Demand Theory and General Equilibrium: From Explanation to Introspection, a Journey down the Wrong Road

Alan Kirman[1]

Richard B Fisher Member,

Institute for Advanced Study, Princeton,

GREQAM, EHESS, Université d'Aix-Marseille lll

> 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

The complexity of an economy stands in sharp contrast to the simplicity of a question that must be raised about its operation. Many agents compose the economy, and they have to deal with a large number of commodities. Each one of those agents makes decisions about the quantity of each one of those commodities that he will produce or consume: the number of variables involved is the product of the number of agents and the number of commodities. Moreover, in this decision-making process the agents act

[1] I would like to thank the Institute for Advanced Study for its hospitality and the participants of the HOPE conference for their helpful comments. In particular I have appreciated the valuable remarks of Phil Mirowski and John Chipman.

> independently of each other, and they are guided by self-interest. Why is high disorder not the result?
>
> The agents of an economy are counted in millions, if not billions. The number of commodities is similarly large. The self-interests of the independent decision-makers are sometimes in agreement, sometimes in conflict. Why does one not observe for every commodity a large excess of demand, evidenced for instance, by lengthy waiting times for orders to be filled, or a large excess of supply over demand, evidenced, for instance, by massive inventories?
>
> Agents no longer make independent decisions, and they interact with each other, if there are markets for commodities. Their interaction then reduces the difference between demand and supply.
>
> —*Gerard Debreu, "Existence"* 1998

After the publication of the Arrow-Debreu model, economists were awestruck by the rigor and consistency of the reasoning. Finally, it was agreed, all the surplus flesh had been stripped off the skeleton of economic theory, and we now had new levels of analytic rigor to live up to if we were to be taken seriously as theorists. This was the evaluation of Gerard Debreu's Theory of Value (1959), in particular, and few, I think, were inclined to quarrel with the idea that the way forward was through modification and improvement of the general equilibrium model. After a long climb we had reached an intellectual plateau where, if one could breathe at such heights, everyone would like to be. Indeed, this idea fits well with a metaphor used by Einstein:

> Creating a new theory is not like destroying an old barn and erecting a skyscraper in its place. It is rather like climbing a mountain, gaining new and wider views, discovering new connections between our starting point

> and its rich environment. But the point from which we started still exists and can be seen, although it appears smaller and forms a tiny part of our broad view gained by the mastery of the obstacles on our way up."
>
> *quoted in Hahn* 1974

There were not many who would have argued that the mountain climbed by Arrow and Debreu was not the right one and that, in a relatively short time, even some of the stronger climbers would have reluctantly concluded that we were on a lonely peak far from the path traced out by earlier economists. What I will argue here is that the basic framework to which economic theory and, in particular, demand theory was reduced is inappropriate as a model of economic choice, since it depends on assumptions about individual agents derived not from observation but rather from introspection. Furthermore, these individuals are assumed to operate in an environment devoid of institutional content. Last, analytically the model contained the seeds of its own destruction. I would identify the crucial moment at which we committed ourselves to this rather futile path as coinciding with the appearance of The Theory of Value.

This route had three main features: first, the characteristics of economic agents; second, the environment within which they operate; and third, a notion of what would constitute an equilibrium state of a set of agents functioning in that environment. Suppose that, to simplify, we say that we are interested in the allocations of resources that are self-sustaining in some sense, and that such a state would constitute an equilibrium. By self-sustaining we might mean that nobody in the economy would, given the rules of the game, prefer to make another choice. That poses two immediate questions. Do such states exist, and will they be attained? But already I am jumping the gun. We must have a notion of what it means to prefer, and what the rules of the game are. Both of these have evolved along the path that I examine below and both help explain how we arrived at the wrong peak.

Before entering the heart of the matter, I want to indicate briefly what is involved here. The theory of individual preferences has been intensively developed, not because we are interested in the intrinsic qualities of those preferences but rather because we are concerned to model the choices that those preferences result in.[1] The various refinements and purification of preferences have been made with a view to maintaining certain properties of demand that, in turn, were necessary to guarantee the existence of equilibrium within the market framework I have alluded to. Thus preferences and demand have been intimately linked. The second remark relevant to the whole of what follows is that the market as an institution hardly figures in the theoretical literature. Indeed, as Douglas North (1977, 710) says, "It is a peculiar fact that the literature on economics . . . contains so little discussion of the central institution that underlies neoclassical economics—the market."

For the moment, let me briefly mention what is meant by market in the history of general equilibrium theory. I will then discuss the difficulties that have arisen with this choice of framework and then come back to the importance of taking an alternative view and an alternative path.

When Debreu talks about markets, all that he has in mind is that the prices of all goods at all times and in all places should be determined and known to all participants in the economy. This vector of prices constitutes what he refers to as "market prices." In particular, he is interested in those prices for which the excess demand is zero for every commodity, and it is these that he refers to as "equilibrium prices," and in this he follows exactly in Léon Walras's footsteps. On the subject of how and by whom those prices are established, he is silent, as he is on the subject of how the trades that correspond to those prices are effected. Yet there is the illusion that such considerations are present. To be more specific, consider the last part of the second epigraph, which reveals why the model proposed does not answer the question posed. Debreu says, "Agents no longer make independent decisions, and they interact with each other, if

there are markets for commodities. Their interaction then reduces the difference between demand and supply." But this is precisely the basic weakness of the model. Nowhere is there any explanation of how this happens or, beyond what I have already said, what is meant by "if there are markets for commodities." Yet these problems were central to the questions posed by Walras, for example, as Donald Walker (1996) has pointed out. Indeed, at first glance, it seems that Debreu is alluding not only to the equilibrium but also to an adjustment mechanism when the prices are not at their equilibrium values. Yet all who are familiar with Debreu's work know that, unlike Kenneth Arrow, Frank Hahn, and Leonid Hurwicz, Debreu was not preoccupied with the problem of how prices might adjust to equilibrium. He was concerned with the existence of equilibrium and only in a very tangential way, through his work on local uniqueness, with its stability.

By the time that we have arrived at the peak first climbed by Arrow and Debreu, the central question boils down to something rather simple. We can phrase the question in the context of an exchange economy, but producers can be, and are, incorporated in the model. There is a rather arid economic environment referred to as a purely competitive market in which individuals receive signals as to the prices of all goods. All the individuals have preferences over all bundles of goods. They also have endowments or incomes defined by the prices of the goods, and this determines what is feasible for them, and the set of feasible bundles constitutes their budget set. Choosing the best commodity bundle within their budget set determines their demand at each price vector. Under what assumptions on the preferences will there be at least one price vector that clears all markets, that is, an equilibrium? Put alternatively, can we find a price vector for which the excess demand for each good is zero? The question as to whether a mechanism exists to drive prices to the equilibrium has become secondary, and Herb Scarf's famous example (1960) had already dealt that discussion a blow.

The warning bell was sounded by such authors as Donald Saari and Carl Simon (1978), whose work gave an indication, but one that has been somewhat overlooked, as to why the stability problem was basically unsolvable in the context of the general equilibrium model. The most destructive results were, of course, already there, those of Hugo Sonnenschein (1974), Rolf Mantel (1974), and Debreu (1974) himself. But those results show the model's weakness, not where that weakness comes from. Nevertheless, the damage was done. What is particularly interesting about that episode is that it was scholars of the highest reputation in mathematical economics who brought the edifice down. This was not a revolt of the lower classes of economists complaining about the irrelevance of formalism in economics; this was a palace revolution.

How did we wind up on this peak, and why has it been so difficult to leave it? On my bookshelf sit several volumes of Bourbaki's monumental Elements de mathematiques. Not far from them is a book edited by, among others, John Chipman. And nearby is Debreu's Theory of Value. The three epitomize a view of theoretical economics that flourished from the sixties to the seventies. 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 I would like to explain. 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). The second is the nature of the preferences, which are held to characterize economic agents. If one looks back to that classic book edited by Chipman et al. (1971), one sees how far we have moved in our vision of what constitutes rational choice. It would be difficult to imagine a more erudite, arid, and inaccessible volume than theirs, yet it was widely considered as exemplary scholarship. There was little discussion of the underlying foundations of

the very notion of preferences but rather a series of contributions trying to establish various relations between preferences and demand. On what preferences are, Hirofumi Uzawa (1971,) says simply,

> Preference relations, in terms of which the rationality of human behaviour is postulated, are precisely defined as irreflexive, transitive, monotone, convex and continuous relations over the set of all conceivable commodity bundles. A demand function associates with prices and incomes those commodity bundles that the consumer chooses subject to budgetary restraints.

Uzawa's basic and important contribution was to show that if a demand function satisfies the weak axiom of revealed preference (WARP), then there exists a preference relation from which it is derived. Furthermore, demand functions derived from preferences satisfy WARP. Both Jean Ville and Hendrik Houthakker (see Gardes and Garrouste, this volume) had shown that a necessary and sufficient condition for a demand function to be derived from preferences was that the strong axiom of revealed preference (SARP) be satisfied. Uzawa completed the picture by showing that, under certain regularity conditions, WARP implies SARP. This is more than a little disappointing, since it was originally thought that revealed preference theory was somehow more satisfying than utility-based theory, since it is based, at least in principle, on observable behavior. Thus, with these results, an avenue out of the abstract world of preference relations satisfying certain axioms derived from introspection was closed off.

One might have thought that revealed preference would have lost its appeal after this result, but this was not the case. The reason for this is clear. Although demand choices are in fact observable (in principle) in the Arrow-Debreu framework, an individual is never faced twice with the same situation. Choices are made once and for

all at the beginning of time. Since all commodities are dated, seeing an individual choosing bundle x when she could have chosen y; then, subsequently choosing y when she could have chosen z; and then later, choosing z when she could have chosen x is not a violation of WARP, since the x that figures the second time is not the same as the one in the first choice. The commodities are available at a later time and are therefore no longer the same. Furthermore, if any consumption took place between the choices, the preferences that determine the later choices may well be affected. Therefore, if we adhere to the general equilibrium framework, we can never actually observe a contradiction with the hypotheses that characterize revealed preference. The only way out of this would be to pose the hypothetical question repeatedly to an individual: which of these two bundles would you choose? At this point the advantage of the "observability" of the choices has been lost. Yet revealed preference still plays a prominent role in theory and in the teaching of economics (see Mas-Colell, Whinston, and Green 1995). It is prompted by the simplicity with which it can be explained, and this is reinforced by its widespread use in experimental economics. In the experimental laboratory the practice of posing successive hypothetical choice questions and then checking to see whether these choices are coherent is widespread. There are technical questions, such as whether to prefer Sidney Afriat's (1967) strong axiom, which is convenient when one is faced with a finite set of observations. But the underlying logic remains: what we are demanding of our unsuspecting experimental subjects is no more or less than what we require of our traditional economic agents endowed with preferences with all the standard properties.

Moreover, a great deal of effort and mathematical skill were spent on weakening the conditions imposed on preference relations. It was argued that weakening the assumptions in this way would make them more acceptable. Little was said about the basic underlying assumption that individuals are indeed characterized by such preference orders. Taking this as given, the goal was to weaken the basic axioms as

much as possible. Today, it is recognized that none of the standard axioms, even in their weakened form, are derived from observation of choice behavior but rather are the result of pure introspection by economists. Yet this recognition has nothing to do with recent developments. Werner Hildenbrand (1994) cites quotations from economists such as Lionel Robbins (1935), Tjalling Koopmans (1957), and John Hicks (1956), all of whom were well aware of this. Worse, later in his life, Vilfredo Pareto concluded that individuals make quite arbitrary choices and spend the rest of their time justifying them! Thus skepticism about the bases of the utility theory on which the theory of demand is based is long-standing among highly reputable economists.

If one does embark on this theoretical route, it seems natural to make the assumptions as weak as possible. But Pareto himself was not happy with this insistence on weakening assumptions to the maximum. He was still, for example, convinced that the idea of measurable utility had some merit. As Chipman (1973) pointed out, "Thus altering slightly a suggestive metaphor of Georgescu Roegen, just because the equilibrium of a table is determined by three of its legs, we are not required by any scientific principle to assume that actual tables have only three legs, especially if direct observation suggests that they may have four." Other economists on the same road were less reticent. Hicks, for example, suggested that the principle of Occam's razor should always apply, and he was far from alone. Indeed, as we move down the road from Walras and Pareto we observe a clear tendency to do this, and the driving force is not economic plausibility but mathematical convenience. With the formalization and axiomatization of the basic model, it became clear that what was required for equilibrium to exist was some sort of continuity of demand and hence of excess demand. This was what pushed economists toward assumptions of convexity and continuity. If the continuity of demand could be guaranteed by weaker assumptions than the standard ones, this was welcomed. Thus the name of the game was to find the weakest conditions under which equilibrium exists.

Why this insistence on the existence problem? Simply because given how the program was formulated, the natural idea was to look at "states of the economy" and then to characterize the equilibrium states. This preoccupation was not new, but it came sharply into focus in the Arrow-Debreu setting. When economists talk about the state of the economy, they may mean different things. Depending on the particular problem at hand, the economist may be interested in a detailed description of some part of the economy or may simply wish to examine some highly aggregate variables. At the most general level we might be interested in specifying everything about the allocation and activity of every agent in the economy, and, indeed, this was the lofty ambition of general equilibrium theory. At the other end of the spectrum we might simply be interested in the number of people "out of work" or the "price level." Theoretical micro economists have typically been interested in the first problem and have been preoccupied since the early 1900s with, for example, the characteristics of the competitive equilibrium of a fully specified economy.

It is important to recall at this point that all of this theory is developed in the context of the perfectly competitive system. In the general equilibrium model, no "organization" or specification of interpersonal relationships is assumed. The consistency of allocations in the sense that what individuals receive is just what they desire is achieved by an anonymous price system that all agents take to be given. Thus there is no need for any specification of the relationships within the economy; the only way in which agents communicate is through the price system. Indeed, to pursue their analyses, economists have stripped away all the institutional details and structures that were still very much present in the work of both Walras and Pareto and even more so in the work of Alfred Marshall. In particular, economists have focused on the assumptions about individual characteristics and resultant behavior, which will ensure achieving an "equilibrium outcome."

This is, of course, a very static view of economics. Even when looking at economic dynamics, economists have, in general, concentrated on looking at "steady states" or equilibrium growth paths. How did economists become so wedded to this view of the world? A neat and clear explanation of the development of economic theory in this century would be that it was essentially dominated by the analytic methods of classical mechanics and that over most of the last century, physics triumphed as a model for economics. This is indeed the view advanced by Bruna Ingrao and Giorgio Israel (1990) and Philip Mirowski (1989). In other words, the peak, which we reached in the Arrow-Debreu model, was indicated by signposts from physics, which have been there since the nineteenth century. Yet, it seems reasonable to ask, is this explanation justified?

Economics and Physics

What is clear from both Pareto's analysis and that of many of his contemporaries such as F. Y. Edgeworth, William Stanley Jevons, and Franklin Fisher is that they all shared a conviction that there was an analogy between economic systems and those of classical mechanics. Edgeworth was quite explicit in suggesting that a "mecanique sociale" would take its place alongside the "mecanique celeste" (quoted in Cohen 1994, 90). Jevons (1905, 631) said that economics resembles physics in that "the equations employed do not differ in general character from those which are really treated in many branches of physical science." Another contemporary, John Elliott Cairnes was even more explicit: "Political Economy is as well entitled to be considered a 'positive science' as any of those physical sciences to which this name is commonly applied" (quoted in Cohen 1994, 42). He went on to argue that the principles of economics have identical features to those "of the physical principles, which are deduced from the laws of gravitation and motion" (198). Mirowski (1989), Ingrao and Israel (1990), and Bernard Cohen (1994) have examined at length the validity and consequences of such assertions.

The extent to which the analogy between physics and economics has ensnared economics in a position that it could have avoided had it found its source of inspiration elsewhere—for example, in biology, as Marshall suggested—is well documented by these authors. Pareto (1953, 185) himself remarked that, when examining the equations that have to be solved to determine an economic equilibrium, someone well versed in mathematics or physics would say, "These equations do not seem new to me, they are old friends. They are the equations of rational mechanics." He went so far, in the Cours, as to draw up a table of analogies between the two disciplines.

Although this argument is persuasive and allows us to establish a simple route from Walras to Debreu, it is not completely convincing. The vision of equilibrium is subtly different in the two cases. In economics it is really the solution of a system of equations, a fixed point; in the physics interpretation, at least the metaphor of a resting point of a dynamical system persists as, indeed, it did in the writings of Walras and Pareto.

In the economics view, agents in an economy maximize some concave or quasi-concave objective function over a convex set defined by parameters over which they have no control. The natural way to analyze such a system is to look for an equilibrium state, that is, values of the parameters determining the constraints of the individuals and choices of those individuals such that none of them has any incentive to modify his or her actions. The passage from individual to aggregate behavior is simply additive. Furthermore, no explanation is given as to how the equilibrium comes about; this is the subject of stability analysis, which has usually been reduced to examining the convergence of artificial price-adjustment processes. Yet, as Michio Morishima (1964,) pointed out,

> 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.

Uniqueness and Stability of Equilibrium

As I have said, there is literally nothing in the Arrow-Debreu model about how an economy out of equilibrium could attain that state. If we are interested in that problem, then we must show how a nonequilibrium price vector would be altered toward or into equilibrium. The simplest adjustment process is the well-known tâtonnement process suggested by Walras, which he argued would lead naturally to equilibrium. He assumed that the level of prices for which there is a positive excess demand should rise and the level for those for which the excess demand is negative, that is, for which there is excess supply, should fall. The tâtonnement process can be written as follows:

$$p_{t+1} = p_t + \lambda Z(p_t)$$

Just writing this expression presents a problem. What is the time involved? The usual argument is that one should think of the adjustment as taking place in virtual time. It is common practice also to write this process in continuous time as

$$\dot{p} = \lambda Z(p)$$

Since the time involved is not related to the model, this does not really matter. Yet this is purely artificial. Although it is easy to convince oneself of the basic intuition behind the process, the way in which it works is very odd. Indeed, it usually assumed that no trade takes place until equilibrium prices are established.

Whenever one looks closely at the mechanics of the adjustment mechanisms that have been proposed, however, they almost all refer back to Walras. The economists just mentioned saw themselves as being the direct descendants of Walras and Pareto. Hicks

(1939, 2), for example, says, when he wishes to spell out the adjustments on several markets,

> 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 is 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.

Despite the contribution of Edgeworth, the "written pledges" model of Walras, and later brave efforts by Hicks, Hahn, Takashi Negishi, and others, analysis of so-called non-tâtonnement processes, in which more realistically economic activity is allowed before equilibrium is reached, has received much less attention. The desire to pursue a more realistic mechanism through which market equilibrium could be attained led Hicks to establish the well-known "Hicksian week" during which trade would take place at nonequilibrium prices. In a sense, this is an alternative to Walras's "written pledges" model, described in detail by Walker (2005). Yet in all the non-tâtonnement adjustment processes proposed, there is always some artificial argument about the consistency between the sign of individual and aggregate excess demands as the process proceeds. In effect, "nobody is on the wrong side of the market." This is disappointing, since non-tâtonnement is clearly more realistic than tâtonnement. However, let us go back to the route that was followed and leave non-tâtonnement processes to one side, as the literature did, and assume for a moment that we accept the artificial tâtonnement process.

If such a process did actually converge to equilibrium from an arbitrary starting point, could it be said that things would surely be much simpler? Not really, because the fundamental problem is that the conditions, which are known, which guarantee the stability of this process, cannot be obtained from assumptions on the behavior of the individuals. To be absolutely clear, what Sonnenschein (1972), Mantel (1974), and Debreu (1974) 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's law, homogeneity of degree zero, and boundary conditions that guarantee that aggregate excess demand "explodes" if any price goes to zero. Since there are functions satisfying these conditions that are not stable with respect to the tâtonnement process, and that do not have a unique equilibrium, these properties cannot be guaranteed by the assumptions on individual characteristics.

The full force of the Sonnenschein, Mantel, and Debreu (SMD) result is often not appreciated. 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 compares equilibrium and another one, predicated on a change in the parameters, makes no sense in the presence of multiple equilibria. 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 familiar criticisms (Kirman 1992; Stoker 1995), and recourse to this creature raises one of the basic problems encountered on the route to the place where general equilibrium has found itself: the problem of aggregation. In fact, we know that, in general, there is no simple relation between individual and aggregate behavior, and to assume that behavior at one level can be assimilated to that at the other is simply erroneous.

At this point it is worth mentioning an attempt to restore some structure that had been abstracted away and, hence, to overcome the difficulties produced by the fact that uniqueness and stability of equilibria cannot be guaranteed in the basic model. Interestingly, this stems from ideas advanced already by Augustin Cournot. This approach suggests that, if the economy consists of a large number (necessary for price-taking behavior to make sense) of sufficiently heterogeneous agents, then properties like uniqueness and stability of equilibrium may be restored (Grandmont 1987, 1992; Hildenbrand 1983, 1994). Structure may be introduced into aggregate behavior by the presence of sufficient differences between the characteristics of the agents. There is, of course, an important deviation from the conventional route here, the idea being to introduce assumptions on the distribution of preferences rather than on the individual preferences themselves. Unfortunately, this approach has not yet led far, as recent work by Etienne Billette de Villemeur (1998) and Hildenbrand and Alois Kneip (2005) has shown. The problem seems to reside with the definition of heterogeneity. What has been shown so far by Hildenbrand is that heterogeneity of behavior because of particular income distributions imposes structure, a version of the "law of demand," on aggregate behavior. What has not been shown, as yet, is how one can make assumptions on the distribution of the underlying preferences in the standard model that will guarantee the dispersion of behavior. It is conceivable that dispersion of the parameters of utility functions may well not lead to different behavior of the individuals. The increasing dispersion introduced by Grandmont essentially makes the economy behave as if it were one giant Cobb-Douglas individual and the representative agent appears again. However, my basic argument here would be that the difficulties with the basic model are too fundamental to be solved by such assumptions on the distribution of characteristics.

Another argument that has been used to diminish the impact of the SMD result is that of Donald Brown and Rosa Matzkin (1996). They argue that excess demand should

be thought of as depending on both prices and income, and that doing this will impose some structure on aggregate excess demand. To take the simplest example: suppose that we bound endowments and then insist that consumption shall not be negative; then we may have restrictions on possible excess demand functions. At this point the argument becomes almost philosophical. We know from an early result of Alan Kirman and Karl Koch (1986) that we can posit a function and an income distribution and still find an economy that corresponds to it. However, what is also true is that, to do this, we cannot bound the resources a priori. Chipman (this volume) gives an elegant account of related problems. Few will, I think, find that the basic message of the SMD result has been really weakened. As Hahn (2002, 224) has written, 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." Having examined the weakness of the general equilibrium model and its incapacity to tell us anything about the functioning of the economy out of equilibrium, we might ask whether we cannot establish the source of the difficulty.

Information and Stability

One of the major attributes of general equilibrium theory also reveals its Achilles' heel. It is often asserted that one of the major achievements of general equilibrium theory is its ability to demonstrate that the competitive mechanism is remarkably parsimonious in terms of the amount of information that it uses. Such an observation is couched in terms of the mechanism design literature because of Hurwicz and others (for a discussion of the mechanism literature, see Lee, this volume). A mechanism for attaining allocations for a given economic environment specifies the messages that have to be transmitted by the economy and then a mapping that translates those messages into allocations. The measure of how much information such a mechanism uses is

considered to be the dimension of the message space. Suppose that we are interested in attaining Pareto efficient outcomes. We know that the outcomes of the competitive process achieve this. The standard theorem—derived, in its most general form, from 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 this is so, consider a simple exchange economy. At equilibrium every agent except one (since at equilibrium it is enough to know the excess demands of n-1 agents) needs to transmit his vector of excess demands (l-1 goods because of Walras's law), and, thanks to the homogeneity of degree zero of excess demand functions, a vector of l- 1 prices is needed. This result seems, at least at first sight, to justify the claims for the efficiency of the competitive market mechanism, since one can show that no other mechanism that achieves Pareto efficiency uses less information. This result can be extended to include cases in which we are interested in attaining allocations that satisfy other criteria.

For example, consider a very old problem in economics, that in which one is interested in mechanisms that not only will achieve Pareto efficiency but will also be "fair" in the sense that no individual prefers someone else's allocation to his or her own. This would seem to be a very demanding requirement in terms of information, since every individual must be able to compare his or her allocation with that of all the other agents. Nevertheless, one can show that a mechanism with a message space of only nl dimensions can achieve this result (Calsamiglia and Kirman 1993). In other words, the information required is hardly more than that used by the competitive mechanism to achieve Pareto efficient outcomes. The secret of this result is to use the competitive mechanism from an equal division of resources. Thus the Walrasian mechanism permits an enormous economy of information. One might argue that this is a strong reason for thinking that we have not come down the wrong road, and that those who defend the modern version of general equilibrium analysis are justified in doing so. In my view

this is wrong; to see why, one has to recall that the results just mentioned show only that little information is needed for the economy to function at equilibrium. But what this says is that if the economy is actually at an equilibrium, the amount of information needed for it to function is very limited indeed.

But what should interest us as economists is not only how informationally demanding the mechanism is at equilibrium but also how much information it requires to get there. We find ourselves back at the problem of stability, one that, and it is worth insisting on this again, Debreu consistently avoided and indeed later claimed that he had always considered to be beyond hope. Yet, as I have already said, it is surely the case that economic equilibria are of interest only if they can be attained through a reasonable adjustment process. So if we want to ask the question as to how much information is involved, we have to rethink the original problem in terms of finding an adjustment process that will modify the messages and be guaranteed to achieve Pareto efficient outcomes. As we have seen, the tâtonnement process from an economic point of view has a basic problem. The SMD result shows that the equilibria of economies are not necessarily stable with respect to that process. It is then natural to argue that 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 Steve Smale (1976) was that stability could be achieved only at the price of an enormous increase in the amount of information required.[2]

Smale's global Newton method extends standard methods to 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. It has two major drawbacks. First, it does not behave well in the interior of the domain that, in the case under consideration, is the space of all strictly positive prices. Second, as already mentioned, it uses a great deal of information. What is needed, at each point in time, is knowledge of all the partial

derivatives, and this greatly increases the size of the message space, without guaranteeing convergence from any arbitrary starting point. An additional objection would contend with the economic content of the process. While the original tâtonnement process has a natural interpretation, this is not the case, despite the efforts of Hal Varian (1977), for the Newton methods.

Do the informationally demanding requirements of the Newton method represent a necessary evil? Saari and Simon (1978) asked the following question: can one find what they called "locally effective price mechanisms," that is, ones that turn all economic equilibria into sinks or attractors and that use less information than the Newton methods? They proved, unfortunately, that this cannot be done. This is odd, since the generalized Newton method has the undesirable and restrictive property that it reduces excess demands monotonically and that any single market in equilibrium is kept in equilibrium. One might have hoped that, by relaxing this requirement, one could have found less informationally demanding mechanisms.

As Jim Jordan (1986) pointed out, all the alternative adjustment processes that had been constructed at the time he wrote had no natural economic interpretation. There have been many efforts to construct globally and universally stable price-adjustment processes since then, and in a certain sense Kazuya Kamiya (1990), Gerrit van der Laan and Dolf Talman (1987), Peter Flaschel (1991), Jean-Jacques Herings (1995), and Jan Tuinstra (2002) have succeeded. Yet if one looks closely at these results, there is always some feature 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 that 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. Herings'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 that will converge from any price vector independent of the economy. Indeed, one characterization of Saari and Simon's result is to say that any adjustment process that would lead to an equilibrium from an arbitrary initial price vector must use an infinite amount of information![3]

Where does all this leave us? It appears that the informational requirements of adjustment processes are so extreme that only economy-specific processes are plausible. 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. If we are interested in describing a stable system, then the arguments for the general equilibrium model as one that is informationally efficient are illusory. The very fact that we observe, in reality, increasing amounts of resources being devoted to informational acquisition and processing implies that the standard general equilibrium model and the standard models of financial markets are failing to capture important aspects of reality. 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, but this track has been regarded as a detour from the mainstream.

The Nature of Prices and the Stability Problem

What I have just argued is that there is something problematic about the very structure of the general equilibrium model if we want to go beyond mere existence of equilibrium. The real issue here is much deeper, however, and the difficulties with stability that I have outlined are symptomatic of more fundamental conceptual problems. With very few exceptions, the literature on the stability of economic equilibrium considers the evolution over time of a price vector, the dimension of which corresponds to the number of commodities. Yet in the Arrow-Debreu model such time

is undefined. Time is arguably present in that commodities are dated, but to consider adjustment to the equilibrium prices we need another notion of time. With respect to time in the model, adjustment must be instantaneous. One could think of this as physicists do, of equilibrium prices being on a surface and of an adjustment process, by rationing quantities, for example, as leading instantaneously to that surface.[4] Yet this well-known problem simply reflects the unrealistic orientation of the basic model. More disturbing are 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 vector of prices of commodities at any one time. Different prices coexist for the same commodity unless one takes absolutely literally the definition of commodity in the theory of value. Individuals set these prices, and they modify them according to what is happening on the market. Rare are the markets in which some central clearing mechanism exists to match demand and supply. Consider financial markets that function on a double auction 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. 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 that may replace those made in the previous period. What is important here is that the trades taking place at each point reveal information. Why did that trader purchase that share at a price that 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. (See Chamley 2004 for a comprehensive account of this problem.) The contrast with the Arrow-Debreu model is striking. For example, transactions take place sequentially, and, at each point, agents observe what other agents are doing. They then obtain information, and

this influences their subsequent conduct. Thus there is interaction among the agents other than through a price system that everyone accepts as given. Such interaction is an essential feature of the real economy but is largely absent from economic models, and this is particularly true for most standard macroeconomic models.

Was the abstraction from such considerations an inevitable result of the formalizing process that culminated in the Arrow-Debreu result? If one reads the Chipman et al. (1971) volume to which I referred earlier, one is struck by the insistence on the idea that the arguments presented are derived from the work of Giovanni Antonelli, Vilfredo Pareto, Eugen Slutsky, Paul Samuelson, and Hendrik Houthakker. So one might have the impression that these pioneers and others along the road were not really interested in how markets actually function. Two observations can be made about this. First, this is a carefully selected list of pioneers, each of whom indeed marked a milepost on the route to the axiomatic approach. However, there were many others such as Walras himself, Pareto in other phases, Marshall, and Hicks who had a very different vision of how the economy should be modeled and devoted a great deal of their work to the functioning of markets. Again, one should insert a caveat at this point: although there was much discussion of the nature and functions of markets, there was rather little discussion of the empirical realities of market functioning. An example of this is Walras's description of the Paris bourse, which, as Walker (2005, 141–42) points out, shows an important misunderstanding of the way that institution functioned. Trade took place at nonequilibrium prices, and even the notion of what the equilibrium price was is not well defined. This did not stop Walras and many of his successors from arguing that their purpose was to understand real economic phenomena. Even the literature from Working 1927 on, discussing the estimation of empirical demand functions, does not have much discussion of what sort of markets generated the data. That literature was largely detached from the route I have been discussing but was, also, in many ways empirically deficient.[5]

It is an open question as to whether those who did not pursue the route all the way to the top were simply discouraged by the difficulty of the formalism or whether they were dissatisfied with the capacity of the theory to match the empirical facts. What is probably beyond dispute is that those who did follow the high road to the axiomatic approach were those who were less concerned with the practical details of economic reality. Yet even when one finds oneself on a peaceful peak in the intellectual landscape, this should not prevent one from tracing the route back to the busy place from which one started. What is important is not to lose sight of the fact that there may be other routes, still much traveled, which lead elsewhere and further. In pursuing a particular route one may lose sight of important features of the landscape. As I have just mentioned, the problem of who sets the prices is absent from the Arrow-Debreu model, and the treatments of this question by economists are worth examining in this regard.

One could argue that the most important question that disappeared as the perfectly competitive model came to dominate the neoclassical scene was who sets the prices? The fiction of an "auctioneer" is not an adequate description for many markets, and the basic problem of where prices come from is only treated, and this is significant, in what are described as models of "imperfect competition." If the economic agents set prices themselves, then we clearly have to dispense with the notion that they take them as given and to return to the previous discussion, which, in turn, was not consistent with the mechanism concept through which informational efficiency is obtained. Although the idea that prices adjust in some systematic way in response to excess demand has been commonplace, since the latter part of the nineteenth century little attention has been paid to how this is done.

Of course, there is a literature in which individuals meet, bargain, trade, and then continue to circulate in the market until all profitable opportunities are exhausted. The work of Fisher (1989), and of Ariel Rubinstein and Asher Wolinsky (1985), is very much in this spirit. Under certain conditions, such meeting and bargaining processes will

converge to the competitive outcome. However, prices now play a very different role, as they are the results of bargains between individuals and are not anonymous signals observed by individuals in isolation. Only as this process continues over time can the prices be thought of as indicating the terms of trade available in the market.

There is a further literature that recognizes explicitly that different prices may exist for the same good on a market (see, e.g., Diamond 1989) and then looks at what might be an equilibrium in such a market in which individuals search, at a cost, to find the lowest price. A number of authors have shown that price dispersion may persist. Here one can take the view that the market signal is now the distribution of prices rather than the price itself, but these models can, at least, allow for the sellers to set their prices. The only point that I am making here is that along the road from Walras to Arrow-Debreu there were many turns, among these ones that sought to take explicit account of how prices are formed. For those of us interested in how the economy's activities are coordinated, those other routes now look more attractive.

Market Organization

Up to this point, the only feature of the economy that somehow got lost on the road to the pure general equilibrium model is how prices are set, and it has to be admitted that our theoretical ancestors did not have a great deal to say about this either. But much more basic is the question as to how the trades necessary to clear the markets are organized. In the absence of any central authority, how do those with excess supply at the competitive price find and trade with those with excess demands? Except for very special markets such as that for gold, the "Walrasian auctioneer" who equilibrates the market after receiving bids and offers from all agents cannot be invoked as a reasonably realistic description. What this suggests is that market organization is important and that the prices obtaining in a market and their informational content may well depend on that outcome. As Walker (1996) clearly explains, Walras was preoccupied with how

exchanges and communication in markets are organized. Although the standard paradigm referred to as "Walrasian" is normally taken to be one in which individuals receive prices from some unspecified source such as an auctioneer, Walras in fact never specified such an auctioneer, and, indeed, each of his models specifies carefully how individuals meet and change prices. Marshall (1938) is also at pains to point out the importance of how markets are organized for economic outcomes. Friedrich Hayek (1989) suggested that organization, or "order," is an emergent phenomenon and argued that what is known as the market, in modern terms, was but one form of spontaneous order. He maintained that this form of organization was superior to others, thereby making explicitly the point that the efficiency of outcomes is intimately linked to how the economy is organized.

For example, consider an individual who wishes to sell one or many goods (the classic example is that of the sale of government bonds). The individual has a choice of different mechanisms to do this. Which one should he or she choose to maximize revenue? A number of authors showed some time ago (see, e.g., Riley and Samuelson 1981, Milgrom and Weber 1982, or McAfee and McMillan 1987) that, in the absence of transaction costs, an auction with reserve price is the optimal selling institution in the sense that it achieves the highest yield to the seller. Once costs are introduced, the situation becomes more complicated, and indeed it has been shown that posted prices may be preferable if the costs of implementing auctions are too high.

The point insisted on here is that the relationship between prices and market organization is important. Since different market mechanisms can yield different prices for the same good, the role of prices needs to be reevaluated. Furthermore, it would seem the price achieved will depend on the information available in the market. If individuals' evaluations of goods are the same, neither the prices nor the optimal mechanism will be the same as in the case where values are private, particularly if they are not drawn from some known distribution. Eric Maskin and John Riley (1989)

demonstrate how, by using a clever pricing schedule, a monopolist who has several units to sell can achieve a better yield than by using an auction if his clients have downward-sloping "demand curves." The quotation marks are deliberate, since demand has to be carefully defined in this sort of situation. Thus even in the case where we accept a standard model in which consumers' demand is derived from the maximization of utility, the analytic categories of mechanism, information, and price are intimately related. One cannot talk of equilibrium prices independently of the market organization. If one accepts this point, then the assumption of the anonymous, perfectly competitive market is far from innocuous. Setting on one side the nature of markets leaves a significant hole in any attempt to explain economic phenomena.

Indeed, the major contribution to economic thought made by Douglass North (1990) was to insist that market institutions are important. To the anthropologist, such an assertion seems trivial.[6] But for the theoretical economist it is much less so. North is at pains to explain that closer analysis of institutions is essential to economic history, while economic theory tends to focus on timeless and frictionless transactions. He suggests, therefore, that institutional analysis is an appropriate way to use some of the lessons from standard economic theory in economic history but does not go on to suggest that theory itself might need to be substantially modified in light of the institutional considerations that he raises. His emphasis is on the constraints and incentives provided by institutional organization but not on the need to pursue different avenues than that associated with the general equilibrium approach.

In fact, I would be inclined to follow Mirowski (2006) and suggest that what we are faced with is a multitude of coexisting institutions that we may care to call markets, but that there is no "standard market." Each institution evolved for social, cultural, and economic reasons, and they may produce very different results. The networks of traders that develop vary across markets, and even where the market is supposed to be essentially anonymous, as in many electronic markets, traders often concentrate rapidly

on a small group with whom they interact regularly. Each market with its own characteristics and network is linked to others, and it is this tissue of trade that organizes itself to govern transactions and the allocation of resources.[7] Demand, in this view, is not defined in any simple and general way but depends on the institutional framework in which it is expressed. While this picture corresponds to Walras's preoccupations with the interaction between various markets, it is surely very different from the portrait that he inscribed in his equations.

Time and Its Role

I have left to last two problems that seem to me to be crucial in the history of demand, the role of time and social interaction. As I have mentioned, time has no real role in the Arrow-Debreu model other than to date commodities. Any notion of an evolution of the system is largely absent. Just dating a finite number of commodities is not a serious acknowledgment of the temporal character of experience, and the extension by Truman Bewley (1972) to an infinity of commodities does not solve the problem of how people deal with the future. Yet if we cannot say something clear about this, our notion of demand is simply not well defined.

To assume that preferences are given from the outset, when time is present in the model, is clearly unrealistic. There has been widespread recognition within recent economic theory that the economic agent is not an unchanging individual whose characteristics are captured by a well-defined set of preferences that do not evolve over time. This recognition unfortunately has not had much impact on economic theory. Having preferences fixed once and for all time involves a thought experiment that represents a choice between alternative selves tied to time and circumstances (Mirlees 1982, 65). This requires, as James Mirlees observes, "that the agent's

> preference regarding what he will be doing at one particular time in one particular set of circumstances be independent of what he may be planning for

> all other times and circumstances". . . . [He thus acknowledges that] everything that has to do with life as a connected whole—such as habit, memory, preparation for future action, anticipation, achievement and failure—seems to have been ignored. (66)

Yet to consider preferences that evolve over time is a serious move away from the structure of the general equilibrium model. At one point, Guinness had a rather successful advertising campaign based on the slogan "I don't like Guinness. That's why I have never tried it." This line, which always provokes an amused reaction, should not seem absurd to an economist. Since we have well-defined preferences over the whole goods space, this sort of statement is perfectly consistent. In the standard model preferences are not formed by experience; they are given from the outset. The agent's characteristics are, in this respect, fixed and immutable, and independent of those around him. This is also an idea with early origins. Thomas Hobbes ([1651] 1949,) said, "Let us return to the state of nature and consider men as if sprung out of the earth, and suddenly, like mushrooms, come to full maturity without any kind of engagement to each other." The idea that preferences might depend on experience, and even other changing features of the environment, poses a real question when one is talking about intertemporal optimization and, in particular, the causal material from which current demand is derived. The idea of retaining the one-period utility function and then simply maximizing the sum or integral of the discounted utilities over time, proposed by Samuelson (1937), added more (unrealistic) structure to the preferences of individuals but did not necessarily lead to stepping out of the basic framework. He himself was surprised that this simplistic idea encountered so much success. Yet this attempt to add dynamic properties to the general equilibrium model has run into major difficulties. Experiments have shown that people seem to violate the conclusions of this extended model, and, recently, much attention has been given to the fact that

individuals seem to place undue weight on the near future, and for many philosophers this vision of the human is simply incoherent (see Frederick, Loewenstein, and O'Donoghue 2002 for a comprehensive and elegant account of such problems). Despite valiant attempts to rescue the model by introducing such notions as "hyperbolic discounting," too many questions remain. Those who investigate this kind of question have joined the alternative paths of the psychologists and philosophers.

To put the problem in perspective, think of the Arrow-Debreu world. If one wants to preserve that framework, one has to say what it is that individuals wish to optimize to make their choices. If their future preferences are subject to complicated changes, which are, in part, endogenous, the problem becomes overwhelming. Within the standard model we avoid this by assuming that preferences, wherever they come from, are unchanging. Yet the latter assumption was certainly not that made by some of our distinguished predecessors. To show that the route goes back a long way, consider the following quotation from Plato:

> A man is said to be the same person from childhood until he is advanced in years: yet though he is called the same he does not at any time possess the same properties; he is continually becoming a new person . . . not only in his body but in his soul besides we find none of his manners or habits, his opinions, desires, pleasures, pains or fears, ever abiding the same in his particular self, some things grow in him while others perish. (Symposium, 207D–208B)

Further down this road, David Hume ([1739–40] 2000, 1.4.6.3) states categorically that when he looks into his most intimate self, he detects "nothing but a bundle or collection of different perceptions, which succeed each other with an inconceivable rapidity, and are in perpetual flux and movement." There is nothing that remains the same over time. Even though each of us believes (or likes to believe) ourselves to be the same and

identical person over time, our supposed identity is built up through continuous change. For Hume, we are like a theater in which there is a continuing sequence of plays and scenes. To think like this does not represent a revolution but simply steps on a route, which is very different from that leading to Arrow-Debreu but nevertheless long and well trodden. There is an extensive literature on the idea that an individual's identity changes over time, and, indeed, philosophers and economists have been preoccupied by what it is that does not change.[8]

Now, if we agree on the fact that in the face of an individual's future identity, which will reflect little of his or her current tastes, that individual is faced with a task of momentous proportions in formulating choices, then either we have to embark on one of the numerous alternative routes or we have to find a way to stay on the general equilibrium standard rationality one. How might we save our optimizing agents in the complicated problem of having to optimize with respect to preferences that are evolving over time? There is one idea that might justify staying where the general equilibrium path has taken us: that individuals do not optimize but simply learn from experience how to make the "best choices."[9] The economic individual makes choices according to simple rules developed from his or her experiences. Thus individuals are not optimizers, they are adaptive and only behave "as if" they optimize. If the underlying preferences change, the individual will learn to make choices that accommodate this.

Learning to Be Rational

This view is admirably summarized by Robert Lucas (1986, S401) when he says,

> In general we view, or model, an individual as a collection of decision rules (rules that dictate the action to be taken in given situations) and a set of preferences used to evaluate the outcomes arising from particular

> situation-action combinations. These decision rules are continuously under review and revision: new decisions are tried and tested against experience, and rules that produce desirable outcomes supplant those that do not. I use the term "adaptive" to refer to this trial-and-error process through which our modes of behaviour are determined.

However, Lucas then goes on to argue that we can safely ignore the dynamics of this process because, "technically, I think of economics as studying decision rules that are steady states of some adaptive process, decision rules that are found to work over a range of situations and hence are no longer revised appreciably as more experience accumulates."

Michel De Vroey (1998) has described Lucas's point of view as one of "benign neglect." However, it is more than that. His argument is, rather, that the evolution of the economic environment is much slower than the speed at which agents adjust to that evolution; hence the two processes can safely be separated. However, this position is open to a few objections. First, even if we were prepared to accept that the collective result of this adaptive, individual interaction converged, it has to be proved that the result will be some standard equilibrium, whether Walrasian or Nash. In that case we have a justification for focusing on the equilibrium and no longer have to worry about the path to it. Second, it may well be the case that the changes in the economic environment are at least in part caused by the modifications in individual behavior arising from their adaptation. In this case the environment will evolve together with individual behavior. The whole system will then be in continual evolution. Thus the configuration of actions being taken by the players may evolve in a complicated way as the result of their collective impact on each other. It may well happen that in some cases, the system converges to an equilibrium of the underlying static game or

economy.[10] But, in general, unless the individuals involved learn extremely rapidly, the other considerations that I have mentioned enter into play.

Thus the "learning to be rational" concept does not seem to adequately answer the difficulties that we find with the model. In particular, there is one question that is fatal: if we take the general equilibrium model at face value, when does the learning take place? Once again, we see how the model's basic structure produces its own difficulties.

The Economic Individual and the Social Context

There has been an increasing recognition within recent economic theory 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 will have consequences for the agent's behavior. Society changes, and with preferences socially influenced, and they in turn influencing the common values of a society (Becker and Murphy 2000**,** Sen 1999), the individual must somehow take account of this when he or she makes decisions.

At the outset I argued that a major drawback of general equilibrium theory is that it assumes that the only interaction among economic agents is through the price system. It is not reasonable to assume that the only social influences 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 staying confined 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". (*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 Hans Föllmer (1974), and a good account is given by Buz Brock and Stephen Durlauf (2001).

However, most important from the point of view of the argument here is that in models when agents are too dependent, if a price equilibrium exists, it may depend in a crucial way on the distribution of preferences that results from interaction, even if the agents are identical a priori. So if our aim is really to characterize aggregate behavior, we have to heed Lucas's (1986, S411) advice when he said, "Applications of economic theory to market or group behavior require assumptions about the mode of interaction among agents as well as about individual behavior."

Interaction and Aggregate Behavior

The theme that has run through the whole of this discussion is the 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 general equilibrium model that macro or aggregate behavior must be derived from underlying rational microfoundations, 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 behavior in that model. 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 the interaction between the individuals and how this is organized.

The discussion of the role of interaction between individuals, whether they be neurons, molecules, or insects, is very much present in other disciplines. There is, for

example, a considerable debate as to whether, in the neurosciences, it is necessary to revert to the study of the behavior of neurons to explain thought processes (see, e.g., Churchland and Sejnowski 1995). However, the situation in economics is complicated by the fact that the practice of analyzing macro relationships, without considering their microfoundations, is now, in economics, almost universally considered as "unscientific."

In economics we are not faced with unthinking particles, or molecules or even social insects; we are faced with beings who have intentions, and a major problem is what happens if interaction is actually chosen consciously. Thus it is not only the interaction that matters but the very nature of the individual and his or her conscious choices concerning that individual and the surrounding community.[11] This leads to a vision very far from that of the Arrow-Debreu world and certainly far from the fixed preferences attributed to the individual in that model. Preferences have to be defined over some set of possibilities, and choices among those possibilities can, in a more realistic world than that depicted by Arrow-Debreu, be much more complicated than just choosing the best element in a budget set.

Once we consider the individual as embedded in a social context, possibly of his or her own choosing, other considerations enter into play; the individual may be concerned with his or her social image (see Bernheim 1995; Akerlof and Kranton 2000, 2002), and this will change over time as the individual and society change. All of this leads once again to a vista of the world far from the one visible from the peak of the general equilibrium mountain.

Conclusion

The purpose of this article has been to show that general equilibrium theory and the demand theory contained in it have not provided a model that has empirically testable content and thus will allow us to explain economic phenomena. Yet this must surely be

the goal of any economist. The theory that we have developed is self-contained and logically consistent but has little to do with the actual functioning of the economy and its markets. The search for empirically refutable propositions has not been a high priority. We have reached an isolated peak far from the other challenging hills that we might profitably try to climb. Sometimes, in moments of weakness, I conjure up the vision of one of those sects that gather on a mountaintop, absolutely sure of their destiny, waiting for the end of the world. However, I would not venture to propose that image as a description of even the most enthusiastic general equilibrium specialist. My pessimistic view is not shared by everyone, and there are distinguished economists who believe that calibrating general equilibrium models may provide a way out".

> The Walrasian model of competition, even though it is sufficiently flexible to incorporate a number of formal modifications, is far from being the exclusive analytical framework for the study of microeconomic problems. But it is an important method of analysis and one whose usefulness, we hope, will be enhanced by the ability to obtain specific numerical solutions.
>
> *Herb Scarf (1973)*

Yet my basic feeling is that in the epigraph at the beginning of this paper, Debreu put his finger on the essential phenomenon that we would like to explain and that all the work by distinguished theoretical economists in this direction has produced essentially no reply. Lest the reader feel that I have been too negative, let me quote from Bob Clower and Peter Howitt (1997,) who believe that the whole enterprise was compromised early on: "Were we seriously, by a simple phrase, to attempt to characterise the modern age—the age of Keynes, Samuelson, we should call it the age of delusion, because it seems to involve nothing so much as a generalised delusion that sheer analytical technique might somehow permit us to resolve most of our problems."

None of this is an argument in favor of taking a road along which there is less rigorous thinking. Our problem is much more that we have developed our models using certain mathematical techniques and that we have become slaves to those techniques. It is surely this more than anything else that has led us to persist with a model that, to any outsider, seems such a poor description of what actually happens in markets. The real world is one in which various market forms coexist, where different prices for goods are observed, and where the individuals who participate have only very local information. The appropriate notion of demand in such a world is certainly not close to the definition that we find in general equilibrium theory, but this does not make it any less interesting to analyze. The only drawback is that we may have a lot more intellectual climbing to do.

Footnotes

1. The recent burgeoning of interest in the relation between psychology and economics (see, e.g., Rabin 1998) might seem to be a counterexample, but this development is hardly relevant to the developments that I am discussing here.

2. For a full account of the problems outlined here, see Flaschel 1991.

3. Those who have worked on this problem do not always agree with Saari and Simon's definition of an adjustment mechanism. Flaschel's mechanism, for example, does not satisfy their definition.

4. Michael Blad and I made an effort in this direction, but it had little echo (see Blad and Kirman 1985).

5. See Chipman, this volume.

6. See, for just one example, Clifford Geertz's (1978) remarkable work on the functioning of the Moroccan bazaar.

7. To look at this requires examining empirical data from real markets. In Weisbuch et al. 2000, for example, we analyzed the evolution of trading relations on a specific market, the wholesale fish market in Marseille.

8. See Sen 1999, Davis 2003, and Parfit 1984.

9. Even to say "best choices" implies that they are best with respect to some criterion, and it is not obvious what this means if preferences evolve relatively fast.

10. For various accounts of this sort of problem, see Kirman and Salmon 1995.

11. I will leave to one side the problem of "self image," which may have an important effect on people's choices (see Bénabou and Tirole 2002, 2003; Köszegi 1999; and Bodner and Prelec 2003).

References

Afriat, S. 1967. The Construction of Utility Functions from Demand Data. International Economic Review 8:67–77.

Bewley, T. 1972. Existence of Equilibria in Economies with Infinitely Many Commodities. Journal of Economic Theory 4:514–40.

Billette de Villemeur, E. 1998. Heterogeneity and Stability: Variations on Scarf's Processes. PhD thesis, European University, EUI Florence.

Blad, M., and A. Kirman. 1985. L'évolution à long terme d'un modèle d'èquilibre avec rationnement. Revue d'économie politique 6:795–808.

Brock, W., and S. Durlauf. 2001. Interactions-Based Models. In vol. 5 of Handbook of Econometrics, edited by J. Heckman and E. Leamer. Amsterdam: North-Holland.

Brown, D. J., and R. L. Matzkin. 1996. Testable Restrictions on the Equilibrium Manifold. Econometrica 64.6:1249–62.

Calsamiglia, X., and A. Kirman. 1993. A Unique Informationally Efficient and Decentralized Mechanism with Fair Outcomes. Econometrica 61.5:1147–72.

Chamley, C. 2004. Rational Herds. Cambridge: Cambridge University Press.

Chipman, J. S. 1971. Introduction to part 2 of Preferences, Utility, and Demand. In Chipman et al. 1971.

———. 1976. The Paretian Heritage. Revue europeene des sciences sociales 14.37:65–173.

Chipman, J. S., L. Hurwicz, M. K. Richter, and H. F. Sonnenschein, eds. 1971. Preferences, Utility, and Demand: A Minnesota Symposium. New York: Harcourt Brace Jovanovich.

Churchland, P., and T. Sejnowski. 1995. The Computational Brain. Cambridge, Mass.: MIT Press.

Clower, R., and P. Howitt. 1997. Foundations of Economics. In Is Economics Becoming a Hard Science? edited by A. D'Autume and J. Cartelier. Cheltenham, U.K.: Elgar.

Cohen, I. B. 1994. Interactions. Cambridge, Mass.: MIT Press.

Davis, J. B. 2003. The Theory of the Individual in Economics: Identity and Values. New York: Routledge.

Debreu, G. 1959. Theory of Value: An Axiomatic Analysis of Economic Equilibrium. New York: Wiley.

———. 1974. Excess Demand Functions. Journal of Mathematical Economics 1:15–23.

———. 1998. Existence. In Elements of General Equilibrium Analysis, edited by A. Kirman. Cambridge: Cambridge University Press.

De Vroey, M. 1998. Is the Tâtonnement Hypothesis a Good Caricature of Market Forces? Journal of Economic Methodology 5:201–22.

Diamond, P. 1989. Search Theory. In The New Palgrave: A Dictionary of Economics, edited by J. Eatwell, M. Milgate, and P. Newman. London: Macmillan.

Fisher, F. M. 1989. Disequilibrium Foundations of Equilibrium Economics. Cambridge: Cambridge University Press.

Flaschel, P. 1991. Stability—Independent of Economic Structure? Structural Change and Economic Dynamics 2.1:9–35.

Föllmer, H. 1974. Random Economies with Many Interacting Agents. Journal of Mathematical Economics 1.1:51–62.

Frederick, S., G. Loewenstein, and T. O'Donoghue. 2002. Time Discounting and Time Preference: A Critical Review. Journal of Economic Literature 40:351–401.

Geertz, C. 1978. The Bazaar Economy: Information and Search in Peasant Marketing. American Economic Review 68:28–32.

Grandmont, J.-M. 1987. Distributions of Preferences and the "Law of Demand." Econometrica 55.1:155–61.

———. 1992. Transformations of the Commodity Space, Behavioural Heterogeneity, and the Aggregation Problem. Journal of Economic Theory 57:1–35.

Hahn, F. 1974. On the Notion of Equilibrium in Economics: An Inaugural Lecture. London: Cambridge University Press.

———. 2002. On the Possibility of Economic Dynamics. In Equilibrium, Markets, and Dynamics: Essays in Honour of Claus Weddepohl, edited by C. H. Hommes, R. Ramer, and C. A. Withagen. Berlin: Springer-Verlag.

Hayek, F. A. 1989. Spontaneous ("Grown") Order and Organized ("Made") Order. In Order—with or without Design? edited by N. Modlovsky. London: Centre for Research into Communist Economies.

Herings, J.-J. 1997. A Globally and Universally Stable Price Adjustment Process. Journal of Mathematical Economics 27:163–93.

Hicks, J. R. 1939. Value and Capital. Oxford: Clarendon.

Hildenbrand, W. 1971. On Random Preferences and Equilibrium Analysis. Journal of Economic Theory 3:414–29.

———. 1983. On the "Law of Demand." Econometrica 51.4:997–1019.

———. 1994. Market Demand: Theory and Empirical Evidence. Princeton, N.J.: Princeton University Press.

Hildenbrand, W., and A. Kneip. 2005. On Behavioral Heterogeneity. Economic Theory 25:155–69.

Hobbes, T. [1651] 1949. De Cive, the Citizen. New York: Appleton Century Crofts.

Hume, D. [1739–40] 2000. A Treatise of Human Nature. Edited by D. F. Norton and M. J. Norton. Oxford: Oxford University Press.

Hurwicz, L. 1986. On Informational Decentralization and Efficiency in Resource Allocation Mechanisms. In Studies in Mathematical Economics, edited by S. Reiter. Vol. 25 of MAA Studies in Mathematics. Washington, D.C.: Mathematical Association of America.

Ingrao, B., and G. Israel. 1990. The Invisible Hand. Cambridge, Mass.: MIT Press.

Jevons, W. S. 1905. The Principles of Economics. In A Fragment of a Treatise on the Industrial Mechanism of Society and Other Papers, edited by H. Higgs. London: Macmillan.

Jordan, J. S. 1982. The Competitive Allocation Process Is Informationally Efficient Uniquely. Journal of Economic Theory 28:1–18.

———. 1986. Instability in the Implementation of Walrasian Allocations. Journal of Economic Theory 28:1–18.

Kamiya, K. 1990. A Globally Stable Price Adjustment Process. Econometrica 58.6:1481–85.

Kirman, A. 1992. What or Whom Does the Representative Individual Represent? Journal of Economic Perspectives 6.2:117–36.

Kirman, A., and K. J. Koch. 1986. Market Excess Demand Functions in Exchange Economies with Identical Preferences and Collinear Endowments. Review of Economic Studies 53:457–73.

Kirman, A., and M. Salmon. 1995. Learning and Rationality in Economics. Oxford: Blackwell.

Koopmans, T. 1957. Three Essays on the State of Economic Science. New York: McGraw-Hill.

Lucas, R. 1986. Adaptive Behavior and Economic Theory. Journal of Business 59:S401–S426.

Mantel, R. 1974. On the Characterisation of Aggregate Excess Demand. Journal of Economic Theory 7.3:348–53.

Marshall, A. 1938. Principles of Economics: An Introductory Volume. 8th ed. London: Macmillan.

Mas-Colell, A., M. D. Whinston, and J. R. Green. 1995. Microeconomic Theory. Oxford: Oxford University Press.

Maskin, E., and J. Riley. 1989. Optimal Multi-unit Auctions. In The Economics of Missing Markets, Information, and Games, edited by F. Hahn. New York: Oxford University Press.

McAfee, R. P., and J. McMillan. 1987. Auctions and Bidding. Journal of Economic Literature 25:699–738.

Milgrom, P., and R. Weber. 1982. A Theory of Auctions and Competitive Bidding. Econometrica 50.5:1089–1122.

Mirlees, J. 1982. The Economic Uses of Utilitarianism. In Utilitarianism and Beyond, edited by Amartya Sen and Bernard Williams. Cambridge: Cambridge University Press.

Mirowski, P. 1989. More Heat Than Light: Economics as Social Physics, Physics as Nature's Economics. Cambridge: Cambridge University Press.

———. 2006. Markets Come to Bits. Journal of Economic Behavior and Organization.

Mirowski, P., and E. R. Weintraub. 1994. The Pure and the Applied: Bourbakism Comes to America. Science in Context 7.2:245–72.

Morishima, M. 1964. Equilibrium, Stability, and Growth: A Multi-sectoral Analysis. Oxford: Clarendon.

North, D. 1977. Markets and Other Allocation Systems in History. Journal of European Economic History 6:703–16.

———. 1990. Institutions, Institutional Change, and Economic Performance. Cambridge: Cambridge University Press.

Pareto, V. 1953. The Economic Phenomenon. International Economic Papers no. 3. London: Macmillan.

Parfit, D. 1984. Reasons and Persons. Oxford: Oxford University Press.

Rabin, M. 1998. Psychology and Economics. Journal of Economic Literature 36:11–46.

Riley, J., and W. Samuelson. 1981. Optimal Auctions. American Economic Review 71:381–92.

Robbins, L. 1935. An Essay on the Nature and Significance of Economic Science. London: Macmillan.

Rubinstein, A., and A. Wolinsky. 1985. Equilibrium in a Market with Sequential Bargaining. Econometrica 53.5:1133–50.

Saari, D., and C. P. Simon. 1978. Effective Price Mechanisms. Econometrica 46.5:1097–1125.

Samuelson, P. 1937. A Note on Measurement of Utility. Review of Economic Studies 4:155–61.

Scarf, H. 1960. Some Examples of Global Instability of the Competitive Equilibrium. International Economic Review 1:157–72.

Sen, A. 1999. Reason before Identity: The Romanes Lecture. New York: Oxford University Press.

Smale, S. 1976. Exchange Processes with Price Adjustment. Journal of Mathematical Economics 3:211–26.

Sonnenschein, H. 1972. Market Excess Demand Functions. Econometrica 40.3:549–56.

Stoker, T. 1995. Empirical Approaches to the Problem of Aggregation over Individuals. Journal of Economic Literature 31:1827–74.

Tuinstra, J. 2002. Nonlinear Dynamics and the Stability of Competitive Equilibria. In Equilibrium, Markets, and Dynamics: Essays in Honour of Claus Weddepohl, C. H. Hommes, R. Ramer, and C. A. Withagen. Berlin: Springer-Verlag.

Uzawa, H. 1971. Preferences and Rational Choice in the Theory of Consumption. In Preferences, Utility, and Demand: A Minnesota Symposium, edited by J. S. Chipman, L. Hurwicz, M. K. Richter, and H. F. Sonnenschein. New York: Harcourt Brace Jovanovich.

Van der Laan, G., and A. J. J. Talman. 1987. A Convergent Price Adjustment Process. Economics Letters 23:119–23.

Varian, Hal R. 1977. Non-Walrasian Equilibria. Econometrica 45.3:573–90.

Walker, D. A. 1996. Walras's Market Models. Cambridge: Cambridge University Press.

———. 2005. Walrasian Economics. Cambridge: Cambridge University Press.

Working, E. J. 1927. What Do Statistical "Demand Curves" Show? Quarterly Journal of Economics 41:212–35.