CHAPTER SIX
On the Interpretation of General Equilibrium Models

In chapter 5 we saw that intertemporal general equilibrium models make claims about rates of interest without mentioning any aggregate quantity of capital or period of production. From the perspective of such models, the Cambridge criticisms are of little interest. Perhaps neoclassical economists have at least arrived at a rigorous, coherent theory of capital and interest and their relations to exchange values. Yet intertemporal equilibrium models like the one in §5 of chapter 5 are puzzling. Do they truly provide an adequate account of capital and interest? Can they help one explain the principal phenomena of interest or profits? We have many different questions to consider.

The elegant models of general equilibrium which have been developed during the past three decades have puzzled even their makers, since they appear to have little to do with real economies. Some, like Debreu (1959:ix) and Malinvaud (1972:242), believe that these models help explain or analyze prices. Others, like Arrow and Hahn (1971:vi–viii) deny that general equilibrium models can be used to explain. To assess general equilibrium models as models of capital and interest, one needs to understand clearly what these models are and whether they have the characteristics models must have when they are used in explanations. In this chapter, after dismissing some misconceived criticisms of general equilibrium models and emphasizing their flexibility, I shall offer a philosophical interpretation of these models. My goal in this chapter is to make the conceptual structure of general equilibrium models and the relations between these models and the whole neoclassical approach to economics as clear and intelligible as possible.

1. Some Misconceived General Criticisms

Intertemporal general equilibrium models are elegant and the existence proofs which employ them are formally valid. Yet these models do not bear their economic meaning on their face. As I have described them thus far, they are liable to several objections. In this section I shall consider some apparent difficulties with the concepts, logical structure, and testability of general equilibrium theories. The three criticisms discussed here all rest on a misunderstanding of general equilibrium theorizing. Clearing away these apparent difficulties helps sharpen one's understanding of this theorizing. I offer here no assessment of general equilibrium theories.

In intertemporal equilibrium models, the quantities which affect the day-to-day operation of an economy—contemporary prices and the expected and actual returns on investments—need never even be mentioned. One has a model of the progress of an economy over time in which there are no decisions or choices made over time and in which no commodity lasts more than one time period. Can such models depict the real course of an economy? (Harcourt 1976, p. 33).

They can. The basic features of a time sequence are present in general equilibrium models. The production functions implicitly embody causal-temporal constraints on production; the utility functions implicitly embody sequences in consumption. The utility functions depend upon the recognition that different commodities are the same good, even though the notion of a good plays no role in the equilibrium solution. Given the assumption of perfect information, there is no difference between deciding on December 1, 1960, or on June 3, 1999, what to do on June 3, 1999. If there is anything strange here, it lies with the assumption of perfect information rather than with the notion of an intertemporal equilibrium.

But, a critic might continue, how can a theory in which contemporary prices and the rate of interest are not even mentioned possibly be correct? As Smith recognized and Ricardo stressed, capital tends to flow into sectors where large profits are being made and out of sectors in which the rate of profit is low. For Ricardo it is this tendency toward the equalization of the rate of profit that explains the uniformity of the wages paid to labor of a given quality or of the rent paid on the land of a given fertility and convenience. The reactions of capitalists to changes in profit margins are fundamental organizing forces. The capitalist's search for profits is the mainspring of economic life. Economists concluded that the rate of interest (or profits) is a crucial theoretical variable in any fundamental economic theory applicable to a capitalist economy. Yet the rate of interest has no explicit role in an intertemporal equilibrium model. How can such models possibly be adequate?

What takes the place in intertemporal equilibrium models of any mention of a general rate of interest is the condition that pure economic
profits be zero. What Ricardo called the average rate of profit is regarded as a sum of interest costs for each of the various commodities. This cost is completely determined by knowledge of the various own rates of return. In fact one can easily recast Ricardo's original corn model (1815:9–42) in the concepts of intertemporal equilibrium models. In that model there is one commodity, corn, which, along with labor is used as an input into its own production. From corn output one subtracts corn input, the corn wages and the corn rent to get net corn output. If the rate of profit (net corn output divided by corn costs) is higher on one piece of land than on another, capital tends to move and rents will be bid up or down. The same story can be told in terms of an intertemporal equilibrium model. The technical interest rate for corn and the own corn rate of interest are positive. The discounted price of next year's corn is less than the price of this year's corn. The discounted net returns from corn production must be zero in equilibrium. If they are not zero, exactly the same processes of bidding up or down of rents and shifting of capital will take place. The notion of interest as a cost rather than a return has merely been taken seriously, and this cost has been disaggregated.

But can this story be told in terms of intertemporal equilibrium, in which the future holds no unexpected changes? Can the notion of an intertemporal equilibrium help one understand real economies, which are never in equilibrium at any moment, let alone in an intertemporal equilibrium? Every economics student learns that with changes in the conditions of production or with changes in tastes, the quantity supplied or demanded at what had been the normal price changes. The result is a bidding up or down of the price and an increase or decrease in the profits in that industry, which will, given competition, cause a movement of capitals until the market clearing price is an equilibrium price at which only normal profits are made. Since economies are never in equilibrium, such processes are always occurring and are central to the subject matter of economics.

Such a critic might go on to argue that stationary equilibrium models cohere, although uneasily, with this model of disequilibrium processes. Initially an economy is near a stationary equilibrium state. That equilibrium is then disturbed by changes in tastes or in production possibilities. As a result of the process of adjustment, a new stationary equilibrium would be achieved, were it not for other disruptions. It has been widely believed that short-run stationary equilibrium models provide a frame of reference or point of comparison for the analysis of actual economies.

An intertemporal equilibrium, on the other hand, is not an economic state toward which an economy can move. There is no sense attached to the notion of restoring an intertemporal equilibrium. If such an equilibrium does not exist during all periods of the relevant time interval, it does not exist at all. Can one tell the story of adjustments to price fluctuations within intertemporal equilibrium models? One might conclude that intertemporal general equilibrium models have no applications.

This criticism is, I think, mistaken. Considering and refuting it helps clarify the logical structure of equilibrium models. First, the assertion that atemporal general equilibrium models cohere or fit in with the story of economic adjustment is incorrect. As Robinson points out (1953–54), it is difficult to imagine any economy getting into a stationary equilibrium. In a stationary equilibrium, both the future and the past are identical to the present. The invidious comparison between stationary and intertemporal equilibrium models is unjustified.

The more important difficulty with this criticism is that it misunderstands the project of the general equilibrium theorist. Intertemporal equilibrium models are fundamentally accounts of the clearing of markets for both current commodities and for titles to future commodities and of the efficient allocation of resources over time. In order for the theory to explain how an economy actually behaves over a whole period (if anyone has such grand ambitions), the notion of a full intertemporal equilibrium is essential. If, on the other hand, one is interested in discussing the reactions of markets for current and future commodities to changes in givens, one can focus on variants of the basic notion of intertemporal equilibrium.

Suppose, for example, one surrenders the condition that everybody has perfect information about future commodities, their prices, and the production possibilities. Suppose instead that individuals have some way of ranking every bundle of commodities that anybody believes will exist at any time and that all have beliefs concerning future utilities and technological possibilities. The easiest, although not the most plausible, way of describing such a situation is to suppose that subjectively every agent is in exactly the same state as in an intertemporal general equilibrium model.\(^1\) Perfect certainty, but not omniscience, rules. The markets for current commodities and commodity futures are assumed to come rapidly to equilibrium. The proof of the existence of an intertemporal equilibrium demonstrates that such an equilibrium will exist, since it will be identical to an intertemporal equilibrium.

\(^1\) This is not the only way in which theorists have dealt with failures of expectations. Debreu (1959, ch. 7) has a different treatment. Arrow (1974a:286–69) introduces uncertainty intuitively via a notion of contingent commodities.
equilibrium. The difference is not in the determination of equilibrium, but in what happens at the end of the first period. Without perfect information, expectations will be disappointed. The economy will not be in the state individuals would have predicted at the beginning of the time interval. A new market equilibrium will need to be established.

The variation on intertemporal equilibrium models sketched above provides us with what are called "temporary" equilibrium models. Unlike either stationary or full intertemporal equilibrium models, they have elements of disequilibrium built into them. They can be meshed easily with the simple story of how an economy responds to changes in tastes and in conditions of production. In leading so directly to temporary equilibrium models, the efforts of the general equilibrium theorists may lead toward a better treatment of disequilibrium rather than, as the criticism above alleges, away from understanding actual (disequilibrium) economic states.

But why then does chapter 5 focus on intertemporal equilibrium models rather than temporary equilibrium models? First, theoretical discussions of capital and interest generally rely on full intertemporal general equilibrium models. Temporary equilibrium models do not have nearly the same range of acceptance. They require further elaboration. Second, temporary equilibrium models, as they currently stand, can provide no theory of actual returns. They are only a fragment of a theory of interest and profits. The markets for current commodities and commodity futures come to equilibrium. There is a going market rate of interest, which is determined by the same factors as in intertemporal equilibrium models. At the end of the period, however, expectations are disappointed. There is no reason why the sum of the actual returns on investments divided by the value of the investments should be the same, even in sign, as the market interest rate of the previous period.

This feature of temporary equilibrium models may turn out to be a great virtue. It might be argued that one should not expect to find any general theory of the actual rate of return. Uncontrolled, irregular exogenous political, sociological, or even meteorological factors perhaps have such a large role to play that no general account will be of any use. Alternatively, one might argue that temporary equilibrium models represent a step toward a more sensible theory of the rate of profit. All one needs is some account of the relations between expectations and past experience which relates expected and actual returns.

Both of these positions are plausible. But economic theorists do not yet possess a more sensible theory of returns and distribution that employs temporary equilibrium analysis, nor are they generally willing to give up attempting to explain actual returns and actual distribution. It thus seems sensible to focus on intertemporal equilibrium models. They purport, at least, to explain more and they remain central to theoretical discussion.

Notice also that the theoretical gains in shifting to temporary equilibrium models are small at present. Temporary equilibrium models avoid the objectionably strong assumptions that all agents have complete knowledge of the economic future and that the economy is in intertemporal equilibrium. Yet the models still stipulate that agents have perfect information about the availability of all current commodities. Second, temporary equilibrium models must assume that at least one complete commodity futures market exists. Third, they assume that individuals are willing and able to rank all possible future consumption bundles. Fourth, they assume that markets come to equilibrium at the beginning of each time period. Either time periods are short and this last assumption is a particularly strong one, or time periods are quite long and the perfect information requirement concerning the present period must bear the theoretical burden. In focusing on intertemporal general equilibrium models, as the basis for a theory of capital and interest, I am not overlooking a clearly superior option. When asking how neoclassical economists explain distribution or interest, one should concentrate on intertemporal general equilibrium models.

As we have seen in this section, several of the apparently counterintuitive features of general equilibrium models are much more reasonable than they seem. To understand these models clearly and to appreciate their importance to neoclassical economics, we must now consider carefully what the assumptions of neoclassical economics are and how they are related to general equilibrium models. Only then will we be in a position to offer a balanced assessment of these models.

2 The idea was first introduced explicitly to English-speaking economists by Hicks (1946:122-23). See Bliss 1975:57) and Grandmont (1977:535-72).

2. Equilibrium Theory and the Basic Equilibrium Model

To understand what general equilibrium models are, it is helpful to distinguish them from what I shall call "the basic equilibrium model," of which general equilibrium models are "augmentations" (and refinements). The basic equilibrium model is constituted by those assumptions which are common to most microeconomic and general equilibrium models. Having already described the basis of equilibrium
models in an informal and intuitive way (chapter 2), I shall now be more rigorous. Rigor can be deceptive, however: the rough descriptions are in some ways more revealing. As a fundamental model or as a foundation to a whole approach to economics, the basic equilibrium model cannot be captured precisely in any given formulation.

Most modern economists (although they do not use my terminology) regard the basic equilibrium model as fundamental to virtually all economic theory. They hope to be able to reduce, or at least relate, macroeconomic theories to the equilibrium model. They hope to be able to augment the basic equilibrium model to deal with questions of economic growth and change. This is the model they rely on in empirical research and in many welfare recommendations. When one has succeeded in saying what equilibrium models are, one has largely succeeded in saying what neoclassical economics is. General equilibrium models, whether abstract or practical, are not themselves the foundations of neoclassical economics. Those assumptions common to most equilibrium models, be they partial or general, are fundamental.

Looking back at the model in §5 of chapter 5 one can distinguish a number of assumptions which are common to most equilibrium models. These are of different kinds. The assumption that individuals are utility maximizers (C2) is perhaps an economic ‘law.’ Assumptions concerning information and the divisibility of commodities are ubiquitous simplifications, but are certainly not regarded as assertions or discoveries of economics. Although such simplifications are essential in most economic theorizing and are common constituents of economic models, they are neither assertions of economics nor, I suggest, part of the fundamental economic theory. I think we can better understand model building in economics if we focus on that subset of the assumptions common to equilibrium models which consists of basic assertions or principles.

‘Equilibrium theory’ is my name for the fundamental theory of microeconomics and indeed of neoclassical economics generally. Equilibrium theory consists of the basic principles or assertions of neoclassical economics. It asserts closures of some of the assumptions common to equilibrium models. Equilibrium theory is basically utility theory coupled with some generalizations concerning production and the motivation of firms or entrepreneurs. It may be formulated roughly as the following nine lawlike statements. Many qualifications are needed and will be discussed below.

(1) For any individual, $A$ and any two options, $x$ and $y$, one and only one of the following is true: $A$ prefers $x$ to $y$; $A$ prefers $y$ to $x$; $A$ is indifferent between $x$ and $y$.

(2) $A$’s preferences among options are transitive.

(3) $A$ chooses that option he or she believes maximizes his or her utility. (The utility of option $x$ is greater than the utility of option $y$ if and only if $A$ prefers $x$ to $y$. The utilities of options are equal just in case $A$ is indifferent between them.)

(4) If option $x$ is acquiring commodity bundle $x'$ and option $y$ is acquiring commodity bundle $y'$, and $y'$ contains as much or more of each commodity as $x'$ and more of at least one commodity, then all agents prefer $y$ to $x$.

(5) The marginal utility of a commodity $c$ to an agent $A$ is a decreasing function of the quantity of $c$ that $A$ has.

(6) When we increase any input into production with other inputs held constant, output increases, but, after a certain point, at a decreasing rate.

(7) Increasing all the inputs into production in the same proportion increases output by that proportion. The production set is weakly convex and additive.

(8) Entrepreneurs or firms perform those actions which they believe will maximize their profits.

(9) Through exchange the economic choices of individuals become compatible.

The talk of marginal utility in (5) and the supposition that utility functions are cardinal (that differences in levels of utility are significant) are easily extendable. Economists generally prefer to talk in terms of marginal rates of commodity substitution. The conceptual issues can be addressed more directly if one continues to speak in terms of marginal utilities. Everything I shall say can be translated into more usual terminology.

Many economists would say that (1)–(3) assert that people are rational and that (1)–(4) assert that people are economically rational (Hausman 1979b:120–21). Notice that (4) has the effect of identifying options and commodity bundles. Diminishing marginal utility is supposed to be a psychological law, which is sometimes (implausibly in my opinion) regarded as part of what it means to be economically rational. Constant returns to scale and diminishing returns to variable inputs are supposed to be in some sense natural laws or technological givens (Rosenberg 1976b:29–30). I think that it is a mistake to regard (6) or (7) as natural laws. Production functions, unlike chemical formulae, are implicitly assertions about our knowledge. To say that firms attempt to maximize profits is supposed to be a lawlike claim concerning motivation, possibly deducible from other features of our theory. If this deduction were carried through, (8) could be dropped from the list. Since entrepreneurs are also consumers, it actually seems that
(8) may conflict with (3). (9) is special. It is not usually an axiom, but is a consequence of (1)–(8) and further simplifications in particular models. Yet (9) is not merely a derivative result. Economists set up their models and organize their theories in order to get this result. Although often not an axiom, (9) should be included among the fundamental “laws.”

This set of lawlike statements is disquieting, since, even with qualifications, most of the claims appear to be false. Economists have, of course, recognized this peculiarity of (1)–(9); it is one of the reasons they prefer to think of their work in terms of models. Controversy concerning these purported laws has been extensive. Debate has focused on (or floundered around) four questions. Are these purported laws analytic or synthetic? Are they a priori or a posteriori? Are they theoretical laws or are they observational (that is, subject individually to direct observational confirmation or refutation)? Are they well confirmed or often falsified? Commentators on economic theory have disagreed drastically. These purported laws have been regarded as synthetic a priori (Von Mises 1960:12–13), (Hollis and Nell 1975, esp. ch. 9); analytic (Hutchinson 1938, ch. 2), false (Friedman 1953), and not fully interpreted (Machlup 1955, 1960). Thinking in terms of models demands a reformulation of the issues, but resolves none of the questions. I will offer my interpretation of these fundamental “laws” in §2 & §3 of chapter 7.

Notice that some of these generalizations seem reliable only in certain circumstances, given a certain institutional setting. One reason why it is reasonable to claim that individuals generally prefer more commodities to fewer (4), is that there is a market on which excess commodities can be easily traded. (4) is not however derivable from (1)–(3) and (5)–(8), conjoined with auxiliary hypotheses and statements of initial conditions concerning the existence of markets. There are, alternatives to (4) from which (4) may be derived, but none of these alternatives is appreciably weaker or more reasonable than (4) itself. (1)–(9) do not exhaust the general “laws” of microeconomics. They (or some reformulation of them) are, however, the basic “laws.” Other important lawlike claims (e.g., the price of a commodity will rise when demand for it exceeds its supply) can be derived from (1)–(8) and various stipulations concerning markets, information, and so forth. The above nine statements, however, express the fundamentals of neoclassical economics.

To assert that equilibrium theory is well formulated by (1)–(9) is rough and somewhat misleading. The assertion is rough because (1)–(9) are not a very precise statement of the theory; it is somewhat mis-

leading, because neoclassical economists do not always make use of all of the above lawlike statements [(7) and (8) are the most likely to be dropped or replaced by contrary generalizations, while revealed preference theory is supposed to supplant (1)–(5)].

Formulating equilibrium theory as (1)–(9) may also be incomplete. My suggestion that one should exclude other common assumptions of equilibrium models from the fundamental theory can certainly be questioned. To understand what equilibrium models are, one must also make reference to commonly used mathematical techniques and standard characterizations of the institutional, informational, and material background. These characterizations may themselves be lawlike to some extent. The notion of a “law” is sufficiently problematic that I do not want to stake my contrast between (1)–(9) and the other assumptions common in equilibrium models on the greater lawlikeness of (1)–(9). Other common assumptions differ in three ways from (1)–(9). First, they have a narrower scope; the assertion that there are many buyers and sellers is not supposed to be true of every market in every economy. Second, these simplifications are not regarded by economists as discovered or asserted by their theory; the simplifications sketch the circumstances in which the theory is applied and are crucial to the derivation of important theorems, but they are not themselves assertions of the theory. Finally, although these claims concerning markets, resources, information, and the like are important to equilibrium models, they are less important than are (1)–(9) or some reformulation of them. Neoclassical economic analyses, like the model in chapter 5, generally assume that commodities are infinitely divisible. This assumption has, as the result of employing better mathematical techniques, become avoidable in certain circumstances. Were it to become fully dispensable, economists would not, I believe, conclude that there had been any great revolution in their fundamental theory. If, however, a number of the mathematical techniques and assumptions concerning prevailing circumstances were to change, one might well regard the fundamental theory as changed.

Replacing any of (1)–(9) with a nonequivalent generalization would, on the other hand, count by itself as a change of theory. The distinction between the fundamental laws and simplifications concerning background conditions is not, however, sharp. (7) for example is a borderline case (Samuelson 1947:84). Most economists would regard it as a simplification rather than as a “law,” yet its scope is wide. It is not so crucial to the identity of equilibrium theory as is, for example, (3). Despite these many qualifications, it still seems to me enlightening to regard (1)–(9) as the fundamental theory of neoclassical economics.
3. General Equilibrium Models and General Equilibrium Theories

Given this analysis of equilibrium theory and of equilibrium models, we can now see that general equilibrium models are *augmentations* of the basic equilibrium model. The assumptions of a general equilibrium model like that of chapter 5 fall into three groups: (a) reformulations of (1)–(9); (b) assumptions like perfect information or infinite divisibility of commodities, which are common to equilibrium models generally; and (c) assumptions that there are many commodities and that there is a general interdependence among the various markets. Assumptions of the third class are what distinguish general equilibrium models from other equilibrium models. What I mean by calling general equilibrium models "augmentations" of the basic equilibrium model should be clear. Further assumptions are added to reformulations (with occasional revisions) of assumptions common to equilibrium models generally.

As mentioned above, there is no non-trivial set of assumptions which is part of every equilibrium model. There is scarcely an assumption which is even *consistent* with the assumptions of every equilibrium model. There are models without perfect competition, without infinite divisibility of commodities, without perfect information, without constant returns to scale, and so forth. All I am claiming is that almost all neoclassical theories assert most of (1)–(9) and that almost all equilibrium models assume most of them. The basic equilibrium model also includes such simplifying assumptions as perfect information, infinite commodity divisibility, and so forth.

Partial equilibrium models augment these assumptions with further axioms of two distinctive kinds. First, certain economic magnitudes like prices, incomes, quantities produced—which are in fact influenced by the workings of the market and which are sensitive to other factors in the economy—are taken as constants whose values are given. This permits independent consideration of single markets and permits one to consider factors influencing supply separately from those influencing demand. Supply and demand are principally partial equilibrium concepts. Second, to simplify matters, one works with certain sorts of aggregates. Instead of considering the complex changes in prices that result when one employs different techniques of production, one might assume as Lange does that all consumption goods can be treated as one commodity and all capital goods as another single commodity. If one is concerned with certain aspects of consumer choice, one might assume that all but one or two commodities which are of particular interest can be considered as a single composite commodity. It is thus incorrect to assert that microeconomics never deals with aggregates. These isolating and aggregating assumptions distinguish partial equilibrium models. In each case the state of mutual adjustment or equilibrium economists are considering is only partial, since they are ignoring adjustments in other markets and in other choices.

General equilibrium models avoid these isolating and aggregating assumptions. In general equilibrium models there are many commodities and a general interdependence among the various markets. There can, of course, be equilibrium analyses which are hard to classify as either partial or general. There are also two quite different varieties of general equilibrium models which need to be sharply distinguished. One of these is of practical use, while the other (like the model of chapter 5) is quite abstract. The first kind is exemplified by input-output models. By assuming, for example, that there are constant production coefficients and ignoring influences of demand, one can set up a model of an economy with perhaps a hundred different commodities and industries and, with the help of a computer, investigate how it operates. Practical general equilibrium models raise no other philosophical questions than do partial equilibrium models.

Models of the second kind, which I shall call "abstract general equilibrium models," place no limitations on the interdependence of markets or on the nature of production and demand beyond those implicit in the lawlike claims. When economists speak of general equilibrium models, it is usually this second, abstract variety that they have in mind. In the quotations near the beginning of chapter 5 it seems to me as if Samuelson and Solow are thinking of both kinds of general equilibrium models at the same time. They are thinking of practical general equilibrium models, since they believe that these models ac-
tually have explanatory and predictive uses (which, as I shall argue in chapter 7, abstract general equilibrium models do not have). Yet they must also be thinking of abstract equilibrium models, since practical models like input-output analysis provide no theory of the relations between interest and prices. With fixed input coefficients one cannot explain own rates of interest in terms of own technical interest rates. Without reference to utility functions, one cannot invoke own subjective interest rates in explaining the rate of interest. Demand and technology cannot be fixed if one wishes to account for the rate of interest as the equilibrium theorists wish to. Only abstract general equilibrium models can provide theories of capital and interest.

To assess what neoclassical economics has to say about capital and interest, one must thus focus on abstract general equilibrium models. It is important to stress that such models are not the fundamental models of neoclassical economics. As I argued in §2, the fundamental theory is equilibrium theory. Paradoxically, the basic equilibrium model is much more general than a general equilibrium model. General equilibrium models are augmentations of the fundamental model. General equilibrium theories (which follow from theoretical hypotheses employing general equilibrium models) are not the fundamental theory. These claims are not arbitrary stipulations. When scientists have some explicit explanatory or predictive purposes or when they want to develop a theory of a specific subject matter, they often augment the axioms of some fundamental model (or theory). Adding further assumptions to Newton’s laws, one can set up a model of a solar system. Such augmentations are precisely what general equilibrium models are. They are unlike fundamental models in the natural sciences. Equilibrium theory serves much of the same role in economics that fundamental theories in the natural sciences serve in their respective domains.

Intertemporal equilibrium models, have the form of specialized models or of potential explanatory arguments. Yet the assumptions of such models concerning information, markets, and the like seem ill-suited for the purposes of explaining or predicting economic phenomena. Models like the one in §5 of chapter 5 assume that agents have complete and accurate knowledge concerning the availability and prices of commodities as well as the present and future production possibilities. They also stipulate that there is a complete set of commodity futures markets on which present commodities (or titles to future commodities) can be freely exchanged for titles to future commodities of every kind and date. Such assumptions render the models so obviously inapplicable that one cannot confirm theoretical hypotheses employing them.

Furthermore, since the assumptions of such models are not even approximately true in real economies, the models have almost no predictive worth. This explains why theorists like Milton Friedman are so unsympathetic to the “Walrasian viewpoint” (1953:89–92). Given the near absurdity of such stipulations as perfect information, one wants to know what the point is of abstract general equilibrium models. Can they really provide a fundamental theory of capital and interest as Samuelson, Solow, and Bliss believe? Can economists employ such models to explain the phenomena of profits? To this crucial problem we must now turn.