CHAPTER 1

Introduction: Disequilibrium analysis and the theory of value

1.1 Introduction

Economists, particularly economic theorists, are most often concerned with the analysis of positions of equilibrium. This is most obviously true in microeconomics, where general equilibrium theory stands as the most complex achievement of rigorous analysis; but it is becoming true of macroeconomics as well, where it has become increasingly popular in rational expectations models to assume that markets always clear.

Less attention is given to disequilibrium. In microeconomics, the subject of the stability of general equilibrium is in poor repute. Too many economists (including economic theorists, who should know better) apparently believe that stability theory means *tâtonnement* – a branch of the subject that died in 1960 and was long ago superseded.[1] They regard it as overformal and empty of results, save under the most extreme ad hoc restrictions, and without much relation to the rich and complex world of real economies.

For macrotheorists the concentration on equilibrium manifests itself in other ways. Aside from the rational-expectations–market-clearing position already mentioned, one currently fashionable branch of the subject investigates fixed-price, quantity-constrained equilibria. Such investigations can be very fruitful, but they are not truly disequilibrium investigations, although they are sometimes misnamed as such. They are analyses of equilibria that are non-Walrasian. The question of whether

[1] I review the stability literature in Chapter 2.

and how the system gets to such an equilibrium is not solved by showing that such points exist.

Yet disequilibrium theory and, in particular, stability theory are of basic importance to economists. The proposition that the equilibria of economic models are not only stable but that convergence to a neighborhood of equilibrium is achieved relatively quickly[2] turns out to be a necessary foundation for the equilibrium analysis of economic theory. If stability theory is unsatisfactory, then that foundation is lacking. The proper conclusion is then either that the foundation must be soundly laid or that the structure based upon it must be drastically altered.

The object of this book is to begin to lay such a foundation. Since the necessary first step is an adequate theory of stability, it is in this area that efforts are largely concentrated. These efforts turn out to be moderately successful; in the model of Part II I am able to show that an economy with rational agents acting on perceived arbitrage opportunities is in fact driven to equilibrium if new opportunities are not continually perceived to arise. I argue that this is the most one can possibly hope for as a basis for a general stability result. It is only a basis, however; the question of just what processes are characterized by such perceptions then becomes central.

The results are not limited to this stability theorem, however. By building a full model of disequilibrium behavior we obtain considerable insight into a number of areas. These include the nature of fixed-price, quantity-constrained equilibria, the role of money, the behavior of arbitraging agents, and the function of the stock market. Although much of what is said is not new, the provision of a full-dress disequilibrium model may help to put these matters in perspective and to provide a framework for answering the many important questions beyond stability that remain open.

The plan of the book is as follows. In the remainder of the present chapter, I discuss the importance of disequilibrium theory and the reasons for a general rather than a partial treatment. Part I considers the existing literature on the stability of general equilibrium (not just tâtonnement!), its successes, its failings, and its methods. That consideration sets the stage for the full-scale model of Part II, which seeks to analyze the disequilibrium behavior of an economy with arbitraging agents.

The matters discussed in this book are of considerable interest to the general economic theorist. I have therefore kept technical matters separate. The nontechnical reader, by omitting those chapters and sections

[2] Throughout this book, the term *speed of convergence* refers to this.

marked with an asterisk should be able to follow the material without loss of continuity, although, inevitably, with some loss of precision. The technical reader, on the other hand, should read the unstarred as well as the starred sections, for the material is not always repeated.

1.2 The importance of disequilibrium analysis

As already stated, microeconomic theory is primarily about positions of equilibrium. The plans of agents (usually derived from the solution of individual optimization problems) are taken together, and certain variables – usually prices – are assumed to take on values that make those plans mutually consistent. Comparative static analysis then proceeds to compare equilibria corresponding to different values of underlying parameters.

In all this, very little is said about the dynamics of the process that leads an equilibrium to be established in the first place or by which the system adjusts to a new equilibrium when the old one is displaced by a parameter shift. Attention is centered on the equilibria themselves (with some awkward problems when they are not unique), and points of nonequilibrium are discussed by showing that the system cannot remain at such points.

But showing that disequilibrium points will not be maintained is necessary but very far from sufficient to justify analyzing only equilibria. The view that equilibria are the points of interest must logically rest on two underlying properties about the dynamics of what happens out of equilibrium. First, the system must be *stable*; that is, it must converge to some equilibrium. Second, such convergence must take place relatively quickly. If the predictions of comparative statics are to be interesting in a world in which conditions change, convergence to equilibrium must be sufficiently rapid that the system, reacting to a given parameter shift, gets close to the predicted new equilibrium before parameters shift once more. If this is not the case, and, a fortiori, if the system is unstable so that convergence *never* takes place, then what will matter will be the "transient" behavior of the system as it reacts to disequilibrium. Of course, it will then be a misnomer to call such behavior "transient," for it will never disappear.

A little more detail may help to bring the point home and may also serve to show that the problem is not limited to microeconomics. Consider models of rational expectations. In such models, analysis generally proceeds by finding positions of rational expectations equilibrium if they exist. At all other points, agents in the model will have arbitrage opportunities; one or another group will be able systematically to improve its

position. The possibility of such arbitrage (plus the assumption that agents are smart enough to take advantage of it) is enough to show that points that are not rational expectations equilibria cannot be points at which the system remains; the process of arbitrage will drive the system away from such points. Yet this, by itself, is not enough to justify analyzing the properties of rational expectations equilibria as though such equilibria were all that mattered. The fact that arbitrage will drive the system away from points that are *not* rational expectations equilibria does not mean that arbitrage will force the system to converge to points that *are* rational expectations equilibria. The latter proposition is one of stability and it requires a separate proof. Without such a proof – and, indeed, without a proof that such convergence is relatively rapid – there is no foundation for the practice of analyzing only the equilibrium points of a system which may spend most or all of its time far from such points and which has little or no tendency to approach them. To sum up, the standard treatment of economic theory as an equilibrium subject is very incomplete without a stability proof (and an analysis of adjustment speeds). This may or may not make disequilibrium theory directly useful as a tool in the study of real-world economic phenomena, but it makes it indispensable as a basis for those tools which are typically employed in such study. (As we shall see, disequilibrium considerations also lead to further insights as to how those tools should be applied.)

The basic issue with regard to stability can be illustrated by considering a remark made to me long ago by an extremely prominent economist. He stated that the study of 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.

Consider first the question of whether it is obvious that the actual economy is stable. Instability need not mean explosion but rather a lack of a tendency to converge to a particular equilibrium. Are real-world economies stable? It is hard to know. Certainly it is not the case that relative prices are constant; they change all the time. It is evident that many or all such changes are caused by exogenous shocks (changes in tastes, technological change, population growth, and so forth) – although the matter of which if any of such changes are properly considered exogenous may be delicate. Yet it is not immediately obvious that *all* that is happening is convergence to new equilibria; still less is it obvious that such convergence is instantaneous or so rapid that the transient disequilibrium behavior of the system responding to such shocks is unimportant.

The more perceptive part of the above-cited remark is the second half, that if the economy is not stable many, if not most economists have been wasting their time. The truth behind this lies in the possible lack of

applicability of equilibrium tools and equilibrium theory to the analysis of a system which may not be close to equilibrium at all. In such a case, however, the study of disequilibrium, if not stability, becomes the main business of economists.

However, the remark misses a point of major importance. Even if the economy is stable *and* even if it converges to equilibrium quite rapidly, this does not make the study of stability theory a matter of little importance. Quite the contrary. Without a stability theory there is no guarantee that the theoretical models used to study the economy will have the stability property which the use of equilibrium tools assumes about the economy. Put succinctly, the question is not merely whether the economy is stable but whether the models we use are stable. If the equilibrium analysis of economic theory is incompatible with stability, then there is something wrong with that analysis whether or not the economy is stable (and particularly if it is stable). If such equilibrium analysis is consistent with stability only under additional assumptions, then the study of those assumptions is surely an important matter and may yield insights about equilibrium itself.[3]

If some but not all equilibria can be the limits of stable adjustment processes this is a matter of great importance. If the equilibrium approached depends on the adjustment process, this needs to be studied. Finally, even if it were to turn out that any equilibrium was necessarily stable and that nothing further needed to be assumed to buttress the use of equilibrium tools, the proof of such a fact would be of considerable importance; it is not a matter to be taken on faith.

In brief, the question of what, if any, disequilibrium stories have equilibrium endings like those assumed ab initio by economic theorists is a question of paramount interest for such theorists especially if the world is stable.

1.3 An alternative possibility: Always-clearing markets

There is, however, another view of these matters according to which equilibrium analysis apparently needs no justification. This view, most closely associated with Lucas,[4] states that markets should always be thought of as being in equilibrium in the following sense:

Supply and demand in any particular market depend on price. Hence a market can only fail to clear because the price involved is "wrong." To fix ideas, suppose that demand could exceed supply. Then price is too low. Buyers unable to purchase will make offers at higher prices, and this

[3] In one sense, this is Samuelson's Correspondence Principle (Samuelson, 1947).
[4] See, for example, Lucas (1975, 1976, 1977).

will continue until either they obtain their desired purchases or the price has risen high enough that they no longer wish to purchase, which comes to the same thing.

Similarly, where supply might exceed demand, as in the labor market in times of unemployment, workers (suppliers) will lower the wage at which they are willing to work – simultaneously lowering the amount they are willing to work at the offered wage – until they can work as much as they want to at such wages. This includes the possibility that the wage drops low enough that they are content not to work at all.

The process just described, apart from the question of its realism, is a dynamic adjustment process. Is is essentially a tâtonnement process in which unsatisfied buyers or sellers attempt to recontract for goods or factors by changing prices. Yet the central feature of analyses based on this view is not a consideration of that adjustment process at all. Rather it is the position that all markets are best viewed as constantly in short-run equilibrium with price offers *instantly* adjusted to the equilibrium point. The movement of actual market prices is then to be analyzed as a sequence of such temporary equilibria.

But the view that markets clear instantaneously in this manner begs the question of stability. If we take the process of price offers as a dynamic one taking place in real time, then it is not obvious without a stability proof that such a process converges at all, let alone quickly enough to warrant treating all markets as perpetually clearing. Indeed, as the analysis of tâtonnement (reviewed below) suggests, stability is far from certain where we take full account (as we must) of the effects that changing prices in one market have on excess demand or supply in other markets. Only if we suppose that agents somehow leap instantly to market-clearing price offers can we avoid such stability considerations.

Can we suppose that agents in fact do so leap? There are two possibilities depending on what view we take of the nature of the prices offered by individual agents. The first possibility is that agents understand the full state of affairs in all markets simultaneously, including the effects of price in each market on all other markets. They calculate market-clearing prices and name the general equilibrium prices as their offers. Plainly this is absurd. It imputes to agents an omniscience and calculating ability that go far beyond the sensible rational expectations position that agents will act upon arbitrage opportunities. Moreover, even if we suppose that agents have complete and costless information and calculating abilities, it does not follow that it is individually optimal for each agent to move instantly to the equilibrium prices. If there are profits to be earned in disequilibrium some agents may find it worthwhile to try to earn them. (To appeal to the frequent possibility that equilibria have the Nash property

of being optimal for each agent given the actions of the others merely begs this question. Why should any agent believe that all other agents will jump to equilibrium positions or that they will have similar beliefs?) Only a complete focus on equilibrium can make the disequilibrium and stability question disappear in this way. Indeed, one might add that having gone so far we might as well go the whole distance and suppose that agents move instantaneously to a position of long-run equilibrium, leaving no need to consider short-run equilibria at all.

The second possibility is to *define* equilibrium in such a way that it is always present. Of course it is possible to do this; any outcome can be considered an equilibrium in the sense that agents do what they do instead of doing something else. But such a treatment does not get us very far; the study of what happens when the optimizing plans of different agents are not compatible simply gets renamed as a study of moving equilibria rather than of disequilibrium. Either way it is important to study.

Disequilibrium questions cannot be avoided. If "equilibrium" is to have any substantive meaning, one must be willing to countenance the possibility of encountering disequilibrium states. Once that is recognized, the stability question becomes of central importance particularly for those who wish to analyze the system as though it were always in equilibrium. A stability proof is the basic underpinning for such a position.

1.4 Why study the stability of general equilibrium?

The conclusion that the widespread use of equilibrium analysis in economic theory requires a study of stability applies with perhaps greatest force to models of general equilibrium. This is so for the following reason. As long as we are merely analyzing positions of equilibrium, partial analysis can sometimes be regarded as a logical simplification. Imagine, for example, that we seek the price that clears a particular market with all outside prices assumed fixed. Assuming a general equilibrium exists, one such vector of fixed outside prices will be that vector which, together with the market-clearing price we seek, generates a general equilibrium. In that sense, partial equilibrium is consistent with general equilibrium and supposing outside prices fixed may be a helpful simplification.

When we come to comparative statics, the situation is rather trickier, because the levels at which outside prices must be fixed to assure compatibility with general equilibrium may very well depend on the parameter the effect of whose shift is being analyzed. This will occur even if

only because such general equilibrium prices depend on the level of the market-clearing price in the particular market under analysis. Nevertheless, such partial analysis can provide insights into the most proximate effects of shifts that directly affect a particular market.

When we come to dynamics, however, it is harder to justify a partial treatment, particularly if we are interested in providing the kind of stability underpinning for equilibrium analysis which I have argued is needed. Consider the following. Convergence of a partial dynamic model in which only a subset of markets and prices is allowed to adjust is neither necessary nor sufficient for convergence of a full general model with all markets and prices adjusting. Suppose then that a particular partial model does converge to equilibrium but the general one in which it is embedded fails to do so. In such a case, the convergence of the partial model – however interesting it may be – provides no truly satisfactory justification for equilibrium analysis. This is because such positions of equilibrium will not in fact be reached unless other variables are artificially held fixed or other effects ignored, something which is not possible in the real economy. Suppose, on the other hand, that a particular partial model fails to converge but that the general one in which it is embedded turns out to be stable. Then the lack of convergence of the partial model – however interesting it may be – does not prevent us from analyzing its equilibria, since the convergence of the general model assures us that the partial model, as part of the general one, will get to equilibrium. It is true that the nature of that equilibrium will generally depend on the behavior of the general model and on the effects which adjustments outside the partial model have on affairs inside it and vice versa. This, however, is no different from the logical problems already mentioned which occur in comparative statics analysis in partial models.

To put one example of this succinctly: If we wish to show that rational agents, availing themselves of arbitrage opportunities, will drive the system to equilibrium, we must do so without artificially restricting the set of arbitrage opportunities which agents see. This means that we cannot hold some prices constant and ignore the effects of arbitrage in some markets on opportunities outside them.

There are, of course, other reasons for being interested in the stability of general equilibrium at least as a first topic in the attempt to provide the necessary foundation for equilibrium analysis. For one thing it (perhaps surprisingly) turns out to be easier in some senses to analyze stability in general models than in all but the simplest partial ones. This is partly because of the generality of the model, which keeps one from having to analyze various special cases and partly because of the general relationships – chiefly Walras' Law – which hold in full but not in partial

models. The literature (reviewed in the next chapter), it is true, has not always taken full advantage of these things, but, over time, they have come to be increasingly well understood. Plainly, if general equilibrium stability is tractable, it is the natural place to begin.

More important than this, however, is the central role which general equilibrium plays in economic analysis. Much of what economists have to say about the results of competition, the usefulness or lack thereof of governmental intervention, and the role of the price system is based on propositions about general equilibrium. These are the propositions rigorously formulated in modern times as the central theorems of welfare economics concerning the relations between Pareto optima and competitive equilibria. These propositions, which may be the single most important set of ideas that economists have to convey to laypeople, implicitly assume that general competitive equilibrium is stable and, indeed, that convergence takes place relatively quickly. If this were not so, welfare comparisons of equilibria would be largely irrelevant since what would matter would be comparison of the relatively "transient" behavior of alternative systems including alternative forms of market organization.

The importance of the stability of general equilibrium is not restricted to microeconomic analysis, however. The central question which Keynes sought to answer in *The General Theory of Employment, Interest and Money* (Keynes, 1936) was that of whether (and how) an economy could get stuck at an underemployment equilibrium. To show this, it is not enough to show that such an equilibrium exists, we must also show that it has at least local stability properties so that an economy that gets close enough to such a point will not escape from it without an exogenous change in circumstances. This problem, however, can be treated rigorously only as a problem in the stability of general equilibrium. The fact that Keynes and later macroeconomists did not so address it (had Keynes done so the *General Theory* might never have been written), but chose instead to use illuminating but fundamentally heuristic tools should not be allowed to obscure this. Like most questions in economic theory – micro or macro – the question of underemployment equilibrium has a general setting in back of it and, like all equilibrium questions, it involves a stability analysis to justify it.

1.5 Toward a satisfactory dynamic theory

I now briefly consider the features that a proper theory of disequilibrium adjustment should have if it is to provide a satisfactory underpinning for the use of general equilibrium tools, that is, if we are to show under what conditions the rational behavior of individual agents drives an

economy to equilibrium. There already emerge from such considerations some lessons for the use of equilibrium tools. The themes sounded here recur with more development in later chapters.

The first thing to say about a satisfactory theory has already been indicated. Such a theory must involve dynamics with adjustment to disequilibrium over time modeled. It will not do to attempt to buttress equilibrium analysis by assuming disequilibrium away. Further, while the analysis of temporary equilibria or of quantity-constrained, fixed-price equilibria,[5] may be both interesting and important, no satisfactory theory can stop without explaining how such points are reached and why they are maintained, if indeed they are.

Clearly, as in any dynamic model, the most satisfactory situation would be one in which the equations of motion of the system permitted an explicit solution with the values of all the variables given as specific, known functions of time. In such a circumstance, not only would the question of stability be settled, but so would the pressing question of convergence speed, which could be directly computed. Further, the transient behavior of the system would be known explicitly. If convergence speed turned out to be low, we could then discard comparative statics for an explicit comparative dynamics. If convergence speed turned out to be high, an explicit closed-form solution would let us decide to which of several equilibria a newly disturbed system would tend – a point often overlooked and quite beyond the power of comparative statics.[6] More fundamentally, the path-dependent nature of the ultimate equilibrium (discussed below) would cease to be a problem if the path were known.

Unfortunately, such a closed-form solution is far too much to hope for. At the level of generality appropriate to a theory which seeks to provide the disequilibrium foundation for general equilibrium analysis, it would be inappropriate to assume specific functional forms for the equations denoting the behavior of agents. With such forms, the obtaining of an explicit solution would be laborious and almost certainly a problem in numerical analysis. Without them it is literally impossible. Whether or not econometric estimation can ever reach the point where satisfactory prediction of the behavior of all prices, supplies, and demands can be obtained for a particular economy, it can never substitute for a satisfactory theory of economies in general.

We must thus work with the kinds of restrictions on functional forms which are generated by theory. These must be grounded in the theory of

[5] There is a very large literature on these topics. Drazen (1980) presents a survey.
[6] See Arrow and Hahn (1971) for a discussion of such difficulties.