10 Stability Analysis in Micro and Macro Theory: An Interview

Franklin M. Fisher

Feiwel: Are there any differences in the treatment of questions of stability by mathematical economists (say, since Samuelson) and the literary economists pursuing Keynesian questions?

Fisher: Aside from the question of tools employed, one could reasonably say that these two groups of economists are talking about the same topic in different ways. More precisely, stability involves the question whether in a dynamic system rest points (I often use this term to mean equilibria) are approached if one departs from them. As I understand the Keynesian question, it asks: if you have a competitive economy that for some reason departs from full employment, will it tend to return to a full-employment equilibrium or will it tend to get stuck in some underemployment equilibrium or, for that matter, will it tend to wander without reaching any equilibrium? That is a macroeconomic problem and like most macroeconomic problems it is a description in very aggregate terms of a much more complicated microeconomic problem. Let me now rephrase the same question, perhaps more generally. One could ask in general equilibrium terms: one has an economy consisting of many different agents, many different markets, many different prices, many different goods, and so on, and there are equilibria at which all markets clear. If one starts from a point at which that is not true, does the dynamic system that describes the entire economy tend to return the economy to a position where all markets clear? Does it tend to return the economy to some other stationary point? Or, does it tend to wander forever?

In the most general terms, the Keynesian problem could be described as exactly the same. The question of approaching other stationary points is the question whether the dynamic system has a tendency to approach positions where the economy gets stuck, perhaps because some people think that they cannot buy as much as they want; they cannot buy as much as they want because they are unsure that they can sell, and that sort of thing. In recent years that has become known as the Clower problem, but, in one form or another, it is the same as the Keynesian problem.

I would say that the Keynesian approach to macroeconomics (or the Keynesian problem in macroeconomics) is an attempt to deal, using rather coarse and aggregate tools, with a central problem of stability analysis more rigorously dealt with, with much finer tools, but unfortunately not with better solutions, within the context of general equilibrium theory.

Feiwel: Some scholars claim that the *General Theory* was about the instability of the capitalist economy and the tools to correct it. It is also this kind of instability with which, say, Hy Minsky is at pesent concerned. Is this sort of question of interest to modern theorists?

Fisher: That is not the question studied by stability theorists, but perhaps it ought to be. One can describe that as an instability question, with the economy embedded in a much larger dynamic system describing politics, history, and so forth, but nobody does that. I suppose it is true that one can have a dynamic model of a general equilibrium system which would show that the inevitable result is some sort of chaos or permanent underemployment and that there is no way to fix it. But that would be a rather special result. There are very few modern stability theorists, in my sense. Three of them are at present in this building [Encina Hall, Stanford University, summer of 1986] – and that is a very large fraction of the total. No, that is not what we mean when we talk about people working on stability in general equilibrium.

Feiwel: In an attempt to correct some of the critics' misperceptions, would you share with us what the notion of equilibrium means to you?

Fisher: Within the context of this discussion an equilibrium has to mean a rest point – and I shall elaborate on this presently. There is a lot of confusion in the use of the terms. A rest point refers to the action of a dynamic system. It is a point such that if the system gets to it, there is no tendency for the system to move away from it. 'Equilibrium' is often used in economics to mean a point at which markets clear. 'Equilibrium' is sometimes used, in general equilibrium theory anyway, to mean 'Walrasian equilibrium'. There certainly are dynamic systems that have the property of having rest points that are not Walrasian, for example, such as the Clower problem and the Keynesian problem. The term 'equilibrium' has no meaning unless it means a point such that when you are at it, there is no tendency to depart from it. Of course, one can speak in terms of temporary equilibria, partial equilibria, moving equilibria, and the like. Those usually mean that some things are held constant, but not all things. For example, one can think of an equilibrium for a given population size. In a dynamic system where the population would be held constant, that would mean a point from which there is no tendency to depart. In a larger system where there is a population dynamic, that would not be a rest point.

Feiwel: What does the concept of stability mean to you? What are the achievements and lacunae of stability analysis?

Fisher: I think that stability and disequilibrium dynamics is *the* principal unsolved problem of economic theory – both macro and micro economic theory. Economists are supposed to know how the price system works and how resources get allocated. We do not actually know how the price system really works in time. We have an elaborate and very elegant theory of individual agents, how they make plans and an equilibrium theory of what they do when these plans get fulfilled. But we have very little theory – and this is what stability is about – about what happens if they start from a position where the plans are not mutually compatible. We do not actually know how these things get adjusted.

Stability theory is very difficult. Modern stability theory began in the late 1930s with the introduction of serious dynamics into the study of markets. But, perhaps because equilibrium theory at the level of the individual agent is so much easier than disequilibrium theory, very little attention was paid for a very long time to the question of modelling the process that goes on out of equilibrium. That had a couple of consequences: one of them is the long-run consequence that people simply ignore the fact that it was a problem at all, and they talk about things as if one could talk about equilibrium all the time without worrying about the dynamics of a system where equilibria are embedded. The other was that the early models worked in terms of *tâtonnements* where the only things that changed out of equilibrium were prices and prices moved in the direction of excess demand. Very little attention was paid to the question of *why* they should move in the direction of excess demand, in the sense of whose behavior was it that was being described. The models resorted to a fiction – the auctioneer. Since, in fact, individual firms adjust pries, that is not very satisfactory. Very little attention was paid until well into the 1960s to the question best put as follows: in a model where everyone takes prices as given, how do prices ever change?

In any event, the study of *tâtonnements* – a study of a very unrealistic model, not only because the price change was obscured, but because it was as if nothing ever happened until equilibrium was reached – turned out to be very unsatisfactory. It did not actually get very far. It ended in a position that said: that sort of thing is stable if one is willing to make very, very strong restrictions on the nature of utility functions, production functions, and so on. About 1960 Herbert Scarf showed that, in some sense, one needed such strong restrictions; that there was not going to be a general theorem.

At about that time the subject began to advance in a more serious way because, in being forced to think about how to model trade out of equilibrium – a departure from *tâtaonnement* – people had to think about sensible trading rules. Two of them were suggested: one by Uzawa who suggested that one might make use of the proposition that people only trade when they think they are going to gain something. The other by Hahn who suggested that what we mean by markets is a system sufficiently organized that after trade there are not both people who want to sell and cannot and people who want to buy and cannot; they can meet each other (this is now sometimes called the orderly markets assumption). Fairly quickly, with both the Uzawa and the Hahn process (Uzawa named his the 'Edgeworth process', Hahn's was called the 'Hahn process' by Hahn's co-author, Negishi), it became evident that we were on to something. We now had simple models of at least exchange where one could prove stability all the time. Thinking about what was wrong with those models, the subject was on to something that turned up increasingly interesting problems. In particular, it is very hard to talk about the Hahn process very long without introducing money in a serious way. And there is a whole set of problems associated with that. That was the early 1960s, the introduction of money is perhaps 1970, the introduction of firms is perhaps the middle 1970s.

Since then not many people have worked on this. My principal work centers on the introduction of a model where the agents are not stupid. One of the problems with stability theory, at least through the mid-1970s, was that the agents involved never realized that they are in disequilibrium. They always behave stupidly as if prices will never change and as if they will complete their transactions, even though common observation reveals that is not the case. An obvious question – perhaps *the* central one – is the following: if you have an economy characterized by sensible agents (possibly rational agents) who understand that they are in disequilibrium and take advantage of arbitrage opportunities, is it or is it not true that the actions of those agents will push the economy towards an equilibrium? And, if it does, will that equilibrium be Walrasian or not? (That, as I said before, is in part the Keynesian question.)

I say that is *the* central question because all of economic theory (at least all of microeconomic theory) presumes that the answer to that question is: yes. It presumes that one really can deal with economies as if they were always in some sort of equilibrium – not necessarily Walrasian equilibrium. Indeed, all the rational expectations literature also assumes that the answer to that question is yes. Yet it is not obvious that the answer to that question is yes. There is – and here I am wandering a little – some confusion between the proposition that if one is not at an equilibrium, there is a tendency to move – which is perfectly true, in fact, that is the definition of not being at an equilibrium – and the proposition that, therefore, there must be a tendency to approach equilibrium – which is a widely different proposition. For instance, rational expectations theorists typically mistake the proposition that if there is a systematic opportunity to make money, people will take advantage of it – which is perfectly true – for the proposition that, therefore, the economy must always be in positions where there are no systematic opportunities to make money.

That may be true, but it requires proof – a proof that lies with what I mentioned before as the central question. Namely, does an economy of rational agents, acting on arbitrage opportunities, make those opportunities disappear and restore equilibrium?

My work on the subject has tried to build models where the agents actually do understand what is going on and plan in quite sophisticated ways to take advantage of it. That is an interesting thing to do, but the answer turns out to be very difficult. I do not know what the answer to that question is. I only know what the answer is under some strong general circumstances. Namely, if in some very strong sense, the agents do not keep on perceiving new opportunities for arbitrage, then it is true that the old opportunities will disappear. One can show that in quite a general way. That is more or less what one wants, but only more or less.

What one would like to have is a proposition that says: suppose there is a shock to the economy – Columbus discovers America or a Schumpeterian entrepreneur makes a new invention. What one would like to be able to show is that the actions of agents who respond to the opportunities turned up by that bring the economy back to some point of equilibrium. And one would even like to show that it happens very quickly so that one can analyze the economy by looking only at equilibrium. And one wants to show that it happens if the economy is not disturbed again. Unfortunately, that is not what I show. The problem is that what I can show is that, if the economy is not disturbed again *and* if the process of adjustment does not keep on turning up opportunities not foreseen by the agents when they started out, one does get to an equilibrium, which may not be Walrasian, by the way. The problem is that second condition: after Columbus discovered America, there were many opportunities no one ever thought of, opportunities turned up by the interaction of agents who acted on that discovery. And, that is not so simple!

What one knows at present is, I think, a very weak, very general stability theorem. It appears as if the more one moves into the direction of a realistic model where agents understand what is happening, the weaker the results tend to be. It is a hard subject. If I had to point to the weakness of stability theory, I would say that for a very long time it did not really ask the central question head on. When it does, it turns out that one gets many interesting digressions and some answer, but not a very satisfactory one. Of course, I would like to turn that around and say: 'The weakness of economic theory is that people do not work on this question.'

Feiwel: If one were to look for imperfections of competition as one of the problems explaining the prevalence of disequilibrium, how would this affect the study of stability?

Fisher: Nobody really understands much about how to deal with that kind of complication. The closest there is is Negishi's *General Equilibrium Theory* *and International Trade* where he does deal somewhat with that problem. Imperfect competition is hard enough to understand in equilibrium terms. Understanding what happens out of equilibrium in a general economy is very difficult. I am not now talking about specific partial models or particular markets. It is hard because until one understands how a competitive economy operates out of equilibrium, it is probably harder to understand in a formal way how a non-competitive economy operates.

Feiwel: Paul Samuelson once quipped: 'My theory is dynamic, every one else's is static.' To dot the i, how do you recognize that a theory is dynamic?

Fisher: It is not dotting the i, but dotting other variables because dots usually stand for time derivatives – putting dots on p, actually. Seriously, dynamic theory has to talk about the way in which things change all the time and the paths that they take. I do not know when Paul said this, but he might well have said it in the early 1940s when he was writing *Foundations of Economic Analysis*. He was probably reacting to the fact that in *Value and Capital* Hicks had introduced what is now known as Hicksian perfect stability. That has to do with whether demand curves slope down in various complicated ways. To a modern eye, the thing that stands out about that is that the discussion about whether or not demand curves slope down in such ways – however interesting it is, and we now know it is very interesting for the question as to whether there is unique general equilibrium – has nothing whatever to do with changes over time. It tells you nothing about what happens out of equilibrium. Of course, what Hicks thought he was doing was describing something which meant that, in some sense, if the economy was displaced there would be a tendency to come back. But there is no explicit modelling of time. What Paul did very early was to observe that fact and then produce the first kind of serious analysis of an explicit adjustment process in general equilibrium theory.

Feiwel: You once mentioned a distinguished economist (Friedman?) who quipped: 'It is obvious that the economy is stable and if it is not we are all wasting our time.' Would you respond in a serious way to this cavalier statement?

Fisher: Certainly. In the first place, I do not think that was a perceptive statement. In the first chapter of my book I said some specific things about this. The only reason I do not name him there is that he sort of said this to me in passing almost thiry years ago and I would not want him to get stuck with it if he does not remember it by now. I remember the occasion, but I bet he does not.

In the first place, it is not obvious that the economy is stable. A position of stability for general equilibrium would be a point at which relative prices do not change. That is not true; relative prices do change. They change all the time! It is perfectly possible that the economy is stable in the sense that the equilibrium is moving and the economy is always rather close to the moving equilibrium and that accounts for the relative price changes. But I do not think that you can look at the economy and say, 'Yes, that is obviously true.' What he probably meant when he made that statement was: 'It is pretty obvious that most markets clear most of the time.' Probably that is true. It is not clear, however, that most markets are in long-run equilibrium much of the time. Supply and demand do change. It is a close question whether it is obvious that the economy is stable.

That, however, is not the unperceptive part of the remark. The unperceptive part of the remark was the rest of it. By the way, the full remark was: 'There is no point in studying stability because it is obvious that the economy is stable and if it is not we are all wasting our time.' The truth of the matter is that we are all wasting our time in some sense if the economy is not stable, because all of microeconomic theory rests on the proposition that one can analyze equilibria. If the economy is not stable, the properties of equilibria do not matter very much and incidentally all of welfare economics is down the tube and so is the prescription that the government ought not to intervene – something in which this particular economist is quite interested.

The real question is: does the theory lead to the proposition that the economy is stable? If it is not true that one can embed competitive equilibrium in a disequilibrium story that is stable, there is no point in discussing competitive equilibrium. So, it is a matter of considerable intellectual importance to study stability theory, in order to understand whether that is true or not. I must say that the study of stability theory does point to a couple of lessons that suggest that these considerations are very important: one proposition is that if one takes a dynamic system that describes a competitive economy, it is quite difficult to make that dynamic system have a competitive ending, even if it is stable. The second proposition – and this is quite a general proposition, one whose importance is simply not perceived – is that everything we know about stability says that one cannot get stability out of a dynamic system where only prices change. One gets stability where trade takes place out of equilibrium. And, if trade takes place out of equilibrium, it means that the place to which the economy goes, given initial conditions, is not the place to which static equilibrium theory would predict the economy would go. What that means in more general terms is that comparative statics – a major tool of static equilibrium theory – is on very, very shaky ground.

Feiwel: Does the real world offer hints to the stability analyst?

Fisher: Certainly. That is why the prominent economist we mentioned before said that obviously the economy is stable. If one looks around one typically does not see long lines of people who want to buy things the stores are not carrying. That suggests that there is no gross kind of instability all the time. The economy appears to function reasonably well. Of course, that was not true during the Great Depression. But it looks as if most of the time assuming markets are in equilibrium is a reasonable approximation. The problem is that we are not really sure why.

Feiwel: What about unemployment? Is it voluntary?

Fisher: That is an important issue. In what I just said I was overlooking unemployment. I did not mean to imply that when one looks one necessarily sees that labor markets clear all the time. On the other hand, even if labor markets do not clear, that would not tell you which of two propositions you were seeing, and I am sure there are more: one proposition is that the economy is not stable. The other proposition is that the economy is stable and it is, in fact, at a rest point, but that rest point happens to be a Keynesian one – that equilibrium involves unemployment. That is possible also. What I said in answer to your previous question was: 'If you look around and it looks as if most markets clear, then you know you are fairly close to a Walrasian equilibrium and that suggests that something good is going on.' But I take the point, we have something like 7 per cent unemployment. We have had it for a long time. I do not believe unemployment is voluntary. That suggests that either we are stuck at an underemployment equilibrium or the economy is not stable. And just from that you cannot tell which it is.

Feiwel: Some macroeconomists tend to see very little difference between the picture of an economy stuck in a very bad equilibrium, say, 10 per cent unemployment, and one where the economy moves exceedingly slowly towards full employment. Theoretically is this a correct approach?

Fisher: I almost never comment directly on macroeconomic issues, but I will comment on that one in a more general form. First of all, in some policy sense this statement is true. In order to justify the use of equilibrium tools, one has to believe the economy is close to equilibrium most of the time. Since we know that there are shocks, that requires believing two things: first, that the economy is stable, in the sense that it can absorb shocks; secondly, that it absorbs shocks quickly so that the speed of adjustment is very fast.

Feiwel: This depends on the endogenous economic mechanism?

Fisher: Yes, the endogenous mechanism that could absorb shocks quickly. The above statement is quite correct in two senses: The first is if one is going to work with theory that says all one has to worry about is full employment, and one never need think about anything else, and, in fact, it takes a very long time for unemployment to be reduced to full employment, then by the time that happens many other shocks would have occurred. Looking at the full employment equilibrium to which the system would move if one left it alone and gave it enough time is not informative as to where the actual economy is going to be. It will never be informative because what will matter is the transient behavior – what happens on the way to equilibrium – and not the equilibrium to which it gets if the system were left undisturbed. There is, of course, the second point in terms of policy, in terms of welfare economics, and in terms of the losses to the people who are unemployed. It would not have been very comforting to the people on the bread lines in the 1930s to have been told that if they just live long enough in 100 years full employment would be restored.

Feiwel: Indeed, broadly speaking, the critics of equilibrium (like Joan Robinson) say that since what characterizes an economy is change, and circumstances always alter, the concept of equilibrium as such is inapplicable. May we have your comments?

Fisher: I think that is wrong. It depends what one means by equilibrium; what dynamic system one has in view. You can imagine certain things that change slowly, let us call them parameters such as population size. That changes, of course, but it does not change very fast. Then there are things that change rapidly, such as prices, quantities, and things like that. It is perfectly possible that the following is true: at any level of the slow moving parameters (for instance, populations size), the fast moving variables adjust very quickly and the economy is stable in the sense that it is near equilibrium all the time, that is, for the fixed level of the parameters. Then the parameters change (for instance, the population grows) and the adjustment process in the fast moving variables takes place very quickly, so that, once again, one is very close to equilibrium. And that happens so fast that the economy can usefully be described as in a moving equilibrium, whose movements are controlled by the movements of the slow moving parameters. That would be a version where the economy is changing all the time. But it does make a lot of sense to talk about it in terms of equilibrium. If one wanted to redefine the problem somewhat differently, once could think of equilibrium as an equilibrium path, rather than an equilibrium state. Equilibrium does not have to be a point; it could be a time path to which the system tends to return if disturbed.

Feiwel: When a good econometrician speaks of structural change, does he in a sense mean what you were saying?

Fisher: In some sense that is quite right. Econometricians specify structural equations – equations that describe behavior and are estimated over some periods of time. Typically econometricians do not attempt to behave as though the same parameters, the same coefficients, and the same structural equations describe phenomena over very, very long periods of time. Nobody supposes that the demand for wheat in the UK is the same in the 1980s as it was in the 1780s, though it may well be characterized by some of the same variables. The reason for that is that, of course, some things do change: tastes change, populations change, new goods appear, and so forth. That does not make it any the less useful to say that over periods of time, involving twenty or thirty years, one can, in fact, describe the economy with stable equations. If one does not think that in some sense that is true – that is, that given the appropriate variables, one can describe the economy and economic behavior in equations that will stay put long enough to be worth looking at – then there is no point in talking about economic theory at all, equilibrium or not.

Feiwel: Is there anything else you would like to emphasize about the difficulties of stability theory?

Fisher: Although I have said much of this in passing, I would like to stress that too many people think that stability theory is a poor subject. In fact it is a subject with poor answers to very important questions. People tend to shy away from it sometimes by confusing the notion of what equilibrium means. One version, for some rational expectations economists, defines everything as an equilibrium, in the sense that something happens; there is an outcome. Therefore, people must have been at least temporarily satisfied to produce that outcome. That is not a sensible way of answering the question whether the economy is stable, because one has to answer why that outcome instead of another. If the notion of equilibrium is to mean anything, one has to admit the possibility of points that are not equilibria. Otherwise there is no content. So, one always has to think in terms of smaller or larger dynamic systems where the rest points are what we call equilibria and then ask: what happens if one is not at that rest point? As I said before, there is a tendency to confuse the view that if one is not at an equilibrium, one will not stay where one is, with the view that one must approach equilibrium – and that is quite a different and much harder proposition. Perhaps because equilibrium tools are so elegant and so relatively easy, economists always work in terms of those. As I guess I have indicated, there is a big gaping hole in the center of what economists know, namely, the question of what happens out of equilibrium and whether we ever get close to equilibrium, and so forth. It is an important gaping hole because most of what we do depends on assuming that it is not a problem. And we really have very little basis for that.

Feiwel: Given differences in taste, why does such a fascinating subject as stability analysis fascinate Frank Fisher and a few others only who are in a minority?

Fisher: Considering who some of the people are who are interested in stability, one might say that it is simply a peculiar perversion, able to be appreciated only by those of us with otherwise jaded palates. I am not sure of the answer; there are several possible ones, some of which are connected: one of them is that it is really hard . . .

Feiwel: Is it mathematically intractable?

Fisher: No. Perhaps if we had more and better mathematical tools it might be easier, I do not know. But I do not think that is the reason. The mathematics do not appear to be particularly complex. The economics is very hard. As I said before, the problem is that we have a very elegant theory of how agents, households, and firms make plans. If those plans are fulfilled, there is no need to go any further. It then becomes a natural question to ask whether there exist situations where all these plans are fulfilled. That is what one means by positions of competitive equilibrium.

To study disequilibrium seriously, one has to study what happens when plans are not fulfilled. That means asking how agents behave when they are disappointed. Well, we do not have a good theory about that. The theory of the individual agent does not help very much. To do that really right one has to endow the agents with uncertainty and to ask how they behave under uncertainty. That adds another level of complication that is not adequately handled – at least, I do not know how to handle it – in the context of a dynamic model where part of the uncertainty is generated by the actions of the agents themselves. That kind of thing is very hard. No one has actually dealt with this in the context of a serious theory. My model deals with it by assuming that agents can be wrong but they have point expectations – which makes them 'economists', by the way; they are often wrong, but they are never uncertain.

Thus one reason why so few people work on stability is that to do so one has to depart from the usual ways of thinking. And the results do not pay off a lot. So, one possibility is that the marginal costs are very high and the marginal revenues very small. Economists can make maximizing calculations better than most people. Yet, there is more to it than that. The fact that there is a major problem is typically unnoticed and is not taught to students as a major problem. Exactly why that is, I am not sure.

Another possibility is historical. As I know somewhat more about the history of this subject than most people, I find it a little hard to understand. It is fair to say that there was a big boom in stability theory, such as it was, in the 1950s. That big boom was the study of *tâtonnement*, produced very largely by the publication of the papers by Arrow and Hurwicz, and Arrow, Hurwicz, and Block. For a short time, it looked as though there was going to be a lot of mileage in this subject. Particularly Japanese authors began to turn out papers that tried to generalize the proofs of *tâtonnement* stability from things like gross substitutes to weak gross substitutes and to various other minor extensions. There was a sort of mini-industry in this. Then about 1960 came Herb Scarf's paper which – in my present interpretation with twenty-six years' hindsight – said pretty clearly: *tâtonnement* is not always stable and, what is more, it is not even almost always stable, because we *now* know that the Scarf example is true on an open set. Without very strong assumptions it is not going to be true that one can prove stability. The assumptions under which one had been able to prove stability under *tâtonnement* were very, very strong and very, very unrealistic. The subject appeared to have come to a dead end right there.

What is peculiar is that it did not in fact come to a dead end. At about the same time the next stage of the subject was taking off, namely, the study of what happens when there is trade out of equilibrium, involving the Hahn and Edgeworth processes with which the names of Hahn, Negeshi, and Uzawa are associated. One could get a lot further with that. I have found, however, that even today, even among my own colleagues (not so much among the latter because I have been bullying them about this for ten years), when one says one is working on stability, it is tacitly assumed that one is working on *tâtonnement*. One's interlocutors immediately think: that is a silly model, it does not involve anyone's behavior, and we know it does not go anywhere, why would anyone want to be working on this? The existence of a whole subsequent literature (few people, but a lot of articles) is simply overlooked. There is then a tendency to say: 'Well, there are not enough results to teach students.' And so, one doesn't. I think that students are, in fact, steered away from the subject.