

5 The stability of general equilibrium—what do we know and why is it important?

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Introduction

Although there were precursors, Léon Walras was the great founder and first expositor of general equilibrium theory. While he did not see or solve all its problems in what we would now consider a satisfactory way, he clearly realized that microeconomics requires that all markets must be studied together and that partial equilibrium analysis, no matter how useful, does not otherwise stand on a secure foundation.

There are four important issues in general competitive equilibrium theory:

1. Existence: Under what circumstances do competitive general equilibria exist?
2. Uniqueness: Under what circumstances will there be a unique general equilibrium?¹
3. Optimality: What is the relationship between positions of competitive general equilibrium and Pareto efficiency?
4. Stability: Under what circumstances (if any) does a competitive economy converge to equilibrium, and, if it does, how quickly does this happen?

By the last third of the twentieth century, a good deal was known about the first three of these questions:

- Under very general (but not universal) conditions, competitive general equilibria do exist.
- They are globally unique, however, only in quite special cases (Gross Substitutes, for example), although they are generally locally unique.
- Under very general circumstances, such equilibria are Pareto-efficient. Under somewhat less general circumstances, any Pareto-efficient allocation can be decentralized as a competitive general equilibrium.

The efficiency property is the most significant one. The two welfare theorems (loosely stated above) provide the rigorous justification for the view that free markets are desirable (although they say nothing about fairness, or any other

desirable attribute other than Pareto-efficiency). It is not an overstatement to say that they are the underpinning of Western capitalism.

So elegant and powerful are these results, that most economists base their conclusions upon them and work in an equilibrium framework—as they do in partial equilibrium analysis. But the justification for so doing depends on the answer to the fourth question listed above, that of stability, and a favorable answer to that is by no means assured.

It is important to understand this point which is generally ignored by economists. No matter how desirable points of competitive general equilibrium may be, that is of no consequence if they cannot be reached fairly quickly or maintained thereafter, or, as might happen when a country decides to adopt free markets, there are bad consequences on the way to equilibrium.

Milton Friedman remarked to me long ago that the study of the stability of general equilibrium is unimportant, first, because it is obvious that the economy is stable, and, second, because if it isn't stable we are all wasting our time. He should have known better. In the first place, it is not at all obvious that the actual economy is stable. Apart from the lessons of the past few years, there is the fact that prices do change all the time.

Beyond this, however, is a subtler and possibly more important point. Whether or not the actual economy is stable, *we largely lack a convincing theory of why that should be so*. Lacking such a theory, we do not have an adequate theory of value, and there is an important *lacuna* in the center of microeconomic theory.

Yet economists generally behave as though this problem did not exist. Perhaps the most extreme example of this is the view of the theory of Rational Expectations that any disequilibrium disappears so fast that it can simply be ignored. (If the 50-dollar bill were really on the sidewalk, it would be gone already.) But this simply assumes the problem away. The pursuit of profits is a major dynamic force in the competitive economy. To only look at situations where the Invisible Hand has finished its work cannot lead to a real understanding of how that work is accomplished.

I first became interested in this problem around 1970, and worked on it for roughly the next 15 years (although it was not my sole occupation). I did not jump into the general equilibrium issue directly, however, but began with the analysis of a single market (Fisher 1970). Indeed, one of the major aspects of the stability problem shows up there. As Tjalling Koopmans (Koopmans 1957), among others, once remarked, in a perfectly competitive economy where all prices are taken as given, how do prices ever change? That problem arises even in a single market, and that was where I began.

That attempt was not really a successful one. I built a model in which buyers searched for low prices and sellers responded to excess demand by moving prices in the direction of excess demand. With these assumptions, one could get to a competitive equilibrium. But, as Michael Rothschild (1972) pointed out, the sellers, at least, were terribly naïve. They failed to notice that the fact that more buyers showed up at the low-priced stores than at the higher-priced ones meant that each of them faced a declining demand curve and hence had some power over

price. While I later produced a paper (Fisher 1973) in which this was changed, it admittedly took very strong assumptions to get the ultimate equilibrium to be a perfectly competitive one.

This issue was to reappear in my later work on the stability of general competitive equilibrium (see especially Fisher 1983), but, before describing that, I shall discuss how the analysis of that problem developed.

The development of stability theory:² *tâtonnement*

It took a long time for stability theory to develop, and its development was marked, in my view, by four major steps:

1. the realization that the subject had to be studied in the context of a formal dynamic model;
2. the realization that global, rather than only local results might be available;
3. the introduction of *non-tâtonnement* processes; and
4. the closely-related insight that attention paid to specifying the dynamic processes involved could lead to far more satisfactory results than restrictions on the utility or production functions of agents.

Interest was spurred by Hicks's view in the late 1930s (Hicks 1939) that stability would be assured if each market adjusted price in the direction of excess demand first with all other prices constant, then with a second price allowed to adjust, then with three prices allowed to adjust, and so on. (This placed a condition of alternating signs on the Jacobian of the excess demand functions.) But Paul Samuelson (1941, 1947) pointed out that stability requires an analysis of the equations of motion when the economy is not in equilibrium. Recognition of this fact was the first major step.

Samuelson proposed a system of differential equations in which each price changes in the direction of the corresponding excess demand. This was a formalization of Walras's *tâtonnement*, and, while no equations of motion were suggested for quantities, Samuelson's equations for price remained part of stability theory even when trading out of equilibrium came to be considered. Note, however, that this begged the question of who changes the price, and that came to be considered the job of a fictional 'auctioneer', an unsatisfactory answer.

Following this, work concentrated on the question of whether the Hicks conditions were necessary or sufficient for local stability, and it was eventually shown (Samuelson 1941, 1947; Metzler 1945) that this was so under the very strong assumption that all goods are gross substitutes, which indeed implied the Hicks conditions on the Jacobian (Hahn 1958; Negishi 1958). That work, however, was superseded in the late 1950s by the introduction of Lyapounov's Second Method (for proving stability) (Lyapounov 1907) and the realization that it would be possible to prove global, rather than local stability. This was the second major step.

Work still concentrated on *tâtonnement*, however. In two major papers (Arrow and Hurwicz 1958; Arrow, Block, and Hurwicz 1959) it was shown that *tâtonnement* was globally stable under several special conditions. Arrow, Block, and Hurwicz unwisely conjectured, however, that *tâtonnement* was always stable, and that conjecture was proved to be false by Herbert Scarf (Scarf 1960). Moreover, instability holds on a set of non-zero measure, and appears to be the exception rather than the rule. The cases studied by Arrow, Hurwicz, and Block (including that of Gross Substitutes) turned out to be special ones in which the Weak Axiom of Revealed Preference holds for *market* excess demand functions—a property that is very special indeed.

This was widely (and wrongly) considered to be the ultimate failure of stability theory. As we shall see, that view only showed how economists had come to view stability theory as synonymous with the analysis of the stability of *tâtonnement*.³ Nevertheless, the fact that *tâtonnement* is generally unstable has major consequences—often ignored. These are as follows:

1. One cannot assume—as is implicitly done in the theory of rational expectations—that if prices move sufficiently quickly, then equilibrium will be reached extremely quickly so there is no need to think about disequilibrium behavior.
2. The literature on computational general equilibrium needs at least to be totally rethought. One cannot look at the underlying relations of an economy (utility functions, production functions, initial allocations) and calculate where such an economy will find general equilibrium. Indeed, the fact that there will be trading out of equilibrium means that what equilibrium is reached (if any is) will be path-dependent. It will depend on the dynamics of the out-of-equilibrium trade.
3. In particular, it is very risky to look at a planned economy and predict where it will end up if it shifts to a system of free markets.

Both of the latter two points can be restated as the proposition that the 'Walras-correspondence' is not a useful device.

The development of stability theory: trading processes

As indicated, however, at about the same time that the search for *tâtonnement* stability proved a dead end, the attention of those still interested in stability turned to *non-tâtonnement* processes—processes in which trading takes place out of equilibrium. (I prefer to call such models 'trading processes' and shall do so in the remainder of this paper.⁴) These comprised the third and fourth major steps.

There are two interesting trading processes: the Edgeworth process, named by Uzawa (1962),⁵ and the Hahn process, named by Negishi (1962).⁶ I believe the Hahn Process to be the more fruitful of the two, but the Edgeworth Process certainly merits discussion, and I shall begin with it.

The basic assumption of the Edgeworth Process is that, trade will take place among a group of agents if and only if the members of the group can increase their utilities by so-trading, i.e. the trades are Pareto-improving for the group.⁷ It has been shown that so long as every agent has a positive endowment of every commodity (a strong assumption), the process is globally stable.⁸ Here, the sum of utilities over all agents acts as a (negative) Lyapounov function.

The basic assumption sounds very reasonable; after all, why else do agents trade? But, in fact, there are strong objections to it.

In the first place, the assumption requires that trade only take place when the participants directly increase their utilities by engaging in it. Real agents, however, may engage in transactions, not because those transactions directly increase their utilities but because they believe that the resulting change in commodities they gain can later be traded advantageously for something they really want, increasing their utilities indirectly. The Edgeworth Process assumption does not permit this to happen, and, while it is quite possible that the assumption could be modified to permit such behavior and still lead to stability, this has not been done, although the suggestion has been around for many years.⁹

Second, the assumption that trade *will* take place whenever it is Pareto-improving for the participants, implicitly supposes that it is easy for the participants to find each other. But this may very well not be the case. I have shown (Fisher 1989) that the number of agents required in such a trade can be extremely large. While it is true that, if every agent owns a positive amount of every commodity, the existence of a Pareto-improving transaction implies the existence of a bilateral Pareto-improving transaction (Madden 1978) that ownership assumption is highly unrealistic,¹⁰ especially if there is trading in future commodities (as there must be in Arrow-Debreu equilibrium) where the number of commodities is infinite. (That same phenomenon much limits the usefulness of a finding by David Schmeidler¹¹ that the number of agents required cannot exceed the number of commodities.) Further, it may be that the only Pareto-improving transaction is extremely complicated,¹² with some agents participating in part of the trade which does not directly improve their utilities so that they can simultaneously engage in another part of the trade which does.¹³

The other trading process, the Hahn Process, also has problems, but these seem easier to overcome. The central assumption here is that markets are sufficiently well-organized so that, after trade, while, at the going prices, either some prospective buyers or some prospective sellers can remain unsatisfied, there cannot be a positive number of both at the same time. In other words, either the sellers have sold all they want or the buyers have bought all they want, but there is not both unsatisfied supply and unsatisfied demand.

The result of this (together with Samuelson's formalization that prices move in the direction of the corresponding excess demands) is that everyone who wants to buy and cannot buy finds that the goods he wants are becoming more expensive, while everyone who wants to sell and cannot sell finds that the goods she wants to sell are becoming cheaper.¹⁴ As a result, every agent's utility is non-increasing over time, and some agents are becoming worse off. The sum of

'target' utilities—the utilities that the agents expect to get out of the market system—is therefore decreasing except in equilibrium and can be used as a Lyapounov function to prove stability.¹⁵

Of course, there are problems here. What if an agent wants to buy some commodity and finds a willing seller but has nothing to offer in trade that the seller wants. I cannot resist quoting (as I have in previous publications) an example familiar to small children:

Simple Simon met a pie-man going to the fair.

Said Simple Simon to the pie-man, 'Let me taste your ware'.

Said the pie-man to Simple Simon, 'Show me first your penny'.

Said Simple Simon to the pie-man, 'Indeed, I haven't any'.

(Goose, various dates)

This is an example of a Hahn-Process economy in crisis. The market for pies is extremely well-organized—so well-organized, in fact, that the prospective buyer and seller even meet before they get to the marketplace. But no trade takes place because the buyer has nothing that the seller wants.

Of course, as the last line of the poem suggests, this cries out for the introduction of money into the model, an introduction first made by Arrow and Hahn (1971). But such introduction does not solve the Simple Simon problem¹⁶ unless it is assumed that agents never run out of money. That is a particularly strong assumption because the agents in all the models I have so far discussed in the general equilibrium stability literature feature agents with particularly naïve expectations who hence take no precautions against bankruptcy.

Towards a more satisfactory treatment: assumptions and equilibria

The naïve expectations of agents in all these models are such that they always believe they are in equilibrium, that they will complete their transactions, and that prices will not change. The failure to take precautions against bankruptcy is only a relatively minor problem that results. Among others is the property that in both the Edgeworth Process and the Hahn Process households do not consume until equilibrium is reached but only plan to do so. Similarly, when firms are introduced, those firms only plan to produce but do not do so until equilibrium is reached. These are very awkward characteristics for models of disequilibrium behavior.

Clearly, the use of such expectations is subject to the same criticism, mentioned earlier, that Rothschild (1972) made of my early single-market disequilibrium paper. It also greatly contributes to the mystery, pointed out by Koopmans, of how prices ever change. A satisfactory disequilibrium model must allow agents to perceive that they are or may be in disequilibrium and to act accordingly. This would also require them to act to change prices in some manner.

I first began to build such a model in the early 1970s (Fisher 1972). I noted that, if we considered each seller as setting his or her own price,¹⁷ then we can also consider each seller as being the only player on the supply side of a 'market'

consisting of that seller's goods. The potential buyers from that seller or 'dealer' constitute the demand side. In that case, the Hahn-Process assumption seems very realistic indeed: Either demand equals supply or the dealer sells out and there are unsatisfied buyers or all the buyers are satisfied but the dealer has goods remaining that he or she had planned to sell. The dealer then moves the price in obvious direction and we have done away with the 'auctioneer'. (I remarked at the end of the paper that I hoped this would be regarded as an attempt to play *Hamlet* without the Prince of Norway. I meant exactly that, since the Prince of Norway is Fortinbras who appears at the end of the play for the sole purpose of cleaning everything up.)

Of course, the difficulty comes when we try to model things so that the different dealers in the same commodity eventually all end up charging the same price. That is not easy to do, and I went on to consider that sort of problem in a far more ambitious model, a descendant of the Hahn Process.

That model appears in full in Part Two of my 1983 book (Fisher 1983). There I allowed agents to have non-naïve expectations and to realize that prices might change and that purchase and sale plans might not be fulfilled. This meant that agents would revise their expectations when they failed to come true and the dynamic process continued. The resulting model was sufficiently complicated that I could not deal with anything more complicated than point expectations. (As I later realized, I was modeling the behavior of a world full of economists: they were often wrong, but never uncertain.)

I found it fairly easy and also instructive to model the behavior of individual agents under these circumstances. Both for firms and for households, sensible results on arbitrage arose from the assumption that price changes were expected. But matters became more complicated when dealing with the realization that agents might expect not to be able to complete their desired transactions. This was part of the interesting difficulties that emerged when analyzing the behavior of all agents together, equilibria, and the stability question.

In this connection, it is appropriate to remark that Walras's Law no longer holds in its original form. Instead of the sum of the money value of all excess demands over all agents being zero, it now turned out that, at any moment of time, the same sum (including the demands for shares of firms and for money) equals the difference between the total amount of dividends that households expect to receive at that time and the amount that firms expect to pay. This difference disappears in equilibrium where expectations are correct, and the classic version of Walras's Law then holds.

As this suggests, money appears in the model, and also, the model is one which runs over time, with commodities dated as to time. Among the results related to this are the following:

1. The usual Arrow-Debreu (Debreu, 1959) formulation of general equilibrium has all markets, including those for future commodities, clearing at the same time, and transactions cease. Hence markets just disappear before consumption and production take place. In the model under discussion, this is not the case.

In equilibrium, transactions continue to take place but at correctly foreseen prices.

2. The model contains markets for government bonds of different maturities. It is a continuous time model. Money becomes a government bond with instantaneous maturity. With this device, the old Patinkin problem (Patinkin 1949, 1950, 1965) as to why agents wish to hold money in equilibrium disappears, because money is still required as a medium of exchange.
3. This also means that agents will realize that they may run out of money, but, in this model, they can take steps to avoid this, making the avoidance of the Simple Simon problem more reasonable for the Hahn Process than simply assuming it away.

Unfortunately (depending on how one looks at it), the equilibria in a model where agents are conscious of disequilibrium do not have to be Walrasian. This is caused by the realization by agents that they may not be able to complete their planned transactions.

More specifically, (for convenience, taking firms to be sellers and households to be buyers), if firms set prices and buyers search for the lowest price, then¹⁸ firms will come to realize that lower prices bring more customers and, hence, that they have some market power because they face declining demand curves. In such a case, the firm's perceived demand curve provides a perceived constraint on how much it can sell at any given price. Since the firm will choose the profit-maximizing price corresponding to its perceived demand curve, this may lead to situations in which customers would in fact buy more at lower prices but no firm lowers the price. Indeed, such situations will persist unless firm's perceived demand elasticities grow (algebraically) larger. In the labor market, this becomes the Keynesian liquidity trap (Keynes 1936).¹⁹ This is, in fact, directly related to constraints on money felt by agents.

To avoid this, requires the assumption that firms experiment with price lowering (or wage raising) and find out what is going on. But it is hard to make that more than an *ad hoc* assumption.

Note that the question of who sets the price has been answered. But note also that the Samuelson-suggested mechanism whereby prices change in the direction indicated by excess demand has been abandoned together with the fictitious auctioneer.

Towards a more satisfactory treatment: what about stability?

But what about stability in all this? Will such a system converge to equilibrium, either Walrasian or not? This is a really interesting question with, I think, a rather disappointing answer.

The stability of the Hahn Process was based on a model in which expectations were constantly disappointed and expected utilities never increased. By contrast, the Edgeworth Process was based on a model in which the utilities that would be

gained if trading stopped were never decreasing. But, of course, these were both models in which agents always expected to complete their transactions at the prices then current. In effect, the agents in both these trading processes act as though they always think the world is in general equilibrium, despite the evidence that it is not.

When we come to a model in which agents understand that prices change and that transactions may not be fulfilled, the stability problem becomes deeper. Here, the first thing to notice is that when agents are disappointed, the utilities they expect to get out of the process decrease. This makes a generalization of the Hahn Process an appealing tool, while it essentially destroys the Edgeworth Process's central mechanism for proving stability.

But there is a deeper lesson here. In such a model, where agents act on perceived opportunities, there can be no hope of proving theorems of general stability without strong assumptions. For consider the following: Equilibrium is about to be reached. The great day and hour are at hand. The *Wall Street Journal* reads the banner headline. But then some agent or group of agents *who have money to back their ideas* suddenly come to believe that the price of some commodity (or stock) is about to rise. It does not matter whether that expectation is correct or not; so long as the agents involved act on it, equilibrium will at least be postponed.

This is not a new thought. It is central to Joseph Schumpeter's vision of how a competitive economy works, first articulated in 1911 (Schumpeter 1951) and then central to his later works (Schumpeter 1939, 1962). A new innovation arises and disturbs the existing equilibrium. Such an innovation need not be technological; it can consist of the discovery of new uses for old products, of new sources of raw materials, of new markets, of efficient forms of organization, or of the coming into wealth of agents who previously could not afford to invest in such discoveries. Once a Schumpeterian shock occurs, the economy works to absorb it and restore equilibrium.

But notice two things. First, the new discovery need not be real. If agents believe it is and are willing and able to invest in it, then equilibrium will be disturbed. Second, only if the adjustment to equilibrium (assuming there is one) is speedy enough to occur before the next shock will equilibrium be reached or approximated.

But, if even stability is difficult to prove in a general model, the analysis of speeds of adjustment is even more so.

Accordingly, I was able to prove a stability theorem for what is, in effect, a generalization of the Hahn Process, only by assuming that generalized Schumpeterian shocks cease. The crucial assumption, which I called 'No Favorable Surprise' (hereafter, 'NFS') was that (after an initial period of time), all sudden changes in expectations are disappointing. That does not mean that things must always change for the worse. It only means that the only surprising changes must be disappointing.

Such an assumption is not easy to support. While it is easy to take the view that, for example, the European discovery of America was an unexpected favorable shock and naturally disrupted any preexisting equilibrium, it is not so clear how

one can rule out the strong possibility that the very adjustment to that shock itself turned up more favorable surprises. My present position on the matter is that this is to be expected (or if not, it comes as an unfavorable surprise). The search for stability at great levels of generality is probably a hopeless one. That does not justify economists dealing only with equilibrium models and assuming the problem away. It is central to the theory of value.

A final word

I wish to dedicate this paper to the memory of the late Carl Kaysen. Fifty-seven years ago, in 1954, Carl discovered me in an elementary undergraduate course, immediately pushed me into Harvard's graduate program, and himself undertook to teach me microeconomic theory. He remained my mentor, my colleague, and my friend until his death in early 2010. When he was my tutor, he urged me to take as my task for an undergraduate thesis the formalization of the Schumpeterian model. I failed to succeed at that, but have now, in a real sense returned to that problem.

Notes

- 1 Of course this means unique for a given set of initial allocations of goods and factors of production. Equilibrium varies when those change.
- 2 For an extended treatment, see Fisher (1983).
- 3 I give two instances of this. Long after the literature on non-*tâtonnement* processes had appeared, a truly great economist asked in a seminar attended by both Frank Hahn and myself 'Whatever became of "trading at false prices"?' And another noted economist who had worked on stability theory asked me following my presidential address to the Econometric Society in 1979 what my discussion of such processes had to do with Scarf's example of instability.
- 4 That is far more descriptive than 'non-*tâtonnement* processes'. Calling a function 'non-linear' does not give much information about it.
- 5 See also Hahn (1962).
- 6 The original paper is Hahn and Negishi (1962).
- 7 The standard case is one of pure exchange. The method can be extended to firms (Saldanha 1982).
- 8 Although, as with all trading processes, the equilibrium to which it converges depends on the trades that take place.
- 9 But see the footnote at the end of the next paragraph.
- 10 Note that it is needed in the stability proof for the Edgeworth Process.
- 11 Private communication.
- 12 NP-hard to find, in fact, as shown by my student A. D. Tsai (1993).
- 13 Note that this partially solves the problem raised above as to trade for later retrade, but that solution is only partial, since agents still cannot trade for expected later gains.
- 14 A common personal experience.
- 15 As with the Edgeworth Process, the Hahn Process can be extended to include firms. I did this in the mid-1970s (Fisher 1974), although, just as the households in pure exchange do not consume until equilibrium is reached, the firms do not produce until that time.
- 16 Known to economists as 'the Clower Problem' (Clower 1965).
- 17 One can also deal with buyers, but, for simplicity, I shall consider only sellers in this regard.

18 See Rothschild (1972) discussed above.

19 It is interesting that the two major realizations of 1930's economic theory—the fact that the liquidity trap can keep an economy in a non-full-employment state and the introduction of monopolistic competition turn out to be related.

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Edited by Pascal Bridel

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