was eventually excluded » *Bridel (1998) p.233*

This impression seems to me to be reinforced by the exclusion of false trades in Walras' last models, which Walker (1996) attributes to his failing health but may also have been a reaction to Bertrand's (1883) attack on this point.

Stability and Information

If we accept that Walras was concerned with the actual adjustment of the economy to equilibrium then we have to examine what happened to that quest. As Hicks (1939) said when discussing how to deal with how several markets might arrive at equilbrium together,

"When looking for such a technique, we are naturally impelled to turn to the works of those writers who have specially studied such interrelations – that it to say the economists of the Lausanne school, Walras and Pareto, to whom, I think, Wicksell should be added. The method of General Equilibrium, which these writers elaborated, was specially designed to exhibit the economic system as a whole, in the form of a complex pattern of interrelations of markets. Our own work is bound to be in that tradition, and to be a continuation of theirs", *Hicks* (1939), p.2

The distinction between a dynamic adjustment process to equilibrium and the economy at static equilibrium is at the heart of the problem and curiously the role of the information needed in these two cases is enlightening. One of the major achievements of the modern version of Walrasian General Equilibrium theory has been to demonstrate that the competitive mechanism is remarkably parsimonious in terms of the amount of information that it uses, and this is consistent with the view that De Vroey takes of the role of information in Walras' work.

Let us look at the Arrow Debreu world, the static view and the modern consensus view of Walras. Here, the standard theorem, due, in its most general form, to Jordan (1982) says that the competitive mechanism needs a "message space" with dimension $n(l-1)$, where n is the number of agents and l the number of goods. This is quite remarkable. To see why it is so consider a simple exchange economy. At equilibrium, every agent except one needs to transmit his vector of excess demands for $l-1$ goods because of Walras' Law, and thanks to the homogeneity of degree 0 of excess demand functions, a vector of $l-1$ prices is needed. This result seems, at first sight, to reinforce the efficiency of the competitive market mechanism, since one can show that no other mechanism which achieves Pareto efficiency uses less information.
Thus the Walrasian mechanism allows an enormous economy of information. Surely such results should strengthen the position of those who defend General Equilibrium analysis and the idea that our Walrasian legacy has been very positive? The answer is negative and to see why, one has to recall that so little information is needed for the economy to function *at equilibrium*. What interests us is not only how informationally demanding the mechanism is at equilibrium but also how much information it requires to get there. Even if we accept the static view of Walrasian equilibrium, unless we are to be satisfied with efficiency and existence we have to examine this question. Thus, whether or not it was Walras' intention we cannot avoid the basic problem of stability. Equilibria are only of interest if they can be attained through a reasonable adjustment process. As Morishima (1984) said,

« 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 realise an equilibrium solution, then the equilibrium solution will amount to no more than a utopian state of affairs which bear no relation whatsoever to the real economy ». Morishima (1984) pp. 68-69

It is often claimed that the Walrasian "tatonnement" process is such a « reasonable » process,

since, as formalised it corresponds to what one might think of as an intuitively plausible mechanism. Raise the prices of goods in excess demand, and lower those of goods in excess supply exactly as Walras himself suggested. If we take the modern view this is a separate consideration from existence might seem reasonable as the second part of a two stage reasoning process. First establish that equilibrium states exist and then check to see whether they are stable. Yet, as we know from the SMD, the equilibria of economies are not necessarily stable with respect to this process. One might then object that, since we have spent so much time refining other aspects of the Walrasian analysis perhaps we should do the same for the adjustment process that we inherited from Walras. In other words, the problem lies with the adjustment process rather than with the General Equilibrium model. If a more general adjustment rule were to be specified, perhaps the equilibria of the economy could be shown to be stable.

Yet, what became immediately clear after the innovative work of Smale (1976) was that stability could only be achieved at the price of a significant increase in the amount of information needed. Smale's Global Newton Method is an extension of standard methods which allow one to find a fixed point of a mapping, such as an aggregate excess demand function, if one starts sufficiently near the boundary of definition[4]. It has two major drawbacks. Firstly, it does not behave well in the interior of the domain which, in the case under consideration, is the space of all strictly positive prices. Secondly, as already mentioned, it uses a great deal of information. What is needed is a knowledge of all the partial derivatives of the aggregate excess demand functions and this increases the size of the message space without guaranteeing convergence from any arbitrary starting point. An additional problem is with the economic content of the process. While the original tatonnement process has a very natural interpretation this is not the case for the Newton Methods, despite the efforts of Varian (1977).

Is the informational content of the Newton Method a necessary evil? Saari and Simon (1978) asked the following question. Can one find what they called "Locally Effective Price Mechanisms," that is, ones which turn all economic equilibria into sinks, which use less information than the Newton Methods? They proved, unfortunately, that this cannot be done.

[4] By this we mean starting from an initial price vector where some of the prices are close to zero.

One might have hope that we had simply made the wrong choice of process, since the Generalised Newton Method has the undesirable property that it reduces excess demands monotonically and one might have hoped that, by relaxing this one could have found less informationally demanding mechanisms. Unfortunately Saari and Simon showed that any process which would lead to equilibrium from any starting price vector would use an infinite amount of information. Many ingenious attempts have been made to construct adjustment mechanisms which would get around this.

However, as Jordan (1986) pointed out, all the alternative adjustment processes that had been constructed, when he wrote, had no economic interpretation. Since he wrote there have been many efforts to construct globally and universally stable price adjustment processes and, in a certain sense, Kamiya (1990), Flaschel (1991) and Herings (1995) succeeded. Yet if one looks closely at these results there is always some feature which is open to objection. In Kamiya's case the excess demand function is artificially defined outside the original price domain. In Flaschel's case the adjustment process depends on a parameter which varies with the economy and indeed, he says that it is too much to hope that one would find a process that would work for all economies. Hering's mechanism has the curious feature that prices are adjusted according to the relation between current price and the starting price.

All of this seems, to me at least, to suggest that there is no hope of finding an economically interpretable adjustment process which will converge from any price vector independent of the economy. Had we been able to do so, this would have rehabilitated Walras' idea of the economy moving towards equilibrium even if it took an arbitrarily long time to reach it. Unfortunately, the Saari and Simon result showe that we had ended up in an impasse. Where does all this leave us? The informational requirements of adjustment processes seem to be so extreme that only economy specific processes could possibly ensure convergence. This is hardly reassuring for those who argue for the plausibility of the equilibrium notion. Any change in the parameters of an economy would entail a change in the price adjustment mechanism that would keep the economy stable. Alternatively, one could argue that economies are not, in general, stable in this sense. If one accepts this point of view then one has to focus on the disequilibrium dynamics of economies as a certain number of authors have done. If we do this then we leave the Walrasian path and have to accept that his legacy was not as rich as we thought.

Some conceptual problems

All of this would suggest that the real issue is deeper and the difficulties with stability that I have outlined are symptomatic of conceptual problems. Almost all of the literature to which I have referred considers the evolution of a price vector, the dimension of which corresponds to the number of commodities, over time. Yet, in the Arrow-Debreu model as in the purest Walrasian position, such time is undefined. In other words, since all commodities are dated in the model, the goods for which prices are quoted change during the adjustment if it takes time. Adjustment must therefore, be instantaneous or virtual and this problem, as we have seen, was raised explicitly by Edgeworth. Yet, this well known problem simply reflects the unrealistic nature of the basic Walrasian model. For, whether time is real or virtual, we come back to the obvious questions, what are prices at any moment in time and who sets them?

In almost any market one can think of, there is no single vector of prices of commodities at any one time. Different prices coexist for the same commodity even if one takes absolutely literally the definition of a commodity in Debreu's *Theory of Value* (1959).[5] In most markets prices are set by individuals and the latter modify them according to what is happening on the market and indeed, this view was often espoused by Walras when he was adopting his realistic position. Rare are the markets in which some central clearing mechanism exists which matches demand and supply. Consider financial markets which function either on a double auction or on an order book basis. At any point in time a bid is matched with an offer and a transaction takes place. The price at which this transaction takes place is, in no sense, an equilibrium price. All those who might have wished to transact at this price would not necessarily have been satisfied, yet it is this price that appears in the high frequency stock price series which are more and more frequently used. What happens at the next point in time? The other traders observe the price and the transaction and now decide on what to bid or to offer. Thus new bids and offers are made which may replace those which were made in the previous period. What is important here, is that the trades taking place at each point in time, reveal information and this is not taken into account in

[5] Indeed, the 2010 Nobel prize in economics was given to three economists who worked on the problem of searching and matching in markets with a distribution of prices and wages.

the basic Walrasian model. Traders do not, as Walras suggests simply walk around observing transactions but reflect on such questions as why did that trader purchase that share at a price which I was not willing to pay? Perhaps he had information that I do not have? Such inferences may have important consequences for the evolution of prices. Where is the difference with the standard model that we have inherited from Walras?

Firstly, transactions take place sequentially and at each point agents observe what other agents are doing or at least the results of what they are doing. This is a major step away from the world envisaged by Walras, since by the time he had finally built his last model, as I have said, he stipulated that no trade would take place out of equilibrium. Yet, not only does trade take place out of equilibrium, it is precisely because this happens that information is transmitted into prices. The very essence of the sort of trading that takes place in many markets is that individuals are continually obtaining information and this influences their subsequent expectations and conduct. It is interesting to note that contemporaneously with Walras, Bachelier (1900) was developing his random walk model of the stock market. However, Poincaré who wrote the report on Bachelier's thesis was not happy with it as a description of the real world for precisely the reasons that I have mentioned. What he objected to was the assumption that people act indiependently on the basis of their information and are not influenced by others. He explained that he thought that there is a natural trait in human nature which is to herd, like Panurge's sheep and this, he said undermined the mathematical structure that Bachelier developed. However, his warning went unheeded and much of modern financial theory is still based on Bachelier's original assumptions. Thus, there is a curious parallel between the reaction of economic theory to Poincaré's criticisms of Walras and that of financial theory to his comments.

What Poincaré was insisting upon was that there is interaction among the agents other than through a price system which everyone accepts as given. Such interaction is an essential feature of the real economy but is essentially absent from the modern versions of the Walrasian economic model, and this is particularly true for most standard macroeconomic models. As I have suggested, the failure to take direct interaction between agents into account is partly due to the basic « pure » Walrasian interpretation in modern economic theory, namely that individuals take decisions in isolation, using only the information received through some general market

signals such as prices. Communication and interaction other than through the price system are ignored altogether. Yet, while coordination through prices is undoubtedly important, can we afford to ignore the influence of other coordination and interaction mechanisms? Unfortunately, whilst, as Walker (1996) points out, Walras did consider and discuss the meeting of participants and the exchange of information, he never gave a formal specification of how this happened and this must, at least in part, explain how this part of his work has largely been forgotten.

The difficulties with formulating a model of how market participants interact and take decisions, can be sidestepped, as is frequently done in macroeconomics, by assuming that a sector of the economy can be described by a "representative individual." This idea of representing a society by one exemplar denies the fact that the organisational features of the economy play a crucial role in explaining what happens at the aggregate level. The way in which markets are organised is assumed to have no influence on aggregate outcomes. Thus aggregate behaviour, unlike that of biological or physical systems, can be reduced to that of a glorified individual. Such an idea has, as a corollary, the notion that collective and individual rationality are similar. This is the antithesis of the way that we should have taken and in this we have strayed far from the original vision of Walras.

The end of the theoretical road

I have spent some time looking at the different interpretations of Walras' contributions. Was he describing a realistic process of price adjustment or was he just seeking a solution to his system and using the adjustment process he described as an argument to show that the prices would converge to an equilibrium ? If he was just trying to show the existence of equilibrium we would then be left with the questions as to the uniqueness and stability of such an equilibrium. Now the most important question is, from the point of view of economic theory, rather than that of the history of thought, does it matter ? When we tried to formalise Walras' description of his tatonnement adjustment process rather than looking at the abstract existence problem did this help? Not really, because the real problem is that the SMD results to which I have already alluded reveal the basic difficulty. What they show is that, the conditions, which are known,

which guarantee the stability of this process cannot be obtained from assumptions on the behaviour of the individuals. To be absolutely clear, what showed is that there is no hope of a general result for stability since the *only* conditions on the aggregate excess demand function that can be derived from even the strongest form of the assumptions on individual preferences are the well known four:

- Continuity,
- Walras' Law,
- Homogeneity of degree zero
- Boundary conditions which guarantee that aggregate excess demand "explodes" if any price goes to zero.

Since there are functions satisfying these conditions which are not stable with respect to the tatonnement process, and which do not have a unique equilibrium these properties cannot be guaranteed by the assumptions on individual characteristics.

The full force of the SMD results is often not appreciated. As I have argued, without stability or uniqueness the intrinsic interest of economic analysis based on the General Equilibrium model is extremely limited. Morishima's observation about stability is well taken, but for macroeconomists uniqueness is also important. "Comparative statics" in which one makes comparisons between equilibrium and another one, which results from a change in the parameters of the first, makes no sense in the presence of multiple equilbria. Here it is worth mentioning what Schumpeter had to say on this subject. He was convinced that Walras had not only been able to show the existence of an equilibrium but also its uniqueness. He said,

« The first and foremost task of economic analysis is to explore the properties of that system… What we want to learn before anything else is whether or not the relations known to subsist between the elements of the system are, together with the data, sufficient to determine these elements, prices and quantities, uniquely. For our system is logically self contained only if this is the case : we can be sure that we understand the nature of economic phenomena only if it is possible to deduce prices and quantities from the data by means of those relations and to prove that no other set of prices and physical quantities is compatible with both the data and the relations. The proof that this is so is the magna charta of economic theory as an autonomous science, assuring that its subject matter is a cosmos and not a chaos. »
Schumpeter (1939) p.41

Here what Schumpeter seems to be saying is that without uniqueness, the system has little interest. Had he been aware of the SMD results one might ask whether his testimony to Walras,

cited at the outset, would have been so enthusiastic. As I have mentioned, the usual way out of this problem is to assume a “representative agent” and this obviously generates a unique equilibrium. However the assumption of such an individual is open to well known criticisms, (see Kirman (1992) and Stoker(1995)) and, recourse to him raises one of the basic problems of the route to the place where General Equilibrium found itself. This is the problem of aggregation. In fact, we know that, in general, there is no simple relation between individual and aggregate behaviour, and to assume that behaviour at one level can be assimilated to that at the other is simply erroneous, and this has been well known for some time. Walras never, as far as I am aware, treated the behaviour of the aggregate economy as that of an optimising individual.

At various points along the Walrasian road there have been attempts to resolve the sort of problem which emerges from the model and its offspring. It is worth mentioning such an attempt to restore structure and hence, to overcome the difficulties produced by the fact that uniquess and stability of equilibria cannot be guaranteed in the basic Walrasian model. Interestingly this stems from ideas advanced already by Cournot. This approach suggests that, if the economy consists of a large number (which is necessary for price-taking behaviour to make sense) of sufficiently heterogeneous agents, then properties like uniquenes and stability of equilibrium may be restored (see Grandmont (1987, 1992) and Hildenbrand (1983, 1994)). Thus structure may be introduced into aggregate behaviour by the presence of enough differences between the characteristics of the agents. There is, of course, an important deviation from the route here. The idea is to introduce assumptions on the distribution of preferences rather than on the individual preferences themselves. Unfortunately this approach has not led far for the present, as work by Billette de Villemeur (1999) and Hildenbrand and Kneip (2005) has shown. Indeed, it seems likely that the difficulties with the basic model are too fundamental to be solved by such assumptions on the distribution of characteristics. As Frank Hahn (1982), says, when talking of the attempts to establish stability results and referring to the SMD result,

> “The enterprise was doomed not to be capable of reaching general conclusions in the Walrasian setting. A theorem not directly related to connected with dynamics did the damage.”

Let me conclude this section by mentioning another approach which left the Walrasian road and, although to some of us very promising, has largely been ignored by the economics profession. Werner Hildenbrand (1994) one of the leading exponents of General Equilibrium Theory, in his

book, « Market Demand », breaks with the Walrasian tradition and embarks on an original and revolutionary approach to showing the stability and uniqueness of equilibrium. What he does is to start with an empirically verifiable fact and then shows that if this obtains empirically, the market equilibrium is unique and stable. Briefly what he does is to make an assumption that the « cloud » of consumption vectors for the individuals corresponding to any income level in an economy is increasingly dispersed. This can be checked against household expenditure data for example. If this property holds it will then guarantee that the generalised Law of Demand will hold. This, in turn will guarantee that the market equilibrium is unique and stable.

The important point here is that this is *not* an assumption derived from individual behaviour but is a property of the distribution of that behaviour. Why do I suggest that Hildenbrand's approach is radical? The main reason is that, normally speaking, economists consider « good aggregate theory » to be that which is based on the « sound micro-economic foundations » which they inherited from Walras. Hildenbrand eschews this approach and starts out with a hypothesis which, with some simple assumptions is empirically verifiabe. He is not concerned with the behavioural origins of this assumption at the individual level. Thus here we have rigourous deductive analysis which simply sidesteps the standard individual model which we have inherited from the Lausanne school. Once again, leaving the path traced by Walras has not met with general approval.

So far, I have discussed the weakness of the Walrasian General Equilibrium model and its incapacity to tell us anything about the functioning of the economy out of equilibrium. Furthermore, I have mentioned examples of efforts to change that route which have not met with great success. A natural question at this point would be what sort of model would be more satisfactory and what sort of features should one include which are absent in the basic Walrasian analysis ?

The economic individual and his social context.

There has been an increasing recognition within economic theory of the last decades that the economic agent is not a totally autonomous or atomistic being who interacts with others through anonymous signals, such as prices alone, but that the economic agent is situated in a social

context and that this social aspect of the agent will have consequences for his behaviour. Society changes and since preferences are socially influenced and influence the common values of a society (Becker and Murphy (2000), Sen (1999)), the individual must somehow take account of this when he makes his decisions.

One of the major drawbacks of the General Equilibrium theory that we have inherited from Walras, is that it assumes that the only interaction among economic agents, other than the rather vague crying out of prices and meetings on the market, is through the price system. It is not reasonable to assume that the only social influences that have an impact on individuals occur before they participate in the market and that after that everything is frozen. What happens if we try to repair this by remaining within the standard model but assume that the preferences and hence the choices of one individual are influenced by others? Will we now be able to retain the equilibrium notion, for example? This is an old problem, as the following observation from Koopmans (1957) shows:

"Changes in consumers' preferences would be a much less important source of uncertainty if in fact such changes occurred for different consumers independently of each other. The law of large numbers would, in that case, reduce the variability in the distribution of aggregate demand at constant prices over the various commodities. It is through waves of imitation... that interacting preferences become an important source of uncertainty." *Koopmans (1957), quoted in Föllmer (1974).*

A number of authors have looked at the problem of the stochastic interaction between individuals particularly as it affects their preferences. This path was pioneered by Foellmer (1974) and a good account is given by Brock and Durlauf (2001). That the interaction between individuals and the way that people influence each others' preferences is important was already mentioned by Edgeworth. So if our aim is really to characterise aggregate behaviour we have to heed Lucas' advice when he said,

"Applications of economic theory to market or group behaviour require assumptions about the mode of interaction among agents as well as about individual behaviour" *Lucas (1986).*

Interaction and aggregate behaviour

What should now be clear from this discussion is that there is, within the Walrasian framework

an inherent conflict between the requirements of the theory of the individual and the desire to have properties of aggregates. If we adhere to the basic tenet of the Walrasian General Equilibrium model that macro or aggregate behaviour must be derived from underlying rational micro foundations then we have to explain how the characteristics of the aggregate are determined by those of the individuals. It is here that the General Equilibrium model has let us down because we can say very little about aggregate behaviour in that model. This is what we have learned from the SMD results. But, if we look at the other side of the coin and argue that there is more interaction between individuals than that foreseen in the General Equilibrium approach then we have to take account of that interaction and how this is organised. Yet Walras bequeathed us nothing of this, and even if we accept Walker's argument that he was interested in the real interaction between individuals, he had little to say about how that interaction was structured. Nor, in his models does he have much to say about the structure of the environment within which the individuals operate. Economics has continued in this vein, despite a number of important contributions on economic networks, (see Jackson (2008) or Goyal(2007) for good surveys) and on the importance of social identity, (see e.g. Akerlof and Kranton (2010)). A deviation from the Walrasian route could well have followed from the work of Walras' successor at Lausanne, Vilfredo Pareto. The latter, spent the last part of his life working on sociology and emphasised the non-rational decisions that individuals make. However, this part of Pareto's work has gone largely unnoticed by economists, and what is remembered in economic theory is essentially a refinement and purification of Walras' work.

The discussion of the role of interaction between individuals whether they be neurons, molecules or insects, is very much present in other disciplines. However the situation in economics is complicated by the fact that the practice of analysing macro-relationships, without considering their micro-foundations, is now, in economics, almost universally considered as « unscientific » and this is because the underlying Walrasian framework has been elevated to the status of being the necessary basis for any rigorous economic analysis. Despite the heavy dependence on physics of the Walrasian model, more recent economists have recoiled from the use of more modern physics in their discipline. Nor are they willing to envisage importing lessons from other sciences. Forgetting the origins of our models, they argue that, in economics we are not faced with unthinking particles, or molecules or even social insects, we are faced with beings that have

intentions. As a result economic models are of a different order from those in the sciences.

However, suppose, for a moment that we had wanted to deviate from the Walrasian route and incorporate the direct interactions into our models as indeed, we have done in game theory. Then it is clear that the intentions of individuals do become important. For example, a major problem is what happens if interaction is actually chosen consciously. Thus it is not only the interaction that matter but the very nature of the individual and his conscious choices concerning himself and the community he interacts with. Once we consider the individual as embedded in a social context, possibly of his own choosing, other considerations enter into play, (see (Bernheim (1994); Akerlof and Kranton 2000, 2010) and this will change over time as he and society change.This is very different from the fixed preferences which are attributed to the individual in the successor to the Walrasian construction, the Arrow-Debreu model. Furthermore it leads to a vision very far from that that one sees from the Walrasian road,

The relation between Economics, Physics and Mathematics

I would now like to offer a final explanation as to why we have pursued the road from Walras to Arrow-Debreu so assiduously, and it concerns the relationship between economic theory and physics and mathematics. Mirowski (1989) has built a painstaking and convincing argument that Walras' theory was built directly on the basis of classical mechanics. There are numerous references in his work to this and it is clear that until at least the 1950s the benchmark Walrasian model was built on physics. But, to go back to Walras, not only was he trying to build a social physics, he was also extremely anxious to obtain the approval of mathematicians. His correspondence with Poincaré and with Laurent is witness to this. However, he did not convince these authorities to the extent that he had hoped as witnessed by Poincaré's rather pointed criticisms of Walras' assumptions of the « infinite egoism » and « infinite farsightedness » of economic agents. The former Poincaré could accept, at a pinch, but the latter seemed, at best, implausible.

Note also that Walras systematically referred to « l'économie mathématique », mathematical economics, to distinguish himself from those whom he regarded as descriptive or institutional economists. He observed with pride in his letter to Hermann Laurent, the mathematician,

All these results are marvels of the simple application of the language of mathematics to the quantitative notion of need or utility. Refine this application as much as you will but you can be sure that the economic laws that result from it are just as rational, just as precise and just as incontrovertible as were the laws of astronomy at the end of the 17th century.

Lettre no. 1454 to Hermann Laurent in Jaffe (1965).

Yet, as I have said, Walras was far from convincing the mathematicians on many points. For example, both Laurent and Poincaré did not agree with the idea that satisfaction or utility could be measurable. Walras cites Laurent as having said in his address to the Institut des Actuaires francais in 1900, « How can one accept the notion that satisfaction can be measured ? No mathematician could agree to this ». Again, in his correspondence with Hermann Laurent he

explained that it was sufficient for a system of equations to have a solution that the number of equations should be equal to the number of unknowns and Laurent expressed some doubt about this. Late in his life Walras, unjustifiably claimed the unqualified support of Poincaré and wrote,

« in 1906 I was in contact with certain eminent French mathematicians who had seen at first glance that my mathematical economics was well founded and who made a declaration to this effect at the St. Louis Exhibition », *Walras lettre 1642 to Charles Gide, in Jaffe (1965).*

This, as Jaffe remarks, despite the explicit denial by Henri Poincaré that he had mentioned mathematical economics in the address to which Walras referred, (Poincaré letter no. 1639 in Jaffe (1965)). It is worth observing that at the time the separation of mathematics from physics was far less marked than it is today. Eminent figures such as Poincaré himself wrote extensively about physics. Thus Walras was also happy with the analogy with physics and this, for us, only adds to the difficulty of interpreting his position.

He was rightly flattered by Pareto's comparing his contributions to those of Newton in physics, but remarkably he still felt aggrieved that Pareto should have criticised some of his ideas, and, worse declared some of his results incorrect. (Lettre 1502 to Leon Winiarski, in Jaffe (1965)). He seemed to be unable to overcome his resentment at what he considered to be his lack of recognition. Pareto, in turn, was contemptuous of Walras' efforts to justify his contributions and he confided to Panataleoni that he found Walras' address on the occasion organised by the University of Lausanne in his honour, ridiculous, since it was totally inappropriate for someone who was being honoured to explain why he was being honoured ! (Letter no. 597 in Pareto (1962)).

Two things are worth observing here. If, as seems clear, the Lausanne school was built on physics why did it not evolve with that science rather than evolve towards pure mathematics? Mirowski's (1989) explanation is clear. Had it done so it would have found itself in a contradictory position. Thermodynamics and the emphasis on entropy would suggest a system which was constantly moving towards disorder, the opposite of the Lausanne view. Samuelson expressed this very clearly when he argued that what economists want is a clear notion of

equilibrium, one which is unique and one which is attained independent of initial conditions. Yet, it was Samuelson who also constantlyalluded to such ideas loosely based on the Le Chatelier and Correspondence principles. But, as Samuelson himself said,

« There really is nothing more pathetic than to have an economist or a retired engineer try to force analogies between the concepts of physics and the concepts of economics. How many dreary papers have I had to referee, in which the author is looking for something that corresponds to entropy or to one or another form of energy » *Samuelson (1972) p.258*

But this somehow seems in contradiction to his own articles on this subject. What Mirowski (1989) suggests is that Samuelson was constantly using the physics metaphor without pursuing the analysis to the point where he was led to a contradiction. So, economics was left in an uncomfortable situation, adhering to its physical model whilst unable to push beyond that to handle either dynamics or even the adjustment from an out of equilibrium position to equilibrium. Although it is sometimes loosely stated that Walras proved he exisence of equilibrium, it was not until the work of Wald (1936) that this was done formally. Walras cannot be blamed for this since proving the existence of equilibrium is equivalent to proving the existence of a fixed point of a continuous mapping. This was not done until 1912, when Brouwer gave the first fixed point theorem, and this was, of course, after Walras' death. However, Walras was convinced that the sort of adjustment process which he described and which he thought was consistent with what actually happened on many markets, would lead the economy or market back to equilibrium. But, in fact, he was far from showing this and there was, as we have seen, a good reason for that. How then did economics resolve this difficulty ?

In my view, in the fifties, theoretical economists, led by Debreu, turned their backs on physics and tried to build and abstract coherent structure in which equilibrium states could be shown to exist. Economics was, at least in the view of those in mathematical economics at the time, ready to build a structure as minimalist and abstract as that provided by Bourbaki for mathematics. It is the emergence of this phenomenon that is worth briefly describing. The first element is the introduction of the axiomatic method, something in which Debreu was a fervent believer and which was certainly not unrelated to his own mathematical education in the shadow of the

Bourbaki crowd. This did not simply reduce the existing framework but changed its orientation (see Mirowski and Weintraub (1994)). Physics envy was thoroughly replaced by mathematics envy. The unbounded admiration of Debreu for mathematicians such as Smale is testimony to this. The epitome of this development is the volume edited by Chipman et al. (1971) where the whole structure of the general equilibrium model, and demand in particular, is developed in as abstract a form as one could imagine. But, at that point, there was still the hope that within the framework which had been developed one could show that under some adjustment process the equilibria that had been shown to exist could be proved to be stable.

Thus, although economists had left the methodological route mapped out by Walras, and moved to an axiomatic approach, it was hoped that they could deal with the problems he raised in a satisfactory and rigorous fashion. Walras, would surely not have been unhappy with this idea. Paradoxically, it was one of the editors of the book, to which I have just referred, (Chipman et al. (1971)) who gave the first clear indication that the Walrasian road was leading into a dead end. As I have said, the SMD results showed that the equilibria of the Walrasian system could not be proved to be either unique or stable. At this point, one might have imagined that the reaction would have been to rethink the fundamental structure of the model and to rebuild it on different theoretical foundations. But inertia is strong and there is little sign that the fascination with the type of mathematics to which Debreu led us, all the while claiming to be doing so in the footsteps of Walras, is diminishing. Already, when Debreu received the Nobel prize the message was clear, the way forward was to use the mathematical approach that he introduced. As the Nobel committee said at the time,

> « Gerard Debreu symbolizes the use of a new mathematical apparatus, an apparatus comprehended by most economists only abstractly. Nevertheless, his work has given us an improved intuitive understanding of the underlying economic relevance. His clarity and analytical rigor, as well as the distinction drawn by him between an economic theory and its interpretation, have given his work important bearing on the choice of methods and analytical techniques within economic theory on a par with any other living economist. »
> —*introduction of the Nobel laureate at the Royal Swedish Academy of Sciences,* 1983

With the SMD results one might have expected a restructuring of the basic of the model, but, in fact, we have, at least in macroeconomics, simply assumed the problems away. It is this, I think

that would have made Walras unhappy. He devoted considerable time and energy to discussing how markets were organised, who was doing what, and claimed that the result was that the economy would constantly tend to equilibrium. He had a clear view of a process and the idea that the actions and interactions of individuals would drive that process towards equilibrium even if that equilibrium was never attained. In what are now called Dynamic Stochastic General Equilibrium models the out of equilibrium dynamics and adjustment are no longer an issue. Nor is there any interaction or trading between individuals and there is no indication as to who is determining the prices. This is very far from Walras' view. The desire to be mathematically rigorous is still strong, but the abandonment of the physics approach in favour of the axiomatic approach has made at least one alternative route, that of statistical physics less accessible and, what is more, unwelcome.

Conclusion

Economists have followed one of the paths outlined by Walras, and we have chosen the view, that he frequently emphasised, that of an economy with an equilibrium state reflecting the reconciliation of the individual demands for and supply of goods through the price system.. We have abandoned the concern with how the equilibrium prices are established, and have concentrated on the properties, in particular, the efficiency of equilibrium states. We have insisted on the rigour of our analysis, but much less on the realism of our assumptions. Again Walras can be held responsible, at least in part. Thus, at the very outset of economics' journey down the path to Arrow-Debreu, Poincaré whose approbation, as a distinguished mathematician, was eagerly sought by Walras, was already pointing out the difficulties, but unfortunately for economics his observations fell on deaf ears.

In the end, the road in pure theory that Walras set us on, petered out and has only remained in macroeconomic theory. Yet, the irony is that the name of Walras, whose major contribution was to recognise the interdependence of various markets, is now enshrined in models which bear no resemblance to what Walras had in mind. In Walras' writing individuals interacted either through some price mechanism or directly with each other and this depends on which part of his work one wishes to focus on. Yet in modern macroeconomic models which still claim to be Walrasian, individuals do not interact at all, since they are subsumed into a representative individual.

Whatever interpretation we put on Walras' vision of economics this is hardly likely to be what he had in mind.

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