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THE FORMATION OF ECONOMIC MAGNITUDES:

DISEQUILIBRIUM AND STABILITY

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Abstract

The analysis of disequilibrium, and especially, stability is essential if equilibrium economics is to be a useful tool and the formation of economic magnitudes understood. The subject is surveyed, with an eye to the "key question" of whether a competitive economy is necessarily driven to equilibrium by the actions of arbitraging agents. Too often analysis has rested with equilibrium modes of thought, being strongest when considering the formulation of plans by individual agents and weakest when analyzing what happens when those plans are frustrated. A more extensive treatment can be found in Fisher, *Disequilibrium Foundations of Equilibrium Economics* (Cambridge: Cambridge University Press, 1983).

Key words: General equilibrium, disequilibrium, stability

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# THE FORMATION OF ECONOMIC MAGNITUDES: DISEQUILIBRIUM AND STABILITY

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## 1. Introduction

The title of this Conference is "The Formation of Economic Magnitudes." Yet, if the papers prepared for it are true to the course of modern economic theory, most of them will, in a very real sense, not concern that subject at all. Modern economic theory is overwhelmingly a theory of equilibrium. It analyzes positions from which there is no incentive to depart, positions at which the plans and expectations of economic agents' are mutually compatible. It is almost silent on the question of how such positions get reached, on how economic magnitudes get formed if they do not happen already to be in equilibrium.

Equilibrium analysis is an elegant and powerful tool, providing considerable illumination of the way in which real economies operate. But the total concentration on equilibrium now characteristic of formal economic models runs the serious risk of misunderstanding the basic insights of economics itself. Thus, the proposition that competitive industries earn no profits in long-run equilibrium is an important (if elementary) theorem. To take this to mean that competitive industries *never* earn profits is not only wrong, it is to lose sight of the fundamental role that profits and losses play in the allocation of resources when

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demand or technology changes. The proposition that competitive equilibria and Pareto-optima are closely related is a basic insight. The policy presecription that (under the conditions of the two Welfare Theorems )government interference with a competitive system is bound to be inefficient requires more than this, however; it requires the assurance that competitive economies are close to equilibrium most of the time. That assurance cannot be provided by only examining the properties of equilibria.

Nor are such issues restricted to microeconomics. To take a leading modern example, the statement that agents will eventually learn about and act on systematic profit opportunities is an appealing assumption. The proposition of the rational expectations literature that agents always instantaneously understand the opportunities thrown up by an immensely complex and changing economy is breathtakingly stronger. That proposition begs the question of how agents learn and of the role that arbitrage plays in the formation of economic magnitudes. To take an older example, the proposition that, under some circumstances, there can exist underemployment equilibria was the major contribution of the Keynesian literature. To show that the economy can tend toward such equilibria is a much harder proposition, requiring analysis of dynamic, disequilibrium behavior.

Indeed, such dynamic, disequilibrium analysis is always required if we are to understand the formation of economic magnitudes. Certainly, if the economy does not spend most of its time near equlibrium, disequilibrium analysis is the only useful kind. Even if equilibrium is the usual case, however, disequilibrium analysis is indispensable. For one thing, only such analysis can provide the assurance that our equlibrium theories are consistent; if equilibrium is the usual case, we need to know why. Further, only analysis of the dynamic path that a stable system follows in disequilibrium can tell us to which of several possible equilibria that system will go. This is a matter of considerable importance, not only because multiplicity of equilibria is the rule rather than the exception, but also because, as we shall see, analysis of disequilibrium shows that the dynamic behavior involved often changes the equilibrium that is eventually reached.

There are two fairly common mistakes that must be avoided in considering such matters. First, one must not confuse the tautology that the economy will move away from positions that are not equilibria with the much deeper and unproven proposition that the economy always converges to equilibrium (let alone the proposition that it spends most of its time near equilibrium). In more specific terms, the fact that agents will seize on profitable arbitrage opportunities means that any situation in which such opportunities appear is subject to change. It does not follow that profitable arbitrage opportunities disappear or that new opportunities do not continually arise in the process of absorbing old ones.

The second mistake is the belief that such problems can be avoided by redefinition of terms so that there is no such thing as disequilibrium. For example, the non-clearing of markets by prices is sometimes said not to be an example of disequilibrium because agents form queues with the length of the queue determined by the shadow price of time as well as by money prices. This may be a valuable way to think about what happens when markets fail to clear, but it reformulates rather than solves that question. (What happens to money prices? How do the queues themselves disappear over time?) Certainly, there is a sense in which the disequilibrium behavior of any given system can be represented as the equilibrium behavior of a larger system in which the original one is embedded. To say this, however, is only to say that there is some definite outcome out of equilibrium in the smaller system. To insist that therefore there is no such thing as disequilibrium is to rob the term "equilibrium" and all equilibrium analysis of meaning. For if "equilibrium" is to be a useful concept in analyzing a particular system, then one must contemplate the possibility of points that are not equilibria of that system. The fact that such points can be represented as equilibria in some larger system does not change this.

If equilibrium analysis is to be justified, the crucial question that must first be answered is one of stability. That question in its most interesting and general form is as follows. Suppose an economy made up of agents who understand that they are in disequilibrium and perceive and act on profit opportunities. Does the action of those agents lead the economy to converge to equilibrium, and, if so, to what sort of equilibrium? I shall refer to this as the "key question" of stability analysis.

It is important to note, however, that, while stability of competitive general equilibrium is perhaps the only disequilibrium question addressed in a long literature, that literature has seldom addressed the key question directly. Rather, as we shall see, writings on the stability of general equilibrium have only recently endowed agents with much perception. Instead, agents have been supposed to make their plans as though disequilibrium did not exist, and the interaction of those plans has been modeled only as an afterthought at best.

Why should this be? The answer may be related to the phenomenon of concentration on equilibrium and to the distaste or at least disinterest with which many theorists regard the stability literature. Economic analysis is extremely powerful when considering the optimizing behavior of the individual agent. It is comfortable with positions in which the plans of those agents are mutually compatible. It must break untrodden ground to describe what happens when this is not so. This means modelling both the way in which trade takes place when agents plans cannot be completely fulfilled and how agents react to frustration. Neither aspect can be properly done by considering equilibrium behavior.

## 2. Tâtonnement and Its Failure

As already indicated, however, the study of stability has historically been marked by failure to model out-of-equilibrium behavior as more than an afterthought. That was particularly true of the development that characterized the first twenty years or so of the subject -- the study of tâtonnement.

It was P. A. Samuelson (1941) who took the first crucial step in the study of stability. Reacting to a suggestion of J. R. Hicks (1939) that "perfect stability" might be defined in terms of demand curves that slope down after various prices are allowed to adjust, Samuelson pointed out that there could be no study of stability without an explicit dynamic model. He assumed that price-adjustment takes place out of equilbrium by prices moving in the direction indicated by the corresponding excess demands[1], an assumption that can be written in its general form as:

$$\dot{p}_i = H^i(Z_i(p)) \qquad (i = 1, \ldots, c) \tag{1}$$

where there are c commodities, subscripted by i, p is the vector of prices, $Z_i(p)$ the excess demand for commodity i when prices are p, and the $H_i(.)$ are continuous and sign-preserving functions. (A dot over a variable denotes differentiation with respect to time.) Samuelson proposed the study of (1) as the only out-of-equilibrium adjustment mechanism.

Models of this type are known as "tâtonnement" models. They suffer from the obvious lack of reality of the assumption that only prices adjust out of equilibrium, with agents constantly recontracting rather than trading (let alone consuming and producing). Yet that assumption (which goes nicely with the fictitious Arrow-Debreu world in which all markets open and close at the dawn of time) may not be the most troublesome one for purposes of understanding disequilibrium behavior. Since price adjustment equations such as (1) are also characteristic of the later, non-tâtonnement literature, it is worth discussing this in detail.

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1. If price is zero and excess demand negative, price is assumed to remain zero. I generally ignore this complication in what follows.

Whose behavior does equation (1) represent? It cannot reflect directly the behavior of the individual agents whose demands are to be equilibrated. Indeed, we now see a central conundrum: In a perfectly competitive economy, all agents take prices as given and outside of their control. Then who changes prices? How do sellers know when demand or costs rise that they can safely raise prices without losing all their customers? At a formal level such questions are deep ones.

It only begs the price-adjustment question to say (as is often done) that (1) reflects the behavior of an "auctioneer" whose job it is to adjust prices in such a way.[2] Most real markets do not have such specialists. Those markets that do have them are such that the specialist is rewarded for his or her endeavors. To understand where and how such price-setting takes place requires analysis of how markets equilibrate. That cannot be done by adding (1) as an afterthought, nor is it likely to be done satisfactorily in the tâtonnement world where only prices adjust and there are no consequences to remaining in disequilibrium.

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2. The auctioneer may have been invented by J. Schumpeter in lectures at Harvard and was probably introduced into the literature by Samuelson. Despite the fact that the construct is often referred to as the "Walrasian auctioneer," it does not appear in the work of L. Walras (who did, however, suppose that prices adjust in the direction indicated by excess demands). Interestingly, F. Y. Edgeworth wrote (1881, p. 30): "You might suppose each dealer to write down his demand, how much of an article he would take at each price, without attempting to conceal his requirements; and these data having been furnished to a sort of market-machine, the price to be passionlessly evaluated." I am indebted to P. Newman for this reference.

The fact that there are no such conseqences provides some justification for the way in which the behavior of the agents themselves is treated in tâtonnement models. Disequilibrium never enters the dreams of those agents; they construct their excess demands as though prices are fixed and unchanging and as though their desired transactions will in fact take place. Since nothing happens until prices have adjusted to equilibrium (assuming that ever occurs), agents have nothing to gain by being more sophisticated about what is really happening.

Tâtonnement models, then, do little about the two basic facets of disequilibrium behavior. They model the out-of-equilibrium interaction of agents in terms of price adjustment only, without any basis for such adjustment mechanism. Further, since such an unsatisfactory adjustment mechanism does not permit agents to find their plans frustrated in any meaningful sense, there is no analysis of the way in which agents react to such frustration.

Despite these defects, the analysis of tâtonnement was the exclusive subject of the first twenty years or so of the stability literature (roughly 1940-60). This is understandable when one recalls that the subject was then in its infancy. Perhaps because the adjustment process in (1) seems the simplest case and perhaps because, even so, until the late 1950s major results seemed very hard to come by, no serious attention seems to have been paid in this period to the underlying defects of the model. What is more surprising is the casual view still sometimes encountered that stability analysis necessarily means the study of tâtonnement. Perhaps partly because of the obvious defects of the tâtonnement model and partly because of the total collapse of the tâtonnement effort in 1960, that casual view tends to be accompanied by a disdain for the entire subject of stability.

As just indicated, however, the late 1950s seemed a time of considerable promise for tâtonnement results. This was largely because of the introduction of Lyapounov's Second Method into the economics literature, rather than because of the attractive nature of the tâtonnement model itself.

Following Samuelson's introduction of equation (1), the literature (which was not voluminous) concentrated on the question of whether (1) was locally stable. Essentially, this is the question of whether (1) tends to converge to a rest point (a point at which $\dot{p} = 0$, here identical with a Walrasian equilibrium) if it begins close enough to that rest point. Such concentration on local properties seemed natural, for it allowed linear approximation and the properties of autonomous linear differential equations are completely known.

Less understandable save in historical terms was the early concentration on the relations between local stability of (1) and the conditions for Hicksian "perfect stability" -- an attribute that, as already mentioned, has nothing directly to do with stability at all. Those conditions -- the alternation in sign of the principal minors of the Jacobian of the excess demand functions -- were shown by Samuelson (1941, 1947) and L. Metzler (1945) to be equivalent to the local stability of (1) on the very strong assumption that all goods are gross substitutes (excess demand for any good goes up when the price of any other good increases).[3]

Since the alternation of the principal minors is not a particularly interpretable property, the Samuelson-Metzler results are properly be regarded as a lemma rather than a theorem, but it was a long while before any further progress was made. That was done independently by F. H. Hahn (1958) and T. Negishi (1958). Each of these authors realized that the economic structure of the problem could be further exploited and each showed -- Hahn using Walras' Law and Negishi the homogeneity of degree zero of the excess demand functions -- that the gross substitutes assumption itself implied the Hicks conditions on the principal minors and hence the local stability of (1).

This quite neat contribution was eclipsed, however, by the really big development of the late 1950s, the introduction of Lyapounov's Second Method.[4] This was done in a pair of papers by K. J. Arrow and L. Hurwicz (1958) and Arrow, H. D. Block, and Hurwicz (1959).

Lyapounov's Second Method works as follows. Continuing with (1) as an example of a differential equation, suppose that there exists a function, V(p), which is continuous, bounded

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3. Years later, D. McFadden (1968), writing in the Hicks *Festschrift*, showed that the Hicks conditions imply global stability of (1) on very strong assumptions about relative speeds of adjustment in different markets.

4. A. Lyapounov (1907). Lyapounov's "First Method" for proving stability is the explicit solution of the differential equations involved, an alternative never available at the level of generality of the stability literature.

below, and decreasing through time except at a rest point of (1). The existence of such a function, called a "Lyapounov function", implies that (1) is *quasi-stable*, that is, that every limit point of the time-path of p is a rest point. If that path can be shown to remain in a compact set, then p approaches the set of rest points. If, in addition, rest points are locally isolated or unique given the initial conditions, then (1) is a *globally stable* process; it converges to some rest point no matter where it starts. (Recall that the rest points of (1) are Walrasian equilibria.)[5]

This powerful tool was used by Arrow, Hurwicz, and Block to demonstrate the global stability of tâtonnement under apparently different strong restrictions on the excess demand functions. The first such restriction was that of gross substitutes, thus completing the early literature. Unfortunately, as we now realize, both this and nearly every other restriction considered was a special case of the assumption that the Weak Axiom of Revealed Preference applies to *market* demand functions -- a very strong restriction indeed. As a result, Arrow *et al.*'s conjecture that tâtonnement is *always* stable given only those restrictions (such as Walras' Law) that stem from the basic assumptions of microeconomic theory, was a bold one indeed.

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5. The limit point, however, generally depends on the initial conditions. For a more extended discussion as well as exact statements and proofs, see F. M. Fisher (1983). Note that G. Debreu has shown that local isolation of equilibria is true almost everywhere in the appropriate space of economies given certain differentiability assumptions.

In fact, that conjecture is wrong. H. Scarf (1960) quickly provided a counter-example of an exchange economy with non-pathological consumers in which (1) is not stable. As we now know from the work of H. Sonnenschein and others, that example implies the existence of an open set of economies for which a similar result holds.[6] Indeed, so far as anything useful is known, it appears to be that stability rather than instability of tâtonnement is a special case.

Scarf's counter-example was thus of major historical importance. Its true analytical importance today, however, is not often realized. Scarf did not show that stability analysis was guaranteed to be unfruitful. (Indeed, as we shall see, a very fruitful development immediately began in the early 1960s.) Rather Scarf showed that *tâtonnement* would not generally lead to stability. This means that the facile proposition that disequilibrium is cured by fast-enough price adjustment is not generally true (although, of course, it may be true in special circumstances).

If price adjustment alone is not sufficient to gurantee stability, however, then equilibrium economics must rest on the assumption that quantities also adjust. While, as we shall see,

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6. Sonnenschein (1972, 1973), Debreu (1974), and R. Mantel (1976) show that the basic assumptions of economic theory do not restrict the excess demand functions except by continuity, homogeneity of degree zero, and Walras' Law. Since Scarf's example shows that such restrictions do not imply stability of (1) and since properties such as the signs of the real parts of the eigenvalues of the Jacobian matrix of (1) are continuous, instability must hold on an open set.

such an assumption does indeed lead to more satisfactory stability results, it has a major consequence. When trade takes place out of equilibrium (and even more when disequilibrium production and consumption occur), the very adjustment process alters the equilibrium set.

This is easily seen even within the simplest model of pure exchange. In such a model, the equilibrium prices and allocations depend on the endowments. If trade takes place out of equilibrium, those endowments change. Hence, even if the trading process is globally stable, the equilibrium reached will generally not be one of those corresponding to the initial endowments in the static sense of the Walras correspondence. Rather the equilibrium reached will be path-dependent, dependent on the dynamics of the process taking place in disequilibrium.

If such effects are large, then the popular enterprise (ironically led by Scarf himself (1973)) of computing points of general equilibrium from the underlying data of the economy is quite misleading. The points computed by such algorithms are the equilibria corresponding statically to the initial endowments of the economy. They are not the equilibria to which the economy actually tends given those endowments. Hence such algorithms make dangerous predictive (or prescriptive) tools.

More important than this, the principal tool of equilibrium analysis -- comparative statics -- is called into question. Displacement of equilibrium will not be followed by convergence to the new equilibrium indicated by comparative statics. Rather it will be followed by a dynamic adjustment process which, if stable, generally converges to a different equilibrium. While comparative-statics results are not plentiful in general equilibrium, the foundation for such results, even in partial equilibrium, has become shaky.

Such out-of-equilibrium effects may, of course, be small. But we have no reason to believe that they are. The failure of tâtonnement means that we cannot escape by assuming that quantity-adjustment effects are negligible relative to price effects. The doubtful project of tacking anonymous price adjustment on to an equilibrium model is known to be a failure. Further progress requires more serious attention to what happens out of equilibrium, and we see that what happens out of equilibrium can have a serious effect on equilibrium itself.

## 3. Trading Processes: The Edgeworth Process

The failure of tâtonnement, however, does not imply the failure of stability analysis, and the early 1960s saw a the beginning of a more fruitful development. Not surprisingly, perhaps, that development involved a closer look at out-of-equilibrium behavior.[7] In particular, while (1) remained the equation supposedly explaining price adjustment, trade was now allowed to take place out of equilibrium, and some thought was

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7. The first paper to suggest (by example) that there might be considerable pay-off in a closer look at the adjustment process appears to have been Hahn (1961) which considered specialization of (1) instead of restrictions on excess demands as a way of making progress in tâtonnement. (See also A. Kagawa and K. Kuga, 1980.)

given to the specification of trading rules. The resulting models were called "non-tâtonnement" processes, but as that name is not particularly descriptive, I prefer to call them as "trading processes."

Trading processes made only a modest concession to realism in allowing trade to take place out of equilibrium. Households (the original models concerned only pure exchange) were permitted to trade endowments out of equilibrium, but no consumption could take place until equilibrium was reached. Indeed, the pre- and post-equilibrium situations were unnaturally separated, for equilibrium involved an exhaustion of trading opportunities with previously planned consumption then allowed but trade already over. This was perhaps an inevitable development, given the dominance of the Arrow-Debreu model of general equilibrium in which markets for all present and future goods clear at the beginning of time, but can be considered only a first step in the analysis of the disequilibrium behavior of actual economies.

As already observed, the price-adjustment equation (1) was retained in trading processes. The task then was to specify the adjustment equations describing changes in endowments. Here there quickly developed one restriction common to all models (in one form or another). That was the assumption that trade at constant prices cannot increase an agent's wealth, since goods of equal value must be exchanged. I shall refer to this as the "No Swindling" assumption.

That progress might be made by considering trading processes becomes apparent when one realizes that the No Swindling assumption alone implies that any Lyapounov function that works in tâtonnement also works for trading processes in pure exchange. Essentially this is because, with prices constant, trade in endowments cannot change any household's ordinary demand for any commodity, since wealth will be unaffected. While such trade can certainly change a particular household's *excess* demand for the commodity traded by changing its actual stock, such effects must cancel out in pure exchange when summing over households. Hence trade in endowments does not change aggregate excess demands, and those demands only move with prices. It follows that if such movement is consistent with a Lyapounov function when only prices move, then it is still consistent when trade in endowments is permitted.

This is an interesting result, incorporating both some consideration about out-of-equilibrium behavior and the properties of the underlying theory of the consumer. Surprisingly, it shows that stability proofs will generally be no harder for trading processes (in pure exchange) than for tâtonnement. Unfortunately, this does not get us very far, since we know that such proofs are usually not available for tâtonnement. Further specification of trading processes beyond the No Swindling assumption is required if real progress is to be made.

Such specification took the form of two alternative assumptions about the way trade takes place. The first of these, the "Edgeworth process" was introduced by H. Uzawa (1962) (see also Hahn, (1961) ; the second, the "Hahn process" (named by Negishi, 1962) made its first published appearance in a paper by Hahn and Negishi (1962). Each of the two processes involves what turns out to be a deceptively simple and appealing assumption about out-of-equilibrium trade.

The basic assumption of the Edgeworth process is that trade takes place if and only if there exists a set of agents whose members can all increase their utilities by trading among themselves at the then ruling prices. With some complications stemming from the possibility that initial prices may not permit any such trade, it is easy to see that at least quasi-stability must follow. This is because, for each agent, the utility that would be achieved were trade to stop and the endowment then held to be consumed must be non-decreasing and strictly increasing if that agent enagages in trade. Hence the sum (or any other monotonic function) of such utilities must be non-decreasing and strictly increasing out of equilibrium. The negative of the sum can then be used as a Lyapounov function.

This is very neat, but problems emerge when one begins to think hard about the basic assumption involved. In the first place, it is easy to construct examples in which the only Pareto-improving trades that are possible involve large numbers of agents. Indeed, the only upper bound on such constructions (other than the number of agents) is the number of commodities itself. Since we wish to deal with models in which all present and future goods are involved, that upper bound cannot be an effective one. Hence the assumption that trade must take place if such a Pareto-improving possibility exists places a massive requirement on the information flow among agents.[8]

A somewhat deeper problem lies in the other part of the Edgeworth-process assumption. Since trade is voluntary, it seems very natural to assume that trade only takes place when the agents engaging in it are all made better off. Once one considers the possibility of moving from trading processes in the direction of what I have referred to above as the "key question," however, the usefulness of this assumption in the form employed in the Edgeworth process becomes very doubtful.

The "key question" is that of whether the economy is driven to equilibrium by the behavior of arbitraging agents taking advantage of the opportunities thrown up by disequilibrium. But speculating agents can certainly engage in trade not because they believe that their utility will be directly increased by each trade but because of the sequence of trades they expect to complete. An agent who trades apples for bananas in the hope that he or she can then make an advantageous trade of bananas for

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8. Let there be n agents and $c \geq n$ commodities. With the exception of agent n, let agent i hold only commodity i and desire only commodity i + 1. Let agent n hold only commodity n and desire only commodity 1. Then the only Pareto-improving trade involves all n agents. The problem is quite similar to that involved in coalition formation in the theory of the core, and D. Schmeidler has shown (privately) that, if $c \lesssim n$, the existence of some Pareto-improving trade implies the existence of such a trade for no more than c agents. P. Madden (1978) proves that the existence of a Pareto-improving trade implies the existence of a Pareto-improving bilateral trade, provided that every agent always has a positive amount of every commodity, but such a condition cannot be reasonably expected to hold. (Whether a weaker condition on agents' holdings might produce a weaker but still interesting result is an open question. The construction of the example above suggests such a possibility.)

carrots may not care for bananas at all. More realistically, agents sell goods for money, not because they expect happily to consume the money they receive but because they expect to use the money to buy something else. The basic assumption of the Edgeworth process, however, is that every individual transaction is utility increasing -- that agents would gain from each leg of a transaction even if trade were to stop so that later legs could not be completed. Whether the fact that individuals engage in trade because they _expect_ to gain can be used to extend the Edgeworth process to cover multi-part transactions is not known and seems doubtful.

One cannot avoid this problem if one wishes to examine the serious out-of-equilibrium behavior of agents who have non-naïve expectations. The fact that the economy is not in equilibrium means that some expected trades may not materialize. In turn this means that agents who expected to gain from such trades will be disappointed. As a result, they may very well regret having taken past actions -- actions they would not have taken had they realized what was to occur.

This phenomenon is not restricted to speculative actions. If one considers the extension of the analysis of trading processes to permit out-of-equilibrium production and consumption, one encounters a similar difficulty with the extension of the Edgeworth process. Both consumption and production involve technically irreversible acts -- the consumption of goods or the transformation of inputs into outputs. If those acts are taken on mistaken expectations about later occurrences -- either later prices or the ability to complete later transactions -- then they will sometimes be regretted. This is hard to accomodate in a model whose Lyapounov function depends on agents always having non-decreasing utilities.

## 4. The Hahn Process

The second of the two important trading processes, the Hahn process, places a much less severe informational requirement on trades than does the Edgeworth process. In the Hahn process it is supposed that goods are traded in an organized way on "markets." (How such markets get organized is a question for a different level of analysis.) It is assumed that prospective buyers and sellers of a given good can find each other and trade if they desire to do so -- indeed, in some versions (Fisher 1972), this is taken to define what is to be meant by a "market."

Naturally, out of equilibrium, it can, and often will happen that prospective buyers and sellers of a given good cannot all complete their planned transactions in that good. There may thus be unsatisfied sellers or unsatisfied buyers. The principal assumption of the Hahn process is that markets are "orderly," in the sense that, *after trade*, there are not both unsatisfied buyers and unsatisfied sellers of the same commodity. Only on one side of a given market are agents unable to complete their planned transactions.

This assumption can easily be seen to lead in the direction of a stability proof. Trade is supposed to take place instantaneously or outside of time relative to the rest of the process, and we look only at post-trade situations. Since markets are orderly, after trade, any agent with unsatisfied excess demand for apples, say, finds that there is aggregate excess demand for apples. Since (1) is retained as the price adjustment equation, the price of apples must be rising. Similarly, any agent with unsatisfied excess supply for bananas finds that there is aggregate excess supply for bananas. Then the price of bananas must be falling, unless that price is already zero. Since anything an agent wants to buy and cannot buy is becoming more expensive, and any non-free good that an agent wants to sell and cannot sell is becoming cheaper, any agent with either unsatisfied excess demand or unsatisfied excess supply of non-free goods is becoming worse off. In slightly more formal terms, the agent's target utility -- defined as the utility that the agent would get if he or she completed all planned transactions -- is non-increasing and strictly decreasing if the agent's plans are frustrated.[9] It follows that the sum of such utilities over agents (or any monotonic function of the utilities of individual agents) will serve as a Lyapounov function, decreasing except in equilibrium when all agents can complete their planned transactions.

This shows the quasi-stability of the Hahn process. If one either assumes or proves boundedness of the prices, it is possible to show global stability, since expenditure minimization and the strict quasi-concavity of indifference curves implies that all limit points must be the same.

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9. With the exception of disposing of free goods. It is tiresome to have to constantly repeat this, and I shall not always do so hereafter.

It is important to understand the difference between the Lyapounov functions of the Edgeworth and Hahn processes. In the Edgeworth process, the utilities that increase out of equilibrium are the actual utilities that agents would obtain if trade ceased and they had to consume their endowments. In the Hahn process, the utilities that decrease out of equilibrium are the target utilities that agents expect to get by completing their transactions at current prices. In effect, out of equilibrium, those expectations are not compatible; agents jointly expect more than can be delivered. As the Hahn process goes on, agents revise their expectations downward until they do become mutually compatible and equilibrium is reached.

Of course, since the two processes are quite different, it will sometimes happen in the Hahn process that trade leads to a decrease in the utility that an agent would get if that were his or her last trade. This is not a defect, however. Indeed, as can be seen from our earlier discussion of the Edgeworth process, such a property is desirable, since we want to focus on ultimate plans, not myopic desires as the reason for trade.

Moreover, continuing to look ahead toward the "key question" and more realistic models, the Hahn process has another desirable feature that the Edgeqworth process lacks. Since the Lyapounov function of the Hahn process involves declining target utilities, it should be fairly easy to accomodate the decline in utility that occurs when an irreversible consumption or production action is taken and later regretted. This turns out to be the case (Fisher 1976a, 1977).

Before we can properly get to such matters, however, we must deal with an underlying problem. The basic assumption of the Hahn process, that markets are "orderly" in the sense described, cannot be reasonably maintained without deeper consideration. The problem at issue can be seen by considering the following example.

Suppose that there are at least three commodities, apples, bananas, and croissants. Suppose that, at non-zero current prices, before trade, apples and bananas are in excess supply and croissants in excess demand. Suppose further that some agent, A, owns only apples and wishes to trade for bananas. Suppose that another agent, B, wishes to sell bananas and buy croissants, but does not wish to sell bananas for apples. Then even though A and B can meet each other, no trade between them will take place at current prices. This means that, post-trade, there can perfectly well be agents with an unsatisfied excess demand for apples and also agents with an unsatisfied excess supply of apples. The apple market in this example is not "orderly," and such situations cannot be ruled out merely by supposing that agents can find each other readily.

This problem appears first to have been recognized in the modern literature by R. Clower (1965) who pointed out (in a different context) the need to sell before one can purchase. But a homely example comes readily to hand.[10] A familiar English nursery rhyme states:

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10. I apologize for using again the same light-hearted example that I have already employed on two previous occasions (Fisher, 1976b, p. 14; 1983, p. 33). It is so apt as to be irresistible.

Simple Simon met a pieman going to the fair.
Said Simple Simon to the pieman, `Let me taste your ware.'
Said the pieman to Simple Simon, `Show me first your penny.'
Said Simple Simon to the pieman, `Indeed, I haven't any.'

This is a clear example of a Hahn process economy in crisis. Markets are sufficiently well organized that willing buyers and willing sellers can meet. Indeed, in the rhyme, the prospective buyer and seller of pies meet on their way to the marketplace (the "fair"). Nevertheless, no trade takes place because the buyer has nothing to offer the seller that the seller is willing to accept.

The case of Simple Simon, however, points up one possible way to think about this problem. It does so by introducing an element so far conspicuously lacking from stability analysis. The pieman does not ask Simple Simon for apples or bananas or croissants; instead he asks for money, and the time has plainly come to consider the introduction of money into stability analysis.

Indeed, that introduction cannot be long delayed in any case. Aside from the Simple Simon problem under discussion and the use of money in the intermediate stages of arbitrage transactions, one cannot get beyond pure exchange without introducing it. This is for a reason that, interestingly, does not apply in equilibrium.

Firms, unlike households, are usually assumed to maximize profits. Suppose that some firm produces a large excess supply of some commodity, say toothpaste. Out of equilibrium, even with toothpaste in aggregate excess supply, the price of toothpaste can be positive. If that price is high enough, and if there is no standard medium of exchange in which profits are measured, the toothpaste producing firm may regard itself as making a positive profit, even_though_it_sells_no_toothpaste. This means that the firm's inventory of toothpaste need not be offered for sale, so that the excess supply of toothpaste will have no effect on the price.[11] Only by insisting that profits be measured in a common medium of exchange (and a common unit of account) can we ensure that firms producing commodities other than the exchange medium have an incentive to sell those commodities. This makes money indispensable.

The introduction of money into Hahn process models was begun by Arrow and Hahn (1971). They assumed that one of the commodities, "money," plays a special role in that all transactions must involve it. They then assumed that agents first formulate "target excess demands" -- excess demands constructed by maximizing utility functions subject to budget constraints in the usual way -- but that these must be distinguished from "active excess demands", constructed as follows. If an agent has a negative target excess demand for a given commodity, then that agent wishes to sell it. Since commodities can be offered for sale whether or not the supplier has any money, active excess demand in such a case is assumed to equal target excess demand. On the other hand, positive target excess demands cannot generate offers to buy unless they are backed up by money, so Arrow and Hahn

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11. The device of assuming that the firm distributes toothpaste dividends to its stockholders hardly seems satisfactory.

assumed that the agent allocates his or her available money stock over the goods for which he or she has a positive excess demand. This leads to the assumption that any good for which the agent has a positive target excess demand is also one for which that agent has a positive active excess demand, with the active excess demand never exceeding the target one (agents do not offer to buy more than they really want and always make a positive offer for anything they want). It is active, rather than target demands that are assumed to obey the orderly markets assumption and unsatisfied aggregate excess active demand that is assumed to affect prices according to (1).

With this in hand, Arrow and Hahn were able to isolate the Simple Simon problem by assuming that no agent ever runs out of money. If this assumption holds, then it is easy to see that the Hahn process stability proof goes through in much the same way as before. Prices change in the direction indicated by unsatisfied aggregate active demands; unsatisfied individual active demands have the same signs (post-trade) as the corresponding aggregate demands; finally, unsatisfied individual target demands have the same signs as the corresponding unsatisfied individual active demands. Hence target utilities are still decreasing out of equilibrium.

As already indicated, the introduction of money permits the introduction of firms, and this was done in Fisher (1974).[12]

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12 ~~11~~. A parallel introduction of firms into the Edgeworth process was accomplished by F. M. C. B. Saldanha (1982).

Firms are assumed to be subject to the orderly markets assumption, but to maximize profits which they ultimately distribute to their shareholders. Shareholders expect to spend those profits. Because of the orderly markets assumption, any firm that cannot complete its planned transactions must revise its forecast of profits downward. Households then find their target utilities decreasing both because of the direct influence of the orderly market phenomenon on their own transactions and because of the declining fortunes of the firms they own. The sum of household utilities can thus again be used as a Lyapounov function. While boundedness is now a more complex matter, a global stability proof follows nicely from it, employing both profit maximization on the part of firms and expenditure minimization on the part of households to show that all limit points are the same. Money and the target-active excess demand distinction are handled as before.

This is a pretty story, and one that can even be extended to permit out-of-equilibrium production and consumption, as indicated above (Fisher, 1976a, 1977). But the difficulties are all too apparent.

The role of money in this model is very much an afterthought. Agents plan their target excess demands as though they were in equilibrium. In so doing, they take no account of the cash constraint imposed by the institutional structure. Instead, they allocate their money stocks to their positive excess demands as though any cash difficulty will necessarily be only temporary, so that ultimately target transactions will be completed.

That naïveté is also reflected in the assumption that agents make a positive offer for every good for which they have a positive target excess demand. So long as we remain in an Arrow-Debreu world where all markets open and close at the dawn of time, this may not matter. Once we begin to be serious about disequilibrium, however, and to permit consumption and production to take place before equilibrium is reached, it matters a lot. It is not reasonable to suppose that agents facing a liquidity crisis always allocate funds to all demanded commodities. Some of those commodities may not be needed for years, while others may be required for near-term consumption.

And of course the afterthought method of allocating cash is related to the most obvious difficulty. The Simple Simon problem has not been solved, but merely well defined. It is still necessary to assume that agents never run out of money. This may be hard to swallow in any case; it is particularly unpalatable when agents make their money-allocation plans as though their planned sales would always materialize.

In the same connection, the time has come to remember how awkward the price-adjustment assumptions are in all these models. We are not dealing with a case in which agents, faced with impending cash shortage when planned sales do not occur, can lower their prices. Rather, we are still in a world in which price is set anonymously, and sellers who might benefit from lower prices are just out of luck.[13]

In other respects as well the model is less than satisfactory. Money is assumed to be a commodity entering the utility function. This is required in order to ensure that agents wish to hold money in equilibrium, avoiding the "Patinkin problem" (D. Patinkin, 1949, 1950, 1965). But that problem arises because equilibrium in this Arrow-Debreu world means a cessation of trading opportunities. If equilibrium had the more natural property of involving the carrying out of previously planned transactions at previously foreseen prices, then the transactions motive for holding money would not disappear. Yet such a version of equilibrium requires agents to care about the timing of their transactions.

In several ways, then, the defects of the more sophisticated Hahn process models point the way toward possible progress. In one way or another, those defects are all related to the fact that the agents in such models (as in all the models considered so far) pay very little attention to the fact that the economy is in disequilibrium. They go on believing that prices will not change and that transactions will be completed. Disequilibrium behavior and phenomena are modeled at best as an afterthought.

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13. Some progress can be made here. Fisher (1972) provides a model in which goods are identified by the dealers who sell them. In such a model, the orderly markets assumption is essentially trivial, since there is only one agent on the supply side of any "market." Since prices are set by suppliers (with buyers searching for low prices), they can be adjusted when planned sales do not occur and cash is low. But there are plenty of other difficulties with such a model. See M. Rothschild (1973).

Plainly, the difficulties encountered cannot be solved in such a context. A full disequilibrium model is required and must be built if we are to address the "key question" of whether arbitraging actions drive the economy to equilibrium.

## 5. Towards a Full Disequilibrium Model

So far as I know, the only attempt to examine the stability question in the context of a full disequilibrium model in which consumption and production take place out of equilibrium and agents consciously act on arbitrage opportunities is that of my recent book (Fisher 1983; see also Stahl and Fisher 1986). As will be seen, that attempt to answer the "key question" cannot be considered truly successful, but there is, I think, much to be learned from it and from its inadequacies.

I begin by considering a problem of only moderate importance which nevertheless exemplifies the need for dropping equilibrium habits of thought when thinking about disequilibrium problems. This problem arises when one allows consumption and production to take place out of equilibrium.

It is common, correct, and necessary to regard commodities consumed or produced at different dates as different commodities even if they are physically indistinguishable. In the Arrow-Debreu world where nothing ever happens until equilibrium is reached, this does not matter; a commodity with a different date is just a different commodity traded on a different market and with its own price. If consumption or production takes place out of equilibrium, however, then commodity dates take on a new significance. Only currently dated commodities can be consumed or produced; future commodities can only be traded. Hence, allowing disequilibrium consumption or production means allowing some commodity dates to be passed before equilibrium is reached. Since there can only be trading in current or future commodities, but no trading in "pasts," this means that trading in some commodities becomes impossible as the adjustment process unfolds.

To see why this creates a difficulty, consider the following example. For simplicity, assume that commodities are dated by year. At midnight on December 31, 1987, trade in 1987 toothpaste ceases. Since we are out of equilibrium, this can mean that there are agents who cannot buy as much 1987 toothpaste as they had planned. Since they must now make do with a different amount than planned, this can cause a discontinuity in their behavior.

An obvious solution to this difficulty presents itself, however. Assume that toothpaste is a durable good (a somewhat different analysis applies to pure perishable commodities). Then, at midnight on December 31, 1987, 1987 and 1988 toothpaste are perfect substitutes. Our agent may not be able to buy the 1987 toothpaste he or she planned, but this will not create any discontinuity, since 1988 toothpaste can be purchased instead.

The problem cannot be made to go away so easily, however. Since 1987 toothpaste is a different commodity from 1988 toothpaste, the two commodities have different prices. If those prices do not coincide at midnight on December 31, 1987, then discontinuity is still a real possibility.

It is very tempting to reply to this that the two prices _must_ coincide at that time, because the two commodities are then perfect substitutes. That temptation must be resisted. The proposition that the prices of perfect substitutes must coincide is an *equilibrium* proposition. It rests on the argument that arbitrage will erase any difference between the prices. But that working of arbitrage is what a full stability model is supposed to be about. We cannot, in a disequilibrium framework, simply assume that arbitrage will be successful by the time the crucial hour arrives.

There is an important sense, however, in which this difficulty is more apparent than real. That difficulty stems from the treatment of the markets for 1987 and 1988 toothpaste as wholly distinct, with prices set anonymously according to some rule such as (1). In fact, this is unlikely to be the case. Instead, the same firms that sell 1987 toothpaste are also likely to sell 1988 toothpaste and to quote prices for both. Similarly, dealers specializing in wheat futures are unlikely to deal in futures for only one date. But if the same seller (or, more generally, the same dealer) quotes prices for both 1987 and 1988 commodities, then he or she will have an active interest in making sure that those prices come together at midnight on December 31, 1987, since otherwise arbitrage at the dealer's expense will be possible.

There are three lessons to be learned from all this. First, one cannot think about disequilibrium problems using only equilibrium habits of thought. Certain issues that seem not to matter in equilibrium can matter quite a lot out of it. Second, the farther one gets into serious disequilibrium analysis, the less satisfactory is the assumption of anonymous price adjustment. Third, disequilibrium considerations have something to do with the institutional structure of transactions and the way in which markets are organized -- subjects on which no work has been attempted in the disequilibrium context, but which are crucial if we are ever to gain a satisfactory understanding of the formation of economic magnitudes.[14]

Such subjects, however, are truly difficult, for they involve analysis of what happens when agents interact and their plans do not mesh. It is far easier to consider how those plans get formulated, and the analysis of Fisher (1983) does this at some length, producing a number of results on the way in which agents plan to take advantage of the arbitrage opportunities they see thrown up by changing prices. In the course of so doing, the positive cash assumption of Arrow and Hahn becomes far less arbitrary, since agents now optimize their planned transactions, paying attention to their money stock. Interestingly, it emerges that one reason for trading in the shares of firms is because anticipated dividend streams permit liquidity transfers from one period to another, and, out of equilibrium, such transfers may be needed.

Such arbitraging actions come principally from allowing agents to expect prices to change. But allowing agents to be conscious of disequilibrium means more than this; it also means allowing them to realize that their transactions may be limited

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14. For work on transaction arrangements in general equilibrium, see D. Foley (1970) and Hahn (1971).

in extent. So long as we retain anonymous price adjustment, we must suppose that such constraints are regarded as absolute. This has led to a literature on the analysis of equilibria under such circumstances -- so called "fixed price equilibria."[15]

More interesting for the study of true disequilibrium is what happens when we allow agents to believe that they can alter the constraints they face by making price offers. Consider, for example, the case of a seller who believes that the amount that can be sold at a given price is limited. If the seller also believes that a lower price will bring more sales, then the constraint expresses expected sales as a function of price and becomes an ordinary, downward-sloping demand curve. In this case, the seller will only refrain from offering a lower price for the usual reason in the analysis of monopoly: a lower price must be given on all units to be sold, and marginal revenue will fall short of marginal cost.

This leads to a number of interesting problems. First, there is the distinct possibility in such cases that equilibrium will be non-Walrasian. Specifically, the economy can be stuck in a position where agents believe they face binding transaction constraints and do not attempt to get round them with price offers because they believe that it would be unprofitable to do so. In macroeconomics, this can be regarded as a version of the original Keynesian question as to underemployment equilibrium.

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15. While such circumstances are sometimes referred to as "disequilibrium," they are not properly so-called, since what is involved is non-Walrasian equilibrium, rather than dynamic adjustment. See A. Drazen (1980) for a survey of the literature.

Hahn (1978) shows that it can happen with the beliefs of the agents rational in some sense. Second, the crucial question of whether an equilibrium is Walrasian or non-Walrasian becomes the question of whether perceived monopoly power vanishes in equilibrium. This is not a question that can be answered by only analyzing equilibria; it pretty clearly depends on the experiences agents encounter on the way to equilibrium (assuming that some equilibrium is reached). In this regard, it is interesting that, as Fisher (1983) shows, there is a relation between the nature of the equilibrium and the question of whether or not liquidity constraints are actually binding therein. Only where perceptions of monopoly power remain (and change over time in certain ways after equilibrium is reached) will the equilibrium be non-Walrasian and cash remain a problem.

Whether or not a given equilibrium is Walrasian, however, some clarification of the role of money is achieved. We saw above that the equilibria of trading processes (or of tâtonnement models, for that matter) were merely an exhaustion of trading opportunities. In a full model, such as the one under discussion, transactions do not cease in equilibrium; rather, equilibrium involves the carrying out of previously made optimal plans involving planned transactions at correctly foreseen prices. This means that the transactions demand for money does not disappear in equilibrium. While money in this model is an interest-bearing asset (so that there is no explanation for equilibrium holding of non-interest-bearing money), this explains why agents hold that asset rather than others bearing the same rate of interest in equilibrium, even though money itself enters neither utility or production functions.

## 6. Dynamics and Stability in a Full Model

All this is very interesting, but it says little about what happens when agents interact out of equilibrium and plans are frustrated. What can be said about such interactions and about the "key question" of whether they lead to stability? Alas, it is here, as already indicated, that the analysis under discussion produces less than satisfactory answers.

We have already seen that one cannot retain the old anonymous price-adjustment equation (1) left over from tâtonnement days. Individual price adjustment is essential. But how does such price adjustment take place? The answer suggested above is that prices are set optimally depending on perceived monopoly (or monopsony) power. That is all well and good, but it does not take us very far. How do such perceptions get formed and change? How do institutions arise determining which agents make price offers and which choose among offers? Out of equilibrium, where offers and acceptances will not match, how does partial matching take place?

On these crucial questions, Fisher (1983) offers relatively little guidance. Rather, price movements, like all other movements in the model are assumed to be restricted by a vague but strong restriction called "No Favorable Surprise" (NFS). To understand that restriction and the motivation for it, requires us to step back for a moment and consider the purpose of stability analysis.

Real economies are subject to a succession of exogenous shocks. The discovery of new products, new processes, new sources of raw materials, new demands, and new ways of organizing production are, as emphasized by J. Schumpeter (1911), the driving forces of economic development and growth. It is unreasonable to suppose that such Schumpeterian shocks are all foreseen and can be incorporated as part of equilibrium. Rather, equilibrium analysis, if it is useful at all, is so because the economy rapidly adjusts to such shocks, approaching a new equilibrium long before the next shock occurs.

The role of stability analysis, then, is to analyze the question of whether such adjustment in fact takes place. This means analyzing the part of the Schumpeterian model occurring after the initial innovation, when imitators enter and act on the profit opportunities they see. What I have called the "key question" can be interpreted as the question of whether such action does in fact lead the system to absorb a given Schumpeterian shock. Evidently, then, the first task of stability analysis is to answer this question on the assumption that further Schumpeterian shocks do not occur.

There is more to it than this, however. In a full model, where agents form their own expectations, there is the possibility that agents will perceive Schumpeterian opportunities that do not exist. If such agents have the resources with which to back their perceptions, equilibrium will at least be postponed. The entrepreneur who believes that he or she can profitably build a better mousetrap and who has the money to invest will affect the economy even if the world does not in fact beat a path to the door. Stability implies that such occasions disappear, at least asymptotically, and no stability proof in a complete model can succeed without either proving or assuming that this happens.

The basic first step in an adequate analysis of stability as a full attack on the "key question," therefore, is the weak one of showing that arbitrage leads to equilibrium if no new unforeseen opportunities arise. This is the assumption of "No Favorable Surprise." More precisely, NFS assumes that agents are never surprised by the unforeseen appearance of new, favorable opportunities causing them to deviate from previously formed optimal plans if those plans are still feasible. In other words, any plan now optimal is assumed to have been feasible a short time ago. Useful new opportunities (technological change, for example) must be foreseen at least a short time before agents actually change plans so as to act on them.

It is not hard to see that, as in the Hahn process which is a special case, NFS implies that agents' target utilities are declining out of equilibrium. While agents can be doing quite well in a foreseen way (including taking advantage of foreseen technological progress), any abrupt departures from what was expected nust mean declines in utility (if they matter at all). With this in hand, a global stability proof can be made to follow, although the details are technically complex and require a number of non-primitive assumptions on the dynamics involved.

The problem with this is that NFS itself is not a primitive assumption, either. It is all very well to argue as above that one must exclude further exogenous Schumpeterian shocks in examining stability. It is far stronger to rule out the favorable opportunities that may suddenly arise in the course of adjustment to an existing shock.

Evidently, this difficulty arises precisely because we have no good model of how agents interact in reacting to disequilibrium. This causes us to be unable to describe exactly how endogenous surprises do or do not arise and makes NFS a somewhat unsatisfactory assumption.[16]

Like earlier models, then, the analysis of Fisher (1983) is only partially successful. It is strongest when dealing with the plans of individual agents or with equilibrium. It is weak when considering how those plans interact when they cannot all be fulfilled and how agents then change their expectations. While it succeeds in doing away with anonymous price adjustment, it tells us very little about how prices are in fact set. We still have much to learn about the formation of economic magnitudes.

To learn how economic magnitudes are formed requires serious modelling of disequilibrium. If we are ever to understand how resources are allocated, how consumption and production are organized, how prices come to be what they are and the role that they play, we must examine disequilibrium behavior. Among other things, this means examining the ways in which agents change

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16. There is at least one other problem with NFS. The agents in the model being described have point expectations and no subjective uncertainty. (They are all economists -- often wrong but never uncertain.) It is an open question as to whether there exists a version of NFS that is both palatable and strong enough to produce a similar stability result when subjective uncertainty is permitted.

their expectations when their plans are frustrated. Obviously, such questions cannot be begged by using equilibrium tools. (In particular, the assumption of rational expectations can tell us nothing at all about how disequilibrium works.) We cannot simply examine positions in which economic magnitudes happen to be such that there is no tendency to change. To understand the workings of the "Invisible Hand" it is not enough to understand what the world looks like when the "Invisible Hand" has nothing to do.

## References


Arrow, K. J., H. D. Block, and L. Hurwicz (1959). "On the Stability of the Competitive Equilibrium II," Econometrica 27, 82-109.

Arrow, K. J. and F. H. Hahn (1971). General Competitive Analysis. San Francisco: Holden-Day/New York: Oliver & Boyd.

Arrow, K. J. and L. Hurwicz (1958). "On the Stability of the Competitive Equilibrium I," Econometrica 26, 522-52.

Clower, R. W. (1965). "The Keynesian Counterrevolution: a Theoretical Appraisal," in F. H. Hahn and F. P. R. Brechling (eds.), The Theory of Interest Rates. London: Macmillan/New York: St. Martin's Press.

Debreu, G. (1970). "Economies with a Finite Set of Equilibria," Econometrica 38, 387-92.

__________ (1974). "Excess Demand Functions," Journal of Mathematical Economics 1, 15-21.

Drazen, A. (1980). "Recent Developments in Macroeconomic Disequilibrium Theory," Econometrica 48, 283-306.

Edgeworth, F. Y. (1881). Mathematical Psychics. Reprinted New York: Augustus M. Kelley, 1967.

Fisher, F. M. (1972). "On Price Adjustment without an Auctioneer," Review of Economic Studies 39, 1-15.

______________ (1974). "The Hahn Process with Firms But No Production," Econometrica 42, 471-86.

_____________ (1976a). "A Non-Tâtonnement Model with Production and Consumption," Econometrica 44, 907-38.

_____________ (1976b). "The Stability of General Equilibrium: Results and Problems," in M. J. Artis and A. R. Nobay (eds.), Essays in Economic Analysis (The Proceedings of the Association of University Teachers of Economics, Sheffield 1975). Cambridge: Cambridge University Press.

_____________ (1977). "Continuously Dated Commodities and Non-Tâtonnement with Production and Consumption," in A. S. Blinder and P. Friedman (eds.), Natural Resources, Uncertainty, and General Equilibrium Systems: Essays in Memory of Rafael Lusky. New York: Academic Press.

_____________ (1983). Disequilibrium Foundations of Equilibrium Economics. Cambridge: Cambridge University Press.

Foley, D. K. (1970). "Economic Equilibrium with Costly Marketing," Journal of Economic Theory 2, 276-91.

Hahn, F. H. (1958). "Gross Substitutes and the Dynamic Stability of General Equilibrium," Econometrica 26, 169-70.

__________ (1961). "A Stable Adjustment Process for a Competitive Economy," Review of Economic Studies 29, 62-5.

__________ (1961). "On the Stability of Pure Exchange Equilibrium," International Economic Review 3, 206-13.

___________ (1971). "Equilibrium with Transaction Costs," Econometrica 39, 417-40.

__________ (1978). "On Non-Walrasian Equilibria," Review of Economic Studies 45, 1-18.

Hahn, F. H. and T. Negishi (1962). "A Theorem on Non-Tâtonnement Stability," Econometrica 30, 463-9.

Hicks, J. R. (1939). Value and Capital. New York: Oxford University Press (Clarendon Press).

Kagawa, A. and K. Kuga (1980). "On Professor Hahn's Tâtonnement Stability Theorem: Comment and Example," Review_of_Economic Studies 47, 813-16.

Lyapounov, A. (1907). "Problème Gènèral de la Stabilitè du Mouvement," Annales_de_la_Facultè_des_Sciences_de_l'Université_de_Toulouse 9, 203-474.

McFadden, D. (1968). "On Hicksian Stability," in J. N. Wolfe (ed.), Value,_Capital,_and_Growth._Papers_in_Honour_of_Sir John_Hicks. Edinburgh: Edinburgh University Press.

Madden, P. (1978), "Why the Edgeworth Process Assumption Isn't That Bad," Review_of_Economic_Studies 45, 279-84.

Mantel, R. (1976). "Homothetic Preferences and Community Excess Demand Functions," Journal_of_Economic_Theory 12, 197-201.

Metzler, L. (1945). "The Stability of Multiple Markets: The Hicks Conditions," Econometrica 13, 277-92.

Negishi, T. (1958). "A Note on the Stability of an Economy Where All Goods Are Gross Substitutes," Econometrica 26, 445-7.

__________ (1962). "The Stability of a Competitive Economy: a Survey Article," Econometrica 30, 635-69.

Patinkin, D. (1949). "The Indeterminacy of Absolute Prices in Classical Economic Theory," Econometrica 17, 1-27.

____________ (1950). "A Reconsideration of the General Equilibrium Theory of Money," Review_of_Economic_Studies 18, 42-61

____________ (1965). Money,_Interest_and_Prices (scond edition). New York: Harper & Row.

Rothschild, M. (1973). "Models of Market Organization with Imperfect Information: A Survey," _Journal of Political Economy_ 81, 1283-1308.

Saldanha, F. M. C. B. (1982). _Essays on Non-Tâtonnement Stability_. Unpublished Doctoral Dissertation, Massachusetts Institute of Technology.

Samuelson, P. A. (1941). "The Stability of Equilibrium," _Econometrica_ 9, 97-120.

_______________ (1947). _Foundations of Economic Analysis_. Cambridge, Mass.: Harvard University Press.

Scarf, H. (1960). "Some Examples of Global Instability of the Competitive Equilibrium," _International Economic Review_ 1, 157-72.

_________ (1973). _The Computation of Economic Equilibria_ (in collaboration with T. Hansen). New Haven: Yale University Press.

Schumpeter, J. (1911). _The Theory of Economic Development_. Fourth printing of English translation, Cambridge Mass.: Harvard University Press, 1951.

Sonnenschein, H. (1972). "Market Excess Demand Functions," _Econometrica_ 40, 549-63.

_________________ (1973). "Do Walras' Identity and Continuity Characterize the Class of Community Excess Demand Functions?" _Journal of Economic Theory_ 6, 345-54.

Stahl, D. and F. M. Fisher (1986). "On Stability Analysis with Disequilibrium Awareness," Massachusetts Institute of Technology Working Paper 432.

Uzawa, H. (1962). "On the Stability of Edgeworth's Barter Process," *International Economic Review* 3, 218-32.